

Front-door Difference-in-Differences Estimators: The Effects of Early In-person Voting on Turnout^{*}

Adam Glynn[†]

Konstantin Kashin[‡]

aglynn@fas.harvard.edu kkashin@fas.harvard.edu

February 24, 2014

Abstract

In this paper, we develop front-door difference-in-differences estimators that utilize information from post-treatment variables in addition to information from pre-treatment covariates. Even when the front-door criterion does not hold, these estimators allow the identification of causal effects by utilizing assumptions that are analogous to standard difference-in-differences assumptions. We also demonstrate that causal effects can be bounded by front-door and front-door difference-in-differences estimators under relaxed assumptions. We illustrate these points with an application to the effects of early in-person voting on turnout. Despite recent claims that early voting had a negative effect on turnout in 2008, we find evidence that early in-person voting had small positive effects on turnout in Florida in 2008 and 2012.

^{*}We thank Manabu Kuroki for comments and suggestions.

[†]Department of Government and Institute for Quantitative Social Science, Harvard University, 1737 Cambridge Street, Cambridge, MA 02138 (<http://scholar.harvard.edu/aglynn>).

[‡]Department of Government and Institute for Quantitative Social Science, Harvard University, 1737 Cambridge Street, Cambridge, MA 02138 (<http://konstantinkashin.com>).

1 Introduction

One of the main tenets of observational studies is that post-treatment variables should not be included in an analysis because naively conditioning on these variables can block some of the effect of interest, leading to post-treatment bias (King, Keohane and Verba, 1994). While this is usually sound advice, it seems to contradict recommendations from the process tracing literature that information about mechanisms can be used to assess the plausibility of an effect (Collier and Brady, 2004; George and Bennett, 2005; Brady, Collier and Seawright, 2006).

The front-door criterion (Pearl, 1995) and its extensions (Kuroki and Miyakawa, 1999; Tian and Pearl, 2002a,b; Shpitser and Pearl, 2006) resolve this apparent contradiction, providing a means for nonparametric identification of treatment effects using post-treatment variables. Importantly, the front-door approach can identify causal effects even when there are unmeasured common causes of the treatment and the outcome (i.e., the total effect is confounded). The basic idea is the following. Suppose the total effect of a treatment on an outcome can be partitioned into a set of pathways and constituent effects. Further suppose that although the total effect is confounded, the constituent effects are identified.¹ When the constituent effects are identified, the front-door adjustment provides a formula for combining these constituent effects into the total effect. Furthermore, unlike traditional path analysis, the front-door adjustment allows for heterogeneous effects and does not require parametric assumptions.

While front-door adjustment seems to be a powerful tool for data analysts, it has been used infrequently (VanderWeele, 2009) due to concerns that the assumptions required for point identification will rarely hold (Cox and Wermuth, 1995; Imbens and Rubin, 1995). A number of papers have proposed weaker and more plausible sets of assumptions (Joffe, 2001; Kaufman, Kaufman and MacLehose, 2009; Glynn and Quinn, 2011) that tend to correspond to conceptions of process trac-

¹This can happen when there is an unmeasured common cause of the treatment and outcome, but there are no unmeasured common causes of the treatment and the mediators or of the mediators and the outcome (precise conditions are discussed below).

ing. However, these approaches rely on binary or bounded outcomes, and even in large samples these methods only provide bounds on causal effects (i.e., partial instead of point identification). Additionally, these bounds on effects typically include zero. Recently, [Glynn and Kashin \(2013\)](#) developed bias formulas that allow the front-door assumptions to be weakened via sensitivity analysis. This allows for any type of outcome variable and increases the possibility that the front-door approach will be informative (e.g., establishing that zero is not a plausible value for the effect).

In this paper, we demonstrate that the bias described in [Glynn and Kashin \(2013\)](#) can sometimes be removed by a difference-in-differences approach when there is one-sided noncompliance and an exclusion restriction. [Glynn and Kashin \(2013\)](#) showed that with one-sided noncompliance, the front-door estimator implies substituting treated noncompliers for controls. For example, if you want to study the effect of signing up for a program, the front-door estimator compares the outcomes of those that sign up (the treated) to the subset of those that sign up but do not show up (the treated noncompliers). Contrast this with standard approaches (e.g., matching and regression) that would compare those that sign up (the treated) with those that do not sign up (the controls).

The front-door difference-in-differences approach extends the front-door approach in the following manner. First, we identify the treated units of interest, which we will refer to as the group of interest. Second, if we can identify a group of treated units distinct from our group of interest, for which we believe the treatment should have no effect, then a non-zero front-door estimate for this group can be attributed to bias. We will refer to this group as the differencing group. For example, in the context of the application to follow, we consider the effects of an early in-person (EIP) voting program on turnout for elections in 2008 and 2012. One differencing group we consider is potential voters that used an absentee ballot in the previous election. EIP was unlikely to have an effect on these voters, as they had already demonstrated their ability to vote by another means of early voting. Therefore, we consider non-zero front-door estimates of the turnout effect for this group to be evidence of bias.

If we further assume that the bias for this group is equal to the bias for our group of interest,

then by subtracting the front-door estimator from this group from the front-door estimator for the group of interest, we can remove the bias from our front-door estimate. Note that if the bias from the differencing group is larger than the bias for the group of interest and/or the treatment has an effect for the differencing group, then this differencing approach can provide a lower bound on the effect of the program.

However, we demonstrate that the bias for each group is related to the proportion of compliers in the group, and therefore, an equal bias assumption is untenable without an additional adjustment. For example, when an exclusion restriction holds such that treatment has no effect for treated noncompliers, then the front-door bias is due entirely to the imputation of the missing potential outcomes for the treated compliers. This means that the front-door bias for a group of units is approximately proportional to the compliance rate for that group (the assumptions necessary for the bias to be exactly proportional to the compliance rate are discussed below). Therefore, when the compliance rates differ between the group of interest and the differencing group, we will often need to re-scale the front-door estimator for the differencing group in order to make the bias comparable. This will be described in detail below.

The paper is organized as follows. Section 2 presents the bias formulas for the front-door approach to estimating average treatment effects on the treated (ATT), both for the general case and the simplification for nonrandomized program evaluations with one-sided noncompliance and an exclusion restriction. Section 3 presents the difference-in-differences approach for front-door estimators for the simplified case and discusses the required assumptions. Section 4 presents an application of the front-door difference-in-differences estimator to election law: assessing the effects of early in-person voting on turnout in Florida. Section 5 concludes.

2 Bias for the Front-Door Approach for ATT

In this section, we present the bias formulas for the front-door approach for estimating the average treatment effect on the treated (ATT). This is often the parameter of interest when assessing the effects of a program or a law. The beginning of this section parallels the early discussion in [Glynn and Kashin \(2013\)](#), but is important for the development of the difference-in-differences estimator presented in this paper. For an outcome variable Y and a binary treatment/action A , we define the potential outcome under active treatment as $Y(a_1)$ and the potential outcome under control as $Y(a_0)$.² Our parameter of interest is the ATT, defined as $\tau_{att} = E[Y(a_1)|a_1] - E[Y(a_0)|a_1] = \mu_{1|a_1} - \mu_{0|a_1}$. We assume consistency, $E[Y(a_1)|a_1] = E[Y|a_1]$, so that the mean potential outcome under active treatment for the treated units is equal to the observed outcome for the treated units such that $\tau_{att} = E[Y|a_1] - E[Y(a_0)|a_1]$. The ATT is therefore the difference between the mean outcome for the treated units and mean counterfactual outcome for these units, had they not received the treatment.

We also assume that $\mu_{0|a_1}$ is potentially identifiable by conditioning on a set of observed covariates X and unobserved covariates U . To clarify, we assume that if the unobserved covariates were actually observed, the ATT could be estimated by standard approaches (e.g., matching). For simplicity in presentation we assume that X and U are discrete, such that

$$\mu_{0|a_1} = \sum_x \sum_u E[Y|a_0, x, u] \cdot P(u|a_1, x) \cdot P(x|a_1),$$

but continuous variables can be handled analogously. However, even with only discrete variables we have assumed that the conditional expectations in this equation are well-defined, such that for all levels of X and U amongst the treated units, all units had a positive probability of receiving either treatment or control (i.e., positivity holds).

²Note that we must assume that these potential outcomes are well defined for each individual, and therefore we are making the stable unit treatment value assumption (SUTVA).

The front-door adjustment for a set of measured post-treatment variables M can be written as the following:

$$\mu_{0|a_1}^{fd} = \sum_x \sum_m P(m|a_0, x) \cdot E[Y|a_1, m, x] \cdot P(x|a_1).$$

We can thus define the large-sample front-door estimator of ATT as:

$$\tau_{att}^{fd} = \mu_{1|a_1} - \mu_{0|a_1}^{fd}.$$

The bias in the front-door estimate of ATT, which is entirely attributable to the bias in the front-door estimate of $\mu_{0|a_1}$, is the following (see Appendix A.1 for a proof):

$$\begin{aligned} B_{att}^{fd} &= \sum_x P(x|a_1) \sum_m \sum_u P(m|a_0, x, u) \cdot E[Y|a_0, m, x, u] \cdot P(u|a_1, x) \\ &\quad - \sum_x P(x|a_1) \sum_m \sum_u P(m|a_0, x) \cdot E[Y|a_1, m, x, u] \cdot P(u|a_1, m, x). \end{aligned}$$

As discussed in [Glynn and Kashin \(2013\)](#), it is possible for the front-door approach to provide an unbiased estimator for the ATT even in the presence of an unmeasured confounder that would bias traditional back-door techniques such as matching and regression adjustment. Specifically, the front-door bias will be zero when three conditions hold: (1) $E[Y|a_1, m, x, u] = E[Y|a_0, m, x, u]$, (2) $P(m|a_0, x) = P(m|a_0, x, u)$, and (3) $P(u|a_1, m, x) = P(u|a_1, x)$. The first will hold when Y is mean independent of A conditional on U , M , and X , while the latter two will hold if U is independent of M conditional on X and a_0 or a_1 .

For the difference-in-differences estimators we consider in this paper, we use the special case of nonrandomized program evaluations with one-sided noncompliance. Following the literature in econometrics on program evaluation, we define the program impact as the ATT where the active treatment (a_1) is assignment into a program ([Heckman, LaLonde and Smith, 1999](#)), and when M

indicates whether the active treatment (a_1) was actually received. We use the short-hand notation m_1 to denote that active treatment was received and m_0 if it was not.

Assumption 1 (One-sided noncompliance)

$$P(m_0|a_0, x) = P(m_0|a_0, x, u) = 1 \text{ for all } x, u.$$

Assumption 1 implies that only those assigned to treatment can receive treatment, and the front-door large-sample estimator reduces to the following under this assumption:

$$\begin{aligned} \tau_{att}^{fd} &= \mu_{1|a_1} - \mu_{0|a_1}^{fd} \\ &= E[Y|a_1] - \sum_x \sum_m P(m|a_0, x) \cdot E[Y|a_1, m, x] \cdot P(x|a_1) \\ &= E[Y|a_1] - \sum_x \underbrace{E[Y|a_1, m_0, x]}_{\text{treated non-compliers}} \cdot P(x|a_1). \end{aligned} \tag{1}$$

This formula is interesting because it does not rely upon outcomes of control units in the calculation of the potential outcome under control for treated units. This is a noteworthy point that has implications for research design that we will revisit subsequently. Compare this to the standard back-door large-sample estimator for ATT:

$$\begin{aligned} \mu_{att}^{fd} &= \mu_{1|a_1} - \mu_{0|a_1}^{bd} \\ &= E[Y|a_1] - \sum_x \underbrace{E[Y|a_0, x]}_{\text{controls}} \cdot P(x|a_1). \end{aligned} \tag{2}$$

Roughly speaking, standard back-door estimates match units that were assigned treatment to similar units that were assigned control. On the other hand, front-door estimates match units that were assigned treatment to similar units that were assigned treatment but did not receive treatment.

The simplified front-door ATT bias can be written as the following (see Appendix A.2):

$$B_{att}^{fd} = \sum_x P(x|a_1) \sum_u E[Y|a_0, m_0, x, u] \underbrace{[P(u|a_1, x) - P(u|a_1, m_0, x)]}_{\varepsilon} \quad (3)$$

$$+ \sum_x P(x|a_1) \sum_u \underbrace{\{E[Y|a_0, m_0, x, u] - E[Y|a_1, m_0, x, u]\}}_{\eta} P(u|a_1, m_0, x). \quad (4)$$

However, ε can be re-written as:

$$\begin{aligned} \varepsilon &= P(u|a_1, x) - P(u|a_1, m_0, x) \\ &= P(m_1|a_1, x)[P(u|a_1, m_1, x) - P(u|a_1, m_0, x)]. \end{aligned}$$

Assumption 2 (Exclusion restriction)

No direct effect for noncompliers: $E[Y|a_1, m_0, x, u] = E[Y(a_0)|a_1, m_0, x, u]$.

Assumption 2 implies that the treatment does not have an effect on the outcome for noncompliers. When combined with the consistency assumption, Assumption 2 can also be written as $E[Y(a_1)|a_1, m_0, x, u] = E[Y(a_0)|a_1, m_0, x, u]$. If this exclusion restriction holds, then η simplifies to the following (see Appendix A.3):

$$\eta = P(m_1|a_1, x, u) \{E[Y(a_0)|a_1, m_1, x, u] - E[Y(a_0)|a_1, m_0, x, u]\}.$$

Assumption 3 (Constant compliance rates across values of u within levels of x)

$P(m_1|a_1, x, u) = P(m_1|a_1, x)$ for all x and u .

If we further assume that compliance rates are constant within levels of x , then bias simplifies to the

following:

$$B_{att}^{fd} = \sum_x P(x|a_1)P(m_1|a_1, x) \sum_u E[Y|a_0, m_0, x, u][P(u|a_1, m_1, x) - P(u|a_1, m_0, x)] \quad (5)$$

$$+ \sum_x P(x|a_1)P(m_1|a_1, x) \sum_u \{E[Y(a_0)|a_1, m_1, x, u] - E[Y(a_0)|a_1, m_0, x, u]\}P(u|a_1, m_0, x). \quad (6)$$

This simplification implies a common and observable scaling factor to the bias within levels of x , $P(x|a_1)P(m_1|a_1, x)$, and this will be important for the difference-in-differences estimators discussed in the next section.

The bias can be simplified further in cases of clustered treatment assignment. Because the front-door estimator uses only treated units under Assumption 1, it is possible that all units within levels of x were assigned in clusters such that U is actually measured at the cluster level. We present an example of this in the application, where treatment (the availability of early in-person voting) is assigned at the state level, and therefore all units within a state (e.g., Florida) have the same value of u . Formally, the assumption can be stated as the following:

Assumption 4 (u is constant among treated units within levels of x)

For any two units with a_1 and covariate values (x, u) and (x', u') , $x = x' \Rightarrow u = u'$.

Note that Assumption 4 is only a statement about the treated units, and it does not make any assumption about the values of u among control units. If this assumption holds, then the bias simplifies further to:

$$B_{att}^{fd} = \sum_x P(x|a_1)P(m_1|a_1, x) \{E[Y(a_0)|a_1, m_1, x, u] - E[Y|a_1, m_0, x, u]\}. \quad (7)$$

3 Front-door Difference-in-Differences Estimators

If we define the front-door estimator within levels of a covariate x as $\tau_{att,x}^{fd}$, then the front-door estimator can be written as a weighted average of strata-specific front-door estimators where the

weights are relative strata sizes for treated units:

$$\tau_{att}^{fd} = \sum_x P(x|a_1) \tau_{att,x}^{fd}.$$

If we further define the group of interest as the stratum g_1 and the differencing group as the stratum g_2 , then the front-door estimators for these groups can be written as:

$$\begin{aligned} \tau_{att,g_1}^{fd} &= \sum_x P(x|a_1, g_1) \tau_{att,x,g_1}^{fd}, \\ \tau_{att,g_2}^{fd} &= \sum_x P(x|a_1, g_2) \tau_{att,x,g_2}^{fd}. \end{aligned}$$

Using these components, the front-door difference-in-differences estimator can be written as

$$\tau_{att}^{fd-did} = \sum_x P(x|a_1, g_1) \left[\tau_{att,x,g_1}^{fd} - \frac{P(m_1|a_1, x, g_1)}{P(m_1|a_1, x, g_2)} \tau_{att,x,g_2}^{fd} \right]. \quad (8)$$

Hence within levels of x , the front-door difference-in-differences estimator is the difference between the front-door estimator from the group of interest and a scaled front-door estimator from the differencing group, where the scaling factor is the ratio of the compliance rates in the two groups. Then, the overall front-door difference-in-differences estimator is a weighted average of the estimators within levels of x , where the weights are determined by the group of interest proportions of x for treated units.

Intuitively, the scaling factor is necessary because it places the front-door estimate for the differencing group on the same compliance scale as the front-door estimate for the group of interest. The necessity of this adjustment can be most easily seen in (7) in that the bias is scaled by the compliance proportion.

In order for the front-door difference-in-differences estimator to give us an unbiased estimate of the ATT in the group of interest, we need subsets of the assumptions that follow. If we further

define bias within levels of x as $B_{att,x}^{fd}$, then the assumption we need for the differencing group is the following:

Assumption 5 (No effect for the differencing group)

$$\tau_{att,x,g_2}^{fd} = B_{att,x,g_2}^{fd} \text{ for all } x.$$

Note that Assumption 5 can often be weakened. If we believe there are effects for the differencing group, but these have the same sign for the group of interest, then subtracting the scaled estimated effect from the differencing group will remove too much from the estimated effect in the group of interest. For example, if we believe that effects for the group of interest and the differencing group would be positive, then the front-door difference-in-differences estimator would tend to be understated. If we additionally believe that the bias in the front-door estimator is positive prior to the differencing, then the front-door and front-door difference-in-differences estimator will bracket the truth in large samples.

We also need to assume that the bias in the group of interest (g_1) can be removed using the bias from the differencing group (g_2):

Assumption 6 (Bias for g_1 equal to scaled bias for g_2 within levels of x)

$$B_{att,x,g_1}^{fd} = \frac{P(m_1|a_1,x,g_1)}{P(m_1|a_1,x,g_2)} B_{att,x,g_2}^{fd} \text{ for all } x.$$

If Assumptions 1, 5, and 6 hold, then τ_{att}^{fd-did} has no large-sample bias for τ_{att} (see Appendix B.1 for a proof). However, the interpretation of Assumption 6 depends on whether Assumptions 2, 3, or 4 hold. If Assumptions 2 and 3 hold, then Assumption 6 will hold when the sums over u in (5) and (6) are equal across g_1 and g_2 .

Assumption 6 is much easier to interpret when Assumptions 2 and 4 hold. When Assumption 4 holds such that the treated units are assigned in clusters and unobserved variables in the assignment mechanism (u) are constant within levels of x , then Assumption 7 is sufficient to imply that Assumption 6 holds (see Appendix B.2 for a proof).

Assumption 7 (Equal bias across compliance groups for g_1 and g_2)

$\{E[Y(a_0)|a_1, m_1, x, g_1] - E[Y(a_0)|a_1, m_0, x, g_1]\} = \{E[Y(a_0)|a_1, m_1, x, g_2] - E[Y(a_0)|a_1, m_0, x, g_2]\}$ for all x .

This is the case for the application in the next section, where treatment (the availability of early in-person voting) is assigned at the state level, and therefore all units within a state (e.g., Florida) have the same value of u . Note that Assumption 7 is analogous to the parallel trends assumption for standard difference-in-differences estimators, except the trend is across compliance groups instead of time.

4 Illustrative Application

In this section, we present front-door and front-door difference-in-difference estimates for the average treatment effect on the treated (ATT) of an early in-person voting program in Florida. We want to evaluate the impact that the presence of early voting had upon turnout for some groups in the 2008 and 2012 presidential elections in Florida. In traditional regression or matching approaches (either cross sectional or difference-in-differences), data from Florida would be compared to data from states that did not implement early in-person voting. These approaches are potentially problematic because there may be unmeasured differences between the states, and these differences may change across elections. One observable manifestation of this is that the candidates on the ballot will be different for different states in the same election year and for different election years in the same state. The front-door difference-in-differences approach allows us to solve this problem by confining analysis to comparisons made amongst modes of voting within a single presidential election in Florida.

Additionally, by restricting our analysis to Florida, we are able to use individual-level data from the Florida Voter Registration Statewide database, maintained since January 2006 by the Florida Department of State's Division of Elections. This allows us to avoid the use of self-reported turnout,

provides a very large sample size, and makes it possible to implement all of the estimators discussed in earlier sections because we observe the mode of voting for each individual. The data contains two types of records by county: registration records of voters contained within *voter extract files* and voter history records contained in *voter history files*. The former contains demographic information - including, crucially for this paper, race - while the latter details the voting mode used by voters in a given election. The two records can be merged using a unique voter ID available in both file types. However, voter extract files are snapshots of voter registration records, meaning that a given voter extract file will not contain all individuals appearing in corresponding voter history file because individuals move in and out of the voter registration database. We therefore use voter registration files from four time periods to match our elections of interest: 2006, 2008, and 2010 book closing records, and the 2012 post-election registration record. Our total population, based on the total unique voter IDs that appear in any of the voter registration files, is 16.4 million individuals. Appendix C provides additional information regarding the pre-processing of the Florida data.

Information on mode of voting in the voter history files allows us to define compliance with the program for the front-door estimator (i.e., those that utilize EIP voting in the election for which we are calculating the effect are defined as compliers). Additionally, we use information on previous mode of voting to partition the population into a group of interest and differencing groups. In order to maximize the data reliability we define our group of interest as individuals that used EIP in a previous election (e.g., 2008 EIP voters are the group of interest when analyzing the turnout effect for the 2012 election). In other words, we are assessing what would have happened to these 2008 EIP voters if the EIP program had not been available in 2012. To calculate the EIP effect on turnout for the 2012 election, we separately consider 2008 and 2010 EIP voters as our groups of interest. For the 2008 EIP effect on turnout, we rely upon 2006 EIP voters as our group of interest. An attempt to define the group of interest more broadly (e.g., including non-voters) or in terms of earlier elections (e.g., the 2004 election) would involve the use of less reliable data, and would therefore introduce

methodological complications that are not pertinent to the illustration presented here.³ Therefore, the estimates presented in this application are confined only to those individuals that utilized EIP in a previous election and hence we cannot comment on the overall turnout effect.

We consider two differencing groups for each analysis: those who voted absentee and those that voted on election day in a previous election. When considering the 2012 EIP effect for 2008 EIP voters, for example, we use 2008 absentee and election day voters as our differencing groups. It is likely that the 2012 EIP program had little or no effect for 2008 absentee voters and perhaps only a minimal effect for 2008 election day voters, as these groups had already demonstrated an ability to vote by other means. Therefore, any apparent effects estimated for these groups will be primarily due to bias, and this bias can then be removed from the estimates for the group of interest. As discussed in earlier sections, the estimates from the differencing groups must be scaled according to the level of compliance for the group of interest. Finally, the existence of two differencing groups allows us to conduct a placebo test by using election day voters as the group of interest and the absentee voters as the differencing group in each case. This analysis is explored below.

Despite the limited scope of the estimates presented here, these results have some bearing on the recent debates regarding the effects of early voting on turnout. There have been a number of papers that find null results for the effects of early voting on turnout ([Gronke, Galanes-Rosenbaum and Miller, 2007](#); [Gronke et al., 2008](#); [Fitzgerald, 2005](#); [Primo, Jacobmeier and Milyo, 2007](#); [Wolfinger, Highton and Mullin, 2005](#)), and [Burden et al. \(2014\)](#) finds a surprising negative effect of early voting on turnout in 2008.⁴ However, identification of turnout effects from observational data using traditional statistical approaches such as regression or matching are hampered by unobserved

³Following [Gronke and Stewart \(2013\)](#), we restrict our analysis to data starting in 2006 due to its greater reliability than data from 2004. We also might like to extend the group of interest to those that did not vote in a previous election, but we avoid assessing either 2008 or 2012 EIP effects for these voters because it is difficult to calculate the eligible electorate and consequently the population of non-voters. In their analysis of the prevalence of early voting, [Gronke and Stewart \(2013\)](#) use all voters registered for at least one general election between 2006 and 2012, inclusive, as the total eligible voter pool. However, using registration records as a proxy for the eligible electorate may be problematic ([McDonald and Popkin, 2001](#)). By focusing on the 2008 voting behavior of individuals who voted early in 2006, we avoid the need to define the eligible electorate and the population of non-voters.

⁴[Burden et al. \(2014\)](#) examine a broader definition of early voting that includes no excuse absentee voting.

confounders that affect both election laws and turnout (Hanmer, 2009). The front-door estimators presented here provide an alternative approach with useful properties. First, front-door adjustment can identify the effect of EIP on turnout in spite of the endogeneity of election laws that can lead to bias when using standard approaches. Furthermore, unlike traditional regression or matching based estimates, the front-door estimators considered here only require data from Florida. This means that we can effectively include a Florida fixed effect in the analysis, and we do not have to worry about cross-state differences. Furthermore, unlike estimators that make comparisons across time, the turnout comparisons of the front-door estimators are entirely within a presidential election. This means that we can effectively include a Florida/year fixed effect in the analysis, and we do not have to worry about the ballot or election differences skewing turnout numbers across elections. We also include county fixed effects in the analysis in order to control for within-Florida differences.

4.1 Results

The front-door difference-in-differences estimates for the 2012 EIP program are presented in Figure 1. The estimates all utilize county fixed effects and are calculated separately across the racial categories.⁵ The orange and the blue estimates are for the 2008 EIP voters (the group of interest), with 2008 absentee voters and 2008 election day voters as the differencing groups, respectively. The former, for example, is constructed as the difference between front-door estimates for 2008 early voters and the front-door estimates for 2008 absentee voters, with the front-door estimates for the differencing group scaled by the ratio of early voter compliance to absentee voter compliance as shown in 8. The green estimates in Figure 1 represent the placebo test, with 2008 election day voters standing in as the group of interest and the absentee voters as the differencing group.

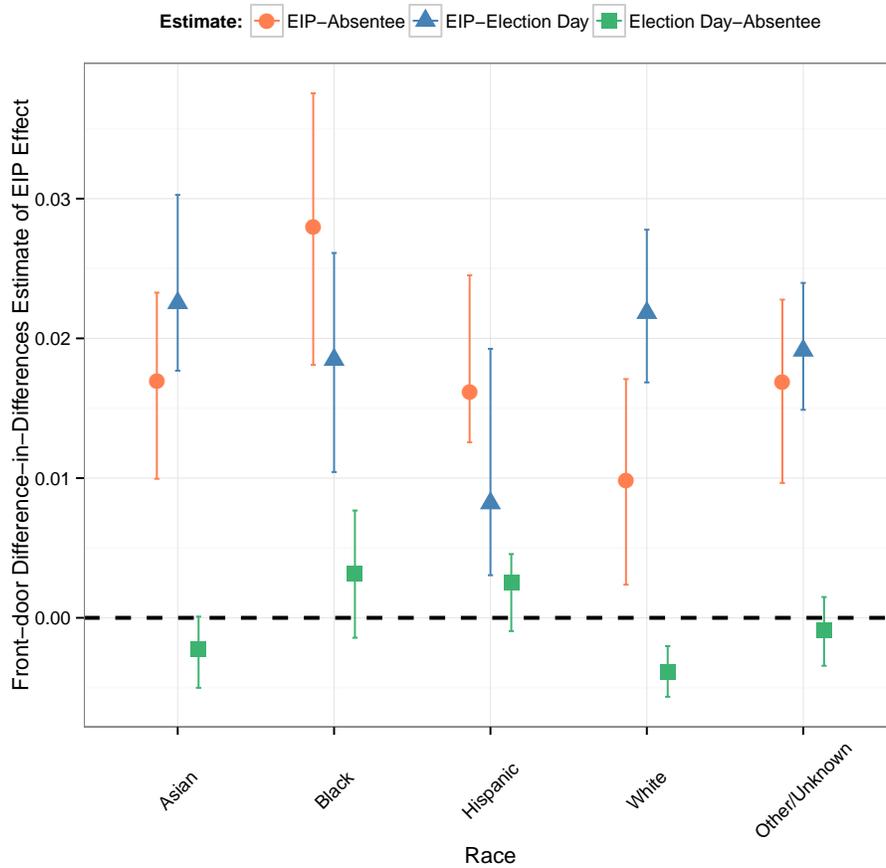
For the 2008 EIP voters, the 2012 EIP program estimates (orange and blue) are positive and

⁵We calculate the FD-DID estimates within each county and then average using the population of the group of interest as the county weight. Due to very small sample sizes in a few counties, we are occasionally unable to calculate front-door estimates. In these cases, we omit the counties from the weighted average when calculating the front-door estimates with fixed effects. We note that due to their small size, these counties are unlikely to exert any significant impact upon the estimates regardless.

significant at the 99% level (based on 10,000 block bootstraps at the county level). There is some evidence of differences between the racial categories, but these differences change depending on which differencing group is used. The green estimates are mostly indistinguishable from zero, indicating that the placebo tests have mostly been passed. The slightly positive green effect for black voters indicates either that there is bias and perhaps a difference-in-differences-in-differences approach should be used (the estimate would remain positive), or that the 2012 EIP program genuinely has an effect for black 2008 election day voters. This is possible if some voters were able to vote on election day in 2008, but not in 2012. The slightly negative green estimate for whites again indicates either bias, or perhaps a negative effect of the 2012 EIP program for white 2008 election day voters. Note that even if we believe this estimate, the weighted average of the orange and the green effects for whites (i.e., the 2012 EIP effect for the 2008 EIP and election day voters together) produces a slightly positive estimate, albeit this estimate is indistinguishable from zero. In sum, the evidence points to a slightly positive turnout effect of the 2012 EIP program on the 2008 EIP users.

In order to make a more direct comparison to the results of [Burden et al. \(2014\)](#), which finds a negative EIP effect for the 2008 election, we present front-door difference-in-differences estimates for the 2008 EIP program in [Figure 2](#). We present the estimates for 2006 EIP voters as the group of interest, using 2006 absentee voters (orange) and 2006 election day voters (blue) as the differencing groups. Unfortunately, we cannot present the estimates with the group of interest and the differencing groups defined in terms of 2004 behavior because the data from 2004 are not reliable (as mentioned above). The placebo test is constructed as the difference in front-door estimates between 2006 election day voters and 2006 absentee voters (green). As before, the EIP program estimates are positive and significant at the 99% level. All placebo tests, with the exception of the white estimate, are indistinguishable from zero, giving us confidence in the estimated EIP effects. Even if the slightly negative placebo estimate for whites indicates a true negative effect of the 2008 EIP program, and not bias, the weighted average of the orange and the green effects (i.e., the 2008 EIP effect for the 2006 EIP and election day voters together), produces a slightly positive estimate,

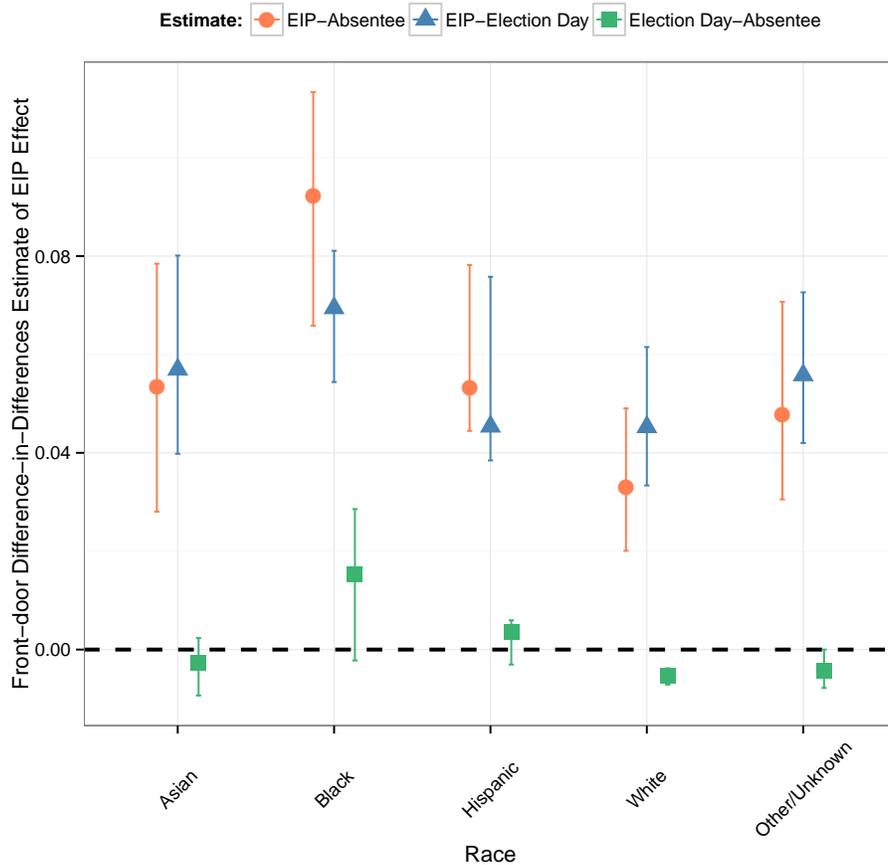
Figure 1: Front-door estimates for the turnout effect in 2012 for voters who voted early in 2008 (by race). All estimates include county fixed effects. 99% block bootstrapped confidence intervals are based on 10,000 replicates.



albeit this estimate is indistinguishable from zero. Therefore, as opposed to [Burden et al. \(2014\)](#), we do not find any evidence that the presence of an EIP program in Florida decreased turnout in 2008. On the contrary, we find evidence that early voting increased turnout for the subset of individuals who voted early in 2006. Moreover, comparing the point estimates across races, we find some evidence that the program had a disproportionate benefit for African-Americans. It is also notable that the size of the estimated EIP effect for 2008 is more than double the estimated EIP effect for 2012 when looking at EIP voters as the group of interest across all races. The difference in effects and also the differences between races might be attributed to two factors. First, since our estimates

for the 2008 EIP program are obtained using groups defined by 2006 midterm election behavior, we note that midterm election early voters are likely different than presidential election year voters. Second, the nature of the early voting program changed between the 2008 and 2012 elections, as described in [Gronke and Stewart \(2013\)](#). One of the most significant alterations to the program was a near-halving of the early voting period from 14 days to 8 days. This change might possibly reduce the effect of the EIP program in 2012 when compared to 2008.

Figure 2: Front-door estimates for the turnout effect in 2008 for voters who voted early in 2006 (by race). All estimates include county fixed effects. 99% block bootstrapped confidence intervals are based on 10,000 replicates.



In order to isolate the consequences of the change in the early voting program from changes in the construction of the group of interest and differencing groups, we re-estimate the effects of

the 2012 EIP program using 2010 EIP voters as the group of interest, and using 2010 absentee (orange) and election day voters (blue) as the differencing groups. Placebo tests are reported using 2010 election day voters as the group of interest and 2010 absentee voters as the differencing group (green). These results are presented in Figure 3, and they are quite similar to the results in Figure 2. This provides some evidence that if we were able to obtain reliable data from the 2004 election, our estimates for the 2008 EIP program using 2004 EIP voters as the group of interest, and using 2004 absentee (orange) and election day voters (blue) as the differencing groups, would likely have produced something similar to Figure 1. However, the estimates in Figure 2 are slightly larger than estimates in Figure 3. This is consistent with the reduction in the early voting window for the 2012 election.

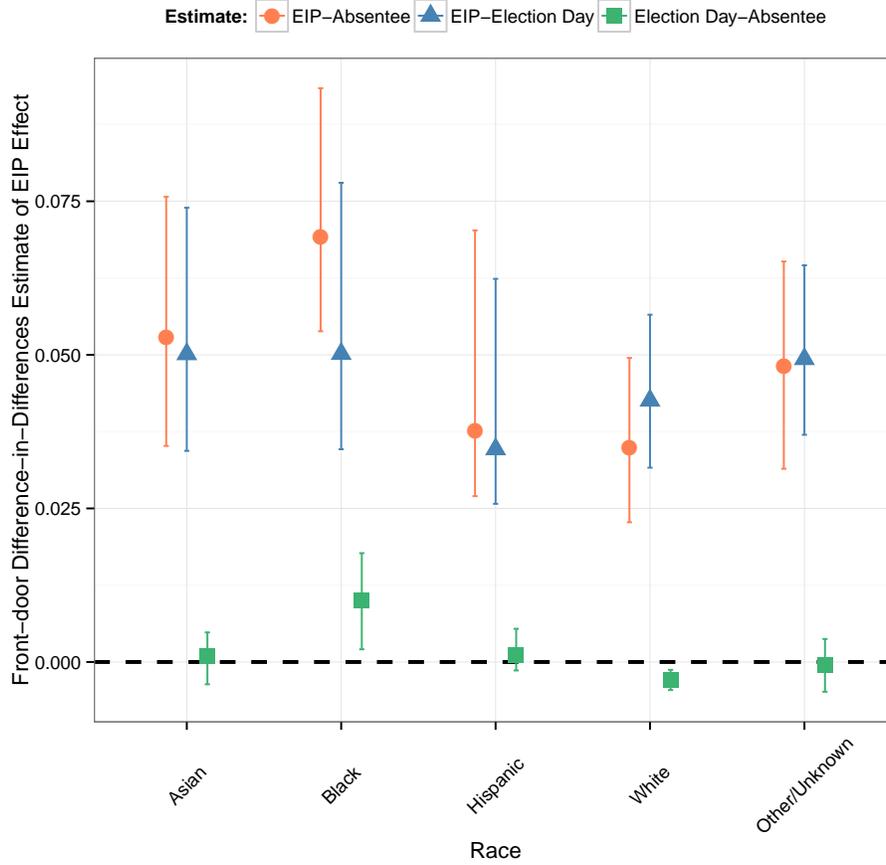
5 Conclusion

In this paper, we have developed front-door difference-in-differences estimators for nonrandomized program evaluations with one-sided noncompliance and an exclusion restriction. These estimators allow for asymptotically unbiased estimation via front-door techniques, even when front-door estimators have significant bias. Furthermore, this allows for program evaluation when all of the relevant units have been assigned to treatment.

We illustrated this technique with an application to the effects of an early in-person (EIP) voting program on turnout in Florida in 2008 and 2012. We found that for two separate differencing groups, the program had small but significant positive effects. This result provides some evidence to counter previous results in the literature that early voting programs had either no effect or negative effects. Hence it is likely that unmeasured confounders were present in those analyses. Additionally, we find some evidence that EIP effects were larger for blacks than for whites. However, this evidence is more tenuous.

Finally, the results in this paper have implications for research design and analysis. First, the

Figure 3: Front-door estimates for the turnout effect in 2012 for voters who voted early in 2010 (by race). All estimates include county fixed effects. 99% block bootstrapped confidence intervals are based on 10,000 replicates.



Florida EIP example demonstrates the importance of collecting post-treatment variables that represent compliance with or uptake of the treatment. Second, the front-door difference-in-differences analysis shows that variation in treatment may not be necessary if variation in compliance is measured. This should make a number of studies feasible that have heretofore been unexamined.

References

- Brady, Henry E., David Collier and Jason Seawright. 2006. "Toward a Pluralistic Vision of Methodology." *Political Analysis* 14:353–368. [2](#)
- Burden, Barry C., David T. Canon, Kenneth R. Mayer and Donald P. Moynihan. 2014. "Election Laws, Mobilization, and Turnout: The Unanticipated Consequences of Election Reform." *American Journal of Political Science* 58(1):95–109. [14](#), [16](#), [17](#)
- Collier, David and Henry E. Brady. 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham, MD: Rowman & Littlefield. [2](#)
- Cox, D. R. and Nanny Wermuth. 1995. "Discussion of 'Causal diagrams for empirical research.'" *Biometrika* 82:688–689. [2](#)
- Fitzgerald, Mary. 2005. "Greater Convenience but not Greater Turnout: The Impact of Alternative Voting Methods on Electoral Participation in the United States." *American Politics Research* 33:842–867. [14](#)
- George, A.L. and A. Bennett. 2005. *Case studies and theory development in the social sciences*. MIT Press. [2](#)
- Glynn, A. and K. Kashin. 2013. "Front-door Versus Back-door Adjustment with Unmeasured Confounding: Bias Formulas for Front-door and Hybrid Adjustments." Working Paper, Winner of the Gosnell Prize for Excellence in Political Methodology. [3](#), [5](#), [6](#)
- Glynn, A. and K. Quinn. 2011. "Why Process Matters for Causal Inference." *Political Analysis* 19(3):273–286. [2](#)
- Gronke, P., E. Galanes-Rosenbaum and P. Miller. 2007. "Early Voting and Turnout." *PS: Political Science and Politics* XL. [14](#)
- Gronke, Paul and Charles Stewart. 2013. "Early Voting in Florida." Working Paper. [14](#), [18](#), [26](#)
- Gronke, Paul, Eva Galanes-Rosenbaum, Peter A. Miller and Daniel Toffey. 2008. "Convenience Voting." *Annual Review of Political Science* 11:437–455. [14](#)
- Hanmer, Michael J. 2009. *Discount Voting: Voter Registration Reforms and Their Effects*. Cambridge University Press. [15](#)
- Heckman, James J, Robert J LaLonde and Jeffrey A Smith. 1999. The Economics and Econometrics of Active Labor Market Programs. In *Handbook of Labor Economics, Volume III*, ed. O. Ashenfelter and D. Card. Elsevier Science North-Holland. [6](#)
- Imbens, Guido and Donald Rubin. 1995. "Discussion of 'Causal diagrams for empirical research.'" *Biometrika* 82:694–695. [2](#)

- Joffe, M.M. 2001. "Using information on realized effects to determine prospective causal effects." *Journal of the Royal Statistical Society. Series B, Statistical Methodology* pp. 759–774. [2](#)
- Kaufman, Sol, Jay S. Kaufman and Richard F. MacLehose. 2009. "Analytic bounds on causal risk differences in directed acyclic graphs with three observed binary variables." *Journal of Statistical Planning and Inference* 139:3473–87. [2](#)
- King, Gary, Robert O. Keohane and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. 1 ed. Princeton University Press. [2](#)
- Kuroki, Manabu and Masami Miyakawa. 1999. "Identifiability Criteria for Causal Effects of Joint Interventions." *J. Japan Statist. Soc.* 29(2):105–117. [2](#)
- McDonald, Michael P. and Samuel L. Popkin. 2001. "The Myth of the Vanishing Voter." *American Political Science Review* 95:963–974. [14](#)
- Pearl, Judea. 1995. "Causal diagrams for empirical research." *Biometrika* 82:669–710. [2](#)
- Primo, David M., Matthew L. Jacobmeier and Jeffrey Milyo. 2007. "Estimating the Impact of State Policies and Institutions with Mixed-Level Data." *State Politics & Policy Quarterly* 7:446–459. [14](#)
- Shpitser, Ilya and Judea Pearl. 2006. "Identification of Conditional Interventional Distributions." *Proceedings of the Twenty Second Conference on Uncertainty in Artificial Intelligence (UAI)*. [2](#)
- Stewart, Charles. 2012. "Declaration of Dr. Charles Stewart III." *State of Florida vs. United States of America*. [26](#)
- Tian, J. and J. Pearl. 2002a. A general identification condition for causal effects. In *Proceedings of the National Conference on Artificial Intelligence*. Menlo Park, CA; Cambridge, MA; London; AAAI Press; MIT Press; 1999 pp. 567–573. [2](#)
- Tian, J. and J. Pearl. 2002b. On the identification of causal effects. In *Proceedings of the American Association of Artificial Intelligence*. [2](#)
- VanderWeele, Tyler J. 2009. "On the relative nature of overadjustment and unnecessary adjustment." *Epidemiology* 20(4):496–499. [2](#)
- Wolfinger, Raymond E., Benjamin Highton and Megan Mullin. 2005. "How Postregistration Laws Affect the Turnout of Citizens Registered to Vote." *State Politics & Policy Quarterly* 5:1–23. [14](#)

A ATT Proofs

A.1 Large-Sample Bias

The bias in the front-door estimate of $E[Y(a_0)|a_1]$ is the following:

$$\begin{aligned}
B_{a_1}^{fd} &= \mu_{0|a_1}^{fd} - \mu_{0|a_1} \\
&= \sum_x \sum_m P(m|a_0, x) \cdot E[Y|a_1, m, x] \cdot P(x|a_1) - \sum_x \sum_u E[Y|a_0, x, u] \cdot P(u|x, a_1) \cdot P(x|a_1) \\
&= \sum_x \sum_m P(m|a_0, x) \sum_u E[Y|a_1, m, x, u] \cdot P(u|a_1, m, x) \cdot P(x|a_1) \\
&\quad - \sum_x \sum_u \sum_m E[Y|a_0, m, x, u] \cdot P(m|a_0, x, u) \cdot P(u|x, a_1) \cdot P(x|a_1) \\
&= \sum_x P(x|a_1) \sum_m \sum_u P(m|a_0, x) \cdot E[Y|a_1, m, x, u] \cdot P(u|a_1, m, x) \\
&\quad - \sum_x P(x|a_1) \sum_m \sum_u P(m|a_0, x, u) \cdot E[Y|a_0, m, x, u] \cdot P(u|a_1, x)
\end{aligned}$$

Note that the bias will be zero when Y is mean independent of A conditional on U , M , and X (i.e., $E[Y|a_1, m, x, u] = E[Y|a_0, m, x, u]$) and U is independent of M conditional on X and a_0 or a_1 (i.e., $P(m|a_0, x) = P(m|a_0, x, u)$ and $P(m|a_1, x) = P(m|a_1, x, u)$). Hence, again it is possible for the front-door approach to provide an unbiased estimator when there is an unmeasured confounder.

A.2 Nonrandomized Program Evaluation with One-Sided Noncompliance

In the special case of nonrandomized program evaluations with one-sided noncompliance, the front-door and the back-door ATT bias can be written as the following, utilizing the fact that $P(m_0|a_0) = 1$ and $P(m_0|a_1) = 0$:

$$\begin{aligned}
B_{att}^{fd} &= \mu_1 - \mu_{0|a_1}^{fd} - (\mu_1 - \mu_{0|a_1}) \\
&= \mu_{0|a_1} - \mu_{0|a_1}^{fd} \\
&= -B_{a_1}^{fd} \\
&= \sum_x P(x|a_1) \sum_u E[Y|u, a_0, m_0, x] P(u|a_1, x) \\
&\quad - \sum_x P(x|a_1) \sum_u E[Y|u, a_1, m_0, x] P(u|a_1, x, m_0)
\end{aligned}$$

Adding and subtracting $\sum_x P(x) \sum_u E[Y|a_0, m_0, u] \cdot P(u|a_1, m_0)$:

$$\begin{aligned}
&= \sum_x P(x|a_1) \sum_u E[Y|u, a_0, m_0, x] \cdot [P(u|a_1, x) - P(u|a_1, x, m_0)] \\
&- \sum_x P(x|a_1) \sum_u \{E[Y|u, a_1, m_0, x] - E[Y|u, a_0, m_0, x]\} \cdot P(u|a_1, m_0, x) \\
\\
B_{att}^{bd} &= \mu_1 - \mu_{0|a_1}^{bd} - (\mu_1 - \mu_{0|a_1}) \\
&= \mu_{0|a_1} - \mu_{0|a_1}^{bd} \\
&= -B_{a_1}^{bd} \\
&= \sum_x P(x|a_1) \sum_u E[Y|u, a_0, m_0, x] \cdot [P(u|a_1, x) - P(u|a_0, x)]
\end{aligned}$$

A.3 Bias Simplification

We can expand η from (4) as:

$$\begin{aligned}
\eta &= -\{E[Y|a_1, m_0, x, u] - E[Y|a_0, m_0, x, u]\} \\
&= -\{E[Y|a_1, m_0, x, u] - E[Y|a_0, x, u]\} \text{ due to one-sided noncompliance} \\
&= -\{E[Y|a_1, m_0, x, u] - E[Y(a_0)|a_0, x, u]\} \text{ due to consistency} \\
&= -\{E[Y|a_1, m_0, x, u] - E[Y(a_0)|a_1, x, u]\} \text{ due to ignorability conditional on } u \text{ and } x \\
&= -E[Y|a_1, m_0, x, u] \\
&+ \{E[Y(a_0)|a_1, m_0, x, u]P(m_0|a_1, x, u) + E[Y(a_0)|a_1, m_1, x, u]P(m_1|a_1, x, u)\}
\end{aligned}$$

If Assumption 2 holds (i.e. $E[Y|a_1, m_0, x, u] = E[Y(a_0)|a_1, m_0, x, u]$), then this simplifies further:

$$\begin{aligned}
\eta &= -E[Y|a_1, m_0, x, u] \\
&+ E[Y(a_0)|a_1, m_0, x, u]P(m_0|a_1, x, u) + E[Y(a_0)|a_1, m_1, x, u]P(m_1|a_1, x, u) \\
&= E[Y(a_0)|a_1, m_0, x, u][P(m_0|a_1, x, u) - 1] + E[Y(a_0)|a_1, m_1, x, u]P(m_1|a_1, x, u) \\
&= -E[Y(a_0)|a_1, m_0, x, u][P(m_1|a_1, x, u)] + E[Y(a_0)|a_1, m_1, x, u]P(m_1|a_1, x, u) \\
&= P(m_1|a_1, x, u)\{E[Y(a_0)|a_1, m_1, x, u] - E[Y(a_0)|a_1, m_0, x, u]\}
\end{aligned}$$

B Front-Door Difference-in-Differences Proofs

B.1 No Large-Sample Bias in the Front-door Difference-in-Differences Estimator

First define $\tau_{att,x} = E[Y(a_1)|a_1, x] - E[Y(a_0)|a_1, x]$. It is well known that $\tau_{att} = \sum_x \tau_{att,x} P(x|a_1)$. Therefore in order to show that τ_{att}^{fd-did} has no bias, we need only show a lack of bias for $\tau_{att,x}$ within levels of x . If Assumptions 5 and 6 hold, then the front-door difference-in-differences estimator has no large-sample bias:

$$\begin{aligned}
 \tau_{att,x}^{fd-did} &= \tau_{att,x,g_1}^{fd} - \frac{P(m_1|a_1, x, g_1)}{P(m_1|a_1, x, g_2)} \tau_{att,x,g_2}^{fd} \\
 &= \tau_{att,x} + B_{att,x,g_1}^{fd} - \frac{P(m_1|a_1, x, g_1)}{P(m_1|a_1, x, g_2)} \tau_{att,x,g_2}^{fd} \\
 &= \tau_{att,x} + B_{att,x,g_1}^{fd} - \frac{P(m_1|a_1, x, g_1)}{P(m_1|a_1, x, g_2)} B_{att,x,g_2}^{fd} \text{ by Assumption 5} \\
 &= \tau_{att,x} + B_{att,x,g_1}^{fd} - B_{att,x,g_1}^{fd} \text{ by Assumption 6} \\
 &= \tau_{att,x}
 \end{aligned}$$

B.2 Assumptions 1, 2, 4, and 7 Imply Assumption 6

Invoking Assumptions 1, 2, and 4, we can apply the simplified bias expression in (7) to write the front-door bias within strata of x for both groups:

$$\begin{aligned}
 B_{att,x,g_1}^{fd} &= P(m_1|a_1, x, g_1) \{E[Y(a_0)|a_1, m_1, x, u, g_1] - E[Y|a_1, m_0, x, u, g_1]\} \\
 B_{att,x,g_2}^{fd} &= P(m_1|a_1, x, g_2) \{E[Y(a_0)|a_1, m_1, x, u, g_2] - E[Y|a_1, m_0, x, u, g_2]\}
 \end{aligned}$$

Assumption 2 allows Assumption 7 to be re-written as the following:

$$\begin{aligned}
 E[Y(a_0)|a_1, m_1, x, g_1] - E[Y(a_0)|a_1, m_0, x, g_1] &= E[Y(a_0)|a_1, m_1, x, g_2] - E[Y(a_0)|a_1, m_0, x, g_2] \\
 E[Y|a_1, m_1, x, g_1] - E[Y|a_1, m_0, x, g_1] &= E[Y|a_1, m_1, x, g_2] - E[Y|a_1, m_0, x, g_2]
 \end{aligned}$$

Therefore using the re-written Assumption 7, we can show that the expression of front-door bias for g_2 , when scaled, is identical to the front-door bias for g_1 :

$$\begin{aligned}
 \frac{P(m_1|a_1, x, g_1)}{P(m_1|a_1, x, g_2)} B_{att,x,g_2}^{fd} &= \frac{P(m_1|a_1, x, g_1)}{P(m_1|a_1, x, g_2)} P(m_1|a_1, x, g_2) \{E[Y(a_0)|a_1, m_1, x, u, g_2] - E[Y|a_1, m_0, x, u, g_2]\} \\
 &= P(m_1|a_1, x, g_1) \{E[Y(a_0)|a_1, m_1, x, u, g_2] - E[Y|a_1, m_0, x, u, g_2]\} \\
 &= P(m_1|a_1, x, g_1) \{E[Y(a_0)|a_1, m_1, x, u, g_1] - E[Y|a_1, m_0, x, u, g_1]\} \\
 &= B_{att,x,g_1}^{fd}
 \end{aligned}$$

C Data Specifics

To construct our population of eligible voters, we examine individuals that have appeared in one of four voter registration snapshots: book closing records from 10/10/2006, 10/20/2008, and 10/18/2010, as well as a 2012 election recap record from 1/4/2013. This yields a total population of 16,371,725 individuals that we are able to subset by race (Asian / Pacific Islander, Black (not Hispanic), Hispanic, White (Not Hispanic), and Other). Note that the Other category contains individuals who self-identify as American Indian / Alaskan Native, Multiracial, or Other, as well as individuals for whom race is unknown. In cases where race changes across the voter registration records, we use the latest available self-reported race. Such changes affect only 1.1% of observations. The breakdown of the the population by race is presented in the rightmost column of Table 1.

We use voter history files from 08/03/2013 to subset the population by voting mode in each election. The voter history files required pre-processing before we could use them for estimation. As mentioned in [Gronke and Stewart \(2013\)](#) and [Stewart \(2012\)](#), there is an issue of duplication of voter identification numbers within the same election. In some cases, this duplication is rather innocuous because the voting mode is identical across records. In these cases, we simply remove duplicate records and include the voter in our analysis. In other cases, voters are recorded as both having voted in a given election and not having voted (code “N”). In these cases, we assume that the voter did indeed cast a ballot and use that code. Finally, there are a few instances in which a voter is recorded to have voted in multiple ways. For example, a voter history file may indicate that a voter voted both absentee and early at a given election. While [Gronke and Stewart \(2013\)](#) indicates that voters may legitimately appear multiple times in the voter history file, this makes the task of stratifying by voting mode difficult. As a result, we choose to exclude individuals who are recorded to have voted using more than one mode. When analyzing the 2008 election subsetting by 2006 voting modes, we exclude 385 individuals. The corresponding numbers for analysis of the 2012 election subsetting using 2008 and 2010 voting groups are 1951 and 2998, respectively. These figures are dwarfed by the sample sizes and thereby highly unlikely to exert any serious effect upon our estimates.

We also made several choices regarding the definition of voting modes. Specifically, we classified anyone who voted absentee (code “A”) and whose absentee ballot was not counted (code “B”) as having voted absentee. We classified anyone who voted early (code “E”) and anyone cast a provisional ballot early (code “F”) as having voted early. Finally, we classify anyone who voted at the polls (code “Y”) and cast a provisional ballot at the polls (code “Z”) as having voted on election day. We do not use the code “P”, which indicates that an individual cast a provisional ballot that was not counted since we cannot ascertain whether it was cast on election day, early, or as an absentee voter.

Another difficulty with the data is defining the eligible electorate and thus individuals who did not vote. While the voter history files have a code “N” for did not vote, most individuals who do not vote are not present in the voter history files at all. For example, for the 2008 election there were no “N” codes at all in the voter history files. Therefore, we count an individual as not having voted in a given election if they appeared in the voter registration files at one point but are either not present in the voter history file for that election or are coded as “N”.

Table 1: Voting modes as percent of population in 2006, 2008, and 2010 elections. Note that percentages of individuals who did not vote, whose provisional ballots were not counted, or who are dropped due to conflicting voting modes are not shown.

Race	2006			2008			2010			Total
	Early	Absentee	Election Day	Early	Absentee	Election Day	Early	Absentee	Election Day	
Asian	2.87	2.61	12.21	15.15	10.45	20.43	4.75	5.69	13.76	233664
Black	2.81	1.85	16.86	27.67	7.14	16.97	6.41	4.24	18.57	2159473
Hispanic	2.46	2.83	12.46	15.05	8.60	22.72	4.11	6.01	13.37	2049683
White	5.89	5.60	23.16	14.48	13.57	25.32	7.39	9.05	20.63	11179293
Other	2.60	2.43	11.97	13.67	8.12	19.31	3.87	4.19	12.26	749612

Table 2: Compliance as percent of voting groups in 2006-2008, 2008-2012, and 2010-2012 transitions. Note that percentages of individuals who did not vote, whose provisional ballots were not counted, or who are dropped due to conflicting voting modes are not shown.

Race	2006-2008			2008-2012			2010-2012		
	Early	Absentee	Election Day	Early	Absentee	Election Day	Early	Absentee	Election Day
Asian	55.43	12.14	27.59	37.48	8.50	12.68	57.16	9.12	24.65
Black	71.53	18.66	53.70	49.12	12.35	19.48	67.91	15.05	43.65
Hispanic	56.70	8.98	29.50	33.18	6.81	11.02	53.03	7.31	21.63
White	53.87	9.19	22.98	40.54	7.69	12.33	56.69	7.11	20.41
Other/Unknown	55.42	10.43	28.10	37.15	7.33	11.06	57.43	8.28	24.50