Long Lines and Voter Purges: The Logistics of Running Elections in America

Long Lines and Voter Purges: The Logistics of Running Elections in America

A dissertation presented

by

Stephen Scott Pettigrew

to

The Department of Government

in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy
in the subject of
Political Science

Harvard University
Cambridge, Massachusetts

May 2017

Dissertation Advisor: Gary King Stephen Scott Pettigrew

Long Lines and Voter Purges: The Logistics of Running Elections in America

Abstract

In recent years, attention to how we run elections in the United States has grown. In this dissertation, I build on our understanding of election administration by focusing on two topics: long lines at voting precincts and the quality of lists of registered voters. I consider the challenges faced by election officials in dealing with these issues, as well as potential consequences that they have on political behavior. Each paper speaks to broader conclusions about electoral integrity and public confidence in the American system of elections. Throughout the dissertation, I highlight how factors like race, citizen engagement, and resource constraints shape the landscape of election administration.

Contents

List of Figures

List of Tables

Acknowledgments

There have been countless people who have helped me to get to where I am today. The most important has been my family. Mom and Dad, thanks for all the support you've given over the past six years and the past three decades. Thanks for all the advice you've given me on the phone as I walked home from the office every day, for all the unexpected shipments of chocolate chip cookies, for listening to me vent about the stressful parts of grad school, for making me do all those math workbooks when I was in elementary school, and for all the sacrifices you've made to put me in a position to be successful in life. Erica, you've been one of my closest friends my whole life and have played a huge part in getting me to where I am today. Thank you for being such a great sister to whom I can turn for advice on anything and who makes me a better person by never hesitating to challenge my thinking.

Julia, many of my best memories in grad school would not have happened if you hadn't entered my life. Your unconditional support has gotten me through some of the roughest patches of grad school, particularly as I've neared the finish line, and I can't express how much that means to me. Thanks for being the biggest cheerleader of my accomplishments, for tolerating three weeks of sharing a car with me as we drove across the country, for taking care of me when I'm sick, and for all the delicious pad thai you've cooked for me.

Gary King, thanks for teaching me to be the best advocate of my ideas and work, and for giving me advice that's always been useful and insightful. The opportunity to twice teach a class with you has been one of the most fulfilling and rewarding experiences I've had in grad school.

Charles Stewart, thanks for all the time you've taken over the past six years to work with me. I've enjoyed and learned so much from all the projects we've worked on together. At least as importantly (and perhaps more), thanks for talking through non-research related topics as I've gone through the highs and lows of graduate school.

Stephen Ansolabehere, thanks for all the doors you've opened for me in my academic and non-academic careers. The discussions we've had and the feedback you've given me on my work has made me a stronger researcher and a better critical thinker.

Thom Wall, thank you for all that you've done to guide me and countless other graduate students through our department. You've had a tangible, positive impact on the lives of hundreds of graduate students in our department, and we could never thank you enough for all that you do.

Matt Blackwell, Roger Porter, and Devin Caughey, thanks for providing me the opportunity to improve my knowledge of political science and statistics and for helping me to develop my teaching and management skills. I think when I look back years from now, I'll find that the lessons and skills I learned from you will be invaluable in my career.

Ryan Enos, thanks for all the helpful advice and feedback you've given me other the years. Also, thank you for giving me the opportunity to help coordinate the Political Analytics Conference. It's been one of the best experiences I've had in grad school, and I'm very excited about continuing to collaborate with you on it in

the coming years.

Jamie Carson, Ryan Bakker, and Keith Poole, thanks for helping to get my career off the ground as an overly ambitious undergrad, and thanks for being so supportive from afar as I've worked toward this accomplishment.

I also owe a huge debt of gratitude for all my friends who have made grad school such a great experience. In particular, I'd like to single out Melissa Sands, who has been there with me since we first met on a walking tour of Stanford and I found out that you'd never read Harry Potter.[1] Your unequivocal support and friendship has meant everything to me over the years. Thanks for the countless game nights you've hosted, for the delicious spinach and artichoke dip you've made, for the useful feedback you've provided for my work, and for the emotional support you've given me through the highest highs and lowest lows over the past several years. I can't express how excited I am to see all the successes you have in your career, and I'm happy to know that I'll have you as a friend through it all.

Thanks also to Jesse, Clucas, Jason, Mayya, Danny Mosk, (Dr.)DDK, Dana, Dominika, Anton, Justin, Sole, Rakeen, Peter, Ariel, Mike, Tess, the numerous occupants of the offices of the third floor of the little house, and countless others. If you've ever come to one of the summertime cookouts[2], Super Bowl parties, or trivia nights that I've organized, I owe you a thanks for making my grad school experience one in which I've been surrounded by great friends who have all been positive influences on my life.

Lastly, thanks to my friends outside of my Harvard bubble: Chris, Kevin, Lauren, Brian, Kelsey, Brett, Shayna, Mark, Lillian, and many others. You all have been there for me since before I started this PhD journey, and I'm excited to know you'll

[1] Thanks to Dan de Kadt for fixing that problem.

[2] Plus one regrettable winter one!

be there in the future journeys of my life.

Introduction

In recent years, attention to how we run elections in the United States has grown. In the wake of the presidential election in 2000, political scientists began to focus more heavily on the impact that administrative decision-making by politicians and bureaucrats might have on electoral and political outcomes. Even more recently, the topics of voter fraud and voter identification laws have become a source of political controversy, and academic research has begun to weigh in on the matter.

In this dissertation, I build on our understanding of election administration by focusing on two topics: long lines at voting precincts and the quality of lists of registered voters. I consider the challenges faced by election officials in dealing with these issues, as well as potential consequences that they have on political behavior. Each chapter speaks to broader conclusions about electoral integrity and public confidence in the American system of elections. Throughout the dissertation, I highlight how factors like race, citizen engagement, and resource constraints shape the landscape of election administration.

The first two chapters in the dissertation focus on the problem of long lines at polling places. In the November 2016 election, roughly one out of every ten in-person voters waited at least thirty minutes to cast their ballot; one out of fifty waited at least an hour. Although these percentages are smaller than those observed in 2008 and 2012, they are still substantively important. In-person turnout in 2016 was roughly 100 million, meaning that millions of hours were spent by people standing in line to cast their ballot.

The first chapter, "The Race Cap in Wait Times: Why Minority Precincts are Underserved by Local Election Officials," seeks to answer the question of which groups bear the most cost of long lines and why. Descriptive statistics about line length and race consistently show that non-white voters, particularly African-Americans, tend to experience the longest Election Day lines. I evaluate whether these racial differences are a consequence of minority voters being more likely to live in urban areas, where a dense population makes election administration more challenging. By analyzing variation within 4,500 counties, cities, and towns responsible for running their own elections, I demonstrate that more than half the race gap in line length occurs locally–a finding that runs counter to the urban sorting hypothesis. The probability of a voter at a predominantly white precinct waiting longer than an hour to vote is about 2%, compared to 13% for somebody at a mostly non-white precinct in the same county.

I then present evidence that these differences are largely attributable to resource allocation decisions made by local election officials. On average, polling places that serve mostly white voters have 20 fewer registered voters per voting machine and 90 fewer registrants per poll worker than polling places in mostly minority areas. The implication of this finding is that sub-optimal resource optimization by local bureaucrats is creating a higher cost of voting for African-American and Hispanic voters.

The second chapter builds on these finding by addressing the consequences that long lines have on political behavior by voters. In "The Downstream Consequences of Long Waits: How Lines at the Precinct Depress Future Turnout," I show that for every additional hour a voter waits in line, her probability of voting in the subsequent election drops by one percentage point. This suggests that nearly 200,000 people did not vote in November 2014 because waiting in a line in 2012 turned them off from the electoral process. Several placebo tests suggest that this finding is not the result of unobserved confounding, but rather an effect of the physical process of standing in a long line.

I corroborate this finding by examining two sources of precinct-level data. Longitudinal vote history data from the state of Florida, coupled with precinct-level indicators of line length, avoids the identification problem imposed by voter mobility and allows for tracking of 2012 voters through the 2014 election, even if they relocated to a new precinct. Data from the City of Boston, where precinct borders have been unchanged since the 1910s, avoids the problem of endogenous re-precincting. In both Florida and Boston, the analyses suggest that longer lines in 2012 drove down the turnout of voters in 2014.

The third and final chapter, "Moved Out, Moved On: Assessing the Effectiveness of Voter Registration List Maintenance," focuses on a different aspect of election administration: voter registration lists.[3] The quality of voter registration lists has been at the center of debates about election administration in the United States for over a decade. Lists with excessive numbers of ineligible registrants, whether due to death, mobility, or other reasons, complicate the logistical task of running elections and an easy target for those concerned with the electoral integrity or voter fraud.

This chapter addresses two questions regarding the accuracy of voter lists. First, how well do the registration cancellation rates line up with the rates we

[3]This paper is coauthored work with Charles Stewart III.

would expect to see given demographic trends? Second, when in the electoral cycle should we expect that voter lists will be the most reflective of the population of registered and eligible voters. Using administrative and demographic data over four two-year election cycles, we find very strong evidence that voter lists are almost entirely absent of deceased registrants. On the other hand, the data suggest that election officials have a much more difficult time removing registration records of those who have moved out of the jurisdiction. Counter to the popular narrative, we find evidence that suggests that areas with higher levels of Republican support are worse at maintaining accurate voter lists than Democratic-leaning areas.

We supplement these findings with data from Florida and Virginia which allow us to explore temporal variation in removal rates. We find that these states employ different paradigms for registration list cleaning. Whereas Florida leaves the bulk of its list maintenance until after elections, Virginia has a system in place that allows them to remove a large number excess registrants prior to an election without risking accidental removals of eligible registrants.

1 | The Race Gap in Wait Times: Why Minority Precincts are Underserved by Local Election Officials

1.1 Introduction

In the November 2012 general election, one in ten voters waited in line for more than thirty minutes to cast a ballot[1] About 3.5 million voters waited in excess of an hour, with some standing in line for longer than five hours. Long lines at the polls became such a hot topic in the media that President Obama acknowledged in his victory speech that the issue was one that needs to be fixed. Despite the growing media attention given to the problem of length lines at precincts (King 2012; Graham 2013; Peters 2013), little political science work has investigated the determinants of long waiting times.

In this paper I demonstrate that a voter in a predominantly minority precinct experiences a line that is twice as long, on average, than a voter in a predominantly white precinct. Additionally, minorities are three times as likely to wait longer than 30 minutes and six times as likely to wait more than 60. While the existence of this "racial gap" has been noted elsewhere (Stewart and Ansolabehere 2013; U.S. GAO 2014; Famighetti, Melilli, and Pérez 2014), this paper is the first to show that for two

[1] The data used throughout the dissertation are available on the Harvard Dataverse (Pettigrew 2017b).

neighborhoods in the same county or town, the neighborhood that is less white is likely to have a longer line. I show that a majority of the racial gap is explained by this variation within the geographic units that administer elections, rather than differences between administrative units. This finding is particularly important because it suggests that local election officials have done a worse job of serving the minority precincts than white ones.

After presenting initial evidence of the racial gap, I discuss how election administration by local county and town officials explains some of the variation in wait times across the country. I then estimate the size of the racial gap that is attributable to both between- and within-jurisdiction factors, and compare it to the its size when only within-jurisdiction factors are considered. I find that more than half of the gap in wait times results from within-jurisdiction differences. I then provide a possible explanation for these within-jurisdiction differences by showing that election officials appear to systematically provide more poll workers and voting machines to white precincts than minority ones. I conclude with a discussion of the role played by voter turnout and reflect on whether the findings of the paper provide evidence for racial discrimination.

1.2 The Race Gap in Wait Times

Throughout this paper, I rely on survey responses of verified voters in the 2006, 2008, 2012, and 2014 Cooperative Congressional Election Study (CCES 2007, 2009, 2013, 2015).[2] Respondents were asked, "Approximately how long did you wait in line to vote?" and were then presented with five possible responses: 'not at all', 'less than 10 minutes', '10 to 30 minutes', '31 minutes to an hour', and 'more

[2]For a discussion of the validity of survey data to measure lines, see Appendix A.

than an hour'. Those who waited longer than one hour specified their wait time in an open-ended followup. Following the convention used in the literature on wait times (Stewart 2013; Pew Center for the States 2014), I coded the responses into minutes. Respondents who fall into the first four categories were recoded to be the midpoint of their response category (i.e. 0, 5, 20, and 45 minutes). I used the open-ended responses for those who waited longer than an hour.[3]

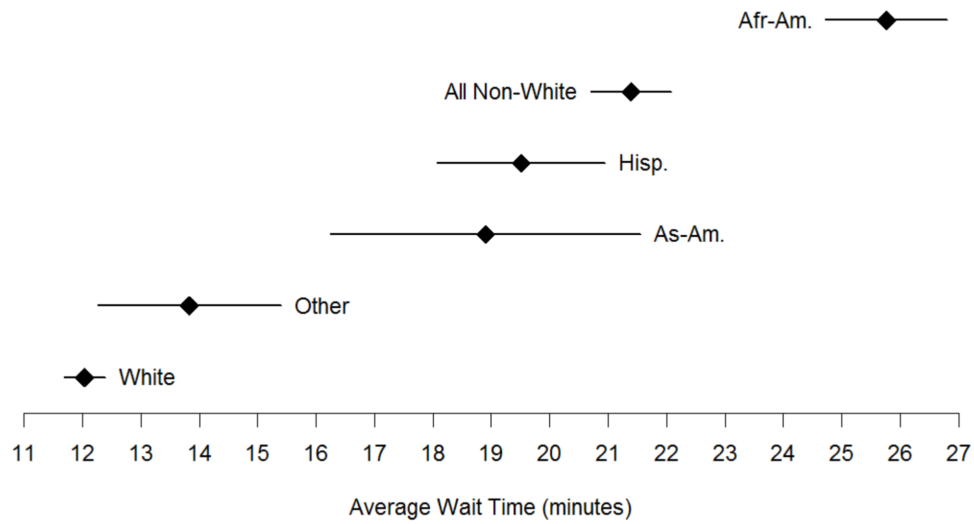
Based on this data, the average voter in the 2012 presidential election waited 14 minutes and 02 seconds to vote. Out of 129 million voters, roughly 11 million (8.8%) spent longer than a half hour in line. Three and a half million (2.7%) reported waiting more than an hour to cast their ballot. While these numbers are large, if all voters are equally likely to experience a long line, then the problem might be viewed as an inconvenience, but one that lacks broader consequences. On the other hand, if long lines systematically afflict certain groups of voters, then there may be political ramifications to consider.

Figure 1.1 provides evidence supporting the latter scenario. While white voters waited an average of 12:02, the average non-white voter waited almost twice as long, 21:24. The difference is even more pronounced when considering only African-American voters, whose average wait was 25:46. This pattern of a racial gap in wait times was not unique to 2012. In 2008 the average wait time for white voters was 13:40, while that of non-whites was 23.47.[4] Even in the 2006 and 2014 midterm elections, when low participation decreases the nationwide average wait

[3]Using an ordered logit to model response categories directly yields the same substantive results. I chose this approach for ease of interpretation. Additionally, Pettigrew (2015) suggests that midpoint imputation will tend to attenuate differences in means between subgroups suggesting that the results in this paper may underestimate the size of the racial gap. Indeed, implementing the method laid out in that paper provides slightly larger estimates of the racial gap.

[4]Appendix A includes replications of Figure 1.1 using data from the 2006, 2008, and 2014 elections.

Figure 1.1: Average Wait Time (with 95% CIs) in November 2012 Election by Voter Race



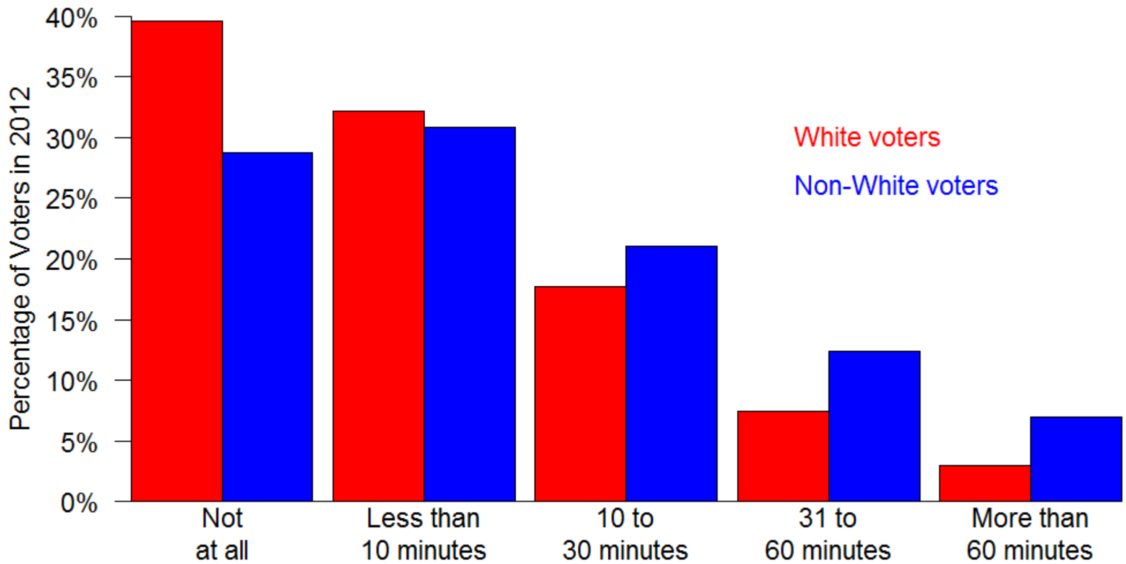
time, a racial gap persists.

There are also dramatic differences in the distribution of wait times between white and non-white voters. Figure 1.2 shows that in 2012 the percentage of white voters who did not wait to vote (39.6%) is significantly larger than of non-white ones (28.8%). Perhaps even more dramatic, there were 19.3% of minority voters who reported a wait of longer than 30 minutes, compared to only 10.5% of white voters. And while only 3.0% of white voters waited longer than one hour, 7.0% of minorities waited at least that long.[5]

Although the existing research has dealt with lines as an individual-level phenomenon, it is virtually impossible for an individual voter to experience a long wait without other voters at that the precinct having a similar experience. Thus electoral precincts are the ideal unit of analysis to study lines. Unfortunately,

[5]The story is similar in the 2006, 2008, and 2014 elections. Appendix A shows the distributions in these years.

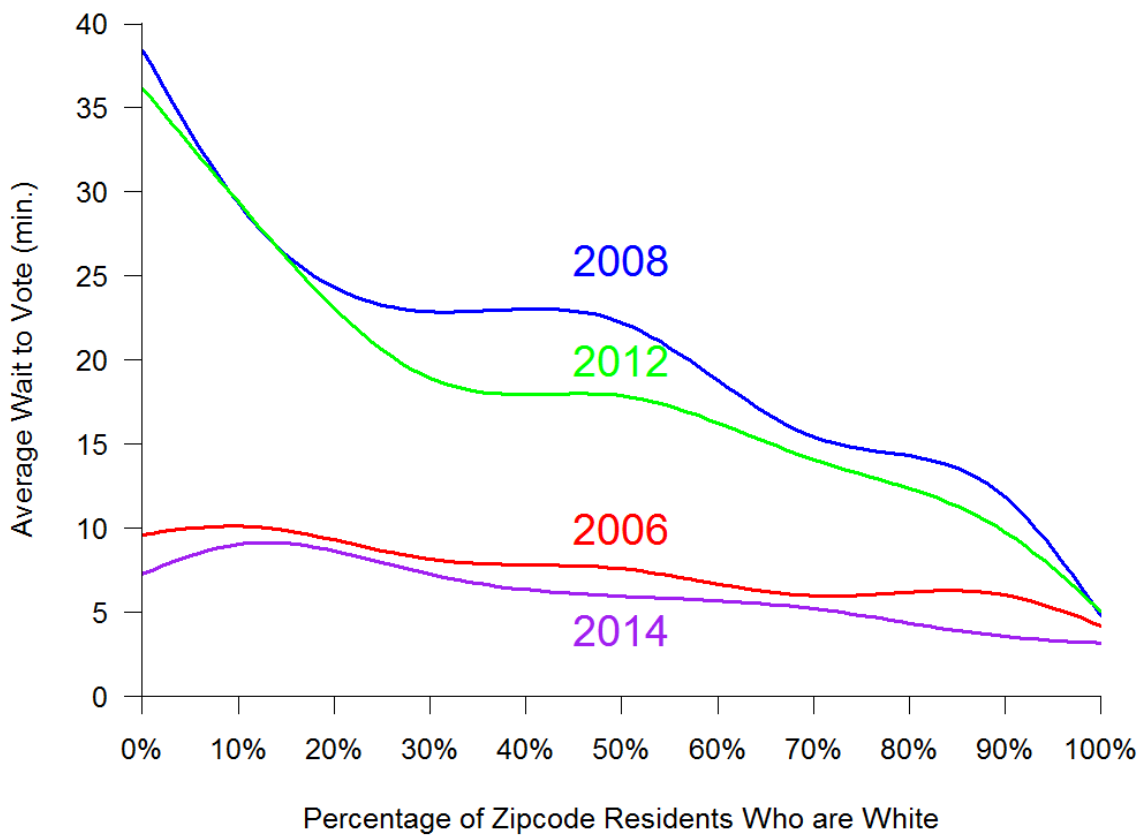
Figure 1.2: Distribution of Wait Times for White and Non-White Voters in Nov. 2012 Election



precinct-level data about lines is virtually non-existant, and the most precise level of geographic identification in the CCES data is the respondent's 5-digit ZIP code. Thus I consider how neighborhood racial diversity, based on ZIP code-level Census data, correlates with line length. Figure 1.3 smooths the average wait time across levels of racial diversity and shows that the racial gap persists in the aggregated data. In both presidential and midterm elections, the average wait in 100% white neighborhoods is lower than 100% minority neighborhoods.

This evidence demonstrate a persistent pattern of white voters having less of a time burden placed upon them at the polls. The question remains whether the racial gap results from predominantly white counties having attributes that decrease wait times compared to more minority-heavy counties, or whether within-

Figure 1.3: Average Wait Time, Conditional on Election Year and Neighborhood Racial Demographics



county variation suggests that county officials are treating white and non-white precincts differently–deliberately or otherwise. After detailing how local election administration can influence lines, I provide evidence to support the latter explanation.

1.3 How Election Administration Can Affect Lines

Unlike many other countries, the authority to administer elections in the United States is vested in state legislatures. Much of that responsibility is further devolved to bureaucrats and elected officials at the local level (Gerken 2009; Tokaji 2009; Hasen 2012). Most states leave responsibilities like training poll workers or allocating voting machines to their individual counties, although a handful of states[6] have city or town officials run their elections. The result is that there are thousands of local election officials throughout the country making these administrative decisions.

Given this, an important question is how much of the racial gap in line length can be attributed to differences between these electoral jurisdictions[7] and how much can be attributed to decision-making within a jurisdiction. It could be that rural areas, which have higher concentrations of white voters, have fewer logistical obstacles in administering an election than densely populated urban areas, where black voters tend to live. In this scenario, the racial gap would be a result of differences between counties or towns that administer elections. If, on the other hand, a sizable portion of the racial gap is explained by differences within a county or town, then we may conclude that minority and non-minority precincts are being handled by an election official in different ways.

In the context of solving the problem of long lines, a substantial amount of between-jurisdiction variance would suggest that more heavily minority counties or towns require in influx of resources or a better regime in training their poll

[6]Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont, and Wisconsin.

[7]Throughout the paper when I refer to jurisdictions, I am speaking of the counties, cities, or towns which administer elections.

workers. Evidence of a racial gap within jurisdictions would suggest that election officials must do a better job of fairly distributing the resources that they have.

In my analysis, I regress wait time on neighborhood racial demographics[8] to establish a baseline for the size of the racial gap that results from both the between- and within-jurisdiction variation. I then include jurisdiction fixed effects in the model to assess the extent to which the racial gap is attributable to differences in racial demographics within a jurisdiction. If the coefficient from the fixed effects regression is substantively large when compared to the baseline coefficient, it suggests that a sizable part of the racial gap results from election officials handling white and minority precincts within their jurisdiction differently.

The existing literature provides several explanations for between-jurisdiction variation in wait times. One is that jurisdictions in which voters cast ballots on a computerized direct-recording electronic (DRE) machines tend to have a longer average wait time than jurisdictions that use a paper ballot and optical scanning system (Edelstein and Edelstein 2010). In 2012, the average wait time in jurisdictions that used DREs was 17 minutes and 34 seconds (SE: $<$ 1 second), while the average voter in an optical scan jurisdiction was 11 minutes and 23 seconds (SE: $<$ 1 second). The main reason for this discrepancy is that DRE systems, which require expensive computer stations, are much less scaleable than optical scan systems, which require an extra table and privacy dividers.[9] Jurisdiction fixed effects

[8] I use aggregate racial demographics as the covariate of interest, rather than individual race, because long lines are afflict entire precincts, not individual voters. In the absence of blatant expressions of racial discrimination like having separate lines for different races, there is not a coherent story to tell about how an individual African-American voter could be forced to wait in line longer than white voters at the same precinct. As such, I am more concerned with neighborhood demographics than individual racial characteristics, although the two will be correlated.

[9] Optical scan systems still require ballot scanners, but scanning the ballot takes much less time than it takes to vote on a DRE. In November 2014 in Boston (an optical scan jurisdiction), there were an average of 19 seconds between completing the ballot and completing the scanning process. In contrast, voters in Orange County, Florida took an average of 8 minutes to fill out their ballot on

will account for this variability since voting technology is almost entirely constant within a jurisdiction. In 2012, 64.9% of jurisdictions used an optical scan system, while 30.7% used a DRE.[10]

Another factor that can impact wait times and is mostly constant within a jurisdiction is the length of the ballot. Ballot length has a strong positive correlation with lines (Edelstein and Edelstein 2010), and queueing theory tells us that when a system includes multiple points of service–i.e. a check-in station and a ballot casting station–a backlog at anywhere in the system will create backlogs at all previous points of service (Gross et al. 2008). Therefore when a long ballot causes all vote casting stations to be occupied, lines will develop for those waiting to check-in.

The fixed effects approach accounts for ballot length and also controls for other known or unknown factors which may vary between, but not within a jurisdiction. These factors include the type of training that poll workers receive, the professionalism of the election administration process, and the total number voting machines and poll workers available. Jurisdiction fixed effects also control for state laws or regulations which may impact line length, most notably voter identification laws (Kimball 2013).

Table 1.1: How did neighborhood demographics impact average wait?

	(1)	(2)	(3)	(4)
White Pct	-14.177^{***}	-17.386^{***}	-7.930^{***}	-12.139^{***}
	(0.286)	(0.561)	(0.430)	(0.743)
Juris. fixed effects			✓	✓
Additional controls		✓		✓
Observations	91,907	78,102	91,907	78,102
R^2	0.066	0.083	0.196	0.227
Note:		*p<0.05; **p<0.01; ***p<0.001		

1.4 Estimating the relationship between racial demographics and average line length

Table 1.1 displays the results of several linear regression models of line length on the percentage of people in a ZIP code who are white.[11] All models include year fixed effects, and models 3 and 4 include jurisdiction fixed effects as well. Models 2 and 4 account for additional demographic control variables.[12] The table shows that the total size of the racial gap in wait times–including both between- and within-jurisdiction variation–is 14:11 (without controls; SE: 17 seconds) or 17:23 (with controls; SE: 34 seconds). In other words, the difference in wait time between an area that 0% white and one that is 100% white is about a quarter of an hour.

the DRE machines (Stewart 2015).

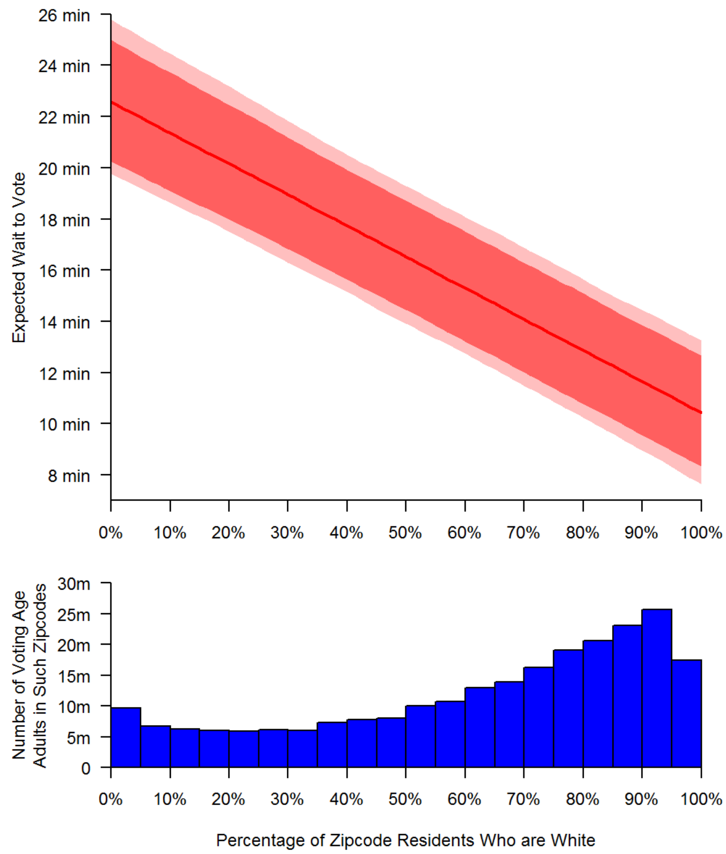
[10]Most of the remaining jurisdictions use a mixture of the two systems. A handful of tiny jurisdictions use a paper ballot. As of 2012, no jurisdictions used a lever voting machine and only four small Idaho counties used a punch card system.

[11]The conclusions presented here do not change when an ordered logit model is instead use to estimate the effects. Similarly, the results are robust to allowing for non-linearities in the covariate of interest. I chose to present the OLS estimates with a linear specification for ease of interpretation.

[12]The ZIP code-level controls are population density, percent of residents over 65 years old, median income, and percent English speakers. The individual-level controls are race, age, party, and an early voter dummy variable. The full results are available in Appendix A.

Including jurisdiction fixed effects, as in models 3 and 4, does not cause the racial gap to disappear; in fact, more than half of it remains. In the model without control variables, the average racial gap is 7:56 (SE: 26 seconds) which represents 55.9% of the overall gap from model 1. When demographic controls are included, the impact of within-county variation is even more pronounced. The racial gap estimate of 12:08 (SE: 45 seconds) from model 4 is 69.8% the size of that from model 2. These findings suggest that a substantial, or perhaps majority, of the difference in wait times between whites and non-whites result from differences within an election administrator's jurisdiction.

Figure 1.4: Marginal effect of neighborhood racial demographics on expected wait time



To further illustrate the magnitude of the within-jurisdiction impact of neighborhood demographics, Figure 1.4 presents the expected wait time (with 95% and 99% confidence intervals) at different levels of neighborhood racial composition.[13] The top of the figure shows that for an entirely non-white ZIP code the average wait time was 22:33 (SE: 1:11), while an entirely white ZIP code the waits just 10:26 (SE: 1:05). The bottom of the figure highlights that although most Americans live in mostly white ZIP codes, there are tens of millions of voters in neighborhoods with high expected wait times.

While the relationship between race and average wait time is striking, the racial gap is even more clear when we consider the probability of experiencing an unacceptably long wait. Survey evidence suggests that the typical American places the threshold of acceptable waiting times to vote somewhere between 30 and 60 minutes (CCES 2014), so I collapsed the data into two dummy variables based on whether or not the respondent waited longer than 30 or longer than 60 minutes.[14]

Using covariate specifications identical to those in Table 1.1, I estimated two sets of logistic regression models where the outcome is whether the voter waited in a long line (over 30 or 60 minutes). The full results of these regressions, which are provided in the Appendix A, are entirely consistent with the story that white neighborhoods are much less likely to experience long lines at their precinct. The results provide further evidence that at least half of the racial gap in wait times is attributable to within-county variation in how elections are being run.

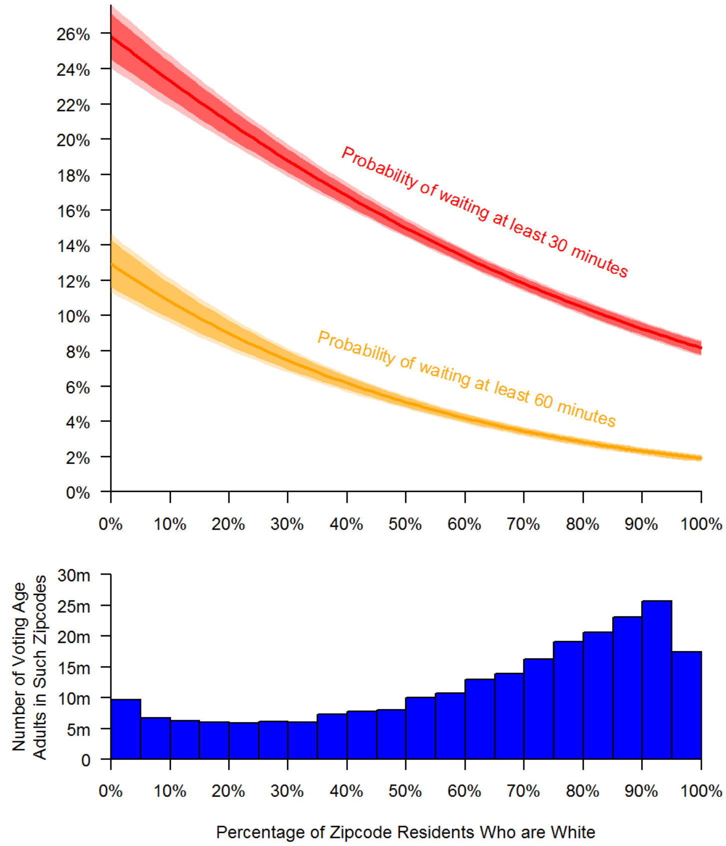
Figure 1.5 reports the predicted probabilities of waiting in a 30 or 60 minute

---

[13] This graph are based on the results of Table 1.1, Model 4 with all ancillary covariates were set to their means.

[14] An added benefit of this approach is that it does not require imputing continuous time values for the categorical survey responses.

Figure 1.5: Marginal effect of neighborhood racial demographics on the probability a voter experiences a long line



line given the racial makeup of a neighborhood.[15] For the millions of voters who live in predominantly non-white neighborhoods, their chances of waiting at least 30 minutes is roughly one in four (25.8%). That is more than triple the probability for voters in white neighborhoods, where the probability is 8.2%. The gap is even more profound when it comes to lines that exceed an hour. There is an 12.9% probability of a voter from a non-white neighborhood waiting more than an hour, compared to just 1.9% at the other end of the demographic spectrum.

These results support the conclusion that a substantial proportion–and per-

[15]These figured are based on the results of model 4 from Tables A.2 and A.3

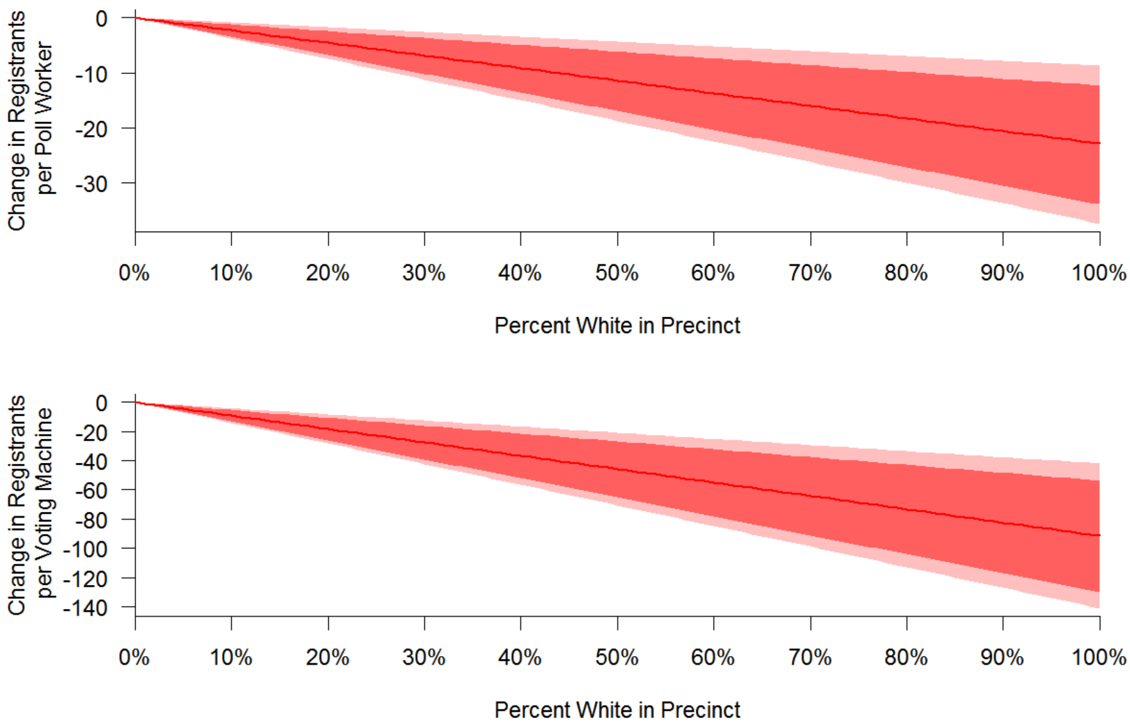
haps most–of the racial gap in wait times results from how white and non-white precincts within the same administrative jurisdiction are handled. In the following section, I provide a potential explanation for why there is such tremendous variation in wait times within jurisdictions.

1.5 Minority precincts receive fewer resources than white precincts

Perhaps the most important factor in determining how long a line to expect on Election Day is the number of resources–particularly voting machines and poll workers–that are provided to a precinct. If every precinct had an huge number of voting machines and poll workers, then every voter who arrived would be able to immediately check-in and fill out their ballot without a delay. As the resource allocation decreases, the strain on the system–and thus the length of the line–will increase. Once a precinct reaches its full operational capacity, the length of the line will increase exponentially, not linearly (Gross et al. 2008). Given that local governments are constrained by the availability of resources, a major responsibility of a county or town election administrator is to decide how many resources to place in each precinct. In this section I demonstrate that precincts with higher concentrations of white voters tend to receive larger numbers of poll workers and voting machines than precincts with more minority voters.

While there is no comprehensive precinct-level database about Election Day resources, Famighetti, Melilli, and Pérez (2014) collected precinct-level voting machine and poll worker allocation data from 6,600 precincts in Florida, Maryland and South Carolina during the November 2012 election. Figure 1.6 uses this data

Figure 1.6: How does poll worker and voting machine allocation change as a precinct becomes more white?



to illustrate that as the size of the white population in a precinct increases, the allocation of resources per registered voter becomes increasingly generous.[16] On average, a mostly white precinct has about 20 fewer registered voters per poll worker than an primarily minority precinct. Likewise, white precincts have 90 fewer registrants per voting machine, compared to minority precincts.

These differences are substantial and consequential. In a average sized minority precinct that is open for 12 hours on Election Day, voting machines must serve an additional 7.5 registered voters per hour–an additional voter every twelve minutes. During a high-turnout presidential election, most precincts are stretched thin

[16]The table with the results that generated this figure is in Appendix A.

as a result of lack of resources (Norden and Famighetti 2015). Figure 1.6 shows why the problem is even worse in minority precincts, where there are fewer machines and poll workers to begin with.

1.6 Discussion: is this racial discrimination?

In this paper I have shown that a majority of the racial gap in wait times can be attributed to factors that vary within election administration jurisdictions. I have also provided evidence that one of these factors is that white precincts tend to get a larger allocation of voting machines and poll workers than non-white precincts. The important remaining question, then, is whether these facts should be taken as evidence of racial discrimination.

Perhaps the most obvious argument to counter the claim of discrimination is that recently there have been big changes in voter turnout patterns by race. The fixed effects models eliminate any variation in turnout between counties, but there is still variation in turnout within a county or town. Lack of data makes it challenging to empirically test the impact of turnout. Precinct-level turnout data is available for recent elections, but I cannot match CCES respondents to specific precincts.[17]

Even without the ideal data, it is still possible to consider how turnout impacts the results of this paper. One might argue that the racial gap is a phenomenon unique to the 2008 and 2012 elections–resuling from an "Obama effect." Historically, turnout tends to be higher in areas with a higher concentration of white vot-

[17]Nor is it possible to identify the ZIP code of precincts in a systematic way, particularly going back in time. One county in Texas, for example, lists "American Legion" as the location of one of its precincts. There are several American Legions in the county and is it not possible to determine which one is the precinct because no additional identifying information is provided.

ers (Verba, Schlozman, and Brady 1995; Rosenstone and Hansen 2002; Wolfinger and Rosenstone 1980). Election officials may use this as a reasonable justification for allocating additional resources to white precincts, where demand is typically highest. In 2008 and 2012, there was a surge in minority voting across the country, surpassing white turnout in many areas (Tesler and Sears 2010). An election official could argue that this surge in 2008 created unpredictable swings in turnout, making it difficult to allocate resources. In 2012, however, election officials cannot make the same argument since they had four years to adjust their allocation strategy. The fact that the gap is nearly the same size in 2012 as 2008 suggests that election administrators are even less responsive to anticipated shocks to turnout than is required to make election run smoothly in all precincts.

Perhaps the best reason to think that much of the racial gap is not a result of the "Obama effect" is that the gap exists in the 2006 and 2014 midterm elections, when Obama was not on the ballot. White turnout in 2014, especially among conservatives, was particularly high compared to minority turnout, which should have put more strain on white precincts. Yet in that year African-American voters waited more than twice as long, on average, than white voters. Additionally we know that voting is habit forming (Prior 2010; Gerber, Green, and Shachar 2003a), so even if the racial gap in 2008 and 2012 came from first-time minority voters, we should expect a large proportion of them to continue voting in future elections. Sticking to the old rules of resource allocation will cause the the gap in wait times for different racial groups to persist.

The more important point is that even if the racial gap is explained by shifts in voter turnout, election officials could be doing a better job of anticipating these shifts. It is difficult to imagine an election official would intentionally decide to provide fewer poll workers or voting infrastructure to a minority precinct, simply because of its racial composition. But prior research has shown that low socioeconomic status individuals are less likely to file complaints to the government, so officials might anticipate more complaints if they under-allocate resources to white precincts where income levels tend to be higher (Frederickson 2010; Jones et al. 1978; Mladenka 1980).

Additionally, resources like poll workers and voting machines are scarce and indivisible. If one precinct has 75 voters, another has 100 voters, and there are 3 voting machines to allocate, the optimal solution is to give one machine to the smaller precinct and two larger. This will create longer lines in the smaller precinct. Better data on precinct resource allocation rules could assess the extent to which this dynamic may explain the racial gap.

I show in this paper that the racial composition of a voter's neighborhood is strongly tied to how long they will wait in line to vote. A substantial amount of the gap between white and non-white wait times is a result of local factors, which provides policymakers a way forward in addressing the problem. Future research could apply the solutions of discrete optimization problems to the topic of resource provision in elections.

2 | The Downstream Consequences of Long Waits: How Lines at the Precinct Depress Future Turnout

2.1 Long lines at voting precincts

For decades political scientists have focused on the question of why some people vote and others do not (Downs 1957; Riker and Ordeshook 1968; Wolfinger and Rosenstone 1980; Verba, Schlozman, and Brady 1995; Gerber, Green, and Larimer 2008; Leighley and Nagler 2013). In recent years, and particularly in the wake of the 2016 election, researchers and political observers have paid more attention to the impact that the administrative component of elections has on voter behavior, particularly through so-called voter suppression.

Existing research has focused largely on the effect that legal changes–such as voter identification laws–have on turnout. In contrast, little consideration has been given to the experience voters have while inside their precinct, despite recent work emphasizing the importance of a person's first-hand experiences in shaping their political participation (Achen and Bartels 2016; White 2016). This paper extends our understanding of the political participation by exploring how one aspect of the precinct experience–standing in line to vote–shapes the turnout behavior of voters in future elections. I find that voters who have worse in-precinct experiences (i.e. those who wait longer to cast their ballot) are less likely to participate in subsequent elections.

Roughly 3.5 million voters waited longer than one hour to cast their ballot in 2012. If a long line is equally likely to occur at every precinct[1] we might characterize the problem as a random nuisance, but not one that has broader implications. Research shows, however, that racial demographics are one of the strongest predictors of how long somebody waits in line (Stewart and Ansolabehere 2013; U.S. GAO 2014; Famighetti, Melilli, and Pérez 2014), with non-white voters being seven times more likely to wait longer than an hour than white voters (Pettigrew 2017a). Even more troubling, these racial differences are largely attributable to local election officials providing more poll workers and voting machines to more heavily white precincts, at the expense of precincts serving minority voters (Pettigrew 2017a).

The focus of this paper is to identify the effect that long lines have on the turnout behavior of voters in future elections. While there may be other consequences of waiting for hours to cast a ballot–for example, a decrease in their confidence in the electoral process–altering future turnout is perhaps the most consequential. When the decision-making of local bureaucrats contributes to longer lines which turn voters off from participating, democratic accountability is eroded. A poor precinct experience may also stymie the development of a voting habit by a new voter. This is particularly relevant given the large number of first-time minority voters in 2008 and 2012. It may also explain some of the drop-off in minority turnout in 2016.

To estimate the effect that waiting in a line has on future turnout, I employ

[1] Although their meanings differ slightly, I use the terms 'precinct' and 'polling place' interchangably throughout the paper for stylistic reasons.

three empirical strategies to show that for each additional hour of waiting in line, turnout in the next election diminishes by about one percentage point. Placebo tests throughout the paper indicate that this result only holds for those who voted in-person in 2012 and not those who voted by mail or did not vote, suggesting that the relationship is not a spurious one.

After developing my hypothesis in Section 2.2, I use a national sample of voter history data to estimate the turnout effect at the voter level. In Section 2.3.1, I show that 2012 wait times predict 2014 turnout for those who voted in-person in 2012, but not for those who voted by mail or who did not vote. Exact matching, coupled with additional placebo tests in Section 2.3.2, deals with selection bias and provides strong evidence that lines depress turnout. In Section 2.4, I focus on analyses in the City of Boston and seventeen counties in Florida, which providing precinct-level evidence of a turnout effect of lines. I then demonstrate, in Section 2.5, that about 200,000 people did not vote in 2014 as a result of their bad precinct experience in 2012, with a skew toward racial minorities. I conclude the paper by discussing the implications these results have on representation, as well as our understanding of citizen participation and habitual voting.

2.2 How lines can affect turnout

Researchers have long emphasized the importance of political institutions in shaping political behavior, focusing mostly on factors on things which influence a person's likelihood of going to the polls, like age requirements (Meredith 2009), get out the vote efforts (Gerber, Green, and Larimer 2008), or primary election eligibility rules (Kaufmann, Gimpel, and Hoffman 2003; Gerber and Morton 1998). Only recently have scholars considered the impact that a voter's experience at their polling place has on their behavior. This paper builds on research about the effect of polling location on vote choice (Gimpel, Dyck, and Shaw 2006; Berger, Meredith, and Wheeler 2008; Rutchick 2010; Brady and McNulty 2011) and furthers our understanding of how an individual's personal experiences shape their political outlook. Why, then, might we expect a bad precinct experience–manifested in a long line–to impact a voter's future turnout? The literature on political participation provides us with two potential answers.

The first explanation comes from the rational choice literature, where the decision to vote is a function of the benefits one gains and the costs one bears from voting (Riker and Ordeshook 1968; Aldrich 1993). Previous work has shown additional costs from changed precinct locations (McNulty, Dowling, and Ariotti 2009) or lengthy commutes to the polls (Gimpel and Schuknecht 2003; Gimpel, Dyck, and Shaw 2006) result in diminished turnout. When a voter waits in a long line, they might update their utility function in future elections by accounting for the cost of waiting again. Also, the mere act of waiting with dozens or hundreds of other voters might remind somebody that their individual vote is unlikely to be pivotal in the outcome of the election, thereby diminishing their chances of turning out in the future. Yet while this framework is a useful start, rational choice cannot completely account for why lines might impact turnout. In some ways, the fact that a voter waited hours to cast a non-pivotal vote suggests that she acts with some degree of irrationality.

The second explanation for why lines may depress future turnout is a psychological and sociological one. Many researchers view electoral participation as more of a consumption good than an investment one (Achen and Bartels 2016; Hamlin and Jennings 2011; Hillman 2010, 1994). By this line of reasoning, voters do not decide formulate political opinions or decide to participate based on a rigorous cost-benefit analysis. Rather, they make their decisions based on a combination their social environment and personal experiences. For many, participation in politics is a source of entertainment which derives social benefits. It stands to reason then, that a bad customer service experience at the polls might make them likely likely to turn out in the future.

Another potential psychological explanation for the hypothesis is that negative experiences with government officials can diminish a citizen's political efficacy. Much of the work on this topic focuses on contact with the criminal justice system (White 2016; Weaver and Lerman 2010, 2014), where an experience as trivial as a traffic stop decreases a person's probability of contacting the police for assistance (Gibson et al. 2010). Other work (Alvarez, Hall, and Llewellyn 2008) has shown that when a voter feels less confident in the effectiveness of the electoral system, they are less likely to participate in the future.

Empirical data suggests that voters who experience long lines express doubt in the electoral system. Those who waited longer than an hour in 2012 were 13.2 percentage points (SE: 3.43 pp) less likely to be "very confident" that their vote was correctly counted, compared to those who did not wait at all. Unsurprisingly, those who waited more than an hour were 43.8 percentage points (SE: 3.25 pp) less likely to rate the performance of their poll workers as "excellent" or "good."[2] These patterns indicate that those who wait to vote tend more frustrated with the system, and thus more likely to be turned off from voting in the future.

One potential objection to the diminished turnout hypothesis is that voters can adjust their behavior to respond to lines in ways other than not voting at all. For example, in the following election a voter could vote at a different time of the

[2]See Figure B.1 in the appendix for the full results of these two analyses.

day, when they anticipate lines to be shorter. While this is certainly plausible, most people (particularly those in areas afflicted by lines) do not tend to have the option but to vote before or after their workday, when lines are at their longest. Voters may also choose to vote early, although evidence shows that early voters tend to experience lines that are longer than Election Day voters. Absentee voting by mail is another option, and I show in the next section that lines do appear to push people toward this mode of voting. The important thing to remember is that these possibilities make the identification of an overall turnout effect more difficult and amplifies the normative implication of such an effect.

2.3 Estimating the effect of lines on turnout

The main challenge to identifying the relationship between long lines and turnout is confounding or selection bias. The strongest predictors of line length are a neighborhood's racial composition and its population density (Pettigrew 2017a; Famighetti, Melilli, and Pérez 2014), but these factors may also be confounders. White voters, who tend to turn out at higher rates than non-whites, are more likely to live in suburban and rural areas where lines tend to be shorter. Minority voters, particularly African-Americans, are more concentrated in urban settings, where lines are longer because high population densities make the administrative task of elections more difficult. State laws and regulations, like voter identification requirements, also muddy the relationship since they have been found to increase the length of lines (Pettigrew 2017a) and may also effect turnout (Ansolabehere 2009; Hood and Bullock 2008).

Disentangling this confounding is difficult in the absence of a randomized experiment, although not impossible. In the next subsection, I use regression to estimate the effect of interest, relying on a conditional ignorability assumption for causal identification. I justify this assumption with placebo tests using voters-by-mail and nonvoters. In the following subsection, I employ exact matching to more effectively eliminate confounding on observables (Iacus, King, and Porro 2011a). By grouping together voters who have identical covariate profiles, but who experienced different line lengths, we can eliminate confounding from those covariates by forcing them to be completely uncorrelated with line length. Finally, I conclude this section with an analysis of how lines impact future in-person versus mail-in absentee voting.

Throughout this section I consider Catalist's nationally representative sample of 1% of all American adults ($n > 3$ million) which includes vote history information from the entire country (Ansolabehere and Hersh 2012).[3] I subset the data to include only individuals who were registered to vote in the November 2012 election.[4]

The outcome variable of interest is whether an individual voted in the November 2014 midterm election. Using 2014 as the outcome provides a tough test for the turnout hypothesis. Midterms have much lower turnout than presidential races, those who do participate tend to be regular voters who would be less sensitive to experiencing a long line.[5]

Ideally, the "treatment" variable would be the amount of time each individual

[3]I remove voters from Washington and Oregon for all analyses, since those states exclusively use a vote-by-mail system. I include Colorado, although it had mostly switched to vote-by-mail in 2014. The results are not sensitive to its inclusion.

[4]Recent work by Jackman and Spahn (2016) shows that non-registered racial minority and low income people are underrepresented in such databases. Restricting my sample to only registered voters mitigates this problem.

[5]Among those who voted in 2014, 68.1% of them had also voted in each of the prior three elections (2008, 2010, and 2012) and 53.9% had voted in the previous four (2006 through 2012). In contrast, only 42.4% of 2012 voters had participated in the prior three elections.

voter in the sample waited in 2012. Unfortunately, this information is only collected for a very small number of voters.[6] As such, I turn to the 2012 Cooperative Congressional Election Study (CCES 2013), which asked its nearly 60,000 respondents, “Approximately how long did you wait in line to vote?” and then were presented with five responses: ‘not at all’, ‘less than 10 minutes’, ‘10 to 30 minutes’, ‘31 minutes to an hour’, and ‘more than an hour’. Following the convention used in this literature (Pettigrew 2017a; Stewart 2013; Pew Center for the States 2014), I recoded the responses as hours and fractions of hours.[7]

I then averaged the wait times within ZIP codes and merged them with the Catalist data. All ZIP codes with at least one response were included in the analysis. This yields estimates of the average line length in 11,819 ZIP codes, covering 79.1% of Americans (when weighting by population).[8] An alternative approach would be to only use ZIP codes with at least $n > 1$ responses. Figure B.4 in the appendix shows that the conclusions drawn do not change when choosing other thresholds.

Despite expected random noise in survey responses, there is also very little variation in line length within a single ZIP code. When randomly selecting two CCES respondents from the same ZIP, there is a 37% chance that they gave identical answers to the line length question, and an 78% chance that they answers

[6]Another alternative is to use the 2010-2014 CCES panel studies, since they include 2012 individual wait time and 2014 turnout data. Attrition is a major problem with this data because it is strongly correlated with turnout. 90% of those who participated in the 2014 wave of the panel voted in that year’s election. This provides virtually no variation in the outcome variable, and the sample would need thousands more respondents to have the power to detect even a large effect.

[7]Respondents who fall into the first four categories were coded at midpoint of their response category (i.e. 0, 5, 20, and 45 minutes). Those who waited more than one hour specified their wait time in an open-ended followup.

[8]Figure B.2 in the appendix shows a map of which ZIP codes were included and which were not.

differed by no more than one response category. This is a significant reduction in variance from comparing two respondents from within the same state, county, or nationally.[9]

2.3.1 Evidence from individual voter records

Table 2.1: How did lines in 2012 impact the turnout of voters in 2014?

	In-person	Mail	Non-voters
	(1)	(2)	(3)
2012 wait (hrs.)	-0.0063^{**}	0.0008	0.0020
	(0.0021)	(0.0034)	(0.0018)
Observations	774,836	166,885	373,595

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
Linear probability model coefficients reported
Controls and state fixed effects included

Model 1 in Table 2.1 shows the results of a linear probability regression model[10] in which the outcome variable is whether the individual voted in 2014 and the covariate of interest is the average wait time for that person's ZIP code in 2012.[11] To account for confounding, the models control for the voter's race, age education, and turnout history in 2006, 2008, and 2010.[12] I also include controls for population density, racial diversity, median income, and percent of non-English speakers in the voter's Census block-group, as well as state fixed effects.[13]

[9]See Figure B.3 in the appendix for additional analysis.

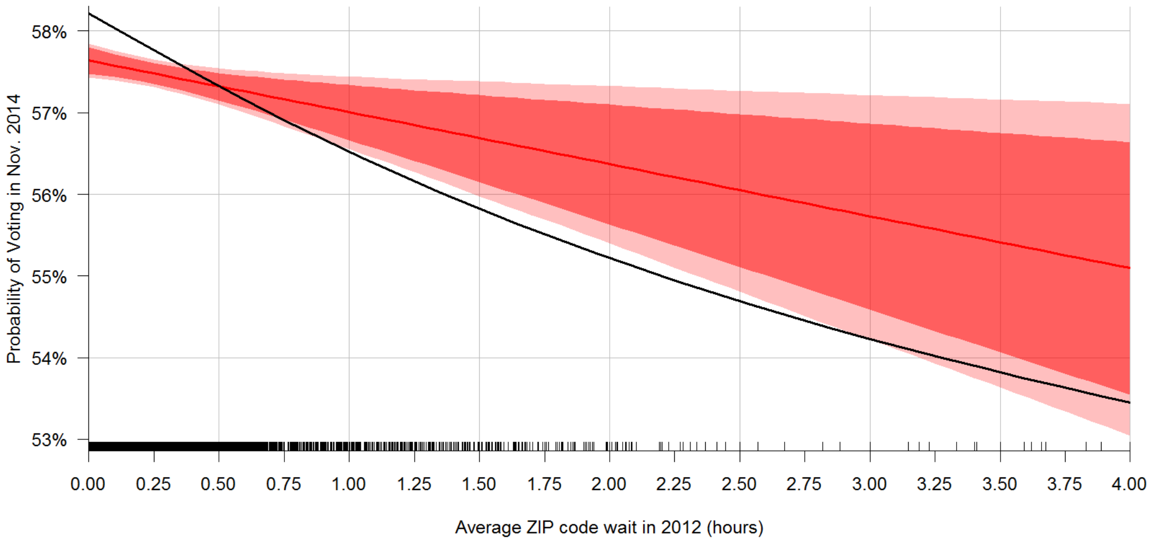
[10]The substantive results are the same when using logistic regression. Those results are reported in appendix Table B.3.

[11]Standard errors throughout the paper are clustered by ZIP code because that was the level at which the treatment was measured.

[12]Fraga (2016) notes that 2006 is the earliest election for which the Catalist data are reliable. Estimating the model using turnout as far back as 2002 does not change the substantive results. Nor does including only 2008 and 2010 turnout or just 2010 in the model.

[13]Table B.1 in the appendix reports the full regression results with all controls.

Figure 2.1: Predicted probability of turnout in 2014, based on wait time in 2012 (with 95% and 99% CIs and loess smoother of bivariate relationship)



As Model 1 demonstrates, there is a significant, negative relationship between the amount of time an in-person voter waited in 2012 and her probability of voting in 2014.[14] Figure 2.1 presents this result graphically. The voters that did not wait in line in 2012 had an expected 2014 turnout probability of 57.6% (95% CI: [57.5, 57.8]).[15] The turnout probability of those who waited one hour in 2012 was 57.0% [56.7, 57.3]–an average of 0.6 percentage points [0.2, 1.1] lower than those who did not wait at all. As the rugplot on the graph illustrates, most ZIP codes had an average wait of less than one hour, yet 5.4 million (4.2%) of voters in 2012

[14]The results also hold when a quadratic term is included for the wait time variable. See Table B.2 in the appendix for these results.

[15]These predicted probabilities of turnout may seem high, given that the 2014 turnout among the voting eligible population was about 36% (McDonald 2016). Recall though that this analysis conditions on people who voted in 2012, when turnout was about 58%. If we assumed that all 2014 voters also voted in 2012, then the probability of a 2012 voter turning out in the midterm would have been roughly 62% (0.36/0.58). Relaxing this assumption would bring this estimate toward the range reflected in Figure 2.1.

lived in a ZIP code with a average wait of greater than 60 minutes (CCES 2013).

Interpreting these results causally requires assuming that there are no confounding variables excluded from the model. I test this assumption using placebo tests. Because the measure of 2012 line length is in terms of the average ZIP code wait, I can approximate the amount of time mail-in absentee and non-voters would have waited if they had voted in-person. If the significant result among in-person voters is the consequence of some unmeasured confounder, we should find a similar result among mail-in and non-voters (Barreto et al. 2006; Dubin and Kalsow 1996).

Using the same specification as Model 1 in Table 2.1, I find (in Models 2 and 3) that the assumption stands up to these placebo tests. No significant relationship 2012 wait time and 2014 turnout exists among those who did not experience a long line.[16] These null results tell us is that the significant result for in-person voters is unlikely to be the consequence of some unmeasured demographic attributes that predict both line length and turnout patterns. The lack of significant results suggests that the shift in future turnout among in-person voters results from the physical act of standing in line.

2.3.2 Using matching to mitigate confounding

Although regression helps to account for confounding, it does not ensure that there will be balance between the treatment and control groups on higher order moments and interactions between covariates (Iacus, King, and Porro 2011b). To deal with this problem, I employ exact matching, which ensures treatment and

[16]There is the possibility that some of these placebo observations did, in fact, receive the treatment, whether from seeing a long line as they drove past a precinct or by actually standing in the line but leaving before they cast a ballot. However this would bias the placebo tests away from a null result, thus making them tougher tests.

control group balance for matched covariates (Angrist and Pischke 2008). This approach follows recent work which has used matching and vote history data to estimate causal effects where turnout is the outcome of interest (Fraga 2016).

Matching requires clearly defined treatment and control groups. Because the treatment of interest (line length) is continuous, I fixed the control group to be people in areas where the average line length was between 0 and 15 minutes, and defined four treatment groups based on where lines were 15-30 minutes, 30-45 minutes, 45-60 minutes, and longer than 60 minutes. I separately matched people in the control category to those in each of the four treatment categories, and used these four matched datasets to estimate four estimates of the treatment effect.[17]

I use exact matching to pair treated and control units within the same state, who are the same race (white, African-American, Hispanic, or other), and who have an identical vote history in the 2006, 2008, and 2010 general elections. Because several neighborhood demographic variables are continuous, I employed coarsened exact matching, wherein continuous variables are partitioned based on cutpoints and then exact matching is done using the discretized data (Iacus, King, and Porro 2011a). CEM allows for matching on Census block-group population density, percentage white, percent non-English speaking, and median income, as well as the voter's age.[18] Applying this matching model to the in-person, mail-in, and non-voter samples from Catalist ensures the treatment and control groups have perfect balance for the exact-matched variables, and statistically indistinguishable means for the coarsened variables.

Table 2.2 reports the post-matching estimates of the effect of a long 2012 wait on

[17] The smallest of these 5 treatment/control categories has 59,605 observations. See Appendix Table B.4 for a the sample sizes of all the groups.

[18] The block-group variables were each divided into twenty strata, based on 5% quantiles. The age variable was divided into five year bins.

Table 2.2: Effect of lines on turnout in matched dataset (2012 in-person voters only)

	(1)	(2)	(3)	(4)
Long wait	-0.0076^{***}	-0.0107^{**}	-0.0116^{**}	-0.0161^{**}
	(0.0019)	(0.0037)	(0.0043)	(0.0049)
'Treatment' group	15-30 min.	30-45 min.	45-60 min.	60+ min.
Observations (weighted)	111,623.7	29,765.9	21,352.8	18,186.4

*p<0.05; **p<0.01; ***p<0.001
OLS coefficients reported
Controls and state fixed effects included
Control group is always people where lines were between 0 and 15 minutes

an in-person voter's probability of turning out in November 2014.[19] Each column reports a separate estimate of the turnout effect, given different definitions of the 'long wait' treatment group. In all four cases, in-person voters who lived in a ZIP code with longer average waits were between 0.7 and 1.6 percentage points less likely to vote in 2014 than those who lived in neighborhoods where the average wait was between 0 and 15 minutes.

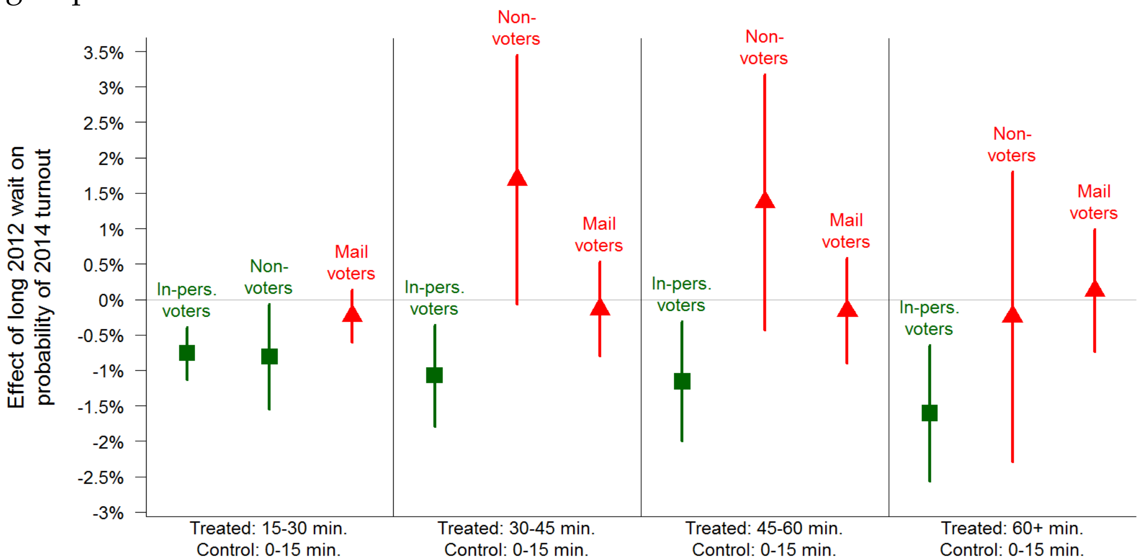
Because these results are based on matching, we can go one step further in interpretation. When selecting two voters from the same state, who are the same race and similar age, have the identical turnout history, and live in neighborhoods with nearly identical demographic profiles, the voter who lives in the neighborhood with an average wait more than an hour was 1.6 percentage points less likely to vote in 2014 than their counterpart in a neighborhood with an average wait of less than 15 minutes.

The four 'in-pers. voters' green bars and squares in Figure 2.2 visualize the results in Table 2.2.[20] The bars labeled 'non-voters' and 'mail voters' present the

[19] ZIP code cluster-robust standard errors are reported. The full results, including control covariates, are presented in Appendix Table B.5.

[20] The bars signify 95% confidence intervals.

Figure 2.2: Effect of 2012 lines on turnout for various definitions of the treatment group

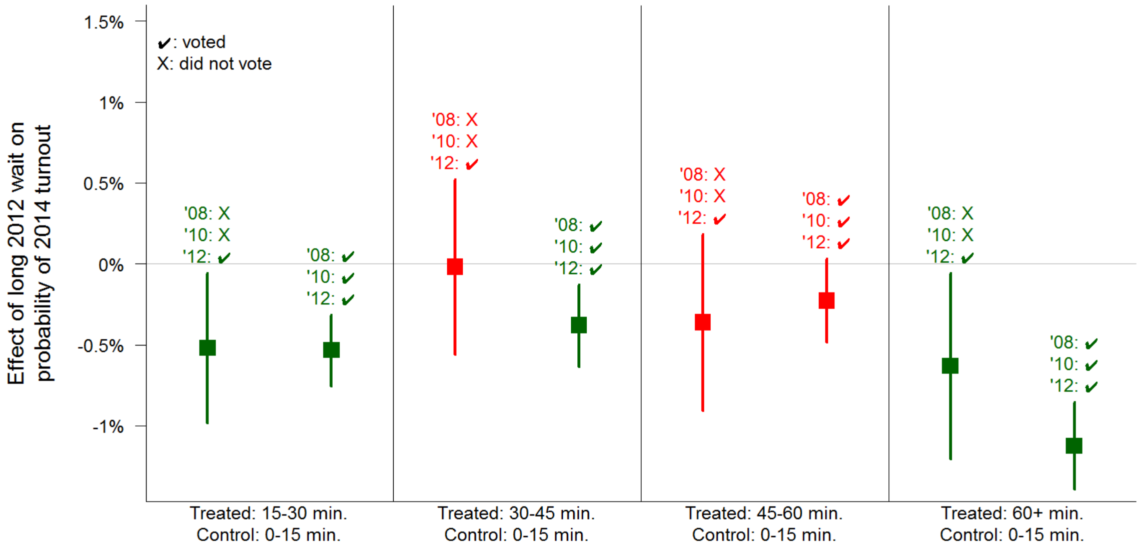


results from the eight placebo tests of the effect of lines on people who did not go to their precinct in 2012.[21] For these tests, the matching process described above was applied to one of the placebo groups, and the effect of wait times on turnout was estimated using the same model specification as Table 2.2. In seven of the eight placebo tests (marked by triangles and red bars), the results do not provide enough evidence to reject the null hypothesis.

These placebo tests, as well as those in the previous section, lend credence to the hypothesis that it is lines that are affecting turnout, rather than the results being driven by an underlying attribute of the people that live in areas with long lines. The placebo checks also hint at the mechanism at work. They suggest that the turnout effects among in-person voters are the result of the physical act of standing in line, rather than the treatment passing by word of mouth to those who did not

[21] Appendix Tables B.6 and B.7 show the full results from these models.

Figure 2.3: The effect of a long wait for people 2012 in-person voters, given their turnout history in 2008 and 2010



directly experience a long line.

One possible explanation for the results thus far is that they are driven largely by spikes in turnout in specific areas. While this would not invalidate the results, it could potentially mute the normative and policy implications of the findings. The argument is that precincts that had an unusually high turnout in 2012 are the exact areas where we would expect a dropoff in turnout in 2014, irrespective of how long the lines actually were. If this were the case, we should see a small effect of lines on people who vote every two years and a larger effect among those who voted in 2012 but do not typically participate (especially in midterms). Figure 2.3 shows the estimated treatment effects for people who voted in-person in 2012, divided out based on whether or not they voted in 2008 and 2010.[22]

[22]These results come from the four matched datasets used in Table 2.2, which were subset based on 2008 and 2010 turnout history prior to estimating the coefficients.

If turnout fully explained the result here, we would expect to see the coefficients for among regular voters to be smaller in magnitude (and perhaps statistically indistinguishable from zero) than the coefficients for more sporadic voters. Instead, Figure 2.3 shows that this pattern does not hold. Within each of the four treatment categories, there is no statistically significant difference in the effect sizes for sporadic voters (on the left) and regular voters (on the right). This evidence pushes back against the idea that the effects found here are simply a matter of low-propensity voters dropping out of the voting pool when faced with long lines. In fact, the result appears to be driven equally by low- and high-propensity voters.[23] These results indicate that the effect of long lines is not simply a story about turnout reverting to the mean, or an unmeasured variable influencing both lines and future turnout. Rather, long lines at precincts appear to have a measurable effect on the future turnout patterns of voters.

2.3.3 Voting in-person versus voting by mail in future elections

Before turning to an analysis of precinct data, I consider alternative ways in which lines may affect voter behavior. In addition to turning some voters off from the process entirely, it may also be the case that some voters shift their behavior toward voting by mail, in order to avoid lines but not withdraw from the electoral process entirely. If this occurs, we should see areas with long lines having an uptick in the proportion of voters who shift from voting in-person in 2012 to by-mail in 2014.

To evaluate this possibility, I use the same data and model as Table 2.1 but

[23]When we apply the same subgroup analysis approach to the placebo groups, I find null effects for all eight regressions using 2012 voters-by-mail and for seven of eight regressions using 2012 non-voters.

Table 2.3: How did 2012 lines impact the mode of voting in 2014?

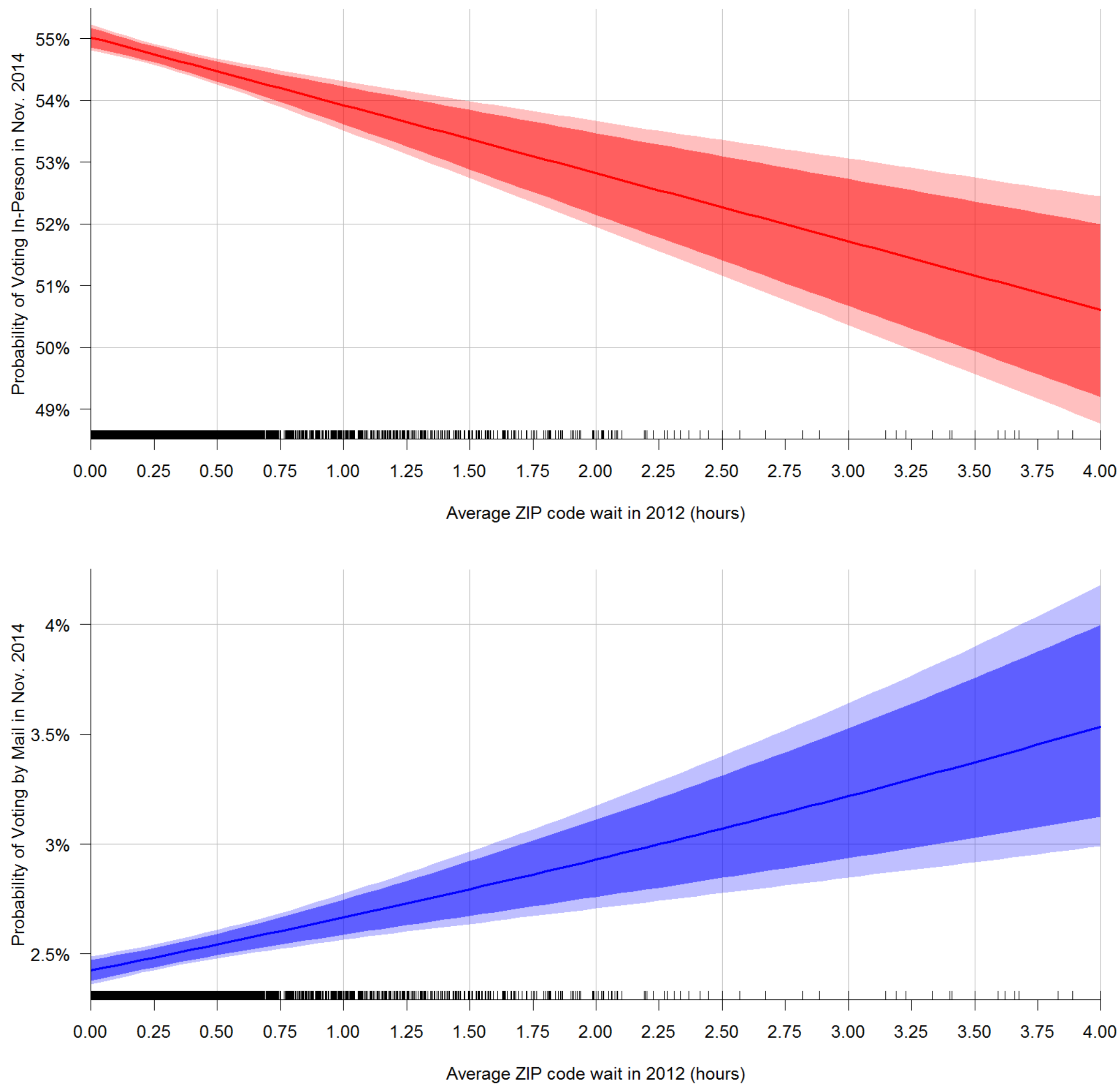
	Mode of Voting in 2012:		
	In-person	Mail	Nonvoters
2012 wait (hrs.) (DV: in-person in 2014)	-0.0443^{***}	0.0274	-0.0040
	(0.0076)	(0.0218)	(0.0154)
2012 wait (hrs.) (DV: voting by mail in 2014)	0.0972^{***}	-0.0110	0.1060
	(0.0173)	(0.0171)	(0.0675)
Observations	774,836	166,885	373,595

*p<0.05; **p<0.01; ***p<0.001
Multinominial logit coefficients reported
DV reference category: Not voting in 2014
Controls and state fixed effects included

change the dependent variable to be a three-category variable for whether the voter voted in-person, by mail, or did not vote at all in 2014. Multinomial logistic regression, summarized in Table 2.3, allows me to simultaneously estimate impact of 2012 lines each of these three outcomes. For in-person voters in 2012 (column 1 in the table), the model suggests that voters in areas with long lines were significantly less likely to vote in-person (relative to not voting at all) and significantly more likely to vote by mail in 2014. For the 2012 mail and nonvoter placebo groups (columns 2 and 3), there was no significant shift in voting patterns as a result of the length of line in a voter's neighborhood.

To better understand the magnitude of the effects from the in-person model, I calculated predicted probabilities of voting in-person or by mail in 2014. The top of Figure 2.4 shows that a voter in an area with no lines had a 55.0% (SE: 0.081%) chance of participating in-person in 2014, while somebody in an area with hour-long lines had a 53.9% (SE: 0.31%) chance of participating. The bottom panel of the figure indicates that those same voters were more likely to vote by mail instead. The magnitude of the effect here is more modest; there was a 0.2 percentage point increase in absentee voting probability (SE: 0.044 pp) for those experiencing lines

Figure 2.4: Changes in mode of voting in 2014, given different line lengths in 2012 (with 95% and 99% CIs)



of one hour compared to those experiencing no line. Combined together, the net impact of a large decrease in in-person voting and a small increase in voting by mail is a negative overall turnout effect, reported in Figures 2.1 and 2.3.

2.4 Precinct level analyses

With such consistent support the turnout hypothesis at the individual-level, I now turn to precinct-level data for further evidence. Although precinct-level data on line length is not readily available, researchers (Pettigrew 2017a; Famighetti, Melilli, and Pérez 2014) have shown that the delay in precinct closing times correlates strongly with line length at precincts. It is a strong proxy because of electoral rules: if a voter is in line when the precinct is supposed to close, they are allowed to cast a ballot. Thus, the delay between the designated and actual closing times of a precinct will be strongly correlated with line length.

One challenge to a precinct-level approach is that precinct boundaries often change between elections, in part, to alleviate long lines. It is also difficult to find the election$_{t+1}$ voting records for the set of voters who voted at a precinct in election$_t$, since the voter file just after $t+1$ only identifies their precinct for election$_{t+1}$ and not their precinct in election$_t$.[24]

To deal with these issues, I take advantage of two different research designs. First, the City of Boston provides a unique opportunity to circumvent the issue of changes to precinct boundaries. In 1920, the Massachusetts state legislature passed legislation requiring that any precinct boundary changes in Boston must be approved by the legislature. As a result, the precinct borders in the city have remained the same for nearly a century (Ryan 2009). Analyzing changes in precinct turnout after 2012 provides a better estimate of the turnout effect than is possible in a city or county where precinct boundaries can move between elections.

The second design uses precinct closing time data from 17 counties in Florida.

[24]Nyhan, Skovron, and Titiunik (2016) discuss this form of post-treatment bias more thoroughly.

Although Florida precinct boundaries were not fixed like Boston, I use snapshots of the state voter file from just after the 2012 and 2014 elections to track an individual's turnout across time. I use the 2012 snapshot to identify every voter in each 2012 precinct, and then reidentify them in the 2014 data. This allows me to calculate the 2014 turnout rates for the set of voters in each 2012 precinct, even when re-precincting or voter mobility has spread the precinct's 2012 voters across the state.

2.4.1 Changes in turnout in Boston precincts

There are 255 voting precincts in the City of Boston. In the November 2012, election the average precinct closed at 8:35 PM–35 minutes later than the designated closing time. The distribution of the closing times is right-skewed: 51.0% of precincts closed before 8:15. On the other end of the distribution, 19.8% of precincts closed more than an hour late. Six precincts had not closed their doors until after 11:00 PM; two of those did not close until 12:09 AM and 12:22 AM.

To measure the impact of lines had on future turnout, I collected the precinct turnout rates for three post-2012 elections, plus one pre-2012 election to serve as a placebo test. The three post-2012 elections – the Sept. 2013 mayoral primary election, the Nov. 2013 mayoral general election, and the Nov. 2014 federal election – were all low turnout contests. This makes them particularly difficult tests of the hypothesis, since most participants in low salience elections have more consistent voting patterns and are less likely to be affected by one bad precinct experience.

Table 2.4 reports the results of these four regressions, where the dependent variable is the change in turnout from the 2012 election.[25] In addition to controlling for

[25] Although there are 255 precincts in Boston, precinct closure time was not available for 8 of them and there is missing demographic data for two more.

Table 2.4: Effect of end-of-day lines in Boston on future turnout

	Dependent variable: Turnout change from 2012 to...			
	Nov. '14	Nov. '13	Sept. '13	Nov. '08
	(1)	(2)	(3)	(4)
Closing delay (hrs.)	-0.0060^{**}	-0.0087^{*}	-0.0058^{*}	-0.0003
	(0.0023)	(0.0035)	(0.0027)	(0.0025)
Observations	245	245	245	245
R^2	0.6540	0.6175	0.2134	0.0362

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
OLS coefficients reported
Control variables included

the 2012 delay in precinct closure, I included several precinct demographic variables,[26] as well as November 2010 turnout, which was the strongest predictor of turnout in 2014. Columns 1, 2 and 3 in the table show that for every additional hour late that a precinct closed, its turnout in subsequent elections dropped between 0.58 and 0.87 percentage points. The null result in column 4 provides evidence that the results that the post-2012 results in the first three columns are not the consequence of confounding by unmeasured factors which predict both line length and turnout in elections before or after 2012.[27]

2.4.2 Changes in turnout in Florida precincts

Like Boston, I proxy for line length using precinct closing times from 3,334 precincts in 17 Florida counties, covering 75.7% of the state's population. Un-

[26]These were percent white, median income, percent with a college degree, percent under 18 years old, and percent over 65. The racial demographics were collected from precinct level Census reports from the 2012 American Communities Survey. The others were aggregated from Census block-group data in the 2012 ACS.

[27]Because I control for 2010 turnout in the model, I chose not report 2010 as a second placebo test, although such a model (which excludes the 2010 turnout control variable) indicates a null effect (p=0.899).

like Boston, however, movement of precinct borders between elections makes it challenging to compare the reported precinct turnout in 2012 to that in 2014.[28] To estimate the effect, I first identify the set of voters in each of the 3,334 precincts in 2012 using the voter file data.[29] I then use a voter-specific identification number to reidentify each of these voters in the 2014 data and determine whether they voted in the midterm election. With this I calculate 2014 turnout rates for each 2012 precinct, including voters that may have moved to a different part of the state.[30]

Table 2.5: Impact of 2012 wait on future turnout in Florida

	Nov. 2014	Aug. 2014	Nov. 2008
	(1)	(2)	(3)
Closing delay (hrs.)	-0.0046^{***}	-0.0003	-0.0004
	(0.0004)	(0.0003)	(0.0003)
Observations (weighted)	3,334	3,334	3,334
Observations (unweighted)	3,012,356	3,012,356	3,012,356
R^2	0.1520	0.1045	0.1608

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
County fixed effects included
WLS coefficients reported

Weighted least squares estimates the relationship between 2012 precinct closing delay and future turnout at the precinct level.[31] Using variables available in the

[28]In 13 of the 17 counties, the number of precincts changed between the two elections, indicating that precinct boundaries throughout the counties were altered.

[29]The voter file snapshot was taken on February 28, 2013. While there are a small number of people who moved to a different precinct and re-registered to vote between November 2012 and February 2013, this data provides the most accurate list of voters in each precinct as is possible, given available data.

[30]This approach cannot account for voters who moved out of the state between 2012 and 2014, but Census data indicates that only about 2% of Florida's 2012 population left the state by 2014 (U.S. Census Bureau 2016) and this percentage is almost certainly smaller for registered voters, who tend to be less mobile (Ansolabehere, Hersh, and Shepsle 2012; Pettigrew and Stewart 2016).

[31]The weight for each voter in the dataset is the reciprocal of the total number of voters in their precinct, thereby ensuring one observation per precinct in the analysis.

voter files, I control for the gender balance, racial composition, average age, party registration, and 2010 turnout rate of each precinct. Table 2.5 presents the results for three regressions.[32] The first two columns test whether the end-of-day lines in 2012 were predictive of turnout rates in the November 2014 general election and the August 2014 statewide primary election. The third column is a placebo test for whether 2012 lines were correlated with November 2008 turnout.

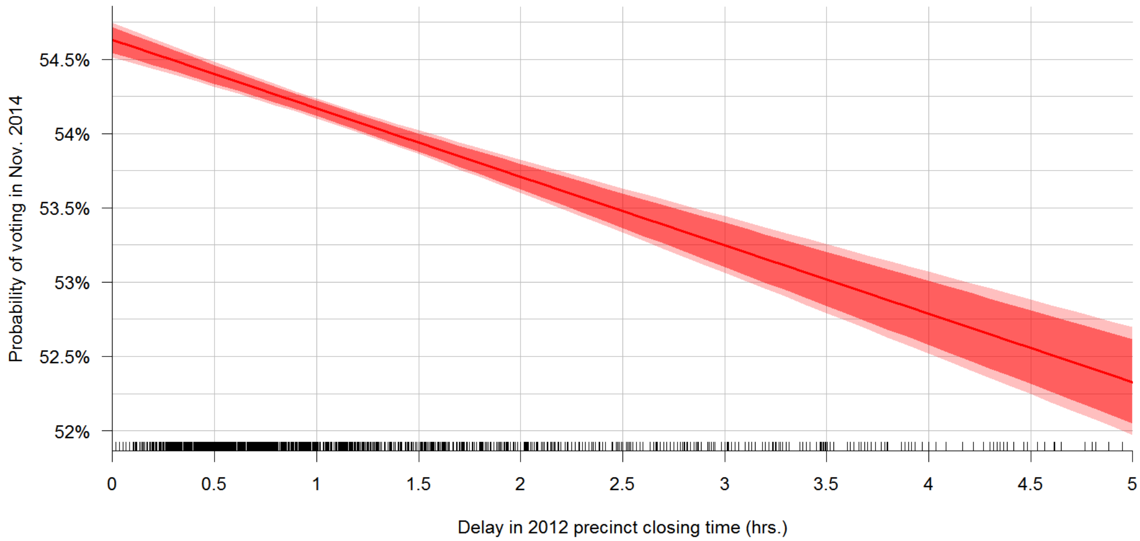
For each additional hour that a precinct stayed open in 2012, its turnout rate in November 2014 decreased by 0.5 percentage points. Additionally, column 3 of Table 2.5 shows that the placebo test checks out: 2012 line length was not predictive of 2008 turnout. The estimates in column 2, however, deviate from the hypothesis. These results suggest that 2012 closing time was not a significant predictor of the turnout in the August 2014 primary election. This finding suggests a limit to the scope of the turnout effect of lines. The turnout in the August 2014 primary was only 18%, which was the second lowest rate for any primary or general election in the state since at least 1954.[33] This makes the primary a particularly difficult test of the hypothesis, given those who did participate were very likely to have consistent turnout records and would the least unlikely to change behavior in response to a long line in 2012.

Figure 2.5 presents the November 2014 result graphically. For the 28.1% of precincts that closed within thirty minutes of the designated closing time, the average turnout in the 2014 general election among 2012 voters was 54.5%. In the 1,193 precincts (35.8%) that closed more than an hour late, the expected turnout rate was 0.46 percentage points lower than a precinct that closed on-time. The expected turnout in the 345 precincts (10.3%) that closed more than two hours late

[32] The full table of results is in Appendix Table B.12.

[33] See: http://dos.myflorida.com/elections/data-statistics/elections-data/voter-turnout/

Figure 2.5: Expected Florida precinct turnout rates in November 2014 based on 2012 end-of-day lines (with 95% and 99% CIs)



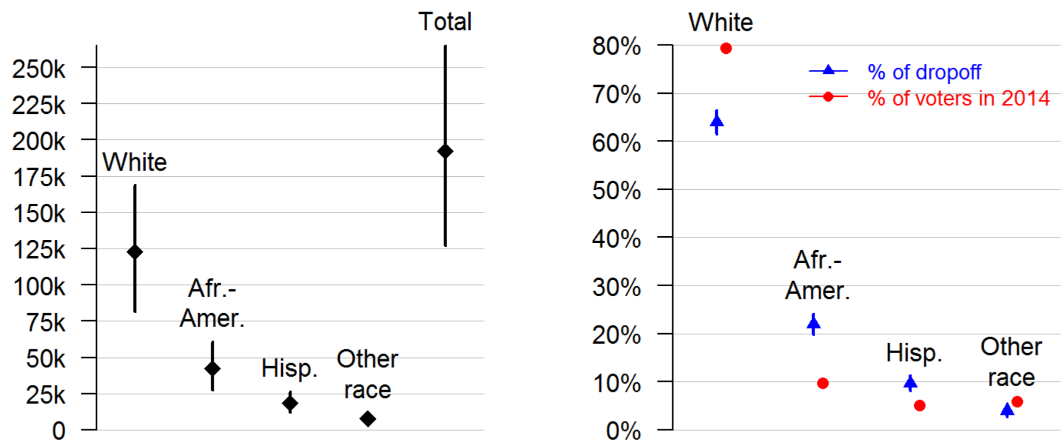
was less than 53.7%–0.92 percentage points lower than on-time precincts.

2.5 Implications and Discussion

The analyses in this paper provide consistent evidence that longer lines diminish voter turnout in future elections. The magnitude of the individual-level effect is roughly 1 percentage point for every additional hour of waiting. Given the literature on turnout, which has found that it is very difficult to change a person's probability of turning out by more than 4 or 5 percentage points (Gerber, Green, and Larimer 2008; Green, Gerber, and Nickerson 2003), an difference of 1 percentage point for the millions of voters who waited at least an hour in 2012 is consequential.

To estimate just how consequential, I used the results from Model 1 in Table 2.1

Figure 2.6: How many voters did not vote in 2014 because of 2012 lines?



to estimate the 2014 turnout probability for every 2012 in-person voter in 1% sample of the Catalist data, based on their observed covariates and their ZIP code average wait. I then estimated their probability of turning out if they had lived in an area where there were no lines to vote. The difference between these two numbers is the expected change in turnout probability for a particular voter. Figure 2.6 shows how these results vary by race. Of the roughly 107 million in-person voters in 2012, 192,100 (SE: 36,332) did not vote in 2014 as a result of waiting to vote in 2012. Given that midterms tend to be low-turnout affairs, an subtraction of 192,000 voters is not a meaningless one. This is especially true in close elections like in Arizona's 2nd congressional district, which was won by a margin of 121 votes in 2014 (out of over 220,000 cast). In that district alone, the model suggests about 258 (SE: 56.4) people did not vote as a result of lines in 2012. This is not to suggest that long lines determined the outcome of this election, only that there is a realistic potential that mismanagement at polling places could have an impact on election results in close races.

The implications of these findings broaden when we consider that minority voters are more likely to be burdened by long lines at the precinct. When voter dropoff is broken down by race, I find that the effect of lines on minority voters is disproportionate to their makeup of the electorate. While African-Americans comprised about 9.7% of the electorate in 2014, they made up 22.0% of voters turned off from voting due to 2012 lines. Similarly, 5.1% of 2014 voters were Hispanic, but 9.7% of depressed turnout came from this group.

The results broaden our understanding of turnout and citizen participation. Given that voting may be habit forming (Meredith 2009; Gerber, Green, and Shachar 2003b; de Kadt 2016), future research can explore whether the effect of lines is ephemeral or whether it persists into the future. And because lines tend to be a persistent problem in specific areas of the country (Pew Center for the States 2014), the compounding effect of regular lines may further magnify their impact on turnout. We also could better understand the role played by a person's expectations about lines. Does waiting for thirty minutes have a different impact on somebody who expected to wait ten, compared to somebody who expected to wait sixty?

From a policy standpoint, the implications of these findings are clear. Poor resource optimization by local bureaucrats is making lines more likely to emerge in minority precincts. This changes not only the racial composition of the electorate, but also the partisan composition, given the level of racial polarization in many areas of the country. It also raises the troubling possibility that individuals seeking to suppress the votes of minority voters could implement policies that are known extend waiting times in minority precincts. And as long lines make voters less likely to vote in the next election, they diminish the quality for democratic accountability for those government officials.

3 Moved Out, Moved On: Assessing the Effectiveness of Voter Registration List Maintenance

3.1 Introduction

The accuracy of voter registration lists has been at the center of debates over improving election administration in the United States for over a decade.[1] Inaccurate voter registration lists pose a problem for both those concerned about the convenient access of voters to the polls and for those concerned about voter fraud. Concern about list accuracy has led academics and advocates alike to scrutinize voter registration rolls to ferret out "deadwood" (Ansolabehere and Hersh 2013; Ansolabehere, Hersh, and Shepsle 2012), defined as obsolete records due to a person moving or dying (Shaw, Ansolabehere, and Charles Stewart III 2015). When voter rolls include more names than eligible voters, a local jurisdiction is an easy target for those concerned with list accuracy.[2]

Despite the great concern with identifying voter registration lists that have excessive deadwood and the negative optics associated with voter registration lists

[1] This chapter is coauthored with Charles Stewart III.

[2] See Judicial Watch, "Judicial Watch Warns Iowa, Colorado, DC of Potential Election Integrity Lawsuits", http://www.judicialwatch.org/press-room/press-releases/judicial-watch-warns-iowa-colorado-dc-of-potential-election-integrity-lawsuits/, March 24, 2014.

that contain more names than eligible voters, little systematic attention has been given to the core questions, "which areas seem to have the most deadwood on their voter rolls?" and "what is the source of the deadwood that does exist?" Related questions follow from these core questions: Are there as many dead people on voter registration lists as many people believe? Do areas with more voter mobility have lists with more deadwood? Are there demographic or political characteristics of electoral jurisdictions that seem to predict the presence of deadwood? When in the electoral cycle should we expect the registration lists to be the cleanest?

The goal of this paper is to begin to address these questions by considering a routine administrative task performed by every county, city, or town that runs elections: the cancellation of ineligible voter registration records, which is sometimes called "purging." To illustrate why registration removal is necessary, imagine a county with 100 adults, 15% annual mobility rates in and out of the county[3], and automatic voter registration. (That is, eligible voters are added to the rolls automatically when they turn 18 or move into the county.) If the county does no registration list cleaning, it takes this just nine years for its voter registration list to have more ineligible registrants than eligible ones. If registrant deaths are taken into account, the deadwood growth rate is even faster.

Recent work by Ansolabehere and Hersh (2013) provides some empirical estimates of how much deadwood there is on voter rolls. Using data from 2012, they estimate that approximately 4% of voter registration records were either "likely" or "probably" deadwood. They also find considerable cross-state variation. In twenty of fifty-one states (including DC), they estimate the deadwood rate at less than 1%. At the other extreme, they find that over 12% of the records in Col-

[3]This is a moderate rate, in light of the fact that the lowest national mobility rate reported by the U.S. Census Bureau was 11.6% rate in 2011.

orado and Arkansas were either likely or probably deadwood. This paper builds on Ansolabehere and Hersh by considering the sources of this deadwood and by analyzing administrative practices of the counties and towns that are responsible for running elections.

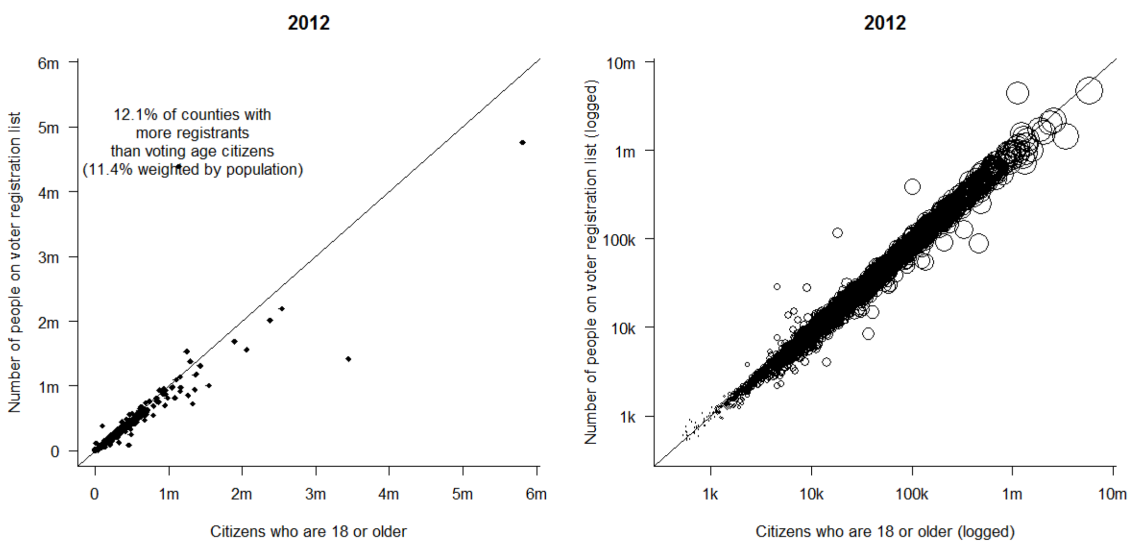
The main analyses in this paper consider registration cancellations that result from two processes: the death of registered voters and the out-migration of registered voters. Small numbers may leave the registration rolls because of felon or other disenfranchisement laws, but the number of those cases is swamped by deaths and mobility. Our main approach is to compare the reported number of registration removals in a county to independent measures of its death rate or out-migration among adults. We find compelling evidence that nearly all counties in the country do a good job of eliminating deceased registrants from their voter lists. On the other hand, our analysis of voter mobility shows a weaker relationship between mobility rates and mobility-related registration cancellations. This dynamic is partly driven by the fact that the tools and data for identifying deceased registrants are much more abundant than those for identifying registrants who have moved away.

We also consider the question of when election jurisdictions perform list maintenance activities. Using monthly county-level data from Florida and Virginia, we describe two distinct paradigms of list maintenance. In Florida, counties appear to remove the bulk of deadwood from their registration lists soon after an election. Virginia counties and cities, on the other hand, perform their list maintenance activities just prior to Election Day. This finding underscores the importance of accounting for between-state procedural differences by researchers who use Election Day voter registration data to study election administration and assess the quality of list-maintenance practices usin aggregate voter registration data.

The paper proceeds as follows. Section 3.2 describes the problems with voter registration lists by highlighting key summary statistics from states and electoral jurisdictions. In Sections 3.3 and 3.4 we analyze the patterns death and mobility-related registration cancellations. Section 3.5 focuses on the temporal patterns of registration removals. We conclude in Section 3.6.

3.2 The accuracy of voter registration lists

Figure 3.1: Citizen voting age population and number of registered voters, by county in 2012



We begin by illustrating key statistics regarding of voter registration lists. Figure 3.1 plots the relationship between the total number of voting age citizens (as estimated by the 2012 American Communities Survey) and the number of registered voters (as in the Election Administration and Voting Survey released by the U.S. Election Assistance Commission) in each county. The graphs in the figure shows that there is a strong correlation ($\rho = .923$) between the population of a county and the number of registered voters. The graphs also show that many counties have an impossibly high number of registrants, given their population. In 2012, 378 counties (12.1%) reported more registered voters than the adult population. Similar results emerge when we inspect data from 2008 (16.9% of counties), 2010 (12.4%), and 2014 (9.8%).

There are several reasons to believe that there are even more excess registrants in counties than Figure 3.1 implies. First, population is measured here as the citizen voting age population, not the voting eligible population. There is not a straightforward way to account at the county level for those who lost their voting rights due to a felony conviction or a declaration of mental incompetency McDonald and Popkin (2001). The implication is that the denominators here are slightly inflated and that some additional counties may have more registered voters than the total number of people eligible to vote in that county.

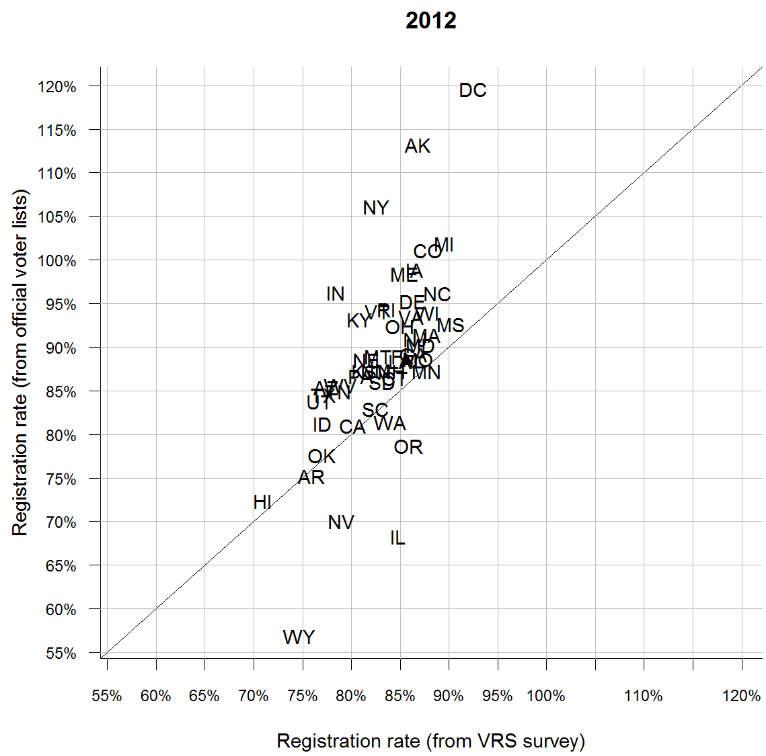
In order to account for the voting eligible population, Figure 3.2 presents two measures of voter registration rates in 2012 at the state level: the "official" registration rates reported by the states for the Election Assistance Commission's biennial National Voter Registration Act (NVRA) report and registration rate calculated using responses to the Voting and Registration Study of the Current Population Survey (VRS).[4] The graph contains one obvious finding and one more subtle one.

The first thing that jumps out is that Alaska and the District of Columbia both report registration rates in 2012 greater than 100%.[5] Another three states (Iowa,

[4]Attention has recently been drawn to the fact that the Census Bureau has long employed an unorthodox approach to calculating the registration rate in VRS reports (Hur and Achen 2013). The percentages reported here are calculated using more conventional methods, that is, dividing the number who report themselves as registered by the voting eligible respondents to the two questions in the VRS that determine registration status.

[5]We exclude North Dakota from this figure because it does not use voter registration.

Figure 3.2: Survey-estimated voter registration rate versus the official registration in 2012



Maine, and Michigan) each reported rates above 95%. The presence of registration rates greater than 100% is unsurprising to those who are familiar with the nuts and bolts of election administration, but it is fodder for those who suspect that bloated registration rolls may lead to fraud.

The more subtle finding in Figure 3.2 is that 28 states lie above the 45° line, indicating that their official registration rates are greater than the rates calculated using responses to the VRS.[6] This result is not unique to 2012; in 2008, 2010, and

[6]Because of the large number of observations for each state, the 95% confidence interval for the VRS registration rates in each state is in the 2-3 percentage point range. As a result, virtually all the data tokens in the figure that do not touch the diagonal line are outside this confidence interval.

2014, we find at least 30 states met this criterion.[7]

What makes this so surprising is that the registration rate calculated using the VRS is almost certainly on over-estimate of the true rate, given that social desirability bias systematically inflates estimates of voting rates. The VRS registration rate estimate is based on answers to two questions. First, responds are asked whether they voted in the most recent federal election. Second, those who said they did not vote are asked if they are registered. The first question produced a 26 percentage point over-estimate of turnout in the 2012 election.[8]

If the registration rate calculated using the VRS are likely over-estimates, it stands to reason that even more — and perhaps all — of the state voter registration rates calculated from official records are inflated. This suggests that in many counties and towns, the voter registration list reflects the population of registered voters, when in fact they should reflect the population of registered *and eligible* voters. Presumably, everybody on the registration list had been eligible to vote when they first registered. However, an eligible voter can become ineligible for a variety of reasons. We have already mentioned loss of eligibility due to a felony conviction or court declaration of mental incompetency.[9] These reasons, however, are dwarfed by number of people who have lost their eligibility to vote in a county because they have either moved away or moved on. One in five Americans change

[7]Figure C.1 in the appendix replicates Figure 3.2 using data from the other time periods.

[8]Using responses to the 2012 VRS, we calculate a turnout rate of 80.0%. Michael McDonald's reported nationwide turnout, using the "VAP highest office" metric is 53.6%.

[9]While there is a considerable literature on the voting propensity of ex-felons (Meredith and Morse 2015), to our knowledge no scholar has focused on whether these individuals remain on voter rolls after their conviction. Although we do not address this question here, it could provide for interesting future research.

residence in a typical two-year period,[10] and one in fifty die.[11] Removing these no-longer-eligible registrants is a crucial task if the voter rolls are to accurately reflect the population of registered and eligible voters.

This raises two important sets of questions that we will consider in the following sections. The first is how good counties and towns are at maintaining their voter rolls. The second is when in the electoral cycle counties tend to do this list maintenance and, by implication, when should we expect registration lists to most accurately reflect the registered and eligible population.

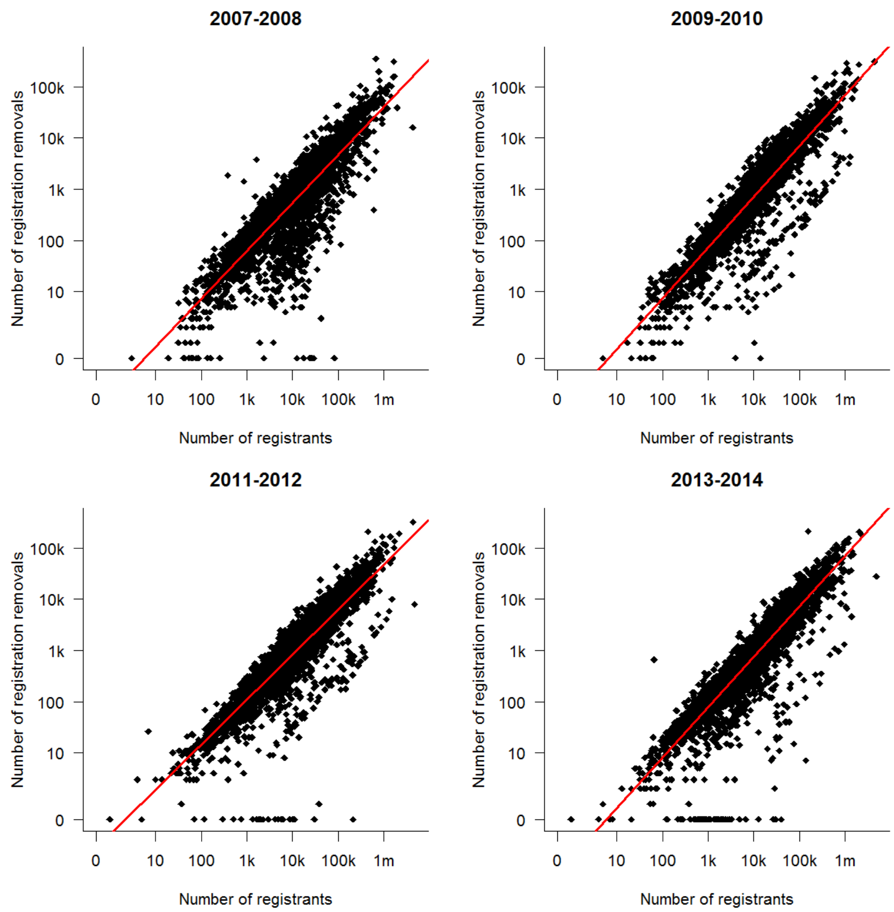
To answer the first question, we investigated the number of voter registration cancellations reported by each county. Counties, cities, and towns that administer elections throughout the country are asked to report these statistics every two years in the Election Administration and Voting Survey (EAVS). In particular, they report the "total number of voters removed from voter registration rolls in [their] jurisdiction in the period between the close of registration for the [second most recent] general election and the close of registration for the [most recent] general election." Figure 3.3 shows the relationship between the number of registered voters and the number of registration cancellations. Points above the red regression lines represent electoral jurisdictions that canceled more registrations than would be expected given the size of their voter rolls. Below the lines are jurisdictions that canceled fewer registrations than would be expect. We should expect some variation in this relationship, given that the rates of mobility, death, felony conviction, etc. will vary across counties. There is however a fairly large number of counties that lie far away from the regression line.

To understand which jurisdictions were effective at different types of registra-

[10]According to the ACS, 21.0% of Americans had moved in 2013 or 2014.

[11]Based on CDC data, 2.2% of adult-aged Americans died in 2013 or 2014.

Figure 3.3: Number of registrants and registration cancellations by electoral jurisdiction



tion removals, we took advantage of another feature of the EAVS data. In the survey, jurisdictions are asked to categorize the registration removals based on the following reasons for cancellation: "moved outside jurisdiction," "death," "disqualifying felony conviction," "failure to respond to notice sent and failure to vote in the two most recent federal elections," "declared mentally incompetent," and "voter requested to be removed." Most jurisdictions have reported data that categorizes their removals into one of these categories. In 2014, 82% of jurisdictions categorized at least 90% of their removals into one of these categories. In some counties, however, a large number of their removals were either uncategorized or categorized the reason as "other." We remove from the analysis any county where at least two-thirds of their removals were uncategorized. This eliminates cases like Jefferson County, TN, which reported 2,858 removals in 2014, but indicated that none were due to death or relocation. Instead, the county lumped all removals into the non-specific "other" category. Also, we conduct all our analyses using county level data, since our other sources of data are not available for sub-county units. This means that data from states where towns or cities run elections, like Massachusetts and New Hampshire, were aggregated to the county level.

3.3 Registration cancellations of dead people

We begin our analysis of cancellations by considering the removal of registrants who have died. The possibility that the deceased may remain registered to vote has generated a considerable amount of attention by groups concerned with voter fraud. Setting aside the fact that little evidence of this sort of voter fraud has been found,[12] the idea that the voter list is filled with dead people does not promote confidence in electoral integrity. Fortunately, as we will show later in this section, counties and states seem to be very effective at removing dead registrants from their voter lists. The National Voter Registration Act provides states tremendous latitude in their procedures for removing voters because of death, and the Help America Vote Act requires that states coordinate their list maintenance activities with other state databases (U.S. Department of Justice, Civil Rights Division 2017).

[12] There has been some recent scholarly attention to the question of non-citizen voting that, despite the controversial nature of the findings (Richman et al. 2014; Ansolabehere, Luks, and Schaffner 2015), confirms that the "likely percent of non-citizen voters in recent US elections is zero."

As a result, election officials take advantage of national and state databases which include lists of the name, residence, and birthday of all those who have died. The Social Security Death Index, as well as state-level vital statistics databases, facilitate the identification and cancellation of registration records that are no longer valid. The law also allows states to perform this specific type of list maintenance at any time, regardless of proximity to an election.

In order to assess how effective counties are at removing dead voters from registration lists, we require an objective measure of how many people ought to have been removed from the rolls. The CDC WONDER database provides a count of the number of deaths of people over the age of 20 for each county.[13] We aggregate the data into two-year windows to match the EAVS data and use it as a proxy for the number of deaths of registered voters in each county.[14]

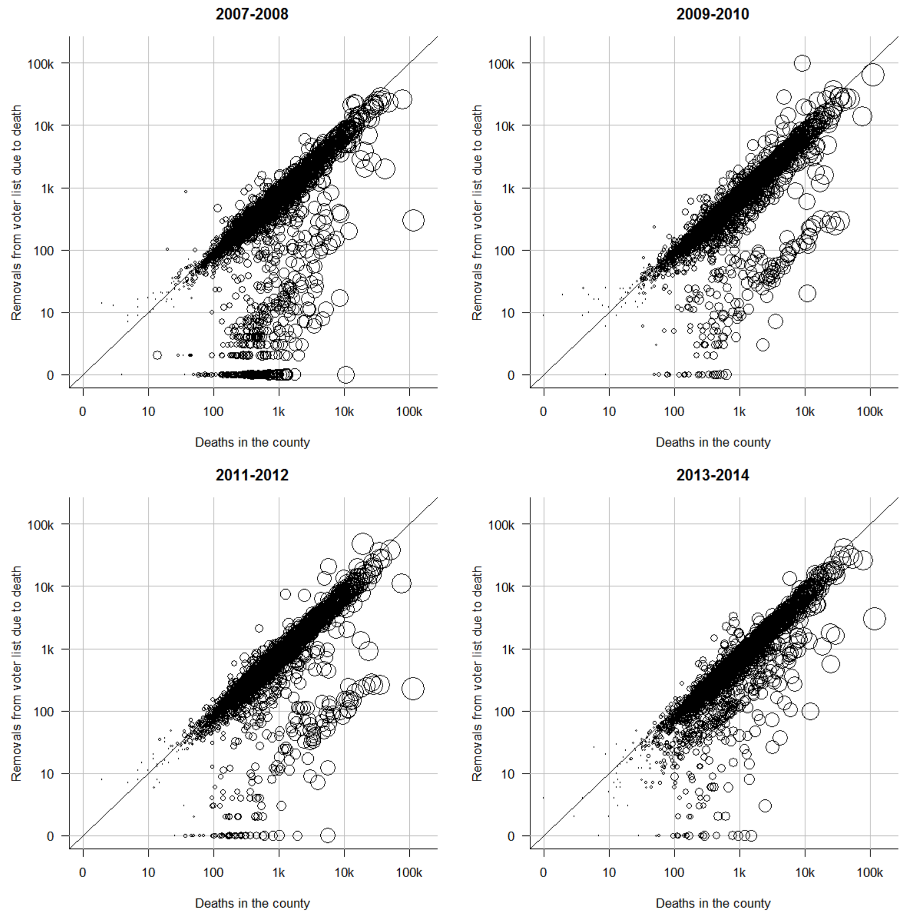
Figure 3.4 shows the relationship between the number of deaths and the number of reported death removals from registration lists in each county. The size of the circular markers is proportional to the number of registered voters in the county. The graphs demonstrate that most counties fall very close to the 45-degree line, indicating that there is a nearly one-to-one relationship between the number of deaths in a county and the number of people removed from the voter rolls due to death. In each 2-year window, there are a small but notable number of counties that purged considerably fewer people than would be expected due to the death rates.

A few counties reported purging zero people due to death. One interesting

[13]The data comes in 5-year increments so inclusion of 18 and 19 year olds is not possible without also including 15, 16, and 17 year olds.

[14]Counties with fewer than 10 deaths in any two-year period are censored in the CDC data. For this small number of cases, we impute the value by randomly sampling a number between 0 and 9. Because these counties are small in number and small in size, the substantive conclusions drawn below are not affected by the choice of imputation strategy.

Figure 3.4: Number of deaths versus number of removals due to death, by county



question is whether these counties were 'repeat offenders' and did not remove dead voters for several election cycles. The left side of Figure 3.5 shows that this does not appear to be the case. After pooling the death and death removal data across all four time periods, the relationship becomes even stronger.[15] After pooling the data, all counties removed at least some voters due to death. There are roughly two dozen counties in which there were noticeably fewer removals than we could expect. Nearly all of these counties were very small in size and, as we

[15] This plot only includes counties that reported death removal data in at least three of the four election cycles. For counties with valid removal data in only three election cycles, the total number of deaths is calculated using the CDC data from only those years.

Figure 3.5: Deaths and death removals by county

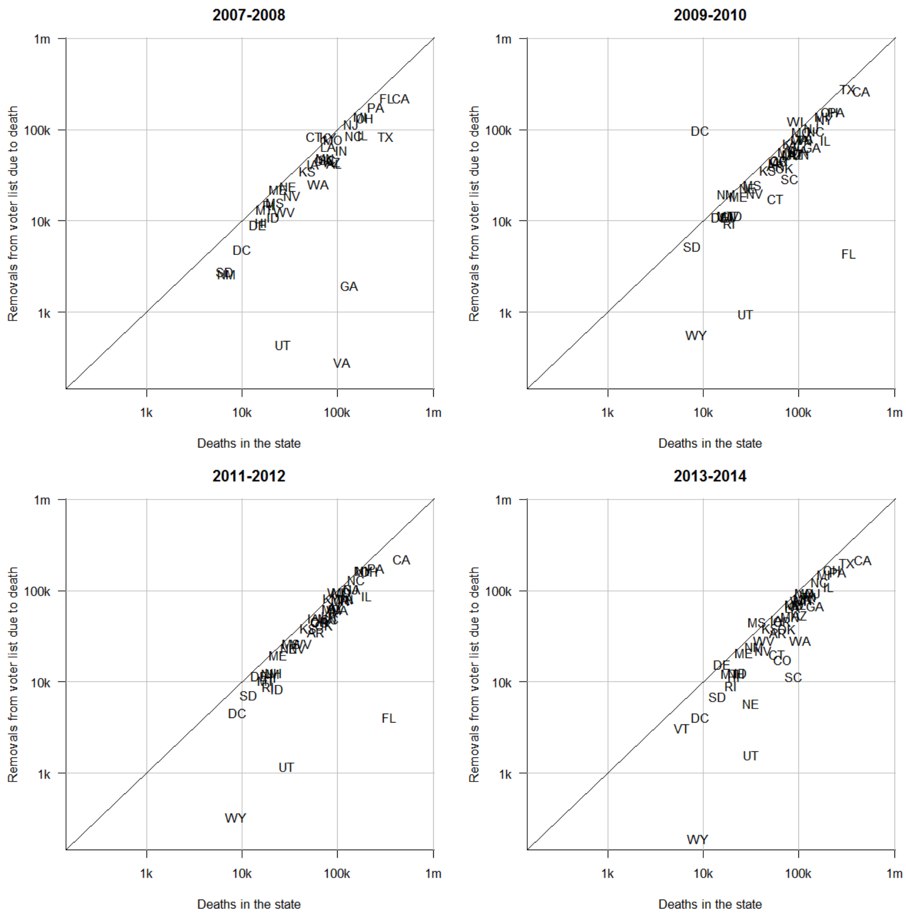
will show, come from just a couple of states.

One potential objection to the strong correlation in the left side of Figure 3.5 is that it may simply be driven by county population. Counties with many people may tend to have many deaths and death removals from their registration list. The right side of Figure 3.5 assuages this concern. This graph presents the relationship between each county's death rate[16] and its death removal rate (using the total number of registered voters as the denominator). A regression of the removal rate on the death rate indicates that for every percentage point increase in the death rate, there is a 0.8 percentage point increase in the death removal rate.

The results so far suggest that most counties perform remarkably well on the task of removing dead people from their voter registration lists. However, there is some variation in the removal patterns. To better get a sense of whether this vari-

[16]We use VAP as the denominator here, rather than CVAP, because the CDC data doesn't allow us to distinguish between citizen and non-citizen deaths. Therefore, using CVAP would give us artificially inflated death rates which would be correlated with the citizenship rate in a county.

Figure 3.6: Number of deaths versus number of removals due to death, by state



ation is simply random noise, or whether it contains patterns, we first aggregated the data to the state level. Figure 3.6 replicates the results in Figure 3.4 using data pooled by state.[17] Most states fall very close to the 45° line, suggesting that nearly all deceased registrants are being removed from voter rolls. It is worth noting that rather than being randomly clustered on either side of the 45-degree line, almost all states fall below the line. This is expected, given that the death of somebody who is unregistered would add to the number of deaths in the state, but not to the

[17] Figure C.2 in the appendix presents the state-level results, pooled across the 8-year time period.

removal number.

The figure also shows a few notable examples of states that were not close to the diagonal line. Utah reported far fewer death removals than would be expected given the number of deaths in each of the four two-year windows. Wyoming also appears to have had fewer removals than would be expected in each of the 3 years it reported data about registration cancellations. Florida experienced an expected number of removals in 2007–2008, but very low numbers in the following two election cycles. The other stand-out feature of the graph is the large number of removals in District of Columbia in 2009–2010. During that time, the District performed an audit of their voter lists and removed a large of registration records of those who had died during previous election cycles, but had not been removed.

To further assess some of the narratives around dead people on voter lists, we used regression to identify county features that correlate with the number of reported death removals. The outcome variable is the log of the total number of registration removals due to death in each county from 2007 through 2014 (as presented in Figure 3.5). In addition to controlling for the log of the number of deaths, we controlled for county partisanship (operationalized as Bush's two-party vote in 2004[18]), the percentage of people what are over the age of 65, the median income, the percent who are non-Hispanic white, and the population density.

Table 3.1 shows the results of four OLS regression models using these covariates. Models 3 and 4 include state fixed effects, while models 2 and 4 weight the observations by the log of the citizen voting age population. As shown by the previous graphs, the number of deaths in the county has the most explanatory power — for every one percentage point increase in the number of deaths, there was a

[18]We chose this year to avoid any concerns that registration removals in year i might be correlated with vote outcomes in year $i + 1$.

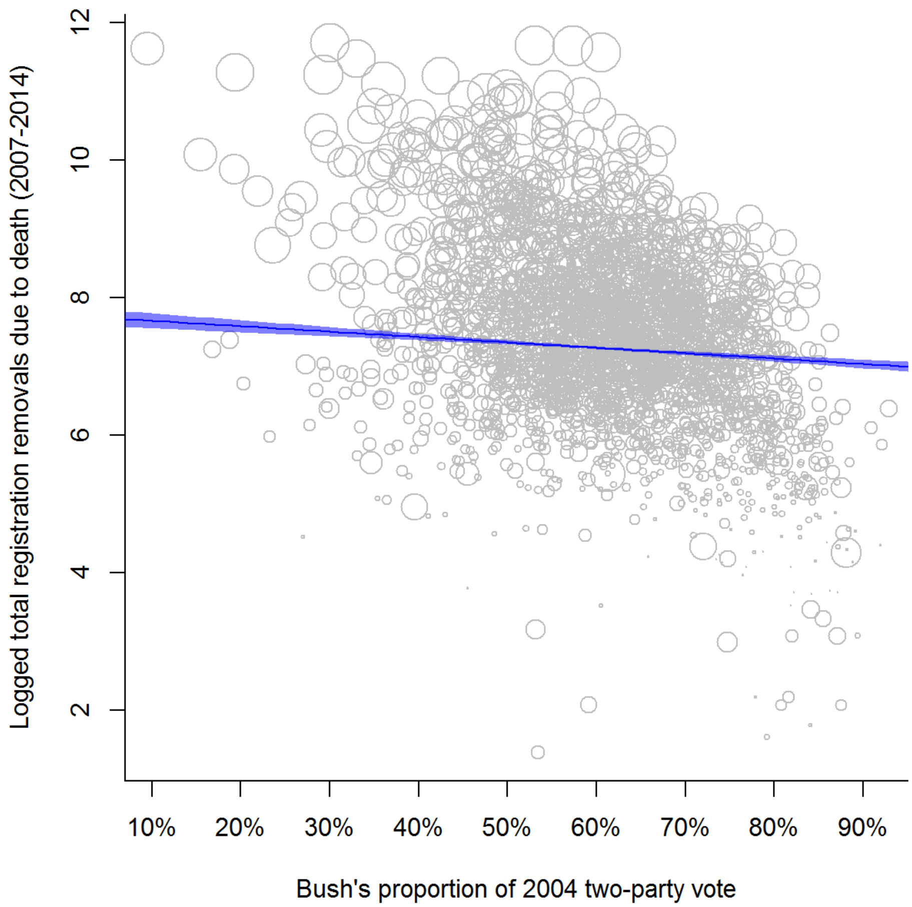
Table 3.1: What predicts cancellation of voter registrations due to death?

	(1)	(2)	(3)	(4)
Intercept	2.771***	2.543***	−0.281	−0.618
	(0.570)	(0.578)	(0.405)	(0.408)
log(Number of Deaths)	0.898***	0.897***	0.924***	0.934***
	(0.014)	(0.015)	(0.011)	(0.011)
Bush 2004 vote	−0.737***	−0.775***	−0.127	−0.111
	(0.101)	(0.103)	(0.078)	(0.079)
Pct Over 65	0.824**	0.693*	0.671***	0.804***
	(0.297)	(0.305)	(0.196)	(0.201)
log(Median Income)	−0.264***	−0.238***	0.047	0.072
	(0.057)	(0.058)	(0.041)	(0.041)
Percent White	0.600***	0.607***	0.063	0.044
	(0.078)	(0.080)	(0.067)	(0.068)
log(Population Density)	0.042**	0.040**	−0.002	−0.007
	(0.013)	(0.013)	(0.010)	(0.010)
State fixed effects			✓	✓
Weighted by CVAP		✓		✓
Observations	2,274	2,274	2,274	2,274
R^2	0.872	0.869	0.959	0.958

*p<0.05; **p<0.01; ***p<0.001
OLS coefficients reported
DV: Log of county death removals from 2007 to 2014

0.89-to-0.93 percentage point increase in the number of death removals. We also find that counties that have larger populations of senior citizens tend to remove more people from their voter lists, even when holding the number of deaths constant. There is some logic to this finding, since election administrators that serve older communities probably pay more attention to removing deceased voters from the rolls. Another interesting feature of these results is the difference between the fixed effects and non-fixed effects models. We find that things like income and population density explain some of the variation in registration removals when excluding state fixed effects, but when the fixed effects are included those correlations disappear.

Figure 3.7: Marginal effect of county partisanship on death removals



The pattern is similar with the county partisanship variable. Counties that showed higher levels of support for George W. Bush in 2004 had significantly fewer removals of dead registrants. There are a few implications of this finding. First, at the national level, predominantly Republican counties reported significantly fewer death removals than Democratic ones with the same number of deaths. This result is illustrated by Figure 3.7, which plots the marginal effect of the partisanship variable from model 2. The null result for partisanship in the fixed effects models suggest that the partisanship phenomenon does not exist within states. This finding suggests that statewide policies are what drives the partisanship result, rather than differences in administrative practice between counties within a state.[19]

Another potential reason for this result is that institutional capacity differs between Republican and Democratic states and counties. Republicans tend to have more support in more sparsely populated areas, while Democratic support is concentrated in large, urban counties and cities. Governments in large counties tend to have more capacity to have sophisticated operations, which would allow for systematic removal of dead registrants by merging the voter rolls with the Social Security Death Index. In tiny counties, like some in west Texas with only a few hundred registrants, removing dead voters can easily be done by hand. This leaves mid-sized counties, too big to conduct removals by hand, but lacking the technical resources to automate the process. Given that most of these counties tend to lean Republican, the "partisanship" result we find here could actually be a proxy for government and institutional capacity. Regardless of the underlying explanation, this result is notable. The narrative among some is that the Democratic Party benefits from an abundance of deceased voters on the registration rolls. Instead, we find that Republican counties remove fewer dead people from the voter registration rolls than we would expect given the actual number of deaths in the county.

[19]The inclusion in the analysis of Utah and Wyoming, where removal rates where abysmally low, is not driving this finding. In fact, when those states are excluded from the regressions, the partisanship coefficient is negative and significant in all four model specifications.

3.4 Registration removals of non-residents

The other major category of registration cancellations comes from those who were registered in one county, but moved to a different one. The laws regarding cancellations of this type are much different from those that govern death removals. The NVRA defines the procedure through which an election official may remove the record of somebody who has moved out of the jurisdiction (U.S. Department of Justice, Civil Rights Division 2017). The most straightforward way for this to happen is that the voter requests that they be removed from the registration list in their old county. This can come in one of three forms: a direct, unsolicited request to be removed, the completion and return of a card confirming the change of address, or the submission of a new voter registration form containing the voter's old address. For all cases which lack these proactive measures by the voter, the removal process is slower and more challenging. The state must have second-hand evidence that the voter has moved, such as their name appearing in the National Change of Address database. They must then attempt to contact the voter by sending them a notification that their registration is subject to cancellation. If the voter does not respond to this notification the state can remove them, but only after they have failed to vote to two federal general elections after the notification was sent. These provisions protect a voter from being removed from the voter rolls simply because they did not vote or because they share a name similar to somebody who is registered to vote elsewhere. They do, however, make it challenging for a county to maintain a voter list that perfectly reflect the population of registered and eligible voters. Nevertheless, we still find that some states and counties appear to be considerably more adept at navigating this list cleaning process than others.

There are two challenges to assessing county effectiveness at canceling the registration of those who moved away. The first challenge is with the measurement of county-level mobility. Ideally we would mirror our analysis of death rates by using the total number of moves out of a county by registered voters in a two year period. The closest we can come to this ideal is by using the migration information from the Statistics of Income data from the IRS.[20] The IRS estimates one-year inflow and outflow of people in each county using changes in the addresses of American taxpayers.[21]

For our purposes, this data is useful but comes with two caveats. First, the IRS data only provides information about those who file tax returns. It tell us nothing about the low income or young Americans who do not file tax returns. Given the correlation between political participation and socioeconomic status (Bartels 2008), we should expect that the migration by tax filers should be similar to that of registered voters. The second caveat concerns how we calculate the two-year county migration rate from this annual data. A tax payer can move twice in subsequent years, causing them to be counted multiple times in the IRS data. This makes it unwise to calculate the total number of two-year moves by simply adding together the county outflows in years 1 and 2. To account for this problem, throughout this section we will assess mobility rates, rather than the absolute number of moves. To calculate the one-year mobility rate, we divide a county's one-year outflow by that county's number of tax returns in that year. This ratio, α_t, tells us the percentage of tax files who moved out of a county in year t. To estimate the two-year mobility rate, we calculate $1 - (1 - \alpha_1)(1 - \alpha_2)$.[22]

[20] https://www.irs.gov/uac/soi-tax-stats-migration-data

[21] The data includes migration information based on both tax returns and tax exemptions. Throughout our analyses we estimate migration based on the returns, although using the number of exemptions does not change any conclusions.

[22] The two quantities in parentheses are the one-year percentage of people who did not move.

The second challenge to assessing mobility-related registration cancellations is in measuring the total number of cancellations that occur. Unlike removals due to death, which all fit into a single category in the EAVS, removals due to voter mobility could be assigned to one of three categories in the survey: "moved outside jurisdiction," "voter requested to be removed," and "failure to respond to notice sent and failure to vote in the two most recent federal elections." In our analyses, we group together removals from the first two categories and consider those to be removals due to voter mobility. Although the third category certainly includes a significant number of removals due to mobility, but it captures the fail-safe mechanism for list cleaning that is built into the law. Whereas the first two categories require proactive steps to confirm the cancellation of moved-away registrants, the third relies on voter inactivity over multi-year period.[23] We believe our measure best captures how adept election officials are at the registration list maintenance task.[24]

Figure 3.8 shows the relationship between county mobility rates and the re-

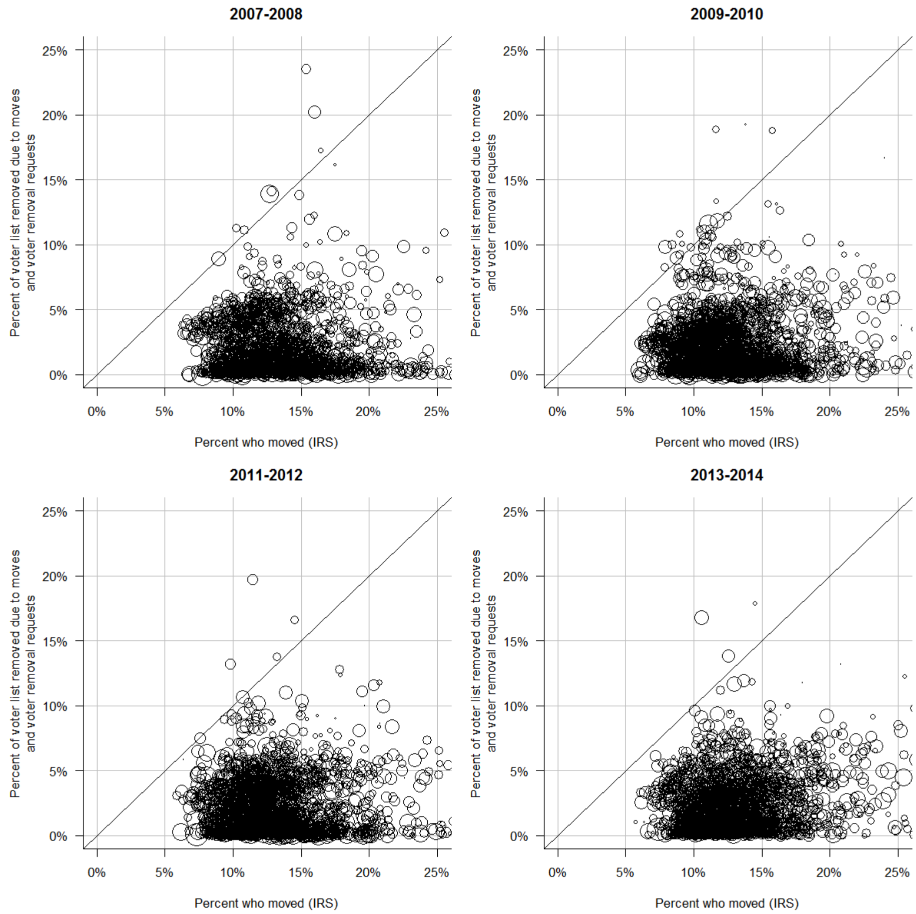
---

Multiplying them together gives us the probability that somebody did not move at all in the two-year period. Subtracting from 1 gives us the probability that a tax filer moved at least once in the two-year window. This approach ignores the covariance between the mobility rates in years 1 and 2 and will thus overestimate the true mobility rate. Despite this, when we apply this approach to the IRS data aggregated to the state-level, we find that the two-year mobility estimates that are well correlated with two-year mobility rates calculable from the American Community Survey. Unfortunately, the ACS does not provide geographic information at the sub-state level, so we cannot use it for our analysis of county level mobility.

[23]When we calculate the removal rate using all three categories, we observe higher correlations with the estimated mobility rate. This suggests that jurisdictions rely on this fail-safe mechanism and underscores the difficulty of identifying registrants who have moved.

[24]It is also worth noting that, although our measure relies somewhat on the actions of voters themselves, we are not setting the bar impossibly high for election officials. Over the three most recent election cycles, we observe the correlation between the mobility rate and this removal rate to be at least 0.85 in Virginia. This high correlation suggests that although the task of removing all registrants who have moved away is difficult, it is certainly possible.

Figure 3.8: Migration rate and voter removal rate due to mobility by county



moval rates due to mobility.[25] The relationship between these two rates is nowhere close to one-to-one, as was the case with death removals. Some of this may be explained by measurement error, although there is almost certainly something else going on here. If counties were doing a terrific job of keeping their voter lists clean of those who moved away, then random measurement error would yield a roughly equal number of counties above and below the black 45-degree line. Instead, we find that in all four time periods nearly every county had a mobility rate that was larger than the removal rate. This is evidence that counties have a difficult time

[25]Figure C.3 shows this relationship at the state-level.

removing moved-away people from their voters rolls. Despite this, these is a positive and significant correlation between mobility and removal rates in all four time periods. The correlation is strongest during the two years prior to a midterm election, which suggests that county election officials might be more cautious with registration cancellations in the lead up to high-turnout presidential election.

Table 3.2: What predicts cancellation of voter registrations due to mobility?

	2011-2012	2011-2012	2013-2014	2013-2014
	(1)	(2)	(3)	(4)
Intercept	-0.062^{**}	-0.047^{*}	-0.127^{***}	-0.101^{***}
	(0.024)	(0.019)	(0.022)	(0.019)
Mobility rate	0.042^{***}	0.093^{***}	0.114^{***}	0.116^{***}
	(0.011)	(0.008)	(0.012)	(0.009)
Bush 2004 vote	-0.013^{**}	-0.004	0.001	-0.005
	(0.004)	(0.004)	(0.004)	(0.003)
Pct Over 65	0.032^{*}	0.008	0.049^{***}	0.012
	(0.013)	(0.010)	(0.013)	(0.010)
log(Med. Income)	0.008^{***}	0.004^{*}	0.013^{***}	0.009^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Pct White	0.001	0.007^{*}	-0.014^{***}	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
log(Pop. Dens.)	0.0001	-0.001^{***}	0.002^{***}	-0.001^{***}
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
State fixed effects		✓		✓
Observations	2,552	2,552	2,510	2,510
R^2	0.016	0.618	0.113	0.620

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
OLS coefficients reported (weighted by CVAP)

We used regression to to further understand the patterns that predict the registration cancellation rate in each county. Because we cannot pool the mobility data across all eight years as we did with the death data, Table 3.2 presents separate regression results using data from 2011-2012 and 2013-2014.[26] In 2013-2014, each percentage point increase in the mobility rate correlated with a 0.11 percentage point increase in the mobility removal rate. This was a larger correlation than existed in 2011-2012. We also find that there is a positive and significant correlation between a county's income and its removal rate. This could be further evidence of large, wealthier counties having better institutional capacity to deal with registration list cleaning.

The models provide mixed evidence regarding the effect of age and population density. When compared across states (columns 1 and 3), counties with larger populations of senior citizens tend to have higher removal rates due to mobility. When the state fixed effects are included (columns 1 and 3), however, this relationship becomes indistinguishable from zero. Similarly when counties within a state are compared to each other, more dense ones tend to have a lower removal rate. The evidence from comparing counties between states suggests the opposite relationship in 2013-2014 and no significant relationship in 2011-2012.

Regarding the role of partisanship, there are some parallels to the findings on death removals, although the evidence is slightly less strong. When state fixed effects are included in the model, there is no significant relationship between county partisanship and mobility-related removals in any of the election periods analyzed here (including those in the appendix). When counties are compared across state boundaries, we find some evidence that Republican areas remove fewer people from their registration roles than would be expected given their mobility rates. While this relationship is indistinguishable from zero in 2013-2014, it is negative and significant in 2011–2012, as well as 2007–2008 and 2009–2010. The magnitude

[26] Appendix Figures C.1 presents similar results for 2007-2010.

of the effect, however, is substantively small. For every percentage point of additional support for Bush, there was a 0.0013 percentage point lower registration cancellation rate in 2011-2012. This means that the expected difference in mobility removal rates in the counties with the most and least support for Bush was just 1 percentage point.

3.5 Timing of registration removals

To this point, we have considered how good a job counties do at cleaning their voter registration lists over two-year windows. We now turn to the question of when this work is being conducted. Whether a county conducts list cleaning on a monthly basis or only once every two years may have big implications for researchers. Similarly, whether a county conducts most of its list cleaning before or after high-turnout elections may impact how campaigns utilize registration lists and how researchers use Election Day registration data.

In this section we present evidence of two paradigms of voter list cleaning utilized by two states: Florida and Virginia. We find that Florida conducts the bulk of its registration list cleaning efforts after an election has occurred. As we will discuss later, this minimizes the risk that eligible voters will erroneously removed from the voter list, which would cause administrative headaches on Election Day. Virginia, on the other hand, preforms a large amount of its list maintenance prior to its major elections. Its state Department of Elections uses well-defined procedures and large supplemental datasets to minimize the number of potential errors.

The data for this section come from the Florida Division of Elections[27] and the

[27] http://dos.myflorida.com/elections/data-statistics/voter-registration-statistics/voter-registration-monthly-reports

Virginia Department of Elections.[28] Each state publishes reports of voter registration statistics which include the number of voters removed from the rolls each month. In Florida, the data are available for every month between January 1995 to December 2016 in 64 of the state's 67 counties, and for at least 80% of months in that time period for the remaining 3 counties.[29] Virginia has reported its data for each of its 134 counties and independent cities in every month from January 2007 to February 2017.

Throughout this section, we focus on the monthly rate of registration cancellation in each county or town. By dividing the number of registration removals by the number of registered voters at the beginning of the month, we ensure that our results provide meaningful results across jurisdictions of vastly different sizes. Because we are interested in deviations from a jurisdiction's typical rate of removal across time, we employ jurisdiction-level fixed effects in each analysis.

3.5.1 List maintenance in Florida

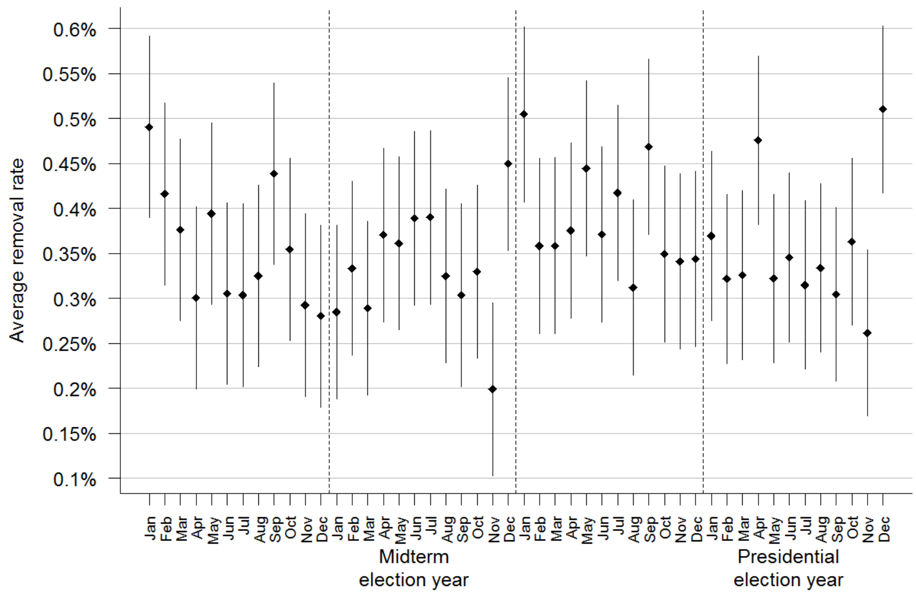
Figure 3.9 presents the average registration cancellation rate in a typical Florida county across the 48 months of a presidential election cycle. The figure shows that the lowest removal rates occur in November of midterm and presidential election years. The fall months prior to these elections do not show unusual average removal rates, while the December and January that follow the elections have cancellation rates that are among the highest in the 4 year cycle.

To more clearly analyze this pattern, we identified the date of all statewide pri-

[28]http://www.elections.virginia.gov/resultsreports/registration-statistics/index.html

[29]In July of 2009, Miami-Dade County reported canceling 123,791 registrations, nearly 10% of registration records. We dropped this observation because we believe this number to be incorrect, since the county reported only 4328 removals in all of 2009 and 2010 in the EAVS.

Figure 3.9: Average number of registration cancellations in Florida by month (1995-2016)

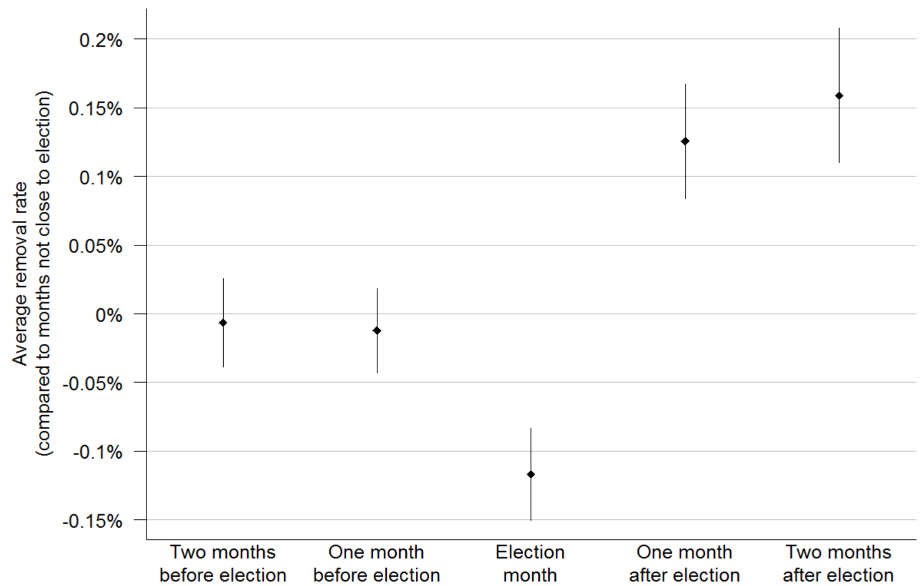


mary and general elections throughout the 22 year time series.[30] We then characterized each month as either an election month, the first or second month preceding an election, or the first or second month following an election. The remaining months that were not temporally proximate to an election serve as a reference, 'control' category in the analysis.

Figure 3.10 displays how the removal rates during the five months surrounding an election differ from all other months in the time series. We find that the two months preceding the election do not have cancellation rates that are statistically distinguishable from months that are not close to an election. During the month of an election, even less emphasis is placed on list maintenance, resulting in fewer cancellations than we would expect in a 'control' month. This is not a

[30]We omit presidential primaries because in some years they fell within two or three months of statewide primary, making it difficult to categorize the months in between.

Figure 3.10: Average rate of registration cancellations in Florida by proximity to a statewide election



surprise given the other responsibilities placed on election administrators in the weeks immediately before and after an election. Following an election, we see a large uptick in list maintenance activity. While some of this may be accounting for the removals that ought of have occurred in November, the size of the coefficients suggests that Florida counties use the months after an election to "catch up" on registration cancellations.[31]

The data conveyed in these two figures indicate that Florida's approach to registration list cleaning is to be more aggressive in canceling registrations after an election than before it. From an administrative perspective, this minimizes the possibility that somebody shows up to vote on Election Day, only to find out that their registration has been canceled. The possible downside to this approach is that

[31]The election itself provides additional information about registrants who may have moved away.

the registration lists on Election Day will include people who are no longer eligible to vote. Such potential inaccuracies in the voter registration list could yield inefficient resource allocation to precincts and possibly open the door to voter fraud, although very little empirical evidence of such activity exists.

3.5.2 List maintenance in Virginia

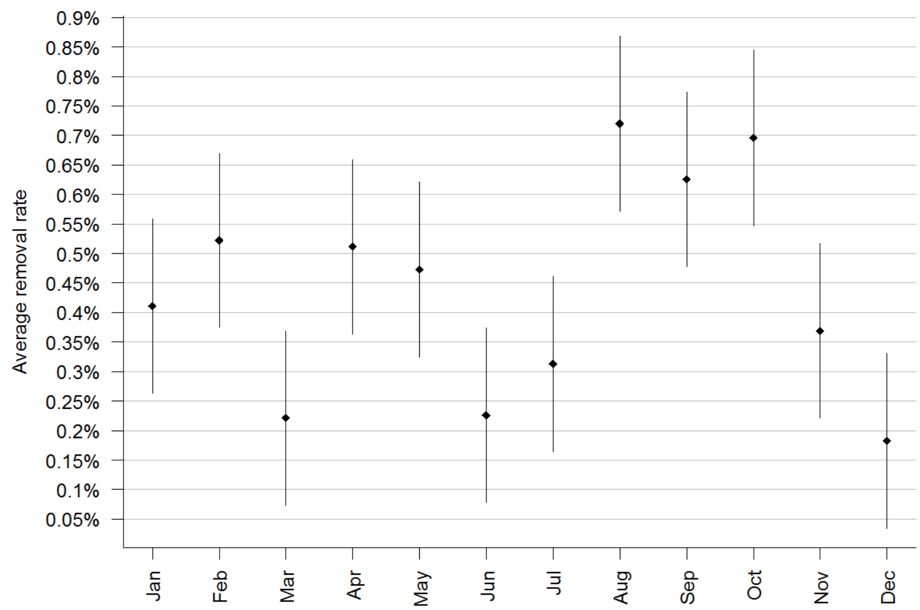
Virginia provides an example of the opposite approach to list maintenance, wherein election officials are aggressive with removals prior to an election. The state also provides a unique case for studying temporal patterns in voter list maintenance because holds elections across the state every November, rather than just in even years.[32] As a result, the monthly trends in removal rates do not vary dramatically between years in a 4-year electoral cycle.

Figure 3.11 reports each month's average rate of registration cancellation in a typical Virginia jurisdiction.[33] The figure shows a pattern to registration cancellation that is very different from the one in Florida. August, September, and October have the highest average rates of registration removals, meaning that Virginia jurisdictions do the bulk of their voter registration list cleaning prior to the annual November general election. We also see that November and December have some of the lowest removal rates, indicating that the state does not focus its effort on list maintenance after an election. A similar pattern emerges around the months which feature the presidential (March) and state (June) primaries. Relatively little

[32]In addition to federal elections in the even years, Virginia elects its governor in the odd year following a presidential election. State legislative elections also occur on odd years, with all members of the state house being subject to reelection every two years.

[33]These results come from a linear regression model which includes jurisdiction fixed effects, indicators that account for the year in the 4-year cycle, and 12 month-specific dummy variables. Omitting the jurisdiction or year control variables does not alter the conclusions drawn.

Figure 3.11: Average number of registration cancellations in Virginia counties and cities by month (2007-2017)



list cleaning occurs during the month which features the election, and most of this work occurs in the month or two before the primaries.

Figure 3.12 shows this pattern more clearly. The two months before an election have removal rates that are 0.33 and 0.25 percentage points higher than a typical 'control' month. In the months during and immediately after the election cancellations hit a trough, with 0.08 and 0.12 percentage point fewer removals than is typical. The rate jumps back up in the second month after an election, although it's possible that this is evidence of the jurisdictions catching up on the removals that had not occurred in the prior two months.

To get a better sense of how Virginia's process limits the accidental removal of eligible voters, we make use of additional details about the list maintenance process in the Virginia data. Whereas the Florida data only provides the total number

Figure 3.12: Average rate of registration cancellations in Virginia by proximity to a statewide election

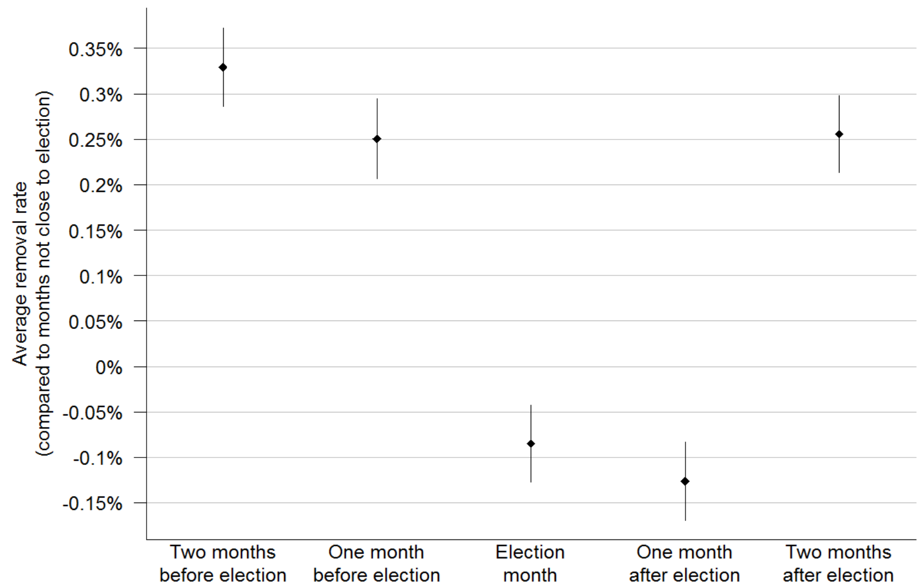
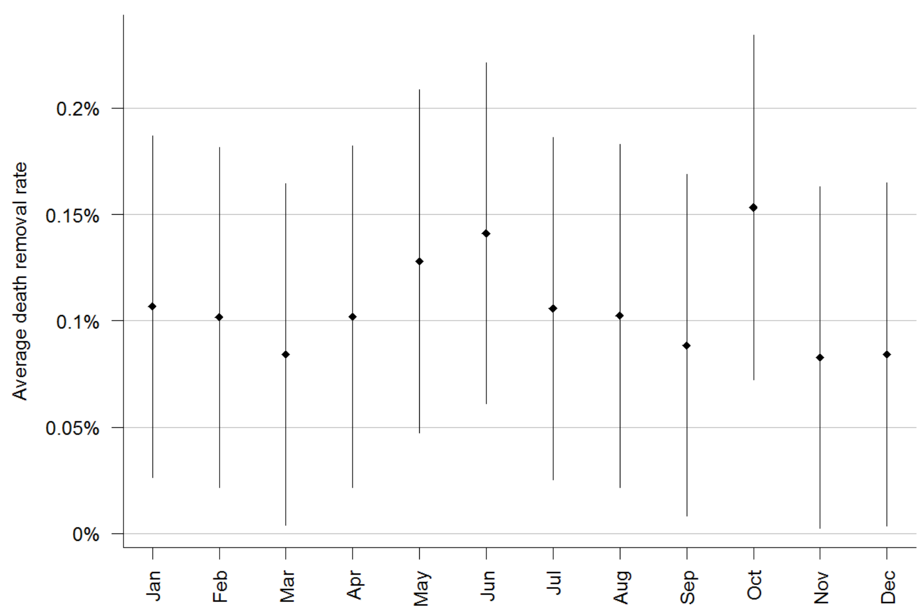
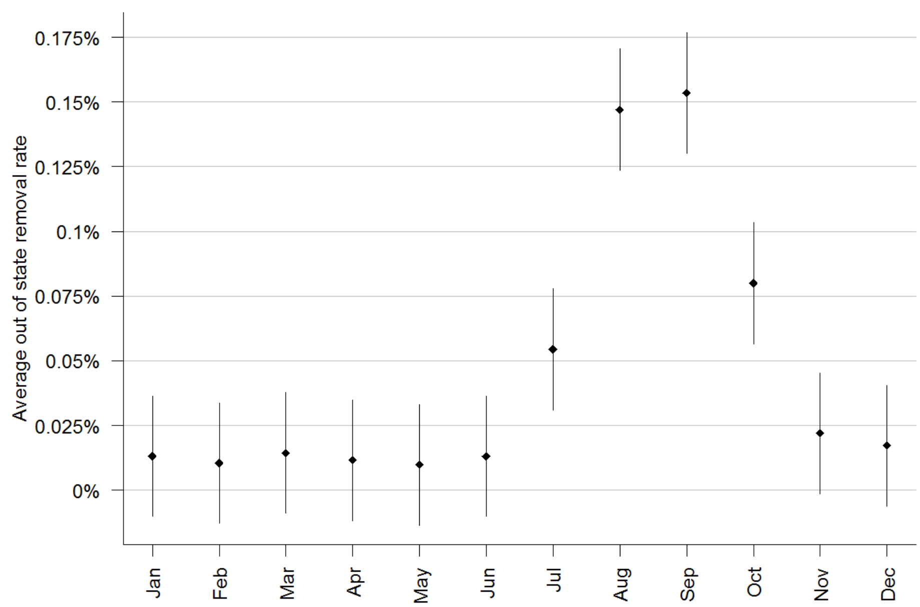


Figure 3.13: Rate of registration cancellations due to death in Virginia



of removals in a month, Virginia's subdivides the data by reason for removal. Figure 3.13 shows the monthly removal rates of deceased registrants. Although October has the highest estimated removal rate, the more fundamental takeaway from this graph is that no month has a death removal rate that is statistically different from any other month. The reason for the 'flatness' of these removal rates can be traced to the way in which the state identifies the records. An annual report by the State Board of Elections 2016 explains that, every month, data from the Social Security Administration and the Virginia Department of Health is merged into the registration database and deceased registrants are flagged for removal. Because of the automated nature of this process, it is no surprise that the removal rates in Figure 3.13 are nearly flat.

Figure 3.14: Rate of registration cancellations due to out-of-state moves from Virginia



The story with mobility-related removals is much different. Figure 3.14 shows the monthly removal rate of people who have moved out of state. The high-variance pattern here clearly contrasts the flat shape of the previous graph. Between November and June virtually no registrations are removed due to mobility. In the late summer and early fall, however, the removal rate dramatically spikes. Again, the state's annual report on voter registration provides insight into this pattern. Removals that fit into this category were identified using two interstate data sharing programs. The Voter Registration Crosscheck Program and the Electronic Registration Information Center provide Virginia with access to the registration lists of 35 states. Voters identified as having moved are sent a mailer to their old and new addresses and removed immediately upon voter confirmation. The pattern of Figure 3.14 tells us that jurisdictions act on these removal confirmations prior to the November election, ensuring that the voter rolls are cleaned of these out-of-state registrants.

This data tells us a few things about the temporal patterns to voter registration list cleaning. There appear to be two approaches to list maintenance, defined by whether jurisdictions perform the bulk of the work just before or just after an election. Florida minimizes the chance of inaccurate registration cancellations by using the more conservative, after-election approach. Virginia, on the other hand, uses the aggressive before-election approach, while minimizing the chances of errors by having the state government leverage large datasets to flag ineligible registrations and allowing the counties and cities to confirm these removals.

3.6 Conclusion

The purpose of this paper is to understand and assess the effectiveness of voter registration list cleaning processes by state and local governments. We find that in most areas, the number of people on registration lists is implausibly high. Our analyses suggest that election administrators are highly effective at removing deceased registrants from voter lists, pushing against the narrative that voter registration lists are filled with an abundance of dead voters. On the other hand, we find that election officials have a much more difficult time identifying and removing registrants who have moved from their jurisdiction. This should come to no surprise to those who read reports, following Donald Trump's tweet about the topic, that the president's daughter, chief strategist, and press secretary were all registered to vote in multiple states.[34]

It is worth pointing out that, without overhauling the electoral system to eliminate voter registration entirely, deadwood on voter registration lists is an inevitability. At some point before an election, a local jurisdiction must "lock" this voter list from having any additional removals. This allows for precinct workers to have a stable list of registered voters, which in some parts of the country are still printed. Even if voter lists are locked the day before an election, there will still be a small number of registered voters who die (or move) and remain on the list. Similarly, as more states adopt early in-person or absentee by-mail voting periods that begin weeks prior to Election Day, there will always be a possibility that somebody casts an early ballot, but dies before it is counted Election Day.[35]

There are, however, good reasons for election officials to make a concerted efforts to have their registration lists be as clean of deadwood as possible, while attempting to keep the number of erroneous removals as close to zero as possible.

[34]"Who is Registered to Vote in Two States? Some of Trump's Inner Circle." *New York Times*. January 27, 2017. https://www.nytimes.com/2017/01/27/us/politics/trump-cabinet-family-voter-registration.html

[35]A large study of voter fraud (Levitt 2007) found that a large number of suspected cases of voter fraud by deceased voters turned out to be cases of early voters dying before Election Day.

Our analysis of Virginia suggests that the system they have in place could serve as a model for other states. They remove deadwood registration records before their high-turnout general elections, which has the added benefit of working with a more accurate set of statistics to use for resource allocation to precincts and other administrative tasks. The state also removes deceased registrants on a monthly basis, ensuring that sporadically scheduled special or primary elections will utilize voter rolls that are largely absent of this deadwood. They have explicit state-level policies and procedures for identifying, contacting, and removing those who moved away from the state. The result is a voter registration list that accurately reflects the true population of registered and eligible voters.

A | Appendix to *The Race Gap in Wait Times*

Table A.1: How did neighborhood demographics impact average wait?

	(1)	(2)	(3)	(4)
Intercept	16.504^{***}	6.671^{***}		
	(0.259)	(0.945)		
White Pct	-14.177^{***}	-17.386^{***}	-7.930^{***}	-12.139^{***}
	(0.286)	(0.561)	(0.430)	(0.743)
Population Dens.		0.0001^{***}		0.0002^{***}
		(0.00001)		(0.00002)
Pct. over 65		5.305^{**}		-1.400
		(1.640)		(2.051)
Median Income		0.066^{***}		0.048^{***}
		(0.004)		(0.006)
Pct. Speak Eng.		10.929^{***}		12.906^{***}
		(0.777)		(1.030)
Afr.-Amer.		5.480^{***}		3.593^{***}
		(0.333)		(0.323)
Hispanic		0.773^{*}		-0.131
		(0.368)		(0.357)
Asian-Amer.		0.706		2.712^{***}
		(0.801)		(0.764)
Other race		-0.614		-0.110
		(0.419)		(0.405)
Age		-0.027		-0.014
		(0.031)		(0.030)
Age^2		-0.0005		-0.001
		(0.0003)		(0.0003)
Party: Indep.		0.402		0.190
		(0.287)		(0.277)
Party: Repub.		-0.627^{**}		-0.905^{***}
		(0.199)		(0.194)
Early Voter		5.537^{***}		5.556^{***}
		(0.230)		(0.254)
2008	9.385^{***}	8.125^{***}	9.836^{***}	8.725^{***}
	(0.239)	(0.260)	(0.231)	(0.250)
2012	7.271^{***}	6.321^{***}	7.553^{***}	6.728^{***}
	(0.206)	(0.226)	(0.200)	(0.219)
2014	-1.784^{***}	-1.441^{***}	-1.563^{***}	-1.027^{***}
	(0.210)	(0.274)	(0.204)	(0.265)
Juris. fixed effects			✓	✓
Observations	91,907	78,102	91,907	78,102
R^2	0.066	0.083	0.196	0.227
Adjusted R^2	0.066	0.083	0.171	0.199

Note: $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
Intercept not calculated with fixed effects models

Table A.2: How did neighborhood demographics impact probability of waiting more than 30 minutes?

	(1)	(2)	(3)	(4)
Intercept	-2.112^{***}	-2.112^{***}	-3.662^{***}	-4.392^{***}
	(0.046)	(0.046)	(0.648)	(0.675)
White Pct	-1.670^{***}	-1.670^{***}	-0.940^{***}	-1.231^{***}
	(0.044)	(0.044)	(0.074)	(0.115)
Population Dens.				0.00002^{***}
				(0.00000)
Pct. over 65				-1.043^{**}
				(0.368)
Median Income				0.008^{***}
				(0.001)
Pct. Speak Eng.				0.973^{***}
				(0.162)
Afr.-Amer.				0.203^{***}
				(0.048)
Hispanic				-0.065
				(0.062)
Asian-Amer.				0.266^{*}
				(0.119)
Other race				0.118
				(0.072)
Age				0.001
				(0.005)
Age^2				-0.0001
				(0.0001)
Party: Indep.				0.059
				(0.050)
Party: Repub.				-0.135^{***}
				(0.034)
Early Voter				0.485^{***}
				(0.040)
2008	1.487^{***}	1.487^{***}	1.716^{***}	1.629^{***}
	(0.045)	(0.045)	(0.048)	(0.050)
2012	1.232^{***}	1.232^{***}	1.375^{***}	1.312^{***}
	(0.042)	(0.042)	(0.045)	(0.048)
2014	-0.530^{***}	-0.530^{***}	-0.525^{***}	-0.538^{***}
	(0.055)	(0.055)	(0.057)	(0.074)
Juris. fixed effects			✓	✓
Observations	91,907	91,907	91,907	78,102
Log Likelihood	−24,786.620	−24,786.620	1,395.000	−20,190.980
Akaike Inf. Crit.	49,583.250	49,583.250	2,790.000	45,961.960
Note:		*p<0.05; **p<0.01; ***p<0.001		

Table A.3: How did neighborhood demographics impact probability of waiting more than 60 minutes?

	(1)	(2)	(3)	(4)
Intercept	-3.312^{***}	-3.312^{***}	-5.253^{***}	-6.873^{***}
	(0.088)	(0.088)	(1.280)	(1.317)
White Pct	-2.253^{***}	-2.253^{***}	-1.264^{***}	-1.899^{***}
	(0.073)	(0.073)	(0.123)	(0.184)
Population Dens.				0.00003^{***}
				(0.00001)
Pct. over 65				-0.224
				(0.576)
Median Income				0.010^{***}
				(0.002)
Pct. Speak Eng.				1.732^{***}
				(0.265)
Afr.-Amer.				0.053
				(0.076)
Hispanic				-0.122
				(0.104)
Asian-Amer.				0.097
				(0.213)
Other race				-0.166
				(0.132)
Age				0.016
				(0.009)
Age^2				-0.0003^{**}
				(0.0001)
Party: Indep.				0.021
				(0.089)
Party: Repub.				-0.052
				(0.059)
Early Voter				0.865^{***}
				(0.063)
2008	1.929^{***}	1.929^{***}	2.264^{***}	2.062^{***}
	(0.087)	(0.087)	(0.093)	(0.096)
2012	1.469^{***}	1.469^{***}	1.705^{***}	1.531^{***}
	(0.085)	(0.085)	(0.090)	(0.094)
2014	-1.196^{***}	-1.196^{***}	-1.089^{***}	-1.124^{***}
	(0.138)	(0.138)	(0.142)	(0.185)
Juris. fixed effects			✓	✓
Observations	91,907	91,907	91,907	78,102
Log Likelihood	$-9{,}948.267$	$-9{,}948.267$	1,395.000	$-7{,}370.696$
Akaike Inf. Crit.	19,906.530	19,906.530	2,790.000	20,321.390
Note:	*p<0.05; **p<0.01; ***p<0.001			

Table A.4: How does precinct racial demographics correlate with allocation of resources to precincts?

	Registrants per Poll Worker	Registrants per Voting Machine
	(1)	(2)
Intercept	196.948^{***}	336.571^{***}
	(10.482)	(41.065)
Pct. White in Precinct	-0.228^{***}	-0.915^{***}
	(0.055)	(0.192)
Observations	5,228	6,601
R^2	0.352	0.609
Note:		$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$ County fixed effects included

Validity of survey-based measures of wait times

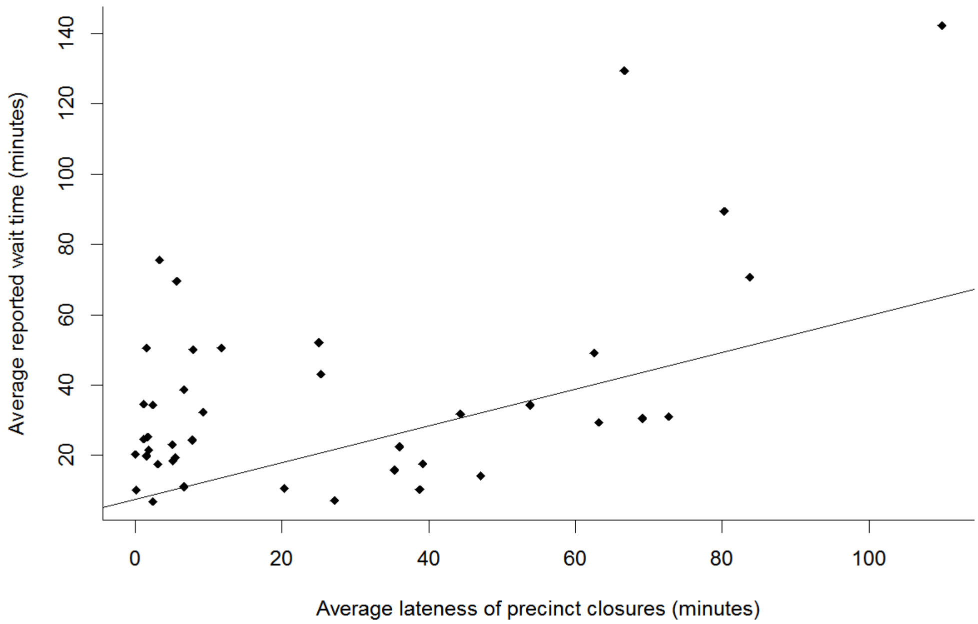
One potential concern with using survey data to estimate wait times is that respondents who wait in long lines might be more likely to recall how long they waited than those who only waited a few minutes. Psychology research (Allan 1979) has shown that the relationship between an individual's perception of time and actual time is linear. This implies that people who reported waiting longer in line did actually wait longer in line.

In the context of this paper, for recall bias to impact the results, it must be the case that the bias is operating differently for white and non-white voters. There is no clear reason to believe that non-white voters who wait in a long line are more (or less) likely to report doing so than white voters who wait in a long line.

Another potential source of bias is from media reports about long lines at minority precincts predisposing minority voters to report longer lines. I subset the data to only include respondents with a verified record of voting, so for media effects to drive my conclusions they would have to trump the impact that a voter's actual voting experience has on their survey response. The racial gap is so large that even a modest amount of bias is unlikely to account for the entirety of the gap.

Although most counties and towns do not collect 'objective' measures of line length in their precincts, there are a handful that do in the form of closing times of precincts. Because anybody in line at the end of the designated closing time is allowed to cast a ballot, the delay between the designated and true closing times provides a proxy for the line length at the precinct. Using data from 41 counties in three states in 2012 (Famighetti, Melilli, and Pérez 2014), I regressed the county average precinct closing delay on county average wait time–from the CCES responses–I find a positive and statistically significant relationship ($\beta =$

Figure A.1: How much do objective and survey-based measures of wait times correlate?



0.524 (0.1328); $p < 0.001$). Figure A.1 shows a scatterplot of this relationship. In the absence of nation-wide objective measures of line length, survey responses provide a reliable measure.

Average wait time by race in various elections

Figure A.2: Average Wait Time in November 2006 Election

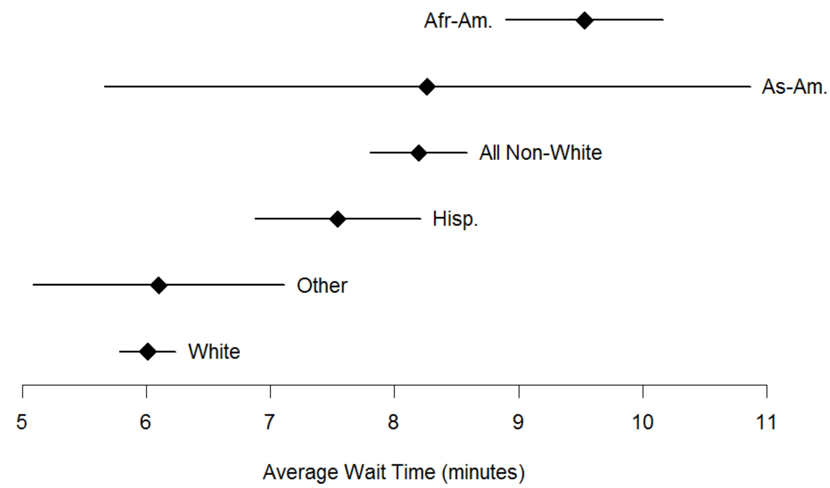


Figure A.3: Average Wait Time in November 2008 Election

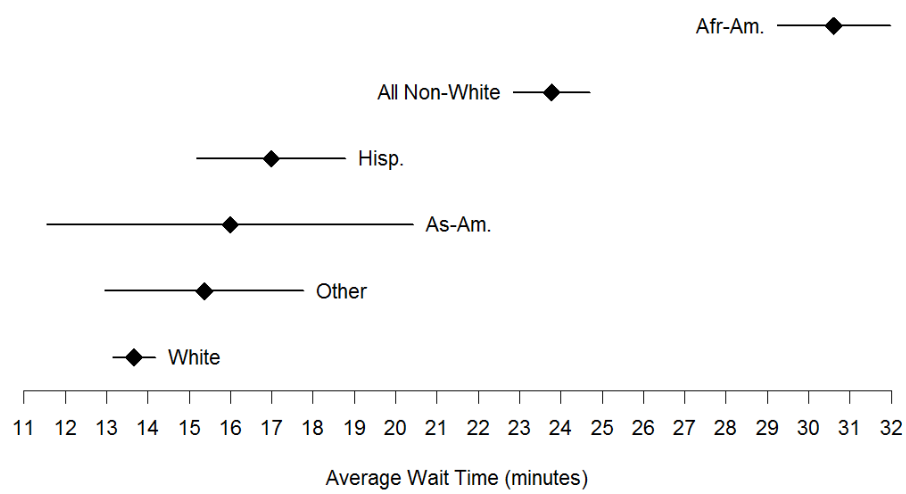
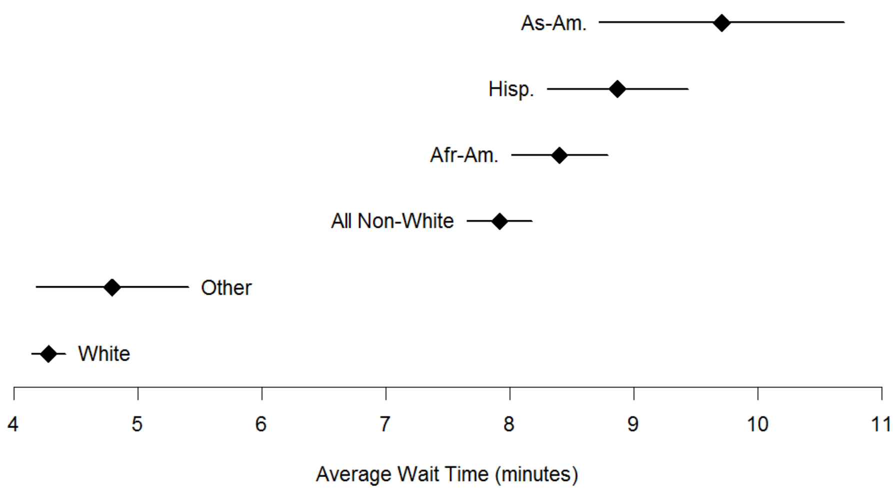


Figure A.4: Average Wait Time in November 2014 Election



Distribution of wait times by race in various elections

Figure A.5: Distribution of Wait Times for White and African-Americans in Nov. 2006 Election

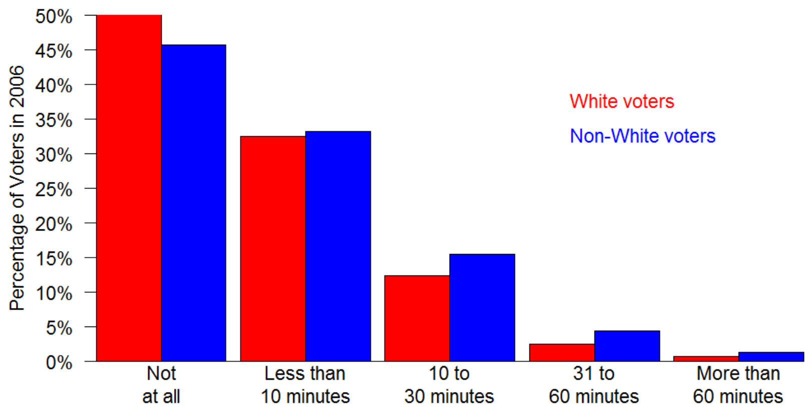


Figure A.6: Distribution of Wait Times for White and African-Americans in Nov. 2008 Election

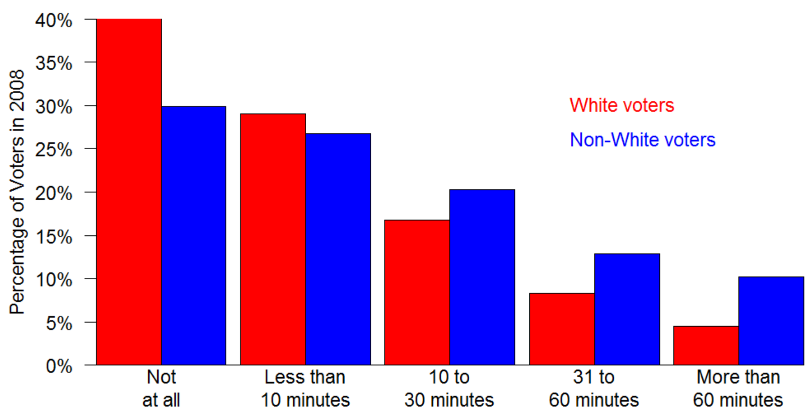
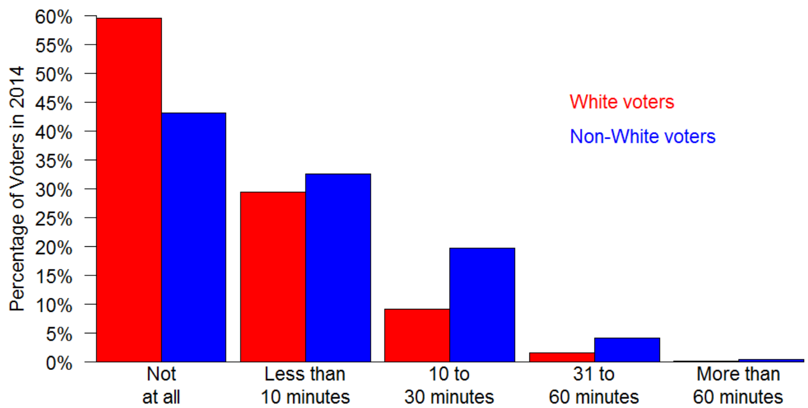
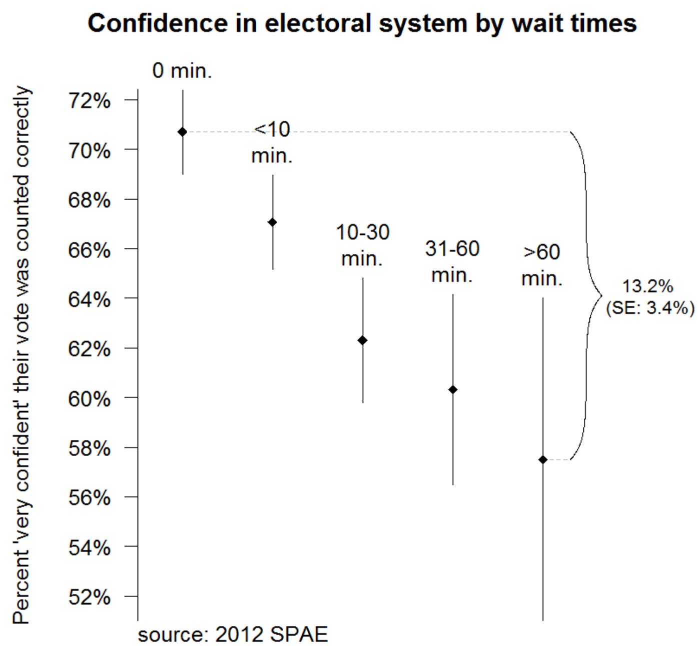


Figure A.7: Distribution of Wait Times for White and African-Americans in Nov. 2014 Election



B | Appendix to *The Downstream Consequences of Long Waits*

Figure B.1: Voter confidence in the electoral system, by 2012 wait time



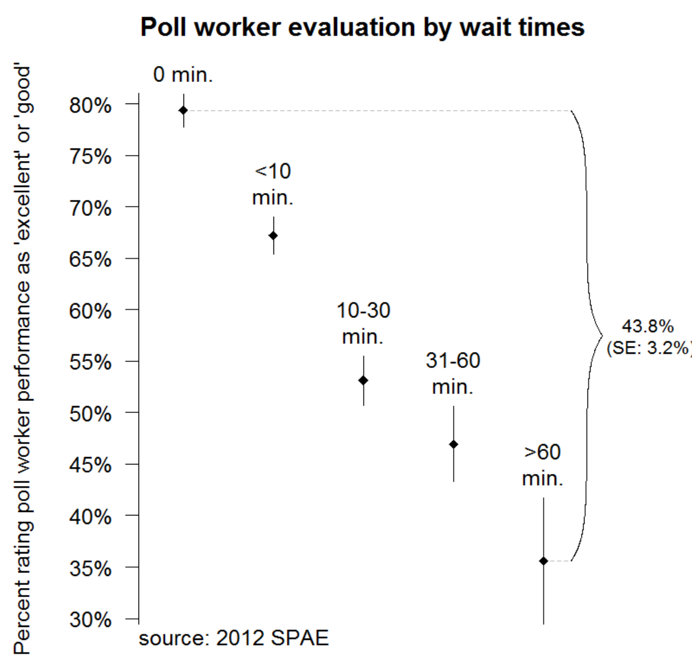


Figure B.2: Map of ZIP codes, colored by whether or not the CCES included data for calculating the average wait time

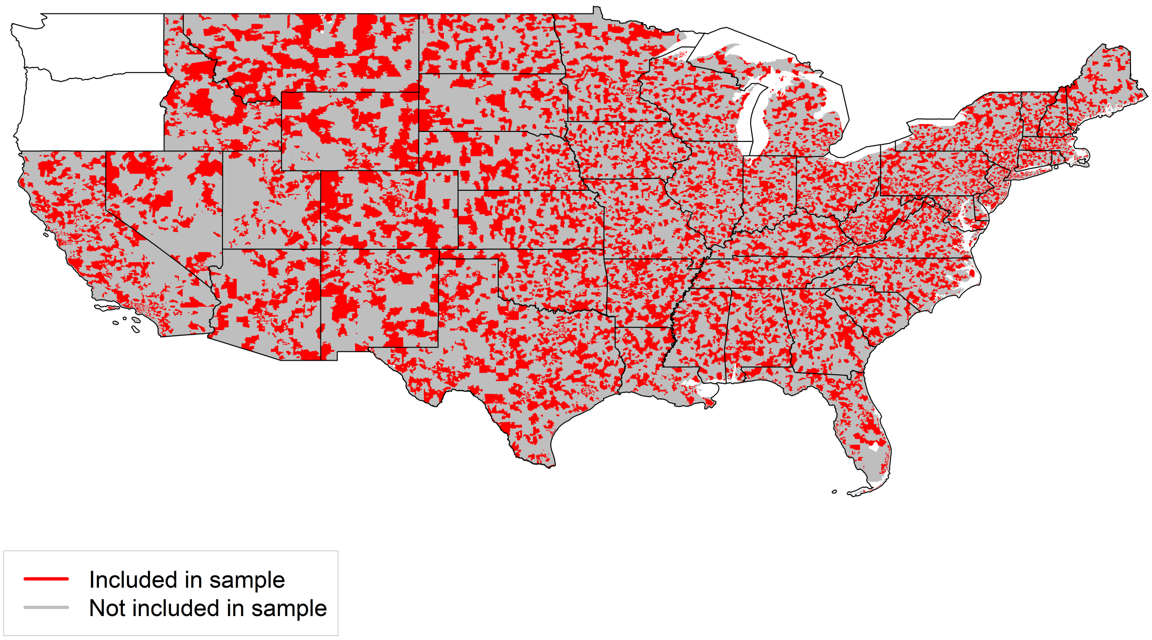


Figure B.3: Similarities of line length experience within various geographic units

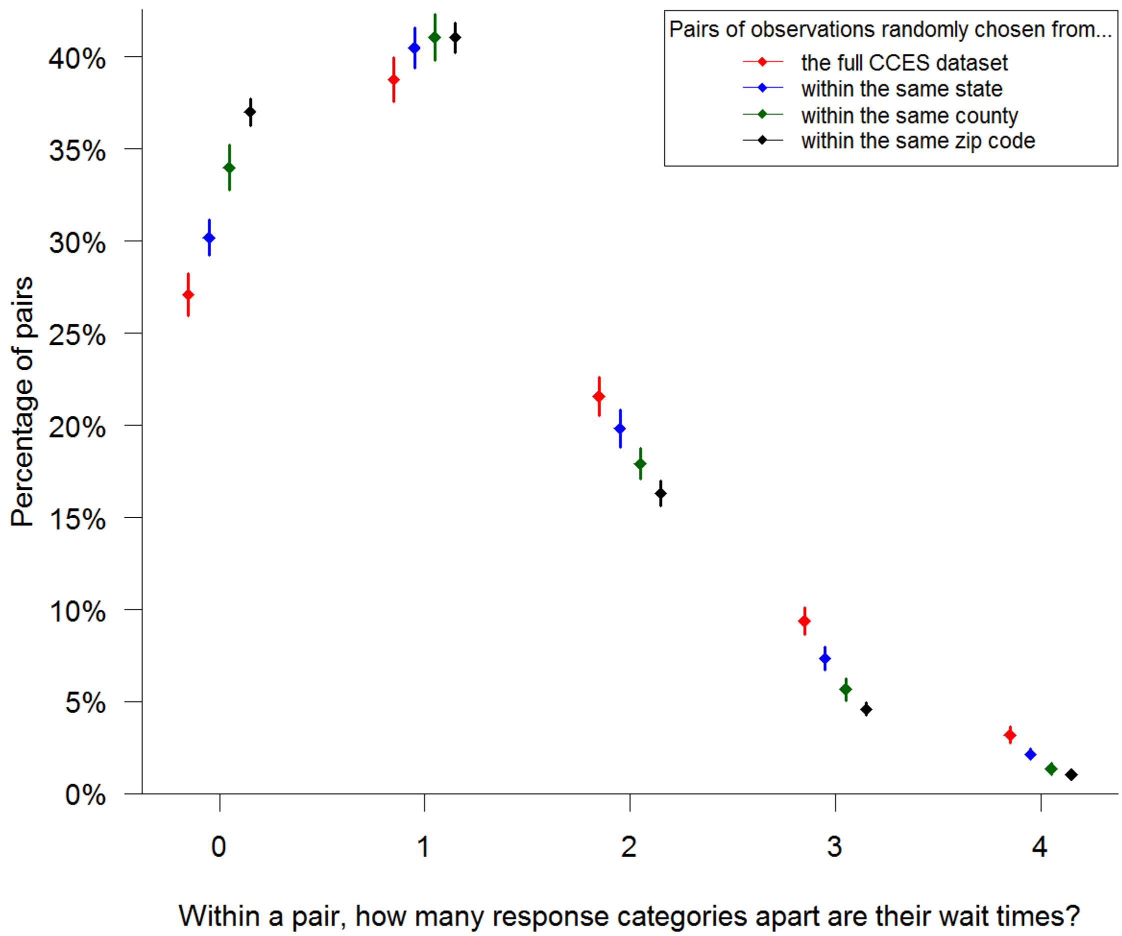
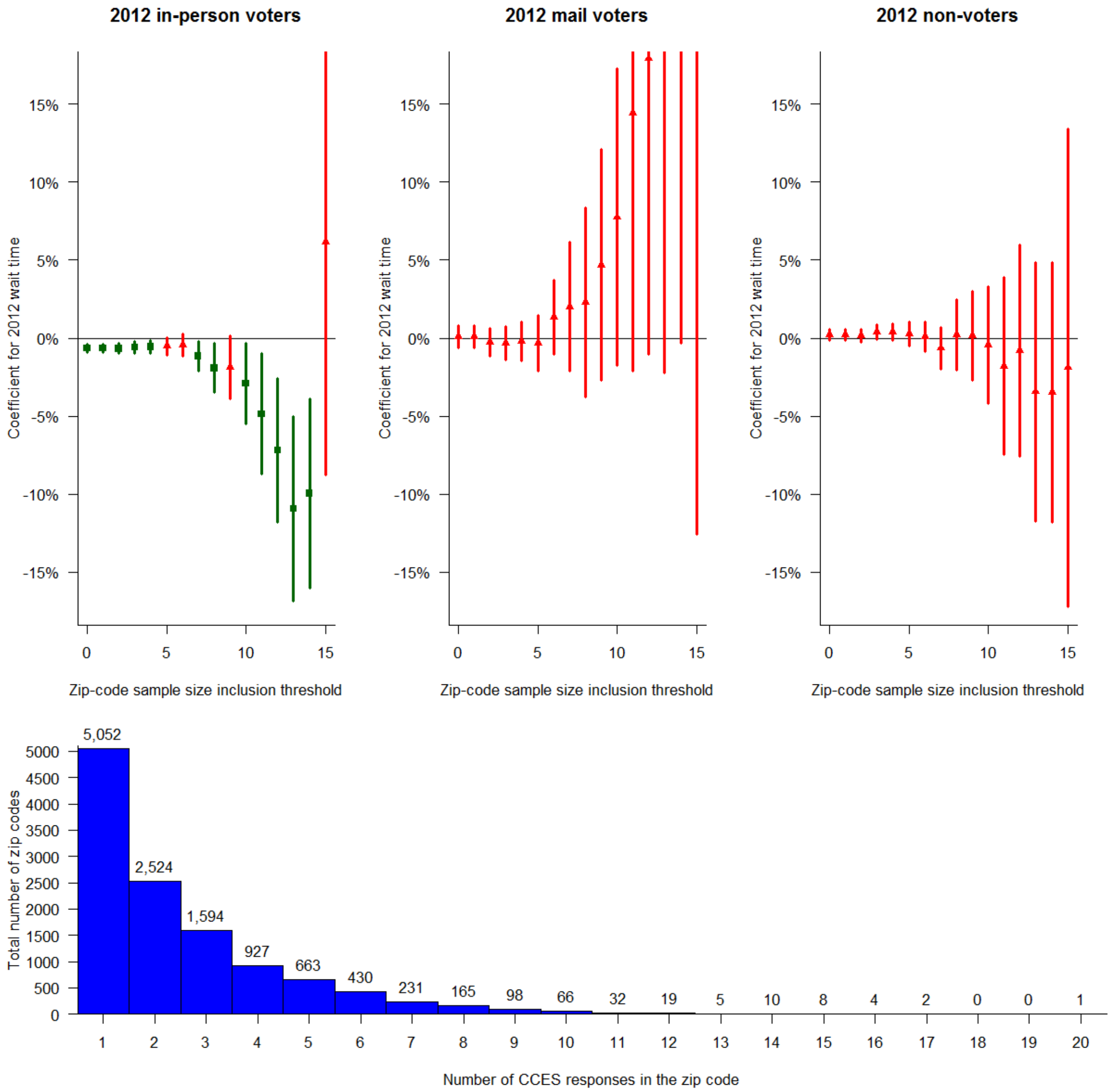


Figure B.4: Relationship between 2012 wait time and 2014 turnout, based on sample size thresholds that dictate whether a ZIP code is included in the analysis



Note: 95% confidence intervals reported. Green intervals are statistically significant ($p < 0.05$); red intervals are not.

Table B.1: How did lines in 2012 impact the turnout of voters in 2014?

	In-person	Mail	Non-voters
	(1)	(2)	(3)
Intercept	0.2123^{***}	0.2116^{***}	0.2068^{***}
	(0.0233)	(0.0423)	(0.0258)
2012 wait (hrs.)	-0.0063^{**}	0.0008	0.0020
	(0.0021)	(0.0034)	(0.0018)
Afr.-Am.	-0.0109^{***}	-0.0046	-0.0084^{***}
	(0.0021)	(0.0048)	(0.0019)
Hispanic	-0.0639^{***}	-0.0492^{***}	-0.0232^{***}
	(0.0023)	(0.0042)	(0.0019)
Other race	-0.0698^{***}	-0.0248^{***}	-0.0263^{***}
	(0.0033)	(0.0052)	(0.0025)
2006 turnout	0.1725^{***}	0.1552^{***}	0.0588^{***}
	(0.0014)	(0.0030)	(0.0021)
2008 turnout	0.0024	-0.0096^{*}	-0.0147^{***}
	(0.0016)	(0.0038)	(0.0013)
2010 turnout	0.2862^{***}	0.2649^{***}	0.1482^{***}
	(0.0014)	(0.0031)	(0.0027)
Age	0.0030^{***}	0.0032^{***}	0.0009^{***}
	(0.00004)	(0.0001)	(0.00004)
College educated	0.0002^{***}	0.0002^{***}	0.0003^{***}
	(0.00001)	(0.00002)	(0.00001)
White pct.	0.0030	-0.0146	0.0075^{*}
	(0.0039)	(0.0086)	(0.0037)
Pop. dens. (logged)	-0.0050^{***}	-0.0027^{*}	-0.0026^{***}
	(0.0005)	(0.0011)	(0.0006)
Non-Eng. speaking pct.	-0.0473^{***}	-0.0663^{***}	-0.0258^{***}
	(0.0059)	(0.0120)	(0.0052)
Med. inc. (logged)	0.0104^{***}	0.0087^{*}	-0.0010
	(0.0018)	(0.0035)	(0.0018)
Observations	774,836	166,885	373,595

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
Linear probability model coefficients reported
Controls and state fixed effects included

Table B.2: How did lines in 2012 impact the turnout of voters in 2014?

	In-person	Mail	Non-voters
	(1)	(2)	(3)
Intercept	0.2105***	0.2080***	0.2061***
	(0.0232)	(0.0423)	(0.0258)
2012 wait (hrs.)	−0.0154***	−0.0119	−0.0011
	(0.0031)	(0.0080)	(0.0025)
2012 wait (hrs.)^2	0.0035***	0.0048	0.0011
	(0.0010)	(0.0026)	(0.0006)
Afr.-Am.	−0.0108***	−0.0044	−0.0083***
	(0.0021)	(0.0048)	(0.0019)
Hispanic	−0.0640***	−0.0492***	−0.0232***
	(0.0023)	(0.0042)	(0.0019)
Other race	−0.0698***	−0.0249***	−0.0263***
	(0.0033)	(0.0052)	(0.0025)
2006 turnout	0.1725***	0.1551***	0.0587***
	(0.0014)	(0.0030)	(0.0021)
2008 turnout	0.0023	−0.0096*	−0.0147***
	(0.0016)	(0.0038)	(0.0013)
2010 turnout	0.2862***	0.2649***	0.1482***
	(0.0014)	(0.0031)	(0.0027)
Age	0.0030***	0.0032***	0.0009***
	(0.00004)	(0.0001)	(0.00004)
College educated	0.0002***	0.0002***	0.0003***
	(0.00001)	(0.00002)	(0.00001)
White pct.	0.0027	−0.0148	0.0074*
	(0.0039)	(0.0086)	(0.0037)
Pop. dens. (logged)	−0.0048***	−0.0025*	−0.0025***
	(0.0005)	(0.0011)	(0.0006)
Non-Eng. speaking pct.	−0.0481***	−0.0669***	−0.0261***
	(0.0059)	(0.0120)	(0.0052)
Med. inc. (logged)	0.0105***	0.0089*	−0.0010
	(0.0018)	(0.0035)	(0.0018)
Observations	774,836	166,885	373,595

*p<0.05; **p<0.01; ***p<0.001
Linear probability model coefficients reported
Controls and state fixed effects included

Table B.3: How did lines in 2012 impact the turnout of voters in 2014? (logit regression)

	In-person	Mail	Non-voters
	(1)	(2)	(3)
Intercept	-1.2485^{***}	-1.5223^{***}	-1.8708^{***}
	(0.1179)	(0.2498)	(0.1860)
2012 wait (hrs.)	-0.0349^{***}	0.0020	0.0219
	(0.0074)	(0.0160)	(0.0136)
Afr.-Am.	-0.0540^{***}	-0.0275	-0.0981^{***}
	(0.0099)	(0.0246)	(0.0206)
Hispanic	-0.3232^{***}	-0.2519^{***}	-0.2563^{***}
	(0.0112)	(0.0211)	(0.0199)
Other race	-0.3557^{***}	-0.1279^{***}	-0.2681^{***}
	(0.0156)	(0.0250)	(0.0247)
2006 turnout	0.8407^{***}	0.8120^{***}	0.4498^{***}
	(0.0062)	(0.0141)	(0.0152)
2008 turnout	-0.0026	-0.0952^{***}	-0.1423^{***}
	(0.0074)	(0.0171)	(0.0130)
2010 turnout	1.3027^{***}	1.2258^{***}	1.0530^{***}
	(0.0060)	(0.0142)	(0.0145)
Age	0.0156^{***}	0.0172^{***}	0.0092^{***}
	(0.0002)	(0.0004)	(0.0003)
College educated	0.0012^{***}	0.0008^{***}	0.0026^{***}
	(0.0001)	(0.0001)	(0.0001)
White pct.	0.0163	-0.0751	0.1081^{**}
	(0.0171)	(0.0427)	(0.0346)
Pop. dens. (logged)	-0.0256^{***}	-0.0147^{**}	-0.0289^{***}
	(0.0021)	(0.0049)	(0.0042)
Non-Eng. speaking pct.	-0.2419^{***}	-0.3428^{***}	-0.2635^{***}
	(0.0245)	(0.0561)	(0.0466)
Med. inc. (logged)	0.0583^{***}	0.0573^{**}	0.0052
	(0.0084)	(0.0182)	(0.0157)
Observations	774,836	166,885	373,595
Log Likelihood	$-427{,}451.9000$	$-86{,}732.5800$	$-124{,}708.6000$

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
Logit coefficients reported
State fixed effects included

Table B.4: Treatment/control group sizes

Control	Treatment 1	Treatment 2	Treatment 3	Treatment 4
0–15 minutes	15–30 minutes	30–45 minutes	45–60 minutes	more than 60 minutes
1,098,983	254,686	78,466	59,605	68,540

Table B.5: Effect of lines on turnout in matched dataset (2012 in-person voters only)

	(1)	(2)	(3)	(4)
Intercept	0.0479	0.1147	−0.0834	0.0838
	(0.0312)	(0.0612)	(0.0673)	(0.0787)
Long wait	−0.0076***	−0.0107**	−0.0116**	−0.0161**
	(0.0019)	(0.0037)	(0.0043)	(0.0049)
Afr.-Am.	−0.0156***	−0.0166*	0.0002	−0.0083
	(0.0046)	(0.0078)	(0.0090)	(0.0099)
Hispanic	−0.0550***	−0.0586***	−0.0479***	−0.0838***
	(0.0049)	(0.0086)	(0.0110)	(0.0120)
Other race	−0.0697***	−0.0780***	−0.0250	−0.0329
	(0.0094)	(0.0173)	(0.0256)	(0.0304)
2006 turnout	0.1808***	0.1774***	0.1728***	0.1606***
	(0.0026)	(0.0050)	(0.0060)	(0.0067)
2008 turnout	−0.0194***	−0.0123*	−0.0150*	0.0006
	(0.0032)	(0.0059)	(0.0069)	(0.0075)
2010 turnout	0.2867***	0.2864***	0.2973***	0.2845***
	(0.0027)	(0.0051)	(0.0061)	(0.0069)
Age	0.0034***	0.0033***	0.0032***	0.0033***
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
College educated	0.0002***	0.0003***	0.0002***	0.0002***
	(0.00002)	(0.00003)	(0.00004)	(0.00005)
White pct.	−0.0086	−0.0009	0.0139	0.0138
	(0.0071)	(0.0133)	(0.0145)	(0.0160)
Pop. dens. (logged)	−0.0062***	−0.0029	−0.0046*	−0.0083**
	(0.0008)	(0.0021)	(0.0022)	(0.0027)
Non-Eng. speaking pct.	−0.0469***	−0.0370*	−0.0538**	−0.0332
	(0.0090)	(0.0165)	(0.0172)	(0.0184)
Med. inc. (logged)	0.0102***	0.0039	0.0199**	0.0036
	(0.0030)	(0.0058)	(0.0062)	(0.0073)
'Treatment' group	15-30 min.	30-45 min.	45-60 min.	60+ min.
Observations (weighted)	111,623.7	29,765.9	21,352.8	18,186.4
Observations	196,128	53,049	38,363	30,200
R^2	0.2679	0.2781	0.2838	0.2829

*p<0.05; **p<0.01; ***p<0.001
OLS coefficients reported
State fixed effects included
Control group is always people where lines were between 0 and 15 minutes

Table B.6: Effect of lines on turnout in matched dataset (mail-in voters placebo tests)

	(1)	(2)	(3)	(4)
Intercept	0.2520*	0.2777	0.1906	0.5666***
	(0.1178)	(0.1618)	(0.1500)	(0.1720)
Long wait	−0.0081*	0.0169	0.0137	−0.0024
	(0.0038)	(0.0090)	(0.0092)	(0.0104)
Afr.-Am.	0.0116	0.0248	0.0757**	−0.0041
	(0.0103)	(0.0211)	(0.0283)	(0.0254)
Hispanic	−0.0507***	−0.0389	−0.0393	−0.0696*
	(0.0077)	(0.0219)	(0.0210)	(0.0271)
Other race	−0.0188	0.0409	0.0579*	−0.0057
	(0.0106)	(0.0317)	(0.0256)	(0.0406)
2006 turnout	0.1706***	0.1621***	0.1485***	0.1162***
	(0.0056)	(0.0133)	(0.0140)	(0.0150)
2008 turnout	−0.0168*	−0.0010	−0.0742***	0.0156
	(0.0080)	(0.0180)	(0.0188)	(0.0214)
2010 turnout	0.2822***	0.2314***	0.3123***	0.2598***
	(0.0067)	(0.0158)	(0.0160)	(0.0184)
Age	0.0036***	0.0032***	0.0029***	0.0024***
	(0.0001)	(0.0004)	(0.0003)	(0.0004)
College educated	0.0001**	0.00004	0.0001	0.0001
	(0.00004)	(0.0001)	(0.0001)	(0.0001)
White pct.	0.0060	0.0479	0.0277	−0.0570
	(0.0152)	(0.0359)	(0.0404)	(0.0382)
Pop. dens. (logged)	−0.0082***	0.0058	0.0069	−0.0028
	(0.0018)	(0.0061)	(0.0055)	(0.0061)
Non-Eng. speaking pct.	−0.0298	−0.0664	−0.0278	−0.0120
	(0.0195)	(0.0480)	(0.0490)	(0.0482)
Med. inc. (logged)	0.0094	−0.0105	0.0081	−0.0069
	(0.0061)	(0.0151)	(0.0145)	(0.0164)
'Treatment' group	15-30 min.	30-45 min.	45-60 min.	60+ min.
Observations (wtd.)	19,782.2	3,595	3,302.1	2,928.6
Observations	41,618	7,582	7,128	5,221
R^2	0.2418	0.2174	0.2162	0.1905

*p<0.05; **p<0.01; ***p<0.001
OLS coefficients reported
State fixed effects included
Control group is always people where lines were between 0 and 15 minutes

Table B.7: Effect of lines on turnout in matched dataset (non-voters placebo tests)

	(1)	(2)	(3)	(4)
Intercept	0.0235	0.0159	-0.0479	0.0071
	(0.0297)	(0.0573)	(0.0569)	(0.0681)
Long wait	-0.0023	-0.0014	-0.0016	0.0012
	(0.0019)	(0.0034)	(0.0038)	(0.0044)
Afr.-Am.	-0.0077	-0.0251^{***}	-0.0063	-0.0232^{**}
	(0.0040)	(0.0066)	(0.0071)	(0.0080)
Hispanic	-0.0192^{***}	-0.0219^{***}	-0.0201^{**}	-0.0304^{***}
	(0.0035)	(0.0060)	(0.0073)	(0.0081)
Other race	-0.0275^{***}	-0.0303^{**}	-0.0227	-0.0312^{*}
	(0.0051)	(0.0098)	(0.0124)	(0.0154)
2006 turnout	0.0589^{***}	0.0334^{***}	0.0620^{***}	0.0309^{*}
	(0.0050)	(0.0095)	(0.0107)	(0.0125)
2008 turnout	-0.0187^{***}	-0.0110^{*}	-0.0141^{**}	-0.0186^{**}
	(0.0026)	(0.0046)	(0.0052)	(0.0060)
2010 turnout	0.1396^{***}	0.1357^{***}	0.1310^{***}	0.1760^{***}
	(0.0054)	(0.0104)	(0.0113)	(0.0146)
Age	0.0010^{***}	0.0012^{***}	0.0011^{***}	0.0018^{***}
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
College educated	0.0003^{***}	0.0003^{***}	0.0002^{***}	0.0002^{***}
	(0.00002)	(0.00003)	(0.00004)	(0.00005)
White pct.	0.0096	-0.0077	0.0228	-0.0037
	(0.0063)	(0.0110)	(0.0118)	(0.0129)
Pop. dens. (logged)	-0.0019^{*}	-0.0023	0.0001	-0.0094^{***}
	(0.0008)	(0.0021)	(0.0020)	(0.0025)
Non-Eng. speaking pct.	-0.0112	-0.0253^{*}	-0.0330^{*}	-0.0193
	(0.0073)	(0.0128)	(0.0129)	(0.0147)
Med. inc. (logged)	-0.0017	0.0008	0.0021	0.0035
	(0.0028)	(0.0049)	(0.0051)	(0.0060)
'Treatment' group	15-30 min.	30-45 min.	45-60 min.	60+ min.
Observations (wtd.)	51,297.3	15,045.4	11,469.7	10,333.9
Observations	88,610	27,634	22,511	18,272
R^2	0.0368	0.0392	0.0360	0.0509

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
OLS coefficients reported
State fixed effects included
Control group is always people where lines were between 0 and 15 minutes

Table B.8: How did lines impact the mode of future voting among 2012 in-person voters?

	DV: Mode of Voting in 2014:	
	In-person	Mail
Intercept	-1.521^{***}	-4.453^{***}
	(0.121)	(0.307)
2012 wait (hrs.)	-0.044^{***}	0.097^{***}
	(0.008)	(0.017)
Afr.-Am.	-0.054^{***}	-0.088^{**}
	(0.010)	(0.033)
Hispanic	-0.318^{***}	-0.383^{***}
	(0.011)	(0.033)
Other race	-0.360^{***}	-0.308^{***}
	(0.016)	(0.045)
2006 turnout	0.845^{***}	0.758^{***}
	(0.006)	(0.019)
2008 turnout	0.012	-0.295^{***}
	(0.008)	(0.025)
2010 turnout	1.312^{***}	1.107^{***}
	(0.006)	(0.021)
Age	0.014^{***}	0.041^{***}
	(0.0002)	(0.001)
College educated	0.001^{***}	0.002^{***}
	(0.0001)	(0.0002)
White pct.	0.014	0.005
	(0.017)	(0.057)
Pop. dens. (logged)	-0.026^{***}	-0.025^{***}
	(0.002)	(0.007)
Non-Eng. speaking pct.	-0.236^{***}	-0.385^{***}
	(0.025)	(0.078)
Med. inc. (logged)	0.061^{***}	0.013
	(0.008)	(0.027)

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
Observations: 774,836
Multinominial logit coefficients from one model reported
DV reference category: Not voting in 2014
State fixed effects included

Table B.9: How did lines impact the mode of future voting among 2012 mail voters?

	DV: Mode of Voting in 2014:	
	In-person	Mail
Intercept	-1.084^{**}	-2.533^{***}
	(0.333)	(0.273)
2012 wait (hrs.)	0.027	-0.011
	(0.022)	(0.017)
Afr.-Am.	0.022	-0.057^{*}
	(0.034)	(0.026)
Hispanic	-0.176^{***}	-0.253^{***}
	(0.040)	(0.022)
Other race	-0.204^{***}	-0.106^{***}
	(0.047)	(0.026)
2006 turnout	0.762^{***}	0.824^{***}
	(0.021)	(0.015)
2008 turnout	0.080^{**}	-0.140^{***}
	(0.027)	(0.019)
2010 turnout	1.157^{***}	1.255^{***}
	(0.022)	(0.016)
Age	-0.003^{***}	0.024^{***}
	(0.001)	(0.0004)
College educated	0.001^{***}	0.001^{***}
	(0.0002)	(0.0001)
White pct.	-0.035	-0.100^{*}
	(0.058)	(0.046)
Pop. dens. (logged)	0.0005	-0.020^{***}
	(0.007)	(0.005)
Non-Eng. speaking pct.	-0.404^{***}	-0.348^{***}
	(0.089)	(0.060)
Med. inc. (logged)	0.014	0.063^{**}
	(0.027)	(0.019)

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
Observations: 166,885
Multinominial logit coefficients from one model reported
DV reference category: Not voting in 2014
State fixed effects included

Table B.10: How did lines impact the mode of future voting among 2012 nonvoters?

	DV: Mode of Voting in 2014:	
	In-person	Mail
Intercept	-2.266^{***}	-2.974^{***}
	(0.203)	(0.393)
2012 wait (hrs.)	-0.004	0.106
	(0.015)	(0.067)
Afr.-Am.	-0.094^{***}	-0.183^{***}
	(0.022)	(0.051)
Hispanic	-0.207^{***}	-0.370^{***}
	(0.022)	(0.038)
Other race	-0.323^{***}	-0.149^{***}
	(0.029)	(0.043)
2006 turnout	0.468^{***}	0.384^{***}
	(0.016)	(0.032)
2008 turnout	-0.083^{***}	-0.411^{***}
	(0.014)	(0.028)
2010 turnout	1.089^{***}	0.936^{***}
	(0.015)	(0.030)
Age	0.005^{***}	0.026^{***}
	(0.0003)	(0.001)
College educated	0.002^{***}	0.003^{***}
	(0.0001)	(0.0002)
White pct.	0.105^{**}	0.037
	(0.038)	(0.085)
Pop. dens. (logged)	-0.012^{**}	-0.114^{***}
	(0.005)	(0.009)
Non-Eng. speaking pct.	-0.131^{*}	-0.734^{***}
	(0.051)	(0.108)
Med. inc. (logged)	0.019	-0.047
	(0.017)	(0.034)

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
Observations: 373,595
Multinominial logit coefficients from one model reported
DV reference category: Not voting in 2014
State fixed effects included

Table B.11: Effect of end-of-day lines in Boston on future turnout

	Dependent variable: Turnout change from 2012 to...			
	Nov. '14	Nov. '13	Sept. '13	Nov. '08
	(1)	(2)	(3)	(4)
Intercept	-0.4524^{***}	-0.4190^{***}	-0.2556^{**}	0.0457
	(0.0650)	(0.0977)	(0.0771)	(0.0688)
Closing delay (hrs.)	-0.0060^{**}	-0.0087^{*}	-0.0058^{*}	-0.0003
	(0.0023)	(0.0035)	(0.0027)	(0.0025)
Nov. '10 turnout	0.2103^{***}	0.3001^{***}	-0.0625	0.0570
	(0.0345)	(0.0518)	(0.0409)	(0.0365)
Pct. White	0.0771^{***}	0.1747^{***}	0.0724^{***}	-0.0262^{*}
	(0.0123)	(0.0186)	(0.0146)	(0.0131)
Median income (log)	0.0059	-0.0018	-0.0213^{**}	-0.0106
	(0.0067)	(0.0101)	(0.0080)	(0.0071)
Pct. under 18	0.0601	0.0909	-0.0266	-0.0014
	(0.0393)	(0.0591)	(0.0466)	(0.0416)
Pct. over 65	0.0984^{*}	0.1173	0.0926	0.0026
	(0.0406)	(0.0611)	(0.0482)	(0.0430)
Pct. college grad	-0.0102	-0.2009^{***}	-0.0464^{*}	0.0394^{*}
	(0.0158)	(0.0237)	(0.0187)	(0.0167)
Observations	245	245	245	245
R^2	0.6540	0.6175	0.2134	0.0362

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
OLS coefficients reported

Table B.12: Impact of 2012 wait on future turnout in Florida

	Nov. 2014	Aug. 2014	Nov. 2008
	(1)	(2)	(3)
Intercept	0.3428***	−0.0816***	0.3191***
	(0.0016)	(0.0012)	(0.0015)
Closing delay (hrs.)	−0.0046***	−0.0003	−0.0004
	(0.0004)	(0.0003)	(0.0003)
Pct. female	−0.0330***	−0.0078***	0.0281***
	(0.0005)	(0.0004)	(0.0005)
Pct. Afr.-Am.	−0.0952***	−0.0300***	−0.0497***
	(0.0023)	(0.0017)	(0.0021)
Pct. Hispanic	−0.0182***	0.0347***	0.0229***
	(0.0009)	(0.0006)	(0.0008)
Pct. other race	−0.0987***	−0.0273***	−0.0305***
	(0.0008)	(0.0006)	(0.0008)
Age	0.0016***	0.0026***	0.0028***
	(0.00002)	(0.00001)	(0.00002)
Pct. Democrat	0.0236***	0.0659***	0.0675***
	(0.0007)	(0.0005)	(0.0007)
Pct. Republican	0.0326***	0.0749***	0.0288***
	(0.0007)	(0.0005)	(0.0007)
2010 turnout	0.3186***	0.1508***	0.3135***
	(0.0006)	(0.0004)	(0.0005)
Observations (weighted)	3,334	3,334	3,334
Observations (unweighted)	3,012,356	3,012,356	3,012,356
R^2	0.1520	0.1045	0.1608

*p<0.05; **p<0.01; ***p<0.001
County fixed effects included
WLS coefficients reported

C | Appendix to *Moved Out, Moved On*

Table C.1: What predicts cancellation of voter registrations due to mobility?

	2007-2008	2007-2008	2009-2010	2009-2010
	(1)	(2)	(3)	(4)
Intercept	-0.095^{***}	-0.052^{*}	-0.080^{***}	-0.042^{*}
	(0.027)	(0.022)	(0.023)	(0.019)
Mobility rate	-0.001	0.078^{***}	0.058^{***}	0.119^{***}
	(0.013)	(0.010)	(0.012)	(0.009)
Bush 2004 vote	-0.019^{***}	-0.005	-0.017^{***}	-0.002
	(0.005)	(0.004)	(0.004)	(0.003)
Pct Over 65	-0.001	-0.001	0.032^{*}	0.010
	(0.014)	(0.011)	(0.013)	(0.010)
log(Median Income)	0.013^{***}	0.005^{*}	0.011^{***}	0.004^{*}
	(0.003)	(0.002)	(0.002)	(0.002)
Pct White	0.0003	0.004	-0.017^{***}	-0.002
	(0.004)	(0.004)	(0.003)	(0.003)
log(Pop. Dens.)	-0.002^{***}	-0.002^{***}	0.0002	-0.001^{***}
	(0.0004)	(0.0003)	(0.0003)	(0.0003)
State fixed effects		✓		✓
Observations	2,216	2,216	2,810	2,810
R^2	0.017	0.559	0.048	0.596

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
OLS coefficients reported (weighted by CVAP)

Figure C.1: Survey-estimated voter registration rate versus the official registration

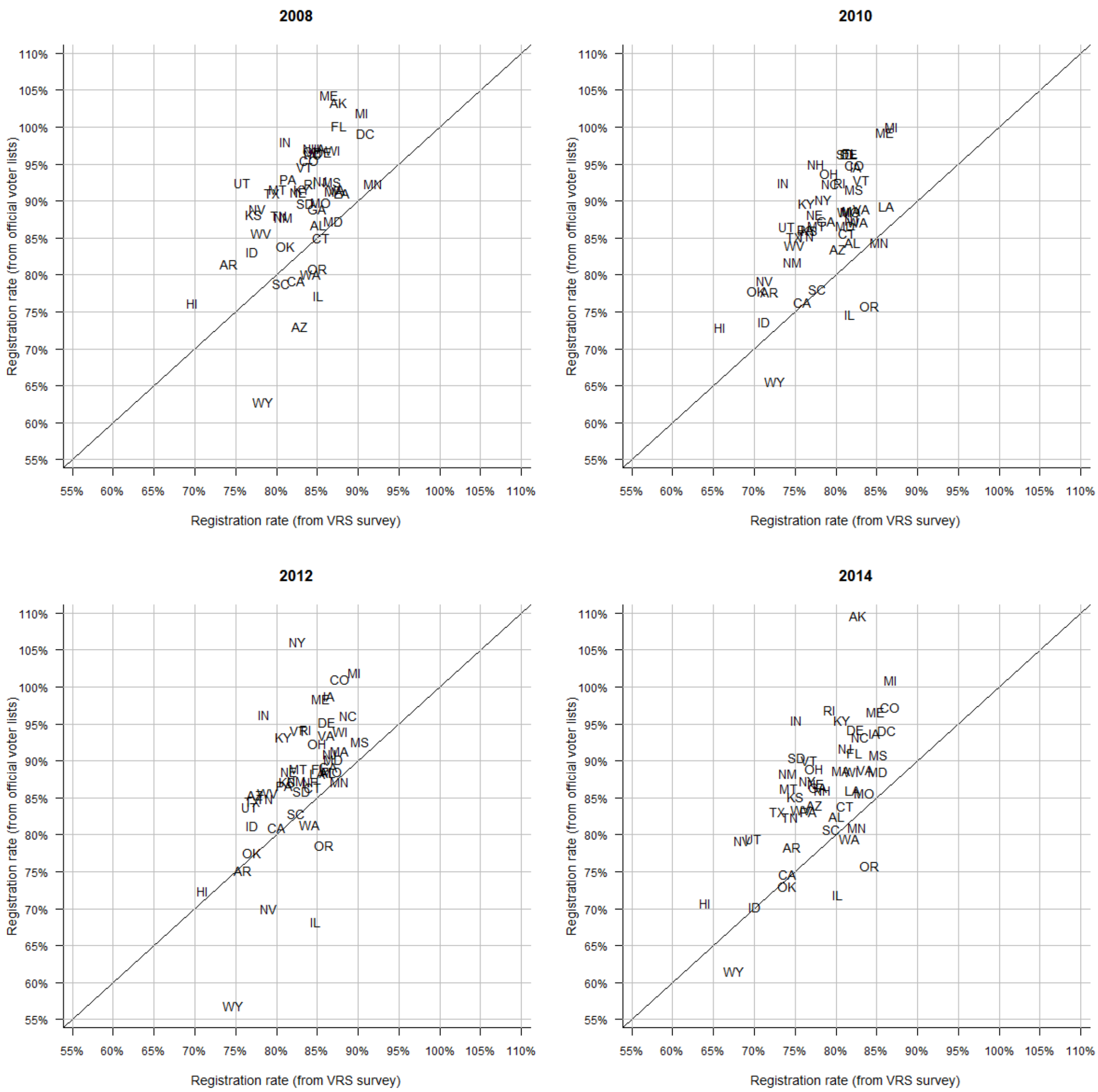


Figure C.2: Deaths and death removals by state

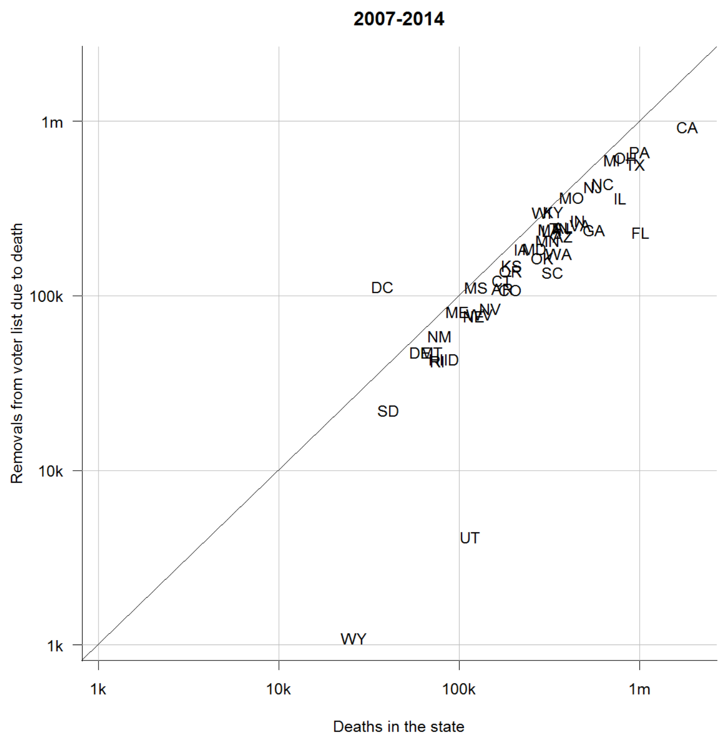
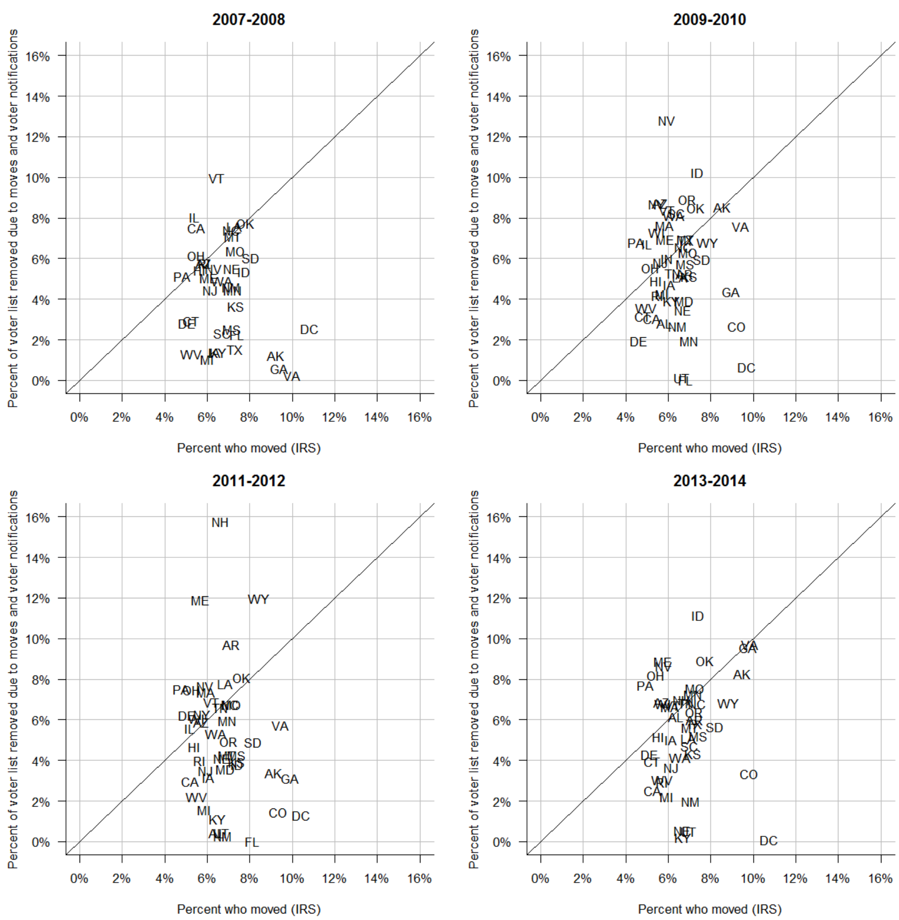


Figure C.3: Migration rate and voter removal rate due to mobility by state



Bibliography

Achen, Christohper, and Larry Bartels. 2016. *Democracy for Realists: Why Elections Do Not Produce Responsive Government*. Princeton University Press.

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

Allan, Lorraine G. 1979. "The Perception of Time." *Perception and Psychophysics* 26 (5): 340–354.

Alvarez, R. Michael, Thad E. Hall, and Morgan H. Llewellyn. 2008. "Are Americans Confident Their Ballots are Counted?" *Journal of Politics* 70 (3): 754–766.

Angrist, Joshua D, and Jorn-Steffen Pischke. 2008. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Ansolabehere, Stephen. 2009. "Effects of Identification Requirements on Voting: Evidence from the Experiences of Voters on Election Day." *PS: Political Science & Politics* 42 (1): 127–130.

Ansolabehere, Stephen, and Eitan Hersh. 2012. "Validation: What Big Data Reveal About Survey Misreporting and the Real Electorate." *Political Analysis* 20 (4): 437–459.

Ansolabehere, Stephen, and Eitan Hersh. 2013. "Voter registration: The process and quality of lists." In *The Measure of American Elections*, ed. Barry C. Burden and Charles Stewart III. New York: Cambridge University Press.

Ansolabehere, Stephen, Eitan Hersh, and Ken Shepsle. 2012. "Movers, Stayers, and Registration: Why Age is Correlated with Registration in the U.S." *Quarterly Journal of Political Science* 7 (1): 1–31.

Ansolabehere, Stephen, Sam Luks, and Brian F. Schaffner. 2015. "The perils of cherry picking low frequency events in large sample surveys." *Electoral Studies* 40: 409–410.

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.

Bartels, Larry. 2008. *Unequal Democracy: The Political Economy of the New Gilded Age*. Princeton University Press.

Berger, Jonah, Marc Meredith, and S. Christian Wheeler. 2008. "Contextual Priming: Where People Vote Affects How They Vote." *Proceedings of the National Academy of Sciences* 105 (26): 8846–8849.

Brady, Henry E., and John E. McNulty. 2011. "Turning Out to Vote: The Costs of Finding and Getting to the Polling Place." *American Political Science Review* 105 (1): 115–134.

CCES. 2007. "2006 Cooperative Congressional Election Study." Data available at: `http://hdl.handle.net/1902.1/14002`.

CCES. 2009. "2008 Cooperative Congressional Election Study." Data available at: `http://hdl.handle.net/1902.1/14003`.

CCES. 2013. "2012 Cooperative Congressional Election Study." Data available at: `http://hdl.handle.net/1902.1/21447`.

CCES. 2014. "2013 Cooperative Congressional Election Study-MIT module.".

CCES. 2015. "2014 Cooperative Congressional Election Study.".

de Kadt, Daniel. 2016. "Voting Then, Voting Now: The Long Term Consequences of Participation in South Africa's First Democratic Election." Working paper.

Downs, Anthony. 1957. *An Economic Theory of Democracy.* New York: Harper and Row.

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

Edelstein, William A., and Arthur D. Edelstein. 2010. "Queueing and Elections: Long Lines, DREs and Paper Ballots." Proceedings of the 2010 Electronic Voting Technology Workshop.

Famighetti, Christopher, Amanda Melilli, and Myrna Pérez. 2014. "Election Day Long Lines: Resource Allocation." *Brennan Center for Justice*.

Fraga, Bernard. 2016. "Redistricting and the Causal Impact of Race on Voter Turnout." *Journal of Politics* 78 (1): 19–34.

Frederickson, H. George. 2010. *Social Equity and Public Administration*. ME Sharpe.

Gerber, Alan, Donald Green, and Christopher Larimer. 2008. "Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment." *American Political Science Review* 102 (1): 33–48.

Gerber, Alan S., Donald P. Green, and Ron Shachar. 2003a. "Voting May be Habit-Forming: Evidence from a Randomized Field Experiment." *American Journal of Political Science* 47 (3): 540-550.

Gerber, Alan S., Donald P. Green, and Ron Shachar. 2003b. "Voting May be Habit-Forming: Evidence from a Randomized Field Experiment." *American Journal of Political Science* 47 (3): 540–550.

Gerber, Elisabeth, and Rebecca Morton. 1998. "Primary Election Systems and Representation." *Journal of Law, Economics, & Organization* 14 (2): 304–324.

Gerken, Heather. 2009. *The Democracy Index*. Princeton University Press.

Gibson, Chris L., Samuel Walker, Wesley G. Jennings, and J. Mitchell Miller. 2010. "The Impact of Traffic Stops on Calling the Police for Help." *Criminal Justice Policy Review* 21 (2): 139–159.

Gimpel, James G., and Jason E. Schuknecht. 2003. "Political Participation and the Accessibility of the Ballot Box." *Political Geography* 22 (22): 471âĂŞ488.

Gimpel, James, Joshua Dyck, and Daron Shaw. 2006. "Location, Knowledge, and Time Pressures in the Spatial Structure of Convenience Voting." *Electoral Studies* 25 (1): 35–58.

Graham, David A. 2013. "Here's Why Black People Have to Wait Twice as Long to Vote as Whites." *The Atlantic*. April 8. `http://www.theatlantic.com/politics/archive/2013/04/heres-why-black-people-have-to-wait-twice-as-long-to-vote-as-whites/274791/`.

Green, Donald P., Alan S. Gerber, and David W. Nickerson. 2003. "Getting Out the Vote in Local Elections: Results from Six Door-to-Door Canvassing Experiments." *Journal of Politics* 65 (4): 1083–1096.

Gross, Donald, John F. Shortle, James M. Thompson, and Carl M. Harris. 2008. *Fundamentals of Queueing Theory*. John Wiley & Sons.

Hamlin, Alan, and Colin Jennings. 2011. "Expressive Political Behaviour: Foundations, Scope and Implications." *British Journal of Political Science* 41 (3): 645–670.

Hasen, Richard L. 2012. *The Voting Wars: From Florida 2000 to the Next Election Meltdown*. Yale University Press.

Hillman, Arye. 1994. "Voting as Investment vs. Voting as Consumption: New Evidence." *Kyklos: International Review for Social Sciences* 47 (2): 197–207.

Hillman, Arye. 2010. "Expressive Behavior in Economics and Politics." *European Journal of Political Economy* 26 (4): 403–418.

Hood, M.V., and Charles Bullock. 2008. "Worth a Thousand Words? An Analysis of Georgia's Voter Identification Statutue." *American Politics Research* 36 (4): 555–579.

Hur, Aram, and Christopher H. Achen. 2013. "Coding Voting Turnout Responses in the Current Population Survey." *Public Opinion Quarterly* 77 (4): 985–993.

Iacus, Stefano, Gary King, and Giuseppe Porro. 2011a. "Causal Inference without Balance Checking: Coarsened Exact Matching." *Political Analysis*.

Iacus, Stefano, Gary King, and Giuseppe Porro. 2011b. "Multivariate Matching Methods that are Monotonic Imbalance Bounding." *Journal of the American Statistical Association* 493 (106): 345–361.

Jackman, Simon, and Bradley Spahn. 2016. "Unlisted in America." Working paper.

Jones, Bryan D, Saadia R Greenberg, Clifford Kaufman, and Joseph Drew. 1978. "Service Delivery Rules and the Distribution of Local Government Services: Three Detroit Bureaucracies." *The Journal of Politics* 40 (02): 332-368.

Kaufmann, Karen, James Gimpel, and Adam Hoffman. 2003. "A Promise Fulfilled? Open Primaries and Representation." *Journal of Politics* 65 (2): 457–476.

Kimball, David C. 2013. "Why are Voting Lines Longer for Urban Voters?" Presented at the 2013 Southwestern Social Science Association annual conference.

King, Bob. 2012. "Confusion Reigns Among Fla. Voters." *Politico*. November 6. `http://www.politico.com/story/2012/11/confusion-reigns-among-fla-voters-083401`.

Leighley, Jan, and Jonathan Nagler. 2013. *Who Votes Now?: Democratphics, Issues, Inequality and Turnout in the United States*. Princeton University Press.

Levitt, Justin. 2007. "The Truth About Voter Fraud." *Brennan Center for Justice*.

McDonald, Michael. 2016. "United States Elections Project." `http://www.electproject.org/home`.

McDonald, Michael P., and Samuel L. Popkin. 2001. "The Myth of the Vanishing Voter." *American Political Science Review* (4): 963–974.

McNulty, John E., Conor M. Dowling, and Margaret H. Ariotti. 2009. "Driving Saints to Sin: How Increasing the Difficulty of Voting Dissuades Even the Most Motivated Voters." *Political Analysis* 17 (4): 435–455.

Meredith, Marc. 2009. "Persistence in Political Participation." *Quarterly Journal of Political Science* 4 (3): 187–209.

Meredith, Marc, and Michael Morse. 2015. "The politics of the restoration of ex-felon voting rights: The case of Iowa." *Quarterly Journal of Political Science* 10 (1): 41–100.

Mladenka, Kenneth R. 1980. "The Urban Bureaucracy and the Chicago Political Machine: Who Gets What and the Limits to Political Control." *American Political Science Review* 74 (04): 991–998.

Norden, Lawrence, and Christopher Famighetti. 2015. "America's Voting Machines at Risk." *Brennan Center for Justice*.

Nyhan, Brendan, Christopher Skovron, and Rocio Titiunik. 2016. "Differential Registration Bias in Voter File Data: A Sensitivity Analysis Approach." *American Journal of Political Science* Forthcoming.

Peters, Jeremy W. 2013. "Waiting Times at Ballot Boxes Draw Scrutiny." *New York Times*. February 4. `http://www.nytimes.com/2013/02/05/us/politics/waiting-times-to-vote-at-polls-draw-scrutiny.htmll`.

Pettigrew, Stephen. 2015. "Inferring Distributional Attributes from Data with Heaping.".

Pettigrew, Stephen. 2017a. "The Race Gap in Precinct Wait Times: Why Minority Precincts are Underserved by Local Election Officials." *Political Science Quarterly* Forthcoming.

Pettigrew, Stephen. 2017b. "Replication data for: Long Lines and Voter Purges: The Logistics of Running Elections in America." `http://dx.doi.org/10.7910/DVN/2DADXK`.

Pettigrew, Stephen, and Charles Stewart. 2016. "A Population Model of Registration and Deadwood." Working paper.

Pew Center for the States. 2014. "Elections Performance Index Methodology." `http://www.pewtrusts.org/~/media/Assets/2014/04/07/EPI_methodology.pdf`.

Prior, Markus. 2010. "You've Either Got It or You Don't? The Stability of Political Interest over the Life Cycle." *Journal of Politics* 72 (3): 747-766.

Richman, Jesse T., Gulshan A. Chattha, , and David C. Earnest. 2014. "Do non-citizens vote in U.S. elections?" *Electoral Studies* 36: 149–157.

Riker, William, and Peter Ordeshook. 1968. "A Theory of the Calculus of Voting." *American Political Science Review* 62 (1): 25–42.

Rosenstone, Steve J., and John Mark Hansen. 2002. *Mobilization, Participation, and Democracy in America*. Pearson.

Rutchick, Abraham M. 2010. "Deus Ex Machina: The Influence of Polling Place on Voting Behavior." *Political Psychology* 31 (2): 209–225.

Ryan, Andrew. 2009. "Phantom precinct shows city's arcane voting laws." *Boston Globe*. November 3. `http://www.boston.com/news/local/massachusetts/articles/2009/11/03/boston_election_officials_rethink_the_phantom_precinct/`.

Shaw, Daron, Stephen Ansolabehere, and Charles Stewart III. 2015. "A Brief Yet Practical Guude to Reforming US Voter Registration Systems." *Election Law Journal* 14 (1): 26–31.

Stewart, Charles. 2013. "Waiting to Vote in 2012." *Journal of Law and Politics* 28.

Stewart, Charles. 2015. "Polling Place of the Future.".

Stewart, Charles, and Stephen Ansolabehere. 2013. "Waiting in Line to Vote." Caltech/MIT Voting Technology Project. `http://dspace.mit.edu/bitstream/handle/1721.1/96640/WP%20114.pdf?sequence=1`.

Tesler, Michael, and David O Sears. 2010. *Obama's Race: The 2008 Election and the Dream of a Post-Tacial America*. University of Chicago Press.

Tokaji, Daniel P. 2009. "The Future of Election Reform: From Rules to Institutions." *Yale Law & Policy Review*: 125-154.

U.S. Census Bureau. 2016. "Migration/Geographic Mobility." `https://www.census.gov/hhes/migration/data/acs/state-to-state.html`.

U.S. Department of Justice, Civil Rights Division. 2017. "The National Voter Registration Act of 1993 (NVRA): Q&A." `https://www.justice.gov/crt/national-voter-registration-act-1993-nvra`.

U.S. GAO. 2014. "Observations on Wait Times for Voters on Election Day 2012." Government Accountability Office-14-850. `http://www.gao.gov/assets/670/666252.pdf`.

Verba, Sidney, Kay Lehman Schlozman, and Henry E Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*. Vol. 4 Cambridge Univ Press.

Virginia Department of Elections. 2016. "Annual List Maintenance Report: July 1, 2015-June 30, 2016." http://www.elections.virginia.gov/Files/maintenance-reports/2016SBEListMaintenancereport.pdf.

Weaver, Vesla M., and Amy E. Lerman. 2010. "Political Consequences of the Carceral State." *American Political Science Review* 104 (4): 817–833.

Weaver, Vesla M., and Amy E. Lerman. 2014. "Staying Out of Sight? Concentrated Policing and Local Political Action." *Annals of the American Academy of Political and Social Science* 651 (1): 202–219.

White, Ariel. 2016. "Misdemeanor Disenfranchisement? The Demobilizing Effects of Brief Jail Spells on Potential Voters." Working paper.

Wolfinger, Raymond E., and Steven J. Rosenstone. 1980. *Who Votes?* New Haven: Yale University Press.