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

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Abstract

Political scientists have increasingly emphasized the role played by an individual's identity and life experiences in their patterns of political participation. In this paper, I explore how one particular type of experience–standing in line at a precinct to vote–shapes the turnout behavior of voters in future election. I demonstrate that for every additional hour a voter waits in line to vote, their probability of voting in the subsequent election drops by 1 percentage point. As a result, nearly 200,000 people did not vote in November 2014 because waiting in a long line in 2012 turned them off from the process. To arrive at these estimates, I analyze vote history files using a combination of exact matching and placebo tests to test the identification assumptions. I then leverage an unusual institutional arrangement in the City of Boston and longitudinal data from Florida to show that the result also holds at the precinct level. The findings in this paper have implications for our understanding of what motivates or demotivates a person from voting. They also suggest that racial asymmetries in precinct wait times are contributing to under-representation of racial minorities in the voter pool.[1]

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). One long-running debate has focused on whether voting behavior is best explained by a model of rational choice or by an individual's identity and social environment. Recent work has emphasized the potential role that a person's experiences can have in their political participation.

Although prior research (Gimpel, Dyck, and Shaw, 2006; Stein and Vonnahme, 2008; McNulty, Dowling, and Ariotti, 2009; Brady and McNulty, 2011) has focused on the effect

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[1]This is still a draft. Please do not circulate it without first checking with me.

that things like polling location and voter ID laws have on turnout, it has largely ignored the impact of the experience a voter has while inside their polling place. This paper focuses on one aspect of that experience–the length of time a person waits to cast their ballot–and shows that voters who have worse in-precinct experiences 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[2] then we might characterize the problem as a 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). Even more troubling, recent research finds that 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, 2016).

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 the creation of lines that 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.

To estimate the effect that waiting in a line has on future turnout, I employ three empirical strategies to show that long lines diminish turnout in by about one percentage point for every additional hour of waiting. Placebo tests throughout the paper indicate that this result only holds up for those who voted in-person in 2012 and not those who voted by mail or did not vote, suggesting that changes in future turnout are a consequence of the act of standing in line and providing support for the conditional ignorability assumption.

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

After developing my hypothesis in Section 2, I use a national sample of voter history data to estimate the turnout effect at the voter level. In Section 3.1, I show that a neighborhood's average wait time predicts November 2014 turnout for those who voted in-person in 2012, but that no such relationship exists for 2012 voters-by-mail and nonvoters. Exact matching, coupled with additional placebo tests in Section 3.2, deals with selection bias and provides strong evidence that lines depress turnout. In Section 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 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. 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."[3] 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 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.

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, 2016; 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, 2016) and may also effect turnout (Ansolabehere, 2009; Hood and Bullock, 2008).

Disentangling this confounding is difficult in the absence of a randomized experiment,

[3]See Figure A.7 in the appendix for the full results of these two analyses.

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 test this assumption with placebo tests using voters-by-mail and nonvoters, finding evidence to support the assumption. In the following subsection, I employ exact matching 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. Additional placebo tests quell concerns about the selection on observables assumption. Finally, before moving on to precinct-level tests of the hypothesis in the next section, I dissect the lines effect by looking at how they impact 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 files from the entire country (Ansolabehere and Hersh, 2012).[4] I subset the data to include only individuals who were registered to vote in the November 2012 election.[5] 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, and the voting habits of those who participate in midterms tend to be more stable than presidential voters. 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). Among those who voted in 2012, only 42.4% had voted in 2006, 2008, and 2010. Because of this stability in midterm voting habits, it is less likely that a voter's turnout decision in 2014 would be swayed by a long line in 2012.

Another possibility is that 2012 lines were the result of mobilization efforts and voter

[4] 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.

[5] 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 should mitigate this problem.

enthusiasm, which would disappear in the 2014 midterms. These explanations are based on the assumption that lines develop due to shocks to turnout. Prior research has shown, however, that poor resource optimization by election officials is the biggest contributor to lines, not turnout (Pettigrew, 2016). Also to the extent that mobilization efforts in 2012 were guided by demographics and prior participation habits, regression and matching will eliminate this bias. Each of these factors suggest that estimating the turnout effect of lines on 2014 turnout may yield a smaller result than if turnout in 2016 were instead used as the outcome.

Ideally, the treatment variable would be the amount of time each individual voter in the sample waited in 2012; unfortunately, this information is not collected.[6] Instead, 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, 2016; 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 A.10 in the appendix shows that the conclusions drawn do not change when choosing other thresholds.

There is also very little variation in line length within ZIP codes. When randomly selecting two CCES respondents from the same ZIP code, there is a 37% chance that they

[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 A.8 in the appendix shows a map of which ZIP codes were included and which were not.

gave identical answers to the line length question, and an 78% chance that they answers 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]

In addition, the unique properties of queues makes it more challenging to identify the effect. In a single precinct, you could have some people who waited no time at all and others who might wait a long time. Because the distribution of wait times is right (positively) skewed, the chances of a voter who waited long time having their wait time represented by a smaller ZIP code average wait is higher than a non-waiting voter having their wait time represented by a very large ZIP code average wait. This makes false negative results more likely (Imai and Yamamoto, 2010). Although average wait times within a voter's ZIP code is still a proxy for their actual experience, such measurement error should attenuate the magnitude of the effect, making it more difficult to identify (Draper and Smith, 1998).

3.1. Evidence from individual voter records

Table 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 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 model includes control variables for the voter's race, age and education, their turnout history

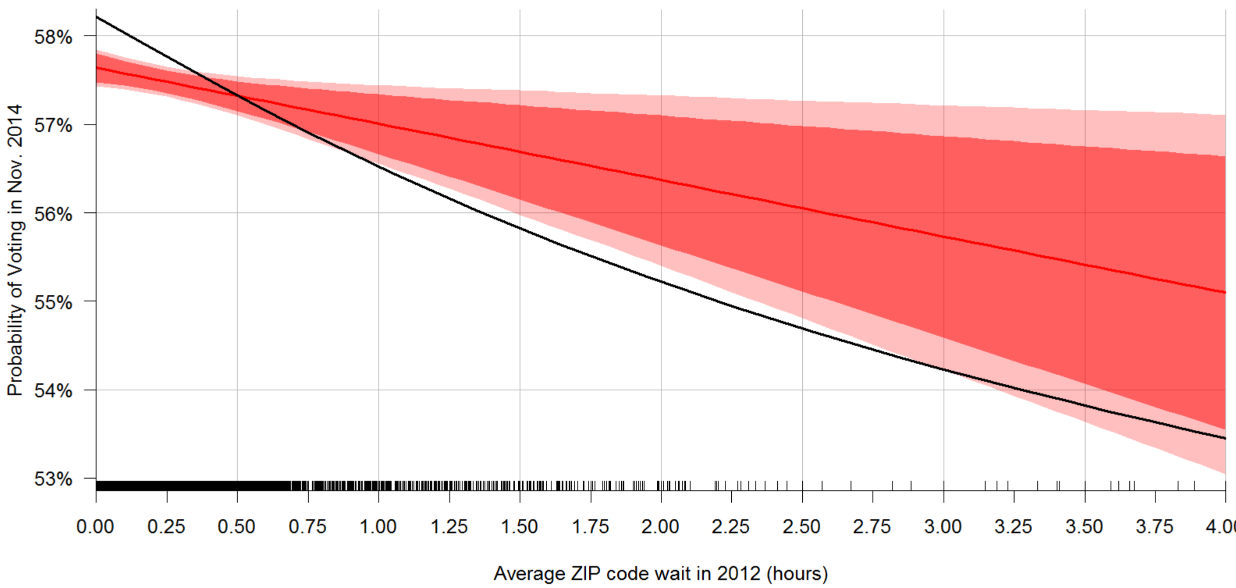
[9]See Figure A.9 in the appendix for additional analysis.

[10]The substantive results are the same when using logistic regression. Those results are reported in appendix Table A.8.

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

in 2006, 2008, and 2010,[12] and the population density, racial diversity, median income, and percent of non-English speakers in their Census block-group, as well as state fixed effects.[13]

Figure 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 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,

[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 A.6 in the appendix reports the full regression results with all controls.

[14] The results also hold when a quadratic term is included for the wait time variable. See Table A.7 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 1.

most ZIP codes had an average wait of less than one hour, yet 5.4 million (4.2%) of voters in 2012 lived in a ZIP code with a average wait of greater than 60 minutes (CCES, 2013).

Interpreting these results causally requires an assumption of no unmeasured confounding, which I test using placebo tests. Because the measure of 2012 line length is in terms of the average ZIP code wait, I can approximate how long mail-in absentee and non-voters would have waited if they had voted in-person. Using the same specification as Model 1 in Table 1, I estimate the relationship between hypothetical wait times and turnout among individuals for whom there should not be an effect. 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.[16] However this would bias the placebo tests away from a null result, thus making them tougher tests.

Models 2 and 3 in Table 1 show that the hypothesis stands up to these placebo tests: no significant relationship between 2012 wait time and 2014 turnout exists among those who did not experience a long line. What 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. If such confounding did exist, we should see significant results for the placebo tests, especially among mail-in voters who are more similar to in-person voters than non-voters (Barreto et al., 2006; Dubin and Kalsow, 1996). 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.

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). The best technique to ensure such balance is exact matching, which guarantees that the correlation between the confounders and the treatment will be precisely zero–providing unbiased estimates of treatment effects

[16]The findings in Spencer and Markovits (2010) suggest that the size of the latter group is very tiny.

(Angrist and Pischke, 2008).[17] Recent work has used matching and vote history data to estimate causal effects where turnout is the outcome of interest, most notably an article by Fraga (2016).

Matching requires clearly defined treated and control groups. Because the treatment of interest–line length–is continuous, I chose several different thresholds to bifurcate the line length variable. The thresholds I present below are 15 minutes, 30 minutes, 45 minutes, and 60 minutes.[18] These thresholds were chosen based on survey evidence about public opinion on line length. When asked how many minutes is an unreasonable amount of time to wait to vote, the national average falls between 30 and 60 minutes (SPAE, 2015).

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 could not employ traditional exact matching methods. Instead I used 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.[19]

I applied this matching model to the 2012 in-person voter sample from Catalist, as well as the 2012 mail-in and non-voters. The exact-matched variables have perfect balance between the treatment and control groups. The coarsened variables also have treatment and control group means that are statistically indistinguishable from each other.[20] In the results presented below I control for the continuous version of these covariates to improve efficiency.

Table 2 reports the post-matching estimates of the effect of a long 2012 wait on an

[17]Unlikely propensity score and other matching metrics, exact matching has the advantage of not require any functional form assumptions in the model.

[18]The smallest of the eight treatment-threshold groups has over 65,000 observations. The number of observations in the treated and control groups for each of these thresholds is reported in Appendix Table A.9.

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

[20]The appendix reports these results in more detail. The love plots in Appendix Figure A.11 present the z-score for the difference in means between treatment and control for each variable in the 2012 in-person sample. Appendix Figure A.12 presents the empirical difference in means and SE for each variable.

Table 2: Effect of lines on turnout in matched dataset (2012 in-person voters only)

	(1)	(2)	(3)	(4)
Long wait	-0.0086^{***}	-0.0079^{***}	-0.0066^{*}	-0.0077^{*}
	(0.0016)	(0.0022)	(0.0027)	(0.0035)
'Treatment' cutoff	15 min.	30 min.	45 min.	60 min.
Observations (weighted)	159,664.6	84,269.7	56,166.6	31,132.1

*p<0.05; **p<0.01; ***p<0.001
OLS coefficients reported
Controls and state fixed effects included

in-person voter's probability of turning out in November 2014.[21] Each column reports a separate estimate of the turnout effect, given different thresholds to define the 'long wait' treatment. In all four cases, in-person voters who lived in a ZIP code with an average wait time at or above the cutoff were approximately 1 percentage point less likely to vote in 2014 than those two lived in a neighborhood below the cutoff.[22]

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 0.8 percentage points less likely to vote in 2014 than their counterpart in a neighborhood with an average wait below an hour.

The four green bars in Figure 2 visualize the results in Table 2.[23] The red bars in the figure present the results from eight placebo tests of the effect of lines on people who did not go to their precinct in 2012.[24] 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

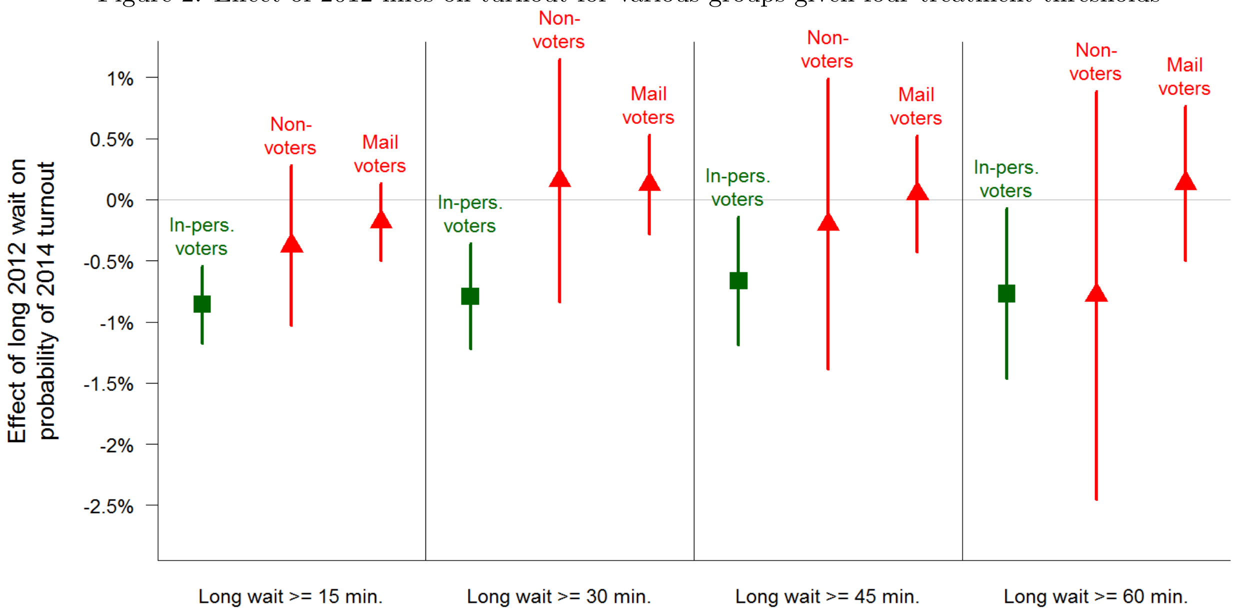
[21] ZIP code cluster-robust standard errors are reported. The full results, including control covariates, are presented in Appendix Table A.10.

[22] It is also worth noting that choosing other thresholds yields similar results–as the threshold increases beyond 60 minutes, the point estimate remain negative, although fewer observations cause the standard errors to grow quickly. As the threshold decreases below 15 minutes, the effect estimates unsurprisingly approach zero.

[23] The bars signify 95% confidence intervals.

[24] Appendix Tables A.11 and A.12 show the full results from these models.

Figure 2: Effect of 2012 lines on turnout for various groups given four treatment thresholds

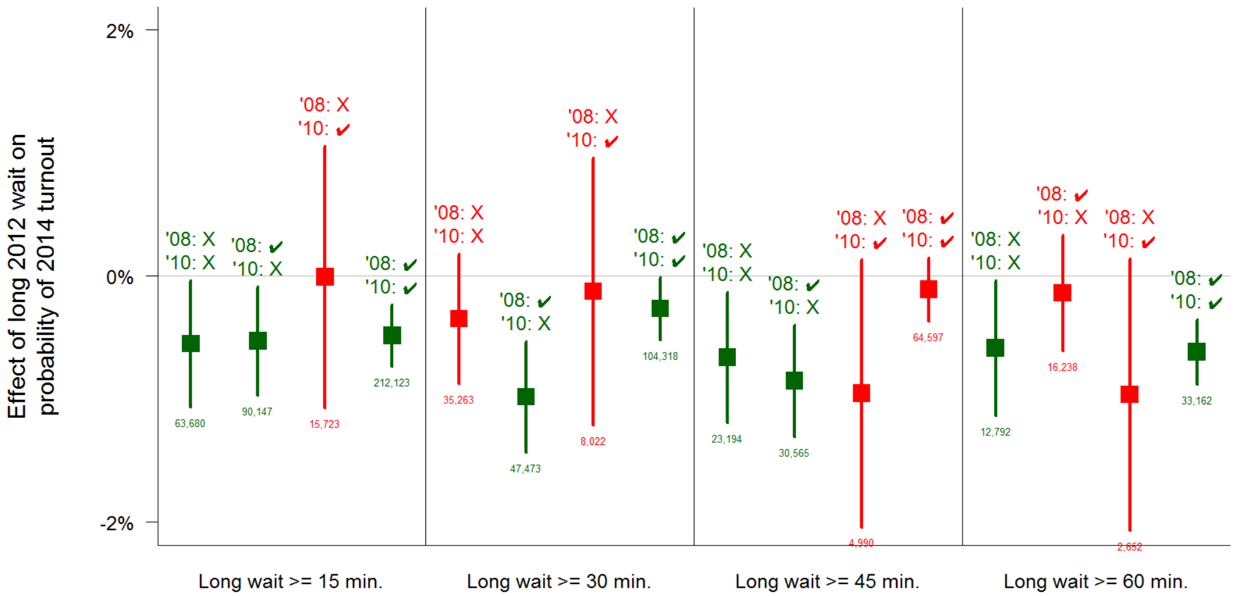


using the same model specification as Table 2. In every case, the results do not provide enough evidence to reject the null hypothesis at the conservative $p < 0.10$ level.

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 directly experience a long line.

One potential criticism of these results is that the findings are largely driven by turnout. 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 turnout effect among those who vote in virtually every election and a larger effect among those who voted in 2012 but do not typically participate (especially in midterms). Figure 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/or

Figure 3: The effect of a long wait for people who voted in 2012, given their turnout pattern in 2008 and 2010



2010.[25]

Figure 3 pushes against the argument that long lines are only depressing the turnout of voters who were not likely to have voted in 2014 in the first place. For nearly all four voting patterns in 2008 and 2010, the turnout effect is negative and significant. The results for those who are regular voters (voted in 2008 and 2010) and very similar to those who rarely participate (did not vote in 2008 and 2010). The results among those who voted in 2010, but did not vote in 2008, are not significant, but one explanation is that the samples in those regressions are substantially smaller than any of the other regressions because very few Americans (roughly 3.0% based on Catalist estimates) have this turnout pattern. Additionally, when we apply the same subgroup analysis approach to the placebo groups, we find non-significant results for 29 of the 32 combinations of turnout patterns and line length thresholds.[26] 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

[25]These results come from the matched dataset used in Table 2, which was subset based on 2008 and 2010 turnout patterns prior to estimating the coefficients.

[26]See Figure A.13 in the appendix for these results.

turnout. Rather, long lines at precincts appear to have a measurable effect on the future turnout patterns of voters.

3.3. Future absentee versus in-person voting

Before turning to an analysis of precinct data, I consider alternative ways in which lines might affect voter behavior. In particular, I explore the extent to which lines are motivating voters to shift toward absentee voting by mail, rather than simply not voting at all. The expectation, which follows from a rational choice explanation laid out above, is that areas with long lines should see upticks in the proportion of voters who shift from voting in-person in 2012 to by-mail in 2014.

Table 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

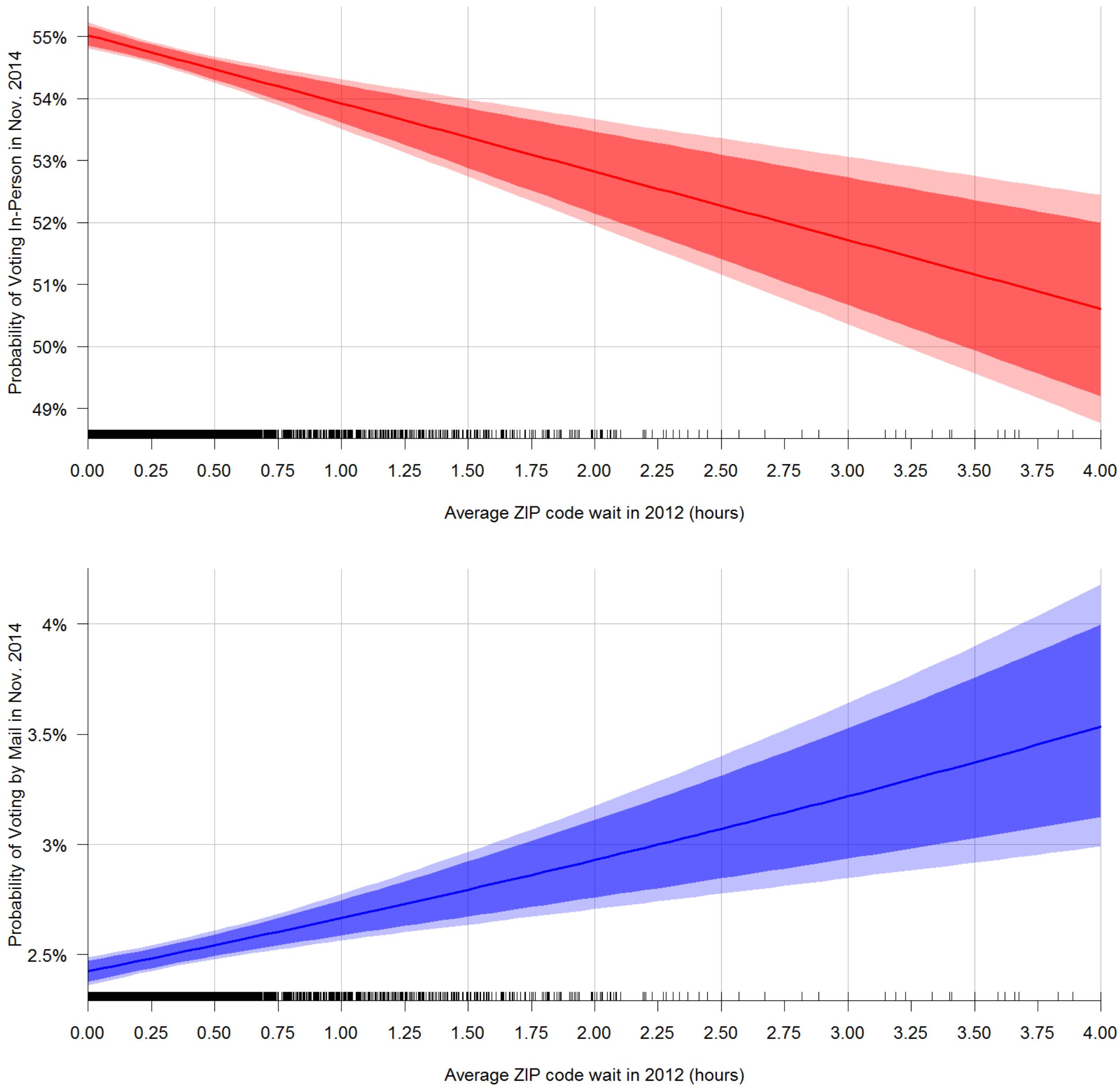
*p<0.05; **p<0.01; ***p<0.001
Multinominial logit coefficients reported
DV reference category: Not voting in 2014
Controls and state fixed effects included

To evaluate this possibility, I use the same data and model as Table 1 but change the dependent variable to be a three-category variable of whether the voter voted in-person, by mail, or did not vote at all in 2014. I use multinomial logistic regression to simultaneously estimate impact of 2012 lines on in-person voting and voting by mail in 2014. Table 3 summarizes these results using 2012 in-person voters, as well as the two placebo groups.

For in-person voters in 2012, the models suggest 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. The same is not true for the 2012 mail and nonvoter placebo groups, for whom there is not significant relationship between lines and 2014 mode of voting.

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 4 shows

Figure 4: Changes in mode of voting in 2014, given different line lengths in 2012 (with 95% and 99% CIs)



that voters who experienced longer lines in 2012 were less likely to vote in-person in 2014. 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 that 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 of one hour compared to those experiencing no line. Combined together, the increased turnout via mail and decreased in-person turnout nets out to an overall turnout effect similar to the results in Figures 1 and 3.

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, 2016; 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$_t$.[27]

To deal with these issues, I use two different research designs. First, the City of Boston provides a unique opportunity to avoid the problem 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 eliminates any measurement error or bias resulting from the movement of precinct boundaries, providing 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. Although the precinct boundaries in these counties were not fixed like Boston, I use snapshots of the state voter file from just after the 2012 election and just after the 2014 election to track

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

individual-level turnout across time. The 2012 snapshot allows me to identify every voter from each 2012 precinct. By merging in the 2014 voter history information for each of these voters, I can estimate the average 2014 turnout of voters in each 2012 precinct even when re-precincting or voter mobility has spread the precinct's 2012 voters across the state.

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. Even worse, 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 data for three post-2012 elections, plus one pre-2012 election to serve as a placebo test. The three post-2012 elections – the Sept. 2013 primary election for mayor, the Nov. 2013 general election for mayor, and the Nov. 2014 federal election – were all low turnout contests. This makes them particularly difficult tests of the hypothesis, since most of the voters who participate in low salience elections have consistent voting patterns and are less likely to be affected by one bad precinct experience.

Table 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

Table 4 reports the results of these four regressions, where the dependent variable is the change in turnout from the 2012 election.[28] In addition to controlling for the 2012 delay in precinct closure, I included several precinct demographic variables,[29] as well as November 2010 turnout, which was the strongest predictor of turnout in 2014.

These models show that every additional hour late that a precinct closed, its turnout in the 2013 and 2014 elections dropped between 0.58 and 0.87 percentage points. The null result in column 4 in Table 4 provides evidence that the results in the first three columns are not the consequence of selection bias, since lines in 2012 did not impact the turnout in 2008.[30] This suggests that the post-2012 results are not the consequence of confounding by unmeasured factors which predict both line length and turnout in elections before or after 2012.

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. The data include the 15 largest counties in the state and cover 75.7% of the state's population. Unlike Boston, however, movement of precinct borders between elections makes it challenging to compare the reported precinct turnout in 2012 to that in 2014. Because these boundary movements are likely to be correlated with line length, approaches using geographic data are not a feasible approach to estimating the changes in precinct turnout.

To to estimate the effect, I first identify the set of individuals who voted in a particular precinct in 2012 by using a snapshot of the Florida voter file from just after that year's election.[31] I then use the voter-specific identification number to merge the sample with turnout information taken from a snapshot of the voter file in 2014. With this I calculate

[28]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.

[29]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.

[30]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).

[31]The 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 snapshot provides the most accurate list of voters in each precinct as is possible with available data.

2014 turnout rates for voters in each 2012 precinct, including voters that may have moved to a different part of the state.[32]

Table 5: 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)
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

I use weighted least squares to estimate the relationship between 2012 precinct closing delay and future turnout at the precinct level.[33] Using variables available in the voter files, I control for the gender balance, racial composition, average age, party registration, and 2010 turnout rate of each precinct. Table 5 presents the results for three regressions.[34] 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.

Tor 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 5 shows that the placebo test checks out: 2012 line length was not predictive of 2008 turnout. The estimates

[32] 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).

[33] 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.

[34] The full table of results is in Appendix Table A.17.

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. As the intercept from model 2 shows, very few people voted in 2012 also voted in the August primary.[35] This makes the primary a particularly difficult test of the hypothesis, given that relatively few people participated at all and those who did tend to have the consistent turnout records that are unlikely to change in response to a long line in 2012.

Figure 5: Expected Florida precinct turnout rates in November 2014 based on 2012 end-of-day lines (with 95% and 99% CIs)

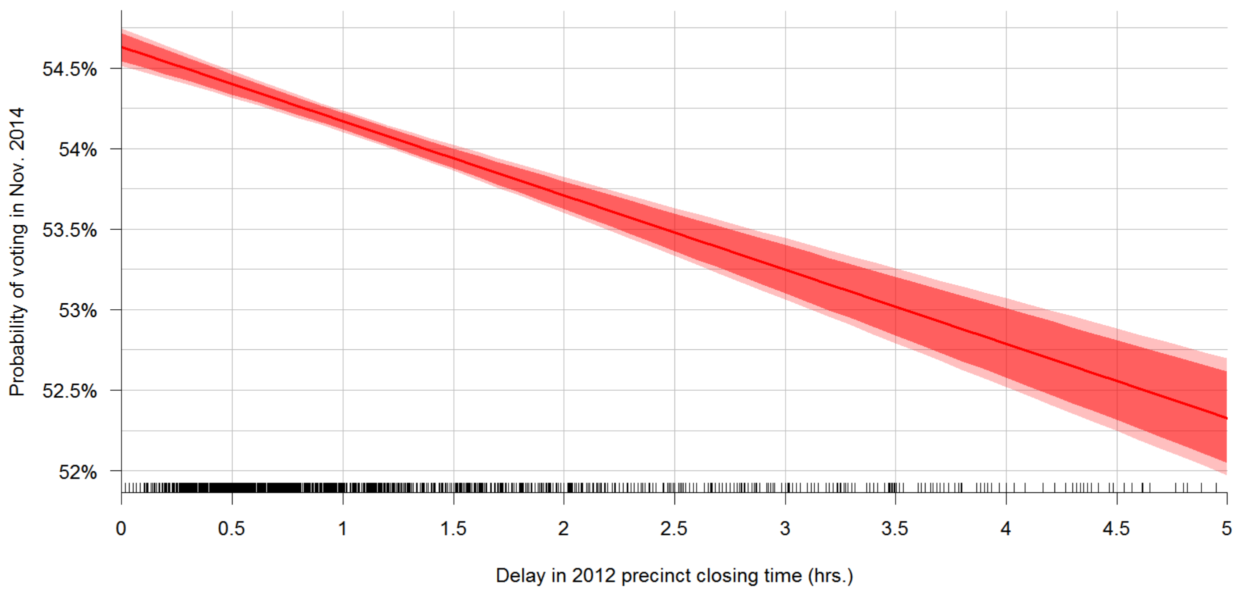


Figure 5 presents the November 2014 result graphically. For the 28.1% of precincts that closed within thirty minutes of the designated closing time, the average turnout in the 2014 general election among 2012 voters was approximately 54.5%. In the 1,193 precincts (35.8%) that closed more than an hour 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 is less than 53.7%–0.92 percentage points lower than

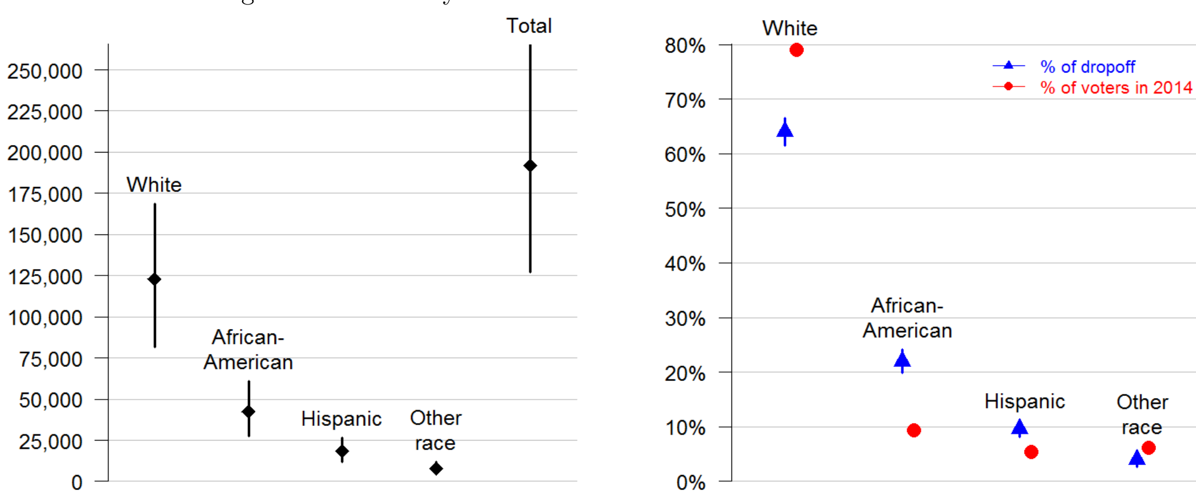
[35]The coefficient here is negative because age was included in the model. For a precinct with an average age of 40 (and all other covariates set to zero), only 2.2% of 2012 voters participated in this primary.

on-time precincts.

5. Implications and Discussion

The analyses in this paper provide consistent support for the hypothesis that longer lines diminish a voter's probability of turning out 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.

Figure 6: How many voters did not vote in 2014 because of 2012 lines?



To estimate just how consequential, I used the results from Model 1 in Table 1 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 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 (out of over 220,000 cast). In that district alone, the model suggests about 258 (SE: 56.4) people did not vote in 2014 as a result of lines in 2012. This is not to suggest that long lines definitely played a pivotal role in the outcome of this election, only that there is a realistic potential for administrative mismanagement at polling places to have an impact on election results in close races.

The implications of these findings become even more stark when we take into account that minority voters are much 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 vastly 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.

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. These lines make voters less likely to vote in the next election, thereby diminishing a key accountability mechanism for those government officials.

The results also have broader implications on our understanding of turnout and citizen participation. Given that voting may be habit forming (Meredith, 2009; Gerber, Green, and Shachar, 2003; 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 problems 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?

There is also the opportunity to further explore the mechanism at play. The discordant results between the placebo and non-placebo tests in Sections 3.1 and 3.2 suggest that the in-precinct experience is what is driving the differences in turnout rates. Does a similar pattern emerge in areas where voters are more likely to be hassled over their registration status or voter identification card? It is worth understanding how each of these factors could play a role in shaping citizen attitude toward the government.

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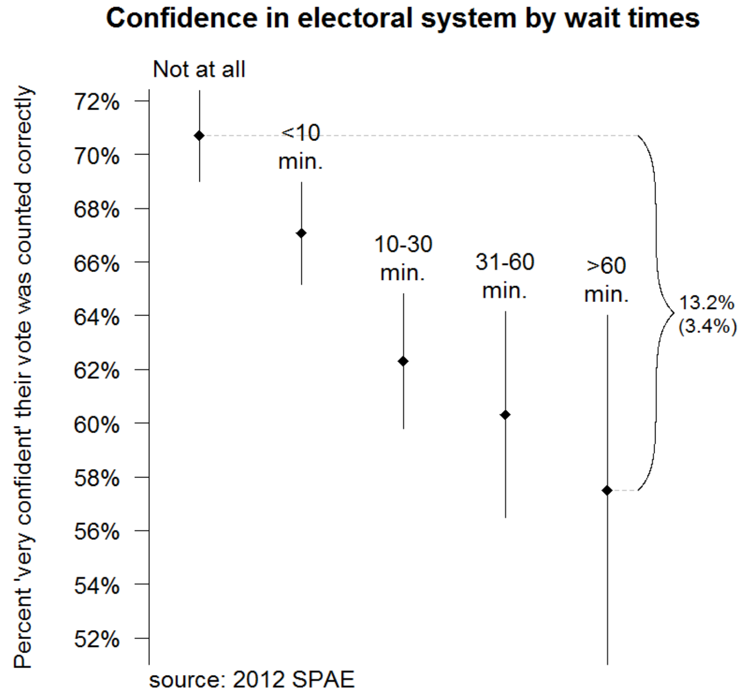
Appendix A.

Figure A.7: Voter confidence in the electoral system, by 2012 wait time



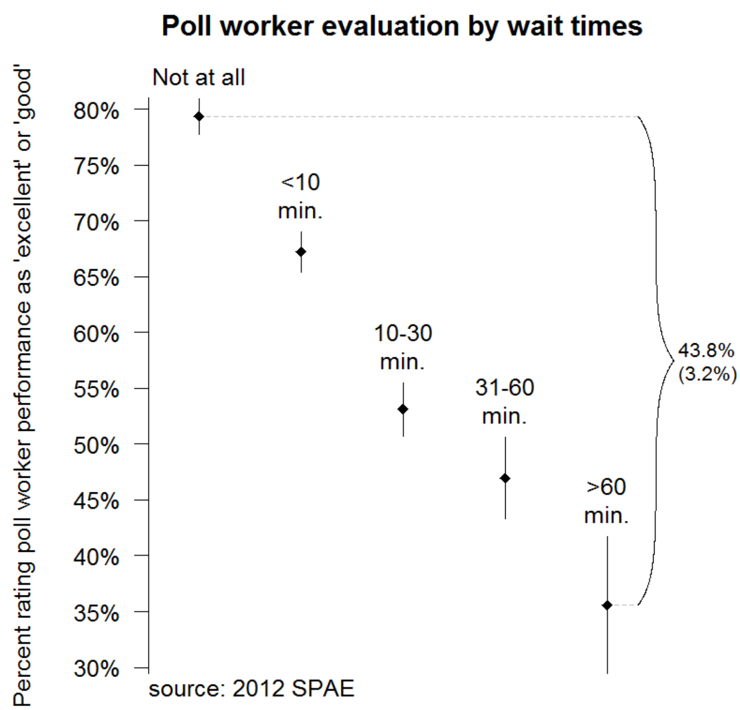


Figure A.8: Map of ZIP codes, colored by whether or not the CCES included data for calculating the average wait time

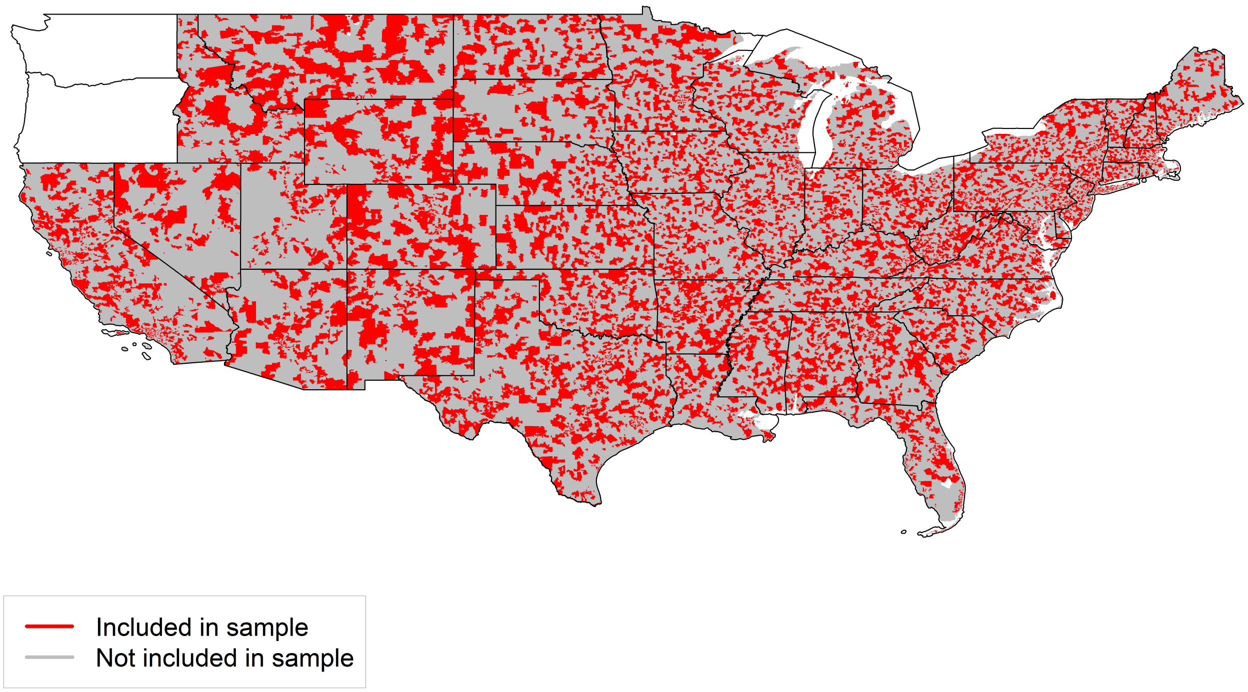


Figure A.9: Similarities of line length experience within various geographic units

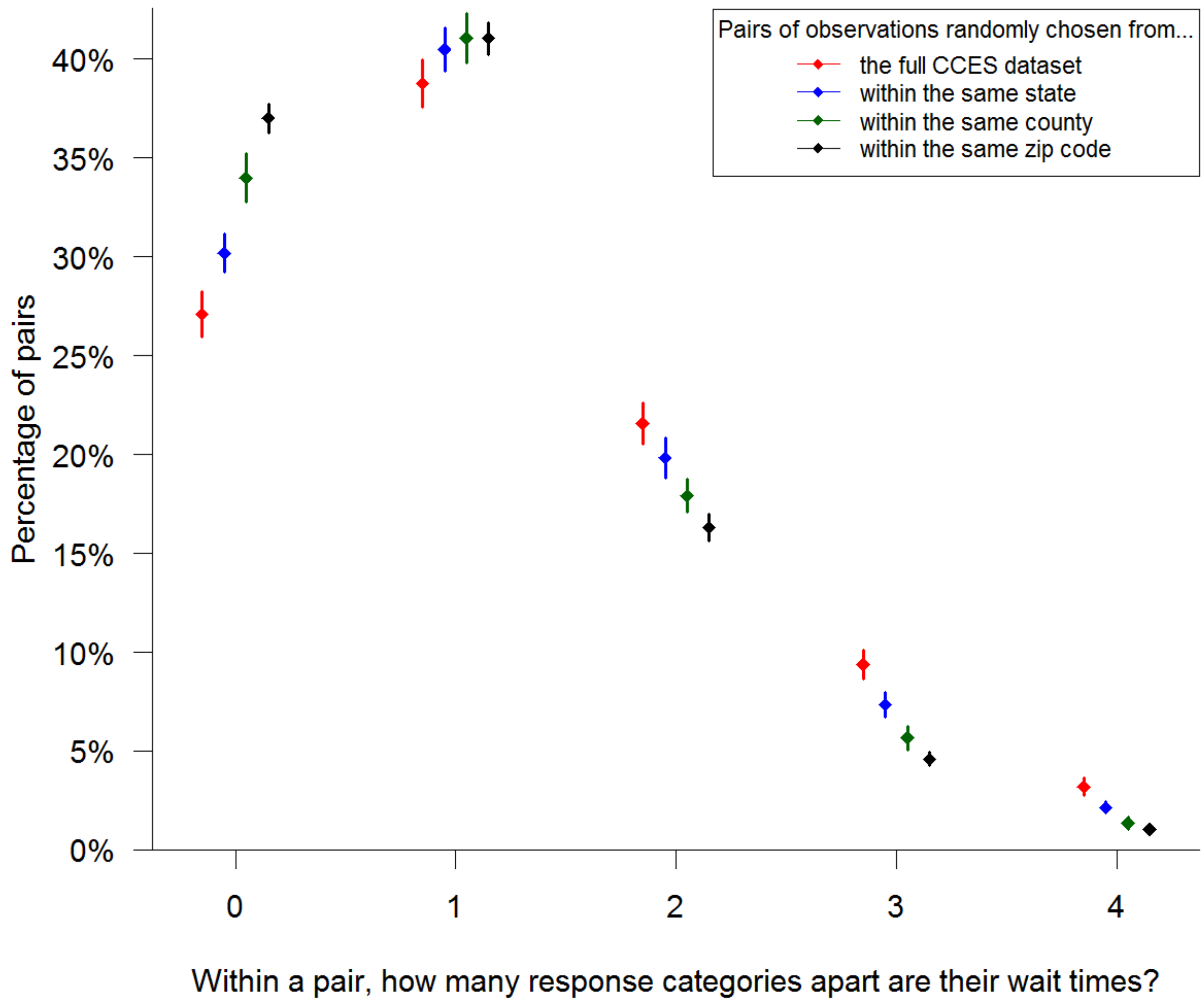
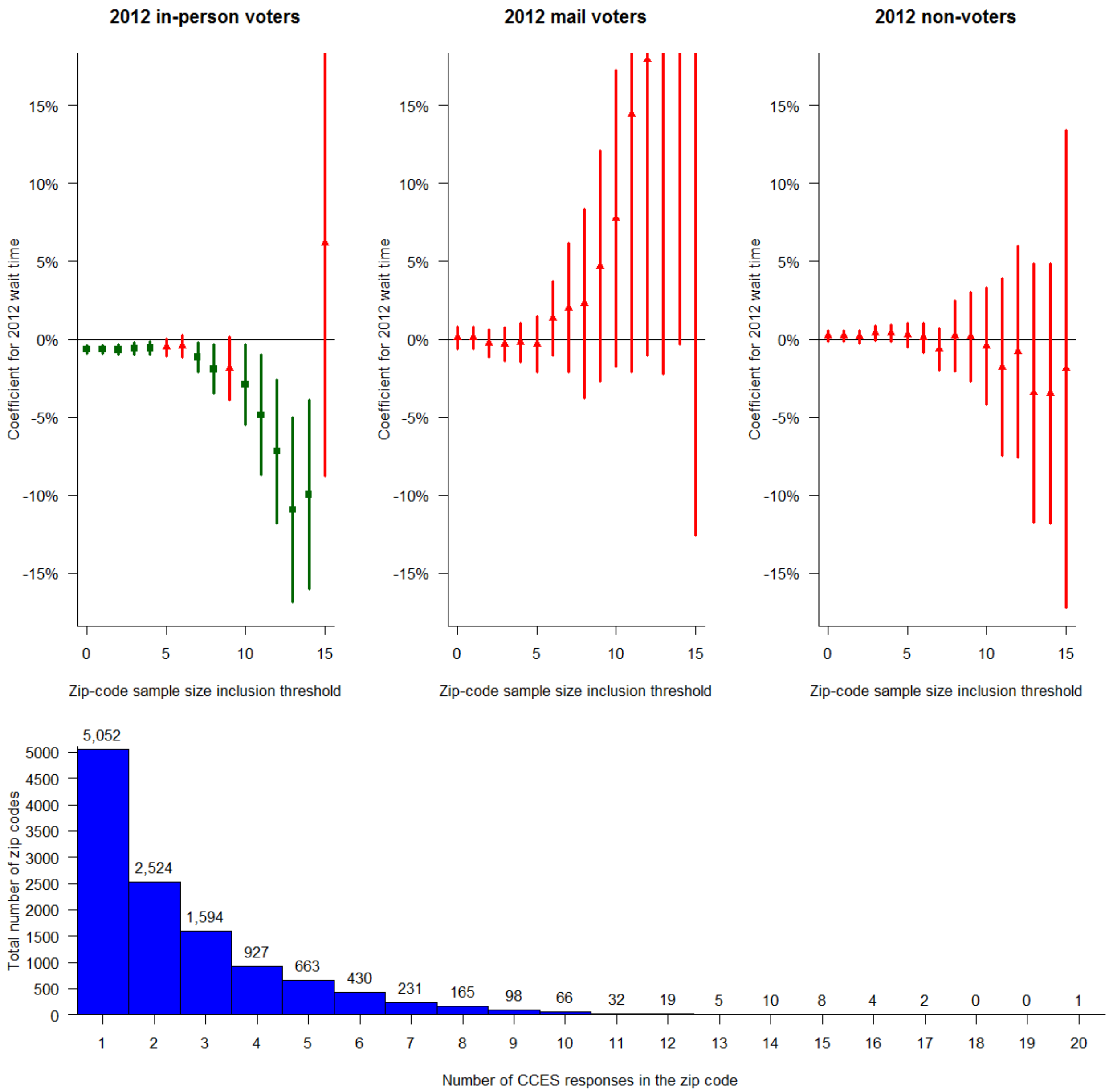


Figure A.10: Relationship between 2012 wait time and 2014 turnout, based on sample size thresholds that dictate whether a ZIP code is included in the analysis



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

Table A.6: 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 A.7: 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 A.8: 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 A.9: Treatment/control group sizes for different treatment cutoffs

	15 minutes	30 minutes	45 minutes	60 minutes
Treated	461,297	206,611	128,145	68,540
	(29.6%)	(13.2%)	(8.2%)	(4.4%)
Control	1,098,983	1,353,669	1,432,135	1,491,740
	(70.4%)	(86.8%)	(91.8%)	(95.6%)

Figure A.11: Post-matching standardized difference in means (μ/σ^2) between treatment and control for several potential confounders

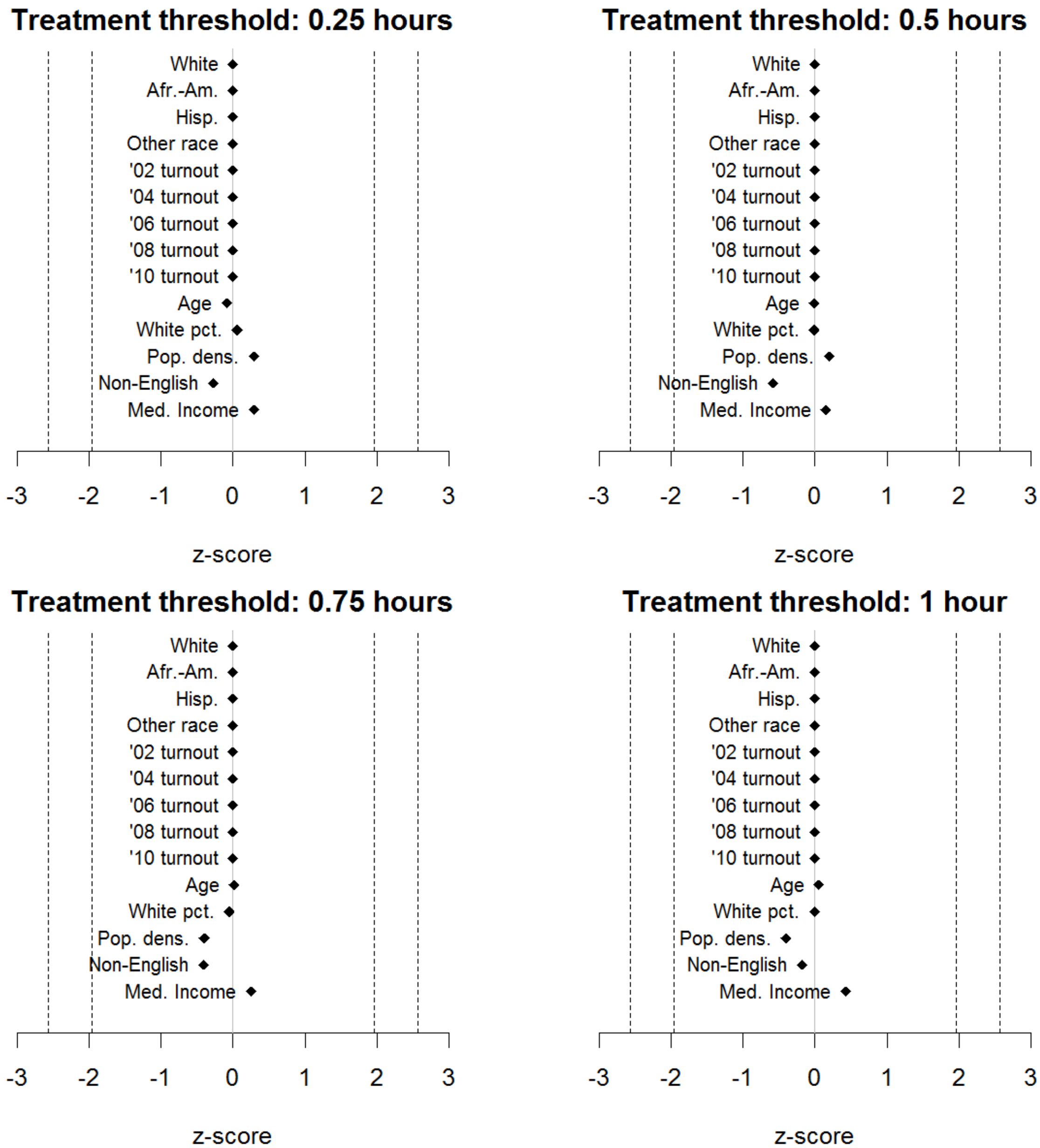


Figure A.12: Post-matching difference in means between treatment and control for several potential confounders

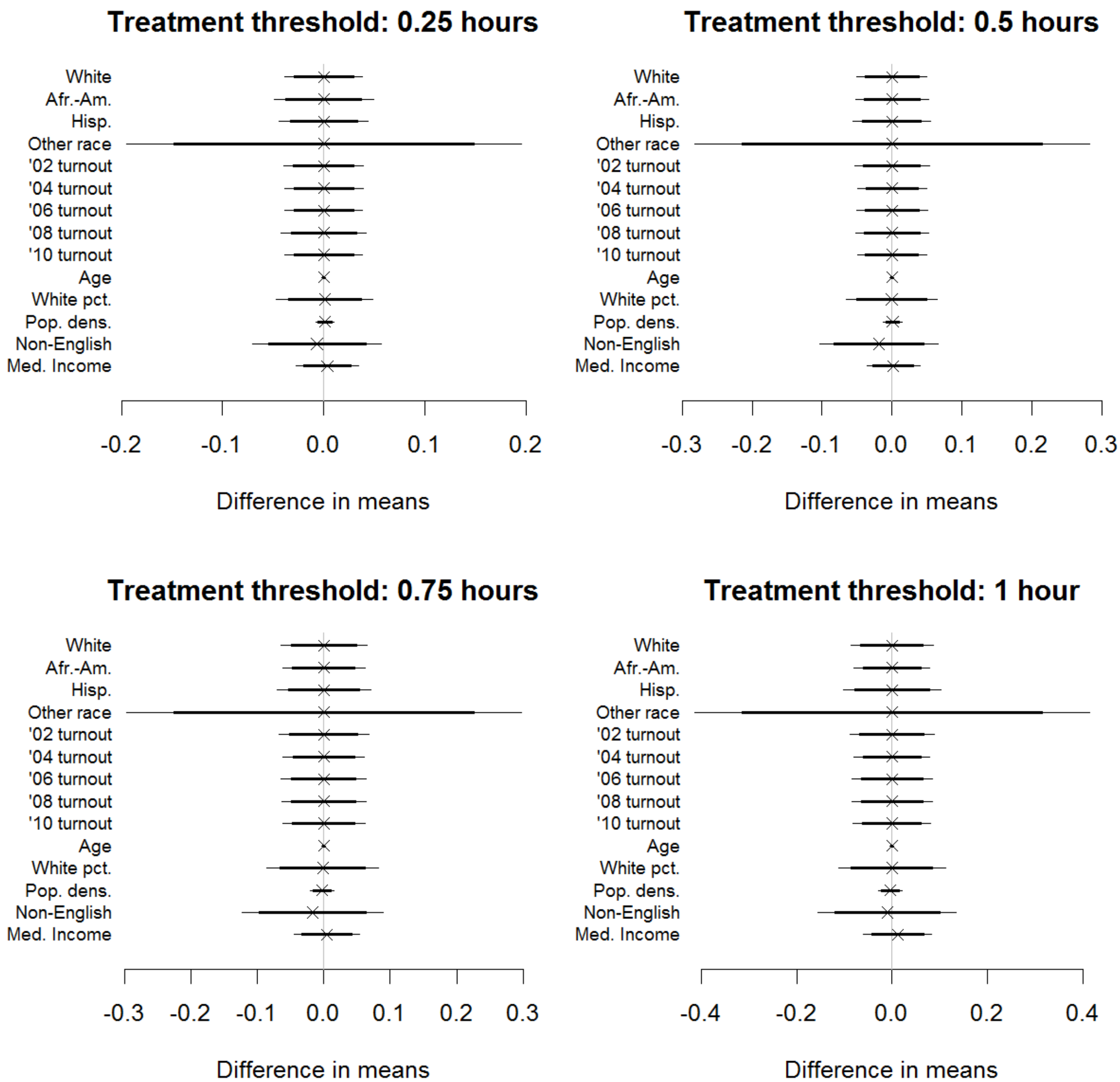


Table A.10: Effect of lines on turnout in matched dataset (2012 in-person voters only)

	(1)	(2)	(3)	(4)
Intercept	0.0465	0.0713*	0.0248	0.0773
	(0.0266)	(0.0357)	(0.0424)	(0.0579)
Long wait	−0.0086***	−0.0079***	−0.0066*	−0.0077*
	(0.0016)	(0.0022)	(0.0027)	(0.0035)
Afr.-Am.	−0.0129***	−0.0094*	−0.0111*	−0.0130
	(0.0037)	(0.0044)	(0.0052)	(0.0067)
Hispanic	−0.0612***	−0.0680***	−0.0753***	−0.0843***
	(0.0041)	(0.0054)	(0.0066)	(0.0084)
Other race	−0.0622***	−0.0710***	−0.0463**	−0.0403
	(0.0080)	(0.0114)	(0.0155)	(0.0212)
2006 turnout	0.1790***	0.1748***	0.1749***	0.1787***
	(0.0022)	(0.0030)	(0.0037)	(0.0048)
2008 turnout	−0.0163***	−0.0139***	−0.0097*	−0.0032
	(0.0027)	(0.0035)	(0.0041)	(0.0053)
2010 turnout	0.2885***	0.2895***	0.2871***	0.2800***
	(0.0023)	(0.0031)	(0.0037)	(0.0049)
Age	0.0033***	0.0033***	0.0031***	0.0030***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
College educated	0.0002***	0.0002***	0.0002***	0.0002***
	(0.00002)	(0.00002)	(0.00003)	(0.00004)
White pct.	−0.0099	0.0079	0.0141	0.0202
	(0.0059)	(0.0073)	(0.0085)	(0.0111)
Pop. dens. (logged)	−0.0058***	−0.0055***	−0.0063***	−0.0062**
	(0.0007)	(0.0012)	(0.0014)	(0.0021)
Non-Eng. speaking pct.	−0.0569***	−0.0259**	−0.0237*	−0.0174
	(0.0076)	(0.0090)	(0.0104)	(0.0132)
Med. inc. (logged)	0.0100***	0.0065	0.0105**	0.0037
	(0.0026)	(0.0033)	(0.0039)	(0.0052)
'Treatment' cutoff	15 min.	30 min.	45 min.	60 min.
Observations (weighted)	159,664.6	84,269.7	56,166.6	31,132.1
Observations	270,177	146,233	100,098	57,442
R^2	0.2671	0.2760	0.2761	0.2755

*p<0.05; **p<0.01; ***p<0.001
OLS coefficients reported
State fixed effects included

Table A.11: Effect of lines on turnout in matched dataset (mail-in voters placebo tests)

	(1)	(2)	(3)	(4)
Intercept	0.2392***	−0.0362	0.4526***	0.7429***
	(0.0590)	(0.2891)	(0.1003)	(0.1428)
Long wait	−0.0038	0.0016	−0.0020	−0.0078
	(0.0033)	(0.0051)	(0.0061)	(0.0085)
Afr.-Am.	0.0173	0.0272*	0.0384*	0.0001
	(0.0088)	(0.0123)	(0.0153)	(0.0209)
Hispanic	−0.0505***	−0.0415**	−0.0399**	−0.0481*
	(0.0069)	(0.0127)	(0.0150)	(0.0219)
Other race	−0.0009	0.0368*	0.0366	−0.0125
	(0.0095)	(0.0176)	(0.0210)	(0.0394)
2006 turnout	0.1626***	0.1381***	0.1285***	0.1173***
	(0.0049)	(0.0074)	(0.0089)	(0.0122)
2008 turnout	−0.0204**	−0.0171	−0.0205	0.0227
	(0.0069)	(0.0101)	(0.0120)	(0.0168)
2010 turnout	0.2759***	0.2625***	0.2784***	0.2582***
	(0.0058)	(0.0087)	(0.0103)	(0.0147)
Age	0.0035***	0.0029***	0.0027***	0.0020***
	(0.0001)	(0.0002)	(0.0002)	(0.0003)
College educated	0.0001**	0.0001**	0.0002**	0.0002
	(0.00003)	(0.00005)	(0.0001)	(0.0001)
White pct.	0.0026	−0.0051	−0.0025	−0.0399
	(0.0133)	(0.0197)	(0.0233)	(0.0309)
Pop. dens. (logged)	−0.0051**	0.0047	0.0022	−0.0066
	(0.0016)	(0.0031)	(0.0036)	(0.0054)
Non-Eng. speaking pct.	−0.0241	−0.0412	−0.0169	−0.0391
	(0.0167)	(0.0251)	(0.0286)	(0.0378)
Med. inc. (logged)	0.0069	−0.0059	−0.0098	−0.0217
	(0.0053)	(0.0083)	(0.0096)	(0.0134)
'Treatment' cutoff	15 min.	30 min.	45 min.	60 min.
Observations (wtd.)	27,821.3	11,951	8,307.8	4,668.7
Observations	53,873	23,307	16,217	8,126
R^2	0.2307	0.2066	0.2023	0.1883

*p<0.05; **p<0.01; ***p<0.001
OLS coefficients reported
State fixed effects included

Table A.12: Effect of lines on turnout in matched dataset (non-voters placebo tests)

	(1)	(2)	(3)	(4)
Intercept	0.0095	0.0088	0.0212	0.0902
	(0.0252)	(0.0326)	(0.0374)	(0.0524)
Long wait	−0.0018	0.0013	0.0005	0.0013
	(0.0016)	(0.0021)	(0.0024)	(0.0032)
Afr.-Am.	−0.0124***	−0.0111**	−0.0096*	−0.0216***
	(0.0033)	(0.0038)	(0.0043)	(0.0057)
Hispanic	−0.0217***	−0.0207***	−0.0227***	−0.0264***
	(0.0030)	(0.0038)	(0.0046)	(0.0059)
Other race	−0.0295***	−0.0270***	−0.0280***	−0.0287*
	(0.0044)	(0.0064)	(0.0084)	(0.0122)
2006 turnout	0.0544***	0.0464***	0.0535***	0.0581***
	(0.0043)	(0.0056)	(0.0065)	(0.0086)
2008 turnout	−0.0195***	−0.0155***	−0.0175***	−0.0180***
	(0.0022)	(0.0028)	(0.0032)	(0.0043)
2010 turnout	0.1367***	0.1583***	0.1660***	0.1861***
	(0.0046)	(0.0060)	(0.0072)	(0.0096)
Age	0.0012***	0.0013***	0.0013***	0.0016***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
College educated	0.0003***	0.0003***	0.0003***	0.0003***
	(0.00002)	(0.00002)	(0.00003)	(0.00003)
White pct.	0.0054	0.0079	0.0107	−0.0072
	(0.0052)	(0.0063)	(0.0072)	(0.0093)
Pop. dens. (logged)	−0.0018*	−0.0030**	−0.0037**	−0.0087***
	(0.0007)	(0.0011)	(0.0013)	(0.0020)
Non-Eng. speaking pct.	−0.0191**	−0.0235**	−0.0283***	−0.0293**
	(0.0062)	(0.0074)	(0.0083)	(0.0107)
Med. inc. (logged)	−0.0005	−0.0005	−0.0023	−0.0050
	(0.0024)	(0.0029)	(0.0033)	(0.0044)
'Treatment' cutoff	15 min.	30 min.	45 min.	60 min.
Observations (wtd.)	75,545.7	43,478.8	29,866.9	17,045.8
Observations	126,226	77,596	57,457	34,337
R^2	0.0373	0.0403	0.0406	0.0479

*p<0.05; **p<0.01; ***p<0.001
OLS coefficients reported
State fixed effects included

Figure A.13: The effect of a long wait for people who did not vote in-person in 2012, given their turnout pattern in 2008 and 2010

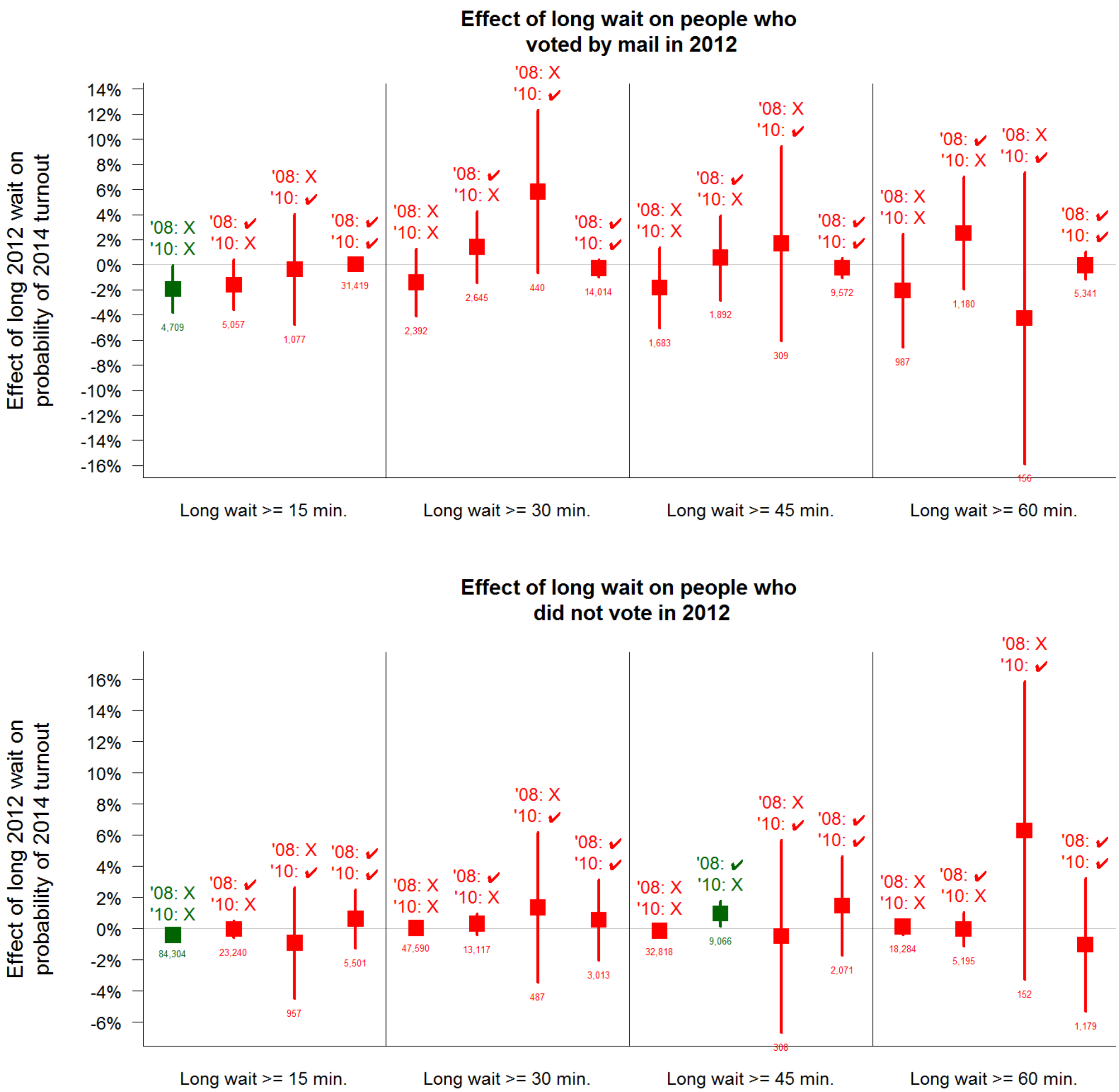


Table A.13: How did lines impact the mode of future voting among 2012 in-person voters?

	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 A.14: 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 A.15: 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 A.16: 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 A.17: 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