Long-Run Effects of Incentivizing Work Post-Birth

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Abstract

This paper provides new evidence of the impact of increasing post-birth work incentives on mothers' long-run career trajectories. We focus on variation in incentives from the substantial expansion of the Earned Income Tax Credit (EITC) in 1994, and use two complementary research designs that exploit variation in the timing of exposure to the expansion as well as in eligibility for the credit. Using a large-scale panel of administrative earnings linked to the CPS, we show that mothers exposed to the expansion immediately after birth ("early-exposed") have 4 to 5 p.p. higher employment in first 5 years after birth than mothers exposed 3 to 6 years after a first birth ("late-exposed"). Fifteen years later, we find that early-exposed moms have the same employment as late-exposed moms, but have accrued 0.6 to 0.8 more years of work experience and 8 percent higher earnings. These impacts on labor market outcomes then feedback into future EITC benefits: early-exposed moms are expected to receive higher EITC benefits in the short-term, but lower EITC benefits over the long run. Our results suggest that there are steep returns to work incentives at child birth that accumulate over the life-cycle.

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1 Introduction

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 demographic groups, and has been shown to persist for at least a decade after a first birth (Kuziemko et al., 2018; Nix and Andresen, 2019; Kleven, Landais, Posch, Steinhauer and Zweimüller, 2019; Angelov et al., 2016; Chung et al., 2017). In response to this, there has been 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?

More so than other populations, new mothers face several distinct challenges that make the magnitude of the return to early career experience uncertain. Many new mothers respond to child-rearing demands by working part-time, or in less-time-intensive occupations, which may be associated with few returns to experience (Goldin, 2014). This may be exacerbated by earlier return to work if there is a trade-off between how soon mothers begin working post-birth and how demanding the position is. However, returning to work sooner may also serve as a costly signal of greater attachment to work to employers. This could be important early in a mother's career as it could lead to earlier promotions or greater wage growth (Thomas, 2019; To, 2018). Notably, some of these benefits may not manifest in the short-run, which implies that the net effects of work experience may not be evident until many years after mothers return to work.

This paper contributes new evidence to the ongoing debate on the long-run returns to post-birth work experience. We focus on a population of single mothers, and obtain variation in the initial incentive to work due primarily to the 1994 expansion of the Earned Income Tax Credit (EITC), which provides work-contingent cash transfers to families. We trace the exact earnings trajectory of mothers with early exposure to the shock – immediately after a first birth – compared with the earnings trajectory of mothers with later exposure to the shock – 3 to 6 years after a first birth. The years-long gap in work incentives sets up a potentially significant shock to post-birth employment and work experience.

The source of variation and population that we analyze are substantially different from existing evidence on returns to experience post-birth. Current estimates are largely identified from changes in paid leave policies,[1] which typically induce 1-to-3-month increases in time

[1]These include Schönberg and Ludsteck (2014), Lalive et al. (2013), Lalive and Zweimüller (2009), Dahl et al. (2016), Stearns (2018), 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).

away from work in the first year after birth. The relatively small change in experience may make it difficult to detect an impact on earnings, and could be less relevant for the large subset of mothers that do not return to work within a year of birth (who account for 40% of US mothers (Laughlin, 2011)).

We use a unique, large-scale panel of women's earnings that links twenty three years of the March Current Population Survey (CPS) spanning from 1991 to 2016 to large-scale administrative data on individual earnings and exact dates of birth from the Social Security Administration (SSA). These data allow us to observe precise, longitudinal measures of employment and earnings before and after a birth for a large sample of over 11,000 single mothers. By comparison, relying on the CPS survey alone would give 90 percent fewer observations for any given year. Additionally, unlike typical administrative records in the US, we also have information on demographics and family composition.

Our identification strategy leverages both across and within- year-of-first-birth exposure to the EITC reform. First, we exploit the timing of the reform in a difference-in-differences design that compares the outcomes of high-EITC-eligible, never-married mothers that gave birth after the reform, and were "exposed early" to this incentive, to never-married women that had a first birth three to six years before the reform and were "exposed late" to this incentive, before and after a first birth. This design allows us to control for differences in pre-birth levels of labor market engagement across childbearing cohorts of mothers, as well as for natural dynamics in the return to work as children grow up. Second, we take advantage of the fact that married mothers were less likely to be eligible for the EITC to implement a triple-difference approach that compares the outcomes of never-married and married mothers that give birth in the same year, before and after a first birth.

A well-known challenge for identifying the impacts of the EITC is separating its effects from other factors in the '90s, such as changes to the welfare system or the booming economy. We address this in two ways. Following prior work, we include rich state-level controls for the adoption of welfare waivers and for local unemployment rates. Alternatively, we try to quantify the potential contribution of each of these other types of work incentives to our estimates, showing that the EITC expansion is indeed a significant driver of our findings. Assigning precise credit across work incentives is not our main objective, though; rather, we view the policy-induced rise in post-birth employment as a means to study impacts on long-term earnings.

We find that in the short term (0-4 years post-birth) early-exposure to work incentives raises post-birth employment by 3.7 to 5.4 p.p., which represents a 5.8 to 8.6 percent increase. This implies that early-exposed moms recovery between 29 and 42 percent of the nearly 13 p.p. post-birth decline in employment in the first five years after birth. These effects appear

in the year following birth, and we find no trend in labor supply leading up to birth. Post-birth earnings also increase, concentrated in the phase-in and flat regions of the EITC. The DDD estimates show that women earn $1,023 more on average, which amounts to a 12% earnings increase.

Fifteen years later, when both early and late exposed mothers could be eligible for the EITC, we find no significant difference in employment rates between early- and late-exposed moms. Hence, late-exposed moms appear equally receptive to work incentives, despite staying out of the labor force longer post-birth. Nevertheless, we find lasting gains in experience and earnings. Twenty years post birth early-exposed moms have 0.7 to 0.8 years more work experience, and $2,000 higher earnings, an 8 percent increase.

Why do early-exposed women earn more? We assemble a collage of evidence by bringing in the CPS survey responses to complement our earnings results. We show that early-exposed mothers avoid non-employment spells of 4.9 years on average, which may be particular harmful to career prospects. We also provide descriptive evidence that early-exposed mothers engage in full-time work more than late-exposed mothers during the early years post birth, and that they experience the largest growth in earnings once they transition to full time work. Quantitatively, this appears to explain the lasting earnings gains, consistent with prior evidence that longer hours fuel early career growth (Goldin, 2014; Wasserman, 2019). We do not find any evidence that early exposure changes in the spacing or number of children.

We perform a number of additional tests to verify our interpretation of the results. First, we show that there was not a general trend of working more post-birth leading up to the reform, suggesting that there was not an ongoing change in child-rearing norms or other cultural shift that would confound our treatment. Second, we reestimate our DDD results using two alternative comparison groups: childless soon-to-be moms and childless never-moms assigned placebo births (Kleven, Landais and Søgaard, 2019).[2] We find very similar results for both of these. Third, we show that the initial rise in employment was larger for 2^{nd}- or higher-parity birth than after a 1^{st} birth, consistent with EITC incentives, but do not find a significantly different employment response between mothers after 2^{nd} or 3^{rd}+ births. Finally, we show that a fully dynamic control for welfare reform only attenuates our short-run estimates by 25%. These results are consistent with the EITC incentives as a main driver of our results.

In the last part of the paper, we consider implications of the change in work incentives for public expenditures on the EITC. We find that simulated EITC benefits increase among early-exposed moms in the short-term due to working more; but, in the long-run these mothers receive fewer EITC benefits due to having higher earnings. Incorporating these

[2]By construction, we are only able to use future moms to examine sensitivity for our short term results.

labor market dynamics into evaluations of the EITC program would be expected to raise its benefit-cost ratio.

Our paper is at the center of two actively debated literatures. 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 (discussed above); and (ii) welfare experiments (Card and Hyslop, 2005).[3] These studies find little effect of extending mothers' time away from work, but also relatively small changes in experience (e.g. 0.2 to 0.3 years in Card and Hyslop (2005)). Additionally, these designs do not necessarily only capture the effect of longer time at home post-birth. Paid leave reforms often also include job protection, which could mitigate negative effects of a longer leave,[4] and welfare experiments target all mothers, not just new mothers. Our experiment is thus better targeted for identifying the impact of curtailing longer stays at home post-birth.

Second, we build on work examining the effect of the EITC on female labor supply (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Bastian, 2018; Kleven, 2019; Hotz and Scholz, 2006; Chetty et al., 2013; Neumark and Shirley, 2017; Eissa and Hoynes, 2004). 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. However, the small sample size in Neumark and Shirley (2017) substantially reduces the power to detect impacts. Our ability to track long-run responses using administrative data is unique in this literature and is a primary contribution of our work. These data allow us to show long-lasting effects of EITC incentives at birth, as well as to control for selection into motherhood with pre-birth data, which is an advantage over existing short-run estimates. We provide evidence of important dynamics in the response of mothers to work incentive over the life-cycle. The persistent earnings gains that we document could serve as an additional mechanism for the transmission of EITC benefits to children (Bastian and Michelmore, 2018; Dahl and Lochner, 2012).

We also contribute to a smaller strand of this literature that considers the benefit-cost ratio of the EITC relying on short-run estimates on labor supply (Bastian and Jones, 2018; Hendren and Sprung-Keyser, 2019). Our results on earnings and EITC benefits suggest that taking dynamics into account is quite important for the inputs into these calculations, and

[3]Other changes in the employment of mothers typically either bundle multiple treatments or target mothers with older children. For example, studies of fertility treatments (Lundborg et al., 2017) bundle greater presence of children and time away from work, while studies of increased child care availability analyze mothers with toddlers (Lefebvre et al., 2009).

[4]Notably, the most negative estimates of paid leave are Schönberg and Ludsteck (2014), who find negative impacts of extending maternity benefits beyond the span of job protection, and Bailey et al. (2019), who find negative effects of California's paid leave on women's earnings after a first birth.

that existing estimates are thus likely to be a lower-bound.

2 Institutional Background

The EITC is a refundable tax credit for working families that is currently one of the largest cash transfers to low-income households (Nichols and Rothstein, 2015). Appendix Figure A.1 presents EITC benefit eligibility as a function of household earnings for 1-child families in 1993 and 1994. In the "phase-in" region, individuals receive a tax credit for each dollar earned, up to a yearly maximum amount. In the second region, called the "flat" region, individuals receive the maximum credit. Finally, in the "phase-out" region, individuals start losing the credit with each dollar earned, but at a slower rate than in the phase-in region.

While the general shape of the EITC schedule has remained the same over the years, several EITC expansions have affected phase-in and phase-out rates, the maximum EITC credits, and whether these differed by number of children. For example, in 1992 the maximum EITC benefit was $2,265 ($2016), and was only available to parents. The 1993 EITC expansion, which went into effect in 1994 and is the focus of our paper, established a small, $495 credit for the childless, and it substantially increased the maximum amount available for families with 1 child, from $2380 to more than $3,300 ($2016). The credit was increased even more for parents with two or more children (to $4,094). Moreover, the law included a gradual increase in benefits for those with two or more children over the period 1994 to 1996. To illustrate the major EITC expansions in its early years, Figure A.2 shows the maximum EITC benefits by number of children between 1975 and 2000.

The 1993 expansion of the EITC was a major policy development during the 1990s, but this period also saw other important labor market policies and changes. Among them are the strong performance of the labor market in the second half of the decade and the 1996 welfare reform, which was preceded by a few years of state-level welfare program experimentation. Since both of these events could have also influenced the labor supply decisions of new mothers, our robustness section contains several checks that suggest that our results are likely driven by the EITC.

3 Data

3.1 Primary Data

Our primary data builds on the microdata from the 1991, 1994, and 1996 to 2016 Annual Social and Economic Supplements of the Current Population Survey (ASEC CPS). These

23 survey years are the only ones that both have a protection identification key (PIK) – a unique mapping to a Social Security Number (SSN) – and are covered by the data agreement between SSA, IRS, and Census.[5] We use individual responses to the CPS survey to obtain demographics for the individuals in our sample, including race, years of completed education, state of residence, and marital status, as well as information on work activity, including hours worked in the past week, which is unavailable in administrative data. Moreover, we use CPS-provided parent identifiers to connect parents and children in the survey. We then determine birth order by sorting individuals in the household that have same parent identifying number by year of birth.

The demographics from the CPS serve two purposes. First, we use marital status reported in the CPSs – which typically occur after birth – as a noisy proxy for marital status at birth. More specifically, we use never married mothers as our high-impact population, as they were also never-married at birth and likely to be eligible for and incentivized to work by the EITC (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001). [6] We then also use married mothers as a low-impact, comparison, group. Our reliance on this noisy proxy of eligibility raises several potential concerns regarding the representativeness of the sample and potential selection bias. We discuss representativeness below and discuss selection bias in Section 4.1. Second, the CPS demographic information allow us to include individual characteristics and state policy parameters as covariates in our analysis. This is an advantage over typical administrative sources, such as tax records, where these characteristics are not observed.

We match the CPS survey responses to Social Security earnings records for each individual from 1978 to 2015.[7] 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 do not observe informal employment or any income beyond these measures, which is an inherent limitation of tax data.[8] We also obtain precise dates of birth by merging this data to Social Security Administration records (the "numident" file). We use this as our preferred measure of year of birth for adults and children.[9]

[5]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).

[6]Mothers who are divorced or widowed at birth are also likely to be EITC-eligible, but we cannot consistently identify these mothers as divorce or widowhood could have occurred in the time period between birth and the CPS interview.

[7]Specifically, we merge CPS records to the Detailed Earnings Record

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

[9]In most cases, this aligns with an imputed year of birth using CPS age and year; however when the numident year of birth differs by more than 5 years from the CPS response, we assume that the numident match is incorrect, and use our imputed year of birth.

Across all CPS years, we match between 75% and 80% of the women that meet our sample criteria (described below). Match rates are similar by year of first birth and by marital status – see Appendix Table A.12 for detailed statistics on match rates. The one exception is the 2001 CPS, which we drop for having a particularly low match rate.

Among matched individuals, we restrict our sample to women that meet the following criteria. First, we drop women that had a first birth at age 18 or younger, who may still be attending high school and could still be claimed as dependents themselves. Second, we drop women interviewed in the CPS after age 50, since the children of older women are less likely to be living in the household. To the extent that some of the first-born children of women will have already left home when we observe them, this will create measurement error in our assignment of birth order. Third, we limit the sample to women that were never-married or married at the time of interview, following the reasoning discussed above.

Appendix Table A.1 provides summary statistics for our main sample of never-married women relative to married women. 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, have half the earnings of married women ($12,000 vs. $24,289.) Further, 87% of never-married women would qualify for any EITC benefits based on the minimum of her (household) earnings in the three years prior to birth, compared with 51% of married households. Even so, a majority of married women are the "secondary earner" (account for less than 40% of household earnings), which makes them less susceptible to increasing employment in response to household-income-based incentives (Eissa and Hoynes, 2004). Hence, never married women are 1.7 times as likely to be in an EITC-eligible household, and over 4 times as likely to be the primary earner in an EITC-eligible household (87% relative to 20% ($51\% \cdot 0.4$)), relative to married women. EITC benefits are also much lower for married households.

In order to consider the representativeness of the sample, in Appendix Table A.2, we compare how the characteristics of our never-married sample compares to the sample of women that meets the same sample restrictions, but who appear in the earliest CPS surveys up to 1996. Since a large share of the latter sample is surveyed within a few years of a first birth, the difference in the samples illustrates how the demographics of our sample compares to a typical sample of women never-married at birth. Comparing across the two samples, including additional CPS years skews the sample towards a higher fraction of women that are non-white and a lower share with high school or less relative. However, the pre-birth labor market outcomes of our sample is quite similar to the never-married at birth sample in terms of employment, earnings and earnings conditional on working. Hence, for these key labor market outcomes, our sample appears to have significant overlap with women who are

never-married at birth. Further, the additional CPS years increases the number of unique individuals by an order of magnitude. In Section 7, we show that our qualitative results are unchanged by including the additional CPS years, although the addition of more CPS samples reduces our standard errors.

3.2 Comparing CPS and administrative earnings

The link between CPS and administrative data allows us to compare earnings across these sources. Comparing lagged administrative earnings to self-reported CPS wage and salary earnings among women in our sample reveals several important discrepancies. First, 10% of the comparisons 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. To quantify the difference in reported earnings, we restrict our attention to individuals that have any earnings in both sources, and taking 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, implying that over 50% of the comparisons differ by over 20 percent, and that these discrepancies do not go in a consistent direction. These comparisons suggest that we will have significant additional precision by using the administrative earnings measures, in addition to having a larger sample size.

3.3 Supplementary Data

We merge on state-by-year data on a number of state-level economic conditions and policy variables from Bitler and Hoynes (2010). These include the unemployment rate, the maximum level of AFDC 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 use information on states' implementation of six different types of welfare waivers from Crouse (1999) and Gallagher et al. (1998).

4 Empirical Strategy

Our research design follows women around the year of their first childbirth, which makes them eligible for a significantly higher EITC benefit. We compare the post-birth outcomes of women that gave birth between 1993–1996 to women that gave birth between 1988–1991. The former group was exposed to the 1993 EITC expansion in the first year after childbirth ("early-exposed" or "treated at birth"), and thus had strong incentives to return to work immediately after birth. The latter group does not become exposed to post-expansion

benefits until 3 to 5 years after childbirth ("late-exposed"), which provides a clean comparison group for the first years after birth. This follows in the spirit of Chetty et al. (2013), who also examine the change in post-birth labor supply to identify EITC responses, but exploit geographical variation in EITC-induced bunching to generate comparison groups.

Figure 1 illustrates the idea behind this empirical strategy by showing EITC credit eligibility by child age, for mothers who gave birth in 1993-1996 (early-exposed) relative to 1988-1991 (late-exposed). All mothers were eligible for (near) zero benefits prior to giving birth and for around $3000 when their child was 6 or older, but early-exposed mothers received higher benefits when their child was between 0 and 5.

We start with a dynamic difference-in-differences (DD) analysis that estimates the difference in labor outcomes between early- and late-exposed never-married women in each year relative to childbirth. Denoting an individual as i, the year of first childbirth as b, the current year with t, and the year relative to childbirth with τ, we estimate:

$$Y_{ibt} = \alpha + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}(\tau = k) \cdot EarlyExposed_b + \theta_\tau + \chi_b + \gamma X_{ist} + \delta P_{st} + \epsilon_{ibt} \qquad (1)$$

where $EarlyExposed_b$ is an indicator for having a first birth between 1993 and 1996. The summation term represents the difference in outcomes for never-married women that were exposed at birth (1993-1996) relative to women that were exposed later (1988–1991), for each year relative to birth, τ. We omit $\tau = -1$, such that these coefficients are estimated relative to the difference in outcomes in the year before birth. θ_τ capture average effects for each year relative to childbirth, and χ_b are year-of-childbirth fixed effects, which control for changes in women's labor supply in $\tau = -1$ across women that give birth in different years. This includes, for example, changes in selection into giving birth or new policies across cohorts. X_{ist} includes fixed effects for mother's year of birth, age, race, state, and education group; P_{st} controls for state level parameters, including the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, the adoption of any welfare reform (TANF or waivers), and the adoption of six different types of welfare waivers.

The coefficients on the post-childbirth event-time indicators in this specification have a DD interpretation, since they trace out how the gap between the childbearing cohorts exposed at birth (treated) and exposed later (comparison) varies over time. To summarize the post-birth EITC treatment effect, we replace the indicators for each year relative to birth with a $PostBirth_\tau$ dummy, which is equal to 1 in the year of childbirth and the following years.

Second, we take advantage of the fact that there is significant variation in EITC eligibility among women with different marital statuses giving birth in a given year. This sets up a

triple-difference 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 triple-difference (DDD) model:

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

Here the $\beta_{k,DDD}$ coefficients trace out the DDD, comparing never-married mothers to married mothers, with first births before and after 1993. We include fixed effects for marital status (ρ_m), year of birth (λ_b), and years from birth (θ_τ), and 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 as a source of bias. Moreover, we allow all individual and state-level controls, X_{ist} and P_{st}, to vary by marital status. As before, we also replace the indicators for each year around birth with a $PostBirth_t$ dummy to estimate average treatment effects.

For all analyses, we include standard errors clustered at the state level. As a robustness check, we also obtain confidence intervals using randomization inference.

4.1 Threats to Identification and Testable Implications

One of the major identification assumptions in our DD design is that there are no shifts in norms regarding employment across women that gave birth in 1993-1996 vs 1988-1991. For example, it would be problematic if early-exposed women were increasing their labor supply faster leading up to a birth relative to late-exposed women. Year-of-childbirth fixed effects help to address level changes in labor supply across childbirth cohorts, but not potential differential changes in trends after childbirth. We test the feasibility of this assumption by looking for "pre-trends" before birth, i.e changes in *pre-birth* labor market participation across cohorts. We also test whether women's post-birth employment was changing across child cohorts *prior* to 1993. Both of these tests pass easily.

A second assumption in the DD is that there are no other contemporaneous policies or economic conditions that differentially affect the maternal labor supply of early- and late-exposed women, either in the short or the long-run. For example, the 1993 EITC expansion occurred close to welfare reform and the economic boom of the 1990s, which could confound our short-term employment effects. We discuss in detail whether, and how much, these contemporaneous shocks may drive our short-term effects, and find suggestive evidence of a stand-alone EITC impact. Nonetheless, we can also conservatively interpret our long-term results as reflecting the confluence of these work incentives immediately after birth. The

most important part of the second DD assumption therefore is that there are no differential shocks affecting early and late-exposed women in the long-run. Note that since year t is a linear combination of event-time τ and year of birth b, we can not include unrestricted time fixed effects to control for national level policies and economics conditions in this design. For this reason, our baseline specification controls for state welfare policies and unemployment rates, as is common in the literature.

Our DDD design allows us to relax both of these assumptions. In particular, the interaction between year-of-birth and years-since-birth, $\theta_b \cdot \lambda_b$, controls for all contemporaneous shocks that are common to all mothers, such as social norms or federal policies.

However, the DDD approach also carries some important assumptions. First, we assume married and never-married women participate in similar labor markets and that there are no national policies and shocks that affects them differentially. As a robustness check, we use never-married childless future mothers as a comparison group instead of married women, as never married future mothers could arguably more closely mimic the national labor market patterns of never married mothers. Our findings are unchanged with this alternative comparison group (see Section 5.1). Second, we assume that married women were less-affected by the EITC expansion, such that the change in married mother's behavior across childbearing cohorts reflects non-EITC labor market incentives. One potential violation of this is the finding in Eissa and Hoynes (2004) that the EITC expansion caused low-educated married mothers to reduce employment relative to low-educated married women without kids. This could then cause us to *overestimate* effects on labor supply of never-married mothers. However, the effects in Eissa and Hoynes (2004) are concentrated among mothers with two or more children, while we focus on behavior after a first birth, which partially alleviates this concern. Low-educated mothers are also a minority of our married sample overall, which further makes the behavior of this group unlikely to drive our DDD results.

Finally, an issue common to both the DD and DDD is the reliance on marital status for assignment of treatment, which could introduce bias if the timing of exposure to the EITC impacts decisions to marry.[10] This is an issue relevant for nearly all EITC papers, which commonly rely on current marital status as a measure of EITC eligibility. Importantly, for changes in marital status to bias our DD results, it is not enough for the fraction of women that choose to marry for the first time after birth to be differential across early- and late-exposed women, it must also be the case that the probability of marrying is correlated with one's earnings trajectory. For example, it would be a concern if the early-exposed women

[10]Existing estimates of the effect of the EITC on marriage find a range of estimates from a small negative effect (Dickert-Conlin and Houser, 2002), ambiguous impacts (Ellwood, 2000) or a small positive effect (Bastian, 2017). This suggests that marriage effects are likely to be small, although these studies do not focus on new moms.

who are induced to earn more after a birth marry at lower rates than mothers that are late-exposed or mothers that are early-exposed women but have no earnings growth – since this would cause our early-exposed sample to overrepresent mothers with a steeper earnings trajectory, generating upward bias. In such a scenario, we would then expect the DDD to further exacerbate this bias, as the pool of early-exposed married women becomes filled with lower-growth mothers, making the early-exposed never-married mothers appear to have higher relative earnings growth.

Since the issue of selective marriage increases with time since birth, we can quantify the size and directions of the resulting bias by dropping more recent CPS interviews or CPS interviews that occur after a longer period since birth. This provides an additional improvement over earlier works relying on the CPS alone, where it is not possible to perform such a robustness analysis. We run both of these variations of the analyses in Section 7, and find no substantial changes in our estimates as we change the sample.

Finally, Appendix Table A.3 shows that there is imperfect balance in pre-birth characteristics across early- and late-exposed women. In particular, early-exposed women are less likely to be non-white, to have a high school education or less, and more likely to have any earnings prior to birth. On the other hand, we see no difference in age at first birth, household EITC eligibility pre-birth, and annual earnings conditional on working. Our panel DD approach allows us to absorb level differences in these pre-birth characteristics, which makes this less of a concern. As a robustness check in Section 7, we use inverse p-score weights to impose balance in these characteristics, and find the same results.

5 "First Stage:" Returning to Work Post-Birth

Panel (a) of Figure 2 presents regression-adjusted means for employment, defined as having any positive earnings, including wage/salary and self-employment earnings. It shows that "late-exposed" women slightly increase their likelihood of working leading up to childbirth, followed by a 12.6 p.p. decline in the year of birth. This decline grows in the year after birth to almost 20 p.p., followed by a 4 p.p recovery in the following three years. For "early-exposed" women, the patterns before birth are similar, as is the drop in the year of birth, captured by the fact that the dots for each of the cohorts lie on top of each other in nearly every year relative to birth. However, beginning in the first year after birth, employment rebounds relative to the late-exposed moms. For the next 4 years, early-exposed moms have a 5 to 6 p.p. higher rate of employment.

Panel (b) of Figure 2 presents the difference in employment between early and late-exposed mothers, our dynamic DD coefficients. The coefficients in the years up to birth are

close to zero, mirroring panel (a), which indicates that there was no difference in pre-birth likelihood of work between women giving birth between '88-91 and '93-96. The labor supply of early-exposed mothers is significantly higher beginning in the year after birth. These women have a 3 p.p. higher employment in the year after birth, which grows to a 6 p.p. gap in years 2–4.

Panel (c) of Figure 2 presents the dynamic DDD, where we introduce married mothers as a comparison group. The patterns remain quite similar, however the post-birth coefficients are now larger, between 6 and 8 p.p. in years 2–4.[11] For both the DD and DDD the effects on employment are relatively level between the third and fourth year post-birth, suggesting that the response to early incentives was relatively immediate.

Table 1 presents the regression coefficients from the DD and DDD specifications. In the first column, we show that the change in the maximum eligible EITC credit between early and late-exposed mothers was $1,009. The next two columns show the DD results for never-married and married women, respectively. We find a statistically significant 3.7 p.p. increase in post-birth employment following the EITC expansion. This represents a 5.8% increase in the likelihood of working relative to the late-exposed post-birth mean, and a 27% recovery relative to the drop in employment in the year of birth.[12]

In contrast, we find a statistically insignificant and negligible impact on married women (point estimate = 0.001). In Appendix Table A.4 we investigate whether these null impacts mask heterogeneity across married households' EITC eligibility before childbirth. We find no significant effect for eligible or ineligible married women, perhaps both because the credit for these households remains small as a proportion of household income and because married women are secondary earners.

The DDD coefficient in the third column of Table 1 suggests that the EITC increased the employment of never-married women by 5.4 p.p, an 8.6 percent increase relative to the late-exposed post-birth employment. Placing these effects in perspective, the range of estimates imply that early-exposure causes mothers to recover 29–42% of the initial drop in work within the first 5 years of birth, relative to late-exposure.

In the following columns, we examine whether women have any *wage* earnings, which allows us to abstract from changes in self-employment. The coefficients are slightly smaller (by 13% for the DD and 4% for the DDD), but we find very similar patterns to the initial effects on any earnings. Thus, the majority of the increase in employment is due to a rise in

[11] In line with this, we do not find an increase in married women's likelihood of working – see Appendix Figure A.3.

[12] Although we focus on a population of women with children under 5, this effect is similar in size to the rise in LFP among all single women with children in earlier work, such as the 3.1 p.p increase in Meyer and Rosenbaum (2001).

third-party employment, rather than self-employment.

Figure 3 shows the effects of early-exposure on the distribution of earnings when we zoom in on the period 0 to 3 years post-birth, as women are first returning to the workforce. We construct this figure by running our DDD model with outcomes being a series of indicators for having earnings ($2016) above X, with X = 0, 2500, 5000,....60,000 (Duflo, 2001). 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. The figure shows a significant shift in the distribution in the phase-in region, resulting in an increase in the probability of earning at least $7,500. There is also some additional mass in the flat region, including beyond the poverty line.[13] Effects in the phase-out region are small and statistically insignificant, implying that mothers are not more likely to have high enough earnings to reduce the EITC credit from its maximum.

Integrating over these effects, the DDD implies that early-exposed women earn $1,023 (se: 336) more on average ($1,367 for the DD). Relative to the mean short-term earnings for late-exposed women ($9,926), this amounts to a 12% earnings increase. This increase in earnings are too large to be explained by the increase in the likelihood of working alone. Assuming that early-exposed new mothers that enter the labor market earn as much as the average late-exposed working mother ($15,737), average earnings would be expected to rise by $818, rather than by $1,023. This implies that there are also likely some intensive margin effects: early-exposed new mothers earn slightly more conditional on working.

5.1 Sensitivity and Interpretation

These results suggest that the EITC expansion increased mother's employment after childbirth. We now discuss evidence addressing potential threats to identification from section 4.1.

First, we check that this shift in post-birth work was not part of a confounding ongoing trend across childbearing cohorts leading up to the reform. We separately estimate the change in post-birth employment for first births between '88-89, '90-91, '92-93, '94-95, and '96-97. We then test whether post-birth labor supply for births from 1990 onwards was different than that of the '88-89 cohorts.

Figure A.4 plots the coefficients for the never married DD and DDD from this specification, which tell a very similar story. Leading up to the expansion, post-birth employment had been declining (between 1988–1991 birth) or steady (between 1988–1993). After the expansion, post-birth employment increased by between 5 and 10 p.p., reaching a new equilibrium

[13] While married women earn more than never-married women, Appendix Figure A.6 shows that there is significant overlap in the earnings distributions of earnings 0 to 3 years post birth for late-exposed women.

for fully exposed cohorts.[14]

Second, we test the sensitivity of our results to including alternative controls for individual characteristics or sample restrictions. We present the results in Table 2, with the never married DD and DDD in Panel A and B, respectively. Column 1 repeats our baseline results. Column 2 allows for greater flexibility in our age fixed effects, by substituting them for age-at-birth by years-since-birth fixed effects. The effects are unchanged. Column 3 introduces individual fixed effects, which likewise has minimal effect on our estimates. Column 4 restricts the sample to moms that are the head of the household in the CPS survey, and therefore may be more likely to file as head of household for tax purposes. Our findings are the same for heads.

Third, we test whether controlling more carefully for economic conditions or welfare reform alters our conclusion. While we have argued that our effects could plausibly have resulted from the 1993 EITC expansion, these effects could in theory have been caused by the robust economy or changes in the availability of welfare during the 1990s (Meyer and Rosenbaum, 2001). In Column 5 of Table 2 we allow for differential effects of economic conditions by child age by interacting the unemployment rate controls with indicators for years since birth. The coefficients are substantively unchanged.

Column 6 introduces more detailed controls for welfare reform. We include indicators for the presence of 6 different types of state waivers used to alter different aspects of welfare requirements, which we obtain from Crouse (1999) and Gallagher et al. (1998).[15] We also allow for a different coefficient post-birth for each of these waivers-types as well as our main reform variable. This leads to a somewhat larger decline in the DD point estimate, but the effects are still significant at the 5% level and within the 95% confidence interval of our main estimates.

To further isolate EITC incentives from welfare reform, Columns 7 and 8 restrict our sample to states that experienced changes in welfare policies after 1996. Column 7 shows that the overall employment effects in these states is similar or larger to the effects for the whole sample. Column 8 restricts the sample to the years up to 1996, prior to any welfare change. Even with this significantly smaller sample, we find marginally 2 to 3 p.p. increases in work.

These results suggest that while welfare reform may have reinforced the return to work after birth, it can not explain the majority of our findings. This is consistent with Meyer

[14]Figure A.5 shows the dynamics behind the coefficients for each child cohort.

[15]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.

and Rosenbaum (2001) and Grogger (2003) who find that the EITC contributed relatively more to labor supply changes than welfare reform.

Fourth, we consider using never-married women without children as an alternative comparison group for the DDD approach. In order to make these groups as comparable as possible, we restrict the childless sample to women that will become mothers in the future. Mechanically, we compare women that give birth in year t in our main analysis to women that give birth in year $t+3$. We then assign each woman in the future mothers sample the same τ corresponding to her matched sample for each calendar year. For example, we match mothers that have a first birth in 1994 with mothers that give birth in 1997, and assign the 1997 mothers to have a $\tau = 0$ in 1994, $\tau = 1$ in 1995, etc. This guarantees that conditional on dummies for $\beta \cdot \tau$, we are always comparing a mother and a future mother that are in the same calendar year. We then run the DDD model (Equation 2) replacing married women with future mothers, and restricting our attention to the period up to three years post-birth. The final columns of Table A.7 show that there is no rise in employment among childless women, and that as a result we find very similar results for mothers when we use this comparison group.

Next, we look at whether women are responding to *EITC-specific* labor supply incentives by looking for previously-documented bunching at the first EITC kink (Chetty et al., 2013; Saez, 2010). We use the EITC schedule in each year to determine income relative to the kink, and focus on women ever reporting self-employment income, who have been found in other samples to more frequently report income at EITC kinks.

Figure 4 shows the number of self-employed women with income in \$2,500 (\$2016) bins centered around the first EITC kink as a share of all mothers exposed late (panel a) and early (panel b), separately for before and after birth. There is no noticeable sharp bunching for any women before birth. We also do not find any sharp bunching post-birth for late-exposed births, despite the fact that there was a (smaller) EITC available in those years.

In contrast, we find a sharp increase in mass around the kink post-birth for early-exposed moms. Nearly 1% of early-exposed women report being self-employed and having an income within \$2,500 of the kink, almost twice the pre-birth share. We interpret this as evidence that new mothers responded more strategically to the 1993 expansion than earlier EITC incentives.[16] This indicates that the increase in bunching post-reform found in earlier work was also reflected in the behavior of newly-EITC-eligible moms (Saez, 2010).[17]

[16] We also find little to no evidence of bunching for women that were never self employed, consistent with earlier work.

[17] Appendix Table A.5 quantifies the impact of early-exposure to the expansion on having any self-employment and bunching. We find positive and statistically significant impacts on both margins. Consistent with the earlier results, the impacts on having any self-employment income are less than 25% of the

Finally, in Appendix Table A.6 we ask whether women return to work more after a second or higher order birth, since the EITC expansion increased benefits more for women with two children or more. We find that employment increases more after a second or higher order birth. We also are not able to reject that the effects are the same for women after a 2^{nd} or 3^{rd} (or higher birth), consistent with the EITC incentives.[18]

6 Long-Run Effects

Our ability to track women's earnings up to 20 years after childbirth gives us the unique ability to discover how early incentives to return to work affect earnings over the medium and long-run, which we will use as shorthand for the period 5 to 9 and 10 to 20 years post birth, respectively.

We begin by examining the long-run impacts on the likelihood of working. Recall that Figure 1 illustrated that the gap in EITC incentives across early- and late-exposed mothers closes between 3 and 6 years after a first birth. This implies that we may also expect the gap in employment to attenuate over time. Figure 5 shows that the "first stage" on employment begins to decline around 4 years after birth, and that over the next 5 years the difference nearly completely disappears. The fact that the difference in work persists beyond the gap in benefits is consistent with the ramp-up in the labor-force response seen for new mothers, or could be due to frictions in returning to work for older mothers.

Columns 1 to 2 of Table 3 shows the DD and DDD coefficients, which tell a very similar story. Early-exposed women continue to have 4 to 6 p.p. higher employment in the medium-term, but there is no significant difference in the long run. Given that mothers return to work sooner after childbirth, we next look at impacts on cumulative years of experience, which we calculate by adding the the estimated effects on employment. As a result, Columns 3 and 4 show that the effects on years of experience build primarily in the first 9 years, with effects between 0.72 years and 0.80 years in the long run. We note that these effects could be a lower bound if earlier return to work induces more full-time work among workers, instead of part-time work, and vice versa if it induces longer spells of part-time work. We will return to this in Section 6.1.

Figure 5 shows how these changes in labor market experiences map to earnings. The patterns show that early-exposed mothers earned increasingly more than late-exposed mothers in the first years after birth, an effect that is likely driven both by extensive and intensive

total effects on LFP.

[18]Similarly, Hotz and Scholz (2006) find no difference in the responses of 2- and 3-child families to the EITC reform using a household fixed effects model.

margin responses. This gap declined by roughly half by 10 years post-birth, mimicking the decline in the gap in employment but, unlike the employment results, effects on earnings remain positive and roughly constant over the long run. The final columns of Table 3 present the corresponding estimated effects on earnings. The DDD estimates indicate that earnings increase by $3,105 in the medium run and $2,022 in the long run. Relative to mean earnings for late-exposed women, these estimates imply that early-exposed women have 20% higher earnings in the medium run, and 9% higher in the long run.

There are several things worth noting about these results. First, the medium term effects on earnings are much larger in percent terms than the short-term effects (20% vs 12%), even though the employment effects are only slightly higher in the medium-run. The most likely explanation for this is that early-exposed women experience higher earnings *growth*, conditional on working, which leads to a widening of the gap in earnings. In particular, if we assume that the entirety of the increase in short-term earnings was due to more women working, then in the medium term, the difference in employment alone would justify an earnings gap of 14% [12%/5.4 · (6.3-5.4)]. The residual increase from 14% to 20%, then, is likely to reflect differences in earnings growth. Concretely, early-exposed women must have experienced 5% (1.2-1.14/1.14) higher earnings growth in order to explain the difference between short- and medium-run earnings.

Second, the effect on the level of earnings over the long term is relatively constant. This implies that mothers that enter the workforce earlier post-birth are not on a permanently steeper earnings trajectory. Hence, while mothers that return to work later do not seem to be able to recoup the post-birth loss in human capital, experience, etc. that arises from not working, they do not appear to face more adverse conditions than those that return earlier.

Third, since earnings continue to increase over the long term, the earnings advantage in percentage terms is declining. This underscores the fact that these initial gains are likely to reflect a fixed advantage that accumulates over the first ten years post birth, but does not continue to grow beyond that point. This may be due to the fact that the low-income mothers in our sample have less room for advancement, i.e. a ceiling effect, or face ongoing barriers that slow advancement, to give a few examples.

To unpack these effects, we next quantify the duration of leave avoided by early-exposed mothers. In particular, early exposure could have raised employment by reducing either frequent and shorter, or fewer and longer non-employment spells. To calculate the average duration of non-employment spells avoided by early exposure, we first estimate effects on the cumulative number of non-employment spells, where a non-employment spell is defined as a continuous span of years without positive income. Appendix Figure A.7 shows that the cumulative number of non-employment spells is 0.13 lower for early-exposed mothers.

Dividing the rise in years of experience by this, we find that average duration of avoided non-employment spells was 4.9 years [0.64/0.13]. This implies that our effects should be interpreted as the average effect of avoiding a relatively long period of non-employment post birth, which may include being on a steeper earnings-employment profile.

6.1 Mechanisms

Why may increased employment shortly after childbirth lead to long-term higher earnings? We start by considering whether early-exposed women might have experienced higher levels of earnings growth in the medium-term, greater hours of work or higher pay per hour. Unfortunately, our data do not include either of these variables, therefore we can only get suggestive evidence on these margins.

To examine hours of work, we take two complementary approaches. Descriptively, we compare hours of work in the CPS across early vs. late-exposed mothers by age of child. Column 1 of Appendix Table A.8 shows that early exposed mothers are more likely to work positive hours in the short and medium-run, but not in the long-run, consistent with our employment effects. Columns 2 and 3 show that these results are driven by part-time work ($<$ 35 hrs/wk) in the short-run and full-time work ($\geq$ 35 hrs/wk) in the medium one. Consistent with these, column 4 shows that the effect on the number of hours was larger in the medium run. This suggests that part of the higher earnings that we observe is likely to be due to early-exposed moms shifting from part-time to full-time work in the medium term.

Second, we develop a proxy for full-time work in the administrative data: whether earnings surpass the earnings corresponding to full-time work at the minimum wage.[19] Using CPS information on hours and weeks worked, we estimate that 60–75% of never-married mothers in our sample earned the minimum wage pre-birth. Hence, for the majority of women, this proxy will capture full-time work, although it is imperfect. To understand the drivers of wage growth, we focus on the sample of workers. Since this sample is changing over time, we interpret these results only as suggestive evidence. Appendix Figure A.9 shows that this measure of full time work increases roughly 5 years after a first birth, and disappears by 10 years after birth. This timing corresponds well with the descriptive evidence from the CPS, as well as the period of faster growth in earnings.

Taken together, these analyses provide a consistent picture that early-exposed moms shift into full-time work earlier, which likely contributes to the higher earnings that persist in the long run. This also implies that our estimates of growth in years of experience are

[19] We calculate this as the minimum wage in the state of residence $\times$ 35 hours per week *times* 40 weeks per year.

an underestimate of the gains in total hours of experience, since increases in full-time work imply more hours worked per year.

Next, we consider whether early-exposed women make different fertility choices. Since early-exposed women work more, but also have higher income, the predicted effect on number of children is ambiguous. Appendix Table A.9 shows no effect on fertility in the DD, but a reduction of 0.06 children in the DDD. However, while these are quantitatively different estimates, given that the decline in the DDD amounts to just a 4.5 percent reduction, both approaches yield the same qualitative conclusion of "little effect" on fertility.

7 Robustness

We now perform a number of robustness exercises to verify the interpretation of these medium- and long-term results as stemming from differences in post-birth work experience.

A first concern is that our samples based on marital status may be non-representative due to the fact that we are unable to observe marital status at the time of childbirth for most women. Since this measurement error will be worse for women (i) observed in later CPS surveys and (ii) who are observed a larger number of years after birth, we reestimate our main results using samples that drop the latest survey years (i.e 2016, 2015 and 2016, etc.) or restrict the sample to women observed within a fixed number of years of birth.

We present the coefficients from restricting the CPS surveys we use by the number of years a woman is observed after birth and by the CPS survey year, respectively, in Appendix Figures A.11 and A.10. If our effects were driven by positive selection of never-married women into the sample from measurement error, we would expect to see an upward trend in the coefficients over time. In general, the point estimates vary little across these sample, and while the coefficients are sometimes larger when we include more data, the effects are not meaningfully different than our results using more restricted samples.

Second, we test whether our effects are robust to using inverse p-score methods to balance observables across early- and late-exposed mothers. Appendix Table A.10 shows that imposing this kind of balance does not change our conclusions for the short- or long-term results.

Third, we use randomization inference to obtain confidence intervals. In particular, we randomly assign a placebo "early-exposure" to four randomly chosen years of birth drawn without replacement, and estimate a placebo effect for this sample. We do this 500 times for each of our main outcomes, and plot the resulting distribution of estimates in Figure A.14. The one-sided p-values for short-term employment and long-term earnings are between 0.01 and 0.02.

Fourth, we perform a similar set of sensitivity checks on the medium- and long-term earnings results as we did for the short term results. Appendix Table A.11 shows that the results can not be explained by individual characteristics, economic conditions, or the timing of welfare waivers. The last columns of the table show effects on log earnings, an alternative measure of returns in the labor market. This has the advantage of allowing for non-linearity in earnings growth over time, but raises typical questions about interpretation due to changes in selection into working over time. We continue to find strong effects for all DD specifications, but the DDD gives noisier, though positive effects over the long term. This could reflect the fact that we find small extensive margin effects in the DDD, which are not accounted for in the log specification. Nonetheless, the confidence intervals include the 8% effect that we estimate in our main earnings specification.

Fifth, we consider an alternative comparison of the early- and late-exposed mothers, in which we hold constant the calendar year and observe mothers with different-aged children – rather than vice versa, as in our main specification. This allows us to observe mothers at the same part of the business cycle, and eliminates potential concerns that the lasting effects are simply an artifact of observing early-exposed women at a more advantageous part of the business cycle. We estimate regressions of the form:

$$Y_{ibt} = \sum_{g} \sum_{k \neq g-1} \mathbb{1}(t = k) \times \mathbb{1}(g(b) = g) + \chi_b + \gamma X_{ist} + \epsilon_{ibt} \tag{3}$$

We group together early- and late- exposed mothers and include mothers that have a first birth in the two years before (86-87) and three years after (97-99) for context. Therefore, $g(b)$ represents the year of the earliest birth in the group that includes year of first birth b, and the year before g is the omitted category. We estimate coefficients for the five years before g up to 2012 for all women, and thus plot coefficients for the years 1981 to 2012. Appendix Figures A.12 and A.13 present the results for employment and real earnings. The effects on employment confirm that early- and late-exposed mothers end up with rates of working within 1 p.p. or so of each other, typically slightly above the pre-birth participation. Nonetheless, the graph of real earnings shows a roughly $2,000 gap between early- and late-exposed mothers. This reflects the fact that early-exposed mothers are able to return to pre-birth earnings, while late-exposed mothers do not.

8 Implications for the EITC

Taking a step back, we now examine the implications of the change in mother's labor market outcomes on EITC benefits. Early exposure to the 1993 EITC expansion could have

any direction of effect on EITC expenditures by affecting the likelihood that women work and conditional on working, whether they have earnings in the phase-in, flat, or phase-out regions of the EITC benefit distribution.

In order to examine effects on the phase-in and phase-out margins, we follow Chetty et al. (2013) and construct simulated measures of total EITC benefits, phase-in benefits, and phase-out benefits. Denote $r_{i\tau}$ as the 1-child phase-in rate, $r_{o\tau}$ as the 1-child phase-out rate, $z_{i\tau}$ as household earnings, and E_τ as the maximum credit τ years from birth. We simulate phase-in benefits as $\min(r_{it} \cdot z_{i\tau},\ E_t)$ and phase-out benefits as $\max(r_{o\tau} \cdot z_{i\tau},\ E_\tau)$. Total benefits are simply the EITC credit corresponding to household income in year τ.[20]

Additionally, to account for the fact that increases in the generosity of the EITC would be expected to mechanically increase benefits, we decompose changes in EITC benefits into the "mechanical" and "behavioral" response. In particular, we quantify the "behavioral" response by calculating an alternative set of simulated EITC measures that hold constant the EITC schedule experienced by mothers that had a first birth in 1994. In practice, this means that we calculate the level of total, phase-in, and phase-out benefits in year τ by using the EITC schedule for year t= 1994 + τ. Thus, if early-exposed and late-exposed mothers have the same labor market outcomes, they will have the same simulated EITC benefits (total, phase-in, and phase-out) using this measure. The "mechanical" change is then the difference between the total, phase-in, and phase-out benefits and the behavioral EITC benefits.

Table 4 presents the DD results for total, phase-in, and phase-out benefits in Panels A–C, respectively. Beginning with the short-term (0–4 year) effects, we estimate that early-exposed women receive $521 more in EITC benefits post-birth. This represents a substantial 50% increase in benefits post-birth relative to the mean. Decomposing this effect, 20% of the change in benefits is due to increases in earnings, while 80% is due to the expansion in the generosity of the program. When we analyze effects on phase-in and phase-out benefits separately, we see that the rise in EITC benefits is driven by a rise in women working in the phase-in region, and that this effect is partially offset by an increase in women in the phase-out region. We interpret the latter effect as growth in earnings that occurs after returning to work, since we find no change in density in the phase-out region in the first three years post-birth.

In the medium term, the difference between early- and late-exposed women is insignificant, which reflects two opposing effects in the phase-in and phase-out region: Benefits in

[20]For simplicity, we assign all women in the early-exposed and late-exposed groups the schedule experienced by the earliest birth in the group (i.e 1989 or 1994). In practice, since the most significant change during this period occurred in 1994, this makes little difference.

the phase-in region remain higher due to the employment effects seen in the short-run, but phase-out benefits decline further due to the fact that mothers experience important earnings growth during this period. Finally, over the long run, early exposed women receive $74 less in EITC benefits. Once the gap in employment closes between early- and late-exposed mothers, early-exposed women only have slightly higher phase-in benefits, but they have significantly lower phase-out benefits due to having higher earnings. The net effect is the observed decline in EITC benefits.

9 Conclusion

A large literature has documented that child birth leads to immediate and large drops in mothers' labor force participation and earnings, which persist up to 20 years later and could drive a large portion of the gender gap. However, there is little evidence on which policies, if any, can ameliorate these long-term earnings losses. In this paper, we exploit the expansion of the EITC in 1993, which provided a large subsidy to work for single mothers, to analyze the long-term effects of incentivizing work during the first few years after child birth.

Using large-scale SSA administrative data on earnings linked to CPS data on family characteristics, we compare women who first gave birth in 1993-1996, and thus were exposed to the EITC immediately after birth, to women whose first child was born in 1988-1991, and were thus exposed when their child was 3-6 years old, before and after birth. We do this both in a difference-in-differences specification using only never married women, who were likely eligible for the EITC, and in a DDD specification adding married women as an additional comparison group, as they were likely non-eligible.

Our results show that early-exposure to the EITC led to increased employment in the short run but not in the long-run, increased years of experience that accrued mainly in the medium-run, and increased earnings, which were larger in the short and medium run but persisted in the long-run as well. The magnitudes of these effects are significant, with a 3.7 to 5.4 p.p. increase in short-term LFP and a $2,000 increase in long-run earnings. When we analyze the mechanisms that could drive the gain in long-term earnings, we find suggestive evidence that the EITC led to a faster transition to full time work. Lastly, we analyze how incentivizing work among new mothers affects long-run program participation in the EITC. On net, early-exposure to the EITC post-birth leads to lower average long-term EITC benefits.

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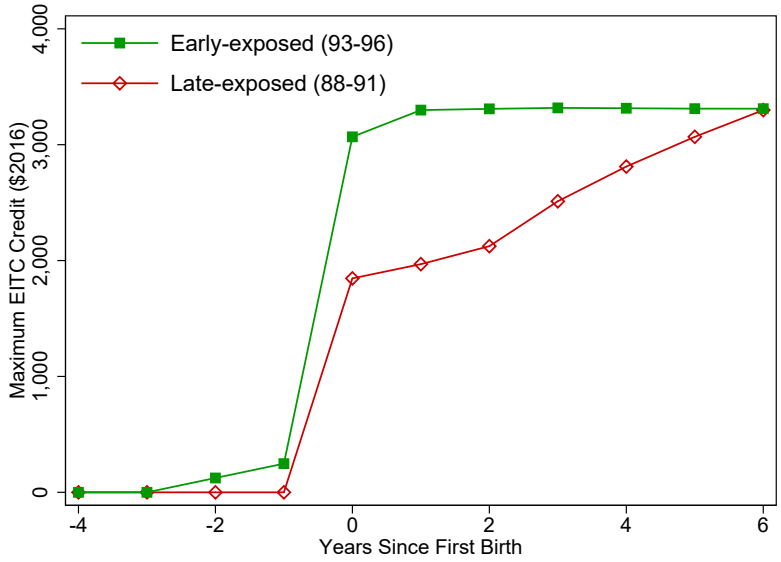
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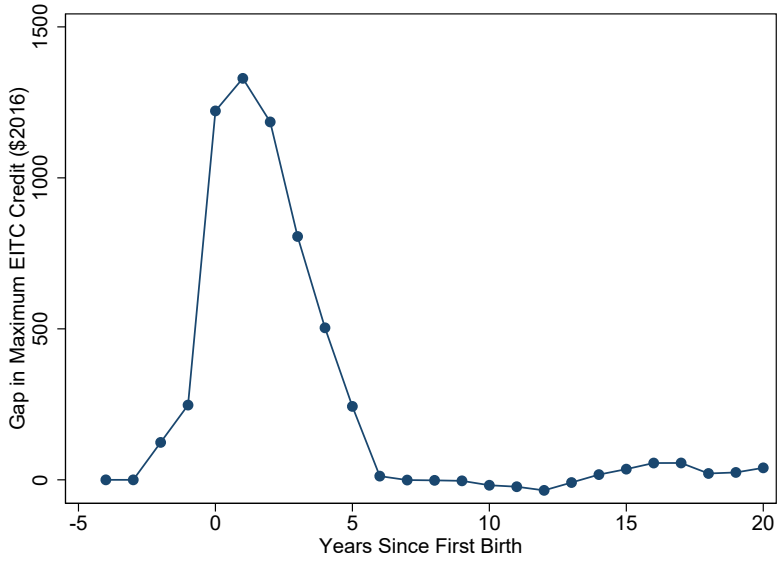
10 Figures

Figure 1: Maximum EITC Credit by Child Age, 1988-1996 Births

(a) Levels, Up to Age 6



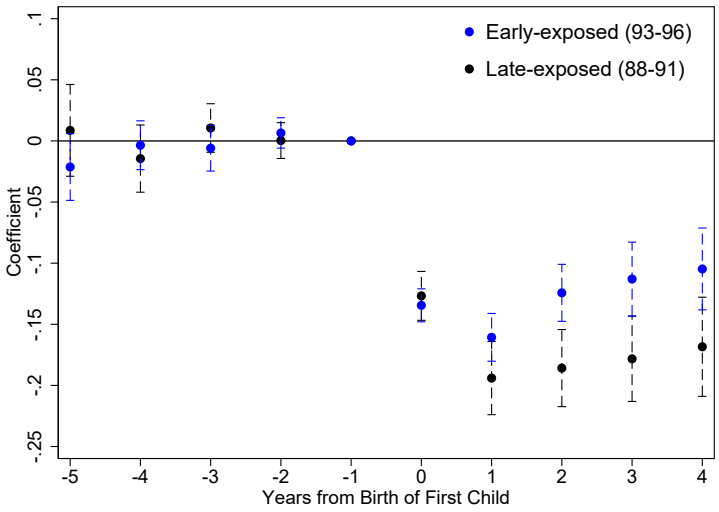
(a) Difference Between Early- and Late-Exposed, Up to Age 20



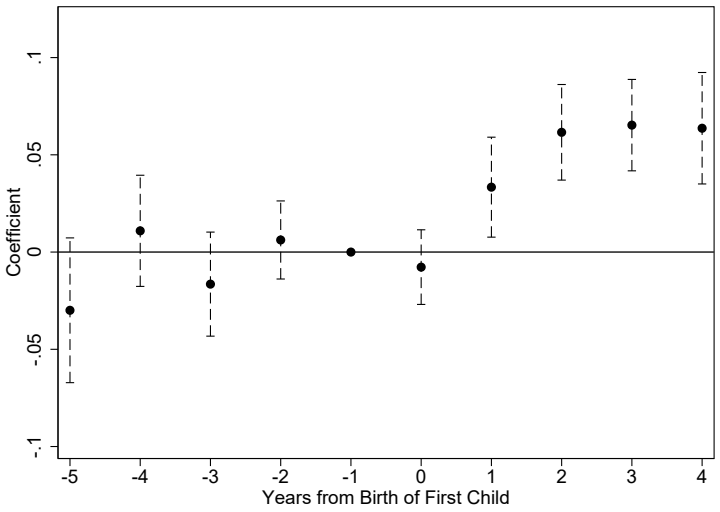
Notes: Panel (a) shows maximum EITC benefits at each year since birth, average across children born in 93-96 or 88-91. Panel (b) presents the difference in these benefits between the two groups. Data on nominal EITC benefits are from the Tax Policy Center. Data on CPI are from the BLS.

Figure 2: Effect of Child Birth on Short-Run Employment

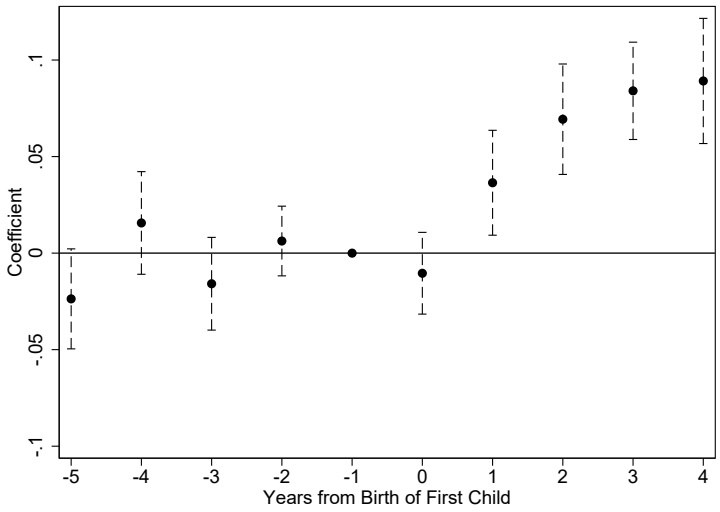
(a) Early- and Late-Exposed Never Married



(b) DD

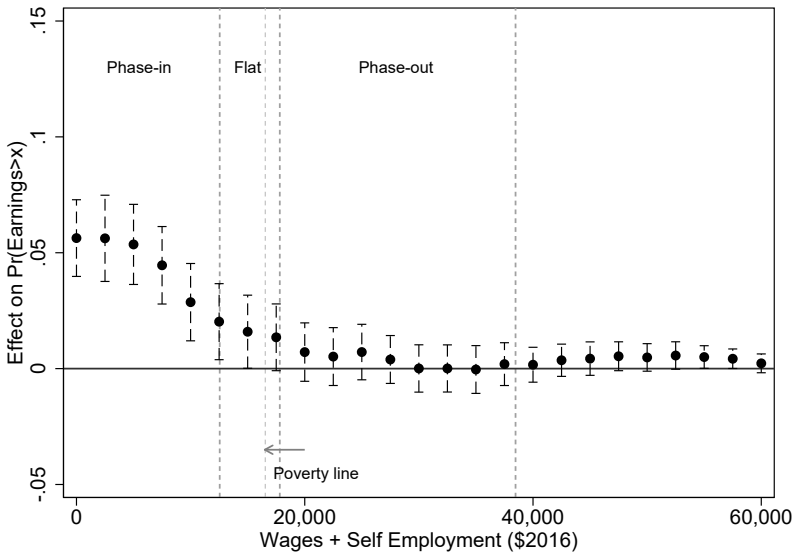


(c) DDD



Notes: These figures show coefficients and 95% confidence intervals from regressions estimating employment, defined as having positive annual earnings, by age of first child. Panel (a) presents effects for early EITC exposure (93-96 child births) vs late-exposure (88-91 child births) for never married mothers. Panel (b) presents the difference between the two, our dynamic difference-in-differences (DD). Panel (c) presents the DDD coefficients, which compare the DD for never-married relative to married mothers. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

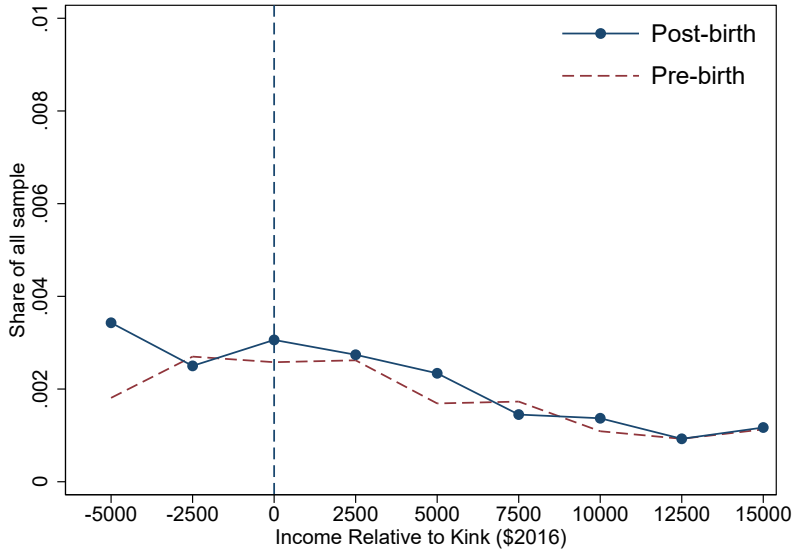
Figure 3: Effect of Work Incentives on Earnings Density – DDD



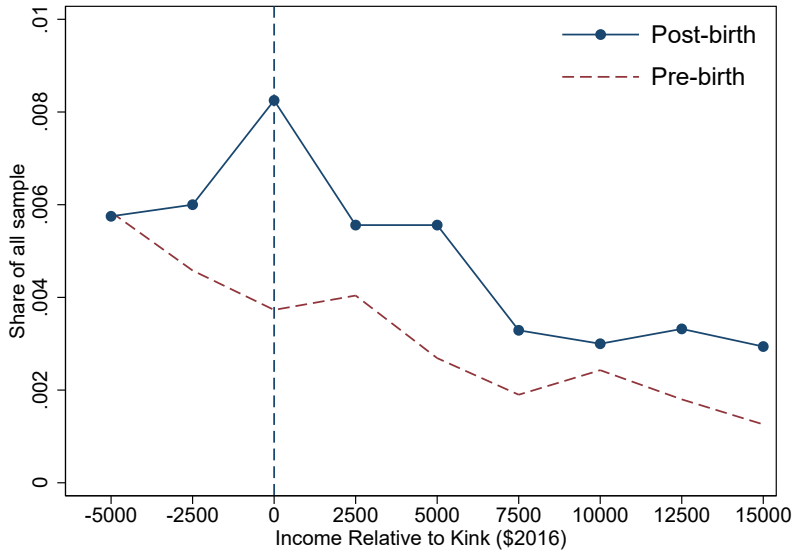
Notes: This figures show coefficients and 95% confidence intervals from DDD regressions that compare the labor market outcomes of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), for never-married vs. married mothers, by age of first child. Each circle is obtained from a different regression, where the outcome is an indicator for average annual earnings of at least X – the amount shown on the X-axis – during years 0-3 since birth. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group, allowed to differ by marital status. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records; nominal EITC benefits from the Tax Policy Center; CPI from the BLS. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure 4: EITC Expansion and Bunching Before and Post-Birth

(a) Late-Exposed



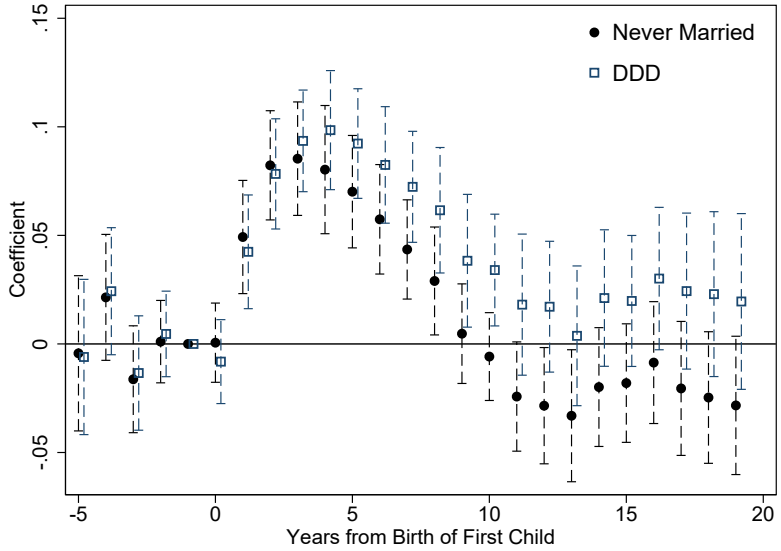
(b) Early-Exposed



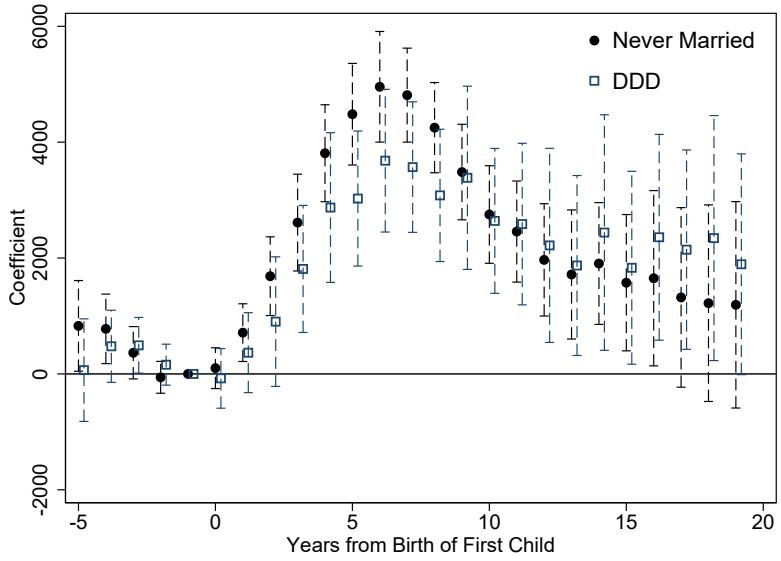
Notes: This figures shows the number of self-employed women with income in $2,500 ($2016) bins centered around the first EITC kink as a share of all late (Panel a) and early-exposed mothers (Panel b), separately for pre-birth and post-birth. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records; nominal EITC benefits from the Tax Policy Center; CPI from the BLS. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure 5: Effect of Work Incentives on Long-Run Outcomes

(a) Employment



(b) Annual Earnings



Notes: These figures show coefficients and 95% confidence intervals from regressions comparing the labor market outcomes of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. For each outcome we present both the dynamic difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as control group for never married ones. Panel (a) contains estimates for employment, defined as having positive annual earnings, and Panel (b) for annual earnings. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

11 Tables

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

	Max 1 Child	LFP (Earnings>0)			Wage Earnings>0		
	EITC ($2016)	Never Married	Married	DDD	Never Married	Married	DDD
PostBirth * 93-96	1009.175	0.037***	-0.001		0.032***	-0.004	
		(0.009)	(0.005)		(0.009)	(0.005)	
PostBirth * 93-96 * NM				0.054***			0.052***
				(0.011)			(0.011)
Mean Y		0.682	0.748	0.733	0.677	0.731	0.719
Individuals		112910	369210	482120	112910	369210	482120

Notes: This table shows results from regressions comparing the labor market outcomes of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. For each outcome we present both the dynamic difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones. In the first column, the outcome of interest is the maximum EITC benefit the mother is eligible for, columns (2) to (4) present results where the outcome is an indicator for positive annual earnings, and in the remaining columns the outcome is an indicator for positive annual wages (excluding self-employment earnings). All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Table 2: Effect of Work Incentives on Short-Run Employment – Sensitivity to Alternative Specifications

		Add Controls:		Sample:	Add UR and Welfare Controls		Late Reform States	
	Base	AFB*YSB	Ind FE	Heads	UR Dyn	(Ref+Waivs)*Dyn	All Years	Up to 1996
A: Never Married								
PostBirth * 93-96	0.037***	0.036***	0.033***	0.041***	0.034***	0.028***	0.034***	0.022*
	(0.009)	(0.009)	(0.009)	(0.010)	(0.008)	(0.009)	(0.009)	(0.011)
Mean Y	0.682	0.682	0.682	0.688	0.682	0.682	0.645	0.625
Individuals	112910	112910	112910	89220	112910	112910	30730	26371
B: DDD								
PostBirth * 93-96 * NM	0.054***	0.054***	0.050***	0.057***	0.045***	0.054***	0.067***	0.026*
	(0.011)	(0.011)	(0.010)	(0.012)	(0.011)	(0.011)	(0.019)	(0.014)
Mean Y	0.733	0.733	0.733	0.742	0.733	0.733	0.717	0.712
Individuals	482120	482120	482120	441610	482120	482120	121240	104840

Notes: This table shows results from regressions comparing the employment, defined as having positive annual earnings, of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. Panel (a) presents the dynamic difference-in-differences using never married mothers only, and Panel (b) contains the DDD estimates obtained when we add married women as a control group for never married ones. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. On top of our baseline controls, we also include: age-at-birth by years-since-birth fixed effects in Column (2); individual fixed effects in Column (3); interactions between the unemployment rate and indicators for years-since-birth in Column (5); indicators for the presence of 6 different types of state waivers and the adoption of welfare reform, allowed to differ pre and post-birth, in Column (6). Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview. Column (4) restricts the sample to heads of household, Column (7) and (8) restrict it to mothers living in states adopting welfare reform after 1996, and Column (8) further restricts to observations up to year 1996.

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

	LFP (Earnings>0)		Years of Experience		Earnings ($2016)	
	Never Married	DDD	Never Married	DDD	Never Married	DDD
PostBirth * 93-96 * 5-9 Yrs From Birth	0.041***		0.827***		4129.153***	
	(0.009)		(0.091)		(325.598)	
PostBirth * 93-96 * 10+ Yrs From Birth	-0.007		0.802***		1637.988***	
	(0.011)		(0.114)		(542.795)	
PostBirth * 93-96 * Yrs 5-9 * NM		0.063***		0.640***		3105.403***
		(0.011)		(0.092)		(535.049)
PostBirth * 93-96 * 10+ Yrs From Birth * NM		0.018		0.718***		2022.808***
		(0.012)		(0.136)		(742.784)
Mean Y	0.761	0.761	10.193	11.901	16989.762	22160.623
Individuals	282275	1205300	282275	1205300	282275	1205300

Notes: This table shows results from regressions comparing the labor market outcomes of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. For each outcome we present both the dynamic difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones. The outcome of interest is employment, defined as having positive annual earnings, in the first two columns, years of experience in columns (3) and (4), and annual earnings in the final two columns. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Table 4: Effect of Work Incentives on EITC Benefits

	Total	Behavioral	Mechanical
A: EITC Benefits ($2016)			
0-4 years	521.107***	110.746***	410.361***
	(31.800)	(29.465)	(14.855)
5-9 years	-19.265	-34.900	15.635***
	(29.664)	(29.939)	(1.057)
10+ years	-74.038***	-77.422***	3.384**
	(21.475)	(21.839)	(1.421)
Mean 0-4	947.578	1127.783	-180.205
Mean 5-9	1190.084	1198.547	-8.463
Mean 10+	1098.978	1103.429	-4.451
B: Phase-in Benefits ($2016)			
0-4 years	982.436***	247.671***	734.765***
	(34.110)	(38.658)	(20.141)
5-9 years	296.494***	278.639***	17.855***
	(38.106)	(38.700)	(1.652)
10+ years	56.523*	52.635	3.888**
	(31.013)	(31.846)	(1.606)
Mean 0-4	1749.461	2070.070	-320.609
Mean 5-9	2504.436	2513.131	-8.695
Mean 10+	2667.881	2673.179	-5.298
C: Phase-out Benefits ($2016)			
0-4 years	817.404***	-136.925***	954.329***
	(23.458)	(27.821)	(16.408)
5-9 years	-306.781***	-313.539***	6.758
	(27.784)	(27.718)	(4.181)
10+ years	-126.208***	-130.057***	3.849*
	(34.561)	(34.380)	(2.247)
Mean 0-4	1954.110	2365.469	-411.359
Mean 5-9	1996.485	1998.665	-2.180
Mean 10+	1736.985	1743.053	-6.067
N	282275	282275	282275

Notes: This table shows results from regressions comparing EITC benefits of never married mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births) – our dynamic difference-in-differences – by age of first child. The outcomes of interest are eligibility for EITC benefits in the whole EITC region (Panel a), in the phase-in region only (Panel b), and in the phase-out region (Panel c). Column (1) contains results for actual EITC eligibility, in Column (2) we analyze an alternative set of simulated EITC measures that hold constant the 1994 EITC schedule, and in Column (3) we analyze the difference between EITC eligibility in the first two columns. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

A Supplemental Tables and Figures

Table A.1: Pre-Birth Characteristics – By Marital Status

	Never Married	Married
Share Non-White	0.522	0.202
	(0.500)	(0.401)
Age at First Birth	23.61	26.30
	(4.393)	(4.567)
HH EITC Eligibility Pre-Birth	0.871	0.515
	(0.335)	(0.500)
Secondary Earner Pre-Birth	–	0.601
	–	(0.490)
HH EITC 0-4 Yrs Post-Birth ($2106, 94 Sched.)	1260.1	564.0
	(1310.8)	(1046.7)
Share High School or Less	0.557	0.349
	(0.497)	(0.477)
Any Earnings Pre-Birth	0.894	0.898
	(0.308)	(0.303)
Mean of Any Earnings Pre-Birth	0.660	0.775
	(0.474)	(0.417)
Mean Earnings if Working ($2016) Pre-Birth	12073.7	24289.1
	(14264.9)	(25368.7)
Unique Women	11291	36921
Observations	282275	923025

Notes: This table shows summary statistics for our never married and married samples, respectively. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Table A.2: Sample Characteristics by CPS Survey

	Never-Married	
	1996	2016
Share Non-White	0.386	0.522
Age at First Birth	23.00	23.61
HH EITC Eligibility Pre-Birth	0.780	0.871
Share High School or Less	0.623	0.557
Any Earnings Pre-Birth	0.922	0.894
Mean of Any Earnings Pre-Birth	0.677	0.660
Mean of Any Earnings 0-4 yrs Post-Birth	0.704	0.705
Mean of Any Earnings 5-9 yrs Post-Birth	0.807	0.812
Mean of Any Earnings 10+ yrs Post-Birth	0.806	0.815
Years of Experience Pre-Birth	3.711	4.012
Years of Experience 0-4 yrs Post-Birth	7.230	7.537
Years of Experience 5-9 yrs Post-Birth	11.27	11.60
Years of Experience 10+ yrs Post-Birth	19.33	19.74
Mean Earnings ($2016) Pre-Birth	7452.3	7969.9
Mean Earnings ($2016) 0-4 yrs Post-Birth	10956.8	11656.9
Mean Earnings ($2016) 5-9 yrs Post-Birth	17211.8	18271.2
Mean Earnings ($2016) 10+ yrs Post-Birth	22867.3	23525.4
Mean Earnings if Working ($2016) Pre-Birth	11016.2	12073.7
Mean Earnings if Working ($2016) 0-4 yrs Post-Birth	15565.8	16577.9
Mean Earnings if Working ($2016) 5-9 yrs Post-Birth	21472.5	22715.5
Mean Earnings if Working ($2016) 10+ yrs Post-Birth	28967.2	29558.0
Unique Women	1289	11291
Observations	32225	282275

Notes: This table shows summary statistics for our sample of never married women restricting the data to the CPS's up to 1996 (1991, 1994, 1996) and using all of the CPS's (1991, 1994, 1996–2000 and 2002–2016 ASEC CPS). Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Table A.3: Pre- and Post-Birth Characteristics of Never Married Women – By Early- or Late-Exposure to EITC Expansion

	All	Late Exposure (88-91)	Early Exposure (93-96)	P-value
A: Pre-Birth Outcomes				
Share Non-White	0.522	0.546	0.502	<0.01
	(0.500)	(0.498)	(0.500)	
Age at First Birth	23.61	23.54	23.67	0.170
	(4.393)	(4.173)	(4.557)	
HH EITC Eligibility Pre-Birth	0.871	0.875	0.869	0.349
	(0.335)	(0.331)	(0.338)	
Secondary Earner Pre-Birth	–	–	–	–
	–	–	–	–
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. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were never married at the time of the CPS interview.

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

	Never Married			Married		
	All	Eligible	Non-Eligible	All	Eligible	Non-Eligible
PostBirth * 93-96	0.037***	0.042***	0.005	-0.001	0.000	-0.006
	(0.009)	(0.011)	(0.026)	(0.005)	(0.006)	(0.006)
Mean Y	0.682	0.661	0.825	0.748	0.637	0.866
Individuals	112910	98380	14530	369210	190020	179190

Notes: This table shows results from regressions comparing the employment of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births) in the first 5 years since the birth of a first child, by EITC eligibility pre-birth. We present both the difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones.All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

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

	Self-Employed Earnings >0		Bunching	
	Never Married	DDD	Never Married	DDD
PostBirth * 93-96	0.010***		0.010***	
	(0.003)		(0.004)	
PostBirth * 93-96 *NM		0.010***		0.009**
		(0.003)		(0.003)
Mean Y	0.013	0.031	0.032	0.029
Individuals	112910	482120	112910	482120

Notes: This table shows results from regressions comparing the labor market outcomes of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births) in the first 5 years since the birth of a first child. For each outcome we present both the difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones. Columns (2) and (3) present results where the outcome is an indicator for positive self-employment earnings, and in the remaining two columns the outcome is an indicator for bunching, defined as having earnings at the EITC kink. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Table A.6: Effect of Work Incentives on Short-Run Employment – By Birth Parity

	Never Married		
	2+ vs 1	3+ vs 1	3+ vs 2
PostBirth * 93-96 * Child 2+	0.034**		
	(0.014)		
PostBirth * 93-96 * Child 3+		0.045**	0.013
		(0.020)	(0.021)
Mean Y	0.647	0.655	0.583
Individuals	174050	131160	61140

Notes: This table shows results from regressions comparing employment of never married mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births) and among mothers with different birth parity, by age of child. Column (1) presents the different impact on mothers with a second (or more) vs first child, Column (2) the different impact on mothers with a third (or more) vs first child, and Column (3) the different impact on mothers with a third (or more) vs second child. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose child was born in 1988–1991 or 1993-1996 and who were at least 19 or older with their first child, women who were aged 15-49 and were never married at the time of the CPS interview.

Table A.8: Effect of Work Incentives on Hours of Work – CPS

	Hours Worked Last Week			
	Positive	Full Time	Part Time	Level
Panel A: Never Married				
PostBirth * EarlyExp * Yrs 0-4	0.081*	0.003	0.078***	2.706
	(0.042)	(0.037)	(0.029)	(1.725)
PostBirth * EarlyExp * Yrs 5-9	0.098***	0.102***	-0.004	4.537***
	(0.029)	(0.031)	(0.032)	(1.200)
PostBirth * EarlyExp * Yrs 10+	-0.027	-0.017	-0.010	-0.935
	(0.031)	(0.034)	(0.027)	(1.440)
Mean Y	0.659	0.473	0.186	24.279
Individuals	9923	9923	9923	9923
Panel B: DDD				
PostBirth * EarlyExp * Yrs 0-4 * NM	0.059	-0.000	0.059*	1.849
	(0.043)	(0.038)	(0.030)	(1.788)
PostBirth * EarlyExp * Yrs 5-9 * NM	0.052	0.086**	-0.033	3.340**
	(0.033)	(0.035)	(0.036)	(1.297)
PostBirth * EarlyExp * Yrs 10+ * NM	-0.007	-0.005	-0.002	-0.136
	(0.030)	(0.036)	(0.029)	(1.490)
Mean Y	0.694	0.450	0.243	24.426
Individuals	94494	94494	94494	94494

Notes: This table shows the difference in working for a positive amount of hours, full time ($\geq$ 35 hours per week), part time, and the amount of hours between early- and late-exposed women. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were never married or married at the time of the CPS interview.

Table A.7: Effect of Work Incentives on Short-Run Employment – Adding Future Moms as Comparison

	Baseline		0-3 Yrs Post			
	Never-Married	DDD	Never-Married	DDD	No Kids	DDD
A: LFP (Earnings >0)						
PostBirth * 93-96	0.037***		0.032***		0.010	
	(0.009)		(0.009)		(0.009)	
PostBirth * 93-96 *NM		0.011		0.009		
		(0.008)		(0.008)		
PostBirth * 93-96 *Kids						0.043***
						(0.011)
NM Moms	Yes	Yes	Yes	Yes	No	Yes
Married Moms	No	Yes	No	Yes	No	No
NM Future Moms	No	No	No	No	Yes	Yes
N	112910	482120	101619	433908	78828	146574

Notes: This table shows results from regressions comparing short-run (0-4 years after birth) employment of never married mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births) vs. future mothers. Column (1) and (2) present our baseline DD and DDD estimates, columns (3) and (4) repeat these results but for the outcome measured 0-3 years after birth, and columns (5) and (6) use future moms as a comparison group. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose child was born in 1988–1991 or 1993-1996 and who were at least 19 or older with their first child, women who were aged 15-49 and were never married at the time of the CPS interview.

Table A.9: Effect of Work Incentives on Number of Children

	0-4 Yrs	5-9 Yrs	10+ Yrs
A: Never Married			
PostBirth * 93-96 * X Yrs From Birth	-0.018	0.003	0.003
	(0.011)	(540.074)	(0.030)
Mean Y	0.601	1.309	1.309
Individuals	112910	282275	282275
B: DDD			
PostBirth * 93-96 * NM * X Yrs From Birth * NM	-0.032**	-0.063***	-0.058*
	(0.012)	(0.023)	(0.033)
Mean Y	0.639	1.479	1.479
Individuals	482120	1205300	1205300

Notes: This table shows impacts on number of children in the household for the DD (Panel A) and DDD (Panel B) specifications. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were never married at the time of the CPS interview.

Table A.10: Effect of Work Incentives on Labor Market Outcomes – Sensitivity to Inverse P-Score Reweighting

	LFP (Earnings > 0)		Earnings ($2016)	
	Never-Married	DDD	Never-Married	DDD
PostBirth * 93-96 * 0-4 Yrs From Birth	0.062***		1214.150***	
	(0.010)		(259.722)	
PostBirth * 93-96 * 5-9 Yrs From Birth	0.051***		3686.216***	
	(0.009)		(314.470)	
PostBirth * 93-96 * 10+ Yrs From Birth	0.005		993.482*	
	(0.011)		(524.929)	
PostBirth * 93-96 * Yrs 0-4 * NM		0.067***		570.632
		(0.010)		(409.061)
PostBirth * 93-96 * Yrs 5-9 * NM		0.058***		2463.734***
		(0.011)		(523.062)
PostBirth * 93-96 * 10+ Yrs From Birth * NM		0.013		1283.114*
		(0.012)		(739.300)
N	282275	1205300	282275	1205300

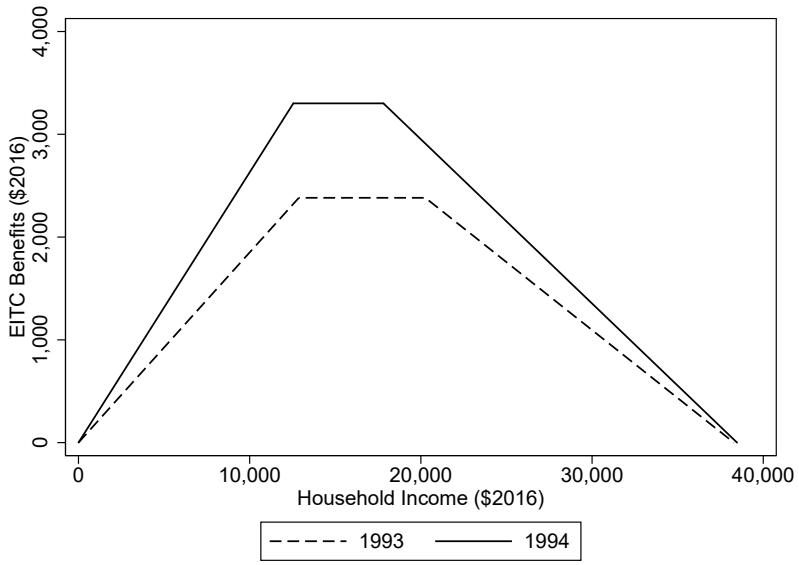
Notes: This table shows results from regressions comparing the labor market outcomes of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. For each outcome we present both the dynamic difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones. The outcome of interest is employment, defined as having positive annual earnings, in the first two columns, and annual earnings in the other two columns. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state, and all regressions are reweighted so that late and early-exposed mothers are similar on pre-birth characteristics. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Table A.11: Effect of Work Incentives on Long-Run Earnings – Sensitivity to Alternative Specifications

		Add Controls:			Sample:		Welfare Controls:	Ln Earnings:	
	Base	AFB*YSB	UR Dyn	Ind FE	Elig.	Heads	(Ref+Waivs)*Dyn	All	Elig.
A: Never Married									
PostBirth * 93-96 * 5-9 Yrs From Birth	4129.153***	4145.375***	4181.917***	4097.802***	4135.796***	4282.402***	3919.267***	0.268***	0.267***
	(325.598)	(316.181)	(361.754)	(314.939)	(294.354)	(395.573)	(432.853)	(0.031)	(0.033)
PostBirth * 93-96 * 10+ Yrs From Birth	1637.988***	1665.836***	1556.688***	1618.696***	1541.315***	2095.930***	1531.461***	0.081**	0.076**
	(542.795)	(512.653)	(554.919)	(529.207)	(533.490)	(660.958)	(570.126)	(0.032)	(0.034)
Mean Y	16989.762	16989.762	16989.762	16989.762	15190.576	17244.022	16989.762	9.397	9.343
Indivs.	282275	282275	282275	282275	245950	223050	282275	282275	245950
B: DDD									
PostBirth * 93-96 * Yrs 5-9 * NM	3105.403***	2797.430***	2967.240***	3033.749***	2153.895***	3228.796***	3083.665***	0.142***	0.140***
	(535.049)	(520.123)	(516.213)	(535.518)	(479.432)	(616.074)	(526.048)	(0.034)	(0.040)
PostBirth * 93-96 * 10+ Yrs From Birth * NM	2022.808***	1635.713**	1855.882**	1854.518**	1457.288*	2424.758***	2040.455***	0.035	0.079**
	(742.784)	(710.083)	(699.421)	(760.090)	(766.680)	(1756.173)	(732.070)	(0.033)	(0.039)
Mean Y	22160.623	22160.623	22160.623	22160.623	15625.718	31889.440	22160.623	9.686	9.409
Indivs.	1205300	1205300	1205300	1205300	721000	484300	1205300	1205300	721000

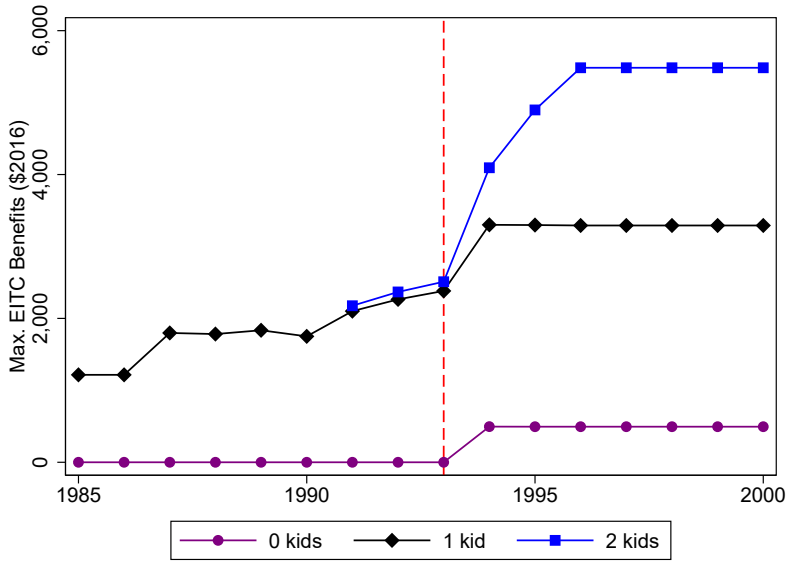
Notes: This table shows results from regressions comparing annual earnings of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. Panel (a) presents the dynamic difference-in-differences using never married mothers only, and Panel (b) contains the DDD estimates obtained when we add married women as a control group for never married ones. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. On top of our baseline controls, we also include: age-at-birth by years-since-birth fixed effects in Column (2); individual fixed effects in Column (3); interactions between the unemployment rate and indicators for years-since-birth in Column (5); indicators for the presence of 6 different types of state waivers and the adoption of welfare reform, allowed to differ pre and post-birth, in Column (6). Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview. Column (4) restricts the sample to heads of household, Column (7) and (8) restrict it to mothers living in states adopting welfare reform after 1996, and Column (8) further restricts to observations up to year 1996.

Figure A.1: EITC Schedule for Individuals with One Child – 1993 vs. 1994



Notes: This figure shows EITC benefits at each earnings level for individuals with one child, in 1993 and in 1994. Data on nominal EITC benefits are from the Tax Policy Center.

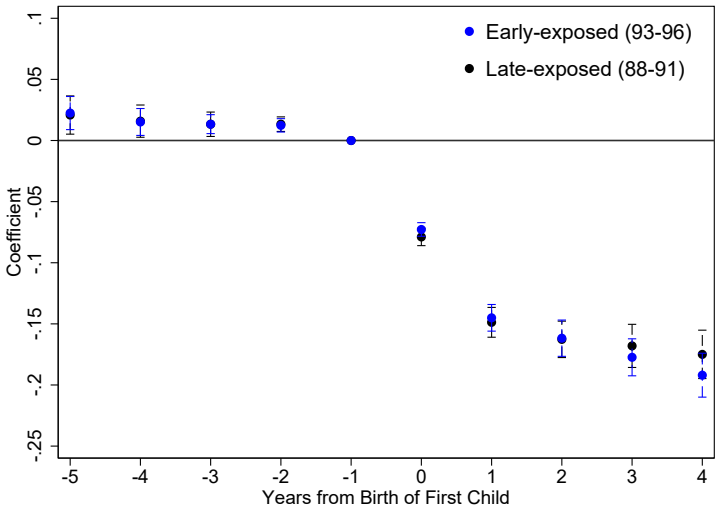
Figure A.2: Maximum EITC Benefits by Number of Children



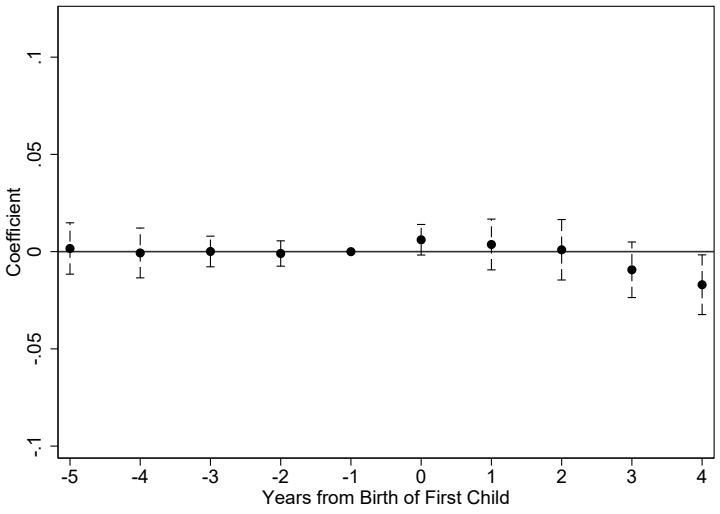
Notes: This figure shows real maximum EITC benefits over time and by number of children, in 1993 and in 1994. Data on nominal EITC benefits are from the Tax Policy Center. Data on CPI are from the BLS.

Figure A.3: Effect of Child Birth on Short-Run Employment – Married

(a) Early- and Late-Exposed

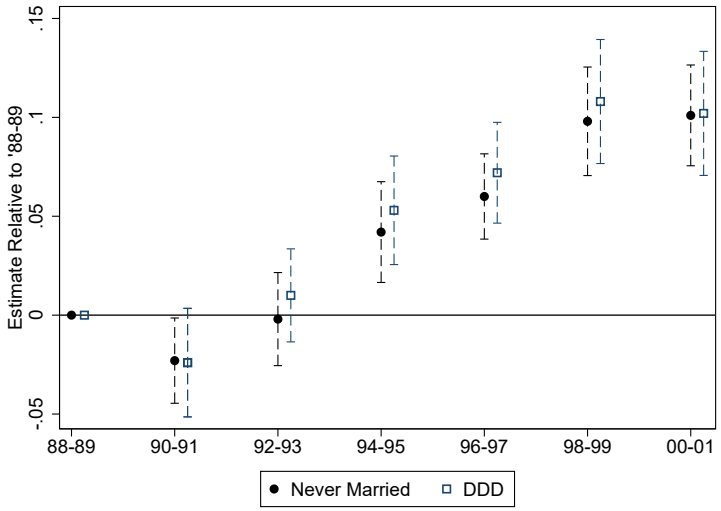


(b) Difference



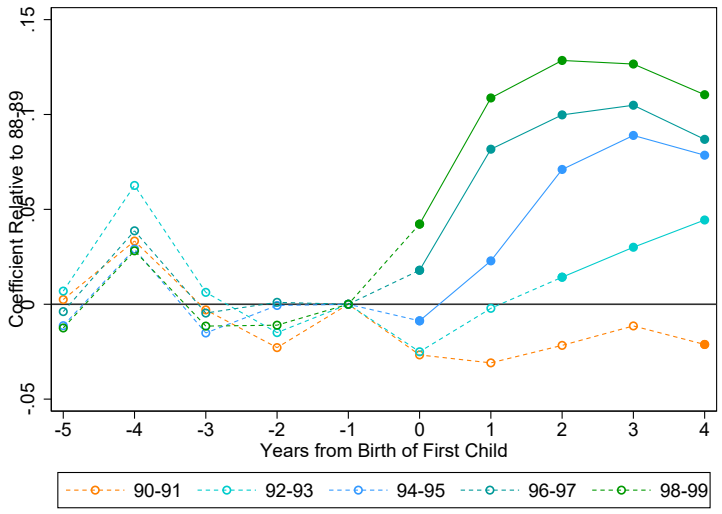
Notes: These figures show coefficients and 95% confidence intervals from regressions estimating employment, defined as having positive annual earnings, by age of first child among married mothers. Panel (a) presents effects for early EITC exposure (93-96 child births) vs late-exposure (88-91 child births) separately, while Panel (b) presents the difference between the two, our dynamic difference-in-differences (DD). All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were married at the time of the CPS interview.

Figure A.4: Effect of EITC Expansion on Short-Run Employment – By Child Cohort



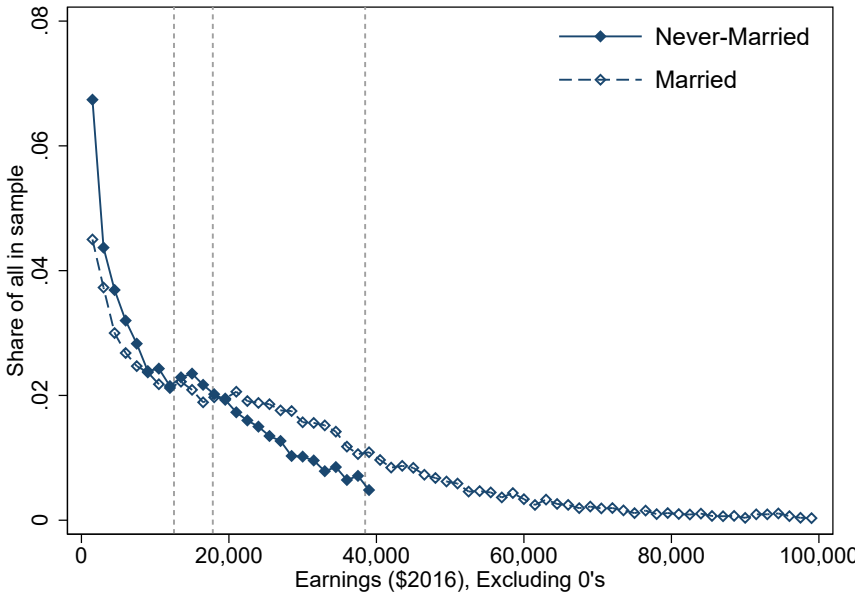
Notes: These figures show coefficients and 95% confidence intervals from regressions estimating differential employment, defined as having positive annual earnings, for two-year child cohorts relative to mothers with a first child in years 88-89. We present both the dynamic difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure A.5: Effect of EITC Expansion on Short-Run Employment – By Child Cohort and Age



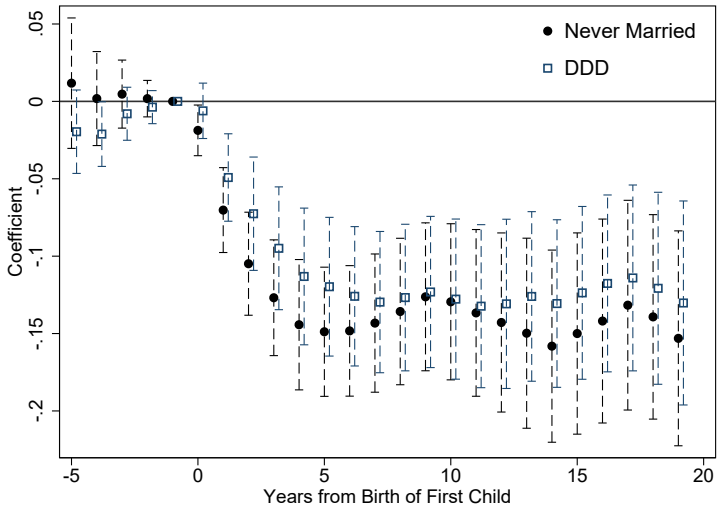
Notes: These figures show coefficients and 95% confidence intervals from regressions estimating never married mothers' employment, defined as having positive annual earnings, by child age for two-year child cohorts relative to mothers with a first child in years 88-89. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were never married at the time of the CPS interview.

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



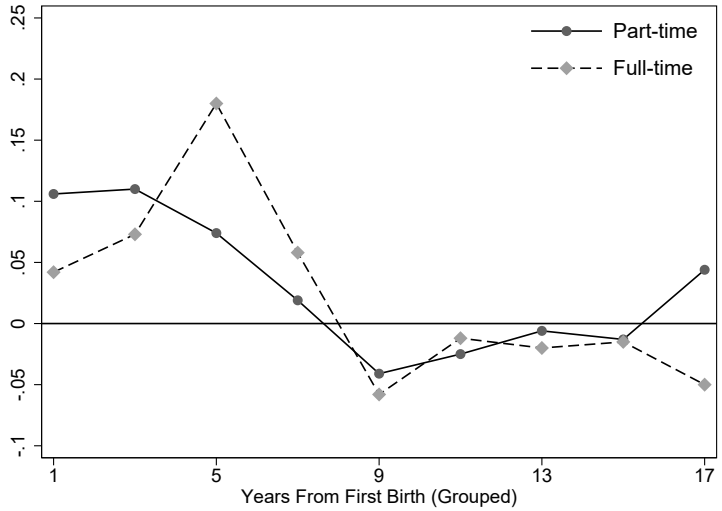
Notes: This figure shows the distribution of earnings, excluding 0's, zero to 3 years post birth for never-married and married mothers that were late-exposed to the EITC reform (gave birth between 1988 and 1991.) Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure A.7: Effect of EITC Expansion on Long-Run Non-employment Spells



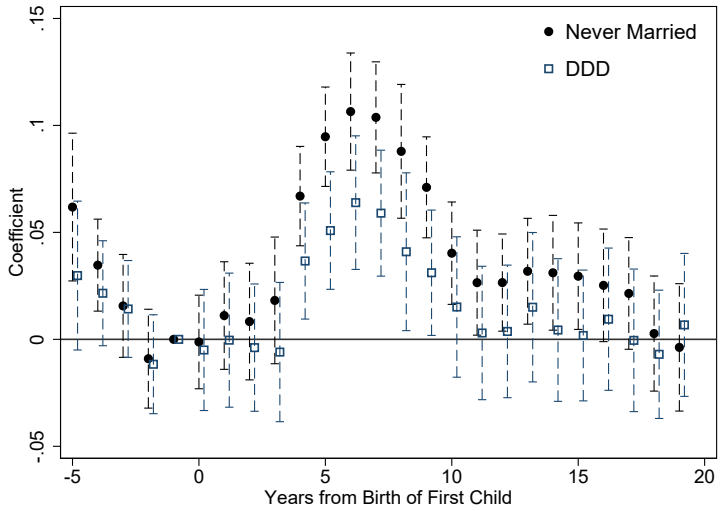
Notes: This figure shows results from regressions comparing the cumulative number of non-employment spells of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. We present both the dynamic difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure A.8: Effect of EITC Expansion on Part and Full-Time Work



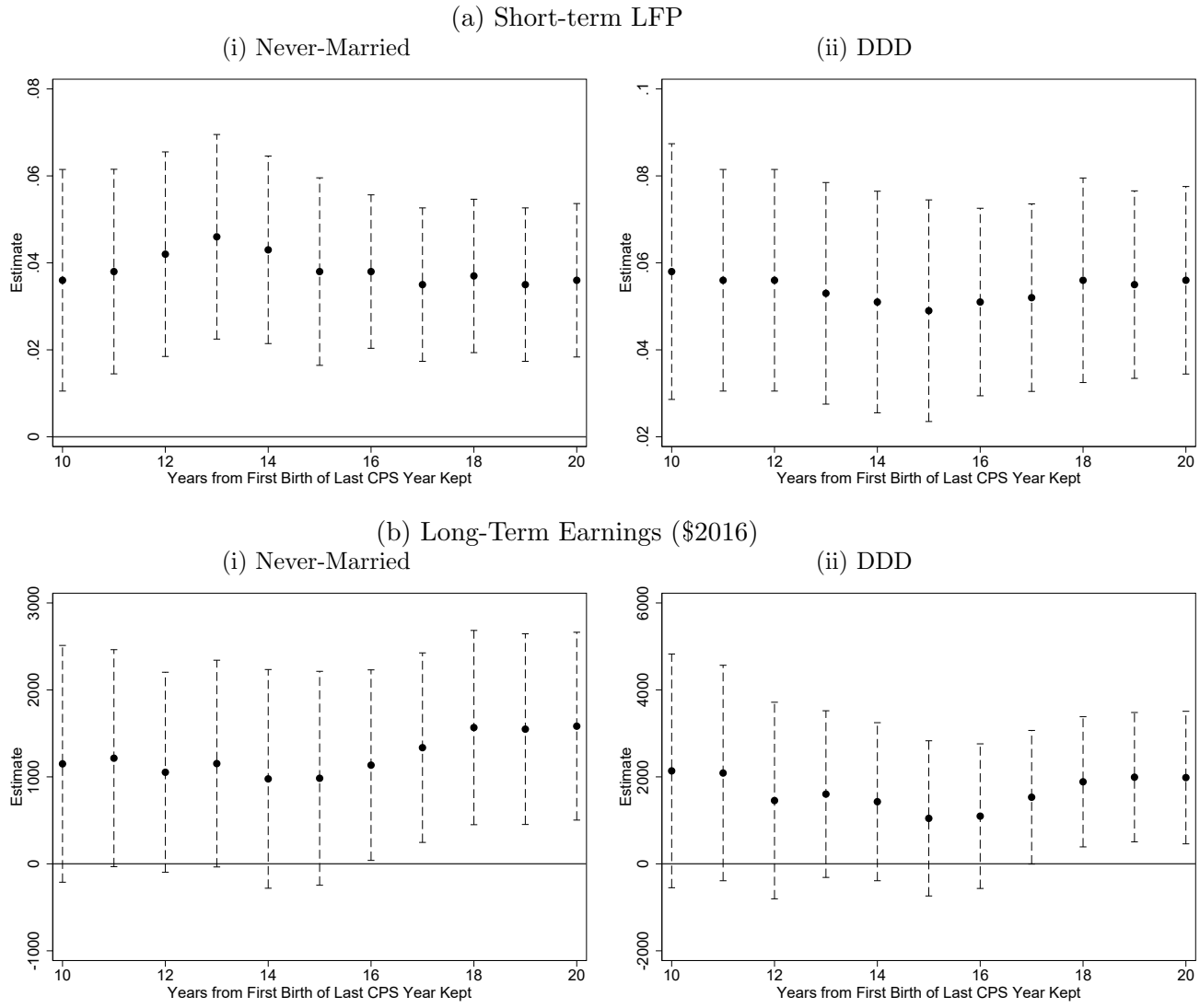
Notes: This tables shows results from regressions comparing the likelihood of part and full-time work of never-married mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure A.9: Effect of EITC Expansion on Propensity to Earn Full-Time Full-Year Minimum Wage, Conditional on Working



Notes: This figure shows results from regressions comparing the propensity to earn full-time full-year minimum wage, conditional on working, of never-married mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births), by age of first child. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure A.10: Effect of EITC Expansion on Labor Market Outcomes – Sensitivity to Using CPS Years within 10 to 20 Years of Birth

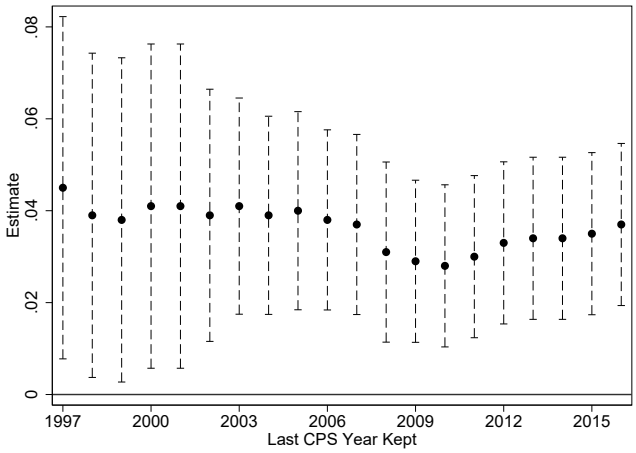


Notes: These figures shows results from regressions comparing the labor market outcomes of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births). For each outcome we present both the dynamic difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones. The outcomes of interest are employment, defined as having positive annual earnings, in Panel (a), and annual earnings in Panel (b). All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state, and all regressions are reweighted so that late and early-exposed mothers are similar on pre-birth characteristics. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

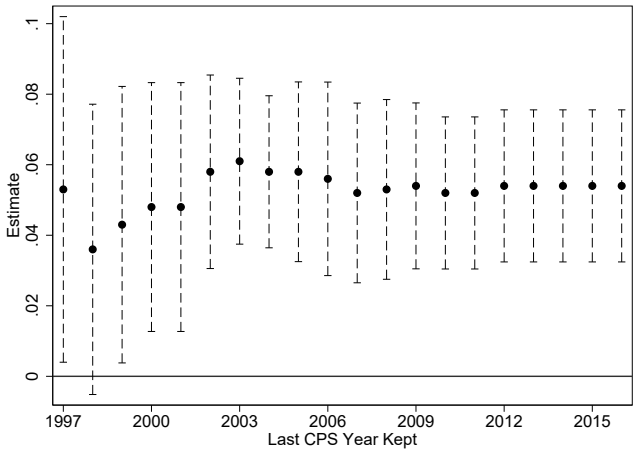
Figure A.11: Effect of EITC Expansion on Labor Market Outcomes – Sensitivity to Dropping Later CPS Years

(a) Short-term LFP

(i) Never-Married

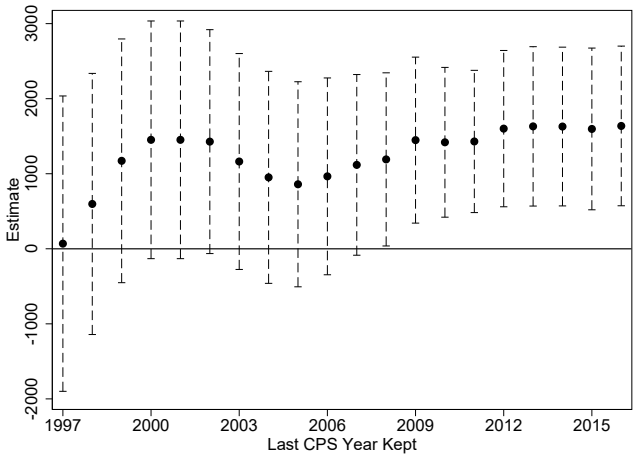


(ii) DDD

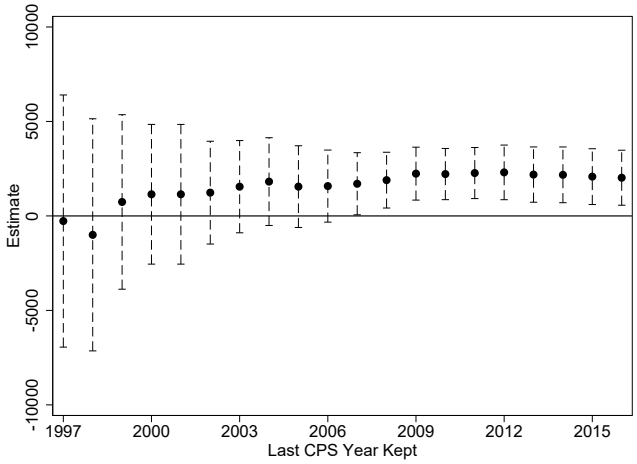


(b) Long-Term Earnings ($2016)

(i) Never-Married

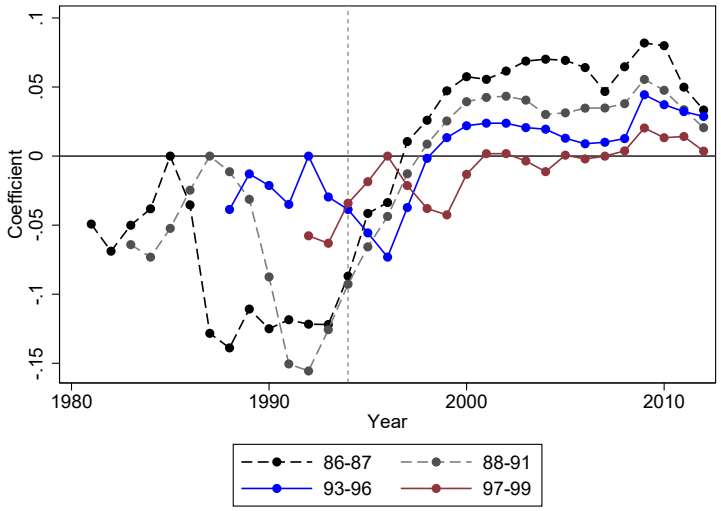


(ii) DDD



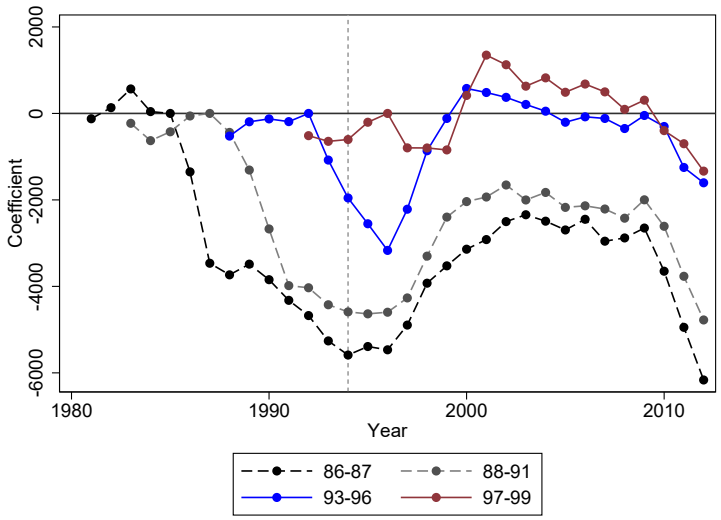
Notes: These figures shows results from regressions comparing the labor market outcomes of mothers with early EITC exposure (93-96 child births) vs late-exposure (88-91 child births). For each outcome we present both the dynamic difference-in-differences using never married mothers only, as well as the DDD estimates obtained when we add married women as a control group for never married ones. The outcomes of interest are employment, defined as having positive annual earnings, in Panel (a), and annual earnings in Panel (b). All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state, and all regressions are reweighted so that late and early-exposed mothers are similar on pre-birth characteristics. Data: 1991, 1994, 1996–Y, where Y is year indicated on the x-axis, ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure A.12: Employment by Calendar Year and Year of First Birth



Notes: This figure shows results from regressions comparing the employment of mothers with early EITC exposure (93-96 child births), late-exposure (88-91 child births) by year. We also include mothers with very late exposure (86-87 births) and very early exposure (97-99 births) for context. All regressions include indicators for child birth year and age, as well as mother's age, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

Figure A.13: Real Earnings by Calendar Year and Year of First Birth

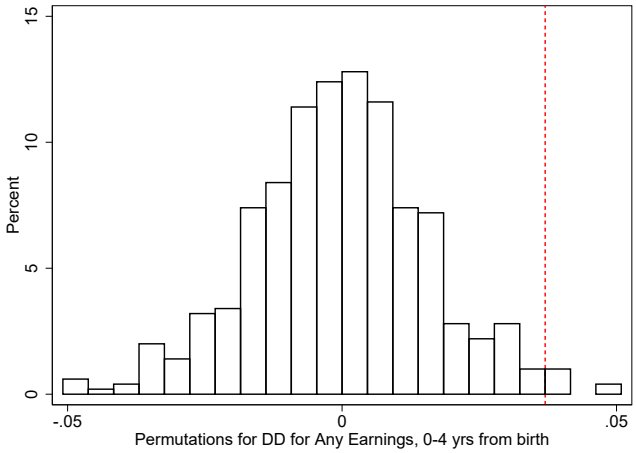


Notes: This figure shows results from regressions comparing the employment of mothers with early EITC exposure (93-96 child births), late-exposure (88-91 child births) by year. We also include mothers with very late exposure (86-87 births) and very early exposure (97-99 births) for context. All regressions include indicators for child birth year and age, as well as mother's age, race, state, and education group. Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

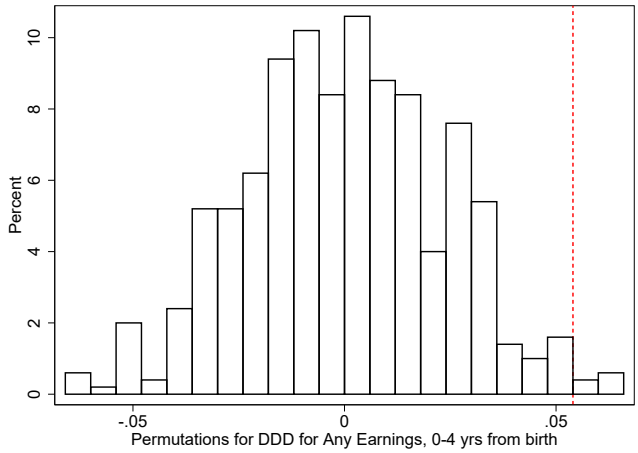
Figure A.14: Effect of EITC Expansion on Labor Market Outcomes – Sensitivity to Randomization Inference

(a) Short-term LFP

(i) Never-Married

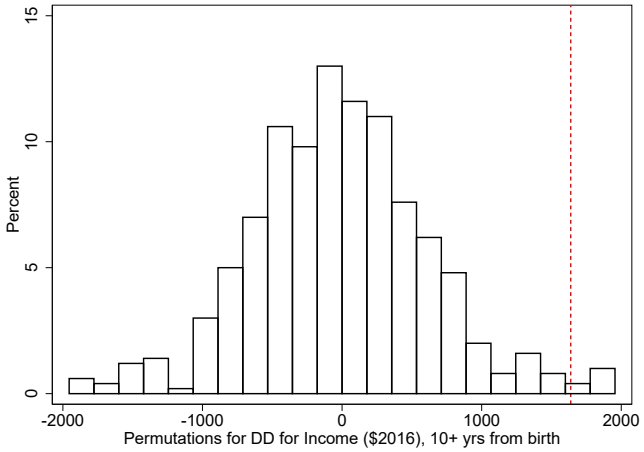


(ii) DDD

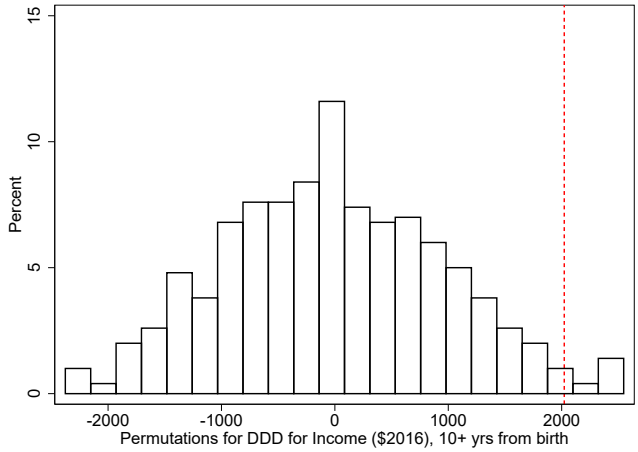


(b) Long-Term Earnings ($2016)

(i) Never-Married



(ii) DDD



Notes: The outcomes of interest are employment, defined as having positive annual earnings, in Panel (a), and annual earnings in Panel (b). The one-sided p-values for short-term LFP DD and DDD are both 0.01. The one-sided p-values for the DD and DDD for long-term earnings are 0.01 and 0.02, respectively. All regressions include indicators for child birth year and age, as well as mother's age, birth year, race, state, and education group (the DDD regressions allow for differential effects by marital status for these controls). Moreover, all regressions control for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, and adoption of welfare reform. Standard errors are clustered by state, and all regressions are reweighted so that late and early-exposed mothers are similar on pre-birth characteristics. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.

B Matching CPS to Administrative Earnings Records

Table A.12: CPS-SSA Data Matching Rates – By Year, Marital Status and EITC Exposure

	Never Married		Married	
	88-91	93-96	88-91	93-96
1991	0.819		0.794	
1994	0.789	0.750	0.772	0.708
1996	0.796	0.816	0.834	0.795
1997	0.731	0.812	0.756	0.750
1998	0.696	0.762	0.741	0.705
1999	0.683	0.661	0.695	0.691
2000	0.680	0.696	0.703	0.697
2001	0.222	0.264	0.255	0.284
2002	0.772	0.784	0.812	0.780
2003	0.758	0.788	0.783	0.776
2004	0.732	0.670	0.726	0.715
2005	0.730	0.675	0.720	0.686
2006	0.914	0.918	0.918	0.881
2007	0.918	0.874	0.917	0.881
2008	0.933	0.864	0.904	0.885
2009	0.857	0.883	0.904	0.892
2010	0.868	0.859	0.899	0.884
2011	0.874	0.893	0.909	0.892
2012	0.873	0.906	0.869	0.892
2013	0.887	0.891	0.858	0.901
2014	0.921	0.894	0.879	0.894
2015	0.900	0.864	0.900	0.867
2016	0.841	0.871	0.818	0.848
Total	0.762	0.776	0.788	0.799

Notes: This tables shows the share of CPS observations that we match to SSA records. Data: 1991, 1994, 1996–2000 and 2002–2015 ASEC CPS linked to 1978–2016 longitudinal SSA earnings and disability records. Sample: women whose first child was born in 1988–1991 or 1993-1996 and who were at least 19 or older, women who were aged 15-49 and were either married or never married at the time of the CPS interview.