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Attorneys for Plaintiffs Montana Democratic Party and Mitch Bohn

IN THE MONTANA THIRTEENTH JUDICIAL DISTRICT COURT YELLOWSTONE COUNTY

Montana Democratic Party, Mitch Bohn,

Plaintiffs,

WESTERN NATIVE VOICE, Montana Native Vote, Blackfeet Nation, Confederated Salish and Kootenai Tribes, Fort Belknap Indian Community, and Northern Cheyenne Tribe,

Plaintiffs,

Montana Youth Action; Forward Montana Foundation; and Montana Public Interest Research Group

Plaintiffs,

v.

Christi Jacobsen, in her official capacity as Montana Secretary of State,

Defendant.

Consolidated Case No. DV 21-0451

DECLARATION OF MATTHEW GORDON

I, Matthew Gordon, declare as follows:

My name is Matthew Gordon. I am over 18 years old and am an attorney with the law firm of Perkins Coie LLP. I am admitted to practice law in the State of Montana and am an attorney for Plaintiffs Montana Democratic Party and Mitch Bohn in this matter. I submit this declaration to provide the Court with true and correct copies of certain documents submitted in connection with Plaintiffs' Combined Response to Defendant's Motion for Summary Judgment in this matter.

1. Exhibit 1 is a true and correct copy of MIT Election Data + Science Lab, Voter Confidence (April 2021), https://electionlab.mit.edu/research/voter-confidence.

2. Exhibit 2 is a true and correct copy of the Expert Rebuttal Report of Dr. Alex Street.

3. Exhibit 3 is a true and correct copy of the NPR/PBS NewsHour/Marist Poll of 1,209 National Adults, downloaded from: https://maristpoll.marist.edu/wp-content/uploads/2021/10/NPR_PBS-NewsHour_Marist-Poll_USA-NOS-and-Tables B 202110251104.pdf (last visited Apr. 5, 2022).

4. Exhibit 4 is a true and correct copy of Andrew C. Eggers, Haritz Garro & Justin Grimmer, No Evidence for Systematic Voter Fraud: A Guide to Statistical Claims About the 2020 Election, 118 PNAS 1 (2021), downloaded from:

https://www.pnas.org/doi/pdf/10.1073/pnas.2103619118.

5. Exhibit 5 is a true and correct copy of Amanda Zoch, *Then & Now: How 8 Election Policies Have Changed Since 2000*, Nat'l Conference of State Legislatures (Feb. 16, 2021), downloaded from: https://www.ncsl.org/research/elections-and-campaigns/then-and-nowelection-policies-in-2000-and-2020-magazine2021.aspx/.

6. Exhibit 6 is a true and correct copy of Wendy Weiser, Justin Levitt, Catherine Weiss, & Spencer Overton, Response to the Report of the 2005 Commission on Federal Election Reform 2, 7 (2005), downloaded from: https://www.brennancenter.org/sites/default/files/2019-08/Report_Response%20to%20the%20Report%20of%20the%202005%20Commission%20on% 20Federal%20Election%20Reform.pdf.

- 2 -

Exhibit 7 is a true and correct copy of Enrico Cantoni & Vincent Pons, Strict ID
 Laws Don't Stop Voters: Evidence from a U.S. Nationwide Panel: 2008-2018, Q.J. Econ. 2615, 2653-54 (2021), downloaded from: https://doi.org/10.1093/qje/qjab019.

Exhibit 8 is a true and correct copy of the Expert Rebuttal Report of Dr. Kenneth
 R. Mayer submitted in this matter.

9. Exhibit 9 is a true and correct copy of Michael G. DeCrescenzo & Kenneth R. Mayer, *Voter Identification and Nonvoting in Wisconsin—Evidence from the 2016 Election*, 18 Election L.J. 342, 342 (2019), downloaded from:

https://www.liebertpub.com/doi/10.1089/elj.2018.0536.

Exhibit 10 is a true and correct copy of John Kuk, Zoltan Hajnal, & Nazita
 Lajevardi, A Disproportionate Burden: Strict Voter Identification Laws and Minority Turnout,
 Pol. Groups, & Identities 1 (2020), downloaded from:

https://doi.org/10.1080/21565503.2020.1773280.

11. Exhibit 11 is a true and correct copy of Bernard L. Fraga & Michael G. Miller,
Who Does Voter ID Keep from Voting?, 84 J. Pol. 1 (2022), downloaded from:
https://www.journals.uchicago.edu/doi/10.1086/716282.

12. Exhibit 12 is a true and correct copy of Justin Grimmer & Jesse Yoder, *The Durable Differential Deterrent Effects of Strict Photo Identification Laws*, Pol. Sci. R. & Methods 1 (2021), downloaded from: https://doi.org/10.1017/psrm.2020.57.

13. Exhibit 13 is a true and correct copy of Matt A. Barreto, Gabriel R. Sanchez, and Hannah L. Walker, *Battling the Hydra: the Disparate Impact of Voter ID Requirements in North Dakota*, J. Race, Ethnicity, & Pol. 1 (2022), downloaded from:

https://doi.org/10.1017/rep.2022.1.

14. Exhibit 14 is a true and correct copy of the Expert Report of Barry C. Burden submitted in *Andrew Goodman Foundation v. Bostelmann*, No. 19-cv-955 (Jan. 20, 2020).

15. Exhibit 15 is a true and correct copy of Charles Stewart III et al., *Revisiting Public Opinion on Voter Identification and Voter Fraud in an Era of Increasing Polarization*, 68 Stan. L. Rev. 1455 (2016).

16. Exhibit 16 is a true and correct copy of Nat'l Public Radio, *Here's Why Concerns* About Absentee Ballot Fraud are Overhyped (Oct. 20, 2020),

https://www.pbs.org/wgbh/frontline/article/heres-why-concerns-about-absentee-ballot-fraud-areoverhyped/ (last accessed Mar. 23, 2022).

17. Exhibit 17 is a true and correct copy of Keith Schubert, '*Practically a unicorn*': *Profs say voter fraud allegations in Phillips Co. not part of larger issue*, Daily Montanan (Feb. 14, 2022), downloaded from: https://dailymontanan.com/2022/02/14/practically-a-unicorn-profssay-voter-fraud-allegations-in-phillips-co-not-part-of-larger-issue/.

 Exhibit 18 is a true and correct copy of Lisa Baumann, *Ending Election Day* registration sees little support, Associated Press (Oct. 19, 2014), downloaded from: https://www.greatfallstribune.com/story/news/local/2014/10/19/ending-election-day-registrationsees-little-support/17583087/.

19. Exhibit 19 is a true and correct copy of U.S. Election Assistance Commission, *Election Crimes: An Initial Review and Recommendations for Future Study* 9 (Dec. 2006), downloaded from:

https://www.eac.gov/sites/default/files/eac_assets/1/6/Initial_Review_and_Recommendations_fo r_Further_Study.pdf.

20. Exhibit 20 is a true and correct copy of Steven H. Huefner, Daniel P. Tokaji, Edward B. Foley, and Nathan A. Cemenska, *From Registration to Recounts: The Election Ecosystems of Five Midwestern States* 120 (2007).

21. Exhibit 21 is a true and correct copy of Steven F. Huefner, Nathan A. Cemenska, Daniel P. Tokaji, and Edward P. Foley, *From Registration to Recounts Revisited: Developments in the Election Ecosystems of Five Midwestern States* 41 (2011).

22. Exhibit 22 is a true and correct copy of Michael W. Sances and Charles Stewart III, *Partisanship and Confidence in the Vote Count: Evidence from U.S. National Elections Since* 2000, Electoral Studies 40:176-188 (2015), downloaded from: https://doi.org/10.1016/j.electstud.2015.08.004.

23. Exhibit 23 is a true and correct copy of Stephen Ansolabehere, *Effects of Identification Requirements on Voting: Evidence from the Experiences of Voters on Election Day*, PS: Political Science & Politics 42:127-130 (2009), downloaded from: https://doi.org/10.1017/S1049096509090313.

24. Exhibit 24 is a true and correct copy of Shaun Bowler, Thomas Brunell, Todd Donovan, and Paul Gronke, *Election Administration and Perceptions of Fair Elections*, Electoral Studies 38:1-9 (2015), downloaded from: https://doi.org/10.1016/j.electstud.2015.01.004.

25. Exhibit 25 is a true and correct copy of Keila Szpaller, *Election Security Bill Heads to Gov. Gianforte's Desk* (Apr. 27, 2021), downloaded from: https://dailymontanan.com/2021/04/27/election-security-bill-heads-to-gov-gianfortes-desk/.

26. Exhibit 26 is a true and correct copy of Sam Wilson, *GOP in Missoula Pays for Recount to Ease Fraud Concerns* (Mar. 29, 2022), downloaded from: https://missoulian.com/news/state-and-regional/govt-and-politics/gop-in-missoula-pays-forrecount-to-ease-fraud-concerns/article 0304fa52-a9c0-5502-ad63-78fa2938af19.html.

27. Exhibit 27 is a true and correct copy of Alex Sakariassen, *Missoula County GOP to Republican Election Skeptics: 'No Voter Fraud*' (Apr. 1, 2022), downloaded from: https://montanafreepress.org/2022/04/01/missoula-election-allegations-challenged/.

I declare under penalty of perjury that the foregoing is true to the best of my knowledge and belief.

Dated this 5th day of April, 2022.

Math

Matthew Gordon

Exhibit 1

+ MIT ELECTION DATA + Science Lab

Voter confidence

The presidential election of 2000 and the highly controversial recount in Florida brought the term "voter confidence" into the political lexicon. However, the rhetorical pull of the term "voter confidence" is much stronger than proof of any actual empirical relationship between voter confidence and voter turnout.

This explainer was last updated on April 2, 2021.

INTRODUCTION

In the summer of 2001, the National Commission on Federal Election Reform issued its report, <u>To</u> <u>Ensure Pride and Confidence in American Elections</u>. Seven years later, the <u>U.S. Supreme Court</u> decided in *Crawford v. Marion County Board of Elections* that strict photo voter ID laws were constitutional. Justice John Paul Stevens wrote that "public confidence in the integrity of the electoral process has independent significance because it encourages citizen participation in the democratic process." Against the aftermath of the 2020 elections, many state legislatures considered a variety of restrictive election laws, including in Georgia. In the remarks he made after signing SB 202, Governor Brian Kemp <u>stated</u>, "There's no doubt there were many alarming issues with how the election was handled, and those problems understandably led to a crisis of confidence in the ballot box here in Georgia."

Viewing voter confidence as a measure of the quality of American elections and assuming that voter confidence is linked to the willingness of Americans to vote are intuitively appealing. However, research indicates only a weak causal connection between voter confidence and voter turnout, and it does not show clear causal links between certain high-profile election administration practices, such as voter ID laws, and voter confidence. Surveys have been measuring public opinion about voter confidence for two decades. How the question about confidence is asked generally determines whether voters are deemed to have high or low confidence in elections. The strongest influence on levels of voter confidence, regardless of how the question is asked, is whether one's candidate has won or lost an election. Beyond the "losers' regret" phenomenon, voter confidence is influenced in smaller ways by the intensity of partisan competition and the experience of casting ballots.

MEASURING CONFIDENCE

In measuring any concept, it's important to know what's being measured. When we try to see if voters are confident, we need to ask exactly what they're confident about—that the election was fair? That the votes were counted as they were cast?

When political scientists study voter confidence, they ask voters some variant of the question, "Do you believe that votes in the most recent election were counted as cast?" Note that this question focuses on the mechanics of marking a ballot and having it counted accurately, which is narrower than asking whether the last election was fair—and asking whether the last election was fair is narrower than asking whether "elections in America are usually fair."

Answers about voter confidence are fairly consistent regardless of the precise wording of the question. However, there is one important exception to this pattern of consistency. When survey respondents are asked about their confidence in the parts of the electoral process with which they have direct contact, such as their own vote, they are much more confident than when they are asked about parts of the electoral process they have *indirect* contact with, such as the process in the nation as a whole. One consequence of this is that in political debates involving voter confidence, it is possible to cherry-pick survey research in support of arguments that voters either have high levels of confidence in American politics or do not.

Here's an example of how degrees of voter confidence vary with the degree of direct voter contact with the process. In the MIT module to the 2020 <u>Cooperative Congressional Election Study</u> (CCES), respondents were asked the following question before the election: "How confident are you that your vote in the General Election will be as you intend?" They were also asked, "How confident are you that votes in [your county/your state/nationwide] will be counted as voters intend?" The response categories were very confident, somewhat confident, not too confident, not at all confident, and don't know.

In looking at summaries of the answers to these questions (Figure 1), two patterns immediately jump out. First, regardless of the target of the question ("your vote," the "local vote," etc.), confidence was greater after the election than before. For instance, the percentage of voters who said they were very confident their vote would be counted as they intended was 38% before the election, but this jumped to 61% when they were asked after the election.



Figure 1: Voter confidence in the 2020 gener

Second, confidence was greatest—both before and after the election—as the target of the question got closer to the voter. For instance, while 38% of respondents overall were very confident their own vote would be counted as intended in the pre-election survey, 35% were very confident that votes would be counted as intended in their own county or community. These percentages dropped to 29% and 15% when the question asked about the state and the nation, respectively.

Questions about the mechanical aspects of voting tend to elicit more optimistic responses in publicopinion surveys than vague questions about the honesty of elections. For instance, immediately before the 2016 election, the <u>Gallup organization</u> asked respondents, "How confident are you that, across the country, the votes will be accurately cast and counted in this year's election?" To this question, 69% responded that they were either very or somewhat confident. However, the poll also asked about how much confidence they had in the "honesty of elections." Here, only 30% answered they were confident.

CONFIDENCE AND OUTCOMES

We've already seen that answers to voter-confidence survey questions vary as the target of the question changes. What else affects voter confidence? The main answer points us in two directions related to political outcomes—who wins, and how close elections are.

The "winner's effect" is illustrated by the changing answers to voter confidence questions since they were first asked in 2000. Figure 2 shows the percentage of respondents who reported they were very confident their vote was counted as intended in the Survey of the Performance of American Elections, which was conducted immediately after the 2008, 2012, 2016, and 2020 elections. The data for prior elections shows the percentages taken from commercial public opinion polls and reported in research by <u>Michael Sances and Charles Stewart</u>.





Note: Data from 2008-2020 are from the Survey of the Performance of American Elections

In the first two elections, Republicans were more likely by a margin of 20 percentage points to say they were confident that their vote was counted as intended. The relative opinions of Democrats and Republicans switched in 2008 and became even more entrenched in 2012. Republican confidence rebounded in 2016 while Democratic confidence sagged.

All the major changes in confidence correspond with changes in the parties' electoral fortunes. This pattern held true in 2020, but we observe the largest partisan gap to date at 32 percentage points between Democrats and Republicans. This can likely be attributed to sitting president Trump's attempts to cast doubt on the results throughout the electoral process. (<u>Sources</u> show surveyed Republicans cite Trump as their primary source for believing fraud occurred) In spite of increased party polarization, notice that the overall average level of voter confidence has not change very much across the past six elections.

Another way elections influence voter confidence is through the sniping that goes on between candidates in the heat of a campaign. In a close contest, it's common for both sides to accuse the other of dirty tricks. The effects of such sniping are evident when we asked respondents whether votes in their own state were counted as intended. Figure 3 shows the percentage of voters who were very confident that their own states' votes were counted as intended in the 2020 presidential election on the y-axis, and the percentage share of the vote for Trump on the x-axis. The two lines display the relationship between the points in states that Trump lost (left) and Trump won (right), respectively. Note that regardless of the winner, the closer the elections are (proximity to 50% on the x-axis) the less confident voters are in the result.



Figure 3: Voter confidence in the 2020 elec

Proportion of votes for Trump

CONFIDENCE AND ADMINISTRATION

It's common to justify election reforms by arguing they will increase voter confidence in the electoral system. However, there's little evidence that election administration has a direct effect on voter confidence. The major exception to this statement is that voters who experience problems at

polling places tend to be less confident than voters who don't.

The issue of voter confidence and election reforms has been front and center in justifications for stricter voter ID laws. As noted above, Justice Stevens's decision justifying the constitutionality of strict voter ID laws in *Crawford v. Marion County Board of Elections* credited Indiana's argument that strict voter ID laws could increase voter confidence. However, subsequent research on this question reveals no correlation between the adoption of strict voter ID laws and increases in voter confidence. Indeed, if anything, the political climate created by debates about strict ID laws could actually be reducing confidence and further polarizing opinions along partisan lines.

Where election administration has some influence on voter confidence is on how voters experience the process and whether that experience is positive. Research by scholars such as <u>Lonna Atkeson</u>, <u>Mike Alvarez</u>, <u>Thad Hall</u>, and <u>Paul Gronke</u> tells us that voters tend to be more confident when they don't wait a long time to vote, when they encounter polling place officials who seem competent, and when they vote in person rather than by mail. Some of these factors certainly can be affected by state policies, but more often, they are influenced by local administrators' decisions about how to allocate resources to polling places and how rigorously they train poll workers.

DATA SOURCES

Pew Research Center | Public opinion research and reports

Roper Center | Public opinion research

SUGGESTED READINGS

<u>Alvarez, R. Michael, Thad E. Hall, and Morgan H. Llewellyn. 2008. "Are Americans Confident their</u> <u>Ballots are Counted?" *The Journal of Politics* 70 (3): 754–766.</u>

<u>Atkeson, Lonna Rae, and Kyle L. Saunders. 2008. The Effect of Election Administration on Voter</u> <u>Confidence: A Local Matter?" *PS: Political Science & Politics* 40 (4): 655–660.</u>

<u>Atkeson, Lonna Rae, R. Michael Alvarez, and Thad E. Hall. 2015. "Voter Confidence: How to Measure It</u> and How It Differs from Government Support." *Election Law Journal* 14 (3): 207–219.

<u>Gronke, Paul, Eva Galenes-Rosenbaum, Peter A. Miller, and Daniel Toffey. 2014. "Voter Confidence as a</u> <u>Metric of Election Performance." In *The Measure of American Elections*, eds. Barry C. Burden and <u>Charles Stewart III. New York: Cambridge University Press, 248–270.</u></u>

Hall, Thad E., J. Quin Monson, and Kelly D. Patterson. 2009. "The Human Dimension of Elections: How Poll Workers Shape Public Confidence in Elections." *Political Research Quarterly* 62 (3): 507–522. <u>Sances, Michael W., and Charles Stewart III. 2015. "Partisanship and Confidence in the Vote Count:</u> <u>Evidence from U.S. National Elections since 2000." *Electoral Studies* <u>40 (December 2015): 176–188.</u></u>

<u>Sinclair B, Smith SS, Tucker PD. "It's Largely a Rigged System": Voter Confidence and the Winner</u> <u>Effect in 2016. Political Research Quarterly. 2018;71(4):854-868. doi:10.1177/1065912918768006</u>

Exhibit 2

Response of Alexander Street, Ph.D., to reports by Mr. Sean P. Trende and Mr. Scott E. Gessler, in the case of *Western Native Voice v. Jacobsen* March 25, 2022

I. Purpose

The reports submitted in the case of *Western Native Voice v. Jacobsen* by the Defendant's experts, Mr. Gessler and Mr. Trende, feature errors and gaps that risk leading the court to mistaken conclusions.¹ I am writing this response to explain three classes of problems with those reports and to offer evidence to counter mistaken assertions therein. First, Mr. Gessler and Mr. Trende present incomplete accounts of previous research and thereby risk giving the inaccurate impression that the academic literature on the effect of Election Day Registration (EDR) on turnout is equivocal. Second, their reports cite little evidence about the circumstances of Montana elections, and no evidence at all on the particular conditions of Native American voters living on rural reservations in Montana. Instead, they speculate that ending EDR and banning groups such as Western Native Voice from helping to return ballots would do little harm and might even confer some benefit (e.g., shorter lines on Election Day)—I respond by providing further evidence on the particular importance of EDR in Montana, and by explaining why these alleged benefits of ending EDR are unlikely to ensue. Third, Mr. Gessler's report includes unfounded claims about the impact of HB 530 on voter confidence, while Mr. Trende's report includes errors on this topic. I respond by placing those claims in context using scholarship on reported voter confidence and perceptions of election fraud, and by explaining, using survey data from Montana, why HB 530 is unlikely to increase voter confidence.

II. A more complete account of the academic literature on EDR

Mr. Gessler's report misconstrues this field of research. Mr. Gessler refers to a Government Accountability Office (GAO) report on election administration that reviews scholarly research on the effects of Same Day Registration (SDR) and/or EDR on turnout.² As that report states,

¹ In this response I focus on claims from Mr. Gessler and Mr. Trende about HB 176 and HB 530. I do not offer any comment on their claims about issues relating to the other cases that have been consolidated with this one. ² See https://www.gao.gov/assets/gao-16-630.pdf, pages 88-92, accessed March 4, 2022

"We reviewed 33 studies in 17 publications, and 21 of these studies found increases in turnout, 3 studies found mixed effects (positive increases combined with non-significant findings), while 9 studies found no statistically significant effects." Mr. Gessler's own rewording of this statement is incorrect, however, since Mr. Gessler reinterprets this statement as implying that 9 studies "found that SDR or EDR did not increase turnout" (Gessler report at page 16). Under the frequentist statistical framework, absence of evidence for an effect is not the same as evidence for the absence of an effect, since the convention is to reject the "null" hypothesis of no effect only when there is strong evidence to the contrary. The most common threshold is to reject the null hypothesis when there is only a five percent chance, or less, of any observed deviation from the null having arisen merely due to chance, if the null were true; this is commonly expressed as a *p*-value < 0.05 or as "statistical significance." It is also worth noting that the GAO report is not an exhaustive summary of all research on the effects of EDR on turnout. Other studies, not included in that report, also tend to find that EDR has a positive effect on turnout.³ Mr. Gessler's report understates the breadth of support for the research finding that EDR tends to result in higher turnout.

Mr. Trende contends that research on the effects of EDR⁴ on turnout is "a fraught exercise at best" (Trende report at page 7). However, this account of the literature is incomplete and, potentially, misleading. Contrary to the dire account in Mr. Trende's report, the most plausible

³ The GAO report excludes some older research on the topic, but it also lacks some of the most recent evidence (published since the GAO report). See, for instance, another recent study finding a positive effect of EDR (which the authors construe as a form of Same-Day Registration or SDR) on turnout, especially for younger citizens: Jacob M. Grumbach and Charlotte Hill, "Rock the Registration: Same Day Registration Increases Turnout of Young Voters," *The Journal of Politics* 84, no. 1 (January 2022), 405-417. Papers using specific statistical techniques for combining the results from multiple studies—a more formal approach than is taken by the GAO report, which simply lists the findings from various studies—also find evidence for effects of voter registration requirements, including EDR, on turnout. See, e.g., João Cancela and Benny Geys, "Explaining voter turnout: A meta-analysis of national and subnational elections," *Electoral Studies* 42 (June 2016), 264-275.

⁴ I understand Election Day Registration (EDR) to be a special form of Same Day Registration (SDR). SDR allows voters to register and vote in a single trip to an elections office. Since 2005 in Montana, it has been possible to register and vote at a county elections office (or some satellite offices) during the 30 days after the close of regular registration, i.e., during the "late registration" period. Crucially, this has been possible on Election Day itself. EDR has special importance because voter interest peaks on Election Day. As I will show at greater length in Section III of this report, around half of *all* the registrations during the late period have, in recent Montana elections, happened on Election Day.

overview of the scholarly literature is that EDR tends to increase turnout, and, correspondingly, that eliminating EDR is likely to reduce turnout. This much is clear. Mr. Trende describes the challenges that scholars face in showing causality using observational data (e.g., data on the observed behavior of voters, as opposed to experimental data). However, Mr. Trende does not adequately describe the ways in which researchers have risen to these challenges, nor does he acknowledge that the great majority of the scholarly literature on voters and elections uses observational data, including research that he relies upon in his own report. For example, all of the empirical studies that Mr. Trende cites when discussing the effects of requiring certain forms of voter ID on turnout, in section V of his report, use observational data (one of them also includes an experimental manipulation of a survey question). Many other fields of research also rely heavily on non-experimental data, from astronomy to zoology, but that does not preclude scientific progress in those fields, either. There are even advantages to working with observations of real-world behavior as opposed to focusing on instances of experimental manipulation.⁵

It is also quite common for researchers to discuss the limitations of earlier scholarship, and indeed Mr. Trende quotes several scholars critiquing earlier research on EDR. However, scholars often do this in order establish the relevance of their own research which, they propose, will help to surmount those limitations. For example, Mr. Trende quotes from a paper in which Luke Keele and William Minozzi question the assumptions that underpin some earlier studies on the effects of election laws on turnout, and in which Keele and Minozzi call for "design-based" research using careful comparisons to ensure that researchers only capture the effects of any particular election law rather than also drawing in other factors which may influence turnout.⁶ However, Mr. Trende pays little attention to relevant examples of such

⁵ Using observational data on real-world behavior, in contexts that have not been manipulated by researchers, may make it more credible to generalize from a particular study to the broader phenomenon of interest, although, as with any research, scholars should be explicit about the assumptions required and should ideally provide evidence on the plausibility of those assumptions. See, e.g., Michael G. Findlay, Kyosuke Kikuta and Michael Denly, "External Validity," *Annual Review of Political Science* 24 (2021), 365-393.

⁶ See page 8 of Mr. Trende's report. See also Luke Keele and William Minozzi, "How Much is Minnesota like Wisconsin? Assumptions and Counterfactuals in Causal Inference with Observational Data," *Political Analysis* 21, no. 2 (Spring 2013), 209. Some scholars are specialists in the "methodology" of research, and commonly offer

design-based research, e.g., the paper that Luke Keele himself recently published, using observational data, on the disparate effects of ostensibly neutral voter eligibility requirements.⁷

Indeed, there are several strong recent examples of design-based research on EDR, typically finding that EDR has a positive impact on turnout on the order of a few percentage points. For instance, Neiheisel and Burden find a 3.3 percentage point effect of EDR on turnout using a design that compares the same jurisdictions over time, using "fixed effects" statistical models that control for temporally stable differences across counties while also controlling for broad time trends.⁸ In my own co-authored research I took a different approach—using direct evidence from Google web search data to measure interest in voter registration through Election Day, and using data on the timing of registration from millions of voters in states with different deadlines, including states that allowed EDR—to estimate the post-deadline potential for additional registrations, yielding an estimate that keeping registration open through Election Day would lead to a 3 percentage point increase in turnout nationwide.⁹ The article that I co-authored was published in the same journal as the article by Keele and Minozzi, shortly after theirs, and adhered to their suggestions that scholars should specify their assumptions, should provide evidence on the plausibility of key assumptions and on the extent to which those assumptions would need to be broken in order to produce different results, and should employ a research design allowing for clean comparisons.¹⁰ Notably, other welldesigned research using clean comparisons to study the impact of imposing voter registration requirements for the first time have also produced a symmetrical range of estimates, showing a

critiques of existing approaches and propose innovations; for instance, Keele and Minozzi (210), note that "our main goal is to present a methodological argument."

⁷ Luke Keele, William Cubbison and Ismail White, "Suppressing Black Votes: A Historical Case Study of Voting Restrictions in Louisiana," *American Political Science Review* 115, no. 2 (2021), 694-700.

⁸ Jacob R. Neiheisel and Barry C. Burden, "The Impact of Election Day Registration on Voter Turnout and Election Outcomes," *American Politics Research* 40, no. 4 (July 2012), 636-664. Keele and Miniozzi (207) explain that their estimates differ from those of Neiheisel and Burden because the former opt to exclude city voters from their analysis on the grounds that their behavior may not be readily comparable to other voters; it is not obvious which is the better approach in such cases and it is thus appropriate to acknowledge the implications of such choices. ⁹ Alex Street, Thomas A. Murray, John Blitzer and Rajan S. Patel, "Estimating Voter Registration Deadline Effects with Web Search Data," *Political Analysis* 23, no. 2 (2015), 225-241.

¹⁰ See Keele and Minozzi, 209, drawing in part on suggestions from Guido Imbens, "Better LATE Than Nothing: Some Comments on Deaton (2009) and Heckman and Urzua (2009)," *Journal of Economic Literature* 48, no. 2 (June 2010), 399-423.

negative effect of requiring pre-registration on turnout in the range of 2 to 5 percentage points.¹¹ In my view, it is striking that a number of carefully designed studies, each focusing on somewhat different contexts and using different statistical techniques that rest on varying assumptions, have so consistently led to the finding that EDR has a positive effect on turnout.

To be sure, research on the effects of EDR will continue, and further refinements can be expected. Indeed, this field of research has been fruitful for methodological debates and innovations in part because of the rich data available—data that can extend to thousands of electoral jurisdictions across states that change election laws quite frequently, and, in some cases, data from voter files that provide information on millions of registered voters in one or more states (in addition to some research using sample surveys, which have some advantages but yield far fewer data points). Contrary to Mr. Gessler's misconstrual of the research, and contrary to the dire wording in Mr. Trende's report, the most plausible overview of the scholarly literature is that EDR tends to increase turnout, and, correspondingly, that eliminating EDR is likely to reduce turnout. This question has been intensely studied, resulting, in my professional judgment, in a set of empirical findings that are among the more consistent in the political science literature on voting.

III. A more accurate picture of Montana elections

a) Evidence on the particular importance of EDR in Montana and for on-reservation voters Neither Mr. Gessler nor Mr. Trende refers to much existing scholarship or to direct evidence on the context and conduct of elections in Montana. Regrettably, there are some errors among the few references that they do make. Mr. Trende refers to only a single piece of published research on Montana (page 9 of his report), claiming that it had "cast doubt on whether EDR

¹¹ See Barry C. Burden and Jacob Neiheisel, "Election Administration and the Pure Effect of Voter Registration on Turnout," *Political Research Quarterly* 66, no. 1 (March 2013), 77-90, for the finding that requiring people to register before voting reduced turnout by about two percentage points, using "fixed effects" models to control for enduring differences across jurisdictions and for broad temporal differences. See also Stephen Ansolabehere and David M. Konisky, "The Introduction of Voter Registration and Its Effect on Turnout," *Political Analysis* 14, no. 1 (Winter 2006), 83-100, who find using fixed effects and "difference-in-differences" models to control for differences across jurisdictions and for broad time trends, that, as counties rolled out voter pre-registration requirements at different points in time, turnout declined by 3 to 5 percentage points.

increased turnout in states like New Hampshire, Wyoming and Montana." However, the author of the cited text is clear that he did not actually study the effect of EDR in Montana since EDR had been introduced only shortly before his book was published and there had not yet been a sufficient number of elections conducted under the new system, at the time of writing, for him to assess its effects.¹² Mr. Trende gets this wrong.

Neither Mr. Trende nor Mr. Gessler makes a single reference to direct evidence on the unique conditions of "Native Americans in rural tribal communities across the seven Indian reservations located in Montana," despite the fact that this is the core issue in the case of *Western Native Voice v. Jacobsen* (my quote here is from page 2 of Plaintiffs' complaint). Nor do Mr. Gessler or Mr. Trende cite any of the academic research on Native American voting, even though this has been an active field of research in recent years.

Indeed, in my opinion, both Mr. Trende and Mr. Gessler adopt an unhelpfully abstract stance on the central issues in this case, distant from the practice of voting in Montana, without recourse to relevant data (some of it maintained by the Defendant in this case, who retained them as experts). As I will explain below, this is inappropriate because we do actually know the relative importance of SDR and EDR in Montana elections.

For example, Mr. Trende speculates that, since HB 176 has not removed the option of SDR before 12pm on the day before an election in Montana, "This may serve to minimize the impact of the elimination of election-day registration laws, should such an impact exist" (page 10).¹³

¹² As Hanmer writes, "Montana and Iowa adopted EDR prior to the 2008 presidential election; due to the lack of appropriate data at the time of this writing, EDR in these states is not studied here." See Michael J. Hanmer, *Discount Voting: Voter Registration Reforms and Their Effects*, Cambridge University Press (2009), 14.

¹³ Similarly, on page 7 of his report, Mr. Trende speculates that "Montana retains same-day registration during early voting, which should soften whatever impact there is to the elimination of election-day registration." This is not how most scholars who do research in this area tend to think of the impact of EDR. In any election, there will be some people who need to register or re-register, at various points in time, for reasons that may be beyond their control. There is not some pre-defined set of voters who will either register on Election Day, or beforehand. Instead, the question of who will register and vote remains open right up to the very end of the registration period. This is why politicians, political parties, and various other civic groups work hard to persuade and mobilize people and communities throughout the period, often, up to and including Election Day. Indeed, this is presumably why the metaphor of the election "campaign" evokes a logistical struggle, as in a military conflict.

Mr. Gessler also offers what he refers to as a "crude extrapolation" from one estimate of the positive effect of SDR on turnout to infer that a 29-day SDR period, that does not include Election Day, would yield similar turnout to EDR alone in the state of Montana (page 17 of Mr. Gessler's report). As Mr. Gessler's use of the word "crude" implies, this is certainly not how a social scientist would approach the question. The lone study that Mr. Gessler cites as his source assesses the effects of combining different policies—including not only SDR and EDR but also periods of "early voting" under which previously registered voters can cast ballots, including, as the authors of that paper construe the term, using an absentee ballot or by mail and the results do not support a crude linear extrapolation of the kind that Mr. Gessler employs, since the key finding of the study is that particular combinations of these policies offset each other to varying extents.¹⁴ Mr. Gessler appears to have picked the wrong numbers from this paper, since, as the authors define the terms, Montana has allowed early voting, SDR and EDR (a combination associated, according to the authors, with a statistically significant and positive effect on turnout), but, under HB 176, Montana will switch to early voting plus SDR (not associated with a significant effect on turnout).¹⁵ Even if the comparison were more relevant to the actual issues at stake in Montana, in academic research it does not suffice to offer a single "crude extrapolation," as Mr. Gessler does (page 17). At the very least, an academic study would report the uncertainty associated with any linear projection. Preferably, scholars would report a range of estimates from multiple academic studies. Additionally, a social scientist would seek corroborating evidence, such as data on the timing of voter

¹⁴ Mr. Gessler refers to a report presented at the Pew Charitable Trusts, available at the following link: <u>https://www.pewtrusts.org/~/media/legacy/uploadedfiles/pcs_assets/2009/uwisconsin1pdf.pdf</u> (accessed March 20, 2022). This appears to be an earlier draft of a paper that eventually went through the peer review process and was published as follows: Barry C. Burden, David T. Canon, Kenneth R. Mayer and Donald P. Moynihan, "Election Laws, Mobilization, and Turnout: The Unanticipated Consequences of Election Reform," *American Journal of Political Science* 58, no. 1 (January 2014), 95-109. As the authors in the published version of their paper write (page 108), of the various reforms they studied, "The only consistent way to increase turnout is to permit Election Day registration." My own research on Google searches showed that the greatest interest in voter registration is often on Election Day itself, even in states with an earlier voter registration deadline; see Street, Murray, Blitzer and Patel, "Estimating Voter Registration Deadline Effects."

¹⁵ See Table 1 on page 27 of the Pew report

^{(&}lt;u>https://www.pewtrusts.org/~/media/legacy/uploadedfiles/pcs_assets/2009/uwisconsin1pdf.pdf</u>). Or, preferably, see the results in the peer-reviewed version, Burden et al., "Election Laws, Mobilization, and Turnout," pages 101 and 102.

registration and/or evidence on the timing of interest in voter registration, as, indeed, I did in my published research on EDR.

There is, in fact, no need for the kind of abstract speculation in which Mr. Trende and Mr. Gessler engage since Montana *has* allowed EDR in recent elections, in addition to SDR during the late registration period, so that one can simply calculate the relative importance of SDR and EDR in Montana over recent years. The "late registration" reports for the final 30 days leading up to each election, provided by the Montana Secretary of State for analysis in this case, show that, in federal primary and general elections since 2008, 44.5% of those who registered during the entire late registration period did so on Election Day itself. This means that there were approximately *23 times* as many registrations on Election Day as there were during the average pre-election day under SDR.

In Table 1, I show evidence from Montana, as provided by the Secretary of State's office, on the proportions of ballots cast in several recent federal general elections by SDR (during the late registration period, but not including Election Day) and by EDR (on Election Day itself).¹⁶ Roughly, in general elections since 2014, between 1 and 4 percent of all votes in each election were cast by people using SDR, plus an additional 1 to 3 percent due to EDR alone. In Table 1, I show the numbers separately for voters living on Indian Reservations in Montana and for those living in the remainder of the state. Additionally, I show the percentage of all late registrations on Election Day—these results, in the column furthest to the right, only compare people who used either SDR or EDR and so they do not capture the generally higher reliance on both SDR and EDR among Montanans who live on reservations (as shown in the first two numeric columns from the left). Several patterns emerge in Table 1. First, on-reservation voters are

¹⁶ Specifically, the numbers for SDR and EDR are from the late registrant reports for the respective elections. In order to compare on- and off-reservation voters I combined them with information from statewide voter files, geocoded to show who lives where (as I explained in more detail in my original report for this case). All of the people who appear in these datasets either registered for the first time, or had to update their registration, that is, they relied on either EDR or SDR. As I discussed in my initial report for this case, most people in similar circumstances after the passage of HB 176 will no longer be able to register and vote. No first-time registrants will be allowed on Election Day under HB 176. And HB 176 will also prevent county-to-county movers, those whose registration had been cancelled, and some others from registering and voting on Election Day.

generally more reliant on both SDR and EDR. Second, between 42% and 53% of *all* of ballots cast by late registrants were cast on Election Day itself. This shows the particular importance of EDR, especially for Native American voters living on reservations in Montana. In the general elections, EDR accounted for between 30 and 22 times as many registrations as the average number during the preceding 29 days of the late registration period. I found broadly similar but more variable patterns for primary elections in the same years. Those results are shown in Appendix Table A1. For the primaries, EDR accounted for between 34% and 66% of all late-registration votes in that period, yielding 56 to 15 times more registrations than on the typical day in the preceding 29 days of the late registration period.

	% of all votes cast	% of all votes cast	% of all "late
	by SDR	by EDR	registration" by EDR
2020 general election			
On-reservation voters	1.35%	1.48%	52.9%
Off-reservation voters	1.29%	1.33%	51.1%
2018 general election			
On-reservation voters	3.52%	2.64%	42.8%
Off-reservation voters	2.08%	1.56%	43%
2016 general election			
On-reservation voters	4.23%	3.04%	41.8%
Off-reservation voters	2.92%	2.33%	44.4%
2014 general election			
On-reservation voters	1.56%	1.55%	49.8%
Off-reservation voters	1.4%	1.27%	47.6%

Table 1. Reliance on EDR and SDR in Montana elections among on- and off-reservation voters

Source: late registrant reports, MT Secretary of State's office.

This evidence on the special importance of EDR is, of course, in line with the claims of the Plaintiffs in this case, and also aligns with much of the research I have cited, since Election Day

is when campaigns culminate, media coverage peaks, and attention is greatest.¹⁷ The prominence of Election Day may also boost the role of EDR due to peer effects, whereby the fact that so many people are voting and/or registering on that day inspires others to do so.¹⁸

b) Evidence on voting wait times in Montana

Mr. Gessler asserts, without citing any evidence, that "EDR can dramatically increase wait times for in-person voters" (page 14 of his report). Mr. Gessler also writes that "Long wait times often leave voters frustrated and disdainful of election administration competence, and it is not unusual for voters to leave a polling place (sometimes due to necessity) because of long wait times" (page 14). Mr. Gessler speculates, without citing any evidence on elections in Montana, that ceasing to allow EDR in this state "provides substantial benefits" in part because it might reduce voter wait times on Election Day (pages 12, 13). It is not fully clear to me which benefits Mr. Gessler has in mind, but he implies that, if indeed removing EDR in Montana served to reduce wait times, this would induce additional turnout, since it would prevent others who want to vote but do not need to use EDR from leaving "because of long wait times" (page 14). The logic of the claim is questionable, since the presence of more than one person in line indicates that a line, in itself, is not enough to deter all of the other people in the line. It is also possible that putting an end to EDR will shift some of the need to wait in line to other days in the "late registration" period.

¹⁷ Alex Street, Thomas A. Murray, John Blitzer and Rajan S. Patel, "Estimating Voter Registration Deadline Effects." See also Erika Franklin Fowler, Michael M. Franz, Gregory J. Martin, Zachary Peskowitz and Travis N. Ridout, "Political Advertising Online and Offline," *American Political Science Review* 115, no. 1 (February 2021), 130-149. ¹⁸ On peer influence on Election Day, Vonnahme argues that later registration deadlines, including Election Day itself, give people more opportunities to stimulate others into registering and/or voting. See Greg Vonnahme, "Registration Deadlines and Turnout in Context," *Political Behavior* 34, no. 4 (December 2012), 765-779. Bond et al. report results of randomly encouraging some people to vote, via an "I voted" image on Facebook, on the day of the midterm general elections of 2010, and found a positive effect on those individuals in addition to positive effects on other people in their close networks. Under previous law, such peer effects could have included people who would have been able to use EDR, but, after the passage of HB 530, they would only work for people who were already registered. See Robert M. Bond, Christopher J. Fariss, Jason J. Jones, Adam D. I. Kramer, Carmeron Marlow, Jaime E. Settle and James H. Fowler, "A 61-million-person experiment in social influence and political mobilization," *Nature* 489 (September 2012), 295-298.

Furthermore, extremely unusual circumstances would need to hold in order for HB 176 to cause additional turnout, let alone for it to cause enough additional turnout to fully offset or exceed the negative effect on turnout of removing EDR, an option that tens of thousands of Montanans have relied upon to vote in recent years. Putting an end to EDR means that nobody who needs to register for the first time, or significantly update their registration, will be able to vote, so there is no way that any effect of ending EDR on lines would help those people. Most in-person voting in Montana is at precinct polling places, not at the county election offices where EDR is generally handled, so there is likewise no way for most in-person voters to be deterred by lines due to the need to process EDR. In my experience as a voter and from conducting research on Election Day in Montana, and from having spoken with local election officials, there is no need to wait in line to drop off an absentee ballot at an elections office—so absentee voters, even if they opt to drop off their ballot rather than return it through the mail, are also unlikely to be deterred by lines due to EDR. There are a few other circumstances under which voters might need to go to their elections office, including on Election Day, e.g., if they had not received or had misplaced an absentee ballot. Such people might have to wait in line on Election Day. Having discussed the matter with elections officers in my county over recent years, and having visited my county elections office on Election Day several times, I understand that, at least in my county, most people in line at the elections office on Election Day were there to use EDR, although the situation was more mixed in 2020, when both the federal primary and general elections were held almost entirely by mail (with the result that some people who, in previous years, had voted in-person, received a ballot in the mail; such people may have been less accustomed to voting by mail and more likely to request a replacement). I am aware of no evidence that substantial numbers of Montanans have been deterred from voting by lines at elections offices. Mr. Gessler does not cite any such evidence. If the Defendant's expert, or the Defendant, had information showing that many Montanans were deterred from voting by lines at elections offices on Election Day then it would also seem more appropriate to provide that

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information to the people working at those offices, so that they could plan to hire more staff, rather than to defend putting an end to EDR on this basis.¹⁹

Rather than rely merely on speculation, I believe the court would be better served by evidence on the question of wait times for voters in Montana. To that end, I have analyzed a public opinion survey on voter experiences. The Survey of the Performance of American Elections (SPAE) has been conducted since 2008 by leading elections scholar Charles Stewart III of the Massachusetts Institute of Technology.²⁰ It includes samples of 200 registered voters from each state, including Montana. This is a larger Montana sample than is typical in national public opinion research, since Montanans make up a small share of the total US population. The SPAE survey is fielded in the days following a general election and includes the question, for people who had voted in-person, "Approximately, how long did you have to wait in line to vote?" The response options are as follows: "not at all," "less than 10 minutes," "10-30 minutes," "31minutes to 1 hour," and "more than 1 hour." To be sure, this is only a sample survey, and it does not allow comparisons of Native American and other voters due to the limited sample size. The SPAE does not include information that would allow me to identify the people who waited at an elections office rather than at precinct polling places, although, as I have explained, relatively few people cast their ballots in-person at the elections office. Nonetheless, the SPAE datasets do include relevant evidence on the 2020, 2016, and 2012 general elections.²¹ To my knowledge, this is the best available source of data on the question of voter wait times in Montana.

¹⁹ Elections offices in Montana do hire additional workers on Election Day (including, in recent years in my county, several of my students). I find Mr. Gessler's claim that elections workers sometimes find it hard to predict demand for EDR relevant, but I am skeptical of his claim that putting an end to EDR will improve that situation since, at least in the first few years, this will be a break with their experience, which will actually make predictions of the need for extra workers on Election Day harder.

²⁰ For further details, see <u>https://electionlab.mit.edu/research/projects/survey-performance-american-elections</u> (accessed March 18, 2022).

²¹ SPAE data are also available for the 2014 midterm elections but, since they are not available for 2018, I opt to focus only on the recent presidential-year elections, for the sake of comparability over time. For these results, I use state weights provided with the survey data (state weights are included to better match the demographics of the voting population in each state, as estimated from census data).

	% of <i>in-person</i> voters who	% of <i>all</i> voters who waited	
	waited > 10 minutes to vote	> 10 minutes to vote	
2020 general election			
Montana	10%	1%	
Nationwide	40%	21%	
2016 general election			
Montana	17%	6%	
Nationwide	25%	20%	
2012 general election			
Montana	32%	16%	
Nationwide	29%	24%	

Table 2. Reported voter wait times for recent general elections

Source: Survey of the Performance of American Elections, 2012, 2016, 2020.

The results in Table 2 show that relatively few Montana voters have spent even 10 minutes waiting in line to vote.²² The first column of numbers, at the center of the table, shows the percentage of *in-person* voters in Montana who report having waited 10 minutes or more. The column to the right shows the share of *all* voters who waited that long. Table 2 shows that, in recent years, Montanans were generally less likely to spend even 10 minutes waiting to vote than other voters nationwide. Table 2 also shows that wait times have tended to decline over recent years. This may be due to the fact that increasing numbers of Montanans now vote using absentee ballots, reducing the number of in-person voters and potentially making in-person voting easier to arrange; this trend was already well-established before 2020. For this reason, I find the column of results on the right side of Table 2 most instructive, since, by showing evidence on wait times for *all* voters, it reflects the importance of absentee voting in Montana. Overall, the results in Table 2 cut against Mr. Gessler's unsupported assertion that, "even with a large percentage of voters voting by mail, EDR can dramatically increase wait

²² I focus on wait times above 10 minutes on the grounds that I see a shorter wait time as a minimal concern. I also find generally consistent patterns using different thresholds, e.g., focusing on the considerably smaller set of people who had to wait 30 minutes or longer before voting.

times for in-person voters" (Gessler report, page 14). Perhaps Mr. Gessler has good reason for believing that to be true elsewhere; it does not appear to be true in Montana.

IV. A more cautious approach to the tricky question of public opinion about election integrity

Both Mr. Gessler and Mr. Trende discuss voter confidence in elections in relation to the laws being challenged in this (consolidated) case. Mr. Gessler argues that, by banning organizations such as Western Native Voice from paying people who help to collect ballots, HB 530 would "help foster confidence in elections" (page 28 of Mr. Gessler's report). Yet, this claim is not based on any relevant evidence, nor is it consistent with academic research on this topic.

Mr. Gessler writes that HB 530 would "foster confidence" because he believes, and he asserts many others believe, that paying people to help return ballots, as Western Native Voice does, provides an incentive to commit fraud (page 28).²³ Mr. Gessler provides no evidence to support these claims. Beyond the lack of evidence, another reason to question the logic of Mr. Gessler's claim about the impact of paid work for people who help to return ballots is that the kind of fraud Mr. Gessler envisages would already be illegal under Montana law. Mr. Gessler speculates that paying people who help voters to apply for and return ballots, as Western Native Voice does, "creates a temptation to cut corners or perhaps blatantly violate the law" (Gessler report, page 26). He continues, "For example, a collector may pressure a voter to hand over or immediately fill out a ballot before the voter is ready to choose which candidates to support. And a ballot collector has a financial motive to take a discarded ballot that a voter does not intend to vote, fill out that ballot, and then turn it in" (page 26). The actions that Mr. Gessler describes appear already to be illegal under Montana law, regardless of HB 530. Voters

²³ Without citing any source for the information, Mr. Gessler makes the extraordinarily strong claim that "many believe that permitting *any* ballot "harvesting" or "collecting" inevitably leads to voter manipulation and voter fraud" (page 28, emphasis in original). I find the claim that many people have such a specific and strong connection in mind (*inevitably*) implausible. Decades of research on public opinion show that most people think about politics quite rarely and typically with some level of uncertainty about the details of policy or about the details of the political system. See, for example, Philip E. Converse, "The Nature of Belief Systems in Mass Publics," *Critical Review* 18, nos. 1-3 (2006), 1-74, originally published in David E. Apter, ed., *Ideology and Its Discontents* (New York: The Free Press of Glencoe, 1964). Also, John R. Zaller, *The Nature and Origins of Mass Opinion* (Cambridge University Press, 1992).

are not allowed to show anyone their marked ballot, nor is anyone allowed to solicit the voter to show their ballot.²⁴ It is already illegal to change a ballot after it has been marked by an elector.²⁵ It is illegal to use "duress" or "any fraudulent contrivance" to compel a voter to vote, or refrain from voting, in an election.²⁶ It is illegal to provide electors with incorrect or misleading information about election procedures.²⁷ It is illegal to mutilate or destroy someone's ballot,²⁸ apply for a ballot under someone else's name,²⁹ register under someone else's name,³⁰ vote someone else's ballot,³¹ or to vote more than once in an election.³² Some of these illegal actions could result in a misdemeanor charge while others are felonies; for example, some count as tampering with a public record, punishable by up to 10 years in state prison and/or a fine up to \$50,000.³³ The consequences of breaking these laws are serious. This casts doubt on Mr. Gessler's claim that paying people to help return ballots would be sufficient to substantially affect the incentives for or against attempting to commit electoral fraud. As far as I know, Western Native Voice does not pay at unusually high rates; for example, I have seen a hiring announcement (from 2019) quoting an hourly rate of \$12-\$15 for community organizers (who also do other work besides helping to collect ballots). To my knowledge, Western Native Voice does not pay based on the number of ballots collected. The suggestion that paying people at modest hourly rates for their work as community organizers creates a sufficient financial incentive to commit election fraud, in ways that are already illegal, is not, in my opinion, credible.

For his part, Mr. Trende also refers to "voter confidence" while discussing research on the effects of requiring certain forms of voter identification, although, regrettably, his account of that research contains further errors. Mr. Trende writes, "Finally, although I am not convinced

²⁴ Mont. Code Ann. § 13-35-201(1), (3).

²⁵ Mont. Code Ann. § 13-35-205(4).

²⁶ Mont. Code Ann. § 13-35-218(2).

²⁷ Mont. Code Ann. § 13-35-235.

²⁸ Mont. Code Ann. § 13-35-206.

²⁹ Mont. Code Ann. § 13-35-210(2).

³⁰ Mont. Code Ann. § 13-35-207(1).

³¹ Mont. Code Ann. § 13-35-207(6).

³² Mont. Code Ann. § 13-35-210(1).

³³ Supra nn.24-32; Mont. Code Ann. § 45-7-208.

that voter fraud is a substantial problem in Montana, there is some evidence the photographic identification laws bolster confidence in elections. After all, even studies that are critical of photographic identification laws find these laws to be popular and effective in reducing fraud. (Atkeson et al. 2014; Stewart et al., 2016)" (this is on page 12 of Mr. Trende's report). Regrettably, Mr. Trende neglects to provide full citations, but he appears to be referring to the following articles. In 2014, Lonna Rae Atkeson et al. published a paper on data from a 2008 public opinion survey from New Mexico showing that many voters think requiring certain forms of voter identification can help to prevent fraud, but also that variation in responses to the question was driven by cues from party leaders, making self-identified Democrats more worried about infringing access to the vote whereas self-identified Republicans were more worried about fraud.³⁴ That article certainly did not include any claim that such laws are "effective in reducing fraud," as Mr. Trende wrongly claims (page 12 of his report). In 2016, Charles Stewart, Stephen Ansolabehere, and Nathaniel Persily published an article showing that public perceptions of fraud and confidence in the integrity of the electoral system are not connected to actual state variation in voter identification requirements, of which voters are often poorlyinformed, and also showing that survey responses on this issue are influenced, instead, by cues from party leaders.³⁵ They find that perceptions of the electoral system are linked to wider attitudes about government, and, as such, are linked to ideological and partisan differences. As Stewart, Ansolabehere, and Persily explain, these findings are consistent with a much larger body of research on public opinion which finds that political partisanship is an important social identity and that voters often form beliefs on particular policy issues based on party identity and party cues rather than based on specific knowledge of the policy area.³⁶ Once again, their

³⁴ Lonna Rae Atkeson, R. Michael Alvarez, Thad E. Hall and J. Andrew Sinclair, "Balancing Fraud Prevention and Electoral Participation: Attitudes Toward Voter Identification," *Social Science Quarterly* 95, no. 4 (December 2014), 1381-1398.

³⁵ Charles Stewart III, Stephen Ansolabehere and Nathaniel Persily, "Revisiting Public Opinion on Voter Identification and Voter Fraud in an Era of Increasing Partisan Polarization," *Stanford Law Review* 68 (June 2016), 1455-1489.

³⁶ Citing seminal work by John Zaller, they write that "Voters take their cues from party leaders when they judge how well policies are working," ibid. 1459. See Zaller, *The Nature and Origins*, or Gabriel S. Lenz, *Follow the Leader? How Voters Respond to Politicians' Policies and Performance* (University of Chicago Press, 2012). On public perceptions of voter ID laws, in particular, see also Paul Gronke, William D. Hicks, Seth C. McKee, Charles Stewart III and James Dunham, "Voter ID Laws: A View from the Public," Social Science Quarterly 100, no. 1 (February 2019), 215-232. These authors conclude that their results "are consistent with an elite-to-mass message

article does *not* include any claim that such laws are actually "effective in reducing fraud," as Mr. Trende wrongly claims.

Mr. Trende also cites another study that involved mailing fliers informing some registered voters of ID requirements in Virginia, and then surveying some of those people, after a gubernatorial election, to see whether they had become more knowledgeable about the requirements and whether there was any impact of having informed them about ID requirements on their perceptions of the prevalence of fraudulent votes.³⁷ Mr. Trende says the authors "find that information about the existence of these laws do [sic] reduce the perception of fraud" (page 12 of Mr. Trende's report). The actual results of that study are more nuanced, however. The study used a sample of 28,000 people who gave landline telephone numbers, resulting in a sample that is older than the full voting population and who were more likely to have voted in the past.³⁸ Of the 1090 people who responded to the survey, which used "interactive voice response technology," i.e., a robotic voice, only 431 gave age and gender responses that were consistent with the record in the voter file from which the sample was drawn. The authors focus their analysis on those 431 people (1.5% of the original sample) and find a modest and non-significant effect of the fliers on perceptions of fraud (p < 0.17).³⁹ Only in a statistical model that also includes other variables do they find a significant effect (p < 10.05).⁴⁰ The practice of "adjusting" an experimental estimate for other factors using a statistical model ("adjusting for covariates") is controversial in scientific circles in part because it creates the risk that the results are contingent upon the particular choices by researchers of which other factors to include in the model (and how to include them).⁴¹ The evidence from this study is not so clear-cut as Mr. Trende implies.

transmission reflecting the current context of polarized party politics and the variation in the voter coalitions comprising the Democratic and Republican parties" (page 215).

³⁷ Kyle Endres and Costas Panagopoulos, "Photo identification laws and perceptions of electoral fraud," *Research and Politics* 8, no. 3 (July-September 2021), 1-7.

³⁸ Endres and Panagopoulos, "Photo identification laws," 2.

³⁹ Endres and Panagopoulos, "Photo identification laws," 3.

⁴⁰ Endres and Panagopoulos, "Photo identification laws," 4.

⁴¹ Commendably, the authors present both the unadjusted and the adjusted results. Scholars recommend that any adjustment should include only "pre-treatment" variables, as opposed to "post-treatment" variables that might themselves have been affected by the experiment. The authors of the study in question do appear to have used

In recent years, partisan cues on the prevalence of voter fraud and the integrity of the electoral system have become even more distinctive, driven, in particular, by repeated claims of fraud from former President Trump.⁴² Scholarship on "expressive responding" suggests that people may treat political questions in public opinion surveys as opportunities to cheerlead for the stance of their preferred party, rather than as an opportunity to reveal their considered beliefs.⁴³ In this context, self-identified Republicans may use a survey as a chance to express support for former President Trump, whereas, in order to signal their views of Mr. Trump, selfidentified Democrats may actually become less likely to express concern over voter fraud than in the past. This is consistent with findings that survey responses of greater or lesser confidence in the election system are not associated with substantial differences in the likelihood of actual turnout.⁴⁴ Overall, one should expect, in this context, that partisanship now shapes reports of voter confidence and perceptions of election integrity even more strongly than in recent years, and indeed, this is what early studies of the 2020 general election reveal. For example, Persily and Stewart find that in recent survey data, "After November 3 [2020], the overall measures of confidence remained roughly unchanged, but the degree of partisan polarization exploded," jumping from a 10.9-perecentage-point gap between Democrats and Republicans to a 51.7 point gap.⁴⁵ This is a strong example of the "winner's effect," whereby people who supported the presidential candidate who has just won become more likely to

pre-treatment variables although their inclusion of party affiliation (at the time of an earlier election) raises the prospect of some correlated long-term factor driving both party affiliation and perceptions of the prevalence of election fraud. Even when scholars only adjust for pre-treatment variables the choice of which variables to include, and how to specify the model, can still affect the results. On these matters see, for example, David A. Freedman, "On Regression Adjustment in Experiments with Several Treatments," *The Annals of Applied Statistics* 2, no. 1 (March 2008), 176-196. See also Diana C. Mutz, Robin Pemantle and Philip Pham, "The Perils of Balance Testing in Experimental Design: Messy Analyses of Clean Data," *The American Statistician* 73, no. 1 (2019), 32-42. ⁴² See, e.g., <u>https://www.nytimes.com/2020/12/26/us/politics/republicans-voter-fraud.html</u>. For a statistical debunking of some of the claims of fraud, see Andrew C. Eggers, Haritz Garro and Justin Grimmer, "No evidence for systematic voter fraud: A guide to statistical claims about the 2020 election," *Proceedings of the National Academy of Sciences* 118, no. 45 (November 2021).

 ⁴³ See, e.g., Brian F. Schaffner and Samantha Luks, "Misinformation or Expressive Responding? What An Inauguration Crowd Can Tell Us About The Source of Political Misinformation In Surveys," *Public Opinion Quarterly* 82, no. 1 (Spring 2018), 135-147.

⁴⁴ Stewart, Ansolabehere and Persily, "Revisiting Public Opinion," 1473-1476.

⁴⁵ Nathaniel Persily and Charles Stewart III, "The Miracle and Tragedy of the 2020 U.S. Election," *Journal of Democracy* 32, no. 2 (April 2021), 171. See also Bruce E. Cain, "The Elections of 2020," in Gillian Peele, Bruce E. Cain, Jon Herbert and Andrew Wroe, eds., *Developments in American Politics 9* (Palgrave MacMillan, 2022).

express confidence in the process, whereas those whose preferred candidate has just lost become less likely to do so.⁴⁶

Again, rather than rely on speculation, I hope that the court will find it helpful to see some analysis of Montana data on this matter. To that end, I again analyzed data from the Survey of the Performance of American Elections (SPAE), which was fielded immediately after general elections in 2020, 2016 and 2012, including a sample of 200 registered voters from Montana in each year.⁴⁷ The results from Montana comport with nationwide trends towards party-based polarization in opinions on the theme of confidence in elections. In Figure 1, I show the percentage of Montana survey respondents who said they were "very confident" that their vote in the General Election had been counted as they had intended, separately for those who identified as Democrats and Republicans.⁴⁸ The Figure shows that, when surveyed in mid-November 2020, 97% of self-identified Democrats said that they were "very" confident their vote had been counted as intended, compared to just 54% of self-identified Republicans—this in the wake of Joe Biden's victory in the presidential election.⁴⁹ This was a reversal from 2016, when self-identified Republicans had been more confident (80% very confident) than Democrats (73% very confident) after Donald Trump won, which itself was a reversal of the survey results as of November 2012 when 86% of Democrats said they were very confident

⁴⁶ Stewart, Ansolabehere and Persily, "Revisiting Public Opinion," 1480. And see Morris Levy, "Winning cures everything? Beliefs about voter fraud, voter confidence, and the 2016 election," *Electoral Studies* 74 (December 2021).

⁴⁷ For these results, I again use the publicly available SPAE data provided by MIT and I apply state weights to better match state demographics. For further details on the survey, see

https://electionlab.mit.edu/research/projects/survey-performance-american-elections (accessed March 14, 2022). ⁴⁸ The full question wording is as follows. "How confident are you that **your vote** in the General Election was counted as you intended?" (emphasis in original). The response options are: "Very confident," "Somewhat confident," "Not too confident," "Not at all confident," and "I don't know." I used the 7-point party identification scale in these surveys and I included people who said they were Independent but "lean" towards one party or the other along with others who identify with that party, since previous research shows that the "leaners" tend to be similar to the outright party identifiers, e.g., they tend to vote the same way; see Bruce E. Keith, David B. Magleby, Candice J. Nelson, Elizabeth Orr, Mark C. Westlye and Raymond E. Wolfinger, *The Myth of the Independent Voter* (University of California Press, 1992).

⁴⁹ I also confirmed that the difference is most clearly associated with party-identity rather than other potentially related factors, using multiple regression analysis that controlled for variation by age, education, gender and household income.

compared to 67% of Republicans, shortly after Barack Obama had won re-election.⁵⁰ Notably, for all the partisan back-and-forth, the overall level of confidence changed little over this time, from 74% very confident in 2012, to 76% in 2016, to 72% in 2020.





The patterns in Figure 1 show clear signs of the "winner's effect," and also show that Democrats are typically more likely to express confidence in the election system than Republicans (the peak responses for Republicans are not as high as the peaks for Democrats, and the lows are lower). These findings align with research by scholars who are skeptical of claims that changes in election laws, such as new voter identification requirements, can be justified on the basis that the changes are likely to improve voter confidence, since this is an

⁵⁰ Many other federal and state positions were elected at the same time, of course, but the presidential election tends to draw the most attention, and this has been the focus of scholars who see a "winner's effect."
issue in which opinions are dominated by partisanship.⁵¹ Scholars have found little evidence that voter confidence actually changes after states pass new election laws that were justified as efforts to reduce the potential for fraud. For instance, Stewart, Ansolabahere and Persily, report that, comparing SPAE survey responses over time in two states that introduced photographic voter ID requirements, "identifiers of both parties were virtually unchanged in believing that voter impersonation was frequent."⁵² Another recent article using the SPAE data from multiple years, studying how perceptions of fraud changed over time as several states passed photographic voter ID laws, likewise finds no effect.⁵³ And a paper using a separate survey, with a different sampling method and question wording, also finds that voter identification laws "are not associated with greater confidence in elections."⁵⁴

Another reason for caution in response to Mr. Gessler's claim that measures such as HB 530 will "foster confidence" (page 28 of Mr. Gessler's report) is that voters tend not to distinguish in their opinions about the prevalence of various kinds of electoral fraud. This suggests that the responses are driven by a single underlying attitude, perhaps with a partisan basis, rather than by specific concerns that might respond to a particular policy (such as banning organizations like Western Native Voice from paying people who help voters to return ballots). I find clear evidence that a single dimension underpins expressed perceptions of fraud in the Montana survey data, regardless of the details. The SPAE includes questions about the perceived prevalence of several forms of voter fraud and also about fraud on the part of the public officials in charge of counting the votes.⁵⁵ And yet, although these questions cover a wide

⁵¹ See Stewart, Ansolabahere and Persily, "Revisiting Public Opinion," 1484.

⁵² Stewart, Ansolabahere and Persily, "Revisiting Public Opinion," 1473, footnote 51.

⁵³ See pages 2651-2653 in Enrico Cantoni and Vincent Pons, "Strict ID Laws Don't Stop Voters: Evidence from a U.S. Nationwide Panel, 2008-2018," *Quarterly Journal of Economics* 136, no. 4, 2615-2660.

⁵⁴ See Shaun Bowler, Thomas Brunell, Todd Donovan and Paul Gronke, "Election administration and perceptions of fair elections," *Electoral Studies* 38, (June 2015), 1, 6.

⁵⁵ The question wording is as follows: "The following is a list of activities that are against the law. Please indicate how often you think these activities occur **in your county or city**" (emphasis in original). The following items are then presented in random order. "People voting more than once in an election," "People stealing or tampering with ballots that have been voted," "People pretending to be someone else when going to vote," "People voting who are not U.S. citizens," "People voting an absentee ballot intended for another person," "Officials changing the reported vote count in a way that is not a true reflection of the ballots that were actually counted." The response options in each case are: "It is very common," "It occurs occasionally," "It occurs infrequently," "It almost never

range of activities by different actors, survey responses are remarkably similar. The average correlation across responses to these items is extremely high, at r = 0.87 for the 2020 data. To give a few examples from the 2020 survey, of those who say that it is "common" for officials in their county or city to change the reported vote count, 72% also say that it is common for people in the county/city to vote more than once, 88% say it is common for people in the county/city to steal or tamper with ballots, 67% say it is common for people in the county/city to pretend to be someone else when voting, 82% say it is common for non-citizens in the county/city to vote, and 88% say it is common for people in the county/city to vote an absentee ballot intended for another person. I also find that, for their part, Democrats actually became less likely to express concerns over fraud involving absentee ballots in 2020, compared to 2016 and 2012, perhaps as a way of signaling their disagreement with then-President Trump and his claim that, if he lost in 2020, it would be due to absentee ballot fraud.⁵⁶ Over the same time, Montanans became much less likely to say they didn't actually know whether absentee ballot fraud was happening in their county or city, from 35% "don't know" in 2012, to 19% in 2015%, to 15% in 2020. This pattern is more likely due to (partisan) cue-taking on this issue than to variation based on actual cases of fraud, which, to my knowledge, are exceedingly rare in the state of Montana.

In short, the logic that Mr. Gessler uses to link HB 530 to improved voter confidence in the electoral process is tenuous, Mr. Trende mischaracterizes scholarly research on this topic, and neither of their reports offers evidence to support that link, whereas my analysis of public opinion data relating to Montana elections suggests that such an effect is unlikely since opinions on this topic are dominated by partisan reasoning. The available evidence from Montana, in line with theories of public opinion and wider trends, indicates that in the current political climate it is not such policies as HB 530 that affect reported voter confidence, but rather partisanship, elite cues, and the winner's effect.

occurs," or "I'm not sure." To calculate the correlations across items I discarded the "not sure" responses and I coded common as 1, occasionally as 0.67, infrequently as 0.33, and almost never as 0.

⁵⁶ Only 3% of self-identified Democrats in Montana said in 2020 that people voting an absentee ballot intended for another person was either common, or happened occasionally—compared to 10% of Democrats in 2016 and 12% in 2012. Each of the differences (2020 vs. 2016, and 2020 vs. 2012) is statistically significant at p < 0.02.

V. Conclusion

Nothing in the Defendant's reports submitted in the case of Western Native Voice v. Jacobsen has caused me to revise my professional opinion, as evidenced and explained in my initial report in this case, that HB 176 and HB 530 will disproportionately reduce rates of registration and voting among Native Americans living on the largely rural Indian reservations in the state of Montana. This is partly because Mr. Gessler and Mr. Trende refer to little of the relevant scholarship that might help illuminate the likely effects of HB 176 and HB 530, and because they each mischaracterize some of the scholarship that they do mention. It is also because Mr. Gessler and Mr. Trende cite very little evidence that is directly relevant to the conduct of Montana elections, relying instead on assertion and speculation. In attempting to provide the court with the opportunity to review actual evidence that speaks to the credibility of their assertions, I have further confirmed the importance of Election Day registration in Montana, particularly for voters living on Indian reservations, I have found no evidence that EDR has resulted in undue wait times for Montana voters, and I have shown that public attitudes around issues of election integrity and voter confidence are heavily shaped by partisan reasoning and appear unlikely to shift in response to the passage and enforcement of a law such as HB 530. My review of the relevant scholarship and of all the evidence I have presented in this case, using high-quality data from multiple sources and tools of statistical analysis as employed by experts in my profession, consistently corroborates the concerns of the Plaintiffs.

I declare under penalty of perjury that the foregoing is true and correct to the best of my knowledge.

Signed this 25th day of March, 2022.

Alexander Street.

Appendix

Table A1. Reliance on EDR and SDR in Montana elections among on- and off-reservation

	% of all votes cast	% of all votes cast	% of all "late
	by SDR	by EDR	registration" by EDR
2020 primary election			
On-reservation voters	0.28%	0.59%	65.6%
Off-reservation voters	0.22%	0.4%	68.8%
2018 primary election			
On-reservation voters	1.57%	0.79%	34.1%
Off-reservation voters	0.61%	0.32%	33.5%
2016 primary election			
On-reservation voters	2.02%	1.52%	42.9%
Off-reservation voters	0.95%	1.13%	54.4%
2014 primary election			
On-reservation voters	1.03%	0.86%	45.4%
Off-reservation voters	0.7%	0.42%	37.9%

voters, primary elections

Source: late registrant reports, MT Secretary of State's office.

Erratum

In preparing Table A1 for this report, I noticed I had made a mistake in my original report. Specifically, for Figure 2 of that report (page 17), the share of all votes cast in the 2020 primary by people living on-reservation should be 0.59, not 0.49. In other words, in the original report, I slightly under-reported the reliance of on-reservation voters on EDR in the 2020 federal primary election. My original calculation was correct, but I made a mistake in data entry for the Figure. The *p*-value as reported in the original version of Figure 2 was also correct. I apologize for my mistake. Here is the corrected version of the chart.

Figure 2. Montanans who live on reservations are more reliant upon EDR, in primary elections – CORRECTED after error in the version in original report



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Exhibit 3

Nature of the Sample: NPR/PBS NewsHour/Marist Poll of 1,209 National Adults

This survey of 1,209 adults was conducted October 18th through October 22nd, 2021 by The Marist Poll sponsored in partnership with NPR and PBS NewsHour. Adults 18 years of age and older residing in the United States were contacted on landline or mobile numbers and interviewed by telephone using live interviewers. Survey questions were available in English or Spanish. Mobile telephone numbers were randomly selected based upon a list of telephone exchanges from throughout the nation from Dynata. The exchanges were selected to ensure that each region was represented in proportion to its population. Mobile phones are treated as individual devices. After validation of age, personal ownership, and nonbusiness-use of the mobile phone, interviews are typically conducted with the person answering the phone. To increase coverage, this mobile sample was supplemented by respondents reached through random dialing of landline phone numbers. Within each landline household, a single respondent is selected through a random selection process to increase the representativeness of traditionally undercovered survey populations. The samples were then combined and balanced to reflect the 2019 American Community Survey 1-year estimates for age, gender, income, race, and region. Assistance was provided by Luce Research for data collection. Results are statistically significant within ±4.0 percentage points. There are 1,032 registered voters. The results for this subset are statistically significant within ±4.3 percentage points. There are 469 Democrats and Democratic leaning independents and 413 Republicans and Republican leaning independents. The results for these subsets are statistically significant within ±6.4 percentage points and ±6.8 percentage points, respectively. Tables include results for subgroups to only display crosstabs with an acceptable sampling error. It should be noted that although you may not see results listed for a certain group, it does not mean interviews were not completed with those individuals. It simply means the sample size is too small to report. The error margin was adjusted for sample weights and increases for cross-tabulations.

		National Adults	National Registered Voters
		Column %	Column %
National Adults		100%	
National Registered Voters		85%	100%
Party Identification	Democrat	n/a	31%
	Republican	n/a	27%
	Independent	n/a	40%
	Other	n/a	2%
Gender	Men	49%	48%
	Women	51%	52%
Age	Under 45	43%	39%
	45 or older	57%	61%
Age	18 to 29	18%	16%
	30 to 44	25%	23%
	45 to 59	26%	27%
	60 or older	31%	34%
Generation	Gen Z/Millennials (18-40)	35%	31%
	Gen X (41-56)	27%	28%
	Baby Boomers (57-75)	26%	29%
	Silent-Greatest (Over 75)	12%	13%
Race/Ethnicity	White	60%	62%
	Black	11%	12%
	Latino	16%	14%
	Other	12%	12%
Pegion	Northeast	12%	16%
Region	Midwost	21%	22%
	South	2170	2270
	West	3070	249/
Household Income	Loss than \$50,000	24 %	2470
Household Income	£50,000	39%	30%
Education	\$50,000 OF more	61%	62%
Education		50%	52%
	College graduate	44%	48%
Education by Race	White - Not College Graduate	33%	33%
	White - College Graduate	28%	30%
	Non-White - Not College Graduate	23%	20%
	Non-White - College Graduate	16%	18%
Education - Race - Gender	Men - White - Not College Graduate	16%	15%
	Men - White - College Graduate	14%	15%
	Men - Non-White - Not College		
	Graduate	12%	11%
	Men - Non-White - College Graduate	7%	7%
	Warran White Net Callers Ore ducts	170/	1701
	women - white - Not College Graduate	17%	17%
	Women - White - College Graduate	14%	15%
	Women - Non-White - Not College		
	Graduate	11%	9%
	Women - Non-White - College Graduate	0%	10%
White Evangelical Christians		5 /0 1 Q0/_	1070
Area Description	Big site	10%	1970
Alea Description	Dig City Small site	21%	2070
	Sinan City	17%	17%
	Suburban Small tours	24%	25%
	Sman town	18%	1/%
A	Kural	14%	15%
Area Description - Gender	Small City/Suburban Men	21%	20%
	Other area Men	28%	29%
	Small city/Suburban Women	20%	23%
	Other area Women	31%	29%
Interview Type	Landline	39%	42%
	Cell phone	61%	58%

NPR/PBS NewsHour/Marist Poll National Adults. Interviews conducted October 18th through October 22nd, 2021, n=1,209 MOE +/- 4.0 percentage points. National Registered Voters: n=1,032 MOE +/- 4.3 percentage points. Totals may not add to 100% due to rounding.

BIDJP105. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

			National Adults	
		Do you approve	or disapprove of the doing as president	e job Joe Biden is ?
		Approve	Disapprove	Vol: Unsure
		Row %	Row %	Row %
National Adults		44%	49%	8%
National Registered Voters		44%	50%	6%
Party Identification	Democrat	85%	10%	5%
	Republican	6%	90%	4%
	Independent	39%	52%	9%
Region	Northeast	46%	49%	5%
	Midwest	43%	45%	12%
	South	42%	51%	7%
	West	45%	47%	7%
Household Income	Less than \$50,000	49%	44%	7%
	\$50,000 or more	43%	50%	7%
Education	Not college graduate	39%	51%	10%
	College graduate	50%	46%	4%
Race/Ethnicity	White	38%	56%	6%
,	Non-white	55%	37%	8%
Race/Ethnicity	White	38%	56%	6%
, , , , , , , , , , , , , , , , , , ,	Black	70%	19%	11%
	Latino	55%	36%	9%
Race and Education	White - Not College Graduate	28%	64%	8%
	White - College Graduate	50%	46%	4%
Gender - Race - Education	Men - White - Not College Graduate	20%	72%	8%
	Men - White - College Graduate	44%	53%	3%
	Women - White - Not College Graduate	35%	56%	9%
	Women - White - College Graduate	56%	40%	5%
Age	Under 45	43%	48%	9%
	45 or older	45%	49%	6%
Generation	Gen Z/Millennials (18-40)	45%	45%	11%
	Gen X (41-56)	43%	52%	5%
	Baby Boomers (57-75)	44%	52%	5%
	Silent-Greatest (Over 75)	52%	43%	5%
Gender	Men	38%	56%	6%
	Women	49%	42%	9%
White Evangelical Christians		17%	77%	6%
2020 Support	Biden	80%	13%	7%
	Тгитр	5%	92%	3%
Area Description	Big city	50%	42%	8%
	Small city	43%	47%	11%
	Suburban	40%	40%	7%
	Small town		50%	6%
		44 %	620%	70/
Small city/Suburban Man	INUIDI	30%	570/	(70
		30%	D/ %	/ %
Small city/Suburban Women		51%	39%	9%

NPR/PBS NewsHour/Marist Poll National Adults. Interviews conducted October 18th through October 22nd, 2021. Totals may not add to 100% due to rounding.

BIDJP105TRND. Marist Poll National Trend

		National Adults	
	Do you approve or o	lisapprove of the job Joe Biden i	s doing as president?
	Approve	Disapprove	Vol: Unsure
	Row %	Row %	Row %
November 2021	44%	49%	8%
September 30th, 2021	45%	46%	9%
September 3rd, 2021	43%	51%	7%
August 2021	49%	44%	6%
July 2021	50%	43%	7%
June 2021	51%	46%	1%
May 27th, 2021	52%	41%	7%
May 17th, 2021	53%	41%	6%
April 27th, 2021	54%	44%	3%
April 16th, 2021	53%	39%	8%
March 30th, 2021	52%	40%	8%
March 11th, 2021	49%	42%	10%
February 2021	51%	38%	11%
January 2021	49%	35%	16%

Marist Poll National Adults

National Adults

Do you approve or disapprove of the job Joe Biden is doing as president? [And, would you say you strongly approve/disapprove of the job he is doing or just approve/disapprove?]

				Strongly		
		Strongly approve	Approve	Disapprove	disapprove	Vol: Unsure
		Row %	Row %	Row %	Row %	Row %
National Adults		15%	28%	12%	37%	8%
National Registered Voters	-	16%	27%	12%	39%	6%
Party Identification	Democrat	35%	49%	8%	3%	5%
	Republican	1%	5%	10%	80%	4%
	Independent	13%	26%	16%	36%	9%
Region	Northeast	14%	32%	9%	40%	5%
	Midwest	13%	30%	12%	33%	12%
	South	15%	26%	12%	39%	7%
	West	18%	27%	14%	34%	7%
Household Income	Less than \$50,000	16%	33%	12%	32%	7%
	\$50,000 or more	15%	28%	11%	39%	7%
Education	Not college graduate	13%	26%	13%	38%	10%
	College graduate	18%	31%	11%	35%	4%
Race/Ethnicity	White	15%	23%	12%	44%	6%
	Non-white	17%	37%	13%	25%	8%
Race/Ethnicity	White	15%	23%	12%	44%	6%
	Black	36%	35%	10%	9%	11%
	Latino	10%	45%	15%	21%	9%
Race and Education	White - Not College Graduate	10%	18%	14%	50%	8%
	White - College Graduate	21%	29%	9%	37%	4%
Gender - Race - Education	Men - White - Not College Graduate	9%	11%	15%	56%	8%
	Men - White - College Graduate	14%	30%	10%	42%	3%
	Women - White - Not College Graduate	11%	24%	12%	44%	9%
	Women - White - College Graduate	27%	29%	7%	32%	5%
Age	Under 45	6%	37%	16%	31%	9%
	45 or older	23%	22%	9%	41%	6%
Generation	Gen Z/Millennials (18-40)	5%	39%	17%	28%	11%
	Gen X (41-56)	16%	27%	13%	39%	5%
	Baby Boomers (57-75)	24%	20%	6%	45%	5%
	Silent-Greatest (Over 75)	28%	24%	8%	35%	5%
Gender	Men	12%	26%	12%	43%	6%
	Women	19%	30%	12%	30%	9%
White Evangelical Christians		6%	11%	7%	71%	6%
2020 Support	Biden	33%	47%	8%	5%	7%
	Trump	1%	4%	10%	81%	3%
Area Description	Big city	18%	33%	12%	29%	8%
	Small city	16%	26%	10%	37%	11%
	Suburban	14%	30%	16%	33%	7%
	Small town	15%	29%	11%	39%	6%
	Rural	12%	18%	8%	55%	7%
Small city/Suburban Men		11%	25%	14%	43%	7%
Small city/Suburban Women		20%	32%	13%	26%	9%

BIDJP105RTRND. Marist Poll National Trend

	National Adults						
	Do you approve or disapprove of the job Joe Biden is doing as president? [And, would you say you strongly approve/disappro doing or just approve/disapprove?]						
	Strongly approve	Approve	Disapprove	Strongly disapprove	Vol: Unsure		
	Row %	Row %	Row %	Row %	Row %		
November 2021	15%	28%	12%	37%	8%		
September 30th, 2021	17%	28%	9%	37%	9%		
September 3rd, 2021	19%	23%	10%	41%	7%		
August 2021	19%	30%	15%	30%	6%		
July 2021	21%	29%	11%	32%	7%		
June 2021	24%	28%	13%	34%	2%		
May 27th 2021	21%	31%	13%	28%	7%		
May 17th, 2021	26%	26%	11%	30%	6%		
April 27th, 2021	25%	29%	12%	32%	3%		
April 16th, 2021	25%	28%	10%	29%	8%		
March 30th, 2021	22%	30%	11%	29%	8%		
March 11th, 2021	24%	24%	12%	30%	10%		
February 2021	23%	29%	13%	25%	11%		
January 2021	25%	25%	11%	24%	16%		

Marist Poll National Adults

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			National Regi	stered Voters	
		If next year's elect are	ion for Congress we e you more likely to v	re held today, whic ote for in your dist	ch party's candidate rrict:
		Democrat	Republican	Vol: Other	Vol: Unsure
		Row %	Row %	Row %	Row %
National Registered Voters		44%	41%	4%	11%
Party Identification	Democrat	92%	3%	0%	5%
	Republican	1%	96%	0%	2%
	Independent	38%	32%	9%	21%
Region	Northeast	39%	47%	4%	10%
	Midwest	49%	34%	7%	11%
	South	44%	44%	2%	9%
	West	44%	38%	3%	14%
Household Income	Less than \$50,000	51%	32%	3%	14%
	\$50,000 or more	43%	44%	4%	9%
Education	Not college graduate	38%	46%	3%	13%
	College graduate	50%	36%	5%	9%
Race/Ethnicity	White	38%	47%	3%	12%
	Non-white	54%	31%	5%	10%
Race/Ethnicity	White	38%	47%	3%	12%
	Black	81%	4%	6%	8%
	Other	42%	42%	5%	11%
Race and Education	White - Not College Graduate	29%	55%	2%	13%
	White - College Graduate	48%	38%	4%	10%
Gender - Race - Education	Men - White - Not College Graduate	23%	62%	4%	10%
	Men - White - College Graduate	44%	43%	3%	10%
	Women - White - Not College Graduate	34%	49%	1%	16%
	Women - White - College Graduate	51%	33%	5%	11%
Age	Under 45	46%	39%	5%	11%
	45 or older	43%	42%	3%	11%
Generation	Gen Z/Millennials (18-40)	49%	38%	5%	8%
	Gen X (41-56)	45%	42%	4%	10%
	Baby Boomers (57-75)	40%	44%	3%	14%
	Silent-Greatest (Over 75)	46%	38%	3%	14%
Gender	Men	37%	49%	5%	10%
	Women	51%	33%	3%	12%
White Evangelical Christians		15%	75%	3%	6%
2020 Support	Biden	83%	6%	2%	10%
	Trump	2%	88%	4%	6%
Area Description	Big city	56%	34%	3%	7%
	Small city	37%	43%	5%	15%
	Suburban	46%	37%	5%	12%
	Small town	42%	50%	2%	6%
	Rural	-⊤∠ /u 300⁄2	10%	2 /0 10/	17%
Small city/Suburban Men	i turai	320%	40%	+ 70 50/	1204
Small city/Suburban Warsan		53%	210/	5%	10%
Undit Gity/OupUrban women		U 170	0,170	U70	1.370

 Small city/Suburban Women
 51%
 31%
 5%
 13%

 NPR/PBS NewsHour/Marist Poll National Registered Voters. Interviews conducted October 18th through October 22nd, 2021. Totals may not add to 100% due to rounding.
 Totals may

USCNGS01TRND. Marist Poll National Trend

	National Registered Voters						
	If next year's election for Congress were held today, which party's candidate are you more likely						
	Democrat	to vote for in Republican	Your district:	Vol Ungura			
	Row %	Row %	Row %	Row %			
November 2021	44%	41%	4%	11%			
September 2021	46%	38%	4%	12%			
August 2020	49%	43%	2%	6%			
February 2020	48%	40%	1%	11%			
November 2019	46%	41%	2%	11%			
October 2019	43%	40%	2%	15%			
November 2018	50%	44%	3%	4%			
October 26th, 2018	50%	40%	5%	6%			
October 3rd, 2018	48%	42%	4%	5%			
September 26th, 2018	48%	41%	5%	6%			
September 13th, 2018	50%	38%	7%	6%			
July 2018	47%	40%	7%	7%			
April 2018	44%	39%	8%	9%			
March 2018	44%	39%	6%	12%			
February 23, 2018	46%	39%	6%	10%			
February 9, 2018	49%	38%	5%	8%			
January 2018	46%	40%	6%	9%			
December 2017	50%	37%	7%	7%			
November 21, 2017	43%	40%	6%	10%			
November 14, 2017	51%	36%	6%	8%			
August 2017	47%	40%	5%	8%			
June 2017	48%	38%	6%	8%			
April 2017	45%	38%	7%	10%			
March 2017	47%	38%	8%	7%			
August 2014	38%	43%	6%	12%			
April 2014	48%	42%	4%	6%			
February 2014	46%	44%	4%	5%			
December 2013	43%	43%	6%	8%			

Marist Poll National Registered Voters

TRUSTOR1DR. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

		National Adults			
		How much d	o you trust that elect	ions are fair:	
		A great deal/A good amount	Not very much/Not at all	Vol: Unsure	
		Row %	Row %	Row %	
National Adults		58%	39%	2%	
National Registered Voters		60%	38%	2%	
Party Identification	Democrat	86%	13%	2%	
	Republican	34%	64%	2%	
	Independent	60%	39%	1%	
Region	Northeast	64%	36%	0%	
	Midwest	57%	37%	6%	
	South	56%	42%	2%	
	West	58%	41%	1%	
Household Income	Less than \$50,000	60%	37%	3%	
	\$50,000 or more	61%	38%	1%	
Education	Not college graduate	50%	47%	3%	
	College graduate	69%	29%	2%	
Race/Ethnicity	White	59%	39%	3%	
,	Non-white	60%	40%	1%	
Race/Ethnicity	White	59%	39%	3%	
	Black	70%	28%	1%	
		59%	41%	1%	
Race and Education	White - Not College Graduate	48%	49%	3%	
	White - College Graduate	72%	26%	3%	
Gender - Race - Education	Men - White - Not College Graduate	45%	53%	2%	
Race and Education Gender - Race - Education	Men - White - College Graduate	71%	29%	0%	
	Women - White - Not College Graduate	49%	46%	5%	
	Women - White - College Graduate	72%	23%	5%	
Age	Under 45	57%	41%	2%	
	45 or older	60%	39%	2%	
Generation	Gen Z/Millennials (18-40)	58%	40%	2%	
	Gen X (41-56)	59%	39%	2%	
	Baby Boomers (57-75)	57%	42%	1%	
	Silent-Greatest (Over 75)	64%	33%	3%	
Gender	Men	56%	43%	1%	
	Women	60%	36%	4%	
Vhite Evangelical Christians		37%	60%	2%	
020 Support	Biden	86%	13%	2%	
	Trump	33%	65%	2%	
Area Description	Big city	58%	40%	2%	
	Small city	56%	42%	2%	
	Suburban	68%	29%	3%	
	Small town	56%	43%	2%	
	Rural	48%	48%	3%	
Small city/Suburban Men		59%	40%	1%	
		0370	+0 /0	1 /0	

TRUSTORD1TRND. Marist Poll National Trend

		National Adults				
	How much do you trust that	elections are fair: A great deal, a much, not at all?	good amount, not very			
	A great deal/A good amount	Not very much/Not at all	Unsure			
	Row %	Row %	Row %			
November 2021	58%	39%	2%			
September 2021	56%	42%	2%			
February 2020	53%	43%	4%			
January 2020	62%	37%	2%			
October 2019	51%	46%	3%			
October 2017	51%	47%	2%			
July 2017	50%	47%	2%			
March 2017	55%	43%	2%			

Marist Poll National Adults

TRUSTOR1D. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

		National Adults				
	-		How much do	you trust that electi	ions are fair	
	-					
		A great deal	A good amount	Not very much	Not at all	Vol: Unsure
	-	Row %	Row %	Row %	Row %	Row %
National Adults		30%	28%	22%	18%	2%
National Registered Voters		31%	29%	21%	17%	2%
Party Identification	Democrat	52%	34%	7%	5%	2%
	Republican	11%	23%	36%	28%	2%
	Independent	30%	30%	21%	18%	1%
Region	Northeast	36%	28%	22%	13%	0%
	Midwest	33%	24%	18%	19%	6%
	South	25%	32%	25%	16%	2%
	West	33%	25%	19%	22%	1%
Household Income	Less than \$50,000	27%	33%	21%	16%	3%
	\$50,000 or more	33%	28%	20%	18%	1%
Education	Not college graduate	24%	25%	26%	22%	3%
	College graduate	38%	31%	17%	12%	2%
Race/Ethnicity	White	32%	26%	21%	17%	3%
	Non-white	28%	32%	22%	18%	1%
Race/Ethnicity	White	32%	26%	21%	17%	3%
,	Black	38%	32%	20%	8%	1%
	Latino	30%	28%	17%	23%	1%
Race and Education	White - Not College Graduate	23%	24%	26%	23%	3%
	White - College Graduate	43%	28%	16%	10%	3%
Gender - Race - Education	Men - White - Not College Graduate	20%	25%	28%	25%	2%
	Men - White - College Graduate	44%	27%	20%	9%	0%
	Women - White - Not College Graduate	26%	24%	24%	22%	5%
	Women - White - College Graduate	42%	30%	12%	11%	5%
Age	Under 45	23%	33%	23%	18%	2%
	45 or older	36%	24%	21%	18%	2%
Generation	Gen Z/Millennials (18-40)	23%	35%	26%	14%	2%
	Gen X (41-56)	29%	30%	16%	23%	2%
	Baby Boomers (57-75)	36%	21%	23%	19%	1%
	Silent-Greatest (Over 75)	41%	23%	20%	13%	3%
Gender	Men	28%	28%	23%	20%	1%
	Women	32%	28%	21%	15%	4%
White Evangelical Christians		16%	21%	31%	30%	2%
2020 Support	Biden	52%	33%	10%	3%	2%
	Trump	10%	23%	33%	32%	2%
Area Description	Big city	30%	28%	25%	15%	2%
	Small city	31%	25%	24%	18%	2%
	Suburban	32%	36%	14%	15%	3%
	Small town	31%	25%	23%	20%	2%
	Rural	28%	21%	27%	22%	3%
Small city/Suburban Men		28%	31%	20%	20%	1%
Small city/Suburban Women		35%	32%	15%	14%	4%

TRUSTORD1TRND. Marist Poll National Trend

	National Adults								
	How muc	How much do you trust that elections are fair: A great deal, a good amount, not very much, not at all?							
	A great deal	A good amount	Not very much	Not at all	Unsure				
	Row %	Row %	Row %	Row %	Row %				
November 2021	30%	28%	22%	18%	2%				
September 2021	30%	26%	24%	18%	2%				
February 2020	17%	35%	30%	14%	4%				
January 2020	19%	43%	25%	11%	2%				
October 2019	17%	35%	30%	16%	3%				
October 2017	17%	34%	30%	17%	2%				
July 2017	17%	33%	29%	18%	2%				
March 2017	22%	33%	27%	16%	2%				

Marist Poll National Adults

CONFLCGVT2R. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

National Adults

How confident are you that your state or local government will conduct a fair and accurate election in 2022: Very confident, confident, not very confident, or not confident at all?

		Very confident/ Confident	Not very confident/Not confident at all	Vol: Unsure
		Row %	Row %	Row %
National Adults		70%	29%	1%
National Registered Voters		72%	27%	1%
Party Identification	Democrat	91%	9%	1%
	Republican	60%	39%	0%
	Independent	67%	32%	1%
Region	Northeast	67%	33%	0%
	Midwest	72%	27%	1%
	South	70%	28%	2%
	West	69%	31%	0%
Household Income	Less than \$50,000	68%	31%	1%
	\$50,000 or more	71%	29%	1%
Education	Not college graduate	62%	37%	1%
	College graduate	80%	19%	1%
Race/Ethnicity	White	72%	27%	1%
	Non-white	68%	32%	0%
Race/Ethnicity	White	72%	27%	1%
	Black	71%	29%	0%
	Latino	68%	32%	0%
Race and Education	White - Not College Graduate	64%	35%	1%
	White - College Graduate	81%	18%	1%
Gender - Race - Education	Men - White - Not College Graduate	61%	38%	1%
	Men - White - College Graduate	81%	19%	0%
	Women - White - Not College Graduate	67%	32%	1%
	Women - White - College Graduate	81%	18%	2%
Age	Under 45	68%	32%	0%
	45 or older	71%	27%	1%
Generation	Gen Z/Millennials (18-40)	68%	32%	0%
	Gen X (41-56)	73%	27%	1%
	Baby Boomers (57-75)	68%	30%	2%
	Silent-Greatest (Over 75)	74%	25%	2%
Gender	Men	68%	31%	1%
	Women	71%	28%	1%
White Evangelical Christians		60%	39%	0%
2020 Support	Biden	87%	13%	1%
	Trump	58%	41%	1%
Area Description	Big city	68%	30%	1%
	Small city	68%	31%	1%
	Suburban	80%	19%	1%
	Small town	67%	32%	1%
	Rural	60%	38%	2%
Small city/Suburban Men		68%	31%	1%
Small city/Suburban Women		81%	18%	1%

CONFLCGVT2. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

National Adults

How confident are you that your state or local government will conduct a fair and accurate election in 2022: Very confident, confident, not very confident, or not confident at all?

					Not confident at	
		Very confident	Confident	Not very confident	all	Vol: Unsure
		Row %	Row %	Row %	Row %	Row %
National Adults		32%	38%	18%	11%	1%
National Registered Voters		33%	39%	16%	12%	1%
Party Identification	Democrat	50%	41%	6%	3%	1%
	Republican	18%	42%	19%	20%	0%
	Independent	32%	35%	20%	12%	1%
Region	Northeast	35%	32%	19%	14%	0%
	Midwest	36%	36%	14%	13%	1%
	South	25%	45%	21%	7%	2%
	West	35%	34%	15%	16%	0%
Household Income	Less than \$50,000	29%	40%	18%	13%	1%
	\$50,000 or more	34%	37%	19%	10%	1%
Education	Not college graduate	25%	37%	23%	14%	1%
	College graduate	40%	39%	11%	8%	1%
Race/Ethnicity	White	35%	37%	17%	11%	1%
·	Non-white	28%	40%	20%	12%	0%
Race/Ethnicity	White	35%	37%	17%	11%	1%
	Black	29%	41%	24%	6%	0%
	Latino	22%	46%	21%	11%	0%
Race and Education	White - Not College Graduate	28%	36%	20%	15%	1%
	······	2070	0070	2070	10/0	170
	White - College Graduate	44%	37%	13%	6%	1%
Gender - Race - Education	Men - White - Not College Graduate	26%	35%	24%	14%	1%
	Men - White - College Graduate	47%	34%	15%	4%	0%
	Women - White - Not College Graduate	29%	38%	17%	15%	1%
	Women - White - College Graduate	40%	40%	10%	8%	2%
Age	Under 45	25%	43%	22%	10%	0%
	45 or older	37%	35%	15%	13%	1%
Generation	Gen Z/Millennials (18-40)	24%	43%	23%	9%	0%
	Gen X (41-56)	29%	43%	14%	13%	1%
	Baby Boomers (57-75)	41%	28%	17%	14%	2%
	Silent-Greatest (Over 75)	37%	36%	16%	9%	2%
Gender	Men	32%	36%	18%	13%	1%
	Women	31%	41%	18%	10%	1%
White Evangelical Christians		24%	37%	24%	15%	0%
2020 Support	Biden	52%	35%	9%	3%	1%
	Trump	17%	41%	21%	20%	1%
Area Description	Big city	31%	37%	17%	13%	1%
	Small city	28%	40%	18%	13%	1%
	Suburban	36%	44%	13%	6%	1%
	Small town	32%	36%	20%	12%	1%
	Rural	30%	31%	25%	13%	2%
Small city/Suburban Men		32%	37%	18%	13%	1%
Small city/Suburban Women		33%	48%	12%	6%	1%

ELCONFV20. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

National Adults

Compared to 2020, do you have more confidence or less confidence that your state or local government will conduct a fair and accurate election?

		Vol: About the			
		More confidence	Less confidence	same	Vol: Unsure
		Row %	Row %	Row %	Row %
National Adults		43%	37%	19%	2%
National Registered Voters		42%	36%	20%	2%
Party Identification	Democrat	55%	20%	23%	2%
	Republican	28%	60%	11%	1%
	Independent	42%	32%	23%	2%
Region	Northeast	42%	42%	15%	1%
	Midwest	43%	36%	18%	3%
	South	44%	37%	17%	2%
	West	40%	34%	23%	2%
Household Income	Less than \$50,000	47%	37%	13%	3%
	\$50,000 or more	41%	37%	22%	1%
Education	Not college graduate	40%	44%	13%	3%
	College graduate	46%	27%	26%	1%
Race/Ethnicity	White	39%	36%	22%	2%
	Non-white	50%	37%	12%	1%
Race/Ethnicity	White	39%	36%	22%	2%
	Black	59%	28%	10%	3%
	Latino	46%	41%	12%	0%
Race and Education	White - Not College Graduate	37%	46%	13%	4%
	White - College Graduate	40%	25%	34%	1%
Gender - Race - Education	Men - White - Not College Graduate	34%	50%	14%	3%
	Men - White - College Graduate	43%	25%	31%	0%
	Women - White - Not College Graduate	40%	43%	12%	5%
	Women - White - College Graduate	37%	25%	37%	1%
Age	Under 45	40%	39%	21%	0%
	45 or older	44%	35%	17%	3%
Generation	Gen Z/Millennials (18-40)	39%	42%	19%	0%
	Gen X (41-56)	41%	35%	22%	2%
	Baby Boomers (57-75)	44%	37%	18%	2%
	Silent-Greatest (Over 75)	53%	26%	14%	7%
Gender	Men	42%	38%	19%	2%
	Women	44%	36%	18%	3%
White Evangelical Christians		37%	47%	13%	3%
2020 Support	Biden	59%	16%	24%	1%
	Trump	26%	59%	13%	2%
Area Description	Bia city	49%	32%	17%	2%
	Small city	47%	36%	15%	2%
	Suburban	41%	34%	24%	1%
	Small town	38%	41%	10%	2%
	Rural	34%	46%	16%	2%
Small city/Suburban Men	ital	120/	36%	20%	0%
Small city/Suburban Women		40/0	330%	20%	20/

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		National Adults			
		If your candidate f you trust that	or Congress does the results are ac	not win in 2022, do curate, or not?	
		Yes	No	Vol: Unsure	
		Row %	Row %	Row %	
National Adults		71%	22%	7%	
National Registered Voters		73%	20%	7%	
Party Identification	Democrat	88%	8%	4%	
	Republican	53%	36%	11%	
	Independent	77%	18%	5%	
Region	Northeast	72%	22%	6%	
	Midwest	73%	19%	9%	
	South	72%	20%	8%	
	West	67%	27%	5%	
Household Income	Less than \$50,000	68%	25%	7%	
	\$50,000 or more	77%	18%	5%	
Education	Not college graduate	64%	28%	9%	
	College graduate	81%	14%	6%	
Race/Ethnicity	White	72%	20%	8%	
	Non-white	72%	23%	5%	
Race/Ethnicity	White	72%	20%	8%	
	Black	78%	13%	9%	
	Latino	71%	26%	3%	
Race and Education	White - Not College Graduate	62%	28%	10%	
	White - College Graduate	83%	11%	5%	
Gender - Race - Education	Men - White - Not College Graduate	63%	25%	12%	
	Men - White - College Graduate	84%	12%	4%	
	Women - White - Not College Graduate	62%	30%	9%	
	Women - White - College Graduate	83%	11%	7%	
Age	Under 45	75%	22%	3%	
	45 or older	69%	21%	10%	
Generation	Gen Z/Millennials (18-40)	77%	19%	4%	
	Gen X (41-56)	71%	25%	4%	
	Baby Boomers (57-75)	66%	24%	10%	
	Silent-Greatest (Over 75)	70%	15%	16%	
Gender	Men	71%	22%	7%	
	Women	72%	21%	7%	
White Evangelical Christians		58%	33%	10%	
2020 Support	Biden	93%	5%	2%	
	Trump	53%	35%	12%	
Area Description	Big city	71%	25%	4%	
	Small city	73%	21%	6%	
	Suburban	79%	17%	4%	
	Small town	67%	21%	12%	
	Rural	64%	24%	12%	
Small city/Suburban Men		76%	18%	6%	
Small city/Suburban Women		77%	19%	4%	

CNWN24.	NPR/PBS	NewsHour/Ma	arist Poll Nation	al Tables	October	18th through	October	22nd,	2021
								,	

		National Adults			
		If your candidate for president does not win in 202 you trust that the results are accurate, or not			
		Yes	No	Vol: Unsure	
		Row %	Row %	Row %	
National Adults		62%	31%	7%	
National Registered Voters		62%	31%	7%	
Party Identification	Democrat	82%	13%	5%	
	Republican	33%	59%	8%	
	Independent	68%	26%	6%	
Region	Northeast	62%	28%	10%	
	Midwest	67%	27%	7%	
	South	60%	32%	8%	
	West	60%	35%	5%	
Household Income	Less than \$50,000	64%	30%	6%	
	\$50,000 or more	64%	30%	6%	
Education	Not college graduate	55%	37%	8%	
	College graduate	70%	24%	6%	
Race/Ethnicity	White	60%	32%	8%	
	Non-white	66%	29%	5%	
Race/Ethnicity	White	60%	32%	8%	
	Black	76%	17%	6%	
	Latino	59%	38%	4%	
Race and Education	White - Not College Graduate	52%	38%	10%	
	White - College Graduate	69%	25%	7%	
Gender - Race - Education	Men - White - Not College Graduate	52%	38%	10%	
	Men - White - College Graduate	69%	28%	3%	
	Women - White - Not College Graduate	52%	38%	10%	
	Women - White - College Graduate	68%	21%	10%	
Age	Under 45	67%	30%	3%	
	45 or older	57%	32%	11%	
Generation	Gen Z/Millennials (18-40)	69%	29%	2%	
	Gen X (41-56)	58%	35%	7%	
	Baby Boomers (57-75)	57%	34%	9%	
	Silent-Greatest (Over 75)	58%	24%	17%	
Gender	Men	60%	35%	5%	
	Women	64%	27%	9%	
White Evangelical Christians		41%	51%	9%	
2020 Support	Biden	87%	8%	5%	
	Trump	33%	58%	9%	
Area Description	Big city	64%	31%	5%	
	Small city	62%	32%	5%	
	Suburban	69%	25%	5%	
	Small town	57%	32%	10%	
	Rural	49%	38%	12%	
Small city/Suburban Men		65%	30%	4%	
- Small city/Suburban Women		68%	26%	6%	

NPR/PBS NewsHour/Marist Poll National Adults. Interviews conducted October 18th through October 22nd, 2021. Totals may not add to 100% due to rounding.

CNCDDM1. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

		National Adults			
		Regardless of political party, when candidates who elections don't concede, do they:			
		Do more harm than good to the democracy	Do more good than harm to the democracy	Vol: Unsure	
		Row %	Row %	Row %	
National Adults		70%	18%	12%	
National Registered Voters		70%	17%	13%	
Party Identification	Democrat	86%	10%	4%	
	Republican	56%	24%	20%	
	Independent	69%	17%	14%	
Region	Northeast	73%	13%	13%	
	Midwest	69%	18%	13%	
	South	66%	19%	15%	
	West	76%	17%	7%	
Household Income	Less than \$50,000	68%	21%	11%	
	\$50,000 or more	74%	16%	10%	
Education	Not college graduate	68%	19%	12%	
	College graduate	73%	15%	12%	
Race/Ethnicity	White	67%	19%	13%	
	Non-white	76%	15%	10%	
Race/Ethnicity	White	67%	19%	13%	
	Black	77%	18%	5%	
	Latino	79%	12%	9%	
Race and Education	White - Not College Graduate	63%	22%	15%	
	White - College Graduate	73%	16%	11%	
Gender - Race - Education	Men - White - Not College Graduate	62%	23%	15%	
	Men - White - College Graduate	73%	18%	10%	
	Women - White - Not College Graduate	63%	21%	16%	
	Women - White - College Graduate	73%	14%	13%	
Age	Under 45	77%	13%	10%	
	45 or older	65%	21%	14%	
Generation	Gen Z/Millennials (18-40)	79%	12%	9%	
	Gen X (41-56)	67%	23%	10%	
	Baby Boomers (57-75)	66%	20%	15%	
	Silent-Greatest (Over 75)	64%	18%	18%	
Gender	Men	69%	20%	11%	
	Women	71%	16%	13%	
White Evangelical Christians		53%	29%	18%	
2020 Support	Biden	88%	9%	3%	
	Trump	50%	27%	23%	
Area Description	Big city	74%	16%	9%	
	Small city	72%	16%	12%	
	Suburban	74%	12%	14%	
	Small town	68%	20%	12%	
	Rural	58%	26%	16%	
Small city/Suburban Men		73%	16%	11%	
Small city/Suburban Women		72%	12%	16%	

THRTELE2. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

			-	National	Adults		
		Which do you think is the biggest threat to fair elections:					
		Voter suppression	Voter fraud	Vote tampering by the opposing political party	Interference from another country such as Russia or China	Vote tampering by local election officials	Vol: Unsure
		Row %	Row %	Row %	Row %	Row %	Row %
National Adults		29%	21%	18%	14%	13%	6%
National Registered Voters		29%	21%	18%	14%	13%	6%
Party Identification	Democrat	55%	7%	12%	17%	6%	4%
	Republican	2%	34%	29%	13%	17%	6%
	Independent	29%	22%	15%	12%	15%	7%
Region	Northeast	31%	18%	18%	14%	13%	6%
	Midwest	29%	21%	16%	17%	8%	8%
	South	27%	21%	19%	14%	14%	5%
	West	29%	22%	18%	10%	15%	6%
Household Income	Less than \$50,000	26%	18%	20%	17%	14%	5%
	\$50,000 or more	32%	22%	18%	12%	12%	5%
Education	Not college graduate	22%	23%	22%	13%	15%	6%
	College graduate	38%	18%	13%	14%	11%	6%
Race/Ethnicity	White	26%	24%	18%	15%	11%	7%
	Non-white	32%	16%	19%	13%	16%	4%
Race/Ethnicity	White	26%	24%	18%	15%	11%	7%
	Black	39%	12%	18%	17%	10%	4%
	Latino	29%	18%	13%	11%	26%	3%
Race and Education	White - Not College Graduate	16%	28%	22%	16%	11%	7%
	White - College Graduate	38%	20%	12%	13%	10%	7%
Gender - Race - Education	Men - White - Not College Graduate	16%	39%	18%	13%	9%	5%
	Men - White - College Graduate	34%	20%	12%	12%	13%	9%
	Women - White - Not College Graduate	17%	18%	26%	19%	12%	9%
	Women - White - College Graduate	41%	21%	13%	13%	7%	4%
Age	Under 45	37%	17%	19%	9%	15%	3%
	45 or older	23%	23%	17%	17%	12%	8%
Generation	Gen Z/Millennials (18-40)	38%	15%	19%	9%	17%	2%
	Gen X (41-56)	27%	27%	16%	17%	9%	4%
	Baby Boomers (57-75)	23%	25%	19%	16%	12%	6%
	Silent-Greatest (Over 75)	17%	17%	17%	17%	14%	19%
Gender	Men	27%	26%	15%	10%	15%	7%
	Women	31%	16%	21%	17%	11%	5%
White Evangelical Christians		9%	32%	27%	13%	11%	8%
2020 Support	Biden	56%	7%	12%	15%	6%	4%
	Trump	2%	36%	27%	11%	17%	7%
Area Description	Big city	31%	18%	18%	11%	18%	4%
	Small city	28%	23%	16%	11%	14%	8%
	Suburban	35%	19%	13%	17%	11%	5%
	Small town	26%	19%	25%	13%	12%	5%
	Rural	19%	29%	20%	17%	8%	6%
Small city/Suburban Men		30%	26%	13%	9%	13%	8%
Small city/Suburban Women		34%	15%	15%	21%	11%	4%

Republicans and Republican leaning independents

Do you think Republicans have a better chance of winning the presidency in 2024 if Donald Trump is the party's nominee, or if someone else is the party's nominee?

		Donald Trump	Someone else	Vol: Unsure
		Row %	Row %	Row %
Republicans and Republican	eaning independents	50%	35%	14%
Party Identification	Republican	57%	29%	13%
	Independent	37%	47%	16%
Household Income	Less than \$50,000	60%	29%	11%
	\$50,000 or more	50%	36%	14%
Education	Not college graduate	58%	28%	14%
	College graduate	41%	45%	15%
Race and Education	White - Not College Graduate	52%	31%	17%
	White - College Graduate	35%	48%	17%
Gender	Men	50%	36%	14%
	Women	51%	35%	14%
White Evangelical Christians		53%	30%	17%
Area Description	Big city	56%	35%	9%
	Small city/Suburban	43%	45%	12%
	Small town	56%	29%	14%
	Rural	53%	22%	25%

NPR/PBS NewsHour/Marist Poll National Republicans and Republican leaning independents. Interviews conducted October 18th through October 22nd, 2021. Totals may not add to 100% due to rounding.

Democrats and Democratic leaning independents

Do you think Democrats have a better chance of winning the presidency in 2024 if Joe Biden is the party's nominee, or if someone else is the party's nominee?

		Joe Biden	Someone else	Vol: Unsure
	-	Row %	Row %	Row %
Democrats and Democratic leaning independents		36%	44%	20%
Party Identification	Democrat	41%	41%	18%
	Independent	26%	51%	23%
Region	Northeast	34%	40%	26%
	Midwest	36%	51%	13%
	South	31%	51%	19%
	West	45%	31%	24%
Household Income	Less than \$50,000	35%	49%	16%
	\$50,000 or more	36%	44%	21%
Education	Not college graduate	36%	45%	19%
	College graduate	35%	45%	20%
Race/Ethnicity	White	30%	44%	26%
	Non-white	43%	43%	13%
Race and Education	White - Not College Graduate	33%	43%	24%
	White - College Graduate	28%	45%	27%
Gender	Men	38%	49%	13%
	Women	35%	41%	24%
Area Description	Big city	37%	53%	10%
	Small city/Suburban	39%	38%	23%
	Small town/Rural	30%	45%	25%

NPR/PBS NewsHour/Marist Poll National Democrats and Democratic leaning independents. Interviews conducted October 18th through October 22nd, 2021. Totals may not add to 100% due to rounding.

National Adults

In January, the U.S. Congress certified the results of the 2020 presidential election making Joe Biden president. Since then some states have conducted additional recounts of the 2020 presidential election results. Do you think these recounts have been done:

		Mostly because there are real cases of fraud in these states	Mostly because state officials just don't like the outcome	Vol: Unsure
		Row %	Row %	Row %
National Adults		40%	53%	7%
National Registered Voters		39%	54%	7%
Party Identification	Democrat	9%	84%	6%
	Republican	76%	18%	6%
	Independent	36%	56%	8%
Region	Northeast	38%	56%	6%
	Midwest	42%	52%	6%
	South	40%	52%	8%
	West	40%	53%	7%
Household Income	Less than \$50,000	38%	56%	6%
	\$50,000 or more	40%	55%	5%
Education	Not college graduate	45%	47%	8%
	College graduate	34%	60%	6%
Race/Ethnicity	White	44%	48%	7%
	Non-white	34%	62%	4%
Race/Ethnicity	White	44%	48%	7%
	Black	18%	76%	6%
	Latino	40%	57%	3%
Race and Education	White - Not College Graduate	54%	38%	8%
	White - College Graduate	33%	60%	7%
Gender - Race - Education	Men - White - Not College Graduate	60%	33%	7%
	Men - White - College Graduate	36%	56%	8%
	Women - White - Not College Graduate	47%	43%	9%
	Women - White - College Graduate	31%	63%	6%
Age	Under 45	38%	57%	5%
	45 or older	42%	50%	8%
Generation	Gen Z/Millennials (18-40)	35%	60%	5%
	Gen X (41-56)	45%	50%	5%
	Baby Boomers (57-75)	45%	49%	5%
	Silent-Greatest (Over 75)	31%	52%	17%
Gender	Men	45%	49%	6%
	Women	36%	57%	7%
White Evangelical Christians		67%	24%	9%
2020 Support	Biden	9%	86%	5%
	Trump	76%	15%	8%
Area Description	Big city	34%	59%	8%
	Small city	40%	50%	10%
	Suburban	37%	59%	4%
	Small town	45%	49%	6%
	Rural	53%	41%	6%
Small city/Suburban Men		42%	51%	7%
Small city/Suburban Women		34%	60%	6%

DTELFR20. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

National Adults

Since the U.S. Congress certified the results of the 2020 presidential election in January making Joe Biden president, Donald Trump has continued to say the 2020 election was rigged. Do you think he continues to say this:

		Mostly because he is right, there were real cases of fraud that changed the	Vol: Unover	
		Row %	Row %	Row %
National Adults		34%	62%	5%
National Registered Voters		33%	63%	4%
Party Identification	Democrat	2%	96%	2%
r arty horitinoation	Republican	75%	21%	4%
	Independent	26%	69%	4%
Region	Northeast	32%	64%	4%
lingion	Midwest	34%	62%	4%
	South	35%	61%	4%
	West	32%	61%	7%
Household Income	Less than \$50.000	29%	67%	4%
	\$50,000 or more	35%	61%	4%
Education	Not college graduate	38%	56%	6%
	College graduate	27%	69%	3%
Race/Ethnicity	White	39%	57%	5%
	Non-white	25%	71%	4%
Race/Ethnicity	White	39%	57%	5%
	Black	11%	85%	4%
	Latino	25%	73%	2%
Race and Education	White - Not College Graduate	46%	48%	6%
	White - College Graduate	30%	67%	3%
Gender - Race - Education	Men - White - Not College Graduate	50%	45%	5%
	Men - White - College Graduate	32%	64%	4%
	Women - White - Not College Graduate	43%	51%	6%
	Women - White - College Graduate	27%	70%	2%
Age	Under 45	29%	66%	4%
	45 or older	37%	59%	5%
Generation	Gen Z/Millennials (18-40)	27%	68%	5%
	Gen X (41-56)	38%	57%	5%
	Baby Boomers (57-75)	38%	59%	3%
	Silent-Greatest (Over 75)	31%	60%	9%
Gender	Men	38%	57%	5%
	Women	30%	66%	4%
White Evangelical Christians		64%	32%	5%
2020 Support	Biden	3%	96%	1%
	Trump	74%	20%	6%
Area Description	Big city	24%	71%	5%
	Small city	30%	63%	7%
	Suburban	30%	68%	2%
	Small town	46%	49%	5%
	Rural	45%	50%	5%
Small city/Suburban Men		32%	62%	6%
Small city/Suburban Women		29%	70%	1%

THRTDEM1. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

			National Adults		
		When thinking about the issues that divide the nation do you think:			
		There is a serious threat to the future of our democracy	There is not a serious threat to the future of our democracy	Vol: Unsure	
		Row %	Row %	Row %	
National Adults		81%	15%	4%	
National Registered Voters		82%	14%	4%	
Party Identification	Democrat	79%	17%	4%	
	Republican	89%	9%	2%	
	Independent	80%	15%	5%	
Region	Northeast	83%	15%	2%	
	Midwest	83%	12%	6%	
	South	81%	16%	4%	
	West	79%	17%	4%	
Household Income	Less than \$50,000	77%	18%	5%	
	\$50,000 or more	83%	14%	2%	
Education	Not college graduate	80%	15%	5%	
	College graduate	83%	14%	3%	
Race/Ethnicity	White	83%	13%	4%	
	Non-white	78%	19%	3%	
Race/Ethnicity	White	83%	13%	4%	
	Black	78%	18%	4%	
	Latino	69%	28%	3%	
Race and Education	White - Not College Graduate	82%	12%	5%	
	White - College Graduate	83%	13%	3%	
Gender - Race - Education	Men - White - Not College Graduate	85%	11%	4%	
	Men - White - College Graduate	79%	18%	3%	
	Women - White - Not College Graduate	80%	13%	7%	
	Women - White - College Graduate	88%	8%	3%	
Age	Under 45	81%	16%	3%	
	45 or older	81%	14%	5%	
Generation	Gen Z/Millennials (18-40)	78%	18%	3%	
	Gen X (41-56)	84%	13%	3%	
	Baby Boomers (57-75)	84%	12%	4%	
	Silent-Greatest (Over 75)	74%	17%	10%	
Gender	Men	77%	20%	3%	
	Women	85%	10%	5%	
White Evangelical Christians		87%	8%	4%	
2020 Support	Biden	80%	17%	3%	
	Trump	87%	10%	2%	
Area Description	Big city	81%	16%	2%	
	Small city	74%	20%	5%	
	Suburban	79%	17%	4%	
	Small town	83%	11%	5%	
	Rural	90%	8%	2%	
Small city/Suburban Men		75%	21%	4%	
Small city/Suburban Women		80%	15%	5%	

USCNGS01TRND. Marist Poll National Trend

	National Adults			
	When thinking about issues that divide the nation, do you think:			
	There is a serious threat to the future of There is not a serious threat to the			
	our democracy	future of our democracy	Vol. Unsure	
	Row %	Row %	Row %	
November 2021	81%	15%	4%	
January 2021	81%	15%	3%	

Marist Poll National Adults

PRTTHRDM1. NPR/PBS NewsHour/Marist Poll National Tables October 18th through October 22nd, 2021

National Adults

In general, which party do you think is the bigger threat to democracy in the United States:

		The Democratic Partv	The Republican Partv	Vol: Both	Vol: Neither	Vol: Unsure
		Row %	Row %	Row %	Row %	Row %
National Adults		42%	41%	8%	5%	4%
National Registered Voters		43%	42%	7%	4%	3%
Party Identification	Democrat	5%	87%	3%	3%	2%
	Republican	88%	2%	5%	2%	3%
	Independent	41%	37%	12%	7%	4%
Region	Northeast	45%	42%	8%	2%	3%
-	Midwest	39%	45%	8%	4%	4%
	South	44%	38%	8%	5%	4%
	West	37%	42%	8%	7%	6%
Household Income	Less than \$50,000	37%	43%	8%	6%	6%
	\$50,000 or more	44%	43%	7%	4%	2%
Education	Not college graduate	46%	36%	7%	5%	6%
	College graduate	36%	48%	10%	4%	2%
Race/Ethnicity	White	47%	37%	7%	5%	4%
	Non-white	35%	49%	10%	3%	.3%
Race/Ethnicity	White	47%	37%	7%	5%	4%
	Black	13%	70%	7%	3%	8%
	Latino	39%	45%	10%	6%	1%
Race and Education	White - Not College Graduate	56%	27%	5%	6%	6%
	White - Not Conege Craduate	50 %	21 /0	570	070	070
	White - College Graduate	36%	48%	9%	5%	2%
Gender - Race - Education	Men - White - Not College Graduate	58%	25%	5%	6%	6%
	Men - White - College Graduate	44%	45%	5%	4%	2%
	Women - White - Not College Graduate	54%	30%	4%	6%	7%
	Women - White - College Graduate	28%	52%	13%	5%	1%
Age	Under 45	37%	43%	11%	6%	4%
	45 or older	45%	41%	6%	4%	5%
Generation	Gen Z/Millennials (18-40)	36%	43%	10%	6%	5%
	Gen X (41-56)	43%	43%	7%	5%	2%
	Baby Boomers (57-75)	46%	38%	7%	4%	5%
	Silent-Greatest (Over 75)	42%	44%	5%	2%	6%
Gender	Men	48%	35%	9%	5%	3%
	Women	35%	47%	7%	5%	6%
White Evangelical Christians		74%	16%	5%	3%	2%
2020 Support	Biden	10%	80%	4%	4%	2%
	Trump	86%	2%	5%	4%	3%
Area Description	Big city	39%	48%	6%	3%	4%
	Small city	45%	33%	10%	3%	8%
	Suburban	32%	46%	13%	7%	1%
	Small town	47%	39%	4%	5%	5%
	Rural	51%	31%	6%	5%	6%
Small city/Suburban Men		45%	34%	12%	5%	4%
Small city/Suburban Women		31%	48%	11%	6%	4%
Exhibit 4



No evidence for systematic voter fraud: A guide to statistical claims about the 2020 election

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After the 2020 US presidential election Donald Trump refused to concede, alleging widespread and unparalleled voter fraud. Trump's supporters deployed several statistical arguments in an attempt to cast doubt on the result. Reviewing the most prominent of these statistical claims, we conclude that none of them is even remotely convincing. The common logic behind these claims is that, if the election were fairly conducted, some feature of the observed 2020 election result would be unlikely or impossible. In each case, we find that the purportedly anomalous fact is either not a fact or not anomalous.

election security | fraud detection | science communication

Collowing the 2020 US elections, President Trump and other Republicans questioned Biden's victory in public statements and lawsuits. Although Trump's legal challenges were unsuccessful, many of his supporters were apparently convinced by his claims that the election was stolen: A survey in December 2020 found that over 75% of Republican voters found merit in claims that millions of fraudulent ballots were cast, voting machines were manipulated, and thousands of votes were recorded for dead people (1).

In this paper, we consider several widely disseminated claims purporting to call into question the 2020 US presidential election result. We focus on statistical claims, i.e., claims that are based on allegedly anomalous patterns in the official vote counts. The common logic of these claims is that some aspect of the 2020 result would be highly unlikely or even impossible if the election had been properly administered. We performed an extensive search to identify the most pervasive such claims appearing in social media posts, expert witness testimony, and research papers.^{*} Our purpose in this paper is to address several of the most pervasive statistical claims in one place and using a common conceptual framework.

We conclude that each of the statistical claims we consider fails in one of two ways. In some instances, accurate claims are made about the election results but they are not actually inconsistent with a free and fair election. In other instances, the supposedly anomalous fact about the 2020 election result turns out to be incorrect.

The 2020 election was remarkable in many ways (e.g., unusually high levels of mail-in voting and turnout), and election administration may well have been imperfect. But we see nothing in these statistical tests that supports Trump's claim of a stolen election.

This research builds on efforts to assess the prevalence of fraud in prior elections in the United States (2–4) and other democracies (5). We also work in parallel with a large number of legal briefs filed by political science experts after the 2020 election (for example, refs. 6 and 7).

Claims Based on Facts That Are Not Actually Anomalous

Biden's Share of US Counties Is Not Anomalous. Conservative radio talk show host Charlie Kirk tweeted on 20 December 2020, "Does

anyone else have a hard time believing Joe Biden won a recordhigh number of votes despite winning a record-low number of counties?"[†] Later that day, he provided numbers to back up the claim, stating that Barack Obama won 69 million votes and 873 counties (in 2008) and Donald Trump won 74 million votes and 2,497 counties (in 2020), while Biden won 81 million votes and just 477 counties (also in 2020).[‡] While Kirk understated the number of counties Biden won (537, not 477), the basic fact is correct: Biden won far more votes than Trump or Obama while winning far fewer counties than Trump and somewhat fewer counties than Obama.[§] If Biden won so few counties, how could he have legitimately won so many votes?[¶]

Adding minimal context to Kirk's numbers reveals that there is nothing remotely suspicious or even anomalous about them. The reason Biden won a clear majority of votes while winning a minority of counties is that his support was concentrated in populous counties. This is typical of recent Democratic presidential candidates. Fig. 1 shows the proportion of votes and counties won by Democratic presidential candidates over the last several decades. As Democratic support has become more concentrated in cities, Democratic candidates have tended to win a smaller

Significance

President Donald Trump claimed that the 2020 US presidential election was stolen; millions of Americans apparently believed him. We assess the most prominent statistical claims offered by Trump and his allies as evidence of election fraud, including claims about Dominion voting machines switching votes from Trump to Biden, suspiciously high turnout in Democratic strongholds, and the supposedly inexplicable failure of Biden to win "bellwether counties." We use a combination of statistical reasoning and original data analysis to assess these claims. We hope our analysis contributes to public discussion about the integrity of the 2020 election and broader challenges of election security and election administration.

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[†] https://twitter.com/charliekirk11/status/1340692425635979266. [‡] https://archive.vn/0phvm#selection-3045.143-3045.190.

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[§]By "counties" we mean counties and county equivalents, e.g., parishes in Louisiana.

¹Turning Kirk's question around, one could ask, If Trump won so few votes, how could he have legitimately won so many counties? The same point could be made for many of these claims.

^{*}SI Appendix, section A describes our search process.



Fig. 1. Biden's share of votes and counties won in 2020 is typical of that of recent Democratic presidential candidates.

share of counties even as their share of votes holds steady. Judging by both votes and counties, Biden did slightly better than Hillary Clinton in 2016 and worse than Obama in 2008. (Biden won many more votes than Obama, as Kirk pointed out, but a smaller share of votes; turnout in 2020 was extraordinarily high.) Thus, the supposedly incredible discrepancy Charlie Kirk highlighted is simply the continuation of a stable trend in US presidential elections.

Biden's Share of Bellwether Counties Is Not Anomalous. A related claim was made about Biden's performance in "bellwether" counties, which are counties where a majority of voters have supported the election winner in several consecutive elections (8, 9). Of the 19 counties that voted for the eventual winner in every presidential election from 1980 to 2016, Biden defeated Trump in only one. Several commentators viewed this fact as anomalous. As stated in *The Federalist*, "Amazingly, [Biden] managed to secure victory while also losing in almost every bellwether county across the country. No presidential candidate has been capable of such electoral jujitsu until now" (10). Trump recited this fact in a rally in Georgia (11).

Biden's poor performance in bellwether counties makes sense given two facts. First, at the county level there was remarkable continuity between 2016 and 2020.[#] Not only did Biden win roughly the same proportion of counties as Clinton in 2016 (as shown in Fig. 1), but also he won almost the same set of counties: As shown in Fig. 2A only 63 counties switched from Trump to Biden. (For each county, we show Democratic vote margin in 2016 on the horizontal axis and in 2020 on the vertical.) The 19 bellwether counties are highlighted in red. Visual inspection suggests that, like other counties, they voted in 2020 roughly as they did in 2016; given this (and given that many of these counties went solidly for Trump in 2016), it is unsurprising that Biden won only one of them. Indeed, if we model the probability of Biden winning a county as a function of the county's Democratic margin in 2016 (making no distinction between bellwethers and others), we find that Biden would be expected to win between one and two bellwethers. Fig. 2B shows the probability of Biden winning a county in 2020 given the 2016 Democratic margin in the county, with the conditional relationship calculated using a generalized additive model. The expected number of bellwethers won by Biden is just 1.65 under this model; with alternative models we get estimates between 1.2 and 1.8.

Fig. 2*A* suggests, and the analysis in Fig. 2*B* assumes, that bellwether counties have no special tendency to side with the winner, conditional on the prior election result. Further analysis indicates that this has long been the case (8). To assess whether bellwethers are more likely than other counties to side with the winner in the future, we analyzed each election since 1996. We modeled a county's probability of correctly choosing the winner in a given election as a function of the Democratic margin in the county in the previous election and an indicator for whether the county had sided with the winner in each past election since 1980. We find only one election since 1996 in which bellwethers were more likely to side with the winner than other counties conditional on the county's previous election result (*SI Appendix*, Fig. 1).

Considering that bellwether counties appear to have no special prognostic value in general, and that county-level results were very similar in 2020 and 2016, it is neither surprising nor suspicious that Biden won just one of 19 bellwethers in 2020.

Differences between 2016 and 2020 Are Not Anomalous. Trump advocates argued on the basis of a statistical analysis that there was a "one-in-a-quadrillion" chance that Joe Biden legitimately won the election. This claim comes from an expert report submitted as part of Texas Attorney General Ken Paxton's lawsuit against the Commonwealth of Pennsylvania. In that report (12), Paxton claims that the expert, Charles Cicchetti, calculated a one-in-a-quadrillion chance of Biden winning; Cicchetti concludes his report by arguing that "In my opinion, the outcome of Biden winning . . . is so statistically improbable, that it is not possible to dismiss fraud and biased changes in the ways ballots were processed, validated, and tabulated" (p. 9a).

Cicchetti's assertion that Biden's victory was "statistically improbable" is based on a deeply misguided application of null hypothesis significance testing. Cicchetti never actually computes the probability of Biden winning. Instead, he tests the null hypothesis that Joe Biden in 2020 and Hillary Clinton in 2016 had the same expected number of votes in particular states.^{||} But if the objective is to assess whether Biden won legitimately, then it is beside the point whether Biden and Clinton enjoyed the same expected support. Support can differ across candidates for any number of reasons, and it is absurd to think that any such difference constitutes evidence of election fraud.

More specifically, Cicchetti treats the number of Democratic votes in an election as a binomially distributed random variable and tests the hypothesis that the expected number of Democratic votes (e.g., in Arizona) was the same for Joe Biden in 2020 as it was for Hillary Clinton in 2016. Let SupportShare_t denote the true probability that each voter votes Democratic in an election at time t, let Voters_t denote the total number of voters in that election, and let VoteShare_t denote the observed share of votes for the Democrat in that election. Then Cicchetti tests the null hypothesis that SupportShare_t × Voters_t = SupportShare_{t-1} × Voters_{t-1} using the test statistic

 $= \frac{\text{VoteShare}_t \text{Voters}_t - \text{VoteShare}_{t-1} \text{Voters}_{t-1}}{\sqrt{\frac{\text{VoteShare}_t (1 - \text{VoteShare}_t) \text{Voters}_t}{\sqrt{+\text{VoteShare}_{t-1} (1 - \text{VoteShare}_{t-1}) \text{Voters}_{t-1}}}}}$

[#]SI Appendix, Fig. 2 shows that the serial correlation in county-level election results has increased steadily to a new high in 2020.

He also tests the hypothesis that Biden's early and late vote counts were the same in specific states. This test is subject to the same critique, which we show in *SI Appendix*, section D.



Fig. 2. A plot shows Democratic vote margin in 2016 (horizontal axis) and 2020 (vertical axis) by county: Support in most counties did not shift much, and bellwethers (colored red) were no exception. *B* plot calculates the expected share of counties Biden won given the 2016 Democratic margin. Trump's margin in bellwether counties (red plus) was large and Biden won only a small share of those. We use several flexible models to calculate Biden's expected number of bellwether county wins if they behave like other counties and we find that Biden would be expected win between 1.24 and 1.75 bellwethers.

For example, Biden won 0.494 of 3.33 million votes in Arizona in 2020, while Clinton won 0.446 of 2.41 million votes in Arizona in 2016; this yields z = 477.09, for a *P* value very close to zero. Given that Biden won a substantially larger share of a much larger total, it should not be surprising that we soundly reject the null hypothesis that the two candidates had the same expected vote total. But it is preposterous to attribute that difference to fraud rather than the myriad innocuous differences between the two elections. It would be similarly preposterous to conclude that something was suspicious about TV ratings because fewer people watched the Super Bowl in 2020 than in 2016 (*z* statistic: 1,495) or to suspect foul play in COVID-19 vaccine trials because the number of infected participants differs between two trials using different vaccines on different numbers of participants.

To further highlight the absurdity of Cicchetti's test, we applied it to other years and states since 1960. Unsurprisingly, we nearly always reject the null hypothesis (1,488 state–year combinations of 1,498). By Cicchetti's logic, this suggests that fraud is commonplace across nearly all US states and elections. In fact, the test indicates simply that elections differ from each other, an unsurprising conclusion that tells us nothing about fraud.

Patterns of Straight-Ticket and Split-Ticket Voting in Michigan Not Anomalous. In a YouTube video with over 1 million views, Shiva Ayyadurai claimed to provide evidence that voting machines in Michigan decisively switched votes from Trump to Biden (13). The analysis compares Trump's share of straight-ticket votes and Trump's share of split-ticket votes across precincts in four Michigan counties. (Voters in Michigan can tick a single box to vote straight ticket for all candidates of one party or vote split ticket for individual candidates.) Ayyadurai argues that, if ballots were counted properly, the difference between those two proportions in a precinct should be unrelated to Trump's success among straight-ticket voters in that precinct. In the four counties he analyzes, Ayyadurai finds instead a negative linear relationship, which he interprets as evidence that Biden stole votes from Trump.

Ayyadurai's argument has been debunked by others, including two analysts who point out that the same logic would also imply that Trump stole votes from Biden in the same counties (14, 15). We show that the negative relationship Ayyadurai takes as evidence of fraud is an expected consequence of regression to the mean and that the same pattern should be found when fraud is absent.

Let X_i and Y_i denote Trump's share of straight-ticket votes and split-ticket votes in precinct *i*, respectively. Ayyadurai's observation is then that $Y_i - X_i$ is negatively related to X_i . Now, note that the slope coefficient from the regression of $Y_i - X_i$ on X_i is

$$\frac{\operatorname{Cov}(Y_i - X_i, X_i)}{\operatorname{Var}(X_i)} = \frac{\operatorname{Cov}(Y_i, X_i)}{\operatorname{Var}(X_i)} - \frac{\operatorname{Cov}(X_i, X_i)}{\operatorname{Var}(X_i)}$$
$$= \frac{\operatorname{Cov}(Y_i, X_i)}{\operatorname{Var}(X_i)} - 1,$$

which is the slope coefficient from the regression of Y_i on X_i minus 1. Thus the relationship Ayyadurai investigates will be negative if the slope coefficient from regressing Y_i (Trump's split-ticket share) on X_i (Trump's straight-ticket share) is less than 1. But regression to the mean implies that this should be the case: If split-ticket support for Trump and straight-ticket support for Trump are noisy measures of the same thing (support for Trump), then regressing one on the other will yield a coefficient less than 1, and the relationship Ayyadurai investigates should be characterized by a negative slope.^{**} Thus Ayyadurai has it backward: The flat relationship he says would characterize a valid election would be highly surprising, and the relationship he observes is what we would expect if two measures of Trump support were imperfectly correlated, as they typically would be.

This suggests that we should find Ayyadurai's negative relationship in other elections in which voters may vote straight ticket or split ticket and fraud is not suspected. Conveniently, in a follow-up video Ayyadurai points out that the 2008 presidential election in Alabama was just such an election (16). We therefore check the 2008 Alabama election returns for patterns like the one Ayyadurai observes in Michigan in 2020. As expected, many Alabama counties exhibit precisely the negative relationship in 2008 that Ayyadurai considers evidence of fraud in Michigan counties in 2020, as shown in Fig. 3. (Each dot is a precinct, scaled

^{**}To see this, suppose that underlying Trump support is given by T_i and that $Y_i = T_i + \epsilon_i$ and $X_i = T_i + \gamma_i$, where ϵ_i and γ_i are independent random draws from a distribution with mean zero and constant variance. Then $\operatorname{cov}(Y_i, X_i)/\operatorname{var}(X_i) = \operatorname{var}(T_i)/(\operatorname{var}(T_i) + \operatorname{var}(\gamma)))$ and $0 < \operatorname{var}(T_i)/(\operatorname{var}(T_i) + \operatorname{var}(\gamma)) < 1$.



Fig. 3. Several counties in Alabama in 2008 show the same relationship between split-ticket voting and straight-ticket voting that Ayyadurai interprets as evidence of fraud in Michigan in 2020.

by the number of votes cast in the precinct; the red line is the linear prediction.) This confirms that the relationship Ayyadurai highlights is a feature of normal elections and not proof of fraud. In *SI Appendix*, Fig. 3 we show that in 32 of 35 Alabama counties the slope coefficient from a regression of McCain's split-ticket share on his straight-ticket share is less than 1, and in 29 of those counties we reject the null that the slope is 1.

Claims Based on Facts That Are Not Actually Facts

Dominion Voting Machines Do Not Decrease Trump Vote Share. Trump's legal team claimed after the election that voting machines run by Dominion Voting Systems switched votes from Trump to Biden. Trump lawyers Rudy Giuliani and Sidney Powell argued for a global conspiracy that undermined democracy everywhere Dominion was present. In late December, an anonymous analysis was widely circulated on social media claiming to show that Biden outperformed expectations in counties that used Dominion voting machines (17). The right-wing news outlet The Epoch Times reported that the analysis showed Biden outperformed expectations in 78% of the counties that use Dominion or Hart voting machines and that the analysis "also indicates that Biden consistently received 5.6 percent more votes in those counties than he should have" (18). Assessing whether a particular set of voting machines caused Biden to receive more votes is difficult, because machines are not randomly assigned to counties (19). Further, in *SI Appendix*, section E we present analyses indicating that the original study was the result of P hacking and careless data analysis.

Given these problems with the original analysis, we carry out our own analysis to check for evidence that Dominion machines switched votes from Trump to Biden. In Table 1, column 1 we show the results of a bivariate regression of Biden's share in 2020 on an indicator for whether the county used a Dominion machine, finding a very slight and statistically insignificant difference. In Table 1, column 2 we adjust for Clinton's share of the vote in 2016, which strongly predicts the 2020 outcome (note the R^2 of 0.964); the Dominion coefficient becomes very slightly negative, although again it is not significant. In Table 1, column 3 we add a

Table 1. Dominion voting systems did not cause an increase in Biden votes

	-					
	1	2	3	4		
Dominion machines Clinton share of vote, 2016	0.007 (0.010)	-0.002 (0.002) 1.032 (0.004)	-0.009 (0.002) 1.029 (0.004)	-0.006 (0.003) 1.011 (0.004)		
Observations R ² Dummy for "Dominion state" State fixed effects	3,111 0.0002	3,111 0.964	3,111 0.965 √	3,111 0.975 √		

Data from all states and the coding of Dominion voting systems from the US Election Assistance Commission are used. SEs in parentheses.

dummy variable indicating whether the county is in a state where any Dominion machines were used and in Table 1, column 4 we add a fixed effect for each state; in both cases we find coefficients that are statistically significant in the negative (i.e., pro-Trump) direction, although very small in magnitude. In Table 1 we find the same null effect of Dominion voting machines persists regardless of how we classify a county as using Dominion machines, once we account for confounding at the state level and for county-level demographics. In short, using the most rigorous specifications we find no evidence that Biden outperformed expectations in counties where Dominion machines were used.

Absentee Ballot Counting Procedures Do Not Decrease Trump Vote Share. Another focus of the Trump team's accusations was the processing of absentee ballots in key states that Biden narrowly won. Among other claims, they alleged that Fulton County, GA, and Allegheny County, PA, were major centers of voter fraud in the 2020 election. Most of these allegations relied upon hearsay affidavits or debunked videos purportedly showing voters stuffing ballots. But in a paper posted in late December 2020, Lott (20) claims to provide statistical evidence that irregularities in the absentee vote counting procedure in Fulton County and Allegheny County suppressed votes for Trump and bolstered Biden's vote count. Lott examined precincts along the border of Fulton and Allegheny Counties and argued that he detected anomalous support for Biden in his absentee ballot share relative to his in-person share of ballots in Fulton and Allegheny Counties. Lott's paper received immediate and widespread attention. Peter Navarro, Assistant to the President and Director of the Office of Trade and Manufacturing Policy, touted the claim as solid evidence of fraud. President Trump tweeted out a link to the paper.

Lott's claims, however, do not withstand scrutiny. Using Lott's own data, we show in SI Appendix, section G that the specification he uses to analyze absentee voting patterns produces different conclusions depending on the entirely arbitrary order in which counties are entered in the dataset. Briefly, Lott posits that, if absentee ballots were correctly handled, the difference in Trump support across a boundary that separates a Democratic county from a Republican county should be similar to the difference in Trump support across a boundary that separates one Republican county from another. But Lott's conclusion depends entirely on the order in which the differences are computed for the Republican-Republican pairs. The conclusion is reversed when an alternative and equally justified order is used.

To achieve Lott's objective of comparing voting patterns across county boundaries, we reanalyze Lott's data using a more standard specification that does not suffer from these problems. We use the same pairs of precincts that Lott (20) used in his analysis to limit the confounding between precincts in different counties, but we now use a simple fixed-effects model that resolves the issue with Lott's (20) original specification. The regression equation for this model can be written as

Absentee_i = β_1 InPerson_i + δ SuspectCounty_i

$$+\sum_{k=1}^{K} \alpha_k I(\text{pair}_i = k) + \epsilon_i, \qquad [1]$$

where $Absentee_i$ and $InPerson_i$ denote Trump's share of the absentee and in-person vote (respectively) in precinct *i*; SuspectCounty, indicates whether precinct i is located in a "suspect" county (Fulton or Allegheny, depending on the state being analyzed); and each precinct is identified with one of Kprecinct pairs indexed by k, with α_k denoting the fixed effect for pair k. In the updated analysis, there is no significant difference in Trump's absentee support (conditional on his in-person support) across the key county boundaries, consistent with the null hypothesis that absentee ballots were handled correctly. We report the results of the fixed-effect analyses for Georgia and Pennsylvania in Table 2. In column 1, we regress Trump's share of the absentee vote on Trump's share of the in-person vote and a dummy for Fulton County; in column 2 we add precinct-pair fixed effects as in Eq. 1, essentially allowing the intercept to vary across Lott's precinct pairs. Neither specification shows a substantively or statistically significant difference between Trump's share of the absentee vote in Fulton County precincts and other precincts. The same is also true in Pennsylvania, as reported in Table 2.

Turnout Was Not Unusually High in Counties Where Republicans Made Fraud Accusations. Lott (20) also claims to show that 2020 turnout rates were higher than one would otherwise expect in a set of counties where Republicans have alleged that fraud took place. Lott argues that there was an "unexplained increase in voter turnout" ref. 20, p.13 in the key counties of between 1.26 and 2.42%, which Lott says is equivalent to 150,000 to 289,000 votes

Table 2. Examining Lott's (20) claims about Allegheny and Fulton Counties

	Dependent variable: Trump share absentee					
	Georgia		Pennsylvania			
	1	2	1	2		
Trump share, in person	0.760	0.606	0.511	0.307		
Suspect county	0.019 (0.019)	-0.003 (0.020)	0.003	0.003		
Observations	44	44	174	174		
Precinct-pair fixed effects		\checkmark		\checkmark		

A fixed-effects specification shows nothing suspicious in Fulton County, GA, and nothing suspicious in Allegheny County, PA. SEs in parentheses.

in those states. Lott concludes that this is evidence consistent with fraud

To determine whether the "suspicious" counties had higher turnout, Lott checks whether turnout in the 2020 election was higher than would be expected (given previous turnout, political leaning, and local demographics) in counties where, according to Republican lawsuits filed after the election, fraud may have taken place. Lott identifies 19 counties across six swing states where Republicans made fraud allegations.^{††} He then compares turnout in these counties to turnout in other counties in the same six states plus all counties in three other swing states (Florida, Ohio, and North Carolina). He argues that, if turnout is higher in these counties than would be expected given covariates, it would be evidence of fraud.

As we explain in SI Appendix, section H we dispute the premise of this analysis: Turnout varies across counties for many reasons, and it is unreasonable to ascribe a small unexplained difference to fraud. As it happens, Lott's finding is not robust to sensible departures from his chosen specification, so it is not necessary to dispute the premise.

Our analysis of county-level voting data for 2016 and 2020^{‡‡} indicates that Lott's conclusions are driven by the inclusion of states that have lower turnout increases and no suspicious countiesnamely Florida, North Carolina, and Ohio. Fig. 4A shows that, conditional on turnout in 2016, turnout in these three states was lower than turnout in the six states that contain a suspicious county in Lott's analysis. This is relevant because Lott's analysis compares changes in turnout in suspicious counties with changes in turnout in all other counties, so these smaller increases in turnout rates across states will be conflated with the suspicious county indicator in his analysis. The smaller the turnout increase in these three "nonsuspect" states, the more turnout in the suspect counties will appear to be suspiciously high, even if the changes in turnout in these suspect counties are unremarkable relative to the changes in turnout in other counties in their own state.

Fig. 4B shows that, once we address the level differences across states, Lott's (20) estimates of the turnout differences in suspicious counties go to zero and become insignificant. We examine POLITICAL SCIENCES

⁺⁺Lott identifies the following suspicious counties—in Georgia, Fulton and DeKalb; in Pennsylvania, Allegheny, Centre, Chester, Delaware, Montgomery, Northampton, and Philadelphia; in Arizona, Apache, Coconino, Maricopa, and Navajo; in Michigan, Wayne; in Nevada, Clark and Washoe; and in Wisconsin, Dane.

 $^{^{\}pm\pm}$ We use turnout rates for the county citizen voting-age population. For the number of voting-aged citizens we use the 5-y American Community Survey from 2019 and 2015. This follows best practice from McDonald (21). For total votes, we use Leip (22). We note that our estimates of turnout are lower than Lott's (20) average turnout rates, but closer to official statistics.



Fig. 4. No evidence suspicious counties had higher turnout. (A) Swing states without suspicious counties had smaller average turnout increases, which drives Lott's (20) results. (B) Lott's (20) estimates of suspicious county differences in turnout are zero and null once we address state-level differences.

all four of Lott's (20) models (organized on the vertical axis) and present the estimated coefficient on an indicator for "suspicious county" in a regression of 2020 turnout on that indicator plus 2016 turnout and covariates. The circle/purple estimates of suspicious county turnout depict the estimates using the four specifications for which Lott (20) presents results in his table 10. The triangle/dark-green estimates depict our estimates when we exclude Florida, Ohio, and North Carolina-three states in which no fraud was alleged. Across models, the difference in suspicious counties is close to zero and-in the case of model 4-the estimate is negative. The square/light-green estimates are from a model where we include all of Lott's states but add an indicator for a state that has suspicious counties. Again, this reduces the estimate to null. Finally, the last estimates (plus/lime green) include state-level fixed effects. Across models, this gives a close to zero and null difference for suspicious counties. Thus, simply by focusing only on states where at least one county had alleged fraud (i.e., swing states that Biden won) or allowing that state-wide turnout trends may differ across states or groups of states, we are able to explain what Lott (20) claimed was unexplained turnout in counties where Republicans had claimed fraud.

In short, there is no evidence that turnout was unusually high in the suspicious counties, let alone that turnout was inflated in these counties by fraud.

Statistical Analyses of Elections, the Detection of Fraud, and the Spread of Misinformation

Even though the 2020 election is over and Donald Trump's attempt to overturn the results failed, the effects of the claims will reverberate for years. A large segment of the public remains skeptical that Biden won the election legitimately and Republican state lawmakers are taking steps to alter voting access in the name of preventing fraud. The Trump campaign delivered a blueprint for losing candidates to undermine support for the

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winner or even steal the election. It seems unlikely that he will be the last to try these tactics.

We have closely examined what we consider the most prominent statistical claims of fraud in the 2020 election. Although the claims are diverse, our conclusion is consistent: For each claim, we find that what is purported to be an anomalous fact about the election result is either not a fact or not anomalous. In many cases the alleged fact, if shown to withstand scrutiny, would hardly constitute convincing evidence that Biden was elected due to fraud: A modest advantage to Biden in counties that chose to use Dominion machines, for example, could be explained by chance, by factors not accounted for in statistical models, or indeed by pro-Trump fraud undertaken using other voting machines. As it happens, the allegedly anomalous features we consider appear mundane once properly measured or placed in the appropriate context.

In some cases, members of the public who are confronted with a statistical claim of election fraud can apply the approach we took in this paper: First, ask whether the allegedly anomalous fact is a fact; if so, ask whether it is anomalous. In many cases, assessing the validity and unexpectedness of an allegedly anomalous fact requires some statistical sophistication and even original data analysis. For these cases, we think academics (and data journalists and others with appropriate skills) have an important role to play. To safeguard future election results, it will be essential to have elections experts ready to evaluate claims made about whether an election is free and fair. We think that social media organizations can do more to broadcast these evidencebased claims rather than merely flagging questionable assertions as disputed or asserting that the election was free and fair.

Rebuilding trust in American elections requires that we fairly evaluate claims about their failures and communicate those claims to a skeptical public. This paper is an effort in that direction.

Data Availability. Election results data have been deposited in Code Ocean at https://codeocean.com/capsule/0007435/tree/v2.

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Exhibit 5





Voters demonstrate outside the U.S. Supreme Court after the 2000 presidential election. "2000 woke up the world to election administration issues," says one elections policy expert. (Photo: Elvert Barnes via Wikipedia)

Then and Now: How 8 Election Policies Have Changed Since 2000

By Amanda Zoch | Feb. 16, 2021 | A State Legislatures Magazine

Besides being agonizingly close, the presidential contest of 2000 illuminated flaws in our election system and made election administration a priority issue in legislatures across the country.

"2000 woke up the world to election administration issues," says Ben Ginsberg, former co-chair of the Presidential Commission on Election Administration (PCEA) and GOP elections law expert extraordinaire.

In fact, the presidential elections of 2000 and last year put election administration front of mind for lawmakers and the public alike. But the issues at hand couldn't be more different.

As Bob Bauer, co-chair of the PCEA and equally distinguished expert from the Democratic side, explains, most partisan disagreement in the 2000 contest focused on how to resolve who won the presidential election, George W. Bush or Al Gore. In 2020, though, "the battle [is] right at the source— about how the rules are drawn, the role of courts in changing the rules, the role of state executive officials in implementing the rules. It's a very different fight."

Indeed, "fight" is an apt word. One of the biggest takeaways from our conversation with these election veterans is that election-related litigation has been on the rise since 2000. (We wrote about that in the September 2020 Canvass, and you can check out the case tracker here.)

Other notable changes? Election administration has become more professionalized, and voters have more options and access than ever before. "Democrats and Republicans were under pressure from the voters to modernize," Bauer says. "Americans are used to convenience."

And that change, more than any other, is borne out by the numbers. Between 2000 and 2020, voting options—all-mail voting, no-excuse absentee voting, early in-person voting—have become more widespread. Other policies, like voter registration, have adapted to the 21st century with the advent of online registration and the marked increase in same-day registration. Read on for the details of policy choices that have seen big changes in the last 20 years.

All-Mail Voting

2000: One state (Oregon)

2020: Five states (Colorado, Hawaii, Oregon, Utah and Washington)

In 1998, Oregon became the first state to enact all-mail elections when voters approved a citizen initiative requiring the state to mail ballots to all registered voters. The Beaver State implemented the law in 2000. By 2020, however, four more states joined Oregon in conducting elections mostly by mail. In 2012, Utah permitted jurisdictions to choose whether to conduct elections by mail, and all Utah counties did so by 2019. Washington implemented all-mail elections in 2012, Colorado in 2014, and Hawaii in 2020.

When the COVID-19 pandemic disrupted elections across the country, four states (California, Nevada, New Jersey and Vermont) and the District of Columbia chose to run elections entirely by mail—for 2020 only. Now that they've had a trial run, will any of those states make the shift permanent? It's too early in the 2021 legislative sessions to know but seeing five grow to six or seven isn't an impossibility this year.

For more information, see the "All-Mail Elections" section of NCSL's Voting Outside the Polling Place report.

No-Excuse Absentee Voting

2000: 22 states (Alaska, Arizona, California, Colorado, Hawaii, Idaho, Kansas, Maine, Montana, Nebraska, Nevada, New Mexico, North Carolina, North Dakota, Oklahoma, Oregon [all-mail elections], Tennessee, Utah, Vermont, Washington, Wisconsin and Wyoming)

2020: 34 states (Alaska, Arizona, California, Colorado [all-mail elections], Florida, Georgia, Hawaii [all-mail elections], Idaho, Illinois, Iowa, Kansas, Maine, Maryland, Michigan, Minnesota, Montana, Nebraska, Nevada, New Jersey, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Oregon [all-mail elections], Pennsylvania, Rhode Island, South Dakota, Utah [all-mail elections], Vermont, Virginia, Washington [all-mail elections], Wisconsin and Wyoming)

Since the creation of absentee voting during the Civil War, voters needed to supply an excuse—away from home, ill, etc.—to receive an absentee ballot. That changed in the 1980s, when California became the first state to allow voters to request an absentee ballot without a reason, and by 2000 nearly half of the states had followed suit and enacted no-excuse absentee voting.

Now, voters in over two-thirds of the states can request an absentee ballot without providing any reason at all. Virginia joined this group most recently, enacting no-excuse absentee voting in early 2020.

For more information, see the table of states with no-excuse absentee voting from NCSL's Voting Outside the Polling Place report.



Early In-Person Voting

2000: 22 states (Alaska, Arizona, Arkansas, California, Colorado, Hawaii, Iowa, Idaho, Illinois, Indiana, Kansas, Maine, Montana, Nebraska, Nevada, New Mexico, North Carolina, Oklahoma, Oregon, Tennessee, Texas and Vermont)

2020: 43 states (Alabama, Alaska, Arizona, Arkansas, California, Colorado [all-mail elections], Florida, Georgia, Hawaii [all-mail elections], Idaho, Illinois, Indiana, Iowa, Kansas, Louisiana, Maine, Maryland, Michigan, Massachusetts, Minnesota, Montana, Nebraska, Nevada, New Jersey, New Mexico, New York, North Carolina, North Dakota, Ohio, Oklahoma, Oregon [all-mail elections], Pennsylvania, Rhode Island, South Dakota, Tennessee, Texas, Utah [all-mail elections], Vermont, Virginia, Washington[all-mail elections], West Virginia, Wisconsin and Wyoming)

Early voting has been around since the very first presidential election, when, by necessity, elections were conducted over a season, rather than on a single Election Day. Texas, however, pioneered what we think of when we say "early voting"—in-person voting at a polling place during an established period of time prior to the designated Election Day. The Lone Star State implemented the voting option in 1991, and legislatures around the nation quickly followed its lead. By 2000, 22 states offered early in-person voting.

Interest in early voting has continued to grow, with 43 states having adopted early voting periods as of 2020. And that number will increase to 44 soon when Delaware's early voting law goes into effect in 2022.

For more information, see NCSL's Early Voting webpage.

Voter Identification Requirements

2000: 13 states (Alaska, Arizona, Connecticut, Delaware, Florida, Georgia, Hawaii, Kentucky, Louisiana, South Carolina, Tennessee, Texas and Virginia)

2020: 34 states (Alabama, Alaska, Arizona, Arkansas, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Missouri, Montana, New Hampshire, North Dakota, Ohio, Oklahoma, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Washington, West Virginia and Wisconsin)

Requiring voters to show some form of identification at the polls has become much more widespread since 2000. Then, 13 states had voter identification requirements—by 2020, the number had tripled.

NCSL categorizes voter identification requirements by type (photo and non-photo) and options for alternatives (strict and non-strict). When states first establish voter identification requirements, they often do so with a non-photo, non-strict law, though there is a trend toward strengthening those laws once they're in place. Of the 21 states that started with non-photo, non-strict requirements, eight have strengthened their laws. That number used to be nine, but in 2020 Virginia returned to a non-photo, non-strict requirement and became one of just a few states to eased their ID requirements. Through the last decade, courts have also stayed busy parsing whether voter identification laws do or do not burden voters unduly.

For more information, see NCSL's Voter Identification Requirements webpage.



Election Day (Same-Day) Voter Registration

2000: Six states (Idaho, Maine, Minnesota, New Hampshire, Wisconsin and Wyoming)

2020: 21 states (California, Colorado, Connecticut, Hawaii, Idaho, Illinois, Iowa, Maine, Maryland, Michigan, Minnesota, Montana, Nevada, New Hampshire, New Mexico, North Carolina, Utah, Vermont, Washington, Wisconsin and Wyoming)

In 2000, six states allowed any qualified individual to both register to vote and cast a ballot on the same day, which may be Election Day or a different day during the early voting period. Maine, Minnesota and Wisconsin were the first states to allow this practice, doing so in the 1970s. More states established same-day voter registration in the 1990s and 2000s (the 1993 National Voter Registration Act required states to offer voter registration at DMVs, unless the state had same-day registration), but it wasn't until the 2010s when the number shot up to nearly half of all states. Nevada and New Mexico are the most recent additions to this group—both enacted same-day voter registration in 2019, though the Land of Enchantment plans to implement the policy in stages over several years.

For more information, see NCSL's Same-Day Voter Registration webpage.



Online Voter Registration

2000: No states

2020: 40 states (Alabama, Alaska, Arizona, California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Missouri, Nebraska, Nevada, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, Tennessee, Utah, Vermont, Virginia, Washington, West Virginia and Wisconsin)

Online voter registration didn't exist in 2000, and the first state to implement a paperless voter registration option—Arizona—did so in 2002. Washington followed in 2008 and since then, online voter registration has essentially swept the nation, going from zero states to 40 in under two decades. In fact, most of the implementation has happened in just the past decade.

Most states have enacted specific legislation to authorize online voter registration, and New Jersey did so most recently in January 2020. Some states, however, have created online voter registration systems under existing authority, as North Carolina did in 2020. Some states may opt to pass legislation after the fact that specifically authorizes the system, as Minnesota did in 2014.

For more information, see NCSL's Online Voter Registration webpage.

Membership in the Electronic Registration Information Center (ERIC)

2000: No states (ERIC not yet established)

2020: 30 states (Alabama, Alaska, Arizona, Colorado, Connecticut, Delaware, Florida, Georgia, Illinois, Iowa, Kentucky, Louisiana, Maryland, Michigan, Minnesota, Missouri, Nevada, New Mexico, Ohio, Oregon, Pennsylvania, Rhode Island, South Carolina, Texas, Utah, Vermont, Virginia, Washington,

West Virginia and Wisconsin)

ERIC was founded in 2012 by seven states—Colorado, Delaware, Maryland, Nevada, Utah, Virginia and Washington—with the goal of modernizing voter registration records. This compact among states helps states improve the accuracy of their voter rolls by matching records against those of other states and expanding access to voter registration for all eligible citizens.

Texas was the most recent state to join, doing so in March 2020.

For more information, see NCSL's Voter List Accuracy webpage and the official ERIC website.

Risk-Limiting Audits

2000: No states

2020: Required in three states (Colorado, Rhode Island and Virginia)

While 38 states and the District of Columbia ask for postelection audits of some kind to test that vote tabulations are correct, a risk-limiting audit (RLA) is a new option that has received growing interest from election officials and legislators over the past few years. An RLA is an incremental audit system designed to limit the risk that a contest is certified with the wrong winner. The larger the margin of victory, the fewer ballots need to be reviewed—and vice versa.

Colorado was the first state to establish RLAs in statute in 2009, though it took until 2017 to work out the kinks and run its first RLA. Rhode Island and Virginia followed in 2017. Several states are testing the RLA waters now: Georgia, Indiana and Nevada have statutory pilot programs, with Nevada's becoming required statewide in 2022. Michigan and New Jersey have administrative pilot programs, and four states—California, Ohio, Oregon and Washington—make RLAs optional.

For more information, see NCSL's Risk-Limiting Audits webpage.

What Does the Future Hold?

There's no doubt that these shifts in election policies were enacted to help voters and increase election efficiency, accuracy and security. It's also true that many changes have been adopted with bipartisan support. Yet many believe that political parties push for the policies that conventional wisdom says help them and hinder their opponents.

But Ginsberg, the GOP elections expert, asked us to reconsider that logic in light of the 2020 election. "Republicans should take away that they actually can do well in high turnout elections," he says. "And Democrats should realize that they won states—at least on the presidential level—with some of the strictest voter ID laws... There should be a reevaluation by both parties and people looking at election laws to see how some of the dogma of both parties was refuted in this election."

Food for thought.

Amanda Zoch is an NCSL policy specialist and Mellon/ACLS Public Fellow.

Additional Resources

- "The American Voting Experience," Report and Recommendations of the Presidential Commission on Election Administration (2014)
- Subscribe to The Canvass, NCSL's Elections newsletter
- NCSL's Elections Legislation Database

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Exhibit 6

RESPONSE TO THE REPORT OF THE 2005 COMMISSION ON FEDERAL ELECTION REFORM

BRENNAN CENTER FOR JUSTICE AT NYU SCHOOL OF LAW WENDY R. WEISER JUSTIN LEVITT CATHERINE WEISS

and

SPENCER OVERTON, COMMISSIONER AND LAW PROFESSOR AT GEORGE WASHINGTON UNIVERSITY SCHOOL OF LAW



on behalf of

THE NATIONAL NETWORK ON STATE ELECTION REFORM

available at

www.carterbakerdissent.com www.brennancenter.org

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ABOUT THE AUTHORS AND THE NETWORK

The Brennan Center for Justice at New York University School of Law unites thinkers and advocates in pursuit of a vision of inclusive and effective democracy. The organization's mission is to develop and implement an innovative, nonpartisan agenda of scholarship, public education, and legal action in the areas of Democracy, Poverty, and Criminal Justice, promoting equality and human dignity while safeguarding fundamental freedoms. The Center's Democracy Program supports practical and administrable election reforms that foster full and equal political participation.

Authors Wendy R. Weiser, Justin Levitt, and Catherine Weiss are Associate Counsel at the Brennan Center.

Spencer Overton is a professor at The George Washington University Law School, and he served as a commissioner on the 2005 Commission on Federal Election Reform. Professor Overton specializes in voting rights and campaign finance law, and his academic articles on election law have appeared in several leading law journals. His book "Stealing Democracy: The New Politics of Voter Suppression," will be published and released by W.W. Norton in June 2006. Professor Overton formerly taught at the University of California, Davis and before that served as the Charles Hamilton Houston Fellow at Harvard Law School. He currently serves on the boards of Common Cause, the National Voting Rights Institute, and the Center for Responsive Politics.

The National Network on State Election Reform is a newly-formed collective of civil rights, voting rights, civic participation and legal organizations dedicated to advancing meaningful election reform. The foundation of the collaboration is the organizations' mutual commitment to ensuring that all eligible voters have the opportunity to cast meaningful ballots that are accurately counted. The National Network represents a diverse group of organizations with longstanding experience in analyzing and advocating for electoral practices that expand voter participation. The Network's members include the Brennan Center for Justice; Demos: a Network for Ideas and Action; the Lawyers' Committee for Civil Rights Under Law; the People for the American Way Foundation; and over a dozen other civil and voting rights organizations.

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On September 19, 2005, the Commission on Federal Election Reform, co-chaired by former President Jimmy Carter and former Secretary of State James Baker III, issued a report with recommendations for reforming the administration of U.S. elections.¹ Unfortunately, the Commission did so after only two limited hearings and no call for public comment. The Commission's final report betrays the cursory nature of its study, proceeding in places based on anecdote and supposition, rather than on rigorous analysis and empirical fact. As a result, although a number of its recommendations could improve our electoral system, several of its suggestions would be damaging and should not be included in any proposal for election reform.

Election reform should seek to ensure that every eligible American citizen has a meaningful opportunity to participate in a fair political process. If that opportunity is to be restricted, it must be absolutely clear that the benefits of such a restriction outweigh its costs. The sections of the Commission's report addressed in this paper depart from this fundamental standard.

While our election system is undeniably in need of substantial structural and administrative improvement, the burden of reform must not be borne by voters. The problems with American elections are *not* caused by American voters. They are caused by inadequate attention to election administration, insufficient resources, and unfair and unreasonable rules and procedures often designed and administered by elected or partisan individuals with an interest in the outcome of elections. Unfortunately, several sections of the Commission's report seem to shift the blame to regular Americans, and as a result, make recommendations that are likely to exclude a significant number of citizens from the political process—especially those who have traditionally been disadvantaged by restrictions at the polls.

Commissions can serve a vital public purpose in focusing the nation's attention on issues of national importance. Final action on those issues, however, deserves more careful study than was provided by the Carter-Baker Commission. The Commission's report, though helpful in some respects, should be viewed as no more than a contribution to the national conversation on election reform—and a call for further research, analysis, participation, and discussion on those issues. Nonetheless, because the Commission's report delivers specific recommendations on many pressing issues of election administration—describing itself as "a comprehensive proposal for modernizing our electoral system"²—it is necessary to confront directly several of its conclusions.

This paper addresses the main substantive flaws in the Report, refuting in detail its recommendations that "Real ID" cards be used for voter identification, that Social Security numbers be spread through interstate databases and on ID cards, and that states restore voting rights to people convicted of felony convictions only in certain cases and only after they have completed all the terms of their sentence.³ These recommendations are ill-advised and should not set the standard for election reform in the states.

Chapter I

THE IDENTIFICATION RECOMMENDATIONS (SECTION 2.5) WILL UNJUSTIFIABLY EXCLUDE MILLIONS OF LEGITIMATE AMERICAN VOTERS

The Report's most troubling recommendation is that states require voters to present a "Real ID" card or a similar "template" ID as a condition of voting. Recommendation 2.5.1 provides:

To ensure that persons presenting themselves at the polling place are the ones on the registration list, the Commission recommends that states require voters to use the REAL ID card, which was mandated in a law signed by the President in May 2005. The card includes a person's full legal name, date of birth, signature (captured as a digital image), a photograph, and the person's Social Security number. This card should be modestly adapted for voting purposes to indicate on the front or back whether the individual is a U.S. citizen. States should provide an EAC-template ID with a photo to non-drivers free of charge.⁴

This recommendation is more onerous than the photo ID proposal rejected by the Commission's predecessor in 2001 and is more restrictive than any ID requirement adopted in any state to date.⁵ It would impose substantial—and for some, insurmountable—burdens on the right to vote.

Unfortunately, the Report fails to undertake a serious cost-benefit analysis of the advantages that would supposedly be realized by a "Real ID" requirement and the harms it will produce. This ID requirement is purportedly intended to prevent "voter fraud," and yet the Report itself concedes that "[t]here is no evidence of extensive fraud in U.S. elections or of multiple voting" before asserting, without any meaningful support, that "both occur."⁶ As discussed at length below, the forms of fraud that could be prevented by voter ID are exceedingly rare and risky. In contrast, compelling evidence shows that the "Real ID" proposal will disenfranchise countless eligible voters. Rather than analyzing the empirical data to assess whether its recommendations are sensible, however, Section 2.5 of the Report begins and ends with anecdote and supposition. The lack of rigor exhibited in the Report on this politically controversial issue undermines its credibility and appearance of objectivity. And while it might be true that in a close election "a small amount of fraud could make the margin of difference,"⁷ it is equally true that the rejection of a much larger number of eligible voters could make a much bigger difference in the outcome. In the end, the exclusion of voters through restrictive ID requirements will erroneously determine the outcome of many more elections than any speculative fraud by individual voters at the polls.

Not only does the Report fail to justify the creation of stringent identification requirements, but it also does not explain why the goals of improved election integrity will not be met through the existing provisions in the Help America Vote Act of 2002 (HAVA),⁸ which have only recently been implemented in the states, and the effects of which have not yet been

fully analyzed. Nor does the Report consider alternative measures to advance its goals that are less restrictive to voters.

For the reasons discussed below, it is apparent that the Commission has not adequately examined the real impact of its ID recommendations. The costs of those recommendations far outweigh any benefits they may achieve.

A. THE PROPOSED ID REQUIREMENTS WILL SEVERELY BURDEN VOTERS

The Commission's recommendation that eligible citizens be barred from voting unless they are able to present a souped-up "Real ID" card is a proposal guaranteed to disenfranchise a substantial number of eligible voters.

Millions of Americans currently do not have driver's licenses or government-issued photo ID cards. Millions more may never get the new "Real ID" card, which requires substantially *more* cost and effort. The Report's proposal to use "Real ID" as a condition of voting is so excessive that it would prevent eligible voters from proving their identity with even a valid U.S. passport or a U.S. military photo ID card. While Americans of all backgrounds would be excluded by the Report's ID proposal, the burden would fall disproportionately on the elderly, the disabled, students, the poor, and people of color.

The exclusionary effects of the Commission's ID proposal are most vividly illustrated by some of the people it is most likely to disenfranchise—the victims of Hurricane Katrina. Many who were left behind in hurricane-torn New Orleans are poor and did not have access to a car, and thus are among those least likely to have a driver's license. The hundreds of thousands of displaced citizens will find it difficult, if not impossible, to secure the identity papers they left behind or to obtain new records from government offices and hospitals that have been destroyed. These forgotten Americans—and many like them across our nation—are the ones the Commission's ID proposal will leave out of our democracy.

1. Many Americans Do Not And Will Not Have The Requisite State-Issued Photo ID

As the Report estimates, twelve percent of voting-age Americans do not have driver's licenses.⁹ The research collected by the 2001 National Commission on Federal Election Reform shows that between six and ten percent of voting-age Americans do not have driver's licenses or state-issued non-driver's photo ID.¹⁰ That translates into as many as 20 million eligible voters.¹¹

The Commission's recommendation is even more restrictive than other photo ID standards. Under the Real ID Act, as of 2008, a state may not issue a driver's license or non-driver's ID card unless the individual presents *documentary proof* of: (a) her full legal name and date of birth, (b) her Social Security number (or the fact that she is not eligible for one), (c) the address of her principal residence, and (d) her citizenship.¹²

Although there are no studies showing how many Americans lack readily available proof of citizenship, Arizona's recent experience under the state's Proposition 200 (which requires

proof of citizenship in order to register to vote) suggests that the number is extremely high. For instance, one county reported in February 2004 that it was forced to reject nearly 75% of new voter registration forms for failure to provide adequate proof of citizenship.¹³ The percentage of Americans without the documentary proof of citizenship necessary to obtain "Real IDs" is likely to remain high because, as discussed below, the requisite documents are both expensive and burdensome to obtain.

The percentage of citizens that do not have, and will not obtain, the enhanced state-issued photo identification cards is even greater for the elderly, students, people with disabilities, urban residents, low-income individuals, and people of color. According to the Georgia chapter of the AARP, 36 percent of Georgians over age 75 do not have a driver's license.¹⁴ In Wisconsin, approximately 23 percent of persons aged 65 and older do not have driver's licenses or photo ID, and fewer than 3 percent of students have driver's licenses listing their current address.¹⁵ Across the country, more than 3 million Americans with disabilities do not have a driver's license or other form of state-issued photo ID.¹⁶ In the sections that follow, this paper examines the expense of IDs to low-income voters,¹⁷ and documents the enormous racial disparities in access to state-issued photo ID.¹⁸

Moreover, given the frequency with which Americans move residences,¹⁹ it is likely that a far greater percentage of citizens lack driver's licenses or photo IDs bearing their current addresses.²⁰ Since voting generally depends on the voter's address, and since many states will not accept IDs that do not bear an individual's current voting address, an additional 41.5 million Americans each year²¹ will have ID that they may not be able to use to vote.²²

The Report's "Real ID" proposal will only exacerbate these existing disparities between communities with the requisite identification and those without; once the Real ID Act has been implemented, those who have traditionally had difficulty obtaining state-issued photo identification will find that the difficulty has significantly increased.

2. The ID Recommendations Will Operate as a Poll Tax Because "Real IDs" Are Expensive and Difficult to Obtain

As the Report recognizes, government-issued photo identification costs money. Thus, if required as a precondition for voting, photo identification would operate as a *de facto* poll tax that could disenfranchise low-income voters. To alleviate this burden, the Report appropriately recommends that the "Real ID" card itself be issued free of charge. This safeguard, however, does not address some of the most significant predicate costs in obtaining photo identification— costs incurred whether or not the card itself is free.

First, each of the documents that an individual is required to show in order to obtain a "Real ID" card or other government-issued photo ID card costs money or presumes a minimal level of economic resources. A certified copy of a birth certificate costs from \$10.00 to \$45.00, depending on the state; a passport costs \$85.00; and certified naturalization papers cost \$19.95. Unless the federal and all state governments waive the cost of each of these other forms of identification, the indirect costs of photo IDs will be even greater than their direct costs.

In addition, since government-issued IDs may only be obtained at specified government offices, which may be far from voters' residences and workplaces, individuals seeking such IDs will have to incur transportation costs and the costs of taking time off from work to visit those offices during often-abbreviated business hours. These are not insignificant burdens. For example, as the Report notes, there are only 56 locations in the state of Georgia that issue IDs for residents of all the state's 159 counties.²³ Of the ten Georgia counties with the highest percentage of minority residents, only one has an office where driver's licenses and other photo IDs are available.²⁴ In fact, there is no office that issues driver's licenses and non-drivers' IDs in the city of Atlanta.²⁵ Moreover, although most states prohibit employers from penalizing employees for taking time off to vote, no state has similar protections for individuals taking time off to obtain government-issued identification. These costs must also be considered in conjunction with the significant burden the identification requirements will impose on voters' time.²⁶

In short, the Report's "Real ID" proposal would introduce substantial additional costs to voting; these naturally fall most harshly on low-income voters. As the earlier Commission's Task Force on the Federal Election System found in its August 2001 report, a photo ID requirement would "impose an additional expense on the exercise of the franchise, a burden that would fall disproportionately on people who are poorer and urban."²⁷

3. The "Real ID" Recommendations Will Disproportionately Burden People of Color

Strong empirical evidence also shows that photo ID requirements disproportionately burden people of color.

In 1994, the U.S. Department of Justice found that African Americans in Louisiana were 4 to 5 times less likely to have government-sanctioned photo ID than white residents. As a result, the Department denied pre-clearance for that state's proposed photo ID requirement because it "would lead to retrogression in the position of racial minorities with respect to their effective exercise of the electoral franchise."²⁸ Similarly, a June 2005 Wisconsin study found that the rate of driver's license possession among African Americans was half that for whites.²⁹ The disparity increased among younger drivers, where white adults aged 18-24 were three times as likely as their black peers to possess a driver's license. Only 22% of black males in that age group had a driver's license.³⁰

The lack of government-issued photo ID is also particularly acute among Native Americans, many of whom have religious objections to such ID. Reports of the 2004 primary in South Dakota showed that voters in the predominantly Native American counties of Shannon, Todd, Corson, Dewey and Zieback were 2 to 8 times more likely to not bring IDs to the polls than other voters in the state.³¹

In addition, the ID recommendations reduce the benefits of voter registration at disability and other social service agencies provided by the National Voter Registration Act of 1993.³² Individuals who seek to register at those offices—which generally do not issue IDs—will also have to make an additional visit to the motor vehicle department in order to obtain the

documentation necessary to vote. Census data demonstrate that African Americans and Latinos are more than three times more likely than whites to register to vote at a public assistance agency, and that whites are more likely than African Americans and Latinos to register when seeking a driver's license.³³ Accordingly, the voter registration procedure far more likely to be used by minorities than by whites will no longer provide Americans with full eligibility to vote.

Not only are minority voters less likely to possess the requisite ID, but they are also more likely than white voters to be asked to furnish ID at the polls. As the Task Force Report of the prior Commission found, identification requirements create the opportunity for selective enforcement—either innocuous or invidious—when poll workers request photo ID only from voters unknown to them.³⁴ This discretion has often led to special scrutiny of minority voters at the polls. In New York City, for example, which has no photo ID requirement, a study showed that poll workers illegally asked one in six Asian Americans for ID at the polls, while white voters were permitted to vote without showing ID.³⁵ There is little reason to think that universal ID requirements would not be similarly undermined by exemptions for white voters who arrive at the polls without ID.

Even in the extremely unlikely event that the discriminatory application of identification requirements will disappear in the future, its history cannot be ignored. Significant populations of minority voters justifiably *believe* that identification requirements will be used to harass them, notwithstanding the general call for new and untested ombudsmen institutions as a stopgap.³⁶ This may further discourage voter participation among those that have traditionally faced barriers to the franchise. Although the Report is quick to cite the perception of fraud as a basis for recommending ID, it fails to acknowledge the perception that ID requirements will be unjustly applied as a valid reason for second thoughts. The latter perception just as surely "undermines confidence in the system" among populations that have previously been subjected to discriminatory application of ID requirements.

In part because of the disparate impact that a photo identification requirement would have on minority voters, Congress rejected such a requirement in HAVA,³⁷ opting instead for a more expansive list of identification documents (such as a current utility bill, bank statement, paycheck, government check, or other government document), for a smaller category of voters (first-time voters who registered by mail), coupled with fail-safe provisional voting for those voters who cannot meet HAVA's less stringent identification requirements.³⁸ The facts giving rise to Congress's concerns have not changed. Indeed, no new evidence provides any basis for challenging Congress's conclusion—and the conclusion of this Commission's predecessor—that photo identification requirements are ill-advised.

4. The Report's Efforts to Mitigate the Exclusionary Effects of Its "Real ID" Proposal Fall Short

Faced with overwhelming evidence that "Real IDs" are both costly and difficult to obtain, the Report suggests that "Real ID" cards be made "easily available and issued free of charge."³⁹ While this is a laudable goal, the evidence suggests that it will not be attained. First, no state currently issues photo IDs free of charge to all voters.⁴⁰ And even if the card itself were free, the

"Real ID" would not be "free of charge" unless all documents required to obtain the "Real ID" were also "free of charge."

In addition, no state makes photo IDs "easily available" to all its citizens. As discussed above, photo IDs are issued by driver's license bureaus, which are located far from the residences and work places of many state residents. The Report suggestion that states use mobile offices to issue driver's licenses.⁴¹ Such a program would not solve the problem. Despite the fact that Michigan has a mobile ID program that the Report praises, at least eight percent of voting-age citizens in Michigan are still without driver's licenses and non-driver's photo IDs.⁴² Moreover, the implementation in Michigan is the result of a relatively robust "mobile office" program. Far more likely in cash-strapped states is a "program" like the one recently implemented in Georgia: one bus, traveling to one location for a day or two at a time, available from 9 a.m. until 3 p.m., during the heart of the work day.⁴³ A spokesperson for Georgia Governor Sonny Perdue aptly described some of the barriers to implementing an effective mobile ID program. Discussing the state's plan to use a hand-me-down bus from another agency, Heather Hendrick said: "We've got to start with the resources we've got and can't spend money we don't have."⁴⁴

Far too many American citizens already suffer for their lack of government-issued photo IDs. Ensuring that those citizens have access to free IDs is an important goal that should be pursued by every state. But the solution is not to pile another hardship on those citizens – the denial of their right to vote – especially when it has not yet been shown that states can meaningfully reduce the number of citizens without photo IDs.

It is also troubling that the Report fails to include in its recommendations an effective "safety net" for eligible voters who do not have or are unable to obtain "Real IDs" and proof of citizenship, who have had their cards lost or stolen, or who have simply forgotten to bring their IDs to the polls and are unlikely to track down an election official within the Report's 48-hour deadline. Virtually all states that require identification as a condition of voting have some alternative option for voters who lack identification, such as an option to show a utility bill or to sign a sworn affidavit containing information that can later be verified by election officials.⁴⁵ Even the recent and controversial Florida voter ID law was pre-cleared by the Department of Justice with such a safety net: the law permits voters who cannot meet the ID requirements to sign an affidavit on the envelope of a provisional ballot, which will be counted if the signature matched that on the voter's registration form.⁴⁶ The Report's attempt to support its extreme proposal by reference to the minority of states that require some form of identification for voting – without even mentioning that few of these states make identification an absolute condition of voting – is misleading.⁴⁷

Although the Report recognizes this problem, the solution it proposes—a signature match option only until January 1, 2010⁴⁸—is woefully inadequate. Since the Real ID Act goes into effect in 2008, this recommendation will provide a safety net only for *one* federal election.⁴⁹ There is no evidence that the states will ever correct the differential access to Real IDs, let alone in only two years. More important, there is no valid reason why the signature-match failsafe should ever be discarded.

After the brief two-year window, the Report recommends that a voter who does not furnish ID at the polls may cast a provisional ballot that can be counted if the voter returns "to the appropriate election office within 48 hours with a valid photo ID."⁵⁰ A voter who does not have "Real ID" will find no comfort in a two-day extension, which is not sufficient time to obtain ID and which will not alleviate the costs of the ID. Even those voters who have but forgot to bring their Real IDs to the polls will face the difficulty of determining "the appropriate election office" to which to bring their IDs.

The real empirical data show that a substantial percentage of Americans will not be able to meet the Commission's proposed ID requirements. The defect is no simple matter to overcome. No state in the union has yet succeeded in ensuring that all (or even almost all) of its voting-age citizens possess government-issued photo identification or proof of citizenship. Until most states demonstrate that they have successfully undertaken such steps, it is premature to consider such identification as a prerequisite to voting.

B. THE LIMITED TYPES OF FRAUD THAT COULD BE PREVENTED BY "REAL ID" REQUIREMENTS ARE EXTREMELY RARE

The Report premises its burdensome identification proposals on the need to ensure ballot integrity and on the existence of or potential for widespread fraud. There is no question that fraud and misconduct—such as purges of eligible voters from voter rolls, distribution of false information about when and where to vote, and even occasional stuffing of ballot boxes or tampering with registration forms—persist in American elections. But as the Report admits, there is simply "no evidence" that the type of fraud that could be solved by stricter voter identification—individual voters who misrepresent their identity at the polls—is a widespread problem.⁵¹ Indeed, the evidence that does exist shows that this sort of fraud occurs only at an extremely low rate.

The Commission's recommended photo identification requirements do not prevent fraud by absentee voting. Nor do they prevent voting by ineligible persons with felony convictions who are misinformed of their voting rights. They do not prevent unsubstantiated purges or stuffing of ballot boxes by election officials. Rather, the Report's photo ID proposal guards against only one type of fraud: individuals arriving at the polls to vote using false information, such as the name of another registered voter, or a recent but not current address. These are extraordinarily inefficient means to influence the results of an election. Since the costs of this form of fraud are extremely high (federal law provides for up to five years' imprisonment⁵²), and the benefits to any individual voter are extremely low, it is highly unlikely that this will ever occur with any frequency.

The barriers to fraud by individual voters at the polls have rendered such fraud a statistical anomaly in practice. The limited types of fraud that could be prevented by a "Real ID" requirement are, in fact, extremely rare. The Report concedes that "the evidence of multiple voting is thin"⁵³ and cites no meaningful evidence of identity misrepresentation at the polls. Independent research confirms the fact that the hypothetical specter of fraud raised in the Report is without basis.

In the most comprehensive survey of alleged election fraud to date, Professor Loraine Minnite and David Callahan have shown that the incidence of individual voter fraud at the polls is negligible.⁵⁴ A few prominent examples support their findings. In Ohio, a statewide survey found four instances of ineligible persons voting or attempting to vote in 2002 and 2004, out of 9,078,728 votes cast—a rate of 0.00004%.⁵⁵ Earlier this year, Georgia Secretary of State Cathy Cox stated that she could not recall one documented case of voter fraud relating to the impersonation of a registered voter at the polls during her ten-year tenure as Secretary of State or Assistant Secretary of State.⁵⁶ A similar finding prompted the Michigan Attorney General to find that the state's proposed identification requirement would violate the U.S. Constitution by unduly burdening the right to vote without a compelling state interest.⁵⁷

The Report attempts to support its burdensome identification requirements on four specific examples of purported fraud or potential fraud. *None* of the Report's cited examples of fraud stand up under closer scrutiny. Because similar examples have been used in the past to invoke the need for photo identification requirements, it is worthwhile to address each cited example in turn.

The Report first cites voting by ineligible persons with felony convictions and votes cast in the names of the dead in Washington State in 2004.⁵⁸ Photo identification requirements, of course, do not solve the first problem; they merely prevent a person from fabricating his name or address, and have absolutely no impact on an ineligible person arriving at the polls to vote under his own name. Moreover, both circumstances are addressed by HAVA provisions that had not yet been implemented in Washington (or, for that manner, in most states) in 2004: HAVA requires regular cleaning of the registration lists to remove persons rendered ineligible by felony conviction or death.⁵⁹ Once HAVA's provisions are implemented, persons who have been rendered ineligible by a felony conviction or death will simply not be listed on the voter rolls as eligible voters. Thus, if such persons—or others purporting to be them—show up at the polls, they will not be able to cast a regular ballot. Finally and most importantly, further investigation in Washington State—one of the most substantial investigations into voter fraud in recent history—uncovered only six cases of alleged double voting and 19 cases of alleged voting in the name of deceased individuals (several by recently deceased family members), out of a total 2,812,675 ballots cast.⁶⁰ The rate of ineligible voting that could possibly have been remedied by identification requirements was 0.0009%.

The Report also cites a Milwaukee investigation into alleged voting by ineligible persons with felony convictions, votes cast in the name of the dead, double-voting, and voting in another's name.⁶¹ The Report, however, cites only the investigation's preliminary findings. Further investigation has *completely* cleared the first nine cases to be resolved, attributing the suspected irregularities to clerical errors, mismatches, and computer glitches.⁶² There are, thus far, *no* proven cases of fraud in Milwaukee that might have been remedied by identification requirements.

The Report next cites the general potential for fraud from inactive or ineligible voters left on voter registration lists.⁶³ As noted above, this is precisely what HAVA's database-cleansing requirements were specifically intended to solve. The Report in no way suggests that these requirements have failed or will fail to address the issue. Finally, the Report cites the conviction of 52 individuals since October 2002 for federal crimes relating to election fraud and ineligibility, including vote buying, submitting false voter registration information, and "voting-related offenses" by non-citizens.⁶⁴ Vote buying cannot be addressed by an identification requirement, as it does not involve misrepresentation of the voter's identity. And the Report fails to examine the records of any of the other crimes to determine whether any of them could have been prevented by mandating photo ID. But even if every single such crime could have been deterred by photo identification, the overall context is critical; during the same period in which these 52 individuals voted illegally (or procured an illegal vote), 196,139,871 ballots have been cast in federal elections—yielding a proven fraud rate of 0.00003%.⁶⁵ Statistically, Americans are more likely to be killed by a bolt of lightning.⁶⁶

Thus, even those examples cited by the Report show that individual election fraud of the sort deterred by photo identification requirements at the polls is extremely rare. In contrast, there is hard evidence that such requirements will unduly burden millions of eligible voters who currently do not have photo ID and for whom restrictive photo ID will be difficult to obtain. As discussed above, more than ten percent of eligible Americans will likely face difficulty in obtaining ID conforming to the Commission's recommendations. And these individuals will also disproportionately be members of groups that have traditionally faced barriers to voting: the poor, the elderly, the disabled, students, the transient, and people of color.

Perhaps recognizing the weakness of the fraud justification, the Report deploys a crafty rhetorical device to attempt to shift the rationale for the ID requirement. The Report states: "Photo IDs currently are needed to board a plane, enter federal buildings, and cash a check. Voting is equally important."⁶⁷ There is no question, of course, that voting is important. The importance of the act, however, has absolutely nothing to do with an ID requirement. At least since "Publius" and "Brutus" publicly debated the wisdom of the Constitution, American citizens have been engaging in activities critically "important" to personal and civic life without needing to provide documentary proof of their identity. Identification requirements for activities like boarding a plane, entering a federal building, and cashing a check have been imposed only to the extent that they are necessary and proportional responses to a real and empirically demonstrated security threat. As shown in this report, the sort of fraud remedied by identification requirements rests on no such foundation. Moreover, a burden on a privilege like boarding a plane is not nearly as troubling as a burden on the exercise of a right so fundamental as voting. Indeed, voting differs from air travel, check-cashing, and entering federal buildings. Airlines, for example, have no incentives to exclude legitimate travelers, while some politicians have incentives to exclude legitimate voters who are likely to cast ballots for their opponents (as we see in the redistricting context). The very purpose of voting is to ascertain the will of the people, and the Report's exclusionary ID requirement would do much more to thwart that goal than to advance it.

The Report's effort to justify its ID proposal by a need for national uniformity is similarly unavailing. There is no reason to believe that statewide differences in identification requirements are any more discriminatory or problematic than any other election procedures and requirements that vary from state to state, such as voter registration requirements or mail-in voting availability. The Report does not recommend that we abandon state control over election procedures and federalize all aspects of election administration, possibly because the Commission recognizes the benefits of state experimentation in expanding access to the franchise. While national uniformity at times is desirable, uniformity does not always promote fairness. A rule that uniformly excludes certain classes of voters is not an improvement over disparate state rules that are more protective of the franchise.

C. THE COMMISSION UNJUSTIFIABLY APPLIES A DOUBLE STANDARD TO ABSENTEE VOTERS

The Report's lack of attention to the empirical impact of its recommendations—and the shoddy logic of the enhanced photo identification requirement—is shown most clearly in the Report's differential treatment of absentee ballots and ballots voted in-person at the polls.

The Report provides no reason to create greater hurdles for voters who vote at the polls than for those who vote absentee. Yet despite the fact that absentee ballots are *more* susceptible to fraud than regular ballots,⁶⁸ the Report exempts absentee voters from its proposed "Real ID" and proof of citizenship requirements. The Report does not propose that state officials go out to collect ballots and check the photo IDs of absentee voters, nor does it recommend that absentee ballots (or ballots in a mail-in state like Oregon) be certified by a notary public who has checked the photo ID of the absentee voter. Instead, the Report permits absentee voters to be identified by matching the voter's signature on the absentee voter must produce only his signature; an individual voting in person must submit photo identification. The Report fails to explain why Americans who travel to the polls to vote should be denied the same opportunity to establish their identity through signature verification.

This double standard is especially disturbing in light of data, examined by the Commission's predecessor, that white voters are about twice as likely as black voters to cast an absentee ballot.⁶⁹

D. THE SPECULATIVE PROBLEMS CAN BE ADDRESSED BY LESS BURDENSOME ALTERNATIVES

As shown above, the identification proposal is in fact an unwarranted "solution" in search of a problem. It will not correct or deter any practice widely manifest in American elections, or even any practice with the realistic potential to corrupt an election. It will not prevent misconduct using absentee ballots. It will not prevent voting by ineligible persons with felony convictions who are misinformed of their voting rights. It will not prevent the intentional dissemination of misinformation about polling times, places, and procedures. It will not prevent unsubstantiated purges of eligible voters. It will not prevent stuffing of ballot boxes by election officials. It *will*, however, burden a substantial segment of the eligible voting population.

Individual voter fraud at the polls is largely a problem of perception, and perception alone. The appropriate—and proportional—remedy is education, not barriers to the ballot.

To the extent that any limited fraud by individuals at the polls does trickle into the system, it can be addressed by far less restrictive alternatives. The first step is to recognize that only voters who appear on the registration list may vote a regular ballot. Proper cleaning of registration lists—and proper use of the lists at the polls—will therefore go a long way toward ensuring that every single ballot is cast by an eligible voter.

Existing law has *already* accounted for this need—with proper safeguards for individual voters—and needs only adequate implementation. If inflated rolls create the specter of potential fraud, for example, the problem will be addressed by proper execution of the registration list-related provisions of NVRA and HAVA, which are designed in part to remove ineligible voters from the rolls. In addition to the better registration lists that full implementation will provide, better recordkeeping and administration at the polls will reduce the limited potential for voting by ineligible persons.

In the unlikely event that implementation of current law is not able to wipe out whatever potential for individual fraud remains, there are several effective and less burdensome alternatives to the Report's "Real ID" recommendation that received wholly insufficient consideration. As discussed above, one less restrictive alternative was even recognized in the Report in a different context: verifying identity by matching the voter's signature on the absentee ballot envelope with a digitized version maintained by election officials. Other proposals that have been advanced by election law scholars such as Edward Foley and Rick Hasen expressly condition identification requirements on a substantial affirmative government effort to reach out to underserved populations, and make accommodations for voters who do not bring a photo ID to the polls to cast a vote that will be counted.⁷⁰

The Report's failure to consider these and other less restrictive alternatives for preventing the negligible problem of individual voter fraud further calls into question the legitimacy of its conclusions.

E. THE REPORT DRAMATICALLY UNDERESTIMATES THE FINANCIAL COST OF ITS "REAL ID" PROPOSAL

The Report dramatically – and dangerously – underestimates the cost of its identification recommendations. As an attempt to compensate for the burden that strict ID requirements tend to place on traditionally marginalized groups, the Report recommends that government-issued photo identification be made "available without expense to any citizen" and that government efforts be made "to ensure that all voters are provided convenient opportunities to obtain" the ID in question.⁷¹ More specifically, the Report recommends that the government affirmatively deploy "mobile offices," to reach out to individuals who do not currently have the ID that they will need, and establish new "ombudsman institutions" to address concerns regarding abuse or mismanagement of the ID card system.⁷²

It is quite costly to fully implement all of these mechanisms, which are admittedly necessary to ensure that the government affirmatively provides ID to those who find it difficult to acquire the ID on their own, and that the ID requirement is deployed with minimal discrimination or misuse. In addition to the production and delivery cost associated with the

turbo-charged photo ID card itself, the government will have to provide for increased staff and staff training, for a variety of mobile offices and the new and underarticulated ombudsman institutions, as well as for each existing registrar's office.

These costs far exceed the costs delineated in the Commission's report. The report estimates the cost of its identification card proposal at \$115 million, at \$5 per card, and states that this \$5 estimate includes approximate administrative, infrastructure, and issuance costs.⁷³ However, this lowball estimate is belied by the very sources cited in the report itself. In 1997 testimony before the U.S. House of Representatives, Stephen Moore of the Cato Institute predicted that mass production of smart ID cards could cost \$5.00 per person, but included none of the administrative or infrastructure costs—much less the cost of "mobile offices," ombudsmen, staff, and training—in this estimate.⁷⁴ Five years later, Tova Wang of the Century Foundation cited several sources stating that smart cards would cost *at least* \$5-\$8 per person for the card itself, without any consideration of administrative costs, as well as others estimating the cost at \$10-\$35 per person.⁷⁵

Furthermore, real-world experience shows that the total costs of a new ID system will far exceed initial estimates. Even three years before its effective date, states are already encountering problems with implementing the Real ID Act—which is more limited than the Commission's affirmative proposal to provide enhanced ID to those who find it difficult to acquire on their own. For example, the National Conference of State Legislatures estimated that the actual cost for implementing the Real ID Act would be between *nine and thirteen billion dollars*,⁷⁶ a stark contrast to the Congressional Budge Office's original estimate of only \$120 million.⁷⁷ Indeed, some states struggling with the Real ID Act have discovered that their initial start-up costs exceed the figure initially projected as the total cost *nationwide*.⁷⁸

Similarly, the real costs of the Commission's proposal will be much higher than the figures provided by the Commission. At a recent National Conference of State Legislatures conference, cost estimates for a non-driver's ID card of the sort recommended here were 7-10 times larger than the amount listed in the Commission's report.

In this circumstance, the cost estimate is not merely artificially low, but also extremely dangerous. A high price tag is of minor concern if the government is willing to provide the necessary funding. But a low price tag risks consequently meager appropriations. Underfunding of implemented ID programs would seriously compromise any limited merit—and legality—of the Commission's recommendation. If sufficient money is not appropriated to ensure that the government affirmatively provides ID to those who find it difficult to acquire, this Commission's recommendation will create two stark categories of citizens: those who have the means to procure ID and may vote; and those who do not, and are barred from voting solely by virtue of their limited means.

This is not merely speculation. States with existing strict ID requirements are already reluctant to ensure adequate access for poor and rural voters to the necessary ID. Georgia, for example, recently passed the strictest photo identification law in the country, despite the fact that there are only 56 offices in 159 counties where this ID can be acquired, that only one of the ten counties with the highest percentage of African-American residents has an office where this ID
can be acquired, and that no location where this ID can be acquired is within the Atlanta city limits.⁷⁹ To remedy the disparate impact that this new law will have on minority voters and those of limited means, Georgia has announced its own "mobile office" program: one bus, traveling to one location for a day or two at a time, available from 9am until 3pm, during the heart of the work day.⁸⁰

Such a program is patently insufficient to fulfill the government's obligation "to ensure that all voters are provided convenient opportunities to obtain" the ID they need. If this is the solution envisioned at the outset of the budgeting process for a new program, the slapdash solution provided in the event of underfunding will be, *a fortiori*, even more limited. The low cost estimate in the Commission report risks a dramatically underfunded program – which, in turn, will only increase the burdens on those who do not currently have sufficient identification: the poor, elderly, disabled, and people of color.

F. THE "REAL ID" RECOMMENDATIONS WILL MAKE THE UNITED STATES AN OUTLIER AMONG THE WORLD'S DEMOCRACIES

As the Report acknowledges, the United States has one of the lowest voter participation rates among the world's democracies. Our nation trails many other developed and developing democracies in voter turnout by 20 to 30 points.⁸¹ The identification recommendations will further depress voter participation.

The Report seeks to justify its proposed identification requirement in part by asserting that voter registration in many other countries is tied to photo identification.⁸² But most of the established democracies with which we usually compare the United States—such as the United Kingdom, Australia, Canada, Ireland, New Zealand, Sweden, and Denmark—do not require identification as a condition of voting.⁸³ A few established democracies that require identification for voting do so only in special circumstances. Germany, for instance, requires identification only of those voters who do not furnish their "notice of polling" or who appear to vote in a polling place other than that in which they are registered.⁸⁴ As a recent book surveying the election procedures of 62 countries found, unlike emerging democracies, "established democracies are less likely to require voters to identify themselves other than verbally."⁸⁵

The Report's claim that citizens of "nearly 100 democracies use a photo identification card" in order to vote is contrary to fact and wholly without support. The sole document the Report cites in support of that assertion does not even mention voting (except to note that India has a voter registration card).⁸⁶ What is more, that document says that "virtually no common law country" —like the United States—has an identification card, and only a minority of those countries with identification cards include photographs on those cards.

The Report also fails adequately to address the other ways in which the United States is distinct from those foreign countries that do require photo ID. For example, unlike the United States, France currently issues its government ID cards to all citizens free of charge.⁸⁷ In addition, unlike the United States, most other nations do not have an election system that is administered at the local level, often by partisans, with minimal oversight. This would allow for inconsistent and unequal application, and perhaps even partisan abuse, of ID requirements.

Moreover, as the Report does note, most other countries "have more effective voter registration" systems than the United States because election authorities abroad "take the initiative to contact and register voters and conduct audits of voter registration lists to ensure that they are accurate."⁸⁸ These affirmative measures, which are not undertaken in the United States, counterbalance the depressive effects of voter ID requirements. They do, however, cost much more than Americans have been willing to spend on elections thus far. Mexico, for example, spent *twice* what California spent for its most recent general election per registered voter, and *four times* what Wyoming spent.⁸⁹ Finally, unlike the United States, many other countries with national identification cards also have established privacy laws, with government structures specifically devoted to vigorous enforcement of those laws, to safeguard against abuse of the information contained on the cards.⁹⁰

<u>G. THE "REAL ID" RECOMMENDATIONS ARE INCONSISTENT WITH RIGHTS</u> <u>GUARANTEED IN THE U.S. CONSTITUTION AND FEDERAL STATUTES</u>

The Report's identification proposals will not only exclude millions of legitimate American voters; they are also inconsistent with the rights guaranteed by the U.S. Constitution and federal statutory law.

First, restrictive identification requirements would unconstitutionally deprive many Americans of their right to vote. The right to vote has long been recognized as a fundamental right protected by the First and Fourteenth Amendments of the U.S. Constitution.⁹¹ Voting is a primary avenue through which most citizens express their support or opposition to government policies. A government regulation that severely burdens the right to vote for some or all voters is presumptively invalid unless the state can show that the regulation is "narrowly drawn to advance a state interest of compelling importance."⁹²

There is no question that the proposed identification requirements would impose severe burdens on the right to vote. Indeed, as explained above, the millions of Americans who will not have "Real IDs" would be absolutely denied their right to vote under the Commission's proposed scheme. Regulations which present an absolute bar to a citizen's ability to vote represent the most severe burden on the right to vote and trigger heightened scrutiny under the Constitution.⁹³

This significant barrier to voting is by no means narrowly tailored to serve a compelling state interest. While the interest in preventing voter fraud is an important one, the incidence of the types of fraud targeted by ID requirements is negligible. A rule that would bar millions of citizens from the franchise in an attempt to prevent a tiny fraction of them from attempting to commit a rare form of fraud cannot be said to be "narrowly drawn," especially when less restrictive alternatives can accomplish the same goal. The Constitution does not sanction the use of such a blunt instrument against our most cherished right.

Second, the Report's identification proposals would create an unconstitutional poll tax. The Constitution and the Voting Rights Act forbid attaching a monetary cost to voting.⁹⁴ By preventing those without means to procure the costly proof of identity necessary to obtain "Real IDs" from voting, the ID requirement would "make[] the affluence of the voter or payment of any fee an electoral standard" and would be unconstitutional.⁹⁵

Third, because of the disproportionate effects that ID requirements have on minority voters, they undermine the principles of the federal Voting Rights Act. Section 2 of the Voting Rights Act prohibits any voting procedure that has, in the totality of circumstances, a discriminatory effect on the ability of minority voters to participate in the political process, even if the procedure is adopted and applied without the intent to discriminate.⁹⁶ Since "Real ID" requirements will exclude African Americans, Native Americans, Latinos, and Hispanics to a much greater extent than they will white voters, they contravene this important legal protection. Moreover, to the extent that ID requirements are applied in a discriminatory manner—as they have been throughout American history—they may also run afoul of the Constitution's prohibition of intentional racial discrimination.⁹⁷

Finally, the Report's identification proposals will undermine the careful balance Congress crafted in the Help America Vote Act to enhance states' ability to verify the accuracy of their voter registration lists without unduly infringing on voters' rights. Indeed, it is irresponsible to recommend ID requirements at a time when states are first implementing some of the most important provisions of HAVA—provisions designed to remedy the same problem the Report claims to address. Moreover, as noted above, the ID recommendations will also undermine important provisions of the National Voter Registration Act designed to provide lowincome individuals with greater access to voter registration.

* * *

The Report's zeal for an identification requirement at any cost reflects a general misconception of election integrity. An election with integrity is one that allows every eligible voter—and only eligible voters—the opportunity to cast a ballot and to have that ballot counted accurately. The Report's ID recommendation fails this standard. It is unjustified as a matter of both policy and law, and must not be included in any legitimate proposal for meaningful election reform.

Chapter II

THE RECOMMENDATIONS THAT FULL SOCIAL SECURITY NUMBERS BE USED IN VOTER DATABASES (SECTION 2.2) AND ON ID CARDS (SECTION 2.5) POSE SERIOUS PRIVACY AND SECURITY PROBLEMS

With regard to the Report's interoperability recommendations, it is unquestionably beneficial to account for voters who move across state lines. Nonetheless, the Report fails to consider the serious efficacy, privacy, and security concerns raised by a nationally distributed database of the magnitude it contemplates. These problems are exacerbated by the Report's recommendation that an individual's Social Security number be used as the broadly disseminated unique voting identifier.

The Report's recommendation creates substantial privacy and security hazards. Social Security numbers unlock a vast array of information regarding private financial, employment, and medical data – and, as a result, must be kept with ironclad security. Unfortunately, existing legal limitations on and protections for Social Security numbers have been consistently whittled away over time and frequently disregarded in practice. The media regularly reports on breaches of security concerning public and private data files containing Social Security numbers. Hackers, however, are not the only concern. Social Security numbers are also disclosed by officials entrusted with their safekeeping, despite criminal penalties against distribution.⁹⁸ For example, in 1997, Georgia's Secretary of State contracted with a credit reporting corporation in an effort to "capture" the Social Security numbers of some 400,000 registered voters without such a number on file. In due course, Georgia's entire voter registration list—records for more than four million citizens, and the associated Social Security numbers of those who had provided their number upon registration—was disclosed to the corporation, with no restrictions on the corporation's use of those numbers.⁹⁹

The Report's recommendation to use the Social Security number as the unique identifier for tracking voters across state lines would only increase the general circulation of this financial keystone—and there is no reason to believe that new legal protections would be any more effective than their existing counterparts. The potential for improper use and disclosure will only increase.

Moreover, the Report recommends not only that the Social Security number be used as a unique interstate identifier, but also that it be placed physically on the voting ID card.¹⁰⁰ A misplaced or stolen card would contain, readily available on the face of the card, all information necessary to perpetrate identity theft with ease: name, signature, date of birth, current address, and Social Security number. Similarly, such personal information would be contained on photocopies of drivers licenses maintained for other purposes: for example, copies held (and potentially misplaced) by clerks at car rental agencies or volunteer poll workers. Such a card would become a treasure chest for wrongdoers, and would expose countless Americans to privacy violations, identity theft, and variety of other crimes.

In addition to the substantial privacy concerns, the Social Security number is a flawed key for tracking (and potentially purging) voters across states. The Social Security

Administration was not established to construct a system of national identification, and its database contains substantial errors. For example, the SSA's Director of Information Exchange and Computer Matching has admitted that at least ten percent of the information obtained when attempting to match identifying information in the SSA database with identifying data collected in other systems by other government entities may be inaccurate.¹⁰¹ The SSA's systems may be adequate for disbursing funds, but they were never intended to track individuals from one state to another for voting purposes.¹⁰²

Finally, the Report recommends—without any discussion—that the information used as an individual's unique fingerprint to track a voter across state lines include not merely the date of birth, but also the person's "place of birth."¹⁰³ As with the Social Security number, this information is often used as a key to private information wholly unrelated to voting, and as such, disclosure presents a substantial security hazard. Moreover, this information seems particularly susceptible to use in harassing legitimate voters, particularly naturalized citizens. The reasons to protect against broad disclosure of a voter's place of birth are at least as serious as those confronting the widespread distribution of a voter's full Social Security number. Yet, as with many other issues, the Report wholly fails to consider these important concerns.

Chapter III

THE RECOMMENDATION ON RE-ENFRANCHISEMENT OF PERSONS WITH FELONY CONVICTIONS (SECTION 4.6) IS OVERLY RESTRICTIVE, OUT OF STEP, AND UNWORKABLE

The section of the Report on felony re-enfranchisement lacks the strong language, found in much of the rest of the Report, concerning individual rights and the perception of a fair process. The substance of the Commission's principal recommendation reflects this apparent indifference to the voting rights of people with criminal convictions.¹⁰⁴ The Report recommends that states restore voting rights only to certain people with criminal convictions, and only after they have "fully served their sentence." This overly restrictive standard places the Commission out of step with the states, the American public, and the laws of other nations.

Recommendation 4.6.1 provides:

States should allow for restoration of voting rights to otherwise eligible citizens who have been convicted of a felony (other than for a capital crime or one which requires enrollment with an offender registry for sex crimes) once they have fully served their sentence, including any term of probation or parole.

This recommendation would set a standard more generous than the policies of the most regressive thirteen states in the nation but more restrictive than the remaining thirty-seven. The thirteen regressive states permanently disenfranchise some or all people with criminal convictions even after they have completed their sentences. The thirty-seven other states either leave intact the voting rights of people with criminal convictions or re-enfranchise them, without exception, upon completion of sentence or sooner.¹⁰⁵ Adoption of the Commission's principal recommendation would, therefore, be a step backward for the large majority of states.

The trend in the states is toward extension of the franchise. Since 1997, twelve states have reformed their laws or policies to allow more people with convictions to vote.¹⁰⁶ In 2005 alone, Nebraska repealed a permanent ban on voting and restored the franchise to people with felony convictions two years after the completion of sentence;¹⁰⁷ Iowa's governor issued an executive order restoring voting rights to people with criminal convictions when they complete their sentences;¹⁰⁸ and the Rhode Island General Assembly passed and sent to referendum a resolution to amend the state constitution to re-enfranchise people with felony convictions upon their release from prison.¹⁰⁹

These reforms are driven by some startling numbers. Approximately 4.7 million Americans have lost the right to vote because of a criminal conviction.¹¹⁰ This number includes 1.4 million African-American men, whose 13% rate of disenfranchisement is seven times the national average.¹¹¹ More than 670,000 of the disenfranchised are women;¹¹² more than 580,000 are veterans;¹¹³ and 1.7 million have completed their sentences.¹¹⁴ This astonishing rate of criminal disenfranchisement is a blot on our democracy, an affront to racial justice, an

impediment to rehabilitation, and a quagmire for election officials. The Report's recommendation does not improve the situation.

The Report suggests that its regressive recommendation is in line with the views of "proponents of re-enfranchisement." This is a mischaracterization. On the contrary, mainstream proponents of re-enfranchisement reject the notion that re-enfranchisement should await the completion of *all* terms of a criminal sentence, and generally favor restoration of voting rights when a person reenters the community as a citizen and taxpayer. The American Bar Association is but one of many organizations to support this position, urging re-enfranchisement immediately following incarceration.¹¹⁵ A person convicted of a crime must and will serve all terms of a sentence, but disenfranchisement is not part of criminal sentencing, and voting is a civic duty that a person should reassume as he or she reintegrates into society. As to victim restitution, it should be paid, but a person's voting rights should never depend on the ability to pay this or any other sum of money.¹¹⁶

The American people also support more generous re-enfranchisement than the Commission Report recommends. In a 2002 telephone survey of 1,000 Americans nationwide, researchers found that substantial majorities (64% and 62% respectively) supported allowing probationers and parolees to vote.¹¹⁷ Fully 80% favored restoring the franchise to people who had completed felony sentences.¹¹⁸ Even when questions were asked about certain unpopular offenses, majorities supported voting rights. Two-thirds of respondents supported allowing violent ex-felons to vote; 63% supported allowing ex-felons convicted of illegal stock-trading to vote; and 52% supported restoring the franchise to ex-felons who had been convicted of a sex crime.¹¹⁹

International norms are even more favorable to voting rights. Inmates may cast ballots while incarcerated in many democracies, including Australia, Canada, Denmark, France, Germany, Israel, Ireland, Japan, Peru, Poland, Spain, South Africa, and Sweden.¹²⁰ Others including Argentina, Brazil, Egypt, India, Portugal, Russia, and the United Kingdom—restore the franchise to prisoners once they have completed their sentences.¹²¹ Florida, Kentucky, and Virginia now stand alone with Armenia as the only democracies in the world that permanently disenfranchise all citizens who have committed a felony.¹²² The United States accounts for 5% of the world's population—and almost half of those who cannot vote because of a felony conviction.¹²³

The Report advises exceptions even to the narrow post-sentence re-enfranchisement it recommends. Under the Report's rule, capital offenders and those whose names are entered in a sex crimes registry would never vote. These exceptions may be politically expedient, but they are unjustified. A "capital crime" is one for which the death penalty may, but need not necessarily, be imposed. When a person is executed, there is of course no question of re-enfranchisement. Even when a capital offender is not sentenced to death, however, he or she is increasingly unlikely ever to get out of prison and even more unlikely ever to be released from parole. Forty-eight states, plus the District of Columbia and the federal government, employ some form of sentence of life imprisonment without parole.¹²⁴ For those capital offenders not serving such a sentence, the minimum time in prison still stretches into multiple decades.¹²⁵ Those few offenders who may complete parole as older adults after serving twenty-five to fifty

years in prison ought to regain the franchise, as should those who are exonerated and pardoned after years of wrongful confinement on death row. Likewise, sex offenders who have served their sentences are no less entitled to vote than others. Studies show that voting advances rehabilitation,¹²⁶ and no past or potential victim is endangered when a former offender votes.

Moreover, the Report's recommendation is unworkable. The general rule—that reenfranchisement should follow the completion of a criminal sentence—is itself difficult to administer. In Washington State, this rule caused much controversy in the dead-heat gubernatorial election of 2004. Scores of people with felony convictions apparently voted without knowing that it was illegal, and others were prevented from voting although their rights should have been restored.¹²⁷ The confusion was attributable in part to the multiplicity of government agencies involved, including the courts, the department of corrections, the offices of parole and probation, and the county boards of elections. With the relevant information for maintaining the voter rolls divided among so many, errors were inevitable. As Washington Secretary of State (and current president of the National Association of Secretaries of State) Sam Reed concluded, "the simplest way to fix confusion over tracking felons would be to automatically restore voting rights when people are released from prison, regardless of whether they've paid all their court debts."¹²⁸

Add to this base-level confusion the difficulties of tracking the exceptions the Report recommends, and the errors will compound. Not all states have capital offenses, and in those that do, there is variation in which crimes are punishable by death. For example, Florida punishes certain types of sexual battery and drug-trafficking as capital offenses, whereas in Arizona, the death penalty is available only for aggravated first-degree murder.¹²⁹ There are similar inconsistencies in the states' designations of crimes requiring registration as a sex offender. In Arkansas, for example, a person convicted of "distributing, possessing, or viewing matter depicting sexually explicit conduct involving a child" or of "computer exploitation of a child," among many other crimes, must register as a sex offender.¹³⁰ In Rhode Island, however, only people who have been convicted of violent or aggravated sex offenses or who have been designated "sexually violent predators" or recidivists must register.¹³¹ In Delaware, adult and juvenile sex offenders must register; in Alabama, only adult (and not juvenile) sex offenders must register.¹³² Because a capital offender in one state may not be one in another, and because a person who must register as a sex offender in one state need not do so in another, maintaining accurate voter rolls as people move from state to state would present nearly insurmountable challenges under the Report's recommendation.

Any rule other than one that restores voting rights to all citizens upon completion of incarceration creates the opportunity for erroneous—and even malicious—purges of eligible citizens from the voting rolls. Take for example the infamous purges of the Florida voter rolls of supposedly ineligible felons. In 2000, Katherine Harris, who was both the Secretary of State and the state co-chair of George W. Bush's presidential campaign, implemented a program purging any Florida voter whose name shared 80 percent of the letters of a name in a nationwide felon database; a California felon named John Michaelson would cause an eligible Floridian named John Michaels to be purged. Unsurprisingly, over half of those who appealed the purge after the 2000 election were deemed eligible.¹³³ In 2004, the state again developed a now-discredited "list of suspected felons" for the purpose of facilitating purges. Fortunately, the state was forced to

withdraw that list before the election when it was revealed that the list included many citizens who had never been convicted of a felony; included many whose voting rights had been restored; and was racially biased, containing 22,000 African-Americans but only 61 Hispanics.¹³⁴ These problems would not have arisen had Florida law restored voting rights to its citizens upon release from incarceration. In that case, one single agency—the department of corrections—would be responsible for notifying the state's chief election official both when people lost their rights upon sentencing, and when they regained their rights upon release; any person who showed up at the polls to vote would clearly be a person who was not in prison. Thus, the systems for purges and restorations would be streamlined, avoiding the kinds of abuses much publicized in Florida. A well-functioning system would inform election officials only of the names of all persons in prison; others would not be rendered ineligible by a felony conviction.

The Commission missed an important opportunity to recommend that states automatically restore the franchise to people with criminal convictions when they have served their time in prison. This rule would: (1) strengthen our democracy by encouraging broader and more equitable participation in electing our leaders; (2) encourage the rehabilitation of those most in need of building connections to their communities; and (3) streamline the restoration process by making the state departments of correction the exclusive sources of the relevant information to be transmitted to election officials. Instead, the Report adopted a disappointing and unworkable recommendation that the states should leave behind as they continue to move in the right direction on this issue. For its part, Congress should pass legislation permitting people with criminal convictions to vote in federal elections as soon as they are released from incarceration.

ENDNOTES

¹ Commission on Federal Election Reform, *Building Confidence in U.S. Elections* (Sept. 2005) [*hereinafter* "Report"].

² Report at iv.

³ Although the Report's section on provisional ballots (Section 2.3) is also deficient in that it fails to address many of the problems with state administration of provisional ballots and makes too weak a recommendation for counting provisional ballots cast by eligible voters in the wrong precinct, we do not address that section in this white paper since the Commission's report does not purport to comprehensively analyze provisional balloting. ⁴ *Id.* at 21.

⁵ "Real ID" cards are driver's licenses and non-driver's identification cards issued to those who meet rigorous documentation requirements. *See* section A.1.

⁶ Report at 18.

 7 Id.

⁸ 42 U.S.C. § 15481 *et seq.*

⁹ Report at 73 n.22.

¹⁰ See John Mark Hansen, Coordinator, Task Force on the Federal Election System, *Report*, at VI-4 *in Task Force Reports to Accompany the Report of the National Commission on Election Reform* (Aug. 2001); National Commission on Election Reform, *To Assure Pride and Confidence in the Electoral Process*, at 32 (Aug. 2001) ("5-7% of adults do not possess a driver's license or other photo identification, and are disproportionately poor and urban") [*hereinafter* "Ford-Carter Report"].

¹¹ A comparison of national driver's license records and the most recent census data shows that approximately 23 million voting-age individuals in the United States do not have driver's licenses; using 2000 citizenship rates, about 22 million of those individuals are citizens. *See* Fed. Highway Admin., U.S. Dep't of Transp., *Licensed Total Drivers, By Age 2003* tbl. DL-22 (Oct. 2004), <u>http://www.fhwa.dot.gov/policy/ohim/hs03/htm/dl22.htm;</u> U.S. Census Bureau, *Annual Estimates of the Population by Selected Age Groups and Sex for the United States: April 1, 2000 to July 1, 2004* (June 2005), <u>http://www.census.gov/popest/national/asrh/NC-EST2004-sa.html;</u> U.S. Census Bureau, *Census 2000 PHC-T-31 Voting-Age Population and Voting-Age Citizens*, tbl. 1-1, <u>http://www.census.gov/population/www/cen2000/phc-t31.html</u>. Other means to triangulate the figure yield similar results. For example, in 2000, there were 193 million voting-age citizens; at the country's 3.3% national growth rate from 2000 to 2003, that would yield 200 million voting-age citizens in 2003. As noted above, the Ford-Carter Commission estimated that up to ten percent of these Americans – or approximately 20 million -- do not have driver's licenses or state-issued photo ID. U.S. Census Bureau, *Census 2000 PHC-T-31 Voting-Age Population-- do not have driver*, s licenses, tbl. 1-1,

http://www.census.gov/population/www/cen2000/phc-t31.html; U.S. Census Bureau, USA QuickFacts from the US Census Bureau, at http://quickfacts.census.gov/qfd/states/00000.html. In New York City, as many as three million registered voters do not have a driver's license, and 1990 census data revealed that more than fifty percent of voting-age city residents overall did not have a license. See Elizabeth Daniel, The New Voter Identification Requirement, THE GOTHAM GAZETTE, April 2002, at http://www.gothamgazette.com/article/ 20020401/17/728.

¹² Real ID Act of 2005, Pub. L. No. 109-13, Div. B, Title II, § 202(c), 119 Stat. 231, 302. The Report understates the burdens of acquiring Real ID. The Real ID Act does more than require states to "verify an individual's full legal name, date of birth, address, Social Security number, and U.S. citizenship," Report at 19; it requires individuals to present documentary proof of each such fact.
¹³ Press Advisory, Maricopa County Recorder and Elections Department, Voter Registration is Different Following

¹³ Press Advisory, Maricopa County Recorder and Elections Department, Voter Registration is Different Following DOJ Approval (Feb. 4, 2005), <u>http://recorder.maricopa.gov/pressrelease.aspx</u>.

¹⁴ See Deanna Wrenn, *Three States Debate Requiring Voters to Show ID*, VENTURA COUNTY STAR, Mar. 31, 2005, at 6.

¹⁵ John Pawasarat, *The Driver License Status of the Voting Age Population in Wisconsin* 1, 11 (June 2005), *available at* <u>http://www.uwm.edu/Dept/ETI/barriers/DriversLicense.pdf</u>.

¹⁶ See American Association of People with Disabilities et al, *Statement in Opposition to a National Voter Identification Card*, at 2 (June 29, 2005), <u>http://www.commoncause.org/atf/cf/%7BFB3C17E2-CDD1-4DF6-92BE-BD4429893665%7D/NATIONAL_ID_STATEMENT.PDF</u>.

¹⁷ See section A.2.

¹⁸ See section A.3.

¹⁹ According to the U.S. Census Bureau, "[a]bout 1 in 6 Americans move each year." Kristin A. Hansen, U.S. Census Bureau, Geographical Mobility (Jan. 2001), http://www.census.gov/population/www/pop-

profile/geomob.html. Students and people of color move even more frequently than the average. Id. ²⁰ Based on the figures cited in the Report, approximately 75% of the moving individuals move within the same state. Report at 9. The vast majority of individuals moving would therefore have no reason unrelated to voting to obtain a new driver's license or state-issued ID.

²¹ Report at 9 (citing average number of Americans who move each year).

²² Although the Report does not specifically recommend that Real ID cards include an individual's address, the Real ID Act does, See Pub. L. No. 109-13, Div. B, Title II, § 202(b)(6) (minimum requirements for Real ID cards). ²³ Report at 10.

²⁴ See Letter from Adam Cox et al. to John Tanner, Chief, Voting Section, Civil Rights Div., U.S. Dep't of Justice (Aug. 18, 2005), at 3. ²⁵ See Ga. Dep't of Driver Servs., Driver's License Customer Service Centers, at

http://www.dds.ga.gov/locations/index.aspx (office locator on Georgia Department of Driver Services web site). ²⁶ There are other collateral costs to the Commission's proposal. For example, many states revoke driver's licenses when drivers fail to pay fines associated with traffic violations; a citizen who cannot pay the fine will not be able to renew his or her driver's license. Thus, by relying on Real IDs, the vast majority of which will be driver's licenses, the Commission's identification proposal incorporates the cost of maintaining a current and valid driver's license. In practice, this means that the fundamental right to vote could well be denied due to an eligible voter's unpaid parking ticket.

²⁷ Ford-Carter Report at 77.

²⁸ Letter from Deval L. Patrick, Assistant Att'y Gen., Civil Rights Div., U.S. Dep't of Justice, to Sheri Marcus Morris, La. Assistant Att'y Gen. (Nov. 21, 1994). Louisiana now allows voters to establish their identity through signed affidavit. ²⁹ Pawasarat, *supra* note 15.

³⁰ *Id*.

³¹ Tova Wang, 2004: A Report Card, AM. PROSPECT, Jan. 2004, at A5.

³² 42 U.S.C. §§ 1973gg et seq.

³³ U.S. Census Bureau, Voting and Registration in the Election of November 2000, tbl. 14, http://www.census.gov/ population/www/socdemo/voting/p20-542.html.

⁴ See Hansen, supra note 10, at VI-4 ("Poll workers with the worst of motives might deliberately use the requirement to confront and intimidate 'strangers.' Either way, voters who were asked to show identification when others were not might come to feel that they were singled out."). ³⁵ See Asian American Legal Defense and Education Fund, *The Asian American Vote 2004* (April 2005)

(documenting discriminatory application of ID rules to Asian-Americans), http://www.aaldef.org/images/04-20-05 exit poll report.pdf; Editorial, Lessons of the Ballot Box, N.Y. TIMES, Nov. 4, 2004, at A24 (complaining that "voter identification requirements were arbitrarily, and often incorrectly, enforced" in 2004 election); cf. Chandler Davidson et al., Republican Ballot Security Programs: Vote Protection or Minority Vote Suppression - or Both? 93-95 (Sept. 2004) (describing reports of Native Americans not being informed of option to sign an affidavit instead of furnishing photo ID in South Dakota).

³⁶ See Report at 20-21 & Recommendation 2.5.4.

³⁷ See, e.g., 148 Cong. Rec. S1227 (daily ed. Feb. 27, 2002) (statement of Sen. Landrieu) ("History has shown that requiring photo identification or certain other documents most significantly impacts minority voters."); 148 Cong. Rec. S1224 (daily ed. Feb. 27, 2002) (statement of Sen. Schumer) ("The intent of this legislation is to take people, particularly those who live in the corners of America who do not fly airplanes and use their credit cards all the time but rather people who may not have a driver's license, who may not have a utility bill, and allow them to vote, our most sacred right.").

³⁸ States vary widely in how they treat provisional ballots cast by first-time voters who do not present some form of identification at the polls: some count such provisional ballots unless there is affirmative evidence that the voter is ineligible; some count them if election officials match the signature on the provisional ballot envelope with the signature the voter provided at registration; some count them only if the voter returns with ID within a specified period; and some refuse to count them under any circumstances. Regardless of whether HAVA permits states to refuse to count provisional ballots cast by eligible and registered first-time voters who do not have the requisite ID. it is clear that HAVA does not prohibit the counting of such ballots.

³⁹ Report at 19.

⁴⁰ While some States do not require payment from individuals who sign an affidavit of indigence, few Americans are willing to call themselves indigents or to face the collateral consequences of signing such an oath.

⁴¹ Report at 33.

⁴² Affidavit of Christopher M. Thomas in Michigan State Conf. of NAACP Branches v. Land, No. 04-CV-10267 (E.D. Mich. Oct. 7, 2004), ¶ 36 (asserting that 92% of Michigan voting age population has driver's license or nondriver's ID); Tel. Conf. with Christopher Thomas, Michigan Director of Elections, Sept. 21, 2004 (estimating that 90% of eligible voters in Michigan possess driver's licenses or state-issued ID).

⁴³ georgia.gov, DDS Begins Mobile Licensing Tours & Center Reservations for Photo IDs, at http://www.georgia.gov/00/article/0.2086,4802_4961_41800330,00.html. ⁴⁴ Nancy Badertscher, *State Bus Will Roll for Voter IDs*, ATLANTA J.-CONST., Aug. 9, 2005, at 1B.

⁴⁵ See Nat'l Conference of State Legislatures, State Requirements for Voter Identification (July 26, 2005), at http://www.ncsl.org/programs/legman/elect/taskfc/voteridreg.htm (summary of state laws regarding voter ID).

⁴⁶ See FLA. STAT. ch. 101.048 (2005); Letter from Martha R. Mahoney, Chair, Racial Impact Committee, Miami Dade Election Reform Coalition, to Timothy Lambert, Civil Rights Div., U.S. Dep't of Justice (Oct. 2, 2003), http://www.reformcoalition.org/Ressources/MDERC%20Preclearance%20Letter1.pdf.

See Report at 18.

⁴⁸ The text of this recommendation is as follows: "We recommend that until January 1, 2010, states allow voters without a valid photo ID card (REAL or EAC-template ID) to vote, using a provisional ballot by signing an affidavit under penalty of periury. The signature would then be matched with the digital image of the voter's signature on file in the voter registration database, and if the match is positive, the provisional ballot should be counted. Such a signature match would in effect be the same procedure used to verify the identity of voters who cast absentee ballots. After January 1, 2010, voters who do not have their valid photo ID could vote, but their ballot would only count if they returned to the appropriate election office within 48 hours with a valid photo ID." Report at 21

(Recommendation 2.5.3). ⁴⁹ While the Report claims that its proposal will provide a safety net for two election cycles, that claim ignores the fact that the Real ID Act does not go into effect until 2008.

⁵⁰ Recommendation 2.5.3.

⁵¹ Report at 18.

⁵² 42 U.S.C. § 1973i(c).

⁵³ Report at 18.

⁵⁴ Lorraine Minnite & David Callahan, Securing the Vote: An Analysis of Election Fraud (2003), available at http://www.demos.org/pubs/EDR - Securing the Vote.pdf.

⁵⁵ Coalition on Homelessness & Hous. in Ohio & League of Women Voters Coalition, Let the People Vote (2005). available at http://www.cohhio.org/alerts/Election%20Reform%20Report.pdf.

⁵⁶ Letter from Cathy Cox, Ga. Sec²y of State, to Sonny Perdue, Ga. Governor (Apr. 8, 2005), available at http://www.aclu.org/Files/OpenFile.cfm?id=18651.

Mich. Op. Att'y Gen. No. 6930, 1997 WL 37560, at *3 (Jan. 29, 1997).

⁵⁸ Report at 4.

⁵⁹ 42 U.S.C. § 15483(a)(2)(A)(ii).

⁶⁰ Borders v. King County, No. 05-2-00027-3 (Wash. Super. Ct. Chelan County June 24, 2005), available at http://www.secstate.wa.gov/documentvault/694.pdf. Of these cases, at least two individuals voted on behalf of spouses who had died earlier in 2004, one voted on behalf of a roommate who had died earlier in 2004, and two were a mother and daughter who voted on behalf of their father who had died earlier in 2004. Gregory Roberts, Six More Charged With Offenses in 2004 Election, SEATTLE POST-INTELLIGENCER, June 22, 2005, at B1.

⁶¹ Report at 4.

⁶² Greg J. Borowski, Nothing Points to Fraud in 9 Double Voting Cases, MILWAUKEE J.-SENTINEL, Aug. 22, 2005, at B1.

⁶³ Report at 4.

⁶⁴ Report at 45.

⁶⁵ Office of the Clerk, U.S. House of Representatives, *Election Statistics* (2005), http://clerk.house.gov/members/ electionInfo/elections.html.

⁶⁶ In 1990, there were 89 lightning fatalities, out of 248,709,873 Americans, for a rate of 0.00004%. stats at George Mason University, How Likely Are You to Be Struck by Lightning?, May 1, 1995, available at http://www.stats.org/ record.jsp?type=news&ID=402.

⁶⁷ Report at 18.

⁶⁸ According to Hans A. von Spakovsky, Counsel to the Assistant Attorney General of the Civil Rights Division, "absentee ballots represent the easiest way to steal an election." Memorandum from Hans A. von Spakovsky, Attorney and Government Affairs Consultant, on No Fault Absentee Balloting, p. 1 (Mar. 29, 2001), available at http://www.hss.caltech.edu/~voting/von spakovsky-1.pdf.

⁶⁹ See Hansen, supra note 10, at V-1 (2001 analysis comparing the relative rates of absentee voting by race using 1996 data); Kimball W. Brace & Michael P. McDonald, Final Report of the 2004 Election Day Survey, submitted to the U.S. Election Assistance Commission (Sept. 27, 2005), at 5:8 (analysis of 2004 election survey data showing that jurisdictions with predominantly non-Hispanic White populations reported an absentee ballot request rate of 10.9%, while jurisdictions with predominantly non-Hispanic Black populations reported a rate of 5.7%).

⁷⁰ Edward B. Foley, Is There a Middle Ground in the Voter ID Debate?, available at http://moritzlaw.osu.edu/ electionlaw/comments/2005/comment0906.html; Rick Hasen, Beyond the Margin of Litigation: Reforming U.S. Election Administration to Avoid Electoral Meltdown, 62 WASH. & LEE L. REV. (forthcoming 2005), available at http://electionlawblog.org/archives/margin.3.pdf.

⁷¹ Report at 20.

⁷² Report at 20-21.

⁷³ Report at 71 & n.81.

⁷⁴ Legislation Concerning Immigrant Issues: Hearing on H.R. 231, H.R. 429, H.R. 471 and H.R. 1493 Before the Subcomm. on Immigration and Claims of the House Comm. on the Judiciary, 105th Cong. 107 (1997) (statement of Stephen Moore, Economist, Cato Institute), http://commdocs.house.gov/committees/judiciary/hju43829.000/ hiu43829 0f.htm.

Tova Andrea Wang, Century Found., The Debate Over a National Identification Card (May 10, 2002), available at http://www.tcf.org/Publications/HomelandSecurity/National ID Card.pdf.

⁷⁶ Brad Shannon, *Federal Practices Hurt States, Officials Say*, THE OLYMPIAN, Aug. 17, 2005, at 1B.

⁷⁷ Cong. Budget Office, H.R. 418: REAL ID Act of 2005, CBO Cost Estimate (Feb. 9, 2005).

⁷⁸ Dibya Sarkar, States May Face Higher Costs for Real ID, FEDERAL COMPUTER WEEK, Aug. 19, 2005, http://www.fcw.com/article90196-08-19-05-Web.

² Letter from Adam Cox et al., to John Tanner, Chief, Voting Section, Civil Rights Div., U.S. Dep't of Justice (Aug. 18, 2005).

⁸⁰ See Georgia.gov, supra note 43.

⁸¹ See Rafael López Pintor et al., International IDEA, Voter Turnout since 1945: A Global Report 78-85 (2002), http://www.idea.int/publications/vt/upload/VT screenopt 2002.pdf.

⁸² Report at 5.

⁸³ LOUIS MASSICOTTE, ANDRE BLAIS & ANTOINE YOSHINAKA, ESTABLISHING THE RULES OF THE GAME: ELECTION LAWS IN DEMOCRACIES (2004).

⁸⁴ Id.

⁸⁵ Id.

⁸⁶ See Privacy International, Identity Cards: Frequently Asked Questions (Aug. 24, 1996) (cited in Report at 5), at http://www.privacy.org/pi/activities/idcard/idcard faq.html (cited in Report at 5).

⁸⁷ See Ministère de l'intérieur, Votre carte nationale d'identité (May 24, 2004), at http://www.interieur.gouy.fr/ rubriques/b/b8 teleservices/ENTAM.

⁸⁸ Report at 5.

⁸⁹ Report at 59; EPIC Project, Election Process Information Collection, What Was the Spending (in USD) per Registered Voter ...?, http://epicproject.org/ace/compepic/en/getAnswer\$ALL+EM10.

⁹⁰ See Mark Rotenberg & Cedric Laurant, Privacy & Human Rights 2004: An International Survey of Privacy Laws and Developments (2004), at http://www.privacyinternational.org/survey/. ⁹¹ See, e.g., Reynolds v. Sims, 377 U.S. 533, 561-62 (1964).

⁹² Burdick v. Takushi, 504 U.S. 428, 434 (1992).

⁹³ See generally Anderson v. Celebrezze, 460 U.S. 780, 789 (1983); Carrington v. Rash, 380 U.S. 89, 96 (1965)

("[A]t the least, [] States may not casually deprive a class of individuals of the vote because of some remote administrative benefit to the State."); Ayers-Schaffner v. Distefano, 37 F.3d 726, 729 (1st Cir. 1994) (total denial of the right to vote for some citizens in an election is "obviously severe").

⁹⁴ U.S. CONST. amend. XXIV, § 1 ("The right of citizens of the United States to vote in any [federal election] shall not be denied or abridged by the United States or any State by reason of failure to pay any poll tax or other tax."): U.S. CONST. amend. XIV, § 1, cl. 4 ("... nor shall any State ... deny to any person within its jurisdiction the equal protection of the law"); Harper v. Virginia State Bd. of Elections, 383 U.S. 663, 666 (1966); 42 U.S.C. § 1973h.

⁹⁵ *Harper*, 383 U.S. at 666.

⁹⁶ 42 U.S.C. § 1973(a) ("No voting qualification or prerequisite to voting or standard, practice, or procedure shall be imposed or applied by any State or political subdivision in a manner which results in a denial or abridgement of the right of any citizen of the United States to vote on account of race or color"); see also 42 U.S.C. § 1973(b); S. Rep. No. 97-417 (1982), reprinted in 1982 U.S.C.C.A.N. 177 et seq.; Thornburg v. Gingles, 478 U.S. 30, 43-45 (1986); Black v. McGuffage, 209 F. Supp. 2d 889, 892 (D. Ill. 2002).

See, e.g., Washington v. Davis, 426 U.S. 229, 241 (1976) ("A statute, otherwise neutral on its face, must not be applied so as invidiously to discriminate on the basis of race").

⁹⁸ 42 U.S.C. §§ 408(a)(8), 405(c)(2)(C)(vii)(I).

⁹⁹ See Schwier v. Cox, No. 1:00-cv-2820-JEC (N.D. Ga. Jan. 31, 2005), slip op. at 5, 21 & n.9, available at http://www.acluga.org/briefs/ssn/SSNotoVoteOrder.pdf.

100 Report at 21.

¹⁰¹ Remarks of Pete Monaghan, Director of Information Exchange and Computer Matching of the Social Security Administration, at the February 2004 meeting of the National Association of Secretaries of State.

¹⁰² Hearing, *supra* note 74.

¹⁰³ Report at 12.

¹⁰⁴ As elsewhere, although the principal recommendation in this section of the Report is deeply flawed, not all of the Commission's recommendations in this area are uniformly bad. For example, Recommendation 4.6.2 proposes two measures that would affirmatively improve the restoration process. First, the states should inform people with criminal convictions regarding voter registration rules and procedures when they become eligible to vote. And second, the state departments of corrections should notify state election officials when a person regains the franchise.

¹⁰⁵ Two states and Puerto Rico do not disenfranchise on the basis of criminal convictions at all. Twelve states and the District of Columbia permit people with convictions to vote as soon as they are released from prison. An additional five states allow probationers to vote, disenfranchising people with felony convictions only while they are in prison or on parole. Eighteen more restore the franchise, without exception for any class of offender (other than, in some cases, those convicted of election-related crimes), when a person completes probation or prison and parole. The Sentencing Project, Felony Disenfranchisement Laws in the United States (Sept. 2005), http://www.sentencingproject.org/pdfs/1046.pdf.

¹⁰⁶ *Id.* ¹⁰⁷ *Id.*; 2005 Neb. Laws L.B. 53, § 1 (codified at NEV. REV. STAT. § 29-112 (2005)). ¹⁰⁸ The Sentencing Project, *supra* note 105; Exec. Order No. 42 (July 4, 2005), *available at* http://www.johnsoncounty.com/auditor/voter/EO_42.pdf. ¹⁰⁹ H.J.R. 6579, 2005 Leg. (R.I. 2005).

¹¹⁰ The Sentencing Project, *supra* note 105.

¹¹¹ Id.

¹¹² Id.; see also The Sentencing Project, Felony Disenfranchisement Rates for Women (Aug. 2004), http://www.sentencingproject.org/pdfs/fvr-women.pdf.

¹¹³ The Sentencing Project, Disenfranchised Veterans in the United States (June 2003), http://www.sentencingproject.org/pdfs/1023.pdf.

¹¹⁴ The Sentencing Project, *supra* note 105.

¹¹⁵ ABA Standards for Criminal Justice, Collateral Sanctions and Discretionary Disgualification of Convicted Persons (3d ed. Aug. 2003), Standard 19-2.6(a), http://www.abanet.org/leadership/2003/journal/101a.pdf.

¹¹⁶ Cf. 42 U.S.C. § 1973h ("The Congress finds that the requirement of the payment of a poll tax as a precondition to voting (i) precludes persons of limited means from voting or imposes unreasonable financial hardship upon such persons as a precondition to their exercise of the franchise, (ii) does not bear a reasonable relationship to any legitimate State interest in the conduct of elections, and (iii) in some areas has the purpose or effect of denving persons the right to vote because of race or color."). ¹¹⁷ Jeff Manza, Clem Brooks & Christopher Uggen, *Public Attitudes Towards Felon Disenfranchisement in the*

United States (2002), Summary, at http://www.sentencingproject.org/pdfs/ManzaBrooksUggenSummary.pdf. ¹¹⁸ Id. ¹¹⁹ Id.

¹²⁰ Brandon Rottinghaus, Int'l Found. for Election Sys., Incarceration and Enfranchisement: International Practices, Impact and Recommendations for Reform 20-26 (2003), http://www.sentencingproject.org/pdfs/ Rottinghaus.pdf.

¹²¹ Id.

¹²² Alabama also permanently disenfranchises individuals who have been convicted of crimes of "moral turpitude," ALA. CONST. art. VIII, § 5, but not all felonies in Alabama meet this definition, see Ala. Op. Atty. Gen. No. 2005-092, and thus some citizens who have committed a felony are not disenfranchised under Alabama law. In addition, although Iowa's constitution permanently disenfranchises all individuals with felony convictions who have not received executive clemency, IOWA CONST. art. II, § 5, an Executive Order issued by the Governor restores voting rights to all individuals convicted of felonies once they complete their sentences. Exec. Order No. 42 (July 4, 2005), available at http://www.johnson-county.com/auditor/voter/EO 42.pdf. Thus, neither Alabama nor Iowa permanently disenfranchises all citizens with felony convictions. 123 Id.

¹²⁴ Death Penalty Info. Ctr., *Life Without Parole News and Developments (2001-2004)*,

http://www.deathpenaltyinfo.org/article.php?did=555&scid=59. ¹²⁵ The Sentencing Project, *The Meaning of "Life": Long Prison Sentences in Context* (May 2004), http://www.sentencingproject.org/pdfs/lifers.pdf.

¹²⁶ Christopher Uggen & Jeff Manza, Voting and Subsequent Crime and Arrest: Evidence from a Community Sample, 36 COLUM. HUM. RTS. L. REV. 193, 205-08 (2004) (noting that people who have been arrested and who vote are only half as likely as those who do not vote to be rearrested or to self-report criminal activity). ¹²⁷ Seattle Times Staff, Scores of Felons Voted Illegally, SEATTLE TIMES, Jan. 23, 2005, at A1.

¹²⁸ Id.

¹²⁹ Compare FLA. STAT ANN. §§ 794.011(2)(a), 893.135(1)(b) with ARIZ. REV. STAT. § 13-703(A),(E).

¹³⁰ ARK. CODE ANN. §§ 12-12-903(12)(A)(i)(w), (y), 12-12-905(a)(1).

¹³¹ R.I. GEN. LAWS § 11-37.1-3.

¹³² Compare DEL. CODE ANN. tit. 11, § 4120 with ALA. CODE § 13A-11-200.

¹³³ John Lantigua, How the GOP Gamed the System in Florida, NATION, Apr. 30, 2001, at 11.

¹³⁴ See, e.g., Ford Fessenden, Florida List for Purge of Voters Proves Flawed, N.Y. TIMES, July 10, 2004, at A13; Bob Kemper, Carter Rips Florida's Election Fixes, ATLANTA J. CONST., Sept. 28, 2004, at A14; Maya Bell, Campaign 2004, Is Florida Ready?, ORLANDO SENTINEL, Aug. 25, 2004, at A1.

Exhibit 7

STRICT ID LAWS DON'T STOP VOTERS: EVIDENCE FROM A U.S. NATIONWIDE PANEL, 2008–2018*

ENRICO CANTONI AND VINCENT PONS

U.S. states increasingly require identification to vote-an ostensible attempt to deter fraud that prompts complaints of selective disenfranchisement. Using a difference-in-differences design on a panel data set with 1.6 billion observations. 2008–2018, we find that the laws have no negative effect on registration or turnout, overall or for any group defined by race, gender, age, or party affiliation. These results hold through a large number of specifications. Our most demanding specification controls for state, year, and voter fixed effects, along with state and voter time-varying controls. Based on this specification, we obtain point estimates of -0.1 percentage points for effects both on overall registration and turnout (with 95% confidence intervals of [-2.3; 2.1 percentage points] and [-3.0; 2.8 percentage]points], respectively), and +1.4 percentage points for the effect on the turnout of nonwhite voters relative to whites (with a 95% confidence interval of [-0.5; 3.2]percentage points]). The lack of negative impact on voter turnout cannot be attributed to voters' reaction against the laws, measured by campaign contributions and self-reported political engagement. However, the likelihood that nonwhite voters were contacted by a campaign increases by 4.7 percentage points, suggesting that parties' mobilization might have offset modest effects of the laws on the participation of ethnic minorities. Finally, strict ID requirements have no effect on fraud, actual or perceived. Overall, our findings suggest that efforts to improve elections may be better directed at other reforms. JEL Codes: D72.

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I. INTRODUCTION

A tension exists in democracies between safeguarding the integrity of the vote and ensuring broad participation. Electoral fraud-which takes the form of stuffing ballot boxes, buying or intimidating voters, or impersonating citizens who are deceased, absentee, or no longer in residence—was prevalent in the early decades of Western democracies (e.g., Garrigou 1992; Lehoucg 2003; Stokes et al. 2013) and is still widespread in developing democracies today (e.g., Collier and Vicente 2012). Combating such fraud is critical to build citizen confidence in election results and to consolidate democratic regimes (Diamond 1999; Berman et al. 2019). However, rules pursuing those objectives can also weaken democracy if they keep eligible citizens away from the polling booth. Compounding the matter, legislators have an incentive to push for restrictions if citizens enfranchised by flexible rules will likely vote for rival parties-or oppose restrictions if that will widen their base.

This article presents empirical evidence on the consequences of strict ID laws in the context of the United States, where the debate on control versus enfranchisement is particularly heated. Between 2006 and 2018, 11 states, mostly with Republican majorities, adopted strict voter identification measures (Hicks et al., 2015).¹ Strict ID laws require voters to present an accepted form of identification document before voting. Voters who fail to do so can cast a provisional ballot, but their vote will not be counted unless they present proper ID to election officials within the next few days. In contrast, all other states allow people without ID to vote. They either have a nonstrict ID law requesting voters to show an ID but allowing those without it to cast a regular ballot, typically by signing an affidavit; check voters' identity by asking them to sign the poll book and verifying their signature; or simply ask voters for their name and check it against a list of eligible citizens.

The effects of strict ID laws on overall participation are ex ante ambiguous. Although these laws create additional costs for people without ID, those who want to vote can acquire it before the election, and it is unclear what share of non–ID holders would vote otherwise: groups of voters less likely to hold an

^{1.} These states are Arizona, Georgia, Indiana, Kansas, Mississippi, North Dakota, Ohio, Tennessee, Texas, Virginia, and Wisconsin. North Dakota and Texas are the only states that experienced a reversal: both states adopted a strict ID law in 2014, and both laws were struck down by federal courts in 2016. In 2018, North Dakota reinstituted a strict, nonphoto ID law.

ID include Blacks and Hispanics, the young, voters older than 70, and poorer and less-educated voters (Barreto, Nuo, and Sanchez 2009; Stewart 2013; Ansolabehere and Hersh 2017), who have long shown lower propensity to vote than other groups (Wolfinger and Rosenstone 1980; Verba, Schlozman, and Brady 1995; Schlozman, Verba, and Brady 2012; Fraga 2018). Moreover, some citizens may become more likely to vote if the laws enhance their confidence in the fairness of the election.

Using a nationwide individual-level panel data set, 2008–2018, and a difference-in-differences (DD) design, we find that strict ID laws have no significant negative effect on registration or turnout, overall or for any subgroup defined by age, gender, race, or party affiliation. These results hold through a large number of specifications and robustness checks. Our most demanding specification controls for state, year, and voter fixed effects, along with state and voter time-varying controls. Based on this specification, and considering the lower bound of the 95% confidence interval, we can rule out that strict ID laws reduce aggregate registration and turnout by more than 2.3 and 3.0 percentage points. Focusing on voters living in adjacent counties across state borders, we can further rule out that the laws reduce their participation by more than 0.5 percentage points.

Most important, given the complaints of selective disenfranchisement, strict ID requirements do not decrease the participation of ethnic minorities relative to whites. The lower bound of the 95% confidence interval from our voter fixed effects regression rules out that the laws decrease nonwhite turnout (relative to white) by more than 0.5 percentage points. Focusing specifically on Black voters, we can rule out that strict ID laws reduce their turnout by more than 1.3 percentage points, relative to white, and by more than 3.1 percentage points in total.

Strict ID laws' overall effects do not increase over time, they remain close to zero and nonsignificant whether the election is a midterm or presidential election, and whether the laws are the more restrictive type that stipulate photo IDs. Our identification assumption is that treated states (which adopted a strict ID law between 2008 and 2018) would have experienced the same changes in turnout as other states, absent the treatment. We find that voters in treated states did have different turnout levels prior to the laws, but they did not show different participation trends than others, lending support for our identification strategy. Finally, in line with the lack of negative effect on the participation of any subgroup of voters, strict ID laws do not affect the relative vote share of Democratic and Republican candidates either.

These results contrast with the large participation effects of other dimensions of election administration: voter registration laws (Rosenstone and Wolfinger 1978; Braconnier, Dormagen, and Pons 2017), convenience voting (Gerber, Huber, and Hill 2013; Hodler, Luechinger, and Stutzer 2015; Kaplan and Yuan 2020), voting technology (Fujiwara 2015), and distance to a polling station (Cantoni 2020). It could be that our null findings reflect two mutually opposing forces: the laws' negative effect on participation versus a reaction of voters against a threat to their right to vote (Citrin, Green, and Levy 2014; Biggers and Smith 2020). We do not find evidence of such a backlash on the part of voters. Strict ID laws have no significant effect on total campaign contributions, measured using administrative records from Bonica (2016), or on an index of voter activity aggregating people's self-reported donations to a candidate, the amount donated, their having attended a political meeting, put up a campaign sign, and volunteered for a campaign, all measured using the Cooperative Congressional Election Study surveys. However, the laws increase the likelihood that nonwhite voters report being contacted by a campaign by 4.7 percentage points, suggesting that parties and candidates who fear they might lose votes as a result of strict ID requirements mobilize their supporters around this issue. These mobilization efforts might have offset small direct negative effects on the participation of ethnic minorities.

In a 2017 review of the literature, Highton notes that contemporary concerns and controversies about voter identification requirements date back to the adoption of Indiana's and Georgia's strict ID laws in 2005, but he finds only limited evidence about the effect of this type of laws on turnout (Highton 2017). Early studies based on cross-state comparisons were unable to isolate the effect of strict ID laws (which, again, are characterized by the fact that they prevent citizens without identification from voting) due to the relative recency of these laws and to the slow increase in the number of states enforcing them. Instead, these studies focused on other types of voter identification requirements or, to address the issue of the low number of states enforcing strict ID laws, pooled together strict ID laws with other methods of voter identification. Estimates ranged from negative effects, overall or specifically for ethnic minorities (de Alth 2009; Vercellotti and Andersen 2009), to null (Muhlhausen and Sikich 2007; Mycoff, Wagner, and Wilson 2009; Rocha and Matsubayashi 2014) or even positive effects (Larocca and Klemanski 2011). Alvarez, Bailey, and Katz (2008, 2011) are the first to estimate the effects of strict ID laws specifically. They find a voter turnout difference of 2 percentage points between states with strict laws and states simply verifying voters' name. However, this difference is imprecisely estimated because the most recent data analyzed in the study are from 2006, the first general election in which strict ID laws were implemented. Using similar data, Erikson and Minnite (2009) conclude that the effect of strict ID laws is not significantly different from zero. The Government Accountability Office (2014) finds excess average turnout declines of up to 3.2 percentage points in two states that implemented strict photo ID laws between 2008 and 2012, compared with states that did not change their voter identification requirements, and larger drops among Blacks than among whites and Hispanics. Pryor, Herrick, and Davis (2019) and Hajnal, Lajevardi, and Nielson (2017) use data going until 2014, and they respectively report negative turnout effects of strict ID laws across all races, with disproportionately large and negative effects on the participation of Blacks and Hispanics.

We improve on this literature in three critical ways. First, existing estimates rely on state-level turnout aggregates, which make estimating heterogeneous effects by voter characteristics difficult, or on national surveys, which have limited representativeness and accuracy. National surveys' samples can fail to reflect state voting populations; voters' likelihood to respond can differ across groups: and their turnout data are based on self-reports. which are untrustworthy (Silver, Anderson, and Abramson 1986; Ansolabehere and Hersh 2012), or they use validation procedures which vary across states and over time (Grimmer et al. 2018). By contrast, we use administrative records of individual registration and turnout. Our data, collected by the political data vendor Catalist, combine official voter registration and turnout records from all states and cover the near universe of U.S. votingage individuals, 2008-2018, resulting in a total of more than 1.6 billion observations. This comprehensive individual-level data set enables us to accurately measure the effects of strict ID laws for different subgroups, which is critical given the concern of differential negative effect on ethnic minorities. In addition, the fact that the data follow individuals over time allows us to test the robustness of the results to specifications controlling for voter fixed effects and estimating the laws' effect on individuals who faced them for some but not all years.

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Differently from the rest of the literature, Hood and Bullock (2012) and Esposito, Focanti, and Hastings (2019) use individuallevel administrative data and DD designs like we do. They find that the participation of voters without photo ID decreased relative to voters with ID following the implementation of new voter identification requirements in Georgia and Rhode Island in 2008 and 2014, respectively. However, unlike our analysis, these studies are each restricted to a unique state. Because all individuals in their sample experienced the new law in the postperiod, these papers' estimates correspond to the differential effects of the law for people without photo ID. But people with ID may also be affected by changes in voter identification requirements. as discussed in Section II.B. Therefore, the relative decline in the participation of voters without ID reported in these papers is consistent with overall negative, null, or even positive turnout effects of the law change. By contrast with Hood and Bullock (2012) and Esposito, Focanti, and Hastings (2019), our estimates compare turnout changes in states that adopted a strict ID law with states that did not; therefore, they capture total, not differential effects. On the other hand, unlike these papers, our data do not allow us to distinguish people who were initially with or without ID.

Second, except for Esposito, Focanti, and Hastings (2019), prior research has examined the effects of ID laws using samples of registered citizens only, neglecting possible effects on voter registration (citizens who expect not to be able to vote may not register in the first place), and possibly obtaining downward-biased estimates of the laws' effects on turnout (if citizens deterred from registering and absent from the sample have a low propensity to vote). By contrast, Catalist data include unregistered voters, allowing us to measure effects on both registration and turnout.

Third, previous papers have used unconvincing or untestable identification assumptions, such as cross-sectional regressions or DD regressions with only two cross sections. We use the full length of our panel to show parallel pretrends and bring support for the identification assumption underlying our design; we demonstrate the robustness of our estimates to alternative specifications including state and voter controls, linear state time trends (or state-by-year fixed effects, for heterogeneous effects), and voter fixed effects; we show that our results hold when comparing voters in contiguous county-pairs straddling a state border, which further enhances the causal credibility of our estimates. This alternative estimation strategy requires restricting the sample to adjacent counties in neighboring states and including county-pair-by-year fixed effects. It is only possible because our data set provides the location of each individual and contains a sufficiently large number of people living in these counties, thanks to its nearly universal coverage of the U.S. voting-age population. We also show that our results remain very similar using novel estimators proposed by de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (forthcoming) to address possible shortcomings of two-way fixed effects estimators. Finally, while the control group of our main regressions includes all states without strict ID laws, we also estimate specifications distinguishing all types of identification requirements. These regressions allow us to compare strict ID laws to nonstrict laws, thus isolating the effect of the one characteristic of strict laws that is most susceptible of raising voting costs: requiring voters to show an ID to be able to vote. Again, we find effects that are close to null and not statistically significant.

Other studies also based on administrative data consider nonstrict ID law states, which request but do not require voters to present an ID and record ballots cast without identification. These studies use counts of people voting without ID to estimate how many voters would be disenfranchised by a shift to a strict ID law (Henninger, Meredith, and Morse forthcoming; Hoekstra and Koppa 2019). While ingenious, this method may severely overestimate the effects of strict laws. Many of the people voting without identification under a nonstrict law actually have a valid ID (Henninger, Meredith, and Morse forthcoming) and would bring it to the polls if required, and some of those without ID could acquire one before the election. Beyond the approximations required to estimate the direct effects of strict laws, descriptive analyses of the prevalence of voting without identification suffer from a second important limitation: they do not take into account indirect effects that may result from increased trust in the electoral process, anger against the laws, countermobilization efforts, and other mechanisms discussed in Section II.B. In contrast, we estimate the net overall effect of strict ID laws, and we exploit variation from all states that have adopted them.

Furthermore, we give evidence on both sides of the debate: while most existing research has focused on the effects of strict ID laws on participation, we also measure their

effects on voter fraud-the laws' ostensive target. Research has shown that interventions such as deploying observers (Ichino and Schündeln 2012) or informing voters (Vicente 2014) can successfully reduce fraud in contexts where it is prevalent. Even if fraud is much more limited in the United States, the extensive attention paid to existing cases could make any reduction consequential. We use two data sets listing cases of voter fraud: one by the Heritage Foundation, a conservative think tank, and another one by News21, a more liberal initiative. We find no significant negative effect in either data set. Irrespective of any effect on fraud, the very existence of stricter controls at polling places could be perceived as an improvement in election administration and increase voter confidence (Norris 2004; Atkeson and Saunders 2007). Stewart, Ansolabehere, and Persily (2016) uses the Survey of the Performance of American Elections to show that perceived occurrence of different types of fraud is similar in states with and without strict ID laws. Using the same survey, our DD estimates show no significant effect on this outcome. In addition, we use the American National Election Studies surveys to measure the laws' impact on citizens' belief that elections were fair. Again, we find no significant effect.

Our finding that voter ID laws have null effects is particularly salient in the United States, given the country's history of balancing the threat of fraud against the promise of enfranchisement. Well into the nineteenth century, political parties took advantage of the lack of control over the identity of people coming to vote. They hired large groups of "repeaters," who walked from one polling place to another and voted over and over again (Converse 1972). After 1890, many states addressed widespread fraud by requiring citizens to prove their identity and eligibility and sign a register before voting. Registration laws reduced voter impersonation, as voters' signatures could be verified on Election Day, and the registers were frequently purged of nonresidents and the deceased. However, they also created an additional burden for eligible voters, which has prevented many from participating in elections ever since (Nickerson 2015). Conversely, voting by mail, early voting, and other forms of convenience voting, which have become more widespread since the turn of the century, facilitate participation (e.g., Gerber, Huber, and Hill 2013) but are more susceptible to fraud than in-person voting on Election Day (Gronke et al. 2008).

Since about 2010, strict ID laws have become one of the country's most polarizing issues (Hasen 2012): they are supported by a large majority of the overall population, but with a growing gap between Republicans and Democrats (Stewart, Ansolabehere, and Persily 2016). Advocates and opponents of these laws disagree both on their benefits and costs.

On benefits, advocates insist that electoral fraud still exists today-about one-third of Americans believe it is widespread (Kobach 2011; Richman, Chattha, and Earnest 2014). They argue that strict ID laws are required to deter voter impersonation, double voting, and noncitizen voting, and to boost public confidence in the integrity of elections (von Spakovsky 2012). Opponents argue that voter fraud, extremely rare, results from individual cases of initiative or error rather than a coordinated effort (Minnite, 2010: Cottrell, Herron, and Westwood 2018). On costs, advocates of strict laws argue that they impose only a minor burden on voters. as proof of identification is also required for other activities, like cashing a check. They point to the fact that most other Western democracies also require voters to show identification (Commission on Federal Election Reform 2005). Opponents observe that unlike other countries, the United States does not require its citizens to hold a national ID card (Schaffer and Wang 2009), and as a result 5% to 19% of eligible voters (depending on the state) lack any accepted form of identification (Government Accountability Office 2014; Ansolabehere and Hersh 2017). They see these laws as a deliberate and politically motivated attempt to disenfranchise minorities, akin to the poll taxes, literacy tests, and other Jim Crow legislation prevalent before the 1965 Voting Rights Act (Rocha and Matsubayashi 2014). The laws are enforced more stringently against Blacks and Hispanics (Atkeson et al. 2014; White, Nathan, and Faller 2015), who favor the Democratic Party and are less likely to hold an ID in the first place.

Our results suggest that efforts both to safeguard electoral integrity and enfranchise more voters may be better served through other reforms.

The remainder of the article is organized as follows. Section II summarizes the history of strict ID laws and outlines the main mechanisms through which these laws may affect participation and other outcomes. Section III provides more information on Catalist's voter-level panel data and the other data sets we use. Section IV presents the empirical specifications and results. Section V concludes.

II. RESEARCH SETTING

II.A. History of Strict ID Laws

In the United States, laws requiring voters to present a document verifying their identity are relatively recent. In 1950, South Carolina became the first state to request-but not requirevoters present an ID at the polls. By 2000, 14 states had adopted a similar law, under both Democratic and Republican majorities. without generating much discussion. New voter identification requirements were adopted as part of election reform efforts following the disputed 2000 presidential election and the ensuing anxiety on electoral integrity (Minnite 2012). In 2002, Congress passed the Help America Vote Act, which prescribed that first-time voters who registered by mail show identification at the polling place, but refrained from establishing uniform ID requirements for other voters (Ansolabehere 2008). In 2005, the bipartisan Commission on Federal Election Reform recommended the adoption, at the federal level, of a photo voter ID card (Carter-Baker Commission 2005). Soon afterward, Georgia and Indiana became the first states to require a photo ID at the polls. In 2008, the Supreme Court upheld the constitutionality of Indiana's law in Crawford v. Marion County, thereby paving the way for similarly restrictive ID laws in other states, mostly by Republican-controlled legislatures (Hicks et al., 2015; Biggers and Hanmer 2017).

Following the National Conference of State Legislatures (NCSL), we distinguish between two main categories of ID laws: strict and nonstrict. In states with nonstrict laws, voters are asked to show an ID, but are still allowed to vote without identification. For their ballot to be counted, voters without ID simply need to sign an affidavit identifying themselves (in most states) or have their signature checked against the voter registration record. In contrast, strict ID laws (such as Georgia's and Indiana's current laws) require all voters to show an ID. People without one may cast a provisional ballot, but this ballot will only be counted if they return within a few days to the polling place, election board, or county election office to show an accepted form of identification. In other words, citizens without ID are prevented from voting.²

2. The distinction between states requesting versus requiring an ID is generally straightforward. However, one state is at the limit between these two categories: Alabama. The NCSL classifies Alabama's ID law as nonstrict because people without ID can vote if they are identified by two election officials. It remains Strict ID laws further differ by the type of ID they consider valid. Although some accept a wide range of documents, including utility bills or bank statements, most require a document bearing a photo, such as a driver's license, state-issued ID card, or U.S. passport, and are therefore referred to as strict photo ID laws. Online Appendix Table A.1 details the requirements associated with each strict ID law enforced in at least one general election.

Due to their restrictive nature, strict ID laws are very controversial, and they have come under immense scrutiny by state and federal courts, as well as by the U.S. Supreme Court. In addition to its 2008 judgment ruling Indiana's strict ID law as constitutional, the Supreme Court effectively upheld a federal court's ruling that Wisconsin's strict ID law was constitutional when it rejected a challenge to this law in 2015. By contrast, in 2017, it declined to hear an appeal to a federal court's striking down a strict law adopted but not implemented by North Carolina, thereby allowing the federal court's decision to stand. Beyond courtrooms, strict ID laws have generated heated partisan debates and received large media coverage and public interest.

States without any ID law do not request, let alone require, any identification document. They verify voters' identity in one of the two following ways. Some states ask voters to sign the poll book or an affidavit of vote eligibility and, in some cases, ask poll workers to verify that this signature matches the one on file. Others simply check voters' name (and sometimes other personal information such as voters' address) against a list of eligible citizens.³

Online Appendix Figures A.1 and A.2 plot the overall distribution of the four types of voter identification requirements (strict ID law, nonstrict law, signature, and checking voters' name) as well as the requirements enforced in each state and general election since 2004. The most important shift in this period is the implementation of strict ID laws by a growing number of states and the simultaneous decline in the number of states with nondocumentary ID requirements.

that voters without ID who are not identified by election officials are prevented from voting. For that reason, some studies that otherwise follow the NCSL classification count Alabama as a strict ID law state (e.g., Highton 2017; Kuk, Hajnal, and Lajevardi 2020). Relabeling Alabama's law as strict would not affect our results, since we control for state fixed effects and Alabama's request to show identification dates back to 2003 (i.e., before our sample period).

^{3.} See https://www.ncsl.org/research/elections-and-campaigns/voter-verification-without-id-documents.aspx.

II.B. Conceptual Framework

Strict ID laws are commonly hypothesized to have negative turnout effects by increasing the cost of voting (Highton 2017), which is a low-benefit activity (Downs 1957; Riker and Ordeshook 1968). However, other indirect mechanisms make the overall effects of the laws ex ante ambiguous.

To the extent that strict ID laws decrease participation by preventing eligible citizens without ID from voting, minority voters and other groups who are less likely to have an ID should be the most affected. However, this effect will be reduced if people without ID are willing to spend the time (and sometimes the money) required to obtain an ID or if their propensity to vote is low even without any ID requirement.

Beyond administrative costs, strict ID laws also create information costs for all voters. Whether or not they have an ID, all voters need to be aware that a new law was implemented and they need to learn which forms of identification are accepted. If they are unaware of the ID requirement, voters who have a valid ID may not bring it to the polling station. In that case, they will be asked to return with the document for their vote to be counted, and only a subset of voters will do so. Others may wrongly believe their ID is not accepted and thus refrain from even trying to vote.

Several forces may reduce these costs or mitigate their effects. First, states implementing strict ID laws may conduct educational campaigns to inform voters and they may facilitate the acquisition of state-issued IDs (e.g., Hopkins et al. 2017; Bright and Lynch 2017). Second, Democratic candidates and interest groups opposing strict ID laws may respond strategically by conducting outreach information programs and helping people obtain proper identification (Citrin, Green, and Levy 2014; Neiheisel and Horner 2019). In addition, they may use the laws as an argument to mobilize their entire base, including voters who are not personally affected (Endres and Panagopoulos 2018). Third, media coverage asserting that the goal of the laws is to disenfranchise some citizens may cause anger among voters who feel their group or their party is targeted, thus increasing turnout among these voters (Valentino and Neuner 2017; Smith, Jo, and Lazer 2020).

The net effects of the laws on Democratic turnout may be null or even positive if these different responses are sufficiently strong. Differences across groups of voters in the strength of the mechanisms through which strict ID laws affect turnout might generate heterogeneous effects. In addition, these effects may change over time. Early declines in participation may subside as voters learn about the laws, or negative effects may appear after a few years if countermobilization weakens gradually.

On the opposite side of the aisle, Republican voters may become more likely to vote if the laws increase their confidence in election integrity (Endres and Panagopoulos 2018) and if enhanced trust in elections, in turn, boosts participation. The decision of the Supreme Court in *Crawford v. Marion County* draws the latter connection when it asserts that perceptions of voter fraud depress turnout, but we are not aware of any empirical evidence establishing this relationship. An experiment by Gerber et al. (2013) studies beliefs on ballot secrecy, not voter fraud, and shows that improving these beliefs causes participation to increase. It is possible that other policies also affect turnout if they improve trust in elections.

Finally, the participation of Democrats and Republicans may endogenously adjust to the expected level of participation of the other side, a mechanism highlighted in group rule-utilitarian models by Coate and Conlin (2004) and Feddersen and Sandroni (2006). Such strategic responses may amplify the aforementioned effects, whether they are positive or negative. For instance, Republicans may be less likely to vote if they expect the laws to reduce the participation of Democrats and infer that the number of votes required to obtain a plurality is now lower.

Beyond voter turnout, the laws may also affect vote shares and election outcomes, if they have different overall effects on the participation of Democratic- and Republican-leaning voters. Moreover, strict ID laws have become such a politicized issue that some voters in implementing states may change the orientation of their vote if, on this particular issue, they disagree with the party they usually vote for. Substantial effects on voter fraud are perhaps less likely, given the low baseline level of fraud (Minnite, 2010).

We estimate the impact of strict ID laws on these different outcomes (participation, vote shares, and voter fraud), and we unpack net effects on participation by examining subsets of voters defined by race or party affiliation, studying changes in effect size over time, and checking whether the laws generated backlash or countermobilization efforts.

III. DATA

III.A. Catalist Voter-Level Panel Data

We measure voter turnout and registration using a novel individual-level panel data set collected by Catalist, a U.S. company that provides data and data-related services to progressive organizations and has a long history of collaborating with academics (e.g., Nickerson and Rogers 2014; Hersh and Nall 2016). The panel covers the near universe of the U.S. voting-eligible population in the 2008, 2010, 2012, 2014, 2016, and 2018 presidential and midterm elections, resulting in a total of about 1.6 billion observations.

For each voter-election, the data report state and county of residence, registration status, voter turnout, and party affiliation (in the 30 states in which it is available). The data also contain age, race, and gender. These demographic characteristics are available for nearly all voters and have been shown to be very reliable (Fraga 2016, 2018). In eight states-Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Tennessee-Catalist uses self-reports of race that come directly from the voter rolls. For unregistered voters in these eight states and all voters in other states. Catalist estimates race using voters' full names, sociodemographic information about their census block groups or tracts of residence, and, where available, self-reported race from commercial and nonprofit databases. According to Fraga (2018), the average accuracy of Catalist's proprietary race model is very high (93.1%), with race-specific accuracy of 77.1%, 79.8%, and 97.8% for Black, Hispanic, and white voters, respectively.⁴ Next to race, the Catalist data contain a categorical variable for the degree of confidence in a voter's race estimate (featuring five possible values: "highly likely," "likely," "possibly," "uncoded," and "no code assigned"). For example, Catalist predicts some voters' races with a relatively higher degree of confidence when they reside in racially homogeneous areas or when they carry racially distinctive names (Hersh 2015). Online Appendix Table A.11 shows that race-specific impact estimates remain very close to those of Table III if we restrict the sample to voters

^{4.} These estimates indicate the fraction of 2016 CCES respondents matched to Catalist registration records with 90% match confidence or greater and self-identifying with the indicated racial/ethnic group who have the same race/ethnicity listed in the Catalist database.

whose race is estimated with highest confidence. This indicates potential race misclassification is unlikely to bias our results.

Catalist's data on registered voters primarily come from official voter registration and turnout records from all states. In addition, about 55 million unregistered voters are covered thanks to three different data sources. First, Catalist keeps track of voters present in past voter files and absent from the most recent one. Second, it identifies unregistered voters using information from data aggregation firms (so-called commercial data) and customer files of retailers and direct marketing companies. Finally, unregistered voters include individuals who moved to a state without registering, according to commercial data or USPS National Change of Address data (NCOALink[®]).

Despite Catalist's efforts and multiple data sources, coverage of the unregistered population is likely incomplete: Jackman and Spahn (2018) estimate that at least 11% of the adult citizenry and a disproportionate share of minority voters-do not appear in commercial voter lists like Catalist's. This generates the following risk. Suppose some voters only register absent strict ID laws. We will observe all these marginal registrants in states without ID requirements-as the data cover the universe of the registered population, but might only observe a subset of them in states with ID requirements, as they would not register in these states and coverage of the unregistered population is incomplete. Under this scenario, our estimated registration effects would be biased upward as we would underestimate the share of unregistered voters in state-years with strict ID laws. Reassuringly, Online Appendix Table A.3 shows that the probability of voters appearing in or disappearing from the Catalist data is (conditionally) orthogonal to the presence of strict ID laws. Specifications controlling for voter fixed effects further assuage this concern because they estimate the effects on individuals who faced a strict ID law for some but not all years. These individuals are present in our sample before the implementation of the law. reducing the risk of sample selection bias.

Another potential issue is that some unregistered individuals in Catalist data may be ineligible to vote. Yet it seems implausible that the implementation of strict ID laws correlates systematically with the presence of ineligible voters in the data. In addition, Table I and Online Appendix Table A.12 show that our results hold when we restrict attention to registered voters, all of whom should be voting-eligible individuals. Furthermore, Online Appendix Figure A.3 plots the relationship between total state-by-year headcounts in the Catalist data and estimates of the citizen voting-age population from the U.S. Census Bureau. The nearly perfect linear correlation between the two variables shown in the figure ($R^2 = 0.986$) indicates that variations in headcounts in the Catalist data across states and years nearly perfectly mirror underlying fluctuations in the citizen voting-age population, thus alleviating concerns that our data do not adequately reflect the population of interest.

Further details on the Catalist panel data are given in Online Appendix 2.

III.B. Data on Mobilization and Campaign Contributions

Measures of campaign contact and voter engagement come from the 2006–2018 postelectoral Cooperative Congressional Election Study (CCES) surveys. We use questions on whether the interviewee was contacted by a campaign, donated to a candidate or campaign (and how much they contributed), attended a political meeting, posted a campaign sign, or volunteered for a campaign.⁵ We construct a summary index of voter activity, defined to be the equally weighted average of the z-scores of its components. An important caveat is that survey data on campaign activities may suffer from misreporting, for instance because of social desirability bias or misremembering. Misreporting would bias our estimates if its prevalence changes differentially across treated and control states following the implementation of strict ID laws.

Information on state-level campaign contributions is from Bonica (2016)'s Database on Ideology, Money in Politics, and Elections (DIME), version 3.0. The data contain all political contributions recorded by the Federal Election Commission, 2004–2018. We compute the total dollar-value contributed by residents of each state in each election cycle, normalize it by the state population in that election year, and take the log, to reduce the impact of outlier states like New York.

Data on total expenditures and campaign-related expenditures by candidates running for the U.S. House of Representatives,

^{5.} For all survey data we use, exact questions are detailed in Online Appendix 3. Beyond questions on campaign contact and voter engagement, we use the CCES surveys to check the robustness of the effects on turnout estimated with the Catalist data. These results are shown in Online Appendix Tables A.13 and A.14 and discussed in Section IV.B.

2004–2018, are also based on records from the Federal Election Commission and compiled by the Center for Responsive Politics. We also obtained data on estimated TV ad expenditures spanning most down-ballot, state, and federal electoral races held in 2004 and 2008–2018 from the Wisconsin Advertising Project and the Wesleyan Media Project.⁶ Similarly as for total contributions, we measure total expenditures, campaign-related expenditures, and TV ad expenditures in logs after normalizing by the state population.

III.C. Voter Fraud

Measuring voter fraud represents a challenge, as federal and state agencies vary in the extent to which they collect and share information on it (Government Accountability Office 2014).

We found two data sets covering reported cases of voter fraud. The first is by News21, an investigative project funded by the Carnegie Corporation and the John S. and James L. Knight Foundation. For the project, 24 students from 11 U.S. universities submitted more than 2,000 public-records requests and combed through nearly 5,000 court documents, official records, and media reports about voter fraud. The result is a collection of 2,068 cases of suspected voter fraud reported from 2000 through 2012. The database is admittedly incomplete, as the research team received partial or no responses from several states, and even replying jurisdictions may have failed to include some cases.⁷ The second data set, by the Heritage Foundation, includes 1,277 proven cases. Again, the foundation's website indicates that this database is nonexhaustive.⁸

We define two outcomes separately in either data set: the number of fraud cases documented in each state-year per 100,000 residents, and the number of cases potentially preventable by

6. See https://elections.wisc.edu/wisconsin-advertising-project/ and https:// mediaproject.wesleyan.edu/. Estimated expenditures on TV ads for down-ballot races are available for the 2010–2018 elections, while expenditures for congressional, gubernatorial, and presidential races are available starting from 2004. To focus on general elections (instead of primaries), we restrict attention to TV ad expenditures occurring in even-numbered years from June onward.

7. Further details on News21 are available at https://votingrights. news21.com/article/election-fraud-explainer/.

8. See https://www.heritage.org/voterfraud.

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strict identification requirements.⁹ We restrict attention to cases of fraud reported in or after 2004, the last election year before the implementation of the country's first strict ID law.

In both data sets, the summaries are typically insufficient to reconstruct the election year the alleged fraud took place. We thus take the reported years as given. We assign records with odd years (i.e., years in which no general election took place) to the previous year's treatment status and covariates.

Despite their limitations, these two data sets allow us to propose the first estimates of the effect of strict ID laws on voter fraud.

III.D. Surveys on Perceived Election Integrity

To assess if strict identification laws alter the perceived integrity of the electoral process, we use the 2004, 2012, and 2016 waves of the American National Election Studies (ANES) survey and the 2008–2016 waves of the Survey of the Performance of American Elections (SPAE). From the ANES, we construct a dummy identifying respondents who think the past election was very fair or fair. From the SPAE, we construct separate dummy outcomes for whether the respondent believes the following frauds happen commonly or occasionally: pretending to be another voter, casting multiple votes, noncitizens casting a ballot, casting an absentee ballot intended for another person, officials changing the vote counts, and stealing or tampering with ballots. As with voter activity, we construct a standardized index of perceived election integrity based on the individual voter fraud outcomes.

III.E. Calendars of Voter Identification Requirements, Election Laws, and State Party Control

We identify the type of voter identification requirement enforced in each state-year based on information provided by the NCSL. We also use the NCSL, together with data from Biggers and Hanmer (2015), to construct the following state-level covariates. We build state-by-year indicators for the availability of no-excuse absentee voting, early voting, all-mail voting, and Election Day registration. Partisan control of the state legislature is identified by three dummies indicating whether the state

^{9.} We classify voter impersonation, duplicate voting, false registrations, and ineligible voting as preventable frauds. Other categories are buying votes, altering the vote counts, fraudulent use or application of absentee ballots, illegal assistance at the polls, and intimidation.

legislature was controlled by Republicans, Democrats, or split among the two main parties.¹⁰ Similarly, the party affiliation of the governor can take three possible values: Democratic, Republican, and independent.¹¹

IV. RESULTS

IV.A. Impact on Turnout

We first estimate the average effect of strict ID laws on all voters with DD specifications of the following form:

(1)
$$Y_{ist} = \beta I D_{st} + X'_{ist} \gamma + \alpha_s + \delta_t + \mu_{ist},$$

where Y_{ist} is a dummy equal to 1 if individual *i* in state *s* voted in election year *t*, ID_{st} is a dummy for whether the state used a strict ID law in that year, X_{ist} is a vector of individual and state controls, α_s are state fixed effects, and δ_t election year fixed effects. Our individual controls include both time-invariant (gender and race-by-state fixed effects) and time-varying covariates (age and race-by-year fixed effects). All our state controls are time dependent (partisan control of the state legislature, governor's party, and other election administration rules affecting turnout: no-excuse absentee voting, early voting, same-day registration, and all-mail voting). Since the treatment varies at the state-year level, we follow Bertrand, Duflo, and Mullainathan (2004) and conservatively cluster standard errors by state.¹²

The coefficient of interest, β , measures the difference in average participation between states with and without strict ID laws (henceforth, treated and control states), conditional on controls. This represents the causal effect of the laws under the assumption that treated and control states were on parallel trends, so that year-to-year turnout changes in control states correspond to the counterfactual evolution in treated states, had they not implemented the law.

11. We include the District of Columbia in the final category.

12. Online Appendix Tables A.32–A.36 and A.37–A.41 show that the stateclustered asymptotic p-values of Tables I–V's coefficients are very close both to their wild cluster bootstrap counterparts (Cameron, Gelbach, and Miller 2008) and to the randomization inference p-values based either on t-statistics or on regression coefficients (MacKinnon and Webb 2020).

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^{10.} We include Nebraska's nonpartisan state legislature in the final category.

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		1(Vo	ted)			1(Regis	stered)	
	(1)	(2)	(3)	(4)	(2)	(9)	(2)	(8)
Panel A: Only registered 1 1(Strict ID law)	voters -0.001 (0.013)	-0.001 (0.011)	-0.011 (0.019)	-0.008 (0.017)	I	I	I	I
Outcome mean	0.620	0.620	0.620	0.620				
Panel B: Registered and v 1(Strict ID law)	inregistered v -0.007 (0.015)	$\begin{array}{c} ext{oters} \\ -0.001 \\ (0.012) \end{array}$	-0.008 (0.014)	-0.001 (0.014)	-0.015 (0.012)	-0.004 (0.011)	-0.008 (0.007)	-0.001 (0.011)
Outcome mean	0.428	0.428	0.428	0.428	0.686	0.686	0.686	0.686
Year FEs State FEs State & voter controls State linear trends Voter FEs	>>	>>>	>>>>	>>> >	>>	>>>	>>>>	>>> >

Notes. Each cell reports estimates from a separate regression run on the Catalist data. The sample for Panels A and B consists of, respectively, registered voters and both registered and unregistered voters. The sample size in the two panels is 1,100,864,799 and 1,604,600,607 respectively. State controls are dummies for the availability of no-excuse absentee voting, early in-person voting, all-mail voting, and Election Day registration, along with indicators for the partisan composition of the state legislature and the governor's party as of Election Day. Voter controls are gender, dummies for the voter's age ventile (defined in the full panel data and including an additional dummy for voters with missing age information), and dummies for whether the voter is Black, Hispanic, or of other non-Hispanic (or unknown) race, along with interactions of these race dummies with states and years. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.). *** p < .05, * p < .05, * p < .10.

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The results from equation (1) are presented in Table I. Panel A restricts the sample to registered citizens, following the existing literature. Using a specification with state and election-year fixed effects but without any other controls, we obtain an effect close to null and not statistically significant (column (1)). Angrist and Pischke (2015) suggest that credible DD estimates should be robust to the inclusion or omission of covariates and linear state time trends. Accordingly, we test the robustness of our result to three additional specifications.

Namely, our second specification includes individual and state controls. Our third specification also adds state time trends, to allow treated and control states to be on differential linear trajectories. Although controlling for state time trends relaxes our identification assumption, it also decreases the precision and accuracy of the estimates for at least two reasons. First and most important. using linear time trends in DD specifications is a source of bias. Neumark, Salas, and Wascher (2014), Meer and West (2016), and Goodman-Bacon (2019) note that with time-varying treatment effects, linear time trends tend to absorb part of the effect of interest (i.e., to "overfit"), thus leading to attenuation bias. Goodman-Bacon (2019) also points out that controlling for time trends implicitly overweights observations at the end of the panel, adding another source of bias (of a priori unknown direction and magnitude). Second, controlling for linear trends reduces the available treatment variation, making resulting estimates less precise than undetrended ones. These caveats mean that results obtained using the third specification should be interpreted with caution. Our fourth and most demanding specification includes voter fixed effects. While identification continues to rely on states that changed voter identification requirements, this specification estimates the effect using only within-individual variation, out of voters who faced a strict ID law for some but not all years (because they experienced a change in their state's law or because they moved across states with different voter identification requirements and their state of origin or destination is one of the states that adopted a strict ID law after 2008). Corresponding estimates are unaffected by the possibility that strict ID laws changed people's likelihood to appear in the Catalist sample, which is otherwise a possible source of bias, as discussed in Section III.A. We find no significant effect in any of these alternative specifications (columns (2) through (4)).

In Panel B, we use the same specifications as in Panel A but include both registered and unregistered individuals in the sample, which the existing literature has typically failed to do. This is important, first, because effects on the turnout of registered citizens shown in Panel A miss possible effects on registration: although strict ID laws do not change registration requirements, citizens who expect not to be able to vote might decide not to register in the first place, and citizens who stop voting are more likely to be purged from voter rolls. In addition, restricting the sample to registered voters might lead us to underestimate the laws' true effects on turnout if they decrease registration of citizens with lower propensity to vote than the average registrant. In other words, the estimated null effect on registered voters' turnout could reflect two negative effects: decreased registration (leading to increased turnout of registered citizens, if those deterred from registering have low propensity to vote) and decreased turnout of voters whose registration is unaffected. The inclusion of both registered and unregistered individuals in Panel B addresses both issues. The results reported in this panel are thus our main estimates of the effects of strict ID laws on overall participation.

Panel B considers two outcomes: unconditional turnout (equal to 1 if the individual is registered and votes, and 0 otherwise), in columns (1)–(4), and registration, in columns (5)–(8). The effects of strict ID laws on both outcomes are close to null, and point estimates are not statistically significant in any specification. Based on our most demanding specification controlling for state, year, and voter fixed effects, along with state and voter controls, and considering the lower bound of the 95% confidence interval, we can rule out that strict ID laws reduce aggregate registration and turnout by more than 2.3 and 3.0 percentage points, respectively (columns (4) and (8)). The precision of our estimates is comparable across specifications.

In Online Appendix Table A.4, we implement an alternative strategy based on Dube, Lester, and Reich (2010). We restrict our sample to adjacent counties in neighboring states to compare voters in contiguous county pairs straddling a state border. Focusing on voters living in adjacent counties across state borders (and controlling for county-pair-by-year fixed effects) further enhances the causal credibility of our estimates. In this table as well as in the remaining analysis on turnout, we use unconditional turnout on the full sample as our outcome, unless specified otherwise. Again, we find no effect of strict ID laws on turnout. Considering the lower bound of the 95% confidence interval, we can rule out that strict ID laws reduce overall turnout by more than 0.5 percentage points.

	(1)	(2)	(3)	(4)
Panel A: Ballots cast/VE	P (McDonald's o	lata)		
1(Strict ID law)	0.006 (0.012)	0.006 (0.013)	0.001 (0.012)	0.002 (0.014)
Outcome mean N	$\begin{array}{c} 0.528\\ 408 \end{array}$	$\begin{array}{c} 0.528\\ 408 \end{array}$	$\begin{array}{c} 0.517\\ 408 \end{array}$	$\begin{array}{c} 0.517\\ 408 \end{array}$
Year FEs State FEs State-year controls VEP weights State linear trends		 	$\begin{array}{c} \checkmark\\ \checkmark\\ \checkmark\\ \checkmark\\ \checkmark\end{array}$	$\begin{array}{c} \checkmark\\ $
Panel B: Democratic two	-party vote sha	re		
1(Strict ID law)	0.001 (0.020)	0.009 (0.017)	0.005 (0.010)	-
Outcome mean N	$0.520 \\ 3,684$	$0.520 \\ 3,684$	$0.520 \\ 3,684$	
Year FEs State FEs State-year controls State linear trends		 	\checkmark \checkmark \checkmark	

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EFFECTS OF STRICT ID LAWS ON AGGREGATE OUTCOMES

Table II, Panel A, shows the robustness of the null result to different data. Specifically, instead of using individual-level turnout data, we use McDonald's aggregate state-level estimates, whose denominator for turnout excludes noncitizens and ineligible felons (McDonald and Popkin 2001; McDonald 2002, 2010). Because the share of ineligible voters fluctuates wildly across states and over time, McDonald's turnout estimates are considered more reliable than alternative measures using the Census Bureau voting-age (or citizen voting-age) population, and are widely used (e.g., Leighley and Nagler 2013; Burden, 2014; Taylor et al. 2015; Fraga 2018). We use McDonald's data for 2004– 2018, since 2004 is the last year before Arizona, Indiana, and Ohio became the first states in the country to implement a strict ID

Notes. Panel A reports estimated turnout effects based on McDonald's state turnout data, 2004–2018 (2004 is the last year before strict ID laws were implemented). Turnout is defined as the ratio between ballots cast for the highest office on the ballot and the voting-eligible population (VEP) in a given state-year. Panel B reports estimated effects on the Democratic two-party vote share based on constituency-level election results, 2004–2018, collected by the MIT Election Data and Science Lab. The sample in Panel B pools together congressional and presidential elections; units of observation are state-years (or D.C.) or congressional district-years. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.). *** p < .01, ** p < .05, * p < .10.

law.¹³ Also this strategy confirms the null result. Similarly, we do not find any significant effect on aggregate state-level registration rates, 2008–2018, computed as counts of registered voters in the Catalist data divided by McDonald's figures for the voting-age or voting-eligible population (Online Appendix Table A.6).

While regressions with time and state fixed effects in the form of equation (1) are widely used, a recent literature documents possible shortcomings of these two-way fixed effects specifications (Borusvak and Jaravel 2017: Goodman-Bacon 2019: de Chaisemartin and D'Haultfœuille 2020a: Callaway and Sant'Anna 2020: Sun and Abraham forthcoming). In particular, de Chaisemartin and D'Haultfœuille (2020a) show that the underlying estimator can be written as a weighted sum of the average treatment effects in each state and period, with some possibly negative weights. When treatment effects vary over time or across states, negative weights may result in a negative estimate even if all the average treatment effects are positive. Reassuringly, using de Chaisemartin and D'Haultfœuille (2020a)'s twowavfeweights Stata command, we find that less than one-third of the weights are negative, and their sum is only 0.087. Furthermore, Online Appendix Table A.7, Panel A (resp. A.8, Panel A) checks the robustness of the results obtained with the Catalist data (resp. McDonald's aggregate state-level turnout estimates) to alternative estimators proposed by de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (forthcoming). Columns (1) and (2) report the estimated effects in the first election after the implementation of strict ID laws, and columns (3) and (4) the aggregate effects across all elections post implementation. The point estimates are very close in magnitude to our baseline estimates, and none are statistically significant.¹⁴

13. As shown in Online Appendix Table A.5, we obtain very similar results when using the voting-age population instead of the voting-eligible population as the denominator (Panel A, columns (5) through (8)) or when using McDonald's turnout data for 2008–2018, the period corresponding to the Catalist sample, instead of 2004–2018 (Panel B).

14. We use the Stata *did_multiplegt* command to compute de Chaisemartin and D'Haultfœuille (2020a)'s estimator and run a linear regression interacting relative-year fixed effects with cohort fixed effects to compute the estimator by Sun and Abraham (forthcoming). Our design includes three cohorts, each designating a group of states which first implemented their strict ID law in the same year: 2012, 2014, and 2016. Cohort-specific relative-year fixed effects are then aggregated using weights which correspond to the share of observations of that Finally, to corroborate the validity of the parallel-trend assumption, we plot estimates of β_{τ} 's from the following leads-and-lags regression:

(2)
$$Y_{ist} = \sum_{\tau} \beta_{\tau} I D_{st}^{\tau} + X_{ist}^{'} \gamma + \alpha_s + \delta_t + \mu_{ist},$$

where ID_{st}^{τ} is a dummy equal to 1 if election year t occurs τ elections after state s first implemented its strict ID law. τ ranges between -4 and +3. The β_{τ} 's measure the difference in participation between treated and control states before ($\tau < 0$) or after ($\tau \ge 0$) the first implementation of the law, conditional on controls. All coefficients are normalized relative to the last pretreatment election ($\tau = -1$).

Figure I shows that turnout does not change differentially in treated states after the first implementation of the law, consistent with the estimates in Table I. Corroborating our identification strategy, we also find no evidence of differential trends before

relative year which fall in that cohort. Sun and Abraham (forthcoming)'s method does not provide a clear way to aggregate relative-year fixed effects across years, so we only show the effects in the first election after implementation of the law. We compare the estimates obtained with these two estimators to two sets of estimates obtained with the two-way fixed effects estimator: estimates based on the full sample, and estimates obtained after dropping always-treated states and transforming our data into a staggered design, where states always remain treated after they first adopted a strict ID law. To do so, we recode the reversals that took place in North Dakota and Texas by assigning positive treatments to the corresponding years. Indeed, negative weights that arise with the two-way fixed effects estimator are only on always-treated states, and both de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (forthcoming)'s estimators drop always-treated states. In addition, Sun and Abraham (forthcoming) focus on staggered designs, and thus require the aforementioned transformation. In contrast, de Chaisemartin and D'Haultfœuille (2020a)'s estimator of the effect immediately following the change in treatment applies to any two-way fixed effects regressions. not just to those with staggered adoption, so the corresponding estimates use the untransformed data. The *did_multiplegt* command collapses data at the cell level (i.e., by state-year) and computes bootstrap standard errors by resampling entire clusters (states). The command can accommodate covariates, which are averaged at the cell level. However, due to the state-level bootstrap resampling, including a large number of controls may cause some bootstrap replications to run regressions with more covariates than observations. To avoid this issue, when using *did_multiplegt*, we only include state-level controls (i.e., we do not include the voter-level controls race-by-year, race-by-state, age ventile, and gender fixed effects). To ensure comparability across methods, all other estimates in the table similarly control for state-level covariates, but not for voter-level ones.





Event Study Graph of the Turnout Effects of Strict ID Laws

The figure plots event study estimates and 95% confidence intervals from a regression (in the form of equation (2)) run on all registered and unregistered voters. The sample includes treated and control states. To avoid picking up variation from 2016 North Dakota, 2016 Texas, and 2018 Texas (which, unlike 2014 and 2018 North Dakota and 2014 Texas, did not enforce a strict law), we define $ID_{ND,2016}^{r=1} = ID_{TX,2016}^{r=2} = ID_{TX,2018}^{r=2} = 0$.

implementation: though strict ID laws are not randomly assigned to states (Online Appendix Table A.2 shows slightly lower turnout level in treated states), their implementation does not correlate with differential pretrends in turnout.¹⁵

IV.B. Heterogeneity Analysis

The null effects of strict ID laws on overall registration and turnout could potentially mask negative effects on minorities (who are less likely to have an accepted ID) and positive effects on whites, or differences along other dimensions. To assess treatment impact heterogeneity, we estimate regressions of the

^{15.} Online Appendix Figure A.4 reports event study graphs based on McDonald's turnout data, 2008–2018. The resulting plots are remarkably similar to the main event study graph based on the individual-level Catalist data (Figure I).

following form:

(3)
$$Y_{ist} = ID_{st} \times Z_{ist}^{'}\lambda + Z_{ist}^{'}\eta + X_{ist}^{'}\gamma + \alpha_s + \delta_t + \mu_{ist},$$

where Z_{ist} is the vector of characteristics along which we allow for heterogeneity in the treatment effects. Because this specification does not include ID_{st} uninteracted, the coefficients on the interactions between ID_{st} and Z_{ist} directly indicate the effects of strict ID laws on the corresponding groups. In addition, we test for heterogeneous effects across groups.

Table III reports the results for the main dimension of heterogeneity: race. We use the same specifications as in Table I, with two differences. First, all specifications control for race-by-year and race-by-state fixed effects, to ensure that the interaction between ID_{st} and race dummies is not biased by race-specific shocks occurring in a given year (across all states) or in a given state (across all years). Second, in column (4), we control for state-by-year fixed effects instead of state time trends, thereby using a triple-difference framework. The inclusion of state-byyear fixed effects allows us to account for a larger set of possible confounders. It precludes estimating the overall effect of the laws, which varies at this level, but not differential effects by race.

As shown in Panel A, in all specifications the point estimates are close to null for whites and positive but statistically nonsignificant for nonwhites. We cannot reject the null of identical effects on both groups. Considering the lower bounds of the 95% confidence intervals of the differential effects estimated using our voter fixed effects specification (column (5)), we can reject that strict ID laws decrease nonwhite turnout (relative to white turnout) by more than 0.5 percentage points. Various other policies and institutions have been shown to induce substantially larger differential turnout effects. For example, Cantoni (2020) estimates that the disproportionate effect of distance to polling location widens the turnout gap between whites and nonwhites by 1.6 to 4.0 percentage points, depending on the election; White (2019) shows that receiving a short jail sentence causes Black turnout to drop in the next election by approximately 13 percentage points, with small and nonsignificant effects on white turnout; and Fraga (2016) reports that increasing the within-district share of a race group from 10% to 50% would raise Black and Hispanic general election turnout by 9.3 to 6.4 percentage points, respectively, while the predicted effect on white turnout is 0.6 percentage points.

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		Ou	tcome: 1(V	oted)	
	Outcome mean	е	Impact e	estimates	
	(1)	(2)	(3)	(4)	(5)
Panel A: Whites versus nonwh	ites				
1(Strict ID law) \times white	0.458	$-0.006 \\ (0.015)$	-0.003 (0.014)		-0.005 (0.016)
1(Strict ID law) \times nonwhite	0.340	$0.006 \\ (0.014)$	0.006 (0.010)		0.009 (0.012)
$\beta^{\text{nonwhite}} - \beta^{\text{white}}$		0.013 (0.008)	0.010 (0.007)	0.007 (0.007)	0.014 (0.009)
Panel B: By detailed race					
1(Strict ID law) \times white	0.458	$-0.006 \\ (0.015)$	-0.003 (0.014)		$-0.005 \\ (0.016)$
1(Strict ID law) \times Hispanic	0.295	0.025* (0.015)	0.022** (0.008)	*	0.026** (0.010)
$1(Strict \ ID \ law) \times Black$	0.380	-0.009 (0.014)	-0.006 (0.013)		-0.004 (0.014)
$1($ Strict ID law $) \times $ other race	0.330	0.013 (0.028)	$0.007 \\ (0.022)$		0.008 (0.024)
$\beta^{\text{hispanic}} - \beta^{\text{white}}$		0.032^{**}	* 0.026** (0.011)	0.026^{**}	* 0.030**
$\beta^{\text{black}} - \beta^{\text{white}}$		(0.011) -0.003	(0.011) -0.003	(0.000) -0.003	(0.014) 0.001 (0.007)
$\beta^{\text{other}} - \beta^{\text{white}}$		0.019 (0.016)	0.010 (0.010)	(0.000) -0.001 (0.006)	0.013 (0.011)
Race-by-year FEs Race-by-state FEs State and voter controls		$\sqrt[]{}$			
State-by-year FEs Voter FEs			v	$\sqrt[n]{}$	v √

TABLE III	
TURNOUT EFFECTS OF STRICT ID LAWS BY RAC	CE

Notes. The sample (N = 1,604,600,607) consists of both registered and unregistered voters. See notes to Table I for details on the controls. Column (1) reports mean turnout in the interacting category. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.). *** p < .001, ** p < .001, ** p < .001, ** p < .001, **

In Panel B, we allow the effects to differ by detailed race. Surprisingly, we find a large, positive, and significant effect on Hispanics. The sign and magnitude of this effect are robust across specifications. The estimated difference relative to whites is 2.6 to 3.2 percentage points, depending on the specification. The next subsection discusses one possible mechanism underlying





Event Study Graphs of the Turnout Effects of Strict ID Laws by Race

Each panel plots event study estimates and 95% confidence intervals from a separate regression (in the form of equation (2)) run on all registered and unregistered voters of a given race. The sample includes treated and control states. To avoid picking up variation from 2016 North Dakota, 2016 Texas, and 2018 Texas (which, unlike 2014 and 2018 North Dakota and 2014 Texas, did not enforce a strict law), we define $ID_{T,2016}^{r=1} = ID_{T,2016}^{r=1} = ID_{T,2016}^{r=2} = 0$.

this effect. Instead, we do not find any significant direct or differential effect of the laws on Blacks and on voters of other races. The bottom line is that strict ID laws did not decrease the participation of any race group.

The validity of this result relies on the assumption that turnout trends were parallel between treated and control states for each race, which is supported by the lack of differential pretrends in race-specific event studies plotted in Figure II.

Estimates obtained when restricting attention to voters in adjacent counties across state borders yield the consistent conclusion that strict ID laws did not decrease the participation of any race group (Online Appendix Table A.4, columns (2)–(5)). In Online Appendix Table A.9, we also test the robustness of

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the race heterogeneity results to state-by-race-level regressions. Specifically, we collapse the data by race-state-years, counting ballots cast by voters of different races. We then construct two outcomes: the natural log of ballots cast and total ballots cast divided by estimates of the citizen voting-age population based on U.S. Census data in a given race-state-year. Point estimates and resulting patterns of race heterogeneity are very similar to those reported in Table III.¹⁶ Finally, Panels B–E of Online Appendix Tables A.7 and A.8 show the robustness of the race heterogeneity results to using de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (forthcoming)'s estimators.

A possible concern is that our estimates might miss actual effects of strict ID laws on the participation of Black voters or other ethnic minorities due to the miscategorization of some of these voters' race. Because many campaigns use data similar to ours, minority voters who may be miscategorized in our data may also be less likely to be targeted by campaigns and, thus, more negatively affected by strict ID laws. However, Online Appendix Tables A.11 and A.12 show the robustness of our race heterogeneity results to restricting the sample to voters whose race is estimated with highest confidence and to registered voters, respectively. (Online Appendix Table A.12 uses the turnout of the registered voters as the outcome, as in Table I, Panel A.) Furthermore, Online Appendix Tables A.13 and A.14 measure the effects of strict ID laws, overall and separately by race, using the CCES selfreported turnout data. Despite the limited representativeness and accuracy of national surveys, discussed in Section I, one strength of the CCES is that it includes self-reported race. Reassuringly, our null results are robust to using this alternative source of data.

Online Appendix Table A.15 explores treatment impact heterogeneity along other individual characteristics. We find that the laws did not negatively affect the participation of any group of voters defined by age, gender, or party affiliation.¹⁷ This makes

16. Online Appendix Table A.10 replicates Online Appendix Table A.9 for voter registration (instead of voter turnout). We construct again two outcomes for each race group: the natural log of registered voters and the number of registered voters divided by the citizen voting-age population. The race-specific point estimates are generally nonsignificant, and we do not find any significant differential effect of strict ID laws on minority voters, compared to whites.

17. Party affiliation is only available for two treated states (Arizona and Kansas), one of which is always treated over our sample period (Arizona). Corresponding estimates should thus be interpreted with caution.

it unlikely that the laws changed electoral outcomes. We test this prediction in Table II, Panel B, and find that strict ID laws did not affect the two-party Democratic vote share in elections from 2004 to 2018. In this panel, we pool results from presidential and U.S. House elections. Units of observation are thus state-years, for presidential elections, and congressional district-years, for U.S. House elections. All point estimates are positive but lower than 1 percentage point and not statistically significant. As shown in Online Appendix Table A.16, the results remain close to null and nonsignificant when we consider congressional and presidential elections separately.

IV.C. Effects Due to Specific Components of the Laws or Specific Contexts

We do one last step to challenge our result that strict ID laws have null effects on participation: we test whether specific components of the laws or contextual factors are associated with larger effects.

First, we isolate the effect of requiring an ID from the effect of requesting one. As discussed in Section II.A, the distinctive feature of strict ID laws is that they require voters to show an ID, meaning that people without proper ID are prevented from voting. In contrast, nonstrict laws request voters to show an ID but they allow those without ID to vote, typically by signing an affidavit of identity. Although our regressions so far have included all states without a strict ID law in the control group, we isolate the effect of requiring an ID by comparing strict ID laws to nonstrict laws, in a specification distinguishing between all four types of voter identification requirements: requiring an ID, requesting an ID, requiring voters to sign the poll book or an affidavit, and checking their name against a list of eligible citizens. Formally, we run a regression in the form of equation (1), in which we replace the dummy *ID*_{st} with three dummies, respectively for nonstrict law, requiring a signature, or simply asking to state one's name.¹⁸ This regression allows us to run pairwise comparisons between states with strict ID laws (the default group) and any of the three other

18. Colorado (2014–2018), Oregon (throughout our sample years), and Washington state (2012–2018) implemented all-mail voting. Since voters in all-mail states must sign ballot return envelopes for their votes to be counted, we classify all-mail state-years as "signature." All results are substantively unaffected by alternative classifications of voter identification requirements in these state-years.

types of requirements. An important caveat is that when multiple treatment effects are estimated at once, the coefficient on each treatment is contaminated by a weighted sum of the effects of the other treatments in each state and period, with weights summing to zero (de Chaisemartin and D'Haultfœuille 2020b). Unfortunately, the novel estimators proposed by de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (forthcoming) to improve on the two-way fixed effects estimator do not address this specific issue, and they cannot be readily used to estimate the effects of multiple treatments. Therefore, the results of this model may be biased, and they should be interpreted with caution.

We report the results obtained with the Catalist data and Mc-Donald's aggregate turnout data in Online Appendix Tables A.17 and A.18, respectively. The sign on the nonstrict ID law dummy is generally negative, indicating that strict ID laws have a modest positive effect compared with nonstrict laws, but the point estimates are small, and they are nonsignificant in all specifications, overall and for whites and nonwhites considered separately. In comparison to states with strict ID laws, voter turnout tends to be higher when voters are required to sign the poll book, and lower when they are only asked to state their name, but these differences are generally not statistically significant. The first difference dampens and the second increases when the sample is expanded to also include the 2004 and 2006 elections (Online Appendix Table A.18). Importantly, the effect of strict ID laws, whether measured against nonstrict laws, requiring a signature, or asking to state one's name, is never significantly different across whites and nonwhites (Online Appendix Table A.17, Panel B).

Second, strict ID laws requiring photo identification (like a driver's license or a state-issued identification card) could affect participation more negatively than those also allowing nonphoto IDs (like a bank statement or utility bill). However, we do not find support for this hypothesis: all results are substantively identical using strict photo ID laws as treatment (Online Appendix Figures A.5 and A.6 and Tables A.24–A.28). Out of 30 coefficients shown in Online Appendix Tables A.24 and A.26, only one is negative and significant (at the 10% level). It corresponds to the overall effect of strict photo ID laws on registration, in the specification controlling for state time trends, which is the least reliable, as discussed in Section IV.A.

Third, the effects of strict ID laws could also vary over time: they could be largest immediately following implementation, if people are confused by the new rules, or escalate later, if the laws become more stringently enforced.¹⁹ Alternatively, the effects might vary with election type: they might be larger in presidential elections, if these attract more voters unlikely to have an ID (Burden 2018), or in midterms, if these elections' lower salience makes the administrative cost of acquiring an ID more prohibitive. However, we find no evidence of differential effects along any of these dimensions (Online Appendix Table A.19). If anything, the overall and race-specific event studies show more positive (although generally nonsignificant) effects on turnout in later elections (Figures I and II).

IV.D. Mobilization against the Laws

The null average effect of strict ID laws on participation and the positive effect on Hispanics could result from the combination of a direct negative effect of the new requirements imposed by the laws, on the one hand, and mobilization against them, on the other.

First, parties and candidates who fear they might lose votes as a result of the laws might mobilize their supporters around this issue, and they might help voters without an ID acquire one (Citrin, Green, and Levy 2014; Neiheisel and Horner 2019). A large body of evidence shows that get-out-the-vote campaigns can have large participation effects (Gerber and Green 2000, 2015). including among disenfranchised members of ethnic minorities (Garcia Bedolla and Michelson 2012; Pons and Liegev 2019), and that information and administrative help provided in person to voters can help them overcome obstacles to voting such as registration requirements (Nickerson 2015; Braconnier, Dormagen, and Pons 2017). Although we do not measure the extent to which electoral campaigns specifically refer to the laws or provide assistance to obtain acceptable ID, people's self-reported likelihood to be contacted by a campaign, in the CCES postelection survey data, is a good proxy for campaign intensity. We report the effects of strict ID laws on this outcome in Table IV, columns (1) and (2).

19. Relatedly, in North Dakota and Texas, where strict ID laws were implemented and later repealed, the effects of the laws may persist even after they were abandoned (Grimmer and Yoder 2021). To account for this possibility, Online Appendix Figures A.7 and A.8 and Tables A.29–A.31 replace the treatment dummy ID_{st} , equal to 1 if state s used a strict ID law in year t, with the dummy \tilde{ID}_{st} , equal to 1 if the state used a strict ID law in that year or in any year before. The results leave our conclusion unchanged: strict ID laws have no negative effect on registration or turnout, overall or for any race.

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	Was contacte	d by campaign	Index of vo	ter activity	Contribution resid	.s ln(\$1/100k ents)
	(1)	(2)	(3)	(4)	(2)	(9)
Panel A: Average effect I(Strict ID law)	0.015 (0.020)	0.014 (0.019)	-0.002 (0.016)	-0.008 (0.016)	0.024 (0.102)	0.031 (0.103)
Year and state FEs State and voter controls	>	>>	>	>>	>	>>
Panel B: Whites versus nonwh 1(Strict ID law) × white	nites 0.006 0.01)	0.004	- 0.003	- 0.011 (0.016)		
$1({\rm Strict~ID~law}) \times \\ {\rm nonwhite}$	0.047^{**} (0.019)	0.046^{***} (0.016)	(0.015) (0.015)	0.001 0.001 (0.014)		
etanonwhite – eta white	0.041^{**} (0.016)	0.042^{***} (0.015)	0.005 (0.011)	0.011 (0.010)		
Race-by-year FEs Race-by-state FEs State and noter controls	>>	>>`	>>	<i>>></i> `	>>	>>`
Outcome mean	0.640 221,926	$^{\vee}_{221,926}$	0.000 $308,704$	0.000 308,704	$\begin{array}{c} 14.682\\ 408\end{array}$	14.682 408

election and recorded in the CCES data: whether people attended political meetings, posted a campaign sign, volunteered for a campaign, donaded to a camfidate or a campaign, and how much they contributed. The outcome for columns (5) and (6) is the log of political contributions to candidates and parties by state-year per 100k residents, 2004–2018. For a The voter-level outcome for columns (3) and (4) is a summary index (i.e., sum of z-scores of individual components) of five variables measuring voter engagement in the last general description of state controls, see the notes to Table I. Voter controls in columns (1)-(4) are education, gender, income, and race-by-state fixed effects. Standard errors Notes. The voter-level outcome for columns (1) and (2) is a dummy for whether a CCES survey respondent reported being contacted by a campaign in the last general election. clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.). *** p < .01, ** p < .05, * p < .10.

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Second, even without party mobilization, voters belonging to groups least likely to have an ID might perceive these laws as an attempt to deprive them of their rights and become more likely to vote and engage politically as a result (Valentino and Neuner 2017). Biggers and Smith (2020) report large effects on turnout of being threatened to be purged from voter rolls, particularly for Hispanics, and explain it based on psychological reactance theory (Brehm 1966). According to this theory, a threat to a right (here, the right to vote) can enhance its perceived value and lead individuals to take steps to protect it even if they rarely used it previously. We do not have data on feelings associated with strict ID laws, but we can estimate their effects on forms of political engagement beyond voting. After each election, the CCES surveys record whether people attended political meetings, posted a campaign sign, volunteered for a campaign, donated to a candidate or a campaign, and how much they contributed. We report effects on a standardized index aggregating these five variables in Table IV, columns (3) and (4), and on the individual outcomes in Online Appendix Table A.20. Finally, we measure effects on total campaign contributions by state and election vear using official data from the Federal Election Commission collected by Bonica (2016) (Table IV, columns (5) and (6)).

Table IV, Panel A shows the average effect of strict ID laws on these outcomes for all voters. We find no significant overall impact on any variable, whether we only control for year and state fixed effects or also include state controls and, for individual-level outcomes, voter controls.

Panel B explores treatment impact heterogeneity along race. The effect on the CCES index of voter activity is small and nonsignificant for both whites and nonwhites. As shown in Online Appendix Table A.20, Panel B, we only find a positive and significant effect (at the 10% level) for nonwhites on one out of five components of the index (i.e., volunteered for a campaign, in column (9)). For this outcome, the differential effect on nonwhites compared to whites is significant at the 5% and 10% levels in the specifications with and without state and voter controls, respectively. But overall, we do not find any systematic evidence that individual reaction against the laws alleviated direct negative effects.

Instead, we do observe a large and positive effect on campaign contact among nonwhite voters. The laws increased the likelihood that these voters were contacted by a campaign

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by 4.7 percentage points, which is significant at the 5% level (column (1)). This effect is of similar magnitude and significant at the 1% level when including state and voter controls (column (2)). White voters were not more likely to be contacted by campaigns, differently than nonwhites, leading to a differential effect of 4.1 percentage points. This differential effect remains significant (at the 5% level) and of almost identical magnitude when using strict photo ID laws as treatment (Online Appendix Table A.27).²⁰

This result should be interpreted with caution because it is based on self-reported survey data, and voters may misremember whether they were contacted during the campaign. In addition, even if the increase in campaign contact is real, parties might have targeted a subset of nonwhite voters unlikely to increase their participation as a result of being contacted. Our data do not allow us to directly measure the consequences of increased party mobilization for voter participation. However, we can check whether increases in the likelihood of being contacted by a campaign and in participation are observed for the same groups of voters. Interestingly, as shown in Online Appendix Table A.22, Panel B. columns (1) and (2), the effect on campaign contact is particularly strong (around 5 percentage points) among Hispanics, who also showed a positive effect on participation, suggesting that the former impact could contribute to explaining the latter. The effect on campaign contact is less precisely estimated but also large and positive for the residual race category and it is smaller and

20. Ideally, we would have liked to corroborate this result based on survey responses with data from political parties or from the Federal Election Commission. Unfortunately, we were not able to find administrative data isolating expenditures and activities specifically related to field campaigns, let alone a breakdown of such data by the race of targeted voters. Online Appendix Table A.21 shows effects on coarser outcomes measured at the state-year level: total expenditures and total campaign-related expenditures (encompassing the following expenditure categories: "Campaign data and technology," "Campaign events and activities," "Campaign mailings and materials," "Campaign strategy and communications consulting," and "Polling and surveys") by candidates running for the U.S. House of Representatives, from the Center for Responsive Politics; and TV ad expenditures spanning down-ballot, state, and federal candidates from the Wisconsin Advertising Project and the Wesleyan Media Project. The point estimates are generally positive but modest, and none reach statistical significance.

nonsignificant for Blacks, whose participation was not affected by strict ID laws.²¹

Overall, these patterns are suggestive indirect evidence that the increase in campaign contact was consequential, but they do not allow us to estimate the magnitude of plausible downstream effects on voter turnout. For this, we turn to the existing get-out-the-vote literature. In their review of a large number of experiments conducted in the United States. Gerber and Green (2015) report that it takes about 15 canvassing contacts to generate one vote among voters whose baseline propensity to vote lies between 30% and 50%. The average turnout of nonwhite voters in the sample was within this range, as shown in Table III, Panel A, column (1). Therefore, taken at face value, the increase in campaign contact might have increased the participation of nonwhite voters by about 0.31 percentage points (4.7 percentage points divided by 15). In other words, mobilization against strict ID laws might have offset direct negative effects on the participation of ethnic minorities of about one-third of a percentage point.

IV.E. Voter Fraud and Perception of Fraud

Finally, we explore the effects of strict ID laws on voter fraud and beliefs on election integrity. Studies of crime face a well-known challenge: increases in crime statistics can reflect changes in both the number of committed and reported crimes, and many treatments can have direct and reporting effects (e.g., Bhuller et al. 2013; Draca, Koutmeridis, and Machin 2019). Similarly, strict ID laws might affect the actual number of fraud cases and the likelihood that they get detected and reported. Other limitations inherent to the data available to us and discussed in Section II compound this issue. With these caveats in mind, we report the effects on the extent of fraud in Table V. We consider both the total number of cases (columns (1)-(2) and (5)-(6)) and the subset of cases belonging to categories more directly addressed by strict ID requirements (columns (3) and (4) and (7) and (8)), as described in Section III.C. The total number of cases reported in the News21 and Heritage Foundation data sets is very low, corroborating existing studies

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^{21.} The effect on the CCES index of voter activity is nonsignificant for any race, in any specification, except for Blacks, in the specification without state and voter controls (column (3)), where it is positive and marginally significant. When adding these controls, the effect is no longer statistically significant (column (4)).

	News21 fr resid	auds/100k lents	News21 pr frauds/100k	eventable t residents	Heritage fr resid	auds/100k ents	Heritage pr frauds/100k	reventable t residents		
	(1)	(2)	(3)	(4)	(2)	(9)	(2)	(8)		
1(Strict ID law)	0.045 (0.113)	0.025 (0.109)	0.014 (0.046)	0.001 (0.050)	0.009 (0.007)	0.006 (0.008)	0.013^{**} (0.006)	$0.011 \\ (0.007)$		
Year and state FEs State and voter controls	>	>>	>	>>	>	>>	>	>>		
Outcome mean	0.078	0.078	0.033	0.033	0.020	0.020	0.013	0.013		
N	459	459	459	459	765	765	765	765		
	SPAE perc	eived fraud lex	SPAE imperso	voter nation	SPAE mult	iple voting	SPAE noncit	izen voting	ANES fai	r election
	(6)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
1(Strict ID law)	0.003 (0.030)	0.007 (0.029)	-0.004 (0.017)	-0.002 (0.015)	-0.009 (0.023)	-0.013 (0.022)	-0.020 (0.024)	-0.024 (0.024)	0.008 (0.045)	0.020 (0.038)
Year and state FEs State and voter controls	>	>>	>	>>	>	>>	>	>>	>	>>
Outcome mean N	$0.000 \\ 42,600$	$0.000 \\ 42,385$	$0.210 \\ 42,488$	$0.210 \\ 42,277$	0.209 30,534	0.209 $30,424$	0.275 30,533	0.275 30,423	0.698 11,396	0.698 11,396
Notes. Regressions in c respectively, 2004–2012 a (10) described in the test	columns (1)–(4) and 2004–2018.	are at the state Preventable fra	-year level and t tuds include vote	their sample in r impersonatio	cludes both ever n, duplicate voti	ı (i.e., general e ing, false regist	election) and odd ration, and inelig	years. The Nev gible voting. The	vs21 and Herit	age data c blumns (9

for columns (11)–(16) are dummies for whether SPAE survey respondents perceive different types of fraud as happening frequently or occasionally. The outcome for columns (17) and (18) is a dummy for whether ANES survey respondents agree the last election was "very fair" or "fair" (ANES 2004) or whether they agree ballots were counted fairly "very often" or "fairly often" (ANES 2012), "all of the time" or "most of the time" (ANES 2016). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.). *** p < .01, ** p < .05, * p < .10.

TABLE V

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(Minnite, 2010; Cottrell, Herron, and Westwood 2018): 0.08 and 0.02 cases per year per 100,000 residents, respectively. About one-third (0.03) and one-half (0.01) of these cases were directly addressed by the laws. We do not find any significant negative effect of the laws on either outcome in either data set.

The lack of effect on detected fraud does not preclude effects on voters' beliefs about election integrity. However, using SPAE data, we find the laws had no significant effect on the perceived occurrence of voter impersonation, multiple voting, and noncitizen voting (columns (11)–(16)). The effect on an index aggregating these outcomes (along with the other outcomes reported in Online Appendix Table A.23) is small and nonsignificant (columns (9) and (10)). Similarly, the laws did not significantly affect citizens' belief that the election was fair, recorded in the ANES (columns (17) and (18)).

V. CONCLUSION

For all the heated debates around strict voter ID laws, our analysis of their effects obtains mostly null results. First, the fears that strict ID requirements would disenfranchise disadvantaged populations have not materialized. Using the largest individual-level data set ever assembled to study voter participation, we do not find any negative effect on overall turnout and registration rates or on any group defined by race, age, gender, or party affiliation. Close to null turnout effects are robust to the choice of the DD specification and to a large number of robustness checks. Although we cannot entirely rule out the interpretation that this null result may be due to voters reacting against laws they felt could disenfranchise them, we do not find any effect on campaign contributions or on other forms of political engagement different than voting. However, we find a 4.7 percentage point increase in the fraction of nonwhite voters contacted by parties, bringing some support for the alternative interpretation that parties responded to the laws by mobilizing their supporters around them. It remains that based on existing estimates of the impact of campaign contact, these mobilization efforts might only have offset direct negative effects on the participation of ethnic minorities by about one-third of a percentage point.

Second, contrary to the argument used by the Supreme Court in the 2008 case *Crawford v. Marion County* to uphold the constitutionality of one of the early strict ID laws, we find no significant effect on fraud or public confidence in election integrity. This result weakens the case for adopting such laws in the first place.

Because states adopted strict ID laws only 4 to 14 years ago, our results should be interpreted with caution: we find negative participation effects neither in the first election after the adoption of the laws nor in following ones, but we cannot rule out that such effects will arise in the future. Enforcement of the laws already varies across locations and could very well become more stringent over time, especially if polarization on the issue increases. Partisan mobilization against the laws could also weaken over time. So we do not see our results as the last word on this matter-quite the opposite, we hope that they will provide guidance on the types of data and empirical strategies others can use to analyze the longer-run effects of the laws in a few years. For now, there is a real need to improve the administration of U.S. elections, including voting technology, and increase faith in elections (Alvarez et al. 2012), but strict ID laws are unlikely to do that. At the same time, low and unequal participation represent real threats to democracy (e.g., Meltzer and Richard 1981; Miller 2008; Cascio and Washington 2014; Fujiwara 2015)-but these may be more effectively addressed by reducing other barriers to voting, such as voter registration costs (Braconnier, Dormagen, and Pons 2017) or long travel and waiting times in areas with low polling station density (Cantoni 2020).

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

DATA AVAILABILITY

Code replicating the tables and figures in this article can be found in Cantoni and Pons (2021) in the Harvard Dataverse, https://doi.org/10.7910/DVN/YGQOSO.

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Exhibit 8

EXHIBIT 1

Rebuttal Expert Report of Kenneth R. Mayer, Ph.D.

Montana Democratic Party and Mitch Bohnv. Christi Jacobsen

Consolidated Case No. DV 21-0451

March 25, 2022

In this rebuttal report, I review the claims made by defendant's experts Sean P. Trende and Scott F. Gessler.

I. SUMMARY OF OPINIONS

The Gessler and Trende reports consist largely of unsupported assertions, subjective assessments, speculation offered with no evidence, qualified conclusions, and misstatements about the empirical literature on election administration, voter ID, and election-day registration.

The authors make a series of generic claims (often without any evidence at all) that fail to utilize data or evidence specific to Montana. Both authors are incorrect about key aspects of election administration in Montana, particularly Mr. Gessler on the administrative effects of election day registration ("EDR") and Mr. Trende on the process of curing a provisional ballot. Many of the opinions consist of legal conclusions, not empirical claims. Nothing in these reports changes the conclusions and opinions I expressed in my January 12, 2022 report.

Neither Mr. Trende nor Mr. Gessler use reliable methods, and their opinions are uninformative and unhelpful to understanding the issues in this case.

II. GESSLER REPORT

A. Mr. Gessler's conclusions are unreliable because they lack supporting data or evidence.

Mr. Gessler's report consists entirely of his subjective assessments of election practices in Montana based primarily on his personal experience elsewhere and conclusory statements, often entirely inapplicable to the issues in this case. His report demonstrates a lack of familiarity with or understanding of the extensive peer reviewed literature on election administration, the turnout effects of election-day registration, the effects of voter-ID on turnout (in particular), or the rarity of voter fraud. None of the claims or conclusions are based on any actual data, and many of his observations are based on pure speculation (particularly with respect to college students).

B. Gessler's claims about voter ID are uninformed and incorrect.

Mr. Gessler claims that "it is well established that voter identification requirements do not reduce turnout or create undue burdens." This claim is incorrect, and Mr. Gessler's reliance on a single empirical study (Cantoni and Pons 2021) as support for it betrays his unfamiliarity with the literature on voter ID and with social science research, generally. In fact, there is an extensive literature on the effects of voter ID that contradicts Gessler's claim.

First, a large body of work has established that voter ID laws have a demonstrable effect on reducing turnout, and more specifically that such laws impose burdens on *individual voters*, even if the aggregate turnout effects are difficult to estimate. (See Baretto, Sanchez and Walker 2022; DeCrescenzo and Mayer 2019; Fraga and Miller 2022; Henninger, Meredith, and Morse 2021; and Kuk, Hajnal and Lajevardi 2020 as examples of work over just the last three years). In particular,

Henninger, Meridith and Morse (2021) and Fraga and Miller (2022) use specific administrative data on *who votes without an ID* in states allowing affidavit exceptions to their voter ID requirements. These are directly observable individual effects, do not depend on a statistical model or estimates of aggregate turnout, and show that voter ID has disproportionate effects on minorities and other populations with observably lower possession rates of qualifying ID.

Second, not even the authors of the study Mr. Gessler cites consider their work as settling the question about the effects of voter ID laws on turnout. In fact, the authors warn that their results "should be interpreted with caution" because voter ID laws are a recent phenomenon and they cannot rule out longer-term negative effects (2664). And they go on to note that their analysis does not account for whether someone possesses an ID or not, (Cantoni and Pons 2021, 2620), and does not include complete estimates for nonregistered eligible voters (2629).

C. Mr. Gessler's opinion about student IDs is unsupported and based on his apparent belief that students should not be permitted to vote where they attend school.

Mr. Gessler claims that relegating student IDs to secondary status will not affect student turnout. Again, he cites no evidence or data for this claim, and instead justifies his argument by making unsupported assertions about the capabilities and residency of college students.

Much of Mr. Gessler's argument consists of his position that college students are not residents of the area where they attend school, and that "it is exceedingly easy" for students to falsely register where they do not live:

- 36. To begin, students are a highly mobile population, concentrated in small geographical areas. And usually only a very small minority of students are actually residents of the political jurisdiction where they are housed. For example, Montana State University has 16,766 students, only 7,742 of whom are Montana residents. Furthermore, it seems likely that a very large percentage of students attending Montana State University are not residents of Bozeman, but rather are residents of other towns and cities in Montana.
- 37. Without a document showing a student's address, it is exceedingly easy for a student to improperly claim residency in the local jurisdiction and illegally vote there. For example, absent the recent change in Montana law a student could register to vote using the last four digits of his or her social security, improperly claim local residence, and then vote using only a student identification card

(Gessler Report, p. 21, footnotes omitted).

Here, Mr. Gessler is arguing that students who attend, say, Montana State University in Bozeman are not *really* residents of Bozeman if they were originally from other cities in Montana or from out of state and should not be allowed to vote. This is irrelevant to the question of burden imposed on college students from the relegation of their student IDs to second-tier status and is flatly

inconsistent with the clear eligibility requirements for voting in Montana law requiring only 30 days of residence to register. Mr. Gessler's position boils down to his personal opinion that students are not legitimate voters, which only supports my conclusion in my expert report that restrictions on student voters like the one at issue in this case "are part of a long-standing pattem of states attempting to discourage student voting, or attempting to define the legitimate electorate in a way that excludes students." (Mayer Report, p. 15.)

Moreover, Mr. Gessler's argument about student IDs and voter eligibility is a complete nonsequitur because he fails to note that *none* of the required forms of primary voter ID—a Montana driver's license, Montana state ID, U.S. passport, Tribal ID, U.S. military ID, or Montana concealed carry permit—actually prove residence, as none are required to show the voter's current registered address.

Mr. Gessler's opinion in no way refutes my conclusion about relegating student IDs to secondary status—that it "has no effect other than to make it more difficult for students who are otherwise eligible to vote where they attend school." (Mayer Report, p. 17).

Finally, Mr. Gessler's conclusion that restricting student voting will increase public confidence in elections (Gessler Report, p. 23) is not merely unsupported, it is actually contradicted by the research he cites. Mr. Gessler presents no evidence to support this conclusion and relies instead on a few conversations he had when he was running for local office in Boulder, Colorado. There is very little actual evidence of a relationship between voter confidence and voter ID laws, and the research instead shows the largest factor in voter confidence is whether a voter's preferred candidate won.¹ Notably, even the research Gessler cites finds that voter ID laws have "no significant effect on fraud or public confidence in election integrity." (Cantoni and Pons 2021, 2654).²

D. Mr. Gessler's claims about EDR are unsupported and wrong.

a. Mr. Gessler's claims about research on EDR are wrong.

Mr. Gessler's conclusions about EDR are unsupported and contrary to the evidence. Mr. Gessler admits that the academic literature shows that EDR boosts turnout by five percentage points on average but then claims that this literature is unreliable because "many of these studies compare EDR to a 30-day registration deadline and do not reflect the difference between EDR and Montana's deadline one day before election day." (Gessler Report, p.15). Mr. Gessler, who does not have a research background, is wrong about the literature. Research into the effects of EDR routinely distinguishes between the different types of convenience voting (SDR, early voting, no excuse absentee voting), the length of the registration cutoff period, and the independent effect of EDR (See, as examples, Leighley and Nagler 2014). Burden, et al. (2014) specifically examines registration cutoff dates, estimating the effect of the number of days before the election that

¹ MIT Election Data and Science Lab, "Voter Confidence," April 5, 2021. <u>https://electionlab.mit.edu/research/voter-confidence</u>.

² Notably, Montanans are already confident in their state election processes, ranking in the top 10 of states in both 2016 and 2020 (Mayer Report, p. 7).

registration ends. Mr. Gessler highlights his lack of familiarity with the literature by citing a 2009 study by Burden, et al., which was a very early version of research that was refined and published in a peer-reviewed journal five years later. (Burden et al. 2014).

Mr. Gessler is also wrong that the academic literature lumps EDR and non-EDR states into binary categories irrespective of other administrative practices such as registration deadlines or same-day registration.

b. Mr. Gessler's claims about EDR are not based on any data and are not applicable to Montana.

Mr. Gessler's criticism of EDR is similarly unfounded and ungrounded in any data. Mr. Gessler claims that EDR can increase wait times and impose costs on election officials, and that moving the registration deadline to the day before "provides substantial benefits." (Gessler Report, p. 12). Mr. Gessler's claims are backed by no actual data or evidence from Montana and appear to be based solely on his personal views and a single literature review (which he cites out of context).

Mr. Gessler makes a series of general claims about EDR: that eliminating election day registration "provides substantial administrative benefits" (Gessler Report, paragraph 16); that "processing voter registrations takes substantial time per voter" (Gessler Report, paragraph 18); and that eliminating EDR would reduce the election day workload, result in shorter wait times (Gessler Report, paragraph 20), and reduce "confusion and mistakes" by poll workers (Gessler Report, paragraph 21).

Again, Mr. Gessler provides no data in support of these arguments. But the more serious problem is that Gessler misstates how election-day registration actually occurred in Montana. Election day registration does not typically occur at polling places, but at county clerk's offices. (Mont. Admin. R. 44.3.2015).³

Mr. Gessler also claims that "one review of the academic literature concluded that "Implementing same day registration can have cost implications," including longer lines and confusion" (Gessler report, p. 14). Mr. Gessler is eliding the distinction between *same day registration* (voters registering and voting on the same day in an early voting period prior to election day) and *election day registration* (registering and voting on election day), and he conflates monetary costs and the administrative burdens that Gessler claims exist but does not document. Moreover, the source Gessler cites is clearly referring to *implementing* same day registration in a state that does not already have it (Government Accountability Office 2016, 90), not to the effects of *eliminating* it.

³ This rule applies to late registration, and prior to HB 176 included EDR. The Montana Secretary of State maintains a list of late registration locations corresponding to county election offices (<u>https://sosmt.gov/Portals/142/Elections/Forms/electionadministrators.pdf</u>).

c. Mr. Gessler's claim about the effects of ending EDR is uninformed and unsupported.

While inaccurately summarizing the academic literature and erroneously describing how this research was conducted, Mr. Gessler hedges his view of EDR. "How much of a difference does that one day difference make? This is uncertain. But on balance the difference for turnout is likely low." (Gessler Report, p. 16). Mr. Gessler's assertion is speculation, as Mr. Gessler is not relying on any actual data, and is clearly guessing. Moreover, he does not address the fact that 70,277 Montanans have actually relied on EDR and does not consider why they have relied on it. (Mayer Report, p. 10).

d. Mr. Gessler's claim about absentee ballot collection is unsupported.

Mr. Gessler opines that the restriction on ballot collection "places minimal burdens on voters." Like his other opinions, this claim is offered with no evidence, and is based solely on his unsupported personal opinion. His claims about voter fraud and the alleged risks of ballot collection are speculation: there is no evidence of a single case of absentee ballot collection fraud in Montana. (Mayer Report, p. 7).

III. TRENDE REPORT

A. Mr. Trende's claims about causality are oversimplified.

Like Mr. Gessler, Mr. Trende admits that the academic literature shows a positive correlation between EDR and turnout but then tries to dismiss that extensive body of research. Mr. Trende, in essence, suggests that he knows better. These studies, he claims, are based on observational data and are therefore "plagued with causal inference problems." (Trende Report, p. 7). According to Mr. Trende, although EDR is associated with higher turnout, it has not been shown to *cause* that higher turnout, because states adopting election day registration already had populations that were likely to vote. (Trende Report, p. 9).

Mr. Trende does not actually dispute that election day registration increases turnout, but instead hedges: the literature "struggles to find a causal linkage" (Trende Report, p. 6); the study of the topic is "a fraught endeavor" (Trende Report, p. 7); the relationship depends on the "untestable assumptions" (Trende Report. p. 9). His criticism of the literature is incorrect—scholars who investigate voter turnout are well aware of the importance of controlling for exogenous factors that can complicate the process of estimating the effects of administrative practices on turnout and have used different approaches for research designs that approximate the setting that Mr. Trende demands.

In particular, Mr. Trende cites a peer-reviewed article on turnout that I co-authored (Burden et al. 2014), describing it as a "large scale observational study." (Trende Report, p. 9). This is a mischaracterization of that work—we specifically noted the causal issues that observational data present and relied on methods that create quasi-experimental designs, including "statistical

matching, dose response, and difference in difference models [which] should be less subject to potential endogeneity concerns." (Burden et al. 2014, 100).

In any event, Mr. Trende is wrong in claiming that research on EDR is always observational, ignoring analysis that has shown causal relationships with research designs that approximate randomized experimental trials. (Nieheisel and Burden 2012).

B. Mr. Trende's criticism is misplaced.

The bulk of Mr. Trende's claim about EDR and turnout consists of a lengthy discussion of randomization, experiments, and causal inference. Mr. Trende claims that observational research—meaning what a researcher observes in a real-world setting—does not establish causal claims because it is not possible to control for all possible factors that might affect outcomes.

It is, of course, true that a perfectly randomized double-blind experiment would be the "gold standard" for conclusively establishing causal relationships. But, as Mr. Trende admits, that is not possible. And Mr. Trende's dichotomy between observational and experimental research is false: an observational analysis can yield accurate inferences when researchers control for substantive endogenous effects. Observational research is ubiquitous in social science research, particularly when researchers are examining actual outcomes in which randomization or manipulation of variables is not possible, and it can be relied on to generate plausible causal mechanisms. (Imai et al., 2011).

Mr. Trende's criticism of the research regarding EDR is ultimately a dismissal of virtually all social science research. But there is nothing magic about experimental data, nor anything inherently flawed about using observational data as the basis for causal inference, as *all* social science causal inference is probabilistic and depends on the rigor of the underlying theory. Cofounders, exogenous factors, and unobserved variables are ubiquitous features of all empirical social science research, whether observational or experimental. (King, Keohane and Verba 1994, 79). In criticizing the body of literature regarding EDR, Mr. Trende is essentially dismissing decades of research into *every conceivable topic* in political science, and even social science altogether.

Notably, in arguing against observational research, Mr. Trende relies on statistician R. A. Fisher as the authority who introduced randomization and is, according to Mr. Trende, "responsible for much of our modern understanding of statistics." (Trende Report, p. 8). But in the 1950s, Dr. Fisher was infamously wrong in dismissing evidence of the relationship between cigarette smoking and lung cancer based on the same sort of criticism of social science research Mr. Trende makes in his report. Dr. Fisher erroneously concluded that observational and epidemiological studies showing a relationship between smoking and lung cancer were, in his view, unreliable because they were not randomized experimental studies. (Fisher 1958). Mr. Fisher's error resulted from his "unwillingness to examine the entire body of data available" and his own "prematurely drawn conclusions." (Stolley 1991, 416).

To the extent that researchers have not conclusively established that eliminating EDR will reduce turnout, the main reason is that *it has never occurred before*. No state with EDR has ever eliminated it for a general election.⁴

C. Mr. Trende's claim that EDR minimizes the burden of eliminating EDR is unsupported speculation and is wrong.

Mr. Trende argues that eliminating EDR in Montana will not reduce turnout because Montana maintains same-day registration until the day before the election. (Trende Report, p. 10). He is incorrect for three reasons.

First, we can observe directly that 70,277 people have relied on EDR in Montana since 2006. (Mayer Report, p. 11). EDR voters constituted over 1.4% of general election voters over this period. (Mayer Report, p. 12). Moreover, we can also observe directly that reliance on EDR is higher for younger voters, (Mayer Report, p. 13), that nearly a third of voters who rely on EDR do so because they have moved, (Mayer Report, p. 14), and that more than 7% of currently registered Montana voters relied on EDR at least once since 2008. (Mayer Report, p. 13).

Second, election day is not just any other day—it is by far the most prominent day of the election cycle and provides cues to potential voters. Parties, candidates, and institutions take special note of election day, often organizing around efforts to mobilize voters and get them to the polls. (Burden et al. 2014). Both Montana State University and the University of Montana make election day a holiday, giving students a day off from classes and employees a paid day off.⁵ Moreover, moving the deadline to the day before imposes informational burdens on voters who may not realize the deadline has been moved (particularly because EDR has been in effect in Montana since 2006—or for at least 16 statewide general, primary, or special elections). And a voter who discovers an error in their registration on election day may not have a chance to correct it that day.⁶

Finally, EDR far exceeds the average number of daily late registrations. Table 1 shows the average number of late registrations (excluding election day) over the 29 days of pre-election registration in Montana from 2008 to 2020 for both primary and general elections.

⁴ In 2011, Maine abolished election-day registration, but voters overturned the legislation in a referendum held later than year, restoring the practice before the 2012 elections (Russell 2011). ⁵ Montana State University policy:

https://www.montana.edu/policy/hr policies/holiday holiday pay.html. University of Montana policy:

https://view.officeapps.live.com/op/view.aspx?src=https%3A%2F%2Fwww.umt.edu%2Fhuman -resources%2Fforms-docs%2Fholiday-schedule-2021-2022.docx&wdOrigin=BROWSELINK.

⁶ Declaration of Sarah Denson, January 11, 2022. Declaration of Thomas Bogle, January 12, 2022.
Table 1 - Late Registration vs. EDR			
Year	Election	Average Daily Late Registration	Election Day Registration
2020	General	269	8,172
2020	Primary	29	1,618
2018	General	373	8,053
2018	Primary	64	951
2017	Special	80	2,074
2016	General	528	12,055
2016	Primary	100	3,346
2014	General	180	4,677
2014	Primary	52	953
2012	General	412	8,053
2012	Primary	59	1,178
2010	General	156	3,735
2010	Primary	39	836
2008	General	369	7,547
2008	Primary	124	2,678

In every election, election day far exceeds the average daily rate of late registrations, often by more than a factor of 20. Over the entire period, 23 times as many people registered on election day than on the average pre-election late registration day. As demonstrated by this data, election day has a unique status that no other day in the registration period can match.

D. Mr. Trende is wrong about the effects of voter ID.

Like Mr. Gessler, Mr. Trende claims that voter ID laws do not affect turnout. And like Mr. Gessler, Mr. Trende also has never published any peer-reviewed research on voter ID. They both miss a key point: *even if* voter ID laws have not always been shown to have a measurable effect on aggregate voter turnout, the academic literature has repeatedly shown effects on *individual* voters. (Fraga and Miller 2022; Henninger, Meredith and Morse 2021; DeCrescenzo and Mayer 2019).

Mr. Trende provides no data or analysis to support his argument that relegating student IDs to secondary status will have no effect. Instead, he questions the validity of the canonical Cost of Voting framework as "[overstating] the degree to which people apply coldly rational decision-making to the choice of whether to turn out or not." (Trende Report, p. 11). In asserting this, Mr. Trende ignores that election researchers have confronted this issue directly, and that researchers in the field generally don't assume that voters "apply coldly rational decision making" when deciding to vote—instead, the Cost of Voting framework offers a general set of predictive claims that have been repeatedly verified in the academic literature.

Mr. Trende claims that voter ID has been shown to increase confidence in the election process, citing a single study that concluded voters in Virginia who were informed about the state's voter

ID requirements in a 2017 election thought that less voter fraud occurred than voters who were not informed. (Endres and Panagopoulous 2021). But Mr. Trende fails to mention that other research he cites in his report (Cantoni and Pons 2021) finds no relationship between voter ID laws and confidence in the election process.

Finally, Mr. Trende is completely wrong in his claim that voters without an ID can vote provisionally and have their ballots count if election officials match their provisional ballot signature with the signature in voter registration records. (Trende Report, p. 14). Montana law requires that voters who cast a provisional ballot because they lack ID must provide acceptable identification by 5 p.m. on the day after the election *and* have their signatures match:

(2) If a legally registered individual casts a provisional ballot because the individual failed to provide sufficient identification as required pursuant to 13-13-114(1)(a):

(a) the elector has until 5 p.m. on the day after the election to provide identification information pursuant to the requirements of 13-13-114 or as provided in subsection (3) of this section; and

(b) the election administrator shall compare the signature of the individual or the individual's agent designated pursuant to 13-1-116 on the affirmation required under 13-13-601 to the signature on the individual's voter registration form or the agent's designation form. If the signatures match, the election administrator shall handle the ballot as provided in subsection (7). If the signatures do not match and the individual or the individual's agent fails to provide valid identification information by the deadline, the ballot must be rejected and handled as provided in 13-15-108.

Section 13-15-107, MCA.

Mr. Trende ends his argument about voter ID by writing "I am not convinced that voter fraud is a substantial problem in Montana" (Trende Report, p. 12). This misstates the question. What matters is not whether Mr. Trende is "convinced" that voter fraud is a problem, but rather *what the evidence* shows. And as I note in my report, voter fraud is vanishingly rare in Montana, and there is no evidence that the changes made to voting in HB 176, SB 169, and HB 530 will improve election security.

E. Mr. Trende's claim about the ease of voting in Montana is inapposite.

In the final section of his report, Mr. Trende dismisses the plaintiffs' claims because, in his opinion, "voting in Montana remains generally easy." (Trende Report, p. 13). He states that 13 states allow student ID as a form of voter ID, (Trende Report, p. 14), and that only eighteen states have EDR. (Trende report, p. 10).

Here, Mr. Trende is focusing on the wrong question. The relevant question is how the changes implemented in HB 176, SB 169, and HB 530 affect the ability of Montanans to vote, and who is

most likely to be affected, not how Montana compares to other states. In any event, Montana is the only state to have *eliminated* EDR. In this regard, it stands alone.

Mr. Trende's claim that "[it] is hardly unusual for states to limit the people who may handle absentee ballots," (Trende Report, p. 14), also misses the point. The question is whether the change will result in voters having difficulty returning their absentee ballots, and who may have become accustomed to relying on others to return their ballot.

Finally, Mr. Trende's claim about student ID also misses the point that not all states require a photo ID for voting, and even among the 7 states with a strict voter ID law, 7 5 (or 71.4%) allow a student ID with no additional documentation required.⁸

IV. **CONCLUSION**

The reports submitted by Mr. Gessler and Mr. Trende do not provide reliable information about the effects of HB 176, SB 169, or HB 530. Their arguments and conclusions are generic, they present no data on the effects of eliminating EDR or relegating student ID to secondary status, and they are frequently wrong about election processes in Montana.

Neither report changes my conclusions in my January 12, 2022 report.

March 25, 2022

Date of Signature:

Place of Signature: Madison, WI

Camp for Kenneth R. Mayer, Pl

⁷ National Conference of State Legislatures, https://www.ncsl.org/research/elections-andcampaigns/voter-id.aspx.

⁸ Arkansas, Georgia, Indiana, Kansas, and Mississippi allow student IDs. Tennessee and Wisconsin either prohibit student IDs or require additional documentation. Arkansas's strict voter ID law was permanently enjoined on March 24, 2022. League of Women Voters of Ark. v. Thurston, No. 60CV-21-3138 (Ark. Cir. Ct. Mar. 24, 2022). If I exclude Arkansas from the calculation, 4 of 6 strict voter ID states allow student IDs with no additional documentation (66.7%).

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Exhibit 9

Voter Identification and Nonvoting in Wisconsin—Evidence from the 2016 Election

Michael G. DeCrescenzo and Kenneth R. Mayer

ABSTRACT

How much did Wisconsin's voter identification requirement matter in 2016? We conducted a survey of registered nonvoters in the counties surrounding the cities of Milwaukee and Madison to estimate the number of registrants who experienced ID-related voting difficulties in the 2016 presidential election. We estimate that 10 percent of nonvoters in these counties lack a qualifying voter ID or report that voter ID was at least a *partial* reason why they did not vote in 2016, and six percent of nonvoters lacked a voter ID or cited voter ID as their *primary* reason for not voting. Theoretically, we argue that voter ID requirements "directly" affect voters who lack qualifying IDs but also "indirectly" affect voters who are confused about their compliance with the law. We find evidence of such confusion, with many respondents mistakenly believing that they did not have the necessary ID to vote when they actually did. Our analysis permits us to calculate bounds on the possible turnout effect in 2016. Most of our credible estimates suggest that the voter ID requirement reduced turnout in these counties by up to one percentage point.

Keywords: voter identification, turnout, administrative burdens, voting rights, election administration

INTRODUCTION

THE NOVEMBER 2016 PRESIDENTIAL CONTEST was the first major election in which Wisconsin's voter ID requirement was in effect. Statewide turnout was the lowest it had been in 16 years, with an especially notable drop in the city of Milwaukee, where it fell from 66 percent of the votingage population in 2012 to 56 percent in 2016 (Wisconsin Elections Commission 2018).

The full implementation of Wisconsin's ID requirement offers an opportunity to assess its effects and explore the broader characteristics of voter ID laws. We present the results of a survey of nonvoting registrants in the state's two largest counties (Milwaukee and Dane) that asked about reasons for nonvoting, understanding of the voter ID law, and the forms of ID a respondent possessed. Using a Bayesian analysis, we estimate that a mean of 10.2 percent of nonvoting registrants were deterred from voting by the ID law (defined as lacking a qualifying ID or citing lack of ID as a reason for not voting). Using a more restrictive classification (respondents who lacked a qualifying ID or noted that lack of ID was their main reason for not voting), a mean of 5.8 percent of nonvoters were prevented from voting by the ID law. These estimates are larger among individuals who are black, earn lower incomes, and have less formal education. Credible intervals

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indicate that between 8,000 and 17,000 nonvoters in these two counties were deterred from voting, and between 4,000 and 11,000 were prevented from voting. Boundary analyses suggest that registered voter turnout in these two counties may have been reduced by up to one percentage point.

Theoretically, we elaborate on a broader conception of how voter ID requirements and other election laws can impede voters. Voters may be "directly" affected by a voter ID requirement if they lack a qualifying ID, but many additional voters may be "indirectly" affected if the details of the law confound voters' understanding of their compliance with the requirements. Consistent with this argument and with other similar work (Hasen 2016; Hobby et al. 2015), we find evidence that voters are confused about the ID requirement. When asked whether different forms of ID qualify or not under the ID law, respondents classify an average of only 5.4 out of 12 accurately. Individuals who were less knowledgeable of the law were in turn more likely to cite a lack of ID as a reason for not voting. Further, we find that many of the individuals who claim to be affected by the requirement actually report having a qualifying ID, and this pattern is stronger among individuals who were less knowledgeable of the law's details.

Our analysis has its limits. Because our sample is drawn from registered nonvoters, we can only make statements about those who were contemporaneously registered to vote. We do not know many individuals never bothered to register because they lacked (or thought they lacked) a qualifying voter ID.¹ Our estimates apply only to Milwaukee and Dane counties; while we are confident that the effects elsewhere in the state are nonzero,² we do not extrapolate to statewide estimates. We cannot definitively control for all possible sources of misreporting or response error, but our findings reflect conservative decisions throughout the analysis are consistent with prior research and are plausible in their magnitude. Finally, our survey does not capture the administrative burdens experienced by those who undertook the steps necessary to obtain a new ID in order to vote. We elaborate on these limitations and discuss potential improvements below.

WISCONSIN'S VOTER ID REQUIREMENT

Prior to enacting its voter ID requirement, Wisconsin law emphasized voter participation and access, with relaxed early voting and absentee voting rules, same-day registration, and local control of election administration. Voters were required to show ID only if they were first-time voters who had not verified their identity when registering by mail. Wisconsin Act 23 (Wisconsin Statutes 5.02(6m)), which became law in May 2011, requires voters to present one of the following forms of identification before casting a ballot:

- 1. A Wisconsin driver's license
- 2. A Wisconsin Department of Transportation (DOT) photo ID
- 3. A receipt for a driver's license or DOT-issued photo ID (used between the time of application and the time that the ID is received)
- 4. A U.S. military ID
- 5. A U.S. passport
- 6. A certificate of naturalization that is less than two years old
- 7. An ID issued by a federally recognized Native American tribe
- 8. A qualifying ID card issued by an accredited Wisconsin college or university

IDs in categories 1–4 must be either current or have an expiration date after the previous two-year general election. Student IDs must be unexpired, have an expiration date of two years or less from date of issue, and contain a signature; a person using a student ID is also required to show proof of current enrollment.³ After reports of military veterans unable to use their Veterans Affairs (VA) IDs to vote in a 2016 primary election (including the uncle of a sitting Wisconsin Supreme Court Justice), the legislature added VA IDs as qualifying form of identification (Marley 2016).

Wisconsin's law is among the strictest in the country. It allows fewer forms of IDs, requires photo ID for mail absentee voting, and does not have an

¹A qualifying photo ID is not required for voter registration in Wisconsin if the registrant uses the last four digits of their social security number.

²More than half of all ID-related rejected provisional ballots cast in 2016 were in counties other than Dane or Milwaukee. ³Standard student IDs at many state universities (including the University of Wisconsin–Madison) are not compliant with the statute because they lack a signature or have expiration dates beyond two years from issuance. Students can obtain an addi-

tional voting-only ID that contains the necessary elements. See State of Wisconsin Government Accountability Board (2017) for a state guide to student voter IDs.

election-day affidavit exemption for individuals without ID.⁴ There is a process by which people without the necessary underlying documentation could obtain an ID for voting, but a federal judge ruled it unconstitutional in 2016, ordering the state Department of Motor Vehicles (DMV) to provide an ID to anyone who asked for one.⁵ The process still requires a trip to the DMV to submit forms in person.

The ID law was in effect for a judicial primary in February of 2012 that was held in just six of the state's 72 counties. (Turnout was under three percent.) A series of injunctions in state and federal court blocked the law until the presidential primary election in early 2016 and the general election in November.

THE EFFECTS OF VOTER ID

A large literature has demonstrated that individual decisions to vote, and therefore aggregate turnout, are affected by administrative practices and legal requirements for voting (e.g., Wolfinger and Rosenstone 1980; Leighley and Nagler 1992; Hanmer 2009; Gronke, Galanes-Rosenbaum, and Miller 2007; Green and Gerber 2008; Burden et al. 2014). Voter ID laws can raise barriers for individuals who don't possess a qualifying ID or who lack the documentation required to obtain one. The laws also increase information costs by requiring voters to navigate the details of the law to determine their compliance.

We argue that voter ID requirements affect voting costs through two conceptually distinct pathways. The first is a *direct effect* on people who are kept from voting primarily due to bureaucratic obstacles imposed by the law. Some individuals will not attempt to vote because they lack and cannot obtain ID or are told at the polling place that their ID is insufficient. If ID is a *necessary* condition for voting, we characterize those who do not satisfy that condition as directly affected.

ID laws also have an *indirect effect*. Although possessing a qualifying ID is a necessary condition for voting, mere possession is not a sufficient condition after the law is imposed; people who have qualifying IDs can still have their path to the voting booth impeded by information costs imposed by the law. Voter ID laws contain technical and administrative details that are not widely understood. As a result, voters who possess qualifying IDs could mistakenly believe that they do not.

How might confusion impede someone who actually has a qualifying ID? The devil is in the details. Even a registered voter who possesses a driver's license or state ID might still be confused about the exact circumstances when these common IDs do and do not qualify. For example, WI driver's license holders who move residences must update their address information with the state Department of Transportation but are not required to obtain a new physical license until their current license expires (Wisconsin Statutes 341.335). As a result, many Wisconsinites hold driver's licenses with outdated addresses. Importantly, a driver's license used as a voter ID does not need to show the voter's registered address, but otherwise eligible voters may not know this. The number of residents potentially implicated in this detail of the law is far from trivial: the 2012–2016 American Community Survey estimated that each year, 547,819 Wisconsin votingage residents relocated within-state (U.S. Census Bureau 2016). Voters whose names have changed (e.g., through marriage or divorce) might be able to vote using an ID with their previous name, depending on whether they registered to vote under the new name or if the new name is hyphenated in a way that includes the full previous surname. Again, the numbers are not trivial; Wisconsin recorded 32,385 marriages and 14,986 divorces in 2015 (Wisconsin Department of Health Services 2016). Expired driver's licenses also qualify as long as the expiration date is more recent than the previous two-year November general election, as do suspended or revoked licenses.⁶ Wisconsin licenses are valid for eight years. Assuming a regular pattern of license issuance, oneeighth of the state's 4.3 million licenses (about 540,000) will expire each year. Most of these licenses will be renewed (483,000 were renewed in 2017), but many will not be.

Moving, marriage, divorce, and license expiration are ordinary facts of life, but they bear on the

⁴According to the National Conference of State Legislatures as of January 2019, most states with strict photo ID requirements exempt mailed absentee ballots from the ID requirement. All but Wisconsin and Tennessee accept employee IDs issued by a body of federal, state, or local government. Indiana and Tennessee also have an indigence exception (Underhill 2019). ⁵The judge called the program "pretty much a disaster" (*One*

Wisconsin Institute, Inc. et al. v. Thomsen et al. 2016: 964).

⁶The minutia can become hyper-technical. Someone whose license has been confiscated by law enforcement can ask for a receipt documenting that the license has been taken away, which can be used as a voter ID.

ability to comply with the voter ID requirement in ways that are neither obvious nor commonly discussed in academic literature. The number of people affected by these technical details, and for whom compliance may not be straightforward, is potentially large.

Research from other states finds evidence that voters are confused by voter ID requirements. In a study of Harris County, Texas, and Texas's 23rd Congressional District, three-quarters of 2016 nonvoters surveyed incorrectly believed that an unexpired Texas driver's license was the only form of ID that was acceptable for voting. Fewer than 20 percent of nonvoters knew what ID requirements were in effect for the election (Jones, Cross, and Granato 2017).

Direct effects are more closely related to bureaucratic hurdles (Herd and Moynihan 2018) that might be mitigated through what Hasen calls a "softening of the harshest aspects of voter identification laws" (Hasen 2016: 102). Indirect effects might be alleviated through voter education efforts or more efficient administrative practices. But both direct and indirect effects impede otherwise eligible people from accessing the voting booth.

Measuring the effects

Estimating the turnout effect of voter ID laws is a challenging empirical problem. The question behind many studies-how many people did not vote who would otherwise have voted in the absence of an ID law-is not directly answerable with observable data (McConville, Stokes, and Gray 2018). We know whether someone votes (and therefore know that their intention to vote was not overcome by the ID requirement), but the causes of nonvoting are harder to pinpoint. ID requirements are one of many things that affect turnout. Individuals without ID might not vote because they dislike the candidates, do not think their vote matters, and so on, so they may not have voted even if they possessed a qualifying ID. Furthermore, individuals may possess the documents needed to obtain an ID but lack the ability or inclination to travel to the appropriate government office to get it. Still others might be able to obtain the required documents but decide that the endeavor would be too costly. People may not realize that they possess a valid form of voter ID because they are unaware of the details of the requirement. And election workers

might not administer an ID law in an evenhanded or accurate manner (Cobb, Greiner, and Quinn 2010; White, Nathan, and Faller 2015).

Researchers have used four main methods to estimate the effects of ID laws: analyzing the number of provisional ballots cast for ID-related reasons (Hopkins et al. 2017; Pitts 2008, 2013; Stewart 2013); using surveys to identify individuals who lack qualifying forms of ID (Alvarez, Bailey, and Katz 2008; Barreto, Nuño, and Sanchez 2009; Barreto and Sanchez 2012a; Barreto and Sanchez 2012b; Barreto and Sanchez 2014; Barreto et al. 2019; Hajnal, Lajevardi, and Nielson 2017; Stewart 2013); record-linkage techniques to identify registered voters who lack a form of qualifying ID in government databases (Ansolabehere and Hersh 2017; Government Accountability Office 2014; Hood 2015; Mayer 2015; Stewart 2013); and analyses of aggregate or individual turnout to isolate the effects of ID laws, typically using differencein-difference methods to compare states that do and do not have strict ID requirements (Erikson and Minnite 2009; Government Accountability Office 2014; Hood and Bullock 2012; Mycoff, Wagner, and Wilson 2009). Although research consistently shows that rates of ID possession are lower among minority and low-income populations, studies of turnout have returned a range of conclusions, from no or inconclusive effects (Mycoff, Wagner, and Wilson 2009; Erikson and Minnite 2009; Grimmer et al. 2018; Cantoni and Pons 2019) to aggregate estimates of a turnout decline up to a few percentage points (Alvarez, Bailey, and Katz 2008; Government Accountability Office 2014; Hood and Bullock 2012).

All of the methods used to study voter ID-the number of provisional ballots, estimates of ID possession, and studies of aggregate turnout-provide insight into the consequences of voter ID laws. They have drawbacks, however. Some methods will underestimate the number of individuals affected because the data are produced only after several self-selection processes (e.g., provisional ballots). Other methods will overestimate the effect on turnout due to unobserved confounders. One way to improve the measurement of voter ID's impact on voters, first employed in a 2015 study of the Texas 23rd Congressional District, is to survey nonvoters from voter files (Hobby et al. 2015; Jones, Cross, and Granato 2017). This approach is consistent with recommendations from Grimmer et al., who describe "custom-sampled surveys of individuals affected by voter ID laws" as an improvement over broad national surveys (2018: 1051). Asking nonvoters directly about their experiences with voter ID requirements provides more leverage to identify direct and indirect effects. We extend this approach using a similar survey instrument in Wisconsin.

DATA AND EMPIRICAL METHODS

Original survey of Wisconsin nonvoters

We surveyed Wisconsin registrants to measure the rate of ID-related nonvoting in the 2016 presidential election. We built our sample from the voter histories of registered Wisconsin voters (the state "WisVote" file), using the voter file generated on February 20, 2017. We limited the voter file to include only registrants from Dane and Milwaukee Counties who did note vote in the 2016 election. These counties contain the two largest metro areas in the state (Milwaukee and Madison) and have the largest low-income and minority populations, which existing research suggests are most likely to be affected by voter ID requirements. Because the sampling frame contains only these counties, we do not extrapolate our estimates to represent the state of Wisconsin as a whole.⁷

We used a stratified design with oversampling from Census tracts with lower aggregate measures of socioeconomic status (SES). We divided the sample into three strata and drew a sample of 2,400 nonvoters in total: 650 from Dane County, 750 from high-SES tracts in Milwaukee County, and 1,000 from low-SES tracts in Milwaukee County. We conducted all analyses using sampling weights to adjust for unequal sampling probability across strata.⁸

We mailed our survey to each sampled individual in March 2017. The survey asked registrants about their engagement with and interest in the campaign, as well as their reasons for not voting. We embedded questions about respondents' knowledge of the voter ID requirement and the forms of ID they possessed. Because the study was supported by a government entity, we did not ask about party affiliation or vote intentions in the 2016 election season. The full questionnaire can be viewed in Supplementary Appendix A.

We received 288 valid responses, with 75 from Dane County and 213 from Milwaukee County.

This gives us a nominal response rate of 12.0 percent. The response rate is 27.5 percent after we adjust for deadwood in the sample, which we explain in more detail when we describe our statistical modeling approach below.⁹

Identifying the "affected group"

Because the survey asked respondents several questions about their experiences with voter ID during the 2016 election, we can construct multiple measures of who was affected. We asked respondents why they did not vote, offering voter ID as one of several reasons.¹⁰ These questions were modeled after items routinely used in the Cooperative Congressional Election Study (CCES) and in the November Voting and Registration Supplement to the Census Bureau's Current Population Survey (CPS). Voters could initially select several partial reasons for not voting and then were asked to select their main reason¹¹ for not voting. Potential reasons

⁷Our choice to limit the sample to Dane and Milwaukee Counties was driven by two considerations. First, the survey was funded by a government office that was interested in local effects of the ID requirement. Second, we faced a trade-off between a two-county study and a statewide study. We were particularly interested in quantifying the effects among registrants most likely to be affected, who would be concentrated in the state's urban counties. The two-county survey allowed more flexibility in the sampling design but at the expense of our ability to draw statewide inferences. We concluded that estimating individual effects was the more important matter.

⁸Population weights for each sampled individual were generated by the UW Madison Survey Center. For in-sample analysis, we rescale these weights such that the largest weight is equal to 1.0. We show in Supplementary Appendix B that our results are almost identical under other weighting approaches, including poststratification weights that adjust for response rates across strata and a by-stratum estimation method where each stratum is treated as an independent sample.

⁹Supplementary Appendix B contains a section on the demographic composition of the sample. Because there is no Census for our target population (nonvoting registrants in Dane and Milwaukee Counties), we are limited in the explicit judgments we can make about the representativeness of the sample. We were able to compare the racial distribution of our sample to the distribution of *modeled race* as estimated from surnames in the voter file. Sample strata in Milwaukee contain a larger share of whites than the modeled race estimates suggest, which would likely lead us to underestimate voter ID's impact in the sample.

¹⁰Original text: "There are many reasons why people are not able to vote or choose not to vote. Please tell us whether or not each of the following are reasons why you did <u>not</u> vote in the November 8, 2016 general election." (Underline in original.) ¹¹Original text: "Which of the following was the <u>primary or</u> <u>main reason</u> why you did <u>not</u> vote in the recent presidential election? Please check only one." (Underline in original.) included being ill or disabled, being out of town, not having enough time, not being interested in voting, having a transportation problem that prevented them from getting to the polls, not liking the choice of candidates or issues, being unable to obtain an absentee ballot, lacking a qualifying ID, attempting to vote but being told at the polls that their ID was not qualifying, long lines at the polls, encountering a problem with early voting, and believing that one's vote would not matter. Later in the survey, respondents were asked about the forms of ID they possessed, which we used to determine whether respondents lacked a qualifying voter ID.¹² Tables 1 and 2 summarize these variables with all responses weighted.

Table 1 displays respondents' reasons for not voting. The most common reason for not voting was displeasure with the choice of candidates and issues of the campaigns (cited as a partial reason for not voting by 50.8 percent of the sample and the primary reason for not voting by 33.0 percent of the sample). Other reasons for not voting included lack of interest, a feeling that one's vote did not matter, and other time, location, or ability constraints; 6.5 percent of respondents reported that they did not have adequate ID, and 2.9 percent said that they were turned away at the polls because they lacked ID. Fewer respondents cited voter ID as the main reason they did not vote, with 1.7 percent of the sample saying they lacked adequate ID and

TABLE 1. PARTIAL AND MAIN REASONS FOR NOT VOTING

	Partial reason (%)	Main reason (%)
Unhappy with choice of candidates or issues	50.8	33.0
Not interested	27.5	8.8
Not enough time	26.7	9.3
Vote would not have mattered	26.2	6.6
Away from home	20.1	13.5
Ill or disabled	18.4	13.6
Problem with early voting	12.5	2.9
Couldn't get absentee ballot	8.1	1.3
Transportation problems	7.7	2.1
Did not have adequate photo ID	6.5	1.7
Lines too long	3.0	0.9
Told at polling place that ID inadequate	2.9	1.4
No reason given	-	4.9

Percentages for partial reasons sum to more than 100 because respondents could indicate multiple partial reasons. Estimates reflect sample weighting.

TABLE 2. ID POSSESSION AMONG SURVEY RESPONDENTS, INCLUDING FORMS OF ID THAT DO AND DO NOT QUALIFY AS VALID VOTER IDS IN WISCONSIN

ID form	Possess (%)	Lack (%)	DK (%)	NA (%)	Qualifying
WI driver's license	79.7	14.8	0.8	4.6	Yes
U.S. passport	42.3	43.2	0.4	14.1	Yes
WI DOT ID card	21.7	59.2	3.1	15.9	Yes
Military ID	5.7	74.3	0.9	19.1	Yes
Naturalization certificate	3.2	75.7	1.7	19.4	Yes
WI voter ID card	2.4	75.2	1.7	20.7	Yes
Native Am. tribe ID	1.2	78.4	0.4	19.9	Yes
Social Security card	89.0	3.9	0.7	6.4	No
Credit card	73.8	18.2	0.4	7.6	No
Concealed carry permit	6.8	74.4	0.4	18.3	No
Non-WI driver's license	5.6	74.7	0.4	19.3	No
State/federal employee ID	5.0	74.6	0.4	19.9	No

Table includes "Don't know" responses (DK) and non-responses (NA). Estimates reflect sample weighting.

1.4 percent saying that they were turned away at the polls.

The percentage of respondents indicating IDrelated reasons for not voting is low but not zero. This makes sense, given what we already know about the impact of voter ID requirements. We have strong a priori expectations that the number of individuals facing ID-related obstacles should be relatively small (Erikson and Minnite 2009). Furthermore, we know that most Wisconsin registrants possess qualifying forms of ID. In litigation over Wisconsin's ID requirement, a federal court concluded that 9.4 percent of registrants in Wisconsin lack a qualifying form of identification (*Frank v. Walker* 2014).

Table 2 shows rates of ID possession in the sample. We find that 3.0 percent of respondents lack all

¹²"Currently, do you have each of the following forms of identification?" Respondents could separately indicate that they possessed several forms of ID, only some of which would satisfy the voter ID requirement. The survey does not indicate to the respondent which forms of ID satisfy the voter ID requirement. The qualifying IDs included a Wisconsin driver's license, Wisconsin Department of Transportation ID, a voting-only ID, a military or veteran's ID, a Native American tribal ID, a certificate of recent naturalization, and a U.S. passport. The nonqualifying IDs included a driver's license from another state, a credit card, a permit to carry a concealed weapon, a state or federal government ID, and a Social Security card.

forms of qualifying ID in the survey item.¹³ We note that this estimate is lower than the estimates of non-possession produced by expert witnesses on both sides of the federal litigation over Wisconsin's voter ID law (*Frank v. Walker*), suggesting that respondents are not misreporting their ID possession.

We construct two measures of the group of affected citizens. We refer to registrants as *deterred* from voting if they lack a qualifying ID or mention ID as a reason for not voting. We also construct a stricter definition, referring to registrants as *prevented* from voting if they lack a qualifying ID or list voter ID as their primary reason for not voting. We present analyses for both outcome variables throughout the article. In the interest of brevity, we refer to nonvoters more generally as "affected" in contexts where we do not need to distinguish between deterred and prevented classifications.

Even though "prevented" is a more conservative definition than "deterred" and might be interpreted as more robust, we caution against invoking such a heuristic. Electoral reforms can reduce an individual's ability to vote even if they do not constitute an outright ban. Partial reasons for nonvoting remain an important measure of voter ID effects. Furthermore, even if citizens possess a qualifying ID, confusion about the law can lead them to mistakenly believe that they cannot vote (Hobby et al. 2015).

While there is always a possibility for noise in survey responses, closer analysis of our data suggests that these forces do not drive our results (see sections about race and socioeconomics and about confusion). We also find it unlikely that ID-related responses are driven by social desirability because respondents give ID-related responses at a much lower rate compared to other more socially desirable responses (disliking the candidates, away from home, not enough time, etc.). The distribution of responses resembles those of other similar items in the CCES and the CPS for 2016 (see Supplementary Appendix B).

Modeling the impact of voter ID

In this section, we describe a model to estimate the number of individuals affected by the voter ID requirement in the population of these two counties. This process is not as simple as multiplying the number of nonvoters in the voter file by the percentage in the sample who said that they were affected by the voter ID requirement. Not every record in the voter file was eligible at the time of the election. Voter files include deadwood: individuals who are no longer eligible to vote at their registered addresses because they moved, died, or fall into another category of ineligible voters (Pettigrew and Stewart 2016). Identifying this deadwood is crucial to generating population estimates of voter ID effects from our survey results. We build a population model that estimates this eligibility rate as well as the proportion of nonvoters who were affected by the voter ID requirement.

Eligibility rate. To identify deadwood, we tracked survey nonrespondents to identify individuals who no longer lived at the addresses listed in their voter histories. The Survey Center used Lexis/Nexis commercial data to identify nonrespondents who submitted a National Change of Address form with the U.S. Postal Service, registered to vote at another address, appeared on a credit report at a different address, appeared in public records as deceased, incarcerated, in the military and stationed abroad, or underwent a name change. Of the 2,112 nonrespondents, this method identified 1,049 as eligible and 1,063 as ineligible. Counting all respondents as eligible, this gives a point estimate of the deadwood rate of 44.3 percent (or an eligibility rate estimate of 55.7 percent).

Let *i* index all individuals (respondents and nonrespondents) in the full sample of 2,400. We model the eligibility of each individual in the sample, *Eligible_i*, as a Bernoulli outcome,

$$Eligible_i \sim Bernoulli(\varepsilon), \tag{1}$$

where ε is the population eligibility rate among nonvoters in these two counties.¹⁴

Response rate. Individuals who remain at their registered address can then respond to the survey or not. In the interest of specifying the full probability, we model the probability that an individual responds, conditional on eligibility, as another Bernoulli outcome.

¹³We code "don't know" responses as lacking ID because if a respondent is unaware whether they possess a form of ID, presumably they would not be able to use it to vote. Non-responses to these questions are more ambiguous, however, so we make the conservative decision to code non-responses as possession. This probably undercounts the extent of ID non-possession.

¹⁴Although the sample was drawn in February 2017, we must assume that ε is representative of nonvoter eligibility as of the presidential election in November 2016.

$$Respond_i \sim Bernoulli(\rho),$$
 (2)

where ρ represents the response probability among eligible nonvoters.¹⁵

Affected rate. Respondents to the survey then indicate whether they were affected by the ID requirement (deterred or prevented). We model the probability that an individual is affected as a final Bernoulli outcome.

$$Affected_i \sim Bernoulli(\pi), \tag{3}$$

where π represents the probability that a randomly selected nonvoter was affected by voter ID, conditional on eligibility. We refer to π as the "affected rate." We generate separate estimates of π for the "deterred" and "prevented" outcome variables.

Population estimate. After estimating these parameters from survey data, we generate population estimates for the number of affected nonvoters in Dane and Milwaukee counties. The voter file contains 229,625 nonvoters for these two counties, but not all of these individuals were eligible at their listed addresses in 2016. We calculate the number of eligible nonvoters in the population by penalizing the number of nonvoters by ε .

Number Eligible =
$$\varepsilon \times Nonvoters$$
 (4)

We then calculate the number of nonvoters affected by ID by multiplying the number of eligible nonvoters by the affected rate π .

Number Affected =
$$\pi \times (Number \ Eligible)$$
 (5)

We are careful to note that the number of individuals affected by the voter ID requirement is not equivalent to the number of individuals who *would have voted* if not for the requirement. Our survey design does not permit rigorous enough assumptions about counterfactuals to estimate the causal effect on turnout—we cannot know whether affected individuals would have voted if the ID law were not in effect. Absent these counterfactuals, we instead use our estimates in a bounding analysis to simulate a range of plausible turnout effects.

Bayesian priors and estimation

We estimate this model with a Bayesian approach, which offers a number of advantages. Primarily,

Bayesian methods allow us to improve our estimates by including prior information from past studies of voter ID, voter registration, and deadwood. This is particularly useful for moderately sized samples such as ours, where priors can stabilize and regularize estimates against sampling error when trustworthy external information about parameters is available. We find that our estimates are consistent with previous studies, which increases our confidence in the reliability of our sample. We also show that our estimates using flat and informed priors are similar, demonstrating that posterior inferences do not merely reflect the priors used.

We design informative prior distributions to reflect conservative assumptions about the unknown parameters in our model. The analysis is most sensitive to the rate at which nonvoters were affected by the voter ID requirement, π , so we take special care to design a prior that minimizes the risk of overestimating this quantity. Our prior always regards smaller values of π as more likely than larger values, and it places 95 percent of the prior probability below the largest credible estimate of π supported by relevant research. When we examine studies of voter ID in Wisconsin, estimates of ID non-possession rates among registered voters range from 4.5 percent (Hood 2015) to 8.5 percent (Mayer 2015), and 9.5 percent in Milwaukee (Barreto and Sanchez 2012a). A federal court concluded that 9.4 percent of registrants in Wisconsin lacked qualifying identification (Frank v. Walker 2014). With this information, we specify the skeptical prior $\pi \sim \text{Beta}$ (1,30), which has an always-decreasing density as π increases, an expected value of 3.3 percent, and 95 percent of its mass below 9.5 percent. This choice of prior is especially conservative because the estimates on which it is based are under-inclusive of the affected population as we define it, which contains both directly and indirectly affected nonvoters. Furthermore, we use the same prior for "deterred" and "prevented" nonvoters, even though existing estimates more closely reflect the narrower "prevented" group.

¹⁵The exact value of the response rate parameter should not affect our estimates because we employ the canonical assumption in survey research that survey response is independent of item response. Researchers wishing to extend our methods in the future could collect background characteristics on all sampled individuals to model the response rate and modify this independence assumption.



FIG. 1. Flat and informed priors for the eligibility and affected rates. Shaded regions indicate the inner 90 percent of the informative prior distributions.

We also develop a prior for the eligibility rate ε using estimates of voter registration deadwood developed by Pettigrew and Stewart (2016). Their model estimates that the deadwood rate in Wisconsin is roughly 7.5 percent of the total registration file. Assuming that the deadwood rate is about the same statewide as it is in Dane and Milwaukee Counties, this implies that of the 19 percent of registrants in these counties who did not vote in 2016, roughly 61 percent were eligible.¹⁶ Using this information, we place a Beta (15,10) prior on the eligibility rate ε , which has an expected value of 60 percent with 90 percent of its prior mass between 44 percent and 75 percent eligibility. We include this wider variance to account for the possibility that the deadwood rate in Dane and Milwaukee Counties is different from the statewide deadwood rate.

Translating past research into prior distributions is never a precise exercise, but we believe that our approach offers an improvement over flat priors that regard all estimates as equally plausible, especially in a moderately sized sample like ours. We also present estimates using flat priors for the eligibility and affected rates, Beta (1,1), to show that prior information improves our estimates without over-determining them. Figure 1 compares the flat and informative prior distributions for the eligibility rate (ε) and the affected rate (π). The response rate (ρ) is omitted from the plot because we always give it a flat prior.¹⁷

We fit all Bayesian models with Stan (Carpenter et al. 2016), which generates posterior samples using a variant of Hamiltonian Monte Carlo. We collect 12,000 draws for each model parameter.¹⁸ We incorporate sample weights into the Bayesian analysis by weighting the log likelihood of each observation.¹⁹ The log likelihood of the data (the data's contribution to the log posterior distribution) can be generically expressed as follows:

$$\log[p(y \mid \theta)] = \sum_{i=1}^{n} \ell(y_i \mid \theta) w_i \tag{6}$$

where each $\ell(y_i \mid \theta)$ and w_i represent the log likelihood and sample weight, respectively, for each outcome observation y_i and parameter vector θ .

¹⁸For each model, we generate four Markov chains with 5,000 iterations per chain. The first 2,000 iterations of each chain are used as an adaptive warm-up period to tune the sampling algorithm and before being discarded. Following the advice of Link and Eaton (2011), we do no thinning of parameter chains. We show in Supplementary Appendix B that our chains mix well and exhibit essentially zero autocorrelation despite no thinning, owing to the design of Stan's Monte Carlo algorithm. This results in 12,000 samples per parameter. We also show in Supplementary Appendix B that various Hamiltonian Monte Carlo diagnostics exhibit no problematic behavior.

¹⁶If the deadwood rate among nonvoters is $\frac{0.075}{0.19} = 0.39$, then the eligibility rate is 1 minus the deadwood rate.

¹⁷Additionally, we make the typical assumption in survey research that other model parameters are independent of the response rate. Supplementary Appendix B also contains analyses where all probability parameters are given non-informative Jeffreys priors, Beta (0.5,0.5). Results are nearly identical.

¹⁹The most complete way to include weights in Bayesian analysis would be to specify a probability model for the weights (Gelman 2007). Stan's developers recommend pseudo-likelihood as a next-best method for including weights that cannot be estimated de novo (e.g., Survey Weighted Regression, 2017). The analysis in the article uses pre-sample weights, but we show in Supplementary Appendix B that our results are unchanged by various weighting methods.



FIG. 2. Posterior estimates of the affected rate (*top*) and total number of affected nonvoters (*bottom*). Each panel shows histograms of posterior samples with means and credible intervals plotted below each histogram.

FINDINGS

Thousands of individuals were impeded by the voter ID requirement

How many nonvoters in Dane and Milwaukee counties were affected by Wisconsin's voter ID requirement? We plot estimates of the affected rate (π) and the total number of affected nonvoters in Figure 2. We show estimates for individuals both "deterred" and "prevented" from voting, using both flat and informed priors.

The top panel of Figure 2 shows our estimates of the affected rate. We visualize the posterior estimates with histograms of Markov chain Monte Carlo samples. Below each histogram, we show a point and error bars for posterior means and 95 percent compatibility intervals. For comparison, we also plot the informative prior distribution (Beta (1,30)) as a dashed line. We do not explicitly plot the flat prior because it has the same density across all supported values. Using flat priors, our mean estimate is that 11.5 percent of nonvoters were deterred from voting due to voter ID, with 95 percent of posterior samples falling between 7.3 and 15.6 percent. Estimates using informed priors are marginally more conservative, with a smaller mean of 10.2 percent and a narrower interval from 6.7 to 14.0 percent. Using the stricter "prevented" definition of the effect, we estimate with flat priors that a mean of 6.5 percent of nonvoters were prevented from voting due to voter ID (3.4 to 9.7 percent). With informed priors, we estimate a mean of 5.8 percent of nonvoters prevented from voting (3.1 to 8.7 percent).

The posterior estimates are similar regardless of the prior used. This is because the data send a strong signal about the affected rate irrespective of the prior. There is not much variance in our estimates because they are close to zero. This is true even for the flat prior, which will naturally have wider variance because it gives greater prior weight to larger values of the affected rate. The informative prior, on the other hand, has the intended effect of regularizing estimates against noise, producing slightly smaller means and smaller variances. This regularization safeguards against overestimating the affected rate due to random sampling error. It is also important to note that the regularization is minor compared to the signal obtained from the data, so the posterior distribution does not merely reflect the prior. The similarity of estimates from flat and informed priors also shows that our data are consistent with previous studies of ID nonpossession in Wisconsin, since models that contain no external information nonetheless produce similar posteriors to models that contain information from past studies.

Estimates of the eligibility rate are not plotted because they are virtually identical across models. All model specifications generate mean eligibility rate estimates of 52.5 percent with credible intervals between 50.2 and 54.8 percent. This is slightly below the Pettigrew and Stewart (2016) estimate of 60.5 percent but entirely consistent with the informative prior.

We use the affected rate and the eligibility rate to calculate the total number of eligible nonvoters in the population who were affected by the voter ID requirement. We plot these estimates in the lower panel of Figure 2. According to our model, thousands of individuals in Dane and Milwaukee Counties were deterred or prevented from voting in 2016 due to voter ID requirements. Using flat priors, we estimate a mean of 13,900 nonvoters deterred from voting (to the nearest hundred, 95 percent interval from 9,000 to 19,000) and a mean of 7,900 nonvoters prevented from voting (interval from 4,100 to 11,700). Estimates from informed priors reflect regularization of the affected rate and are thus slightly lower than the estimates from flat priors: 12,300 nonvoters deterred from voting (95 percent interval from 8,100 to 17,000) and a mean of 7,000 nonvoters prevented from voting (interval from 3,700 to 10,500).

Our models show that Wisconsin's voter ID requirement affected a far greater number of individuals than implied by the 821 ID-related provisional ballots cast in 2016 (of which just 173 were counted) (Wisconsin Elections Commission 2016). Our best estimates using informed priors suggest that thousands of individuals in these two counties alone were deterred or prevented from voting in 2016 by the voter ID requirement. We find that nonvoters report being affected by the ID requirement at roughly twice the rate at which they actually lack ID (3.0 percent in the sample, which is lower than past studies of Wisconsin). We show below in a discussion on "indirect effects" that this difference can be at least partly explained by confusion about the law's details, consistent with our argument that voter ID requirements both directly and indirectly affect voters.

Are these effects real? A look at race and socioeconomics

How confident can we be that the effects we observe are real and not driven by an accumulation of measurement error? Recent controversies in survey research highlight the risks of making inferences about rare events in large datasets, since small data errors can accumulate as sample sizes grow larger (Ansolabehere, Luks, and Schaffner 2015). If the patterns we observe in the data are real, we should be able to observe other implications of the underlying theory and find evidence consistent with the topline results. Furthermore, we should be able to derive implications from alternative explanations (measurement error, misreporting) and show that the data are inconsistent with these notions. An analysis of race and socioeconomics underscores the validity of our data.

What should we observe if our findings are accurate? Previous research suggests that stricter identification requirements raise voting costs disproportionately for nonwhite and lower SES voters. If our results are driven by the real effects of Wisconsin's voter ID requirement, we would expect the affected rate to be higher among these subgroups. If, by contrast, our results are driven by measurement error, we expect these patterns to be attenuated and resemble statistical noise. Further still, if our results are driven by intentional misreporting, social desirability, or expressive responding (Berinsky 2018), we might even expect greater effects among white and higher-SES individuals because the tendency to misreport or engage in expressive behaviors is strongest among higher-SES individuals (Ansolabehere and Hersh 2012; Schaffner and Luks 2018; Sciarini and Goldberg 2016).

Figure 3 puts these contrasting hypotheses to the test, comparing the affected rates across race (black and white), income, and formal education.²⁰ Point estimates suggest that individuals who are black, lower income, and have less formal education are all more likely to be affected than white, higher income, and higher educated individuals, respectively. We do not observe the reverse pattern that would have strongly indicated expressive responding or social desirability bias. We lose statistical power by dividing our sample into these subgroups, so variation introduced either by sample size or by measurement error is difficult to distinguish formally. However, the point estimates are universally consistent with the hypothesis that the data reflect true effects rather than random measurement error, and the probability of this pattern under measurement error alone is small.²¹ While we cannot definitely rule out survey misreporting or response error, these subgroup estimates are consistent with the existing literature and provide reassurance about the quality of our data.

Evidence for ''indirect effects:'' knowledge and confusion

We theorized that voter ID requirements affect voters by directly raising the bureaucratic costs of voting and by indirectly confusing voters about

²⁰Confidence intervals are estimated with the Clopper-Pearson method (Clopper and Pearson 1934), which have better coverage in smaller samples and near the probability bounds than intervals based on the approximation of the Normal distribution. ²¹If we define a Bernoulli success as a group comparison where the "theoretically expected" group has a higher estimated affected rate, and we conduct a two-sided significance of the null hypothesis that the expected group in each of 12 comparisons (14 comparisons minus two over-determined comparisons) has a higher affected rate with probability 0.5 (pure noise), the *p*-value for that test would be 0.0005.



FIG. 3. Estimates of the affected rate (π) within race, income, and education categories.

which IDs are compliant under which circumstances. The details surrounding expiration dates, conforming names and addresses, and the validity of common forms of ID such as university IDs and government employee IDs are complicated, and people may be confused or misinformed about whether they have the necessary ID to vote (Hasen 2016; Hobby et al. 2015; Jones, Cross, and Granato 2017). Our results are consistent with this argument. Many of the individuals we identify as deterred or prevented from voting report that they possess a qualifying form of ID: while roughly 11.2 percent of the sample was deterred and 6.1 percent was prevented from voting due to ID, just 3.0 percent lacked a qualifying ID.

We explore this argument further by measuring respondents' knowledge of the voter ID requirement. We asked respondents to classify 12 forms of ID as satisfying or not satisfying the voter ID requirement. Seven of the 12 forms of ID qualify, and five do not.²² If confusion about the voter ID requirement drives some respondents to report that their ability to vote was hindered by the law, this

would imply that voters who are less knowledgeable of the law are more likely to be affected by it. Furthermore, voters with less knowledge about the law may be more likely to believe that they cannot vote when they in fact possess a qualifying ID.

Figure 4 contains a descriptive picture of the ID classification item. The left panel shows the percent of individuals who classify each form of ID correctly, with each qualifying ID indicated by a dot and each non-qualifying ID indicated by an *x*. We omit nonresponses, so missing data do not "count against" respondents. While almost everyone knew that a Wisconsin driver's license was a qualifying form of ID (about 95 percent), only 70 percent knew that a Wisconsin DOT ID qualifies or that a credit card does not. The typical respondent was uncertain (between 40 and 60 percent correct) about half of the IDs included in the questionnaire, and a majority of respondents were incorrect about

²²These are the same 12 forms of ID included in our battery of ID possession items.



FIG. 4. Results from ID classification task. The *left* panel indicates, for each form of ID, the percent of respondents who correctly classify the ID as qualifying or not qualifying. The *right* panel is a histogram that indicates the number of IDs correctly classified by each respondent.

Social Security cards and government employee IDs (which do not qualify), as well as Native American tribal IDs and naturalization certificates (which do).

The right panel in Figure 4 plots the number of correct respondents at the individual level. Just 15 percent of respondents classified nine or more IDs correctly. The mean number of correct classifications was 5.4 out of 12, or 45% correct. Counting the number of correct responses implicitly penalizes the respondent for item non-response (since non-responses can't be correct), but the impact of this decision is minor. If we omit non-responses, the typical respondent classifies 56% of IDs correctly.

Not only do registrants appear to be confused about which IDs qualify, we also find that respondents who knew less about the law were more likely to be affected by it. We use logistic regressions to estimate the probability that individuals are deterred or prevented from voting as a function of their knowledge of the law, measured as the number of IDs a respondent correctly classified as qualifying or not qualifying. Figure 5 plots predicted probabilities from these regressions with coefficient estimates, standard errors, and *p*-values shown in each panel. The top two panels show the estimated relationship in the full sample. Regardless of whether we measure the effect as "deterred" (left) or "prevented" (right) we find that individuals who are less knowledgeable about the law are more likely to be affected, with both coefficient estimates significant at or below p = .001. For the 15 percent of individuals who correctly classify at least nine forms of ID, the predicted probability that they were affected by the ID law was low (below 10 percent). Respondents who classified fewer IDs correctly were more likely to be deterred or prevented from voting. Respondents who classified just two IDs correctly had more than double the predicted probability of being affected as individuals who classified nine or more forms correctly.

We test a stricter implication of the "indirect effects" argument in the bottom two panels of Figure 5. If confusion about the ID requirement leads even *individuals who possess qualifying IDs* to report being affected by the requirement, we should find a similar relationship when we limit the sample only to individuals who possess a qualifying ID. We find relationships of a similar form, but coefficients are weaker and statistically more uncertain (p < .1 for "deterred," p < .05 for "prevented").²³

Our results suggest that confusion about the law leads some individuals to misunderstand whether they are able to vote. We cannot make a strict causal inference that variation in the affected rate among ID-possessing respondents is directly attributable to their knowledge of the ID requirement, but these patterns are consistent with the argument that voter ID requirements can affect a broader population of voters than those without a qualifying ID.

 $^{^{23}}$ All of these regressions are estimated using sampling weights, which downweights the majority of observations. If we assume that the sample design is ignorable (Rubin 1976) and estimate the regression with equal respondent weights, coefficients are essentially identical but *p*-values are smaller.



FIG. 5. The relationship between knowledge of the ID requirement and the probability that an individual is affected. Curves and confidence intervals show predicted probabilities from logistic regressions from the full sample (*top*) and limited to individuals who possess qualifying ID (*bottom*). Rugs on the *top* and *bottom* edges of each panel show the observed values of the dependent variable and are jittered for legibility.

These findings provide further reassurance that our results are not driven by misreporting, since we would expect individuals engaging in expressive behavior to be more knowledgeable of the ID requirement.

LESSONS ABOUT TURNOUT, FUTURE RESEARCH, AND POLICY

A simulation of counterfactual turnout

How did Wisconsin's voter ID requirement affect turnout in 2016? This is a difficult counterfactual question. Although we estimate that thousands of registrants were deterred or prevented from voting, we do not know how many of them would have voted if the law were never implemented. Many nonvoters may have had other reasons for not voting. For these individuals, the ID requirement increased the costs of voting, but removing it would not have made the difference.

To estimate turnout effects, we need an estimate of the *counterfactual turnout rate* among affected nonvoters—what turnout among the affected group would have been if no ID requirement existed. While our study design does not permit a direct estimate of the counterfactual turnout rate, we can simulate the aggregate turnout effect at hypothetical levels of counterfactual turnout. Mathematically, this simulation calculates the number of *suppressed votes* as the number of affected individuals in the population, multiplied by the counterfactual turnout rate.



FIG. 6. Simulated effects on voter turnout for hypothetical levels of counterfactual turnout (posterior means and 95 percent credible intervals).

We set the counterfactual turnout rate to a series of values between 0 and 1, and we use the simulated number of suppressed votes to calculate the difference between the observed level of registered voter turnout and what turnout would have been under the counterfactual simulation. The method reveals the upper and lower bounds on the turnout effect given the parameters estimated from the above model, and it permits a direct assessment of the turnout effect for a fixed level of counterfactual turnout.

Figure 6 plots the predictions from this simulation. If all affected nonvoters would have voted at 100 percent turnout in the absence of the ID requirement, then turnout in Milwaukee and Dane Counties would have been 0.8 to 1.9 percentage points higher (using "deterred") or between 0.4 and 1.2 percentage points higher (using "prevented"). These are upper-bound predictions on how large the turnout effect could have been, conditional on the estimates from our data. The turnout effect was almost certainly smaller, since a counterfactual turnout rate of 100 percent is unlikely. Respondents to our survey voted in the 2012 presidential election at about 86 percent turnout, and it is likely that turnout would have been lower in 2016 even without the voter ID requirement.²⁴ If counterfactual turnout in the affected group was just 60 percent, turnout in these two counties would have been between 0.5 and 1.2 percentage points greater (using "deterred") or between 0.2 and 0.7 percentage points (using "prevented"). At the lower bound, the only way to conclude that the voter ID requirement had no effect on turnout among the affected group is to assume that the affected group contains zero people or that counterfactual turnout in the affected group is exactly zero. Both of these assumptions are virtually impossible, so there must have been some nonzero turnout effect. On the upper side, there are essentially no scenarios that support a conclusion that turnout was reduced by two points or more. Most plausible scenarios yield turnout effects that could be as high as one percentage point among registered voters.

Although we are confident that our results reflect real patterns in nonvoting, measurement error may play some role in the effects we find. At the same time, our results reflect conservative modeling assumptions meant to guard against overestimating the effects. Moreover, our results may underestimate the number of individuals affected by the voter ID requirement because our survey cannot measure the decision of unregistered individuals not to register in the first place (Stein and Tchintian 2017).

²⁴We restrict this comparison to registrants whose 2012 registrations we could verify. We deem a registrant eligible if their registration date was before the election or if the voter file indicates that they voted in an election prior to 2012. This excludes individuals with registration dates since the 2012 election.

Improvements in research design

The secondary analyses presented reassure us that our core findings are real. Our estimates are consistent with previous studies of ID possession in Wisconsin, and secondary tests are consistent with hypotheses about race, socioeconomics, and knowledge of the ID requirement. Nonetheless, we believe that future studies implementing surveybased methods can improve on our design in a number of ways to identify more precise effects.

First, it is possible to directly confront a "lowerbound" problem in our survey responses. By fielding simultaneous surveys in states that do and do not enforce strict photo ID requirements, researchers can compare the "base rate" of individuals who report being affected by voter ID requirements even in states where no ID requirement exists. This not only would allow researchers to control for survey error, it may also shed light on citizens' knowledge about their voting systems. If voters are confused about a broader set of electoral rules (such as same-day registration, early voting, felon disenfranchisement, etc.), the notion that voters are "indirectly affected" by election laws may be a more widespread phenomenon than the literature currently acknowledges.

We can also collect more detailed information about which IDs registrants possess and their beliefs about whether those IDs satisfy the voter ID requirement (e.g., expiration dates, address changes). Surveys can also ask a broader set of questions about citizen experiences with voter ID laws, including whether voters say that they do or do not need certain IDs to live their lives. Indirect effects also have methodological implications that cannot be ignored. Although record-linkage methods provide accurate estimates of the share of registrants who lack driver's licenses (e.g., Ansolabehere and Hersh 2017), there are strong reasons to believe that voter ID requirements raise voting costs on individuals beyond those lacking ID.

Voter ID in academic and political debate

Our data show that Wisconsin's voter ID requirement impeded many voters' access to the voting booth. We find evidence of both direct and indirect effects, by which outright barriers to voting combine with confusion about the voter ID requirements to reduce the number of eligible people who went to the polls in November 2016. The effects are consistent with other studies of voter ID and with the literature on election administration: electoral reforms can impose a variety of costs on citizens, with larger effects on vulnerable populations. We also find that voter ID laws have indirect effects by raising informational demands on voters even if they possess ID. We make progress exploring survey-based methods for learning about direct and indirect effects, and we believe that future studies can improve upon our instrument to collect more detailed information on the sources of confusion about voter ID.

The number of people affected by voter ID requirements exceeds-by orders of magnitude-the number of cases of voter impersonation that ID laws are purportedly designed to prevent. The literature on vote fraud has repeatedly shown that voter impersonation is exceedingly rare, with just handfuls of confirmed cases over the span of decades (Ahlquist, Mayer, and Jackman 2014; Levitt 2007; Minnite 2010). In federal litigation over Wisconsin's voter ID requirement in particular, the court concluded that "the [state] could not point to a single instance of known voter impersonation occurring in Wisconsin at any time in the recent past" (Frank v. Walker 2014: 847). By contrast, we estimate that Wisconsin's voter ID law affected the ability of thousands of registrants to vote in 2016.

SUPPLEMENTARY MATERIAL

Supplementary Appendix A Supplementary Appendix B

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Exhibit 10





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A disproportionate burden: strict voter identification laws and minority turnout

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A disproportionate burden: strict voter identification laws and minority turnout

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ABSTRACT

Critics of the recent proliferation of strict photo identification laws claim these laws impose a disproportionate burden on racial minorities. Yet, empirical studies of the impact of these laws on minority turnout have reached decidedly mixed results. State and federal courts have responded by offering mixed opinions about the legality of these laws. We offer a more rigorous test of these laws by focusing on more recent elections, by relying on official turnout data rather than surveys, and by employing a more sophisticated research design that assesses change over time using a difference-in-difference approach. Our analysis uses aggregate county turnout data from 2012 to 2016 and finds that the gap in turnout between more racially diverse and less racially diverse counties grew more in states enacting new strict photo ID laws than it did elsewhere. This analysis provides additional empirical evidence that strict voter ID laws appear to discriminate.

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Strict voter identification laws are proliferating around the country. Prior to 2006, no state required citizens to provide a valid photo identification in order to vote. Today, 11 states have strict ID laws in place and more states appear to be waiting in the wings. Critics have vilified these laws as anti-democratic and anti-minority (Weiser 2014). From this perspective, strict voter ID laws have little purpose other than to limit the legitimate participation of racial and ethnic minorities and other disadvantaged groups, and to bias outcomes in favor of the Republican legislators who pass them.

But on the other side of the debate supporters have been just as vocal. They argue that voter identification laws are necessary to reduce voter fraud and instill greater legitimacy in the democratic process (Kobach 2011). Advocates also argue that voter identification laws do not reduce the participation of citizens because they do not prevent legitimate voters – almost all of whom have identification – from entering the voting booth. The only thing that is clear is that the stakes for American democracy are high and growing higher by the year.

In many ways, the courts have served as the primary battle site over these laws. Almost every strict ID law has been challenged in the courts. In one of the most important cases,

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Crawford vs Marion County, the Supreme Court ruled that a 2005 strict voter identification law passed in Indiana was constitutional. But that has not stopped opponents from filing suit against different versions of the law. Currently, voter identification laws are being litigated in at least six states with laws being challenged in four states as unconstitutional (14th and 15th Amendments) and/or in violation of the Voting Rights Act (Alabama, North Carolina, North Dakota, and Wisconsin) and in two others as violating state law (Iowa and Missouri).¹

In past legal proceedings, the court's ruling has appeared to rest more than anything else on the balance between the burden that these laws pose on racial and ethnic minorities and the state's interest in the integrity of the electoral process. And, that balance often seems to rest on the weight of the empirical evidence about the burden these laws pose to minorities. When the empirical evidence to document a substantial burden has been found wanting, the courts – including the Supreme Court – have generally ruled that these laws are constitutional.² When in other cases, more convincing evidence of a real burden has been put forward, several courts have ruled against these laws.³ With the fate of these laws continues to be adjudicated by the courts, more rigorous empirical evidence is needed.

In all of this, it is important to note that no two voter ID laws are identical and different laws in different states may be targeting different groups. For example, North Dakota's strict ID law requires an ID with a residential street address which may disproportionately target and impact Native Americans many of who live on reservations without official street addresses. By contrast, Texas's initial ID law allowed residents to use a concealed carry gun license but not a state-issued student ID – a pattern that critics felt favored Whites and disproportionately impacted Blacks and Hispanics.

Existing evaluations of voter ID laws

Unfortunately, despite all of the attention given to these laws, the empirical evidence is not yet entirely convincing one way or another. Crucially, we know that racial and ethnic minorities are less likely than whites to have ready access to valid identification (Ansolabehere 2014; Stewart 2013; GAO 2014; Barreto et al. 2019; Hood and Buchanan 2019). But would these individuals actually vote in the absence of these laws? And would mobilization in opposition to these laws by parties, non-profit organizations, or others actually increase turnout among some voters (Citrin et al. 2014; Valentino and Neuner 2017)?

When studies go one critical step further and focus on voter turnout and seek to directly assess whether these laws reduce participation and skew the electorate in favor of one racial group over another, the results have been decidedly more mixed. Earlier studies tended to find few effects (Alvarez, Bailey, and Katz 2008; de Alth 2009; Mycoff, Wagner, and Wilson 2009; Hood and Bullock 2012). More recent studies tend to demonstrate a significant, if sometimes inconsistent, racially disproportionate impact (Dropp 2013; GAO 2014; Hajnal, Lajevardi, and Nielson 2017, 2018; Fraga 2018). Critics are, however, quick to note the data limitations of these studies (Grimmer et al. 2018).

Given the mixed findings to date and given the importance and necessity of persuasive empirical evidence for the courts to decide the future of voter identification laws in the states, it is clear that we need a stronger test that will provide greater insight into the impact of these laws on the minority population and in so doing offer more compelling results for the courts and policy makers.

A stronger test

In order to advance the empirical literature and to effectively contribute to the legal debate, any new study needs to address three critical flaws evident in much of the existing empirical studies. First, it must focus on recent elections and distinguish between strict photo ID laws and other less stringent ID laws. One reason for the difference in findings between earlier and later studies seems clear. Much of the research published before 2013 focused almost exclusively on the impact of *non-strict* voter identification laws. That is understandable since the strictest versions of the laws were not implemented until recently, but it is also problematic given that it is only strict ID laws that require identification in order to vote.

Second, a new study should rely on official turnout data rather than on potentially problematic survey data as much of the research has done. Much of the scholarship on strict voter ID laws has focused on self-reported turnout – a major problem since substantial and racially uneven shares of the public over-report turnout (Abramson and Claggett 1991; Ansolabehere and Hersh 2012).

The final and perhaps most important concern with the research to date is methodological. As Highton (2017) and others have noted, most studies use cross-sectional data when assessing the impact of ID laws but since states that pass these laws so clearly differ from states that do not, causal inference is limited. The solution according to Highton (2017) and Erikson and Minnite (2009) is to focus on over time changes through a difference-in-difference approach. Unfortunately, no study has yet incorporated each of these three elements into a more definitive test.

In this article, we seek to move forward on all three fronts and thus to contribute both to the empirical debate and to the legal discussion by providing concrete evidence about the consequences of voter identification laws for turnout among marginalized segments of the American public. Specifically, our analysis uses a difference-in-difference approach to compare turnout changes in states that recently implemented strict photo ID laws with turnout changes in states not implementing strict ID laws over the same time period. We focus on turnout changes across the two most recent presidential elections in 2012 and 2016. Alabama, Mississippi, Virginia, and Wisconsin all implemented strict photo ID laws over this period. We define a strict voter identification law as any electoral law that requires voters to present identification before their ballot will be officially counted.⁴ Our test also employs official turnout data, namely official county-level aggregate vote totals for all 3142 counties in the United States.⁵

Our analysis uses two official data sources. First, to measure *aggregate* turnout in each county in each contest, we compile the official vote totals for each county in each election and Census data on the voting age population in each county.⁶ Second, we add Census data on the racial and ethnic breakdown of the voting age population by county. By combining these two data sources, we can look at how turnout changes from 2012 to 2016 in each county vary by the racial and ethnic composition of each country. If strict voter identification laws disproportionately impact racial and ethnic minorities, we would expect aggregate turnout in racially diverse counties to fall more (relative to aggregate turnout in largely White counties) in states that implement new strict ID laws, than it does in states that don't enact new ID laws.⁷

To try to address the concern that we are using aggregate turnout to try to make inferences about individual voter behavior (the ecological fallacy problem), we perform two key tests in the online appendix (Section 11). One uses data from a state where turnout by race is officially recorded to show that aggregate country turnout is a reasonable proxy for the turnout of the majority racial group in each county. The other employs a similar difference-in-difference design using validated individual-level vote data from a national survey to show that strict identification laws have a similar pattern of racial effects at the individual level. However, we want to be very clear that neither test can definitively rule out all concerns related to the ecological inference problem. Ultimately, we can only say how aggregate turnout changes as counties become more or less racially diverse and cannot be certain how turnout by race differs within each county.

Testing the impact of ID laws by modeling changes in turnout between 2012 and 2016

The basic test is at its heart direct and straightforward. To determine if the implementation of strict photo ID laws has a racially disparate impact, we look to see if turnout in racially diverse counties declines relative to turnout in predominantly white counties more in states enacting strict voter IDs than it does in states not enacting strict ID laws over the same time period. In other words, we utilize a difference-in-difference design. We perform that basic test in several different ways to ensure the robustness of our findings.

We first undertake a state fixed effects regression analysis that includes all counties in all states.⁸ By including state fixed effects, we essentially control for all state-level characteristics that don't change over this time period. If a state was more Republican or more hostile to minority voting rights in ways that we did not measure, or in ways that are not measurable at all, that difference would be accounted for in the fixed effects model. But state fixed effects do not control for factors that are changing in each state. Thus, we also include controls for change in every factor that we think could impact turnout in each state. Specifically, we include the following measures of state electoral conditions: (a) the share of the state's population that identifies as Democratic, (b) the amount of campaign spending in the state in the federal election, (c) the margin of victory in the state in the presidential election, (d) partisan control of the state Senate, House, and Governor's office, (e) whether or not statewide contests were contested, (f) whether or not statewide contests are open seats, and (g) candidate vote shares in statewide contests. In terms of state electoral laws, we control for changes in (a) the registration deadline and whether or not the state has (b) early voting, (c) vote-by-mail, (d) no excuse absentee ballots, and (e) same day registration. Finally, we also control for the following county-level demographics: (a) educational makeup (percent of adults with a bachelor's degrees), (b) income (median income), (c) age distribution (median age), (d) gender (percent female), (e) economic conditions (unemployment rate), family structure (share of households with children), and religion (percent Protestant, percent Catholic, and percent Jewish) of each county. Sources for all variables are detailed in Section 1 of the online appendix. For brevity purposes, only the key interaction terms are included in the table. The full regressions are included in Section 2 of the online appendix.

The first model uses change in turnout between 2012 and 2016 as the dependent variable, while the second model employs county turnout in 2016 as the dependent variable and includes county turnout in 2012 as a lagged independent variable.

The key variable in Table 1 is the interaction between the racial demographics of a given county and the implementation of a new strict ID law in the state. As the negative and significant interactions in both models in Table 1 show turnout declines significantly more in racially diverse counties relative to less diverse counties in states that enact strict ID laws over this period than it does in other states. Substantively, the effect is sizeable. Using the estimate from model 2 which is the more conservative estimate of the two regressions, we find that turnout in counties where 75% of the population was non-White declined 2.6 percentage points (relative to turnout in all White counties) more in Alabama, Mississippi, Virginia, and Wisconsin after those states instituted their strict photo ID laws, than it did in other states.⁹

Difference-in differences with mean balancing

One concern with the analysis to this point is that the states in the control group that have not implemented strict ID laws in our time frame may not represent ideal counterfactuals. If turnout trends in these states differ from turnout trends in the four new strict ID states, our results may be skewed. To address this concern, we construct a comparable control group through a mean-balancing method that balances on pre-treatment turnout and other key covariates in the years 2000–2012 before these strict ID laws were put in place (Hazlett and Xu 2018; see online appendix for details about the method).

Our results using the balancing method match what we found earlier. In Figure 1, we illustrate the impact of strict ID laws for counties with different racial demographics after balancing. The figure clearly shows that as the share of the county that is non-white increases, the negative impact of strict ID laws also increases. The model estimates that relative to turnout in all White counties, turnout in counties with a 75% non-White population declines 1.5 points more in states that just adopted strict ID laws than in states that didn't implement a strict ID law. Given that the margin of victory in Wisconsin in the 2016 Presidential election was only 0.77 percentage points, this is a meaningful effect.

In an alternative test, we balanced treated and control counties not only on the outcome variable – pre-treatment turnout– but also on key covariates like the racial makeup of each county. Fortunately, when we add percent non-white, percent Black, and percent Hispanic to our mean-balancing procedure, we arrive at nearly identical results (see online appendix).

	Change in county turnout (2012–2016)	2016 Turnout (w/lagged 2012 turnout)
Percent Minority * New Strict States	060 (.020)**	034 (.015)*
Percent Minority	037 (.013)**	056 (.008)**
New Strict States	044 (.006)**	026 (.005)**
R Squared	.66	.98
Number of Observations	2599	2599
County Demographic Controls	Υ	Y
Changes in State Political Context and State Electoral Laws Controls	Y	Y

Table 1. Testing the racial disparate of strict photo ID laws: 2012–2016.

Note: Figures are the regression coefficient and the standard error in parantheses.

**Difference is significant at the .01 level.



Figure 1. The marginal effect of strict photo ID laws conditional on percent minority.

Robustness checks

As a check on the robustness of these results in the online appendix, we engaged in a series of different tests which are included in the online appendix. First, since no two voter ID laws are the same and different laws in different states may be targeting different groups we looked at each strict ID state separately (see Section 6). We find closer to a consistent effect. The four states that initiated strict ID laws in our period – Alabama, Mississippi, Wisconsin, and Virginia – all experienced exceptionally high declines in turnout in racial diverse counties (relative to largely white counties) after those states instituted strict photo ID laws.

It is also possible that the same law affects different racial and ethnic groups differently. Thus, in Section 7 of the online appendix, we looked at the effects of these laws on Blacks and Hispanic separately. Our various tests were, however, inconclusive with some pointing to Blacks being disproportionately targeted by these laws, while others suggested that Hispanics were more impacted. In addition, we document other robustness checks that (a) exclude states with preexisting strict ID laws from the comparison set (Section 4), (b) conducted a placebo test using the years prior to the implementation of strict ID laws in our four states (Section 5), (c) employed a hierarchical linear model (Section 8), (d) only compared strict ID states to other Republican-led states (Section 9), and (e) used data on individual level turnout from North Carolina and the Cooperative Congressional Election Survey to help address concerns of the ecological fallacy (Section 10). These tests help to confirm the racially disparate impact of these laws.

Implications

Voter ID laws are becoming more common and more strict. The stakes for American democracy are high and growing higher by the year. In this article, we have attempted

to provide a rigorous empirical assessment of these laws. By focusing on data from recent elections after strict photo ID laws have been widely implemented, by using official turnout data to eliminate concerns over inflated and biased turnout patterns from selfreported survey data, and by employing a research design that incorporates longitudinal data and difference-in-difference tests, our analysis overcomes many of the core problems faced by previous studies.

The findings presented here strongly suggest that these laws do, in fact, represent a major burden that disproportionately affects minorities and significantly alters the makeup of the voting population. Where these laws are enacted, turnout in racially diverse counties declines, it declines more than in less diverse areas, and it declines more sharply than it does in other states. As a result of these laws, the voices of racial minorities become more muted and the relative influence of white America grows. An already significant racial skew in American democracy becomes all the more pronounced. If courts are indeed trying to gauge the burden these laws impose on minorities and others, then this new data should help the courts with their deliberations.

Notes

- 1. For a review of active voter identification cases see: https://www.brennancenter.org/our-work/research-reports/state-voting-rights-litigation-july-2019.
- 2. For example, Crawford vs Marion County Election Board (2008).
- 3. For example, United States Courts of Appeals for the Fourth Circuit No. 16-1468 (2016).
- 4. Coding for strict ID laws is based on the National Conference of State Legislators (2019) except for Alabama which is coded as a strict ID state because the only alternative to presenting an ID in that state is to have two election officials sign a sworn statement saying that they know the voter.
- 5. Data for the count- level vote totals are from the Atlas of US Elections and the Congressional Quarterly Voting and Election website.
- 6. To address migration into or out of the county, we also control for change in the county voting age population.
- 7. Only eight states (AL, GA, FL, LA, NC, PN, SC, and TN) ask for race/ethnicity when citizens register to vote.
- 8. Regressions include standard errors clustered at the state level and are weighted by county population size.
- 9. For this comparison, we drop states that already have strict ID laws. If we include states that implemented strict photo ID laws before 2012, the pattern is similar.

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Disclosure statement

No potential conflict of interest was reported by the author(s). Zoltan Hajnal worked as an expert witness for the NAACP in Greater Birmingham Ministries, et al. vs John. H. Merrill (a strict voter identification case in Alabama).

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CERTIFICATE OF SERVICE

I, Matthew Prairie Gordon, hereby certify that I have served true and accurate copies of the foregoing Affidavit - Affidavit in Support to the following on 04-06-2022:

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Attorneys for Plaintiffs Montana Democratic Party and Mitch Bohn

IN THE MONTANA THIRTEENTH JUDICIAL DISTRICT COURT YELLOWSTONE COUNTY

Montana Democratic Party, Mitch Bohn,

Plaintiffs,

WESTERN NATIVE VOICE, Montana Native Vote, Blackfeet Nation, Confederated Salish and Kootenai Tribes, Fort Belknap Indian Community, and Northern Cheyenne Tribe,

Plaintiffs,

Montana Youth Action; Forward Montana Foundation; and Montana Public Interest Research Group

Plaintiffs,

v.

Christi Jacobsen, in her official capacity as Montana Secretary of State,

Defendant.

Consolidated Case No. DV 21-0451

DECLARATION OF MATTHEW GORDON

I, Matthew Gordon, declare as follows:

My name is Matthew Gordon. I am over 18 years old and am an attorney with the law firm of Perkins Coie LLP. I am admitted to practice law in the State of Montana and am an attorney for Plaintiffs Montana Democratic Party and Mitch Bohn in this matter. I submit this declaration to provide the Court with true and correct copies of certain documents submitted in connection with Plaintiffs' Combined Response to Defendant's Motion for Summary Judgment in this matter.

1. Exhibit 1 is a true and correct copy of MIT Election Data + Science Lab, Voter Confidence (April 2021), https://electionlab.mit.edu/research/voter-confidence.

2. Exhibit 2 is a true and correct copy of the Expert Rebuttal Report of Dr. Alex Street.

3. Exhibit 3 is a true and correct copy of the NPR/PBS NewsHour/Marist Poll of 1,209 National Adults, downloaded from: https://maristpoll.marist.edu/wp-content/uploads/2021/10/NPR_PBS-NewsHour_Marist-Poll_USA-NOS-and-Tables B 202110251104.pdf (last visited Apr. 5, 2022).

4. Exhibit 4 is a true and correct copy of Andrew C. Eggers, Haritz Garro & Justin Grimmer, No Evidence for Systematic Voter Fraud: A Guide to Statistical Claims About the 2020 Election, 118 PNAS 1 (2021), downloaded from:

https://www.pnas.org/doi/pdf/10.1073/pnas.2103619118.

5. Exhibit 5 is a true and correct copy of Amanda Zoch, *Then & Now: How 8 Election Policies Have Changed Since 2000*, Nat'l Conference of State Legislatures (Feb. 16, 2021), downloaded from: https://www.ncsl.org/research/elections-and-campaigns/then-and-nowelection-policies-in-2000-and-2020-magazine2021.aspx/.

6. Exhibit 6 is a true and correct copy of Wendy Weiser, Justin Levitt, Catherine Weiss, & Spencer Overton, Response to the Report of the 2005 Commission on Federal Election Reform 2, 7 (2005), downloaded from: https://www.brennancenter.org/sites/default/files/2019-08/Report_Response%20to%20the%20Report%20of%20the%202005%20Commission%20on% 20Federal%20Election%20Reform.pdf.

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Exhibit 7 is a true and correct copy of Enrico Cantoni & Vincent Pons, Strict ID
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Exhibit 8 is a true and correct copy of the Expert Rebuttal Report of Dr. Kenneth
 R. Mayer submitted in this matter.

9. Exhibit 9 is a true and correct copy of Michael G. DeCrescenzo & Kenneth R. Mayer, *Voter Identification and Nonvoting in Wisconsin—Evidence from the 2016 Election*, 18 Election L.J. 342, 342 (2019), downloaded from:

https://www.liebertpub.com/doi/10.1089/elj.2018.0536.

10. Exhibit 10 is a true and correct copy of John Kuk, Zoltan Hajnal, & Nazita
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11. Exhibit 11 is a true and correct copy of Bernard L. Fraga & Michael G. Miller,
Who Does Voter ID Keep from Voting?, 84 J. Pol. 1 (2022), downloaded from:
https://www.journals.uchicago.edu/doi/10.1086/716282.

12. Exhibit 12 is a true and correct copy of Justin Grimmer & Jesse Yoder, *The Durable Differential Deterrent Effects of Strict Photo Identification Laws*, Pol. Sci. R. & Methods 1 (2021), downloaded from: https://doi.org/10.1017/psrm.2020.57.

13. Exhibit 13 is a true and correct copy of Matt A. Barreto, Gabriel R. Sanchez, and Hannah L. Walker, *Battling the Hydra: the Disparate Impact of Voter ID Requirements in North Dakota*, J. Race, Ethnicity, & Pol. 1 (2022), downloaded from:

https://doi.org/10.1017/rep.2022.1.

14. Exhibit 14 is a true and correct copy of the Expert Report of Barry C. Burden submitted in *Andrew Goodman Foundation v. Bostelmann*, No. 19-cv-955 (Jan. 20, 2020).

15. Exhibit 15 is a true and correct copy of Charles Stewart III et al., *Revisiting Public Opinion on Voter Identification and Voter Fraud in an Era of Increasing Polarization*, 68
Stan. L. Rev. 1455 (2016).

16. Exhibit 16 is a true and correct copy of Nat'l Public Radio, *Here's Why Concerns About Absentee Ballot Fraud are Overhyped* (Oct. 20, 2020),

https://www.pbs.org/wgbh/frontline/article/heres-why-concerns-about-absentee-ballot-fraud-areoverhyped/ (last accessed Mar. 23, 2022).

17. Exhibit 17 is a true and correct copy of Keith Schubert, '*Practically a unicorn*': *Profs say voter fraud allegations in Phillips Co. not part of larger issue*, Daily Montanan (Feb. 14, 2022), downloaded from: https://dailymontanan.com/2022/02/14/practically-a-unicorn-profssay-voter-fraud-allegations-in-phillips-co-not-part-of-larger-issue/.

 Exhibit 18 is a true and correct copy of Lisa Baumann, *Ending Election Day* registration sees little support, Associated Press (Oct. 19, 2014), downloaded from: https://www.greatfallstribune.com/story/news/local/2014/10/19/ending-election-day-registrationsees-little-support/17583087/.

19. Exhibit 19 is a true and correct copy of U.S. Election Assistance Commission, *Election Crimes: An Initial Review and Recommendations for Future Study* 9 (Dec. 2006), downloaded from:

https://www.eac.gov/sites/default/files/eac_assets/1/6/Initial_Review_and_Recommendations_fo r_Further_Study.pdf.

20. Exhibit 20 is a true and correct copy of Steven H. Huefner, Daniel P. Tokaji, Edward B. Foley, and Nathan A. Cemenska, *From Registration to Recounts: The Election Ecosystems of Five Midwestern States* 120 (2007).

21. Exhibit 21 is a true and correct copy of Steven F. Huefner, Nathan A. Cemenska, Daniel P. Tokaji, and Edward P. Foley, *From Registration to Recounts Revisited: Developments in the Election Ecosystems of Five Midwestern States* 41 (2011). 22. Exhibit 22 is a true and correct copy of Michael W. Sances and Charles Stewart III, *Partisanship and Confidence in the Vote Count: Evidence from U.S. National Elections Since* 2000, Electoral Studies 40:176-188 (2015), downloaded from: https://doi.org/10.1016/j.electstud.2015.08.004.

23. Exhibit 23 is a true and correct copy of Stephen Ansolabehere, *Effects of Identification Requirements on Voting: Evidence from the Experiences of Voters on Election Day*, PS: Political Science & Politics 42:127-130 (2009), downloaded from: https://doi.org/10.1017/S1049096509090313.

24. Exhibit 24 is a true and correct copy of Shaun Bowler, Thomas Brunell, Todd Donovan, and Paul Gronke, *Election Administration and Perceptions of Fair Elections*, Electoral Studies 38:1-9 (2015), downloaded from: https://doi.org/10.1016/j.electstud.2015.01.004.

25. Exhibit 25 is a true and correct copy of Keila Szpaller, *Election Security Bill Heads to Gov. Gianforte's Desk* (Apr. 27, 2021), downloaded from: https://dailymontanan.com/2021/04/27/election-security-bill-heads-to-gov-gianfortes-desk/.

26. Exhibit 26 is a true and correct copy of Sam Wilson, *GOP in Missoula Pays for Recount to Ease Fraud Concerns* (Mar. 29, 2022), downloaded from: https://missoulian.com/news/state-and-regional/govt-and-politics/gop-in-missoula-pays-forrecount-to-ease-fraud-concerns/article 0304fa52-a9c0-5502-ad63-78fa2938af19.html.

27. Exhibit 27 is a true and correct copy of Alex Sakariassen, *Missoula County GOP to Republican Election Skeptics: 'No Voter Fraud*' (Apr. 1, 2022), downloaded from: https://montanafreepress.org/2022/04/01/missoula-election-allegations-challenged/.

I declare under penalty of perjury that the foregoing is true to the best of my knowledge and belief.

Dated this 5th day of April, 2022.

Nati

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Exhibit 11

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Voter identification (ID) laws have sparked concerns of vote suppression, but existing evidence relies on aggregate analyses or survey self-reports. We leverage unique information from Texas, where registrants without ID filed "reasonable impediment declarations" (RIDs) before voting. Linking 16,000 RID forms to the Texas voter file, we provide the first direct documentation of the traits of voters who would be stopped from voting under strict identification laws. Our preregistered analysis finds registrants voting without ID in 2016 were disproportionately Black and Latinx when compared to voters voting with ID. Examining voters' stated reasons for not providing ID, we find socioeconomic hardships are not the most commonly cited impediment, but voters with hardships were less likely to vote in a strict-ID election than those who previously had identification. Our findings indicate that strict identification laws will stop a disproportionately minority, otherwise-willing set of registered voters from voting.

tates exercise a great deal of control over their voting laws, resulting in substantial cross-state variation in voters' experiences on Election Day. In the wake of the contested 2000 presidential election, the 2002 Help America Vote Act (HAVA), and the Supreme Court's decision in Shelby County v. Holder 570 U.S. 529 (2013), there was a flurry of activity as states modified their election regulations. Some of the most controversial new policies are "voter ID" laws, which require poll workers to request photo identification (ID) from registered voters. The recent trend in voter ID laws has been toward "strict" policies, which mandate that in person voters cannot cast a regular ballot without first presenting a photo ID from a predefined list. Such laws are controversial because some otherwise-eligible voters may not have a qualifying ID and thus could be considered disenfranchised or suppressed by voter ID laws.

The groups least likely to possess qualifying ID, such as young, Black, Latinx, less frequent, or less affluent voters, are also more likely to support the Democratic Party. As such, the debate over voter ID has taken on a partisan hue and has attracted attention from policy influencers and scholars seeking to determine whether the laws prevent individuals from voting and, if so, whether their impact disproportionately burdens voters from certain groups. While journalists and advocates strongly assert both of these claims (Berman 2015; Wang 2012), the broader body of academic work appears less certain (Ansolabehere 2009; Barreto, Nuño, and Sanchez 2009; Burden 2018; Erikson and Minnite 2009; Grimmer et al. 2018; Hajnal, Kuk, and Lajevardi 2018; Hajnal, Lajevardi, and Nielson 2017; Highton 2017).

We leverage a unique change in a state's voter ID statute to examine who is affected by voter ID mandates. While Texas implemented a strict voter ID law in the 2014 election, a lastminute federal court decision allowed Texans without qualifying ID to vote in the 2016 election. These voters were required to submit a paper declaration listing the reason they lacked ID. We link these declarations to entries in the Texas voter file, extracting turnout data and address information that allows us to model individual race/ethnicity. Using this information, we are able to study the characteristics and geographic distribution of the over 16,000 Texans who arrived at polling

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places without proper ID—and would have been turned away under the previous strict ID policy.

We find evidence pointing to the deleterious effects of voter ID laws for particular subsets of the population. Our preregistered analysis establishes that at minimum, more than 16,000 Texans would have been disenfranchised for lack of compliant ID in 2016. We also demonstrate that registrants voting without ID in 2016 were less likely to vote when a strict ID mandate was in place, and significantly more likely to be Black and Latinx, than the population voting with ID. Evaluating the mechanisms that produce these effects, we find that the most commonly cited reason for not providing ID is not related to socioeconomic hardships: instead, most voters who voted without ID possessed photo ID but for some reason could not produce it on Election Day. County-level factors do not appear to explain these results. Taken together, our analyses demonstrate that strict voter ID laws prevent otherwise eligible voters from voting-including a large group that possesses photo ID-and that such laws have disproportionately negative impacts on minority citizens.

HOW MIGHT VOTER ID LAWS SHAPE THE ELECTORATE?

Canonical theories of voter turnout posit that the decision to vote is a cost-benefit calculation (e.g., Downs 1957) influenced by the availability of individual resources (e.g., Verba et al. 1993). Among adults who do not already possess qualifying ID, voter ID mandates increase the cost of voting and, in theory, will reduce turnout. Furthermore, for those with economic hardships, disabilities, or family care responsibilities preventing them from acquiring photo ID, such laws add to existing formidable hurdles to political participation. Yet this assumes that voters have the resources, in the form of political knowledge, necessary to know that they lack qualifying ID. In states with the strictest forms of photo ID laws, some may incorrectly assume that an ID they possess-such as a university-issued ID or out-of-state driver's license-is acceptable, while other voters who lack ID might not hear about a voter ID law at all. Voters in either group may be turned away despite the ability to acquire ID. Thus, regardless of desire to overcome hurdles to voting, voter ID laws have clear implications for turnout among voters who lack ID.

Well before the post-2008 spike in voter ID laws, the bipartisan National Commission on Federal Election Reform indicated that a substantial share of "poor and urban" adults did not have photo ID (National Commission on Federal Election Reform 2001). Conforming with the resource-based models of who is already less likely to vote, subsequent research has found that low-turnout-prone subsets of the population are less likely to have compliant ID (Barreto et al. 2009; Highton 2017; Stewart 2013).¹ A multistate and multiyear meta-analysis by Barreto and colleagues (2018) established that minority citizens are consistently less likely to have photo ID than non-Hispanic Whites. Importantly, this differential may not be solely a result of economic hardship, as legislators may intentionally target minorities in crafting the list of acceptable IDs or when deciding to introduce voter ID laws at all (Bentele and O'Brien 2013; Biggers and Hanmer 2017; Hicks et al. 2015; Highton 2017; Rocha and Matsubayashi 2014).² Minority voters might interpret these conditions as a signal of hostility, which may result in feelings of alienation from the political process that could deter even minority voters who have acceptable ID.

Despite these factors, analyses of the effect of voter ID laws on turnout have produced mixed results. Early studies indicated that few individuals cited ID requirements as keeping them from turning out to vote (Ansolabehere 2009; Hershey 2009; Mycoff, Wagner, and Wilson 2009). Highton's (2017) review of the literature notes that previous work has not uncovered a large effect of voter ID on turnout and underscores methodological challenges that exist when examining statelevel aggregate data. Furthermore, while the possibility of a disparate effect of voter ID laws is clear, again, results are mixed (Government Accountability Office 2014; Hood and Bullock 2012; Rocha and Matsubayashi 2014). Recent work by Hajnal and colleagues (2017) uses survey data to investigate the relationship between implementation of voter ID statutes and the overall Black-White and Latinx-White turnout gaps, finding racial/ethnic disparities in turnout grow when strict ID laws are implemented. Grimmer et al. (2018) contest these findings, again indicating that debates regarding the impact of voter ID laws are ongoing (Burden 2018; Hajnal et al. 2018).

However, there may be theoretical reasons for the apparently limited relationship; for instance, any negative impact on turnout could be matched (or exceeded) via a "backlash effect" as Democrats, in particular, mobilize in response to what they perceive as an unjust law (Valentino and Neuner 2017). Civic education campaigns can offset decreases in turnout resulting from voter ID laws (Citrin, Green, and Levy 2014; Hopkins et al. 2017); Mayer and DeCrescenzo (2018) find that a substantial share of nonvoters believe they do *not* have qualifying ID, when

^{1.} See also Stephen Ansolabehere, 2014, "Corrected Supplemental Report," *Marc Veasey, et al. v. Rick Perry, et al.*: United States District Court, Southern District of Texas, Corpus Christi Division: 2:13-cv-193, document 600-1, September 16, 2014.

^{2.} E.g., in North Carolina State Conference of NAACP v. McCrory 831 F.3d 204 4th Cir. (2016), the Fourth Circuit Court of Appeals found that North Carolina lawmakers "target(ed) African Americans with almost surgical precision" when lawmakers sought lists of residents' ID possession by race, found African Americans were less likely to have driver's licenses, and barred alternative IDs that they were more likely to possess.

in reality they would be allowed to vote even under a strict ID mandate. Quantifying the effect of ID mandates—while combining direct, deterrent, and mobilizing forces—remains elusive.

While deterrent and backlash effects of voter ID laws are important to analyze, the desire to quantify the net effect of voter ID laws distracts from deeper analyses of who is most affected by these laws: voters without ID. Only a handful of studies have focused on this population, finding that registrants without qualifying ID prior to ID law implementation are less likely to vote in the subsequent strict ID election (Hood and Bullock 2012) or nonstrict ID election (Henninger, Meredith, and Morse 2018). While minority voters' lower rates of photo ID possession suggests that minority voters will be most affected, mixed evidence emerges on the racial/ethnic composition of these nonvoters as well. Notably, Hood and Bullock (2012) find White registered voters without ID were more likely to stay home as a result of Georgia's strict ID law than minority registrants. We seek to return attention to this theoretically crucial population and, as we detail in the next section, a unique sequence of election law changes allows us to better understand whose turnout is affected by voter ID mandates.

LEVERAGING CHANGING VOTER ID MANDATES

Though many states have long had some sort of ID requirement for voters, in the mid-2000s, Indiana became the first state to require that voters present government-issued photo ID. After the Supreme Court upheld Indiana's law in Crawford v. Marion County Election Board 553 U.S. 181 (2008), other states soon followed suit. In 2011, Texas enacted Senate Bill 14 (Election Code \$63.001 et seq., hereafter SB 14), shifting from a more common non-photo ID requirement to the strictest photo ID requirement in the nation, designating only three types each of acceptable federal and Texas-issued IDs.3 Coupled with the fact that Texas was by far the largest state to pass a strict voter ID law, the small number of acceptable IDs meant that SB 14 had the potential to impede voting for an especially large number of people. Indeed, expert testimony in subsequent litigation revealed that more than 600,000 registered voters in Texas lacked adequate ID under SB 14.4

The Department of Justice initially blocked SB 14 under the preclearance provisions of the Voting Rights Act (VRA), but it was ultimately implemented hours after the US Supreme Court struck down the VRA's coverage formula in *Shelby County v*.

Holder 570 U.S. 529 (2013). SB 14 was challenged in federal court as discriminatory, continuing a protracted battle over the ID provision in litigation that became known as Veasey v. Perry 71 F. Supp. 3d 627, S.D. Tex. (2014). In October 2014, the Fifth Circuit Court of Appeals temporarily blocked a previous district court ruling striking down the law, so SB 14 was fully in place for the 2014 general election. However, in 2015, a threejudge panel in the Fifth Circuit affirmed a district court ruling that the law had a "discriminatory effect in violation of Section 2 of the Voting Rights Act" (Veasey v. Abbott, 830 F.3d 216, 86 [5th Cir. 2016]). The entire Fifth Circuit affirmed this ruling in July 2016, sending the case back to the US District Court for the Southern District of Texas, whose job it was to find an interim solution "that disrupts voter identification rules for the 2016 election season as little as possible, yet eliminates the [VRA] discriminatory effect violation."

In August 2016, the district court crafted such a remedy, ordering that all voters who possessed a required ID must produce it before voting.⁵ However, the court mandated that voters who lacked ID that would satisfy SB 14's requirements should be allowed to vote if they met two conditions: First, voters had to complete a "reasonable impediment declaration" (RID) attesting that they did not possess a valid photo ID and stating the reason they could not obtain one. Second, before obtaining an RID form, voters had to produce "supporting identification" from a wider list of sources largely coinciding with the previous non–photo ID requirement, including a government document, utility bill, bank statement, paycheck, or birth certificate.

The district court thus weakened SB 14 to a nonstrict photo ID requirement by allowing voters lacking the required photo ID to vote-so long as they completed an RID. However, this change from the 2014 election was not well advertised by Texas election officials-who spent about one-fifth the sum that the much smaller state of Missouri allocated to educate voters about a similar law-and often provided incomplete, unclear, or inaccurate information (Huseman 2017). Indeed, in September 2016, the Department of Justice found the state was using "incorrect and far harsher" language in poll worker training regarding circumstances under which individuals could vote without qualifying photo ID (Malewitz 2016). Later, as early voting began, Bexar County (San Antonio) was sued by Mexican American Legal Defense and Educational Fund for displaying and providing misleading information regarding the change to voter ID laws (Zielinski 2016). While the court ruling made it possible for individuals to vote without qualifying ID, many people likely assumed (or were told) that the strict ID regime was in place when deciding to vote.

^{3.} Acceptable ID includes US military ID, US passport, US citizenship certificate, Texas election ID certificate, Texas ID or driver's license, or Texas license to carry a concealed handgun.

^{4.} Stephen Ansolabehere, 2014, "Corrected Supplemental Report," *Marc Veasey, et al. v. Rick Perry, et al.*: United States District Court, Southern District of Texas, Corpus Christi Division: 2:13-cv-193, document 600-1, September 16, 2014.

^{5.} Texas was also required to accept ID that had been expired for up to four years, as opposed to the statute's 60-day limit.

ooo / Who Do Voter ID Laws Keep from Voting? Bernard L. Fraga and Michael G. Miller

In mandating that individuals who arrive at the polls without qualifying ID sign an RID, the district court's ruling creates a unique opportunity to observe a population that would have been turned away from the polls in the absence of the eleventhhour order-and which has heretofore been impossible to observe.6 The RIDs include voters' names and in many cases other identifying information, as well as the reason(s) they cited for lacking appropriate ID. Merged with other data sources, these records therefore facilitate unprecedented insight into the demographics and previous voting behavior of Texas voters lacking ID in 2016, while avoiding the documented problems associated with survey data in this area.7 Notably, Henninger and colleagues (2018) employ a similar strategy to the one we use in this article, exploiting Michigan's nonstrict voter ID law that requires voters lacking ID to sign an affidavit. They find that a very small minority (0.6%) of Michigan voters lack ID, but also that non-White voters were between two-and-a-half and six times more likely than Whites to arrive at the polls without qualifying ID.

We believe that administrative records may allow researchers to better understand the impact of voter ID laws. While existing analyses make important progress, the particulars of Texas' voter ID implementation allow us to go even further. Like Henninger and colleagues (2018), we can match RID filers to voter records and other databases to compare their demographics to voters who presented ID. Moreover, because the district court ordered voters to state the reason they lacked ID, we can include a descriptive element in our analysis, examining whether voters would be deterred as a result of enduring socioeconomic hardships or more ephemeral issues. In short, the RID data allow us to engage important, largely unanswered questions of paramount importance to assessing the impact of strict voter ID laws: Whom does voter ID legislation keep from voting, and why?

DATA

We obtained copies of each RID that voters completed via requests made under the Texas Public Information Act. In total, we received 16,097 unique RID forms organized by county. An example of the most common RID form may be found in figure 1. The upper portion of each RID provides a space for



Figure 1. Example: reasonable impediment declaration

the voter to print and sign her name and to indicate the date, a brief statement indicating that the voter faces a "reasonable impediment or difficulty that prevents [her] from getting an acceptable form of identification," and a series of boxes allowing the voter to claim one of eight reasons for lacking proper ID.⁸ These options included lack of necessary documents, disability, family or work obligations, lack of transportation, lost or stolen ID, an application for an ID that was not yet received,

^{6.} Under most strict ID statutes, voters without ID can cast a provisional ballot that is counted if they provide ID within a narrow time frame after voting. However, election administrators exercise some discretion in offering this opportunity. One study indicates that more than 75% of individuals casting provisional ballots for lack of ID do not return with ID (Pitts 2013). Thus we do not consider the availability of provisional ballots to be equivalent to the regular ballots that could be cast without ID under SB 14.

^{7.} In their failed replication of Hajnal et al. (2017), Grimmer et al. (2018, 1051) note, "National surveys are ill-suited for estimating the effect of state election laws on voter turnout . . . researchers should turn to data that allow more precision than surveys offer."

^{8.} More than 90% of RID forms we received were as depicted here or translated into Spanish, Vietnamese, or Chinese. For Maverick County, we also received 133 forms that combined a RID with an in person absentee ballot request. We have not included these forms in our analysis, as absentee ballot submissions were not provided for other counties. Five additional non-standard RID forms are excluded.

or some other reason.⁹ The RIDs also include a section completed by the election judge listing precinct location, the name of the certifying election judge, and which alternate form of ID the voter provided.

As figure 1 indicates, RIDs were completed at the polling place with much of the information handwritten. The authors and a team of research assistants coded each RID form by hand, entering the name, impediment, date, judge, precinct, and all other information into a spreadsheet with one entry per RID form. Occasionally, additional information was provided on the RID form, either because the form included the voter's (handwritten) Texas Voter Unique Identifier (VUID) number or because a pollbook-generated sticker was placed on the form providing full name, address, and/or VUID.¹⁰

In February 2017, we acquired a copy of the current Texas voter registration file from the secretary of state's office, along with turnout history for each registrant (including canceled registrations) for federal general elections in 2008, 2010, 2012, 2014, and 2016. The Texas voter file contains far more information about each registrant than did the RID forms, and given that individuals had to be registered to vote to file an RID, we merged each unique RID form to a unique record in the voter file. We found that 2,297 RID forms included the Texas VUID for the voter (14.3% of RIDs) and could be matched directly into the voter file. For the remaining RIDs, we used a combination of county, name, information about whether the RID was filed on Election Day or in early voting, and any additional information on the form to match RIDs to voter file records. Of RIDs, 12,624 (78.4%) matched with precisely one Texas voter file record using this information, and 761 (4.7%) matched to multiple records in the voter file.11

After merging RID forms with Texas voter file records, we sought to add an additional key demographic variable: voter race/ethnicity. The Texas voter file does indicate whether a registrant has a Spanish surname, but does not differentiate between Whites, African Americans, and Asian Americans. To do so, we used address, sex, date of birth, and surname information to generate probabilistic estimates of the race of every individual in the Texas voter file. Geocoding each address to the census tract level with Open Street Map data, Google Geocoding application programming interface data, and Federal Communications Commission block information, we used the wru package in R to generate these estimates (Imai and Khanna 2016).¹² For each individual, we thus gained a probability that the registrant is (non-Hispanic) White, Black, Hispanic/Latinx, Asian, or "Other Race."¹³

Combining all of the above match types, 15,682 RID forms were matched to Texas voter records. Excluding the small number of RIDs with no name or other identifying information, 98.7% of RIDs were successfully matched. Thus, the RID data, merged with the Texas voter file and modeled race/ethnicity, provide a complete picture of the voters who reported a reasonable impediment to obtaining ID in the 2016 election, along with their stated rationale, age, race, gender, and vote history. That said, given that there are certainly voters who did not receive information about the possibility of filing an RID, there is likely a nontrivial number of Texans who mistakenly believed that their lack of ID would bar them from voting-and who therefore did not turn out to vote.¹⁴ In terms of the number of voters affected by the Texas law, our tally of RID forms is therefore best understood as a lower bound of the overall effect of Texas's voter ID law.

AREAS OF INQUIRY AND ESTIMATION STRATEGY

Prior to conducting the above matching process, we outlined our areas of inquiry and preregistered key parts of our analysis.¹⁵ In our preanalysis plan, we focused on three questions: Were RID filers less likely to vote under a strict ID mandate? Are RID filers disproportionately non-White? What impediments to obtaining ID do voters who can produce ID cite? Hypotheses and estimation strategies related to these questions were preregistered while research assistants were coding the RIDs by hand—but prior to merging RIDs with the Texas voter file. We believe that the decision to preregister our analyses is important to consider when evaluating the credibility of our findings; past work on voter ID laws has been critiqued for unclear and nonobvious estimation strategies (Grimmer et al. 2018).

First, we sought to determine whether Texas' strict photo ID law barred individuals who lacked qualifying ID from

^{9.} These options were mandated by the district court and were listed on all RID forms.

^{10.} In fig. 1, the voter's last name and signature is hidden. In the forms with which we were provided, this information was not hidden, though in some counties additional information provided by a pollbook sticker (such as address) was redacted. For approximately 1% of provided RIDs (211), there was no voter name information provided on the form, the signature was not legible, and no other identifying information was provided.

^{11.} A disproportionate share of these multiple matches have a Spanish surname in the Texas voter file. We discuss how we account for multiply matched individuals below.

^{12.} The appendix provides more details regarding individual race estimation.

^{13.} As a result of difficulties in parsing addresses and the distinctiveness of some surnames, 0.7% of voters in 2016 do not have race estimates (1.1% of matched RID filers).

^{14.} It is also possible that though the "secondary" ID required for RID filers was the same as the pre-strict law requirement in Texas, voters did not hear about the need to bring a secondary ID to file an RID and were similarly turned away.

^{15.} Evidence in Governance and Politics ID #:20180205AA. Available at https://osf.io/c58qm.

voting in 2014. SB 14 was in full effect in 2014 before being reduced to a nonstrict form by the district court's injunction to vote via the RID process in 2016. We cannot directly observe which people lacked ID in 2014 yet would have voted in the absence of the strict law. Thus, we cannot make assertions about the effect of the 2014 strict ID law on non–ID holders who did not file an RID in 2016 and did not vote, since our measure of ID holding is posttreatment. However, 2016 RID filers may be a population more likely to lack ID in 2014 and thus be prevented from voting in 2014. If RID filers were less likely to vote under a strict ID regime than those who voted with ID, we should expect that voter turnout in 2014 was lower for 2016 RID filers than for non-RID-filing voters in 2016.

In our preanalysis plan, we stated that we would use a nonparametric difference-in-differences model that uses the RID data to identify individuals who voted without qualifying ID in 2016 and then examine whether those individuals were less likely to vote in the 2014 election than 2016 voters who did have ID, after accounting for trends in pre-2014 turnout at the individual level. Full results and discussion of this analysis may be found in appendix A2, but in short, we find that RID filers were significantly less likely to vote in the previous strict ID election than those who voted with ID in 2016. As a result, we are confident that RID filers are at least part of the population of voters who would be turned away from the polls under a strict ID mandate. Such an interpretation is supported by contextual information indicating that most individuals did not intentionally avoid producing ID because of the availability of the RID option. As mentioned above, awareness of the court order was (and is) not high, and election officials were accused of not devoting enough preelection resources to advertising the change to the strict voter ID mandate. The analysis of cited impediments below also indicates a small portion of individuals voting without ID to "protest" ID mandates or because they were not aware they needed ID to vote. While we cannot directly measure the population that was deterred from attempting to vote by the strict photo ID law, it is clear that individuals arriving at the polls without ID in 2016 were substantially less likely to vote when the strict regime was in force.

With this in mind, our first set of main results is a distributional comparison with individual-level race/ethnicity estimates to determine whether non-White voters in 2016 were disproportionately likely to vote using RIDs. In our preanalysis plan, we indicated that we would compare the share of the 2016 voting population without qualifying ID that is Black, Latinx, and/or Asian—as well as the overall non-White share—to the share of the 2016 voting population with qualifying ID that is Black, Latinx, Asian, and/or non-White. Guided by previous literature (e.g., Barreto et al. 2009; Barreto et al. 2018; Stewart 2013), we hypothesize that non-White registered voters

are less likely to have qualifying ID and thus will be less likely to present said ID at the polling place; non-Whites should make up a disproportionate share of RID filers as a result. If Texas's strict voter ID law disproportionately affected racial and/or ethnic minorities, we should therefore observe a higher proportion of non-Whites among RID filers than among voters overall.¹⁶ In making this distributional comparison, we make no assumptions regarding the population that is deterred from attempting to vote or registering to vote for reasons associated with the strict voter ID laws. Instead, we examine whether non-White individuals are less likely to have qualifying ID when arriving at the polling place.

The preregistered distributional comparison allows us to reevaluate claims made in previous work regarding populations most affected by voter ID laws. However, drawing on the rich data set provided by the RID filings, we extend our preregistered analyses and examine the mechanisms that shape the patterns explored above. The first exploration of potential mechanisms focuses on the reasons voters give for not providing qualifying ID, an underexplored area of inquiry in previous research. The policy debate surrounding voter ID laws often centers on lowering hurdles to obtaining ID for the subpopulation of voters who lack it under the assumption that they have never had a photo ID. Another common theme in public debate over voter ID laws is that older and/or less affluent voters, as well as those from minority groups, find it more difficult to obtain ID because of a lack of necessary documents (Horwitz 2016). The RIDs require voters to list the reason they cannot obtain ID, such that we can scrutinize the checkboxes on the RIDs and the rationales that voters wrote after choosing the "Other" option. We also examine differences in rates of voter turnout in the previous strict ID election depending on the impediment type that an RID filer listed in 2016.

As a final step, we evaluate whether county-level factors shape the rate of RID filing among 2016 voters. Using information about the county of the RID filer, we constructed rates of RID filing among all 2016 voters and evaluated how countylevel factors affected them. Such an analysis clarifies whether administrative discretion produced differences in rates of RID filing, a mechanism that would imply individual-level

^{16.} To account for the 4.7% of RID filers who matched to multiple records in the Texas voter file, we weight each entry in the voter file with a value of 0 if she was not matched to an RID, 1 if she was a unique match to an RID, and a value inversely proportional to the number of other voter file records to which the single RID matched if she was not a unique match. For example, a voter file entry would have an RID value of 0.5 if it was one of two matches to a single RID, as it has a 50/50 chance of being an RID filer. The existence of multiply matched individuals was not anticipated when developing the preanalysis plan. See the appendix for a more extended discussion of this deviation.

	White	Black	Latinx	Asian	Other
Voted with ID (%)	63.5	11.4	19.8	3.6	1.7
Ν	5,662,757	1,014,706	1,764,490	316,159	153,748
Voted without ID (%)	57.7	16.1	20.7	2.9	2.6
Ν	8,409	2,353	3,014	418	383

Table 1. 2016 Voters	by	Race	and	ID	Usage
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Note. Includes individuals marked as having cast a ballot in the Texas voter file. "Voted with ID" represents the percentage or number of voters who were not matched to reasonable impediment declarations (RIDs). "Voted without ID" represents the percentage or number of voters who were matched to RIDs. Of RIDs, 1.3% have not been matched to voter file records and are thus included in the "Voted with ID" category. Race could not be estimated for 0.7% of 2016 voters; these individuals are excluded.

correlations could be a product of jurisdiction-level variation in implementation.

RESULTS

Non-Whites are more likely to vote without ID than Whites

We first examine the racial/ethnic composition of the population that votes without qualifying ID, compared to those that voted with qualifying ID. We term this test a "distribution comparison" in the preanalysis plan. To estimate the racial/ ethnic composition of the 2016 voting population that voted with ID versus without, we summed the probabilities that each voter was of a particular racial/ethnic group (Elliott et al. 2008). In effect, this allows us to account for uncertainty in race/ethnicity estimates, and when combined with uncertainty in who filed an RID for multiply matched records, ensures that our results are not influenced by differences in unique match likelihood across racial/ethnic groups.¹⁷

Table 1 provides estimates of the racial/ethnic composition of the population voting with ID in 2016 (non–RID filers) and the population voting without ID in 2016 (RID filers). Both the percentage and the estimated *N* for each group of 2016 voters are provided. The first three columns of table 1 demonstrate that the population voting without ID in 2016 was disproportionately Black and Latinx and substantially less (non-Hispanic) White when compared with the population voting with ID. We estimate that 63.5% of Texas voters voting with ID were White in 2016, while only 57.7% of Texans voting without ID were White—a difference of 5.8 percentage points. Of 2016 voters voting with ID, 11.4% were African American, while over 16% of non-ID voters were African American. For Latinx people, we see a smaller difference between the RID- and non-RID-filing populations, but again, RID filers are disproportionately Latinx. Asian Americans, on the other hand, are slightly less likely to file RIDs than other groups. Individuals grouped as "Other" in the race/ethnicity estimates are a 1 percentage point larger share of RID filers versus non–RID filers.

In the preanalysis plan, we wrote that a two-sample *t*-test would be used to compare the racial/ethnic distribution of RID filers versus 2016 voters who did not file an RID. In independent tests, we indicated that we would examine the Black, Latinx, Asian, and overall non-White share across RID filing status. To do so, we are forced to separate RID filers from non–RID filers strictly, removing the roughly 5% of multiply matched individuals. Tests of statistical significance indicate a p < .001 for African Americans, Latinxs, and the overall non-White share. For Asian Americans, the *t*-test indicates that Asians are significantly less likely to be in the RID-filing group. To incorporate multiply matched individuals, we instead estimate a linear regression model with our nonbinary RID measure, which accounts for uncertainty in who filed an RID. Under this test, all of the differences in table 1 are statistically significant.

Previous literature asserts that racial/ethnic minority groups are less likely to have qualifying forms of ID in strict voter ID states. Thus, in our preanalysis plan, we hypothesized that the composition of the RID-filing population would be more minority, and less non-Hispanic White, than the non-RID-filing population of 2016 voters. Table 1 confirms our preregistered hypothesis, with the strongest difference for African Americans, who are substantially more likely to not provide qualifying ID at the polls. Black voters were approximately 54% more likely to vote without ID than non-Hispanic Whites, while Latinx voters were 14% more likely to do so than non-Hispanic Whites. Under a strict voter ID law, such as that in force in Texas in 2014, minority voters would be disproportionately likely to show up to vote, but be turned away at the polls and thus prevented from

^{17.} For example, Latinx RID filers are disproportionately likely to match to multiple voter file records and are easier to classify than African Americans or Whites. Categorical methods of estimating race, or categorical definitions of who filed an RID, could produce an upwardly biased estimate of this population.



Figure 2. Impediments cited by Texas voters in 2016

participating in an election that they would like to participate in and are eligible to vote in absent the strict ID law.

Many voting without photo ID possess photo ID

Are voters' traits associated with specific reasons they cited for lacking qualifying ID, and are those reasons consistent with some of the commonly posited narratives surrounding voter ID laws? As described above, the mandated RID format required eight checkboxes allowing voters to say why they lacked photo ID. An examination of these responses allows us to both assess the veracity of commonly held assumptions about voters who lack ID and better understand the mechanisms that induce the disparate turnout and racial/ethnic patterns found above. If voter traits are correlated with cited impediments, policy efforts to diminish the deleterious effects of voter ID laws might also be improved.

Figure 2 depicts the percentage of RID-filers citing each impediment (some voters selected more than one option). Family obligations are the least cited, with 3.8% of people selecting that reason; 6–8% of voters cited either pending application, lack of transportation, problems with necessary documents, or an illness or disability as impeding them from obtaining appropriate ID. About 11.7% of voters cited work obligations. Of the categories listed as checkoffs on the RID petitions, "Lost or stolen" is the most widely chosen, with 27% of voters selecting it. That said, a clear plurality (36%) of voters chose the "Other" option, writing their own explanation for why they lacked ID.¹⁸

The frequency with which voters opted for the "Other" category suggests that the potential impediments that the district court mandated for the RID forms were not all-encompassing. We therefore further coded the RID petitions into categories based on the explanation that voters wrote in the "Other" field on the RID document. The frequencies of those responses appear in table 2.¹⁹ The clear leader among these responses was a change of address that had not yet been reflected on the voter's ID. A majority of the voters (nearly 3,000) marking the "Other" option on the RID form explicitly mentioned a recent move; an additional 338 voters explicitly mentioned their relocation-related status as students.²⁰ More than 650 voters said that while they possessed ID, they forgot it on Election Day. About 230 voters presented a nonqualifying ID (such as an expired driver's license), while an additional 101 cited legal issues such as a suspended driver's license. Finally, about one-fifth of voters marking "Other" did not offer further insight into the impediment they faced. For instance, many voters simply reiterated that they did not have ID rather than stating why they lacked it.

In tandem with the marked checkboxes, the recoded "Other" responses can shed additional light on the broad reasons why voters lacked acceptable ID. For instance, we can see which voters are "ID-Capable"—those who have demonstrated a previous ability to obtain photo ID—by binning those who said they had either lost or forgotten their ID or were refusing to show it in order to protest SB 14. We can also identify voters with a relocation-related problem by combining voters who referenced a student status, a recent move, or awaiting new ID after applying for it. We classify all other RID-filers (except for those for whom the reason was unclear) as having an enduring hardship that impedes them from acquiring acceptable ID.

Figure 3 depicts the percentage of RID-filers falling into each of these bins. For more than 33% of RID filers, the impediment appeared to be an enduring hardship. This is consistent with much of the popular conversation surrounding voter ID laws, which often assumes that a lack of ID is a longstanding and difficult-to-overcome condition. However, figure 3 also shows that more than 5,000 voters—whose petitions made up more than 31% of all RIDs—could be classified as being ID capable.²¹ Furthermore, about 29% of voters had recently relocated, so their ID may have been from another state. If we assume that voters who cited a recent move had obtained

^{18.} This pattern holds up fairly well when examined by race, while White, Black, and Latinx voters all selected "Other" and "Lost" most frequently. See fig. A1 (figs. A1–A3 are available online).

^{19.} These responses were coded by hand. When voters marked "Other" and then described a reason consistent with one of the checkbox categories such as an illness—we recoded "Other" as zero and reassigned the voter to the appropriate category.

^{20.} It is often difficult to discern from the RIDs why a voter has recently moved. While students broadly fit in the "Relocation" category, we placed voters in the "Student" category if they referenced their status as a student on the RID.

^{21.} The same relative ranking is observed for Black and White voters separately. Latinx voters were less likely to report a relocation and more likely to be in the "ID Capable" category. A multivariate regression also indicates Black and Latinx RID filers were less likely to cite a relocation-related impediment and more likely to be in the "ID Capable" category, controlling for age and gender. See the appendix for more details.

Table	2.	Coded	Respon	ises	from	Written	Descriptions	of
Voters	Se	lecting	"Other"	Opt	ion			

	Count	%
Recent relocation	2,971	51.2
Other/unclear	1,236	21.3
Forgot ID	658	11.4
Student	338	5.8
Presented noncompliant ID	232	4.0
Legal issues	101	1.7
Cost	81	1.4
Lack of time	70	1.2
Protesting law	46	.8
Administrative decision	41	.7
Ignorance of law	23	.4
Religious objection	2	.03

photo ID while living at their prior address, a combination of the "Relocation" and "ID-Capable" categories in figure 3 indicates that a majority of voters who filed RIDs in 2016 had demonstrated the capability to obtain compliant ID at some point.²² Our results therefore suggest that the reasons people have for lacking photo ID might be more varied than previously thought, implying that some voters are more susceptible to enduring disenfranchisement as a result of voter ID laws than others.

RID filers were less likely to vote in 2014

These results beg a question: Do we see lower participation in the 2014 election (when the law was fully implemented with no RID option) for those indicating a hardship-related impediment in 2016? The models in table 3 compare turnout in previous elections among those who filed RIDs in 2016 and those who did not, while separating RID filers by the binned impediment they listed.²³ Here we restrict the analysis to those who were registered on or before October 1, 2014, and were thus eligible to vote in the 2014 election.²⁴ Across all groups, we see substantially lower turnout in 2014, when the law was fully implemented with no RID option. Turnout in 2010 and 2012 is also lower for RID filers regardless of impediment—indicating that as a group, they are less habitual voters—but in no circumstance is the decrease in turnout as large as in 2014.²⁵ Thus, regardless of the impediment listed, turnout for RID filers was significantly lower in that strict ID election than in other years.

Yet in theory, individuals indicating a hardship—such as lacking necessary documents or work obligations—should be even less likely to vote in 2014 than those who may have temporarily lacked ID. This hypothesis is confirmed in table 3, where RID filers listing a hardship were 24 percentage points less likely to vote in comparison to ID-capable RID filers, who were only 14 percentage points less likely to vote in 2014 as compared to non–RID filers. Individuals who stated that they relocated recently, yet were registered to vote in Texas in previous elections, look more similar to "Hardship" RID voters. However, it is difficult to make firm conclusions about the mechanisms at work for this population, as their turnout is also substantially lower than other RID filers in the 2010, nonstrict ID election.²⁶

As outlined in our preanalysis plan, we also compared the rate of voter turnout of 2016 RID filers in the previous strict ID election (2014) to those who voted with ID in 2016, using exact matching to gain balance on pretreatment (that is, pre-2014) patterns of voter turnout across the RID and non-RID groups. We assume that accounting for the pretreatment trend in voter turnout accounts for underlying vote propensity in the absence of a strict voter ID law and that a parallel trend in turnout would be observed otherwise. In case the parallel trends assumption does not hold, we also produce estimates with a lagged dependent variable model conducted via a least squares regression. Those results can be seen in appendix A2. Notably, models with lagged dependent variables in table A1 (tables A1–A10 are available online)—which control for previous turnout—are generally consistent with table 3.

^{22.} Fig. A3 is also consistent with this conclusion; the majority of RID filers showed a Texas Voter Certificate when they arrived at the polls. For all but first-time voters who registered by mail, possessing a valid certificate means that voters had presented ID at some point in the past.

^{23.} These are least squares models regressing individual turnout in the indicated election on the type of RID filed, with or without county fixed effects. The indicated coefficients may therefore be interpreted as the difference in turnout rates between RID filers of the indicated type and non-RID-filing voters, or the difference in means after removing county-level variation. For a more detailed discussion of differences in turnout between RID-filing voters and non-RID voters, see app. A2.

^{24.} Such a restriction is important, because many RID filers were too young to vote in 2014. Our voter file snapshot was acquired in February 2017, shortly after the November 2016 election, but when turnout data were available. Since it was acquired from the state of Texas, it only covers

turnout in Texas elections. Individuals who were registered to vote in Texas on October 1, 2014, but voted in another state in November 2014 would appear as nonvoters here. See Yoder (2019) for a discussion of the use of multiple voter file snapshots to track the turnout of intra- and interstate movers over time. We also use the same weighting technique outlined in the results section for RID filers matched to multiple Texas voter file records.

^{25.} These analyses only include those registered by October 1, 2010 or 2012, respectively.

^{26.} As indicated above, individuals who voted in states other than Texas would be counted as nonvoters. This may explain the lower rate of turnout for "Relocation" RID filers versus other categories, though importantly, all of the "Relocation" RID filers voted in the state of Texas in 2016.

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The fact that many RID-filers had previously demonstrated a capacity to obtain ID might be taken as evidence that voter ID laws are not a burden. However, we believe this conclusion should be weighed against three other facts. First, regardless of their reason for lacking appropriate ID, all 16,000 voters who filed RIDs would likely have been disenfranchised in the absence of federal court intervention. Second, our tabulation implies that examining state ID databases in an effort to identify the voters likely to be disenfranchised-as is commonly done in litigation surrounding voter ID laws (e.g., Ansolabehere and Hersh 2017)-might still not capture the true impact of these policies because it will fail to count as disenfranchised the voters who have obtained state ID that cannot be presented for voting purposes. Finally, approximately one-third of RID filers do have a hardship posing a meaningful impediment to obtaining ID. This condition is associated with lower 2014 turnout and would presumably manifest in future elections, as well, in the absence of a RID option.

County-level factors do not explain individual-level racial disparities

As is the case in most states, county officials in Texas enjoy considerable discretion when it comes to election administration. As such, it is possible that election officials in counties with larger minority populations—who may have believed that their voters were particularly likely to lack compliant ID—were more actively communicating the possibility that voters could file RIDs. If so, this might have increased the probability that a given minority voter filed a petition relative to a given White voter, which could affect the conclusions we report above with respect to the disproportionately non-White population who filed RIDs. We therefore conclude by considering the possibility that our results could be spuriously driven by countylevel factors.

If the opportunity to file an RID was presented more often to Black and/or Latinx voters than it was to White/Anglo voters, we would expect to find rates of RID filing to be positively and significantly correlated with a county's percentage of Black and/ or Latinx residents. Table 4 contains ordinary least squares regression coefficients and robust standard errors for models of the percentage of voters casting ballots in 2016 who filed RIDs in a given county.²⁷ We fit models of the overall county RID percentage, the percentage of voters filing RIDs for reasons relating to a relocation or hardship, and the rate of ID-capable RID filers. These models clarify whether county-level attributes affected the county's rate of RIDs that were filed out of all ballots cast and also whether those same attributes were related to rates of RIDs binned in the three categories we describe above: relocation, ID capable, and hardship. Model coefficients indicate county factors that were associated with more/ fewer RIDs filed and are therefore broadly informative about the probability that voters were offered the chance to file an RID if they lacked adequate ID.

All models in table 4 indicate that the percentage of the two-party vote that Barack Obama received in a given county during the 2012 election is positively, meaningfully, and significantly associated with the percentage of people whom SB 14 would have deterred from voting. Thus, the county-level models could offer evidence that SB 14 may be disproportionately burdensome in Democratic-leaning counties, but could also indicate that officials in those areas are more likely to offer voters RIDs. Yet, holding Democratic support constant, the filing rate does not appear to rise as a result of higher concentrations of two core Democratic constituencies in a given county: Black and Latinx voters. Indeed, the coefficients for the percentage of both African American and Latinx residents are negatively signed in all models and achieve statistical significance in all but one. That said, counties with a large proportion of young voters (another traditionally Democratic-leaning group) do see more voters reporting an impediment. The burden on young voters (those 18-24) appears to accrue especially in the "Relocation" and "Hardship" categories. Finally, the coefficient for median household income is positive and statistically significant, indicating that more hardship petitions are filed in wealthier counties. Though this effect is quite small, it could indicate that poll workers in these counties are more proactive in offering petitions.²⁸

As we suspect that county noncompliance may not be orthogonal to the racial/ethnic composition of a jurisdiction, we also conduct this test on a subset of the data where counties filed at least one RID petition, which can help to determine

^{27.} The dependent variable is a percentage ranging from 0 to 1.

^{28.} A \$10,000 shift in median household income—about 1 standard deviation—is associated with an increase in the rate of "Hardship" RID filing of about one percentage point.

	All RII) Filers	Hardsh	ip Only	Relocati	on Only	ID Capal	ole Only
	(1)	(2)	(3)	(4)	(2)	(9)	(2)	(8)
2014	194 [194 [246	246 [291 [293 [141 [124]	139 [156139
2012	[007,006]	[-007, -077]	$\begin{bmatrix} -200, -2620 \end{bmatrix}$ 101 $\begin{bmatrix} -119, -084 \end{bmatrix}$	$\begin{bmatrix} .207, .224 \end{bmatrix}$ 093 $\begin{bmatrix} -11, -076 \end{bmatrix}$	[-167,]	$\begin{bmatrix} 1022 & 0.222 \\131 \\131 \\161 \\161 \end{bmatrix}$	[-081, -055]	[-058]058
2010	[- 105 - 077]	[[11]059]		[[-195 - 103]		[
County FE?	No No	Yes	No	Yes	No	Yes	oNo	Yes

Type
Impediment
þ
Voters
Regular
versus
Filers
RID
Turnout:
.⊑
Difference
Table 3.

Note. FE = fixed effect, RID = reasonable impediment declaration. Includes individuals marked as having cast a ballot in the Texas voter file for the 2016 election and who were registered to vote in each of the indicated election years. "Hardship Only" compares RID filers who listed a hardship as their impediment to non-RID voters. "Relocation Only" compares RID filers who have demonstrated a previous ability to obtain ID to non-RID voters. 95% confidence intervals displayed in brackets.

	All RIDs	Relocation Only	ID Capable Only	Hardship Only
% Obama vote,				
2012	.50*	.08*	.24*	.14*
	(.12)	(.03)	(.08)	(.04)
% Black	49*	02	28*	11*
	(.20)	(.07)	(.12)	(.05)
% Latinx	20*	05^{*}	09*	05*
	(.06)	(.02)	(.04)	(.02)
% no college	.13	05	.17*	.02
	(.18)	(.06)	(.08)	(.07)
% age 18-24	1.09*	.44*	.31	.40*
	(.48)	(.19)	(.20)	(.12)
% age 75+	.46	.33	00	.46
	(.92)	(.24)	(.40)	(.28)
Median house-				
hold income	.02	.01	.00	.01*
	(.02)	(.00)	(.01)	(.01)
Constant	26	05	16	12
	(.33)	(.09)	(.15)	(.10)
R^2	.17	.16	.16	.13
F statistic	4.88	3.26	2.17	6.18
Root mean				
square error	.13	.04	.07	.05

Table 4. Determinants of County-Level RID Rates

Note. N = 254, RID = reasonable impediment declaration. Robust SEs in parentheses. The rate for counties filing no RIDs is set to zero. Demographic information from US Census 2015 five-year American Community Survey (ACS) estimates, except household income in Loving County, which is an inflationadjusted imputation from the 2016 five-year estimates to account for missingness in the 2015 ACS estimates. "Obama, 2012 (%)" is Obama's share of the county two-party vote. "Relocation" are voters marking "Other" and noting a recent move and/or student status. "ID Capable" are voters who claimed to have lost an ID, as well as those choosing "Other" and writing that they forgot their ID or were protesting the law. All other voters fall into the "Hardship" category. * p < .05.

whether disparate implementation affects the results in table 4.²⁹ The results of the models in table 4 are generally consistent with those using data only from counties filing at least one RID,

which can be found in table A9.³⁰ Whether we examine RID filing rates from all counties or just those reporting at least one RID, we see no evidence that voters in counties with higher minority populations were more likely to be offered an RID option.

As such, the models in table 4 yield further evidence that the results we describe above should be taken as a lower-bound estimate of the number of voters disenfranchised by SB 14. Specifically, in table 1, we showed that at the individual level, people who voted without an ID were significantly less White than those who voted with compliant ID. However, this individual-level result is apparent despite the fact that a higher proportion of minority voters is associated with lower rates of RID filing at the county level. Put another way, the county-level results in table 4 could be indicative of the kind of backlash effect Valentino and Neuner 2017 describe whereby voters lacking ID seek to obtain it before the election-perhaps aided by community organizations. Even so, our individual-level results suggest that minority voters were more likely to lack compliant ID in the 2016 election and would therefore have been more likely to be turned away from the polling place absent the federal court order mandating the usage of reasonable impediment declarations.

Returning to the potential mechanisms that induce voting without ID, these county-level results also appear to discount the possibility that differential enforcement of the RID mandate produces the racial/ethnic differences found in table 1. A plausible mechanism for producing this effect is that heavily minority counties may be the types of places where minority advocacy groups, coethnic election officials, or campaigns put extra effort into advertising the availability of the RID option. However, we see the opposite relationship at the county level. Thus, the individual-level patterns we find are not likely to be a product of mechanisms relating to selective advertising of the RID option.

CONCLUSION

Because of their potential to disenfranchise otherwise-eligible voters who lack photo ID, strict voter ID laws have proven controversial. Indeed, these policies are an important component in a larger debate about how nonfacially discriminatory laws described as targeting voter fraud might instead be used as instruments of voter suppression (Bentele and O'Brien 2013). This is particularly salient after the Supreme Court's ruling in

^{29.} Sixty-five counties reported no RIDs. There are a number of possible reasons for this. Given the potential for election officials to exercise discretion in administrative decisions (Kimball, Kropf, and Battles 2006), some counties may have refused to comply with the court order. This could result in some counties turning away voters who tried to vote without qualifying ID. However, it is equally plausible that these counties followed the process and still had no RIDs. In table A10, we demonstrate that neither county partisanship nor race/ethnicity is associated with filing at least one RID. Indeed, the most important factor appears to be county population. The overall rate of RID filing statewide is 0.2%. The mean population of counties reporting no

RIDs is 10,312, and 49 had populations under 10,000. Statistically, we may expect some of these counties to have zero RID filers.

^{30.} Percentage of Latinx voters fails to achieve statistical significance in those models, but they are negatively signed. The coefficient for median household income is also insignificant in the hardship model, but is equal in size to that in table 4.

Shelby County v. Holder 570 U.S. 529 (2013) struck down the preclearance provisions in the VRA that required municipalities with a history of discriminatory voting laws to receive advance permission from the Department of Justice before changing their election procedures. Characterized as "old poison, new bottles" (Berman 2015, 245), voter list maintenance, gerrymandering, and limits on early voting join strict ID laws as new policies achieving old aims of reducing minority voter participation, but voter ID laws continue to take on a particularly large share of the blame for recent reductions in minority turnout (Anderson 2018; Clinton 2017, 420–21).

In the case of strict voter ID laws specifically, past research points to the patterns described above. Yet, data with which we can answer the question of whether strict voter ID laws actually do have disproportionately disenfranchising effects have been difficult to acquire. Particularly when it comes to understanding subgroup effects, survey data have proven to be a suboptimal platform by which to examine the effects of state laws on individual behavior (Ansolabehere, Luks, and Schaffner 2015; Grimmer et al. 2018). A district court ruling in Texas created an invaluable source of such data. Examining the RIDs that Texas officials produced under court order, we identify more than 16,000 Texans who arrived at polling places in 2016 who would have been turned away had SB 14 been in full effect. This is a very small percentage of voters: about 0.18% of ballots cast. Normatively, this can be taken as good news in one respect-relatively few people seem to have been disenfranchised for lack of ID, which is broadly consistent with previous findings (Highton 2017).

However, some caution in interpreting this result is in order, as it should be taken as a lower bound of the net number of voters SB 14 would have disenfranchised in 2016. Proponents of voter ID policies argue that in combating perceived voter fraud, the laws increase public confidence in the election process. If this effect is real, it might increase turnout among those who previously had low confidence in the democratic process. Similarly, previous work (Valentino and Neuner 2017) has found that voter ID laws can lead to a "backlash effect"; individual voters might see the policies as intended to disenfranchise them and, in response, obtain ID and register to vote. Given that fact that voters successfully mobilized by SB 14 did not file RIDs, and taking into consideration the (likely substantial) number of voters who erroneously believed that SB 14 would bar them from voting and never turned out at all, the effect we report may be a conservative estimate of the number of voters who lack compliant ID when strict voter ID laws are passed. That said, the percentage of would-be disenfranchised voters we report is more than seven times greater than the rate of double voting in the United States (Goel et al. 2020) and many times greater still than reported instances of election fraud (e.g., Levitt

2007), both of which are cited as rationales for strict voter ID policies. Even the small effects we find suggest that the costs of strict voter ID laws in terms of disenfranchised voters exceed the benefit of fraud prevention.

That said, if a law is disproportionately burdensome on racial minorities, then the number of voters it disenfranchises must be a secondary consideration in a legal debate. We determine that the population voting without ID was disproportionately Black and Latinx and overall less White than the population of 2016 voters who provided qualifying ID. Buttressing work that indicates a disparate racial impact of voter ID laws, we find significant differences in the racial/ethnic composition of the population that shows up at the polls without ID versus those meeting strict ID mandates. Such a finding has important implications for ongoing investigations of Texas's voting practices, especially its potential violation of section 2 of the VRA. County-level factors do not explain these differential racial impacts, implying that differences in implementation do not explain the racial/ethnic effects we find. More broadly, the fact that racial/ethnic minority groups would be disproportionately turned away from the polls under the strictest forms of voter ID laws suggests the need to soften such laws if all Americans are to have equal access to the ballot.

The court's ruling also allows us not only to identify voters who arrived at the polls without proper ID in 2016, but also to gain an understanding of why they could not produce ID. Going beyond survey reports that indicate what share of nonvoters claim lack of ID as an impediment to voting, we find direct evidence that a majority of the individuals who showed up to the polls without qualifying ID in 2016 had demonstrated the ability to obtain qualifying ID at some point in the recent past or were actively trying to acquire it. This suggests that estimates of the population affected by ID laws relying on measures of the population without ID, such as those relying on matching to driver's license databases, are likely underestimating the pool of potential individuals who would be turned away under the strictest forms of ID laws.

While we provide evidence regarding the impact of ID laws in at least two domains, it is important to recognize the limitations inherent in our study. First among these is the fact that our study relies on individuals deciding to try to cast a ballot. The RIDs do not provide a total measure of how many voters SB 14 even the softened ID requirements deterred from voting, not to mention how many did not attempt to vote. Relatedly, our test for disparate impacts of voter ID laws is inherently strict. Despite evidence indicating limited publicity of the RID option, individuals who had awareness of the law, perhaps from experience voting in 2014, may have shifted their behavior and produced qualifying ID for the 2016 election when they would otherwise have not done so. Thus it is possible that our findings underestimate racial/ethnic disparities produced by voter ID laws, making the measurable overall and racial/ethnic disparities in participation all the more important to emphasize.

Our analysis also leverages a single state where circumstances allowed us to view who votes without ID. The enhanced internal validity provided by the RID provision meant a focus on Texas was obligatory. That said, we do not believe this focus diminishes the impact of our study. In the 2020 election, six other states employed strict photo ID laws that function in essentially the same way that SB 14 did during the 2014 election. In addition, besides Texas, 11 other states have "photo identification requested" policies, whereby voters lacking ID on Election Day may either submit an RID-like affidavit or vote on a provisional ballot; many of these policies function in much the same way SB 14 did under the federal court order. As such, the experience of voters in Texas is not unique. Moreover, Texas is the largest and most diverse state to put in place a voter ID law of any sort; at the time of Texas's move to a strict voter ID law, its residents made up more than half of all voting-age Americans subject to such laws. Moreover, when it was fully implemented in 2014, the list of six acceptable forms of ID for voting in Texas was narrower than any other state. Voter ID was therefore likely to affect more Texans in both absolute terms and as a percentage of state residents than in any other state. The scope of affected voters is especially important. Studies relying on jurisdiction-level turnout or survey data often report that voter ID laws have no effect on turnout or no disparate effect on minority populations (e.g., Highton 2017). However, a recent shift to individual-level designs employing administrative records has discovered detectable-but small-effects (Grimmer and Yoder 2021; Henninger et al. 2018). If we expect the affected population to be relatively small, then examining the largest population of voters subject to a strict voter ID law (Texans) may provide the best opportunity to isolate effects.

Implementation of the strictest form of Texas's law continues to be the subject of legal action, and in 2017, Texas enacted a new voter ID law that includes an RID provision. Variation induced by both the legal and lawmaking processes provides a unique opportunity to understand the impact of voter ID laws on infrequent voters and racial/ethnic minority voters more broadly. We find substantial evidence that strict voter ID laws impede voting for otherwise eligible citizens, many of whom are only temporarily unable to produce qualifying ID, and a disproportionate share of whom belong to historically disadvantaged groups. While debates may continue regarding the magnitude of negative impacts resulting from voter ID laws, our evidence clearly indicates that a negative impact exists and further diminishes the political voice of those already less likely to participate in politics.

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Exhibit 12





The durable differential deterrent effects of strict photo identification laws

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Abstract

An increasing number of states have adopted laws that require voters to show photo identification to vote. We show that the differential effect of the laws on turnout among those who lack ID persists even after the laws are repealed. We leverage administrative data from North Carolina and a photo ID law in effect for a primary, but not the subsequent general, election. Using exact matching and a difference-in-differences design, we show that for the 3 percent of voters who lack ID in North Carolina, the ID law caused a 0.7 percentage point turnout decrease in the 2016 primary election relative to those with ID. After the law was suspended, this effect persisted: those without ID were 2.6 percentage points less likely to turnout in the 2018 general.

Keywords: Causal inference; voter ID

"The proposed [voter ID] law puts up barriers to voting that will trap honest voters in confusion and discourage them [...]."

– Roy Cooper, Governor of North Carolina $(2018)^1$

1. Introduction

A growing number of states have enacted laws that require voters to show photo identification (ID) to cast a ballot.² Meanwhile, a flurry of legal challenges and scholarly analyses have sought to estimate the effect of the voter ID laws. Do strict ID laws deter voters from turning out, and if so, how? In this paper, we demonstrate that photo ID laws differentially deter voters without state identification, relative to voters with identification, while in place and continue to differentially deter voters *even after those laws are repealed*. The differential deterrent effect for individuals without state ID occurs while the law is in effect both because voters without a state ID are deterred from voting once they arrive at the polls, what we call a mechanical effect, and because voters without state ID decide to not turnout at all, a differential deterrent effect. Once the law is removed, the differential deterrent effect can persist because voters without ID may be unaware of the requirements for voting have changed.

To assess the persistent deterent effect of voter ID laws, we use administrative data from North Carolina and exploit variation in a photo ID law that was in effect for the 2016 primary election but not the 2016 general election and beyond. The North Carolina legislature passed a law in 2013 requiring photo ID to vote starting in the 2016 primary election. To inform voters of the

¹https://www.newsobserver.com/news/politics-government/article223103100.html

²http://www.ncsl.org/research/elections-and-campaigns/voter-id.aspx

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law change, the North Carolina State Board of Elections (NCSBE) sent a mailer to all voters who lacked a state-issued ID (about 3 percent of registrants in North Carolina), informing them that photo identification would be required to vote. To assess the effect of this law, we use exact matching on a variety of individual characteristics, including turnout history prior to the ID law, and a difference-in-differences design, to find that the strict photo voter ID caused a 0.7 percentage point decline in turnout in the primary election among those who did not match to a state-issued ID, compared to those who did have state ID. After the primary election in July 2016, an appellate court overturned the photo ID law, removing the requirement for photo identification to vote. But there was no subsequent letter sent to inform voters that photo identification was no longer required.

The change in the law without notifying voters created confusion about what was necessary to vote and as a result the effect of the ID law persisted after the law was repealed. Even though the ID law was suspended, individuals without a state-issued ID were 2.6 percentage points less likely to turnout in the 2016 general election than voters with ID. This effect persisted beyond the 2016 election, though the magnitude of the effect decreased: in the 2018 general election, individuals without valid identification were 1.7 percentage points less likely to turnout and there was no deterrent effect in the primary election. This pattern is consistent with the change in laws sowing confusion: the effect is concentrated among voters who participated occasionally in prior elections and have a low probability of participating in primaries.

The persistent effect of voter ID laws demonstrates the need to revisit common research designs used to assess the effect of voter ID laws. To assess the effects of voter ID laws, scholars compare turnout in states that have voter ID requirements with those that do not, using either aggregate state-level turnout or self-reported turnout data (Erikson and Minnite, 2009). Some designs have found no effect on turnout (Mycoff *et al.*, 2009; Hood and Bullock, 2012; Highton, 2017; Cantoni and Pons, 2019), while others have found negative effects on turnout (Alvarez *et al.*, 2008; Barreto *et al.*, 2009; Alvarez *et al.*, 2011; Hajnal *et al.*, 2017). Yet, if the effect of voter ID laws persists, then states that are classified as "control" states after a law is repealed will still have voters affected by the law being in place; and as a result, the most common research designs will be biased toward finding no effect.

The magnitude of the effects that we estimate also demonstrates that prior studies lack the appropriate data and statistical power to detect this effect. The voter ID law continued to deter voters, but our best estimate of the overall effect of voter ID laws on aggregate turnout is small enough to show that studies assessing the effect of voter ID laws on aggregate turnout lack the power to detect the effect of the laws. Using a simple set of assumptions to calculate the number of voters affected by the differential deterrent effect, we find the law caused approximately 5,110 individuals without identification to not vote in the 2016 general election. This accounts for approximately 0.1 percent of the total votes cast in the election. Designs that use aggregate level turnout rates or administrative data without information about who holds identification will fail to detect this change in turnout rates. And while other studies sometimes use survey research to learn who holds identification, this group is so small that it is difficult to estimate the effect with policy-relevant precision (Erikson and Minnite, 2009; Grimmer *et al.*, 2018).³

And our results suggest that debates and policy disputes about voter ID laws could dampen turnout, even when photo identification laws are removed. This has important implications for assessing turnout in states where voter ID laws are still actively debated. For example, in the wake of North Carolina's strict photo ID law being struck down in 2016, voters in North

³Many studies have used surveys to estimate the rate at which individuals have ID required to vote under different voter ID laws, often finding that rates of having inadequate photo ID under a strict photo ID requirement range from about 1 to 15 percent (Hood and Bullock, 2008; Ansolabehere, 2012; Barreto *et al.*, 2012; Beatty, 2012; Hobby *et al.*, 2015). These studies often find racial minorities are less likely to have ID compared to other registered voters.

Carolina approved a ballot measure in November 2018 to amend the state constitution to require photo identification to vote.⁴ While the new requirements are already the subject of pending lawsuits,⁵ North Carolina Governor Roy Cooper signed a bill into law in March 2019 to delay the voter ID requirements until 2020.⁶ Given the turbulent nature of debates and implementation of voter ID laws along with our findings, officials and researchers should carefully consider how these processes affect who votes, regardless of if or when these requirements are actually implemented.

2. Voter ID laws in North Carolina and their effect on voter turnout

In 2013, North Carolina passed legislation to implement a strict photo voter ID law, beginning with the 2016 election.⁷ In accordance with the law, in 2015 the North Carolina State Board of Elections (NCSBE) generated a list of registered voters who did not match to a state-issued photo ID issued by the North Carolina Division of Motor Vehicles (DMV) at the time of the 2014 general election.⁸ Using this list, the NCSBE sent a mailer to each of these registrants, which stated that photo ID would be needed to vote in 2016, listed resources for obtaining free photo ID, and provided a postage pre-paid response card where recipients could indicate they needed assistance in acquiring a photo ID.9 Registered voters were required to show an acceptable form of photo ID to cast a ballot, which included a North Carolina driver's license, US passport, or US military ID card, among others. If voters were unable to present acceptable photo ID, they could cast a provisional ballot, which would only be counted if the voter took additional action after Election Day by presenting ID in person at their County Board of Elections.¹⁰ In July 2016, after the 2016 primary but before the general election, the 4th US Circuit Court of Appeals overturned the voter ID law, citing its discriminatory intent.¹¹ After the ruling, the ID law was not in place for the 2016 general election. While the court ruling blocked the enforcement of voter ID laws in the general election, it did not require a new letter to voters without identification to inform them that identification was no longer required to vote.

We use variation in the voter ID law's implementation for the 2016 primary and general elections to estimate the persistent effect of voter ID laws among those who lack state ID. The effect of the North Carolina law on those without state identification combines two distinct ways the laws could deter voters: a mechanical effect that occurs at the polls and an overall deterrent effect that causes individuals to not turnout at all.

Once the law was struck down by the appellate court, the mechanical effect immediately is removed, but the differential deterrent effect remains until voters without state-issued identification learn about the change in rules about required identification. One impediment to learning about the change is that there was no official notice from the state: the appellate court struck down the law, but did not order a second letter sent to voters about the different requirements. Political campaigns or the media could provide information to voters, but this will necessarily be

⁴https://dashboard.ncleg.net/api/Services/BillSummary/2017/H1092-SMBK-165(sl)-v-4

⁵https://www.wunc.org/post/nc-naacp-challenges-new-voter-id-law-federal-court

⁶https://www.wfae.org/post/gov-cooper-signs-law-delay-voter-id#stream/0

⁷https://www.ncleg.net/Sessions/2013/Bills/House/PDF/H589v9.pdf

⁸https://moritzlaw.osu.edu/electionlaw/litigation/documents/NAACPPlaintiffsPreTrialBrief011916.pdf

⁹https://www.ncmd.uscourts.gov/sites/ncmd/files/opinions/13cv658moo_0.pdf.

¹⁰Along with their provisional ballot, voters could also complete a "reasonable impediment declaration" to indicate a reasonable impediment to obtaining photo ID (https://law.justia.com/codes/north-carolina/2015/chapter-163/article-14a/section-163-166.15). Voters also provide the last four digits of their social security number and date of birth, or a copy of a non-photo ID document. In the 2016 primary, 864 voters had their provisional ballots cast for lack of ID ultimately accepted under the reasonable impediment declaration (https://www.newsobserver.com/news/politics-government/article222949830.html). For a full explanation of acceptable forms of ID and procedure for casting provisional ballots for lack of ID in North Carolina for the 2016 primary, see Hopkins *et al.* (2017).

¹¹http://pdfserver.amlaw.com/nlj/7-29-16%204th%20Circuit%20NAACP%20v%20NC.pdf

less likely to reach potential voters than the information from the NCSBE. For example, as we show in Appendix A.1, newspaper coverage of the voter ID law in both North Carolina and national news sources tended to occur when the appellate court decision was made, rather than immediately prior to election day when voters are most interested in the campaign.

We interpret our treatment effect as the differential effect of the law on those without state-issued identification, relative to those with a state ID. We focus on this interpretation because imposing a strict photo ID law could affect the turnout decision of all registered voters and our design is unable to address potential changes in voting behavior for individuals who hold valid identification. Individuals could be angered by the law increasing turnout among those who already hold valid identification (Valentino and Neuner, 2017), campaigns might exert compensatory effort to turn out individuals with valid ID, or increased controversy around the law might cause more voters to be more interested in the election. Our interpretation—as the differential effect of the law for those without identification—is necessarily different than the policy question of what turnout would have been if the voter ID law had never been put in place. As we explain below, we can impose plausible and transparent assumptions about the broader effect of the law to generate estimates for this different policy question. And our results provide important insights into who is deterred when the law is put into place.

Measuring Who Holds Identification

To measure who lacks a state-issued ID, we combine administrative data from North Carolina voter files—which includes information on a voter's address, age, race, and turnout history in every primary and general election from 2008 to 2018—with individual-level administrative data on who possesses a state-issued ID.¹² We use the unique identifier the NCSBE generated to identify voters without identification, which we use to merge to the voter file to measure an individual's lack of ID.

Our measure of who might lack photo ID, then, is the list of voters who the NCSBE identified as possibly lacking ID through this matching process. We discuss possible sources of measurement error in Appendix Section A.2 and show that the measure error from this matching is likely to be small.

Table 1 shows descriptive statistics for the full voter file compared to registrants who do not match to an ID. About 3.0 percent of registrants fail to match, indicating that they might lack adequate ID to vote under the strict ID law. The composition of registrants without ID differs from the full voter file: those lacking ID have lower turnout on average, and they are more likely to be non-white as well as registered Democrats. The standard deviation of birth year is also larger among those without ID, which comports with previous survey work showing those who lack ID often tend to be young or elderly (e.g., Stewart, 2013).

We also rely on individual-level data from the NCSBE on which voters cast a provisional ballot. Not only can we observe whether a provisional ballot was cast and ultimately counted, but we can also observe the stated reason for needing to cast a provisional ballot. We use this to estimate the mechanical effect of the voter ID law—that is, how many voters are turned away at the polls for lack of ID on Election Day.

3. Strict photo ID laws decrease turnout among those without ID while in effect

In this section, we show that North Carolina's voter ID law decreased turnout among those without state ID. In our most stringent specification, the ID law decreased turnout by about 0.7 percentage points among those without ID. This decrease comes both through the differential deterrent effect and through a mechanical effect where voters without ID are turned away at the polls on Election Day. This is consistent with voters who lack identification knowing,

¹²All of this information is publicly available and provided by the North Carolina State Board of Elections (NCSBE).

	Mean (1)	Deviation (2)	Minimum (3)	Maximum (4)
		(A) Full voter file	(# Voters = 6,480,423)	
Voted	0.382	0.486	0	1
Hispanic	0.020	0.139	0	1
Black	0.229	0.420	0	1
White	0.715	0.451	0	1
Other Non-White	0.036	0.186	0	1
Democrat	0.420	0.494	0	1
Republican	0.308	0.461	0	1
Unaffiliated	0.273	0.445	0	1
Birth year	1966	18	1910	1999
,		(B) No DMV matcl	h (# Voters = 196,544)	
Voted	0.229	0.420	0	1
Hispanic	0.047	0.212	0	1
Black	0.390	0.488	0	1
White	0.504	0.500	0	1
Other Non-White	0.059	0.235	0	1
Democrat	0.575	0.494	0	1
Republican	0.192	0.394	0	1
Unaffiliated	0.233	0.423	0	1
Birth year	1965	23	1910	1999

 Table 1. Descriptive statistics, individual level, 2008–2018

The unit for voted an individual-year. Panel A presents descriptive statistics for the full file of North Carolina voters registered as of the 2014 general election. In this panel, every observation is a voter within an election period. Panel B presents descriptive statistics for the individuals in the voter file who do not match to a DMV identification record.

based on the letter they received, that they would be turned away at the polls because they lack identification and be unable to vote.

3.1 Evidence for a mechanical effect

First, we estimate the mechanical effect of the voter ID law, where a voter shows up to the polls intending to cast a ballot but does not have their vote counted because they do not meet the requirements of the new ID law. This mechanical effect can only plausibly decrease the turnout rate (Fraga and Miller, 2018; Henninger *et al.*, 2018), where voters show up to the polls but are challenged to present appropriate ID and fail to do so.¹³ In North Carolina, this mechanical effect is present in the 2016 primary, the only election for which the voter ID law is actually in place.¹⁴ To estimate the mechanical effect, we subset to provisional ballots that were ultimately rejected, and in Figure 1, we plot the share of provisional ballots cast by reason for each election.¹⁵ The figure on the left shows provisional ballots for primary elections, while the figure on the right shows them for general elections. In each year, the vast majority of provisional ballots that were rejected for lack of ID account for a very small share of rejected ballots, even in the 2016 primary, where they comprised less than 10 percent of rejected ballots. Overall, a total of 1169 provisional ballots were rejected for lack of ID in the 2016 primary election, approximately 0.05 percent of the 2,332,428 total votes cast statewide in that election.

¹³It is also possible that voters produce valid ID at the polls but are inappropriately rejected. For more discussion of how voter ID requirements are applied on Election Day, see Atkeson *et al.* (2014).

¹⁴See Appendix Section A.3 for a brief discussion of the small number of provisional ballots cast for lack of ID in election before 2016.

¹⁵For ease of presentation, we show provisional ballots for the two most relevant types of reasons in our study: not being registered and not providing ID. We show a more complete set of provisional ballot reasons in Section A.4 of the Appendix.



Figure 1. Reasons for provisional ballots. The left panel plots the share of provisional ballots cast for different reasons in primary elections. The right plots the same series for general elections. The blue line indicates the share of provisional ballots cast because the voter lacked proper ID. The only election for which the ID law was in effect was the 2016 primary, where about 10 percent of provisional ballots were cast for lack of ID.

How many of these provisional ballots rejected for lack of ID come from individuals who do not match to a state ID? In Figure 2, we plot the share of individuals who fail to match to a state ID, first for all registrants on the left and then for provisional balloters on the right. Light grey bars indicate individuals who lack ID, and dark grey bars indicate individuals who have ID. The light grey bar on the right shows that of the 1,169 provisional ballots that were rejected of lack of ID, about 18.2 percent of them were identified as lacking ID. This rate of 18.2 percent is much higher than the rate among all registrants of 3.0 percent, meaning the mechanical effect censors out individuals who lack ID at a much higher rate than those who have ID. However, the other 81.8 percent of provisional balloters actually linked to a state ID. This suggests that many rejected votes come from individuals who hold a valid ID, likely because they forgot their photo ID on Election Day.

3.2 Evidence for a deterrent effect

Next, we estimate the deterrent effect of the voter ID law, which yields conflicting theoretical predictions. On one hand, voters without state ID might abstain from voting because they infer correctly or incorrectly—that they do not have adequate ID to vote and are not willing to pay the costs of acquiring acceptable ID.¹⁶ On the other hand, the voter ID law might anger or otherwise motivate voters, making them more likely to overcome the law by obtaining valid identification and participation (e.g., Claibourn and Martin, 2012; Valentino and Neuner, 2017).

To estimate the differential deterrent effect of the ID laws, we estimate equations of the form

$$Turnout_{it} = \beta * \text{No DMV Match}_{i} * 2016 + \gamma_{i} + \delta_{t} + \epsilon_{it}, \qquad (1)$$

where $Turnout_{it}$ is an indicator for whether individual *i* turns out to vote in the election at time *t*. The variable NoDMV Match_i * 2016 is an indicator for whether individual *i* does not match to photo ID and the election year is 2016, the year the voter ID law goes into effect. We include individual and year fixed effects, modeled by γ_i and δ_t , respectively.¹⁷ This is a difference-in-differences design, which relies on the common trends assumption. For this to be satisfied, it must be that the change in turnout behavior for treated units (those without

¹⁶These costs could include the time it takes to gather information about the law's requirements along with the effort to apply for an acceptable ID.

¹⁷The equation does not include No DMV Match_i because it is absorbed by the individual fixed effects.



Figure 2. DMV match status: comparing registrants to provisional balloters. The light grey bars indicate individuals who lack ID, while the dark grey bars indicate individuals who match to a DMV-issued ID. We plot these proportions separately for all registered voters on the left and for those who cast a provisional ballot in the 2016 primary election but had it rejected because they failed to present adequate ID.

state ID) would have been the same as control units (those with state ID), had they not been treated. There are likely many differences between those who have a state-issued ID and those who do not, and these differences might drive changes in turnout behavior. Specifically, the main concern for our design is that there are compositional differences between those who have a state ID and those who do not, as we show in Table 1. Voters without state ID are more likely to be Democrats, minorities, not middle-aged, and have low-turnout in prior elections. If trends in turnout behavior are different on the basis of any of these compositional differences, this would bias the estimate in Equation (1). After showing results from the simplest version of the difference-in-differences and make the common trends assumption more plausible.

Table 2 shows the effect of the voter ID law on primary election turnout. In column 1, we estimate Equation (1), simply including individual and year fixed effects. This specification, therefore, estimates the counterfactual turnout trends for those without ID using all voters with ID. The estimate suggests that the voter ID law caused a 7.8 percentage point decline in primary election turnout among those without ID relative to those with ID. We have a reason to be skeptical of this baseline specification. Because there are many observable differences between those who have photo ID and those who do not, the two groups are likely to have different turnout trends. As a first way to address this, in columns 2, 3, and 4, we include race-by-year, age-by-year, and race-by-age-by-year fixed effects, respectively. This means that in column 4, for example, we compute counterfactual trends for individuals without ID using only voters with ID that are the same age and race. The point estimate in column 4 shrinks substantially to a 4.8 percentage point decline in primary turnout among those with-out ID.

Trends in turnout behavior, however, still might be different among those with and without ID, even within age and racial group. In fact, we can show some evidence for this: in Figure A.3 in the Appendix, we plot the mean turnout rate in each primary and general election from 2008 to 2016 separately for those who have and do not have ID, including race by birth year fixed

			Vote	d in primary	(0-1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No DMV match * Year = 2016	- 0.078 (0.001)	- 0.073 (0.001)	- 0.054 (0.001)	- 0.048 (0.001)	- 0.009 (0.001)	- 0.009 (0.001)	-0.007 (0.001)
Ν	33,136,560	33,136,560	33,089,505	33,089,485	33,136,560	33,136,560	33,136,560
# Voters	6,627,312	6,627,312	6,617,901	6,617,897	6,627,312	6,627,312	6,627,312
Individual FEs	Y	Y	Y	Y	Ν	Ν	Ν
Year FEs	Y	Ν	Ν	Ν	Y	Y	Y
Race by year FEs	Ν	Y	Ν	Ν	Ν	Ν	Ν
Age by year FEs	Ν	Ν	Y	Ν	Ν	Ν	Ν
Race by age by year FEs	Ν	Ν	Ν	Y	Ν	Ν	Ν
Exact match on turnout	Ν	Ν	Ν	Ν	Y	Y	Y
Exact match on race	Ν	Ν	Ν	Ν	Ν	Y	Y
Exact match on age bin	Ν	Ν	Ν	Ν	Ν	Ν	Y

Table 2.	Effect	of voter	ID la	aw on	primary	election	turnout	among	those	without	ID,	individual	level,	2008-2	2016
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Robust standard errors clustered by individual in parentheses. Main effects for No DMV Match and 2016 are absorbed by fixed effects. Exact matching on turnout matches units based on all primary and general elections from 2008 to 2014. For exact matching on age, we construct a separate age bin for each group of voters who were under 18 for a given set of elections, so the cohort of voters who became newly eligible to participate in 2010, 2012, 2014, and 2016 each have their own age bin. For voters who were eligible for all elections since 2008, we construct age deciles.

effects. Even within race and birth year, those who do not have photo ID have slightly different turnout trends in the pre-treatment period (2008–2014) than those who have ID.¹⁸

To make the common trends assumption more plausible, we exactly match individuals on the basis of their pre-treatment turnout history. This constructs a control group more likely to provide accurate counterfactual trends because we constrain the trends to be the same in the pre-treatment periods. We have pre-treatment turnout history for every voter from every primary and general election in 2008, 2010, 2012, and 2014. This means that we can put each voter into one of $2^8 = 256$ unique pre-treatment turnout histories. For every individual without a state-issued ID, we can find control units with the same pre-treatment turnout history. In column 5 of Table 2, we implement this exact matching design, where we average the differences in 2016 primary turnout among those without ID and those with ID for each of these turnout histories, weighting the estimate by the number of treated observations in each bin. After exact matching voters on their pre-treatment turnout, we find that the voter ID law caused about a 0.9 percentage point decline in turnout among those without ID. For the specifications where we carry out the exact matching, we do not include individual fixed effects. Individual fixed effects are no longer necessary because, within each turnout history bin, there is no difference in turnout between treated and control units in the pre-period. In columns 6 and 7, we further refine the exact match. In column 6, we exact match on turnout history and race. In column 7, our most stringent specification, we exact match on turnout history, race, and age decile; we find that the voter ID law caused a precisely estimated 0.7 percentage point decrease in turnout among those without ID.¹⁹

3.3 Voter file purges and sample selection

One concern with using voter files for these types of analyses is that their composition changes over time. Voter file purges could bias our estimates of the effect of the voter ID

¹⁸We also evaluate the parallel trends assumption further in Section A.6 of the Appendix.

¹⁹For exact matching on age, we construct a separate age bin for each group of voters who were under 18 for a given set of elections, so the cohort of voters who became newly eligible to participate in 2010, 2012, 2014, and 2016 each have their own age bin. This ensures that we are not mistakenly comparing individuals who did not vote in past elections but were eligible to individuals who would have voted in past elections but did not because they were too young to be eligible.

law. For example, if voter ID laws decrease participation among those without ID, and participation influences a voter's likelihood of being purged from the voter rolls, we would be inducing bias if we were to condition on those who remain in the voter file through 2018 (e.g., Nyhan *et al.*, 2017). For this reason, we limit our sample to those who were registered pre-treatment—those who were registered as of the 2014 general election. This is also the voter file snapshot that the North Carolina State Board of Elections used to match voters to their DMV records, so by limiting our sample to this set of voters, we know that the voter was included in the state's ID matching procedure. We do not include those who newly registered after the 2014 general election in our analysis because they were not included in the state's ID matching, so we do not observe their ID holding status.

Our sample selection presents one source of measurement error in individuals' voting histories. Imagine a voter who newly registers in 2012, for example. We code this as not having voted in all elections prior to their registration. It could be, however, that the voter moved in from out of state and had indeed been voting in another state. In that case, we would incorrectly be coding this voter as not having voted in elections prior to 2012, when in fact they had been. To circumvent this potential source of measurement error, in Section A.7 of the Appendix, we estimate the deterrent effect of the ID law, but we limit the sample only to those who were registered to vote in North Carolina prior to 2008. These are voters for which we can be sure that there is no measurement error in their turnout history because they are registered throughout our entire panel. The deterrent effects of the ID law among these long-time registrants are smaller in magnitude compared to our estimates using the full sample, but they are still negative and substantively meaningful.

3.4 The deterrent effect of the voter ID law persisted even after the law was overturned

North Carolina's voter ID law was overturned in July 2016, about three months before the 2016 general election. As a result of the appellate court striking down the law, the mechanical effect necessarily goes to zero. But, as discussed above, voters might incorrectly believe that an ID is still required to vote in the general election, resulting in the law still exerting a differential deterrent effect on voting on individuals without identification.

To investigate this differential deterrent effect, in Table 3, we estimate the effect on turnout of not having ID for the 2016 general election. Each column mirrors the specification described in Table 2, except that the outcome is now voting in the general election. For brevity, we focus on our most stringent specification (column 7), where we use exact matching on turnout history, race, and age.

In this most stringent specification, we find that voters without a state ID were 2.6 percentage points less likely to vote in the 2016 general compared to individuals with a state ID. The deterrent effect of the law persisted after the law was struck down and the effect of the law increased in size. This increase, however, reflects the higher participation rate in general rather than primary elections. In 2016, for example, 36 percent of registered voters participated in the primary election, while 69 percent participated in the general election. Therefore, this increase in size reflects the higher baseline rate of vote propensity in North Carolina elections.

To assess how the differential effect persists, we estimate the effect of not holding identification on turnout in the 2018 primary (Table 4) and the 2018 general election (Table 5). Again, focusing on our most stringent specification (column 7 in both tables) we find that there was essentially no differential effect in the 2018 primary election—with those without identification only 0.1 percentage point less likely to turnout. But in the 2018 general election, those without identification were 1.7 percentage points less likely to vote. This 35 percent reduction in magnitude compared to the effect in the 2016 general election is consistent with voters' confusion about what is

			Vote	d in general	(0-1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No DMV match * Year = 2016	- 0.119 (0.001)	- 0.122 (0.001)	- 0.106 (0.001)	-0.101 (0.001)	- 0.032 (0.001)	- 0.031 (0.001)	- 0.026 (0.001)
Ν	33,136,560	33,136,560	33,089,505	33,089,485	33,136,560	33,136,560	33,089,505
# Voters	6,627,312	6,627,312	6,617,901	6,617,897	6,627,312	6,627,312	6,617,901
Individual FEs	Y	Y	Y	Y	Ν	Ν	Ν
Year FEs	Y	Ν	Ν	Ν	Y	Y	Y
Race by year FEs	Ν	Y	Ν	Ν	Ν	Ν	Ν
Age by year FEs	Ν	Ν	Y	Ν	Ν	Ν	Ν
Race by age by year FEs	Ν	Ν	Ν	Y	Ν	Ν	Ν
Exact match on turnout	Ν	Ν	Ν	Ν	Y	Y	Y
Exact match on race	Ν	Ν	Ν	Ν	Ν	Y	Y
Exact match on age bin	Ν	Ν	Ν	Ν	Ν	Ν	Y

Table 3.	Effect o	of voter	ID	law	on	general	election	turnout	among	those	without	ID,	individual	level,	2008	-2016
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Robust standard errors clustered by individual in parentheses. Main effects for No DMV Match and 2016 are absorbed by fixed effects. Exact matching on turnout matches units based on each primary and general election from the 2008 primary through the 2014 general. For exact matching on age, we construct a separate age bin for each group of voters who were under 18 for a given set of elections, so the cohort of voters who became newly eligible to participate in 2010, 2012, 2014, and 2016 each have their own age bin. For voters who were eligible for all elections since 2008, we construct age deciles.

necessary to vote dissipating, but the smaller effect in 2018 could also simply reflect lower rates of turnout in midterm elections.²⁰

3.5 Interpretation of the voter ID law's effect

Interpreting these results requires caution. We estimate the differential deterrent effect, which is not estimating how overall turnout change in North Carolina as a result of the voter ID law. Instead, it is the differential effect on turnout for those without a state-issued ID relative to those with a state-issued ID. It is possible that the voter ID law could have depressed turnout among all voters, regardless of ID holding status, if voters that did have ID were also confused about the new requirements to vote. Alternatively, it could be that the voter ID law increased overall turnout among all voters if they were angered or otherwise motivated to vote because of the new requirements (e.g., Valentino and Neuner, 2017). The quantity we estimate captures the effect of the ID law on turnout among those without ID relative to those with ID. We believe that, given the compositional differences between the types of voters with and without ID, this theoretical quantity is interesting and important. Later, we estimate the effect of the voter ID law on overall turnout in North Carolina. To do so, we use our estimated mechanical effects for individuals with and without valid ID as well as our differential deterrent effect; but calculating the overall effect of the law on turnout requires an additional assumption that the law has no deterrent effect among those who have ID, an effect not identified by our design.

4. Who is deterred by the ID law?

In this section, we evaluate which types of voters are most likely to be deterred by the voter ID law. We find that not holding identification has the biggest effect for occasional voters, with only small effects for voters who rarely turned out in prior elections or who participated in all prior elections. We also find that the effect of the voter ID law does not vary substantially by race or by

²⁰Another explanation for the persistent deterrent effect of the ID law could be habit disruption. Even if voters are not confused about the ID law's requirements, it could be that because they were prevented from voting in the 2016 primary, their "habit" of voting was disrupted, causing them to be less likely to turn out in future elections (e.g., Brody and Sniderman, 1977; Gerber *et al.*, 2003).

	Voted in primary (0–1)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)		
No DMV match * Year \geq 2016	-0.078	-0.073	- 0.054 (0.001)	-0.048					
No DMV match * Year \geq 2018	0.102	0.087	0.088	0.077	-0.003	-0.003	-0.001		
Ν	39,763,872	39,763,872	39,707,406	39,707,382	33,136,560	33,136,560	33,089,505		
# Voters	6,627,312	6,627,312	6,617,901	6,617,897	6,627,312	6,627,312	6,617,901		
Individual FEs	Y	Y	Y	Y	N	N	N		
Year FEs	Y	Ν	Ν	Ν	Y	Y	Y		
Race by year FEs	Ν	Y	Ν	Ν	Ν	Ν	Ν		
Age by year FEs	Ν	Ν	Y	Ν	Ν	Ν	Ν		
Race by age by year FEs	Ν	Ν	Ν	Y	Ν	Ν	Ν		
Exact match on turnout	Ν	Ν	Ν	Ν	Y	Y	Y		
Exact match on race	Ν	Ν	Ν	Ν	Ν	Y	Y		
Exact match on age bin	Ν	Ν	Ν	Ν	Ν	Ν	Y		

Table 4. Effect of voter ID law on primary election turnout among those without ID, individual level, 2008-2018

Robust standard errors clustered by individual in parentheses. Main effects for No DMV Match and 2016 are absorbed by fixed effects. Exact matching on turnout matches units based on each primary and general election from the 2008 primary through the 2014 general. For exact matching on age, we construct a separate age bin for each group of voters who were under 18 for a given set of elections, so the cohort of voters who became newly eligible to participate in 2010, 2012, 2014, and 2016 each have their own age bin. For voters who were eligible for all elections since 2008, we construct age deciles.

Table 5. Effect of voter ID law on general election turnout among those without ID, individual level, 2008–2018

	Voted in general (0–1)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)			
No DMV match * Year \geq 2016	- 0.119	- 0.122	- 0.106	- 0.101						
	(0.001)	(0.001)	(0.001)	(0.001)						
No DMV match * Year \geq 2018	0.048	0.049	0.060	0.060	- 0.024	- 0.023	-0.017			
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)			
Ν	39,763,872	39,763,872	39,707,406	39,707,382	33,136,560	33,136,560	33,089,505			
# Voters	6,627,312	6,627,312	6,617,901	6,617,897	6,627,312	6,627,312	6,617,901			
Individual FEs	Y	Y	Y	Y	Ν	Ν	Ν			
Year FEs	Y	Ν	Ν	Ν	Y	Y	Y			
Race by year FEs	Ν	Y	Ν	Ν	Ν	Ν	Ν			
Age by year FEs	Ν	Ν	Y	Ν	Ν	Ν	Ν			
Race by age by year FEs	Ν	Ν	Ν	Y	Ν	Ν	Ν			
Exact match on turnout	Ν	Ν	Ν	Ν	Y	Y	Y			
Exact match on race	Ν	Ν	Ν	Ν	Ν	Y	Y			
Exact match on age bin	Ν	Ν	Ν	Ν	Ν	Ν	Y			

Robust standard errors clustered by individual in parentheses. Main effects for No DMV Match and 2016 are absorbed by fixed effects. Exact matching on turnout matches units based on each primary and general election from the 2008 primary through the 2014 general. For exact matching on age, we construct a separate age bin for each group of voters who were under 18 for a given set of elections, so the cohort of voters who became newly eligible to participate in 2010, 2012, 2014, and 2016 each have their own age bin. For voters who were eligible for all elections since 2008, we construct age deciles.

party, but this does not imply that there is not a disproportionate burden for minorities or democrats. Although the effect of not holding identification does not differ from members of different racial groups or partisans, voter ID laws do disproportionately burden Democrats and racial minorities because they are less likely to hold photo identification (see Table 1).

4.1 Voter ID laws mostly deter occasional voters

To examine how the effect varies based on turnout history, we construct strata of treated and control units based on the total number of times a voter casted a ballot in primary and general
elections before the law was implemented. The strata that we use for this analysis are similar to the strata we use in our exact matching design, except for ease of presentation we merely count the number of primary and general elections an individual had participated in previously, rather than differentiating based on which primary or general elections the individual had participated in. In Table A.5, we estimate the difference in primary turnout among those without ID and those with ID in the 2016 primary election in each of the turnout strata, and in Table A.6, we estimate the difference in each strata, but for general election turnout.

For brevity, we do not review the estimation procedure here, but instead do so in detail in Section A.8 of the Appendix. But, to summarize, we show the voter ID law did not seem to deter low propensity or high propensity voters without state-issued ID relative to those with ID. Instead, the voter law deterred voters without ID who participated only occasionally before the law went into effect.

4.2 The effect of the law does not vary by race or party, but there is still a deterrent effect for minorities anddemocrats

Perhaps one of the most important legal questions about voter ID laws is assessing whether the laws impose a disproportionate burden on racial minorities. And as a public policy question, it is important to know whether the laws change the partisan composition of the election. To make this evaluation, we need to combine information about the rate groups hold valid identification for voting and how the effect of the law varies by group.

We first test for whether the effect of not holding valid identification varies depending on an individual's race and party affiliation. For brevity, we focus only on the effect in the 2016 general election, but we show the results for the 2016 primary election in Section A.9 of the Appendix. Table 6 shows the effect of the voter ID law on general election turnout by race. We interact the treatment variable, not holding valid identification, with indicators for whether the voter is listed as Black, Hispanic, and Other Non-White in the voter file. In our most stringent exact matching specification in column 7, we do not find evidence that the effect of the voter ID law on those without ID varies substantially by race, with individuals from all racial groups deterred by the law.

We also investigate whether the effect of the voter ID law varies by party registration. Table A.9 shows that in our preferred specification, the effect of the voter ID law does not vary substantially by party registration.

Critically, the lack of heterogeneous effects *does not* imply that the voter ID laws did not affect the composition of the electorate or that they did not disproportionately deter minorities. In fact, combining the results of this analysis and the information in Table 1 suggests that minority voters and Democrats are disproportionately deterred from voting. Because minority voters are less likely to hold the required identification to vote, the homogeneous effects imply that minority voters were less likely to participate in the 2016 primary and general elections as the result of the law. Similarly, because registered Democrats were less likely to hold valid identification, they were disproportionately deterred. We demonstrate this directly in the next section, showing how many voters were deterred and how this altered the composition of the electorate.

5. Characterizing the overall effect of the ID law

Here, we incorporate our findings so far along with the composition of voters with and without ID to try to answer three questions about the effect of the voter ID law in North Carolina. First, what effect did the law have on overall turnout? Second, what was the total reduction in overall turnout by race and by party affiliation? Third, how did the voter ID law change the racial and partisan composition of the electorate?

	Voted in general (0–1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No DMV * 2016	- 0.121	- 0.130	- 0.095	- 0.103	- 0.032	- 0.034	- 0.028
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
No DMV * 2016 * Black	- 0.003	0.029	-0.021	0.020	-0.001	0.009	0.007
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)
No DMV * 2016 * Hispanic	0.057	- 0.033	- 0.028	- 0.063	0.004	- 0.005	-0.010
	(0.004)	(0.004)	(0.004)	(0.004)	(0.001)	(0.006)	(0.006)
No DMV * 2016 * Other NW	0.013	- 0.024	- 0.049	- 0.044	- 0.001	- 0.001	- 0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.001)	(0.005)	(0.005)
Ν	33,136,560	33,136,560	33,089,505	33,089,485	33,136,560	33,136,560	33,089,505
# Voters	6,627,312	6,627,312	6,617,901	6,617,897	6,627,312	6,627,312	6,617,901
Individual FEs	Y	Y	Y	Y	Ν	Ν	Ν
Year FEs	Y	Ν	Ν	Ν	Y	Y	Y
Race by year FEs	Ν	Y	Ν	Ν	Ν	Ν	Ν
Age by year FEs	Ν	Ν	Y	Ν	Ν	Ν	Ν
Race by age by year FEs	Ν	Ν	Ν	Y	Ν	Ν	Ν
Exact match on turnout	Ν	Ν	Ν	Ν	Y	Y	Y
Exact match on race	Ν	Ν	Ν	Ν	Ν	Y	Y
Exact match on age bin	Ν	Ν	Ν	Ν	Ν	Ν	Y

Table 6. Effect of voter ID law on general election turnout among those without ID, individual level, 2008-2016

Robust standard errors clustered by individual in parentheses. Main effects for No DMV Match and 2016 are absorbed by fixed effects. Exact matching on turnout matches units based on all primary and general elections from the 2008 primary through the 2014 general. For exact matching on age, we construct a separate age bin for each group of voters who were under 18 for a given set of elections, so the cohort of voters who became newly eligible to participate in 2010, 2012, 2014, and 2016 each have their own age bin. For voters who were eligible for all elections since 2008, we construct age deciles.

To answer the first question, we define the overall effect as the following:

$$Overall Effect = Mechanical Effect + Deterrent Effect$$
(2)

where the mechanical effect is given by the number of provisional ballots that were ultimately rejected for lack of adequate photo ID, and the deterrent effect is given by the following equation:

Deterrent Effect = (Deterrent Effect | No ID) * (#No ID) + (Deterrent Effect | ID) * (#ID) (3)

The deterrent effect among those who have photo ID is not identified using our research design. Our estimates in this section, therefore, assume this quantity to be equal to zero.²¹ We compute the probability of not holding ID simply as the share of registrants that do not match to a DMV record (3.0 percent), and we compute the deterrent effect using the point estimates from our most stringent specifications (column 7) in Tables 2 and 3 for the primary and general elections, respectively. The overall effect of the voter ID law on primary election turnout in number of votes, therefore, is

Overall Effect in Primary = Mechanical Effect in Primary + Deterrent Effect in Primary

= -1,169 - (0.007) * 196,544 $\approx -1,169 - 1,376$ = -2,545

²¹Substantively, this would mean that the implementation of the voter ID law had no effect on turnout among those who have photo ID. This could be violated in a few ways. First, those with photo ID might nonetheless be confused by the law and be deterred from voting. Second, those with photo ID might be motivated by the ID law, either because they are angry that those without ID could be disenfranchised, or because they are less costly for parties and interest groups to mobilize.

which is about 0.116 percent of the 2016 primary electorate in North Carolina. This is consistent with Cantoni and Pons (2019), which finds small effects of voter ID laws on overall turnout using a nationwide voter file panel. Even in a large nationwide panel researchers lack the power to detect this sized effect. Similarly, the overall effect in the general election is

Overall Effect in General = $\underbrace{\text{Mechanical Effect in General}}_{0}$ +Deterrent Effect in General = 0 - (0.026) * 196,544 \approx -5,110

which is about 0.122 percent of the 2016 general electorate in North Carolina.²²

Reconciling Null and Deterrent Effects

These findings help reconcile two competing claims that can both be true. First, prior research has shown that strict voter ID laws cause a decline in turnout among those without photo ID. And second, strict photo ID laws do not cause substantively large declines in overall turnout resulting in null effects for many aggregate-level analyses. Our results show that because the vast majority of potential voters hold valid identification, the effect of the voter ID law is to deter only a small number of voters. In an aggregate setting, this number of voters is sufficiently small that there is no design, to our knowledge, with sufficient power to detect the effect on this small number of voters.

5.1 How voter ID laws change the composition of the electorate

We perform similar calculations to assess how voter ID laws affect partisan and racial composition of the electorate. To do this, we decompose overall turnout effects by race and by party using the following procedure. For the mechanical effect in the 2016 primary election, we simply sum the number of voters who had provisional ballots ultimately rejected for lack of ID for each racial group and for each party affiliation. For the deterrent effect, we multiply our estimated deterrent effect (from column 7 of Tables 2 and 3, respectively) among those without ID by the total number of voters without ID who belong to each racial group and partisan affiliation.²³ Adding the mechanical effect and the deterrent effect gives an estimate of the overall vote reduction, which we summarize in Table 7. Panel A decomposes the vote reduction by race, and Panel B decomposes the vote reduction by party affiliation. For example, we estimate that in the 2016 primary election, the strict photo ID law reduced turnout among White voters by about 1,342 votes, 648 of which came through a mechanical effect and 694 of which came through a deterrent effect. We also show the overall effect as a fraction of the total number of voters in that group who lack ID, and as a fraction of the total number of registrants in that group. For example, the overall vote reduction of 1,342 votes in the 2016 primary among Whites represents about 1.3 percent of the total number of White voters without an ID, and it represents 0.03 percent of the total number of White registrants.

Reporting the effect as the share of registrants in that group makes the disproportionate effect of the law by race and by party clear. Looking at the 2016 general election, for example, 0.06 percent of registered White voters were deterred by the ID law, while 0.13 percent of registered Black voters were deterred by the law. This illustrates how the voter ID law can have disproportionate

²²The mechanical effect in the 2016 general election is equal to zero because the strict photo ID law was not in effect for this election.

²³This assumes that the effect of the voter ID law among those without ID is constant across race and across party. We find in Tables VI and A.9 that the effects do not seem to vary substantially by these characteristics.

	2016 Primary			2016 General				
	Mech.	Det.	Overall	Effect as % of Grp. w/o ID	Effect as % of Grp. Reg.	Overall	Effect as % of Grp. w/o ID	Effect as % of Grp. Reg.
				(A)	Decomposition b	y race		
White Black Hispanic Other Non-White Total	648 421 22 78 1,169	694 536 65 81 1,376	1,342 957 87 159 2,545	1.3% 1.2% 0.9% 1.4% 1.3% (B)	0.03% 0.06% 0.07% 0.07% 0.04% Decomposition b	2,577 1,993 241 299 5,110 y party	2.6% 2.6% 2.6% 2.6% 2.6%	0.06% 0.13% 0.18% 0.13% 0.08%
			2	016 Primary			2016 Gener	al
	Mech.	Det.	Overall	Effect as % of Grp. w/o ID	Effect as % of Grp. Reg.	Overall	Effect as % of Grp. w/o ID	Effect as % of Grp. Reg.
Democrat Republican Unaffiliated Total	670 265 234 1,169	791 264 321 1,376	1,461 529 555 2,545	1.3% 1.4% 1.2% 1.3%	0.05% 0.03% 0.03% 0.04%	2,936 982 1,192 5,110	2.6% 2.6% 2.6% 2.6%	0.11% 0.05% 0.07% 0.08%

Table 7. Vote reduction from voter ID law by race and party Affiliation

Note: Each cell presents an estimated total reduction in turnout as a consequence of the voter ID law. Panel A decomposes the effect of the voter ID law by racial group. Panel B decomposes the effect of the voter ID law by party affiliation. The first five columns decompose the effect in the 2016 general. "Mech" represents the mechanical effect, and "Det" represents the deterrent effect. Effect as percent of Grp. w/o ID is the overall effect for the group divided by the total number of voters without ID in that group. Note that the mechanical effect is zero in the 2016 general election because the law was not in effect for that election, so the deterrent effect.

effects on turnout by race. Black registrants are much more likely to lack ID than White registrants, so they bear a larger burden of the turnout reduction even though the size of the effect among those without ID is not larger for Black voters than for White voters.

In Panel B, we decompose the vote reduction by party affiliation. We find that the ID law reduced turnout among Democrats by about 1,461 votes, 670 of which came from a mechanical effect and 791 of which came from a deterrent effect. As a share of the total vote reduction, Democratic registrants account for about 57.4 percent of the total vote reduction in the 2016 primary and about 57.5 percent of the reduction in the 2016 general election.

The size of the effect as a percentage of voters in that group without an ID does not vary by party. But because Democrats are much more likely to lack ID than are Republicans, Democrats shoulder a disproportionate share of the overall vote reduction as a share of total registrants. In total, 0.11 percent of registered Democrats did not vote in the 2016 general election because of the ID law, while only 0.05 percent of Republicans did not vote in the 2016 general because of the ID law.

To situate the vote reductions due to the voter ID law in a broader context, in Table A.10 in the Appendix, we add the vote reductions from Table 7—that is, the number of additional voters we estimate would have participated had the law not been passed—to the observed number of voters in each election. It shows that, because the effects among those without ID are relatively small, along with the fact that those without ID make up a small portion of the electorate, these vote reductions have only a small effect on the overall composition of the electorate, at least along racial and partisan dimensions. We reiterate, that this interpretation of the findings requires relatively strong assumptions about voter behavior in the absence of the law. Specifically, that the law had no effect on the participation decision of those with the required identification.²⁴

²⁴We could assess the sensitivity of this assumption by making assumptions about how the size of the effect among those with ID compares to the effect size among those without ID.

6. Conclusion

Voter ID laws are politically contentious, with Republican legislatures often implementing strict requirements, and these laws are occasionally softened or overturned as a result of legal challenges (Hicks *et al.*, 2015). How these laws affect voter turnout is an important question, but there is little empirical work on the effects of these laws that use administrative data on individual turnout over time along with a measure of which individuals actually might lack ID. And no studies have considered how the effect of the laws could remain even after the law is repealed. We leverage information in North Carolina on which voters lack photo ID to estimate the effect of North Carolina's voter ID law on turnout among those identified as possibly lacking photo ID and how the extent to which the law's effects remained in place even after an appellate court struck down the law. We find that the North Carolina voter ID law deterred voters both in the primary and general election and that this occurred both because the law mechanically made individuals unable to vote because they lacked the required identification and because the law exerted a general deterrent effect as voters inferred they would be unable to vote and therefore declined to turnout at all.

These findings also help reconcile two seemingly disparate findings in the voter ID literature, but which we argue need not be mutually exclusive, because they are estimating two different quantities of interest. The first quantity of interest is the differential effect of voter ID laws on turnout among those without ID. Our findings-along with other recent work that combines individual-level information on who actually lacks ID in Rhode Island, measured at scale (Esposito et al., 2019)-show that voter ID laws cause a differential decrease in turnout among those without ID, leaving minorities and Democrats disproportionately burdened because they are less likely to have photo ID. The second quantity of interest is the effect of voter ID laws on overall turnout, including both individuals with and without identification. Using our estimates of differential turnout and plausible assumptions about the effect of the law on those with identification, we find that the effects of voter ID laws on overall turnout are very small because very few voters lack photo ID. This is consistent with recent work using a nationwide voter file panel (Cantoni and Pons, 2019). Both of these theoretical quantities are important, and our work furthers our understanding on both of these fronts. Moreover, this paper illustrates that the widely varying findings in the voter ID literature stem from the combination of lack of clarity about which is the estimand of interest along with limited data and to estimate either of these quantities with precision.

Our results also suggest important policy considerations as states consider voter identification laws and then those laws are contested in courts. If voters are not informed about changing laws they may continue to be deterred by the incorrect belief that they lack proper identification to vote. Future research should examine effective ways to inform voters about the changing requirements to cast ballots.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/psrm.2020.57.

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Exhibit 13

RESEARCH ARTICLE

Battling the Hydra: the disparate impact of voter ID requirements in North Dakota

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Abstract

Minority voters have experienced a renewed effort to curtail their access to the ballot box in recent years. Although a host of research has examined the impact of election changes on Black and Latino voters, scholars have dedicated much less attention to the rights of Native Americans, even as they face challenges to voting in states where they comprise a significant portion of the population. Many of these states are likewise increasingly important to national elections. Such laws may impact Native Americans when they intersect with the political geography of living on a reservation, and voting rights advocates have challenged them in places like MT, NV and ND. This paper empirically evaluates how such laws might uniquely impact Native American voters. We draw on North Dakota's voter identification law as a case study, but our analysis has wider implications, since residency is the primary means by which election administration uniquely impacts this group. Drawing on two rich survey datasets collected in 2015 and 2017, we offer descriptive evidence of the barriers individuals may encounter while trying to obtain an ID under North Dakota's law, and find that Native Americans are statistically less likely to have access to an ID than are whites. This gap is largely due to the requirement that an ID has a physical address and attendant difficulties in obtaining such an ID, given the remote nature of reservations. We bring needed attention to the impact of carefully crafted electoral rules on this often-overlooked group.

Keywords: Voter identification laws; indigenous politics; state politics; voting rights act

Introduction

Since *Shelby County v. Holder* (2013), which undercut key enforcement provisions of the Voting Rights Act, minority voters have faced increased barriers to accessing the vote in states across the country (Barreto *et al.*, 2019; Fraga and Miller, Forthcoming; Walker *et al.*, 2019). Native Americans are no exception in states where they comprise a substantial portion of the population and are potentially decisive to election outcomes. In the few years leading up to the 2020 election, voting rights advocates have challenged election rules in several states on the grounds that they are likely [©] The Author(s), 2022. Published by Cambridge University Press on behalf of The Race, Ethnicity, and Politics Section of the American Political Science Association

to disproportionately impact Native American voters. Two months before the election, the courts in MT struck down provisions that restricted how ballots could be collected for the disproportionate burden such provisions would present for Native American people living on remote reservations (*Court permenantly strikes down MT law that restricts voting rights of* Native Americans, 2020). Advocates have likewise challenged election rules in NV, which moved to only vote-by-mail in response to the Novel Coronavirus Pandemic, but which again threatens Native Americans' capacity to vote, since many people living on reservations do not receive residential mail delivery (*NV tribes seek to protect Native* voters, 2020).

Perhaps most notably, ND instituted a voter identification requirement that seemed targeted to Native American residents, insofar as it required one's ID to list a physical address, which many living on reservations do not possess. North Dakota's residential address requirement recalls those employed by states to disenfranchise Native Americans under Jim Crow, which for a variety of reasons very often turned on whether one lived on a reservation (described in detail below). Yet, even as election administration rules intersect with residency and geography in ways that increase barriers to voting for this group, little empirical evidence exists around how such laws systematically impact Native Americans. This paper addresses this gap in the literature. We draw on two unique surveys of eligible Native American voters in ND that measure access to a valid piece of identification under the state's law. While our analysis is limited to one specific state, the richness of our data allow us to draw out how election administration may impact Native American voters when those rules intersect with the specific nature of Native American residency and the political geography of living on a reservation. Our analysis thus offers insight into the ongoing contest around voting rights for this group across multiple contexts.

Over 30 states have laws requiring identification to vote (Underhill, 2019). Since the Supreme Court ruled that such laws do not violate the Voting Rights Act legal challenges have shifted to the states, yielding inconsistent outcomes (Barreto et al., 2019). Legislators craft laws specific to state context, and ND is a key example. At 5% of the population, Native Americans are the state's largest minority group (Statistics, 2019). Prior to 2013, the state employed a non-strict identification requirement that allowed vouchers and affidavits as failsafe measures (Campbell and De Leon, 2020). Following Democrat Heidi Heitkamp's successful senate campaign, to which Native Americans were crucial, lawmakers passed a strict version eliminating the failsafe measures (Reilly, 2018). Acceptable identification must include one's name, date of birth and current residential address (Campbell and De Leon, 2020). While tribal identification is accepted, such IDs often list a mailing address instead of a residential one (Campbell and De Leon, 2020). Over half the Native population resides on a reservation (Statistics, 2019), which are so remote they are not provided residential delivery by the postal service. Instead, individuals receive mail at a post office box, and it is this address that is printed on tribal IDs (Campbell and De Leon, 2020). While advocates successfully challenged the law in 2015, the amended version preserved the residential address requirement, and it was again challenged in 2017 (Campbell and De Leon, 2020).

Lawsuits challenging North Dakota's law draw on survey data examining access to an ID among Native and non-Native residents, but more rigorous analyses than a simple comparison of the haves and have-nots is lacking. Moreover, existing scholarship overlooks the extent to which such laws are tailored to the populations in the states that adopt them. North Dakota's residential address requirement recalls those employed by states to disenfranchise Native Americans under Jim Crow. As states with large Native populations, like NV, AZ, TX, and FL, become important to national elections, and their minority populations to local ones, it behooves scholars to understand the means by which otherwise seemingly innocuous policies disproportionately inhibit access to the vote. To that end, we employ two unique surveys to detail the impacts of the voter identification laws on Native voters in ND. We ask: *Do Native Americans in ND possess a valid identification at rates similar to their white counterparts, or like minorities in other states are they less likely to possess such an ID? Does any observed relationship between race and possession of a valid ID persist after accounting for socio-economic status? What are the specific obstacles to voting faced by this population?*

In what follows, we begin by detailing the historical disenfranchisement of Native Americans. We then review the specific context in ND, drawing out how we might expect such laws to negatively impact Native voters. We then detail our data and methods, before turning to an explanation of our findings. We find that Native Americans are seven percentage points less likely to possess a valid piece of identification than are whites, a gap that replicates across both datasets and remains statistically meaningful after including relevant covariates. Among those without an ID, Native Americans are significantly less likely to possess the necessary documents to obtain an ID. Finally, Native Americans are more likely to report that they would face significant barriers to getting an ID. We bring needed attention to the impact of carefully crafted electoral rules on this often-overlooked group.

Relevant literature

The historical disenfranchisement of Native American voters

Historically, Native Americans have faced institutional barriers to voting that centered around their status as members of sovereign nations and without formal citizenship. At the turn of the 20th century when federal policy focused largely on assimilating the Native population, tribal members could gain citizenship and the right to vote through demonstrably cutting all ties with their tribe and traditional ways of life (Wolfley, 1991). Subsequent to the passage of the Indian Citizenship Act of 1924, which granted Native Americans formal citizenship, institutional barriers to voting stemmed from the threat they posed to the dominant racial order in states where they comprised a large enough portion of a given electorate to sway election outcomes (Ferguson-Bohnee, 2015). States employed Jim-Crow style tactics to deny Native Americans the right to vote. In some places officials argued that tribal sovereignty precluded participation in state and local government, even as citizenship permitted voting in federal elections; drawing on similar logic, administrators in AZ employed competency requirements, where they argued that residents of reservations were technically dependents of the federal government, and thus not fit to cast a ballot (Ferguson-Bohnee, 2015). In NM, Native Americans were summarily barred from

voting, despite gaining citizenship, until 1948 (Ferguson-Bohnee, 2015; Kickingwoman, 2020). Access to the ballot was won when Miguel Trujillo, a World War II Marine veteran from the Pueblo of Isleta, used litigation to extend the right to vote for all Native American people in the state (Kickingwoman, 2020).

Especially pernicious were residency requirements and literacy tests. States like UT leveraged residency requirements to exclude Native Americans, delineating in-state residency along reservation lines, and in some cases arguing that tribal citizenship precluded state citizenship (Wolfley, 1991; McDonald, 2004). Literacy tests likewise disproportionately impacted Native Americans, a substantial portion of whom were not English literate (Wolfley, 1991). Literacy requirements in particular led to special coverage under the Voting Rights Act, extended to protect language minorities in 1975, where substantial evidence supported that Native Americans had routinely had their rights curtailed (McDonald, 2004; Schroedel and Aslanian, 2015). Between 1970 and 1975 alone the Department of Justice was involved in over 30 lawsuits regarding the disenfranchisement of this group (McDonald, 2004; Schroedel and Aslanian, 2015). In the wake of the 1975 extension, efforts to diminish the electoral power of Native Americans again mirrored those directed toward other minority groups, centering on vote dilution through districting, election and nomination schemes that prevented them from electing their candidates of choice (Wolfley, 1991; McDonald, 2004; Wang 2012; Pryor et al., 2019).

The history of the suppression of Native American votes provides insight into how more recent changes to election administration, like voter identification laws, might disproportionately impact Native American voters. Requirements that one's ID carry a physical address beyond proof of residency on a reservation within a given state establish extra barriers related to reservation dwelling, reminiscent of these Jim Crow era laws. The historical institutional context around voting restrictions suggests that to the extent that changes to election administration condition access to the ballot box on residency in ways that uniquely intersect with tribal affiliation in particular, we should expect disproportionate outcomes for Native American voters. This is true above and beyond the sorts of barriers introduced by the low socio-economic status that Native Americans may likewise face, but which changes related to residency may exacerbate. For example, reducing the number of polling locations available at which to cast ballots may be expected to disparately negatively impact Native Americans living on reservations insofar as they may have to travel further to vote. Thus, in assessing how identification requirements might negatively impact Native Americans, we center the challenges posed by residing on a reservation.

The impact of voter identification laws on Native Americans

As a consequence of this long history with institutional discrimination, researchers find that Native Americans vote at exceptionally low rates, even relative to other minority groups (Peterson, 1997; Huyser *et al.*, 2017). Yet, the turnout gap is narrowing and Native Americans participate at higher rates in other types of activities, suggesting that low voter turnout is not due to efficacy or interest (Skopek and Garner, 2014; Huyser *et al.*, 2017; Herrick and Mendez, 2019). At the same time, data limitations inhibit our knowledge around Native American political attitudes

and behavior. Surveys standard in the discipline of Political Science, like the American National Election Survey and the General Social Survey, include too few Native American respondents to generate reliable analyses. Thus, little systematic evidence around the impact of voter identification laws on Native American populations exist. We therefore turn to research on the impact of voter identification laws on other non-white groups, together with what we know about the historical exclusion of Native Americans specifically, to develop a theoretical foundation for the analysis that follows.

Research around the impact of voter identification laws on turnout is mixed (Erikson and Minnite, 2009; Hershey, 2009). Several studies conclude that the impact of voter ID laws on turnout is negligible (Erikson and Minnite, 2009; Mycoff et al., 2009; Grimmer et al., 2018; Cantoni and Pons, 2021). Others find that the strictest laws have a depressive effect, but do not find differences among racial and ethnic groups (Hood and Bullock, 2012; Hood and Buchanan, 2020). In keeping with this, using an opt-in survey of Native Americans, Herrick and Mendez (2019) find that voter ID laws and court cases related to American Indian voting rights have no effect on the participation of Native Americans. In contrast, a handful of studies demonstrate the negative consequences of identification laws for voting among Black Americans and Latinos (Vercellotti and Anderson, 2006; Hajnal et al., 2017; Kuk et al., 2020; Fraga and Miller, Forthcoming; Grimmer and Yoder, 2021). For example, Hajnal et al. (2017), use validated vote data from the Cooperative Congressional Election Study to examine turnout across states and elections, and find diminished turnout among Latino and Black Americans in states with the strictest laws. In what is perhaps the first direct test, Fraga and Miller (Forthcoming) link voter files in TX with records of individuals who cast a provisional ballot when unable to present an ID at the polls, and report that, "Black voters were approximately 54% more likely to vote without identification than non-hispanic whites, while Latinx voters were 14% more likely to do so than non-hispanic whites," (pg. 17). Using administrative records from NC and exploiting changes in the ID law in 2016, Grimmer and Yoder (2021) find that the law deterred turnout among those without ID, and that non-white voters were less likely to possess an ID than were white voters. In keeping with this, Henninger et al. (2021) found that minority voters in MI who ostensibly had ID as verified by administrative records were five times more likely to fail to produce that ID at the polls than were white voters.

These studies are laudable for their precise empirical approach to evaluating the extant effects of voter identification laws. However, they are focused on turnout, evaluating access to a valid piece of identification among individuals who are already registered to vote. An exclusive focus on how such laws may impact individuals already registered to vote or with a voting history obscures the full extent of the harm incurred by laws apparently structured to disenfranchise minority voters. Assessments of these laws should include an account of the real barriers they erect for all eligible voters. One does not lose the right to vote simply because one is unlikely to use it, and the importance of access to the franchise to the health of American democracy should remain at the center of our inquiry if we are interested in evaluating power. Moreover, administrative records do not permit an analysis of the unique barriers ID laws present to members of hard to reach populations for the same reasons that they are hard to reach: such individuals may live on the social margins and in ways that evade capture by government records. This is particularly true in the case of Native Americans who live on reservations, whose residency does not make their claim to the right to vote less valid.

In order to answer the normatively democratic important question of the disparate impact of identification laws, scholars have leveraged surveys that allow them to employ culturally competent methods of reaching minority populations, to ask specific questions specific to a states' law, and around access to relevant resources to obtain a compliant ID. For example, leveraging six datasets collected between 2008 and 2014, Barreto et al. (2019) find that Blacks and Latinos were almost ten percentage points less likely to have an appropriate ID than were whites. Even given the breadth of their data, however, Barreto et al. (2019) do not offer insight into how such laws might uniquely impact the Native American population. Recent findings that Black Americans and Latinos are less likely to possess a valid piece of identification together with the specific history of the disenfranchisement of Native Americans suggests that this group is likely to be vulnerable to such laws. As noted above, election rules are likely to disproportionately impact Native Americans when access to the vote is conditioned on residency, wherein tribal affiliation is relevant. The requirement that one shows a piece of ID to vote is not facially problematic, but it may become so depending on the conditions one's ID has to meet. For example, if one must possess an identification issued by the state where tribal identification would not suffice, we would expect Native Americans to be disproportionately impacted by that law. The ND law includes a requirement that one's identification list a physical address. The physical address requirement itself likewise becomes problematic because while tribal ID's are accepted, they often do not include a physical address. As a consequence, the law has been challenged in court.

North Dakota

ND first introduced a voter identification requirement in 2004 pursuant to the Help American Vote Act (HAVA). The initial law required voters to provide a piece of ID at the polls, but allowed a wide variety of types of ID, and individuals had the option to vote by affidavit or for poll workers to vouch for them (Campbell and De Leon, 2020). In 2012, the race for North Dakota's open Senate seat was extraordinarily close with Democrat Heidi Heitkamp beating out her Republican opponent by less than 3,000 votes (Nichols, 2018). In the months after her victory, the Republican-dominated legislature began considering proposed changes to their existing voter ID law, eventually passing a strict version in 2013. The 2013 law eliminated fail-safe options including voting by affidavit and with the affirmation of one's identity by a poll worker, and tightened restrictions on the types of ID poll workers may accept (Campbell and De Leon, 2020). Under the new law, identification must be unexpired and must include one's name, date of birth, and current physical address.

The requirement that the piece of identification includes a current physical address was understood to potentially disproportionately impact Native Americans, who are less likely to have a piece of identification with a physical address and more likely to possess an ID that contains a P.O. Box (Feldman, 2018). Native Americans were

crucial to Heitkamp's victory. The Native American Rights Fund (NARF) challenged the law in 2016, and the courts blocked its implementation without some kind of failsafe measure, in recognition of its potentially disparate impact on Native Americans (Campbell and De Leon, 2020). Therefore, in 2017 ND law-makers rewrote the law, this time including the option to fill out a provisional ballot contingent on the presentation of a valid piece of identification within 6 days of the election (Astor, 2018). Thus, while the new law addressed the concerns outlined by courts insofar as it ensured no one would be turned away at the polls, it still inhibited voting for those who lacked a piece of ID with a physical address in the first place. The U.S. District Court therefore blocked the enforcement of the 2017 law, issuing an order that allowed identification with P.O. boxes and expanded the types of acceptable identification. The state of ND challenged the district court's ruling, but their request to stay the order during the June primaries was denied by the 8th circuit court of appeals in June of 2018 (Feldman, 2018). In the lead up to the 2018 midterms, the 8th circuit again held a special hearing and this time overturned the circuit court's order, meaning that the law would be in effect during the November election, at which point early voting had already begun in ND. NARF filed an emergency application with the Supreme Court. The Court denied the application on the grounds that they did not want to introduce additional confusion during the election process (Campbell and De Leon, 2020).

At 5.6% of the population, Native Americans are the largest minority in ND (Statistics, 2019). The key provision in North Dakota's law that has natural implications for Native Americans is the requirement that one's ID list a physical address. A P.O. Box is considered unacceptable. Over half of the Native American population lives on a reservation (Statistics, 2019). Reservations are remote, so much so that they are not provided residential delivery by the postal service. Moreover, most residences located within the reservation are not assigned an official address, colloquial knowledge standing in for formal mapping and directions. For these reasons, Tribal governments and the Bureau of Indian Affairs print ID cards with a P.O. Box. Thus, the means by which the voter ID law may directly, uniquely and negatively impact Native Americans in ND is straight forward. As a consequence of this potential impact, in February of 2020 ND entered into an agreement with two tribes that makes suitable provision for individuals who may not have a physical address listed on their ID (Campbell and De Leon, 2020). Thus, the law both remains in effect while making an exception for potentially impacted Native Americans. The case of ND provides insight into how administrative laws in other states with significant Native American populations may likewise impact those voters.

Voter identification laws may both impact racial minorities directly via tailored clauses like the address requirement in North Dakota's law, but they also do so indirectly via the costs that are associated with accessing a valid piece of government-issued identification. For example, it may be difficult for some eligible voters to access offices where they can get a piece of identification, and researchers estimate that in states with restrictive laws substantial portions of the voting age population live at least 10 miles from an office that issues state IDs. In MS, WI and AL over 30% of the voting-age population resides more than 10 miles away from such an office, and these same states very often have a low investment in public transportation

(Gaskins and Iyer, 2012). In the case of ND, law makers appeared to have taken efforts to substantially reduce access to offices providing state-issued IDs. The 2017 version of the law reduced the hours of the driver's license site that is most accessible to the specific tribes named in the suit such that it, "now operates the most restrictive hours of any location; it is open for less than five hours one day a month; there is not a single driver's license site on an in Indian reservation in ND," (Campbell and De Leon, 2020).

The cost of accessing a valid piece of ID further includes those associated with getting the underlying documentation, like a birth certificate or bank statement or utility bill that bears one's address. Native Americans in ND lag behind their non-Native counterparts socio-economically across a variety of indicators. While over 90% of the total population in the state has at least a high school diploma, this decreases to 79% of Native Americans (*The American Community Survey*, 2019). Almost 40% of Native Americans live below the poverty line, compared to about 10% of the overall population (*The American Community Survey*, 2019). They are likewise less likely to have full employment, to have access to health insurance and to have achieved a college degree than are non-Natives living in ND (*The American Community Survey*, 2019).

One aspect of identification laws that has been lost in the scholarly shuffle around the impact of these laws on turnout is the extent to which identification laws are tailored to the populations in the states that pass them. Identification laws are not created equal, but instead employ unique clauses and caveats that impact the marginalized in unique ways. North Dakota's law now includes appropriate protections for Native American voters, but those protections were won because of a lengthy and costly court battle waged by voting rights activists that spanned multiple election cycles. As states with large indigenous populations become increasingly important to national elections, and these same voters to local ones, it behooves scholars to understand the means by which otherwise seemingly innocuous policies and procedures disproportionately inhibit or support access to the ballot box. To that end, we now turn to unique survey data to detail the potential impacts of the voter ID law on Native and non-Native voters in ND.

Data and methods

This project assesses access to a valid piece of voter identification under North Dakota's law among Native and non-Native Americans in the state. In order to do this, we draw on two original surveys, one collected in 2015, reflecting the specifications of the 2013 law; and one in 2017, after the 2017 amendment which allowed individuals to cast a provisional ballot. The chief alternative approach to measuring rates of possession of a valid ID leverages large, administrative databases collected by relevant public entities, such as the department of motor vehicles (DMV). However, this approach faces a number of challenges. Not all acceptable types of identification may be included in administrative records of this sort, and such records do not account for factors like whether one has lost their ID, had it stolen, changed their name due to marriage, and so forth (DeCrescenzo and Mayer, 2019). Reliance on administrative datasets, moreover, preclude the opportunity to evaluate whether

any observed differences persist after controlling for relevant socio-economic factors and the specific obstacles one might experience in an effort to obtain an ID. In order to overcome these challenges, researchers have leveraged survey methods, which allows one to ask individuals directly about the specific nature of their identification in ways tailored to the laws in a given state, and about various other challenges that might arise from having lost one's ID, and confusion around the exact nature of a given law (Barreto *et al.*, 2019; DeCrescenzo and Mayer, 2019).

Surveys are not without their own limitations, and chief among them are challenges related to generating a representative sample of the population of interest. These challenges are particularly acute with respect to hard-to-reach groups. Native Americans are considered hard to reach via traditional survey methods due to the fact that they are numerically small, geographically dispersed, more likely to lack telephones relative to other populations, and language and cultural barriers decrease trust in the interviewer and the surveying organization (Lavelle *et al.*, 2009). Thus, multiple modes of contact, including mail, is the best strategy for collecting data that accurately represent the population (De Leeuw, 2005; Dillman *et al.*, 2007). A multi-mode approach can also help mitigate other limitations of surveys, including response and sampling bias (Fowler *et al.*, 1998; Chang and Krosnick, 2009; Rao *et al.*, 2010; Sakshaug *et al.*, 2010; Ansolabehere and Schaffner, 2011; Atkeson *et al.*, 2014). This is the strategy developed by the U.S. Census Bureau in an effort to mitigate undercounting the population, and is thus the most rigorous means of sampling Native Americans available to researchers (Hatcher, 2002; Lavelle *et al.*, 2009).

Thus, we followed best practices and composed a sampling frame from lists of landline and cellphone numbers, as well as an addressed-based list administered via the mail. The telephone portion of our list came from a combination of random digit dial landline and cell phone sample for the entire state of ND, and a targeted phone sample for Native Americans. For the mail portion, we relied on a combination of randomly selected home addresses and randomly selected Post Office boxes for the state. To ensure that we were reaching a representative sample of Native Americans, we further received a member enrollment roster from each of the major tribes in ND and selected a random sample to contact via mail, after removing individuals already present in the mail/phone sampling frame. Finally, to encourage participation individuals contacted via the mail received multiple follow-up contacts including a letter from tribal leaders, which was intended to increase compliance by heightening trust in the researchers and a sense of importance around participating.

The resulting sample reflects the very deep socioeconomic disparities between Native and non-Natives in the state. For example, according to data from the 2016 American Community Survey, the median income among for whites was \$61,387 compared to \$33,122 for the American Indian population.¹ In the 2015 survey, the Native American sample had a mean income between \$30,000 and \$40,000 annually, relative to non-Natives who had a mean income between \$50,000 and \$60,000 annually. Our strategy yielded a slightly more educated sample than the statewide population, where 29% of whites in the state hold a college degree compared to 16% of Native Americans.² Comparatively, in our 2015 sample 32% of Non-Native respondents and 25% of Native respondents held a college degree. To correct for

these discrepancies, the samples were weighted on age, education, gender, income and reservation dwelling to bring them in line with census estimates.

The final samples included 456 Native American and 875 non-Native respondents in the 2015 survey, and 434 Native American and 869 non-Native respondents in the 2017 survey. Because the method of contacting folks by mail was developed specifically to bolster the hard-to-reach Native American population, nearly all non-Native respondents in both waves completed the survey via phone (97% in 2015, and 98% in 2017). In contrast, 64% of Native American respondents in 2015 and 48% in 2017 completed the survey by mail.³ In addition to large sub-samples of Native American voters, the primary analytic value in repeated cross-sections is replication and (dis)confirmation (rather than allowing, for example, an assessment of change over time, which our data are unable to address). The majority of non-Native respondents in both surveys are white. In the 2015 survey, only 88 respondents identified as non-white and non-Native, and in 2017 this was true for only 61 respondents. This reflects North Dakota's overall population, which is 87% white (*The American Community Survey*, 2019).

Both surveys were designed to evaluate the extent to which voters in ND have access to the sort of identification required to vote under the state's law. Thus, questions were tailored to the law's specifications. For example, respondents were asked to indicate whether they were in possession of any of the accepted forms of identification: a ND driver's license, a ND non-driver identification card that was issued by the ND Department of Transportation, a tribal government-issued identification card or one issued by the Bureau of Indian Affairs, or a long-term care identification certificate issued by a ND facility. Respondents were then asked to indicate whether they had their ID, or whether it had been lost, stolen or revoked, and those who indicated they had their ID were prompted to take it out and look at it to answer questions that followed. Respondents were asked to confirm whether their ID included an address, that the address was a physical address (rather than a P.O. Box), and that it was current.⁴

The law included additional provisions that the ID carry one's date of birth and that it be unexpired. However, the provision of the law thought to disproportionately impact Native Americans concerned whether one's ID listed a current, physical (residential) address. Therefore, individuals who affirmed that they possessed an accepted piece of identification that listed a current, physical address were coded as having an acceptable ID; everyone else was coded as not having an acceptable ID.⁵ In the 2015 survey dataset, 12% of respondents indicated that they did not have access to a valid piece of identification, as did 13% of respondents in the 2017 dataset.⁶

In addition to exploring racial differences among those who do not have access to a valid piece of identification, we are further interested in the various obstacles individuals might face should they want to obtain an ID. In both surveys, we therefore followed up and asked respondents whether they had access to the underlying documents required to obtain identification issued by the state of ND. These documents include proof of citizenship, such as a birth certificate, documentation of a name change if circumstances require it; proof of residency; and proof of social security. In the 2015 survey, among the total population who lacked a valid piece of identification, 60% lacked the appropriate underlying documents to obtain one; the same was true for 51% of respondents in the 2017 survey. In order to evaluate racially disparate rates of access to a valid piece of identification, we proceed in the following way. Leveraging logistic regression analysis, we begin by evaluating bivariate rates of possession of a valid ID among Native and non-Native respondents. We then subject these findings to more rigorous analysis, including a variety of relevant covariates. Factors that are likely to impact access to a valid piece of identification include socio-economic status, age and gender; those with low levels of education and income are less likely to have a driver's license, and also more likely to face obstacles obtaining such an ID; women face unique challenges, due to the cultural norm related to changing one's name upon marriage; younger voters are more likely to be transient and to not have an updated address; and the elderly are more likely to have expired identification (Barreto *et al.*, 2019). Researchers have likewise found that gender is an important variable to the political incorporation of Native Americans, where men lag behind their female counterparts (Sanchez *et al.*, 2020). We then repeat the analysis, but among those without a valid piece of identification, and with respect to barriers to obtaining such identification.

Findings

At the bivariate level, Native Americans are statistically less likely to possess a valid piece of identification than are their white counterparts (p < .01). In the 2015 survey 82% of Native Americans had a valid piece of identification, relative to 89% of white Americans (Figure 1, Table 1). Likewise, in the 2017 survey 81% of Native Americans relative to 88% of whites had an ID acceptable under North Dakota's law. For context, Barreto *et al.* (2019) report that in their combined sample, 90.5% of white eligible voters had access to a valid piece of identification, compared with 81.2% of Black eligible voters and 82% of Latino eligible voters. Thus, the findings from ND comport with known estimates derived from similar methods.

Although the size of the effect of being Native American on the likelihood of possessing a valid piece of identification diminishes after controlling for a variety of different covariates, the impact of being Native American remains statistically significant (Table 1). Figure 2 displays the average marginal effects of each variable included in the models displayed in Table 1 on the likelihood of possessing a valid piece of identification. Native American status is as important to having access to a piece of identification as is making less than 40k annually in the 2015 dataset and being a young person in the 2017 dataset. We therefore take this as strong evidence that, like Latino and Black Americans in other states, Native Americans are disproportionately burdened by North Dakota's voter identification law. This is particularly striking, given that racial differences persist even after accounting for education and income. Although it is the case that Native Americans are on average poorer than non-Native Americans in ND, material deprivation alone does not account for disparate rates of access to a valid ID. The data allow us to drill down further to examine what percentage of each group lack a valid ID specifically due to the address requirement. Among Native Americans without a valid ID, 20% are due to the address requirement, compared to only 8.2% of non-Native residents for whom the same is true. Like Jim Crow era residency requirements, the unique structure of the law itself and its intersection with the political geography of reservations account for differences in access to a valid piece of identification.

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Figure 1. Rates of possession of a valid piece of voter identification among Native and Non-Native Americans in ND in 2015 and 2017.

Next, we examine the specific barriers Native Americans face to obtaining a valid piece of identification. We begin by examining access to the underlying documents needed to obtain a government-issued ID. This is particularly relevant because in 2016, ND amended their rules, allowing Native Americans to cast a provisional ballot, assuming they could provide proof of residency within a week of the election. Yet, Native Americans may disproportionately lack these documents for the same reasons that they are less likely to have a valid piece of identification. Table 2 displays rates of possession of the various required documents, including proof of address, to obtain a government-issued piece of identification among survey respondents who did not already have one. Across both surveys, non-Natives are more likely to possess each type of required document, and more likely to possess all three types of required documents. In the 2015 survey, 77% of non-Native respondents who did not already possess a valid piece of identification indicated they could provide some other form of identification, such as a passport or birth certificate, relative to only 62% of Native Americans for who the same was true. Similar differences persisted in the 2017 survey.

Specifically with reference to proof of residence in ND, which would also suffice when paired with an affidavit and an otherwise valid ID, Native Americans are likewise disadvantaged. Such documents might include a utility bill or a financial statement with a ND address listed. In the 2015 survey 81% of non-Natives indicated they had such proof, relative to 71% of Native Americans. In the 2017 survey, the gap is dramatic at over a 30 percentage point difference. We do not make too much of this large gap, however, given that respondents without an appropriate piece of ID are a fairly small portion of our sample overall, and smaller still when split among Native and non-Native respondents. Instead, we examine these trends in order to illustrate that the gap between the two groups persists across nearly all ways of looking at access to an ID and relevant underlying documents.⁷

The specific disadvantage experienced by Native Americans is further demonstrated by rates of use of the affidavit failsafe at the polls in 2016—among those counties with the highest concentrations of Native Americans 5% of ballots cast were via affidavit, while among those counties with the lowest concentration of Native Americans only 2% of votes cast were via affidavit (Barreto and Sanchez, 2017).

	2015	2017	2015	2017
Native	.637***	581***	.351**	313*
	(.146)	(.155)	(.162)	(.169)
Female			.094	181
			(.152)	(.163)
<40k			544**	814***
			(.212)	(.206)
100k+			059	.648**
			(.241)	(.286)
Missing income			149	.165
			(.260)	(.286)
Age: 18–34			898***	483***
			(.165)	(.177)
Age: 65+			.621**	182
			(.263)	(.241)
<high school<="" td=""><td></td><td></td><td>915***</td><td>.530*</td></high>			915***	.530*
			(.214)	(.274)
College grad+			413**	.037
			(.194)	(.213)
Missing education			263	.537
			(.728)	(1.225)
Constant	2.129***	2.032***	2.687***	2.384***
	(.098)	(.102)	(.228)	(.233)
Observations	1,331	1,303	1,331	1,303
Log Likelihood	-634.419	-573.927	-593.858	-536.546
Akaike Inf. Crit.	1,272.838	1,151.853	1,209.716	1,095.092

Table 1. Logistic regression results—possession of a valid piece of voter identification by year

*Note:***p* < .1; ***p* < .05; ****p* < .01.

Moreover, the ID law changed between the 2012 and 2016 elections. Likewise, the use of the affidavit failsafe method increased in high Native American counties by 665%, but declined by 11% in low Native American counties (Barreto and Sanchez, 2017).

Finally, in addition to proof of identification and residency, individuals must provide evidence that they have a social security number. In 2015, 93% of Native and 98% of non-Native American respondents indicated they could provide a social security number. In 2017, 96% of respondents indicated that they could provide proof of a social security number, relative to 83% of Native Americans for whom the same was true. Overall, in 2015 66% of non-Native respondents indicated they



Figure 2. Factors impacting possession of a valid piece of voter identification among Native and Non-Native Americans in ND in 2015 and 2017.

	2015		2017		
	Non-native	Native	Non-native	Native	
% Proof of ID	76.9	61.9	83.6	71.1	
% Proof of residency	81.0	77.1	74.5	43.3	
% Social security	98.3	93.3	96.4	83.3	
% All documents	66.4	46.7	63.6	34.4	
N-Value	98.0	62.0	100.0	64.0	

Table 2. Percent possessing necessary documents to obtain an ID by race, among those without an ID

had all of the documents required to obtain a government-issued ID, relative to 47% of Native Americans. In 2017, this gap grew to a 30 percentage point difference, where 64% of non-Native respondents indicated they had all required documents relative to 34% of Native American respondents. In both surveys, these bivariate differences are statistically meaningful (Table 3).

The 2015 survey afforded the opportunity to explore even further the kinds of barriers individuals may experience when attempting to obtain a government-issued piece of identification that complied with North Dakota's law. Respondents were asked whether a variety of circumstances would pose a problem for getting to the DMV in order to obtain an ID. They included the following: finding out where the nearest DMV office was located; getting a ride to the DMV office; getting time off of school or work to make the trip; accessing public transportation to make the

	2015	2017	2015	2017
Native	805***	-1.192***	272	-1.087***
	(.277)	(.298)	(.343)	(.328)
Female			.531*	661**
			(.312)	(.330)
<40k			543	414
			(.411)	(.412)
100k+			.968*	397
			(.559)	(.622)
Missing income			-1.755***	783
			(.593)	(.624)
Age: 18–34			.109	.068
			(.323)	(.358)
Age: 65+			1.318**	.372
			(.640)	(.499)
<high school<="" td=""><td></td><td></td><td>529</td><td>-1.069</td></high>			529	-1.069
			(.437)	(.702)
College grad+			.736*	030
			(.428)	(.417)
Missing education			.614	-14.498
			(1.561)	(902.600)
Constant	.667***	.563***	.294	1.262**
	(.196)	(.199)	(.471)	(.520)
Observations	160	164	160	164
Log likelihood	-146.337	-131.479	-126.993	-126.587
Akaike Inf. Crit.	296.675	266.958	275.986	275.175

Table 3.	Logistic regre	ession resul	ts—possessi	on of u	nderlying	documents	necessary to	obtain	an id	by
year										

Note: p < .1; p < .05; p < .01.

trip; making it to the office during their regular business hours; and making it to their office during regular business hours, specifically during the week. In almost all cases, Native Americans without an ID were more likely to indicate these items would pose a problem for them in trying to obtain an ID (Table 4). Almost 50% of Native Americans indicated taking time off of work or school would be a problem, relative to 29% of non-Native respondents who said so. About 25% of Native Americans likewise indicated it would be a problem to get a ride to the DMV. Fully 67% of Native American respondents without an ID indicated that at least one or more items in the battery posed a problem, relative to 60% of non-Native respondents for whom the

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	Non-native	Native
% Nearest DMV	8.6	11.4
% Ride to DMV	13.8	24.8
% Time off work	29.1	45.7
% Public transit	21.6	48.6
% Business hours	35.0	33.6
% Weekdays	38.8	40.0
% One or more	59.5	67.3
N-Value	98.0	62.0

 Table 4. Percent facing issues getting to the DMV to get an ID, among those without an ID in the 2015

 survey

same was true. The findings with respect to these kinds of issues that might arise when getting an ID indicate that getting an appropriate ID is not an easy task for those who do not already have one, and in turn highlight the real barrier voter identification laws pose to accessing the vote. These barriers are problematic for all voters, but Native Americans in ND face a disproportionate burden relative to their non-Native counterparts.⁸

Descriptive findings around access to the DMV provide useful context when thinking through our final research question, which asked what sorts of barriers individuals face when attempting to obtain an ID. Our primary point of inquiry pertains to differences between non-Native and Native American respondents, and we endeavor to rigorously assess these differences by ruling out other explanations, like socio-economic status, and subjecting them to replication. Table 3 displays the results of logistic regression analysis. As mentioned above, at the bivariate level the difference between rates of access to documents required to obtain a government-issued ID are statistically significant.

When subjected to a more rigorous analysis that includes control variables, we find that these differences only achieve significance in the 2017 sample (Table 3). In the 2015 sample, while the impact of being Native American remains negative, the size of the effect is relatively small. These relationships are likewise displayed in Figure 3. Here we can see that in 2015, age, wealth and education have the strongest impact on access to underlying documents required to obtain an ID. In 2017, identifying as Native American was statistically and negatively associated with access to relevant underlying documents among those without an ID, and the size of the effect is commensurate with not having graduated from high school.

We interpret these findings as suggestive that Native Americans lack access to underlying documents necessary to obtain a piece of identification above and beyond issues associated with material deprivation. Together with contextual evidence that Native Americans would face a variety of issues even getting to a DMV, which are likewise associated with the remote nature of living on a reservation, it is clear that the voter identification law in ND poses a significant challenge to accessing the ballot box for this group.



Figure 3. Factors impacting possession of the necessary documents to obtain an ID among Native and Non-Native Americans without a valid ID in ND in 2015 and 2017.

Conclusion

This paper has examined disparate rates of access to a valid piece of identification required to vote among Native and non-Native Americans in ND. We undertake this inquiry because in the wake of court decisions removing important protections for the right to vote for minority populations, attempts at curtailing or diluting minority voting power have proliferated. Scholars elsewhere have paid close attention to the impact of changes to a variety of aspects of election administration on Black and Latino voters. Much less attention as been paid to how election administration may impact Native American voters. At the same time, the importance of the Native American vote to the outcome of federal and local elections is heightened as once reliably red states become purple with changing demographics. Such states include NM, OK, where at least one house district was competitive in the 2018 midterm election, AZ, NV and CO. These states are among the top 15 in terms of size of the Native American population, and NM and OK are among the top five. Even states like MT and ND that are not important to presidential outcomes, where election laws have been challenged specifically in reference to Native Americans, take on national importance because they have (or have had) representation from both parties in the national legislature (the contest over which in ND directly preceded a stricter ID law) (Reilly, 2018).

In 2013 ND passed one of the strictest voter identification laws in the country. Although unlike select other states the law does not require one to present an ID with a photo, it does include the requirement that one's ID list a physical address. Native Americans make up the largest minority group in ND, and the physical address requirement has unique implications for this group. A substantial portion of Native Americans live on reservations, which are vast and remote, so much so that the postal service does not provide residential delivery. As such, identification issued by tribal governments often includes a post office box instead of a physical address. While previous scholarly research has examined differential rates of access to a valid ID among whites, Blacks, Latinos and Asian Americans, very little work has examined the consequences of these laws for Native Americans.

Thus, an inquiry into the impact of North Dakota's voter identification law yields important scholarly insight across a variety of dimensions. First, it highlights an oft-overlooked dimension in the literature around voter identification laws-changes to election administration rules designed to deter participation are often tailored to the specific populations of the state. In the case of ND, the law echos residency requirements employed to disenfranchise Native Americans under Jim Crow, insofar as the physical address requirement raises additional burdens for Native Americans that turn on whether one lives on a reservation. Most often research examining the consequences of voter identification laws use one-size-fits-all measures of identification, or examines pre-post turnout at the state level, looking for national consequences. More research is needed around the impact of election administration on important population subgroups, with competency around parochial contexts, and attention to the unique barriers that racial and ethnic marginalization produces for local populations. That is, scholars should not simply query whether an individual has a state-issued ID, but should instead pay attention to how the specific requirements for an acceptable ID map on to the lived experiences of the minoritized populations in a given state. Second, this research offers important insight around Native Americans, about whom we know very little, politically, but who comprise a not-insignificant portion of the population in states where elections are becoming more competitive because of changing demographics.

In order to address these issues, we collected two unique datasets in ND, employing culturally competent survey methods to ensure a representative and robust sample. In addition to a high-quality sample, we also tailored our instrument to the specifics of the law, and asked additional follow up questions about the challenges one might face should they try to obtain an ID. The result is a dataset that is both rich and broad, allowing us to provide detailed contextual information about the barrier to voting North Dakota's law poses for Native Americans, relative to their non-Native counterparts. We find that Native Americans are about seven percentage points less likely to have access to a valid ID than are non-Native respondents, a relationship that is statistically significant, and replicates across both datasets after the inclusion of socio-economic co-variates. They are likewise more likely to lack access to all the documents needed to obtain a piece of government-issued identification.

Our analysis is not without limitations. Despite our best efforts to generate a robust and representative sample of Native Americans in ND, it is modest enough to make subgroup analysis difficult. We do not delineate between those who do and do not live on a reservation; it may be that the impact of these laws are negligible for those who do not live on a reservation. We cannot offer an evaluation of Native Americans by income, gender, or age, all three important factors attenuating the effect of ID laws. Finally, our surveys lack some key measures important to ascertaining the extent effects of election administration procedures, including political factors

like interest, attention to news, and past voting behavior (Vercellotti and Anderson, 2006; Mycoff *et al.*, 2009; Barreto *et al.*, 2019). While we center questions of access to an ID, questions about turnout which we are unable to address naturally follow. Issues related to sampling and turnout thus turn attention toward administrative records, of the sort employed by Fraga and Miller (Forthcoming), Henninger *et al.* (2021), and Grimmer and Yoder (2021). For all these reasons, our intervention is a starting point for thinking about the unique effects of election administration on highly contextualized minoritized populations.

The differences we do observe between Native and non-Native eligible voters in ND are due, in no small part, the requirement that one's ID must include a physical address, which presents a unique challenge for Native Americans, many of whom live in the remoteness of the reservation. ND has at present modified its law to ensure Native Americans without an address on their ID can have their voices heard. This change is a consequence of a lengthy and costly court battle spanning two election cycles. It therefore behooves those concerned with questions of normative democracy to heed the call for continuing research on the specific racial and local nature of election administration raised by this piece, which operate, "within the subtext of racial power to reproduce the inequalities that demand the attention of political scientists in the first place" (Barreto *et al.*, 2019, 3).

Supplementary material. The supplementary material for this article can be found at https://doi.org/10. 1017/rep.2022.1.

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Notes

1 Estimates come from the American Community Survey for 2016, table DP03 for ND.

2 Estimates come from the American Community Survey for 2016, table S1501 for ND.

3 Because so few non-Native respondents completed the survey by mail, and because we leveraged mail outreach to ensure we had a representative sample of Native Americans, we do not include a control for mode in our models. Modeling Native Americans alone and including a control for mode indicates that mode is not statistically associated with whether one has an appropriate ID. A table of responses by mode can be found in the appendix.

4 Copies of both the phone and mail instruments used in 2015 are located in the appendix. The wording in the 2017 survey was substantially similar.

5 It may be that some individuals possessed an ID that was expired or lacked a birthdate. The legal case itself did not hinge on whether one's ID was expired, and it was suggested that election officials would not enforce the provision that one's ID be unexpired. We therefore coded individuals as having an appropriate ID, even if it was expired. Respondents were not queried as to whether their ID listed a date of birth. It is also important to note that the data collected in 2015 were coded at the time to reflect the law as written in 2013. Here, we code the data in-line with the litigation of the 2016 version of the law, since our aim is to bolster our analysis through replication across two datasets. That is, we seek to determine whether there is a gap in access to an ID that persists across two different samples. We therefore present findings using the 2015 data that may differ from those presented in court documents in the initial filing.

6 For context, Barreto *et al.* (2019)'s pooled data suggests that 12.9% of individuals lack an appropriate piece of identification. A very rough estimate drawing on public estimates of the number of individuals with a license in ND suggests that 95% of individuals have a drivers license. However, this estimate is very rough, derived from the estimated population over the age of 18 and highway patrol information estimating the number of license holders. This estimate therefore does not include individuals who may have

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had their licenses lost or revoked and does include individuals over the age of 16. Thus, it is likely an overestimate. Data points come from https://www.fhwa.dot.gov/policyinformation/statistics/2016/dl201.cfm and https://www.census.gov/quickfacts/ND.

7 For this same reason, we do not make much of differences between 2015 and 2017. Instead, we present two samples at two different points in time to assess the robustness of the gap between Native and non-Native Americans in ND. We do not and cannot make claims based on our data about change over time.

8 There are myriad ways confusing election laws can introduce barriers to the ballot box. One not directly related to whether one lives on a reservation has to do with knowledge of and confusion around provisions in the law, which Henninger *et al.* (2021) highlight is a particular issue in states with non-strict laws. In our own data, we observe that in both years Native Americans were less likely to know that the law required that they have an ID than were non-Native respondents. In 2017, they were more likely to mistakenly believe that they had an ID that met the criteria set out by the law. A table of these findings is located in the appendix.

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Exhibit 14

UNITED STATES DISTRICT COURT WESTERN DISTRICT OF WISCONSIN

THE ANDREW GOODMAN FOUNDATION,

Plaintiff,

v.

MARGE BOSTELMANN, JULIE M. GLANCEY, ANN S. JACOBS, DEAN KNUDSON, ROBERT F. SPINDELL, JR., and MARK L. THOMSEN, in their official capacities as Wisconsin Elections Commissioners,

Defendants.

Civil Action No. 19-cv-955

January 20, 2020

EXPERT REPORT: BURDENS OF STUDENT ID RESTRICTIONS

Barry C. Burden

Barny Benden

Barry C. Burden

Introduction

Wisconsin Act 23, enacted in 2011, implemented a strict voter ID requirement in Wisconsin that prohibits use of college and university IDs unless they meet unique specifications and are accompanied by proof of enrollment.¹ I was asked to offer a preliminary opinion on the purpose and effect of the student identification requirements on young voters in Wisconsin. I am being compensated at a rate of \$300 per hour.

It is my opinion that the provisions related to college student IDs in Act 23 are tenuous in their ability to meet state interests and are burdensome on young adults in Wisconsin who wish to vote. The provisions do not enhance election security or public confidence in the election system. Further, by demanding more of college student IDs than other forms of identification, the provisions place a disproportionate burden on young adults in Wisconsin who wish to vote. These burdens are more consequential for young people because they have less familiarity with voting requirements and are in the process of establishing nascent habits of voter participation that are more easily disrupted. In addition, many standard-issue college IDs in Wisconsin do not comply with the unusual requirements in the law, thus necessitating a second ID that requires additional time and resources to obtain. As a result, college students and other young adults will be hindered and deterred from voting by the poorly justified ID requirements in Act 23.

Qualifications and Basis of Opinion

I am a Professor of Political Science at the University of Wisconsin-Madison, where I have taught since Fall 2006. I earned my Ph.D. at The Ohio State University in 1998. From 1999 to 2006, I was a faculty member in the Department of Government at Harvard University.

My expertise lies generally in American politics with a focus on elections and voting, public opinion, representation, political parties, and research methodology. I teach courses on these topics at both the undergraduate and graduate levels. I am the author of *Personal Roots of Representation* (Princeton University Press), co-author of *Why Americans Split Their Tickets* (University of Michigan Press), and co-editor of *The Measure of American Elections* (Cambridge University Press). I have also published articles in respected scholarly peer-reviewed journals such as the *American Political Science Review*, *American Journal of Political Science*, *Electoral Studies*, *Public Opinion Quarterly*, *Legislative Studies Quarterly*, *Public Administration Review*, *Election Law Journal*. I am a member of the American Political Science Association and have been active in the profession, giving presentations at many conferences and universities.

I have particular expertise in elections and election administration. I am Director of the Elections Research Center at the University of Wisconsin-Madison, a nonpartisan institute that provides rigorous analysis of elections. One of the Center's key areas of focus is election

¹ In the remainder of my report I will use the term "college" to refer to both colleges and universities. My use of the term "student" will refer to students enrolled in colleges and universities within the state of Wisconsin. High school IDs are not acceptable for voting under Act 23 even if a high school student is eligible to vote.

administration. I have testified before state officials and the bipartisan Presidential Commission on Election Administration and provided expert advice to the U.S. Government Accountability Office. I serve on the advisory board of the MIT Election Data and Science Lab. I am frequently contacted by journalists and civic organizations to speak about election administration and have been quoted in several national media outlets.

I have provided expert reports and testified in several federal and state cases concerning changes in election law. This includes testimony in two prior Wisconsin cases involving Act 23. The case of *League of United Latin American Citizens of Wisconsin et al. v. Judge David G. Deininger et al.* (2013) in the Eastern District of Wisconsin concerned whether the voter law violated the federal Voting Rights Act. The case of *One Wisconsin Institute Incorporated et al. v. Judge Gerald V. Nichol et al.* (2016) in the Western District of Wisconsin concerned in part whether the voter ID law violated the Voting Rights Act and the U.S. Constitution. A full list of cases in which I have provided expert testimony as well as more information about my academic and scholarly experience is included in my curriculum vitae, included as Appendix A.

As a faculty member at the University of Wisconsin-Madison, I also have firsthand knowledge of how election laws and practices affect college students in Wisconsin. During the 2018 election season, I was the faculty lead of the campus coalition supporting the Big Ten Voting Challenge, a friendly competition within the Big Ten Conference to encourage student voters in the midterm elections. My involvement with the group allowed me to learn from local election officials, campus staff, and students themselves about the impacts of the voter ID requirements contained in Act 23.

To establish an expert opinion in this case, I reviewed an array of materials from academic, governmental, legal, and media sources. Building on my existing knowledge, expertise, and experience, I consulted scholarly research on the general causes and effects of changes in state election laws. My review also included data sources and statutes made available by agencies of the Wisconsin and federal governments. I also drew on direct knowledge of the state election system and observations of students attempting to vote in Wisconsin. All of the sources and methodologies I used are standard in my field.

Overview of 2011 Act 23

In 2011, the Wisconsin state legislature passed and Governor Scott Walker signed into law Act 23. Act 23 created a strict voter ID requirement for voting in all Wisconsin elections, whether at a traditional polling place or by absentee. The law requires a prospective voter to provide one of the following forms of ID to receive a ballot:

- Wisconsin driver's license,
- Wisconsin non-driver ID issued by the Department of Transportation,
- U.S. Passport,
- military ID card,
- certificate of naturalization,
- federally-recognized Indian tribe ID,

- Veteran's Health Administration ID card,² or
- ID from an accredited college located within the state.

Act 23 singles out college student IDs in four specific ways. First, a student ID must contain an expiration date.³ Second, a student ID must also include a date of issuance. Third, the listed date of expiration on a student ID must be no later than two years after the date of issuance. Fourth, a voter using a student ID must also provide proof of current enrollment such as a tuition fee receipt or a letter from the college attesting to enrollment status.

No other form of ID under Act 23 must be accompanied by these additional requirements, yet the legislature and governor nonetheless deemed those non-student forms of ID to be satisfactory for establishing a voter's identity.

Consider how the requirements are applied inconsistently in practice.

- For voting purposes, being a current military service member is akin to being a currently enrolled student. In both cases, a person is issued an ID when they join the organization. One would expect proof of current membership or enrollment to be required in either both cases or neither case. But this is not the practice in Wisconsin. A retired military member may use a card issued by a uniformed service as an acceptable form of ID even when the person is no longer actively serving.⁴ Even the ex-spouse or former dependent of a military service member may continue to use a military ID for voting purposes after the legal relationship between the service member and card holder has been severed (as in the case of a divorce or a dependent turning 18 years old).
- Proof of enrollment makes the required issuance and expiration dates unnecessary. Nonetheless, all three things are required of student IDs. This court's findings of fact in *One Wisconsin Institute* has already found that the "three requirements…are redundant,"⁵ and that requiring student IDs to be unexpired is a violation of the Fourteenth Amendment (though the court made no finding as to the requirement that the expiration date be listed at all or that the specific requirements at issue here should be removed or otherwise changed).

² VA cards were technically established as acceptable forms of ID on March 16, 2016 by Wisconsin Act 26.

On the distinction between two types of Veteran Affairs cards, see https://elections.wi.gov/sites/elections.wi.gov/files/news/137/veterans ids for voting pdf 12338.pdf (last visited December 13, 2019). The law also allows an individual to receive a ballot who was approved for a driver's license or state ID card in close proximity to the election but has not yet received it. In these cases, use of a driver's license receipt or state ID card receipt is accepted for a limited time period. As a result of the decision of this court in One Wisconsin Institute, a person may also for a limited time period use a temporary ID card issued by the DOT as part of the identification petition process (IDPP).

⁴ The forms of military ID provided by the Department of Defense are listed here: https://www.cac.mil/uniformed-services-id-card/ (last visited December 9, 2019).

⁵ One Wisconsin Inst., Inc. v. Thomsen, 198 F. Supp. 3d 896, 961 (W.D. Wis. 2016), order enforced, 351 F. Supp. 3d 1160 (W.D. Wis. 2019).

- Although a Wisconsin driver's license includes a date of issuance, this is not required by Act 23.
- Generally speaking, an acceptable ID under Act 23 must either be unexpired or expire after the date of the most recent general election, but there are exceptions to the expiration date requirement. For example, tribal IDs that lack expiration dates, naturalization certificates, which do not include expiration dates, and Veteran ID Cards (VICs), which do not include expiration dates are all exempted from this requirement. Because Act 23 explicitly permits use of IDs without expiration dates, the law makes evident that printed expiration dates are not necessary to establish a voter's identity. Nevertheless, without sound justification, it requires that student IDs contain expiration dates.

The additional demands layered on voters using student IDs place heavier burdens on young adults with no apparent benefit to the state. If the name and photo on an ID are sufficient for an election worker to confirm the identity of a voter, then the issuance date and proof of enrollment are superfluous requirements. As noted in the complaint filed in this case, it is possible under Act 23 for a poll worker to confirm that the name and photograph on a student ID match that of a prospective voter but nonetheless prohibit the person from acquiring a ballot due to lack of an issuance date or proof of enrollment. Voters using other forms of ID are permitted to receive ballots without these requirements. As I describe in the following section, social science research on the factors that affect voter turnout demonstrates that young adults and college students face more challenges than do other potential voters, and the additional requirements on college IDs serve to exacerbate those disparities.

Theoretical Frameworks for Understanding the Effects of Act 23 on College Students

Experts on voter participation use two frameworks to understand why some people vote in an election and others do not. The two frameworks are (a) the theory of the *calculus of voting*, and (b) research on voting as a *habit*. These theories are relevant because they identify the burdens on voting that the state may impose and how those burdens may inhibit the practice of voting.

The "calculus of voting" is the dominant theoretical framework used by scholars to study voter turnout. Under this theory, researchers conceptualize the likelihood of voting as a formula that involves benefits and costs. An individual will generally vote if the *probability* of their vote determining the outcome (P) multiplied by the net psychological *benefit* of seeing one's preferred candidate win the election (B) is greater than the *costs* of voting (C). The theory does not necessarily imply that these parameters are the only factors influencing voter turnout, but rather are important variables that can explain why turnout varies between elections, across groups, or even by an individual in different contexts.

The cost term, C, is the most relevant parameter for understanding the student voter ID requirement in Act 23. This is because C is the only term that the state can manipulate directly, by setting election rules that affect the costs of voting. The costs include the effort required to become informed about the candidates and issues over which the state has only limited influence. But costs also include the time, resources, and activity needed to overcome the administrative requirements

and other barriers to registering to vote and successfully casting a ballot.⁶ The state has almost exclusive control over these costs. Raising the value of C increases the burden on potential voters.

Any voting process incurs some costs; people use *resources* available to them to pay these costs. The "calculus of voting" framework suggests that, for many individuals, small changes in benefits or costs may alter the likelihood of voting dramatically. The decision to vote is sensitive enough to costs that even election day weather has been shown to depress turnout.⁷ It is little surprise, then, that adding more costs to the voting process is enough to deter voting.⁸ For example, relocating polling places has been shown to decrease turnout by several percentage points.⁹ Implementing new registration requirements also reduces turnout.¹⁰

The second framework I use to understand the effects of Act 23 on young people views voter participation as a *habit*. Like many other repetitive behaviors that people adopt and sustain, voter turnout may be understood as habitual. Having paid the costs to participate in a first election, a person then becomes more likely to participate at later opportunities, thus contributing to the establishment of a persistent habit. After a person becomes a voter, they tend to remain a regular voter, at least in major general elections.¹¹

What may appear to be equal costs imposed by a restriction on voting practices are in fact often more acute for young people. The college student ID requirements in Act 23 place unique and additional burdens on students at a time in their lives when the voting process is most

⁶ Some formulations add a "duty" term to indicate the positive effect of norms supporting the democratic system. This addition might not be necessary because the cost term can be viewed as the net costs that encompass one's sense of duty. *See* John H. Aldrich (1993), "Rational Choice and Turnout," *American Journal of Political Science* 37:246-78. Alternatively, it has been suggested that costs matter more for individuals with a low sense of duty. *See* André Blais, Robert Young, and Miriam Lapp (2000), "The Calculus of Voting," *European Journal of Political Research* 37:181-201.

⁷ Thomas G. Hansford and Brad T. Gomez (2010), "Estimating the Electoral Effects of Voter Turnout," *American Political Science Review* 104:268-88.

⁸ Henry E. Brady and John E. McNulty (2011), "Turnout Out to Vote: The Costs of Finding and Getting to the Polling Place," *American Political Science Review* 105:115-34. John E. McNulty, Conor M. Dowling, and Margaret H. Ariotti (2009), "Driving Saints to Sin: How Increasing the Difficulty of Voting Dissuades Even the Most Motivated Voters," *Political Analysis* 17:435-55. Moshe Haspel and H. Gibbs Knotts (2005), "Location, Location, Precinct Placement and the Costs of Voting," *Journal of Politics* 67:560-73.

⁹ Brady and McNulty (2011). McNulty, Dowling, and Ariotti (2009). Hapsel and Knotts (2005).

¹⁰ Barry C. Burden and Jacob R. Neiheisel (2013), "Election Administration and the Pure Effect of Voter Registration on Turnout," *Political Research Quarterly* 66:77-90.

¹¹ Alan S. Gerber, Donald P. Green, and Ron Shachar (2003), "Voting May Be Habit-Forming: Evidence from a Randomized Field Experiment," *American Journal of Political Science* 47:540-50. Eric Plutzer (2002), "Becoming a Habitual Voter: Inertia, Resources, and Growth in Young Adulthood," *American Political Science Review* 96:41-56. Alexander Coppock and Donald P. Green (2016), "Is Voting Habit Forming? New Evidence from Experiments and Regression Discontinuities," *American Journal of Political Science* 60:1044-1062.
challenging. For young people who are in the midst of establishing a voting habit, costs loom larger than for older individuals. This can have the effect of delaying the onset of the practice of voting.

Professor Eric Plutzer's research has demonstrated that voter turnout is affected by developmental stages over the course of the life cycle. It is worth quoting from his study at length:

As young citizens confront their first election, all of the costs of voting are magnified: they have never gone through the process of registration, may not know the location of their polling place, and may not have yet developed an understanding of party differences and key issues. Moreover, their peer group consists almost entirely of other nonvoters: their friends cannot assure them that voting has been easy, enjoyable, or satisfying. (p. 42)

A voting requirement imposed on young people is more consequential than a similar requirement imposed on older cohorts. The lack of experience with the voting process and lack of an established voting habit make the costs of meeting legal requirements to vote for the first time more challenging. When the requirements for young people go beyond the standards applied to other kinds of voters, as is the case with college IDs under Act 23, the costs will be even more difficult for voters to pay. The next section of the report analyzes the degree to which Wisconsin college students are likely to possess acceptable ID and how actions by colleges to offer compliant ID have affected student voter participation.

Student Possession of Compliant IDs

Approximately 300,000 students are enrolled in Wisconsin colleges. ¹² Data from the Wisconsin Department of Public Instruction indicate that approximately 59.4% of high school completers enroll in a college immediately after graduation and that 67.9% enroll at some point after graduating from high school. ¹³ Because such a large share of young adults are enrolled in postsecondary educational institutions, any voting requirements that affect college students in Wisconsin have immediate implications for young voters (i.e., voters ages approximately 18 to 29).

The circumstances that college students in Wisconsin encounter suggest that many will wish to use their student IDs for voting purposes. This is because young adults often lack other common forms of identification and because the student ID is often an essential form of identification that is routinely carried and frequently used for a variety of essential purposes. For many college students, especially those enrolled full-time on residential campuses, the student ID

https://nces.ed.gov/programs/digest/d18/tables/dt18_304.10.asp (last visited December 12, 2019). In contrast, the National Student Clearinghouse Research Center, which relies on spring semester enrollment numbers, estimates fall 2018 enrollment in Wisconsin of 289,086, *see*

¹² The National Center for Education Statistics, which relies on fall semester enrollment numbers, estimates fall 2017 enrollment in Wisconsin of 340,770, *see*

http://nscresearchcenter.org/currenttermenrollmentestimate-spring2018/ (last visited December 12, 2019).

¹³ Wisconsin Information System for Education Data Dashboard (WISEdash), available at https://wisedash.dpi.wi.gov/Dashboard/portalHome.jsp (last visited December 12, 2019).

is a daily necessity. It provides access to classrooms, their dormitories, libraries, recreational facilities, parking structures, transportation, and laboratories while also serving as a financial tool that may be used to make purchases at dining facilities and book stores. Indeed, the fact that student IDs are explicitly named in Act 23 indicates that the legislature and governor who enacted the law believed that they would be the primary form of identification for many voters.

Despite the value of such IDs to students, many standard-issue student IDs at Wisconsin colleges did not conform to the requirements of Act 23 when the law was enacted. This required colleges to issue secondary forms of ID if they wanted their students to be able to participate in the voting process, requires students to acquire other forms of acceptable ID, or both.

Because many students lack other forms of acceptable ID, they would be likely to rely on their college IDs to vote. Research by professor Kenneth Mayer prepared for the *One Wisconsin Institute* case provides an estimate of how many Wisconsin college students who are registered to vote lack a Wisconsin driver's license or state ID card.¹⁴ Mayer linked records in the statewide voter registration database with information on cardholders from the Department of Transportation as of late 2014. That analysis showed that about 8% of registrants lacked a license or state ID card. Although the files did not identify conclusively who was a college student, limiting the analysis to "student wards" (defined as those with colleges nearby or with large concentrations of registrants who are 18 to 24 years old) revealed a non-possession rate of 21%.

Based on what scholars have learned about the demographic differences between registrants and non-registrants, this can be regarded as a lower bound estimate. For example, U.S. Census Bureau data show that people who are unregistered have lower incomes, have lower levels of formal education, are more likely to be unemployed, and are more likely to have disabilities.¹⁵ If it had been possible to examine the full electorate in Mayer's analysis, including eligible voters who are not yet registered, it would have almost certainly produced an even higher non-possession rate.

Students and other young people who lack Wisconsin driver's licenses and state IDs are also not likely to possess other kinds of compliant IDs such as U.S. Passports, veteran IDs, and military IDs. Support for this assertion comes from an expert report by professor Stephen Ansolabehere in the case of *Marc Veasey et al. v. Rick Perry et al.* concerning a strict voter ID law implemented in Texas. ¹⁶ In research similar to Mayer's analysis in the Wisconsin case, Ansolabehere linked records in the statewide voter file to information on license and state ID holders maintained by the Department of Motor Vehicles to determine which registrants lacked

¹⁴ Expert report dated December 10, 2015 of Kenneth R. Mayer in the case of *One Wisconsin Institute*, *Inc. et al. v Judge Gerald C. Nichol et al.*

¹⁵ For example, see the Census Voting and Registration Supplement tables for the 2016 election available at https://www.census.gov/data/tables/time-series/demo/voting-and-registration/p20-580.html (last visited December 18, 2019).

¹⁶ Corrected Supplemental Report dated September 16, 2014 of Stephen D. Ansolabehere, *Marc Veasey et al. v. Rick Perry et al.*, Southern District of Texas (No. 2:13-cv-193). See also Stephen Ansolabehere and Eitan D. Hersh (2017), "ADGN: An Algorithm for Record Linkage Using Address, Date of Birth, Gender, and Name," *Statistics and Public Policy* 4:1-10.

acceptable ID for voting. However, Ansolabehere was also able to include data from federal agencies such as the Department of Defense (to capture military IDs), the Department of State (to capture U.S. Passports), and the Department of Veterans Affairs (to capture veteran IDs). Adding these sources to the record linkage process demonstrated that many individuals lack *any* of the non-student forms of ID. Depending on the exact methodology used, matching on the state-issued forms of ID alone produced a non-possession rate of 6 to 7% (just below Mayer's estimate in Wisconsin). ¹⁷ Adding federal IDs reduced the non-possession somewhat to 4 to 4.5% (again depending on the methodology). Although Ansolabehere did not disaggregate the results by age, his report showed that non-Hispanic whites are much more likely than blacks and Hispanics to possess compliant IDs. Because blacks and Hispanics (in both Texas and the country overall) are substantially younger on average than are non-Hispanic whites, it must be the case that state and federal ID possession in Texas is lower among young people than in the general population. I have no reason to believe the age disparity is absent in Wisconsin.

College Student Voter Turnout in Wisconsin Under Act 23

To understand how the reliance on college student IDs affects young adults who wish to vote in Wisconsin, it is helpful to consider rates of voter turnout among students who attend college in the state. Information on student voter turnout is made available through the National Study of Learning, Voting, and Engagement (NSLVE). NSLVE is a project of the Institution for Democracy and High Education (IDHE) at Tufts University. The project covers more than 10 million students enrolled at over 1,000 institutions of higher education. Universities participate in the service by providing NSLVE staff with enrollment records for their students. Those records are matched against a national database of official voting data from the states to determine the voter turnout rate at each participating campus. Rates are adjusted to reflect the degree to which students are known to be noncitizens who are ineligible to vote in federal elections, but the inability to identify the total number of enrolled noncitizens means that estimated student turnout rates are slightly deflated. Each participating campus has received a report on its students' voting participation in general elections starting with the 2012 presidential election.

The NSLVE data show how student voter turnout rates changed in Wisconsin between elections before and after 2015, when the student ID requirements in Act 23 went into continuous effect. ¹⁸ As a reference point, the national data show that overall student turnout among participating institutions across the country rose by 3.2 percentage points between the 2012 and 2016 elections.¹⁹ According to the NSLVE reports, 743 out of 973 participating institutions (76% of the total) saw increases in student turnout between 2012 and 2016. In contrast, all sixteen of the

¹⁷ See Tables V.3.A through V.4.B of the Ansolabehere report.

¹⁸ Recall that the law was briefly in effect in February 2012 but was then put on hold by court action. It has been in effect continuously since April 2015.

¹⁹ "Democracy Counts: A Report on U.S. College and University Student Voting," https://idhe.tufts.edu/sites/default/files/NSLVE%20Report%202012-2016-092117%5B3%5D.pdf (last visited December 9, 2019).

Wisconsin institutions had changes in turnout that were below the national median, and thirteen of the sixteen colleges saw outright decreases.²⁰

All of the publicly available NSLVE reports for individual colleges that I was able to find online show a drop in Wisconsin student turnout. Student turnout at UW-Madison dropped by 4.3 percentage points.²¹ UW-Superior fell by 6.6 points.²² UW-Parkside fell by 4.1 points.²³ UW-Whitewater fell by 9.4 points.²⁴ Private colleges around the state also saw declines. Turnout among students at St. Norbert College fell by 8.0 points.²⁵ Madison Area Technical College fell by 3.9 points.²⁶ Edgewood College dropped 7.3 points.²⁷

It would be a mistake to attribute all of the turnout decline in Wisconsin in 2016 to the suppressive effect of the voter ID requirement in Act 23. Some of the decline, both in the state overall and among students, is due to the lighter presences of the presidential campaigns of Hillary Clinton and Donald Trump compared to prior Democratic and Republican nominees. Clinton has been criticized for not appearing in the state, the first major party nominee to skip Wisconsin since 1972.²⁸ Her campaign also purchased much less advertising in Wisconsin than is typical. Both Clinton and Trump invested less in the "ground game," opening fewer field offices around the state than Barack Obama and Mitt Romney had done in 2012.²⁹ Research shows that engagement by campaigns helps to stimulate voter participation.³⁰

²³ https://www.uwp.edu/learn/colleges/socialsciencesprofessionalstudies/upload/2012-and-2016-NSLVE-Report-University-of-Wisconsin-Parkside.pdf (last visited December 9, 2019).

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https://www.uww.edu/Documents/ir/Compliance%20and%20Reporting/External/2012%20and%202016 %20NSLVE%20Report-University%20of%20Wisconsin-Whitewater.pdf (last visited December 9, 2019).

²⁵ http://www.allinchallenge.org/wp-content/uploads/St.-Norbert-College-NSLVE-2016.pdf (last visited December 9, 2019).

²⁶ http://www.allinchallenge.org/wp-content/uploads/Madison-Area-Technical-College-NSLVE-2016.pdf (last visited December 9, 2019).

²⁷ http://www.allinchallenge.org/wp-content/uploads/Edgewood-College-NSLVE-2016.pdf (last visited December 9, 2019).

²⁸ Barry C. Burden and Evan Crawford, "Hillary Clinton's Absence in Wisconsin Reflects Her Strength and One Weakness," *Wisconsin State Journal*, October 29, 2016.

²⁹ Joshua P. Darr (forthcoming), "Abandoning the Ground Game? Field Organization in the 2016 Election," *Presidential Studies Quarterly*.

³⁰ See Robert A. Jackson (2008), "Macro Research on Campaign Mobilization in the United States," *Journal of Political Marketing* 2:25-45.

²⁰ *Id.* at 14.

²¹ https://www.allinchallenge.org/wp-content/uploads/University-of-Wisconsin-Madison-NSLVE-2016.pdf (last visited December 9, 2019).

²² http://www.allinchallenge.org/wp-content/uploads/University-of-Wisconsin-Superior-NSLVE-2016.pdf (last visited December 9, 2019).

Although it is not surprising in retrospect that voter turnout declined in Wisconsin between 2012 and 2016, it is noteworthy that student turnout fell more sharply than among the electorate as a whole. Statewide turnout fell from 72.9% of eligible voters to 69.5%, a decline of 3.4 points.³¹ All of the Wisconsin college turnout rates documented above dropped by a larger amount.

Even if less robust presidential campaigns in 2016 contributed to the decline in student turnout compared to 2012, campaign activity alone cannot explain the pattern in the midterm election of 2018. Nationwide turnout among all eligible voters in 2018 was 50.3%, the highest rate in a midterm election in more than a century and the highest rate since constitutional amendments provided for the direct election of U.S. Senators and women's suffrage. ³² This represents a remarkable increase of 19.4 percentage points over the previous midterm election in 2014.³³ NSLVE data coincidentally show the same 19.4-point increase in student voting at participating institutions. But colleges in Wisconsin saw smaller increases than in most other states. While turnout was up at all but a handful of the campuses participating in NSLVE, the rise in Wisconsin institutions between 2014 and 2018 ranked 37th out of 42 states for which data were reported.³⁴

This makes two elections in a row (the 2016 presidential and the 2018 midterm) in which student turnout in Wisconsin underperformed in terms of movement relative to the rest of the country, other U.S. college students, and the Wisconsin electorate. Wisconsin students withdrew from voting more sharply in 2016 and took to voting less dramatically in 2018. Given the common pattern despite the great differences between these two elections (one favoring a Republican presidential candidate and the other favoring Democratic gubernatorial and other statewide candidates), I conclude the student voter ID requirement was one of multiple factors that suppressed student voter participation. This conclusion is supported in part by the following examination of the usability of student ID cards for voting in Wisconsin.

Usability of Student ID Cards for Voting

A key reason why young people have difficulty overcoming the costs of the voter ID law and, more specifically, the student ID requirements, is that only some of the ID cards provided to students enrolled in Wisconsin colleges are compliant with Act 23. According to recent research conducted by Common Cause Wisconsin, the standard ID cards issued to students are compliant

³¹ United States Election Project, http://www.electproject.org/home/voter-turnout/voter-turnout-data (last visited December 9, 2019).

³² United States Election Project.

³³ United States Election Project, http://www.electproject.org/national-1789-present (last visited December 9, 2019).

³⁴ "Democracy Counts 2018: Increased Student and Institutional Engagement," available at https://idhe.tufts.edu/sites/default/files/DemocracyCounts2018.pdf, at 9 (last visited December 10, 2019).

at only four of the thirteen four-year UW universities,³⁵ twelve of the fifteen technical colleges,³⁶ and eight of the 23 private colleges and universities.³⁷

Some of the compliant IDs were made so intentionally, as colleges attempted to mitigate the impact of Act 23 on their students. At other colleges, such as UW-Madison, a second ID was created and offered as a way to facilitate student voting without modifying the standard-issue ID to make it compliant. Students must typically make a separate trip to an ID office on campus to acquire the second form of ID.

Structural changes have taken place at the state's thirteen UW Colleges that require separate discussion. In 2017 the UW Board of Regents proposed a restructuring of the state university system that transformed the state's 13 two-year college campuses into branch campuses of proximate four-year UW universities. For example, Waukesha and Washington County colleges are now part of UW-Milwaukee. The reconfiguration of campuses was officially instituted on July 1, 2018, but implementation is ongoing. This restructuring has consequences for how and what kinds of IDs are issued to almost 9,000 students. It is unclear to me at this time whether an ID issued by a UW College that satisfies Act 23's requirements is still acceptable if the student holding it is now enrolled at a branch campus that has been renamed. It is possible that a student who received an ID less than two years ago must not only obtain a new school ID but also a new voting compliant ID from their new branch campus. In this evolving administrative environment, it is likely that some students will be confused or uncertain about which IDs are accepted for voting and how to acquire them. This environment will be especially challenging for lower resource students enrolled at the branch campuses that were previously part of the UW Colleges, which the UW System describe as being "critical higher education access points, particularly for first generation, low income, and under-served students."38

Setting aside the unique administrative complexities of IDs at the former UW Colleges, the general picture is that many students enrolled in Wisconsin colleges do not have driver's licenses or other non-student IDs acceptable for voting and are not issued standard student ID cards that are acceptable for voting. It is my understanding that most Wisconsin colleges make some form of acceptable ID available for students to acquire,³⁹ but that it is often provided separately from the regular ID. This approach to offering voting-compliant IDs has left many students unsatisfied. When it became clear that Act 23 was going into full effect, the College Democrats and College Republicans student groups at UW-Madison issued a joint statement in October 2015 calling on

³⁸ https://www.wisconsin.edu/uw-restructure/access/ (last visited January 7, 2020).

³⁹ It appears that at least two of the state's technical colleges do not offer their students any form of ID that is compliant with Act 23. See http://www.commoncause.org/wisconsin/wp-

content/uploads/sites/28/2019/08/Wisconsin-Tech-Colleges_Student-ID-Voter-ID-Compliance.pdf (last visited January 7, 2020).

³⁵ http://www.commoncause.org/wisconsin/wp-content/uploads/sites/28/2019/02/Univ-of-WI-4-Year-Colleges_Student-ID-Voter-ID-Compliance.pdf (last visited December 10, 2019).

³⁶ http://www.commoncause.org/wisconsin/wp-content/uploads/sites/28/2019/08/Wisconsin-Tech-Colleges_Student-ID-Voter-ID-Compliance.pdf (last visited December 10, 2019).

³⁷ http://www.commoncause.org/wisconsin/wp-content/uploads/sites/28/2019/02/Wisconsin-Private-Univ-Colleges_Student-ID-Voter-ID-Compliance.pdf (last visited December 10, 2019).

the administration to modify the existing Wiscard ID so that it would be acceptable for voting.⁴⁰ For a variety of security, administrative, and financial reasons, the university decided instead to issue a secondary card that could be used periodically for voting.⁴¹

Because of the extra administrative steps required to acquire the second ID, the rarity with which it is used compared to the standard ID, and lack of knowledge students may possess about what is needed to vote, many students are likely to be unpleasantly surprised when they attempt to vote but lack an acceptable ID that includes a signature and appropriate issuance and expiration dates.

The insufficiency of a secondary voting-compliant student ID can be demonstrated by what happened when UW-Madison attempted to mitigate the costs of Act 23 for students in the 2016 and 2018 elections. In addition to the plastic cards available at the regular Wiscard student ID office in one of the student unions, the university placed computers and paper printers operated by staff members at seven on-campus voting sites. The service was provided to generate temporary paper IDs for students who arrived at the polls and discovered that they lacked ID for voting. Staff members at each location were able to print the paper IDs for students who provided adequate documentation of their identities.

A total of 989 IDs were issued on election day in 2016 and 1,189 were issued on election day in 2018.⁴² Only 345 and 322 of these were plastic cards issued in the Wiscard office. The vast majority were thus issued at voting sites on demand as surprised students realized what was needed for them to receive ballots. Without the printing service, it is unclear if students would have learned about the possibility of acquiring the plastic ID at the Wiscard office. If they did know about the opportunity, it is not likely that all of them would have had the interest, time, and ability to travel to what would have been an extremely busy office and return to the polling place before voting ended at 8:00 p.m.

Because the number of IDs printed on election day actually increased between 2016 and 2018, it appears that students' ID needs have not been satisfied over time through university or other efforts. To put the need in perspective, the NSLVE report states that 16,806 UW-Madison students voted in the 2016 election. The 989 students who were issued voting-compliant IDs on election day represent one out of every 17 students who voted. In the 2018 election, the 1,189 IDs issued on election day amount to one of every 16 of the 18,470 students who voted. Without the unnecessary Act 23 requirements for a signature, issuance date, and expiration date to appear on the ID, these and other students could have simply used their standard campus Wiscards that they carry on a regular basis for many other commercial, academic, and personal purposes. Based on a variety of indicators, I expect UW-Madison student turnout in the 2020 election to exceed the levels it reached in 2016 and 2018 and for an even larger number of election day IDs to be issued.

⁴⁰ https://web.archive.org/web/20151026120815/http://collegedemsuwmadison.org/?p=765 (last visited December 18, 2019).

⁴¹ https://chancellor.wisc.edu/blog/campus-support-for-student-voting-under-new-wisconsin-voter-id-law/ (last visited December 18, 2019).

⁴² Big Ten Voting Challenge Recap Memo to Chancellor Blank from Barry Burden and Megan Miller, January 18, 2019.

Qualitative Evidence of the Effects of the Student ID Restrictions

While the constraints of this preliminary report do not permit a full causal analysis of all the factors that led to the disproportionate drop in student turnout, my knowledge of academic research, understanding of the situation faced by Wisconsin college students, and firsthand observations and interactions with student voters, poll workers, and university partners indicates that students were deterred by the voter ID requirements in Act 23.

Students enrolled in one of my courses during the fall 2016 semester were required to observe Wisconsin polling places one election day and write reports on the functioning of the administrative process they observed. In another course I taught that semester, many students voluntarily served as poll workers at polling locations on or near the UW-Madison. Both groups of students reported cases of students being turned away for lack of acceptable ID. Other media coverage highlighted similar instances of students lacking ID at the polls.⁴³ In my subsequent work as faculty lead of the Big Ten Voting Challenge, I heard from multiple stakeholders about student voters who lacked IDs and knowledge about how to acquire them.

More systematic evidence on the effects of Act 23 on young voters in 2018 is reported by the Wisconsin chapter of the League of Women Voters. As it has done in several recent elections, the League dispatched its volunteers as observers at a large number of different kinds of polling places around the state on election day in 2018. Their firsthand observations about the administration of the election were summarized in a report.⁴⁴ Their observers reported that 58 people were not able to vote because they lacked approved forms of ID and that "[s]ites with student populations were more than twice as likely to report voters having difficulty producing a photo ID than sites without such populations" (p. 19). Observers saw many instances where a voter without ID was not offered a provisional ballot as required by state law; this problem was more common in areas known to have large student populations (p. 21). In Madison, multiple voters mistakenly believed they could vote using the standard Wiscard ID issued by UW-Madison. Even more than three years after Act 23 was permitted to go into continuous effect, it is apparent that young voters continue to be challenged by the college ID requirements.

Lack of Public Knowledge about Act 23 is Likely to Affect College Student Turnout

Public awareness of photo ID requirements is essential so that aspiring voters can successfully navigate the law to cast ballots that will be counted. A lack of public information has been a concern in other states where photo ID laws have been implemented because it has apparently depressed voter turnout. Both courts and academic research have concluded that public uncertainty and misinformation can damage the implementation of voter ID requirements.

⁴³ Carrie Scherpelz, "Open Letter from a Poll Worker to the UW Student Who Tried to Vote Yesterday," *Isthmus*, February 17, 2016, available at https://isthmus.com/opinion/opinion/open-letter-from-a-poll-worker-to-uw-student/ (last visited December 18, 2019).

⁴⁴ "Wisconsin Election Protection 2018 Midterm Election Report,"

https://drive.google.com/file/d/1jMNNrkeG982rZ5VWE8cpZG0fjInfzFnc/view (last visited December 18, 2019).

In permanently blocking a photo ID law adopted in 2012, the Supreme Court of Pennsylvania cited in its opinion an expert witness who provided evidence about public knowledge of the law. A statewide survey analyzed by the expert showed that 38% of eligible voters did not know that the state had an ID requirement.⁴⁵ In addition, while 98% respondents believed that they had valid ID for voting, subsequent questions about specific forms of identification revealed that only 86% in fact did have acceptable forms of IDs.

In Wisconsin, a statewide survey conducted by Marquette University Law School was conducted following a highly publicized federal appeals court ruling enforcing the state's photo ID law.⁴⁶ The survey showed that 20% of registered voters erroneously believed that a photo ID would not be required to vote in the upcoming election.⁴⁷

A more recent study in Wisconsin by Michael DeCrescenzo and Kenneth Mayer also finds that misinformation or lack of information about the state's voter ID requirement deters voter participation.⁴⁸ Examining the state's two most populous counties, the authors estimated that 5.8% of nonvoting registrants who wished to vote were prevented from participating in the 2016 presidential election because they lacked acceptable ID or named the voter ID requirement as the *main* reason for not voting. Using a more expansive definition of who was impeded by the law, 10.2% were deterred from voting because they lacked acceptable ID or named the voter ID requirement as *one* of the reasons for not voting. In addition to the direct effect of Act 23 on the ability to vote, the authors found that much of the deterrence was an indirect effect due to misinformation. Upon detailed questioning of the respondents, the authors determined that only three percent of nonvoters actually lacked acceptable ID for voting. ⁴⁹ Less knowledgeable individuals were also more likely to report being deterred or prevented from voting by the ID requirement.

Research suggests that inaccurate information will be a significant reason for non-voting even among those who have acceptable ID. Problems of misinformation are more likely among youngpeople who are new to the voting process who tend to have lower levels of knowledge about the laws. This makes the additional burdens of enrollment verification, issuance dates, and expiration dates on student IDs more concerning. The complexity of the law and unique demands

⁴⁵ Expert report dated July 16, 2012 of Matt A. Barreto in the case of *Applewhite, et al. v. Commonwealth of Pennsylvania et al.* (No. 330 MD 2012). [Insert date of declaration? Insert Westlaw cite for case? (2012 WL 4497211)

⁴⁶ The decision was issued on September 12, 2014. The Marquette University Law Poll was in the field September 25-28. A subsequent order from the U.S. Supreme Court reinstated the injunction on October 9, 2014.

⁴⁷ The rate was a similar 18% for "likely" voters.

⁴⁸ Michael G. DeCrescenzo and Kenneth R. Mayer (forthcoming) *Election Law Journal*.

⁴⁹ These estimates of the percentage of people affected by the law are lower than the estimated percentages of the overall population that lacks acceptable ID cited earlier in my report. The DeCrescenzo and Mayer study focused on possession rates among registered nonvoters in two counties rather than the entire eligible population.

placed on student IDs, along with the patchwork of approaches being taken by the state's colleges, will contribute to misunderstanding and will inhibit some young adults in Wisconsin from taking part in the 2020 elections.

Effects of Act 23 on Election Security and Public Confidence in Elections

The most common rationales offered for a strict voter ID requirement are that it protects election security and increases public confidence in the election system. However, a wide variety of scholarly and legal evidence indicates that voter ID requirements target some of the rarest kinds of election crimes and have no overall effect on public confidence in the integrity of Wisconsin elections.

The main form of election crime that can be deterred by a strict voter ID law such as Act 23 is voter impersonation. However, this is one of the riskiest and less common forms of election crimes. It is risky to impersonate another voter because of the high likelihood of being caught and the severe penalties that may follow. Impersonating someone else at the polls must be done in a public polling place that is typically administered by poll workers representing both major political parties and in the presence of other voters and election observers. The impersonating voter also needs to cast the ballot in the name of a registered voter, either by registering (and thus showing proof of residence) multiple times or by identifying a legitimate registered voter who has not yet cast a ballot and is not personally known to the poll workers. This is a difficult crime to commit without detection. Moreover, the penalties for voter impersonation are substantial: up to five years in prison and a fine of \$10,000 in a federal election⁵⁰ and up to nine months in prison and a fine of \$10,000 in a state election.

A bipartisan study of election crimes by the U.S. Election Assistance Commission found that "[m]any [experts] asserted that impersonation of voters is probably the least frequent type of fraud because it is the most likely type of fraud to be discovered, there are stiff penalties associated with this type of fraud, and it is an inefficient method of influencing an election."⁵²

A report by researchers at the Ohio State University law school explored this issue in Wisconsin and four other Midwestern states. They interviewed state and local election officials and attorneys in the Milwaukee district attorney's office. Despite the fact that "[t]here are few states in which allegations of voter fraud have received greater scrutiny than Wisconsin...On the whole, voting fraud in exceedingly rare."⁵³ Their follow-up report on Wisconsin concluded that

https://www.eac.gov/sites/default/files/eac_assets/1/6/Initial_Review and Recommendations for Furthe r_Study.pdf, at 9 (last visited January 16, 2020).

⁵⁰ 52 U.S.C. § 10307(c).

⁵¹ Wisc. Stat. 12.60(1)(bm).

⁵² U.S. Election Assistance Commission. "Election Crimes: An Initial Review and Recommendations for Future Study." December 2006. Available at

⁵³ Steven H. Huefner, Daniel P. Tokaji, Edward B. Foley, and Nathan A. Cemenska (2007) *From Registration to Recounts: The Election Ecosystems of Five Midwestern States*, The Ohio State University Moritz College of Law, p. 120.

"[t]here is no evidence of any serious problem with voter impersonation fraud, the only form of illegal voting that a strict ID law could hope to address."⁵⁴

This conclusion is further supported by an exhaustive analysis of voter fraud allegations by the News21, an investigative reporting project based at Arizona State University. To tabulate instances of vote fraud, the organization made public records requests to election and law enforcement agencies, drew upon a list of vote fraud cases generated by the Republican National Lawyers Association, and culled other sources. They found 57 allegations of election fraud in Wisconsin between 2000 and 2013.⁵⁵ But only 45 of those cases implicated voters, as opposed to campaign staff or election officials. More importantly, none of the cases involved voter impersonation. Most of the cases involved voting by a person who was ineligible due to a felony record, double voting, and voter registration fraud. It does not appear that a voter ID requirement such as Act 23 would prevent any of these activities. More to the point in this litigation, none of the cases appear to involve a college student voting illegally as someone else.

A listing of voter fraud "cases" compiled by the Heritage Foundation generates similar results. ⁵⁶ The Heritage database lists 46 such "cases" in Wisconsin from 2004 to 2018. ⁵⁷ Its web site describes the information as a "sampling" of "proven" instances of vote fraud. Some of the "cases" actually involve multiple individuals accused of the same crime or broader actions such as overturning an election, so the total number of allegations may be higher than 46. Although the conservative ideological stance of the Heritage Foundation would incline it to find as many cases of election fraud as possible, the survey failed to find instances of voter impersonation in Wisconsin. However, in line with the News21 analysis discussed above, most of the cases involve people with felony convictions or non-citizens attempting to register or vote. None of the cases in the Heritage database appear to involve voter impersonation that would be inhibited by Act 23.

Depending on whether one accepts the accounting by News21 or by the Heritage Foundation, there is little trace of voter impersonation in a time period in which millions of votes were cast and counted in the general, primary, special, and other elections that took place in Wisconsin. This is a minuscule rate of voter impersonation crimes given the millions of ballots cast during this time period. The number of plausible voter impersonation attempts is also far, far smaller than the number of college students who lack acceptable ID (as estimated by Mayer in the *One Wisconsin Institute* litigation) and even the number of UW-Madison students who acquired last-minute IDs at the polls on election day in 2016 and 2018. The absence of any voter impersonation crimes or fraud perpetrated by Wisconsin college students in the periods before or after implementation of Act 23 reveals the tenuous nature of the ID requirements and particularly

⁵⁴ Steven F. Huefner, Nathan A. Cemenska, Daniel P. Tokaji, and Edward P. Foley (2011), *From Registration to Recounts Revisited: Developments in the Election Ecosystems of Five Midwestern States*, The Ohio State University Moritz College of Law, p. 41.

⁵⁵ See https://votingrights.news21.com/interactive/election-fraud-database/ (last visited December 16, 2019).

⁵⁶ The instances listed are not "cases" in the legal sense.

⁵⁷ The Heritage Foundation, "Voter Fraud Cases," https://www.heritage.org/voterfraud/search?state=WI (last visited December 16, 2019).

the extra demands placed on students. Voter impersonation is extremely rare and not more prevalent among students, yet they are obligated to show college IDs with additional elements not needed by other voters and to display proof of enrollment that has no analog among non-student voters.

Even if voter impersonation is extremely rare, the state might nonetheless justify Act 23 on the grounds that it helps bolster the confidence the public has in the election system. After all, public opinion surveys in Wisconsin generally show majority support for a generic voter ID requirement. ⁵⁸ However, the academic literature finds little to no evidence that voter ID laws buoy public confidence in election systems or motivate more citizens to participate as a result of public perception that the integrity of the voting system is improved. Extensive research instead finds that public confidence in the voting system is largely colored by partisanship, such that supporters of losing candidates have less trust in the election system than do supporters of winning candidates.⁵⁹ More to the point, research by professor Stephen Ansolabehere shows that there is no overall relationship between the strictness of state voter ID laws and voter confidence.⁶⁰ A more recent study suggests that a strict voter ID law might raise confidence among Republicans but lower it among Democrats.⁶¹

Ansolabehere's study cited in the previous paragraph concludes that an individual's "[b]elief in the frequency of election fraud is uncorrelated with propensity to vote" (p. 129). Related research that Ansolabehere conducted with law professor Nathaniel Persily similarly finds that "[t]here is little or no relationship between beliefs about the frequency of fraud and electoral participation (reported, validated, or intended). Nor does it appear to be the case that universal voter identification requirements will raise levels of trust in the electoral process."⁶² An even more recent study of public opinion and ID laws similarly concludes that "the presence of a photo ID requirement does not affect the public's belief in the frequency of voter fraud."⁶³ That study's

⁵⁸ For example, an October 2014 statewide Marquette University Law Poll found 60% of respondents in favor of a photo ID requirements and 36% opposed. See https://law.marquette.edu/poll/2014/10/29/final-pre-election-marquette-law-school-poll-finds-walker-leading-burke-in-wisconsin-governors-race/ (last visited December 17, 2019).

⁵⁹ Michael W. Sances and Charles Stewart III (2015), "Partisanship and Confidence in the Vote Count: Evidence from U.S. National Elections Since 2000," *Electoral Studies* 40:176-188.

⁶⁰ Stephen Ansolabehere (2009), "Effects of Identification Requirements on Voting: Evidence from the Experiences of Voters on Election Day," *PS: Political Science & Politics* 42:127-130. Shaun Bowler, Thomas Brunell, Todd Donovan, and Paul Gronke (2015), "Election Administration and Perceptions of Fair Elections," *Electoral Studies* 38:1-9.

⁶¹ Shaun Bowler and Todd Donovan (2016), "A Partisan Model of Electoral Reform: Voter Identification Laws and Confidence in State Elections," *State Politics and Policy Quarterly* 16:340-361.

⁶² Stephen Ansolabehere and Nathaniel Persily (2007-2008), "Vote Fraud in the Eye of the Beholder: The Role of Public Opinion in the Challenge to Voter Identification Requirements," *Harvard Law Review* 121:1737-1773 (p. 1759).

⁶³ Charles Stewart III, Stephen Ansolabehere, and Nathaniel Persily (2016), "Revisiting Public Opinion on Voter Identification and Voter Fraud in an Era of Increasing Partisan Polarization," *Stanford Law Review* 68:1455-1489 (p. 1483).

statistical analysis of national survey data also generally shows no effect of the strictness of a state's voter ID law on confidence that votes were counted correctly.

If the goal of Wisconsin's photo ID requirement and the more demanding requirements placed on student IDs is to improve public trust in the election system or increase voter turnout, it is highly likely to fail.

Conclusion

Based on my scholar expertise, review of relevant materials, and firsthand experience, I conclude in this preliminary expert report that the requirements for college student IDs contained in Act 23 are unjustified, redundant, and excessive compared to what is required of voters who use other forms of identification. The college ID provisions place a disproportionate burden on young adults in Wisconsin who wish to vote. The burden is especially problematic for students due to the costs of establishing a voting habit in young adulthood. Under Act 23, college students face a more difficult time voting than other voters and must overcome peculiar ID requirements that are not well designed to confront weaknesses in the state's election security or increase public confidence that Wisconsin elections are conducted properly.

Exhibit 15





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ARTICLE

Revisiting Public Opinion on Voter Identification and Voter Fraud in an Era of Increasing Partisan Polarization

Charles Stewart III, Stephen Ansolabehere & Nathaniel Persily*

Abstract. This Article updates previous findings concerning the relationship between voter identification laws and perceptions of voter fraud. Courts have established that voter identification laws can be justified as measures that safeguard "voter confidence." We demonstrate once again, but with the benefit of new survey data, that people who live in states with voter identification laws do not have greater confidence in elections or perceive lower rates of voter impersonation fraud. Since we last published on the subject, however, we notice an increase in the partisan structure of public opinion on voter identification and voter fraud. As the issue has become more salient and partisan in tone, partisan identity has become a more powerful variable in predicting both support for voter identification laws and beliefs in the prevalence of voter fraud. We note, however, that strong majorities continue to support such laws, even though a large share of the population remains unaware of the existence of voter identification requirements.

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Introduction

Eight years ago, we published the first study on the relationship between voter identification laws and public attitudes concerning voter fraud.¹ That article, like this one, was motivated by the unique constitutional argument gaining favor in the courts positing that voter identification laws could be justified as a measure to instill voter confidence in elections.² Some advocates and judges argued that such laws would do so by convincing voters that, whatever the reality, such laws decrease voter fraud at the polls. We demonstrated then that public perception of voter fraud was unrelated to the presence or absence (or stringency) of voter identification laws.³ Although voter identification laws were popular among the general population, as well as among subsets based on race and party, such laws did not have any appreciable impact on voters' perceptions of the prevalence of voter fraud or on voter turnout.⁴ We revisit those findings here with the benefit of new public opinion data and more experience with voter identification laws in a greater number of states.

One additional motivating factor behind this Article is the debate concerning our earlier article that occurred among judges in the Seventh Circuit Court of Appeals in a recent challenge to Wisconsin's voter identification law, *Frank v. Walker.*⁵ Striking down the law, the district court in that case had credited expert testimony that relied on our earlier article.⁶

- 3. Ansolabehere & Persily, supra note 1, at 1757 & tbl.4, 1758, 1760.
- 4. Id. at 1754-58.
- 5. 768 F.3d 744, 751 (7th Cir. 2014).
- 6. Frank v. Walker, 17 F. Supp. 3d 837, 851 (E.D. Wis. 2014) ("[T]he defendants produced no empirical support for the notion that Act 23's photo ID requirement actually furthers this interest [in protecting the public's confidence in the integrity of elections]. In contrast, one of the plaintiffs' expert witnesses, Barry Burden, a professor of political science at the University of Wisconsin-Madison, testified that the available empirical evidence indicates that photo ID requirements have no effect on confidence

footnote continued on next page

Stephen Ansolabehere & Nathaniel Persily, Vote Fraud in the Eye of the Beholder: The Role of Public Opinion in the Challenge to Voter Identification Requirements, 121 HARV. L. REV. 1737, 1760 (2008) ("The use of photo identification requirements bears little correlation to the public's beliefs about the incidence of fraud. The possible relation of such beliefs to participation appears even more tenuous. This lack of empirical support leads us to conclude that, at least in the context of current American election practices and procedures, public perceptions do not provide a firm justification for voter identification laws.").

^{2.} See, e.g., Crawford v. Marion Cty. Election Bd., 553 U.S. 181, 197 (2008) (plurality opinion) ("[P]ublic confidence in the integrity of the electoral process has independent significance, because it encourages citizen participation in the democratic process."); Purcell v. Gonzalez, 549 U.S. 1, 4 (2006) (per curiam) ("Voter fraud drives honest citizens out of the democratic process and breeds distrust of our government. Voters who fear their legitimate votes will be outweighed by fraudulent ones will feel disenfranchised.").

The panel opinion reversed, dismissing our *Harvard Law Review* study as not being published in a "refereed scholarly journal" and as "report[ing] the results of one opinion poll of people living throughout the country."⁷ According to the panel, "[i]f the public thinks that photo ID makes elections cleaner, then people are more likely to vote or, if they stay home, to place more confidence in the outcomes. These are substantial benefits."⁸ In a dissent joined by four other judges from the denial to rehear the case en banc, Judge Posner relied on our article to argue:

[T]hese laws do not reduce such fraud, for if they did one would expect perceptions of its prevalence to change. The study also undermines the suggestion in the panel's opinion (offered without supporting evidence) that requiring a photo ID in order to be allowed to vote increases voters' confidence in the honesty of the election, and thus increases turnout. If perceptions of the prevalence of voter-impersonation fraud are unaffected by the strictness of a state's photo ID laws, neither will confidence in the honesty of elections rise, for it would rise only if voters were persuaded that such laws reduce the incidence of such fraud.⁹

With the benefit of new survey data, we explore in this Article whether experience with these laws over the past half-decade has changed public beliefs about the incidence of fraud and the tendency to participate in elections. We conclude that there continues to be no empirical evidence that the presence of photo ID laws has a salutary effect on voter confidence. If anything, the evidence we present here suggests that the rise of these laws has coincided with a politicization of opinions about election administration, leading to a slight *increase* in voter beliefs about the prevalence of fraud.

In revisiting previous empirical research about the relationship between strict voter ID laws, citizen confidence, and voter turnout, we also aim to

- 7. Frank, 768 F.3d at 751. To be clear, we conducted two public opinion surveys for the earlier article and mentioned others on the same topic. See Ansolabehere & Persily, supra note 1, at 1739, 1742-43.
- 8. Frank, 768 F.3d at 751.
- 9. Frank v. Walker, 773 F.3d 783, 794 (7th Cir. 2014) (Posner, J., dissenting from denial of rehearing en banc); see also id. ("The panel opinion dismisses the Absolabehere [sic] and Persily article on the ground that because it was published in the Harvard Law Review, it was not peer-reviewed. So much for law reviews. (And what about Supreme Court opinions? They're not peer-reviewed either.)").

or trust in the electoral process. He described a study conducted by Stephen Ansolabehere and Nathaniel Persily and published in the Harvard Law Review which looked at the relationship between photo ID laws and voter confidence in the electoral process. See Stephen Ansolabehere & Nathaniel Persily, Vote Fraud in the Eye of the Beholder: The Role of Public Opinion in the Challenge to Voter Identification Requirements, 121 HARV. L. REV. 1737, 1756 (2008). Burden explained that this study employed multivariate analysis of survey data and found 'zero relationship' between voter ID laws and a person's level of trust or confidence in the electoral process."), rev'd, 768 F.3d 744 (7th Cir.), reh'g en banc denied by an equally divided court, 773 F.3d 783 (7th Cir. 2014), cert. denied, 135 S. Ct. 1551 (2015).

demonstrate that courts (and many social scientists) have erred by adopting an obsolete theory about how citizens incorporate information about public policy into their attitudes about government. The theory that has underlain analysis of the effects of strict voter identification laws assumes that direct experience with a photo identification requirement will demonstrate to voters that mechanisms are in place to safeguard against voter fraud; seeing or hearing about these laws being passed and implemented will reassure voters about the honesty of the electoral process. Thus, by this argument, even if strict voter ID laws do not actually have a deterrent effect on fraud, and even if impersonation fraud occurs with a frequency that approaches zero, when a voter observes a strict ID law being implemented, she or he will be reassured about the efficacy of elections, and this reassurance redounds to the benefit of democracy.

However, this theory underlying how courts and many scholars have thought about the link in the public mind between voter ID laws and voter reassurance is contrary to the prevailing view within the public opinion literature about how mass publics assess the efficacy of public policy, especially policies that have partisan overtones. The prevailing view is that political partisanship is a deeply held identity among many (and even most) voters, and it is through this identity that they judge government policies.¹⁰ Voters take their cues from party leaders when they judge how well policies are working.¹¹ If elites associated with the two parties take similar positions on an issue, or if they take no identifiably partisan positions, the mass public will not judge the issue in a partisan manner and is likely to judge it based on factors such as demographics, economic interests, etc. If statements and positions by party elites diverge, opinions by followers will diverge as well, sometimes overshadowing objective interests that the followers themselves might otherwise have. This view has been confirmed across a wide variety of policy domains, both in domestic and foreign policy.¹² There is no reason to believe election policy should be any different. Indeed, given the direct relationship between election regulation and party success at the polls, we should expect partisanship to be an even more powerful predictor of opinion in this domain than in others.

We begin in Part I by examining attitudes toward voter identification laws. We find continued support for such laws but a widening gap in support between Democrats and Republicans. The partisan structure of opinion is particularly pronounced among those who follow the news closely and are

^{10.} DONALD GREEN ET AL., PARTISAN HEARTS AND MINDS: POLITICAL PARTIES AND THE SOCIAL IDENTITIES OF VOTERS 204-10 (2002).

^{11.} JOHN R. ZALLER, THE NATURE AND ORIGINS OF MASS OPINION 310-32 (1992).

^{12.} See, e.g., ADAM J. BERINSKY, IN TIME OF WAR: UNDERSTANDING AMERICAN PUBLIC OPINION FROM WORLD WAR II TO IRAQ 85-126 (2009) (describing public opinion concerning military and foreign policy).

therefore more likely to pick up signals as to the Democratic and Republican positions on voter identification. Most of the movement in public opinion that we discern from the half-decade of polls has occurred among Democrats, who are now much less likely to support voter identification laws than Republicans. Part II examines attitudes about the prevalence of voter fraud. Here, too, we find increasing divergence between partisans, with Republicans more likely than Democrats to believe that voter impersonation fraud is very common. However, as in our earlier article, we find no relationship between the presence of a voter ID law and respondents' belief in the frequency of fraud. We also do not find any relationship between belief in voter fraud and either reported turnout in a previous election or intention to vote in an upcoming election. Part III briefly discusses survey results concerning why people support voter identification and whether voters know of the existence of restrictive photo identification laws. Although, once again, we find widespread support for such laws, we also find that a substantial share of the population does not know whether or not their state requires photo identification in order to vote. Part IV presents our conclusions, which can be stated succinctly here. Attitudes have changed and become more polarized with respect to voter ID requirements and voter fraud, but we continue to find no relationship between the existence of an ID law and greater voter confidence. This lack of a relationship may be due to the fact that such laws are unevenly enforced or are not salient to a large group of voters. It could also be due, we suspect, to the fact that attitudes about voter fraud have less to do with the precise electoral legal regime in place and its success or failure and more to do with attitudes toward the political system as a whole. In this respect, attitudes about voter fraud are like other measures of political alienation or lack of trust in government: they piggyback onto larger concerns about government competence and specific opinions about the incumbents currently in control.

I. The Changing Partisan Structure of Support for Voter Identification Laws

We begin our empirical investigation by reviewing support for voter ID laws and the degree to which that support has become associated with partisanship. Recent survey data suggest partisan identity is a powerful variable in predicting attitudes toward voting in general. Democrats are more likely than Republicans, for example, to consider voting to be a right rather than a privilege. Moreover, when forced to choose, Democrats are more likely to say it is more important to make voting easy, rather than more secure.¹³

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^{13.} These claims are based on analysis of the MIT module to the 2013 Cooperative Congressional Election Study (CCES). The data from the 2013 MIT module is available for download from the Harvard Dataverse at the following URL: http://dx.doi.org /10.7910/DVN/AHHNTP. Respondents were asked to place themselves on a scale (0-

Because most new voting restrictions, such as ID laws, have been passed by Republican-controlled legislatures,¹⁴ we should not be surprised to find that when such policy debates become salient and well known, Republican and Democratic respondents will favor positions consistent with party elites.

The partisan division on voter identification laws, though it has always been present, has grown in recent years. In a review of support for photo voter ID laws, Gronke et al. examined all publicly available public opinion data about support for strict (i.e., photo) voter ID laws since the mid-2000s.¹⁵ They found that when questions were asked about the matter in 2006, a vast majority of Americans (over 80%) agreed that one should be required to produce a photo ID in order to vote.¹⁶ Even then, there was a party divide with about 90% of Republicans and 70% of Democrats favoring strict ID laws. This state of affairs continued into 2008 without much change. However, between 2008 and 2012, public opinion shifted significantly. By 2012, overall support for strict ID laws had fallen slightly, to approximately 75%, but more importantly, the gap between Democrats and Republicans had doubled, from approximately 17 percentage points to over 35 percentage points.¹⁷

We expand on those earlier studies with data on attitudes about photo voter ID laws taken from the Survey of the Performance of American Elections (SPAE), administered in 2008, 2012, and 2014, and the common content from the 2008 Cooperative Congressional Election Study (CCES). The SPAE is administered to 200 registered voters in each state, meaning that we have both a large number of respondents overall (10,000 in 2008 and 10,200 in 2012 and 2014, when Washington, D.C. was added to the study) and a relatively

¹⁰⁰⁾ that described how they would balance the following considerations: (1) It is important to make voting as easy as possible even if voting is not easy/it is important to make voting as secure as possible, even if there are some security risks (MIT408A, B) and (2) Voting is a right/voting is a privilege (MIT410A, B). Respondents were randomly assigned different anchoring points on the scale. (For instance, half the respondents had "voting is a right" set to 100 points while the other half had "voting is a privilege" set to 100 points.) If we label the two scales so that 100 is assigned to "voting should be easy" and "voting is a right," then the average Democratic responses were 43.4 and 69.6, respectively, while the average Republican responses were 26.1 and 52.6. These differences, measured by a *t*-test, were significant at the p < .0005 level in each case.

See William D. Hicks et al., A Principle or a Strategy?: Voter Identification Laws and Partisan Competition in the American States, 68 POL. RES. Q. 18, 19-20 (2015); Seth C. McKee, Politics Is Local: State Legislator Voting on Restrictive Voter Identification Legislation, RES. & POL., July-Sept. 2015, at 1, 3, 5; Ari Berman, The GOP War on Voting, ROLLING STONE (Aug. 30, 2011), http://rol.st/OXmTRa.

Paul Gronke et al., Voter ID Laws: A View from the Public 5, 22 fig.1 (Mass. Inst. of Tech., Pol. Sci. Dep't, Research Paper No. 2015-13), http://ssrn.com/abstract=2594290.

^{16.} Id. at 22 fig.1.

^{17.} Id. at 22 fig.1b.

large and constant number of respondents from each state.¹⁸ The latter feature comes in handy when we assess the effects of state-specific identification laws on attitudes about strict photo ID requirements. The CCES is a much larger national survey,¹⁹ administered in the same fashion as the SPAE, except it is organized around a national sample, rather than fifty-one separate state samples.²⁰

In each of these surveys, respondents were asked whether they supported the requirement that all voters show photo ID at the polling place. Consistent with the overall pattern reported by Gronke et al., support has generally been high in each of the surveys examined here. In 2008, support for strict photo ID was 70.7% in the CCES and 76% in the SPAE.²¹ Overall support fell to 71.0% in the 2012 SPAE and 70.4% in 2014.²²

- 18. All of the data and documentation related to the various releases of the SPAE are available for download from the Harvard Dataverse at Survey of the Performance of American Elections Dataverse, HARV. DATAVERSE, https://dataverse.harvard.edu/dataverse/SPAE (last visited June 6, 2016). Throughout this Article, questions that appear in the survey are referenced by the year of the survey and the question number. For instance, "2008 SPAE, Q36" refers to Question 36 of the 2008 SPAE survey.
- The 2008 CCES common content had 32,800 respondents. STEPHEN ANSOLABEHERE, GUIDE TO THE 2008 COOPERATIVE CONGRESSIONAL ELECTION SURVEY 4 (July 15, 2011), http://cces.gov.harvard.edu [hereinafter 2008 CCES].
- 20. We use a combination of the CCES and SPAE because neither study had both critical variables we rely on—a measure of support for photo ID laws and a measure of the degree one follows government and public affairs—in each year.
- 21. 2008 CCES, supra note 19, at 72; R. MICHAEL ALVAREZ ET AL., 2008 SURVEY OF THE PERFORMANCE OF AMERICAN ELECTIONS: FINAL REPORT 50, 174 [hereinafter 2008 SPAE]. One difference between the SPAE and the CCES is that the former is a sample of registered voters, whereas the latter is based on a sample of adults. Compare 2008 SPAE, supra, at i, with Sample Design, COOPERATIVE CONG. ELECTION STUDY, http://projects.iq .harvard.edu/cces/book/sample-design (last visited June 6, 2016). To make the analysis comparable between the two surveys, in this Article we restrict ourselves to self-reported registered voters in the CCES.
- 22. CHARLES STEWART III, 2012 SURVEY OF THE PERFORMANCE OF AMERICAN ELECTIONS: FINAL REPORT 48, 58 tbl.IV-5 (2013) [hereinafter 2012 SPAE]; CHARLES STEWART III, 2014 SURVEY OF THE PERFORMANCE OF AMERICAN ELECTIONS: FINAL REPORT, at Q42f (2015) [hereinafter 2014 SPAE]. In 2008, the SPAE asked respondents the following question: "Do you support or oppose any of the following proposals for new ways of voting or conducting elections?" 2008 SPAE, supra note 21, at 135 Q43. One of the proposals was "Require all people to show government issued photo identification when they vote." Id. at 174. The response categories were "Support" and "Oppose." Id. In 2012 and 2014 respondents were presented with the following introduction to the battery of questions about reform proposals: "Do you support or oppose any of the following proposals for new ways of voting or conducting elections?" 2012 SPAE, supra, at 159; 2014 SPAE, supra, at Q42f. One of the proposals was "Require all people to show government issued photo ID when they vote." 2012 SPAE, supra, at 159; 2014 SPAE, supra, at Q42f. The response categories were "Support strongly," "Support somewhat," "Oppose somewhat," and "Oppose strongly." 2012 SPAE, supra, at 159; 2014 SPAE, supra, at Q42f. The statistics reported here for 2012 and 2014 combine the "Support strongly" and "Support somewhat" answers into a single "Support" response

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Most telling, however, has been the change among partisans. In 2008, close on the heels of the decision in *Crawford v. Marion County Election Board*,²³ majorities of Democrats and Republicans favored photo ID requirements: in the SPAE, support levels were 65% for Democrats and 90% for Republicans; in the CCES, support levels were 59.7% and 86.9%, respectively.²⁴ Since then, support among Democrats has fallen, while support among Republicans has held firm. In 2012, Democratic support for photo ID laws had fallen to 54.4%; by 2014 it had fallen even further, to 51.8%.²⁵ Republican support measured 88.4% in 2012 and 91.2% in 2014.²⁶ In the end, these differences in support among Republicans and Democrats in the three SPAE studies grew from 25.0 percentage points in 2008 to 39.4 percentage points in 2014.

As we discussed earlier, the public opinion literature that focuses on the cue taking of partisans finds that not everyone is equally susceptible to receiving and internalizing partisan cues. In particular, partisans who do not stay current on political issues—such as those who were socialized into one of the parties as a child based on parental influences, but who later avoided political news and activities—will be slow to change their opinions to align with changing party orthodoxy. Conversely, a partisan who is obsessed with following the political news but had previously held a position out of step with the growing elite consensus will be in a good position to learn about the positions that party leaders take on them and to adapt accordingly. If this pattern applies to the issue of photo identification, then the most politically informed partisans of both parties should be the ones moving the fastest to align themselves with party leaders on the photo ID issue. Certainly, because of ceiling effects, highly informed Republicans have less room to move toward elite party orthodoxy than highly informed Democrats.

We can measure attention to the news and public affairs using a question that was asked on the 2008 CCES and the 2012 and 2014 SPAE: "Some people seem to follow what's going on in government and public affairs most of the

- 23. 553 U.S. 181 (2008).
- 24. 2008 SPAE, supra note 21, at 55. Data and codebooks for CCES calculations are available at http://cces.gov.harvard.edu.
- 25. 2012 SPAE, supra note 22, at 59 tbl. IV-5.
- 2012 SPAE, supra note 22, at 59 tbl. IV-5. Throughout this period, support among independent has remained relatively flat, going from 76.5% in 2008, to 74.4% in 2012, to 74.8% in 2014. 2012 SPAE, supra note 22, at 59 tbl. IV-5.

and the "Oppose strongly" and "Oppose somewhat" answers into a single "Oppose" response.

Here and in other parts of this Article, where nationwide statistics are reported, we rely on survey weights that adjust respondents depending on the size of the state where the respondent lives. Thus, for instance, although we have 200 respondents from both California and Wyoming, California respondents are up-weighted relative to Wyoming respondents to produce an estimate of the proportion of nationwide voters who favor photo identification laws.

time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs...?²⁷

Available responses included "Most of the time" (62.2% in 2014), "Some of the time" (23.2%), "Only now and then" (9.1%), and "Hardly at all" (3.9%).²⁸ (Another 1.5% responded that they "Don't know.") Because roughly half the respondents answer the first category, "Most of the time," we treat these respondents as constituting the high-information group and all others as belonging to the low-information group.

In Table 1, we report the fraction of partisans who stated that they supported photo voter ID laws, broken down by information level. These results confirm the partisan cue-taking theory in recent years. By 2008, the gap in support for the photo voter ID "party positions" on the issue had already emerged: 94.1% of high-information Republicans supported photo ID laws, compared to only 57.8% of high-information Democrats, for a difference of 36.3 percentage points. On the other hand, because low-information Democrats supported strict ID laws at a much higher level than their high-information copartisans, the gap in support between low-information Republicans and Democrats was a much smaller 8.0 points.

Year	All		Democrats		Republicans		Republicans – Democrats Difference	
	High Info	Low Info	High Info	Low Info	High Info	Low Info	High Info	Low Info
2008	77.2% (16,013)	85.0% (8014)	57.8% (5100)	81.9% (3048)	94.1% (6204)	89.9% (2304)	36.3	8.0
2012	66.9% (6034)	76,4% (4152)	37.0% (2091)	72.7% (1710)	91,5% (1973)	83.1% (1059)	54.5	10.4
2014	68.5% (6341)	75.2% (3835)	39.3% (2069)	67.2% (1434)	93.5% (1839)	87.3% (924)	54.2	20.1

Table 1

Support for Photo Voter ID Laws, by Party and Interest in Public Affairs

 In all years, this variable is named NEWSINT, except for the 2008 CCES, when it is named V244. See 2008 CCES, supra note 19, at 32; 2014 SPAE, supra note 22, at newsint.

 2014 SPAE, supra note 22, at newsint. The distribution of responses was very similar in all the other years. See 2008 CCES, supra note 19, at 32.

The heightened partisanship of the photo ID issue after the 2010 election is evident in the public opinion shifts among the different groups of partisans. Between 2008 and 2012, support for photo ID laws dropped significantly among high-information Democrats (from 57.8% to 37.0%) and somewhat less among low-information Democrats (from 81.9% to 72.7%); support among Republicans sagged a little bit but overall held firm. As a consequence, the gap in support for strict ID laws among high-information partisans grew significantly, from 36.3 points in 2008 to 54.5 points in 2012. Two years later, the gap was virtually unchanged.

Consistent with the expectation that it would take longer for lowinformation partisans to alter their opinions about strict voter IDs, the gap between low-information Republicans and Democrats was virtually unchanged between 2008 and 2012 (8.0 percentage points in 2008 vs. 10.4 points in 2012). However, just two years later, the gap had doubled to 20.1 percentage points.

It is natural to ask whether the growing partisan gap in opinion about strict ID laws is more accurately attributed to confounding factors that may be influencing opinion on the issue, such as ideology, race, and beliefs about voter fraud. To confirm that the cue-taking theory is operating in the realm of photo identification policy despite the presence of potentially confounding factors, we conducted a multiple regression analysis to predict support for strict photo ID laws using data from the 2014 SPAE. The dependent variable was a dummy variable equal to one if the respondent was in favor of requiring photo identification in order to vote and zero otherwise. The independent variables included measures of partisanship, beliefs about the frequency of voter impersonation, ideology, and race. We conducted the analysis separately for high- and low-information respondents as described above. We describe the remaining details of this multiple regression analysis in Appendix A.

To summarize the analysis in Appendix A, once we control for beliefs about voter impersonation, ideology, and race, there was still an 18percentage-point difference in support for photo ID laws among highinformation partisans in 2014.²⁹ Among low-information partisans, the gap (after controls) was only 12 percentage points.³⁰ The reduction in the influence of partisanship *per se* on attitudes about photo voter ID laws in the multiple regression analysis comes primarily from the fact that ideology, which of course is highly correlated with partisanship, is also highly correlated with attitudes about ID laws. This is true for both high- and low-information respondents, but the influence of ideology is much greater for the highinformation group.

^{29.} Recall that before the controls were entered, this gap was 54.2 points. See supra Table 1.

^{30.} Recall that before the controls were entered, this gap was 20.1 points. See id.

The multiple regression analysis suggests that cue taking acts through two paths to reinforce partisan attitudes about voter ID among the mass public. The *direct path*, which is measured by the "party" coefficient in the multiple regression analysis, reflects opinions about how photo ID laws influence the fortunes of the party; such laws are perceived to help Republican fortunes and hurt Democratic fortunes, and partisans react accordingly. The *indirect path* moves through other attitudes that are correlated with partisanship, such as ideology and attitudes about fraud, which have also shifted as a consequence of the heightened awareness that strict voter ID laws are now a political, not a valence, issue.

II. Beliefs About the Prevalence of Voter Fraud

Beliefs about voter fraud have been at the center of justifications for the passage of voter ID laws. If it is true that "[v]oter fraud drives honest citizens out of the democratic process and breeds distrust of our government,"³¹ and that the presence of strict voter ID laws instills a greater sense of citizen trust and confidence in our government, then the growth in the number of voter ID laws over the past decade should have decreased the public's belief that fraud is prevalent in elections and increased citizen trust and confidence in government. The public opinion evidence is contrary to this expectation and, once again, consistent with the pattern of opinion we would expect from this becoming an issue polarized by partisanship.

In the years since the publication of the 2008 Ansolabehere and Persily article,³² the SPAE has continued to probe attitudes about voter fraud among registered voters. In 2008, borrowing question wording from the Ansolabehere and Persily article, SPAE respondents were asked the following three questions:

- It is illegal to vote more than once in an election or to vote if not a U.S. citizen. How frequently do you think this occurs in your community? [voter fraud]
- Another form of fraud occurs when votes are stolen or tampered with. How frequently do you think this occurs in your community? [vote theft]
- It is illegal for a person to claim to be another person, who is registered to vote, and to cast that person's vote. How often do you think this occurs in your community? [voter impersonation]³³

^{31.} Purcell v. Gonzalez, 549 U.S. 1, 4 (2006) (per curiam).

^{32.} Ansolabehere & Persily, supra note 1.

^{33. 2008} SPAE, supra note 21, at 171-72.

The response categories in each case were (1) It is very common, (2) It occurs occasionally, (3) It occurs infrequently, (4) It almost never occurs, and (5) Not sure.

Beginning in 2012, the SPAE changed its strategy somewhat in asking about fraud. Instead of a series of separate questions, respondents were presented with a grid of illegal activities associated with elections. The grid was preceded with the following prompt: "The following is a list of activities that are usually against the law. Please indicate how often you think these activities occur in your county or city."³⁴ Six activities were then presented to the respondent:

- People voting more than once in an election³⁵
- People stealing or tampering with ballots that have been voted
- People pretending to be someone else when going to vote
- People voting who are not U.S. citizens
- People voting an absentee ballot intended for another person
- Officials changing the reported vote count in a way that is not a true reflection of the ballots that were actually counted³⁶

The response categories were the following: (1) It is very common, (2) It occurs occasionally, (3) It occurs infrequently, (4) It almost never occurs, and (5) I'm not sure.³⁷

Table 2 reports the distribution of responses to these questions. What is striking about these results is the stability of aggregate responses over this period.³⁸ It is also the case that the high degree of correlation *across* fraud items that was noted in the Ansolabehere and Persily article has continued to the

- 37. 2012 SPAE, supra note 22, at 154; 2014 SPAE, supra note 22, at Q37a-f.
- 38. The responses to the questions asked in the SPAE have been consistent over time, but they are not entirely consistent with the results associated with the 2007 CCES survey, which formed part of the empirical grounding for the Ansolabehere and Persily article; Ansolabehere and Persily, supra note 1, at 1745 n.25, previously noted that the question wording in 2007 'may have primed respondents to express their concerns about voter fraud more generally," rather than to express their opinions about the specific form of fraud. As a consequence, the 2008 questions, and those that followed, replaced "such vote fraud" with "this," or "these activities" to make it clear that the question was about specific forms of fraud, not fraud in general. See 2008 SPAE, supra note 21, at 171-72; 2012 SPAE, supra note 22, at 154; 2014 SPAE, supra note 22, at Q37a-f. The 2007 CCES results are so different from the ones that follow that it suggests Ansolabehere and Persily's conjecture was correct. Therefore, we focus here on responses to a set of questions that were subsequently developed to prompt opinions about specific forms of voter fraud.

^{34. 2012} SPAE, supra note 22, at 154 (emphasis omitted).

^{35.} Note that the SPAE had previously combined into one question the topics of multiple voting and noncitizen voting. 2008 SPAE, supra note 21, at 171. From 2012 onward, beliefs about these two topics have been assessed separately. 2012 SPAE, supra note 22, at 154; 2014 SPAE, supra note 22, at Q37a, Q37d.

^{36. 2012} SPAE, supra note 22, at 154; 2014 SPAE, supra note 22, at Q37a-f.

present.³⁹ Among those expressing an opinion about the prevalence of fraud, the average intercorrelation of the fraud items was .74 in the 2008 SPAE, .74 in 2012, and .71 in 2014.

a.	Voter frau	l (voting more t	han once and no	oncitizens v	oting)	
Year	Very Common	Occasionally	Infrequently	Almost Never	Not Sure	N
2008	9%	19%	17%	31%	24%	9987

Table 2	
Distribution of Voter Fraud Responses,	2008-201440

b. Voting more than once in an election

Year	Very Common	Occasionally	Infrequently	Almost Never	Not Sure	N
2012	11%	15%	16%	36%	21%	10,191
2014	8%	15%	16%	40%	22%	10,164

Year	Very Common	Occasionally	Infrequently	Almost Never	Not Sure	N
2012	18%	16%	14%	31%	21%	10,191
2014	13%	17%	14%	33%	23%	10,160

d.	Voter im	personation
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Noncitizane votine

Year	Very Common	Occasionally	Infrequently	Almost Never	Not Sure	N
2008	5%	15%	16%	35%	28%	9954
2012	12%	18%	17%	32%	22%	10,193
2014	8%	1 6%	15%	38%	23%	10,158

39. See Ansolabehere & Persily, supra note 1, at 1749.

40. 2014 SPAE, supra note 22, at Q37a-f.

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e.	Vote theft					
Year	Very Common	Occasionally	Infrequently	Almost Never	Not Sure	N
2008	6%	16%	15%	37%	26%	9986
2012	9%	17%	19%	33%	23%	10,190
2014	7%	14%	16%	40%	24%	10,158

f.	Absentee ballot fraud							
Year	Very Common	Occasionally	Infrequently	Almost Never	Not Sure	N		
2012	12%	20%	18%	24%	25%	10,190		
2014	9%	19%	17%	28%	27%	10,162		

g. Officials changing results ill	egall	y
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Year	Very Common	Occasionally	Infrequently	Almost Never	Not Sure	N
2012	9%	15%	18%	33%	25%	10,192
2014	7%	14%	16%	38%	26%	10,161

We believe that this high intercorrelation of beliefs about election fraud has serious implications for understanding how the mass public processes the debate about topics such as strict voter IDs. This high intercorrelation suggests that beliefs about fraud derive from a single underlying attitude about the fairness of elections and, quite likely, about generalized trust in government itself. In other words, despite the fact that the legal- and electionadministration communities make fine distinctions between the sources of election fraud, survey respondents who see one type of fraud as prevalent tend to see other types of fraud as prevalent as well.

A traditional way that social scientists measure the degree to which answers to a battery of survey questions may be caused by a single underlying attitude is through factor analysis, which quantifies the degree to which answers to the battery of questions are correlated with the hypothesized unmeasured underlying (or "latent") attitude.⁴¹ When we conduct a principal

^{41.} See generally JAE-ON KIM & CHARLES W. MUELLER, FACTOR ANALYSIS: STATISTICAL METHODS AND PRACTICAL ISSUES (1978).

components factor analysis on the battery of fraud questions in the 2014 SPAE, we find strong support for the hypothesis that answers to the full battery of questions are primarily tapping into attitudes about election fraud in general. This hypothesized single dimension explains 76% of the variance in the answers to the fraud battery in 2014 (among those who expressed an opinion), and no other dimension explains more than an additional 10% of the variance.

A. Beliefs About the Prevalence of Voter Impersonation

The type of fraud most appropriately targeted by photo voter ID laws is voter impersonation, namely, the situation when voters attempt to vote under another person's name.⁴² Voter identification laws usually address voter impersonation by requiring voters who vote in a polling place to present certain forms of identification to verify their identity. Here, we delve more deeply into partisan polarization over attitudes about voter impersonation fraud in recent years.

As reported in Table 2d, since 2008, there has been little net movement in overall attitudes about the frequency of voter impersonation at the national level. Table 3a allows us to explore attitudes about impersonation fraud by party, in this case, by combining SPAE respondents who reported that they believed impersonation fraud was either "Very common" or occurred "Occasionally." (To aid in discussion, we will term those who believe that voter impersonation fraud is either very common or occurs occasionally as believing that voter impersonation fraud occurs "frequently.")

a. By party: percentages reporting "Very common" or "Occasionally"					
Year	Democrats	Republicans	Republicans-Democrat		
2008	13.4	29.5	16.1		
2012	16.0	42.2	26.2		
2014	12.6	28.4	15.9		

Table 3							
D.1:.C:	E	CM	2000	201 443			

42. See Crawford v. Marion Cty. Election Bd., 553 U.S. 181, 225 (2008) (Souter, J., dissenting) ("[R]equiring a voter to show photo identification before casting a regular ballot addresses only one form of voter fraud: in-person voter impersonation.").

43. 2014 SPAE, supra note 22, at Q37c.

Year	Democrats		Republicans		Republicans-Democrats Difference	
	High Info	Low Info	High Info	Low Info	High Info	Low Info
2012	12.6	19.6	47.3	34.4	34.7	14.8
2014	10.2	18.2	32.6	30.3	22.4	12.1

b. Difference among high- and low-information partisans

Year	Democrats		Republicans		Republicans-Democrats Difference	
	Strict Photo	HAVA Minimum	Strict Photo	HAVA Minimum	Strict Photo	HAVA Minimum
2008	11.6	14.1	25.1	30.6	13.5	16.5
2012	17.1	15.5	33.7	44.0	16.6	28.5
2014	13.5	13.2	30.3	34.7	16.8	21.5

c. Difference among partisans by strictness of photo ID regime

Republicans have been much more likely than Democrats to believe that voter impersonation occurs frequently. The Republican-Democratic difference was about 16 percentage points in 2008 and 2014 and 26 points in 2012. Whether the 2012 surge in Republicans believing voter impersonation fraud was frequent is due to the particular circumstances of the 2012 election or just a result of random variation, the existence of a persistent difference across the two parties seems clear.⁴⁴

44. Whether this surge in the partisan gap that we see in 2012 over whether voter fraud occurs frequently, as compared to 2008 and 2014, is a statistical anomaly or was due to priming associated with the specific circumstances associated with the 2012 presidential election is difficult to tell. We know from a series of questions included in the 2012 MIT/Reed College module of the CCES that a large minority of Republican voters reported that they believed the outcome of the 2012 presidential election was the result of election irregularities. For instance, respondents were asked the following question intended to probe whether they regarded the 2012 presidential election to have been fairly decided: "Taking everything into account concerning the 2012 presidential election, indicate which statement most closely describes how you believe the outcome was decided." Three options were given to respondents. The first, most benign option was "Votes were counted accurately nationwide. The man who actually received the most votes was elected president in a fair election," to which 79.1% of Democrats but only 40.4% of Republicans agreed. The third, least benign option was

footnote continued on next page

In Table 3b we examine whether the gap in the perception of the frequency of impersonation fraud has been the same for high- and low-information partisans. It has not. In both 2012 and 2014, the gap between high-information Republicans and high-information Democrats was much greater than the difference between low-information partisans. In 2012, for instance, 12.6% of high-information Democrats believed voter impersonation was frequent, compared to 47.3% of high-information Republicans, for a 34.7 point gap. In contrast, 19.6% of low-information Democrats and 34.4% of low-information Republicans believed voter impersonation was frequent, a smaller 14.8 point gap. (Unfortunately, the 2008 SPAE did not include the "news interest" question, and therefore it is not possible to test whether the relative gap between high- and low-information partisans was as great in 2008.)

Finally, in Table 3c we explore whether living in a state that had adopted a strict photo ID law influenced attitudes about voter impersonation fraud.⁴⁵ The most direct comparison is between respondents living in states that had adopted strict photo ID laws and those that had maintained "HAVA minimum" laws, i.e., laws that only required documentary identification under the conditions specified in the Help America Vote Act (HAVA) for first-time voters who had registered by mail.⁴⁶ In 2008, there were only two states in the "strict photo ID" category, Georgia and Indiana.⁴⁷ In 2012 that number had grown to four (adding Kansas and Tennessee);⁴⁸ in 2014, three more states had become strict photo ID states (adding Mississippi, Texas, and Virginia).⁴⁹ Conversely, there were 24 states (including the District of Columbia) that had HAVA-minimum laws in 2008, dropping to 19 in 2014.⁵⁰

The results in Table 3c suggest that if there has been an effect of enacting strict photo ID laws, it has been subtle. Among Democrats and Republicans, for

- 45. States are coded in Table 3c according to the photo ID regime that was in place in their state as of the year of the study.
- 46. Help America Vote Act of 2002, 52 U.S.C. § 21083(b)(2)(A) (2014).
- 47. The coding of states by strictness of voter ID laws was based on information contained in the Voter ID History website maintained by the National Conference of State Legislatures. Voter ID History, NAT'L CONF. ST. LEGISLATURES, http://www.ncsl.org /research/elections-and-campaigns/voter-id-history.aspx (last visited June 6, 2016) [hereinafter NCSL Voter ID History Website].
- 48. Id.
- 49. Id.
- 50. Id.

[&]quot;There was a lot of fraud in counting the votes after the election. Because of that, the man who actually received the most votes nationwide was denied the presidency," to which only 2.1% of Democrats, but fully 31.6% of Republicans agreed. If less than half of Republicans expressed an opinion that President Obama was elected president in 2012 fairly, and nearly one-third stated the election was stolen from the rightful winner, it may not be surprising that there was a surge among Republicans who believed that voter impersonation was common in 2012, compared to both 2008 and 2014.

both those in strict photo ID states and those in HAVA-minimum states, the fraction believing that voter impersonation was frequent increased between 2008 and 2012, before falling back in 2014. In the end, the difference between partisans in strict photo ID states and HAVA minimum states was slightly greater in 2014 than in 2008, but again, the overall movement has been relatively small.⁵¹

As with attitudes about voter ID, it is important to check to see whether different attitudes about impersonation fraud across states with and without strict photo ID hold up in the face of controls for factors such as ideology and race. We do this through a multivariate analysis that is described more fully in Appendix B below.

To summarize the analysis in Appendix B, in 2014 there was a 9.3 percentage point difference in the degree to which Democrats and Republicans nationwide believed that voter impersonation was frequent, after controlling for voter ID regime, ideology, and race. This is only somewhat less than the 15.9 percentage point difference reported in Table 3a before statistical controls are applied. When we perform separate analyses among respondents who lived in strict photo ID states in 2014 and compare the results to respondents who lived in HAVA-minimum states, we see a similar gap between Democrats and Republicans of almost exactly ten percentage points in each case.

In summary, there is no evidence that the passage of strict photo ID laws has led to a decrease in the belief of the frequency of voter impersonation. Overall beliefs have remained stable, as has the gap between Republicans and Democrats.

B. Perceptions of Fraud and the Likelihood of Voting

Both Purcell v. Gonzalez and Crawford v. Marion County asserted that perceptions of voter fraud depress turnout.⁵² Previously, Ansolabehere and Persily showed that there was little correlation between fraud perceptions and reported turnout in the 2008 presidential primaries or in the 2008 general election.⁵³ This conclusion was reached based on a number of turnout measures, including both validated and reported vote in 2006 and a measure of

52. See supra note 2.

53. Ansolabehere & Persily, supra note 1, at 1753-54.

^{51.} Of course, without a panel study on the matter, we are not in a position to conclude whether these changes are due to changes in attitudes among voters in the various states or just simply a result of the changing composition of states with different types of laws. Two states—Kansas and Mississippi—went from being a HAVA minimum state to a strict photo ID state during this time. *Id.* In these two states, identifiers of both parties were virtually unchanged in believing that voter impersonation was frequent. From 2008 to 2014, the percentage of Democrats who believed that voter impersonation was frequent went from 8.2% to 7.6%, compared to the Republican percentages, which went from 19.8% to 21.0%.

the intent to vote in the 2008 presidential election. We confirm those findings with data from a new survey we commissioned from YouGov in December $2015.^{54}$

We find, first, that in a simple bivariate test, the previous Ansolabehere and Persily findings hold: not only is the correlation between beliefs in the frequency of voter fraud and self-reported turnout nearly zero, the sign of the relationship is negative.⁵⁵ Notice that reported vote in 2014 and intention to vote in 2016 is *highest* among those who think fraud is "very common": 70.2% in that category say they voted in 2014 and 76.7% say they will vote in 2016. This is higher than respondents in each other category and is, respectively, about 17 percentage points and 16 percentage points higher than the average. The lowest turnout group is comprised by those who are not sure about the frequency of voter fraud their turnout rates are less than half of the average.

Second, we tested this relationship through a multivariate probit analysis that was identical to that conducted by Ansolabehere and Persily.⁵⁶ We tested for each of these effects directly, and for the possibility that the two factors interact. That is, it may be the case that people who believe fraud is common might be particularly unlikely to vote in states that lack a photo ID law, because the photo ID law provides protection against such fraud. We performed a test for the statistical significance of these factors, controlling for age, gender, education, income, employment status, race, and party identification. These demographic factors are commonly found to predict past participation and intentions to vote in the future.⁵⁷ We examined how well these factors explain self-reported votes in the 2014 midterm election and intent to vote in the 2016 general election.

The results of the multivariate analysis are reported in Table 4b. In that analysis, the standard demographic factors are highly significant in predicting turnout, especially age, income, partisanship, and employment status. Beliefs about the frequency of voter fraud and the presence of a state photo ID law had no explanatory power. The coefficient on beliefs is 0.004 (*p*-value = .953). Substantively, this is trivially small, and statistically insignificant. The same is true for beliefs in explaining intent to vote in 2016, in which the coefficient is 0.076 (*p*-value = .338). The coefficient on whether a state has photo ID is somewhat larger, both for 2014 and 2016, but in both instances is statistically

^{54.} See infra Table 4a.

^{55.} The dataset containing these survey results are available from the Authors upon request.

^{56.} See Ansolabehere & Persily, supra note 1, at 1770, 1772.

See generally JAN E. LEIGHLEY & JONATHAN NAGLER, WHO VOTES NOW?: DEMOGRAPHICS, ISSUES, INEQUALITY, AND TURNOUT IN THE UNITED STATES (2014); RAYMOND E. WOLFINGER & STEVEN J. ROSENSTONE, WHO VOTES? (1980).

indistinguishable from zero.⁵⁸ And, the interaction effects are substantively small and statistically insignificant.⁵⁹ The most recently available data, then, display no evidence that either beliefs about the frequency of voter fraud or photo ID laws designed to combat voter fraud have any relationship to or effect on turnout or intentions to vote.

	Voted in 2014		Intends to Vote in 2016	
Frequency of Voter Impersonation	%	N	%	N
Very common	70.2%	184	76.7%	184
Occasionally	55.2%	320	62.0%	320
Infrequently	53.9%	140	65.6%	140
Never	61.8%	183	70.4%	183
Not sure	24.4%	173	28.7%	173
Total	53.6%	1000	61.0%	1000
Correlation b/t vote & belief	-0.05	865	-0.02	865
Correlation b/t vote & not sure	27	1000	30	1000

 Table 4a

 Turnout and Beliefs About Fraud, 2015

 The p-value of the photo ID coefficient in 2014 is 0.222 and 0.499 for 2016 vote intention. See infra Table 4b.

The p-value of the 2014 and 2016 interaction terms are 0.189 and 0.229, respectively. See id.
	Did You Vote in 2014?		Do You Inter 201	nd to Vote in 16?
	Coefficient	Standard Error	Coefficient	Standard Error
Belief in Frequency of Fraud	0.004	0.072	0.076	0.079
State Has Photo ID Requirement	0.357	0.292	0.211	0.312
Belief in Frequency of Fraud and State Has Photo ID Requirement	-0.141	0.108	-0.141	0.117
Age (years)	0.029	0.005	0.026	0.005
Gender (male)	0.153	0,117	0.285	0.127
Level of Education (1-6)	0.162	0.044	0.088	0.047
Democrat	0.529	0.131	0.775	0.142
Republican	0.379	0.150	0.609	0.161
White	0.283	0.124	0.384	0.131
Employed	0.461	0.148	0.516	0.158
Homemaker	0.091	0.215	0.120	0.223
Retired	0.347	0.215	0.454	0.231
Student	-0.118	0.294	-0.203	0.292
Income Category (1-4)	0.261	0.068	0.288	0.073
Constant	-2.619	0.357	-2.445	0.382
N	753		75	3
Log-Likelihood	-336	5.0	-287.0	
Pseudo R-Square	.224 .234		34	

Table 4b Turnout and Beliefs in Voter Fraud (Probit Analysis)

C. Perceptions About Fraud and Voter Confidence

Finally, there is the question whether perceptions of voter fraud influence opinions more broadly. We can explore this topic by examining the degree to which perceptions about fraud influence confidence that votes were counted as cast.

Questions about voter confidence in the vote count have appeared in public opinion surveys since the 2000 election controversy, including in the SPAE. Recently published research by Michael Sances and Charles Stewart examined answers to "voter confidence" questions since 2000.⁶⁰ They find two strong patterns in public confidence about counting votes. First, voter confidence follows the election returns.⁶¹ Answers to questions about confidence in the electoral system are strongly correlated with electoral outcomes—when the Democratic candidate for president wins, Democratic identifiers are much more confident, and vice versa. In the years immediately after the 2000 election, Republicans tended to be more confident than Democrats that their votes were counted as cast; since 2008, Democrats have been more confident.

Second, Sances and Stewart find that voters are more confident about *their* own votes being counted as cast than they are that the votes of others are counted as cast.⁶² The degree of confidence is a declining function of social distance between the survey respondent and the level at which votes are cast. Respondents are overwhelmingly confident that *their own vote* was counted as cast, less likely to believe that votes in their own county were counted as cast, even less likely to believe this of votes counted at the *state level*, and the least likely to express confidence in the quality of the vote count *nationwide*.

This second conclusion is illustrated in Table 5a, which reports the fraction of respondents in the 2012 SPAE who responded that they were "Very confident" their votes were counted as cast in different settings—their own vote, votes in the county, votes in the state, and votes nationwide.⁶³ Among all voters, the percent answering "Very confident" ranges from 61.1% for their own vote to 22.3% for votes nationwide. The Democratic-Republican

Michael W. Sances & Charles Stewart III, Partisanship and Confidence in the Vote Count: Evidence from U.S. National Elections Since 2000, 40 ELECTORAL STUD. 176, 176 (2015).

^{61.} Id. at 180-83.

^{62.} Id. at 179.

^{63.} Respondents who reported that they voted in the 2012 general election were asked "How confident are you that your vote in the General Election was counted as you intended?" 2012 SPAE, supra note 22, at 151. All respondents, regardless of whether they voted, were asked "How confident are you that votes [geographic area] were counted as voters intended," with [geographic area] replaced with "in your county or city," "in [state of residence]," and "nationwide." The response categories in all cases were "Very confident," "Somewhat confident," "Not too confident," and "Not at all confident." *Id.* at 152-53.

differences in responses show a roughly 20-point difference for all levels of vote aggregation, except for votes nationwide, where the gap is only 12.2 points.⁶⁴

	All	Democrats	Republicans	Democrats-Republicans Difference
Your Vote	61.1% (9335)	75.9% (3500)	52.0% (2880)	23.9
Votes in County	48.4% (10,199)	61.3% (3808)	40.9% (3036)	20.4
Votes in State	39.3% (10,199)	51.8% (3808)	31.6% (3036)	20,2
Votes Nationwide	22.3% (10,199)	35.2% (3807)	12,3% (3036)	22.9

 Table 5

 Confidence That Votes Were Counted as Cast in 2012 General Election

Percentage Answering "Very confident" (N's in parentheses)

	All	Democrats	Republicans	Democrats–Republicans Difference
Your Vote	63.7% (3870)	81.4% (1573)	48.3% (1133)	33.1
Votes in County	51.0% (4200)	67.1% (1710)	38.0% (1185)	29.1
Votes in State	42.7% (4200)	60.1% (1710)	27.5% (1185)	32.6
Votes Nationwide	22.3% (4199)	35.6% (1709)	12.2% (1185)	23.4

b. HAVA minimum states

Percentage Answering "Very confident" (N's in parentheses)

64. 2012 SPAE, supra note 22, at 151-53.

	A11	Democrats	Republicans	Democrats–Republicans Difference
Your Vote	60.8% (721)	68.3% (266)	63.4% (243)	4.9
Votes in County	48.7% (800)	53.0% (260)	54.7% (260)	-1.7
Votes in State	38.6% (800)	40.0% (292)	46.4% (260)	-6.4
Votes Nationwide	20.6% (800)	34.0% (292)	12.2% (260)	21.8

c. Strict photo ID states

Percentage Answering "Very confident" (N's in parentheses)

d. Non-HAVA/non-strict photo ID states

	All	Democrats	Republicans	Democrats-Republicans Difference
Your Vote	58.4% (4744)	70.3% (1661)	53.1% (1504)	17.2
Votes in County	45.6% (5199)	55.6% (1806)	40.6% (1591)	15.0
Votes in State	35.9% (5199)	43.8% (1806)	32.1% (1591)	11.7
Votes Nationwide	21.2% (5200)	34.9% (1806)	12.3% (1591)	22.6
Percentage Answerin	g "Very c	onfident" (N's	in parentheses)	

The question for us is whether voter ID laws have had any effect in improving voter confidence beyond the "winner effect." The answer is "no." In addition, what evidence there is that these laws have had an effect on confidence is in keeping with the other evidence we have presented—ID laws have helped to politicize the issue of voter confidence.

The role of ID laws in helping to further politicize voter confidence is illustrated in the remaining subtables of Table 5. In these subtables, we have divided respondents according to the voter ID regime in the 2012 election. We focus here on the comparison between HAVA-minimum states and strict photo ID states. Voters in the states "in the middle" as far as the strictness of the voter ID regime in 2012 is concerned are also "in the middle" in terms of voter confidence.

First, overall, respondents from both the HAVA-minimum and strict photo ID states had similar levels of confidence at all levels of the vote count; if anything, respondents in the HAVA-minimum states were slightly more confident than respondents in the strict photo ID states. Second, however, Democratic and Republican respondents reported different levels of confidence, depending on the photo ID regime in 2012. (The one exception here is in assessing confidence nationwide.) Democrats in HAVA-minimum states were more confident their votes were counted as cast than Democrats in strict photo ID states.⁶⁵ Conversely, Republicans in strict photo ID states were more confident than Republicans in HAVA-minimum states.⁶⁶

Third, the Democrat-Republican gap in confidence is greater in HAVAminimum states than in the nation as a whole. This is caused by Democrats being a little more confident and Republicans being a little less confident than their copartisans in the rest of the country.

Fourth, the Democrat-Republican gap was much smaller—and in two cases is even negative—in strict photo ID states, for all levels of vote counting from the state level to the personal level. This is due to Democrats being much less confident and Republicans being much more confident in strict photo ID states than their copartisans nationwide.

In Appendix C below, we put these bivariate observations to a multivariate test. Consistent with Sances and Stewart,⁶⁷ we find that by far the most important predictor of whether a respondent believes votes were counted as cast, at all levels of government, is whether the candidate from the respondent's party won the popular vote in the respondent's state. After controlling for the "winner's effect," there is generally only a weak and nonsignificant relationship between the stringency of ID laws and a belief that votes were counted as cast. (The one exception is that the negative regression coefficient is statistically significant in the case of one's own vote being counted as cast.) Furthermore, the interaction between partisanship and the stringency of ID laws tends to be negative and statistically significant, with one exception (concerning the vote nationwide). In other words, at the level of one's own vote and the counting of votes in the voter's own county and state, Democrats are less confident as voter ID laws become more stringent.

III. Knowledge of Voter Identification Laws and Beliefs About Their Effects

Thus far we have established that the passage of strict voter ID laws has not been associated with a heightened degree of confidence in election

^{65.} This difference is statistically significant in a t-test at a p-value of less than .0005.

^{66.} This difference is statistically significant in a t-test at a p-value of less than .0005.

^{67.} Sances & Stewart, supranote 60, at 180-83.

outcomes or a decline in the belief that voter fraud is common. Findings such as these raise questions about what voters know about their states' ID laws and what they expect them to accomplish.

The 2015 YouGov survey probed why people support photo ID laws and what effects those laws might have on the electorate. Large majorities reported believing that photo ID requirements combat election fraud (71%), prevent noncitizens from voting (67%), improve election administration (58%), and make elections fair (63%). Large majorities also felt that photo ID laws would not create barriers to voting. Only 28% felt that photo ID laws would make lines longer on Election Day; 25% felt that photo ID laws would make it harder for minorities to vote; 25% felt these laws would make it harder for poor people to vote; and 20% felt these laws would make it harder for the average person to vote. These results are reported in Table 6.

(i	Yes	No	Don't Know/ Skipped
Combats Election Fraud	70.8%	15.5%	13.6%
Makes Lines Longer on Election Day	28.4%	48.2%	23.4%
Makes Lines Shorter on Election Day	21.2%	49.2%	29.6%
Makes It Harder for Minorities to Vote	24.5%	57.2%	18.3%
Makes It Harder for Democrats to Vote	15,4%	66.2%	18.4%
Makes It Harder for the Average Person to Vote	20.1%	65.5%	14.5%
Makes It Harder for Poor People to Vote	25.1%	59.1%	15.8%
Prevents Noncitizens from Voting	67.3%	15.7%	17.0%
Improves Election Administration	57.6%	20.1%	22.3%
Makes Elections Fair	63.2%	19.2%	17.6%

Table 6	
What Effect Does Photo ID Have on Elections?	

The December 2015 survey also sought to ascertain what identification people think their state's law requires in order to vote. A plurality of people (42%) thought that their state required that people show photo ID in order to vote, and a quarter of respondents thought that their state did not

require showing photo ID. Importantly, a third of respondents (34%) did not know what their state's laws required.⁶⁸

The accuracy of those beliefs can be gauged upon comparing what voters think their state's law requires and what the state's law actually requires. Nineteen states require no document in order to vote. In those states, 30% said that they believe that a photo ID is required to vote and 32% said that it was not. The single largest category of response was "Don't Know," at 38%. At the other extreme are seven states that require showing a photo ID in order to vote. In these states, 57% of respondents say that a photo ID is required; 9% say that no document is required, and a third of respondents (34%) say that they do not know what the state requires.

	Answer to the question: "Does your state require that voters show photo ID at the polls?"			
State ID requirement	Yes	No	Don't Know/ Skipped	
No document required	29.37%	32.13%	38.6%	
ID requested; photo not required	50.8%	25.2%	24.0%	
Photo ID requested	47.0%	16.2%	36.8%	
Strict non-photo ID	49.8%	32.2%	18.0%	
Strict photo ID	57.3%	8.9%	33.7%	

 Table 7

 Knowledge of State Photo ID Requirement⁶

Put simply, considerable confusion about ID laws exists among the American public. Roughly a third of all people are not sure what ID is required, a finding that is as true in states with no document required as it is in states where photo ID is required. In states that require photo ID in order to vote, only 57% of people know that is the law, while 43% either do not know or say that no such documentation is required. This finding that voters are unfamiliar with their states' voter ID laws is consistent with previous research that suggested that poll workers themselves are often unsure of the laws, or at least implement voter ID laws inconsistently.⁷⁰

^{68.} See supra note 55.

For information about states' voter ID requirements, see NCSL Voter ID History Website, supra note 47.

See Lonna Rae Atkeson et al., A New Barrier to Participation: Heterogeneous Application of Voter Identification Policies, 29 ELECTORAL STUD. 66, 71-72 (2010); Charles Stewart III, Voter ID: Who Has Them? Who Shows Them?, 66 OKLA. L. REV. 21, 30-32 (2013).

Conclusion

The United States now has a decade's worth of experience with photo identification requirements for voting. Although the Supreme Court upheld a photo ID requirement against a facial constitutional challenge,⁷¹ with each new law comes new legal challenges. For the most part, litigation over such laws has focused on how severe the burden of producing a photo ID will be and for how many potential voters. However, when the evidence of actual fraud is elusive (as it usually is) and extent of the burden inconclusive, advocates turn to the vague state interest in promoting public confidence in elections as a makeweight constitutional argument.

This Article confirms what we found when we first entered this particular fray. The public continues to support photo ID requirements, remains concerned about voter fraud, and believes that photo ID laws will combat fraud. However, when we compare states with and without such laws, we find that the presence of a photo ID requirement does not affect the public's belief in the frequency of voter fraud, nor does it promote voter turnout. Or at least, the first decade's worth of experience with such laws has not yet demonstrated that they do.

Over that same period, though, we have witnessed the politicization of opinions on voter ID. Republicans (especially more informed ones) have become slightly more supportive of such laws, while Democrats (especially more informed ones) have become more opposed. In this respect, opinion on voter ID has become much like opinions on other subjects: as elite discussion of the issue has become more salient and partisan, the mass public has taken those cues, which become reflected in public attitudes on voter ID.

Like most issues of election administration, though, the public often knows very little about the legal regime about which they have an opinion. The fact that voters in ID states and in non-ID states very frequently do not know whether their state requires ID offers one reason we should not expect such laws to promote public confidence in elections. Because instances of actual voter impersonation fraud are rare, it would be difficult for anyone to observe the impact of such laws on fraud prosecutions. (At least in theory, photo ID laws, like newly enacted criminal laws that lead to new types of prosecutions, might lead people to observe greater rates of legal violations.) Attitudes about voter fraud, as the data presented here suggest, have deeper ideological or political roots, which remain unaffected by a state's election law regime. Low levels of knowledge about existing voter ID laws or even the frequency of fraud, therefore, do not affect these deeply held beliefs that feed into voter perceptions of fraud.

^{71.} Crawford v. Marion Cty. Election Bd., 553 U.S. 181, 188-89 (2008).

We return, then, to the admonition we made eight years ago: that the litigation over voter ID should focus on actual fraud and actual burdens on voting. Relying on public perceptions in any constitutional setting seems fraught with dangers⁷²—no one would suggest that public perception of a potentially nonexistent threat would justify relaxing constitutional speech, religion, or criminal rights, for example. The same should be true with voting. Because public attitudes on voter fraud are unaffected by the stringency of a voter ID law, such laws cannot be justified on that basis.

^{72.} See Nathaniel Persily & Kelli Lammie, Perceptions of Corruption and Campaign Finance: When Public Opinion Determines Constitutional Law, 153 U. PA. L. REV. 119, 122 (2004) (discouraging reliance on public opinion in campaign finance jurisprudence); see also Stephen Ansolabehere & Nathaniel Persily, Testing Shaw v. Reno: Do Majority-Minority Districts Cause Expressive Harms?, 90 N.Y.U. L. REV. 1041, 1042 (2015) (rejecting public opinion basis for concerns about effects of majority-minority districts on racial attitudes).

Appendix A

Multivariate Linear Regression Analysis of Support for Photo ID Laws

Indepe	ndent Variables	All Coeff. (s.e.)	High-information Coeff. (s.e.)	Low-information Coeff. (s.e.)
High	Information	-0.046 (0.009)	-	—
1	Democrat	-0.166 (0.013)	-0.184 (0.017)	-0.120 (0.021)
Imper	sonation fraud*			
0	ccasionally	-0.059 (0.018)	-0.057 (0.021)	-0.054 (0.031)
In	frequently	-0.110 (0.018)	-0.123 (0.022)	-0.093 (0.031)
Al	most Never	-0.300 (0.016)	-0.291 (0019)	-0.272 (0.029)
	Not Sure	-0.084 (0.017)	-0.078 (0.021)	-0.082 (0.029)
1	Ideology*			
	Liberal	0.083 (0.017)	0.075 (0.019)	0.031 (0.034)
1	Moderate	0.265 (0.016)	0.315 (0.018)	0.139 (0.032)
C	onservative	0.351 (0.018)	0.441 (0.021)	0.157 (0.035)
Very	Conservative	0.376 (0.021)	0.452 (0.023)	0.184 (0.041)
	Not Sure	0.274 (0.022)	0.326 (0.035)	0.149 (0.037)
	Race*			
	Black	0.010 (0.014)	-0.015 (0.019)	0.003 (0.020)
	Hispanic	0.031 (0.015)	0.056 (0.020)	0.002 (0.025)
	Asian	-0.001 (0.034)	0.108 (0.042)	-0.119 (0.058)
Nati	ive American.	-0.084 (0.058)	0.130 (0.070)	-0.380 (0.101)
	Mixed	-0.069 (0.030)	-0.053 (0.041)	-0.105 (0.044)
	Other	0.104 (0.039)	0.175 (0.043)	-0.064 (0.077)
Mi	ddle Eastern	-0.163 (0.266)	-0.599 (0.328)	0.179 (0.438)
E	ducation*			

	Table A1
DDO	ort for Requiring Photo ID in Order to Vote, Data from SPAE 2014

1485

Independent Variables	All Coeff. (s.e.)	High-information Coeff. (s.e.)	Low-information Coeff. (s.e.)
High School	0.046 (0.025)	0.097 (0.035)	0.002 (0.037)
Some College	0.032 (0.025)	0.079 (0.035)	-0.014 (0.039)
2-year Degree	0.010 (0.027)	0.055 (0.038)	-0.022 (0.041)
4-year Degree	-0.015 (0.026)	0.041 (0.035)	-0.073 (0.040)
Post-Graduate Degree	-0.036 (0.027)	0.004 (0.036)	-0.027 (0.044)
Constant	0.726 (0.033)	0.583 (0.043)	0.861 (0.055)
N	9237	5807	3430
R ²	.28	.39	.12

* Omitted categories:

Impersonation fraud: "it is common" Ideology: "very liberal" Race: "white"

Education: "No high school"

Appendix B

Multivariate Analysis of Attitudes About Impersonation Fraud

Independent Variables	All Coeff. (s.e.)	Strict Photo ID States Coeff. (s.e.)	HAVA Minimum States Coeff. (s.e.)
Photo ID law*			alara a sana 20 milang kanalara kata atau kata
Strict Photo ID State	0.020 (0.031)	-	-
HAVA Minimum State	0.024 (0.017)	-	-
Democrat	-0.093 (0.028)	-0.099 (0.070)	-0.101 (0.029)
Ideology*			
Liberal	-0.041 (0.028)	-0.149 (0.100)	-0,013 (0,033)
Moderate	0.061 (0.031)	-0.076 (0.099)	0.108 (0.032)
Conservative	0.133 (0.038)	-0.008 (0.140)	0.197 (0.031)
Very Conservative	0.192 (0.042)	0.080 (0.103)	0.263 (0.071)
Not Sure	0.071 (0.042)	0.062 (0.102)	0.103 (0.067)
Race*		PROPERTY OF A CONTRACT OF A	
Black	0.050 (0.025)	0.056 (0.051)	0.037 (0.042)
Hispanic	0.060 (0.027)	0.127 (0.020)	0.054 (0.044)
Asian	-0.035 (0.082)	0.298 (0.212)	-0.113 (0.050)
Native American	0.333 (0.095)	0.395 (0.126)	0.147 (0.195)
Mixed	0.068 (0.067)	0.228 (0.221)	0.028 (0.073)
Other	0.195 (0.080)	0.421 (0.200)	0.245 (0.119)
Middle Eastern	-0.155 (0.062)	-	-0.233 (0.038)

Table B1 Opinion About Impersonation Fraud Being Frequent

Education*

Independent Variables	All Coeff. (s.e.)	Strict Photo ID States Coeff. (s.e.)	HAVA Minimum States Coeff. (s.e.)	
High School	-0.012 (0.054)	-0.230 (0.096)	-0.068 (0.083)	
Some College	-0.017 (0.065)	-0.289 (0.115)	0.096 (0.103)	
2-year Degree	0.036 (0.060)	-0.234 (0.135)	0.127 (0.095)	
4-year Degree	-0.013 (0.058)	-0.270 (0.105)	0,081 (0.088)	
Post-Graduate Degree	-0.041 (0.058)	-0.335 (0.091)	0.081 (0.076)	
Constant	0.202 (0.073)	0.576 (0.162)	0.095 (0.096)	and the state of
N	9252	1416	3474	
R2	.06	.09	.08	1

* Omitted categories:

Photo ID law: neither HAVA minimum nor strict photo Ideology: "very liberal" Race: "white" Education: "No high school"

Appendix C

Multivariate Analysis of Voter Confidence and Stringency of ID Laws

	Own Vote	County Vote	State Vote	National Vote
	Coeff. (s.e.)	Coeff. (s.e.)	Coeff. (s.e.)	Coeff. (s.e.)
Democrat	0.211	0.200	0.205	0.181
	(0.014)	(0.019)	(0.019)	(0.016)
Candidate Won State	0,152	0.190	0.214	0.043
	(0.011)	(0.010)	(0.010)	(0.008)
Law Stringency	-0.010	-0.002	-0.007	-0.005
	(0.005)	(0.005)	(0.005)	(0.004)
Democrat × Law	-0.032	-0.016	-0.024	0.008
Stringency	(0.032)	(0.008)	(0.007)	(0.006)
Constant	0.473	0.331	0.243	0.137
	(0.012)	(0.012)	(0.012)	(0.010)
N	9335	10,199	10,199	10,199
R ²	.08	.08	.09	.06

Table C1									
Confidence That	Votes	Were Counted	as Cast,	2012					

CERTIFICATE OF SERVICE

I, Matthew Prairie Gordon, hereby certify that I have served true and accurate copies of the foregoing Affidavit - Affidavit in Support to the following on 04-06-2022:

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> Electronically Signed By: Matthew Prairie Gordon Dated: 04-06-2022

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Attorneys for Plaintiffs Montana Democratic Party and Mitch Bohn

IN THE MONTANA THIRTEENTH JUDICIAL DISTRICT COURT YELLOWSTONE COUNTY

Montana Democratic Party, Mitch Bohn,

Plaintiffs,

WESTERN NATIVE VOICE, Montana Native Vote, Blackfeet Nation, Confederated Salish and Kootenai Tribes, Fort Belknap Indian Community, and Northern Cheyenne Tribe,

Plaintiffs,

Montana Youth Action; Forward Montana Foundation; and Montana Public Interest Research Group

Plaintiffs,

v.

Christi Jacobsen, in her official capacity as Montana Secretary of State,

Defendant.

Consolidated Case No. DV 21-0451

DECLARATION OF MATTHEW GORDON

I, Matthew Gordon, declare as follows:

My name is Matthew Gordon. I am over 18 years old and am an attorney with the law firm of Perkins Coie LLP. I am admitted to practice law in the State of Montana and am an attorney for Plaintiffs Montana Democratic Party and Mitch Bohn in this matter. I submit this declaration to provide the Court with true and correct copies of certain documents submitted in connection with Plaintiffs' Combined Response to Defendant's Motion for Summary Judgment in this matter.

1. Exhibit 1 is a true and correct copy of MIT Election Data + Science Lab, Voter Confidence (April 2021), https://electionlab.mit.edu/research/voter-confidence.

2. Exhibit 2 is a true and correct copy of the Expert Rebuttal Report of Dr. Alex Street.

3. Exhibit 3 is a true and correct copy of the NPR/PBS NewsHour/Marist Poll of 1,209 National Adults, downloaded from: https://maristpoll.marist.edu/wp-content/uploads/2021/10/NPR_PBS-NewsHour_Marist-Poll_USA-NOS-and-Tables B 202110251104.pdf (last visited Apr. 5, 2022).

4. Exhibit 4 is a true and correct copy of Andrew C. Eggers, Haritz Garro & Justin Grimmer, No Evidence for Systematic Voter Fraud: A Guide to Statistical Claims About the 2020 Election, 118 PNAS 1 (2021), downloaded from:

https://www.pnas.org/doi/pdf/10.1073/pnas.2103619118.

5. Exhibit 5 is a true and correct copy of Amanda Zoch, *Then & Now: How 8 Election Policies Have Changed Since 2000*, Nat'l Conference of State Legislatures (Feb. 16, 2021), downloaded from: https://www.ncsl.org/research/elections-and-campaigns/then-and-nowelection-policies-in-2000-and-2020-magazine2021.aspx/.

6. Exhibit 6 is a true and correct copy of Wendy Weiser, Justin Levitt, Catherine Weiss, & Spencer Overton, Response to the Report of the 2005 Commission on Federal Election Reform 2, 7 (2005), downloaded from: https://www.brennancenter.org/sites/default/files/2019-08/Report_Response%20to%20the%20Report%20of%20the%202005%20Commission%20on% 20Federal%20Election%20Reform.pdf.

- 2 -

Exhibit 7 is a true and correct copy of Enrico Cantoni & Vincent Pons, Strict ID
 Laws Don't Stop Voters: Evidence from a U.S. Nationwide Panel: 2008-2018, Q.J. Econ. 2615, 2653-54 (2021), downloaded from: https://doi.org/10.1093/qje/qjab019.

Exhibit 8 is a true and correct copy of the Expert Rebuttal Report of Dr. Kenneth
 R. Mayer submitted in this matter.

9. Exhibit 9 is a true and correct copy of Michael G. DeCrescenzo & Kenneth R. Mayer, *Voter Identification and Nonvoting in Wisconsin—Evidence from the 2016 Election*, 18 Election L.J. 342, 342 (2019), downloaded from:

https://www.liebertpub.com/doi/10.1089/elj.2018.0536.

10. Exhibit 10 is a true and correct copy of John Kuk, Zoltan Hajnal, & Nazita
Lajevardi, A Disproportionate Burden: Strict Voter Identification Laws and Minority Turnout,
Pol. Groups, & Identities 1 (2020), downloaded from:

https://doi.org/10.1080/21565503.2020.1773280.

11. Exhibit 11 is a true and correct copy of Bernard L. Fraga & Michael G. Miller,
Who Does Voter ID Keep from Voting?, 84 J. Pol. 1 (2022), downloaded from:
https://www.journals.uchicago.edu/doi/10.1086/716282.

12. Exhibit 12 is a true and correct copy of Justin Grimmer & Jesse Yoder, *The Durable Differential Deterrent Effects of Strict Photo Identification Laws*, Pol. Sci. R. & Methods 1 (2021), downloaded from: https://doi.org/10.1017/psrm.2020.57.

13. Exhibit 13 is a true and correct copy of Matt A. Barreto, Gabriel R. Sanchez, and Hannah L. Walker, *Battling the Hydra: the Disparate Impact of Voter ID Requirements in North Dakota*, J. Race, Ethnicity, & Pol. 1 (2022), downloaded from:

https://doi.org/10.1017/rep.2022.1.

14. Exhibit 14 is a true and correct copy of the Expert Report of Barry C. Burden submitted in *Andrew Goodman Foundation v. Bostelmann*, No. 19-cv-955 (Jan. 20, 2020).

15. Exhibit 15 is a true and correct copy of Charles Stewart III et al., *Revisiting Public Opinion on Voter Identification and Voter Fraud in an Era of Increasing Polarization*, 68
Stan. L. Rev. 1455 (2016).

16. Exhibit 16 is a true and correct copy of Nat'l Public Radio, *Here's Why Concerns About Absentee Ballot Fraud are Overhyped* (Oct. 20, 2020),

https://www.pbs.org/wgbh/frontline/article/heres-why-concerns-about-absentee-ballot-fraud-areoverhyped/ (last accessed Mar. 23, 2022).

17. Exhibit 17 is a true and correct copy of Keith Schubert, '*Practically a unicorn*': *Profs say voter fraud allegations in Phillips Co. not part of larger issue*, Daily Montanan (Feb. 14, 2022), downloaded from: https://dailymontanan.com/2022/02/14/practically-a-unicorn-profssay-voter-fraud-allegations-in-phillips-co-not-part-of-larger-issue/.

 Exhibit 18 is a true and correct copy of Lisa Baumann, *Ending Election Day* registration sees little support, Associated Press (Oct. 19, 2014), downloaded from: https://www.greatfallstribune.com/story/news/local/2014/10/19/ending-election-day-registrationsees-little-support/17583087/.

19. Exhibit 19 is a true and correct copy of U.S. Election Assistance Commission, *Election Crimes: An Initial Review and Recommendations for Future Study* 9 (Dec. 2006), downloaded from:

https://www.eac.gov/sites/default/files/eac_assets/1/6/Initial_Review_and_Recommendations_fo r_Further_Study.pdf.

20. Exhibit 20 is a true and correct copy of Steven H. Huefner, Daniel P. Tokaji, Edward B. Foley, and Nathan A. Cemenska, *From Registration to Recounts: The Election Ecosystems of Five Midwestern States* 120 (2007).

21. Exhibit 21 is a true and correct copy of Steven F. Huefner, Nathan A. Cemenska, Daniel P. Tokaji, and Edward P. Foley, *From Registration to Recounts Revisited: Developments in the Election Ecosystems of Five Midwestern States* 41 (2011). 22. Exhibit 22 is a true and correct copy of Michael W. Sances and Charles Stewart III, *Partisanship and Confidence in the Vote Count: Evidence from U.S. National Elections Since* 2000, Electoral Studies 40:176-188 (2015), downloaded from: https://doi.org/10.1016/j.electstud.2015.08.004.

23. Exhibit 23 is a true and correct copy of Stephen Ansolabehere, *Effects of Identification Requirements on Voting: Evidence from the Experiences of Voters on Election Day*, PS: Political Science & Politics 42:127-130 (2009), downloaded from: https://doi.org/10.1017/S1049096509090313.

24. Exhibit 24 is a true and correct copy of Shaun Bowler, Thomas Brunell, Todd Donovan, and Paul Gronke, *Election Administration and Perceptions of Fair Elections*, Electoral Studies 38:1-9 (2015), downloaded from: https://doi.org/10.1016/j.electstud.2015.01.004.

25. Exhibit 25 is a true and correct copy of Keila Szpaller, *Election Security Bill Heads to Gov. Gianforte's Desk* (Apr. 27, 2021), downloaded from: https://dailymontanan.com/2021/04/27/election-security-bill-heads-to-gov-gianfortes-desk/.

26. Exhibit 26 is a true and correct copy of Sam Wilson, *GOP in Missoula Pays for Recount to Ease Fraud Concerns* (Mar. 29, 2022), downloaded from: https://missoulian.com/news/state-and-regional/govt-and-politics/gop-in-missoula-pays-forrecount-to-ease-fraud-concerns/article 0304fa52-a9c0-5502-ad63-78fa2938af19.html.

27. Exhibit 27 is a true and correct copy of Alex Sakariassen, *Missoula County GOP to Republican Election Skeptics: 'No Voter Fraud*' (Apr. 1, 2022), downloaded from: https://montanafreepress.org/2022/04/01/missoula-election-allegations-challenged/.

I declare under penalty of perjury that the foregoing is true to the best of my knowledge and belief.

Dated this 5th day of April, 2022.

Nati

Matthew Gordon

Exhibit 16

Here's Why Concerns About Absentee Ballot Fraud Are Overhyped

Share:



Vote-by-mail ballots received by the Miami-Dade County Elections Department had been sorted into accepted and rejected piles due to signature discrepancies on October 15, 2020. A new investigation finds that concerns about widespread absentee ballot fraud in the 2020 presidential election are unfounded. (Joe Raedle/Getty Images)



OCTOBER 20, 2020

- by · Pat Beall (https://www.pbs.org/wgbh/frontline/person/pat-beall/)
 - Catharina Felke (https://www.pbs.org/wgbh/frontline/person/catharina-felke/)
 - Sarah Gelbard (https://www.pbs.org/wgbh/frontline/person/sarah-gelbard/)
 - Jackie Hajdenberg (https://www.pbs.org/wgbh/frontline/person/jackie-hajdenberg/)
 - Elizabeth Mulvey (https://www.pbs.org/wgbh/frontline/person/elizabeth-mulvey/)
 - Aseem Shukla (https://www.pbs.org/wgbh/frontline/person/aseem-shukla/)

We analyzed a conservative foundation's catalog of absentee ballot fraud and found no credible threat to the 2020 election.

 EDITOR'S NOTE: This story is part of an ongoing investigation by FRONTLINE MENU

 (https://www.pbs.org/wgbh/frontline/), Columbia Journalism Investigations

 (https://journalism.columbia.edu/columbia-journalism-investigations">and USA TODAY Network

 (http://voteraccess.usatoday.com)
 reporters that examines allegations of voter disenfranchisement and how the pandemic could impact turnout. It includes the film Whose Vote Counts

 (https://www.pbs.org/wgbh/frontline/film/whose-vote-counts/), premiering on PBS and online Oct. 20 at 10 p.m.

 EST/9 p.m. CST.

Leila and Gary Blake didn't want to miss elk hunting season.

It was 2000, and the election conflicted with their plans (https://billingsgazette.com/news/state-andregional/wyoming/pair-admits-guilt-to-voting-fraud/article_3bf66f52-a167-522b-83fc-5797314a3e11.html), so the Wyoming couple requested absentee ballots.

But the Blakes had moved from 372 Curtis Street five miles down the road to 1372 Curtis Street, crossing a town line. When they mailed their votes using the old address, they were criminally charged. The misdemeanor case was settled with \$700 in fines and a few months' probation, but two decades later, the Blakes are still listed as absentee ballot fraudsters in the Heritage Foundation's Election Fraud Database.

Far from being proof of organized, large-scale vote-by-mail fraud, the Heritage database presents misleading and incomplete information that overstates the number of alleged fraud instances and includes cases where no crime was committed, an investigation by USA TODAY, Columbia Journalism Investigations and the PBS series FRONTLINE found.

Although the list has been used to warn against a major threat of fraud, a deep look at the cases in the list shows that the vast majority put just a few votes at stake.

The database is the result of a years-long passion project by Hans von Spakovsky, a former member of the U.S. Department of Justice during the George W. Bush administration and a senior legal fellow with the Heritage Foundation, a conservative think tank. The entire Election Fraud Database contains 1,298 entries of what the think tank describes as "proven instances of voter fraud." It has been amplified by <u>conservative media stars</u> (<u>https://www.youtube.com/watch?reload=9&v=2EuhJ3L_VT4</u>) and was submitted to the White House document archives as part of a failed effort to prove that voter fraud ran rampant during the 2016 election.

But the Blakes' address violation is typical of the kind of absentee ballot cases in the database. It appears along with widows and widowers who voted for a deceased loved one, voters confused by recent changes to the law and people never convicted of a crime.

The Heritage database does not include a single example of a concerted effort to use absentee ballot fraud to steal a major election, much less a presidential election, as <u>President Donald Trump has suggested</u> (<u>https://www.politifact.com/factchecks/2020/aug/24/donald-trump/donald-trump-says-joe-biden-can-only-win-rigged-el/</u>) could happen this year. Though Trump has repeatedly claimed that absentee ballot fraud is wide-

spread, only 207 of the entries in the Heritage database are listed under the fraudulent absentee ballot category. Not only is that a small slice of the overall Heritage database, it represents an even smaller portion of the number of local, state and national elections held since 1979, which is as far back as the database goes.

To examine the facts behind the rhetoric, reporters looked at each case in Heritage's online category of "Fraudulent use of absentee ballots," comparing them with state investigations, court documents and news clips. Roughly one in 10 cases involves a civil penalty and no criminal charge. Some of the cases, such as the one involving the Blakes, do not match the online definition of absentee fraud

(https://www.heritage.org/voterfraud/search#key) as stated by the Heritage Foundation itself. Four cases did not involve absentee ballots at all, including a 1996 murder-for-hire case that included a person persuaded to illegally vote (https://www.documentcloud.org/documents/7244840-Oliveros-PC-Affidavit-1.html#document/p1/a585977) using a wrong address.

Read: 2020 Election Could Hinge on Whose Votes Don't Count (https://www.pbs.org/wgbh/frontline/article/2020-election-could-hinge-on-whose-votes-dont-count/)

In recent months, von Spakovsky has cited the database to warn about the dangers of voting by mail, including during podcast interviews with U.S. Rep. Dan Crenshaw and former U.S. House Speaker Newt Gingrich.

In a <u>written response (https://cji-usat.s3.us-east-2.amazonaws.com/HF-response-1018.pdf)</u> for this story, von Spakovsky — the manager of the Heritage Foundation's Election Law Reform Initiative — called the database "factual, backed up by proof of convictions or findings by courts or government bodies in the form of reports from reputable news sources and/or court records."

He acknowledges that the database is elastic enough to pull in civil cases, as well as criminal cases closed with no conviction. "Some suffered civil sanctions. Others suffered administrative rebukes," von Spakovsky said (https://cji-usat.s3.us-east-2.amazonaws.com/HF-response-1018.pdf). In the case of criminal convictions, the database "does not discriminate between serious and minor cases." Charges listed in the description "add the necessary context," he wrote.

Even with such a broad definition, the Brennan Center for Justice in its <u>2017 examination of the full database</u> (<u>https://www.brennancenter.org/our-work/research-reports/heritage-fraud-database-assessment</u>) found scant evidence supporting claims of significant, proven fraud. It did conclude the cases added up to "a molecular fraction" of votes cast nationwide. Von Spakovsky has countered that the database is a sampling of cases that have publicly surfaced.

"We simply report cases of which we become aware," he said.

But if the Heritage database is a sample, it points to a larger universe of cases that are just as underwhelming.

"It illustrates that almost all of the voting fraud allegations tend to be small scale, individual acts that are not calculated to change election outcomes," said Rick Hasen, an election law author and professor of law and political science at the University of California, Irvine. To be sure, there are exceptions. In North Carolina, a Republican political consultant was indicted and the results of a 2018 congressional race overturned (https://www.npr.org/2019/07/30/746800630/north-carolina-gop-opera-tive-faces-new-felony-charges-that-allege-ballot-fraud) based on an absentee ballot operation.

"But by and large the allegations are penny-ante," Hasen said. "Some are not crimes at all."

Relatively Small Number of Votes at Stake

Following <u>unsubstantiated claims (https://www.nytimes.com/2017/01/25/us/politics/trump-voting-fraud-false-claim-investigation.html)</u> that "millions and millions" of fraudulent votes cast in the 2016 election had cost him the popular vote, Trump in 2017 created the <u>Presidential Advisory Commission on Election Integrity</u> (https://www.nytimes.com/2017/05/11/us/politics/trump-voter-fraud.html) to investigate stories of voter fraud.

Joining the panel was von Spakovsky, whose appointment was considered controversial. In an email obtained by the <u>Campaign Legal Center (https://www.documentcloud.org/documents/7244299-Campaign-Legal-Ctr-</u> <u>Complaint-Von-Spakovsky-Email.html</u>), he urged that Democrats should be barred from the task force, arguing they would obstruct the panel's work. He also wrote, of moderate Republicans: "There aren't any that know any-thing about this or who have paid attention to the issue over the years." He submitted the Heritage database almost immediately (https://www.whitehouse.gov/articles/presidential-advisory-commission-election-integrity-resources-2/) into the commission's official documents.

The task force disbanded seven months after its first meeting with no report substantiating fraud. The <u>White</u> <u>House blamed (https://www.whitehouse.gov/briefings-statements/statement-press-secretary-presidential-advi-</u> <u>sory-commission-election-integrity/)</u> the potential cost of lawsuits and uncooperative states for the failure to produce evidence of widespread voter fraud.

A review of the absentee cases in the Heritage Foundation database helps explain why the panel came up short, and why such fraud is not a reasonable threat to undermine the 2020 general election.

In multiple instances, only one or two votes were involved. In other cases, no fraudulent votes were involved but are still included in the database because people ran afoul of rules on helping others fill out ballots or ballot requests. For example, a <u>nursing home worker (https://www.documentcloud.org/documents/7037975-Peeps-</u> <u>Corrigan-Eva-CT-2002-Commission-Summary.html)</u> was civilly fined \$100 because she did not sign her name and address as an "assistor" on ballots she helped four elderly patients fill out. In another case, a mother was fined \$200 because she signed her sons' <u>requests for absentee ballots</u>

(https://www.documentcloud.org/documents/7037973-Peeps-Stevenson-Lillian-CT-Commission-Findings.html).

Events in the database also can be older than they seem because Heritage frequently categorizes entries by dates of an indictment, report or conviction, which may come years after the fraud. Using the year of the incident, 137 of 207 cases occurred before 2012.

Overall, the total number of absentee cases in the Heritage Foundation database is 153, with 207 entries in the category because multiple people are sometimes listed for the same case. Of those cases, 39 of them — involving 66 people — represent cases in which there seemed to be an organized attempt to tip an election, based on re-

MENU

Further, the database describes "cases," not individuals charged. However, the total number of cases became inflated after Heritage began counting every person involved in a criminal ring as a separate case.

"Each individual is a separate case and involved different ... acts of voter fraud," even if the parties conspired, von Spakovsky said. The Heritage Foundation may reconsider how groups of defendants are counted, but if anything, he said, the number of cases is undercounted, not overcounted.

But the details of the cases compiled in the database undermine the claim that voter fraud is a threat to election integrity.

In Seattle, an elderly widow and a widower appeared in court the same day, having voted for their recently deceased spouses — two of 15 in the database where an individual cast the ballot of a recently deceased parent, wife or husband. "The motivation in these cases was not to throw an election," the prosecutor of the Seattle case told the Seattle Post-Intelligencer (https://www.seattlepi.com/local/article/Two-who-voted-for-dead-spousesare-fined-250-1175087.php). "The defendants are good and honorable people."

Lorraine Minnite, a Rutgers University political science professor who has written extensively on voter behavior, said of the Heritage Foundation database: "They slapped it together.

"They must have thought people would not think about it in a deep way," Minnite said. "They can just slam it on the desk, say some number. The context and accuracy goes out the window."

Andrea "Andy" Bierstedt was accused of taking one ballot belonging to another voter to the post office in a 2010 Texas sheriff's race. Campos said prosecutors allowed her to donate \$3,500 to the county food bank as part of a plea. She wrote the check and she has no conviction. Yet she's in the database.

"This database is really saying that I'm guilty when even the courts say I'm not guilty," said Bierstedt, who did not know her name was on a compilation of voter fraud cases. "It's slander."

Read: How to Run a Primary in a Pandemic: Michigan Clerks Get a Crash Course (https://www.pbs.org/wgbh/frontline/article/primary-election-michigan-covid-pandemic/)

Others captured in the database stumbled on changes in law. Providing assistance, such as the delivery of an absentee ballot, <u>had been legal in 2003 in Texas (https://www.myplainview.com/news/article/Hardeman-County-</u> <u>Commissioner-given-probation-8621777.php</u>), and in 2004, that's what Hardeman County Commission candidate Johnny Akers did. "I didn't understand you couldn't mail some little old lady's ballot," Akers <u>told the Wichita Falls</u> <u>Times Record News (https://www.myplainview.com/news/article/Hardeman-County-Commissioner-given-proba-</u> tion-8621777.php).

After Brandon Dean won the Brighton, Alabama, 2016 mayor's race, a losing candidate sued over absentee ballots.

"<u>This isn't about voting fraud (https://www.wbrc.com/story/36102643/this-isnt-about-voter-fraud-judge-ex_{MENU} plains-brighton-election-trial-on-final-day/)</u>," the judge in the civil trial said. Ballots rejected by the judge for apparent voter mistakes triggered a runoff, and Dean declined to run.

Dean's case, however, appears in the Heritage database.

Percy Gill's re-election to the Wetumpka, Alabama, town council the same year also prompted a rival to sue, and a civil judge also <u>overturned the election (https://www.wsfa.com/story/36203315/court-declares-former-we-</u> <u>tumpka-city-councilor-true-winner-of-2016-election/)</u> because of defective absentee ballots. <u>Gill died last year</u> (https://www.al.com/news/montgomery/2019/03/percy-gill-former-wetumpka-councilman-businessman-anddemocratic-party-leader-dies-of-stroke.html).

"I don't know why they put him on the [Heritage] database," said his friend Michael Jackson, the District Attorney for Alabama's Fourth Judicial District. "He was a very honest man, an upstanding official."

'It Wasn't Anything Big to Begin With'

The Heritage voter fraud database correctly notes that Miguel Hernandez was arrested as part of a larger voting fraud investigation in the Dallas area.

Hernandez, who pleaded guilty to improperly returning a marked ballot in a city council election, had knocked on voters' doors, volunteered to request absentee ballots on their behalf, signed the requests under a forged name and then collected ballots for mailing.

But Heritage did not include the fact that the investigation went nowhere. Voters told prosecutors their mailed votes were accurately recorded.

"It did not materialize into anything bigger simply because it wasn't anything big to begin with," said Andy Chatham, a former Dallas County assistant district attorney who helped prosecute Hernandez. "This was not a voter fraud case."

Yet according to the Heritage Foundation's fraud database, Hernandez's scheme involved up to 700 ballots.

"Absolutely hilarious," said Bruce Anton, Hernandez's defense attorney. "There is no indication that anything like that was ever, ever considered."

The legend of Hernandez's activities grew even more when U.S. Attorney General William Barr recently held Hernandez out as an example of fraud, boosting the number of ballots. "We indicted someone in Texas, 1,700 ballots collected, he — from people who could vote, he made them out and voted for the person he wanted to."

The Department of Justice had not indicted Hernandez. A spokesperson <u>told reporters</u> (https://www.washingtonpost.com/national-security/barr-claims-a-man-collected-1700-ballots-and-filled-themout-as-he-pleased-prosecutors-say-thats-not-what-happened/2020/09/03/923aafac-ee2e-11ea-ab4e-581edb849379_story.html) Barr had been given inaccurate information.

Where Fraud Exists, the System to Catch It Works

While fewer and farther between, legitimate absentee fraud is also reflected in the database. Ben Cooper and 13 other individuals faced 243 felony charges in 2006 in what was described as Virginia's worst election fraud in half a century. The mayor of tiny Appalachia, Cooper and his associates stole absentee ballots and bribed voters with booze, cigarettes and pork rinds so that they could repeatedly vote for themselves.

But the case is an example of just how difficult it is to organize and execute absentee fraud on a scale significant enough to swing an election while also avoiding detection. Heritage's compilation of known absentee cases show the schemes repeatedly occurred in local races, frequently in smaller towns where political infighting can be fierce and fraudsters easily identified. Just one voter who told her story to *The Roanoke Times* unraveled Cooper's ring.

The idea that absentee fraud frequently involves few votes and is easily caught is "laughable," <u>von Spakovsky</u> <u>said (https://cji-usat.s3.us-east-2.amazonaws.com/HF-response-1018.pdf)</u>. He cited as an example the 1997 Miami mayoral race, which was riddled with absentee fraud.

However, that fraud scheme also quickly collapsed: The election took place in November, the *Miami Herald* began exposing the fraud in December, a civil trial started in February and a judge overturned the election in March.

Read: <u>As Trump And Biden Battle</u>, Election Officials Are Running Out of Time, Money For November (<u>https://www.pbs.org/wgbh/frontline/article/covid-voting-mail-in-ballots-election-officials-running-out-of-</u> time-money-for-november/)

"There have been some ham-handed attempts in small scale fraud, but I would be very surprised to see large scale efforts that go undetected," Hasen said. "It is very hard to fly under the radar."

The Heritage database also illustrates an aggressive system capable of catching and harshly punishing violators. When a Washington State woman registered her dog and put his paw print on an absentee ballot, she risked felony charges. Forging his ex-wife's name on her ballot earned the former head of the Colorado Republican Party four years on probation.

"The mechanisms to safeguard the integrity of the vote are in place in every jurisdiction in the country," said Chatham, the former Texas prosecutor. "Anybody who says differently hasn't done the research that I have. They haven't done the research at all and they just want to believe in conspiracy theories."

USA TODAY NETWORK reporters Zac Anderson, Joey Garrison, Jimmie Gates, Frank Gluck, Eric Litke, Brian Lyman, Will Peebles and Katie Sobko contributed to this report.

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As part of our investigation with the AP into potential Russian war crimes in Ukraine, here's a look at what constitutes a war crime and what options exist for bringing those responsible to justice.

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Exhibit 17

Government & Politics

'Practically a unicorn': Profs say voter fraud allegations in Phillips Co. not part of larger issue

Two women in Phillips County have been accused of falsifying voter registration forms

By: Keith Schubert - February 14, 2022 12:00 pm



The Montana Capitol in Helena, Montana. The building was built in 1899, and an addition completed in 1911. Eric Seidle For the Daily Montanan.

4/5/22, 5:31 PM

Two allegations of voter fraud in Phillips County are not indicative of widespread election insecurity but instead reinforce the dependability of current election systems, two political scientists said this week.

The charges came to light after a series of conflicting reports from the Secretary of State's Office, which originally reported in a news release Friday before last incorrect information that the women at the center of the charges were arrested and pleaded guilty. Secretary of State Christi Jacobsen's office subsequently corrected the release to indicate the women had pleaded not guilty.

"People have looked so hard for (voter fraud) and found so little, (this case) should reassure people that our elections are already run with so much security and integrity," said Jeremy Johnson, associate professor of political science at Carroll College.

After the election, complaints from Dodson residents led to an investigation of the women's voting records.

On Jan. 11, the Phillips County Sheriff's Office referred misdemeanor Deceptive Election Practices charges against Grace O. Albia and Jannet Benitez Zeta for prosecution. Both women are Philippine citizens in the United States on work visas, making them ineligible to vote in U.S. elections. Allegations from the Phillips County Attorney say the women claimed to be U.S. citizens on voter registration forms.

According to a press release from the Secretary of State's Office, the two women voted in the 2021 mayoral race in Dodson, which was won by only two votes. Both women pleaded not guilty to the charges at their initial appearances and have omnibus hearings set for Feb. 23, according to the district justice court clerk.

Contrary to original reports from the Secretary of State's Office, the Phillips County Attorney is investigating the case and not Attorney General Austin Knudsen.

Given the small scale of the election, it's unlikely that this is a case of malicious manipulation of the voting system in the way that Republicans have been alleging in recent years, said Paul Pope, an associate professor of political science at Montana State University-Billings.

"Usually, instances of voter fraud have minimal impact on the election," he said. "If this were a county-wide election, it probably would not be enough to change the election. I am almost certain the election officials dropped the ball in this case, and situations where this kind of fraud happens are incredibly rare, practically a unicorn."

Phillips County Clerk and Recorder Lynnel LaBrie declined to comment on how the women were able to vote in the election without definitely proving they were U.S. Citizens but said her office "follows the guidance from the Secretary of State and Montana State Statute" when administering elections. As of now, she said the canvas stands pending the outcome of the court cases.

Both professors said cases of voter fraud in the country and Montana are almost non-existent — a point demonstrated by Phillips County Sheriff Jerry Lytle, who told Lee Newspapers this was the <u>first time</u> he has cited someone for voter fraud in his 18 years as sheriff.

"Most of these kinds of cases don't even result in charges because they are usually flagged before it gets to this point," Pope said. "And regardless of the circumstance, it can be corrected."

According to records shared with the Daily Montanan, the votes cast by the women were counted in the nonpartisan municipal mayoral race in Dodson — a town of around 100 people — where incumbent Mayor Angel Arocha defeated Glenn Dolphay 21-19. The same records show that their registrations have since been canceled.

Election officials in Montana can provisionally register voters if they do not have the necessary documentation proving their identity at the time of registering. When this occurs, the voter's registration is flagged until they provide the required documentation.

The Legislative Chair of the Montana Association of Clerk and Recorders, Regina Plettenberg, told <u>Lee Newspapers</u> that provisionally registered voters can still receive a provisional mail-in ballot, but said ballots cast by those voters are normally flagged by election administrators and do not factor into the final vote tallies until proper documentation is verified.

While voter fraud cases may be rare, Johnson said the issue has been used by Republicans to justify narrowing voting opportunities.

"We have heard so many worries about voting fraud from Republicans, I don't think they are going to stop talking about it," he said. "So much discussion in recent years about voting fraud is totally separated from the actual facts on the ground because there has been so much scrutiny, and it's been so rare."

The charges come as GOP lawmakers push for a special committee to investigate the 2020 election results, despite instances of voter fraud in Montana being scarce. In October, 86 of Montana's 98 Republican lawmakers signed a <u>letter pushing party leadership in</u> the House and Senate to appoint a special committee to investigate the 2020 election — where Montana Republicans swept top offices — citing conspiracies of widespread voter fraud.

<u>Rep. Brad Tschida, R-Missoula</u>, who is one of the authors of the letter, has long pointed to unfounded allegations of discrepancies with mail-in ballots in his county by Tschida. Missoula County Commissioners have <u>remained steadfast</u> that the 2o20 election went off without a hitch, and allegations levied by Tschida are baseless.

In a text message to the Daily Montanan, Sen. Theresa Manzella, R-Hamilton, another sponsor of the letter, said the case in Dodson is an example of the state's challenge law being put to good use as well as an example of an election security shortcoming.

"It's my opinion that we need a more comprehensive and seamless way of confirming citizenship in the critical infrastructure controls on our election process," she wrote. "This vulnerability, as well as a number of others we've identified, is the reason we've asked that a Special Legislative Committee be appointed to review our election laws and process."

She continued saying that she and others want to "evaluate the certification process of the tabulators to be sure it's comprehensive enough for today's technology."

4/5/22, 5:31 PM

Manzella has been a loud voice in calling for an investigation into the state's 2020 election results and was one of six state GOP lawmakers to attend an August "cyber symposium" hosted by right-wing conspiracy theorist and MyPillow CEO Mike Lindell in South Dakota. The Montana Free Press reported in December that Members of Attorney General Austin Knudsen's office also met with Lindell in November, while he was crusading for attorneys general across the country to challenge the 2020 elections results at the U.S. Supreme Court, which has <u>denied hearing</u> multiple cases on the subject.

But Johnson and Pope said this case shows the opposite of what people like Manzella and Lindell are calling for.

"This case proves we already have plenty of election security," Pope said. "The level of voter fraud is not even a quarter of 1% percent nationwide. "Even with zero change to existing law or process, you are not going to see this kind of situation like we have seen in Dodson on any level that will have an impact other than in a small race in a small town."

A post-election audit conducted in 2020 by former Republican Secretary of State Corey Stapleton found<u>no evidence</u> of widespread fraud in Montana's elections, something that has been reinforced by court rulings and officials at multiple levels of government. Still, the GOP made it a priority during the 67th Legislature to pass laws they deemed necessary to further secure election integrity in the state, prompted in part by Secretary of State Jacobsen.

With GOP support, the Legislature passed a handful of laws regulating elections like restricting ballot collection efforts, ending same-day voter registration, and limiting political activity on college campuses. In the wake of the session, union groups, the Democratic Party and others challenged five of the laws in various district courts. A Lewis and Clark County District judge ruled last week that the provision in Senate Bill 319 that limited political activity at colleges was <u>unconstitutional</u>.

And, Johnson said, GOP politicians may use this case to rally their base further: "Anything (related to voter fraud) captures people's attention right now ... politicians are priming people, and the whole issue is being moved by not real facts on the ground."

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Exhibit 18

Ending Election Day registration sees little support

greatfallstribune.com/story/news/local/2014/10/19/ending-election-day-registration-sees-little-support/17583087/

Lisa Baumann, Great Falls Tribune



Lisa Baumann | AP

HELENA – A referendum on the November ballot could repeal Election Day voter registration, but voters haven't seen one television ad, mailer or person mobilize in favor of the measure.

The only noise is coming from a group against the measure and they've thrown money and manpower at urging people to vote no.

If the legislative referendum appearing on the ballot as LR 126 passes, people could not register to vote on Election Day in future elections. The voter registration deadline would move to 5 p.m. on the Friday before Election Day.

"All Montanans should have their voices heard in democracy and LR 126 is one of those efforts to take away that voice," said Kate Stallbaumer, deputy campaign manager with Montanans for Free and Fair Elections. "We're focused on protecting and safeguarding the constitutional right to vote."

The group started organizing after the 2013 Legislative session when the Republican-led Legislature voted to put the issue on this year's ballot. Making it a referendum instead of a bill sidestepped a potential veto by Democratic Gov. Steve Bullock.

Many affiliated with Montanans for Fair and Free Elections have worked hard in recent years to thwart similar legislative bills, Stallbaumer said.

The group is using social media, television ads, mailers and people knocking on doors in 17 towns to get their message out, she said.

The American Civil Liberties Union of Montana, Montana Women Vote, AARP and Western Native Voice are among the more than two dozen groups supporting the efforts. Most of the groups are based in Montana although a few such as the Natural Resources Defense Council are national with ties to the state, Stallbaumer said.

At least a dozen groups have contributed with money, time or other in-kind donations, according to the group's filings with the state Commissioner of Political Practices. The ACLU, MEA-MFT, AFSCME, Northern Plains Resource Council, Western Native Voice, Montana Public Interest Research Group and Montana Conservation Voters have given the most in amounts ranging from \$15,000 to just over \$45,000.

Those supportive of the measure haven't organized or spent any money on promoting their views, according to the main sponsor of the referendum, Republican Sen. Alan Olson of Roundup.

"There isn't any organized effort to pass it," he said. "I just thought let the voters decide."

Olson said he's somewhat disappointed by the efforts of Montanans for Free and Fair Elections.

"If people sit down and pay attention to what it really does, instead of listening to the hype on TV where veterans and ranchers and little old ladies can't vote, I think people should make the right decision," he said. You're shortening it (voter registration) up by two business days. That's it."

Rosebud County Clerk and Recorder and Election Administrator Geraldine Custer agrees with Olson.

"We really do not have time to be registering people walking through the door," she said. "It's a lengthy process."

Montana is one of 10 states plus the District of Columbia that allow voter registration on Election Day, according to the National Conference of State Legislatures.

None of the states that have it have repealed it.

The Montana bill that included Election Day voter registration passed in 2005 with only two votes against it, according to Secretary of State and Chief Elections Officer Linda McCulloch, who appears in an ad created by Montanans for Free and Fair Elections.

"Virtually everyone supported it," she said. "Election Day voter registration is the ultimate failsafe."

Online

View the Tribune's Voters' Guide at www.greatfallstribune.com/news/montana-politics/

Exhibit 19



Election Crimes: An Initial Review and Recommendations for Future Study

December 2006





EXECUTIVE SUMMARY

The Help America Vote Act of 2002 (HAVA) requires the U.S. Election Assistance Commission (EAC) to study a host of topics, including "voting fraud" and "voter intimidation." In 2005, EAC embarked on an initial review of the existing knowledge of voting fraud and voter intimidation. The goal of that study was to develop a working definition of "voting fraud" and "voter intimidation" and to identify research methodology to conduct a comprehensive, nationwide study of these topics.

EAC staff along with two, bipartisan consultants reviewed the existing information available about voting fraud and voter intimidation, including reading articles, books and reports; interviewing subject matter experts; reviewing media reports of fraud and intimidation; and studying reported cases of prosecutions of these types of crimes. It is clear from this review that there is a great deal of debate on the pervasiveness of fraud in elections as well as what constitute the most common acts of fraud or intimidation. There is also no apparent consensus on the meaning of the phrases "voting fraud" and "voter intimidation." Some think of voting fraud and voter intimidation only as criminal acts, while others include actions that may constitute civil wrongs, civil rights violations, and even legal activities.

In order to facilitate future study of these topics, EAC developed a working definition of "election crimes." "Election crimes" are intentional acts or willful failures to act, prohibited by state or federal law, that are designed to cause ineligible persons to participate in the election process; eligible persons to be excluded from the election process; ineligible votes to be cast in an election; eligible votes not to be cast or counted; or other interference with or invalidation of election results. Election crimes generally fall into one of four categories: acts of deception, acts of coercion, acts of damage or destruction, and failures or refusals to act.

From EAC's review of existing information on the issue, it was apparent that there have been a number of studies that touched on various topics and regions of the country concerning voting fraud and intimidation, but that there had never been a comprehensive, nationwide study of these topics. EAC will conduct further research to provide a comprehensive, nationwide look at "election crimes." Future EAC study of this topic will focus on election-related, criminal activity and will not include acts that are exclusively civil wrongs, campaign finance violations, and violations of ethical laws and regulations. EAC will study these concepts by surveying the states' chief election officials about complaints they received, election crime investigation units regarding complaints received and those referred to law enforcement, and law enforcement and prosecutorial agencies regarding complaints received, charges filed, and final disposition of each complaint.



INTRODUCTION

Voting fraud and voter intimidation are phrases familiar to many voting-aged Americans. However, they mean different things to different people. Voting fraud and voter intimidation are phrases used to refer to crimes, civil rights violations, and, at times, even the lawful application of state or federal laws to the voting process. Past study of these topics has been as varied as its perceived meaning. In an effort to help understand the realities of voting fraud and voter intimidation in our elections, the U.S. Election Assistance Commission (EAC) has begun this, phase one, of a comprehensive study on election crimes. In this phase of its examination, EAC has developed a working definition of election crimes and adopted research methodology on how to assess the existence and enforcement of election crimes in the United States.

PURPOSE AND METHODOLOGY OF THE EAC STUDY

Section 241 of the Help America Vote Act of 2002 (HAVA) calls on the EAC to research and study various issues related to the administration of elections. During Fiscal Year 2006, EAC began projects to research several of the listed topics. These topics for research were chosen in consultation with the EAC Standards Board and Board of Advisors. Voting fraud and voter intimidation are topics that the EAC as well as its advisory boards felt were important to study to help improve the administration of elections for federal office.

EAC began this study with the intention of identifying a common understanding of voting fraud and voter intimidation and devising a plan for a comprehensive study of these issues. The initial study was not intended to be a comprehensive review of existing voting fraud and voter intimidation actions, laws, or prosecutions. To conduct that type of extensive research, a basic understanding had to first be established regarding what is commonly referred to as voting fraud and voter intimidation. Once that understanding was reached, a definition had to be crafted to refine and in some cases limit the scope of what reasonably can be researched and studied as evidence of voting fraud and voter intimidation. That definition will serve as the basis for recommending a plan for a comprehensive study of the area.

To accomplish these tasks, EAC employed two consultants, Job Serebrov and Tova Wang,¹ who worked with EAC staff and interns to conduct the research that forms the basis of this report. The consultants were chosen based upon their experience with the topic and the need to assure a bipartisan representation in this study. The consultants and EAC staff were charged with (1) researching the current state of information on the topic

¹ Biographies for Job Serebrov and Tova Wang, the two consultants hired by EAC, are attached as Appendix "1".



of voting fraud and voter intimidation; (2) developing a uniform definition of voting fraud and voter intimidation; and (3) proposing recommended strategies for researching this subject.

EAC consultants reviewed existing studies, articles, reports and case law on voting fraud and intimidation and conducted interviews with experts in the field. EAC consultants and staff then presented their initial findings to a working group that provided feedback. The working group participants were:

The Honorable Todd Rokita

Indiana Secretary of State Member, EAC Standards Board and the Executive Board of the Standards Board

Kathy Rogers

Georgia Director of Elections, Office of the Secretary of State Member, EAC Standards Board

J.R. Perez

Guadalupe County Elections Administrator, Texas

Barbara Arnwine

Executive Director, Lawyers Committee for Civil Rights under Law Leader of Election Protection Coalition

Benjamin L. Ginsberg

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Barry Weinberg

Former Deputy Chief and Acting Chief, Voting Section, Civil Rights Division, U.S. Department of Justice

Technical Advisor: **Craig Donsanto** Director, Election Crimes Branch, U.S. Department of Justice

Throughout the process, EAC staff assisted the consultants by providing statutes and cases on this subject as well as supervision on the direction, scope and product of this research.

The consultants drafted a report for EAC that included their summaries of relevant cases, studies and reports on voting fraud and voter intimidation as well as summaries of the interviews that they conducted. The draft report also provided a definition of voting fraud and intimidation and made certain recommendations developed by the consultants



or by the working group on how to pursue further study of this subject. This document was vetted and edited by EAC staff to produce this final report.

EXISTING INFORMATION ABOUT FRAUD AND INTIMIDATION

To begin our study of voting fraud and voter intimidation, EAC consultants reviewed the current body of information on voting fraud and voter intimidation. The information available about these issues comes largely from a very limited body of reports, articles, and books. There are volumes of case law and statutes in the various states that also impact our understanding of what actions or inactions are legally considered fraud or intimidation. Last, there is anecdotal information available through media reports and interviews with persons who have administered elections, prosecuted fraud, and studied these problems. All of these resources were used by EAC consultants to provide an introductory look at the available knowledge of voting fraud and voter intimidation.

Reports and Studies of Voting fraud and Intimidation

Over the years, there have been a number of studies conducted and reports published about voting fraud and voter intimidation. EAC reviewed many of these studies and reports to develop a base-line understanding of the information that is currently available about voting fraud and voter intimidation. EAC consultants reviewed the following articles, reports and books, summaries of which are available in Appendix "2":

Articles and Reports

- People for the American Way and the NAACP, "The Long Shadow of Jim Crow," December 6, 2004.
- Laughlin McDonald, "The New Poll Tax," *The American Prospect* vol. 13 no. 23, December 30, 2002.
- Wisconsin Legislative Audit Bureau, "An Evaluation: Voter Registration Elections Board" Report 05-12, September, 2005.
- Milwaukee Police Department, Milwaukee County District Attorney's Office, Federal Bureau of Investigation, United States Attorney's Office "Preliminary Findings of Joint Task Force Investigating Possible Election Fraud," May 10, 2005.
- National Commission on Federal Election Reform, "Building Confidence in U.S. Elections," Center for Democracy and Election Management, American University, September 2005.



- The Brennan Center for Justice at NYU School of Law and Spencer Overton, Commissioner and Law Professor at George Washington University School of Law "Response to the Report of the 2005 Commission on Federal Election Reform," September 19, 2005.
- Chandler Davidson, Tanya Dunlap, Gale Kenny, and Benjamin Wise, "Republican Ballot Security Programs: Vote Protection or Minority Vote Suppression – or Both?" A Report to the Center for Voting Rights & Protection, September, 2004.
- Alec Ewald, "A Crazy Quilt of Tiny Pieces: State and Local Administration of American Criminal Disenfranchisement Law," The Sentencing Project, November 2005.
- American Center for Voting Rights "Vote Fraud, Intimidation and Suppression in the 2004 Presidential Election," August 2, 2005.
- The Advancement Project, "America's Modern Poll Tax: How Structural Disenfranchisement Erodes Democracy" November 7, 2001
- The Brennan Center and Professor Michael McDonald "Analysis of the September 15, 2005 Voting Fraud Report Submitted to the New Jersey Attorney General," The Brennan Center for Justice at NYU School of Law, December 2005.
- Democratic National Committee, "Democracy at Risk: The November 2004 Election in Ohio," DNC Services Corporation, 2005
- Public Integrity Section, Criminal Division, United States Department of Justice, "Report to Congress on the Activities and Operations of the Public Integrity Section for 2002."
- Public Integrity Section, Criminal Division, United States Department of Justice, "Report to Congress on the Activities and Operations of the Public Integrity Section for 2003."
- Public Integrity Section, Criminal Division, United States Department of Justice, "Report to Congress on the Activities and Operations of the Public Integrity Section for 2004."



- Craig Donsanto, "The Federal Crime of Election Fraud," Public Integrity Section, Department of Justice, prepared for Democracy.Ru, n.d., at http://www.democracy.ru/english/library/international/eng_1999-11.html
- People for the American Way, Election Protection 2004, Election Protection Coalition, at <u>http://www.electionprotection2004.org/edaynews.htm</u>
- Craig Donsanto, "Prosecution of Electoral Fraud under United State Federal Law," *IFES Political Finance White Paper Series*, IFES, 2006.
- General Accounting Office, "Elections: Views of Selected Local Election Officials on Managing Voter Registration and Ensuring Eligible Citizens Can Vote," Report to Congressional Requesters, September 2005.
- Lori Minnite and David Callahan, "Securing the Vote: An Analysis of Election Fraud," Demos: A Network of Ideas and Action, 2003.
- People for the American Way, NAACP, Lawyers Committee for Civil Rights, "Shattering the Myth: An Initial Snapshot of Voter Disenfranchisement in the 2004 Elections," December 2004.

Books

- John Fund, *Stealing Elections: How Voting Fraud Threatens Our Democracy*, Encounter Books, 2004.
- Andrew Gumbel, *Steal this Vote: Dirty Elections and the Rotten History of Democracy in American*, Nation Books, 2005.
- Tracy Campbell, *Deliver the Vote: A History of Election Fraud, An American Political Tradition – 1742-2004*, Carroll & Graf Publishers, 2005.
- David E. Johnson and Jonny R. Johnson, *A Funny Thing Happened on the Way to the White House: Foolhardiness, Folly, and Fraud in the Presidential Elections, from Andrew Jackson to George W. Bush*, Taylor Trade Publishing, 2004.
- Mark Crispin Miller, Fooled Again, Basic Books, 2005.



During our review of these documents, we learned a great deal about the type of research that has been conducted in the past concerning voting fraud and voter intimidation. None of the studies or reports was based on a comprehensive, nationwide study, survey or review of all allegations, prosecutions or convictions of state or federal crimes related to voting fraud or voter intimidation in the United States. Most reports focused on a limited number of case studies or instances of alleged voting fraud or voter intimidation. For example, "Shattering the Myth: An Initial Snapshot of Voter Disenfranchisement in the 2004 Elections," a report produced by the People for the American Way, focused exclusively on citizen reports of fraud or intimidation to the Election Protection program during the 2004 Presidential election. Similarly, reports produced annually by the Department of Justice, Public Integrity Division, deal exclusively with crimes reported to and prosecuted by the United States Attorneys and/or the Department of Justice through the Public Integrity Section.

It is also apparent from a review of these articles and books that there is no consensus on the pervasiveness of voting fraud and voter intimidation. Some reports, such as "Building Confidence in U.S. Elections," suggest that there is little or no evidence of extensive fraud in U.S. elections or of multiple voting. This conflicts directly with other reports, such as the "Preliminary Findings of Joint Task Force Investigating Possible Election Fraud," produced by the Milwaukee Police Department, Milwaukee County District Attorney's Office, FBI and U.S. Attorney's Office. That report cited evidence of more than 100 individual instances of suspected double-voting, voting in the name of persons who likely did not vote, and/or voting using a name believed to be fake.

Voter intimidation is also a topic of some debate because there is little agreement concerning what constitutes actionable voter intimidation. Some studies and reports cover only intimidation that involves physical or financial threats, while others cover non-criminal intimidation, including legal practices that allegedly cause vote suppression.

One point of agreement is that absentee voting and voter registration by nongovernmental groups create opportunities for fraud. For example, a number of studies cited circumstances in which voter registration drives have falsified voter registration applications or have destroyed voter registration applications of persons affiliated with a certain political party. Others conclude that paying persons per voter registration application creates the opportunity and perhaps the incentive for fraud.

Interviews with Experts

In addition to reviewing prior studies and reports on voting fraud and intimidation, EAC consultants interviewed a number of persons regarding their experiences and research of voting fraud and voter intimidation. Persons interviewed included:



Wade Henderson Executive Director, Leadership Conference for Civil Rights

Wendy Weiser Deputy Director, Democracy Program, The Brennan Center

William Groth Attorney for the plaintiffs in the Indiana voter identification litigation

Lori Minnite Barnard College, Columbia University

Neil Bradley ACLU Voting Rights Project

Pat Rogers Attorney, New Mexico

Nina Perales Counsel, Mexican American Legal Defense and Education Fund

Rebecca Vigil-Giron Secretary of State, New Mexico

Sarah Ball Johnson Executive Director, State Board of Elections, Kentucky

Stephen Ansolobohere Massachusetts Institute of Technology

Chandler Davidson Rice University

Tracey Campbell Author, *Deliver the Vote* **Douglas Webber** Assistant Attorney General, Indiana

Heather Dawn Thompson Director of Government Relations, National Congress of American Indians

Jason Torchinsky Assistant General Counsel, American Center for Voting Rights

Robin DeJarnette Executive Director, American Center for Voting Rights

Harry Van Sickle Commissioner of Elections, Pennsylvania

Tony Sirvello Executive Director International Association of Clerks, Recorders, Election Officials and Treasurers

Joseph Sandler Counsel Democratic National Committee

John Ravitz Executive Director New York City Board of Elections

Sharon Priest Former Secretary of State, Arkansas

Kevin Kennedy Executive Director State Board of Elections, Wisconsin



Evelyn Stratton Justice Supreme Court of Ohio

Joseph Rich Former Director Voting Section, Civil Rights Division U.S. Department of Justice **Craig Donsanto** Director, Public Integrity Section U.S. Department of Justice

John Tanner Chief Voting Section, Civil Rights Division U.S. Department of Justice

These interviews in large part confirmed the conclusions that were gleaned from the articles, reports and books that were analyzed. For example, the interviewees largely agreed that absentee balloting is subject to the greatest proportion of fraudulent acts, followed by vote buying and voter registration fraud. They similarly pointed to voter registration drives by nongovernmental groups as a source of fraud, particularly when the workers are paid per registration. Many asserted that impersonation of voters is probably the least frequent type of fraud because it is the most likely type of fraud to be discovered, there are stiff penalties associated with this type of fraud, and it is an inefficient method of influencing an election.

Interviewees differed on what they believe constitutes actionable voter intimidation. Law enforcement and prosecutorial agencies tend to look to the criminal definitions of voter intimidation, which generally require some threat of physical or financial harm. On the other hand, voter rights advocates tended to point to activities such as challenger laws, voter identification laws, polling place locations, and distribution of voting machines as activities that can constitute voter intimidation.

Those interviewed also expressed opinions on the enforcement of voting fraud and voter intimidation laws. States have varying authorities to enforce these laws. In some states, enforcement is left to the county or district attorney, and in others enforcement is managed by the state's attorney general. Regardless, voting fraud and voter intimidation are difficult to prove and require resources and time that many local law enforcement and prosecutorial agencies do not have. Federal law enforcement and prosecutorial agencies but have limited jurisdiction and can only prosecute election crimes perpetrated in elections with a federal candidate on the ballot or perpetrated by a public official under the color of law. Those interviewed differed on the effectiveness of the current system of enforcement. Some allege that prosecutions are not sufficiently aggressive. Others feel that the current laws are sufficient for prosecuting fraud and intimidation.

A summary of the each of the interviews conducted is attached as Appendix "3".



Case Law and Statutes

Consultants reviewed more than 40,000 cases that were identified using a series of search terms related to voting fraud and voter intimidation. The majority of these cases came from courts of appeal. This is not surprising, since most cases that are publicly reported come from courts of appeal. Very few cases that are decided at the district court level are reported for public review.

Very few of the identified cases were applicable to this study. Of those that were applicable, no apparent thematic pattern emerged. However, it did seem that the greatest number of cases reported on fraud and intimidation have shifted from past patterns of stealing votes to present problems with voter registration, voter identification, the proper delivery and counting of absentee and overseas ballots, provisional voting, vote buying, and challenges to felon eligibility.

A listing of the cases reviewed in this study is attached as Appendix "4".

Media Reports

EAC consultants reviewed thousands of media reports concerning a wide variety of potential voting fraud or voter intimidation, including:

- absentee ballot fraud,
- voter registration fraud,
- voter intimidation and suppression,
- deceased voters on voter registration list and/or voting,
- multiple voting,
- felons voting,
- non-citizens voting,
- vote buying,
- deceptive practices, and
- fraud by election officials.

While these reports showed that there were a large number of allegations of voting fraud and voter intimidation, they provided much less information as to whether the allegations were ever formalized as complaints to law enforcement, whether charges were filed, whether prosecutions ensued, and whether any convictions were made. The media reports were enlightening regarding the pervasiveness of complaints of fraud and intimidation throughout the country, the correlation between fraud allegations and the perception that the state was a "battleground" or "swing" state, and the fact that there were reports of almost all types of voting fraud and voter intimidation. However, these



reports do not provide much data for analysis as to the number of complaints, charges and prosecutions of voting fraud and intimidation throughout the country.

DEFINITION OF ELECTION CRIMES

From this study of available information on voting fraud and voter intimidation, EAC has learned that these terms mean many things to many different people. These terms are used casually to refer to anything from vote buying to refusing to register a voter to falsifying voter registration applications. Upon further inspection, however, it is apparent that there is no common understanding or agreement of what constitutes "voting fraud" and "voter intimidation." Some think of voting fraud and voter intimidation only as criminal acts, while others include actions that may constitute civil wrongs, civil rights violations, and even legal activities. To arrive at a common definition and list of activities that can be studied, EAC assessed the appropriateness of the terminology that is currently in use and applied certain factors to limit the scope and reach of what can and will be studied by EAC in the future. As a result, EAC has adopted the use of the term "election crimes" for its future study.

Current Terminology

The phrase "voting fraud" is really a misnomer for a concept that is much broader. "Fraud" is a concept that connotes an intentional act of deception, which may constitute either a criminal act or civil tort depending upon the willfulness of the act.

Fraud, n. **1.** A knowing misrepresentation of the truth or concealment of a material fact to induce another to act to his or her detriment. • Fraud is usu[ally] a tort, but in some cases (esp. when the conduct is willful) it may be a crime.

Black's Law Dictionary, Eighth Edition, p. 685.

"Voting" is the act of casting votes to decide an issue or contest. Black's Law Dictionary, Eighth Edition, p. 1608. Using these terms to form a definition of "voting fraud," it means fraudulent or deceptive acts committed to influence the act of voting. Thus, a voter who intentionally impersonates another registered voter and attempts to vote for that person would be committing "voting fraud." Similarly, a person who knowingly provides false information to a voter about the location of the voter's polling place commits fraud on the voter.

The phrase "voting fraud" does not capture a myriad of other criminal acts that are related to elections which are not related to the act of voting and/or do not involve an act of deception. For example, "voting fraud" does not capture actions or willful inaction in the voter registration process. When an election official willfully and knowingly refuses



to register to vote a legally eligible person it is a crime. This is a crime that involves neither the act of voting nor an act of deception.

To further complicate matters, the phrases "voting fraud" and "voter intimidation" are used to refer to actions or inactions that are criminal as well as those that are potentially civil wrongs and even those that are legal. Obviously, criminal acts and civil wrongs are pursued in a very different manner. Criminal acts are prosecuted by the local, state or federal government. Generally, civil wrongs are prosecuted by the individual who believes that they were harmed. In some cases, when civil rights are involved, the Civil Rights Division of the Department of Justice may become involved.

New Terminology

The goal of this study was to develop a common definition of what is generically referred to as "voting fraud" and "voter intimidation" that would serve as the basis for a future, comprehensive study of the existence of these problems. Because the current terminology has such a variety of applications and meanings, "voting fraud" and "voter intimidation" can be read to encompass almost any bad act associated with an election. Such broad terminology is not useful in setting the boundaries of a future study. A definition must set parameters for future study by applying limitations on what is included in the concepts to be studied. The current terminology applies no such limitations.

Thus, EAC has adopted the use of the phrase "election crimes" to limit the scope of its future study. This term captures all crimes related to the voter registration and voting processes and excludes civil wrongs and non-election related crimes. EAC adopted this definition because it better represents the spectrum of activities that we are able to and desire to study. In addition, EAC recognizes that the resources, both financial and human capital, needed to study all "voting fraud" and "voter intimidation," including criminal acts, civil actions, as well as allegations of voter suppression through the use of legal election processes are well beyond the resources available to EAC. Finally, by limiting this definition to criminal acts, EAC can focus its study on a set of more readily measurable data. Criminal behavior is readily defined through state and federal statutes and is prosecuted by individuals and/or government entities. Furthermore, what constitutes civil action is far less defined, subject to change, and can vary from case to case. A more complete discussion of the concept of "election crimes" follows along with a list of excluded actions.



The Definition of an Election Crime for Purposes of this Study

Election crimes are intentional acts or willful failures to act, prohibited by state or federal law, that are designed to cause ineligible persons to participate in the election process; eligible persons to be excluded from the election process; ineligible votes to be cast in an election; eligible votes not to be cast or counted; or other interference with or invalidation of election results. Election crimes generally fall into one of four categories: acts of deception, acts of coercion, acts of damage or destruction, and failures or refusals to act.

Election crimes can be committed by voters, candidates, election officials, or any other members of the public who desire to criminally impact the result of an election. However, crimes that are based upon intentional or willful failure to act assume that a duty to act exists. Election officials have affirmative duties to act with regard to elections. By and large, other groups and individuals do not have such duties.

The victim of an election crime can be a voter, a group of voters, an election official, a candidate, or the public in general. Election crimes can occur during any stage of the election process, including but not limited to qualification of candidates; voter registration; campaigning; voting system preparation and programming; voting either early, absentee, or on election day; vote tabulation; recounts; and recalls.

The following are examples of activities that may constitute election crimes. This list is not intended to be exhaustive, but is representative of what states and the federal government consider criminal activity related to elections.

Acts of Deception

- Knowingly causing to be mailed or distributed, or knowingly mailing or distributing, literature that includes false information about the voter's precinct or polling place, the date and time of the election or a candidate;
- Possessing an official ballot outside the voting location, unless the person is an election official or other person authorized by law or local ordinance to possess a ballot outside of the polling location;
- Making or knowingly possessing a counterfeit of an official election ballot;
- Signing a name other than his/her own to a petition proposing an initiative, referendum, recall, or nomination of a candidate for office;
- Knowingly signing more than once for the proposition, question, or candidate in one election;
- Signing a petition proposing an initiative or referendum when the signer is not a qualified voter.
- Voting or attempting to vote in the name of another person;
- Voting or attempting to vote more than once during the same election;



- Intentionally making a false affidavit, swearing falsely, or falsely affirming under an oath required by a statute regarding their voting status, including when registering to vote, requesting an absentee ballot or presenting to vote in person;
- Registering to vote without being entitled to register;
- Knowingly making a materially false statement on an application for voter registration or re-registration; and
- Voting or attempting to vote in an election after being disqualified or when the person knows that he/she is not eligible to vote.

Acts of Coercion

- Using, threatening to use, or causing to be used force, coercion, violence, restraint, or inflicting, threatening to inflict, or causing to be inflicted damage harm, or loss, upon or against another person to induce or compel that person to vote or refrain from voting or to register or refrain from registering to vote;
- Knowingly paying, offering to pay, or causing to be paid money or other thing of value to a person to vote or refrain from voting for a candidate or for or against an election proposition or question;
- Knowingly soliciting or encouraging a person who is not qualified to vote in an election;
- Knowingly challenging a person's right to vote without probable cause or on fraudulent grounds, or engaging in mass, indiscriminate, and groundless challenging of voters solely for the purpose of preventing voter from voting or to delay the process of voting;
- As an employer, attempting by coercion, intimidation, threats to discharge or to lessen the remuneration of an employee, to influence his/her vote in any election, or who requires or demands an examination or inspection by himself/herself or another of an employee's ballot;
- Soliciting, accepting, or agreeing to accept money or other valuable thing in exchange for signing or refraining from signing a petition proposing an initiative;
- Inducing or attempting to induce an election official to fail in the official's duty by force, threat, intimidation, or offers of reward;
- Directly or through any other person advancing, paying, soliciting, or receiving or causing to be advanced, paid, solicited, or received, any money or other valuable consideration to or for the use of any person in order to induce a person not to become or to withdraw as a candidate for public office; and
- Soliciting, accepting, or agreeing to accept money or other thing of value in exchange for registering to vote.



Acts of Damage or Destruction

- Destroying completed voter registration applications;
- Removing or destroying any of the supplies or other conveniences placed in the voting booths or compartments;
- Removing, tearing down, or defacing election materials, instructions or ballots;
- Fraudulently altering or changing the vote of any elector, by which such elector is prevented from voting as the person intended;
- Knowingly removing, altering, defacing or covering any political sign of any candidate for public office for a prescribed period prior to and following the election;
- Intentionally changing, attempting to change, or causing to be changed an official election document including ballots, tallies, and returns; and
- Intentionally delaying, attempting to delay, or causing to be delayed the sending of certificate, register, ballots, or other materials whether original or duplicate, required to be sent by jurisdictional law.

Failure or Refusal to Act

- Intentionally failing to perform an election duty, or knowingly committing an unauthorized act with the intent to effect the election;
- Knowingly permitting, making, or attempting to make a false count of election returns;
- Intentionally concealing, withholding, or destroying election returns or attempts to do so;
- Marking a ballot by folding or physically altering the ballot so as to recognize the ballot at a later time;
- Attempting to learn or actually and unlawfully learning how a voter marked a ballot;
- Distributing or attempting to distribute election material knowing it to be fraudulent;
- Knowingly refusing to register a person who is entitled to register under the rules of that jurisdiction;
- Knowingly removing the eligibility status of a voter who is eligible to vote; and
- Knowingly refusing to allow an eligible voter to cast his/her ballot.

What is not an Election Crime for Purposes of this Study

There are some actions or inactions that may constitute crimes or civil wrongs that EAC does not include in its definition of "election crimes." All criminal or civil violations related to campaign finance contribution limitations, prohibitions, and reporting either at the state or federal level are not "election crimes" for purposes of this study and any



future study conducted by EAC. Similarly, criminal acts that are unrelated to elections, voting, or voter registration are not "election crimes," even when those offenses occur in a polling place, voter registration office, or a candidate's office or appearance. For example, an assault or battery that results from a fight in a polling place or at a candidate's office is not an election crime. Last, violations of ethical laws and regulations and the Hatch Act are not "election crimes." Similarly, civil or other wrongs that do not rise to the level of criminal activity (i.e., a misdemeanor, relative felony or felony) are not "election crimes."

RECOMMENDATIONS ON HOW TO STUDY ELECTION CRIMES

As a part of its study, EAC sought recommendations on ways that EAC can research the existence of election crimes. EAC consultants, the working groups and some of the persons interviewed as a part of this study provided the following recommendations.

Recommendation 1: Conduct More Interviews

Future activity in this area should include conducting additional interviews. In particular, more election officials from all levels of government, parts of the country, and political parties should be interviewed. It would also be especially beneficial to talk to law enforcement officials, specifically federal District Election Officers ("DEOs") and local district attorneys, as well as civil and criminal defense attorneys.

Recommendation 2: Follow Up on Media Research

The media search conducted for this phase of the research was based on a list of search terms agreed upon by EAC consultants. Thousands of articles were reviewed and hundreds analyzed. Many of the articles contained allegations of fraud or intimidation. Similarly, some of the articles contained information about investigations into such activities or even charges brought. Additional media research should be conducted to determine what, if any, resolutions or further activity there was in each case.

Recommendation 3: Follow Up on Allegations Found in Literature Review

Many of the allegations made in the reports and books that were analyzed and summarized by EAC consultants were not substantiated and were limited by the date of publication of those pieces. Despite this, such reports and books are frequently cited by various interested parties as evidence of fraud or intimidation. Further research should include follow up on the allegations identified in the literature review.



Recommendation 4: Review Complaints Filed With "MyVote1" Voter Hotline

During the 2004 election and the statewide elections of 2005, the University of Pennsylvania led a consortium of groups and researchers in conducting the MyVote1 Project. This project involved using a toll-free voter hotline that voters could call for poll locations, be transferred to a local hotline, or leave a recorded message with a complaint. In 2004, this resulted in more than 200,000 calls received and more than 56,000 recorded complaints.

Further research should be conducted using the MyVote1 data with the cooperation of the project leaders. While perhaps not a fully scientific survey given the self-selection of the callers, the information regarding 56,000 complaints may provide insight into the problems voters may have experienced, especially issues regarding intimidation or suppression.

Recommendation 5: Further Review of Complaints Filed With U.S. Department of Justice

According to a recent GAO report, the Voting Section of the Civil Rights Division of the Department of Justice has a variety of ways it tracks complaints of voter intimidation. Attempts should be made to obtain relevant data, including the telephone logs of complaints and information from the Interactive Case Management (ICM) system. Further research should also include a review and analysis of the Department of Justice/Office of Personnel Management observer and "monitor field reports" from Election Day.

Recommendation 6: Review Reports Filed By District Election Officers

Further research should include a review of the reports that must be filed by every District Election Officer (DEO) to the Public Integrity Section of the Criminal Division of the Department of Justice. The DEOs play a central role in receiving reports of voting fraud and investigating and pursuing them. Their reports back to the Department would likely provide tremendous insight into what actually transpired during the last several elections. Where necessary, information could be redacted or made confidential.

Recommendation 7: Attend Ballot Access and Voting Integrity Symposium

Further activity in this area should include attending the next Ballot Access and Voting Integrity Symposium. At this conference, prosecutors serving as District Election Officers in the 94 U.S. Attorneys' Offices obtain annual training on fighting election fraud and voting rights abuses. These conferences are sponsored by the Voting Section of the Civil Rights Division and the Public Integrity Section of the Criminal Division, and



feature presentations by Civil Rights officials and senior prosecutors from the Public Integrity Section and the U.S. Attorneys' Offices. By attending the symposium researchers could learn more about the following: how District Election Officers are trained; how information about previous election and voting issues is presented; and how the Voting Rights Act, the criminal laws governing election fraud and intimidation, the National Voter Registration Act, and the Help America Vote Act are described and explained to participants.

Recommendation 8: Conduct Statistical Research

EAC should measure voting fraud and intimidation using interviews, focus groups, and a survey and statistical analysis of the results of these efforts. The sample should be based on the following factors:

- Ten locations that are geographically and demographically diverse where there have been many reports of fraud and/or intimidation;
- Ten locations (geographically and demographically diverse) that have not had many reports of fraud and/or intimidation;

EAC should also conduct a survey of elections officials, district attorneys, and district election officers. The survey sample should be large in order to be able to get the necessary subsets, and it must include a random set of counties where there have and have not been a large number of allegations.

Recommendation 9: Explore Improvements to Federal Law

Future research should review federal law to explore ways to make it easier to impose either civil or criminal penalties for acts of intimidation that do not necessarily involve racial animus and/or a physical or economic threat.

Recommendation 10: Use Observers to Collect Data on Election Day

Use observers to collect data regarding fraud and intimidation at the polls on Election Day. There may be some limitations to the ability to conduct this type of research, including difficulty gaining access to polling places for the purposes of observation, and concerns regarding how the observers themselves may inadvertently or deliberately influence the occurrence of election crimes.

Recommendation 11: Study Absentee Ballot Fraud

Because absentee ballot fraud constitutes a large portion of election crimes, a stand-alone study of absentee ballot fraud should be conducted. Researchers should look at actual



cases to see how absentee ballot fraud schemes are conducted in an effort to provide recommendations on more effective measures for preventing fraud when absentee ballots are used.

Recommendation 12: Use Risk Analysis Methodology to Study Fraud

Conduct an analysis of what types of fraud people are most likely to commit. Researchers will use that risk analysis to rank the types of fraud based on the "ease of commission" and the impact of the fraud.

Recommendation 13: Conduct Research Using Database Comparisons

Researchers should compare information on databases to determine whether the voter rolls contain deceased persons and felons. In addition, the voter rolls can then be compared with the list of persons who voted to determine whether a vote was recorded by someone who is deceased or if felons are noted as having voted.

Recommendation 14: Conduct a Study of Deceptive Practices

The working group discussed the increasing use of deceptive practices, such as flyers and phone calls with false and/or intimidating information, to suppress voter participation. A number of groups, such as the Department of Justice, the EAC, and organizations such as the Lawyers Committee for Civil Rights, keep phone logs regarding complaints of such practices. These logs should be reviewed and analyzed to see how and where such practices are being conducted and what can be done about them.

Recommendation 15: Study the Use of HAVA Administrative Complaint Procedure as Vehicle for Measuring Fraud and Intimidation

EAC should study the extent to which states are utilizing the administrative complaint procedure mandated by HAVA. In addition, EAC should study whether data collected through the administrative complaint procedure can be used as another source of information for measuring fraud and intimidation.

Recommendation 16: Examine the Use of Special Election Courts

Given that many state and local judges are elected, it may be worth exploring whether special election courts should be established to handle fraud and intimidation complaints before, during, and after Election Day. Pennsylvania employs such a system that could be investigated to determine how well that system is working.



Accepted Recommendations

There has never been a comprehensive, national study that gathered data regarding all claims, charges, and prosecutions of voting crimes. EAC feels that a comprehensive study is the most important research that it can offer the election community and the public. As such, EAC has adopted all or a part of six of the 16 recommendations made by EAC consultants and the working group.

While several of the other recommendations could be used to obtain more anecdotal information regarding election crimes, EAC believes that what is needed is a comprehensive survey and study of the information available from investigatory agencies, prosecutorial bodies and courts on the number and types of complaints, charges and prosecutions of election crimes. Additional media reviews, additional interviews and the use of observers to collect information from voters on Election Day will only serve to continue the use of anecdotal data to report on election crimes. Hard data on complaints, charges and prosecutions exists and EAC should gather and use that data, rather than rely on the perceptions of the media or the members of the public as to what might be fraud or intimidation.

Some of the recommendations are beyond the scope of the current study. While election courts may be a reasonable conclusion to reach after EAC determines the volume and type of election crimes being reported, charged or prosecuted, it is premature to embark on an analysis of that solution without more information. Last, some of the recommendations do not support a comprehensive study of election crimes. While a risk analysis might be appropriate in a smaller scale study, EAC desires to conduct a broader survey to avoid the existing problem of anecdotal and limited scope of information.

In order to further its goal of developing a comprehensive data set regarding election crimes and the laws and procedures used to identify and prosecute them, EAC intends to engage in the following research activities in studying the existence and enforcement of election crimes:

Survey Chief Election Officers Regarding Administrative Complaints

Likely sources of complaints concerning election crimes are the administrative complaint processes that states were required to establish to comply with Section 402 of HAVA. These complaint procedures were required to be in place prior to a state receiving any funds under HAVA. Citizens are permitted to file complaints alleging violations of HAVA Title III provisions under these procedures with the state's chief election official. Those complaints must be resolved within 60 days. The procedures also allow for alternative dispute resolution of claims. Some states have expanded this process to include complaints of other violations, such as election crimes.



In order to determine how many of these complaints allege the commission of election crimes, EAC will survey the states' chief election officers regarding complaints that have been filed, investigated, and resolved since January 1, 2004. In addition, we will seek information about any complaints of fraud or intimidation filed with the election official outside of the administrative complaint procedure. EAC will use the definition of election crimes provided above in this report in its survey so that data regarding a uniform set of offenses will be collected.

Survey State Election Crime Investigation Units Regarding Complaints Filed and Referred

Several chief state election officials have developed investigation units focused on receiving, investigating, and referring complaints of election crimes. These units were established to bolster the abilities of state and local law enforcement to investigate allegations of election crimes. California, New York and Florida are just three examples of states that have these types of units.

EAC will use a survey instrument to gather information on the numbers and types of complaints that have been received by, investigated, and ultimately referred to local or state law enforcement by election crime investigation units since January 1, 2004. These data will help EAC understand the pervasiveness of perceived fraud, as well as the number of claims that state election officials felt were meritorious of being referred to local and state law enforcement or prosecutorial agencies for further action.

Survey Law Enforcement and Prosecutorial Agencies Regarding Complaints and Charge of Voting Crimes

While voters, candidates and citizens may call national hotlines or the news media to report allegations of election crimes, it is those complaints that are made to law enforcement that can be investigated and ultimately prosecuted. Thus, it is critical to the study of election crimes to obtain statistics regarding the number and types of complaints that are made to law enforcement, how many of those complaints result in the perpetrator being charged or indicted, and how many of those charges or indictments result in pleas or convictions.

Thus, EAC will survey law enforcement and prosecutorial agencies at the local, state and federal level to determine the number and types of complaints, charges or indictments, and pleas or convictions of election crimes since January 1, 2004. In addition, EAC will seek to obtain an understanding of why some complaints are not charged or indicted and why some charges or indictments are not prosecuted.



Analyze Survey Data in Light of State Laws and Procedures

Once a reliable data set concerning the existence and enforcement of election crimes is assembled, a real analysis of the effectiveness of fraud prevention measures can be conducted. For example, data can be analyzed to determine if criminal activities related to elections are isolated to certain areas or regions of the country. Data collected from the election official surveys can be compared to the data regarding complaints, charges and prosecutions gathered from the respective law enforcement and prosecutorial agencies in each jurisdiction. The effect and/or effectiveness of provisions such as voter identification laws and challenger provisions can be assessed based on hard data from areas where these laws exist. Last, analyses such as the effectiveness of enforcement can be conducted in light of the resources available for the effort.

CONCLUSION

Election crimes are nothing new to our election process. The pervasiveness of these crimes and the fervor with which they have been enforced has created a great deal of debate among academics, election officials, and voters. Past studies of these issues have been limited in scope and some have been riddled with bias. These are issues that deserve comprehensive and nonpartisan review. EAC, through its clearinghouse role, will collect and analyze data on election crimes throughout the country. These data not only will tell us what types of election crimes are committed and where fraud exists, but also inform us of what factors impact the existence, prevention, and prosecution of election crimes.

Exhibit 20

From Registration to Recounts:

The Election Ecosystems of Five Midwestern States

A Project of *ELECTION LAW @ MORITZ* AT THE OHIO STATE UNIVERSITY MORITZ COLLEGE OF LAW

By Steven F. Huefner, Daniel P. Tokaji, & Edward B. Foley with Nathan A. Cemenska Copyright© 2007 by Steven F. Huefner, Daniel P. Tokaji, and Edward B. Foley. All rights reserved.

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Cover and design by Andrea Reinaker. Select photos taken by Jo McCulty. Photographs made possible courtesy of the Franklin County Board of Elections. amount of evidence available in Wisconsin, makes the state an especially valuable one in which to investigate this claim.

FRAUD AND THE VOTER ID DEBATE

Examination of Wisconsin's system also is especially useful given the fierce debate over voter fraud and the related debate over voter identification. The focal point for concerns about fraud has been the City of Milwaukee, in which there have been media reports of ineligible voting in recent election cycles.⁴⁹ As required by state law,⁵⁰ these matters have been referred to prosecutors, who have engaged in extensive investigations of voter fraud. Concerns about voter fraud have also led some Wisconsin legislators to propose that voters be required to show photo identification in order to have their votes counted.

At the outset, it is helpful to recognize two distinctions. The first distinction is between fraud and mistake. An example of fraud is someone knowingly attempting to vote twice, or a noncitizen intentionally attempting to cast a vote despite knowing that she is not eligible. An example of mistake is an ineligible felon voting in an election, without knowing that state law prohibits him or her from doing so. Under Wisconsin law, such a voter would not be guilty of fraud.⁵¹ The second distinction is between fraud on the part of voters and fraud on the part of insiders such as election officials. An example of voter fraud is people double voting or pretending to be someone else they are not. Insider fraud, by contrast, involves an election official (or someone else with special access) tampering with the voting process. Examples include stuffing ballot boxes or tampering with electronic voting machine's software to alter vote tallies.

There are few states in which allegations of voter fraud have received greater scrutiny than Wisconsin – and few municipalities in which they have received greater attention than the City of Milwaukee. In the course of preparing this report, we spoke to attorneys in the Milwaukee district attorney's office, as well as local and state election officials, in an effort to understand the allegations that EDR leads to increased voter fraud. On the whole, voting fraud is exceedingly rare. Although allegations of voting fraud have been widely publicized in the media, most all of these have evaporated upon closer investigation. We found a handful of documented instances of disenfranchised felons voting, but almost all of these appear to be people who did not know that they were prohibited from voting. Few documented cases of voter fraud exist, and, in the rare instances when it does occur, such fraud is of the "retail" (isolated incidents) rather than the "wholesale" (systemic) variety.⁵² Almost all the documented incidents of ineligible voting, including both fraud and mistake, involve people who are ineligible due to felony convictions.

After allegations of fraud surfaced during the November 2004 election, a joint task force of the Milwaukee County District Attorney's Office, the Milwaukee Police Department, the U.S. Attorney's Office, and the Federal Bureau of Investigation began an inquiry.⁵³ The probe included allegations of double voting and of voting by felons who had not completed probation or parole.⁵⁴ After nearly a year of investigation, the task force found only a handful of isolated cases and no evidence of any broad conspiracy to engage in fraud. The U.S. Attorney's Office ultimately brought fourteen prosecutions for suspected violations in Milwaukee, twelve percent of all federal voting fraud cases
Exhibit 21

A Project of *ELECTION LAW @ MORITZ* at The OHIO STATE UNIVERSITY MORITZ COLLEGE OF LAW





FROM REGISTRATION TO RECOUNTS REVISITED:

Developments in the Election Ecosystems of Five Midwestern States





By Steven F. Huefner, Nathan A. Cemenska, Daniel P. Tokaji, & Edward B. Foley

From Registration to Recounts Revisited: Developments in the Election Ecosystems of Five Midwestern States

A Project of *ELECTION LAW* @ *MORITZ* at THE OHIO STATE UNIVERSITY MORITZ COLLEGE OF LAW

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generated only a handful of prosecutions, and apparently only one that resulted in a felony conviction.¹⁹ These low numbers indicate that unlawful felon voting is rare in Wisconsin.

The standards of a criminal prosecution for election fraud are, of course, different from the question of whether the outcome of an election was tainted by ineligible ballots. But even assuming that all 195 suspected instances of unlawful voting by felons turned out to generate invalid ballots, that number is extremely low in the context of a statewide election of almost 3 million ballots cast. To put it in context, consider a recent report that FairVote has issued on all statewide recounts between 2000 and 2010.²⁰ That report showed that of the 2,884 statewide elections in the U.S. during that decade, only four races had a margin of victory of less than 200 votes, as shown in Table Five below.

Not even the 2008 U.S. Senate election in Minnesota produced a margin below 200, although it came close (225), and had the smallest margin in percentage terms (0.009%). Moreover, it must be remembered that 200 invalid ballots, because they will not all be cast for the same candidate, cannot affect the outcome of an election with a 200-vote margin of victory. Even if the 200 ballots favored one candidate 75%-25%, or 150-50—an extraordinarily lopsided result—the margin of victory would need to be 100, not 200, for the election to be undermined by these ineligible ballots. Therefore, although it is prudent for states to remain watchful for signs of unusually large numbers of ineligible ballots—no state wants to be in the position that Washington was in 2004, where it had some 1,678 invalid ballots dwarfing its 127-vote gubernatorial margin of victory—the evidence from Wisconsin indicates that it remains far from that unfortunate situation.

Moreover, the legislature has examined the possibility of making it legal for paroled felons to vote, which would eliminate this particular type of ineligible ballot altogether.²¹ Although the proposal was rejected, it may reappear in the next legislative session. The recently enacted photo identification law would do nothing about felons illegally voting, as the possession of ID tells nothing about whether one is an ineligible felon. There is no evidence of any serious problem with voter impersonation fraud, the only form of illegal voting that a strict ID law could hope to address. In fact, out of the twenty individuals prosecuted for crimes arising out of the November 2008 election, none of them were accused of impersonating another voter.22

If Minnesota is instructive, another area where allegations of ineligible ballots could be expected is absentee balloting. Absentee ballots, compared to ballots cast on Election Day, are "low-hanging fruit" in an election

State	Year	Office	Margin	Total Votes
Colorado	2000	State Ed. Board	90	1,535,032
Montana	2000	Super. Public Instruction	61	63,207
Vermont	2006	Auditor	102	223,438
Washington	2004	Governor	129	2,746,593

TABLE FIVE: NARROWEST VICTORY MARGINS IN U.S. ELECTIONS, 2000-2010

Exhibit 22

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Partisanship and confidence in the vote count: Evidence from U.S. national elections since 2000^{*}



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ABSTRACT

To what degree are evaluations of political processes affected by political outcomes? In this paper, we explore this question by combining 30 national U.S. surveys from 2000 to 2012, improving on previous analyses in three ways. First, our measure asks directly about the counting of votes, rather than broader democratic processes. Second, we control for endogeneity by comparing the same respondents pre- and post-election, and by comparing respondents whose preferred candidate barely won to those whose candidate barely lost. Third, we reveal previously unknown within-country variation in this effect. We find losers are significantly more likely to believe votes were improperly counted, an effect that has grown over time and that is stronger for more remote levels of government.

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To what degree do election outcomes affect citizens' evaluations about the trustworthiness and fairness of election procedures? This question is of enormous importance for both new and old democracies. In young democracies, the very survival of the regime hinges on the ability of electoral losers to accept unfavorable outcomes (Moehler, 2009; Cantú and Garcia-Ponce, 2015). More established democracies may survive this lack of faith, but will risk gridlock (Dalton, 2004; Hetherington, 2005) and demands for institutional reform. For example, the U.S. Supreme Court ruled that ensuring public perceptions of electoral integrity partially justifies requiring photo identification in order to vote.¹ If such perceptions are influenced by subjective factors, such as the electoral performance of one's preferred candidate, then the stability of democratic institutions is thrown into doubt.

Despite the importance of this question, however, our knowledge of the relationship between electoral outcomes and perceptions of electoral fairness is still limited. While numerous studies have shown that electoral outcomes affect broader measures of institutional legitimacy, such as political trust and faith in the general conduct of elections, there are virtually no studies that directly test for an impact on perceptions of the actual vote count. Further, existing studies overwhelmingly rely on perceptions of electoral fairness using cross-sectional data, and no study has examined variation within a single country. This makes it difficult to separate the effect of voting for the winner from other factors, while also limiting our knowledge of how this effect varies across time and across levels of government.

In this paper, we take advantage of the United States as a unique opportunity for addressing these concerns. Due to the 2000 presidential election debacle and the doubts it raised regarding electoral integrity, U.S. opinion surveys now regularly ask voters whether they believe votes at various levels of government were counted accurately in the most recent national election. Exploiting this fact, we combine answers from nearly 30 surveys between 2000 and 2012, testing whether perceptions of fairness vary with support for the winning candidate over six national elections.

Using these extensive data, we make three contributions to the existing literature. First, these U.S. surveys are unique in that they ask directly about the accuracy of the vote count, rather than the fairness of electoral processes in general. Second, we explicitly control for the potential endogeneity of voting for the winner by using both a panel design, where the same respondents are interviewed before and after elections, and a regression discontinuity





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¹ Crawford v Marion County, 553 U.S. 181 (2008). See also Ansolabehere and Persily (2008).

design, where we exploit the fact that close electoral outcomes are as-if random. Finally, the scope of our data allow us to examine, for the first time, how the effect of winning on fairness perceptions varies across time and levels of government within a single country.

Consistent with past studies, we find there is indeed a consistent relationship between voting for the winning candidate and the degree of confidence expressed in election administration. This relationship is remarkably robust across surveys and estimation strategies, yet also varies as a function of both the electoral context and question wording. While Democrats began the 2000s less confident than Republicans, Democratic victories in the 2006 and 2008 elections caused the partisan gap to narrow considerably, then reverse with Barack Obama's re-election in 2012. We also find that partisan considerations matter more for perceptions of vote counting at the state and national level, where confidence is lower, as opposed to at the local level, where confidence is higher.

The remainder of the paper proceeds as follows. In Section I, we review the previous literature on effect of voting for the winner on attitudes toward institutions. In Section II we describe our data and state our hypotheses. In Section III, we examine national trends in confidence and the winner-loser gap, showing that Democratic and Republican confidence both move as a function of the parties' electoral fortunes. In Section IV, we estimate the aggregate winner effect using both a quasi-difference-in-differences estimator and a regression discontinuity estimator where we exploit close state vote shares as a natural experiment. In Section V we test the robustness of the aggregate patterns to individual-level analysis. First, we conduct a series of cross-sectional regressions in each of our surveys, asking whether the effect is robust to adjusting for demographics and broader political trust. Next, we leverage the panel nature of three of the surveys to estimate the effect of election outcomes on post-election confidence, controlling for confidence prior to the election. Section VI concludes by summarizing our findings, discussing their external validity beyond the U.S., and suggesting future directions for research.

1. Previous literature

A core tenet of democratic theory is that citizens will tolerate and comply with outcomes they disagree with, provided they view the processes that generated those outcomes as fair. Yet for this to work in practice, citizens' evaluations of democratic processes must be independent of political outcomes. For these reasons, the relationship between outcomes and process evaluations in democracies has long been of interest to political scientists. For example, Clarke and Acock (1989) find that participating in an election increases political efficacy among U.S. voters, but only among voters who chose the winning candidate (see also Ginsberg and Weissberg, 1978; Finkel, 1985). Anderson and LoTempio (2002) find further evidence of this effect by examining pre- and postelection measures of political trust in the 1972 and 1996 U.S. presidential elections; Craig et al. (2006), find the relationship between winning and trust holds cross-sectionally when pooling data from 1964 to 2004 (see also Keele, 2005). Outside of the U.S., Nadeau and Blais (1993) reveal evidence of a winner effect in the 1988 Canadian election, surmising that "the most powerful source of consent to the election is winning the election" (562).

Anderson and Guillory (1997) provide evidence that the winner effect generalizes beyond the particular contexts of the U.S. and Canada. Using survey data from eleven European democracies, they show that those who vote for the winner express more satisfaction with democracy than losers in every single country examined. Anderson et al. (2005) extend this analysis, combining data from both North America, advanced European democracies, and new democracies in Eastern Europe and Latin America. They replicate past findings of a legitimacy gap between winners and losers, while also noting that this gap is larger in new and developing democracies as opposed to more established democracies. They argue that the larger gap found in developing democracies is due to these citizens having less experience with democratic losses.

Consistent with the findings of Anderson et al. (2005), other studies of developing democracies have found sizable winner effects. Cho and Bratton (2006) find that Lesotho's transition to a proportional electoral system is associated with a general increase in perceived legitimacy, but that this effect is concentrated among past losers who view the reform as more beneficial to their own party. Moehler (2009) shows that electoral winners exhibit more institutional trust than losers in each of twelve African democracies, and Cantú and Garcia-Ponce (2015) show a similar effect in the 2012 Mexican election. Finally, Maldonado and Seligson (2014) find a winner–loser legitimacy gap in nearly all of the 24 Latin American countries they examine.

While past research has found extensive evidence of a winner-loser gap in numerous countries, there are several open questions that have yet to be addressed. Most importantly, there is a disconnect between conventional measures of "legitimacy" and the actual theoretical construct, namely the perceived fairness of the election. Many of the studies reviewed above measure the winner-loser gap in terms of general trust in government or support for democracy as a political system. Other studies do ask about the conduct of elections in general, but not the counting of votes in particular. For instance, the Comparative Study of Electoral Systems, utilized by Anderson et al. (2005) and many others, asks respondents:

In some countries, people believe their elections are conducted fairly. In other countries, people believe that their elections are conducted unfairly. Thinking of the last election in [country], where would you place it on this scale of one to five where ONE means that the last election was conducted fairly and FIVE means that the last election was conducted unfairly?

While not as imprecise as general trust in government, this question still allows respondents a great deal of leeway in terms of interpretation. For instance, does "the election" refer to the counting of the votes on Election Day, or the broader electoral campaign, which may entail considerations of campaign finance and media systems? If respondents uniformly misinterpret the question, then this may introduce random error, leading to an understatement of the true winner—loser gap. Alternatively, interpretations could vary based on winner—loser status: those who lose the election may read the question as asking about genuinely unfair aspects of the electoral system (for instance, a lack of proportional representation or the use of an electoral college), while winners may answer in terms of vote counts. In this scenario, the actual winner—loser gap will be biased upwards.

A second issue is that the "effect" of voting for the winner may be correlated with other factors that also affect fairness perceptions. For instance, it may be that citizens with low levels of legitimacy tend to support parties that lose (Moehler, 2009, 348). While a handful of past studies attempt to rule out this possibility using panel designs that compare the same respondents before and after an election (Anderson et al., 2005; Anderson and LoTempio, 2002), such designs are the exception rather than the rule. Further, the few panel studies that do exist measure the winner–loser gap using imprecise measures of perceived fairness, as discussed above.

Finally, while several studies have examined how the winner–loser gap varies across countries (e.g., Anderson and LoTempio, 2002; Curini et al., 2012), no study has asked how this gap varies *within* countries, either across time or across levels of government. This latter oversight is particularly striking, given that general trust in government has been declining over time, as well as the importance of subnational politics in numerous federalist democracies. Indeed, while the U.S. may be an outlier in the extent to which it decentralizes election administration, many other countries also administer elections at the subnational level. For instance, each of the 290 Swedish municipalities operates its own election committee that is responsible for, among other duties, counting the votes (Brogren, 2015). Thus while looking at a single country may introduce costs in terms of external validity, it also allows us to generate new knowledge regarding how fairness perceptions vary across time and across levels of government.

2. Data and hypotheses

To improve our understanding of how political outcomes affect perceptions of fairness, we focus on the United States, a country that is particularly well-suited to addressing the three challenges that we discuss above. First, since the 2000 election, surveys in the United States have consistently asked respondents about their perceptions of electoral fairness. Importantly, and unlike existing studies of the winner-loser gap, this question asks voters directly about the fairness of the vote count, rather than the general fairness of the election or the electoral system. A representative version of this question reads, "How confident are you that your vote was counted as intended?" with five response categories ranging from "very confident" to "not at all confident." Unlike existing measures, there is little room for diverging interpretations of this question: voters are being asked directly whether they think their vote is being counted as they intended, and a negative response to this question signals that the voter doubts the integrity of the vote count process.²

The second advantage of studying the U.S. is the wealth of survey data available across time and space, which creates the possibility of using natural experiments to better identify the winner effect. As noted above, panel surveys will allow us to rule out the possibility that the winner—loser gap is a result of selection bias, because the comparison is done within respondents. In addition, the varying electoral fortunes of different parties across states allows us to perform a regression discontinuity design, comparing fairness perceptions among respondents whose preferred candidate barely won or barely lost in their state. Together, the panel and discontinuity designs provide powerful tests of whether the winner—loser gap represents a causal effect.

Finally, our data are advantageous for examining how the winner—loser gap varies at the subnational level. For one, we are looking just within one country, which eliminates comparability concerns that would arise were we to compare subnational institutions across countries. For another, U.S. surveys typically ask voters both about their confidence that their own vote was counted as intended, as well as their confidence that votes *around the country* were counted as intended; other surveys also ask voters to evaluate the vote count in their state. By comparing responses to these questions, we can test how the winner—loser gap varies depending on whether voters are asked about electoral integrity in their own locality versus other parts of the country.

As in existing studies of the winner effect (e.g. Anderson and

Tverdova, 2001; Anderson et al., 2005), we theorize that the winner—loser gap results from losers' desire to reduce cognitive dissonance: my preferred candidate is obviously the best, therefore there must be something flawed about the vote count if my favored candidate loses. Thus, our first hypothesis is that voting for the winning candidate will have a positive effect, relative to voting for the loser, on perceptions that votes were counted as intended. We emphasize that while such an effect would be consistent with the theoretical arguments of prior studies, no existing study has tested whether winning impacts perceptions of the actual vote count.

Second, we believe there will be less potential for cognitive dissonance when voters are asked to evaluate their own polling place relative to polling places elsewhere around the country. For one, they have actually visited their own polling place, and so have more information about what actually occurs there. For another, that their preferred candidate likely did win at their own polling place obviates the need to reconcile the polling place outcome with their own belief about who should have won. Voters are also generally more trusting of local institutions as opposed to more remote institutions (Mutz and Flemming, 1999); thus they will have a harder time convincing themselves that fraud is a real possibility at the local level. For these reasons, voters should have more difficulty convincing themselves that votes were not fairly counted at the state and local level as opposed to the national level. Our second hypothesis is therefore that the winner-loser gap will be larger when voters evaluate the national vote count relative to their local vote count.

3. General patterns in voter confidence

We begin our analysis by studying aggregate responses to voter confidence questions asked in national surveys before and after each U.S. federal election from 2000 to 2012.³ We included these polls based on our own knowledge of polls that have been taken by various organizations over the past decade, augmented by a search of the Roper Center database to find other polls that asked questions about vote counting and confidence.⁴ The Appendix details the sources, the precise questions asked, the sampling frames, and the survey modes. The survey research organizations or projects include New York Times/CBS, the Los Angeles Times, ABC/Washington Post, the Pew Research Center, the National Annenberg Election Study, CNN, Gallup/USA Today, and the Survey of the Performance of American Elections.

In general, there are two ways that polling houses have asked about confidence in the vote count. The first is to ask about the belief concerning whether the respondent's "own" vote—as opposed to votes elsewhere in the country—was counted correctly. The following are representative examples of questions that seek to assess this level of voter confidence:

- Given the kinds of problems that have been reported in Florida, how much confidence do you have that your [2000 presidential] vote was counted properly? (12 November 2000, CBS/N.Y. Times)
- Are you confident that your vote will be counted accurately, or are you doubtful? (1 November 2004, National Annenberg Election Study [NAES])

² Most of the existing research on this survey question has asked what types of election procedures make for more confident voters (Stewart, 2009; Alvarez et al., 2008, 2009; Hall et al., 2009; Atkenson and Saunders, 2007; Stein et al., 2008). For example, Gronke and Hicks (2009) argue that responses to questions such as these on modules of the CCES are associated most strongly with the experience voters had on Election Day casting a ballot.

³ The exception is 2002, when no national survey asked this question.

⁴ We searched the Roper Center iPoll database using the terms "voter and count" and confide". In addition to the Roper database, we also searched the Pew Research Center web site.



Notes: Each point represents an individual poll. Trend lines are calculated using median-spline regressions.

Fig. 1. Voter confidence in the accuracy of the vote count, 2000–2012.

 How confident are you that your vote was accurately counted? (8 August 2004, 12 November 2006, 9 November 2008, 7 November 2010, Pew Research Center)

The second way is to ask about vote counting generally, or nationwide. The following are examples of questions taken from surveys conducted over the past decade that seek to assess voter confidence in the "country's" vote:

- All things considered, do you think we will have an accurate count of the votes in Florida and other close states, or not? (12 November 2000, Pew Research Center)
- How confident are you that, across the country, the votes will be accurately cast and counted in next year's election? (2 December 2007, Gallup/USA Today)
- How confident are you that the votes across the country were accurately counted on Election Day? (31 January 2009, National Annenberg Election Study)

The top panel of Fig. 1 summarizes answers to these two types of

questions across a number of different public opinion polls from 2000 to 2012. The points in this plot are the proportions of respondents who gave the "most confident" answer to the question posed.⁵ The trend lines represent moving averages constructed by median-splines.

The top panel of Fig. 1 reveals one important pattern right away: confidence in one's own vote has generally been about thirty percentage points higher than confidence in the country's vote, with the two series moving in parallel. The second noteworthy feature of the series is its variability: while voters have become much less confidence in the country's vote since 2000, aggregate confidence in one's own vote does not seem to have changed much overall since the 2000 election.

To begin an exploration of the partisan dimension of confidence,

⁵ This answer category was typically "very confident," although in a few cases it was something else, such as "a lot" (CBS/N.Y. Times 12 November 2000).

the middle and bottom panels of Fig. 1 disaggregate the data by party identification of respondents.⁶ Doing so yields four patterns worth noting. First, for both parties, confidence in one's own vote is higher than confidence in the country's vote. Second, as in the plot for all voters, confidence in one's own vote is more stable over time. Third, until the 2008 presidential election, Republicans were more confident about the quality of the vote count than Democrats, both locally and nationwide. Since 2008, both sets of partisans have been much more similar in how they judge election counts. Finally, by 2012 Democrats had surpassed Republicans in their confidence on both measures.

Fourth, despite the fact that the top panel of Fig. 1 paints a picture of relative stability for aggregate confidence, the next two panels reveal that this aggregate stability hides important countervailing shifts among the two partisan sub-aggregates. Across the decade, aggregate confidence in the country's vote declined about 30 percentage points. However, this overall decline is due almost entirely to a forty-point decline among Republicans during this period. The biggest change occurred between 2004 and 2008, which saw a shift, from Republicans being more confidence. There was again a shift in partisan assessments between 2008 (with the election of a Democratic president) and 2010 (when the Republicans regained control of the House) that is masked if we only look at Fig. 1.

4. Aggregate winner effects

We can begin to get a sense of the causal impact of election outcomes by focusing on shifts in the aggregate levels of confidence before and after elections. To do this, we compare the average confidence among winners—those who voted for the winning candidate — and losers — those who voted for the loser — before and after each election. For example, in the 2008 election, we calculate,

$$\begin{split} & \left\{ E \Big[confidence_p \Big| Obama \ voters_p, \ post - election_p \Big] \\ & - E \Big[confidence_p \Big| Obama \ voters_p, \ pre - election_p \Big] \right\} \\ & - \left\{ E \Big[confidence_p \Big| Romney \ voters_p, \ post - election_p \Big] \right\} \\ & - E \Big[confidence_p \Big| Romney \ voters_p, \ pre - election_p \Big] \right\} \end{split}$$

where p indexes polls, and post- and pre-election are defined as 365 days after or before the election date. We construct this estimate for the 2004, 2006, 2008, and 2012 election, and average across elections. As we have only about 18 polls for each confidence measure, we obtain standard errors via the block bootstrap, blocking on polling house. The resulting "quasi-difference-in-differences" estimates are shown in Fig. 2.

Fig. 2 reveals more clearly how aggregate confidence is affected by election results. In the top part of the figure, we see that among those who voted for the winning candidate, confidence in one's own vote increases by about 20 percentage points after an election. Among those who voted for the loser, confidence declines slightly, by about 2 percentage points. Subtracting the second difference from the first gives an overall effect of 0.22, with a standard error of 0.08. In the bottom part of the figure we see a similar, but larger effect for confidence in the country's vote: the estimate is 0.32, with a standard error of 0.09. We therefore find support for both of our hypotheses in these

aggregate data: there is a clear winner effect for confidence in general, and this effect is larger when voters are asked to evaluate the national vote count as opposed to their own polling place.

While these aggregate patterns suggest a causal effect of winning on confidence in the vote count, we cannot definitively establish causality until we examine confidence at the individual level. Before moving to the individual level, however, we present one additional piece of evidence for causality at the aggregate level using a regression discontinuity estimator (Thistlethwaite and Campbell, 1960). In some states, the Democratic candidate just barely won election, while in other states the Democrat barely lost. Because the election outcome is unpredictable and essentially random in these close contests, we can be confident that comparing voters just above and just below the threshold captures the effect of winning or losing.

To perform this test, we focus on one of our surveys in particular, the 2012 Survey of the Performance of American Elections (SPAE). This survey has the advantage of including 200 voters in each state, as well as asking about confidence in one's own vote, the vote count in the state, and the national vote count. We construct average levels of confidence for each party subgroup in each state, and then estimate how state-level confidence varies as a function of Obama's vote share in that state. Specifically, we estimate the change in confidence that occurs when Obama's vote share exceeds the 50% mark. And because we are interested in the party differential, we estimate this jump for both party subgroups, then take the difference in the two jumps as our estimate of the state-level "winner's effect."

Unlike the over-time comparisons in Fig. 2, the variation in winner status occurs at the state level in this context. Thus, in this comparison we would only expect a jump in confidence in the state vote, and smaller or nonexistent jumps for confidence in one's own vote or the national vote. In Fig. 3, we show this expectation is borne out in the data. In the first panel, we plot the average level of confidence in one's own vote, among Republicans and Democrats for each state, on the y-axis; Obama's vote share is plotted on the x-axis. As expected, there is both a slight jump up for Democrats at the 50% mark, and a slight jump down for Republicans; taking the difference in these two discontinuities gives an estimate of the winner effect of 0.13, with a standard error of 0.05 (we calculate standard errors using the block bootstrap, blocking on states). In the middle plot, which describes confidence in the state vote count, we see much larger jumps for confidence, for an overall effect of 0.28 (0.06). Although these first two effects have overlapping confidence intervals, we can reject the hypothesis that the effects are equal (p = 0.01).⁷ Finally, in the last panel, there is no detectable jump for either partisan subgroup, with an overall effect no different from zero (-0.02, SE = 0.03).

5. Micro-level winner effects

To address the possibility that there is an omitted variable that correlates both with vote choice and confidence, we now move to the micro-level. First, we perform a series of cross-sectional analyses where we control for possible confounders. We estimate a linear regression for each of our 28 surveys in which the dependent variable was the same binary voter confidence variable we explored

⁶ We rely on the initial partisanship question asked by each survey house. Thus, "leaners" are not included in these series.

⁷ We test whether the discontinuity estimates are similar using seemingly unrelated regression (Zellner, 1962).



Notes: Plot combines data from surveys around the 2004, 2006, 2008, and 2012 elections, in a 365-day window around each election. Estimates are difference-in-differences, with standard errors in parentheses calculated via block bootstrap (blocking on survey house). There are 21 polls for confidence in own vote, and 16 polls for confidence in the country's vote.

Fig. 2. Effect of election outcomes on national-level confidence: quasi-difference-in-differences estimates.

before.⁸ The primary independent variables of interest are vote choice (1 = voted for Democratic candidate, 0 otherwise). We then add a battery of demographic controls, including education, income, age, gender, race, and state (or region if state is unavailable). All covariates are entered as indicators for each value of the categorical variable, except for age, which is entered linearly. Finally, we also include measures of general trust in government in the four

surveys where this is available.⁹ To account for possible autocorrelation within geographic areas, we cluster standard errors by

⁸ As a reminder, the dependent variable is equal to one if the respondent gave the "most confident" response to the voter confidence items, zero otherwise. We show the results are unchanged when using probit regressions in the Appendix.

⁹ Trust in government is typically measured using the question, "How much of the time do you think you can trust the government in Washington to do what is right – just about always, most of the time, or only some of the time?" The response options were "Just about always", "Most of the time", and "Only some of the time", with some respondents volunteering the "Never" option. We rescaled this measure to lie between 0 and 1, where higher values indicate greater trust in government.



Notes: Effects are calculated as the difference in regression discontinuity estimates for Democratic and Republican voters, using 2012 Democratic voteshare as the forcing variable. Standard errors calculated using block bootstrap, blocking on states.



state.¹⁰ Fig. 4 plots the point estimates and 95% confidence intervals from these regressions for each of the surveys.

Fig. 4 shows that the effect of party on confidence is strongly robust to the inclusion of these individual-level characteristics. Indeed, the pattern is substantively the same as the simple bivariate comparisons included in Fig. 1 previously: Democrats are between 20 and 50 percentage points less confident than Republicans for much of the decade; the gap then narrows, and flips to a 20 point advantage following the 2012 election. These effects are typically precisely estimated, with confidence intervals only crossing zero in one or two cases. Notably, adjusting for trust in government in the surveys where it is available (indicated in the figure with asterisks) has no substantive impact on the estimates.¹¹

Note also that we replicate the aggregate result that the winner effects are generally larger when respondents are asked about confidence in the national vote count relative to their own polling place. While this difference is nearly always in the expected direction, the statistical significance varies across surveys. To the right of the point estimates, we display p-values of two-sided tests of equality of the coefficients.¹² Note that the differences in effects

are strongest and generally significantly in the presidential elections of 2004, 2008, and 2012, but are not significantly different in the midterm elections of 2006 and 2010. We return to this point in the conclusion.

In addition to these cross-sectional regressions, we also conduct individual panel analysis similar to the aggregate difference-indifference results presented in Fig. 2 above. In three of the surveys we collected, the same individuals were asked about their confidence in the vote count before and after the election. These three studies were the 2004 NAES, the 2008 Pew survey, and the 2008 NAES. For each study, we conduct a difference-in-difference analysis similar to that presented in Fig. 2 previously, but now at the individual level. Thus we estimate,

 $\{E[confidence_i | winner, post - election_i]\}$

- $E[confidence_i | losers_i, pre election_i] \}$
- $\{E[confidence_i | winner, post election_i]\}$
- $E[confidence_i | losers voters_p, pre election_i] \}$

separately for each survey, where i indexes individual respondents. We cluster standard errors by survey respondent. Fig. 5 presents the results.

In two of the three elections, we see patterns that reflect the results shown earlier: there is a clear effect of voting for the winner on the perceived fairness of the vote count, and this effect is larger when respondents are asked about the national vote count as opposed to the local vote count. The exception is the first panel, which shows results for the 2004 election. In this election, both winners and losers became more confident in their own vote once the election was over; however, the gain in confidence was actually smaller among those who chose the winner, for an overall effect of -0.05(SE = 0.04). This may be due to a ceiling effect: in 2004, Republicans (whose candidate won) were already about 85% likely to be very confident in the accuracy of the election, and

¹⁰ We choose to account for possible serial correlation using a simple linear model with clustered standard errors. While one could also adjust for this issue using a hierarchical model, this would require additional functional form assumptions that could bias our estimates if not specified correctly (Primo et al., 2007; Steenbergen and Jones, 2002).

¹¹ We include trust in government as a control because it is plausibly related to both the treatment (the election outcome) and the outcome (confidence in the vote count), and so excluding it may bias our estimate of the winner effect. Of course, given that trust and vote count perceptions both tap into the same theoretical construct, it is difficult for us to fully disentangle them using these data. None-theless, we are reassured as to the stability of the winner effect given that the addition of trust as a control has little or no effect on our estimates.

¹² Some surveys (such as the SPAE) ask respondents both questions, while others (such as Pew) use split ballot designs. When the survey asks all respondents both confidence questions, we conduct this test using Seemingly Unrelated Regression (Zellner, 1962); when the survey uses a split ballot design, we interact the question wording with the winner variable.



Notes: Plot shows coefficients from linear probability models where the outcome is the probability of a "very confident" response and the key independent variable is whether the respondent voted for the Democratic candidate in the most recent national election. Regressions also adjust for education, income, age, gender, race, and state (or region if state is unavailable). * indicates that regressions also control for trust in government. All covariates are entered as indicators, except age which is entered linearly. Horizontal lines span 95% confidence intervals constructed from robust standard errors.

Fig. 4. Effect of election outcomes on individual-level confidence: regression estimates.

thus did not have a lot of room to increase. In the 2008 and 2012 elections, in contrast, we see effects of 0.15 (SE = 0.04) and 0.11 (SE = 0.05), respectively, for confidence in one's own vote; and 0.47 (0.01) and 0.15 (0.04) for confidence in the country's vote. Thus, even looking within the same respondents before and after an election, we see the powerful impact that an election outcome has on perceptions of the fairness of the vote count.¹³

6. Discussion and conclusion

In this article, we have examined an important topic in democratic governance: the conditions under which voters believe that votes have been counted as cast, and when these beliefs are influenced by the subjective experience of voting for the winner. We have taken advantage of special features associated with American elections to make advances in this field along three fronts. First, because of the nature of surveys conducted in the decade-and-a-half following the 2000 U.S. presidential election, we have been able to focus specifically on the accuracy of the vote count itself. Second, we examined responses given before and after elections have been held, and exploited the as-if random nature of close elections, to estimate causal effects. Finally, we have exploited American federalism to examine, for the first time, how the effect of voting for a winning national candidate affects perceptions of fairness at multiple layers of election administration.

Our findings strongly confirm our first hypothesis, that voting for the winner affects the perceptions that voters have about the fairness of the vote count. We tested for the presence of the winner's effect using a variety of specifications and methods, and discovered that the effect is generally in the double digits. We also find strong evidence that voting for the winner (compared to voting for the loser) not only influences how a voter judges whether his own vote was counted accurately, but is also strongly influences whether a voter judges that the votes of others were counted

¹³ We are unable to test whether the winner effects are the same for local vs. national vote counts in these surveys, as both questions were generally not asked in the same survey. The exception is the 2012 CCES. Here, a test of equality of coefficients (estimated using Seemingly Unrelated Regression) gives a p-value of 0.55, which is consistent with the cross-sectional estimate from this survey.



Notes: Estimates shown are difference-in-differences, with standard errors in parentheses (clustering on respondent). The data from the first panel are from the 2004 NAES (N=548); the data in the second panel are from the 2008 Pew survey for own-vote confidence (n=586) and the 2008 NAES for country-level confidence (N=7,409); the data from the third panel are from the 2012 CCES (n=1,847).

Fig. 5. Effect of election outcomes on individual-level confidence: difference-in-differences estimates.

properly. However, the evidence for our second hypothesis – that winner effects are larger for the national vote count versus the local vote count – was more mixed. While it is nearly always the case that the size of the winner effect is larger for national vote counts, these differences are not always statistically significant, as Fig. 4 demonstrates. Generally speaking, surveys around the presidential elections of 2004, 2008, and 2012 show a significantly higher winner effect for national vote counts, while surveys from the midterm elections of 2006 and 2010 do not. We speculate that this discrepancy is due to differences in how voters react to midterm and presidential election outcomes. In the former case, voters may base their evaluation of the national vote count based primarily on whether their favored candidate won in their own legislative district, as opposed to which party won control of the national legislature. This could be because voters care more about their own legislative race relative to the national outcome, or because they are simply unaware of which party controls the legislature. Given the increasing partisan homogeneity of U.S. legislative districts, most of our midterm respondents likely cast votes in districts where their preferred candidate won. Although none of our midterm surveys includes a large enough sample to drill down to the district level, future research could test this hypothesis using the 2014 SPAE and CCES surveys.

An important question about the research presented here is the degree to which it travels to other settings. We presented no direct evidence about the generalizability of these findings to other countries. Our conclusions on this score are therefore speculative, but, we hope, constructive for subsequent research.

First, the magnitude of the effect may be either higher or lower outside of the United States. As noted above, the existing literature has shown winner effects to be the largest in developing democracies, where elections are relatively novel. This suggests that the effects we find would be larger in other countries, especially in new democracies. On the other hand, our results may be inflated by the polarized nature of U.S. politics, which may cause partisans to distrust institutions controlled by the rival team. Comparing the effect of winning on vote count perceptions across countries is an important direction for future work, and a necessary first step toward conducting such comparisons is to include measures of vote count perceptions on future crossnational surveys.

Second, our reading of the extant literature also leads us to conclude that the issue of federalism in cross-national studies of election administration has been neglected; we believe our findings would have the greatest impact in helping to extend the comparative literature in new directions. In particular, the matter of how elections are administered — locally, or controlled by a central authority — varies across the world. Our findings here suggest that skepticism about the veracity of election results among those who voted for the losing party is especially great when considering election administration that is at a far geographic remove from the voter. This suggests new avenues of research for those concerned about what administrative practices reassure citizens that the results of elections are determined fairly — as well as lessen the tendency of voters to base fairness judgments on subjective factors.

Appendix 1. Data Sources and Question Wordings Polls used in the analysis

Table A1 Confidence in own vote.

Date	House	Mode	Sample
11/12/2000	CBS / New York Times	Telephone	Voters
12/16/2000	LA Times	Telephone	Registered Voters
07/15/2004	CBS / New York Times	Telephone	Voters
10/19/2004	Pew	Telephone	Registered Voters
10/26/2004	ABC / Washington Post	Telephone	Voters
11/01/2004	National Annenberg Election Study	Telephone	Likely Voters
11/08/2004	Pew	Telephone	Registered Voters
12/19/2004	ABC	Telephone	Voters
12/24/2004	National Annenberg Election Study	Telephone	Voters
10/04/2006	Pew	Telephone	Registered Voters
10/15/2006	CNN	Telephone	Registered Voters
10/25/2006	Fox News	Telephone	Likely Voters
11/04/2006	Pew	Telephone	Registered Voters
11/04/2006	ABC / Washington Post	Telephone	Voters
11/12/2006	Pew	Telephone	Registered Voters
12/02/2007	Gallup / USA Today	Telephone	Eligible Voters
10/19/2008	Pew	Telephone	Registered Voters
11/09/2008	Pew	Telephone	Voters
11/11/2008	Survey of the Performance of American Elections	Internet	Voters
11/07/2010	Pew	Telephone	Voters
11/05/2012	CCES	Internet	Eligible Voters
11/11/2012	Pew	Telephone	Voters
11/28/2012	Survey of the Performance of American Elections	Internet	Voters
12/12/2012	CCES	Internet	Eligible Voters

Table A2Confidence in country's vote.

Date	House	Mode	Sample
11/12/2000	Pew	Telephone	Voters
01/19/2001	National Annenberg Election Study	Telephone	Registered Voters
10/30/2004	CBS / New York Times	Telephone	Voters
11/08/2004	Pew	Telephone	Registered Voters
12/19/2004	ABC	Telephone	Voters
10/15/2006	CNN	Telephone	Registered Voters
10/22/2006	Gallup / USA Today	Telephone	Eligible Voters
11/12/2006	Pew	Telephone	Voters
12/02/2007	Gallup / USA Today	Telephone	Eligible Voters
01/01/2008	National Annenberg Election Study	Internet	Voters
10/29/2008	CBS / New York Times	Telephone	Registered Voters
11/09/2008	Pew	Telephone	Voters
01/31/2009	National Annenberg Election Study	Internet	Voters
11/07/2010	Pew	Telephone	Voters
11/05/2012	CCES	Internet	Registered Voters
11/11/2012	Pew	Telephone	Voters
11/28/2012	Survey of the Performance of American Elections	Internet	Voters
12/12/2012	CCES	Internet	Registered Voters

-

Confidence in own vote

11/12/2000 CBS/New York Times Given the kinds of problems that have been reported in Florida, how much confidence do you have that your (2000 presidential) vote was counted properly—a lot, some, not much, or no confidence at all?

12/16/2000 LA Times Do you personally have a lot of confidence that your (2000) vote for president was counted, or some confidence, or no confidence at all that your vote for president was counted?

07/15/2004 CBS/New York Times How much confidence do you have that the votes in your state will be counted properly this November – a lot, some, not much, or no confidence at all?

10/19/2004 Pew How confident are you that your vote will be accurately counted in the upcoming election?

10/26/2004 ABC/Washington Post And how confident are you that your own vote for president (in 2004) will be accurately counted this year: very confident, somewhat confident, not too confident or not confident at all?

11/01/2004 National Annenberg Election Study Are you confident that your vote will be counted accurately, or are you doubtful?

11/08/2004 Pew How confident are you that your vote was accurately counted?

12/19/2004 ABC How confident are you that your own vote for president (in 2004) was accurately counted this year: very confident, somewhat confident, not-too-confident or not confident at all?

12/24/2004 National Annenberg Election Study Are you confident that your vote has been counted accurately, or are you doubtful?

10/04/2006 Pew How confident are you that your vote will be accurately counted in the upcoming election?

10/15/2006 CNN How confident are you that your vote and the votes cast by people in your family will be counted accurately in this year's (2006) election-very confident, somewhat confident, not too confident, or not confident at all?

10/25/2006 Fox News How confident are you that your vote will be accurately counted in this year's (2006) election?

11/04/2006 Pew How confident are you that your vote will be accurately counted in the upcoming election?

11/04/2006 ABC/Washington Post How confident are you that your own vote in this election will be accurately counted this year (2006): very confident, somewhat confident, not too confident or not confident at all?

11/12/2006 Pew How confident are you that your vote was accurately counted?

12/02/2007 Gallup/USA Today Thinking about the general election for president to be held in November 2008, How confident are you that, at the voting facility where you vote, the votes will be accurately cast and counted in next year's election—very confident, somewhat confident, not too confident, or not at all confident?

10/19/2008 Pew How confident are you that your vote will be accurately counted in the upcoming election?

11/09/2008 Pew How confident are you that your vote was accurately counted?

11/11/2008 Survey of the Performance of American Elections How confident are you that your vote in the General Election was counted as you intended?

11/07/2010 Pew How confident are you that your vote was accurately counted?

11/05/2012 YouGov/Polimetrix How confident are you that your vote in the General Election was counted as you intended?

11/11/2012 Pew How confident are you that your vote was accurately counted?

11/28/2012 Survey of the Performance of American Elections How confident are you that your vote in the General Election was counted as you intended?

12/12/2012 CCES How confident are you that your vote in the General Election was counted as you intended?

Confidence in country's vote

11/12/2000 Pew As you may know, the outcome of this year's presidential election will be decided by a very narrow margin in Florida and several other states. All things considered, do you think we will have an accurate count of the votes in Florida and other close states, or not?

01/19/2001NationalAnnenbergElectionStudyAr-

eyouconfidentthatthevotesinthis{through 30 Dec 00: year's | starting 2 Jan 01: past} presidential election {through 12 Dec 00: are being | starting 13 Dec 00: have been} counted fairly, or don't you feel this way? Q410 (Yes or No).

10/30/2004 CBS/New York Times How much confidence do you have that the votes for president will be counted properly this November (2004)—a lot, some, not much, or no confidence at all?

11/08/2004 Pew How confident are you that the votes across the country were accurately counted?

12/19/2004 ABC On another subject, how confident are you that the votes for president across the country were accurately counted this year?

10/15/2006 CNN How confident are you that, across the country, the votes will be accurately counted in this years election – very confident, somewhat confident, not too confident, or not confident at all?

10/22/2006 Gallup/USA Today How confident are you that, across the country, the votes will be accurately cast and counted in this year's election?

11/12/2006 Pew How confident are you that the votes across the country were accurately counted?

12/02/2007 Gallup/USA Today How confident are you that, across the country, the votes will be accurately cast and counted in next year's election.

01/01/2008 National Annenberg Election Study When Election Day comes, how confident are you that the votes across the country will be accurately counted?

10/29/2008 CBS/New York Times How much confidence do you have that the votes for president will be counted properly this November (2008)—a lot, some, not much, or no confidence at all?

11/09/2008 Pew How confident are you that the votes across the country were accurately counted?

01/31/2009 National Annenberg Election Study How confident are you that the votes across the country were accurately counted on Election Day?

11/07/2010 Pew How confident are you that the votes across the country were accurately counted?

11/05/2012 YouGov/Polimetrix Think about vote counting throughout your county or city, and not just your own personal situation. How confident are you that votes in your county or city were counted as voters intended?

11/11/2012 Pew How confident are you that the votes across the country were accurately counted?

11/28/2012 Survey of the Performance of American Elections Think about vote counting through-out your county or city, and not just your own personal situation. How confident are you that votes in your county or city were counted as voters intended?

12/12/2012 CCES Think about vote counting throughout your county or city, and not just your own personal situation. How confident are you that votes in your county or city were counted as voters intended?

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Appendix 2. Additional Specifications



Probit coefficient on Democrat (baseline is Republican)

Fig. A1. Replication of Fig. 4 using Probit regressions.

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Exhibit 23

Effects of Identification Requirements on Voting: Evidence from the Experiences of Voters on Election Day

Stephen Ansolabehere, Harvard University

t the heart of the efforts to improve elections in the United States are two important values: access and integrity. To guarantee the right to vote, the polls must be accessible to all who wish to vote. To guarantee legitimate elections, only eligible people should be allowed to vote, and all votes must be tabulated correctly. These values have different implications for administrative procedures, ranging from the implementation of registration systems to the choice of voting equipment to the set up of polling places and training of poll workers. Often these values work hand in hand, but at times they are at odds. Such is the case with the authentication of voters at the polls (see National Commission on Federal Election Reform 2002).

The debate over voter identification is usually framed as a tradeoff between the goals of guaranteeing access and ensuring integrity. Stricter authentication procedures, including photographic identification and proof of citizenship, provide high levels of assurance that those voting are in fact legal voters. Such procedures may, however, create burdens that prevent many legitimate voters from participating. The debate further divides along partisan lines, as it is commonly conjectured that those least likely to have the required identification readily available are disproportionately poor, racial minorities, and elderly, and, thus, more likely Democratic. Consequently, efforts to create voter-authentication procedures in states quickly become partisan political matters.

State laws stipulate the acceptable ways that poll workers may verify that an individual is a legitimate voter, and is who he or she claims to be. There is considerable variation among the states in the methods allowed. The different rules group roughly into two polar cases: (1) those states that allow poll workers to request identification, and (2) those states that do not. The most stringent form of authentication currently in use requires that all voters present a government-issued photographic identification at the polls. Only two states currently have such laws, although another two dozen allow poll workers the discretion to request identification. In these states, voters may ultimately be asked for identification in order to vote. The other 25 states employ a range of less stringent identification rules, including signature on an affidavit or a registration list; providing proof of residence, such as a utility bill; or simply stating one's name and address. In these states, voters need not present identification in order to vote. Before 2000, less stringent identification rules were the norm, but the election-reform efforts following the 2000 election led many states to adopt stricter voter-authentication rules.

Not surprisingly, these laws have been challenged in court, and the question before the Supreme Court was how to balance the twin goals of access and integrity. Those challenging photo-ID laws argued that they place undue burden on voters and have discriminatory consequences; those defending the laws argued that they are essential to ensure the legitimacy of elections. The State of Missouri struck down a Missouri photo-identification law as too restrictive, but ithe federal courts let stand such laws in Arizona and Indiana and left open the possibility of further challenges to identification laws in other contexts. The majority opinion in the Indiana case (Crawford v. Marion County) further redefined the tradeoff. Voter-identification laws, although they may place some burdens on voters, must be weighed against not just actual instances of fraudulent voting but against the potentially corrosive effects of the perception of corruption in the electoral process. The Court's reformulation of the objectives, which ironically echoes Buckley v. Valeo (424 US 1 [1976]), the signature case in campaign finance, raises a host of fundamentally empirical questions about the use of identification procedures:

- Are poll workers in compliance with their own state laws? Are voter-ID requests equitably applied across groups?
- 2. Do voter-identification procedures affect turnout of legal voters? Do they prevent and deter people from voting?
- 3. Do identification procedures improve confidence in the election? Do those asked to show ID or in states with more stringent ID laws express lower beliefs in the incidence of fraud?

The answer to all three of these questions appears to be *no*, a conclusion that ought to cause legal scholars, political strategists, state legislators, and jurists to rethink the justification for and likely consequences of voter-identification rules.

METHODOLOGY

These are empirical questions of the sort that political scientists are quite good at addressing. I and colleagues affiliated with the Cooperative Congressional Election Study and the Caltech/MIT Voting Technology Project decided to examine these questions directly using a simple battery of questions about voters' election experiences in the 2006 general election and the 2008 presidential primaries on Super Tuesday.¹ Specifically, the battery of questions asks whether the respondent (1) is registered; (2) voted in the election in question; (3) voted at the polls, early, or absentee; (4) was asked to show photo ID; (5) had a registration problem or difficulty obtaining a ballot; and (6) waited to vote. Of particular importance for this issue, the surveys asked respondents whether they were asked to show photographic identification at the polls when they voted. Follow up questions were asked of those who showed ID or had registration problems to ascertain whether they were, then, not allowed to vote, voted a provisional ballot, or allowed to vote.

The surveys were conducted over the Internet by YouGov/ Polimetrix over the course of the week following each of the elections. The 2006 sample consisted of 36,500 adults; the 2008 sample contained 4,000 adults. An additional sample was conducted over the phone in 2008 to cross-validate the Internet survey. Although reported turnout was slightly lower among those in the phone survey, the proportions reporting being asked for identification, having registration problems, and so forth were not statistically distinguishable across the two modes. At the time of this writing, similar studies are underway for the 2008 general election.

In addition, 1,000 subjects from the 2006 survey were interviewed again in 2007. These respondents were asked their beliefs about the incidence of voter fraud and election tampering as well as their intentions to vote in the future. This sample allows us to address whether those actually asked to show ID feel that fraud is less of a problem.

Finally, data on state laws come from the National Council of State Legislatures (2008) and Alvarez, Bailey, and Katz (2008). These laws divide roughly into two categories: those states that allow poll workers to request voter identification and those that do not. I will refer to these below as Voter ID states and Non-ID states.

RESULTS

1. What Is the Incidence of Voter-ID Requests?

Half of all voters are asked to show photographic identification at the polls. In the 2006 sample, 49% of respondents reported that the poll workers asked them to show photo ID when they voted. In the 2008 sample that figure had risen to 56%. This rate is strikingly high. In 2006 only two states actually required photo identifications; the other Voter ID states allowed poll workers to request ID. In other words, poll workers are using their discretion and asking voters to show photo ID.

The incidence of requests for ID varies considerably across states, with requests exceeding 90% of all voters in some states and below 10% in others. The main reason for the variation is state law. In the Voter ID states, nearly 80% of voters on average are asked to show ID. In the Non-ID states, less than 20% are asked to show ID (see Ansolabehere 2008). This pattern suggests that simply allowing poll workers to request voter ID triggers a very high rate of requests for photographic identification at the polls. The 2008 Super Tuesday survey probed whether voters showed photo ID because that was convenient or because that was what the poll worker requested. Approximately half of those who showed ID said that they did so because photo ID was convenient, but half said that the poll worker asked for photo ID.

The incidence of requests for photo ID at the polls also varies across region. In the Northeast 22% reported that poll workers asked for photo ID in the general election, compared with 65% in the South. Approximately 45% were asked for ID in the West and Midwest.

Regional variation in requests for photo identification at the polls mainly reflects the laws. Southern states tend to have more restrictive voter-ID laws. Only Mississippi and North Carolina do not ask for voter ID at the polls. The Northeast is the opposite. Only Connecticut does allow poll workers to request ID. As a result, relatively few voters are asked for ID in the North, but most voters are asked for ID in the South. The pattern of ID laws is less uniform among the Western states and among the midwestern states.

For voting-rights advocates, racial differences in these data are of greatest concern. Differential application of election laws at the polling places was the target of the Voting Rights Act and civil rights litigation from the 1940s onward. The Voting Rights Act expressly forbids the use of "tests" at the polls, and the U.S. Supreme Court has struck down the use of poll taxes as violations of equal protection. Literacy tests, poll taxes, and other administrative procedures were widely used before the 1960s to keep certain groups, especially southern blacks, from voting. There is the very real possibility that voter-ID laws provide an opening for the reemergence of such practices.

Both the 2006 and 2008 surveys show considerable racial differences. In the 2006 general election, 47% of white voters reported being asked to show photo identification at the polls, compared with 54% of Hispanics and 55% of African Americans. In the 2008 Super Tuesday primary states, 53% of whites were asked to show photo ID, compared with 58% of Hispanics and a staggering 73% of African Americans. These racial differences persisted upon holding constant income, education, party identification, age, region, state laws, and other factors (see Ansolabehere and Persily 2008 for details). Opponents of voter-identification laws charge that they amount to a new form of "test" or "tax." These surveys provide the first individual-level data that poll workers commonly ask voters for photo identification, even in places where they are not allowed to. The data further show that poll workers do not administer this procedure fairly or without regard to race, which raises the important possibility that in practice voteridentification procedures violate the Voting Rights Act.

2. What Is the Effect of Voter ID Requests on Access?

The immediate voting-rights concern with photo-identification laws is that they prevent people from voting and affect the access to the polls of different groups or classes of voters. How many people were denied the vote as a result of voteridentification requests?

The answer is—very few. If respondents reported that they were asked to show photo identification, the 2006 and 2008 surveys probed whether the respondents were then allowed to vote. In the 2006 survey, out of 22,211 voters only 25 said that they were asked for identification and, then, disallowed from voting—that is one-tenth of 1% of the sample of voters. In the 2008 survey, three out of 2,564 respondents said that they tried to vote but were not allowed because of voter ID, a fraction of a percent.

This is an exceptionally low rate of denial of access to the vote. Some of these denials may have been legitimate, and some may have been erroneous. But the actual denials of the vote in these two surveys suggest that photo-ID laws may prevent almost no one from voting.

One rejoinder to these findings is that the very presence of ID laws may discourage some voters from even attempting to vote. To examine this possibility we asked registered non-voters in the 2008 survey why they did not vote. This question parallels the question on the Current Population Survey with the addition of "I did not have proper identification." Of the 1,113 non-voters in the survey, four cited this as a reason, and these individuals cited other reasons as well— "bad weather" and "forgot to vote." All told, then, only seven out of 4,000 people (less than two-tenths of 1% of the electorate) could be considered non-voters at least in part because of voter identification.

Voter ID does not appear to present a significant barrier to voting. Although poll workers widely request ID, such requests rarely result in voters denied the franchise. Moreover, very few people chose not vote in the 2008 primaries for lack of identification. Although the debate over this issue is often draped in the language of the civil and voting rights movements, voter ID appears to present no real barrier to access. An important caveat accompanies these findings. These surveys covered a midterm election and presidential primary elections. Although the particular elections drew relatively high numbers of voters, the turnout was not nearly as high as in presidential general elections. High turnout is widely thought to put additional strains on the election-administration system. Some have argued that the denial of access is most likely to occur in those circumstances. Whether that is true is the subject for further study. At the time of this writing, the 2008 Cooperative Congressional Election Survey (CCES) and a separate survey sponsored by the Pew Charitable Trusts are planned for the 2008 general election and will replicate these questions and methods.

3. Voter ID and Confidence

The justification for photo-identification requirements rests on concerns about voter fraud. Requiring all voters to show photo identification at the polls may be justified in order to prevent people from impersonating actual voters or committing other sorts of voter fraud. Voter fraud has been perhaps been the most elusive election-related phenomenon on which to get hard facts. Large majorities believe that fraud occurs at least somewhat often in elections, but social scientists have been unable to develop unambiguous measures of the incidence of fraud, and legal cases find very little hard evidence on the matter. In fact, the case in *Crawford v. Marion County Board of Election* (553 US [2008]) produced no actual instances of voter fraud occurring. Even still the courts upheld the law.

The justification for the law in the majority opinion of the Supreme Court arises not necessarily from the fact of fraud but from the perception of or belief in fraud. The opinion in *Purcell*, and later *Crawford*, argues that the government's interest in limiting corruption or perceived corruption of the electoral process must be weighed against the constitutionally guaranteed right to vote.²

The Court's concern with the perception of corruption certainly receives support from the public opinion surveys. In a follow-up survey to the 2006 CCES, the 2007 CCES asked about people's perceptions of fraud. Over half of respondents felt that voter fraud, as occurs when ineligible people vote, occurs "somewhat often" or "very often." Similar majorities felt that election fraud, such as occurs when ballots are tampered with, also occurs somewhat or very often. Only about one in 10 respondents believe that such fraud occurs "rarely" (See Ansolabehere and Persily 2008, 1754).

The opinion of the Court, however, asserts that a widely held belief that fraud occurs often will erode the legitimacy of elections. People will come to view elections as illegal or not reflecting the will of the people. This will discourage people from voting, further weakening the democratic process. Stronger identification laws offer a cure, as they can reassure voters that only legitimate votes are cast.

These claims are testable. First, those who believe that fraud is common ought to be less likely to vote. Second, those in states with stricter ID laws ought to perceive less fraud in their elections. Neither appears true.

The first claim asserts a mechanism through which fraud lowers voting. Those who see fraud as occurring often in elections, it is argued, will view elections as less legitimate and their votes as less effective. As a result, those who believe that fraud is common ought to be less likely to vote. In the 2007 survey, of those who thought fraud a very common occurrence, 47% voted, and of those who thought fraud rare, 44% reported voting.³ Controlling for education, income, partisanship, and other factors did not change this non-finding. Belief in the frequency of election fraud is uncorrelated with propensity to vote.

The second claim asserts a solution—stricter identification will shore up confidence in the process and, hence, turnout. This claim also lacks empirical support. Those voters living in states with stricter identification laws did not report higher levels of confidence or higher rates of voting than those living in states with relatively weak identification rules. In states with the weakest ID rules, 26% think fraud occurs very often and 10% think it occurs rarely. In states with the strictest ID rules, 29% think fraud occurs very often and 9% think it occurs rarely. Moreover, individual voters who were asked to show ID at the polls in 2006 were not more likely to assert higher levels of confidence in the electoral process or higher intentions to vote than those who were not asked to show ID (Ansolabehere and Persily 2008). These twin findings reveal that ID laws will have little or no effect on the confidence in the electoral system or the belief in the incidence of fraud. Those beliefs, wherever they come from, are no different when a stricter ID law is in place and enforced than when less invasive voter-authentication methods are used. These findings also call into question the assumptions underlying the majority's opinion in *Crawford*. People may think voter fraud occurs often, but that belief appears disconnected from the likelihood that someone engages with politics and votes.

DISCUSSION

The experience of individuals at the polls on Election Day suggests that there is much less to the voter-identification controversy than appears in the pages of the court decisions or the debates in public forums. Approximately half of all people are asked for ID when they vote, but almost no one reports subsequently being denied the vote or reports that lack of ID was a reason for not attempting to vote. A majority of Americans say that voter fraud is common, but voter-identification laws and practices have little effect on those beliefs, and those beliefs have no effect on rates of electoral participation.

This is the picture that emerges from voters' reports about what actually happened at the polls on Election Day. Most studies of aggregate election returns are consistent with such non-findings, but some researchers do find that the state law used corresponds to lower turnout rates (e.g., Alvarez, Bailey, and Katz 2008). Why the inconsistency? One possibility is methodological—aggregate indicators do not measure who is asked for ID and run the risk of committing the ecological fallacy (even when the aggregate indicator is used in a survey). The survey data are superior, as they reveal whether voteridentification requests are in fact an instrument of exclusion.

There is another possibility. Both sets of results may be right. It may be the case that total votes cast drop once states adopt identification requirements and that only a very small number of individuals are prevented from voting at the polls by such rules. Two potential explanations may resolve this difference. First, identification requirements may deter people from voting without actually excluding them at the polls. Second, identification requirements may reduce the incidence of voting among those not registered or eligible to vote, that is, fraud.

The first of these explanations looks like it has no basis in fact, as the 2008 survey found that almost no one reported that they stayed away from the polls for want of appropriate identification. That leaves the tantalizing, yet unresolved, possibility that the differences between aggregate results and individuals' experiences at the polls may reflect a reduction in fraud. To date, there are certainly cases of fraud, but no evidence of systematic or extensive voting fraud, despite concerted investigations by the Department of Justice. It will require more intensive survey research to track the voters' (and non-voters') experiences and careful modeling of aggregate election returns to determine whether the introduction of ID laws caused a drop in the total number of votes recorded. The conclusion supported by the data examined here, however, is that voter-ID laws have no effect on turnout, and hence little or no fraud, little or no denial of access, and little or no effect of on confidence in the electoral system.

NOTES

- The Cooperative Congressional Election Study of 2006 was created through the consortium of 37 different universities. The 2008 Super Tuesday survey was paid for through a grant from the Pew Foundation.
- 2. Purcell v. Gonzalez 127 S. Ct. 5, 7 (2006).
- 3. The 2006 CCES includes validated vote, and the numbers reported are of actual votes, not reported. These figures come from the MIT Module of the 2007 CCES.

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Exhibit 24

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Election administration and perceptions of fair elections*

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A R T I C L E I N F O

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ABSTRACT

Scholars of democracy proposes an important relationship between the quality of elections and democratic legitimacy, but there are few studies of how the conduct of elections affects perceptions of elections being fair. We examine how election administration and individual-level demographic traits affect public perceptions of fair elections in the US. Since administration of US elections is largely the responsibility of individual states we are able to exploit variation in the quality of how elections are conducted to assess effects of electoral administration on public perceptions. We find evidence that administrative performance is positively and significantly related to perceptions of elections being fair. Voter identification laws, in contrast, are not associated with greater confidence in elections. We also find some evidence that speaks to the limits of these findings, as individuallevel factors such as partisanship and minority status have larger effects than administration on perceptions of electoral fairness.

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1. Introduction

It is well established that, after an election, winners and losers differ in their attitudes about the winner's right to govern (Nadeu and Blais, 1993), their trust in government, their satisfaction with democracy, and their views that elections make officials respond to the public (Anderson and LoTempio, 2002; Banducci and Karp, 2003; Bowler and Donovan, 2007; Esaiasson, 2011; Singh et al., 2012). Yet as some basic level, democratic elections 'work' because (or if) losers and winners see the outcome as the result of a fair, legitimate process. One important theme from a recent body of research on electoral integrity is that the procedural quality of elections should contribute to democratic legitimacy (for an overview, see Norris, 2014; Birch, 2008). Part of the process by which supporters of losing parties and losing candidates see winners as having legitimate authority is that at some level, they view the electoral process as fair, and consent to the results of elections they lose (Anderson et al., 2005).

But how is it that people come to perceive outcomes of elections as legitimate and procedurally fair? In older, established democracies, it is likely that citizens have some base level of political socialization that causes them to view electoral procedures as fair in themselves. In these nations, the same social processes that transmit civic duty (Blais et al., 2004; Blais, 2006), patriotism, or even party loyalties (Campbell et al., 1960; Niemi and Jennings, 1991) likely also build some reservoir of support for the outcomes of democratic institutions (Dalton, 2009). Regardless winning or losing, and regardless of procedural faults or glitches on election day, socialization processes may cause



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people in established democracies to see elections as routine events and to regularly accept election results as legitimate (Mozaffar and Schedler, 2002). Political socialization is not, however, sufficient to explain how all people view electoral integrity at a particular point in time. Although socialization may well provide a reservoir or benchmark of support it is not plausible to suggest that the level of support remains unchanged through a life cycle of perhaps a dozen or more national elections. Several scholars note that younger generations are being socialized toward democracy differently (Denemark et al., 2012), with less deference to authority (Inglehart, 1990) and with civic duty acting as a weaker force in motivating political participation (Blais et al., 2004). The media environment that generates information about democratic institutions has also changed (Moy and Pfau, 2000) - a competitive, partisan media context can increase incentives news outlets have to bring attention to procedural flaws in elections, and allegations of fraud.

Beyond any socialized acceptance of election results then, citizens' views of electoral legitimacy are conditioned by their *perceptions* of electoral and political performance (Norris, 2014, 2004; Elklit and Reynolds, 2005; Elklit and Reynolds, 2002). For example, Europeans who perceived that officials were bribed were less trusting of democratic institutions (Anderson and Tverdova, 2003). Russians who perceived elections as unfair were less supportive of political parties, parliament, and their government (McAllister and White, 2011). Although we have evidence that satisfaction with democracy is related to broad measures of procedural performance of government (Norris, 2004),¹ and evidence that specific electoral rules (proportional representation and publicly financed elections) correspond with greater popular confidence in elections (Birch, 2008), we know less about how the quality of how election administration affects mass perceptions of electoral performance in established democracies.

2. The research question

In light of the preceding discussion our research question can be stated: To what extent are perceptions of electoral performance affected by the actual procedural quality of elections? By investigating this question, we can broaden our understanding of how citizens reason about political institutions in general. That is, are popular attitudes about democratic institutions, at least in part, structured by the quality of institutional practice?

Our primary question also has implications for the utility of efforts to improve the administration of elections. We know that, independent of the procedural quality of elections, a particular event or election rule can be viewed quite differently by different groups. In the US, for example, partisanship plays a major role in structuring whether or not people view key aspects of elections as unfair or corrupt. Party structures how people perceive the role of campaign finance in elections (Persily and Lammie, 2004), how they view the relationship between campaign finance and the legitimacy of election results, how they viewed the legitimacy of the disputed 2000 presidential election (Craig et al., 2006), and how they viewed the utility and fairness of voter identification laws (Bowler and Donovan, 2013:30–31; Bentele and O'Brien, 2013). Indeed, at the mass and elite levels, Americans' attitudes about what does and what does not constitute electoral 'fraud' are defined sharply by their partisanship (Wilson and Brewer 2013; Ansolobehere and Persily 2008).

However, if we find that people view elections as more legitimate where objective measures show they are better administered, this would suggest that efforts that succeed at improving electoral performance can enhance the ability of democratic elections to impart legitimate political authority. We should note that there are some grounds for scepticism that election administration will have an independent effect on public opinion. Bowler and Donovan (2013) have demonstrated a wide range of electoral rules and reforms have little identifiable relationship with political trust, efficacy, and citizen engagement with politics. These findings suggest a more limited role for "institutional effects" than one might expect given the argument that "institutions matter." We might also see these results as suggesting a limited role for the effect of election administration. One reason for such null results with respect to broad institutional changes is that although an electoral rule may exist, it need not necessarily be implemented in a fashion that citizens are able to detect. In this study, however, we are not assessing how the presence or absence of an electoral institution affects attitudes, rather, we examine how the implementation of elections affects attitudes.

At this point we should note that the general hypothesis of interest is quite straightforward: better administration of elections should produce more positive views of the electoral process among mass publics. In order to test that general argument, however, we need to substantiate both that the US case is an appropriate case study and that appropriate measures of election administration exist.

3. The advantage of the American case

As Norris notes (2014), there are a number of problems with attempts at establishing causality when investigating the relationships between electoral performance and public attitudes about elections. For one, there are very few cases where we have survey data measuring attitudes about electoral performance collected before and after a jurisdiction transitioned to democratic elections. Even in established democracies, it is rare to find polls with suitable items conducted over a time span that is adequate enough to capture the potential effects on attitudes of problems with electoral performance. As such, cross-national studies of opinions taken as a snapshot in time have been our best chance for teasing out the effects of electoral performance on popular attitudes.

An additional research design problem is that of being able to measure electoral performance objectively, across a large number of jurisdictions (see Elklit and Reynolds, 2002). Up to this point, most studies have relied on subjective measures of electoral performance (e.g. corruptions

¹ Also see Putnam et al. (1994), who argue that government performs better where there is greater civic engagement.

perceptions indices, and/or experts' subjective perceptions of electoral performance). It is quite possible that popular perceptions of electoral performance are closely bound to objective measures of performance. Scholars, however, have not always been well-positioned to objectively measure election performance with a standard that can be applied across a large number of jurisdictions. By examining the US case, we can model individual citizens' perceptions as a function of an objective measure of electoral performance that is applied across a large number of cases. In sum, we can test if mass perceptions reflect the reality of how elections are conducted.

As noted earlier, the United States has one of the world's most decentralized systems of electoral administration. The level of decentralization makes for a range of variation in administration that, in turn, makes the US an especially interesting case for examining variation in the impact of administration. Non-US based scholars – and even many US students – may be surprised by the variation within the US: surely the Federal Election Commission plays a role in standardizing elections? Given both that it may be surprising to see how much variation exists and that the variation in administration is a rationale for choosing the US case it is worth spending a little time establishing just how varied is US experience.

There is no formal US equivalent of The Australian Election Commission, Elections Canada, or even the UK Electoral Commission. Although there are some federal statutes and Supreme Court rulings that create (weak) national guidelines for the conduct of elections, the primary responsibility for administering elections remains in the hands of 50 different state governments.² As examples of standardization we note that the Help America Vote Act (2002) set minimum standards for the maintenance of voter rolls, types of voting equipment used, and rules for provisional ballots. The Voting Rights Act (1965, and various amendments) regulates ballot information (non-English language) that certain county governments must use. The Federal Election Commission also helps to regulate and publicize campaign spending in a standard manner (for federal races) across the states.

Nevertheless, the individual states are left to fund and conduct federal elections. States have substantial discretion in how they may comply with national standards, and states have autonomy in setting rules for such things as voter registration processes and requirements, rules for showing identification, types of voting equipment used, rules for early voting, absentee voting, rules for recounts, and myriad other factors. As a concrete example consider voter registration. Although the Court has ruled that states cannot require a pre-election registration deadline greater than 30 days before an election there remains considerable variation. In practice state registration deadlines range from 0 (election day) to 30 days prior. States also have substantial discretion in determining the level of resources they invest in the conduct of federal elections, in managing voter registration (there is no national roll), in designing

ballots, setting rules for absentee voting and use of provincial ballots, determining how (and whether) recounts and post-election audits are conducted, and in determining the number of polling places, their locations, and their hours of operation.

The result of this is that the elections are conducted in one state at a very different level of quality than in another state. Moreover, unlike cross-national analysis, in the US case variation across jurisdictions in major cultural factors is much more limited. Although we cannot definitively establish that *changes* in electoral quality affected feelings about the legitimacy of US elections, the states provide an excellent opportunity for testing how variation in the quality of the conduct of elections affects popular perceptions about whether or not elections were conducted fairly.

4. Measuring the performance of elections, and election laws

Despite the decentralization of election administration in the US, and the resulting variety in the conduct of elections across the states, there are also sufficient features in common among the states such that election performance can be measured in a standardized manner. For example, federal elections are all conducted at the same time under the same electoral system, and each state maintains similar records about: 1) the number of absentee ballots unreturned, 2) absentee ballots rejected, 3) the number of provisional ballots cast, 4) provisional ballots rejected, and 5) registrations rejected. Data are also available for each state on 6) wait times for voting, 7) registration rates, 8) registration problems, 9) whether or not the state allows registrations on-line, 10) whether it requires post-election audits, 11) the accuracy of voting technology (residual votes), 12) completeness of data records, 13) voting information lookup tools, 14) military and overseas ballots rejected, 15) military and overseas ballots not returned, 16) disability-related voting problems and 17) turnout.

Indeed, the Pew Center for the States has used these seventeen qualities of election administration to create an Elections Performance Index (EPI) that makes it possible to compare the quality of election performance across the states, circa 2012.³ Pew rates each state with an overall average score that represents a summary measure of how it performed on the seventeen items (with each item given equal weight).⁴ In 2012 the top scoring states on Pew's election performance index were North Dakota (86), Minnesota (80), Wisconsin (79), Colorado (79) and Nevada (77). The lowest ranked were Mississippi (44), Oklahoma (54), California (54), Alabama (56) and New York (58).

In addition to estimating how state-level election performance may have affected attitudes about electoral

² Election administration is further decentralized in the US, with states delegating the conduct of elections to thousands of county governments.

³ Pew worked with Charles Stewart III (MIT), Steven Ansolabehere (Harvard), Barry Burden (Wisconsin), Heather Gerken (Yale Law), Paul Gronke (Reed), Christopher Mann (Miami), Nathan Persily (Columbia Law), Bob Stein (Rice), Daniel Tokaji (Ohio State Law), and a group of state and local election officials to create the index.

⁴ State-level measures of election performance are also available for 2008 and 2010. Most states' performance scores were improving from 2010 to 2012, and scores from those years are well correlated (.82).

integrity in the US, we also account for how a specific election rule - photo identification - may have affected how people viewed the electoral process. A major reason for including this measure is that, as of 2012, the most contentious election rule in the United States was likely photo identification laws. In the mid 2000s a number of states began adopting strict laws that required voters to provide election officials a government issued photo identification when voting at a polling place. Republicancontrolled state governments promoted the laws as a tool for preventing voter fraud (voter impersonation), while Democrats alleged incidences of voter impersonation were rare and that the laws were designed to supress turnout among potential Democratic voters (Bowler and Donovan, 2013; Bentele and O'Brien, 2013). It is a highly charged political issue that has potentially large implications for election results (Richman et al., 2014) and voter participation but, more to the point, it is one that is talked about by both main political parties as a major issue in election administration.

The National Conference of State Legislatures (NCSL) maintains a database that records which states had photo identification laws in effect at the time of the 2012 US presidential election, and the type of identification requirement that the state had. The NCSL reports a fivepoint scale, ranging from 1) strict rules where photo identification is required, 2) strict identification rules that allow the use of non-photo documentation, 3) rules that allow photo identification to be requested (but a person can still vote if she lacks identification), 4) rules that allow nonphoto identification to be requested, and 5) states with no identification requirements. We code states on a five-point scale according to these NCSL classifications, with a range from one (if the state had no identification requirement) to five (if the state had a strict photo requirement). At the 2012 election, 30 states had some form of identification requirement, with strict photo requirements in Georgia, Indiana, Kansas, and Tennessee.⁵

These two measures – the Pew EPI index and the NCSL photo id categorization – provide us with measures of the variation of state level election administration which we can then use to explore citizen opinion towards elections.

5. Measuring perceptions of electoral integrity in the US

Wave 6 of World Values Survey (2010–2014) included several items designed to measure respondent's perceptions of electoral integrity, and some of these items were included on the 2012 American National Election Study. Respondents were prompted with, "in your view, how often do the following things occur in this country's elections." They were then asked (separately) if votes are counted fairly, and if election officials are fair. These two items reflect key principles of electoral integrity recognized by international institutions (Hall and Wang, 2008:43). With each item, response categories ranged from "very often", "fairly often," "not often," to "not often at all." The distribution of responses to these questions from the US and several other nations are reported in Figs. 1 and 2. Wave 6 of the WVS was conducted in many nations with limited experience with democratic elections. Data displayed in Figs 1 and 2 reflect attitudes about elections in the four nations where the WVS reported data from established, affluent democracies, with six additional cases included for comparative perspective.

Overall, Americans appeared to have been moderately confident in the integrity of their elections in 2012 – more so than respondents from Columbia and Mexico, but, depending on the item, less confident than respondents from Australia, the Netherlands, and Germany. Over seventy per cent of US respondents stated, respectively, that election officials were fair and votes were counted fairly at least "fairly often." By this measure, Americans' perceptions of elected officials appear similar to those of Uruguayans and Poles. But there is substantial variation in Americans' attitudes about elected officials, and evidence of pessimism. Barely one-fifth of US respondents were confident that election officials were fair "very often." Less than onethird of Americans believed that votes were counted fairly "very often." Americans, then, were far more sceptical about the integrity of vote counting than respondents from Australia, Germany, and the Netherlands. Our task in the analysis that follows is to understand how state-level electoral performance (as measured by the Pew EPI) and a key state election rule (identification laws) explains variation in Americans' attitudes about elections that are illustrated in Figs. 1 and 2.

6. Hypotheses and models

As noted above we expect that partisanship will structure attitudes about the integrity of elections in substantial ways, particularly in terms of fairness of process. Supporters of the party in power (or depending on the timing of a survey, the winning party) are likely to be satisfied with the result and that satisfaction with may project on to their views on the legitimacy of the process. The opposite applies for electoral losers. In the US in 2012, Democrats were the incumbent party in the White House, they controlled a



Fig. 1. Perceptions of electoral integrity: Per cent of respondents saying election officials are fair "very often" and "fairly often."

⁵ Mississippi, Pennsylvania, South Carolina, Texas, and Wisconsin had adopted strict photo id rules prior to the 2012 election, but these laws were not in effect at the time due to court or Department of Justice challenges.



Fig. 2. Perceptions of electoral integrity: Per cent of respondents saying votes are counted fairly "very often" and "fairly often." *Sources*: American National Election Study, 2012. World Values Survey 6: Australia 2012, Estonia 2011, Germany 2013, Netherlands 2012, Chile 2011,

Columbia 2012, Uruguay 2011, Mexico 2012.

majority in one chamber of Congress (the Senate), and won additional House and Senate seats when President Obama was re-elected in November. Given this and given the elections questions were asked after results of the election were known, we expect people who identified as Democrat to be more optimistic about electoral practices when asked in 2012, and we expect people who identified as Republican to be less so. Our models of perceptions of electoral integrity thus include respective dichotomous markers for Democrats and Republicans.

In addition, dichotomous measures identify white, African American, and Latino respondents (0/1), respectively. Given the historic institutionalization of African American and Latino voter suppression in the US (Kousser, 1999; Keyssar, 2000; Davidson and Fraga, 1988) we expect that members of these minority groups may be more likely to view the conduct of elections as unfair. There may also be effects here associated with the lack of descriptive representation. Members of groups who are historically under represented in elected offices (relative to their share of the population) may be more likely to view elections as unfair. Although African Americans have achieved levels of descriptive representation at some levels commensurate to their share of the population, this has been relatively recent. Latinos, in contrast, are represented at levels far below their share of the population. We control for gender as well. Although women are not a demographic minority, they do constitute a minority in terms of their descriptive representation. The enduring underrepresentation of women in American politics may cause women to view elections as unfair on multiple dimensions.

Furthermore, since higher levels of education are known to be associated with greater efficacy and trust (Niemi and Jennings, 1991; Craig et al., 1990), we expect respondents' with higher levels of education to be more likely to think that elections have a meaningful role, and, by extension to perceive that electoral processes are fair. Education is measured here in five categories, ranging from less than high school (1) to graduate degree (5). Our models also include terms for age, media consumption (frequency of TV news viewing per week), and a measure of political trust. It is important that we control for age, given that younger cohorts may experience socialization processes that leave them to be less deferential to elections and democratic institutions (Denemark et al., 2012). Media viewing is included to account for the possibility that people who frequently view TV news are more likely to be exposed to stories that feature suspicion about electoral malpractice and about political scandals, which may cause them to view elections as unfair.

Perceptions of the conduct of elections and of the people who conduct them could be part of an overarching set of attitudes about government and the integrity of public officials in general. Those less trusting of government have been shown to be more likely to worry about problems with election procedures (Nunnally, 2011). The pre-election wave of the 2012 ANES included a standard battery of trust in government questions as well as an item asking "how many of the people running the government are corrupt?" Most Americans indicated they believed that "about half" or more of "people running the government" were corrupt in 2012.⁶ If we consider standard definitions of public corruption this ANES item demonstrates that public perceptions about the extent of corruption in America are grossly inaccurate. However, the item may capture preelection cynicism about public officials that coloured how people responded to post-election questions about election officials. Given this item is similar to the post-election question about election officials, we report models that include and omit it. Including the item as a control provides a very conservative test for the effects of state-level performance on attitudes about elections.

Our state level elections variables include the NCSL measure of photo identification laws and the Pew EPI scores described above. Since we assume that attitudes about electoral practices are structured not only by partisanship, but also in response to how people experience the quality of elections, we expect that higher EPI scores will be correlated with more optimistic perceptions of the fairness of elections. Our expectations about photo identification laws are more conditional. Given the charged partisan context of these laws, we expect Republicans and Democrats viewed them differently, and thus viewed their potential effects differently. To test this, we estimate our models with interaction terms that test if Republicans were more confident in elections in states where stricter identification rules were in place.

Given the nature of our data, we estimate hierarchical linear models with random intercepts and five level-2 covariates: state EPI, state photo id laws, and controls for state median income, 2012 presidential vote margin in the state, and 2012 turnout.⁷ Income is included since state wealth may affect how much a state spends on the administration of elections,⁸ while margin accounts for

 $^{^6}$ Response options included all (4%), most (25.6%), about half (31.4%), a few (36.4%), and none (1%). The variable is recoded such that 1 = none, through 5 = all.

⁷ Models are estimated with Stata xtmixed. When the models were also estimated with ologit, results are similar to what we report (in terms of which effects were significant).

⁸ Median state household income 2012, and EPI for 2012 were correlated at just .03. State income is represented in units such that 41.1 =\$41,100).

Table	1
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Public attitudes about electoral integrity, United States.

	Officials fair		Vote count fair			
	I	II	III	IV	V	VI
Level 1						
TV news viewing	.006 (.004)	.002 (.004)	.002 (.004)	.001 (.004)	001 (.004)	001 (.004)
Democrat	.100** (.025)	.037 (.025)	.037 (.025)	.133** (.026)	.082** (.025)	.083** (.026)
Republican	108** (.047)	126** (.046)	125** (.046)	149** (.049)	160** (.047)	159** (.047)
Black	036 (.046)	074 (.045)	076 (.045)	039 (.029)	097* (.047)	074 (.043)
Latino	058 (.032)	086** (.032)	082** (.032)	070* (.033)	092** (.032)	091** (.032)
Female	125** (.023)	115** (.021)	114** (.021)	118** (.022)	108** (.021)	108** (.021)
Education	.111** (.009)	.085** (.009)	.085** (.009)	.111** (.009)	.090** (.009)	.090** (.009)
Age (groups)	.020** (.003)	.014** (.003)	.014** (.003)	.016** (.004)	.011** (.003)	.011** (.003)
Sample	113** (.023)	088** (.023)	090** (.023)	123** (.023)	101** (.023)	102** (.004)
Officials corrupt?	-	243** (.011)	242** (.012)	-	212** (.012)	211** (.012)
Level 2						
State income	.005** (.002)	.003 (.002)	.002 (.002)	.004** (.002)	.003 (.002)	.002 (.002)
Margin	.002 (.002)	.001 (.002)	.002 (.002)	.003 (.002)	.002 (.002)	.002 (.002)
Turnout	-	-	.0086** (.0032)	-	—	.0049 (.0027)
Election performance	.0059** (.0023)	.0056** (.0022)	-	.0039* (.0019)	.0033 (.0019)	-
Photo ID law	002 (.010)	001 (.010)	.006 (.011)	002 (.002)	010 (.009)	005 (.009)
Republican* ID law	.026 (.015)	.024 (.015)	.024 (.015)	.026 (.015)	.023 (.015)	.024 (.016)
Constant	1.79** (.188)	2.79** (.189)	2.68** (.208)	2.21** (.170)	3.08** (.170)	3.03** (.185)
Wald chi ²	293.1**	745.0**	745.6**	300.0**	630.7**	630.9**
Level 1 n	5307	5236	5236	5343	5261	5261
Level 2 n	50	50	50	50	50	50
Level 1 R ²	.05	.12	.12	.05	.12	.12
Level 2 R ²	.01	.01	.01	.01	.01	.01

Note: HLM with random intercept and level 2 covariates, estimated with Stata xtmixed; **p. < .01, *p. < .05 (two-tail). *Source*: 2012 ANES

variation in electoral context. Turnout is included in some models as a separate measure of electoral quality — it is one of the 17 items in the EPI, and other items in the index (registration rates, registration problems, on-line registration, etc.) and may reflect a better quality of election administration that is expressed in higher turnout. We estimate three models of both dependent variables; one model without the control that accounts for general attitudes about official corruption, one with that control, and one that includes turnout.

7. Results

Table 1 reports results of the models estimating Americans' responses to these WVS/ANES questions about electoral integrity. The estimates of individual level factors are largely consistent with our expectations. Other things equal, partisans differed in how they viewed election officials and vote counting after the 2012 contests. Republicans were consistently less confident that election officials and vote counts were fair, while Democrats were more confident. The effect of Democratic partisanship on perceptions of election officials is muted when the control for attitudes about government officials is included. However, Democrats are consistently associated with greater confidence in vote counting. Women, Latino/as, African Americans,⁹ younger people, and the less educated, respectively, were less likely to respond that election officials were usually fair and less likely to say that votes were usually counted fairly. That is, independent of partisanship, and independent of how well elections were conducted, women and minorities were less likely to think that US elections were fair.¹⁰ Table 2 (below) illustrates the substantive magnitude of these effects. Compared to a baseline respondent (a white male in a state with average EPI), women of colour were much less likely than others to see officials and vote counts as fair.

But what of state-level factors? We find that a state's 2012 Election Performance Index score was associated with how individuals responded to the question about the fairness of election officials and vote counts. Regardless of whether or not the models include controls for general cynicism about government officials, the coefficients for EPI are positive and statistically significant in estimates of perceptions that officials were fair and in estimates that vote counts were fair.¹¹ In states where this objective measure indicates the conduct of elections to be of higher quality, respondents were significantly more likely to say elections were fair. As shown in Table 2, the substantive magnitude of this relationship is modest – at least when compared to the individual-level effect of partisanship and gender.

Models III and VI replace the EPI measure of election administration with a measure of state-level turnout. Turnout also has a significant, positive association with

 $^{^9}$ The coefficients for African Americans in Model II, III and VI are significant at p. < .10 two-tail, or p. = .05 one-tail; the coefficient for Latinos in Model I is significant at p = .08 (two-tail).

¹⁰ Contrary to our expectation, we find no relationship between TV news consumption and attitudes about election officials and vote counts. ¹¹ When the control for attitudes about corruption is included in Model V the estimated coefficient for EPI on perceptions of fair vote counting is slightly smaller, and significant at just p = .09 (two-tail).

 Table 2

 Predicted probability of responding that election officials were fair and vote count was fair "very often."

	Officials fair	Count fair
Baseline ^a	.204 (.009)	.333 (.013)
Female	.154 (.008)	.267 (.012)
Latino	.161 (.016)	.258 (.021)
Latina	.121 (.012)	.203 (.019)
Black, male	.175 (.013)	.279 (.017)
Black, female	.148 (.016)	.215 (.015)
Republican	.182 (.009)	.286 (.013)
Democrat	.244 (.012)	.407 (.015)
Lowest EPI state	.154 (.020)	.284 (.024)
Highest EPI state	.256 (.020)	.378 (.022)

Note: Predicted probabilities generated from post-estimation Clarify simulations of ordered logit models (standard errors clustered by state).

^a Baseline respondent is an independent, white male, with mean values on other variables (age, education, media viewing, perceptions of officials corrupt, state EPI, and state id rules).

Source: ologit models replicating models I in Table 1, available from authors. Dependent variables range from 0 ("not at all often") to 4 ("very often").

perceptions that election officials are fair (p. < .01, two-tail) and that votes were counted fairly (albeit at p. < .07, twotail). It is possible that this reflects people who are more confident about the administration of elections being more likely to vote. However, that causal logic is not consistent with the fact that EPI, and components of EPI that remove turnout (see robustness tests below) predict greater confidence in elections. Turnout and EPI are highly correlated,¹² and we expect this reflects a close relationship between the quality of election administration and turnout. This suggests the potential of a causal process where, over time, improvements in the quality of electoral administration may increase voter access and turnout while also improving voter confidence in elections.¹³

The potential effects of voter identification laws on perceptions of electoral legitimacy appear limited. None of our models yield any significant, direct relationship between the strictness of a state's voter identification laws, and perceptions about the conduct of elections. We did anticipate that Republicans, as supporters of photo ID laws, might view the conduct of elections more positively where strict identification laws were in effect. There might be something to this, but the relationships, if any, are weak. The estimates show that although Republicans generally were less likely to view elections as fair, Republicans in states with strict identification rules were more likely to see elections as fair – but in each case the statistical significance (p = .09 two-tail) does not reach conventional levels.

Our mixed level models allow us an additional tool to assess the substantive magnitude of state-level versus individual-level factors. LR tests comparing the fit of mixed-level random effects ANOVA models to individuallevel OLS models reject the hypothesis that there is no cross-state variation in perceptions of officials being fair (p. < .0000) and in perceptions that vote counts were fair (p. < .000). However, we find that most (nearly all) of the variation in these attitudes is due to individual-level factors.¹⁴

Table 2 displays the substantive magnitude of the estimated relationships between key independent variables (state level EPI and individual-level demographic traits and perceptions of fair elections. For the sake of illustration, and given that individual-level factors are the major influence on these attitudes, the predicted probability of a respondent saying officials and vote counts were "very often" fair were estimated from ordered logit models (available from the authors).¹⁵ A respondent in a median EPI state had an estimated .204 probability of saying election officials were fair "very often," and a .333 probability of saying vote counts were fair very often. The probability of respondent saying this in the state with the highest EPI was predicted to be about .05 higher for each item. In contrast, partisanship had a much larger estimated effect on perceptions of fair vote counts – with Democrats predicted to have a .12 greater probability than Republicans of saying counts were fair. Table 2 also illustrates the extent to which women of colour were less likely to view officials and vote counts as fair. Compared to other respondents in a median EPI state, a Latina had a .12 lower probability of saying counts were fair very often, and a .08 lower probability of saying officials were. These effects of party, race, ethnicity and gender on perceptions of fair elections thus rival or exceed the measured effects of electoral performance.

8. Robustness tests

We conducted additional analysis to assess the veracity of these results. First, we examined if the relationships we detected between EPI and attitudes were a quirk of the 2012 ANES, or if models estimated on a different survey platform produced similar relationships. Second, we decomposed the EPI measure to assess if there were dimensions of the index that were associated with particular attitudes about elections. As for the first matter, the 2012 Cooperative Comparative Election Study (CCES) also included an item that asked respondents, "are election officials fair?" As with results reported here in Table 1, estimates using CCES data yielded a significant positive relationship between EPI and perceptions that officials were fair; with the estimated size of the coefficients of EPI similar regardless of whether CCES or ANES data were used (CCES estimates available from the authors).

We conducted a factor analysis to assess which dimensions of the EPI were related to perceptions of elections. This produced five unique dimensions of electoral performance, allowing us to replicate the ANES models in

 $^{^{12}}$ Being correlated at .73, both items cannot be included in the same model. When they are, neither term is significant.

¹³ Put differently, we have sound reason to expect that the quality of election administration, as measured by Pew, affects perceptions of electoral integrity. We have less reason to expect the opposite.

¹⁴ The inter-class correlation (ICC) for the percent variance explained by level-2 factors is .013 (1.3%) for attitudes about officials being fair, and .008 for attitudes about fair vote counts.

 $^{^{15}}$ The relationships between EPI and both questions about elections are significant (p < .01) when estimated with ologit.

terized by higher registration rates, high turnout, and low rates of disability related voting problems was significantly and positively associated with perceptions that officials were fair, and that votes were counted fairly. A second dimension of administration characterised high rates of data completeness, and low rates of military ballots being rejected also had a significant, positive association with perceptions that officials were fair and votes were fairly counted. Two other factors were associated with specific attitudes: People in states with higher scores on a dimension representing the presence of tools for looking up voting information were significantly more likely to say votes were counted fairly, while those in states scoring higher on a dimension that reflected online registration were significantly more likely to say officials were fair. This analysis suggest that rather than there being a single item in the EPI driving our results, there are multiple aspects of election administration captured by the EPI that are related to perceptions of elections.

9. Discussion

Even with some very conservative tests, we find that an objective measure of the quality of election administration explains some variation in perceptions of fair elections. In states that scored higher on a measure of administrative quality, people were more confident that election officials were fair, and that votes were counted fairly. Champions of reforms designed to improve the administration of elections should find solace in these results. Our analysis demonstrates that people viewed elections as fairer where elections were conducted better. By extension, this implies that improvements in the governance of elections could also promote democratic legitimacy, as fair elections are a key feature of democratic processes.

We suggest it is one thing to find greater confidence where elections are ran reasonably well than where they are notoriously bad, but another thing to find greater confidence across places where, by international standards, all elections are conducted reasonably well. A sensible intuition might have us expect that elections in Robert Mugabe's Zimbabwe do much less in establishing legitimate authority there, than compared to well-administered elections in established democracies. Similarly, many might expect variation in election quality to shape perceptions of legitimacy across emerging democracies, where 'quality' ranges from elections with wide-spread fraud and intimidation to elections that are relatively free and fair. But our study demonstrates that even within an established democracy, where systematic fraud is rare and where institutions and socialization forces produce expectations

that elections will be reasonably fair, people do appear to notice when things are running very well and when they are running less well.

This said, there are several points of caution. Patterns of what, for a want of a better term, we might call a 'democratic divide' in the US structured on demographic lines (race, gender) are both robust and worrying. One of the implications of these results is that large numbers of Americans remain sceptical of a key feature of their democratic process. It is unclear whether the kinds of factors identified in the Pew election performance index can be relied on to, eventually, bring all voters round to the idea that elections are conducted fairly. State election administrators and reforms alike may therefore also find cause for concern – and rationale for more targeted action – in the opinions tied to demographic patterns. It may be that no matter how well things are administered, there are many people – particularly those from groups who are under represented -who might still see the electoral process as flawed, and unfair.

A second note of caution is that it is difficult to evaluate the substantive magnitude of the relationships we observe between election performance, and attitudes about fair elections. The relationships between the Pew measure of state-level election performance and attitudes are significant and robust across many alternative model specifications, but little of the cross-state variation in these attitudes are explained by this measure of election performance. It seems unreasonable to expect large substantive effects from these features on attitudes about democratic processes; that is, we might not expect technical, administrative improvements by election officials to swamp the effects of individual partisanship, minority status, or the episodic effects of controversies such as bitterly contested recounts. It is nevertheless impressive that we find people to be modestly responsive to these rather routine features of elections.

Finally, it is worth underscoring some other limits of the results. It is notable, for example, that for all the sound and fury about voter photo ID there is very little evidence here or in additional results not-reported that photo ID had much effect on making people see the conduct of elections in a more positive light. It is also the case that the EPI measure had only a very modest effect on whether people thought votes were counted fairly, despite the large N of the sample. These points, in turn, raise a broader and possibly quite troubling question namely: in democracies where basic electoral practices are fairly well established, do marginal improvement in the quality of administration of elections 'matter' with respect to attitudes about democracy that are most important? Our results suggest that - in broad terms - technical improvements to electoral administration can improve voter perceptions of elections being fair. These are worthy accomplishments. But another implication of these findings is that substantial numbers of people remain unpersuaded that American elections are fair. We return, then, to an earlier theme. There are limits to what we can expect electoral reform to accomplish, and that applies to reforming election administration as well. None of that should be taken as an argument against improving electoral processes, but, rather, as a suggestion

¹⁶ The first factor represents states that had higher rates of provisional ballots cast, provisional ballots rejected, and higher rates of absentee ballots rejected and unreturned. This dimension of electoral performance was not associated with attitudes about elections.

to have modest expectations about what such reforms may accomplish.

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Exhibit 25

dailymontanan.com /2021/04/27/election-security-bill-heads-to-gov-gianfortes-desk/

Unknown Title

By Keila Szpaller :: 4/27/2021

19:24

News Story

Government & Politics

Election security bill heads to Gov. Gianforte's desk

Opponents say it will be found unconstitutional, like BIPA



Stacks of boxes holding cards and letters are seen at the U.S. Post Office sort center December 15, 2008 in San Francisco, California. On its busiest day of the year, the U.S. Postal Service is expecting to process and mail over one billion cards, letters and packages. (Photo by Justin Sullivan/Getty Images)
Rep. Wendy McKamey, R-Ulm, said people shouldn't give their ballot to someone who is paid to take it to the elections office, and most representatives agreed with her Tuesday, sending House Bill 530 to Gov. Greg Gianforte's desk.

"We want to keep it as clear and transparent and unimpeded and uninfluenced by monies as possible," McKamey said on the House floor.

But Rep. Tyson Running Wolf, D-Browning, said an amendment to the election security bill means many people living in rural and Indigenous communities won't be able to vote. As adopted, the bill states that a person can't provide or accept a pecuniary benefit in exchange for requesting or collecting a ballot.

"This bill effectively ends the legal practice of ballot collection, and it disenfranchises Native American voters en masse in the state of Montana," Running Wolf said.

Rep. Denise Hayman, D-Bozeman, described the amendment to the bill as "a backdoor version of the Ballot Interference Prevention Act, BIPA," a law that was litigated and found to be unconstitutional.

"This bill is certain to be challenged immediately, adding yet another costly lawsuit to the growing list of voting rights litigation," Hayman said.

A similar bill this session, House Bill 406, was indefinitely postponed in the Senate. Many opponents had spoken against the bill, also citing the court ruling against BIPA.

In September, a Montana court ruled in favor of Western Native Voice in its case against Corey Stapleton, then-Montana Secretary of State, over the Ballot Interference Protection Act. The court found the restrictions the law imposed on ballot collection efforts violated the plaintiffs' constitutional right to vote.

"Aside from the bill sponsor's assertion at the hearing, nothing in the legislative record supports a finding that Montana has or ever had a problem with unsolicited ballot collection or that ballot interference represents a compelling government interest in Montana," wrote Yellowstone County District Court Judge Jessica Fehr in the order. "The State admits there is not a single example of voter fraud in Montana caused by ballot collection."

(Even if there was a demonstrated problem, the judge said the law was not narrowly tailored enough.)

But the court ruling also noted the additional barriers Native American voters can face: "CSKT (Confederated Salish and Kootenai Tribes) members are more likely to live in the foothills and more rural parts of the reservation. This makes the travel times to amenities such as the post office more burdensome for many Natives than for non-Natives that live closer to amenities."

In his remarks, Running Wolf described some of the problems voters in rural areas face, which many other members of the public have also presented in testimony. He said in a state where a majority of people vote by mail, rural and tribal communities work with get-out-the-vote organizations to get ballots to election offices, which would otherwise be inaccessible because of distance, lack of access to transportation, or other socio-economic barriers.

"Ballot collection is a lifeline to democracy for our Native voters that pay taxes," Running Wolf said. "Ballot collection is a lifeline to democracy for rural and Indigenous communities. This bill would make it impossible for organizations to engage in that work."

McKamey, though, said while she hadn't asked for the amendment, she believed it strengthened the purpose of keeping elections safe, starting with the protection of a ballot. She said people could still help their family members collect ballots and deliver them, but some things would need to be different.

"There are going to be habits that are going to have to change because we need to keep our security at the utmost, and this is the first line of security is the security of our ballot," McKamey said.

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Exhibit 26

missoulian.com /news/state-and-regional/govt-and-politics/gop-in-missoula-pays-for-recount-to-ease-fraud-concerns/article_03...

GOP in Missoula pays for recount to ease fraud concerns



Sam Wilson

MISSOULA — Alarmed that conservative voters say they'll skip the 2022 elections due to fraud allegations pushed by local right-wing activists over the past year, Missoula County's Republican Party spent two days and \$5,000 reviewing public records this week in an attempt to put the conspiracy theories to bed.

The effort, which wrapped up Tuesday evening, sought to check the work of a hand-count last year led by a group calling itself the Missoula County Election Integrity Project, which has insisted they uncovered a discrepancy of more than 4,500 votes, compared with the official election results.

"You have a lot of new voters moving into Montana," said Missoula County Republican Central Committee Chair Vondene Kopetski. "Certainly I want them all to vote Republican. But I want them to be sure that Montana is a place where you can trust the elections departments, where your votes do count."

Missoula voters cast more than 72,000 ballots in the 2020 general election. The final count Tuesday found a difference of 71 envelopes below the expected total, or 0.1% — similar to typical margins of error for official canvasses in past elections, according to county elections administrator Bradley Seaman.

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Over the past year, Kopetski said the party's voter-registration drives have turned up a troubling number of potential GOP voters who told local organizers they don't plan to participate in the elections because they don't trust local officials to count their votes. They've cited repeated allegations from local activists that there were 4,000-odd fewer votes cast in the 2020 general election than were reported in Missoula County's official results.

Kopetski emphasized the local Republican Party had nothing to do with that previous effort. In January 2021, a group of about 20 activists — organized in part by state Rep. Brad Tschida, R-Missoula — conducted an inspection of ballot envelopes that they hand-counted under the supervision of election staff. Those envelopes, in which ballots are sealed, include voters' addresses and signatures. They are retained by the county and are considered public records. The ballots themselves are confidential and can only be viewed under a judge's order.

Seaman has argued that the 2021 group lacked a system to double-check its numbers, used lax recordkeeping and wasn't able to say whether the volunteers had actually gone through all the boxes of affirmation envelopes.

But over the past year, local Republican activists and right-wing politicians have traveled the state, broadcasting allegations that Montana's most liberal county had an unexplained 4,500-vote discrepancy.

One year ago, Tschida stood on the floor of the state House to speak of "fraudulent activities" in elections and warned cryptically that the group's findings would offer a "much clearer picture of how easy that is to allow to take place." And the following September, Tschida described the findings to a crowd of more than 200 at a widely advertised event at the Ravalli County Fairgrounds.

"We're asking for an opportunity to look at the ballots, to look at the data, and make sure that the information that was used in this election can give us confirmation and confidence that this election was fair," Tschida

told the crowd. "I'm not convinced. I don't believe it was, because there were just far too many little whiffs of smoke, and maybe some open fires, that lead me to believe this was not fair."

That opportunity to look at ballots may have passed, however. Seaman, who has repeatedly challenged the right-wing group to press its allegations in court, noted Monday that the window for doing so has closed. Under state law, official election results must be challenged within a year of the vote certification. For the 2020 general election, that deadline was in November 2021.

"If we were off by 5,000 ballots, that's huge, and it should go through a court process to contest the election," Seaman said Monday. In prepared remarks he delivered at the beginning of this week's hand-count process, he noted that "the previous requester did not stand by their claim enough to follow through on this process (by going to court)."

Asked last year about that possibility, Tschida and Missoula attorney Quentin Rhoades, who has represented the Missoula County Election Integrity Project, repeatedly demurred, saying they were instead bringing their concerns to the Secretary of State Christi Jacobsen, and that the ball was in her court. Jacobsen, a Republican, has never indicated publicly that she gives any credence to the group's findings, and has repeatedly declined to answer questions from the Montana State News Bureau about the issue.

In a pair of text messages Monday to the Montana State News Bureau, Tschida declined to answer questions about either of the envelope-counting efforts. Neither Rhoades nor Tschida attended this week's records review at the county elections office.

Two of the members of the election integrity group showed up Monday morning to observe a portion of this week's effort, including Lyn Hellegaard, a former Missoula City Council member.

Hellegaard, a Republican running for a state House seat this year, said she was "not willing to speculate" on why her group had come up with such a large discrepancy compared with the election results, or how to explain results this time around that conform with the county's numbers.

She added that her biggest concern with the current count was that more than a year had elapsed in the meantime, but declined to elaborate.

She referred follow-up questions to Rhoades, who could not be reached for comment Monday or Tuesday.

The process

On Monday morning, Republican volunteers seated themselves across from four tables each staffed by two election office employees. On one end of the room, 33 boxes of affirmation envelopes sat on a table, numbered and sealed with tape.

An elections staff member would grab a box and open it at each table, dividing the bundles of envelopes equally between the two employees, who would begin their count. After finishing, they switched places, recounting the envelopes to double-check the original tally.

No boxes were left open at the end of the day, and the ballot envelopes remained locked inside the building. After finishing a little less than half of the boxes Monday, the two groups returned Tuesday morning for a marathon second day, working through the final 18 boxes over the course of about nine hours.

A total of 72,632 ballots were cast in Missoula County in the 2020 general election. While the election was mail-ballot-only, variations like ballots submitted electronically from overseas, confidential voters and provisional write-in ballots meant that the county should have retained 71,924 ballot envelopes. The Republican group's difference of 71 amounted to a 0.1% variation from the official election results.

That's a nominal level of variance that is typical of election results, Seaman said, explainable by sorting errors, voided ballots that get left off reports and other isolated instances of human error.

"Humans are an element of this election. It was not exactly perfect — fractions of a percent," Seaman said.

The process took substantially longer than the 2021 count, which election integrity project members said lasted about five to six hours.

In response to an interview request about that initial count, Tschida sent a text message referring questions to Hellegaard.

"Any outcome other than the outcome of the first count would be highly suspicious, especially given that Bradley Seaman was effusive in his praise of the group's efforts," Tschida wrote in a text message Monday.

Seaman acknowledged Tuesday that he had complimented the group of volunteers during the 2021 envelope count.

"I think the people were great — it's a process issue," Seaman said. "The records requesters, Tschida and Rhoades, didn't have a process. And it's clear because their number was so far off from the actual, certified number. The people who came in had the best of intentions ... those are the type of people that we want involved in elections."

A national trend

That the allegations in Missoula County have found a receptive audience among Montana Republicans reflects a national trend.

Since former President Donald Trump lost the 2020 election and began blaming his loss on unsubstantiated election-fraud allegations, conspiracy theories to back up that assertion have proliferated.

A recent survey by the conservative American Enterprise Institute found cratering confidence in the election system among Republican voters, with as many as two-thirds believing that President Joe Biden's 2020 victory was illegitimate.

And some studies have found a clear line between the steady drumbeat of election-fraud theories and waning voter confidence in America's election system. A June 2021 study, published in the Journal of

Experimental Political Science, found that "unsubstantiated claims of voter fraud undermine the public's confidence in elections" — especially when those claims come from a politically comfortable source.

They added that fact-checking did little to change those perceptions.

More recently, a national survey by the Brennan Center for Justice and the New York University School of Law found that those allegations are having a dual effect of driving out experienced staff in the country's elections offices. Three-quarters of election officials responded that threats against staff have increased in recent years, and 20% said they are unlikely to stay in their jobs through the 2024 elections.

Seaman said after the voter integrity project published its findings last year, he received "a few phone calls and emails" that he found threatening. Rina Moore, the Cascade County Clerk and Recorder, said she's also felt threatened by people pushing election-fraud allegations.

During a recent election judge training in Great Falls, she said one of the attendees began yelling at her and her staff during a discussion on election fraud, alleging without evidence that there were 3,000 fraudulent votes in the county in 2020, and calling Moore and her staff "traitors."

Moore said she's also been kept busy with similar public-records requests, while pushing back against suggestions that vote-counting machines connect to the internet, an allegation Montana's Republican Secretary of State has personally rebuffed, and unsubstantiated allegations that her office was covering up possible fraud.

"How many times do we have to ram our head into a wall?" Moore said during an interview Monday. "It's so impossible to defend yourself against something that doesn't exist."

Correction

An earlier version of this story incorrectly referred the group that conducted a records request of ballot envelopes in 2021. The group is called the Missoula County Election Integrity Project, not the Montana Election Integrity Project.

Sam Wilson

State Bureau Reporter

Exhibit 27

montanafreepress.org /2022/04/01/missoula-election-allegations-challenged/

Missoula County GOP to Republican election skeptics: 'no voter fraud'

: 4/1/2022



Posted inElections

Despite repeated allegations by Republican lawmakers, a count of ballot envelopes by Missoula's Republican Party turned up no evidence of voting irregularities in 2020.



by Alex Sakariassen 04.01.202204.01.2022



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The Missoula County Elections Office and the Missoula County Republican Party spent roughly 20 hours this week counting ballot affirmation envelopes from the county's 2020 general election. Local party chair Vondene Kopetski said the effort was designed to "lay to rest once and for all" allegations of voting irregularities that arose from a citizen count of the envelopes early last year.

The latest count, conducted by elections office staff and observed by GOP volunteers, came up with a total of 71,853 envelopes — 71 envelopes shy of the 71,924 listed as received by the county in November 2020. Kopetski characterized the discrepancy as "statistically insignificant."

"I'm 100% confident in the results of our process," Kopetski told Montana Free Press, adding that "everybody, regardless of their party, can feel confident that their vote counts in Missoula County."

Missoula County Elections Administrator Bradley Seaman echoed that confidence, noting that the 71envelope discrepancy is within the margin-of-error range typical of a post-election canvass, and is attributable to what he called "the human element."

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3/28/2022 3 of 32	2045	3/29/2022	18 of 32	2194	3/29/2022	Set Aside by Previous Requestor	66	
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3/28/2022 4 of 32	2278	3/29/2022	19 of 32	2347	3/29/2022	Applied	528	
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A spreadsheet showing the final tally results this week from the Missoula County Republican Party's recount effort of 2020 ballot affirmation envelopes. Credit: Vondene Kopetski

Missoula's 2020 election became the focus of considerable debate last spring after Rep. Brad Tschida, R-Missoula, spearheaded a citizen effort to hand-count affirmation envelopes submitted by voters with their mail-in ballots. That count, made possible by a public records request filed by Tschida, took 20 volunteers five hours to complete and resulted in the group reporting a 4,592-vote discrepancy between the number of envelopes and the number of votes cast countywide. Tschida's subsequent allegations of voting irregularities prompted a rebuke from the Missoula County Commission, which questioned the accuracy of the group's count and its process and dismissed Tschida's claims as "baseless."

Kopetski said she grew concerned about the impact of Tschida's allegations after hearing from numerous voters last year that they felt "disenfranchised" and didn't plan to vote again because their vote "wouldn't count." She said the county GOP wasn't consulted about the Tschida-led effort, and she began working with Seaman in October to craft a process for a second count. After months of negotiation, the two finally settled on the process conducted March 28 and 29. This time, staff from the elections office did the counting while party volunteers observed and kept their own tally. Kopetski and Seaman said that after each bundle of envelopes was counted, Seaman's staff would switch seats and count the bundles again for verification.

"That's the biggest difference [from the previous count], is that cross-confirmation on the count helps ensure accuracy," Seaman said. "The other big difference is that we tracked the work that we were doing, so the

people who were going through and doing this process signed off on their work and showed that they'd counted each box and which box they had counted."

Kopetski estimated that with volunteers and supervisors also observing, each envelope was counted between three and five times. She added that the party's volunteers also supervised the transportation and unloading of the boxes containing the envelopes, and went so far as to run their hands along the tops of them "to make sure there was an appropriate amount of dust on that pallet, indicating that they hadn't been tampered with."

Related



Citizens allege election discrepancy

A records request for ballot envelopes in Missoula County has generated allegations about the 2020 election results, and given legislators fodder for debates about changing statewide election laws.

by Alex Sakariassen 04.02.202104.05.2021

Seaman said his office invoiced the Missoula County GOP \$5,000 for fulfilling its records request, but will likely refund half that amount as they'd originally budgeted for four full days of staff time.

While Kopetski hopes this week's results will finally put the Missoula County issue to rest, she said it will ultimately depend on people's willingness to believe in the accuracy of the work.

Lyn Hellegaard, a former Missoula City Council member who was involved in the previous envelope count, told MTFP that she stands by the process her group used in January 2021. She noted that unlike last year, the latest count was conducted not by volunteers but by election office employees. As far as resolution goes, Hellegaard said her underlying problem is that 15 months have passed since the first count was conducted.

"Too much time has passed for anything meaningful to happen," she said, adding that she plans to remain focused on potential "fixes" to state law in the 2023 Legislature that will address the concerns raised by last year's count.

Tschida did not immediately respond to a voicemail and text seeking comment. He was one of 10 lawmakers last month who requested that Republican Secretary of State Christi Jacobsen poll the Legislature about calling a special session in May for the purpose of voting on the establishment of a special committee to investigate election security in Montana.

"The conditions warranting the call," Tschida and the others wrote in their request, "are the continuing and widespread belief, among a significant majority of Montana voters, that sufficient irregularities in election security in Montana create serious doubt as to the integrity of elections in our state."

In light of the Missoula County GOP's effort this week, Kopetski sees that call as a needless waste of taxpayer dollars.

"Our results show that there is no voter fraud, at least the type that was being alleged as with missing affirmation envelopes," Kopetski said. "So I think it's disingenuous to be calling for a special session that would use taxpayer dollars when there is no problem."

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by Alex Sakariassen 03.31.202203.31.2022

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More by Alex Sakariassen

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