How to measure and assess
the turnout effects of election reforms

Andrew Menger
Washington University-St. Louis

Robert M. Stein
Rice University

Abstract: Many states allow some form of non-traditional precinct voting, from voting early in-person to various forms of mail-assisted balloting. Research on how these voting reforms impact voter participation has produced a wide range of mixed findings. We review this literature and describe how the research designs and measures used by past studies may have biased their results. We then offer a new theoretical approach for testing the relationship between convenience voting reforms and voter turnout that addresses some of the pitfalls in previous measurements of reforms. We also identify best practices for how to implement our new measures using a difference-in-difference research strategy that is robust to many confounding factors and describe a potential sustainable database for undertaking this research over time.

1. Introduction

The conduct, administration, and even the outcome of American elections has become increasingly contentious. Legislation at the federal, state, and local level has sought to address issues related to how we conduct our elections, count ballots, and vote on or before Election Day. This condition has spawned a demand for research on the efficiency, reliability, integrity, and convenience of our elections.   Unfortunately, the supply of data on election reforms and the quality of research has not met this demand. To date, research on how alternative voting methods affect participation has produced a diversity of findings, with only modest consensus on how convenience voting reforms, and election administration more broadly, affect voter participation. The confusion associated with the impact of how we conduct elections on the electorate has significant consequences.   Research has shown that voter confidence in the integrity of election administration waxes and wanes with new election laws (Suances and Stewart 2011; Alvarez, Hall and Llewellyn 2008; Atkeson, Alvarez, and Hall 2008). Voters who lack confidence in the administration and outcome of elections might be less likely to participate in and accept the outcome of future elections (Atkeson, Alvarez, and Hall 2008). Furthermore, it is not clear how decisions by election officials to pursue different voting policies, such as limiting the days or hours of early voting, will affect the composition and participation of the electorate.

This article offers an assessment of how research on election administration is conducted and a strategy to remedy many of the issues with research to date. While there are also cost issues and demographic composition questions related to voting reforms, we focus specifically on the quest for greater participation in elections, a central tenet to the legitimacy of our elections and their outcomes. The quest for greater participation in elections has led states and many of their jurisdictions to offer voters more options to cast their ballot beyond Election Day precinct

polling places (Stein and Vonnahme 2012). These options include various methods of mailing ballots to voters before Election Day to be filled out at their convenience, allowing voters to cast their ballots prior to Election Day at centralized early voting centers, and replacing neighborhood precinct locations with Election Day Vote Centers (EDVCs). Since these policies were adopted with voter convenience as at least one of the major considerations, we refer to these collectively as "convenience voting reforms." If we make voting more convenient and less costly, how many more people will vote? The literature does not provide a clear answer to this question, with many studies presenting conflicting findings on voting reforms' turnout effects.

What is the reason for the diversity of the findings on convenience voting reforms' effects on voter turnout? We argue that the quality and type of data on elections and how researchers have sought to explain the effect of election laws on turnout are deficient in several ways. First, previous research has mostly relied on a limited definition of convenience voting which uses simple measures for the presence of a voting option regardless of how convenient it actually is to the voter. These measures group together voting reforms that are implemented in various ways, some of which may not offer accessible options to most voters that reduce their difficulty of voting. Second, research on the turnout effects of convenience voting has largely been limited to cross-sectional or time-series designs rather than panel data. These studies often employ either aggregate state-level data, data from a few selected counties, or individual-level analyses of voter files. These units and levels of analysis fail to capture how convenience voting is implemented across and within states and limit researchers' abilities to control for unmeasured confounding factors. A time-series cross-sectional (panel) research design that captures how counties across the country implement different options for convenience voting allows us to better measure the turnout effects of voting reforms. Part of the reason for the lack of panel data

studies at the county-level is the absence of a common set of recurring data on all elections by jurisdiction that can serve as the basis for studying the impact of common election laws across time and jurisdictions. We identify the Election Assistance Commission's semi-annual *Election Administration and Voting Survey (EAVS)* as filling this need. We discuss how the *EAVS* might be used and enhanced for studying election administration in the U.S, and the data cleanup work that is necessary for these data to be useful.

Our paper proceeds in the following way. In section 2 we briefly describe the most common and popular methods of convenience voting and summarize the existing research on their impacts on voter participation. In section 3 we offer an alternative conceptualization for measuring convenience voting options and describe its advantages for studying voting reform policies. In section 4 we present best practices for creating a research design for testing and measuring the impact of convenience voting on turnout. In section 5 we introduce the EAC's *Election Administration and Voting Survey* as standard for conducting research on election administration, and describe the work needed on the *EAVS* to make it useful as well as how the *EAVS* can be enhanced. We conclude in section 6 with a summary of our recommendations for future research on the turnout effects of convenience voting methods.

2. *Convenience voting methods*

The literature identifies several popular types of convenience voting reforms that have been adopted by states or smaller voting jurisdictions over the last 50 years. These include various forms of mail-assisted voting (i.e., absentee voting by mail with and without excuse, permanent absentee voting, and universal vote by mail), in-person early voting, Election Day vote centers, and Election Day and/or same day voter registration.

Included in convenience voting reforms are several forms of mail-assisted voting, with the use of mailed ballots increasing from no-excuse absentee balloting (currently available in 27 states), to permanent absentee mailing lists (practiced in 8 states), to universal vote-by-mail (VBM) elections, available in 3 states. All three of these voting systems send ballots to voters through the mail and allow voters to return their ballots either by mail or in-person at centrally located facilities. Mail-assisted voting eliminates some of the costs of voting such as the time spent traveling to polling places, waiting in line to vote, and using unfamiliar voting equipment. Universal VBM elections also eliminate the initial cost of requesting a mailed ballot by automatically mailing ballots to all registered voters, and permanent absentee lists only require the voter to sign up once to receive mailed ballots for all subsequent elections.

Scholarly analyses of the turnout effects from mail-assisted elections have produced inconsistent findings, with no clear consensus on a positive turnout effect. Table 1 shows an overview of major studies on no-excuse absentee and permanent absentee list policies conducted in the United States in the last 25 years. Depending on the setting and research design used, estimates vary from a positive effect (Karp and Banducci 2001; Larocca and Klemanski 2011) to a null or even negative effect (Giammo and Brox 2010). Even if no-excuse and permanent absentee voting do not have a clear effect, we would expect that universal VBM elections should have a positive effect on turnout due to both the reminder effect for all registered voters and the elimination of the cost of requesting to be mailed ballots ahead of time. Table 2 shows the research designs, contexts, and findings of these studies, which tend to find positive effects but not across the board. Studies of the turnout effects of universal VBM in federal elections are generally limited to two states, Oregon and Washington (Southwell and Burchett 2000; Gronke and Miller 2012; Berinsky et al. 2001; Gerber et al. 2013). Most of these studies have found a

positive effect of VBM on turnout, although not all of them (Gronke and Miller 2012; Southwell 2009). A few other studies have examined whether VBM increases turnout in a variety of local, municipal, and special elections, including mandated vote-by-mail in some California precincts (Arceneaux et al. 2012; Bergman and Yates 2011; Kousser and Mullin 2007). More of these studies find a positive effect in lower-salience elections, but even here some studies like Bergman and Yates (2011) find negative effects. Clearly there is no unequivocal link in the literature between mail-assisted voting and voter turnout. The overwhelming majority of these studies are either time-series studies in a single state or cross-sectional studies comparing small jurisdictions. Time-series studies of mail voting of any type are generally limited to the state level of analysis and often study only a single state (like Oregon) over time. One exception to these designs is the panel dataset used by Gerber et al. (2013), who report a significant and substantial turnout effect from adopting VBM elections using both county-level data and matched individual-level data in Washington.

Early voting began in Texas in 1992 and was intended to increase the convenience of voting by increasing the length of the voting period over several weeks and by providing large, centralized voting locations. Beginning with Texas in 1991, 37 states have adopted some form of early voting if filling out absentee ballots in-person is counted as early voting, or 21 states under a stricter definition of early in-person voting using non-absentee ballots at centralized polling places. This policy should have the effect of decreasing voters' costs of voting by allowing voters to pick the time and location that provides them the lowest cost in terms of difficulty and inconvenience. Since the task of voting that is reported to be difficult by the most people is finding time to vote (Menger 2018) providing more times to vote should increase turnout by allowing voters to minimize this cost.

The consensus in the literature is that early voting has not produced consistent and positive effects on turnout. Table 3 presents a summary of past studies on early voting including their detected turnout effects as well as their research designs and settings. While two studies (Wolfinger, Highton, and Mullin 2005; Stein and Garcia-Monet 1997) have found small increases in turnout associated with early voting, the majority of the studies (Fitzgerald 2005; Gronke, Galanes-Rosenbaum, and Miller 2008; Primo, Jacobsmeier, and Milyo 2007) have failed to find a turnout effect or even found a negative effect (Burden et al. 2014; Larocca and Klemanski 2011; Richey 2008). Moreover, others failed to detect an increase in turnout among historically under-represented populations including younger voters and non-Anglo voters as a result of early voting (Stein 1998; Hanmer and Traugott 2004; Southwell and Burchett 2000; Berinsky 2005). As for other voting reforms, the modal design for studying the turnout effect of early in-person voting has been cross-sectional analyses in single elections or time-series conducted within single states. As we will describe in more detail later, these research designs are open to many types of confounding factors.

Election Day Vote Centers do not allow voters to ballot on different days, but similarly to early voting they provide the ability to vote at any location, which should decrease transportation costs for most voters. We would expect that Election Day Vote Centers should have similar null or negative effects as early voting since it changes the types of voting locations, but there have not been enough studies to confirm this. One study conducted in Colorado showed positive effects of EDVCs on turnout (Stein and Vonnahme 2008), but this voting reform needs to be tested in other settings. Cortina and Rottinghaus (2018; 2019) have confirmed these findings in Texas, albeit for lower turnout elections.

Many states have also implemented convenience voting reforms related to voter registration, ranging from motor-voter laws to allowing voters to register at the same time that they vote. Table 4 shows a summary of the major studies on voter registration reforms adopted in the United States over the last few decades, from increasing the registration period to same day or Election Day registration (Burden et al. 2014). Unlike studies on other types of voting reforms, the research on relaxed registration requirements has mainly found positive and statistically significant effects from these policies on turnout. While more of these studies have used time-series cross-sectional datasets than have been used in studying other reforms, these studies have often failed to include fixed effects for units and time periods or adequately account for serial correlation between observations as recommended by Bertrand et al. (2004).

In summary, the research on the turnout effects of convenience voting reforms is at best mixed, leaning towards a null effect and in some instances a negative finding (e.g., early voting). Although some reforms like universal VBM and relaxed registration requirements show more positive effect estimates on average, even these findings are contradicted by some of the studies on these policies. These conclusions are drawn from research that shares two common elements that may have biased their results and limited their ability to detect relationships between reduced voting costs and turnout. First, researchers have measured convenience voting reforms using simplistic measures of policy adoption that do not capture the actual accessibility and convenience of these policies to the voters. Their measures of convenience voting methods are usually simple dichotomous variables for whether the state or jurisdiction allows voters to choose from one or more convenience voting options. We will return to this point and our recommendations for improving these measures in section 3.

Second, the research designs and datasets used in these studies have not used the best practices of policy analysis research to reduce the impact of unmeasured confounding factors on the relationships between reforms and turnout. The use of cross-sectional studies in single elections and time-series studies within single states are most problematic in their inability to control for many confounding pathways. Single state studies cannot leverage information about the variation in elections laws and their implementation across states and jurisdictions, but they have the advantage of studying the effect of one clear policy change. However, they are susceptible to confounding factors over time, such as differences in turnout from election salience. Cross-sectional studies can use information on the variation in laws across jurisdictions, but often group together policies that share only superficial similarity. They are also highly susceptible to confounding factors like demographics and political culture that may shape both the turnout levels and voting policies of jurisdictions. Some studies have occasionally used matched individual-level data, which is more successful at blocking pathways related to individual characteristics but does nothing to deal with unmeasured factors like mobilization differences between voting jurisdictions. Although some studies have used limited panel designs, even these studies do not always follow best practices for accounting for confounding factors and other issues like serial correlation in observations (Erikson and Minnite 2009; Wing et al. 2018).

Many past studies have also relied on datasets that are open to other types of issues that arise from their method of data collection, such as self-reported voting in survey datasets and purging of registered voters in individual-level voter files. Several studies of the impact of election laws on voter turnout have relied on individual level data compiled from surveys (Hajnal, Lajevardi and Nielson 2017; Grimmer et al. 2018; Burden et al. 2014). Survey-

generated reports of voter turnout and method of voting are subject to error arising from misreporting by the respondent, either intentional or unintentional. Overreporting of voter turnout from self-reported voting measures is a well-known phenomenon (Brenner 2012; McDonald 2003; Belli et al. 2001). Some other studies rely on voting histories from official individual-level voter registrar files (Citrin, Green, and Levy 2014; Alvarez 2011). Most studies that use official voter files to verify voting histories employ cross-sectional designs for a single or small number of states or jurisdictions. These studies are limited in addressing the threats from omitted variables and alternative explanations of turnout other than those caused by individual-level characteristics. When voter files are used across multiple elections to address these threats, issues arise from people who enter and leave the registered voter pool between elections. How people are treated when they enter and leave the pool of registered voters can introduce bias in measures of turnout that rely on these files over time since turnout rates depend on a pool of registered voters that is constantly changing. Such datasets can be reduced to those people who were continuously registered over the time period being examined, but these voters may differ in relevant factors like residential stability from the entire voter pool.

We suspect the mixed results on the turnout effects of convenience voting may arise from the limited ability of these research designs to control for confounding factors as well as past studies' reliance on simplistic measures of convenience voting policies employed in these studies. Combining both cross-sectional and time-series data together into panel datasets allows researchers to include control variables that vary both between units and over time. Even when information across observations and time is limited, researchers can employ statistical methods like fixed effects or difference-in-difference designs to run statistical analyses that are robust to any unmeasured factors affecting turnout that are constant over time inside of geographic units

(or uniform across units over time). Following the best practices recommendations that other public policy scholars have recommended (Erikson and Minnite 2009; Wing et al. 2018) requires collecting panel datasets, preferably at the jurisdictional level where policies are actually implemented, which are typically counties. In the next section, we propose new measures for convenience voting reforms and justify how they may be used to study turnout effects from alternative voting methods. After this, in section 4 we will return to the idea of panel datasets and best practices for studying reforms that were not implemented randomly.

3. *Proposing a new measure for convenience voting reforms*

Most electoral reforms provide voters with choices for how they may cast a ballot, for example, on or before Election Day, at different locations, days and times. Unlike other election laws (e.g., voter identification requirements) voters are free to choose from any method of voting that is available in their state and jurisdiction. Previous research has measured the impact of electoral reforms on turnout by examining the opportunity to use alternative modes of voting, typically employing a dichotomous measure of whether the reform is or is not available to voters. This choice of measurement carries with it an implicit assumption about how electoral reforms affect turnout, which is that all policies coded in the same way (for example, all types of early voting laws) are equivalent. However, the convenience to the voter of balloting early, by mail, or in any other manner depends on a number of implementation decisions by the state and county. Additionally, the presence of numerous alternative voting options may reduce the impact of adding one more method since most voters may already be able to minimize their costs to low levels using their already available methods.

The implementation of electoral reforms varies by jurisdiction and may influence the likelihood and incidence that a specific method of voting is used. For example, in the case of

early voting policies the opportunity to vote early is not uniform across or within states, so its effect on turnout may not be adequately captured with a simple dichotomous measure of opportunity. The days and hours of voting and types of days available (weekends vs. weekdays) depend on the details of state policies. Additionally, the number, location, staffing, and equipping of early and Election Day polling locations varies by county. Each of these conditions has been shown to have an independent effect on how, where, when, and whether a person chooses to vote (Fullmer 2015; Stein and Vonnahme 2011, 2012; Losco, Sheele and Hall 2010; Neely and Richardson 1996; Stein and Garcia-Monet 1997) and election outcomes (Meredith and Malhotra 2011; Berger et al. 2006). Distance from a polling location, which varies by geography and population density, can also significantly influence whether, when, where, and how a voter chooses to cast their ballot (Gimpel and Schuknecht 2003; Dyck and Gimpel 2005; Haspel and Knotts 2005). As Fullmer observes "[T]he literature widely omits consideration of important differences in early voting implementation within states (2015:21)," which may account for the indeterminate findings about the effect of early voting on turnout.

Counties also vary in their efforts to inform and educate their electorate about alternative voting options (Hood and Bullock III 2011). Candidates and parties also have strong incentives to mobilize voters by encouraging them to use more convenient and less costly means of voting (Leighley, Stein and Owens 2003). Candidates may or may not choose to mobilize some voters depending on how the costs and benefits of mobilizing voters varies with different election systems (Burden et al. 2014; Oliver 1996; Dubin and Kalsow 1996; Patterson and Calideria 1985; Thompson 2004; Fortier 2006). If voters are not aware of alternative voting methods, their potential convenience will not matter to their decisions over whether to vote or not.

We can think of convenience as a complex concept that involves a variety of factors that are influenced both by election administration and by the voter's characteristics. These include a voter's travel time and method, hours of operation, ease of parking or disabled access, and availability of information, to name a few. Simple dummy variables for voting reforms do not capture this variation, and even measuring number of locations or hours of operation is missing important factors, including individual-level variation in the convenience of the method. For example, we would not expect early voting to provide a relevant cost-reducing alternative voting option if it were only offered at times that are not convenient to most voters, such as between 12 AM and 4 AM on weekdays. Likewise, if early voting is only offered at one location that is far from most voters and has little parking and few public transportation options, we would not expect that this offers a cost-reducing voting method for most voters. The convenience of a voting method largely depends on the ways in which it is implemented, and the details of this implementation depend on local circumstances like population density, typical working hours and transportation methods, and other factors like parking. This makes comparison of voting methods' convenience very difficult using most observable factors, since the overall convenience depends on many combinations of factors. Should we measure the number of days of early voting? Or the overall number of hours or early voting? What about the number of locations? Or the parking and public transit options at these locations? There are so many relevant factors to measure that creating concise measures of these policies' implementation is very difficult.

Although we cannot measure the true convenience of alternative voting methods to the voters directly, we can let the voters demonstrate this convenience through their behavior. We believe that voters' decisions to use alternative methods of voting provide clear signals about the convenience of these methods to them. For this reason, we propose a new method for measuring

voting reforms: measuring the convenience of alternative voting methods by their rate of use. Although the true convenience of these policies cannot easily be measured, we can use a proxy measure drawn from voters' behavior. We propose using the proportion of voters who balloted using each alternative method of voting to estimate the association between convenience voting reforms like early voting and voter turnout. Skeptical readers might note that these bivariate relationships are not necessarily the direct effect of a policy on turnout, as some factors could influence both voters' use of methods and turnout simultaneously. Interpreting these relationships as turnout effects depends on the assumption derived from the extended rational choice theory of voting that some of the people who use different voting methods would not have voted without them. It also depends on the assumption that there are no unobserved variables that cause people to be both more likely to use convenience voting methods and more likely to vote in general. To make these assumptions more plausible, these proportion measures (which we term "percent usage measures") should be used with research designs that control for unmeasured confounding factors by including adequate control variables or through the research design. We will return to best practices for research design and the data required for these designs in section 4, but first we will elaborate more on the theoretical basis for using percent usage measures as proxies for convenience voting reforms.

Since it may not be immediately clear how our measurement strategy of percent usage measures captures the convenience of available voting methods, it is worthwhile to detail the theoretical basis for this strategy. In doing so, we hope to quell some of the concerns that interested readers may have regarding using variables that measure the use of voting methods rather than dummy variables for the presence of a voting method option. Our theoretical basis begins with an extended cost-benefit rational choice model of voting, in which a potential voter

weighs the costs that must be "paid" in order to cast a ballot against his or her motivation to vote. In using this framework, we are not restricting the benefits of voting to economic utility alone, but consider that emotional suasion, social rewards, and other types of motivations may lead someone to want to vote. Furthermore, we consider that the costs of voting include opportunity costs such as the substitution cost of time and mental energy in addition to more explicit expenses like childcare and transportation costs which must be incurred to vote. This model depends on the assumption that individuals try to maximize their overall utility by taking actions that increase their utility and avoiding actions that would decrease it.

Even if rational voting at the individual level does not hold perfectly (i.e., voting only when benefits outweigh costs), our characterization of how voters choose to vote holds as long as voters consider these costs of voting against their various motivations to cast a ballot and respond to increased or reduced costs in expected ways when observed in aggregated groups. In other words, as is the case for price models in economics, if the model decently characterizes the behavior of large groups of people, it is useful even if it is not a perfect model of behavior for any given individual. When observed in the aggregate, we expect voter turnout to respond to costs and benefits in ways that correspond to this model, even if some individuals deviate from this decision-making method in random ways. As long as these deviations are not systematically toward taking actions that involve higher costs than benefits, we should observe a "rational voting market" in that voter turnout will go up the benefits increase, such as when elections are particularly salient, and it will go down if costs increase dramatically, such as by major storms on Election Day or threats of violence at polling places.

A logical extension of the cost-benefit model of voting is that if voters are offered more than one option in how to cast a ballot, they will adopt the method that incurs the lowest level of

costs to them. We label this proposition on voters' behavior "cost-minimizing." Note that this extension of the cost-benefit model relies on fewer assumptions than the original model, because while the cost-benefit model of voting assumes that voters weigh costs against benefits, cost-minimizing only assumes that voters weigh packages of costs against each other. Regardless of whether one believes that voters can actually weigh the sometimes intangible benefits of voting in a utilitarian way, as long as voters can logically consider cost factors like how much time it takes to vote (and what else they could be doing with that time), then they can be cost-minimizers even if they are not strictly "utility maximizers" according to the cost-benefit model of turnout. In other words, cost-minimizing behavior only assumes that voters can consider how difficult it is to cast a ballot by each method (i.e., in-person on Election Day vs. early in-person) and will typically choose the method that requires them to "pay" the smallest overall package of costs.

If we assume that people are cost-minimizing, then when offered multiple voting methods as options, their observed choice of voting method will reflect the method that required the overall smallest package of costs to them. Since the costs of voting are different for each individual depending on life circumstances and environment (Menger 2018), this choice will not be the same for all voters. When observed in the aggregate, however, the pattern of voting methods chosen by voters will reflect which methods offer the most convenience (lowest costs) to the average voter. When voters are observed in large groups, this pattern will be present even if some individuals randomly deviate from choosing the lowest cost method, as long as such deviations are randomly driven by other factors and are not systematically toward higher costs.

If we can safely assume that when voters are observed in the aggregate they exhibit cost-minimizing behavior in choosing voting methods, then the rate by which various voting methods

are used reflects their level of convenience to the average voter. In other words, if a policy

requires much lower costs of voting to most potential voters than in-person Election Day polling,

then we should observe the majority of voters using this alternative voting method. There could

be some exceptions to this logic, such as in the first year of implementation when voters have not

yet identified how convenient the method is to them. Overall, however, we expect that the rate

by which large groups of people vote by a particular method like mail balloting should reflect the

convenience of this method to them relative to other methods of voting.

If we can assume that the rate by which a group of people vote using each available

voting method represents the convenience of these methods relative to other methods, we can use

the rates by which these voting methods are used as proxies for their convenience to the average

voter. To be clear, we are not proposing that the method of voting people use is a direct measure

of their voting costs, at least at the individual level. We are merely proposing that when

observed in a larger aggregate group, the costs (or conversely convenience) of each method

relative to others should be reflected in its rate of usage by the group. This makes percent usage

measures for each voting method excellent proxies for the convenience of these voting options to

the average voter, which depends largely on how these policies are implemented and how this

application addresses the costs unique to each voting environment and population.

Interested readers may raise some concerns over using our proposed measures of percent

usage for each voting method in a model studying how convenience voting affects voter turnout.

The most immediately obvious concern arises from including the number of voters who cast

ballots in both the numerator of the dependent variable (# of voters who cast ballots divided by #

of registered voters) and the denominator of the independent variable (# of voters who used a

particular method divided by # of voters who cast ballots). One may be concerned that since the

number of voters who cast ballots is included on both sides of the model's equation, there may

be systematic dependencies between the measures that would result in false statistically

significant relationships between the variables. Others may be concerned that since a person

must cast a ballot in order to vote by a particular method, and likewise must choose some method

to vote in order to cast a ballot, these two variables are inter-related in a way where they should

not be measured for correlation with each other. For these reasons, an exploration of how a

correlation could be present (or not) between these measures is in order, which we make

available for interested readers in Appendix A. In the situation of a perfect counterfactual with

no confounding factors, these variables should only be correlated in the case of actual turnout

effects from the convenience of the alternative voting options, and if no turnout effect is present,

they will show no correlation.

4. *Best practices for research designs*

In the previous section and Appendix A, we described how the percent usage measures

capture the degree to which convenience voting policies affect voting by reducing the costs of

voting, potentially driving some voters to vote who would not do so otherwise. If we could

observe a true counterfactual situation, where the same jurisdictions are observed in the same

elections both with and without these voting options, simply correlating these two measures

would provide a test of our cost-benefit theory. Since we cannot observe a true counterfactual

situation, whether a correlation between the percent usage variables and voter turnout represents

a causal effect of voting policies on voter turnout depends on the assumption that there are no

confounding factors creating backdoor pathways between the use of convenience voting methods

and voter turnout. This assumption may not be justified under naïve models that run simple

correlations between these measures, either across jurisdictions or in the same jurisdictions over

time. However, by thinking carefully about research design and which types of factors are most plausible at confounding these relationships, we can use best practices in policy analysis research to make this assumption justifiable. We can also use robustness checks such as sensitivity analysis to confounding factors in order to examine how large of an impact these factors would need to have on our variables to falsely generate any observed relationships between them.

There are several sources of potential confounding between percent usage measures and the rate of voter turnout. The first, which has been mentioned by many scholars (Stein and Vonnahme 2008) is that low voter turnout in the past may have led states or counties to adopt convenience voting reforms in efforts to drive up low voter turnout. If this is true of the policymaking process, simple regression to the mean after low turnout may be falsely attributed to the effects of these reforms. Alternatively, we may observe lower turnout associated with these policies in cross-sectional analyses if they were only implemented in areas with historically low turnout. Another potential source of confounding between these two measures comes from demographics and political culture, which may influence both the adoption of voting reforms and high or low rates of voter turnout. For instance, many scholars have noted that Oregon, which was the first state to adopt universal mail balloting, also has a tradition of high voter participation which may have led to constituents pressuring the state to adopt this policy (Gronke and Miller 2012; Southwell 2000, 2009).

In addition to demographics influencing the adoption of convenience voting policies, they may also affect voters' use of alternative voting methods in addition to their more well-known effects on voter turnout. For this reason, when using our proposed measures of convenience voting options, demographic and geographic factors may create backdoor pathways in less direct ways that do not involve policy adoption. For example, if rural voters are more likely to ballot

by mail due to their long commutes to in-person voting locations, and they are disproportionately more likely to vote due to their age, race, religion, or social networks, this could create a positive correlation between percent mail balloting and voter turnout that is not fully caused by the turnout effects of being able to use a mailed ballot. For this reason, when using our proposed percent usage measures researchers need to be especially careful about controlling for confounding pathways caused by demographic characteristics.

There are a number of approaches that researchers could take in order to reduce the possibility of these factors confounding the relationships between convenience voting policies, or our proposed percent usage measures, and voter turnout. Some of these approaches are more promising than others due to their ease of implementation and which types of backdoor pathways they might block. One strategy would be to measure and control for all observable variables that differentiate jurisdictions and may create these pathways, such as political culture, race/ethnicity, age, past levels of voter turnout, etc. While this may be feasible for studies with a small number of units being studied, it becomes increasingly difficult for larger datasets. It also depends on being able to think of all potential confounding pathways and including the proper variables to control for them, which is quite a challenge since we do not have many studies on which factors drive states or counties to adopt voting reforms or many studies on which factors affect how people decide which voting method to use. Another approach is to conduct analyses within single jurisdictions that include observations both before and after policy adoption. While this approach controls for differences between jurisdictions by design, it is open to confounding from time-varying factors like the record turnout levels in the 2008 election from the Obama candidacy. Another strategy would be to use matching techniques, either at the individual or

jurisdiction level. Like including control variables in a regression, this method requires

including variables that block all potential confounding pathways, which is typically not feasible.

We recommend using the difference-in-difference (DID) method for testing the effect of

using alternative methods of voting on voter turnout. This research design is used to study the

effects of a variety of public policies in political science, economics, and public health. It is

often recommended as a best practices research design for estimating causal effects of public

policies when such policies cannot be randomly assigned (Erikson and Minnite 2009). The basic

logic of the DID research design comes from the idea of observing two units in two time periods,

where one unit does not experience a policy change and the other adopts a policy change

between the time periods. The difference between the time periods would then be subtracted

from the difference between the units to see how the time trend varied between the unit that

adopted the policy and the unexposed unit. This method controls for differences between

affected and unaffected units that are constant over time as well as differences over time that are

constant between these units by twice differencing the data—the first difference being between

units, and the second being over time. If units can reasonably be assumed to follow parallel

trends in relevant confounding factors over time, then we can attribute the difference over time in

the difference between units to the effect of the policy change. In other words, this design

controls for confounding factors if we can reasonably assume that the relevant confounding

factors like demographics did not change differently across the units over time.

The DID design can be extended from using two units to a larger dataset of affected and

unaffected units and can also be extended over time to include more observations from before

and after policy adoptions (Wing et al. 2018). In such cases, it is typically implemented by using

regressing the policy variable on the dependent measure of interest while also including fixed

effects for cross-sectional units and fixed effects for time periods (Bertrand et al. 2004; Wing et al. 2018). After running a difference-in-difference model, the parallel trends assumption can be partially verified by several types of robustness checks, and its results can be subjected to sensitivity analysis to see how large potential confounders would have to be in order to cause the observed relationships (Wing et al. 2018).

Most difference-in-difference models use binary variables for the presence of a policy in some units, where the policies are treated as the same across these units. While this may be a reasonable approach for some types of public policies like marijuana legalization, public smoking bans, or even some voting policies like photo ID requirements, it does not seem well-suited for studying convenience voting policies. These policies vary in a number of ways across different states, and their implementation may vary widely across counties within a single state. As discussed earlier in section 3, factors like the number of early voting locations, their hours and days of operation, and even geographical factors like their distance to voters can all affect how convenient these options are to voters. For this reason, we propose using measures of how many people actually use these alternative voting options as proxies for the overall convenience of the policies. However, this approach is not common within DID research designs, which are usually focused on the effect of clear policy changes as measured by dichotomous variables. Furthermore, the "traditional" logic of thinking about differencing does not seem to apply here, since it is unclear which observations are exposed to the policy of interest and which are not.

We do not think these concerns should deter researchers from using this approach for two reasons. First, the method can easily be implemented by including fixed effects for time and jurisdictions within a basic regression model without having to difference any data. Even when the traditional binary logic of affected and unaffected units does not apply, using fixed effects in

both dimensions means that the observed correlations are only due to changes within units that are more affected by the convenience voting policy (such as higher % early voting) over time compared to changes in other units that are less affected by the policy over time. Second, using non-binary measures for policies in DID designs is not entirely without precedent. While we are not aware of any studies that employ the usage rate for a policy option as an independent variable in a DID design, some public health studies have used linear measures summarizing complex policies like Medicare physician's fees (Bullinger, 2017), minimum wage laws (Wehby and Kaestner, 2016) and fast food prices (Cotti and Tefft, 2013). For all of these measures, the traditional DID notion of binary policy exposure is replaced by continuous measures for the severity of a policy change on existing laws or markets.

Combining a DID research design with percent usage measures also provides a substantial advantage over using binary policy variables. Changes in these dichotomous variables occur rarely since they depend on large changes in the laws regarding convenience voting methods, such as Colorado's adoption of universal VBM in 2013. On the other hand, percent usage measures change in response to smaller implementation changes, such as adding more early voting locations, mobilizing people to use mailed ballots through public information campaigns, or changes in the days and hours of polling place operations. Since these measures contain more variation, they can be studied in more contexts and time periods that did not contain large policy changes as long as jurisdictions changed at least some relevant factors in their implementation of voting policies.

Researchers should be careful to follow best practices for difference-in-difference studies, all of which should apply to a research design using percent usage measures as much as they do to studies with binary policy adoption variables. These include using statistical

techniques like clustered standard errors or bootstrapping to account for serial correlation between observations of the same unit over time (Bertrand et al. 2004). They also include examining the plausibility of the parallel trends assumption and conducting sensitivity analysis to see how strong confounding factors would have to be in order to cause the observed relationships between the variables (Wing et al. 2018). We believe that when researchers properly use the DID method, it can better control for many confounding factors than other designs that are commonly used in studying the impact of voting policies on voter turnout, such as time-series studies, cross-sectional analyses, or individual-level matched data.

5. *Potential data sources*

The difference-in-difference research design should be used with aggregate-level data on voter turnout and voting methods with a number of jurisdictions over time (multiple elections). If researchers are interested in the effects of how county-level variation in how convenience voting policies are implemented, they can use observations of counties within a single state over multiple elections. As long as the counties changed some aspects of their policy implementation over time, and these changes were not the same across counties, with enough observations this type of dataset should be able to detect how the convenience of these voting methods from differential implementation affects voter turnout. If researchers are more interested in the effects of the adoption of policies like universal VBM or early voting, they can use a dataset containing state-level or county-level data with multiple elections across a number of states with differing policy changes in some of the states. All that this research design requires is data from multiple jurisdictions over a time period in which policy changes occurred differentially in the jurisdictions. Since counties are responsible for implementing the laws regulating when, where, and how voters cast their ballots, we believe researchers should strive to obtain county-level data

rather than state-level aggregated measures. Given the variation within and across jurisdictions in election laws and their implementation, measuring voting and voting by alternative measures requires data on these measures well before and after the adoption of new election laws across the universe of jurisdictions conducting elections.

We believe one dataset has substantial potential for studying the turnout effects of convenience voting policies using a DID design and percent usage measures. The U.S. Election Assistance Commission's (EAC) *Election Administration and Voting Survey* (*EAVS*) compiles information on the administration and operation of elections from more than 6,000 U.S. jurisdictions (usually counties, but occasionally cities or other units). The survey, conducted biennially since 2004, includes information on voter registration, turnout, alternative methods of voting, overseas voting, polling places, poll workers, and the devices on which voters cast their ballots. These data represent the most granular, comprehensive time-series database on election administration in the U.S. currently available from a single source. The Help American Vote Act of 2002 that established the EAC has mandated the collection of *EAVS* every two years, assuring its sustainability for future studies.

Using data from the EAC's *Election Administration and Voting Survey* poses several challenges. Reported usage rates, especially for early voting, are inconsistent within some counties and over time. Many of these inconsistencies probably arise from the way that local election officials completed the survey. We suspect the inconsistencies are in many cases attributable to real differences in how election officials perceive different methods of voting. For example, in some states the definition of in-person early voting includes an individual filling out an absentee ballot at a central location like a courthouse. In other states, these absentee ballots,

which are filled out and submitted in-person, are counted as absentee (mailed) ballots rather than early in-person votes.

Another issue arises from the differences in how states have approached filling out the *EAVS*; in some states the counties each fill complete the survey without direction from the state, and in other states the Secretary of State's office completes the survey for all counties. Finally, there are significant data issues within the *EAVS* where counties report measures that are either implausible or do not make sense in the context of previous elections or other counties within the same state. These may be caused by data entry errors such as decimals being placed behind the wrong digit or placing a number in the wrong box of the survey.

These anomalies in *EAVS* need to be remedied before moving forward with analyses of data. Many of these issues have been addressed with improved post survey audits (Election Assistance Commission 2018:160). Misreported and unreported responses to survey items have been significantly reduced over time through changes in how *EAVS* is conducted (e.g., the move to an online survey instrument and combining the survey instrument with consistent instructions). In 2018 the EAC inaugurated a *State Statutory Survey* that compiled information on state laws and practices within and across states and jurisdictions regulating common election activities. This inventory will remain a permanent companion to the biennial *EAVS*. We expect that pairing data on state mandated election practices with the *EAVS* will help reconcile apparent anomalous data reports arising from the use of different definitions of common election practices across survey respondents. To take full advantage of past *EAVS* data it will be necessary to assess the quality of these data, their suitability for use in research and whether and how deficiencies can be corrected. Here we propose employing data quality standards used by others (MEDSL 2016).

6. *Discussion*

Various convenience voting policies from same day registration to universal VBM were often adopted with a goal of increasing voter turnout, but the scholarly literature has not consistently identified clear positive effects of many of these policies. We attribute these mixed findings to past scholars' use of research designs that are subject to considerable bias from unobserved variables as well as overly simplistic measures of convenience voting policies. As we detailed in section 2, most of the research designs of past studies are highly susceptible to confounding pathways caused by unmeasured factors that vary between jurisdictions and / or unmeasured factors that change over time. Many studies may also experience biased turnout rate calculations caused by self-reported voting measures or issues with using individual-level voter data. Employing state or county-level measures can avoid the problems associated with voter files as well as issues with self-reported turnout in survey measures. Using a difference-in-difference research design and sensitivity analyses provides researchers with an effective means for correcting many of the biases associated with previous research caused by unmeasured factors in the temporal and geographic dimensions. Since these research designs require panel (time-series cross-sectional) data, scholars looking to examine the effects of convenience voting policies should seek to obtain data from a number of jurisdictions over multiple time periods.

We introduced a new method of examining voting reforms that has not been used in previously published studies: the percentage of the vote cast using different methods. While these measures rely on some assumptions to attribute their effects to the voting policies, we believe these assumptions are reasonable when combined with the difference-in-difference research design. They also allow the impact of policies to differ depending on how much of the population uses the available options, which we think captures an important aspect of election

reforms: their implementation. For example, early voting can vary greatly in its convenience depending on which days and times are available and the number and placement of early voting centers. Even mail-assisted voting options like no-excuse absentee voting can be administered differently in factors like the publicity of the method's availability, the convenience of the requesting method, and the availability of ballot drop-off stations or pre-stamped return envelopes. We think future studies should examine differences in implementation and administration in more detail rather than treating all types of early voting or other policies as the same. If there is evidence that the use of a specific method of voting affects turnout, either in a positive or negative direction, attention can then be directed to the sources of this effect. Researchers might examine the factors that shape the convenience of this voting method by looking at what implementation decisions are correlated with higher use of it, such as days, hours, or types of locations for early voting to identify those elements of the policy that most impact voter participation.

Finally, we recommend using the EAC's *EAVS* data for estimating and monitoring over time the impact of alternative methods of voting on voter turnout. The *EAVS* provides a convenient data source for core measures of voting, voting methods, and election practices for the entire universe of voting jurisdictions in the United States. Combining multiple years of these surveys creates a panel dataset necessary for a difference-in-difference research design to measure the turnout effects of convenience voting reforms and their implementation. These data are readily available to researchers and election officials, and unlike many state or county data they do not cost much time or money to obtain. Since Congress mandated the biennial collection and dissemination of these data, the sustainability of the survey in the future is assured.

Earlier editions (i.e, before 2014) of the *EAVS* exhibit anomalies in the turnout rates and the measures of voting method use. With a minimal amount of effort these data can be cleaned by removing anomalous values that do not reflect realistic voting and usage rates, and with more time and effort these problematic data can be replaced with proper values by re-contacting the states or counties responsible for retaining election data. Given the advantages of the *EAVS* for implementing difference-in-difference research designs, using percent usage measures, and studying voting policies nationwide using county level data, we believe this is a useful avenue for future research.

References

Aldrich, John. 1993. "Rational Choice and Turnout." *American Journal of Political Science* 37(1): 246-278.

Arceneaux, Kevin, Thad Kousser, and Megan Mullin. 2012. "Get Out the Vote-by-Mail? A Randomized Field Experiment Testing the Effect of Mobilization in Traditional and Vote-by-Mail Precincts." *Political Research Quarterly* 65(4): 882–894.

Barreto, Matt, Matthew Streb, Mara Marks, and Fernando Guerra. 2006. "Do Absentee Voters Differ from Polling Place Voters? New Evidence from California." *Public Opinion Quarterly* 70(2): 224-234.

Bergman, Elizabeth and Philip A. Yates. 2011. "Changing Election Methods: How Does Mandated Vote-By-Mail Affect Individual Registrants?" *Election Law Journal* 10(2): 115-127.

Belli, Robert F., Michael W. Traugott, and Matthew N. Beckmann. 2001. "What leads to voting overreports? Contrasts of overreporters to validated voters and admitted nonvoters in the American National Election Studies." *Journal of Official Statistics* 17(4): 479.

Berinsky, Adam J. 2005. "The Perverse Consequences of Electoral Reform in the United States." *American Politics Research* 33(4): 471–491.

Berinsky, Adam, Nancy Burns, and Michael Traugott. 2001. "Who Votes by Mail? A Dynamic Model of the Individual-level Consequences of Voting-by-Mail." *Public Opinion Quarterly* 5: 178-197.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How much should we trust difference-in-differences estimates?" *The Quarterly Journal of Economics* 119(1): 249-275.

Brady, Henry E. and John McNulty. 2011. "Turning out to vote: the costs of finding and getting to the polling place." *American Political Science Review* 105(1): 115-134.

Brenner, Philip S. 2012. "Overreporting of voting participation as a function of identity." *The Social Science Journal* 49(4): 421-429.

Bullinger, Lindsey Rose. 2017. "The effect of minimum wages on adolescent fertility: a nationwide analysis." *American Journal of Public Health* 107(3): 447-452.

Burden, Barry, David T. Canon, Kenneth R. Mayer, and Donald P. Moynihan. 2014. "Election Laws, Mobilization and Turnout: The Unanticipated Consequences of Election Reform." *American Journal of Political Science* 58(1): 95-109.

Citrin, Jack, Donald P. Green and Morris Levy. 2014. "The Effects of Voters ID Notification on Voter Turnout: Results from a Large-Scale Field Experiment." *Election Law Journal* 13:228-242.

Cortina, Jeronimo and Brando Rottinghaus. 2019. "Vote centers and turnout by election type in Texas." *Research and Politics* July-September 2019: 1-8 DOI: 10.1177/2053168019864224

Cotti, Chad, and Nathan Tefft. 2013. "Fast food prices, obesity, and the minimum wage." *Economics & Human Biology* 11(2): 134-147.

Dubin. Jeffrey A. and Gretchen A. Kalsow. 1996. "Comparing Absentee and Precinct Voters: A View over Time." *Political Behavior* 18(4): 369-392.

Dyck, Joshua and David Gimpel. 2005. "Distance, Turnout, and the Convenience of Voting." *Social Science Quarterly* 86(3): 531-548.

Election Assistance Commission. 2018. *Election Administration and Election Survey Report*. U.S. Government Printing Office, Washington, D.C.

Erikson, Robert and Lorraine Minnite. 2009. "Modeling Problems in the Voter Identification—
Turnout Debate." *Election Law Journal* 8(2): 85-101.

Fitzgerald, Mary. 2005. "Greater Convenience but Not Greater Turnout: The Impact of
Alternative Voting Methods on Electoral Participation in the United States." *American
Politics Research* 33(6): 842–867.

Fortier, John C. 2006. *Absentee and Early Voting: Trends, Promises, and Perils*. Washington,
DC: American Enterprise Institute Press.

Fullmer, Elliott B. 2015. "Early Voting: Do More Sites Lead to Higher Turnout?" *Election Law
Journal* 12(2):81-96

Gerber, Alan, Gregory A. Huber, and Seth J. Hill. 2013. "Identifying the Effect of All-Mail
Elections on Turnout: Staggered Reform in the Evergreen State." *Political Science
Research and Methods* 57(3): 91-116.

Grimmer, Justin, Eitan Hersh, Marc Meredith, Jonathan Mummolo and Clayton Nall. 2018.
"Obstacles to Estimating Voter ID Laws' Effect on Turnout." *Journal of Politics*.
80:1045-1051.

Gronke, Paul and Peter Miller. 2012. "Voting by Mail and Turnout in Oregon: Revisiting
Southwell and Burchett." *American Politics Research* 40(6): 976-997.

Gronke, Paul, Eva Galanes-Rosenbaum, and Peter A. Miller. 2007. "Early Voting and Turnout."
PS: Political Science and Politics 40(4): 639–645.

Gronke, Paul, Eva Galanes-Rosenbaum, Peter A. Miller, and Daniel Toffey. 2008. "Convenience
Voting." *Annual Review of Political Science* 11: 437–455.

Hajnal, Zoltan, Nazita Lajevardi and Lindsay Nielson. 2017. "Voter Identification Laws and the
Suppression of Minority Votes." *Journal of Politics* 79:363-342

Hale, Kathleen and Mitchell Brown. Forthcoming 2020. *How America Votes: Innovations in the*

    Public Management of Elections. Georgetown University Press.

Hanmer, Michael. 2009. *Discount Voting: Voter Registration Reforms and their Effects*.

    Cambridge University Press.

Haspel, Moshe and H. Gibbs Knotts. 2005. "Location, Location, Location: Precinct Placement

    and the Costs of Voting." *Journal of Politics* 67: 560-573.

Hening, William Waller. 1823. *The Statutes at Large; Being a Collection of all the Laws of*

    Virginia, from the First Session of the Legislature, in the Year 1619. Accessed at:

    https://catalog.hathitrust.org/Record/009714930

Herron, Michael C. and Daniel A. Smith. 2012. "Souls to the Polls: Early Voting in the Shadow

    of House Bill 1355." *Election Law Journal* 11(3): 331-347

Hill, Kim Quaile, and Jan E. Leighley. 1993. "Party Ideology, Organization, and

    Competitiveness as Mobilizing Forces in Gubernatorial Elections." *American Journal of*

    Political Science 37(4): 1158-1178.

Hood, M. V. and Charles S. Bullock III. 2011. "An Examination of Efforts to Encourage the

    Incidence of Early In-Person Voting in Georgia, 2008." *Election Law Journal* 10(2): 103-

    113

Kahn, Kim Fridkin, and Patrick J. Kenney. 1999. "Do negative campaigns mobilize or suppress

    voter turnout? Clarifying the relationship between negativity and participation."

    American Political Science Review 93(4): 877-889.

Karp, Jeffrey A., and Susan A. Banducci. 2001. "Absentee Voting, Mobilization, and

    Participation." *American Politics Research* 29(2): 183–195.

Kousser, Thad, and Megan Mullin. 2007. "Does Voting by Mail Increase Participation? Using Matching to Analyze a Natural Experiment." *Political Analysis* 15(4): 428–435.

Larocca, Roger and John S. Klemanski. 2011. "U.S. State Election Reform and Turnout in Presidential Elections." *State Politics and Policy Quarterly*. 11: 76-101.

Leighley, Jan. 2008. "Commentary on 'Attitudes, Opportunities, and Incentives: A Field Essay on Political Participation." *Political Research Quarterly* 61: 46-49.

McDonald, Michael P. 2003. "On the overreport bias of the National Election Study turnout rate." *Political Analysis* 11(2): 180-186.

Menger, Andrew. 2018. *The Costs of Voting and their Impacts*. Dissertation written for the Doctor of Philosophy degree at Rice University. Available from the author upon request.

Meredith, Marc and Neil Malhotra. 2011. "Convenience Voting Can Effect Election Outcomes." *Election Law Journal* 10(3):227-253.

MIT Election Data and Science Lab (MEDSL). 2016. *Elecions Performance Index: Methodology Report* https://medium.com/mit-election-lab/filling-in-the-gaps-fffd01758053

National Conference of State Legislatures (NCSL). "Absentee and Early Voting." Accessed at: http://www.ncsl.org/research/elections-and-campaigns/absentee-and-early-voting.aspx

Patterson, Samuel and Gregory Calederia. 1985. "Mailing in the Vote: Correlates and Consequences of Absentee Voting." *American Journal of Political Science* 29: 766-788.

Pearl, Judea. 2009. *Causality*. Cambridge University Press.

Richey, Sean. 2008. "Voting by Mail: Turnout and Institutional Reform in Oregon," *Social Science Quarterly* 89(4): 902-915.

Rosenstone, Steven and Raymond Wolfinger. 1978. "The effect of registration laws on voter turnout." *American Political Science Review* 72(1): 22-45.

Southwell, Patricia L., and Justin Burchett. 2000. "The Effect of All-Mail Elections on Voter Turnout." *American Politics Quarterly* 28: 72–79.

Southwell, Patricia L. 2009. "Analysis of the turnout effects of vote by mail elections, 1980-2007." *The Social Science Journal* 46: 211-217.

Stein, Robert M. 1998. "Early voting." *Public Opinion Quarterly* 62: 57–69.

Stein, Robert M., Andrew Menger, and Greg Vonnahme. 2015. "The impact of vote by mail on the cost and performance of elections in Colorado." A report prepared for the Pew Charitable Trusts. Available from the authors upon request.

Stein, Robert M., and Patricia A. García-Monet. 1997. "Voting Early but not Often." *Social Science Quarterly* 78: 657–671.

Stein, Robert M. and Greg Vonnahme. 2011. "Voting at Non-Precinct Polling Places: A Review and Research Agenda." *Election Law Journal*, 10: 307-311.

Stein, Robert M. and Greg Vonnahme. 2008. "Engaging the Unengaged Voter: Voter Centers and Voter Turnout." *Journal of Politics* 2: 487-497.

Stein, Robert M. and Greg Vonnahme. 2012. "The Effect of Election Day Vote Centers on Voter Participation." *Election Law Journal* 11(4): 291-301.

Stein, Robert M., Jan Leighley, and Chris Owens. 2004. "Voting, Early Voting and Party Mobilization: Is Timing Everything." Paper presented at the Annual Meeting of the Midwest Political Science Association, Chicago, IL.

Verba, Sidney, Norman H. Nie, and Jae Kim. 1978. *Participation and political equality: A seven-nation comparison*. Chicago: University of Chicago Press.

Wehby, George L., Dhaval M. Dave, and Robert Kaestner. forthcoming. "Effects of the minimum wage on infant health." *Journal of Policy Analysis and Management*.

White, Jeremy. 2015. "Alex Padilla unveils California voting overhaul bill." Sacremento Bee
June 10, 2015. Accessed at: http://www.sacbee.com/news/politics-government/capitol-
alert/article23671420.html

Wilson, Sven and Daniel Butler. 2007. "A Lot More to Do: The Sensitivity of Time-Series
Cross-Section Analyses to Simple Alternative Specifications." *Political Analysis* 15: 101-
123.

Wing, Coady, Kosali Simon, and Ricardo A. Bello-Gomez. "Designing Difference-in-Difference
Studies: Best Practices for Public Health Policy Research." *Annual Review of Public
Health* 39: 453-469.

Tables and Figures

Table 1: Past Studies on No-Excuse Absentee and Permanent Absentee Lists

Authors / Year	Setting	Type of Elections	Research Design	Observed Effect
Karp & Banducci (2001)	All states	Federal	CS: Individual-level survey (NES) over 5 elections	positive
Larocca & Klemanski (2011)	All states	Federal	CS: Individual-level survey (CPS) over 3 elections	positive
Oliver (1996)	All states	Pres.	CS: Individual-level survey (CPS) over one election	null to modestly positive
Gronke, Galanes-Rosenbaum, & Miller (2008)	All states	Federal	TSCS: state level data	null
Richey (2008)	All states	Federal	TSCS: state level data	null
Fitzgerald (2005)	All states	Federal	TSCS: state level data	null
Giammo & Brox (2010)	Random counties	Pres.	TSCS: 500 counties over 8 elections	null for 2 elections, then negative after

Table 2: Past Studies on Universal VBM

Authors / Year	Setting	Type of Elections	Research Design	Observed Effect
Southwell & Burchett (2000)	OR	Federal	TS: state level data	positive
Gronke et al. (2007)	All states	Pres.	TSCS: state level data	positive
Richey (2008)	All states	Federal	TSCS: state-level data	positive
Gerber et al. (2013)	Counties in WA	Federal	TSCS: Individual-level matching quasi-experiment	positive
Larocca & Klemanski (2011)	All states	Federal	CS: Individual-level survey (CPS) over 3 elections	positive
Gronke & Miller (2012)	OR	Federal & Special	TS: state level data	null / positive
Southwell (2009)	OR	All	TS: state level data	null / positive
Bergman & Yates (2011)	Precincts in CA	Local	CS: Individual-level quasi-experiment	negative
Kousser & Mullin (2007)	Precincts in CA	Federal	CS: Individual-level quasi-experiment	negative
Sled (2008)	8 states	All	TSCS: locality-level data	positive

Table 3: Past Studies on Early Voting

Authors / Year	Setting	Type of Elections	Research Design	Observed Effect
Wolfinger, Highton, & Mullin (2005)	Almost all states	Pres.	CS: Individual-level survey (CPS) in 2000	positive
Stein & Garcia-Monet (1997)	TX	Pres.	TS: Before/after comparison of 170 counties	modestly positive
Gronke, Galanes-Rosenbaum, & Miller (2008)	All states	Federal	TSCS: state level data	null
Fitzgerald (2005)	All states	Federal	TSCS: state-level data	null
Primo, Jacobsmeier, & Milyo (2007)	Almost all states	Pres.	CS: Individual-level survey (CPS) in 2000	null
Richey (2008)	All states	Federal	TSCS (state level)	null to modestly negative
Giammo & Brox (2010)	Random counties	Pres.	TSCS: 500 counties over 8 elections	positive in first year, then negative
Burden et al. (2014)	All states	Pres.	CS: Individual-level survey (CPS) over 2 elections	negative
Larocca & Klemanski (2011)	All states	Federal	CS: Individual-level survey (CPS) over 3 elections	negative

Table 4: Past Studies on Registration Reforms

Authors / Year	Setting	Type of Elections	Research Design	Observed Effect
Gronke, Galanes-Rosenbaum, & Miller (2008)	All states	Federal	TSCS (state level)	null
Neiheisel & Burden (2012)	Wisconsin	Federal	County-level quasi-experiment	positive
Larocca & Klemanski (2011)	All states	Federal	CS: Individual-level survey (CPS)—3 elections	positive
Burden et al. (2014)	All states	Pres.	CS: Individual-level survey (CPS)– 2 elections	positive
Fitzgerald (2005)	All states	Federal	TSCS (state level)	positive
Brians & Grofman (2001)	All states	Federal	Individual-level quasi-experiment (CPS)	positive
Fenster (1994)	MN, WI, ME	Federal	State-level quasi-experiment	positive
Knack (2001)	All states	Federal	Before/after comparison	positive

Appendix A: Exploration of possible correlations in percent usage measures

We begin this exploration by returning to the cost-benefit model of voting and the cost-minimizing extension of this model discussed in the previous section. Recall that the cost-benefit model assumes that potential voters will ballot only if their perceived benefits of voting outweigh their perceived costs of voting, while the cost-minimizing extension merely assumes that if they vote, people will choose the voting method that minimizes their costs. Let us consider different ways in which potential voters will be measured in the aggregate voting rates under different applications of these models, where voters may respond in varying ways to a convenience voting policy or its counterfactual absence. First, we will create a classification of simplistic types of potential voters by how their voter turnout is affected by the presence of an alternative voting method or its absence in the counterfactual scenario. Then, we will consider how an electorate composed of these types of voters would be measured by our variables, and how this would result in a correlation, or lack of correlation, between the percent usage measure and the turnout dependent variable.

Let us consider the hypothetical situation of a midterm federal election where a new voting method (early voting) is added as an option for voters in a jurisdiction. The first type of potential voter to consider is one who would not vote regardless of what voting options are available to her. Although she is registered to vote, she will never cast a ballot in the election being considered regardless of the voting methods that she could use. Perhaps she only votes in Presidential elections, or she is disillusioned with the current political landscape and does not wish to support either candidate or party. We will label this registered voter who will never cast a ballot in this election as a "never voter" (type NV). The next type of potential voter to consider is one who definitely will vote in this election regardless of what voting options are available to

her. She is highly motivated to vote, and the costs of voting will not deter her from balloting

even if early voting is not available. We will label this registered voter as an "always voter"

(type AV).

There are two more potential types of voters in this simplistic categorization. The first,

which is highly unlikely to exist based on cost-benefit theory, is one who would vote only in the

counterfactual, that is, if the early voting option was not available. Perhaps she is so angry about

early voting being adopted that she decides not to vote in this election as a protest. We will label

this registered voter as a "only counterfactual voter" (type OCV). The other is the type about

which cost/benefit theory predicts turns out as a result of the convenience voting policy; she will

vote only because early voting was added as an option that reduces her voting costs, but would

not vote in the counterfactual of no early voting. We will label this registered voter as an "only

early voter" (type OEV).

We can consider as a baseline a 100-person hypothetical electorate where the adoption of

early voting has no impact on voter turnout—that is, if our hypothetical electorate was composed

only of types NV and AV. Let us consider, as an example, 50 people in the electorate are type

AV and 50 are NV. Since the presence of early voting has no effect on turnout for these types,

we can consider this mixture of NV and AV registered voters as a starting point for whether we

would observe a correlation between our two variables. Some of the AV voters switched to

using early voting as a method, say 50%, but since they would have balloted anyway the turnout

rate is unchanged. So we have an independent variable of 50% early voting (25 early ballots cast

/ 50 total ballots cast) and a dependent variable of 50% turnout (50 ballots cast / 100 registered

voters). If we could replicate this electorate in the counterfactual universe where early voting

was not adopted in this jurisdiction, the turnout rate is entirely unchanged. None of the NV

people in the early voting universe have switched to early voting, since they did not cast a ballot by any method. In the counterfactual universe where early voting was not adopted, we have an independent variable of 0% early voting (0 early ballots cast / 50 total ballots cast) and a dependent variable of 50% turnout (50 ballots cast / 100 registered voters). If these two hypothetical jurisdictions were put together into a dataset (and replicated many times to have a large number of total observations) we would observe exactly zero correlation between % early voting and % turnout. So in the hypothetical case, where no one is affected by the presence of early voting, we observe no systematic relationship between early voting and turnout even though the number of ballots cast is used in the denominator of the independent variable and the numerator of the dependent variable.

What would happen if we change the composition of the hypothetical electorate to include some OCV or OEV registered voters? First let us consider a scenario where one person in the 100 casts a ballot only because early voting reduced her costs enough to make voting worthwhile. If we keep everything else the same, but switch just one "never voter" (NV) to one "only early voter" (OEV), then our independent variable becomes 50.9% early voting (26 early ballots / 51 total ballots cast) and our dependent variable becomes 51% turnout (51 ballots cast / 100 registered voters). In our counterfactual universe, however, the one OEV does not vote and we observe 0% early voting (0 early ballots cast / 50 total ballots cast) and 50% turnout (50 ballots cast / 100 registered voters). If we replicate these hypothetical jurisdictions again and put them in a dataset, we would now observe a small positive correlation between % early voting and % turnout, since those jurisdictions with the early voting rate above zero would have slightly higher voter turnout. The presence of even a small number of "only early voters" in the

electorate causes a positive correlation between these measures since we observe marginally higher turnout only in the presence of an early voting policy.

Likewise, changing the electorate to include a single "only counterfactual voter" (OCV) in place of one "always voter" (AV) would cause a negative correlation between early voting and turnout. Our independent variable would become 49.0% early voting (24 early ballots / 49 total ballots cast) and our dependent variable would become 49% turnout (49 ballots cast / 100 registered voters). Since the counterfactual does not change, the lower turnout in the early voting universe would result in a small negative correlation between the rate of early voting and the turnout rate.

While this scenario is obviously contrived and artificial, the logic of how these variables works extends to real world electorates, in which there are more than 100 registered voters and types are not as clear-cut. While it may seem unreasonable that some people will only vote when given the option of early voting, by the cost-benefit theory of voting we expect to observe some people who become substantially more likely to vote because of this available voting method. When voters are observed in the aggregate, this increase in their probability of voting has the same impact as an electorate composed of some "only early voters." Leaving aside other confounding factors for the moment (assuming a perfect counterfactual), whether we observe a positive correlation between the rate of early voting and the turnout rate clearly depends on what proportion of the population becomes more likely to vote under an early voting policy. The amount of positive correlation between early voting and turnout depends on what proportion of voters behaved according to the cost-benefit model, where reducing costs should lead to a higher probability of turnout.

If we observe no positive correlation between early voting and turnout, it could result from several causes: 1) a lack of reduction in the costs of voting from the policy, 2) an absence of voters who behave according to the cost-benefit model, or 3) a mixture of people who become more likely to vote when allowed to vote early and others who become less likely to vote under this policy. Since it is unlikely that we would observe "only counterfactual voters" in the real world who become less likely to vote under early voting policies, if we observe no correlation between early voting and turnout we can assume that either the voting policy did not actually reduce voting costs, or that the entire electorate is composed of "never voters" and "always voters"—i.e., no one is affected by having this voting option. On the other hand, if we observe a positive correlation, we can assume that at least some percentage of the electorate are "only early voters" who became more likely to vote because they could use the early voting option. Therefore, using the percent early voting measure to see how the added convenience of early voting affects voter turnout creates precisely the test needed to see if the policy change results in higher turnout as the cost-benefit model predicts. Under a situation where the policy change has no effect on turnout, no correlation is present between the variables (again assuming a perfect counterfactual to eliminate confounding). Under a situation where the policy change has an effect on turnout, the correlation between the measures will detect this change, the magnitude of which depends on how many people become voters only because of being given the option of this alternative voting method. The same logic applies to other percent usage measures such as % vote cast by mailed ballots, which depends on a variety of policies like no-excuse absentee voting, permanent absentee lists, and universal mail balloting.