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A Propensity Score Reweighting Approach to Estimating the Partisan Effects of Full Turnout in American Presidential Elections

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Borrowing an approach from the literature on the economics of discrimination, we estimate the impact of nonvoters on the outcome of presidential elections from 1952–2000 using data from the National Election Study (NES). Our estimates indicate that nonvoters are, on average, slightly more likely to support the Democratic Party. Of the 13 presidential elections between 1952 and 2000 we find no change in the eventual outcome of the election with two possible exceptions: 1980 and 2000. Thus our results are not all that dissimilar from other research on participation. Higher turnout in the form of compulsory voting would not radically change the partisan distribution of the vote. When elections are sufficiently close, however, a two percentage point increase may suffice to affect the outcome. Limitations of the NES data we use suggest that our estimates underestimate the impact of nonparticipation. We also compare our method with other econometric techniques. Finally, using our findings we speculate as to why the Democratic Party fails to undertake widespread "get out the vote" or registration drives.

1 Introduction

There has been a considerable amount of attention paid to the link between turnout and the ability of parties to win elections. Given the significantly lower levels of participation in American elections relative to other advanced Western democracies, this link is perhaps even more important in the study of American politics. Indeed, Lijphart (1997, p. 1) regards unequal participation as "democracy's unresolved dilemma." Higher turnout is important both as an "intrinsic good" and in terms of representation and therefore policy outputs. Others find the problem of low turnout less important. That is, if election outcomes with 50% turnout yield identical results to those with 100% turnout, then the

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Table 1 An example of the potential effects of turnout on election outcomes

	Potential voters		*Actual voters*	
Group	*Percent of population*	*Participation*	*Democrat*	*Republican*
Poor	80	20	75	25
Rich	20	90	11	89

Note. Entries represent percentages.

"problem" of low turnout vanishes. If, however, turnout biases election outcomes in one direction, then there is all the more reason to be concerned.

In this paper we use an easy-to-implement semiparametric method to test the counterfactual "what would happen if everyone voted" in presidential elections for the period 1952–2000. Simply put, our method infers how 28-year-old white nonvoting women from Vermont would have voted by studying the behavior of 28-year-old white women from Vermont who did vote.

Our statistical approach is, of course, not the only method available to try to answer this question. In this paper we discuss another methodological approach to this problem and compare it with our reweighting approach. In a recent paper Citrin et al. (2003) use the Blinder/Oaxaca method to estimate changes in Senate election outcomes. We utilize a weighting method based on propensity scores for presidential elections between 1952 and 2000.

After formally developing the aforementioned methods, we discuss the relationship between the two approaches and related literatures on estimation of average treatment effects, estimation of counterfactual densities, random coefficient models, and Heckman "selection bias" frameworks. Finally, we apply a reweighting method to presidential races from 1952 to 2000 by using data from the American National Election Study to estimate the partisan effects of full turnout in American elections.

Results show that, by adding the imputed preferences of nonvoters to actual presidential election returns, the Democratic Party would be marginally advantaged. While the impact of this increase in the two-party share of the vote varies from year to year, typically the difference is not sufficient to change the election outcome. The two possible exceptions to this generalization are the elections of 1980 and 2000.

The article proceeds in the following fashion: Section 2 describes our statistical model. Section 3 discusses the link between the methods proposed in this paper and the methods proposed by others. Section 4 presents the results of our analysis. The final section concludes and speculates further about the link between parties and participation.

2 Methodology

2.1 *Overview of Procedure*

Before developing the technique formally, it may be useful to first give a more intuitive and broader overview (see Table 1). It will also be helpful in relating to other techniques common in the literature. It has been noted in the literature that the methodology we discuss more formally below has a simple analog in the case in which the relevant characteristics are categorical. For simplicity, assume that there are are two parties

(Democrats and Republicans) and only two types of voters—poor and rich—and suppose the following:

In this simple example, there are more poor people than rich. Poor voters tend to vote Democratic, but the poor are less likely to vote than the rich. A straightforward calculation verifies that the Republicans win the election with 59% of the vote compared with 41% of the vote for the Democrats.

We are now interested in answering the question, "What would happen if everyone showed up?" for an election. We have an actual distribution of voters—53% are poor and 47% are rich, and we know their voting propensities. We are interested in the distribution of votes that would obtain under the counterfactual that everyone votes. The problem with assuming that the outcome would be the same in the 100% turnout case is that the observed sample puts the same weight on both poor and rich—it gets the weights wrong. In particular, since the poor are less likely to vote, our counterfactual requires that we put more weight on poor voters and less on rich voters. As we explain below, the correct weights for this exercise are proportional to (1/0.2 = 5) for poor persons and (1/0.9 = 1.1) for the rich: the weights are inversely proportional to the likelihood of voting. This of course suggests that an election in which everyone shows up will result in a higher vote share for Democrats than in the actual election.

In the case in which all the variables are continuous, the extension is straightforward but becomes increasingly cumbersome and the problem of potentially small cells grows more important. The complexity also grows larger if the number of outcomes is greater than two.

2.2 *Formal Development*

Our method to estimate the effect of 100% voter participation is an adaptation of the method discussed in DiNardo et al. (1996) to the present context; the weighting procedure we implement was first proposed in different contexts by Horvitz and Thompson (1952) and Rosenbaum and Rubin (1983). In what follows we briefly review the approach in this setting and describe the necessary modifications. Additional detail can be found in DiNardo et al.

Without loss of generality, we can consider the case in which the underlying variable is continuous instead of categorical.[1] The simplest way to proceed would be to postulate two equations for underlying voter preferences (w), which are a function of characteristics observed by the statistician (X) and unobserved characteristics ϵ that are not observed by the statistician and are uncorrelated with X. We can then imagine two outcome equations. The first relates voter preferences to covariates and a stochastic disturbance term for nonparticipants (denoted by the superscript n)—i.e., the voting preferences of nonvoters. The second equation relates voter preferences to covariates for participants (denoted by superscript p)—i.e., the preferences of those who actually vote:

$$w^n = X^n \beta^n + \epsilon^n, \tag{1}$$

$$w^p = X^p \beta^p + \epsilon^p. \tag{2}$$

The simplest interpretation of this setup is that when the X's include exogenous demographic variables that are related to economic outcomes, then individual preferences are based on their economic position, although other interpretations are possible. For this

[1]The symmetry with the case in which some of the variables are categorical is explained in more detail in Biewen (1999).

approach to make the most sense we require that there is some relationship between a person's demographic characteristics and his preference over candidates. For simplicity, we have described a situation in which a single index characterizes preferences. Such would be the case, for example, if the U.S. political scene were well described by an underlying left/right continuum. This is not required for what follows.

Regarding the error terms, the most sensible base case, and the one we use in this paper, is to assume that *conditional on the X's*, the equation that determines voter preferences is the same for both groups (i.e., voters and nonvoters). However, it is possible that there are unobservables that are related to voter preferences that are also correlated with participation.[2] Given the fairly limited range of variation in actual participation behavior, it is difficult to see how one could verify this reliably with data, absent a great deal more (implausible) structure or assumptions (Heckman 1990). This is the most significant limitation of this framework or any framework that is unable to utilize credibly exogenous variation in voter participation. However, if our *X*'s are sufficiently variable and they capture a large share of the nonidiosyncratic variation in voting behavior, the extent of this bias would necessarily be minimized. While it is possible, in principle, to make progress on the case in which there are unobservable differences between voters and nonvoters, the NES data are not well suited to this purpose and we leave this for future research.

The other limitation of the approach is its reliance on self–reported voting behavior. A well–known disadvantage of the NES is that reported participation is significantly higher than actual participation and that conditional on voting, the NES overpredicts the vote for the winner. The consequences of these biases on our estimates are analogous to the biases in switching models with imperfect sample separation information—our counterfactual outcomes will be biased toward the winner (Lee and Porter 1984). However, we suspect that demographic data reported by respondents are likely to be more reliable than non-voters' responses to the question "Who would you have voted for?" Moreover, notwithstanding the fact that looking at respondents' reports to specific issue questions potentially suffers from the same sort of response bias as do our survey data on presidential choice, an issue-by-issue focus will tend to understate the importance of estimates of the effect of nonparticipation unless they can be optimally combined.

Given this formulation, the simplest way to proceed is to estimate a voter preference model such as a logit or probit (or their multivariate extensions) for just the voters and use the coefficients from Eq. (2) to predict the behavior of nonvoters. The tally from the complete set of predictions would be the appropriate counterfactual. Indeed, as described, this is the well–known "Blinder/Oaxaca method" (Blinder 1973; Oaxaca 1973), which has found extensive use in the courtroom and elsewhere as a tool for analyzing wage discrimination (Ashenfelter and Oaxaca 1987). The method we employ here is an extension of this procedure and is better adapted to an analysis of the effect of non-participation when there are more than two choices (i.e., more than two candidates).

We begin with the definition of conditional probability that yields the following representation of the overall distribution of voter preferences:

$$g(w) = \int f(w \mid x)h(x)dx. \tag{3}$$

When the conditional expectation is linear in the x's, $f(w \mid x)$ is closely related to the regression function.

[2]See DeNardo (1980).

Because the counterfactual distribution we wish to generate involves combining distributions with different types of voters, it will be helpful to establish notation for the observed distributions that incorporate this.

The density of preferences for voters is given by

$$g_p(w \mid t = p) = \int f^p(w \mid x)h(x \mid t = p)dx, \tag{4}$$

where $f^p(w \mid x) \equiv f(w \mid x, t = p)$. This expression merely indicates that the conditional density of voting preferences for participants can be recovered by integrating the conditional density of preferences given the covariates x over the conditional distribution of x among voters. As before, in the special case in which the conditional expectation and the linear projection are the same, $f^p(w \mid x)$ is closely related to the regression function (for preferences) or the suitable binary choice model.

Suppose we wish to compute the density of preferences for nonvoters, or what would happen if only nonvoters voted. That is, we are interested in

$$g_p(w \mid t = n) = \int f^p(w \mid x)h(x \mid t = n)dx. \tag{5}$$

As is turns out, estimation of the above density can be made simple by noting that Bayes's Law implies

$$h(x) = \frac{h(x \mid t = p)Pr(t = p)}{Pr(t = p \mid x)} \tag{6}$$

$$h(x) = \frac{h(x \mid t = n)Pr(t = n)}{Pr(t = n \mid x)}. \tag{7}$$

We do not observe Eq. (5) but we can easily compute it. Using Eqs. (6) and (7) we can rewrite Eq. (5) as

$$g_p(w \mid t = n) = \int f^p(w \mid x)h(x \mid t = p)\frac{Pr(t = n \mid x)}{Pr(t = p \mid x)}\frac{Pr(t = p)}{Pr(t = n)}. \tag{8}$$

Note that Eqs. (8) and (4) are similar up to a factor ϕ where

$$\phi = \frac{Pr(t = n \mid x)}{Pr(t = p \mid x)}\frac{Pr(t = p)}{Pr(t = n)}.$$

That is, the distribution of voter preferences of nonvoters is equal to the distribution of voter preferences appropriately weighted. We can ignore the term $\frac{Pr(t=p)}{Pr(t=n)}$, which is a constant for all observations and therefore does not change the relative weighting of the data. Note that the weight is a monotone increasing function of the probability that one's characteristics "look" like those of a nonvoter. Simply put, if nonvoters are poorer than voters, a reasonable base estimate of the voting shares is merely a reweighted average of the voters' preferences with poor people given more weight.

Our method is to simply estimate these weights with flexible functional forms. A simple nonparametric estimator could be calculated by merely dividing the data into cells, although there is little gain relative to simple parametric binary choice models.

We choose a logit and estimate the probability of voting with independent variables such as age, schooling, percentile in income distribution, etc. One advantage of the logit relative to the probit in this setting is that the logit forces the sum of the predicted weights to equal the empirical probability in the sample, a feature not shared by the probit. As a practical matter, as long as one allows the x variables to enter the model flexibly, the actual binary choice model matters little. Once one has computed the weights, any software package that can compute weighted means can trivially compute the counterfactuals.

Similar logic establishes that to get the distribution of voting preferences if everyone voted requires a slightly different weight, ϕ':

$$\phi' = \frac{1}{Pr(t = p \mid x).} \tag{9}$$

Computation of the standard errors by either resampling or parametric bootstrap is straightforward.

By way of comparison with the other literature, although similar in spirit to calculations used in Teixeira (1992; see especially pp. 91–92), his method can be viewed as a special case of ours in which the set of variables included in the nonvoting logit equation is extremely limited, typically using one or two demographic attributes at a time, say either class or race.

2.3 *An Illustration*

Given the more formal development above, it will be instructive to return to our earlier simple example. For illustration purposes we will calculate the weight in Eq. (9) and apply it to the artificial example we developed earlier.

Using sample data above, some simple calculations show that information yields a sample with an overall turnout rate of 34%. We now use our technique in this extremely simple case to illustrate the calculations. The discussion above has suggested applying the weight ϕ' to a sample of voters to generate the appropriate counterfactual—what would happen if everyone turned up. In this case, we need only manipulate the predicted value from a logit with participation as the dependent variable and class as the single covariate. Specifically our weight is merely the inverse of the predicted probability from a simple logit. The appropriate logit that results is

$$\text{votes} = \Lambda(\alpha + \beta\ rich),$$

where "votes" and "rich" are simple dichotomous variables that take a value of 1 if the person votes or is rich, respectively, and 0 otherwise. The resulting estimated equation is

$$\Lambda^{-1}(\widehat{\text{votes}}) = -1.39 + 3.58\ \text{rich} = (0.027) + (0.080).$$

The calculations are easy because in this example there are only two predicted probabilities. The predicted probability for rich persons is 0.9—the population proportion of rich who turn out to vote. The predicted value for the poor person is 0.2. Again from Eq. (9) the weight is just the inverse of these predicted probabilities—5 for the poor person and 1.1 for the rich—just as in our simple example earlier. These weights are then applied to the sample of voters to yield the desired counterfactual estimate.

2.4 *A Comparison with Citrin et al.*

In a recent published study, Citrin et al. (2003) demonstrate that nonvoters are slightly more likely to vote for a Democratic candidate than for a Republican in recent U.S. Senate elections. However, they also find cases (states) in which nonvoters are, on average, more likely to vote for Republicans. Furthermore, they also show that the partisan effects of full turnout differ by year as well as by state. When certain assumptions are satisfied, the econometric approach used by Citrin et al. (2003) will yield asymptotically equivalent answers to ours. However, in some contexts our approach will be more convenient. The key assumption our approach shares with Citrin et al. 2003 is that "selection into voting" is solely a function of *observables*—specifically, voters are different from nonvoters only in ways observable by the econometrician. That is, as will become clear below, we assume that the relationship between voting outcomes and individual characteristics is the same for both voters and nonvoters. While such an assumption may or may not be correct, given the fact that we do not observe the choices of nonvoters, it is difficult to imagine an empirically verifiable estimate that proceeds without such an assumption. Under stringent conditions that do not obtain in the case we examine, it would be "theoretically" possible to obtain a valid estimate that does not make this assumption. Moreover, to the extent that participation depends on the choices (or lack thereof) facing voters, absent significant variation in this aspect of the process and credible estimates of the impact of choices on participation, the best one can do, it would appear, is to proceed as we do.

3 Weighting, Regression Prediction, and the Propensity Score

In this section we present a nontechnical discussion of the relationship between two "different" ways of calculating counterfactuals and take a particular special case to show when they are numerically identical. *Inter alia* we make two points:

1. The propensity score reweighting method proposed by Horvitz and Thompson (1952) and Rosenbaum and Rubin (1983) is a special case of the counterfactual density estimation proposed by DiNardo et al. (1996).
2. Blinder/Oaxaca, DiNardo et al., and propensity score methods (including matching) are all examples of what might be called the "selection on observables" framework, which are special cases of the approach pioneered by Heckman that might be called "selection on unobservables" (or, more correctly, selection on observables and unobservables).

Average treatment effects (Rosenbaum and Rubin 1983) are a specific counterfactual in the Blinder (1973) and Oaxaca (1973) framework (see, for example, Hirano et al. 2000) and, as has been noted in this literature, can often conveniently be estimated by reweighting. The Blinder/Oaxaca framework is a special (easy) case of the more general Heckman framework (see citations below).

3.1 *The Most General Framework and Caveats*

For simplicity, it will be helpful to consider the case of a simple randomized trial in which we are interested in a continuous outcome, y, which is related to one categorical characteristic and a treatment variable T − (0 if control and 1 if treatment).

Because the randomization insures the basic orthogonality condition $E[T'\epsilon] = 0$, the simplest (consistent) estimator of the effect of the treatment might be an OLS regression of the following sort:

$$y_i = \alpha + \beta T_i + \epsilon. \tag{10}$$

Note that if the β is truly a constant for all observations, then providing T is "as good as random" and the OLS estimate (the difference between the mean of the treatment and control groups) is a consistent estimate of the treatment effect. In this simplest of frameworks, there is no distinction between types of treatment effects, since they are the same for all individuals. Moreover, as a consequence of random assignment, the introduction of additional covariates will not affect the probability limit of the OLS estimator of β.

Let us now introduce the possibility that the outcome possibly varies with some characteristic X. As will be clear below, the method in our paper and in papers by Citrin et al. (2003) are all special cases of the following model:

$$y_i = \alpha_i(X_i) + \beta_i(X_i)T_i + \epsilon, \tag{11}$$

where we have let the constant term α_i and the slope coefficient β_i vary with X_i. This is just a general way to write down a random coefficient model (see Wooldridge 2002 for a discussion or Wooldridge 1997 for an application to instrumental variables; see also the excellent discussion in Card 1999). Note that although the coefficients vary, they vary with respect to *observables* only. That is, if two persons have the same values of X, in this framework (henceforth selection on observables) they have the same treatment effect.

Note that the model in Eq. (10) is just a special case of this model in which the coefficients are identical for all individuals, regardless of X. The key assumption of this more general framework—unconfoundedness in the language of Rosenbaum and Rubin 1983—is that the heterogeneity in the treatment effect is reflected in the fact that β is now a random variable and quite importantly ε_i is assumed to be independent of assignment to the treatment. In particular, assignment to treatment is assumed to depend only on the observables.

To understand the implication of this fact, consider the case in which the observables can be partitioned into a small discrete number of "types." Under the assumptions, we can apply simple OLS as in Eq. (10) to each subgroup—the coefficients in this case are constant within types.

However, if individuals "select in" to treatment on the basis of factors not observed by the econometrician, an important literature, pioneered by James Heckman, shows that the proposed estimators can be seriously biased. Intuitively, selection on observables is about averaging the difference between treated and untreated individuals who are identical in all observable respects save the treatment across individuals so paired. However, if we observe two individuals who are identical in a long list of observables, save the fact that one receives the treatment and the other does not, in many contexts it is not appropriate to assume that the individuals are identical in ways not observed by the econometrician.

Using similar notation as above, a simple version of the Heckman selection framework can be written as follows:

$$y_i = \beta_i^0(x_i, \epsilon_i) + \beta_i^1(x_i, \epsilon_i)T_i + \epsilon_i. \tag{12}$$

Viewed this way, the selection on observables framework is just a special case of the Heckman selection framework. Put somewhat too simply, estimation in this framework requires knowledge of the selection process. If, for example, people are rational, and people who benefit most from a treatment are most likely to take it, it is clear that the simple estimator given by Eq. (10) will not be informative about the likely effect of the treatment on those who have chosen not to take the treatment. To make the analogy to voting, if those who vote do so because they receive a benefit from voting, then we would expect voters to be different in both observable and unobservable ways from nonvoters. Absent intimate knowledge of the selection process and the relationship between unobservable determinants of voter participation and their relationship to voter preferences, it would be impossible to use data to uncover the relationship either in a selection framework or with instrumental variables. For some recent work, see Heckman and Vytlacil (2001a, b) and Heckman et al. (2001).[3]

Henceforth we will not focus on the more general problem posed by Heckman and instead deal with the selection on the observables only.

3.2 *Blinder/Oaxaca Counterfactuals—The Citrin et al. Method*

Consider the basic setup given by Eq. (11):

$$y_i = \alpha_i(X_i) + \beta_i(X_i)T_i + \epsilon.$$

The method used by Citrin et al. (2003) is known in the labor economics literature as Blinder/Oaxaca and it involves parameterizing the conditional expectation or mean of y so that the estimating equation corresponding to Eq. (11) is of the following simple form:

$$y = X\beta_0 + TX\beta_1 + \epsilon,$$

where TX is the matrix formed by multiplying each column of X by T (i.e., the interaction of T and X), or equivalently:

$$\begin{aligned} y &= X\beta_0 + \epsilon \quad \text{if } T = 0 \\ y &= X\beta_1 + \epsilon \quad \text{if } T = 1. \end{aligned}$$

In this case, we have allowed our coefficients to vary in a *limited way* with X—one set of coefficients relating the X to the outcome if one is treated, and another set of coefficients relating X to the outcome if one is not treated. To specialize to the question of interest in this paper, the treatment under consideration is voting: What would the outcome of an election look like if everyone voted? In this case, we model the conditional mean function of the outcome (say, a vote for the Republican candidate) using just the data on voters. We then apply these coefficients to both voters and nonvoters. To wit:

$$\frac{1}{N}\left\{\sum X_i\hat{\beta}_1\right\} = \frac{1}{N}\left\{\sum_{i\in T=0} X_i\hat{\beta}_1 + \sum_{i\in T=1} X_i\hat{\beta}_1\right\} \tag{13}$$

$$= \frac{1}{N}\left\{\sum_{i\in T=0} X_i\hat{\beta}_1 + \sum_{i\in T=1} y\right\}. \tag{14}$$

In the last line, we are making use of the fact that we are running an OLS regression that will equal the actual mean within the sample. (This would not necessarily be true, for

[3]For further discussion of the evaluation problem in this more general framework, see Winship and Morgan (1999), Heckman et al. (1996, 1999), Heckman and Hotz (1985), and Heckman and Robb (1985).

instance, if we were to use a probit, but it would be true if we used a logit and it is true for OLS regression generally.)

It will be useful to write the population analog of this estimate (when the conditional mean function is correct, i.e., the regression function is correctly specified) as

$$\int yf^1(y \mid x)dx = t\int yf^1(y \mid x)h(x \mid T=1)dx + (1-t)\int yf^1(y \mid x)h(x \mid T=0)dx, \tag{15}$$

where y is the outcome, $f^1(y \mid x)$ is the conditional density of y given covariates x among the "treated" (voters) integrated over the values of x for voters ($T=1$) and nonvoters ($T=0$), and t is the proportion treated.

3.3 *Reweighting for Estimates of the Average Treatment Effects*

DiNardo et al. (1996) and propensity score methods address the same issues as Blinder/Oaxaca, but it will be necessary to go into a bit of detail about the average treatment effect (ATE). The reader should keep in mind that the basic issues are the same; the only substantive difference is that in the ATE literature one is interested in the difference between two counterfactuals; the question of "what would happen if everyone voted" requires only one.

The simplest treatment effect would simply be the difference in means between the control and treatment groups, which is merely the OLS estimate of Eq. (10). However, we want to consider the case in which the treatment may vary among observables. In this case we can consider many different types of average effects: the effect of the treatment on those who take the treatment, the effect of the treatment if everyone took the treatment, etc.

Rosenbaum and Rubin (1983) and others define the ATE as

$$\text{ATE} = E[y \mid T=1] - E[y \mid T=0]. \tag{16}$$

Consider the evaluation of a randomized experiment in which $T=1$ if the person gets the treatment and zero otherwise. Again, let the outcome y depend on some covariates so we have the distribution of outcomes in the treatment group and the distribution of outcomes in the control group ($T=0$):

$$\int f^{T=1}(y)dy = \int f^1(y \mid x)h(x \mid T=1)dx \tag{17}$$

$$\int f^{T=0}(y)dy = \int f^0(y \mid x)h(x \mid T=0)dx. \tag{18}$$

In that literature, the focus of much attention has been on ATE, which is merely the treatment effect averaged over all types, given by x:

$$\text{ATE} = \int yf^1(y \mid x)h(x)dx - \int yf^0(y \mid x)h(x)dx. \tag{19}$$

In general, the simple difference between the treatment and control group means will not be the average treatment effect. Denote this simple difference in means as the "usual":

$$\text{Usual} = \int yf^1(y \mid x)h(x \mid T=1)dx - \int yf^0(y \mid x)h(x \mid T=0)dx. \tag{20}$$

The key is that, in general, $h(x \mid T = 1) \neq h(x)$ and likewise for the control group so that the estimates in Eqs. (19) and (20) will generally not be equal.

From the perspective of DiNardo et al. (1996), ATE is actually the difference between two counterfactuals:

1. The average outcome using the "structure of outcomes under treatment" *as if* the treatment had been given to both the treatment and the controls (i.e., the general population).
2. The average outcome using the "structure of outcomes under control" *as if* the control had been given to both the treatment and the controls (i.e., the general population).

Using the same trick as before, we can reweight the usual estimator so that both means are averaged over the entire population and take the difference. Using the definition of conditional probability and Bayes's rule as in DiNardo et al., we see that each term on the right-hand side of Eq. (19) is merely a weighted version of the actual means of the treated and control groups:

$$\text{Actual Treatment Mean} = \int y f^1(y \mid x) h(x \mid T = 1) dx \tag{21}$$

$$\text{"CF" Treatment Mean} = \int y f^1(y \mid x) h(x) dx \tag{22}$$

$$= \int \left(\frac{P_1}{\rho(x)}\right) f^1(y \mid x) h(x \mid T = 1) dx \tag{23}$$

$$\text{Actual Control Mean} = \int y f^0(y \mid x) h(x \mid T = 0) dx \tag{24}$$

$$\text{"CF" Control Mean} = \int y f^0(y \mid x) h(x) dx \tag{25}$$

$$= \int \left(\frac{P_0}{1 - \rho(x)}\right) y f^0(y \mid x) h(x \mid T = 0) dx \tag{26}$$

$$\text{ATE} = \int \left(\frac{P_1}{\rho(x)}\right) f^1(y \mid x) h(x \mid T = 1) dx \tag{27}$$

$$- \int \left(\frac{P_0}{1 - \rho(x)}\right) y f^0(y \mid x) h(x \mid T = 0) dx \tag{28}$$

$$\widehat{\text{ATE}} = \sum_{i \in T} \hat{\omega}_i^T y_i - \sum_{i \in C} \hat{\omega}_i^C y_i, \tag{29}$$

where P_1 is the fraction of treatment observations and P_0 is the fraction of control observations, and where

$$\hat{\omega}_i^T = \frac{P_1}{\hat{\rho}^T(x)}$$

and

$$\hat{\omega}_i^C = \frac{P_0}{1 - \hat{\rho}^T(x)},$$

where $\hat{\rho}^T x$ is the predicted probability from a binary dependent variable model and the dependent variable is equal to one when the observation is from the treatment group and zero otherwise. If the $\hat{\omega}$ do not sum to one over their respective samples, they should be normalized so that they do.

Indeed, the weighting estimator proposed by Rosenbaum and Rubin (1983) and Hirano et al. (2000) is this estimator.

3.4 *Effect of the Treatment on the Treated (TOT)*

Likewise, the same type of estimator can be used to estimate other outcomes. One might be interested, for example, in the effect of the treatment on the treated:

$$\text{Actual Treatment Mean} = \int yf^1(y \mid x)h(x \mid T=1)dx \tag{30}$$

$$\text{Actual Control Mean} = \int yf^0(y \mid x)h(x \mid T=0)dx \tag{31}$$

$$\text{CF Control Mean} = \int yf^0(y \mid x)h(x \mid T=1)dx \tag{32}$$

$$\begin{aligned}\text{TOT} &= \int yf^1(y \mid x)h(x \mid T=1)dx \\ &\quad - \int yf^0(y \mid x)h(x \mid T=1)dx \qquad (33) \\ &= \int yf^1(y \mid x)h(x \mid T=1)dx \\ &\quad - \int \omega' yf^0(y \mid x)h(x \mid T=0)dx, \qquad (34)\end{aligned}$$

where

$$\omega' = \frac{\rho(x)}{1-\rho(x)}\frac{P_0}{P_1} \tag{35}$$

and as before, the term $\frac{P_0}{P_1}$ is merely a constant and the weight can be renormalized to sum to one.

That is, the effect of the treatment on the treated is merely the treatment mean less the reweighted control group mean.

3.5 *Other Remarks*

The only difference between the framework proposed by DiNardo et al. and by the literature on ATE and related work is the object of interest. In the propensity score reweighting literature, only the mean is estimated. As DiNardo et al. and Barsky et al. (2002) point out, however, once the weights are estimated, one can estimate either the entire distribution of the outcome or specific moments of the distribution, such as the mean, median, etc. Blinder/Oaxaca, which focuses exclusively on conditional mean functions, can be used to analyze means but is less well suited for other moments of the distributions.

A pertinent question at this point is, when are the methods equal and what are the advantages and disadvantages?

When the object of interest is merely the mean, if the conditional mean function (Blinder/Oaxaca) is correctly specified and the propensity score is correctly specified (DiNardo et al. and ATE), the methods produce the same answers (see Hirano et al. 2000).

One special case that can be worked out (albeit tediously) is that in which individuals can be grouped into a finite number of "types" on the basis of their covariates, in which case it is easy to ensure that one has the correct model.[4] That is, as long as the regression model is saturated (the effect is allowed to differ for every type) or the binary choice model is also saturated, hence estimates separate probabilities for each type, then Blinder/Oaxaca, ATE, and DiNardo et al. decomposition amount to reweighting differences in group means and the answers will be numerically identical.

Why, then, should one method be preferred over the other? Essentially practical considerations are the most important, although Hirano et al. (2000) show that, in the context of estimating ATE, propensity score reweighting and DiNardo et al. methods are somewhat more efficient asymptotically. When the object of interest is purely the mean, all three methods are equally as easy and will be appropriate, although some authors (Olson 1998) have argued that the DiNardo et al./propensity score reweighting approach is sometimes useful if the conditional mean is more difficult to estimate correctly than the propensity score equation. Because the Tobit is particularly sensitive to mild departures from the ideal assumptions (see Deaton 1997 for a nice discussion), Olson (1998) argues that it is sometimes not well suited for Blinder/Oaxaca-type analysis; propensity score methods may be preferable if the the likelihood of severe misspecification for the underlying binary choice model is less than the likelihood of misspecification in the Tobit.

Related arguments for preferring reweighting are also made by Barsky et al. (2002). In some cases, both methods can be combined: for a particularly nice exposition see Lemieux (2002). Another case, in which reweighting seems more straightforward, is that of categorical outcomes—the reasoning is the same as in Olson 1998.

4 Results

4.1 *Turnout in American Elections*

Conventional wisdom in modern American politics holds that higher voter turnout helps the Democrats. However, the applicability of this conventional wisdom has been called into question in the political science literature. The current common wisdom in American politics is that while nonvoters may look like Democrats demographically, there is no reason to expect them to vote for the Democratic Party if indeed they do vote (DeNardo 1980). In the face of the evidence regarding the small differences between voters and nonvoters, the conventional wisdom among political scientists is that given voters' alternatives, the differences between those who vote and those who do not is not large enough to have consequences for electoral outcomes (Cambell et al. 1960; Wolfinger and Rosenstone 1980; Tucker et al. 1986; Teixeira 1992). DeNardo (1980, p. 409) considers the effect of "news and propaganda rain[ing] down on voters in such prodigious quantities as to reach even the outermost periphery of the electorate." He argues that the "peripheral" voters identified by the American Voter study (Cambell et al. 1960)—voters who respond primarily to the "brouhaha that surrounds a campaign"—are more likely to defect (not vote for the candidate of the party with whom the individual is [weakly] allied). If peripheral voters make up a larger share of Democratic support, increased turnout will work against the Democratic candidate.

While low levels of voter turnout are not a new phenomenon in American electoral politics, we have witnessed a marked decline in turnout since the 1960s (Teixeira 1992).

[4]See DiNardo (2002) for a tedious demonstration.

With participation hovering at roughly 50% of eligible voters in a presidential election, one begins to wonder why more people do not vote and, further, what would happen if they did. If the ideological variety of viable presidential candidates is not too circumscribed and nonvoters differ sufficiently from voters in terms of their preferences, the outcomes of presidential elections will be affected by this nonparticipation. The prima facie evidence suggests that the effect of 100% turnout would lead to election outcomes more favorable to the Democratic Party. The average nonvoter has a lower socioeconomic status, and individuals with lower socioeconomic status are more likely to support the Democratic Party (Piven and Cloward 1988). Previous research on the preferences of nonvoters has found that, although they are not systematically different from one another across all policy issues (Wolfinger and Rosenstone 1980; Verba et al. 1995) and although nonvoters are significantly more liberal on spending and welfare policies, as well as being more egalitarian in general (Bennett and Resnick 1990), the inclusion of nonvoters in the electoral process would not significantly affect the makeup of government.

4.2 *The Partisan Effects of Higher Turnout*

As we outlined above, our first step is to run a logit predicting whether or not the respondent voted. Appendix A (available on the *Political Analysis* Web site) lists the variables we use in our equation. Using those results we reweight the votes for president for all respondents in each year. Table 2 presents the results of our analysis for the presidential elections from 1952–2000. We report actual turnout and what we call NES turnout, which is simply the number of respondents who self-reported having voted. Next, we report the percentage of the respondents who reported voting for the Democratic candidate in the survey. The next column shows the hypothetical percentage of the vote that the Democratic candidate would have received had everyone voted in the election. The final four columns report similar data for Republicans as well as votes for third parties in each election.

In 1952 Dwight Eisenhower handily beat Adlai Stevenson. Tabulating the results from the respondents in the NES data, 57.9% reported voting for Eisenhower and 41.8% for Stevenson. After the inclusion of nonvoters the outcome would have been 54.5% to 45.3%, an increase of nearly three and a half percentage points for the Democrat.[5] For all 11 elections analyzed here, the Democrats would be advantaged with the addition of nonvoters. The degree to which they would have been advantaged varies from year to year, ranging from a low of 0.18% in 1956 to a high of 6.08% in 1980. The average increase for the Democratic candidate over these 13 elections is 2.3%, with a standard deviation of 1.59. Thus our methods indicate that with all eligible voters casting a vote the Democrats would reap a modest electoral benefit.

Depending on the election, a 2.3% increase may be completely meaningless, as in 1984, or it could easily be decisive, as in the 2000 election. Our estimates indicate that Al Gore would have picked up 2.2% of the total vote, which would almost certainly have guaranteed him a victory.

Interestingly, the third parties rarely make significant gains in our estimates for 100% turnout. While this could conceivably be a function of the lower number of votes that third-party candidates garner in the first place, even in 1992 when Ross Perot received 18.5% of the votes (NES), his campaign would have benefited by only roughly a tenth of

[5]As we noted earlier, because the NES surveys typically indicate a higher percentage of votes from the winner, our results will be skewed. However, this fact will typically skew our results towards the winner, which in most cases is the Republican. Thus our estimates for those years in which a Republican won will be conservative.

Table 2 Effects of 100% turnout

Year	*Actual[a] Turnout*	*NES Turnout*	*NES Democrat*	*Simulation Democrat*	*NES Republican*	*Simulation Republican*	*NES Other*	*Simulation Other*
1952	61.6%	65.2%	41.8%	45.5%	57.9%	54.3%	0.2%	0.2%
				(2.1)		(2.1)		(.14)
1956	59.3	68.5	40.2	40.4	59.5	59.4	0.3	0.2
				(1.6)		(1.6)		(.1)
1960	62.6	69.5	49.3	50.2	50.0	49.0	0.7	0.7
				(1.6)		(1.6)		(.3)
1964	61.9	67.8	67.4	69.2	32.4	30.6	0.2	0.2
				(1.2)		(1.3)		(.1)
1968	60.9	65.6	40.9	42.0	47.6	45.0	11.5	13.0
				(1.5)		(1.6)		(1.2)
1972	55.4	61.8	35.3	36.6	63.6	62.2	1.1	1.2
				(1.1)		(1.1)		(.3)
1976	54.4	62.4	49.7	52.4	48.4	45.8	1.9	1.8
				(1.2)		(1.2)		(.5)
1980	53.4	63.0	39.4	45.5	50.8	45.3	9.8	9.2
				(3.5)		(2.9)		(1.1)
1984	53.3	63.2	41.4	45.5	57.7	53.6	0.9	0.9
				(1.6)		(1.7)		(.3)
1988	50.1	62.1	46.6	48.5	52.3	50.3	1.2	1.2
				(1.5)		(1.5)		(.4)
1992	55.2	66.2	47.6	48.7	33.9	32.7	18.5	18.6
				(1.2)		(1.3)		(.9)
1996	49.1	66.1	52.9	55.6	38.3	34.4	8.8	9.9
				(1.5)		(1.4)		(1.0)
2000	49.3	64.5	50.6	52.8	45.5	42.8	3.9	4.4
				(1.7)		(1.7)		(.8)

Note. Standard errors in parentheses.
[a]Source for actual turnout is Ornstein et al. (1997).

a percentage point. Given the fact that those who tend not to vote are less likely to be identified with a particular party, this finding is somewhat unexpected.

Given the problem of overreporting in the National Election Survey, there are some elections in which the survey "validated" voters to try to mitigate this problem. We have rerun our models presented in Table 1 using only validated voters as actual voters, rather than everyone who self-reported being a voter. The elections in which this validation process occurred in our sample are 1964, 1976, 1980, 1984, and 1988. Results are presented in Table 3. The differences between the models in Tables 2 and 3 are typically trivial—the simulation results for percent voting Democratic and Republican differ by less than one percentage point in all years with the exception of 1976, when the differences were roughly 1.5 percentage points. This leads us to conclude that our results are robust even in the face of the self-reporting problems inherent in NES data.

5 Discussion

We have developed a simple method of estimating presidential election outcomes when everyone participates in an election. Using a semiparametric model borrowed from the economics of discrimination we can easily obtain a reasonable estimate of the effect of

Table 3 Effects of 100% turnout using only validated voters

Year	*NES Democrat*	*Simulation Democrat*	*NES Republican*	*Simulation Republican*	*NES Other*	*Simulation Other*
1964	66.91	69.91	32.84	30.00	0.24	0.14
		(1.50)		(1.50)		(.09)
1976	47.41	54.64	50.63	43.59	1.91	1.76
		(1.56)		(1.52)		(.44)
1980	37.85	45.90	52.06	45.00	10.09	9.10
		(2.24)		(1.92)		(1.11)
1984	41.04	46.27	57.95	52.85	1.01	0.88
		(1.72)		(1.68)		(.30)
1988	47.04	48.66	51.86	50.10	1.10	1.25
		(1.67)		(1.69)		(.35)

Note. Standard errors in parentheses.
[a]Source for actual turnout is Ornstein et al. (1997).

complete turnout on the popular vote percentage for both of the major party candidates. We have also compared the reweighting approach used in this paper with the Oaxaca/ Blinder approach used in Citrin et al. (2003). One major advantage to our approach is that testing these counterfactuals is easily generalizable to elections with three or more candidates. Our results show that the low levels of turnout in American elections create a (generally small) electoral bias against the Democratic Party for presidential elections. The bias is typically two to three percentage points, which is usually insufficient to overturn a presidential election, in large part because most presidential elections are won by more than a percentage point or two. The two exceptions that stand out in the data are Carter's 1980 reelection bid and Al Gore's very narrow loss in 2000.

While we have demonstrated that the Democratic Party may be the beneficiary of higher vote totals with full turnout, it is not immediately transparent that it would be in the interest of the party establishment to invest more in "getting out the vote."

First, at best we have established that given the types of Democrats "available to voters," increases in voter turnout would likely result in better electoral outcomes for these types of Democrats. Moreover, we have focused on a counterfactual of 100% turnout and the net effect for presidential elections is modest, because the election outcome would necessarily have to be one in which a Democratic candidate narrowly loses to a Republican in order for these differences to change the outcome.

Second, while the Democrats would clearly benefit sometimes and even a small swing in the nationwide vote in favor of the Democrats might mean a nontrivial increase in the number of Congressional seats that they would win, the net effect is relatively small given the strictly prohibitive cost of increasing turnout to complete, or near complete, levels given the types of Democrats available to voters. This is the likely explanation for why the political parties do not concentrate much time and money trying to mobilize habitual nonvoters.[6]

Third, for reasons discussed in the political economy literature on democracy, the interests of the Democratic establishment and the largest potential pool of Democratic

[6]Getting nonvoters to become voters is even more difficult because prior to election day one needs to get nonvoters registered. One then needs to find these newly registered folks again on election day and get them to the polls.

voters may not be identical. Therefore acts that induce more turnout (either directly or indirectly by creating choices that induce more turnout), while beneficial in the short run, can have detrimental long-run consequences by altering the distribution of power between the interests represented by the Democratic establishment and the largest potential pool of Democratic voters.[7]

Finally, a substantial increase in the number of left-leaning voters would likely elicit a response from the Republican party. Namely, acting strategically, the GOP could nominate and run candidates with more moderate policy positions.

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[7]For a formal game theoretic treatment of a related argument, see Acemoglu and Robinson (2003).

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