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ARTICLE

Does threatening their franchise make registered voters more likely to participate? Evidence from an aborted voter purge

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Abstract

Prior research predicts that election administration changes that increase voting costs should decrease participation, but it fails to consider that some interpret those changes as attacking their franchise. Drawing on psychological reactance theory, this study tests whether such perceived attacks might instead activate those citizens. It leverages the State of Florida's multi-stage effort in 2012 to purge suspected non-citizens from its voter rolls, comparing the voting rates of suspected non-citizens whose registration was and was not formally challenged by the state. Within-registrant difference-in-difference and matching analyses estimate a positive, significant participatory effect of being challenged, particularly for Hispanics (the vast majority of the sample). Placebo tests show that those challenged were no more likely than those not challenged to vote in previous elections.

Keywords election reform and administration; voter turnout; psychological reactance; voter purge; Florida

Scholars have long predicted that changes to electoral policy and administration affect the decision to vote by altering the costs of turning out, which raises significant concerns about the participatory consequences of efforts to restrain the decades-long movement toward easier ballot access in the United States. Whereas relaxed registration procedures, early voting opportunities and easier poll access are expected to increase turnout, recent state actions that raise voting costs, including additional barriers to registration, the reduction of early voting days, proof of citizenship requirements and strict voter identification laws, are predicted to deter participation for at least some who might otherwise vote. Of particular concern is that these new costs often fall disproportionately on those already under-represented in the electorate, such as minorities, and thus might further exacerbate existing inequalities in participation (Barreto, Nuño and Sanchez 2007, 2009; Hajnal, Lajevardi and Nielson 2017; Herron and Smith 2014, 2016; Hood and Bullock 2008; Keyssar 2009; Minnite 2010; Wang 2012).

Although the 'costs' framework generates sensible predictions that are supported by considerable (though not unanimous) empirical evidence, it fails to account for the potential backlash from individuals who perceive such policy changes as threatening their right to vote (because of their difficulty in 'paying' that new cost and/or their membership in a group they believe the change is designed to target) (Alvarez, Bailey, and Katz 2010; Amos, Smith, and Ste. Claire 2017; Brady and McNulty 2011; Hajnal, Lajevardi, and Nielson 2017; Hanmer 2009; Haspel and Knotts 2005; Herron and Smith 2014; Leighley and Nagler 2014; Stein and Vonnahme 2008; Wolfinger and Rosenstone 1980; but see Berinsky 2005; Burden et al. 2014;

Grimmer et al. 2018).[1] For at least some of these potential voters, we posit that this affront to their democratic right to vote may actually stimulate turnout. We explain this response by drawing on psychological reactance theory (Brehm 1966; Brehm and Brehm 1981; Wicklund 1974), which predicts that a threat to a freedom enhances its perceived value and generates a motivation to reclaim that right. This response can be sufficiently strong that the individual takes steps to protect or engage in that right even if it was rarely used previously (see Burgoon et al. (2002) for a review), an expectation consistent with prior work demonstrating that threats across different contexts can spur participatory action (Miller and Krosnick 2004; Valentino and Neuner 2017). Thus policies that increase the cost of voting in this manner might increase the importance one attaches to the act and motivate even infrequent participants to turn out.

Our analysis, which examines the turnout of registered voters who were targets of the State of Florida's 'non-citizen' registrant purge prior to the 2012 presidential election, provides the first empirical evidence that such costs can, in fact, heighten the propensity to vote. Under this administrative action, the state initially identified roughly 180,000 registrants it suspected of being non-citizens, and thus ineligible to vote, before selecting a subset from this list to receive letters officially challenging their eligibility. Those challenged were threatened with removal from the voter rolls unless they provided documentary proof of their citizenship.[2] Consistent with other purging efforts in the United States that ensnared eligible (and disproportionately minority) registrants (Keyssar 2009; Minnite 2010; Scher 2011; Wang 2012), inaccuracies in Florida's purge list created a situation in which, absent a reply from those under suspicion, some individuals legally entitled to vote would be disenfranchised by this process. Due to pending lawsuits, the purge was abandoned before the election (but not before a large number of those challenged faced this additional voting cost), and all challenged citizens (even those who did not respond to their challenge) were allowed to vote in the November 2012 general election.[3]

We exploit the multi-stage process of this challenge to identify the effect of the state threatening registered voters' right to vote on turnout.[4] In lieu of comparing the participation of those whose eligibility was challenged by the state to the average registrant, who differs on a number of observable and unobservable characteristics, we instead compare within the original list of registered voters who the state suspected were 'non-citizens' (that is, those on the list who were challenged to those who were not). Although the state did not randomly select from this list those registered voters it initially selected to be purged, the two groups share very similar (observable) demographic, partisan and vote history profiles, and presumably also share the same set of unobservable characteristics that contributed to their identification as a potential non-citizen. We then estimate the effect of assignment to purge on turnout using two distinct approaches that account for remaining covariate imbalances and permit us to overcome the non-randomness of that assignment: a within-registrant difference-in-difference analysis, and pre-processing the data through matching.

In both sets of analyses, we find that the non-citizenship challenge increased overall voter turnout by a substantively meaningful 2.2–3.0 percentage points. This effect is an even larger (often by at least one-third) and consistently statistically significant 2.5–4.8 points for Hispanics, who as more than three-quarters of both the treatment and control groups are the subgroup for which we can generate the most precise estimates. Placebo tests reveal that registered voters on the state's final purge list were no more likely (and possibly less likely) to vote in previous contests than those on the state's more expansive initial purge list who were not ultimately challenged, signaling that the observed treatment effect does not stem from some unobservable

[1]We refer to perceived, not necessarily actual, motivations of electoral administrative policies.

[2]In some American states (but not Florida), non-citizens can register and vote in local but not state or federal races. The federal registration form that all states and locales must accept, however, requires only that an individual attest to their citizenship (not prove it) (Hayduk 2006).

[3]All challenged registrants were mailed a second letter informing them of their eligibility.

[4]We refer to those challenged who are legally entitled to vote. We discuss the likelihood of non-citizens voting, the evidence of which is rare (Minnite 2010), below.

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factor correlated with a general higher propensity to vote. Additional discussion of alternative mechanisms rules out information or reminder effects as explanations for the observed higher turnout.

Our findings speak broadly to the participatory effects of election administration changes and the response of individuals when their rights are threatened, with implications that are increasingly relevant outside the American context. On the issue of citizenship and voting, non-citizens in numerous countries are permitted to participate in subnational elections. European Union (EU) member states, for example, allow citizens of one member to cast ballots in the local elections of another in which they habitually reside (Ziegler 2017), and in the 2016 'Brexit' referendum immigrants from over 50 Commonwealth countries residing in the United Kingdom (UK) were permitted to vote on the ballot measure (Ponniah 2016). However, with increasing concerns over both legal and illegal migration patterns that challenge the definition of 'eligibility' – epitomized by movement within Europe and by African and Middle Eastern refugees migrating to Europe – domestic pressures are likely to continue to mount in some European countries to strip non-citizens of their voting rights and/or impose increased costs on eligible voters. (See, for example, the vigorous campaigning of anti-immigration groups against the right of foreign nationals to vote on whether Britain should exit the EU or the voter identification requirements being piloted in UK cities with large Muslim communities (Barret 2015; Swinford 2016)). Given such political pressures, even countries with (semi-)independent election administration agencies may not be immune to calls to place barriers on the franchise or perhaps even to cleanse the voter registry of eligible non-citizens from the rolls. We expand upon this discussion with regard to the policy consequences of our findings and avenues for future work in the conclusion.

The Threat to Voting and Psychological Reactance

Despite the well-documented concerns regarding the precision of the calculus of voting model (see Blais (2000) for a review), the impact of an electoral policy change is predicted primarily in terms of how it affects the cost of voting. According to the model, a reduction in costs through easier registration procedures and poll access, election day registration and early voting policies should lead to higher turnout rates (Hanmer 2009; Leighley and Nagler 2014; Stein and Vonnahme 2008; Wolfinger and Rosenstone 1980).[5] In contrast, imposing additional voting hurdles (for example, strict voter photo identification laws or citizenship challenges), or even rolling back these reforms (for example, curtailing weekend early voting days, eliminating same-day registration or moving election day polling places), should lead to lower turnout rates (Alvarez, Bailey and Katz 2010; Amos, Smith, and Ste. Claire 2017; Brady and McNulty 2011; Hajnal, Lajevardi and Nielson 2017; Haspel and Knotts 2005; Herron and Smith 2014).[6] Though the costs of voting literature is not conclusive and has not yet empirically investigated proof of citizenship as a potential barrier, it does point to a possible differential demobilizing effect that such a cost would have for at least some groups of otherwise eligible voters.

That expectation, however, ignores any backlash that perceived efforts to restrict the freedom to vote, including the rolling back of convenience voting reforms, might engender. Such negative perceptions are likely particularly prevalent among certain groups for whom these costs are consistently shown to be larger, especially given how opponents often discuss and frame their purpose (Hasen 2012). In the American context, blacks and Latinos possess acceptable photo identification at lower rates than the general population (Barreto, Nuño and Sanchez 2007, 2009; Hajnal, Lajevardi and Nielson 2017; Hood and Bullock 2008), rely more heavily on in-person early voting (Herron and Smith 2014, 2016), and are disproportionately selected in voter purges for removal from the registration rolls (Keyssar 2009; Minnite 2010; Wang 2012). As such,

[5]But see Berinsky (2005); Burden et al. (2014) on convenience voting.

[6]But see Grimmer et al. 2018.

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members of these racial and ethnic groups may believe that they are the intended targets of such policies and interpret them as an attack on their voting right because of the group with which they identify.

We suspect that for some that backlash has positive participatory consequences. Specifically, we draw on psychological reactance theory, which suggests that threatening a right arouses a motivational state designed to protect and, if eliminated, reinstate the freedom (Brehm 1966; Brehm and Brehm 1981; Wicklund 1974). That 'threat' can be any action that increases the perceived difficulty of being able to exercise (or not exercise) a specific right (Brehm and Brehm 1981). The consequences of this heightened state depend on the importance of the right that is threatened; if the victim cares little about the lost freedom, then the restriction could elicit some reactance but is unlikely to generate a strong response and may be largely ignored (Burgoon et al. 2002; Stewart and Martin 1994). It should be noted, though, that the extent to which an individual values the right under attack does not necessarily correspond to the frequency with which she engages in it (Wicklund 1974). That is, the limitation of a rarely used right can still generate psychological reactance as long as the individual considers important the option to partake in it should she choose to do so.

In general, the magnitude of the reactance increases with the level of importance attached to the right and is amplified when the motivation behind the threat is perceived as personal (that is, intentionally targeted at the individual) rather than impersonal (Brehm 1966). For those freedoms considered important enough, the restriction may have the opposite impact intended. Consequently, this 'boomerang effect' frequently enhances the attractiveness of the right under attack and can cause hostility toward the limitation's source. If the freedom is sufficiently valued, the reactance may manifest itself as a desire to reclaim the right or reduce the probability of its loss, including a heightened interest in, and actual attempts to, engage in the prohibited behavior (Wicklund 1974). This reaction serves to not only re-establish or protect the freedom, but to demonstrate that one's ability to partake in the right has not been eliminated or otherwise diminished.

Research in social psychology extensively demonstrates the consequences of psychological reactance. For example, efforts designed to reduce alcohol consumption (Albarracin, Cohen and Kumkale 2003; Bensley and Wu 1991), smoking (Grandpre et al. 2003) and unsafe driving habits (Laurin, Kay and Fitzsimons 2012) often elicit sufficient reactance that those targeted subsequently value that freedom to a greater degree and/or report greater intentions to partake in the discouraged activity. This response alters actual behavior as well. In the lab, individuals who receive anti-drinking messages consume more alcohol in taste tests (Bensley and Wu 1991), and those exposed to labels warning of the negative health effects of foods tend to select the less healthy options (Bushman 1998). In the field, forced changes in consumer product use and efforts to encourage pro-social behavior (for example, recycling or not littering) may generate pushback toward the desired action.[7] Relatedly, the failure of some public health campaigns to reduce alcohol and drug use is often attributed (at least in part) to their outreach eliciting reactance among those targeted (Ringold 2002; Stewart and Martin 1994).

As those studies illustrate, reactance can be aroused by threats specifically targeted at an individual as well as broader-based restrictions that she perceives as personally targeting her. Thus in the political sphere, a universal policy that affects all potential voters (for example, a photo identification requirement) may elicit this response if an individual believes it is designed to restrict *their* right to vote (perhaps because of her race or ethnicity). Yet despite this ability to affect behavior, we are aware of only one study that examines the possibility that reactance may drive political participation.[8] In that investigation, Biggers (2014) demonstrates through a field

[7]For a review, see Burgoon et al. (2002).

[8]To our knowledge, the only other efforts to explain political action using reactance theory involve understanding how this response influences the effectiveness of social pressure mobilization (Gerber et al. 2008; Gerber et al. 2010; Mann 2010; Matland and Murray 2012; Murray and Matland 2014).

experiment that a mailing stressing the discriminatory purpose of voter identification laws can engage at least some segment of the population perceived to be the policy's target (African Americans and Latinos). More broadly, related work points to the potential mobilizing influence of threat. Feelings of threat toward political candidates or an undesired change in policy, for example, can induce letter writing to Congress, donations to groups fighting the policy change and greater engagement with campaign activities (Campbell 2003; Marcus, Nouman and MacKuen 2000; Miller 2000; Miller and Krosnick 2004; Tam Cho, Gimpel and Wu 2006). The arousal of anger in response to threats is associated with increased intent to participate and self-reported participation in non-voting political activities (Groenendyk and Banks 2014; Valentino et al. 2011). That relationship extends to new electoral barriers, as the anger caused by voter ID requirements increases reported intention to both vote and donate money, and potentially counterbalances any demobilizing effect the laws might have on voters (Valentino and Neuner 2017). But because the anticipated mechanism is distinct, this work does not speak to whether those potentially disenfranchised by an administrative policy, or who interpret it as an affront to their voting right, experience reactance. We now turn to this question.

Registered Voter Purges

We seek to clarify whether the threat to one's franchise raises the likelihood of a registered voter turning out through the examination of a statewide 'non-citizen' registrant purge. In the United States, election administration is handled by the states, with local election officials ultimately responsible for the day-to-day management. As part of this administration, all states except North Dakota require a citizen to register before she can vote, and that record serves as verification of her eligibility to participate (Wolfinger and Rosenstone 1980). State and/or local officials maintain these records, or voter rolls, including the routine removal, or purge, of registrants who are no longer eligible to vote. Federal legislation provides broad guidelines for this process, but officials have considerable leeway, and in practice there are substantial discrepancies in this procedure's implementation across states and local jurisdictions. In theory, purges help ensure registration accuracy and electoral integrity. Most often, this action removes duplicate names or registrants who have changed their address, died, been convicted of a disenfranchising crime or deemed mentally incapacitated, or failed to vote over a specified time period (Ansolabehere and Hersh 2014).

The identification of those to purge, however, is often at best imperfect and at worst severely flawed (Hasen 2012; Stewart 2014; Wang 2012). Generally, election administrators match voter registration records with another administrative database, which creates two possible sources of error (McDonald and Levitt 2008). First, the criteria used to conclude that two records correspond to the same person may be insufficient to prohibit false positives. For example, matching only on first name, last name and date of birth can frequently identify different people as the same individual. Secondly, one or more matched records may include information entered incorrectly (for example, a name or date of birth) or that is outdated (for example, a legal immigrant record created before the person became a naturalized citizen or a felony conviction record that does not note the restoration of the right to vote).

Either type of error may result in eligible registrants being wrongly targeted for removal from the voter rolls, thus rendering them unable to cast a regular ballot.[9] When Florida purged convicted felons from its lists in 2000 and 2004, for example, the state also cancelled the registration of thousands of eligible voters (Keyssar 2009; Wang 2012). Similarly, the St. Louis Elections Board wrongly disenfranchised many eligible voters by removing from the rolls tens of

[9]Under Section 302 of the Help America Vote Act (2002), individuals whose names are not on the registration list at a polling station are permitted to cast a provisional ballot, but many are rejected (Kimball, Kropf and Battles 2006; Merivaki and Smith 2016).

thousands of inactive Missouri registrants prior to the 2000 presidential election (Minnite 2010), a practice that was also illegally carried out in Ohio prior to the 2016 general election (Johnson 2016). In Georgia in 2008, many registrants challenged as non-citizens (and thus threatened with removal from the voter rolls) were able to produce the required proof of citizenship. That state's actions were challenged by the US Department of Justice, which alleged that it sought to use a 'flawed system [which] frequently subjects a disproportionate number of African American, Asian, and/or Hispanic voters to additional, and more importantly, erroneous burdens on the right to register to vote' (Wang 2012, 97). The Justice Department's claim that the laws disproportionately targeted minorities (either by intent or otherwise) reflects a pattern in voter purges, both historically and in recent state actions (Keyssar 2009; Kousser 1974; Minnite 2010; Wang 2012). This holds true in the Florida case, as the majority of those whose citizenship was challenged were Hispanic.

2012 Florida 'Non-Citizen' Registrant Purge

Our analysis concerns Florida's efforts to purge non-citizens from its voter rolls before the 2012 presidential election. In the spring of 2011, the Florida Department of State (FDOS) worked with the Department of Highway Safety and Motor Vehicles (DHSMV) to access its Drivers and Vehicle Information Database (DAVID), which includes citizenship and immigration-status information for those who have a driver's license or state-issued identification card. Through an automated process that matched names from DAVID and the FDOS voter file, the DHSMV identified 180,506 potential non-citizens (hereafter the 'suspect list'). Based in part on that list, the FDOS in April 2012 identified 2,625 individuals to formally challenge (hereafter the 'challenge list') (Detzner 2012). The methodology used by the FDOS to select those on the final challenge list is unclear, as many registrants (1,206, or 46 per cent) not on the original suspect list ended up on the challenge list, and thus apparently were not identified through the use of DAVID records.[10] That same month, the FDOS disseminated the challenge list to the state's sixty-seven county Supervisors of Elections (SOEs) and directed them to send a letter to the suspected non-citizens requiring them to present evidence of citizenship within thirty days of the notice to remain on the voter rolls (Detzner 2012).

In response, many Florida SOEs voiced significant doubt about the challenge list's validity, especially as many of the flagged registrants turned out to be citizens.[11] In a June 2012 letter to the Secretary of Homeland Security, the FDOS acknowledged as much, stating that it 'soon recognized that the most significant limitation in its process was outdated citizenship-status information contained in a person's DAVID record, which is only as current as the person's last interaction with DHSMV' (Detzner 2012). Evidently, the FDOS' challenge list was based on dated information and contained a potentially large number of individuals who had become naturalized citizens since their last interaction with the DHSMV. These inaccuracies led some county election officials to question the purge effort and, facing legal action, the Florida Division of Elections ordered SOEs to halt the purge effort (Brown 2012). By this time, roughly 45 per cent

[10]The FDOS claimed at the time to the Department of Homeland Security, the media and one of the authors (via Twitter) that it sampled challenge list names from the suspect list. This is clearly not the case, and the state must have used a separate list of unknown origin to identify those registrants not on the suspect list. Names taken from the suspect list were described by the FDOS as 'more random' and 'a mix of random samples', despite the covariate imbalances discussed below between those who do and do not appear on the challenge list (suggesting non-random selection). At no time during the litigation, including during the discovery process in federal court, did the FDOS provide an explanation for these discrepancies (see page 14 of the Plaintiffs-Appellants' Initial Brief: http://moritzlaw.osu.edu/electionlaw/litigation/documents/Appellant-Brief_000.pdf).

[11]For example, within 30 days of receiving the initial notification, over 35 per cent (588) of the 1,600 Miami-Dade County challenged registrants presented evidence of citizenship or were otherwise determined by the SOE to be citizens, while only 16 admitted ineligibility or requested removal from the voter rolls.

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of those in our analyses (below) who were challenged had already paid the additional voting cost of proving their citizenship. SOEs were then required to mail a second letter to those registered voters on the challenge list that informed them that their voting rights had been restored, even if they did not respond to the initial challenge.[12]

Data and Methods

To estimate the effect of assignment to the challenge list on one's propensity to vote, we constructed a dataset from multiple Florida voter files and administrative records. We first obtained a 2 April 2012 voter file from the FDOS that contained all registered voters at that date and corresponded to when the FDOS generated the challenge list and distributed it to county SOEs. We then joined this file with the suspect and challenge lists using the unique voter identification number assigned to each record and present in each file to identify these registrants in the voter file. Through the same process, we appended information about participation in the November presidential election from a March 2013 voter file. Individuals recorded as having voted are assigned a value of 1. Those who did not vote receive a value of 0, as do those not in the post-election file (as the failure to vote could have affected their propensity to be removed from the rolls).[13]

From these data we selected treated (challenged) and control (unchallenged) registrants for analysis, a task complicated by the nature of treatment assignment. For example, one could compare all challenged registrants to all unchallenged registrants. Considering the former as a single entity, however, ignores the fact that this group consists of two samples: those drawn from the suspect list, who were identified by merging their registration record to DAVID records, and those not from the suspect list, identified through an unknown process. In addition, those challenged are not a random sample of Florida registrants, but instead exhibit some set of known and unknown characteristics correlated with being a suspected non-citizen.

Covariate information from the Florida statewide voter file confirms that these groups possess distinct (observable) demographic, political and participatory profiles. As we report in the Appendix, since 2008, those challenged but not on the suspect list have voted at substantially lower rates than those challenged from the suspect list. In turn, the latter voted at a lower rate than unchallenged registrants in every statewide election since 2004. According to the voter file information, they are also (compared to the average unchallenged registrant) less likely to be white or black, more likely to be Hispanic, younger and more recent registrants, and less likely to be affiliated with a major or minor party. Finally, those challenged from the suspect list are geographically clustered: 87.3 per cent come from Miami-Dade County, while only 11.1 per cent of all Florida registrants reside there. These significant observable differences, and the unobservable differences correlated with suspicion of being a non-citizen, signal that we cannot treat the challenge list as a homogenous group or the average registrant as a proper counterfactual for that subset of individuals.

Instead, we construct more comparable treatment and control groups using two sample restrictions (a diagram outlining sample construction appears in the Appendix). First, we leverage the multi-stage nature of the state's challenge process to examine turnout within the

[12]This context is somewhat unique, in that while the challenge imposes a clear voting cost (the registrant cannot vote without providing proof of citizenship) that cost is ultimately lifted. However, when their citizenship was initially challenged, those targeted had no way to know that the purge would ultimately be aborted. As such, for at least this subset of registrants the challenge did increase the cost of voting, and many of these individuals did bear that cost.

[13]To ensure that no November voters were removed from the rolls before March, we also appended a January 2013 voter file (which provided no additional turnout history). The resulting dataset contains 96.1 per cent and 98.3 per cent of the suspect and challenge list observations, respectively. Missing observations are due to normal voter file upkeep (as the suspect list was generated roughly a year before the statewide voter file) and, in the case of the challenge list, the state's non-citizen purge. Crucially, the subset of data we analyze has no missing treatment cases.

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suspect list. That is, we compare those challenged on the suspect list (the treatment group) to those not challenged from the suspect list (the control group). Although the former are not a random sample of the latter, both groups share similar covariate profiles and the same set of observable and unobservable characteristics that led to their initial identification as non-citizens, making the selected control group a better counterfactual than the average registrant. We thus exclude from analysis the 1,206 challenged registrants who were not on the initial suspect list.

Secondly, we restrict our attention to Miami-Dade County. As the vast majority of treated units are from this county, we gain little by expanding the analysis to the entire state. This restriction also permits the inclusion of more precise indicators to address unobservable geographic-specific effects (namely, precinct fixed effects).[14] The resulting dataset contains 66,676 registrants from the suspect list, including all 1,239 registrants from this list challenged by the state.[15] In our sample, 68.9 per cent and 69.2 per cent of the suspect list that was and was not challenged voted in the 2012 general election, respectively.

Table 1 reports the covariate distributions for this final sample, which suggest that challenge assignment was non-random even within the suspect list. Those challenged were less likely to vote in the 2008 and 2010 general elections (both $p = 0.06$), as well as the 2011 county mayor recall election, 2004 and 2010 primaries, and 2004 general contest (all $p < 0.05$). These registrants voted at a higher rate in the 2004 presidential primary ($p = 0.08$), though participation rates for the two groups were essentially zero (0.2 per cent and 0.1 per cent, respectively). The treated group also contains a smaller proportion of black registered voters ($p < 0.01$), who participated at a higher rate in Miami-Dade in 2012 than other registrants; a greater percentage of Hispanics ($p < 0.01$); and a lower percentage of Republicans and minor party identifiers ($p < 0.05$).

In an effort to avoid the selection concerns raised by these imbalances, we exploit the data's panel nature to generate a within-registrant difference-in-difference estimate of the treatment effect. In doing so, we compare the difference in participation rates between the treatment and control groups across the 2008 and 2012 presidential elections under the assumption that, absent treatment, the difference in 2012 would be the same as in 2008. This strategy eliminates concerns about (time-invariant) unobserved confounders and the possibility that one group's baseline propensity to vote may be higher.[16]

Our unit of analysis is the registrant-year, meaning that we observe each individual twice: once for 2008 and once for 2012. The dependent variable is whether the registrant voted (1 = yes, 0 = no), which corresponds to the 2008 race for the first observation and the 2012 election for the second observation. As independent variables, we include indicators for assignment to the challenge list and for the 2012 election observations (both 1 = yes, 0 = no), as well as their interaction. In doing so, we follow the standard approach for the difference-in-difference estimation.[17] The coefficient on the interaction term represents the change in the voting rates for the two groups between the two elections, which we attribute to treatment assignment. We also include in the model the available voter file demographic, political and vote history covariates.

Our assumption that the voting gap across elections would not vary absent treatment requires that both conditions equally experience any other factors that influence the propensity to vote.

[14]In addition, the treatment across counties differs slightly, as each SOE was given some latitude in how to word the initial letter to those whose citizenship was being challenged.

[15]The 1,239 treated subjects include two who admitted ineligibility and one who requested removal from the rolls in response to the citizenship challenge, the only challenged individuals purged in Miami-Dade County because of this process (Sherman 2012). As we cannot remove similar registrants from the control group (i.e., those who would admit ineligibility if challenged), these observations remain in the treatment group.

[16]For example, if a 3-percentage-point higher turnout rate in 2008 for control units would remain constant in 2012 absent treatment, then a cross-sectional analysis would not identify a treatment effect that completely erased that gap (as we would observe only equal turnout for the groups).

[17]See, e.g., Card and Krueger (1994); Gruber (1994).

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Table 1. Sample characteristics based on covariates in Florida voter file

	(1)	(2)	(3)
	Suspect List, Not Challenged	Suspect List, Challenged	Difference (Not Challenged - Challenged)
Voted 2012 Pres Primary (1 = yes, 0 = no)	0.107	0.100	0.007
	[0.3095]	[0.3002]	[0.0086]
Voted 2011 County Mayor Run-Off (1 = yes, 0 = no)	0.181	0.180	0.001
	[0.3847]	[0.3843]	[0.0110]
Voted 2011 County Mayor Special (1 = yes, 0 = no)	0.150	0.143	0.007
	[0.3574]	[0.3501]	[0.0100]
Voted 2011 County Mayor Recall (1 = yes, 0 = no)	0.175	0.153	0.022**
	[0.3796]	[0.3597]	[0.0103]
Voted 2010 General (1 = yes, 0 = no)	0.352	0.326	0.026*
	[0.4776]	[0.4690]	[0.0134]
Voted 2010 Primary (1 = yes, 0 = no)	0.110	0.091	0.019**
	[0.3128]	[0.2880]	[0.0082]
Voted 2008 Special (1 = yes, 0 = no)	0.044	0.041	0.003
	[0.2053]	[0.1987]	[0.0057]
Voted 2008 General (1 = yes, 0 = no)	0.568	0.542	0.026*
	[0.4954]	[0.4985]	[0.0143]
Voted 2008 Pres Primary (1 = yes, 0 = no)	0.107	0.096	0.011
	[0.3095]	[0.2948]	[0.0085]
Voted 2008 Primary (1 = yes, 0 = no)	0.065	0.066	− 0.002
	[0.2459]	[0.2487]	[0.0071]
Voted 2007 Special (1 = yes, 0 = no)	0.019	0.016	0.003
	[0.1377]	[0.1261]	[0.0036]
Voted 2006 General (1 = yes, 0 = no)	0.067	0.058	0.009
	[0.2496]	[0.2340]	[0.0067]
Voted 2006 Primary (1 = yes, 0 = no)	0.020	0.017	0.003
	[0.1413]	[0.1291]	[0.0037]
Voted 2005 Special (1 = yes, 0 = no)	0.005	0.006	0.000
	[0.0734]	[0.0750]	[0.0021]
Voted 2004 General (1 = yes, 0 = no)	0.042	0.028	0.013**
	[0.1999]	[0.1657]	[0.0048]
Voted 2004 Primary (1 = yes, 0 = no)	0.008	0.002	0.006**
	[0.0899]	[0.0492]	[0.0014]
Voted 2004 Pres Primary (1 = yes, 0 = no)	0.001	0.002	− 0.002*
	[0.0298]	[0.0492]	[0.0014]
Female (1 = yes, 0 = no)	0.522	0.532	− 0.010
	[0.4995]	[0.4992]	[0.0143]
Gender Unknown (1 = yes, 0 = no)	0.033	0.028	0.005
	[0.1797]	[0.1657]	[0.0048]
Black (1 = yes, 0 = no)	0.103	0.061	0.041***
	[0.3036]	[0.2400]	[0.0069]
Hispanic (1 = yes, 0 = no)	0.774	0.810	− 0.035***
	[0.4180]	[0.3928]	[0.0113]
White (1 = yes, 0 = no)	0.048	0.045	0.002
	[0.2130]	[0.2078]	[0.0060]
Asian (1 = yes, 0 = no)	0.016	0.020	− 0.004
	[0.1258]	[0.1407]	[0.0040]
Other Race (1 = yes, 0 = no)	0.059	0.064	− 0.005
	[0.2359]	[0.2444]	[0.0070]
Age (in years)	49.219	48.745	0.474
	[15.9131]	[15.8384]	[0.4563]
Year of Registration	2007.744	2007.842	− 0.098
	[2.289]	[2.1452]	[0.0656]
Democrat (1 = yes, 0 = no)	0.372	0.358	0.015
	[0.4834]	[0.4795]	[0.0137]
Republican (1 = yes, 0 = no)	0.245	0.215	0.031**
	[0.4303]	[0.4108]	[0.0118]
Other Party (1 = yes, 0 = no)	0.015	0.007	0.008**
	[0.1213]	[0.0801]	[0.0023]

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Table 1. *Continued*

	(1)	(2)	(3)
	Suspect List, Not Challenged	Suspect List, Challenged	Difference (Not Challenged - Challenged)
Active Voter (1 = yes, 0 = no)	0.967	0.968	− 0.001
	[0.1791]	[0.1768]	[0.0051]
Observations	65,437	1,239	

Note: Columns 1 and 2 report means with standard deviations in brackets. Column 3 reports difference in means test for age and registration year, and difference in proportions test for all other variables (standard errors in brackets). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The differences reported in Table 1, however, could signal a violation of this assumption. For example, the control group's higher turnout in the previous two general elections might make them likelier targets for mobilization, which could expand the gap compared to 2008 and reduce the likelihood of identifying a treatment effect. As an additional test, we adjust the treatment and control group covariate distributions through matching.[18] We first exactly match treated individuals to control group registrants on all voter file covariates (vote history, demographics, registration year, registration (or not) with a major or minor party, and voter status). These matched cases are essentially the same assuming equivalency on unobservables. To maximize the number of treated units matched (given the difficulty of identifying individuals with the same covariate profile), we also use coarsened exact matching (CEM) (Iacus, King and Porro 2012) to 'coarsen' registrant age into three- and five-year bins (the latter based on the default binning algorithm) and then match based on whether two subjects' ages are in the same bin (that is, within three (five) years of each other).[19] This approach yields more matches with a potential tradeoff in their quality, as some are not exactly equivalent on observables. With both exact matching and CEM, we match with replacement and permit more than one-to-one matching to increase the number of matches and the balance.

Description of treatment

Before proceeding to our results, we briefly describe the treatment, which consists of two letters from the county SOE. The first letter, sent in April or May 2012, challenged the recipient's eligibility to register based on their citizenship status. The opening paragraph read:

> The Miami-Dade County Elections Department has received information from the Florida Division of Elections that calls into question your eligibility to be registered to vote. Based on information available from the Florida Department of Highway Safety and Motor Vehicles (DHSMV), you are not a US citizen.

The letter noted that only US citizens can register to vote, and that a non-citizen doing so constitutes a felony. It then detailed the steps necessary to prove one's citizenship and stated the consequence of ignoring the notice: 'If you fail to respond within thirty (30) days, we may determine that you are ineligible and remove your name from the voter registration rolls. You will then no longer be eligible to vote.'

[18]See, e.g., Ho et al. (2007).

[19]All other covariates except year of registration are dichotomous and matched exactly. We match exactly on registration year to best exploit the limited voter history available for our predominately recent registrant sample (less than 3 per cent and 30 per cent in both groups registered before 2004 and 2008, respectively) and to (largely) ensure that matched individuals had the same opportunity to participate in each contest (i.e., for matched registrants, failure to vote in an election derives from both being ineligible (i.e., unregistered) or both choosing to abstain).

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The second letter, sent June 14, contained the following text:

> The Miami-Dade County Elections Department recently notified you of your ineligibility as a registered voter.
> After further review of your voter registration record and pending additional information from the State of Florida, a determination has been made to restore your name in the statewide voter registration system.
> Information regarding your citizenship status may be provided by contacting the Registration Section at […]

Note that this letter informs the recipient of the restoration of her or his voting right but makes no mention of the upcoming August primary election or the November presidential contest (that is, it does not also serve as an election reminder). We stress that the estimated treatment effect pertains to both letters (we cannot isolate their individual impact as all challenged registrants are sent both letters) and, as is the case with any study involving mail outreach (randomly assigned or not), is for assignment to treatment (we cannot guarantee that those challenged received and/or read the letters).[20]

Results

Table 2 presents ordinary least squares regression results.[21] We report only those coefficients necessary to interpret the impact of assignment to purge on the propensity to vote, along with one-sided p-values for that treatment effect given our directional hypothesis (see the Appendix for fully specified models). Column 1 estimates the within-registrant difference in turnout between those challenged and not challenged across the 2008 and 2012 presidential elections (standard errors clustered by registrant). The interaction between the indicators for treatment assignment and post-treatment observation corresponds to the treatment effect. Turnout was 2.6 percentage points lower for the treatment group in 2008 (prior to assignment, which matches the difference reported in Table 1), but this gap shrinks to only 0.4 points in 2012, a substantial treatment effect of 2.2 points ($p = 0.10$). This finding is consistent with our expectation that being targeted for purge from the voter rolls causes (through reactance) some individuals to vote. The results are unchanged when we control for prior participation in all countywide and statewide elections since 2004 and for individual-level factors, as well as include precinct fixed effects (Column 2).

This finding persists when we examine differences in 2010 and 2012 general election participation rates. Although comparing two distinct election types (midterm and presidential) might violate the assumption that the voting gap between the treatment and control groups would remain constant absent treatment, we do so to ensure that the observed treatment effect is not driven by differences in vote propensity among the 19 per cent of the sample in both groups that registered after 2008 but before the 2010 contest. Column 3 reveals that turnout in 2010 was 2.6 points higher for those not challenged (as it was in 2008), but assignment to treatment again reduced this gap in 2012 by an identical 2.2 points ($p = 0.08$). This effect holds when we add individual-level covariates and precinct fixed effects to the model (see Column 4).

Alternatively, accounting for covariate imbalances by pre-processing the data similarly yields sizable treatment effects on turnout. Column 5, where we first exactly match 728 of the 1,239 treated units (58.8 per cent) on all the voter file covariates (so that both groups share identical,

[20]Mailing dates were confirmed by the Miami-Dade County Chief Deputy SOE via email. Both letters were sent in English, Spanish and Creole (copies appear in the Appendix).

[21]Estimating all model specifications in this section using logistic regression yields similar results (see the Appendix). Bivariate cross-sectional analysis (in the Appendix) estimates the effect of treatment assignment on 2012 turnout as −0.4 percentage points, while including covariates increases the estimate to 0.8 ($p = 0.24$) to 1.1 ($p = 0.18$) percentage points (with and without precinct fixed effects, respectively).

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Table 2. Effect of treatment assignment on 2012 presidential election participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Within-Registrant Difference-in-Difference				Data Pre-Processed					
	2012–2008		2012–2010		Exact Matching		Exact Matching, Age Coarsened to 3 Year Bins		Exact Matching, Age Coarsened to 5 Year Bins	
Treatment Assignment (1 = yes, 0 = no)	− 0.026*	− 0.018	− 0.026*	− 0.008	0.028*	0.030*	0.026*	0.030**	0.023*	0.022*
	[0.014]	[0.013]	[0.013]	[0.012]	[0.018]	[0.019]	[0.016]	[0.017]	[0.015]	[0.016]
2012 Observation (1 = yes, 0 = no)	0.125***	0.125***	0.340***	0.340***						
	[0.002]	[0.002]	[0.002]	[0.002]						
Treatment*2012 Observation	0.022*	0.022*	0.022*	0.022*						
	[0.017]	[0.017]	[0.016]	[0.016]						
Constant	0.568***	23.928***	0.352***	− 58.392***	− 162.695***	− 166.145***	− 131.542***	− 127.476***	− 131.073***	− 134.297***
	[0.002]	[2.004]	[0.002]	[1.388]	[15.632]	[12.931]	[11.243]	[8.944]	[8.873]	[7.223]
Observations	133,352	133,352	133,352	133,352	7,398	7,398	14,771	14,771	18,959	18,959
R-squared	0.017	0.131	0.116	0.312	0.177	0.335	0.190	0.291	0.205	0.285
Demographic, Political, and Vote History Covariates?	N	Y	N	Y	Y	Y	Y	Y	Y	Y
Precinct Fixed Effects?	N	Y	N	Y	N	Y	N	Y	N	Y
Number of Treated Individuals	1,239	1,239	1,239	1,239						
Number of Matched Treated Individuals					728	728	869	869	930	930
Number of Unmatched Treated Individuals					511	511	370	370	309	309

Note: dependent variable is whether the registrant voted (1 = yes, 0 = no) in the specified general election (2008 or 2012 in Columns 1 and 2, 2010 or 2012 in Columns 3 and 4, and 2012 in Columns 5–10). Errors clustered by registrant in brackets in Columns 1–4. Robust standard errors in brackets in Columns 5–10. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, one-sided for treatment effect observable demographic, political and participatory characteristics), reports the impact of the challenge on turnout as 2.8 points ($p = 0.07$).[22] This increases slightly to 3.0 points when we control for precinct fixed effects ($p = 0.06$; see Column 6).[23]

Results are similar if we coarsen age into three- or five-year bins to increase the number of matched treated units. The use of three-year bins raises the percentage of challenged registrants matched to 70.1 per cent, but the estimated treatment effect (reported in Columns 7 and 8) closely mirrors that generated through exact matching (2.6 points ($p = 0.06$) without and 3.0 points ($p = 0.04$) with precinct fixed effects). Expanding the bin size to five years (the size selected by the default CEM algorithm) in Columns 9 and 10 matches 75.1 per cent of treated cases and reduces somewhat the treatment coefficient estimates to 2.3 and 2.2 points, respectively ($p = 0.06$ and $p = 0.08$).

In light of the discussion above that specific racial and ethnic minority groups are disproportionately targeted in registrant purges and generally face greater voting costs under election administration rules often perceived to threaten the right to vote, we additionally test whether an individual's race/ethnicity conditions the challenge's participatory effect. We focus on the impact for Hispanics, who constitute the vast majority of our sample (81.0 per cent and 77.4 per cent of those challenged and not challenged, respectively). As with the entire sample, treated and untreated Hispanic subjects differ significantly on some factors (for example, those in the control may have a slightly higher propensity to vote). That said, the distributions of available covariates are more balanced for this subset of the data than for the entire sample (see the Appendix).[24] Although we do not suspect this effect is necessarily limited to Hispanics, given our sample size they likely represent the only group for which we can generate precise estimates.

Table 3 reveals that the relationship is even stronger, and consistently statistically significant, when we focus on the participatory consequences of the citizenship challenge for this population subgroup. Columns 1–4 replicate the corresponding analysis in Table 2 interacting the three reported variables with each race indicator (black, white, Asian and other race). Because Hispanics serve as the baseline group, the key term remains the interaction of treatment assignment and the 2012 election observation indicator (full model details in the Appendix). Column 1 reports that treated Hispanics were 2.4 points less likely to vote than untreated Hispanics in 2008, but assignment to purge increased their participation rates in 2012 by a statistically significant 3.4 points relative to unchallenged co-ethnics ($p = 0.03$).[25] This effect is roughly 55 per cent larger than that estimated for the entire sample. Results are the same when we add covariates and precinct fixed effects (see Column 2). In Columns 3 and 4, we examine the change in voting rates in 2012 to the rates in 2010, when the gap between those treated and untreated (1.5 points higher for the control) was 0.9 points smaller than the gap in 2008. We thus estimate a slightly smaller, but still substantively meaningful, 2.5-point treatment effect without ($p = 0.07$) and with covariates ($p = 0.08$).

We similarly identify positive, substantively large and statistically significant treatment effects for Hispanics when we interact the treatment variable with the race/ethnicity indicators after preprocessing the data. Using exact matching, we successfully match 632 of 1,003 treated Hispanics (63.0 per cent). Among these matches, Column 5 in Table 3 estimates that Hispanics whose

[22]Matching exactly achieves balance on all covariates, while coarsening age into 3- or 5-year bins (as we do in Columns 7 –10) improves the existing balance on this covariate.

[23]Exactly matching on precinct yields too few matches to draw meaningful conclusions, and precinct location cannot be coarsened before matching.

[24]Unchallenged Hispanics voted at higher levels in the 2011 county mayor recall ($p < 0.05$), 2004 primary ($p = 0.06$) and 2004 general election ($p < 0.05$). (Treated Hispanics were more likely to vote in the 2004 presidential primary ($p < 0.01$), though participation rates were essentially zero (0.3 per cent and 0.1 per cent)). Control group subjects also identify with the Republican Party at a higher rate ($p < 0.01$).

[25]Cross-sectional analyses in the Appendix estimate Hispanic treatment effects between 1.4 ($p = 0.13$) and 1.8 ($p = 0.09$) points (with and without precinct fixed effects).

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Table 3. Effect of treatment assignment on 2012 presidential election participation for Hispanics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Within-Registrant Difference-in-Difference				Data Pre-Processed					
	2012–2008		2012–2010		Exact matching		Exact matching, age coarsened to 3 year bins		Exact matching, age coarsened to 5 year bins	
Treatment Assignment (1 = yes, 0 = no)	−0.024	−0.022	−0.015	−0.005	0.038**	0.048***	0.035**	0.040**	0.033**	0.031**
	[0.016]	[0.015]	[0.015]	[0.013]	[0.020]	[0.021]	[0.017]	[0.018]	[0.016]	[0.017]
2012 Observation (1 = yes, 0 = no)	0.116***	0.116***	0.341***	0.341***						
	[0.003]	[0.003]	[0.002]	[0.002]						
Treatment*2012 Observation	0.034**	0.034**	0.025*	0.025*						
	[0.019]	[0.019]	[0.018]	[0.018]						
Constant	0.575***	23.906***	0.350***	−58.399***	−162.696***	−166.572***	−131.543***	−127.575***	−131.074***	−134.407***
	[0.002]	[2.003]	[0.002]	[1.389]	[15.559]	[12.911]	[11.212]	[8.932]	[8.862]	[7.204]
Observations	133,352	133,352	133,352	133,352	7,398	7,398	14,771	14,771	18,959	18,959
R-squared	0.019	0.132	0.119	0.313	0.177	0.335	0.190	0.291	0.205	0.286
Demographic, Political, and Vote History Covariates?	N	Y	N	Y	Y	Y	Y	Y	Y	Y
Precinct Fixed Effects?	N	Y	N	Y	N	Y	N	Y	N	Y
Number of Treated Individuals	1,239	1,239	1,239	1,239						
Number of Matched Treated Individuals					728	728	869	869	930	930
Number of Unmatched Treated Individuals					511	511	370	370	309	309
Number of Treated Hispanics	1,003	1,003	1,003	1,003						
Number of Matched Treated Hispanics					632	632	736	736	782	782
Number of Unmatched Treated Hispanics					371	371	267	267	221	221

Note: dependent variable is whether the registrant voted (1 = yes, 0 = no) in the specified general election (2008 or 2012 in Columns 1 and 2, 2010 or 2012 in Columns 3 and 4, and 2012 in Columns 5–10). Errors clustered by registrant in brackets in Columns 1–4. Robust standard errors in brackets in Columns 5–10. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, one-sided for treatment effect citizenship was challenged were a significant 3.8 percentage points more likely to vote than untargeted members of their community ($p = 0.03$). This effect increases 1 point (to 4.8 points; $p < 0.01$) when we include precinct fixed effects (see Column 6).

Coarsening age into three-year bins matches 73.4 per cent of treated Hispanics and yields a slightly reduced effect that is a still significant and impressive 3.5 ($p = 0.02$) to 4.0 ($p = 0.01$) points (Columns 7 and 8). With five-year bins, we identify matches for 78.0 per cent of Hispanics, and while the treatment effect sizes are again somewhat smaller (see Columns 9 and 10), they remain sizable and statistically significant (3.3 ($p = 0.02$) and 3.1 ($p = 0.03$) points).[26] Thus through two distinct strategies that address the non-random assignment to purge, we consistently find that those whose citizenship status (and thus eligibility to vote) was challenged by the FDOS were more likely to vote than their unchallenged counterparts by a significant 2.2 to 3.0 points, and that for challenged Hispanics (the vast majority of the sample) the impact was an even larger (by 14–60 per cent) and significant 2.5 to 4.8 points.[27]

Placebo tests

The findings in Tables 2 and 3 suggest that those accused of being non-citizens, especially Hispanics, were more likely to vote than their unchallenged counterparts. As the challenges were not random, however, one might still suspect that some *unobservable, time-variant* difference between the two groups explains the higher propensity to vote in the 2012 general election. To assuage this concern, we conduct several placebo tests that use treatment assignment to explain changes in the voting gap between previous elections, as well as participation in those elections. As assignment could not have influenced the decision to vote in those elections (since it occurred years later), evidence that it did would raise concerns about the causal link between treatment and participation (and possibly signal some greater participatory inclination among those targeted). In contrast, if assignment lacks explanatory power, then we can be more confident in attributing the effects estimated in Tables 2 and 3 to the two letters sent by the SOE.

The first three columns of Table 4A report treatment effect coefficients corresponding to the difference in within-registrant turnout across the treatment and control groups between the 2004 and 2008 presidential elections, 2006 and 2010 midterm elections, and 2008 and 2010 general elections, respectively (full models in the Appendix). In Columns 4–9 we pre-process the data using either exact matching or CEM to estimate the treatment effect on turnout in the two previous general elections (2008 and 2010).[28] Consistent with the expectation that assignment should not affect participation in these contests, all estimates are either zero or negative. Table 4B replicates Table 4A conditioning the challenge effect by race/ethnicity. All estimates are negative for Hispanics with the exception of the change in the voting rate gap between 2008 and 2010 (0.9 points; $p = 0.32$, one-tailed).[29]

A potential concern with the analyses in Tables 4A and 4B, however, is that the samples include individuals who could not vote in those contests because they were unregistered. Conceivably, there could be some difference between the treatment and control groups with regard to those who could not vote (perhaps because they were non-citizens at the time or too young

[26]With few exceptions, across all columns the treatment effect for Hispanics is statistically indistinguishable from that for the other racial groups, though linear combination of coefficients tests reveal that the effect is not statistically significant for any other racial group (possibly due to imprecisely estimated effects from their small sample sizes or covariate imbalances between treatment and control groups).

[27]Additional tests (not reported) replicating the Table 2 and 3 analyses identified no evidence that the treatment effect is conditioned by the challenged registrant's age or prior vote history (participation in the two prior general elections or the number of elections voted in since 2008).

[28]We match on the same set of covariates used in Tables 2 and 3 except turnout in contests after the dependent variable election.

[29]Cross-sectional estimates of the treatment effect for the entire sample and for Hispanics for 2008 and 2010 are all negative (see the Appendix).

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Table 4A. Placebo tests: effect of treatment assignment on prior election participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Within-registrant difference-in-difference			Exact matching		Exact matching, age coarsened to 3 year bins		Exact matching, age coarsened to 5 year bins	
	2008 − 2004	2010 − 2006	2010 − 2008	2008	2010	2008	2010	2008	2010
Treatment Assignment (1 = yes, 0 = no)	− 0.007	0.006	− 0.013	− 0.027*	− 0.018	− 0.023	− 0.006	− 0.028*	− 0.002
	[0.005]	[0.006]	[0.013]	[0.015]	[0.015]	[0.015]	[0.014]	[0.015]	[0.014]
Later Election Observation (1 = yes, 0 = no)	0.526***	0.285***	− 0.216***						
	[0.002]	[0.002]	[0.002]						
Treatment*Later Election Observation	− 0.013	− 0.017	0.000						
	[0.015]	[0.014]	[0.017]						
Constant	81.910***	− 6.165***	44.824***	299.446***	− 48.426***	227.205***	− 42.840***	214.969***	− 46.946***
	[2.326]	[0.777]	[2.158]	[27.518]	[7.162]	[25.089]	[4.807]	[17.208]	[4.474]
Observations	133,352	133,352	133,352	19,985	14,072	33,417	25,950	38,353	31,610
R-squared	0.419	0.293	0.190	0.331	0.280	0.270	0.271	0.252	0.262
Demographic, Political, and Vote History Covariates?	Y	Y	Y	Y	Y	Y	Y	Y	Y
Precinct Fixed Effects?	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Treated Individuals	1,239	1,239	1,239						
Number of Matched Treated Individuals				1,060	970	1,134	1,077	1,161	1,111
Number of Unmatched Treated Individuals				179	269	105	162	78	128

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Table 4B. Placebo tests: effect of treatment assignment on prior election participation for Hispanics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Within-registrant difference-in-difference			Exact matching		Exact matching, age coarsened to 3 year bins		Exact matching, age coarsened to 5 year bins	
	2008–2004	2010–2006	2010–2008	2008	2010	2008	2010	2008	2010
Treatment Assignment (1 = yes, 0 = no)	−0.008	0.005	−0.017	−0.027*	−0.019	−0.022	−0.008	−0.027*	−0.006
	[0.005]	[0.007]	[0.015]	[0.016]	[0.017]	[0.016]	[0.015]	[0.015]	[0.015]
Later Election Observation (1 = yes, 0 = no)	0.536***	0.283***	−0.225***						
	[0.002]	[0.002]	[0.003]						
Treatment*Later Election Observation	−0.012	−0.009	0.009						
	[0.016]	[0.016]	[0.020]						
Constant	81.900***	−6.166***	44.813***	299.484***	−48.434***	227.219***	−42.822***	214.979***	−46.955***
	[2.326]	[0.777]	[2.157]	[27.518]	[7.166]	[25.098]	[4.811]	[17.215]	[4.471]
Observations	133,352	133,352	133,352	19,985	14,072	33,417	25,950	38,353	31,610
R-squared	0.420	0.294	0.191	0.331	0.280	0.270	0.271	0.252	0.262
Demographic, Political, and Vote History Covariates?	Y	Y	Y	Y	Y	Y	Y	Y	Y
Precinct Fixed Effects?	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of Treated Individuals	1,239	1,239	1,239						
Number of Matched Treated Individuals				1,060	970	1,134	1,077	1,161	1,111
Number of Unmatched Treated Individuals				179	269	105	162	78	128
Number of Treated Hispanics	1,003	1,003	1,003						
Number of Matched Treated Hispanics				903	836	946	904	961	929
Number of Unmatched Treated Hispanics				100	167	57	99	42	74

Note: dependent variable is whether the registrant voted (1 = yes, 0 = no) in the specified general election (2004 or 2008 in Column 1; 2006 or 2010 in Column 2; 2008 or 2010 in Column 3; 2008 in Columns 4, 6 and 8; and 2010 in Columns 5, 7, and 9). Errors clustered by registrant in brackets in Columns 1–3. Robust standard errors in brackets in Columns 4–9. See Appendix for full model results. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

to do so) but who would have if they had been eligible. In the Appendix we replicate Columns 4–9 from both tables after excluding those who did not meet the registration requirement of 29 days before the election.[30] For both the entire sample and Hispanics, all but the Column 6 replication (0.2 points, $p = 0.45$, one-tailed) estimate negative, sometimes significant, treatment effects. As such, those challenged were no more likely, and possibly less likely, to vote in prior elections than those not challenged, signaling that the positive effect of treatment assignment does not correspond to a greater general propensity to participate.

Is reactance the mechanism?

Although treatment assignment appears to have caused higher turnout, one might question whether the mechanism is, as we argue, reactance. As our estimated treatment effect is the combined effect of being sent the two letters from the SOE and their actual content,[31] one possible alternative is that the contact itself from the SOE increased voting rates. We do not find this potential explanation plausible. The effects for both the average challenged registrant (2.2 to 3.0 points) and Hispanics (2.5 to 4.8 points) are comparable to that of the average social pressure mailing, frequently the most successful type of mobilization message (2.3 points estimated across all types of contests) (Green and Gerber 2015). In contrast, the average effect of a non-partisan piece of mobilization mail in a presidential contest, based on a meta-analysis of studies reported in Green and Gerber (2015) that we describe in the Appendix, is −0.03 points (95 per cent confidence interval [−0.28, 0.22]). As such, our best estimate of the effect of direct mail contact during a presidential race is 0, and any impact greater than 0.22 points falls outside the 95 per cent confidence interval.

Furthermore, the 0.22 point estimate almost certainly overestimates the upper bound of the impact of mere contact in this instance. Despite referencing one's right to vote, neither letter mentions the November (or any other) election. In addition, both letters were sent five to seven months before the November contest. While (to our knowledge) there is no empirical evidence about how the timing of an outreach effort conditions its effectiveness, we would not expect such contact months before the election that does not reference that election to be as successful as a simple election reminder sent within a week of the contest (as they were in those studies used for the meta-analysis). As such, none of the estimated effect likely derives from contact alone or the provision of election information.

A second possibility is that restoring registrants' names to the voter rolls mobilized eligible and/or ineligible registrants. Theoretically, confirming eligible registrants' voting rights could have caused those who were unsure or unaware of this right to turn out at higher rates. We find this explanation unconvincing, however, as one can plausibly expect the vast majority of those treated who voted in 2012 to be aware of this right (53 per cent – and 55 per cent of Hispanics – provided proof of citizenship in response to the challenge, while an additional 36 per cent and 35 per cent had voted in the past four years, respectively). We also find it unlikely that ineligible individuals became more inclined to vote, given that non-citizen voting is extremely rare in general (Minnite 2010), as well as in our data (for example, only four of the sixteen registrants from the entire challenge list who admitted ineligibility or requested removal from the rolls in Miami-Dade County previously voted – and it may have been that they were indeed eligible to vote at that time). Furthermore, it is highly unlikely that ineligible voters who were contacted would become more inclined to vote, given the knowledge of the heightened scrutiny applied to their registration (the restoration is only made 'pending additional information') and the steep penalty for and felony nature of such an action (as explicitly stated in the first letter). As such, we

[30]Similar restrictions for the within-registrant difference-in-difference analyses are largely uninformative given the small number of subjects registered before the earliest elections used in the analyses.

[31]It is possible that the second letter (as a reminder of the challenge) contributes to the reactance aroused by the first letter, though any such effect is consistent with our proposed mechanism.

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are confident that the citizenship challenge, and reminder of that challenge, was the sole motivation for subsequent electoral participation.

Discussion and Conclusion

Although electoral administration rules that increase voting costs are generally predicted to reduce participation, this expectation fails to account for any potential backlash against the threat (real or perceived) that those policies pose to the ballot access of certain individuals. Drawing on psychological reactance theory, we anticipated that, for these targeted individuals, attacking their voting right would raise the value attached to that freedom and heighten the interest in engaging in it, ultimately increasing their propensity to turn out. Leveraging the multi-stage process of a non-citizen registrant purge by the State of Florida in 2012, we found that those challenged voted at a substantially higher rate than those not challenged, a conclusion robust to a variety of modeling strategies. Furthermore, the effect was consistently statistically significant (and noticeably larger) for Hispanics, the overwhelming majority of both the treatment and control groups. Additional tests showed that treatment assignment did not predict turnout in previous general elections, suggesting that the higher 2012 turnout derived from the citizenship challenge, not a general higher inclination to vote.

We must acknowledge, however, an important limitation of our study: the necessary reliance on observational data to determine the effect of challenging one's citizenship on participation. Of course, our reliance on observational data is unavoidable, as it is not feasible (or ethical) to test this relationship through a field experiment. We attempted to estimate an unbiased treatment effect by comparing within the suspect list, limiting the analysis to one county, and employing multiple designs, including placebo tests, to address (potential) imbalances on observable and unobservable covariates. That said, we cannot rule out the possibility that some unobservable factor correlated with both selection for challenge from the suspect list and the propensity to vote instead explains the higher turnout. If future state-sanctioned attempts to purge voters prior to an election were to occur in Florida or another state, scholars might try to test experimentally whether (and for whom) citizenship challenges elicit reactance, and whether that response is correlated with intention to vote.

That concern aside, we warn against the potentially hazardous interpretation of our results that asking registrants to prove their citizenship, or imposing some other reform that threatens the right to vote, increases turnout. Our theory does not predict that all individuals affected by increased costs will respond in this manner, but rather some proportion of those individuals. Our sample is not representative of all registrants, but is instead dominated by Hispanics and (presumably) many recently naturalized citizens. It is plausible that this latter group possesses greater motivation to reclaim their (new) franchise than the average citizen. That is, the positive effect on turnout could be a function of who was targeted, not some inherent feature of demonstrating one's eligibility. Our results do not, and cannot, speak to how the average (potential) registrant would respond to this requirement or, more broadly, how she might react to other electoral changes (for example, proof of citizenship to register to vote or strict voter identification requirements when casting a ballot) that raise the costs of participation. As such, our findings should not be understood as an endorsement of states or other jurisdictions implementing similar policies targeting the voting rights of eligible voters to potentially raise participation rates, a caution particularly necessary given that some American state legislatures and election administrators are attempting to require proof of citizenship from new registrants.[32]

[32]While the Supreme Court ruled in 2013 that states and locales must allow those registering with the federal form (which requires no proof of citizenship) to vote in federal elections, these entities are free to mandate this evidence to vote in state and/or local contests.

More importantly, unlike other systematic purges, the targeting of potential non-citizens in Florida was halted before the election. This has significant implications for interpreting our estimated treatment effect. As it is unlikely that the aborted purge demobilized any potential voters, our estimates only capture the positive changes in vote propensity we would expect to observe for some had the purge been conducted. In the absence of a halt to the process, the increased costs would have undoubtedly kept some previously registered individuals from voting. While we cannot determine that exact number, we do know that 446 challenged registrants in Miami-Dade County who voted in the 2012 general election would have been barred from doing so, as they did not provide proof of citizenship in response to the challenge. The disenfranchisement of those individuals would serve to counteract (in part or completely) the increase in turnout among others and reduce our estimated average treatment effect toward (or perhaps even below) zero, meaning we cannot conclude how these efforts affect overall turnout levels. Finally, as the costs of proving eligibility (or overcoming other threats to one's franchise) fall disproportionately on low-resource (often minority) individuals, any change in who turns out might exacerbate existing inequalities in participation.

Future work should address these questions and continue to examine the scale and scope of responses to policies perceived to target the voting rights of specific communities beyond Florida. Do non-registered but otherwise eligible citizens, for example, respond differently to perceived efforts to keep them from the polls (for example, efforts that increase the costs of registration) than those already registered to vote (who may attach greater value to their franchise)? Is the participatory effect larger in the relatively limited instances in which election officials target the individual through direct contact (for example, challenge their eligibility to initially register or when already registered, or alter polling place location/availability in a manner perceived to threaten their franchise) than when perceived threats are more broad-based in nature (for example, strict voter ID laws, the reduction or elimination of convenience voting, and election day registration procedures)?

Other work should examine whether reactance exhibits long-term participatory consequences, or if its positive influence on turnout dissipates after a single (or couple) elections, a response akin to the possible novelty effect of convenience voting (Giammo and Brox 2010; Gronke and Miller 2012). Finally, scholars might draw upon Biggers (2014) to better understand how (and for whom) reactance, when elicited, can mobilize political action, as well as expand tests of psychological reactance theory to other political reforms and contexts. As legislative and administrative decisions perceived by some as an affront to their democratic rights continue to proliferate in the United States and beyond, it is imperative to better understand the consequences of the backlash that some of these actions may incur, as well as appreciate in which contexts reactance might not occur.

Supplementary Material. Replication data sets can be found in Harvard Dataverse at https://doi.org/10.7910/DVN/LIWMYQ. The supplementary material for this article can be found at https://doi.org/10.1017/S0007123418000157

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