

Long-Run Effects of Incentivizing Work After Childbirth

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Abstract

This paper uses a panel of SSA earnings linked to the CPS to estimate the impact of increasing post-childbirth work incentives on mothers' long-run career trajectories. We implement a novel research design that exploits variation in the timing of the 1993 reform of the Earned Income Tax Credit (EITC) around a woman's first birth and in eligibility for the credit. We find that single mothers exposed to the expansion immediately after a first birth ("early-exposed") have 3 to 4 p.p. higher employment in the five years after a first birth than single mothers exposed 3 to 6 years after a first birth ("late-exposed"). Ten to nineteen years after a first birth, early-exposed mothers have the same employment and hours as late-exposed mothers, but have accrued at least 0.5 to 0.6 more years of work experience and have 4.2 percent higher earnings conditional on working. We provide suggestive evidence that these higher earnings are primarily explained by the increase in work experience, rather than a change in marriage, fertility, or occupation. Our results suggest that there are steep returns to work incentives at childbirth that accumulate over the life-cycle.

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

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

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

We rely on a novel, large-scale panel of household earnings that we construct by linking two data sources: (i) longitudinal earnings data from 1978 to 2015 and individual dates of birth from the Social Security Administration (SSA); and (ii) twenty three years of the March Current Population Survey (CPS), spanning from 1991 to 2016. We use the detailed demographics in the CPS to identify a “high impact” sample of never-married mothers and their children, and the SSA records to track annual earnings and employment (defined as positive earnings) around a first birth for each of these mothers. This gives us annual earnings for roughly ten times as many sample mothers as appear in the CPS in each March survey. Further, we use the snapshot of employment and fertility

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

²We use “exposed at first birth” or “exposed at birth” to refer to mothers who had a first birth in or after 1993.

information in the CPS to provide suggestive evidence on part-time and full-time work, as well as on occupation choice and fertility, which may be potential mechanisms for our long run effects.

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

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

Using these dual strategies, we find that early-exposed mothers have 3.4 to 3.7 percentage points (p.p.) higher employment in the first five years after a first birth (which we refer to as the “short run”). This represents an 18 percent recovery of the 20 p.p. drop in employment in the year after a first birth experienced by late-exposed mothers. We show that these effects are concentrated among wage earners — rather than self-employed workers — which validates the effects as real changes in labor supply. Further, in favor of our research design, we find parallel trends between early- and late-exposed mothers prior to childbirth.

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

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

These results hint at the fact that post-birth work experience may be rewarded with steep

returns. As further evidence for this mechanism, we find that the increase in early-exposed mothers’ long-run earnings is driven by a rise in the share of mothers who jointly have high earnings (in the top 25%) and also worked during the first three years after a first birth. Moreover, this effect appears to entirely reflect changes in the *quantity* of experience among early-exposed mothers, rather than a change in the *return* to experience, as we find that this (correlational) return is the same for an early-exposed mother as for the average single mother. If experience was the only source of early-exposed mothers’ earnings gains, the implied return to a year of full-time, full-year experience would be 6.2 percent. This is within the range of estimates for similar populations (Adda et al., 2017; Gladden and Taber, 2000; Looney and Manoli, 2013; Card and Hyslop, 2005), but our larger shock to experience gives us substantially more precision than other causal estimates.

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

As an extension of the results on earnings, we close the paper by examining whether early-exposed mothers have higher total *net income*, taking into account potential offsetting effects from taxes, transfers, childcare costs, and the discounting of future earnings. We estimate impacts on each of these flows, and find that the net present value of total income accumulated over the long run is \$16,737 higher for early-exposed mothers than late-exposed mothers. Moreover, we find that incorporating long-run effects on taxes and transfers doubles the implied marginal value of public funds (MVPF) of the expansion – i.e., the benefit-cost ratio – relative to the “medium-run” MVPF (Hendren, 2016; Hendren and Sprung-Keyser, 2019; Bastian and Jones, 2020). This suggests that policies to promote work after childbirth, such as tax incentives for new mothers, could have meaningful effects that accrue over the long run.

We present multiple pieces of additional evidence to address potential threats to the interpretation of our short- and long-run findings. We first use an expanded sample of births to show that, consistent with EITC incentives, post-birth employment responses are larger for mothers after a second birth, but not additionally larger after a third or higher-order birth. We also show that the rise in post-birth employment can not be explained by welfare reform or the economic boom because it persists even when we only use (i) the states that did not enact welfare waivers and the years prior to federal welfare reform, or (ii) the states that experienced little change in the unemployment rate in the 1990s. For these reasons, we view our long-run results as a dynamic response to the EITC reform. However, the precise incentive that causes mothers to work sooner is not critical for our interpretation of our focal later-life effects.³

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

Additionally, we show that our short- and long-run results persist across alternative research designs and specifications. We rule out potential bias from across-year comparisons in the DD by presenting transparent graphs of *within-year* differences in the earnings of early- and late-exposed mothers. Further, we confront possible concerns with the married comparison group by instead using childless women (with placebo births) or low-income married mothers as comparison groups, and find the same results. Finally, we find no evidence of bias from selective marriage or mis-measurement of marital status in the CPS.

Our paper is at the center of three active literatures. First, we contribute to work on the role of lost work experience after childbirth on the long-run child penalty, and particularly on wages. The most relevant estimates on this topic come from paid leave extensions,^{4,5} which have found inconsistent, and often small effects of increasing mothers' time away from work.⁶ However, these papers typically examine the effect of a relatively small change in experience that is also often simultaneous with another treatment (e.g., job protection). This could make it difficult to detect an impact on earnings (e.g. Stearns, 2018; Lalive et al., 2013). Additionally, paid leave reforms often require mothers to be back at work within a year of childbirth, which makes it difficult to extrapolate to the 40% of mothers who remain out of the labor force for a longer period (Laughlin, 2011).

Our study has several unique features relative to this body of work. First, we leverage variation from substantial reductions in non-employment *beyond* the first year after a first childbirth. Our impacts on employment are largest in the first six years after birth, but extend up to nine years after birth. Second, we can estimate long-run impacts on *wages* because we find convergence in employment and hours (unlike, e.g., Schönberg and Ludsteck, 2014; Bailey et al., 2019; Grogger, 2009; Lequien, 2012), which enables us to calculate the return to experience. We find that extending a post-childbirth leave by a year could be expected to reduce wages by 6 percent in the long run through the impact of lost experience.

We also contribute to the literature on the return to work experience for low-income women and particularly single mothers, who account for 40% of US births. The closest benchmarks provide a wide range of estimated returns, and include Looney and Manoli (2013), who estimate an insignificant 0.4% return using variation in experience across synthetic cohorts of US single mothers; Gladden and Taber (2000), who estimate a 4–5% return for low-educated US women using an IV approach; Adda et al. (2017), who estimate a 9 to 12% return using individual variation in experience across German mothers; and Card and Hyslop (2005) and Grogger (2009), who leverage randomized welfare experiments in Canada and the US and estimate an insignificant -7%

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

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

⁶See, e.g., Bailey et al., 2019; Lequien, 2012; Schönberg and Ludsteck, 2014, for negative effects, or Stearns, 2018, for positive effects.

and significant 13% return, respectively. However, these estimates are subject to concerns about measurement error in self-reported earnings and experience (Looney and Manoli, 2013; Gladden and Taber, 2000; Card and Hyslop, 2005), selection into employment and endogenous experience (Looney and Manoli, 2013; Adda et al., 2017; Grogger, 2009), and little identifying variation (Card and Hyslop, 2005). Our design addresses each of these concerns.

Finally, we contribute to the literature on the impacts of the EITC on the labor market outcomes of single mothers. A substantial body of work has shown positive impacts of the EITC on short-run employment,⁷ largely using difference-in-difference comparisons of single mothers to childless women, or single mothers with multiple children to single mothers with one child, around an EITC reform. Our large-scale panel data permit us to improve on these designs by controlling flexibly for unobserved selection into giving birth using pre-birth outcomes of mothers. It also allows us to inspect for pre-birth trends around the reform, which has not been tested in prior work, and to run a suite of empirical tests to address recent critiques in Kleven (2019).

The long-run effects of the EITC have only been examined in one recent paper by Neumark and Shirley (2020), who use the PSID paired with state-by-cohort-by-parity variation in the EITC. They find imprecise positive impacts of a higher credit on work experience and long-run earnings for less-educated single mothers. However, because this analysis relies on a sample of fewer than 800 women and self-reported earnings, the small and imprecise effects could reflect attenuation or sampling error. We substantially improve upon Neumark and Shirley (2020) in the scope and quality of our administrative data, the transparency of our empirical analysis, and the detailed controls and robustness checks that we include to validate our results.

1 Background

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

Eligibility for the EITC depends on two key inputs: number of qualifying children and household earnings.⁸ Although the EITC is available to childless households, the childless credit is restricted to very low income households and is quite small – e.g., in 2016, the maximum credit was \$506, and was only available to families with annual earnings between \$6,610 and \$8,270. The maximum

⁷See, e.g. Eissa and Liebman (1996); Meyer and Rosenbaum (2001); Grogger (2003*a*); Hotz and Scholz (2006); Chetty et al. (2013); Neumark and Shirley (2020); Hoynes and Patel (2018); Bastian (2020); Bastian and Jones (2020); Wilson (2020); Micheltore and Pilkauskas (forthcoming); Schanzenbach and Strain (2020).

⁸A qualifying child must be in the household for at least half of the tax year and either be under the age of 19 (24 if in school full-time) or permanently disabled.

credit for families with one child (two children) is typically six (ten) times higher than the childless credit. Thus, in practice the EITC is primarily a child-based credit.

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

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

To consider the impact of the 1993 expansion on individuals’ behavior, it is useful to scale the change in benefits by household earnings in each EITC region. Panel (b) of Appendix Figure A.1 shows that, as a percent of pre-tax earnings, the additional benefits represented 8% growth in the “phase-in” region (or 16%, accounting for the 1995 adjustment), 5 to 8% growth in the “flat” region, and 0 to 2% growth for most of the “phase-out” region. Put differently, post-reform low-income households could expect to receive the equivalent of an additional month’s wages in the phase-in, or three-quarters of a month’s wages in the flat region. On the margin, this would be expected to encourage more low-income mothers to work.¹⁰

The 1993 reform also raised the real maximum credit for households with two or more children, more so than the one-child credit (see Appendix Figure A.2).¹¹ The real maximum credit for these families rose by \$1,584 in 1994, and by an additional \$1,400 over the next two years. Because of our interest in first births, this variation is less relevant for our main analysis; however, we return to exploit the variation in EITC benefit amount as a secondary identification strategy in Section 5.1.

Consistent with the substantial financial incentives at stake, EITC take-up during this period is quite high, between 80 and 86 percent (Scholz, 1994), based on data from the 1990 tax year. This compares favorably with the take-up of other low-income programs around the same time, such as traditional welfare (AFDC, which has 60–65% take-up) or Food Stamps (which has 55-60%

⁹The second, smaller EITC reform during our period was in 1990, and raised the maximum credit from \$1,750 to \$2,381 over three years. We emphasize this reform less because it contributes relatively little to our identifying variation, as we discuss in Section 1.1.

¹⁰There could also be intensive margin responses, although knowledge of the non-linear incentive structure of the EITC appears to be limited, which makes extensive margin responses more likely (Chetty and Saez, 2013).

¹¹The 1993 expansion also established the first credit for households with no dependents, although this is less relevant for our analysis because we difference out pre-birth outcomes.

take-up) (Blank and Ruggles, 1996). Further, Saez (2010) finds strategic “bunching” at the first kink of the EITC schedule beginning in the early 1990s, suggestive of spreading awareness of the program’s incentives.

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

The close timing of these events with the EITC reform raises some challenges for the identification of EITC effects, as recently highlighted in Kleven (2019). Nevertheless, because the timing and details of welfare and other low-income policies vary across states, we are able to control for these in our analysis, which we do at baseline and with increasing flexibility as a robustness exercise in Section 5.1. An alternative approach could be to exploit changes in welfare as a secondary source of post-birth work incentives. Doing so would change the policy attribution of our short-run effects but would be immaterial for the interpretation of our long-run effects as stemming from early work incentives. In that sense, while we are careful to show that our results are not driven by other policies, our long-run results would remain valid even if our estimates incorporate spillovers from welfare policies.

1.1 Identifying Effects on *New Mothers*

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

To illustrate the variation in work incentives for *new mothers*, we compute the average maximum EITC available in each year around a first birth for two groups of interest. “Early-exposed” mothers have a first birth between 1993 and 1996 and therefore are exposed to the EITC expansion at or around a first birth. “Late-exposed” mothers have a first birth between 1988 and 1991 and therefore are exposed to the EITC expansion three to six years after a first birth.¹² Because EITC benefits are higher for two-child families, we consider both a scenario in which all mothers have only one child and an alternative scenario in which all mothers have a second child with uniform probability between two and four years after the first (such that the average spacing is three years, as in our

¹²We omit 1992 first-births in these comparisons in order to augment the difference in the benefits of early- and late-exposed mothers. We include 1992 first births when we examine outcomes across more-continuous bins of cohorts (see, e.g., Section 5.1).

sample).¹³ We then assign all women the zero-child maximum credit prior to a first birth, and either the one- or two-child maximum credit after childbirth, depending on the number of years since birth and the scenario.

Panel (a) of Figure 1 shows that in both of these scenarios early-exposed mothers are eligible for higher maximum credit than late-exposed mothers for at least the first five years after childbirth. The gap in incentives when we assume mothers have only one child in subfigure (i) is \$1,222 at birth; \$1,185 to \$1,329 in years 1 and 2, \$500 to \$800 in years 3 and 4, and zero in year 6.¹⁴ When we allow for a second child in subfigure (ii), the pattern remains the same, but the scale expands: the gap is the same in the first two years, then grows to a peak of \$2,066 in year 3, and declines thereafter. Both of these figures suggest that early-exposed mothers would be expected to work more than late-exposed mothers for at least the first five years after birth. Panel (b) shows the gap in EITC incentives between early- and late-exposed mothers over twenty years after birth. Importantly, under both scenarios, the only period when there is a meaningful gap between early- and late-exposed mothers is in the first five to seven years after a first birth. This ensures that long-run differences in behavior can not be due to differences in contemporaneous EITC incentives.

Our analysis leverages this variation in EITC incentives in the years around first birth to identify the effect of early work incentives. We formalize our estimation strategy in Section 3.

2 Data

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

We use the CPS survey responses primarily to obtain demographics for our sample. CPS-provided parent identifiers allow us to connect parents and children in the survey, which we use to identify the first birth for each woman and to measure her total fertility. We also observe marital status, which we use to assign treatment; as well as race (white, black, hispanic, or other), age, completed education (less than or equal to high school, some college, or college graduate), and state of residence, which serve as control variables. Because we assign demographics at the time of the CPS survey, rather than at the time of first birth, this introduces measurement error to our analysis. This is a particular concern for marital status because of the link to treatment status. We provide a detailed discussion of potential sources of bias from mismeasurement, and of evidence

¹³We verify in Section 4.3 that early exposure to the reform does not change birth spacing.

¹⁴The vast majority of this difference (75%) is generated by earlier exposure to the 1993 reform.

that this is not empirically relevant for our results, in Section 3.1, after we introduce our empirical strategy.

The CPS also serves as a supplementary source of information on labor market outcomes and program participation. The labor outcomes of interest are hours worked in the past week, weeks of work last year, and current occupation (grouped into 15 categories as in Appendix B.1), which allow us to explore intensive margin employment responses. We also take advantage of information on the value of benefits received from public programs for our calculations of net income and fiscal externalities. Because we only observe CPS outcomes of mothers at one point in time, our sample for these analyses is smaller and imbalanced relative to our administrative outcomes. Nevertheless, we find qualitatively similar employment results across the CPS and the administrative data (see Section 4.3).

Our main labor market outcomes are obtained from SSA earnings records (the “Detailed Earnings Record” files). Earnings information includes aggregate annual wages, salary, and tips from Box 1 of the W-2 form as well as earnings from covered self-employment from Form 1040-SE. We have access to earnings from 1978 to 2015 for individuals that appear in the CPS. We convert all dollar values to 2016 real dollars using the CPI from the Bureau of Labor Statistics. From these records, we construct “total earnings” which includes the aggregate earnings from all W-2 forms (“wage earnings”) and self-employment filings (“self-employment earnings”). We also calculate “household earnings” which is equal to total earnings for single individuals and is equal to the sum of own and spouse’s total earnings for married individuals.¹⁵ If an individual has positive total earnings, we consider her to be employed during the year.¹⁶

Along with this earnings information, we have access to the SSA NUMIDENT file, which contains information on individuals’ exact dates of birth. We use this to determine the year of birth for mothers and children. We then determine birth order among children by sorting on this year of birth within a mother, and set the year of first childbirth as the year of birth of the eldest child.¹⁷

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

Core sample We construct our core sample of first-time mothers from the set of individuals who are matched to the administrative data. In particular, we keep all women who (i) were interviewed in the CPS before age 50, whose children are more likely to have been present at the time of

¹⁵Spousal information is also subject to measurement concerns, which we address in Section 3.1.

¹⁶We discuss differences in individual earnings and employment across the CPS and SSA records in Appendix B.2.

¹⁷In the few cases where the numident year of birth for children differs by more than 5 years from imputed year of birth from the CPS (CPS age - year - 1), we assume that the numident match is incorrect, and use the imputed year of birth.

interview; (ii) had a first birth at age 19 or older, which reduces the role of high school attendance or own dependent status in our results; and (iii) are exposed “early” or “late” to the reform due to having a first birth between 1988–1991 or 1993–1996. To examine broader trends, we create an extended sample that retains all women who had a first birth between 1986 and 1999.

In a similar spirit to earlier work, we use never-married mothers as a “high-impact” sample. To validate this choice, we use the three years of pre-birth household earnings to project likely EITC eligibility after a first birth. We find that 97% of never-married mothers have at least one year of earnings that falls strictly below the maximum earnings for one-child EITC benefits (i.e. are likely to be eligible for some EITC benefits).¹⁸ Further, the average never-married working woman could expect the EITC reform to increase her earnings by 8 percent based on her pre-birth earnings and Appendix Figure A.1. Additionally, unlike divorcees and widowers, who are often included in the EITC “high impact sample,” we can be certain that the never-married mothers that we observe were also single at the time of first birth. This combination of factors gives us confidence that never-married mothers would be highly eligible for the EITC at the time of first birth.

For analogous reasons, we identify married mothers as a “low impact” sample. Based on pre-birth household earnings, 49% of married households are likely to be eligible for some EITC benefits. However, because married households have higher earnings, the 1993 reform would have a smaller percent effect on household earnings. The average earnings of a working married woman would place her in the phase-out region, and thus make her only eligible for a 2% increase in her earnings post-reform. Incorporating spousal earnings would further reduce the expected increase in benefits. We discuss the advantages and limitations of using married women as a comparison group, and robustness to alternative comparison groups, in Section 3.1.

Our final sample consists of 11,291 never-married women and 97,288 married women, for whom we have SSA earnings for 25 years (from five years before to 19 years after they first give birth).¹⁹ We provide descriptive statistics in Section 3.1.

All births sample In Section 5.1, we compare the change in post-birth employment after a second-or-higher-order birth compared to a first. Thus, we create a separate sample that includes all childbirths that occurred between 1988–1991 or 1993–1996, and treat each childbirth as a separate event. We do this by creating 10-year mother-birth panels around each birth, and then stacking these panels.

State-level controls We obtain annual measures of state-level economic conditions and policy parameters from Bitler and Hoynes (2010), including the unemployment rate, the maximum level of AFDC/TANF benefits, the minimum wage, the mean poverty threshold for Medicaid, and an indicator for whether a state has implemented any welfare reform (waiver or TANF). We merge

¹⁸Different than other studies of the EITC, we do not further restrict the sample by education because (i) education is typically observed many years after birth, and is potentially an outcome of the policy; (ii) we find a high rate of pre-birth EITC eligibility among all education groups for never-married women.

¹⁹Relative to using the CPS alone, our panel has roughly ten times as many observations per calendar year for women that meet our sample criteria.

these to our data using each woman’s state of residence. We also create indicators for the presence of each of six types of welfare waivers in a state using the dates of implementation from the tables in Crouse (1999) (as in Kleven, 2019), as well as additional information from the tables in Gallagher et al. (1998).²⁰

Supplemental data Because we are not able to observe changes in marital status in the CPS, we turn to the Survey of Income and Program Participation (SIPP) to examine the detailed marital and fertility histories for SIPP mothers who gave birth in the same years as our core sample. This allows us to determine whether the likelihood of the timing of first marriages post-birth varies across early- and late-exposed mothers (see Section 3.1).

3 Estimation Strategy

Our goal is to identify the causal effect of early exposure to work incentives after a first childbirth on labor market outcomes. We thus compare the labor market outcomes of women who were exposed “early” to the expanded 1993 credit (i.e., at first birth) to women who were exposed “late” (i.e., 3 to 6 years after first birth).

We start with a dynamic DD analysis that estimates the difference in labor outcomes between early- and late-exposed never-married women in each year relative to a first birth – our event time. Denoting an individual as i , the year of first birth as b , and the year relative to first birth as τ , and defining $EarlyExposed_b$ to be an indicator for having a first birth between 1993–1996, we estimate:

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

The β_k coefficients in the summation term give the difference in outcomes between early- and late-exposed never-married women in each year relative to a first birth, τ . We omit $\tau = -1$, such that these coefficients are estimated relative to the difference in outcomes in the year before childbirth. θ_τ are years-since-first-birth fixed effects, which capture the behavior of late-exposed mothers in each year relative to first birth. χ_b are year-of-childbirth fixed effects, which capture differences in average outcomes in the year prior to childbirth across childbearing cohorts (e.g., selection into giving birth or new policies across cohorts). Because year is a linear combination of event-time τ and year of childbirth b , we can not also include year fixed effects in this model – we address this limitation in the DDD design.²¹ $X_{is\tau}$ includes fixed effects for mother’s year of birth, age, state, race, and education group, as well as interactions between an indicator for post-birth and race and education fixed effects to account for potential differences in maternal employment

²⁰These 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.

²¹This limitation is analogous to the omission of quarter-of-birth-by-year effects in Bailey et al. (2019). Within-year comparisons of early- and late-exposed mothers (that omit τ) produce similar results – see Section 5.2.

across these groups. $P_{s\tau}$ controls for state-level variables, including the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, the adoption of any welfare reform (TANF or waivers), the adoption of six different types of welfare waivers, as well as an indicator for the implementation of the 2009 EITC reform.

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

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

$$Y_{imbr\tau} = \alpha + \sum_{k \neq -1} \beta_{k,DDD} \cdot \mathbb{1}(\tau = k) \cdot EarlyExposed_b \cdot NM_m + \theta_\tau \cdot \lambda_b + \theta_\tau \cdot \rho_m + \lambda_b \cdot \rho_m + \gamma_m X_{is\tau} + \delta_m P_{s\tau} + \epsilon_{imbr\tau} \quad (2)$$

The $\beta_{k,DDD}$ coefficients trace out the DDD, comparing the difference between the gap in outcomes for early- and late-exposed never-married mothers and for early- and late-exposed married mothers. We include fixed effects for marital status, ρ_m ; year of childbirth, λ_b ; and years since birth θ_τ ; as well as the two-way interactions between these. Importantly, the inclusion of year-of-birth-by-years-since-birth fixed effects, $\theta_\tau \cdot \lambda_b$, allow us to control for year-specific shocks to the labor supply of mothers with children of a particular age. These could include, for example, changes in federal policies protecting mothers' jobs after childbirth, broad tax policy (e.g., child tax credits), or the availability of new technology for caring for infants.²² Moreover, we allow all individual and state-level controls, $X_{is\tau}$ and $P_{s\tau}$, to vary by marital status. As with the DD, we also replace the indicators for each year around birth with a $PostBirth_\tau$ dummy to estimate DDD average treatment effects.

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

Relationship to prior EITC analyses There are three key differences between our empirical design and the designs used in prior analyses of this EITC reform. First, our difference-in-difference takes comparisons across event time (i.e., pre- and post-childbirth), rather than across calendar time (i.e., pre- and post-reform). This allows us to richly control for potential unobserved changes in the composition of who has a birth using pre-birth outcomes. We find in Section 3.1 that such changes are generally positive and statistically significant, but small, which suggests that prior estimates

²²Our results are robust to allowing these year effects to vary by state — see Section 5.2.

could have a slight upward bias. Second, our main results rely on a triple-difference design, rather than a difference-in-difference design. This allows us to include more rigorous child-age-by-year fixed effects – rather than year fixed effects – to control for the increasing presence of mothers with *young* children in the labor force (e.g., Goldin, 2006). Third, we use married mothers as a primary comparison group, different than the more common use of childless unmarried women (e.g. Meyer and Rosenbaum, 2001; Eissa and Liebman, 1996). This allows us to control for other potential changes in the child penalty after a first birth over this period. However, we show that our results are robust to using childless women or low-income married women as comparisons in Section 5.2.

3.1 Identification Assumptions and Testable Implications

Our identification relies on the assumption that early exposure to the EITC reform is uncorrelated with other predictors of mothers’ labor market outcomes that vary by year of first birth. It is thus crucial to address two primary threats to identification. First, early-exposed mothers may have been on a different labor market trajectory than late-exposed mothers even in the absence of the reform. Second, early- and late-exposed mothers may have been differentially exposed to other policies or economic conditions in the short- or long-run, which we could mistakenly attribute to the EITC. We discuss the plausibility of each of these threats in the context of our DD and DDD strategies.

DD We assess potential violations of the common trends assumption in two ways. First, we check for differences in the *pre-birth* employment trajectories of early- and late-exposed women. Second, we look at whether women’s employment after a first birth was changing differentially by year of childbirth for women who gave birth *prior* to the reform. Previous studies of the 1993 EITC expansion have shown evidence along the lines of our second test, but our panel allows us to validate this assumption in its entirety. Both of these tests pass easily in Sections 4.1 and 5.1, suggesting that early- and late-exposed mothers’ outcomes were not diverging prior to “treatment.”

To address contemporaneous economic and policy confounds to the EITC, we control for the generosity and availability of a suite of government programs across states as well as for state unemployment rates in our baseline specifications. Moreover, we take additional steps to rule out potential bias from welfare reform and the 1990’s boom (for the short run), and the Great Recession (for the long run), which were arguably the largest shocks for single mothers during this period (see Sections 5.1 and 5.2 for details). As we discuss in Section 5.2, the Great Recession coincides precisely with the long run only for early-exposed mothers – thus, to the extent that we do not perfectly control for these effects, our long-run DD effects would likely give a lower bound on the true effect.

We also perform a number of tests to show that differences in the individual characteristics of early- and late-exposed mothers can not explain our results. In particular, while Panel (a) of Appendix Table A.1 shows imperfect balance in the demographic characteristics across these

mothers,²³ we are reassured that these mothers have indistinguishable employment trends prior to birth and that including flexible controls for these characteristics, individual fixed effects, or using inverse p-score reweighting (in Section 5.2) make no difference to our results. This makes it very unlikely that individual characteristics could explain our findings.

DDD We address differential trends in the DDD similarly to the DD. Again, we find no evidence that the gap between married and never-married women was diverging prior to the reform – see Sections 4.1 and 5.1.

Unlike the DD, in the DDD we use the outcomes of married mothers to control for *unobserved* contemporaneous shocks (which are outside of our control variables). This relies on an assumption that married and never-married mothers would have responded similarly to such shocks, and hence evolved in parallel in the absence of the EITC reform.

To gauge whether this is a plausible assumption, we first study responses by married and never-married women to childbirth – a very large and salient shock – prior to the EITC reform. In particular, we focus on mothers giving birth between 1986 and 1991, and estimate a version of Equation 1 that allows the coefficients on the event-time indicators to vary by grouped years of birth (1986-87, 1988-89, 1990-91) and marital status. Reassuringly, Appendix Figure A.4 shows that married and never-married mothers exhibit very similar employment patterns in the years around childbirth, including a nearly-identical “child penalty.” It also shows that these patterns were not changing across these cohorts – which is echoed in regressions in Section 3.1.^{24,25} Additionally, when we follow these cohorts over a longer period in Section 5.2, we find that never-married and married mothers have similar increases in labor market outcomes as their children grow up. Hence, it appears that married and never-married women who gave birth pre-reform exhibited very similar responses to childbirth over the short- and the long-run.

Second, consistent with this, prior work has found that never-married and married women have had similar labor supply elasticities over many decades (Blau and Kahn, 2007; Heim, 2007; Bishop et al., 2009).²⁶ At face value, this implies that married and unmarried mothers are likely to exhibit similar responses to changes in economic opportunities. Nevertheless, we allow our controls for unemployment rates to vary by marital status to mitigate remaining concerns about differential responses to economic conditions.

Third, we ask: did never-married and married mothers experience similar *changes* in observ-

²³We find no difference in age at first birth, household EITC eligibility pre-birth, or annual earnings conditional on working across these mothers; however, early-exposed mothers are more likely to be white, work a higher share of years pre-birth, and have higher levels of education (although this could be an outcome of a higher EITC, since it is reported after childbirth – see Manoli and Turner, 2018).

²⁴Similarly, Kuziemko et al. (2018) also show that the child penalty for overlapping cohorts of women does not vary across married and single women.

²⁵We note that these stagnating employment patterns for married mothers after childbirth are somewhat in contrast to the raw time trends, which show steady gains for married mothers with young children pre-EITC reform (e.g., Goldin, 2006). This is likely because we control for pre-birth employment, which we find has increased slightly over time (consistent with Mulligan and Rubinstein, 2008), and focus on employment immediately after birth.

²⁶These elasticities ranged from 0.5 to 0.7 for 1980, and declined at similar rates over the next several decades (Blau and Kahn, 2007; Heim, 2007; Bishop et al., 2009).

able characteristics between early- and late-exposed mothers (the difference-in-differences in observables)? This provides insight into the extent to which we would expect the gap in outcomes between never-married and married mothers to change after childbirth in the absence of the reform. Importantly, the blue diamonds in Appendix Figure A.5 show that married and never-married mothers experienced the *same* change in pre-birth employment and completed education; while never-married mothers experienced a slightly smaller change in age at first birth, EITC eligibility, and earnings. This suggests that, if anything, the gap in labor market outcomes between never-married and married mothers might have been expected to slightly *worsen* between early-exposed and late-exposed mothers based on observable characteristics.²⁷ As mentioned above, in practice we find no evidence of a DDD “pre-trend” before childbirth, and imposing balance with inverse p-score weighting does not affect our results.

Fourth, there may be a potential concern that low-educated married mothers would be treated *negatively* by the reform (Eissa and Hoynes, 2004), which would bias our DDD results upward. However, this is unlikely to affect our results because (i) the majority of our married sample has more than a high-school diploma, which puts them outside of the Eissa and Hoynes (2004) sample; and (ii) we focus on first births, for whom Eissa and Hoynes (2004) found small effects. Consistent with this, we find little effect of early exposure on the labor supply of married mothers in Section 4.1.

Finally, we note that we use multiple alternative comparison groups of single, childless, and lower-income women to verify that our results are not driven by any particularity of married women. Our results are very similar across these comparisons (see Section 5.2). A confound that survives this battery of comparisons would have to impact unmarried mothers more than married mothers, but not affect any other group of unmarried or lower-income women.

Measurement error Aside from these identification assumptions, our reliance on marital status reported at the time of CPS interview (after childbirth), instead of in the year of first birth, could raise two potential concerns about the role of measurement error in our results. First, relative to a representative sample of women who were never-married at first birth, our sample will have a higher share of women that *remain* unmarried after childbirth. This could make our results less generalizable if the impacts of early exposure are different for mothers who remain unmarried post-childbirth. We test for this by dropping mothers that are observed in CPS surveys further from a first birth, and find no impact on the size of our estimates (see Section 5.2).

Second, one might worry that there could be a correlation between EITC eligibility, marriage decisions and earnings growth. This could occur if, for example, early exposure to the reform leads early-exposed mothers to have higher earnings and, in turn, be less likely to marry. In that case, never-married early-exposed mothers that “survive” to be found in the CPS would have a

²⁷As comparison, the black dots in the figure show the differences in the average characteristics between never-married and married mothers, which are much larger than the difference-in-differences in the blue diamonds. See Appendix Table A.2 for these summary statistics. This reinforces the importance of using a DDD, which allows us to difference out fixed gaps by marital status.

different earnings trajectory than the average early-exposed mother, which would, in turn, bias our estimates upwards. Prior studies have found small, mixed, and often insignificant evidence for this channel (Ellwood, 2000; Dickert-Conlin and Houser, 2002; Herbst, 2011; Bastian, 2017; Neumark and Shirley, 2020; Michelmore, 2018) – nevertheless, we also investigate this in our setting.

To assess the potential bias from selective marriage, we first examine whether there is a difference in the marriage rates of early- and late-exposed mothers who were never-married at first birth. Because we do not observe marital status at birth in the CPS, we can not examine this within our main analytic sample. Instead, we construct an analogous sample from the SIPP of early- and late-exposed mothers who were never-married at first birth, and then calculate the share of these mothers that remain single in each year after childbirth. Contrary to the concerns about selective marriage, Appendix Figure A.6 shows that SIPP early- and late-exposed mothers have *the same* likelihood of remaining single in the short- and long-run. The average difference between early- and late exposed mothers is negligible (-1.3 p.p.) and statistically insignificant. This suggests that there is no significant impact of early exposure to the reform on marriage decisions.²⁸

As another test for selective marriage, we test whether the gap in characteristics between early- and late-exposed mothers widens in CPS surveys further from a first birth – as might be expected if “surviving” mothers are selected. Specifically, we regress a series of individual characteristics on a linear trend in “survey years from first birth” interacted with an indicator for being an early-exposed mother. Appendix Table A.3 shows that the coefficients on this interaction are always insignificant, including for levels of earnings after a first birth. Further, the estimates are typically negative, implying that, if anything, early-exposed mothers are relatively more negatively selected due to attrition. Moreover, our results are also unaffected by limiting our sample to mothers in CPS surveys soon after a first birth, where bias from selective marriage is less relevant (see Section 5.2).

There are also two more minor potential measurement issues. The first of these is that we observe a higher fraction of early-exposed mothers in the years immediately after birth (by virtue of only linking CPS’s in 1991 on), and thus require that mothers that we observe closer to first birth are not positively selected on unobservables. We test for this by verifying that our results are robust to dropping individuals from CPS surveys closer to birth (see Section 5.2.) Second, we may misassign child birth order since some children may have left home by the time mothers are surveyed. We test for this in Section 5.2 by restricting our sample to women surveyed at younger ages and find similar results.

²⁸If anything, the sign of our effect suggests that early-exposed mothers marry slightly more, similar to the effects for young mothers in Bastian (2017). If we assume that women that marry are positively selected, then the early-exposed mothers that we observe in the CPS (who do not marry) would be *negatively* selected.

4 Main Results

4.1 Short-run Impacts on Working After a First Birth

We focus first on the impact of work incentives on labor market outcomes in the five years after a first birth ($0 \leq \tau \leq 4$). This is a natural starting point for the analysis because the gap in work incentives between early- and late-exposed mothers is largest during this window, which generates a clear prediction for impacts on new mothers' employment and establishes a channel for long-run impacts on mothers' careers.

Panel (a) of Figure 2 presents regression-adjusted means of the employment rate of early- and late-exposed never-married women around a first childbirth. Leading up to birth, both groups of mothers show a roughly constant probability of working, exhibiting little, if any, anticipatory response to pregnancy. In the year of birth employment falls by 13 p.p. for both groups, a 20 percent decline from pre-birth levels. In the following year, late-exposed mothers' employment falls 7 p.p. further and remains lower relative to early-exposed mothers in the five years after childbirth.

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

The DDD event study shown in Panel (c) is almost identical to the DD. Importantly, the coefficients prior to birth are flat and close to zero, indicating that the outcomes of married and single mothers were not diverging prior to birth. Further, the fact that the DD and DDD coefficients are also the same in the periods post-childbirth implies that there is no effect of early exposure on married mothers, consistent with our expectations.²⁹

Table 1 presents our estimated average effects on employment for the short run. Our primary DD estimate in column 1 shows that never-married mothers' post-birth employment increases by 3.7 p.p. ($p < 0.01$); which represents a 5.9 percent increase relative to late-exposed mothers' employment and an 18% recovery relative to the drop in employment in the year after birth. It also implies an elasticity of employment to labor earnings between 0.54 and 0.72, based on early-exposed mothers' expected percent change in EITC benefits (see Appendix D for details). This is similar to estimates of the participation elasticity to wages of single women with children in the 1980's and 1990's, which fall between 0.5 and 0.7 (Bishop et al., 2009).

Relative to prior work, our point estimate sits at the lower end of the estimated average effects of the EITC for all single mothers (Meyer and Rosenbaum, 2001; Grogger, 2003*a*; Hoynes and Patel, 2018; Bastian and Jones, 2020; Kleven, 2019), and is noticeably smaller than estimates for mothers

²⁹For the DD using married mothers see Appendix Figure A.7.

with young children (Kleven, 2019; Micheltmore and Pilkauskas, forthcoming).³⁰ This could reflect our ability to control for unobserved selection into motherhood, unlike these earlier studies.

Column 2 shows that married women, in contrast, have no significant change in employment (point estimate = 0.003). To look for potential heterogeneity in these results, Appendix Table A.4 separately estimates effects based on pre-birth household EITC eligibility. We find a positive effect only among *eligible* never-married mothers, and no effect among any subset of married mothers. This empirically validates our DDD strategy, which assumes that married mothers do not respond to the expansion.³¹ The DDD coefficient in column 3 of Table 1 is thus very similar to the DD effect (3.4 p.p.).

Impacts on positive *wage* earnings, shown in the following columns, are only slightly smaller than our initial effects on employment (by 13% for the DD and 9% for the DDD). Since wage earnings are reported through third-party services, this provides reassuring evidence that our employment effects are driven by a real increase in work. In line with this, we find that early-exposed mothers' self-employment rate rises by less than 1 p.p. (see Appendix Table A.5).

To better understand these employment effects, Figure 3 shows the effect of early-exposure on the distribution of mothers' earnings in the first four years after a first birth.³² We examine a shorter window here to focus on mother's jobs when they initially join the workforce after birth. For reference, we label the areas of the distribution corresponding to the 1994 EITC phase-in, flat, and phase-out regions, as well as the poverty threshold.

We find a significant increase in density in the phase-in and flat regions, where the increase in EITC was largest. Effects in the phase-out region are small and statistically insignificant. Importantly, this is not simply because never-married mothers never have earnings in the phase-out region – Appendix Figure A.8 shows that they do, and that there is substantial overlap with married mothers. Rather, it seems more likely that the increase in mass in the phase-in and flat regions reflects the larger change in incentives. Consistent with prior work, we find that only a small share of early-exposed mothers target earnings precisely at the refund-maximizing first EITC kink post-birth, and find no evidence of bunching among late-exposed mothers (Saez, 2010) – see Appendix D for details.

On average, early-exposed mothers earn \$657 (se: 327) more in the short run based on the DDD, which represents a 7% increase in earnings relative to the mean for late-exposed women (\$9,926).³³ If the short-run earnings increase was due to increases in working alone, and early-exposed mothers that enter the labor market earn as much as the average late-exposed working mother (\$15,737),

³⁰In particular, Kleven (2019) finds that the 1993 reform led to a 2.2 to 7.6 p.p. increase in employment for mothers with children under 13, and Micheltmore and Pilkauskas (forthcoming) estimates that raising the EITC by \$1,000 leads to a 6 to 9 p.p. increase in the employment of mothers with children ages 0–2.

³¹One explanation for this is the small incentive for married households (particularly after a first birth), as we discussed previously. A second explanation is that the null effect reflects a mix of positive responses among primary earners and negative responses among secondary earners (Eissa and Hoynes, 2004). Contrary to this, we find insignificant results even in households where the wife is the primary earner.

³²The figure shows estimates from DDD regressions where the outcomes are indicators for having earnings above X, with X = 0, 2500, 5000, ..., 80,000, i.e., 1-CDF (Duflo, 2001). For the DD estimates, see Appendix Figure A.9.

³³The effects are smaller in the 4 years after a first birth, and larger in the DD — see Appendix Table A.6.

then we would expect average earnings to rise by \$487, rather than by \$657. This leaves room for some small intensive margin effects.

4.2 Long-Run Effects

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

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

Panel (a) of Figure 4 shows that the impacts on employment begin to fade four to five years after birth, and completely disappear by year 9 or 11, depending on the specification. After that, we find a mixed pattern of small positive (DDD) or negative (DD) effects, which we suspect may be due to imperfect controls for the effects of the Great Recession. Controlling for state-level unemployment rates among low-skilled individuals or women rather than among all individuals reduces the modest long-run fluctuations (see Section 5.2). Moreover, the long-run fluctuations in employment seem to reflect entry decisions about relatively small earnings amounts, as indicated by the results on earnings below. Given this and the overall small point estimates beyond year 11, there appears to be no lasting impact of early exposure on employment.³⁴

The estimates in columns 1 and 2 of Table 2 show that early-exposed women have a 4.3 to 5.5 p.p. higher employment rate per year in the medium-run. This difference fades to an insignificant -1.7 p.p. to 1 p.p. in the long-run.

Although early-exposed mothers do not have a permanently higher rate of employment, the additional time they accumulate in the labor market may improve long-run earnings through increases in labor market experience. We calculate impacts on work experience by taking a cumulative sum of the annual DD or DDD impacts on employment in Figure 4, and then dividing by the number of years to get the average effect.³⁵ Columns 3 and 4 of Table 2 show that early-exposed mothers

³⁴For interested readers, we show the separate estimates for early- and late-exposed mothers – akin to Panel (a) of Figure 2 – for employment and earnings in Appendix Figure A.10.

³⁵An alternative approach would be to use observed years of experience as an outcome. This approach would difference out gaps in pre-birth experience between early- and late-exposed mothers; but would not account for gaps in pre-birth *employment*, which could create bias in experience. For this reason, we prefer to take a sum over the

have 0.46 to 0.47 years of additional experience in the medium-run, which becomes 0.45 to 0.62 additional years of experience in the long-run. The long-run DDD estimates correspond to a 5.7% increase in years of experience.

A limitation of this experience measure is that we are only able to measure the change in the number of years with *any* work experience. This potentially misses intensive margin responses, and thus may be less correlated with long-run outcomes than the change in the number of hours of experience or the number of years of full-time experience. To address this, in Section 4.3, we use the CPS to calculate impacts of early exposure on hours and weeks of work, and estimate the implied change in years of full-time full-year experience. Despite the different sources and measures, we come to similar conclusions about gains in experience.

Panel (b) of Figure 4 presents the dynamic impacts of early exposure on earnings. Early-exposed mothers experience increasing earnings gains over the first six years after birth, following the impacts on employment. However, unlike employment, impacts on earnings only decline slightly over the next few years, do not exhibit non-monotonicities over time, and remain positive and often statistically significant over the long run.³⁶ The persistent impact on earnings is particularly apparent in the DDD, where the point estimates are nearly constant between years 10 to 19. The DD impacts on earnings decline moderately between years 15 to 19, but we show in Section 5.2 that this reflects the recession (which is controlled for better in the DDD), rather than true convergence in earnings.³⁷ Hence, early-exposed mothers have long-lasting earnings gains, which are not readily explained by differences in the rate of employment.

Table 2 shows that early-exposed mothers earn \$2,618 to \$3,655 more per year in the medium-run and \$1,206 to \$1,396 more per year in the long run. The majority of this (87%) is due to increases in pay from employers (see Appendix Table A.7). Relative to the average annual earnings of late-exposed mothers, the DDD estimates imply that early-exposed mothers experience a 17% earnings gain in the medium-run and a 6% earnings gain in the long run. Going to work earlier after a first birth thus has a meaningful and persistent effect on earnings.

Further, consistent with the lack of long-run impacts on employment, we find similar long-run effects on earnings when we restrict the sample to those with positive earnings or analyze log earnings in Appendix Table A.8 (columns 1-4). The DDD estimate on earnings conditional on working represents a 4.2% increase relative to the average earnings of late-exposed mothers with positive earnings. Winsorizing at the top one percent of earnings to avoid the influence of outliers also makes little difference (columns 5-6).

Examining the earnings distribution, we find that these earnings effects are concentrated in the EITC-qualifying region in the medium run, and become more diffuse in the long run (see Appendix Figure A.11). This suggests that the long-run impacts in earnings reflect impacts throughout the employment coefficients. In practice, the two strategies yield similar results.

³⁶The absence of non-monotonicities in the long-run earnings effects is consistent with the long-run employment fluctuations being concentrated on extensive margin decisions about small earnings amounts.

³⁷In particular, we find that early-exposed mothers have higher earnings than late-exposed mothers in each calendar year, and that the moderation of the DD coefficients reflects early-exposed mothers' earlier exposure to the Great Recession.

earnings distribution – possibly enabled by the accumulation of experience during early career – and not simply incremental growth in earnings.

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

4.3 Survey Evidence on Hours of Work and Fertility

For evidence on hours of work and fertility, we now turn to our sample’s survey responses in the CPS.³⁸ Recall that, unlike the administrative data, the CPS only contains outcomes for each mother for a single year and always after a first birth. As a result, we can not take differences in CPS outcomes between pre- and post birth outcomes, and instead implement either a single-difference design, comparing early- and late-exposed never-married mothers’ outcomes at each child age; or a double-difference design, adding a comparison across marital status.³⁹ To get closer to our main analysis, we also add controls for average employment and earnings in the five years prior to childbirth from the SSA records. Nevertheless, because we can not fully control for pre-birth outcomes, these results are more susceptible to bias, and thus more suggestive than our main results.

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

For hours of work, we find that in the short run, early-exposed mothers’ additional employment is concentrated in part-time work (column 2), and amounts to an increase of 2 to 3 hours more work per week ($p \geq 0.10$). However, in the medium-run, higher employment is completely concentrated in full-time work (column 3), and amounts to a 3 to 4 hour increase in hours work per week ($p < 0.01$). Hence, early-exposed mothers appear to switch to full-time work when their children

³⁸The results are the same if we include all (matched and unmatched) CPS mothers that meet our sample criteria.

³⁹We estimate the single-difference as:

$$Y_{ib\tau} = \alpha + \beta_1 \cdot \text{EarlyExposed}_b \cdot 0-4_\tau + \beta_2 \cdot \text{EarlyExposed}_b \cdot 5-9_\tau + \beta_3 \cdot \text{EarlyExposed}_b \cdot 10pl_\tau + \theta_\tau + \gamma X_{is\tau} + \delta P_{s\tau} + \epsilon_{ib\tau}$$

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

$$Y_{ib\tau} = \alpha + \beta_1 \cdot \text{EarlyExposed}_b \cdot 0-4_\tau \cdot NM_m + \beta_2 \cdot \text{EarlyExposed}_b \cdot 5-9_\tau \cdot NM_m + \beta_3 \cdot \text{EarlyExposed}_b \cdot 10pl_\tau \cdot NM_m + \theta_\tau \cdot \lambda_b + \theta_\tau \cdot \rho_m + \gamma_m X_{is\tau} + \delta_m P_{s\tau} + \epsilon_{ib\tau}$$

enter into primary school (as in Duchini and Van Effentere, 2018).⁴⁰ This increase in hours of work likely contributes to the growth in the earnings effects between the short- and medium-run documented above.

In the long run, there is no difference in the hours of work between early- and late-exposed mothers. We also do not find any long-run (nor any) effect on weeks of work (see Appendix Table A.9). Thus, the long-run increase in earnings that we document appears to reflect higher *wages*. In particular, our DDD estimate of earnings gains among workers above suggests that early-exposed mothers earn 4.2% higher wages.

To estimate impacts on hours of experience, we take a cumulative sum of impacts on annual hours of work (hours per week times weeks worked last year). Appendix Table A.9 shows the results. We find that early-exposed mothers accrue between 945 ($p < 0.05$) and 1,125 ($p > 0.10$) hours of work in the long run for the double difference and single difference, respectively. This represents between 0.675 and 0.80 of a year of full-time full-year work experience, if we use the common definition of working 35 hours per week and 40 weeks per year (e.g., Goldin, 2014; Autor et al., 2008).

These experience effects are between 0.06 and 0.35 years larger than we found using the administrative data, which suggests that the intensive margin effects that we observe in the CPS contribute meaningfully to experience. Consistent with this, we find that two-thirds of the increase in experience effect that we find in the CPS occurs during the medium-run – the period with the largest impact on hours worked. This underscores the value of having data on hours worked in order to get an accurate estimate of experience.

Finally, we consider whether early-exposed mothers make different fertility choices, in terms of number of children or birth spacing. For this analysis, we limit our sample to women between the ages of 36 and 44, who are more likely to have completed their childbearing (although our results are not sensitive to this restriction). We present our results in Appendix Table A.10. We find no significant effect on any outcome, and the magnitudes allow us to rule out effects larger than a 0.15 increase in early-exposed mothers’ number of children (a 7% effect).

5 Robustness

In this section, we address the threats to identification previewed in Section 3.1. We first focus in Section 5.1 on threats to the short-term results, showing that the rise in employment can not be explained by unobserved trends, welfare reform, or other contemporaneous shocks. While earlier EITC papers have run versions of some of these tests, our large-scale panel data permit us to uniquely examine trends in pre-birth employment, and to run more demanding specifications that control for pre-birth maternal employment. In Section 5.2, we then run a battery of tests centered on potential confounds for the long-run results. In particular, we rule out potential concerns that

⁴⁰This does not appear to be a purely “mechanical” effect of children aging. While 67% of late-exposed employed mothers work full-time in the CPS in the medium run, our estimates suggest that 100% of marginal early-exposed mothers work full-time.

our results are driven by comparisons across years; using married women as a comparison group; our choice of unemployment controls; or the CPS surveys that are included in our sample. For transparency we also show that our short-run results survive these tests, although this is not our focus.

5.1 Are Mothers Responding to EITC Work Incentives in the Short Run?

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

We implement the first test by re-estimating our DD and DDD models replacing “*PostBirth · EarlyExposed*” with separate interactions between “*PostBirth*” and a set of indicators for having a first birth between ’90-91, ’92-93, ’94-95, or ’96-97. If our effects were driven by an ongoing upward trend, then we would expect all four coefficients to be positive and to increase across cohorts. Contrary to this, Appendix Figure A.12 shows little change in employment upon motherhood for pre-reform cohorts: mothers that have a first birth in ’92-’93 work as much after childbirth (relative to pre-childbirth) as those with a first birth in ’88-89. Importantly, this is true both for the DDD as well as the DD, which implies that married and unmarried mothers’ employment was not diverging pre-reform. Subsequent cohorts have a sharp change in post-birth behavior. For births beginning in 1994, post-birth employment increases by 5 to 7 p.p. We find slightly larger effects on the employment of ’96-97 mothers than ’94-95 mothers, which is consistent with increasing awareness of the program as well as with the more generous phase-in rate that took effect in 1995.⁴¹

To implement the second test, we use the never-married “all births” sample. We then modify our triple-difference model to ask: is the change in employment after a 2+ birth between early- and late-exposed never-married mothers larger than the change between early- and late-exposed never-married mothers after a first birth?⁴² Column 1 of Table 4 shows that employment increases by 3.2 p.p. more after a 2+ birth relative to a first birth. Column 3 shows that the rise in working is slightly higher for 3+ births relative to second births, but the difference is not statistically significant. This pattern aligns with EITC incentives, and is inconsistent with an alternative explanation that predicts strictly increasing effects by birth parity, such as from higher rates of welfare participation or lower base rates of employment (Kleven, 2019).⁴³

⁴¹For dynamic effects on employment by year of first birth, see Appendix Figure A.13.

⁴²Specifically, we redefine ρ_m and NM_m in Equation 2 to be indicators for being a 2+ mother.

⁴³Nevertheless, we do not focus on the comparisons across parity of child as a primary specification since mothers

Third, we consider the potential role of confounding variation due to the booming economy and welfare reform, a point recently emphasized by Kleven (2019). For an in-depth discussion of the argument in Kleven (2019), and how it relates to our paper, see Appendix C. For all following analyses in this section, we include the DD results for never married mothers in Table 4, and present the very similar DDD results in Appendix Table A.11.

To test for confounders related to the economy, we examine whether early-exposed mothers' employment increased more in states that experienced larger declines in unemployment rates during the 1990s. We do not find that this is the case: columns 3 and 4 of Table 4 show that the employment effects are very similar for states with above-median and below-median changes in the unemployment rate between 1994–2000 and 1988–1993. This is despite the fact that the average change in unemployment was three times as large in the above-median states (-1.8 p.p. versus -0.6 p.p.). Hence, our employment effects hold to a similar degree even in states that experienced relatively weak economic growth.⁴⁴

Further, in columns 5 and 6 of Table 4, we allow the coefficients on our baseline unemployment and welfare controls to vary by the age of one's first child, to address potentially larger responses to the economy and welfare reform for mothers with young children (Kleven, 2019). The additional unemployment controls have virtually no effect. The additional welfare controls reduce the coefficients by up to 18 percent, but our conclusions are substantively unchanged.

In the last two columns of Table 4, we restrict our analysis to the years up to 1996 to limit the potential influence of federal welfare reform. We present event study coefficients for these results to address the fact that this restricted window creates imbalance in event time, and show the results for all states (column 7) and for states that did not pass any welfare waivers prior to 1997 (column 8).⁴⁵ The coefficients are similar to our main event study, and statistically significant in years 2 and 3 (see Appendix Figure A.14 for the complete graphs). Further, we do not find meaningful differences across waiver and non-waiver states. This suggests that while welfare reform may have reinforced the return to work after birth, it can not explain the majority of our findings, consistent with Meyer and Rosenbaum (2001), Grogger (2003b), and Bastian and Jones (2020).

Fourth, we examine whether the effects on employment vary with the presence of a supplemental EITC in the mothers' state of residence. Indeed, Appendix D shows that early-exposed mothers' employment increases more in states that have a supplemental EITC and higher state supplements. Thus, early-exposed mothers' employment grew more in states that had a larger EITC incentive to work.

Finally, while our results suggest a primary role of the EITC in our employment results, we can also conservatively interpret our long-term results as reflecting the weighted sum of work incentives stemming from the EITC and welfare changes. As mentioned in Section 1, this would not change

are likely to have already experienced child-related gaps in work experience prior to a 2nd (or higher-order) birth. Thus, immediate return to work may have a different (and possibly weaker) effect for mothers after a 2nd birth than for mothers after a 1st birth.

⁴⁴Earnings effects are also similar across these states – see Appendix Table A.12.

⁴⁵The no-waiver states include Alaska, Colorado, Hawaii, Idaho, Louisiana, Minnesota, North Dakota, New Mexico, Nevada, New York, Pennsylvania, Rhode Island, and Wyoming, as well as Washington DC.

our interpretation of the long-run results as stemming from exogenous changes in early employment, and would only minimally change the attribution of our short-term results.

5.2 Robustness of Long-Run Effects

Calendar-year event studies First, a potential concern with our DD estimates is that they may reflect differences in year effects across early- and late-exposed mothers. Therefore, we use an alternative estimation strategy to compare early- and late-exposed mothers in the *same calendar year*. In particular, we plot calendar-year event studies (i.e., coefficients on calendar-year dummies) for early- and late-exposed mothers and, for context, also show year effects for mothers that have a first birth in the years around our core sample (i.e., 1986-87 and 1997-99). Similar to the main analysis, we omit the year prior to the earliest childbirth in each group.⁴⁶

Consistent with our DD results, Appendix Figure A.15 shows that early- and late-exposed never-married mothers converge to a similar rate of employment in the long-run (which is roughly equal to pre-birth employment), but that early-exposed mothers earn on average \$1,500 to \$2,000 more per year than late-exposed mothers. Importantly, the *gap* in earnings does not attenuate over time, although not surprisingly the earnings of *all* mothers dip around the Great Recession at the end of our period. This supports our hypothesis that the moderation of the DD effects relative to the DDD in our main event study reflects these year effects.

The figures also confirm that these mothers' employment increased immediately post-reform: after 1994, employment declines less after childbirth for early-exposed mothers, and also rebounds more quickly for late-exposed mothers. Post-reform earnings increase more slowly, but this is to be expected given our evidence that many mothers initially worked part-time and had relatively low earnings.

Additionally, we find that the long-run coefficients on earnings for early- and late-exposed never-married mothers are very similar to those for mothers that have a first birth in the surrounding years. In other words, there is a clear chasm in earnings between mothers who are exposed to the reform at birth (first births: 1993–1999) compared with those exposed several years after birth (first births: 1986–1991), but not within each of these groups. Hence, our conclusions are neither sensitive to making within-calendar-year comparisons nor to adding more cohorts of mothers.

For married mothers, we continue to find negligible impacts across early- and late-exposed mothers using this calendar-year design. While Appendix Figure A.16 shows that there are some gaps in within-year employment between the early- and late-exposed mothers, these appear to entirely reflect predictable differences in child age.

Notably, for both married and never-married mothers that gave birth *pre-reform*, we find similar

⁴⁶Specifically, denoting calendar year as t , we estimate:

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

patterns of employment around birth. We highlight this by plotting these mothers together in Appendix Figure A.17. While the levels are not identical across the groups, they exhibit comparable fluctuations in employment and earnings post-childbirth. This provides yet another piece of support for our use of married mothers as a comparison group.

Assessing the role of economic conditions Second, one could still be worried that the long-run improvements in earnings are driven by exposure to different economic conditions across early- and late-exposed mothers. To allay such concerns, we consider how much of our results this could quantitatively explain in Appendix Figure A.3. The figure shows that early-exposed mothers experienced higher employment-to-population rates relative to late-exposed mothers in the years after childbirth (part of which could be due to the EITC). However, the average magnitude of the difference is small – only one-fourth of our short-run effects on employment. Further, it shows that early-exposed mothers experienced 2–4 p.p. *lower* employment rates 14 to 19 years since childbirth, suggesting that national employment trends could bias our long-run DD effects downwards.

We also test the sensitivity of our results to allowing for greater flexibility in our controls for economic conditions. Allowing the impact of unemployment rates and welfare reform to vary with the age of one’s child (as in our short-run results) does not change any of our long-run effects – see Appendix Table A.14. We also try controlling for state-level unemployment rates specific to women or low-skilled individuals (calculated from the March CPS) — which might better proxy for single mothers’ employment opportunities — and including state-year fixed effects. Appendix Figure A.18 shows that our results are very similar across these specifications, though using group-specific unemployment rates yields smoother long-run impacts on employment.

Childless and lower-income comparison groups Third, we test the sensitivity of our DDD results to using single women without children as a comparison group instead of married mothers. In particular, we use as comparison either (i) women who will *soon* be mothers (“future mothers”) or (ii) women who will *never* be mothers (“childless”). This addresses the potential concern that the earnings of all single women may have improved during the 1990s (e.g., from the economy), and more so than the earnings of married women.

Our “future mothers” comparison group consists of never-married women who have first births four years after the mothers in our core sample (i.e., in 1992–1995, 1997–2000),⁴⁷ which ensures overlap in current age with actual mothers. We assign each future mother a fake year of childbirth, \hat{b} , equal to her true b minus 4, and set her fake “years since first birth” as the current year minus \hat{b} . Hence, “pre-birth” and “post-birth” consist of the same sets of calendar years for all mothers that have the same “year of childbirth.” By construction, we can only use this comparison to estimate short-run effects (prior to a future mother’s first birth). If there is a confound that differentially affects single women, then future-mothers with post-1993 “births” should experience gains in employment relative to future mothers with pre-1993 “births”, and drive our DDD to

⁴⁷We construct this sample independently, such that a woman can be both an “actual” and “future” mother.

zero. Reassuringly, the DDD in Table A.13 produces similar-sized increases in employment as our baseline results.⁴⁸

Our “childless” comparison group consists of women that we observe between the ages of 40 to 45 without any children in the household. To assign \hat{b} , we follow Kleven, Landais and Sogaard (2019), and take a random draw from the distribution of b among never-married mothers who have the same year of birth and level of education as a given childless woman. Then, as above, the fake “years since first birth” is the current year minus \hat{b} . Again, if there is a confound, then childless women with post-1993 “births” should have better outcomes relative to childless women with pre-1993 “births,” and lead our DDD to produce no effect. To get a population closer to never-married mothers, we also run this analysis restricting the comparison to childless women who have at most (i) some college or (ii) a high school degree; and for completeness, also try using married mothers who were EITC eligible pre-birth, extending our short-run analysis.

Figure 5 plots all of our long-run DDD estimates against the average labor market outcomes (employment and earnings conditional on working) of the comparison group over the whole sample period, and includes a reference line with the average outcome for never-married mothers. This allows us to visualize how similar each comparison group is to never-married mothers, and whether our estimates vary when we use the lower-income comparison groups. We note that each of these comparison groups is much smaller than our married comparison, which reduces the power of these analyses.

The figure shows that across these childless and low-income comparisons, early-exposed mothers earn \$605 to \$1,900 more in the long-run, with most estimates above \$1,000 and confidence intervals that easily include our main estimate. Further, we see no systematic relationship between the size of our estimate and the average employment or earnings across our comparison groups.⁴⁹ This makes clear that our effects can not be explained by shocks to either single women or lower-income women.

Additional specifications Fourth, we show that our results are unchanged when we introduce richer controls for individual characteristics. Appendix Table A.15 shows that our short- and long-run results hold when we use inverse p-score reweighting to impose balance in pre-birth employment and demographics. Similarly, Appendix Table A.16 shows that our results are not sensitive to allowing the effect of mother’s age to vary with the age she first gave birth; including individual fixed effects; or restricting the sample to mothers who are CPS heads of household, who may be more strongly affected by EITC incentives.

Alternative sample restrictions Fifth, we re-run our results using alternative sample restrictions to address potential concerns about bias from measurement error, previewed in Section 3.1.

⁴⁸Consistent with this, trends in employment for future mothers and married mothers are very similar over this period, and much flatter than the trend for never-married mothers. See Appendix Figure A.19.

⁴⁹Our short-run results are also the same when we use these alternative groups, and we find no evidence of pre-trends — we omit these results for brevity.

To test for positive selection among “surviving” never-married mothers across survey years, we look for an upward trend in our estimates when we successively only keep individuals interviewed in the CPS between 0–8, 0–9, ..., and 0–20 years from first birth. We find no such trend: Appendix Figure A.20 shows that our earnings results are nearly identical when we only keep mothers interviewed within 8 or within 20 years of birth, and are generally similar across years (although the confidence intervals are wider when we use a smaller sample). We also do not find smaller impacts on earnings when we successively only keep mothers interviewed *further* from first birth (see Appendix Figure A.21).⁵⁰ Moreover, we also find similar effects when we successively drop mothers who were relatively older (39–49) at CPS interview, some of whose children may have already left home (see Appendix Figure A.22). This assures us that our qualitative results are robust to a variety of assumptions about how measurement error could affect our sample.

Randomization inference Last, we use randomization inference as an alternative method of obtaining confidence intervals for our estimates. In particular, we randomly assign a placebo “early-exposure” to four randomly chosen years of first birth drawn without replacement, and estimate a placebo effect using this definition. We do this 500 times for each of our main outcomes, and plot the resulting distribution of estimates in Appendix Figure A.23. The one-sided p-values for short-term employment and long-term earnings are between 0.01 and 0.02.

6 Why do Early-Exposed Mothers Earn More?

Our results show that early-exposure to work incentives causes mothers to earn more at every stage of their careers. In this section, we explore potential explanations for higher long-run wages.⁵¹

A leading explanation for early-exposed mothers’ higher wages is increases in experience.⁵² Our earlier results provide some indirect evidence for this mechanism: correlationally, earnings and experience increased together. Also, consistent with concave returns to experience, early-exposed mothers’ earnings gains make up a decreasing share of earnings over time (i.e., from 10.8% to 5.1% between years 10 and 19 after first birth).

As a more direct test of this mechanism, we ask whether the mothers that experience higher earnings are the *same* mothers that were induced to work after a first birth. To avoid conditioning on post-birth experience (which is an outcome of early exposure), we run regressions where the outcomes are indicators for the four possible combinations of having “high” or “low” earnings crossed with having “high” or “low” experience. We define “high experience” as having worked during each of the first three years after a first birth ($1 \leq \tau \leq 3$) to capture short-run responses to

⁵⁰Earnings effects are also similar for women surveyed 0–4, 5–9, 10–14, 15–20, or 20+ years from first birth.

⁵¹In the short run, higher earnings are largely explained by increases in employment. In the medium-run, the effects on earnings appear to be explained by a mix of growth in employment, hours, and wages. We provide a back-of-the-envelope calculation about the role of each of these in explaining the difference between the short- and medium-run impacts on earnings in Appendix E.

⁵²A related possible explanation is that our earnings effects reflect the impact of gaining experience during a good economy. This seems unlikely because we find similar short- and long-run earnings effects across women that experienced weak or strong economic growth in their state of residence post-childbirth.

post-birth work incentives. We define “high earnings” as having earnings in the top 25 percent of mothers in each year, which we find is the best binary proxy for the impact of early exposure on earnings.⁵³ If greater experience is driving our effects on earnings, then we would expect to find an increase in the likelihood of having “high earnings and high experience,” but a decrease or no change in the likelihood of having “high earnings and low experience.” We also do not expect any effect on the share of mothers that have high earnings among “low experience” mothers (i.e., in the return to low experience).

Panel (a) of Figure 6 presents long-run DDD effects (and 95% confidence intervals) on indicators for these four outcomes: having “high earnings and high experience,” “high earnings and low experience,” “low earnings and high experience,” and “low experience and low earnings.”⁵⁴ In line with our hypotheses, we find that early-exposed mothers are significantly more likely to have “high earnings and high experience,” and are less likely to have “low earnings and low experience.” They are also more likely to have “low earnings and low experience,” consistent with the idea that high experience does not correlate perfectly with high earnings.

Panel (b) shows that, as shares, 21% of the additional early-exposed mothers that obtain high experience end up having “high earnings.” We obtain this by dividing the first coefficient in Panel (a) by the sum of the first and third coefficients in Panel (a). Notably, this is very similar to the 19% share of high-earners among all high-experience never-married mothers, shown in the second bar in Panel (b). Early-exposed mothers also have a similarly small share of low-experience mothers that have high earnings as all mothers (3–6%), as shown in the third and fourth bars.⁵⁵ Hence, early-exposed mothers appear to have similar returns to experience as the average never-married mother. These results support changes in the quantity of early experience as a main mechanism for our earnings gains.

If experience were the only source of wage gains, the implied return to a full-time full-year of work would be 6.2 percent ($4.2\%/0.675$), based on our estimate of the average impact on earnings conditional on working and our smaller estimate of the hours-inclusive increase in experience from the CPS. As discussed in the introduction, this falls in the range of prior estimates for similar populations,⁵⁶ although our precision contrasts with the imprecise and negative returns in Card and Hyslop (2005). Relative to Card and Hyslop (2005), our shock to experience is twice as large, which increases our precision. The return to experience may also be higher in our setting because working after childbirth provides a costly signal to employers of one’s commitment to work (Thomas, 2019; To, 2018).

Second, it is possible that early-exposed mothers obtain higher returns to experience by choosing different occupations. For instance, Adda et al. (2017) find that the returns to experience are higher

⁵³We provide further details on this as well as results using an alternative measure of “high experience” in Appendix E.

⁵⁴See Appendix Table A.17 for the DD estimates.

⁵⁵See Appendix E for the details of this calculation.

⁵⁶Prior studies such as Gladden and Taber (2000) and Adda et al. (2017) do not provide information about the average number of working hours per year of experience, though, which makes it difficult to directly compare our estimates.

in “abstract” occupations that have more analytic or interactive tasks. We find some imprecise support for this channel when we look at CPS occupations – see Appendix Tables A.18 for impacts on service occupations and A.19 for impacts on non-service occupations. In the long run, early-exposed mothers are 4 p.p. more likely to be in health occupations ($p < .05$) and 5 p.p. less likely to be in clerical occupations ($p < 0.1$). However, we find inconsistently-signed and noisily-estimated changes across the thirteen other job categories. Given this, it is unclear whether the increase in health occupations is a true effect of early exposure or noise in the data. However, even taking the increase in health occupations at face value, the effect is too small to explain much of the total increase in earnings.⁵⁷

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

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

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

7 Effects on Taxes, Transfers, and Net Income

Thus far, we have focused on the impact of early-exposure to work incentives on *gross* earnings in order to measure the return to experience. However, another relevant question is: do early-exposed mothers have more *net* income, taking into account income taxes, government transfers, childcare expenses? This exercise allows us to get closer to understanding the potential impacts of early exposure on the long-run well-being of mothers and children.

Our baseline calculations of impacts on net use estimates from our DDD specification and a discount factor of 5 percent to obtain the present value (PV) of the impact of early exposure. For brevity, we sum up these effects to obtain the total effect over the medium-run (i.e., years 0 to 9 post-birth) and the long-run (i.e., years 10 to 19 post-birth). For the very similar DD estimates, see Appendix Figure A.24.

⁵⁷In order to explain the entire increase in long-run earnings, the average earnings in health services would have to be \$34,825 ($\frac{1393}{.04}$) higher than in early-exposed mothers’ other occupations.

Earnings The first two bars of Figure 7 show the PV of the impacts on early-exposed mothers earnings', which are \$15,348 and \$7,956 in the medium- and long-run, respectively.

EITC Next, we simulate the potential EITC benefits for each mother and child age using household earnings and the 1-child EITC schedule for 1989 first births (if late-exposed) or for 1994 first births (if early-exposed).⁵⁸ This gives the EITC amount that a household is *eligible* to receive in each year. The third bar in Figure 7 shows that over the medium run the present value of early-exposed mothers' total EITC benefits increases by a substantial \$2,570. Not surprisingly, 81% of this increase in benefits is experienced during the short-run, consistent with the large post-childbirth increase in employment near the first EITC kink. However, the fourth bar shows that over the long run, the present value of early-exposed mothers' EITC benefits decreases by a total of \$240, as their earnings begin to surpass the EITC benefits region.

Income taxes To obtain a back-of-the-envelope estimate of federal income taxes owed, we take the product of early-exposed mothers' average tax rate and their additional annual earnings. We estimate early-exposed mothers' average tax rates from our distributional earnings results and the NBER TAXSIM federal tax rates (Feenberg and Coutts, 1993): 0% in the short run, 5% in the medium run, and 13% in the long run (see Section F.1 for details). Based on this, early-exposed mothers would be expected to pay the equivalent of \$524 and \$1,034 more in federal income taxes in the medium- and long-run, respectively, in present value terms (which *reduces* net income, as shown in the third pair of bars in Figure 7).

Means-Tested Transfers To estimate effects on program participation, we rely on self-reported measures from the CPS and use the estimation strategies in Section 4.3. We focus on impacts on the value of benefits received from the largest transfer programs, including welfare benefits, disability benefits, food stamps/SNAP, the value of Medicaid, and housing subsidies. The fourth pair of bars in Figure 7 shows the sum of the effects across all of these categories. We find that transfers decline by \$6,534 during the medium-run – consistent with prior evidence of meaningful reductions in program participation from the EITC (Hoynes and Patel, 2018; Bastian and Jones, 2020) – and by \$123 during the long-run. See Appendix Table A.20 for estimated effects on individual programs, and Appendix F.2 for a detailed discussion of the definitions and availability of these CPS variables, as well as the potential for misreporting to affect our results (see, e.g., Meyer et al., 2015).

Child care costs Last, we conservatively estimate child care costs using the average weekly cost of care for unmarried mothers during the early 1990s from Anderson and Levine (2000) (\$41.60 in 2016 dollars).⁵⁹ If we assume that care is needed for 52 weeks, then the annual cost for each

⁵⁸In particular, the EITC benefit for an early-(late-) exposed mother with a child of age τ is calculated using the one-child EITC schedule from tax year $t = 1994 (1989) + \tau$ applied to household earnings in τ . We assign zero EITC in the years pre-birth. The results do not change if we allow the EITC schedule to vary for each year of first birth.

⁵⁹We calculate this as the inflation-adjusted weighted average of the cost of each type of child care, where the weight is the share of unmarried moms that use each type of care times the share that pay anything for care. See the

early-exposed woman who is induced to work is \$2,163. In turn, the present value of the cost for all early-exposed women over the first five years of a child’s life would be \$600, based on the 0.37 cumulative increase in the share of early-exposed mothers employed over the short run (which *reduces* net income, as shown in the fifth pair of bars of Figure 7).

Net Income Based on these calculations, early-exposed mothers are expected to have a higher net income in the medium- and long-run. The last pair of bars in Figure 7 shows that the accumulation of these effects leads to a \$10,060 increase in net income in the medium run, and an additional \$6,560 in the long run. Hence, over twenty years, maternal income increases by a substantial \$16,620 in present value terms. While this is not an exhaustive accounting, it suggests that early-exposed mothers have more financial resources over any horizon. Moreover, our results show that following women up to 20 years after childbirth yields significantly larger estimates on their well-being relative to studies focusing on the short- or medium-run only.

Even so, it is difficult to conclude whether early-exposed mothers’ *welfare* is improved from the expansion. Such an argument would require incorporating information on, e.g., non-wage forms of compensation, the value of lost leisure, and impacts on children, which are outside the scope of this study. Nevertheless, our estimates on earnings are a necessary input for this assessment.

MVPF With these inputs in hand, we can also assess the long-run fiscal impact of the expansion as given by the MVPF, building on existing short-run estimates (Hendren and Sprung-Keyser, 2019; Bastian and Jones, 2020). In particular, we compare the value of the additional EITC transfer to mothers to the net cost to the government, inclusive of effects on taxes and transfers, following Hendren and Sprung-Keyser (2019) and Bastian and Jones (2020). For details, see Appendix F.3. A key caveat is that we calculate the MVPF under the assumption that these responses are solely due to changes in the generosity of the EITC after a first birth.

If we conservatively focus only on the impacts of early exposure on earnings and taxes (ignoring the reduction in means-tested transfers), we obtain an MVPF between 1.26 and 1.30, depending whether we use the DDD estimates or the DD estimates. The MVPF increases to at least 5.6 if we incorporate the (at least) \$831 decline in transfers.⁶⁰ In words, this means that the value of the expansion for never-married mothers is between 1.3 and 5.6 times as large as the cost to the government. Importantly, we find that including long-run impacts makes a meaningful difference in our MVPF. If we focus only on effects up to the medium-run, we obtain an MVPF that is between 0.4 to 0.6 of our baseline long-run MVPF. This implies that previous MVPF estimates that focused on short- and medium-run effects of the EITC on earnings (Hendren and Sprung-Keyser, 2019; Bastian and Jones, 2020), are likely to be lower bounds on the long-run MVPF.

fourth panels of Tables 2 and 3 of Anderson and Levine (2000) for inputs.

⁶⁰We show a range of MVPFs across a variety of different assumptions in Appendix Figure A.25.

8 Conclusion

This paper provides new evidence on the impact of increasing post-birth work incentives on mothers' long-run career trajectories. For identification, we use a large-scale panel of administrative earnings linked to the CPS, and variation in the timing of the 1993 EITC expansion around a first childbirth. We find that never-married mothers exposed to more generous EITC incentives at first birth, rather than 3 to 6 years after birth, have 3.4 to 3.7 p.p. (5.9 percent) higher rates of employment in the first five years after a first birth. Ten to nineteen years after a first birth, early-exposed mothers have the same employment rate as late-exposed mothers, but have accumulated at least 0.5 to 0.6 years of additional work experience. They also earn \$1,206 to \$1,392 more on average, which translates to a 4.2 percent increase in earnings conditional on working. We find no effect on hours of work in the long run which suggests that early-exposed mothers earn higher *wages*. These results suggest that there are steep returns to work incentives at childbirth that accumulate over the life-cycle.

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

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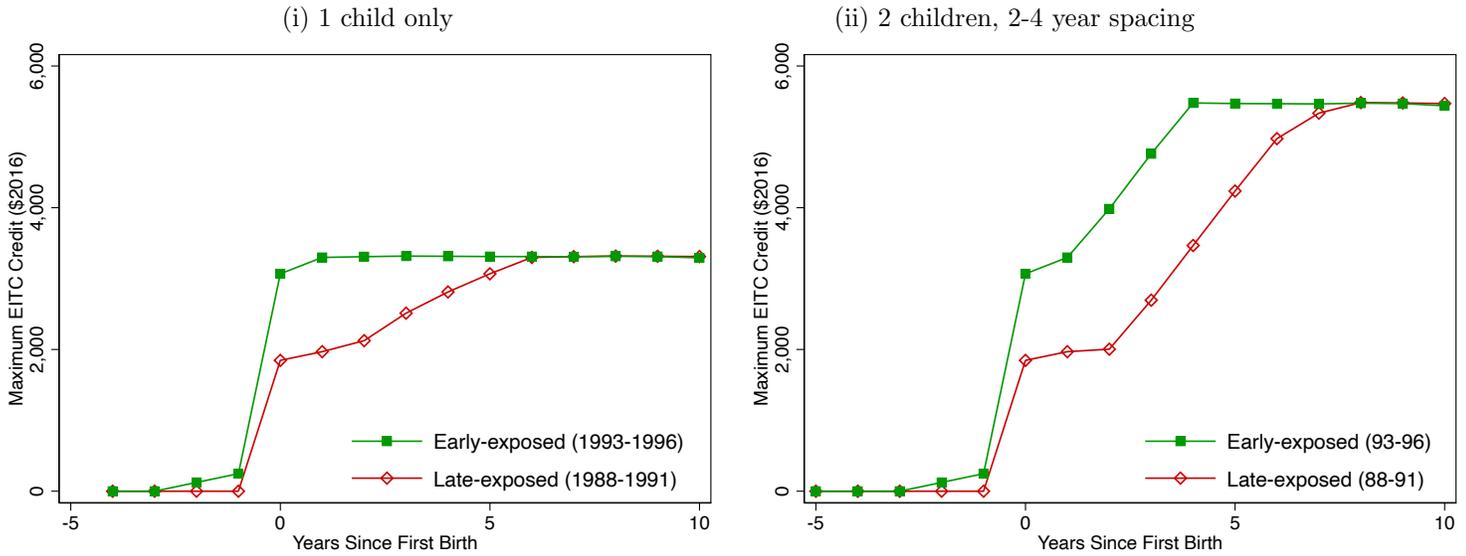
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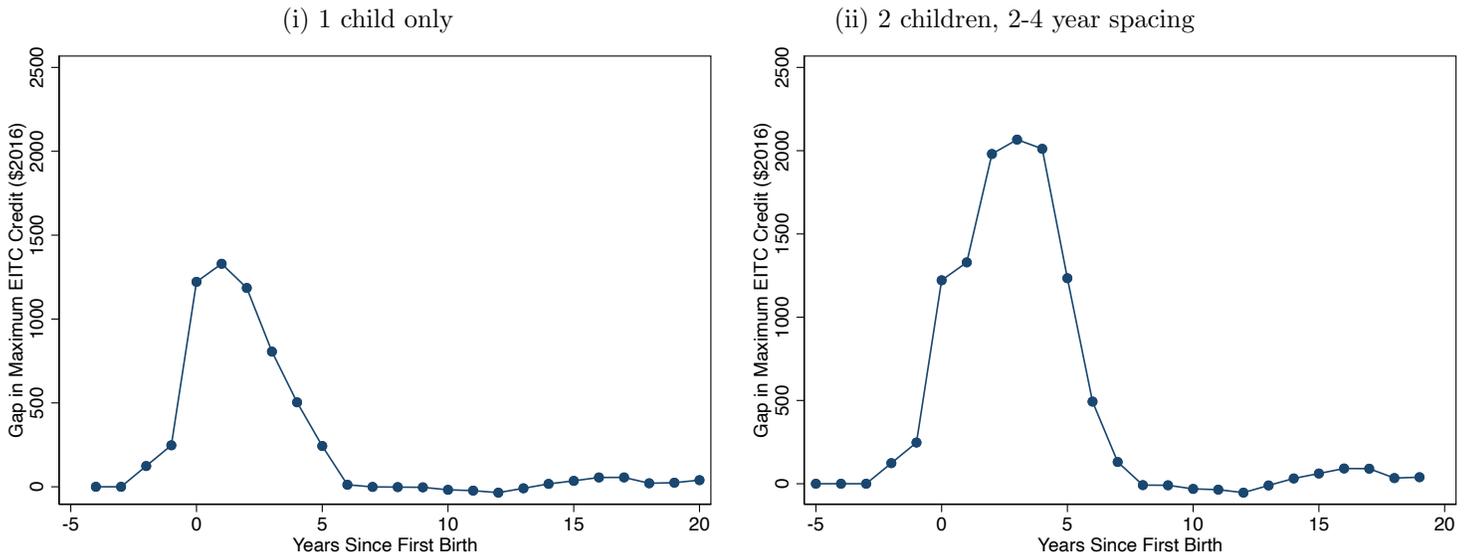
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Figure 1: Illustration of the Gap in the Maximum EITC Between Early- and Late-Exposed Mothers

(a) Average Max. EITC

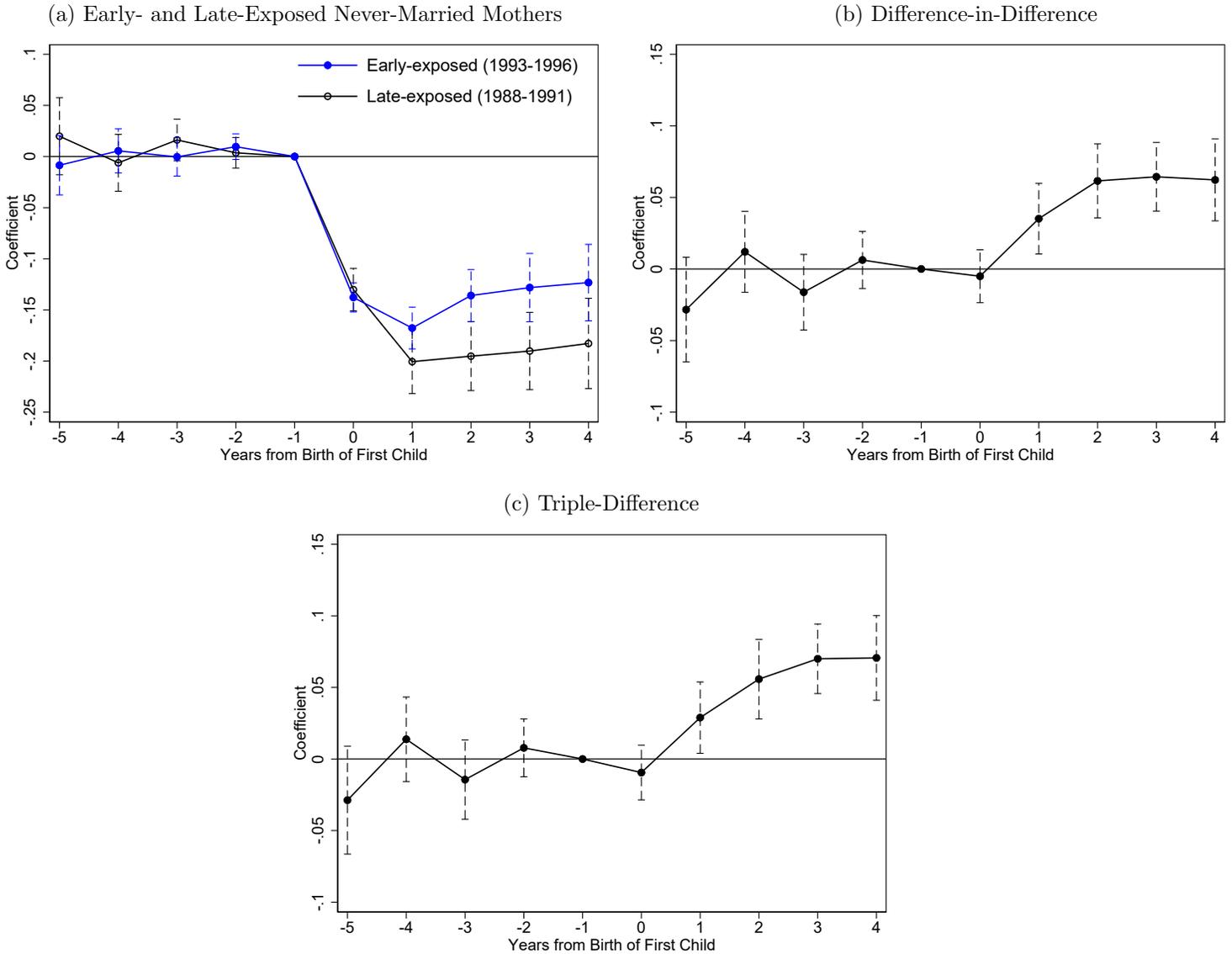


(b) Difference Between Early- and Late-Exposed Max. EITC



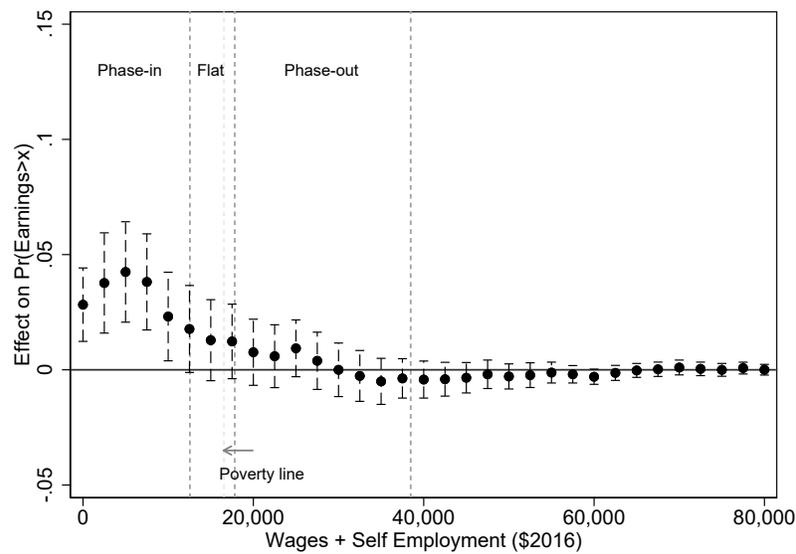
Notes: Panel (a) shows the average maximum EITC benefits in each year since first birth for mothers that are exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991) assuming that mothers only have 1 child (subfigure i) or assuming that mothers have two children (subfigure ii), spaced with a uniform probability between years 2 and 4, such that the average spacing is three years. Panel (b) presents the difference in benefits between early- and late-exposed mothers shown in Panel (a). *Data:* Nominal EITC benefits are obtained from the Tax Policy Center (<https://www.taxpolicycenter.org/statistics/eitc-parameters>), and have been converted to 2016 dollars using the CPI from the Bureau of Labor Statistics.

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



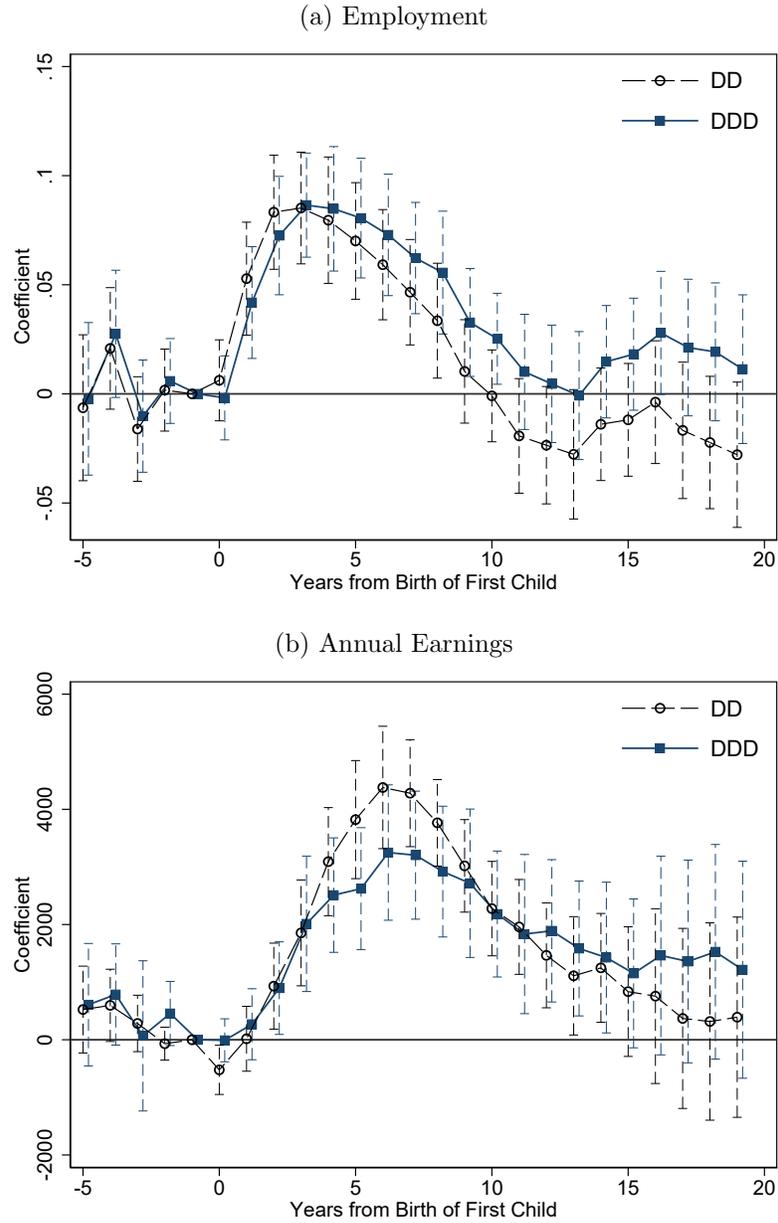
Notes: These figures present the point estimates and 95% confidence intervals from event studies of employment around birth for mothers who were exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). Panel (a) plots the estimates on indicators for years since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

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



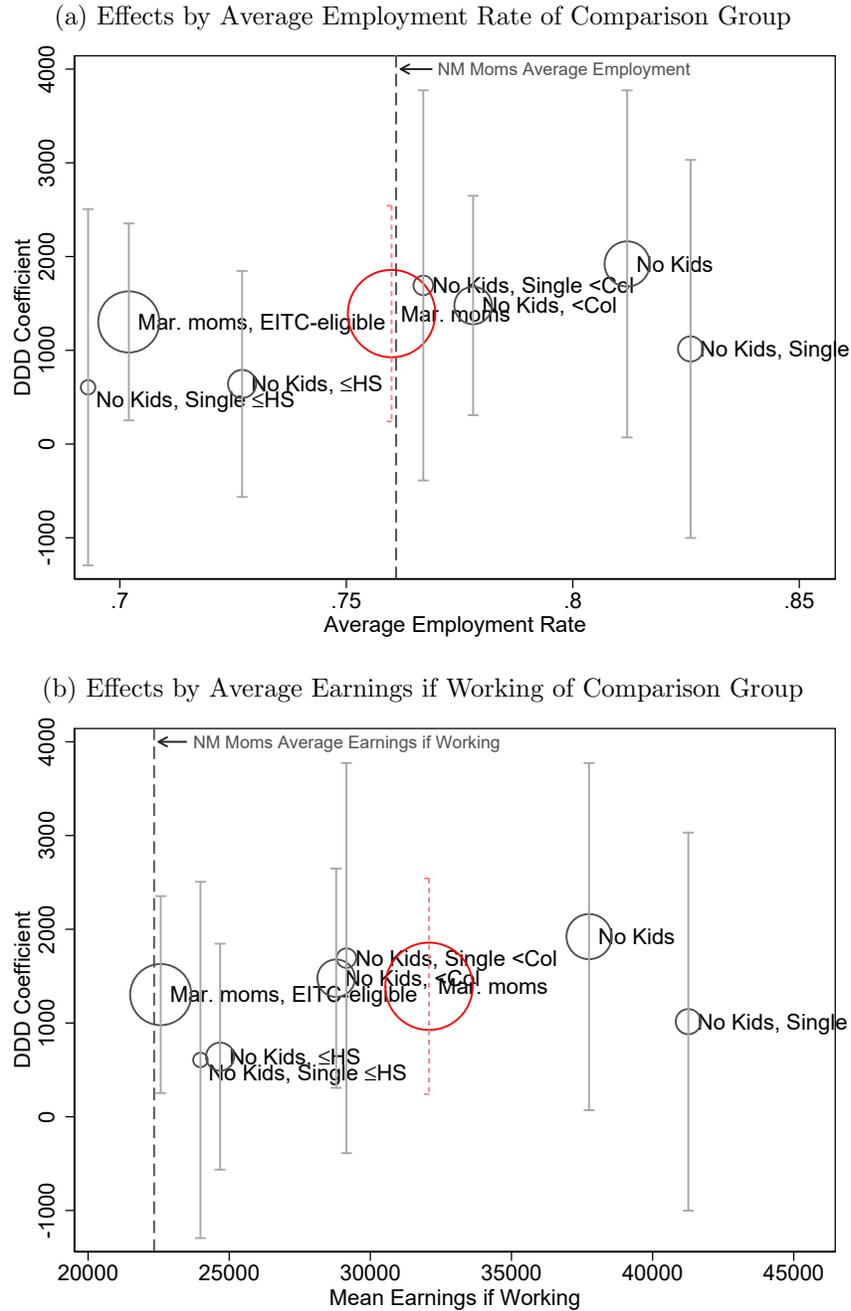
Notes: This figure shows the coefficients and 95% confidence intervals from DDD regressions that compare the earnings distribution of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), across never-married and married mothers. Each marker is obtained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

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



Notes: These figures present the coefficients and 95% confidence intervals from event studies that compare the employment (Panel a) or earnings (\$2016, Panel b) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), in each year from a first birth. For each outcome we present both the DD using never-married mothers as well as the DDD in which we use married mothers as an additional comparison group. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

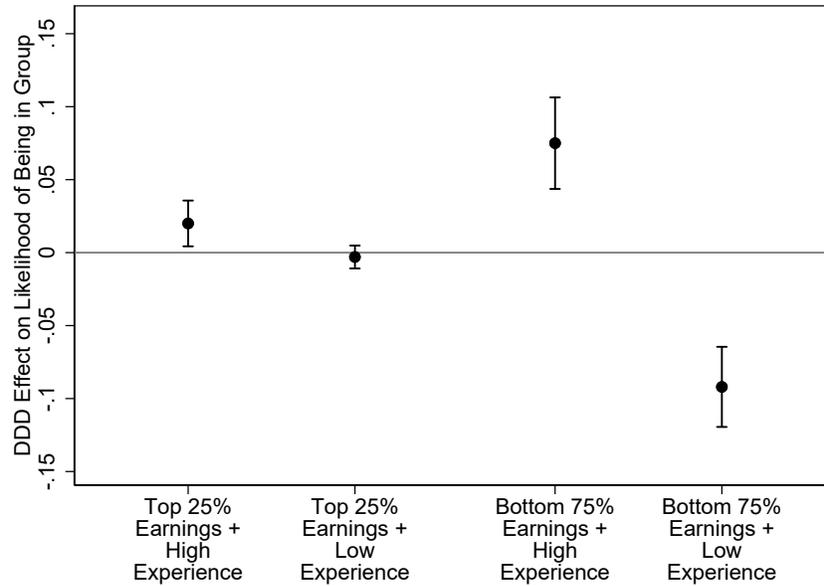
Figure 5: Long-Run DDD Effects on Earnings Across Comparison Groups



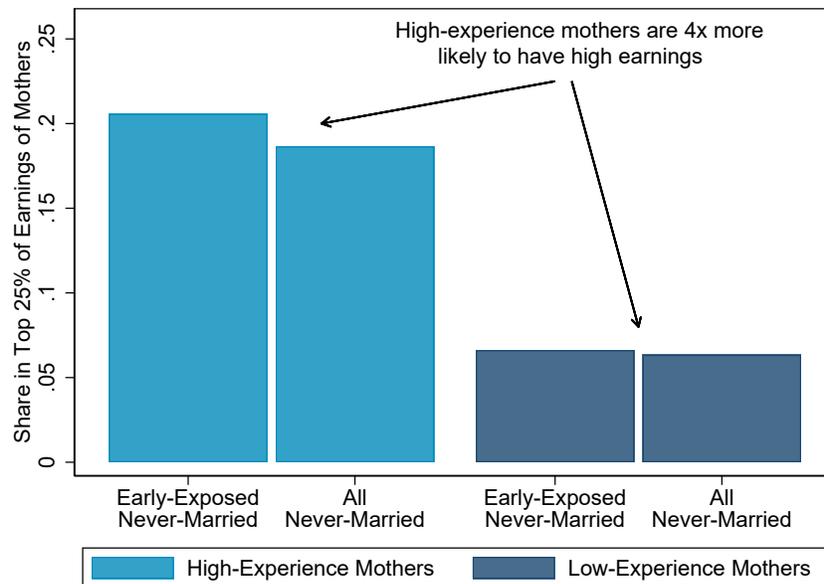
Notes: These figures show coefficients and 95% confidence intervals for the interaction “10+ Yrs From Birth * EarlyExp * NM” from DDD regressions of employment (Panel a) or earnings conditional on working (Panel b) using varying comparison groups (indicated by the label next to the marker). The size of the marker is proportional to the number of individuals in the comparison group. The red marker shows our main DDD estimate using married mothers. The x-axis shows the average employment (Panel a) or average earnings conditional on working (Panel b) for each comparison group measured over all years. The grey vertical line shows the corresponding average outcome for never-married mothers. Childless women (labeled as “no kids”) are assigned a placebo year of first birth by taking a draw from the distribution of years of birth for never-married mothers who have the same year of birth and level of education as a given childless woman. All regressions include fixed effects for the year of first childbirth, mother’s age, race, education, state of residence, the state-level unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, implementation of six types of welfare waivers, implementation of any waiver of TANF, and implementation of the 2009 EITC reform. See the notes of Figure 2 for information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a “first birth” up to 19 years after a “first birth.”

Figure 6: Effect of Early Work Incentives on Jointly Having “High Earnings” (Top 25%) and “High Experience” (Worked in 3 Years Post-birth)

(a) Estimated Effect on Joint Earnings and Experience Outcomes

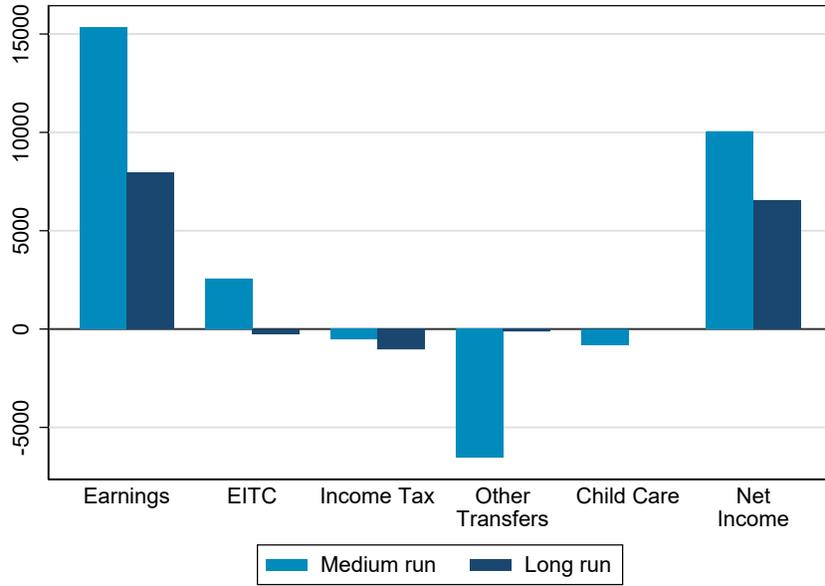


(b) Proportion of High-Earners among High- or Low-Experience Mothers



Notes: Panel (a) present the coefficients on the interaction “10+ Yrs From Birth * EarlyExp * NM” from DDD regressions using never-married and married mothers where the outcomes are indicators for jointly having: “high earnings and high experience”, “high earnings and low experience”, “low earnings and high experience”, and “low earnings and low experience”. “High earnings” and “low earnings” are defined as having earnings in the top 25% or bottom 75% of the earnings distribution. We define these distributions separately for each year since first birth and include both married and never-married mothers. “High experience” and “low experience” are defined as having worked in each of the three years after a first birth or not, respectively. Panel (b) presents the proportion of high earners among high-experience early-exposed mothers, low-experience early-exposed mothers, and all low-experience mothers. These proportions are computed using the coefficients in Panel (a) for early-exposed never-married mothers, and the sample means in Appendix Table A.17 for all never-married mothers. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to 19 years after a first birth.

Figure 7: Effect of Early Work Incentives on Net Income through Changes in Taxes, Transfers, and Child Care



Notes: This figure presents the impact of early exposure on the present value of net income in the medium run (years 0 to 9 post-childbirth) and long run (years 10-19 post-childbirth) stemming from changes in (i) earnings, (ii) EITC benefits, (iii) federal income taxes, (iv) other public transfers, and (v) child care costs. The direction of the effects is set to show effects on net income (i.e., increases in income are positive and increases in costs are negative). The estimates for (i)-(iii) come from DDD specifications that compare mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) across never-married and married mothers using the SSA administrative data on earnings, which we combine with information on the EITC benefits schedule for (ii), and estimates of average tax rates from NBER TAXSIM for (iii). See Section F.1 for details about our estimates of average tax rates. We use a 5% annual discount rate to obtain the present value of estimates. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. We include data from 5 years prior to a first birth up to 19 years after a first birth. The estimates for (iv) come from a DD specification that compares early- and late-exposed never-married and married mothers using CPS survey data. See Section for details. We calculate (iv) using estimates of child care costs from Anderson and Levine (2000).

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

	Employed (Earnings>0)			Wage Earnings>0		
	Never-Married (1)	Married (2)	DDD (3)	Never-Married (4)	Married (5)	DDD (6)
PostBirth * EarlyExp	0.037*** (0.009)	0.003 (0.003)		0.032*** (0.009)	0.001 (0.003)	
PostBirth * EarlyExp * NM			0.034*** (0.008)			0.031*** (0.008)
Mean Y	0.682	0.753	0.746	0.678	0.736	0.730
Observations	112910	972880	1085790	112910	972880	1085790

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

Table 2: Effect of Early Work Incentives on Long-Run Labor Market Outcomes

	Employed (Earnings>0)		Years of Experience		Earnings	
	Never Married (1)	DDD (2)	Never-Married (3)	DDD (4)	Never-Married (5)	DDD (6)
5-9 Yrs From Birth * EarlyExp	0.043*** (0.010)		0.471*** (0.075)		3655.9*** (362.6)	
10+ Yrs From Birth * EarlyExp	-0.017 (0.011)		0.449*** (0.139)		1206.1*** (444.1)	
5-9 Yrs From Birth * EarlyExp * NM		0.055*** (0.010)		0.464*** (0.070)		2617.6*** (526.6)
10+ Yrs From Birth * EarlyExp * NM		0.010 (0.011)		0.617*** (0.129)		1392.7** (587.3)
Mean Y	0.761	0.765	0.761	0.765	17000.050	23612.672
Observations	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing the employment (columns 1–2), years of experience (columns 3–4), and annual earnings (\$2016, columns 5–6) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 5-9 and 10+ years from first birth. For each outcome we present both the DD using never-married mothers as well as the DDD in which we use married mothers as an additional comparison group. See Table 1 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table 3: Effect of Early Work Incentives on Hours of Work –
CPS Responses

	Hours Worked Last Week (including 0's)			
	Positive (1)	Part Time (2)	Full Time (3)	Level (4)
<i>A: Never-Married</i>				
0-4 Yrs From Birth * EarlyExp	0.092** (0.042)	0.082*** (0.029)	0.010 (0.036)	3.074* (1.700)
5-9 Yrs From Birth * EarlyExp	0.100*** (0.029)	-0.001 (0.033)	0.100*** (0.029)	4.533*** (1.146)
10+ Yrs From Birth * EarlyExp	-0.015 (0.033)	-0.008 (0.027)	-0.007 (0.034)	-0.484 (1.493)
Mean Y	0.659	0.186	0.474	24.313
Individuals	9907	9907	9907	9907
<i>B: Add Married Comparison</i>				
0-4 Yrs From Birth * EarlyExp * NM	0.069 (0.043)	0.063** (0.030)	0.006 (0.037)	2.161 (1.748)
5-9 Yrs From Birth * EarlyExp * NM	0.054 (0.032)	-0.030 (0.037)	0.084** (0.033)	3.324*** (1.239)
10+ Yrs From Birth * EarlyExp * NM	0.004 (0.033)	0.000 (0.030)	0.004 (0.035)	0.277 (1.545)
Mean Y	0.694	0.243	0.451	24.443
Individuals	94414	94414	94414	94414

Notes: This table shows the results from regressions comparing the likelihood of working any hours last week (column 1), the likelihood of working part time (>0 and <35 hours per week, column 2), the likelihood of working full time (≥ 35 hours per week, column 3), and the average hours of work (column 4) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0-4, 5-9 and 10+ years from first birth. Panel (a) shows estimates from a single difference. Panel (b) shows estimates from a DD in which we use married mothers as an additional comparison group. All regressions include indicators for mother's age, birth year, race, education group, state, and average pre-birth employment and earnings, as well as controls for the state-level unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, implementation of six types of welfare waivers, implementation of any waiver or TANF, and implementation of the 2009 EITC reform. The DD regressions allow for differential effects of these controls by marital status. See Table 1 for information on standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table 4: Testing Alternative Explanations for Short-Run Employment Effects – Heterogeneity and Sensitivity of Effects for Never-Married Mothers

	By Birth Parity		By Change in U-Rate		Control for Dynamics		Up to 1996	
	2+ vs.1 (1)	3+ vs 2 (2)	High (3)	Low (4)	U-Rate (5)	Ref+Waivs (6)	All (7)	No Waiver (8)
PostBirth * EarlyExp * Child 2+	0.032** (0.014)							
PostBirth * EarlyExp * Child 3+		0.011 (0.022)						
PostBirth * EarlyExp			0.032*** (0.011)	0.033** (0.015)	0.033*** (0.009)	0.032*** (0.009)		
EarlyExp * 1 Yr. From Birth							0.020 (0.013)	0.012 (0.015)
EarlyExp * 2 Yr. From Birth							0.041*** (0.013)	0.051** (0.022)
EarlyExp * 3 Yr. From Birth							0.043*** (0.015)	0.053* (0.032)
Parity:								
1 st child	X	-	X	X	X	X	X	X
2 nd + child	X	X	-	-	-	-	-	-
Mean Y	0.648	0.583	0.701	0.664	0.682	0.682	0.659	0.625
Chg. U-Rate: 94-00 - 88-93	-	-	-0.018	-0.006	-	-	-	-
Observations	174050	61140	55860	57050	112910	112910	96795	26371

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

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

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November, 2020

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A Supplemental Tables and Figures

Table A.1: Characteristics of Never-Married Mothers by Early- or Late-Exposure

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

Notes: This table shows summary statistics for our early and late-exposed never-married samples, respectively, for pre-birth outcomes (Panel a) and post-birth outcomes (Panel b). While we include "Share High School or Less" in Panel (a) along with the other demographic characteristics, we actually observe this outcome after a first birth, which makes it a potential outcome of early exposure. "HH EITC eligibility Pre-Birth" is an indicator equal to one if own earnings were less than or equal to the maximum earnings to receive any EITC benefits in any of the three years prior to a first childbirth. "Any Earning Pre-Birth" is equal to one if a woman had positive earnings in any of the four years prior to a birth. "Mean of Any Earnings Pre-Birth" is the share of years that a woman worked in the four years prior to a first birth. "Mean Earnings if Working (\$2016) Pre-Birth" is the average earnings if working over the four years prior to a first birth. See Table 1 for information on the data and sample construction.

Table A.2: Summary Statistics by Marital Status

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

Notes: This table shows summary statistics for our sample of never-married and married mothers. “Secondary earner pre-birth” is an indicator equal to one if the woman ever earns less than 40% of total household earnings in the four years prior to a first birth. “HH EITC eligibility Pre-Birth” is an indicator equal to one if the sum of own earnings and husband’s earnings (if relevant) was less than or equal to the maximum earnings to receive any EITC benefits in any of the three years prior to a first childbirth. “HH EITC 0-4 Yrs. Post-Birth (\$2016, 94 sched)” is the average EITC amount that a household would qualify for in the five years after a first birth, based on the 1994 EITC schedule. “Any Earning Pre-Birth” is equal to one if a woman had positive earnings in any of the four years prior to a birth. “Mean of Any Earnings Pre-Birth” is the share of years that a woman worked in the four years prior to a first birth. “Mean Earnings if Working (\$2016) Pre-Birth” is the average earnings if working over the four years prior to a first birth. See Table 1 for information on data and sample construction.

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

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

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

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

	Never Married			Married		
	All (1)	Eligible (2)	Non-Eligible (3)	All (4)	Eligible (5)	Non-Eligible (6)
PostBirth * EarlyExp	0.037*** (0.009)	0.039*** (0.009)	-0.017 (0.017)	0.003 (0.003)	0.002 (0.005)	0.002 (0.004)
Mean Y	0.682	0.673	0.982	0.753	0.636	0.866
Observations	112910	109320	3590	972880	476700	496180

Notes: This table shows the results from regressions comparing the employment of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) in the first 5 years since the birth of a first child, by pre-birth EITC eligibility. We define a household as EITC-eligible if the minimum household income in the three years prior to a first birth is less than or equal to the maximum earnings in the phase-out region of the one-child EITC schedule. We present the DD using never-married mothers (columns 1–3) as well as the DD using married mothers (columns 4–6). See Table 1 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 5th year after a first birth.

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

	Self-Emp. Earnings >0		Bunching (\$1500 bins)		Bunching (\$2500 bins)	
	Never Married (1)	DDD (2)	Never Married (3)	DDD (4)	Never Married (5)	DDD (6)
PostBirth * EarlyExp	0.010*** (0.003)		0.015*** (0.004)		0.020*** (0.005)	
PostBirth * EarlyExp * NM		0.006* (0.003)		0.011** (0.004)		0.014*** (0.005)
Mean Y	0.013	0.034	0.047	0.043	0.077	0.071
Observations	112910	1085790	112910	1085790	112910	1085790

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

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

	Earnings (\$2016)		Wage Earnings (\$2016)	
	0-3 (1)	0-4 (2)	0-3 (3)	0-4 (4)
<i>A: Never Married</i>				
PostBirth * EarlyExp	834*** (224)	1121*** (244)	746*** (237)	1011*** (261)
Mean Y	9251	9821	9144	9698
Individuals	101619	112910	101619	112910
<i>B: DDD</i>				
PostBirth * EarlyExp * NM	464 (325)	657** (327)	404 (340)	591* (343)
Mean Y	18087	18273	17740	17901
Individuals	977211	1085790	977211	1085790

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

Table A.7: Long-Run Effects on Wage Earnings and Self-Employment Earnings

	Wage Earnings		Pos. Self-Emp.		Self Emp Earnings	
	NM (1)	DDD (2)	NM (3)	DDD (4)	NM (5)	DDD (6)
5-9 Yrs From Birth * EarlyExp	3390*** (372)		0.019*** (0.005)		265*** (81)	
10+ Yrs From Birth * EarlyExp	1054** (446)		0.013*** (0.004)		152* (78)	
5-9 Yrs From Birth * EarlyExp * NM		2468*** (515)		0.014*** (0.005)		150 (101)
10+ Yrs From Birth * EarlyExp * NM		1353** (566)		0.004 (0.005)		40 (95)
Mean Y	16540	22846	0.045	0.061	460	767
Individuals	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing wage earnings (\$2016, columns 1–2), positive self-employment earnings (columns 3–4), self-employment earnings (\$2016, columns 5–6) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 5–9 and 10+ years since first birth. The odd columns show DD regressions using never-married (NM) mothers. The even columns show DDD regressions that use married mothers as an additional comparison group. See Table 1 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.8: Long-Run Effects on Alternative Measures of Earnings

	Earnings, if Positive		Log Earnings		Winsorized	
	NM (1)	DDD (2)	NM (3)	DDD (4)	NM (5)	DDD (6)
5-9 Yrs From Birth * EarlyExp	3648*** (355)		0.261*** (0.030)		3543*** (360)	
10+ Yrs From Birth * EarlyExp	1981*** (421)		0.076** (0.030)		1026** (407)	
5-9 Yrs From Birth * EarlyExp * NM		1516** (580)		0.165*** (0.034)		2510*** (452)
10+ Yrs From Birth * EarlyExp * NM		1190* (683)		0.050 (0.034)		1201** (479)
Mean Y	21938	30705	9.399	9.750	16915	22971
Individuals	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing the earnings of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 5–9 and 10+ years since first birth. “Earnings, if Positive” (columns 1–2) is missing for all individuals that have zero earnings. “Winsorized” earnings (columns 5–6) have been top-coded at \$175,000 (\$2016), which is the top 1% of married mothers’ earnings. DD regressions using never-married (NM) mothers are shown in the odd columns, while the DDD regressions that use married mothers as an additional comparison are shown in the even columns. See Table 1 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.9: Effect of Early Work Incentives on Hours and Weeks of Work – CPS Responses

	Level (including 0's)			Cumulative		
	Weekly Hours (1)	Annual Weeks (2)	Hours × Weeks (3)	Weekly Hours (4)	Annual Weeks (5)	Hours × Weeks (6)
<i>A: Never Married</i>						
0-4 Yrs From Birth * EarlyExp	3.074* (1.700)	4.191** (1.830)	145.0* (72.9)	8.915* (4.633)	12.208** (5.204)	428.1** (210.0)
5-9 Yrs From Birth * EarlyExp	4.533*** (1.146)	1.039 (1.218)	84.0 (60.8)	33.494*** (8.905)	26.861** (10.212)	1245.1*** (442.3)
10+ Yrs From Birth * EarlyExp	-0.484 (1.493)	0.127 (1.303)	22.4 (74.8)	33.360* (18.217)	22.578 (17.443)	1125.0 (905.1)
Scaled β 0-4				0.255	0.305	0.306
Scaled β 5-9				0.957	0.672	0.889
Scaled β 10+				0.953	0.564	0.804
Observations	9907	10020	9921	9907	10020	9921
<i>B: Add Married Comparison</i>						
0-4 Yrs From Birth * EarlyExp * NM	2.161 (1.748)	3.137 (1.907)	32.0 (75.3)	7.938*** (2.598)	12.882*** (4.083)	283.7** (135.8)
5-9 Yrs From Birth * EarlyExp * NM	3.324*** (1.239)	1.264 (1.408)	107.1 (64.6)	27.294*** (4.508)	36.054*** (7.545)	1192.2*** (244.5)
10+ Yrs From Birth * EarlyExp * NM	0.277 (1.545)	0.528 (1.293)	30.9 (75.4)	28.455*** (7.004)	31.706*** (8.727)	944.8** (355.1)
Scaled β 0-4				0.227	0.322	0.203
Scaled β 5-9				0.780	0.901	0.852
Scaled β 10+				0.813	0.793	0.675
Observations	94414	98077	96918	94414	98077	96918

Notes: This table shows the results from regressions comparing labor market outcomes between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9 and 10+ years since first birth. Panel (a) shows a single-difference using never-married mothers. Panel (b) shows the DD estimates in which we use married mothers as an additional comparison group. The outcomes are number of hours worked per week (column 1), number of weeks worked last year (column 2), and hours times weeks (column 3), as well as the cumulative sums of each these (columns 4–6). For interpretation, we calculate a “scaled β ” for the short-run, medium-run, and long-run, shown at the bottom of each panel, by dividing the cumulative total for each of these periods by the number of hours in a full-time, full-year of work (i.e., 35 hours \times 40 weeks). The scaled β is thus the additional years of full-time, full-year work accrued by early-exposed mothers. See Table 3 for information on control variables, and Table 1 for information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.10: Effect of Early Work Incentives on Completed Fertility –
CPS Responses

	Number of Kids		2+ Kids		3+ Kids		Yrs b/w 1 and 2	
	NM (1)	+ Married (2)	NM (3)	+ Married (4)	NM (5)	+ Married (6)	NM (7)	+ Married (8)
EarlyExp	0.008 (0.068)		-0.005 (0.044)		0.005 (0.035)		-0.288 (0.414)	
EarlyExp * NM		0.010 (0.070)		0.012 (0.045)		-0.006 (0.036)		-0.117 (0.439)
Mean Y	1.834	2.222	0.537	0.771	0.207	0.317	4.313	3.619
Observations	3638	45392	3638	45392	3638	45392	1953	34981

Notes: This table shows the results from regressions comparing completed fertility between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991). The odd columns show estimates from a single difference using never-married mothers. The even columns show the DD estimates in which we use married mothers as an additional comparison group. The outcomes are total number of children in the household (columns 1–2), an indicator for having at least 2 children (columns 3–4), having at least three children (columns 5–6), and the number of years between one’s first and second child (columns 7–8). We restrict the sample to mothers interviewed in the CPS between the ages of 36 to 44, who are more likely to have completed their childbearing. See Table 3 for information on control variables, and Table 1 for additional information on standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.11: Testing Alternative Explanations for Short-Run Employment Effects – Triple-Difference Estimates

	By Change in U-Rate		Control for Dynamics		Up to 1996	
	High (1)	Low (2)	U-Rate (3)	Ref+Waivs (4)	All (5)	No Waiver (6)
PostBirth * EarlyExp * NM	0.028** (0.010)	0.031** (0.013)	0.032*** (0.008)	0.028*** (0.008)		
EarlyExp * NM * 1 Yr. From Birth					0.020 (0.014)	0.013 (0.017)
EarlyExp * NM * 2 Yr. From Birth					0.039*** (0.015)	0.053** (0.027)
EarlyExp * NM * 3 Yr. From Birth					0.050*** (0.017)	0.056 (0.040)
Mean Y	0.759	0.732	0.746	0.746	0.745	0.739
Chg. U-Rate: 94-00 - 88-93	-0.018	-0.006	-	-	-	-
Observations	548800	536990	1085790	1085790	946146	232607

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

Table A.12: Effect of Early Work Incentives on Short-Run Earnings – By the Size of the Economic Boom

	> Med.		< Med.	
	Decline U-Rate		Decline U-Rate	
	NM (1)	DDD (2)	NM (3)	DDD (4)
PostBirth * EarlyExp	1050*** (302)		951** (351)	
PostBirth * EarlyExp * NM		502 (440)		779* (414)
Mean Y	9579	17925	10057	18628
Chg. U-Rate: 94-00 - 88-93	-0.018	-0.018	-0.006	-0.006
Individuals	55860	548800	57050	536990

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

Table A.13: Effect of Early Work Incentives on Short-Run Employment –
Using Future Mothers for Comparison

	Baseline		0-3 Yrs Post			
	Never Married (1)	DDD (2)	Never Married (3)	DDD (4)	No Kids (5)	DDD (6)
PostBirth * EarlyExp	0.037*** (0.009)		0.032*** (0.009)		0.010 (0.008)	
PostBirth * EarlyExp * NM		0.034*** (0.008)		0.028*** (0.008)		
PostBirth * EarlyExp * Kids						0.026** (0.012)
Never Married Moms	Yes	Yes	Yes	Yes	No	Yes
Married Moms	No	Yes	No	Yes	No	No
Never Married Future Moms	No	No	No	No	Yes	Yes
Observations	112910	482120	101619	433908	78828	146574

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

Table A.14: Effect of Early Work Incentives on Long-Run Earnings – Sensitivity to Controls for Unemployment and Welfare

	Base		UR Dynamics		(Ref+Waivs)*Dynamics	
	NM (1)	DDD (2)	NM (3)	DDD (4)	NM (5)	DDD (6)
5-9 Yrs From Birth * EarlyExp.	3656*** (363)		3707*** (378)		3442*** (418)	
10+ Yrs From Birth * EarlyExp	1206*** (444)		1185*** (427)		1070** (471)	
5-9 Yrs From Birth * EarlyExp * NM		2618*** (527)		2533*** (516)		2338*** (524)
10+ Yrs From Birth * EarlyExp * NM		1393** (587)		1340** (569)		1159* (612)
Mean Y	17000	23613	17000	23613	17000	23613
Observations	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing earnings (in 2016 dollars) of never-married mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 5–9 and 10+ years from first birth. The odd columns show the DD using never-married mothers. The even columns show the DDD in which we use married mothers as an additional comparison group. Columns 1–2 present our baseline results. Columns 3–4 show the estimates when we add to our baseline specification interactions between the age of one’s first child and the unemployment rate. Columns 5–6 show the estimates when we add to our baseline specification interactions between the age of one’s first child and our indicators for welfare reform and waivers. See Table 1 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.15: Effect of Early Work Incentives on Labor Market Outcomes – Sensitivity to Inverse P-Score Reweighting

	Employed (Earnings > 0)		Earnings (\$2016)	
	Never Married (1)	DDD (2)	Never Married (3)	DDD (4)
0-4 Yrs From Birth * EarlyExp	0.054*** (0.010)		1120*** (293)	
5-9 Yrs From Birth * EarlyExp	0.045*** (0.010)		3706*** (365)	
10+ Yrs From Birth * EarlyExp	-0.011 (0.011)		1092** (442)	
0-4 Yrs From Birth * EarlyExp * NM		0.040*** (0.009)		537 (327)
5-9 Yrs From Birth * EarlyExp * NM		0.048*** (0.010)		2327*** (526)
10+ Yrs From Birth * EarlyExp * NM		0.003 (0.011)		1101* (592)
Observations	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing the employment (columns 1–2) and earnings (columns 3–4) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9 and 10+ years from first birth. The odd columns show the DD using never-married mothers. The even columns show the DDD in which we use married mothers as an additional comparison group. All regressions are reweighted using inverse p-scores so that late and early-exposed mothers are balanced on pre-birth characteristics. See Table 1 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.16: Effect of Early Work Incentives on Labor Market Outcomes –
Sensitivity to Alternative Specifications

	Base		Add AFB*YSB		Add Ind FE		Sample: Heads	
	NM (1)	DDD (2)	NM (3)	DDD (4)	NM (5)	DDD (6)	NM (7)	DDD (8)
<i>A: Short-Run Employment</i>								
PostBirth * EarlyExp	0.037*** (0.009)		0.036*** (0.009)		0.031*** (0.009)		0.042*** (0.009)	
PostBirth * EarlyExp * NM		0.034*** (0.008)		0.033*** (0.008)		0.027*** (0.008)		0.039*** (0.008)
Mean Y	0.682	0.746	0.682	0.746	0.682	0.746	0.688	0.751
Observations	112910	1085790	112910	1085790	112910	1085790	89220	1039940
<i>B: Long-Run Earnings</i>								
5-9 Yrs From Birth * EarlyExp	3656*** (362.6)		3693*** (366.11)		3612*** (349.6)		3660*** (377.4)	
10+ Yrs From Birth * EarlyExp	1206*** (444.1)		1259*** (446.2)		1177*** (429.6)		1491*** (479.7)	
5-9 Yrs From Birth * EarlyExp * NM		2618*** (526.6)		2422*** (533.2)		2574*** (515.3)		2648*** (520.9)
10+ Yrs From Birth * EarlyExp * NM		1393** (587.3)		1170* (605.0)		1341** (576.1)		1695*** (597.6)
Mean Y	17000	23613	17000	23613	17000	23613	17259	23936
Observations	282275	2714475	282275	2714475	282275	2714475	223050	2599850

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

Table A.17: Effect of Early Work Incentives on Jointly Having “High Earnings” (Top 25%) and “High Experience” (Work 3 Yrs. After a First Birth)

	Pr(High Earn + High Exp) (1)	Pr(High Earn + Low Exp) (2)	Pr(Low Earn + High Exp) (3)	Pr(Low Earn + Low Exp) (4)
<i>A: Never-Married</i>				
10+ Yrs From Birth * EarlyExp	0.028*** (0.006)	-0.009*** (0.003)	0.108*** (0.016)	-0.127*** (0.015)
Mean Y	0.125	0.021	0.545	0.310
Observations	282275	282275	282275	282275
<i>B: DDD</i>				
10+ Yrs From Birth * EarlyExp * NM	0.020** (0.008)	-0.003 (0.004)	0.075*** (0.016)	-0.092*** (0.014)
Mean Y	0.230	0.020	0.472	0.278
Observations	2714475	2714475	2714475	2714475

Notes: This table shows the results from regressions comparing outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from first birth. The outcomes are indicators for having “high earnings” (top 25%) or “low earnings” (bottom 75%) crossed with indicators for having “high experience” (having worked in each of the three years after a first birth) or “low experience” (not having worked in each of the three years after a first birth). We show estimates for having “high experience and high earnings” (column 1), having high earnings and low experience (column 2), “low earnings and high experience” (column 3), and “low earnings and low experience” (column 4). Panel (a) presents the DD using never-married mothers. Panel (b) presents the DDD in which we use married mothers as an additional comparison group. See the text and Appendix E for more details. See Table 1 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.18: Effect of Early Work Incentives on Service Occupations –
CPS Responses

	Service							
	Housekeep (1)	Janitor (2)	Food (3)	Child (4)	Beauty (5)	Recreation (6)	Protect (7)	Health Serv (8)
<i>Panel A: Never Married</i>								
0-4 Yrs from Birth * EarlyExp	-0.019* (0.011)	-0.007 (0.009)	0.027 (0.022)	0.012 (0.007)	-0.003 (0.008)	0.001 (0.003)	0.008 (0.006)	0.013 (0.018)
5-9 Yrs from Birth * EarlyExp	-0.011 (0.012)	-0.001 (0.008)	0.015 (0.020)	0.012* (0.007)	0.004 (0.010)	0.002 (0.003)	0.010 (0.009)	0.033* (0.018)
10+ Yrs from Birth * EarlyExp	-0.009 (0.011)	-0.004 (0.007)	0.019 (0.013)	0.003 (0.006)	-0.004 (0.009)	0.000 (0.005)	0.006 (0.007)	0.040** (0.018)
Mean Y	0.025	0.017	0.064	0.015	0.013	0.006	0.012	0.066
Individuals	10006	10006	10006	10006	10006	10006	10006	10006
<i>Panel B: Add Married Comparison</i>								
0-4 Yrs from Birth * EarlyExp * NM	-0.019* (0.010)	-0.007 (0.009)	0.029 (0.022)	0.010 (0.009)	-0.007 (0.010)	0.004 (0.004)	0.008 (0.006)	0.013 (0.020)
5-9 Yrs from Birth * EarlyExp * NM	-0.010 (0.012)	-0.005 (0.008)	0.012 (0.020)	0.011 (0.007)	0.002 (0.011)	0.003 (0.003)	0.015 (0.009)	0.027 (0.019)
10+ Yrs from Birth * EarlyExp * NM	-0.007 (0.011)	-0.006 (0.007)	0.021 (0.013)	0.002 (0.007)	-0.004 (0.009)	0.002 (0.005)	0.008 (0.007)	0.035** (0.017)
Mean Y	0.013	0.008	0.034	0.018	0.012	0.004	0.006	0.034
Individuals	95573	95573	95573	95573	95573	95573	95573	95573

Notes: This table shows the results from regressions comparing the probability of reporting being in each service occupation (including mothers that are not working) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9, and 10+ years from a first birth. Panel (a) presents the single-difference using never-married mothers. Panel (b) presents the DD in which we use married mothers as an additional comparison group. Occupation definitions are in Appendix B.1. See Table 3 for information on control variables, and Table 1 for information on standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.19: Effect of Early Work Incentives on Non-Service Occupations –
CPS Responses

	Non-Service						
	Exec/Man (1)	Prof/Tech (2)	Fin Sales (3)	Ret Sales (4)	Cleric (5)	Agricultural (6)	Mech/Constr/Min (7)
<i>Panel A: Never Married</i>							
0-4 Yrs from Birth * EarlyExp	0.019 (0.021)	-0.028 (0.021)	0.017 (0.012)	-0.021 (0.025)	-0.005 (0.032)	0.001 (0.005)	-0.003 (0.005)
5-9 Yrs from Birth * EarlyExp	0.013 (0.020)	-0.009 (0.025)	0.007 (0.010)	-0.013 (0.018)	0.000 (0.025)	0.009 (0.006)	-0.002 (0.005)
10+ Yrs from Birth * EarlyExp	0.018 (0.015)	-0.031 (0.023)	0.002 (0.007)	-0.013 (0.013)	-0.038* (0.023)	0.008* (0.004)	-0.002 (0.004)
Mean Y	0.064	0.107	0.022	0.064	0.188	0.006	0.005
Individuals	10006	10006	10006	10006	10006	10006	10006
<i>Panel B: Add Married Comparison</i>							
0-4 Yrs from Birth * EarlyExp * NM	0.032 (0.023)	-0.026 (0.024)	0.014 (0.012)	-0.008 (0.026)	-0.006 (0.034)	-0.007 (0.006)	-0.002 (0.006)
5-9 Yrs from Birth * EarlyExp * NM	0.012 (0.020)	-0.002 (0.030)	0.004 (0.011)	-0.007 (0.019)	-0.012 (0.026)	0.006 (0.006)	0.001 (0.005)
10+ Yrs from Birth * EarlyExp * NM	0.025 (0.016)	-0.023 (0.024)	-0.001 (0.009)	-0.014 (0.014)	-0.051** (0.025)	0.008 (0.005)	-0.000 (0.004)
Mean Y	0.105	0.204	0.032	0.044	0.177	0.008	0.004
Individuals	95573	95573	95573	95573	95573	95573	95573

Notes: This table shows the results from regressions comparing the probability of reporting being in each non-service occupation (including mothers that are not working) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9, and 10+ years from a first birth. Panel (a) presents the single-difference using never-married mothers. Panel (b) presents the DD in which we use married mothers as an additional comparison group. Occupation definitions are in Appendix B.1. See Table 3 for information on control variables, and Table 1 for information on standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

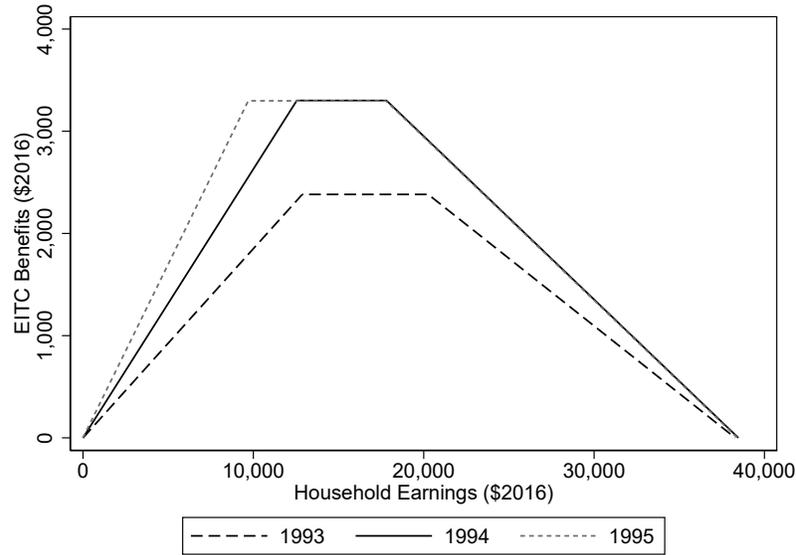
Table A.20: Effect of Early Work Incentives on Government Transfers –
CPS Responses

	Welfare (1)	Disability (2)	SNAP (3)	Medicaid (4)	Hous Sub (5)	Total (6)
<i>A: Never Married</i>						
0-4 Yrs from Birth * EarlyExp	-740.9*** (254.6)	51.9 (69.4)	-402.1** (192.6)	-273.9 (186.8)	3.2 (11.2)	-1298.3*** (480.7)
5-9 Yrs from Birth * EarlyExp	-846.8*** (148.8)	-23.9 (57.5)	-780.1*** (157.0)	-49.5 (194.0)	-19.2 (12.8)	-1551.7*** (358.1)
10+ Yrs from Birth * EarlyExp	17.8 (116.3)	24.0 (67.5)	-231.2 (139.4)	53.8 (178.8)	-14.5* (8.4)	-31.8 (325.3)
Mean Y	864.630	79.039	1309.217	1334.773	60.216	3628.724
Observations	10020	10020	9438	8228	9193	8228
<i>B: Add Married Comparison</i>						
0-4 Yrs from Birth * EarlyExp * NM	-724.8*** (243.1)	120.0 (104.4)	-343.5* (182.0)	-85.6 (168.7)	4.0 (11.1)	-936.1* (475.7)
5-9 Yrs from Birth * EarlyExp * NM	-824.0*** (158.5)	-54.9 (73.3)	-710.4*** (153.5)	-134.9 (200.2)	-18.2 (12.7)	-1599.2*** (356.5)
10+ Yrs from Birth * EarlyExp * NM	-8.960 (110.5)	-7.3 (77.4)	-237.1* (130.5)	81.541 (195.1)	-14.6* (8.3)	-61.3 (302.9)
Mean Y	138.6	136.7	281.9	866.2	8.7	1405.2
Observations	98077	98077	91689	80508	89921	80508

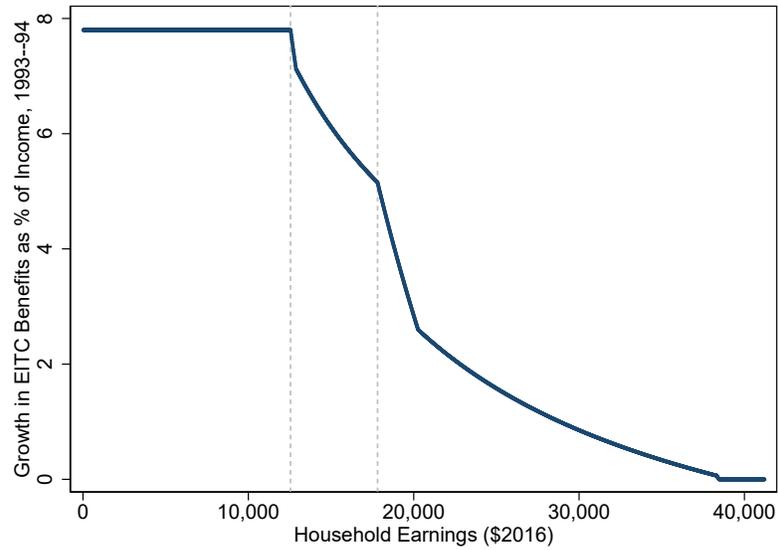
Notes: This table shows the results from regressions comparing the amount of cash and in-kind transfers from each government program (shown in the headers) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0–4, 5–9, and 10+ years from a first birth. Panel (a) presents the single-difference using never-married mothers. Panel (b) presents the DD in which we use married mothers as an additional comparison group. See Table 3 for information on control variables, and Table 1 for information on standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

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

(a) 1993, 1994 and 1995 Benefit Levels

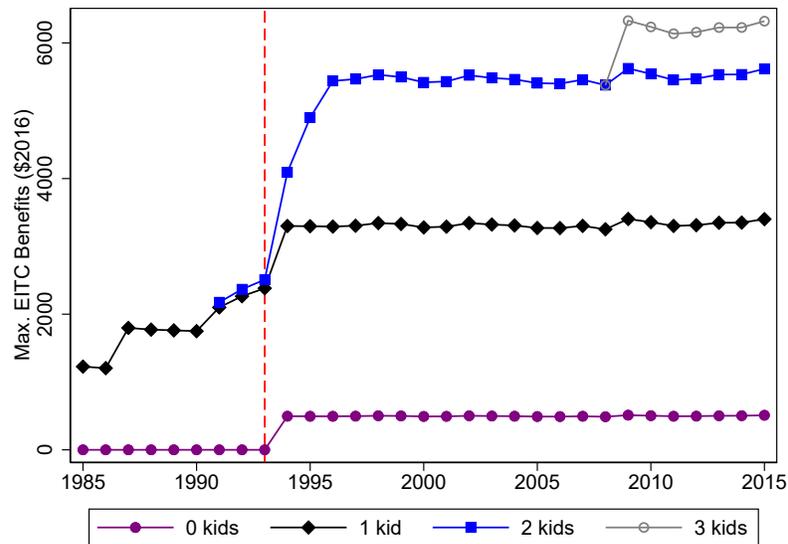


(b) 1993-1994 Change in Benefits as % of Earnings



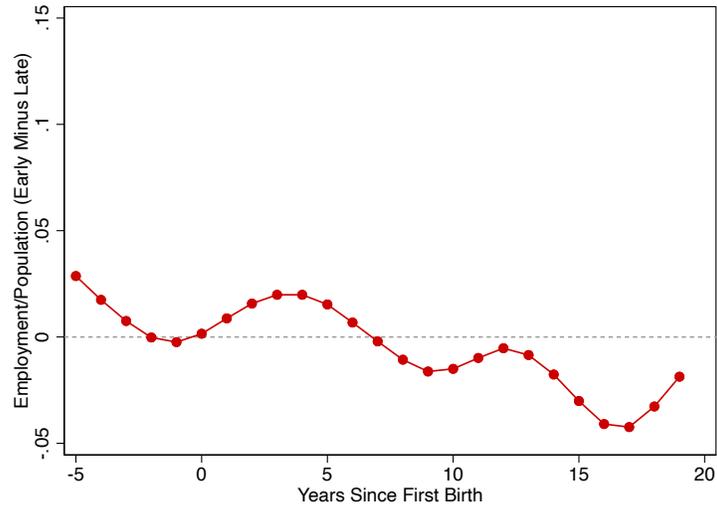
Notes: Panel (a) shows EITC benefits (\$2016) at each level of earnings for households with one child in 1993, 1994, and 1995. Panel (b) shows the difference between 1994 and 1993 benefits as a share of household income. *Data:* Nominal EITC benefits are obtained from the Tax Policy Center (<https://www.taxpolicycenter.org/statistics/eitc-parameters>),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

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



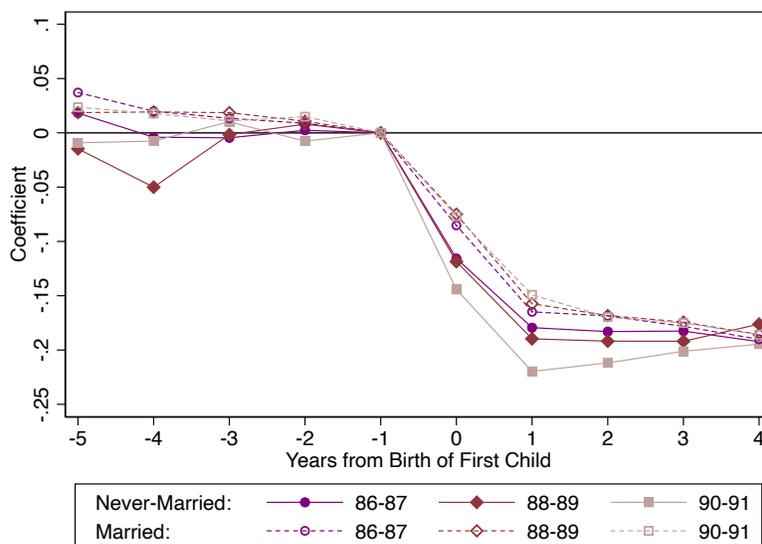
Notes: This figure shows the maximum EITC benefits (\$2016) in each year and by number of qualifying children. *Data:* Nominal EITC benefits are obtained from the Tax Policy Center (<https://www.taxpolicycenter.org/statistics/eitc-parameters>), and have been converted to 2016 dollars using the CPI from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

Figure A.3: Difference in the Average Employment-to-Population Ratio Between Early- and Late-Exposed Mothers



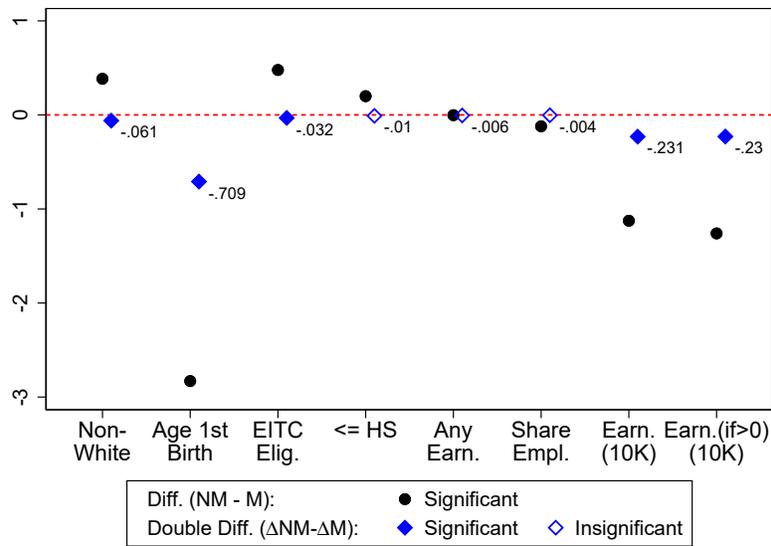
Notes: This figure shows the difference between the average employment to population ratio for mothers who were exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). Hence, a positive value implies that early-exposed mothers had better employment conditions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

Figure A.4: Employment Relative to Year Prior to Childbirth for Mothers Giving Birth Prior to the EITC Reform



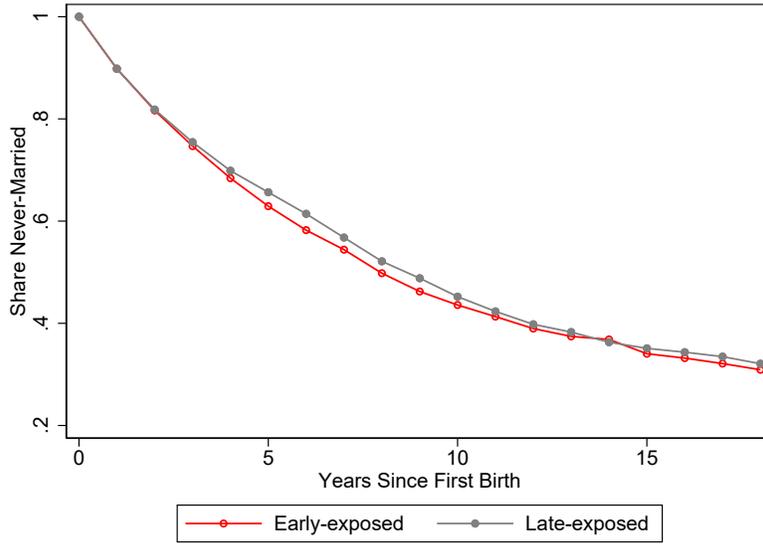
Notes: This figure presents point estimates and 95% confidence intervals from event studies of employment around birth for never-married and married women who were exposed to the 1993 EITC reform late (first birth: 1986–1991). See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 5th year after a first birth.

Figure A.5: Difference and Difference-in-Difference in Observables Across Married and Never-Married Mothers



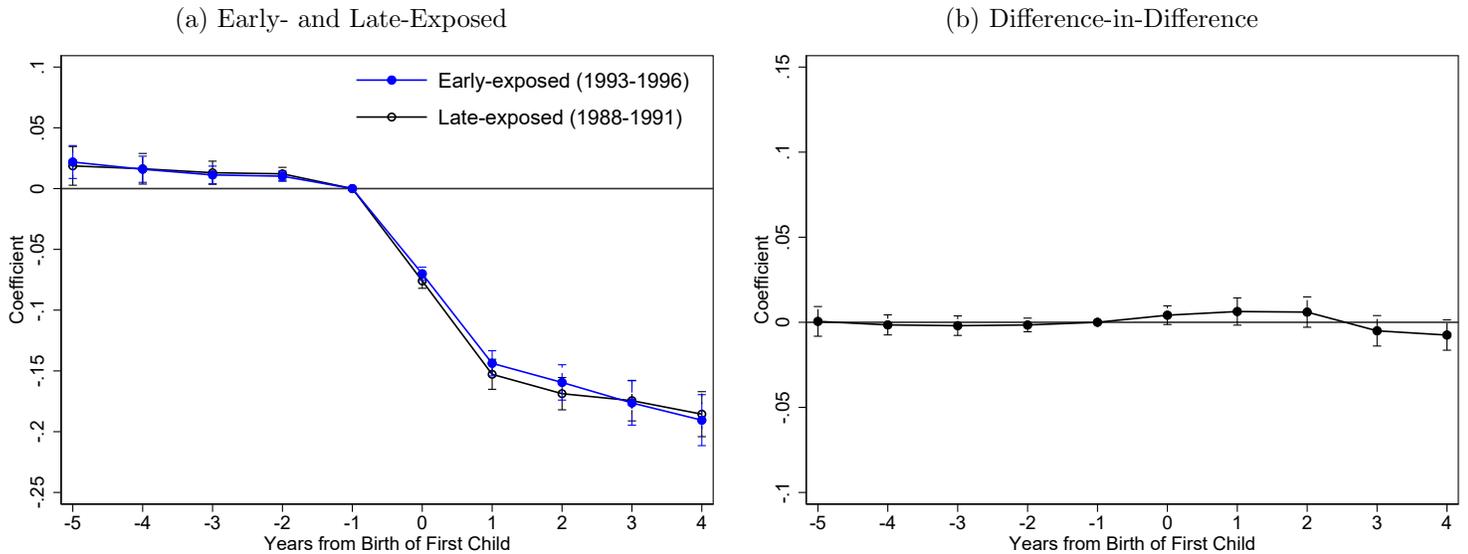
Notes: This figure presents differences between never-married and married women’s average level of observables characteristics (circles) and in late- vs. early-exposed women’s changes in these characteristics (diamonds). EITC eligibility is equal to one if the sum of own earnings and husband’s earnings (if relevant) was less than or equal to the maximum earnings to receive any EITC benefits in any of the three years prior to a first childbirth. “Any Earn.” is equal to one if a woman had positive earnings in any of the four years prior to a birth. “Share empl.” is the share of years that a woman worked in the four years prior to a first birth. “Earn (10K)” and “Earn. (if>0) (10K)” are the average earnings and the average earnings if working over the four years prior to a first birth, measured in \$10,000 (\$2016). See the notes of Figure 2 for information on data and sample construction.

Figure A.6: Share of Mothers Remaining Never-Married in Each Year Since First Birth (SIPP)



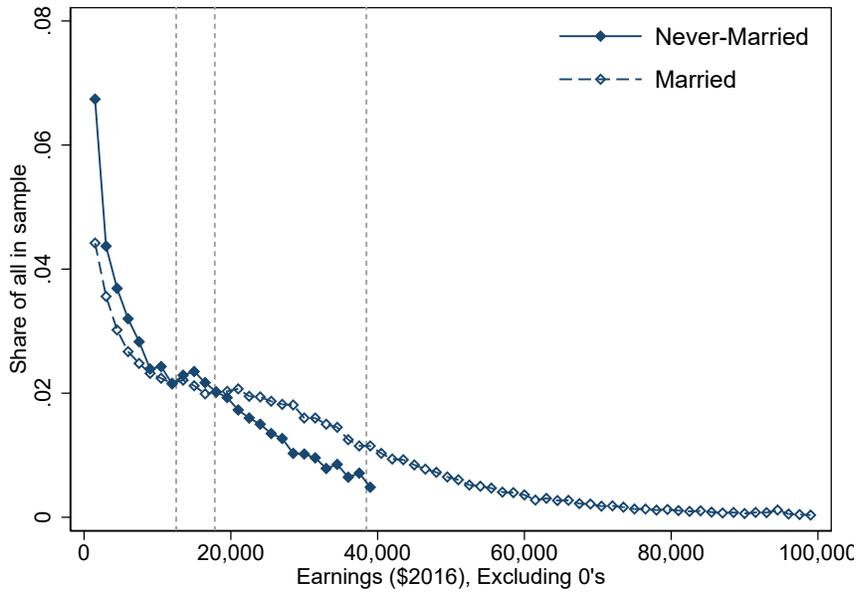
Notes: This figure presents the share of mothers who were never-married at first birth that remain never-married in each year since first birth. We plot this separately for mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991). We estimate the gap between early- and late-exposed mothers to be -1.3 p.p (se: 0.9 p.p.) by regressing an indicator for whether an individual is single on indicators for the years since first birth and an indicator for being early-exposed, and clustering standard errors by individual. *Data:* 1990, 1993, 1996, 2001, 2004, 2008 SIPP Wave 2 Topical Modules and 2014 SIPP. *Sample:* women whose first child was born in 1988–1991 or 1993–1996, and who were never married at the time of first birth. Estimates weighted by SIPP weights.

Figure A.7: Effect of Early Work Incentives on Short-Run Employment – Married Mothers



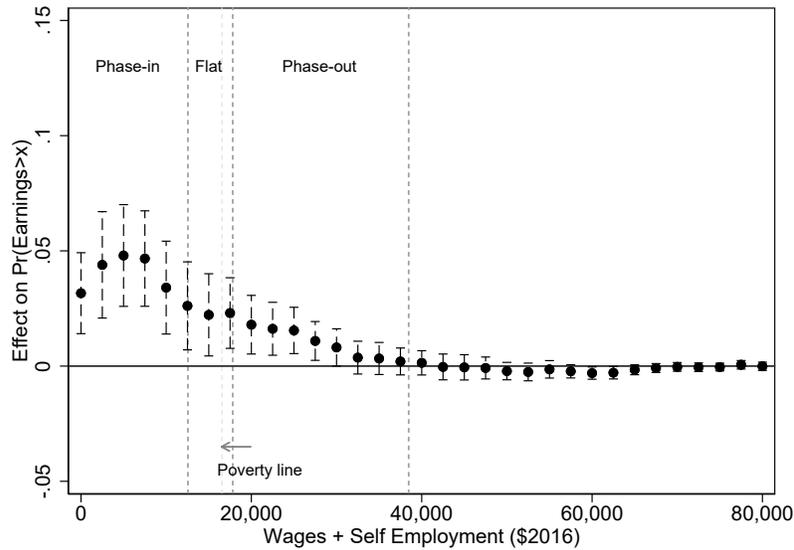
Notes: These figures present point estimates and 95% confidence intervals from event studies of employment around birth for married mothers who were exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). Panel (a) plots the estimates on indicators for years since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

Figure A.8: Distribution of Post-birth Earnings, Excluding 0's – Late-Exposed Mothers



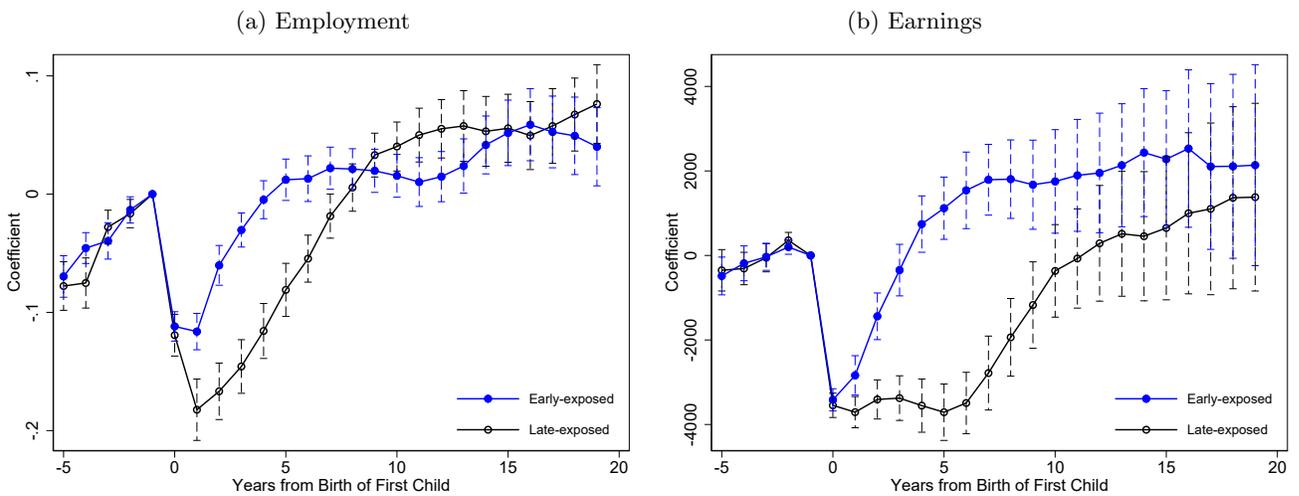
Notes: This figure shows the truncated distribution of earnings, excluding 0's, zero to three years after a first birth for never-married and married mothers who were exposed to the 1993 EITC reform late (first birth: 1988–1991). We omit the never-married distribution beyond \$40,000 due to having too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

Figure A.9: Effect of Early Work Incentives on Earnings Density – Difference-in-Difference Estimates



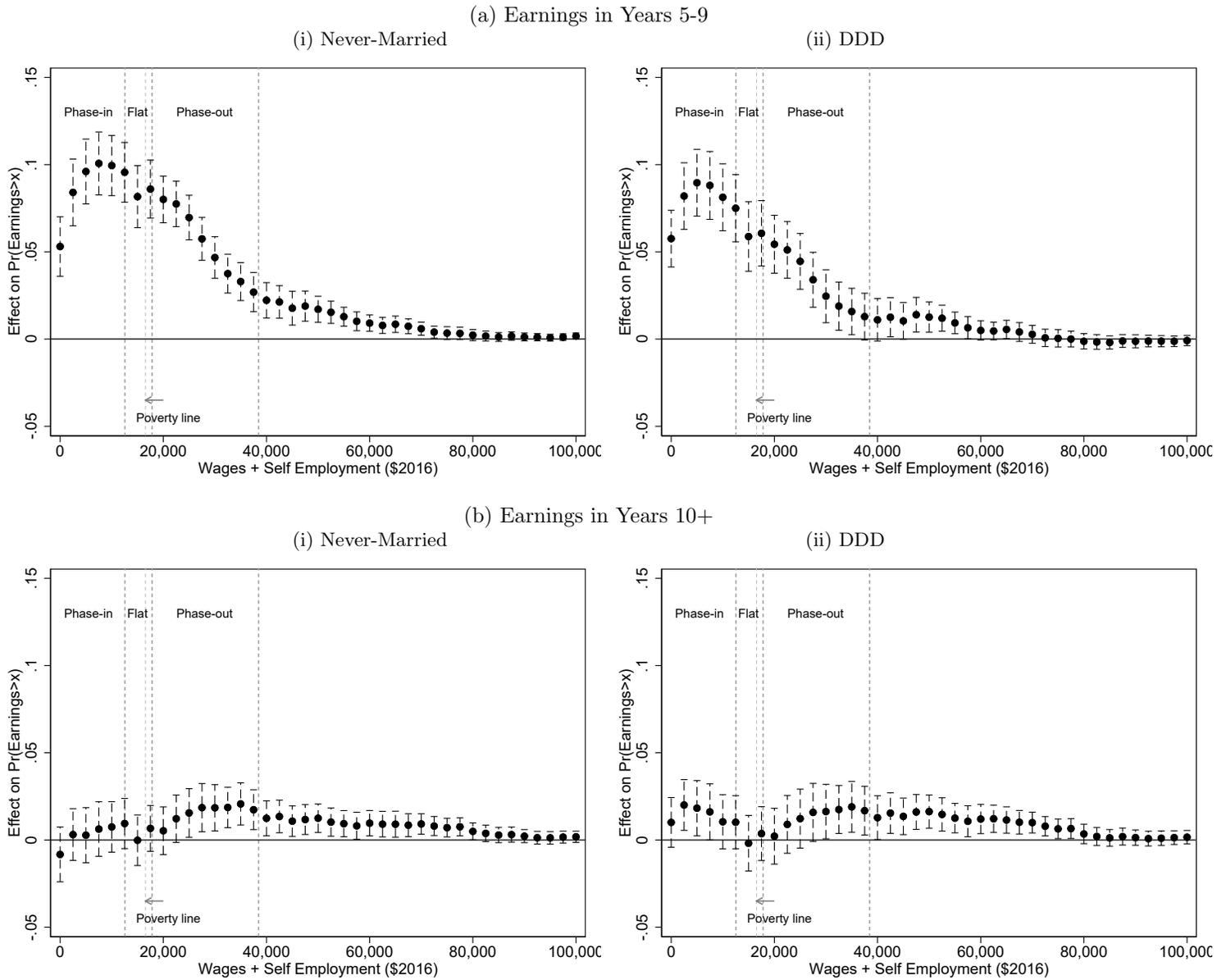
Notes: This figure shows coefficients and 95% confidence intervals from difference-in-difference regressions that compare the earnings distribution of never-married mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991). Each marker is obtained from a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

Figure A.10: Long-Run Child Penalty for Early- and Late-Exposed Never-Married Mothers



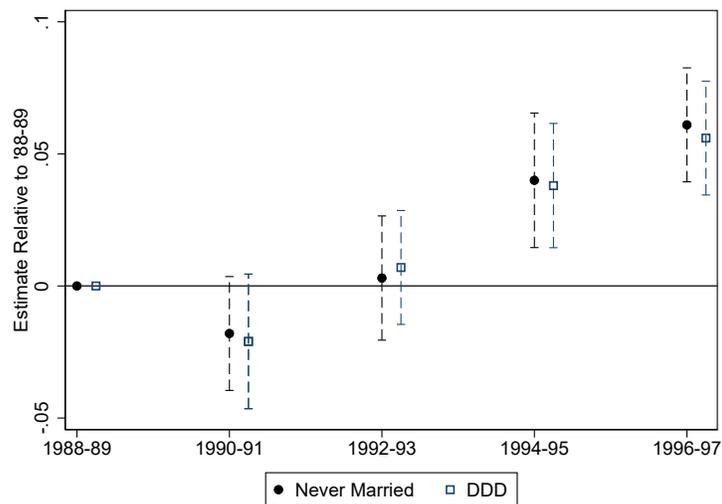
Notes: These figures present the point estimates and 95% confidence intervals from event studies of never-married mothers' employment (Panel a) or earnings (Panel b) around birth interacted with indicators for being exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). See the notes of Figure 2 for information on control variables, standard errors, data and sample construction.

Figure A.11: Effect of Early Work Incentives on Earnings Density – Medium- and Long-Run



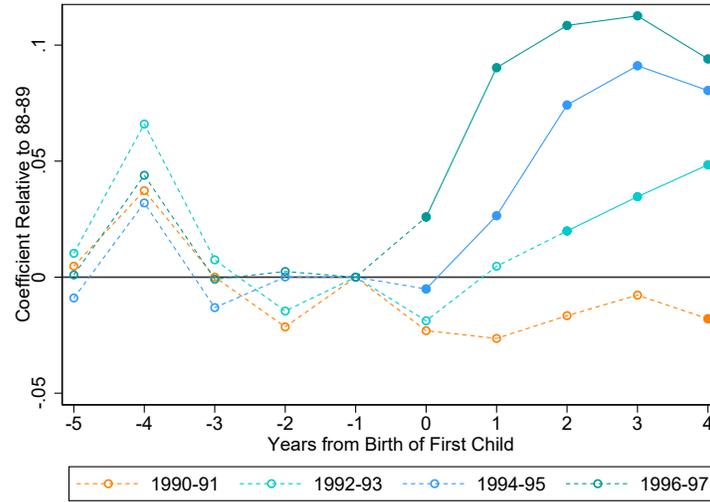
Notes: These figures show coefficients and 95% confidence intervals from DDD regressions that compare the earnings distribution of mothers with early exposure (first birth: 1993–1996) and late-exposure (first birth: 1988–1991) to the EITC reform, across never-married and married mothers. Each marker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

Figure A.12: Effect of Early Work Incentives on Short-Run Employment –
By Year of First Birth



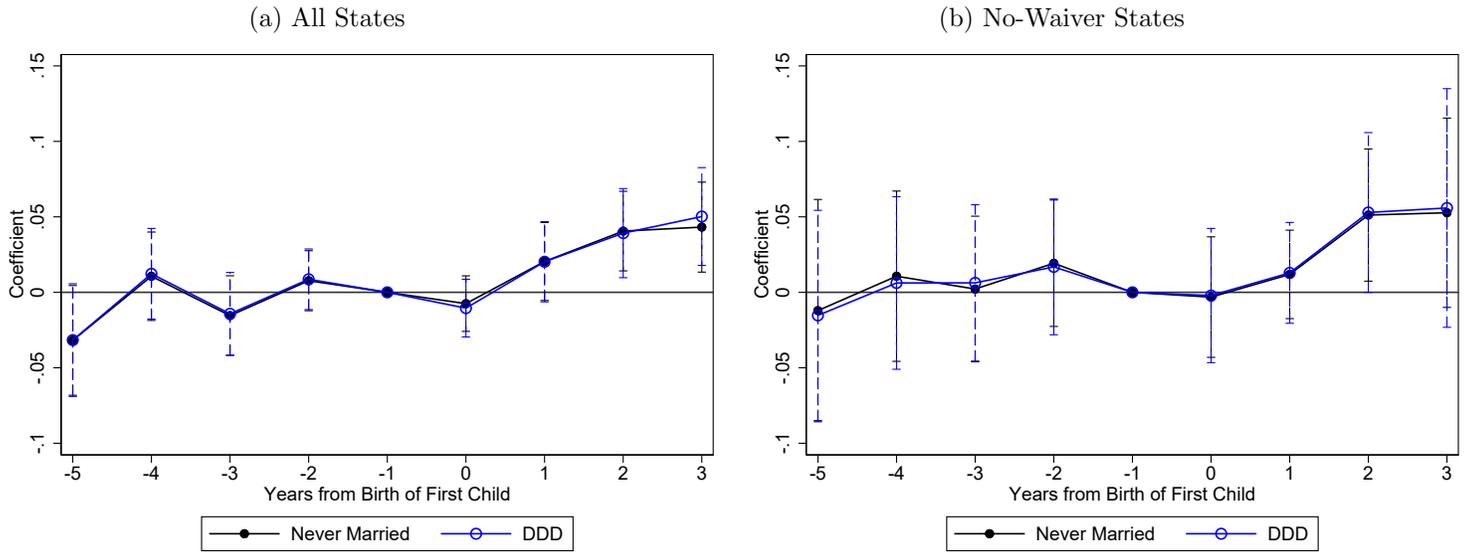
Notes: These figures show coefficients and 95% confidence intervals from regressions of employment on an indicator for “Post-Birth” interacted with indicators for having a first birth in 1990–91, 1992–93, 1994–95, or 1996–97. The omitted category (reference group) is first births in 1988–89. We present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

Figure A.13: Effect of Early Work Incentives on Short-Run Employment –
By Year of First Birth and Years Since Birth



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

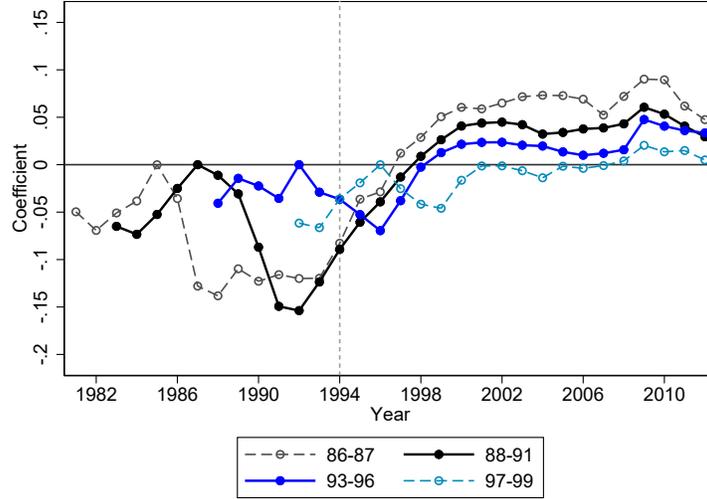
Figure A.14: Effect of Early Work Incentives on Short-Run Employment –
Prior to Federal Welfare Reform



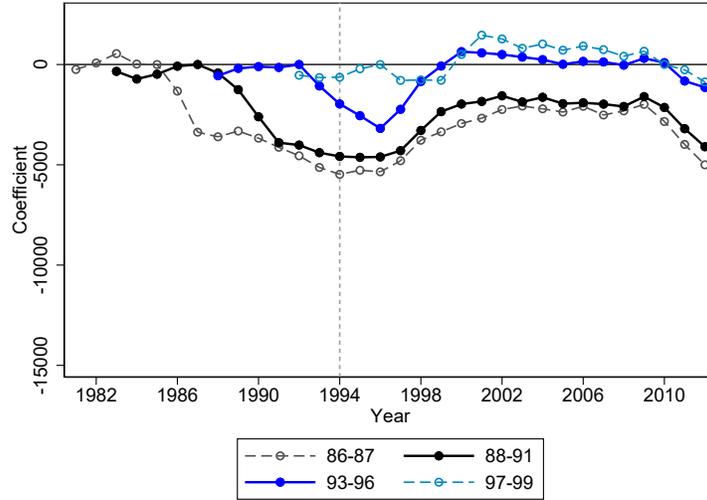
Notes: These figures present estimates and 95% confidence intervals from event studies of employment up to five years after a first birth, for mothers who were exposed to the 1993 EITC reform early (first birth: 1993–1996) or late (first birth: 1988–1991). For each figure we show the estimates for the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

Figure A.15: Effect of Early Work Incentives on Labor Market Outcomes –
Never-Married Mothers, By Year of First Birth and Calendar Year

(a) Employment

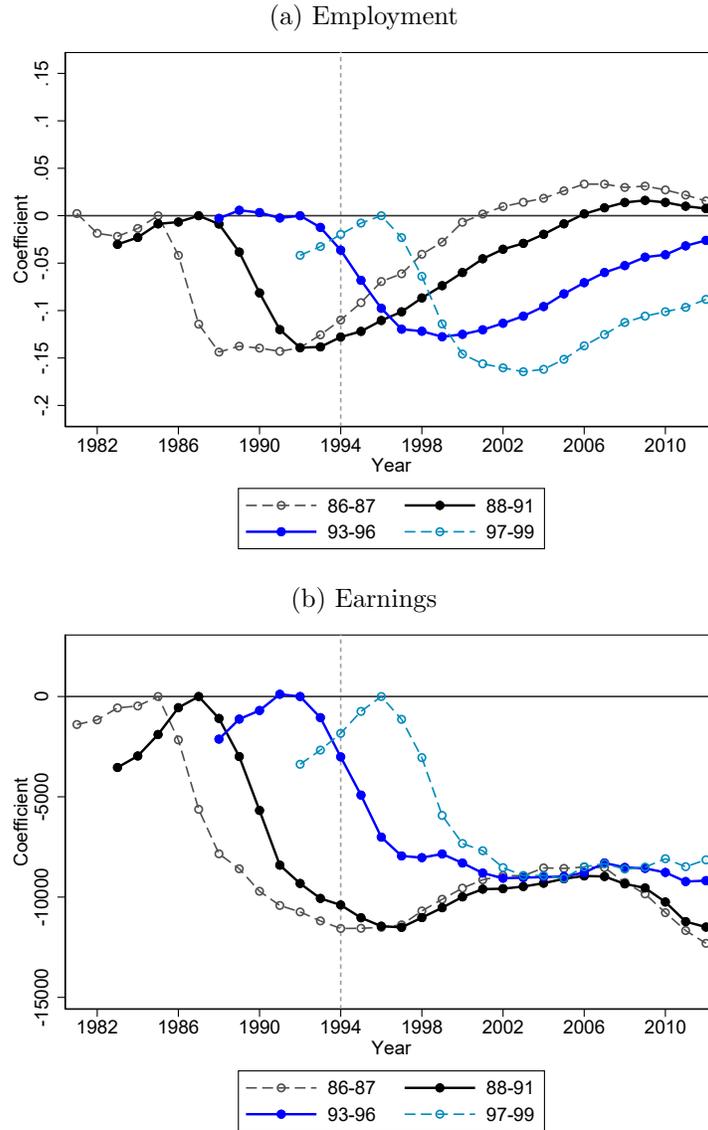


(b) Earnings



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

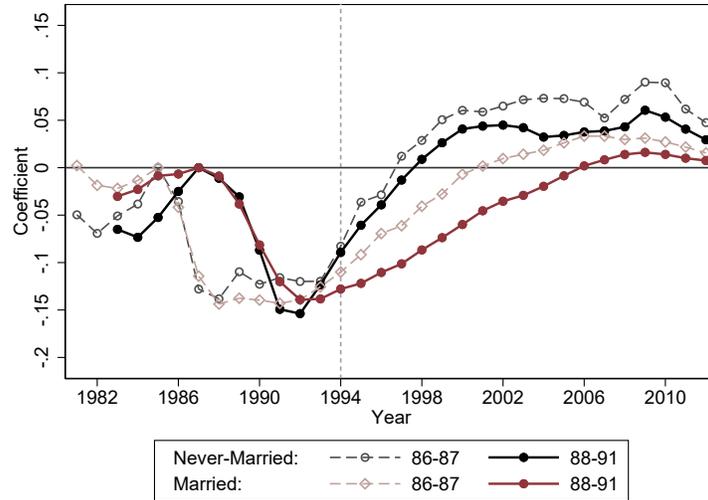
Figure A.16: Effect of Early Work Incentives on Labor Market Outcomes – Married Mothers, By Year of First Birth and Calendar Year



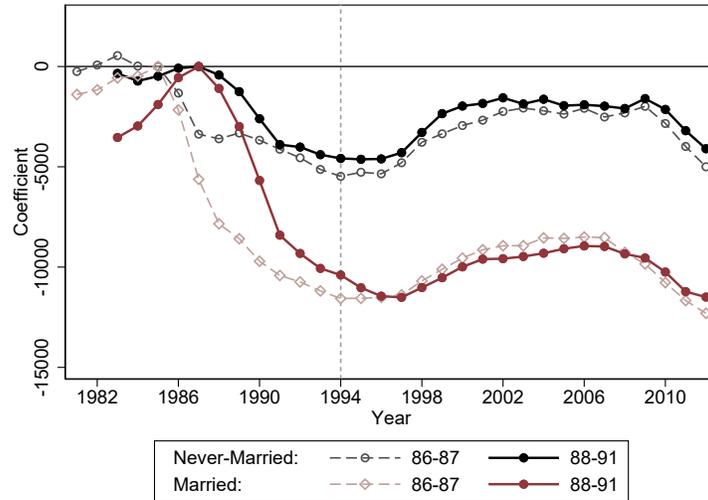
Notes: These figures show coefficients and 95% confidence intervals from calendar-year event studies of the employment (Panel a) or earnings (\$2016, Panel b) of married mothers. We show the estimates on indicators for calendar years interacted with an indicator for being exposed to the 1993 EITC reform early (first birth: 1993–1996), late (first birth: 1988–1991), very late (first birth: 1986–1987) or very early (first birth: 1997–1999). For each group of mothers, the omitted category (reference group) is the year prior to the earliest birth (e.g. 1992, for 1993–1996 births). All regressions include fixed effects for the year of first childbirth, mother’s age, race, education, state of residence, the state-level unemployment rate, minimum wage, AFDC/TANF maximum benefit level, Medicaid generosity, implementation of six types of welfare waivers, implementation of any waiver or TANF, and implementation of the 2009 EITC reform. See the notes of Figure 2 for information on standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to 2012.

Figure A.17: Never-Married and Married Mothers with a First Birth Pre-Reform, By Year of First Birth and Calendar Year

(a) Employment

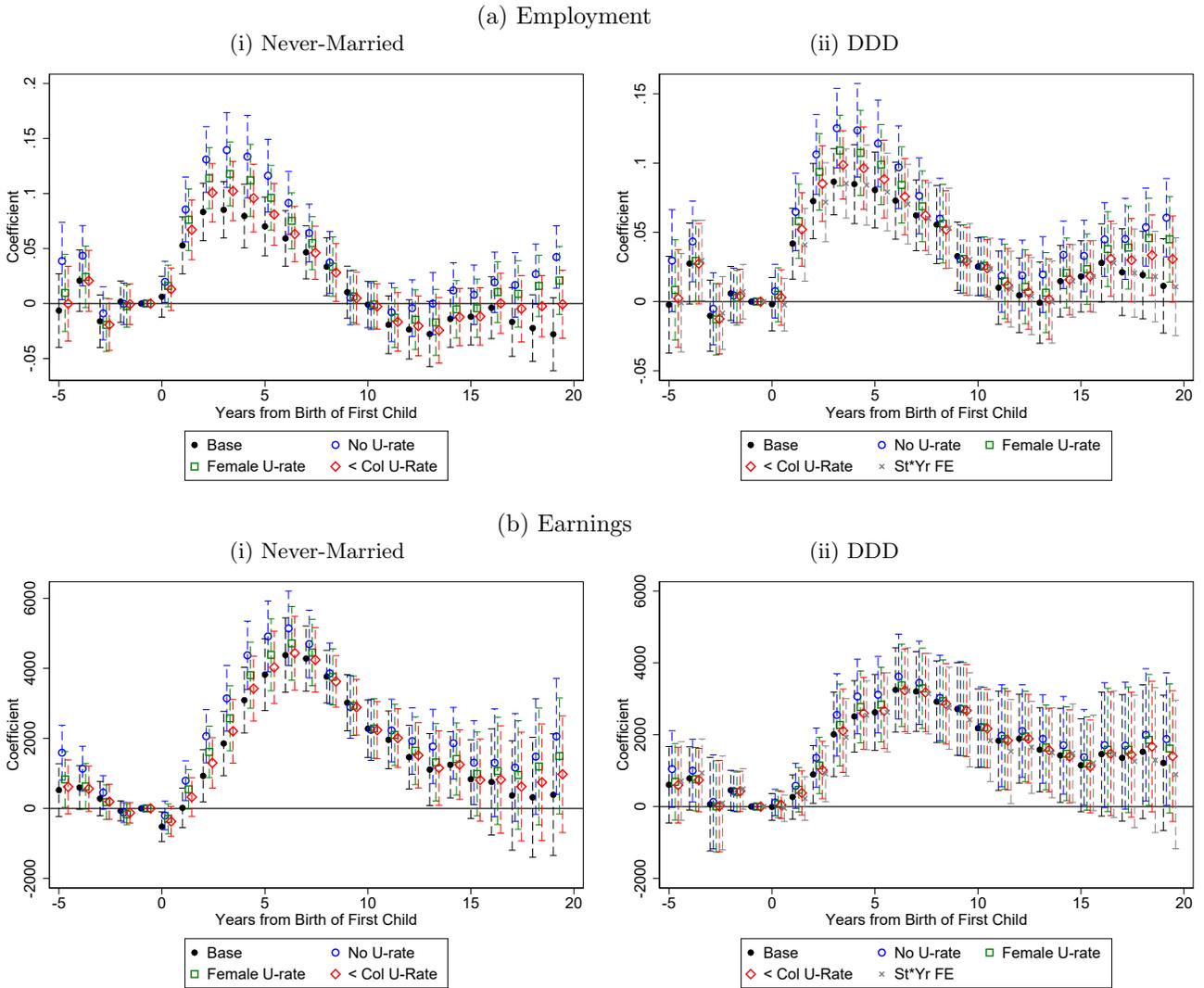


(b) Earnings



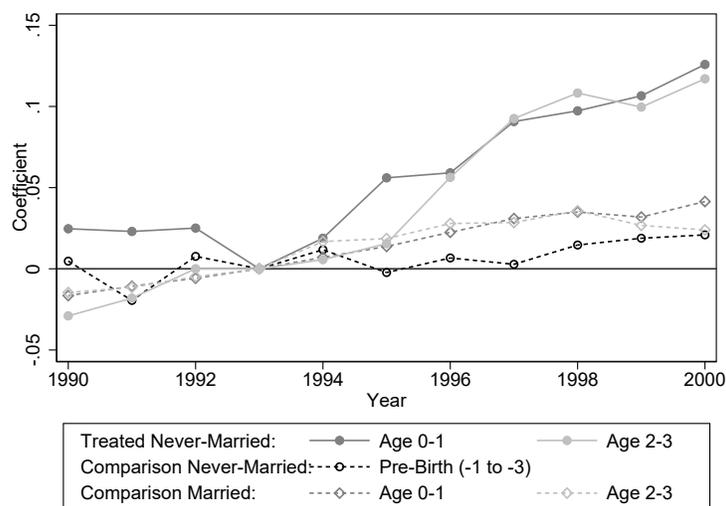
Notes: These figures show coefficients and 95% confidence intervals from calendar-year event studies of the employment (Panel a) or earnings (\$2016, Panel b) of never-married and married mothers that had a first birth pre-EITC reform. We show the estimates on indicators for calendar years interacted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

Figure A.18: Effect of Early Work Incentives on Labor Market Outcomes – Sensitivity to Alternative Unemployment Rate Measures and State-Year Fixed Effects



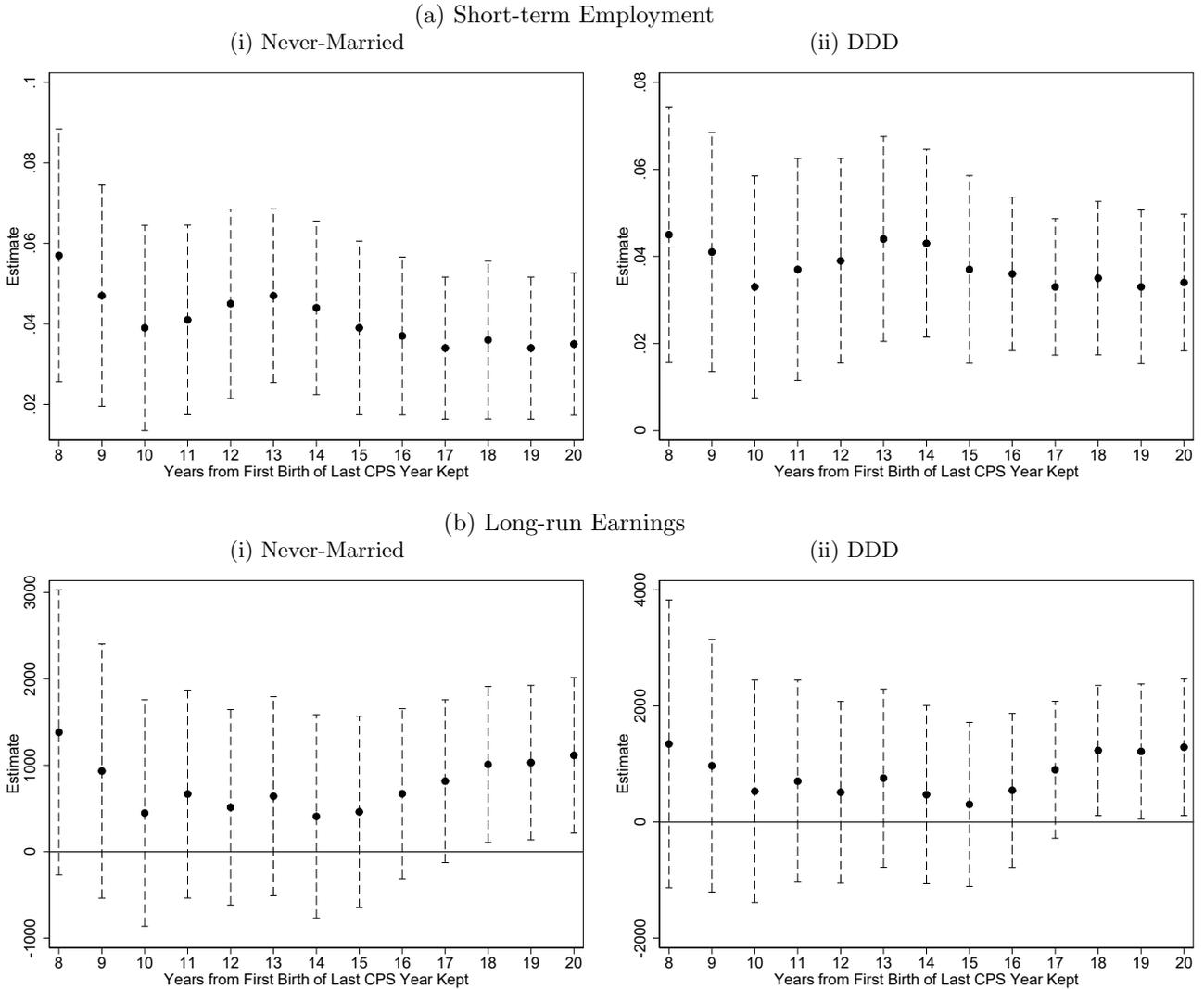
Notes: These figures present the sensitivity of the coefficients and 95% confidence intervals for event studies that compare the employment (Panel a) or earnings (\$2016, Panel b) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), in each year from a first birth. Figure (i) in each panel presents the dynamic DD using never-married mothers. Figure (ii) in each panel presents the DDD in which we use married mothers as an additional comparison group. In addition to the baseline estimates, we show results from specifications where we remove all unemployment rate controls (blue circles); substitute the state-level unemployment rate with a control for the average unemployment rate for women in the state (green squares) or with a control for the average unemployment rate in the state for individuals with less than a college education (red diamonds). See the notes of Figure 2 for information on baseline control variables, standard errors, data and sample construction. We calculate the unemployment rate for women and for individuals with less than college education from the 1983–2015 March CPS. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Figure A.19: Employment of Mothers with Young Children and Future Mothers by Year



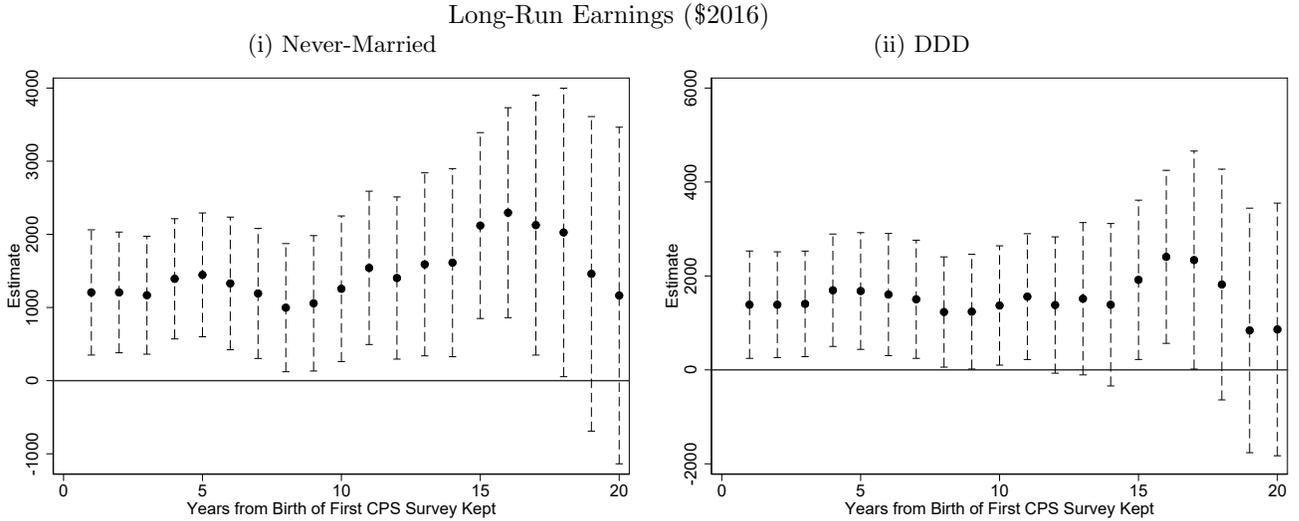
Notes: This figure shows coefficients and 95% confidence intervals from regressions of employment on fixed effects for the calendar year, where 1993 is the omitted category. Controls include indicators for: years since first birth, mother's age, race, education, state, implementation of six types of welfare waivers, implementation of any waiver or TANF, and implementation of the 2009 EITC reform, as well as controls for the state unemployment rate, minimum wage, AFDC/TANF maximum benefit level, and Medicaid generosity. Future mothers consist of mothers that give birth between $t+1$ and $t+3$. "Treated Never Married" includes both early- and late-exposed mothers. *Years*: We include data from 1990 to 2000.

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



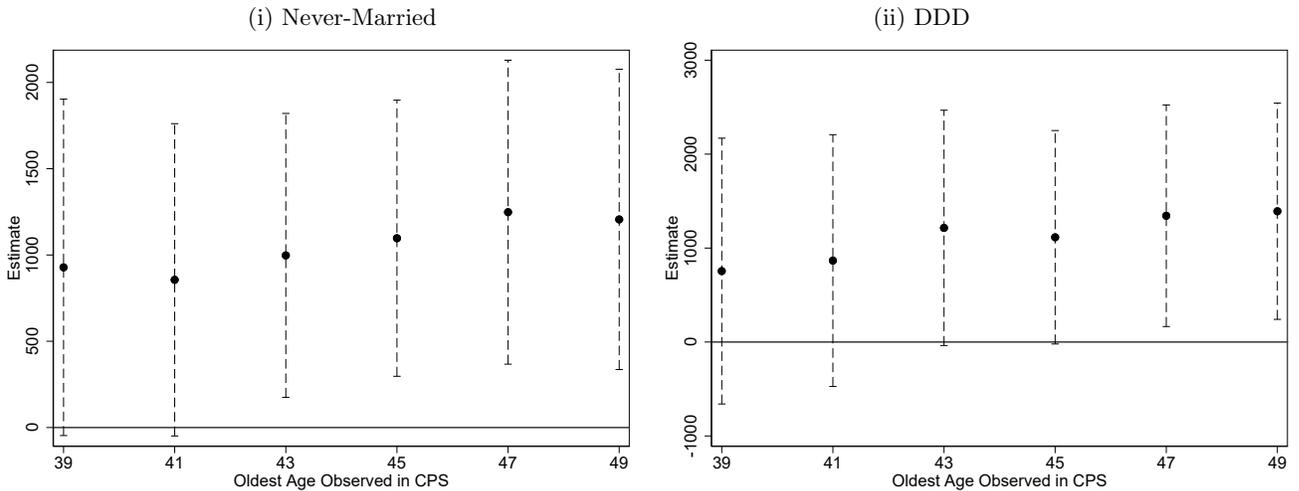
Notes: These figures present estimates and 95% confidence intervals from regressions comparing the labor market outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), as we vary the sample restrictions. Each marker comes from a separate regression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

Figure A.21: Effect of Early Work Incentives on Labor Market Outcomes – Sensitivity to Keeping CPS Surveys *at least* 1 to 20 Years after Birth



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

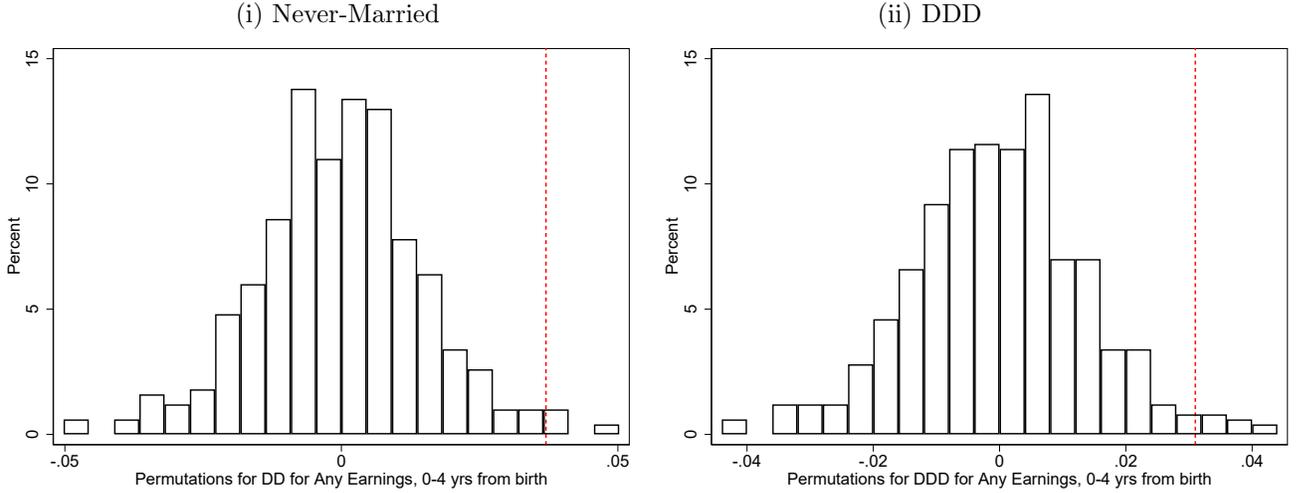
Figure A.22: Effect of Early Work Incentives on Long-Run Earnings – Sensitivity to Using Women Interviewed By Age 39 to 49



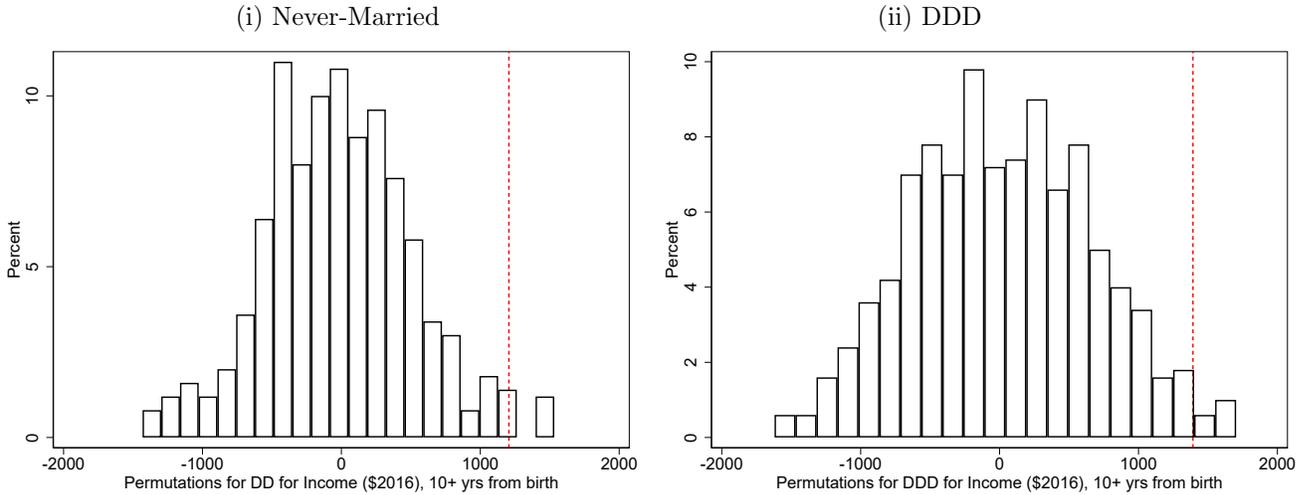
Notes: These figures shows estimates and 95% confidence intervals from regressions comparing the earnings (\$2016) of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from a first birth, as we vary the sample restrictions. Each marker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

Figure A.23: Effect of Early Work Incentives on Labor Market Outcomes – Randomization Inference

(a) Short-term Employment

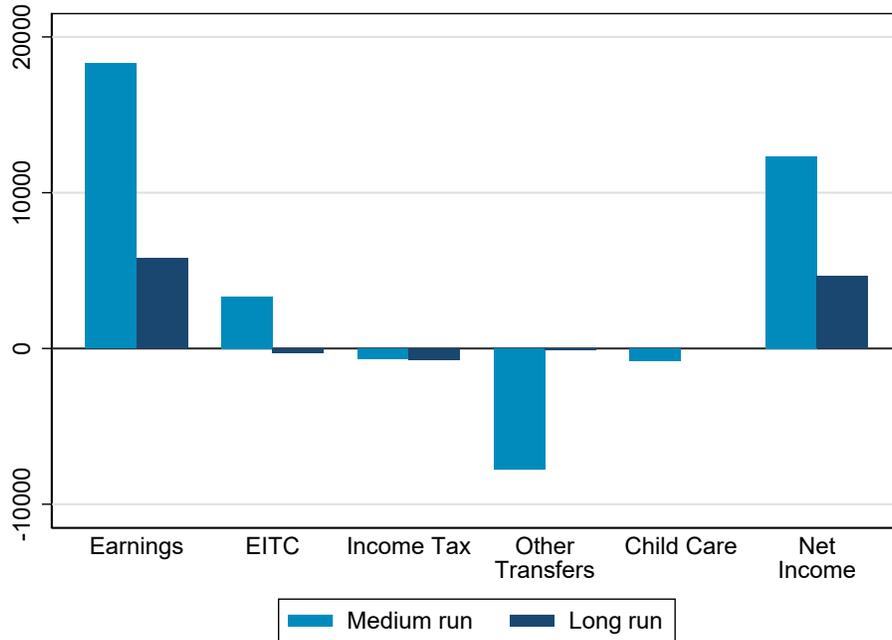


(b) Long-Term Earnings (\$2016)



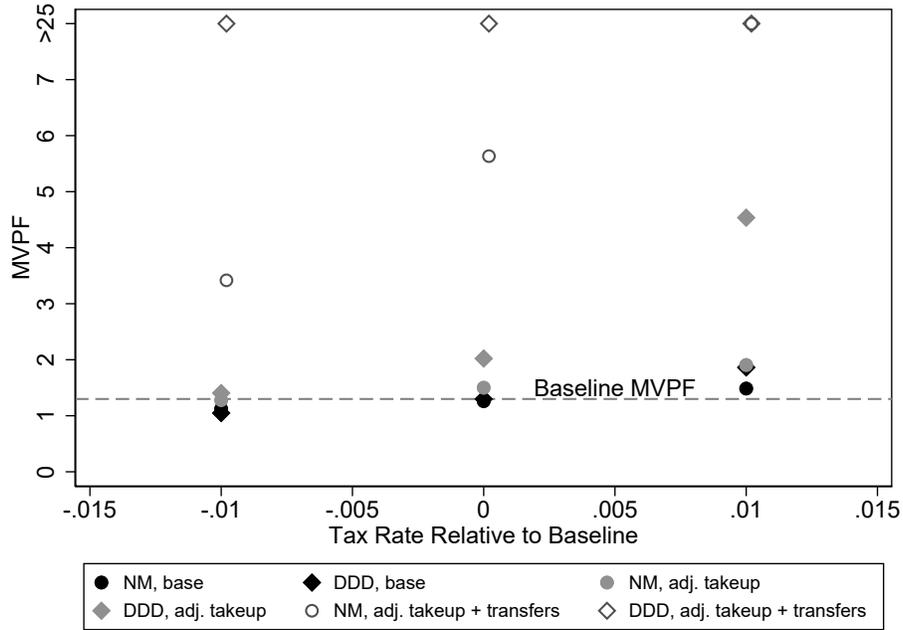
Notes: These figures show the distribution of estimates from 500 placebo experiments comparing the employment (Panel a) or earnings (\$2016, Panel b) of mothers exposed to the EITC “reform” with “early” and “late”, where early and late exposure are randomly assigned. In particular, for each placebo experiment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

Figure A.24: Effect of Early Work Incentives on Net Income
Using DD Estimates



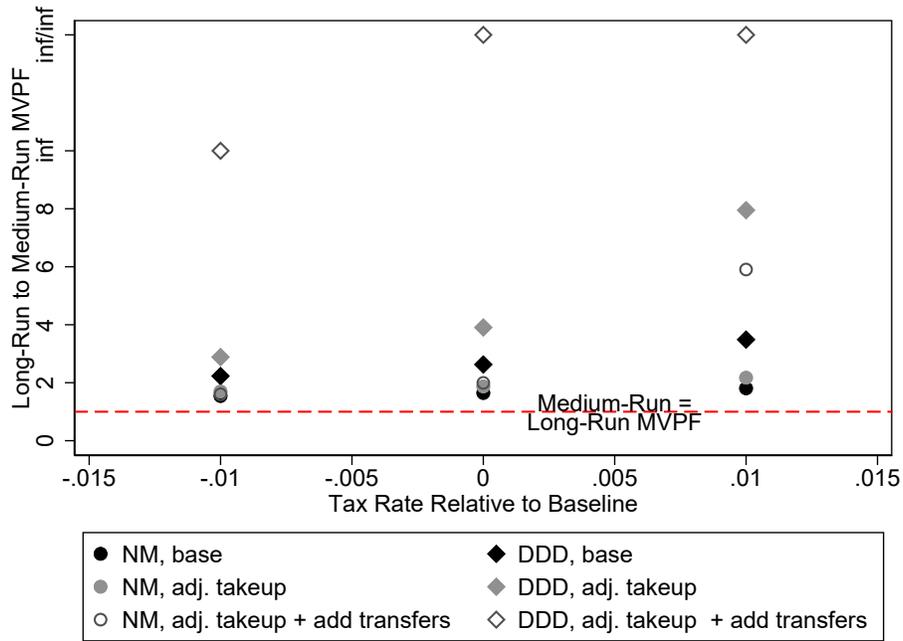
Notes: This figure presents the impact of early exposure on the present value of net income in the medium run (years 0 to 9 post-childbirth) and long run (years 10-19 post-childbirth) stemming from changes in (i) earnings, (ii) EITC benefits, (iii) federal income taxes, (iv) other public transfers, and (v) child care costs. The estimates for (i)-(iii) come from DD specifications that compare mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991) across never-married mothers using the SSA administrative data on earnings, which we combine with information on the EITC benefits schedule for (ii), and estimates of average tax rates from NBER TAXSIM for (iii). See Section F.1 for details about our estimates of average tax rates. We use a 5% annual discount rate to obtain the present value of estimates. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. We include data from 5 years prior to a first birth up to 19 years after a first birth. The estimates for (iv) come from a first-difference specification that compares early- and late-exposed never-married mothers using CPS survey data. See Section for details. We calculate (iv) using estimates of child care costs from Anderson and Levine (2000).

Figure A.25: Long-Run MVPF Across Varying Assumptions



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

Figure A.26: Ratio of Long-Run to Medium-Run MVPF Across Varying Assumptions



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

B Appendix to Section 2

B.1 CPS occupations

Because the CPS occupation categories vary over time, we first create a harmonized occupation variable that spans our entire sample period using the IPUMS “occ1990” classification (Flood et al., 2020).⁶¹ In particular, we downloaded the March CPS from IPUMS for the CPS surveys in our sample, and then collapsed the data by “occ1990” and the original CPS occupation variable to create a crosswalk. We then merge the crosswalk on to our data, which gives us the “occ1990” corresponding to each individual in our sample.

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

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

⁶¹See https://cps.ipums.org/cps-action/variables/OCC1990#codes_section for a description of these codes.

B.2 Matching CPS to Administrative Earnings Records

The match between CPS and SSA records is performed using the PIK, which is a unique mapping to a Social Security Number (SSN) created by the Census Bureau. Until 2006, PIKs were assigned using validated SSN's, if available, or a probabilistic match using name, address, and demographic information, such as date of birth. Since 2006, the PIK has been assigned solely using the probabilistic match, which prevents the need to request an SSN from respondents (Czajka et al., 2008). This match is only available for the 23 CPS surveys in our sample (1991, 1994, and 1996 to 2016). Conditional on an individual being matched to the SSA records, we observe W-2 and self-employment earnings in each year. Below we show the share of married and never-married women that meet our sample criteria who are matched in each March CPS.

Table A.21: CPS-SSA Data Matching Rates –
By Year, Marital Status and EITC Exposure

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

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

Comparing CPS and administrative earnings To compare earnings in the CPS and SSA records, we use the “wage and salary” earnings reported in our linked CPS surveys and the sum of the W2 and self-employment earnings (for the year prior to the survey). We find several discrepancies across these sources. First, we find that 10% of the observations differ on whether an individual had any earnings. Over 60% of these errors are due to an individual reporting no earnings in the CPS, but having some earnings in the administrative data. Among individuals that have any earnings in both sources, there are substantial differences between the log of the administrative earnings and the log of the CPS earnings. The interquartile range for this measure ranges from -0.27 to 0.20, centered around 0, implying that discrepancies do not go in a consistent direction. Assuming that individuals can not earn less than what is reported in the administrative records, this suggests that at least half of the CPS earnings in our sample are reported with error.⁶²

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

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

⁶²See Abowd and Stinson (2013) for a discussion of possible sources of discrepancies between self-reported earnings and administrative records.

C Relation to Kleven (2019)

As we note in the paper, the close timing of welfare reform with the 1993 EITC reform (hereafter, the reform) raises a number of potential challenges for our estimation. Kleven (2019) highlights a number of specific concerns in this vein. In this section, we outline the key points in Kleven’s analysis of the reform and how our results address or differ from Kleven’s findings.⁶³

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

1. Impacts by number and age of children Different than Kleven, we do not find strictly increasing employment effects by family size or decreasing effects by child age. In particular, while we find that post-birth employment increases more after a second birth than after a first birth; we do not find a statistically significant difference between third or higher-order births and second births (see Section 5.1). These patterns are consistent with EITC incentives. Moreover, we do not find different employment effects between mothers whose first child at the time of the reform was no older than 1 (“early-exposed”), between the ages of 3-6 (“late-exposed”), or between the ages of 7 and 8 (supplementary group) – see Appendix Figure A.15. Our patterns are consistent with Grogger (2003a), who also does not find that the impact of the EITC varies by the age of one’s youngest child.

One potential explanation for the difference in our results is that Kleven’s analysis does not account for changes in *unobservable* characteristics of mothers over time, while our panel difference-in-difference strategy does. In support of this hypothesis, Hotz and Scholz (2006) employ a panel family fixed effects strategy and find the same patterns by family size as we do.

2. Elasticity estimates Our back-of-the-envelope calculation in Section 4.1 suggests that the elasticity of employment to pre-tax labor earnings is between 0.54 and 0.72, or roughly 27% and 40% as large as the estimate for mothers with one child in Kleven (2019). The discrepancy between

⁶³Kleven also raises concerns with estimated effects of other EITC reforms – we do not address those here, since they are not relevant for our analysis.

our estimates and Kleven’s estimates reflect differences both in the numerator and the denominator of the elasticity. First, our employment effects in percent terms are half the size of Kleven’s: 5.9 percent ($\frac{3.7}{63.1}$) vs. 12.4 percent ($\frac{8.5}{68.1}$).⁶⁴ Second, Kleven calculates a 6.8% average change in tax rates. He obtains this by simulating taxes across years using observed earnings for working single mothers and predicted earnings for non-workers (based on individual characteristics). Instead, we calculate the change in EITC benefits between early- and late-exposed mothers using the post-birth distribution of late-exposed never-married mothers for workers, and imputing EITC benefits in three ways for non-workers. Specifically, we impute benefits assuming that non-workers earnings’: (i) fall only in the phase-in region (ii) fall only in the phase-in or flat regions (weighted using the distribution of workers across these regions); or (iii) have the same distribution of earnings as working single mothers.⁶⁵ This produces changes in EITC benefits equal to an 8.2, 9.9, or 10.9 percent change as a share of pre-tax earnings, respectively. Our higher change in benefits primarily reflects our lower-income and younger population and the longer period over which we estimate changes in the EITC (e.g., we include the 1990 reform as part of our treatment). It also reflects the fact that, for some estimates, we allow the distribution of earnings for non-workers to be more concentrated in the phase-in regions post-reform. Thus, we do not make the strong assumption that the earnings distribution remains the same post-reform, as Kleven does.

3. Controlling for welfare waivers and business cycle In a similar spirit to Kleven, we allow our unemployment rate and welfare waiver controls to be “dynamic” in allowing differential impacts by the age of one’s first child.⁶⁶ Our estimates are not affected by allowing for this flexibility (see Section 5.1). We also show that our employment effects are present when we restrict our sample period up to 1996 and limiting our sample to states that did not pass any waivers prior to 1996 (e.g., Table 4, columns 7–8). Thus, these effects do not appear to be sensitive to these welfare controls, different than Kleven (2019). Further, we note that Kleven’s effects inclusive of these controls are quite imprecise, and could not reject our estimated effects.⁶⁷

⁶⁴Again, we speculate that part of this difference is due to the fact that we control for pre-birth differences in labor market outcomes.

⁶⁵The first two assumptions are motivated by the idea that non-workers are likely to be negatively selected on wages, or might be more likely to prefer part-time work.

⁶⁶We do not model event-year dynamics for the welfare waivers as in some of the specification in Kleven (2019) because with six welfare waivers, passed largely in the 1990s, the dynamic waiver-event-time indicators quickly become collinear with our effects of interest. Nonetheless, given the strong relationship that Kleven shows between welfare response and child age, we would expect that these controls would account for important differences in incentives.

⁶⁷For example, our effect inclusive of these controls is 3.2 pp. (column 5, Table 2), which is within the confidence interval of his 1.06 p.p. (s.e = 1.5 p.p.) in column 3 of Table 6.

D Appendix to Section 4.1

Elasticity calculation To translate our impacts on employment into an elasticity of employment to labor earnings, we need to scale the 5.9% change in employment by the percent change in average EITC benefits between early- and late-exposed mothers. We calculate this latter change using the one-child EITC benefit schedule for early- and late-exposed mothers weighted by the post-birth earnings distribution of late-exposed never-married mothers (see Appendix Figure A.8), and assign non-workers either (i) the change in benefits in the phase-in region; (ii) the average change in benefits in the phase-in and flat regions; or (iii) the average change in benefits among all workers, in a similar spirit to Kleven (2019).⁶⁸ This produces a 10.9%, 9.9% and 8.2% change in average EITC benefits, respectively, and a range of elasticities between 0.54 ($\frac{5.9}{10.9}$) and 0.72 ($\frac{5.9}{8.2}$).

Bunching at the first EITC kink We find little pre- or post-birth bunching when we examine all early-exposed mothers. However, consistent with, e.g., Chetty et al. (2013) and Saez (2010) we do find evidence of post-birth bunching among mothers who are ever self-employed in Appendix Figure A.27 and Appendix Table A.5. Hence, while some early-exposed mothers appear to be aware of the incentive for bunching at the EITC kink, this is not a primary driver of earnings responses. Further, we do not detect any pre- or post-birth bunching among late-exposed mothers in Appendix Figure A.27, in line with previous evidence that bunching increased after the 1993 reform (Saez, 2010).⁶⁹

Heterogeneity by state-level EITCs We estimate the interaction between the impacts of the EITC reform and state EITC supplements in Appendix Table A.22.⁷⁰ Columns 1 and 3 show that, on average, post-birth employment does not vary with the presence of a state EITC supplement (column 1) or with the generosity of the supplement (column 3). This may reflect the small number of EITC's during the early 1990s, or the lack of salience of these benefits. However, we find that early-exposed mothers' employment increases more in states that have an EITC supplement (column 2) or have a more generous EITC supplement (column 4). This is consistent with early-exposed mothers' responding to the generosity of work incentives after the EITC reform. Nevertheless, because state EITCs are not randomly assigned, we view this evidence as only suggestive.

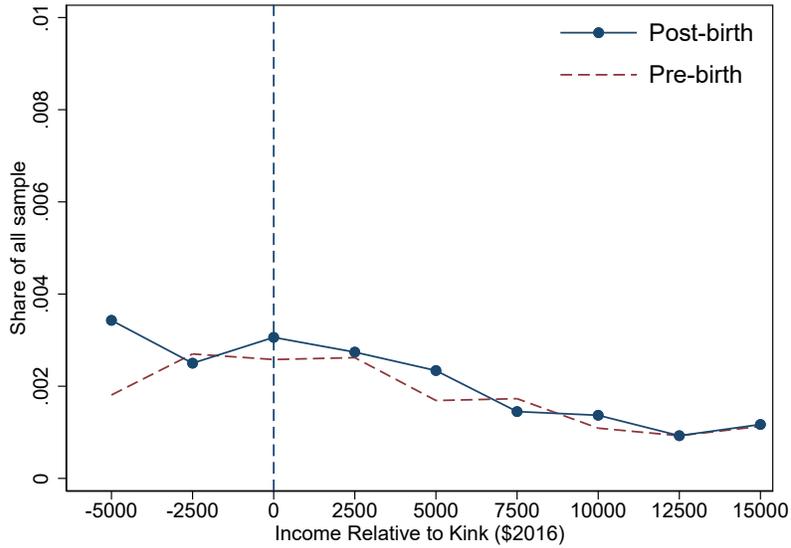
⁶⁸This will underestimate the change in benefits if mothers have more than one child.

⁶⁹We find no evidence of bunching at the second EITC kink, as in prior work (e.g., Saez, 2010).

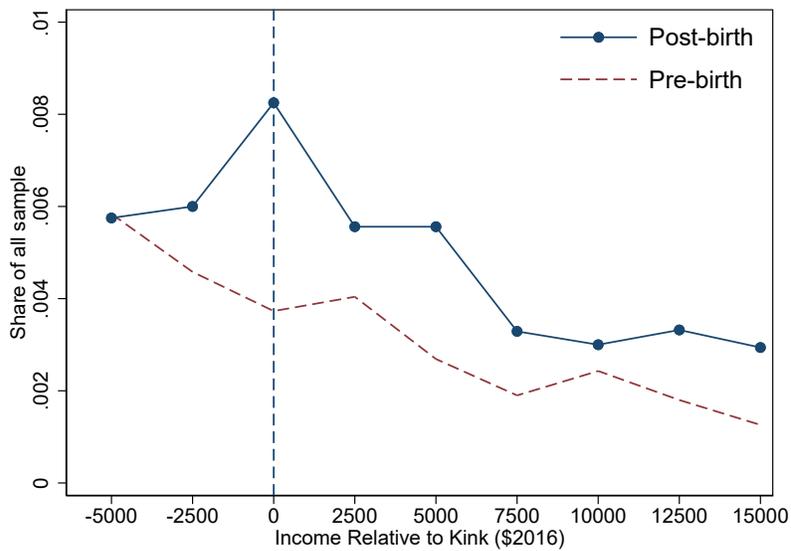
⁷⁰We obtain information on state EITC supplements from <https://users.nber.org/~taxsim/state-eitc.html>. Supplementary EITC's are typically set as a percentage of the federal EITC; thus, a mother living in a state with a supplement is eligible for a more generous credit, and can expect a larger increase in her credit after a federal reform.

Figure A.27: EITC Expansion and Bunching Before and After Birth – Never-Married Mothers

(a) Late-Exposed Self-Employed Mothers



(b) Early-Exposed Self-Employed Mothers



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

Table A.22: Effect of Early Work Incentives on Short-Run Employment – Heterogeneity by the Presence and Generosity of a State EITC Supplement

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

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

E Appendix to Section 6

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

Impacts on “high earnings” and “high experience” We first provide further justification and detail about the variables that we use in this analysis. As discussed in the text, we measure “high earnings” using an indicator for being in the top 25% of the earnings distribution of all mothers, defined in each year since first birth. We use this measure because early exposure has a larger and more precise effect on being in the top 25% of earnings in the long-run than being in the top 75% or top 50% of the earnings distribution (see Panel (a) of Appendix Table A.23). Thus, we consider this to be the best proxy for the impacts of early exposure. As also discussed in the text, we measure “high experience” using an indicator for whether a mother worked in the first three years after her first birth. To construct this variable, we create a measure of “potential experience” which is equal to one’s actual total experience for $\tau \leq 0$, increases by one in each year for $1 \leq \tau \leq 3$, and increases by 1 in each year that a mother works for $\tau > 4$. We then define a mother as having “high experience” if her actual experience is equal to her potential experience.

Next, we calculate the share of high- or low-experience mothers with high earnings. The DDD coefficients in Panel (b) of Appendix Table A.17 imply that early-exposed mothers have a 2 p.p. higher likelihood of having jointly high earnings and high experience, and that they have a 9.5 p.p. (2+7.5) higher likelihood of having high experience. Thus, the proportion of (marginal) early-exposed mothers with high earnings among those with high experience is 21 percent (2/9.5). Conversely, early-exposed mothers have a 0.3 p.p lower likelihood of jointly having high earnings and low experience, and a 9.5 p.p. lower likelihood of having low experience (0.3 + 9.2). Thus, the proportion of (marginal) early-exposed mothers with high earnings among those with low experience is 3.2 percent.

Among all never-married mothers with high experience, the share of high earnings is 19 percent (12.5/(12.5+54.5)), using the averages at the bottom of Panel (a) of Appendix Table A.17. Among all never-married mothers with low experience, the share with high earnings is 6.3% (2.1/(2.1+31)). Thus, we conclude that early-exposed mothers have similar returns to experience as the average never-married woman in our sample.

Finally, we consider the sensitivity of our results to instead measuring “high experience” using

an indicator of whether an individual is in the top 75% of the experience distribution of all mothers, where the distribution is defined separately in each year since first birth. We focus on the top 75% of experience because Appendix Table A.23 shows that early exposure has a larger and more precise effect on being in the top 75% of experience in the long-run than being in the top 25% or top 50% of the experience distribution. On average, this is a higher threshold for “high experience:” it includes just 58% of never-married mothers, compared to 67% using the “worked 3 years after first birth” variable.

In line with our main results, Appendix Table A.24 shows that there are increases in the probability of being “high earning and high experience” and no effect on being “high earning and low experience” with this measure. We also find no change in the share of low experience mothers with high earnings (using the calculation described above). Interestingly, as a share of the additional early-exposed mothers that have high experience, 40 to 63% end up being “high earning” across the DD and DDD. This is higher than the share in our main results, which is consistent with the fact that this is a higher threshold of experience.

Table A.23: Effect of Early Work Incentives on Having Earnings or Experience in the Top 75%, 50%, or 25%

	Top 75 Percent		Above Median		Top 25 Percent	
	NM (1)	DDD (2)	NM (3)	DDD (4)	NM (5)	DDD (6)
<i>A: Earnings</i>						
PostBirth * EarlyExp * 10+ Yrs From Birth	-0.007 (0.012)		0.007 (0.010)		0.019*** (0.006)	
PostBirth * EarlyExp * 10+ Yrs From Birth * NM		0.019 (0.012)		0.016 (0.011)		0.017** (0.008)
Mean Y	0.724	0.738	0.418	0.500	0.145	0.250
Individuals	282275	2714475	282275	2714475	282275	2714475
<i>B: Experience</i>						
PostBirth * EarlyExp * 10+ Yrs From Birth	0.052*** (0.008)		0.028*** (0.006)		-0.018*** (0.005)	
PostBirth * EarlyExp * 10+ Yrs From Birth * NM		0.028*** (0.008)		0.011 (0.007)		0.005 (0.005)
Mean Y	0.582	0.719	0.251	0.470	0.093	0.214
Individuals	282275	2714475	282275	2714475	282275	2714475

Notes: This table shows the results from regressions comparing the outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from first birth. The outcomes are indicators for being at or above a threshold in the earnings (Panel a) or experience (Panel b) distributions. The thresholds are: top 75% (columns 1-2), top 50% (columns 3-4), or top 25% (columns 5-6). The distributions are defined separately for each year since first birth and include both married and never-married mothers. The odd columns show the DD using never-married mothers. The even columns show the DDD in which we use married mothers as an additional comparison group. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 19th year after a first birth.

Table A.24: Effect of Early Work Incentives on Jointly Having “High Earnings” (Top 25%) and “High Experience” (Top 75%)

	Pr(High Earn + High Exp)	Pr(High Earn + Low Exp)	Pr(Low Earn + High Exp)	Pr(Low Earn + Low Exp)
	(1)	(2)	(3)	(4)
<i>A: Never-Married</i>				
10+ Yrs From Birth * EarlyExp	0.021*** (0.005)	-0.002 (0.002)	0.031*** (0.009)	-0.050*** (0.009)
Mean Y	0.134	0.012	0.449	0.406
Observations	282275	282275	282275	282275
<i>B: DDD</i>				
10+ Yrs From Birth * EarlyExp * NM	0.017** (0.007)	0.000 (0.002)	0.010 (0.010)	-0.028*** (0.008)
Mean Y	0.240	0.010	0.478	0.272
Observations	2714475	2714475	2714475	2714475

Notes: This table shows the results from regressions comparing outcomes of mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 10+ years from first birth. The outcomes are indicators for having “high earnings” (top 25%) or “low earnings” (bottom 75%) crossed with indicators for having “high experience” (top 75%) or “low experience” (bottom 25%). We show estimates for having “high experience and high earnings” (column 1), having high earnings and low experience (column 2), “low earnings and high experience” (column 3), and “low earnings and low experience” (column 4). Panel (a) presents the DD using never-married mothers. Panel (b) presents the DDD in which we use married mothers as an additional comparison group. See the text and Appendix E for more details. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 19th year after a first birth.

F Appendix to Section 7

F.1 Calculation of Average Tax Rate

In Section 7, we estimate the effect of early exposure to the EITC expansion on federal income tax revenue. This requires an estimate of the average tax rate for the additional dollars earned by early-exposed mothers in the short-, medium-, and long-run. In this section, we explain how we calculate this tax rate.⁷¹

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

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

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

First, we use the coefficients from our distributional regressions (i.e., Figures 3, A.9 and A.11) to generate estimates of $\Delta f_{j,\tau}$. Recall that the distributional regressions give estimates of the difference in the cdf of earnings between early- and late exposed mothers for the short-, medium-, and long-run.⁷² In particular, we have estimates of $Pr(Y > y)^{early} - Pr(Y > y)^{late}$ for $y \in \{0, 2500, \dots, 100000\}$. We can use these estimates to obtain $\Delta f_{j,\tau}$ for \$2,500 bins of earnings. To do so, we take the difference between the distributional estimates for two sequential y . For instance, the change in the density of earnings between \$5,000 and \$7,500 is equal to the difference between the change in the cdf at $y = 7500$ and $y = 5000$.⁷³

Second, we obtain an estimate of $\rho_{j,\tau}$ for each bin from NBER TAXSIM (Feenberg and Coutts, 1993). In particular, we obtain $\rho_{j,t}$ for calendar year t as the ‘‘Income Tax Before Credits’’ (for a head of household with one dependent) divided by z_j . We calculate this for each z_j in each calendar

⁷¹Another approach would be to calculate taxes directly for each mother using TAXSIM, however TAXSIM is not available to be used from the SSA data center.

⁷²We use the same estimates for all τ within the short-, medium-, and long-run.

⁷³E.g.,

$$\begin{aligned} & [Pr(Y > 5000)^{early} - Pr(Y > 5000)^{late}] - [Pr(Y > 7500)^{early} - Pr(Y > 7500)^{late}] \\ &= [Pr(Y > 5000)^{early} - Pr(Y > 7500)^{early}] - [Pr(Y > 5000)^{late} - Pr(Y > 7500)^{late}] \\ &= Pr(7500 \geq Y > 5000)^{early} - Pr(7500 \geq Y > 5000)^{late} \\ &= \Delta f_{7500 > y > 5000} \end{aligned}$$

year. We then take averages over calendar years to obtain $\rho_{j,\tau}$.

Third, combining the inputs from the previous two steps, we calculate ρ_{avg} for the short-, medium-, and long-term. For instance, for the long-run, this is equal to:

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

where j denotes \$2,500 bins of earnings.⁷⁴ We obtain average tax rates that range from 0–0.04, 0.05–0.07, and 0.13–0.14, for the short-, medium-, and long-run, respectively, using the DD and DDD distributional estimates. We use the minimum of the tax rate for each period to calculate tax revenue: 0, 0.05, and 0.13.

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

F.2 Government Transfers

In Section 7 we estimate the impact of work incentives on government transfers using information on self-reported income from various government programs from the CPS. In particular, we analyze government transfers to a woman’s family from the following 5 programs, and total benefits as the sum of benefits from these five categories:⁷⁵

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

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

⁷⁴Since we estimate our distributional regressions over groups of τ , in practice we only have one value of $\Delta f_{j,\tau}$ for the short-, medium-, and long-run (each).

⁷⁵We use household information for food stamps, as family food stamp information is not collected in the 1991 CPS. Note that we observe 1 unique woman in 99.9% of households, so the risk of double counting food stamp receipt because of multiple treated women in the same household is minimal.

mothers in the first couple of years after birth, and that the differential effects for early-exposed mothers are estimated only in post-birth years 3 and 4. The missing data after 2011 makes it such that we have little information on early-exposed mothers in the long-run, and that their differential effects are estimated only in some of the long-run years.

For these reasons, we interpret our estimates of the impact of early-exposure on transfers in Appendix Table A.20 with caution. The reasoning above suggests that these estimates are likely to be an upper bound on the (absolute) decline in transfers, and leads us not to incorporate this into our baseline MVPF estimates (see more below).

F.3 Calculation of MVPF

Separating the “behavioral” and “mechanical” change in EITC benefits For the MVPF calculation, we need to decompose the impact on total EITC benefits (calculated in Section 7) into changes in benefits stemming from labor supply responses (“behavioral”) and changes in EITC generosity (“mechanical”). In the MVPF framework, the “mechanical” growth is a pure transfer to recipients and thus gives the lower bound of the value of the benefits to mothers (Hendren and Sprung-Keyser, 2019). We continue to focus on the EITC benefits that a household is *eligible* for, but discuss incomplete take-up below.

We capture these two channels of impacts on EITC benefits as follows. To estimate the “behavioral” response, we simulate a *hypothetical* EITC benefit at each child age based on household earnings and the EITC schedule for 1994 first births. This is the EITC amount that a household would receive in each year if its first birth had been in 1994 – hence, it incorporates changes in earnings while holding the EITC schedule constant. To estimate the “mechanical” impact on EITC benefits, we take the difference between total benefits and this hypothetical “behavioral” benefit. This is the *additional* amount of benefits that a household would receive in each year if its first birth was in 1994 instead of 1988 (i.e the “mechanical” change in benefits from the expansion).

Columns 1, 2 and 3 of Appendix Table A.25 presents the estimated effects for our simulated total EITC benefits, benefits through the “behavioral” channel, and benefits through the “mechanical” channel, respectively. The DD and DDD estimates are presented in Panels (a) and (b) of the table, but for brevity we will discuss our results as a range between these. In the short-run, early-exposed mothers’ EITC benefits increase by \$400 to \$619. Over half of this increase (54–70%) is accounted for by greater generosity (column 3), which implies that a large share of the increase in EITC spending was a transfer to already-working mothers. In the medium-run, early-exposed mothers’ EITC benefits increase by \$53 to \$93 (4–7%). There is no meaningful “mechanical” difference in benefits and, consistent with the substantial earnings growth during this period, the “behavioral” response is roughly half the size of the short-run estimate. In the long run, early-exposed mothers’ EITC benefits decrease by \$89 to \$99, an effect driven by the behavioral response. Over twenty years, early-exposed mothers are eligible for \$2,626 to \$3,229 more in EITC benefits, which has a present value between \$2,328 and \$3,027 using a 5% discount rate.

MVPPF estimates Our estimates imply that, over twenty years, early-exposed mothers are eligible to receive in present value terms at least \$2,328 in EITC benefits (\$1,000 of which is a pure transfer to recipients), pay at least \$1,441 more in taxes, and receive at least \$831 less in government transfers (excluding welfare and the value of Medicaid).

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

$$MVPPF = \frac{WTP}{\text{Cost} + \text{Fiscal Externality}} \leq \underbrace{\frac{WTP}{\text{Cost} + \text{Add'l Taxes}}}_{\text{Our baseline estimate}} \quad (3)$$

Depending whether we use the DDD estimates or the DD estimates, we obtain a long-run MVPPF between 1.26 ($\frac{2,000}{3,027-1,441}$) and 1.30 ($\frac{1000}{2,328-1,559}$). The MVPPF increases to 1.5 if we account for incomplete take-up of the EITC, or to at least 5.6 if we incorporate the (at least) \$831 decline in transfers. We show a range of MVPPFs across specifications and tax rate assumptions in Appendix Figure A.25.

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

Comparison to Bastian and Jones (2020) and Hendren and Sprung-Keyser (2019) It is worth noting that our focus on new mothers and never-married mothers implies that our MVPPF is not the same as the overall MVPPF of the 1993 EITC expansion (i.e., for all eligible families). Inclusive of transfers, our MVPPF estimate of 5.6 is larger than prior EITC MVPPFs, which range from 1.08 to 1.12 (Hendren and Sprung-Keyser, 2019) for the 1993 expansion, or from 3.18 to 4.23 (Bastian and Jones, 2020) for all post-1990 EITC expansions.⁷⁶ Our higher estimate likely reflects a couple of key factors. First, as mentioned above, incorporating long-run earnings increases the MVPPF. Second, we show that new mothers experience larger changes in work experience and thus greater gains from work incentives. Third, our estimates exclude married mothers, who generally reduce the MVPPF of the EITC. In that sense, our estimates are a more relevant benchmark for the benefits of a work incentive for new mothers or single mothers than for evaluating the EITC.

⁷⁶In other respects, our estimates align closely with this prior work. Our estimated “mechanical” share of the EITC increase is identical to Bastian and Jones (2020) (who estimate this to be between 54–72%), and is slightly lower than Hendren and Sprung-Keyser (2019) (who estimate this to be 89.5% using estimates from Hoynes and Patel, 2018).

Table A.25: Effect of Early Work Incentives on EITC Benefits

	Total (1)	Behavioral (2)	Mechanical (3)
<i>A: Never-Married</i>			
0-4 Yrs From Birth * EarlyExp	619.3*** (38.1)	188.0*** (34.8)	431.3*** (17.3)
5-9 Yrs From Birth * EarlyExp	52.9 (32.5)	35.0 (32.8)	17.9*** (1.5)
10+ Yrs From Birth * EarlyExp	-99.1*** (31.0)	-95.7*** (31.8)	-3.5** (1.6)
Observations	282275	282275	282275
<i>B: DDD</i>			
0-4 Yrs From Birth * EarlyExp * NM	400.3*** (45.0)	186.2*** (39.5)	214.1*** (16.1)
5-9 Yrs From Birth * EarlyExp * NM	92.9*** (33.2)	81.9** (33.0)	11.0*** (1.8)
10+ Yrs From Birth * EarlyExp * NM	-89.0** (33.7)	-85.1** (34.0)	-3.9*** (1.2)
NM Mean 0-4 Yrs From Birth	1068.5	–	–
NM Mean 5-9 Yrs From Birth	1423.3	–	–
NM Mean 10+ Yrs From Birth	1280.4	–	–
Observations	2714475	2714475	2714475

Notes: This table shows the results from regressions comparing the simulated EITC benefits (in 2016 dollars) between mothers exposed to the 1993 EITC reform early (first birth: 1993–1996) and late (first birth: 1988–1991), 0-4, 5-9 and 10+ years from first birth. Panel (a) shows the results from the DD using never-married mothers. Panel (b) shows the results from the DDD in which we use married mothers as an additional comparison group. The outcomes are simulated total EITC eligibility (column 1); the “behavioral” change in EITC benefits, estimated using a simulated EITC that assigns all mothers the EITC schedule of 1994 first births (column 2); and the “mechanical” change in benefits, estimated using the difference between simulated benefits in columns 1 and 2 (column 3). See the text for details. See Table 1 for information on control variables, standard errors, data and sample construction. *Years*: We include data from 5 years prior to a first birth up to the 19th year after a first birth.