IRLE
Institute for Research on Labor and Employment

IRLE WORKING PAPER
#103-19
April 2019
Updated October 2021

Parental Labor Supply: Evidence from Minimum Wage Changes

Anna Godøy, Michael Reich, Jesse Wursten and Sylvia A. Allegretto

Cite as: Anna Godøy, Michael Reich, Jesse Wursten and Sylvia A. Allegretto (2021). "Parental Labor Supply: Evidence from Minimum Wage Changes". IRLE Working Paper No. 103-19.
http://irle.berkeley.edu/files/2021/11/Parental-Labor-Supply-Evidence-from-Minimum-Wage-Changes.pdf

http://irle.berkeley.edu/working-papers

Berkeley
UNIVERSITY OF CALIFORNIA

Forthcoming in *Journal of Human Resources*

Parental Labor Supply: Evidence from Minimum Wage Changes*

Anna Godøy† Michael Reich‡ Jesse Wursten§ Sylvia Allegretto¶

October 18, 2021

Abstract

We analyze effects of the minimum wage on the labor supply of parents of young children. Distributional difference-in-differences and event study models document a sharp rise in employment rates of single mothers with children ages 0 to 5 following minimum wage increases. Effects are concentrated among jobs paying close to the minimum wage. We find corresponding drops in the probability of staying out of the labor force to care for family members. Results are consistent with simple labor supply models in which childcare costs create barriers to employment. Minimum wage increases then enable greater labor force participation and reduce child poverty.

Keywords: Labor supply, minimum wage, parents, child poverty
JEL Classification Numbers: J22, J38

*We are grateful to the Robert Wood Johnson Foundation for research support and to David Card, Arindrajit Dube, Hilary Hoynes, Patrick Kline, Edward Leamer, Carl Nadler, Jesse Rothstein, Bryan Stuart, and participants at the UC Berkeley labor lunch, the 2019 Society of Labor Economists meetings, the 2019 Labor and Employment Relations Association annual meeting and the 2019 Western Economic Association International Annual Conference for valuable comments.

†IRLE, University of California, Berkeley, the Norwegian Institute of Public Health, Oslo, Norway, and the Department of Health Management and Health Economics, University of Oslo, Norway.aago@niph.no

‡IRLE, University of California, Berkeley. mreich@econ.berkeley.edu

§KU Leuven, Leuven, Belgium. jesse.wursten@kuleuven.be. The author gratefully acknowledges financial support of the Research Foundation Flanders (FWO) under grant number 1125818N.

¶IRLE, University of California, Berkeley. allegretto@berkeley.edu

‖The authors do not have any conflicts of interest to disclose. Data and programs for replication will be available through the Harvard Dataverse at https://dataverse.harvard.edu/dataverse/pls-replication-package.

1 Introduction

We provide causal evidence on the effects of higher minimum wages on the employment rates of parents with young children. Low-income adults with minor children differ in two important ways from low-income adults without children: They may receive means-tested transfers and they potentially face fixed costs of employment in the form of childcare. Moreover, childcare costs tend to be much higher for children ages 0 to 5 (Cogan 1981, Eissa et al. 2008). As a result, the budget constraints of parents may contain kinks and non-convexities. Small increases in wages can then induce significant labor supply effects, where affected workers go from non-participation to supplying a large number of working hours. Indeed, empirical research on the earned income tax credit (EITC) has found that the labor supply of parents increases significantly in response to higher take-home wages, especially for single mothers of children five and under (Michelmore & Pilkauskas 2021). Much like the EITC, higher minimum wages raise the return to market work for workers at the bottom of the wage distribution. In this paper then, we examine whether higher minimum wages could have similar positive impacts on parental employment rates.

Our empirical analysis follows the *distributional difference-in-differences* approach to identification (Cengiz et al. 2019), exploiting variation in minimum wage policies (152 events) and publicly available data from the Current Population Survey (CPS).[1] We show that minimum wages have no significant effect on overall low wage employment, consistent with recent literature. However, this pattern changes when we distinguish effects by the presence of children. Specifically, we find significant and substantial positive employment effects for single mothers with children ages 0 to 5. For this group, we estimate a positive own-wage employment elasticity of 1.1 (s.e. 0.4), considerably higher than in the minimum wage literature, but in the same range as studies of other groups who face high fixed employment costs. The employment effect occurs in jobs paying at or just above the new minimum wage - higher wage employment is not affected. Moreover, our event study models find no increase in employment rates in the years prior to minimum wage increases, lending support to the validity of our identification strategy.

Our finding that employment effects are concentrated among single mothers with children 0-5 years old suggests the importance of child care cost constraints for this group. This fixed cost constraint is supported by our analyses of the CPS-ASEC data, which find

[1] The distributional difference-in-differences estimator is also known as the bunching or bin-by-bin estimator.

that minimum wages reduce the share of single mothers who report not being in the labor force because of family obligations. Moreover, our analysis of childcare data from the Survey of Income and Program Participation indicates that higher minimum wages increase single mothers' use of formal childcare arrangements.

Besides shedding light on the impact of minimum wages on parents, we make two additional contributions. First, our results illustrate how employment responds to higher wages. By studying minimum wage events that raise real wages for low-wage workers, we provide a window into the determinants of the decision to supply labor.

Second, our findings suggest the potential of minimum wages as a policy instrument for reducing child poverty. A recent consensus report from the National Academy of Sciences (2019) included raising the minimum wage as a part of a proposed policy package to cut child poverty by 50 percent. Our findings support this recommendation and show that even though minimum wage policy is not targeted at families with young children, it could still be an effective instrument against child poverty.

Our paper contributes to four literatures: family labor supply models, the effects of childcare on labor supply, the labor supply effects of the Earned Income Tax Credit (EITC), and the effects of minimum wages on employment. Ashenfelter et al. (1974) and Blundell et al. (2016) review the literature on family labor supply models. Angrist & Evans (1998) extend these models to examine the interactions between the number of children and labor supply, but do not examine wage elasticities. Cogan (1981) finds that reducing fixed costs, such as from commuting to work, has substantial positive effects on labor supply. Our paper builds especially on the fixed costs model.

The literature on the effects of child care costs on labor supply, reviewed by Havnes & Mogstad (2011), finds ambiguous effects of the price of childcare on female labor supply (Blau & Currie 2006). Quasi-experimental evidence on effects of expanding access to preschool also finds mixed results. Fitzpatrick (2010) finds no effects of free pre-K on the labor supply of single or married mothers. On the other hand, several papers (Gelbach 2002, Fitzpatrick 2012, Cascio 2009) find that access to publicly provided preschool significantly increases employment of single mothers with pre-school age children. Moreover, the effects appear to vary with marital status: both Fitzpatrick (2012) and Cascio (2009) find no significant effects on married women. Consistent with this finding, Winkle & Wilson (2018) find that expansions in Head Start funding significantly raised employment and earnings of single mothers, suggesting that access to subsidized care is a significant determinant of labor force participation for this group. Our paper builds on this literature, showing that childcare constitutes an important fixed cost of work for single mothers with children five and under.

A large literature examines labor supply effects of the EITC and other tax reforms (see, e.g., Eissa & Liebman 1996, Meyer & Rosenbaum 2001). With the notable exception of Kleven (2019), this literature finds that the EITC significantly increases the labor supply of single mothers, reporting a wide range of elasticities. Effects are concentrated on the extensive margin, with estimated participation elasticities ranging from 0.69 to 1.17 in one study (Meyer & Rosenbaum 2001); the typical study finds effects that are much smaller and well below our own-wage employment elasticity(Hotz & Scholz 2003). The EITC phases out at higher earnings, leading theoretically to negative effects on the intensive margins. However, the EITC literature generally does not find intensive margin effects (Chetty et al. 2013 constitute a weak exception; they find small positive effects). Michelmore & Pilkauskas (2021) finds that the EITC's positive employment effects concentrate entirely among single mothers with very young children, as we do here, but their elasticities are much smaller than ours.

The literature on the effect of minimum wages on parental employment is scant. Page et al. (2005) find that higher minimum wages increase welfare caseloads, implying negative employment effects. Dube (2019*b*) estimates an elasticity of poverty with respect to the minimum wage of around -0.3 for single mothers. In contrast, Sabia (2008) finds that the minimum wage substantially reduces employment of low skill single mothers, leading to increased poverty. We discuss the differences between our analysis and theirs later in this paper.

With some notable exceptions (Borgschulte & Cho 2018, Agan & Makowsky 2018), discussions of minimum wage effects on employment have been less concerned with supply side explanations. This lacuna may reflect a consensus that male labor supply elasticities are close to zero. However, estimated female labor supply elasticities are sizeable and positive, including for low-wage women, as the EITC literature has shown.

Overall, the research literature on the labor supply of parents finds that the positive substitution effects of higher wages are greater than negative income effects. However, the literature is mixed. By using plausibly exogenous and multiple policy changes, our results help to clarify this literature.

We have organized the rest of the paper as follows: Section 2 first discusses the theoretical underpinnings of this paper and then introduces our empirical strategy and data. Section 3 presents our results, together with a battery of robustness and specification tests, section 4 discusses possible mechanisms and section 5 provides a brief discussion. Section 6 concludes.

2 Methods and data

2.1 Theoretical considerations

With standard (convex) preferences, we would typically expect small changes in wages to have only small effects on labor supply. However, when institutional factors give rise to kinks and non-convexities in the budget constraint, a small wage increase can induce larger shifts in employment, even in standard labor supply models (Blundell & MaCurdy 1999).

First, parents of minor children may be eligible for means-tested transfers to a greater extent than adults without dependents. This possibility includes not only cash transfers (AFDC/TANF), but also in-kind transfers and programs such as nutritional programs (WIC), childcare subsidies, Head Start and government-sponsored health insurance. These programs depend on income in a complex manner and have changed over time: For instance, TANF recipients are typically required to meet work requirements, while other programs may exhibit sharp cutoffs in eligibility at set income levels, leading to so-called benefit cliffs. In Appendix A we present a descriptive analysis of the relationship between family earnings and program participation.

A second source of non-convexities comes from the fixed costs of work (Cogan 1981, Heim & Meyer 2004). These costs include childcare costs (assuming they are not perfectly proportional to hours worked) as well as non-monetary costs (stress and worry about coordinating work and family). We argue that these fixed costs of work are typically larger for parents than for adults without children. In turn, they should also be larger for parents of younger, pre-school age children than for parents whose youngest child is of school age. In Appendix A we provide evidence that these fixed costs are especially substantial for single mothers.

2.2 Empirical models

2.2.1 The distributional difference-in-differences estimator

Our empirical analysis of employment effects follows the distributional difference-in-differences approach developed by Cengiz et al. (2019). We identify the overall effects of minimum wages on parental employment by analyzing changes in the number of jobs paying close to the new minimum wage. This estimator allows us to examine whether jobs below the new minimum wage are either eliminated or converted into jobs that pay just above the new minimum wage.[2] There may also be indirect effects on jobs paying slightly higher wages, such as if employers seek to preserve the internal wage structure of the firm. The estimator will show those shifts throughout the wage distribution. We measure the overall employment effect by comparing changes to jobs at or above the new minimum to the change in jobs below the new minimum.

We conduct two tests of the validity of the model. First, we verify that minimum wage increases do not affect earnings and employment in the years preceding minimum wage changes. Second, we verify that any employment effects occur only among jobs paying close to the new minimum wages; changes to the upper tail of the wage distribution thus provide another falsification test.

We consider all state minimum wage increases above \$0.25 between 1980-2016 (cf. Cengiz et al. 2019), yielding 152 events with an average increase of 10%. We include controls for federal changes and state level changes that do not meet the 25 cent criterion.[3]

The estimator counts employment in wage bins, which we define as 25 cent groups, from \$1.25 to \$30 plus two endpoints bins, $[0, 1.25)$ and $[30, +\infty)$.[4] In each bin, we count the number of workers (adjusted by the basic Outgoing Rotation Group weights and a QCEW multiplier).[5] We then divide that number by total population, which provides us with the dependent variable in Equation 1,

$$\frac{E_{sjt}}{N_{st}} = \sum_{\tau=-3}^{4} \sum_{k=-5}^{5} \alpha_{\tau k} * I_{sjt}^{\tau k} + \mu_{sj} + \rho_{jt} + \Omega_{sjt} + u_{sjt} \tag{1}$$

where s indexes states, j the wage bins and t are quarters. We generate treatment dummies $I_{sjt}^{\tau k}$ that turn on if bin j is within k dollars of a new minimum wage effective in

[2]Under imperfect compliance and wage misreporting, there will continue to be jobs paying less than the new minimum wage. This condition does not affect the validity of the estimator.

[3]The 25 cent restriction is in real terms to stay as close to Cengiz et al. (2019) as possible. We also estimated the distributional difference-in-differences models with a nominal 25 cent event restriction, which retains more events. We find that this increases precision across the board without substantively affecting the coefficients.

[4]These are real wages in 2019 dollars.

[5]The QCEW multiplier adjusts the employment to population ratios derived from the CPS survey data to those implied by administrative QCEW data, improving precision. See Cengiz et al. (2019) for an extensive discussion and validation exercise.

state s at time $t + \tau$. τ ranges from -3 to +4 years and we omit the pre-treatment year ($\tau = -1$) such that all coefficients can be interpreted as changes relative to the year before the minimum wage change. We restrict k to $[-4, 4]$ but include endpoints -5 and 5 which group any bins outside the $[-4, 4]$ range.[6] We include bin-by-state fixed effects μ_{sj} and bin-by-quarter fixed effects ρ_{jt}. Ω_{sjt} includes the controls for small and federal increases.[7] u_{sjt} is the error term, clustered at the state level.

2.2.2 Alternative model specifications

For robustness, we also estimate a set of alternative model specifications: a) a simple event-study, b) a scaled event-study model, where event time coefficients are interacted with the magnitude of minimum wage changes, and c) an event-by-event analysis (Sun & Abraham 2020). In all specifications, the unit of analysis is event by year; that is, we aggregate across wage levels and employment statuses. As a consequence, these models do not give the same granular view of the data. However, this specification allows for a more straightforward analysis of the outcomes for non-workers, for whom we do not observe any wage.[8]

To estimate these models, we treat educational attainment as our primary proxy for exposure to minimum wage jobs. Our primary results from these models are estimated on people with high school or less education. For robustness, we also identify a high exposure group where we exclude workers with hourly earnings of $15 or higher from the high school or less sample. Comparing these results to our preferred specification will indicate whether our estimates are driven by spurious changes to higher wage employment. As a placebo test, we also estimate models on those with a bachelor's degree or higher educational attainment. This group is unlikely to work in minimum wage jobs, thus any effects on this group are likely spurious, pointing to model mis-specification.

[6] Cengiz et al. (2019) does not include those endpoint bins, but they do let k go up to +17 dollars. We depart from their approach to improve efficiency in our smaller samples.

[7] These are finer than specified in Cengiz et al. (2019). They interact three timing indicators: EARLY (three to two years ahead), PRE (one year ahead) and POST (up to four years past) with two wage bin indicators (four dollars above or below new minimum wage). We retain the timing indicators, but include two more wage indicators for the bins outside the four dollars above/below range to be consistent with our specification of k.

[8] To illustrate, we wish to analyze how minimum wage changes affect the probability of being out of the labor force, by stated reason for non-participation. For these individuals, we do not observe the wage they would have earned if they had been in the labor force. Implementing the distributional difference-in-differences estimator is similarly not straightforward for outcomes that are measured over a longer time period, e.g. annual earnings.

Simple and scaled event-study models Our starting point is the following simple event-study specification:

$$y_{\tilde{s}t} = \theta_t + \theta_{\tilde{s}} + \sum_{k=-4,k\neq-1}^{4} \pi_{k(\tilde{s},t)}\rho^k + \varepsilon_{\tilde{s}t} \tag{2}$$

where $y_{\tilde{s}t}$ is the outcome of interest, θ_t are year dummies and $\theta_{\tilde{s}}$ are event-specific state fixed effects.[9] The $\pi_{k(\tilde{s},t)}$ are a set of event time indicators:

$$\pi_{k(\tilde{s},t)} = 1(t - t^*_{\tilde{s}} = k)$$

where $t^*_{\tilde{s}}$ is the year in which the event s takes place. The parameters of interest are then the ρ^k coefficients, capturing the treatment effect k years after the minimum wage increase.

We omit the year before the initial minimum wage increase $k = -1$ as the ρ^k coefficients are only identified relative to each other. Because there are no never-treated states - all states experience multiple minimum wage changes - we bin event time following Schmidheiny & Siegloch (2019), allowing us to achieve identification under the assumption that effects are constant beyond this point Borusyak & Jaravel (2017)[10] such that

$$\pi_{-4(\tilde{s},t)} = 1(t - t^*_{\tilde{s}} \leq -4)$$

The scaled version of the event-study model incorporates variation in the magnitude of minimum wage changes by interacting event time with a measure of the total minimum wage change over the period. Let

$$\delta_{\tilde{s}} = log(MW^{max}_{\tilde{s}}) - log(MW^{min}_{\tilde{s}})$$

denote the change in state minimum wage over the event period, including any phase-ins. Then the regression model of the scaled event-study can be written as

$$y_{\tilde{s}t} = \theta_t + \theta_{\tilde{s}} + \sum_{k=-4,k\neq-1}^{4} \left(\delta_{\tilde{s}} \times \pi_{k(\tilde{s},t)}\right)\rho^k + \varepsilon_{\tilde{s}t} \tag{3}$$

[9]When a state has multiple minimum wage increases, these are treated as separate events.

[10]The binned end point is excluded from the event study figures. As pointed out by Borusyak & Jaravel (2017), the choice of where to bin event time can potentially affect estimated event time coefficients. Alternative specifications, binning event time at -5 and -7, find overall similar results.

To compare the employment effects from the alternative specifications to the effects found in the distributional difference-in-differences approach, we summarize the treatment effects in a single point estimate, using the event study model of equation (3) to estimate the semi-elasticity of outcome y with respect to the minimum wage. In these models, the individual event time indicators are replaced by a single indicator variable equal to one in the years prior to the minimum wage increase, and 1 in the year of the first increase and subsequent years. Here, we estimate the following two-equation system, in which we use the (interacted) post-increase dummy as an instrument for the log min wage, effectively scaling the estimated change in employment with the corresponding change in the log minimum wage.[11]

$$\begin{aligned} logmw_{\tilde{s}t} &= \theta_t + \theta_{\tilde{s}} + \left(\delta_{\tilde{s}} \times 1(t - t^*_{\tilde{s}} \geq 0)\right) \beta^{FS} + \varepsilon_{\tilde{s}t} \\ y_{\tilde{s}t} &= \theta_t + \theta_{\tilde{s}} + \widehat{logmw}_{\tilde{s}t} \beta^{2SLS} + \varepsilon_{\tilde{s}t} \end{aligned} \tag{4}$$

Event-by-event analysis We also implement an event-by-event analysis following the approach of Sun & Abraham (2020) and implemented as in Cengiz et al. (2019), with some modifications. This approach starts from a sample of events with a group of clean (untreated) control states for each treated state (duplicating observations).[12]

We implement two versions of this analysis. The first stacks all events, aligned by event-time, in one single regression. We include event-specific state and year fixed effects:

$$y_{hst} = \theta_{ht} + \theta_{hs} + \sum_{k=-3, k\neq -1}^{2} \pi_{k(hst)} \rho^k + \varepsilon_{hst} \tag{5}$$

where the h subscript identifies events and ρ^k is the average effect of the minimum wage on outcome y_{hst}.

The alternative implementation keeps each event separated, and thus involves estimating event-specific coefficients ρ^{hk}:

$$y_{hst} = \theta_{ht} + \theta_{hs} + \sum_{k=-3, k\neq -1}^{2} \pi_{k(hst)} \rho^{hk} + \varepsilon_{hst} \tag{6}$$

[11]This is similar in spirit to Cengiz et al. (2019) scaling the event study derived estimates by the percent change in the minimum wage.

[12]Untreated here refers to untreated over the (five year) time window considered. We use the same events as Cengiz et al. (2019).

We then average the ρ^{hk} estimates afterward, using the estimated population in each event as weights:

$$\rho^k = \frac{1}{H} \sum_h \omega_h \rho^{hk} \tag{7}$$

Two-way fixed effects model While our empirical analysis primary utilizes the distributional difference-in-differences and event study models, we also report results from two-way fixed effects models in some contexts, such as in our analysis of the SIPP, where data is available only for non-consecutive years.[13] The formal two-way fixed effects model is:

$$y_{ist} = \theta_t + \theta_s + X_{ist}\beta + logmw_{st} + \varepsilon_{ist} \tag{8}$$

2.3 Data

The main data source is the Current Population Survey, covering the years 1982-2019. Our analysis of employment effects relies primarily on the Merged Outgoing Rotation Group (CPS MORG), where employed respondents report earnings and hours worked. We retain individuals aged 16-55. The data includes a rich set of demographic variables, such as age, education, marital status, race and ethnicity, and age and number of children in the household. For additional analyses, we also use the Annual Social and Economic Supplement (CPS ASEC).[14] The ASEC contains information about respondents' stated reasons for not working as well as information about income from various sources. We adjust nominal variables for inflation using the CPI-RS. Both the CPS-MORG and the CPS-ASEC are downloaded from IPUMS.[15] We then merge these samples to data on state level minimum wages (Vaghul & Zipperer 2019).

To analyze the effect on childcare needs as a potential mechanism, we estimate auxiliary models of paid child care utilization using the Survey of Income and Program Participation

[13]In addition, we also estimate this specification to compare our findings with the literature; see the discussion section for details. In order to facilitate these comparisons, equation (8) includes context-specific covariates X_{ist}. The distributional difference-in-differences approach of Cengiz et al. (2019) does not include covariates; for consistency, we also omit these from the alternative models in (2) - (6). Results from these models were robust to the inclusion of state-level controls for welfare reform, medicaid eligibility thresholds, population, and state EITC, as well as cell-level characteristics.

[14]The ASEC is a retrospective survey in which respondents answer question about the previous year. For the ASECs, we use the 1983-2020 survey years, which cover the 1982-2019 period.

[15]IPUMS-CPS, University of Minnesota, www.ipums.org (Flood et al. 2017).

(SIPP). The SIPP is a longitudinal survey of U.S. households administered by the U.S. Bureau of the Census. The primary purpose of the survey is to collect detailed information on all sources of income, including labor income and income from various government programs. To provide context for these income measures, the SIPP also asks questions on a range of other topics, including disability, assets, well-being, and childcare. The SIPP child care module has more detailed data on child care, particularly by type of arrangement, than any other publicly available large survey dataset; however, these topical modules are not administered at every interview. We include a more detailed description of the SIPP in Appendix C.

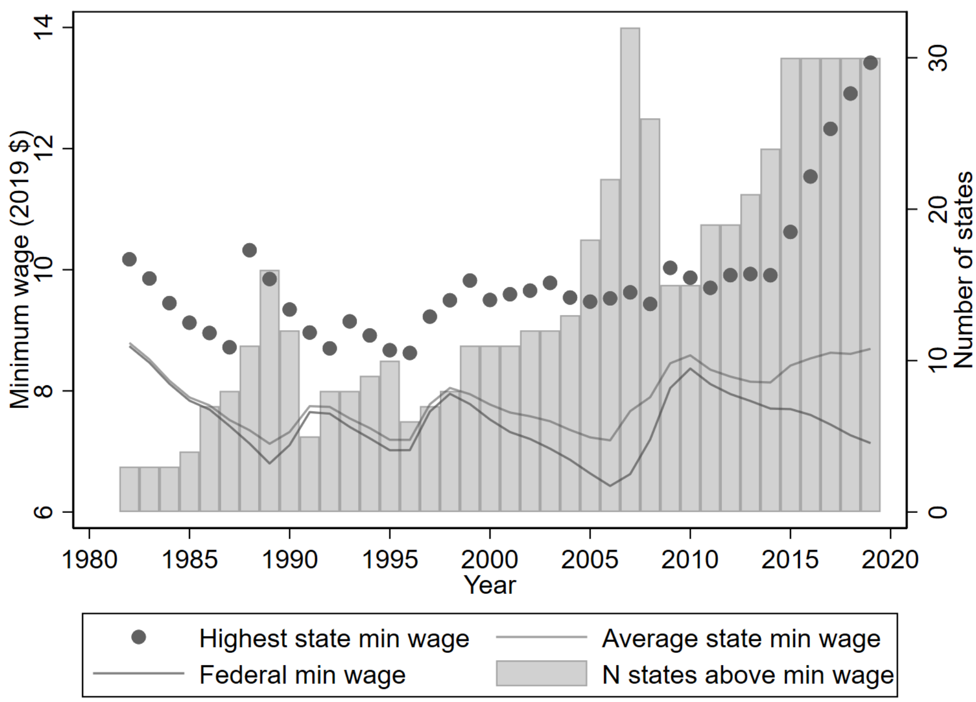
Figure 1 illustrates the variation in minimum wages over the sample period. As the figure illustrates, minimum wages vary considerably, in particular in the last half of the sample period, as an increasing number of states implement minimum wages that are above the federal minimum.

2.4 Descriptives

Table 1 shows the characteristics of workers who earn up to 110% of the applicable minimum wage. Teens - workers aged 16-19 - make up 28.5% of minimum wage workers, while just under half of all minimum wage workers can be classified as prime-age (25 or older). Over the sample period, teens make up a declining share of minimum wage workers. In 1982, 33% of all minimum wage workers were aged 19 or under; by 2019 this number had dropped to 20 percent.

Three in ten minimum wage workers have children. Of these, a majority are married: just under one fifth of all minimum wage workers are married with minor children. Consistent with female over-representation in low wage jobs, parents earning the minimum wage are more likely to be female. The share of parents stays remarkably similar over the sample period. Compared to minimum wage workers without children, parents in minimum wage jobs are more likely to have a high school diploma; they are also significantly older (average age is 35 years for parents, 25 years for workers without co-resident children; see Appendix Figure B1 for the estimated age distribution). This suggests minimum wage work is not only a transient early-career state for low wage parents.

Figure 1: Minimum wage policies, 1982-2019



Note: Figure shows variation in state minimum wage policies over the sample period. "Highest state min wage" refers to the highest annual state minimum wage (averaged over the calendar year). The bars count the number of states with minimum wage policies exceeding the Federal minimum at the end of each calendar year. The black and grey solid lines plot the federal minimum wages and the average state minimum wage, averaged over each calendar year. Source: Vaghul & Zipperer (2019)

Table 1: Characteristics of minimum wage workers

	(1) All years mean	(2) 1982-1996 mean	(3) 1997-2019 mean
Teens	0.285	0.321	0.255
Age 20+	0.715	0.679	0.745
Age 25+	0.475	0.441	0.504
No children	0.687	0.704	0.673
Parent	0.313	0.296	0.327
Mothers	0.236	0.235	0.236
Fathers	0.077	0.061	0.091
Unmarried 1+ child	0.104	0.076	0.127
Single mothers	0.091	0.071	0.107
Single fathers	0.013	0.005	0.020
Married 1+ child	0.209	0.220	0.200
Married mothers	0.145	0.164	0.129
Married fathers	0.064	0.056	0.071
Observations	443771	241950	201821

Notes: Workers earning wage $w < MW \times 1.1$ Source: CPS MORG. Weighted using the sample weights.

3 Results

3.1 Results from the distributional difference-in-differences estimator

To illustrate the various outcomes produced by the distributional difference-in-differences estimator, we first show results for the entire working population. We then discuss how these results differ along parental dimensions.

All results are derived from the $\alpha_{\tau k}$ coefficients in Equation (1), which are estimates of the change in the employment to population ratio in each wage bin due to minimum wage increases. We aggregate these estimates in different ways to present our results over the wage distribution, over time and overall. We describe the intuition behind the main outcomes below and refer to Cengiz et al. (2019, pp. 1417-1419) for the minutiae (we maintain their notation).

3.1.1 Employment effects among all workers

Panel (a) of Figure 2 shows the effect of minimum wages over the relative wage distribution, averaged over the post period. Each bar thus averages over five coefficients (the number of post treatment years) and involves twenty wage bin observations per event (five times the four 25 cent bins per value of k, measured on the x axis). After a minimum wage increase, we observe a decline in jobs paying one dollar below the new minimum wage and a corresponding increase in the number of jobs paying up to four dollars above the new minimum. Reassuringly, we do not observe any changes in the number of jobs paying more than $5 above the new minimum wage (grouped in the +5 bar).

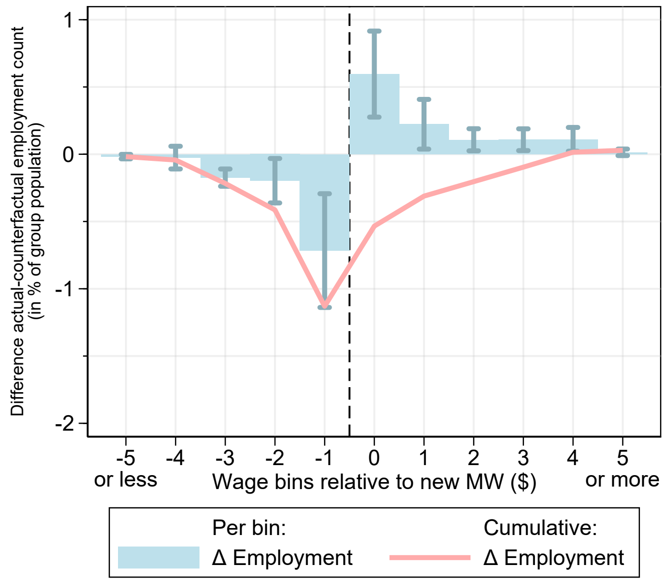
The red line shows the cumulative employment response. Consistent with the description above, we observe a substantial dip in the number of jobs paying up to the new minimum wage, which is fully compensated by the increase in the number of higher paying jobs.

Panel (b) of Figure 2 show the effects of minimum wage policy over time, averaged over each side of the relative wage distribution. For example, the blue solid line at $t = 0$ averages over the six positive coefficients, $k \in \{0, 1, ..., 4, 5\}$ and the dashed red one over the five negative ones, $k \in \{-5, -4, .., -2, -1\}$.

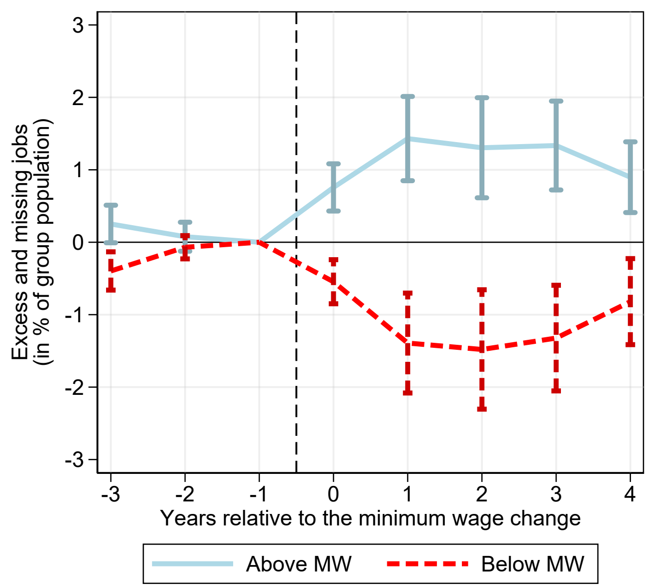
The minimum wage affects employment immediately and the effects persist several years after the minimum wage increase.[16] Moreover, the decline in jobs below the new

[16]The effects decline somewhat over time, which is unsurprising given that inflation gradually erodes the

Figure 2: Distributional difference-in-differences estimator. Full population, 1982-2019.



(a) Employment effects over the relative wage distribution



(b) Employment effect over time

Note: Figure plots estimated effects on employment to population ratios with 95% confidence intervals. Estimates derived from the distributional difference-in-differences specification from equation (1). See Section 3 for details on the calculations.

minimum wage is approximately the same as the increase in the number of jobs paying above, consistent with the lack of an overall employment effect. Pre-treatment trends are small to non-existent.

The overall employment effects are summarized in column (1) of Table 2. The first two rows show the post-treatment impact on the number of jobs above and below the new minimum wage respectively, normalized by population and the pre-treatment employment to population ratio (EPOP). The normalization implies that these estimates can be interpreted as the percentage changes in EPOPs. The third row shows the sum of the two previous rows and thus captures the overall change in employment. Consistent with the earlier figures, the employment gains and losses are equal and the effect on net employment is zero.

Row four relates this zero change in employment to the pre-treatment affected working population, which Cengiz et al. (2019) define as everyone earning less than the new minimum wage in the year before an increase. Row five provides the estimated employment elasticity, defined as the percentage change in EPOP (row three) divided by the average minimum wage change (10 percent).

To facilitate comparison of this employment elasticity to the estimated labor supply elasticities in the literature, we also provide the own-wage employment elasticity (OWE). These calculations leverage that our estimated coefficients provide information on changes in employment over the wage distribution (relative to the minimum wage). Combined with the average level of new minimum wages, we can then calculate the change in the total wage bill of affected workers (net of changes in employment). In turn, the post-treatment average wage of affected workers is simply the new wage bill divided by post-treatment employment and the percentage wage change is the post-treatment wage divided by the pre-treatment wage (minus one, presented in row 6).

Finally, the own-wage employment elasticity is the ratio of the percentage change in affected employment (row 4), divided by the percent increase in the average wage (row 6). These are shown in row 7.

3.1.2 Employment effects by parental status

We estimate equation (1) separately by gender, marital status and the presence of children. Due to low sample size, we do not report results for single fathers. We expect childcare

real minimum wage. This pattern is visible in Figure 2 because the bins are defined in real terms.

Table 2: Distributional difference-in-differences estimator. Summary table of major groups, 1982-2019

Marital status	Any	Single woman			Married woman			Married man		
Parental status	Any	Age 0-5	Age 6-17	No Kids	Age 0-5	Age 6-17	No Kids	Age 0-5	Age 6-17	No Kids
1. Δ EPOP above MW: Δa	0.01***	0.05***	0.02***	0.03***	0.01***	0.01	0.01	0.00*	0.00**	0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
2. Δ EPOP below MW: Δb	-0.01***	-0.02***	-0.01*	-0.03***	-0.01**	-0.01***	-0.01***	-0.00	-0.00	-0.01**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
3. Δ EPOP around MW: Δe	0.00	0.02**	0.00	-0.00	-0.00	0.00	0.00	0.00	0.00	-0.00
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
4. %Δ affected employment	0.00	0.14**	0.06	-0.01	-0.02	0.03	0.03	0.05	0.04	-0.00
	(0.02)	(0.06)	(0.09)	(0.03)	(0.06)	(0.08)	(0.12)	(0.07)	(0.11)	(0.10)
5. Employment elasticity w.r.t. MW	0.00	0.21**	0.05	-0.02	-0.02	0.02	0.01	0.02	0.01	-0.00
	(0.02)	(0.09)	(0.07)	(0.04)	(0.05)	(0.05)	(0.06)	(0.02)	(0.03)	(0.03)
6. %Δ affected wages	0.06***	0.13***	0.05	0.06***	0.06**	0.05	0.07	0.06*	0.08***	0.07
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.05)	(0.03)	(0.03)	(0.04)
7. Emp. elasticity w.r.t. affected wage	0.09	1.12***	1.19	-0.21	-0.34	0.62	0.44	0.80	0.50	-0.03
	(0.41)	(0.36)	(1.64)	(0.44)	(1.07)	(1.36)	(1.47)	(1.04)	(1.37)	(1.49)
Number of events	152	152	152	152	152	152	152	152	152	152
Number of bins	914736	913084	914736	914736	914736	914736	914736	914736	914736	914736
Number of workers	4603241	99714	165501	709018	301580	423178	373302	448284	459955	361563

Note: Estimates derived from the distributional difference-in-differences specification from equation (1). See Section 3 for details on the calculations.

costs to be a substantially greater barrier to employment for parents of pre-school age children, relative to parents of older children and adolescents. For parents, we thus split the sample further by age of the youngest child, estimating models separately for parents who have at least one child age 0-5 and parents whose youngest child is between 6 and 17.

Columns (2) - (10) of Table 2 summarize these models. We do not find any evidence of disemployment effects in any of the groups. However, we do find a large positive employment effect for single mothers with at least one child age five or younger (+0.02, se 0.01).[17] For this group, the change in employment in jobs paying above the new minimum wage is substantially larger than the reduction in employment in jobs that pay less than the new minimum wage. The net employment response is positive and statistically significant. Quantitatively, the effect is economically meaningful: we estimate an elasticity of employment with respect to the minimum wage of 0.21. At 1.12, the elasticity of employment with respect to the *own wage* is positive, significant and fairly large (see Section 5 for further discussion).

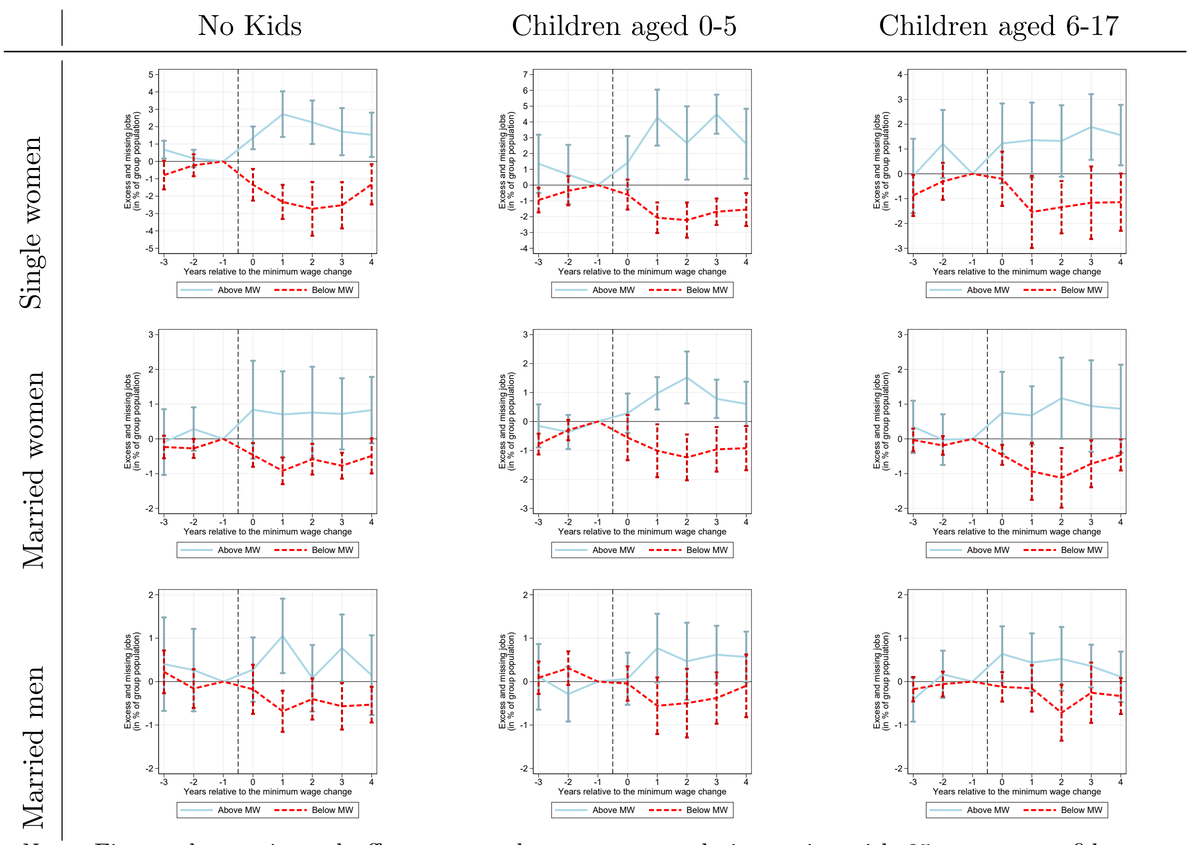
Figure 3 shows effects of the minimum wage over time for each of these nine sub-samples. Pre-treatment trends in employment are largely flat for all groups. Similar to what we found for the full working population, the minimum wage induces a shift in employment from jobs paying below the minimum wage to jobs that pay above the minimum wage. For most of the sub-samples these shifts balance each other, so that the net employment effect is zero. For single mothers of young children, however, the minimum wage increase significantly increases the number of higher paying jobs, outweighing the loss of jobs below the new minimum wage.

Figure 4 shows that the employment gains for single mothers of children age 0 to 5 all occur in jobs paying up to $4 dollars above the new minimum wages. For jobs that pay $5 dollars or more above the new minimum wage, we find no changes in employment. That is, we are able to rule out that the positive employment effect for single mothers of preschool-age children is driven by spurious changes at the top of the wage distribution.

To summarize, the distributional difference-in-differences estimator finds a significant increase in the employment rate of single mothers of children age 0-5, driven by increased employment in jobs that pay close to the new minimum wage. This positive employment effect is consistent with a labor supply story in which minimum wage increases push wages above the reservation wages of single mothers with young children. For all other groups,

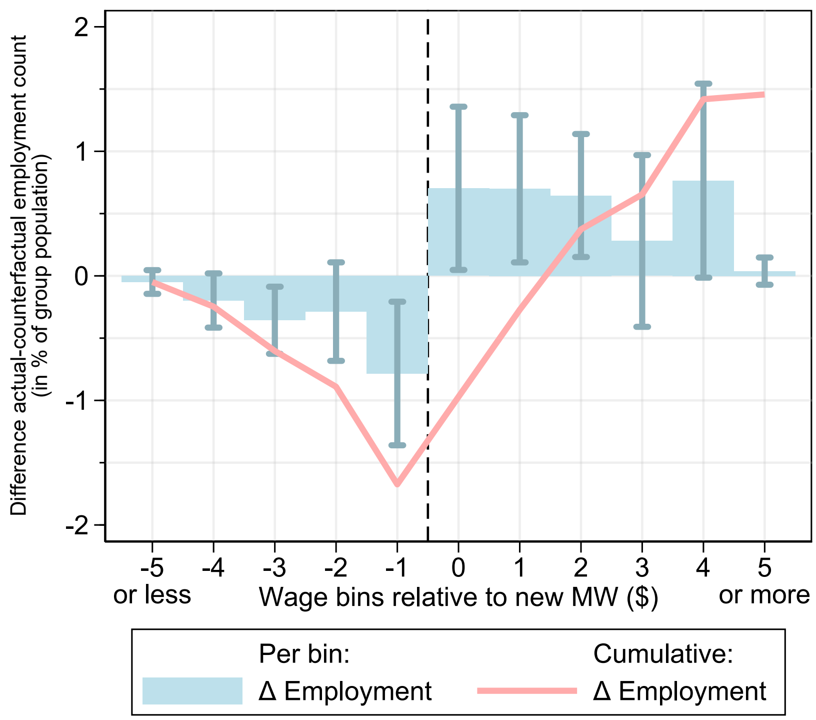
[17] This positive employment effect for mothers with young children is significantly different from the null effects in the other groups (mean p-value: 6.0%, median: 4.1%).

Figure 3: Distributional difference-in-differences estimator, effects over time by marital and parental status, 1982-2019



Note: Figure plots estimated effects on employment to population ratios with 95 percent confidence intervals. Estimates derived from the distributional difference-in-differences specification from equation (1). See Section 3 for details on the calculations.

Figure 4: Distributional difference-in-differences estimator, effects over relative wage distribution. Single mothers with youngest child younger than five, 1982-2019



Note: Figure plots estimated effects on employment to population ratios with 95% confidence intervals. Estimates derived from the distributional difference-in-differences specification from equation (1). See Section 3 for details on the calculations.

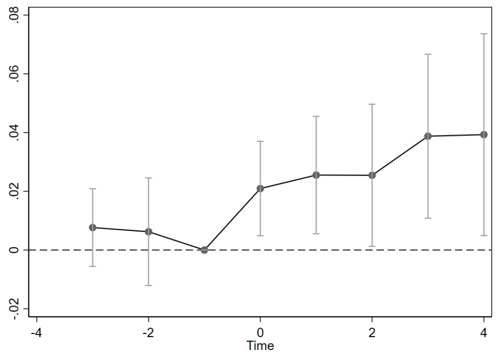
we continue to find employment effects indistinguishable from zero.

3.2 Robustness

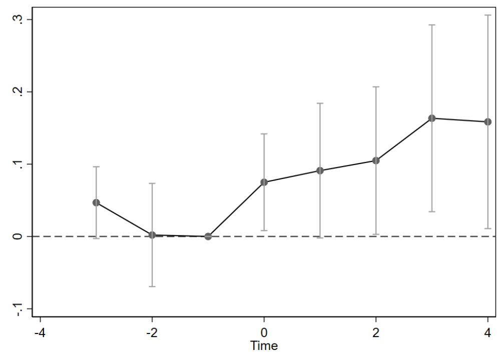
3.2.1 Results from alternative event study specifications

The distributional difference-in-differences estimator finds a significant positive effect of the minimum wage on the employment rate of single mothers with children 0-5. To assess the robustness of this finding, Figure 5 presents results from alternative event study regression specifications, based on workers with at most a high school diploma. Panels (a) and (b) consider the simple and scaled event study specifications of equations (2) and (3). Panels (c)

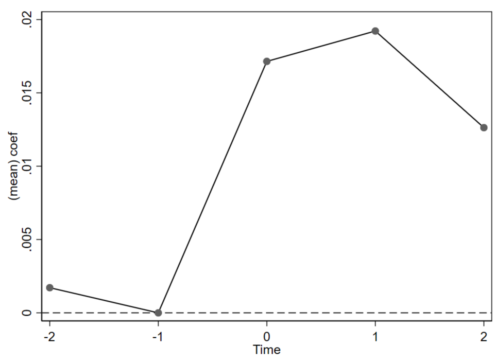
Figure 5: Employment, single mothers with young children, alternative specifications



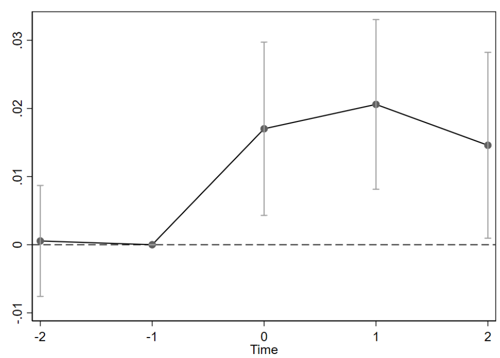
(a) Simple event study (unscaled)



(b) Scaled event study model



(c) Event-by-event, averaged



(d) Stacked events sample

Notes: Figure plots selected coefficients from event study models with 95% confidence intervals. All models estimated on single mothers with high school or less education, with one or more children age five or younger. Standard errors clustered at the state level.

and (d) represent the two versions of Sun & Abraham (2020)'s event-by-event approach.[18]

The results are consistent with those from the baseline model. In particular, the figure shows a clear increase in employment rates of single mothers with a child 5 or under across all specifications. As an additional robustness test, we also estimate all models on a subset of parents excluding workers earning \$15/hour or more (see Appendix Figure B2). Results remain stable when these higher wage workers are excluded from the sample, suggesting the

[18]In panel (c), the event-specific estimates are manually averaged (weighted by population), cf. Equation 7, while the estimates in panel (d) refer to the aggregated coefficients estimated in the stacked approach described in Equation 5.

estimates are not driven by spurious changes in high wage employment. We also conduct a placebo test on single mothers with at least a bachelor's degree. Reassuringly, our models fail to find any indication of positive employment effects for this plausibly unaffected group (see Appendix Figure B3).

Appendix Table B2 summarizes the scaled event study model, presenting two stage least squares estimates of elasticities with respect to the minimum wage (equation 4). The estimated increases in employment are statistically significant at the 5 percent level and imply an elasticity of employment with respect to the minimum wage of 0.41 for single mothers with children 0-5.[19]

We use the ASECs to estimate the effect on labor force participation (defined as having worked at any time during the reference year), annual weeks worked, weekly hours and annual hours worked. These models, presented in Appendix Figure B4, suggest a marginally significant positive effect on participation, and significant increases in annual weeks, weekly hours, and annual hours worked.

3.2.2 Robustness of the distributional difference-in-differences specification

Our specifications estimate effects on the extensive margin of employment. Appendix Table B1 presents results from a set of models of full-time equivalent job counts.[20] The elasticity of FTE employment to the minimum wage remains very similar at 0.19 (compared to 0.21 for headcounts), ruling out large changes in hours worked.

The baseline model includes state-bin and quarter-bin fixed effects, effectively controlling for bin-specific (time-invariant) state characteristics as well as (nationwide) changes in parental employment patterns over time. We estimate more saturated models to account for potential unobserved spatial variation in outcomes (Table 3). The results are robust to adding bin-state linear time trends, quadratic trends and census division-specific time fixed effects. The employment effect for single mothers with a child 5 or under is positive and significantly different both from zero and from the employment effect for all workers.[21]

[19]This estimate is larger than the 0.21 estimated in the baseline model. However, the samples are not directly comparable, as the models in Appendix Table B2 are estimated on a low education subsample, while our preferred distributional difference-in-differences models are estimated on the full sample of single mothers.

[20]In the baseline models, we count the number of workers in each bin. In the full-time equivalent models of Appendix Table B1, we sum up FTEs instead. E.g. someone working twenty hours per week would only count as half a worker.

[21]The models with quadratic and/or linear time trends without population weights are the exception. The difference is entirely due to reduced efficiency as the effect size even increases.

Table 3: Distributional difference-in-differences estimator. Different specifications, 1982-2019

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. Δ EPOP above MW: Δa	0.05***	0.04***	0.04***	0.05***	0.05***	0.05***	0.05***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
2. Δ EPOP below MW: Δb	-0.02***	-0.03***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
3. Δ EPOP around MW: Δe	0.02**	0.02**	0.02*	0.03**	0.03*	0.03*	0.03	0.05**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
	[0.03]	[0.04]	[0.04]	[0.01]	(0.02)	[0.14]	[0.16]	[0.03]
4. %Δ affected employment	0.14**	0.12**	0.11*	0.20**	0.24*	0.23*	0.22	0.35**
	(0.06)	(0.06)	(0.06)	(0.08)	(0.14)	(0.13)	(0.14)	(0.14)
5. Employment elasticity w.r.t. MW	0.21**	0.18**	0.17*	0.30**	0.31*	0.30*	0.28	0.45**
	(0.09)	(0.09)	(0.09)	(0.12)	(0.18)	(0.18)	(0.18)	(0.18)
6. %Δ affected wages	0.13***	0.12***	0.12***	0.15***	0.16***	0.16***	0.16***	0.17***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
7. Emp. elasticity w.r.t. affected wage	1.12***	0.98***	0.98***	1.37***	1.45**	1.40**	1.33**	2.04***
	(0.36)	(0.33)	(0.36)	(0.42)	(0.60)	(0.61)	(0.63)	(0.56)
Number of events	152	152	152	152	152	152	152	152
Number of bins	913084	913084	913084	913084	961401	961401	961401	961401
Number of workers	99714	99714	99714	99714	99714	99714	99714	99714
State population weights	Y	Y	Y	Y				
Bin-state linear trends		Y	Y	Y		Y	Y	Y
Bin-state quadratic trends			Y				Y	
Bin-division period FE				Y				Y

Note: P-value of difference to group of all workers shown in brackets for the employment effect. Estimates derived from the distributional difference-in-differences specification from equation (1). See Section 3 for details on the calculations.

Removing the population weights increases the employment effect in size but reduces statistical significance, suggesting the effect is more precisely estimated in states with a large populations.[22]

Our estimated models focus on the population of parents with co-resident children by marital status. During the sample period the composition of these sub-samples may have changed. These changes could be correlated with the minimum wage, leading to risks of endogenous sample selection.[23] For example, Bullinger (2017) finds that minimum wages reduce teen births.

To address this question, we have estimated a set of event study models of the fraction of women who are single mothers of preschool-age children, using the aggregate specification from Equation 3. These models are estimated on the full population of women (all education levels), capturing changes in marital status and parenthood.[24] Results from these models, presented in Appendix Figure B8, do find that the share of women who are single mothers falls gradually following minimum wage increases.

This reduction in the share of women that are single mothers could lead to shifts in the demographic characteristics of our estimation sample over time.[25] Therefore, we reweight the sample such that potentially relevant characteristics (age, education and race) in each state-year are similar to the sample average (Appendix Figure B5).[26] The positive employment elasticity remains essentially unchanged at 0.26.

3.3 Effects on income, poverty, and welfare receipt

We find significant increases in the employment of single mothers with children ages 0 to 5 following minimum wage increases, pointing to possible reductions in poverty for this group. In this section, we address this question by estimating event study models of annual earnings, poverty, and welfare receipt using data from the ASECs (see Equation 3). Results for single mothers with at least one child age five or younger are shown in Figure 6.

[22] This is unsurprising since larger states contain more observations in each wage bin.

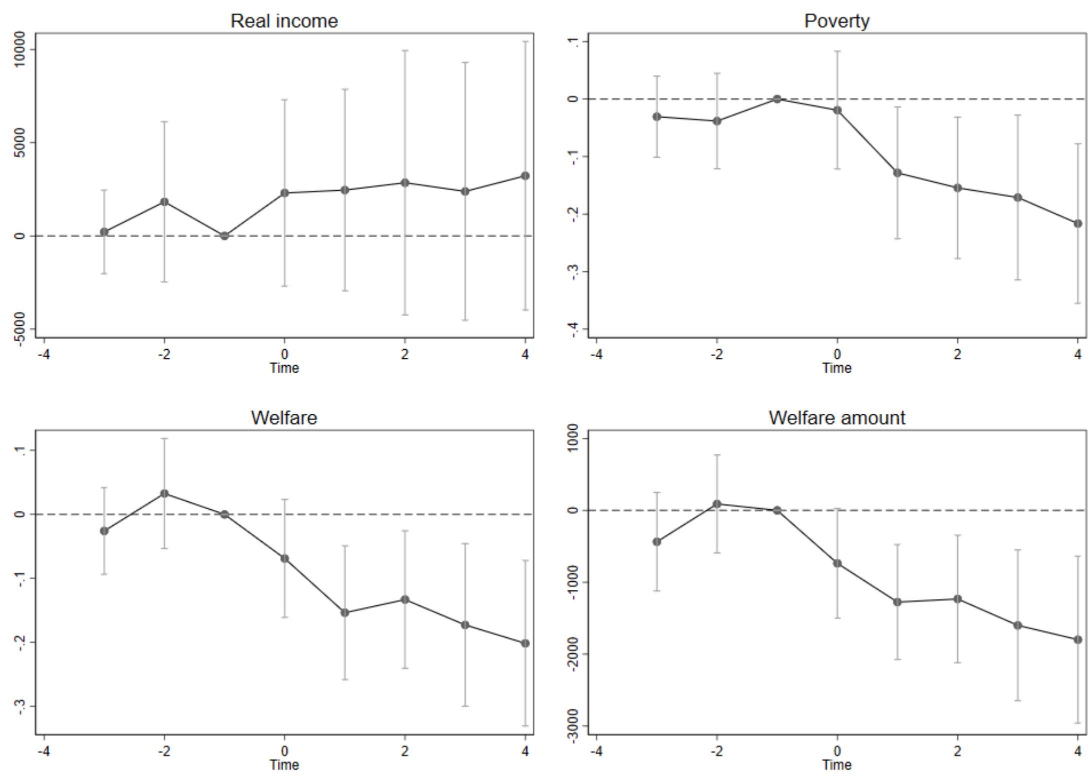
[23] That is, if the minimum wage affects the probability that low wage workers have at least one child, the estimated effects could include effects resulting from compositional changes in the population of parents.

[24] We also estimate these models on a sample of nonworking and low-wage women, excluding workers earning more than $15/hour. The results are consistent across specifications

[25] E.g. the fertility reduction could be particularly pronounced in certain age, education or race groups.

[26] The approach is similar in spirit to Bailey & Goodman-Bacon (2015) and Callaway & Sant'Anna (2019)'s propensity score reweighting, where observations in the control group are weighted by (a function of) the propensity score making them more similar to the treatment group, see Appendix Figure B5 for details.

Figure 6: Income and poverty, single mothers of young children



Notes: Figure plots selected coefficients from event study models in equation (3) with 95% confidence intervals. All models estimated on single mothers with high school or less education, with one or more children age 5 or younger. Standard errors clustered at the state level.

The panel on the upper left shows that minimum wage increases did not lead to an increase in total personal earned income (adjusted for inflation). This result may reflect the excess weight this regression places on higher incomes; we do find a significant increase in income when we exclude single mothers earning more than $15/hour (see Appendix Figure B6). The upper right panel shows a clear negative effect on single mothers' poverty rates, consistent with an increase in incomes at the low end of the income distribution.

The two bottom panels show the effects on welfare receipt. On the left, the dependent variable is an indicator equal to one if the family received any income from public assistance or welfare, while the panel on the right refers to the dollar amount of assistance. For both measures, the estimated models indicate parallel pre-trends followed by sharp reductions in welfare receipt following minimum wage increases.

We estimated the models in Figure 6 on the sample of single mothers with children 0-5 and who have at most a high school education. As a robustness check, we also estimate the models on a sub-sample that excludes women who are employed in jobs with annual earnings corresponding to an hourly wage of $15 or higher. Results from this exercise, presented in Appendix Figure B6, are nearly identical to our preferred estimates, suggesting the effects are not driven by spurious changes to higher wage jobs.

3.4 Heterogeneous impacts of minimum wages on employment

The large positive effects we estimate for single mothers with children 0-5 could mask heterogeneous responses for other subgroups. In particular, employers may substitute away from less skilled workers, which would be reflected in negative demand effects on disadvantaged groups, such as mothers who have not completed high school. To examine these questions, we have estimated separate versions of the baseline model by race/ethnicity, education and age (see Appendix Table B3). Employment effects remain positive across all subgroups. However, the reduced sample sizes substantially reduce statistical power; as a result, few of the point estimates remain statistically significant. The exceptions suggest that the positive employment effects are particularly large for black, Hispanic or young mothers (age 16-24). Reassuringly, none of the models point to the disemployment effects that would be implied by labor-labor substitution.

Additionally, the sample years, 1982-2019, span a period with significant changes in safety net programs targeting families with children. To assess the impact of these changes on our findings, we have estimated the panel models separately on pre- and post welfare reform samples. We present the results from this exercise in Appendix Figure B9. The positive employment effects occur mainly in the post-welfare reform period; we do not find any significant effects in the pre-reform 1982-1996 sample. This difference could reflect institutional factors, such as the passage of PROWRA, which changed participation incentives for many low-income families. It is also consistent with Cengiz et al. (2019)'s finding that employment shocks in the 1980s correlate with later state minimum wage changes, potentially leading to negative bias in estimates of minimum wage employment effects.

4 Mechanisms: childcare costs and family responsibilities

4.1 Child care costs

Single parents may face fixed costs of working in the form of childcare costs. Higher minimum wages may enable parents to overcome these costs and enter the labor force. Our preferred specifications found that the positive employment effects were concentrated among mothers of pre-school age children; for mothers of school-age children, employment effects were not statistically different from zero. These differential impacts by child age point to childcare availability as a barrier for single mothers' employment, consistent with women being primary caregivers.

To further analyze the role of childcare costs, we use data from the Survey of Income and Program Participation (SIPP) to construct measures of overall childcare use, as well as use of formal care (day care centers and family day care) and informal care provided by grandparents and other relatives.[27] We then estimate the two-way fixed effect panel models of Equation 8 on samples of parents with high school or less, as well as on a placebo sample of parents with at least a bachelor's degree. We present the results from this exercise in Table 4. Higher minimum wages increase the overall amount that single mothers spend on childcare, particularly on higher quality formal childcare. We do not find any significant effect on the probability of paying for childcare in general. These results suggest that

[27] See Appendix C for details. Ideally, we would also want to see how employment effects vary across states with higher and lower childcare costs. However, this approach is difficult to implement. First, we would need to estimate the expected childcare costs a parent would incur in order to enter the workforce. For low income workers, this number would depend not only on the market price and availability of childcare, but also on the availability of subsidized care. Indeed, Winkle & Wilson (2018) find that access to subsidized care increased employment of single mothers. Second, we would need to disentangle variation in childcare costs from other variation in living costs. To adequately address these two points is beyond the scope of this paper.

minimum wage increases may allow single mothers to pay for higher quality childcare and for more hours of child care. However, we cannot test this question with available data. Moreover, as we discuss in Appendix C, since child care data is available in the SIPP only for a limited set of years, effects will be estimated with low precision.

Distinguishing between formal childcare arrangements and care by relatives, the models find that higher minimum wages are associated with a marginally significant increase in the probability that single mothers pay for formal childcare, as well as a significant increase in the average weekly payment to formal providers. While some effects are estimated with low precision, the point estimates indicate large impacts. Relative to the sample mean, a ten percent higher minimum wage increases formal childcare use by 20 percent, and childcare expenditures increase by 28 percent. For married parents, we do not find any significant effects on childcare use: some point estimates are negative but never significant.

A complicating factor in interpreting these estimates is that higher minimum wages may also affect the price of childcare, as childcare is labor intensive, and childcare workers are typically paid low wages (Thomason et al. 2018). This price effect could show up as higher weekly childcare spending. However, we do not find any effects on childcare payments for married parents, suggesting that price effects are unlikely to drive the estimated increase in single mothers' childcare expenses. Similarly, we fail to find evidence that higher minimum wages increase childcare utilization in the placebo sample.

4.2 Family responsibilities

We consider here whether our finding significant positive effects of minimum wages on employment rates of single mothers with children 0-5 is driven by mothers choosing paid employment rather than staying home to care for young children. The CPS ASEC queries respondents who did not work the previous year for the reason why. Using this information, we estimate effects of the minimum wage on the probability that a person responds that they did not work in order to take care of home and family, as well as the probability that they did not work for all other reasons.[28] The intuition is that higher market pay induces non-workers to enter the labor force. Our models, presented in Figure 7 find that single mothers are indeed less likely to stay at home to take care of families when the minimum wage increases. The models find no significant reductions in the probability that single

[28] The other categories of non-work include going to school, illness/disability, inability to find work, retirement and *other*.

Table 4: Effects of minimum wages on childcare utilization

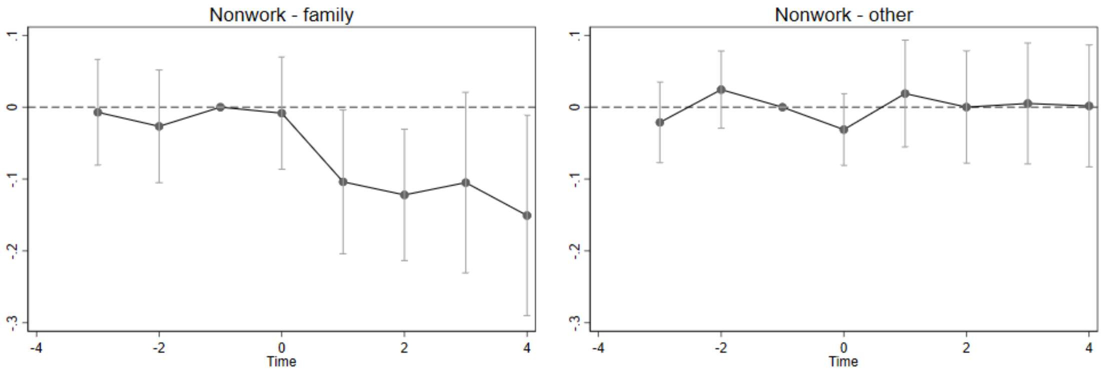
	(1)	(2)	(3)	(4)	(5)	(6)
	Single mothers		Married mothers		Married fathers	
	Paid	Amt	Paid	Amt	Paid	Amt
Sample: High school or less						
Any care arrangement	0.112	15.56**	-0.0214	1.947	-0.0350	6.955
	(0.0987)	(6.986)	(0.0679)	(8.475)	(0.0687)	(7.666)
Formal care	0.183*	16.58***	0.0166	1.196	0.0175	6.586
	(0.0924)	(5.987)	(0.0466)	(3.977)	(0.0560)	(5.475)
Relative care	-0.0530	-2.351	-0.0501	-3.780	-0.0412	-1.261
	(0.0464)	(4.351)	(0.0330)	(2.968)	(0.0298)	(2.537)
Observations	23402	23402	45371	45371	50646	50646
Sample: BA+						
Any care arrangement	-0.482	-13.50	0.0641	-1.648	0.00653	-0.482
	(0.322)	(23.74)	(0.0883)	(7.520)	(0.0867)	(7.653)
Formal care	-0.574**	-2.287	0.0825	-0.436	0.0906	3.157
	(0.247)	(15.42)	(0.0703)	(6.046)	(0.0637)	(6.580)
Relative care	0.0782	-2.728	-0.0197	0.665	-0.0148	1.443
	(0.0810)	(4.765)	(0.0248)	(2.639)	(0.0268)	(2.086)
Observations	5025	5025	46140	46140	43767	43767

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table shows selected estimated effects of the log minimum wage from panel models estimated on individuals with high school or less with one or more children aged 12 or younger. In columns 1, 3 and 5, the dependent variable is an indicator variable equal to one if the family paid any money for the arrangement, in columns 2, 4 and 6 the dependent variable is the total weekly amount spent on the arrangement. "Formal care" includes childcare centers and family day care. Models include controls for state characteristics, demographics, state and year fixed effects. Standard errors clustered at the state level. Source: SIPP.

Figure 7: Nonparticipation by stated reason, single mothers, youngest child age 0-5



Notes: Figure plots selected coefficients from the event study model in equation (3) with 95% confidence intervals. All models estimated on single mothers with high school or less education, with one or more children age 5 or younger. Standard errors clustered at the state level.

mothers stay out of the labor force for other reasons.

Appendix Figure B7 presents additional models assessing the robustness of these findings. We find that the estimated effects on non-participation are robust to excluding higher wage single mothers from the sample. Moreover, placebo models find no corresponding reductions in the probability that college-educated single mothers stay out of the labor force for family reasons.

5 Discussion

Both our baseline and alternative models find an own-wage employment elasticity of around 1.1 for single mothers with children 0-5, much larger than the typical own-wage elasticities estimated in the minimum wage literature (Dube 2019*a*). However, Wursten & Reich (2021) estimate own-wage elasticities for black workers of a similar magnitude. They further find that minimum wages help black workers overcome their larger fixed costs of commuting. Both studies thus find that modest minimum wage increases generate large positive employment effects when non-convexities (fixed costs) are present.

Our estimated own-wage employment elasticity is larger than the typical participation elasticities estimated in the EITC literature (Hotz & Scholz 2003). It is also greater than the EITC employment elasticities estimated by Michelmore & Pilkauskas (2021) for single mothers with children 5 and under. The differences between our estimates and those in the EITC literature could reflect institutional differences between the EITC and the minimum wage. Although both policies raise after-tax wages of low wage workers, the EITC is paid as an annual lump-sum with a lag. Consequently, in the presence of imperfect borrowing a higher EITC would have less impact on overcoming the fixed costs of work. The EITC phases out at higher earnings; with eligibility for the credit determined based on household income, the phase-out gives rise to potential negative effects on the intensive margin (though such effects are difficult to identify empirically) and negative participation effects among secondary earners. The differences could also reflect differing periods. The data we use in this paper cover a period of 37 years from 1982 to 2019, but the positive employment effects occur almost entirely in the second half of this period, as we show in Section 3.4.

The different effects by sample period also explains differences between our findings and existing literature. Our findings contrast strongly with Sabia (2008), who reports significant negative effects on single mother employment. Sabia's paper and our own both use the CPS. However, the two papers differ regarding model specification (we use distributional difference-in-differences and event study methods, Sabia uses a twoway fixed effects model), sample period (Sabia uses 1999-2004 only, we retain 1982-2019), sample selection criteria (we focus on mothers of children age 0-5, while Sabia excludes high school graduates as well as women who are not head of household), functional form (Sabia's preferred specification includes state quadratic time trends) and data quantity (the Outgoing Rotation Group data we use is much larger than the ASEC subsamples used in Sabia 2008). [29]

The positive effect of minimum wages on parental employment has implications for child poverty: as parents are induced to enter the labor force, child poverty rates decline. Reducing child poverty is an important policy objective in itself as it is a strong predictor of negative outcomes later in life, suggesting further positive downstream effects.

While the minimum wage unambiguously reduces child poverty, the effects on child welfare are less clear cut. Effects on disposable income will be smaller than our estimates

[29] In Appendix Table B4, we illustrate how panel difference-in-differences estimates (variations on Equation 8) change when we adopt the choices made in Sabia (2008). In our baseline sample, this specification yields statistically significant positive employment effects. In specifications and sample selection criteria more similar to Sabia's preferred sample, we do find negative point estimates. Overall, our analysis finds that this negative effect is not robust to changing functional form and sample restriction, indicating that the negative relationship uncovered by Sabia may not hold more generally.

suggest, if mothers spend some of their increased earnings on paid childcare. Moreover, as parents enter the workforce and/or work longer hours, higher minimum wages may induce them to spend less time with their children. The net effect on the children will then depend on the quality of the parenting time and the quality of the children's alternative care arrangements. Our data does not include information on child outcomes. However a number of existing papers examine how the EITC's impact on increasing maternal labor force participation affects child outcomes. The evidence on the net effect of this policy on child outcomes is mixed. While some studies find positive impacts on test scores and health (Dahl & Lochner 2012, Braga et al. 2019), Agostinelli & Sorrenti (2018) find that the substitution effect (less time with children) dominates the income effect for low wage workers. Løken et al. (2018), studying a work-encouraging welfare reform targeting single mothers in Norway, similarly find significant negative effects on children's test scores when their mothers were incentivized to increase their labor supply.

6 Conclusions

In this paper, we estimate effects of the minimum wage on parental employment. For single mothers with children ages 0-5, we document statistically significant, economically meaningful increases in employment with an employment elasticity with regard to the minimum wage of 0.21 and own-wage employment elasticities around 1.1. We find corresponding reductions in poverty, and a significant reduction in the share of mothers reporting that they stay out of the labor force to care for children and family. Auxiliary analyses of the SIPP data document significant increases in the use of paid childcare. Overall, our findings point to childcare costs as a significant barrier to employment for many low wage single mothers.

References

Agan, A. & Makowsky, M. (2018), The minimum wage, eitc, and criminal recidivism, Working Paper 25116, National Bureau of Economic Research.

Agostinelli, F. & Sorrenti, G. (2018), 'Money vs. time: family income, maternal labor supply, and child development', *University of Zurich, Department of Economics, Working Paper* (273).

Angrist, J. D. & Evans, W. N. (1998), 'Children and their parents' labor supply: Evidence from exogenous variation in family size', *The American Economic Review* **88**(3), 450–477.

Ashenfelter, O., Heckman, J. et al. (1974), 'The estimation of income and substitution effects in a model of family labor supply', *Econometrica* **42**(1), 73–85.

Bailey, M. J. & Goodman-Bacon, A. (2015), 'The war on poverty's experiment in public medicine: Community health centers and the mortality of older americans', *American Economic Review* **105**(3), 1067–1104.

Blau, D. & Currie, J. (2006), 'Pre-school, day care, and after-school care: who's minding the kids?', *Handbook of the Economics of Education* **2**, 1163–1278.

Blundell, R., French, E. & Tetlow, G. (2016), Retirement incentives and labor supply, *in* 'Handbook of the economics of population aging', Vol. 1, Elsevier, pp. 457–566.

Blundell, R. & MaCurdy, T. (1999), Labor supply: A review of alternative approaches, *in* 'Handbook of labor economics', Vol. 3, Elsevier, pp. 1559–1695.

Borgschulte, M. & Cho, H. (2018), Minimum wages and retirement, Discussion paper, Institute for the Study of Labor (IZA).

Borusyak, K. & Jaravel, X. (2017), 'Revisiting event study designs', *Available at SSRN 2826228* .

Braga, B., Blavin, F. & Gangopadhyaya, A. (2019), The long-term effects of childhood exposure to the earned income tax credit on health outcomes, Discussion paper, Institute for the Study of Labor (IZA).

Bullinger, L. R. (2017), 'The effect of minimum wages on adolescent fertility: a nationwide analysis', *American journal of public health* **107**(3), 447–452.

Callaway, B. & Sant'Anna, P. H. (2019), 'Difference-in-differences with multiple time periods', *Available at SSRN 3148250* .

Cascio, E. U. (2009), 'Maternal labor supply and the introduction of kindergartens into American public schools', *Journal of Human resources* **44**(1), 140–170.

Cengiz, D., Dube, A., Lindner, A. & Zipperer, B. (2019), 'The Effect of Minimum Wages on Low-Wage Jobs', *The Quarterly Journal of Economics* **134**(3), 1405–1454.
URL: *https://doi.org/10.1093/qje/qjz014*

Chetty, R., Friedman, J. N. & Saez, E. (2013), 'Using differences in knowledge across neighborhoods to uncover the impacts of the eitc on earnings', *American Economic Review* **103**(7), 2683–2721.

Cogan, J. F. (1981), 'Fixed costs and labor supply', *Econometrica: Journal of the Econometric Society* pp. 945–963.

Dahl, G. B. & Lochner, L. (2012), 'The impact of family income on child achievement: Evidence from the earned income tax credit', *American Economic Review* **102**(5), 1927–56.

Dube, A. (2019*a*), Impacts of minimum wages: review of the international evidence, Independent report PU2287, HM Treasury.
URL: *https://www.gov.uk/government/publications/impacts-of-minimum-wages-review-of-the-international-evidence*

Dube, A. (2019*b*), 'Minimum wages and the distribution of family incomes', *American Economic Journal: Applied Economics* **11**(4), 268–304.

Eissa, N., Kleven, H. J. & Kreiner, C. T. (2008), 'Evaluation of four tax reforms in the united states: Labor supply and welfare effects for single mothers', *Journal of Public Economics* **92**(3-4), 795–816.

Eissa, N. & Liebman, J. B. (1996), 'Labor supply response to the earned income tax credit', *The Quarterly Journal of Economics* **111**(2), 605–637.

Fitzpatrick, M. D. (2010), 'Preschoolers enrolled and mothers at work? the effects of universal prekindergarten', *Journal of Labor Economics* **28**(1), 51–85.

Fitzpatrick, M. D. (2012), 'Revising our thinking about the relationship between maternal labor supply and preschool', *Journal of Human Resources* **47**(3), 583–612.

Flood, S., King, M., Ruggles, S. & Warren, R. J. (2017), Integrated public use microdata series, current population survey: Version 5.0, Dataset, Minneapolis: University of Minnesota.

Gelbach, J. B. (2002), 'Public schooling for young children and maternal labor supply', *American Economic Review* **92**(1), 307–322.

Havnes, T. & Mogstad, M. (2011), 'Money for nothing? universal child care and maternal employment', *Journal of Public Economics* **95**(11-12), 1455–1465.

Heim, B. T. & Meyer, B. D. (2004), 'Work costs and nonconvex preferences in the estimation of labor supply models', *Journal of Public Economics* **88**(11), 2323–2338.

Hotz, V. J. & Scholz, J. K. (2003), The earned income tax credit, *in* 'Means-tested transfer programs in the United States', University of Chicago press, pp. 141–198.

Kleven, H. (2019), The eitc and the extensive margin: A reappraisal, Working Paper 26405, National Bureau of Economic Research.

Knop, B. & Mohanty, A. (2018), 'Child Care Costs in the Redesigned Survey of Income and Program Participation (SIPP): A Comparison to the Current Population Survey Annual Social and Economic Supplement (ASEC)', Working paper, SEHSD.

Løken, K. V., Lommerud, K. E. & Reiso, K. H. (2018), 'Single mothers and their children: Evaluating a work-encouraging welfare reform', *Journal of Public Economics* **167**, 1–20.

Meyer, B. D. & Rosenbaum, D. T. (2001), 'Welfare, the earned income tax credit, and the labor supply of single mothers', *The Quarterly Journal of Economics* **116**(3), 1063–1114.

Michelmore, K. & Pilkauskas, N. (2021), 'Tots and teens: How does child's age influence maternal labor supply and child care response to the earned income tax credit?', *Journal of Labor Economics* **39**(4), 895–929.
URL: *https://doi.org/10.1086/711383*

National Academies of Sciences, Engineering, and Medicine (2019), *A roadmap to reducing child poverty*, National Academies Press.

Page, M. E., Spetz, J. & Millar, J. (2005), 'Does the minimum wage affect welfare caseloads?', *Journal of Policy Analysis and Management* **24**(2), 273–295.

Sabia, J. J. (2008), 'Minimum wages and the economic well-being of single mothers', *Journal of Policy Analysis and Management* **27**(4), 848–866.

Schmidheiny, K. & Siegloch, S. (2019), On event study designs and distributed-lag models: Equivalence, generalization and practical implications, Discussion Paper 13477, CEPR.

Sun, L. & Abraham, S. (2020), 'Estimating dynamic treatment effects in event studies with heterogeneous treatment effects', *Journal of Econometrics* .
URL: *https://www.sciencedirect.com/science/article/pii/S030440762030378X*

Thomason, S., Austin, L., Bernahrdt, A., Dresser, L., Jacobs, K. & Whitebook, M. (2018), 'At the Wage Floor: Covering Homecare and Early Care and Education Workers in the New Generation of Minimum Wage Laws', Report, Center for Labor Research and Education.

Vaghul, K. & Zipperer, B. (2019), 'Historical state and sub-state minimum wage data', *Washington Center for Equitable Growth* .

Winkle, J. & Wilson, R. (2018), The 1990s Head Start Expansion and the Employment Decisions of Single Mothers, Working paper.

Wursten, J. & Reich, M. (2021), Racial inequality and minimum wages in frictional labor markets, Working paper, Institute for Research on Labor and Employment.

Online Appendix to Parental Labor Supply: Evidence from Minimum Wage Changes

Anna Godøy
Michael Reich
Jesse Wursten
Sylvia Allegretto

Appendix A Childcare costs and means-tested transfers

As noted in the text, standard simple labor supply models predict that fixed costs of employment can make it unprofitable to be employed in jobs that offer few hours. Higher wages could then generate substantial positive participation effects. The literature is ambiguous on the exact nature of these costs: the costs could encompass non-monetary costs, such as the stress of preparing for work. In this appendix, we examine two particular sources of fixed costs of work, namely childcare and means-tested transfer payments.

Childcare First, we examine the extent to which childcare costs represent a fixed cost of work for mothers: That is, are childcare costs concave in hours worked? Second, we analyze how this pattern differs between single and married mothers.

To examine childcare costs, we use data from the 2010-2020 Annual Social and Economic Supplement (ASEC) of the Current Population Survey (the "March CPS"), covering the years 2009-2019. This data includes information on annual childcare costs, as well as weekly hours worked and weeks worked per year. Using the CPS has the advantage that the hours measure - usual hours worked over the reference year - is identical to the one studied in our main analysis. However, estimated child care costs tend to be lower in the CPS compared to the Survey of Income and Program Participation (Knop & Mohanty 2018), meaning the figures we report here may actually underestimate the childcare costs that families face. We retain all women with at most a high school degree, who have at least one child younger than 6 and reported working between 1 and 49 hours per week. We further divide this sample into groups defined by weekly hours in five hour bins and by marital status. We then calculate measures of childcare utilization for each cell.

Figure A1 plots childcare and hours worked. Panel (a) shows the share of women who report paying for childcare. For both single and married mothers, the share that pays for childcare is increasing in hours worked, though the increase is less than proportional. Meanwhile, this pattern appears to vary with marital status. For jobs that are approximately full time (35-40 hours), married and single mothers are equally likely to pay for childcare. Among mothers working 15 hours per week or less, the figure reveals a significant discrepancy: the part-time married mothers are much less likely to pay for childcare compared to the part-time single mothers. A simple linear probability model of paying for childcare (controlling for age) shows that this difference is statistically significant.

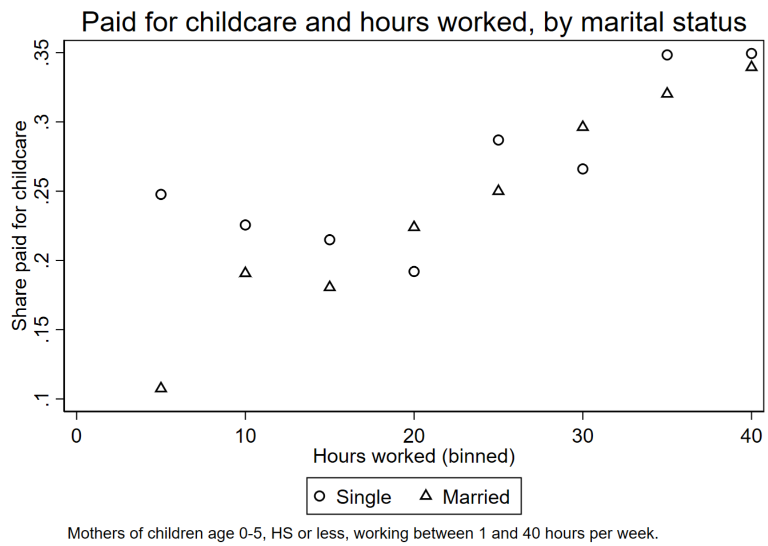
Panel (b) shows the childcare costs and hours worked including only women with positive childcare expenses. This figure illustrates that conditional on paying, childcare costs are relatively high for both single and married parents. A similar pattern emerges: while childcare costs are increasing in hours work, the increase is less than proportional.

To summarize, the fixed cost element of childcare costs appears to be substantial for many women, in particular for single mothers. This makes it unprofitable to work very few hours. In this context, our simple labor supply model predicts potentially large positive labor supply effects for single mothers.

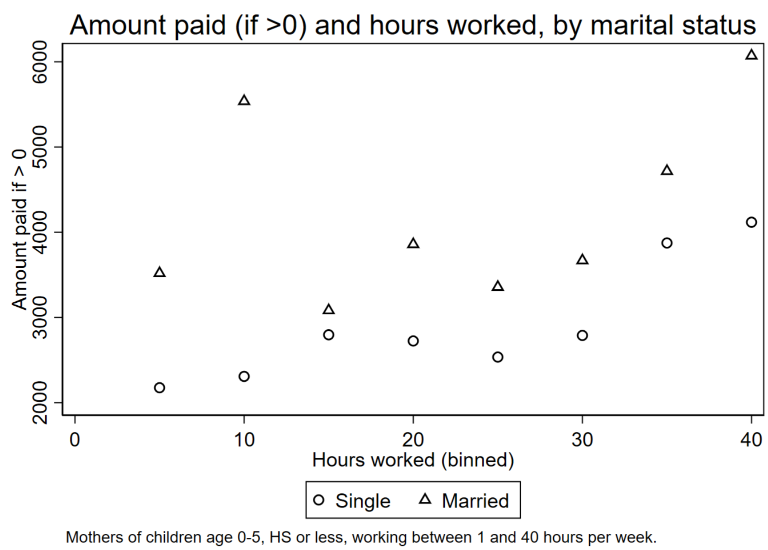
Means-tested transfers In addition to childcare costs, means-tested transfers could also play a role in shaping families' labor supply responses to higher wages. Figure A2 uses ASEC data to illustrates the relationship between earnings and program participation. The figure plots average participation of parents of minor children with high school or less for three key means-tested programs: welfare or public assistance, food stamps/SNAP and government-provided health insurance/Medicaid. To construct this figure, we divide the sample into 20 approximately equally-sized bins based on real earnings. We then calculate average earnings and program participation rates in each bin. As the passage of the Personal Responsibility and Work Opportunity Act (PRWORA) in 1996 introduced major changes to the welfare system, we plot figures separately for the pre- and post PRWORA period.

The figure illustrates that a significant share of parents with zero or very low earnings receive some means-tested transfers. The relationship between income and program participation varies by type of program as well as by time period. In particular, public assistance and welfare tends to be concentrated among families with zero or very low earnings, while SNAP and food stamps reaches families further up in the earnings distribution. Overall, going from zero or very low earnings to working at the minimum wage produces cliff-like reductions in public assistance and welfare payments.

Figure A1: Childcare spending and hours worked



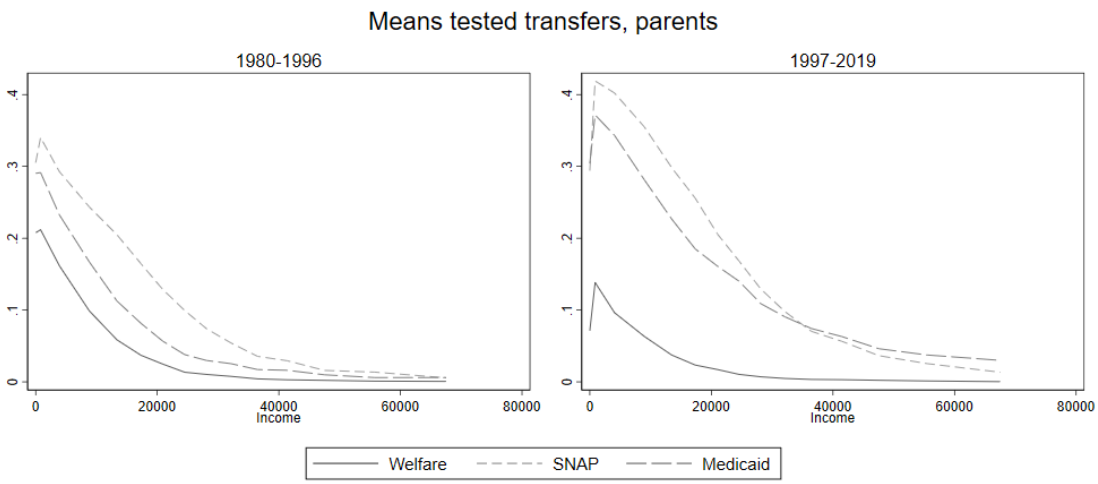
(a) Share paid for childcare



(b) Amount paid for childcare (if >0)

Notes: Figure plots childcare use and average childcare spending by average weekly hours worked in 5 hour bins. The sample includes women with at most a high school degree, working between 1 and 40 hours a week, who have at least one child younger than 6. The figures indicate childcare spending increases less than one to one with hours worked, in particular for unmarried mothers. Source: CPS ASEC

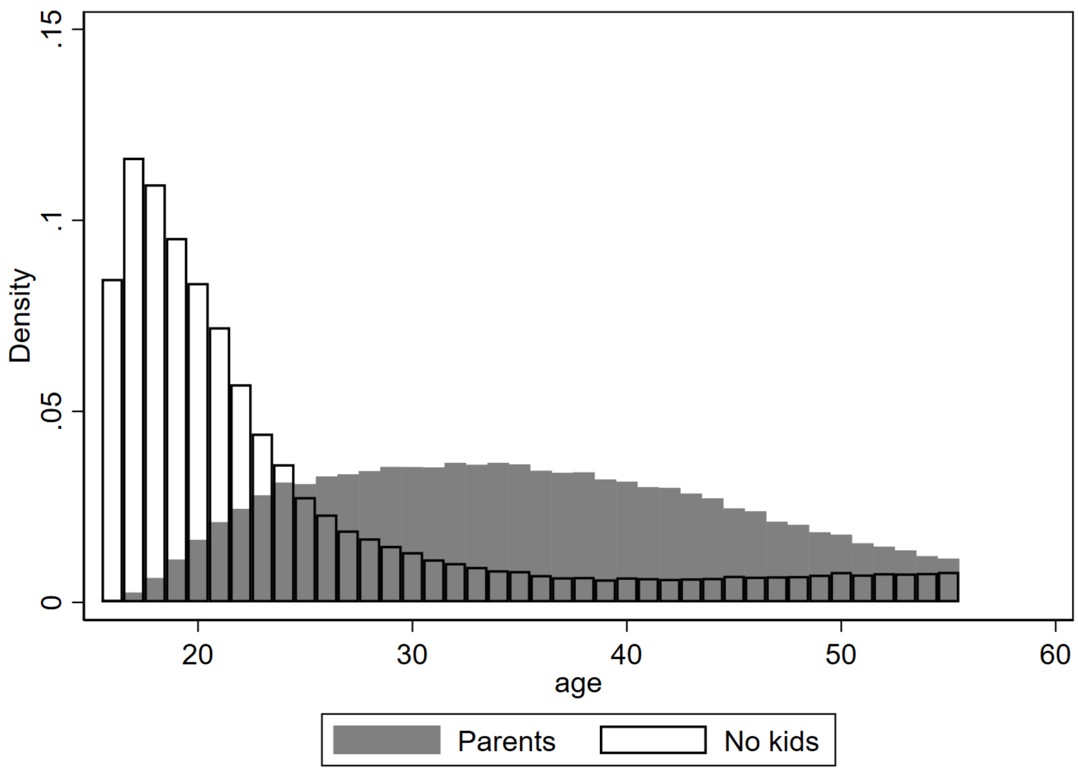
Figure A2: Means-tested transfers, parents



Notes: Figure plots program participation rates for parents of minor children, with high school or less, by ventile of family earnings distribution (adjusted for inflation).

Appendix B Supplementary tables and figures

Figure B1: Age distribution, minimum wage workers, by presence of children



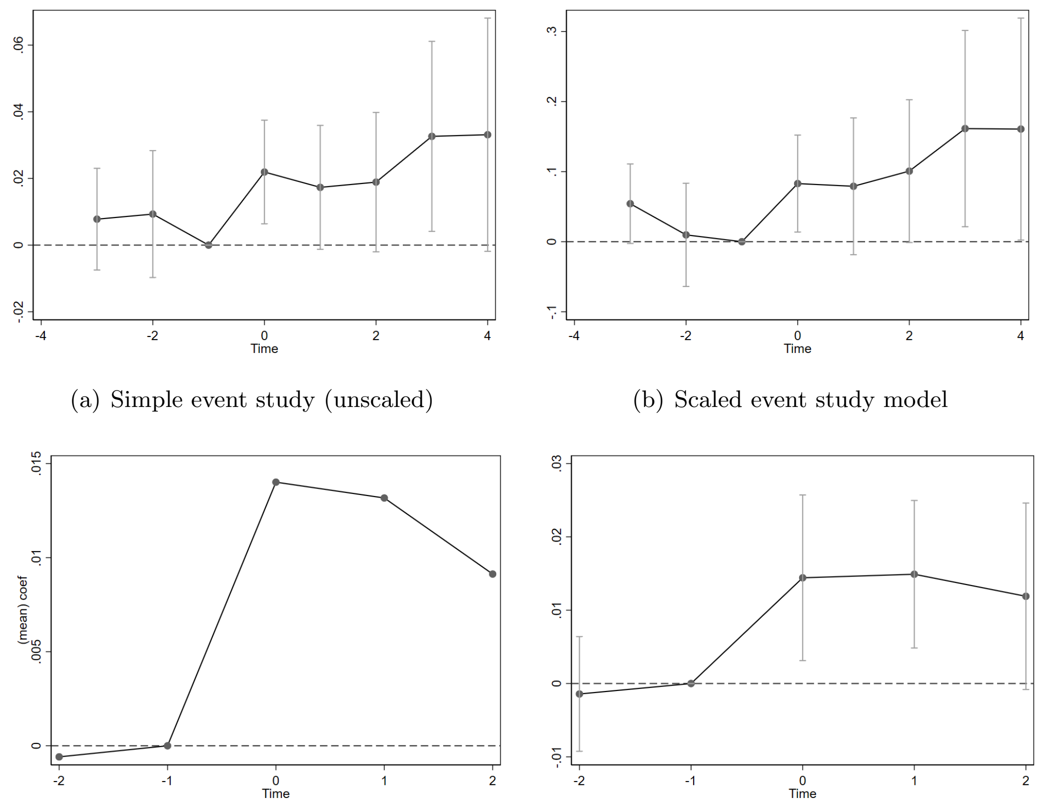
Note: Figure plots the estimated age frequency distribution of minimum wage workers, by presence of co-resident children (estimates calculated using the CPS weights).

Table B1: Distributional difference-in-differences estimator. Summary table of major groups, 1982-2019. Full Time Equivalents (FTE).

Marital status	Any	Single woman			Married woman			Married man		
Parental status	Any	Age 0-5	Age 6-17	No Kids	Age 0-5	Age 6-17	No Kids	Age 0-5	Age 6-17	No Kids
1. Δ EPOP above MW: Δa	0.01***	0.04***	0.02**	0.02**	0.01**	0.01	0.01	0.00*	0.00*	0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
2. Δ EPOP below MW: Δb	-0.01***	-0.02***	-0.01	-0.02***	-0.01**	-0.01**	-0.01***	-0.00	-0.00	-0.00**
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
3. Δ EPOP around MW: Δe	0.00	0.02*	0.00	-0.00	-0.00	0.00	0.00	0.00	0.00	-0.00
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
4. $\%\Delta$ affected employment	0.01	0.15*	0.07	-0.02	-0.03	0.07	0.07	0.06	0.05	-0.01
	(0.03)	(0.08)	(0.11)	(0.03)	(0.06)	(0.11)	(0.14)	(0.07)	(0.10)	(0.11)
5. Employment elasticity w.r.t. MW	0.01	0.19*	0.05	-0.02	-0.02	0.04	0.03	0.02	0.01	-0.00
	(0.02)	(0.10)	(0.08)	(0.04)	(0.04)	(0.06)	(0.06)	(0.02)	(0.02)	(0.03)
6. $\%\Delta$ affected wages	0.06***	0.13***	0.04	0.05**	0.07**	0.05	0.08	0.07**	0.07**	0.07
	(0.01)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)	(0.05)
7. Emp. elasticity w.r.t. affected wage	0.22	1.19***	1.57	-0.42	-0.39	1.62	0.86	0.93	0.70	-0.21
	(0.48)	(0.39)	(2.03)	(0.70)	(0.93)	(1.19)	(1.17)	(0.82)	(1.34)	(1.59)
Number of events	152	152	152	152	152	152	152	152	152	152
Number of bins	914736	913084	914736	914736	914736	914736	914736	914736	914736	914736
Number of workers	4603241	99714	165501	709018	301580	423178	373302	448284	459955	361563

Note: Estimates derived from the distributional difference-in-differences specification from equation (1). See Section 3 for details on the calculations.

Figure B2: Employment, single mothers with young children excluding workers with $w > 15$, alternative specifications



(a) Simple event study (unscaled)

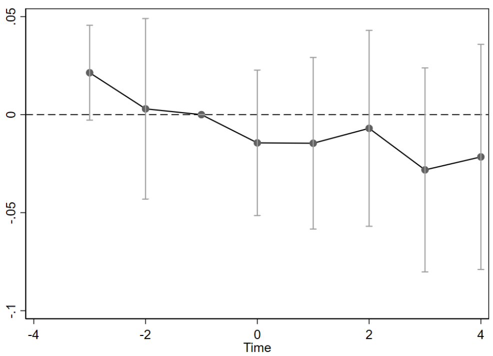
(b) Scaled event study model

(c) Event-by-event, averaged

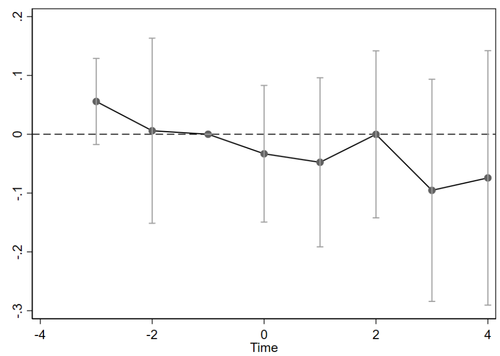
(d) Stacked events sample

Notes: Figure plots selected coefficients from event study models with 95% confidence intervals. All models estimated on single mothers with high school or less education, who have at least one child age 5 or younger. Women earning \$15/hour or more are excluded from the sample. Standard errors clustered at the state level.

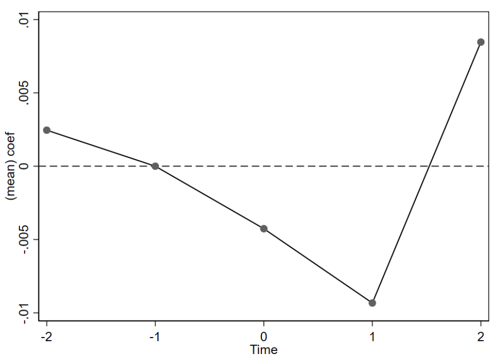
Figure B3: Employment, college educated single mothers with young children, alternative specifications



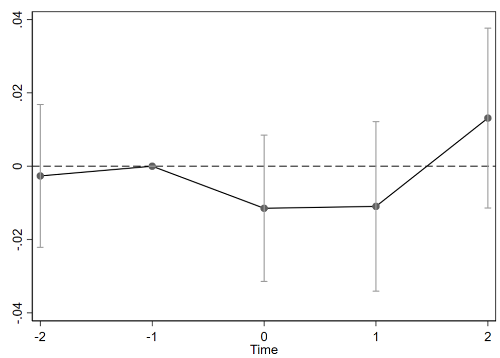
(a) Simple event study (unscaled)



(b) Scaled event study model



(c) Event-by-event, averaged



(d) Stacked events sample

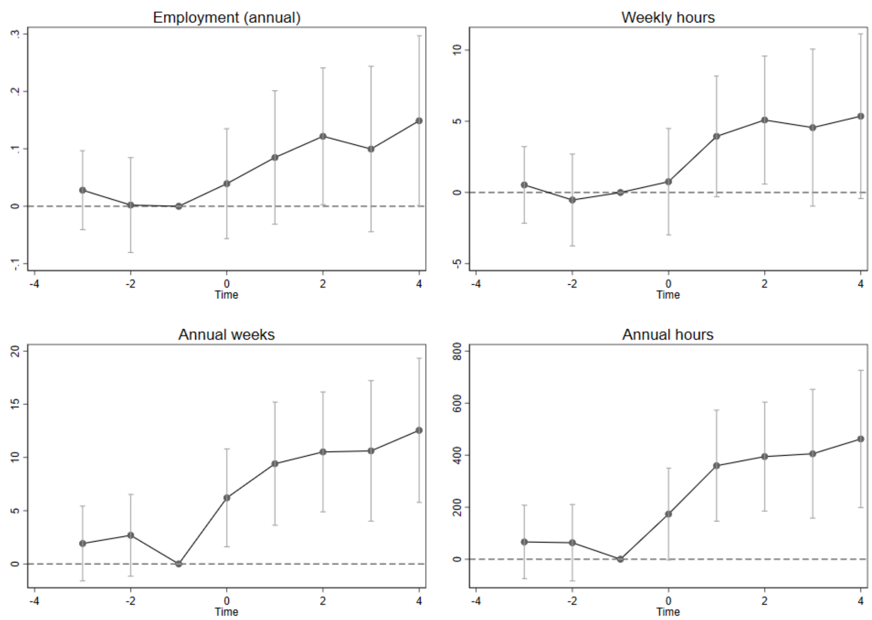
Notes: Figure plots selected coefficients from event study models with 95% confidence intervals. All models estimated on single mothers with a bachelor's degree or higher education, who have at least one child age 5 or younger. Standard errors clustered at the state level.

Table B2: Employment, single mothers, IV estimates from event study model

	(1) HS or less	(2) HS or less, $w < 15$	(3) BA or higher
Log min wage	0.195**	0.186**	-0.127
	(0.0767)	(0.0868)	(0.133)
F excluded instruments	67.79	66.03	82.99
Sample mean	0.471	0.426	0.838
N	2213	2213	2191

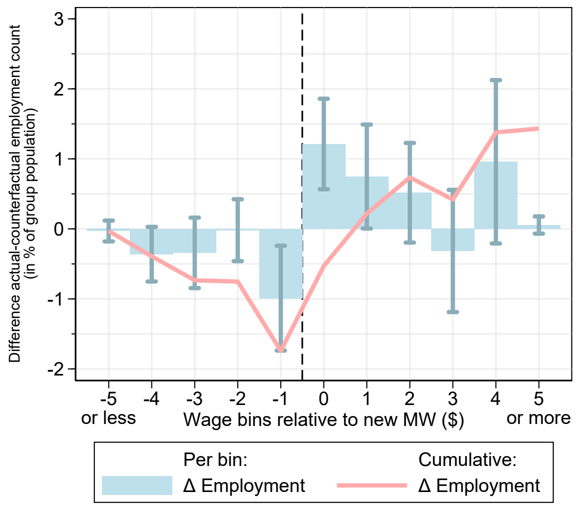
Notes: Table shows estimates from Equation 4, summarizing the scaled event study model in Equation 3. All models include event and year fixed effects and are estimated on the sample of single mothers with children age 0-5 with high school or less. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

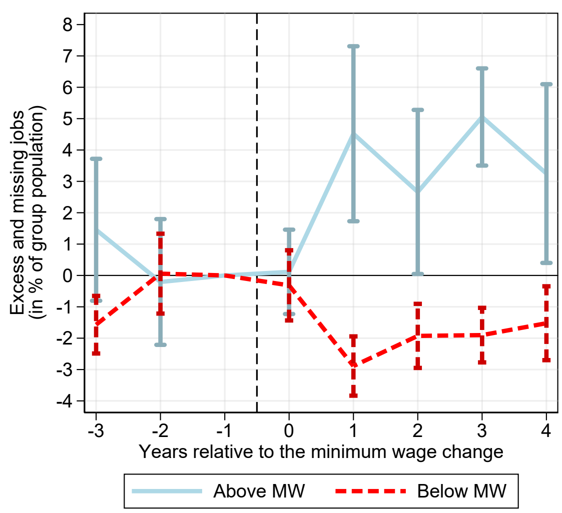
Figure B4: Employment, hours and weeks worked, single mothers, youngest child age 0-5



Notes: Figure plots selected coefficients from event study models with 95% confidence intervals. Models estimated on single mothers with at least one child age 0-5, and high school or less education. Standard errors clustered at the state level.

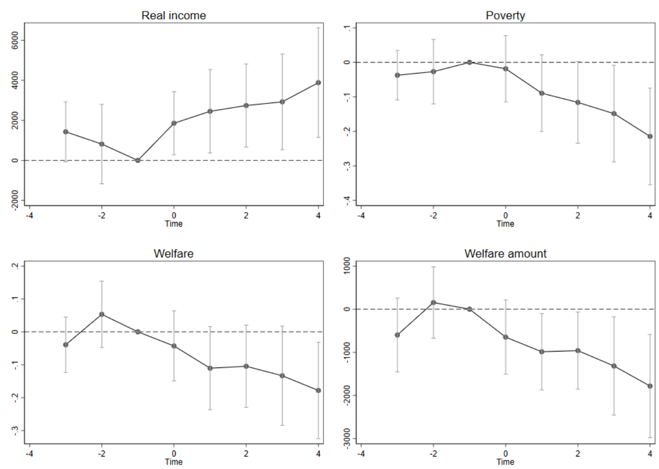
Figure B5: Distributional difference-in-differences estimator, effects over time and relative wage distribution. Single mothers with youngest child aged 0-5, reweighted. 1982-2019.





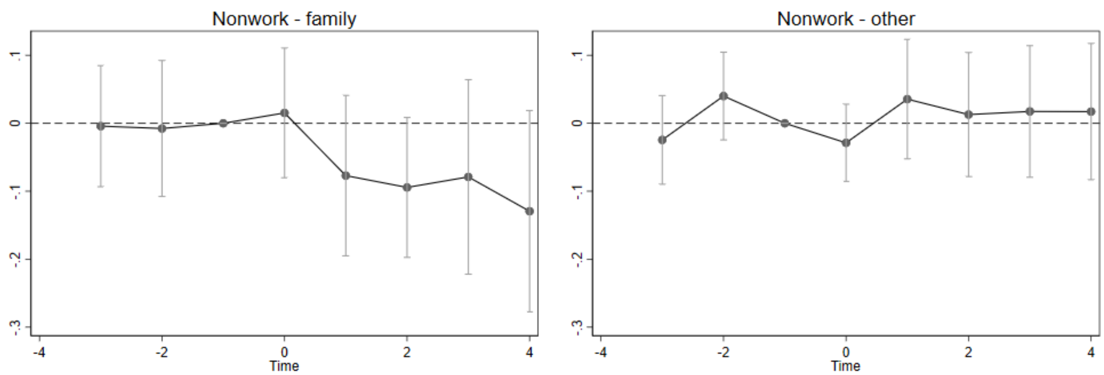
Notes: Figure plots estimated effects on employment to population ratios with 95% confidence intervals. Estimates derived from the distributional difference-in-differences specification from equation (1). See Section 3 for details on the calculations. To account for compositional changes over time, observations have been reweighted so that pre-determined characteristics (age, education, Hispanic origin) in each state-year are similar to the sample average: let g index demographic groups defined by age (1-year intervals), education (high school graduates vs. less than high school) and ethnicity (Hispanic/non-Hispanic). Each observation is then reweighted using the ratio of the share of demographic group g in state s year t to the average share of demographic group g across all state-years in the estimation sample: $\tilde{w}_i = \frac{\bar{s}_g}{s_{gst}} w_i$ Where w_i denote the CPS sample weights. Standard errors clustered at the state level.

Figure B6: Income and poverty, single mothers of young children, excluding high wage workers

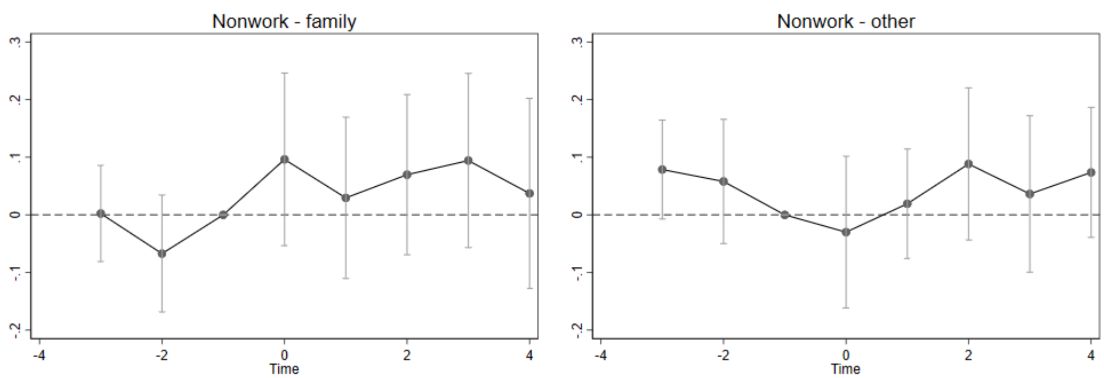


Notes: Figure plots selected coefficients from the event study model in equation (3) with 95% confidence intervals. All models estimated on single mothers with high school or less education, excluding workers earning $15/hour or higher wages, who have at least one child age 5 or younger. Standard errors clustered at the state level.

Figure B7: Nonparticipation by stated reason, single mothers, youngest child age 0-5



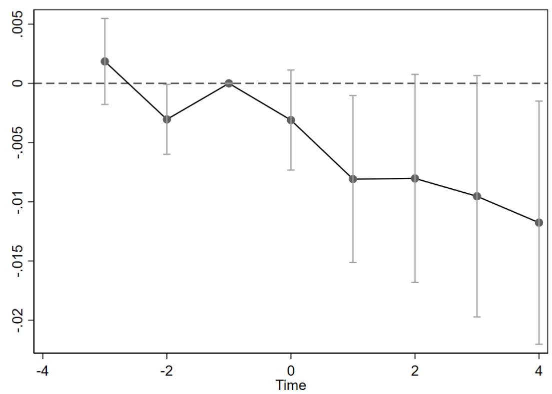
(a) HS or less, excluding workers earning $w > 15$



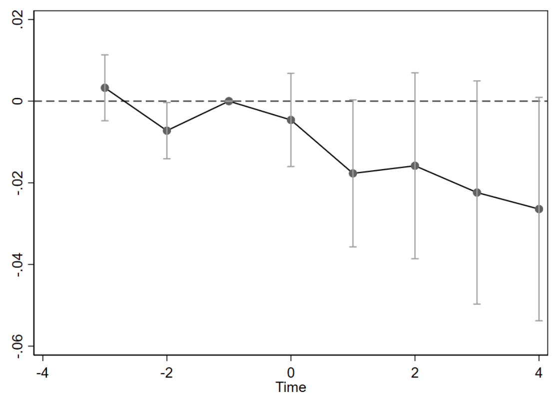
(b) BA+

Notes: Figure plots selected coefficients from the event study model in equation (3) with 95% confidence intervals. All models estimated on single mothers who have at least one child age 5 or younger. Standard errors clustered at the state level.

Figure B8: Event study models of sample selection



(a) Full sample ages 15-55



(b) Excluding $15+

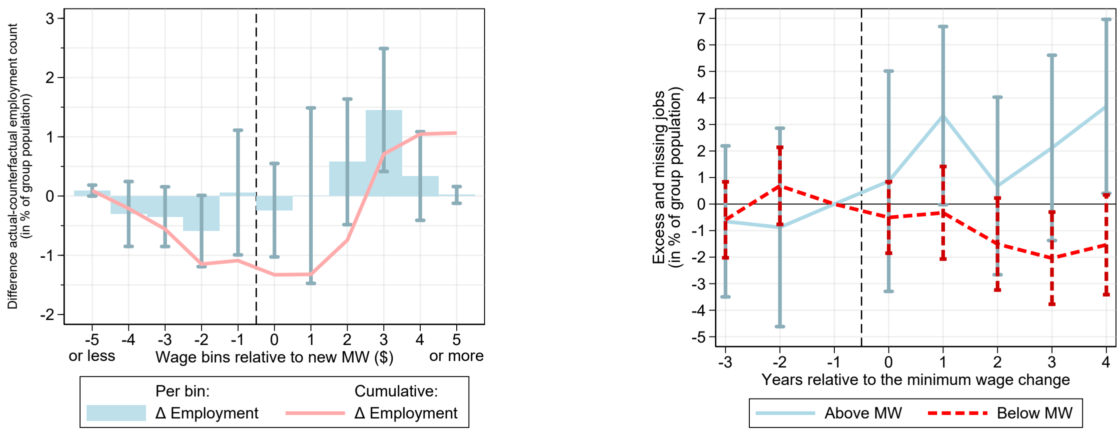
Notes: Figure plots selected coefficients from event study models with 95% confidence intervals. Dependent variable is an indicator variable equal to 1 if the person is a single mother of preschool age children. Models estimated on either the sample of all women (panel a), or excluding workers earning $15/hour or more (panel b). Models control for event and year fixed effects. Standard errors clustered at the state level.

Table B3: Distributional difference-in-differences estimator. Summary table by demographic group, 1982-2019, all single mothers whose youngest child is less than six years old.

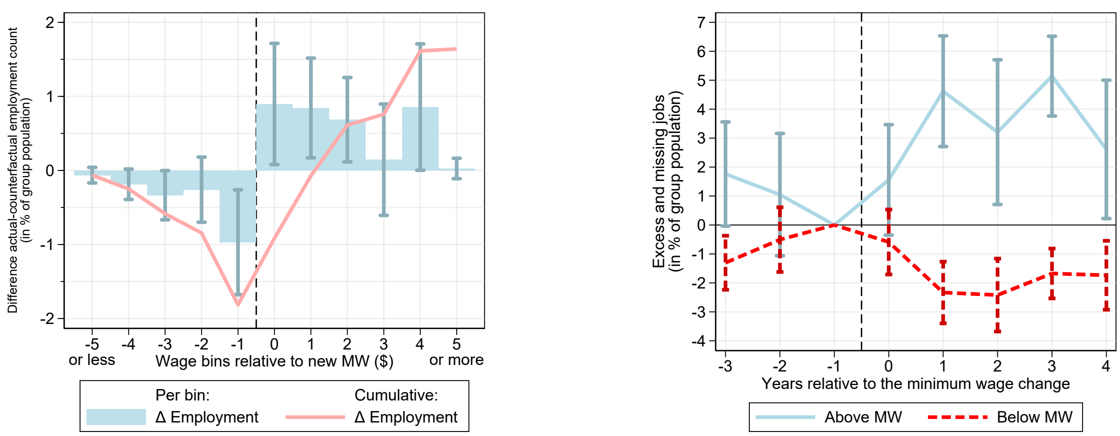
		Race				Education		Age of parent		
Demographic group	Any	White	Black	Hispanic	B or H	LTHS	HSOL	16-24	25-34	35-44
1. Δ EPOP above MW: Δa	0.05***	0.03*	0.02	0.09***	0.05**	0.06**	0.04***	0.08***	0.04***	0.03
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
2. Δ EPOP below MW: Δb	-0.02***	-0.03***	-0.00	-0.04	-0.02	-0.02	-0.03***	-0.03*	-0.02***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)	(0.03)	(0.01)	(0.02)	(0.01)	(0.01)
3. Δ EPOP around MW: Δe	0.02**	0.00	0.02	0.05**	0.04***	0.04	0.01	0.05**	0.01	0.00
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)	(0.01)	(0.03)
4. %Δ affected employment	0.14**	0.02	0.17	0.20**	0.22***	0.13	0.05	0.23**	0.10	0.01
	(0.06)	(0.10)	(0.18)	(0.09)	(0.08)	(0.09)	(0.06)	(0.11)	(0.10)	(0.25)
5. Employment elasticity w.r.t. MW	0.21**	0.02	0.19	0.43**	0.36***	0.36	0.10	0.50**	0.13	0.01
	(0.09)	(0.14)	(0.21)	(0.20)	(0.13)	(0.25)	(0.13)	(0.23)	(0.14)	(0.25)
6. %Δ affected wages	0.13***	0.11*	0.07	0.16***	0.11***	0.11***	0.10***	0.12***	0.12***	0.19**
	(0.02)	(0.06)	(0.06)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.08)
7. Emp. elasticity w.r.t. affected wage	1.12***	0.14	2.46*	1.28*	1.95***	1.21	0.48	1.87***	0.81	0.03
	(0.36)	(0.82)	(1.47)	(0.64)	(0.61)	(0.75)	(0.55)	(0.60)	(0.69)	(1.31)
Number of events	152	152	152	152	152	152	152	152	152	152
Number of bins	913084	895738	608290	395654	720980	716732	898216	826472	900340	725818
Number of workers	99714	56770	25343	11831	37174	23855	54354	28589	51816	17521

Note: Estimates derived from the distributional difference-in-differences specification from equation (1). The *B or H* column collects black and Hispanic workers. See Section 3 for details on the calculations.

Figure B9: Distributional difference-in-differences estimator. Figures by time period. All single mothers whose youngest child is less than six years old.



(a) 1982-1996



(b) 1997-2019

Notes: Figure plots estimated effects on employment to population ratios with 95% confidence intervals. Estimates derived from the distributional difference-in-differences specification from equation (1). Model estimated on the sample of single mothers with one or more children age 5 or younger.

Table B4: Employment effects, single mothers, comparison with Sabia (2008)

	(1) TWFE	(2) LT	(3) QT
(0) Youngest age 0-5	0.168***	0.0755*	0.0600*
	(0.0435)	(0.0394)	(0.0325)
(1) Youngest age 0-17	0.133***	0.0314	0.0292
	(0.0344)	(0.0353)	(0.0345)
(2) Less than HS only	0.153**	0.0260	0.0108
	(0.0691)	(0.0783)	(0.0839)
(3) 1991-2004 only	0.0987*	-0.173	-0.114
	(0.0492)	(0.108)	(0.105)
(4) Head of household only	0.166***	0.0547	0.0390
	(0.0445)	(0.0462)	(0.0482)
(4) Sabia (2007) sample	-0.00284	-0.226	-0.0756
	(0.118)	(0.162)	(0.133)
State trend	None	Linear	Quadratic

Notes: Table shows selected estimates from panel models estimated on single mothers, aged 15-55, with high school or less (unless otherwise noted). The dependent variable is an indicator equal to one for individuals who reported working at some time during the year. Models include controls for state characteristics (log state population, AFDC/TANF and SNAP benefit levels, TANF implementation, major AFDC waiver), demographics (age, education, race and ethnicity, marital status, number of children, number of children interacted with calendar year), state and year fixed effects. "Sabia (2008) sample" incorporates all restrictions (1) - (3), retaining single mothers with no high school degree, using the years 1991 to 2004 only, excluding women who were not head of household. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Source: CPS ASEC.

Appendix C Analysis of Childcare Utilization in the SIPP

We use the NBER extracts of the Survey of Income and Program Participation (SIPP). Our analysis uses data from the 1996 – 2008 panels. The childcare data in earlier and later rounds of the SIPP are not comparable because of survey redesigns. In the sample, we retain all parents of children aged 12 or younger. To ensure comparability with our CPS sample, we exclude parents aged older than 55 and younger than 15.

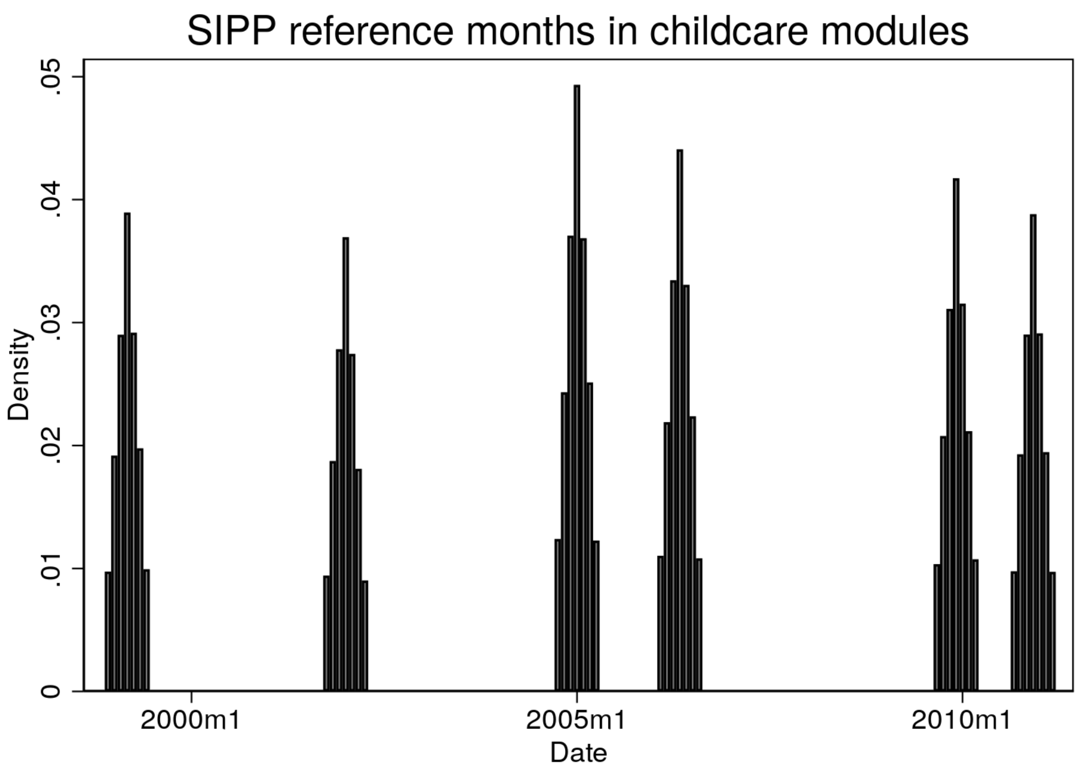
Unfortunately, childcare questions are not asked in every survey wave. In the years we use, questions on childcare use are asked only one to two times per survey panel. As a consequence, we only have six waves of data. Figure C1 plots the relative frequencies of each reference month: as the figure shows, we lack data for several years, and in some years we have very few observations. These shortcomings make it difficult to estimate event study models, which rely on high frequency data; moreover, estimates will be less precise than if we had data for all waves.

The administration of the topical module on childcare utilization complicates the estimation of our event study specification. For this reason, our analysis of the SIPP still focuses on the two-way fixed effects - log minimum wage regression model. We have estimated models of employment, full-time work, full-time work conditional on employment (capturing intensive margin effects) and AFDC/TANF receipt. We show the results in table C1. Overall, the results are estimated with low precision; however, the results are qualitatively consistent with our preferred estimates.

The SIPP contains detailed data on all regular childcare arrangements for each child. The reference parent (typically the mother) is asked about all child care arrangements used last month, together with costs for each type of arrangement in a typical week. These questions differ from the childcare information collected in recent years of the CPS; here parents are asked about the previous calendar year. This difference likely leads to undercounts of childcare expenditures and utilization in the CPS (Knop & Mohanty 2018).

In this analysis, we look at rates of overall childcare use, as well as use of formal care (daycare centers and family day care) and relative care (grandparents and other relatives). For each of these three categories, we construct indicator variables equal to one if anyone in the family paid for this type of care for one or more children, as well as the total weekly amount paid (inflation-adjusted to 2016 dollars). To be clear, we do not condition on employment status: nonworking parents are included in the sample. Table C2 shows sample averages for these variables for mothers and fathers with high school or less as well

Figure C1: SIPP reference months



Notes: Figure plots the distribution of reference months from the SIPP sample of childcare utilization, see text for details.

Table C1: Employment and welfare, SIPP

	(1)	(2)	(3)	(4)	(5)	(6)
	Single mothers		Married mothers		Married fathers	
Age youngest	0-12	0-15	0-12	0-15	0-12	0-15
Employment	0.116	0.255	0.113	0.0576	0.0317	0.0103
	(0.137)	(0.198)	(0.129)	(0.141)	(0.0436)	(0.0623)
Full time (all)	-0.0400	-0.0200	0.233*	0.0954	-0.0172	-0.0525
	(0.149)	(0.177)	(0.117)	(0.162)	(0.0787)	(0.122)
Full time (if work)	-0.222	-0.348	0.298**	0.107	-0.0445	-0.0698
	(0.152)	(0.210)	(0.142)	(0.242)	(0.0674)	(0.0969)
AFDC receipt	-0.367***	-0.641***	-0.0411	0.0213	0.000233	0.0163
	(0.0797)	(0.141)	(0.0263)	(0.0336)	(0.0340)	(0.0243)
Observations	23402	13629	45371	25054	50646	29023

Notes: Table shows selected estimated effects of the log minimum wage on employment and welfare receipt (outcomes in italics), from panel models estimated on individuals with high school or less with one or more children aged 5 or younger. Models include controls for state characteristics (log state population, AFDC/TANF and SNAP benefit levels, TANF implementation, major AFDC waiver), demographics (age, education, race and ethnicity, marital status, number of children, number of children interacted with calendar year), state and year fixed effects . Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Source: SIPP.

as for parents in the higher education placebo sample. Overall, rates of paid childcare are higher in the college educated sample, though less educated workers are more likely to pay for relative care.

Table C2: Summary statistics, SIPP sample

	(1)	(2)	(3)	(4)	(5)	(6)
		HS or less			BA+	
	Sing mom	Mar moms	Mar dads	Sing mom	Mar moms	Mar dads
Age 0-12						
Any CC pay	0.190	0.135	0.162	0.323	0.299	0.268
Any formal CC pay	0.092	0.064	0.081	0.212	0.173	0.149
Any rel CC pay	0.056	0.034	0.038	0.032	0.021	0.016
Amt CC pay	12.489	9.432	11.230	19.841	16.991	14.590
Amt formal CC pay	6.014	4.387	5.502	12.476	9.000	7.493
Amt rel CC pay	3.164	2.240	2.573	1.553	1.404	0.947
Observations	23402	45371	50646	5025	46140	43767
Age 0-5 only						
Any CC pay	0.241	0.189	0.225	0.541	0.419	0.385
Any formal CC pay	0.120	0.090	0.114	0.361	0.240	0.211
Any rel CC pay	0.071	0.046	0.051	0.050	0.030	0.024
Amt CC pay	15.652	13.758	16.130	34.313	23.461	20.621
Amt formal CC pay	7.370	6.425	7.943	20.824	12.089	10.085
Amt rel CC pay	4.252	3.130	3.632	2.915	2.041	1.453
Observations	13629	25054	29023	1904	28148	26018

Notes: Table shows summary statistics of parents reported paid childcare use and weekly childcare amounts. Source: SIPP.

Table C3 shows models estimated on the sample of parents of preschool age children. The results are similar to the estimates presented in table 4, although some effects are estimated with less precision. As before, we find a significant increase in single mothers' expenditures on formal childcare. Moreover, we now find a negative effect on expenditures on relative care in this sample.

Table C3: Childcare utilization - youngest age 0 - 5

	(1)	(2)	(3)	(4)	(5)	(6)
	Single mothers		Married mothers		Married fathers	
	Paid	Amt	Paid	Amt	Paid	Amt
Sample: High school or less						
Any care arrangement	-0.0750	4.269	-0.0798	-7.985	-0.0528	8.347
	(0.124)	(10.43)	(0.121)	(15.47)	(0.101)	(12.36)
Formal care	0.197	16.62**	0.0183	0.106	0.0525	13.42
	(0.129)	(7.901)	(0.0679)	(5.535)	(0.0895)	(8.624)
Relative care	-0.158**	-7.772	-0.0537	-5.465	-0.0631	-1.859
	(0.0626)	(7.551)	(0.0464)	(4.697)	(0.0403)	(4.313)
Observations	13629	13629	25054	25054	29023	29023
Sample: BA+						
Any care arrangement	-0.316	71.65	0.149	3.520	-0.0460	-6.064
	(0.606)	(50.04)	(0.135)	(12.37)	(0.119)	(10.27)
Formal care	-0.656	59.17	0.130	-0.788	0.0753	0.709
	(0.409)	(40.76)	(0.104)	(9.507)	(0.0994)	(10.44)
Relative care	-0.0106	-7.716	-0.0184	3.246	-0.0260	2.749
	(0.141)	(11.58)	(0.0436)	(3.924)	(0.0486)	(3.788)
Observations	1904	1904	28148	28148	26018	26018

Notes: Table shows selected estimated effects of the log minimum wage from panel models estimated on individuals with high school or less with one or more children aged 5 or younger. In columns 1, 3 and 5, the dependent variable is an indicator variable equal to one if the family paid any money for the arrangement, in columns 2, 4 and 6 the dependent variable is the total weekly amount spent on the arrangement. "Formal care" includes childcare centers and family daycares. Models include controls for state characteristics (log state population, AFDC/TANF and SNAP benefit levels, TANF implementation, major AFDC waiver), demographics (age, education, race and ethnicity, marital status, number of children, number of children interacted with calendar year), state and year fixed effects . Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Source: SIPP.