

More Money, More Turnout?

Minimum Wage Increases and Voting*

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Abstract

Do minimum wage increases mobilize low-income people? We measure the effect of minimum wage increases on voting behavior in two ways. First, we merge public records of New York City municipal employee wages to voting records to observe voting by people affected and unaffected by the minimum wage across multiple elections. Difference-in-differences estimates indicate that recent increases in New York’s minimum wage increased voter turnout among low-income workers by several percentage points. Second, we incorporate county-level panel data from 1980-2016 and find that an eight percent increase in the minimum wage (the median increase in our dataset) is associated with a one-third of one percentage point increase in aggregate voter turnout. These results imply that economic policy can have democratic implications, with minimum wage increases also serving to increase turnout among low-wage workers and make the electorate more representative.

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1 Introduction

Since 2014, 27 states and the District of Columbia have increased their minimum wage rates, many of them in the wake of large public campaigns and even ballot measures to raise wages (Economic Policy Institute, N.d.). We often think of such minimum wage increases as the end of a long process of political mobilization. But what if they were just the beginning of the political story?

A rich literature on “policy feedbacks” describes how policies, once enacted, can produce ongoing political action. We might expect that minimum wage increases could have such an effect, through their effects on both economic resources and people’s views on government (Pierson, 1993). People may vote more because higher incomes mean that they are better able to vote: less likely to encounter life crises that prevent them from voting, and better able to bear the logistical costs of voting (Rosenstone, 1982; Denny, 2016). Or they could vote more because seeing the government impose a higher minimum wage drives home the importance of political engagement, or improves their sense of efficacy (Campbell, 2003; Lawless and Fox, 2001).

But past research on policy feedback has focused largely on tangible government-run benefits programs, such as Social Security or the GI Bill, concluding that these visible and empowering government interventions in people’s lives can produce dramatic shifts in political participation (Campbell, 2003; Mettler et al., 2005). The minimum wage does not fall into this category of benefits programs administered by the government; nor is it a part of the nearly-invisible “submerged state” that is thought to be ineffective at spurring the public to political action (Mettler, 2011). As such, it is not clear whether we should expect increases in the minimum wage to have any effect on political participation; existing theories of policy feedback have little to say about this particular policy realm.

Though minimum wage policies are widespread and politically-contested, we are aware of no attempts to empirically estimate the impact these policies have on political participation. In this project, we use two empirical approaches to explore the participatory effects of increasing the minimum wage.

First, we focus on individual workers. Using data on New York City municipal employees’ wages and public voting records, we trace these employees’ voting participation across several elections, between which there was an increase in New York’s minimum wage. This design allows us to use a difference-in-differences approach: we examine changes in minimum-wage workers’ turnout before and after the wage increase, and compare those changes to the behavior of higher-wage workers (unaffected by the minimum wage increase) over the same

time period.

We find that an hourly wage increase of up to \$1.75 increased voter turnout by several percentage points between 2012 and 2016, compared to what we would have expected absent an increase in the minimum wage. These effects are also visible in midterm elections, and are robust to a range of model specifications.

Next, we measure the aggregate effect of minimum wage increases on turnout using panel data. Here, we use county-level voter turnout estimates and minimum wage rates from 1980-2016 to estimate a first-differences model, using changes in the minimum wage from one election year to the next to predict changes in voter turnout. We find that an 8 percent increase in the minimum wage (the median increase over this period) is associated with a one-third of one percentage point increase in aggregate voter turnout. These estimates do not rely on potentially-unrepresentative public employees, nor are they narrowly focused on turnout effects among low-wage workers. They suggest that minimum wage increases have net participatory impacts large enough to be detected in overall turnout measures.

Taken together, these two sets of results paint a picture of minimum wage increases as a meaningful political force driving voting participation among low-wage workers. The analysis of individual-level municipal employee data allows us to carefully pinpoint the effect of wage increases on low-wage employees' turnout, while the panel data analysis suggests that these results generalize beyond public employees and that they are big enough to be reflected in overall turnout measures.

These results are both policy-relevant and theoretically interesting. Political scientists stand to learn about a different sort of “policy feedback” than is usually studied; rather than measuring the participatory impacts of high-profile government benefits programs, we are exploring a policy that is high-profile but not implemented by the government itself. This builds on other recent work, such as Feigenbaum, Hertel-Fernandez and Williamson (2017) and Akee et al. (2018), that highlights the political importance of economic policy.

More significantly, we note that minimum wage increases appear to specifically increase voting among people paid the minimum wage, who are usually under-represented in the electorate. This means that minimum wage policies, usually thought of as a tool to reduce economic inequality, could also serve to reduce political inequality by making the electorate more economically-representative of the American people.

2 Minimum Wages, Income, and Participation

As Caporale and Poitras (2014) point out, both the minimum wage and voter turnout are topics of paramount interest to social scientists, with thousands of articles listed in JSTOR and Google Scholar about each of them. Nonetheless, we know very little about the impact of minimum wage laws on voter participation. As dozens of states increase their minimum wages, it is worth knowing what the political impacts of these policy choices will be. In particular, could they drive turnout among otherwise low-propensity voters, and change the composition of the electorate in future elections?

Research on income (and other resources) and participation suggest that increasing the minimum wage should increase participation among affected workers. Cross-sectional studies have found that people with higher incomes tend to vote more than those with lower incomes (Verba, Schlozman and Brady, 1995; Rosenstone, 1982; McDonald, 2009), though such research rarely examines within-person changes in income.¹ As Rosenstone (1982) points out, people with very low incomes may be focused on “holding body and soul together,” not “remote concerns like politics.” Receiving a raise might allow some low-wage workers to attend to such remote political concerns. More recent work suggests that economic well-being can help make it possible for people to absorb shocks that might otherwise prevent them from voting, such as bad weather (Jae and Loose, 2011) or life crises (Denny, 2016).

Further, a change in the minimum wage does not only increase workers’ pay. It is also a meaningful political symbol that could well change their attitudes toward government. The literature on policy feedbacks describes not only the sort of economic “resource effects” discussed above, but also the possibility of “interpretive effects,” where exposure to government policy can change people’s attitudes (Pierson, 1993; Mettler and Soss, 2004). Receiving government benefits such as Social Security (Campbell, 2003) or GI Bill benefits (Mettler et al., 2005) is associated with greater civic engagement and stronger beliefs in one’s own “stake” in government. Similarly, we might expect that being positively affected by this policy (receiving a raise due to a minimum wage increase) could change people’s perceptions of government’s role in their lives.

However, the minimum wage is different from most policies studied in the policy-feedback

¹For some notable exceptions to the cross-sectional rule, see the studies of income shocks on political *attitudes* by Doherty and Gerber (2006), Brunner, Ross and Washington (2011), Margalit (2013), and Hopkins and Parish (2019). The only paper we have seen to measure income shocks’ effect on *turnout* in the United States is Akee et al. (2018), which relies on an unconditional cash transfer program and finds no turnout effect among adults receiving the transfers (but does see long-run effects on the turnout of children in these households).

literature. Canonical examples of policy feedback involve highly-visible government programs providing direct payments or services, such as Social Security, TANF (welfare), or Medicaid (Campbell, 2003; Bruch, Ferree and Soss, 2010; Michener, 2018; Baicker and Finkelstein, 2018). Meanwhile, less-direct forms of tax expenditure policies, such as the mortgage interest tax deduction, form a “submerged state” that does not seem to shape citizens’ attitudes, perhaps because they are not even seen as government policies (Mettler, 2011). Minimum wage laws are different from both these groups of policies: they are highly visible and clearly linked to government action, but they are not paid for or administered by government. If such policies impact voter participation, they could represent a new and less-well-understood form of “policy feedback.” Perhaps the most related work we have seen is Feigenbaum, Hertel-Fernandez and Williamson (2017) on right-to-work laws and their political implications. Our study adds to an emerging understanding of how government interventions in the labor market can shape political outcomes.

If minimum wage increases can drive voter turnout, via any mechanism, the next question is what such turnout would mean for election outcomes and policy. We note that low-income people generally participate in politics at lower rates than high-income ones (McDonald, 2009; Schlozman, Verba and Brady, 2012; Einstein, Palmer and Glick, 2017). This is true for voting, which means that the people who vote in any given election tend to have higher incomes than the full population of eligible voters (Leighley and Nagler, 2013). Some studies have linked this “class bias” in turnout to inequalities in representation, suggesting that more equal turnout rates would translate into policy outcomes that would be more liberal and more reflective of public preferences overall (Avery, 2015; Franko, Kelly and Witko, 2016).

If minimum wage increases stimulate voting among people earning the minimum wage, that should not only change overall turnout, but should also make the electorate more economically representative of the American people. The current study focuses on measuring that first step (increased turnout), but work on class inequality in politics suggests that increasing turnout among low-wage workers could potentially yield better representation and policy wins for these constituents.² This would represent a substantial political dividend from what is usually thought of only as an economic policy.

²That said, we note critiques from Hacker and Pierson (2010) and others that elections are not the affluent’s only source of influence over policy.

3 Individual Workers: New York City Municipal Data

Between the 2012 and 2016 elections, the minimum wage in New York City increased several times. A state law enacted in early 2013 produced staggered increases, with the minimum wage increasing to \$8 an hour at the beginning of 2014, \$8.75 in 2015, and \$9.00 an hour in 2016. Over this period, workers who had earned the previous minimum wage of \$7.25 per hour in 2012 received raises of \$1.75 per hour, a 24% increase in two years.

Among the people seeing pay increases were thousands of city employees who had been paid below the new minimum wage rates, from school classroom aides to parks and recreation workers. Municipal employees' pay is a matter of public record, and several years of detailed records (employees' full names, job titles, active-employee status, and pay rates) are available through the city's open data portal.³ We join this dataset to the New York State voter file⁴ to observe whether municipal employees voted.

We merge the municipal employees data to the list of New York City voters using employee/voter names. First, we merge exactly on first and last names, and discard matches where the middle initial are mismatched (though we allow matches between missing values and actual initials).⁵ Then, ties between potential matches (such as when one employee record matches to multiple voter records) are adjudicated using several additional pieces of information. We use employment start dates to narrow down matches: if an employee matches to multiple voter records, but some records have birthdates suggesting that the employee would have started working before they were 18, we discard those matches in favor of ones with more plausible hiring ages. We also discard matches in which the voter file record suggests the voter has been purged due to death at a time that the employee is still working, and preferentially choose matches in which employees match to voters under the age of 70.

³A dataset covering fiscal years 2014-2017 was downloaded from <https://data.cityofnewyork.us/City-Government/Citywide-Payroll-Data-Fiscal-Year-/k397-673e> in February 2018. We also collected equivalent data on fiscal years 2011-2013 through a freedom-of-information request, so our dataset covers municipal employees that worked for the city anytime between 2011 and 2017. Most of our analyses rely on people who began working for the city before the minimum wage began rising in 2014, but we include later employees in robustness checks.

⁴We use a snapshot of the voter file collected from the state in 2018, and focus on voters currently living in the five boroughs, or those who are recorded as having previously lived there, as the most likely matches to municipal employees. Most municipal employees live in the city. Expanding the merge area to include six additional counties that municipal workers may move to does not substantially increase the match rate, nor does it change the main estimates presented here.

⁵Since we are matching between two official records, we expect that many people will provide their full names in the same format to both their employer for payroll purposes and to election officials, making this approach fairly effective, if conservative. In the online appendix, we also present a probabilistic merge approach and reach broadly similar conclusions.

We then collapse any still-duplicated voter file matches, averaging their voter histories and using binomial draws to produce a predicted vote history for each employee with multiple matches. Section 1.1 of the online appendix presents this merge process in more detail.

The resulting sample includes 175281 hourly employees, 66% of whom match to the voter file. Consistent with the recommendations of Nyhan, Skovron and Titiunik (2017), we consider anyone who did not merge to the voter file to be a non-voter, rather than dropping them from the sample. Voter turnout within the sample (unconditional on registration) was 30% in 2012 and 32% in 2016. As expected, turnout was higher among higher-wage workers: 2016 turnout among workers paid under \$12/hour was 28%, while for workers paid more than \$12/hour it was 39%.

3.1 Main Individual-Level Estimates

One possible approach to measuring the effect of the minimum wage increase on voting would be to compare low-wage workers' turnout rates between 2012 and 2016. If turnout rose, we might conclude that the minimum wage increase boosted turnout. However, we might worry that the increase in voting wasn't actually caused by the wage increase: maybe election differences meant that everyone voted more between 2012 and 2016, regardless of whether they were affected by the minimum wage. Rather than simply making an over-time comparison, we use a difference-in-differences design. We compare the 2012-to-2016 increase in minimum-wage workers' turnout to the change in higher-wage workers' turnout (theoretically unaffected by the minimum wage increase) over the same period. This approach relies on the parallel-trends assumption; we assume that the turnout of high-wage and low-wage workers would have moved in the same ways if there had been no minimum wage increase. The baseline *levels* of turnout may be different across the groups (and indeed, they are), but we assume that they follow parallel trends.⁶

Table 1 and Figure 3.1 present the main estimates from regression models based on this difference-in-differences approach, beginning with presidential election years. To determine who was affected by the minimum wage increase, we focus on employees that worked for the city before the policy change. We identify employees who were paid below \$8/hour any time in fiscal years 2011-2014 as very likely to be affected by the minimum wage increases that began in January 2014.⁷ By this definition, about 17% of the hourly workers we observe

⁶We discuss parallel trends in more detail in Section 3.3, as well as Section 2.1 of the online appendix.

⁷We rely on workers that were employed with the city before the policy change so that we have a clearly pre-treatment measure of who is affected, but robustness checks including 2014-2016 municipal employees result in very similar estimates.

over this period were treated by the minimum wage increase.⁸ We regress our outcome of interest (voting) on an indicator for treatment, an indicator for the year in question (2012 or 2016), and the interaction between these two indicators.

Our quantity of interest is the interaction term between the treatment indicator (“Under New Minimum Wage”) and the indicator for the 2016 (post-wage-increase) election: this tells us the amount that turnout increased among affected workers due to the minimum wage change. The first column of Table 1 estimates the model on all hourly municipal employees, finding that the minimum wage increase boosted turnout by about two and a half percentage points among affected workers between 2012 and 2016. The second column restricts the sample to relatively low-wage workers (those making up to \$15 an hour), where we might think the parallel-trends assumption would be especially plausible, and finds a similarly-sized increase in voting of about 2.8 percentage points. We note that this difference-in-differences stems from a change in behavior among affected workers: unaffected workers’ turnout remained essentially flat (at 36%) between 2012 and 2016, while affected workers’ turnout increased by over two points over the same period.

Then, we repeat the same exercise, this time looking at voting in midterm and off cycle mayoral elections. The middle two columns of Table 1 present difference-in-differences estimates for the period from 2010-2014, and the last two columns present estimates for 2013-2017. The estimated turnout effects are similar in magnitude: 1-3 percentage points depending on specification. That said, they represent a larger percentage increase in turnout, because turnout in mayoral and midterm elections is lower than in presidential years. We urge caution in directly comparing the size of estimates across elections due to the various differences between them. First, affected workers had only received a 75-cent raise by the 2014 election (the minimum wage had increased from \$7.25 to \$8 at the beginning of 2014, and would continue to increase in 2015 and 2016). But there could also be differences in how accurately we have identified workers affected by the minimum wage increase at various points in time.⁹ Still, we note that these estimates suggest minimum wage increases can drive turnout change even in lower-turnout midterm elections, not just in presidential years.

⁸This does not necessarily mean that 17% of hourly municipal workers make minimum wage in any given year; we expect that minimum-wage positions have more turnover, so we see more individual people in these roles than in others.

⁹We focus on workers that were employed with the city by fiscal year 2014 to ensure that we have a measure of who is affected that predates the policy change. But over time, it is possible that some of the people we think of as “affected” by the policy change would have left their jobs or gotten raises unrelated to the minimum wage, potentially eroding the accuracy of our “affected by the minimum wage increase” classification.

Table 1: Main Individual Difference-in-Differences Estimates

	<i>Dependent variable:</i>					
	Voted in Presidential		Voted in Midterm		Voted in Mayoral	
	(1)	(2)	(3)	(4)	(5)	(6)
Under New Min. Wage	−0.197* (0.003)	−0.190* (0.003)	−0.111* (0.002)	−0.088* (0.002)	−0.099* (0.002)	−0.079* (0.002)
2016	−0.001 (0.001)	−0.005* (0.002)				
Under New MW * 2016	0.022* (0.002)	0.025* (0.002)				
2014			−0.039* (0.001)	−0.034		
Under New MW * 2014			0.025* (0.001)	0.020		
2017					−0.027* (0.001)	−0.023* (0.001)
Under New MW * 2017					0.018* (0.001)	0.014* (0.002)
Constant	0.359* (0.002)	0.352* (0.002)	0.180* (0.002)	0.156* (0.002)	0.159* (0.002)	0.139* (0.002)
Included Employees	All Hourly	Hourly Under \$15	All Hourly	Hourly Under \$15	All Hourly	Hourly Under \$15
Observations	223,114	175,450	223,114	175,450	223,114	175,450
R ²	0.022	0.025	0.014	0.012	0.012	0.010
Adjusted R ²	0.022	0.025	0.014	0.012	0.012	0.010

Note:

*p<0.05

3.2 Importance

It is hard to think through the political implications of our estimates without knowing how many people are affected by minimum wage increases. If a change to the minimum wage could boost turnout among affected workers by ten percent, but those workers only constituted one percent of the voting-eligible population, even that large increase in voting would not yield a large shift in overall turnout or the composition of the electorate. So how many people were actually affected by New York’s minimum wage increase?

We note that a 2012 report from the Economic Policy Institute estimated that a potential \$1.25/hour increase in the state minimum wage (as of late 2012) would directly affect 609,000 workers in New York, with as many as 473,000 also receiving indirect pay increases as employers shifted pay scales upwards (Cooper, 2012). It suggests that nearly one in ten workers in New York could be affected by such an increase. These estimates do not capture the exact plan that was implemented (which actually raised the minimum wage by a total of \$1.75 across three years), but we think it gives a back-of-the-envelope sense of how many people could have been affected across the state of New York.

If all workers affected by the minimum wage increase responded similarly to the municipal workers we studied above, then this wage increase could translate into as many as 26,000 additional votes cast by low-wage workers statewide during the 2016 election. In a state that cast nearly 7.4 million votes for president in 2016, this may not seem large, but we note that this approximately one-third-of-one-percentage-point increase in aggregate turnout could matter in close local elections, and that places with many minimum-wage workers would likely see larger turnout increases. Further, if minimum wage increases also affected the behavior of people close to minimum-wage workers, such as their families or neighbors, then we could imagine an even larger aggregate effect.

3.3 Robustness

The estimates presented in the last section suggest that increasing the minimum wage can drive voter turnout among affected workers. In this section, we explore how robust that conclusion is to various assumptions and data concerns.

Parallel Trends The difference-in-differences design used here relies on a parallel-trends assumption. Workers affected by the minimum wage increase and those unaffected by it may have different *levels* of turnout, but we assume that their over-time *trends* should be the same

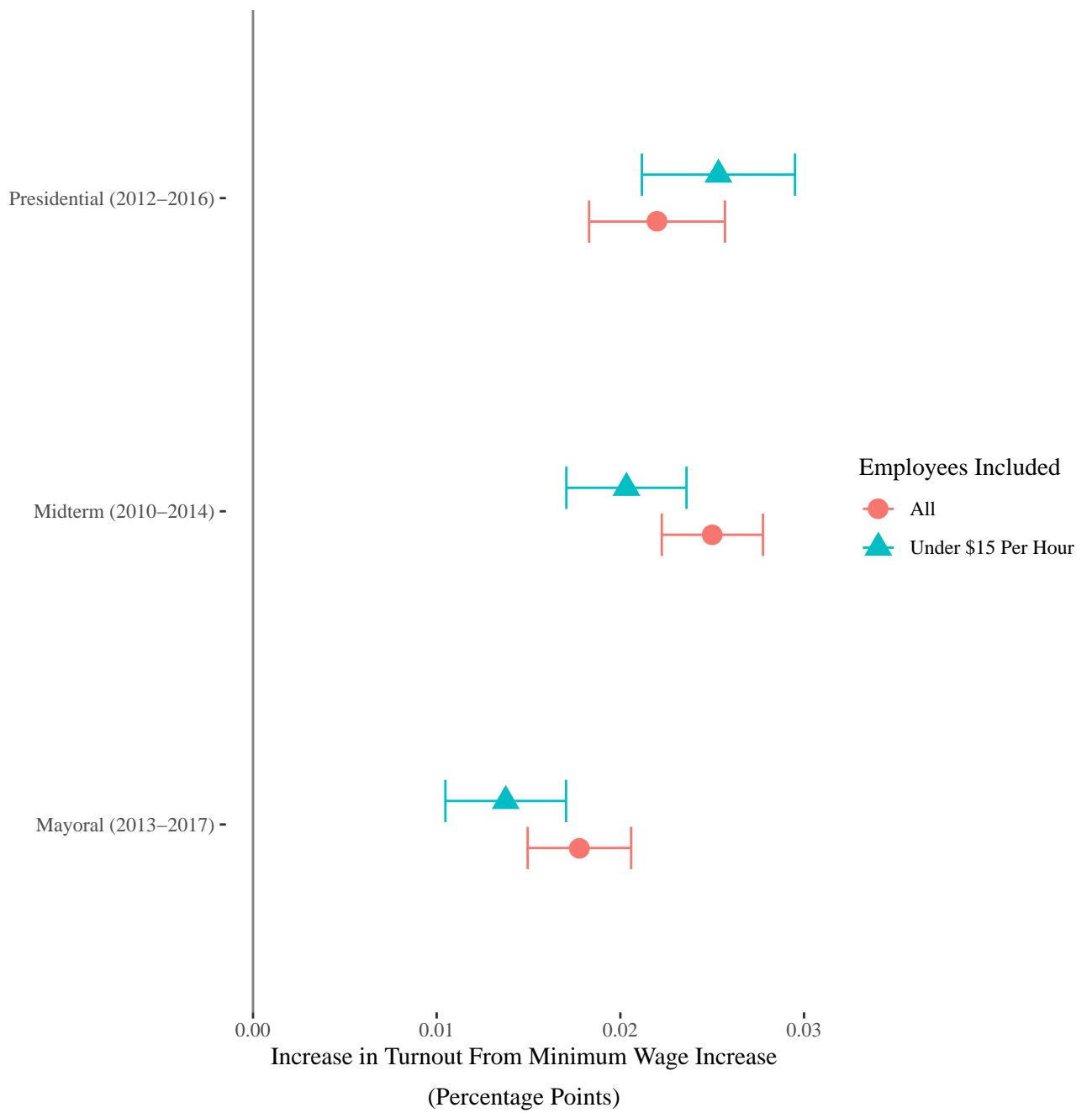


Figure 1: Individual Difference-in-Differences Estimates

in the absence of minimum wage changes. If that assumption did not hold, our estimates could be biased. We address concerns about this parallel trends assumption in several ways. First, we note that the estimates we find here are similar in direction and magnitude to the effects estimated in nationwide panel data in Section 4, which rely on a different design with different assumptions. Second, Section 2.1 in the online appendix presents plots of pre-treatment trends in voter turnout among people affected and unaffected by the minimum wage increase, demonstrating that their turnout trends looked similar before the minimum wage began going up.

Additionally, we conducted two sets of placebo tests that attempted to estimate logically-impossible “effects” of wage increases. Finding effects in these circumstances would shake our confidence in the main estimates; we find no such effects in either our “wage” or “time” placebo tests.

First, we conducted a “wage” placebo test to see whether there were some consistent difference in the turnout trends of people paid higher and lower hourly wages. If it were the case that lower-paid people would have had a more positive turnout trend than higher-paid people over the period we studied regardless of minimum wage policy, that could mean that our main estimates were spurious. We test for this possibility by estimating the “effect” of a series of arbitrarily chosen cutpoints among workers not affected by any minimum wage increase between 2014 and 2017. Specifically, we considered 1,000 meaningless cutpoints (all higher than the actual minimum-wage cutpoint) for each election year; for each, we repeated the analysis from Table 1 and defined the placebo treatment to be the set of workers making below that arbitrary cutpoint. Because we excluded all workers actually affected by a minimum wage increase from this sample, there is no reason to expect any systematic differences in turnout between employees making just above versus just below this cutpoint. But if there were some systematic difference between higher- and lower-income people that were unrelated to the minimum wage (a threat to our inferential strategy), we could see systematic effects in this test.

The distribution of the observed t -statistics from these placebo minimum wage increases is presented in Figure 3.3. The black vertical lines in the figure represent the observed t -statistics from our main analysis for that election year. Consistent with our belief that these arbitrarily chosen cutpoints are meaningless, the t -statistics observed in this placebo test are roughly standard normal distributed and centered on zero. For each year, statistically significant estimates are observed less than five percent of the time and in no case did the meaningless placebo minimum wage increase result in a t -statistic as extreme as those

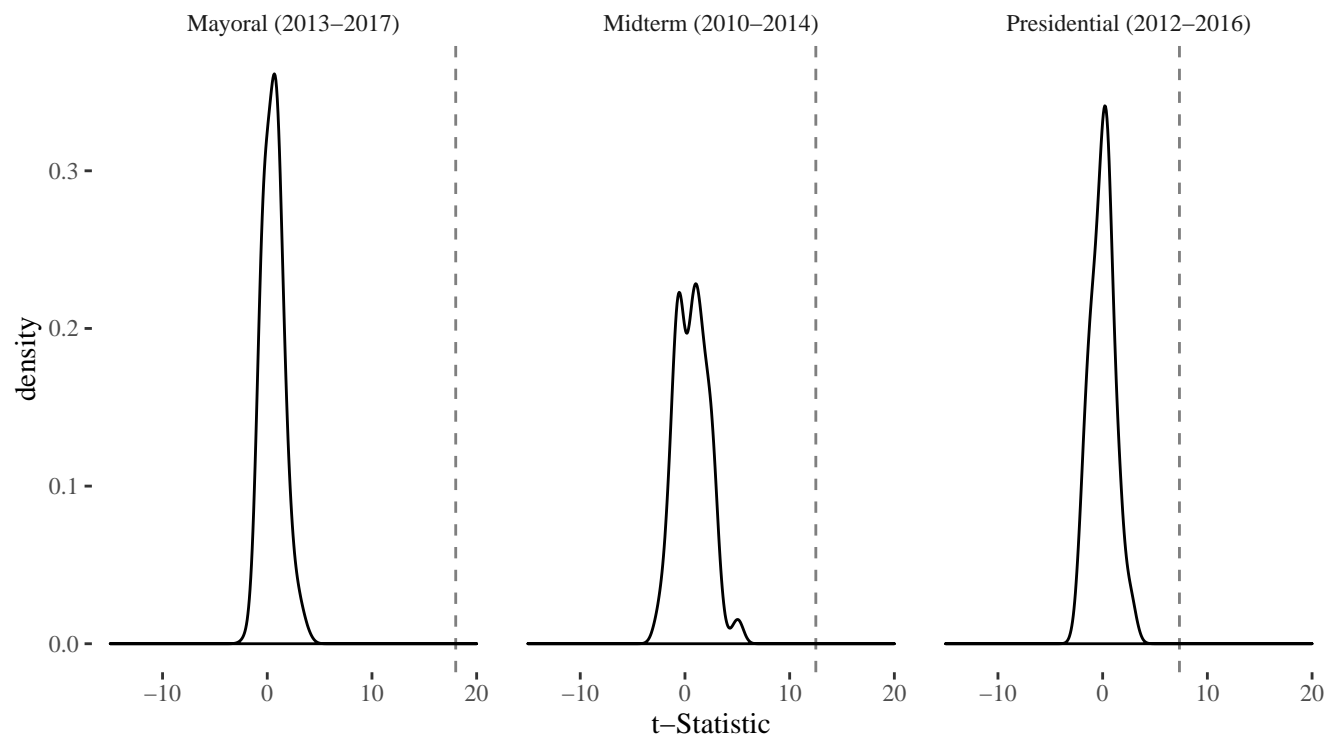


Figure 2: Distribution of t -statistics for Placebo Minimum Wage Increases. Vertical lines indicate the observed t -statistics from the actual minimum wage increases.

observed in table 1.

Second, we conducted a set of “time” placebo tests. In Section 2.1 of the online appendix, we run Table 1’s approach entirely during the pre-treatment period, looking at 2008-2012 changes in turnout among people who would eventually be treated by the minimum wage increase. For the 2008-2012 comparison, we find null results when estimating this impossible “effect,” which is reassuring. In general, we think these placebo test results suggest that the difference-in-differences approach is reasonable.

Merge Issues We have merged employee data to the voter file using only employee/voter names, as there are no dates of birth or addresses provided in the employee dataset. This approach is not ideal, as it allows for many duplicated matches for people with common names, as well as possible undermatches if names are recorded slightly differently across the two datasets. We have used other pieces of information from the voter file and common sense to rule out some implausible matches, such as active workers that match to dead voters. But it is still possible that flaws in the merge process are distorting our estimates of turnout.

For one thing, our approach to merging employee records to the voter file is almost certainly missing some genuine matches, as disparities between how names are recorded across the two datasets could lead us to miss matches.¹⁰ Similarly, we are surely producing some false matches, as different people could have the same name and thus be confused for one another. However, we note that unless such mistakes follow a complicated pattern, they should simply be a source of measurement error that should bias our estimates towards zero, making our results conservative. Figure 4 in Section 2.5 of the online appendix demonstrates this by randomly deleting some of the voter-file matches we do have, resetting people’s vote history to 0 for both 2012 and 2016 (since we assume that unmatched people did not vote, this is analogous to what would happen if we falsely missed a true match). We repeat this process for various proportions of the sample; the more voter-file matches we delete, the more our estimates are attenuated. Because of the nature of the difference-in-differences analysis, we note that slightly more complicated patterns of missed matches, such as disproportionately missing voter file matches for people affected by the minimum wage, also have the effect of attenuating the estimates.

A related concern is that receiving a raise due to the minimum wage increase might make people more (or perhaps less) likely to move than they otherwise would have been, and that

¹⁰In Section 1.3 of the online appendix, we instead use the probabilistic matching approach of the `fastlink()` package, and continue to find estimates consistent with our main conclusion that increases in the minimum wage drive voter turnout among low-wage workers (Enamorado, Fifield and Imai, 2019).

this shift could make them more difficult to find in the voter file than other workers. We do not think that such a pattern is likely to be driving the main results presented here, for several reasons. First, as noted above, a failure to merge a given person to the voter file should cause us to understate their voter turnout across all elections, but it should not yield a positive or negative trend in turnout: missing a match is the same as simply setting their vote history across all years to zero. This kind of measurement error should, if anything, bias us against finding an effect of minimum wages on voting. Further, supplementary analyses that re-run our main analysis on just the subset of employees that were found in the voter file continue to find positive and significant effects on voter turnout.

Other Specifications The results presented here are also robust (they remain positive, statistically significant at $p < .05$, and of similar magnitude) to a range of other specification and merge changes.

Our main specification focuses on employees that worked for the city by 2014, such that we have a pre-wage-change estimate of who is affected by the wage increase. However, extending the data to include workers employed by the city between 2014 and 2016 (but not before 2014) does not substantially change the estimates.

Similarly, the main specification focuses on (pre-2014 hired) employees making under \$8 per hour, considering them “affected” by the minimum wage changes. This approach captures people who were affected by all three minimum wage increases 2014-2016, but could potentially miss some employees who made more than \$8 but less than the eventual 2016 minimum wage of \$9 per hour. In practice, the estimates are not particularly sensitive to this choice of cutpoint; setting it at \$9 yields similar estimates, likely because so many of the affected workers we observe are making exactly the minimum wage.¹¹ We also acknowledge that it is possible that some of the workers we consider “untreated” actually received raises as an indirect result of the minimum wage increases; workers covered by certain types of collective bargaining agreements may get a raise to ensure that they are still paid more than their colleagues on a lower rung of the pay scale. To the extent such knock-on raises occurred, we believe they should give the estimates here a conservative bias.

¹¹We have also considered running a regression discontinuity design that would compare people just above the minimum-wage cutpoint to those just below. However, there are few people very near the cutpoint in our data. Further, this setup has the strange feature of the treatment varying with the running variable: that is, exactly at the cutpoint, no one is actually “treated” with a raise. People previously paid just one cent below the new minimum wage would receive a miniscule raise. It is only as we substantially broaden the bandwidth that people in the treated group actually receive substantial wage increases, and at that point it is no longer clear what the value of the RD setup would be.

Our main merge approach focuses on voters in the five boroughs of New York City because most municipal employees live there, but we have also tried increasing the geographic area in which employees could match to voters to include six additional counties where long-time employees are allowed to live. This approach barely increased the proportion of hourly employees that merged to the voter file or the overall estimated rates of voter turnout, and did not substantially change the main estimates.

Employment Effects Some economists predict that minimum wage increases could yield higher unemployment among low-wage workers (Brown, 1999), though recent research rules out large employment effects (Dube, Lester and Reich, 2010). What would it mean for our results if New York’s minimum wage increase caused some people in the sample to lose their jobs?

We note, first, that the design used here estimates an average treatment effect for a set of people who were employed by the city before the minimum wage began to rise. This means that anyone who loses their job after the minimum wage goes up will still appear in our voting data for both 2012 and 2016, regardless of whether they are still employed by the city. And any changes in turnout due to unemployment will be averaged into the treatment effect estimated here. We suspect that any such unemployment effect would tend to make the net effects smaller, as unemployed people are generally less likely to vote (Rosenstone, 1982; Incantalupo, N.d.).¹²

That said, we do not believe that the increase in minimum wage rates studied here translated into a substantial job loss. In addition to the existing economic work on unemployment effects, we have analyzed the payroll records used in this project to calculate the total numbers of hours worked by minimum-wage affected workers between 2013 (pre-increase) and 2016 (post-increase).¹³ The total number of hours worked by workers paid at or near the minimum wage did not decrease over the period of minimum wage increases, as we might expect if there were an employment effect.

¹²Though Burden and Wichowsky (2014) finds that higher unemployment rates are associated with more voter turnout, those are aggregate estimates based on a theory of all voters (not just unemployed ones) being mobilized by information about a worsening economy. We do not think this dynamic is likely during the period we study (in 2016, unemployment in New York State was below 5%), nor does it suggest that people who lose jobs should be especially likely to vote.

¹³Here, we sum up the total number of hours worked each year by people paid up to (and including) \$9/hour, as that would be the minimum wage as of 2016. We do this rather than tracking individual employees’ hours over this period because we anticipate that low-wage jobs always have a high rate of turnover over a multi-year period.

Generalizeability These results are based on one minimum wage change, affecting a subset of one city’s municipal workforce. The next section turns to a national panel dataset to look for results in other contexts, but it is also worth thinking through the ways in which this section’s analysis may not generalize.

There is some demographic information available about New York’s municipal workforce¹⁴, but relatively little on their political engagement. One potential concern is that government workers might be more politically-attentive than private-sector workers, and thus more responsive to policy changes like an increase in the minimum wage. We do not have survey data or other evidence that allows us to fully rule out such a difference, but we note that the hourly workers we focus on in this analysis are generally working in non-political roles. That is, we could imagine a high level of political engagement among legislative staff or mayoral appointees, but such workers are generally salaried. Common job titles among the hourly workers we focus on in this analysis include “Full Time School Lunch Helper,” “City Park Worker,” and “Assistant City Highway Repairer.” We think it is unlikely that people in these roles are much more politically-minded than any given hourly worker in the private sector.

In fact, most mobilization for higher pay or better labor practices for low-wage hourly workers in recent years has taken place in the private sector, with the most notable example being the fast-food-worker strikes and protests that kicked off the “Fight for \$15” movement. A related concern might be that such mobilization could be driving the results we report: is it possible that minimum-wage workers were mobilized not by the 2014-2016 wage increases, but by labor organizing that sought to engage low-wage workers? We note that although some protests associated with the Fight for \$15 began in 2012, the movement received limited media and public attention until a few years later. A Google Trends search for web traffic in the New York City area indicates that “Fight for \$15” was the subject of many searches beginning in 2015 and thereafter, with relatively little interest before that. Such a pattern suggests that the movement is unlikely to have driven the results presented here, particularly the ones focused on midterm election (2010-2014) changes.

¹⁴A 2015 report notes that the city’s workforce has a higher proportion of workers that are women (56%) and that are Black (32%) than the city’s working population as a whole, and fewer Hispanic workers (21%)(Department of Citywide Administrative Services, 2015).

4 Panel Data

In the previous section, we presented estimates suggesting that New York’s \$1.75 increase in the minimum wage had increased voter turnout among minimum-wage workers by several percentage points. But these results are based on a sample of municipal employees in one city. We might wonder whether these results would generalize beyond this group to most or all minimum-wage workers in the United States. Further, would any resulting changes in minimum-wage workers’ voter turnout be large enough to make a difference for aggregate voter turnout? To begin addressing these questions, we turn to a nationwide dataset of county-level voter turnout in presidential elections and minimum wages from 1980 to 2016. This allows us to examine hundreds of state minimum wage increases over the last few decades and their relationship to overall voter turnout.

We construct this panel based on several data sources. For estimates of how many people voted in a given election, we rely on the CQ Elections database. In order to turn these raw vote counts into measures of voter turnout rates, we use estimates of the voting age population from the Census Bureau.¹⁵ Information on state minimum wages came from the Department of Labor and the Correlates of State Policy Project (United States Department of Labor, 2018; Jordan and Grossman, 2016).

We then use this dataset to estimate the following model:

$$\Delta \text{Turnout} = \beta_0 + \beta_1 \frac{\Delta \text{Min Wage}}{\text{Lagged Min Wage}} \quad (1)$$

Essentially, we are using proportional changes in the minimum wage to predict local changes in voter turnout over the next election cycle (similar to a first-differences approach). By using the change in turnout as the outcome variable, we are able to eliminate the threat of time invariant confounders. For example, it might be the case that states that increase the minimum wage do so because of regional influences or the presence of a strong local Democratic party organization. First differencing is able to eliminate such a confounder under the assumption that such an influence is time invariant. An alternative approach for accomplishing the same goal would rely on county or state fixed effects instead; Section 3.3 of the online appendix demonstrates that such an approach yields comparable estimates to those shown here. We focus on proportional minimum wage increases (the difference between

¹⁵We use the Census estimates of the population over 19 archived at <http://data.nber.org/census/popest/>; in recent years, this comes from the American Community Survey, and in years before 2005 we use intercensal estimates. It would be preferable to use a measure of voting-eligible population that included citizenship information, but such estimates are not available for the earlier years of our panel.

the new and old wage, divided by the old wage) because nominal minimum wage increases have gotten progressively larger over the course of our study period.¹⁶ Section 3.2 of the online appendix also presents an alternate specification using the real minimum wage and reaches similar conclusions.

4.1 Main Panel Estimates

We estimate the effect of minimum wage increases on voter turnout in presidential elections (1980-2016). Table 2 and Figure 3 present results from several specifications. Standard errors are clustered at the state level in all specifications.

All of the estimates in Table 2 yield similar interpretations. Column 1 shows the simplest specification, using the proportional change in turnout over the last election cycle to predict the change in turnout from the last presidential election (at the county level). This model suggests that doubling the minimum wage,¹⁷ were we to observe that occurrence, would be expected to yield a four-percentage point increase in a county’s aggregate voter turnout. A more useful substantive interpretation comes from considering the median increase observed in our data, which is an 8% boost in the minimum wage. The estimates in Column 1 indicate that that such a change would translate into a increase in aggregate turnout of about .3%.

One concern with the specification used in column 1 is that there might be unobserved time varying confounding. For example, declining income in a state might cause more demands for a higher minimum wage and also suppress turnout among low income voters. To control for such an effect, column 2 introduces linear county time trends in turnout and observes nearly identical results to those seen in column 1, with larger standard errors due to the degrees of freedom consumed by fitting a time trend unique to each county. In Section 3.5 of the appendix we consider models that further loosen the assumption of no time invariant confounding by including quadratic county time trends, and again observe similar results.¹⁸

¹⁶In 1980, a 10- or 20-cent increase in the minimum wage was relatively large, while in 2000 such an increase would represent a relatively small raise compared to people’s baseline pay.

¹⁷Specifically, the ratio ((new wage - old wage) / new wage) moves from 0 (no wage increase) to 1 (100% wage increase), meaning that the coefficient represents the change in voting from doubling the minimum wage.

¹⁸An alternate strategy for controlling for unobserved time variant confounding would be through the use of year fixed effects. Because federal minimum wage increases cause minimum wage increases to be heavily concentrated within a single year and are the cause of most minimum wage increases early in our dataset, we consider the use of year fixed effects to be inappropriate in this model. Nonetheless we present the results of such a two-way fixed effects estimator in section 3.3 of the online appendix and observe a positive but non-statistically-significant result. It is also worth emphasizing that there are many confounders that the use of county time trends is able to account for, but year fixed effects cannot. In particular, confounders that vary over time within counties (such as increasing liberalism within a county) can be controlled for by

Column 3 restricts the analysis to bordering county pairs that straddle a state boundary. This strategy exploits the geographic discontinuity in economic policy that occurs at state borders to generate estimates of the effect of minimum wage increases on turnout that avoid some threats to causal inference from differences between the counties being compared. This strategy represents the dominant approach for studying the economic effects of the minimum wage (Dube, Lester and Reich, 2010) and effectively generalizes the classic difference in differences approach of Card and Krueger (1994). In political science, approaches premised on geographic discontinuities at state borders have been popular approaches for isolating the effect of public policy on political outcomes (Feigenbaum, Hertel-Fernandez and Williamson, 2018; Clinton and Sances, 2018; Keele et al., 2017; Keele and Titiunik, 2015). While there are certainly other policy and political differences that occur at a state boundary, we consider it unlikely that such differences correlate strongly with the magnitude of minimum wage increases. Regardless, counties which share a border are almost certainly more similar than ones that do not. Consequently, we find it reassuring that such an approach generates an estimate for the effect of the minimum wage on voter turnout very similar to that generated by the other two approaches.

All of the approaches presented in Table 2 suggest that an 8% increase in the minimum wage (the median increase observed in our dataset) would translate into a increase in aggregate turnout of about one-third of one percentage point. An increase of less than one percentage point may sound small, but we note that this is an effect on aggregate voter turnout, not a local effect among people targeted by the minimum wage. To yield a similar effect on turnout through get-out-the-vote activities, we would likely have to send non-partisan direct mailers to nearly every eligible American voter, or to send volunteers to knock on literally millions of doors (Green and Gerber, 2015, p. 196). This is a fairly large increase in turnout to come about as a side effect of economic policy.

We note that using aggregate data does not allow us to directly attribute turnout changes to specific types of voters, but we think it is likely that most of this change is driven by affected voters. The share of potential voters affected by a minimum wage increase depends on the size of the increase and the local wage distribution. We think it is reasonable to assume that 5 to 10% of the population would be directly affected by an average wage increase. If we assume that the bulk of the turnout increase is driven by these directly-affected workers, then an 8% increase in the minimum wage would translate into an increase of about 3.5 to 7

using county time trends, while a year fixed effects model can only account for shocks common to the entire country.

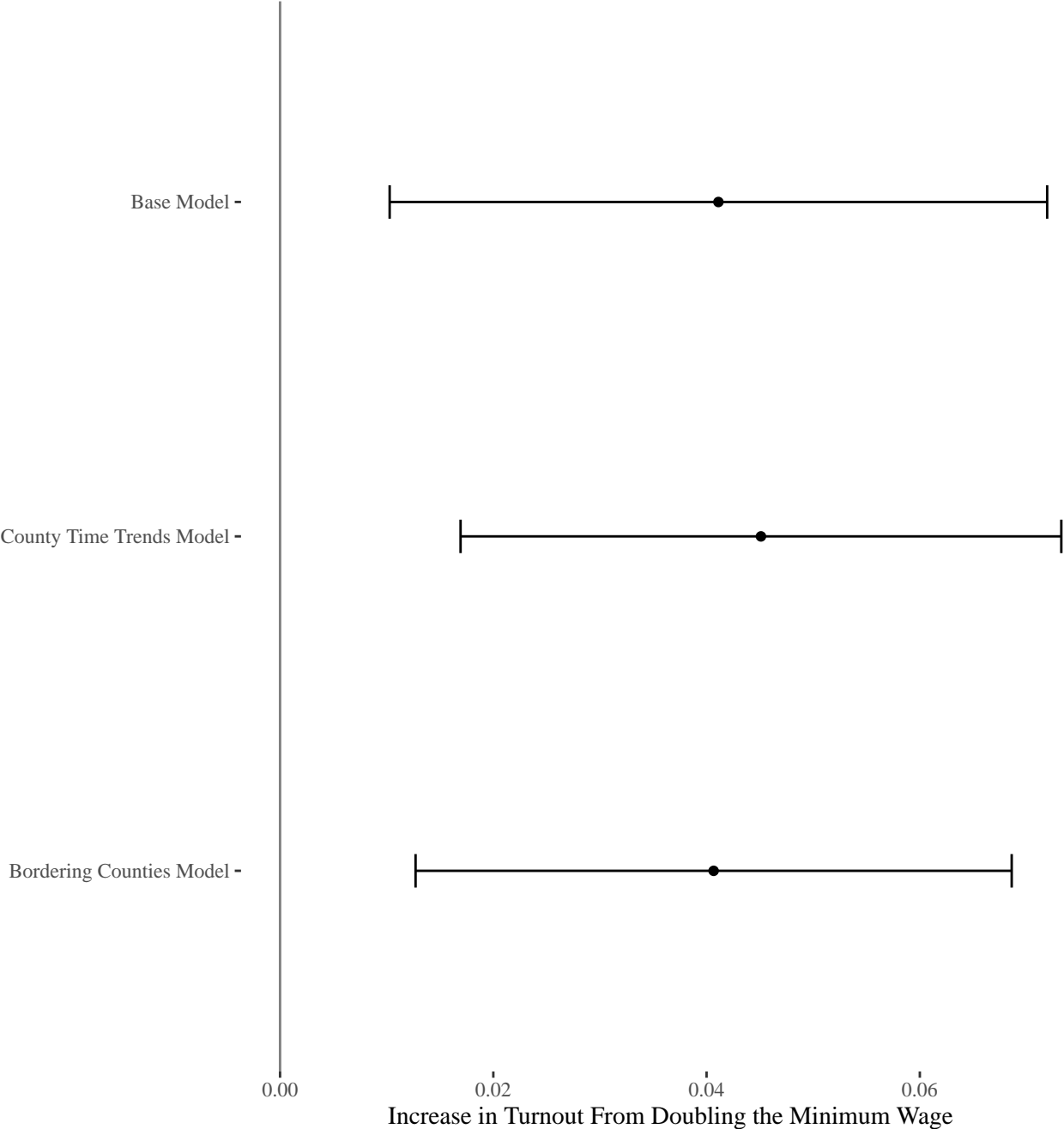
Table 2: Main Estimates from Panel Data

	<i>Dependent variable:</i>		
	Change in Turnout		
	(1)	(2)	(3)
Min Wage Proportional Increase	0.041* (0.014)	0.041* (0.016)	0.045* (0.014)
Intercept	-0.006* (0.001)	-0.803* (0.181)	
State Fixed Effects	No	Yes	Yes
County Time Trends	No	Yes	Yes
Bordering County Pair Fixed Effects	No	No	Yes
Observations	25,061	25,061	9,557
R ²	0.007	0.051	0.053
Adjusted R ²	0.007	-0.068	-0.071
Residual Std. Error	0.054 (df = 25059)	0.056 (df = 22275)	0.059 (df = 8452)

Note:

* $p \leq .05$
 SE's clustered by state

Figure 3: Main Panel Estimates



percentage points in their turnout.¹⁹ If we imagine that some other people may be affected by the wage increase, such as family members of minimum-wage workers, then the implied effect on individual-level turnout could be smaller. These loose estimates are slightly larger than the ones found in the individual analysis in Section 3, but point in the same direction and are not hugely different in magnitude.

Mobilization One particular concern about time-varying confounding arises from stories about political mobilization. We might worry that our results are not actually driven by minimum wage increases themselves, but that the political mobilization of low-wage workers to fight for minimum wage increases could be driving both the policy change and increased turnout in the next election. Here, the omitted time-varying variable of “political mobilization of low-income people” would cause us to overstate the true effect of minimum wage increases. To explore this possibility, we add a variable into our model that captures whether a given minimum wage increase was due to federal minimum wage changes, or was determined at the state level. The intuition here is that stories about mobilization as a confounding variable should mainly apply to state-level minimum wage changes. If we still see turnout effects from federally-determined minimum wage increases, we should be less worried about possible confounding from mobilization. And indeed, when we run a model that interacts the minimum wage increase with an indicator for whether the increase was federally-determined, we find that both federal- and state-generated minimum wage hikes are associated with substantial turnout increases.²⁰ Such a pattern should reassure us that the main estimates are likely not driven entirely by confounding from political mobilization. Conversely, that we also see effects when focusing on state-determined minimum wage increases should reassure us that the results are not being driven by a few national-level coincidences in which a federal minimum wage increase happened to coincide with a national trend in turnout.

Alternate Panel Specifications Aside from the first difference model considered here, we also present results for a number of other potential model specifications in the online appendix and find similar estimates. More specifically, Section 3.6 demonstrates that we find similar estimates when collapsing the data to the state level rather than running county-level models with standard errors clustered on state, Section 3.7 includes a lagged dependent variable as

¹⁹The math here is: coefficient of .044 from Table 2 multiplied by a median minimum-wage increase of .08 and then divided by .05 or .1 as our estimate of the proportion of the population directly affected by the wage increase.

²⁰This regression table appears in Section 3.9 of the online appendix.

a covariate rather than first differencing, and Section 3.8 uses a marginal structural model to control for the possible effects of earlier minimum wage increases on later ones.

5 Conclusion

Existing research on policy feedbacks gives us a rich understanding of how certain types of government policy shape people’s political behavior. Visible public benefits programs are well-theorized, as are “submerged” spending programs that are not visible to the public. But existing policy feedback work does not offer a clear prediction about policies like the minimum wage, which is not a government benefits program yet is extremely visible. As it combines features of programs that both are and are not thought to have participatory effects, it was unclear whether the minimum wage should be expected to drive political participation or not.

We have measured the effect of minimum wage increases on voter turnout in two different ways, and both suggest that increasing the minimum wage can also boost turnout among low-wage workers. This effect is visible at the individual level when using administrative data on wages and voting, and it is also apparent in aggregate local turnout counts when we use county panel data. These findings help to broaden our understanding of where policy feedback effects can be expected, indicating that even non-benefits programs can spur participation if they change people’s material circumstances in a noticeable way.

In addition to teaching us about how policy feedback operates across different policy realms, these findings about the minimum wage have clear political implications for the current moment. They suggest that minimum wage policy has not only economic, but also political effects. Low-income Americans generally vote less than their higher-paid compatriots, so increasing their turnout has the effect of making the electorate more representative. Given that low-wage workers also have different political attitudes than higher-paid ones, such a shift could matter for representation and future policy outcomes. Minimum wage laws are generally thought of as a tool to combat economic inequality, but they appear to also reduce political inequality along economic lines.

Ours is an era of widely divergent minimum wage laws, with some states and municipalities seeking to implement “living wage” rates while the federal minimum wage remains stagnant. We anticipate that upcoming state and local minimum wage changes will only increase these differences. This pattern could mean that electorates in some places will become more economically-representative, while other places see large and growing turnout inequal-

ity along economic lines. It remains to be seen whether the patterns of voter participation we have documented here will translate into observable differences in future economic policy.

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Online Appendix for “More Money, More Turnout? Minimum Wage Increases and Voting”

October 30, 2019

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1 Municipal Employee Merge (NYC)

1.1 Detailed Description of Municipal Employee Merge

We begin with municipal payroll records from New York City from 2011-2017 in several files. We align the column names and formatting across these files and combine them into a single set of payroll records, in which each observation represents a single employee-appointment-year (that is, employees may appear multiple times in the dataset, either because they work for the city for multiple years, or because they hold multiple jobs in the same year). We retain only observations where the pay basis reported is “per hour”, dropping observations where people are paid a yearly salary or wage per day. There are 658596 such hourly observations between 2011 and 2017.

We then restrict this dataset by dropping observations where the reported hourly pay is less than \$7.25 (the minimum wage in 2011), as these observations seem to represent appointments for positions like poll workers (\$1.00/hour) or foster family members, not traditional paid employment. This leaves us with 441873 employee observations. We then reshape this dataset to “wide” format, using employee full name (first/last), agency name, and employee start date as a key, to find individual employees (that is, we want to have only one observation per employee, even if they worked for the city for multiple years). This yields a dataset of 175281 hourly employees who worked for NYC during at least one year between 2011-2017, with information about their pay and job title during each of these years.

We use a copy of the voter file collected from New York State in 2018¹, and restrict it to voters living in the five boroughs of New York City (we anticipate that the vast majority of municipal employees, particularly those paid hourly, will live within the city). Out of concern that people who used to work for the city might have moved out of NYC by the time we collected the voter file, we also include anyone marked as having a “former county” in the five boroughs (we note that this is probably incomplete, as election officials do not always use this field to register within-state movers, but it is our best way of identifying movers from this file). This yields a dataset of 6810818 current or former NYC voters.

We begin by merging the employee data into the voter file, requiring an exact match between employee/voter first and last names. This yields 1822848 potential matches, with many duplicates. We then narrow down some of these matches. First, we discard any

¹We previously did this exercise with a voter file from 2017 and found extremely similar match rates and difference-in-difference estimates, but have updated to the 2018 file in order to include an analysis of voting in the 2017 mayoral election.

matches that are implausible due to mismatched middle initials across records (still allowing records with a missing middle initial to match to those with an existing initial). This reduces the number of matches to 1121705.

We then discard matches that are implausible due to youth or death: we drop matches where the date of birth on the voter record implies that an employee began working for the city before the age of 18, or where the voter record indicates that a voter was purged (due to death) at a time they were still working for the city. This reduces the number of matches to 1043624.

Then, we discard duplicate matches based on the age of the employee (calculated from the date of birth on the voter record). If an employee has duplicate voter matches, and at least one of those matches is below 70 years old, we discard potential matches that are over 70 years old, on the assumption that it is relatively uncommon for people over 70 to work for the city. This reduces the number of potential matches to 1102627.

Finally, we collapse any further duplicated matches to the voter file, taking an average of each year’s vote history for a given employee’s potential voter matches. We then take a single draw from a binomial distribution to yield our guess at whether the employee voted in an election. For example, if an employee matched to three different voter records, two of whom had voted in 2012 and one of whom had not, we would take the average of those voter histories ($2/3$) and use that as the probability for our binomial draw (which would yield a vote history of either 0 or 1, with a vote probability of $2/3$).

This collapsing process yields 115427 matches from the voter file, each of which represents a match to an individual employee. We then merge these matches back to the entire dataset of hourly workers, yielding a dataset of 175281 people who worked for the city between 2011-2017. 66 percent of those have at least one plausible match to the voter file, with unmatched employees assumed to be non-voters.

1.2 Plot of Matched Voters

As one check on the merge to the voter file, we have plotted the neighborhoods where matched minimum-wage voters (registered voters who were matched to a municipal worker who was affected by the minimum wage increase) live in Figure 1. This plot is based on the state assembly districts where voters live (the NYS voter file does not include precise latitude/longitude coordinates, but does include these relatively small legislative districts). Within each district, points are scattered at random, with each point representing 10 voters.

We note that this plot looks about as expected, with low-wage workers clustered in

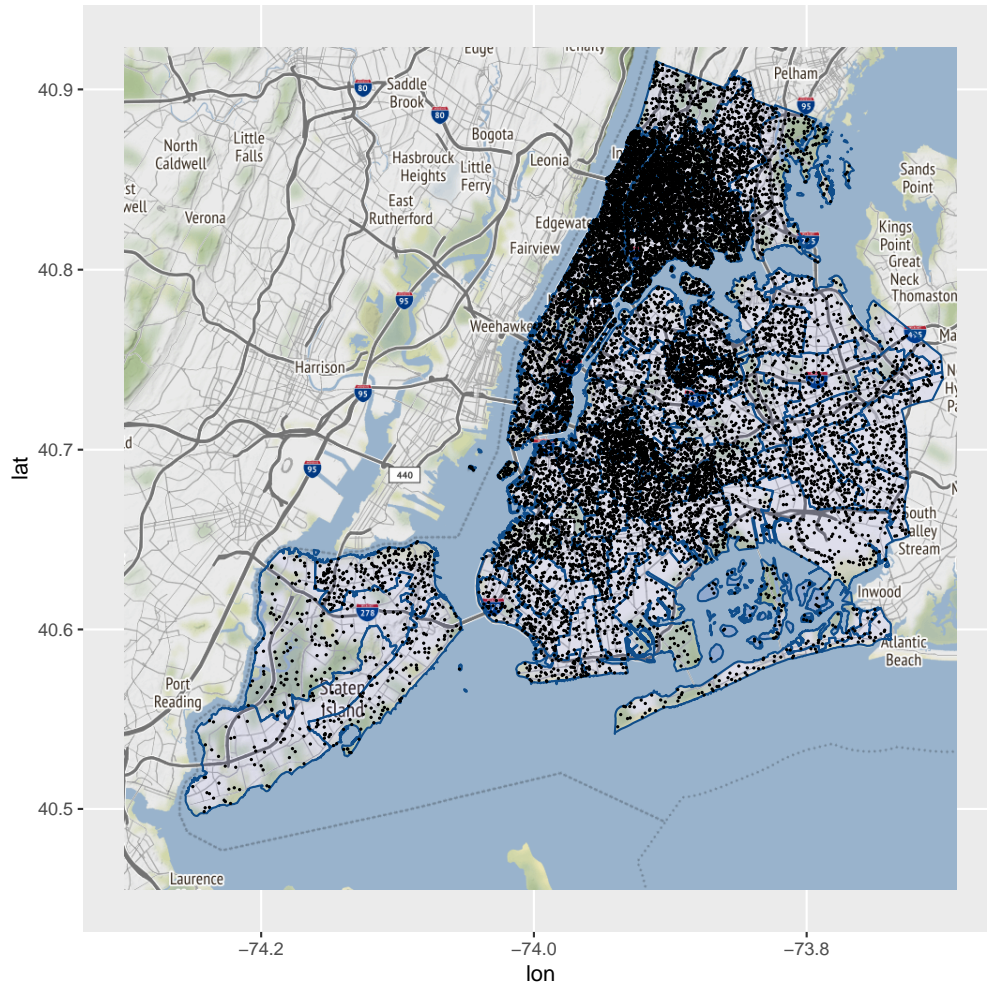


Figure 1: Map of registered voters matched to minimum-wage-affected workers.

relatively lower-rent parts of the city (the Bronx, Harlem, parts of Queens). The distribution of matched voters closely mirrors a map of the areas with the lowest median income in New York City (see an example here: <http://communitystudies.qwriting.qc.cuny.edu/files/2011/11/Median-Income-2010.jpg>).

1.3 Probabilistic Merge using Fastlink

One approach for improving the coverage of our merge is to use a fuzzy merge procedure. To do this, we used the probabilistic merge implemented by the fastlink package (Enamorado, Fifield and Imai, 2019). Due to the large scale of the merge involved, we restricted the matches to be between voters and employees whose first names started with same letter, as Enamorado, Fifield and Imai (2019) suggest when merging to a national voter file. We also weighted this analysis in this section by the probability that a merge is correct so that merge uncertainty is incorporated into these models, as suggested in Enamorado, Fifield and Imai (2019, p. 357).

We present the results of this analysis in table 1. These results are directionally consistent with the main estimates, though they suggest a somewhat larger effect than does the main table; we think this is attributable both to a reduction in missed matches and to the weighting procedure used here.

Table 1: Main Individual Difference-in-Differences Estimates – Fastlink Merge

	<i>Dependent variable:</i>			
	Voted			
	(1)	(2)	(3)	(4)
Under New Min. Wage	−0.179*** (0.004)	−0.164*** (0.004)	−0.136*** (0.003)	−0.102*** (0.003)
2016	0.006*** (0.002)	−0.004* (0.002)		
Under New MW * 2016	0.130*** (0.006)	0.139*** (0.006)		
2014			−0.051*** (0.001)	−0.045*** (0.002)
Under New MW * 2014			0.044*** (0.005)	0.038*** (0.004)
Constant	0.520*** (0.001)	0.504*** (0.001)	0.261*** (0.001)	0.227*** (0.001)
Included Employees	All Hourly	Hourly Under \$15	All Hourly	Hourly Under \$15
Observations	361,602	293,512	361,602	293,512
R ²	0.006	0.006	0.010	0.008
Adjusted R ²	0.006	0.006	0.010	0.008

Note:

*p<0.05

2 Additional Municipal-Employee Analyses

2.1 Parallel Trends and Placebo Tests (NYC Data)

Figure 2.1 below plots pre-treatment trends in voter turnout among people affected and unaffected by the minimum wage increase. We urge caution in using past records of turnout to evaluate parallel trends, both because looking at long-past turnout from a given snapshot of the voter file can yield strange results, and because there have been other minimum-wage increases over this period (for example, in 2005-2007, there were three increases that brought the minimum wage up by a total of \$2) that may have affected unobserved subsets of these groups. However, we plot the trends as a first step.

They look relatively parallel; if anything, it seems that turnout among the “unaffected” group drops off slightly more steeply as we go farther back in time. This is not especially surprising, given that older records of voter turnout may be incomplete (due to people having moved to the city from elsewhere) or may predate young workers’ eligibility to vote. If we think that the unaffected and affected groups face this record-slippage problem at about the same rate, we should expect steeper dropoffs in turnout among the unaffected group (the intuition here is that baseline turnout is higher among this group when they are observed, so losing any given observation and filling it in with a 0 pulls down the mean more). This is a problem of older records and should be less relevant for the recent (2010-2016) turnout data we use for our main analyses, but if there were some sort of systematic “steeper slope” to turnout in the unaffected group, it would tend to make our estimates more conservative.

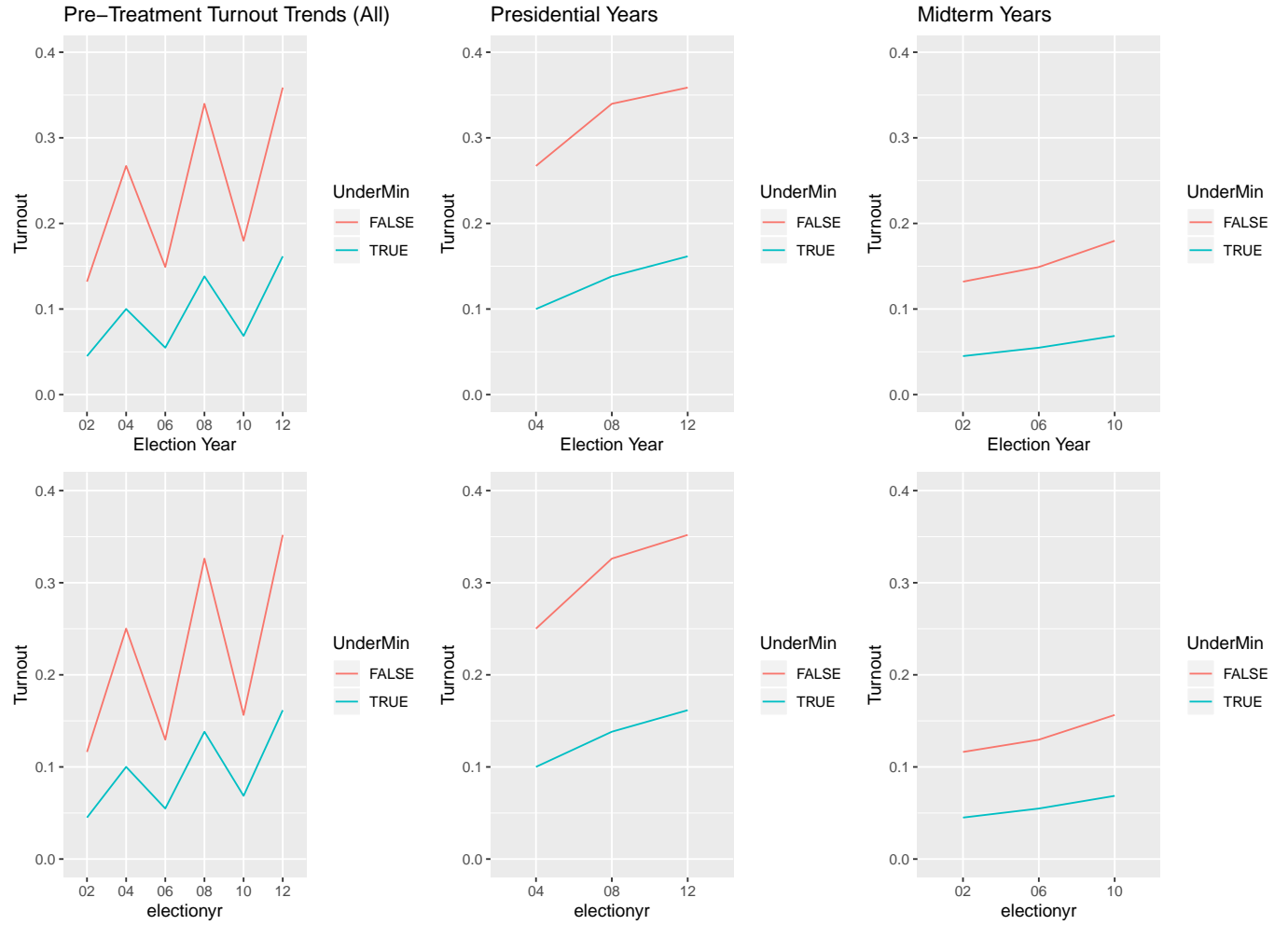


Figure 2: Plots of pre-treatment voter turnout trends among municipal employees affected and unaffected by NY minimum wage increase. The top row shows trends for all hourly workers; the bottom row shows trends for hourly workers paid under \$15 per hour.

Next, we present two types of placebo tests, one based on time and the other on wages.

Table 2 replicates our main analyses for an earlier period. The idea here is that in the period before the minimum wage went up, people making below and above the new minimum wage should have similar turnout trends; there should be no “effect” of a policy increase that has not yet taken place. We estimate the same difference-in-differences model we ran for 2012-2016, but moving it back in time to look at changes in turnout from 2008-2012. There were no major MW increases over this period that should have affected our “treated” group;² we expect to see no effect over this period. We find null effects here, whether we look at all hourly workers or focus on workers below \$15/hour (as in the first two columns of Table 1). We are hesitant to go further back in time than this, both because of past minimum wage increases that could affect differing subsets of the sample, and because of the same data pattern we noted in discussing the parallel-trends plot: the further back you go in the data, the steeper the slope of the non-minimum-wage group looks, because people appear on the file and have relatively high rates of turnout.

Our other placebo approach, discussed in the main paper, involves using the same turnout data as our main analyses, and assigning “treatment” based on hypothetical wage increases rather than the ones that actually happened. We drop the people actually affected by the minimum wage increases that took place between 2014 and 2017, and arbitrarily assign many possible cutpoints to the remaining data, rerunning our difference-in-differences analyses to compare people above or below those meaningless cutpoints. If there were some systematic reason that lower-income people increased their 2016 turnout more than higher-income people, we should see that positive bias in these placebo estimates. If we find that many of these placebo analyses yield positive, significant estimates, we should worry about our main analyses. We essentially replicate the analysis from Table 1, sliding the window of the “affected group” and “unaffected comparison group” up through the wage distribution. So rather than affecting people making from \$7.25 to \$9/hour, we begin by imagining that the wage increase affected people making from \$9/hour to \$10.75/hour, and compare those hypothetically affected people to those making up to \$17.75/hour. We then slide this comparison window further up the wage distribution by .1 percent increments, all the way up to \$40/hour (this is roughly the 90th percentile of hourly wages in our data). To be sure that no workers included in this placebo test were affected by a minimum wage increase, we used a more permissive rule for determining which workers were affected by a minimum

²Though a 2009 federal change did raise the state wage by ten cents per hour, we think this is a relatively small change compared to inflation over this period, and may not even have affected the same group of workers as we include in our 2014 “treated” group.

Table 2: Placebo Individual Difference-in-Differences Estimates

	<i>Dependent variable:</i>	
	Voted	
	(1)	(2)
Under New Min. Wage	-0.202* (0.003)	-0.188* (0.003)
2012	0.019* (0.002)	0.026* (0.002)
Under New MW * 2012	0.004 (0.003)	-0.002 (0.003)
Constant	0.340* (0.002)	0.326* (0.002)
Included Employees	All Hourly	Hourly Under \$15
Observations	223,114	175,450
R ²	0.027	0.030
Adjusted R ²	0.027	0.030

Note:

*p<0.05

wage increase than in the main analysis. Specifically, we excluded all employees who were ever observed making below \$ 13 per hour, which was the New York City minimum wage for large employers at the end of 2017. This approach of running many placebo tests based on arbitrarily-chosen cutpoints yields 1000 estimates per election resulting in 3000 estimates total. Our observed estimates (of the effect of the actual minimum wage increase) are in the extreme tail of these distributions, as shown in Figure 3.3 of the main paper.

2.2 Triple-Differences (Other States)

We might still worry that low-wage workers (at this specific wage rate) might face different trends over this time period than other workers, in ways that might not be picked up by either of our placebo tests. Another possible approach is to look at similarly-paid workers in states that did not implement minimum-wage increases over this time period. We can then implement a triple-differences approach: in addition to comparing near-minimum-wage to higher-wage workers over the period 2012-2016, we can compare these differences in differences estimates across states that did and did not actually implement a minimum wage increase. We should see a bigger effect in NYC than in places with no minimum wage change, since any “difference-in-differences” observed in these states that didn’t change their laws would suggest bias in our design (there should be no effect of being a “minimum-wage-increase-affected” worker if there was no minimum wage increase).

Ideally, we would look to states relatively near New York for this comparison, such as New Jersey. Unfortunately, most northeastern states implemented some sort of minimum wage increase over the time period we study, rendering such a comparison difficult.³ As such, we look for any state that meets three criteria: it did not increase its minimum wage, it has public-employee-salary data that allows us to observe hourly wages and match employees to the voter file, and it has a publicly-available (and not prohibitively expensive) voter file.

The first place we found to meet all these criteria is Idaho, so we present results from an analysis of public employees from there (while acknowledging the many differences between Idaho and NYC). We have public payroll data for people employed by the state in 2013 or 2014, and we reshape and merge this data to the Idaho voter file in a process that parallels as closely as possible the approach we used in New York. Then, we run the same difference-in-differences approach as we ran in New York, again marking as “affected” any workers

³Pennsylvania did not increase its minimum wage over this period, but we were unable to find municipal payroll data that included hourly wages (not just annual salaries, which are not informative about hourly wages if we do not observe how many hours a person works).

that were paid less than \$8/hour in either year (the new minimum wage in New York as of 2014). Of course, we note here that none of these employees were actually affected by New York’s minimum wage increase (Idaho has not increased its state minimum wage since the last federal increase in 2009).

Table 3 presents placebo estimates for Idaho that mirror the main table in the paper, first looking at changes from 2012-2016 among all workers and workers paid under \$15/hour, and then looking at changes from 2010-2014 among the same sets of workers. There are no significant changes here, as we would expect given that the people “Under New Min. Wage” have not received a raise over this period. Most of the difference-in-differences point estimates are near zero, particularly in 2010-2014. But the estimates are relatively noisy here, because relatively few workers fall into our “affected” category: fewer than 400 workers in this dataset were paid less than \$8/hour. This limits our statistical precision when running a triple-differences setup, but we show those results nonetheless.

Table 4 presents triple-differences estimates that compare the diff-in-diff estimates in Idaho to those in New York to see whether they are consistently larger in NYC (as we would expect, given that only in NYC were low-wage workers targeted with a raise). Estimates like this can help to address several concerns: first, we might worry that low-wage workers nationwide faced some sort of “shock” or other trend that could lead to them voting at different rates in 2016 than in 2012. Or, we might think that low-wage workers will always tend to be somewhat different than their higher-paid counterparts: perhaps they are younger on average, and so we should expect their turnout to increase more steeply over any given four-year period as they “age in” to voting. A triple-differences approach can help capture these concerns: to the extent such differences between low-wage workers and their counterparts are also present in Idaho, the triple-differences estimates essentially subtract them off.

We thus focus on the triple-interaction coefficients in Table 4 – “Under New MW * 2016 * NYC” and “Under New MW * 2014 * NYC” – as these tell us how much larger the difference-in-differences estimates are in NYC than in Idaho. These coefficients are generally positive, as we would expect, but are quite imprecise and thus are never statistically distinguishable from zero. We think this may be due to the relatively small number of “affected” (sub-\$8) workers in Idaho.

Depending on the type of concerns we have, it is possible to construct a larger “low-wage” comparison group in Idaho. For our initial analyses, we focused on workers making under \$8/hour, because that was the nominal value of the new minimum wage in NYC. But some of the stories we might tell about low-wage workers being different (younger or more mobile

Table 3: Placebo Individual Difference-in-Differences Estimates, Idaho State Workers

	<i>Dependent variable:</i>			
	Voted			
	(1)	(2)	(3)	(4)
Under New Min. Wage	−0.284* (0.024)	−0.178* (0.024)	−0.268* (0.018)	−0.153* (0.018)
2016	0.067* (0.002)	0.086* (0.004)		
Under New MW * 2016	0.033 (0.022)	0.015 (0.022)		
2014			0.072* (0.003)	0.077* (0.005)
Under New MW * 2014			−0.002 (0.019)	−0.007 (0.019)
Constant	0.553* (0.003)	0.447* (0.005)	0.394* (0.003)	0.279* (0.005)
Included Employees	All Hourly	Hourly Under \$15	All Hourly	Hourly Under \$15
Observations	58,944	17,156	58,944	17,156
R ²	0.008	0.012	0.009	0.011
Adjusted R ²	0.008	0.012	0.009	0.011

Note:

*p<0.05

Table 4: New York vs. Idaho (Triple-Differences)

	<i>Dependent variable:</i>			
	Voted			
	(1)	(2)	(3)	(4)
Under New Min. Wage	−0.284* (0.024)	−0.178* (0.024)	−0.268* (0.018)	−0.153* (0.018)
2016	0.067* (0.002)	0.086* (0.004)		
2014			0.072* (0.003)	0.077* (0.005)
New York City	−0.194* (0.003)	−0.095* (0.006)	−0.214* (0.003)	−0.123* (0.005)
Under New MW * 2016	0.033 (0.022)	0.015 (0.022)		
Under New MW * 2014			−0.002 (0.019)	−0.007 (0.019)
Under New MW * NYC	0.087* (0.024)	−0.012 (0.024)	0.157* (0.018)	0.065* (0.018)
2016 * NYC	−0.068* (0.003)	−0.091* (0.005)		
Under New MW * 2016 * NYC	−0.011 (0.022)	0.011 (0.022)		
2014 * NYC			−0.111* (0.003)	−0.111* (0.005)
Under New MW * 2014 * NYC			0.027 (0.019)	0.027 (0.019)
Constant	0.553* (0.003)	0.447* (0.005)	0.394* (0.003)	0.279* (0.005)
Included Employees	All Hourly	Hourly Under \$15	All Hourly	Hourly Under \$15
Observations	282,058	192,606	282,058	192,606
R ²	0.065	0.035	0.094	0.036
Adjusted R ²	0.065	0.035	0.093	0.036

Note:

*p<0.05

or otherwise prone to having different turnout trends) than other workers aren’t necessarily about a precise \$8/hour cutpoint. The minimum-wage-affected workers we examine in NYC represent approximately the bottom quintile of the pay distribution in our dataset, so another possible comparison would be to workers in the bottom quintile of pay in the Idaho dataset. Table 5 presents triple-differences estimates that use this larger set of “affected” Idaho workers. Again, the estimates are consistently positive, indicating that the difference-in-differences (the change in “affected” workers’ turnout compared to other workers over this period) is larger in New York than in Idaho. These estimates are also somewhat more precise, so in two of the four specifications we reject the null hypothesis of no difference between NYC and ID.

2.3 Robustness: Including Age

Another possible concern about the estimates presented in the paper centers on the age of people in the different groups; perhaps we do not think that the affected and unaffected groups should have parallel trends in voter turnout over the time period studied because the unaffected group is, on average, a few years older than the affected group (and younger voters’ turnout may change more over a four-year period than older voters’ even in the absence of other stimuli). In this section, we present specifications that include our best guess at each employee’s age. These age estimates are based on the dates of birth reported in the voter file⁴, so they implicitly limit the analysis to people who were successfully merged to the voter file. This makes it hard to directly compare the magnitude of the effects to those presented in Table 1 in the main paper, but we note that the difference-in-differences estimates remain positive and significant when conditioning on age.

2.4 Robustness: Including Different Workers

Here, we examine how much our arbitrary decision, in Table 1, to present results for all hourly workers and for all hourly workers paid below \$15/hour, matters for our conclusions. Figure 3 plots the estimated effect of the minimum-wage increase on affected workers, varying the comparison group used in the analysis. On the far left side of the plot, we use very narrow comparison groups, only including people paid below \$9.50 or \$10 per hour. We then loosen the criteria, allowing the analysis to include people paid \$20, \$30, or \$40 per hour. We note that the estimates are generally quite stable across the wage distribution. On the far left

⁴For employees that matched to multiple possible voters, we took the mean across all matches’ ages.

Table 5: New York vs. Idaho (Triple-Differences), Bottom Pay Quintile “Affected”

	<i>Dependent variable:</i>			
	Voted			
	(1)	(2)	(3)	(4)
Under New Min. Wage	−0.142* (0.007)	−0.012 (0.012)	−0.157* (0.006)	−0.016 (0.011)
2016	0.065* (0.002)	0.110* (0.008)		
2014			0.070* (0.003)	0.074* (0.009)
New York City	−0.221* (0.004)	−0.097* (0.010)	−0.245* (0.003)	−0.128* (0.009)
Under New MW * 2016	0.013* (0.005)	−0.032* (0.010)		
Under New MW * 2014			0.009 (0.006)	0.003 (0.010)
Under New MW * NYC	−0.055* (0.008)	−0.178* (0.012)	0.046* (0.007)	−0.072* (0.011)
2016 * NYC	−0.066* (0.003)	−0.114* (0.008)		
Under New MW * 2016 * NYC	0.009 (0.006)	0.058* (0.010)		
2014 * NYC			−0.109* (0.003)	−0.109* (0.009)
Under New MW * 2014 * NYC			0.016* (0.006)	0.017 (0.011)
Constant	0.580* (0.003)	0.449* (0.010)	0.424* (0.003)	0.284* (0.009)
Included Employees	All Hourly	Hourly Under \$15	All Hourly	Hourly Under \$15
Observations	282,058	192,606	282,058	192,606
R ²	0.067	0.034	0.097	0.035
Adjusted R ²	0.067	0.034	0.097	0.035

Note:

*p<0.05

Table 6: Individual Difference-in-Differences Estimates, Including Age

	<i>Dependent variable:</i>			
	Voted			
	(1)	(2)	(3)	(4)
Under New Min. Wage	−0.107* (0.006)	−0.092* (0.006)	−0.059* (0.004)	−0.032* (0.004)
2016	−0.002 (0.002)	−0.006* (0.003)		
2014			−0.056* (0.002)	−0.048* (0.002)
Under New MW * 2016	0.051* (0.007)	0.056* (0.007)		
Under New MW * 2014			0.022* (0.005)	0.014* (0.005)
Constant	0.239* (0.018)	0.230* (0.018)	0.086* (0.008)	0.066* (0.008)
Age (year) dummies	Yes	Yes	Yes	Yes
Included Employees	All Hourly	Hourly Under \$15	All Hourly	Hourly Under \$15
Observations	146,052	114,896	146,052	114,896
R ²	0.023	0.019	0.081	0.070
Adjusted R ²	0.023	0.019	0.081	0.069

Note:

*p<0.05

side of the plot, the results become somewhat larger and more variable as we narrow to increasingly-smaller comparison groups.

We note that the choice of “ideal” comparison group is not clear *ex ante*. We might think that people paid \$40/hour are not particularly comparable to people paid minimum wage; perhaps their turnout patterns are subject to different forces, and so we might worry that the parallel trends assumption is not well founded. This concern argues for a narrower analysis that uses a smaller comparison group. However, doing so discards data; it also introduces possible concerns about whether people just above the minimum wage cutpoint could be experiencing some sort of “reverse treatment.” That is, we might wonder whether people making just above the new minimum wage could actually show a *drop* in turnout because they have seen themselves left out of this policy. If that were the case, our estimates could be slightly upward-biased; the more narrowly we define our comparison group, the more of a concern this sort of bias would become.

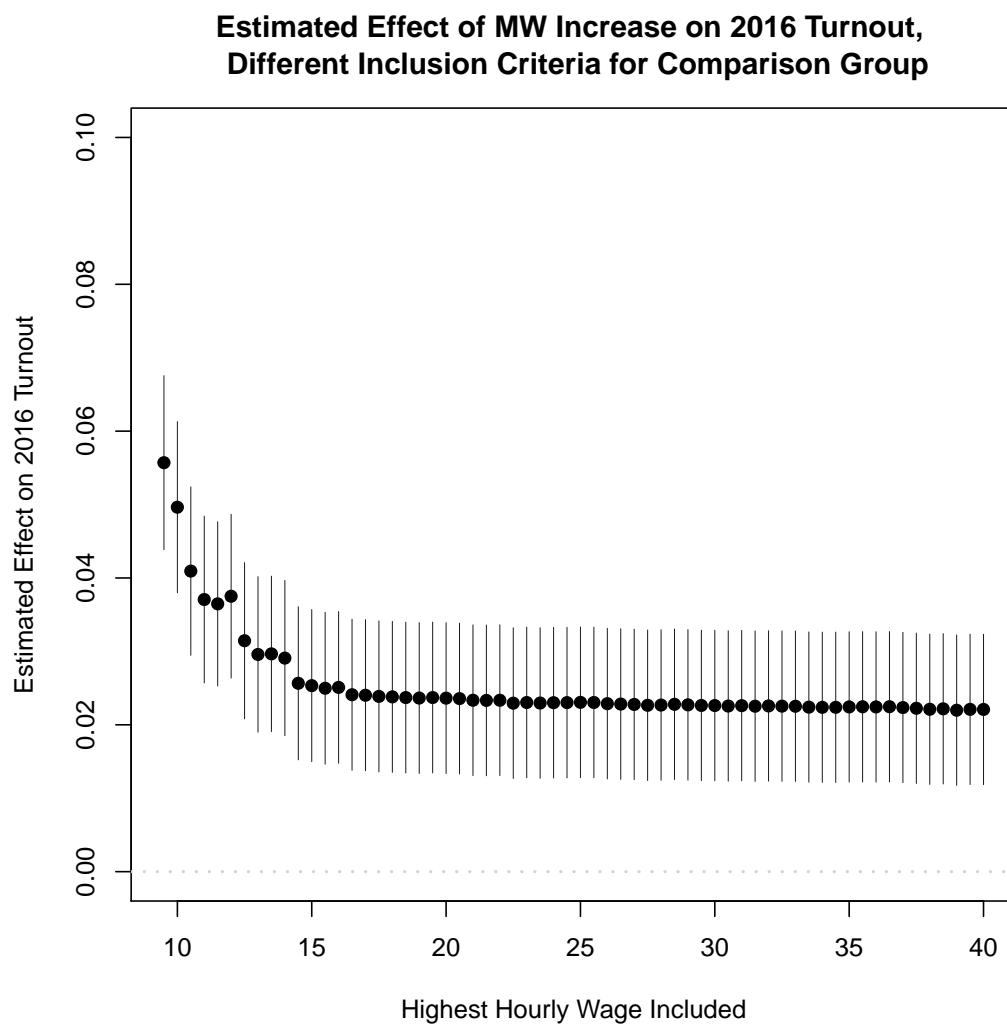


Figure 3: Estimates comparable to column 2 of Table 1, but using different wage cutpoints to determine the comparison group.

2.5 Robustness: Adding Measurement Error to Vote Histories

Figure 4 explores how errors in our voter file match could affect our estimates by deliberately introducing even more error. We discard between 0 and 40% of the voter matches we found, setting both 2012 and 2016 voter turnout to 0 for these (randomly-selected) workers. As we note in the main paper, missing some matches to the voter file should generally bias our estimates towards zero; indeed, as we delete more matches (moving along the x-axis), the estimates shrink towards zero.

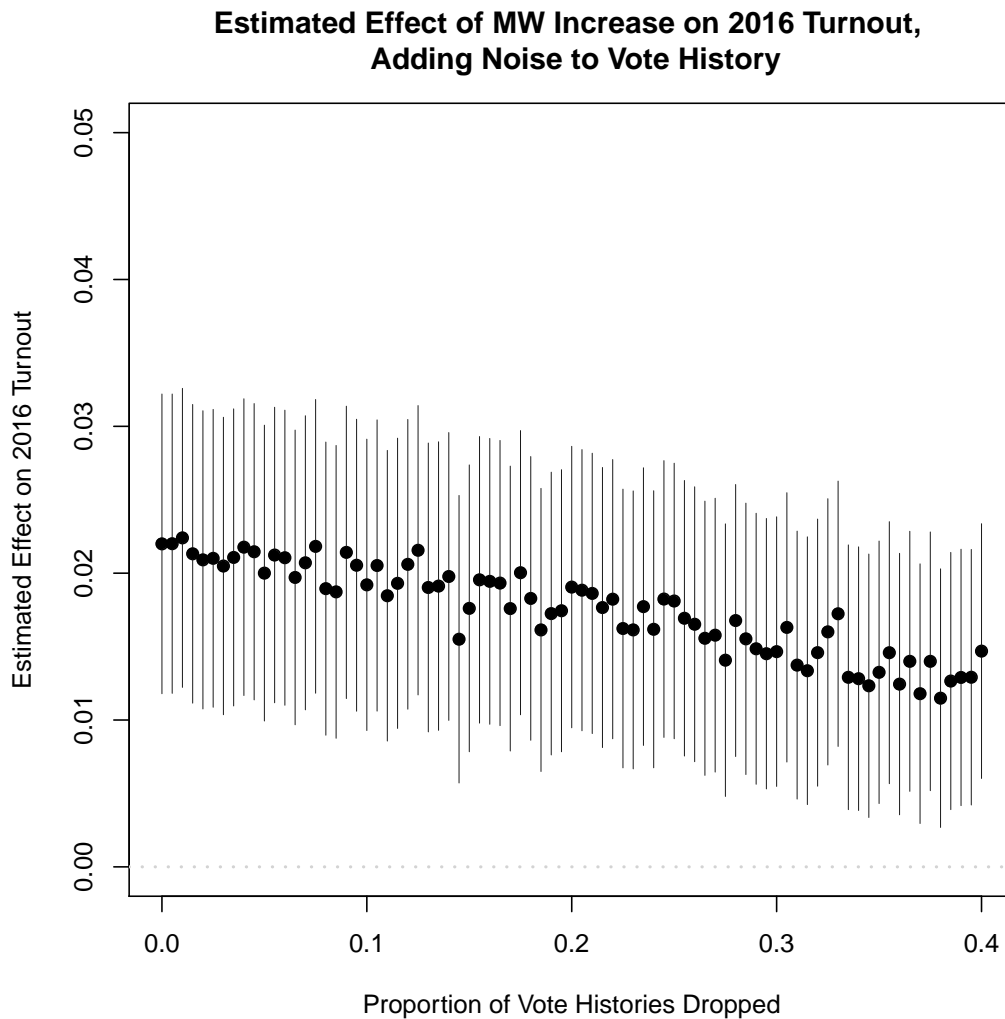


Figure 4: Estimates comparable to column 1 of Table 1, but randomly deleting some vote histories.

3 Additional Panel Analyses

3.1 Visualizing Panel Data

Figure 5 displays the changes in state minimum-wage laws that occur during the period covered by our panel data, to give a sense of the variation used in the panel analysis. Each (four-year) election cycle appears on the x-axis, with the y-axis representing the proportional increase in the state minimum wage that occurred in the prior four years. Each point represents one or more states' law changes over that time, with points scaled by size: bigger points represent more states. Aqua points represent changes that occurred due to increases in the federal minimum wage, while red ones represent state-determined changes.

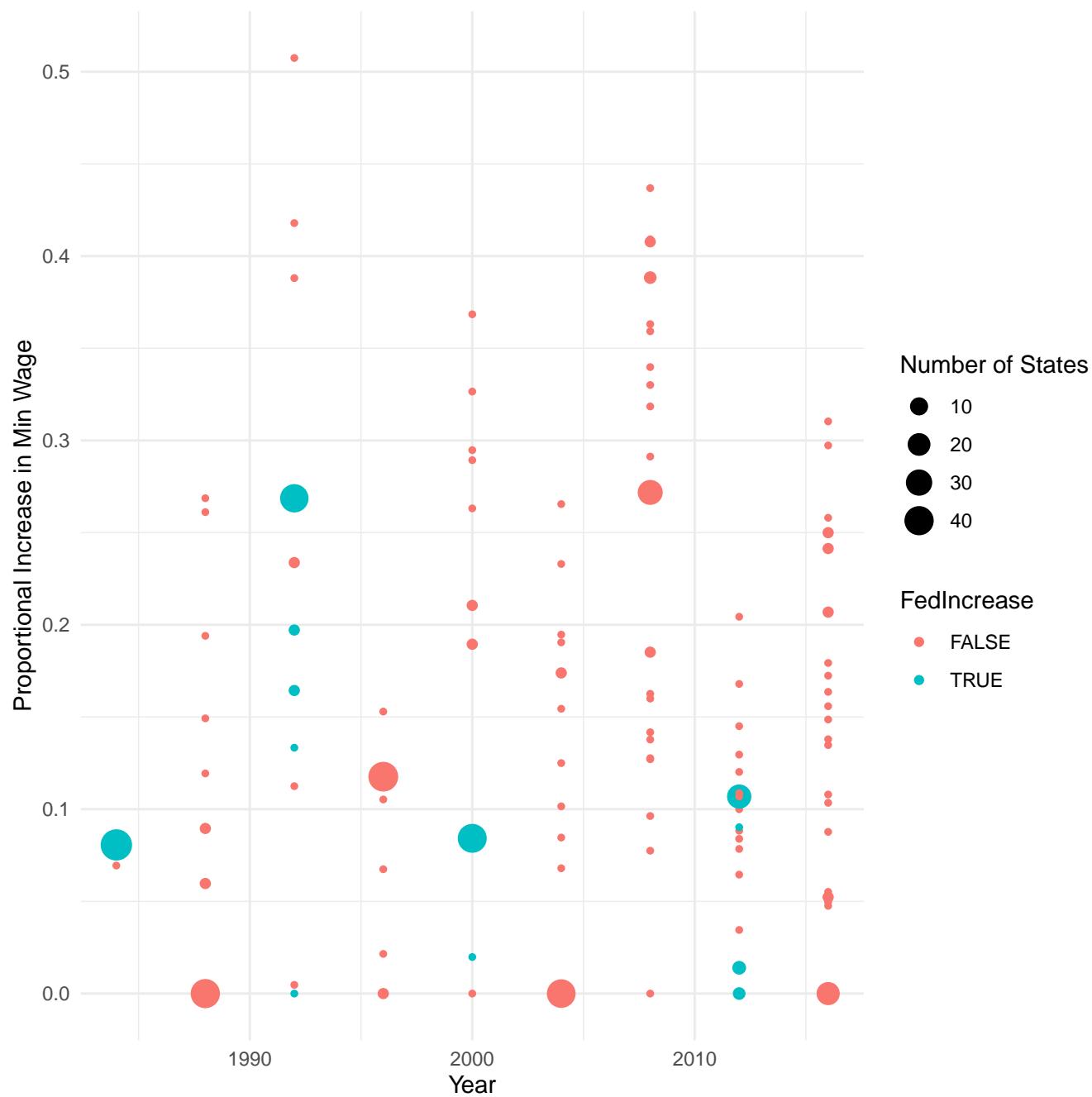


Figure 5: Visualization of the variation in the minimum-wage panel data used for the analyses in main paper.

3.2 Real Minimum Wage

We also operationalized our treatment using the change in the real minimum wage (2018 dollars). Because inflation is continuously reducing the value of the minimum wage, the real minimum wage will provide an estimate of the real buying power of the minimum wage over time. Table 7 provides the results of this analysis. They corroborate the findings from our main analysis and suggest that a one dollar increase in the real minimum wage will increase voter turnout by .4%. A key difference between these models and our main results is that the real minimum wage will decrease in most years if the nominal minimum wage not increased. This model then treats this slow decline from a stagnant nominal minimum wage as a potential force in reducing turnout.

Table 7: First Difference Real Minimum Wage

	<i>Dependent variable:</i>		
	Change in Turnout		
	(1)	(2)	(3)
Change in Real Minimum Wage	0.004* (0.001)	0.004* (0.001)	0.003 (0.002)
Intercept	-0.0001 (0.001)		
County Time Trend	No	Yes	Yes
Counties Included	All Counties	All Counties	Counties Bordering State Edges
Observations	25,061	25,061	9,557
R ²	0.005	0.024	0.048
Adjusted R ²	0.005	0.022	-0.076
Residual Std. Error	0.054 (df = 25059)	0.053 (df = 25017)	0.059 (df = 8452)
<i>Note:</i>			* p ≤ .05 SE's Clustered by State

3.3 Fixed Effects Specification

An alternative to the first differenced outcome variable we use in the main paper is to regress turnout rates on increases in the minimum wage with county fixed effects. We perform this analysis in table 8. The first column of table 8 includes no fixed effects and reveals a coefficient significantly larger than that in our main analysis. Column two introduces county fixed effects and county specific linear time trends. Column 3 performs the same analysis, but limits the regression to county pairs that share a border which crosses a state line and also includes bordering county pair fixed effects.

Column 4 also introduces year fixed effects. Due to changes in the federal minimum wage, minimum wage increases are heavily concentrated within certain years. In particular, early in our sample, minimum wage increases were almost entirely implemented by the federal government. Consequently, the inclusion of year fixed effects effectively reduces the power of our design. It is therefore unsurprising that the estimates lose statistical significance after the inclusion of year fixed effects, though the point estimate remains positive.

Table 8: County Fixed Effects Models

	<i>Dependent variable:</i>			
	Turnout			
	(1)	(2)	(3)	(4)
Min Wage Increase	0.102* (0.014)	0.080* (0.009)	0.078* (0.009)	0.011 (0.018)
Intercept	0.576* (0.013)			
County Fixed Effects	No	Yes	Yes	Yes
County Time Trends	No	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes
Counties Included	All Counties	All Counties	Counties Bordering State Edges	All Counties
Observations	25,062	25,062	9,557	25,062
R ²	0.012	0.877	0.858	0.921
Adjusted R ²	0.012	0.842	0.787	0.898
Residual Std. Error	0.101 (df = 25060)	0.040 (df = 19491)	0.047 (df = 6371)	0.032 (df = 19483)

Note:

* $p \leq .05$
SE's clustered by state

3.4 Jackknife Results

To assess the dependence of our results on a single year or state, we performed a jackknife procedure, both for the estimates presented in the main paper and the fixed effects specification reported in the prior section. We iteratively deleted a state or year and reran the analysis on the smaller dataset. The results of this procedure are presented in figures 6 and 7. The results show a complete robustness to the elementwise deletion of a state. In all cases, the estimates remain positive and statistically significant regardless of which state is deleted.

We also observe robustness to the elementwise deletion of years for the fixed effects model. Although less consistent than is the case for the state jackknife procedure, the results remain positive and statistically significant in all cases for the fixed effects specification. We observe less robustness in the face of the deletion of individual years for the first differenced specification presented in the main paper. The point estimate remains positive when deleting 9 of the 10 years, and the estimate remains statistically significant after eliminating 8 of those 10. But the point estimate becomes negative if the year 1992 is deleted from the dataset. Such an outcome is unsurprising given the nature of the data used here. Presidential elections occur infrequently and minimum wage increases are often heavily concentrated in a single year (such as when there is a federal minimum wage increase). Consequently, some dependence on the choice of years included in the analysis is to be expected.

Figure 6: State jackknife

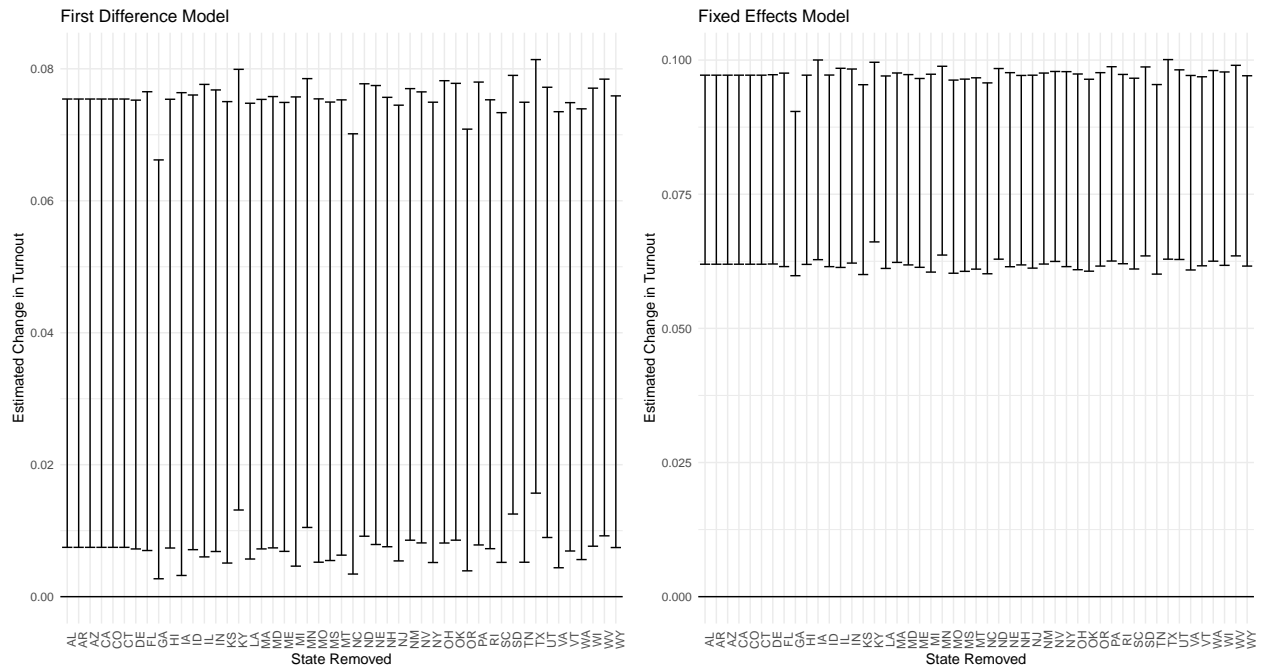
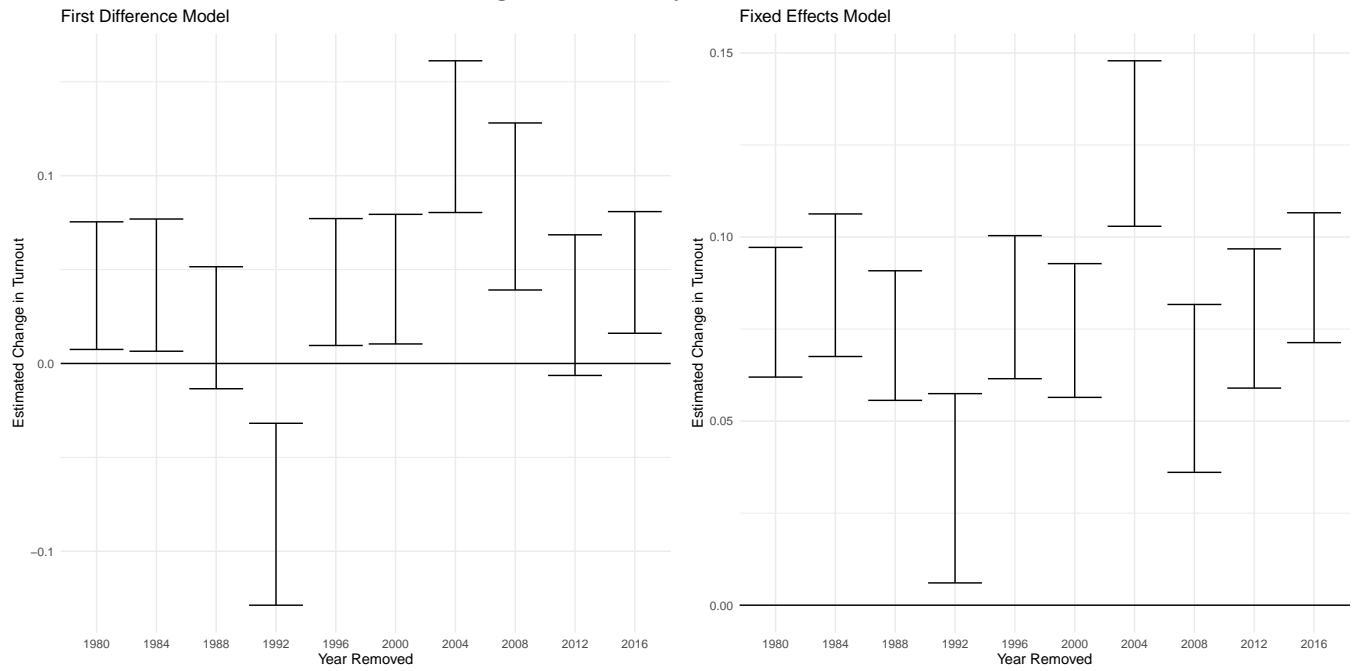


Figure 7: Year jackknife



3.5 Allowing Quadratic County Time Trends

In order to further improve robustness to unobserved time varying confounders, we also considered models that allowed quadratic county time trends. The results are presented in figure 9. The first column presents results for the model which is fit on all counties while the second limits the analysis to bordering counties only. In both cases, the coefficients are similar to those observed in the main results, although the coefficient in the model trained on all counties falls short of statistical significance, likely due to the large number of degrees of freedom absorbed by fitting a quadratic time trend within each county.

Table 9: Main Estimates from Panel Data With Quadratic County Time Trends

	<i>Dependent variable:</i>	
	Change in Turnout	
	(1)	(2)
Min Wage Proportional Increase	0.031 (0.017)	0.035* (0.016)
Intercept	-192.282* (67.070)	-203.560* (60.406)
Counties Included	All Counties	Counties Bordering State Edges
Observations	25,061	9,557
R ²	0.087	0.082
Adjusted R ²	-0.173	-0.180
Residual Std. Error	0.059 (df = 19490)	0.062 (df = 7432)

Note:

* $p \leq .05$
SE's clustered by state

3.6 State-Level Analyses

Table 10 replicates our main panel analysis but uses states as the unit of analysis. Specifically, the coefficient for the proportional minimum wage increase is slightly larger than that seen at the county level, suggesting that our results are not sensitive to changing the geographic level of aggregation that turnout is measured on.

Table 10: State Level Model

	<i>Dependent variable:</i>	
	Change in Turnout	
	(1)	(2)
Min Wage Proportional Increase	0.067* (0.013)	0.069* (0.015)
Intercept	-0.006* (0.002)	-0.284 (0.157)
State Time Trends	No	Yes
Observations	450	450
R ²	0.027	0.056
Adjusted R ²	0.025	-0.062
Residual Std. Error	0.043 (df = 448)	0.045 (df = 399)

Note:

* $p \leq .05$
SE's Clustered by State

3.7 Lagged Dependent Variable Specification

We also considered the performance of the model when including the lagged turnout in a county as a covariate when regressing the increase in the minimum wage on voter turnout. These models provide a useful complement to the fixed effects models presented in 3.3 as the two estimates together provide a useful bounding property: when the fixed effects specification is correct, the lagged dependent variable regression will be biased downwards, but when the lagged dependent variable specification is correct, the fixed effects regression will be biased upwards (Angrist and Pischke, 2008). Consequently, these models provide robust support for the theory that minimum wage increases lead to increases in voter turnout.

Table 11: Replication of Main Panel Models With Lagged Turnout as a Covariate

	<i>Dependent variable:</i>		
	Turnout		
	(1)	(2)	(3)
Min Wage Proportional Increase	0.050* (0.013)	0.070* (0.009)	0.070* (0.008)
Lagged Turnout	0.840* (0.010)	0.260* (0.029)	0.246* (0.028)
Intercept	0.087* (0.007)		
State Fixed Effects	No	Yes	Yes
County Time Trends	No	Yes	Yes
Counties Included	All Counties	All Counties	Counties Bordering State Edges
Observations	25,061	25,061	9,557
R ²	0.745	0.840	0.823
Adjusted R ²	0.745	0.819	0.799
Residual Std. Error	0.051 (df = 25058)	0.043 (df = 22231)	0.046 (df = 8451)

Note: * p ≤ .05
SE's clustered by state in models 1 and 2
SE's Clustered by state and bordering county pair in model 3

3.8 Marginal Structural Models

One concern with our analysis is that there might exist time-varying confounding. For example, previous minimum wage increases might increase turnout leading to both higher turnout and more minimum wage increases in the future. In this case, our modeling approach might overestimate the effect of minimum wage increases on turnout. To address this concern we implemented a marginal structural model (MSM) as suggested by Blackwell and Glynn (2018). More specifically, we estimated the probability of receiving a particular proportional minimum wage increase using a gamma hurdle model which used a county’s previous treatment history and Democratic two party voteshare history as covariates. The results of this MSM are presented below; they also suggest that increases in the minimum wage are associated with increases in aggregate turnout.

Table 12:

	<i>Dependent variable:</i>
	Change in Turnout
Min Wage Proportional Increase	0.111* (0.000)
Intercept	−0.013* (0.000)
Observations	25,061
R ²	0.221
Adjusted R ²	0.221

Note: * $p \leq .05$
SE’s Clustered by State

3.9 Federal/State Minimum-Wage Changes

We begin with the test discussed in the main paper: interacting the minimum wage increase with an indicator for whether the increase resulted from a federal minimum wage change. The intuition here is that political mobilization could be a time-varying confounder that would cause us to overstate the true effects of minimum wage increases. To the extent this is the case, it should be an especially large problem in state-level minimum wage increases,

since it is hard to imagine that mobilization in one election during one election cycle would yield changes in the federal minimum wage during that same election cycle. Table 13 shows the result of this exercise, demonstrating that even federal increases (less plagued by this mobilization concern) are associated with substantial increases in voter turnout.

Table 13: Effect Heterogeneity When Increases are Due to Federal Minimum Wage Increases

	<i>Dependent variable:</i>		
	Change in Turnout		
	(1)	(2)	(3)
Min Wage Proportional Increase	0.034* (0.013)	0.022 (0.018)	0.034 (0.020)
Min Wage Proportional Increase \times Fed Increase	0.011 (0.020)	0.031 (0.025)	0.019 (0.024)
Intercept	-0.006* (0.002)	-0.906* (0.227)	-0.869* (0.202)
County Time Trend	No	Yes	Yes
Counties Included	All Counties	All Counties	Counties Bordering State Edges
Observations	25,061	25,061	9,557
R ²	0.007	0.052	0.046
Adjusted R ²	0.007	-0.066	-0.073
Residual Std. Error	0.054 (df = 25058)	0.056 (df = 22274)	0.059 (df = 8493)

Note:

* p \leq .05
SE's Clustered by State

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