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The downstream consequences of long waits: How lines at the precinct depress future turnout

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A R T I C L E I N F O

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A B S T R A C T

Researchers have increasingly paid attention to the impact that the administrative component of elections has on voter behavior. Existing research has focused almost exclusively on the effect that legal changes–such as voter identification laws–have on turnout. This paper extends our understanding of the electoral process by exploring how one aspect of the precinct experience–standing in line to vote–can shape the turnout behavior of voters in subsequent elections. 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. 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 important policy implications for administrative changes that may impact line length, such as voter identification requirements and precinct consolidation. They also suggest that racial asymmetries in precinct wait times contribute to the gap in turnout rates between white and non-white voters.

1. Long lines at voting precincts

In recent years researchers and political observers have paid increasing attention to the impact that the administrative component of elections has on voter behavior. Existing research has focused largely on the effect that legal changes–such as voter identification laws or early voting–have on turnout (Highton, 2017; Hajnal et al., 2017; Burden et al., 2014). In contrast, little consideration has been given to the experience voters have while inside their precinct, despite recent work showing how first-hand experiences can shape a person's political participation (Achen and Bartels, 2016; White, 2019). 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. Using three different empirical approaches, 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 1 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 (Famighetti et al., 2014; Herron and Smith, 2015a; Stein et al., 2019), with non-white voters being seven times more likely to wait longer than an hour than white voters (Chen et al., 2019). 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 (Herron and Smith, 2016; Pettigrew, 2017).

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 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. 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 three empirical strategies to show that for each additional hour

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

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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 hypotheses in Section 2, I use a national sample of voter history data to estimate the turnout effect at the voter level. In Section 3A, 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 3B, 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 et al., 2008), or primary election eligibility rules (Kaufmann et al., 2003). 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 (Brady and McNulty, 2011; Amos2017) 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 costs and benefits from voting (Riker and Peter, 1968; Aldrich, 1993). Previous work has shown additional costs from changed precinct locations (McNulty et al., 2009) or lengthy commutes to the polls (Gimp el et al., 2006) result in diminished turnout. When a voter waits in a long line, they might update their utility function to accounting for the cost of possibly waiting in a long line again. Also, the mere act of standing in line with dozens or hundreds of other voters might remind a voter that his or her 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, waiting in line in the first place, when that ballot is unlikely to be pivotal, could be construed as irrational behavior.

The second explanation for why lines may depress future turnout is a psychological and sociological one. Recent research views electoral participation as more of a consumption good than an investment one (Achen and Bartels, 2016). By this line of reasoning, voters do not 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 (Hersh, 2020). It stands to reason then, that a bad customer service experience at the polls might make them less 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, 2019; Lerman and Weaver, 2014). Others (Alvarez et al., 2008) have 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 day, when they anticipate lines to be shorter. While this is certainly plausible, most people (particularly those in areas afflicted by lines) only have the option 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. These possibilities make the identification of an overall turnout effect more difficult and amplifies the normative implications 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 selection bias. The strongest predictors of line length are a neighborhood's racial composition and its population density (Pettigrew, 2017; Herron and Smith, 2015a; Famighetti et al., 2014), but these factors may also confound the relationship between lines and turnout. White voters are more likely to live in suburban and rural areas where lines tend to be shorter. Minority voters, particularly those who are Black, are more likely to live 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 and may also effect turnout.

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 strong assumption with placebo tests using voters-by-mail and nonvoters. I then 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

[2] See Figure A1 in the appendix for the full results of these two analyses.

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.

The analyses throughout this section use data from Catalist, a vendor which compiles vote history data from across the country. Specifically, I analyze a 1% random sample from Catalist's database (n > 3 million), which includes a representative sample of vote history information from the entire country.[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 habitual voters who would be less sensitive to experiencing a long line.[5]

Ideally, the ''treatment'' variable would be the amount of time each individual 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, 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 min', '10–30 min', '31 min to an hour', and 'more than an hour'. Following the convention used in this literature (Pettigrew, 2017; Stewart, 2013), I recode 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). An alternative approach would be to only use ZIP codes with at least n > 1 responses. Figure A3 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 a 78% chance that the 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.[8]

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.

3.1. Evidence from individual voter records

Model 1 in Table 1 shows the results of a linear probability regression model[9] 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.[10] To account for confounding, the models control for the voter's race, age, education, and turnout history in 2006, 2008, and 2010.[11] 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.[12]

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.[13] Fig. 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]).[14] The turnout probability of those who waited 1 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 rug plot on the graph illustrates, most ZIP codes had an average wait of less than 1 hour, yet 5.4 million (4.2%) of voters in 2012 lived in a ZIP code with an average wait of greater than 60 min.

Interpreting these results causally requires assuming that there are

[3] I remove voters from Washington and Oregon for all analyses, since those states exclusively used a vote-by-mail system in 2012. 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 Bradley (2019) 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. Another limitation of voter file analyses is the issue of voter mobility creating stale voter registration lists. Commercially curated voter lists like Catalist are more likely to address these issues than public voter files because companies can track voter mobility using proprietary commercial data that state election officials do not have access to. This creates a limitation to working with these data, since it is difficult to assess these curation methods. Nonetheless, using a national file like Catalist is a distinct advantage over uncurated state-level voter files.

[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.

[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 min). Those who waited more than 1 hour specified their wait time in an open-ended follow-up.

[8] See Figure A2 in the appendix for additional analysis.

[9] The substantive results are the same when using logistic regression. Those results are reported in appendix Table A3.

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

[11] 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.

[12] These variables all come from the 2012 estimates of the 5-year American Communities Survey, conducted by the Census Bureau. Table A1 in the appendix reports the full regression results with all controls.

[13] The results also hold when a quadratic term is included for the wait time variable. See Table A2 in the appendix for these results. The marginal effect is more than doubled when we include the reported wait times from only CCES respondents who voted in-person on Election Day. When a similar analysis using only early in-person CCES respondents, there is no effect. This probably results from the inability to distinguish between early and Election Day voters in the Catalist data, and the fact that most 2012 in-person voters cast their ballot on Election Day.

[14] 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 Fig. 1.

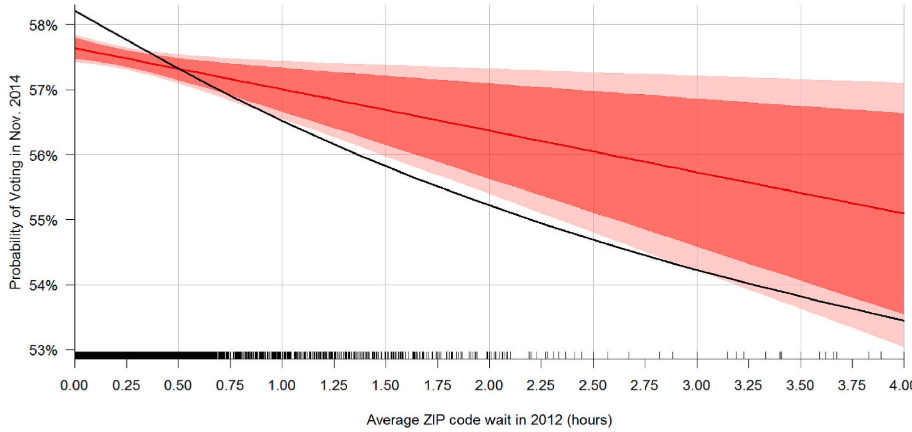


Fig. 1. Predicted probability of turnout in 2014, conditional on wait time in 2012 (with 95% and 99% CIs and loess smoother of bivariate relationship).

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 statistically significant result among in-person voters is the consequence of some unmeasured confounder, we might expect to find a similar result among mail-in and non-voters.

Using the same specification as Model 1 in Table 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.[15] 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.

3.2. *Using matching to mitigate confounding*

Although regression helps to account for confounding, it does not ensure that the treatment and control groups will be balanced on higher order moments and interactions between covariates (Iacus, King, and Porro, 2011b). To deal with this problem, I employ exact matching, which has been used in recent work to estimate causal from vote history data 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 min, and defined four treatment groups based on where lines were 15–30 min, 30–45 min, 45–60 min, and longer than 60 min. 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.[16]

I use exact matching to pair treated and control units within the same state, who are the same race (White, Black, 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 cut-points 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.[17] 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 reports the post-matching estimates of the effect of a long 2012 wait on an in-person voter's probability of turning out in November 2014.[18] 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 min.

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

Table 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 min.

[15] 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.

[16] The smallest of these 5 treatment/control categories has 59,605 observations. See Appendix Table A4 for the sample sizes of all the groups.

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

[18] ZIP code cluster-robust standard errors are reported. The full results, including control covariates, are presented in Appendix Table A5.

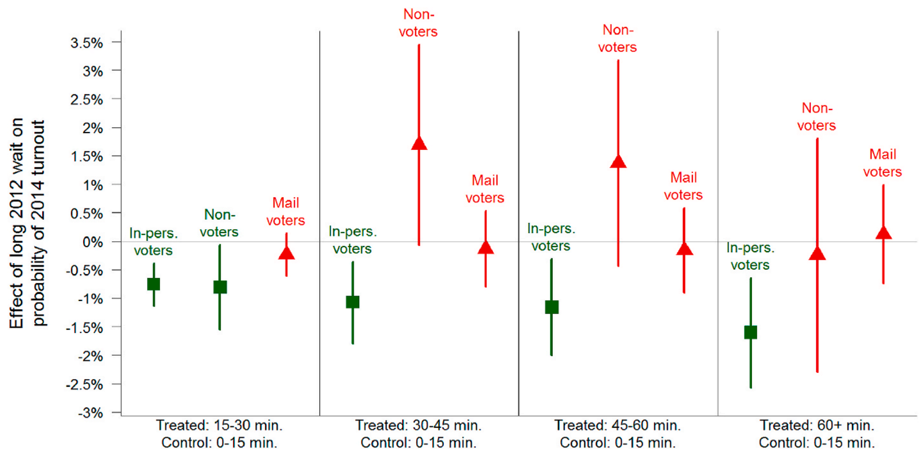


Fig. 2. Effect of 2012 lines on turnout for various definitions of the treatment group.

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 min.

The four 'in-pers. Voters' green bars and squares in Fig. 2 visualize the results in Table 2.[19] The bars labeled 'non-voters' and 'mail voters' present the results from the eight placebo tests of the effect of lines on people who did not go to their precinct in 2012.[20] 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. 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 directly experience a long line.

One possible explanation for the results thus far is that they are driven largely by turnout. 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 drop-off 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). Fig. 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.[21]

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, Fig. 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.[22] 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.

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 1 but 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.[23] Multinomial logistic regression, summarized in Table 3, allows for simultaneous estimation of the 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

[19] The bars signify 95% confidence intervals.
[20] Appendix Tables A6 and A7 show the full results from these models.
[21] These results come from the four matched datasets used in Table 2, which were subset based on 2008 and 2010 turnout history prior to estimating the coefficients.
[22] 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.
[23] See Tables A8, A9, and A10 in the appendix for the full results of these models.

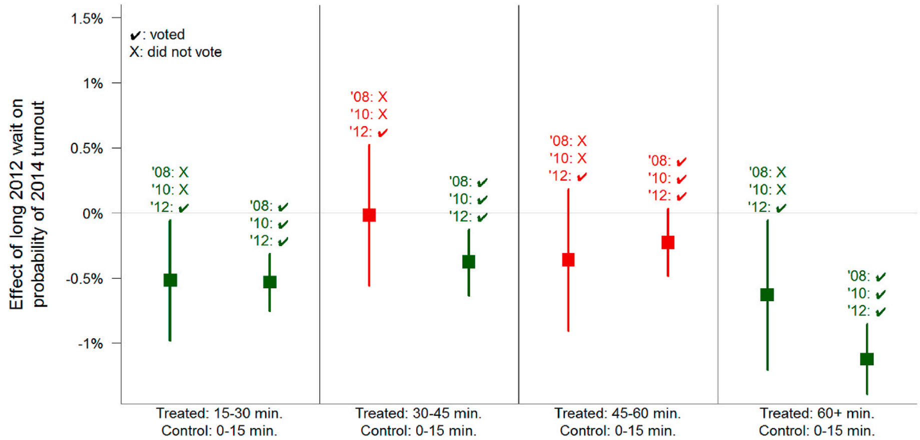


Fig. 3. The effect of a long wait for people 2012 in-person voters, given their turnout history in 2008 and 2010.

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.

in 2014. The top of Fig. 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 of 1 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 Figs. 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, recent work has shown that the delay in precinct closing times correlates strongly with line length at precincts (Pettigrew, 2017; Herron and Smith, 2015b). 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 (Nyhan et al., 2016), 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.

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.[24] 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. 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.

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 min 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 analyzed 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 4 reports the results of these four regressions, where the

[24] ''Phantom Precinct Shows City's Arcane Voting Laws.'' Boston Globe. November 3, 2009.

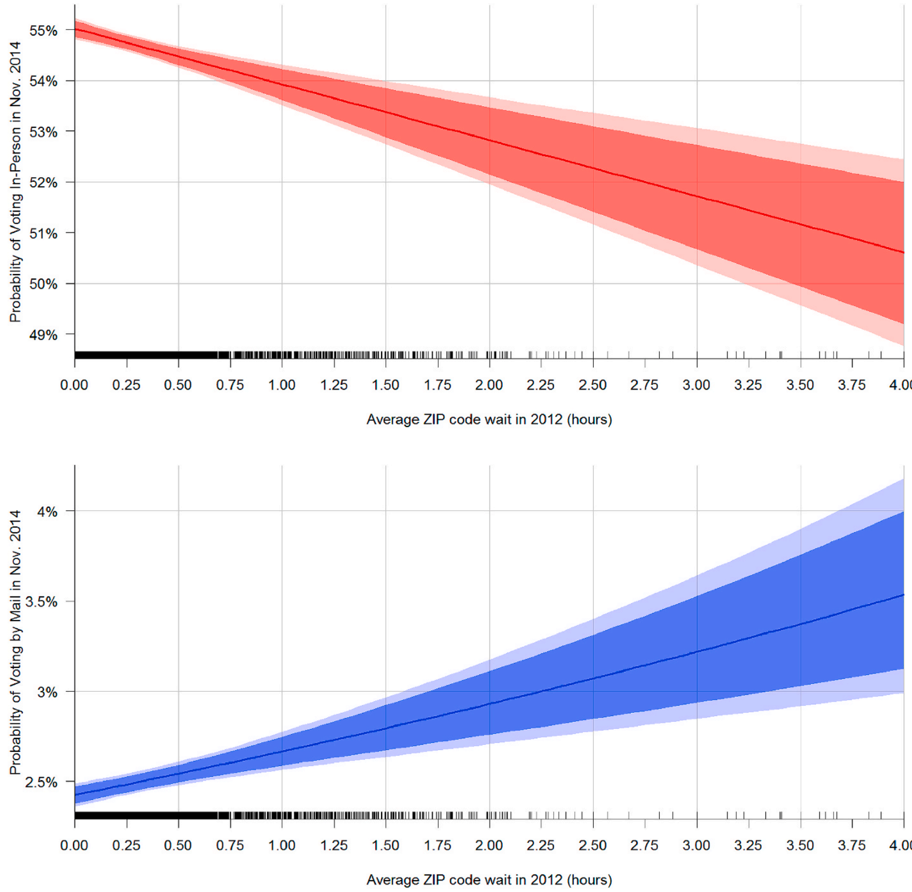


Fig. 4. Changes in mode of voting in 2014, given different line lengths in 2012 (with 95% and 99% CIs).

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-squared	0.6540	0.6175	0.2134	0.0362

*p < 0.05; **p < 0.01; ***p < 0.001.
OLS coefficients reported.
Control variables included.

dependent variable is the change in turnout from the 2012 election.[25] In addition to controlling for 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]

4.2. Changes in turnout in Florida precincts

Like Boston, I proxy for line length using precinct closing times from 3334 precincts in 17 Florida counties, covering 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.[28] To estimate the effect, I first identify the set of voters in each of the 3334 precincts in 2012 using the voter file

[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 A11 presents the full results of this model.

[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).

[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.

Table 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)	3334	3334	3334
Observations (unweighted)	3,012,356	3,012,356	3,012,356
R-squared	0.1520	0.1045	0.1608

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

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.[30] With this I calculate 2014 turnout rates for each 2012 precinct, including voters that may have moved to a different part of the state.[31]

Weighted least squares estimates the relationship between 2012 precinct closing delay and future turnout at the precinct level.[32] 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.[33] 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. This result is consistent with Cottrell et al., (Forthcoming), which uses voter check-in times to find a one percentage point effect of long lines. Additionally, column 3 of Table 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.[34] This makes this election 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.

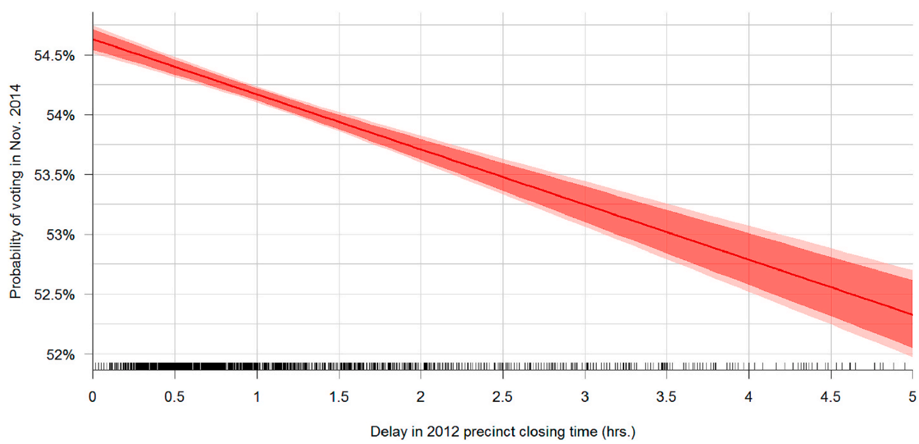


Fig. 5. Expected Florida precinct turnout rates in November 2014 conditional on 2012 end-of-day lines (with 95% and 99% CIs).

Fig. 5 presents the November 2014 result graphically. For the 28.1% of precincts that closed within 30 min of the designated closing time, the average turnout in the 2014 general election among 2012 voters was 54.5%. In the 1193 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 2 hours late was less than 53.7%–0.92 percentage points lower than on-time precincts.

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 et al., 2008; Green et al., 2003), a difference of 1 percentage point for the millions of voters who waited at least an hour in 2012 is consequential.

[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, these data provide the most accurate list of voters in each precinct as is possible, given available data.

[30] Amos et al. (2017) use a similar empirical strategy to study the effect of reprecincting on turnout.

[31] This approach cannot account for voters who moved out of the state between 2012 and 2014, but Census data indicate that only about 2% of Florida's 2012 population left the state by 2014 and this percentage is almost certainly smaller for registered voters, who tend to be less mobile (Ansolabehere et al., 2012).

[32] 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.

[33] The full table of results is in Appendix Table A12.

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

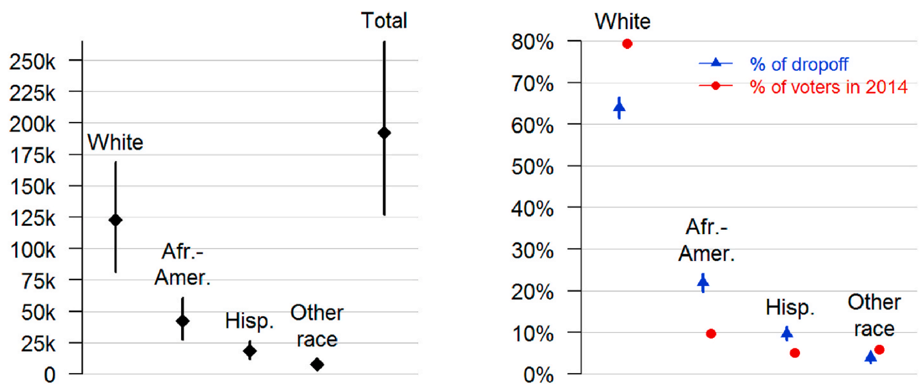


Fig. 6. How many voters did not vote in 2014 because of 2012 lines?.

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. Fig. 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, a 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 poor management at polling places could have an impact on election results in close races.

We must also consider that minority voters are more likely to be burdened by long lines at the precinct. When voter drop-off 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. An implication of this finding is that long lines disproportionately depress turnout among African-Americans, when compared to white voters. If voters no longer had to wait in line, we would expect, in the long run, that turnout rates would increase more in Black and minority neighborhoods than in White neighborhoods.

The results provide broader understanding of turnout and citizen participation. Given that voting may be habit forming (Meredith, 2009; Gerber et al., 2003; de Kadt, 2017; Shino and Smith, 2018), 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, 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 30 min have a different impact on somebody who expected to wait ten, compared to somebody who expected to wait sixty?

Future researchers may also consider the generalizability and scope of this turnout effect by replicating the study in future elections. The administration of elections is a constantly-evolving enterprise in the United States. As states expand vote-by-mail and extend early voting periods, waiting in long lines may become more anomalous, potentially diminishing the effect on future turnout. Conversely, diminished oversight of election administration, resulting in part from changes to the enforcement of the Voting Rights Act, could cause racial disparities in line length to be magnified. Lastly, researchers may consider the generalizability of these findings by conducting similar studies in other countries. This would advance our understanding about the psychology of the costs of political participation. Or it may indicate that the findings here are unique to the American political context.

The results here also raise some interesting considerations, given the COVID-19 pandemic. Many presidential and state primaries in the 2020 election cycle had record numbers of voters casting a ballot by mail. This pattern is likely to continue in the November 2020 general election and beyond–at least until the pandemic eases. The analysis in this article suggests that turnout among these voters-by-mail will not be depressed in future elections. However, many states and counties are responding to the pandemic by decreasing the number of in-person polling places.[35] This limited number of polling places, combined with an ineffective implementation of wide-scale vote-by-mail, could create a figuratively and literally dangerous combination for voters who have no choice but the vote in-person. If the 2020 primary elections in, for example, Wisconsin[36] and Kentucky[37] are an example, lines in some cities could stretch for hours in the general election. And the findings here suggest that the consequence of those lines could linger for years to come.

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.

[35] In many places, this is the result of a practical consideration. The pandemic makes it more difficult to recruit poll workers, who tend to be older and thus more vulnerable to the effects of the virus.

[36] https://www.npr.org/2020/04/07/828835153/long-lines-masks-and-plexiglas-barriers-greet-wisconsin-voters-at-polls.

[37] https://www.kentucky.com/news/politics-government/article243731882.html.

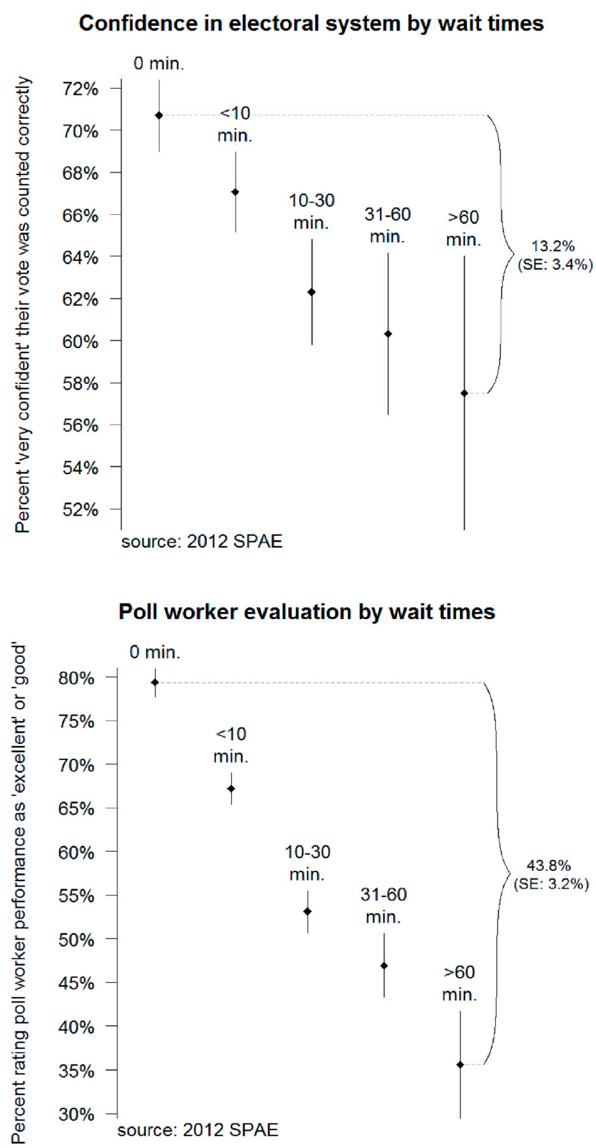


Fig. A1. Poll worker evaluations and voter confidence in the electoral system, by 2012 wait time.

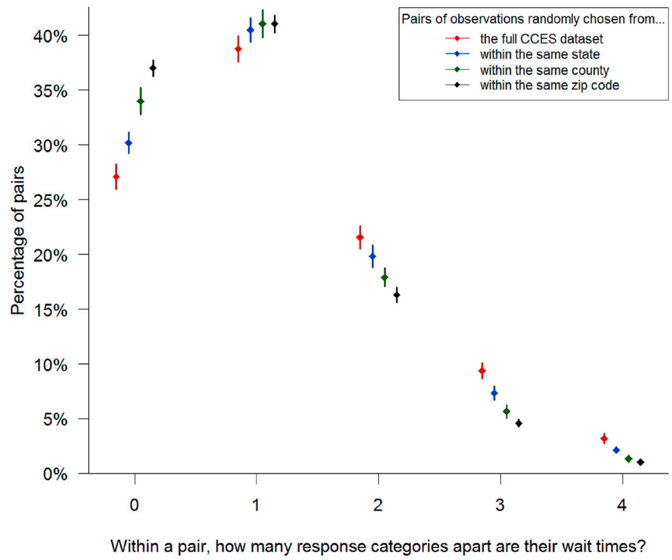
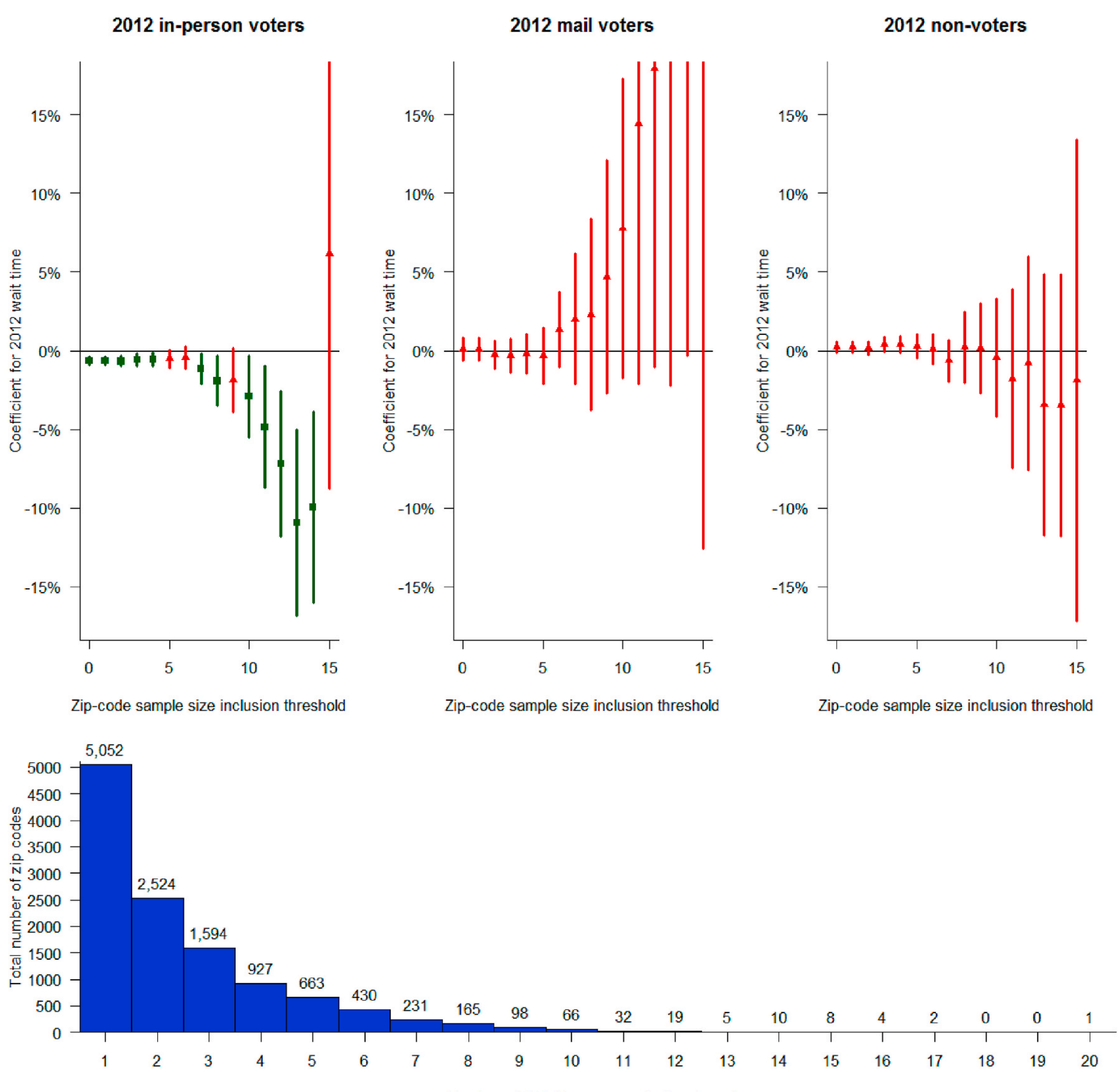


Fig. A2. Similarities of line length experience within various geographic units.



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

Fig. A3. Relationship between 2012 wait time and 2014 turnout, based on sample size thresholds that dictate whether a ZIP code is included in the analysis.[381]

Table A1
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 A2
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 A3
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 A4
Treatment and control group sizes

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

Table A5
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-squared	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 min.

Table A6
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	3595	3302.1	2928.6
Observations	41,618	7582	7128	5221
R-squared	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 min.

Table A7
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-squared	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 min.

Table A8
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 A9
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 A10
How did lines impact the mode of future voting among 2012 non-voters?

	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 A11
Effect of end-of-day lines in Boston on future turnout

	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-squared	0.6540	0.6175	0.2134	0.0362

*p < 0.05; **p < 0.01; ***p < 0.001.
OLS coefficients reported.

[38] 95% confidence intervals reported. Green intervals are statistically significant (p < 0.05); red intervals are not.

Table A12
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)	3334	3334	3334
Observations (unweighted)	3,012,356	3,012,356	3,012,356
R-squared	0.1520	0.1045	0.1608

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

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