Polit Behav (2007) 29:1–30
DOI 10.1007/s11109-006-9022-5

ORIGINAL PAPER

An Alternative Approach to Estimating Who is Most Likely to Respond to Changes in Registration Laws

Michael J. Hanmer

Published online: 21 February 2007

Abstract Scholars often seek to understand which individuals are most responsive to the change in some treatment. Such work inevitably faces issues of identification. When the dependent variable is binary, the assumption that the largest effect occurs where $p = 0.5$ is also encountered. I apply Manski's [(1995). *Identification problems in the social sciences*. Cambridge: Harvard University Press] non-parametric Bounds approach, which relaxes the functional form and distributional assumptions found in traditional models, in an attempt to resolve the long standing debate on which types of individuals are most affected by changes in registration laws. Under the standard assumption that treats the selection of registration laws as exogenous, the results revise the current understanding. By exploring the power of various behavioral assumptions, new insights into the study of policy changes emerge, calling into question some of the assumptions that are standard in the literature.

Keywords Voting · Registration · Election day registration · Treatment effects · Election reform

Two questions are central to all studies of policy change: (1) does the policy have the desired effect?; and (2) if so, on which types of individuals is the policy most effective? One of the most visible and long standing debates regarding which types of individuals are most likely to respond to a policy change can be found in the literature on election day registration (hereafter EDR), a policy that continues to be promoted by scholars and civic groups (see Alvarez & Ansolabehere, 2002; Alvarez, Nagler, & Wilson, 2004).

M. J. Hanmer (✉)
Department of Government, Georgetown University, 656 ICC, Washington, DC 20057, USA
e-mail: mjh72@georgetown.edu

Some have described the unique system of voter registration in the U.S. as the ''linchpin of [our] distorted American democracy'' (Piven & Cloward, 1988, p. 17). It has also been observed that ''registration laws make voting more difficult in the United States than in almost any other democracy'' (Powell, 1986, p. 21). Out of concerns with the status of the U.S. at the bottom of the international turnout rankings and the degree to which the turnout rates of the resource rich exceed the resource poor, registration reform has long been promoted as a mechanism to address these problems (see James, 1987; Piven & Cloward, 1988, 2000). But if registration reform holds the answer to low and unequal turnout, not only must it have an influence on turnout across contexts, it must also have a larger effect on those with low levels of resources. Thus, in order to understand EDR as currently implemented, and inform the development of new policies, it is essential to know which types of individuals will be the most likely to benefit from its implementation. Because the choice of statistical models is critical to the estimated effects, our present understanding is fuzzy at best.

Through the application of a more flexible methodological approach, developed by Manski (1989, 1990, 1994, 1995, 1997), this paper demonstrates how a different approach to inference problems can contribute to both the conceptual and empirical understanding of EDR. Though the approach is applied here to EDR it is suitable when political jurisdictions adopt new policies in areas as diverse as election laws (e.g. early voting), public assistance policy (e.g. welfare to work incentives), health policy (e.g. drug treatment programs), and education (e.g. school vouchers).[1]

The debate surrounding who is most likely to benefit from relaxed registration laws has concentrated on education as the characteristic of interest.[2] Most of the early studies (Highton, 1997; Mitchell & Wlezien, 1995; Nagler, 1994; Teixeira, 1992; Wolfinger & Rosenstone, 1980) suggest that EDR leads to the largest turnout gains among those with the lowest levels of education. Nagler (1991) challenges Wolfinger and Rosenstone's results, but his own 1994 article revises his original claims; and though he argues that the mechanism differs, Nagler (1994) arrives at the same substantive conclusion as Wolfinger and Rosenstone (1980). More recently, Brians and Grofman (1999, 2001) and Huang and Shields (2000) have provided an additional claim, arguing that high school graduates are the most responsive to EDR. At present, favor seems to have swung toward this more recent conclusion (Highton, 2004).

[1] Though the most common examples can be found in studies of governmental policy changes, the general concern is not limited to this line of research. For example, the same general concern arises in observational studies of which types of individuals are most susceptible to various campaign tactics or media messages.

[2] Because the theoretical relationship of interest exists at the individual level (i.e. individuals make the decision to vote or abstain) and the data are readily available at the individual level, the literature I address here uses individual level estimates of voter turnout. Several scholars (see for example Calvert & Gilchrist, 1993; Jackson, Brown, & Wright, 1998; Martinez & Hill, 1999; and Knack & White, 2000) utilize data aggregated at the county or state levels and work from a slightly different perspective. For references to studies that do not investigate the consequences across individual or group traits, see Traugott (2004)

In the context of the current debate that treats the selection of registration laws as exogenous, the results presented here suggest a revision.

The most recognizable problem with the strand of the literature considered here is that the models commonly used to study whether an individual voted or not, logit and probit, impose the assumption that the largest effect is on those with initial probabilities of voting equal to 0.5. When one of the research questions includes learning where the effect is the largest, it is preferable to use methods that are flexible and allow one to estimate where this occurs rather than use methods that assume where the effect is the largest. This point was first articulated by Nagler (1991), who, by introducing the political science literature to the scobit model in Nagler (1994), took up his earlier call to "test [this] important, but currently untested, behavioral assumption" (Nagler, 1991, p. 1403). Unfortunately, (as is discussed further below) for those seeking to resolve this issue, scobit has not lived up to expectations (Achen, 2002; Hanmer, 2002, 2006). As a result, it remains necessary to apply a more flexible methodology that can test successfully the $p = 0.5$ assumption. Manski's (1989, 1990, 1994, 1995, 1997) non-parametric Bounds approach, the methodology employed in this paper, allows for such a test. This method relaxes the distributional assumptions imposed by logit, probit, and scobit as well as all functional form assumptions, thus removing concerns with model specification common to all parametric approaches, but particularly relevant here (see Huang & Shields, 2000; Nagler, 1991). Additionally, the approach makes all assumptions explicit, and provides for a straightforward test of the assumptions imposed by traditional methods (Manski, Sandefur, McLanahan, & Powers, 1992; Pepper, 2000).

Through the application of the Bounds approach, this paper contributes in the following ways. Ultimately, it shows that when relaxing functional form and distributional assumptions but maintaining the standard identification assumption, in the states that first implemented EDR the least educated are expected to see the largest gains in turnout under a change to EDR, thus revising the current understanding. However, when applying the same assumptions to the states that recently adopted EDR, the least educated do not appear to have benefited more from EDR than their more educated counterparts. Overall, when using the Bounds approach to test the functional form and distributional assumptions from a standard probit model, the assumptions maintained by probit cannot be rejected. The importance of this contribution lies in the provision of a proper test of this assumption and the confidence one gains in the traditional methods that impose it. Because the Bounds approach dictates that analysis begins with minimal assumptions and proceeds by explicitly layering on stronger assumptions, the power of various, often implicit, identification assumptions must be confronted. Perhaps the most significant contribution derives from this process that exposes the meaning and consequences of behavioral assumptions that are all too often glossed over. Thus, this paper provides new insights into our understanding of the existing turnout literature and a foundation upon which additional research can build.[3] More generally, the paper contributes by demonstrating the value of the Bounds approach for political science research.

After discussing the standard assumptions and the hypotheses to be tested, I begin the analysis of the effect of EDR on the probability of voting using data from the 1980, 1996, and 2000 Current Population Survey: Voter Supplement Files without making assumptions about the process that determines the selection of election laws and turnout. However, for the 1996 and 2000 elections I make a distinction between EDR as implemented in the states that were the first to adopt it (ME, MN, ND, and WI) and those that did so more recently (ID, NH, and WY). I refer to the early adopting states as mature EDR states and the recent adopters as new EDR states. The separation of EDR is made to account for the distinctive reasons and processes that led to the adoption of EDR and the possibility that the effect is dependent on the length of time the program was in effect; whereas the early adopters implemented EDR on their own, ID, NH, and WY did so in order to avoid implementing the National Voter Registration Act of 1993 (Hanmer, 2004). By doing this, I allow the overall effects and the pattern of results across education categories to vary by the context in which the policy was adopted. I then demonstrate the power of various behavioral assumptions. Next, I use the Bounds approach to estimate who is most sensitive to EDR. Because the approach reveals that assumptions weaker than those commonly utilized are insufficient to perform a test of the $p = 0.5$ assumption, this test is necessarily located toward the end of the paper when stronger assumptions are investigated. When estimating who is most sensitive to changes in registration laws, I characterize individuals based on the demographic characteristic for which theoretical expectations are most clear and that the literature has given the greatest amount of attention-educational attainment. I then compare these estimates to those from a probit model.

Commonly Held Assumptions

The $p = 0.5$ Assumption

Unfortunately, most empirical work proceeds without recognition of the power of the full set of behavioral and methodological assumptions employed. By and large, previous studies use probit or logit models and thus implicitly assume that the largest impact will be on those with an initial probability of

[3] As with all methods, the Bounds approach is not without limitations. Most importantly, the approach as applied to presidential elections requires that one use cross-sectional data; that is, in the absence of panel data there are too many confounding factors that could not properly be controlled for to employ a design based on a natural experiment. However, when extrapolating to other contexts, even those studies designed around a natural experiment (e.g. Brians and Grofman, 1999; Fenster, 1994) jump immediately to the strongest identification assumptions. The Bounds approach is unique in that it offers a series of weaker identification assumptions, does not impose any functional form or distributional assumptions, and makes testing the functional form and distributional assumptions straightforward.

voting equal to 0.5. For this purpose, this assumption is clearly problematic; who is most sensitive to changes in election laws is something that one would want to estimate, not assume. As a possible alternative to logit and probit, Nagler (1994) employed the scobit model and also chose registration laws as the motivating example. While scobit maintains the assumption that the distribution is unimodal (a potentially problematic assumption), the model is intuitively appealing as it relaxes the assumption that the largest impact of a change in an independent variable occurs where the initial probability of success is equal to 0.5. However, scobit has been found to be unreliable in small samples (Hanmer, 2006) and especially sensitive to specification errors (Hanmer, 2002; see also Achen, 2002). Because the scobit estimator fails to live up to its task, estimating which types of individuals are most sensitive to changes in stimuli, it is still unclear which types of individuals will be the most responsive to changes in registration laws.[4] With the method applied here, the $p = 0.5$ assumption can be tested.

Exogenous Selection to Solve the Identification Problem

When using individual level data, one observes the voting outcome for individuals in the context in which they live, but does not observe how individuals would have acted in another context. That is, the identification problem arises. Thinking in terms of treatment effects, each individual receives only one of several possible treatments, here one treatment would be living in a state that allows EDR and another would be living in a state that does not allow EDR. Because the data are inherently censored, more of the same type of data would not eliminate the identification problem. Manski (1995) discusses and evaluates how most researchers deal with the identification problem

> The conventional practice is to invoke assumptions strong enough to identify the exact value of [the parameters of interest]. Even if these assumptions are not plausible, they are defended as necessary for inference to proceed. Yet identification is not an all-or-nothing proposition. Weaker and more plausible assumptions often suffice to bound parameters in informative ways (8).

In the literature on the effect of EDR, in order to obtain point estimates for the effect of EDR researchers impose the exogenous selection assumption, i.e. the effect of EDR found in one context is straightforwardly extrapolated to other contexts. Following the tradition established by Rosenstone and

[4] The small sample problems found with scobit are less of a concern in Nagler's (1994) empirical example which uses a data set containing almost 100,000 individuals. However, specification errors are highly influential even in this very large sample, with small variations in the model specification leading to substantively different conclusions regarding where the largest impact occurs (Hanmer, 2002; see also Achen, 2002). Monte Carlo simulations corroborate the finding that scobit estimates are especially sensitive to model specification (Hanmer, 2002; see also Achen, 2002). To the best of my knowledge, in the more than 10 years since Nagler (1994) introduced the scobit model to the political science literature, it has not been used by empirical researchers in any published work.

Wolfinger (1978), to estimate the effect of EDR using individual level data[5] one usually proceeds in four steps: (1) run a logit, probit, or scobit model on a binary dependent variable indicating whether an individual citizen of voting age voted or not; (2) set the values of the variables to indicate that EDR applies for all and calculate a predicted probability of voting for each individual; (3) set the values of the variables to indicate that EDR does not apply to anyone and calculate a predicted probability of voting for each individual; and (4) subtract the result obtained in step (3) from the result obtained in step (2) to obtain an estimate of the effect of EDR. *Implicit in this procedure is the imposition of the exogenous selection assumption.* Alternatively, some choose to compute predicted probabilities for ''typical'' individuals. Here, rather than calculating predicted probabilities for each individual, the values of the independent variables are set to their means or medians and the probability of voting assuming all have EDR is compared to the probability of voting assuming EDR does not apply to anyone; *again, the exogenous selection assumption is built into this process*.

As demonstrated below, the Bounds approach makes clear what the traditional approach conceals: in order to identify point estimates of the effect of EDR, one must assume that election laws can be treated as if they are assigned randomly. Without prior information, one cannot reject the exogenous selection assumption. Though assessing fully the credibility of this assumption is beyond the scope of this paper, by examining a range of weaker identification assumptions this paper serves to initiate dialogue on this issue.

Although it has not been discussed in the literature, the procedures used to produce predicted probabilities from a probit or logit model can influence the results. An example will illustrate the point. Since Wolfinger and Rosenstone (1980) and Brians and Grofman (1999, 2001) use CPS data, do not include interaction terms, and use models that impose the $p = 0.5$ assumption, the $p = 0.5$ assumption cannot be responsible for the different conclusions these scholars derive. Whereas Wolfinger and Rosenstone (1980) study the effect of EDR by calculating predicted probabilities for each individual, Brians and Grofman (1999, 2001) examine the effect of EDR based on predicted probabilities for a ''typical'' individual. Given the representativeness of the CPS sample and the concern with describing predicted behavior in the U.S., the calculation of predicted probabilities for each individual has a clear theoretical advantage to procedures that limit their attention to just a subset of the sample, usually white, males, in their 40s.[6] An additional advantage of the Bounds approach is that, given any identification assumption, there is only one

[5] As noted earlier, my focus here is on studies that investigate the effect of EDR on individual level behavior and that examine the effects across individual characteristics. Thus, this discussion does not apply to studies such as those cited in footnote 2 or other studies that use aggregate data, such as Fenster (1994) who works from the framework of a natural experiment.

[6] Moreover, in some cases the estimates are quite sensitive to changes in the values chosen to represent the typical individual. Appendix Table A4 uses the model from Brians and Grofman (1999) to show that the substantive conclusions change when the ''typical'' individual is a female rather than a male.

way to calculate the treatment effect (as shown below the method corresponds most closely to the aggregation of individual predictions).

Theoretical Expectations

Because relaxed registration laws lower the cost of voting, EDR is expected to have a positive effect on the probability of voting (Downs, 1957). Though there is disagreement regarding the magnitude of the effect, previous research generally supports this expectation (see Traugott, 2004 for a review). With respect to for whom the effect is the largest, I examine three competing hypotheses. The first hypothesis is derived from Wolfinger and Rosenstone (1980) who contend that education provides individuals with skills and experience that should make registering to vote easier for those with more education. Thus, the least educated are the most likely to benefit from easier registration procedures, mainly because they are less able to overcome more difficult hurdles.[7] Second, Brians and Grofman (1999, 2001) maintain that the largest impact should be found among those who have moderate levels of interest and skills and need just a small reduction in costs to get them involved. Here, it is crucial to note that their model does not necessarily predict that the largest effect will be on those who have an initial probability of voting equal to 0.5. When summing up the predictions generated from their model, Brians and Grofman (1999) hypothesize that "groups that are 'in the middle' with respect to [socioeconomic status] variables...should be obtaining the greatest turnout gains when registration barriers are lowered by the adoption of election day registration" (p. 165). That is, the model makes predictions about where one lies within the distribution of demographic characteristics and *not* the distribution of voting probabilities.[8] The final hypothesis is that registration laws do not differentially affect some types of citizens more than others.

Data and Methods

Using data from the 1980, 1996, and 2000 Current Population Survey: Voter Supplement Files (hereafter CPS) I apply the Bounds approach to provide non-parametric estimates of the probability that an individual will vote under several treatments; the dependent variable is binary and coded as 1 for citizens age 18 or older who reported voting and 0 for citizens age 18 or older who

[7] Though they do not bring data to bear on the issue, Piven and Cloward (1988, 1989, 2000) hold firm to the belief that the least well off stand the most to gain by the reduction of barriers to registration.

[8] Of course, these may be related (though this is an empirical question) but the fact remains that a theory that directly links the largest impact of registration reform to a voting probability of $p = 0.5$ is absent from the literature. Moreover, to test properly a theory that suggests the greatest impact is at $p = 0.5$, one cannot use a method that assumes that this is the case.

report that they did not vote. For 1980 there are two treatments, living in a state with EDR or not;[9] while for 1996 and 2000 there are three treatments, living in a state with mature EDR, living in a state with newly implemented EDR, and living in a state that does not allow EDR.[10] The 1980 data are used to provide baseline estimates of the effect of EDR.[11]

Prior to specifying the equations to be estimated, several definitions are needed. Each individual can be characterized by values for the following (y_1, y_0, t, x). The variable t is a binary variable that indicates which treatment each individual receives.[12] For those living in states that allow EDR, t is set to 1 and for those living in states that do not allow EDR $t = 0$. When studying 1980, for those living in the states of Maine, Minnesota, North Dakota, Oregon, and Wisconsin $t = 1$ and for those living in all other states $t = 0$. For 1996 and 2000, two sets of comparisons are made: (1) between those living in states with mature EDR[13] (for whom $t = 1$) and those in states without any EDR (for whom $t = 0$); and (2) between those living in states with newly implemented EDR (for whom $t = 1$) and those in states without any EDR (for whom $t = 0$).[14] The variable x represents the covariates of interest that describe an individual. The variable y_1 is defined as the voting outcome if an individual were to live in a state with EDR; y_1 takes on a value of 1 if such individuals were to vote and $y_1 = 0$ otherwise. The voting outcome if an individual were to live in a state that does not allow EDR is represented by the variable y_0; y_0 takes on a value of 1 if such individuals were to vote and $y_0 = 0$ otherwise. Due to the identification problem, for each individual only one of the voting outcome variables, y_1 or y_0, is observed. One observes y_1 for those who actually live in states that allow EDR and one observes y_0 for those who live in states that do not allow EDR. That is, when $t = 1$ one observes a value for y_1 and when $t = 0$ one observes a value for y_0.

[9] For the 1980 election ME, MN, ND, OR, and WI are coded as having election day registration. While ND does not have a registration system, like those in states with election day registration, in order to vote, eligible citizens do not need to perform administrative tasks prior to election day. Removing ND does not alter the conclusions.

[10] For the 1996 and 2000 elections ME, MN, ND, and WI are coded as having mature EDR programs and ID, NH, and WY are coded as having newly implemented EDR programs. Due to an incident in which the system was exploited, Oregon eliminated EDR in 1985.

[11] Unfortunately, the 1976 CPS does not have a variable with a unique state identifier. The 1980 election is the first presidential election after EDR was adopted in ME, MN, and WI, for which individual level CPS data are available by state. Analyses using data from other presidential election years (1984, 1988, and 1992) do not substantively alter the conclusions.

[12] I explain the approach presented in Manski (1995) in the context of EDR and turnout, with living in a state with EDR as the treatment and turnout as the outcome. The discussion is easily generalized for any treatment and outcome.

[13] While ME and MN also had versions of motor voter in 1996 and 2000, the effect of this program is not thought to be responsible for differences in turnout between EDR and non-EDR states as analyses excluding these two states are consistent with those presented here (Hanmer, 2004).

[14] In order to simplify the discussion that follows, I temporarily ignore the distinction between mature EDR and newly implemented EDR. Taking the distinction into account is straightforward; simply replace all references to EDR with references to mature EDR or newly implemented EDR.

From these definitions the equations of interest can be specified, thus exposing the consequences of the identification problem. The probability that an individual with covariates x will vote under EDR is represented by $P(y_1 = 1| x)$. Using the law of total probability, $P(y_1 = 1| x)$ is specified as follows:

$$P(y_1=1|x)=P(y_1=1|x,t=1)*P(t=1|x)+P(y_1=1|x,t=0)*P(t=0|x). \quad (1)$$

The data reveal the following components of Eq. 1: $P(y_1 = 1| x, t = 1)$ is the probability of voting for those with covariates x living in EDR states; $P(t = 1| x)$ is the probability of living in an EDR state given covariates x (the selection probability); and $P(t = 0| x)$ is the probability of living in a state that does not allow EDR, given covariates x (the censoring probability). But the counterfactual probability, $P(y_1 = 1| x, t = 0)$, the probability of voting if EDR were available for those who do not receive the EDR treatment is *not* revealed by the data. That is, because the data are inherently censored they do not reveal how those living in states that do not allow EDR would behave if they were to live in a state that allows EDR.

The probability that an individual with covariates x will vote under the treatment that an individual lives in a state that does not allow EDR, $P(y_0 = 1| x)$, can be defined similarly:

$$P(y_0=1|x)=P(y_0=1|x,t=0)*P(t=0|x)+P(y_0=1|x,t=1)*P(t=1|x). \quad (2)$$

Data will reveal: $P(y_0 = 1| x, t = 0)$ as the probability of voting for those with covariates x who live in states that do not have EDR; and $P(t = 1| x)$ and $P(t = 0 | x)$ as they were defined above. However, the data do *not* identify $P(y_0 = 1| x, t = 1)$, the probability of voting in the absence of EDR for those who actually receive the EDR treatment.

Without prior information, $P(y_1 = 1| x)$, the probability of voting if EDR was in effect for all individuals, and $P(y_0 = 1| x)$, the probability of voting if EDR was not an option for anyone, cannot be identified, and thus the classical treatment effect (hereafter CTE) defined as

$$P(y_1 = 1|x) - P(y_0 = 1|x) \quad (3)$$

is also not identified. As noted above, most studies, including all of the empirical work cited from the literature on EDR, begin by imposing assumptions that will serve to identify the counterfactual probabilities and thus $P(y_1 = 1| x)$, $P(y_0 = 1| x)$, and the CTE. The logic of the Bounds approach, however, suggests that one begin by assuming nothing at all about the processes that determine the selection of registration laws and turnout (more generally, selection into the treatment and the outcome of interest).

Even without assuming anything about the processes that determine the selection of registration laws and turnout, we know that the counterfactual probabilities, $P(y_1 = 1|\ x,\ t\ =\ 0)$ (the probability of voting with EDR for those in states that do not currently allow EDR), and $P(y_0 = 1|\ x, t\ =\ 1)$ (the probability of voting without EDR in states that currently allow EDR), must lie in the interval [0, 1]. Using this information, worst case bounds for Eqs. 1 and 2 can be estimated. To calculate the lower bound for Eq. 1 the counterfactual probability, $P(y_1 = 1|\ x,\ t\ =\ 0)$, is set to 0; to calculate the upper bound the counter factual probability is set to 1. That is, the lower bound is derived by proceeding as if none of those who lived in states that did not allow EDR would have voted had they instead lived in states that allow EDR and the upper bound is derived by proceeding as if all of those who lived in states that did not allow EDR would have voted had they instead lived in states that allow EDR. Similar logic applies for the calculation of the bounds on (2). The worst case bounds are defined as follows: For (1),

$$\begin{aligned} P(y_1 = 1|x, t = 1) * P(t = 1|x) &\leq P(y_1 = 1|x) \\ &\leq P(y_1 = 1|x, t = 1) * P(t = 1|x) + P(t = 0|x). \end{aligned} \tag{1a}$$

And for (2),

$$\begin{aligned} P(y_0 = 1|x, t = 0) * P(t = 0|x) &\leq P(y_0 = 1|x) \\ &\leq P(y_0 = 1|x, t = 0) * P(t = 0|x) + P(t = 1|x). \end{aligned} \tag{2a}$$

These bounds are functions of quantities identified by the sampling process alone and thus can be calculated without imposing any assumptions on the process that generates y_1, y_0 and t. It is important to note that the width of the bounds in (1a) is equal to the censoring probability, $P(t\ =\ 0|\ x)$, and the width of the bounds in (2a) is equal to the selection probability, $P(t\ =\ 1|\ x)$.

Worst case bounds on the CTE, $P(y_1 = 1|\ x)–P(y_0 = 1|\ x)$, can be found using the following formula:[15]

$$\begin{aligned} &[P(y_1 = 1|x, t = 1) * P(t = 1|x)] - [P(y_0 = 1|x, t = 0) \\ &\quad * P(t = 0|x) + P(t = 1|x)] \leq \text{CTE} \leq [P(y_1 = 1|x, t = 1) \\ &\quad * P(t = 1|x) + P(t = 0|x)] - [P(y_0 = 1|x, t = 0) * P(t = 0|x)]. \end{aligned} \tag{3a}$$

Before moving to the results, a note on how the covariates, x, are used is necessary. In the traditional approach, covariates are used to control for treatment assignment. With the non-parametric Bounds approach, controlling for treatment assignment is not relevant so covariates are used to designate

[15] The lower bound on the CTE is calculated by subtracting the upper bound of (2a) from the lower bound of (1a) and the upper bound on the CTE is calculated by subtracting the lower bound of (2a) from the upper bound of (1a). Once the data are employed, the bound on the CTE, without making any assumptions, necessarily has a width of 1; in the absence of data the CTE would have a width of two and would lie between –1 and 1.

sub-populations of interest. Thus, regardless of the specification of covariates, the following question is well posed: Among individuals with the selected covariates, what would be the difference in the probability of voting if all such individuals were assigned one treatment rather than the other?[16]

Results[17]

Worst Case Bounds

For the sake of simplicity, I begin by examining the overall effect of EDR, that is, without conditioning on any covariates. Table 1 presents information revealed by the sampling process for 1980, 1996, and 2000. In each of the years, those who lived in states with EDR (column 1, $P(y_1 = 1|\, t = 1)$) turned out at higher rates than those in states without EDR (column 2, $P(y_0 = 1|\, t = 0)$). In both 1996 and 2000, those in mature EDR states turned out at the highest rates followed by those in states with newly implemented EDR. Table 1 also reveals that the proportion of people who lived in states with EDR (column 3, $P(t = 1)$), either mature or newly implemented, was quite small; as mentioned above, this will have important consequences for the width of the bounds.

The worst case bounds for 1980, 1996, and 2000 are reported in Appendix Table A1.[18] Driven by the size of the selection probability, $P(t = 1)$, the bounds on $P(y_1 = 1)$ are extremely wide while the bounds on $P(y_0 = 1)$ are quite narrow.[19] Without making any assumptions, the probability of voting under the EDR treatment lies between 0.03 and 0.99 (depending on the year and maturity of EDR). Obviously, without making any assumptions very little can be said about the probability of voting if all states were to have had EDR. The worst case bounds on the probability of voting if none of the states

[16] Manski and Nagin (1998) provide further explanation: ‘‘Researchers commonly assert that there is some minimal set of ‘correct’ covariates to use in the analysis of treatment effects and that ‘omitted variable bias’ may occur if one conditions on only a subset of these covariates. These statements relate to use of covariates to control for treatment assignment. A set of covariates is said to be ‘correct’ if treatment assignment is random conditional on these covariates; ‘omitted variable bias’ is said to occur if one conditions only on a subset of these covariates and treatment assignment is not random conditional on this subset. We do not assume that treatment assignment is random conditional on ...any...set of covariates. Hence, the concepts of ‘correct’ covariates and ‘omitted variable bias’ are not germane to [the] analysis’’ (p. 107).

[17] The Bounds software is available free from Charles Manski’s homepage: http://faculty.econ.northwestern.edu/faculty/manski/.

[18] The 90% confidence intervals are also reported in Appendix Table A1. The 90% confidence interval for each estimate was computed using the bootstrap method (see Manski, Sandefur, McLanahan, & Powers, 1992; Pepper, 2000). To compute the 90% confidence intervals, first a sample of size N (where N is equal to the size of the sample of interest in the data set) is drawn with replacement. Next, the estimates of interest are computed for this sample. To create a bootstrapped distribution of the estimates, the process is then repeated T times (here T is set to 200). The 0.05 quantile and the 0.95 quantile of the resulting distribution are reported to form the 90% confidence interval.

[19] Those who did not provide an answer of ‘‘yes’’ or ‘‘no’’ to the question asking whether or not the individual voted were eliminated from the sample.

Table 1 Estimated probabilities of voting and residing in a state with EDR (%)

Year and category	Probability of voting in EDR states $P(y_1 = 1 \mid t = 1)$	Probability of voting in non-EDR states $P(y_0 = 1 \mid t = 0)$	Probability of living in an EDR state $P(t = 1)$
1980 (N = 113,123)	76	65	8
1996 Mature EDR (N = 75,116)	71	63	6
1996 New EDR (N = 73,902)	67	63	4
2000 Mature EDR (N = 71,127)	76	67	6
2000 New EDR (N = 69,899)	69	67	4

Source: 1980, 1996, 2000 CPS

allowed EDR are much more informative. For example, in 1980, the probability of voting under the no EDR treatment ranges between 0.60 and 0.68. Having calculated the bounds for $P(y_1 = 1)$ and $P(y_0 = 1)$, using (3a), bounds on the CTE can also be derived. For 1980, the CTE lies between –0.62 and 0.38. These bounds do not allow one to determine the sign of the treatment effect and are not especially informative. They indicate that EDR might reduce the probability of voting by as much as 62 percentage points and might increase the probability of voting by as much as 38 percentage points. It is crucial to note that the interval is continuous so the actual value can lie anywhere within the bounds; a larger negative value does not imply that a negative value is more likely than a positive value.

For each of the years under consideration, given that the selection probability, $P(t = 1)$, is small for each year and scenario the bounds on the probability of voting if all had EDR (mature or newly implemented) are quite large; thus, in the absence of prior information or assumptions, very little about the effect of EDR can be learned. Due to the width of the bounds, comparisons across education categories are not possible as the bounds not only contain positive and negative values but also completely overlap across education categories. Thus, the $p = 0.5$ assumption cannot yet be tested. In order to tighten the bounds, assumptions are necessary. The next section introduces two such assumptions, one that tightens the lower bound (the ordered outcomes assumption), and one that tightens the upper bound (the capped outcomes assumption).

Intermediate Assumptions: Ordered Outcomes and Capped Outcomes

The ordered outcomes assumption (see Manski, 1995, 1997; and Pepper 2000) provides a middle ground between the worst case bounds and the exogenous selection assumption.[20] This assumption implies that EDR can do no harm, i.e. the probability of voting for those in non-EDR states would not decrease if EDR were made available. Having an EDR program available would presumably reduce the costs of voting, and at worst should not deter any of those

[20] Because the primary concern of the paper is what would happen if EDR were adopted more widely, the focus of the remainder of the paper is on $P(y_1 = 1 \mid x)$.

who voted in its absence to abstain in its presence. In this context, the ordered outcomes assumption seems non-controversial as it fits cleanly with the theoretical expectations provided by Downs (1957). Formally, the ordered outcomes assumption implies that: $P(y_1 = 1| x, t = 0) \geq P(y_0 = 1| x, t = 0)$. To compute bounds for $P(y_1 = 1| x)$, set $P(y_1 = 1| x, t = 0)$, the counter factual probability, to $P(y_0 = 1| x, t = 0)$, the realized probability of voting for those in non-EDR states, rather than zero to get the lower bound; the upper bound does not change. By definition the lower bound on the CTE is set to 0, thus this assumption serves to identify the sign of the treatment effect. The CTE upper bound remains the same as in the worst case scenario. This assumption serves to tighten the bounds in a meaningful way, but the lower bound for the entire sample, as well as all sub-groups of interest, is fixed at 0. As a result, the bounds obtained across education categories will necessarily overlap, thus preventing one from determining where the effect is the largest. If one wishes to tighten the upper bound, an additional assumption is required. I now turn to an examination of such an assumption, the capped outcomes assumption.

The states that were first to adopt EDR have a tradition of low barriers to registration and voting and are traditionally high turnout states. Thus, one might contend that if EDR were allowed in states with a social and political context different from that found in states that currently allow EDR, the effect in these new contexts might be lower. The source of the differences might come from unmeasured individual differences, variations in implementation, and/or efforts by groups expecting to be harmed by an expanded or differently composed electorate.[21]

An assumption within the Bounds framework can be constructed to capture this concern. That is, one can reduce the worst case upper bound by assuming that the outcome in new contexts would not exceed the outcome found in current EDR states. Formally, this implies that $P(y_1 = 1| x, t = 0) \leq P(y_1 = 1| x, t = 1)$. I will refer to this assumption as the capped outcomes assumption. Given that this assumption will not change the worst case lower bound, the bounds on $P(y_1 = 1| x)$ will remain relatively wide. However, it stands to reason that if one were to accept this assumption as plausible, he/she would also accept the more innocuous ordered outcomes assumption. When combined, these two assumptions serve to tighten both the lower and upper bounds. Surely, the capped outcomes assumption will spark controversy. It is important to note that my purpose here is not to defend or denounce this assumption. In keeping with the spirit of Manski's approach, this assumption

[21] One might argue that those living in states that have historically made registration difficult might have become so removed from politics that much more than making registration easier will be needed to get them involved. It is also possible that the program could be rendered ineffective due to the failure of elections officials, the media, and political parties to inform the citizenry about the ability to register on election day. Political parties or other organizations might also intervene by placing challengers at the polls to question the eligibility of election day registrants, thus making the process cumbersome.

is demonstrated here because it falls within the realm of plausibility and so that the reader can make up his/her own mind as to its credibility.[22]

Table 2 presents bounds on the probability of voting and the classical treatment effect if all had EDR under the combined ordered and capped outcomes assumptions.[23] The width of the bounds under these two assumptions compared to the worst case bounds is reduced substantially, by as much as 98% for the year 2000 newly implemented EDR scenario. The bounds are reduced by 89% for 1980, by 92% for 1996 mature EDR, by 96% for 1996 newly implemented EDR, and by 91% for 2000 mature EDR. The less controversial ordered outcomes assumption on its own provides the greatest portion of the reduction; the width of the bounds with the ordered outcomes assumption alone is reduced by over 62% in each scenario.

While, by assumption, the sign of the treatment effect is identified, because the bounds on the CTE overlap, these assumptions do not allow one to identify whether the impact of mature EDR is greater than the impact of newly implemented EDR nor whether the impact in mature EDR states declined since 1980. The potential effect under these two assumptions is higher for mature EDR, but as noted above, the estimated effect may lie anywhere within the bounds. Moreover, since the lower bound of the treatment effect remains at zero for all, effect sizes by educational attainment cannot be determined in a manner that would allow one to conclude with any certainty where the effect is the largest. To identify these effects, the much stronger exogenous selection assumption, the assumption implicitly maintained in the literature, will have to be imposed. In the following section, the power of this assumption is explored.

The Exogenous Selection Assumption

It is crucial to keep in mind that in order to obtain identification, the exogenous selection assumption, though it is rarely made explicit, is the most common

[22] Those uncomfortable with this assumption should recognize that the only difference between the probit and Bounds approach on this matter is that when using probit one does this to obtain a point estimate but here this is part of the construction of the bounds. When one estimates the probability of voting if EDR were available everywhere with, say, a probit model, one sets the values of those who did not receive this treatment to the estimated value of those who did receive the EDR treatment. That is, one does not assume that the effect will be less than or greater than that estimated from what was observed in the data from those in the treatment condition but assumes that the effect, if EDR were adopted elsewhere, would be equivalent to the effect observed in the places currently using this policy. In addition, note that any assumption to capture the possibility that the effect would be greater than the effect found in places currently in the treatment condition, in either the bounds or probit set up, would necessarily be arbitrary. That is, rather than using the data, one would have to assume the effect is some fixed amount higher, but such an exercise defeats the entire research enterprise.

[23] Bootstrapped confidence intervals for the results in Table 2 can be found in Appendix Table A2. As Manski and Nagin (1998) note, with sufficiently large samples the identification problem is much more severe than the problem of sampling variation. This can be seen by inspecting Appendix Tables A1 and A2; the estimated bounds are quite large while the confidence intervals around those bounds are only slightly larger.

Table 2 Combined ordered outcomes and capped outcomes bounds if all states allowed EDR (%)

Year and category	Probability of voting if all states allowed EDR		Classical treatment effect	
	$P(y_1 = 1\mid x)$		$P(y_1 = 1\mid x)$–$P(y_0 = 1\mid x)$	
	LB	UB	LB	UB
1980	66	76	0	16
1996 Mature EDR	64	71	0	12
1996 New EDR	63	67	0	6
2000 Mature EDR	67	76	0	13
2000 New EDR	67	69	0	5

Source: 1980, 1996, 2000 CPS

Notes: The ordered outcomes assumption tightens the lower bound, while the capped outcomes assumption tightens the upper bound

LB indicates the lower bound and UB indicates the upper bound

assumption used in work on treatment effects. With respect to the literature addressed here, all previous scholarship has maintained this assumption. The exogenous selection assumption states that $P(y_1 = 1|x) = P(y_1 = 1|x, t = 1) = P(y_1 = 1|x, t = 0)$ and $P(y_0 = 1|x) = P(y_0 = 1|x, t = 0) = P(y_0 = 1|x, t = 1)$. For $P(y_1 = 1\mid x)$, one assumes that if EDR became available to all, the probability of voting for those who did not receive the EDR treatment would be the same as the probability of voting among those who did receive the EDR treatment. Similar logic applies for $P(y_0 = 1\mid x)$. This assumption serves to identify $P(y_1 = 1\mid x)$, $P(y_0 = 1\mid x)$, and the CTE. The CTE can now be calculated as the difference between $P(y_1 = 1\mid x)$ and $P(y_0 = 1\mid x)$. That is $CTE = P(y_1 = 1|x) - P(y_0 = 1|x) = P(y_1 = 1|x, t = 1) - P(y_0 = 1|x, t = 0)$.

Estimates derived under the exogenous selection assumption, along with the 90% confidence interval around the estimates are presented in Table 3. With the exogenous selection assumption, point estimates for the probability of voting if EDR were available to all eligible citizens, the probability of voting if EDR were not available anywhere, and the effect of EDR result. The estimated effect of EDR based on those in the states that first adopted EDR is

Table 3 Estimates of the classical treatment effect (CTE) under the exogenous selection assumption, $P(y_1 = 1\mid x)$–$P(y_0 = 1\mid x)$ (with 90% confidence interval) (%)

Year and category	5% Quantile	CTE	95% Quantile
1980	10	11	11
1996 Mature EDR	7	8	9
1996 New EDR	2	4	5
2000 Mature EDR	8	9	10
2000 New EDR	0	2	4

Source: 1980, 1996, 2000 CPS

as high as 11 percentage points in 1980; while the largest effect of newly implemented EDR is only four percentage points. In both 1996 and 2000 the effect of mature EDR is greater than the effect of newly implemented EDR, with the largest gap occurring in 2000. While the impact of mature EDR appears to have diminished only slightly over time, the estimated effect of newly implemented EDR drops substantially from 1996 to 2000. One of the following might help explain the drop for new EDR states: (1) for the new EDR states a novelty effect boosted turnout in 1996; and/or (2) the implementation of motor voter in all non-EDR states over the 1996 to 2000 period boosted turnout in the comparison states more so in 2000 than in 1996. Since mature and newly implemented EDR are compared to the same base, the second explanation seems less plausible as the estimated effect of mature EDR did not decrease from 1996 to 2000.

The difference in the estimates for mature EDR and newly implemented EDR demonstrate the need to examine the effects of the policy across the two sets of contexts separately. The effect of EDR in the new EDR states is lower than that found in mature EDR states (even over 20 years after its initial implementation), thus casting doubt on the assumption that effects of registration laws can be extrapolated across contexts. If, as is commonly done, states that adopted EDR prior to 1980 had been grouped with states that adopted in the 1990s, the effect of EDR for the new EDR states would have been overstated. As I show below, when one examines the effects across different sub-populations the differences become more pronounced.

The Effects Across Education Categories Under the Exogenous Selection Assumption

As demonstrated above, while the ordered and capped outcomes assumptions are quite plausible and reduce the width of the bounds considerably, in order to obtain point estimates that can be compared across categories, the exogenous selection assumption must be employed. If one is willing to accept the exogenous selection assumption, or if one thinks of studying the effect in terms of the best case scenario under the combined ordered and capped outcomes assumption, the Bounds approach allows one to estimate who will be most sensitive to changes in registration laws without the distributional and functional form assumptions implicit in logit and probit models.

Table 4 presents estimates of the classical treatment effect under the exogenous selection assumption and 90% confidence intervals for each year and EDR treatment by education category.[24] A number of patterns are

[24] Because the covariate values are discrete, cell means were used. Treating the covariate as continuous and estimating the bounds with the Gaussian kernal and Silverman's (1986) rule of thumb bandwidth produced identical results. See Hardle (1990) for a discussion of non-parametric regression estimators.

Table 4 Estimates of the classical treatment effect by education category assuming exogenous selection

Category	1980	1996 Mature EDR	1996 New EDR	2000 Mature EDR	2000 New EDR
0–8 years	14	11	–5	19	–3
90% confidence interval	12 17	6 16	–13 3	12 25	–12 7
9–11 years	12	10	5	14	–1
90% confidence interval	9 15	6 14	0 10	9 18	–6 6
High School degree	10	7	2	8	1
90% confidence interval	9 12	5 9	0 4	6 10	–2 3
1–3 years college	9	7	4	9	3
90% confidence interval	7 10	5 9	2 7	7 11	1 5
College degree	8	8	7	6	4
90% confidence interval	6 10	5 10	4 10	4 8	1 6
College+	5	5	1	6	3
90% confidence interval	2 7	2 8	–3 5	4 8	–1 6

Source: 1980, 1996, 2000 CPS

noteworthy. Concentrating first on 1980 and the mature EDR estimates for 1996 and 2000, the Table reveals that the effect of EDR is the largest for those with the least amount of education and drops with each additional level of educational attainment. For 1980 and 2000, the confidence intervals for those with 0–8 years of education do not overlap with those with a high school degree or more, suggesting that these differences are statistically significant. While for mature EDR in 1996 the point estimates diminish with education, the confidence intervals all overlap, thus the evidence is not strong enough to conclude that the effect of EDR differed by education. Turning to EDR as implemented in Idaho, New Hampshire, and Wyoming (1996 new EDR and 2000 new EDR columns), the patterns in the states first to adopt EDR are not replicated. It appears that the least educated in the new EDR states did not take advantage of easier registration provisions; the effect of new EDR for the least educated is non-existent.[25] In both 1996 and 2000, the effect of new EDR is the highest among those who have college degrees. However, the confidence intervals around these estimates are relatively wide, impeding the ability to draw firm conclusions.

If one is willing to accept the exogenous selection assumption, then the results in Table 4 reveal that the effect of EDR across education categories is not uniform across contexts. Previous studies have not only imposed the exogenous selection assumption, but have made assumptions regarding the form of the probability of voting. Furthermore, these studies have not separately evaluated the impact of mature and newly implemented

[25] While the estimated effect is negative in several cases, the relatively wide 90% confidence intervals reveal that the estimates cannot be distinguished from zero.

EDR by educational attainment; they have either examined EDR in the states that adopted prior to 1980 or have combined mature and new EDR into one category. The Bounds approach results for those in states that adopted EDR prior to 1980 allow for substantive conclusions similar to those from Wolfinger and Rosenstone (1980), Mitchell and Wlezein (1995), Nagler (1994), and Highton (1997), which thus stand in contrast to the results found in Brians and Grofman (1999, 2001) and Huang and Shields (2000). I now turn to a more detailed comparison between results from the Bounds approach and the results from more traditional approaches.

Comparison of Bounds and Probit Results

In addition to providing greater clarity when applying assumptions, the Bounds approach provides a useful tool to assess the validity of the distributional and functional form assumptions employed in traditional models, such as probit (Manski, Sandefur, McLanahan, & Powers, 1992; Pepper, 2000). The test is relatively simple: if voting probabilities estimated using a probit model fall far outside the confidence intervals on the bounds made under weaker assumptions there would be reason to reject the assumptions maintained in the probit model.[26]

Table 5 presents estimates of the probability of voting and the classical treatment effect if all individuals were to have lived in states with EDR. For each of the years and EDR classifications, the table is structured as follows: the first two columns contain the lower and upper bounds, respectively under the combined ordered and capped outcomes assumption; the third column presents the probit model estimates of $P(y_1 = 1| x)$; and the fourth column contains the probit estimate of the CTE.[27]

The probit estimates will be assessed against the Bounds approach estimates based on their closeness to the Bounds approach estimates and the patterns of effects across education categories (see Manski, Sandefur,

[26] As discussed above, the standard procedure for calculating the predicted effect from a probit model incorporates the exogenous selection assumption. The same holds for logit and scobit. Because the bounds and probit results are estimates, the standard concerns with uncertainty apply (see Herron, 2000); thus, when testing the probit results one must consider whether they fall within the respective confidence intervals on the bounds, whether or not the confidence intervals overlap, and if the confidence intervals do not overlap, the magnitude of the differences. The relevant 90% confidence intervals are all reported in Appendix Table A5a and b. The confidence intervals for the probit estimates were also obtained using the bootstrap method.

[27] Both the non-parametric bounds estimates and the probit estimates were generated using the same cross-sectional data from the CPS. The coefficient estimates from which the probit probabilities were generated can be found in Appendix Table A3a–c. These probabilities were produced using the procedure described in Wolfinger and Rosenstone (1980) Appendix C (and summarized earlier).

Table 5 Comparison of estimates of the probability of voting by education if all states had EDR by year and EDR maturity

Category	1980				1996 Mature EDR				1996 New EDR				2000 Mature EDR				2000 New EDR			
	$P(y_1 = 1\|x)$			CTE Probit Model	$P(y_1 = 1\|x)$			CTE probit model	$P(y_1 = 1\|x)$			CTE probit model	$P(y_1 = 1\|x)$			CTE probit model	$P(y_1 = 1\|x)$			CTE probit model
	Ordered and capped outcomes		Probit model		Ordered and capped outcomes		Probit Model		Ordered and capped outcomes		Probit Model		Ordered and capped outcomes		Probit Model		Ordered and capped outcomes		Probit Model	
	LB	UB			LB	UB			LB	UB			LB	UB			LB	UB		
All	66	76	75	9	64	71	71	8	63	67	68	5	67	76	76	9	67	69	70	3
0–8 years	52	65	61	11	46	56	50	9	45	41	47	6	45	62	53	12	43	41	45	4
9–11 years	51	62	65	10	41	51	52	9	41	46	49	6	44	57	56	11	44	43	48	4
High school degree	64	74	74	9	57	64	66	8	57	59	62	6	60	67	70	10	60	60	63	4
1–3 years college	74	82	80	8	69	76	74	8	68	73	71	5	71	80	78	9	71	73	72	3
College degree	85	92	89	6	80	87	85	6	79	86	83	4	84	90	89	6	84	87	84	2
College +	88	93	95	4	88	93	94	3	88	89	93	2	89	96	96	3	89	92	93	1

Source: 1980, 1996, 2000 CPS

Notes: Mature EDR analyses exclude New EDR states and New EDR analyses exclude Mature EDR states

The ordered outcomes assumption tightens the lower bound, while the capped outcomes assumption tightens the upper bound

LB indicates the lower bound and UB indicates the upper bound

CTE stands for Classical Treatment Effect

McLanahan, & Powers, 1992; Pepper, 2000).[28] In addition, the plausibility of the combined ordered and capped outcomes assumption can be evaluated by examining the estimates of the upper bound compared to the lower bound.

For each of the substantively interesting sets of estimates (ordered outcomes (OO)& capped outcomes (CO), $P(y_1 = 1|\ x)$, CTE), there are 35 pairs of bounds and probit estimates. With respect to the OO & CO bounds, in 16 of the 35 comparisons the probit estimates do not fall within the bounds.[29] It is noteworthy that the probit estimates violated the combined OO & CO bounds more so in the new EDR treatment than in the mature EDR treatment. Though violations of the Bounds estimates in 16 of the comparisons may seem problematic for the probit assumptions, in all but 5 comparisons the probit estimates are within the 90% confidence interval on the bounds and in all but 2, the Bounds and probit confidence intervals overlap. In both cases the magnitude of the distance between the confidence intervals is tiny (0.03 and 0.08 percentage points). Thus, the evidence is not sufficient to reject the probit results.

The upper bound under the combined ordered and capped outcomes assumption is equivalent to the estimate of $P(y_1 = 1|\ x)$ under exogenous selection. Here, 12 of the 35 probit estimates fall outside of the 90% confidence interval for the non-parametric estimates of $P(y_1 = 1|\ x)$ made under weaker assumptions. However, just five sets of estimates have non-overlapping confidence intervals. While some judgment is necessary to determine how far "too far" is, in two cases the distance between the confidence intervals is just over 1 percentage point, and in the other three the distance is under 0.5 percentage points, making it hard to argue that the discrepancies are substantively large. While there is some reason for concern, the evidence presented thus far is not sufficiently strong to allow one to reject the probit models.

From a policy perspective, the most interesting comparison is between the Bounds and probit estimates of the classical treatment effect. An examination of the estimated effects of the CTE across education categories casts some doubt on the probit models in the states that recently

[28] Given the size of the selection probability all of the probit estimates lie within the worst case bounds.

[29] For three categories in the new EDR panels, the Bounds OO & CO lower bound is larger than the upper bound. For these categories, the turnout rate in the new EDR states was estimated to be lower than the turnout rate in the comparison states (those without EDR) but in each case, these estimates are very imprecise. That is, in each case the confidence interval around the estimate of the upper bound is quite wide and encompasses the estimate of the lower bound. The confidence intervals for the upper bound (UBCI) in each case follow: (1) UBCI = [31, 49] for 1996 new EDR, 0–8 years; (2) UBCI = [32, 50] for 2000 new EDR, 0–8 years; and (3) UBCI = [38, 49] 2000 new EDR, 9–11 years. This situation is also reflected in Table 4 where the Bounds estimates of the CTE are negative, with confidence intervals that have both negative and positive values. That is, for these cases one would fail to reject the null hypothesis that the effect of new EDR = 0.

adopted EDR. For each year and EDR category, the probit model estimates suggest that the effect of EDR decreases as education increases. That is, the least educated are estimated to be the most sensitive to a change to EDR. This is consistent with the conclusions drawn by Wolfinger and Rosenstone (1980), Mitchell and Wlezein (1995), and Highton (1997). As noted earlier, the estimates from the Bounds approach paint a slightly different picture. For those in states that adopted EDR prior to 1980 the patterns match those from the probit models with only a few exceptions. Yet, in new EDR states the least educated do not seem to gain at all from EDR. If one accepts the exogenous selection assumption, then at least for new EDR states there may be reason to question the functional form and/or distributional assumptions maintained by the probit model. However, when the uncertainty of the estimates is taken into account, the concerns diminish. Although in 10 of the 35 comparisons, the probit estimates do not fall within the 90% confidence interval of the Bounds estimates, in all but two (1996 new EDR 0–8 years of education and 2000 mature EDR college +), the confidence intervals overlap. The distances between the confidence intervals are relatively small, with a distance of just under 1 percentage point for the 1996 new EDR case and a distance of 0.2 percentage points for the 2000 mature EDR case.[30]

Conclusion

As political scientists seek to understand the overall effect of a treatment and/or where the given treatment has its largest effect, the Bounds approach can be included as an additional tool. For those interested in questions that fit into the treatment effect framework, the Bounds approach relaxes functional form assumptions that may cloud the analysis. And when one is concerned that estimates of where the effect is the largest are driven by the distributional assumptions built into the model, this

[30] A series of robustness checks were also performed. The results suggest that the probit estimates are not sensitive to running the models separately for the mature and new EDR states. Since the Bounds approach and traditional approach differ in terms of how they treat covariates, a probit model with education as the only covariate was run so as to make the approaches more comparable, but for probit's functional form and distributional assumptions. Running a probit model with education as the only covariate, does not dramatically alter the number of cases for which the Bounds and probit confidence intervals on $P(y_1 = 1| x)$ do not overlap, but the magnitude of differences does increase substantially. However, with respect to the outcome of greatest interest, the CTE, the number of cases for which the confidence intervals overlap is identical and the magnitude of the differences is relatively small. While this does cast some additional doubt on the probit models and should encourage additional research, the evidence is not strong enough to reject the probit models. The results are reported in Appendix Tables A6–A8.

method can be utilized to relax and even test these assumptions. In addition to providing greater flexibility with respect to functional form and distribution assumptions common in other methods, the Bounds approach allows these and other assumptions to be unearthed and subjected to scrutiny.

This paper has demonstrated that the Bounds approach applies straightforwardly to the study of changes in registration laws and provides useful insights into the standard assumptions applied in this literature. The empirical results show clearly that without making any assumptions, very little, if anything, can be learned about the effect of election day registration. This result is important for the evaluation of the existing literature and as a lesson for future research. If one is willing to impose the exogenous selection assumption, a number of substantive conclusions can be drawn. First, the effect of EDR in states that recently adopted this policy fell short of the effect found in states that were first to adopt EDR. This suggests a more nuanced view should be taken by those seeking to study policy innovations; the same policy should not be assumed to have the same effect across the board. Furthermore, differences in outcomes across contexts become more apparent when sub-populations are considered. In states that adopted EDR prior to 1980, the least educated are estimated to experience the largest gains in turnout, while newly implemented EDR has not had an impact on the least educated. This revises the current understanding of where the effect is the largest. That is, under the exogenous selection assumption, it is not the case that those with moderate levels of education are the most likely to take advantage of EDR. In the case of the early adopters of EDR, the probit assumptions do not appear troublesome, either when EDR was first implemented or decades later. Some suspicion was cast on the probit assumptions when applied to the new EDR states, thus opening an avenue for future research.

In contrast to the approach taken in previous studies, rather than starting by imposing an assumption strong enough to identify the effect of interest, the Bounds approach begins with minimal assumptions and then applies increasingly strong assumptions that serve to provide more informative bounds as well as point estimates. Laying out the assumptions clearly should not only spark discussion regarding the credibility of the assumptions, but should also guide the search for additional information that can be used to evaluate further the assumptions and improve our understanding of the process being studied. Foremost among the topics that have been shown to require additional research is the validity of the exogenous selection assumption. A partial test of this assumption can be found by comparing the separate estimates of EDR in mature and newly implemented contexts. Those who, through the process of scrutinizing these assumptions, come to question the validity of the exogenous selection assumption (or even the weaker assumptions discussed here) are likely to be somewhat disappointed. However, for those in this category, a new challenge has been presented as there is a clear need for additional theoretical and empirical research.

Acknowledgments I would like to thank Chris Achen, Mike Bailey, Matt Beckmann, Adam Berinsky, Marc Busch, Raj Desai, Liz Gerber, Vince Hutchings, Jon Ladd, Anders Olofsgard, Karthick Ramakrishnan, George Shambaugh, Mike Traugott, the editors, and the anonymous reviewers for comments on earlier versions of this paper. I would also like to thank Sean Ehrlich, Cindy Kam, Brian Knight, Charles Manski, Won-ho Park, John Pepper, Chris Seplaki, Clyde Wilcox, and participants at the National Election Studies Fellows Workshop for their advice. I gratefully acknowledge the support of the National Election Studies Research Fellowship program and the University of Michigan, Department of Political Science's Gerald R. Ford Dissertation Fellowship and Research Grant. All errors are my own.

Appendix

Table A1 90% Confidence interval on worst case bounds on the probability of voting

Year	Probability of voting if all states allowed EDR		Probability of voting if no states allowed EDR		Classical treatment effect	
	$P(y_1 = 1\mid x)$		$P(y_0 = 1\mid x)$		$P(y_1 = 1\mid x)$– $P(y_0 = 1\mid x)$	
	LB	UB	LB	UB	LB	UB
1980	5.9	98.1	59.9	67.8	–61.8	38.2
90% confidence interval	5.8	98.1	59.7	68.0	–61.6	38.4
1996 Mature EDR	4.2	98.3	59.6	65.4	–61.3	38.7
90% confidence interval	4.1	98.4	59.3	65.7	–61.0	39.0
1996 New EDR	2.9	98.6	60.6	64.9	–62.0	38.0
90% confidence interval	2.8	98.6	60.2	65.2	–61.7	38.3
2000 Mature EDR	4.5	98.5	62.8	68.8	–64.3	35.7
90% confidence interval	4.4	98.6	62.6	69.1	–64.0	36.0
2000 New EDR	3.0	98.6	63.9	68.3	–65.3	34.7
90% confidence interval	2.9	98.7	63.6	68.6	–64.9	35.1

Source: 1980, 1996, 2000 CPS

Note: LB indicates the lower bound and UB indicates the upper bound

Table A2 90% Confidence interval on combined ordered outcomes and capped outcomes bounds if all states allowed EDR (corresponds to Table 2)

Year	Probability of voting if all states allowed EDR $P(y_1 = 1 \mid x)$		Classical treatment effect $P(y_1 = 1 \mid x)$– $P(y_0 = 1 \mid x)$	
	LB	UB	LB	UB
1980	65.8	75.6	0	15.7
90% confidence interval	65.6	76.4	NA	16.4
1996 Mature EDR	63.8	71.1	0	11.6
90% confidence interval	63.5	72.3	NA	12.7
1996 New EDR	63.4	66.9	0	6.3
90% confidence interval	63.2	68.5	NA	8.0
2000 Mature EDR	67.4	75.7	0	12.8
90% confidence interval	67.1	76.7	NA	14.0
2000 New EDR	66.9	68.7	0	4.7
90% confidence interval	66.6	70.3	NA	6.4

Source: 1980, 1996, 2000 CPS

Notes: The ordered outcomes assumption tightens the lower bound, while the capped outcomes assumption tightens the upper bound

LB indicates the lower bound and UB indicates the upper bound

NA (not applicable) indicates that the value is fixed at 0 by assumption

Table A3 Probability of voting in the (a) 1980 CPS, (b)1996 CPS, (c) 2000 CPS

	Coefficient	Standard error	p value
(a) Voting in 1980 CPS			
EDR	0.0897	0.0224	0.000
Closing date	–0.0087	0.0006	0.000
Education	0.3131	0.0038	0.000
Age	0.0561	0.0013	0.000
Age squared	–0.0004	0.00001	0.000
Black	0.0049	0.0148	0.740
South	–0.0768	0.0105	0.000
Mobility	0.1848	0.0042	0.000
Income quartile	0.1229	0.0043	0.000
Constant	–3.2425	0.0380	0.000
N = 104,725			
Log likelihood = –57383.399			
(b) Voting in 1996 CPS			
Mature EDR	0.1682	0.0326	0.000
New EDR	0.0793	0.0345	0.021
Closing date	–0.0032	0.0009	0.000
Pre-NVRA motor voter	0.0108	0.0125	0.389
Education	0.3179	0.0049	0.000
Age	0.0389	0.0016	0.000
Age squared	–0.0002	0.00002	0.000

Table A3 continued

	Coefficient	Standard error	*p* value
Black	0.2250	0.0181	0.000
South	–0.0442	0.0126	0.000
Mobility	0.1610	0.0051	0.000
Income quartile	0.1454	0.0053	0.000
Constant	–3.3743	0.0485	0.000
N = 71,025			
Log likelihood = –39701.317			
(c) Voting in the 2000 CPS			
Mature EDR	0.2624	0.0359	0.000
New EDR	0.0524	0.0481	0.276
Closing date	–0.0020	0.0009	0.023
Pre-NVRA motor voter	–0.0312	0.0367	0.396
Education	0.3357	0.0053	0.000
Age	0.0349	0.0017	0.000
Age Squared	–0.0002	0.00002	0.000
Black	0.2797	0.0191	0.000
South	–0.0068	0.0138	0.621
Mobility	0.1548	0.0056	0.000
Income quartile	0.1658	0.0058	0.000
Constant	–3.3236	0.0610	0.000
N = 63,826			
Log likelihood = –34234.333			

Table A4 Predicted turnout from EDR by SES and sex using logit coefficients from Brians and Grofman (1999)

Education level	Income quartile	EDR		Difference
		Yes	No	
(a) Men: largest effect is for high school and second income				
Grade school	Lowest	29.8%	21.8%	8.0%
High school	Second	71.3%	61.9%	9.4%
Some college	Third	88.8%	83.9%	4.9%
4 yr. Degree	Highest	94.7%	92.1%	2.6%
(b) Women: largest effect is for grade school and lowest income (though difference with high school and second income is small)				
Grade school	Lowest	34.7%	25.8%	8.9%
High school	Second	75.6%	67.0%	8.6%
Some college	Third	90.9%	86.7%	4.2%
4 yr. Degree	Highest	95.7%	93.6%	2.1%

Notes: Predictions are calculated for employed, married, white citizens, in the base state (with active motor voter set to 0), closing date set to 30 for the No EDR treatment, in 1992

Brians and Grofman's (1999) results for men could not be exactly replicated but the same substantive conclusion is obtained; i.e. the largest effect occurs for those with a high school degree and second lowest income

Table A5 (a) 90% confidence interval on probit estimates, based on results in Table A3a–c (corresponds to Table 5), and (b) 90% confidence interval on Bounds estimates (corresponds to Table 5)

Category	1980		1996 Mature EDR		1996 New EDR		2000 Mature EDR		2000 New EDR	
	5	95	5	95	5	95	5	95	5	95
(a) 90% confidence interval on probit estimates										
$P(y_1 = 1 \vert x)$										
All	74.0	75.5	69.9	71.9	66.8	69.6	74.4	76.9	67.8	71.4
0–8 years	60.1	62.4	48.9	52.3	45.0	48.9	51.1	55.3	42.7	47.8
9–11 years	63.6	65.8	51.3	53.9	47.6	51.2	54.1	57.6	45.9	50.7
High school degree	72.7	74.4	64.5	66.8	60.9	64.2	68.4	71.4	60.7	65.0
1–3 years college	79.6	81.1	73.2	75.3	70.0	72.9	77.3	79.7	70.5	74.2
College degree	88.4	89.5	83.9	85.5	81.6	83.8	87.6	89.4	83.0	85.8
College +	94.4	95.1	93.2	94.1	91.9	93.1	95.1	95.9	92.5	94.1
CTE										
All	8.1	9.8	6.6	8.6	3.5	6.4	7.6	10.2	1.2	5.0
0–8 years	10.3	12.5	8.1	10.6	4.2	7.8	9.9	13.4	1.5	6.3
9–11 years	9.5	11.4	7.7	10.1	4.0	7.4	9.5	12.9	1.4	6.0
High school degree	8.6	10.4	7.5	9.8	4.0	7.2	9.0	12.0	1.4	5.8
1–3 years college	7.5	9.0	6.7	8.7	3.6	6.5	7.8	10.4	1.2	5.1
College degree	5.5	6.5	5.2	6.7	2.8	5.1	5.6	7.3	0.9	3.8
College +	3.5	4.2	3.0	3.8	1.7	3.0	3.0	3.9	0.5	2.1
(b) 90% confidence interval on Bounds estimates										
$P(y_1 = 1 \vert x)$										
All	74.8	76.4	70.1	72.3	65.6	68.5	74.5	76.7	67.1	70.3
0–8 years	62.8	67.2	51.3	61.0	31.0	48.9	56.5	67.8	31.9	50.0
9–11 years	59.5	64.7	46.8	55.4	40.8	50.9	53.6	61.8	37.6	49.2
High school degree	72.6	75.0	61.7	65.9	56.6	60.9	65.6	69.2	57.5	63.2
1–3 years college	80.2	83.3	73.6	77.6	70.3	75.1	77.9	81.3	71.0	75.1
College degree	90.7	93.9	84.5	88.7	83.5	88.7	88.0	91.8	84.7	89.6
College +	90.8	94.3	89.8	95.7	84.3	92.5	93.3	97.2	88.7	95.4
CTE										
Category	5	95	5	95	5	95	5	95	5	95
All	9.7	11.3	6.8	9.0	2.2	5.3	7.7	10.0	0.4	3.5
0–8 years	12.1	16.6	6.1	15.7	–13.2	3.3	12.3	25.0	–11.6	6.6
9–11 years	9.3	15.0	5.9	14.4	–0.2	10.5	9.4	18.5	–6.4	5.5
High school degree	9.0	11.7	5.0	9.1	–0.1	4.2	5.9	9.8	–2.0	3.3
1–3 years college	6.9	10.5	5.1	9.3	1.9	6.8	7.2	10.9	0.6	4.5
College degree	6.2	9.7	5.4	9.7	4.5	9.8	4.3	8.3	0.8	6.1
College +	2.5	6.6	2.0	8.1	–3.3	4.8	4.1	8.3	–0.7	6.4

Source: 1980, 1996, 2000 CPS

Note: The 5 and 95 column headings represent the 5th and 95th quantile of the bootstrapped estimates, respectively

Table A6 Comparison of estimates of the probability of voting by education if all states had EDR by year and EDR maturity, probit model with education as the only covariate, based on results in Table A7

Category	1980				1996 Mature EDR				1996 New EDR				2000 Mature EDR				2000 New EDR			
	$P(y_1 = 1 \mid x)$			CTE probit model	$P(y_1 = 1 \mid x)$			CTE probit model	$P(y_1 = 1 \mid x)$			CTE probit model	$P(y_1 = 1 \mid x)$			CTE probit model	$P(y_1 = 1 \mid x)$			CTE probit model
	Ordered and capped outcomes		Probit model		Ordered and capped outcomes		Probit model		Ordered and capped outcomes		Probit model		Ordered and capped outcomes		Probit model		Ordered and capped outcomes		Probit model	
	LB	UB			LB	UB			LB	UB			LB	UB			LB	UB		
All	66	76	75	10	64	71	71	8	63	67	67	3	67	76	75	9	67	69	68	1
0–8 years	52	65	56	12	46	56	42	9	45	41	37	3	45	62	44	10	43	41	35	1
9–11 years	51	62	67	12	41	51	56	9	41	46	50	3	44	57	59	11	44	43	50	1
High school degree	64	74	75	11	57	64	67	9	57	59	61	3	60	67	70	10	60	60	62	1
1–3 years college	74	82	82	9	69	76	76	8	68	73	72	3	71	80	80	8	71	73	73	1
College degree	85	92	88	7	80	87	84	6	79	86	81	2	84	90	88	6	84	87	83	1
College +	88	93	92	6	88	93	90	5	88	89	88	2	89	96	93	4	89	92	90	1

Source: 1980, 1996, 2000 CPS

Notes: Mature EDR analyses exclude New EDR states and New EDR analyses exclude Mature EDR states

The ordered outcomes assumption tightens the lower bound, while the capped outcomes assumption tightens the upper bound

LB indicates the lower bound and UB indicates the upper bound

CTE stands for Classical Treatment Effect

Table A7 Probit estimates with education as the only covariateSource: 1980, 1996, 2000 CPS

	Coefficient	Standard error	*p* value
1980			
EDR	0.3070	0.0154	0.000
Education	0.2422	0.0030	0.000
Constant	–0.5888	0.0126	0.000
N = 113,123			
Log likelihood = –68984.768			
1996 Mature EDR			
EDR	0.2262	0.0210	0.000
Education	0.2880	0.0039	0.000
Constant	–0.9503	0.0180	0.000
N = 78,309			
Log likelihood = –48282.756			
1996 New EDR			
EDR	0.0785	0.0240	0.001
Education	0.2876	0.0039	0.000
Constant	–0.9396	0.0180	0.000
N = 78,309			
Log likelihood = –48336.174			
2000 Mature EDR			
EDR	0.2703	0.0221	0.000
Education	0.3169	0.0042	0.000
Constant	–1.0058	0.0195	0.000
N = 74,174			
Log likelihood = –43670.615			
2000 New EDR			
EDR	0.0290	0.0249	0.242
Education	0.3166	0.0042	0.000
Constant	–0.9906	0.0194	0.000
N = 74,174			
Log likelihood = –43746.634			

Table A8 90% Confidence interval on probit estimates with education as the only covariate, based on results in Table A6

Category	1980		1996 Mature EDR		1996 New EDR		2000 Mature EDR		2000 New EDR	
	5	95	5	95	5	95	5	95	5	95
$P(y_1 = 1 \mid x)$										
All	74.3	76.0	70.2	72.2	65.1	68.0	74.3	76.5	67.1	69.5
0–8 years	55.2	57.6	41.2	44.0	35.3	39.0	42.5	45.5	33.6	36.8
9–11 years	66.2	68.1	54.4	57.0	48.3	52.0	57.0	59.9	47.8	51.1
High school degree	74.6	76.2	65.5	67.9	59.7	63.1	68.9	71.4	60.4	63.3
1–3 years college	81.6	83.1	75.4	77.4	70.4	73.2	79.1	81.1	71.9	74.5
College degree	87.3	88.5	83.5	85.2	79.5	81.8	87.0	88.5	81.5	83.6
College +	91.7	92.6	89.6	91.0	86.7	88.5	92.5	93.6	88.7	90.2
CTE										
All	9.2	10.9	6.7	8.8	1.3	4.3	7.4	9.6	–0.4	2.2
0–8 years	11.1	13.2	7.5	10.0	1.4	4.6	8.7	11.6	–0.4	2.4

Table A8 continued

Category	1980		1996 Mature EDR		1996 New EDR		2000 Mature EDR		2000 New EDR	
	5	95	5	95	5	95	5	95	5	95
9–11 years	10.7	12.7	7.9	10.5	1.5	4.9	9.2	12.1	–0.4	2.6
High school degree	9.7	11.4	7.5	9.9	1.5	4.7	8.6	11.2	–0.4	2.5
1–3 years college	8.3	9.7	6.6	8.6	1.3	4.2	7.2	9.3	–0.4	2.1
College degree	6.7	7.8	5.4	6.9	1.1	3.5	5.5	7.0	–0.3	1.7
College +	5.1	5.9	4.0	5.1	0.8	2.6	3.8	4.8	–0.2	1.2

References

Achen, C. H. (2002). Toward a new political methodology: Microfoundations and ART. *Annual Review of Political Science, 5*, 423–450.

Alvarez, R. M., & Ansolabehere, S. (2002). California votes: The promise of election day registration. DEMOS: A network for ideas and action. http://www.electiondayreg.com/files/research/california_votes.pdf, visited 7/26/02.

Alvarez, R. M., Nagler, J., & Wilson, C. H. (2004). Making voting easier: Election day registration in New York. DEMOS: A network for ideas and action. http://www.demos-usa.org/pub198.cf visited 8/1/05.

Brians, C. L., & Grofman, B. (1999). When registration barriers fall, who votes? An empirical test of a rational choice model. *Public Choice, 99*, 161–176.

Brians, C. L., & Grofman, B. (2001). Election day registration's effect on U.S. voter turnout. *Social Science Quarterly, 82*, 170–183.

Calvert, J. W., & Gilchrist, J. (1993). Suppose they held an election and almost everyone came! *PS: Political Science and Politics, 26*, 695–700.

Downs, A. (1957). An economic theory of democracy. New York: Harper and Row.

Fenster, M. J. (1994). The impact of allowing day of registration voting on turnout in U.S. presidential elections from 1960 to 1992, a research note. *American Politics Quarterly, 22*, 74–87.

Hanmer, M. J. (2002). *A Monte Carlo simulation and empirical investigation of scobit*. Paper Presented at the Annual Meeting of the American Political Science Association, August 29–September 1, 2002, Boston, MA.

Hanmer, M. J. (2004). *From selection to election and beyond: Understanding the causes and consequences of electoral reform in America*. Ph.D. Dissertation, University of Michigan.

Hanmer, M. J. (2006). An investigation of scobit. Working paper, Georgetown University.

Hardle, W. (1990). *Applied nonparametric regression*. New York: Cambridge University Press.

Herron, M. C. (2000). Postestimation uncertainty in limited dependent variable models. *Political Analysis, 8*, 83–98.

Highton, B. (1997). Easy registration and voter turnout. *Journal of Politics, 59*, 565–575.

Highton, B. (2004). Voter registration and turnout in the United States. *Perspectives on Politics, 2*, 507–515.

Huang, C., & Shields, T. G. (2000). Interpretation of interaction effects in logit and probit analyses: Reconsidering the relationship between registration laws, education and voter turnout. *American Politics Quarterly, 28*, 80–95.

Jackson, R. A., Brown, R. D., & Wright, G. C. (1998). Registration, turnout, and the electoral representativeness of U.S. state electorates. *American Politics Quarterly, 26*, 259–287.

James, D. (1987). Voter registration: A restriction on the fundamental right to vote. *Yale Law Journal, 96*, 1615–1640.

Knack, S., & White, J. (2000). Election-day registration and turnout inequality. *Political Behavior, 22*, 29–44.

Manski, C. F. (1989). Anatomy of the selection problem. *Journal of Human Resources, 24*, 343–360.

Manski C. F. (1990). Nonparametric bounds on treatment effects. *American Economic Review Papers and Proceedings, 80*, 319–323.

Manski, C. F. (1994). The selection problem. In: C. Sims (Ed.), *Advances in econometrics, sixth world congress*. Cambridge: Cambridge University Press.

Manski, C. F. (1995). *Identification problems in the social sciences*. Cambridge: Harvard University Press.

Manski, C. F. (1997). Monotone treatment response. *Econometrica, 65*, 1311–1334.

Manski, C. F., & Nagin, D. S. (1998). ''Bounding disagreements about treatment effects: A case study of sentencing and recidivism.'' In: A. Raftery (Ed.), *Sociological methodology* Basil-Blackwell.

Manski, C. F., Sandefur, G. D., McLanahan, S., & Powers, D. (1992). Alternative estimates of the effect of family structure during adolescence on high school graduation. *Journal of the American Statistical Association, 87*, 25–37.

Martinez, M. D., & Hill, D. (1999). Did motor voter work? *American Politics Quarterly, 27*, 296–315.

Mitchell, G. E. & Wlezien, C. (1995). The impact of legal constraints on voter registration, turnout, and the composition of the American electorate. *Political Behavior, 17*, 179-202.

Nagler, J. (1991). The effect of registration laws and education on U.S. voter turnout. *American Political Science Review, 85*, 1393–1405.

Nagler, J. (1994). Scobit: An alternative estimator to logit and probit. *American Journal of Political Science, 38*, 230–255.

Pepper, J. V. (2000). The intergenerational transmission of welfare receipt: A nonparametric bounds analysis. *The Review of Economics and Statistics, 82*, 472–488.

Piven, F. F., & Cloward, R. (1988). *Why Americans don't vote*. New York: Pantheon Books.

Piven, F. F., & Cloward, R. (1989). Government statistics and conflicting explanations of non-voting. *PS: Political Science and Politics, 22*, 580–588.

Piven, F. F., & Cloward, R. (2000). *Why Americans still don't vote: And why politicians want it that way*. Boston: Beacon Press.

Powell, G. B. Jr. (1986). American voter turnout in comparative perspective. *American Political Science Review, 80*, 17–43.

Rosenstone, S. J., & Wolfinger, R. E. (1978). The effect of registration laws on voter turnout. *American Political Science Review, 72*, 22–45.

Silverman, B. (1986). *Density estimation for statistics and data analysis*. London: Chapman & Hall.

Teixeira, R. A. (1992). *The disappearing American voter*. Washington DC: Brookings Institute.

Traugott, M. W. (2004). Why electoral reform has failed: If you build it, will they come? In: A. N. Crigler, M. R. Just & E. J. McCaffery (Eds.), *Rethinking the vote: The politics and prospects of American electoral reform*. New York: Oxford University Press.

U.S. Dept. of Commerce, Bureau of the Census. CURRENT POPULATION SURVEY: VOTER SUPPLEMENT FILE, 1980 [Computer file]. ICPSR version. Washington, DC: U.S. Dept. of Commerce, Bureau of the Census [producer], (1981). Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 1999.

U.S. Dept. of Commerce, Bureau of the Census. CURRENT POPULATION SURVEY: VOTER SUPPLEMENT FILE, 1996 [Computer file]. ICPSR version. Washington, DC: U.S. Dept. of Commerce, Bureau of the Census [producer], (1997). Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 1997.

U.S. Dept. of Commerce, Bureau of the Census. CURRENT POPULATION SURVEY: VOTER SUPPLEMENT FILE, 2000 [Computer file]. ICPSR version. Washington, DC: U.S. Dept. of Commerce, Bureau of the Census [producer], (2001). Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2001.

Wolfinger, R. E., & Rosenstone, S. J. (1980). *Who votes?* New Haven: Yale University Press.