

More Money, More Turnout?

Minimum Wage Increases and Voting*

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Abstract

Minimum wage increases often result from large-scale political debates and mobilization. But do increases in the minimum wage also have an independent effect on political participation? We measure the effect of minimum wage increases on the voting behavior of low-wage workers in two ways. First, we merge public records of municipal employee wages from New York City to public voting records, allowing us to observe voting behavior of people at different pay rates across several elections. Difference-in-differences estimates indicate that 2014 and 2015 increases in New York’s minimum wage increased voter turnout among people earning close to minimum wage by two to three percentage points between 2012 and 2016. Second, we incorporate county-level panel data on minimum wages and voter turnout from 1980 to 2016 to see whether our initial results generalize to other contexts. We 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?

We test whether raising the minimum wage increases political participation among low-wage workers. There are several reasons to expect such an effect, as increases in the minimum wage could yield both resource and interpretive effects (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 may 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).

Whatever the mechanism, we are aware of no research that attempts to empirically test the effect of minimum wage increases on voter 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 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 employees data allows us to carefully pinpoint the effect of wage increases on low-wage employees' turnout, while the panel data analysis allows us to ensure 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 Political 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).

But a change in the minimum wage does not only increase workers’ pay. It is also a

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 (2017). 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).

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 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; 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 similar 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.

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 for New York City workers 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

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.

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 full 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 of the 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 actually 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. 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. Appendix A presents this merge process in more detail.

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 late 2017, 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.

⁵We recognize that a more-inclusive merge approach, such as allowing some fuzziness in first name spellings, might capture some true matches that we will miss with our exact-match approach. However, since 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. Further, because we do not have much additional information (such as birthdates) as a means of narrowing down matches, we did not want to generate many more potential matches, most of which would be spurious.

The resulting sample includes 175281 hourly employees, 62% of whom match to the voter file. Consistent with the recommendations of Nyhan and Skovron (2015), 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 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 presents the main estimates from this 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 anytime in fiscal years 2011-2014 as very likely to be affected by

⁶We discuss parallel trends in more detail in Section 3.3 and Appendix B.

the minimum wage increases that began in January 2014.⁷ By this definition, about 17% of the hourly workers we observe 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.

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.

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.

Table 1: Main Individual Difference-in-Differences Estimates

	<i>Dependent variable:</i>			
	Voted			
	(1)	(2)	(3)	(4)
Under New Min. Wage	−0.201* (0.003)	−0.194* (0.003)	−0.110* (0.002)	−0.087* (0.002)
2016	−0.002 (0.002)	−0.006* (0.002)		
Under New MW * 2016	0.024* (0.003)	0.028* (0.003)		
2014			−0.039* (0.001)	−0.035* (0.001)
Under New MW * 2014			0.025* (0.002)	0.021* (0.002)
Constant	0.360* (0.002)	0.353* (0.002)	0.179* (0.001)	0.156* (0.001)
Included Employees	All Hourly	Hourly Under \$15	All Hourly	Hourly Under \$15
Observations	223,114	175,450	223,114	175,450
R ²	0.023	0.026	0.014	0.012
Adjusted R ²	0.023	0.026	0.014	0.012

Note:

*p<0.05

Then, we repeat the same exercise, this time looking at voting in midterm elections. The last two columns of Table 1 present difference-in-differences estimates for the period from 2010-2014. The estimated turnout effects are similar in magnitude: 2-3 percentage points depending on specification. That said, they represent a larger percentage increase in turnout, because turnout in midterm elections is lower than in presidential years. We urge caution in directly comparing the size of the midterm and presidential estimates from this approach 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.

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

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.

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.¹⁰

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

¹⁰We’re currently working on our own estimates based on ACS microdata; this should let us do a better job of estimating the number of people affected in New York state, and also to project what would have happened if all states had seen similarly-sized increases in the minimum wage over this period.

the same in the absence of minimum wage changes. If that assumption did not hold, our estimates could be biased.

We address concerns about the 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, Appendix B 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.

Appendix B also presents several types of placebo tests that seek to estimate impossible effects, such as the “effect” of MW increases on turnout before the wage increases were implemented or even proposed, or the “effect” of wage increases among workers that were not affected by them. First, we run many analyses that repeat the approach from Table 1 on different subsets of the data, comparing people above and below a range of arbitrarily chosen hypothetical minimum wages rather than the real minimum wage (for example, people above and below \$15 an hour). We find that only a small proportion of these placebo tests (on the order of 5%) return a statistically-significant estimate, and that the mean of all these arbitrary estimates is zero; this assures us that our approach of comparing lower- to higher-wage workers over this time period does not have some built-in positive bias. We also present placebo tests that 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. As we go even further back in time, the placebo test estimates gradually become negative, which we attribute to the vagaries of using vote history data from several election cycles before our intervention of interest.¹¹ In

¹¹Specifically, we think that municipal workers are less and less likely to have been on the voter file the further back we go; people working for the city in 2014, for example, may well not have been eligible to vote in 2006, either because they were not living in the city or were not yet of voting age. This kind of “dropping

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. 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 if such mistakes happen essentially at random (that is, if they happen without regard to whether someone is affected by the minimum wage increase), they should simply be a source of measurement error that should bias our estimates towards zero, making our results conservative. Figure 3 in Appendix D 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

off” the voter file as we go further back disproportionately affects estimates of higher-wage workers, since they have higher turnout to start with. Thus, voter ineligibility or mobility as we go back in time should generally bias the diff-in-diff estimates downward. We discuss this point further in Appendix B.

missing voter file matches for people affected by the minimum wage, also have the effect of attenuating the estimates.

4 Panel Data

In the last 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, we might wonder whether the changes in minimum-wage workers’ voter turnout would be large enough to make a difference for aggregate voter turnout. To address these questions, we now turn to a nationwide dataset of county-level voter turnout 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 impact on 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.¹² 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:

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

¹²We 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.

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). We focus on proportional minimum wage increases (the difference between 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.¹³

Using panel data, rather than only making comparisons cross-sectionally or over-time, allows us to rule out some simple forms of confounding.¹⁴ Here, we rely on *changes* in the minimum wage to see whether they seem to cause corresponding changes in turnout. But such an approach is still subject to concerns about time-varying confounders: if some other turnout-increasing event happens at the same time as minimum wage increases, we might worry that our results could be biased. We discuss such concerns more after the main results.

4.1 Panel Estimates: Voter Turnout

We begin by estimating the effect of minimum wage increases on voter turnout in presidential elections (1980-2016). Table 2 presents results from several specifications. 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). Standard errors are clustered at the state level. Column 2 then includes state fixed effects, and Column 3 adds in a county time trend in turnout to account for general over-time changes in turnout.

All of the estimates in Table 2 yield similar interpretations. The coefficient on “Min Wage Proportional Increase” captures the effect of doubling the minimum wage over a four-

¹³In 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.

¹⁴For example, if we simply looked at whether voter turnout was higher in places with higher minimum wages at one point in time, it could be that any observed differences were driven by some other joint cause of both turnout and minimum wages, like a strong Democratic party organization, or regional factors.

year period¹⁵, obviously an unlikely occurrence. The median minimum-wage increase in our dataset is 8 percent; the estimates in Table 2 suggest that such a change 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.

¹⁵Specifically, the ratio $((\text{new wage} - \text{old wage}) / \text{new wage})$ moves from 0 (no wage increase) to 1 (100% wage increase).

Table 2: Main Results from Panel Data

	<i>Dependent variable:</i>		
	Change in Turnout		
	(1)	(2)	(3)
Min Wage Proportional Increase	0.044* (0.013)	0.046* (0.014)	0.044* (0.014)
Year			0.0004* (0.0001)
Intercept	-0.006* (0.001)		
State Fixed Effects	No	Yes	Yes
Secular Time Trend	No	No	Yes
Observations	27,961	27,961	27,961
R ²	0.008	0.026	0.033
Adjusted R ²	0.008	0.025	0.031
Residual Std. Error	0.054 (df = 27959)	0.053 (df = 27911)	0.053 (df = 27910)

Note:

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

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 affected by an average wage increase.¹⁶ 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 percentage points in their turnout.¹⁷ 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 are in the same direction and not hugely different in magnitude.

4.2 Additional tests

Using panel data rather than making only cross-sectional or over-time comparisons helps to guard against many possible types of confounding. But, as noted above, it is still possible that the estimates presented here are being driven by some force other than minimum-wage increases. In this section, we present some tests to look for additional observable implications of our theory about minimum wage hikes increasing turnout.

One reason we use panel data at the level of the county, rather than using states as the unit of analysis, is so that we can run an additional test that asks whether we see the effects

¹⁶For now, we rely on rough estimates based on the EPI report referenced in Section 2, but we are currently working on estimates based on ACS microdata that should let us say something more concrete about how many people tend to be affected by minimum-wage increases. We note that it's also possible a larger proportion of the public was affected by minimum-wage increases earlier in our panel, when the minimum wage was larger in real terms.

¹⁷The 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.

of minimum wage increases concentrated in areas that should be more affected by them. We do not have good county-level measures of how many people will be affected by a given minimum wage increase, so we use median income as a proxy; we assume that places with lower median incomes should have more low-wage workers, and likely more people who would be directly affected by a minimum wage increase. In a supplementary analysis (table available on request), we interact the proportional minimum wage increase with county median income (in 2018 dollars). The coefficient on the minimum wage increase continues to be positive and significant, as in the main analysis, but the interaction with county median income is negative: counties with higher median incomes appear to show smaller turnout effects from a minimum wage increase (though this coefficient is not statistically distinguishable from zero). We think this is consistent with our effects genuinely being driven by minimum wage increases and not some larger trend that happens to coincide with wage increases. If our analysis is genuinely capturing the effect of a minimum-wage increase and not some other confounder, we should see the effects of minimum-wage increases being concentrated in areas with lower median incomes. This is generally the pattern we see in this test, which is reassuring.

It is also useful to think through the possible stories that could yield spurious estimates. In particular, an analysis like this is subject to concerns about time-varying confounders. For one thing, 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” could be making us 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 yield substantial turnout increases.¹⁸ 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.

The main estimates are also robust to a range of other specifications. Analyses that weight by county population, or that use states as the unit of analysis rather than counties, continue to return comparably-sized, statistically-significant ($p < .05$) positive estimates. Results are also robust to sequentially excluding each state’s observations from the analysis, to ensure we are not being driven by outliers.

Finally, Appendix E repeats our main analysis with a different outcome variable, to see whether minimum wage increases are associated with increased Democratic voteshare. It appears that they are, though we note that such a relationship could occur for many possible reasons.

5 Conclusion

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

¹⁸The main effect of a minimum wage increase (in this model, a state-determined one) is positive and significant. The interaction term between an increase and the “federally-determined raise” indicator is also positive, suggesting that federally-determined increases have larger turnout effects than state-determined ones. But this difference is only statistically significant in some specifications.

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 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.

We acknowledge that this is a preliminary cut at a large question, and the datasets used are imperfect for the task. We hope future researchers will do better, and will also uncover nuances we have missed. In particular, we hope that future work will be able to measure how persistent such increases in turnout are: changes in turnout that last for multiple election cycles will have more of a political impact than ephemeral ones. Further, we look forward to future research on the mechanisms underlying these effects: how much of the story is about economic resources, and how much is about the ways that raising the minimum wage changes people's attitudes about the relevance of government policy to their lives?

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A 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 at the end of 2017, 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 in 2017, 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 6736244 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 1808711 potential matches, with many duplicates. We then narrow down some of these matches. First, we discard any 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 1112398.

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 1026421.

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 895099.

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 109139 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. 62 percent of those have at least one plausible match to the voter file, with unmatched employees assumed to be non-voters.

B Parallel Trends and Placebo Tests (NYC Data)

Figure B 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 unidentified 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.

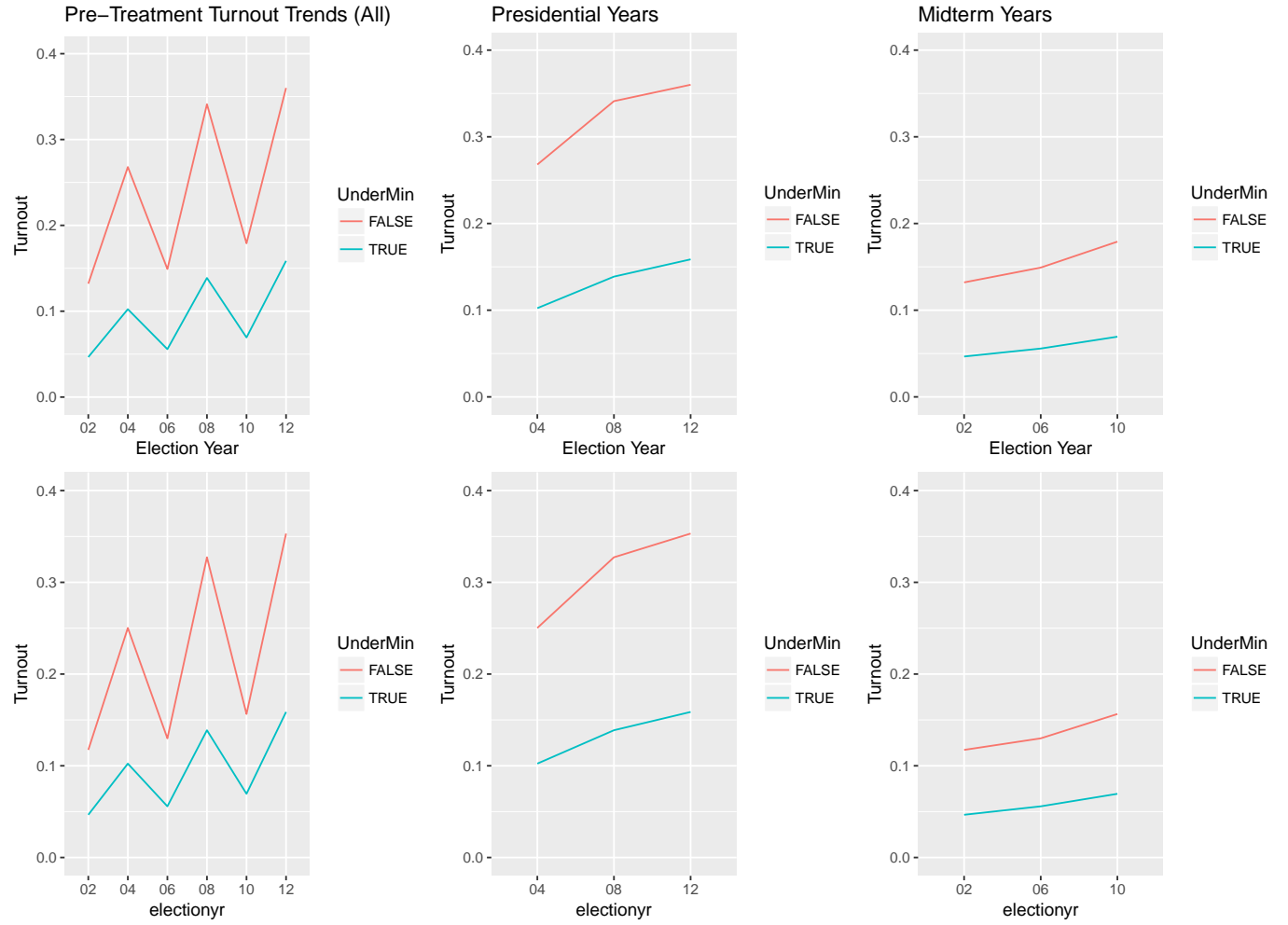


Figure 1: Plots of pre-treatment voter turnout trends among municipal employees affected and unaffected by NY minimum wage increase. The top row shows trends for all hourly workers; the bottom row shows trends for hourly workers paid under \$15 per hour.

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

Table 3 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;¹⁹ 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 involves using the same turnout data as our main analyses (2012-2016), and assigning “treatment” based on hypothetical wage increases rather than the ones that actually happened. That is, people paid below \$8/hour received a raise in 2014, while people paid more than that did not receive a raise due to the minimum wage increase. In this analysis, we drop the people actually affected by the minimum wage increases that took place between 2014 and 2016, 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

¹⁹Though 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.

analyses yield positive, significant estimates, we should worry about our main analyses. We essentially replicate the analysis from column 2 of 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 5-cent increments, all the way up to \$40/hour (this is roughly the 90th percentile of hourly wages in our data).

This approach of running many placebo tests based on arbitrarily-chosen cutpoints yields 586 estimates. Of these, 4.6% are statistically significant at $p < .05$, and the median of these estimates is 0.001 (mean 0.007). We think this distribution is consistent with chance; that is, we do not think these placebo tests suggest that our basic approach of comparing lower- and higher-paid workers’ voter turnout over time has any sort of inherent positive bias.

Table 3: Placebo Individual Difference-in-Differences Estimates

	<i>Dependent variable:</i>	
	Voted	
	(1)	(2)
Under New Min. Wage	−0.202* (0.003)	−0.189* (0.003)
2012	0.019* (0.002)	0.026* (0.002)
Under New MW * 2012	0.001 (0.003)	−0.006 (0.003)
Constant	0.341* (0.002)	0.327* (0.002)
Included Employees	All Hourly	Hourly Under \$15
Observations	223,114	175,450
R ²	0.027	0.031
Adjusted R ²	0.027	0.031
<i>Note:</i>		*p<0.05

C Different Wage Cutpoints for NYC Analyses

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 2 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 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. That’s an argument for a narrower analysis that uses a smaller control 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.

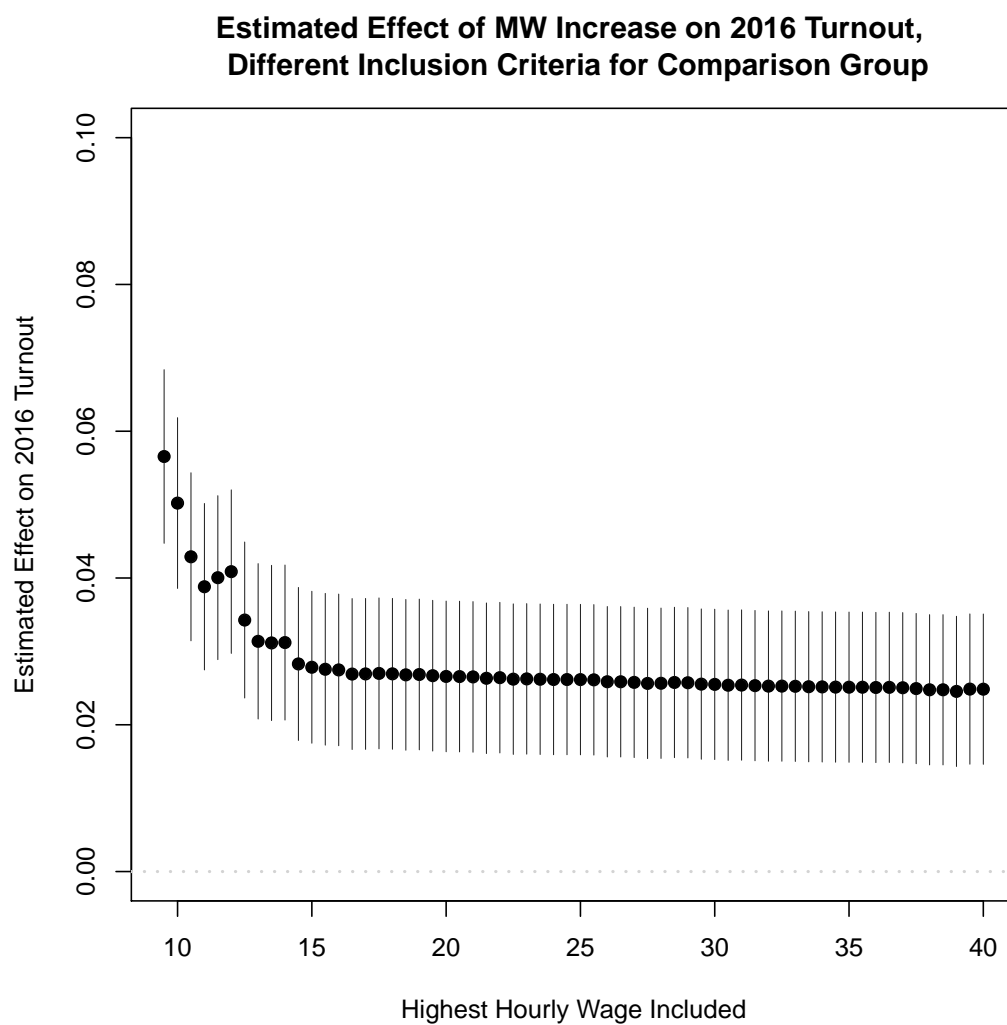


Figure 2: Estimates comparable to column 2 of Table 1, but using different wage cutpoints to determine the comparison group.

D Introducing Measurement Error to Vote Histories

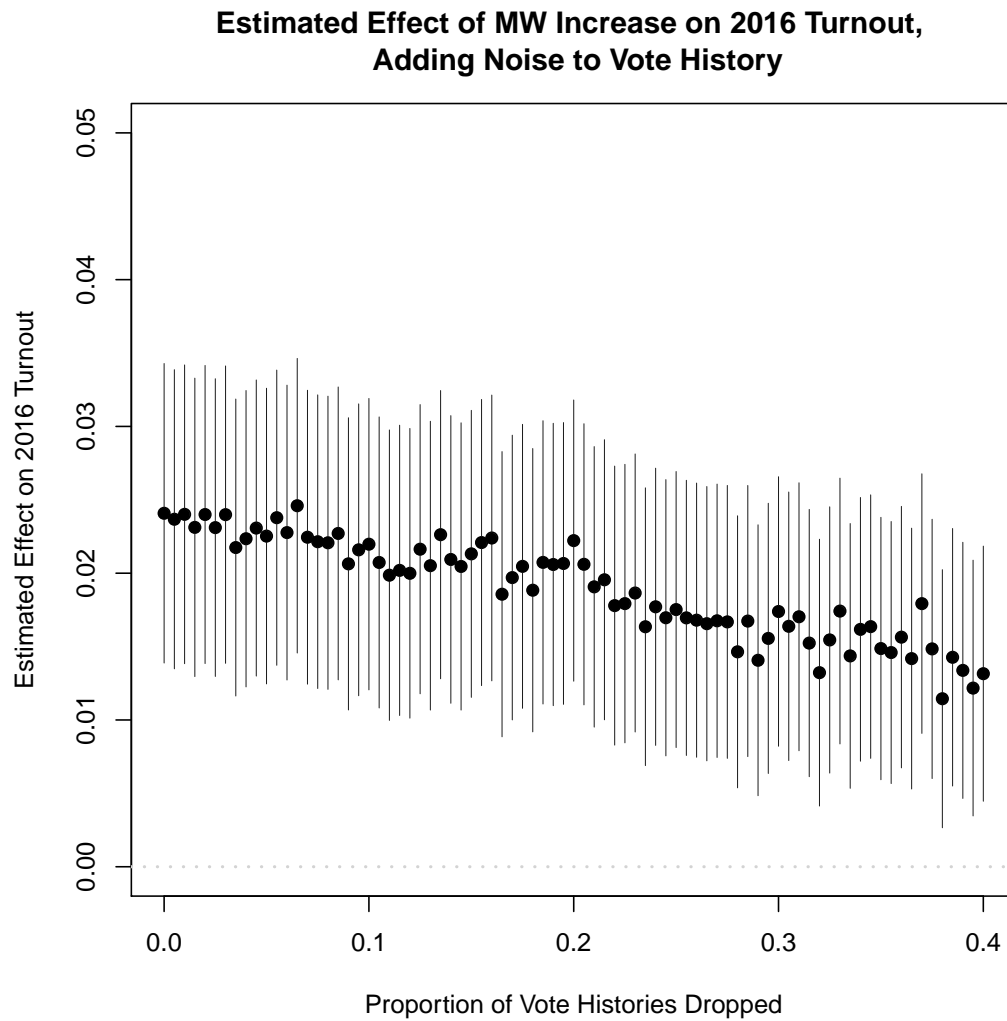


Figure 3: Estimates comparable to column 1 of Table 1, but randomly deleting some vote histories.

E Panel Data: Other Outcomes

Since minimum wage increases appear to increase voter turnout among low-wage workers, we might wonder whether the resulting change in the electorate could matter for who gets elected. Low-income Americans are more supportive of Democrats than high-income voters, so we might wonder whether Democratic candidates benefit from the increased turnout. Here we present the same types of first-difference models as in the last section, but this time we use Democratic voteshare as the outcome.

We note that turnout by low-wage workers is not the only way that a minimum wage increase could drive (or be associated with) high levels of Democratic support. Minimum wage increases have broad support and are often (though not always) enacted by Democratic governors and legislators, so it is possible that voters not personally affected by the increase could still reward Democrats for their role in raising the minimum wage. Or perhaps Democratic lawmakers fear backlash against them for raising the minimum wage, so they only do so when they anticipate a lot of support in the next election; this story would suggest that the following results do not carry a causal interpretation. Or we might envision complicated time trends in Democratic presidential support that could bias our estimates. There are a range of possible confounding stories here that lead us to urge caution in interpreting these estimates.

Table 4 presents these results for presidential election years. Here, an 8% increase in the minimum wage is associated with about a 1.3 percentage point increase in Democratic voteshare in the next election.

Table 4: Democratic Voteshare R

	<i>Dependent variable:</i>		
	Change in Democratic Voteshare		
	(1)	(2)	(3)
Min Wage Proportional Increase	0.167* (0.025)	0.162* (0.025)	0.168* (0.023)
Year			-0.002* (0.0003)
Intercept	-0.030* (0.004)		
State Fixed Effects	No	Yes	Yes
Secular Time Trend	No	No	Yes
Observations	27,972	27,972	27,972
R ²	0.065	0.090	0.142
Adjusted R ²	0.065	0.088	0.141
Residual Std. Error	0.068 (df = 27970)	0.067 (df = 27922)	0.065 (df = 27921)

Note:

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