

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

Brendan Nyhan Dartmouth College
Christopher Skovron University of Michigan
Rocío Titiunik University of Michigan

Abstract: *The widespread availability of voter files has improved the study of participation in American politics, but the lack of comprehensive data on nonregistrants creates difficult inferential issues. Most notably, observational studies that examine turnout rates among registrants often implicitly condition on registration, a posttreatment 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 posttreatment registration data, but it 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.*

Replication Materials: Replication materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: <http://dx.doi.org/10.7910/DVN/LCDBRU>.

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 2016a), 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 United States, 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.

Brendan Nyhan is Professor, Department of Government, Dartmouth College, Hinman Box 6108, 305 Silsby Hall, Hanover, NH 03755 (nyhan@dartmouth.edu). Christopher Skovron is Ph.D. Candidate, Department of Political Science, University of Michigan, 5700 Haven Hall, 505 South State Street, Ann Arbor, MI 48109 (cskovron@umich.edu). Rocío Titiunik is James Orin Murfin Associate Professor, Department of Political Science, University of Michigan, 5700 Haven Hall, 505 South State Street, Ann Arbor, MI 48109 (titiunik@umich.edu).

We thank Matias Cattaneo, Simon Chauchard, Kevin Collins, Alexander Coppock, Jeff Friedman, Brian Greenhill, Michael Herron, Yusaku Horiuchi, Jeremy Horowitz, Dean Lacy, Jacob Montgomery, Phil Paolino, Jason Reifler, Daniel Smith, Brad Spahn, Thomas Zeitzoff, seminar participants at the UCLA American Politics Workshop and the UC-Davis Political Science Speaker Series, the editor, and three anonymous reviewers for helpful feedback; Bob Blaemire at Catalist for his assistance in procuring our data; and John Holbein and Sunshine Hillygus for providing replication data. All errors are our own. Nyhan acknowledges support from the Robert Wood Johnson Foundation's Scholars in Health Research Program. Skovron acknowledges funding from the NSF Graduate Research Fellowship Program. Titiunik acknowledges financial support from the National Science Foundation (SES 1357561).

American Journal of Political Science, Vol. 61, No. 3, July 2017, Pp. 744–760

©2017, Midwest Political Science Association

DOI: 10.1111/ajps.12288

As a result, researchers often estimate turnout effects using voter registration files, which can take at least two different forms. In what we call *pretreatment 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 nonexperimental. 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., Citrin, Green, and Levy 2014; Gerber, Green, and Larimer 2008). Other studies identify the period when a nonexperimental 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 2016b). The common feature of this type of study is that the registration decisions that determine the study population occur *before* the treatment is assigned.¹

In contrast, in what we call *posttreatment registrant studies*, researchers are typically interested in the effect of a nonexperimental 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 coethnic candidates on turnout that are calculated among registrants.

Although both types of studies rely on registration files, they differ crucially in their study populations. Pretreatment registrant studies consider an initial group of registrants that is defined and observed before the treatment is assigned. In contrast, posttreatment registrant studies consider treatments that could affect both registration and voter turnout decisions while relying on posttreatment 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 posttreat-

ment 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 posttreatment 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 posttreatment 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 or partisans among registrants (e.g., Barreto 2007; Barreto, Segura, and Woods 2004; Fraga and Merseeth 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 (Cepaluni and Hidalgo 2016; Fraga 2016a; Meredith 2009).

The sensitivity approach we propose is useful in both cases. When the only data available are a posttreatment 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., Coppock and Green 2015; Dinas 2012, 2014; Holbein and Hillygus 2015; Meredith 2009; Mullainathan and Washington 2009). These

¹This characterization of *pretreatment* registrant studies implicitly assumes that all subjects registered pretreatment 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 assess the robustness of results in cases where this assumption is violated.

studies typically compare voter turnout and political attitudes among individuals whose 18th 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 4 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 nonexperimental turnout studies when turnout rates are calculated as a proportion of total registration.

Our second study is a reanalysis 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 a moderately 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 to 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 ($2,500/5,000 = 50\%$ vs. $2,500/4,000 = 62.5\%$ among controls). However, the true turnout rate of 25% is identical in both groups (column 5).

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 are 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

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

	Total (1)	Registered (2)	Overall Population		
			Voted (3)	Turnout (% Reg.) (4)	Turnout (% Pop.) (5)
Treated	P_T	5,000	2,500	50%	$2,500/P_T$
Control	P_C	4,000	2,500	62.5%	$2,500/P_C$

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 supporting 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 3) for each group, which allows us to calculate turnout rates among registrants by group (column 4). However, letting the subscripts T and C denote the treatment and control groups, respectively, the total eligible population counts in each group, which we denote by P_T and P_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.

We introduce some additional notation. We let R_T and R_C denote the total registration counts in the treatment and control groups, respectively, and V_T and V_C denote the total numbers of voters who turned out to vote in each group. The desired—but unavailable—turnout rates are thus

$$T_T^{Pop} = \frac{V_T}{P_T} \quad \text{and} \quad T_C^{Pop} = \frac{V_C}{P_C},$$

²For 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).

where the superscript Pop denotes that these turnout rates are calculated as a proportion of the total (eligible) population. Henceforth, we refer to T_T^{Pop} and T_C^{Pop} as the *turnout-to-population* rates or simply the *true turnout* rates. Without observing the eligible population counts P_T and P_C , we cannot calculate these turnout-to-population rates directly.

We define the registration rates $r_T = R_T/P_T$ and $r_C = R_C/P_C$ in the treatment and control groups, respectively. Given these rates, the total registration counts, R_T and R_C , can be expressed as $R_T = r_T \times P_T$ and $R_C = r_C \times P_C$, and we can express the unknown total eligible population as total registration divided by the probability of registration:

$$P_T = R_T/r_T \quad \text{and} \quad P_C = R_C/r_C.$$

We then define the *turnout-to-registration* rate in each group, T_T^{Reg} and T_C^{Reg} , as follows:

$$T_T^{Reg} = \frac{V_T}{R_T} \quad \text{and} \quad T_C^{Reg} = \frac{V_C}{R_C}.$$

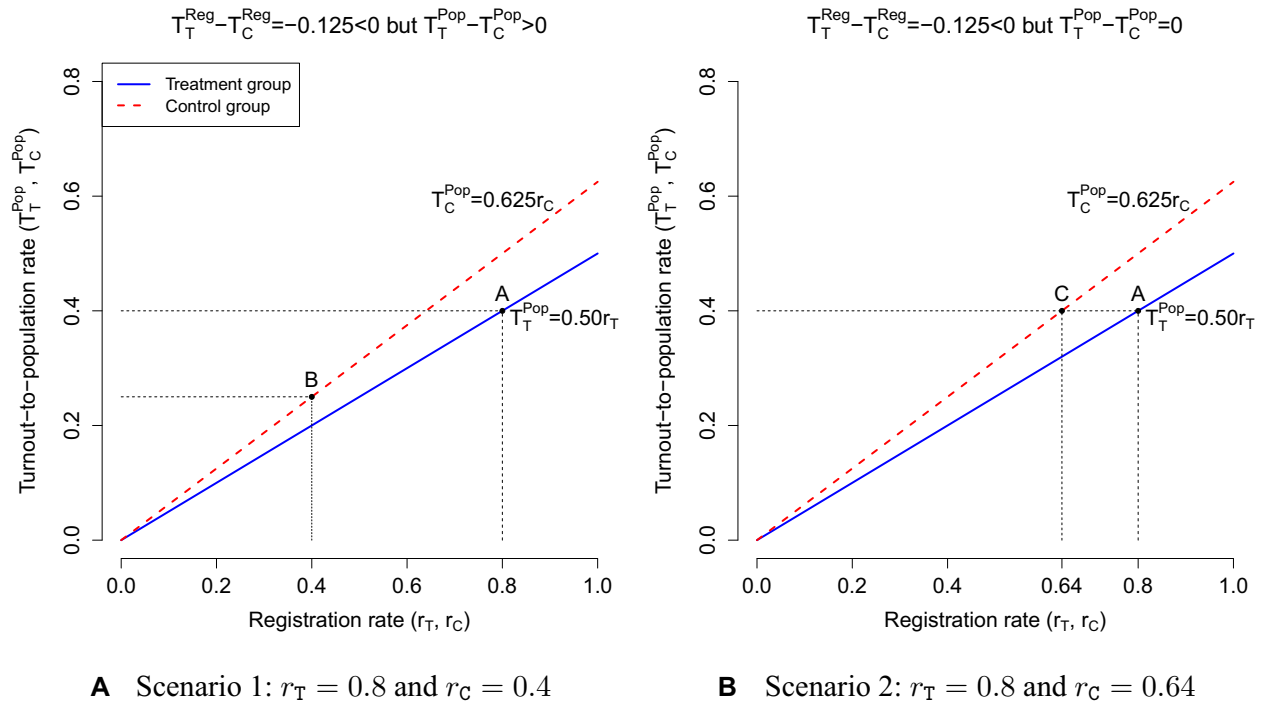
Given these definitions, we can express the desired turnout-to-population rates as

$$\begin{aligned} T_T^{Pop} &= \frac{V_T}{P_T} = \frac{V_T}{R_T/r_T} = T_T^{Reg} \times r_T \quad \text{and} \\ T_C^{Pop} &= \frac{V_C}{P_C} = \frac{V_C}{R_C/r_C} = T_C^{Reg} \times r_C. \end{aligned} \tag{1}$$

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_T and r_C are unknown. Our sensitivity analysis considers how large the difference between r_T and r_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 $T_T^{Reg} < T_C^{Reg}$, 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_T and r_C are unknown, $T_T^{Reg} < T_C^{Reg}$ is not enough to conclude that $T_T^{Pop} < T_C^{Pop}$.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 $T_T^{Reg} - T_C^{Reg}$, we can estimate how different the registration rates would have to be between the treatment and the control groups for

³Likewise, we cannot conclude that $T_T^{Pop} > T_C^{Pop}$ or $T_C^{Pop} = T_T^{Pop}$ without knowing r_T and r_C .

FIGURE 1 True Turnout Rates as a Function of Registration Rate


Note: We assume $T_T^{\text{Reg}} = 0.50$ and $T_C^{\text{Reg}} = 0.625$ in both scenarios.

this difference to be observed when there is no difference in turnout-to-population rates (i.e., when $T_T^{\text{Pop}} - T_C^{\text{Pop}} = 0$).

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

$$\Delta = T_T^{\text{Pop}} - T_C^{\text{Pop}} = (T_T^{\text{Reg}} \times r_T) - (T_C^{\text{Reg}} \times r_C).$$

We note two points. First, if $r_T = r_C = 1$, the expression simplifies to $\Delta = T_T^{\text{Reg}} - T_C^{\text{Reg}}$. 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 ($r_T = r_C = r \neq 1$), the unknown turnout share difference simplifies to $\Delta = r(T_T^{\text{Reg}} - T_C^{\text{Reg}})$. Since $0 \leq r \leq 1$, in this case the sign of Δ is equal to the sign of $T_T^{\text{Reg}} - T_C^{\text{Reg}}$ and $|\Delta| \leq |T_T^{\text{Reg}} - T_C^{\text{Reg}}|$; 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_T and r_C are nonzero and $r_T \neq r_C$. 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 ($T_T^{\text{Pop}}, T_C^{\text{Pop}}$)

against the registration rates (r_T, r_C) separately for each group, holding turnout-to-registration rates ($T_T^{\text{Reg}}, T_C^{\text{Reg}}$) fixed. We adopt the scenario in Tables 1 and 2 where $T_T^{\text{Reg}} = 0.50$ and $T_C^{\text{Reg}} = 0.625$. Thus, we plot the linear functions $T_T^{\text{Pop}} = 0.50 \times r_T$ and $T_C^{\text{Pop}} = 0.625 \times r_C$. 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 $T_C^{\text{Reg}} = 0.50$ and $T_C^{\text{Reg}} = 0.625$, which means that the turnout-to-registration difference is always negative (-0.125).

Imagine first that the probability of registration in the treatment group is 0.8 ($r_T = 0.8$) and the probability of registration in the control group is 0.4 ($r_C = 0.4$). In this case, the true turnout rate in the treatment group (T_T^{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_C^{Pop}) is obtained from the y-coordinate of point B on the dashed line. Under this scenario, $\Delta = T_T^{\text{Pop}} - T_C^{\text{Pop}} = 0.4 - 0.25 = 0.15$. 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 $r_T = 0.8$ and $r_C = 0.64$. In this case, both T_T^{Pop} and T_C^{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:

$$k = \frac{r_T}{r_C}$$

We assume that both r_T and r_C are nonzero. Moreover, because r_T and r_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 $r_T/k = r_C$ and r_C is a rate, k must satisfy the restriction $0 < r_T/k \leq 1$; that is, the smallest value that k can take is r_T .⁴ Our sensitivity analysis explores, for a given treatment group registration rate r_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 $T_T^{\text{Reg}} - T_C^{\text{Reg}}$, 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_T and the ratio of treatment/control registration rates k :

$$\Delta(r_T, k) = (T_T^{\text{Reg}} \times r_T) - (T_C^{\text{Reg}} \times \frac{r_T}{k}) = r_T \left(T_T^{\text{Reg}} - \frac{T_C^{\text{Reg}}}{k} \right),$$

where we make the arguments k and r_T explicit. Thus, for any nonzero value of r_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 $r_T > 0$, we find this value, which we call k^* , as the solution to $(T_T^{\text{Reg}} - \frac{T_C^{\text{Reg}}}{k^*}) = 0$, leading to

$$k^* = \frac{T_C^{\text{Reg}}}{T_T^{\text{Reg}}}$$

By definition, $\Delta(r_T, k^*) = 0$ for any $0 < r_T \leq 1$.

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_T and r_C). In particular, we can calculate k^* , 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 exam-

ple, $T_T^{\text{Reg}} = 2,500/5,000 = 0.5$ and $T_C^{\text{Reg}} = 2,500/4,000 = 0.625$, so the difference in turnout rates among registered voters is negative ($T_T^{\text{Reg}} - T_C^{\text{Reg}} = -0.125$). In this case, $k^* = 0.625/0.50 = 1.25$, 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.

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 $\Delta(r_T, k)$ as a function of r_T for differing values of k (which implicitly fixes r_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^* curve, when $k = k^* = 1.25$, the difference in true turnout rates is zero for every value of r_T . In contrast, when the registration rate is equal in both groups and thus $k = 1$, the observed difference in turnout-to-registration rates ($T_T^{\text{Reg}} - T_C^{\text{Reg}} = -0.125$) is equal to the true difference in turnout rates if $r_T = 1$.

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^* . For $k > k^* = 1.25$, the difference in true turnout is positive and the sign of the turnout-to-registration rate difference is reversed. For $1 \leq k < k^* = 1.25$, the true turnout effect is negative but smaller in absolute value than the difference in turnout-to-registration rates for all r_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_T .

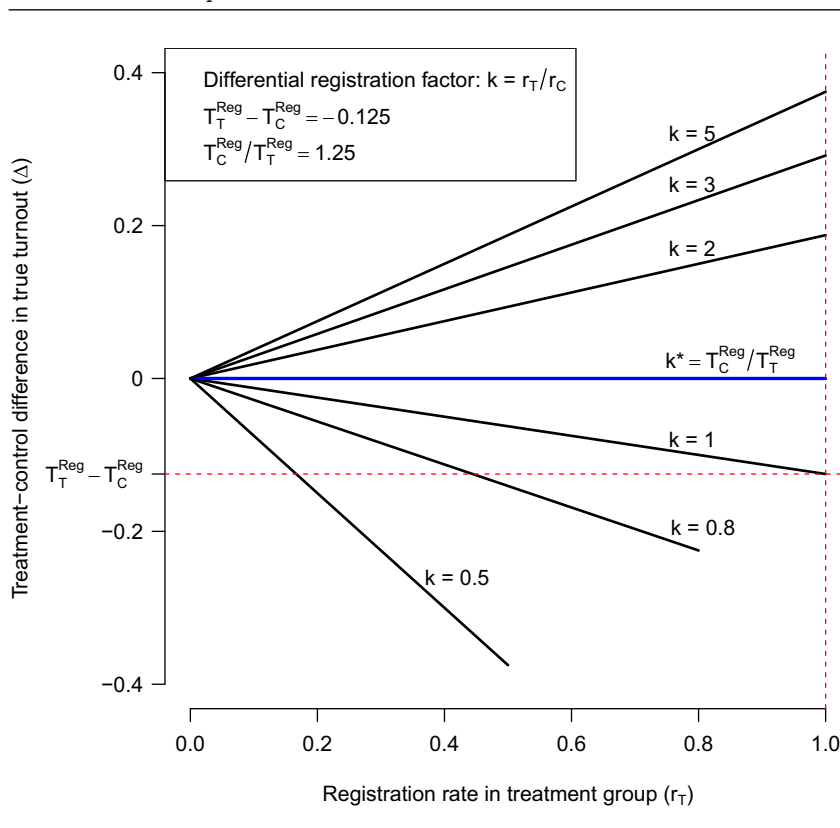
Incorporating Prior Knowledge of Differential Registration

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^* 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_T and r_C are unknown, scholars often have prior information about the range of plausible values they can take. Imagine that we

⁴In other words, values of $k \in [0, r_T)$ are not allowed because they would imply $r_C \geq 1$.

FIGURE 2 Difference in True Turnout Rates as a Function of k and r_T



Note: We assume $T_T^{\text{Reg}} = 0.50$ and $T_C^{\text{Reg}} = 0.625$.

use a survey estimate of the registration rate in the overall U.S. population as a guess for r_T , the true registration rate in the treatment group. We call this guess \tilde{r}_T . Our concern is that r_T differs from r_C —in other words, 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 $k = r_T / r_C$. Imagine that our guess for the differential registration factor is \tilde{k} . We can use \tilde{r}_T and \tilde{k} to calculate the guessed treatment-control difference in true turnout rates, $\tilde{\Delta} = \tilde{r}_T (T_T^{\text{Reg}} - T_C^{\text{Reg}} / \tilde{k})$. If $\tilde{\Delta}$ is of the same sign as $T_T^{\text{Reg}} - T_C^{\text{Reg}}$, we can conclude that the 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^*).

Consider our example above in which $T_T^{\text{Reg}} = 0.50$ and $T_C^{\text{Reg}} = 0.625$. Let us assume that our guess for \tilde{r}_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 10 percentage points relative to the control group. Since $\tilde{r}_T = 0.59$, our guess for r_C is about 0.49. This yields $\tilde{k} = 0.59 / 0.49 = 1.20$, which is less than $k^* = 1.25$ 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 18th 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 United States, a discontinuity in voting eligibility occurs when citizens turn 18 years of age. As a result of the 26th Amendment to the U.S. Constitution (adopted in 1971), people who turn 18 on or before Election Day can cast a vote, but those who will turn 18 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 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 18 within 8 days of Election Day and assume that eligibility can be considered as-if randomly assigned between those individuals born on Election Day or 3 days earlier (the treatment group) and those born 1–4 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 4 days of the election eligibility cutoff—a far narrower window than previous studies, which have used windows measured in months (Dinas 2012, 2014; Holbein and Hillygus 2015; Meredith 2009) or years (Coppock and Green 2015; Mullainathan and Washington 2009).

⁵See Cattaneo, Titiunik, and Vazquez-Bare (2017) for a comparison of this randomization-based RD approach to the more standard continuity-based approach.

⁶Specifically, we construct confidence intervals for the difference-in-means between just-eligible and just-ineligible voters 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.

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 collected a random sample of voters in the Catalist file born in the 8 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 November 1–4 and were thus 18 years old and eligible to vote on November 4, 2008, whereas the control group was born 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 data set 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 or on 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.

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 find-

⁷Colorado, 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.

⁸See the supporting information for more details on birthdates in the Catalist data.

⁹We 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.

¹⁰Balance tests are reported in the supporting information.

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	—	—

ings initially seem to contradict findings that eligibility increases subsequent turnout (e.g., Coppock and Green 2015; Dinas 2012; Meredith 2009). While we find a statistically 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 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 just-eligible registered voters born in 1990 in 2010. (RD plots illustrating these estimates are included in the supporting information.)

Sensitivity Analysis: Assessing Differential Registration Scenarios

We now conduct a sensitivity analysis, which is presented in Table 5. Again, the key term is k^* —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^* 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^* is 0.87, which means that just-eligibles would have

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	—	—

Note: 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)	
$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*	$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*	$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*
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)	
$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*	$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*	$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*
-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)	
$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*	$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*	$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*
-3.06	1.15	—	—	—	—

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

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 preregistration 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^* 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^* values range from 1.03 to 1.15, which means that only slight registration differentials in the expected direction (i.e.,

$r_T > r_C$) could produce the observed negative turnout-to-registration effects. If the registration differentials were larger than k^* , the effects on turnout-to-population rates would be positive.

Assessing k^* Using External Data

The values of k^* reported above indicate that relatively small differences in registration rates between the treatment and control groups could explain the observed

TABLE 6 Registration Rates as of 2011 by Voting Eligibility as Proportion of Births

Year	Treated	Control	\hat{k}	Sensitivity (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	—	—

negative results. We now use birth totals as a proxy for the voting-eligible 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 8-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_T and r_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 group than in the control group in each row (all $p < .01$). These differences are greatest for the 1990 cohort, possibly because just-eligibles in that cohort had less time to “catch up” to

just-eligibles by 2011, but persist even among the 1986 cohort 7 years after turning 18.

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^* 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^* 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^* 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 largely the opposite of what we found when we conditioned on registration (significantly positive for E2 and E3 for the 1986 cohort and E2 for the 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 of 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 4 days in the 1986, 1988, and 1990 cohorts all include two weekend days, when birth rates are typically lower in the United States, whereas the control windows include only weekdays. These findings underscore the sensitivity of these results to VEP approximations.

Study 2: Preregistration Effects in Florida

Our second study is based on recent work by Holbein and Hillygus (2015), who investigate the effects of preregistration on future turnout among young people.

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

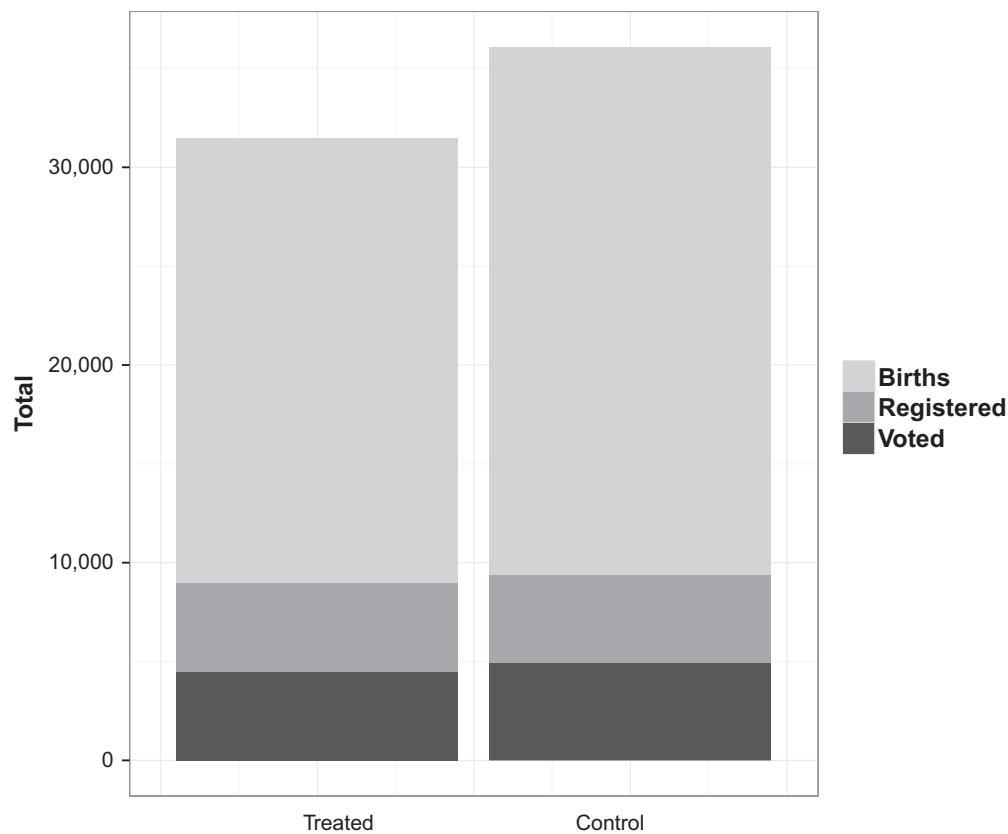
¹³As we show in the supporting information, our results are unchanged when we exclude states with preregistration.

¹¹The 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.

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	—	—

Note: 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.

FIGURE 3 Total Population, Registration, and Voters for the 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 still estimate the difference in turnout-to-population rates between groups. Voting is measured in the 2008 election; registration is measured in 2011.

TABLE 8 Eligibility Effects on Turnout-to-Registration Rates: 1990 Florida Data

Window	T_T^{Reg} (Turnout-to-Reg. eligibles)	T_C^{Reg} (Turnout-to-Reg. Ineligibles)	$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	k^*
± 1 month	49.08	51.88	-2.80 [-3.91, -1.68]	1.057
± 2 months	49.29	51.84	-2.55 [-3.33, -1.77]	1.052

Note: Source is Holbein and Hillygus's Florida voter file for citizens with 1990 births within 1-2 month(s) of November 4. Sample size for ± 1-month window is 30,979; sample size for ± 2-month window is 64,286. Brackets show 95% confidence intervals based on a differences-in-means Wald test.

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 18 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) 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, whereas most of those who are narrowly eligible to vote register "regularly" (i.e., when they are already 18). 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 reanalysis of Holbein and Hillygus's (2015) 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 reanalyze 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 whether the results could be driven by differential registration bias. For our main analysis, we subset the Florida data to people born October 4–December 4, 1990, to match the Holbein and Hillygus (2015) window of approximately 1 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 2 months on either side of the cutoff.

Table 8 reports the results for both windows. In the ± 1-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 3 percentage-point effect that Holbein and Hillygus (2015) report for their ITT estimate.¹⁴ In the larger window of 2 months on either side of 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 the 5% level.

¹⁴The 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.

TABLE 9 RD Estimates of Eligibility Effect in 1990 Florida Cohort

Denominator	T_T^{Reg} (Turnout Eligibles)	T_C^{Reg} (Turnout Ineligibles)	$T_T^{\text{Reg}} - T_C^{\text{Reg}}$	\hat{k}
Window of ± 1 Month				
Registration	49.08	51.88	-2.80 [-3.91, -1.68]	—
Births	44.81	47.48	-2.67 [-3.74, -1.60]	0.998
Window of ± 2 Months				
Registration	49.29	51.84	-2.55 [-3.33, -1.77]	—
Births	48.76	45.41	3.36 [2.61, 4.10]	1.129

Note: Source of registration data is Holbein and Hillygus's Florida voter file for citizens with 1990 births within 1-2 month(s) of November 4. Sample size for ± 1 -month window is 30,979; sample size for ± 2 -month window is 64,286. Estimated births calculated from Florida vital records for September-December 1990. Brackets show 95% confidence intervals based on a differences-in-proportions Wald test.

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^* value of 1.057 for the ± 1 -month window used in Holbein and Hillygus (2015), which means that if the rate of registration were 5.7% higher in the treatment group 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 6% would switch the effect from negative to positive). In the ± 2 -month window, the k^* value is 1.052, 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^* value of 1.057 implies that the rate of registration among just-ineligible (control) voters would have to be approximately 46.36% or lower to change the sign of the point estimate—a difference of just 2.64 percentage points. We therefore conclude that, as in Study 1, the negative eligibility effect found in Florida is sensitive to *positive* differential registration—that is, to a situation where the registration rate is higher in the treatment group than in the control group.¹⁵

¹⁵The main focus of Holbein and Hillygus (2015) is not the ITT, but rather the treatment on the treated (TOT) effect. This quantity

Assessing k^* 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 state and exact date in 1990 or later years, we cannot use an exact ± 1 -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 ± 1 -month window, the effect remains negative when we calculate turnout based on approximate births in the window, which is consistent with the fact that the estimated value \hat{k} (0.998) is less than the corresponding k^* (1.057). 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 2 months on either side of the cutoff. Table 9

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.

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; however, we present them because we believe they illustrate the challenges involved in obtaining accurate VEP estimates.

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 United States, 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 rates 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 nonexperimental 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 it 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 the 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 in the control group.

These findings illustrate the complex trade-offs 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 suffer from two primary limitations: measurement error due to self-reporting, especially overreporting (e.g., Ansolabehere and Hersh 2012; Fraga 2016a), 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 consist 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 unrestricted 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 sizes 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 posttreatment 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.

References

- Ansolabehere, Stephen, and Eitan Hersh. 2012. "Validation: What Big Data Reveal about Survey Misreporting and the Real Electorate." *Political Analysis* 20(4): 437–59.
- Barber, Michael, and Kosuke Imai. 2014. "Estimating Neighborhood Effects on Turnout from Geocoded Voter Registration Records." Working paper, Princeton University. <http://imai.princeton.edu/research/files/neighbor.pdf>.
- Barreto, Matt A. 2007. "Sí Se Puede! Latino Candidates and the Mobilization of Latino Voters." *American Political Science Review* 101(3): 425–41.
- Barreto, Matt A., Gary M. Segura, and Nathan D. Woods. 2004. "The Mobilizing Effect of Majority-Minority Districts on Latino Turnout." *American Political Science Review* 98(1): 65–75.
- Cattaneo, Matias D., Brigham Frandsen, and Rocío Titiunik. 2015. "Randomization Inference in the Regression Discontinuity Design: An Application to Party Advantages in the U.S. Senate." *Journal of Causal Inference* 3(1): 1–24.
- Cattaneo, Matias D., Rocío Titiunik, and Gonzalo Vazquez-Bare. 2017. "Comparing Inference Approaches for RD Designs: A Reexamination of the Effect of Head Start on Child Mortality." *Journal of Policy Analysis and Management*, forthcoming.
- Cepaluni, Gabriel, and F. Daniel Hidalgo. 2016. "Compulsory Voting Can Increase Political Inequality: Evidence from Brazil." *Political Analysis* 24(2): 273–80.
- Citrin, Jack, Donald P. Green, and Morris Levy. 2014. "The Effects of Voter ID Notification on Voter Turnout: Results from a Large-Scale Field Experiment." *Election Law Journal* 13(2): 228–42.
- Coppock, Alexander, and Donald Green. 2015. "Is Voting Habit Forming? New Evidence from Experiments and Regression Discontinuities." *American Journal of Political Science* 60(4): 1044–62.
- Couper, Mick P. 1997. "Survey Introductions and Data Quality." *Public Opinion Quarterly* 61(2): 317–38.
- Dinas, Elias. 2012. "The Formation of Voting Habits." *Journal of Elections, Public Opinion & Parties* 22(4): 431–56.
- Dinas, Elias. 2014. "Does Choice Bring Loyalty? Electoral Participation and the Development of Party Identification." *American Journal of Political Science* 58(2): 449–65.
- Elwert, Felix, and Christopher Winship. 2014. "Endogenous Selection Bias: The Problem of Conditioning on a Collider Variable." *Annual Review of Sociology* 40(1): 31–53.
- Enos, Ryan D. 2016. "What the Demolition of Public Housing Teaches Us about the Impact of Racial Threat on Political Behavior." *American Journal of Political Science* 60(1): 123–42.
- Erikson, Robert S., and Laura Stoker. 2011. "Caught in the Draft: The Effects of Vietnam Draft Lottery Status on Political Attitudes." *American Political Science Review* 105(2): 221–37.
- Fowler, James H., Laura A. Baker, and Christopher T. Dawes. 2008. "Genetic Variation in Political Participation." *American Political Science Review* 102(2): 233–48.
- Fraga, Bernard L. 2016a. "Candidates or Districts? Reevaluating the Role of Race in Voter Turnout." *American Journal of Political Science* 60(1): 97–122.
- Fraga, Bernard L. 2016b. "Redistricting and the Causal Impact of Race on Voter Turnout." *The Journal of Politics* 78(1): 19–34.
- Fraga, Bernard L., and Julie Lee Merseth. 2016. "Examining the Causal Impact of the Voting Rights Act Language Minority Provisions." *Journal of Race, Ethnicity, and Politics* 1(1): 31–59.
- Gerber, Alan S., Donald P. Green, and Christopher W. Larimer. 2008. "Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment." *American Political Science Review* 102(1): 33–48.
- Herman, Jody, and Lauren Forbes. 2010. "Engaging America's Youth through High School Voter Registration Programs." Project Vote. http://funderscommittee.org/files/high_school_voter_registration_programs.pdf.
- Hersh, Eitan D. 2013. "Long-Term Effect of September 11 on the Political Behavior of Victims' Families and Neighbors." *Proceedings of the National Academy of Sciences* 110(52): 20959–63.
- Hersh, Eitan D. 2015. *Hacking the Electorate: How Campaigns Perceive Voters*. Cambridge: Cambridge University Press.
- Hersh, Eitan D., and Clayton Nall. 2015. "The Primacy of Race in the Geography of Income-Based Voting: New Evidence from Public Voting Records." *American Journal of Political Science* 60(2): 289–303.
- Holbein, John B., and D. Sunshine Hillygus. 2015. "Making Young Voters: The Impact of Preregistration on Youth Turnout." *American Journal of Political Science* 60(2): 364–82.

¹⁶See 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.

- Jennings, M. Kent, Laura Stoker, and Jake Bowers. 2009. "Politics across Generations: Family Transmission Reexamined." *Journal of Politics* 71(3): 782–99.
- Manski, Charles. 2003. *Partial Identification of Probability Distributions*. New York: Springer-Verlag.
- McDonald, Michael P., and Samuel L. Popkin. 2001. "The Myth of the Vanishing Voter." *American Political Science Review* 95(4): 963–74.
- Meredith, Marc. 2009. "Persistence in Political Participation." *Quarterly Journal of Political Science* 4(3): 187–209.
- Mullainathan, Sendhil, and Ebonya Washington. 2009. "Sticking with Your Vote: Cognitive Dissonance and Political Attitudes." *American Economic Journal: Applied Economics* 1(1): 86–111.
- Newcombe, Robert G. 1998. "Interval Estimation for the Difference between Independent Proportions: Comparison of Eleven Methods." *Statistics in Medicine* 17(8): 873–90.
- Rosenbaum, Paul R. 1984. "The Consequences of Adjustment for a Concomitant Variable That Has Been Affected by the Treatment." *Journal of the Royal Statistical Society, Series A (General)* 147(5): 656–66.
- Sears, David O., and Nicholas A. Valentino. 1997. "Politics Matters: Political Events as Catalysts for Preadult Socialization." *American Political Science Review* 91(1): 45–65.

Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

- S1:** Quality of Catalist data
- S2:** Excluded Catalist data in Study 2
- S3:** Balance checks for Study 1
- S4:** RD plots for Study 1
- S4.1:** Turnout-to-registration rates
- S4.2:** Turnout-to-births rates
- S5:** Results for Study 1 in states without preregistration
- S6:** Birth, registration, and vote totals: Study 1
- S7:** Total births by day and month
- S8:** Sensitivity analysis example: Voting rights restorations