We All Agree: Strict Voter ID Laws Disproportionately Burden Minorities

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Voter ID laws determine who can and who cannot vote. Given the recent propagation of these laws by Republican legislatures, efforts by the Trump administration and other state legislatures to expand their reach, and the wide-ranging impact that all of this could have on racial and ethnic minorities, it is imperative that we understand their consequences. The research presented by Grimmer et al. misleads more than it informs. Their comment seeks to convince readers that voter ID laws help minorities as much as they help whites. That conclusion, however, contrasts with their own results. Although Grimmer et al. choose not to mention it, their reanalysis of our data confirms the core finding of our research, which is that strict voter ID laws discriminate. Far from raising questions about the impact of voter identification laws, their research serves to confirm our study and to demonstrate the racially disparate nature of these laws.

We thank Justin Grimmer and his coauthors for the chance to have a deeper conversation about the merits of strict voter identification laws. This discussion is extremely valuable and timely given the recent promulgation of voter ID laws by Republican legislatures, the enthusiastic support these laws have from the Trump administration and Attorney General Jeff Sessions, and the widespread impact that all of this could have on racial and ethnic minority voters.[1] At the very least, these laws will decide who can and cannot vote in the United States. It is critical that we get this right.

Ultimately, Grimmer et al. (2018, in this issue) does not offer a compelling criticism of our work. Nor does it add appreciably to the debate over voter identification laws. Instead, it presents a misleading and flawed picture of the impact of strict ID laws. Critically, the comment neglects to admit that the authors' reanalysis confirms the core conclusion of our 2017 article—that strict voter identification laws have a racially disparate impact. Other errors and exaggerations render the critique largely uninformative.

STRICT VOTER ID LAWS DO HAVE A RACIALLY DISPARATE IMPACT—WE *ALL* AGREE

Let us begin with the big picture. The main question we ask in our original *Journal of Politics* article is whether strict ID laws have a racially disparate impact on turnout. That is the critical question that the courts, scholars, party leaders, governors, voting rights advocates, and lawmakers ultimately want answered. More than anything else, the answer to that question will determine whether these laws are constitutional. As we wrote in our article, "the key test is not whether turnout is lower in strict voter ID states but instead whether there is a differential impact of these laws on racial and ethnic minorities, ceteris paribus" (Hajnal, Lajevardi, and Nielson 2017, 366). This point is not in dispute.

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1. Strict voter ID laws are currently in place in Arizona, Georgia, Indiana, Kansas, Mississippi, North Dakota, Ohio, Tennessee, Virginia, and Wisconsin. Lawmakers in several other states including Arkansas, Iowa, Oklahoma, and Texas are actively considering strict voter ID legislation (NCSL 2017).

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Fortunately, on this core question both sets of authors agree—strict voter ID laws do have a racially disparate impact. While Grimmer et al. do not mention this point in their comment, even a cursory look at their results reveals a racially disparate impact.

If we want to know whether these laws have a racially differential impact, the key variables in all of the regression models are the interactions between race and the presence of strict voter ID laws. Are these racial interactions negative and significant? On this most important test, Grimmer et al.'s replication of our analysis reconfirms our main findings.

Our regression models find that strict voter ID laws have a disproportionately negative effect on Latinos in general elections and on blacks, Latinos, Asian Americans, and multiracial Americans in primary elections. Grimmer et al.'s (2018) regression models in tables A.6–A.8 find the same pattern. Every one of their models finds a negative and significant interaction for Latinos in general elections.[2] Likewise, they find significant and negative effects for Latinos, blacks, Asian Americans, and multiracial Americans in primary elections.[3] None of the relevant interactions is positive, let alone positive and significant. To highlight the agreement between the two sets of authors on this fundamental and crucial point, table 1 displays the relevant interaction coefficients from both sets of analyses.[4]

Importantly, Grimmer et al. also neglect to mention in their comment that their results reconfirm our main findings in one other way. In our original article, we performed an additional difference-in-difference test that compared turnout changes in states that switched to strict ID laws with turnout changes in states that did not switch over the same time period. Our results (which are displayed in fig. 4 in Hajnal et al. [2017]) and Grimmer et al.'s results (which are displayed in table A10 of their online appendix) both demonstrate that the racial gap in turnout increases when strict ID laws are implemented. Again, it is unclear why Grimmer et al. do not mention this in their comment.

We do not know why Grimmer et al. neglect to reveal that their replication confirms our core analysis. This is especially surprising given the enormous policy implications of this finding. If voter ID laws have a racially disparate impact, which both Grimmer et al. (2018) and Hajnal et al. (2017) find, then a great deal of empirical evidence exists that these laws are discriminatory and unconstitutional. This evidence is likely to be used in lawsuits designed to overturn these rules. Such an important finding should be highlighted, not hidden.

Instead of focusing on this core issue, Grimmer et al. concentrate almost entirely on secondary findings and our data source. We address each of their criticisms in turn.

CCES TURNOUT ESTIMATES CLOSELY MATCH OFFICIAL ESTIMATES

The first issue they address has to do with our data source. Grimmer et al. (2018) contend that the survey data we use to test the impact of strict voter identification laws is flawed. They argue that the Cooperative Congressional Election Study (CCES) voter validation process is problematic and leads to estimates of turnout that "differ drastically from official administrative records" (**XXX**). To test whether CCES turnout figures are accurate, Grimmer et al. compare our estimates of turnout of *registered voters* in the CCES with turnout of the *voting eligible population* from official records. They then find that the turnout of registered voters and the turnout of eligible voters does not match and conclude that the CCES is inaccurate and that its matching procedure is flawed.

There is an enormous difference, however, between registered voters and eligible voters. The voting eligible population includes large numbers of citizens who are not registered to vote and who therefore cannot vote. Comparing these two populations is like comparing apples to oranges. The turnout of registered voters should be significantly higher than that of eligible voters because the masses of unregistered voters are not allowed to vote. Indeed, when Grimmer et al. do their test, their figures show that in the vast majority of cases, CCES estimates of turnout of registered voters by state are 10–20 points higher than official turnout figures for the voting eligible population. That is exactly what you find if you compare official registered voter turnout to official voting eligible turnout.[5] Rather than leading to concerns about the data, this should give us confidence in the results.

We performed one additional check to further assess the accuracy of the CCES turnout data. Although turnout of

2. The Extra Political Controls model contains a corrected version of three variables related to Republican control of the House, Senate, and governor's office. The findings from the original article hold with the corrected measures.

3. Grimmer et al.'s results in primary elections are a little less consistently significant for the two smallest groups—Asian Americans and multiracial Americans—across different model specifications. We believe that some of this is due to some questionable modeling choices by Grimmer et al. that we will address in more detail later, but even here the relevant coefficients are all negative and almost all close to significantly negative.

4. For comparison with Grimmer et al. (2018), we use ordinary least squares regressions in tables 1 and 2 rather than the logistic regressions in our original article.

5. For example, in 2004, the last year for which we have official registered voter turnout, turnout of registered voters (69.9%) outpaced turnout of the voting eligible population (60.1%) by 10 points.

Table 1. Do Voter ID Laws Have a Disparate Racial Impact? Key Racial Interactions

	Hajnal et al. Regressions						Grimmer et al. Replication		
	Table 1	Democrats Only	Extra Political Controls	Extra Demographics	Southern Control	State Fixed Effects	Apply Sampling Weights	Include Nonregistered	And Include Nonmatches
General election:									
Hispanic × strict ID	−.089**	−.105**	−.063*	−.071**	−.089**	−.070**	−.061**	−.053*	−.047+
	(.027)	(.025)	(.024)	(.021)	(.027)	(.025)	(.022)	(.026)	(.024)
Primary election:									
Black × strict ID	−.075**	−.062*	−.076**	−.071**	−.074**	−.067**	−.069**	−.061*	−.047*
	(.023)	(.030)	(.025)	(.022)	(.024)	(.024)	(.026)	(.026)	(.021)
Hispanic × strict ID	−.085**	−.074**	−.064**	−.088**	−.085**	−.075**	−.071*	−.058*	−.034
	(.022)	(.022)	(.020)	(.021)	(.022)	(.025)	(.027)	(.029)	(.028)
Asian × strict ID	−.100*	−.120*	−.087+	−.116**	−.098*	−.049	−.084+	−.048	−.024
	(.042)	(.058)	(.049)	(.043)	(.042)	(.051)	(.042)	(.036)	(.029)
Multiracial × strict ID	−.059+	−.063	−.057+	−.063*	−.058+	−.043	−.050	−.057+	−.047+
	(.032)	(.049)	(.033)	(.029)	(.032)	(.032)	(.034)	(.030)	(.025)

Note. All regressions are ordinary least squares to match with Grimmer et al. Logistic regressions lead to the same pattern. Standard errors in parentheses.

+ Significant at $p < .10$.

* Significant at $p < .05$.

** Significant at $p < .010$.

Table 2. Dropping Problematic Matching Data Does Not Alter Core Results

	General Elections		Primary Elections	
	Original Regression (State Fixed Effects)	Original Dropping All of 2006 and VA in 2008	Original Regression (State Fixed Effects)	Original Dropping All of 2006 and VA, LA in Every Year
Hispanic × strict ID	−.070**	−.061**		
	(.025)	(.022)		
Black × strict ID			−.067**	−.069**
			(.024)	(.026)
Hispanic × strict ID			−.075**	−.071*
			(.025)	(.027)
Asian × strict ID			−.049	−.084+
			(.051)	(.042)
Multiracial × strict ID			−.043	−.050
			(.032)	(.034)

Note. Standard errors in parentheses. VA = Virginia; LA = Louisiana.
+ Significant at $p < .10$.
* Significant at $p < .05$.
** Significant at $p < .010$.

eligible and registered voters should not be identical, they should be correlated since over two-thirds of eligible voters are registered. When we tested this, we found that CCES estimates of registered voter turnout by state and year were highly correlated with official data on voting eligible turnout ($r = .60$, $p < .01$).[6] Rather than demonstrating the erroneousness of our data, Grimmer et al.'s data seem to be demonstrating its accuracy.

At the same time, it is important to note that Grimmer et al. do correctly identify one particular state where CCES turnout estimates are clearly off—Virginia in 2008—and one particular year (2006) when CCES turnout estimates are problematic. We appreciate that Grimmer et al. were able to uncover these two issues. However, it is extremely important to note that none of these data issues affects our results.[7] There is no theoretical reason to expect that any of these matching problems is in any way correlated with the implementation of strict voter identification laws and thus no reason to expect that any of this can account for our findings. At most, these problems with matching should lead to greater error. In the end, they should, if anything, lead to null findings, not to consistently significant racial effects.

When we drop all of the questionable data and redo our analysis, it does not affect the core conclusions of our study. As table 2 demonstrates, dropping all of the 2006 data and the 2008 data from Virginia does not change the key coefficients in any meaningful way. We still find that strict ID laws differentially affect minority turnout. If anything, dropping the problematic cases that Grimmer et al. identify leads to even stronger conclusions about the disparate impact of voter identification laws.

THE EVIDENCE POINTS TO DECLINES IN MINORITY TURNOUT UNDER STRICT ID LAWS

The second claim put forward by Grimmer et al. (2018, **XXX**) is that our results imply that "voter ID laws increased turnout across all racial and ethnic groups." This is simply not true. Figures 1 and 2 present predicted probabilities for minority turnout in general and primary elections. These predicted probabilities are drawn from a range of different specifications from our analysis, as well as from Grimmer et al.'s alternate models.[8]

We present turnout estimates for all racial and ethnic minority groups where we found significant effects for strict

6. It is also worth noting that the correlations between estimated CCES registered voter turnout and official eligible voter turnout figures by state do not decline over time, a pattern we would see if the CCES data were getting worse over time, as Grimmer et al. imply. In fact the correlation is highest in 2014 ($r = .69$, $p < .01$).

7. Looking more closely at the data, we also discovered that in primary elections, the validated vote data for both Virginia and Louisiana are incorrect for all years. Dropping these additional data points also does nothing to alter the results.

8. We follow Grimmer et al.'s advice and drop data from 2006 and Virginia in 2008.

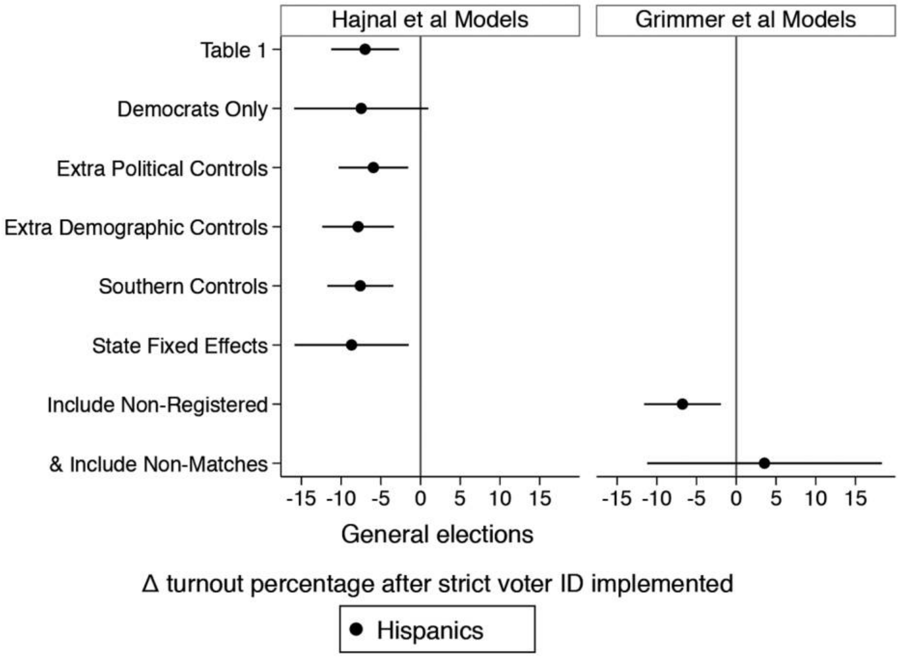

Figure 1. Declines in minority turnout under strict ID laws, general elections.

voter ID laws.[9] As can be seen on the left half of figures 1 and 2, all of the models from our original article estimate negative changes in minority turnout in strict ID states. That change is both negative and significant in almost all of the models. Across all specifications in general elections, we find that Hispanic turnout declines significantly. For primary elections, we also generally find negative and significant changes in minority turnout. Here, in short, the data point clearly to declines in minority turnout—not increases—when states enact strict ID laws.

If we shift to Grimmer et al.'s models, the results are similar. As the right halves of figures 1 and 2 illustrate, all but one of the point estimates are negative (if not always significantly so). There is one case when a Grimmer et al. model predicts slightly positive change in turnout (see Hispanic estimate at the bottom right of fig. 1). That positive point estimate, however, is not even close to significant. Moreover, there is one fundamental problem with this particular model in that it relies on questionable decisions about whom to include as nonvoters and whom to include in the analysis. Grimmer et al. include all respondents who do not match the Catalist voter file and assume they are all nonvoters. This is a deeply problematic assumption. Respondents will not match the Catalist file for a variety of reasons including if their names are misspelled and if their listed addresses are incorrect. These are not nonvoters; these are simply people whose information was notated incorrectly. Grimmer et al.'s decision to include nonmatches as nonvoters, therefore, is likely to introduce error and bias estimates downward. Although there is a debate in the literature, the norm in the field is not to include nonmatches as nonvoters. It is also worth mentioning that Grimmer et al.'s decision to count all nonregistered Americans as nonvoters is likely to bias estimates downward because it arbitrarily adds a mass of people who are unable to vote whether a strict ID law is in place or not. If we want to assess the impact of voter ID laws, the focus should be on registered voters and not the masses of Americans who cannot vote regardless of what law is in place.

It is impossible to argue from all of these results that strict voter ID laws lead to greater minority turnout. It would even be a stretch to argue that the results are inconclusive. Once again, when Grimmer et al.'s analysis is examined more closely, it tends to confirm the results of our original analysis. Minority turnout decreases in states that enact voter ID laws.

How then do Grimmer et al. claim to find one case in which strict ID laws lead to increases in minority turnout? They find and highlight the one model that actually does predict significant positive change in minority turnout. Namely, they focus on a state and year fixed effects model

9. Even if we calculate predicted turnout estimates for the other racial and ethnic minority groups where we found that strict voter identification laws did not have a significant impact, we still find no case in which strict voter identification leads to a significantly positive change in turnout. Generally, speaking for these other minority groups, we find that predicted turnout under strict ID laws is not significantly different from zero—exactly what you would expect given the lack of a significant relationship in the first place.

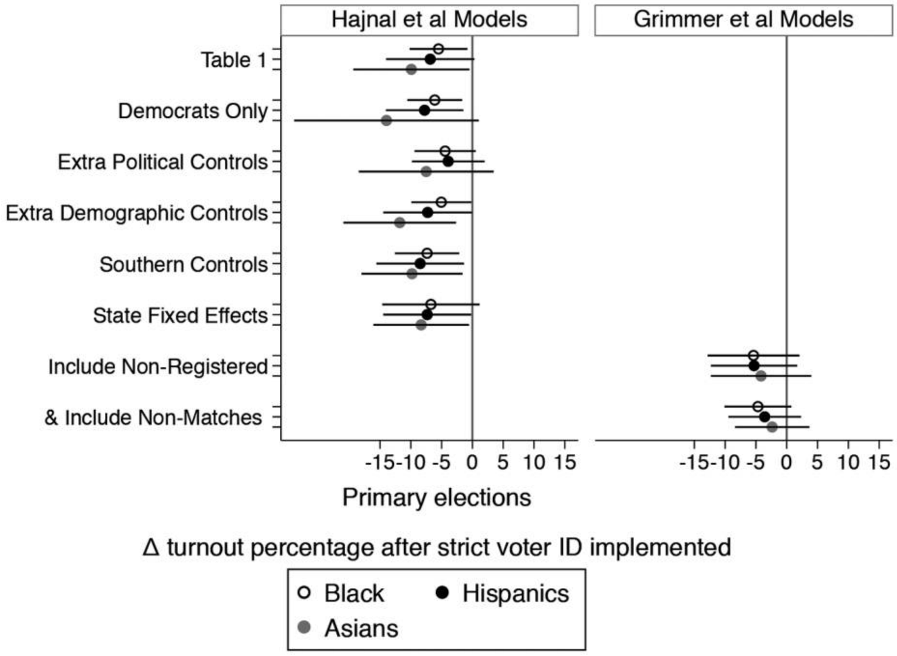

Figure 2. Declines in minority turnout under strict ID laws, primary elections.

from the online appendix of our 2017 article. Unfortunately, as we clearly communicated to Grimmer and his coauthors, that model is flawed. It is the only one of our regression models from the article or the online appendix that does not incorporate clustered standard errors or sample weights. It is also missing critical control variables. It should not be used by anyone and should certainly not be used as a linchpin for a critique. We do not know why after being informed of the mistake, Grimmer et al. continued to highlight a model that does not include clustered errors and sample weights—two inclusions that are standard in this kind of case and that Grimmer et al. themselves say are necessary. In short, Grimmer et al. ignore all of the results that reconfirm our original story while highlighting the one clearly incorrect specification that suggests otherwise.

Placebo effects

Grimmer et al. also run a placebo test that leads them to conclude that there is omitted-variable bias in our models. There are two problems with their test and with their conclusion. First, they only run the test on an incomplete and less rigorous model and neglect to run it on a more complete model that includes state and year fixed effects. Had they run the placebo test on the complete state and year fixed effects model, they would have found that the placebo test confirms that omitted-variable bias does not account for our results. When we rerun their placebo test with our state and year fixed effects models, we find absolutely no significant interaction effects for blacks, Hispanics, and Asian Americans with states that will enact strict voter ID laws in the future.[10] That is true whether we focus on turnout in the primary or turnout in the general election. The results of a placebo test for general and primary elections are displayed in table 3.

All of this indicates that the findings from the more complete model are not driven by omitted-variable bias. In writing our *Journal of Politics* article, we were aware of concerns related to omitted-variable bias, and we duly noted those concerns in the article. That is exactly why we sought to reconfirm our results with a state fixed effects model. Ignoring that state fixed effects model when running the placebo tests, therefore, makes little sense.

Second, even if we focus on Grimmer et al.'s limited placebo test, a closer read indicates the test, in many ways, helps to confirm our story. When Grimmer et al. run the test on primary elections, they find that future implementation of strict voter ID laws has no significant effects on overall turnout and no significant effects for any of the key Hispanic or black interactions (see models 7–12 of table A4 in Grimmer et al. [2018]). In other words, the test suggests that there is no omitted-variable bias in the model for primary elections. The only reasonable conclusion to draw from the Grimmer

10. There is one significant interaction for multiracial Americans in primary elections but given the small number of respondents in our surveys in that racial group and the fact that our original results related to multiracial Americans were often not significant, we did not emphasize the multiracial American results and we consider any conclusions about multiracial Americans to be tentative at best.

Table 3. Placebo Test with Full Model Shows No Omitted-Variable Bias in General Election

	General Elections	Primary Elections
Black × future strict voter ID state	.081	.032
	(.157)	(.135)
Hispanic × future strict voter ID state	.085	.087
	(.112)	(.083)
Asian × future strict voter ID state	.505	.244
	(.548)	(.192)
Multiracial × future strict voter ID state	−.204^{+}	−.726**
	(.123)	(.122)

Note. Standard errors in parentheses.
$^{+}$ Significant at $p < .10$.
* Significant at $p < .05$.
** Significant at $p < .010$.

et al. analysis, then, is to have greater confidence in our findings for primary elections.[11]

We do not argue that omitted-variable bias is not a concern. Indeed, we ran a state and year fixed effects model for this very reason. But given the evidence that Grimmer et al. themselves provide, there is little reason to believe that omitted-variable bias is driving our results.

Other concerns

Both in the text and in the footnotes of their comment Grimmer et al. claim to uncover a number of smaller errors. In a small number of those cases we readily admit error and have made adjustments to correct those errors. In most of the cases, however, we heartily dispute the claims, and in all of those cases, the issues are minor and inconsequential to an overall assessment of strict voter ID laws. Given space constraints we do not engage these lesser concerns in our response.[12]

Another issue is that Grimmer et al. pick and choose numbers and examples throughout their comment to make it seem as if our data are limited and our results flawed. They note, for example, that of all the respondents in Kansas in 2014 in our data "only 17 and 24 . . . are black and Hispanic, respectively" (2018, n. 2). They also highlight problems in one or two other states with tiny minority populations. These critiques ignore the fact that we are assessing strict ID laws across 51 elections (26 general election contests and 25 primary contests) in 10 states with strict voter identification laws in place. In reality, our data include over 10,000 votes/nonvotes by racial and ethnic minorities in states with strict voter ID laws in place. Our conclusions do not rest on one tiny state with few minorities, as many of their examples suggest, but rather on a reasonable sample of minorities across multiple elections in multiple states. This particular Grimmer et al. critique also conveniently ignores the fact that we highlighted the same concern in our original article. Noting our concern about interpreting tests from a small number of states, we wrote: "we view these tests with considerable skepticism . . . although we have a large data set, when we focus on the turnout of a particular minority group in a particular state in a particular year, our *N*s get quite small, samples are less likely to be representative, and presumably the errors in our estimates get very large. This is less of a problem when looking at overall aggregate turnout, but it becomes severe when focusing on differential changes in turnout for minority groups like Latinos and African Americans" (Hajnal et al. 2017, 376). There is nothing new about this Grimmer et al. critique. It distorts more than it enlightens.

Finally, it is important to address Grimmer et al.'s concern that our results "deviate substantially from other published findings of a treatment effect of zero or close to it" (2018, **XXX**). It is true that our results differ from some published studies. But it is also true that our results mirror new studies that employ rigorous difference-in-difference tests (GAO 2014). More importantly, Grimmer et al.'s concern ignores all of the major research design differences between our study and previous research—differences that we highlighted in the text of the original article. As we note in the article, published work on strict ID laws has only looked at the effect of these laws in one or two states and in one or two elections. We examine the effect of these laws across over 51 elections over 5 election cycles in 10 states with strict ID laws. We also use the validated vote

11. Similarly, when Grimmer et al. run their placebo test on general elections, they find that future implementation of strict voter ID laws has no significant effects for any of the key Hispanic or black interactions (see models 1–6 in table A4 in Grimmer et al. [2018]). Thus, it seems very unlikely that omitted-variable bias in this model is driving our core finding—that strict ID laws have a disproportionate impact on Hispanic turnout in general elections.

12. One of these smaller claims that we cannot help but engage relates to a dummy variable for the first year of strict ID laws. According to Grimmer et al. (2018, **XXX**), we include that variable "without sufficient explanation." In response, we will simply quote from our original article: "Given the claim that the initial passage of these laws can temporarily fuel anger and voter mobilization, we add a dummy variable to test for this temporary mobilization (Citrin, Green, and Morris 2014)" (Hajnal et al. 2017, 367 n. 12). We also note that across our different models and specifications, the dummy variable for the first year of a strict ID law is always insignificant in general elections and usually insignificant in primary elections.

rather than more error-prone self-reported turnout data. And finally, unlike most other past studies, we single out strict ID laws for testing, rather than looking to see whether there is a pattern of effects from less strict to more strict states. It would be remarkable if our results matched previous published studies.

Ongoing concerns about voter identification laws

We appreciate the time and effort that Grimmer, Hersch, Meredith, Mummolo, and Nall have devoted to this important topic. With strict ID laws already in place in 10 states, several other states considering similar laws, and the Trump administration actively seeking to expand the reach of these laws, we need to know what they do and most importantly how they affect racial and ethnic minorities.

Unfortunately, the research presented by Grimmer et al. misleads more than it informs. Their comment seeks to convince readers (and even perhaps lawmakers and the courts) that strict voter ID laws help minorities as much as they help whites. That conclusion, however, contrasts sharply with the empirical record and critically with their own results.

Although Grimmer et al. choose not to mention it, their reanalysis of our data confirms the core finding of our research, which is that strict voter ID laws discriminate. When strict ID laws are put in place, the already significant gap in turnout between whites and racial and ethnic minorities grows, and American democracy becomes even more skewed. That is what we find. That is also what their results show. Even on the narrower question of whether minority turnout goes up or down when ID laws are put in place, the data are clear. Our own results point to declines in minority turnout. A closer inspection of the Grimmer et al. analysis reinforces this view. Far from raising questions about the impact of voter identification laws, their research serves to both confirm our study and demonstrate the racially disparate nature of these laws.

Voter identification laws are too important and too harmful to racial and ethnic minorities to obfuscate the truth with debates about minor and inconsequential data issues. At the end of the day the empirical record is clear. Readers, the courts, and politicians all need to know the consequences of these laws. The evidence put forward in both our article and in Grimmer et al.'s critique confirms the negative impact of these laws. All of this should continue to sound alarm bells to those who wish for equal access to the ballot in American democracy.

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