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ABSTRACT

This paper uses a panel of SSA earnings linked to the CPS to estimate the impact of increasing post-childbirth work incentives on mothers' long-run career trajectories. We implement a novel research design that exploits variation in the timing of the 1993 reform of the Earned Income Tax Credit (EITC) around a woman's first birth and in eligibility for the credit. We find that single mothers exposed to the expansion immediately after a first birth ("early-exposed") have 3 to 4 p.p. higher employment in the 5 years after a first birth than single mothers exposed 3 to 6 years after a first birth ("late-exposed"). Ten to nineteen years after a first birth, early-exposed mothers have the same employment and hours as late-exposed mothers, but have accrued 0.5 to 0.6 more years of work experience and have 6 percent higher earnings. Incorporating long-run effects on EITC benefits and earnings increases the implied marginal value of public funds (MVPF) of the expansion. Our results suggest that there are steep returns to work incentives at childbirth that accumulate over the life-cycle.

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In recent decades, motherhood has become an increasingly important factor in the gender gap in earnings (Kleven, Landais and Søgaard, 2019). The substantial "child penalty" in women's earnings has been documented globally across various demographic groups, and has been shown to persist for at least a decade after a first birth.[1] In response to this, there is growing interest in policies that could accelerate or increase mothers' labor force participation as a step towards promoting their career advancement (Rossin-Slater, 2017). Nevertheless, the effectiveness of such policies is unclear because, to date, there is no consensus on the answer to a key question: does going to work sooner after childbirth yield long-run earnings gains for mothers?

Importantly, the gains from promoting work for new mothers are uncertain because of the coinciding and, possibly, overriding, demands of childrearing. On the one hand, part-time work and less-time-intensive occupations are common among new mothers, which may entail a lower return to experience (Goldin, 2014). Women also tend to sort into lower-paying firms, which could be associated with a flatter earnings trajectory (Card et al., 2015). On the other hand, precisely because the opportunity cost of work is higher for new mothers, employers may view work after childbirth as a signal of commitment, which could be rewarded with higher returns to early experience (Thomas, 2019; To, 2018). Reducing time out of the labor force after childbirth may also make it easier to find future employment or encourage mothers to work full-time earlier. These conflicting channels make the size and the duration of the effect of early work experience ambiguous.

In this paper, we estimate the long-run impact of post-childbirth work incentives on maternal labor market outcomes. We obtain variation in work incentives from the 1993 expansion of the Earned Income Tax Credit (EITC), a federal cash transfer program for working families. Effective in 1994, the reform increased the post-tax earnings of low-income families by up to 16%, and thus raised the expected benefit of work, particularly for single mothers (e.g., Meyer and Rosenbaum, 2001). Unlike the prior literature on the labor market impacts of the EITC that uses variation in the number of children across households, we isolate the impact of exposure to the reform *at first birth* by exploiting variation in the timing of a first birth around the reform and in eligibility for the credit.[2] We use a panel of administrative earnings to implement this new design, which allows us to follow the precise career trajectories of mothers around a first birth. We hypothesize that exposure at first birth may lead mothers to begin working sooner after childbirth, accrue valuable work experience, and have higher earnings in the long-run. We test each of these predictions, and use our estimates to calculate implied fiscal externalities of the expansion.

We rely on a large-scale panel of household earnings that we construct by linking two data sources: (i) longitudinal earnings data from 1978 to 2015 and individual date of birth from the Social Security Administration (SSA); and (ii) twenty three years of the March Current Population Survey (CPS), spanning from 1991 to 2016. We use the detailed demographics in the CPS to identify a "high impact" sample of never-married mothers and their children, and the SSA records

[1] See Kuziemko et al. (2018); Nix and Andresen (2019); Kleven, Landais, Posch, Steinhauer and Zweimüller (2019); Angelov et al. (2016); Chung et al. (2017).

[2] We use "exposed at first birth" or "exposed at birth" to refer to mothers who had a first birth in or after 1993.

to track annual earnings and employment (defined as positive earnings) around a first birth for each of these mothers. This gives us annual earnings for roughly ten times as many sample mothers as appear in the CPS in each March survey. Further, we use the snapshot of employment and fertility information in the CPS to provide suggestive evidence on part-time and full-time work, as well as on occupation choice and fertility, which may be potential mechanisms.

We use two complementary research designs to estimate the impact of work incentives after a first birth. First, we estimate a difference-in-difference (DD) model using never-married mothers, nearly all of whom are eligible for the EITC. We compare the post-childbirth outcomes of mothers who are exposed to the 1993 expansion at first birth ("early," i.e whose first birth was in 1993–1996) to the post-childbirth outcomes of mothers who are exposed 3 to 6 years after a first birth ("late," i.e whose first birth was in 1988–1991), and to pre-childbirth outcomes. This approach follows in the spirit of Chetty et al. (2013) and Bailey et al. (2019), who compare treated and untreated mothers around childbirth to estimate the impact of EITC knowledge and paid leave, respectively.

Second, we take advantage of the fact that married mothers are less likely to be eligible for the EITC to implement a triple-difference (DDD) approach. We compare the gap in outcomes between early- and late-exposed never-married mothers to the gap for married mothers, before and after a first birth. This allows us to rule out potential confounders common to all mothers in each calendar year, such as the booming economy or changes in policies or norms around maternal work. Reassuringly, the DD and DDD designs yield similar estimates throughout our analysis, as do alternative DDD comparisons using childless women who become mothers in the future or who never become mothers. This suggests that any bias from unobserved shocks is small.

For both of these designs, we present our results using event studies. In favor of our approach, we find parallel trends between early- and late-exposed mothers prior to childbirth. Moreover, we show that changes in employment upon motherhood were constant prior to the reform.

Using these dual strategies, we find that early-exposed mothers have 3.4 to 3.7 percentage points (p.p.) higher employment in the first five years years after a first birth (which we refer to as the "short run.") This represents a 5.9 percent increase relative to the mean post-birth employment rate of late-exposed mothers, which is equivalent to 18 percent of the 20 p.p. drop in employment in the year after birth. We show that these effects are concentrated among wage earners — rather than self-employed workers — which validates the effects as real changes in labor supply.

We present multiple pieces of evidence supporting a causal link between these short-run changes in employment and the EITC expansion. First, we use an expanded sample of births to show that, consistent with EITC incentives, post-birth employment responses are larger for mothers after a second birth, but not additionally larger after a third or higher-order birth. Second, we use subgroup analyses to demonstrate that our results can not be explained by welfare reform or the economic boom (Kleven, 2019). In particular, we show that our results persist even when we restrict our analysis to the subset of states that did not enact welfare waivers and to the period prior to federal welfare reform; or to states that experienced little change in the unemployment rate in the 1990s. For these reasons, we view our long-run results as a dynamic response to the

EITC reform. However, the precise incentive that causes mothers to work sooner is not critical for our interpretation of later-life effects, which are our focus.[3]

Examining outcomes ten to nineteen years after a first birth (which we refer to as the "long run"), we find that early-exposed mothers have the same employment rate as late-exposed mothers, but have accumulated 0.5 to 0.6 years of additional experience. Early-exposed mothers also earn $1,206–$1,392 ($2016) more on average in the long-run, which is 6% (4.2%) higher than the average earnings of all (working) late-exposed mothers. In total, over twenty years after a first birth, early-exposed mothers earn an additional $36,702 to $37,945 in labor income, up to 41% of which is earned over the long run.

More descriptively, we show that early-exposed mothers' short-run increase in employment is driven by part-time work, but that they transition to full-time work in years five to nine years after a first birth (the "medium run"), when children reach schooling age. This growth in hours has a visibly apparent impact on medium-run earnings, and contributes 70% of an estimated 0.68 years of additional full-time, full-year experience in the CPS. However, we find no effect on hours of work in the long run, which suggests that the long-run increase in earnings that we document reflects higher *wages*.

These results hint at the fact that post-birth work experience may be rewarded with steep returns. As further evidence for this mechanism, we find that the increase in early-exposed mothers' long-run earnings is driven by a rise in the share of mothers who jointly have high earnings (in the top 25%) and also worked during the first three years after a first birth. If experience was the only source of earnings gains, the implied return to a year of full-time, full-year experience would be 6.2 percent. This is within the range of estimates for similar populations (Adda et al., 2017; Gladden and Taber, 2000; Looney and Manoli, 2013; Card and Hyslop, 2005), but our larger shock to experience gives us more precision than other causal estimates (Card and Hyslop, 2005).

We find weaker evidence for other potential mechanisms for increased earnings. Early-exposed mothers appear to be slightly more likely to work in health service occupations in the long-run, but this effect is too small to explain a large share of the increase in earnings. We also examine changes in family structure, but find no impact on completed fertility, birth spacing, or marriage rates. Finally, it is possible that mothers experience higher wages due to increases in post-birth resources (which could facilitate, e.g., better health); however, we argue that the lack of any long-run impact on labor supply makes this less likely.

We estimate that the sustained increase in early-exposed mothers' earnings leads to a reduction in EITC payments and an increase in tax contributions in the long-run. Thus, while early-exposed mothers are expected to receive higher EITC benefits over the short-run, the net fiscal cost of increasing generosity in the long run is much smaller than the gross cost (and possibly close to zero). Incorporating these long-run effects raises the implied marginal value of public funds (MVPF) of the expansion (Hendren and Sprung-Keyser, 2019): our lower bound of the "long-run MVPF" is

[3] In particular, we interpret the long-run effects as a by-product of having worked sooner after childbirth. For this to be valid, we only need exogenous variation in the timing of work after childbirth. This could in principle include responses to welfare policy (although as we discuss, we find evidence against this interpretation.)

around 1.3 (i.e., the value to recipients is at least 1.3 times as large as the cost), which is at least twice as large as the implied "medium-run MVPF." This suggests that policies to promote work after childbirth, such as tax incentives for new mothers, could largely pay for themselves.

Our paper is at the center of two active debates. First, we provide new evidence on the role of lost work experience after childbirth on the long-run child penalty. The most relevant estimates on this topic come from two sources of variation: (i) paid leave extensions;[4] and (ii) welfare experiments (Card and Hyslop, 2005).[5] These studies generally find little effect of changing mothers' time away from work, with a few exceptions showing mixed effects (see, e.g., Rossin-Slater, 2017; Bailey et al., 2019). However, they also utilize relatively small changes in experience (e.g., 0.2 to 0.3 years in Card and Hyslop, 2005), which may make it difficult to detect an impact on long-run earnings. Additionally, paid leave reforms commonly include job protection, which could mitigate the negative effects of a longer leave (Stearns, 2018). Finally, paid leave often requires mothers to be back at work within a year of childbirth, which makes it difficult to extrapolate to the 40% of mothers who remain out of the labor force for a longer period (Laughlin, 2011).

Our study has several unique features relative to this body of work. First, we leverage variation from substantial reductions in non-employment *beyond* the first year after childbirth. Our impacts on employment are largest in the first six years after birth, but extend up to nine years after birth. This large shock provides us with substantial power to identify the return to working in the first years after birth. Second, we can estimate long-run impacts on *wages* because we find convergence in employment and hours. In contrast, the paid leave studies that find long-run impacts on earnings also find significant reductions in employment (e.g., Schönberg and Ludsteck, 2014; Bailey et al., 2019).

Third, we provide new estimates of the returns to post-birth work experience for single mothers, who are not commonly studied in this literature. The closest benchmarks come from non-experimental or quasi-experimental studies, including Looney and Manoli (2013), who estimate an insignificant 0.4% return using variation in experience across synthetic cohorts of US single mothers; Gladden and Taber (2000), who estimate a 4–5% return for low-skilled US women using potential experience as an instrument; Adda et al. (2017), who estimate a 9–12% return to experience using individual variation across new mothers in Germany. However, these estimates are subject to concerns about measurement error in self-reported earnings and experience (Looney and Manoli, 2013; Gladden and Taber, 2000) and endogenous experience (Looney and Manoli, 2013; Adda et al., 2017). Card and Hyslop (2005), instead, leverage a series of welfare experiments in Canada and estimate an insignificant -7% return (but have little variation, as mentioned above). We thus provide the most precise evidence of the *causal* return to experience for low-income mothers.

[4]These include Schönberg and Ludsteck (2014), Lalive et al. (2013), Lalive and Zweimüller (2009), Dahl et al. (2016), Stearns (2018), Lequien (2012), Canaan (2019) in European contexts, or Bailey et al. (2019) and Rossin-Slater et al. (2013), in the US context. For a summary, see Rossin-Slater (2017).

[5]Expansions in child care availability or changes in fertility provide two other potentially useful sources of variation in maternal experience. To our knowledge, there are no estimates of the effect of the availability of child care on experience. Lundborg et al. (2017) measure the impact of fertility on work experience, but those estimates are not comparable to ours since children are a potential confound for impacts on earnings.

We also contribute to the literature on the effect of the EITC on female labor supply (e.g., Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Hotz and Scholz, 2006; Eissa and Hoynes, 2004; Chetty et al., 2013; Neumark and Shirley, 2017; Hoynes and Patel, 2018; Bastian, 2018; Kleven, 2019; Bastian and Jones, 2020; Wilson, 2020). Existing studies focus on short-run responses, with the exception of Neumark and Shirley (2017) who find imprecise impacts on long-run earnings in the PSID, but also have a small sample of women.

Relative to these studies, we provide the first short- and long-run estimates of the impact of a more generous EITC at first birth on labor market outcomes. To identify these effects we implement a new panel DD identification strategy, which, unlike prior work, allows us to flexibly control for potential selection into giving birth and to inspect for trends in labor market outcomes prior to a first birth. Moreover, we provide transparent and precise evidence of long-run impacts of the 1993 expansion, which improves upon the suggestive results in Neumark and Shirley (2017).

We also contribute to a smaller strand of the EITC literature that considers the benefit-cost ratio of the EITC using short-run, static estimates of impacts on labor supply (Bastian and Jones, 2020; Hendren and Sprung-Keyser, 2019). We improve upon earlier estimates for this critical component of the safety net by (i) directly estimating the mechanical and behavioral change in EITC benefits, which are necessary inputs for the MVPF calculation; and (ii) incorporating long-run effects on EITC benefits, income tax revenue, and program participation for never-married mothers. Importantly, we show that the long-run MVPF for this population is at least twice as large as a "medium-run" MVPF. This suggests that prior estimates of the EITC MVPF may underestimate the long-run MVPF (Bastian and Jones, 2020; Hendren and Sprung-Keyser, 2019).[6]

1 Background

The EITC is a refundable tax credit that is currently one of the largest cash transfers to low- and middle-income households in the United States (Nichols and Rothstein, 2015). In 2014 there were 28.5 million EITC recipients — roughly 1 in 5 tax filers — who received a total of $68.3 billion (Bitler et al., 2017; Hoynes and Patel, 2018). Single mothers make up the largest group of taxpayers eligible for the credit, and receive almost 75% of EITC dollars (Bitler et al., 2017). Married couples with children make up the second-largest group, and receive 20% of EITC dollars. Benefits are claimed by filing a tax return, and refunds are typically issued within a few weeks (Nichols and Rothstein, 2015).

Eligibility for the EITC depends on two key inputs: number of qualifying children and household earnings.[7] Although the EITC is available to childless households, the childless credit is restricted to very low income households and is quite small – e.g., in 2016, the maximum credit was $506,

[6]Because we focus on never-married mothers, our MVPF is not directly comparable to the earlier MVPF estimates (Bastian and Jones, 2020; Hendren and Sprung-Keyser, 2019), which incorporate effects for the whole population. In that sense, our MVPF is most relevant for evaluating the benefits of a hypothetical policy that incentivizes post-birth employment for single mothers.

[7]A qualifying child must be in the household for at least half of the tax year and either be under the age of 19 (24 if in school full-time) or permanently disabled.

and was only available to families with annual earnings between $6,610 and $8,270. The maximum credit for families with one child (two children) is typically six (ten) times higher than the childless credit. Thus, in practice the EITC is primarily a child-based credit.

Conditional on number of children, the EITC amount varies non-linearly with household earnings (wages plus self-employment). To fix ideas, Panel (a) of Appendix Figure A.1 shows the EITC schedule for households with one child in 1993, 1994, and 1995. To receive any credit, a household must have earned income that is positive, but does not exceed a household-size-specific earnings threshold. Within that range, EITC benefits are first calculated as a percent of household earnings up to a yearly maximum credit (in the "phase-in region"), then are flat, and then are equal to the maximum benefit less a fixed percent for each additional dollar earned (in the "phase-out" region).

Outside of small inflation adjustments, the maximum federal EITC credit for one-child households was increased twice over our period of study. The larger of the two, the 1993 EITC expansion, is the focal point of our analysis.[8] Effective in 1994, the expansion increased the real maximum credit for one-child families ($2016) from $2,381 to $3,300, and augmented benefits at every level of eligible earnings (see Panel (a) Appendix Figure A.1.) The minimum real earnings to qualify for the maximum credit was initially set as $12,550 in 1994; but the following year, this was reduced to $9,701, which made the more generous credit available to a broader set of households with low incomes.

To consider the impact of the 1993 expansion on individuals' behavior, it is useful to scale the change in benefits by household earnings in each EITC region. Panel (b) Appendix Figure A.1 shows that, as a percent of pre-tax earnings, the additional benefits represented 8% growth in the "phase-in" region (or 16%, accounting for the 1995 adjustment), 5 to 8% growth in the "flat" region, and 0 to 2% growth for most of the "phase-out" region. Put differently, post-reform low-income households could expect to receive the equivalent of an additional month's wages in the phase-in, or three-quarters of a month's wages in the flat region. On the margin, this would be expected to encourage more low-income mothers to work.[9]

The 1993 reform also raised the real maximum credit for households with two or more children, more so than the one-child credit (see Appendix Figure A.2).[10] The real maximum credit for these families rose by $1,584 in 1994, and by an additional $1,400 over the next two years. Because of our interest in first births, this variation is less relevant for our main analysis; however, we return to exploit the variation in EITC benefit amount as a secondary identification strategy in Section 4.3.

Consistent with the substantial financial incentives at stake, EITC take-up during this period is quite high, between 80 and 86 percent (Scholz, 1994), based on data from the 1990 tax year.

[8]The second, smaller EITC reform during our period was in 1990, and raised the maximum credit from $1,750 to $2,381 over three years. We emphasize this reform less because it contributes relatively little to our identifying variation, as we discuss in Section 1.1.

[9]There could also be intensive margin responses, although knowledge of the non-linear incentive structure of the EITC appears to be limited, which makes extensive margin responses more likely (Chetty and Saez, 2013).

[10]The 1993 expansion also established the first credit for households with no dependents, although this is less relevant for our analysis because we difference out pre-birth outcomes.

This compares favorably with the take-up of other low-income programs around the same time, such as traditional welfare (AFDC, which has 60–65% take-up) or Food Stamps (which has 55-60% take-up) (Blank and Ruggles, 1996). Further, Saez (2010) finds strategic "bunching" at the first kink of the EITC schedule beginning in the early 1990s, suggestive of spreading awareness of the program's incentives.

Welfare Reform Along with the 1993 EITC expansion, the other major policy development for single mothers in the 1990s was a series of reforms that tightened the requirements for cash welfare. Modifications to welfare took place first through piecemeal waivers at the state-level (1992-1996) and then nationally with the replacement of traditional welfare with the Temporary Assistance for Needy Families (TANF) program in 1996. The reforms included several key elements intended to encourage work among recipients: work requirements, time limits on the duration of welfare, sanctions, and earnings disregards.

The close timing of these events with the EITC reform raises some challenges for the identification of EITC effects, as recently highlighted in Kleven (2019). Nevertheless, because the timing and details of welfare and other low-income policies vary across states, we are able to control for these in our analysis, which we do at baseline and with increasing flexibility in Section 4.3. An alternative approach could be to exploit changes in welfare as a secondary source of post-birth work incentives. Doing so would change the policy attribution of our short-run effects but would be immaterial for the interpretation of our long-run effects as stemming from early work incentives. In that sense, while we are careful to show that our results are not driven by other policies, our long-run results would remain valid even if our estimates incorporate spillovers from welfare policies.

1.1 Identifying Effects on *New* Mothers

By substantially increasing the expected benefits of working, the EITC expansion created a sharp increase in the incentive to work for all mothers in 1994. Multiple papers study the employment response to this incentive. Our goal is to identify whether a mother that experiences this incentive *immediately* after a first birth, and thus begins working soon after birth, has better labor market outcomes than a mother that experiences the incentive, e.g., five years after a first birth, after potentially having been out of the labor force for some years.

To illustrate the variation in work incentives for *new mothers*, we compute the average maximum EITC credit available in each year around a first birth for two groups of interest. "Early-exposed" mothers have a first birth between 1993 and 1996 and therefore are exposed to the EITC expansion at or around birth. "Late-exposed" mothers have a first birth between 1988 and 1991 and therefore are exposed to the EITC expansion three to six years after birth.[11] For simplicity, we assume that all mothers have at most one child, and we assign them the no-child EITC before a first birth

[11] We omit 1992 first-births in these comparisons in order to create a larger difference in the benefits of early- and late-exposed mothers. We include 1992 first births when we examine outcomes across more-continuous bins of cohorts (see, e.g., Appendix Figure A.9).

and the one-child EITC after birth. Because EITC benefits became larger for 2-child families after 1993, this will underestimate the gap between early- and late-exposed mothers' maximum benefits if early-exposed mothers have more than one child (see Appendix Figure A.3).

Panel (a) of Figure 1 shows that early-exposed mothers are eligible for higher maximum one-child credit than late-exposed mothers for the first five years after childbirth. The gap in incentives is $1,222 at birth (year 0); $1,185 to $1,329 in years 1 and 2, $500 to $800 in years 3 and 4, and near zero in year 6. The vast majority of this difference (75%) is generated by early exposure to the 1993 reform. Consequently, early-exposed mothers could be expected to work more for at least the first five years after birth while there remains a gap in EITC incentives. Panel (b) of Figure 1 shows the gap in EITC incentives between early- and late-exposed mothers over twenty years after birth. The only period when there is a meaningful gap between these groups is in the first six years after a first birth. This ensures that long-run differences in behavior can not be due to differences in contemporaneous EITC incentives.

Our analysis leverages this variation in EITC incentives in the years around first birth to identify the effect of early work incentives. In particular, we compare the gap in career outcomes between early- and late-exposed single mothers, pre- and post- birth, in a difference-in-difference design. To control for other coinciding changes, we compare gaps across eligible moms and less-eligible women in a triple-difference design. Our main group of less-eligible women are married mothers, but our results are the same when we instead use childless women in Section 5.2. We formalize our estimation strategy in Section 3.

2 Data

Our analysis takes advantage of a novel link between two Social Security Administration (SSA) administrative databases that include individual earnings records and survey responses from the 1991, 1994, and 1996 to 2016 Annual Social and Economic Supplements of the Current Population Survey (CPS). The CPS is an annual survey of 60,000 households that collects information on demographic characteristics as well as on recent labor market activity and program participation. It is crucial that we have both these sources of data, as neither one is sufficient for our purposes: the administrative data do not have any demographic information, and the CPS have just a single year of reported earnings, which are potentially mismeasured.

We use the CPS survey responses primarily to obtain demographics for our sample. CPS-provided parent identifiers allow us to connect parents and children in the survey, which we use to identify the first birth for each woman and to measure her total fertility. We also observe marital status, which we use to assign treatment, as well as race (white, black, hispanic, or other), age, completed education (less than or equal to high school, some college, or college graduate), and state of residence, which serve as control variables. Because we assign demographics at the time of the CPS survey, rather than at first birth, this introduces a form of measurement error to our analysis. This is a particular concern for marital status, which we use to assign treatment. We provide a detailed discussion of potential sources of bias from mismeasurement, and of evidence showing that this is not empirically relevant for our results, after we introduce our empirical strategy, in Section 3.1.

The CPS also serves as a supplementary source of information on labor market outcomes and program participation. The labor outcomes of interest are hours worked in the past week, weeks of work last year, and current occupation (grouped into 15 categories as in Appendix C.1), which allow us to explore intensive margin responses. We also take advantage of information on the value of benefits from public programs for our calculations of fiscal externalities.

Our main labor market outcomes are obtained from SSA earnings records (the "Detailed Earnings Record" files). Earnings information includes aggregate annual wages, salary, and tips from Box 1 of the W-2 form as well as earnings from covered self-employment from Form 1040-SE. We have access to earnings from 1978 to 2015 for individuals that appear in the CPS. We convert all dollar values to 2016 real dollars using the CPI from the Bureau of Labor Statistics. From these records, we construct "total earnings" which includes the aggregate earnings from all W-2 forms ("wage earnings") and self-employment filings ("self-employment earnings"). We also calculate "household earnings" which is equal to total earnings for single individuals and is equal to the sum of own and spouse's total earnings for married individuals.[12] If an individual has positive total earnings, we consider her to be employed during the year.[13]

Along with this earnings information, we have access to the SSA NUMIDENT file, which contains information on individuals' exact date of birth. We use this to determine year of birth for mothers and children. We then determine birth order among children by sorting on this year of birth within a mother, and set the year of first childbirth as the year of birth of the eldest child.[14]

We match the SSA records to the CPS using a unique identifier (PIK) created by the Census Bureau. Across all CPS years, we match between 75% and 80% of the women that meet our sample criteria. Match rates are similar by year of first birth and marital status, and are generally similar across CPS years. For details on the matching procedure and match rates, see Appendix Table A.26 for details. The one exception to this is the 2001 CPS, which we drop for having a particularly low match rate.

Core sample We construct our core sample of first-time mothers from the set of matched individuals. First, we limit the sample to women interviewed in the CPS before age 50, whose children are more likely to have been present at the time of interview. Second, we drop women that had a first birth at age 18 or younger. We do this to limit the role of high school attendance in our results, and because these women could be claimed as dependents themselves. Third, we only keep women who are in our early- or late-exposed birth cohorts, who had a first birth between 1988–1991 or 1993–1996. To examine broader trends, we create an extended sample that retains all first births

[12] Spousal information is also subject to measurement concerns, which we address in Section 3.1.

[13] We discuss differences in individual earnings and employment across the CPS and SSA records in Appendix C.2.

[14] In the few cases were the numident year of birth for children differs by more than 5 years from the CPS age - year - 1, we assume that the numident match is incorrect, and use CPS age - year - 1.

from 1986 to 1999.

Fourth, in a similar spirit to earlier work, we use never-married mothers as a "high-impact" sample. To validate this choice, we use the three years of pre-birth household earnings to project likely EITC eligibility after a first birth. Based on pre-birth earnings, nearly all of never-married women are likely to be eligible for the EITC: 97% have earnings that fall strictly below the maximum earnings for one-child EITC benefits.[15] They also would be expected to highly benefit from the 1993 reform: the average working woman could expect the reform to increase her earnings by 8 percent (recall Appendix Figure A.1). Further, unlike divorcees and widowers, who are often included in the EITC "high impact sample," we can be certain that the never-married mothers that we observe were also single at the time of first birth. This combination of factors gives us confidence that never-married mothers would be highly-eligible for the EITC at the time of first birth.

For analogous reasons, we identify married mothers as a "low impact" sample. Based on pre-birth household earnings, 49% of married households are likely to be eligible for some EITC benefits. However, because married households have higher earnings, the 1993 reform would have a smaller percent effect on household earnings. Even if we conservatively exclude spousal earnings, the average working married woman's earnings place her in the phase-out region, and thus she would be expected to experience a 2% increase in her earnings post-reform.

One potential complication in including married mothers is the finding in Eissa and Hoynes (2004) that the EITC expansion reduced the employment of low-educated married mothers. However, this may be less relevant for our sample. First, the majority of our married sample has more than a high-school diploma, which puts them outside of the Eissa and Hoynes (2004) sample. Second, the responses in Eissa and Hoynes (2004) were concentrated among mothers with at least two children, who obtain more generous EITC benefits than the first-time mothers in our sample. Nevertheless, we empirically test for these effects in Section 4.

Appendix Table A.1 provides summary statistics for our final sample by marital status. The sample consists of 11,291 never-married women and 97,288 married women, who we each observe for 25 years (five years before and up to 20 years after they first give birth).[16] Never-married women are more disadvantaged than married women in nearly every dimension: more likely to be non-white, have a high school or less, and, conditional on working pre-birth, have half the earnings of married women ($12,074 vs. $24,672.)

All births sample In Section 4.3, we compare the change in post-birth employment after a second-or-higher-order birth compared to a first. Thus, we create a separate sample that includes any childbirth that occurred between 1988–1991 or 1993–1996, and treat each childbirth as a separate event. We do this by creating 10-year mother-birth panels around each birth, and then stacking these panels.

[15] Different than other studies of the EITC, we do not further restrict the sample by education because (i) education is typically observed many years after birth, and is potentially an outcome of the policy; (ii) we find a high rate of pre-birth EITC eligibility among all education groups for never-married women.

[16] A given CPS survey-year has roughly one-tenth as many responses from women that meet our criteria.

State-level controls We obtain annual measures of state-level economic conditions and policy parameters used in Bitler and Hoynes (2010), which we merge to our data using each woman's state of residence. These include the unemployment rate, the maximum level of AFDC/TANF benefits, the minimum wage, the mean poverty threshold for Medicaid, and an indicator for whether a state has implemented any welfare reform (waiver or TANF.) We also create indicators for the presence of each of six types of welfare waivers in a state using the dates of implementation from the tables in Crouse (1999) (as in Kleven (2019)), as well as additional information from the tables in Gallagher et al. (1998).[17]

Supplemental data For supplemental information on child care costs and marriage patterns during this period, we turn to the Survey of Income and Program Participation (SIPP). State-level measures of weekly child care costs are constructed by averaging the reported costs of SIPP mothers with a child under age 3 in the 1990–1993 panels. We use these measures to examine heterogeneous employment responses in Section 4.3. We also use the marital and fertility histories reported in the 1990, 1993, 1996, 2001, 2004, 2008, and 2014 SIPP panels to examine post-birth marriage patterns over this period in Section 3.1. For further detail on the construction of these data, see Appendix C.3.

3 Estimation Strategy

Our goal is to identify the causal effect of early exposure to work incentives after a first birth on labor market outcomes. Following the framework in Section 1.1, we start with a dynamic DD analysis that estimates the difference in labor outcomes between early-exposed and late-exposed never-married women in each year relative to first birth. Denoting an individual as i, the year of first birth as b, and the year relative to first birth with τ, we estimate:

$$Y_{ib\tau} = \alpha + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}(\tau = k) \cdot EarlyExposed_b + \theta_\tau + \chi_b + \gamma X_{is\tau} + \delta P_{s\tau} + \epsilon_{ib\tau} \tag{1}$$

The summation term represents the difference in outcomes between early- and late-exposed never-married women, for each year relative to a first birth (i.e., event-time), τ, where $EarlyExposed_b$ is an indicator for having a first birth between 1993–1996. We omit $\tau = -1$, such that these coefficients are estimated relative to the difference in outcomes in the year before childbirth. θ_τ capture the behavior of late-exposed mothers in each year relative to first birth. χ_b are year-of-childbirth fixed effects, which capture differences in average outcomes in the year prior to childbirth across childbearing cohorts (e.g., selection into giving birth or new policies across cohorts.) Because year is a linear combination of event-time τ and year of childbirth b, we can not also include year fixed

[17]These waiver types include changes to: (i) time limits for welfare receipt; (ii) exemptions from participation in the JOBS (Job Opportunities and Basic Skills) program; (iii) sanctions for non-compliance with JOBS requirements; (iv) earnings disregards; (v) family caps (reductions in benefits for children conceived while on AFDC); (vi) time limit for not complying with work requirements.

effects in this model – we address this limitation in the DDD design.[18] $X_{is\tau}$ includes fixed effects for mother's year of birth, age, state, race, and education group. Moreover, we include interactions between race and education fixed effects and an indicator for post-birth to account for potential differences in maternal employment across groups. $P_{s\tau}$ controls for state-level variables, including the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, the adoption of any welfare reform (TANF or waivers), the adoption of six different types of welfare waivers, as well as an indicator for the implementation of the 2009 EITC reform.

To summarize the EITC treatment effect, we replace the τ indicators in the summation with a $PostBirth_\tau$ dummy, which is equal to one in the year of childbirth and the following years. We interpret this as the DD intent-to-treat impact of exposure to EITC incentives at first birth. We discuss the assumptions needed for the validity of this and the DDD estimates in Section 3.1.

Second, we introduce married mothers as a comparison group for never-married mothers, based on the lower relative importance of the EITC for married households. This sets up a DDD design, comparing never-married mothers to married mothers, for early- and late-exposed births. Using NM_m as an indicator for being a never-married woman, we estimate the following dynamic DDD model:

$$\begin{aligned} Y_{imb\tau} = \; & \alpha + \sum_{k \neq -1} \beta_{k,DDD} \cdot \mathbb{1}(\tau = k) \cdot EarlyExposed_b \cdot NM_m + \\ & \theta_\tau \cdot \lambda_b + \theta_\tau \cdot \rho_m + \lambda_b \cdot \rho_m + \gamma_m X_{is\tau} + \delta_m P_{s\tau} + \epsilon_{imb\tau} \end{aligned} \tag{2}$$

The $\beta_{k,DDD}$ coefficients trace out the DDD, comparing the difference between the gap in outcomes for early- and late-exposed never-married mothers and the gap for early- and late-exposed married mothers. We include fixed effects for marital status, ρ_m; year of childbirth, λ_b; and years since birth θ_τ; as well as the two-way interactions between these. Importantly, the inclusion of year-of-birth-by-years-since-birth fixed effects, $\theta_\tau \cdot \lambda_b$, allow us to control for year-specific shocks to the labor supply of mothers with children of a particular age. This could include, for example, changes in federal policies protecting mothers' jobs after childbirth, broad tax policy (e.g., child tax credits), or the availability of new technology for caring for infants.[19] Moreover, we allow all individual and state-level controls, $X_{is\tau}$ and $P_{s\tau}$, to vary by marital status. As with the DD, we also replace the indicators for each year around birth with a $PostBirth_\tau$ dummy to estimate average treatment effects.

For all analyses, we include standard errors clustered at the state level. To account for potential correlated shocks across states, we also obtain confidence intervals using randomization inference and include those results in Section 5.2.

[18] This limitation is analogous to the omission of quarter-of-birth-by-year effects in Bailey et al. (2019). Within-year comparisons of early- and late-exposed mothers (that omit τ) produce similar results – see Section 5.2.

[19] Our results are robust to allowing these year effects to vary by state — see Section 5.2.

3.1 Identification Assumptions and Testable Implications

Common trends Our first identification assumption is that early- and late-exposed never-married mothers share common trends in labor market outcomes prior to "treatment." Treatment in our setting is defined both by becoming a mother and by having a first birth as of 1993. It would thus be problematic if either (i) early-exposed women were increasing their labor supply faster *pre-birth* relative to late-exposed women; or (i) mother's *post-birth* employment was changing across child cohorts *prior* to 1993.

We implement two tests of this assumption. First, we check for "pre-trends" before giving birth, i.e., changes in *pre-birth* labor market participation across cohorts. Second, we look at whether women's *post-birth* employment was changing across child cohorts *prior* to 1993. Previous studies of the 1993 EITC expansion have shown evidence along the lines of our second test. Our novel panel allows us to validate this assumption in its entirety. Both of these tests pass easily in Sections 4.1 and 4.3.

As a more stringent extension of these tests, we also check for balance in the demographic characteristics and pre-birth *levels* of work across early- and late-exposed mothers in Panel A of Appendix Table A.2. We see no difference in age at first birth, household EITC eligibility pre-birth, or annual earnings conditional on working. However, early-exposed mothers are more likely to be white, work a higher share of years pre-birth, and have higher levels of education (although since education is reported after childbirth, this could be an outcome of a higher EITC – see Manoli and Turner, 2018). Importantly, these differences have no impact on employment rates leading up to birth, as shown in our pre-birth trends, which suggests that they are also unlikely to explain differences in work after birth. Further, controlling for these characteristics, either explicitly (our baseline approach) or through inverse p-score reweighting (in Section 5.2) makes little difference to our results. Thus, differences in individual characteristics do not appear to affect our findings.

Contemporaneous shocks A second assumption is that there are no other contemporaneous shocks that differentially affect the labor supply of early- and late-exposed never-married mothers. For example, the 1993 EITC expansion was concurrent with the 1990s economic expansion and followed by welfare reform, both of which could also have also raised maternal employment.

We address this first by following prior work in controlling for the generosity and availability of a suite of government programs across states as well as for state unemployment rates in our baseline specifications. Additionally, we show our results are robust to a variety of alternative parameterizations of these controls (see Sections 4.3 and 5.2). The movement in the DD and DDD coefficients across more flexible control specifications suggests that welfare reform can explain at most 20 percent of our estimates.

Second, we use our DDD design to rule out potential unobserved shocks to early-exposed mothers (e.g., changes in cultural norms around mothers working, changes in firm' policies in promoting women). Our main DDD estimates use married mothers as a comparison group, who allow us to absorb common shocks to mothers giving birth in a particular year. To account for unobserved shocks specific to *unmarried* women, we also present two alternative DDD designs. In the first design, we compare never-married mothers with two or more children to those with one child, and find larger effects for two-or-more-child families, consistent with EITC incentives (see Section 4.3.) We also do not find a differential response within multiple-child families (i.e., for mothers after a third-or-higher-parity birth versus a second birth), consistent with EITC incentives. In the second design, we use two groups of never-married women without children as comparison groups, future mothers or women that never become mothers, and find similar results across these (see Section 5.2). A confound that survives these tests would have to involve shocks that are larger for mothers with one or two children after 1993, that are similar for mothers with two or three children after 1993, and that do not change with time for women that will become mothers or who never become mothers.

Third, we can conservatively interpret our long-term results as reflecting the weighted sum of work incentives stemming from the EITC and welfare changes. As mentioned in Section 1, this would not change our interpretation of the long-run results as stemming from exogenous changes in early employment (and would only minimally change the attribution of our short-term results.)

Measurement error Finally, because of our reliance on survey measures of marital status, we require an additional assumption that rules out selection bias stemming from a correlation between the timing of exposure to the EITC, never-married mothers' decisions to marry, and earnings growth. This could be violated, for example, if early-exposed mothers who are induced by the EITC to have strong earnings growth marry at lower rates than both late-exposed mothers and early-exposed mothers who experience weak earnings growth. Never-married early-exposed mothers that "survive" to be found in the CPS would thus have a steeper earnings trajectory than the average early-exposed mother, which would, in turn, bias our estimates upwards.

To gauge the bias from selective marriage, we need a measure of the difference in post-birth marriage rates across early- and late-exposed mothers in our sample. The raw difference in the share married between early- and late-exposed mothers is 1.9% (90.6% vs 88.7%). However, this gap combines differences in marriage rates at first birth with differences in the rate of marriage after a first birth.[20] If this gap were driven primarily by post-birth marriage rates, we would expect the gap to grow over time. Instead, we find that the gap in marriage rates is unchanged whether we look at surveys 7 years or 20 years after first birth, suggesting that early- and late-exposed mothers marry at similar rates after birth. To confirm this, we use the rich marital histories in the SIPP to calculate marriage rates among mothers who had a first birth in the same years as our sample and who were never-married at first birth. Appendix Figure A.4 shows that SIPP "early-" and "late-exposed" mothers have identical marriage rates after a first birth, suggesting that there is no impact of early exposure on marriage rates.

As another test for selective marriage, we check whether the gap in characteristics between early- and late-exposed mothers widens in surveys further from first birth. Specifically, we regress a series

[20]Other studies of the EITC also typically find small impacts on marriage (Dickert-Conlin and Houser, 2002; Ellwood, 2000; Bastian, 2017) as well as on fertility (Baughman and Dickert-Conlin, 2003).

of individual characteristics on a linear trend in "survey years from first birth" interacted with an indicator for being an early-exposed mother. Appendix Table A.3 shows that the coefficients on this interaction are always insignificant, including for levels of earnings after a first birth. Further, the estimates are typically negative, implying that, if anything, early-exposed mothers are relatively more negatively selected due to attrition. Along similar lines, we also show our results are unaffected by dropping CPS surveys further from first birth, where selection is likely to be more severe (see Section 5.2).

There are also two more minor potential measurement issues. The first of these is that we observe a higher fraction of early-exposed mothers in the years immediately after birth (by virtue of only linking CPS's in 1991 on), and thus require that mothers that we observe closer to first birth are not positively selected on unobservables. We test for this by verifying that our results are robust to dropping individuals from CPS surveys closer to birth (see Section 5.2.)

The second more minor measurement issue is that we may misassign child birth order by including observations from women up to age 50, when children may have already left home. We test for this in Section 5.2 by restricting our sample to women observed at younger ages, and find similar results.

4 Short-run Impacts on Working After a First Birth

We focus first on the impact of work incentives on labor market outcomes in the five years after a first birth ($0 \leq \tau \leq 4$). This is a natural starting point for the analysis because the gap in work incentives is largest during this window, which generates a clear prediction for impacts on mother's careers.

4.1 Employment

Panel (a) of Figure 2 presents regression-adjusted means of employment around a first birth for early- and late-exposed never-married women. Leading up to birth, both groups of mothers show a roughly constant probability of working, exhibiting little, if any, anticipatory response to pregnancy. In the year of birth, employment for both groups falls by 13 p.p., a 20 percent decline from pre-birth levels. Late-exposed mothers' employment falls 7 p.p. further in the year after birth, and remains at this lower level. Early-exposed mothers, instead, experience a smaller fall in employment in the year after birth, and maintain relatively higher levels of employment for the next three years.

Panel (b) of Figure 2 presents our DD event study, which takes the difference between these two series. The coefficients hover around zero in the years leading up to birth, indicating that early- and late- exposed women were not trending differentially prior to childbirth. In the year after birth, early-exposed mothers have roughly 3 p.p. higher employment, which grows to 6 p.p. in the following year, and remains steady thereafter. The fact that the effect on early-exposed mothers' employment levels off after the first two years suggests that the response to work incentives was relatively immediate.

The DDD event study, shown in panel (c) of Figure 2, is almost identical to the DD. This is consistent with there being little change in married mothers' employment behavior across cohorts, which we visually confirm in Appendix Figure A.5.

Table 1 presents estimated effects on employment. As a way of empirically validating our DDD strategy, we present DD treatment effects for both never-married and married mothers. We include in column 1 the additional (maximum) EITC credit available to early-exposed cohorts, $1,009, as one way of scaling these effects. This amounts to an 8.5% increase in earnings relative to pre-birth average earnings among never-married workers.

Our primary DD estimate in column 2 shows that never-married mothers' post-birth employment increases by a 3.7 p.p. ($p < 0.01$) in response to higher work incentives, which is a 5.9 percent increase relative to late-exposed mothers' employment. This represents an 18% recovery relative to the drop in employment in the year after birth. It also implies an elasticity of employment to labor earnings between 0.54 and 0.72, based on early-exposed mothers' expected percent change in EITC benefits (using late-exposed mothers' post-birth earnings – see Appendix D for details).[21]

Column 3 shows that married women, in contrast, have no significant change in employment (point estimate = 0.003). To look for potential heterogeneity in these results, Appendix Table A.4 separately estimates effects based on pre-birth household EITC eligibility. We find a positive effect only among *eligible* never-married mothers, and no effect among any subset of married mothers.[22] The DDD coefficient in column 4 of Table 1 is thus very similar to the DD effect (3.4 p.p.).

Impacts on positive wage earnings, shown in the following columns, are only slightly smaller than our initial effects on employment (by 13% for the DD and 9% for the DDD). Since wage earnings are reported through third-party services, and therefore are not susceptible to manipulation, this result is consistent with the fact that our employment effects are driven by a real increase in work. In line with this, we find that early-exposed mothers' self-employment rises by less than 1 p.p. (see Appendix Table A.5).

4.2 Distribution of Earnings

Figure 3 shows the effect of early-exposure on the distribution of mothers' earnings in the first four years after first birth.[23] We examine a shorter window here to focus on mother's jobs when they initially join the workforce after birth. For reference, we label the areas of the distribution corresponding to the 1994 EITC phase-in, flat, and phase-out regions, as well as the poverty threshold.

[21] Our point estimate is close to the 3.1 p.p. effect for all single mothers in Meyer and Rosenbaum (2001), and our elasticity is similar to the estimate in Chetty et al. (2013) for zip codes in the top decile of EITC knowledge, 0.60.

[22] One explanation for this is the small incentive for married households (particularly after a first birth), discussed previously. A second explanation is that the null effect reflects a mix of positive responses among primary earners and negative responses among secondary earners (Eissa and Hoynes, 2004). Contrary to this, we find insignificant results even in households where the wife is the primary earner.

[23] The figure shows estimates from DDD regressions where the outcomes are indicators for having earnings above X, with X = 0, 2500, 5000,....80,000, i.e., 1-CDF (Duflo, 2001). For the DD estimates, see Appendix Figure A.7.

We find a significant increase in density in the phase-in and flat regions, where the increase in EITC credits was largest. Effects in the phase-out region are small and statistically insignificant. Importantly, this is not simply because never-married mothers never have earnings in the phase-out region – Appendix Figure A.6 shows that they do, and that there is substantial overlap with married mothers. Rather, it seems more likely that the increase in mass in the phase-in and flat regions reflects the larger change in incentives.

Consistent with prior work, only a small share of early-exposed mothers target earnings precisely at the refund-maximizing (first) EITC kink.[24] We find little pre- or post-birth bunching when we examine all early-exposed mothers, although we do find evidence of post-birth bunching among the self-employed, as in Chetty et al. (2013) and Saez (2010) (see Appendix Figure A.8 and Appendix Table A.5). Hence, while early-exposed mothers appear to be aware of the incentive for bunching at the EITC kink, this does not appear to be a primary driver of earnings responses. Further, we do not detect any pre- or post-birth bunching among late-exposed mothers, consistent with previous evidence that bunching increased after the 1993 reform (Saez, 2010).

On average, early-exposed mothers earn \$657 (se: 327) more based on the DDD, which represents a 7% increase in earnings relative to the mean for late-exposed women (\$9,926).[25] If the short-run earnings increase was due to increases in working alone, and early-exposed mothers that enter the labor market earn as much as the average late-exposed working mother (\$15,737), then we would expect average earnings to rise by \$487, rather than by \$657. This leaves room for some small intensive margin effects.

4.3 Are Mothers Responding to EITC Work Incentives?

Our hypothesis that the rise in employment is primarily a response to EITC incentives generates several testable predictions. First, there should be an evident break in post-birth employment for mothers that have a first birth after the expansion. Second, because the rise in the EITC maximum was larger for mothers with two or more children, we expect a proportionally larger response among mothers after a second or higher-order birth (2+) relative to a first birth; but not for mothers after a third-or-higher births (3+) relative to a second birth (Hotz and Scholz, 2006). Third, we expect our results to extend beyond states with high employment growth, and to begin prior to the implementation of federal welfare reform in 1997. We can also test the stability of our coefficients to introducing more detailed controls for welfare waivers and unemployment rates to bound the importance of these explanations.

We implement the first test by re-estimating our DD and DDD models replacing "$PostBirth \cdot EarlyExposed$" with separate interactions between "$PostBirth$" and a set of indicators for having a first birth between '90-91, '92-93, '94-95, or '96-97. If our effects were driven by an ongoing trend, we would expect all four coefficients to be positive and to increase across cohorts. Contrary to this, Appendix Figure A.9 shows little change in employment upon motherhood for pre-reform cohorts:

[24] We find no evidence of bunching at the second EITC kink, as in prior work (e.g., Saez, 2010).

[25] The effects are smaller in the 3 years after a first birth, and larger in the DD — see Appendix Table A.6.

mothers that have a first birth in '92-'93 work as much after childbirth (relative to pre-childbirth) as those with a first birth in '88-89. Subsequent cohorts have a sharp change in post-birth behavior. For births beginning in 1994, post-birth employment increases by 5 to 7 p.p.[26]

To implement the second test, we use the never-married "all births" sample. We then adapt our triple-difference model to ask: is the change in employment after birth between 2+ "early-exposed" and "late-exposed" mothers larger than the change between early- and late-exposed mothers after a first birth?[27] Column 1 of Table 2 shows that employment increases by 3.2 p.p. more after a 2+ birth relative to a first birth. Column 3 shows that the rise in working is slightly higher for 3+ births relative to second births, but the difference is not statistically significant. This pattern aligns with EITC incentives, and is inconsistent with an alternative explanation that predicts strictly increasing effects by birth parity, such as from higher rates of welfare participation or lower base rates of employment (Kleven, 2019).[28]

Third, we consider the potential role of confounding variation due to the booming economy and welfare reform, a point recently emphasized by Kleven (2019). For an in-depth discussion of the argument in Kleven (2019), and how it relates to our paper, see Appendix B. For all following analyses, we include the DD results for never married mothers in Table 2, and present the (very similar) DDD results in Appendix Table A.7.

To test for confounders related to the economy, we examine whether early-exposed mothers' employment increased more in states that experienced larger declines in unemployment rates during the 1990s. We do not find that this is the case: columns 3 and 4 of Table 2 show that the employment effects are very similar for states with above-median and below-median changes in the unemployment rate between 1994–2000 and 1988-1993. This is despite the fact that the average change in unemployment was three times as large in the above-median states (-1.8 p.p. versus -0.6 p.p.). Hence, our employment effects hold to a similar degree even in states that experienced relatively weak economic growth.[29]

Further, in Columns 5 and 6 of Table 2, we allow the coefficients on our baseline unemployment and welfare controls to vary by the age of one's first child, to address potentially larger responses to the economy and welfare reform for mothers with young children, which was raised in Kleven (2019). The additional unemployment controls have virtually no effect. The additional welfare controls reduce the coefficients by up to 18 percent, but our conclusions are substantively unchanged.

In the last two columns of Table 2, we limit our analysis to the years up to 1996 to limit the potential influence of federal welfare reform. We present event study coefficients for these results to address the fact that this restricted window creates imbalance in event time, and show the results for all states (column 7) and for states that did not pass any welfare waivers prior to 1997 (column

[26] For dynamic effects on employment by year of first birth, see Appendix Figure A.10.

[27] Specifically, we redefine ρ_m and NM_m in Equation 2 to be indicators for being a 2+ mother.

[28] Nevertheless, we do not focus on the comparisons across parity of child as a primary specification since mothers are likely to have already experienced child-related gaps in work experience prior to a 2^{nd} (or higher-order) birth. Thus, immediate return to work may have a different (and possibly weaker) effect for mothers after a 2^{nd} birth than for mothers after a 1^{st} birth.

[29] Earnings effects are also similar across these states – see Appendix Table A.8.

8).[30] The coefficients are similar to our main event study, and statistically significant in years 2 and 3 (see Appendix Figure A.11 for the complete graphs, which resemble Figure 2). Further, we do not find meaningful differences across waiver and non-waiver states. This suggests that while welfare reform may have reinforced the return to work after birth, it can not explain the majority of our findings, consistent with Meyer and Rosenbaum (2001) and Grogger (2003).

Finally, we explore the role of two additional potential mechanisms, child care costs and state EITC benefits, to gain insight into our results. In the interest of space, we summarize the results here, and include a more complete discussion in Appendix D. First, we do not find a clear relationship between the impact of work incentives and (state-level) child care costs (see Appendix Table A.9). This does not imply that child care costs are not relevant for mothers' employment decisions; rather, we suspect that our observed child care costs proxy for multiple factors that we cannot fully disentangle. Second, we find that early-exposed mothers' employment increases more in states that have a supplemental EITC and higher state supplements (see Appendix Table A.10). Thus, early-exposed mothers' employment grew more in states that had a larger EITC incentive to work.

5 Long-Run Effects

We now use the full scope of our data to examine how incentives to work after a first birth affect outcomes over the medium- and long-run. We thus modify our DD and DDD estimating equations to allow interactions between "$EarlyExposed$" or "$EarlyExposed \cdot NM$" and indicators for each of the three periods of interest; the short-run, years 0 to 4, the medium-run, years 5 to 9, and the long-run, years 10 to 19.

We begin by looking at the persistence of the short-run impacts on the likelihood of working. Because the gap in incentives between early- and late-exposed mothers closes over time (Figure 1), we expect that the difference in employment should also attenuate. However, it is not clear that employment outcomes should fully converge, nor do so within six years, as incentives do. Early-exposed women could have higher employment over the long-run, for example, if they are more elastic to incentives, or if job finding rates are higher for women with recent employment (Kroft et al., 2013). Late-exposed women may also catch up more slowly if there is a lag in the spread of information about the EITC credit, or if there are other frictions that would similarly delay responses, such as an insufficient supply of affordable child care.

Panel A of Figure 4 shows that the impacts on employment begin to fade four to five years after birth, and completely disappear by year 9 or 11, depending on the specification. After that, we find a mixed pattern of small positive (DDD) or negative (DD) effects, which we suspect may be due to imperfect controls for the effects of the Great Recession.[31] Controlling for state-level

[30] The no-waiver states include Alaska, Colorado, Hawaii, Idaho, Louisiana, Minnesota, North Dakota, New Mexico, Nevada, New York, Pennsylvania, Rhode Island, and Wyoming, as well as Washington DC.

[31] Early-exposed mothers experienced the Great Recession between years 12–19 after a first birth (see Appendix Figure A.12).

unemployment rates among low-skilled individuals or women rather than among all individuals reduces the modest long-run fluctuations (see Section 5.2). Moreover, the long-run fluctuations in employment seem to reflect entry decisions about relatively small earnings amounts, as indicated by the results on earnings below. These issues notwithstanding, the point estimates beyond year 11 are overall small and imply no lasting impact on employment. The estimates in Columns 1 and 2 of Table 3 show that early-exposed women have a 4.3 to 5.5 p.p. higher employment rate per year in the medium-run. This difference fades to an insignificant -1.7 p.p. to 1 p.p. in the long-run.

Although early-exposed mothers do not have a permanently higher rate of employment, the additional time they accumulate in the labor market may improve long-run earnings through increases in labor market experience. We calculate these impacts on experience by taking a cumulative sum of annual impacts on employment (shown in Figure 4), and then taking averages over the medium- and long-run.[32] Columns 3 and 4 of Table 3 show that early-exposed mothers have 0.46 to 0.47 years of additional experience in the medium-run, which becomes 0.45 to 0.62 additional years in the long-run. The long-run DDD estimates correspond to a 5.7% increase in years of experience. To complement these results, we measure impacts on *hours* of experience in Section 5.1.

Panel B of Figure 4 presents the dynamic impacts of early exposure on earnings. Early-exposed mothers experience increasing earnings gains over the first six years after birth, following the impacts on employment. However, unlike employment, impacts on earnings only decline slightly over the next few years, do not exhibit non-monotonicities over time, and remain positive and often statistically significant over the long run.[33] The persistent impact on earnings is particularly apparent in the DDD, where the point estimates are nearly constant between years 10 to 19. The DD impacts on earnings decline moderately between years 15 to 19, but this appears to reflect a timing effect (which is controlled for in the DDD), rather than true convergence in earnings (as we show in Section 5.2). Early-exposed mothers thus appear to have long-lasting earnings gains, which are not readily explained by differences in the rate of employment.

Table 3 shows that early-exposed mothers earn \$2,618 to \$3,655 more per year in the medium-run and \$1,206 to \$1,396 more per year in the long run. The majority of this (87%) is due to increases in pay from employers (see Appendix Table A.11). Relative to the average annual earnings of late-exposed mothers, the DDD estimates imply that early-exposed mothers experience a 17% earnings gain in the medium-run and a 6% earnings gain in the long run. Going to work earlier after a first birth thus appears to have a meaningful and persistent effect on earnings.[34]

Further, consistent with the lack of long-run impacts on employment, we find similar long-run earnings effects when we restrict the sample to those with positive earnings or analyze log earnings in Appendix Table A.12 (columns 1-4). The DDD estimate on earnings conditional on working represents a 4.2% increase relative to the average earnings of late-exposed mothers with positive

[32] Using observed years of experience as an outcome instead produces slightly larger results. This is because differencing out gaps in pre-birth *experience* does not fully account for gaps in the pre-birth *employment rate*.

[33] The absence of non-monotonicities in the long-run earnings effects is consistent with the long-run employment fluctuations being concentrated on extensive margin decisions about small earnings amounts.

[34] For distributional effects on earnings, see Appendix Figure A.13.

earnings. Winsorizing at the top one percent of earnings to avoid the influence of outliers (columns 5-6) also makes little difference.

Cumulatively, we estimate that early-exposed mothers earn between \$36,702 and \$37,945 more over twenty years after a first birth, 29 to 41% of which is earned over the long run. Discounting at a 5% rate produces a present value of earnings gains between \$23,307 and \$24,056. If the average impacts on earnings levels in years 18 and 19 were to be sustained until the average age of early-exposed mothers is 60 (for an additional 17 years), the present value of earnings gains would be between \$25,872 and \$30,335. In Section 7 we discuss how this translates into impacts on total income, taking into account changes in transfers from the EITC and other government programs.

5.1 Survey Evidence on Hours of Work and Fertility

For evidence on hours of work and fertility, we turn to our sample's survey responses in the CPS.[35] Recall that, unlike the administrative data, the CPS only contains outcomes for each mother for a single year and almost always after a first birth. As a result, we can only implement a single-difference design, comparing early- and late-exposed never-married mothers' outcomes at each child age; or a double-difference design, adding a comparison across marital status.[36] To get closer to our main analysis, we also add controls for average employment and earnings in the five years prior to childbirth from the SSA records. Nevertheless, because we can not fully control for pre-birth outcomes, these results are more susceptible to bias, and thus more suggestive than our main results.

Table 4 presents the impacts of early exposure on various measures of weekly hours of work (including 0's). Columns 1-3 show effects on indicators for any, part-time ($0 < \text{hours} < 35$) and full-time (≥ 35 hours) employment, while column 4 shows effects on average hours of work. Relative to our main results, column 1 shows qualitatively similar, but larger, effects on early-exposed mothers' employment in the short- and medium-run, and the same (null) effects in the long-run.

For hours of work, we find that in the short run, early-exposed mothers' additional employment is concentrated in part-time work, and amounts to an increase of 2 to 3 hours more work per week ($p \geq 0.10$). However, in the medium-run, higher employment is completely concentrated in full-time work (column 3), and amounts to a 3 to 4 hour increase in hours work per week ($p < 0.01$). Hence, early-exposed mothers appear to switch to full-time work when their children enter into primary school (Duchini and Van Effentere, 2018).[37] Further, this increase in hours of work likely

[35] The results are the same if we include all (matched and unmatched) CPS mothers that meet our sample criteria.

[36] We estimate the single-difference as:

$$\begin{aligned} Y_{ib\tau} =& \alpha + \beta_1 \cdot EarlyExposed_b \cdot 0\text{–}4_\tau + \beta_2 \cdot EarlyExposed_b \cdot 5\text{–}9_\tau + \beta_3 \cdot EarlyExposed_b \cdot 10pl_\tau \\ &+ \theta_\tau + \gamma X_{is\tau} + \delta P_{s\tau} + \epsilon_{ib\tau} \end{aligned}$$

where $0\text{–}4_\tau$, $5\text{–}9_\tau$, and $10pl_\tau$ are indicators for years 0-4, 5-9, and 10+ after a first birth. The double-difference uses:

$$\begin{aligned} Y_{ib\tau} =& \alpha + \beta_1 \cdot EarlyExposed_b \cdot 0\text{–}4_\tau \cdot NM_m + \beta_2 \cdot EarlyExposed_b \cdot 5\text{–}9_\tau \cdot NM_m + \beta_3 \cdot EarlyExposed_b \cdot 10\text{pl}_\tau \cdot NM_m \\ &+ \theta_\tau \cdot \lambda_b + \theta_\tau \cdot \rho_m + \gamma_m X_{is\tau} + \delta_m P_{s\tau} + \epsilon_{ib\tau} \end{aligned}$$

[37] This does not appear to be a purely "mechanical" effect of children aging. 67% of late-exposed employed mothers

contributes to the growth in the earnings effects between the short- and medium-run documented above.

In the long run, there is no difference in the hours of work between early- and late-exposed mothers. We also do not find any long-run (nor any) effect on weeks of work (see Appendix Table A.13). Thus, the long-run increase in earnings that we document appear to reflect higher *wages*. In particular, our DDD estimates of earnings gains among workers above suggest that early-exposed mothers earn 4.2% higher wages.

Appendix Table A.13 shows effects on total hours of experience. We obtain this by summing annual impacts on total hours of work, which we calculate as hours per week times weeks worked last year. Using the married comparison group, we find that early-exposed mothers accrue an additional 945 hours of work in the long run ($p < 0.05$), which represents 0.675 of a year of full-time work experience (defined as 35 hours per week, 40 weeks per year). Moreover, two-thirds of this experience is accumulated in the medium-run, which underscores the relative importance of full-time work towards amassing experience.

Finally, we consider whether early-exposed mothers make different fertility choices, in terms of number of children or birth spacing. For this analysis, we limit our sample to women between the ages of 36 and 44, who are more likely to have completed their childbearing (although our results are not sensitive to this restriction). We present our results in Appendix Table A.14. We find no significant effect on any outcome, and the magnitudes allow us to rule out effects larger than a 0.15 increase in early-exposed mothers' number of children (a 7% effect).

5.2 Robustness

To verify the interpretation of our results, we perform additional analyses to rule out potential confounds and check the sensitivity of our estimates.

Calendar-year event studies First, a potential concern with our DD estimates is that they may reflect differences in year effects across early- and late-exposed mothers. Therefore, we study if our conclusions about employment and earnings hold when we compare early- and late-exposed mothers in the *same calendar year*. In particular, we plot calendar year event studies (i.e., coefficients on year dummies) for early- and late-exposed mothers and, for context, also show year effects for mothers that have a first birth in the years around our core sample (i.e., 1986-87 and 1997-99). Similar to the main analysis, we omit the year prior to the earliest childbirth in each group.[38]

Consistent with our DD results, Appendix Figure A.14 shows that early- and late-exposed mothers converge to a similar rate of employment in the long-run (roughly equal to pre-birth

work full-time (in the CPS), while our estimates suggest that 100% of marginal early-exposed mothers work full-time.

[38] Specifically, denoting calendar year as t, we estimate:

$$Y_{ibt} = \sum_{j \neq 1985} \alpha_j \cdot \mathbb{1}(t=j) \times \mathbb{1}(b < 1987) + \sum_{j \neq 1987} \lambda_j \cdot \mathbb{1}(t=j) \times LateExposed_b + \sum_{j \neq 1992} \beta_j \cdot \mathbb{1}(t=j) \times EarlyExposed_b + \sum_{j \neq 1996} \phi_j \cdot \mathbb{1}(t=j) \times \mathbb{1}(b > 1996) + \chi_b + \gamma X_{ist} + \delta P_{st} + \epsilon_{ibt}$$

employment), but that early-exposed mothers earn on average $1,500 to $2,000 more per year than late-exposed mothers.[39] Further, we find that the coefficients for early- and late-exposed mothers are very similar to those for mothers that give birth in the following or preceding years, respectively. This makes clear that our conclusions are not sensitive to making within-calendar-year comparisons or to adding more cohorts of mothers.

Alternative comparison groups Second, we test the sensitivity of our DDD results to using single women that do not have children as a comparison group instead of married mothers. In particular, we use as comparison either (i) women who will *soon* be mothers ("future mothers") or (ii) women who will *never* be mothers ("childless"). This addresses the potential concern that the earnings of all single women may have improved during 1990s (e.g., from the economy), and moreso than the earnings of married women.

Our "future mothers" comparison group consists of never-married women who have first births four years after mothers in our sample (i.e., in 1992–1995, 1997–2000).[40] This ensures that these women are close in age to our core sample. We assign each future mother a fake year of childbirth, $\hat{b}$, equal to her true b minus 4, and set her fake "years since first birth" as the year minus $\hat{b}$. Hence, "pre-birth" and "post-birth" consist of the same sets of calendar years for all mothers that have the same "year of childbirth." We use this comparison only to estimate effects on outcomes 3 years after a first birth (prior to a future mother's first birth). If there is a confound that differentially affects single women, then future-mothers with post-1993 "births" should experience gains in employment relative to future mothers with pre-1993 "births", and drive our DDD to zero. Reassuringly, the DDD in Appendix Table A.15 produces similar-sized increases in employment as our baseline results.

Our "childless" comparison group consists of women that we observe between the ages of 40 to 45 without any children in the household. To assign $\hat{b}$, we follow Kleven, Landais and Søgaard (2019), and take a random draw from the distribution of b among never-married mothers who have the same year of birth and level of education as a given childless woman. Then, as above, the fake "years since first birth" is year minus $\hat{b}$. Again, if there is a confound, then childless women with post-1993 "births" should have better outcomes relative to childless women with pre-1993 "births," and lead our DDD to produce no effect. Contrary to this, we find that — in line with our main results — early-exposed mothers earn $1,000 to $1,900 more in the long-run when we use either all childless women or never-married childless women as comparison groups (see Appendix Table A.16).[41] This underscores the fact that our effects on employment and earnings are specific to *never-married mothers*, and can not be explained by broader changes among women.

Alternative control variables Third, we test whether our results change when we allow for greater flexibility in our controls for economic conditions or individual characteristics. Allowing

[39]Mothers with earlier exposure have weakly higher earnings – see Appendix Figure A.15.

[40]We construct this sample independently, such that a woman can be both an "actual" and "future" mother.

[41]Our short-run results are also the same when we use this group — we omit those results for brevity.

the impact of unemployment rates and welfare reform to vary with the age of one's child (as in our short-run results) does not change any of our long-run effects (see Appendix Table A.17). We also control for state-level unemployment rates specific to women or low-skilled individuals (calculated from the March CPS) — which might better proxy for single mothers' employment opportunities — or including state-year fixed effects. Appendix Figure A.16 show that our results are very similar across these specifications, though using group-specific unemployment rates yields smoother long-run impacts on employment.

Our results are similarly unchanged when we introduce richer controls for individual characteristics. Appendix Table A.18 shows that our short- and long-run results hold when we use inverse p-score reweighting to impose balance in pre-birth employment and demographics. Similarly, Appendix Table A.19 shows that our results are not sensitive to allowing the effect of mother's age to vary with the age she first gave birth; including individual fixed effects; or restricting the sample to mothers who are CPS heads of household or were income-eligible for the EITC pre-birth, both of whom may be more strongly affected by EITC incentives.

Alternative sample restrictions Fourth, we re-run our results using alternative sample restrictions to address potential concerns about bias from measurement error (which we previewed in Section 3.1). To test for positive selection among "surviving" never-married mothers across survey years, we look for an upward trend in our estimates when we successively only keep individuals interviewed in the CPS between 0–8, 0–9, ..., and 0–20 years from first birth. We find no such trend: our earnings results are nearly identical when we only keep mothers interviewed within 8 or within 20 years of birth, and are generally similar across years (although the confidence intervals are wider when we use a smaller sample). See Appendix Figure A.17. We also do not find smaller impacts on earnings when we successively only keep mothers interviewed *further* from first birth (see Appendix Figure A.18).[42] Moreover, we also find similar effect when we successively drop mothers who were relatively older (39–49) at CPS interview, some of whose children may have already left home (see Appendix Figure A.19). This assures us that our qualitative results are robust to a variety of assumptions about how measurement error could affect our sample.

Randomization inference Last, we use randomization inference as an alternative method of obtaining confidence intervals for our estimates. In particular, we randomly assign a placebo "early-exposure" to four randomly chosen years of first birth drawn without replacement, and estimate a placebo effect using this definition. We do this 500 times for each of our main outcomes, and plot the resulting distribution of estimates in Appendix Figure A.20. The one-sided p-values for short-term employment and long-term earnings are between 0.01 and 0.02.

[42] Earnings effects are also similar for women surveyed 0–4, 5–9, 10–14, 15–20, or 20+ years from first birth.

6 Why do Early-Exposed Mothers Earn More?

Our results show that early-exposure to work incentives causes mothers to earn more at every stage of their careers. This primarily reflects increased employment in the short run, and higher wages in the long run. In this section, we explore potential explanations for higher long-run wages.[43]

A leading explanation for early-exposed mothers' higher wages is increases in experience. Our earlier results provide some indirect evidence for this mechanism. Correlationally, earnings and experience increased together. Also, consistent with concave returns to experience, early-exposed mothers' earnings gains make up a decreasing share of earnings over time (i.e., from 10.8% to 5.1% between years 10 and 19 after first birth). As a more direct test of this mechanism, we ask whether the mothers that experience higher earnings are the *same* mothers that were induced to work after a first birth. To avoid conditioning on post-birth experience (which is an outcome of early exposure), we run regressions where the outcomes are indicators for the four possible combinations of having "high" or "low" earnings crossed with having "high" or "low" experience. We define "high experience" as having worked during each of the first three years after a first birth ($1 \leq \tau \leq 3$) to capture short-run responses to post-birth work incentives. We define "high earnings" as having earnings in the top 25 percent of mothers in each year, which we find is the best (binary) proxy for the impact of early exposure on earnings.[44] If greater experience is driving our effects on earnings, then we would expect to find an increase in the share of mothers with "high earnings and high experience," but a decrease or no change in the share of mothers that have "high earnings with low experience." We also do not expect any effect on the share of "low experience" mothers with high earnings (i.e., in the return to low experience).[45]

Appendix Table A.21 presents long-run effects on indicators for these four outcomes: having "high earnings and high experience" (column 1), having "high earnings and low experience" (column 2), having "low earnings and high experience" (column 3), and having "low experience and low earnings" (column 4). In line with our hypotheses, we find that early-exposed mothers are significantly more likely to have "high earnings and high experience," and are less likely to have "high earnings and low experience." We find no change in the share of "low-experience" mothers that have high earnings (see Appendix E for details). As a share of the additional early-exposed mothers that have high experience (the sum of columns 1 and 3), 21% end up being "high earning," which is similar to the share among all never-married mothers (19%). Hence, women induced to work immediately after a first birth have similar returns to experience as the average never-married mother. These results support experience as a main mechanism for our earnings gains.

If experience were the only source of wage gains, the implied return to a full-time full-year of

[43] In the medium-run, earnings gains do not appear to be explained by employment alone, since we find larger earnings gains but the same employment effects as in the short-run. We examine the role of growth in hours versus growth in wages for explaining the difference between short- and medium-run gains in Appendix E.

[44] We provide further details and results using an alternative measure of "high experience," see Appendix E.

[45] A related possible explanation is that our earnings effects reflect the impact of gaining experience during a good economy. This seems unlikely, since we find similar effects across states with weak and strong economic growth (Appendix Table A.8), and our long-run effects do not vary by post-birth unemployment rates.

work would be 6.2 percent (4.2%/0.675), using the estimates of impacts on earnings conditional on working and of (hours-inclusive) experience gains from the CPS. This falls in the range of prior estimates for similar populations, including the 5% return per year for women with at most a high school education in Gladden and Taber (2000), or the 9-12% return per year of uninterrupted experience among German mothers in Adda et al. (2017). However, our precision contrasts with the imprecise and negative returns in Card and Hyslop (2005) which have clean identification from a randomized experiment. Relative to Card and Hyslop (2005), our shock to experience is twice as large, which increases our precision. The return to experience may also be higher in our setting because working after childbirth provides a costly signal to employers of one's commitment to work (Thomas, 2019; To, 2018).

Second, it is possible that early-exposed mothers obtain higher returns to experience by choosing different occupations. For instance, Adda et al. (2017) find that the returns to experience are higher in "abstract" occupations that have more analytic or interactive tasks. We find some imprecise support for this channel when we look at CPS occupations (see Appendix Tables A.23 for impacts on service occupations and A.24 for impacts on non-service occupations). In the long run, early-exposed mothers are 4 p.p. more likely to be in health occupations ($p < .05$) and 5 p.p. less likely to be in clerical occupations ($p < 0.1$) in the CPS. However, we find inconsistently-signed and noisily-estimated changes across the thirteen other job categories. Given this, it is unclear whether the increase in health occupations is a true effect of early exposure or noise in the data. However, even taking the increase in health occupations at face value, the effect is too small to explain much of the total increase in earnings.[46]

Third, early-exposed mothers may avoid skill depreciation by reducing the number or length of time out of work. We do not have any direct evidence on this; however, Adda et al. (2017) find that annual skill depreciation is low ($< 1\%$ per year) during mothers' early careers. Hence, mothers in our sample would be expected to experience little depreciation.

Finally, having additional resources after childbirth may have lasting impacts through purchases of productivity-enhancing durables, such as a car, or through improvements in well-being. For instance, expansions of the EITC have been shown to increase maternal and child health (Evans and Garthwaite, 2014; Hoynes et al., 2015). If such improvements were major factors in our results, we might also expect to find increases in employment alongside with wages (e.g., Frijters et al., 2014). The fact that we do not find any such effects suggests that these improvements are likely to have muted effects on wages.

Overall, we find the strongest empirical support for the role of higher experience as a primary channel for early-exposed mothers' higher earnings. However, changes in occupation, reductions in skill depreciation, and higher income immediately after a first birth may also contribute to long-run earnings gains.

[46] In order to explain the entire increase in long-run earnings, the average earnings in health services would have to be \$34,825 ($\frac{1393}{.04}$) *higher* than in early-exposed mothers' other occupations.

7 Long-Run MVPF

Thus far, we have focused on the EITC expansion as an experiment for estimating the returns to going to work sooner after childbirth. We now consider whether the government funding used for the expansion was *efficient* by quantifying the fiscal impact — through direct costs and associated fiscal externalities — relative to the benefit to mothers. To do so, we measure impacts of early exposure on EITC benefits, income tax revenues, and program participation. We use these inputs to calculate the implied MVPF of the expansion — the ratio of the value of the EITC transfer to mothers to the net cost inclusive of fiscal externalities (Hendren and Sprung-Keyser, 2019). A key caveat is that we calculate the MVPF under the assumption that these responses are solely due to changes in the generosity of the EITC after a first birth.

EITC For the MVPF calculation, we need to estimate impacts on total EITC benefits, as well as to decompose this total effect into changes in benefits stemming from labor supply responses ("behavioral") and changes in EITC generosity ("mechanical.") In the MVPF framework, the "mechanical" growth is a pure transfer to recipients and thus gives the lower bound of the value of the benefits to mothers (Hendren and Sprung-Keyser, 2019). We focus on the EITC benefits that a household is *eligible* for, but discuss incomplete take-up below.

We create three measures of EITC benefits. First, we simulate total EITC benefits for each mother and child age using household earnings and the 1-child EITC schedule for 1989 first births (if late-exposed) or for 1994 first births (if early-exposed).[47] (This simplification makes the decomposition more straightforward, but does not affect the results.) This is the EITC amount that a household is *expected* to receive in each year. Second, to estimate the "behavioral" response, we simulate a *hypothetical* EITC benefit at each child age based on household earnings and the EITC schedule for 1994 first births. This is the EITC amount that a household would receive in each year if its first birth had been in 1994, holding earnings constant. Third, we take the difference between total benefits and this hypothetical "behavioral" benefit. This is the *additional* amount of benefits that a household would receive in each year if its first birth had been in 1994 (i.e the "mechanical" change in benefits from the expansion).[48]

Table 5 presents the estimated effects for these three simulated EITC benefits. Columns (1), (2) and (3) present the effect on total EITC benefits; benefits through the "behavioral" channel; and benefits through the "mechanical" channel, respectively. The DD and DDD estimates are presented in Panels (a) and (b), but for brevity we will discuss our results as a range between these.

In the short-run, early-exposed mothers' EITC benefits increase by \$400 to \$619. This represents a substantial 40% to 60% increase relative to average post-birth benefits. Over half of this increase (54–70%) is accounted for by greater generosity (column 3), which implies that a large share of the increase in EITC spending was a transfer to already-working mothers. In the medium-run,

[47] In particular, the EITC benefit for an early-(late-) exposed mother with a child of age τ is calculated using the one-child EITC schedule from tax year $t = 1994$ (1989) + τ applied to household earnings in τ. We assign zero EITC in the years pre-birth.

[48] For more formal intuition for this decomposition, see Appendix F.1.

early-exposed mothers' EITC benefits increase by \$53 to \$93 (4–7%). There is no meaningful "mechanical" difference in benefits and, consistent with the substantial earnings growth during this period, the "behavioral" response is roughly half the size of the short-run estimate.

In the long run, early-exposed mothers' EITC benefits decrease by \$89 to \$99, an effect driven by the behavioral response. This reduces the fiscal impact of the expansion, but does not fully compensate for the initial increase in benefits. Over twenty years, early-exposed mothers are eligible for \$2,626 to \$3,229 more in EITC benefits, which has a present value between \$2,328 and \$3,027 using a 5% discount rate.

Income taxes and non-EITC transfers In the absence of administrative data on taxes, we generate a back-of-the-envelope estimate of federal income taxes owed by estimating early-exposed mothers' average tax rate from our distributional earnings results and the NBER TAXSIM federal tax rates (Feenberg and Coutts, 1993). This produces an average tax rate of 0% in the short run, 5% for the medium run, and 13% over the long run.[49] Multiplying the annual earnings increases by these rates, early-exposed mothers would be expected to pay the equivalent of \$1,441–\$1,559 more in federal income taxes in present value terms. Hence, over the long run, early-exposed mothers could be expected to pay back in taxes up to two-thirds of what they receive in EITC benefits.

To estimate effects on program participation, we rely on self-reported measures from the CPS and the estimation strategies in Section 5.1. We focus on impacts on the value of benefits received from the largest transfer programs, including welfare benefits, disability benefits, food stamps/SNAP, the value of Medicaid, and housing subsidies. See Appendix F.3 for a detailed discussion of the definitions and availability of these variables, and the potential for misreporting to affect these results (see, e.g., Meyer et al., 2015).

Appendix Table A.25 shows that early-exposed mothers receive fewer benefits from nearly every category, including welfare, food stamps/SNAP, Medicaid, and housing subsidies. If we use the more conservative (smaller) estimates, we find that the value of transfers paid to early-exposed mothers (and their families) is \$936 lower per year in the short run, \$1,599 lower per year in the medium run and \$61 lower per year in the long run. Summing up these annual impacts over twenty years, this amounts to a \$9,155 decline in total government benefits, with a present value of \$6,657. If we further exclude welfare benefits, which may be sensitive to welfare reform, and also the value of Medicaid, which may be mismeasured, net transfers decline by \$4,880 (present value = \$2,791) and \$1,799 (present value = \$831), respectively. This fits with prior evidence that expansions of the EITC lead to important reductions in program participation (Hoynes and Patel, 2018; Bastian and Jones, 2020).

MVPF In sum, we find that over twenty years early-exposed mothers are eligible to receive in present value terms at least \$2,328 in EITC benefits, \$1,000 of which is a pure transfer to recipients, pay at least \$1,441 more in taxes, and receive at least \$831 less in government transfers (excluding

[49] See Section F.2 for greater discussion of the calculation of these tax rates.

welfare and the value of Medicaid).

If we focus only on impacts on earnings and taxes, we can compute a lower bound of the MVPF for our population as:

$$MVPF = \frac{\text{WTP}}{\text{Cost + Fiscal Externality}} \leq \underbrace{\frac{\text{WTP}}{\text{Cost + Add'l Taxes}}}_{\text{Our baseline estimate}} \tag{3}$$

Depending whether we use the DDD estimates or the DD estimates, we obtain an MVPF between 1.26 ($\frac{2{,}000}{3{,}027-1{,}441}$) and 1.30 ($\frac{1000}{2{,}328-1{,}559}$). The MVPF increases to 1.5 if we account for incomplete take-up of the EITC, or to at least 5.6 if we incorporate the (at least) $831 decline in transfers. We show a range of MVPFs across specifications and tax rate assumptions in Appendix Figure A.21. In words, this means that the value of the expansion for never-married mothers is between 1.3 and 5.6 times as large as the cost to the government. Importantly, we find that including long-run impacts makes a meaningful difference in our MVPF. If we focus only on medium-run effects, we obtain an MVPF that is between 0.4 to 0.6 of our baseline long-run MVPF. The ratio between the long-run to the medium-run MVPF is similarly large when we allow for incomplete takeup and include transfers (see Appendix Figure A.22). This implies that previous MVPF estimates that focused on short- and medium-run effects of the EITC on earnings (Hendren and Sprung-Keyser, 2019; Bastian and Jones, 2020), are likely to be lower bounds on the long-run MVPF.

We note that our focus on new mothers and never-married mothers implies that our MVPF is not the same as the overall MVPF of the 1993 EITC expansion (i.e., for all eligible families). Inclusive of transfers, our MVPF estimate of 5.6 is larger than prior EITC MVPFs, which range from 1.08 to 1.12 (Hendren and Sprung-Keyser, 2019) for the 1993 expansion, or from 3.18 to 4.23 (Bastian and Jones, 2020) for all post-1990 EITC expansions.[50] Our higher estimate likely reflects a couple of key factors. First, as mentioned above, incorporating long-run earnings increases the MVPF. Second, we show that new mothers experience larger changes in work experience and thus greater gains from work incentives. Third, our estimates exclude married mothers, who generally reduce the MVPF of the EITC. In that sense, our estimates are a more relevant benchmark for the benefits of a work incentive for new mothers or single mothers than for evaluating the EITC.

Because we do not observe all possible externalities, our MVPF reflects an incomplete accounting of the net cost of the expansion. We have argued that our MVPF is likely to be a lower bound because we are omitting impacts on many non-EITC transfers, particularly cash welfare. However, our calculation also omits intergenerational impacts, which could in theory be either positive or negative. Suggestively, Bastian and Michelmore (2018) and Dahl and Lochner (2012) find that EITC expansions during childhood tend to raise test scores, educational attainment and earnings. These average impacts may not translate completely to our population of mothers exposed at first

[50] In other respects, our estimates align closely with this prior work. Our estimated "mechanical" share of the EITC increase is identical to Bastian and Jones (2020) (who estimate this to be between 54–72%), and is slightly lower than Hendren and Sprung-Keyser (2019) (who estimate this to be 89.5% using estimates from Hoynes and Patel, 2018).

birth; however, at face value they imply that our MVPF is an underestimate.

From another perspective, these calculations also imply that *mothers* have more income after the expansion. Summing impacts on earnings ($23,307), EITC benefits ($2,328), taxes (-$1,441), and the upper bound of the absolute decline in transfers (-$6,657), we find that maternal income increases by $17,537 in present value terms. Even so, it is difficult to conclude whether early-exposed mothers are better off. Such an argument would require information on child care costs, the value of lost leisure, and impacts on children, which are outside the scope of this study.

8 Conclusion

This paper provides new evidence on the impact of increasing post-birth work incentives on mothers' long-run career trajectories. For identification, we use a large-scale panel of administrative earnings linked to the CPS, and variation in the timing of the 1993 EITC expansion around a first childbirth. We find that never-married mothers exposed to more generous EITC incentives at first birth, rather than 3 to 6 years after birth, have 3.4 to 3.7 p.p. (5.9 percent) higher rates of employment in the first five years after a first birth. Ten to nineteen years after a first birth, early-exposed mothers have the same employment rate as late-exposed mothers, but have accumulated 0.5 to 0.6 years of additional work experience. They also earn $1,206–$1,392 more on average, which translates to a 6 percent increase in unconditional earnings. We find no effect on hours of work in the long run which suggests that early-exposed mothers earn higher *wages.* These results suggest that there are steep returns to work incentives at childbirth that accumulate over the life-cycle.

One important caveat to these results is that increases in earnings do not necessarily equate to early-exposed mothers being "better off." A complete accounting would require, for instance, information on costs associated with work (commuting, child care), the value of lost leisure, and spillover effects to children. Nevertheless, quantifying the scope of earnings gains from early return to work is a crucial input to this calculation. It is also critical for understanding the drivers of the child penalty. Finally, these estimates should inform the benefits of policies to encourage maternal work (e.g., job protection, tax incentives, etc.) We leave it to future work to quantify impacts on other dimensions of maternal and child welfare.

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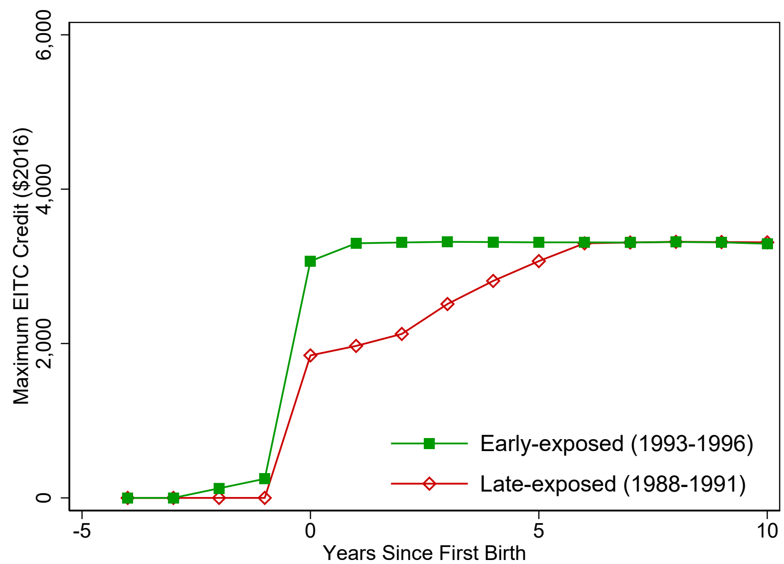
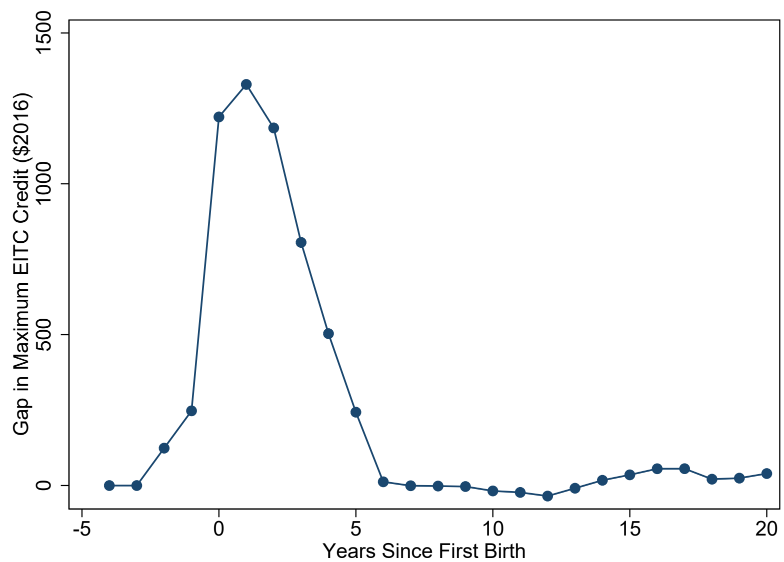
Figure 1: Maximum EITC Credit by Age of First Child and Year of First Birth

(a) Levels, Up to Age 10



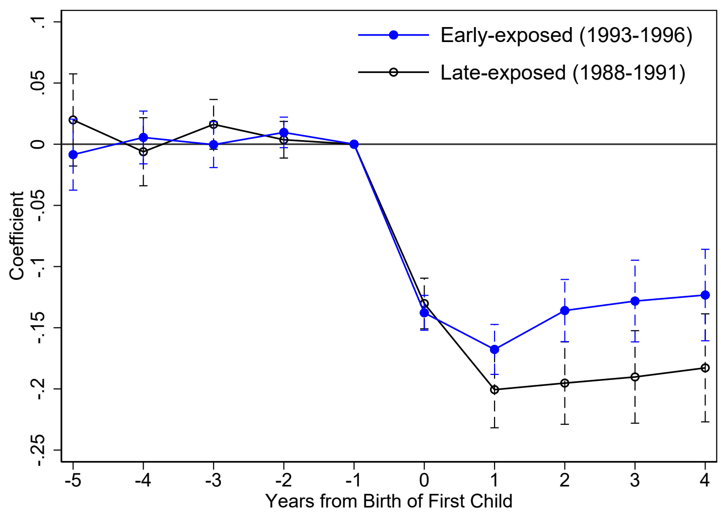
(a) Difference Between Early- and Late-Exposed, Up to Age 20



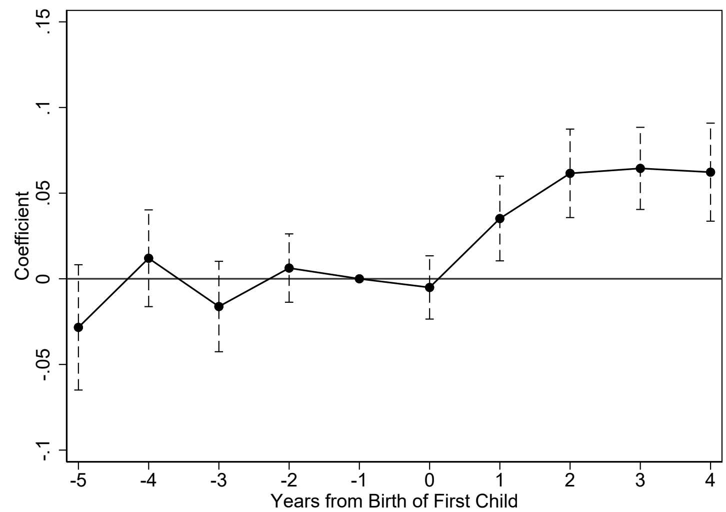
Notes: Panel (a) shows the average maximum EITC benefits in each year since first birth for mothers that are exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). Panel (b) presents the difference in benefits between early- and late-exposed mothers. *Data:* Nominal EITC benefits are obtained from the Tax Policy Center (`https://www.taxpolicycenter.org/statistics/eitc-parameters`), and have been converted to 2016 dollars using the CPI from the Bureau of Labor Statistics.

Figure 2: Effect of Early Exposure to Work Incentives on Short-Run Employment

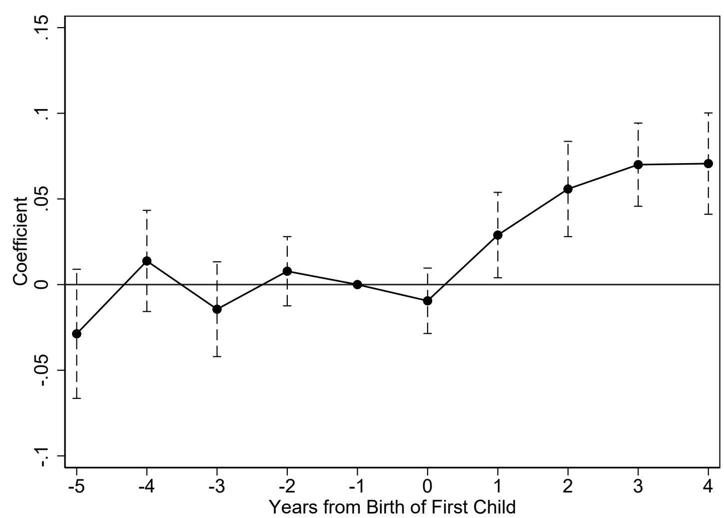
(a) Early- and Late-Exposed Never-Married Mothers



(b) Difference-in-Difference

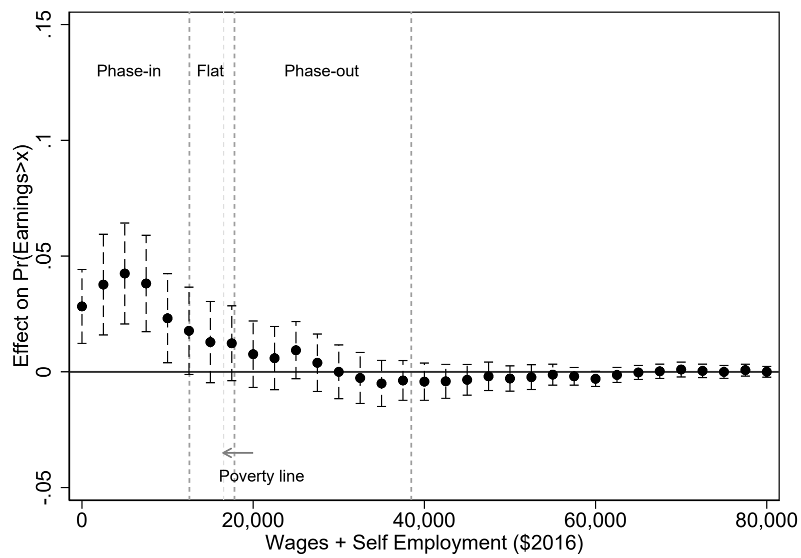


(c) Triple-Difference



Notes: These figures present the point estimates and 95% confidence intervals from event studies of employment around birth for mothers who were exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). Panel A plots the estimates on indicators for years since first birth crossed with being "early-exposed" or "late-exposed" using never-married mothers. Panel B shows the estimates for the dynamic DD using never-married mothers. Panel C shows the estimates for the dynamic DDD, where we use married mothers as an additional comparison group. All regressions include indicators for year of first childbirth and years since first childbirth, mother's age and birth year, mother's race and education group interacted with post-birth, and state, as well as controls for the state-level unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, implementation of six types of welfare waivers, implementation of any waiver or TANF, and implementation of the 2009 EITC reform. The DDD regressions allow for differential effects by marital status for these controls. Standard errors are clustered by state. *Data:* 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2015 longitudinal SSA earnings records. All dollar amounts have been converted to 2016 dollars using the Bureau of Labor Statistics CPI. *Sample:* women whose first child was born in 1988–1991 or 1993–1996, who were at least 19 at first birth and less than 50 years old at CPS interview, and were either married or never married at the time of the CPS interview. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

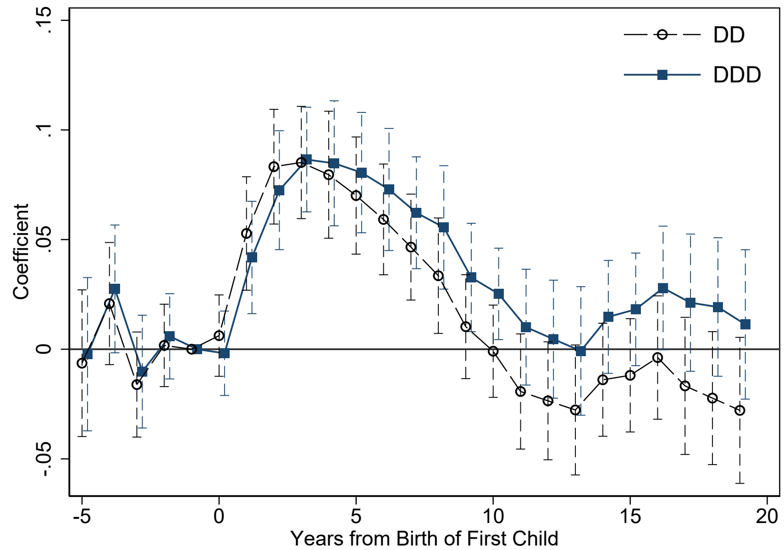
Figure 3: Effect of Early Work Incentives on Earnings Density – Triple-Difference Estimates



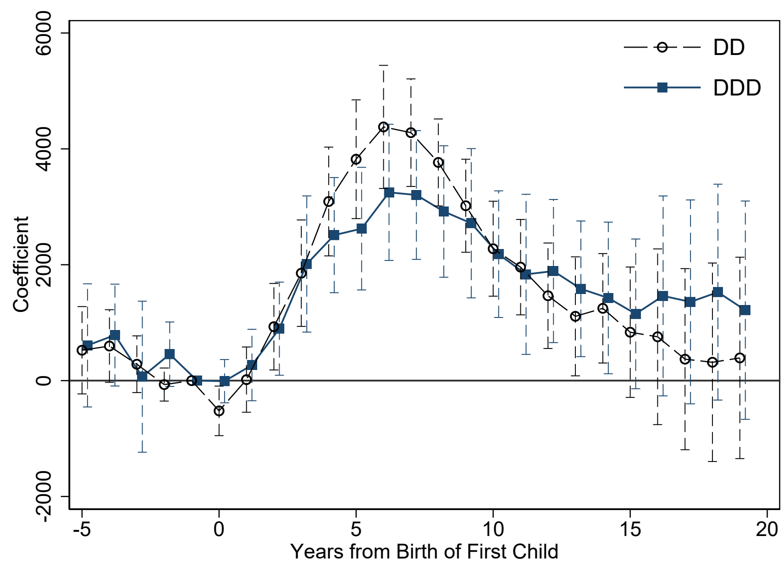
Notes: This figure shows the coefficients and 95% confidence intervals from DDD regressions that compare the earnings distribution of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), across never-married and married mothers. Each marker is obtained from a different regression, where the outcome is an indicator for having annual earnings ($2016) at least as great as X – where X is the amount shown on the x-axis – during years 0-3 since birth. The dashed grey lines show, respectively, the end of the phase-in region on the 1994 EITC schedule; the 1994 poverty line; the end of the flat region on the 1994 EITC schedule; and the end of the phase-out region on the 1994 EITC schedule. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. Nominal EITC benefits are obtained from the Tax Policy Center (`https://www.taxpolicycenter.org/statistics/eitc-parameters`). *Years:* We include data from 5 years prior to a first birth up to the 4^{th} year after a first birth.

Figure 4: Effect of Early Work Incentives on Long-Run Outcomes

(a) Employment



(b) Annual Earnings



Notes: These figures present the coefficients and 95% confidence intervals from event studies that compare the employment (Panel A) or earnings ($2016, Panel B) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), in each year from a first birth. For each outcome we present both the DD using never-married mothers as well as the DDD in which we use married mothers as an additional comparison group. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table 1: Effect of Early Work Incentives on Short-Run Employment

	Max 1-Child	Employed (Earnings>0)			Wage Earnings>0		
	EITC ($2016)	Never-Married	Married	DDD	Never-Married	Married	DDD
PostBirth * EarlyExp	1009.2	0.037***	0.003		0.032***	0.001	
		(0.009)	(0.003)		(0.009)	(0.003)	
PostBirth * EarlyExp * NM				0.034***			0.031***
				(0.008)			(0.008)
Mean Y		0.682	0.753	0.746	0.678	0.736	0.730
Observations		112910	972880	1085790	112910	972880	1085790

Notes: This table shows estimates from regressions comparing the maximum EITC benefits (column 1), employment (positive total earnings, columns 2–4), and positive wage earnings (columns 5–7) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991). We present the DD using never-married mothers (columns 2 and 5), the DD using married mothers (columns 3 and 6), and the DDD (columns 4 and 7). All regressions include indicators for year of first childbirth and years since first childbirth, mother's age and birth year, mother's race and education group interacted with post-birth, and state, as well as controls for the state-level unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, implementation of six types of welfare waivers, implementation of any waiver or TANF, and implementation of the 2009 EITC reform. The DDD regressions allow for differential effects of these controls by marital status. Standard errors are clustered by state. *Data:* 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2015 longitudinal SSA earnings records. All dollar amounts have been converted to 2016 dollars using the Bureau of Labor Statistics CPI. *Sample:* women whose first child was born in 1988–1991 or 1993–1996, who were at least 19 at first birth and less than 50 years old at CPS interview, and were either married or never married at the time of the CPS interview. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table 2: Testing Alternative Explanations for Short-Run Employment Effects – Heterogeneity and Sensitivity of Effects for Never-Married Mothers

	By Birth Parity		By Change in U-Rate		Control for Dynamics		Up to 1996	
	2+ vs.1	3+ vs 2	High	Low	U-Rate	Ref+Waivs	All	No Waiver
PostBirth * EarlyExp * Child 2+	0.032**							
	(0.014)							
PostBirth * EarlyExp * Child 3+		0.011						
		(0.022)						
PostBirth * EarlyExp			0.032***	0.033**	0.033***	0.032***		
			(0.011)	(0.015)	(0.009)	(0.009)		
EarlyExp * 1 Yr. From Birth							0.020	0.012
							(0.013)	(0.015)
EarlyExp * 2 Yr. From Birth							0.041***	0.051**
							(0.013)	(0.022)
EarlyExp * 3 Yr. From Birth							0.043***	0.053*
							(0.015)	(0.032)
Parity:								
1^{st} child	X	-	X	X	X	X	X	X
2^{nd}+ child	X	X	-	-	-	-	-	-
Mean Y	0.648	0.583	0.701	0.664	0.682	0.682	0.659	0.625
Chg. U-Rate: 94-00 - 88-93	-	-	-0.018	-0.006	-	-	-	-
Observations	174050	61140	55860	57050	112910	112910	96795	26371

Notes: This table shows the results from regressions comparing the employment of never-married mothers exposed to the 1993 EITC reform early (birth: 1993–1996) and late (birth: 1988–1991). Column (1) includes all mothers with a birth from 1988–1991 or 1993–1996, and uses mothers after a first birth as comparisons for mothers after a second-or-higher-order birth ("child 2+"). Column (2) includes all mothers with a second-or-higher-order birth from 1988–1991 or 1993–1996, and uses mothers after a second birth as comparisons for mothers after a third-or-higher-order birth ("child 3+"). Columns (3) and (4) compare mothers with early- and late-exposed first births in states that experienced an above-median (column 3) or below-median (column 4) change in the unemployment rate between 1994-2000 and 1988-1993. Columns (5) and (6) present estimates when we add to our baseline DD specification interactions between the age of one's first child and the unemployment rate (column 5) or between the age of one's first child and our indicators for welfare reform and waivers (column 6). Columns (7) and (8) present the DD event study estimates for years 1–3 after a first birth when we restrict the sample to the years prior to 1996 (column 7) and to states that didn't pass a waiver up to 1996 (column 8). See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table 3: Effect of Early Work Incentives on Long-Run Labor Market Outcomes

	Employed (Earnings>0)		Years of Experience		Earnings	
	Never Married	DDD	Never-Married	DDD	Never-Married	DDD
5-9 Yrs From Birth * EarlyExp	0.043***		0.471***		3655.9***	
	(0.010)		(0.075)		(362.6)	
10+ Yrs From Birth * EarlyExp	-0.017		0.449***		1206.1***	
	(0.011)		(0.139)		(444.1)	
5-9 Yrs From Birth * EarlyExp * NM		0.055***		0.464***		2617.6***
		(0.010)		(0.070)		(526.6)
10+ Yrs From Birth * EarlyExp * NM		0.010		0.617***		1392.7**
		(0.011)		(0.129)		(587.3)
Mean Y	0.761	0.765	0.761	0.765	17000.050	23612.672
Observations	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing the employment (columns 1–2), years of experience (columns 3–4), and annual earnings ($2016, columns 5–6) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 5-9 and 10+ years from first birth. For each outcome we present both the DD using never-married mothers as well as the DDD in which we use married mothers as an additional comparison group. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table 4: Effect of Early Work Incentives on Hours of Work – CPS Responses

	Hours Worked Last Week (including 0's)			
	Positive	Part Time	Full Time	Level
A: Never-Married				
0-4 Yrs From Birth * EarlyExp	0.092**	0.082***	0.010	3.074*
	(0.042)	(0.029)	(0.036)	(1.700)
5-9 Yrs From Birth * EarlyExp	0.100***	-0.001	0.100***	4.533***
	(0.029)	(0.033)	(0.029)	(1.146)
10+ Yrs From Birth * EarlyExp	-0.015	-0.008	-0.007	-0.484
	(0.033)	(0.027)	(0.034)	(1.493)
Mean Y	0.659	0.186	0.474	24.313
Individuals	9907	9907	9907	9907
B: Add Married Comparison				
0-4 Yrs From Birth * EarlyExp * NM	0.069	0.063**	0.006	2.161
	(0.043)	(0.030)	(0.037)	(1.748)
5-9 Yrs From Birth * EarlyExp * NM	0.054	-0.030	0.084**	3.324***
	(0.032)	(0.037)	(0.033)	(1.239)
10+ Yrs From Birth * EarlyExp * NM	0.004	0.000	0.004	0.277
	(0.033)	(0.030)	(0.035)	(1.545)
Mean Y	0.694	0.243	0.451	24.443
Individuals	94414	94414	94414	94414

Notes: This table shows the results from regressions comparing the likelihood of working any hours last week (column 1), the likelihood of working part time (>0 and <35 hours per week, column 2), the likelihood of working full time (≥35 hours per week, column 3), and the average hours of work (column 4) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0-4, 5-9 and 10+ years from first birth. Panel A shows estimates from a single difference. Panel B shows estimates from a DD in which we use married mothers as an additional comparison group. All regressions include indicators for mother's age, birth year, race, education group, state, and average pre-birth employment and earnings, as well as controls for the state-level unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, implementation of six types of welfare waivers, implementation of any waiver or TANF, and implementation of the 2009 EITC reform. The DD regressions allow for differential effects of these controls by marital status. See Table 1 for information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table 5: Effect of Early Work Incentives on EITC Benefits

	Total	Behavioral	Mechanical
A: Never-Married			
0-4 Yrs From Birth * EarlyExp	619.3***	188.0***	431.3***
	(38.1)	(34.8)	(17.3)
5-9 Yrs From Birth * EarlyExp	52.9	35.0	17.9***
	(32.5)	(32.8)	(1.5)
10+ Yrs From Birth * EarlyExp	-99.1***	-95.7***	-3.5**
	(31.0)	(31.8)	(1.6)
Observations	282275	282275	282275
B: DDD			
0-4 Yrs From Birth * EarlyExp * NM	400.3***	186.2***	214.1***
	(45.0)	(39.5)	(16.1)
5-9 Yrs From Birth * EarlyExp * NM	92.9***	81.9**	11.0***
	(33.2)	(33.0)	(1.8)
10+ Yrs From Birth * EarlyExp * NM	-89.0**	-85.1**	-3.9***
	(33.7)	(34.0)	(1.2)
NM Mean 0-4 Yrs From Birth	1068.5	–	–
NM Mean 5-9 Yrs From Birth	1423.3	–	–
NM Mean 10+ Yrs From Birth	1280.4	–	–
Observations	2714475	2714475	2714475

Notes: This table shows the results from regressions comparing the simulated EITC benefits (in 2016 dollars) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0-4, 5-9 and 10+ years from first birth. Panel A shows the results from the DD using never-married mothers. Panel B shows the results from the DDD in which we use married mothers as an additional comparison group. The outcomes are simulated total EITC eligibility (column 1); the "behavioral" change in EITC benefits, estimated using a simulated EITC that assigns all mothers the EITC schedule of 1994 first births (column 2); and the "mechanical" change in benefits, estimated using the difference between simulated benefits in columns 1 and 2 (column 3). See the text for details. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

ONLINE APPENDIX:
Long-Run Effects of Incentivizing Work After Childbirth

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June, 2020

A Supplemental Tables and Figures

Table A.1: Summary Statistics by Marital Status

	Never Married	Married
Share Non-White	0.638	0.254
	(0.481)	(0.435)
Age at First Birth	23.61	26.44
	(4.393)	(4.574)
HH EITC Eligibility Pre-Birth	0.968	0.490
	(0.175)	(0.500)
Secondary Earner Pre-Birth	–	0.630
	–	(0.483)
HH EITC 0-4 Yrs Post-Birth ($2016, 94 Sched.)	1127.8	522.2
	(1291.0)	(1013.3)
Share High School or Less	0.557	0.358
	(0.497)	(0.479)
Any Earnings Pre-Birth	0.894	0.897
	(0.308)	(0.304)
Mean of Any Earnings Pre-Birth	0.660	0.781
	(0.474)	(0.414)
Mean Earnings if Working ($2016) Pre-Birth	12073.7	24672.2
	(14264.9)	(44866.8)
Unique Women	11291	97288
Observations	282275	2432200

Notes: This table shows summary statistics for our sample of never-married and married mothers. "Secondary earner pre-birth" is an indicator equal to one if the woman earns less than 40% of total household earnings. See Table 1 for information on data and sample construction.

Table A.2: Characteristics of Never-Married Mothers by Early- or Late-Exposure

	All	Late Exposure (88-91)	Early Exposure (93-96)	P-value
A: Pre-Birth Outcomes				
Share Non-White	0.638	0.674	0.609	<0.01
	(0.481)	(0.469)	(0.488)	
Age at First Birth	23.61	23.54	23.67	0.170
	(4.393)	(4.173)	(4.557)	
HH EITC Eligibility Pre-Birth	0.968	0.967	0.969	0.481
	(0.175)	(0.179)	(0.172)	
Share High School or Less	0.557	0.601	0.523	<0.01
	(0.497)	(0.490)	(0.499)	
Any Earnings Pre-Birth	0.894	0.888	0.898	0.151
	(0.308)	(0.315)	(0.303)	
Mean of Any Earnings Pre-Birth	0.660	0.641	0.674	<0.01
	(0.474)	(0.480)	(0.469)	
Mean Earnings if Working ($2016) Pre-Birth	12073.7	11929.7	12181.2	0.478
	(14264.9)	(14153.4)	(14347.0)	
B: Post-Birth Outcomes				
Mean of Any Earnings 0-4 yrs Post-Birth	0.705	0.631	0.763	<0.01
	(0.456)	(0.483)	(0.425)	
Mean of Any Earnings 5-9 yrs Post-Birth	0.812	0.771	0.844	<0.01
	(0.391)	(0.420)	(0.363)	
Mean of Any Earnings 10+ yrs Post-Birth	0.815	0.823	0.808	0.054
	(0.389)	(0.382)	(0.394)	
Mean Earnings ($2016) 0-4 yrs Post-Birth	11656.9	9926.4	13012.6	<0.01
	(16407.3)	(14750.6)	(17477.6)	
Mean Earnings ($2016) 5-9 yrs Post-Birth	18271.2	15584.2	20376.3	<0.01
	(19672.1)	(17474.0)	(20997.2)	
Mean Earnings ($2016) 10+ yrs Post-Birth	23525.4	22685.0	24183.9	<0.01
	(25116.5)	(22473.7)	(26988.8)	
Mean Earnings if Working ($2016) 0-4 yrs Post-Birth	16577.9	15737.1	17126.8	<0.01
	(17400.4)	(15905.6)	(18289.8)	
Mean Earnings if Working ($2016) 5-9 yrs Post-Birth	22715.5	20373.4	24408.4	<0.01
	(19618.0)	(17486.2)	(20862.1)	
Mean Earnings if Working ($2016) 10+ yrs Post-Birth	29558.0	28107.6	30729.7	<0.01
	(25125.8)	(22012.3)	(27327.8)	
Unique Women	11291	4960	6331	11291
Observations	282275	124000	158275	282275

Notes: This table shows summary statistics for our early and late-exposed never-married samples, respectively, for pre-birth outcomes (Panel A) and post-birth outcomes (Panel B). While we include "Share High School or Less" in Panel A along with the other demographic characteristics, we actually observe this outcome after a first birth, which makes it a potential outcome of early exposure. See Table 1 for information on the data and sample construction.

Table A.3: Do Observables Change Differentially Across CPS Surveys for Early-Exposed Mothers? – Never-Married Mothers

	Beta	P-value
Share Non-White	0.002	0.211
Age at First Birth	0.019	0.185
HH EITC Eligibility Pre-Birth	-0.000	0.780
Share High School or Less	0.000	0.818
Any Earnings Pre-Birth	0.001	0.415
Mean of Any Earnings Pre-Birth	0.002	0.187
Years of Experience Pre-Birth	0.002	0.871
Mean Earnings ($2016) Pre-Birth	-3.510	0.933
Mean Earnings if Working ($2016) Pre-Birth	-4.576	0.920
Mean Earnings if Working ($2016) 0-4 yrs Post-Birth	-51.707	0.150
Mean Earnings if Working ($2016) 5-9 yrs Post-Birth	-1.170	0.984
Mean Earnings if Working ($2016) 10+ yrs Post-Birth	0.366	0.996
Observations	11291	11291

Notes: Each row of this table shows the results from a separate regression of an observable characteristic (column 1) on a linear trend in "survey years from first birth" (CPS year minus year of first birth) and the interaction of "survey years from first birth" and early-exposure. Columns 2 and 3 shows the coefficient on the interaction and its p-value, which indicate whether the characteristics of early-exposed mothers evolve differently than late-exposed mothers over time. See Table 1 for information on standard errors, data and sample construction.

Table A.4: Effect of Early Work Incentives on Short-Run Employment – By Marital Status and Pre-Birth EITC Eligibility

	Never Married			Married		
	All	Eligible	Non Eligible	All	Eligible	Non Eligible
PostBirth * EarlyExp	0.037***	0.039***	-0.017	0.003	0.002	0.002
	(0.009)	(0.009)	(0.017)	(0.003)	(0.005)	(0.004)
Mean Y	0.682	0.673	0.982	0.753	0.636	0.866
Observations	112910	109320	3590	972880	476700	496180

Notes: This table shows the results from regressions comparing the employment of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) in the first 5 years since the birth of a first child, by pre-birth EITC eligibility. We present the DD using never-married mothers (columns 1–3) as well as the DD using married mothers (columns 4–6). See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table A.5: Effect of Early Work Incentives on Short-Run Self-Employment and Bunching

	Self-Emp. Earnings >0		Bunching ($1500 bins)		Bunching ($2500 bins)	
	Never Married	DDD	Never Married	DDD	Never Married	DDD
PostBirth * EarlyExp	0.010***		0.015***		0.020***	
	(0.003)		(0.004)		(0.005)	
PostBirth * EarlyExp * NM		0.006*		0.011**		0.014***
		(0.003)		(0.004)		(0.005)
Mean Y	0.013	0.034	0.047	0.043	0.077	0.071
Observations	112910	1085790	112910	1085790	112910	1085790

Notes: This table shows the results from regressions comparing the labor market outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) in the first 5 years since the birth of a first child. For each outcome we present both the DD using never-married mothers as well as the DDD in which we we use married mothers as an additional comparison group. Columns 1–2 present results where the outcome is an indicator for positive self-employment earnings, while columns 3–6 present results where the outcome is an indicator for bunching at the first EITC kink, which is defined as having earnings within $1,500 (columns 3–4) or $2,500 (column 5–6) of the first EITC kink. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table A.6: Effect of Early Work Incentives on Short-Run Earnings

	Earnings ($2016)		Wage Earnings ($2016)	
	0-3	0-4	0-3	0-4
A: Never Married				
PostBirth * EarlyExp	833.920***	1120.734***	746.304***	1011.188***
	(223.545)	(244.138)	(237.290)	(260.694)
Mean Y	9251.446	9820.838	9144.424	9698.245
Individuals	101619	112910	101619	112910
B: DDD				
PostBirth * EarlyExp * NM	463.846	657.175**	403.693	590.697*
	(325.425)	(326.522)	(340.478)	(343.140)
Mean Y	18087.091	18272.975	17739.758	17900.830
Individuals	977211	1085790	977211	1085790

Notes: This table shows the results from regressions comparing earnings (in 2016 dollars) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) in the first 5 years since the birth of a first child. For each outcome we present both the DD using never-married mothers as well as the DDD in which we use married mothers as an additional comparison group. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table A.7: Testing Alternative Explanations for Short-Run Employment Effects – Triple-Difference Estimates

	By Change in U-Rate		Control for Dynamics		Up to 1996	
	High	Low	U-Rate	Ref+Waivs	All	No Waiver
PostBirth * EarlyExp * NM	0.028**	0.031**	0.032***	0.028***		
	(0.010)	(0.013)	(0.008)	(0.008)		
EarlyExp * NM * 1 Yr. From Birth					0.020	0.013
					(0.014)	(0.017)
EarlyExp * NM * 2 Yr. From Birth					0.039***	0.053**
					(0.015)	(0.027)
EarlyExp * NM * 3 Yr. From Birth					0.050***	0.056
					(0.017)	(0.040)
Mean Y	0.759	0.732	0.746	0.746	0.745	0.739
Chg. U-Rate: 94-00 - 88-93	-0.018	-0.006	-	-	-	-
Observations	548800	536990	1085790	1085790	946146	232607

Notes: This table shows the results from regressions comparing the employment of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), across never-married and married mothers. Columns (1) and (2) compare early- and late-exposed first births in states that experienced above-median or below-median change in the unemployment rate between 1994-2000 and 1988-1993, respectively. Columns (3) and (4) present estimates where we add to our baseline specification interactions between the age of one's first child and the unemployment rate (column 3) or between the age of one's first child and our indicators for welfare reform and waivers (column 4). Columns (5) and (6) present the event study estimates for years 1–3 after a first birth when we restrict the sample to the years prior to 1996 (column 5) and to states that didn't pass a waiver up to 1996 (column 6). See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table A.8: Effect of Early Work Incentives on Short-Run Earnings – By the Size of the Economic Boom

	> Med. Decline U-Rate		< Med. Decline U-Rate	
	NM	DDD	NM	DDD
PostBirth * EarlyExp	1049.840***		951.293**	
	(302.348)		(350.866)	
PostBirth * EarlyExp * NM		502.234		779.074*
		(440.040)		(414.426)
Mean Y	9579.092	17925.061	10057.406	18628.373
Chg. U-Rate: 94-00 - 88-93	-0.018	-0.018	-0.006	-0.006
Individuals	55860	548800	57050	536990

Notes: This table shows the results from DD regressions comparing the earnings (in 2016 dollars) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) in the first 5 years since the birth of a first child. Columns (1) and (2) examine impacts in states that had an above-median change in unemployment rates between 1994-2000 and 1988-1993 (i.e a larger boom); while Columns (3) and (4) examine impacts in states that had a below-median change in unemployment rates (i.e a smaller boom). See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table A.9: Effect of Early Work Incentives on Short-Run Employment – Heterogeneity by State-Level Child Care Costs

	Base	Cost	Cost if >0	Cost=0
A: Never Married				
PostBirth * EarlyExp	0.038***	0.039***	0.031***	0.034***
	(0.010)	(0.010)	(0.010)	(0.010)
PostBirth * EarlyExp * Child Care		0.006	0.022**	0.024**
		(0.016)	(0.008)	(0.009)
PostBirth * Child Care		-0.026	-0.031***	-0.036**
		(0.021)	(0.010)	(0.013)
Mean Y	0.677	0.677	0.677	0.677
Observations	102980	102980	102980	102980
B: DDD				
PostBirth * EarlyExp * NM	0.037***	0.037***	0.030***	0.033***
	(0.009)	(0.009)	(0.009)	(0.009)
PostBirth * EarlyExp * NM * Child Care		0.006	0.021**	0.023**
		(0.016)	(0.008)	(0.009)
PostBirth * NM * Child Care		-0.026	-0.031***	-0.036**
		(0.021)	(0.010)	(0.013)
Mean Y	0.736	0.736	0.736	0.736
Observations	943240	943240	943240	943240

Notes: This table shows the results from regressions comparing the employment of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) in the first 5 years since the birth of a first child. Column 1 shows our baseline estimate. Columns (2)-(4) show interactions between early exposure and three measures of child care costs in the state: (i) the average cost per child; (ii) the average cost per child conditional on having positive costs; and (iii) the share of mothers with zero child care costs. Panel A shows the DD using never-married mothers. Panel B shows the DDD in which we use married mothers as an additional comparison group. See Table 1 for information on control variables, standard errors, data and sample construction. We construct data on child care costs from Wave 3 of the 1990, 1991, and 1993 SIPP panels, and Wave 6 of the 1992 SIPP panel. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table A.10: Effect of Early Work Incentives on Short-Run Employment – Heterogeneity by the Presence and Generosity of a State EITC Supplement

	(1)	(2)	(3)	(4)
PostBirth * EarlyExp	0.039***	0.033***	0.038***	0.035***
	(0.009)	(0.009)	(0.009)	(0.009)
PostBirth * State EITC	-0.015	-0.054***		
	(0.009)	(0.011)		
PostBirth * State EITC * EarlyExp		0.053***		
		(0.012)		
PostBirth * State EITC (%)			-0.007	-0.014***
			(0.005)	(0.004)
PostBirth * State EITC (%) * EarlyExp				0.013**
				(0.006)
Mean Y	0.682	0.682	0.682	0.682
Observations	112910	112910	112910	112910

Notes: This table shows the results from DD regressions comparing the employment of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) in the first 5 years since the birth of a first child. Columns (1) and (2) show interactions between early exposure and whether there is any state EITC supplement available in the current year; while columns (3) and (4) show interactions between early exposure and whether the size (%) of the state EITC supplement available in the current year. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table A.11: Long-Run Effects on Wage Earnings and Self-Employment Earnings

	Wage Earnings		Pos. Self-Emp.		Self Emp Earnings	
	NM	DDD	NM	DDD	NM	DDD
5-9 Yrs From Birth * EarlyExp	3390.469***		0.019***		265.416***	
	(372.353)		(0.005)		(81.426)	
10+ Yrs From Birth * EarlyExp	1053.696**		0.013***		152.367*	
	(445.639)		(0.004)		(78.356)	
5-9 Yrs From Birth * EarlyExp * NM		2467.511***		0.014***		150.071
		(515.123)		(0.005)		(100.623)
10+ Yrs From Birth * EarlyExp * NM		1352.872**		0.004		39.854
		(566.332)		(0.005)		(95.049)
Mean Y	16539.998	22846.020	0.045	0.061	460.053	766.652
Individuals	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing wage earnings ($2016, columns 1–2), positive self-employment earnings (columns 3–4), self-employment earnings ($2016, columns 5–6) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 5–9 and 10+ years since first birth. The odd columns show DD regressions using never-married (NM) mothers. The even columns show DDD regressions that use married mothers as an additional comparison group. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.12: Long-Run Effects on Alternative Measures of Earnings

	Earnings, if Positive		Log Earnings		Winsorized	
	NM	DDD	NM	DDD	NM	DDD
5-9 Yrs From Birth * EarlyExp	3648.069***		0.261***		3542.770***	
	(355.019)		(0.030)		(360.322)	
10+ Yrs From Birth * EarlyExp	1980.859***		0.076**		1025.886**	
	(421.074)		(0.030)		(406.615)	
5-9 Yrs From Birth * EarlyExp * NM		1515.576**		0.165***		2510.213***
		(580.168)		(0.034)		(451.650)
10+ Yrs From Birth * EarlyExp * NM		1190.222*		0.050		1201.351**
		(683.396)		(0.034)		(478.678)
Mean Y	21937.658	30705.208	9.399	9.750	16915.479	22971.402
Individuals	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing the earnings of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 5–9 and 10+ years since first birth. "Earnings, if Positive" (columns 1–2) is missing for all individuals that have zero earnings. "Winsorized" earnings (columns 5–6) have been top-coded at $175,000 ($2016), which is the top 1% of married mothers' earnings. DD regressions using never-married (NM) mothers are shown in the odd columns, while the DDD regressions that use married mothers as an additional comparison are shown in the even columns. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.13: Effect of Early Work Incentives on Hours and Weeks of Work – CPS Responses

	Level (including 0's)			Cumulative		
	Weekly Hours	Annual Weeks	Hours× Weeks	Weekly Hours	Annual Weeks	Hours × Weeks
A: Never Married						
0-4 Yrs From Birth * EarlyExp	3.074*	4.191**	145.0*	8.915*	12.208**	428.1**
	(1.700)	(1.830)	(72.9)	(4.633)	(5.204)	(210.0)
5-9 Yrs From Birth * EarlyExp	4.533***	1.039	84.0	33.494***	26.861**	1245.1***
	(1.146)	(1.218)	(60.8)	(8.905)	(10.212)	(442.3)
10+ Yrs From Birth * EarlyExp	-0.484	0.127	22.4	33.360*	22.578	1125.0
	(1.493)	(1.303)	(74.8)	(18.217)	(17.443)	(905.1)
Scaled β 0-4				0.255	0.305	0.306
Scaled β 5-9				0.957	0.672	0.889
Scaled β 10+				0.953	0.564	0.804
Observations	9907	10020	9921	9907	10020	9921
B: Add Married Comparison						
0-4 Yrs From Birth * EarlyExp * NM	2.161	3.137	32.0	7.938***	12.882***	283.7**
	(1.748)	(1.907)	(75.3)	(2.598)	(4.083)	(135.8)
5-9 Yrs From Birth * EarlyExp * NM	3.324***	1.264	107.1	27.294***	36.054***	1192.2***
	(1.239)	(1.408)	(64.6)	(4.508)	(7.545)	(244.5)
10+ Yrs From Birth * EarlyExp * NM	0.277	0.528	30.9	28.455***	31.706***	944.8**
	(1.545)	(1.293)	(75.4)	(7.004)	(8.727)	(355.1)
Scaled β 0-4				0.227	0.322	0.203
Scaled β 5-9				0.780	0.901	0.852
Scaled β 10+				0.813	0.793	0.675
Observations	94414	98077	96918	94414	98077	96918

Notes: This table shows the results from regressions comparing labor market outcomes between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9 and 10+ years since first birth. Panel A shows a single-difference using never-married mothers. Panel B shows the DD estimates in which we use married mothers as an additional comparison group. The outcomes are number of hours worked per week (column 1), number of weeks worked last year (column 2), and hours times weeks (column 3), as well as the cumulative sums of each these (columns 4–6). For interpretation, we calculate a "scaled β" for the short-run, medium-run, and long-run, shown at the bottom of each panel, by dividing the cumulative total for each of these periods by the number of hours in a full-time, full-year of work (i.e., 35 hours × 40 weeks). The scaled β is thus the additional years of full-time, full-year work accrued by early-exposed mothers. See Table 4 for information on control variables, and Table 1 for information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.14: Effect of Early Work Incentives on Completed Fertility – CPS Responses

	Number of Kids		2+ Kids		3+ Kids		Yrs b/w 1 and 2	
	NM	+ Married	NM	+ Married	NM	+ Married	NM	+ Married
EarlyExp	0.008		-0.005		0.005		-0.288	
	(0.068)		(0.044)		(0.035)		(0.414)	
EarlyExp * NM		0.010		0.012		-0.006		-0.117
		(0.070)		(0.045)		(0.036)		(0.439)
Mean Y	1.834	2.222	0.537	0.771	0.207	0.317	4.313	3.619
Observations	3638	45392	3638	45392	3638	45392	1953	34981

Notes: This table shows the results from regressions comparing completed fertility between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991). The odd columns show estimates from a single difference using never-married mothers. The even columns show the DD estimates in which we use married mothers as an additional comparison group. The outcomes are total number of children in the household (columns 1–2), an indicator for having at least 2 children (columns 3–4), having at least three children (columns 5–6), and the number of years between one's first and second child (columns 7–8). We restrict the sample to mothers interviewed in the CPS between the ages of 36 to 44, who are more likely to have completed their childbearing. See Table 4 for information on control variables, and Table 1 for additional information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.15: Effect of Early Work Incentives on Short-Run Employment – Using Future Mothers for Comparison

	Baseline		0-3 Yrs Post			
	Never Married	DDD	Never Married	DDD	No Kids	DDD
PostBirth * EarlyExp	0.037***		0.032***		0.010	
	(0.009)		(0.009)		(0.008)	
PostBirth * EarlyExp * NM		0.034***		0.028***		
		(0.008)		(0.008)		
PostBirth * EarlyExp * Kids						0.026**
						(0.012)
Never Married Moms	Yes	Yes	Yes	Yes	No	Yes
Married Moms	No	Yes	No	Yes	No	No
Never Married Future Moms	No	No	No	No	Yes	Yes
Observations	112910	482120	101619	433908	78828	146574

Notes: This table shows the results from regressions comparing short-run (0-3 years after birth) employment of "mothers" exposed to the 1993 EITC reform early (first "birth": 1993–1996) and late (first "birth": 1988–1991), across never-married mothers and future mothers with placebo births. Column (1) and (2) present our baseline DD and DDD estimates. Columns (3) and (4) present results limit the sample to up to 3 years after birth. Columns (5) and (6) limit the sample to up to 3 years after birth and use future mothers as a comparison group limiting up. Future mothers are assigned a placebo year of first birth equal to her true year of childbirth minus 4. See Table 1 for information on control variables, standard errors, and data. Sample: women whose child was born in 1988–1991 or 1993–1996 ("actual mothers") or between 1992–1995 or 1997-2000 ("future mothers"), and who were at least 19 at first birth and less than 50 years old at CPS interview, were never married at the time of the CPS interview. *Years:* We include data from 5 years prior to a first birth up to the 4^{th} year after a first birth.

Table A.16: Effect of Early Work Incentives on Long-Run Earnings — Using Childless Women for Comparison

	DD			DDD	
	NM Moms	Childless	NM Childless	All	NM
5-9 Yrs From Birth * EarlyExp	3656***	64	-1293		
	(366)	(785)	(2058)		
10+ Yrs From Birth * EarlyExp	1206***	-1053	-296		
	(444)	(923)	(990)		
5-9 Yrs From Birth * EarlyExp * Mom				2964***	4005**
				(806)	(1989)
10+ Yrs From Birth * EarlyExp * Mom				1922**	1015
				(945)	(1029)
Mean Y	17000	30652	34085	26519	24103
Observations	282275	649875	200750	932150	483025

Notes: This table shows the results from regressions comparing short-run earnings (in 2016 dollars) of "mothers" exposed to the 1993 EITC reform early (first "birth": 1993–1996) and late (first "birth": 1988–1991), across never-married and childless women with placebo births, 5–9 and 10+ years since a first "birth." Column (1) presents our baseline DD estimates for never-married mothers. Columns (2) and (3) present the DD estimates using for all childless women and never-married childless women. Columns (4) and (5) present the DDD using all childless women or never-married childless women as comparison groups. Childless women are assigned a placebo year of first birth by taking a draw from the distribution of years of birth for never-married mothers who have the same year of birth and level of education as a given childless woman. See Table 1 for information on control variables, standard errors, and data. Sample: never-married women who were less than 50 years old at the time of the CPS interview, and whose child was born in 1988–1991 or 1993–1996 ("actual mothers"), or women between the ages of 40 to 45 at the time of the CPS interview without any children ("childless mothers"). *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.17: Effect of Early Work Incentives on Long-Run Earnings – Sensitivity to Controls for Unemployment and Welfare

	Base		UR Dynamics		(Ref+Waivs)*Dynamics	
	NM	DDD	NM	DDD	NM	DDD
5-9 Yrs From Birth * EarlyExp.	3655.885***		3707.143***		3441.965***	
	(362.636)		(379.546)		(418.293)	
10+ Yrs From Birth * EarlyExp	1206.063***		1184.553***		1070.210**	
	(444.053)		(426.525)		(470.978)	
5-9 Yrs From Birth * EarlyExp * NM		2617.581***		2532.927***		2338.119***
		(526.602)		(515.671)		(523.906)
10+ Yrs From Birth * EarlyExp * NM		1392.726**		1340.217**		1158.918*
		(587.335)		(569.402)		(612.296)
Mean Y	17000.050	23612.672	17000.050	23612.672	17000.050	23612.672
Observations	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing earnings (in 2016 dollars) of never-married mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 5–9 and 10+ years from first birth. The odd columns show the DD using never-married mothers. The even columns show the DDD in which we use married mothers as an additional comparison group. Columns 1–2 present our baseline results. Columns 3–4 show the estimates when we add to our baseline specification interactions between the age of one's first child and the unemployment rate. Columns 5–6 show the estimates when we add to our baseline specification interactions between the age of one's first child and our indicators for welfare reform and waivers. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.18: Effect of Early Work Incentives on Labor Market Outcomes – Sensitivity to Inverse P-Score Reweighting

	Employed (Earnings > 0)		Earnings ($2016)	
	Never Married	DDD	Never Married	DDD
0-4 Yrs From Birth * EarlyExp	0.054***		1119.915***	
	(0.010)		(292.660)	
5-9 Yrs From Birth * EarlyExp	0.045***		3705.595***	
	(0.010)		(365.233)	
10+ Yrs From Birth * EarlyExp	-0.011		1092.091**	
	(0.011)		(441.579)	
0-4 Yrs From Birth * EarlyExp * NM		0.040***		536.620
		(0.009)		(326.863)
5-9 Yrs From Birth * EarlyExp * NM		0.048***		2326.752***
		(0.010)		(526.485)
10+ Yrs From Birth * EarlyExp * NM		0.003		1100.900*
		(0.011)		(591.831)
Observations	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing the employment (columns 1–2) and earnings (columns 3–4) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9 and 10+ years from first birth. The odd columns show the DD using never-married mothers. The even columns show the DDD in which we use married mothers as an additional comparison group. All regressions are reweighted using inverse p-scores so that late and early-exposed mothers are balanced on pre-birth characteristics. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.19: Effect of Early Work Incentives on Labor Market Outcomes – Sensitivity to Alternative Specifications

	Base		Add AFB*YSB		Add Ind FE		Sample: Heads		Sample: Eligibles	
	NM	DDD	NM	DDD	NM	DDD	NM	DDD	NM	DDD
A: Short-Run Employment										
PostBirth * EarlyExp	0.037***		0.036***.		0.031***		0.042***		0.039***	
	(0.009)		(0.009)		(0.009)		(0.009)		(0.009)	
PostBirth * EarlyExp * NM		0.034***		0.033***		0.027***		0.039***		0.038***
		(0.008)		(0.008)		(0.008)		(0.008)		(0.009)
Mean Y	0.682	0.746	0.682	0.746	0.682	0.746	0.688	0.751	0.673	0.643
Observations	112910	1085790	112910	1085790	112910	1085790	89220	1039940	109320	586020
B: Long-Run Earnings										
5-9 Yrs From Birth * EarlyExp	3656***		3693***		3612***		3660***		3670***	
	(362.6)		(366.11)		(349.6)		(377.4)		(357.4)	
10+ Yrs From Birth * EarlyExp	1206***		1259***		1177***.		1491***		1232***	
	(444.1)		(446.2)		(429.6)		(479.7)		(411.4)	
5-9 Yrs From Birth * EarlyExp * NM		2618***.		2422***.		2574***.		2648***		2238***
		(526.6)		(533.2)		(515.3)		(520.9)		(469.7)
10+ Yrs From Birth * EarlyExp * NM		1393**		1170*		1341**		1695***		1303**
		(587.3)		(605.0)		(576.1)		(597.6)		(536.5)
Mean Y	17000	23613	17000	23613	17000	23613	17259	23936	15736	15826
Observations	282275	2714475	282275	2714475	282275	2714475	223050	2599850	245950	1465050

Notes: This table shows the sensitivity of our results comparing the labor market outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991). Panel A shows the results for employment 0-4 years from first birth. Panel B shows the results for earnings ($2016) 5-9 and 10+ years from first birth. The odd columns show the DD using never-married mothers. The even columns show the DDD in which we use married mothers as an additional comparison group. Columns 1–2 show our baseline results. To the baseline controls, we add age-at-birth by years-since-birth fixed effects (columns 3–4) and individual fixed effects (columns 5–6). Columns 7–10 use our baseline controls, but restrict the sample to heads of household (columns 7–8) or women whose income in the years prior to childbirth made them eligible for the EITC (columns 9–10). See Table 1 for information on our baseline control variables, standard errors, data and baseline sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} (20^{th}) year after a first birth in Panel A (B).

Table A.20: Effect of Early Work Incentives on Having Earnings or Experience in the Top 75%, 50%, or 25%

	Top 75 Percent		Above Median		Top 25 Percent	
	NM	DDD	NM	DDD	NM	DDD
A: Earnings						
PostBirth * EarlyExp * 10+ Yrs From Birth	-0.007		0.007		0.019***	
	(0.012)		(0.010)		(0.006)	
PostBirth * EarlyExp * 10+ Yrs From Birth * NM		0.019		0.016		0.017**
		(0.012)		(0.011)		(0.008)
Mean Y	0.724	0.738	0.418	0.500	0.145	0.250
Individuals	282275	2714475	282275	2714475	282275	2714475
B: Experience						
PostBirth * EarlyExp * 10+ Yrs From Birth 0.052***		0.028***		-0.018***		
(0.008)		(0.006)		(0.005)		
PostBirth * EarlyExp * 10+ Yrs From Birth * NM		0.028***		0.011		0.005
		(0.008)		(0.007)		(0.005)
Mean Y	0.582	0.719	0.251	0.470	0.093	0.214
Individuals	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing the outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from first birth. The outcomes are indicators for being at or above a threshold in the earnings (Panel A) or experience (Panel B) distributions. The thresholds are: top 75% (columns 1-2), top 50% (columns 3–4), or top 25% (columns 5–6). The distributions are defined separately for each year since first birth and include both married and never-married mothers. The odd columns show the DD using never-married mothers. The even columns show the DDD in which we use married mothers as an additional comparison group. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.21: Effect of Early Work Incentives on Jointly Having "High Earnings" (Top 25%) and "High Experience" (Work 3 Yrs. After a First Birth)

	Pr(High Earn + High Exp)	Pr(High Earn + Low Exp)	Pr(Low Earn + High Exp)	Pr(Low Earn + Low Exp)
A: Never-Married				
10+ Yrs From Birth * EarlyExp	0.028***	-0.009***	0.108***	-0.127***
	(0.006)	(0.003)	(0.016)	(0.015)
Mean Y	0.125	0.021	0.545	0.310
Observations	282275	282275	282275	282275
B: DDD				
10+ Yrs From Birth * EarlyExp * NM	0.020**	-0.003	0.075***	-0.092***
	(0.008)	(0.004)	(0.016)	(0.014)
Mean Y	0.230	0.020	0.472	0.278
Observations	2714475	2714475	2714475	2714475

Notes: This table shows the results from regressions comparing outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from first birth. The outcomes are indicators for having "high earnings" (top 25%) or "low earnings" (bottom 75%) crossed with indicators for having "high experience" (having worked in each of the three years after a first birth) or "low experience" (not having worked in each of the three years after a first birth). We show estimates for having "high experience and high earnings" (column 1), having high earnings and low experience (column 2), "low earnings and high experience" (column 3), and "low earnings and low experience" (column 4). Panel A presents the DD using never-married mothers. Panel B presents the DDD in which we use married mothers as an additional comparison group. See the text and Appendix E for more details. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.22: Effect of Early Work Incentives on Jointly Having "High Earnings" (Top 25%) and "High Experience" (Top 75%)

	Pr(High Earn + High Exp)	Pr(High Earn + Low Exp)	Pr(Low Earn + High Exp)	Pr(Low Earn + Low Exp)
A: Never-Married				
10+ Yrs From Birth * EarlyExp	0.021***	-0.002	0.031***	-0.050***
	(0.005)	(0.002)	(0.009)	(0.009)
Mean Y	0.134	0.012	0.449	0.406
Observations	282275	282275	282275	282275
B: DDD				
10+ Yrs From Birth * EarlyExp * NM	0.017**	0.000	0.010	-0.028***
	(0.007)	(0.002)	(0.010)	(0.008)
Mean Y	0.240	0.010	0.478	0.272
Observations	2714475	2714475	2714475	2714475

Notes: This table shows the results from regressions comparing outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from first birth. The outcomes are indicators for having "high earnings" (top 25%) or "low earnings" (bottom 75%) crossed with indicators for having "high experience" (top 75%) or "low experience" (bottom 25%). We show estimates for having "high experience and high earnings" (column 1), having high earnings and low experience (column 2), "low earnings and high experience" (column 3), and "low earnings and low experience" (column 4). Panel A presents the DD using never-married mothers. Panel B presents the DDD in which we use married mothers as an additional comparison group. See the text and Appendix E for more details. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.23: Effect of Early Work Incentives on Service Occupations – CPS Responses

	Service							
	Housekeep	Janitor	Food	Child	Beauty	Recreation	Protect	Health Serv
Panel A: Never Married								
0-4 Yrs from Birth * EarlyExp	-0.019*	-0.007	0.027	0.012	-0.003	0.001	0.008	0.013
	(0.011)	(0.009)	(0.022)	(0.007)	(0.008)	(0.003)	(0.006)	(0.018)
5-9 Yrs from Birth * EarlyExp	-0.011	-0.001	0.015	0.012*	0.004	0.002	0.010	0.033*
	(0.012)	(0.008)	(0.020)	(0.007)	(0.010)	(0.003)	(0.009)	(0.018)
10+ Yrs from Birth * EarlyExp	-0.009	-0.004	0.019	0.003	-0.004	0.000	0.006	0.040**
	(0.011)	(0.007)	(0.013)	(0.006)	(0.009)	(0.005)	(0.007)	(0.018)
Mean Y	0.025	0.017	0.064	0.015	0.013	0.006	0.012	0.066
Individuals	10006	10006	10006	10006	10006	10006	10006	10006
Panel B: Add Married Comparison								
0-4 Yrs from Birth * EarlyExp * NM	-0.019*	-0.007	0.029	0.010	-0.007	0.004	0.008	0.013
	(0.010)	(0.009)	(0.022)	(0.009)	(0.010)	(0.004)	(0.006)	(0.020)
5-9 Yrs from Birth * EarlyExp * NM	-0.010	-0.005	0.012	0.011	0.002	0.003	0.015	0.027
	(0.012)	(0.008)	(0.020)	(0.007)	(0.011)	(0.003)	(0.009)	(0.019)
10+ Yrs from Birth * EarlyExp * NM	-0.007	-0.006	0.021	0.002	-0.004	0.002	0.008	0.035**
	(0.011)	(0.007)	(0.013)	(0.007)	(0.009)	(0.005)	(0.007)	(0.017)
Mean Y	0.013	0.008	0.034	0.018	0.012	0.004	0.006	0.034
Individuals	95573	95573	95573	95573	95573	95573	95573	95573

Notes: This table shows the results from regressions comparing the probability of reporting being in each service occupation (including mothers that are not working) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9, and 10+ years from a first birth. Panel A presents the single-difference using never-married mothers. Panel B presents the DD in which we use married mothers as an additional comparison group. Occupation definitions are in Appendix C.1. See Table 4 for information on control variables, and Table 1 for information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Table A.24: Effect of Early Work Incentives on Non-Service Occupations – CPS Responses

	Non-Service						
	Exec/Man	Prof/Tech	Fin Sales	Ret Sales	Cleric	Agricultural	Mech/Constr/Min
Panel A: Never Married							
0-4 Yrs from Birth * EarlyExp	0.019	-0.028	0.017	-0.021	-0.005	0.001	-0.003
	(0.021)	(0.021)	(0.012)	(0.025)	(0.032)	(0.005)	(0.005)
5-9 Yrs from Birth * EarlyExp	0.013	-0.009	0.007	-0.013	0.000	0.009	-0.002
	(0.020)	(0.025)	(0.010)	(0.018)	(0.025)	(0.006)	(0.005)
10+ Yrs from Birth * EarlyExp	0.018	-0.031	0.002	-0.013	-0.038*	0.008*	-0.002
	(0.015)	(0.023)	(0.007)	(0.013)	(0.023)	(0.004)	(0.004)
Mean Y	0.064	0.107	0.022	0.064	0.188	0.006	0.005
Individuals	10006	10006	10006	10006	10006	10006	10006
Panel B: Add Married Comparison							
0-4 Yrs from Birth * EarlyExp * NM	0.032	-0.026	0.014	-0.008	-0.006	-0.007	-0.002
	(0.023)	(0.024)	(0.012)	(0.026)	(0.034)	(0.006)	(0.006)
5-9 Yrs from Birth * EarlyExp * NM	0.012	-0.002	0.004	-0.007	-0.012	0.006	0.001
	(0.020)	(0.030)	(0.011)	(0.019)	(0.026)	(0.006)	(0.005)
10+ Yrs from Birth * EarlyExp * NM	0.025	-0.023	-0.001	-0.014	-0.051**	0.008	-0.000
	(0.016)	(0.024)	(0.009)	(0.014)	(0.025)	(0.005)	(0.004)
Mean Y	0.105	0.204	0.032	0.044	0.177	0.008	0.004
Individuals	95573	95573	95573	95573	95573	95573	95573

Notes: This table shows the results from regressions comparing the probability of reporting being in each non-service occupation (including mothers that are not working) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9, and 10+ years from a first birth. Panel A presents the single-difference using never-married mothers. Panel B presents the DD in which we use married mothers as an additional comparison group. Occupation definitions are in Appendix C.1. See Table 4 for information on control variables, and Table 1 for information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

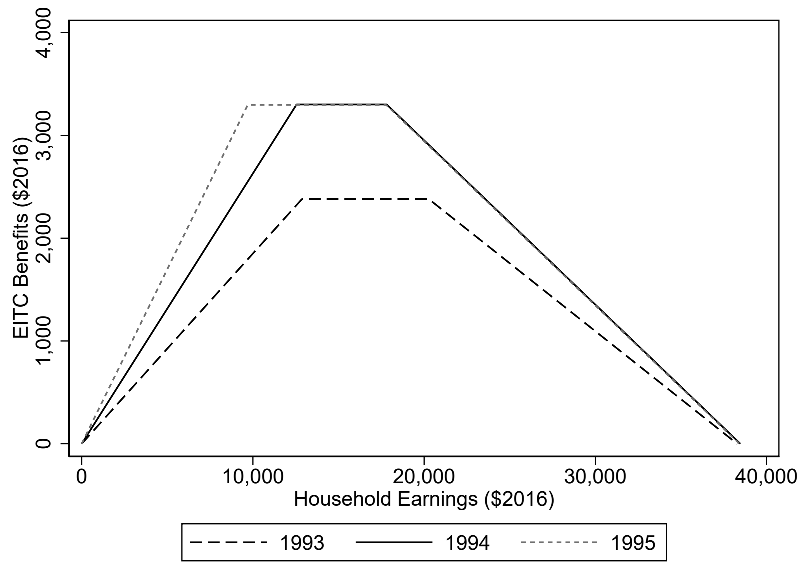
Table A.25: Effect of Early Work Incentives on Government Transfers – CPS Responses

	Welfare	Disability	SNAP	Medicaid	Hous Sub	Total
A: Never Married						
0-4 Yrs from Birth * EarlyExp	-740.874***	51.908	-402.114**	-273.883	3.214	-1298.326***
	(254.608)	(69.386)	(192.637)	(186.802)	(11.235)	(480.694)
5-9 Yrs from Birth * EarlyExp	-846.759***	-23.961	-780.109***	-49.528	-19.218	-1551.727***
	(148.823)	(57.493)	(157.032)	(194.046)	(12.810)	(358.077)
10+ Yrs from Birth * EarlyExp	17.816	23.956	-231.168	53.832	-14.539*	-31.829
	(116.330)	(67.529)	(139.410)	(178.773)	(8.383)	(325.256)
Mean Y	864.630	79.039	1309.217	1334.773	60.216	3628.724
Observations	10020	10020	9438	8228	9193	8228
B: Add Married Comparison						
0-4 Yrs from Birth * EarlyExp * NM	-724.784***	120.044	-343.550*	-85.638	4.018	-936.082*
	(243.054)	(104.412)	(181.961)	(168.744)	(11.141)	(475.686)
5-9 Yrs from Birth * EarlyExp * NM	-823.997***	-54.906	-710.442***	-134.911	-18.236	-1599.150***
	(158.484)	(73.306)	(153.548)	(200.210)	(12.726)	(356.497)
10+ Yrs from Birth * EarlyExp * NM	-8.960	-7.312	-237.127*	81.541	-14.640*	-61.332
	(110.452)	(77.426)	(130.537)	(195.118)	(8.355)	(302.951)
Mean Y	138.584	136.681	281.872	866.221	8.672	1405.240
Observations	98077	98077	91689	80508	89921	80508

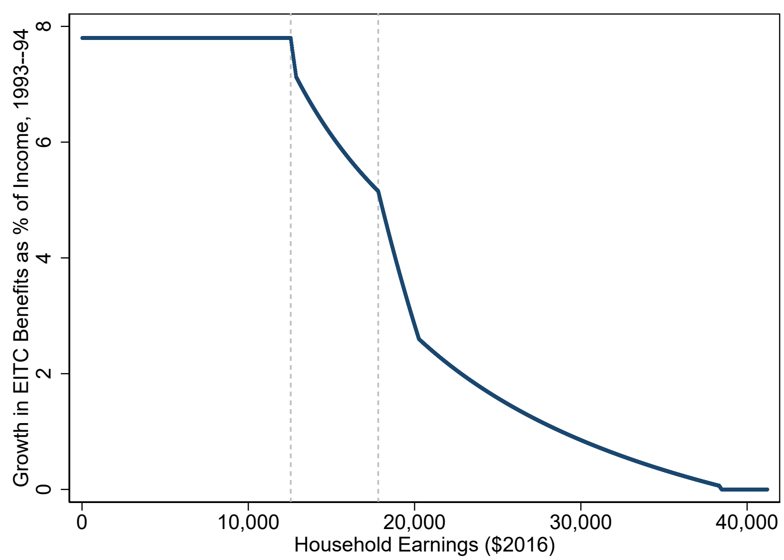
Notes: This table shows the results from regressions comparing the amount of cash and in-kind transfers from each government program (shown in the headers) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9, and 10+ years from a first birth. Panel A presents the single-difference using never-married mothers. Panel B presents the DD in which we use married mothers as an additional comparison group. See Table 4 for information on control variables, and Table 1 for information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

Figure A.1: EITC Schedule for Households with One Child

(a) 1993, 1994 and 1995 Benefit Levels

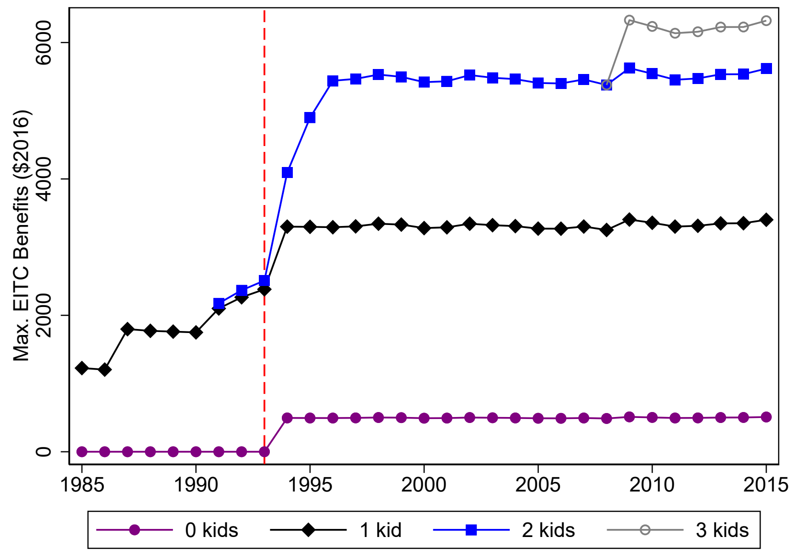


(b) 1993-1994 Change in Benefits (as % of Earnings)



Notes: Panel A shows EITC benefits ($2016) at each level of earnings for households with one child in 1993, 1994, and 1995. Panel B shows the difference between 1994 and 1993 benefits as a share of household income. *Data:* Nominal EITC benefits are obtained from the Tax Policy Center (https://www.taxpolicycenter.org/statistics/eitc-parameters), and have been converted to 2016 dollars using the CPI from the Bureau of Labor Statistics.

Figure A.2: Maximum EITC Benefits by Number of Children

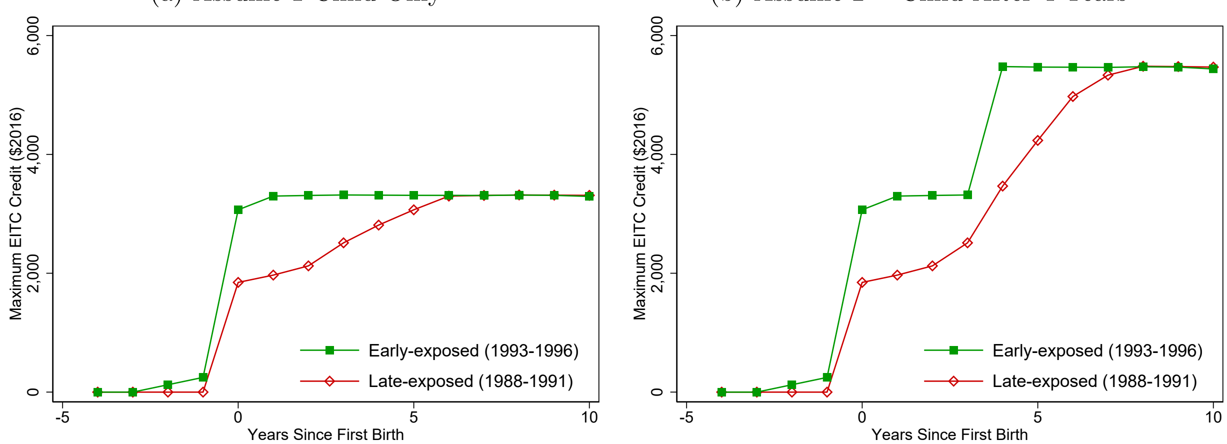


Notes: This figure shows the maximum EITC benefits ($2016) in each year and by number of qualifying children. *Data:* Nominal EITC benefits are obtained from the Tax Policy Center (https://www.taxpolicycenter.org/statistics/eitc-parameters), and have been converted to 2016 dollars using the CPI from the Bureau of Labor Statistics.

Figure A.3: Maximum EITC Credit by Age of First Child and Year of First Birth

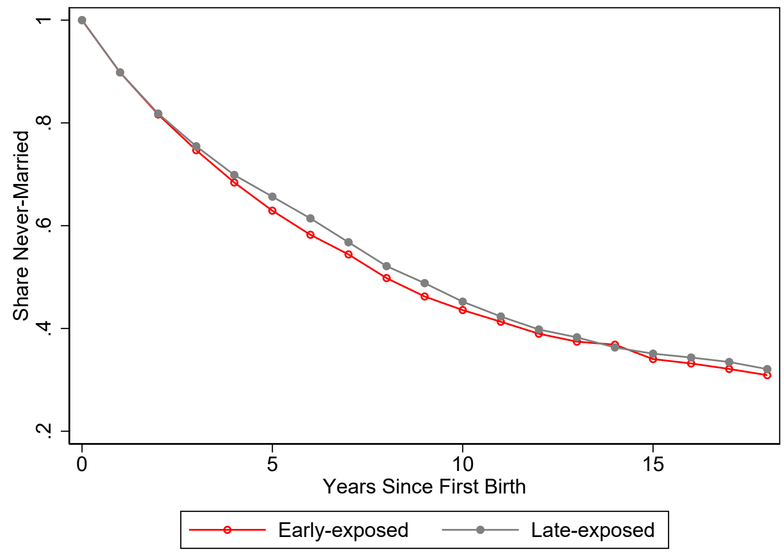
(a) Assume 1 Child Only

(b) Assume 2^{nd} Child After 4 Years



Notes: Panel (a) presents the average maximum EITC benefits in each year since first birth for mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), assuming they have at most 1 child. Panel (b) shows the same outcomes, but assuming all mothers have a second child 4 years after a first birth. The spacing between children only affects where the second bump in Panel (b) occurs, not the maximal difference or the year of convergence. *Data:* Nominal EITC benefits are obtained from the Tax Policy Center (https://www.taxpolicycenter.org/statistics/eitc-parameters), and have been converted to 2016 dollars using the CPI from the Bureau of Labor Statistics.

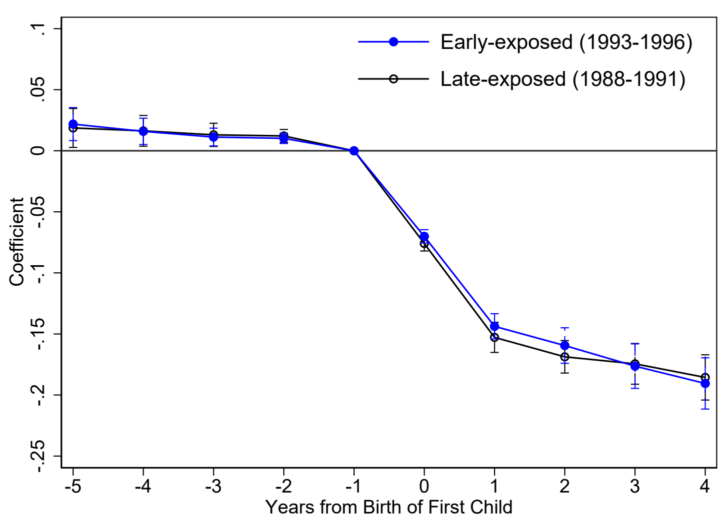
Figure A.4: Share of Mothers Remaining Never-Married in Each Year Since First Birth (SIPP)



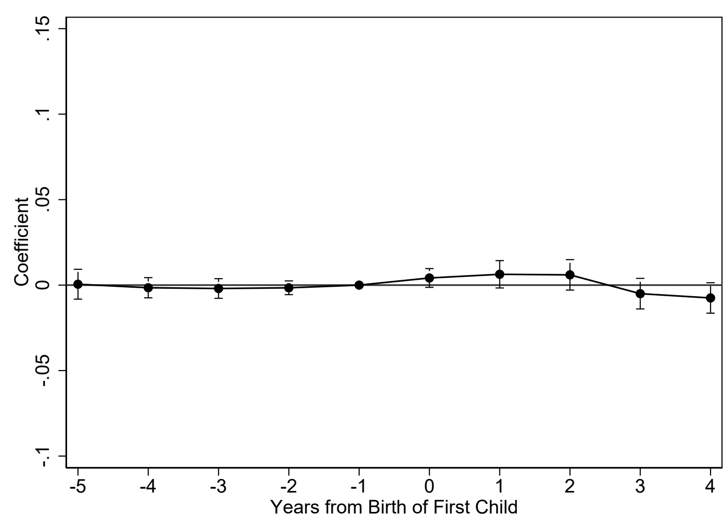
Notes: This figure presents the share of mothers who were never-married at first birth that remain never-married in each year since first birth. We plot this separately for mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991). *Data:* 1990, 1993, 1996, 2001, 2004, 2008 SIPP Wave 2 Topical Modules and 2014 SIPP. Sample: women whose first child was born in 1988–1991 or 1993–1996, and who were never married at the time of this first birth. Estimates weighted by SIPP weights.

Figure A.5: Effect of Early Work Incentives on Short-Run Employment – Married Mothers

(a) Early- and Late-Exposed

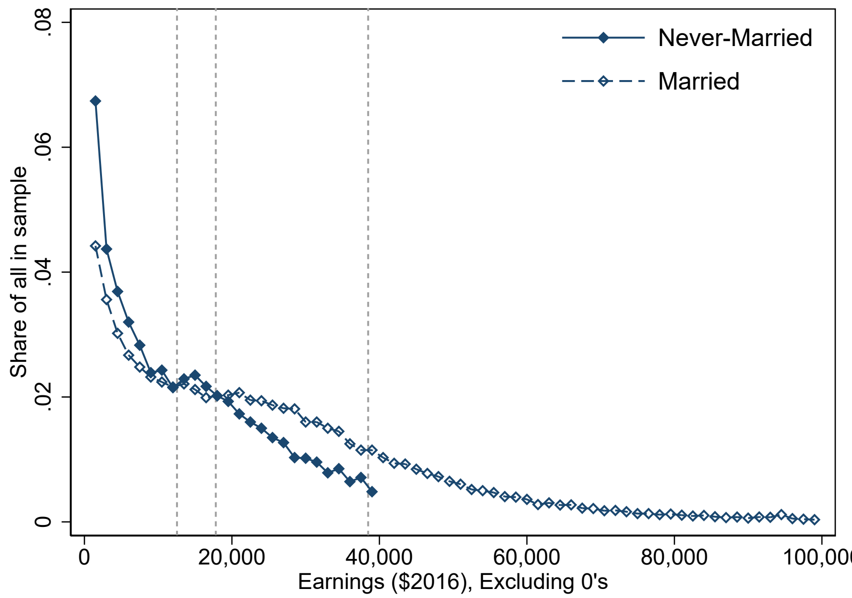


(b) Difference-in-Difference



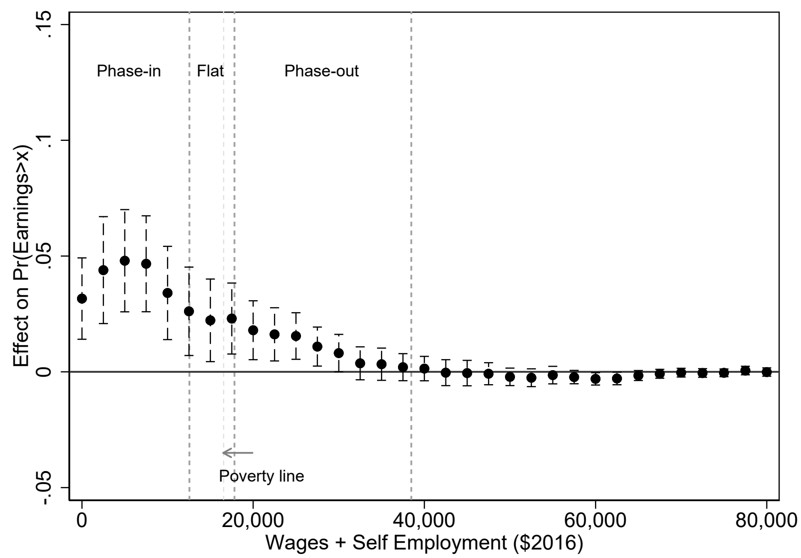
Notes: These figures present point estimates and 95% confidence intervals from event studies of employment around birth for married mothers who were exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). Panel A plots the estimates on indicators for years since first birth crossed with being "early-exposed" or "late-exposed" using never-married mothers. Panel B shows the estimates for the dynamic DD using married mothers. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Figure A.6: Distribution of Post-birth Earnings, Excluding 0's – Late-Exposed Mothers



Notes: This figure shows the truncated distribution of earnings, excluding 0's, zero to three years after a first birth for never-married and married mothers who were exposed to the 1993 EITC reform late (first birth: 1988–1991). We omit the never-married distribution beyond $40,000 due to to having too few observations (less than 20 observations per bin). See the notes of Figure 2 for information on data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 4^{th} year after a first birth.

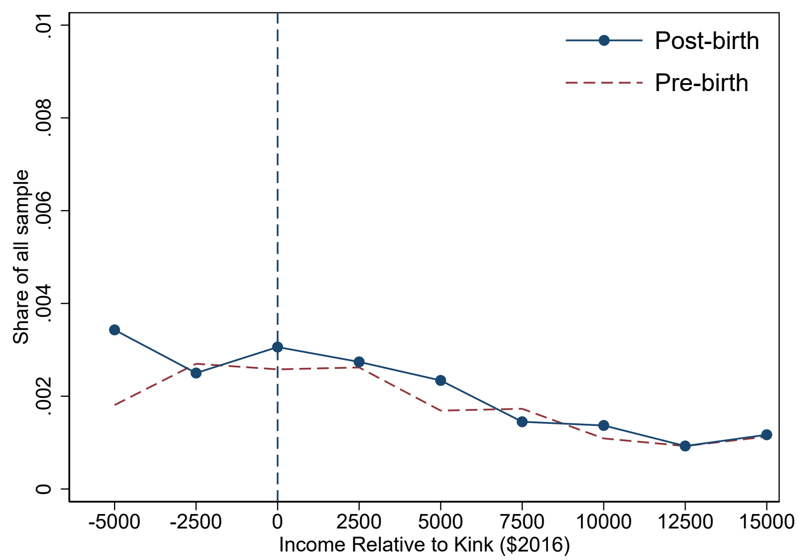
Figure A.7: Effect of Early Work Incentives on Earnings Density – Difference-in-Difference Estimates



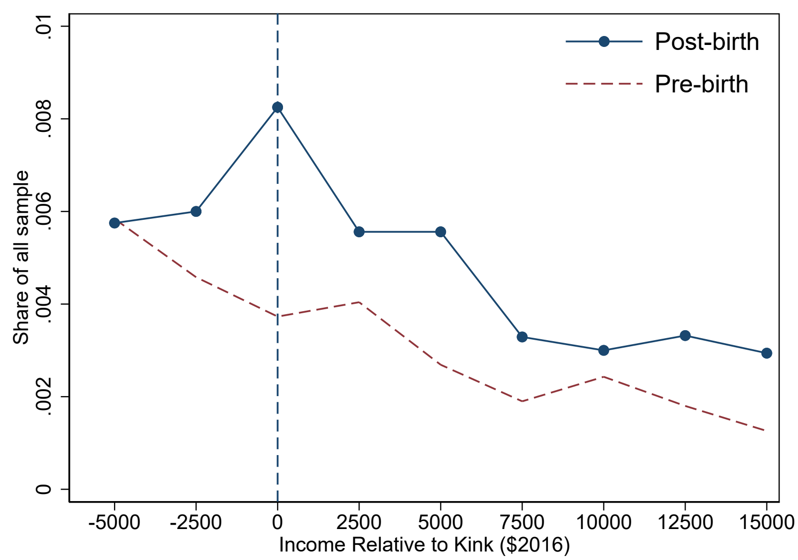
Notes: This figure shows coefficients and 95% confidence intervals from difference-in-difference regressions that compare the earnings distribution of never-married mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991). Each marker is obtained from a separate regression, where the outcome is an indicator for having annual earnings ($2016) at least as large as X – where X is the amount shown on the x-axis – during years 0-3 since birth. The dashed grey lines show, respectively, the end of the phase-in region on the 1994 EITC schedule; the 1994 poverty line; the end of the flat region on the 1994 EITC schedule; and the end of the phase-out region on the 1994 EITC schedule. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. Nominal EITC benefits are obtained from the Tax Policy Center (`https://www.taxpolicycenter.org/statistics/eitc-parameters`). *Years:* We include data from 5 years prior to a first birth up to the 4^{th} year after a first birth.

Figure A.8: EITC Expansion and Bunching Before and After Birth – Never-Married Mothers

(a) Late-Exposed Self-Employed Mothers

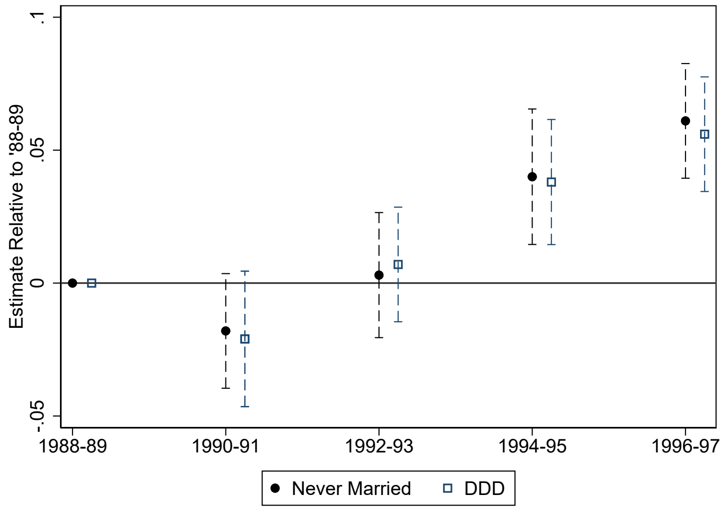


(b) Early-Exposed Self-Employed Mothers



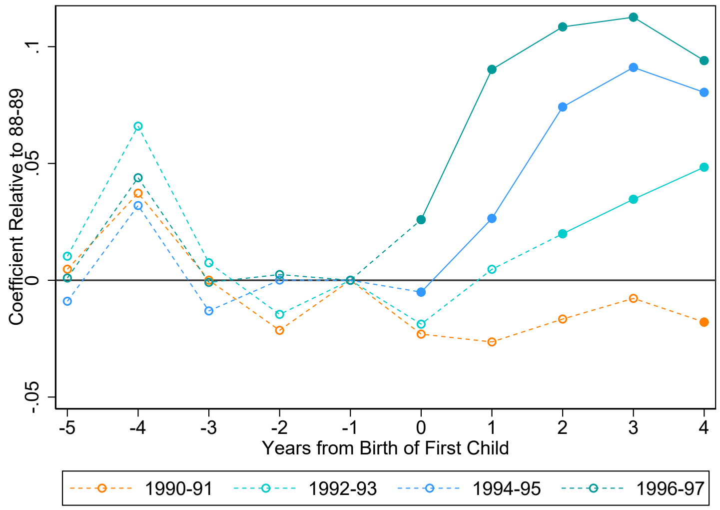
Notes: These figures shows the share of all never-married mothers who are self-employed and have income in $2,500 ($2016) bins centered around the first EITC kink, pre- and post-birth. Panel A shows no post-birth bunching for for mothers exposed to the 1993 EITC reform late (first birth: 1988–1991). Panel B shows post-birth bunching for for mothers exposed to the 1993 EITC reform early (first birth: 1993–1996). "Pre-Birth" includes the 5 years prior to a first birth, and "post-birth" includes up to the fifth year after a first birth. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Figure A.9: Effect of Early Work Incentives on Short-Run Employment –
By Year of First Birth



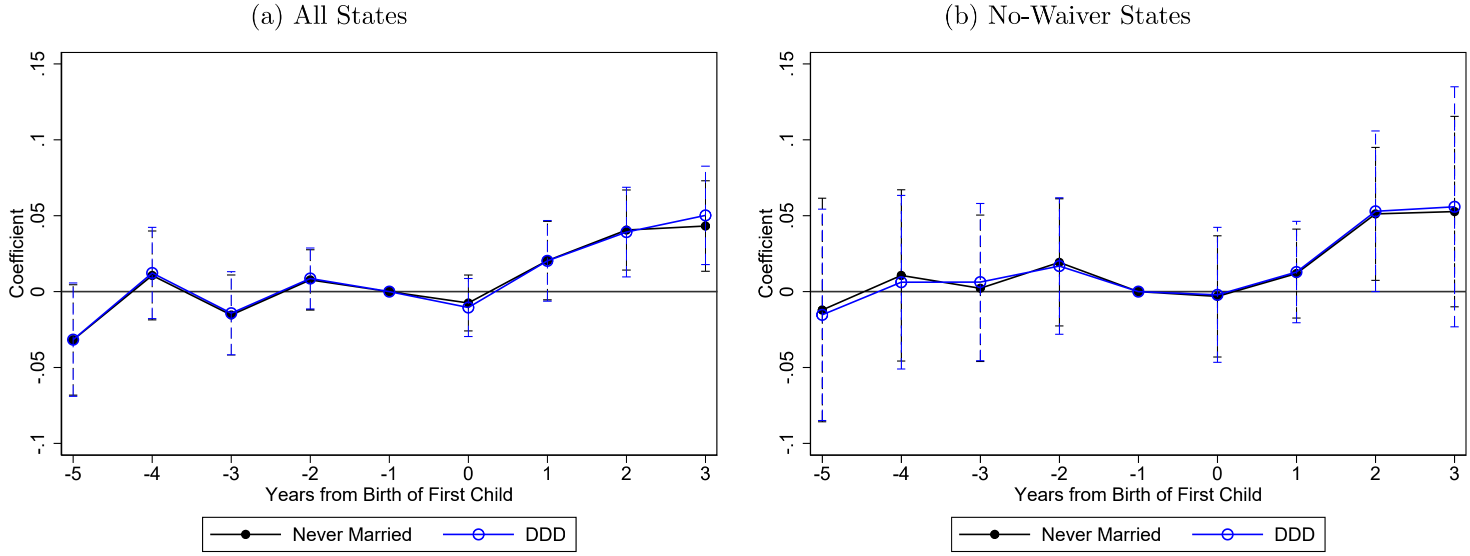
Notes: These figures show coefficients and 95% confidence intervals from regressions of employment on an indicator for "Post-Birth" interacted with indicators for having a first birth in 1990–91, 1992–93, 1994–95, or 1996–97. The omitted category (reference group) is first births in 1988-89. We present both the DD using never-married mothers as well as the DDD in which we use married mothers as an additional comparison group. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Figure A.10: Effect of Early Work Incentives on Short-Run Employment – By Year of First Birth and Years Since Birth



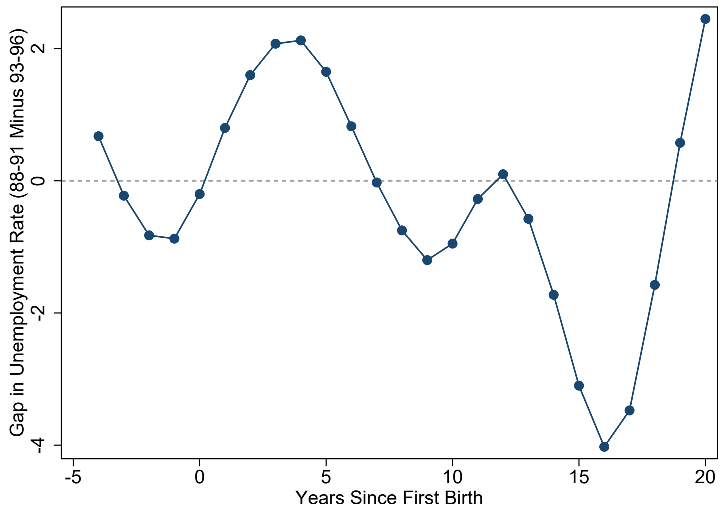
Notes: These figures show coefficients and 95% confidence intervals from event studies of employment in each year around birth by year of first birth. We plot the estimates on indicators for years since first birth interacted with indicators for having a first birth in 1990–91, 1992–93, 1994–95, or 1996–97. The omitted category (reference group) is first births in 1988-89. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Figure A.11: Effect of Early Work Incentives on Short-Run Employment – Prior to Federal Welfare Reform



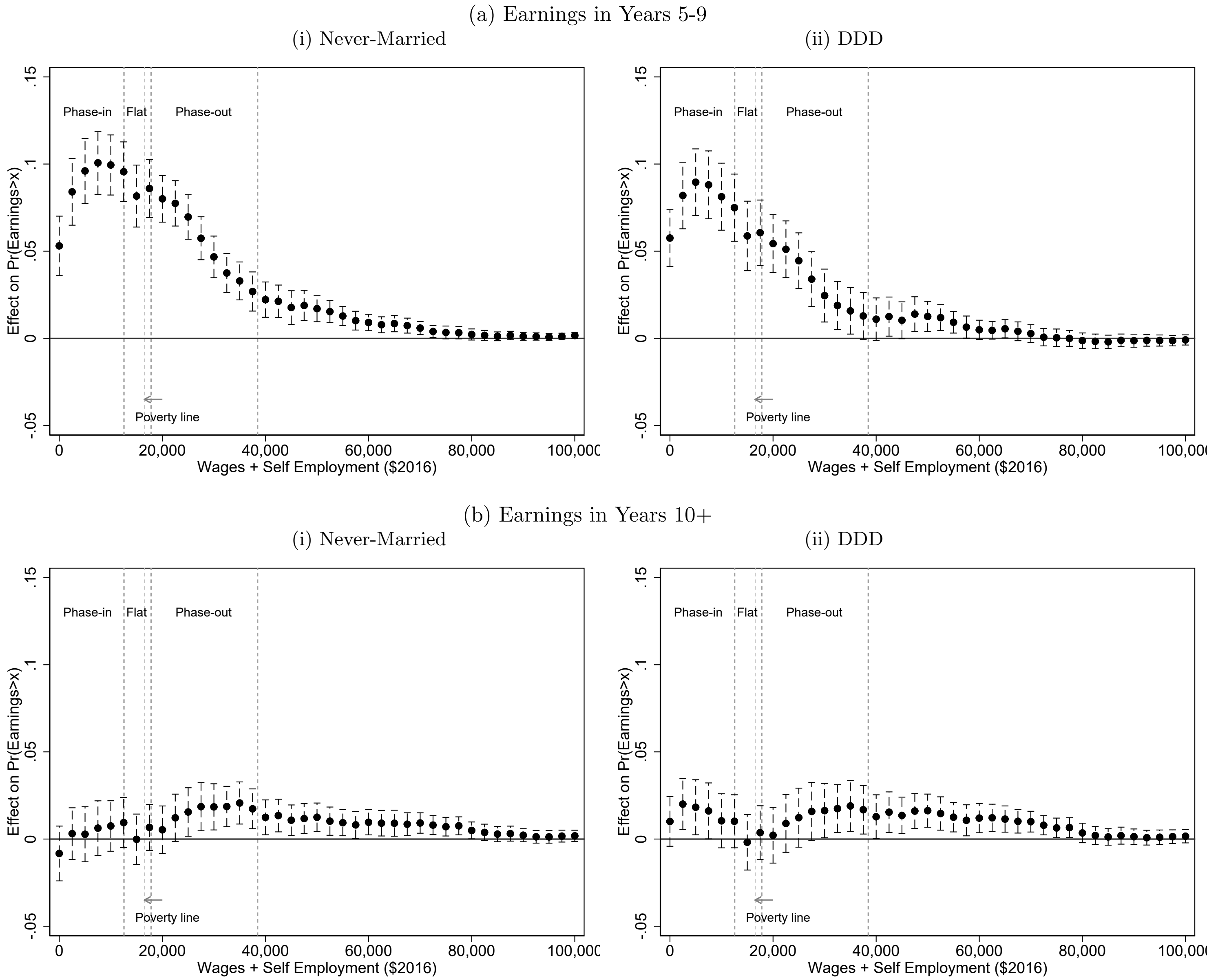
Notes: These figures present estimates and 95% confidence intervals from event studies of employment up to five years after a first birth, for mothers who were exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). For each figure we shows the estimates for the dynamic DD using never-married mothers and the dynamic DDD, where we use married mothers as an additional comparison group. Panel A limits the data to the years up to 1996. Panel B additionally restricts the data to states that had not passed a welfare waiver by 1996. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth or 1996, whichever comes first.

Figure A.12: Difference in the Average Unemployment Rate Between Late- and Early-Exposed Mothers



Notes: This figure shows the difference between the average unemployment rate for mothers who were exposed to the 1993 EITC reform late (first birth: 1988–1991) or early (first birth: 1993–1996). Hence, a positive value implies that early-exposed mothers had better employment conditions than late-exposed mothers at a given number of years since first birth. *Data:* State-level unemployment rates from the Bureau of Labor Statistics.

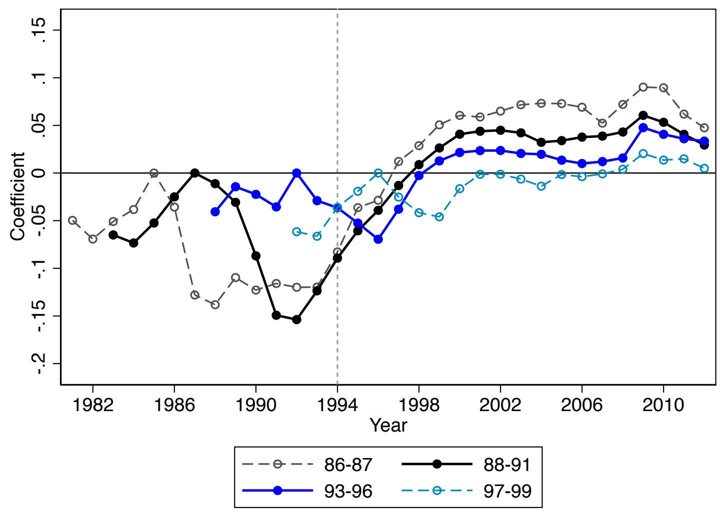
Figure A.13: Effect of Early Work Incentives on Earnings Density – Medium- and Long-Run



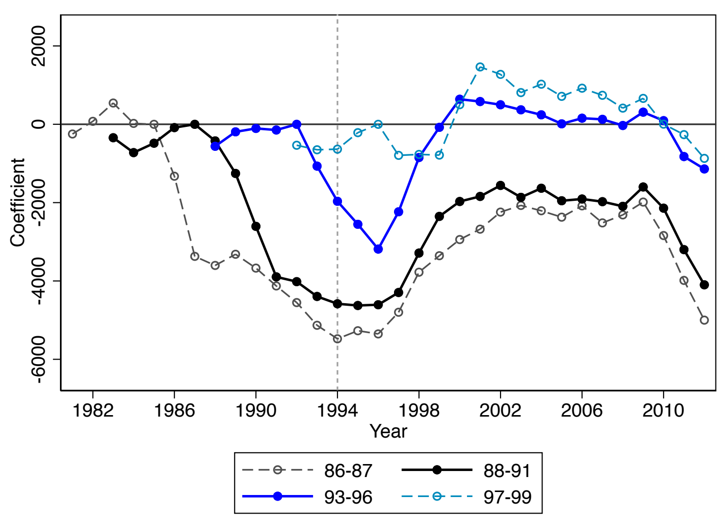
Notes: These figures show coefficients and 95% confidence intervals from DDD regressions that compare the earnings distribution of mothers with early exposure (first birth: 1993–1996) and late-exposure (first birth: 1988–1991) to the EITC reform, across never-married and married mothers. Each marker is obtained from a different regression, where the outcome is an indicator for having annual earnings ($2016) at least as large as X – where X is the amount shown on the x-axis – during years 5-9 (Panel A) or 10+ (Panel B) since birth. The dashed grey lines show, respectively, the end of the phase-in region on the 1994 EITC schedule; the 1994 poverty line; the end of the flat region on the 1994 EITC schedule; and the end of the phase-out region on the 1994 EITC schedule. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. Nominal EITC benefits are obtained from the Tax Policy Center (`https://www.taxpolicycenter.org/statistics/eitc-parameters`). *Years:* We include data from 5 years prior to a first birth up to the 4^{th} year after a first birth.

Figure A.14: Effect of Early Work Incentives on Labor Market Outcomes – Never-Married Mothers, By Year of First Birth and Calendar Year

(a) Employment

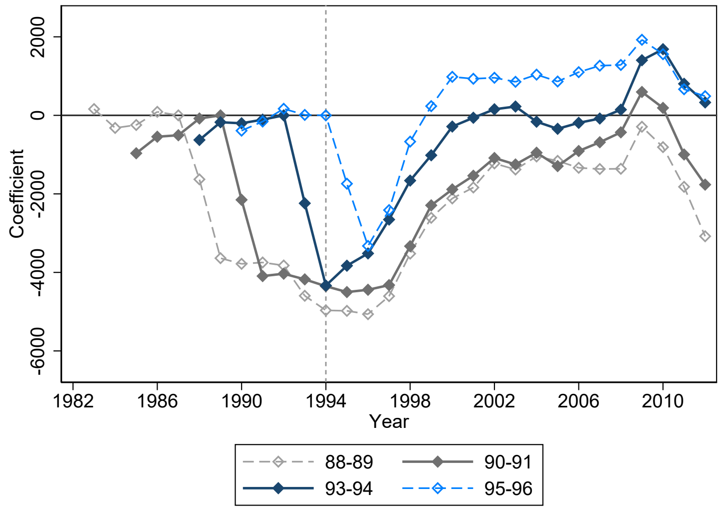


(b) Earnings



Notes: These figures show coefficients and 95% confidence intervals from calendar-year event studies of the employment (Panel A) or earnings ($2016, Panel B) of never-married mothers. We show the estimates on indicators for calendar years interacted with an indicator for being exposed to the 1993 EITC reform early (first birth: 1993–1996), late (first birth: 1988–1991), very late (first birth: 1986–1987) or very early (first birth: 1997–1999). For each group of mothers, the omitted category (reference group) is the year prior to the earliest birth (e.g. 1992, for 1993–1996 births). All regressions include fixed effects for the year of first childbirth, mother's age, race, education, state of residence, the state-level unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, implementation of six types of welfare waivers, implementation of any waiver or TANF, and implementation of the 2009 EITC reform. See the notes of Figure 2 for information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to 2012.

Figure A.15: Zooming in on Earnings of Early- and Late-Exposed Never-Married Mothers – Groups of Two Cohorts, By Calendar Year

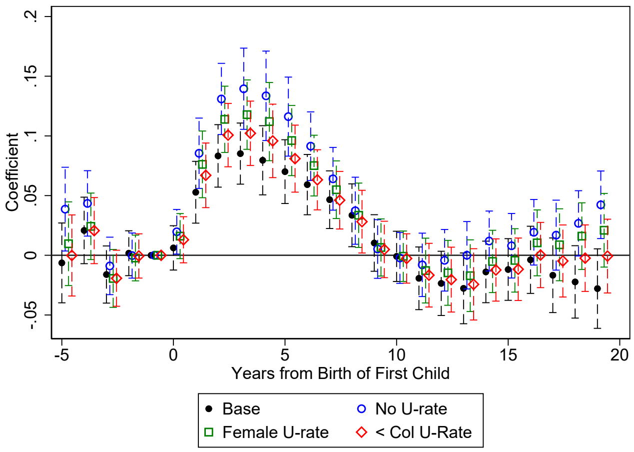


Notes: This figure shows coefficients and 95% confidence intervals from calendar-year event studies of the earnings ($2016) of never-married mothers. We show the estimates on indicators for calendar years interacted with an indicator for being exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). or each group of mothers, the omitted category (reference group) is the year prior to the earliest birth (e.g. 1992, for 1993–1996 births). See the notes of Figure 2 for information on standard errors, data and sample construction, and of Appendix Figure A.14 for control variables. *Years:* We include data from 5 years prior to a first birth up to 2012.

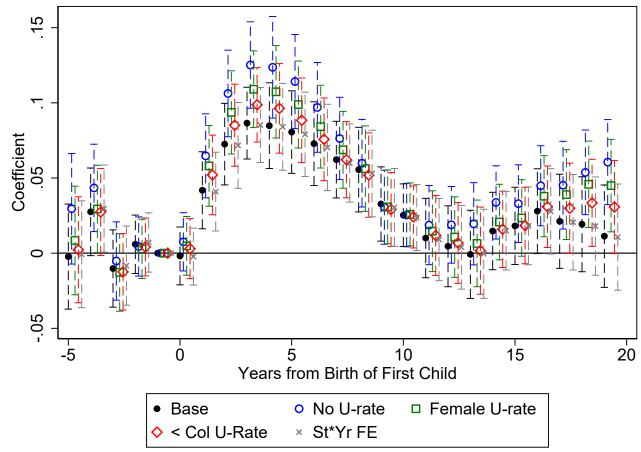
Figure A.16: Effect of Early Work Incentives on Labor Market Outcomes –
Sensitivity to Alternative Unemployment Rate Measures and State-Year Fixed Effects

(a) Employment

(i) Never-Married

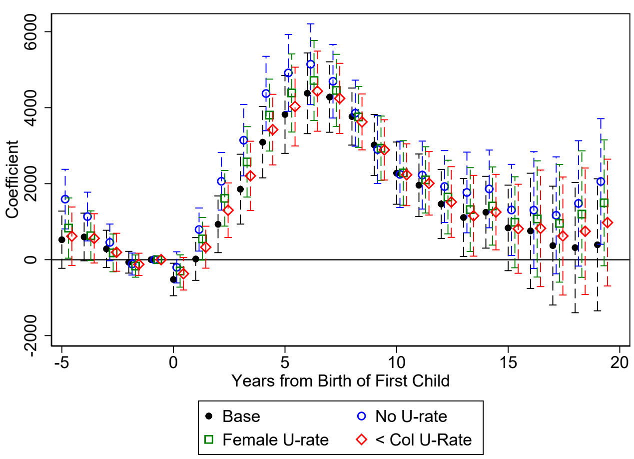


(ii) DDD

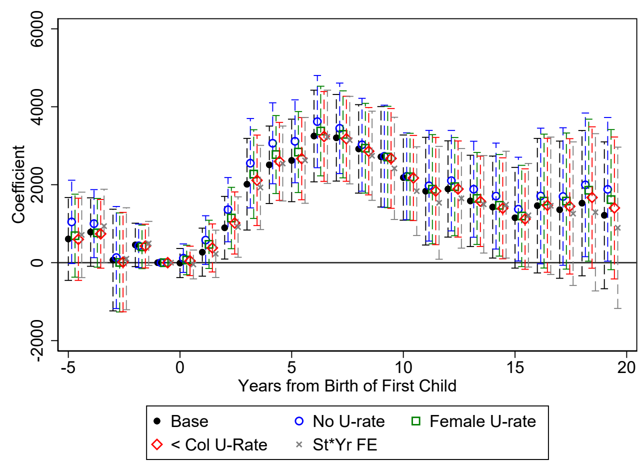


(b) Earnings

(i) Never-Married



(ii) DDD

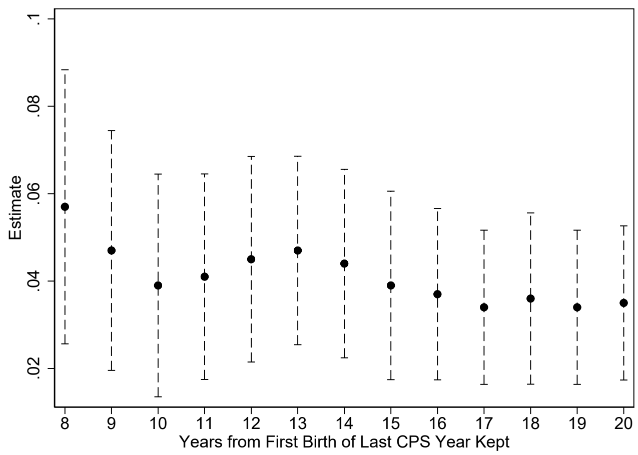


Notes: These figures present the sensitivity of the coefficients and 95% confidence intervals for event studies that compare the employment (Panel A) or earnings ($2016, Panel B) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), in each year from a first birth. Figure (i) in each panel presents the dynamic DD using never-married mothers. Figure (ii) in each panel presents the DDD in which we use married mothers as an additional comparison group. In addition to the baseline estimates, we show results from specifications where we remove all unemployment rate controls (blue circles); substitute the state-level unemployment rate with a control for the average unemployment rate for women in the state (green squares) or with a control for the average unemployment rate in the state for individuals with less than a college education (red diamonds). See the notes of Figure 2 for information on baseline control variables, standard errors, data and sample construction. We calculate the unemployment rate for women and for individuals with less than college education from the 1983–2015 March CPS. *Years:* We include data from 5 years prior to a first birth up to the 20^{th} year after a first birth.

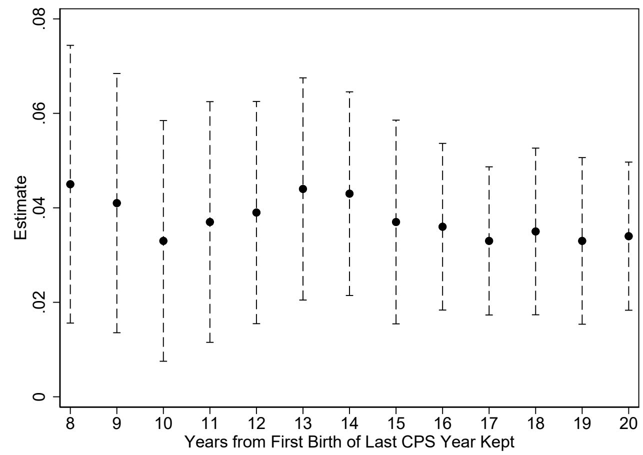
Figure A.17: Effect of Early Work Incentives on Labor Market Outcomes – Sensitivity to Keeping CPS Surveys *at most* from 8 to 20 Years of Birth

(a) Short-term Employment

(i) Never-Married

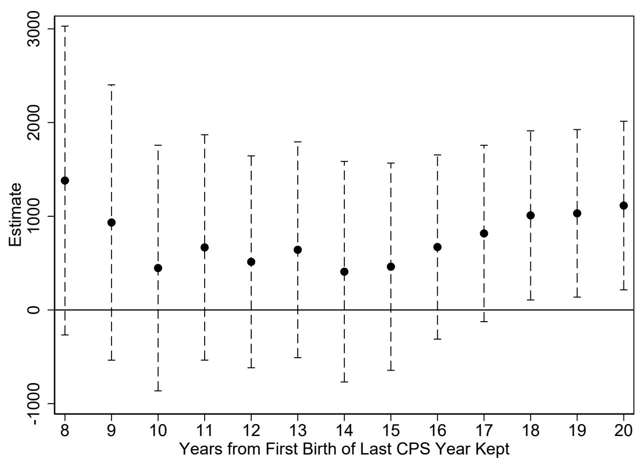


(ii) DDD

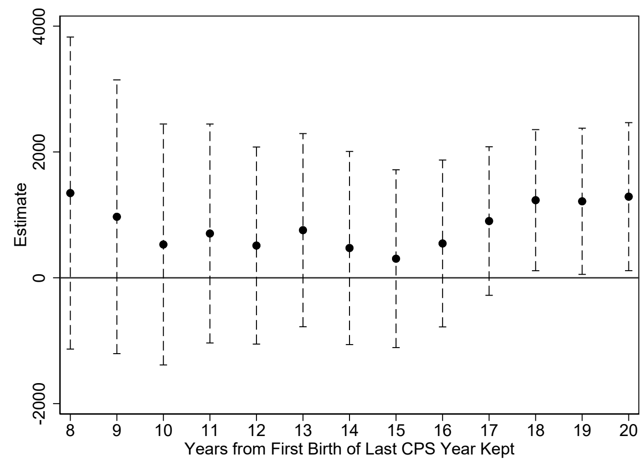


(b) Long-run Earnings

(i) Never-Married



(ii) DDD

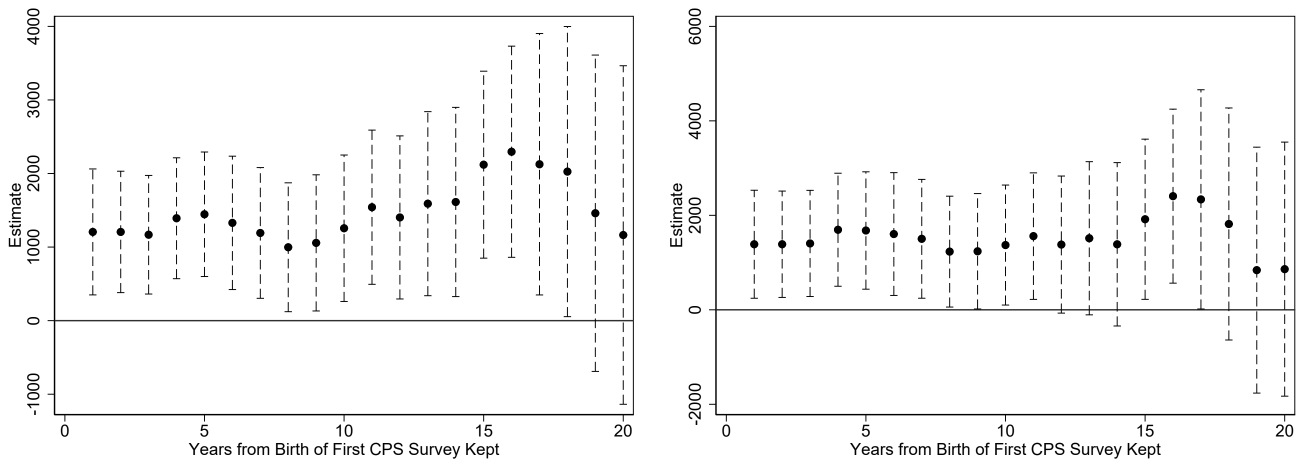


Notes: These figures present estimates and 95% confidence intervals from regressions comparing the labor market outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), as we vary the sample restrictions. Each marker comes from a separate regression where we keep CPS surveys that occurred at most 8, 9, ...20 years from first birth. Panel A shows the results for employment 0-4 years from first birth. Panel B shows the results for earnings ($2016) 5-9 and 10+ years from first birth. Figure (i) in each panel presents the DD using never-married mothers. Figure (ii) in each panel presents the DDD in which we use married mothers as an additional comparison group. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction.

Figure A.18: Effect of Early Work Incentives on Labor Market Outcomes – Sensitivity to Keeping CPS Surveys *at least* 1 to 20 Years after Birth

Long-Run Earnings ($2016)

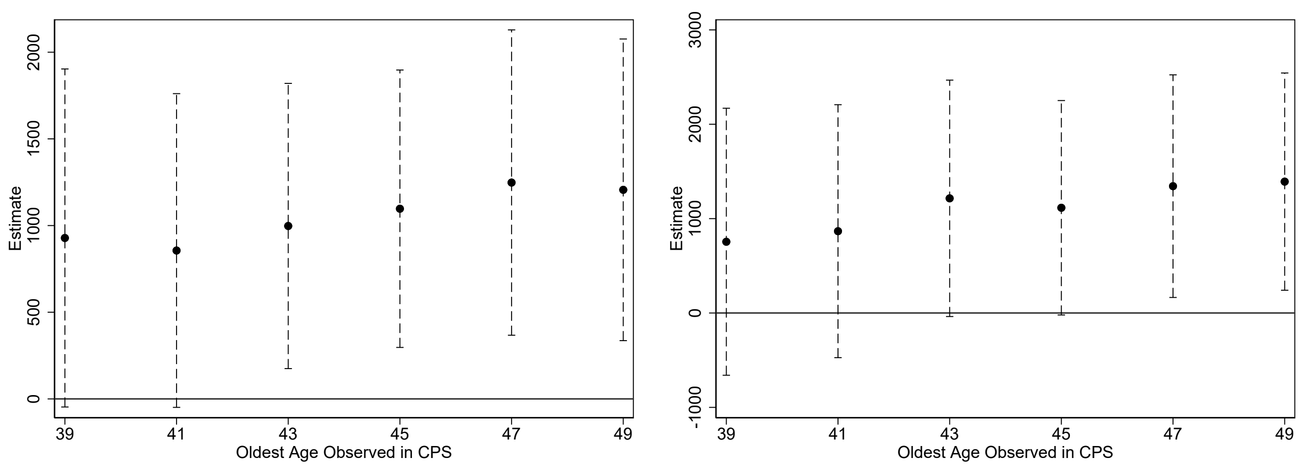
(i) Never-Married (ii) DDD



Notes: These figures shows estimates and 95% confidence intervals from regressions comparing the earnings ($2016) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from a first birth, as we vary the sample restrictions. Each marker comes from a separate regression where we keep CPS surveys that occurred at least 1, 2, ...20 years from first birth. Figure (i) presents the DD using never-married mothers. Figure (ii) presents the DDD in which we use married mothers as an additional comparison group. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction.

Figure A.19: Effect of Early Work Incentives on Long-Run Earnings – Sensitivity to Using Women Interviewed By Age 39 to 49

(i) Never-Married (ii) DDD

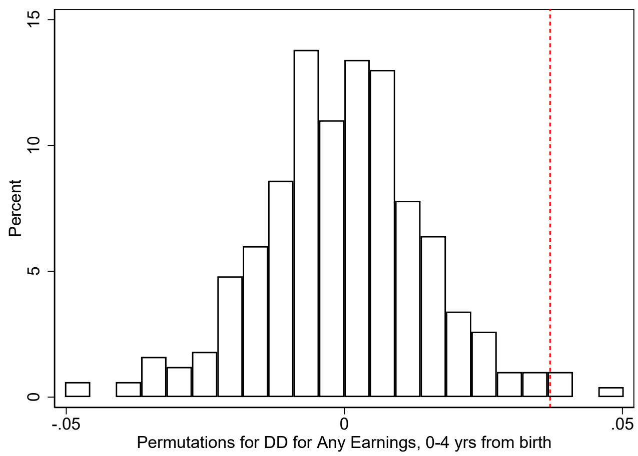


Notes: These figures shows estimates and 95% confidence intervals from regressions comparing the earnings ($2016) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from a first birth, as we vary the sample restrictions. Each marker comes from a separate regression where we restrict our sample to only keep women that were no older than 39, 41...49 when interviewed in the CPS. Figure (i) presents the DD using never-married mothers. Figure (ii) presents the DDD in which we use married mothers as an additional comparison group. See the notes of Figure 2 for information on control variables, standard errors, data and baseline sample construction.

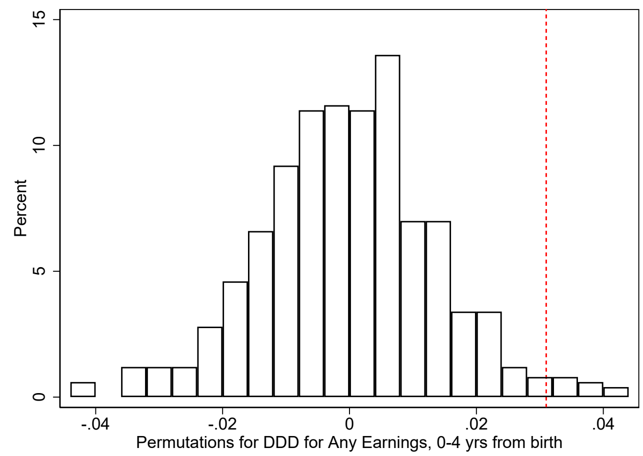
Figure A.20: Effect of Early Work Incentives on Labor Market Outcomes – Randomization Inference

(a) Short-term Employment

(i) Never-Married

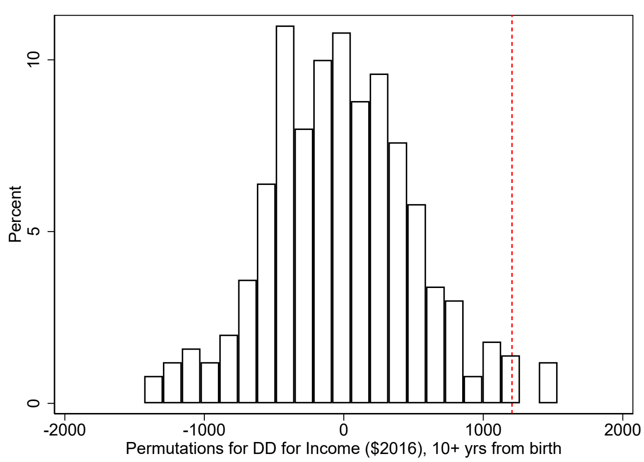


(ii) DDD

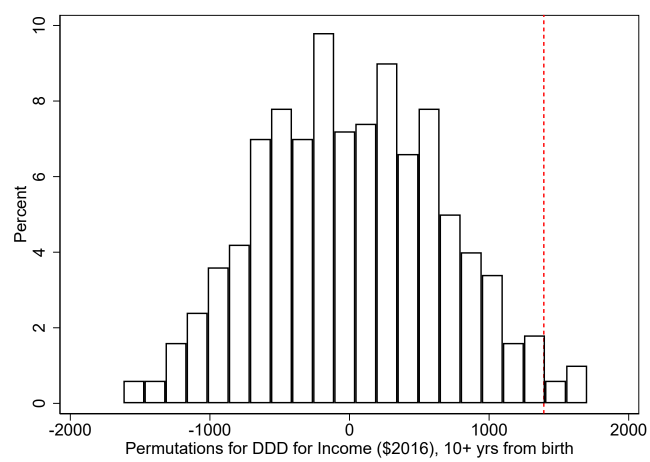


(b) Long-Term Earnings ($2016)

(i) Never-Married

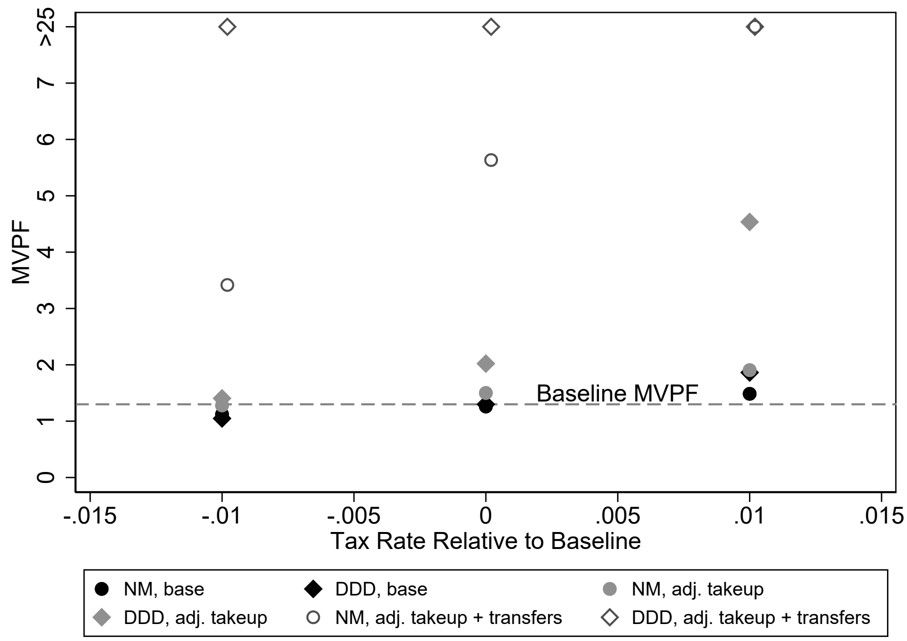


(ii) DDD



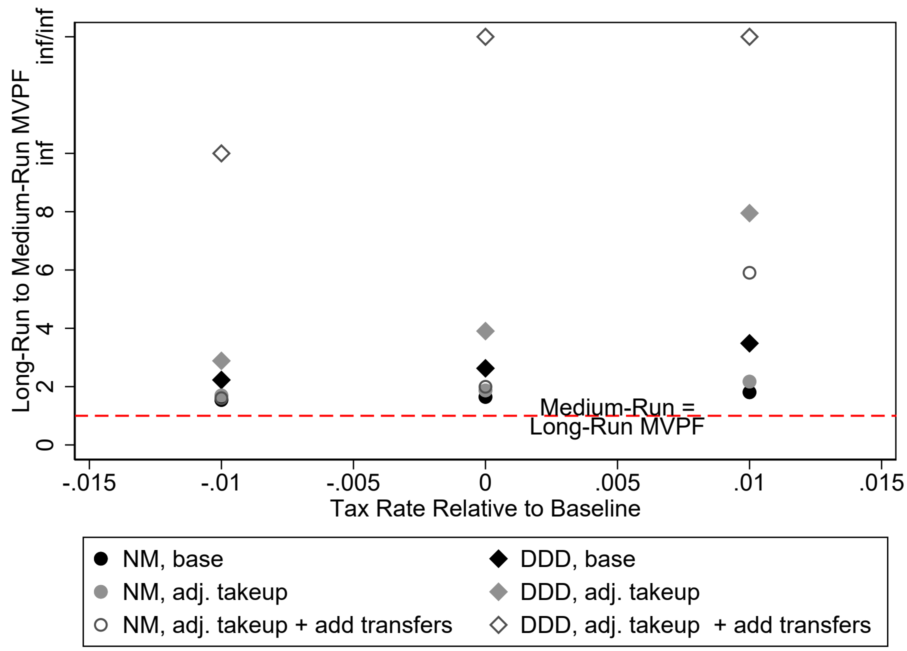
Notes: These figures show the distribution of estimates from 500 placebo experiments comparing the employment (Panel A) or earnings ($2016, Panel B) of mothers exposed to the EITC "reform" with "early" and "late", where early and late exposure are randomly assigned. In particular, for each placebo experiment we randomly assign "early-exposure" to four randomly chosen years of birth drawn without replacement, and estimate a placebo DD and DDD estimate. Panel A keeps data up to 5 years from first birth and reports the DD or DDD coefficient (figures (i) and (ii), respectively). Panel B keeps data up to 20 years from first birth and reports the DD or DDD coefficient on the interaction with "10+ Yrs From Birth" (figures (i) and (ii), respectively). The red dotted line shows our baseline estimate. The one-sided p-values for short-run employment are 0.01 for both the DD and DDD. The one-sided p-values for long-run earnings are 0.02 for both the DD and DDD. See the notes of Figure 2 for information on control variables, standard errors, data and baseline sample construction.

Figure A.21: Long-Run MVPF Across Varying Assumptions



Notes: This figure shows the estimated MVPF of the EITC expansion for early-exposed never-married mothers under varying assumptions about the average income tax rate (shown on the x-axis) and about EITC take-up and fiscal externalities (shown in different markers). The MVPF estimates shown in the "base" markers are calculated as $\frac{WTP}{Cost - \text{Add'l Taxes}}$. The estimates shown in the "adj. takeup" markers multiply WTP and cost by 0.85 to account for incomplete EITC takeup. The estimates shown in the "adj. takeup + transfers" markers apply this rescaling and also subtract our conservative change in transfers (excluding welfare and Medicaid) from the denominator of the MVPF. The tax rate relative to baseline applies to the tax rates that we use for the short-run, medium-run, and long run. In other words, we add (or subtract) 0.01 to the tax rate in each period, or set the tax rate equal to zero if subtracting makes the tax rate less than 0. The grey dotted line shows the MVPF corresponding to our baseline tax rate and assumptions.

Figure A.22: Ratio of Long-Run to Medium-Run MVPF Across Varying Assumptions



Notes: This figure shows the ratio of the "long-run" MVPF to the "medium-run" MVPF (i.e., excluding impacts 10+ years from first birth) under varying assumptions about the average income tax rate (shown on the x-axis) and about EITC take-up and fiscal externalities (shown in different markers). The MVPF estimates shown in the "base" markers are calculated as $\frac{WTP}{Cost - \text{Add'l Taxes}}$. The estimates shown in the "adj. takeup" markers multiply WTP and cost by 0.85 to account for incomplete EITC takeup. The estimates shown in the "adj. takeup + transfers" markers apply this rescaling and also subtract our conservative change in transfers (excluding welfare and Medicaid) from the denominator of the MVPF. The tax rate relative to baseline applies to the tax rates that we use for the short-run, medium-run, and long run. In other words, we add (or subtract) 0.01 to the tax rate in each period, or set the tax rate equal to zero if subtracting makes the tax rate less than 0. The red dotted line shows where the long-run and medium-run MVPFs are equal (i.e., the ratio is 1). Values above this line indicate that the long-run MVPF is greater than the medium-run MVPF

B Relationship to Kleven (2019)

As we note in the paper, the close timing of welfare reform with the 1993 EITC reform (hereafter, the reform) raises a number of potential challenges for our estimation. Kleven (2019) highlights a number of specific concerns in this vein. In this section, we outline the key points in Kleven's analysis of the reform and how our results address or differ from Kleven's findings.[51]

Brief summary of Kleven (2019) Kleven (2019) analyzes the effect of the reform using the 1989 to 2003 March and monthly CPS files, and a sample consisting of single women (never-married, divorced, widowed) between the ages of 20 and 50. His main analysis is a difference-in-difference design comparing women with kids to women without kids, before and after the reform. He presents three main results. First, he shows that the post-reform increase in employment was increasing in family size and decreasing in the age of one's youngest child. Second, he calculates very large implied elasticities of employment (participation), e.g. equal to 2.03 (1.79) for mothers with one child. Third, he shows that introducing dynamic controls for six types of welfare waivers (i.e., allowing the coefficients on these variables to vary by year and by number of children), and allowing the unemployment controls to vary by the presence of children, makes the EITC effect insignificant for the years prior to PRWORA. Kleven concludes from these results that the patterns are consistent with welfare reform, but not with the EITC narrative.

1. Impacts by number and age of children Different than Kleven, we do not find strictly increasing employment effects by family size or decreasing effects by child age. In particular, while we find that post-birth employment increases more after a second birth than after a first birth; we do not find a statistically significant difference between third or higher-order births and second births (see Section 4.3). These patterns are consistent with EITC incentives. Moreover, we do not find different employment effects between mothers whose first child at the time of the reform was no older than 1 ("early-exposed"), between the ages of 3-6 ("late-exposed), or between the ages of 7 and 8 (supplementary group) – see Appendix Figure A.14. One potential explanation for the difference in our results is that Kleven's analysis does not account for changes in *unobservable* characteristics of mothers over time, while our panel difference-in-difference strategy does. Consistent with this, Hotz and Scholz (2006) employ a panel family fixed effects strategy and find the same patterns by family size as we do.

2. Elasticity estimates Our back-of-the-envelope calculation in Section 4 suggests that the elasticity of employment to pre-tax labor earnings is between 0.54 and 0.72, or roughly 27% and 40% as large as the estimate for mothers with one child in Kleven (2019). The discrepancy between our estimates and Kleven's estimates reflect differences both in the numerator and the denominator of the elasticity. First, our employment effects in percent terms are half the size of Kleven's: 5.9

[51] Kleven also raises concerns with estimated effects of other EITC reforms – we do not address those here, since they are not relevant for our analysis.

percent ($\frac{3.7}{63.1}$) vs. 12.4 percent ($\frac{8.5}{68.1}$).[52] Second, Kleven calculates a 6.8% average change in tax rates. He obtains this by simulating taxes across years using observed earnings for working single mothers and predicted earnings for non-workers (based on individual characteristics). Instead, we calculate the change in EITC benefits between early- and late-exposed mothers using the post-birth distribution of late-exposed never-married mothers for workers, and imputing EITC benefits in three ways for non-workers. Specifically, we impute benefits assuming that non-workers earnings': (i) fall only in the phase-in region (ii) fall only in the phase-in or flat regions (weighted using the distribution of workers across these regions); or (iii) have the same distribution of earnings as working single mothers.[53] This produces changes in EITC benefits equal to an 8.2, 9.9, or 10.9 percent change as a share of pre-tax earnings, respectively. Our higher change in benefits primarily reflects our lower-income and younger population and the longer period over which we estimate changes in the EITC credit (e.g., we include the 1990 reform as part of our treatment). It also reflects the fact that, for some estimates, we allow the distribution of earnings for non-workers to be more concentrated in the phase-in regions post-reform. Thus, we do not make the strong assumption that the earnings distribution remains the same post-reform, as Kleven does.

3. Controlling for waivers and business cycle In a similar spirit to Kleven, we allow our unemployment rate and welfare waiver controls to be "dynamic" in allowing differential impacts by the age of one's first child.[54] Our estimates are not affected by allowing for this flexibility (see Section 4.3). We also show that our employment effects are present when we restrict our sample period up to 1996 and limiting our sample to states that did not pass any waivers prior to 1996 (e.g., Table 2, columns 7–8). Thus, these effects do not appear to be sensitive to these welfare controls, different than Kleven (2019). Further, we note that Kleven's effects inclusive of these controls are quite imprecise, and could not reject our estimated effects.[55]

[52] Again, we speculate that part of this difference is due to the fact that we control for pre-birth differences in labor market outcomes.

[53] The first two assumptions are motivated by the idea that non-workers are likely to be negatively selected on wages, or might be more likely to prefer part-time work.

[54] We do not model event-year dynamics for the welfare waivers as in some of the specification in Kleven (2019) because with six welfare waivers, passed largely in the 1990s, the dynamic waiver-event-time indicators quickly become collinear with our effects of interest. Nonetheless, given the strong relationship that Kleven shows between welfare response and child age, we would expect that these controls would account for important differences in incentives.

[55] For example, our effect inclusive of these controls is 3.2 pp. (column 5, Table 2), which is within the confidence interval of his 1.06 p.p. (s.e = 1.5 p.p.) in column 3 of Table 6.

C Appendix to Section 2

C.1 CPS occupations

Because the CPS occupation categories vary over time, we first create a harmonized occupation variable that spans our entire sample period using the IPUMS "occ1990" classification (Flood et al., 2020).[56] In particular, we downloaded the March CPS from IPUMS for the CPS surveys in our sample, and then collapsed the data by "occ1990" and the original CPS occupation variable to create a crosswalk. We then merge the crosswalk on to our data, which gives us the "occ1990" corresponding to each individual in our sample.

Next, we create categories of occupations based on similar types of jobs:

1. Housekeeping ($405 <= occ1990 <= 408$)
2. Janitor ($448 <= occ1990 <= 455$): includes janitors and building operators.
3. Food ($433 <= occ1990 <= 444$): includes bartenders, waiters, and kitchen workers.
4. Child ($occ1990 == 468$): includes child care workers.
5. Beauty ($456 <= occ1990 <= 458$): includes barbers and hairdressers
6. Recreation ($459 <= occ1990 <= 467$): includes guides and public transportation attendants.
7. Protect ($459 <= occ1990 <= 467$): includes firefighters, police, and guards.
8. Health Service ($445 <= occ1990 <= 447$): includes dental assistants and health aides.
9. Execs/Managers ($3 <= occ1990 <= 40$): includes legislators, managers, accountants, and management support.
10. Professional/Tech. ($43 <= occ1990 <= 240$): includes engineers, doctors, therapists, teachers, lawyers, and health technicians.
11. Financial sales ($243 <= occ1990 <= 260$): includes a variety of higher-end sales occupations (insurance, real estate, financial services).
12. Retail sales ($263 <= occ1990 <= 300$): includes salespersons, cashiers, and retail sales clerks.
13. Clerical ($303 <= occ1990 <= 389$): includes bank tellers, data entry, and admin support.
14. Agricultural ($473 <= occ1990 <= 499$): includes farmers, farm workers, and agricultural inspection.
15. Mech/Constr/Min ($503 <= occ1990 <= 617$): includes auto body repair, construction trades, and mining.

[56] See https://cps.ipums.org/cps-action/variables/OCC1990#codes_section for a description of these codes.

C.2 Matching CPS to Administrative Earnings Records

The match between CPS and SSA records is performed using the PIK, which is a unique mapping to a Social Security Number (SSN). Until 2006, PIKs were assigned using validated SSN's, if available, or a probabilistic match using name, address, and demographic information, such as date of birth. Since 2006, the PIK has been assigned solely using the probabilistic match, which prevents the need to request an SSN from respondents (Czajka et al., 2008). This match is only available for the 23 CPS surveys in our sample (1991, 1994, and 1996 to 2016). Conditional on an individual being matched to the SSA records, we observe W-2 and self-employment earnings in each year.

Below we show the share of married and never-married women that meet our sample criteria who are matched in each March CPS.

Table A.26: CPS-SSA Data Matching Rates –
By Year, Marital Status and EITC Exposure

	Never Married		Married	
	Late-Exposed	Early-Exposed	Late-Exposed	Early-Exposed
1991	0.819		0.845	
1994	0.789	0.750	0.786	0.768
1996	0.796	0.816	0.830	0.818
1997	0.731	0.812	0.786	0.777
1998	0.696	0.762	0.731	0.717
1999	0.683	0.661	0.681	0.682
2000	0.680	0.696	0.677	0.679
2001	0.222	0.264	0.216	0.223
2002	0.772	0.784	0.794	0.782
2003	0.758	0.788	0.778	0.763
2004	0.732	0.670	0.704	0.690
2005	0.730	0.675	0.691	0.668
2006	0.914	0.918	0.907	0.880
2007	0.918	0.874	0.907	0.882
2008	0.933	0.864	0.902	0.877
2009	0.857	0.883	0.898	0.881
2010	0.868	0.859	0.887	0.877
2011	0.874	0.893	0.892	0.889
2012	0.873	0.906	0.871	0.888
2013	0.887	0.891	0.873	0.890
2014	0.921	0.894	0.855	0.888
2015	0.900	0.864	0.881	0.867
2016	0.841	0.871	0.832	0.849
Total	0.762	0.776	0.768	0.780

Notes: This table shows the share of CPS women that we match to SSA records among mothers who were exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2015 longitudinal SSA earnings records. Sample: women whose first child was born in 1988–1991 or 1993–1996, who were at least 19 at first birth, and who were less than 50 years old and either married or never married at the time of the CPS interview.

Comparing CPS and administrative earnings To compare earnings in the CPS and SSA records, we use the "wage and salary" earnings reported in our linked CPS surveys and the sum of the W2 and self-employment earnings (for the year prior to the survey). We find several discrepancies across these sources. First, we find that 10% of the observations differ on whether an individual had any earnings. Over 60% of these errors are due to an individual reporting no earnings in the CPS, but having some earnings in the administrative data. Among individuals that have any earnings in both sources, there are substantial differences between the log of the administrative earnings and the log of the CPS earnings. The interquartile range for this measure ranges from -0.27 to 0.20, centered around 0, implying that discrepancies do not go in a consistent direction. Assuming that individuals can not earn less than what is reported in the administrative records, this suggests that at least half of the CPS earnings in our sample are reported with error.[57]

C.3 Survey of Income and Program Participation (SIPP)

All raw SIPP files were downloaded from
http://data.nber.org/data/survey-of-income-and-program-participation-sipp-data.html, and were imported using the posted dictionary files.

Child care costs To examine heterogeneous employment effects by child care costs in Section 4.3, we obtain state-level estimates of weekly child care costs using responses from topical modules in Wave 3 of the 1990, 1991, and 1993 SIPP panels, and Wave 6 of the 1992 SIPP panel. In each of these topical modules, households were asked to report the type of care and weekly cost of child care for each of the three youngest children under age 15. These data have been used to measure child care costs in many previous studies, such as Anderson and Levine (2000).

We construct three measures of household child care costs for children up to age three. These include the (unconditional) average cost of care, the cost of care conditional on having a positive cost, and the share of children for whom there was zero cost. In cases were child care costs were reported for multiple children, we assigned the average cost of care to each child. We then collapsed these three measures to the state-level using SIPP weights. We dropped households in states that are not individually identified in the SIPP.[58]

Marital histories To examine post-birth marriage rates in Section 3.1, we use the reported marital and fertility histories in Wave 2 of the 1990, 1993, 1996, 2001, 2004, and 2008 panels, and Wave 1 of the 2014 panel. Importantly, each of these surveys asks about year of first birth and year of first marriage, which allow us to identify mothers that were never-married at first birth. The survey also asks about subsequent marriages, which allows us to determine the share of mothers that remain never-married in each year after birth.

[57]See Abowd and Stinson (2013) for a discussion of possible sources of discrepancies between self-reported earnings and administrative records.

[58]These states include: Maine, Vermont, Iowa, North Dakota, South Dakota, Alaska, Idaho, Montana, and Wyoming.

D Appendix to Section 4

Elasticity calculation To translate our impacts on employment into an elasticity of employment to labor earnings, we need to scale the 5.9% change in employment by the percent change in average EITC benefits between early- and late-exposed mothers. We calculate this latter change, we using EITC benefit schedule for early- and late-exposed mothers weighted by the post-birth earnings distribution of late-exposed never-married mothers (see Appendix Figure A.6), and assign non-workers either (i) the change in benefits in the phase-in region; (ii) the average change in benefits in the phase-in and flat regions; or (iii) the average change in benefits among all workers, in a similar spirit to Kleven (2019). This produces a 10.9%, 9.9% and 8.2% change in average EITC benefits, respectively, and a range of elasticities between 0.54 ($\frac{5.9}{10.9}$) and 0.72 ($\frac{5.9}{8.2}$).

Heterogeneity by child care costs and state-level EITCs In these analyses, we use observed variation across states in child care costs or work incentives. Because these features of states are not randomly assigned, we view this evidence as only suggestive.

First, we examine whether impact of work incentives varies with childcare costs. The predicted sign of this interaction is ambiguous: additional income from the EITC could be particularly important for the employment of mothers with high costs of care, or could be most effective in areas where mothers have access to low-cost care. Using the SIPP, we construct three state-level measures of early child care costs for children: the average weekly cost of child care, the average weekly cost conditional on paying for care, and the share of children with zero care costs (e.g., using relatives for care).[59,60]

Appendix Table A.9 shows that early-exposure has a larger impact on employment in higher cost areas (columns 2–3), but also in areas with a higher share of children receiving free care (column 4). While the latter result may seem unintuitive, we suspect that this may reflect greater use of relatives for child care in areas with higher costs. In support of this, we find a positive correlation between all of our child care cost measures. Thus, these results are potentially consistent with a larger impact of work incentives in high-cost areas. However, we acknowledge that it is difficult to interpret our current measures of childcare costs.

Second, we consider whether the impacts of the EITC reform are larger in states that have a supplementary EITC.[61] Columns (1) and (3) of Appendix Table A.10 show that, on average, post-birth employment does not vary with the presence of a state EITC supplement (column 1) or with the generosity of the supplement (column 3). This may reflect the small number of EITC's during the early 1990s, or the lack of salience of these benefits. However, we find that early-exposed mothers' employment increases more in states that have an EITC supplement (column 2) or have

[59] We have slightly fewer observations for these analyses because the SIPP does not identify the state of residence for households in small states.

[60] See Appendix C.3 for further information on the SIPP data.

[61] We obtain information on state EITC supplements from `https://users.nber.org/~taxsim/state-eitc.html`. Supplementary EITC's are typically set as a percentage of the federal EITC; thus, a mother living in a state with a supplement is eligible for a more generous credit, and can expect a larger increase in her credit after a federal reform.

a more generous EITC supplement (column 4). This is consistent with early-exposed mothers' responding to the generosity of work incentives after the EITC reform.

E Appendix to Section 6

Role of growth in hours in medium-run results We now ask: how much of the growth in the earnings advantage from the short-run to the medium-run (7% to 17%) can be explained by the growth in weekly hours over that time? As a back-of-the-envelope estimate, we scale up our short-run impacts on earnings by the medium-run growth in labor supply to get a "predicted effect" on medium-run earnings. This yields that early-exposed mothers' earnings advantage would be expected to grow from 7% to 11.3% based on growth in any employment $[7\% \cdot (1 + \frac{(5.5-3.4)}{3.4})]$, and to 10.7% based on growth in weekly hours $[7\% \cdot (1+\frac{(3.3-2.16)}{2.16})]$. As a fraction of the 10% actual change, this implies that 37% to 43% of the earnings growth from the short-run to the medium-run can be explained by changes in hours of work. Hence, 63% of the medium-run earnings growth could be due to wage growth (e.g., due to higher wages associated with full-time work).

Impacts on "high earnings" and "high experience" We begin by providing further justification and detail about the variables that we use in this analysis. As discussed in the text, we measure "high earnings" using an indicator for being in the top 25% of the earnings distribution of all mothers, defined in each year since first birth. We use this measure because early exposure has a larger and more precise effect on being in the top 25% of earnings in the long-run than being in the top 75% or top 50% of the earnings distribution (see Panel A of Appendix Table A.20). Thus, we consider this to be the best proxy for the impacts of early exposure. As also discussed in the text, we measure "high experience" using an indicator for whether a mother worked in the first three years after her first birth. To construct this variable, we create a measure of "potential experience" which is equal to one's actual total experience for $\tau \leq 0$, increases by one in each year for $1 \leq \tau \leq 3$, and increases by 1 in each year that a mother works for $\tau > 4$. We then define a mother as having "high experience" if her actual experience is equal to her potential experience.

Next, we describe the calculation justifying our claim that the share of low-experience mothers with high earnings does not change with early exposure. To calculate the share of early-exposed mothers with "low-experience", we sum from Appendix Table A.21 the mean share of never-married women with low experience (0.021 + 0.310) and the impact of early exposure on being low experience (-.009 + 0.127). This gives 19.5%. We then calculate the share of early-exposed mothers with "high earnings and low experience" as the mean share of this group plus the effect of early exposure (0.02-0.009), which is 1.2%. The ratio of these two numbers gives that 6.2% of early-exposed mothers with low experience have high earnings. Among all never-married mothers, this ratio is 6.3% $(\frac{0.02}{0.02+0.31})$ based on the means in Appendix Table A.21. Thus, this ratio is not higher for early-exposed mothers.

Finally, we consider the sensitivity of our results to instead measuring "high experience" using an indicator of whether an individual is in the top 75% of the experience distribution of all mothers, defined in each year since first birth. We focus on the top 75% of experience because Appendix Table A.20 shows that early exposure has a larger and more precise effect on being in the top 75% of experience in the long-run than being in the top 25% or top 50% of the experience distribution.

On average, this is a higher threshold for “high experience”: it includes just 58% of never-married mothers, compared to 67% using the “worked 3 years after first birth” variable.

In line with our main results, Appendix Table A.22 shows that we also find increases in the probability of being “high earning and high experience” and no effect on being “high earning and low experience” with this measure. We also find no change in the share of low experience mothers with high earnings (using the calculation described above). Interestingly, as a share of the additional early-exposed mothers that have high experience (the sum of columns 1 and 3), 40 to 63% end up being “high earning” across the DD and DDD. This is much higher than the share in our main results (20%), which is consistent with the fact that this is a higher threshold of experience.

F Appendix to Section 7

F.1 Decomposition of EITC benefits

In this section we show how we can decompose simulated EITC benefits into the mechanical and behavioral effects described in Section 7. First, we introduce a bit of notation for convenience.

For each year from first birth τ, the mean EITC credit for early- and late-exposed mothers can be written as a weighted sum of benefits in the phase-in region, i.e., earnings times the phase-in credit rate ($y_i \cdot r_{pt}$); benefits in the flat region, i.e., the maximum benefit (M), and the benefits in the phase-out region, i.e., the maximum credit minus the difference between earnings minus the minimum earnings for the phase-out times the phaseout credit rate ($M - (y_i - \overline{y}) \cdot r_o$). The weights for this sum are the share of mothers in each region. For example, for early-exposed mothers:

$$\begin{aligned}\overline{E_{early}} =& \frac{1}{N}\left[\sum_{i \in p} y_i \cdot r_p + \sum_{i \in f} M + \sum_{i \in o}(M - \underbrace{(y_i - \tilde{y})}_{\text{define as } \overline{z_o}} \cdot r_o\right] \\ =& \underbrace{\frac{N_p}{N} \cdot r_p \cdot \overline{y_p}}_{\text{Phase-in}} + \underbrace{\frac{N_f}{N} \cdot M}_{\text{Flat}} + \underbrace{\frac{N_o}{N}(M - r_o \cdot \overline{z_o})}_{\text{Phase-out}}\end{aligned}$$

To differentiate between early- and late-exposed mothers, we denote the EITC parameters (r_p, $\tilde{y}$, r_o, M) for early-exposed mothers, and as (r'_p, $\tilde{y}'$, r'_o, M') for late-exposed mothers. Thus, the mean EITC benefit for late-exposed mothers is:

$$\begin{aligned}\overline{E_{late}} =& \frac{1}{N'}\left[\sum_{i \in p} y'_i \cdot r'_p + \sum_{i \in f} M' + \sum_{i \in o}(M' - (y'_i - \tilde{y}') \cdot r'_o\right] \\ =& \underbrace{\frac{N'_p}{N'} \cdot r'_p \cdot \overline{y_p}'}_{\text{Phase-in}} + \underbrace{\frac{N'_f}{N'} \cdot M'}_{\text{Flat}} + \underbrace{\frac{N'_o}{N'}(M' - r'_o \cdot \overline{z_o}')}_{\text{Phase-out}}\end{aligned}$$

For simplicity, we set these as the EITC parameters corresponding to $t = 1994 + \tau$ or $t = 1989 + \tau$, for early- and late-exposed mothers, respectively. These will be slightly different than the true EITC parameter experienced by a mother, but is a convenient assumption for our decomposition.

Using a standard Oaxaca-type decomposition strategy, we can write the difference in EITC benefits between early- and late-exposed mothers as a function of two parts. The first "behavioral" difference gives the difference in EITC benefits based on the gap in labor supply, holding constant the early-exposed EITC schedule. The second "mechanical" difference gives the difference in EITC benefits based on the gap in generosity, holding constant the late-exposed labor supply.

$$
\begin{aligned}
\overline{E_{early}} - \overline{E_{late}} = \\
& \underbrace{(\frac{N_p'}{N'} \cdot \overline{y_p}') \cdot (r_p - r_p')}_{\text{Mechanical}} + \underbrace{(\frac{N_p}{N} \cdot \overline{y_p} - \frac{N_p'}{N'} \cdot \overline{y_p}') \cdot r_p}_{\text{Behavioral}} \\
& + \underbrace{\frac{N_f'}{N'} \cdot (M - M')}_{\text{Mechanical}} + \underbrace{(\frac{N_f}{N} - \frac{N_f'}{N'}) \cdot M}_{\text{Behavioral}} \\
& + \underbrace{\frac{N_o'}{N'} \cdot (M - M')}_{\text{Mechanical}} + \underbrace{(\frac{N_o}{N} - \frac{N_o'}{N'}) \cdot M}_{\text{Behavioral}} \\
& - \underbrace{(\frac{N_o'}{N'} \cdot \overline{z_o}') \cdot (r_o - r_o')}_{\text{Mechanical}} - \underbrace{(\frac{N_o}{N} \cdot \overline{z_o} - \frac{N_o'}{N'} \cdot \overline{z_o}') \cdot r_o}_{\text{Behavioral}}
\end{aligned}
$$

Thus, the overall treatment effects that we estimate in Section 7 give the sum of the "behavioral" and "mechanical" elements in this equation. Then, the "behavioral" treatment effect is the sum of the "behavioral" terms, and the "mechanical" treatment effect is the sum of the "mechanical" terms.

F.2 Calculation of Average Tax Rate

In Section 7, we estimate the effect of early exposure to the EITC expansion on federal income tax revenue. This requires an estimate of the average tax rate for the additional dollars earned by early-exposed mothers in the short-, medium-, and long-run. In this section, we explain how we calculate this tax rate.[62]

The average tax rate, $\rho_{avg,\tau}$ paid on the additional earnings of early-exposed mothers in each year from first birth τ is a function of the additional share of women at each level of earnings multiplied by the taxes owed at each level of earnings. In particular, if we discretize the earnings distribution, $\rho_{avg,\tau}$ is:

$$\rho_{avg,\tau} = \frac{\sum_j \rho_{j,\tau} \cdot z_j \cdot \Delta f_{j,\tau}}{\sum_j z_j \cdot \Delta f_{j,\tau}}$$

where j denotes a discrete value of earnings. For our purposes, j will be a bin of earnings. $\rho_{j,\tau}$ is the average tax rate for the bin with average earnings equal to z_j; and $\Delta f_{j,\tau}$ is the difference in the earnings density between early and late-exposed mothers for bin j. Our goal is to estimate an average ρ_{avg} for the short-, medium-, and long-run.

First, we use the coefficients from our distributional regressions (i.e., Figures 3, A.7 and A.13) to generate estimates of $\Delta f_{j,\tau}$. Recall that the distributional regressions give estimates of the difference in the cdf of earnings between early- and late exposed mothers for the short-, medium-, and long-run.[63] In particular, we have estimates of $Pr(Y > y)^{early} - Pr(Y > y)^{late}$ for $y \in \{0, 2500, ...100000\}$. We can use these estimates to obtain $\Delta f_{j,\tau}$ for \$2,500 bins of earnings. To do so, we take the difference between the distributional estimates for two sequential y. For instance, the change in the density of earnings between \$5,000 and \$7,500 is equal to the difference between the change in the cdf at $y = 7500$ and $y = 5000$.[64]

Second, we obtain an estimate of $\rho_{j,\tau}$ for each bin from NBER TAXSIM (Feenberg and Coutts, 1993). In particular, we obtain $\rho_{j,t}$ for calendar year t as the "Income Tax Before Credits" (for a head of household with one dependent) divided by z_j. We calculate this for each z_j in each calendar year. We then take averages over calendar years to obtain $\rho_{j,\tau}$.

Third, combining the inputs from the previous two steps, we calculate ρ_{avg} for the short-,

[62] Another approach would be to calculate taxes directly for each mother using TAXSIM, however TAXSIM is not available to be used from the SSA data center.

[63] We use the same estimates for all τ within the short-, medium-, and long-run.

[64] E.g.,

$$[Pr(Y > 5000)^{early} - Pr(Y > 5000)^{late}] - [Pr(Y > 7500)^{early} - Pr(Y > 7500)^{late}]$$

$$= [\text{Pr(Y>5000)}^{early} - Pr(Y > 7500)^{early}] - [Pr(Y > 5000)^{late} - Pr(Y > 7500)^{late}]$$

$$= \text{Pr}(7500 \geq Y > 5000)^{early} - Pr(7500 \geq Y > 5000)^{late}$$

$$= \Delta f_{7500>y>5000}$$

medium, and long-term. For instance, for the long-run, this is equal to:

$$\rho_{avg}^{long-run} = \frac{\sum_{\tau=10}^{\tau=19} \sum_j \rho_{j,\tau} \cdot y_j \cdot \Delta f_{j,\tau}}{\sum_{\tau=10}^{\tau=19} \sum_j y_j \cdot \Delta f_{j,\tau}}$$

where j denotes \$2,500 bins of earnings.[65] We obtain average tax rates that range from 0–0.04, 0.05–0.07, and 0.13–0.14, for the short-, medium-, and long-run, respectively, using the DD and DDD distributional estimates. We use the minimum of the tax rate for each period to calculate tax revenue: 0, 0.05, and 0.13.

Note that because we only calculate tax rates for late-exposed mothers, our estimated increase in tax revenue does not take into account any changes in the progressivity of the tax schedule over time (i.e., between early- and late-exposed mothers.) The advantage of holding tax rates fixed is that it allows greater transparency into these calculations.

[65] Since we estimate our distributional regressions over groups of τ, in practice we only have one value of $\Delta f_{j,\tau}$ for the short-, medium-, and long-run (each).

F.3 Government Transfers

In Section 7 we estimate the impact of work incentives on government transfers using information on self-reported income from various government programs from the CPS. In particular, we analyze government transfers to a woman's family from the following 5 programs, and total benefits as the sum of benefits from these five categories:[66]

1. Food stamps: household value of food stamps (*hfdval*)
2. Welfare: family value of welfare (*fpawval*)
3. Disability: family disability income (*fdisval*)
4. Medicaid: family fungible value of Medicaid (*ffngcaid*)
5. Housing subsidy: family market value of housing subsidy (*fhoussub*)

Several caveats apply to this analysis. First, program participation is increasingly underreported in the CPS, which implies that early-exposed mothers are likely to underreport transfers more than late-exposed mothers (Meyer et al., 2015). Second, married mothers have much lower rates of program participation than never-married mothers, which makes them a less useful comparison group for these outcomes. Third, we expect welfare reform to mechanically lead to a reduction in benefit dollars. Because we do not have controls for the potential duration of benefits or dollar amounts, our estimates will likely partly reflect this mechanical change. Finally, the value of housing subsidy is missing for the 1991 CPS, and the value of Medicaid is missing for the 1991 and 2012+ CPSs. The missing data in 1991 makes it such that we have little information on late-exposed mothers in the first couple of years after birth, and that the differential effects for early-exposed mothers are estimated only in post-birth years 3 and 4. The missing data after 2011 makes it such that we have little information on early-exposed mothers in the long-run, and that their differential effects are estimated only in some of the long-run years.

For these reasons, we interpret our estimates of the impact of early-exposure on transfers in Appendix Table A.25 with caution. The reasoning above suggests that these estimates are likely to be an upper bound on the (absolute) decline in transfers, which leads us not to incorporate this into our baseline MVPF estimates.

[66]We use household information for food stamps, as family food stamp information is not collected in the 1991 CPS. Note that we observe 1 unique woman in 99.9% of households, so the risk of double counting food stamp receipt because of multiple treated women in the same household is minimal.