The Impact of Preregistration on Youth Turnout

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Abstract

Can registration reforms increase voter turnout? In this paper we examine the impact of preregistration, a reform adopted in 12 states that allows young citizens to register before being eligible to vote. To do so we use two complimentary approaches. First, using a nationally representative, pooled cross-section from the Current Population Survey (2000-2012) we implement a difference-in-difference approach combined with lag models to bracket the causal effect of preregistration implementation. Second, focusing on the state of Florida we leverage a discontinuity around voter eligibility to estimate the effect of increased preregistration exposure on turnout of those in the voter file. In both approaches we find that preregistration noticeably increases voter turnout among young people. Implementing preregistration increases youth turnout 2-13%. Those exogenously nudged towards preregistration (because of their date of birth) are 8% more likely to vote than a comparable control group, with turnout gains benefiting both Democrats and Republicans.

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Even within federal constraints, there remains considerable variation across states in the ease of voter registration. Political scientists have long debated the extent to which voter participation might be fostered or hindered by various institutional rules, including registration windows (Brians and Grofman 2001; Hanmer 2009; Neiheisel and Burden 2012; Keele and Minozzi 2013), voter identification restrictions (Vercellotti and Anderson 2006; Mycoff, Wagner and Wilson 2007; Alvarez, Bailey and Katz 2008; Erikson and Minnite 2009; Atkeson et al. 2010), and online registration tools (Herrnson et al. 2008; Niemi et al. 2009; Hanmer et al. 2010; Ponoroff and Weiser 2010; Bennion and Nickerson 2011). While it once was common for scholars to argue that lowering legal obstacles would increase voter turnout (Powell Jr 1986; Burnham 1987; Wolfinger, Glass and Squire 1990; Lijphart 1997) more recent scholarship has challenged such optimistic conclusions; often finding that electoral reforms have little effect (Erikson and Minnite 2009; Keele and Minozzi 2013; Burden and Neiheisel 2013) or even depress political engagement (Burden et al. 2014).

One electoral rule that has not been extensively studied is preregistration laws, whereby individuals younger than 18 are able to complete their registration application so that they are automatically added to the registration rolls once they come of age. Advocates argue that preregistration laws will increase civic engagement among young people (Cherry 2011), a group with notoriously low levels of voter participation (e.g., Dalton 2008). Yet, empirical evaluations of other electoral rules have shown that the expected benefits of removing legal obstacles are not always realized (McDevitt and Kiousis 2006; Hoover and Orr 2007). Given these conflicting viewpoints the question remains: are preregistration laws effective at increasing youth turnout?

Answering this question will inform a timely policy debate as many states consider (or reconsider) preregistration laws. Twelve states have recently implemented preregistration laws and at least 19 states have considered legislation to introduce, amend, or eliminate preregistration.[1] The topic is of particular salience and controversy in North Carolina. In 2009, North Carolina House Bill 908 was passed with wide bipartisan support, enabling 16 and 17-year-olds to preregister. At the time of passage, the law was praised by many as a model for reform — "an excellent example of how to engage young voters."[2] In July 2013, however, a new Republican majority repealed preregistration while instituting a number of other voting restrictions (SB 657 and HB 589). Allegations swirled that the move was driven by partisan strategy; an attempt to make it more difficult for young voters—who had disproportionately voted Democratic in the 2012 election—to participate in future elections.[3] Proponents of the repeal argued that preregistration was confusing to young voters and that the law was a bureaucratic burden to administer.[4] Both sides seem to assume that preregistration laws have an impact (positive or negative) on youth turnout. In this paper,

[1] Proposed bills include those in: Arizona (SB1033, 2013), Colorado (HB1135, 2013), Connecticut (HB5902, 2013), Georgia (HB1335, 2010), Iowa (SB3093, 2012), Illinois (SB1249, 2012), Kansas (HB2256, 2010), Kentucky (SB109, 2009), Massachusetts (SB316, 2013), Michigan (SB101, 2012), Minnesota (HB 2764, 2012), Nebraska (LB127, 2013), Nevada (SB375, 2013), New Hampshire (HB1360, 2010), New Jersey (SB526, 2012), New York (AB306, 2013), Ohio (SB 110, 2013), South Carolina (HB 3697, 2012), Texas (SB1507, 2013), and Washington (HB 1279, 2013). Some states (e.g. New York and Washington) have long debated these reforms, with multiple bills proposed over time.

[2] Quote drawn from the Fair Vote organization. See www.fairvote.org/voter-preregistration-legislation/.

[3] For examples of the media coverage surrounding the repeal see "North Carolina Voter ID Law Targets College Students" in the *Huffington Post* (7/7/13) or "President Obama May Hit Political Turbulence in North Carolina visit" in the *Washington Post* (1/14/14).

[4] For example, State Senator Bob Rucho argued that repeal added "clarity and ... certainty as to when ... young [people are] eligible to vote". See "No More Preregistration Puts Youth Vote in Question" in *WUNC* (9/6/13).

we attempt to empirically scrutinize this claim.

We estimate the effect of preregistration on youth turnout using two complimentary approaches. First, using a nationally representative, pooled cross-section from the Current Population Survey November Supplement (2000-2012) we implement a difference-in-difference approach. We supplement this with lag models to create bracketed estimates of preregistration's impact (Guryan 2004; Angrist and Pischke 2008). Second, focusing on the state of Florida, we leverage a discontinuity around voter eligibility in the 2008 election, using fuzzy regression discontinuity models to estimate the effect of preregistration on turnout in 2012. Whereas the first approach offers strong external validity, the second approach offers strong internal validity as to the estimated effect of preregistration. In both approaches we find that preregistration has positive, non-negligible effects on young voters' participation rates. From the CPS models we find that preregistration laws increased turnout rates by 2 to 13%. Among those who comply, by preregistering, voter turnout is about 8 percentage points higher than a comparable control group. Finally, our analysis finds that preregistration is equally effective among young Democrats and young Republicans, with some indication that preregistration might actually help Republicans slightly narrow the Democratic advantage among young voters. On the whole, our analysis suggests that preregistration is a viable electoral reform for increasing youth turnout.

Background

Political scholars and public policy makers have long puzzled about the depressing (and worsening) participation rates of young Americans. In 1972, 53 percent of 18-to-29-year-olds cast a ballot; In 2000, the number was just 36 percent, a historic low. From 2000-2012, the turnout gap between 18-29 and 30-44 year olds averaged 14%.[5] The low levels of turnout among young Americans has been attributed to a variety of factors. Some researchers focus on the lower levels of resources among young people that can impede participation (Wolfinger and Rosenstone 1980). Others emphasize that younger Americans are less likely to have the psychological affiliations and attachments thought to be so important for political engagement (Timpone 1998; Highton and Wolfinger 2001). Young voters may also face higher transaction costs as they figure out the ins and outs of voter participation. For example, newly eligible voters may be unfamiliar with the registration system, including how and where to register to vote, making them more likely to miss registration windows or be unaware of election day requirements (McDonald 2009). Even if these informational costs are small, this may be enough to deter young voters (e.g., Brady and McNulty 2011). Geographic mobility can make these costs be incurred repeatedly, so that registration requirements make turnout even less likely (Highton 2000; McDonald 2008; Ansolabehere, Hersh and Shepsle 2012).

Preregistration reforms are intended to lower the institutional obstacles of engaging in the political process. These allow citizens younger than 18 to add their name to the voter rolls.

[5]Gaps from the CPS November supplements indicate a stubborn gap since 2000: 17% (2000), 18% (2002), 12% (2004), 16% (2006), 9% (2008), 14% (2010), 11% (2012).

By filling out an application nearly identical to a regular voter registration form, minors add themselves to a queue, automatically activated upon reaching voting eligibility at age 18.[6] Pre-registrants are entered into the state's voter database as a pending registration that becomes active when the individual becomes eligible to vote. Although proponents of preregistration argue that it will increase young voter turnout (Cherry 2011), there have been limited empirical evaluations of that claim. In the one exception, McDonald and Thornburg (2010) find higher turnout rates among those who preregistered than those who registered after they turned 18 in Florida and Hawaii. In the 2008 election, for instance, McDonald and Thornburg (2010) finds that pre-registrants were 4.7% more likely to vote than those who registered after they turned eighteen. Although consistent with the claim that preregistration impacts turnout, these findings are far from conclusive. Looming is the issue of self-selection: we might expect that those individuals who are especially interested in politics will both be more likely to preregister and more likely to vote. If so, then any relationship between preregistration and turnout is spurious, an artifact of unobserved levels of political interest, motivation, or propensity to turnout.

Indeed, this is the key explanation offered for the null findings in the growing body of research using more sophisticated causal techniques to examine the effectiveness of electoral reforms. While early observational work concluded that "burdensome registration requirements [are] a major institutional deterrent to voting" (Lijphart 1997, 7), more recent

[6] Many states allow 17 year olds who will turn 18 by election day to be added to the rolls when they are 17. We, and others (McDonald and Thornburg 2010), make the distinction between this and pre-registration, which allows young people to register at even if they won't be eligible in the next election. Our coding does not include Alaska, Georgia and Iowa because of the unique restrictiveness associated with their preregistration-like provisions.

empirical analyses reach a more sanguine conclusion. As Timpone explains, "chronic non-participants are not likely to flood the polls simply because registration barriers diminish" (Timpone 1998, 155).[7] Recent work supports this view, finding that many institutional reforms have little to no effect (Erikson 1981; Highton 1997; Martinez and Hill 1999; Berinsky, Burns and Traugott 2001; Highton 2004; Ansolabehere and Konisky 2006; Hanmer 2009; Keele and Minozzi 2013; Burden and Neiheisel 2013) or may even depress turnout (Burden et al. 2014).

Despite the evidence that other electoral reforms are ineffective, we believe a strong theoretical case can be made that preregistration laws should in fact increase turnout among young voters. Critically, preregistration laws leverage the powerful contextual forces of campaigns. For a subset of the electorate it removes an obstacle to voting (registration) when an individual is more likely to be attentive to politics—during a political campaign. That is, sixteen year olds who might not be eligible to vote in an election can nonetheless join the political system in the heightened political salience of an electoral campaign. Once in the political system other mechanisms may then come into play. Being a part of the political system might, for instance, change a young person's identity as participant rather than outsider, which could in turn affect her efficacy, attentiveness, and ultimately participation in future elections (Bryan et al. 2011). Many scholars have concluded that there is a strong habitual nature of political engagement (e.g., Plutzer 2002; Fowler 2006; Meredith 2009), so earlier integration into the political system might set those forces in action. Sears and

[7] Quoted in Burden and Neiheisel (2013).

Valentino (1997, 45) argue that relatively small contextual factors may "catalyze pre-adult socialization, generating predispositions that persist into later life stages."

Preregistration might also increase the political information and mobilization attempts aimed at young citizens. Once a young person is part of a state's voter file, they are more likely to be contacted by candidates, parties, and interest groups who use registration lists in targeting their campaign communications and mobilization efforts (Hillygus and Shields 2009; Hersh 2014). Finally, we might expect more powerful effects from preregistration laws than other electoral reforms because it is aimed at young people, and research suggests that the impact of electoral reforms face diminishing returns over the life course (Butler and Stokes 1974). Indeed, preregistration laws might piggy-back on education institutions as a mobilizing force. That is, most of the individuals who are eligible to preregister will be in high school, and research has shown that civics curriculum interact with voting eligibility to increase turnout later in life (e.g., Niemi and Junn 2005). This may be important as some states' preregistration laws mandate that state election officials hold registration drives within high schools. McDonald (2009) concludes that these civic programs play an important role in the effectiveness of preregistration laws. Although our analyses cannot tease these mechanisms apart, it seems likely these factors may well interact with preregistration laws in shaping mobilization effects.

In sum, there are clear theoretical reasons to suspect that preregistration is an effective reform for increasing turnout. The empirical challenge is how to estimate an unbiased effect of preregistration given the powerful role of individual motivation in explaining turnout

(Erikson 1981; Berinsky, Burns and Traugott 2001). Within the electoral reforms literature many studies rely on state level "treatment" with a strong assumption about how electoral reforms originate (Erikson and Minnite 2009). They assume that election reforms generate exogenously, outside of the control of vested parties. These studies take variation in implementation as evidence of exogenous assignment. In this view, models need only control or match on observable traits. However, increasing evidence indicates that this approach can produce misleading results. Some evidence shows that election laws are endogenous to political participation (Hanmer 2009; Erikson and Minnite 2009), either due to simultaneity (i.e., reforms result as responses to turnout) or to complex, not well-understood networks of unobserved variables (e.g., motivation). For example, quasi-experimental studies of the impact of election day registration have shown marked differences to observational studies on the same topic (Burden et al. 2014; Keele and Minozzi 2013). This has implications for the effectiveness of traditional observational and panel identification strategies in studying electoral systems (Keele and Minozzi 2013).

Our approach takes two steps to move towards a more compelling identification of preregistration's impact. First, we examine the broader impact of preregistration across multiple states using a pooled cross-section. This approach combines difference-in-difference and lag models to bracket the aggregate effect of preregistration. We then narrow our focus to one state with a preregistration law (Florida) and use a discontinuity in take-up of preregistration, running regression discontinuity models to estimate the causal impact of increased preregistration exposure, among those in the voter file. This combination of approaches give us a complimentary picture of the impact of preregistration on young voter turnout.

Analysis #1: Current Population Survey

We first draw from a pooled cross-section of the 2000-2012 Current Population Survey (CPS) to examine the effect of preregistration laws on turnout levels of among young voters. The CPS gives us a reliable, large, nationally-representative sample with coverage of both registered and non-registered individuals, with the bi-annual November Supplement offering self-reported measures of individuals' voting and registration behavior in the most recent election. Because our focus is on young voters we restrict our sample to individuals ages 18-22.[8]

As electoral reforms go, preregistration is relatively new, with most laws being adopted in the last 5 years. Table 1 shows the basic trends in voter-turnout among young voters (18-22) from 2000-2012 for those states with preregistration laws in place. **Bolded** values indicate preregistration laws being in effect. For comparison, the final rows in the table show the average turnout rate of those states with preregistration and states without.

[8] We estimated the models using a variety of age cutoffs ranging from 18-29, with no change in the substantive conclusion. Results available on request.

Table 1: Turnout Among Young Voters (CPS, 2000-2012)

State	2000	2002	2004	2006	2008	2010	2012
Hawaii	**24.5**	**18.9**	**29.9**	**17.3**	**25.8**	**23.1**	**27.6**
Florida	**36.3**	**23.5**	**41.4**	**16.3**	**44.0**	**21.4**	**40.9**
Oregon	42.5	27.7	55.7	36.0	**53.4**	**34.5**	**48.0**
California	30.0	18.3	35.6	20.3	40.5	**24.0**	**37.7**
North Carolina	38.6	21.4	45.3	19.2	52.1	**23.4**	**50.2**
Rhode Island	46.4	22.5	40.8	31.4	49.1	**23.8**	**46.8**
DC	52.7	31.1	54.8	32.0	65.0	**31.9**	**61.9**
Maryland	37.7	23.6	44.1	30.7	50.0	**22.6**	**46.2**
Maine	54.9	35.2	61.4	32.4	57.2	33.5	**51.3**
Delaware	44.1	18.7	46.8	24.3	48.4	28.9	**43.4**
Pre-Reg States	36.1	22.5	42.6	23.5	46.0	25.3	43.3
Other States	40.9	25.4	47.6	26.9	47.8	24.7	43.8

Notes: Young voters are defined as those 18-22. States with preregistration laws in effect are bolded.

Table 1 offers "smoking-gun" evidence that preregistration might increase young voter turnout. Using the data in the table, two simple comparisons can be made. First, we can compare turnout patterns within preregistration states over time. Second, we can compare turnout patterns between preregistration states and non-preregistration states. Combining these comparisons offers a very simple difference-in-difference estimate. Depending on which years are considered pre v. post treatment the simple difference-in-difference estimates of preregistration's impact on young voter turnout are somewhere between 1.3 and 4 points.[9] Of course, such a comparison is overly simplistic, likely subject to bias from aggregation or omitted variables. For example, states like Oregon are surely different from Hawaii in systematic ways not accounted for by this simple comparison. A more nuanced approach would attempt to account for these types of heterogeneities across states.

[9] With 2008 as pre and 2012 as post: (43.3 - 46) - (43.8 - 47.8)=1.3; With 2006 as pre and 2010 as post: (25.3 - 23.5) - (24.7 - 26.9)=4.0.

Methods: Current Population Survey

The pooled CPS data allows us to estimate both differences (across time and states) mentioned in the last section. In a difference-in-difference model we are able account for some unobserved variation, eliminating many sources of bias (Gelman and Hill 2007). State fixed effects can account for permanent characteristics of the state (e.g., persistent electoral institutions or social capital), year fixed effects for shared time trends (e.g., electoral context or national campaigns), and interactions between the two for year state-specific year effects (e.g., specific candidates or state-level campaigns). These fixed-effects when combined together form a standard difference-in-difference model (Gelman and Hill 2007, 228).[10]

Difference-in-difference models are fairly standard in electoral reform studies (e.g., Knack 1995; Fitzgerald 2005; Burden and Neiheisel 2013) as they offer a powerful antidote for many potential sources of bias left unaccounted for in cross-sectional models. Equation (1) shows the form of this model. The first difference in equation (1) is between states with and without preregistration and the second is before and after implementation.

$$Y_{it} = \lambda_0 + \lambda_p P_{st} + \lambda_{\alpha} \alpha_s + \lambda_{\delta} \delta_t + \lambda_{\gamma} \gamma_{st} + \lambda_X X_{it} + \epsilon \quad (1)$$

In the model, the key predictor variable is an indicator if the respondent's state had a preregistration law in effect (P_{st}) and the outcome is whether or not the individual reported voting (Y_{it}).[11] The analysis is restricted to young citizens, defined as individuals age 18-22.

[10] We use the terms "fixed effects models" and "difference-in-difference models" interchangeably. Difference-in-difference models are "a special case of the ... fixed effects model" and are fitted "with a regression of the outcome on an indicator for the groups, an indicator for the time period, and the interaction between the two" (Gelman and Hill 2007, 228).

[11] Like others (Burden et al. 2014) we code voting as 1 if the individual indicated that they voted in the

To be sure, this model offers only a rough approximation of exposure to preregistration. For one, there is not a clear age threshold that should be used for the analysis since exposure to preregistration varied by age, state, and year.[12] Second, individuals of the same age can have different opportunities to preregister or regular register simply because of the nuances of date of birth and election timing — a fact we leverage in the next section of the paper. Unfortunately, the CPS (and other comparable multi-state data sources) include age rather than date of birth in their public-use files, so we have a less precise exposure measure than that allowed by date of birth. Nonetheless, the model offers a reasonable approach for testing if preregistration laws are related to *aggregate* changes in turnout among this age group. In other words, the estimated effects can be thought of as analogous to intent-to-treat rather than treatment on the treated estimates (Bloom 1984).

Other model parameters for equation (1) include α_s for the state fixed effects, δ_t for the year fixed effects, and γ_{st} for the full set of interactions between the two. Additionally X_{it}, a matrix of time varying controls, is included to absorb some time varying heterogeneity. The λ's represent the effect of preregistration and the other model components on turnout. To adjust for potential in-cluster correlations we cluster our standard errors to the state-year level. As is common, we report results from a linear specification of the dependent variable for simplicity in the interpretation of coefficients (e.g., Olken 2010). As fully reported in the

most recent election and as 0 if they answered "no," "don't know," "refuse to answer," or have no response recorded.

[12] The exact age range exposed to preregistration varies across states and years, but there were always at least two states for whom there were individuals in this age range were were exposed. For example, in 2008, only 22 year olds in HI and FL would have been exposed to preregistration opportunities; in 2012, 18 year olds in at least 8 states had been exposed to preregistration. However, as we will show in subsequent sections, these borderline ages vary substantially in their exposure to preregistration.

online appendix a probit specification yields similar (even stronger) results.

The difference-in-difference in equation (1) accounts for a wide variety of potential biases. However, this approach has a key limitation: it is unable to control for unobserved time-varying factors. In examining the effect of preregistration we might be worried that these laws are endogenous—states with higher turnout could be more likely to implement preregistration because of pressure from vested constituencies or perhaps states with attentive political elites might implement preregistration when youth turnout is particularly low. Either scenario would introduce simultaneity concerns that could bias difference-in-difference estimates of preregistration's impact on voter turnout. Indeed, this type of bias has increasingly troubled scholars of electoral reforms (Ansolabehere and Konisky 2006; Burden and Neiheisel 2013; Keele and Minozzi 2013). This bias is difficult, but not impossible, to address.

Our approach takes one step beyond a difference-in-difference in an attempt to address the endogeneity concern associated with time-varying unobservables. To do so we compliment our difference-in-difference with a set of models with lags. When used in separate, but similar, models the estimates from these two models can provide bracket estimates; a range of values in which the "true" effect falls (Angrist and Pischke 2008).[13] Angrist and Pischke (2008, 243-247) and Guryan (2004) show this point formally. They prove that fixed effects and lag models when used together "have a useful bracketing property" assuming lagged outcomes or fixed characteristics are behind selection into treatment (Angrist and Pischke 2008, 246). In using this approach the deciding factor is the relationship between the treatment and the

[13]Lags should not be included in difference-in-difference models as the error term and the lagged dependent variable are related through the lagged error term (Angrist and Pischke 2008, 245)

lagged dependent variable. When the relationship is positive the fixed effects models set the lower bound and lagged models set up the upper bound. When the relationship is negative the opposite is true (Angrist and Pischke 2008; Guryan 2004).

Thus, if we are concerned that the difference-in-difference fails to account for endogenous variation in the adoption of preregistration laws, biasing the results upward, we can estimate, as a robustness check, the lower bounds of the preregistration effect using lag models. To do so equation (2) sets aside the fixed effects and uses a lagged turnout at the state level.

$$Y_{it} = \lambda_0 + \lambda_p P_{st} + \lambda_Y Y_{s,t-2} + \lambda_X X_{it} + \epsilon \tag{2}$$

Equation (2) is similar to equation (1) in its unit of analysis, outcome, treatment, controls. As in equation (1), the preregistration treatment is at the state-year level, necessitating standard error adjustments.

Although the unit of analysis in our model is an individual (with state-clustered standard errors), we do not have an individual's turnout in the previous election. Unfortunately the CPS does not ask individuals about turnout across elections. Moreover, even if the CPS did have turnout measures across years, missing data would pose a significant problem in our application as many young voters were not eligible to vote in the previous election. Thus, lagged turnout ($Y_{s,t-2}$) is aggregated to the state level.[14]

In the next section we estimate our difference-in-difference and lag models, illustrating

[14] We use the lagged presidential election year turnout (2008). As a robustness check on our lower bound estimate, we estimated with all variables aggregated to the state level. Although this reduces the predictors to just state-level variables and reduces the sample size substantially, we are reassured by the fact that the state-level results are similar—a coefficient of .01 with p-value of .09, one-tail (p<.05 in baseline model with no controls).

the bracketing property in our preregistration application.

Results: Current Population Survey

Table 2 reports the bracketed effects of preregistration laws on youth turnout rates, using the Current Population Survey from 2000-2012.[15] Column 1 corresponds to equation (1), the difference-in-difference model. Column 2 corresponds to equation (2), the lag model. The dependent variable is whether or not a young individual reported voting in the previous election. The full set of model results are reported in the online appendix.

Table 2: Voting, Bracketing Bias

	(1) Diff/Diff Model (Upper Bound)	(2) Lagged Model (Lower Bound)
Preregistration State	0.13*	0.02*
	(0.00)	(0.01)
Controls?	Yes	Yes
State FE?	Yes	No
Year FE?	Yes	Yes
State x Year FE?	Yes	No
State Lagged Vote?	No	Yes
Constant	0.16*	0.04
	(0.03)	(0.03)
R^2	0.53	0.52
N	44,821	39,023
Number of Clusters	357	357

Notes: Dependent variable is whether or not an individual voted. Sample is voters ages 18-22. Cluster-robust standard errors in parentheses (* $p < 0.05$) Controls: marital status, age, gender, family income, college attainment, metropolitan status, race (hispanic and white), length at current address, interview type, year, and registration status.

The difference-in-difference model finds a substantial increase in turnout when a preregistration law is implemented. Our lower bound estimate indicates a smaller, but still

[15] For the lag models we include information from the 1996 and 1998 November Supplements.

positive and significant, effect. The bracketing properties of the models suggest that the true effect of preregistration on voting is somewhere between 2% and 13%. All estimates are positive and statistically distinct from 0. Thus, while the effects could be small it seems that preregistration does increase turnout among young voters.

Although this approach is better able to account for unobserved heterogeneities than a naive analyses between preregistration and turnout, it still has limitations. The treatment is rather crude, not able to cleanly identify who was exposed to preregistration. As such, we cannot speak to the particular causal mechanism by which preregistration laws are related to increased turnout. Though panel models allow us to rule out some unobserved factors, they may not capture all unobserved heterogeneity (Keele and Minozzi 2013). We can never be certain that models rule out all important time-varying confounders; that lagged outcomes or fixed characteristics describe selection into treatment. Put simply, even with the most sophisticated panel techniques may not go far enough to help us precisely estimate the impact of preregistration on youth turnout.

Thus, to get around these limitations, we move from an across-state to a within-state comparison. By using the Florida voter files we are able to generate a more precise measure of preregistration enrollments and leverage a source of exogenous variation in preregistration exposure. In this analysis we use jumps in exposure to preregistration depending on individuals' date of birth. Such an approach trades the breadth of the analysis given in this section for more rigorous, internally valid estimates that also hints at potential causal mechanisms.

Analysis #2: Florida Voter File

In this alternative approach for estimating the effects of preregistration we focus on the state of Florida. We chose Florida for several reasons. First, Florida had a preregistration law in place early enough to examine relevant outcomes. For reasons that will become apparent below our analysis requires a state that has had preregistration laws in effect through at least two election cycles. Second, Florida's registration files contain full date of birth, which is necessary to precisely determine exposure to preregistration.[16] Finally, Florida has the advantage of having a large, diverse population with a sizable pool of young potential voters.

Florida was the first state in the U.S. to implement a preregistration law. Since 1990, 17 year olds could be added to the voter rolls, even if they would not be eligible to vote in the upcoming election. Since 2007, 16 year olds have also been able to preregister. Take-up of preregistration has grown over time from about 10,000 (representing 10% of 17 year olds) in 1992 to about 60,000 (30%) in 2004 (McDonald 2009). In the May 2013 voter file approximately 300,000 of 4 million voters (8%) had been added to the Florida voter file through preregistration.

In estimating the impact of preregistration on youth engagement, we focus turnout in the 2012 election among young Florida registrants age 21-22.[17] Simply by accident of the

[16] In 2008 only three states had preregistration in place: Oregon, Hawaii, and Florida. Hawaii does not have DOB in their voter file, eliminating it from potential consideration (Ansolabehere and Hersh 2014). Florida's voter file has birthdays for 99.95% of our sample. We selected Florida over Oregon because the unique vote-by-mail rules in Oregon might have undermined the generalizability of the results.

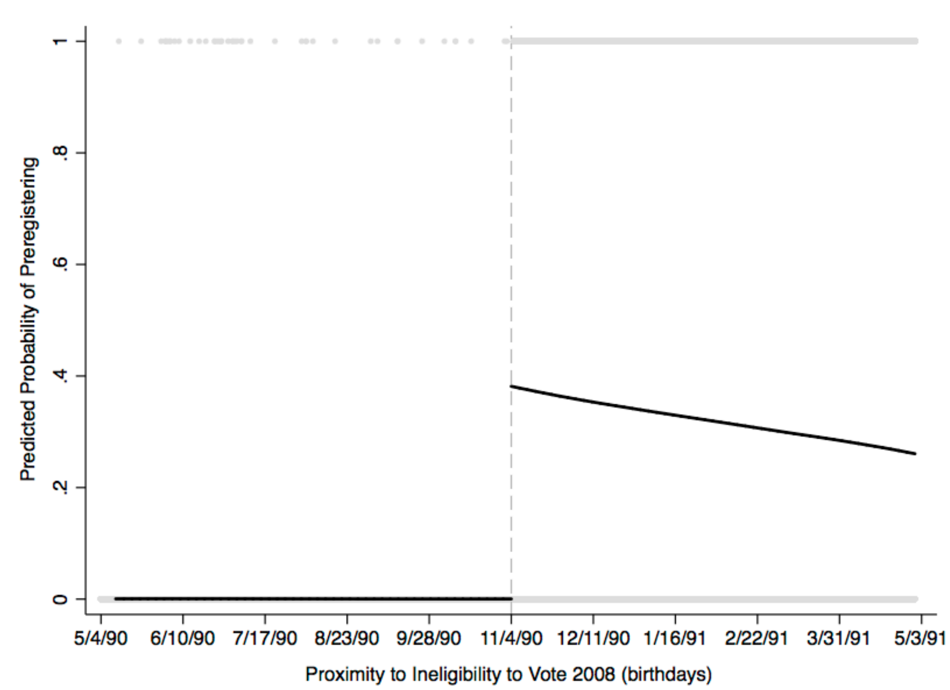
[17] We use the voter file as it was downloaded in May 2013. Given Florida voter rules, purging of the voter file should not have occurred between when individuals came of preregistration age and 2012. Nor is it likely to have occurred in the time between the 2012 election and when we downloaded the data file, with media reports and the state Board of Elections confirming that purges largely occurred after May 2013.

date when individuals were born relative to election day, individuals in this small sample are divided into two seemingly arbitrary groups: those *marginally eligible* to vote in the 2008 election (17 turning 18 by November 4, 2008) and those *marginally ineligible.* We use this discontinuity in date of birth as sorting mechanism that assigns individuals to preregistration and non-preregistration groups (with some non-compliance) in an as-good-as random fashion. These individuals are similar on many characteristics but differ as to when in their life they are encouraged to register, with marginal ineligibles being encouraged to preregister at a higher rate than marginal ineligibles. To be clear, marginal eligibles also had the opportunity to preregister, but that opportunity occurred outside the context of an election because they were able to regular register for the 2008 election. Thus, our sample consists of young adults in the 2012 Florida voter file who were marginally eligible or ineligible to vote in 2008; Our treatment is eligibility to preregistration during the 2008 campaign and our control is eligibility to regular registration during the same time.

Figure 1 shows the resulting variation in preregistration rates graphically, plotting each individual as a preregistrant (1) or regular registrant (0) across birthdays for a 6 month window on either side of the eligibility cut-off (marked by dashed line). Those to the left of the cutoff were marginally eligible to vote in 2008. Those to the right were marginally ineligible in 2008. A local linear regression on either side of the cutoff is also displayed to show the jump in preregistration enrollment rates around the cutoff.

Figure 1 shows two things: first, a clear discontinuity exists in preregistration rates at the eligibility to vote cutoff and second, it is substantial. According to local linear models, marginal ineligibles were nearly 40 percentage points more likely to have preregistered than marginal eligibles.[18]

Figure 1: Fuzzy Preregistration Treatment



Note: The figure plots preregistration (1) and regular registration(0) in the 2012 Florida voter file across birthdays for the 6 month window on either side of 2008 voter eligibility cutoff. Those to the left of the cutoff are *marginally eligibles*. Those to the right are *marginally ineligibles*.

As is also seen in the graph, there is some non-compliance (about 30% of sample). Those who are marginally ineligible sometimes wait until they are older to regular register (most of our noncompliance comes from this behavior). And those marginally eligible sometimes pre-register long before the election, when they are 15 or 16. Nonetheless, on average marginal ineligibles are much more likely to preregister than marginal eligibles. In other words, in-

[18] A similar discontinuity is observed in the 2008 voter file, in which the percentage of individuals preregistering in the 2004 election was about 30% higher among marginal ineligibles than marginal eligibles (see online appendix).

dividuals marginally ineligible are exposed to an increased dosage of preregistration. This difference forms the essence of our identification strategy.

Why does this discontinuity exist? We expect the timing in which elections occur in one's life course is likely key. Elections encourage registration. When an election approaches, both marginal ineligibles and eligibles are exposed to similar appeals to register. Both groups are exposed to in-school registration drives, campaigns, and the overall excitement surrounding an election. The sum result is that many will enter the political systems at this time. Preregistration laws simply allow younger people to do so.

Methods: Florida Voter File

To estimate the impact of preregistration on turnout, we use a fuzzy regression discontinuity approach. This approach is required as compliance is not 100%: those who are marginally ineligible sometimes wait until they are older to regular register and those marginally eligible sometimes preregister.[19] Still, as we saw in Figure 1 there is a discrete jump at the eligibility cutoff. So long as the eligibility discontinuity is as good as random, this approach will produce estimates of preregistration's mobilizing power that are free of omitted variable bias (from observables and unobservables) and simultaneity.

Fuzzy regression discontinuity utilizes an instrumental variables approach, with the sorting rule (eligibility to vote in 2008) serving as an instrument of the treatment behavior (preregistration). Equations (3) and (4) show the two-stage form of this approach, common

[19] This approach was pioneered by Trochim (1984) and has been increasingly used in public policy, economics, and political science (e.g., Ferraz and Finan 2009; Eggers and Hainmueller 2009; Burden and Neiheisel 2013).

to those familiar with two-stage least squares.

$$P_{i,2008} = \gamma_0 + \gamma_1 I_{i,2008} + \gamma_2 R_{i,2008} + \epsilon \tag{3}$$

$$Y_{i,2012} = \beta_0 + \beta_1 P_{i,2008} + \beta_2 R_{i,2008} + \epsilon \tag{4}$$

Equation (3) displays the first stage. In it, ineligibility in 2008 ($I_{i,2008}$) and proximity to ineligibility ($R_{i,2008}$) to predict whether an individual preregistered in 2008 ($P_{i,2008}$). The proximity variable allows us to estimate the effect of preregistration at the eligibility cutoff using data further from that margin. The $\gamma' s$ in this equation represent estimated first stage parameter estimates, with γ_1 revealing the estimated difference in preregistration rates between marginal ineligibles and marginal eligibles (on average).

Equation (4) displays the second stage. In it, the influence of preregistration in 2008 ($P_{i,2008}$) on voter turnout in the next presidential election ($Y_{i,2012}$) is estimated. It is important to note that we observe whether individuals *ever preregister* during their window of opportunity to do so. Thus, we can estimate not only the impact of offering preregistration (the ITT) but also the effect of preregistration take-up (the TOT). Thus, the coefficient of interest in our models is β_1 (for the TOT) and the coefficient on $I_{i,2008}$ when it is substituted into the second stage and run in a normal OLS model (for the ITT).[20]

Equations (3) and (4) are simplifications of the regression discontinuity model. In practice we vary the parameterization of the proximity to ineligibility variable and the bandwidth of the data used. Our models are generally robust to variations in these model specifications,

[20] As is done in other applications, we use OLS with a binary dependent variable for simplicity in interpretation (e.g., Olken 2010). The results do not change with probit regression (see online appendix).

further evidence of the strength of our discontinuity as valid sorting mechanism (Imbens and Kalyanaraman 2012; Lee and Lemieux 2010).

Specification Checks

Comparing our two groups within a very narrow bandwidth allows us to look at the impact of exogenous variation in preregistration on turnout among registered voters in the Florida voter file. Establishing similar treatment and control group relies on the assumption that eligibility in 2008 sorts individuals in an "as good as random" manner (Lee and Lemieux 2010). This assertion may be challenged if the discontinuity can be precisely manipulated or treatment at the margin is confounded by some alternative treatment. As with randomized experiments certain specification checks are illuminating to see if the underlying assumptions are supported. In our application, for all checks run, our discontinuity appears to be valid.

Similar to a classic randomized-control experiment an informal way to check exogenous assignment involves examining the balance of observable covariates across treatment and control groups (Lee and Lemieux 2010). As regression discontinuity relies on the assumption of local balance, we test for covariate similarity within a narrow bandwidth. Table 3 shows the balance of several potentially important variables close to the eligibility to vote discontinuity. It uses a bandwidth of 18 days around the eligibility margin.[21]

[21] We choose this bandwidth as there is some evidence in the results section that this is the range where we have sufficient power to make clean inferences.

Table 3: Balance at the Eligibility to Vote Margin

Variable	Minimum	Maximum	Control	Treatment	Prob. T=C
Preregistering (Treatment)	0	1	0.02	38.17	0.00
African American (individual)	0	1	21.38	21.51	0.83
White (individual)	0	1	49.93	49.58	0.64
Hispanic (individual)	0	1	18.70	19.53	0.17
Other Race (individual)	0	1	4.45	4.34	0.73
Democrat (individual)	0	1	42.92	41.31	0.03
Female (individual)	0	1	50.06	52.81	0.01
% HS Degree '08 (County)	59.9	92.8	85.44	85.44	0.99
% Poverty '08 (County)	7.9	29.3	15.07	15.15	0.27
$ Median Income '08 (County)	31,443	67,238	45,435	45,246	0.10
Population '08 (County)	8,260	2,472,387	541,659	528,706	0.14
Median Age '07-'11 (County)	29.2	62.2	40.42	40.35	0.34
% White '10 (County)	16.0	9.0	65.83	65.86	0.90
% Af. American '10 (County)	3.0	56.0	14.13	14.11	0.88
% Hispanic '10 (County)	2.0	65.0	15.97	15.93	0.80
#Churches '08 (County)	13	1437	397	390	0.18
Obama Office '08 (County)	0	1	59.61	58.54	0.16
% Married '07-'11(County)	37.0	62.0	48.52	48.54	0.90

Notes: The first two columns show the minimum and maximum values of the variables in our sample. *Coverage* is similar across treatment and control groups, with both groups having the same range for these explanatory variables. The next two columns test for covariate *balance* at the eligibility cutoff. For 0/1 variables, covariate means are scaled to percentages. The window used is 18 days on both sides of the November 4th cutoff. Wider bandwidths show similar results, with Prob. T=C decreasing as bandwidths increase, but substantive differences remaining small.

We again see the substantial jump in the rate of preregistration in the treatment group. We also see that most potential confounders are statistically balanced at the cutoff and have similar coverage. Thus, we find little evidence that our discontinuity is not locally as-good-as random. And this result also provides evidence of a lack of precise sorting at the eligibility threshold.[22]

Another concern might be less with the assignment of treatment and control than with interpretation of the treatment effect. If a treatment other than preregistration varies at the same cutoff our results could be misattributed. Although we know of no institutional

[22] In the online appendix, we provide the more formal McCrary density test for precise manipulation of the running variable suggested by McCrary (2008).

cutoff that shares the November 4th cutoff, our treatment and control group differ in two fundamental ways besides whether or not individuals preregistered.[23] First, they differ in their age at being added to the voter rolls (preregistering marginal ineligibles are added about a year younger on average than regular registering marginal eligibles). This is by design. The age difference is part of the preregistration treatment. Second, individuals marginally ineligible to vote in 2008 (those nudged towards preregistration) obviously could not vote in 2008 whereas the control group could, and thus may have developed more of a habit for voting (Meredith 2009). The concern is that this may conflate our estimates of preregistration with habitual forces. We address this possibility in a subsequent section of the paper (see the habitual biases section). We simply note here that both of these differences—the younger age (part of preregistration treatment) and the less voting experience—likely bias our estimates downward, because of expectations that a slightly older, more politically experienced control group would vote at higher rates than our treatment group. This likely makes our estimates conservative of preregistration's true impact on young voter turnout.

Finally, we should emphasize that our treatment effect is localized to the 2008 election. Thus, we cannot separate out the effect of being eligible to preregister from the effect of being eligible to preregister *within the context of a presidential campaign*—a point we return to in the conclusion.

[23]We do not observe discontinuities in the probability of preregistration or in our outcome for any given random point on our forcing variable not at the eligibility cutoff. The cutoff for eligibility to enter school occurs within our window (on 9/1/1990), but not at the margin for eligibility. When we control for the school eligibility cutoff our results do not change.

Results: Florida Voter File

Table 4 shows our key results. The model controls for a variety of pre-treatment factors both at the individual and geographic level; Coefficients for the controls are reported in the online appendix. Reassuringly, the estimated effect is not sensitive to the controls included.[24] This suggests that where covariates are not balanced it does not influence the estimated effect, beyond influencing its precision.

The estimates in Table 4 are based on an RD model with arbitrarily narrow bandwidths, using a linear specification of the running variable.[25] Reported in column 1 is the intent to treat (ITT) effect of preregistration on voter turnout. In this case, the ITT is the effect of offering preregistration, not accounting for program take-up. It is equivalent to running model (4) substituting ineligibility $I_{i,2008}$ for the preregistration variable.

[24] Models without controls indicate an ITT of 0.024 and a TOT of 0.08. Results are also robust to alternative standard error adjustments: clustering by county, precinct, birthday, and birth week and various bootstrapping procedures.

[25] As suggested by Lee and Lemieux (2010) we checked our models with a number of specifications. Because of space restrictions we do not report all possible models. However, our results are robust to alternative specifications including a wide bandwidth with a higher order polynomial of the running variable and a variety of bandwidths using local linear regression.

Table 4: Florida RD Estimates

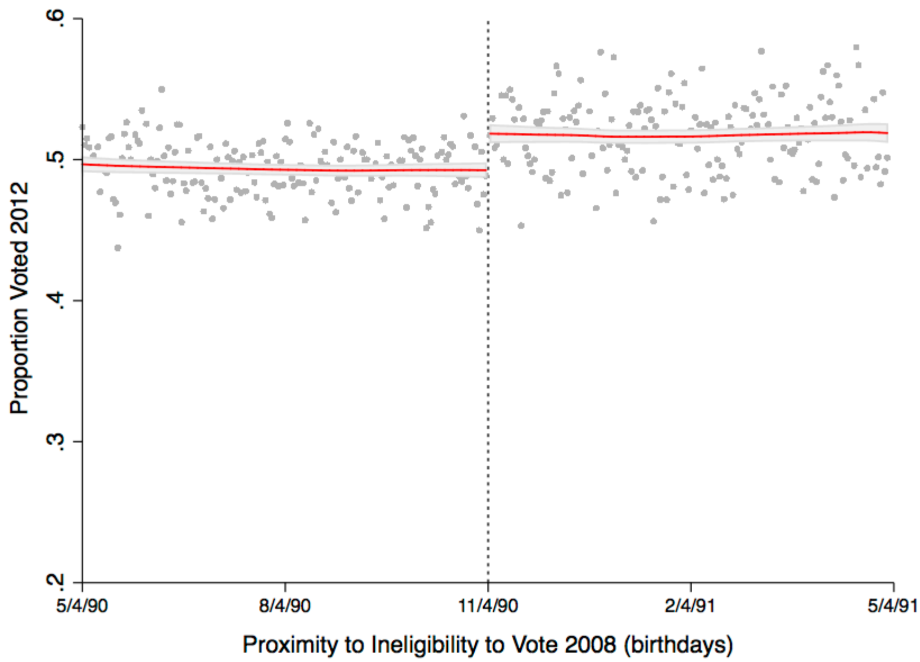
	(1) ITT	(2) TOT	(3) ITT: FE	(4) TOT: FE
Preregistration	0.03*	0.08*	0.02*	0.08*
	(0.01)	(0.03)	(0.01)	(0.03)
Controls	Yes	Yes	Yes	Yes
Fixed Effects?	No	No	Birthday	County
Constant	0.048	0.083	-0.024	0.00
	(0.088)	(0.089)	(0.058)	(0.000)
F-Weak Instruments	.	2,963	.	2,007
MSE	0.24	0.24	0.24	0.24
N	36,790	36,790	71,251	36,790

Robust standard errors in parentheses. $* \ p < 0.05$. For models 1-2 and 4 the sample is registered voters born from May 4, 1990- May 3, 1991. Model 3 includes similar dates for those born in 1988-1989, to estimate birthday fixed effects. Estimates are based on a 2 month window, and a linear specification of the running variable. Controls: race (individual and county), party, gender, proportion of population that graduated high school (county), poverty (county), voter turnout (county), median age (county), and an indicator for the Democratic presidential party having a campaign office in the county.

The model in column (1) indicates that the effect of offering preregistration on young voter turnout is a 3% bump on average in the probability of voting. Noticeably, this estimate is in the bracketed range from the CPS model estimates provided in a previous section. The TOT, reported in Column 2, takes into account take-up of treatment, estimating the effect of preregistration on turnout. In our case, compliers are those who 1) were ineligible to vote in 2008 and preregistered and 2) were eligible to vote in 2008 and regular registered. Non-compliers are the others who 1) were eligible to vote in 2008 and preregistered 2) were ineligible to vote in 2008 and regular registered.

The results show that the effect of preregistering among compliers was to increase the probability of voting by 8% (on average). Across alternative specifications of the running variable and the bandwidth the effect size remains in the same vicinity, with coefficients not being statistically distinct from each other but statistically different from 0 at the 95% level. Adding fixed effects to this model has little impact (see column 4).

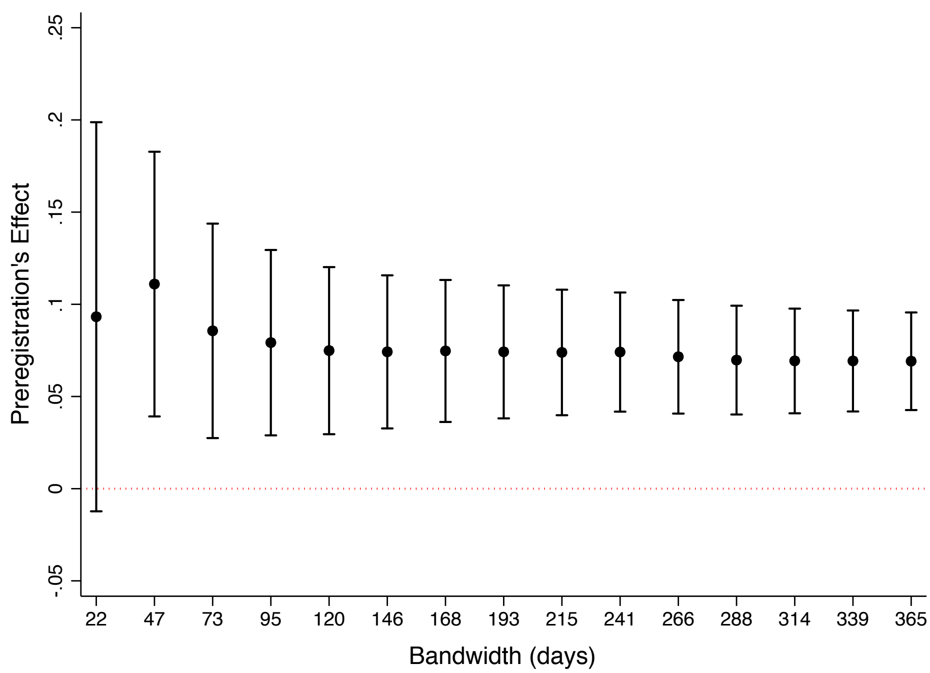
Figure 2: Preregistration's Effect



A local linear regression—shown in Figure 2—offers a stylized illustration of the overall causal effect of preregistration on turnout. Notice in Figure 2 the jump in the plotted line at the eligibility cutoff. This jump represents the causal effect of preregistration on voter turnout, local to the eligibility threshold. Elsewhere on the graph the slope of the smoothed function is relatively flat: generally turnout varies smoothly across birthdays. This suggests that, other than at the eligibility discontinuity, voters born on different days tend to vote at relatively similar levels. If there were a trend in voting over time, or discontinuities at other points, our preregistration effect may be capturing these patterns rather than the true effect of preregistration exposure at the eligibility cutoff. In effect, this serves as an informal placebo test for jumps at points other than the discontinuity.

An informal robustness check of our estimates comes from examining the impact estimates across a variety of bandwidths. Figure 3 shows this visually. On the horizontal axis we plot the bandwidth used to estimate preregistration's coefficient; bandwidth is split evenly on either side of the discontinuity. On the vertical axis, we plot the estimated coefficient showing the effect of preregistration on turnout.

Figure 3: Varying Bandwidths



The results also generally hold across bandwidth of the running variable. Following the decision rules in Imbens and Kalyanaraman (2012) to minimize mean squared error, the optimal bandwidth is 241 days. Estimates with more data support are more precise, but less accurate. Estimates with less data support are more accurate, but less precise, losing significance when we restrict the sample to around 18 days (likely a power issue). Across a range of data support, the effect size remains the same. When we restrict our sample to 12 days (6 on either side of the discontinuity) the effect loses significance at the 95% level ($p \approx .081$). However, the estimated coefficient remains in the same neighborhood as previous estimates (if anything it shows that our effect size may be biased slightly downwards in wider bandwidths). Losing significance at this level is likely a power issue. The consistency across multiple bandwidths is evidence that preregistration's impact is consistent across a variety of sub-samples within our data.

Varying the bandwidth is also a useful robustness check to ensure that small changes in Florida law that affected the eligibility of some voters younger than 17 to preregister are not influencing our results.[26] Prior to 2007, only 17 year olds were eligible to preregister. In a law taking effect in 2007, which is what applies for the vast majority (75%) of our sample, preregistration was available to minors upon turning 17 or upon obtaining a Florida driver's license, whichever occurred first. In effect, 15 and 16 year olds with licenses could preregister as well. This bill became effective on May 21, 2007 (see FL HB 537). From 2008 to the present, the driver's license stipulation was dropped, and the minimum preregistration age was set at 16 (FL SB 866). We can see that when we restrict our results to the sample who were registering under a consistent set of laws, our results do not change. The bandwidths

[26] Most of the individuals in the sample were subject to the 2007 preregistration law when they came of age to preregister (75%). The older end of our sample was exposed the law as it was written before 2007, allowing only 17 year olds to preregister (10%). The younger end were exposed to the 2008 changes, receiving approximately 4 months of preregistration exposure without the precondition of having a drivers' license (15%).

from 0 days to 120 days were exposed only to the 2007 law (as were an additional 100 birthdays just before that bandwidth). Bandwidths from 120 to 332 days include the 2008 law (only for those born later). Bandwidths wider include the pre 2007 law (on those born earlier). As can be seen in Figure 3 our results do not change across these minor variations in the law.

A second robustness check of the impact of minor variations in Florida's preregistration law can be estimated using data from marginal eligibles/ineligibles in 2004, before the law change occurred. This approach has the added virtue of leveraging panel techniques with our regression discontinuity models, absorbing other potential confounders that vary systematically across elections. For example, if there were concerns that parents plan births around the eligibility to vote cutoff, a panel component could adjust for this.[27] If this or any other unobserved time-invariant factor were driving our results then adding a panel adjustments should wash out our result.

To add a panel component to our RD models we use data from individuals born 4 years earlier on the same calendar birthdays. These models add data from the 2008 voter file.[28] These individuals were marginally eligible/ineligible around the 2004 election, and were all able to vote in the 2008 election. In 2004 we see a similar jump in preregistrations at the eligibility cutoff ($\approx 30\%$ increase in preregistration enrollments for marginal ineligibles).[29]

[27] A birthday fixed-effect would account for this as long as this behavior was consistent for a given birthday from one election to the next.

[28] We use the 2008 voter file as it was on election day: November 4, 2008

[29] As additional evidence that preregistration mobilizes, when we use this discontinuity to estimate the effect of preregistration in 2004 on turnout in 2008 we get an ITT of approximately 1% and a TOT of approximately 3%. That these sizes are smaller makes sense given the smaller population allowed to preregister in 2004 relative to 2008 (McDonald 2009).

Thus, the discontinuity we observe in 2008 is not unique to that year, reflective of a broader trend in preregistration rates (see online appendix for a graphical representation). Because preregistration differs across years we don't estimate a comparable TOT. However, we can estimate an ITT effect (the effect of being marginally ineligible, regardless of preregistration take-up). This model is equivalent to combining a difference-in-difference with our regression discontinuity models. In this model the first difference is between marginal ineligibles (nudged toward preregistration) and marginal eligibles (regular registration). The second difference is between those in 2004 vs. 2008, removing any time-invariant potential biases. Equation (5) displays the form of this regression discontinuity, difference-in-difference model.

$$Y_{i,\alpha} = \gamma_0 + \gamma_1 I_{i,\lambda} + \gamma_2 2008_i + \gamma_3(2008_i * I_{i,\lambda}) + \gamma_4 R_{i,\lambda} + \epsilon \tag{5}$$

In equation (5) the outcome remains whether or not the individual turned out to vote in the first subsequent presidential election ($Y_{i,\alpha}$). For those who were marginally eligible/ineligible in 2004 we considered their turnout in 2008 ($\alpha = 2008$). The model includes and indicator for marginal ineligibility ($I_{i,\lambda}$) and proximity to ineligibility ($R_{i,\lambda}$) for those both in 2004 and 2008 ($\lambda = 2004$ or $\lambda = 2008$). Also included is an indicator for whether or not the individual was a marginal eligible/ineligible in 2004 or 2008 (2008_i). The variable of interest in equation (5) is the interaction between eligibility in 2008 (when preregistration was possible) and being ineligible to register. The coefficient on this interaction term (γ_3) shows the ITT effect of being offered preregistration holding constant things that remained fixed on a given birthday over time. This combined RD/difference-in-difference offers a powerful antidote for possible omitted variables involved with birthday discontinuities (Jacob and Lefgren 2004).

The results for this robustness check are reported in column (3) of table 4. This model produces a similar result to that in column (1), giving powerful evidence that our results are not driven by unobserved time-invariant factors. Being offered preregistration in 2004 increases turnout by approximately 2-3% in 2008. Finally, we estimate the TOT model with county fixed effects to account for unobserved variation that is constant over time. For example, if our effect were thought to be an artifact of variation in mobilization efforts across counties, we would expect our TOT effects to disappear. It does not.

To summarize then, across a variety of model specifications our results consistently show a modest increase in the probability of voting for those exogenous nudged towards preregistering. This result holds regardless of how the running variable is specified, what controls are included, or what bandwidth is used. In addition, this effect is not due to time-invariant differences across counties or nuances of turnout on different days of the calendar. Our result is a result of varying levels preregistration exposure at the eligibility to vote cutoff.

Having examined a diverse set of alternative explanations, we now turn to the potential confounder that does simultaneously vary at the eligibility cutoff, namely the exposure to a "habit for voting" treatment.

Addressing Concerns about Voting Habit

Our results show that those who were marginally *ineligible* are more likely to vote in subsequent elections than marginal eligibles. This result may seem counterintuitive for a couple of reasons. First, this group is slightly younger, with the treatment group 1 to 365 days (mean=181 days) younger than our control group. More importantly, marginally eligible individuals had the opportunity to vote in the 2008 election, whereas marginally ineligibles did not. Given the habitual nature of voting we might expect that the control group, with more voting experience on average, would vote at higher rates in the subsequent election than the treatment group (Meredith 2009; Plutzer 2002; Fowler 2006).[30]

An ideal control group would have comparable levels of "habit for voting" with the treatment group. However, this is not possible as our identification strategy specifically utilizes the eligibility discontinuity in order to draw causal inferences. And no other useful discontinuities exist: exogenous jumps in preregistration are not abundant.[31]

However, we are able to identify the likely sign of bias from imbalanced habit. This potential source of bias (along with the age difference) means that we likely underestimate our treatment effect. If there is a habitual turnout boost that comes from voting in a election, those who received the "habit treatment" should have a higher likelihood of voting on average. Because only our control group received this treatment, our treatment effect is likely biased downwards.

As a test of this hypothesis we use as a control group individuals who were marginally

[30] It should be noted that our results do not conflict with the notion of habitual forces. That literature comes predominately from states without preregistration laws.

[31] Two other cutoffs seem appealing. First, cutoffs on the young end of the preregistration (comparing those who were eligible to preregister in an election vs not) are confounded by preregistrations in subsequent elections. Second, a discontinuity does exist at the May 21, 1990 cutoff (corresponding with those effected by the 2007 law change vs. not). However, the law change appeared to be so minor that treatment at this margin is relatively weak (This cutoff allowed preregistration among 15-16 year olds. Most preregister when they are 17), making it unusable for our question of interest.

eligible but *did not vote in 2008*. By not participating these individuals did not receive the "habit treatment." To be clear, voting in the 2008 election is not randomly assigned and there is little doubt that those who did not vote in 2008 are inherently different from those who voted.

Table 5 displays the results from this rough test, replicating these models alongside our original results. The first and third columns replicate estimates from Table 4, the effect of preregistration among marginal ineligibles compared to marginal eligibles. The second and fourth columns display similar models, only using individuals who did not receive the "habit treatment."

Table 5: Mitigating Bias from Habitual Voting

	(1) ITT: All	(2) ITT: Not Vote '08	(3) TOT: All	(4) TOT: Not Vote '08
Preregistration	0.03*	0.14*	0.08*	0.35*
	(0.01)	(0.01)	(0.03)	(0.03)
Controls?	Yes	Yes	Yes	Yes
Constant	0.048	0.003	0.083	0.197
	(0.54)	(0.03)	(0.93)	(1.86)
N	36,790	26,466	36,790	26,466

Robust standard errors in parentheses. $* \ p < 0.05$. Estimates are based off a 2 month window, and a linear specification of the running variable. Controls: race (individual and county), party, gender, proportion of population that graduated high school (county), poverty (county), voter turnout (county), median age (county), and an indicator for the Democratic presidential party having a campaign office in the county. *Coefficients for the controls are reported in the online appendix.*

The results indicate that preregistration's ITT when fully purged from habitual voting is somewhere between 3 and 14%. For *compliers*, preregistration's mobilizing effect likely is somewhere between 8% and 35%. The coefficients in models (1) - (2) and (3) - (4) are statistically and substantively different from each other. These models provide evidence that our treatment effect may be underestimated by the habitual nature of voting.

Partisan Heterogeneities?

In previous sections we have attempted to address *whether* preregistration mobilizes young voters. Our evidence suggest that it does, and noticeably so. However, also of substantive interest is *who* among young voters are mobilized by preregistration. As mentioned in the introduction, there has been speculation that preregistration disproportionately benefits Democratic candidates. To address this we ran the model separately, stratifying on party. The model run was the same as the model run to create Figure 2. From this model we use estimates from the "optimal" bandwidth from Imbens and Kalyanaraman (2012), though the results generalize to other bandwidths.

The models indicate that the effects of preregistration are quite similar for Democrats and Republicans. The stratified RD models indicated that among Democrats, preregistration increases turnout by about 7.6 points. For Republicans this estimate is similar, indicating a 7.4 point gain. These effects are not statistically different from one another. Thus, both Democrats and Republicans exogenously nudged towards preregistration are similarly mobilized above those brought in by regular registration.[32]

Democrats benefit more in the absolute number of votes from preregistration than Republicans simply because there are more registered young Democrats than young Republicans.[33]

[32]The model from half the optimal bandwidth indicates an 11 point bump for Democrats and a 6.1 point bump for Republicans, with the difference not being statistically different. The model from twice the optimal bandwidth indicates a 7.1 point gain for Democrats and a 9.2 point gain for Republicans, with again the different not being statistically distinct.

[33]In Florida 18-year olds register primarily as Democrats ($\approx$ 42%) over Republicans ($\approx$ 25%).

But, in contrast to popular assumptions, our results suggest that preregistration actually helped Republicans to slightly narrow the Democratic advantage among young people. A back of the envelope calculation illustrates this. Using the estimates from the "optimal" bandwidth model, we calculate that for every 10 Democrats mobilized by preregistration 6 Republicans were mobilized.[34] Put another way, approximately 37% of partisan voters mobilized by preregistration in 2008 were likely to vote Republican in 2012.[35] In comparison, only 32% of all voters age 18-29 in Florida voted Republican in 2012. Based on this cursory analysis, it appears that preregistration actually helps Republicans slightly narrow the Democratic advantage among young voters. This pattern is quite consistent with previous studies, which find that institutional programs tend to *register* more Democrats (Herron and Smith 2012) but *mobilize* more Republicans (Cain and McCue 1985; Burden and Neiheisel 2013). Thus, contrary to conventional wisdom, preregistration laws actually help Republicans slightly narrow the traditionally large Democratic advantage among young voters.

Discussion

Previous work has cast doubt on the ability of institutional reforms to increase turnout (Erikson 1981; Highton 1997; Martinez and Hill 1999; Berinsky, Burns and Traugott 2001; Ansolabehere and Konisky 2006; Keele and Minozzi 2013; Burden and Neiheisel 2013) with some evidence that poorly designed reforms can actually depress turnout (Burden et al.

[34] Voters Mobilized By Preregistration=(% Mobilized * # in Base) - (% crossover * % Mobilized * # in Base). For Democrats: (.076 * 78,270) - (.09 * .076 * 78,270) $\approx$ 5400. For Republicans: (.074 * 46,753) - (.08 * .074 * 46,753) $\approx$ 3200. % crossover is drawn from exit polls conducted by Edison Media Research. If crossover is assumed to be 0, a similar result holds.

[35] Alternative bandwidths indicated that this split is 38% (half optimal bandwidth) or 43% (twice the optimal bandwidth.

2014). In a noticeable departure, we find that preregistration laws are effective at increasing turnout among young voters. Using panel techniques and the ITT estimates from our RD models we find that preregistration increases young voter turnout out by 2-12.5%. Those who receive preregistration are 8% more likely to vote than comparable individuals who use more traditional means of registering. However, these estimates are likely conservative, biased downward by the habitual nature of voting. In addition, the estimates of preregistration's effectiveness are similar for Republicans and Democrats.

In considering why preregistration reforms might be effective at increasing turnout where other electoral reforms have failed, we speculated that a number of different factors could be in play. Unlike other institutional reforms, preregistration specifically targets individuals at the right time in their life cycle: focusing directly on voters coming of age. Preregistration might also piggy-back on complementary institutions, such as a high school civics curriculums and voter registration drives.

While recent research highlights the point that electoral reforms can't make individuals interested in politics, perhaps what electoral reformers need to recognize is that such interest is not a stable character trait, but rather one that varies depending on the political context. Preregistration laws may be effective because they lower the obstacles to voting and bring individuals into the political system when motivation and interest in politics are high. This means, of course, that preregistration is not a solve-all, as it would suggest that those who come of age outside a campaign year are not exposed to this mobilizing force. Indeed, a basic comparison of preregistration rates across the 2008 and 2004 election cycles finds sizable spikes around elections, with many fewer enrollments during other periods (see online appendix). Nevertheless, preregistration appears to be an effective electoral reform to boost turnout for some young voters. These observed effects should be of interest not only to policy-makers as they consider the potential impact of electoral reforms, but also to scholars who might find possible lines of future research that marry the literature on campaign effects and electoral institutions.

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