The Turnout Effects of Early Voting, Election Day Registration, and Same Day Registration in the 2008 Presidential Election

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Advocates, journalists, and politicians frequently argue for changes to election laws out of the belief that making voting easier will increase turnout. Deductively, it stands to reason that making voting more convenient – through easier registration, voting prior to election day, or expanded absentee voting, to name a few common reforms – will encourage more voters to cast ballots. However, in this paper we challenge this conventional wisdom, and show that while some reform practices increase turnout, others have little effect. Most importantly, we find that the most popular proposal – early voting – actually *decreases* turnout.

Further, we show that analysis of voting reforms must consider the interactions among the different practices. We are particularly interested in the synergistic effects of early voting combined with Election Day registration (EDR). 2008 was the first year that this combination was available in enough states to permit an accurate estimation of its effect on turnout. We examine the effects using aggregate county-level election returns along with an individual-level analysis of the voting supplement to the November 2008 Current Population Survey.

We begin by providing a picture of prior research on state election laws and turnout. We argue that that distinct combinations of EDR, SDR, and early voting need to be considered. In studying the effects of these treatments, one must consider how they interact. Focusing on individuals who are marginally likely to vote, we contend that while EDR should has positive effects on turnout, early voting should not. We test these hypotheses by examining the impact on aggregate turnout levels and the individual probability of voting. Using both traditional regression and matching analysis, we demonstrate that any package of laws that includes EDR increases turnout, but that early voting options by themselves decrease turnout. We conclude by discussing the broader implications of this research for future innovations and reforms in election administration.

Previous Research

Policymakers and reform groups across the country have long been interested in reorganizing the voting process in ways that will increase turnout. One common option is to allow individuals to register on the day they vote. This is expected to foster turnout by reducing the need for individuals to take two separate actions to exercise their franchise: registration and the actual process of voting. As Wolfinger and Rosenstone (1980, 61) famously summarized, "Registration is usually more difficult than voting, often involving more obscure information and a longer journey at a less convenient time, to complete a more complicated procedure. Moreover, it must usually be done before interest in the campaign has reached its peak." Indeed, related work shows that the registration "closing date" is the most consequential aspect of registration. This is in part because it disenfranchises recent movers (Squire, Wolfinger, and Glass 1987; Timpone 1998) but more so because it excludes potential voters who do not become engaged until the very end of the campaign.[1] There are several solutions to this alleviate this concern.

EDR permits people who wish to vote on Election Day but have not registered to do both in "one essentially continuous act" (Wolfinger, Highton, and Mullin 2005, 3).

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[1] See recent reviews by Highton (2004) and Hershey (2009) for further discussion of the importance of closing dates.

EDR would thus appear to alleviate the barriers highlighted by Wolfinger and Rosenstone: it collapses two steps into one and permits voters to register at the last moment when interest is highest.[2] Using the modified definition we employ below, 11 states had EDR in 2008 (including North Dakota, which technically does not have voter registration).

Research consistently shows that EDR boosts turnout.[3] The early adopters, Minnesota, Wisconsin, and Maine, consistently lead the nation in voter participation. A sizeable number of voters take advantage of EDR in states that offer it: 15.6% of voters in Minnesota, 16.5% in Wyoming, 13.5 % in Idaho, and 11.4% in Wisconsin registered to vote on Election Day (EAC 2009, Table 5). Estimates of the turnout effects of EDR range from three to seven points (Brians and Grofman 2001; Fenster 1994; Hanmer 2009; Knack 2001.[4] Highton (2009, 509) nicely summarizes the impact of EDR on voter turnout as "about five percentage points."

A second innovation is voting before the traditional Election Day. Early voting technically encompasses a range of practices: absentee voting, other forms of voting-by-mail, and in-person early voting. In 2008 there were 21 states in which at least 30% of voters cast their ballot before Election Day (up from 15 in 2004; McDonald 2009). This percentage of early voting was up from 20% in 2004 and just 7% in 1992.[5] Early voting may have been the most touted reform in the 2008 elections, with at least one prominent newspaper calling it a "big transformation in the way Americans exercise their most fundamental right as citizens"[6] and academic observers concluding that the "United States is in the midst of a revolution in voting" (Fortier 2006, 1).

Early voting takes on a variety of forms across the states (Fortier 2006; Gronke et al. 2008). On a spectrum from most restrictive to least restrictive, these include traditional absentee voting, no-excuse absentee, permanent absentee, in-person early voting, and voting by mail. There is additional variation in where people vote: in-person early voting may take place either at central election offices or at voting centers in locations such as shopping malls.

In contrast to the positive findings surrounding EDR, most studies have found that the availability of early voting has no effect on voter turnout. For example, in a study of national elections from 1980-2004, Gronke et al. (2007) find that the availability of early voting does not influence turnout. Although the special case of vote-by-mail has a positive effect in presidential elections, none of the early or absentee voting laws relate to

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[2] The EDR reform spread in three waves . See Hanmer's (2009) comprehensive analysis of EDR for a fuller review of the history and reasons for adoption.

[3] A prominent line of scholarship investigates the degree to which election reforms exacerbate inequalities in turnout (Berinsky 2005; Hanmer 2009; Highton 2004; Knack and White 2000). .

[4] Knack (2001) finds a bigger turnout effect among second wave adopters than first wave adopters. Hanmer (2009) and Leighley and Nagler (2009) finds an effect of 4.3 to 4.5 points among first wave adopters but a minimal effect among second wave adopters. Neither examines the most recent wave of states that we are able to study.

[5] Michael MacDonald, "(Nearly) Final 2008 Early Voting Statistics," updated January 11, 2009, <http://elections.gmu.edu/Early_Voting_2008_Final.html>. Also see Paul Gronke's Early Voting Information Center at <http://www.earlyvoting.net/blog/>.

[6] Janet Hook and Noam M. Levey. "Early Voting Hits Record High." *Los Angeles Times*. November 4, 2008.

turnout in either presidential or midterm elections.[7] Several other studies have shown that none of the forms of early voting on their own (other than sui generis vote-by-mail) improves turnout (Fitzgerald 2005; Giammo and Brox Forthcoming; Gronke et al. 2008; Oliver 1996; Primo, Jacobsmeier, and Milyo 2007; Scheele et al. 2008; cf. Wolfinger, Highton, and Mullin 2005).[8]

One underappreciated state election law is same day registration (SDR). SDR effectively marries the ideas behind EDR and early voting by permitting people to both register and vote in a single act, and to do so in advance of the election. It theoretically circumvents the inconvenience of needing to vote on a specific Election Day, eliminates the registration closing date, and permits "one-stop shopping" by combining registration and balloting. By our definition, a dozen states permitted some form of SDR in 2008, with windows ranging from just one day to a month. Despite its widespread use, we know of no studies that have analyzed SDR's effects on turnout directly. Indeed, one of our messages is to urge researchers to carefully distinguish EDR, SDR, and early voting. As we document below, it is possible for a state to have one, two, or all three of these features. By ignoring these combinations, previous estimates may have mistakenly attributed the effects of any single practice.

Election Laws and Turnout Mechanisms

Both EDR and early voting are designed to increase turnout by lowering the costs of voting. But upon further probing the mechanisms are quite different. EDR lowers costs by providing "one-stop shopping," eliminating one bureaucratic step in the process and providing individuals who become interested late in the campaign the opportunity to register. Early voting, in contrast, lowers costs by making voting accessible over an extended period, rather than making the election a one-day event. SDR effectively combines these options by permitting "one-stop shopping" to occur before Election Day.

Any discussion of turnout must focus on the costs of voting. At the same time, focusing exclusively on costs may miss the importance of mobilization in encouraging potential voters to become actual voters. We expect EDR to be a particularly effective mechanism, because it permits those who come late to the campaign to still become participants, even those who become engaged only days before the election. In contrast, we expect early voting to matter less, because it may simply provide an outlet for those already likely to vote, and who are attentive enough to know that alternative voting processes even exist. The effects same day registration and one-stop shopping most likely depends on the timing of the early voting window.

On this point we are sympathetic to Highton's discussion of closing dates. As he explains:

> People who are most interested in politics are very likely to make sure they are registered. Only rarely will they fail to register by the waning weeks of a national campaign. As a result, closing

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[7] Previous research also shows a positive effect of vote-by-mail (Magleby, 1987; Southwell and Burchett, 2000; Karp and Banducci, 2000), but these studies have largely been confined to Oregon and Washington. However, Kousser and Mullin (2007) find that a shift to vote-by-mail in California would result in a three-point drop in turnout. We do not study vote-by-mail directly but effectively account for it with state fixed effects.

[8] Stein and Vonnahme (2008) find a small positive effect of non-precinct voting centers on turnout among younger, infrequent voters and those who have not yet developed the voting habit.

dates influence the turnout of these highly motivated people very little. Those least interested in politics are also unlikely to be influenced by closing dates. These citizens have virtually no motivation to vote; their voting benefits are nearly zero. They pay little, if any, attention to political campaigns and are therefore unlikely to be activated by them. Late closing dates, or even election day registration will not bring these people to register and vote. Between these extremes are individuals who take some interest in politics, and who may be spurred to register and vote by the increased campaign interest that attends the approach of election day. A late closing date allows for this possibility. If the deadline for registration is well before election day, however, it is unlikely that campaign interest will be translated into turnout. For this group of people, registration closing dates ought to matter more (2004, 509).

This view comports with Berinsky's (2005) distinction between reforms that *stimulate* new voters and those that merely *retain* existing voters. He contends that most voting reforms are better at retention than they are at stimulation. We refine this argument by identifying differences between EDR and early voting. In particular, we expect early voting to enhance retention, and EDR to enhance stimulation. Some preliminary evidence suggests that early voting should have a minimal effect on turnout, because it is merely a convenience for those who were already planning to vote,. In fact, a few analyses indicate that early voting actually *lowers* turnout. Smith and Comer (2005) find negative effects, but others (Gronke et al. 2008; Leighley and Nagler 2009; Tolbert et al. 2008) find negative effects only in particular specifications.

One explanation for the apparent depressive effect of early voting is that it robs Election Day of the stimulating effect it would otherwise have on nonvoters. Early voting siphons away and thus dilutes the concentrated activities of Election Day itself that would likely stimulate turnout. Fourtier (2006) suggests as much when he speculates that a loss of the "civic day of election" could lower turnout. At least one empirical study shows that Election Day social activities increase turnout (Addonizio, Green, and Glaser 2007). Traditional Election Day is as much a social event as a political one. For at least some voters, it is the stimulation of the day's news, observation of activities as polling places, and conversations with friends and neighbors that gets them to the polls. When these activities are suppressed or diluted, so is the stimulating effect.

Towards an Interactive Model

We argue that it is crucial to isolate the independent effects of EDR, SDR, and early voting and to consider their various combinations. Because there is variation in how states design and implement each practice, there is also variation in whether states truly fall into one of the three categories we study. Studies of early voting's effects have been careful to distinguish various forms of early, absentee, and mail balloting, but have ignored whether these features coincide with SDR. Any study of "one-stop shopping" and early voting must consider direct effects, interactions in which two features are present, and a three-way confluence when all options are available. These can be thought of interaction terms or different configurations of election laws. Much like a physician who prescribes medication for a patient, the interactions among treatments should be seriously considered. A drug can have a direct effect of its own but also joint effects in conjunction with other medicines the patient is taking. To this point the literature on election reform has largely ignored these interactive effects.

Studies of election laws have not considered how they overlap in practice, or what the implications might be for turnout. For example, one explanation of the relative

failure of early voting policies is that it is the inconvenience of registration, rather than the difficulty of voting itself, that deters most citizens from participating (Erikson 1981). Early voting might make the act of voting more convenient, but without allowing registration and voting in a single step, it still requires an individual to register in advance, often several weeks before the vote is actually cast. Without SDR, a person who encounters an early voting center in a shopping center or visits an administrative building in the days preceding an election may not stop to vote because that demands not only an interest in voting prior to Election Day, but also advance registration. If a voter failed to register before the closing date, early voting will not help her. In contrast, a voting center that allows "one-stop shopping" may facilitate voting by citizens who would not have been traditional Election Day voters.

Before we categorize which election reforms states pursue, we offer operational definitions. First, **EDR** permits eligible voters to both register and vote on Election Day. Studies of EDR have generally identified eight states from the first two waves of reform. After carefully reading state statutes and consulting with state election officials, we modify this classification. In 2008 we consider the usual suspects including North Dakota (although it technically has no registration), but excluding North Carolina (which only has SDR). But we also include Alaska and Connecticut, both of which permitted non-registrants to vote for president. Breaking with tradition, we suggest that these later two states should be treated as EDR states in a presidential election year. EDR states may still have closing dates for traditional registration, but nonetheless permit last-minute registrations on Election Day itself.

Second, our criterion for defining **SDR** is that the practice must be widely available to eligible voters without significant administrative barriers. We thus excluded states that allowed some form of "one-stop shopping" if this was available only to limited portions of the population. For example, Colorado permitted SDR only for a small set of "emergency" registrants who moved across county lines after the closing date. Nationally, 17 states reported that 3.6 million same day registration applications were filed; of those, 963,144 new voters were added to the registration rolls.[9] States also vary in how many days SDR is permitted.

Finally, **early voting** allows registrants to cast ballots without excuse before Election Day. By itself early voting does not provide the opportunity to register. That would be captured by SDR. The category includes true early voting (as in Florida) and no-excuse absentee ballots (as in Wisconsin), but not states that require voters to have an excuse to vote absentee (as in Minnesota).[10]

Figure 1 uses a Venn diagram to illustrate our codings for the 2008 election. In practice, it is clear states have been experimenting with the three potentially overlapping policy alternatives. There are another 12 states that have none of the three. The most common approach, used by 20 states, is to allow early voting in isolation. In many cases these three features are implemented as complementary rather than competing policies.

Figure 1 about here

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[9] The EAC collected data on SDRs for the first time in 2008; the EAC defines SDR as "registering to vote on the same day in which a vote may be cast" (EAC 2009).

[10] Codings are drawn from the National Conference of State Legislature's listing at <http://www.ncsl.org/LegislaturesElections/ElectionsCampaigns/AbsenteeandEarlyVoting/tabid/16604/Default.aspx> accessed in July 2009.

Relative to states with none of these reforms, there are seven possible configurations: (1) EDR alone, (2) SDR alone, (3) early voting alone, (4) EDR and SDR, (5) EDR and early voting, (6) SDR and early voting, (7) or all three. In practice there are no states with just SDR, or the two-way combination of SDR and EDR. As a result, there are effectively five combinations relative to the dozen baseline states that have none. In retrospect, this is obvious: "one-stop shopping" before Election Day is not possible if a state does not also allow early voting. Our models will incorporate five dichotomous variables to reflect each of the configurations.

No previous study has investigated this potentially positive relationship between EDR and early voting because until recently no state had extensive use of both. The 2008 cycle was the first presidential election in which states that permitted EDR also had high rates of early voting.[11] In the next section we investigate this question using several data sources and methods to identify the precise effects of each configuration of election features.

Empirical Results

Data and Methods

We use a variety of multivariate statistical techniques to determine how EDR, SDR, and early voting affect voter turnout. First, we analyze a county-level dataset from the November 2008 presidential election. In these models we are able to include county-level variables but also state fixed effects to ensure that unmeasured characteristics such as state culture are not producing spurious findings. Second, we make use of the Current Population Survey's (CPS) November 2008 Voting and Registration Supplement. The large sample size permits careful comparisons among the states in each part of the Figure 1 and inclusion of wide range of individual-level control variables.

We believe our models improve upon earlier analyses by explicitly considering how the combinations of EDR, SDR, and early voting affect turnout. We are able to reveal, for example, whether EDR's positive effects on turnout depend on the presence of early voting or are undermined by it. In addition, we augment the standard approach by comparing the results of logistic regressions predicting registration and turnout with more advanced matching techniques. Matching is an exciting technique that has only recently appeared in political science. It permits us to configure the data into balanced treatment and control groups, providing more confidence in our causal estimates of the effects.

County Level Regression Analysis

We begin with aggregate analysis of turnout at the county level. The dependent variable is turnout in the November 2008 presidential elections as a percentage of the voting age population. The key explanatory variables are dichotomous indicators for each of the five possible realizations in Figure 1. The signs and significance levels of these coefficients will suggest the contribution that each distinct combination makes to voter turnout. To avoid spurious findings, we include an array of control variables

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[11] In 2004 no state with EDR had over 30% of its votes cast early. In 2008 several EDR states were near or above that mark.

suspected to affect turnout and estimate multiple specifications to increase confidence in the robustness of the findings. We also adjust the standard errors to account for clustering of counties by state (Primo, Jacobsmeier, and Milyo 2007).

The control variables include state election laws, county demographic measures, and one indicator of the competitiveness of the presidential campaign. State election law variables include a measure of the closing date for voter registration, a dummy for whether votes are required to show identification at the polls, and a dummy indicating whether ex-felons are barred from voting. To the degree that these laws matter once our new variables are included, we expect all three to have negative effects as early closing dates, ID requirements, and felon disenfranchisement lower turnout. Demographic variables include the percent black, median income, percentage of the county with bachelor degrees, percentage 65 or older, population, and population density. Our measure of campaign intensity is the difference between the final pollster.com survey estimates for McCain and Obama. The effect should be negative because a larger gap between the candidates ought to be reflected in lower turnout.

Our simplest specification is model I in Table 1. The model indicates that EDR alone or in combination with other laws has positive effects. The effects range from 3.9 points for the three-way combination to 6.4 points for EDR in isolation. In contrast, early voting on its own has a negative effect of 3.4 points, and the combination of SDR and early voting has no effect. Control variables mostly perform as expected. A longer closing date depresses turnout and turnout is higher in county with more African-Americans, higher incomes, more college graduates, smaller and less dense populations, and where the McCain-Obama campaign was close. The one unexpected finding is that turnout is slightly higher when an ID requirement is present.[12]

Table 1 about here

To explore the robustness of these findings, we present two alternative models. Model II expands model I by including state fixed effects. State dummies help to eliminate endogeneity whereby a state adopts a particular election law because of its turnout and omitted variable bias in which unmeasured state characteristics such as political culture are absent from the model (Fitzgerald 2005; Hanmer 2009). Including fixed effects poses a more conservative approach because it reflects past election laws, which are slow to change, and because it reduces the degrees of freedom. Model III modifies this approach slightly by weighting the observations by population. This is to account for the wide variance in county populations that may contribute to heteroskedasticity.

Even with these measures, the basic pattern of results holds and the effects of our key variables actually increase in magnitude. In both alternative models any combination including EDR increases turnout by 6.3 to 11.6 percentage points. Including state fixed effects makes the effect of EDR in isolation appear most powerful while adding population weights leads to a larger effect for the three-way combination. The variable tapping the SDR-early voting pairing is positive in all three models but only achieves a reasonable level of statistical significance in model II. Finally, the effect of early voting alone remains consistently negative, ranging as high as six percentage points.

The effects of demographic variables on turnout are quite consistent across models with factors such as income and education levels having strong positive effects.

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[12] This is similar to the negative ID effect found by Milyo (2009).

The two surprising results are that the effects of ID and felon voting laws change signs across models and that campaign competitiveness has a counterintuitive effect in model II.

The results of these county regression models suggest that voter turnout is indeed sharply influenced by state laws concerning registration and early voting. These findings are relatively robust across different specifications, including population weighting and state-level dummies. The two key results are that (1) early voting by itself has a negative effect and (2) EDR by itself has a positive effect. Combining early voting with SDR appears to have little effect while combining EDR with early voting results does result in a significant and positive outcome. States that have all three approaches have a significant and sizeable increase in turnout. Indeed, any combination that includes EDR increases turnout. Cumulatively, the results suggest that creating the opportunity for voters to "one-stop shop" offers a way to turn the negative of early voting into a net positive.

One reason the SDR effects are somewhat inconsistent is that the models ignore the substantial variation in how SDR is implemented across the states. Particularly important is the length of time in which "one-stop shopping" is available. In 2008 this window ranged from just one day in New Mexico to over 40 days in three states. We can test whether this variation is correlated with turnout. To investigate this possibility we reestimate model II on states that have SDR. We include a new key variable: the length of time the SDR window is open. The results in Table 2 show that each additional day when voters can avail of "one-stop shopping" results in a .29% increase in turnout. Increasing the window length by 12 days (the standard deviation of the variable) thus increases turnout by 3.5 points. The control variables largely operate as expected. The window finding reinforces our expectation that it is not just important that states offer the change to both register and vote early, but also demonstrates that how these options are implemented matters. Two states could both have SDR "on the books" but the state that offers it longer will see a greater positive effect.

Table 2 about here

County Level Matching Analysis

Matching techniques offer another way of testing the relationships we study. Matching permits sharper comparisons of treatment and control groups, in a manner that makes efficient use of the data and is less sensitive to specification error (Ho et al, 2007). In this case, the various voting administration practices are analogous to a "treatment" effect applied to counties (and individuals, below): for example, a county in a state with EDR experiences a treatment distinct from a county in a state without EDR (which we can consider as analogous to a control group). Matching in this case, roughly speaking, creates two balanced groups, one consisting of "treated" observations, the other of "control" observations.

There are three steps to the matching method. First, we separate the data into treatment and control groups for each of the five categories of voting and registration system types. In each case, the "treated" group consists of individuals in a state with EDR, early voting, or the different combinations of EDR, SDR, and early voting. For each treated group, we construct a control consisting of respondents in states that have

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none of the practices in the treatment group. The early voting/EDR group, for example, is matched with a control group of counties in states that do not have early voting, EDR, or the combination. Similarly, counties in early voting states are matched with counties in states without early voting. In this way, we are able to test for the specific effect of each individual practice, or combination of practices.

Second, we use a propensity score matching process (Ho et al. 2009) to pair each individual in the treatment group with a demographically similar individual in a control group.[13] Finally, we used the resulting pre-processed and balanced data set in a logistic regression model equivalent to the basic individual level model of voter turnout.

Table 3 reports the regression results using the matched data. Note that we now must estimate a separate model for each causal effect of interest. The benefit of the matched analysis is that we have more confidence in our key variable estimates, with the disadvantages being smaller sample sizes and unrepresentative samples that could affect some of the control variables. Some state level variables such as the closing date and campaign competitiveness will be particularly affected. Because our interest is in the effects of election laws, this is a tradeoff well worth making.

The key results of our matched county turnout models largely confirm the standard regression analyses in Table 1, as illustrated by the summary Figure 2. In Figure 2, the dots represent the five coefficient estimates for the standard models and for the matched data. Horizontal lines running through the dots show 95% confidence intervals. The figure shows that EDR's effect is positive on its own and to a lesser degree when paired with early voting. But the combination of early voting and SDR has a marginal effect on turnout, and early voting on its own actually decreases voter turnout. Finally, the combination of all three has a strong positive effect.

<u>Individual Level Regression Analysis</u>

We now turn to estimating turnout effects at the individual level. Here we are generally interested in the factors that make individuals more (or less) likely to cast a ballot. Most turnout analysis take a standard form, using logit or probit regression with the vote (or reported vote) as the dependent variable, and with the right-hand side consisting of various demographic and systemic independent variables that purport to capture the important causal factors. A benefit of survey analysis is that individual-level factors can be incorporated into the models.

Because our paper includes both models of aggregate and individual turnout, it is worth cautioning against the ecological fallacy that the factors affecting *aggregate* outcomes, such as the percentage of the voting age population that votes, have the same effect on *individual* outcomes, as measured by the estimated probability that an

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[13] We used the "MatchIT" module written for the R statistical package (Ho, et al. 2009), using nearest-neighbor propensity score matching with replacement. We balanced on subset of demographic variables, including education, income, sex, age categories, and political competitiveness. The efficiency of the matching process increases with better balance on these covariates between the treatment and control groups. The crucial element of preprocessing is that matching may not be conditioned on the treatment variables used in any subsequent analysis.

individual will vote.[14] Our dual-track analysis is an effort to gain leverage on both elements of the modeling problem.

This may look like a straightforward process: regress whether someone voted on a group of variables, and the results tell us how education, income, race, competitiveness, registration requirements, or anything else that we choose to measure, affect that decision. It is well established, for example, that turnout increases among highly educated and high-income individuals, and that aggregate turnout is higher in states that have less stringent registration requirements.

But these aggregate effects could be different at the individual level: a very small change in the probability that an individual votes might be nearly undetectable, but significant in the aggregate voting results of a large population. This effect can be especially acute with voting system variables, as there are large differences in the way that states administer policies such as SDR and voter ID requirements. No two states have precisely the same voting administration practices, which makes it hard to characterize the different systems for analytic purposes.[15] Moreover, the current questions of interest involve much more difficult estimation problems, and variables that will have a much smaller effect on turnout. The effects of changes in voting practices can run in both directions, by making it harder for some individuals to vote (and thus decreasing turnout among some groups; see Erikson and Minnite 2009), or by increasing the confidence of potential voters that their ballots will be counted fairly and accurately (and thereby increase turnout; see Lott 2006). The possible effects may be small and difficult to estimate.

Our individual-level analysis uses the 2008 Voting and Registration Supplement File of the CPS. The CPS, a common data set in voting analysis, is a large-scale sample survey of the noninstitutionalized population, normally used to collect labor force data. In November of election years, surveyors ask a short set of voting and registration items to a sample of about 130,000 people. Most questions have between 60,000 and 90,000 valid observations.

The voting item (PES1) asks whether people voted in the 2008 presidential election, and has several response categories: respondents can answer yes, no, don't know, refuse to answer, or have no response recorded. Following the common practice, we divide the number of "yes" responses by the total number of individuals who are asked the question, counting as non voters those who refused to answer, did not know, or did not respond. Since the voting items are only asked of individuals 18 years or older, this gives us an estimate of turnout as a percentage of the voting age population.[16] Using

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[14] The effects need not match across the two levels of analysis. For example, variable that significantly increases the likelihood of voting by a small amount could affect aggregate turnout even more strongly as these small individual probabilities cumulate. Kramer (1975) demonstrated that individual and aggregate effects can even run in opposite directions.

[15] There are, for example, at least eight different voter identification requirements, depending on the state: declaring one's name, signing one's name, matching the signature with a signature card on file, a request for voter registration card or proof of identification, a requirement that a the voter present a registration card or proof of ID, proof of ID and signature matched against signature card on file, a request for photo ID, a requirement of photo ID. Alvarez, Bailey and Katz (2008, 7) not further that "combinations of [these] requirements are often in place, such as requiring a voter to state *and* sign his/her name."

[16] At the same time, the CPS excludes the institutionalized population, estimated at about four million in 2000. In other calculations of the voting age population, these individuals are counted.

this method, 64.9% of respondents in the CPS reported voting in 2008 ($n = 92,360$).[17]

  We use a larger number of independent variables than most other models of turnout. Alvarez, Bailey, and Katz (2008, 8-9) describe the "canonical model of voter turnout using CPS data" as using age, residence in a Southern state, education, income, squared values of age and education, and non-White as independent variables (see Wolfinger and Rosenstone 1980). However, the CPS includes a wide range of other data that seem plausible and theoretically justifiable turnout covariates: questions provide data on length of residence, gender, marital status, multi-category racial identity, whether a respondent is a naturalized or natural born citizen, and if naturalized the year of entry into the U.S, and whether a respondent's voting status is self-reported or reported by proxy.[18] Given our interest in estimating the effects of different voting and registration systems, it makes sense to include this additional information about respondents. As in the aggregate model, we include variables describing the five possible combinations of early voting, SDR, and EDR.

  The basic individual turnout model is reported in Table 4. The results are roughly consistent with the aggregate county-level model. EDR has a significant positive effect on the individual likelihood of voting, while early voting has a significant negative effect, even when combined with SDR or early voting. The combination of EDR, SDR and early voting (which offers the maximum of voter convenience) has a small positive effect.

Table 4 about here

Individual Level Matching Analysis

  The results of the individual model with the matched data are reported in Table 5. These results confirm the inferences we drew from the basic model applied to the full sample. Early voting by itself (model V) has a strong negative effect on the individual probability of voting. EDR (model I) increases the likelihood of voting. Same day registration does not improve the effect of early voting; the combination of SDR and early voting decreases turnout likelihood by 1.83 points. EDR and early voting, and the full interactive package of EDR, SDR, and early voting have no effect at all. Most of the

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[17] This is significantly higher than the actual turnout as a percentage of voting age population, estimated at 56.8% (McDonald 2009). This happens for a variety of reasons. Part of the discrepancy is attributed to sampling bias (Burden 2000). Much of it is due to the desire to give socially desirable answers whereby some nonvoters falsely report that they did vote (Gerber, Green, and Larimer 2008). Some of these voters may *think* that they voted, possibly confusing the most recent election with earlier contests. Many studies have concluded that overreporting is most common among people otherwise most likely to vote; there is also evidence, however, that overreporting is also more likely among African Americans (Bernstein, Chadha, and Montjoy 2001). Highton (2005) found that the correlates of turnout were about the same among self-reported and proxy-reported turnout, despite the fact that self-reporters are more likely to overreport their own voting, suggesting that overreporting may not be a significant problem for inference. Unfortunately, there is no easy way to correct for overreporting or estimate the effect it might have on the inferences drawn from empirical models. Katz and Katz (2009) have developed one method, but it requires external information about the probabilities of misreporting.

[18] This latter information is an unusual feature of the CPS survey: respondents can self-report their vote, or have their vote status given by another member of the household (by proxy). Previous research has found that reported turnout among "self reporters" is consistently higher than reported turnout among proxy reporters, by about four percentage points (Highton 2005).

demographic coefficient estimates are similar across models, increasing our confidence in the inferences we draw from the analysis.

Table 5 about here

Because the logit model is nonlinear, the coefficients cannot be interpreted directly. In Figure 3 we display the marginal effects of the registration variables, setting all other independent variables to their means. In the unmatched data we find that EDR increases the likelihood of an individual voting by 2.7 percentage points versus 3.2 points in the matched data. The combination of EDR, SDR, and early voting increases the likelihood of voting by by a small but statistically significant amount using the unmatched data, but is not significant using the matched data. As in the aggregate models, early voting alone lowers the likelihood of voting.

The individual model produces one result that differs sharply from the aggregate results. At the aggregate level, the EDR and early voting combination significantly increases turnout (4.8 percentage points), while it has a significant *negative* effect on the likelihood of an individual voting (-2.8 percentage points). The most likely cause is the small subsample size of this category: in our classification, only Alaska and Idaho combine early voting with EDR, and Alaska is excluded from the aggregate analysis as it does not have county-level jurisdictions. As such, we are cautious about making inferences with so little data.

Overall, the individual-level analysis confirms the basic elements of the aggregate model. EDR has an unambiguous positive effect on the likelihood of voting (as expected), and early voting has a sharp negative effect on the probability that an individual votes.[19]

We have four different approaches that produce a consistent result: early voting has a strong *negative* effect on turnout that is not significantly mitigated by the addition of SDR. If the motivation for election reform is increasing turnout, states would not be wise to pursue early voting, especially on its own. EDR, in contrast, provides a substantial boost in turnout. In three of our four models, an interactive model of EDR, SDR and early voting also increases turnout. Of course, turnout is not the only consideration when states consider changes to election practices. In particular, implementation of new laws requires consideration of both the costs and the ability and willingness of local election officials to comply.

Conclusion

This paper makes three contributions. Methodologically, we have argued that election reforms should not be considered in isolation, as is standard practice in the multivariate models estimated by researchers and in the arguments made by advocates and policy makers. Theoretically, we have distinguished between reforms that are expected to stimulate new voters and those that merely retain continuing voters, and as a result may depress the stimulating effects of Election Day. Substantively, because of our methodological and theoretical innovations, we have shown that some election laws increase turnout but others actually decrease it.

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[19] Clustering the data by state increases the standard errors of the voting practice variables; only the early voting coefficient retains its statistical significance. However, the results are broadly consistent with the unclustered model.

If reformers do want to improve turnout, the only consistent way to achieve this is to permit EDR. SDR itself can raise turnout if the widow for registration and voting is sufficiently long. It appears that early voting on its own robs Election Day of its stimulating effects on marginal voters, unless EDR is also present to provide a vehicle for their mobilization at the last moment. The most common practice in the states is to offer early voting in isolation. If the goal is higher turnout, our findings show that it should be supplemented with SDR or, even better, EDR. It is only by being combined with "one-stop shopping" that early voting yields positive effects.

Figure 1: Combinations of EDR, SDR, and Early Voting in 2008

EDR

SDR

CT
MN
NH

None

None

AK
ID

AZ   KS   OR
CO   LA   SD
FL   MD   TN
GA   NE   TX
HI   NV   UT
IN   NJ   WA
    OK   WV

IA   ND
ME   WI
MT   WY

CA   NC
IL   OH
NM   VT

Early Voting
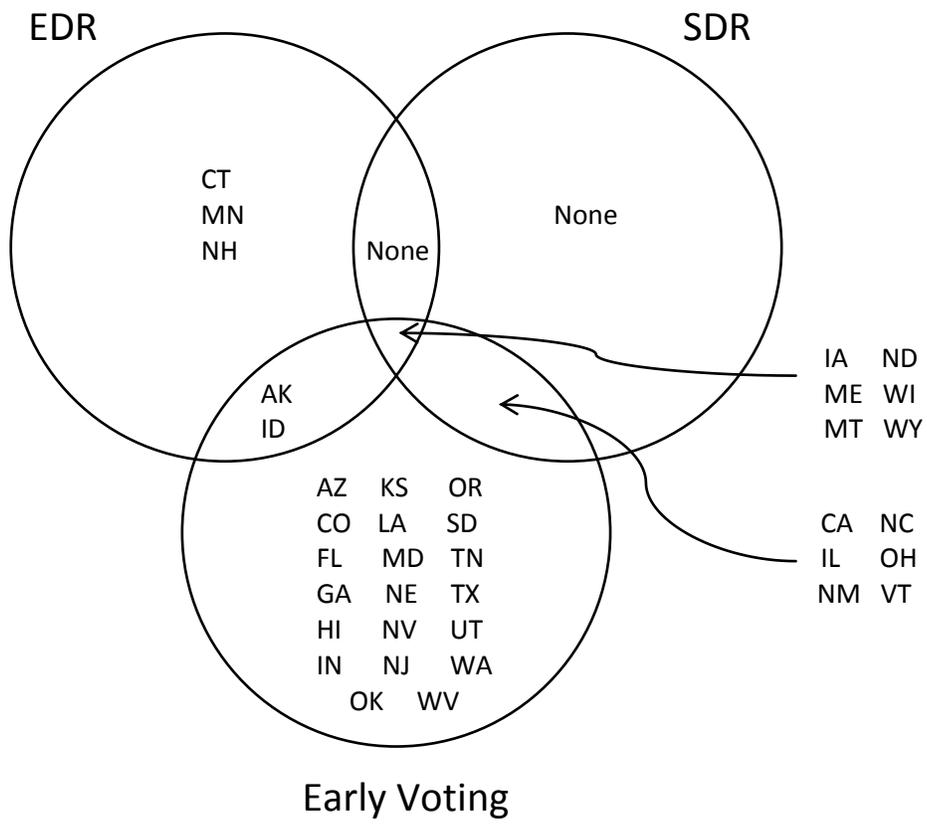
Figure 2: Effects on Aggregate Turnout
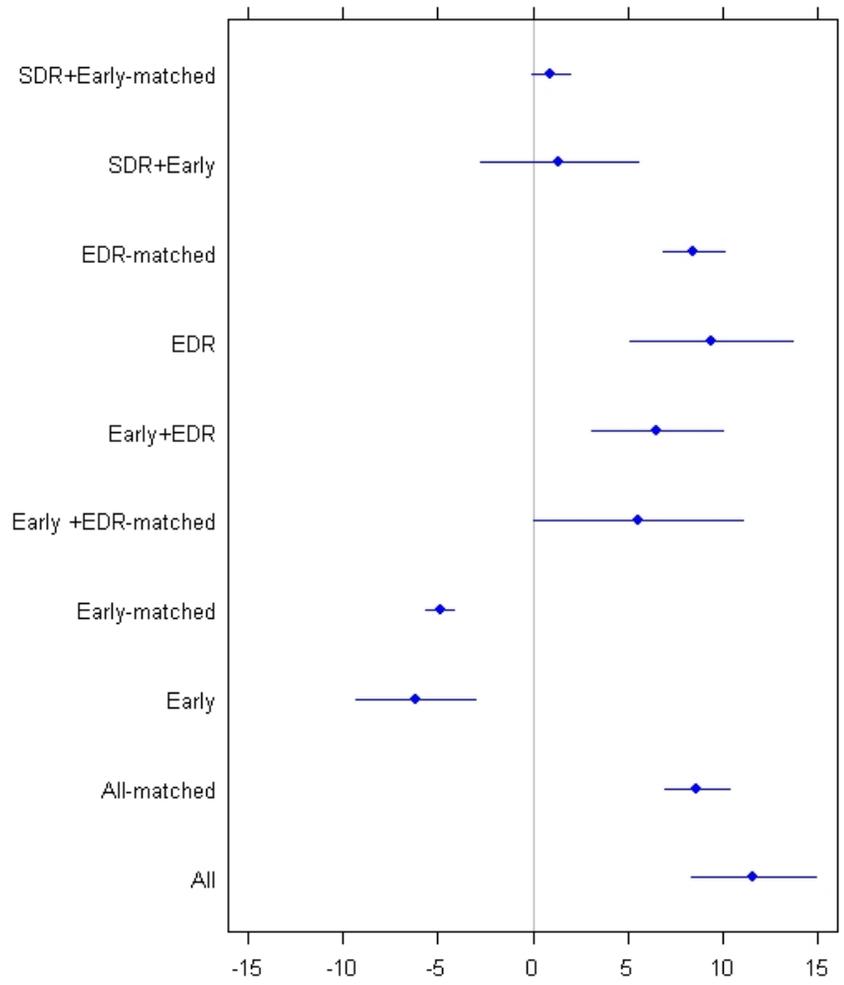
Notes: Dots are effect estimates and lines represent the 95% confidence intervals. Data are based on results in Tables 1 and 3.
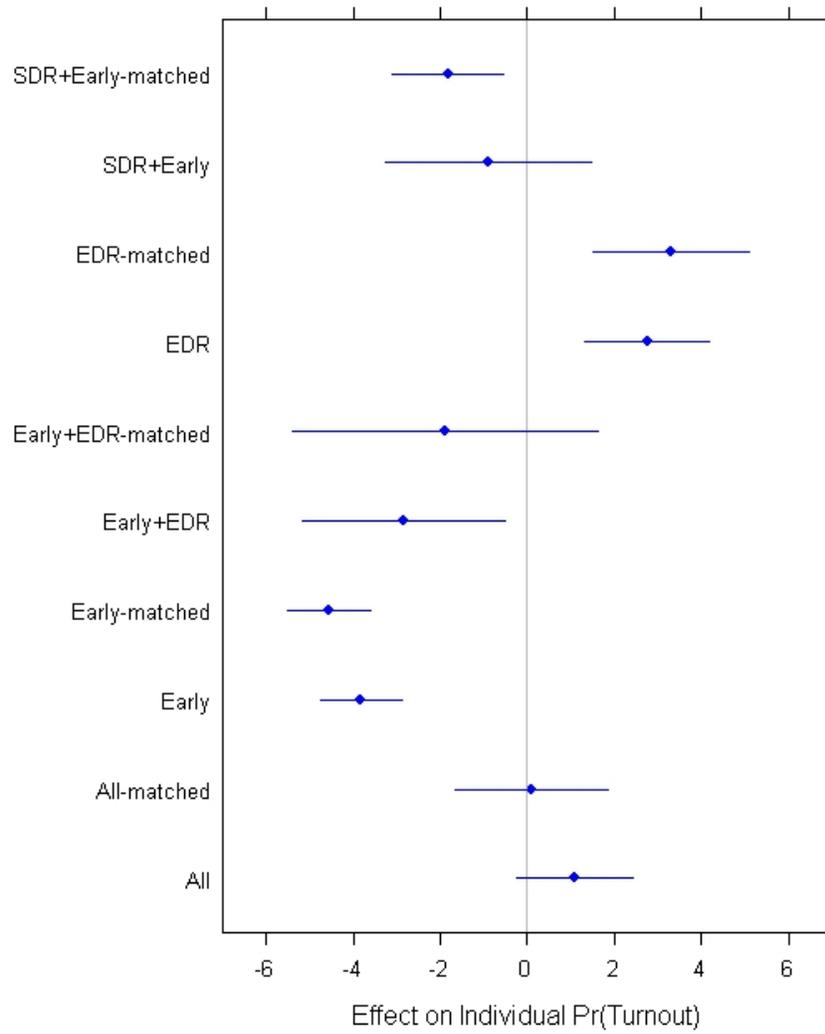
Figure 3: Effects on Individual Turnout

Notes: Dots are effect estimates and lines represent the 95% confidence intervals.
Data are based on results in Tables 4 and 5.

Table 1: Regression Estimates of EDR, SDR, & Early Voting Effects on County Turnout

	I	II	III
EDR	6.37***	10.05***	9.38***
	(.63)	(2.74)	(2.20)
EDR + Early Voting	4.79***	6.29***	6.54***
	(1.14)	(2.18)	(1.75)
EDR + SDR + Early Voting	3.88***	6.84***	11.58***
	(.59)	(1.99)	(1.67)
Early Voting + SDR	.19	3.37**	1.39
	(.42)	(1.59)	(2.12)
Early Voting	-3.36***	-2.53***	-6.13***
	(.31)	(.54)	(1.61)
Closing Date	-.12***	.004	-.05
	(.02)	(.07)	(.07)
ID Requirement	.68*	1.84**	-6.02***
	(.30)	(1.52)	(1.30)
Ex-Felons Barred	-.06	-3.01**	9.45***
	(.35)	(1.00)	(1.23)
Percent Black	.12***	.12***	.08**
	(.01)	(.12)	(.03)
Median Income	.0003***	.0004***	.0002***
	(.0003)	(.00003)	(.00004)
Percent College Graduates	.31***	.25***	.27***
	(.31)	(.03)	(.03)
Percent 65 or Older	.86***	.88***	.62***
	(.04)	(.04)	(.07)
Population (in 100,000s)	-.38***	-.25***	-.08**
	(.10)	(.07)	(.04)
Population Density	-.0004**	-.0003***	-.0003***
	(.0001)	(.0001)	(.0001)
Campaign Competitiveness	-.09***	.18***	-.09***
	(.02)	(.04)	(.04)
Constant	32.55***	21.07***	34.85***
	(1.57)	(3.88)	(3.22)
R^2	.420	.571	.736
Weighted by Population	No	No	Yes
State Fixed Effects	No	Yes	Yes

Notes: $N = 3109$. ***$p < .001$ **$p < .01$,*$p < .05$, one-tailed test.
Cell entries are OLS regression estimates with standard errors in parentheses.
Robust standard errors clustered at the state level.
Dummies for individual states in models II and III are not reported.

Table 2: Effect of SDR Window Length on County Turnout

Length of Window (in Days)	.29***
	(.03)
Closing Date	.03
	(04)
ID Requirement	.77
	(.88)
Percent Black	.05*
	(.03)
Median Income	.0002*
	(.0001)
Percent with BA	.32***
	(.05)
Percent 65 or Older	.67***
	(.07)
Population (in 100,000s)	-.14**
	(.05)
Population Density	-.0003
	(.0002)
Campaign Competitiveness	-.06**
	(.04)
Constant	34.05***
	(2.88)
R^2	.464

Notes: $N = 713$. ***$p < .001$ **$p < .01$,*$p < .05$, one-tailed test.
Cell entries are OLS regression estimates with standard errors in parentheses.
Analysis is limited to states with same day registration.
Robust standard errors clustered at the state level.
Dummies for individual states not reported.
Ex-felon disenfranchisement variable omitted because it does not vary in SDR states.

Table 3: Regression Estimates of EDR, SDR, & Early Voting Effects on County Turnout, Using Matched Data

	I	II	III	IV	V
EDR	8.44***				
	(.82)				
EDR + Early Voting		5.54*			
		(2.81)			
EDR+ SDR + Early Voting			8.61***		
			(.87)		
Early Voting + SDR				-.96*	
				(.50)	
Early Voting					-4.87***
					(.38)
Closing Date	.21*	.10	.08**	-.04	-.39***
	(.10)	(.38)	(.03)	(.05)	(.03)
ID Requirement	-1.24	a	2.41**	3.43***	.91*
	(1.72)		(1.00)	(.58)	(.43)
Ex-Felons Disenfranchised	a	a	-.38	a	-.34
			(6.06)		(.40)
Percent Black	-.48*	-3.14*	-.88***	.15***	.13***
	(.21)	(1.55)	(.29)	(.02)	(.01)
Median Income	.0001	.0005*	.0001*	.0004***	.0004***
	(.0001)	(.0002)	(.0001)	(.00004)	(.00003)
Percent with BA	.44***	.35*	.12*	.26***	.36***
	(.09)	(.16)	(.06)	(.04)	(.03)
Percent 65 or Older	.56***	.77**	.37***	1.21***	.95***
	(.13)	(.30)	(.09)	(.09)	(.04)
Population (in 100,000s)	-.76**	2.38	1.79**	-.22***	-.52***
	(.27)	(2.16)	(.64)	(.05)	(.09)
Population Density	.002	-.03	.003	-.001***	-.001***
	(.002)	(.02)	(.002)	(.0003)	(.0004)
Campaign Competitiveness	-.25*	.04	.12*	-.15***	-.22***
	(.12)	(.25)	(.06)	(.03)	(.03)
Constant	38.65***	20.71	41.37***	23.91***	37.93***
	(5.01)	(12.85)	(3.44)	(.05)	(1.78)
Adjusted-R^2	.563	.375	.282	.405	.438
N	180	78	407	611	1,931

Notes: ***$p < .001$ **$p < .01$,*$p < .05$, one-tailed test.
Cell entries are OLS regression estimates with standard errors in parentheses.
[a]Variable was dropped due to lack of variation.

Table 4: Logit Estimates of EDR, SDR, & Early Voting Effects on Individual Turnout

EDR	.142***
	(.04)
EDR + Early Voting	-.138***
	(.080)
EDR + SDR + Early Voting	.055*
	(.034)
SDR + Early Voting	-.043*
	(.029)
Early Voting	-.189***
	(.024)
Education	.601***
	(.010)
African-American	.728***
	(.032)
Hispanic	-.062**
	(.033)
Self-Reported Vote	.827***
	(.018)
Naturalized Citizen	-1.05 ***
	(.102)
Naturalized 10+ years	.471***
	(.108)
30-day Registration close	-.116***
	(.022)
Married	.424***
	(.02)
Residence 1 Year	.267***
	(.026)
Income	.081***
	(.003)
Gender	.148***
	(.018)
Age	.025***
	(.0008)
Age 18-24	.418***
	(.033)
Age over 75	-.117**
	(.042)
South	-.064**
	(.025)
Campaign Competitiveness	-.006***
	(.001)
Constant	-3.82
Pseudo-R^2	.1446
Pct. Correct Predicted (null)	73.5 (68.8)
N	74,327

Notes: ***$p < .001$ **$p < .01$,*$p < .05$, one-tailed test.
Cell entries are OLS regression estimates with standard errors in parentheses

Table 5: Logit Estimates of EDR, SDR, & Early Voting Effects on Individual Turnout, Using Matched Data

	I	II	III	IV	V
EDR	.185*** (.052)				
EDR + Early Voting		-.09 (.086)			
EDR+ Early Voting + SDR			.052 (.047)		
Early Voting + SDR				-.094*** (.033)	
Early Voting					-.226*** (.025)
Education	.606*** (.028)	.69*** (.047)	.66*** (.025)	.598*** (.183)	.593*** (.013)
African-American	.522*** (.122)	.074 (.252)	.66*** (.157)	.728*** (.06)	.718*** (.041)
Hispanic	-.297*** (.122)	-.544*** (.174)	-.025 (.127)	.078 (.052)	-.138*** (.042)
Self-Reported Vote	.855*** (.052)	.727*** (.081)	.721*** (.044)	.819*** (.034)	.858*** (.024)
Naturalized Citizen	-1.27*** (.291)	-1.47** (.547)	-1.16*** (.297)	-.70*** (.164)	-1.18*** (.136)
Naturalized 10+ years	.545** (.307)	.912 (.574)	.29 (.318)	.147 (.172)	.587*** (.143)
Married	.518*** (.057)	.376*** (.086)	.408*** (.048)	.395*** (.037)	.418*** (.026)
Residence 1 Year	.289*** (.076)	.032 (.109)	.21*** (.061)	.249*** (.049)	.337*** (.034)
Income	.085*** (.008)	.09*** (.011)	.086*** (.006)	.074*** (.005)	.084*** (.003)
Gender	.176*** (049)	-0.25 (.077)	.094** (.041)	.109*** (.032)	.167*** (.023)
Age	.02*** (.002)	.026 (.034)	.03*** (.002)	.02*** (.001)	.02*** (.001)
Age 18-24	.475*** (.09)	.344** (.141)	.582*** (.079)	.408*** (.06)	.36*** (.043)
Age over 75	.198* (.12)	-.137 (.214)	-.232** (.097)	-.051 (.08)	-.107** (.055)
South	-.062 (.09)	.001 (.134)	-.239*** (.072)	-.121** (.051)	-.10*** (.028)
Campaign Competitiveness	-.009*** (.003)	-.005 (.006)	-.003* (.002)	-.005*** (.002)	-.006*** (.001)
Constant	-3.8	-3.74	-4.09	-3.54	3.9
Pseudo-R^2	.145	.152	.141	.131	.15
Pct. Correctly Predicted	76.5%	72.5%	73.8	73.6%	73.3%
(null)	(73.7)	(67.1)	(70.4)	(70.0)	(68.1)
N	10594	3811	13847	22396	43302

Notes: ***p < .001 **p < .01,*p < .05, one-tailed test.
Cell entries are logit regression estimates with standard errors in parentheses

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