8-11-2015

Essays on Women's Employment and Children's Well-Being

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ABSTRACT

ESSAYS ON WOMEN'S EMPLOYMENT AND CHILDREN'S WELL-BEING

By

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August 2015

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This dissertation explores issues on women's employment and children's health in economics.

In chapter I, I investigate the causal effects of maternal employment on childhood obesity. Empirical analysis of the effects of maternal employment on childhood obesity is complicated by the endogeneity of mother's labor supply. A mother's decision to work likely reflects underlying factors – such as ability and motivation – that could directly influence child health outcomes. To address this concern, this study implements an instrumental variables (IV) strategy which utilizes exogenous variation in maternal employment coming from the youngest sibling's school eligibility. With data on children ages 7-17 from the 1979 cohort of the National Longitudinal Survey of Youth linked to the Child Supplement, I explore the effects of maternal employment on children's BMI z-score and probabilities of being overweight and obese. OLS estimates indicate a moderate association, consistent with the prior literature. However, the IV estimates show that an increase in mothers' labor supply leads to large weight gains among children, suggesting that not addressing the endogeneity of maternal employment leads to underestimated causal effects.

Chapter II examines the effects of Walmart Supercenters on household and child food insecurity. Walmart Supercenters may reduce food insecurity by lowering food prices and expanding food availability. Our food insecurity-related outcomes come from the 2001-2007 waves of the December Current Population Survey Food Security Supplement. We match these data to our hand-collected data of Walmart Supercenters at the census tract-level. First, we estimate a naïve linear probability model and find that households and children who live near Walmart Supercenters are more likely than others to be food insecure. Since the location of Walmart Supercenters might be endogenous, we then turn to instrumental variables models that utilize the predictable geographic expansion patterns of Walmart Supercenters outward from Walmart's corporate headquarters. The IV estimates suggest that the causal effect of Walmart Supercenters is to reduce food insecurity among households and children. The effect is largest among low-income families.

In the third paper, I investigate the effects of the Family and Medical Leave Act (FMLA) on women's labor market outcomes. The FMLA is a federal policy that aims to help workers balance job and family responsibilities. However, it may have unintended consequences on employment because it imposes costs on firms. In this study, I investigate the impact of the FMLA with labor market flows—i.e., hires, separations and recalls. Focusing on labor market flow outcomes is crucial to identifying the immediate impact of the policy because employment and wages adjust slowly when there is a policy change while labor market flows are flexible. Using data from the Quarterly Workforce Indicators and adopting a triple-difference model, I get results that are unlikely to be interpreted as causal because the data are insufficient to obtain precise estimates. However, the idea of using labor market flows can be easily applied to a broad range of topics relate to workplace mandates.

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**A Dissertation Submitted in Partial Fulfillment of the Requirements for the Degree
of
Doctor of Philosophy
in the
Andrew Young School of Policy Studies
of
Georgia State University**

**GEORGIA STATE UNIVERSITY
2015**

ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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August 2015

ACKNOWLEDGMENTS

I am heavily in debt of gratitude to many people for helping me along the way accomplishes this work. This dissertation is an intellectual product of group work. I appreciate the discussions and suggestions from many great professors and colleagues.

First, I cannot thank enough for my committees. They offered me generous help and mentorship that enabled me to successfully achieve my degree. Dr. Rusty Tchernis gave me innumerable advises on how to be a researcher. I am inspired by him to choose health field as my career pursuit. Dr. Barry Hirsch gave me useful feedbacks on my dissertation. He is always patient and supportive. I am amazed by his knowledge. I enjoyed Dr. Sara Markowitz's excellent teaching in health economics, which provided me the knowledge in the health field. I am grateful to Dr. Charles Courtemanche, who provided valuable suggestions. I would not be where I am today without his help.

Second, I want to thank my friends and colleagues at the AYSPS. My friends, Guanlin, Lily, Xiaoxue, Bo, Marietou, Fernando, Lorenzo, and Chris. We share so many happy moments. I also received countless assistances and guidance from Bess Blyler. She keeps me on track and jumps in whenever I meet difficulties. There are still many others I do not mention. I will cherish the unforgettable experiences at the AYSPS.

Finally, and most importantly, I want to thank for the support and unconditional love from my family. My husband, Yong, always be my side in good times and in bad. He tolerates me of my occasional moody times, and he encourages me when I doubt myself. His unwavering love is the bedrock of my life in the past six years.

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CHAPTER I: MATERNAL EMPLOYMENT AND CHILDHOOD OBESITY: EVIDENCE USING INSTRUMENTAL VARIABLES RELATED TO SIBLING SCHOOL ELIGIBILITY[1]

1. Introduction

The statistics on childhood obesity in the U.S. are alarming. From 1971 to 2010, the childhood obesity rate rose from 5 percent to 17 percent. The increase is especially notable among school-aged children, as the obesity rate quadrupled for children ages 6 to 11 and tripled for those ages 12 to 19, compared to doubling for those ages 2 to 5 (Fryar, Carroll, and Ogden, 2012). The prevalence of obesity among children is considered a major public health concern because of its immediate and long-term effects on health and well-being. According to the Centers for Disease Control and Prevention (CDC), obese children are at a higher risk of developing cardiovascular disease, prediabetes, bone and joint problems, sleep apnea, and psychological problems. Obese children are also likely to grow up as obese adults and therefore face the risk of adult obesity-related health problems.[2] Obesity imposes substantial costs on society. The annual medical costs of treating childhood obesity is $14.1 billion for outpatient costs and $237.6 million for inpatient costs. The estimated cost for treating obese adults is $147 billion per year. In addition, obesity is also associated with indirect costs such as unfavorable labor market outcomes.

Another remarkable trend in the second half of the 20th Century was increased employment among women. From 1960 to 2010, the labor force participation rate (LFPR) of women increased by more than 50 percent. A similar pattern is observed among mothers, as the LFPR for mothers with children under age 18 increased by 42 percent from 1975 to 2010 (U.S.

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[1] This research was conducted using restricted data from the Bureau of Labor Statistics. The views expressed in this paper do not reflect those of the BLS. Any errors are mine.
[2] http://www.cdc.gov/healthyyouth/obesity/facts.htm

Bureau of Labor Statistics 2014). The concurrent nature of these trends has led researchers to ask whether the rise in female labor force participation could have contributed to the increase in childhood obesity.

Theoretically, the effect of maternal employment on children's development is ambiguous. Maternal labor supply could benefit children via increased income, or adversely affect them due to reduced supervision and less time for home food production (Cawley and Liu, 2012; Duncan, Ziol-Guest, and Kalil, 2010; Fertig, Glomm, and Tchernis, 2009; Sztainer et al., 2003; Yeung, Linver, and Brooks-Gunn, 2002). This ambiguity underscores the need for empirical evidence when assessing the impacts of maternal work on outcomes such as childhood obesity.

Such empirical analyses, however, are complicated by the endogeneity of maternal employment. A mother's unobserved characteristics – such as general ability level, preferences, and motivation – likely affect both her labor supply and child care decisions. This creates the potential for biased estimates of the effects of maternal employment on child health outcomes. To address the endogeneity concern, this study implements an instrumental variables strategy. The approach is based on the idea that the opportunity cost of maternal employment is substantially reduced when the youngest child is attending school, potentially increasing a mother's labor supply. Cascio (2009) and Gelbach (2002) established that free public schooling has a significant effect on maternal employment. Mothers increase labor supply when their youngest child enrolls in public school. Mothers with younger children, however, exhibit no employment response when one of the older siblings starts attending school. Observational data also show that LFPR of women with older children has consistently been larger than that of women with younger children, with a particularly large jump occurring when the youngest child becomes eligible for kindergarten.[3]

Specifically, I use two instrumental variables for mother's labor supply. The first instrument is an indicator for the kindergarten eligibility of the mother's youngest child. Since some children may not enroll in full-time school until first grade, the second instrument is a binary variable indicating whether the youngest child is eligible for first grade. Data come from the restricted-access version of the National Longitudinal Survey of Youth 1979 Cohort (NLSY79), linked to the NLSY79 Child and Young Adult Supplement. The sample is restricted to school-aged children with younger siblings, with identification coming from the youngest sibling's school eligibility status. This ensures that the school eligibility of sample children is not changing during the sample period. Using the sample of school-aged children, I first find that mother's work hours are associated with positive but small increases in children's BMI z-score and probabilities of being overweight and obese. These results are consistent with much of the existing literature. I then implement the instrumental variables strategy in an attempt to obtain causally interpretable evidence. The results from the IV estimates, both with and without child fixed effects, suggest that an increase in mother's work leads to much larger weight gains among children than those suggested by the naïve specification without instruments. The point estimates indicate that a mother working ten more hours per week significantly increases the BMI z-score of her children by 0.07-0.35 points and the probability of being overweight by 4-6 percentage points. The point estimates for obesity are around 5-8 percentage points. These estimates are between two and twenty-seven times larger than the corresponding OLS results. Prior studies may therefore have underestimated the extent to which the rise in female labor force participation has contributed to the childhood obesity epidemic.

[3] See U.S. Bureau of Labor Statistics (2014) and Morrill (2011).

2. Literature review

Some important changes occur when mothers increase their labor supply that could influence childhood obesity. Research finds that working mothers cook less than other mothers, and that their children eat more away-from-home meals. Working mothers also spend less time doing physical activities with children. Their children have less supervision and watch TV much longer (Fertig, Glomm, and Tchernis, 2009; Cawley and Liu, 2012). Although there is some evidence of offsetting behavior by fathers, it is not nearly enough to offset the changes by mothers (Cawley and Liu, 2012).

Accordingly, several studies document a positive relationship between maternal employment and childhood weight problems. Seminal work by Anderson et al. (2003) measures the effect of a mother's work over the lifetime of a child on childhood obesity. They find no evidence that a mother's number of weeks worked is associated with the probability a 3-11 years old child is obese. They do, however, find evidence of an effect on the intensive margin: an extra ten hours per week during weeks worked increases the probability of a child being obese by around one percentage point (with the exact magnitude varying slightly across specifications). The impact is largest for higher socioeconomic status (SES) children.

Subsequent studies also indicate positive associations between mothers' labor supply and childhood obesity. Ruhm (2008) finds that an extra 20 hours of weekly employment is predicted to raise obesity (overweight) by 1.6-2.7 (3-4.5) percentage points. Courtemanche (2009) estimates that a mother working 10 additional hours per week increases a child's probability of being obese by 1.6 percentage points, while a mother's spouse's work hours have no effect. Fertig et al. (2009) find that a 10% increase in mothers' work hours is associated with approximately a 1.6 percentage point rise in the probability a child is obese. Liu et al. (2009)

show that full-time working mothers raise a child's BMI by about 0.581 units and probability of being obese by 12.3%. Morrissey et al. (2011) and Morrissey (2012) estimate that every period[4] a mother is employed is associated with increases in her child's BMI Z-score of 0.02 and 0.03, respectively.

While the existing literature has therefore provided robust evidence of a small positive association between maternal work and childhood obesity, the extent to which these associations reflect causal effects remains an open question. For instance, if ambitious, highly-skilled women are both more likely to enter the labor force and to carefully monitor their children's eating and exercise habits, the estimated effect of maternal work on childhood obesity would be understated. On the other hand, if entering the labor market reflects an underlying preference for income versus family time, the estimate could be overstated. Reverse causality is another possible concern. Having a child with health problems may cause a mother to either exit the labor force to care for the child or enter the labor force to obtain health insurance or extra income. Measurement error in reported work hours is another potential source of bias. This would attenuate the estimated effects if the amount of reporting error is random. Some of the papers in the literature have implemented panel data methods to control for unobserved heterogeneity (Anderson et al., 2003; Courtemanche, 2009; Miller, 2011; Morrissey et al., 2011), but these methods do not account for time-varying sources of bias, reverse causality, or measurement error. Anderson et al. (2003) also estimated an IV specification, using as instruments state-level variables including unemployment rate, child care regulations, wages of child care workers, welfare benefit levels, and the status of welfare reform in the state. However, these instruments were relatively weak in terms of their predictive power on maternal work.

[4] A period refers to a data collection period. If a mother answers affirmatively for her employment status during an interview, she is considered as employed for a period.

They also relied on questionable exclusion restrictions, as the instruments could influence childhood obesity through pathways besides maternal work.[5] Further investigation is therefore necessary.

I contribute to the literature by implementing an IV method that aims to produce causally interpretable evidence on the effect of maternal work on childhood obesity. My approach exploits plausibly exogenous variation in maternal labor supply coming from the timing of school eligibility of the youngest child of a mother. The idea behind the instruments comes from Morrill (2011). She uses a binary indicator for kindergarten eligibility of the youngest child in the family to instrument for maternal labor supply. Her outcome variables are acute health conditions that could arise when mothers are working, including hospitalizations, asthma episodes, injuries, and poisonings. She finds that maternal employment substantially increases the risk of all these health incidents. In contrast, I focus on a chronic health condition: childhood obesity.

3. Methodology

The effect of maternal employment on childhood weight status can be estimated by the following model for child i in year t:

$$WeightStatus_{it} = \alpha + \beta MLS_{it} + \gamma X_{it} + \tau_t + \sigma_s + \varepsilon_{it} \qquad (1)$$

where children's weight status is the outcome of interest, MLS is maternal labor supply, X is a set of demographic characteristics of both the child and mother, and τ and σ represent year and state fixed effects. The coefficient of interest is β, which indicates the effect of maternal employment on children's weight status.

[5] For instance, unemployment rate and the generosity of a state's welfare program could be associated with changes in household disposable income or wealth even if mothers' work hours do not change, and this in turn could affect children's weight.

Estimating this reduced form equation may generate inconsistent results due to the endogeneity problem of maternal labor supply. Abilities, preferences, and motivation of a mother could affect both her work decision and her child care quality. If maternal employment reflects these unobserved factors, the sample of working mothers may not be a random selection of mother population. As a first attempt to address this issue, I add child fixed effects to equation (1). Fixed effects will account for time-invariant unobservables, but they do not correct for time-varying unobservables, reverse causality, or measurement error in the independent variable of interest. Without addressing all sources of endogeneity bias, estimates of β do not reflect the causal effect of maternal employment on child obesity.

A possible solution to the problems stated above is IV estimation. In this study, I use two instrumental variables for maternal employment, both of which are constructed based on the age of the youngest child of a mother. The first one is a binary variable indicating whether the youngest child is eligible for public kindergarten. In the U.S., children age five or older are eligible to attend free public kindergarten. Kindergarten eligibility of the youngest child substantially reduces the opportunity cost of a mother's working, providing an incentive for the rise in maternal employment. This instrument captures a particular discrete jump in maternal labor supply that is documented by Morrill (2011). However, there is a reason to suspect that the increase in maternal labor supply from children reaching school age does not entirely occur at kindergarten. In many states, public kindergarten is either not mandatory or only lasts a half day. As of 2012, only 16 states mandated kindergarten attendance among age-eligible children, and only 12 states were required to offer a full-day kindergarten program while another 34 states required half-day kindergarten.[6] It is therefore possible that another discrete jump in maternal labor force participation or working hours occurs when the youngest child becomes eligible for

[6] http://nces.ed.gov/programs/statereform/tab5_3.asp

first grade. According to the National Center for Education Statistics, In 1980s and 1990s, although 85-89 percentage of age-eligible children enrolled in kindergarten, only 30-50 percentage were enrolled full-day.[7] Therefore, for a fair amount of mothers first grade might be the time at which the youngest child is able to attend school for a full day. I therefore use as a second instrument a binary variable equals to the eligibility for the first grade of the youngest child.[8]

A child's public schooling has been found to have significant predictive power on maternal labor supply (Cascio 2009; Fitzpatrick, 2012; Gelbach, 2002; Morrill, 2011). However, children's actual school enrollment age is endogenous because it is determined not only by state legislation but also by parents' willingness to enroll their children, as "redshirting" is popular (Anderson et al., 2011). Therefore, the predicted school enrollment age, instead of the actual school attending age, is a preferred instrument since it provides an intent-to-treat effect for maternal labor supply.

Using these instruments requires the assumption that they do not affect children's weight through any pathways other than maternal labor supply. An obvious concern is that the change in school eligibility of a youngest child may have a direct effect on his/her own health (Anderson et al., 2011; Zhang and Zhang, 2011). To improve the validity of the instruments, I therefore restrict the sample to the older children (aged 7 to 17) in the family whose school eligibility status remains the same when their youngest sibling enters school. The IV estimator evaluates the local average treatment effect (LATE) of maternal employment for school-aged children whose mother's employment was previously constrained by child care responsibilities.

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[7] https://nces.ed.gov/programs/digest/d14/tables/dt14_202.10.asp

[8] Another discrete jump could occur with pre-kindergarten, which serves 4 years old children. However, unlike kindergarten and first grade, pre-kindergarten is not free and not universally available in many states. Cascio (2009) and Fitzpatrick (2010) find limited impact of pre-kindergarten on women's labor supply.

Identification of the instrumental variables estimation comes from variations in the date of birth (DOB) of the youngest child and the state-specific cutoff date for kindergarten and first grade eligibility. The first source of variation means that two otherwise identical children have mothers facing different incentives to work as a result of the age of the youngest sibling. One child's mother may be working since the youngest sibling is six years old while the other child's mother may not be working because the youngest sibling is four years old. Even if the two children have youngest siblings of the same age, there could be variation in public school eligibility if they live in different states. In the U.S., individual states have the authority to determine the cutoff dates by which a child must have turned 5 (6) years old in order to be eligible to enroll in kindergarten (first grade). Assume two youngest siblings both born on September 15th 2005, but one of them lives in Georgia and one lives in Louisiana. They will turn 5 years old on September 15th 2010. However, the one in Louisiana can go to kindergarten in 2010 because he/she fulfills the age requirement before the September 30th cutoff; while the one in Georgia has to wait for the next year since the child is still four years old on the cutoff date of September 1st in 2010. The cutoff dates of school-year vary across states and also change during the sample period. The combination of the DOB and cutoff dates therefore generates substantial variation.

The IV model takes the following form:

$$MLS_{it} = \alpha_{FS} + \delta_{FS} Z_{it} + \gamma_{FS} X_{it} + \tau_{FSt} + \sigma_{FSs} + \upsilon_{it} \qquad (2)$$

$$WeightStatus_{it} = \alpha_{SS} + \beta_{SS} \widehat{MLS}_{it} + \gamma_{SS} X_{it} + \tau_{SSt} + \sigma_{SSs} + \varepsilon_{it} \qquad (3)$$

Equation (2) gives the first stage regression, with coefficients denoted by the FS subscript, while equation (3) is the second stage regression, with coefficients having the SS subscript. Z_{it} indicates the two instrumental variables. I estimate the model both with and without child fixed effects, which I refer to as the IV-FE specification and the IV specification, respectively. The IV-FE model goes further than the IV model toward accounting for endogeneity bias, as it controls for possible unobserved heterogeneity in fertility patterns. However, the IV model is likely more efficient than the IV-FE specification. We therefore present and discuss results from both models.

4. Data and descriptive statistics

Data come from the restricted version of the National Longitudinal Survey of Youth 1979 Cohort (NLSY79). The original sample of the NLSY79 contains 12,682 individuals, half male and half female, who were between the ages of 14 and 21 in 1979. These individuals were followed annually from 1979 to 1994 and then biannually through 2010. Starting in 1986, a supplemental survey, the NLSY79 Child and Young Adult (NLSCYA), was conducted biannually. It includes assessments of all biological children of the original female participants. Information was collected from either the mother or the child[9], and it can be linked to the main NLSY79 through the mother's identifier. I use the matched mother-children records from all waves in which both the NLSY79 and the NLSCYA are available, i.e. biannually from 1986 to 2010.

There are three outcomes of interest, BMI z-score (BMIZ) and indicators for overweight and obesity, all of which are constructed based on children's BMI[10]. BMI is a commonly-used proxy for body fat in adults. However, it is not a proper measure for adiposity in children due to biological reasons. Therefore, the CDC suggests using BMIZ instead of BMI for children. BMIZ is a standardized measure of BMI using age-and-gender specific BMI distributions from the 2000 growth chart (National Center for Health Statistics, 2002). The main advantage of BMIZ is

[9] Before 1994, a mother reported information for all her children regardless of the age of children. After 1994, children above 15 years of age answered interview questions by themselves.
[10] BMI is calculated as weight in kilograms divided by height in meters squared (kg/m^2).

that it is comparable across age and sex. The other two outcomes are also computed according to the 2000 growth chart. If a child's BMI is above the 85th (95th) percentile of the BMI distribution of the corresponding reference population, the child is considered overweight (obese).

Maternal employment is computed using the week-by-week array of the complete work history for each participant. The longitudinal work history data in the NLSY79 include an indicator for whether the participant is working and the number of working hours in each week from January 1, 1978 to the latest interview date[11]. Utilizing work history data, I construct employment in a retrospective manner during two time spans[12]: a month (4 weeks) before the interview and a year (52 weeks) prior to the interview. In each time span maternal employment is assessed by two variables: the fraction of weeks employed[13] and the average hours worked per week. There are therefore four measures of maternal labor supply, and I use each of them separately in a single regression.[14] Using one-month labor supply variables captures the immediate impact of maternal employment on child weight, while using one-year variables captures a slightly more cumulative effect. Some papers in the literature estimate an even more cumulative effect by measuring maternal work as average work hours over the course of the child's entire life (e.g. Anderson et al., 2003; Courtemanche, 2009). My IV strategy – which relies on sharp discontinuities at a particular age – is inherently better suited for the identification

[11] See the NLSY79 codebook supplement for more information: http://www.bls.gov/nls/79quex/r19/y79r19append18.pdf

[12] The NLSY79 provides two sets of ready-to-use employment measures: employment since date of last interview and employment in last calendar year. Despite that these measures are easily accessible, they are not able to capture the abrupt change in employment at the point when the youngest child is eligible to kindergarten. In addition, some participants may have skipped one or more waves of interview, thus result in imprecise measurements of employment since date of last interview.

[13] For example, if a mother works 3 weeks in the month before the interview, then she works 75 percent of the time. The advantage of using the fraction of weeks worked, in contrast to using the number of weeks worked, is that it makes the scale of the variables comparable across the two time spans.

[14] I considered including two or more of these measures together in the same regression, but I was unable to find a set of instruments that was strong enough to precisely identify the impacts of multiple endogenous variables.

of short-run rather than long-run impacts. It is therefore noteworthy that, despite this limitation, I will still obtain much larger effects than these prior studies.

A key component to this analysis is the use of instrumental variables. The restricted NLSY79 has not only the DOB of all children but also their states of residence. I first calculate the age in weeks for the youngest child in each household. Then the first IV, an indicator for whether the youngest sibling is eligible for kindergarten, is given by whether the youngest child reaches 260 weeks of age (5 years old) before his/her state of residence's cutoff date for the school year. State cutoff dates come from Evans et al. (2010). The second IV, an indicator for first grade eligibility, equals one if the youngest child is 312 weeks of age (6 years old) before the cutoff date.

I also utilize the extensive information available in the NLSY79 to include a set of demographic control variables. These include gender, race/ethnicity (Hispanic, African American, and others[15]), whether the child had a high birth weight (> 8.8 pounds)[16], whether the child was breastfed, child's age in years, family income, and family size (one to three persons, four persons, and five or more persons). Regressions also control for the mother's age, the mother's AFQT score, education level of the mother (less than high school, high school graduate, some college, and college degree or higher education), and whether the mother is married and lives with a spouse. I also use limited information available in the NLSCYA for father to construct an indicator for close child-father attachment (equals one if father lives in the household, father lives within 10 miles, or child has seen father at least once a week in the past year). In addition, self-reported data on height and weight usually suffer from systematic reporting error that leads to underestimation of the prevalence of obesity (Goodman, Hinden, and

[15] Other races include those who identify themselves as nonblack/non-Hispanic.
[16] The threshold of high birth weight can be found http://www.cdc.gov/pednss/what_is/pednss_health_indicators.htm

Khandelwal, 2000; Kuczmarski, Kuczmarski, and Najjar, 2001). I thus add indicators for whether height or weight are self-reported[17] in all specifications.

The regression sample is restricted to children who meet three criteria: 1) they are seven to seventeen years old, 2) they have at least one younger sibling, and 3) their youngest sibling's schooling eligibility changed during the sample period. The first restriction ensures that school eligibility does not change for the sample children. As discussed above, this eliminates the obvious concern that one's own school eligibility could affect one's own weight for reasons other than maternal employment. The second restriction is necessary for the use of IVs. The third restriction is used to eliminate those whose mothers never receive the treatment induced by the IVs. I name the resulting sample the "main sample".[18]

Since employing several sample restrictions raises obvious concerns about external validity, I also prepare two alternative samples with fewer constraints. One alternative is called the "extended sample". This sample only restricts the age of the focal children; it does not employ the second and third restrictions. The other alternative, named the "comparison sample", is the same as the extended sample but only includes 1986 to 1996 – the years used by Anderson et al. (2003). This enables assessment of whether differences in our results and theirs could simply be attributable to the longer sample period.

The main sample includes 16,535 observations from 4,119 children of 2,575 mothers. Table 1 presents descriptive statistics for the main sample, with all estimates weighted by the

[17] The interview questions concerning the measurements of height and weight change frequently during the research period. There were only two modes at the beginning: mother report and interviewer measure. The questions evolved gradually and eventually there are four options: mother report, child report, interviewer measure, and others. In addition, young adults who are above age 15 were interviewed independently since 1996. Their height and weight data are all self-reported. To simplify the classifications, I create an indicator which represents all modes except interviewer measure for height and weight data and call it self-reported mode.

[18] Other steps to improve data quality include dropping children with extreme BMIs (z-score exceeding +/-5), children who have shrinking height from the previous interview, children who do not live with their mothers, female children who are pregnant or ever have given birth, and those whose mothers do not have any valid employment data.

children's sampling weights. The average BMI z-score is 0.29, and 27% of the children are overweight while 12% of them are obese. On average, the fractions of weeks that mothers worked in the past month and in the past year are both 65%, equivalent to 2.6 weeks and 33.8 weeks, respectively. The average hours worked per week is around 23.2, both for employment measured in a month and in a year. One concern about the data quality is for family income. In the sample about 14% of children do not have income data. I impute missing values of income as the sample mean, and I add an indicator for observations if income is imputed in all regressions to mitigate bias.

The extended sample has a larger sample size of 34,939 observations, and the comparison sample has a smaller size of 15,602 observations. Table 18 in Appendix A shows the summary statistics for the main, extended, and the comparison samples. The key characteristics in the two alternative samples are similar to those from the main sample. The extended sample has a BMIZ of 0.35 and 29% (13%) overweight (obese) children, while the comparison sample has a BMIZ of 0.24 and the same proportions of overweight and obese children as the main sample. Other minor differences among the three samples are as expected given the nature of the restrictions. For instance, the education level of mothers in the comparison sample is lower because these mothers were still young during 1986 and 1996 and some of them had not graduated from school yet.

5. Results

5.1 OLS estimates

Table 2 presents the results from estimating the associations between maternal employment and the child weight outcomes using each of the three samples. A single cell reports the coefficient estimate for maternal employment from separate OLS regressions. In other words, the four maternal employment variables are *not* included together in the same regression, in order to retain comparability with the subsequent IV estimates.[19] The first column uses the main sample, the second column utilizes the extended sample, and the third column uses the comparison sample. As discussed previously, all specifications include year and state fixed effects as well as children's and mothers' demographic controls. Estimates are weighted using the child sampling weights. Standard errors are heteroskedasticity-robust and clustered by mothers.

The results in Table 2 indicate a positive and significant relationship between the labor supply of mothers and the weight status of children across different specifications. In the main sample, ten more work hours per week raise children's BMIZ by 0.03-0.04 points and the likelihood of being overweight (obese) by 0.5-0.8 (0.3-0.4) percentage points. The weeks worked is also positively and significantly related to weight outcomes. Since it is measured as the faction of weeks a mother worked in a month or in a year, the point estimates for the one-month and one year measures are comparable. If a mother works one additional week (25 percent of her time) in a month, that will increase her children's BMIZ by 1.6 points and the probability of being overweight (obese) by 0.2 (0.21) percentage points. Similarly, if a mother works one additional month (8 percent of her time) in a year, her children will have 0.77 points of higher BMIZ and 0.14 (0.1) percentage points of higher likelihood of being overweight (obese). This positive and significant association between maternal employment and children's weight status persists across the three samples. In short, Table 2 suggests that my results are not likely to differ from those obtained by other studies (particularly Anderson et al., 2003) merely because of differences in the sample. I therefore only use the main sample throughout the duration of the paper.

[19] I also tried probit models for outcomes overweight and obesity. The results are close to the OLS estimates in terms of magnitude and significance.

5.2 IV estimates

Using the main sample, Table 3 evaluates the sensitivity of the results to the inclusion of child fixed effects and the use of IV. The first column in Table 3 simply reprints the first column of Table 2, which reports the coefficient estimate from the OLS regression with the main sample. Column (2) adds the child fixed effects. Interestingly, accounting for time-invariant unobserved heterogeneity eliminates most of the significant associations between the maternal employment variables and child weight outcomes. However, the fixed effects models likely do not reveal causal effects, as concerns about time-variant omitted variables, reverse causality, and measurement error still remain.

Column (3) reports the second-stage results of the IV model without child fixed effects. The coefficients for all maternal employment measures are positive and large in magnitude, although insignificant for BMIZ. A mother's additional week (25 percent of her time) employed in the past month increases a child's P(Overweight) by 4 percentage points, and P(Obese) by 4.5 percentage points. Ten more work hours per week in the past month increases P(Overweight) by 4.7 percentage points and P(Obese) by 5.2 percentage points. One more month (8 percent of her time) worked in the past year raises P(Overweight) by 1.2 percentage points and P(Obese) by 1.4 percentage points. An additional ten hours worked per week over the past year increases P(Overweight) by 4.4 percentage points and P(Obese) by 4.9 percentage points. Compared to the OLS estimates without child fixed effects, the IV estimates are 2 to 4 times larger when the outcome is BMIZ and 6 to 21 times larger when the outcome is overweight or obesity. Despite the fact that the standard errors rise considerably when using IV, IV estimates for overweight and obesity are significant at the 5% level or better. The IV estimates for BMIZ are insignificant because of the large standard errors, but their magnitudes still suggest that the corresponding

OLS estimates are likely conservative. In short, the results using IV without child fixed effects suggest that maternal employment causes children to be overweight and obese, and the effect is larger than documented in the naïve OLS estimates.

Column (4) presents the IV-FE estimates. This model goes the furthest toward addressing causality, as it addresses endogeneity bias in two ways: FE to eliminate time-invariant unobserved heterogeneity and IV to address remaining issues with time-variant omitted variable bias, reverse causality, or measurement error. The second-stage coefficient estimates for BMI z-score, overweight, and obesity are all positive and large. A mother's additional week (25 percent of her time) employed in the past month increases a child's BMIZ by 0.29, P(Overweight) by 6 percentage points, and P(Obese) by 7 percentage points. An additional ten hours per week in the current month raises BMIZ by 0.35, P(Overweight) by 5.5 percentage points, and P(Obese) by 7.7 percentage points. One more month (8 percent of a mother's time) worked in the past year increases BMIZ by 7.4, P(Overweight) by 0.019 percentage points, and P(Obese) by 0.02 percentage points. An increase of ten hours per week over the past year raises BMIZ by 0.26, P(Overweight) by 4.3 percentage points, and P(Obese) by 5.8 percentage points. The IV-FE standard errors are even larger than those using IV without FE, nonetheless the effects on BMIZ and obesity are all significant at the 10% level or better. The magnitudes of the IV-FE estimates are between 14 and 157 times larger than those of the corresponding FE estimates without IV. Compared to the IV estimates without child FE, the IV-FE estimates are roughly four times as large when BMIZ is the outcome, similarly sized when overweight is the outcome, and about half larger when obesity is the outcome. Therefore, there is clear evidence that implementing the IV approach increases the estimated effects, but it is not clear whether adding FE to the IV model makes a meaningful difference. Importantly, the IV-FE estimates are 3 to 15 times larger than those from the prior studies that used comparable specifications[20] (Anderson et al., 2003; Courtemanche, 2009; Ruhm, 2008). In short, these comparisons suggest that insufficiently addressing the endogeneity problem of maternal labor supply leads to underestimation of its causal effect of child weight.

5.3 Validity of IVs

Table 4-6 present tests of the key assumptions required by the IV approach. First, the IV model requires that the IVs influence the endogenous labor supply variables. Table 4 therefore presents the first-stage coefficient estimates of interest from the IV and IV-FE models, along with the F statistic from a test of the joint significance of the instruments. The first-stage F-statistic is much larger than the standard criteria of 10 in most specifications. The instruments therefore have sufficient predictive power to rule out a weak instrument problem. Turning to the individual coefficient estimates, in the IV model, the binary instrument for kindergarten eligibility is only significant in one of the four maternal employment specifications (fraction of weeks employed in the past year), whereas the indicator for first grade eligibility is strongly significant in all specifications. In the IV-FE model, the indicator for kindergarten eligibility has strong significant coefficients in all specifications, while the indicator for first grade eligibility has significant coefficient only for fraction of working weeks.

The IV strategy also assumes that the instruments can be excluded from the second stage regression; i.e. they only influence child weight through their effect on maternal work, conditional on the controls. Table 5 presents the Hansen J statistics from one test of this assumption: the over-identification test. Each cell represents p-values from a separate over-identification test, where the null hypothesis is that the set of instruments is valid. The p-values

[20] Anderson et al. (2003) find that 10 hours of additional work every week increase children's probability to be obese by 1 percentage point. Courtemanche (2009) and Ruhm (2008) document a 2 percentages points of magnitude.

are all over 0.3 in the IV specifications. The p-values are never below 0.1 in the IV-FE models. The second test for this condition is to regress both the endogenous variable and the IVs in the same regression. Table 6 shows results from this test, with obesity for the outcome variable. While controlling for maternal employment, the coefficients for both IVs are never significant. In other words, I do not find clear evidence that the set of instruments is invalid.

5.4 Subgroup analysis

In this section, I explore the heterogeneous effects of maternal employment on child weight status for subgroups, with obesity as the outcome variable. I divide the main sample based on children's gender, race, poverty status, and mother's marital status. Each cell of Table 7 reports a coefficient coming from a separate regression. I only use the IV specifications because insufficient subgroup sample size prevents the IV-FE models from producing precise estimates.

The first column of Table 7 reprints the IV estimates in Table 3 for reference. Columns (2) and (3) record the results for girls and boys. Maternal employment has a significant and large impact on girls' obesity. If a mother works 10 more hours every week in the past year, her girl children are 7.7 percentage points more likely to be obese. The estimates for boys are small in magnitude and insignificant. Column (4)-(6) disaggregate the main sample by race/ethnicity. Among Hispanics, blacks, and those of other races/ethnicities, maternal employment has significant and substantial effects on children in other races (such white, Asian, etc.). If a mother works 10 additional hours every week in the past year, her white children will be 6.5 percentage points more likely to be obese. However, the effects of maternal employment on Hispanic and black children are small and insignificant. The effects are even negative in sign for the latter.

Column (7) and (8) present the results for poor and non-poor children. The NLSY79 classifies a family as in poverty if family income over the past year below the family size adjusted poverty level. Because some participants do not report their financial status, the subsample analysis only applied to those who have valid data for poverty status[21]. Compared to poor children, maternal employment has significant effects on children from non-poor families. The last two columns reports results for children with married mother and non-married mothers. If a mother is married and live with spouse, her employment will have a significant effects on her children's obesity. Results documented in these subgroup analysis consistent to findings documented in other papers. Anderson et al. (2003) and Ruhm (2008) also find that maternal employment has a larger deleterious impact on children from high socioeconomic status families.

6. Conclusion and discussion

This paper explores the causal effect of maternal employment on child weight status. With panel data on children age seven to seventeen from the NLSY79, I first replicate previous research by demonstrating a small, positive association between maternal employment and childhood obesity. Then, I use kindergarten and first grade eligibility as instruments for maternal employment. IV and IV-FE models suggest that maternal employment increases children's BMI z-score and probabilities of being overweight and obese. The effects are much larger once the endogeneity of labor supply is addressed, and they are also larger than those previously estimated in the literature.

The existing literature has documented a small positive association between maternal work and childhood obesity. However, these studies either do not address the endogeneity of maternal labor supply or only partially account for unobserved heterogeneity. Prior papers on this topic urged further investigation with instruments for maternal employment which are valid and have strong predicting power (Anderson et al., 2003; Cawley and Liu, 2012; Ruhm, 2008). I

[21] Around 2,500 observations have missing data for poverty status, thus are excluded from this group of subsample analysis.

therefore utilize an IV strategy that exploits plausibly exogenous variation in maternal labor supply coming from the timing of school eligibility of the youngest child of a mother. My findings contribute to the literature by adding causally interpretable evidence that maternal employment raises the risk of children having unfavorable weight outcomes. However, the design of the IV strategy requires that only children who are school-aged with siblings are included in the sample. Therefore, the results presented here are not necessarily applicable to all children. Further investigation is also needed to explore how the effects might vary by subgroups, such as by gender, race, socioeconomic status, and by father's work status.

The results, however, should not be interpreted as discouraging women's labor supply, or as claiming that mothers' employment has had a negative net impact on society. My findings instead highlight the importance of further investigation into the mechanisms through which maternal employment might affect children's health. Possible mechanisms include the changes in family routine, diet, and time allocation induced by mothers' labor supply. Maternal employment is likely to reduce beneficial routines, such as regular family meals and physical activities with children. At the same time, maternal employment might lead to unhealthy routines, such as television watching and restaurant meals. Prior research has found associations between maternal employment and time use (Cawley and Liu, 2012; Fertig et al, 2009), but causal evidence – perhaps using the identification strategy from this paper – is needed.

Another possible mechanism is the child care setting. For example, if child care subsidies, such as the child care and development fund (CCDF), encourage working mothers to rely on center-based child care service, the use of non-parental child care may influence children's diet and activity to some extent (Blau and Tekin, 2007; Herbst and Tekin, 2010). In addition, the availability of relative care, especially from grandparents, has substantial positive effect on mothers' labor supply (Compton and Pollak, 2014). Grandparents may put fewer restrictions on their grandchildren's diet and activities (Maher et al., 2008), thus increasing the risk of children being obese.

Understanding the mechanisms of the effects of maternal employment on childhood obesity is not only of academic interest, but it would also shed light on policies to help reverse the obesity epidemic. For example, if supervision is an important mechanism, promoting after-school programs would be a beneficial policy. Such programs not only increase children's physical activity level directly, they also help children to form healthy habits and promote health education among parents (Annesi et al., 2007; Annesi, Moore, & Dixon, 2008). Alternatively, if nutrition is the main mechanism, policies related to food labeling (Bollinger, Leslie, and Sorensen, 2010; Tandon et al., 2010) and quality of school meals (Foster et al., 2007; Story, Nanney, and Schwartz, 2009) could have a strong effect. Understanding the relative impact of each of the mechanisms would be the first step to inform policy makers.

7. Tables

Table 1 Summary Statistics, all estimates are weighted using the child's sampling weight.

	Main Sample (N=16535)			
	Mean	SD	Min.	Max.
Children's Info				
BMI z-score	0.29	1.15	-4.98	3.12
Overweight	0.27	0.44	0	1
Obesity	0.12	0.33	0	1
Height is self-report	0.51	0.5	0	1
Weight is self-report	0.52	0.5	0	1
Family size, less than 3 persons	4.86	1.28	1	15
Family size, 4 persons	0.09	0.28	0	1
Family size, 5 or more persons	0.36	0.48	0	1
Family income, $1000	60.58	78.21	0	974.1
Child's age, in year	12.02	3.09	7	17
Child is Hispanic	0.08	0.27	0	1
Child is African American	0.15	0.36	0	1
Attachment to father	0.77	0.41	0	1
Child is female	0.49	0.5	0	1
High birth weight	0.1	0.3	0	1
Breastfed	0.57	0.48	0	1
Mother's Info.				
Edu.,less than high school	0.12	0.32	0	1
Edu.,high school graduate	0.44	0.5	0	1
Edu.,some college	0.23	0.42	0	1
Edu.,college degree or higher	0.21	0.41	0	1
Mother's age	36.64	5.42	21	53
AFQT score (2006 standard)	47.64	28.22	0	100
Married, live with spouse	0.73	0.44	0	1
Mothers' Employment				
Fraction of weeks worked in past month	0.65	0.46	0	1
Hours worked per week in past month, in unit 10	2.32	2	0	9.6
Fraction of weeks worked in past year	0.65	0.43	0	1
Hours worked per week in past year, in unit 10	2.32	1.9	0	9.6
IV				
Binary if the yst child is eligible for kindergarten	0.53	0.5	0	1
Binary if the yst child is eligible for first grade	0.46	0.5	0	1

Table 2 OLS estimates of the impact of maternal employment on children' weight status (robust standard errors in parentheses)

Outcome	Time span	Maternal emp.	(1) Main (N=16535)	(2) Extended (N=34939)	(3) Comparison (N=15602)
BMI Z-score	Past month	Fraction of weeks worked	0.062**	0.049***	0.054**
			(0.024)	(0.017)	(0.027)
		Hours worked per week	0.026***	0.021***	0.021***
			(0.0055)	(0.0038)	(0.0060)
	Past year	Fraction of weeks worked	0.096***	0.084***	0.093***
			(0.027)	(0.019)	(0.029)
		Hours worked per week	0.036***	0.032***	0.030***
			(0.0058)	(0.0041)	(0.0063)
Overweight	Past month	Fraction of weeks worked	0.0068	0.0032	0.0056
			(0.0091)	(0.0066)	(0.0097)
		Hours worked per week	0.0045**	0.0031**	0.0036
			(0.0021)	(0.0015)	(0.0022)
	Past year	Fraction of weeks worked	0.017*	0.017**	0.016
			(0.0098)	(0.0072)	(0.011)
		Hours worked per week	0.0074***	0.0069***	0.0052**
			(0.0022)	(0.0016)	(0.0023)
Obesity	Past month	Fraction of weeks worked	0.0077	0.0087*	0.012*
			(0.0066)	(0.0049)	(0.0068)
		Hours worked per week	0.0029*	0.0037***	0.0043***
			(0.0015)	(0.0011)	(0.0016)
	Past year	Fraction of weeks worked	0.012*	0.012**	0.019**
			(0.0070)	(0.0053)	(0.0074)
		Hours worked per week	0.0044***	0.0053***	0.0055***
			(0.0016)	(0.0012)	(0.0017)

Note: hours worked per week is in unit 10.

Table 3 IV estimates with/without child FE of the impact of maternal employment on children' weight status (robust standard errors in parentheses)

Outcome variable	Time span	Maternal emp.	(1) OLS	(2) FE	(3) IV	(4) IV-FE
BMI Z-score	Past month	Fraction of weeks worked	0.062**	0.0074	0.24	1.16**
			(0.024)	(0.024)	(0.24)	(0.53)
		Hours worked per week	0.026***	0.0039	0.069	0.35**
			(0.0055)	(0.0057)	(0.072)	(0.16)
	Past year	Fraction of weeks worked	0.096***	0.049*	0.25	0.93**
			(0.027)	(0.028)	(0.24)	(0.38)
		Hours worked per week	0.036***	0.016**	0.066	0.26**
			(0.0058)	(0.0065)	(0.067)	(0.11)
Overweight	Past month	Fraction of weeks worked	0.0068	-0.0052	0.16*	0.24
			(0.0091)	(0.0097)	(0.095)	(0.20)
		Hours worked per week	0.0045**	-0.0024	0.047*	0.055
			(0.0021)	(0.0023)	(0.028)	(0.057)
	Past year	Fraction of weeks worked	0.017*	0.011	0.15	0.15
			(0.0098)	(0.011)	(0.092)	(0.15)
		Hours worked per week	0.0074***	0.0016	0.044*	0.043
			(0.0022)	(0.0027)	(0.026)	(0.041)
Obesity	Past month	Fraction of weeks worked	0.0077	0.0042	0.18**	0.29*
			(0.0066)	(0.0071)	(0.071)	(0.16)
		Hours worked per week	0.0029*	-0.0014	0.052**	0.077*
			(0.0015)	(0.0017)	(0.021)	(0.046)
	Past year	Fraction of weeks worked	0.012*	0.014	0.17**	0.21*
			(0.0070)	(0.0085)	(0.068)	(0.11)
		Hours worked per week	0.0044***	0.00080	0.049**	0.058*
			(0.0016)	(0.0020)	(0.020)	(0.032)

Note: hours worked per week is in unit 10.

Table 4 First-stage estimate of the IV and the IV-FE models.

Time span	Maternal emp.	(1) Kindergarten eligibility	(2) First grade eligibility	(3) F-test
Panel A: IV model				
Past month	Fraction of weeks worked	0.020	0.093***	54.4
		(0.015)	(0.015)	
	Hours worked per week	0.058	0.32***	33.4
		(0.062)	(0.063)	
Past year	Fraction of weeks worked	0.030**	0.088***	67.3
		(0.014)	(0.014)	
	Hours worked per week	0.073	0.34***	42.3
		(0.059)	(0.060)	
Panel B: IV-FE model				
Past month	Fraction of weeks worked	0.027**	0.023*	9.7
		(0.013)	(0.013)	
	Hours worked per week	0.12**	0.044	7
		(0.050)	(0.052)	
Past year	Fraction of weeks worked	0.044***	0.020*	22.3
		(0.011)	(0.011)	
	Hours worked per week	0.16***	0.071	16
		(0.046)	(0.045)	

Table 5 Over-identification test. Chi-sq(1) P-value are reported.

Outcome	Time span	Maternal emp.	(1) Hansen J Stat. P-value
Panel A: IV model			
BMI Z-score	Past month	Fraction of weeks worked	0.32
		Hours worked per week	0.31
	Past year	Fraction of weeks worked	0.35
		Hours worked per week	0.32
Overweight	Past month	Fraction of weeks worked	0.48
		Hours worked per week	0.5
	Past year	Fraction of weeks worked	0.42
		Hours worked per week	0.48
Obese	Past month	Fraction of weeks worked	0.97
		Hours worked per week	0.99
	Past year	Fraction of weeks worked	0.85
		Hours worked per week	0.97
Panel B: IV-FE model			
BMI Z-score	Past month	Fraction of weeks worked	0.41
		Hours worked per week	0.71
	Past year	Fraction of weeks worked	0.6
		Hours worked per week	0.6
Overweight	Past month	Fraction of weeks worked	0.21
		Hours worked per week	0.14
	Past year	Fraction of weeks worked	0.14
		Hours worked per week	0.14
Obese	Past month	Fraction of weeks worked	0.63
		Hours worked per week	0.43
	Past year	Fraction of weeks worked	0.43
		Hours worked per week	0.44

Note: hours worked per week is in unit 10.

Table 6 Endogenous variables and the IVs in one regression with obesity for the outcome variable.

	Maternal emp.	(1) Endogenous variable	(2) Kindergarten eligibility	(3) First grade eligibility
Past month	Fraction of weeks worked	0.0061	0.0030	0.016
		(0.0066)	(0.010)	(0.011)
	Hours worked per week	0.0026*	0.0030	0.016
		(0.0015)	(0.010)	(0.011)
Past year	Fraction of weeks worked	0.010	0.0028	0.016
		(0.0071)	(0.010)	(0.011)
	Hours worked per week	0.0041**	0.0028	0.015
		(0.0016)	(0.010)	(0.011)

Table 7 Subgroup analysis of the impact of maternal employment on children' weight status (robust standard errors in parentheses)

Outcome	Time span	Maternal emp.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Main sample	Girl	Boy	Hispanic	African America	White and other	Poor	Not poor	Married	Not married
Obesity	Past month	Fraction of weeks worked	0.18**	0.25**	0.096	0.053	-0.063	0.24***	0.12	0.20*	0.17**	0.14
			(0.071)	(0.12)	(0.088)	(0.16)	(0.14)	(0.090)	(0.13)	(0.10)	(0.078)	(0.17)
		Hours worked per week	0.052**	0.086**	0.026	0.018	-0.018	0.073***	0.037	0.057*	0.052**	0.043
			(0.021)	(0.044)	(0.024)	(0.051)	(0.037)	(0.028)	(0.038)	(0.031)	(0.025)	(0.049)
	Past year	Fraction of weeks worked	0.17**	0.23**	0.098	0.043	-0.039	0.23***	0.16	0.21**	0.17**	0.12
			(0.068)	(0.11)	(0.087)	(0.16)	(0.13)	(0.085)	(0.14)	(0.11)	(0.079)	(0.14)
		Hours worked per week	0.049**	0.077**	0.025	0.012	-0.013	0.065***	0.046	0.055*	0.051**	0.037
			(0.020)	(0.037)	(0.023)	(0.047)	(0.038)	(0.024)	(0.041)	(0.028)	(0.024)	(0.039)
		Number of obs.	16,535	8,182	8,353	3,686	5,080	7,769	3,957	10,085	10,626	5,909

Note: hours worked per week is in unit 10.

CHAPTER II: DO WALMART SUPERCENTERS IMPROVE FOOD SECURITY?[22]

1. Introduction

The United States Department of Agriculture (USDA)'s Food and Nutrition Service seeks to eliminate child hunger by 2015. At the same time, certain municipalities (like Los Angeles, Chicago, and New York City) are working to prevent big box retailers like Walmart from opening within the city limits, while some states and municipalities have passed laws, taxes, and mandates targeting Walmart specifically and seeking to make it harder for them to do business (Hicks 2007: 267-293). This is in spite of research showing that entry by Walmart Supercenters leads to lower food prices, particularly for low-income consumers (Basker and Noel, 2009). Are these new barriers to entry at odds with the Food and Nutrition Service's goal of eliminating child hunger? How should the Food and Nutrition Service advise governments that are making local development policy? We add to existing research on food security by exploring the relationship between the diffusion of new retail technologies—specifically, the Walmart Supercenter mass merchandiser format—and various outcomes indicative of food insecurity.

Addressing food security is an important part of a broad anti-poverty strategy. Existing research shows that higher food prices increase food insecurity (Gregory and Coleman-Jensen, 2011) and that families with more workers in "nonstandard" sectors tend to be less food secure (Coleman-Jensen, 2011). Some programs (like the Supplemental Nutrition Assistance Program) have improved food security (Gunderson et al., 2011). We expand on the existing literature by examining how a broad structural change in the American retail sector affects food security.

[22] This research was conducted using restricted data of the Current Population Survey at the Atlanta Research Data Center (ARDC). The views expressed in this paper do not reflect those of the Bureau of Census, the Bureau of Labor Statistics, or the ARDC. Any errors are mine.

Carden (2012) surveys the history of American retail and discusses "the rise of mass-market merchandisers" like Walmart and Costco. In 1988, Walmart expanded into large-scale food retail when it opened its first Walmart Supercenter. Today, Walmart is the country's largest grocer. Walmart Supercenters are by far the dominant supercenter chain, with 2907 locations in 2011 compared to 251 Super Targets and 26 Super K-marts.[23] Courtemanche and Carden (2011) find that Walmart Supercenters increase obesity, but they did not examine the other side of the coin: Walmart's "everyday low prices" could also reduce hunger and improve nourishment. We aim to fill this void.

There are a number of mechanisms by which Walmart Supercenters might affect food security. The first (and most obvious) was identified by Basker and Noel (2009). Walmart Supercenters offer low prices, but they also exert pressure on existing food retailers to lower their prices. Indeed, Basker and Noel (2009) find that the largest price reductions after Walmart Supercenter entry come at stores that serve primarily low-income consumers. Hausman and Leibtag (2009) and Furman (2005) argue that the consumer benefits from diffusion of mass-market merchandisers are considerable and progressively distributed: the major beneficiaries of these firms' lower prices are low-income consumers who spend large percentages of their incomes on food. It is possible that, in light of these lower prices and progressively-distributed benefits, entry by Walmart Supercenters improves food security.

There are also other mechanisms by which Walmart Supercenters could conceivably affect food security. First, they add to the local food supply, and Walmart often opens stores in rural "food deserts" that lack sufficient options for fresh produce. Second, these stores could impact food security through their effects on employment and wages, which could work in either direction and are the subject of debate in the literature (Basker, 2005; Neumark et al., 2008;

[23] These numbers come from the three companies' 2011 annual reports.

Dube and Jacobs, 2004; Dube et al., 2007). Moreover, Coleman-Jensen (2011) argues that “nonstandard” work is associated with greater food insecurity, and big box chains’ effect on the share of “nonstandard” versus “standard” work is also ambiguous. Pope and Pope (2012) claim that Walmart entry raises real estate values, and Guo (2011) argue s that more home assets are associated with more food security.

We estimate the impacts of Walmart Supercenters on food security using data from the 2001-2007 waves of the December Current Population Study Food Security Supplement (CPS-FSS) matched with primary data on Walmart Supercenter locations. Our outcomes are counts of the number of affirmative responses on the household and child-specific portions of the food insecurity questionnaire, along with binary variables for household food insecurity, household very low food security, child food insecurity, and child very low food security. Naïve regressions that control for demographics and year fixed effects produce negative associations between distance from the nearest Walmart Supercenter and several of the food insecurity measures, suggesting that closer proximity to a Walmart Supercenter actually worsens food insecurity. Since these associations may reflect underlying differences in population characteristics rather than causal effects of the stores, we then turn to an instrumental variables (IV) analysis that utilizes the predictable geographic expansion patterns of Walmart Supercenters outward from corporate headquarters. Specifically, we instrument for Walmart Supercenters with the interaction of distance from Bentonville, Arkansas (Walmart’s headquarters) and year. The IV results show that a greater distance from the nearest Walmart Supercenter significantly increases food insecurity according to all three household measures as well as two of the three child measures. In other words, after accounting for the endogeneity of Walmart location decisions, closer proximity to a Walmart Supercenter improves food security. Subsample analyses reveal that the effect is especially large for low-income households.

2. Methods

We begin by estimating linear probability models (LPMs) of the form

$$Y_{ict} = \beta_0 + \beta_1 \ln(DIS\_WS_{ct}) + \sum_{j=1}^{J} \gamma_j X_{jict} + \sum_{y=1}^{Y} \tau_y YR_y + \varepsilon_{ict} \tag{1}$$

where Y_{ict} is the outcome (each of the aforementioned food insecurity variables) for household *i* living in census tract *c* in year *t*, DIS_WS_{ct} is distance in miles from census tract c to the nearest Walmart Supercenter in year *t*, X_{jict} is a set of *J* control variables, YR_y is a set of Y year fixed effects ($YR_y = 1$ if $y = t$), ε_{ict} is the error term, and the other Greek letters are parameters to be estimated.[24] Distance from a census tract to the nearest Walmart Supercenter indicates to what extent residents are exposed to Walmart Supercenters, and therefore β_1 measures the effect of Walmart Supercenters on households' food insecurity. We take the natural logarithm of distance since it seems reasonable to expect a diminishing marginal effect. For instance, if a new Walmart Supercenter reduces a household's distance to the nearest Walmart Supercenter from 50 to 40 miles, this is unlikely to matter since both stores are prohibitively far away. The conclusions reached, however, are the same if we use a linear specification. Standard errors are heteroskedasticity-robust and clustered by census tract, since census tract is the geographic level at which we measure Walmart Supercenter exposure.

A concern with equation (1) is the possible endogeneity of Supercenter locations. Omitted variable bias could result if changes over time in unobserved area characteristics

[24] In unreported regressions (available upon request), we have verified that the estimated marginal effects are virtually identical using probit and logit models instead of linear probability models. This is not surprising since LPMs have been shown to give reliable estimates of average effects (e.g. Angrist and Pischke, 2008, Section 3.4.2). We prefer to focus on the LPM estimates since they are easier to implement in the subsequent instrumental variables regressions.

influence both the entry of Walmart Supercenters and residents' levels of food security. We are able to control for some obvious confounders such as income, but it is difficult to account for all of them. Reverse causality is also a concern, as big box grocers may specifically target areas lacking sufficient food supply.

We attempt to overcome these endogeneity concerns by using instrumental variables, or variables that are strongly correlated with the endogenous store variables but otherwise uncorrelated with the outcome (food insecurity) variables conditional on the controls. We adopt a similar strategy used by Courtemanche and Carden (2011) to investigate the impact of Walmart Supercenters on obesity. This strategy is based on the observation that the pattern of Walmart Supercenter expansion starting in 1988 was to radiate outward from Walmart's headquarters in Bentonville, AR, gradually reaching the entire continental United States by the late 2000s.[25] The expansionary pattern indicates that in the first few years, areas close to northwest Arkansas were the most likely to experience Walmart Supercenter entry, then in the next few years areas slightly further away were the most likely to have new Walmart Supercenters. The process continues until in the later year states on the coasts were the most likely to be exposed to the entry of Walmart Supercenters. In other words, during our sample period distance from Bentonville influenced the probability an area experienced Walmart Supercenter entry in a given year, and this effect changed over time. The interaction of distance from Bentonville with time therefore serves as a plausibly exogenous instrument that can identify the causal impact of Walmart Supercenters on food security.[26]

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[25] See Courtemanche and Carden (2011) for maps of Supercenters' expansion over time.
[26] This distance-from-Bentonville identification strategy has also been used by Neumark et al. (2008) and Dube et al. (2007) in studies of Walmart's effect on local labor markets. Basker (2006) critiqued the use of this strategy in the labor market context, but Courtemanche and Carden (2011) conduct a wide array of robustness checks and placebo tests to verify that Basker's criticism did not apply to health-related contexts such as obesity.

Specifically, we divide the U.S. into 17 distance rings reflecting 100-mile increments of distance from Bentonville (e.g. less than 100 miles, 100-200 miles, …, 1600 or more miles) and create an indicator variable for each ring.[27] The distance ring dummies are included as controls, while the interactions of the distance ring dummies with year are used as instruments. The resulting two-stage IV model therefore has the first-stage equation

$$\ln(DIS\_WS_{ct}) = \delta_0 + \sum_{j=1}^{J} \theta_j X_{jict} + \sum_{y=1}^{Y} \rho_y YR_y + \sum_{d=1}^{D} \varphi_d DIS\_BEN_d + \sum_{d=1}^{D} \sum_{y=1}^{Y} \varphi_{dy} \left(DIS\_BEN_d * YR_y\right) + \mu_{ict} \quad (2)$$

where DIS_BEN_d is the distance from census tract c to Bentonville while μ_{ict} is the error term. The second-stage regression is

$$Y_{ict} = \beta_0 + \beta_1 \ln(\widehat{DIS\_W}S_{ct}) + \sum_{j=1}^{J} \gamma_j X_{jict} + \sum_{y=1}^{Y} \tau_y YR_y + \sum_{d=1}^{D} \alpha_d DIS\_BEN_d + \varepsilon_{ict} \quad (3)$$

which differs from the naïve regression (1) by replacing the distance to Walmart Supercenters with the predicted values of this variable estimated in equation (2) and adding the distance ring fixed effects as controls.

Identification of β_1 in the IV model comes from the assumption that the distance*year interactions can be excluded from the second-state regression (3) – i.e. that these interactions are uncorrelated with changes over time in food security conditional on the controls. By including the distance ring fixed effects in (3), we allow for the distances to be correlated with *levels* of food security; we only need to assume that they are uncorrelated with *trends*. We test the validity

[27] The 100-mile distance ring classification follows Neumark et al. (2008) and Dube et al. (2007). In unreported specifications we found that the results are robust to the use of various other specifications for distance (e.g. linear, quadratic) and year (e.g. dummies for each year).

of the identifying assumption by checking the robustness of the results to the inclusion of the various combinations of control variables and performing the over-identification test.

3. Data

Our source of individual-level data on food security is the Current Population Survey Food Security Supplement (CPS-FSS), an annual household survey conducted by the U.S. Census Bureau for the USDA. The CPS-FSS is the December supplement to the monthly Current Population Survey (CPS), which is a nationally representative survey on labor force statistics. The participants of the CPS-FSS are the same as those interviewed by the original monthly CPS. In the month when the CPS-FSS is conducted, the participants answer the labor force questions as well as a series of questions concerning food security, food consumption, and the usage of food assistance programs. We currently focus on the CPS-FSS from 2001-2007 because we can match the CPS data with our data on Walmart Supercenters until 2007. However, we expect to extend the research period in the future since we are still in the process of collecting reliable data on Walmart Supercenter openings after 2007.

The CPS-FSS includes the standard set of 18 questions that are used to assess household and child food security. These questions ask whether in the last 12 months: 1) the statement "We worried whether our food would run out before we got money to buy more" was often or sometimes true; 2) the statement "The food that we bought didn't last and we didn't have money to get more" was often or sometimes true; 3) the statement "We couldn't afford to eat balanced meals" was often or sometimes true; 4) you or other adults in the household ever cut the size of meals or skipped meals because there wasn't enough money for food; 5) #4 happened in more than two months; 6) you ever ate less than you felt you should because there wasn't enough money for food; 7) you were ever hungry but didn't eat because you couldn't afford food; 8) you lost weight because you didn't have enough money for food; 9) you or other adults in your household ever did not eat for a whole day because there wasn't enough money for food; 10) #9 happened in more than two months; 11) the statement "We relied on only a few kinds of low-cost food to feed our children because we were running out of money to buy food" was sometimes or often true; 12) the statement "We couldn't feed our children a balanced meal, because we couldn't afford that" was sometimes or often true; 13) the statement "The children were not eating enough because we just couldn't afford enough food" was sometimes or often true; 14) you ever cut the size of any of the children's meals because there wasn't enough money for food; 15) the children were ever hungry but you just couldn't afford more food; 16) any of the children ever skipped a meal because there wasn't enough money for food; 17) #16 happened in more than two months; and 18) any of the children ever did not eat for a whole day because there wasn't enough money for food. Following convention (e.g. Nord et al., 2005), we use six outcome variables to measure food insecurity. Two of them are continuous variables indicating the counts of affirmative responses to the above questions for household and children separately. Another four are dummy variables summarizing the answers to these 18 questions: "household food insecurity" (three or more "yes" answers), "household very low food security" (eight or more "yes" answers or six or more in households without children), "child food insecurity" (two or more "yes" answers on child questions), and "child very low food security" (five or more "yes" answers on child questions).

We also use the CPS-FSS to construct three sets of individual-level control variables: demographic characteristics, economic characteristics, and participation in government food assistance programs. The demographic variables include adult responder's age; number of own children (dummies for 1, 2, 3, 4, and 5+, with 0 as the omitted base category); dummies for whether race/ethnicity is non-Hispanic white, non-Hispanic black, or Hispanic (other race/ethnicity is the omitted category); dummies for married and formerly married (never married is the omitted category); and dummies for high school degree but no further, some college, college degree, and graduate degree (less than high school degree is the omitted category). The economic variables are household income (dummies for the 16 categories given by the CPS), occupation (dummies for 17 categories), and median income in the census tract.[28] The food assistance variables are indicators for whether any household member received Supplemental Nutrition Assistance Program (SNAP) benefits; Women, Infants, and Children (WIC) benefits; and free/reduced-price school breakfasts or lunches in the past year.

We also include county-level food availability variables as control variables in some regressions. These include numbers of restaurants, grocery stores/supermarkets, convenience stores, and supercenters/warehouse clubs (subtracting out Walmart Supercenters), obtained from the United States Census Bureau's County Business Patterns. We scale these variables by population (per 100,000 capita) using annual population estimates from the Census Bureau.

Our independent variable of interest is distance from each respondent's census tract of residence to the nearest Walmart Supercenter as of the end of each year. Walmart Supercenter entry dates and locations through 2007 were hand-collected and used in Courtemanche and Carden (2011, 2014). The census tract geographic data come from the U.S. Census Bureau Tiger/Line Shapefiles 2000. For each census tract, an internal point, usually a geographic center of the area, is identified, and its latitude and longitude coordinates are used to label the census tract. The distance from a census tract to the nearest Walmart Supercenter is then computed using the geodetic distance between the two sets of corresponding coordinates. The geodetic

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[28] About 15% of the sample has missing income data. We drop these individuals for the reported regressions, but the results are very similar if we include them and indicate them with a dummy variable.

distance measures the length of the shortest arc between two points on the ellipsoid surface of the earth. The distance from a census tract to the headquarters of Walmart at Bentonville, AR, is calculated following the same algorithm, then categorized into 17 distance rings.

Merging the CPS-FSS to the census tract-level geographic data and county-level store variables require**s** precise geographic identifiers that are not available in the public-use data**.** We therefore use the restricted version of the CPS-FSS, provided by the Census Bureau after an application process and accessed in the Atlanta Census Research Data Center.

Dropping observations with missing data yields a final analysis sample of approximately 236,000 households, 75,000 of which have children. Following Research Data Center disclosure policies, we are only able to report sample sizes rounded to the nearest 1,000 observations. Table 8 presents the summary statistics for the food security and Walmart variables, both for the full sample and subsamples that we will consider in our regression analyses: low income households (<$25,000), households living in MSA central cities, and households living in rural areas.

In the full sample, on average, households affirmatively answer 0.7 questions for the 18-item food security module. There are 11 percent of households have experienced food insecurity, and 2 percent of them have very low food security. Turning to child food insecurity, the situation is a bit better. On average, children affirmatively answer 0.46 questions of the 18-items. Among all children, 6 percent of them have food insecurity, and only 0.5 percent of them have very low food security.

Low income households and children from low income families experience much worse food insecurity. They affirmatively answer twice as many questions as those in the full sample. The percentage of food insecurity and very low food security for both households and children roughly doubled in the low income households sample compared to the full sample. Dividing the sample by MSA central cities and rural areas, the food insecurity measures are similar to those of the full sample.

4. Results

Tables 9-10 report the results for food insecurity outcomes for households as a whole and for children separately. The first column is for the linear probability model without instruments. The last five columns are for the full-sample IV regressions, with distance ring from Bentonville*year as the instruments for distance from the nearest Walmart Supercenter. The five IV columns report from specifications with: 1) only demographic controls, 2) demographic and economic controls, 3) demographic and food assistance controls, 4) demographic and food availability controls (warehouse clubs, restaurants, grocery stores/supermarkets, and convenience stores), and 5) all sets of controls. We experiment with different combinations of control variables because it is not clear whether it is appropriate to control for the economic, food assistance, and food availability variables. On one hand, they might help capture unobservable determinants of both Walmart entry decisions and food insecurity. On the other hand, they could potentially be endogenous to Walmart presence: Walmart entry could influence local incomes (which in turn affect eligibility for food assistance programs) as well as other food retailers' entry and exit decisions. It is therefore important to verify that including these sets of variables does not meaningfully impact the results.

Table 9 presents the results for household food insecurity. Estimates from the LPM model demonstrate a significant and negative association between Walmart Supercenters and all three household food insecurity measures. However, the LPM estimates are unlikely to reflect a causal effect because of the aforementioned endogeneity problems. We therefore turn to the IV models. All IV estimates are positive and significant, indicating that the causal effect of having a

Walmart Supercenter nearby is to reduce the likelihood of being food insecure. Since we use the natural log of the distance from the nearest Supercenter variable, the coefficient estimates can be interpreted as the approximate effect of a 100% increase in distance. The IV results therefore suggest that 100% greater distance from Walmart leads to 0.06-0.08 more affirmative responses to the household portion of the food insecurity questionnaire, while increasing the rate of household food insecurity by 0.99-1.2 percentage points and the rate of household very low food security by 0.52-0.67 percentage points. In Table 9, we also present the first-stage F tests and the over-identification test p-values as evidence for the validity of the instrument. In all specifications, the first-stage F statistics are always safely over the rule-of-thumb critical value of 10, which is the conventionally accepted levels for instruments to be considered sufficiently strong. Moreover, the over-identification tests are insignificant in most specifications, which suggests that the instruments are appropriate.

The results for child food insecurity, shown in Table 10, are generally similar to those for household food insecurity. We find negative, though insignificant, correlations between the three child food insecurity outcomes and distance from the nearest Walmart Supercenter. The IV estimates are positive and significant for count of affirmative child responses and child food insecurity, indicating that a shorter distance to Walmart Supercenters reduces the likelihood of children being food insecure. A 100% increase in distance from the nearest Walmart Supercenter increases the number of affirmative responses to the child portion of the food insecurity questionnaire by 0.028-0.033 and the probability of a household's children being food insecure of 1.3-1.4 percentage points. However, the IV estimates are small and insignificant for the third child outcome: child very low food security. We suspect that this is because child very low food security is an extreme condition that applies to a very small portion of our sample (under 1%).

For all outcomes, the results are robust if we add the economic, food assistance programs, and county food availability controls. The first-stage F statistics are again larger than 10 and the over-identification tests are insignificant in most specifications, both indicating that the instrument performs well.

Table 11 reports for subsamples for which we hypothesize the effects of Walmart on food security might be particularly strong: low-income households (<$25,000), households living in MSA central cities, and households living in rural areas. Low income households might be particularly sensitive to the reduction in food prices brought about by Walmart entry. Walmart Supercenters are also likely to improve food security in areas with relatively little food availability, as households in these areas experience large reductions in the time cost of obtaining food. In this version of the paper, we proxy for low food availability areas by examining subsamples of MSA central cities and rural areas, the two types of locations most commonly alleged to be food deserts. The first column of Table 11 simply reprints the estimates from the full sample IV regressions for comparison purposes. By restricting the sample to low income households, we again find positive and significant effects of Walmart Supercenters, and the effects are substantially larger than those from the full sample. The results for the subsamples of households in MSA central cities and rural areas are mixed. The estimates are larger than those from the full sample in some specifications while smaller in others, and they are fairly imprecise. In future versions of the paper, we will attempt to improve on this portion of the analysis by identifying the exact census tracts designated as food deserts by the USDA and running subsample regressions for households in these areas.

5. Conclusion

This paper asks whether Walmart Supercenters, which lower food prices and expand food availability, improve food security. We estimate instrumental variables (IV) models that exploit the predictable geographic expansion patterns of Walmart Supercenters outward from corporate headquarter. Our results rely on data from the restricted-access 2001-2007 waves of the December CPS Food Security Supplement. These data allow us to investigate the impact of Walmart Supercenters on households' and children's food insecurity at the census tract-level. We find that the entry of Walmart Supercenters helps to alleviate food insecurity for both households and children. The results are robust to the inclusion of controls for households' economic status, food assistance program participation, and county food availability.

Our finding contributes to the literature in multiple ways. First, we provide new evidence on the causes of food insecurity. Considerable resources are allocated through food assistance programs toward protecting households, especially children, from food insecurity. However, no research to date has examined the influence of big box grocers on food insecurity. Second, we contribute to the debate about Walmart's health effects. Big box grocers, Walmart Supercenters in particular, are blamed for causing obesity (Courtemanche and Carden, 2011; Courtemanche et al., 2015). However, we are the first to study the other side of the coin: how the same cheap and readily available food that drives big box grocers' effect on obesity may also help in fighting food insecurity. This improvement in food security adds another factor local governments should consider when deciding whether to use policy levers (e.g. taxes, zoning laws) to either incentivize or prevent entry from bog box stores.

6. Tables

Table 8 Summary Statistics for Key Variables

	Full sample	Low-income households	MSA central cities	Rural areas
Count of affirmative household responses	0.71	1.65	0.7	0.74
	(1.93)	(2.77)	(1.94)	(1.93)
Household food insecurity	0.11	0.25	0.11	0.11
	(0.31)	(0.43)	(0.31)	(0.31)
Household very low food security	0.02	0.06	0.02	0.02
	(0.15)	(0.23)	(0.15)	(0.15)
Count of affirmative child responses	0.46	0.88	0.45	0.48
	(0.81)	(0.19)	(0.82)	0.78
Child food insecurity	0.06	0.16	0.06	0.06
	(0.24)	(0.36)	(0.24)	(0.24)
Child very low food security	0.005	0.01	0.005	0.004
	(0.07)	(0.12)	(0.07)	(0.06)

Table 9 Regression Results for Household Food Insecurity Outcomes

	LPM	Instrumental Variables				
	(1)	(2)	(3)	(4)	(5)	(6)
Count of Affirmative Household Responses						
ln(Distance to Walmart Supercenter)	-0.027***	0.073***	0.07***	0.062***	0.078***	0.065***
	(0.005)	(0.025)	(0.023)	(0.023)	(0.025)	(0.0022)
First Stage F Statistic	--	133	134	133	130	130
Over-identification Test P-Value	--	0.13	0.47	0.094	0.11	0.27
Household Food Insecurity						
ln(Distance to Walmart Supercenter)	-0.004***	0.012***	0.011****	0.0099***	0.012***	0.01***
	(0.0008)	(0.0039)	(0.0037)	(0.0037)	(0.004)	(0.0036)
First Stage F Statistic	--	133	134	133	130	130
Over-identification Test P-Value	--	0.22	0.59	0.27	0.19	0.55
Household Very Low Food Security						
ln(Distance to Walmart Supercenter)	-00018***	0.0059***	0.0054**	0.0052**	0.0067***	0.0055**
	(0.0005)	(0.0023)	(0.0022)	(0.0022)	(0.0023)	(0.0022)
First Stage F Statistic	--	133	134	133	130	130
Over-identification Test P-Value	--	0.22	0.43	0.17	0.22	0.35
Demographic Controls	YES	YES	YES	YES	YES	YES
Economic Controls	NO	NO	YES	NO	NO	YES
Food Assistance Controls	NO	NO	NO	YES	NO	YES
County Food Availability Controls	NO	NO	NO	NO	YES	YES
Distance Ring Fixed Effects	NO	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Standard errors, heteroskedasticity-robust and clustered by census tract, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. CPS household sampling weights are used. Sample size is approximately 236,000.

Table 10 Regression Results for Child Food Insecurity Outcomes

	LPM	Instrumental Variables				
	(1)	(2)	(3)	(4)	(5)	(6)
Count of Affirmative Child Responses						
ln(Distance to Walmart Supercenter)	-0.007**	0.033**	0.032**	0.03**	0.033**	0.028*
	(0.0034)	(0.016)	(0.015)	(0.015)	(0.016)	(0.015)
First Stage F Statistic	--	100	101	100	94	95
Overidentification Test P-Value	--	0.018	0.072	0.063	0.017	0.098
Child Food Insecurity						
ln(Distance to Walmart Supercenter)	-0.0012	0.014***	0.014***	0.013***	0.014***	0.013***
	(0.0011)	(0.0052)	(0.005)	(0.005)	(0.0052)	(0.005)
First Stage F Statistic	--	100	101	100	94	95
Overidentification Test P-Value	--	0.034	0.081	0.095	0.031	0.11
Child Very Low Food Security						
ln(Distance to Walmart Supercenter)	-0.0003	0.0005	0.0004	0.0004	0.0004	0.0002
	(0.0003)	(0.0014)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
First Stage F Statistic	--	100	101	100	94	95
Overidentification Test P-Value	--	0.81	0.87	0.81	0.82	0.85
Demographic Controls	YES	YES	YES	YES	YES	YES
Economic Controls	NO	NO	YES	NO	NO	YES
Food Assistance Controls	NO	NO	NO	YES	NO	YES
County Food Availability Controls	NO	NO	NO	NO	YES	YES
Distance Ring Fixed Effects	NO	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: Standard errors, heteroskedasticity-robust and clustered by census tract, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. CPS household sampling weights are used. Sample size is approximately 75,000.

Table 11 Subsample Regression Results

	Full Sample	Subsamples of Interest		
		Income < $25,000	MSA Central City	Rural Area
Count of Affirmative Household Responses				
ln(Distance to Nearest Walmart Supercenter)	0.07***	0.17***	0.051	0.07
	(0.023)	(0.06)	(0.042)	(0.063)
Household Food Insecurity				
ln(Distance to Nearest Walmart Supercenter)	0.011****	0.03***	0.011*	0.0058
	(0.0037)	(0.0093)	(0.0067)	(0.0097)
Household Very Low Food Security				
ln(Distance to Nearest Walmart Supercenter)	0.0054**	0.014**	0.0055	0.012*
	(0.0022)	(0.0064)	(0.0043)	(0.0066)
Number of Observations	236,000	70,000	68,000	61,000
Count of Affirmative Child Responses				
ln(Distance to Nearest Walmart Supercenter)	0.032**	0.063	0.01	0.034
	(0.015)	(0.045)	(0.029)	(0.046)
Child Food Insecurity				
ln(Distance to Nearest Walmart Supercenter)	0.014***	0.027*	0.0032	0.012
	(0.005)	(0.015)	(0.0098)	(0.015)
Child Very Low Food Security				
ln(Distance to Nearest Walmart Supercenter)	0.0004	0.0001	0.001	0.0048
	(0.0014)	(0.0046)	(0.0028)	(0.0042)
Number of Observations	75,000	17,000	20,000	19,000

Notes: Standard errors, heteroskedasticity-robust and clustered by census tract, are in parentheses. *** indicates statistically significant at 1% level; ** 5% level; * 10% level. CPS household sampling weights are used. All regressions include demographic and economic controls and distance ring and year fixed effect

CHAPTER III: EXAMINING THE EFFECTS OF THE FAMILY AND MEDICAL LEAVE ACT USING LABOR MARKET FLOWS

1. Introduction

Workplace-mandated maternity benefits are essential to support working mothers in order to maintain a continuous labor supply. In the U.S., the Family and Medical Leave Act (FMLA) is such a federal policy mandating the provision of maternity leave to covered employees. The FMLA was legislated in February 1993 and went into effect in August 1993. Under the FMLA, eligible employees in establishments with 50 or more employees are entitled to take up to twelve weeks of unpaid yet job-protected leaves for health related issues, and, most importantly, for the birth and care of newborns.[29]

Despite the fact that FMLA leaves are unpaid and subject to a rigorous application process,[30] they have been broadly used since its legislation. According to the 2000 FMLA survey report,[31] 16.5% of all employees in the U.S., 23.8 million, had taken FMLA leaves in the 18 months prior to the survey. Both male and female workers are entitled to FMLA leaves. Among eligible leave-takers, 57.7% were women and 42.3% were men. However, females (75.8%) with children under age 18 were significantly more likely than males with young children (45.1%) to take leaves. In addition, 42.8% of female leave-takers took maternity disability, for which only female employees are eligible. Maternity leave was also the longest among all the types of leaves. While most leaves were short-term (83.3% of leaves lasted less than ten days), 68.4% of maternity leaves lasted for more than 30 days.

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[29] Detailed benefits are provided on this website: http://www.dol.gov/whd/fmla/

[30] To apply for FMLA leaves, an employee has to fulfill these requirements (not exhaustive): The employee has worked for the employer for at least one year and 1,250 hours or more; the employer is an establishment with 50 or more employees within a geographic radius of 75 miles; and the employee has to inform the employer at least 30 days in advance.

[31] The full report is available on this website: http://www.dol.gov/whd/fmla/toc.htm

Proponents of maternity leave legislation believe that the FMLA could "promote the goal of equal employment opportunity for women and men" (The Family and Medical Leave Act of 1993: Public Law 103-3, 107 Stat. 6-7). However, like any policy designed to ease work-family conflicts, the FMLA might have unexpected effects. Economic analyses indicate that the costs of providing maternity leave will be largely borne by those who most value the benefits (Summers, 1989). In the case of the FMLA, the principal "costs" are disruption costs to employers due to worker leaves. Leave taking disturbs production routine, requires temporary replacement hiring, and adds uncertainty since there might be a chance that leave-takers won't return. These costs shift downward the labor demand curve for young women as they are most likely to benefit directly from maternity leave. Because labor supply is generally more inelastic than labor demand, the costs of the mandate are likely to be shifted to women of childbearing age, either in the form of lower wages or as a reduction in employment if wages are rigid.

In this paper, I take the legislation of the FMLA as a natural experiment and examine its impact on young women's labor market outcomes. The identification of the treatment effect is three-fold. The first source of variation comes from the difference between the pre- and the post-FMLA periods. The second source lies in the fact that some states passed state-level maternity leave mandates before 1993, while others did not. Therefore, the FMLA should have a greater impact on states without a prior maternity leave policy than on states with a mandate in place before the federal FMLA. The third source of variation arises because women of childbearing age differ from other workers in that they are eligible for maternity leave. Employees can take FMLA leaves due to various types of health-related reasons. While other types of leaves are extended to all workers, maternity leave is a group-specific benefit that applies only to women.

Therefore, I expect that the FMLA would have larger effect on women of childbearing age. These variations enable me to adopt a triple-difference (DDD) method.

The novel feature of this study is that I use six labor market outcomes, including employment and wages, new hires and starting salaries, as well as job separations and recalls. Previous studies on this topic focus predominantly on how the policy change affects employment and wages. However, such stock variables consist mostly of existing workers, and thus, are unlikely to adjust instantly at the time that the FMLA was legislated. It takes time for the labor market to develop a new equilibrium level. I seek to improve upon the literature by using flow variables, i.e., hiring, separations and recalls. Firms' hiring decisions are more flexible, and thus might change instantly when the FMLA was enacted. Separations evaluate how the FMLA influences work tenure. Recalls were once widespread in the U.S., particularly in the industrial and unionized sectors of the economy, with 70% of workers temporarily laid off being rehired by their former employers (Katz and Meyer, 1990). Temporary layoffs and recalls are far less likely today. That said, temporary leaves of absence, common among child-bearing women, and other labor market flows reflect the dynamics of the labor market, and therefore may help one understands the immediate causal effects of the FMLA.

To conduct the analysis, I use data from a relatively new data set, the Quarterly Workforce Indicators (QWI). This publicly accessible data set contains rich information on labor force measurements, including the six labor market outcomes. It also has almost universal coverage on employment in the private sector. Using the QWI data and applying the triple-difference method, I obtain little evidence that the FMLA reduces the starting salaries offered to young women and increases recalls among them when using older women as the comparison group. When compared with young men, the FMLA has no significant effect on any labor market outcomes. However, I am unable to determine whether the lack of evidence indicates a true zero effect or if it is due to imprecise estimation and relatively uninformative data.

2. Previous Literature

The FMLA is the first federal mandate in the U.S. that requires the provision of maternity leave benefits among covered establishments. Since its legislation, the FMLA has generated a far-reaching effect on society. Both male and female employees take advantage of the policy, while working mothers are more likely to take leaves or take longer leaves than working fathers (Berger and Waldfogel, 2004; Han, Ruhm, and Waldfogel, 2009; Han and Waldfogel, 2003). Research exploring the consequences of the FMLA generally find that it encourages the fertility rate among working women (Averett and Whittington, 2001; Rossin, 2011); promotes breastfeeding (Berger, Hill and Waldfogel, 2005); improves infants' health (Rossin, 2011); and enhances new mothers' mental and physical health (Chatterji and Markowitz, 2012; Chatterji et al., 2013).

Several studies have examined the effects of the FMLA on employment and wages, but they have reached no consensus. Waldfogel (1999) uses the variations generated from the passing points of state maternity leave laws and the federal FMLA as a natural experiment to estimate the effects of the FMLA. Using data from the Current Population Survey (CPS), she finds that the FMLA has no significant impact on women's employment or wages. Using data from the National Longitudinal Survey of Youth 1979 Cohort (NLSY79), Baum (2003) improves the estimation by identifying whether women work for establishments that are large enough to fulfill the FMLA's coverage requirements. He also finds insignificant effects of the FMLA on both employment and wages.

Another study by Espinola-Arredondo and Mondal (2009) examines the impact of the FMLA while taking into account a related policy--Temporary Disability Insurance (TDI). They compare the effect of the FMLA in states with TDI and those without. Their findings suggest that the FMLA significantly increases female employment. Observing the declining labor force participation rate among mothers with infants, Goodpaster (2010) analyzes whether the FMLA has an impact on women's labor force participation. She finds that almost two-thirds of the reduction in labor force participation among new mothers in the mid-1990s can be explained by the legislation of the FMLA. To examine the gender-wage gap, Manchester et al. (2008) use the FMLA as a policy shock to work commitment. They find a negative impact of maternity leave usage on women's wages, which suggests that firms either discriminate or have expectations of lower job commitment and productivity for young women. Thomas (2014) suggests that women are more likely to remain employed after the legislation of the FMLA, but at the same time, they are also less likely to be promoted.

Previous studies provide little knowledge on the effects of workplace mandates, mainly because we generally do not have much information to measure the effects in a precise way. Almost all of the literature of the FMLA focuses on how the policy change affects women's employment and wages. This study is innovative in the sense that I explore the immediate impacts of the FMLA on labor market flows, including hires and starting salaries, as well as separations and recalls. While it takes time for employment and wages to adjust to a new equilibrium level, firms have flexible control on hiring decisions. Intuitively, if firms believe that the FMLA imposes a cost of hiring young women, they may adjust hiring decisions accordingly. On the other hand, if such family benefit attracts young women to join the labor force, the firms may be more likely to hire young women as the pool of candidates is larger. Job separations may also be affected by the FMLA. Now that employees have the option to take leaves when necessary, the policy may enhance job commitment among young women. Yet, firms might be more likely to lay off potential leave-takers. The last outcome, recalls, refers to rehired workers who are temporarily laid off or on a lengthy leave by the same employer. The FMLA might affect recalls if choices by the firm or the employee result in lengthy leave-taking (consecutive quarters off the payroll in the QWI), followed by a return to work (returning to an establishment's payroll), defined as a recall in the QWI.

The QWI not only provides the labor measures needed for this investigation, but also empowers the generalizability of study results since it has universal coverage of private establishments. To the best of my knowledge, this is the first study that utilizes labor market flow measurements to identify the causal effects of the FMLA on women's labor market outcomes.[32] I consider the analysis in this paper a complement to the existing studies. In addition, the hires and separations measurements can be applied to broad research questions concerning the effects of policies that change labor demand and supply.

3. Data

I use the Quarterly Workforce Indicators to estimate the effects of the FMLA on young women's labor market outcomes. QWI is a publicly available dataset that contains rich information, collected quarterly, on labor force measures at local levels. This data set is based on a wide variety of employment records, such as unemployment insurance data, social security data, federal tax records, etc. From these data sources, individual-level data are linked to firm information, then further aggregated based on locations, quarters, and demographic categories. Within each geographic location, the data are divided into 16 data cells corresponding to 16 sex-

[32] Curtis et al. (2014) use the QWI to examine the effects of California's 2004 paid family leave policy on new hire earnings, employment, separations, and recalls.

age groups.[33] Therefore, the level of observation is at the sex and age group-county-quarter level. For example, a single data cell may represent an employment measurement for workers who are females aged 25-34 and employed in establishments in a particular county in certain quarter of the year.

There are at least three advantages of using QWI data to investigate the current research question. First, QWI data have almost universal coverage on employment statewide. The data cover 98% of all private sector employment in non-agricultural industries. Second, QWI contains various labor supply measurements, including labor market flows that are at the heart of this study. Last but not least, although QWI data are aggregated, they are detailed enough to let me identify the demographic groups that might be affected most by the FMLA, i.e., women of childbearing age. Hence, I can determine the causal effects of the FMLA by comparing labor market outcomes between young women and other workers.

Two limitations of the QWI are that it fails to measure either employer size or hours of work. Because the FMLA covers only those employees with least one year of job tenure and work in establishments with 50+ employees (roughly 58 percent of all employees), estimates of FMLA effects from the QWI are likely to be seriously attenuated. Although the QWI measures quarterly earnings, the inability to measure hours makes it difficult to distinguish differences in wages from differences in hours worked.

An additional shortcoming of the data available in the QWI is that it relies on the voluntary participation of individual states, and only six states reported information in QWI in the early 1990s. They are California (CA), Idaho (ID), Maryland (MD), Oregon (OR), Washington (WA), and Wisconsin (WI). Despite this limitation, the six states still provide a fair amount of variations because they differ in terms of the passing dates of maternity leave benefits.

[33] By each gender, there are eight age groups: 14-18, 19-21, 22-24, 25-34, 35-44, 45-54, 55-64, and 65-99 years old.

Table 12 presents the legislation dates for each of the six states. Among them, four states, CA, OR, WA and WI, had state-level mandated maternity leave before 1993. The remaining two, ID and MD, had no such policy prior to the legislation of the federal FMLA. Therefore, we would expect that when the FMLA was enacted in 1993, the former four states would have been impacted less than the latter two. For convenience, we refer to the former four states as the non-experimental states and the latter two as the experimental states.

The six states started to participate in QWI at different times. Therefore, instead of restricting the study to a fixed research period, I include all the information available before 1996. The cutoff is set at 1996 to avoid the confounding factors induced by the legislation of the Personal Responsibility and Work Opportunity Reconciliation Act (i.e., so-called welfare reform), which had a profound impact on employment among women in low-income households. Table 13 shows the data periods for each state. Among the six states, MD, WA and WI have the longest data history while CA has the shortest.

I measure the impact of the FMLA on three sets of outcome variables. The first set of outcomes, employment and monthly wages (i.e., earnings), makes up a standard labor market measurement used in most labor studies. In QWI, these measurements are assessed as the number of employed workers and the corresponding monthly wage. One limitation of such measures is that they consist of a stock of existing workers. Thus, the effects of policy change on employment and wages are unlikely to occur instantly, but instead to develop gradually as the labor market moves toward a new equilibrium. For this reason, I focus on another two sets of outcomes that specify labor market flows.

The second set of outcomes, the number of newly hired workers and the associated monthly starting salaries, reflects the flow of employment. Firms have relatively flexible control over hiring decisions. If they believe that the mandate imposes a cost of hiring women of childbearing age, such as the cost of temporarily replacing leave-takers and the cost of providing health insurance for leave-takers, firms might adjust their hiring decisions accordingly. For example, firms might avoid hiring young women or incorporate a "wage penalty" into the starting salaries that are offered to young females. On the other hand, if the availability of the maternity leave attracts women whose labor supply might otherwise be constrained by binding family responsibilities, the firms will have a larger pool of candidates. Thus, firms might end up hiring more women of childbearing age, albeit at lower salaries. As previously stated, the QWI does not provide measures of hours worked, hence, the employment wages and starting salaries are measured by monthly rather than average hourly rates.

The third set of variables also are flows, includes the number of workers separated from jobs and the number of workers recalled by the original employers. These variables are interesting because it is not unusual that pregnant women and new mothers leave jobs, voluntarily or non-voluntarily, absent maternity leave benefits and/or job protection. Since the most important feature of the FMLA is job-protection, the policy might reduce job separation among young women. However, it may also be true that the FMLA might lead to leave-takers deciding to quit jobs after taking family leave if they value staying at home more than working. The outcomes, job separations possibly accompanied by eventual recalls (rehires), provides insight into the effects of the FMLA.

I restrict employment, new hires, and separations to be "stable" measurements that last for at least a whole quarter. This simple yet essential step mitigates the influence of seasonal and temporary change in employment which might confound the results. The new hire measurement differs from recalls in the sense that recalls refer to workers that are rehired by the original employers while new hires include workers that were not employed by the same firms in the past four quarters. The labor market flows, new hires, separations, and recalls, are particularly suitable in the case of capturing the immediate impact of the policy change on labor demand and supply.

I use the natural logarithm form for all of the outcome variables, and I add one to each value before taking logs in order to avoid extreme outliers when the original value is zero. Table 14 presents the summary statistics for outcomes before taking logarithms. The unit of observation is at the demographic group-location-quarter level. Overall, there are 19 quarters, 9 sex-and-age groups, and 273 counties. In the most narrowly defined data cells, i.e., observations based on few persons or establishments, the data are suppressed as missing values for confidentiality reasons. Such suppression only applies to counts, such as new hires, but not to continuous variables, such as the starting salaries.

I divide the sample into three subgroups, one treatment group and two control groups. The treatment group includes women of childbearing age (aged 19-34). Their close counterparts are young men aged 19 to 34, who make up one of the control groups. I refer to the treatment group as “young women” and their male counterparts as “young men”. I also use a second control group, called “older women”, which contains female workers aged 35-64. The summary statistics of the three subgroups are listed in panel A, B, and C.

Comparing across the three subgroups, the employment and monthly wages of young women are consistently less than that of young men and older women. On average, there are less newly hired young women than young men, but there are more newly hired young women than older women. The starting salaries for young women are lower than that of both young men and older women. The number of separations for young women is smaller than that of young men, but larger than that of older women. There are fewer young women being recalled compared to young men and older women.

In the comparison between non-experimental states and experimental states, the former have more observations because they include more states. Wages and starting salaries of young women in experimental states are slightly lower than that of young women in non-experimental states. However, wages and starting salaries of young men in experimental states are higher than that of their counterparts in non-experimental states. For older women, they have higher wages in non-experimental states, but newly hired older women receive higher starting salaries in experimental states. The numbers of employment, new hires, and separations for the three subgroups are larger in non-experimental states. The numbers of recalls for young women and older women are lowers in non-experimental states but higher for young men in non-experimental states.

The six graphs in Figure 1 show the ratio of each outcome variable of the treated group to one of the two control groups, separated by experiment states, “exp”, and non-experimental states, “non-exp”. For example, the first curve in the first graph represents the ratio of employment of young women to young men in experimental states. These six graphs present the following evidence. First, while both new hires and employment fluctuate seasonally, the fluctuation in new hires is much more substantial than in employment. Second, the ratios of employment and new hires of young women to older women decline during the research period. Third, the ratios of the starting salaries of young women to the two control groups in non-experimental states peaked at the time the FMLA was legislated. There is no such pattern in experimental states.

4. Empirical Analysis

I take advantage of the legislation of the FMLA as a natural experiment to estimate its effects on young women's labor market outcomes. I begin the analysis by setting up a difference-in-difference (DD) model that uses two sources of variations-- time and geographic variations. The 1993 FMLA requires firms to provide maternity leave to eligible employees. Since some states had state-level maternity leave mandates before 1993 while others did not, I expect that the FMLA would have larger impacts on the latter states. Using such variations, the DD model identifies the difference between the changes in outcomes among young women for the experimental states and the non-experimental states before and after the 1993 FMLA. The sample is restricted to women of childbearing age, including those aged 19-34. The regression equation is as follows:

$$\ln(Y_{gt}) = \beta_0 + \beta_1 yq_t + \beta_2 location_g + \beta_3 FMLA_{gt} + \varepsilon_{gt}$$

The subscript g denotes each county, and t indicates each quarter-year. yq_t, and $location_g$ are sets of dummies representing quarter-year fixed effects and county fixed effects. $FMLA_{gt}$ is the policy variable which equals one if employees work in experimental states after 1993. β_3 is the coefficient in interest which measures the effect of the FMLA.

The DD estimates provide a baseline for the analysis. It allows a simple comparison for changes in labor outcomes for young women following passage of the FMLA in experimental states with young women in non-experimental (non-affected) states. However, the FMLA variable will not only capture the effects of the policy change, but also pick up labor market effects not controlled for but that are correlated with the adoption of the FMLA. Therefore, it may be preferable to include employees that do not use maternity leave benefits as the comparison group, and thus controls for labor market changes for young women as compared to other workers that are not due to the FMLA. The difference between the treatment group and the control groups provide the third source of variation for a standard triple difference model.

$$\ln(Y_{dgt}) = \beta_0 + \beta_1 yq_t * location_g + \beta_2 sexage_d * location_g + \beta_3 sexage_d * yq_t + \beta_4 FMLA_{dgt} + \varepsilon_{dgt}$$

The subscript d denotes each sex-age groups, and $sexage_d$ are sets of dummies representing 16 sex-age groups. The inclusion of the three pair-wise intersections among quarter-years, locations, and sex-age groups capture unobserved factors that might confound the causal effect of the policy change. $FMLA_{dgt}$, is the policy variable which equals one for women aged 19-34 and work in experimental states after 1993. β_4 is the coefficient of interest that measures the effect of the FMLA.

The inclusion of these fixed effects controls for factors that vary across sex-age groups, counties, and quarter-years. For example, the interactions of demographic and county fixed effects capture the time invariant characteristics for workers in certain county, such as education, family structure, etc. If these characteristics vary across time, the interactions of quarter and county fixed effects or the interactions of demographic and quarter fixed effects are likely to pick up the changes.

5. Results

I present the DD estimates in Table 15. Standard errors are robust and clustered at the state level. The sample is restricted to women of childbearing age. The DD estimates compare the treatment effect of the FMLA on young women work in experimental states and non-experimental states. The FMLA has a positive and insignificant effect on employment and new hires, while it has a negative and insignificant effect on wages of employment and starting salaries of hires. In addition, the FMLA has negative and insignificant effects on both separations and recalls. It is a little surprising that the FMLA reduces separations while increases new hires. Intuitively, more separations imply more vacant positions and job openings, which result in more hires. In figure 2, I depict the trends of new hires and separations along the research period. Both variables are averaged by quarter. As expected, the trends of these two labor market outcomes generally move coordinately.

The DD estimates only provide simple comparison between female workers in experimental states and non-experimental states. It is unlikely that the DD estimates reflect the causal effect of the FMLA because factors that are correlated with employment in experimental states after 1993 might confound the results. Therefore, I next turn to the triple difference estimates to explore the causal effect of the policy change on young women's labor outcomes.

Table 16 presents the estimates from the DDD model. Again, standard errors are clustered at the state level. The upper panel uses young men as the control group, while the lower panel uses older women as comparison. When using young men as the control group, I observe no evidence that the FMLA affects any of the labor market outcomes. The estimates for new hires and employment are negative yet insignificant, while the estimates for wages and starting salaries are positive and still insignificant. In addition, the FMLA has a positive and insignificant effect on separations, and it has a negative still insignificant effect on recalls. Although none of the estimates are statistically significant, it is not clear that the insignificant effects indicate true zero effects or are simply due to insufficient precision. Taking the number of new hires as an example, the point estimate is small and the standard error is six times the coefficient size. Therefore, with 95% confidence the true effect could lie in an interval between -0.038 and 0.032.

Using older women as the control group, I again observe insignificant effects for most of the labor market outcomes. However, I find evidence that the legislation of the FMLA reduces the starting salaries offered to young women by 2.5 percentage points. In addition, the estimate for recalls is positive and significant, indicating that the policy change increases recalls for young women by 3.9 percentage points. Comparing between panel A and panel B, half of the estimates flip signs when using different control groups. The magnitudes of coefficients and the associated standard errors also vary by using different comparison groups. The differences are likely due to imprecise estimation. I do not have strong priors as to which of the control groups is better suited for this research question.

The idea of using labor market flows to measure the immediate impact of a policy change is desirable. However, most data sets available do not provide the information needed for such research design. The QWI contains labor market flow variables and was expected to be the most suitable data set among current available data. Unfortunately, it is not sufficiently powerful to produce informative results.

6. Robustness check

To test the robustness of results, I replicate the triple difference estimates with falsified policy variables. In particular, I first remove the true experimental states, Idaho and Maryland, from the sample, and arbitrarily designate Wisconsin as the placebo experimental state. The remaining three, California, Oregon, and Washington, form the non-experimental states. The choice of states bundled together is based on geography and group size[34]. These placebo estimates are shown in Table 17.

As expected, the placebo policy tests provide noisy estimate results. The magnitude of the estimates are sometimes much larger and sometimes much smaller than the original DDD results. Half of the estimates are significant. In addition, most coefficients have difference signs

[34] Wisconsin has 12,172 observations, which is almost one third of the overall sample size.

compared to the original DDD estimates. The evidence suggests that the sample or the empirical approach may not be sufficient to provide reliable estimates for the impact of the FMLA.

7. Conclusion

Workplace mandated benefits are typically designed to help workers and/or mitigate market failures in the workplace (Summers 1989). However, nonwage benefits might have consequences on employment and wages, both predictable and unintended, due to the costs of providing them. Empirical examination of the effects of mandated benefits is difficult because most data sets available are incapable of identifying small and immediate changes for labor demand and supply. Existing studies on such topics usually use the Current Population Survey, the National Longitudinal Survey of Youth, and the Decennial Census data. However, these data sets either have small individual-level sample sizes or have a prolonged interval between waves. In addition to data limitations, a more fundamental issue is that employment and wages respond to policy change gradually. Therefore, the literature provides limited knowledge on the effects of mandated benefit on labor market outcomes.

This paper seeks to investigate the effects of the FMLA on young women's labor market outcomes using data from the Quarterly Workforce Indicators. I pay particular attention to the immediate impact of the policy change that is measured by labor market flows, including the number of new hires and the starting salaries, as well as the numbers of job separations and recalls. Unlike stock variables such as employment and wages, labor market flows reflect the immediate changes in labor demand and supply, thus they are potentially more informative measuring the effects of the FMLA. In triple difference estimations with young men as the control group, I find no evidence that the FMLA has a significant effect on young women's labor market outcomes. When using older women as the comparison group, I find significant evidence that the policy reduces the starting salaries offered to young women, and it increases recalls among them. Using young men as the comparison group provided very different results. And a placebo test produced coefficients and levels of significance that were as large or larger than the FMLA results.

The impreciseness and inconsistency of evidence is likely due to insufficient detailed data and the small number of "treated" groups coupled with a relatively small number of untreated control states. Although the QWI contains labor market flow variables, it has several shortcomings that hinder precise estimation. As in early 1990s, only six states reported information in the QWI, and two small states, ID and MD, form the group of experimental states. The pre-FMLA period is also short. As shown in Table 13, the earliest data are recorded in the second quarter of 1991, which is only nine quarters before the legislation of the FMLA. In addition, it is impossible to exclude workers from establishments uncovered by the FMLA. The QWI data set has no information on firm size, which is a crucial criterion for FMLA coverage. Moreover, the QWI payroll variables are averaged monthly but not hourly. Thus, worker and employer decisions on working hours independent of the FMLA might affect hiring and starting salaries.

Despite the inherent deficiencies in the analysis, my approach is of some importance in various ways. For policy purposes, it is necessary to examine how workplace mandates affect labor market outcomes. Understanding the effects of past policies, such as the FMLA, provides insights into policies that are currently under discussion, such as paid maternity leave programs and the extension of the current FMLA. In addition, the idea of using labor market flows to capture the immediate impact of a policy change can be applied to estimating the effects of a broad range of mandates that shift labor demand and supply.

8. Tables and figures

Table 12 Maternal Leave Legislation by individual states

State	Weeks of Leaves	Level	Date of Enforcement
California	17	State	Jan. 1992
Oregon	12	State	Jan. 1988
Washington	12	State	Sept. 1989
Wisconsin	6	State	Apr. 1988
Idaho	12	Federal	Aug. 1993
Maryland	12	Federal	Aug. 1993

Data source: Baum (2003).

Table 13 Data periods by states.

	Data period
California	1992Q4-1995Q4
Idaho	1992Q2-1995Q4
Maryland	1991Q2-1995Q4
Oregon	1992Q2-1995Q4
Washington	1991Q2-1995Q4
Wisconsin	1991Q2-1995Q4

Table 14 Summary statistics for labor market outcomes

Variables	Obs.	Mean	Std. Dev	Obs.	Mean	Std. Dev
Panel A: treatment group -young women						
Non-Experimental States				Experimental States		
Employment	10151	3548	16571	3297	1851	5460
Employment wage	10189	997	428	3329	970	343
New hires	9981	456	1799	3145	250	607
Starting salaries	10080	801	832	3217	795	257
Separations	9975	490	2022	3162	312	842
Recalls	9672	159	613	3033	171	508
Panel B: control group 1 -young men						
Non-Experimental States				Experimental States		
Employment	10179	4244	21138	3333	2012	5848
Employment wage	10197	1476	509	3301	1491	520
New hires	9980	531	2255	3201	269	674
Starting salaries	10110	1172	360	3222	1218	411
Separations	10013	569	2506	3205	336	915
Recalls	9793	240	929	3114	199	566
Panel C: control group 2 -older women						
Non-Experimental States				Experimental States		
Employment	10206	4554	17724	3345	2302	5619
Employment wage	10209	1389	383	3348	1319	371
New hires	9913	305	1230	3093	160	396
Starting salaries	10086	894	293	3206	911	336
Separations	10012	383	1551	3156	260	659
Recalls	9705	165	644	3083	182	495

Table 15 Double difference estimates of the effects of the FMLA on young women.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(NH)	Ln(NH salaries)	Ln(separation)	Ln(recalls)	Ln(emp.)	Ln(wages)
FMLA	0.0105	-0.00935	-0.0719	-0.0951	0.0147	-0.0113
	(0.0143)	(0.0152)	(0.0853)	(0.0893)	(0.0401)	(0.0219)
Obs.	13,126	13,297	13,137	12,705	13,137	12,705
R-squared	0.923	0.337	0.917	0.872	0.872	0.275

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are robust and clustered at state level. Sample are restricted to young women aged 19-34. To save space, I use abbreviations for some the outcomes. NH stands for new hires, NH salaries stands for starting salaries for new hires, and emp. for employment.

Table 16 Triple difference estimates of the causal effect of the FMLA on young women.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(NH)	Ln(NH salaries)	Ln(separations)	Ln(recalls)	Ln(emp.)	Ln(wages)
Control 1- young men						
FMLA	-0.00303	0.00217	0.00605	-0.0203	-0.00180	0.0113
	(0.0178)	(0.00897)	(0.0304)	(0.0244)	(0.0181)	(0.00605)
Obs.	26,307	26,629	26,355	25,612	26,960	27,016
R-squared	0.991	0.847	0.991	0.979	0.998	0.954
Control 2- older women						
FMLA	-0.0510	-0.0247**	0.0615	0.0392***	0.00494	0.000587
	(0.0305)	(0.00810)	(0.0352)	(0.00518)	(0.0474)	(0.0220)
Obs.	26,132	26,589	26,305	25,493	26,999	27,075
R-squared	0.989	0.721	0.990	0.978	0.998	0.951

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are robust and clustered at state level. The treatment group consists young women aged 19-34. The upper panel uses young men aged 19-34 as the control group, while the lower panel uses older women aged 35-64 as the comparison. Estimations include quarter fixed effects, county fixed effects, demographic fixed effects, and the pairwise interaction among them. To save space, I use abbreviations for some the outcomes. NH stands for new hires, NH salaries stands for starting salaries for new hires, and emp. for employment.

Table 17 Triple difference estimates of the effect of the FMLA from state group placebo policies

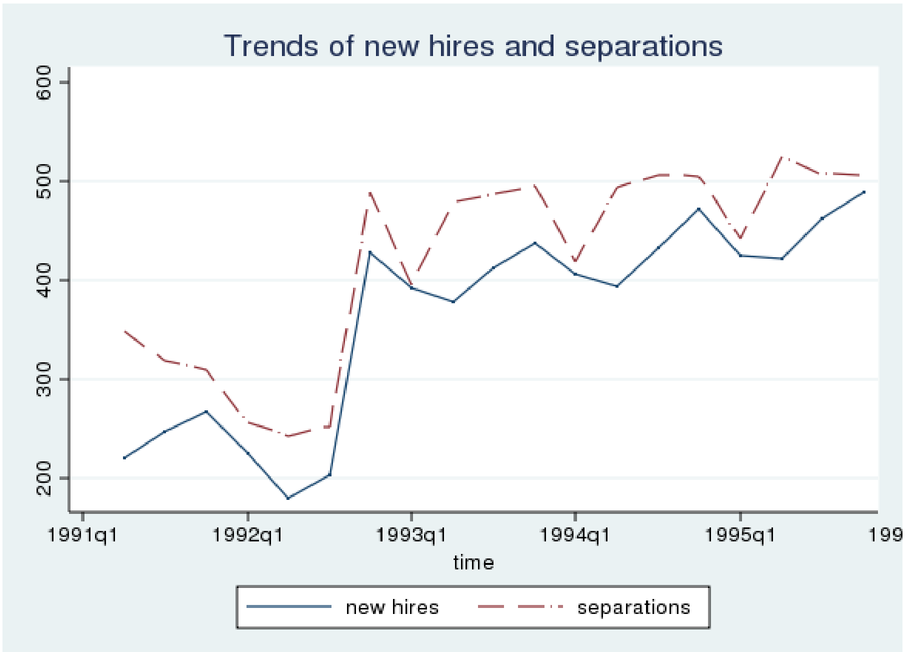
	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(NH)	Ln(NH salaries)	Ln(separations)	Ln(recalls)	Ln(emp)	Ln(wages)
Control 1- young men						
FMLA	-0.0314	-0.00974*	-0.0710***	0.00500	-0.0528**	-0.00656
	(0.0282)	(0.00392)	(0.00532)	(0.0144)	(0.0146)	(0.0100)
Obs.	19,961	20,190	19,988	19,465	20,330	20,386
R-squared	0.992	0.855	0.991	0.979	0.998	0.956
Control 2- old women						
FMLA	-0.0550**	0.00726	-0.0423	-0.0103	-0.0525**	0.0232*
	(0.0137)	(0.00647)	(0.0274)	(0.00799)	(0.0157)	(0.00816)
Obs.	19,894	20,166	19,987	19,377	20,357	20,398
R-squared	0.990	0.734	0.990	0.977	0.999	0.953

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are robust and clustered at state level. The placebo tests replace the experimental states with Wisconsin, while the non-experimental states include California, Oregon, and Washington. Estimations include quarter fixed effects, county fixed effects, demographic fixed effects, and the pairwise interaction among them. To save space, I use abbreviations for some the outcomes. NH stands for new hires, NH salaries stands for starting salaries for new hires, and emp. for employment.

Figure 1 Ratio of labor market outcomes between demographic groups

Note: Each curve represents a ratio of one labor market outcome between young women and young men/ older women, separated by experimental and non-experimental states.

Figure 2 Trends of new hires and separations along the research period.



Note: The two curves represent the changes in new hires and separations along the research period. Each variable is averaged by quarter.

APPENDIX A

Table 18 Summary Statistics for three samples, all estimates are weighted using the child's sampling weight

	Main Sample (N=16,535) Mean	Extended Sample (N=34,939) Mean	Comparison Sample (N=15,602) Mean
Children's Info			
BMI z-score	0.28	0.35	0.24
Overweight	0.27	0.29	0.27
Obesity	0.12	0.13	0.12
Height is self-report	0.51	0.51	0.38
Weight is self-report	0.53	0.53	0.4
Family size, less than 3 persons	0.09	0.22	0.21
Family size, 4 persons	0.36	0.39	0.37
Family size, 5 or more persons	0.55	0.39	0.42
Family income, $1000	60.58	61.56	42.89
Child's age, in year	12.02	11.82	10.67
Child is Hispanic	0.08	0.07	0.08
Child is African American	0.15	0.15	0.19
Attachment to father	0.77	0.77	0.72
Child is female	0.49	0.48	0.48
High birth weight	0.1	0.11	0.1
Breastfed	0.57	0.54	0.45
Mother's Info.			
Edu.,less than high school	0.12	0.11	0.19
Edu.,high school graduate	0.44	0.44	0.52
Edu.,some college	0.23	0.24	0.22
Edu.,college degree or higher	0.21	0.21	0.08
Mother's age	36.64	38.04	32.69
AFQT score (2006 standard)	47.64	47.23	39.47
Married, live with spouse	0.73	0.7	0.66
Mothers' Employment			
Fraction of weeks worked in past month	0.65	0.69	0.65
Hours worked per week in past month, in unit 10	2.32	2.56	2.39
Fraction of weeks worked in past year	0.65	0.7	0.65
Hours worked per week in past year, in unit 10	2.32	2.59	2.39

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