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ABSTRACT

The Effects of Minimum Wage Increases on Poverty and Food Hardship*

We study how minimum wage (MW) increases affect poverty and food hardship in the United States from 1981–2019. Applying stacked difference-in-difference models and the Supplemental Poverty Measure (SPM), we find that a $1 MW increase reduces poverty by 0.3 to 0.7 percentage points among all working-age adults, and by 1.2 to 1.7 percentage points among individuals most likely to work in MW jobs. We also find that a $1 MW increase reduces food insufficiency by 1.5 percentage points among likely-MW workers. Effects on poverty are partially offset by higher living costs in MW-increasing states. Our findings are robust across methodological choices that have divided the recent literature. Overall, MW increases meaningfully reduce poverty and food hardship for the workers most directly affected and deliver modest improvements for the broader working-age population.

JEL Classification: I32, I38, J23, J38, J88

Keywords: minimum wages, poverty, food hardship, stacked difference-in-differences

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1 Introduction

Studies on the consequences of minimum wage (MW) increases in the United States (U.S.) have largely focused on changes in employment and hourly wages since the foundational study of Card and Krueger (1994). A growing body of evidence suggests that MW increases can compress the wage distribution, and often with small effects on employment (Dube et al., 2010; Autor et al., 2016; Cengiz et al., 2019; Harasztosi and Lindner, 2019; Cengiz et al., 2022; Bossler et al., 2024; Wiltshire et al., 2025; Vergara, 2024). The extent to which MW increases lead to reductions in poverty, however, remains contested (MaCurdy, 2015; Dube, 2019; Burkhauser and Sabia, 2007; Burkhauser et al., 2025). Most studies find small or null effects of higher MW levels on poverty (Sabia, 2008; Sabia and Burkhauser, 2010; MaCurdy, 2015; Burkhauser et al., 2025; Neumark and Wascher, 2007), while a few find more favorable effects (Dube, 2019; Addison and Blackburn, 1999; DeFina, 2008). Nearly all studies, however, apply two-way fixed effects designs that are now known to lead to potentially-biased estimates of the effects of MW increases, use an outdated poverty measure that excludes the largest tax and transfer programs, focus solely on income-based measures of poverty, examine effect heterogeneity across narrowly-defined sub-populations, and/or give insufficient attention to the mechanisms that undercut the ability of MW increases to more strongly reduce poverty.

In our investigation of how MW increases affect poverty and food hardship, we diverge from past research in five key ways. First, we advance beyond the standard two-way fixed effects estimators applied in most prior research on MWs and poverty and instead implement an identification strategy that adheres to best practices in contexts of a non-binary treatment indicator (such as MW levels). Specifically, we produce stacked difference-in-differences (DiD) estimates that match treatment and control units based on pre-treatment MW levels (Chaisemartin and D'Haultfœuille, 2024). In doing so, we produce treatment effects from a binary variable (a MW increase) for individuals in a state with a MW increase, relative to individuals in all states where the baseline MW level was comparable to that of the treatment state. We show that selecting comparison states based on similarity to treated states' pre-treatment MW levels strongly improves comparability of treated versus control units in our stacked DiD setting (see Appendix Figure A4). We validate our approach in the Merged Outgoing Rotation Groups (MORG) of the Current Population Survey (CPS), documenting that MW increases do lead to estimated increases in hourly wages in our framework, particularly for workers who are more likely to be in MW jobs. We present event study specifications and regression estimates that directly account for any differential linear pre-trends between the treatment and control groups.

Second, we focus our analyses on the Supplemental Poverty Measure (SPM), a poverty measure that includes near-cash transfers (such as those from the Supplemental Nutrition Assistance Program, or SNAP) and all tax liabilities and tax credits (such as benefits from the Earned Income Tax Credit, or EITC). The official poverty measure (OPM), in contrast, excludes such transfers and relies on an arguably-outdated poverty line; there is widespread agreement among poverty researchers that the SPM is preferable to the OPM (National Academies of Sciences and Medicine, 2023). Despite this, the OPM has been the primary focus of nearly all studies that estimate how MW increases affect poverty. The one slight exception is the working paper version of Burkhauser et al. (2025), which includes estimates based on the SPM from 2010–2019 in its appendix (the published version excludes the SPM altogether); in contrast, we use historical data on the SPM from Fox et al. (2015) to estimate how MW increases from 1981–2019 have affected the SPM, providing the first long-run estimates of how MW increases affect SPM poverty. We also document how specific features of the SPM—such as its adjustment of poverty thresholds according to local living costs (see details in Appendix Table A1)—influence how it responds to MW increases. Given that high-cost states are more likely to implement MW increases (see Appendix Figure A2), and that high-cost states have higher poverty thresholds in the SPM framework, the poverty-reduction effectiveness of MW increases may be partially offset by the higher living costs in MW-increasing states.

Third, we also investigate how MW increases affect two non-monetary dimensions of poverty: food insecurity and food insufficiency. Using the Food Security Supplement of the CPS, we replicate our primary analyses on these two dimensions of food hardship. Sabia and Nielsen (2015) are among the few to study effects on material hardship, finding no effects of MW increases on food insecurity among the working-age population (using SIPP data), but *increases* in food hardship for younger adults without a college degree. These results again rely on two-way fixed effects estimators and end in 2007, thus missing the MW increases that occurred between 2008 and 2019. Our findings update these results with more-recent data, a stronger analytical approach, and a direct comparison to effects on income-based poverty.

Fourth, our analysis primarily focuses on a subgroup of adults who, based on many observable characteristics, share a high likelihood of working in a MW job. Minimum wage workers are a small yet heterogeneous group; as such, it is unsurprising that estimates of how MW increases affect poverty at-large tend to detect small or null effects. At the same time, subgroup analyses based solely on demographic characteristics (such as low levels of education) are generally insufficient as proxies of likely-MW workers. We follow Cengiz et al. (2022) in using historical CPS data to construct a group of workers who are demographically diverse, yet share a high predicted probability of being affected by a MW increase. In doing so, we can produce more-precise estimates of how MW increases affect poverty among workers who are most likely to be directly affected by MW increases.[1]

Fifth, we investigate how certain factors may undercut the ability of MW increases

[1]Similarly, studies finding null effects of MW increases on poverty tend to point out that MW increases are poorly targeted at poor adults, given that most MW workers are not in poverty, and that most individuals in poverty are not working (Burkhauser et al., 2025). We document in Figure A3, however, that individuals who are more likely to work in MW jobs do face higher rates of poverty and food hardship than the rest of the population.

to reduce poverty. Specifically, we study (1) the extent to which positive gains from MW increases are partially offset by higher living costs in states where MW increases occur; (2) whether MW increases may reduce employment or work intensity among likely-MW workers; and (3) the extent to which MW increases reduce participation in, and/or benefit levels from, means-tested transfers such as SNAP. While there has been some evidence to suggest MW increases reduce SNAP and SSI participation[2], and substantial debate over the MW's employment effects, the role of local living costs in moderating the MW's anti-poverty efficacy has received scant attention to-date.

Our findings reveal that, across the full working-age population, the effect of MW increases on SPM poverty is generally modest in absolute magnitude. We find evidence that a $1 MW increase leads to a 0.4 percentage-point reduction in poverty in the first full year following a MW increase (90% confidence intervals: -0.7 to -0.1 p.p.), though these results decline in magnitude and precision when accounting for differential linear pre-trends. Converted to elasticities, our preferred estimates suggest that a 10 percent MW increase reduces SPM poverty by 1 percent for the working-age population, corresponding to an elasticity of –0.10. These effects are more pronounced among individuals with a high predicted likelihood of working in minimum wage jobs (elasticity of -0.20). These values are lower than those reported in Dube (2019), who finds long-run poverty elasticities between –0.220 and –0.459 for the non-elderly population, albeit focusing on the OPM.[3] In contrast, Burkhauser et al. (2025) report null or slightly positive elasticities, concluding that minimum wage increases do not reduce poverty.[4] Our results fall in between: we find consistent evidence

[2]Page et al. (2005) finds that MW "increase welfare dependence," though caution that their findings are strongly sensitive to sample period and assumptions regarding state trends. Reich and West (2015) find that a 10 percent increase in MW levels "reduces SNAP enrollment between 2.4 and 3.2 percent", while Regmi (2024) finds that MW increases reduce SSI receipt.

[3]As with most studies, this one uses two-way fixed effects models and the OPM, though the study does offer an extra analysis that includes near-cash transfers and refundable tax credits, which diminish the anti-poverty effectiveness of the MW by around one-third.

[4]When we focus on the OPM in Appendix Table B6, we find elasticities for the OPM comparable to Burkhauser et al. (2025), while finding notably larger elasticities for the SPM.

that MW increases reduce SPM poverty, but generally at smaller magnitudes than those reported in Dube (2019). Beyond our baseline results, we also reveal several subtleties regarding *who* benefits from MW increases, and *why* MW increases are not more effective at reducing SPM poverty.

We find that among full working-age population, the poverty-reduction effects of MW increases are partially offset by the higher living costs in states that have implemented MW increases (given that the SPM thresholds adjust for local living costs). Specifically, we show that MW levels are very strongly, positively correlated with the local living cost adjustment applied to SPM thresholds, and we find that the effect of MW increases on poverty are reduced by around 30 percent or greater (depending on the specification) when accounting for variation in living costs. Concretely, in our preferred set of estimates that account for differential linear pre-trends, we find that a $1 MW increase reduces poverty by 0.7 percentage-points (6 percent relative to the pre-treatment mean) when we do not factor local living costs into the SPM threshold, in contrast to a 0.3 percentage-point reduction when accounting for local living costs. We do not find evidence of meaningful employment effects or interactions with major tax and transfer programs.

Regardless of the particular SPM measure applied, we consistently find stronger (more negative) effects of MW increases on poverty when focusing on our sub-sample of likely-MW workers. Among this group, we find that a $1 MW increase reduces SPM poverty by 1.2 percentage points in our preferred set of estimates (90% CIs: -2.4 to -0.1 p.p.). The effect climbs to a 1.7 percentage-point reduction (90% CIs: -2.5 to -0.9 p.p.) when we do not account for the SPM's geographic cost adjustments. Our results consistently suggest that MW increases are effective at reducing poverty among the adults who are more likely to benefit from such changes.[5]

[5] As we detail in Section 2, our classification of likely-MW workers is not conditional on current employment status; thus, our favorable effects of MW increases on poverty for this group take into account potential employment responses, which we also provide evidence on.

We also find that MW increases reduce food hardship, particularly for our likely-MW sub-sample. Among this group, a $1 MW increase reduces food insecurity by 2 percentage points (90% CIs: -4.1 to 0.1 p.p.), and food insufficiency by 1.5 percentage points (90% CIs: -2.7 to -0.4 p.p.), with the strongest negative effects occurring in the first full year after the implementation of the MW increase (similar to our poverty findings). The consistency of results across poverty and food hardship adds further confidence that MW increases are effective at promoting economic well-being, particularly for the individuals who are most likely to work in MW jobs. We conclude our study with a broad set of sensitivity tests, and we confirm that our findings are not sensitive to the inclusion of the control variables and place-based fixed effects that are central to recent disputes between Dube (2019) and Burkhauser et al. (2025).[6]

2 Data and Research Design

This section describes our data sources and key outcome measures, outlines the research design and identification strategy, details our estimation and inference procedures, defines the analytic sample, and concludes with a validation of the identification strategy.

2.1 Data Sources

Our analytical aim is to estimate the effect of MW increases on SPM poverty and food hardship. To do so, we use three versions of the Current Population Survey (CPS). To estimate effects on SPM poverty, we use the Annual Social and Economic Supplement (ASEC)

[6]Burkhauser et al. (2025), which is largely a rebuke of Dube (2019), argues that (1) states outside of a treatment state's region should be allowed to serve as control units in a difference-in-differences setting (in contrast to the results produced by Dube (2019)) and that (2) controlling for "state house price index and the unemployment and average wage rates among more highly educated individuals" is more appropriate for capturing macroeconomic trends than the inclusion of measures of state GDP or the unemployment rate, which could be affected by MW increases. Under these conditions, the authors find no effects of MW increases in poverty. We show that our framework produces consistent conclusions across these modeling decisions.

of the CPS. Our ASEC data span 1981 to 2019 (the latest year available prior to the onset of the COVID-19 pandemic). We download the ASEC data from IPUMS and add the SPM poverty measure for the pre-2010 period from the historical SPM series provided by Columbia University's Center on Poverty and Social Policy (Fox et al., 2015; Flood et al., 2024). For our food hardship analyses, we use data from the December Food Security Supplement of the CPS from 2001 (first year of consistent data availability) through 2019.

To validate that our stacked DiD models (discussed below) find positive effects of MW increases in hourly wages, we use the Merged Outgoing Rotation Groups (MORG) of the CPS. The MORG provides the highest-quality information on hourly wages among CPS files. As with the CPS ASEC, we use data from 1981 to 2019 when demonstrating effects of MW increases on hourly wages.

We will assess the sensitivity of our results to the inclusion of several state- and year-level controls, including data on house prices from the OECD; the unemployment rate and annual earnings of high-skilled workers from the ASEC; and the generosity of Earned Income Tax Credit (EITC), Supplemental Nutrition Assistance Program (SNAP), and Aid to Families with Dependent Children/Temporary Assistance for Needy Families (AFDC/TANF) transfers from the University of Kentucky's Center for Poverty Research (UKCPR).

2.2 Outcomes of Interest

Our primary outcome is poverty status according to the Supplemental Poverty Measure (SPM). Different from the official poverty measure (OPM), the SPM includes non-cash benefits (such as those from SNAP) and tax-based transfers, such as benefits from the EITC. Moreover, the SPM poverty threshold is based on recent consumptions standards of food, clothing, shelter, utilities, and a little more, while thresholds vary geographically based on local housing costs. We elaborate on differences between the two measures, and validate the usefulness of the SPM's geographic price adjustments, in Appendix A. We prioritize the SPM given its inclusion of all taxes and transfers, but we provide results with the OPM as the outcome in Appendix B6.

In our food hardship analyses, our primary outcomes of interest are food insecurity and food insufficiency. Food insecurity refers to "low or very low food security" in the reference year as defined following the US Department of Agriculture's standard definition.[7] Low food security occurs when "Households reduced the quality, variety, and desirability of their diets, but the quantity of food intake and normal eating patterns were not substantially disrupted," while very low food security entails that "At times during the year, eating patterns of one or more household members were disrupted and food intake reduced because the household lacked money and other resources for food." By contrast, food insufficiency refers only to the latter, representing a more severe subset of food insecurity.

In supplementary analyses, we also use data on hourly wages (self-reported, continuous indicator within the MORG), whether active in the labor force (ASEC), whether employed (ASEC), hours worked per week (ASEC), weeks worked per year (ASEC), SPM unit's receipt of various income transfers (ASEC), and SPM unit's benefit levels of various income transfers (ASEC), including SNAP, EITC, AFDC/TANF, and Supplemental Security Income (SSI) Benefits.[8] We provide more information on all variables used and their data sources in Appendix Table A2.

2.3 Research Design and Identification

We employ a stacked difference-in-differences design to estimate the effects of MW increases, which accommodates staggered treatment adoption, continuous treatment intensity, and

[7] In the CPS, the food security measures are based on a set of 18 questions regarding the household's food intake.

[8] SPM unit refers to the resource-sharing unit when defining poverty status. In more than 95 percent of cases, the SPM unit is identical to the household.

repeated treatments. Minimum wage increases vary in timing and magnitude across states, with jurisdictions often implementing multiple increases over time. This structure poses challenges for traditional two-way fixed effects (TWFE) estimators, which assume homogeneous treatment effects and rely on untreated units, whether never-treated or not-yet-treated, as controls across all periods. Such pooling of controls can violate the parallel trends assumption and generate biased estimates. Indeed, recent work has highlighted that TWFE estimators may fail to satisfy basic identification properties, including the no-sign-reversal condition, and often yield results that are difficult to interpret when treatment timing and intensity vary across units (Chaisemartin and D'Haultfœuille, 2020; Chaisemartin and D'Haultfœuille, 2023; Dube and Lindner, 2024; Callaway et al., 2024).

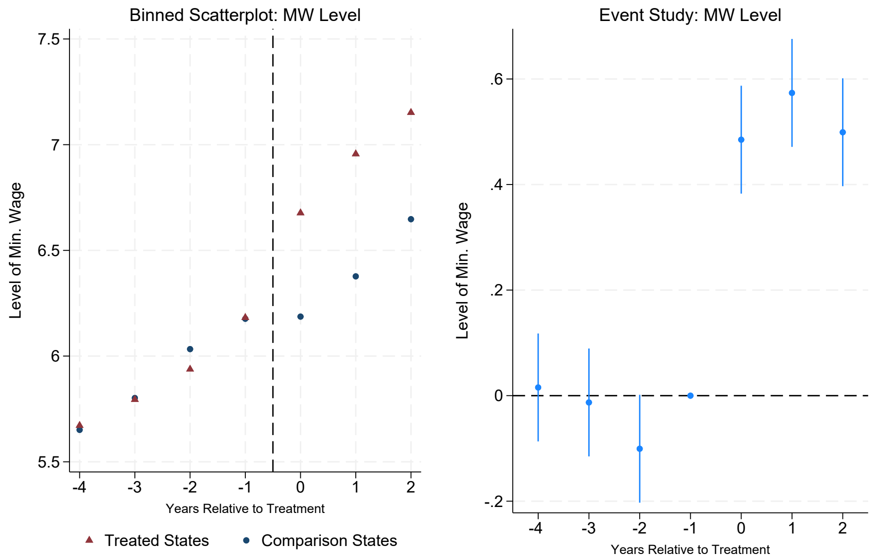
To select appropriate controls under staggered treatment adoption (e.g. Goodman-Bacon (2021); Chaisemartin and D'Haultfœuille (2023)), we construct a separate event-specific dataset for each MW increase following Cengiz et al. (2019). These "stacks" restrict observations to a seven-year window, which include four years before and three years after the policy change. This design fixes comparisons in the same calendar years for both treated and control units, limiting exposure to unrelated temporal shocks and improving the credibility of the parallel trends assumption. For example, in evaluating a 2007 MW increase, we use data only from 2003 to 2010 for both treated and control groups.

We follow best practices from Chaisemartin and D'Haultfœuille (2024) in (1) operationalizing MW increases as binary events rather than as continuous treatments while (2) selecting control units based on their MW level in the year prior to treatment ($D_{t=-1}$) relative to treated units. The latter step, which differentiates our setting from those of Burkhauser et al. (2025) or Dube (2019), allows us to address a key challenge in settings with non-binary treatments: defining a credible counterfactual when treatment varies not only in timing but also in intensity (Baker et al., 2022; Chaisemartin and D'Haultfœuille, 2024; Callaway et al., 2024). Matching on pre-treatment MW levels strengthens identification by mitigating biases that arise under overly stringent parallel trends assumptions. As Chaisemartin and D'Haultfœuille (2024) emphasize, omitting such matching implicitly assumes that units with divergent policy histories would have followed parallel trends absent treatment, which is a strong and often untenable assumption.[9]

In our setting, matching treated and control units on their baseline MW levels achieves two objectives. First, it provides a transparent and replicable selection rule for control units, which has been a focal point of debate in the MW literature (Burkhauser et al., 2025; Dube, 2019). Second, and importantly, it allows us to retain a wider pool of control units by not automatically excluding states that have experienced wage increases within the treatment window. Unlike the approach in Cengiz et al. (2019), who exclude any control unit with non-trivial prior increases, we allow for policy changes in both treated and control states, as long as their pre-treatment MW levels are comparable. As Dube and Lindner (2024) note, the high frequency of policy changes complicates the isolation of causal effects, especially when earlier increases have persistent impacts. We argue that, conditional on pre-treatment MW levels, the timing of *other* MW increases—that is, those that occur in the event-study window but do not define treatment—is orthogonal to our treatment indicator. In other words, we assume that past and future MW increases are approximately evenly distributed between our treatment and control groups. We demonstrate the validity of our argument in Figure 1. As shown, MW levels evolve in parallel for treated and control units, except at treatment time. By anchoring treatment comparisons on shared policy baselines, our design improves credibility in the presence of staggered and continuous treatment variation. That said, we will also present results that condition on no subsequent MW increases in the post-treatment window (Appendix Table B4).

[9] In the present case, this essentially requires that states with different MW baselines would have followed parallel trends under no further increases, which in turn requires that any increases that generated those differences do not affect the trajectory of the outcome of interest.

Figure 1: Minimum wage trends for treatment and comparison units as binned scatterplot (left) and event study (right)



Note: The figures plot MW levels for treated and control units before and after treatment. The estimates in the right panel are from a stacked event study centered on state-year minimum wage increases of at least \$0.10 and with comparison groups having near-identical (within \$0.10) MW levels in the year prior to treatment. We provide empirical tests of pre-treatment trends and levels in Appendix Figure A4. MW levels are presented in nominal terms.

Parameters: Our approach requires us to set two key parameters in defining our treatment and control units: the minimum size of a MW increase to qualify as a treatment event, and the "tolerance" level of differences in pre-treatment MW levels to match control units to treated units. Selecting these two parameters reflects trade-offs between stricter matching criteria that limit the number of eligible events versus more-relaxed criteria that increase the count of events but may introduce imbalance between treatment and control units. In our baseline specification, we focus on MW increases of at least \$0.10 and restrict comparison states to those with pre-treatment MW levels in the year prior to treatment within \$0.10 of the treated state's MW.[10] In practice, these criteria produce 156 unique

[10] A 10-cent tolerance allows us to retain states with nearly identical baseline MWs. By contrast, exact match would force us to drop several states that increase MWs from idiosyncratic baseline levels that lack exact matches (imagine a situation where only one state has an MW with exact \$7.73/hr).

events (or stacks), which we list in Table A5. We show in Figure A4 that our preferred parameters are effective in generating a balanced set of treatment and control units with common pre-treatment MW levels and trends. That said, we test the sensitivity of our results to a wide range of MW increases and tolerance thresholds (see Figure B4), as we discuss later.[11]

Figure 1 supports the plausibility of a conditional parallel trends assumption, in which differences in outcomes are driven primarily by the treatment itself, conditional on similar pre-treatment trajectories. To further validate our matching strategy based on pre-treatment MW levels, we also compare pre-treatment trends and average levels of the MW across different matching tolerances and treatment thresholds, as well as against a broader set of untreated states. Appendix Figure A4 shows that differences in pre-treatment MW levels and trends decline substantially as the matching tolerance narrows. Our preferred specification, indicated by the blue line at a $0.10 matching tolerance, is effective in aligning both pre-trends and pre-treatment means between treated and control units. In contrast, the "No Exclusion" specification, which mechanically includes all untreated states, exhibits large divergence in both trends and levels. These results support our decision to restrict the control groups to units with comparable baseline MW levels instead of using broad comparison groups of all untreated states, or all untreated states within a given region.

[11] Our framework mechanically rules out inclusion of federal MW increases (which affect all states with MW levels below the new mandated MW level) as treatments. This is because a state-year that is treated (i.e. experiences a MW increase of at least $0.10) cannot serve in the control group for other states within the same year. For federally-mandated increases, treated states thus have no set of comparison states that meet our criteria for matching, as all other states with comparable MW levels prior to treatment have also received the treatment.

2.4 Estimation and Inference

To estimate the treatment effect, we stack the event-specific datasets described above and estimate the following equation:

$$\text{outcome}_{g,s,t} = \sum_{\tau=-4}^{-2} \delta_\tau \cdot D_{g,s,\tau} + \sum_{\tau=0}^{3} \beta_\tau \cdot D_{g,s,\tau} + \mathbf{X}'_{g,s,t}\theta + \mu_{g\times t} + \gamma_s + \varepsilon_{g,s,t} \tag{1}$$

The subscript g denotes the stack, s the state, t the calendar year and τ the year relative to the event. δ_τ collect the pre-treatment coefficients, and β_τ the post-treatment coefficients of interest, $D_{g,s,\tau}$ is an indicator for event time τ relative to the first year of treatment. The vector $\mathbf{X}_{g,s,t}$ includes covariates. $\mu_{g\times t}$ denotes stack-by-year fixed effects and γ_s represents state fixed effects. $\varepsilon_{g,s,t}$ is the residual. The stack-by-year fixed effects $\mu_{g\times t}$ absorb all time-varying shocks common to each stack, while state fixed effects γ_s control for persistent differences across states. The treatment indicator $D_{g,s,\tau}$ equals 1 for units in state s and stack g at event time τ, where treatment corresponds to an increase in the minimum wage as defined above. Our baseline specification includes a large set of person-level covariates detailed in Appendix Table A2. We cluster standard errors at the state level, where treatments take place to control for potential serial correlation in the error term within each state.

To emphasize, control states used for each MW event are selected based on (1) not being treated in period t but having (2) a minimum wage level at $t-1$ that is within 10 cents of the treated state's MW at $t-1$. In our baseline specification, control states can experience increases in MW levels in $t+1$ to $t+3$, but we show in Figure 1 that control states exhibit a similar treatment path as the treated states throughout the post-treatment window. We provide a list of all 156 treatment events (state-years with qualifying MW increases) and the comparison states used in Appendix Table A5. For reference, the average MW increase is around \$1, which represents a 13 percent increase from the pre-treatment MW. Further, our preferred specification accounts for differential linear pre-trends, which helps mitigate concerns that treatment and control groups were evolving along distinct trajectories even in the absence of treatment. Allowing for such group specific trends ensures that our estimates are not mechanically driven by systematic differences in pre-treatment dynamics.

To account for the continuous dose of the treatment, we rescale the estimated effects by the size of the MW increase ($\hat{\beta}_{\tau}^{\$1} = \frac{\hat{\beta}_{\tau}}{\Delta MW_{g,s}}$). This approach allows us to interpret the coefficients as the effect of a \$1.00 increase in the minimum wage, standardizing treatment intensity across events (Chaisemartin and D'Haultfœuille, 2024). By scaling outcomes relative to the dose, we ensure that the estimated effects reflect marginal responses rather than conflating large and small policy changes, which enhances the comparability of estimates across stacks. We also present results without rescaling in Appendix Table B8.

Controls: Our primary estimates include controls for sex, race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic), marital status, presence of children, attainment of high school degree, attainment of university degree, number of families sharing the household, and a cubic of age. As noted before, all models also include stack-by-year and state fixed effects. One source of contention in recent studies on the MW and poverty is which macro-level and place-based fixed effects (beyond state dummies) to include in models (Burkhauser et al., 2025; Dube, 2019). Burkhauser et al. (2025) show that the decision to include controls for state house prices, the unemployment rate among higher-educated (prime-age) adults, and the mean wage for higher-educated adults can affect estimates of the MW's effects on poverty. Moreover, limiting comparison states to "close controls" through region-by-year fixed effects can also affect findings (Dube, 2019). Given our approach to matching treated and control states on pre-treatment MW levels, we do not include additional place-based or macro-level controls in our analysis. We document later, however, that estimates from our identification strategy are not sensitive to the inclusion of the Burkhauser et al. (2025) controls or the Dube

(2019) fixed effects. This is likely due to our identification strategy being built, at baseline, to compare treated states only to states with similar MW levels at baseline (given also that state MW levels are correlated with other state-year economic indicators) (Chaisemartin and D'Haultfœuille, 2024).

2.5 Sample

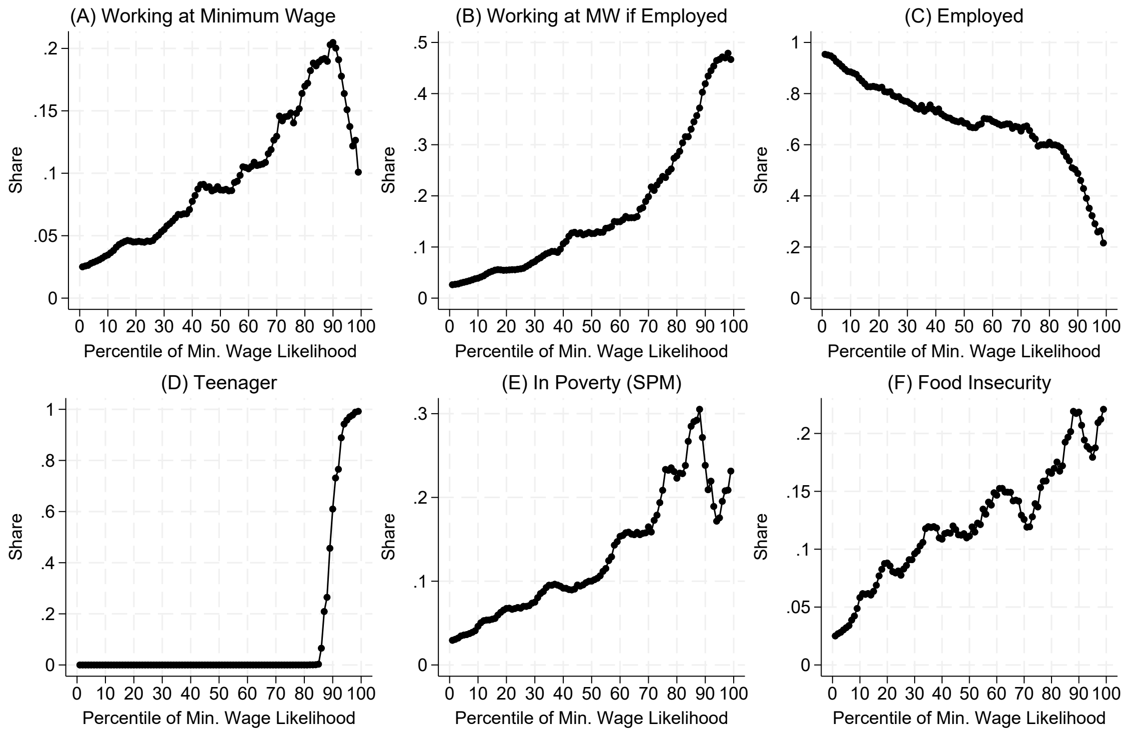
Our initial sample includes all individuals who are between ages 16 and 64. Throughout much of our analysis, however, we focus on a group of demographically-heterogeneous workers who share a high predicted probability of working in MW-level jobs. This narrowed focus allows us to produce estimates that are closer to a local average treatment effect of MW increases on the group most likely exposed. To identify this group, we follow Cengiz et al. (2022) and Card and Krueger (2015) in applying a linear probability model to predict the likelihood that individuals earn an hourly wage below 125% of the statutory minimum wage based on a set of demographic characteristics. Specifically, the model includes three-way interaction terms between teenage status, non-White racial identification, and sex; three-way interactions between young adulthood status (ages 20-25), non-White racial identification, and sex; three-way interactions of age, education status, and sex; quadratic and cubic terms for age; and indicator variables for Hispanic and non-White individuals. We predict this model in the MORG, which has direct information on hourly wages, and we export the predicted probabilities into the CPS ASEC based on shared demographic characteristics in the MORG and ASEC.[12] In our primary sample, we define workers to be at high likelihood of being in MW jobs if they are in the 20% of the population with the highest predicted probability of being exposed to the minimum wage. Importantly, inclusion in this group is not conditional on actual employment (otherwise, our sample would only include employed adults,

[12] We cannot produce estimates of poverty status in the MORG, while the ASEC does not have a direct measure of hourly wages, hence our use of both datasets in this process.

empirically preventing the possibility that MW increases could negatively affect poverty through employment declines). In sensitivity analyses, we show estimates across the full range of predicted probabilities for maximum transparency.

Figure 2 visualizes employment, age, poverty, and food insecurity characteristics across the full distribution of the predicted probability of MW likelihood. Panel A shows the share of each percentile working near the minimum wage (within 20 percent of the state-year statutory MW level), while Panel B plots the same but conditional on employment. Panel B, in particular, shows that our model works well in predicting MW work: among the employed, MW employment increases linearly with the predicted probability ranking. In Panel A, this is true up until the 90th percentile, after which the share drops sharply; this is due to the fact that employment in general declines in this upper part of the distribution, as Panel C makes clear. Panel D shows that age is an important factor in this set of patterns: teenagers are strongly concentrated in the highest percentiles of MW-likelihood (and are also less likely to be employed in a given year). To emphasize, Panel D does *not* suggest that most MW workers are teenagers; instead, it indicates that, conditional on being a teenager, one is at high likelihood of working in a MW-level job, if employed.

Figure 2: Characteristics of adults (whether employed or not) across the distribution of predicted likelihood of being a minimum wage worker



Note: The likelihood of being an adult who would work in a MW-level job is defined as the predicted probability of earning an hourly wage below 125% of the statutory minimum wage based on age, race/ethnicity, sex, education, and their interactions (see Table A2). In Appendix Figure A3, we present results only for employed adults.

Finally, Panels E and F present the share of the percentiles who are in SPM poverty and experiencing food insecurity. Poverty increases linearly with MW likelihood until the 80th percentile, after which the poverty status dips slightly. Similarly, food insecurity increases near-linearly with MW likelihood. Recall that our primary analyses will focus on the top 20 percent of this predicted likelihood distribution, though we present alternative results with all possible cutoffs in Figure 8. These figures add high confidence that our subgroup of likely-MW worker are, indeed, more likely to have hourly wages near the MW if employed, and tend to have higher levels of poverty and food insecurity than the rest of the population.

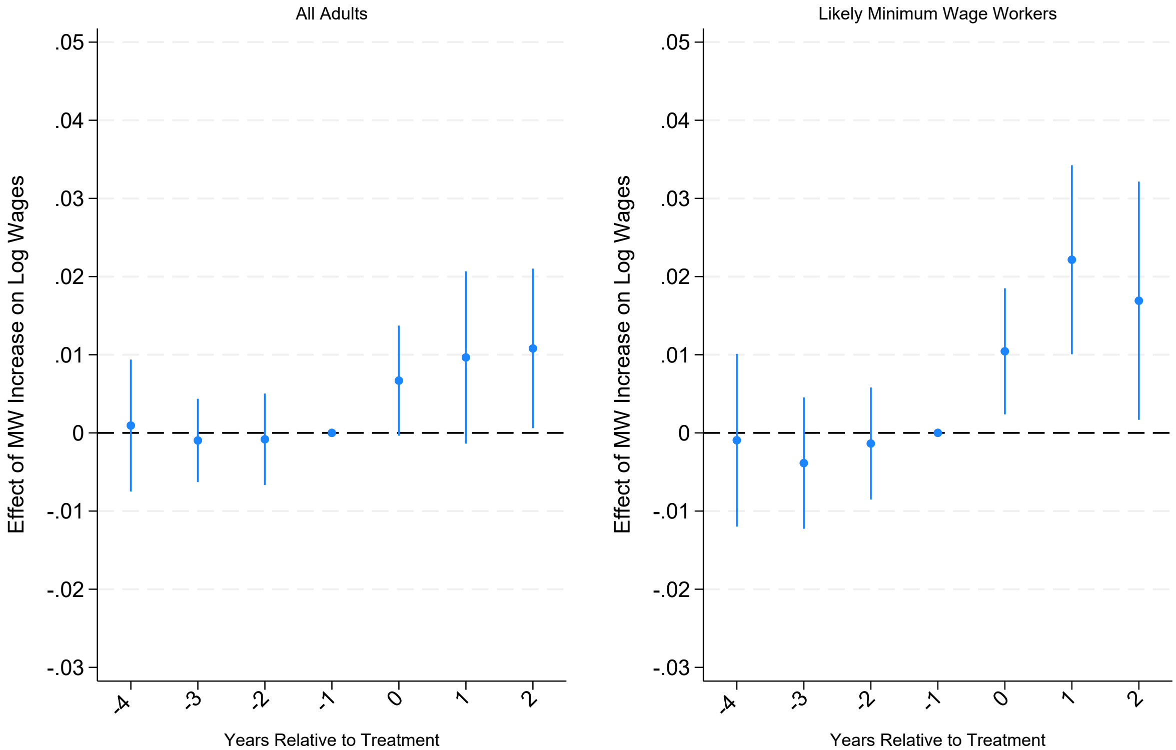
Appendix Table A3 provides direct comparisons of characteristics for workers with wages near the MW versus workers with wages above at least 20 percent of the MW. (Near-)MW workers are 13.8 percent of the employed labor force in our sample. Of this group, 15.3 percent are teenagers (compared to 3 percent for non-MW workers), 17.4 percent are Hispanic (compared to 10.4 percent), and 13.4 percent are Black (compared to 10.4 percent). MW workers are less likely to be lead earners in their household (36 percent), and are far more likely to be in poverty (23.9 percent of such workers, compared to 5.3 percent for non-MW workers). Consistent with most studies of the MW increases on poverty, these descriptive findings make clear that most MW workers are not in poverty, yet MW workers still have much higher poverty rates compared to workers with jobs above the MW (Card and Krueger, 1995).

2.6 Validation of Identification Strategy

As a final step before presenting our findings, we test the validity of our identification strategy on the most direct outcomes that MW increases should influence: hourly wages. We perform this estimate directly in the MORG for working adults at-large and for our subgroup of likely-MW workers (defined above). We present the event study specification in Figure 3. With flat pre-trends across all three subgroups, we find that MW increases do, indeed, increase wages, and with stronger treatment effects for likely-MW workers relative to our full-adult sample (Cengiz et al., 2019).

For the likely-MW workers, the point estimate is strongest in *t+1*, or the first full year after the MW increase. In many states, MW increases in the treatment year are not implemented at the start of the new year (January 1), and thus may have reduced effects on hourly wages relative to MW levels that are enforced for a full year.

Figure 3: Effects of minimum wage increases on log wages



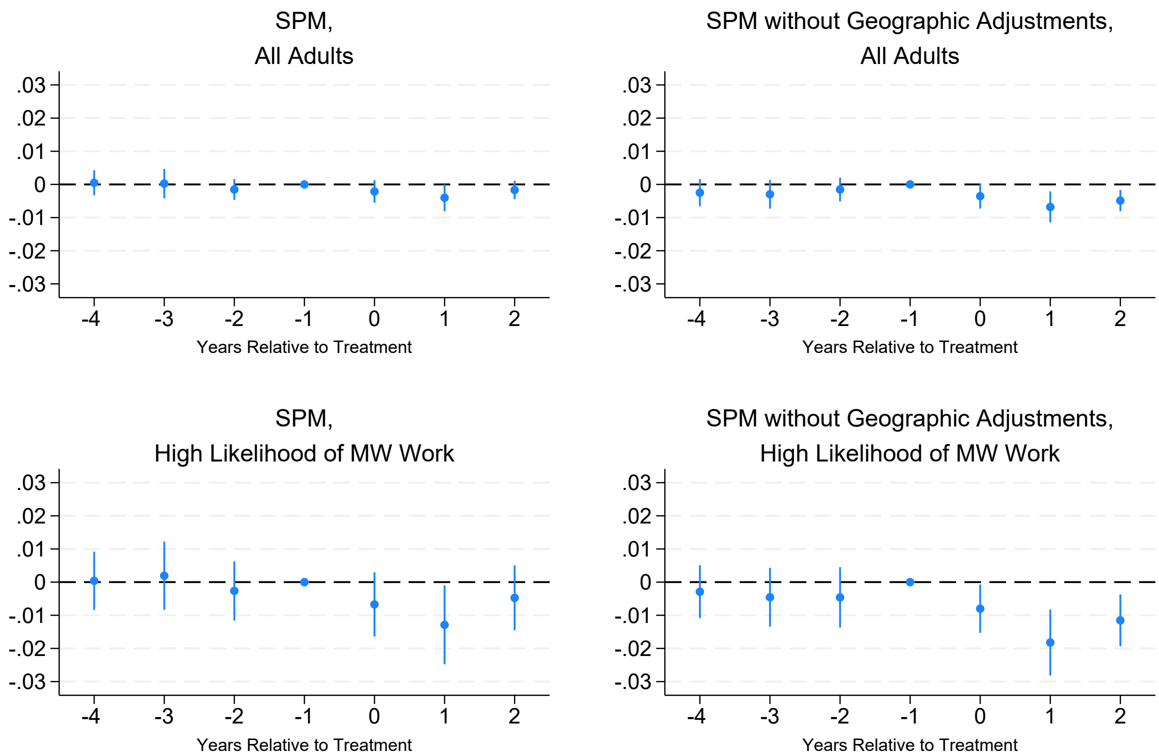
Note: The estimates are from a stacked event study centered on state-year minimum wage increases of at least $0.10 and with comparison groups having near-identical (within $0.10) MW levels in the year prior to treatment. The model controls for sex, racial and ethnic status, marital status, presence of children, education, number of families living in a household, age, state fixed effects, and stack-by-year fixed effects. The data are from the Merged Outgoing Rotation Groups of the Current Population Survey. The sample is limited to working adults between the ages of 16 and 64. Hourly wages are reported directly in the data but are deflated to 2014 USD using the consumer price index. Likely Minimum Wage Workers are those with a predicted level of minimum wage work above the 80th percentile based on demographic characteristics. Point estimates presented with 95% confidence intervals.

3 Results

3.1 Effects of Minimum Wage Increases on Poverty

Figure 4 presents our event study specification for the effects of MW increases on SPM poverty rates among all workers (top row) and likely-MW workers (bottom row). The left-most panels presents results for the standard SPM, whereas the right-most panels present results without the SPM's geographic adjustments of the poverty line. Recall that we analyze this latter outcome to understand whether the higher geographically-adjusted poverty thresholds in states that are more likely to increase the MW dampens some of the MW's anti-poverty effects. Along all adults, we find evidence of small, negative effects of MW increases on the SPM poverty rate, particularly in the first full year of treatment (-0.4 p.p. decline). Effects are slightly stronger when applying the SPM without geographic adjustments (upper-right panel), particularly in the first full year after treatment.

Figure 4: Effects of minimum wage increases on poverty (SPM) among working-age adults



Note: The estimates are from a stacked event study centered on a state-year minimum wage increase and with comparison groups having near-identical minimum wage levels prior to treatment. "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "SPM" refers to Supplemental Poverty Measure. Sample limited to individuals between ages 16 to 64. Point estimates presented with 95% confidence intervals.

It is perhaps unsurprising to find small effects of MW increases on poverty among all working-age adults, given that MW workers are a small share of the employed adult population, let alone the full (working-age) adult population regardless of employment status. The bottom row of Figure 4 presents results when limiting our sample to our subset of likely-MW workers. Applying the standard SPM, we find evidence of a reduction in poverty that again peaks in the first full year after treatment (1.3 p.p. decline). The right panel shows estimated effects without the SPM's geographic adjustments applied to poverty thresholds.

The results suggest declines in the likelihood of poverty after the MW treatment that persist throughout the post-treatment window. The effect size remains largest in the first year after treatment, consistent with the year that produced the largest wage gains from the MW increase (see Figure 3).

Table 1: Estimated effect of minimum wage increases on poverty

	Average Effect from t=0 to t=2		**Effect at t=1**		**Effect at t=1 (with DLP)**	
Group	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)
All	−0.002	−0.003**	−0.004**	−0.005**	−0.003	−0.007***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
High Likelihood of MW Work	−0.008**	−0.010***	−0.013**	−0.015***	−0.012+	−0.017***
	(0.004)	(0.003)	(0.005)	(0.005)	(0.007)	(0.005)

Note: Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The pre-treatment mean SPM poverty rates for the demographic groups are 11.5% (All) and 20.7% (High Likelihood of MW Work). "DLP" refers to differential linear pre-trends, which are accounted for in Columns 5 and 6. "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "SPM" refers to Supplemental Poverty Measure.

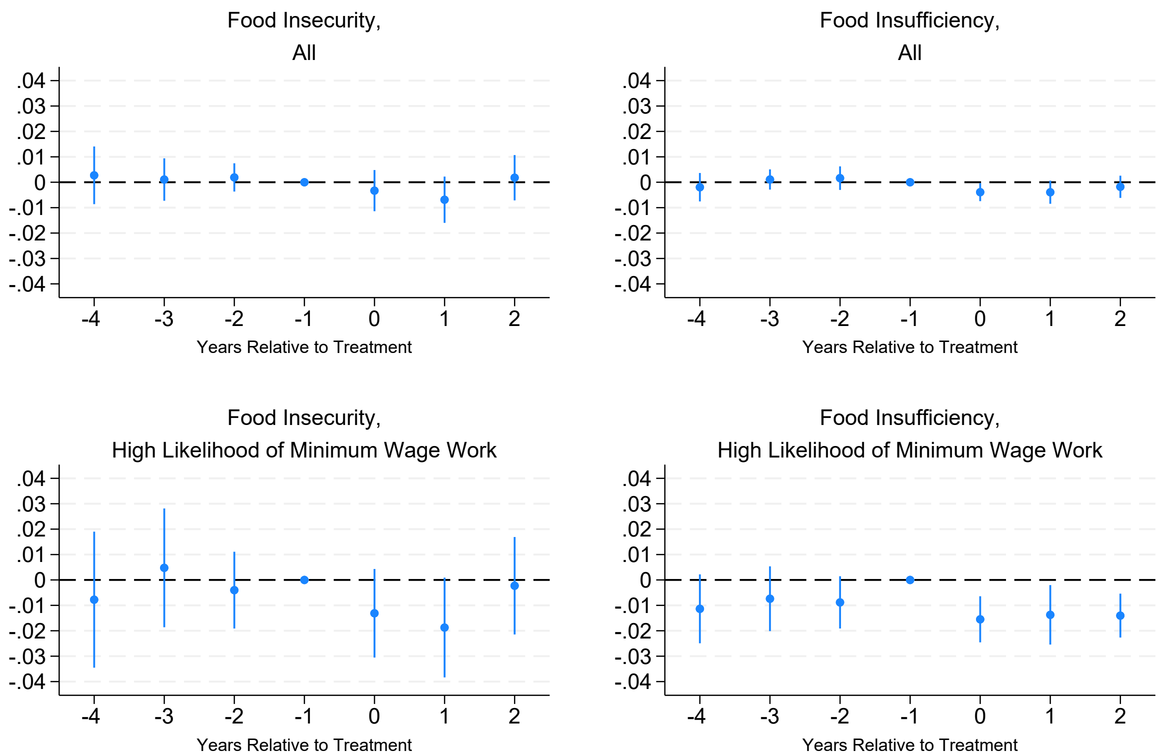
Table 1 extends the results, in table form, to quantify the magnitudes of these declines and to present our preferred results that directly account for differential linear pre-trends. The first two columns present our average effects during our full post-treatment period. Among our likely-MW workers sample, a $1 MW increase leads to a decline in poverty between 0.8 to 1.0 percentage points (for the SPM with and without geographic cost adjustments, respectively). As Figure 4 suggested, and as this table confirms, the strongest reductions due to MW increases occur at $t = 1$, or the first full year after treatment. Among our full sample, we find that a $1 MW increase leads to declines in poverty of 0.4 or 0.5 percentage points, depending on the measure; among our likely-MW sample, we find declines of 1.3 or 1.5 percentage points. The final two columns present our preferred estimates: effects at $t = 1$ that directly account for any differential linear pre-trends. Among our full sample, we find a decline in SPM poverty of 0.3 percentage points (90% CIs: -0.63 to 0.03 p.p.), though this climbs to a 0.7 percentage point decline for the SPM without geographic price adjustments (90% CIs: -1.03 to -0.37 p.p.). Among our likely-MW sample, a $1 MW increase leads to a decline in SPM poverty of 1.2 percentage points (-2.35 to -0.05 p.p.), and 1.7 percentage points (-2.52 to -0.88 p.p.) without the geographic adjustment. Consistently, we find that the effects of MW increases are stronger when we do not adjust for geographic price differences, in line with our earlier observation that MW increases tend to occur in higher-cost states (which have higher poverty thresholds). In our preferred estimates, geographic price differences "offset" about one-third of the MW's poverty reduction effects for our likely-MW worker sample, and more than half of the effect for the sample at-large (consistent with Godoey and Reich (2021)'s estimated effects for wages).

For context, the pre-treatment mean SPM rate for our likely-MW worker sample was 21.5 percent (or 20.5 percent without the geographic adjustments); thus, a 1.2 percentage point decline represents a 6 percent relative decline in poverty as a result of a $1 MW increase (or an 8 percent decline for the SPM without geographic adjustments). Among all individuals, the 0.7 percentage point decline in the SPM without geographic adjustments amounts to a 6 percent decline due to a $1 MW increase. We convert these effects to elasticities in Appendix Table B7. We present heterogenous effects across other demographically-defined groups in Appendix Table B1.

3.2 Effects of Minimum Wage Increases on Food Hardship

Figure 5 presents our event study specification for our measure of our two measures of food hardship: food insecurity and food insufficiency. Results generally mirror our findings when evaluating poverty: a $1 MW increase produces its strongest negative effects for each group and outcome in the first full year after treatment, and produces stronger negative effects for our likely-MW sample.

Figure 5: Effects of minimum wage increases on food hardship among working-age adults



Note: The Y-axis represents the average effect of a $1 minimum wage increase on food hardship (food insecurity in the left panels, and food insufficiency in the right panels). Authors' analyses from the Food Security Supplement of the Current Population Survey (2001 to 2019). Estimates bounded by 95% confidence intervals. "Food Insecurity" refers to either low or very low rates of food security. "Food Insufficiency" refers only to very low rates of food security. The pre-treatment mean food insecurity rates for the treatment group are 13.6% (All) and 21.5% (High Likelihood of MW Work). The pre-treatment mean food insufficiency rates for the treatment group are 4.9% (All) and 7.5% (High Likelihood of MW Work).

Table 2 summarizes the magnitude of the point estimates and presents our results when accounting for differential linear pre-trends. Among all adults, we find reductions in food insecurity that range from 0.4 to 0.6 percentage points depending on the model; however, our preferred results that account for differential pre-trends produce a small decline of 0.6 percentage points (90% CIs: -1.6 to 0.4 p.p). Point estimates are similar when evaluating food insufficiency, albeit with greater precision: our preferred model suggests that a $1 MW increase leads to a decline in food insufficiency of 0.6 percentage points (90% CIs: -1.1 to -0.1 p.p; 12 percent decline relative to the pre-treatment mean).

Among likely-MW workers, a $1 MW increases reduces food insecurity by 1 to 2 percentage points depending on the model. Focusing on the first year after treatment and accounting for differential pre-trends, we find a treatment effect of -2 percentage points (90% CIs: -4.1 to 0.1 p.p.). With respect to food *insufficiency*: We find that a $1 MW increase leads to a 1.5 percentage point (90% CIs: -2.7 to -0.4 p.p.) decline for our likely-MW workers in the first full year after treatment.

Table 2: Estimated effect of minimum wage increases on food insecurity

	Average Effect from t=0 to t=2		**Effect at t=1**		**Effect at t=1 (with DLP)**	
Group	Food Insecurity	Food Insufficiency	Food Insecurity	Food Insufficiency	Food Insecurity	Food Insufficiency
All	-0.004	-0.003^{**}	-0.008^{**}	-0.004^{**}	-0.006	-0.006^{+}
	(0.003)	(0.001)	(0.004)	(0.002)	(0.006)	(0.003)
High Likelihood of MW Work	-0.010	-0.008^{**}	-0.017^{**}	-0.007	-0.020	-0.015^{**}
	(0.006)	(0.003)	(0.008)	(0.005)	(0.013)	(0.007)

Note: Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. "Food Insufficiency" occurs when eating patterns of one or more household members were disrupted and food intake reduced because the household lacked money and other resources for food. "Food Insecurity" occurs when households reduced the quality, variety, and desirability of their diets, but the quantity of food intake and normal eating patterns were not substantially disrupted.

Though income-based poverty and food hardship are distinct concepts, they are also highly-correlated and both act as indicators of financial well-being. Our findings thus far indicate that, among individuals likely to work in MW jobs, a $1 (or 13 percent, on average) increase in MW levels leads to a 1.5 percentage point decline in food insufficiency, as well as a 1.2 to 1.7 percentage point decline in SPM poverty, depending on whether we adjust for geographic price differences. Together, the findings suggest that MW increases have improved economic well-being among the subgroup that is most likely to directly benefit from such changes.

3.3 Potential Threats to Ability of Minimum Wage Increases to Reduce Poverty

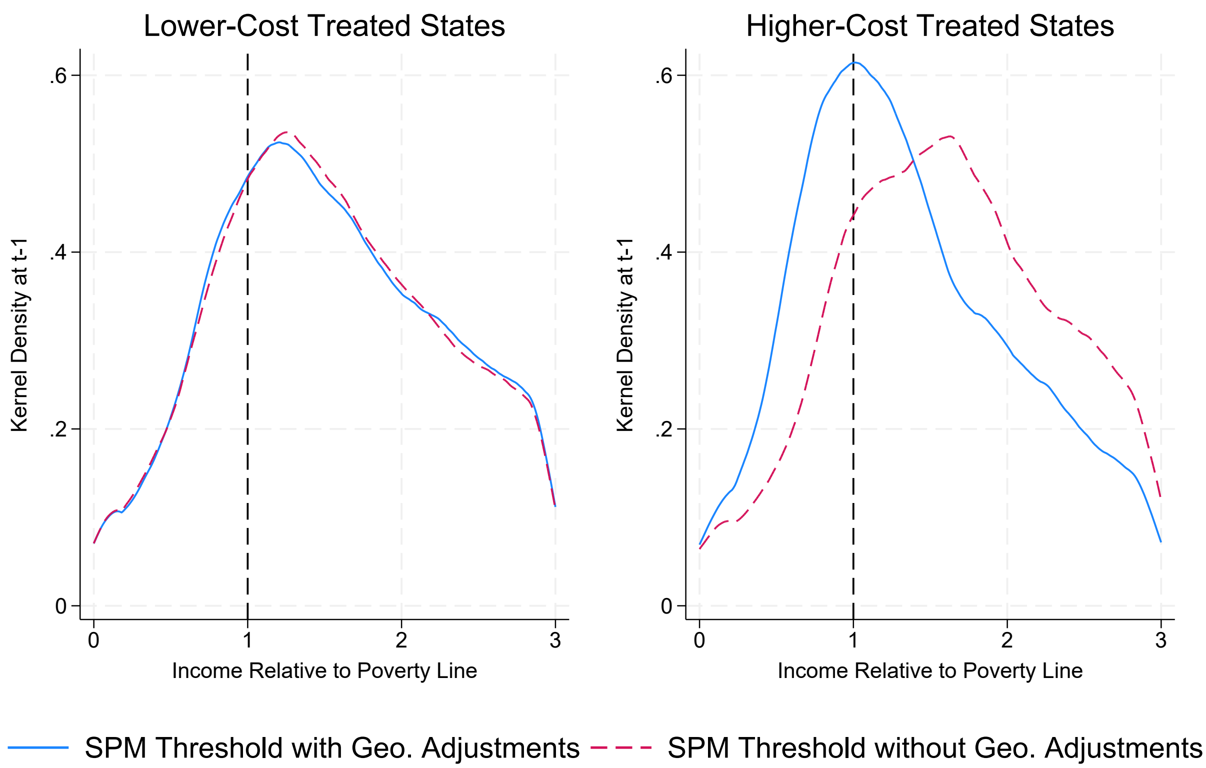
Our findings suggest that MW increases have modest reductions on poverty and food insecurity for the working-age population. We now add context to our findings that differences in geographic living costs offset some of the poverty-reduction effects of MW increases. Moreover, we examine several intermediate indicators to understand if the modest effects of MW increases on poverty are undercut by its consequences for employment or receipt of public income transfers.

3.3.1 Minimum Wage Increases Occur in High-Cost Places

We previewed earlier that MW increases primarily occur in higher-cost states, while the SPM poverty measure directly accounts for differences in housing costs in setting its poverty threshold (specifically, geographic adjustments to the SPM threshold are based on variation in 5-year averages of median gross rents for quality-adjusted two-bedroom households; see Appendix Table A1 for more detail and a comparison to the official poverty measure). Our empirical findings further supported that the effects of MW increases on poverty are weaker when we adjust for local housing costs relative to when we apply a SPM measure that does not make such adjustments. Whether focusing on all working-age adults, our likely-MW worker sample, or the demographically-defined groups presented in Table B1, we find that the geographic cost adjustments generally offset 30 percent or more of the poverty-reduction effect of MW increases in our preferred analyses.

Appendix Figure A2 clarifies the association between SPM thresholds and MW levels. Higher-cost states, such as California and New York, are more likely to implement higher MWs. Among all states in 2019, the correlation of the SPM geographic adjustment (based on local rental prices) and the MW level was r=0.67. Regarding changes: from 1975 to 2019, the correlation between states' changes in their SPM geographic adjustment with changes in their MW levels is r=0.32. Thus, in 2019, the four states with MW levels of at least $12 per hour had an average annual SPM poverty threshold of $36,500, compared to an average of $27,741 in the 22 states with MW levels lower than $8 per hour. We make no suggestion that MW increases *lead to* higher living costs; while possible (see Allegretto and Reich (2018)), this is not our analytical focus, and we do not find independent evidence to support the claim. But even when MW increases do not occur in tandem with rising living costs, the presence of higher living costs alone can reduce the poverty-reduction potential of a MW increase, as $1 increase in hourly wages is a smaller gain relative to the average poverty line in higher-cost states.

Figure 6: Pre-treatment incomes relative to poverty lines in higher-cost versus lower-cost treated states



Note: The figure plots pre-treatment kernel densities of incomes relative to the SPM poverty threshold in lower-cost states (those with a geographic adjustment below 1.25) versus higher-cost states (geographic adjustments of 1.25 or higher). The sample is limited to our likely-MW workers. The "geographic adjustment" refers to the adjustment that the Census Bureau applies to poverty thresholds based on regional variation in typical rental costs for a quality-adjusted two-bedroom home (see Appendix A).

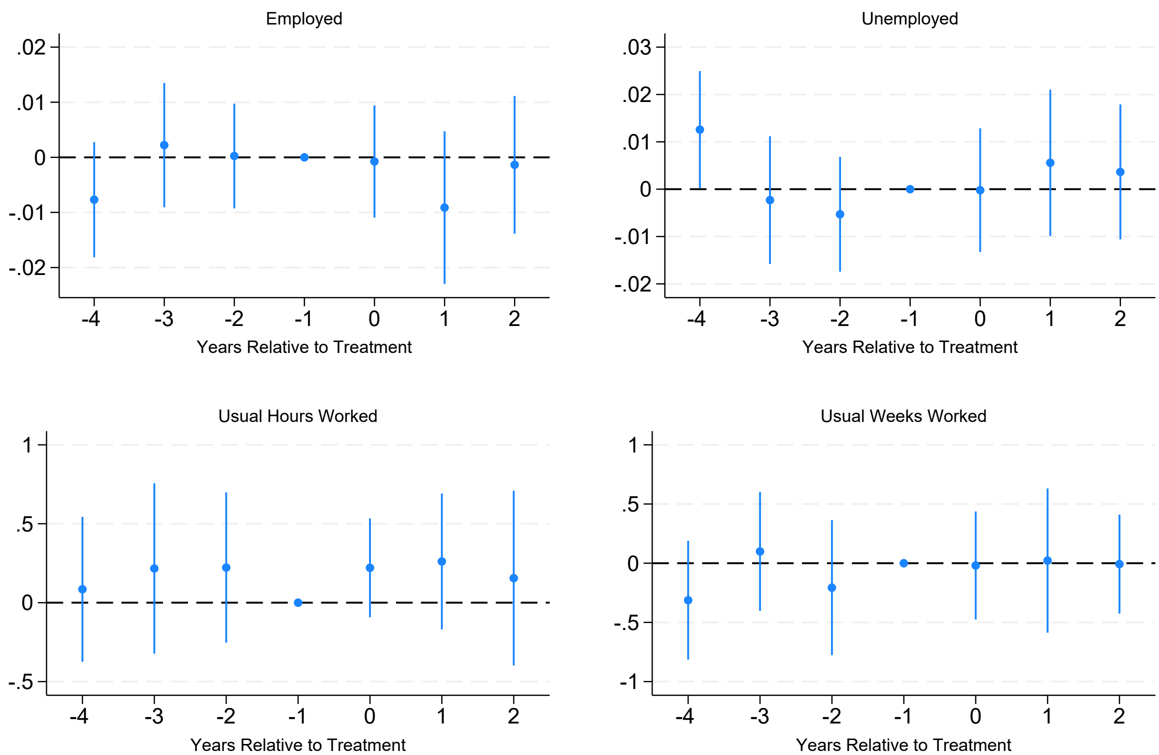
Figure 6 presents further context on how the geographic price adjustments factor into the poverty-reduction effect of MW increases. In lower-cost states that experience a MW increase (left panel), there is very little difference between the distribution of incomes relative to the poverty line when applying the adjusted vs. non-adjusted SPM thresholds. In higher-cost states that experience a MW increase (right panel), in contrast, the distribution of pre-treatment incomes relative to the poverty line differs substantially depending on whether the SPM is adjusted for local housing costs. When using the unadjusted SPM (red line), the mean income relative to the poverty line for likely-MW workers is 1.61. With the geographic adjustments (blue line), the poverty threshold is higher and the distribution of household incomes is shifted downward; the mean income relative to the poverty line for likely-MW workers is instead 1.37, lower than with the unadjusted threshold. Put differently, the cost-adjusted thresholds push likely-MW workers lower down the income distribution (relative to the poverty line) so that marginal gains from MW increases are less likely to lift such families out of poverty. These patterns help to explain why poverty-reductions appear stronger when not adjusting poverty thresholds for cost differences.

3.3.2 Minimum Wage Increases May Affect Employment

If MW increases were to reduce employment or work intensity, their favorable effects on poverty may be smaller than what might otherwise be observed (Burkhauser et al., 2025). We now employ our same estimation strategy, but with four employment outcomes: employment, unemployment (conditional on being active in the labor force), usual hours worked (conditional on being employed), and usual weeks worked per year (conditional on being employed).[13] We focus on our group of likely MW workers in all estimates.

[13] In all cases, our outcomes refer to the survey's reference year, and not the respondents' employment status at the time of survey. This ensures that our employment outcomes are in line with the timing of our treatment events.

Figure 7: Effects of $1 increase in minimum wage on employment outcomes



Note: The estimates are from a stacked event study centered on a state-year minimum wage increase and with comparison groups having near-identical minimum wage levels prior to treatment. "Unemployed" is conditional on being active in labor force. "Employed" is not conditional on being active in labor force. The sample is limited to our likely-MW worker sample, which consists of individuals between ages 16 to 64. Point estimates presented with 95% confidence intervals.

Our findings do not suggest that the MW increases in our sample have meaningful effects on employment, unemployment, or work intensity among our sample of likely-MW workers. That said, we cannot rule out that even small potential effects on employment may undercut the favorable effects of MW increases on poverty. Consider that the direction of the effects for employment status is in line with evidence of negative employment effects of MW increases, but the point estimates are small and imprecise (-0.8 p.p. decline; 90% CIs: -1.7 to 0.1 p.p.). The pre-treatment employment rate for members of our likely-MW sample was 44 percent. Thus, a 0.8 percentage point decline, if taken at face value, would represent a

2 percent decline in employment as a result of a \$1 increase in the MW for our likely-MW worker sample. Meanwhile, the pre-treatment poverty rate of employed members of this subsample was 20.5 percent, compared to 28.8 percent for non-employed individuals. If we assume, for simplicity, that the individuals who lose out on potential employment as a result of the MW increases were to have the same likelihood of poverty as the pre-treatment jobless, the \$1 MW increase may lead to post-treatment poverty rates being 0.17 percentage points higher (the product of an 8.3 percentage point higher poverty rate for jobless individuals multiplied by a 2 percent increase in likelihood of joblessness) for the treatment group than otherwise observed. This equates to around 15 percent of the average treatment effect of a \$1 MW increase on the SPM poverty rate, as displayed in Table 1. To emphasize, our primary estimates of how MW increases affect poverty already factor in any consequences of employment changes; the estimates here suggest that MW increases could reduce poverty at a slightly larger magnitude if there were to be no observable employment effects.

3.3.3 Minimum Wage Increases May Affect Receipt of Income Transfers

Beyond effects on employment, it is possible that higher incomes as a result of MW increases could reduce income transfers received from the state, partially offsetting their poverty-reduction potential. Income criteria for eligibility for programs such as SNAP and SSI are set federally and do not vary among the 48 contiguous United States; thus, when MW increases lead to higher wages, family units with low-pay workers may either lose eligibility for a given means-tested support, or receive a lower benefit level.

Table 3: Effect of minimum wage increases on benefit levels from tax and transfer programs among adults with high likelihood of working minimum wage jobs

	(1) SNAP Benefit Levels	(2) SNAP Participation	(3) SNAP Benefit Levels if Participating	(4) EITC Benefit Levels	(5) EITC Participation	(6) EITC Benefit Levels if Participating
Post * Treatment	−72.37	-0.02^{+}	217.37	51.58	0.00	48.21
	(63.89)	(0.01)	(311.43)	(45.93)	(0.01)	(74.55)

	(7) AFDC/TANF Benefit Levels	(8) AFDC/TANF Participation	(9) AFDC/TANF Benefit Levels if Participating	(10) SSI Benefit Levels	(11) SSI Participation	(12) SSI Benefit Levels if Participating
Post * Treatment	−42.01	−0.01	1425.88^{+}	−81.71	-0.01^{**}	1681.46
	(152.07)	(0.01)	(830.01)	(78.47)	(0.01)	(1515.31)

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Sample limited to high-probability group. We present estimates at t+1 that include our baseline controls and account for differential linear pre-trends. "AFDC/TANF" refers to Aid to Families with Dependent Children or Temporary Assistance for Needy Families. "SSI" refers to Supplemental Security Income.

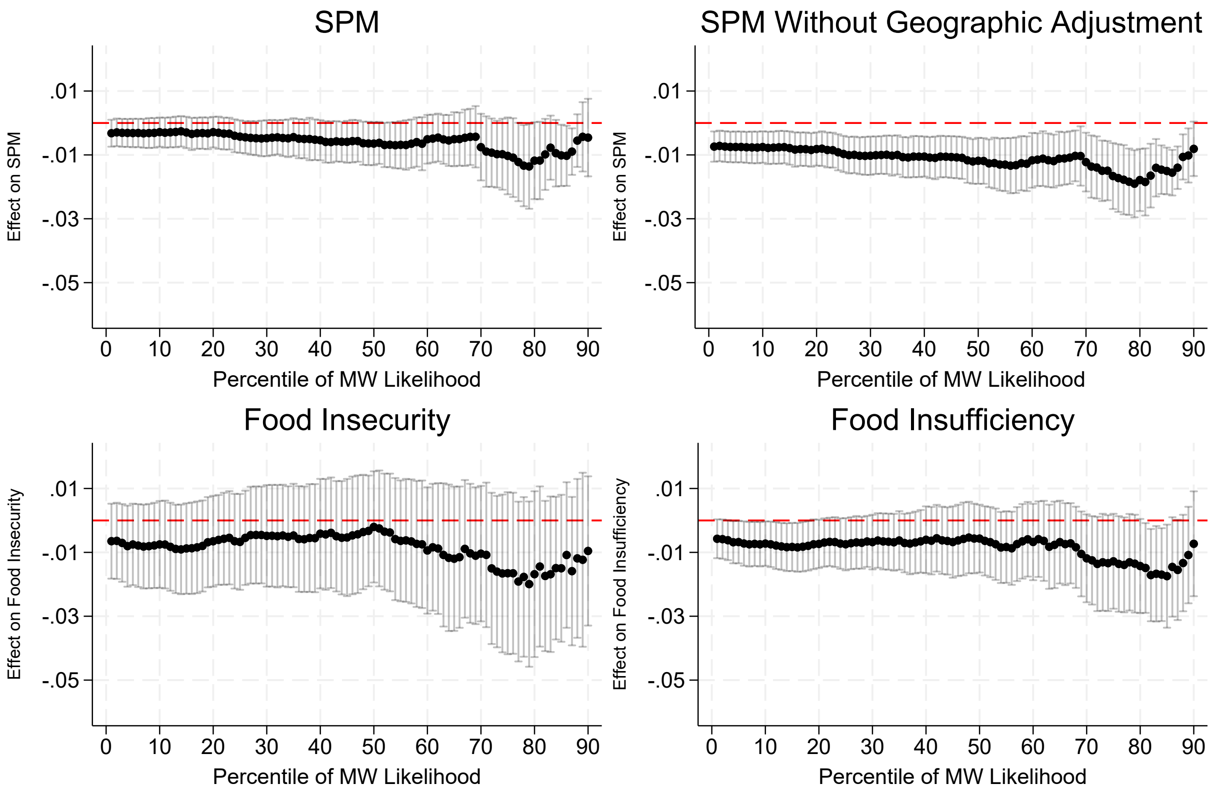
Table 3 presents intensive- and extensive-margin effects of MW increases for SNAP, EITC, AFDC/TANF, and SSI benefits among our likely-MW workers sample. The first three models, focused on SNAP, suggest that MW increases lead to a small ($72) and insignificant decline in annual SNAP benefits received, a result of a 2 percentage point decline in SNAP participation as a result of a $1 MW increase. We also find evidence of a 1 percentage point decline in SSI participation, which reduces total SSI benefits received by around $81 annually. We do not find meaningful changes in benefits received from the EITC or AFDC/TANF. In sum, these results suggest that MW increases do not meaningfully affect income transfers received among our sample of likely-MW workers; in turn, we cannot conclude that the interaction of higher MWs with the U.S. tax and transfer system meaningfully affects poverty rates for our focal group.

3.4 Additional Analyses and Sensitivity Tests

We assess the sensitivity of our core findings to many alternative model specifications, sample selection criteria, treatment group specifications, and more, in the following analyses.

Alternative Specifications of Likely Minimum Wage Workers: The focal group in our primary analyses is a set of individuals with high predicted probabilities of working at or near the MW, based on the methodology detailed previously. Our baseline results select the top 20 percent of the predicted probability distribution to classify as likely-MW workers. In Figure 8, we display our primary treatment effects across the full distribution of the predicted probability of working at or near the MW. Doing so serves two primary purposes. First, we can evaluate if the effects of MW increases became more favorable (in this case, more negative) with increases in the likelihood of working in MW jobs. For each of the four indicators in Figure 8, we find that this is the case: treatment effects become more negative at higher likelihood of working in MW jobs. Specifically, treatment effects appear to peak when focusing on the 75th through 90th percentiles of this distribution. Second, this figure demonstrates that our conclusions are not sensitive to our precise cutoff of 80th percentile for distinguishing likely-MW workers; setting the threshold at the 75th or 85th percentiles, for example, would have led to similar conclusions.

Figure 8: Effects of minimum wage increases on SPM poverty and food hardship by definition of high probability of minimum wage work



Note: Each point estimate refers to the estimated effect of a $1 MW increase on the outcome with 95% confidence intervals. In each model, we include all respondents with a predicted likelihood of working in MW jobs at and above the specified percentile on the X-axis. We present coefficients from our preferred estimates that account for differential linear pre-trends and interpret the effects of MW increases in the first full year after treatment.

Alternative Tolerance Thresholds and Levels of MW Increases: In our primary analyses, we evaluate minimum wage increases of at least $0.10. Within each event stack, we restrict control states to those whose pre-treatment minimum wage level is within $0.10 of the treated state's pre-treatment MW level ("tolerance levels"). In Appendix Figure B4, we present results across 36 combinations of MW increase and tolerance levels, displaying results for our two main poverty outcomes (with and without the geographic adjustments) among our likely-MW worker sample for levels of MW increases that vary from all increases to only \$0.50+ increases, and with tolerance levels for the control groups that range from exact matches of pre-treatment MW levels (our most conservative estimates) to all untreated states regardless of pre-treatment MW level (our least conservative estimates). The top panel presents results for our SPM poverty outcome. Point estimates do not vary meaningfully across most model specifications. For example, none of the differences between our preferred estimate—a \$0.10+ increase with \$0.10 tolerance levels—and the alternative estimate is statistically significant at conventional levels. Point estimates do become more negative (but also less precise) for larger MW treatment events (\$0.40 or greater) when tolerance thresholds are relaxed to at least \$0.20. With respect to SPM poverty rates without geographic cost adjustments, we continue to find negative and statistically-significant effects across nearly all models, including all tolerance thresholds for MW increases of at least \$0.10.

Placebo Tests: In Appendix Figures B5 and B6, we conduct two time-based placebo exercises by constructing "fake" treatment events using past or future MW increases. First, for each treated unit we assign placebo treatment at $t-3$, three years prior to the actual policy change. We then match these placebo-treated states to controls with similar MW levels in the year prior to the fake treatment, following the same baseline matching procedure as in our main analyses. Because no genuine policy change occurs at the placebo date, any estimated effects would indicate that treated state-years were already on differential trajectories prior to the true treatment. In practice, we find no such effects. Second, we assign placebo treatment at $t+3$, three years after the actual policy change. This test asks whether our design would mistakenly detect "effects" at a future fake event date, which could occur if overlapping MW increases biased our estimates. The absence of effects here indicates that future MW changes do not contaminate the comparison of treated and control units, further supporting the validity of our identification strategy.

Matching Prominent MW Events from Dube and Lindner (2024): As an additional sensitivity test, we re-estimate our primary results using the 60 prominent minimum wage events identified by Dube and Lindner (2024). Dube and Lindner define prominent events as those involving an increase of at least \$0.25 and 5 percent, occurring outside federal increase years, and separated by at least three years from other minimum wage hikes in the same state. In applying this framework, we retain our restricted set of comparison states but allow a tolerance of \$0.25 in the minimum wage level in the year prior to treatment. This ensures that comparison states remain similar in baseline wage policy while aligning with the minimum treatment threshold used by Dube and Lindner. Appendix Table B5 presents our findings, which are largely consistent with our main results. Accounting for differential linear pre-trends, we find statistically-insignificant reductions in SPM poverty of 0.4 percentage points for our full sample and for likely-MW workers, but statistically-significant declines of 0.8 percentage points (all individuals) and 2 percentage points (likely-MW workers) when evaluating the SPM without geographic adjustments. Recall that our primary results suggested 0.7 percentage point (all individuals) and 1.7 percentage point (likely-MW workers) declines for these same estimates, very similar to the results when limiting our sample to these 60 prominent treatment events.

Alternative Model Controls: Burkhauser et al. (2025) argues that key differences between the results in their study (finding no effects of MW increases on poverty) relative to that of Dube (2019) (which finds large negative effects on poverty) are the decisions of (1) whether to include macroeconomic controls such as the state unemployment rate and per-capita state GDP, which may capture mechanisms through which MW increases affect poverty; and (2) whether to include census division (hereafter "region") fixed effects, essentially limiting comparison states to those that are geographically proximate to treated states. Burkhauser et al. (2025, p. 11) argues against limiting comparison states to those that are geographically proximate, noting that "controls outside of a jurisdiction's census division often serve as more credible counterfactuals than controls inside the census division."

Moreover, their study does not use state unemployment rates of per-capita GDP as controls, but instead includes information on the state house price index and unemployment and mean wages among highly-educated individuals to account for state macro-economic conditions.

Our study's identification setting differs from both of these settings in that we match comparison groups based on pre-treatment MW levels, rather than geographic proximity (as achieved through preferred controls in Dube (2019)) and rather than comparing treated state-years to all untreated states. We have demonstrated that our approach is generally successful at naturally matching treated and control states on pre-treatment levels and trends of the MW (see Figure A4). In other words, we enable a closer comparison of treated and control states in our identification strategy rather than through additional model controls. Nonetheless, in Appendix Figure B1, we re-run our main analyses adopting three sets of model controls, each specified with and without region-by-year FE, to assess sensitivity to the preferred models in Dube (2019) versus Burkhauser et al. (2025). In "Control Set 1", we include demographic controls (sex, race/ethnicity, marital status, whether children are present, educational attainment, number of families in the household, and the cubic of age) plus preferred controls from Burkhauser et al. (2025), including the state house price index, unemployment rate of highly educated individuals, and mean wages among highly educated individuals. In "Control Set 2", we include demographic controls plus state per-capita GDP and the state unemployment rate. In "Control Set 3," we only include our demographic controls. For each of these three sets of controls, we also include results by whether we also include region-by-year FE or not. As anticipated, point estimates for our primary outcomes change only slightly across the six models, and we continue to find negative and statistically significant effects of MW increases on poverty and food insufficiency for the likely-MW worker sample across all models. In short, our findings are not sensitive to decisions on whether to prioritize the preferred controls from Dube (2019) or Burkhauser et al. (2025).

Pre and Post Great Recession: A separate difference between Dube (2019) and Burkhauser et al. (2025) is the span of treatment years examined. The former ends its analysis in 2013, while the latter extends results to 2019. Moreover, Burkhauser et al. (2025) notes that many large and frequent minimum wage increases occurred after the Great Recession, and finds that the poverty-reducing effects reported by Dube (2019) do not hold in this more recent period. In particular, they find no statistically significant or economically meaningful effects of post-Great Recession minimum wage increases on poverty, even when applying Dube's (2019) preferred specifications and controls. In Appendix Table B3, we split our sample into two based on the year in which the MW increase occurred: prior to 2010 or in/after 2010. In both time periods, we find small but negative effects of MW increases on SPM poverty among the full sample of working-age adults, though only with statistical significance in the post-Recession time period. Among our likely-MW worker sample, we find negative and significant short-run effects of MW increases on poverty without geographic price differences in both time periods. For treatments prior to 2010, we find evidence that a $1 MW increase leads to a decline in poverty of 1.1 percentage points. For treatments during or after 2010, we find stronger point estimates: 2 percentage point declines in poverty due to MW increases. We prefer to include all years, as in our primary results, but these findings demonstrate that our broader conclusions hold regardless of whether we examine pre- or post-Recession outcomes.

No MW Increase in Post Period for Control Group: In Appendix Table B4, we re-estimate our primary findings while restricting control groups to those that meet our original specifications *and*, in this case, also do not have their own MW increase of $0.10 or greater in the post-treatment window. This is closer to a 'clean controls' specification for our comparison group. We continue to find evidence of declining poverty as a result of MW increases; a $1 MW increase leads to a decline in poverty between 1.3 to 1.6 percentages points for the likely-MW sample, depending on whether we adjust for geographic price differences in the SPM threshold.

Leave-One-Out Estimates: In Appendix Figures B2 and B3, we perform leave-one-out estimates in which we re-estimate our primary models after removing a given treatment event and treatment year, respectively. This process helps to ensure that our aggregate treatment effects are not overly influenced by a single treatment event or treatment year. Point estimates do not meaningfully vary across the models.

Alternative Poverty Lines: In Appendix Table B2, we present results using alternative poverty lines, namely estimates of how MW increases reduce the likelihood that individuals are below 25 percent of the poverty line up to 200 percent of the poverty line at 25 percentage-point intervals. Focusing on the standard SPM poverty measure, we find negative effects that peak at 100 percent of the poverty line; estimates below that value are smaller and statistically insignificant, while estimates are small, insignificant, and positive above 150 percent of the poverty line. Without the geographic price differences, we find negative effects at 75 percent, 100 percent, and 125 percent of the poverty line, with a peak effect at 100 percent of the poverty line. Estimates below 75 percent and above 150 percent are small, negative, and statistically insignificant. These results are intuitive: workers who can benefit from MW increases are unlikely to be so poor as to have incomes below 50 percent of the poverty line, as households below that line are more likely to be composed of non-working individuals (or individuals working at very low work intensity).

Official Poverty Measure: In Appendix Table B6, we present results using the official poverty measure (OPM). The shortcomings of the OPM are well-documented (e.g. Fox et al. (2015) and Appendix Table A1); the measure is based on an out-dated poverty line and does not include the largest tax and transfer programs in the U.S. today. Nonetheless, many prior studies of MW effects on poverty focus on the OPM. For benchmarking purposes, we replicate our results with the OPM and find very small treatment effects of a $1 MW increase in all models. Our preferred estimates, which directly account for differential linear pre-trends, suggest an effect on the working-age population that falls between -0.3 to 0.3 p.p., and between -0.8 to 0.8 for the likely-MW worker sub-sample (ranges representing the 90% CIs). Put differently, a 10 percent increase in the MW leads to a change in OPM poverty for the working-age population between -0.2 and 0.2 p.p. This range is consistent with conclusions from Burkhauser et al. (2025), who apply a different methodological approach relative to ours. Given the conceptual superiority of the SPM relative to the OPM, however, our primary results still suggest that a $1 MW increase leads to a decline in poverty, particularly for likely-MW workers.

Elasticity Comparisons: To more directly facilitate comparison to other recent studies, we present elasticities of MW increases on percent changes in SPM poverty in Appendix Table B7. Our preferred estimates, which account for differential linear pre-trends and focus on effects one year after implementation, suggest that a 10 percent MW increase reduces SPM poverty rates by 1 percent for the working-age population, corresponding to an elasticity of –0.10. These effects are more pronounced among individuals with a high predicted likelihood of working in minimum wage jobs (elasticity of -0.20). These values are lower than those reported in Dube (2019), which reports long-run poverty elasticities between –0.22 and –0.46 for the non-elderly population. In contrast, Burkhauser et al. (2025) estimate that a 10 percent increase in the minimum wage is associated with a statistically insignificant 0.17 percent increase in the probability of poverty—an implied elasticity of +0.017, and conclude that MW increases are ineffective in reducing poverty. They further argue that poverty elasticities more negative than –0.13 can be ruled out at the 95 percent confidence level. Our results fall in between these two studies: we find consistent evidence that MW increases reduce SPM poverty, but generally at smaller magnitudes than those reported in Dube (2019). However, we do not find negative effects on OPM poverty, consistent with Burkhauser et al. (2025) (see Appendix Table B6). Our broader conclusions from studying MW effects on SPM poverty and food hardship, however, nonetheless align with the directionality of Dube (2019) in suggesting that MW increases can be effective at promoting economic well-being.

4 Conclusions

To what extent do MW increases affect poverty rates in the U.S.? Prior research has produced mixed findings on this question, with many studies finding small to null effects (Sabia, 2008; Sabia and Burkhauser, 2010; MaCurdy, 2015; Burkhauser et al., 2025; Neumark and Wascher, 2007), while a few find more favorable effects (Dube, 2019; Addison and Blackburn, 1999; DeFina, 2008). Nearly all of these studies, however, apply two-way fixed effects designs, use an outdated poverty measure, or face other shortcomings as documented previously. Our study adds to this line of work using a stacked difference-in-differences setting that matches treated state-years to comparison state-years with comparable pre-treatment MW levels (Chaisemartin and D'Haultfœuille, 2024). We offer the first study of how MW increases throughout the past four decades have affected the Supplemental Poverty Measure (SPM), and we also presents results for two measures of food hardship.

Among all working-age adults, we find that a \$1 MW increase leads to a modest reduction in SPM poverty (between 0.3 and 0.7 percentage points depending on whether we apply geographic adjustments to poverty thresholds). Our preferred estimate corresponds to an elasticity of –0.10, suggesting that a 10 percent increase in the minimum wage would reduce poverty by 1 percent relative to baseline levels. Our findings partially contrast with those of Burkhauser et al. (2025), who report statistically insignificant or slightly positive effects of MW increases on (OPM) poverty and rule out elasticities more negative than –0.13. In contrast, our estimates are smaller than the long-run elasticities reported by Dube (2019). Our evidence suggests that MW increases likely have a meaningful, if modest, role in reducing SPM poverty. We further contextualize this finding in several ways.

First, we clarify that the effects of MW increases are partially offset by the higher living costs in states that have implemented MW increases (given that the SPM thresholds adjust for local living costs). MW levels are very strongly, positively correlated with the local living cost adjustment applied to SPM thresholds (Appendix Figure A2); in turn, the effect of MW increases on poverty are reduced by around 30 percent or greater (depending on the specification) when accounting for variation in living costs.

Second, we produce results for a diverse subsample of workers who share a common likelihood of working in low-pay jobs. Estimating results among this subgroup follows Schanzenbach and Strain's (2024) claim that "theory and evidence [are] in the strongest alignment when the research design ... is focused on the demographic groups most likely to be affected by the expansion." We find that a $1 MW increase reduces poverty by 1.2 to 1.7 percentage points and food insufficiency by 1.5 percentage points among our likely-MW worker sample. Moreover, we do not find evidence to suggest that employment effects or interactions with tax and transfer programs meaningfully undercut the effect of MW increases on poverty. Even among our likely-MW worker sample, however, we continue to find that the higher living costs of MW-increasing states undercut the poverty-reduction effect of MW increases. Nonetheless, our core findings reveal strongly favorable effects of MW increases on this subgroup even when accounting for geographic price differences.

Our conclusions are consistent across many different empirical specifications, such as whether to include the preferred set of controls in Burkhauser et al. (2025) or in Dube (2019), whether focusing on pre- or post-Great Recession MW changes, whether restricting our treatment events to the "prominent" MW changes specified in Dube and Lindner (2024), and more. That our results are consistent when examining two measures of food hardships offers greater certainty that MW increases are, indeed, effective at reducing poverty and hardship for individuals most-likely to be affected by MW increases.

That said, our conclusions face several limitations and caveats. First, like nearly all studies using a nationally-representative sample from the CPS ASEC, our estimates of poverty are vulnerable to benefit underreporting of income-transfer programs, and our findings rest on a (plausible) assumption that benefit underreporting is distributed approximately evenly across treatment and control groups, and across treatment timing. Second, we acknowledge that there is no single test to determine whether one identification strategy is preferable to another; our approach to matching comparison state-years based on similarity to treated state-years' MW levels offers a balanced comparison of treatment and control units (see Appendix Table B4 and Figure A4). Specifically, we show that pre-treatment MW levels and trends are substantially more comparable in our preferred specification relative to, say, including all potential state-years without a MW increase in one's comparison group. We offer our identification strategy as a useful and valid alternative to stacked DiD settings that do not restrict comparison state-years based on pre-treatment MW levels, and an unambiguous improvement from the TWFE approaches that have otherwise long dominated the literature. On balance, our findings suggest that MW increases meaningfully reduce poverty and food hardship for the workers most directly affected and deliver modest improvements for the broader working-age population.

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Appendices

A Additional Details on Data and Sample

A.1 Comparison of the SPM and OPM

The Supplemental Poverty Measure is commonly used in US-focused poverty research. Unlike the US official poverty measure, the SPM includes all taxes and transfers, including benefits from refundable tax credits and food/nutrition assistance (such as benefits from the Supplemental Nutrition Assistance Program). The resource definition of the SPM also deducts expenses related to work, medical care, and child support. The SPM thresholds vary based on family size, local housing costs, and whether the resource unit is renting or owns its place of residence (and, among owners, whether the mortgage is being paid or is paid off).

SPM Geographic Cost Adjustment: The SPM's geographic cost adjustment reflects that the cost of living — especially housing — varies substantially across different parts of the United States. Rather than applying a single poverty threshold nationwide, the SPM adjusts thresholds to account for these geographic differences, ensuring that families in high-cost areas (like San Francisco or New York City) are not compared to the same needs standard as those in lower-cost regions (like rural Mississippi or the Midwest). This adjustment is based on differences in median gross rents for two-bedroom rental units as reported in the five-year American Community Survey (ACS). The Census Bureau creates an index that compares local housing costs to the national average and applies it to the base SPM threshold. While the base thresholds reflect expenditures on food, clothing, shelter, and utilities (FCSU), the geographic adjustment only modifies the shelter and utilities portion of the threshold, which accounts for roughly half of the overall FCSU threshold.

Formally, the SPM threshold for a given family type in a specific geographic area (p) is calculated as:

$$\text{SPM Threshold}_p = \text{Base Threshold} \times \left[\omega_H \left(\frac{\text{Local Median Rent}_p}{\text{National Median Rent}}\right) + \omega_{1-H}\right]$$

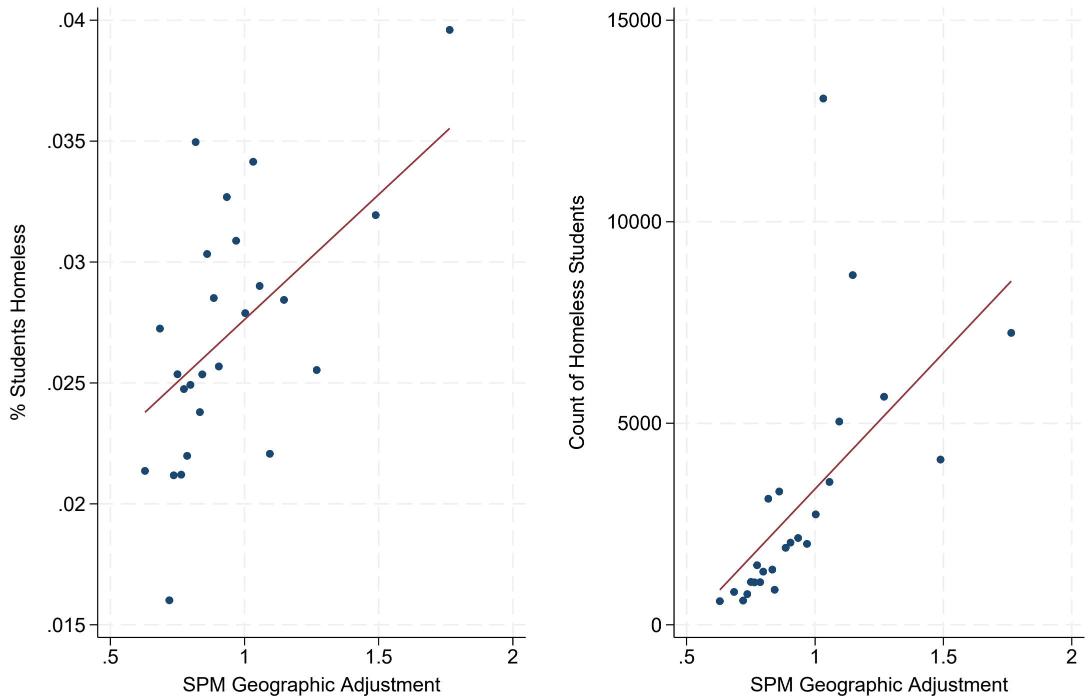
In this formula, "Base Threshold" is the national FCSU threshold for a family type with a given housing tenure, before geographic adjustment; ω_H is the housing portion of the SPM threshold, which is typically around 0.48; ω_{1-H} is the remaining share of the threshold attributed to food, clothing, and other expenses; and the two rent indicators reflect the median rent of a given place (p) relative to the national median. In Figure A1, we document that the SPM's geographic price adjustments are positively correlated with a separate measure of material hardship: levels and shares of student homelessness in public schools. The strong relationship between these indicators is one way of validating the SPM's inclusion of geographic price adjustments into its poverty threshold.

Table A1 summarizes the key differences between the SPM and OPM. Aside from differences in what counts as income (with the SPM being far more complete) and how to set poverty thresholds, the two measures also differ slightly (generally inconsequentially) in the definition of their resource-sharing unit.

Table A1: Summary of Differences Between SPM and OPM

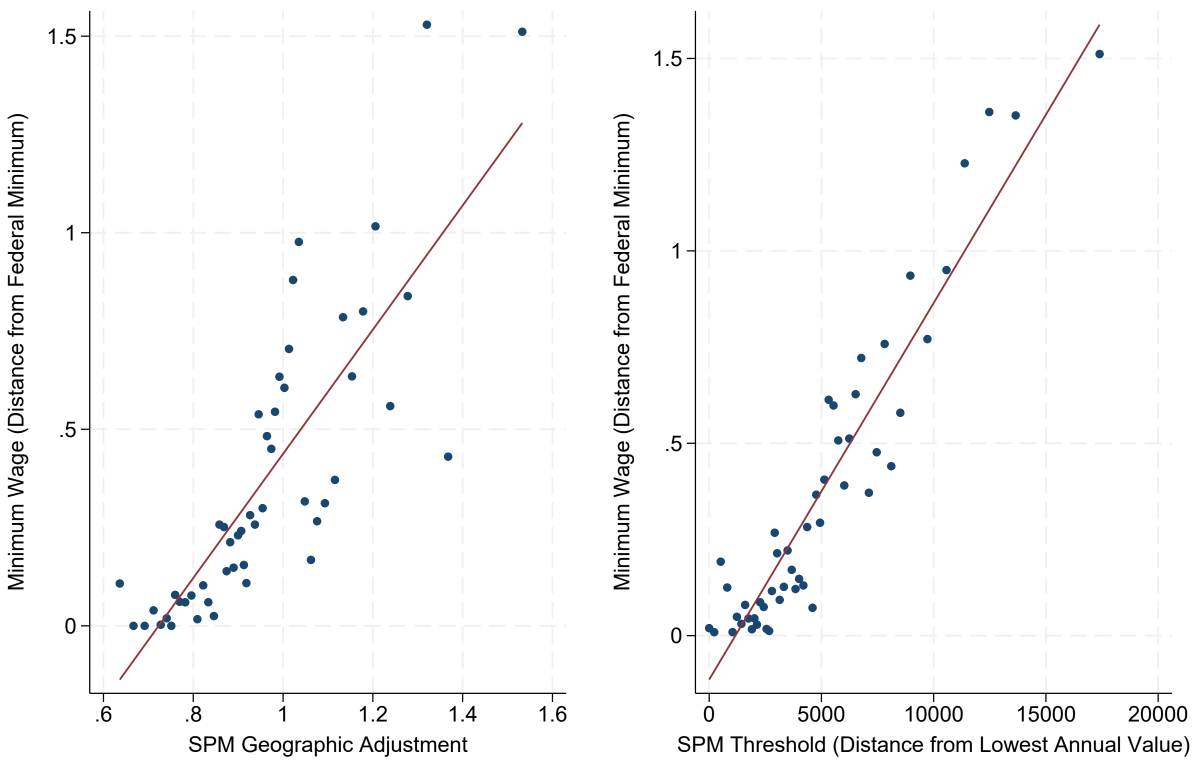
	Supplemental Poverty Measure (SPM)	**Official Poverty Measure (OPM)**
Measurement of Income	Includes all taxes and transfers (including near-cash transfers and refundable tax credits), minus out-of-pocket expenses related to work, medical care, and child support paid to other households	Pre-tax cash income only (excludes taxes, tax credits, and near-cash transfers)
Poverty Threshold	Set based on a five-year moving average of expenditures on food, clothing, shelter, and utilities; varies regionally based on local housing costs	Set in the early 1960s based on three times the cost of a minimum food diet; updated annually for inflation using the CPI; does not vary geographically among contiguous states
Unit of Analysis	Resource-sharing units (in 95%+ of cases, this is equivalent to the household, but some households have multiple units)	Family unit (individuals related by birth, marriage, or adoption and living together)
Equivalence Scale	Poverty thresholds vary by family size, so household incomes are not directly applied an equivalence scale	Poverty thresholds vary by family size, so household incomes are not directly applied an equivalence scale
Income Accounting Period	Annual income received during the calendar year	Annual income received during the calendar year

Figure A1: Binned Scatterplot: SPM Geographic Adjustments and Student Homelessness in Public Schools



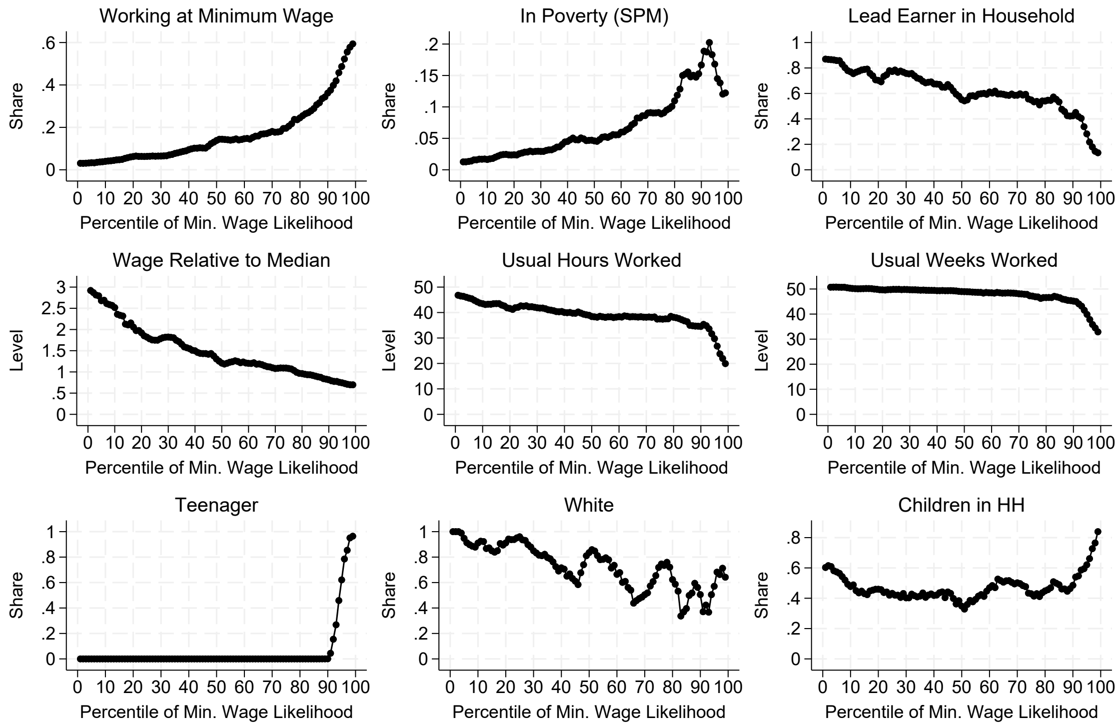
Note: This binned scatterplot includes data from 953 core-based statistical areas (CBSAs) comparing SPM geographic adjustments, derived from the CPS ASEC, to shares and counts of students experiencing homelessness from the National Center for Homelessness Education in the U.S. Department of Education. CBSA-level values are weighted averages of district-level homelessness data, with weights applied based on the number of students in a school. The figure shows that the SPM geographic adjustment is positively correlated with levels of homelessness among families with children.

Figure A2: Association of SPM threshold with minimum wage levels



Note: The figure presents binned scatterplots (that absorb year effects) that compare state-year MW levels (Y-axis) to the SPM geographic adjustment (left panel) and SPM thresholds for the average family unit (right panel). Minimum wage levels and SPM thresholds are in 2019 USD. "SPM Geographic Adjustment" refers to the extent to which SPM thresholds are adjusted to account for local living costs. The figure shows that MW levels are generally higher in higher-cost states.

Figure A3: Conditional on employment: Characteristics of employed adults across the distribution of predicted likelihood of being a minimum wage worker



Note: The likelihood of being an adult who would work in a MW-level job is defined as the predicted probability of earning an hourly wage below 125% of the statutory minimum wage based on age, race/ethnicity, sex, education, and their interactions (see Data section). Note that in our primary sample, we do not condition our likely-MW worker on current employment status.

Table A2: Description of Variables

Type	Variable	Definition	Source
Primary Outcomes	Poverty	Continuous variable for SPM [0.5;2.2], geo-adjusted[1]	ASEC
	Poverty (binary)	Binary variable for SPM non-geo-adjusted[1]	ASEC
	Food insecurity	Binary variable for low or very low food security (FSSTATUS)	CPS FSSUPINT[2]
	Food insufficency	Binary variable for very low food security (FSSTATUS)	CPS FSSUPINT[2]
Secondary Outcomes	Hourly wages	Continuous variable on level of USD received	MORG
	Labor force status	Binary variable on labor force status	ASEC
	Employment status	Binary variable on employment status	ASEC
	Hours worked	Continuous variable on hours worked per week	ASEC
	Weeks worked	Continuous variable on weeks worked per year	ASEC
	SNAP level	Continuous variable on level of USD received	ASEC
	SNAP participation	Binary variable on benefit receipt	ASEC
	SSI level	Continuous variable on level of USD received	ASEC
	SSI participation	Binary variable on benefit receipt	ASEC
	ACDF/TANF level	Continuous variable on level of USD received	ASEC
	ACDF/TANF participation	Binary variable on benefit receipt	ASEC
	EITC level	Continuous variable on level of USD received	ASEC
	EITC participation	Binary variable on benefit receipt	ASEC
Baseline Covariates	Sex	Binary variable for woman/man	ASEC
	Race	Categorical variable for white/black/other	ASEC
	Hispanic	Binary variable for Hispanic/non-Hispanic	ASEC
	Age	Continuous variable for the cube root of the average number of years	ASEC
	Married Household	Continuous variable for the share of workers in married households	ASEC
	Children	Continuous variable for the average number of children	ASEC
	Education (high)	Continuous variable for the share of workers with a 3-year College degree or higher	ASEC
	Education (low)	Continuous variable for the share of workers with a high school diploma or lower	ASEC
	Household size	Continuous variable for the number of persons in the household	ASEC
Additional Covariates	House prices	Continuous variable for the state-level house price index	OECD
	High-skilled prime-age unemployment	Continuous variable for unemployed workers aged 25-64 with a 3-year College degree or higher as a share of the total population aged 25-64	ASEC
	High-skilled prime-age earnings	Continuous variable for the average annual real pre-tax wage and salary income of individuals aged 25-64 with a 3-year College degree or higher	ASEC
	EITC rate	Continuous variable for the state EITC rate	UKCPR
	SNAP rate	Continuous variable for the maximum allotment food stamp (SNAP) benefit for a 4-person family	UKCPR
	TANF rate	Continuous variable for the AFDC/TANF maximum monthly benefit for a 4-person family	UKCPR

[1] Geo-adjusted refers to an adjustment for geographical differences in prices.
[2] FSSUPINT refers to the Food Security Supplement administered by the CPS annually in December.

Table A3: Comparison of characteristics by wage relative to minimum wage

Variable	**Workers Near Minimum Wage**	**Workers Above Minimum Wage**
Wage Relative to Median Wage	37.1%	168.0%
Lead Earner in Household	36.0%	64.0%
Poverty (SPM)	23.9%	5.3%
Usual Hours Worked Per Week	37.3	39.4
Usual Weeks Worked Per Year	43.7	47.3
White (non-Hispanic)	64.7%	74.3%
Black (non-Hispanic)	13.4%	10.4%
Hispanic	17.4%	10.4%
Teenager	15.3%	3.0%
Children (under age 18) in HH	50.4%	46.8%
Children (under age 6) in HH	19.1%	19.9%
Share of All Workers	13.8%	86.2%

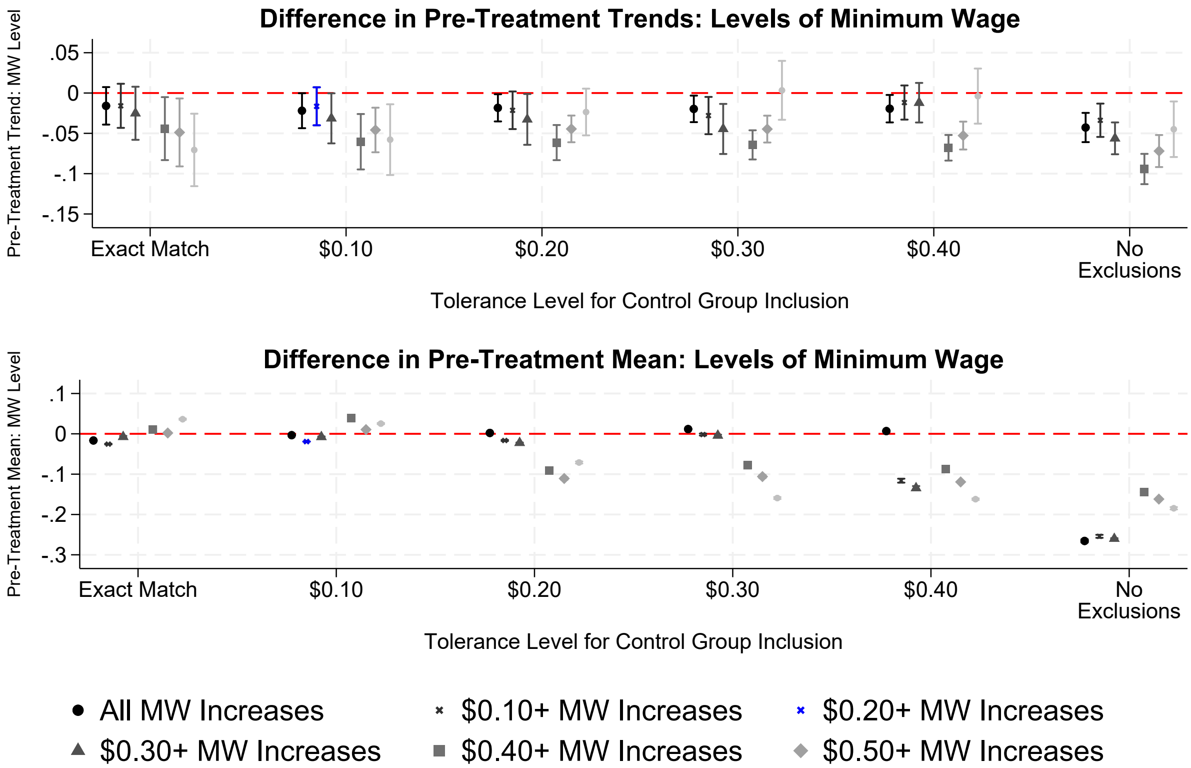
Note: The data compares workers near the minimum wage to those above it across various variables such as hours worked, racial demographics, and household characteristics. refers to workers with hourly wages that are at, below, or within 20 percent of the statutory minimum wage that applies in the given state and year. Sample size = 3,330,474 employed individuals between 1976 to 2019.

Table A4: Pre-treatment outcomes for treated and untreated state-years

	All		Likely MW Workers	
	Treated	Untreated	Treated	Untreated
Food Insecurity	0.136	0.143	0.215	0.219
Food Insufficiency	0.049	0.049	0.075	0.070
SPM Poverty	0.123	0.123	0.215	0.222
SPM Poverty (no geo adjust)	0.120	0.161	0.205	0.276

Note: "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "SPM" refers to Supplemental Poverty Measure.

Figure A4: Pre-treatment levels and trends of MW for treated versus comparison states



Note: Coefficients (with 95% confidence intervals) represent the differential pre-treatment trend or level for treated units compared to control units. Pre-treatment trends are estimated by interacting a linear time trend with the treatment indicator (top panel). Pre-treatment means are computed by averaging minimum wage levels from $t = -4$ to $t = -1$ (bottom panel). "Tolerance Level" refers to the maximum difference of pre-treatment MW levels relative to the treated state-year to be included as a comparison unit. The point estimates vary based on the necessary increase in the MW to qualify as an event (see figure legend). Our preferred specification, in blue, is a $0.10 or greater MW increase to qualify as an event, with a $0.10 tolerance threshold.

Table A5: Treatment states, comparison states, and treatment details for events in our primary specification

Year	Treated	Comparison	Base MW	Change in MW
1990	AK	HI	3.85	0.45
1991	AK	CA, CT, WA	4.30	0.45
1996	AK	OR	4.75	0.50
1997	AK	HI	5.25	0.40
2010	AK	AL, AR, AZ, DE, FL, GA, HI, IA, ID, IN, KS, KY, LA, MD, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NY, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	7.25	0.50
2012	AZ	MI, RI	7.35	0.30
2013	AZ	AK	7.65	0.15
2017	AZ	FL, MT	8.05	1.95
2006	AR	AL, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MO, MS, MT, NC, ND, NE, NH, NM, OH, OK, PA, SC, SD, TN, TX, UT, VA, WY	5.15	1.10
2015	AR	AL, GA, IA, ID, IN, KS, KY, LA, MS, NC, ND, NH, OK, PA, SC, TN, TX, UT, VA, WI, WY	7.25	0.25
2016	AR	ME, NM	7.50	0.50
2017	AR	FL, MT	8.00	0.50
1988	CA	AL, AR, AZ, CO, DE, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	3.35	0.90
1998	CA	AL, AR, AZ, CO, DE, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, PA, RI, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	5.15	0.60
2001	CA	AK	5.75	0.50
2002	CA	VT	6.25	0.50
2008	CA	RI	7.50	0.50
2014	CA	MA	8.00	1.00
2011	CO	AL, AR, AZ, DE, FL, GA, HI, IA, ID, IN, KS, KY, LA, MD, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NY, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	7.25	0.11
2012	CO	MI, RI	7.36	0.28
2014	CO	AZ, MT	7.78	0.22
2017	CO	DE, IL, NV	8.31	0.99

Year	Treated	Comparison	Base MW	Change in MW
1987	CT	AL, AR, AZ, CA, CO, DE, FL, GA, HI, IA, ID, IL, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	3.37	0.38
1988	CT	AK, MA, ME	3.75	0.50
1999	CT	AL, AR, AZ, CO, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	5.18	0.47
2000	CT	AK	5.65	0.50
2001	CT	DC, DE, RI	6.15	0.25
2002	CT	OR	6.40	0.30
2003	CT	CA, MA	6.70	0.20
2006	CT	AK	7.10	0.30
2010	CT	CA, MA	8.00	0.25
2014	CT	IL, NV	8.25	0.45
2017	CT	RI	9.60	0.50
1999	DE	AL, AR, AZ, CO, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	5.15	0.50
2000	DE	AK	5.65	0.50
2007	DE	MN	6.15	0.50
2014	DE	AL, AR, GA, HI, IA, ID, IN, KS, KY, LA, MD, MS, NC, ND, NE, NH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	7.25	0.50
1987	DC	AK	3.90	0.95
1993	DC	AK, OR	4.85	0.40
1996	DC	HI	5.25	0.50
2005	DC	DE	6.15	0.45
2006	DC	IL	6.60	0.40
2014	DC	IL, NV	8.25	1.25
2005	FL	AL, AR, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NH, NM, NV, OH, OK, PA, SC, SD, TN, TX, UT, VA, WV, WY	5.15	1.00
2006	FL	DE, MN	6.15	0.25
2007	FL	WI	6.40	0.27
2012	FL	AL, AR, DE, GA, HI, IA, ID, IN, KS, KY, LA, MD, MN, MO, MS, NC, ND, NE, NH, NJ, NY, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	7.31	0.36
2013	FL	AK	7.67	0.12
2014	FL	AZ, MT	7.79	0.14

Year	Treated	Comparison	Base MW	Change in MW
1988	HI	AL, AR, AZ, CO, DE, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	3.35	0.50
1992	HI	AL, AR, AZ, CA, CO, DE, FL, GA, ID, IL, IN, KS, KY, LA, MA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, VT, WA, WI, WV, WY	4.25	0.50
1993	HI	AK, OR	4.75	0.50
2002	HI	AL, AR, AZ, CO, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	5.25	0.50
2006	HI	DE, MN	6.25	0.50
2015	HI	AL, GA, IA, ID, IN, KS, KY, LA, MS, NC, ND, NH, OK, PA, SC, TN, TX, UT, VA, WI, WY	7.25	0.50
2016	HI	MO	7.75	0.75
2017	HI	SD	8.50	0.75
2004	IL	AL, AR, AZ, CO, FL, GA, IA, ID, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	5.15	0.35
2007	IL	WI	6.50	1.00
2008	IL	RI	7.50	0.25
2010	IL	CA, MA	8.00	0.25
1992	IA	AL, AR, AZ, CA, CO, DE, FL, GA, ID, IL, IN, KS, KY, LA, MA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, VT, WA, WI, WV, WY	4.25	0.40
1985	ME	AL, AR, AZ, CA, CO, DE, FL, GA, HI, IA, ID, IL, IN, KS, KY, LA, MA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, OR, PA, RI, SC, SD, TN, TX, UT, VA, VT, WA, WI, WV, WY	3.35	0.10
1987	ME	VT	3.55	0.10
2002	ME	AL, AR, AZ, CO, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	5.15	0.60
2005	ME	HI	6.35	0.15

Year	Treated	Comparison	Base MW	Change in MW
2006	ME	IL	6.50	0.25
2009	ME	HI, IA, NH, WV	7.25	0.25
2017	ME	NM	7.50	1.50
2015	MD	AL, GA, IA, ID, IN, KS, KY, LA, MS, NC, ND, NH, OK, PA, SC, TN, TX, UT, VA, WI, WY	7.25	1.00
2016	MD	DE, IL, NV	8.25	0.50
2017	MD	WV	8.75	0.50
1986	MA	AL, AR, AZ, CA, CO, DE, FL, GA, HI, IA, ID, IL, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	3.35	0.20
1987	MA	VT	3.55	0.10
2000	MA	HI	5.25	0.75
2008	MA	RI	7.50	0.50
2006	MI	AL, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MO, MS, MT, NC, ND, NE, NH, NM, OH, OK, PA, SC, SD, TN, TX, UT, VA, WY	5.15	1.80
2007	MI	DC	6.95	0.20
2008	MI	AK, NJ, NY, PA	7.15	0.25
2014	MI	ME, NM	7.40	0.75
2016	MI	OH	8.15	0.35
2017	MI	SD	8.50	0.40
1988	MN	AL, AR, AZ, CO, DE, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	3.35	0.20
1990	MN	HI	3.85	0.10
2005	MN	AL, AR, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NH, NM, NV, OH, OK, PA, SC, SD, TN, TX, UT, VA, WV, WY	5.15	1.00
2014	MN	AL, AR, GA, HI, IA, ID, IN, KS, KY, LA, MD, MS, NC, ND, NE, NH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	7.25	0.75
2008	MO	WI	6.50	0.15
2014	MO	AL, AR, GA, HI, IA, ID, IN, KS, KY, LA, MD, MS, NC, ND, NE, NH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	7.35	0.15
2015	MO	ME, NM	7.50	0.15
2012	MT	MI, RI	7.35	0.30
2013	MT	AK	7.65	0.15

Year	Treated	Comparison	Base MW	Change in MW
2015	NE	AL, GA, IA, ID, IN, KS, KY, LA, MS, NC, ND, NH, OK, PA, SC, TN, TX, UT, VA, WI, WY	7.25	0.75
2016	NE	AZ, FL, MT	8.00	1.00
2006	NV	AL, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MO, MS, MT, NC, ND, NE, NH, NM, OH, OK, PA, SC, SD, TN, TX, UT, VA, WY	5.15	1.00
2007	NV	MN	6.15	0.18
2010	NV	ME, NM	7.55	0.70
1987	NH	AL, AR, AZ, CA, CO, DE, FL, GA, HI, IA, ID, IL, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	3.35	0.10
1990	NH	PA	3.65	0.15
2008	NH	WI	6.50	0.75
1992	NJ	AL, AR, AZ, CA, CO, DE, FL, GA, ID, IL, IN, KS, KY, LA, MA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, VT, WA, WI, WV, WY	4.25	0.80
2005	NJ	AL, AR, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NH, NM, NV, OH, OK, PA, SC, SD, TN, TX, UT, VA, WV, WY	5.15	1.00
2006	NJ	DE, MN	6.15	1.00
2014	NJ	AL, AR, GA, HI, IA, ID, IN, KS, KY, LA, MD, MS, NC, ND, NE, NH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	7.25	1.00
2015	NJ	IL, NV	8.25	0.13
2005	NY	AL, AR, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NH, NM, NV, OH, OK, PA, SC, SD, TN, TX, UT, VA, WV, WY	5.15	0.85
2013	NY	AL, AR, DE, GA, HI, IA, ID, IN, KS, KY, LA, MD, MN, MO, MS, NC, ND, NE, NH, NJ, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	7.25	0.75
2014	NY	MA	8.00	0.75
2017	NY	AK	9.70	0.70
2012	OH	MI, RI	7.40	0.30
2013	OH	AK	7.70	0.15

Year	Treated	Comparison	Base MW	Change in MW
1989	OR	AL, AR, AZ, CO, DE, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, SC, SD, TN, TX, UT, VA, WV, WY	3.35	0.50
1990	OR	HI	3.85	0.40
1991	OR	CA, CT, WA	4.25	0.50
2005	OR	CT	7.05	0.20
2006	OR	AK	7.25	0.25
2009	OR	CA, MA	7.95	0.45
2017	OR	AK	9.75	0.50
1989	PA	AL, AR, AZ, CO, DE, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, SC, SD, TN, TX, UT, VA, WV, WY	3.35	0.35
1986	RI	AL, AR, AZ, CA, CO, DE, FL, GA, HI, IA, ID, IL, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	3.35	0.20
1987	RI	VT	3.55	0.10
1988	RI	MA, ME	3.65	0.35
1991	RI	CA, CT, WA	4.25	0.20
1999	RI	AL, AR, AZ, CO, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	5.15	0.50
2000	RI	AK	5.65	0.50
2004	RI	DC, DE	6.15	0.60
2006	RI	CA, MA	6.75	0.35
2007	RI	AK, NJ	7.10	0.30
2013	RI	MI	7.40	0.35
2014	RI	AK	7.75	0.25
2015	SD	AL, GA, IA, ID, IN, KS, KY, LA, MS, NC, ND, NH, OK, PA, SC, TN, TX, UT, VA, WI, WY	7.25	1.25
1986	VT	AL, AR, AZ, CA, CO, DE, FL, GA, HI, IA, ID, IL, IN, KS, KY, LA, MD, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, OR, PA, SC, SD, TN, TX, UT, VA, WA, WI, WV, WY	3.35	0.10
1988	VT	NH	3.55	0.10

Year	Treated	Comparison	Base MW	Change in MW
1995	VT	AL, AR, AZ, CA, CO, DE, FL, GA, ID, IL, IN, KS, KY, LA, MA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	4.25	0.25
1999	VT	HI, MA	5.25	0.50
2001	VT	AK	5.75	0.50
2004	VT	HI, ME	6.25	0.50
2005	VT	CA, MA, RI	6.75	0.25
2007	VT	AK, NJ	7.25	0.28
2017	VT	RI	9.60	0.40
1989	WA	AL, AR, AZ, CO, DE, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, SC, SD, TN, TX, UT, VA, WV, WY	3.35	0.50
1990	WA	HI	3.85	0.40
1994	WA	AL, AR, AZ, CA, CO, DE, FL, GA, ID, IL, IN, KS, KY, LA, MA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, VT, WI, WV, WY	4.25	0.65
1999	WA	AL, AR, AZ, CO, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NJ, NM, NV, NY, OH, OK, PA, SC, SD, TN, TX, UT, VA, WI, WV, WY	5.15	0.55
2000	WA	AK	5.70	0.80
2001	WA	OR	6.50	0.22
2002	WA	MA	6.72	0.18
2005	WA	AK	7.16	0.19
2009	WA	CA, MA	8.07	0.48
2017	WA	MN	9.47	1.53
2006	WV	AL, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MO, MS, MT, NC, ND, NE, NH, NM, OH, OK, PA, SC, SD, TN, TX, UT, VA, WY	5.15	0.70
2008	WV	WI	6.55	0.70
2015	WV	AL, GA, IA, ID, IN, KS, KY, LA, MS, NC, ND, NH, OK, PA, SC, TN, TX, UT, VA, WI, WY	7.25	0.75
2016	WV	AZ, FL, MT	8.00	0.75
1989	WI	AL, AR, AZ, CO, DE, FL, GA, IA, ID, IL, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NJ, NM, NV, NY, OH, OK, SC, SD, TN, TX, UT, VA, WV, WY	3.35	0.30
1990	WI	PA	3.65	0.15

Year	Treated	Comparison	Base MW	Change in MW
2005	WI	AL, AR, AZ, CO, GA, IA, ID, IN, KS, KY, LA, MD, MI, MO, MS, MT, NC, ND, NE, NH, NM, NV, OH, OK, PA, SC, SD, TN, TX, UT, VA, WV, WY	5.15	0.55

B Additional Results

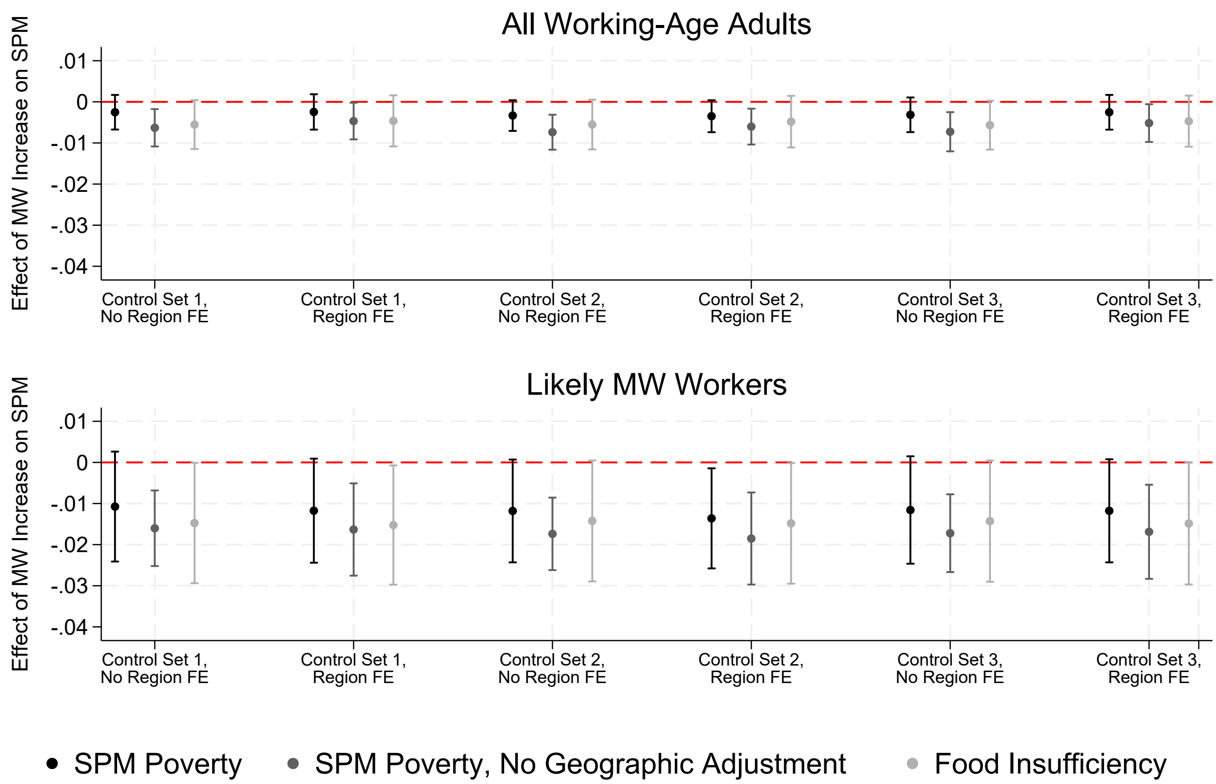
Table B1: Heterogenous effects: Estimated effect of minimum wage increases on poverty and food hardship

Group	**Average Effect from t=0 to t=2**		**Effect at t=1**		**Effect at t=1 (with DLP)**	
	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)
Single Parent	-0.010^{**}	-0.013^{+}	-0.015^{**}	-0.018^{**}	-0.017	-0.028^{**}
	(0.005)	(0.007)	(0.007)	(0.009)	(0.011)	(0.013)
Black	-0.011	-0.011	-0.035^{***}	-0.036^{***}	-0.045^{**}	-0.053^{***}
	(0.008)	(0.009)	(0.010)	(0.012)	(0.017)	(0.017)
Hispanic	0.003	0.003	0.002	-0.000	-0.004	-0.016^{+}
	(0.006)	(0.007)	(0.010)	(0.008)	(0.011)	(0.009)
Teenager	-0.006	-0.007^{**}	-0.011^{**}	-0.013^{***}	-0.012^{+}	-0.017^{***}
	(0.004)	(0.003)	(0.005)	(0.004)	(0.006)	(0.005)
No More Than H.S. Degree	-0.004^{+}	-0.006^{***}	-0.006^{**}	-0.008^{***}	-0.003	-0.008^{**}
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)

Group	**Average Effect from t=0 to t=2**		**Effect at t=1**		**Effect at t=1 (with DLP)**	
	Food Insecurity	Food Insufficiency	Food Insecurity	Food Insufficiency	Food Insecurity	Food Insufficiency
Single Parent	0.000	0.005	-0.006	0.003	0.005	0.002
	(0.007)	(0.005)	(0.012)	(0.007)	(0.016)	(0.009)
Black	-0.024^{**}	-0.006	-0.025^{**}	-0.001	-0.045^{+}	-0.008
	(0.012)	(0.010)	(0.012)	(0.012)	(0.024)	(0.014)
Hispanic	-0.010	-0.001	-0.011	-0.004	0.013	-0.000
	(0.014)	(0.007)	(0.013)	(0.008)	(0.021)	(0.011)
Teenager	-0.005	-0.002	-0.006	0.002	0.001	-0.006
	(0.006)	(0.003)	(0.008)	(0.006)	(0.010)	(0.008)
No More Than H.S. Degree	-0.020^{**}	-0.009^{**}	-0.029^{***}	-0.005	-0.025	-0.008
	(0.009)	(0.004)	(0.010)	(0.006)	(0.017)	(0.008)

Note: Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "SPM" refers to the Supplemental Poverty Measure. "Food Insufficiency" occurs when eating patterns of one or more household members were disrupted and food intake reduced because the household lacked money and other resources for food. "Food Insecurity" occurs when households reduced the quality, variety, and desirability of their diets, but the quantity of food intake and normal eating patterns were not substantially disrupted.

Figure B1: Effects of minimum wage increases on SPM poverty by set of controls and region fixed effects



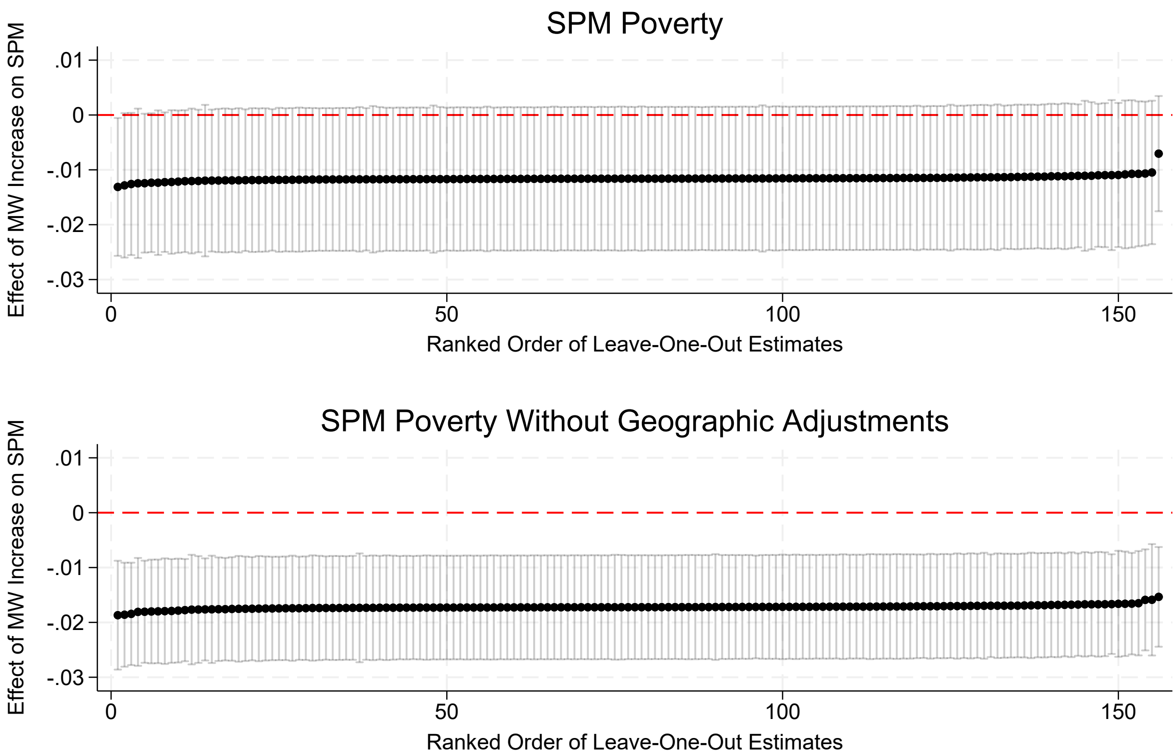
Note: In "Control Set 1", we include demographic controls (sex, race/ethnicity, marital status, whether children are present, educational attainment, number of families in the household, and the cubic of age) plus preferred controls from Burkhauser et al. (2025), including the state house price index, unemployment rate of high-educated individuals, and mean wages among highly-educated individuals. In "Control Set 2", we include demographic controls plus state per-capita GDP and the state unemployment rate. In "Control Set 3," we only include our demographic controls. For each of these three sets of controls, we also include results by whether we also include region-by-year FE or not. Point estimates presented with 95% confidence intervals.

Table B2: Alternative Poverty Lines: Estimated effect of $1 MW increase on SPM poverty

	(1) 25% of SPM Poverty Line	(2) 50% of SPM Poverty Line	(3) 75% of SPM Poverty Line	(4) 100% of SPM Poverty Line	(5) 125% of SPM Poverty Line	(6) 150% of SPM Poverty Line	(7) 175% of SPM Poverty Line	(8) 200% of SPM Poverty Line
Post * Treatment	−0.001	−0.002	−0.004	-0.012^{+}	−0.005	0.002	0.006	0.001
	(0.003)	(0.003)	(0.005)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
	(1) 25% of SPM Poverty Line (without Geo. Adj.)	(2) 50% of SPM Poverty Line (without Geo. Adj.)	(3) 75% of SPM Poverty Line (without Geo. Adj.)	(4) 100% of SPM Poverty Line (without Geo. Adj.)	(5) 125% of SPM Poverty Line (without Geo. Adj.)	(6) 150% of SPM Poverty Line (without Geo. Adj.)	(7) 175% of SPM Poverty Line (without Geo. Adj.)	(8) 200% of SPM Poverty Line (without Geo. Adj.)
Post * Treatment	−0.002	−0.002	-0.009^{+}	-0.017^{***}	-0.011^{+}	−0.005	−0.005	−0.008
	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)	(0.005)

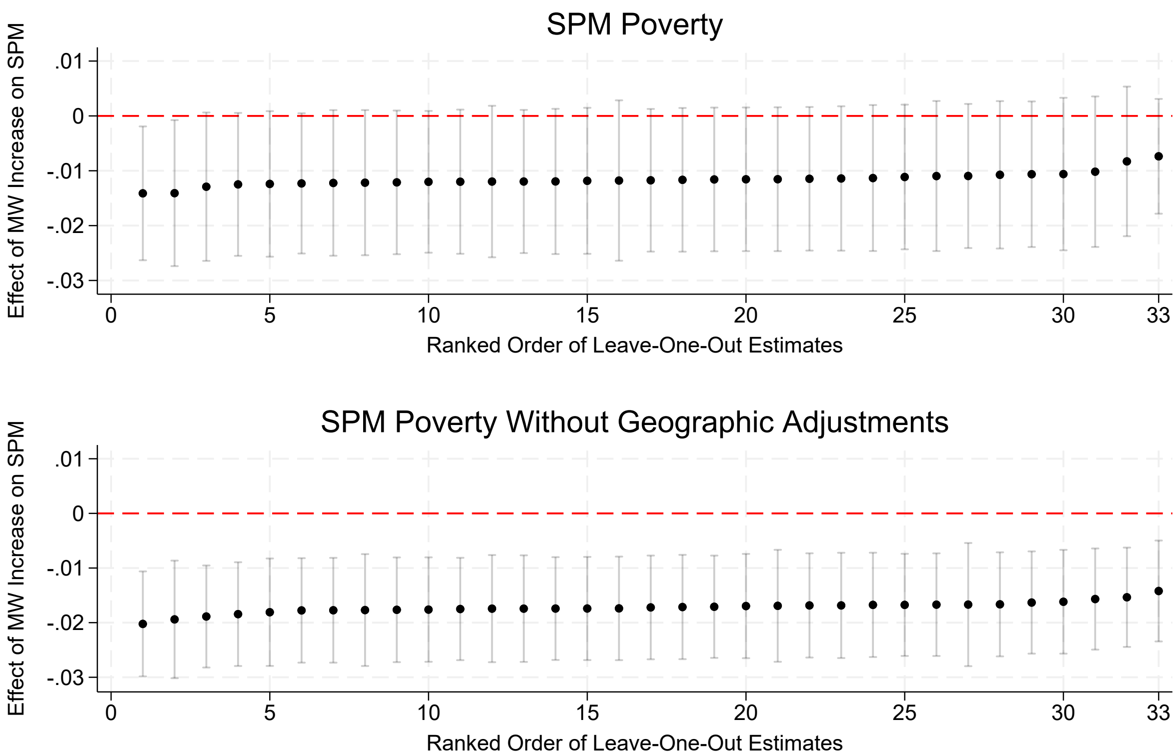
Note: Results are estimated on our subsample of likely-MW workers in the first full year after treatment while adjusting for differential linear pre-trends. "SPM" refers to Supplemental Poverty Measure. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure B2: Leave-one-out estimates by MW increase event



Note: Each coefficient represents the estimated effect of a $1 MW increase on the labeled outcome while excluding one of the 'events' or 'stacks.' Coefficients are ordered from lowest to highest. Point estimates presented with 95% confidence intervals.

Figure B3: Leave-one-out estimates: by year of treatment



Note: Each coefficient represents the estimated effect of a $1 MW increase on the labeled outcome while excluding one treatment year (including all events with the specific treatment year). Coefficients are ordered from lowest to highest. Point estimates presented with 95% confidence intervals.

Table B3: Pre- and Post-Great Recession: Estimated effect of minimum wage increases on poverty

	Average Effect from t=0 to t=2		**Effect at t=1**		**Effect at t=1 (with DLP)**	
Group	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)
Pre-Great Recession (Treatment Prior to 2010)						
All	−0.000	−0.002	−0.001	−0.003	0.002	−0.003^{+}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
High Likelihood of MW Work	−0.004	−0.007^{+}	−0.007**	−0.013**	−0.000	−0.011**
	(0.003)	(0.004)	(0.003)	(0.005)	(0.006)	(0.006)
	Average Effect from t=0 to t=2		**Effect at t=1**		**Effect at t=1 (with DLP)**	
Group	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)
Post-Great Recession (Treatment In or After 2010)						
All	−0.002	−0.004	−0.004	−0.007**	−0.006^{+}	−0.012***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
High Likelihood of MW Work	−0.011	−0.009	−0.014	−0.015	−0.020^{+}	−0.020^{+}
	(0.008)	(0.006)	(0.011)	(0.009)	(0.011)	(0.011)

Note: Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "DLP" refers to differential linear pre-trends, which are accounted for in Columns 5 and 6. "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "SPM" refers to Supplemental Poverty Measure.

Table B4: No treatment in post period for comparison group: Estimated effect of minimum wage increases on poverty

	Average Effect from t=0 to t=2		Effect at t=1		Effect at t=1 (with DLP)	
Group	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)
All	-0.004^{+}	-0.002	-0.006^{**}	-0.003	-0.004	-0.007^{**}
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
High Likelihood of MW Work	-0.014^{***}	-0.010^{**}	-0.020^{***}	-0.014^{**}	-0.013	-0.016^{***}
	(0.005)	(0.004)	(0.007)	(0.006)	(0.008)	(0.006)

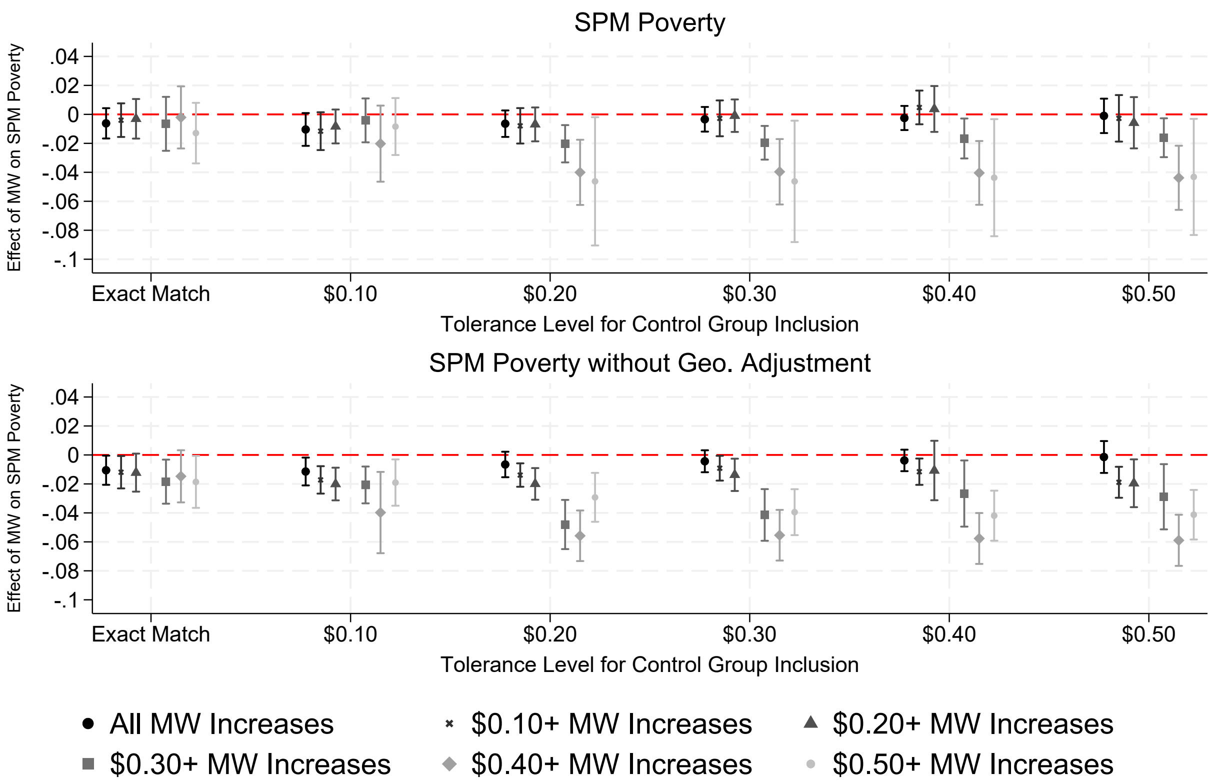
Note: In this specification, a comparison unit is excluded if they have any MW increase in the post-treatment window. Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "DLP" refers to differential linear pre-trends, which are accounted for in Columns 5 and 6. "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "SPM" refers to Supplemental Poverty Measure.

Table B5: Matching Dube and Linder (2024) treatment events: Estimated effect of minimum wage increases on poverty

	Average Effect from t=0 to t=2		Effect at t=1		Effect at t=1 (with DLP)	
Group	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)	SPM Poverty	SPM Poverty (No Geo. Adj.)
All	0.001	-0.001	-0.001	-0.003	-0.004	-0.008^{+}
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)
High Likelihood of MW Work	0.003	-0.003	-0.002	-0.011^{+}	-0.004	-0.020^{**}
	(0.005)	(0.005)	(0.006)	(0.006)	(0.010)	(0.008)

Note: In this specification, treatment events are limited to the prominent events specified in Dube and Linder (2024). Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "DLP" refers to differential linear pre-trends, which are accounted for in Columns 5 and 6. "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "SPM" refers to Supplemental Poverty Measure.

Figure B4: Effects of minimum wage increases on SPM poverty by level of minimum wage increase and tolerance for matching control groups



Note: "Tolerance Level" refers to the maximum difference of pre-treatment MW levels relative to the treated state-year to be included as a comparison unit. The point estimates vary based on the necessary increase in the MW to qualify as an event (see figure legend). Our preferred specification, in blue, is a $0.10 or greater MW increase to quality as an event, with a $0.10 tolerance threshold. Each coefficient represents treatment effects of a $1 MW increase for our likely-MW worker sample at t+1 while accounting for differential linear pre-trends. We present differences in pre-trends and pre-treatment levels for each of these specifications in Appendix Figure A4. Point estimates presented with 95% confidence intervals.

Table B6: Official Poverty Measure: Estimated effect of minimum wage increases on poverty

	Average Effect from t=0 to t=2	**Effect at t=1**	**Effect at t=1 (DLP)**
Group	OPM Poverty	OPM Poverty	OPM Poverty
All	−0.0011	−0.0018	−0.0004
	(0.0015)	(0.0018)	(0.0018)
High Likelihood of MW Work	−0.0045	−0.0047	0.0001
	(0.0037)	(0.0047)	(0.0052)

Note: Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "DLP" refers to differential linear pre-trends, which are accounted for in Columns 5 and 6. "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "OPM" refers to Official Poverty Measure.

Table B7: Elasticities: Estimated effect of 10 percent MW increase on percent change poverty

Group	Average Effect from t=0 to t=2: SPM Poverty	Average Effect from t=0 to t=2: SPM Poverty (No Geo. Adj.)	Effect at t=1: SPM Poverty	Effect at t=1: SPM Poverty (No Geo. Adj.)	Effect at t=1 (with DLP): SPM Poverty	Effect at t=1 (with DLP): SPM Poverty (No Geo. Adj.)
All	-0.008	-0.008^{**}	-0.012^{**}	-0.012^{**}	-0.010	-0.017^{***}
	(0.005)	(0.003)	(0.006)	(0.005)	(0.007)	(0.006)
High Likelihood of MW Work	-0.014^{**}	-0.013^{***}	-0.022^{**}	-0.021^{***}	-0.020^{+}	-0.024^{***}
	(0.006)	(0.004)	(0.008)	(0.006)	(0.011)	(0.007)

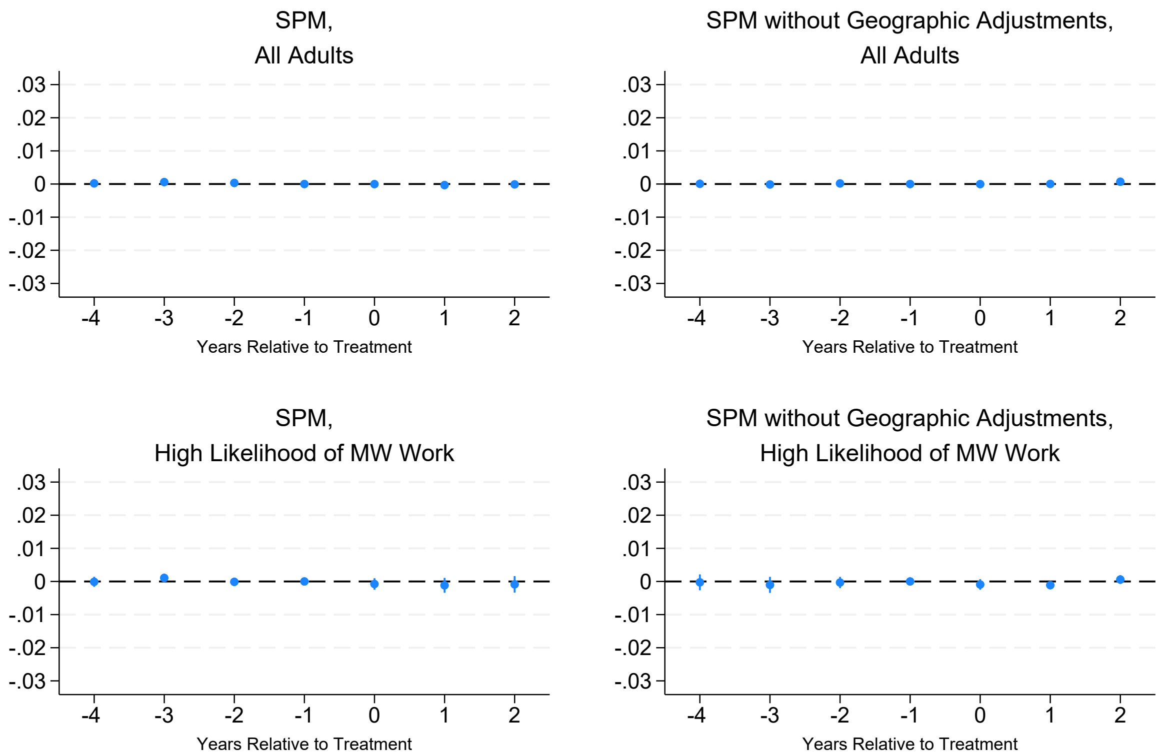
Note: Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. refers to differential linear pre-trends, which are accounted for in Columns 5 and 6. refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. refers to Supplemental Poverty Measure.

Table B8: Estimates without rescaling for treatment size: Estimated effect of minimum wage increases on poverty

Group	Average Effect from t=0 to t=2: SPM Poverty	Average Effect from t=0 to t=2: SPM Poverty (No Geo. Adj.)	Effect at t=1: SPM Poverty	Effect at t=1: SPM Poverty (No Geo. Adj.)	Effect at t=1 (with DLP): SPM Poverty	Effect at t=1 (with DLP): SPM Poverty (No Geo. Adj.)
All	-0.003^{+}	-0.004^{***}	-0.004^{**}	-0.007^{***}	-0.005^{**}	-0.010^{***}
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
High Likelihood of MW Work	-0.007^{**}	-0.010^{***}	-0.011^{**}	-0.015^{***}	-0.009	-0.020^{***}
	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.005)

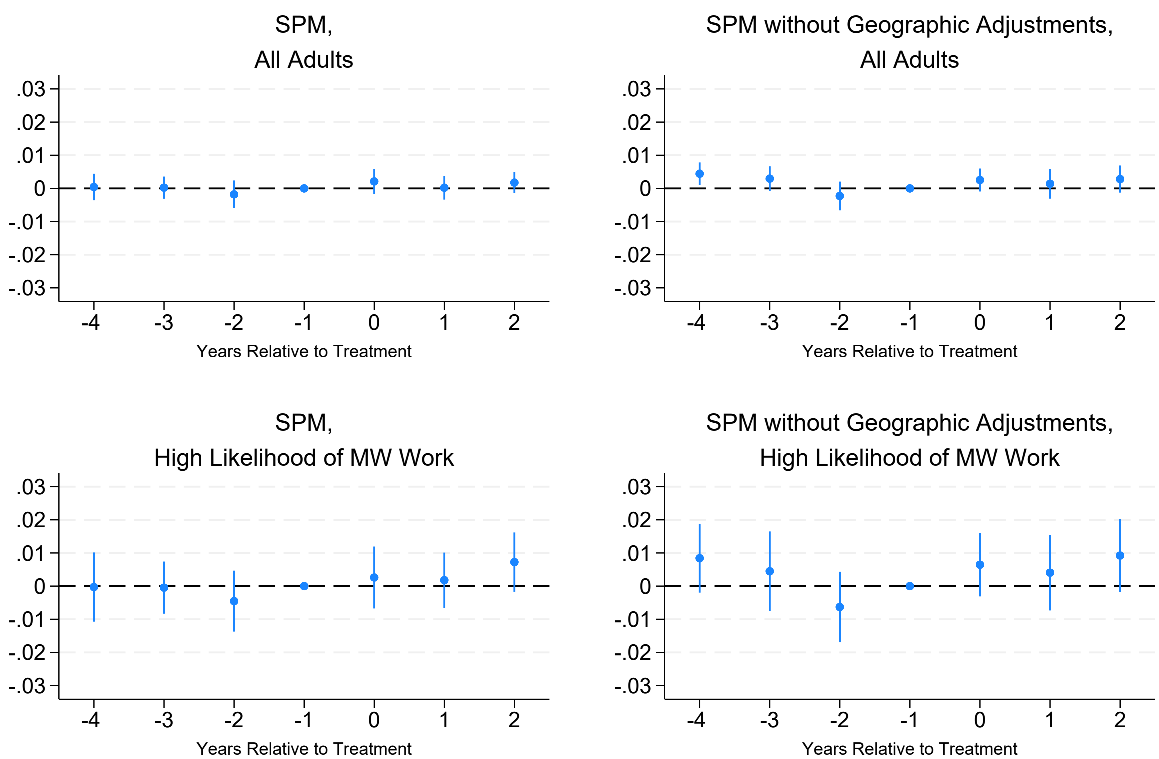
Note: Each row represents a separate model estimated on the labeled subgroup. Standard errors in parentheses. + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. The pre-treatment mean SPM poverty rates for the demographic groups are 11.5% (All) and 20.7% (High Likelihood of MW Work). refers to differential linear pre-trends, which are accounted for in Columns 5 and 6. refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. SPM refers to Supplemental Poverty Measure.

Figure B5: Placebo Test: Fake MW treatments defined at three years after real treatment



Note: The estimates are from a stacked event study centered on a fake state-year minimum wage increase set at three years after a real treatment and with comparison groups having near-identical minimum wage levels in the year prior to the fake treatment. "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "SPM" refers to Supplemental Poverty Measure. Sample limited to individuals between ages 16 to 64. Point estimates presented with 95% confidence intervals.

Figure B6: Placebo Test: Fake MW treatments defined at three years prior to real treatment



Note: The estimates are from a stacked event study centered on a fake state-year minimum wage increase set at three years prior to a real treatment and with comparison groups having near-identical minimum wage levels in the year prior to the fake treatment. "High Likelihood of MW Work" refers to our subsample of workers who are in the top quintile of our predicted likelihood of working in MW jobs, regardless of current employment status. "SPM" refers to Supplemental Poverty Measure. Sample limited to individuals between ages 16 to 64. Point estimates presented with 95% confidence intervals.