Differential Registration Bias in Voter File Data: A Sensitivity Analysis Approach*

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Abstract

The widespread availability of voter files has improved the study of participation in American politics, but the lack of comprehensive data on non-registrants creates difficult inferential issues. Most notably, observational studies that examine turnout rates among registrants often implicitly condition on registration, a post-treatment variable that can induce bias if the treatment of interest also affects the likelihood of registration. We introduce a sensitivity analysis to assess the potential bias induced by this problem, which we call differential registration bias. Our approach is most helpful for studies that estimate turnout among registrants using post-treatment registration data, but is also valuable for studies that estimate turnout among the voting-eligible population using secondary sources. We illustrate our approach with two studies of voting eligibility effects on subsequent turnout among young voters. In both cases, eligibility appears to decrease turnout, but these effects are found to be highly sensitive to differential registration bias.

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The widespread availability of digital voter files has changed the study of voting behavior in American politics. These files offer data on a vast population of U.S. citizens while avoiding the social desirability bias and low statistical power that plague survey studies of self-reported turnout, enabling new studies of how factors as disparate as majority-minority districts (Barreto, Segura and Woods 2004), minority candidates (Barreto 2007; Fraga 2016*a*), genetic similarity (Fowler, Baker and Dawes 2008), and early voting registration (Holbein and Hillygus 2015) affect turnout at the individual or aggregate level. These data have been found to be of high quality, especially when cleaned and aggregated by firms like Catalist (Ansolabehere and Hersh 2012; Hersh 2015).

However, greater attention is needed to the limitations of these data sources. Ideally, the denominator for turnout studies should be the voting-eligible population (VEP). Unfortunately, a lack of precise data on the VEP makes turnout estimates vulnerable to estimation error (e.g., McDonald and Popkin 2001). In the U.S., common choices to approximate the VEP are the voting-age population (VAP) or the citizen voting-age population (CVAP), but these measures are imperfect approximations: both include ineligible populations such as disenfranchised felons; the VAP includes noncitizens; and the CVAP is estimated based on surveys—not census counts—and is unavailable for the smallest census geographies.

As a result, researchers often estimate turnout effects using voter registration files, which can take at least two different forms. In what we call *pre-treatment registrant studies*, scholars study a subset of citizens to whom a treatment of interest is assigned (or not) after they have registered to vote. These studies can be either experimental or non-experimental. For example, studies of get-out-the-vote (GOTV) campaigns typically start with a list of registered voters, randomly assign each citizen in the list to be encouraged to vote (or not), and use future voter files to measure subsequent turnout (e.g., Gerber, Green and Larimer 2008; Citrin, Green and Levy 2014). Other studies identify the period when a non-experimental intervention or treatment is introduced, collect registration files from a period before treatment, and look at the effect of the treatment on the subpopulation of registrants identified before the treatment (e.g., Barber and Imai 2014; Enos 2016; Fraga 2016*b*). The common feature of this type of studies is that the registration decisions that determine the study population occur *before* the treatment is assigned.[1]

In contrast, in what we call *post-treatment registrant studies*, researchers are typically interested in the effect of a non-experimental treatment on voter turnout (or partisan registration) and use voter registration files as the source of outcome data without limiting the sample to registrants prior to treatment. In these studies, the treatment of interest may affect both the likelihood of registration and the likelihood of turning out to vote (or the likelihood of choosing to register as a partisan). For example, the presence of a minority candidate on the ballot could lead to higher minority registration as well as higher minority turnout, which will bias estimates of the effect of co-ethnic candidates on turnout that are calculated among registrants.

Although both types of studies rely on registration files, they differ crucially in their study populations. Pre-treatment registrant studies consider an initial group of registrants that is defined and observed before the treatment is assigned. In contrast, post-treatment registrant studies consider treatments that could affect both registration and voter turnout decisions while relying on post-treatment registration files. In the latter studies, the decision to register may be a consequence of the treatment itself, creating the potential for a form of post-treatment bias (Rosenbaum 1984) sometimes known as "endogenous selection bias" (Elwert and Winship 2014). If a treatment affects registration rates as well as voter turnout (the outcome of interest), a comparison of turnout rates between the treated and control groups among registered voters could lead to mistaken inferences.

We formally show the threat that conditioning on the population of post-treatment registrants poses in studies where the treatment of interest affects the likelihood of registration, characterizing the bias that stems from differential registration in treated and control groups. We also develop a novel sensitivity analysis that makes it possible for scholars to assess the robustness of their treatment effect estimates to (unobserved) potential differences in registration rates between treatment and control groups.

Our survey of the literature showed that most post-treatment registrant studies adopt one of two strategies. Some studies, which are typically cross-sectional, simply use the registration file as the universe of analysis and calculate turnout or partisanship rates as the proportion of voters

[1]This characterization of *pre-treatment registrant studies* implicitly assumes that all subjects registered pre-treatment remain registered or at least that the treatment has no effect on the probability of remaining registered. We do not pursue this issue further, but note that the sensitivity analysis we introduce could be appropriately modified to asses the robustness of results in cases where this assumption is violated.

or partisans among registrants (e.g., Barreto, Segura and Woods 2004; Barreto 2007; Fraga and Merseth 2016; Hersh 2013; Hersh and Nall 2015). Other studies rely on voter files to calculate the numerator of interest (number of voters or partisans), but calculate turnout or partisanship rates using a measure of the potential electorate obtained from a secondary source as the denominator (Meredith 2009; Fraga 2016*a*; Cepaluni and Hidalgo 2016).

The sensitivity approach we propose is useful in both cases. When the only data available is a post-treatment cross-section of registered citizens, estimated treatment effects on turnout or partisanship can be severely biased by differential registration between treatment and control groups. Our approach offers a concrete measure of the robustness of treatment effects that are estimated in this way. Our method is also useful for analyzing the robustness of estimates in which a secondary source is used to calculate the denominator of turnout (or partisanship) rates. In these cases, our approach can determine whether differential registration that would change the study's conclusion is within the plausible margin of error of the measure used to approximate the VEP.

We illustrate the challenge of differential registration and our sensitivity approach with two studies of political socialization, a research area that has recently begun to use quasi-experimental techniques to estimate the effect of initial election eligibility on subsequent voter turnout and other behaviors (e.g., Meredith 2009; Mullainathan and Washington 2009; Dinas 2012, 2014; Coppock and Green 2015; Holbein and Hillygus 2015). These studies typically compare voter turnout and political attitudes among individuals whose 18^{th} birthday fell close to a previous general election. We use a similar research design that compares turnout rates between young citizens who were narrowly eligible or ineligible to vote in a prior election.

Our first study is an original analysis of a sample of registrants from 42 U.S. states and the District of Columbia who were born within four days of the election eligibility cutoff—a far narrower window than previous studies. Our findings illustrate the need to consider the potential bias induced by conditioning on registration. When we naïvely use registered voters in the voter file as the denominator for our turnout analysis, we find results that largely contradict previous studies—initial election eligibility appears to sometimes *reduce* subsequent turnout or have no effect. However, we show that this result is highly sensitive to differing registration rates between the two groups. Consistent with this sensitivity analysis, when we instead use birth totals as the denominator, we find that initial eligibility *increases* subsequent turnout. This application shows that differential registration between treatment and control groups can severely bias non-experimental turnout studies when turnout rates are calculated as a proportion of total registration.

Our second study is a re-analysis of the Florida voter file findings in Holbein and Hillygus (2015), where we again examine the effect of voting eligibility on subsequent turnout. Like the first study, eligibility is found to negatively affect turnout in later elections among young voters when turnout rates are calculated among registered voters. However, a sensitivity analysis of the data indicates that even a slightly higher registration rate in the treatment group would reverse these results. When we use birth counts to approximate the total population, we find that the registration rate is higher in the control group than in the treatment group in the window around the eligibility cutoff originally used by Holbein and Hillygus, but this finding is reversed when a larger window is considered. Our analysis suggests that the negative effects they report for the Florida voter file are potentially sensitive to differential registration and could be consistent with true null or even positive effects. This case illustrates the usefulness of our sensitivity method in cases where approximations of the voting eligible population are imperfect.

These results suggest that scholars who study turnout based on voter files should complement their analysis with a rigorous sensitivity analysis—even small differences in registration rates between treatment and control groups can reverse the conclusions of turnout studies. Our goal is not to claim that past or current studies based on voter files are incorrect or misleading, but instead to help raise awareness of this difficult inferential issue and provide researchers with a simple approach to assess the robustness of their findings.

Defining and assessing differential registration bias

We begin by illustrating the problem of differential registration bias in Table 1 with a hypothetical example of an election in which 10,000 voting-eligible citizens are in the treatment group and 10,000 voting-eligible citizens are in the control group. In addition, the treatment is associated with higher rates of registration—5,000 individuals in the treatment group register compared with 4,000 in the control group (column 2)—but has no effect on turnout (columns 3 and 5). If we simply compare turnout rates among registered voters using voter file data (column 4), we would conclude that turnout rates are *lower* among treated voters (2500/5000=50% versus 2500/4000=62.5% among controls). However, the true turnout rate of 25% is identical in both groups (column 5).

[Table 1 about here.]

This problem would of course be avoided if we could calculate turnout rates using the total voting-eligible population in each group rather than the total number of *registered voters*. When possible, the simplest solution to the problem of differential registration is to obtain the missing population totals from alternative data sources. However, as we discussed above, the necessary data is typically either unavailable or imperfectly approximated.

A formal sensitivity analysis approach

We formalize a sensitivity analysis approach that can be implemented in cases where the needed quantities—the total eligible population in the treatment and control groups—are unknown or imprecisely estimated. Specifically, our approach identifies the differential in registration rates between the treatment and control groups that would reduce the observed difference in turnout rates among registered voters to zero. This method provides a measure of the vulnerability of a treatment effect estimate to differential registration bias when population totals are missing. It can also be used to corroborate findings when population totals from alternative data sources are likely to be estimated with error, which is a pervasive phenomenon because precise counts of the voting-eligible population are not directly available (McDonald and Popkin 2001). Moreover, as we discuss in the conclusion and elaborate in the Supplemental Information, our approach could also be extended to other types of data that share the characteristics of turnout data (unknown population totals and values of missing data known with certainty).

In the example in Table 1, we assumed that all variables were known. We now assume that total eligible population counts for the treatment and control groups are not available and that researchers are working with a voter file that includes only registered voters.[2] This scenario is illustrated in Table 2, which presents the total number of registrants (column 2) and voters (column

[2]For simplicity, we assume the entire registration file is available. However, our argument applies directly to the case where only a random sample from the voter file is available (as in Study 1 below).

3) for each group, which allows us to calculate turnout rates among registrants by group (column 4). However, letting the subscripts $\mathtt{T}$ and $\mathtt{C}$ denote the treatment and control groups, respectively, the total eligible population counts in each group, which we denote by $P_{\mathtt{T}}$ and $P_{\mathtt{C}}$, respectively, are not available. As a consequence, the true turnout rates—the ratio of voters to the total eligible population in each group—are also unavailable, creating the risk of a mistaken inference if differential registration bias is present.

[Table 2 about here.]

We introduce some additional notation. We let $R_{\mathtt{T}}$ and $R_{\mathtt{C}}$ denote the total registration counts in the treatment and control groups, respectively, and $V_{\mathtt{T}}$ and $V_{\mathtt{C}}$ denote the total numbers of voters who turned out to vote in each group. The desired—but unavailable—turnout rates are thus

$$T_{\mathtt{T}}^{\mathtt{Pop}} = \frac{V_{\mathtt{T}}}{P_{\mathtt{T}}} \quad \text{and} \quad T_{\mathtt{C}}^{\mathtt{Pop}} = \frac{V_{\mathtt{C}}}{P_{\mathtt{C}}},$$

where the superscript $\mathtt{Pop}$ denotes that these turnout rates are calculated as a proportion of the total (eligible) population. Henceforth, we refer to $T_{\mathtt{T}}^{\mathtt{Pop}}$ and $T_{\mathtt{C}}^{\mathtt{Pop}}$ as the *turnout-to-population* rates or simply the *true turnout* rates. Without observing the eligible population counts $P_{\mathtt{T}}$ and $P_{\mathtt{C}}$, we cannot calculate these turnout-to-population rates directly.

We define the registration rates $r_{\mathtt{T}} = R_{\mathtt{T}}/P_{\mathtt{T}}$ and $r_{\mathtt{C}} = R_{\mathtt{C}}/P_{\mathtt{C}}$ in the treatment and control groups, respectively. Given these rates, the total registration counts, $R_{\mathtt{T}}$ and $R_{\mathtt{C}}$, can be expressed as $R_{\mathtt{T}} = r_{\mathtt{T}} \times P_{\mathtt{T}}$ and $R_{\mathtt{C}} = r_{\mathtt{C}} \times P_{\mathtt{C}}$, and we can express the unknown total eligible population as total registration divided by the probability of registration:

$$P_{\mathtt{T}} = R_{\mathtt{T}}/r_{\mathtt{T}} \quad \text{and} \quad P_{\mathtt{C}} = R_{\mathtt{C}}/r_{\mathtt{C}}.$$

We then define the *turnout-to-registration* rate in each group, $T_{\mathtt{T}}^{\mathtt{Reg}}$ and $T_{\mathtt{C}}^{\mathtt{Reg}}$, as follows:

$$T_{\mathtt{T}}^{\mathtt{Reg}} = \frac{V_{\mathtt{T}}}{R_{\mathtt{T}}} \quad \text{and} \quad T_{\mathtt{C}}^{\mathtt{Reg}} = \frac{V_{\mathtt{C}}}{R_{\mathtt{C}}}.$$

Given these definitions, we can express the desired turnout-to-population rates as

$$T_{\mathtt{T}}^{\mathtt{Pop}} = \frac{V_{\mathtt{T}}}{P_{\mathtt{T}}} = \frac{V_{\mathtt{T}}}{R_{\mathtt{T}}/r_{\mathtt{T}}} = T_{\mathtt{T}}^{\mathtt{Reg}} \times r_{\mathtt{T}} \quad \text{and} \quad T_{\mathtt{C}}^{\mathtt{Pop}} = \frac{V_{\mathtt{C}}}{P_{\mathtt{C}}} = \frac{V_{\mathtt{C}}}{R_{\mathtt{C}}/r_{\mathtt{C}}} = T_{\mathtt{C}}^{\mathtt{Reg}} \times r_{\mathtt{C}}. \tag{1}$$

In words, the true turnout rate is the turnout-to-registration rate adjusted (multiplied) by the registration rate in each group. In applications where researchers have access to the voter file but not the total population, the registration rates $r_{\mathtt{T}}$ and $r_{\mathtt{C}}$ are unknown. Our sensitivity analysis considers how large the difference between $r_{\mathtt{T}}$ and $r_{\mathtt{C}}$ would have to be to generate the observed difference in turnout-to-registration rates when the true difference in turnout-to-population rates is zero.

Imagine that $T_{\mathtt{T}}^{\mathtt{Reg}} < T_{\mathtt{C}}^{\mathtt{Reg}}$, which means that the turnout-to-registration rate is smaller in the treatment than in the control group (as in the two applications we consider below). Because the registration rates for the two groups $r_{\mathtt{T}}$ and $r_{\mathtt{C}}$ are unknown, $T_{\mathtt{T}}^{\mathtt{Reg}} < T_{\mathtt{C}}^{\mathtt{Reg}}$ is not enough to conclude that $T_{\mathtt{T}}^{\mathtt{Pop}} < T_{\mathtt{C}}^{\mathtt{Pop}}$.[3] In other words, a lower (higher) turnout-to-registration rate in the treatment group does not necessarily imply that this group has a lower (higher) turnout-to-population rate. However, given the observed difference $T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$, we can estimate how different the registration rates would have to be between the treatment and the control groups for this difference to be observed when there is no difference in turnout-to-population rates (i.e., when $T_{\mathtt{T}}^{\mathtt{Pop}} - T_{\mathtt{C}}^{\mathtt{Pop}} = 0$).

We define Δ as the treatment-control difference in true turnout rates:

$$\Delta = T_{\mathtt{T}}^{\mathtt{Pop}} - T_{\mathtt{C}}^{\mathtt{Pop}} = (T_{\mathtt{T}}^{\mathtt{Reg}} \times r_{\mathtt{T}}) - (T_{\mathtt{C}}^{\mathtt{Reg}} \times r_{\mathtt{C}}).$$

We note two points. First, if $r_{\mathtt{T}} = r_{\mathtt{C}} = 1$, the expression simplifies to $\Delta = T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$. In other words, if everyone in the treatment and control groups registers, the ratio of voters to registrants is of course identical to the ratio of voters to total eligible population and there are no complications. Second, if turnout rates are identical between groups but not everyone votes ($r_{\mathtt{T}} = r_{\mathtt{C}} = r \neq 1$), the unknown turnout share difference simplifies to $\Delta = r(T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}})$. Since $0 \leq r \leq 1$, in this case the sign of Δ is equal to the sign of $T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$ and $|\Delta| \leq |T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}|$; moreover, it is straightforward to explore how Δ changes as r varies from 0 to 1.

We are interested in the more general case in which both $r_{\mathtt{T}}$ and $r_{\mathtt{C}}$ are nonzero and $r_{\mathtt{T}} \neq$

[3] Likewise, we cannot conclude that $T_{\mathtt{T}}^{\mathtt{Pop}} > T_{\mathtt{C}}^{\mathtt{Pop}}$ or $T_{\mathtt{C}}^{\mathtt{Pop}} = T_{\mathtt{T}}^{\mathtt{Pop}}$ without knowing $r_{\mathtt{T}}$ and $r_{\mathtt{C}}$.

$r_{\texttt{C}}$. Figure 1 illustrates how a negative "treatment effect" on turnout-to-registration rates can be observed even when the true difference in the turnout-to-population rates is null or even positive. Using the functions defined in Equation (1), the figure plots the turnout-to-population rates ($T_{\texttt{T}}^{\texttt{Pop}}$, $T_{\texttt{C}}^{\texttt{Pop}}$) against the registration rates ($r_{\texttt{T}}$, $r_{\texttt{C}}$) separately for each group, holding turnout-to-registration rates ($T_{\texttt{T}}^{\texttt{Reg}}$, $T_{\texttt{C}}^{\texttt{Reg}}$) fixed. We adopt the scenario in Tables 1 and 2 where $T_{\texttt{T}}^{\texttt{Reg}} = 0.50$ and $T_{\texttt{C}}^{\texttt{Reg}} = 0.625$. Thus, we plot the linear functions $T_{\texttt{T}}^{\texttt{Pop}} = 0.50 \times r_{\texttt{T}}$ and $T_{\texttt{C}}^{\texttt{Pop}} = 0.625 \times r_{\texttt{C}}$. Since the slope in the control group (0.625) is higher than the slope in the treatment group (0.50), the dotted line (control) is always above the solid line (treatment). In other words, Figure 1 fixes $T_{\texttt{C}}^{\texttt{Reg}} = 0.50$ and $T_{\texttt{C}}^{\texttt{Reg}} = 0.625$, which means that the turnout-to-registration difference is always negative (-0.125).

[Figure 1 about here.]

Imagine first that the probability of registration in the treatment group is 0.8 ($r_{\texttt{T}} = 0.8$) and the probability of registration in the control group is 0.4 ($r_{\texttt{C}} = 0.4$). In this case, the true turnout rate in the treatment group ($T_{\texttt{T}}^{\texttt{Pop}}$) of 0.4 is obtained from the y-coordinate of point A on the solid line in Figure 1(a), and the true turnout rate of 0.25 in the control group ($T_{\texttt{C}}^{\texttt{Pop}}$) is obtained from the y-coordinate of point B on the dashed line. Under this scenario, $\Delta = T_{\texttt{T}}^{\texttt{Pop}} - T_{\texttt{C}}^{\texttt{Pop}} = 0.4 - 0.25 = 0.15$. In other words, the treatment effect on the true turnout-to-population rate is positive despite the turnout-to-registration rate being lower in the treatment than in the control group. Figure 1(b) shows a different scenario in which $r_{\texttt{T}} = 0.8$ and $r_{\texttt{C}} = 0.64$. In this case, both $T_{\texttt{T}}^{\texttt{Pop}}$ and $T_{\texttt{C}}^{\texttt{Pop}}$ are equal to 0.4 and thus $\Delta = 0.4 - 0.4 = 0$, though the difference in turnout-to-registration rates is still negative (again, treatment slope is 0.5 and control slope is 0.625).

We propose a sensitivity analysis to explore how inferences are affected by differential registration. First, we define the *differential registration factor* k as the ratio of the registration rate in the treatment group to the registration rate in the control group:

$$k = \frac{r_{\texttt{T}}}{r_{\texttt{C}}}$$

We assume that both $r_{\texttt{T}}$ and $r_{\texttt{C}}$ are nonzero. Moreover, because $r_{\texttt{T}}$ and $r_{\texttt{C}}$ are rates or probabilities, they are both less than (or at most equal to) 1, which means that $k \in (0, \infty)$. Furthermore, since $r_{\texttt{T}}/k = r_{\texttt{C}}$ and $r_{\texttt{C}}$ is a rate, k must satisfy the restriction $0 < r_{\texttt{T}}/k \leq 1$; that is, the smallest value that k can take is $r_{\mathtt{T}}$.[4] Our sensitivity analysis explores, for a given treatment group registration rate $r_{\mathtt{T}}$, how large the differential registration factor k can be before the implied difference in turnout-to-population rates is zero or has the opposite sign from $T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$, the observed difference in turnout-to-registration rates.

Given our definition of k, we can express Δ, the treatment-control difference in true turnout rates, as a function of the treatment group registration rate $r_{\mathtt{T}}$ and the ratio of treatment/control registration rates k:

$$\Delta(r_{\mathtt{T}}, k) = (T_{\mathtt{T}}^{\mathtt{Reg}} \times r_{\mathtt{T}}) - (T_{\mathtt{C}}^{\mathtt{Reg}} \times \frac{r_{\mathtt{T}}}{k}) = r_{\mathtt{T}}(T_{\mathtt{T}}^{\mathtt{Reg}} - \frac{T_{\mathtt{C}}^{\mathtt{Reg}}}{k})$$

where we make the arguments k and $r_{\mathtt{T}}$ explicit. Thus, for any nonzero value of $r_{\mathtt{T}}$, we can calculate the value of k under which a zero difference in true turnout rates between the treatment and control groups would result in the observed difference in turnout-to-registration rates. Since we assume $r_{\mathtt{T}} > 0$, we find this value, which we call $k^\star$, as the solution to $(T_{\mathtt{T}}^{\mathtt{Reg}} - \frac{T_{\mathtt{C}}^{\mathtt{Reg}}}{k^\star}) = 0$, leading to

$$k^\star = \frac{T_{\mathtt{C}}^{\mathtt{Reg}}}{T_{\mathtt{T}}^{\mathtt{Reg}}}.$$

By definition, $\Delta(r_{\mathtt{T}}, k^\star) = 0$ for any $0 < r_{\mathtt{T}} \leq 1$.

We can now explore how the true turnout difference Δ varies with observed turnout-to-registration rates under different assumptions about registration rates in the treatment and control groups ($r_{\mathtt{T}}$ and $r_{\mathtt{C}}$). In particular, we can calculate $k^\star$, the pattern of differential registration that would be required to produce the observed difference in turnout-to-registration rates if there were no difference in turnout-to-population rates.

We again illustrate the procedure with the hypothetical example presented in Tables 1 and 2. In that example, $T_{\mathtt{T}}^{\mathtt{Reg}} = 2500/5000 = 0.5$ and $T_{\mathtt{C}}^{\mathtt{Reg}} = 2500/4000 = 0.625$, so the difference in turnout rates among registered voters is negative ($T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}} = -0.125$). In this case, $k^\star = 0.625/0.50 = 1.25$, which means that the observed difference in turnout-to-registration rates could occur under a zero (or positive) difference in true turnout rates if the probability of registration were (more than) 25% higher in the treatment than in the control group.

[4] In other words, values of $k \in [0, r_{\mathtt{T}})$ are not allowed because they would imply $r_{\mathtt{C}} \geq 1$.

Figure 2 visualizes how the true turnout difference can vary with k—the ratio of the treatment/control registration rates—for a given difference in turnout-to-registration rates. We plot $\Delta(r_{\mathtt{T}}, k)$ as a function of $r_{\mathtt{T}}$ for differing values of k (which implicitly fixes $r_{\mathtt{C}}$). The y-axis is the difference in turnout-to-population rates between the treatment and control groups and the x-axis is the registration rate in the treatment group. As illustrated by the $k^\star$ curve, when $k = k^\star = 1.25$, the difference in true turnout rates is zero for every value of $r_{\mathtt{T}}$. In contrast, when the registration rate is equal in both groups and thus $k = 1$, the observed difference in turnout-to-registration rates ($T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}} = -0.125$) is equal to the true difference in turnout rates if $r_{\mathtt{T}} = 1$.

[Figure 2 about here.]

We can explore sensitivity further in our hypothetical example by calculating the turnout rate difference Δ for values of the registration ratio k above and below the threshold $k^\star$. For $k > k^\star = 1.25$, the difference in true turnout is positive and the sign of the turnout-to-registration rate difference is reversed. For $1 \leq k < k^\star = 1.25$, the true turnout effect is negative but smaller in absolute value than the difference in turnout-to-registration rates for all $r_{\mathtt{T}}$. Finally, for $k < 1$, the true turnout effect is negative and can be larger in absolute value than the difference in turnout-to-registration rates for a high enough $r_{\mathtt{T}}$.

Incorporating prior knowledge of differential registration

The above approach allows researchers to assess a worst-case scenario in which differential registration bias produces the observed difference in turnout-to-registration rates when the true turnout-to-population effect is zero. However, in some applications, the value taken by $k^\star$ may not be plausible or informative. We now describe a variant of this approach in which scholars can incorporate prior knowledge about plausible variation in registration rates in assessing the conditions under which their results will hold.

Although the registration rates $r_{\mathtt{T}}$ and $r_{\mathtt{C}}$ are unknown, scholars often have prior information about the range of plausible values they can take. Imagine that we use a survey estimate of the registration rate in the overall U.S. population as a guess for $r_{\mathtt{T}}$, the true registration rate in the treatment group. We call this guess $\tilde{r}_{\mathtt{T}}$. Our concern is that $r_{\mathtt{T}}$ differs from $r_{\mathtt{C}}$—i.e., that the treatment has an effect on registration. In most cases, researchers will be able to offer some prior knowledge about how large the differential registration effect is likely to be and rule out extreme values of $k = r_{\mathtt{T}}/r_{\mathtt{C}}$. Imagine that our guess for the differential registration factor is $\tilde{k}$. We can use $\tilde{r}_{\mathtt{T}}$ and $\tilde{k}$ to calculate the guessed treatment-control difference in true turnout rates, $\tilde{\Delta} = \tilde{r}_{\mathtt{T}}(T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}/\tilde{k})$. If $\tilde{\Delta}$ is of the same sign as $T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$, we can conclude the that observed difference in turnout-to-registration rates is robust to a plausible scenario of differential registration rates based on prior knowledge (which might be less stringent than the worst-case scenario represented by $k^{\star}$).

Consider our example above in which $T_{\mathtt{T}}^{\mathtt{Reg}} = 0.50$ and $T_{\mathtt{C}}^{\mathtt{Reg}} = 0.625$. Let us assume that our guess for $\tilde{r}_{\mathtt{T}}$ is 0.59, the rate of registration in the overall U.S. population estimated by the Current Population Survey in November 2014. Imagine that, based on prior knowledge, we believe that the treatment of interest is unlikely to increase the registration rate in the treatment group by more than ten percentage points relative to the control group. Since $\tilde{r}_{\mathtt{T}} = 0.59$, our guess for $r_{\mathtt{C}}$ is about 0.49. This yields $\tilde{k} = 0.59/0.49 = 1.20$, which is less than $k^{\star} = 1.25$ and is therefore consistent with a true negative small effect ($\tilde{\Delta} = 0.59 \cdot (0.50 - 0.625/1.20) = -0.012$). In this way, our simple sensitivity analysis approach can also be used to estimate whether an effect is robust to a particular differential rate of registration chosen using prior knowledge.

Application: The effects of election eligibility on subsequent turnout

We now illustrate the problem of differential registration bias and our approach with two empirical studies of political socialization that focus on the relationship between voting eligibility and subsequent voter turnout. In Study 1, we present an original analysis of voter file data from 42 states. In Study 2, we replicate results from a Florida voter file study in a recent article by Holbein and Hillygus (2015).

Research in political socialization has found long-lasting effects of early experiences and events like parent socialization (e.g., Jennings, Stoker and Bowers 2009) and draft status during the Vietnam War (Erikson and Stoker 2011). The most common and important socializing events for many people as they approach or enter adulthood are elections—the time when politics is most salient in national life. Sears and Valentino (1997), for instance, find that presidential elections appear to be especially potent in forming the political views of adolescents.

These topics are the focus of an emerging literature that studies the effects of initial election eligibility on voter turnout and other political behaviors using a quasi-experimental approach based on voting-age eligibility rules (e.g., Coppock and Green 2015; Dinas 2012, 2014; Holbein and Hillygus 2015; Meredith 2009; Mullainathan and Washington 2009). By comparing later turnout and political attitudes among voters whose 18^{th} birthday fell very close to a general election, these studies seek to leverage as-if random variation in birth timing to compare individuals who had the opportunity to take part in an election and those who did not but are assumed to be otherwise identical. This research strategy is an application of a regression discontinuity (RD) design, which we review below. We note, however, that these applications are only illustrations; our approach is general and can be used in all turnout studies based on registration files, not just RD designs.

Studying eligibility effects with a regression discontinuity design

The defining feature of a (sharp) RD design is that subjects are assigned a score and receive treatment if their score exceeds a known cutoff—and do not receive it otherwise. In the U.S., a discontinuity in voting eligibility occurs when citizens turn eighteen years of age. As a result of the 26th Amendment to the U.S. Constitution (adopted in 1971), people who turn eighteen on or before election day can cast a vote but those who will turn eighteen after election day are ineligible to vote. Thus, date of birth exactly determines voting eligibility and an RD design can be used to study the effects of eligibility on turnout.

An important feature distinguishing RD designs based on date of birth from most uses of RD is that the score that determines treatment, birthdate, is a discrete variable, which invalidates most identification and estimation results in the RD literature. To address this issue, we adopt the framework in Cattaneo, Frandsen and Titiunik (2015), which analyzes the RD design as a local randomized experiment in a fixed window around the cutoff and does not require a continuous running variable.[5] In our context, this randomization-based RD approach entails assuming that voting eligibility is as-if randomly assigned for people with birthdays near election day. Since the number

[5] See Cattaneo, Titiunik and Vazquez-Bare (2016) for a comparison of this randomization-based RD approach to the more standard continuity-based approach.

of observations in our applications is large, we do not use the randomization inference methods discussed in Cattaneo, Frandsen and Titiunik (2015). All our inferences are based in large-sample approximations.[6]

In order to adopt this local experiment framework, we must focus on individuals who are born close in time. Thus, in Study 1, we focus our analysis on individuals who turn eighteen within eight days of election day and assume that eligibility can be considered as-if randomly assigned between those individuals born on election day or the three days earlier (the treatment group) and those born one to four days later (the control group). In Study 2, we use a wider window around election day to ensure comparability with the approach used in Holbein and Hillygus (2015).

Both studies estimate the effects of voting eligibility on subsequent turnout using voter file data and are thus vulnerable to differential registration bias: just-eligibles could be more likely to be registered than just-ineligibles due to the longer period in which they could participate in the political process or be mobilized by campaigns.

Study 1: Voter eligibility effects in Catalist data

Our first study is an original application that investigates the effects of voting eligibility on subsequent turnout with an RD design based only on the closest observations to the election day cutoff. Specifically, we examine three cohorts who were narrowly (in)eligible to vote in the 2004, 2006, and 2008 elections, considering only those registrants born within just four days of the election eligibility cutoff—a far narrower window than previous studies, which have used windows measured in months (Meredith 2009; Dinas 2012, 2014; Holbein and Hillygus 2015) or years (Mullainathan and Washington 2009; Coppock and Green 2015).

Our data are drawn from voter files in 42 U.S. states and the District of Columbia and include eligibility variation and turnout data from several national elections. Our data source is voter registration files that were collected, cleaned, and supplemented by the private company Catalist.[7] We

[6]Specifically, we construct confidence intervals for the difference-in-means between just-eligible and just-ineligibles based on Wald tests. We use t-tests for our turnout-to-registration analysis and employ difference of proportions tests (Newcombe 1998) when we consider turnout as proportion of births.

[7]Colorado, Massachusetts, New Jersey, Oklahoma, South Carolina, Vermont, and Washington were excluded due to school entry cutoff dates that overlapped with the election eligibility window, creating potential discontinuous differences in education levels. Illinois was excluded due to legal restrictions on state voter file use.

collected a random sample of voters in the Catalist file born in the eight days around the cutoff date for being eligible to vote (i.e., for being 18 years old on or before election day) in the 2004, 2006, and 2008 elections.[8]

The three cohorts of individuals in our data were born in 1986, 1988, and 1990, respectively. For example, the 1990 cohort treatment group was born from November 1–4 and were thus eighteen years old and eligible to vote on November 4, 2008, while the control group was born from November 5–8, 1990. Unfortunately, Catalist's data on unregistered voters are sparse and unreliable, which forces us to focus—like other analysts—on the universe of registrants and thereby introduces the possibility of differential registration bias. The final dataset includes a total of 49,271 observations in our target windows among the three birth cohorts.[9]

Effects of eligibility on turnout-to-registration rates

Table 3 explains how we present our findings. We compare the behavior of the treatment group of just-eligibles—those who were born just before the election eligibility cutoff—with the control group of just-ineligible voters born just after the cutoff in later elections. The election in the year the cohort turned 18 is denoted E1 and subsequent elections are denoted E2, E3, and E4. For instance, E1 for the 1986 cohort is the 2004 election and the 2006, 2008, and 2010 elections are E2, E3, and E4, respectively, for that cohort.

[Table 3 about here.]

We analyze voting eligibility effects on subsequent turnout-to-registration rates in Table 4, which compares just-eligible and just-ineligible voters who were born in the week surrounding the eligibility cutoff.[10] These findings initially seem to contradict findings that eligibility increases subsequent turnout (e.g., Meredith 2009; Dinas 2012; Coppock and Green 2015). While we find a significant positive effect of eligibility on turnout-to-registration rates for the 1986 cohort in the 2006 election, the estimated effect is *negative* and significant for the 1986 cohort in the 2008 and

[8] See Supporting Information for more details on birthdates in the Catalist data.

[9] We drop all observations missing exact birthdates, those with birthdates outside the target range, and those recorded as voting in elections for which they should have been ineligible given their reported birthdate. See the Supporting Information for details on the number of excluded observations.

[10] Balance tests are reported in the Supporting Information.

2010 elections, the 1988 cohort in the 2008 and 2010 elections, and the 1990 cohort in the 2010 election.

Specifically, registered voters who were born in 1986 and were just eligible to vote in 2004 were significantly more likely to turn out in 2006 than those who were just ineligible. The estimated effect is 2.12 percentage points (95% CI: 1.07, 3.17), which is a substantial increase relative to the low baseline turnout rate for young voters in midterm elections (though relatively modest in absolute terms). However, this effect reverses by the second and third subsequent elections—just-eligible voters born in 1986 were significantly less likely to vote in 2008 and 2010 than their just-ineligible counterparts among the registered voters in our data. We find a similar negative relationship between eligibility and subsequent turnout-to-registration rates for just-eligible registered voters born in 1988 in 2008 and 2010 and for for just-eligible registered voters born in 1990 cohort in 2010. (RD plots illustrating these estimates are included in the Supporting Information.)

[Table 4 about here.]

Sensitivity analysis: Assessing differential registration scenarios

We now conduct a sensitivity analysis, which is presented in Table 5. Again, the key term is $k^{\star}$—the ratio of registration between the treatment and control groups that would produce the observed difference in turnout-to-registration rates under identical turnout-to-population rates. Values of $k^{\star}$ close to 1 indicate high sensitivity to differential registration.

These results indicate that the positive effect we observed for turnout-to-registration rates in 2006 among the 1986 cohort appears to be robust. The estimated value of $k^{\star}$ is 0.87, which means that just-eligibles would have to register at a lower rate than just-ineligibles to explain the result if the true effect on turnout-to-population rates was zero. In the absence of pre-registration laws, it is plausible to assume that just-eligible voters are more likely to register than just-ineligible voters.

By contrast, the other estimated values of $k^{\star}$ suggest that the negative effects of eligibility on subsequent turnout-to-registration rates in Table 5 are highly sensitive to differential registration. The corresponding $k^{\star}$ values range from 1.03 to 1.15, which means that only slight registration differentials in the expected direction (i.e., $r_{\mathtt{T}} > r_{\mathtt{C}}$) could produce the observed negative turnout-to-registration effects. If the registration differentials were larger than $k^{\star}$, the effects on turnout-to-population rates would be positive.

[Table 5 about here.]

Assessing $k^{\star}$ using external data

The values of $k^{\star}$ reported above indicate that relatively small differences in registration rates between the treatment and control groups could explain the observed negative results. We now use birth totals as a proxy for the voting-elegible population (VEP) to briefly explore whether differences of these magnitudes are plausible in this application. Though it is not possible to definitively resolve the issue of whether differential registration exists without true VEP data, we present our best estimates of the values that k could plausibly take.

We calculate daily birth totals within the eight-day window around election day in the 1986 and 1988 cohorts for our sample of 42 states and the District of Columbia using data from *Vital Statistics of the United States*. Exact birth dates were redacted from these data starting in 1989, preventing us from constructing similar estimates for the 1990 cohort. We thus estimate daily birth totals for our sample states by scaling total U.S. births for each birthdate in our window from the 1990 edition of *Vital Statistics* by the proportion of the population living in those states at the time.[11]

Using these data, we divide the total number of registrants in the treatment and control groups by birth totals, producing approximate estimates of $r_{\mathtt{T}}$ and $r_{\mathtt{C}}$. These figures are *not* valid estimates of registration rates because our data are a random sample from Catalist's voter file and do not include every voter registered on the dates in question in our sample states. However, the *difference* between these estimated registration rates is a valid estimate of differential registration bias in our window around election day due to the use of random sampling in our 8-day window (though of course birth counts are only a proxy for the VEP so even this difference is estimated with error).

We report the estimated treated and control registration rates in Table 6 as well as estimates of the differential registration factor $\hat{k}$. The registration rate as a proportion of births is much higher in the treatment than in the control group in each row (all $p < .01$). These differences are greatest for the 1990 cohort possibly because just-ineligibles have had less time to "catch up" to just-eligibles, but persist even among the 1986 cohort seven years after turning 18.

[Table 6 about here.]

Most notably, our estimates of the differential registration factor $\hat{k}$ are well within the range that the sensitivity analysis in Table 5 suggests could explain our negative turnout-to-registration results. For the 1986 cohort, $\hat{k}$ is 1.09 and the values of $k^{\star}$ that could explain the negative turnout-to-registration estimates in E3 and E4 are, respectively, 1.04 and 1.14. Likewise, $\hat{k}$ is 1.05 for the 1988 cohort and the E2 and E3 values of $k^{\star}$ are, respectively, 1.03 and 1.10. Finally, the $\hat{k}$ value of 1.56 for the 1990 cohort greatly exceeds the 1.15 estimate of $k^{\star}$ for E2.[12]

Another way to look at these findings is to perform a second RD analysis comparing turnout rates between just-eligible and just-ineligible voters using birth totals rather than registrants as the denominator. The results of this analysis are shown in Table 7 (corresponding RD plots are provided in the Supporting Information). When we use birth totals in the denominator, the results are

[11] The proportion of the U.S. population living in the states in our sample was stable during this period so we did not further adjust these estimates to account for interstate migration.

[12] Because we observe registration only in 2011, our estimate $\hat{k}$ is constant within each birth cohort.

largely the opposite of what we found when we conditioned on registration (significantly positive for E2 and E3 for 1986 cohort and E2 for 1990 cohort, and null in the other cases).[13]

The reversal of the negative effects on turnout-to-registration rates in Table 7 is the result by differences in birth counts between groups. Figure 3 illustrates the phenomenon using data for the 1986 cohort. Even though the total registration and vote counts are similar between groups, birth counts are higher in the control group, considerably reducing the turnout-to-population rates relative to the treatment group. This phenomenon is consistent with the well-known pattern of day-level variation in birth rates. As we show in the Supporting Information, the treatment windows of four days in the 1986, 1988, and 1990 cohorts all include two weekend days, when birth rates are typically lower in the U.S., while the control windows include only weekdays. These findings underscore the sensitivity of these results to VEP approximations.

[Table 7 about here.]

[Figure 3 about here.]

Study 2: Preregistration effects in Florida

Our second study is based on the recent work by Holbein and Hillygus (2015), who investigate the effects of preregistration on future turnout among young people. Preregistration laws typically allow voting-ineligible 16-year-old or 17-year-old citizens to complete a registration application so that they are automatically added to the registration rolls once they turn eighteen and become eligible to vote. The authors present analyses of both cross-state data from the Current Population Survey and the Florida voter file. In each case, they find evidence that the availability of preregistration has a positive effect on young people's subsequent turnout, *increasing* the probability that people who are narrowly ineligible will vote in future elections.

We focus exclusively on Holbein and Hillygus's (2015) second analysis, which compares voter turnout among narrowly eligible and narrowly ineligible Florida voters who were born in 1990 close to the voting-eligibility cutoff for the 2008 presidential election. Holbein and Hillygus (2015)

[13] As we show in the Supporting Information, our results are unchanged when we exclude states with preregistration.

use this design to estimate the effects of preregistration. In Florida, where preregistration is allowed, narrowly ineligible voters are exposed to the opportunity to preregister, while most of those who are narrowly eligible to vote register "regularly" (i.e., when they are already eighteen). They conceptualize narrowly ineligible voters as the treatment group and narrowly eligible voters as the control group; ineligibility is an instrument for preregistration, which is the treatment of interest. Their analysis is based on a fuzzy RD design where ineligibility induces preregistration.

Our re-analysis of Holbein and Hillygus' Florida results, which uses the comprehensive replication materials they generously provided, differs from their original study in important ways. We are primarily interested in illustrating how differential registration patterns between treatment and control groups can affect turnout studies that calculate turnout rates as a proportion of registration. For this reason, we re-analyze the Florida data using a sharp regression discontinuity design where, as in our Study 1, the treatment of interest is voting eligibility (as opposed to preregistration), narrowly eligible voters are the treatment group, and narrowly ineligible voters are the control group. Our design is thus analogous to the intent-to-treat (ITT) analysis that they report in the article except that the treatment and control group labels are inverted.

Effects of eligibility on turnout-to-registration rates

We first estimate the effect of voting eligibility on future turnout-to-registration rates and then conduct a sensitivity analysis to determine if the results could be driven by differential registration bias. For our main analysis, we subset the Florida data to people born from October 4–December 4, 1990 to match the Holbein and Hillygus (2015) window of approximately one month on either side of election day. Within this window, we treat the assignment of voting eligibility in 2008 as locally random and compare the turnout-to-registration rate in 2012 between just-eligibles and just-ineligibles. We also consider a larger window of two months on either side of the cutoff.

Table 8 reports the results for both windows. In the one-month window, our estimated treatment effect on turnout-to-registration rates is -2.80 percentage points, meaning that just-ineligible registrants who were exposed to the option to preregister in 2008 voted at a higher rate in 2012 (51.88%) than registrants who in 2008 were just-eligible (49.08%). This estimate is very close to the three percentage-point effect that Holbein and Hillygus (2015) report for their ITT estimate.[14] In the larger window of four months surrounding the cutoff, we find a similar pattern, with just-ineligibles again being slightly more likely to turn out than just-eligibles as a proportion of registrants. Both negative effects are significantly different from zero at 5% level.

[Table 8 about here.]

Sensitivity analysis: Assessing differential registration scenarios

Our goal is to establish the robustness of the above finding to potential differential registration. As shown in the last column of Table 8, our sensitivity analysis gives a $k^\star$ value of 1.06 for the one-month window used in Holbein and Hillygus (2015), which means that if the rate of registration were six percent higher in the treatment than in the control group, the negative turnout-to-registration effect we observe would reflect a null turnout-to-population effect (and any difference greater than six percent would switch the effect from negative to positive). In the two-month window, the $k^\star$ value is 1.05, which indicates that the sensitivity of the results is slightly greater.

In 2008, the nationwide percentage of 18-year-olds who reported being registered to vote was approximately 49% (Herman and Forbes 2010). Assuming that the registration rate among just-eligible (treated) Florida voters within the window we consider was also 49%, a $k^\star$ value of 1.06 implies that the rate of registration among just-ineligible (control) voters would have to be 46.23% or lower to change the sign of the point estimate—a difference of just 2.77 percentage points. We therefore conclude that, as in Study 1, the negative eligibility effect found in Florida is potentially sensitive to *positive* differential registration—i.e., to a situation where the registration rate is higher in the treatment than in the control group.[15]

Assessing $k^\star$ empirically using external data

As in Study 1, we now use births in 1990 to provide approximate estimates of the rate of differential registration. Since the CDC's National Vital Statistics do not contain birth rates disaggregated by

[14] The difference is likely due to the fact that, unlike Holbein and Hillygus (2015), our analysis reports a simple difference in means and does not include controls.

[15] The main focus of Holbein and Hillygus (2015) is not the ITT, but rather the TOT effect. This quantity is the ITT effect divided by the rate of preregistration in the just-ineligible group (preregistration is zero by construction among eligibles). A change in the sign of the ITT effect would thus result in a change in sign in the TOT effect as well.

state and exact date in 1990 or later years, we cannot use an exact one-month window around the cutoff date of November 4, 1990. Instead, we approximate the correct birth totals using the October totals for the treatment group and the November totals for the control group. Again, this analysis is intended to provide approximate evidence about plausible values that k can take in this application.

In Table 9, we compare the results using both registered voters and births as the denominator in the two windows, reproducing the effects reported in Table 8 for easy comparability. In the one-month window, the effect remains negative when we calculate turnout based on approximate births in the window. Indeed, the point estimate is very similar to the effect based on registration: we estimate that 2012 turnout among Floridians who were narrowly eligible in 2008 was -2.67 percentage points lower than their just-ineligible counterparts.

This finding changes when we consider a larger window of two months on either side of the cutoff. Table 9 shows that the turnout-to-registration effect estimate in this window continues to resemble the one found by Holbein and Hillygus (2015). However, when we calculate the estimated turnout-to-births rate in this window, the direction of the effect flips and the 2012 turnout rate among just-eligibles is found to be higher than among just-ineligibles. Since narrower windows generally reduce bias in RD designs, we are cautious in our interpretation of these results; but we present them because we believe they illustrate the challenges involved in obtaining accurate VEP estimates.

[Table 9 about here.]

This result can be explained by the fact that the number of births in Florida during November 1990 was significantly lower than in October. Except for November, total monthly births in Florida between July and December 1990 were above 17,000. The total number of births in November 1990, however, was only 16,289. Thus, although the number of people who voted in 2012 was very similar in the treatment and the control groups (7,888 and 7,734, respectively), the control group's approximate size was apparently much smaller. We show in the Supporting Information that there is significant seasonal variation in monthly births in the U.S., which again highlights the importance of accurately estimating the relevant VEP and the necessity of assessing the sensitivity of a study's conclusions to departures from accurate estimation of the VEP. (Moreover, birth data do not account for migration to and from Florida during the 1990–2008 period, which means that birth counts are an inherently imprecise measure of VEP.)

Given the difficulty of correctly approximating the VEP, the results for both windows in Table 9 should be taken with caution. We therefore place more confidence in the sensitivity analysis of the Florida-specific findings in Holbein and Hillygus (2015) reported in Table 8, which concludes that the negative effects of eligibility on turnout-to-registration are sensitive to modest differences in registration rates between the treatment and control groups in the expected direction (i.e., higher in the just-eligible group).

Conclusion

Research using voter file data is more common than ever in American politics. Frequently, scholars use voter files to investigate the effect of a non-experimental treatment on turnout (or partisan registration). However, the voting-eligible population (VEP), which is necessary to calculate the turnout effects of the treatment of interest, is unavailable. Some researchers simply use the population of registrants as the universe of analysis, an approach that implicitly conditions on voter registration and risks what we call differential registration bias, potentially distorting estimates of how a treatment affects turnout rates. Others choose to study only the subset of the population that was registered before the intervention of interest occurred or instead approximate the VEP from secondary sources.

We study formally the problem of differential registration and develop a new approach to sensitivity analysis that allows scholars to assess how robust their estimates are to potential differences in registration rates between treatment and control groups. Our approach is most helpful when only a cross-section of registered citizens is available, but is also useful as a complementary analysis when only a subset of the registered population is studied before and after the treatment or when a secondary source is used as denominator.

We illustrate the use of these methods with two studies of turnout based on voter file data—an original analysis of voter file data from 42 U.S. states and a reanalysis of the Florida voter file study in Holbein and Hillygus (2015). In both cases, we show that comparisons of turnout-to-registration rates are sensitive to differential registration bias and would reverse if the registration rate were moderately higher in the treatment group than the control group.

These findings illustrate the complex tradeoffs associated with changes in the type of data used in studies of voter turnout and political behavior more generally. Until recently, turnout studies often relied on survey data, which suffers from two primary limitations: measurement error due to self-reporting, especially overreporting (e.g., Ansolabehere and Hersh 2012; Fraga 2016*a*), and missing data due to nonresponse, which may lead to overrepresentation of individuals who are more knowledgeable and interested in politics (e.g. Couper 1997). As we show, studies that rely on voter files eliminate the overreporting problem but not the inferential challenge posed by missing data, which now consists of citizens who are not registered rather than those who do not take part in surveys.

Thankfully, though, we show that the missing data problem is less severe for voter file data than for surveys because, by construction, all eligible voters who are not registered did not cast a vote. (In contrast, survey nonrespondents may or may not have voted.) Our sensitivity analysis directly incorporates this additional information about nonregistrants, leaving only one unknown variable (the differential registration factor k) to be varied. By contrast, though a similar sensitivity analysis could be used to assess nonresponse bias in surveys, the fact that the outcome of nonrespondents is completely unknown implies that such an analysis would have to be based on more standard partial identification results (e.g. Manski 2003), leading to a much wider range of possible effects and decreasing its appeal for practitioners.

Our study also offers important methodological lessons. The crucial property on which our approach is based—the size of the treatment and control populations are not known but the values of missing outcomes are known with certainty—is most common in studies of voter turnout, but could also be extended to address other important research questions.[16] In addition, our study illustrates that even rigorous designs can be vulnerable to post-treatment bias despite other assumptions being met. Conditioning on variables affected by the treatment of interest leads to conclusions that may be severely misleading. Differential registration bias is only one way in which failing to take this threat into account can lead us to mistaken inferences about how politics works.

[16] See the Supporting Information for examples of how our approach could be used in two quite different contexts: studies of racial bias in voting rights and whether militarized disputes between countries escalate into war.

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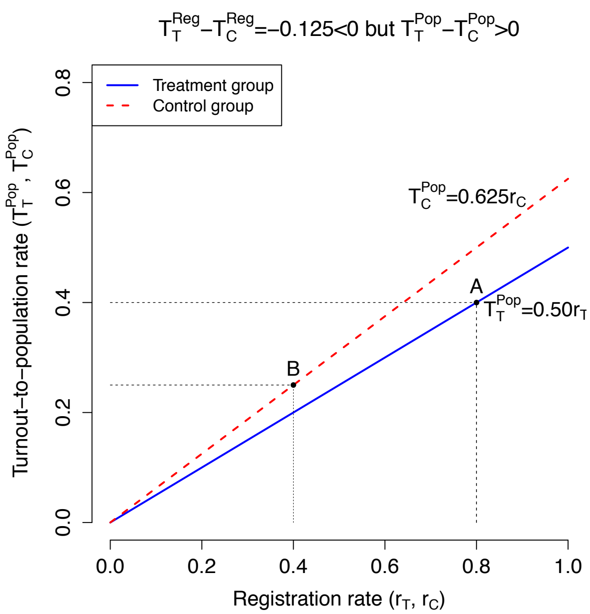
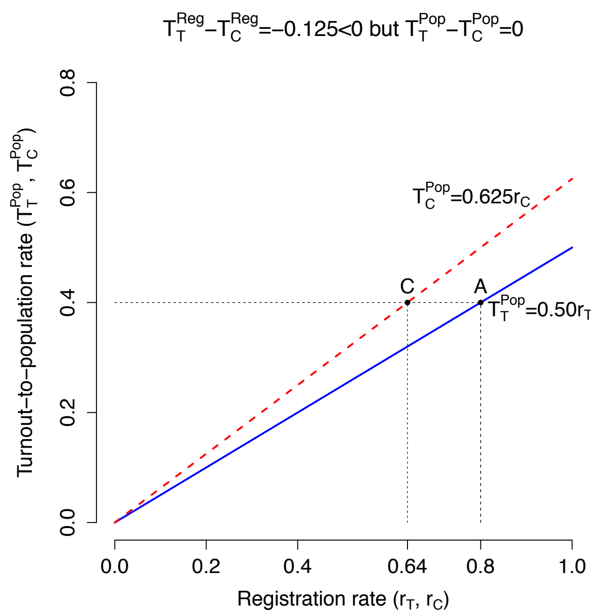
Figure 1: True turnout rates as a function of registration rate



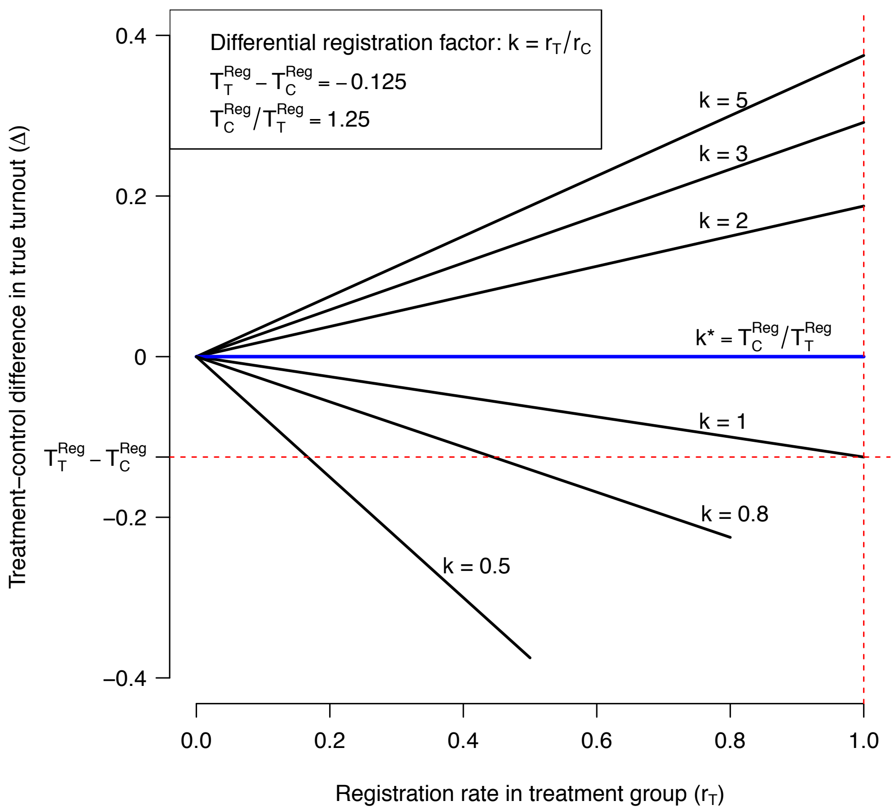
(a) Scenario 1: $r_{\mathtt{T}} = 0.8$ and $r_{\mathtt{C}} = 0.4$



(b) Scenario 2: $r_{\mathtt{T}} = 0.8$ and $r_{\mathtt{C}} = 0.64$

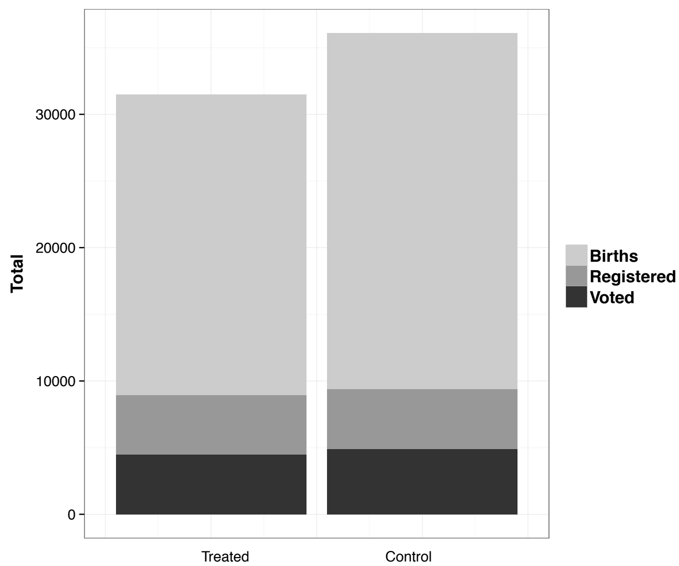
Note: We assume $T_{\mathtt{T}}^{\mathtt{Reg}} = 0.50$ and $T_{\mathtt{C}}^{\mathtt{Reg}} = 0.625$ in both scenarios.

Figure 2: Difference in true turnout rates as function of k and r_{T}



Note: We assume $T_{\mathrm{T}}^{\mathrm{Reg}} = 0.50$ and $T_{\mathrm{C}}^{\mathrm{Reg}} = 0.625$.

Figure 3: Total population, registration, and voters for 1986 cohort



Note: Our data are a random sample from Catalist's voter file and therefore underestimate the turnout-to-population rates for both the treatment and control groups. However, because the data were drawn randomly, we can accurately estimate the difference in turnout-to-population rates between groups. Voting is measured in the 2008 election; registration is measured in 2011.

Table 1: Hypothetical example of differential registration bias

	Total (1)	Registered (2)	Voted (3)	Turnout (% reg.) (4)	Turnout (% pop.) (5)
Treated	10,000	5,000	2,500	50%	25%
Control	10,000	4,000	2,500	62.5%	25%

Table 2: Hypothetical illustration of sensitivity analysis

	Overall population				
	Total	Registered	Voted	Turnout (% reg.)	Turnout (% pop.)
	(1)	(2)	(3)	(4)	(5)
Treated	$P_{\texttt{T}}$	5,000	2,500	50%	2,500/$P_{\texttt{T}}$
Control	$P_{\texttt{C}}$	4,000	2,500	62.5%	2,500/$P_{\texttt{C}}$

Table 3: Birth years and election years in 2011 Catalist data

Year	E1	E2	E3	E4
1986	2004	2006	2008	2010
1988	2006	2008	2010	-
1990	2008	2010	-	-

Table 4: Turnout-to-registration rates by voting eligibility

	A. 1986 cohort (first election for just-eligibles: 2004 presidential)		
	E2 (2006 midterm)	E3 (2008 presidential)	E4 (2010 midterm)
Eligibility effect	2.12	-2.16	-2.56
	[1.07, 3.17]	[-3.61, -0.72]	[-3.70, -1.42]
Control group	14.59	52.18	20.37
	B. 1988 cohort (first election for just-eligibles: 2006 midterm)		
	E2 (2008 presidential)	E3 (2010 midterm)	E4 (2012 presidential)
Eligibility effect	-1.50	-1.91	-
	[-2.99 , -0.01]	[-3.08, -0.73]	-
Control group	55.51	20.11	
	C. 1990 cohort (first election for just-eligibles: 2008 presidential)		
	E2 (2010 midterm)	E3 (2012 presidential)	E4 (2014 midterm)
Eligibility effect	-3.06	-	-
	[-4.45, -1.66]	-	-
Control group	23.10	-	-

2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792). Brackets show 95% confidence intervals based on a differences-in-means Wald test.

Table 5: Sensitivity analysis

A. 1986 cohort (first election for just-eligibles: 2004 presidential)					
E2 (2006 midterm)		E3 (2008 presidential)		E4 (2010 midterm)	
$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$	$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$	$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$
2.12	0.87	-2.16	1.04	-2.56	1.14
B. 1988 cohort (first election for just-eligibles: 2006 midterm)					
E2 (2008 presidential)		E3 (2010 midterm)		E4 (2012 presidential)	
$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$	$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$	$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$
-1.50	1.03	-1.91	1.10	-	-
C. 1990 cohort (first election for just-eligibles: 2008 presidential)					
E2 (2010 midterm)		E3 (2012 presidential)		E4 (2014 midterm)	
$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$	$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$	$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$
-3.06	1.15	-	-	-	-

2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792).

Table 6: Registration rates as of 2011 by voting eligibility as proportion of births

				Sensitivity ($k^{\star}$)		
Year	Treated	Control	$\hat{k}$	E2	E3	E4
1986	28.42	25.99	1.09	0.87	1.04	1.14
1988	24.47	23.41	1.05	1.03	1.10	-
1990	22.59	14.48	1.56	1.15	-	-

Table 7: Turnout rates by voting eligibility as a proportion of births

	A. 1986 cohort (first election for just-eligibles: 2004 presidential)		
	E2 (2006 midterm)	E3 (2008 presidential)	E4 (2010 midterm)
Eligibility effect	0.96	0.65	-0.23
	[0.65, 1.27]	[0.13, 1.18]	[-0.57, 0.10]
Control group	3.79	13.56	5.29
	B. 1988 cohort (first election for just-eligibles: 2006 midterm)		
	E2 (2008 presidential)	E3 (2010 midterm)	E4 (2012 presidential)
Eligibility effect	0.22	-0.25	-
	[-0.28 , 0.72]	[-0.56, 0.06]	-
Control group	12.99	4.71	
	C. 1990 cohort (first election for just-eligibles: 2008 presidential)		
	E2 (2010 midterm)	E3 (2012 presidential)	E4 (2014 midterm)
Eligibility effect	1.18	-	-
	[0.90, 1.47]	-	-
Control group	3.34	-	-

2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792). Brackets show 95% confidence intervals based on a differences-in-proportions Wald test.

Table 8: Eligibility effects on turnout-to-registration—1990 Florida data

Window	$T_{\mathtt{T}}^{\mathtt{Reg}}$ (turnout-to-reg. eligibles)	$T_{\mathtt{C}}^{\mathtt{Reg}}$ (turnout-to-reg. ineligibles)	$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$k^{\star}$
$\pm$ One month	49.08	51.88	-2.80	1.06
			[-3.91, -1.68]	
$\pm$ Two months	49.29	51.84	-2.55	1.05
			[-3.33, -1.77]	

Note: Source is Florida voter file data for 1990 births one month before or after November 4th; sample size = 30,979. Brackets show 95% confidence intervals based on a differences-in-means Wald test.

Table 9: RD estimates of eligibility effect in 1990 Florida cohort

Denominator	$T_{\mathtt{T}}^{\mathtt{Reg}}$ (turnout eligibles)	$T_{\mathtt{C}}^{\mathtt{Reg}}$ (turnout ineligibles)	$T_{\mathtt{T}}^{\mathtt{Reg}} - T_{\mathtt{C}}^{\mathtt{Reg}}$	$\hat{k}$
Window of ± one month				
Registration	49.08	51.88	-2.80 [-3.91, -1.68]	-
Births	44.81	47.48	-2.67 [-3.74, -1.60]	1.11
Window of ± two months				
Registration	49.29	51.84	-2.55 [-3.33, -1.77]	-
Births	48.76	45.41	3.36 [2.61, 4.10]	1.13

Note: Florida voter file data for 1990 births within one/two month(s) of November 4th. One-month window sample size = 30,979. Two-month window sample size = 64,286. Estimated births calculated from Florida vital records for October and November 1990. Brackets show 95% confidence intervals based on a differences-in-proportions Wald test.

Supporting Information for "Differential Registration Bias in Voter File Data: A Sensitivity Analysis Approach"

Brendan Nyhan* Christopher Skovron† Rocío Titiunik‡

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S1 Quality of Catalist data

Ansolabehere and Hersh (2010) use Catalist data to analyze the quality of state voter files and find that "Identifying information such as birthdates are generally well collected." They do identify some problems with missing birth dates and unusual concentrations of voters with particular birth dates but these should not affect the validity of our design.[1] Catalist not only cleans and processes data from state voter files, which includes tracking individuals who move between states and/or are purged from voter files, but fills in exact birth dates from commercial sources when possible for states that only release month of birth, allowing us to use exact birthdates even in states that do not release them.

S2 Excluded Catalist data in Study 1

As mentioned in the paper, we excluded some individuals who were in the raw data provided to us by Catalist. Table S1 describes the observations excluded by category.

Table S1: Observations excluded from analysis in Study 1

Description	Observations
Original dataset	57,031
After dropping missing exact birthdates	54,332
After dropping outside birth targets	51,705
After dropping those recorded as voting when they should have been ineligible	51,472
After dropping those with no registration year listed	49,271

[1]The only unusual date within our window is November 11, which they find to be unusually prevalent in Texas, but we observe no evidence of a problem in our data (results available upon request).

S3 Balance checks for Study 1

We compare the demographic characteristics of just-eligible and just-ineligible voters for Study 1 in Table S2 using covariates in the Catalist data, which combines public and commercial records of gender, marital status, race/ethnicity, and religious affiliation. In the pooled data, the differences in means are small and generally not significant despite the very large sample size.[2] As we show in the paper, though, this seeming balance may mask consequential differences between the two groups in registration patterns.

[2] There are a few imbalances in the 1990 cohort, which may be the result of the shorter interval between the treatment election for this cohort (2008) and the year of data collection (2011).

Table S2: Balance statistics in Catalist data

All	Treatment	Control	p-value
Male	0.464	0.457	0.12
Married	0.095	0.095	0.84
Black	0.160	0.153	0.04
White	0.638	0.645	0.07
Hispanic	0.156	0.158	0.52
Catholic	0.265	0.272	0.09
Protestant	0.275	0.276	0.85
1986	Treatment	Control	p-value
Male	0.460	0.455	0.46
Married	0.126	0.123	0.48
Black	0.156	0.155	0.79
White	0.644	0.647	0.68
Hispanic	0.157	0.154	0.67
Catholic	0.262	0.267	0.50
Protestant	0.283	0.278	0.42
1988	Treatment	Control	p-value
Male	0.472	0.457	0.04
Married	0.085	0.089	0.33
Black	0.161	0.156	0.39
White	0.636	0.645	0.27
Hispanic	0.153	0.153	0.98
Catholic	0.265	0.268	0.67
Protestant	0.274	0.268	0.41
1990	Treatment	Control	p-value
Male	0.461	0.462	0.85
Married	0.070	0.061	0.02
Black	0.164	0.148	0.01
White	0.632	0.645	0.12
Hispanic	0.159	0.173	0.03
Catholic	0.267	0.285	0.02
Protestant	0.269	0.286	0.02

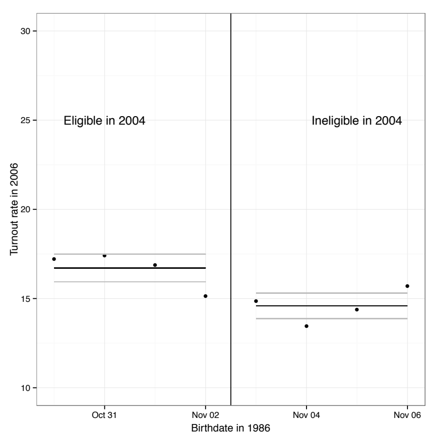
2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792)

S4 RD plots for Study 1

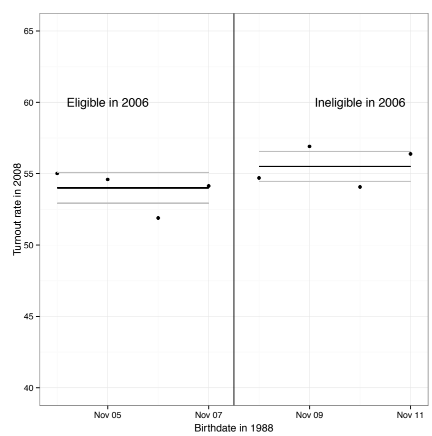
S4.1 Turnout-to-registration rates

As is conventional in RD analyses, we plot raw turnout rates among registrants binned by date of birth. Figure S1 illustrates how turnout varies by eligibility in the election after treatment (which we call E2), while Figure S2 presents corresponding results for the next two elections (E3 and E4).

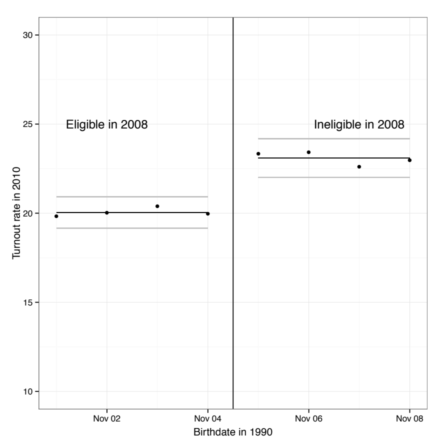
Figure S1: RD effects of voting eligibility on turnout in subsequent election



(a) Turnout effects in 2006 for 1986 cohort (E2)



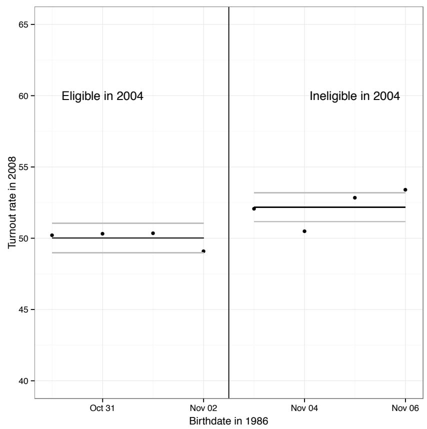
(b) Turnout effects in 2008 for 1988 cohort (E2)



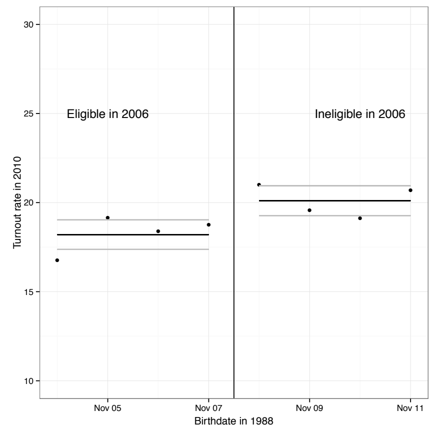
(c) Turnout effects in 2010 for 1990 cohort (E2)

2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792). 95% confidence intervals in parentheses. Lines represent means and 95% confidence intervals for just-eligibles and just-ineligibles.

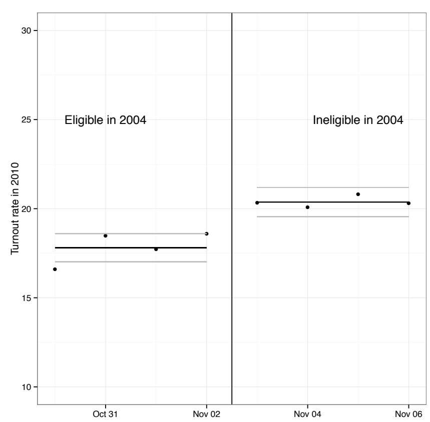
Figure S2: RD effects of voting eligibility on turnout in second and third subsequent elections



(a) Turnout effects in 2008 for 1986 cohort (E3)



(b) Turnout effects in 2010 for 1988 cohort (E3)



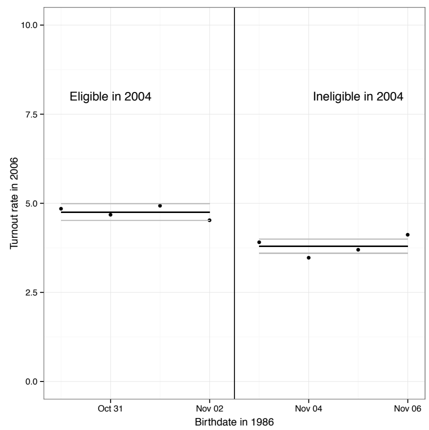
(c) Turnout effects in 2010 for 1986 cohort (E4)

2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792). 95% confidence intervals in parentheses. Lines represent means and 95% confidence intervals for just-eligibles and just-ineligibles.

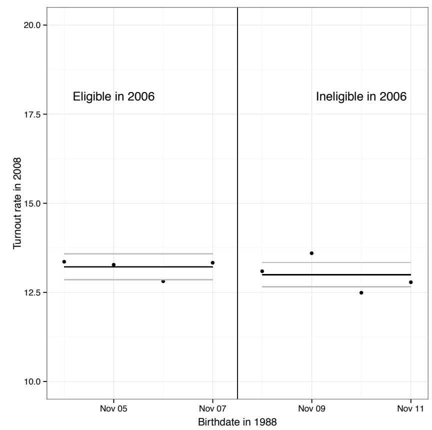
S4.2 Turnout-to-births rates

Figures S3 and S4 plots the raw data for turnout rates by date of birth when adjusted by birth totals rather than the number of registrants in the data.

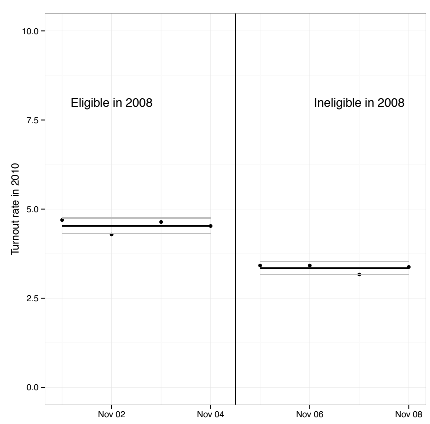
Figure S3: RD estimates of voting eligibility effects on population turnout rates (E2)



(a) Turnout effects in 2006 for 1986 cohort (E2)



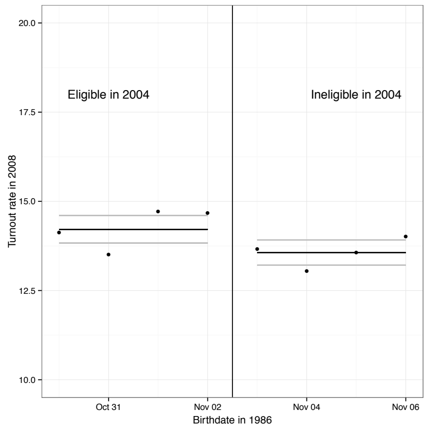
(b) Turnout effects in 2008 for 1988 cohort (E2)



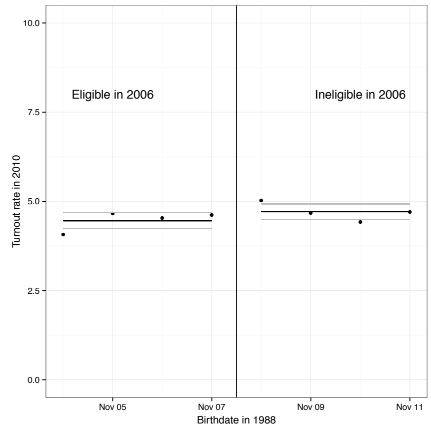
(c) Turnout effects in 2010 for 1990 cohort (E2)

2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792). 95% confidence intervals in parentheses. Lines represent means and 95% confidence intervals for just-eligibles and just-ineligibles.

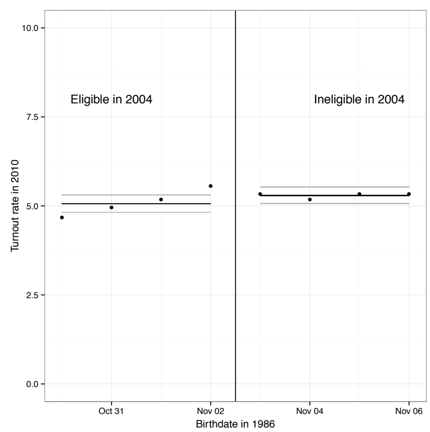
Figure S4: RD estimates of voting eligibility effects on population turnout rates (E3–E4)



(a) Turnout effects in 2008 for 1986 cohort (E3)



(b) Turnout effects in 2010 for 1988 cohort (E3)



(c) Turnout effects in 2010 for 1986 cohort (E4)

2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792). 95% confidence intervals in parentheses. Lines represent means and 95% confidence intervals for just-eligibles and just-ineligibles.

S5 Results for Study 1 in states without preregistration

Holbein and Hillygus (2015) argue that preregistration increases mobilization for just-ineligible voters, who are exposed to the opportunity to preregister. At the time of the elections that we consider in Study 1, only Florida and Hawaii allowed voters to pre-register when they were 17 years old. Table S3 therefore replicates the main turnout-to-birth analysis of Study 1 (Table 7 in the main paper) excluding Florida and Hawaii from both the turnout and birth counts. The results are largely unchanged from the original analysis.

Table S3: Turnout rates by voting eligibility as a proportion of births, excluding preregistration states

	A. 1986 cohort (first election for just-eligibles: 2004 presidential)		
	E2 (2006 midterm)	E3 (2008 presidential)	E4 (2010 midterm)
Eligibility effect	0.89	0.59	-0.28
	[0.57, 1.21]	[0.06, 1.12]	[-0.62, 0.07]
Control group	3.79	12.93	5.16
	B. 1988 cohort (first election for just-eligibles: 2006 midterm)		
	E2 (2008 presidential)	E3 (2010 midterm)	E4 (2012 presidential)
Eligibility effect	0.32	-0.18	-
	[-0.18 , 0.83]	[-0.50, 0.13]	-
Control group	12.38	4.54	
	C. 1990 cohort (first election for just-eligibles: 2008 presidential)		
	E2 (2010 midterm)	E3 (2012 presidential)	E4 (2014 midterm)
Eligibility effect	1.20	-	-
	[0.91, 1.49]	-	-
Control group	3.26	-	-

2011 Catalist data; n = 44,167 (1986: 16,385; 1988: 15,371; 1990: 12,411). Brackets show 95% confidence intervals based on a differences-in-means Wald test.

S6 Birth, registration, and vote totals: Study 1

This table shows the total number of births, registered voters, and votes cast in the first election after the eligibility treatment election.

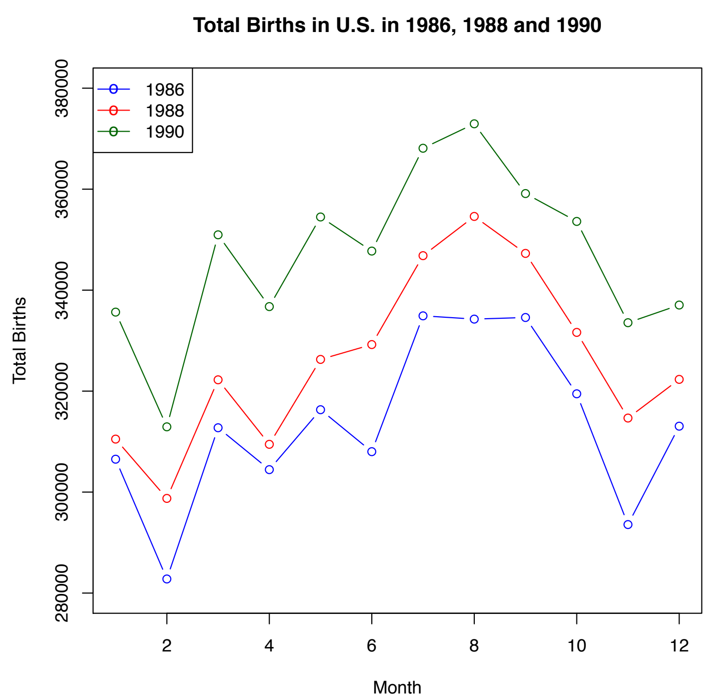
Table S4: Births, registration, and vote totals

	1986 treated	1986 control	1988 treated	1988 control	1990 treated	1990 control
Births	31,476	36,096	34,083	37,646	35,537	39,801
Registration	8,945	9,381	8,340	8813	8,029	5,763
Votes	1,495	1,369	4,504	4,892	1,609	1,331

Registration and vote totals from 2011 Catalist data; n = 49,271 (1986: 18,326; 1988: 17,153; 1990: 13,792).

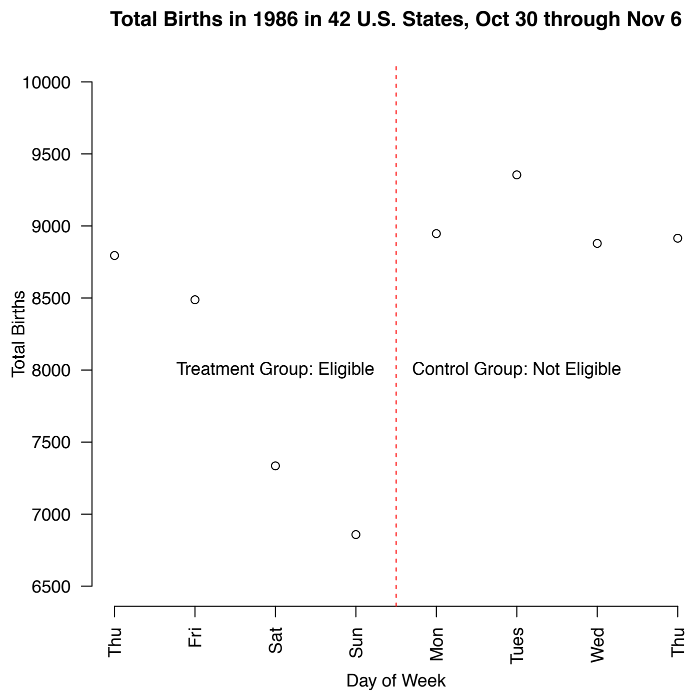
S7 Total births by day and month

Figure S5: Total births by month in the United States

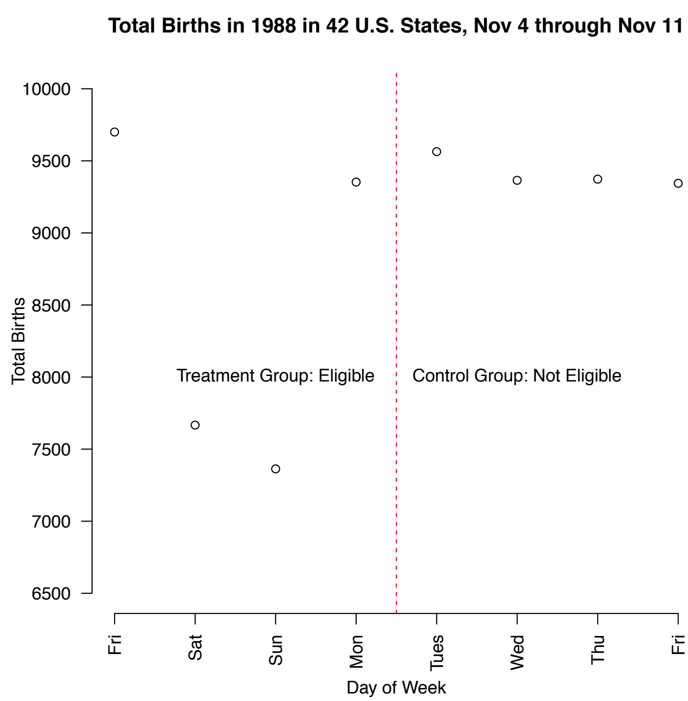


Source: Vital Statistics of the United States for 1986, 1988 and 1990, Volume I, Natality.

Figure S6: Total births by day in 42 U.S. states included in Study 1



(a) Daily birth counts in 1986



(b) Daily birth counts in 1988

Source: Vital Statistics of the United States for 1986 and 1988, Volume I, Natality.

S8 Sensitivity analysis example: Voting rights restorations

The problem of differential registration in studies based on voter files is less severe than many missing data problems because we know that all eligible voters who are not registered did not cast a vote, leaving only the differential registration factor k to be varied. This approach can be applied to other research designs in which the size of the treatment and control populations are not known but outcomes are known with certainty and an intervention could differentially affect the likelihood of a treatment case being observed compared to a control.

Meredith and Morse (N.d.), for instance, consider differences by race in the rate at which voting rights restoration applications by ex-felons in Alabama are denied due to outstanding legal financial obligations (LFOs). In this case, African American ex-felons who are eligible to apply for restoration of their voting rights are the treatment group, and eligible non-African American ex-felons are the control group. All outcomes are observed among individuals who petition to have their voting rights restored—the group that is the equivalent of registered voters in turnout studies. Moreover, the outcome of interest—voting rights—is known to be 0 among those ex-felons who do not apply to have those rights restored. However, the size of the treatment and control group populations are unknown due to limitations on data from the Alabama criminal courts system. The sensitivity analysis approach we propose can be applied in this case to assess how sensitive these results are to potential differences in application rates by race.

Another example comes from the literature on international relations. Many analysts study the likelihood of escalation between states among observed disputes (e.g., Senese 1997). However, this research design neglects how a treatment of interest might also influence the likelihood of dispute initiation among the unknown set of *potential* disputes that could be initiated. One potential approach is to estimate a two-stage selection model (Senese and Vasquez 2003) or a joint model of the likelihood of onset and escalation (Reed 2000), but scholars who prefer to avoid the strong distributional assumptions that these approaches typically require could use our sensitivity analysis approach instead. For instance, Senese (1997) considers the effect of joint democracy (the treatment of interest) on dispute escalation among the set of qualifying observed disputes between states. However, joint democracy might affect the likelihood of a dispute being observed among the universe of potential interstate disputes, producing a form of differential selection bias. Scholars could therefore estimate the sensitivity of an observed difference in escalation rates by dyad regime type to differential selection among the set of potential disputes.[3]

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[3]An alternate approach that is more common in the literature is to consider selection effects among dyad-years where the treatment and control populations can be fully enumerated.