One Step Forward, One Step Back?: Labor Supply Effects of Minimum Wage Increases on Single Parents with Public Child Care Support

Randy Albelda and Michael Carr

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One Step Forward, One Step Back?: Labor Supply Effects of Minimum Wage Increases on Single Parents with Child Care Subsidies

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Randy Albelda and Michael Carr, University of Massachusetts Boston

Abstract
Cliff effects, or high marginal tax rates, occur when low-income workers using anti-poverty supports experience an increase in earnings that results in a substantial decrease in those supports, resulting in no gain in total resources. Cliff effects create a disincentive to work more hours or take a higher paying job. To investigate labor supply responses when faced with cliff effects, we use changes in federal and state minimum wages to estimate the responsiveness of work hours for single mothers with a childcare subsidy. Using single mothers with young children who are eligible to receive a childcare subsidy but do not as a control group, we estimate difference-in-differences models using the 2008 panel of the Survey of Income and Program Participation (SIPP). We find evidence that employed mothers with subsidies reduce hours more than those without subsidies after minimum wage increases, nearly offsetting the earnings increase.

1 Introduction

A built-in feature of means-tested supports is that they decline as earnings increase. Whether the decline in support is abrupt or phases out gradually, earning more generates higher marginal tax rates, sometimes called cliff effects. This feature creates a seemingly intractable dilemma: workers can move up the earnings ladder but in doing so take a trip down the benefit slide. Depending on the new level of earnings and benefits, total family resources may be no more or even less than they were before the earnings increase. A possible response to this dilemma is to forgo earnings increases by passing on a potential wage or salary increase or by reducing work hours in response to a wage increase. The cliff effects dilemma is particularly relevant for individuals with hard-to-get supports such as childcare both because co-payments increase as earnings increase and because, once an individual loses the benefit, there is no guarantee she or he will be able to get the benefit again if earnings decrease in the future. Labor supply responses to cliff effects for those receiving childcare subsidies are also of interest because it is one of the few supports directly aimed at supporting work. The dilemma of cliff effects calls into question the degree to which current anti-poverty programs support employment advancement and will no doubt take on increased importance with state and national "Fight for $15" minimum wage campaigns.

Very little is known about work hours (intensive margin) responses to wage changes for people who face cliff effects. The focus here is on single mothers who receive a childcare subsidy and their response to an increase in the minimum wage. Increases in the minimum wage are an exogenous increase in the hourly wage of low-wage individuals, even when they earn above the new minimum wage. At the current level of hours worked, the hourly wage increase triggers a reduction in the value of the subsidy, creating a cliff effect situation. Specifically, we estimate difference-in-differences models to identify the differential response to a minimum wage increase between single mothers who have a childcare subsidy prior to a minimum wage increase, and single mothers without a subsidy but eligible to receive one. Because minimum wage changes generate this exogenous change in actual hourly wages, changes in state and/or federal minimum wages result in a convenient natural experiment for studying cliff effects. To control for the possibility that individuals with a childcare subsidy are a partially self-selected group, we combine difference-in-differences estimations with two different methods of controlling for selection into treatment: propensity score weighting and individual fixed effects. Data come from the 2008 panel of the Survey of Income and Program Participation (SIPP), which is unique because of the length of the panel, the number of minimum wage changes it covers, and the detailed data on benefits receipt it contains.

Overall, the difference-in-differences estimates show evidence of a small, but statistically significant, additional reduction in hours worked after a minimum wage increase for mothers with a childcare subsidy. Weekly work hours decrease 1 to 1.7 hours per week more for single mothers with a childcare subsidy than single mothers without a childcare subsidy. Though small, the additional decrease in work hours is enough to almost completely offset the increase in earnings that would have resulted from the wage gains for subsidy holders, thus cancelling out any change in childcare costs. The same is not true for those without a subsidy where, even though work hours decrease somewhat following a minimum wage increase, overall earnings increase.

2 Overview and literature review

A primary policy concern over cliff effects is whether individuals who receive anti-poverty supports will accept offers or more hours of work or take better paying jobs, opportunities that might promote their short- and long-term ability to earn enough to be self-supporting. There is also growing concern that minimum wage increases might not provide an overall increase in resources for families that are combining earnings with supports. These concerns are particularly relevant for parents with supply-limited benefits such as childcare subsidies and housing assistance because, once the support is lost, it may be impossible to get back if earning decrease again in the future. But, we know very little about how cliff effects influence work hours. Below we explore the relevant literature on cliff effects/marginal tax rates, childcare subsidies and labor supply, and the minimum wage literature that pertains to hours worked, single mothers and public supports.

2.1 Marginal tax rates for low-wage earners

The loss of public supports when earning increase are most problematic when a family receives more than one public support or when the market replacement of the support may be considerably more expensive or of inferior quality, such as is the case with childcare. Single mothers are more likely than other adults to get public supports and it is not uncommon for a low-wage single mother to receive more than one support (US Government Accountability Office 2016; Kosar and Moffitt 2017).

The magnitudes of cliff effects or marginal tax rates (MTRs) are typically estimated for a specific family type and size using a microsimulator. State MTRs vary widely because eligibility rules, benefit levels, and phase out schemes for different programs differ state by state. Even with this variation, MTRs tend to be highest for those with incomes between 100-200 percent of the federal poverty line (FPL). Maag et al. (2012) estimate the marginal tax rates of a single parent receiving tax credits Supplemental Nutritional Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and Medicaid at various increments of the FPL for each of the fifty states and Washington DC in 2008. At very low incomes (moving from zero to 50 percent of the FPL), in many states the MTR are negative with an average rate of 7.8 percent. Moving from income at 50 percent to 100 percent of FPL the 51-state average is 24.6 percent; from 100 percent to 150 percent of FPL the average is 56.3 percent: and from 150 percent to 200 percent of the FPL the average is 76.1 percent. Kosar and Moffitt (2017) find MTRs to be higher for single parents than for married parents and are exceptionally high between 100 and 150 FPL for families receiving multiple benefits. They report a MTR of 81% for the median single mother of two children with income between 100 and 150% of the poverty line receiving SNAP and Medicaid only. Using administrative data on usage of programs in Wisconsin, Holt and

Romich (2007) estimate that one-quarter of single mother families in that state faced a 50 percent or higher marginal tax rate in the early 2000s.

And while high marginal tax rates are likely to discourage work, given that different programs phase out at different income levels and different rates, it might be hard for any particular family to know when MTRs are highest, making it difficult to know when or even how to respond. Further, some workers may not have control over their work hours so cannot reduce hours. Others may decide that in the long run it is better to receive higher earnings so may increase hours. Qualitative research on marginal tax rates suggests that those facing cliffs do all three in regards to their labor supply: pull back, stand in place, or forge ahead. Romich (2006) interviewed heads of 40 low-income families in the Milwaukee region in the third year of a work promotion anti-poverty experiment about their experiences with loss of benefits as earnings increased. Most felt they were not in control of their situation and expressed anger and frustration. Still, Romich found only one person reporting reducing work. Albelda and Shea (2010) analyzed transcripts from 22 focus groups conducted with 166 low-income parents in four states asking for advice for a parent receiving several public supports that was offered more hours of work that would result in more earnings but fewer supports and perhaps less time with her children. Relying on their own experiences, participants were largely split on their advice for this hypothetical mother as whether to take more hours or not. Roll and East (2014) surveyed 332 parents through child care centers in four counties in Colorado. Of the 109 that indicted they changed strategies about work, family status or income reporting to maintain their child care voucher status, 46 indicated not taking a raise, 61 refused extra hours at work, while 31 declined a job offer.

So while it is clear that MTRs are high for single mothers right below and above the federal poverty income threshold, qualitative research does not provide any clear direction for predicting how, on average, single mothers with childcare subsidies might change their labor supply in response to a wage increase.

2.2 Childcare and labor supply

The literature that examines the impact of childcare and labor supply typically focuses on the relationship of the price of childcare and the extensive margin (employment). A reduction in childcare expenditures (which would be one impact of receiving a child care subsidy) consistently results in increases in single mothers' employment (Herbst 2010, Connelly and Kimmel 2003, Anderson and Levine 2000). A meta-analysis of labor supply elasticity and child care prices finds that US average elasticity is larger than that in Europe, but over time elasticities have decreased everywhere (Akgunduz and Plantenga 2015).

Studies that directly test the effect of subsidy receipt on employment consistently find that childcare subsidy receipt increases the likelihood of employment (Blau and Terklin 2007, Meyers et al. 2002, Teklin 2007). When including the EITC and allowing the price of child care to vary, Herbst (2010) using primarily 1990s panels of the SIPP finds a good deal of heterogeneity in responses, with the strongest employment responses to subsidies in mothers with the highest costs. Only a few studies look at the intensive margin and these focus on the impact of the federal child care tax credit (which at the time was not refundable) on hours worked. Averett et al. (1997), using the 1986 National Longitudinal Survey of Youth, simulate labor supply hours of married mothers in response to an increase in the value or the child care tax credit. They define an effective wage that includes the value of the tax credit which declines as wages increase. The authors find that increases in the child care tax credit increases hours worked. Michalopoulos et al. (1992) develop a structural labor supply model for single and married mothers with young children and estimate it using the 1984 panel of the SIPP. They simulate the number of hours work under various scenarios, including making the credit refundable and increasing its value. In all cases the increase in hours worked by single mothers is negligible. This literature strongly suggests that child care subsidies boost mothers' employment, but provides little guidance on how hours might change when co-payments for those subsidies increase.

2.3 Minimum wages, public supports and single mothers

The minimum wage literature largely focuses on employment effects and typically assumes that any equilibrium response found, either less employment or fewer hours, is a demand response to the increase in the minimum wage. In theory at least, employment changes for workers facing cliff effects could be the result of a supply response.

Some findings in the minimum wage literature, putting aside the methodological divide on the proper specifications for conducting minimum wage research, shed light on cliff effects responses. Dube (2017) finds increases in the minimum wage over the period 1984-2013 causes a reduction of non-elderly poverty, however, single mother poverty reduction is the least responsive among the six subgroups he explores. Further the inclusion of receipt of public supports reduces the impact of poverty reduction, especially at income levels near the poverty line. Looking at state-level SNAP enrollment rates, Reich and West (2015) find a 10 percent increase in the minimum wage reduces SNAP enrollment by 2.4-3.3 percent. Sabia and Nguyen

(2015) look at changes in receipt of six different public supports (three of them are Food Assistance programs, the others are TANF, Medicaid and housing assistance) in response to minimum wage increases from 1979-2013 using the March CPS and several SIPP panels. They look specifically at single mothers ages 16-45 without a high school degree and find no significant changes in public support usage in either data set with one exception -- a decrease in Food Stamps/SNAP usage using the CPS. Because the use of supports does not increase (as it would if unemployment increased or there was an overall loss in hours), the improvement in earnings are swamping negative employment effects. Further, the decline in public supports suggests that overall workers are losing these supports as earnings increase. Together, these studies suggest that cliff effects certainly dampen the impact of minimum wage increases but may not be deterring people from increasing earnings.

3 Child care subsidies

The largest US program that directly provides financial assistance for childcare is through the Child Care and Development Fund (CCDF) block grant to states and the District of Columbia. The federal government sets broad eligibility parameters which act as a ceiling for state rules: children must be under the age of 13; household income can be no more than 85 percent of state median income for a family of the same size; and a parent must be employed or be participating in education or training. In 2012, there were just under 2.2 million children receiving a CCDF childcare subsidy, which is 10 percent of those eligible using the federal eligibility rules and 25 percent of those eligible using state-level eligibility rules (US Department of Health and Human Services 2015). The US Department of Health and Human Services (2015) estimates that two-thirds of children receiving a subsidy are younger than six years old and 63 percent are in families with income below the federal poverty income threshold. Not all eligible families receive subsidies due to insufficient funding which leads states to pursue policies that determine and limit usage. Forry et al. (2013) also points to research that finds lack of knowledge, stigma and administrative procedures also deter usage.

States establish eligibility requirements as well as level and amount of co-payments parents make. States can set income eligibility levels at different rates for initial eligibility versus for those already with a subsidy, which 15 states did in 2009 (Giannarelli et al. 2011). Above family size specific income levels, states require families to contribute a co-payment which increases as income increase. However, levels and the incomes at which co-payments change vary widely. In 2009, a single parent family with a two year old in full-time care earning $15,000 annually would have a monthly co-pay ranging from $0 to $194, depending on which state she lived in (Giannarelli et al. 2011). Parents with subsidies are required to report changes of income no more than 10 days after the change, although states can require shorter amounts of time.

Herbst (2008), using the 2002 National Survey of American Families, simulated eligibility and finds that about 28 percent of all children are eligible with a take-up rate (percent eligible that actually use a subsidy) of 14 percent. Among single mothers, he finds no difference in either eligibility or take-up rates among poor (100 percent of FPL or below) mothers and those with household income between 100-200 percent of FPL. In comparing eligible single mothers that receive a subsidy to those that do not, he finds no statistical difference in the mean number of hours or weeks worked. In probit regression of receipt of a subsidy among eligible single mothers, Herbst (2008) finds that mothers receiving a subsidy are more likely to be black, have beyond a HS diploma, have both a young (0-5) and older child (6-12), be employed, and receive TANF and Food Stamps than mothers that do not. Johnson, Martin and Brooks-Gunn (2011) use the Early Childhood Longitudinal Study--Birth Cohort to predict family background and child care preference differences among eligible parents with preschool-aged children that receive subsidies from those that do not. Using logit regression models, they find that eligible mothers using subsidies tend to be less disadvantaged (have higher English proficiency, higher income) and those without subsidies are more likely to value the costs of care and care that is more flexible (i.e. close to home and provide sick care). This suggests that there may be important differences among eligible parents that use subsidies compared to those that do not.

4 Methods

To explore the work hours response to high marginal tax rates among those with public supports, we use changes in the minimum wage as an exogenous shock to the actual wage earned by a single mother with a childcare subsidy. Although what we are ultimately interested in is whether single mothers with a childcare subsidy adjust work hours in response to actual wage changes, we cannot estimate this directly because the observed wage is endogenous. It represents the combination of individual choices, employer choices, child care cost, local labor market conditions, and so on. Because the population of single mothers we are interested in have low earnings, they likely also have hourly wages low enough to be impacted by a minimum wage increase even if they earn above the minimum wage.

Further, because single mothers with a childcare subsidy have a higher marginal tax rate than mothers without a subsidy, the change in the actual wage that results from a change in the minimum wage affects those with a subsidy differently than those without a subsidy, all else equal. This feature defines treatment and control groups that can be used to estimate the incentive effects of high marginal tax rates on single mothers who receive a childcare subsidy. Specifically, we use single mothers who are eligible to receive a childcare subsidy, but do not, as a control group. A minimum wage increase has two paths through which it may affect work hours or hourly wages: (1) an equilibrium effect that represents both labor supply and labor demand decisions of all single mothers and employers, and (2) the interaction with high marginal tax rates of those with a childcare subsidy. Our estimation strategy uses the fact that the first path is the same for both treatment and control groups, implying that differences between the groups are due to the second path. That is, any difference in work hours or hourly wages that emerge between the treatment and control groups following a minimum wage change is due to having a childcare subsidy.

Described in more detail in the following sections, we estimate the differential response to a minimum wage change using a standard difference-in-differences framework. The validity of our estimation approach rests on addressing two concerns. The first is that single mothers with and without a subsidy, conditional upon being eligible to receive a subsidy, are not different from each other along dimensions that might affect labor supply. But, we find this not be the case. To address this, we employ two different methods to adjust for differences in observable (and unobservable) characteristics: propensity score matching and individual fixed effects. Both approaches can be thought of as an attempt to construct an appropriate counterfactual group for single mothers with a childcare subsidy. The second condition, which we do find holds, is that the minimum wage has sufficient bite on the wage distribution of our sample to make this a viable natural experiment. An implicit assumption is that childcare costs increase when earnings increase for individuals with a childcare subsidy and/or that subsidy holders run the risk of losing the subsidy entirely, or at the very least, that subsidy holders behave as if this is true. Of course, we cannot directly test this implicit assumption because if subsidy holders adjust work hours in response to wage increases, then childcare costs may not increase.

4.1 Difference-in-Differences Framework

We rely on difference-in-differences estimates of the differential effect of the minimum wage on labor supply and hourly wages in the months prior to a minimum wage increase. Because the data pools together states with distinct local labor market conditions, minimum wages that change in different calendar months and have heterogeneous effects on prevailing wages, our estimation strategy must ensure that only individuals in the same state during the same minimum wage event are compared. Individuals in different states at the same point in time, or the same state at different points in time must not be compared to each other. The regression framework present in Equation 1 accomplishes this by combining a large set of fixed effects.

$$l_{iset} = \alpha + \gamma CS_{ise} + \omega T_{set} + \delta(CS_{ise} * T_{set}) + X'_{ise}\beta + Y'_{iset}\theta \ + \tau_{se} + \lambda_{te} + \phi t_s + u_{iset} \quad (1)$$

The dependent variable (l_{iset}) is either usual weekly work hours or the log hourly wage for individual *i* in state *s* during event *e* at time *t*, where *t* runs from -11 to 11 and *t*=0 marks the month when the minimum wage changes in state *s* during event *e*. We control for state by event fixed effects (τ_{se}), time by event fixed effects (λ_{te}), state-specific linear time trend (ϕt_s), controls for race and educational attainment are in X'_{ise} , and time-varying controls including a polynomial in age, receipt of other key benefits including housing assistance, SNAP, and TANF, one-digit industry and occupation dummies, and a dummy for whether the minimum wage event occurred during the Great Recession as dated by the National Bureau of Economic Research are in Y'_{iset}. The combination of state by event and time by event fixed effects ensures that identification of δ uses only within state by event variation. Differences across states in benefit eligibility rules, minimum wage levels, prevailing wage levels, prevailing labor market conditions, and so on have no effect on the difference-in-differences estimate. The state specific linear time trend allows each state to evolve differently through events, but does not allow trends to vary across events within a state. Sample size limitations prevent the use of state by event specific time trends.

The difference-in-differences estimator is δ, the coefficient on the interaction between a dummy for receiving a childcare subsidy at any time in the twelve months prior to a minimum wage change (CS_{ise}) with $CS_{ise} = 1$ if an individual receives a childcare subsidy in time $t \leq 0$, and a dummy for $t > 0$ (T_{set}). It provides an estimate of the difference between the average change in l_{iset} for those with a subsidy minus those without a subsidy. The fact that the aggregate employment effects and the methods used to estimate the effect of a minimum wage increase are contested in the minimum wage literature does not matter here, as long as the effects are the same for the two groups, except for the additional labor supply response due to having a childcare subsidy.

4.2 Propensity Score Matching and Fixed Effects

Herbst (2008) and Johnson et al. (2011), discussed earlier, raise the concern that the characteristics of subsidy-eligible single mothers differ between those with and without a subsidy. We also find (in **Table 1** discussed below) that the observable characteristics of single mothers with and without a subsidy, conditional on being eligible to receive a subsidy, are indeed different along dimensions that are likely correlated with labor supply in our sample.

We use propensity score matching, specifically the kernel matching technique developed in Heckman, Ichimura, and Todd (1998), to address the fact that treatment and control groups are not balanced.[1] The key advantage of this approach is that it uses a weighted average of all observations to create a match for each treated observation, where the weights reflect the similarity in the estimated propensity score between a given treated observation and the pool of untreated observations to be matched. Observations in the control group that are more like a given treated observation according to the propensity score are given more weight when calculating differences between treatment and control groups. Unlike other popular matching

[1] An alternative to propensity score matching sometimes used in economics is Inverse Propensity Weighting (IPW) (Hirano, Imbens, and Ridder, 2003). We prefer kernel weighting because IPW can sometimes lead to a small number of observations playing an outsized role in estimated coefficients. Results using IPW are consistent with those presented here and are available from the authors upon request.

methods, kernel matching results in a match for all treatment group observations that fulfill the common support requirement.[2]

The propensity score is estimated using a logit regression that predicts the probability of having a childcare subsidy at any point in the twelve months prior to a minimum wage change, given in Equation 2. As suggested by Todd (2008), when combining propensity score matching with difference-in-differences estimation, the propensity score should be estimated using only time-invariant characteristics and/or characteristics of individuals from before the change in the minimum wage.[3]

$$Prob(CS_{ise} = 1) = \alpha + A'_{ise}\gamma + X'_{ise}\beta + \tau_s + \varepsilon_{ise} \qquad (2)$$

[2] The foundational work on propensity score matching is Rosenbaum and Rubin (1983). For a current review of the literature on propensity score matching, at least as it is commonly used in economics, see Caliendo and Kopeinig (2008).

[3] This introduces a third concern for our estimation strategy. If the composition of the sample changes following a change in the minimum wage, then what was a good match prior to a minimum wage change may no longer be a good match. This issue is addressed in Appendix A. Appendix Table A.1 shows that this is unlikely to be a problem, as there is no evidence of differential attrition between those with and without a subsidy following a minimum wage change. Appendix Table A.2 shows that the estimated difference in the probability of exiting the sample following a minimum wage change is near zero.

In Equation 2 CS_{ise} is an indicator for having a childcare subsidy prior to a minimum wage change, A'_{ise} is a polynomial in age across the twelve months leading into a minimum wage increase, X'_{ise} is the same vector of human capital and demographic controls as seen in Equation 1, and τ_s is a set of fixed effects. Any control variables included in the propensity score matching estimation are excluded from the difference-in-differences estimation except the state fixed effects which show up in both because the propensity to have a childcare subsidy varies by state, and the state fixed effect plays a critical role in identifying the difference-in-differences estimate. Confidence intervals for the matched models are bootstrapped.[4]

As an alternative to propensity score matching, we also use an individual fixed effects model. The difference between individual fixed effects and propensity score matching is the conceptualization of the counterfactual group. In propensity score matching, the counterfactual for a treated observation is a weighted average of the observations in the treatment group. The hope is that by balancing using the observable propensity score, one is also balancing unobservable characteristics. Individual fixed effects regressions, on the other hand, estimate the difference in the average within person change between treatment and control groups following a minimum wage change. Fixed effects estimation uses an individual in the time period before a minimum wage change as a control for the same individual in the time period after the minimum wage change.

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[4] There has been some debate recently about the validity of bootstrapped standard errors when combined with certain types of propensity score matching (Abadie and Imbens, 2008). However, Todd (2008) shows that bootstrapping is appropriate with kernel matching.

Using individual fixed effects has the advantage of controlling for all time-invariant characteristics of an individual, both observable and unobservable. As such, the vector of time invariant characteristics X'_{ise} is not included in the estimation of Equation 1 with fixed effects. The primary drawback of individual fixed effects in this context is that only the difference between groups following a minimum wage change can be estimated (δ) in Equation 1. It is not possible to estimate the amount of change in wages or work hours that is common to both treatment and control groups (ω) or the average difference between groups prior to a minimum wage change (γ).

We use the matched sample results to provide estimates of the overall change in hourly wages and work hours for those with and without a subsidy, respectively. Because individual fixed effects control for observed and unobserved individual characteristics, the fixed effects estimate of δ is arguably more robust. Because of this, we will ultimately rely on the fixed effects estimate for evidence of a differential change between treatment and control groups.

5 Data

The data come from the 2008 wave of the Survey of Income and Program Participation (SIPP). The SIPP is a nationally representative monthly panel dataset of approximately 52,000 households. The 2008 wave of the SIPP covers four years, with each household included in the survey for as many as sixteen reference months evenly spaced over the four years of data collection. Thus, our data include interviews of approximately 52,000 households for at least one month, and up to sixteen reference months, with interviews occurring every four months. Data collection is retrospective, recording earnings, work, and benefits data about the reference month and the previous three months. We use all calendar months. The SIPP is the best available dataset in the United States on receipt of public benefits, including specific questions about whether an individual receives public support for childcare.

Our identification strategy hinges on (1) being able to identify who receives a childcare subsidy, and (2) being able to identify who is eligible for a childcare subsidy but does not receive one. We use information on eligibility from the CCDF Policies Database compiled by the Urban Institute for every year since 2008 to determine eligibility for a childcare subsidy. Eligibility is typically based on number of hours worked, family income, family size, and age of the child. While the vast majority of eligibility rules do not vary much within a state, income thresholds vary considerably through time due to inflation adjustment, changes in the income eligibility standard (area median income), and policy changes. Income eligibility threshold and minimum work hours requirements vary considerably across states. **Figure 1** shows the income at which a family of three becomes ineligible for a subsidy by state for 2008 and 2012.

[**Figure 1** HERE]

We use all minimum wage changes, both state and federal, which ever happens to be binding in a state in a given month. **Figure 2** depicts nominal minimum wages by state in 2008 and 2012. As is clear, there is considerable variation in minimum wages across states.

[**Figure 2** Here]

Our sample is limited to single mothers between the age of 18 and 45 who are ever eligible for a childcare subsidy in the 11 months prior to a minimum wage change, and not enrolled in school at any time in the 23 months surrounding a minimum wage change. An 11-month window is used because there are a number of states with a series of minimum wage changes every twelve months. Broadly, this means that to be included in the sample a mother must meet the minimum work hours requirement in the state where she resides at any point in the 11 months prior to a minimum wage change, have income below the maximum state specific threshold prior to the minimum wage change, and have at least one child under the age of 11 (or 12 in some states). Following a minimum wage change, to remain in the sample a single mother must be employed for pay, be between the age of 18 and 45, and have a child under the maximum age of eligibility for the single mother's state of residence. We further restrict the sample to individuals where the key variables (work hours, total family income, and presence of children for childcare) are not imputed, and use only those individuals who gave their interview in person or by proxy. Finally, we drop the top and bottom 1 percent of hourly wages due to some extreme outliers at the top and bottom of the wage distribution.

We treat each minimum wage change in each state as a separate event. That is, we create a 23-month window around each minimum wage change in each state (11 months before, 11 months after, and the reference month), center all windows on the month in which the minimum wage increases, and pool all events together analogous to an event study method. Individuals may appear anywhere between 1 and 23 months for a given event, and may appear in more than one event. The final sample for the childcare analysis has 23,337 person-month observations.

Between 2008 and 2012, there are 110 minimum wage changes, with an average change of 6.6 percent.

6 Results

We first discuss the descriptive statistics for our sample, providing the justification for propensity score matching. We then estimate how much hourly wages increase following an increase in the minimum wage. Finally, we estimate the labor supply equations.

6.1 Descriptive Statistics

Table 1 provides the descriptive statistics for the sample. Overall, subsidy holders work about 0.7 hours per week less, and earn about $1.66 per hour less than those without a subsidy. Single mothers with a childcare subsidy are also younger and less likely to be white. About 19 percent of the sample who are eligible for a subsidy actually receive one. Characteristics of the matched samples are quite similar to each other, resulting in no statistically significant differences in observable characteristics between the treatment and control groups. After matching on demographic and human capital characteristics, single mothers without a subsidy earn about $1.13 per hour more than those with one and work about 0.5 hours less. It appears as if the matching process does a good job of balancing observable characteristics of workers pre-minimum wage changes.[5] As discussed, the full regressions include additional controls that are

[5] The number of observations in the matched sample is smaller than the full sample due to the common support requirement for propensity score matching. The distribution of propensity scores is provided in Appendix B.

important for wage and work hours determination, but vary through time and thus cannot be included in the matching process.

[Table 1 About Here]

6.2 Effect of Minimum Wage Changes on Hourly Wages

Our identification strategy for the labor supply regressions requires that an increase in the minimum wage does, in fact, increase wages of workers in our sample. To estimate the degree of bite the minimum wage has on the wage distribution of the sample we estimate Equation 1 with the natural log of the hourly wage as the dependent variable.

The results are reported in **Table 2**. For unmatched and individual fixed effects (FE) regressions, we report clustered standard errors and the associated p-values represented by significance stars. In the matched results, because we bootstrap confidence intervals directly rather than calculating confidence intervals based on the standard error, we report only significance stars associated with the relevant confidence interval. Hourly wages for salaried workers are imputed by dividing monthly salary by monthly hours worked. Monthly hours work is approximated using weeks worked in a month times usual weekly work hours. We present results for the full sample and for hourly workers only because salaried workers' wage changes are sensitive to both salary and work hours changes and may result in measurement error.

In the full sample, subsidy holders earn a statistically significant 10.12 percent less per hour than non-subsidy holders in the matched sample. Among hourly workers, subsidy holders earn 9.27

percent less than non-subsidy holders, a difference that is statistically significant at the 1 percent level. The minimum wage increases hourly wages in the full sample by 7.79 percent for those without subsidy. Hourly wages increase 3.84 percent following a minimum wage change. Neither change is significant at conventional levels, though both are economically meaningful. The larger effect on the sample including salaried workers may in part reflect measurement error as the imputed wage is sensitive to both salary and work hours changes.

The estimate of δ is consistently positive though decreases in magnitude as we move towards our preferred method of estimating differential changes. In the full sample, hourly wages increased an additional 2.89 percent for subsidy holders. For hourly workers, hourly wages increased an additional 2.52 percent for subsidy holders. The fixed effects results suggest smaller differences between the average wage change of subsidy and non-subsidy holders, with subsidy holders in the full sample experiencing a 2.47 percent larger increase and hourly workers a 1.20 percent larger increase. This latter estimate, the fixed effects estimate of δ for hourly works is the most reliable as it reduces the impact of measurement error and comes from differences in the within person change in work hours.

Overall, we take these results as evidence of economically meaningful, but heterogeneous wage increases, with evidence of a small additional increase in hourly wages for subsidy holders. The lack of precision of the estimate of the effect of the minimum wage on hourly wages for individuals without a subsidy should not be a surprise as the average wage for this group is about $12 per hour, indicating that a fraction of these workers should not be affected by minimum wage changes. Specifically, while 69 percent of subsidy holders earn an hourly wage within 150 percent of the new minimum wage in their respective states, only 54 percent of non-subsidy holders fall in this same range. This does not change the fact that subsidy holders receive a statistically significant increase in hourly wages, nor the validity of those without a subsidy as a control group.

6.3 Labor Supply Response to a Minimum Wage Change

Figure 3 depicts average weekly work hours for single mothers with and without a childcare subsidy in the 23 months surrounding a minimum wage change using the matched sample, with accompanying linear trend lines estimated separately in the pre and post periods. Although on average in the matched sample, subsidy holders work slightly more than non-subsidy holders in the period before an increase in the minimum wage, work hours following a minimum wage change are clearly lower for subsidy holders. In the 11 months leading into a minimum wage change ($t \leq 0$) both groups show a pronounced decline in work hours with subsidy holders a slightly steeper trend.[6] Following a minimum wage change, both groups show a general upward trend in work hours. For those without a subsidy the increase is immediate, with evidence of a 1 month anticipatory increase. For those with a subsidy there is an anticipatory downward shock in work hours between months -2 and -1, persistently lower hours from months -1 to 8, and a slight recovery beginning in month 8. The presence of trends entering the minimum wage change is the primary motivation behind the time by event and time-trend controls, however, for difference-in-differences estimation the primary concern is differential trends before the minimum wage

[6] The pretrend for those with a subsidy has an estimated slope of -0.09, while the slope for those with a subsidy is -0.12.

change, not trends per se. There is little evidence of a differential trend in **Figure 3**, and we present further evidence below that this is not a large concern.

[**Figure 3** Here]

Table 3 shows the results of estimating Equation 1 with weekly work hours as the dependent variable. The baseline estimation of Equation 1 is reported in the first column, using the full unmatched sample. In the remaining columns, the results for the full matched sample and the full sample fixed effects regressions are reported, followed by the same set for hourly workers. The results across all estimation techniques and samples indicate a reduction in work hours following a minimum wage change, and that the decrease is larger for those with a subsidy as demonstrated by the negative estimate of δ, the coefficient on the interaction.

As the results are similar across estimation techniques, we will focus on the matched sample results for all workers and hourly workers, respectively. All else equal, all subsidy holders work about 0.6 more hours per week than non-subsidy holders, while those paid hourly work 1.12 hours more. Both differences are significant at the 1 percent level. Following a minimum wage change, work hours decrease 1.3 hours per week for all workers, and 2 hours per week for hourly workers. Neither of these differences are statistically significant, suggesting heterogeneous overall effects of minimum wage changes.

[**Table 3** Here]

The estimate of δ for the full sample indicates that all subsidy holders decrease work hours by a statistically significant 1.4 hours per week more than non-subsidy holders. For hourly workers, the decrease is 1.75 hours per week more, and again statistically significant at 1 percent. In total, while work hours are 1.3 hours per week lower after a minimum wage change for non-subsidy holders, they are 2.7 hours per week lower for subsidy holders.

The fixed effects estimates are generally smaller. Most importantly, while the fixed effects result for hourly workers shows that wage changes are statistically indistinguishable between those with and without a subsidy, there is a statistically significant additional decline in work hours of 1.235 hours per week. This is notable because it rules out the possibility that the driving force behind the differential work hours changes is differential wage changes.

6.4 Trends through Time

Figure 3 also suggests the possibility that the average difference between subsidy holders and non-subsidy holders varies through time potentially before, and definitely after a minimum wage change. To investigate how differences evolve through time, we estimate Equation 1 with 10 leads and 11 lags to identify pre-and post-treatment differences between the two groups.[7] That is, we estimate a model that traces out the difference between the treatment and control groups for the entire window around the minimum wage change in the matched sample. The results are reported in **Figure 4** for hourly workers, which shows the estimated coefficients on the leads and lags, in addition to 90 percent confidence intervals. Ideally, the average of the differences before

[7] Essentially, what we do is estimate a regression that replaces the interaction term in Equation 1 with a full set of interactions between CS_{ise} and λ_t.

a minimum wage change should show no clear pattern, while the average of the differences after the minimum wage change should equal the estimate of δ.

[**Figure 4** Here]

The leads are all statistically insignificant at all conventional levels. Months -10 to -2 show no clear trend, oscillating around a difference of zero. Month -1 shows a downward change of about the same magnitude as the differences seen after a minimum wage change. This suggests that longer-term pretrends are not driving our results. Subsidy holders appear to anticipate the shock to hourly wages one month in advance, and adjust work hours perhaps to avoid an earnings increase. In addition, none of the lags except months 3 and 8 are individually statistically different from zero at the 10 percent level, though months 9 and 10 are close. The differences are fairly stable at about 2 hours per week less, the estimate of δ seen for hourly workers.

7 Discussion and Implications

In response to an increase in minimum wages, on average, all single mothers see a small reduction in work hours, although not at statistically significant levels. This change could be the result of an employer reducing hours or a mother choosing to work less. However, single mothers with a subsidy experience an additional small but statistically significant decline in work hours compared to those without a subsidy, which can be attributed to labor supply decisions. There also appears to be an adjustment process for subsidy holders. One month prior to the minimum wage change there is a decline in weekly work hours of about 1.5 hours per week. Over time, relative work hours continue to decline to about 2 hours per week less, almost negating the differential gains made in wages. There may also be considerable heterogeneity in these responses among our sample that we have yet explored.

Based on predicted wages and work hours from the regression results, assuming full-year employment had not decreased, full-year earnings would have increased from an average of $18,612 to $20,600 for single-mothers with a subsidy. The estimated work hours decrease means that earnings increase to an average $19,050 instead, or $438 per year. We do not know how much co-payments increase due to the slightly higher income, so we cannot say if new costs equal or surpass the additional earnings. For those without a subsidy, the regression results imply that annual earnings increase from an average of $21,950 to $22,840, or about $890 per year after accounting for both wage and hours changes.

We find that in the aggregate, the cliff effects posed by an increase in the minimum wage results in a "pull back" response by single mothers with subsidies compared to those without. This suggests that high marginal tax rates for single mothers create an incentive to cut back on hours when wages increase. In terms of the well-being of single mothers moving up the earnings ladder as well as for policies intended to support employment and boost earnings among low-wage workers, these results are a mixed bag. While 1990s welfare reform changes to anti-poverty programs were intended to promote employment, until anti-poverty supports phase out at earnings levels in which single mothers can support their families, the cliff effects they generate do not support employment. Similarly, minimum wage policy, the employment policies directed at improving overall earnings of low wage workers, especially of those that are primary breadwinners, has mixed effects for single mothers with public supports. On the one hand, the reduction in hours almost entirely offsets the increase in earnings (especially when combined with lost value of supports associated with increased earnings). This suggests that for those relying on the much needed supports, wage increases are a story of two steps forward, 1.5 steps back. On the other hand, the reduction in hours worked prompted by the increase in hourly wages could potential mean more time with children or for self care and should not be overlooked as an improvement in well-being for time-strapped low-income employed single mothers (Albelda 2011).

Ultimately, the labor supply responses associated with cliff effects are not the result of minimum wage policies, but rather the high marginal tax rates embedded in anti-poverty policies. On net, they represent an improvement for single mothers with public supports, but not nearly as much as they are for workers without those supports. Changing anti-poverty policies would be the best way to address cliff effects problems. One way to do this would be to increase income eligibility levels so that benefits stop or phase out less dramatically and at higher levels. A more comprehensive solution for high marginal tax rates associated with child care supports, however, would be universal childcare provision.

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Figure 1: Maximum Income Eligibility Threshold by State for a Family of 3

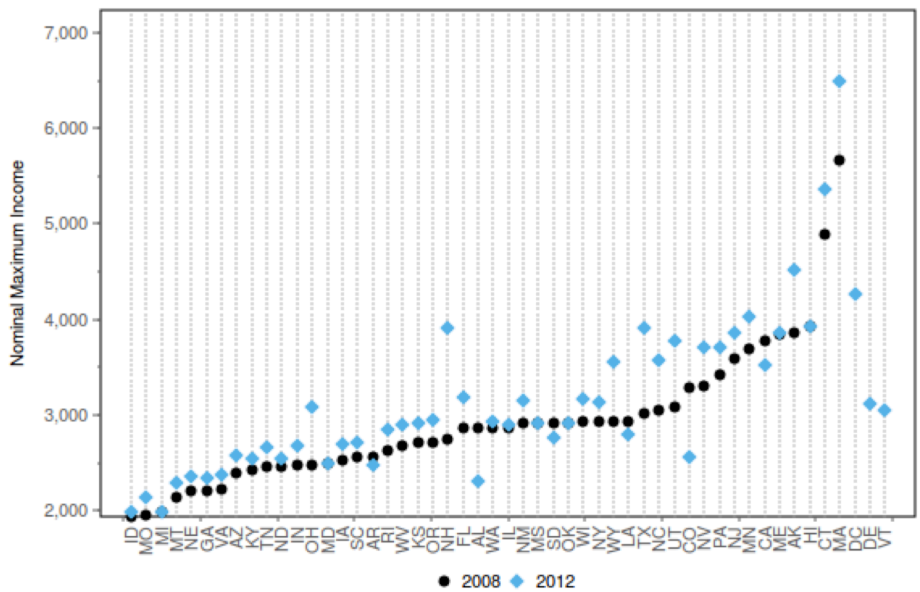

Figure 2: Nominal Minimum Wage by State

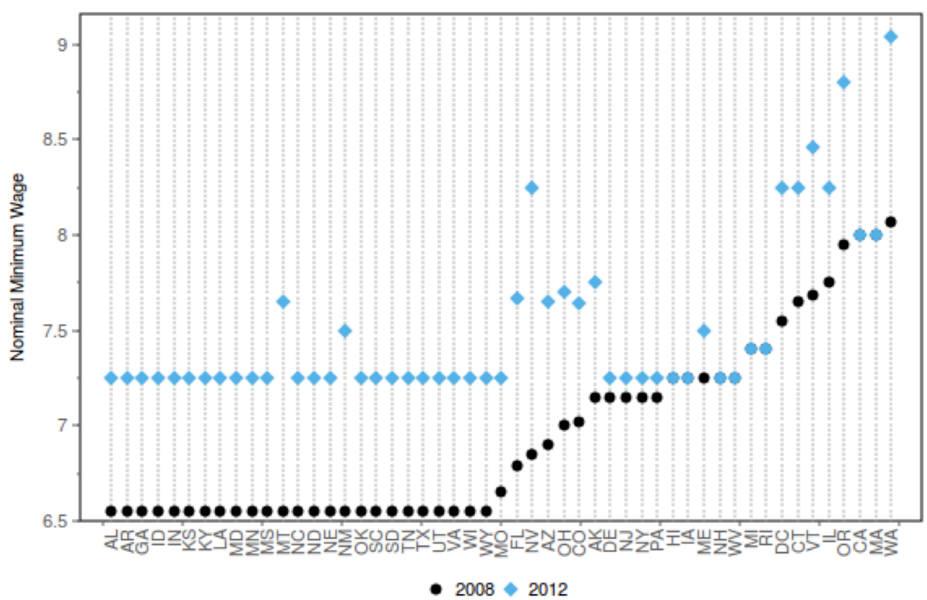

Figure 3: Average Weekly Work Hours Around a Minimum Wage Change

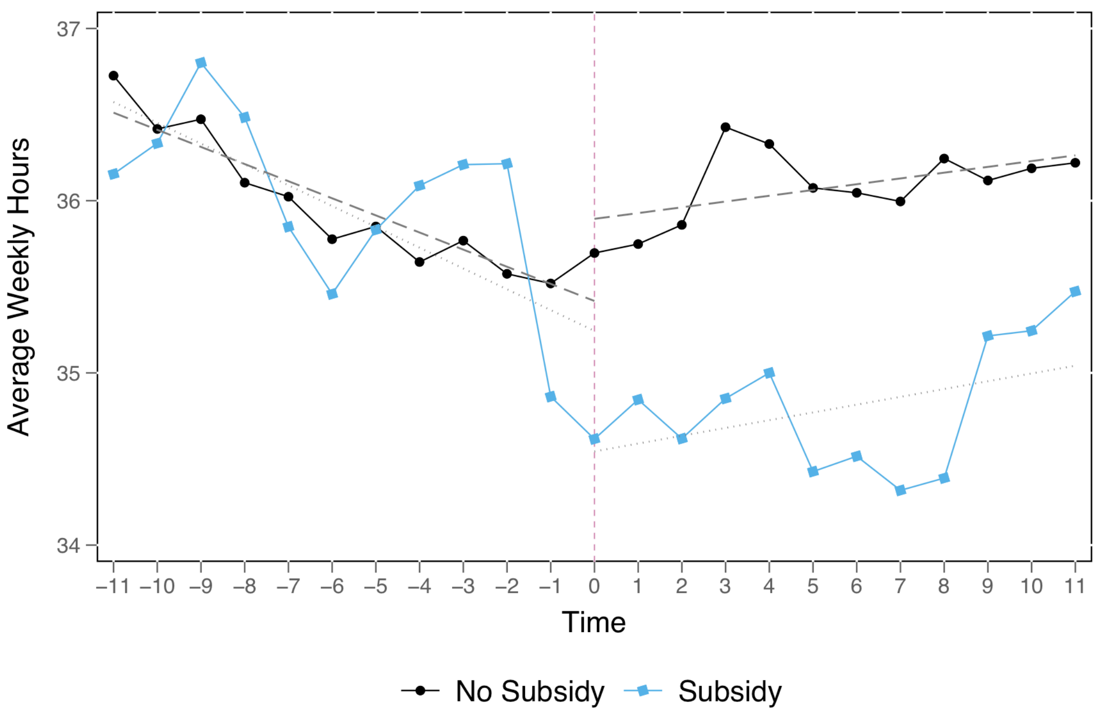

Figure 4: Monthly Treatment Effects for Hourly Workers

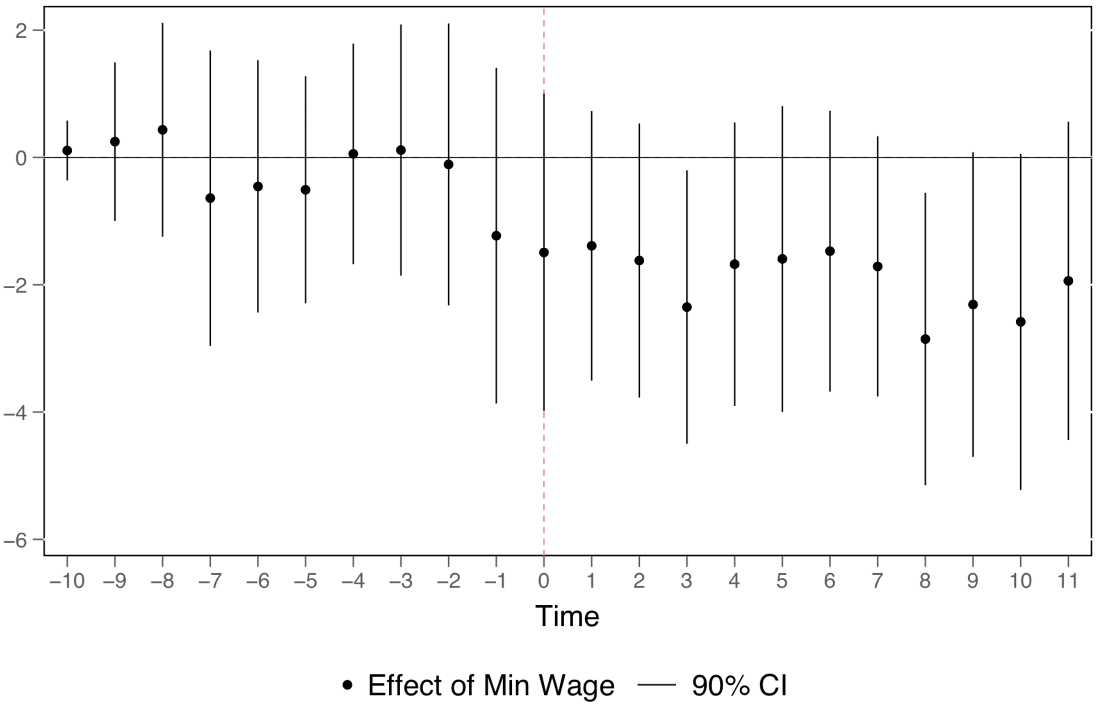

Table 1: Descriptive Statistics by Receipt of Childcare Subsidy

	Unmatched Sample			Matched Sample	
	All Eligible	No Subsidy	Subsidy	No Subsidy	Subsidy
Means					
Hours	36.045	36.172	35.522	35.139	35.403
	[8.976]	[8.862]	[9.419]	[9.630]	[9.056]
Wage	11.815	12.138	10.476	11.669	10.531
	[5.014]	[5.304]	[3.248]	[4.545]	[3.245]
Age	31.710	32.336	29.119	29.176	29.184
	[7.232]	[7.377]	[5.933]	[5.923]	[5.960]
Shares					
White	0.426	0.448	0.338	0.350	0.366
Black	0.329	0.290	0.490	0.472	0.462
Hispanic	0.209	0.228	0.128	0.138	0.132
Other	0.036	0.034	0.043	0.040	0.040
LTHS	0.116	0.110	0.139	0.140	0.155
HS	0.372	0.369	0.384	0.359	0.382
Some Coll. +	0.513	0.521	0.478	0.501	0.464
N	23338	18928	4410	18798	4385

Notes: Sample is all single mothers who are eligible to receive a childcare subsidy at any point in the 11 months prior to a minimum wage change, age 18 to 45. Eligibility is determined by a combination of income, family size, and hours worked. Matching is performed using a kernel matching procedure. All differences in matched sample are statistically insignificant. Standard deviations are in brackets.

Table 2: Difference-in-Differences Regressions for Log Hourly Wage

	Full Sample			Hourly Workers		
	Unmatched	Matched	FE	Unmatched	Matched	FE
Subsidy	-0.0904***	-0.1012***		-0.0791***	-0.0927***	
	[0.0311]			[0.0264]		
Post	0.0683	0.0779		0.0589	0.0384	
	[0.0917]			[0.0405]		
Subsidy x Post	0.0347**	0.0289***	0.0247***	0.0297**	0.0252***	0.0120
	[0.0146]		[0.0079]	[0.0123]		[0.0075]
Constant	2.0900***	2.4568***	2.8181**	1.6638**	2.4142***	2.8463***
	[0.6928]		[1.1581]	[0.6312]		[0.6539]
N	23183	23183	23183	19316	19316	19316

Notes: Sample is all single mothers who are eligible to receive a childcare subsidy at any point in the 11 months prior to a minimum wage change, age 18 to 45, who fulfill the common support requirement. Eligibility is determined by a combination of income, family size, and hours worked. Additional controls include a polynomial in age, education, race/ethnicity, receipt of other benefits, state fixed effects, time fixed effects, and a state specific linear time trend. Matching is performed using a kernel matching procedure. Significance levels: * 10%, ** 5%, *** 1%. Standard errors are clustered at the state.

Table 3: Difference-in-Differences Regressions for Log Usual Weekly Work Hours

	Full Sample			Hourly Workers		
	Unmatched	Matched	FE	Unmatched	Matched	FE
Subsidy	0.6326	0.6047 ***		1.1840**	1.1162***	
	[0.5621]			[0.5264]		
Post	-1.5205	-1.2629		-2.7486**	-2.0491	
	[0.9769]			[1.2794]		
Subsidy x Post	-1.4620***	-1.3978***	-0.9591**	-1.7461***	-1.7534***	-1.2350**
	[0.4086]		[0.4597]	[0.4875]		[0.4615]
Constant	56.3204***	45.8950***	37.1378	54.3877***	46.9255***	35.7710
	[9.9953]		[27.5482]	[11.2948]		[33.9219]
N	23183	23183	23183	19316	19316	19316

Notes: Sample is all single mothers who are eligible to receive a childcare subsidy at any point in the 11 months prior to a minimum wage change, age 18 to 45, who fulfill the common support requirement. Eligibility is determined by a combination of income, family size, and hours worked. Additional controls include a polynomial in age, education, race/ethnicity, receipt of other benefits, state fixed effects, time fixed effects, and a state specific linear time trend. Matching is performed using a kernel matching procedure. Significance levels: * 10%, ** 5%, *** 1%. Standard errors are clustered at the state.

Appendix A

One potential concern about our estimation strategy is that the composition of the sample changes when the minimum wage changes, resulting in a selection problem. Table B1 shows descriptive statistics of the sample of individuals who remain versus exit the sample following a minimum wage change by receipt of a childcare subsidy. To exit the sample, an individual must have zero work hours for the entire period following the minimum wage change and must remain in the SIPP. Table B1 shows that among those with a subsidy, those who exit the sample are slightly younger and more likely to be white. Among those without a subsidy, individuals who exit the sample are slightly younger and less likely to be white. However, the primary concern is not what the characteristics of those who leave the sample differ from those who remain, but instead whether those that receive a subsidy exit a different rate than those without a subsidy, all else equal. Table B2 address this question by estimating the difference in the probability of exiting the sample between those with and without a childcare subsidy prior to the minimum wage change, conditional on having been in the sample prior to a minimum wage change. The estimated coefficient on *CS* is small and statistically insignificant, indicating no evidence of a difference in the probability of exit, thus eliminating concerns over differences in compositional changes in the sample driving our results.

Table A1: Characteristics of Sample by Subsidy Receipt and Sample Exit

	Subsidy		No Subsidy	
	Remain	Exit	Remain	Exit
Means				
Wage	10.786		11.902	
	[3.335]		[5.033]	
Age	29.424	28.807	32.960	31.421
	[5.921]	[5.172]	[7.329]	[7.039]
Shares				
White	0.362	0.444	0.471	0.393
Black	0.462	0.317	0.272	0.364
Hispanic	0.125	0.205	0.220	0.188
Other	0.050	0.033	0.036	0.055
LTHS	0.155	0.196	0.117	0.217
HS	0.386	0.394	0.365	0.427
Some Coll.	0.458	0.411	0.518	0.356
N	1853	419	8335	1365

Notes: Sample is all single mothers who are eligible to receive a childcare subsidy at any point in the 11 months prior to a minimum wage change, age 18 to 45. Eligibility is determined by a combination of income, family size, and hours worked.

Table A2: Effect of Minimum Wage Change on Probability of Exiting Sample

	Full Sample	4 Months
CS	0.0099	0.0018
	[0.0146]	[0.0134]
Constant	0.1794	0.0939
	[0.1873]	[0.2109]
N	28852	22512

Notes: Sample is all single mothers who are eligible to receive a childcare subsidy at any point in the 11 months prior to a minimum wage change, age 18 to 45, who fulfill the common support requirement. Eligibility is determined by a combination of income, family size, and hours worked. Additional controls include a polynomial in age, education, race/ethnicity, receipt of other benefits, state fixed effects, time fixed effects, and a state specific linear time trend. Matching is performed using a kernel matching procedure. Significance levels: * 10%, ** 5%, *** 1%. Standard errors are clustered at the state.

Appendix B

Propensity score weighting requires a sample that exhibits common support. In other words, the range of propensity scores for the two groups must overlap completely in addition to the fact that propensity scores must be between 0 and 1 exclusive. The total number of observations in the matched sample is slightly smaller than the total number of observations that meet the sample selection criteria because of observations that do not fulfill the common support assumption.

Figure A1 shows the distribution of propensity scores for treatment and control groups estimated using Equation 2. Reflecting the differences in observable characteristics, the predicted likelilhood of receiving a subsidy for single mothers who do not receive a subsidy is generally lower than for single mothers who do receive a subsidy. The areas that do not exhibit common support are in the far left-hand portion of the distribution of propensity scores for single mothers without a subsidy, and the far right hand tail of single mothers who do receive a subsidy. These observations are dropped from all further analysis.

Figure B1: Estimate Propensity Scores by Subsidy Receipt