Long-Run Effects of Incentivizing Work After Childbirth

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Abstract

This paper identifies the impact of increasing post-childbirth work incentives on mothers' long-run careers. We exploit variation in work incentives across mothers based on the timing of a first birth and eligibility for the 1993 expansion of the Earned Income Tax Credit. Ten to nineteen years after a first birth, single mothers who were exposed to the expansion immediately after birth ("early"), rather than 3–6 years later ("late"), have 0.62 more years of work experience and 4.2% higher earnings conditional on working. We show that higher earnings are primarily explained by improved wages due to greater work experience.

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The substantial and persistent "child penalty" in women's earnings has been widely documented.[1] However, the source of this penalty, particularly for mothers that return to work, remains unclear. It has long been argued that career interruptions are an important factor in women's wages (Mincer and Polachek, 1974), yet there is little *causal* evidence to corroborate such experience effects.[2]

Importantly, the return to work experience for new mothers is uncertain. On the one hand, new mothers commonly work part-time and in less-time-intensive occupations, which may entail a *lower* return to experience (Goldin, 2014). This could be further amplified if mothers also sort into lower-paying firms (Card et al., 2015). On the other hand, new mothers may obtain a *higher* return to experience if work after childbirth signals commitment to employers; leads to greater on-the-job training (Thomas, 2019; To, 2018); or makes it easier or more desirable for mothers to find future employment.

In this paper, we estimate the long-run impact of temporary 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 low-income 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). We find that exposure to these work incentives at first birth leads mothers to work sooner after childbirth, accrue greater work experience, and have higher earnings in the long-run.

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 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 around a first birth for each of these mothers. This gives us annual earnings for roughly ten times as many sample mothers as appear in the CPS in each March survey. Further, we use the snapshot of employment and fertility information in the CPS to provide suggestive evidence on hours of work, as well as on occupation choice and fertility, which may be potential mechanisms for our long run effects.

We identify the impact of work incentives after a first birth by leveraging variation in the timing of a birth and in eligibility for the credit in a triple-difference model. This strategy consists of two sets of comparisons. First, we compare the change in labor outcomes post-childbirth of never-married mothers who were exposed to the expanded work incentives at first birth ("early-exposed") to the change among never-married mothers who were exposed 3 to 6 years after a first birth ("late-exposed").[3] This difference-in-difference comparison captures variation in work

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

[2]See Blau and Kahn (2017) for a review of existing work on the role of experience in women's wages.

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

incentives across cohorts of mothers, but may be susceptible to time-varying confounds. Thus, to isolate the impact of work incentives, our primary specification compares this difference-in-difference for never-married mothers to the difference-in-difference for married mothers, who are less likely to be eligible for these incentives. This allows us to rule out time-varying confounds that are common to all mothers, such as the booming economy, changes in national policies, or shifting norms around maternal work.

We find that the employment of early-exposed mothers is higher up to ten years after a first birth (the "medium run"), but these differences disappear for the following ten years (the "long run"). Similarly, initial impacts on hours of work fade in the long run. Hence, the temporary difference in work incentives generates a temporary difference in employment. The additional years in the labor market lead early-exposed mothers to have between 0.62 years and 0.91 years of additional full-time, full-year experience (depending on whether we incorporate impacts on hours of work).

Despite the convergence in employment, we find that early-exposed mothers earn $1,393 ($2016) more on average in the long-run. This is 4.2% higher than the average earnings of late-exposed mothers who are employed, or 6% higher than all late-exposed mothers. These effects are entirely explained by improved earnings among wage and salary workers, which, combined with the null effects on labor supply, provides strong evidence that they are due to higher *wages*. In total, during the twenty years following 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.

These results suggest 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 between 4.6 and 6.8 percent. As we discuss below, 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 employment makes this less likely.

To contextualize our estimates in the broader policy space, we assemble a database of causal maternal employment responses to four major categories of policies: child care (provision or subsidies); paid family leave; welfare reforms; and the EITC. The database includes treatment effects on employment, earnings, experience, and imputed experience (which we construct) for 68 papers. All of the studies involve effects on experience smaller than ours, with 90% including a substantially smaller effect on experience of less than 0.2 years. With this in mind, we study how our return to experience compares to the average return, and broadly whether the return varies with the size of effect on experience. We find that the average return is similar across policies, and ranges from 6% to 20%. This is broadly consistent with our estimated return, particularly given the imprecision in past estimates. However, taking the point estimates of average returns at face value could suggest that our 6.8% estimate is a lower bound on the return, that returns are a concave function of the change in experience, or that other estimates incorporate effects of other mechanisms.

We present multiple pieces of additional evidence to address potential threats to the interpretation of our findings. We address possible concerns about comparisons of never-married to married mothers by using lower-earning groups of childless women or married mothers as alternative comparisons, and find the same results. Our conclusions about returns to experience are also similar if we exploit variation in exposure (and thus experience) within early-exposed mothers. We also rule out potential bias from across-year comparisons by presenting transparent graphs of *within-year* differences in the earnings of early- and late-exposed mothers. Finally, we find no evidence of bias from selective marriage or mismeasurement of marital status in the CPS.[4]

Our paper is at the center of three active literatures. First, we contribute to work on the long-run effect of temporary work incentives after childbirth. The most relevant estimates on this topic come from paid leave extensions,[5,6] which have found inconsistent, and often small effects of increasing mothers' time away from work.[7] 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. Additionally, the effects of paid leave reforms are more relevant for mothers who return to work within one year, which leaves out 40% of mothers (Laughlin, 2011).

Our study has several unique features relative to this body of work. First, we estimate the impact of a temporary work *incentive*, while work on paid leave policies identifies the effect of a work disincentive. Second, we leverage variation from substantial reductions in non-employment

[4]Note that the exact incentive that causes mothers to work sooner is not critical for our interpretation of our focal later-life effects. In particular, we interpret the long-run effects as a by-product of having worked sooner after childbirth. For this to be valid, we only need exogenous variation in the timing of work after childbirth, which could in principle include responses to other policies in addition to the EITC.

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

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

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

beyond the first year after a first childbirth. Our impact on experience accrues over the first nine years after birth, and is at least seven times as large as the median effect of other policies. Third, 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), 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 up to 7 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. While other estimates of returns exist for this population, this is far from a settled question. The closest benchmarks provide a wide range of estimated returns. These 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 9–12% 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 -3% 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). Relative to these papers, we leverage a significantly larger change in experience while not being subject to these identification concerns. We also show that our conclusion of positive returns is consistent with the broad picture generated by imputing returns from closely related policies that induce smaller changes in experience.

Finally, we show that U.S. social safety net programs influence the long-term earnings trajectory of adult recipients. This complements the substantial body of work that has shown that public aid affects adult recipients' short-run employment or children's long-run outcomes.[8] We find that safety net programs can have a lasting impact on adults' earnings by incentivizing changes in work experience, and that these long-run effects can play an important role in offsetting early program costs.[9]

[8]For short-run impacts of the safety net on adult employment, see e.g., Nichols and Rothstein (2015), for the EITC; see Blank (2002) for welfare reform; see Baicker et al. (2014), for Medicaid. For the long-run impacts of childhood eligibility for the EITC, see Bastian and Michelmore (2018); for Food Stamps, see Hoynes et al. (2016) and Bailey et al. (2020); for Medicaid, see Goodman-Bacon (2021) and Brown et al. (2019); and for Head Start, see Bailey et al. (2021).

[9]In doing so we substantially improve upon the long-run EITC effects in Neumark and Shirley (2020), which rely on a much smaller sample and are imprecisely estimated.

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). EITC benefits vary non-linearly with the number of qualifying children and earnings in a household (e.g., see Panel (a) of Appendix Figure A.1 for the 1993 to 1995 one-child schedules). 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.

The largest EITC expansion occurred in 1993, and is the focus of our analysis. 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.[10] These additional benefits are substantial, representing 8% income growth for the lowest-income households, or the equivalent of an additional month's wages (see Panel (b) of Appendix Figure A.1, which scales the change in benefits by household earnings across the income distribution). On the margin, this is expected to encourage more low-income mothers to work. In contrast, moderate-to-high income households experienced a much smaller, 0 to 2% growth in benefits.

1.1 Variation in Work Incentives for 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 not working for a few 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.[11] Because EITC benefits are higher for two-child families, we compute average benefits under two different assumptions about fertility: that all mothers have only one child or that all mothers have a second child that is born two to four years after the first, with uniform probability (such that the average spacing is three years, as in our sample).[12] These provide roughly the lower and upper bound of the gap in benefits between these groups.

Panel (a) of Figure 1 shows that in both of these childbearing scenarios early-exposed mothers

[10] The minimum earnings to qualify for the maximum credit, in real terms, was initially set as $12,550 in 1994; but was reduced to $9,701 the following year, making the more generous credit available to a larger number of households.

[11] We omit 1992 first-births in order to augment the difference in the benefits of early- and late-exposed mothers. For results using continuous bins of cohorts, see, e.g., Appendix Figure A.15.

[12] Early exposure does not change birth spacing – see Section 6.

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.[13] 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, both figures show that there is only a meaningful gap between early- and late-exposed mothers during 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.

Notably, this temporary variation in work incentives is similar to other common work incentives for mothers (e.g., expansions of child care tax credits, provision of childcare, and changes in paid leave policies). Like the variation in incentives shown above, these policies are typically temporary in nature, targeted towards mothers with young children, and hold constant long-run incentives for work.[14] Thus, while we obtain variation from the EITC expansion, the results may be applicable for a broad set of policies.

1.2 Welfare Reform as an Additional Work Incentive

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 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 (2021). 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 (see Appendix Table A.5).[15] 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 provide evidence that our results are not

[13] 75% of this difference is generated by earlier exposure to the 1993 reform.

[14] For example, the provision of subsidized childcare for infants increases the short-run incentive to work (i.e., for the year after birth) for eligible mothers; but in the long run, eligible- and non-eligible mothers face the same incentives (e.g., the same schools and tax policy).

[15] As an additional test, we also show that short-run employment responses are heterogeneous across mothers in a manner consistent with EITC incentives – see Appendix C for details.

driven by other policies, our long-run results would remain valid even if our estimates incorporate responses to welfare policies.

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.

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 (subject to some matching limitations, as discussed below). 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.[16] If an individual has positive total earnings, we consider her to be employed during the year.

We use the CPS survey responses to obtain demographics for our sample and as a secondary source of labor market outcomes and program participation. 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 a mother's marital status, which we use to assign her treatment; as well as her 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 evidence that this is not empirically relevant for our results in Section 3.1, after we introduce our empirical strategy.

The CPS 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). These outcomes allow us to explore intensive margin employment responses, which is not possible with administrative data.

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

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 4.1).

We supplement CPS demographics with 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, as well as birth order within children.[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. The one exception to this is the 2001 CPS, which we drop for having a particularly low match rate. For details on the matching procedure, match rates, and the precision gained from using administrative earnings records, see Appendix B.

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 interview; (ii) had a first birth at age 19 or older, which reduces the role of high school attendance or 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.

We use never-married mothers as a "high-impact" sample, who are likely to be eligible for expanded work incentives. To validate this choice, we use the three years of pre-birth household earnings to predict EITC eligibility after a first birth. We define a mother as EITC-eligible if her total family earnings pre-childbirth falls within the EITC-qualifying region for households with one child. We find that 97% of never-married mothers are EITC-eligible under this definition. 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. 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.

[17] In the few cases where the implied age from the NUMIDENT differs by more than 5 years from the age in the CPS, we instead use the CPS age - year - 1.

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). See Appendix Table A.1 for summary statistics.

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 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, 2021), as well as additional information from the tables in Gallagher et al. (1998).[18]

Supplemental data Because we are not able to observe changes in marital status in the CPS, we use the complete marital histories in the Survey of Income and Program Participation (SIPP) to examine whether early exposure alters marriage decisions (see Section 3.1). Our sample consists of SIPP mothers who gave birth in the same years as our core sample.

3 Estimation Strategy

Our primary empirical strategy uses a triple-difference model (DDD) to identify the causal effect of early exposure to work incentives. We first estimate a dynamic DDD using an event-study model:

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

$Y_{imb\tau}$ is an outcome for mother i with marital status m, whose first child is born in in year b, and is observed τ years relative to her first birth. Early exposure to work incentives is captured by the interaction between $EarlyExposed_b$, an indicator for having a first birth between 1993–1996, and NM_m, an indicator for being a never-married woman. Thus, the coefficients $\beta_{k,DDD}$ trace out the impact of early exposure over time, which is identified by comparing the difference between the gap in outcomes between early- and late-exposed never-married mothers and early- and late-exposed married mothers in each τ. We omit $\tau = -1$, such that these coefficients are estimated relative

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

to the difference in outcomes in the year before childbirth. 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 married mothers as an additional comparison allows us to control for year- and child-age specific shocks to labor market outcomes that are not due to the timing of exposure to expanded work incentives ($\theta_\tau \cdot \lambda_b$). These could include, for example, changes in federal policies protecting mothers' jobs after childbirth, tax policy, or the availability of new technology for infant care.

As additional controls, we include vectors of individual characteristics $X_{is\tau}$, and state-level policy covariates, $P_{s\tau}$. $X_{is\tau}$ includes fixed effects for mother's year of birth, age, state of residence, 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 across these groups. $P_{s\tau}$ includes 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. We allow the effect of each of these to vary by marital status.

To summarize the impact of early exposure, we replace the summation term in Equation 1 with interactions between "$EarlyExposed \cdot NM$" and indicators for each of our three post-childbirth 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 interpret this as the intent-to-treat impact of exposure to EITC incentives at first birth.

To increase transparency into the DDD estimates, we also show results from difference-in-difference (DD) event study models for never-married and married mothers:

$$Y_{ib\tau} = \alpha + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}(\tau = k) \cdot EarlyExposed_b + \theta_\tau + \chi_b + \gamma X_{is\tau} + \delta P_{s\tau} + \epsilon_{ib\tau} \tag{2}$$

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.

3.1 Identification Assumptions and Testable Implications

Our identification relies on the assumption that the difference in outcomes between early- and late-exposed never-married mothers, relative to the difference for married mothers, is uncorrelated with other predictors of mothers' outcomes. It is thus crucial to address two primary threats to identification. First, early-exposed never-married mothers may have been on a different labor market trajectory relative to late-exposed never-married mothers and married mothers even in the absence of the reform. Second, the outcomes of married mothers may not be an appropriate counterfactual for the outcomes of never-married mothers. We discuss the plausibility of each of these threats.

Common trends We test for 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 across marital status. Second, we study whether the outcomes of never-married mothers were improving relative to married mothers *prior* to the reform. Both of these tests pass easily in Section 4 and Appendix C.1, respectively, suggesting that the gap between married and never-married mothers' outcomes was not diverging prior to "treatment."

Comparability of married and never-married mothers We provide four empirical facts in favor of using married mothers as a comparison group. First, Appendix Figure A.2 shows that prior to the EITC reform, married and never-married women exhibited very similar employment responses to childbirth (a very large and salient shock).[19] This includes a nearly-identical "child penalty."[20] Moreover, these employment patterns continue to track closely even as children get older (see Section 5).

Second, prior work documents that never-married and married women have similar labor supply elasticities (Blau and Kahn, 2007; Heim, 2007; Bishop et al., 2009). This suggests that married and unmarried mothers are expected to exhibit similar responses to changes in economic opportunities.

Third, never-married and married mothers experienced similar changes in observable characteristics between early- and late-exposed mothers. Appendix Figure A.3 shows that the change in demographics for never-married mothers was the same or smaller than for married mothers along the following dimensions: pre-birth employment, completed education, 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- and late-exposed mothers (based solely on observable characteristics).[21]

Fourth, as we have discussed, married mothers' work incentives were not significantly changed by the reform due to their higher average earnings. Consistent with this, we find little effect of early exposure on the labor supply of married mothers.

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). 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.

[19]Specifically, we focus on mothers giving birth between 1986 and 1991, and estimate a version of Equation 2 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.

[20]We 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, and focus on employment immediately after birth.

[21]As 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. This reinforces the importance of using the DDD to difference out fixed gaps by marital status.

Selection into being single and measurement error Aside from these identification assumptions, our reliance on marital status at the time of CPS interview 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 who are observed in CPS surveys further from a first birth, and find no impact on the size of our estimates (see Section 5).

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 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.

As a first test for selective marriage, we study whether there is a difference in the marriage rates of early- and late-exposed mothers. Because we do not observe marital status at birth in the CPS, we instead use the SIPP to calculate the share of early- and late-exposed mothers that remain single in each year after childbirth. Contrary to the concerns about selective marriage, Appendix Figure A.4 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.[22]

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.2 shows that the coefficients on this interaction are always insignificant and typically negative, implying that, if anything, early-exposed mothers are negatively selected due to attrition. Third, we show that our results are 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).

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.) Second, we

[22] The sign of our effect suggests that early-exposed mothers may 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.

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 by restricting our sample to women surveyed at younger ages and find similar results.

4 Main Results

Employment We begin with the effects of early-exposure on the likelihood of working. Because the gap in incentives between early- and late-exposed mothers is large in the first five years after birth but closes over time (Figure 1), we expect that the difference in employment should attenuate in the medium- and long-run. However, it is not clear that employment outcomes should fully converge, nor do so within the time period that incentives converge. 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 for mothers with older children, or if there are other frictions that would similarly delay responses.

Panel (a) of Figure 2 presents regression-adjusted means of the employment rate of early- and late-exposed never-married women around a first birth. Leading up to childbirth, 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 for the first five years after childbirth. Between years 5 and 9 the employment rates of the two groups converge, and remain at similar levels ten to nineteen years post-birth.

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 5 p.p. higher employment, which grows to 8 p.p. in the following few years. The fact that the effect on early-exposed mothers' employment ramps up quickly in the first two years suggests that the response to work incentives was relatively immediate. The difference in employment between the two groups closes in the medium run, and hovers slightly below zero thereafter.

The DDD event study shown in Panel (c) is very similar 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 the same in the first five years post-childbirth implies that there is no effect of early exposure on married mothers, consistent with our expectations. Estimated effects on employment shrink to zero over the medium run, and become slightly positive thereafter.[23] This indicates that early exposure does

[23] We suspect that the modest long-run fluctuations, and the slight difference in results between the DD and

not have a lasting impact on employment.

Column 1 of Table 1 presents our DDD estimates for employment. Never-married mothers' post-birth employment increases by 5.5 p.p. ($p < 0.01$) in the short-run; which represents a 8.7 percent increase relative to late-exposed mothers' employment and a 27% recovery relative to the drop in employment in the year after birth.[24] In the medium run, early-exposed women have 5.5 p.p. higher employment rate per year. This difference fades to an insignificant 1 p.p. in the long-run.

Work experience 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 impacts on employment in Figure 2, and then dividing by the number of years in each period to get the average effect.[25] Panel (a) of Figure 3 and column 2 of Table 1 show that early-exposed mothers have 0.46 years of additional experience in the medium-run, which becomes 0.62 additional years of experience in the long-run. The long-run estimate corresponds 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 4.1, 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.

Earnings Panel (b) of Figure 3 presents the DDD 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.[26,27] Hence, early-exposed mothers have long-lasting

DDD specifications, 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 these fluctuations (see Section 5). 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.

[24]The short-run impact on employment is smaller (3.7 p.p.) if we only analyze data up to 5 years after childbirth, which allows for the possibility that the covariates affect maternal employment differently in the period immediately after childbirth. This is more closely aligned with, and thus a better comparison for, prior work on the short-run effects of the EITC. In Appendix C, we provide additional evidence on the short-run responses to the reform.

[25]An 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 employment coefficients. In practice, the two strategies yield similar results.

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

[27]See Appendix Figure A.5 for separate event studies for early- and late-exposed mothers and for the DD.

earnings gains, which are not readily explained by differences in the rate of employment.

Table 1 shows that early-exposed mothers earn \$2,618 more per year in the medium run and \$1,393 more per year in the long run. The majority of this (87%) is due to increases in pay from employers (see Appendix Table A.3). Relative to the average annual earnings of late-exposed mothers, these 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, analyze log earnings, or use a Poisson model in Appendix Table A.3 (columns 1-6). The estimate for 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 or dropping the bottom 1% of log earnings to limit the influence of outliers also makes little difference (columns 7-8).

Examining the earnings distribution, we find that these earnings effects are concentrated at lower levels in the short- and medium run, and become more diffuse in the long run (see Appendix Figure A.6).[28] This suggests that the long-run impacts in earnings reflect impacts throughout the 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 order to translate this into impacts of early exposure on the *total income* of mothers, in Appendix Section C.5 we calculate expected changes in taxes, transfers from the EITC and other government programs, and child care costs. Taking these factors into account, we find that in total early-exposed mothers' net income increases by a substantial \$16,620 in present value terms, with 40% of this being earned during the long run.[29]

4.1 Survey Evidence on Hours of Work and Work Experience

For evidence on weekly hours of work and hours of work experience, we now turn to our sample's survey responses in the CPS. 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 a double-difference design, comparing early- and late-exposed never-married mothers' outcomes

[28] The figure shows estimates from regressions where the outcomes are indicators for having earnings above X, with X = 0, 2500, 5000,....80,000, i.e., 1-CDF (Duflo, 2001).

[29] If we further assume that these effects are solely a response to the EITC, we can calculate the fiscal impact of the EITC expansion, as given by the Marginal Value of Public Funds (MVPF). See Appendix C.5.

at each child age relative to married mothers.[30] 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 have only one observation per individual, and thus a smaller sample size, these results are more suggestive than our main results.

Table 2 presents the impacts of early exposure on (i) weekly hours of work; (ii) annual hours of work (weekly hours times weeks worked); (iii) cumulative hours of work experience; and (iv) equivalent years of full-time full-year experience. We obtain effects on cumulative hours of work by taking a running sum of the annual impacts on hours of work (similar to the effects on experience above). These outcomes are unconditional, and therefore capture both intensive and extensive margin effects.

Column 1 of Table 2 shows that in the short- and medium-run, early-exposed mothers work between 2 and 3 hours more work per week. This amounts to an additional 86 to 169 hours per year (column 2). In the long run, we find an insignificant and negligible effect on weekly or annual hours of work. Column 3 shows that in total early-exposed mothers accrue an (imprecisely estimated) additional 1,277 hours of work over the long run. This represents an additional 0.91 years of full-time full-year work experience (column 4), 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). This is a 0.3 year larger effect than in the administrative data, which suggests that intensive margin effects may contribute to greater experience (but to a lesser degree than extensive margin effects).

4.2 Translating Impacts on Earnings to Wages

Next, we consider whether our long-run effects on earnings reflect higher *hourly wages*. Because we do not observe wages, we instead examine the weight of the evidence for alternative explanations. The first alternative is that these effects are driven by changes in income from self-employment. However, earlier we showed that the magnitude of our effects is nearly the same if we examine wage earnings, which rules out this possibility. The second alternative is that our effects reflect changes in hours worked. Contrary to this, we find small and insignificant effects on employment, weekly and annual hours (as discussed earlier), as well as on indicators for part-time and full-time employment (Appendix Table A.4). Moreover, our effects on earnings are similar when we limit the sample to workers, which mechanically eliminates any extensive margin effects. Finally, even if we take seriously the 6.9 mean increase in CPS annual hours in Table 2, this would imply that wages would have to be \$201 per hour in order to generate earnings effects as large as ours. In actuality, mean wages are closer to \$20 per hour. This suggests that for a plausible range of hourly wages, our earnings effects are more likely explained by an increase in wages rather than a change in hours. In particular, our estimate of earnings gains among workers suggests that early-exposed

[30] We estimate the double-difference as:

$$\begin{aligned} Y_{imb\tau} =& \alpha + \beta_1 \cdot EarlyExposed_b \cdot 0\text{–}4_\tau \cdot NM_m + \beta_2 \cdot EarlyExposed_b \cdot 5\text{–}9_\tau \cdot NM_m + \beta_3 \cdot EarlyExposed_b \cdot 10\text{pl}_\tau \cdot NM_m \\ & + \theta_\tau \cdot \lambda_b + \theta_\tau \cdot \rho_m + \gamma_m X_{is\tau} + \delta_m P_{s\tau} + \epsilon_{ib\tau} \end{aligned} \tag{3}$$

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

mothers earn 4.2% higher *wages*.

5 Robustness

In this section, we address the threats to identification previewed in Section 3.1.

Childless and lower-income comparison groups First, we test the sensitivity of our earnings results to using as comparison lower-earning groups of childless women and married mothers. This addresses the potential concern that the earnings of all lower-wage women may have improved during the 1990s (e.g., from the booming economy), and more so than for married women. Specifically, we run our DDD specification using as comparisons childless women who (i) have at most some college; (ii) have at most a high school degree; or (iii) are single and lower-educated; or married mothers who have (iv) at most some college; (v) at most a high school degree; or (vi) were EITC-eligible pre-childbirth.

Our childless comparison groups consist of women that we observe between the ages of 37 to 42 without any children in the household. To assign a fake year of childbirth, $\hat{b}$, we follow Kleven, Landais and Søgaard (2019), and take a random draw from the distribution of b among never-married mothers who have the same year of birth and level of education as a given childless woman. We then assign "years since first birth" as the the current year minus $\hat{b}$, such that "pre-birth" and "post-birth" consist of the same sets of calendar years for all mothers that have the same "year of childbirth." 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.[31]

Figure 4 plots all of the long-run estimates against the average labor market outcomes (employment and earnings conditional on working) of the comparison group over the whole sample period. For reference, we include a vertical line with the average outcome of never-married mothers. The range of estimates spans from \$756 to \$1,613, and fit comfortably within the confidence interval of our main estimate (shown in red).[32] Further, there is no systematic relationship between the size of our estimate and the average employment or earnings across the comparisons. This provides strong evidence that our results are not driven by shocks to lower-earning or single women.

Transparent calendar-year event studies Second, a potential concern with our focus on years relative to birth is that it makes it difficult to examine possible confounds on an annual basis. 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 as well as for mothers that have a first

[31] Because of potential noise in our assignment of placebo births to childless women, we bootstrap our estimates and confidence intervals by running the assignment of placebo births 100 times and taking the mean and 95% confidence interval over the estimated effects.

[32] Event study figures in Appendix Figure A.7 also show similar patterns across the various comparisons.

birth in the surrounding years (i.e., 1986-87 and 1997-99). Similar to the main analysis, we omit the year prior to the earliest childbirth in each group.

Consistent with our main results, Appendix Figure A.8 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. For married mothers, we continue to find negligible impacts across early- and late-exposed mothers using this calendar-year design (see Appendix Figure A.9).[33]

Notably, for both married and never-married mothers that gave birth *pre-reform*, we find similar patterns of employment around birth. We highlight this by plotting these mothers together in Appendix Figure A.10. 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 Third, 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 show that our results are not sensitive to controlling more flexibly for economic conditions. This includes allowing the impact of unemployment rates and welfare reform to vary with the age of one's child (see Appendix Table A.5); and either adding controls for state-level unemployment rates that are specific to women or low-skilled individuals (calculated from the March CPS) or state-year fixed effects (see Appendix Figure A.11).

Additional specifications Fourth, we test the sensitivity of our results to more flexible controls for individual characteristics. Our results are unchanged when we use inverse propensity score weighting (see Appendix Table A.6); or allow the effect of mother's age to vary with the age she first gave birth, add individual fixed effects; or restrict the sample to mothers who are CPS heads of household (see Appendix Table A.7).

Alternative sample restrictions Fifth, we re-run our results using alternative sample restrictions to address potential concerns about measurement error. 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: Panel (a) of Appendix Figure A.12 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 (Panel (b) of Appendix Figure

[33] While 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.

A.12). Moreover, we also find similar effects when we successively drop mothers who were relatively older (39–49), and thus whose children may no longer have been living at home, at the time of CPS interview (Panel (c) of Appendix Figure A.12). 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.13. 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.

Greater work experience A leading explanation for early-exposed mothers' higher wages is increases in years of work experience.[34] 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 a first birth).

As a more direct test of this mechanism, we ask whether the mothers that experience higher earnings are the *same* mothers that were induced to work after a first birth. To avoid conditioning on post-birth experience (which is an outcome of early exposure), we run regressions where the outcomes are indicators for the four possible combinations of having "high" or "low" earnings crossed with having "high" or "low" experience. We define "high experience" as having worked during each of the first three years after a first birth ($1 \leq \tau \leq 3$) to capture short-run responses to post-birth work incentives. We define "high earnings" as having earnings in the top 25 percent of mothers in each year, which we find is the best binary proxy for the impact of early exposure on earnings. 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 5 presents long-run effects (and 95% confidence intervals) on indicators for these four outcomes: having "high earnings and high experience," "high earnings and low

[34] A related possible explanation is that our earnings effects reflect the impact of gaining experience during a good economy. We can not rule this out, but it seems less likely because we find similar effects on earnings for women that experienced weaker economic conditions post-childbirth. See Appendix Table A.8.

experience," "low earnings and high experience," and "low experience and low earnings."[35] 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 high experience," consistent with the idea that high experience does not correlate perfectly with high earnings.

The first bar of 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, as shown in the second bar. 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. 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.[36]

If experience were the only source of wage gains, the implied return to a year of additional work would be between 4.6 (4.2%/0.91) and 6.8 percent (4.2%/0.62), based on our estimate of the average impact on earnings conditional on working and our estimates of the increase in years of experience from the CPS and SSA data, respectively. As discussed in the introduction, this falls in the range of prior estimates for similar populations, although the size of the impact on experience and precision of our effects contrasts with other causal estimates. Nevertheless, the return to experience may also be higher in our setting (relative to, e.g., Card and Hyslop, 2005) because working after childbirth provides a costly signal to employers of one's commitment to work (Thomas, 2019; To, 2018). We provide further evidence of the comparability of our return to those estimated from other policies in Section 7.

Does the return to experience vary by how soon a mother begins working relative to childbirth? To examine this, we estimate separate long-run effects for mothers who had a first birth in 1988-89 (exposed at child age 4-5); 1990-91 (exposed at child age 2-3); 1992-93 (exposed at child age 0-1); 1994-95 (exposed at birth); and 1996 (exposed at birth) – relative to mothers who had a first birth in 1986-87 (exposed at child age 6-7).[37] Appendix Figure A.14 shows that the effect of work incentives on experience and earnings are both decreasing with child age of exposure, consistent with these effects being monotonic with the degree of exposure. Within this group of mothers, the impacts on earnings appear roughly proportional to the impact on experience, with no discrete jump in the effects on earnings. Moreover, consistent with constant returns to experience, Appendix Figure A.15 shows that the relationship between the impacts on earnings and experience is roughly linear, which suggests that returns to experience are roughly constant.[38] However, the estimates

[35] See Appendix Table A.9 for the corresponding estimates for this figure.

[36] See Appendix D for additional details on this calculation and the robustness of these results to using an alternative measure of "high experience".

[37] We do not include mothers with a first birth in 1997 or later because we do not have 19 years of post-childbirth outcomes for these mothers.

[38] Deviations from the best-fit line could suggest that there is a secondary mechanism operating for some cohorts

are imprecise, so we can not reject non-linear effects.

Higher return to experience Second, it is possible that early-exposed mothers obtain a higher return to experience by choosing different occupations. For instance, Adda et al. (2017) find that the returns to experience are higher in "abstract" occupations that have more analytic tasks, and Deming (2017) shows that the returns to social skills increased over our period of study. We find some imprecise support for this channel when we look at specific CPS occupations (see Appendix Table A.10). 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.[39,40]

Other channels Third, early-exposed mothers may avoid skill depreciation by reducing the 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.

Fourth, early-exposed mothers make different fertility choices, in terms of number of children or birth spacing (measured by children in the household at the time of the CPS survey). 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.12. 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).[41]

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

(e.g., mothers with a first birth in 1990-1991) or could reflect noise in the data.

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

[40] We also find imprecise effects on the task content of mothers' occupations. See Appendix Table A.11.

[41] This is consistent with the small effect of the EITC on fertility shown in other studies (Baughman and Dickert-Conlin, 2003; Hoynes et al., 2015)

earnings gains.

7 Returns to Experience Across Policies

Does our estimated return to experience generalize to other policies for a similar population? We have provided suggestive evidence that the return to experience is stable within our sample, but this need not hold across a broader set of policies. One could be concerned, for example, that the returns to experience may vary with the size or sign of the effect on experience, such that our effects on earnings would not necessarily scale across policies, where the estimated effects on experience are much smaller. In this section, we study how the return varies across policies, and in particular how our estimate compares to average return from policies that leverage a smaller change in experience.

To assess this question, we assemble a database of the universe of causal treatment effects on maternal labor market outcomes from four major categories of policies: child care (provision or subsidies); paid family leave; welfare reforms; and the EITC. We include papers that use quasi-experimental or experimental methods, and are either published in a top Economics field or general interest journal or are recent working papers. In total, the database comprises 207, 158, and 49 treatment effects on employment; earnings; and experience, respectively, taken from 68 papers. To supplement these estimates, we also calculate 204 *imputed* treatment effects on experience by taking a sum of effects on employment over the duration of the policy. This allows us to significantly expand the number of policies that we include in our analysis of returns. See Appendix E for further detail about the construction of the database.

We provide transparent estimates of the average return across policies by plotting estimated treatment effects on earnings (in percent terms) against estimated treatment effects on years of experience. The slope of this graph can be interpreted as the average return per year of experience under the assumption that the treatment effect on earnings is due to the treatment effect on experience.

Figure 6 shows the across-policy return to experience for studies (i) that report treatment effects on experience (panel a; N=33); and (ii) for which we impute treatment effects on experience (panel b; N=89). Across both panels, the estimated effects on experience are clustered between -0.2 and 0.2 years of experience. Our study, shown in red diamond, is an important exception to this, appearing in the upper-right corner of the graph. The distance between this point and the mass of other estimates highlights the unique size of our effect on experience.[42]

Importantly, both panels show a clear positive relationship between treatment effects on earnings and experience. The slope of the graph in Panel (a) suggests that the return to experience across policies is roughly 7%. This implied return is very similar if we drop estimates with a significant impact on employment (as shown in the figure) or weight by the inverse-variance of the return

[42] In Appendix E, we show that our larger effect on experience reflects a combination of a moderately larger impact on annual employment (75^{th} percentile) and a substantially longer duration of our treatment (90^{th} percentile).

to experience (see Appendix Figure A.16). The slope in Panel (b), which includes the imputed experience effects, is moderately higher (between 0.14 and 0.20), but the confidence intervals across the two panels are largely overlapping.[43] The patterns are also broadly similar across policies (e.g., disincentives, like paid leave, and incentives, like child care subsidies) as well as within policies (e.g., across paid leave reforms).

These results suggest that the return to experience is largely comparable across settings and policies. The more-conservative slopes of the graphs imply an average return to experience that is similar to our 6.8% estimated return, particularly taking into account the uncertainty in past estimates. However, taking the point estimates at face value, the slightly larger across-policy returns could indicate that our estimated return is a lower bound on the return, that returns are a concave function of the change in experience, or that other estimates incorporate the effects of other mechanisms.

8 Conclusion

This paper provides new evidence on the impact of temporary post-childbirth work incentives on mothers' long-run career trajectories. We find that mothers who are exposed early to work incentives (at birth rather than 3–6 years after birth) have in the long run at least 0.6 years of additional work experience and 4.2 percent higher 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.* We show that higher wages are largely explained by increases in experience, and that the implied return to a year of experience ranges 4.6 to 6.8 percent. 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., child care provision and tax incentives). We leave it to future work to quantify impacts on other dimensions of maternal and child welfare.

[43] The confidence intervals are underestimates of the degree of the uncertainty in the estimated slope because they do not take into account the estimation error for the treatment effects (e.g., 42% of the effects on earnings in panel (b) are insignificant).

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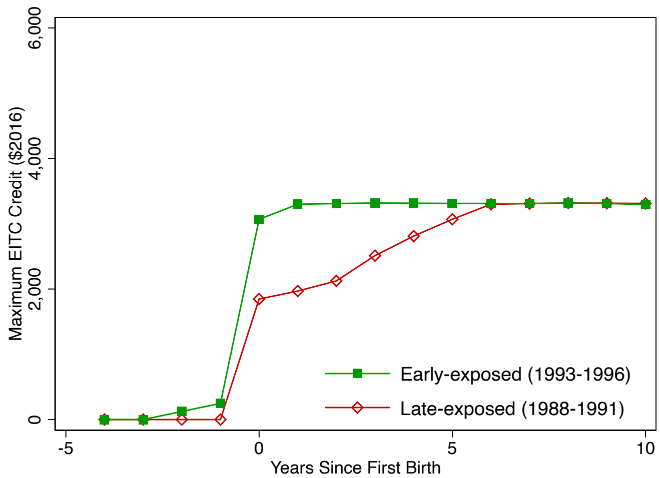
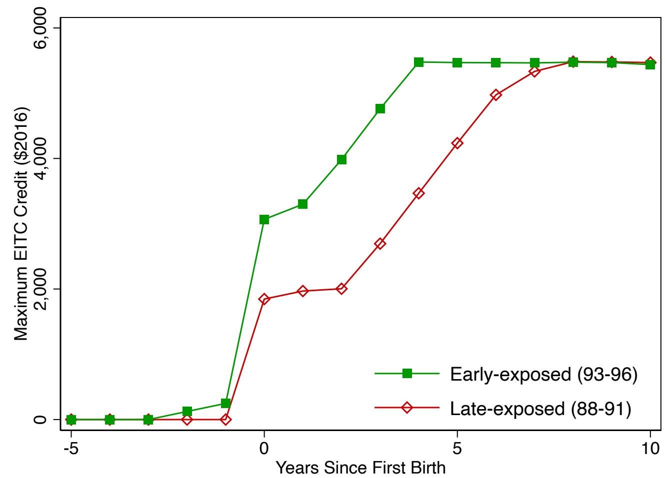
Figure 1: Illustration of the Gap in the Maximum EITC Between Early- and Late-Exposed Mothers

(a) Average Max. EITC

(i) 1 child only

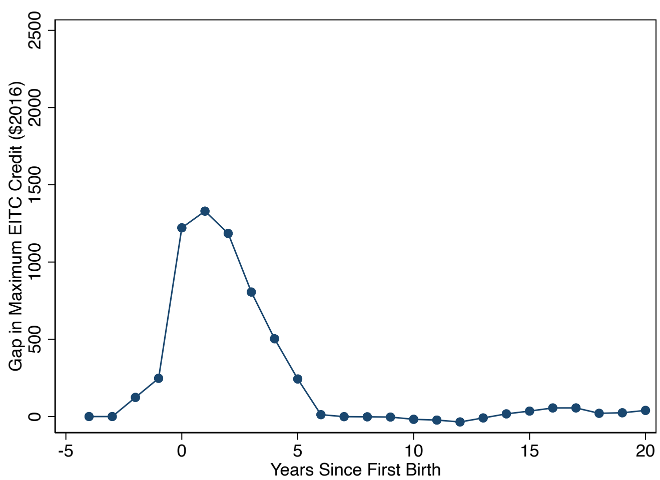


(ii) 2 children, 2-4 year spacing

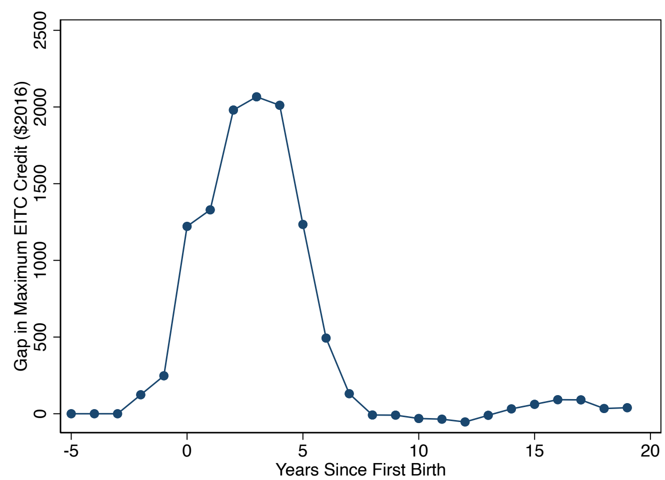


(b) Difference Between Early- and Late-Exposed Max. EITC

(i) 1 child only



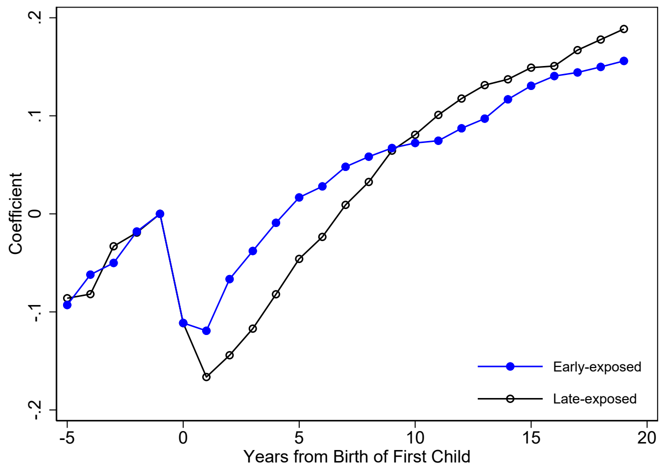
(ii) 2 children, 2-4 year spacing



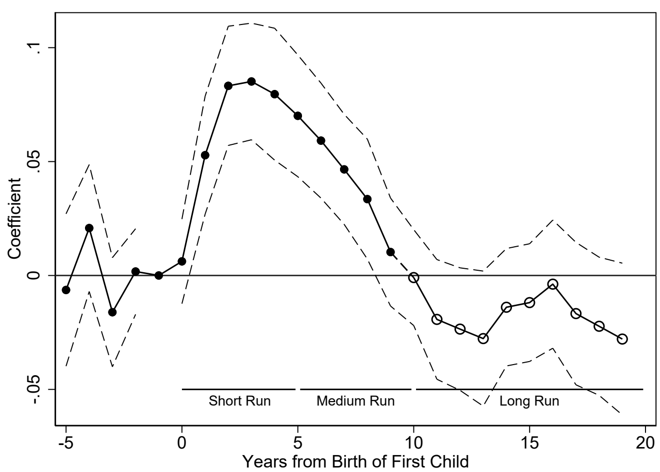
Notes: This figure shows the average maximum EITC benefits in each year since first birth for mothers who are exposed to the 1993 EITC reform early (in the year of first childbirth) or late (3–6 years after childbirth) (panel a), and the difference between these (panel b). Within each panel, subfigure (i) assumes that mothers only have 1 child, and subfigure (ii) assumes that mothers have two children spaced with a uniform probability between years 2 and 4 (such that the average spacing is three years). *Data:* Nominal EITC benefits are 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 Employment

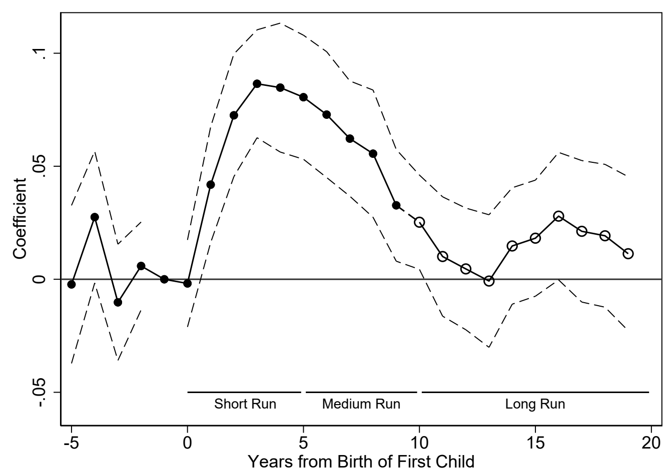
(a) Early- and Late-Exposed Never-Married Mothers



(b) Difference-in-Difference

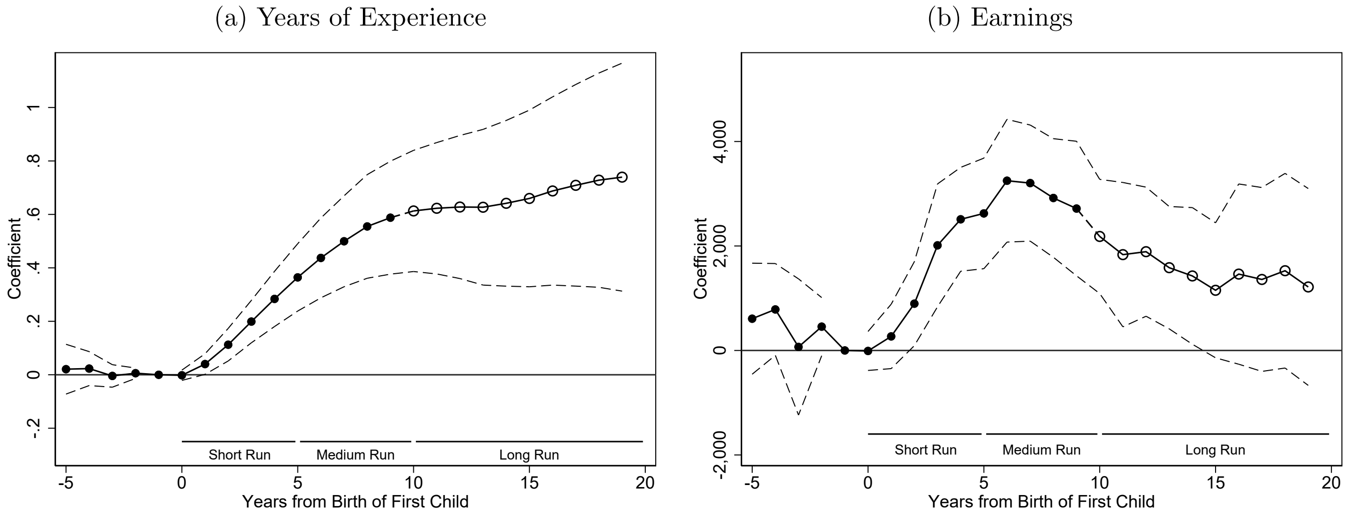


(c) Triple-Difference (Main)



Notes: These figures present the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) in each year from birth on employment, along with 95% confidence intervals. Panel (a) plots event studies of employment around childbirth estimated separately for "early-exposed" and "late-exposed" never-married mothers. Panel (b) shows DD event study estimates. Panel (c) shows DDD event study estimates. 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 19^{th} year after a first birth.

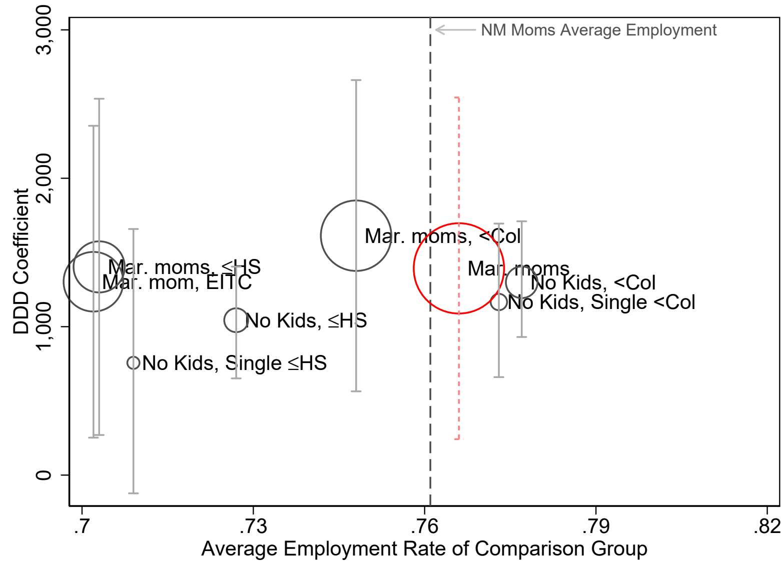
Figure 3: Effect of Early Work Incentives on Experience and Earnings



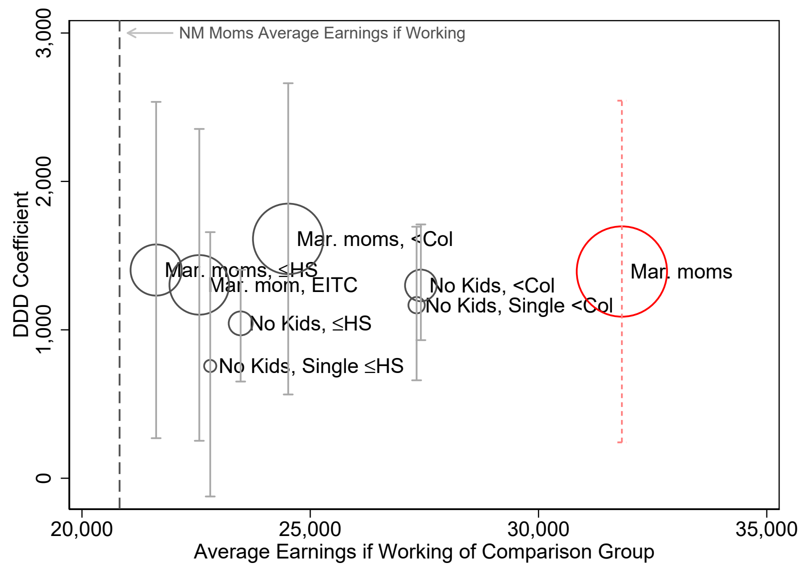
Notes: This figure presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) in each year from birth on experience (panel a) and earnings (panel b), along with 95% confidence intervals. Estimates come from the dynamic DDD specification. 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 19^{th} year after a first birth.

Figure 4: Long-Run Effects on Earnings Across Comparison Groups

(a) Effects by Average Employment Rate of Comparison Group



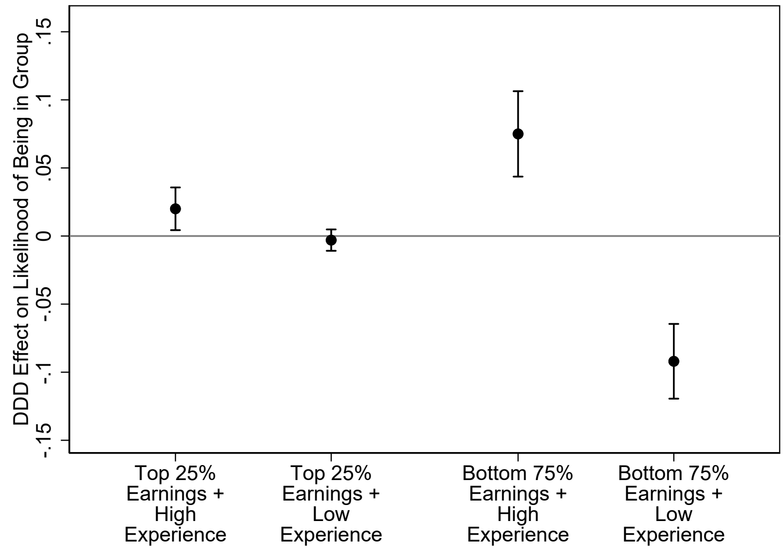
(b) Effects by Average Earnings if Working of Comparison Group



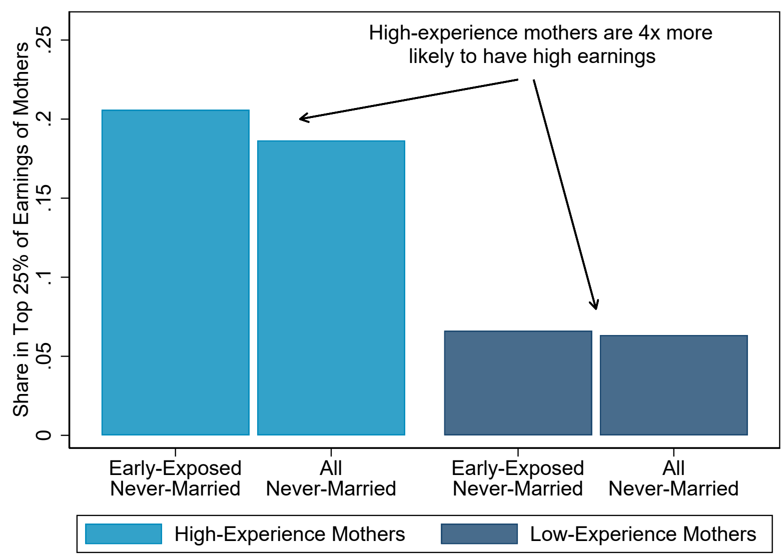
Notes: These figures present the long-run effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on earnings, along with 95% confidence intervals, for each DDD comparison group (indicated by the marker label). Specifically, each marker shows the coefficient and 95% confidence intervals from the interaction "10+ Yrs From Birth * EarlyExp * NM." The size of the marker is proportional to the number of individuals in the comparison group. The red marker shows our main 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. We bootstrap the childless estimates and confidence intervals by running the assignment of placebo births 100 times and taking the mean and 95% confidence interval over the estimated effects. See the notes of Figure 2 for information on standard errors (for the married comparisons), 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 5: 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



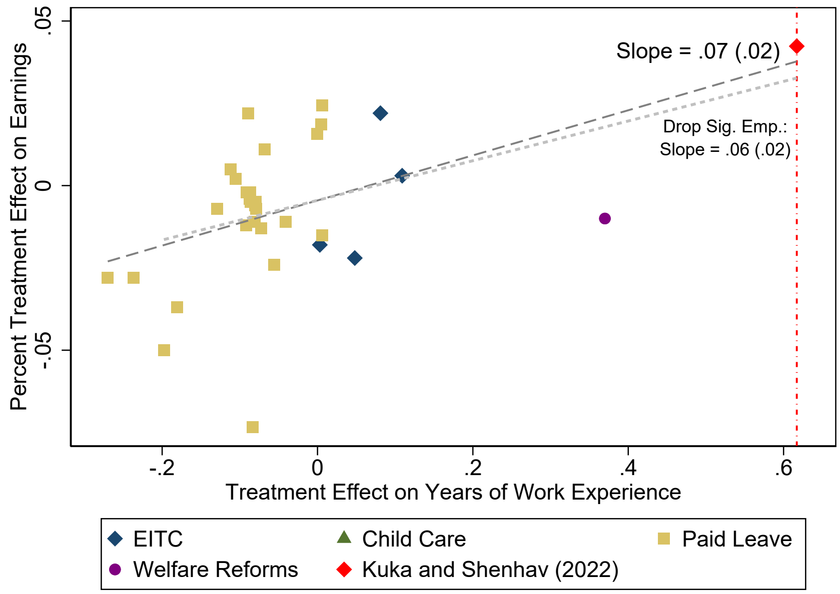
(b) Proportion of High-Earners among High- or Low-Experience Mothers



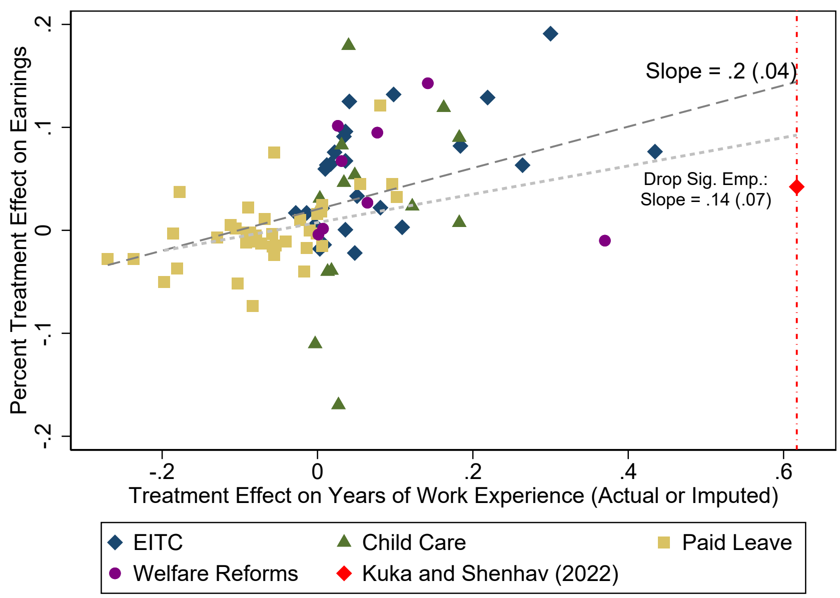
Notes: Panel (a) presents the long-run effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth), along with 95% confidence intervals, on the following four joint outcomes: having (i) "high earnings and high experience"; (ii) "high earnings and low experience"; (iii) "low earnings and high experience"; and (iv) "low earnings and low experience." Specifically, each marker shows the coefficient on the interaction "10+ Yrs From Birth * EarlyExp * NM." "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, all high-experience 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.9 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 6: Returns to Experience Across Policies

(a) Reported Treatment Effects on Experience



(b) Adding Imputed Treatment Effects on Experience



Notes: This figure presents percent treatment effects on earnings (y-axis) and treatment effects on years of work experience (x-axis) from studies in the policy database described in Section 7. Panel (a) includes studies that report treatment effects on years of work experience, while Panel (b) additionally includes studies for which we impute treatment effects on years of work experience. Imputed experience effects are calculated in two steps. First, we obtain the treatment effect on experience for each period of treatment as the running sum of the treatment effect on employment. Second, we obtain the average treatment effect on experience as the total of the treatment effects on experience in each period divided by the duration of treatment. The beige square markers show estimated effects of paid leave; the blue diamond markers show estimated effects of the EITC; the green triangles show estimated effects of child care policies; the purple circles show estimated effects of welfare reforms; and the red diamond marker and vertical dashed red line show our estimated long-run effects using the SSA administrative data. The dark grey dashed line shows the best fit line using all of the effects, and the light grey dashed line shows the best fit line when we drop studies where there is a contemporaneous impact on employment. Panel a includes 32 estimates from 7 papers. Panel B includes 89 estimates from 28 papers.

Table 1: Effect of Early Work Incentives on Labor Market Outcomes

	Employed	Years of Experience	Earnings
	(1)	(2)	(3)
0-4 Yrs From Birth * EarlyExp * NM	0.046***	0.118***	762.6**
	(0.009)	(0.025)	(333.4)
5-9 Yrs From Birth * EarlyExp * NM	0.055***	0.464***	2617.6***
	(0.010)	(0.070)	(526.6)
10+ Yrs From Birth * EarlyExp * NM	0.010	0.617***	1392.7**
	(0.011)	(0.129)	(587.3)
Mean Y	0.765	0.765	23612.672
Observations	2714475	2714475	2714475

Notes: This table shows the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the employment (column 1), years of experience (column 2), and annual earnings ($2016, column 3) of mothers, 0-4, 5-9 and 10+ years from first birth. 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. We 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 19^{th} year after a first birth.

Table 2: Effect of Early Work Incentives on Hours of Work – CPS Responses

	Hours LW (1)	Annual Hrs (Hrs*Wks) (2)	Cum. Hours (3)	Yrs. FT FY Exp. (4)
0-4 Yrs from Birth * EarlyExp * NM	2.161	86.432	302.772	0.216
	(1.748)	(85.828)	(229.978)	
5-9 Yrs from Birth * EarlyExp * NM	3.324***	169.060**	1157.171**	0.827
	(1.239)	(63.348)	(476.681)	
10+ Yrs from Birth * EarlyExp * NM	0.277	6.897	1277.615	0.913
	(1.545)	(76.599)	(943.107)	
Mean Y	24.44	1172.12	–	
Individuals	94414	94414	94414	

Notes: This table shows the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on hours worked last week (column 1); annual hours of work, which is calculated as weeks last year times hours last week (column 2); cumulative hours, which is the average of the running sum of annual effects on annual hours (column 3); and years of full-time full-year experience, which is the effect of cumulative hours divided by 1400 hours. We estimate this using the double-difference model in Equation 3. 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. We 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 19^{th} year after a first birth.

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

Elira Kuka and Na'ama Shenhav

June, 2022

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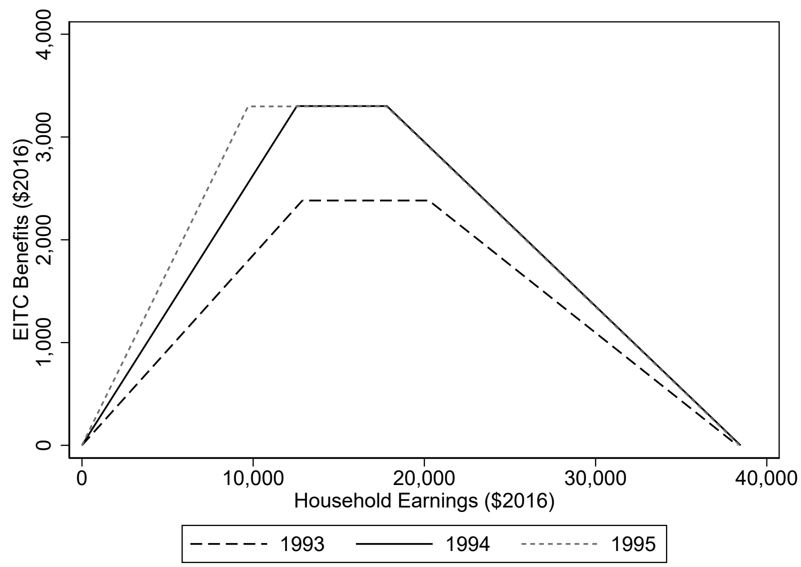
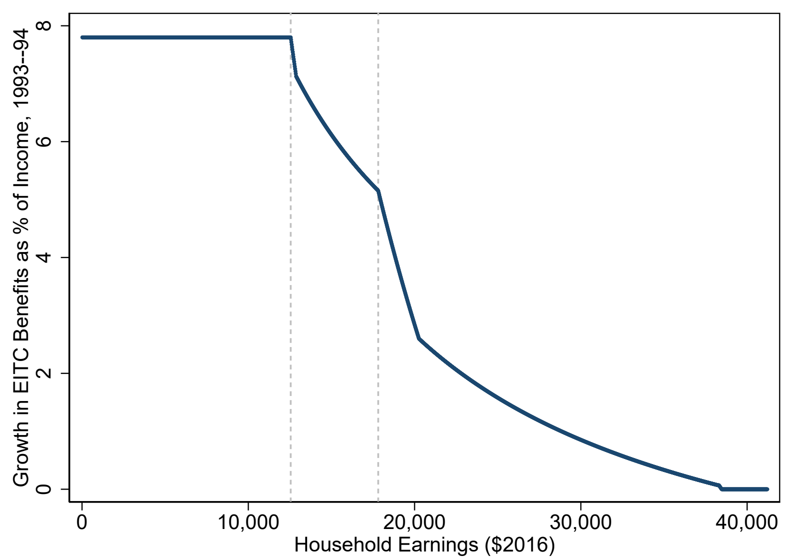
A Supplemental Figures and Tables

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

(a) 1993, 1994 and 1995 Benefit Levels

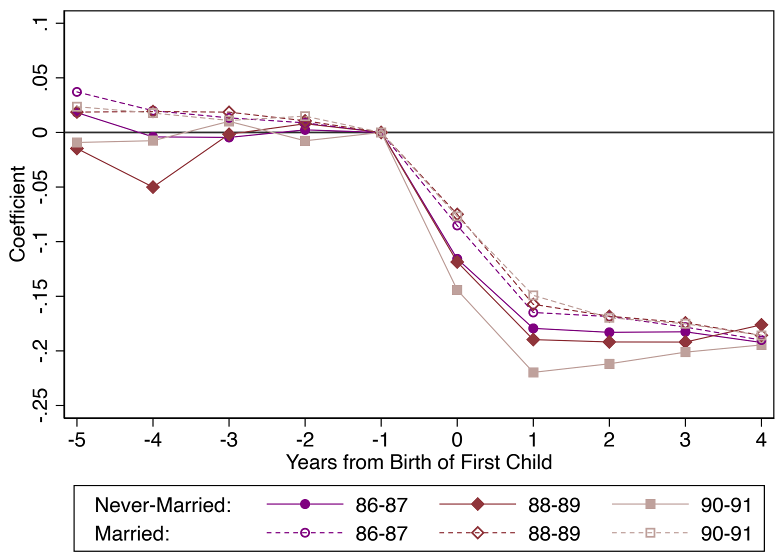


(b) 1993-1994 Change in Benefits as % of Earnings



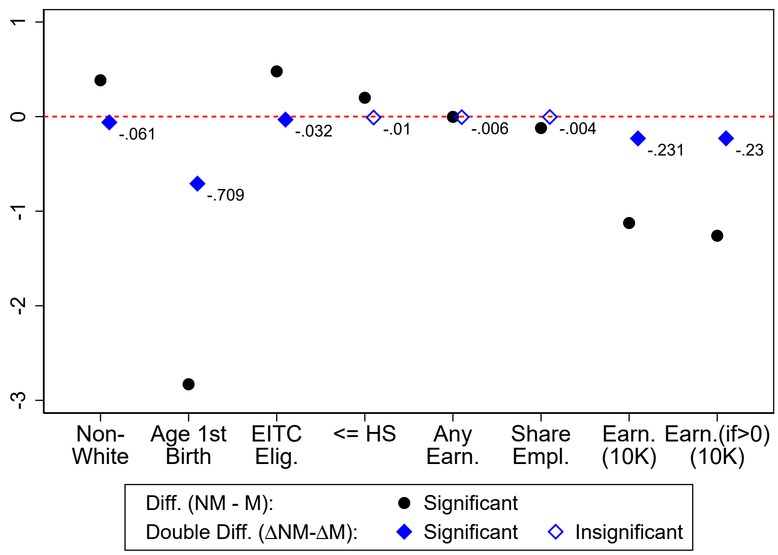
Notes: Panel (a) shows EITC benefits by the level of household earnings for one-child households in 1993, 1994, and 1995. Panel (b) shows the difference between 1994 and 1993 benefits as a share of household income. *Data:* Nominal EITC benefits are from the Tax Policy Center (https://www.taxpolicycenter.org/statistics/eitc-parameters),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

Figure A.2: Employment Relative to Year Prior to Childbirth
for Mothers Giving Birth Prior to the EITC Reform



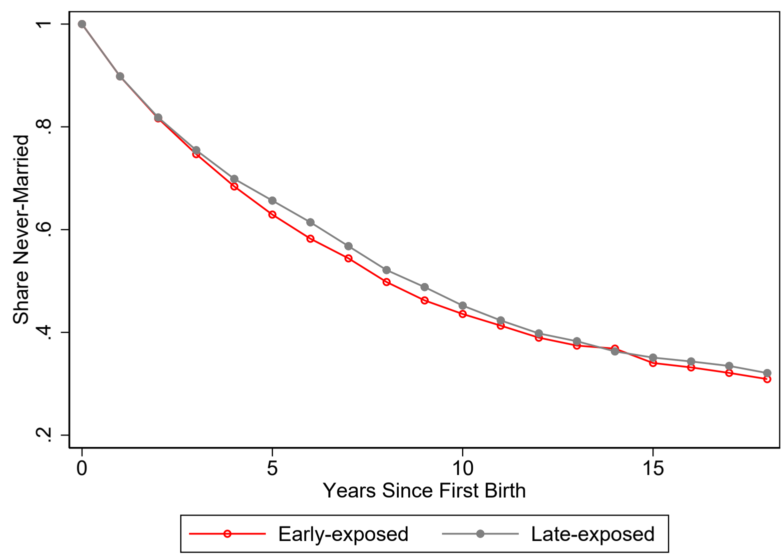
Notes: This figure presents event studies of employment around birth, along with 95% confidence intervals, for never-married and married women who had a first birth prior to the 1993 EITC reform. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Figure A.3: Difference and Difference-in-Difference in Observables Across Married and Never-Married Mothers



Notes: This figure presents single differences between never-married and married mothers' average characteristics (shown in the circular markers) and double-differences between the gap in early- and late-exposed mothers' characteristics across never-married and married mothers (shown in the diamond markers). EITC eligibility is equal to one if a woman's total family earnings pre-childbirth falls within the EITC-qualifying region for households with one child. "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. See the notes of Figure 2 for information on data and sample construction.

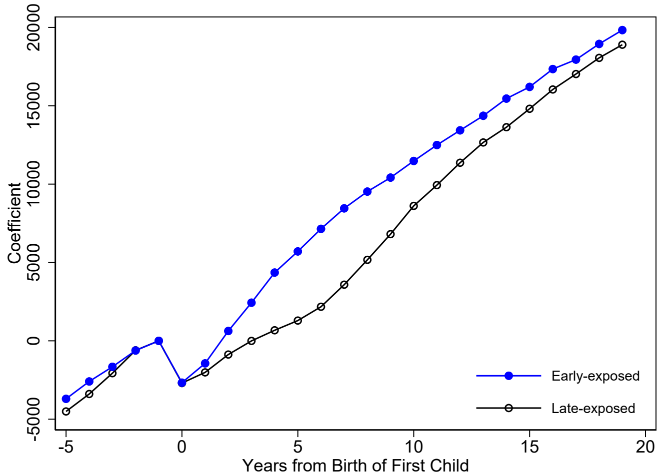
Figure A.4: Share of Mothers Remaining Never-Married in Each Year Since First Birth (SIPP)



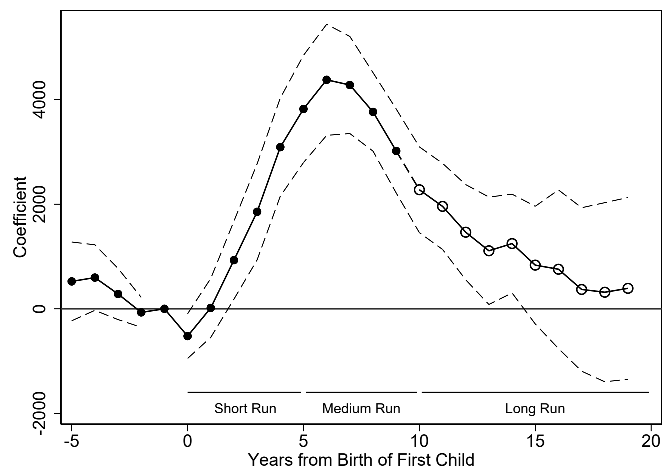
Notes: This figure presents the share of mothers who were never-married at first birth that remain single in each year since first birth. We plot this separately for mothers exposed to work incentives early (in the year of first childbirth) and late (3–6 years after childbirth)). We estimate the gap 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.5: Effect of Early Work Incentives on Earnings

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



(b) Difference-in-Difference

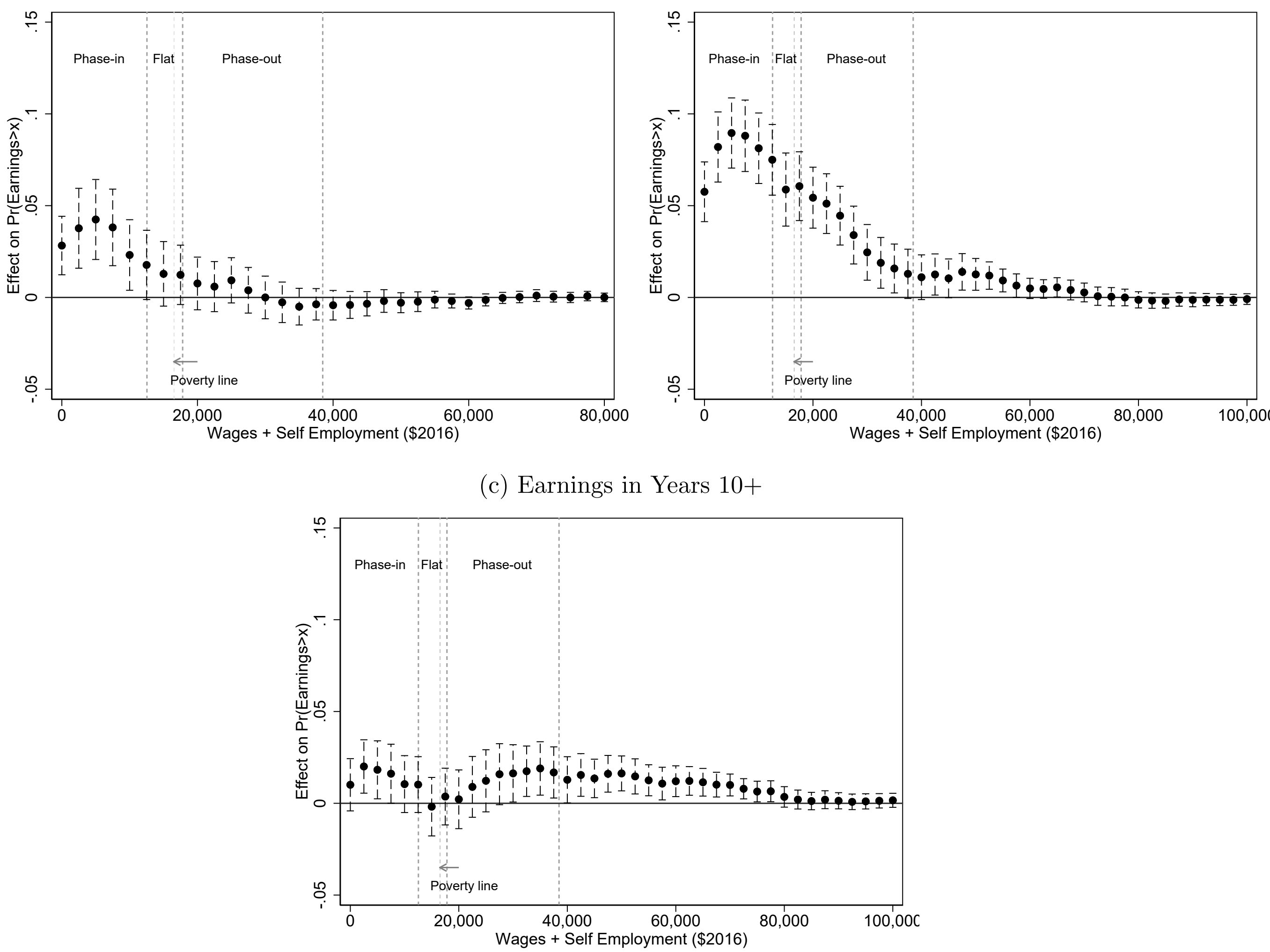


Notes: These figures present the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on earnings in each year from birth. Panel (a) plots event studies of employment around childbirth estimated separately for early- and late-exposed never-married mothers. Panel (b) shows DD event study estimates along with 95% confidence intervals. 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 19^{th} year after a first birth.

Figure A.6: Effect of Early Work Incentives on Earnings Density

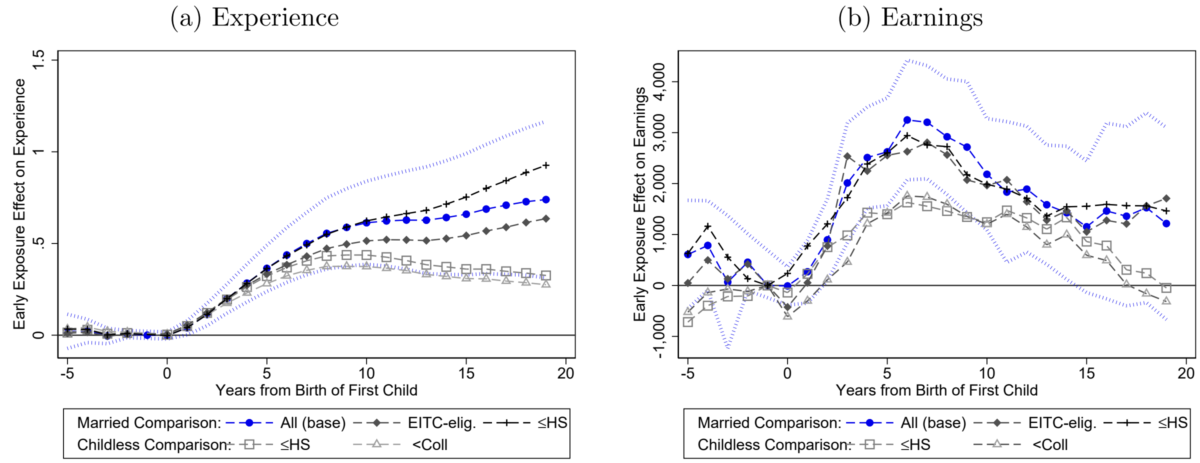
(a) Earnings in Years 0-3 (b) Earnings in Years 4-9

(c) Earnings in Years 10+



Notes: These figures presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the earnings distribution, along with 95% confidence intervals – during the period 0-3 (panel a), 5-9 (panel b) or 10+ years from birth (panel c). Estimates come from the dynamic DDD specification. Each marker is obtained from a different regression, where the outcome is an indicator for having annual earnings at least as large as X – where X is the amount shown on the x-axis. The dashed grey lines show, respectively, the end of the phase-in region on the 1994 EITC schedule; the 1994 poverty line; the end of the flat region on the 1994 EITC schedule; and the end of the phase-out region on the 1994 EITC schedule. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. Nominal EITC benefits are obtained from the Tax Policy Center (`https://www.taxpolicycenter.org/statistics/eitc-parameters`). *Years:* We include data from 5 years prior to a first birth up to the 4^{th} year after a first birth.

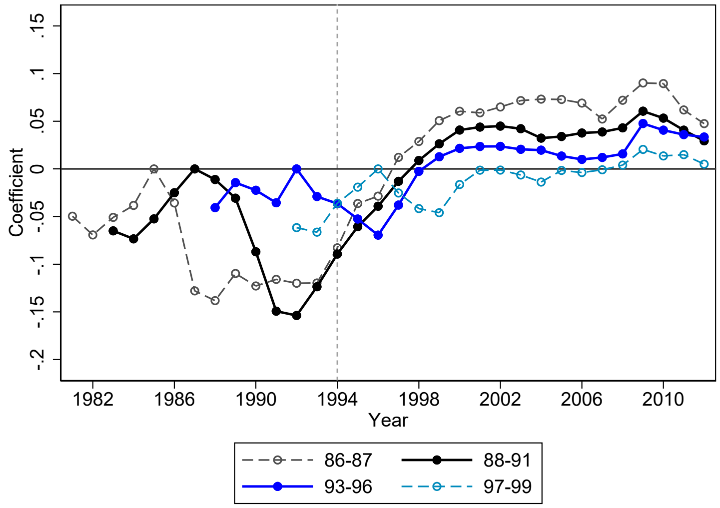
Figure A.7: Long-Run Effect of Early Work Incentives Using Alternative Comparison Groups



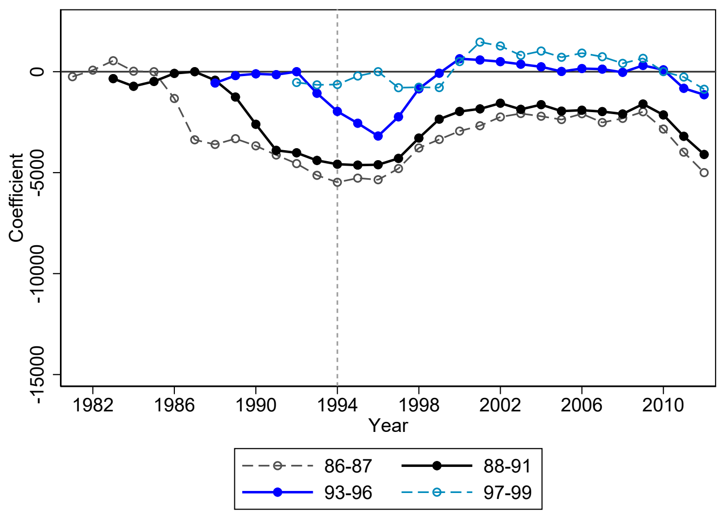
Notes: This figure presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) in each year from birth on years of experience (panel a) or earnings (panel b), along with 95% confidence intervals. Estimates come from the dynamic DDD specification using as comparison either all married mothers (the baseline); EITC-eligible married mothers; mothers with up to high school education; childless women with up to high school education; or childless women with up to college education. 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 19^{th} year after a first birth.

Figure A.8: Effect of Early Work Incentives on Labor Market Outcomes – Never-Married Mothers, By Year of First Birth and Calendar Year

(a) Employment



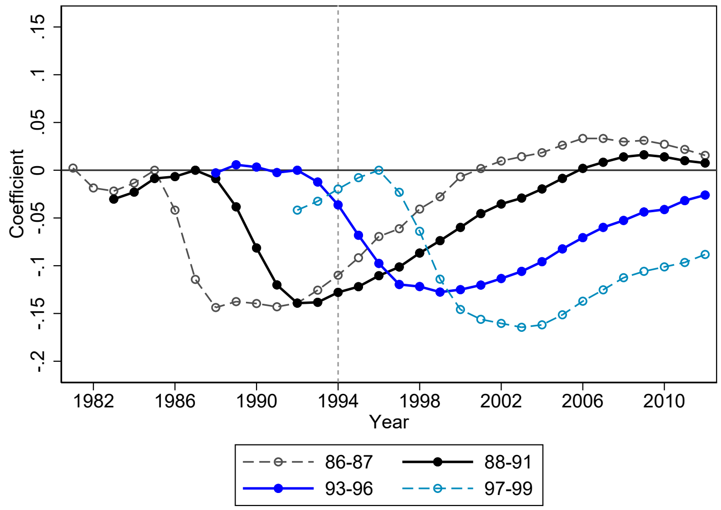
(b) Earnings



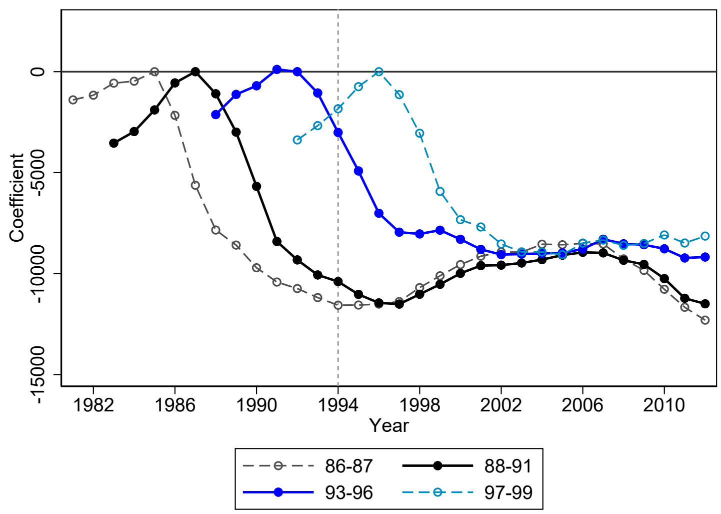
Notes: These figures shows calendar-year event studies of employment (panel a) or earnings (panel b), along with 95% confidence intervals, for groups of never-married mothers who were exposed to work incentives early (first birth: 1993–1996), late (first birth: 1988–1991), very late (first birth: 1986–1987) 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.9: Effect of Early Work Incentives on Labor Market Outcomes – Married Mothers, By Year of First Birth and Calendar Year

(a) Employment



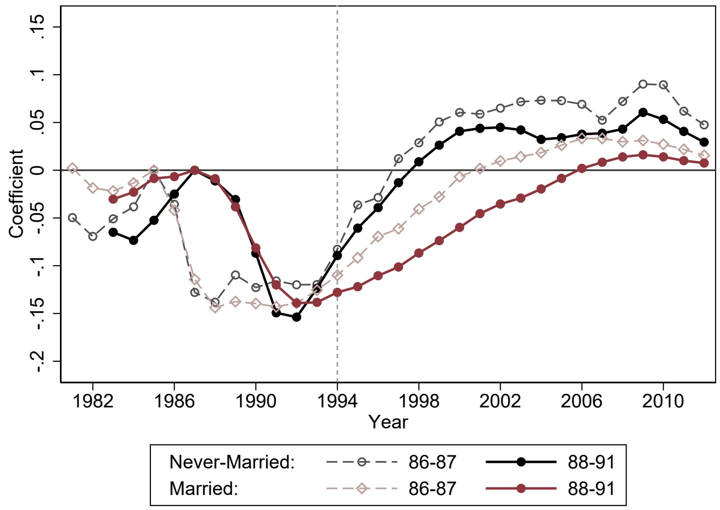
(b) Earnings



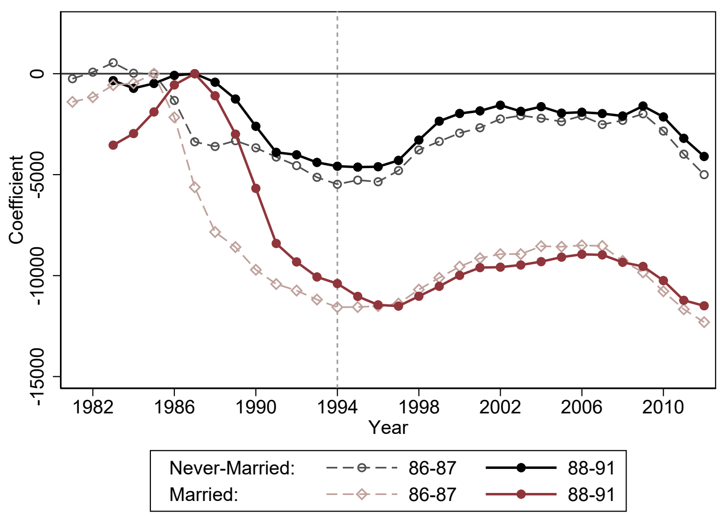
Notes: These figures shows calendar-year event studies of employment (panel a) or earnings (panel b), along with 95% confidence intervals, for groups of married mothers who were exposed to work incentives early (first birth: 1993–1996), late (first birth: 1988–1991), very late (first birth: 1986–1987) 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.10: Never-Married and Married Mothers with a First Birth Pre-Reform, By Year of First Birth and Calendar Year

(a) Employment



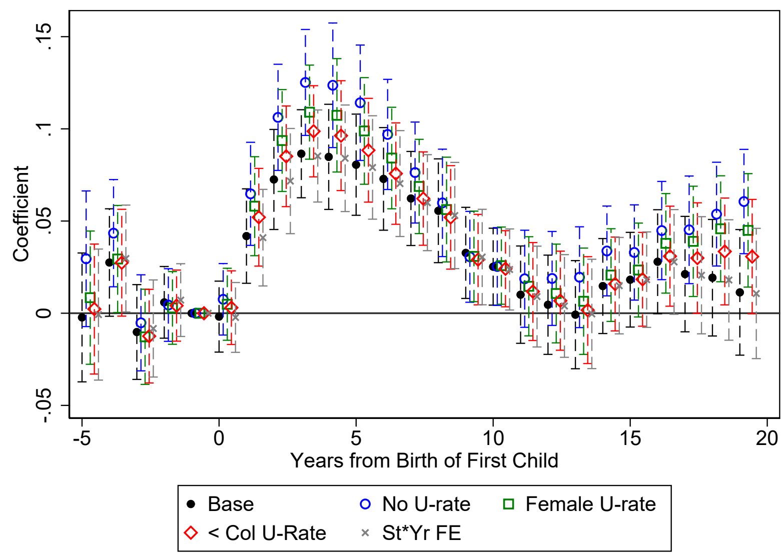
(b) Earnings



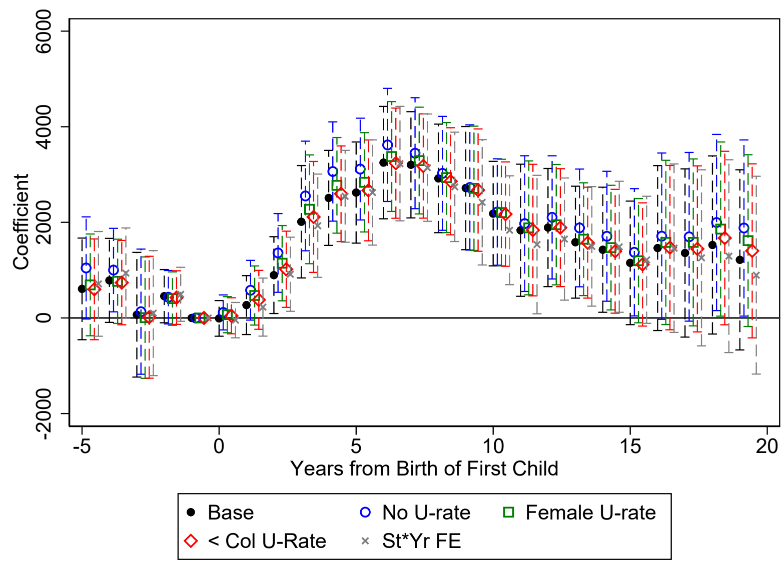
Notes: These figures calendar-year event studies of employment (panel a) or earnings (panel b), along with 95% confidence intervals, for groups of never-married and married mothers who were exposed to work incentives after birth, either late (first birth: 1988–1991) or very late (first birth: 1986–1987). 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.11: Effect of Early Work Incentives on Labor Market Outcomes
Sensitivity to Alternative Unemployment Rate Measures and State-Year Fixed Effects

(a) Employment



(b) Earnings



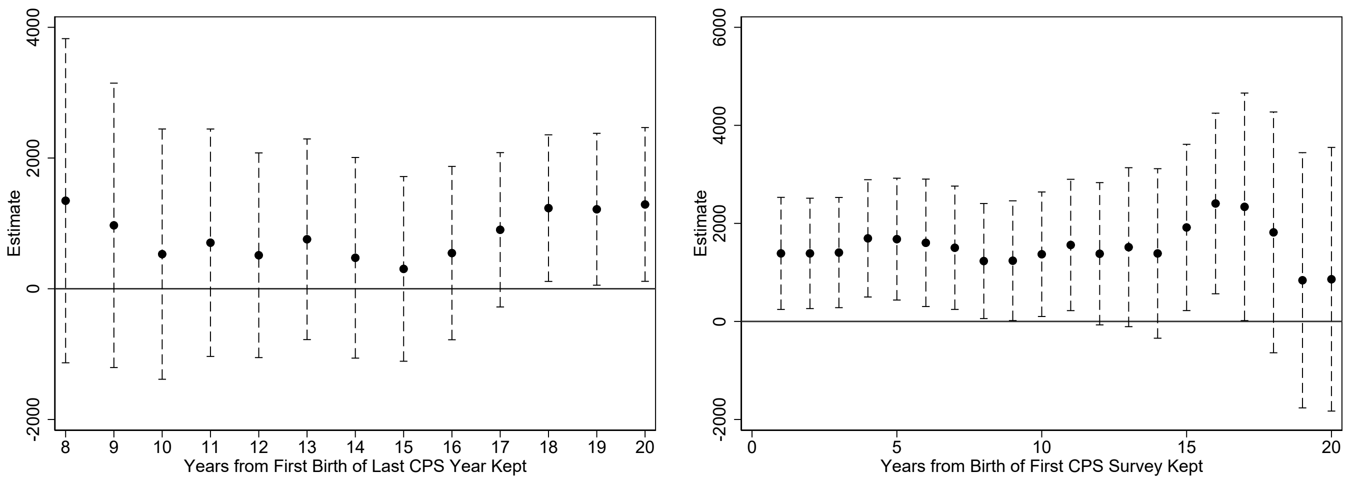
Notes: These figures present the sensitivity of the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on employment (panel a) or earnings (panel b), along with 95% confidence intervals, in each year from first birth. Each panel shows the baseline estimates as well as 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 19^{th} year after a first birth.

Figure A.12: Effect of Early Work Incentives on Long-Run Earnings, Sensitivity to:

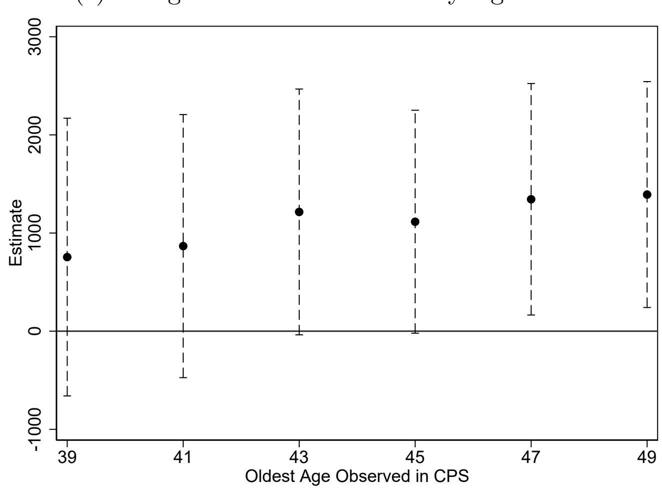
Keeping CPS Surveys

(a) *At most* from 8 to 20 Years of Birth

(b) *At least* 1 to 20 Years after Birth

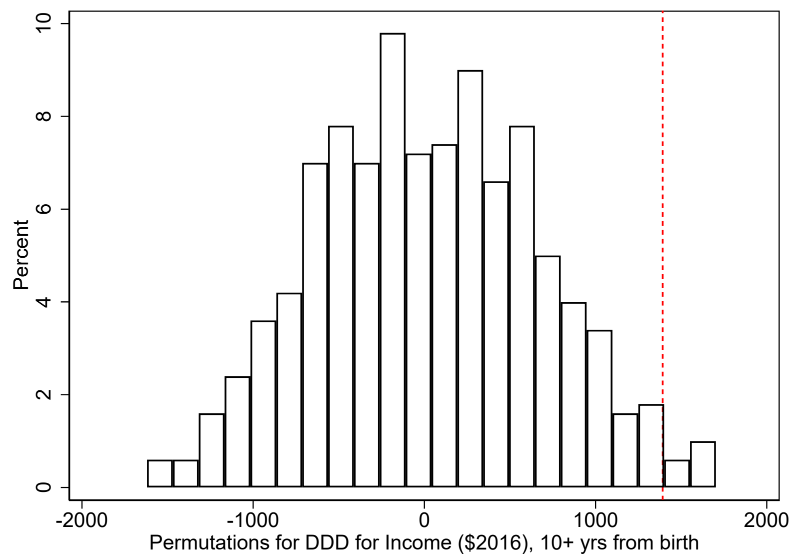


(c) Using Women Interviewed By Age 39 to 49



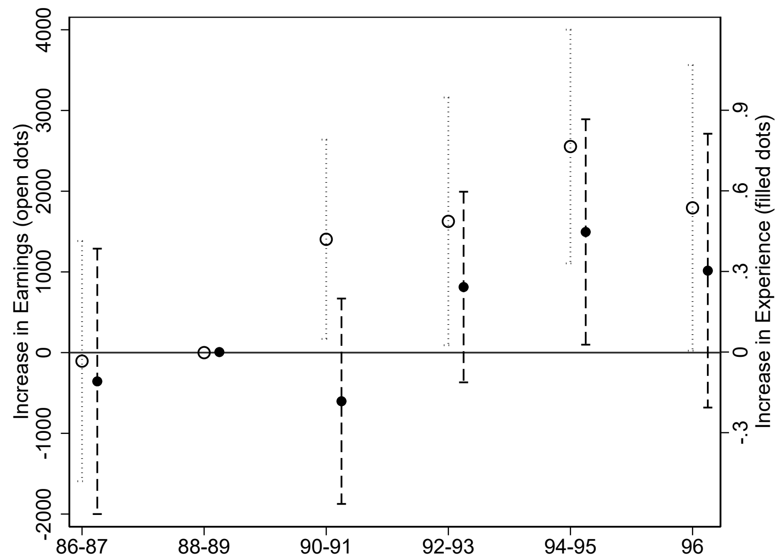
Notes: These figures presents the long-run effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on earnings, along with 95% confidence intervals, as we vary the sample restrictions. Each marker comes from a separate regression where we keep CPS surveys that occurred at least 1, 2, ...20 years from first birth (panel a) (ii) occurred at most 8, 9, ...20 years from first birth (panel b); or keep women that were no older than 39, 41...49 when interviewed in the CPS (panel c). See the notes of Figure 2 for information on control variables, standard errors, data and sample construction.

Figure A.13: Effect of Early Work Incentives on Earnings –Randomization Inference



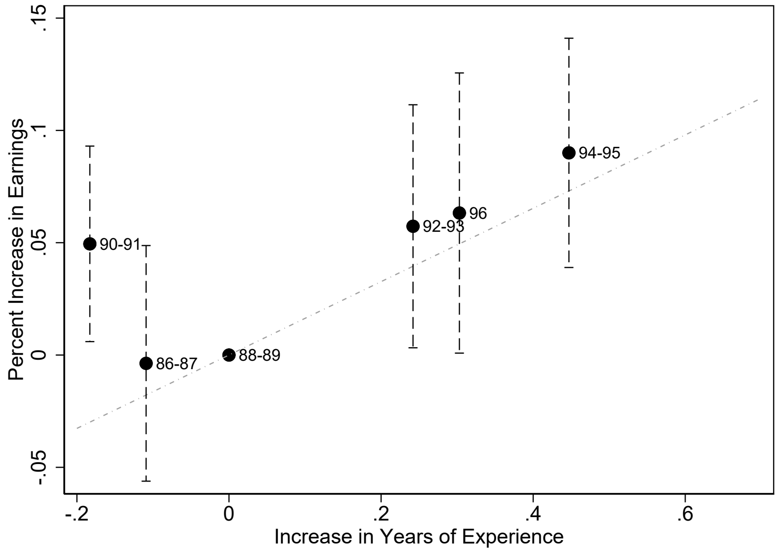
Notes: These figures show the distribution of estimates from 500 placebo experiments of the effect of early exposure to work incentives on long-run earnings (i.e., the coefficient on "10+ Yrs From Birth * EarlyExp * NM", where early and late exposure are randomly assigned. In particular, for each placebo experiment we randomly assign "early-exposure" to four randomly chosen years of birth drawn without replacement, and estimate a placebo DDD estimate. The red dotted line shows our baseline estimate. The one-sided p-values is 0.02. See the notes of Figure 2 for information on control variables, standard errors, data and baseline sample construction.

Figure A.14: Long-Run Effects on Earnings and Experience by Cohort



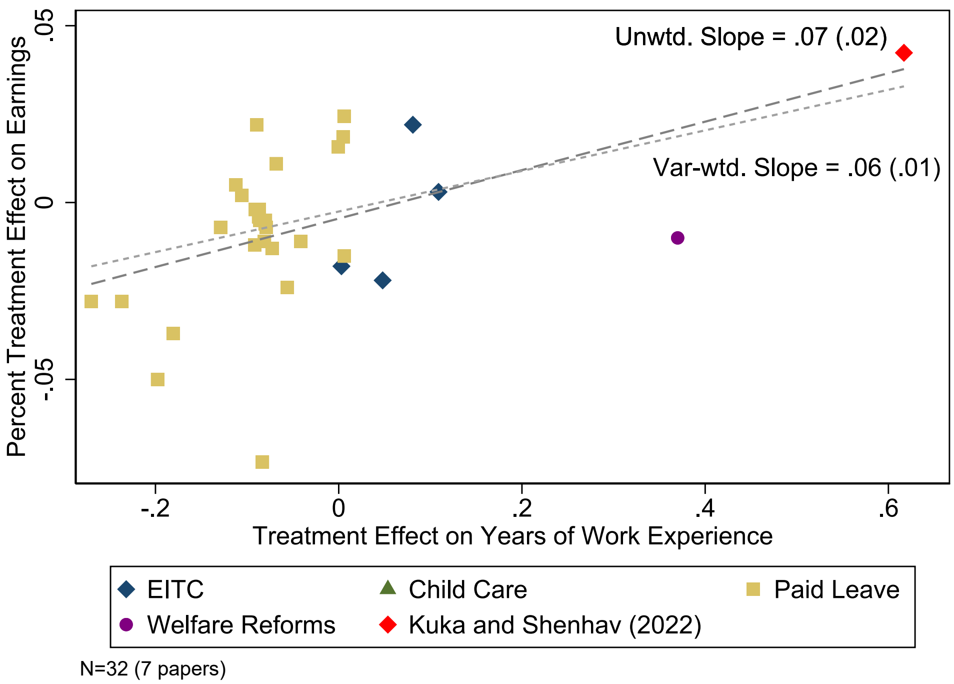
Notes: This figure shows coefficients and 95% confidence intervals from DDD regressions of outcomes on indicators for $EarlyExposed \cdot NM \cdot 10+$ Yrs From Birth interacted with indicators for having a first birth in 1988–89, 1990–91, 1992–93, 1994–95, or 1996. The omitted category (reference group) is first births in 1986-87. The grey open dots markers show impacts on earnings; the black filled markers show impacts on experience, which are calculated as the running sum of treatment effects on employment. See the notes of Figure 2 for information on the data, control variables, and standard errors. *Sample:* women whose first child was born in 1986–1996, 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 19^{th} year after a first birth.

Figure A.15: Correlation Between Cohort-Specific Treatment Effects on Earnings and Experience



Notes: This figure plots estimated effects on earnings (y-axis) and experience (x-axis) from DDD regressions of outcomes on indicators for $EarlyExposed \cdot NM \cdot 10+$ Yrs From Birth interacted with indicators for having a first birth in 1988–89, 1990–91, 1992–93, 1994–95, or 1996. The omitted category (reference group) is first births in 1986-87. Impacts on experience are calculated as the running sum of treatment effects on employment. We include the 95% confidence intervals for the estimated effects on earnings. The grey dashed line shows the best fit line, which we constrain to pass through the origin (i.e., no return to zero experience). See the notes of Figure 2 for information on the data, control variables, and standard errors. *Sample:* women whose first child was born in 1986–1996, 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 19^{th} year after a first birth.

Figure A.16: Returns to Experience Across Policies –
Weighting by Inverse Variance of Estimates



Notes: This figure presents percent treatment effects on earnings (y-axis) and treatment effects on years of work experience (x-axis) from studies in the policy database described in Section 7 using studies that report treatment effects on years of work experience. The beige square markers show estimated effects of paid leave; the blue diamond markers show estimated effects of the EITC; the green triangles show estimated effects of child care policies; the purple circles show estimated effects of welfare reforms; and the red diamond marker and vertical dashed red line show our estimated long-run effects using the SSA administrative data. The dark grey dashed line shows the unweighted best fit line, and the light grey dashed line shows the best fit line when we weight by the inverse of the variance of the estimates.

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

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

Notes: This table presents summary statistics for early and late-exposed never-married mothers. Panel (a) includes pre-birth outcomes and Panel (b) includes post-birth outcomes. We include “Share High School or Less” beis included in panel (a). “HH EITC eligibility Pre-Birth” is an indicator equal to one if a woman’s total family earnings pre-childbirth falls within the EITC-qualifying region for households with one child. “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: 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: This table tests whether early exposed mothers' characteristics have a different trend across "survey years from first birth" (CPS year minus year of first birth) than late-exposed mothers. Column 1 presents the estimated coefficient on an interaction between the trend and an indicator for early exposure for the outcome shown in the row header, and Column 2 presents the associated p-value.. See Table 1 for information on standard errors, data and sample construction.

Table A.3: Effects on Medium- and Long-Run Earnings, Sensitivity to Earnings Definition

	Base	Wage Earnings	Earnings if Pos.	Wage Earn. if Pos.	Log Earnings	Poisson Earnings	Log, drop Bottom 1%	Winsorize
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostBirth * EarlyExp * Yrs 5-9 * NM	2618***	2468***	1516**	1501***	0.165***	0.161***	0.141***	2510***
	(527)	(515)	(580)	(556)	(0.034)	(0.028)	(0.027)	(452)
PostBirth * EarlyExp * 10+ Yrs From Birth * NM	1393**	1353**	1190*	1395**	0.050	0.070***	0.053*	1201**
	(587)	(566)	(683)	(659)	(0.034)	(0.026)	(0.030)	(479)
Mean Y	23613	22846	30705	31119	9.750	23612.723	9.825	22971
Individuals	2714475	2714475	2397737	1990450	2397737	2714475	2053834	2714475

Notes: This table shows the medium- and long-run effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on earnings (columns 1, 6, 8); wage earnings (column 2), earnings conditional on working (column 3), wage earnings if working (column 4), and log earnings (columns 5, 7). Column 6 is estimated using a Poisson regression; the remaining columns are estimated with OLS. Column 7 drops the bottom 1% of observations to reduce the influence of outliers in the log specification. "Winsorized" earnings in column 8 are top-coded at $175,000, which is the top 1% of married mothers' earnings. 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 19^{th} year after a first birth.

Table A.4: Effect of Early Work Incentives on Long-Run Employment – CPS Responses

	Any Hours	Part Time $1(\leq 35hrs)$	Full Time $1(\geq 35hrs)$
10+ Yrs From Birth * EarlyExp * NM	0.004	0.000	0.004
	(0.033)	(0.030)	(0.035)
Mean Y	0.694	0.243	0.451
Individuals	94414	94414	94414

Notes: This table shows the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the likelihood of working any hours (column 1); the likelihood of working part-time ($\leq$ 35 hours) (column 2); and the likelihood of working full-time (¿35 hours) (column 3). We estimate this using the double-difference model in Equation 3. See Tables 1 and 2 for additional 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 19^{th} year after a first birth.

Table A.5: Effect of Early Work Incentives on Earnings – Sensitivity to Controls for Unemployment and Welfare

	Base	UR Dynamics	(Ref+Waivs)*Dynamics
	(1)	(2)	(3)
0-4 Yrs From Birth * EarlyExp * NM	763**	487	346
	(333)	(399)	(321)
5-9 Yrs From Birth * EarlyExp * NM	2618***	2533***	2401***
	(527)	(516)	(567)
10+ Yrs From Birth * EarlyExp * NM	1393**	1340**	1196*
	(587)	(569)	(616)
Mean Y	23613	23613	23613
Observations	2714475	2714475	2714475

Notes: This table tests the sensitivity of effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on earnings to additional control variables. Column 1 presents our baseline results; column 2 show the estimates when we allow the effect of the unemployment rate to vary by the age of one's first child; column 3 shows the estimates when allow the effect of welfare reform and waivers to vary by the age of one's first child. 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 19^{th} year after a first birth.

Table A.6: Effect of Early Work Incentives on Labor Market Outcomes – Sensitivity to Inverse P-Score Reweighting

	Employed	Earnings
	(1)	(2)
0-4 Yrs From Birth * EarlyExp * NM	0.040***	537
	(0.009)	(327)
5-9 Yrs From Birth * EarlyExp * NM	0.048***	2327***
	(0.010)	(526)
10+ Yrs From Birth * EarlyExp * NM	0.003	1101*
	(0.011)	(592)
Observations	2714475	2714475

Notes: This table presents the effects of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) using p-score reweighting to balance covariates across early- and late-exposed mothers. Column 1 presents effects on employment; and column 2 presents effects on earnings. 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 19^{th} year after a first birth.

Table A.7: Effect of Early Work Incentives on Earnings – Sensitivity to Alternative Specifications

	Base	Add AFB*YSB	Add Ind FE	Sample: Heads
	(1)	(2)	(3)	(4)
0-4 Yrs From Birth * EarlyExp * NM	763**	590*	803**	617*
	(333)	(331)	(328)	(345)
5-9 Yrs From Birth * EarlyExp * NM	2618***	2422***	2574***	2648***
	(527)	(533)	(515)	(521)
10+ Yrs From Birth * EarlyExp * NM	1393**	1170*	1341**	1695***
	(587)	(605)	(576)	(598)
Mean Y	23613	23613	23613	23936
Observations	2714475	2714475	2714475	2599850

Notes: This table tests the sensitivity of the effects of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on earnings. Column 1 shows our baseline results; column 2 adds age-at-birth by years-since-birth fixed effects; column 3 adds individual fixed effects; and column 4 restricts the sample to heads of household. See Table 1 for information on our baseline control variables, standard errors, data and baseline sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} and 20^{th} year after a first birth in Panels (a) and (b), respectively.

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

	Employment		Earnings	
	Below Med. U-Rate	Above Med. U-Rate	Below Med. U-Rate	Above Med U-Rate
PostBirth * EarlyExp * Yrs 0-4 * NM	0.053***	0.034	1069*	409
	(0.009)	(0.011)	(531)	(367)
PostBirth * EarlyExp * Yrs 5-9 * NM	0.048***	0.052***	2271**	2670***
	(0.012)	(0.015)	(917)	(602)
PostBirth * EarlyExp * Yrs 10+* NM	0.008	0.013	1100	1800**
	(0.011)	(0.016)	(1079)	(715)
Mean Y	0.797	0.737	24664	22700
Mean U-Rate 94-00	0.039	0.056	0.039	0.056
Individuals	1261950	1452525	1261950	1452525

Notes: This table presents the effects of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on employment (columns 1-2) and earnings (columns 3-4) by whether a mother's state of residence has an above- or below-median average unemployment rate betwee 1994 and 2000. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table A.9: Long-Run 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)
10+ Yrs From Birth * EarlyExp * NM	0.020**	-0.003	0.075***	-0.092***
	(0.008)	(0.004)	(0.016)	(0.014)
Mean Y	0.230	0.020	0.472	0.278
Observations	2714475	2714475	2714475	2714475

Notes: This table presents the long-run effects of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the likelihood of 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). Column 1 presents effects on the likelihood of having "high experience and high earnings"; column 2 presents effects on the likelihood of having high earnings and low experience; column 3 presents effects on the likelihood of having "low earnings and high experience"; and column 4 presents effects on the likelihood of having "low earnings and low experience." See the text and Appendix D 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 19^{th} year after a first birth.

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

	Panel A: Service Occupations							
	Housekeep	Janitor	Food	Child	Beauty	Recreation	Protect	Health Serv
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0-4 Yrs from Birth * EarlyExp * NM	-0.019*	-0.007	0.029	0.010	-0.007	0.004	0.008	0.013
	(0.010)	(0.009)	(0.022)	(0.009)	(0.010)	(0.004)	(0.006)	(0.020)
5-9 Yrs from Birth * EarlyExp * NM	-0.010	-0.005	0.012	0.011	0.002	0.003	0.015	0.027
	(0.012)	(0.008)	(0.020)	(0.007)	(0.011)	(0.003)	(0.009)	(0.019)
10+ Yrs from Birth * EarlyExp * NM	-0.007	-0.006	0.021	0.002	-0.004	0.002	0.008	0.035**
	(0.011)	(0.007)	(0.013)	(0.007)	(0.009)	(0.005)	(0.007)	(0.017)
Mean Y	0.013	0.008	0.034	0.018	0.012	0.004	0.006	0.034
Individuals	95573	95573	95573	95573	95573	95573	95573	95573

	Panel B: Non-Service Occupations						
	Exec/Man	Prof/Tech	Fin Sales	Ret Sales	Cleric	Agricultural	Mech/Constr/Min
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0-4 Yrs from Birth * EarlyExp * NM	0.032	-0.026	0.014	-0.008	-0.006	-0.007	-0.002
	(0.023)	(0.024)	(0.012)	(0.026)	(0.034)	(0.006)	(0.006)
5-9 Yrs from Birth * EarlyExp * NM	0.012	-0.002	0.004	-0.007	-0.012	0.006	0.001
	(0.020)	(0.030)	(0.011)	(0.019)	(0.026)	(0.006)	(0.005)
10+ Yrs from Birth * EarlyExp * NM	0.025	-0.023	-0.001	-0.014	-0.051**	0.008	-0.000
	(0.016)	(0.024)	(0.009)	(0.014)	(0.025)	(0.005)	(0.004)
Mean Y	0.105	0.204	0.032	0.044	0.177	0.008	0.004
Individuals	95573	95573	95573	95573	95573	95573	95573

Notes: This table presents the effects of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the likelihood of being in each service occupation (panel a) or non-service occupation (panel b). We estimate this using the double-difference model in Equation 3. Occupation definitions are in Appendix B.1. See Table 2 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 19^{th} year after a first birth.

Table A.11: Effect of Early Work Incentives on Tasks Performed at Work – CPS Responses

	Autor and Dorn (2013)				Deming (2017)								
	Abstract	Routine	Manual	Offshoreable	Math	Routine	Social	Service	Customer	Reason	Info	Coord	Interact
PostBirth * EarlyExp * Yrs 0-4 * NM	0.335**	0.223	0.009	0.046	0.188	0.138	0.216	0.046	0.132	0.161	0.189	0.138	0.295
	(0.151)	(0.297)	(0.076)	(0.090)	(0.192)	(0.292)	(0.154)	(0.233)	(0.317)	(0.202)	(0.181)	(0.149)	(0.211)
PostBirth * EarlyExp * Yrs 5-9 * NM	0.116	0.098	0.180**	-0.048	0.033	0.193	0.206	0.319*	0.443*	0.158	0.173	0.062	0.299*
	(0.152)	(0.191)	(0.075)	(0.073)	(0.141)	(0.208)	(0.163)	(0.182)	(0.250)	(0.173)	(0.158)	(0.139)	(0.175)
PostBirth * EarlyExp * Yrs 10+* NM	0.039	-0.184	0.087	-0.094	-0.064	-0.199	0.058	0.004	0.069	-0.050	0.031	0.032	0.040
	(0.123)	(0.185)	(0.063)	(0.066)	(0.111)	(0.151)	(0.108)	(0.144)	(0.178)	(0.130)	(0.124)	(0.110)	(0.132)
Mean Y	2.547	2.984	0.691	0.061	3.078	3.127	2.871	3.428	4.310	3.544	3.248	2.306	4.077
Individuals	95573	95573	95573	95573	95441	95441	95441	95441	95441	95441	95441	95441	95441

Notes: This table shows the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the intensity of tasks performed at work. We estimate this using the double-difference model in Equation 3. Columns 1–4 show effects on the intensity of the following types of tasks involved in a worker's job: abstract, routine, and manual; and the offshoreability of the work. These measures are created in Autor and Dorn (2013) using information on tasks by occupation from O*NET. Columns 5–13 shows effects on the level of the following skills or tasks involved in a worker's job: mathematical competence, routine tasks, social skills, service, social interaction, reasoning, information use, coordination, and interaction. These measures are created in Deming (2017) using information on tasks by occupation from O*NET. See Tables 1 and 2 for information on control variables, standard errors, data and sample construction.

Table A.12: Effect of Early Work Incentives on Completed Fertility – CPS Responses

	Number of Kids	2+ Kids	3+ Kids	Yrs b/w 1 and 2
	(1)	(2)	(3)	(4)
EarlyExp * NM	0.010	0.012	-0.006	-0.117
	(0.070)	(0.045)	(0.036)	(0.439)
Mean Y	2.222	0.771	0.317	3.619
Observations	45392	45392	45392	34981

Notes: This table shows the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the total number of children in the household (column 1), the likelihood of having at least 2 children (column 2); the likelihood of having at least three children (column 3), and the number of years between one's first and second child (column 4). We estimate this using the double-difference model in Equation 3. 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 Tables 1 and 2 for additional 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 19^{th} year after a first birth.

B Appendix to Section 2

B.1 Grouping 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).[44] In particular, we downloaded the March CPS from IPUMS for the CPS surveys in our sample, and then collapsed the data by "occ1990" and the original CPS occupation variable to create a crosswalk. We then merge the crosswalk on to our data, which gives us the "occ1990" corresponding to each individual in our sample.

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

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

[44] See 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 B.1: 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 early- and late-exposed mothers. 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.[45]

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.

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

C Appendix to Section 4

In this section, we provide additional results on the short-run effects of work incentives, including evidence on parallel trends post-childbirth; a calculation of the implied labor supply elasticity; and estimation of heterogeneous effects corresponding to EITC incentives. Throughout, we restrict our data to end four years after childbirth in order to better calibrate the parameters on the control covariates to this short run period. As in the main analysis, our preferred estimates are from the DDD design, but for transparency, we also present the (very similar) DD results.

Table C.1 shows our baseline short-run effects on employment using this sample. We find that early exposure to incentives leads to a 3.4 p.p. increase in employment (column 3), a 5.9 percent effect relative to the mean for late-exposed mothers, and that this effect is driven by an increase in the likelihood of having any wage earnings (column 6).[46]

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

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

Notes: This table presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on employment (positive total earnings, columns 1–3), and positive wage earnings (columns 4–6).. 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). See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

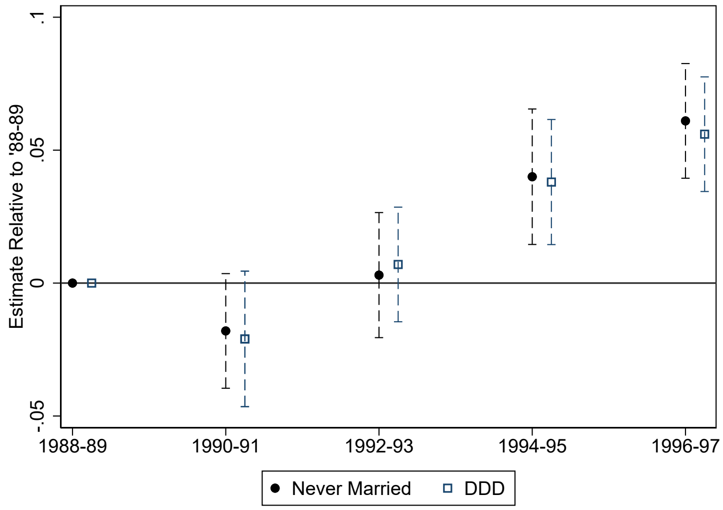
C.1 Pre-reform trend in maternal employment

To complement the evidence in Section 4 of parallel trends *prior* to birth, Figure C.1 examines *post-childbirth* employment by year of first birth. In particular, we re-estimating our DD and DDD models replacing "$PostBirth \cdot EarlyExposed$" with separate interactions between "$PostBirth$" and a set of indicators for having a first birth between 1990-91, 1992-93, 1994-95, or 1996-97. 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. Instead, Figure C.1 shows little change in employment upon motherhood among pre-reform cohorts: mothers that have a first birth in 1992-93 work as much after childbirth (relative to pre-childbirth) as those with a first birth in 1988-89. Subsequent cohorts have a sharp change in post-birth behavior. For births beginning in 1994, post-birth employment increases by 5 to 7 p.p.[47]

[46]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, 2021), and is noticeably smaller than estimates for mothers with young children (Kleven, 2021; Michelmore and Pilkauskas, forthcoming).

[47]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.

Figure C.1: 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. The filled circular markers present the DD using never-married mothers and the open squares present the DDD. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

C.2 Elasticity Calculation

To translate our short-run 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 C.2), 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 (2021).[48] 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 ($\frac{5.9}{8.2}$).

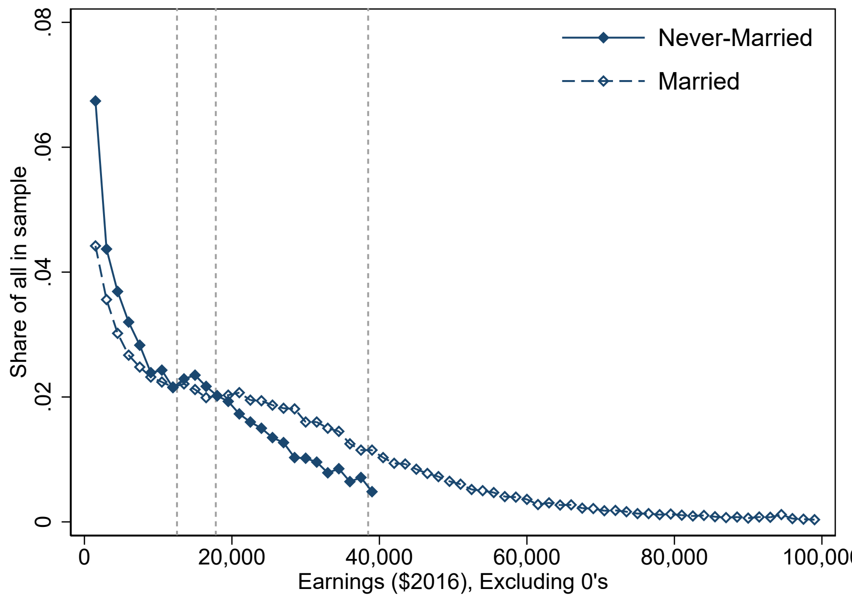
C.3 Are Mothers Responding to EITC Work Incentives in the Short Run?

While identifying the exact incentives that drive increases in experience is not critical for our long-run results (as we explain in Section 1), it is important that these are *exogenous* in order to rule out potential individual-level confounds. For example, it would be problematic if the rise in experience in early-exposed mothers was driven by changes in selection into motherhood based on preferences for maternal employment. To rule out such stories, we test whether our effects are consistent with the *specific* incentives and timing of the EITC reform.

We implement four tests, which we adapt from prior EITC studies. First, 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

[48]This will underestimate the change in benefits if mothers have more than one child.

Figure C.2: Distribution of Post-birth Earnings, Excluding 0's – Late-Exposed Mothers



Notes: This figure shows the truncated distribution of earnings zero to three years after a first birth for never-married and married mothers who were exposed to work incentives late (3–6 years after childbirth. We omit the large mass at 0 and small number of observations in the never-married distribution beyond $40,000. See the notes of Figure 2 for information on data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 4^{th} year after a first birth.

for mothers after a third-or-higher births (3+) relative to a second birth. Second, we expect our results to extend beyond states with high employment growth, to begin prior to the implementation of federal welfare reform in 1997, and to be stable to the introduction of more detailed controls for welfare waivers and unemployment rates. Third, we test for bunching at the EITC-maximizing level of income, as predicted by economic theory (Saez, 2010). Fourth, we test whether the employment effects are larger in states with supplemental state EITC, where the EITC incentives are larger.

Effects by birth order To implement the first test, we use a sample that includes *all births* to never=married women that occurred between 1988–1991 or 1993–1996 (i.e. not just first births). We treat each birth as an independent event by creating a 10-year mother-birth panel around each birth, and stack these panels. We then run a triple-difference model to identify whether the change in employment after a 2+ birth between early- and late-exposed never-married mothers is larger than the change between early- and late-exposed never-married mothers after a first birth.[49]

Column 1 of Table C.2 shows that employment increases by 3.2 p.p. more after a 2+ birth relative to a first birth. Column 2 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, 2021).

[49]Specifically, we redefine ρ_m and NM_m in Equation 1 to be indicators for being a 2+ mother.

Controlling for the booming economy and welfare reform Next, 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 C.2 show that the employment effects are very similar for states with above-median and below-median changes in the unemployment rate between 1994–2000 and 1988-1993. This is despite the fact that the average change in unemployment was three times as large in the above-median states (-1.8 p.p. versus -0.6 p.p.). Hence, our employment effects hold to a similar degree even in states that experienced relatively weak economic growth.

Further, in columns 5 and 6 of Table C.2, we allow the coefficients on our baseline unemployment and welfare controls to vary by the age of one's first child, to address potentially larger responses to the economy and welfare reform for mothers with young children (Kleven, 2021). 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 C.2, 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).[50] The coefficients are similar to our main event study, and statistically significant in years 2 and 3 (see Appendix Figure C.3 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, e.g., Meyer and Rosenbaum (2001), Grogger (2003*b*), and Bastian and Jones (2020).

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., Saez (2010) and Chetty et al. (2013) we do find evidence of a small increase in self-employment as well as post-birth bunching among mothers who are ever self-employed in Appendix Figure C.4 and Appendix Table C.3. 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 C.4, in line with previous evidence that bunching increased after the 1993 reform (Saez, 2010).[51]

Heterogeneity by state EITC supplement Finally, we also examine whether the effects on employment vary with the presence of a supplemental EITC in the mothers' state of residence.[52] Because state EITCs are not randomly assigned, we view this evidence as only suggestive. Columns 1 and 3 of Appendix Table C.4 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.

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

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

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

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

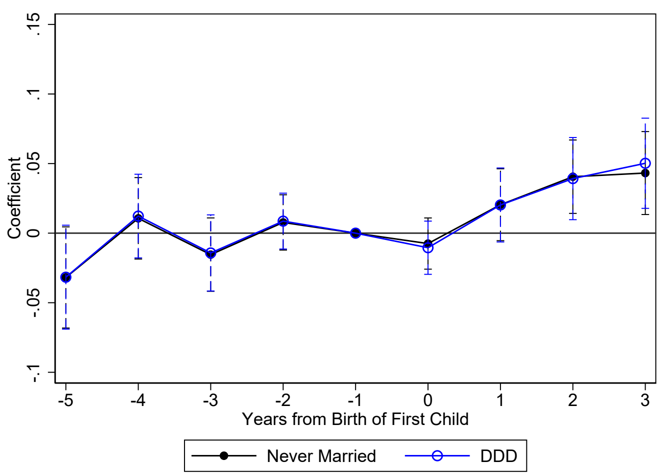
	By Birth Parity		By Change in U-Rate		Control for Dynamics		Up to 1996	
	2+ vs.1	3+ vs 2	High	Low	U-Rate	Ref+Waivs	All	No Waiver
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostBirth * EarlyExp * Child 2+	0.032**							
	(0.014)							
PostBirth * EarlyExp * Child 3+		0.011						
		(0.022)						
PostBirth * EarlyExp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

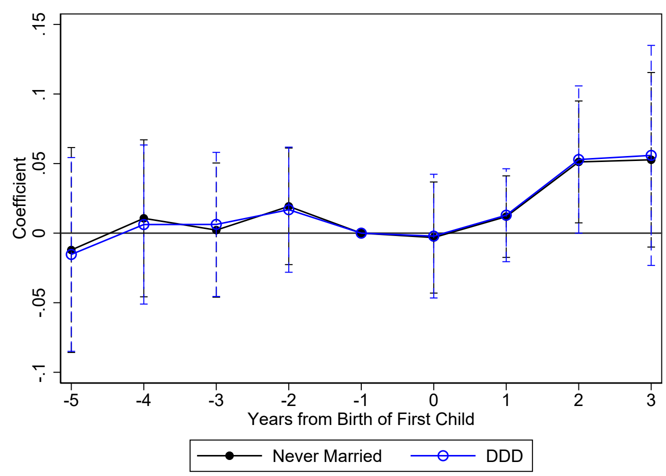
Notes: This table shows the the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on employment. Column 1 includes all mothers with a birth from 1988–1991 or 1993–1996, and uses mothers after a first birth as comparisons for mothers after a second-or-higher-order birth ("child 2+"). Column 2 includes all mothers with a second-or-higher-order birth from 1988–1991 or 1993–1996, and uses mothers after a second birth as comparisons for mothers after a third-or-higher-order birth ("child 3+"). Columns 3 and 4 compare mothers with early- and late-exposed first births in states that experienced an above-median (column 3) or below-median (column 4) change in the unemployment rate between 1994-2000 and 1988-1993. Columns 5 and 6 present estimates when we add to our baseline DD specification interactions between the age of one's first child and the unemployment rate (column 5) or between the age of one's first child and our indicators for welfare reform and waivers (column 6). Columns 7 and 8 present the DD event study estimates for years 1–3 after a first birth when we restrict the sample to the years prior to 1996 (column 7) and to states that didn't pass a waiver up to 1996 (column 8). See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Figure C.3: Effect of Early Work Incentives on Short-Run Employment – Prior to Federal Welfare Reform

(a) All States

(b) No-Waiver States

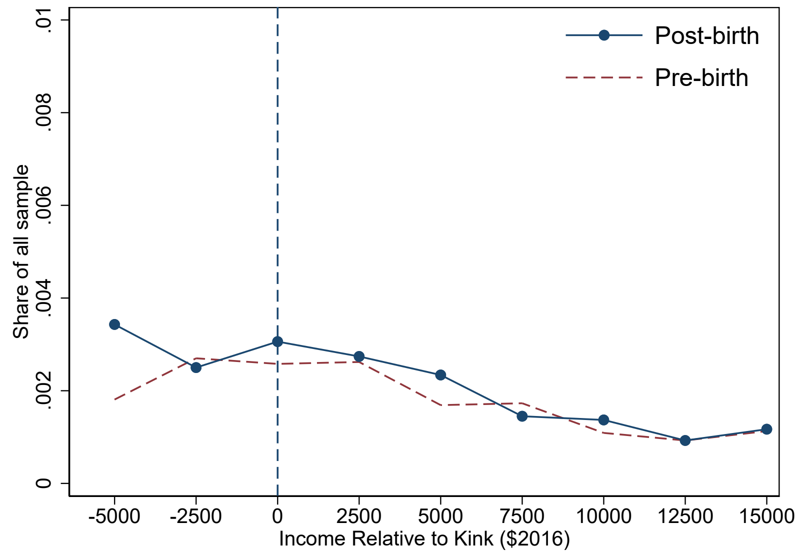




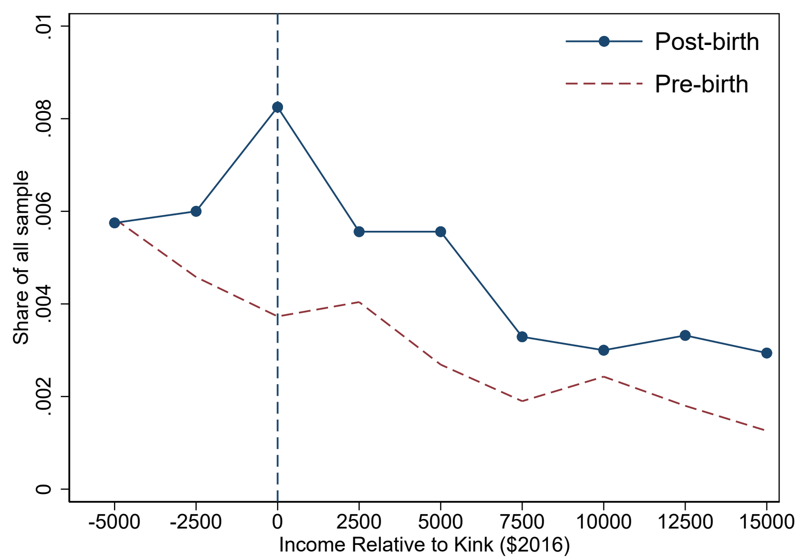
Notes: These figures presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on employment in each year after birth, for the years up to 1996. Panel (a) includes all states; panel (b) focuses on states that had not passed a welfare waiver by 1996 (panel b). See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth or 1996, whichever comes first.

Figure C.4: EITC Expansion and Bunching Before and After Birth – Never-Married Mothers

(a) Late-Exposed Self-Employed Mothers



(b) Early-Exposed Self-Employed Mothers



Notes: These figures shows the distribution of earnings for never-married mothers who are self-employed for mothers who are early-exposed (panel a) and late-exposed (panel b), pre- and post-birth. Pre-Birth includes the 5 years prior to a first birth, and post-birth includes up to the fifth year after a first birth. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table C.3: Effect of Early Work Incentives on Short-Run Self-Employment and Bunching

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

Notes: This table presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the likelihood of having self-employment earnings (columns 1-2); having earnings within $1,500 of the first EITC kink (columns 3–4); or having earnings withing $2,500 of the first EITC kink (column 5–6). For each outcome we present both the DD using never-married mothers as well as the DDD.. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

Table C.4: Effect of Early Work Incentives on Short-Run Employment – Heterogeneity by the Presence and Generosity of a State EITC Supplement

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

Notes: This table presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on employment, by whether there is a state EITC supplement. Columns 1 and 2 show interactions between early exposure and whether there is any state EITC supplement available in the current year; while columns 3 and 4 show interactions between early exposure and whether the size (%) of the state EITC supplement available in the current year. See Table 1 for information on control variables, standard errors, data and sample construction. *Years:* We include data from 5 years prior to a first birth up to the 5^{th} year after a first birth.

C.4 Relation to Kleven (2021)

It is worth noting that some of our short-run estimates differ from those in a recent analysis of the 1993 reform in Kleven (2021). In this subsection, we outline the key points in Kleven's analysis of the reform and discuss potential explanations for the discrepancies in our findings.[53]

Brief summary of Kleven (2021) Kleven (2021) 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 is increasing in family size and decreasing in the age of one's youngest child. Second, he calculates 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 more likely to have been induced by welfare reform than by the EITC expansion.

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. 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.8.[54] 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 suggests that the elasticity of employment to pre-tax labor earnings is between 0.54 and 0.72, or roughly 27% to 40% as large as the estimate for mothers with one child in Kleven (2021). The discrepancy between our estimates and Kleven's estimates reflect differences both in the numerator and the denominator of the elasticity. First, our employment effects in percent terms are half the size of Kleven's: 5.9 percent ($\frac{3.7}{63.1}$) vs. 12.4 percent ($\frac{8.5}{68.1}$).[55] 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 predicting 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. The imputations assume that non-workers earnings': (i) fall only

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

[54] This is in line with Grogger (2003*a*), who also does not find differential effects of the EITC by the age of one's youngest child.

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

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.[56] This produces changes in EITC benefits equal to a 10.9, 9.9, or 8.2 percent change as a share of pre-tax earnings, respectively. Our higher change in benefits reflects our lower-income and younger population, the longer period over which we estimate changes in the EITC (e.g., we include the 1990 reform as part of our treatment), and our more-flexible assumptions aboutthe distribution of earnings for non-workers

3. Controlling for welfare waivers and business cycle Different than Kleven (2021), our estimates are not affected when we allow our unemployment rate and welfare waiver controls to be "dynamic" by allowing differential impacts by the age of one's first child.[57] We also show that our employment effects are present when we restrict our sample period up to 1996 and when we limit our sample to states that did not pass any waivers prior to 1996 (e.g., Appendix Table C.2, columns 7–8). Further, we note that Kleven's effects inclusive of these controls are quite imprecise, and could not reject our estimated effects.[58]

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

[57]We do not model event-year dynamics for the welfare waivers as in some of the specification in Kleven (2021) 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.

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

C.5 Overview of Effects on Taxes, Transfers, Net Income, and MVPF

Our primary focus in the paper is on quantifying 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 income 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). We include more minor details of this exercise in Appendix C.6.

Earnings The first two bars of Figure C.5 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).[59] This gives the EITC amount that a household is *eligible* to receive in each year. The third bar in Figure C.5 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 C.6 below 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 C.5).

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 4.1. We focus on impacts on the value of benefits received from the largest transfer programs, including welfare benefits, disability benefits, food stamps/SNAP, the value of Medicaid, and housing subsidies. The fourth pair of bars in Figure C.5 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 C.5 for estimated effects on individual programs, and Appendix C.6 below 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).

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

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).[60] If we assume that care is needed for 52 weeks, then the annual cost for each 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 C.5).

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 C.5 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). 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.

Our estimates above imply that, over twenty years, early-exposed mothers are eligible to receive in present value terms $2,328 in EITC benefits ($1,000 of which is a pure transfer to recipients) and pay $1,559 more in taxes. If we focus only on these impacts on earnings and taxes, we can compute a lower bound of the MVPF for our population as:

$$MVPF = \frac{\text{WTP}}{\text{Cost + Fiscal Externality}} \leq \underbrace{\frac{\text{WTP}}{\text{Cost + Add'l Taxes}}}_{\text{Our baseline estimate}} \tag{4}$$

Plugging in our estimates, we obtain a long-run MVPF of 1.30 ($\frac{1000}{2{,}328-1{,}559}$), which increases to 2.0 if we account for incomplete take-up of the EITC. We show a range of MVPFs across specifications and tax rate assumptions in Figure C.6. Figure C.7 shows that the MVPF would be at least half as large if we only considered the medium-run effects, highlighting the importance of tracking outcomes over the longer term.

Because we do not observe all possible externalities, our long-run MVPF reflects an incomplete accounting of the net cost of the expansion. We have argued that our MVPF is likely to be

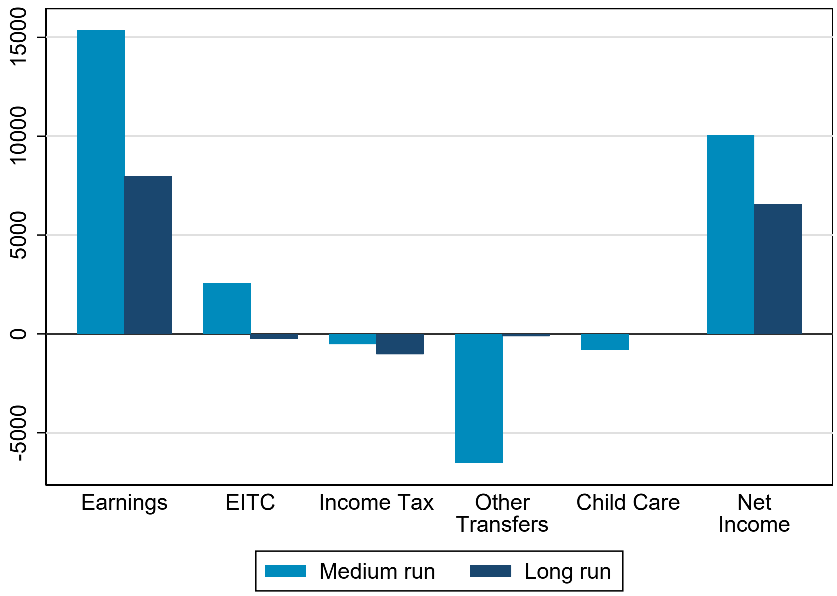
[60] We calculate this as the inflation-adjusted weighted average of the cost of each type of child care, where the weight is the share of unmarried moms that use each type of care times the share that pay anything for care. See the fourth panels of Tables 2 and 3 of Anderson and Levine (2000) for inputs.

a lower bound because we are omitting impacts on many non-EITC transfers, particularly cash welfare. However, our calculation also omits intergenerational impacts, which could in theory be either positive or negative. Suggestively, Bastian and Michelmore (2018) and Dahl and Lochner (2012) find that EITC expansions during childhood tend to raise test scores, educational attainment and earnings. These average impacts may not translate completely to our population of mothers exposed at first birth; however, at face value they are consistent with our MVPF 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 MVPF is not the same as the overall MVPF of the 1993 EITC expansion (i.e., for all eligible families). Inclusive of transfers, our MVPF estimate of 5.6 is larger than prior EITC MVPFs, which range from 1.08 to 1.12 (Hendren and Sprung-Keyser, 2019) for the 1993 expansion, or from 3.18 to 4.23 (Bastian and Jones, 2020) for all post-1990 EITC expansions.[61] Our higher estimate likely reflects a couple of key factors. First, as mentioned above, incorporating long-run earnings increases the MVPF. Second, we show that new mothers experience larger changes in work experience and thus greater gains from work incentives. Third, our estimates exclude married mothers, who generally reduce the MVPF of the EITC. In that sense, our estimates are a more relevant benchmark for the benefits of a work incentive for new mothers or single mothers than for evaluating the EITC.

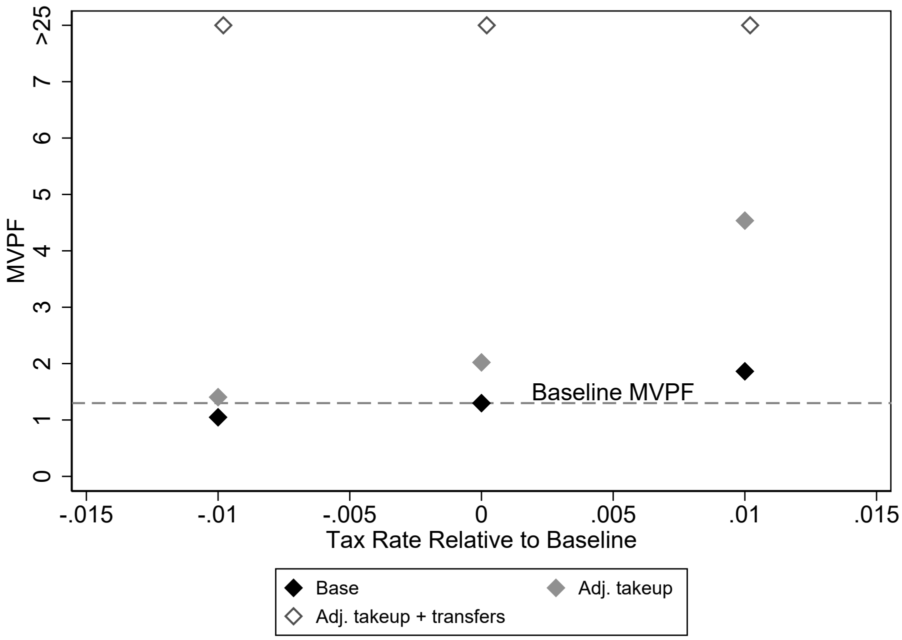
[61] In other respects, our estimates align closely with this prior work. Our estimated "mechanical" share of the EITC increase is identical to Bastian and Jones (2020) (who estimate this to be between 54–72%), and is slightly lower than Hendren and Sprung-Keyser (2019) (who estimate this to be 89.5%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

Figure C.5: Effect of Early Work Incentives on Net Income
through Changes in Taxes, Transfers, and Child Care



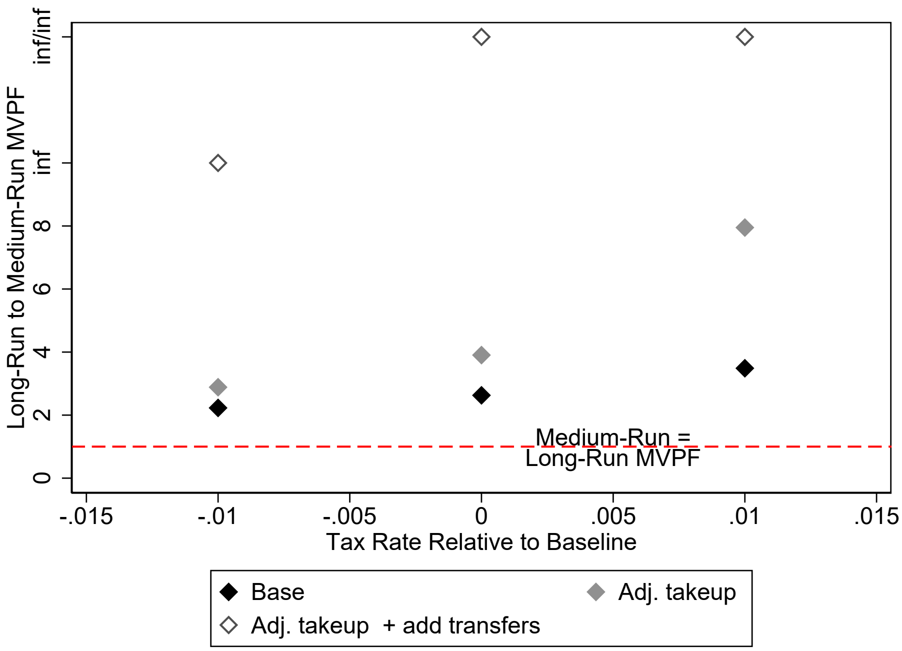
Notes: This figure presents the impact of early exposure to work incentives on the present value of net income in the medium run (years 0 to 9 post-childbirth) and long run (years 10-19 post-childbirth) stemming from changes in (i) earnings, (ii) EITC benefits, (iii) federal income taxes, (iv) other 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 using 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 C.6 for details about the calculation 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 double-difference specification using CPS survey data. See Section for details. We calculate (iv) using estimates of child care costs from Anderson and Levine (2000).

Figure C.6: Long-Run MVPF Across Varying Assumptions



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

Figure C.7: Ratio of Long-Run to Medium-Run MVPF Across Varying Assumptions



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

C.6 Additional Details for Calculation of Net Income and MVPF

Calculation of Average Tax Rate In order to estimate the effect of early exposure to the EITC expansion on federal income tax revenue, we require estimates of the average tax rate for the additional dollars earned by early-exposed mothers in the short-, medium-, and long-run. In this section, we explain how we calculate this tax rate.[62]

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

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

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

First, we use the coefficients from our distributional regressions (Figure A.6) to generate estimates of $\Delta f_{j,\tau}$. Recall that the distributional regressions give estimates of the difference in the cdf of earnings between early- and late exposed mothers for the short-, medium-, and long-run.[63] In particular, we have estimates of $Pr(Y > y)^{early} - Pr(Y > y)^{late}$ for $y \in \{0, 2500, ...100000\}$. We can use these estimates to obtain $\Delta f_{j,\tau}$ for \$2,500 bins of earnings. To do so, we take the difference between the distributional estimates for two sequential y. For instance, the change in the density of earnings between \$5,000 and \$7,500 is equal to the difference between the change in the cdf at $y = 7500$ and $y = 5000$.[64]

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

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

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

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

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

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

[64] E.g.,

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

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

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.

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

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

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

For these reasons, we interpret our estimates of the impact of early-exposure on transfers in Appendix Table C.5 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).

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 C.5) 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

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

Table C.5: Effect of Early Work Incentives on Government Transfers – CPS Responses

	Welfare (1)	Disability (2)	SNAP (3)	Medicaid (4)	Hous Sub (5)	Total (6)
0-4 Yrs from Birth * EarlyExp * NM	-724.8***	120.0	-343.5*	-85.6	4.0	-936.1*
	(243.1)	(104.4)	(182.0)	(168.7)	(11.1)	(475.7)
5-9 Yrs from Birth * EarlyExp * NM	-824.0***	-54.9	-710.4***	-134.9	-18.2	-1599.2***
	(158.5)	(73.3)	(153.5)	(200.2)	(12.7)	(356.5)
10+ Yrs from Birth * EarlyExp * NM	-8.960	-7.3	-237.1*	81.541	-14.6*	-61.3
	(110.5)	(77.4)	(130.5)	(195.1)	(8.3)	(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 presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the level of cash and in-kind transfers from each government program (shown in the headers). We estimate this using the double-difference model in Equation 3.. See Tables 1 and 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 19^{th} year after a first birth.

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 C.6 present the estimated effects for our simulated total EITC benefits, benefits through the "behavioral" channel, and benefits through the "mechanical" channel, respectively. In the short-run, early-exposed mothers' EITC benefits increase by $400. Over half of this increase (54%) 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 $93 (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, an effect driven by the behavioral response. Over twenty years, early-exposed mothers are eligible for $2,626 more in EITC benefits, which has a present value between $2,328 using a 5% discount rate.

Table C.6: Effect of Early Work Incentives on EITC Benefits

	Total (1)	Behavioral (2)	Mechanical (3)
0-4 Yrs From Birth * EarlyExp * NM	400.3***	186.2***	214.1***
	(45.0)	(39.5)	(16.1)
5-9 Yrs From Birth * EarlyExp * NM	92.9***	81.9**	11.0***
	(33.2)	(33.0)	(1.8)
10+ Yrs From Birth * EarlyExp * NM	-89.0**	-85.1**	-3.9***
	(33.7)	(34.0)	(1.2)
NM Mean 0-4 Yrs From Birth	1068.5	–	–
NM Mean 5-9 Yrs From Birth	1423.3	–	–
NM Mean 10+ Yrs From Birth	1280.4	–	–
Observations	2714475	2714475	2714475

Notes: This table presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on simulated EITC benefits. 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 19^{th} year after a first birth.

D Appendix to Section 6

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 D.1). 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.9 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.9. 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 D.1 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 D.2 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." 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 D.1: Long-Run 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
	(1)	(2)	(3)
A: Earnings			
PostBirth * EarlyExp * 10+ Yrs From Birth * NM	0.019	0.016	0.017**
	(0.012)	(0.011)	(0.008)
Mean Y	0.738	0.500	0.250
Individuals	2714475	2714475	2714475
B: Experience			
PostBirth * EarlyExp * 10+ Yrs From Birth * NM	0.028***	0.011	0.005
	(0.008)	(0.007)	(0.005)
Mean Y	0.719	0.470	0.214
Individuals	2714475	2714475	2714475

Notes: This table presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the likelihood of being at or above a threshold in the earnings (panel a) or experience (panel b) distributions. The thresholds are: top 75% (columns 1), top 50% (column 2), or top 25% (column 3). The distributions are defined separately for each year since first birth and include both married and never-married mothers. 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 19^{th} year after a first birth.

Table D.2: 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)
10+ Yrs From Birth * EarlyExp * NM	0.017**	0.000	0.010	-0.028***
	(0.007)	(0.002)	(0.010)	(0.008)
Mean Y	0.240	0.010	0.478	0.272
Observations	2714475	2714475	2714475	2714475

Notes: This table presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on the likelihood of having "high earnings" (top 25%) or "low earnings" (bottom 75%) crossed with indicators for having "high experience" (top 75%) or "low experience" (bottom 25%). Column 1 presents the effect on having "high experience and high earnings"; column 2 presents the effect on having high earnings and low experience; column 3 presents the effect on having "low earnings and high experience" and column 4 presents the effect on having "low earnings and low experience." See the text and Appendix D 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 19^{th} year after a first birth.

E Appendix to Section 7

E.1 Construction of Policy Database

The policy database includes quasi-experimental and experimental evaluations of the maternal labor market impacts of the following four categories of policies: child care (provision or subsidies); paid family leave; welfare reforms; and the EITC. In order to focus on higher quality results, we require the study be a recent working paper or be published in a top Economic field or general interest journal. The published studies in the final database are from the following journals: American Economic Association Papers and Proceedings; American Economic Journal: Applied Economics; American Economic Journal: Economic Policy; American Economic Review; Annals of Economics and Statistics; Econometrica; Journal of Development Economics; Journal of Human Resources; Journal of Labor Economics; Journal of Policy Analysis and Management; Journal of Political Economy; Journal of Population Economics; Journal of Public Economics; National Tax Journal; Quarterly Journal of Economics; the Review of Economic Studies; and the Review of Economics and Statistics. We include multiple estimates per paper if the paper evaluates more than one policy, subgroup, or time period.

We collected from each paper information on (i) treatment effects on women's labor market outcomes and the associated standard errors; (ii) average labor market outcomes; (iii) the treatment group; (iv) the duration of treatment at the time that the outcome was measured; and (v) the year of the policy. We obtain the percent treatment effect on wages/earnings as either the treatment effect on log wages/earnings (if available) or the treatment effect on level of earnings/wages divided by the mean of earnings/wages. We obtain the treatment effect on experience as either the treatment effect on experience (if available) or as the imputed treatment effect on experience. Imputed experience effects are calculated in two steps. First, we obtain the treatment effect on experience for each period of treatment as the running sum of the treatment effect on employment. For example, if the treatment effect on employment is β, the treatment effect on experience in the first period would be β, in the second period would be $2 \cdot \beta$, etc. Second, we obtain the average treatment effect on experience as the total of the treatment effects on experience in each period divided by the duration of treatment.[67]

Table E.1 reports on the studies in the database. The studies are roughly evenly divided across policy types; with 18, 20, 12, and 20 papers on the impacts of child care, paid leave, welfare reform, and the EITC, respectively. The median paper contributes 2, 1, 0, and 2 treatment effects on employment, percent earnings, experience, and imputed experience.

E.2 Comparison of Impacts on Experience Across Policies

We show in Section 7 that the increase in experience in our setting is much larger than any other policy. We now consider to what degree this reflects larger effects on employment versus a longer duration of treatment relative to other policies.

We first perform a graphical analysis of the differential impacts of these policies by comparing the distribution of impacts on employment by policy type. For easier aggregation, we limit this to the 164 estimates of the effect of a binary treatment (as opposed to, e.g., the impact of a \$1 increase in the policy, which are rare). Panel (a) of Figure E.1 below shows the kernel densities of the treatment effects by policy type, along with a dotted line to demarcate our short-run effects. Overall, there is significant overlap in treatment effects across policies, suggesting that the type of

[67] Reported and imputed treatment effects on experience have a correlation of 0.78 (among studies that report treatment effects on experience.

policy is not a strong determinant of the size of response. Notably, paid leave has more negative effects on employment (the distribution is shifted to the left), but the absolute effect sizes are similar to other policies, as shown in Panel (b) of Figure E.1). Relative to these effects, our estimate falls at the 75^{th} percentile of the absolute value of treatment effects. Thus, our EITC treatment generates a response that is moderately larger, but does not appear to be an outlier relative to other papers and policies.

Next, we study how our impacts on experience compare to other policies, and whether this is explained by our larger effects on employment. Figure E.2 shows that our impact on experience (shown on the y-axis) is much larger than other policies, even compared to those with a similar impact on employment (shown on the x-axis). This is primarily because our treatment lasts longer than other policies. Early-exposed mothers are "treated" by early EITC exposure for between 5 and 6 years (as shown by the gap in EITC benefits in Figure 1(b)). This is is greater than 75% of other policies. Moreover, our treatment effects on employment persist for 3 to 4 years beyond the gap in benefits, which puts the duration of our treatment effects in the top 10% of policies and further contributes to our impacts on experience. Thus, it appears possible for other policies to generate this size of impact on experience; it would just require a longer duration of treatment than is typical (e.g., free child care from childbirth to age six). (Note that the average impact of child care policies on experience 0.05 years.)

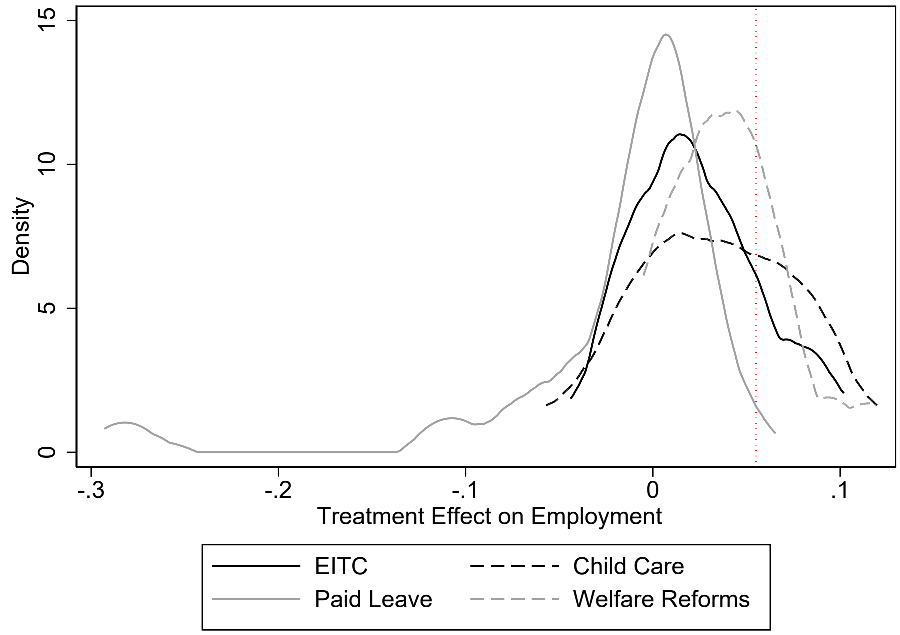
Table E.1: Studies in Policy Database

		Treatment Effect on:			
	Policy Type	Emp.	Pct. Earn	Exp.	Imputed Exp.
Baker et al. (2008)	Child Care	1	0	0	1
Bauernschuster and Schlotter (2015)	Child Care	5	0	0	5
Berger and Black (1992)	Child Care	1	0	0	0
Black et al. (2014)	Child Care	1	1	0	1
Cascio (2009)	Child Care	6	0	0	6
Cascio and Schanzenbach (2013)	Child Care	0	0	0	0
Duchini and Van Effenterre (2021)	Child Care	1	1	0	1
Fitzpatrick (2010)	Child Care	1	1	0	1
Fitzpatrick (2012)	Child Care	4	4	0	4
Gathmann and Sass (2018)	Child Care	2	0	0	2
Gelbach (2002)	Child Care	4	4	0	4
Gelber and Isen (2013)	Child Care	2	0	0	2
Havnes and Mogstad (2011)	Child Care	5	0	0	5
Herbst (2017)	Child Care	6	0	0	6
Kleven et al. (2021)	Child Care	0	3	0	0
Lefebvre and Merrigan (2008)	Child Care	4	4	0	4
Martínez and Perticará (2017)	Child Care	1	3	0	1
Sabol and Chase-Lansdale (2015)	Child Care	6	0	0	6
Bailey et al. (2019)	Paid Leave	4	4	0	4
Baker and Milligan (2008)	Paid Leave	3	0	0	2
Bana et al. (2019)	Paid Leave	1	1	0	1
Baum and Ruhm (2013)	Paid Leave	2	2	0	2
Berger and Waldfogel (2004)	Paid Leave	0	0	0	0
Blau and Kahn (2012)	Paid Leave	1	0	0	0
Campbell et al. (2017)	Paid Leave	4	5	0	4
Dahl et al. (2016)	Paid Leave	6	0	6	6
Frodermann et al. (2020)	Paid Leave	0	4	0	0
Han et al. (2009)	Paid Leave	6	0	0	6
Kleven et al. (2021)	Paid Leave	1	4	0	1
Lalive and Zweimuller (2009)	Paid Leave	2	2	2	2
Lalive et al. (2014)	Paid Leave	6	6	3	6
Lequien (2012)	Paid Leave	0	1	1	0
Rossin-Slater et al. (2013)	Paid Leave	3	3	0	3
Ruhm (1998)	Paid Leave	0	1	0	0
Schönberg and Ludsteck (2014)	Paid Leave	30	20	30	30
Stearns (2018)	Paid Leave	8	0	0	8
Timpe (2021)	Paid Leave	2	2	0	2
Waldfogel (1999)	Paid Leave	2	2	0	2
Bitler et al. (2003)	Welfare Reform	0	9	0	0
Bitler et al. (2006)	Welfare Reform	1	1	0	1
Bitler et al. (2008)	Welfare Reform	0	1	0	0
Blundell et al. (2016)	Welfare Reform	3	0	0	3
Card and Hyslop (2005)	Welfare Reform	1	1	1	1
Dyke et al. (2006)	Welfare Reform	0	0	0	0
Grogger (2003)	Welfare Reform	1	1	0	1
Hotz et al. (2002)	Welfare Reform	2	2	0	2
Hotz et al. (2006)	Welfare Reform	0	8	0	0
Low et al. (2020)	Welfare Reform	2	0	0	2
Michalopolous et al. (2005)	Welfare Reform	3	3	0	3
Milligan and Stabile (2007)	Welfare Reform	0	1	0	0
Bastian (2020)	EITC	3	2	0	3
Bastian and Jones (2021)	EITC	6	6	0	6
Bastian and Lochner (2021)	EITC	6	5	0	6
Bastian and Michelmore (2018)	EITC	4	0	0	4
Cancian and Levinson (2006)	EITC	1	0	0	1
Chetty and Saez (2013)	EITC	0	4	0	0
Chetty et al. (2013)	EITC	3	0	0	3
Dahl et al. (2009)	EITC	5	5	0	5
Eissa and Hoynes (2003)	EITC	2	0	0	2
Eissa and Liebman (1996)	EITC	1	0	0	1
Grogger (2003)	EITC	1	1	0	1
Hotz et al. (2006)	EITC	6	0	0	6
Hoynes and Patel (2018)	EITC	2	0	0	2
Jones and Michelmore (2018)	EITC	0	2	0	0
Kleven (2020)	EITC	12	0	0	12
Kuka and Shenhav (2022)	EITC	3	3	2	3
LaLumia (2013)	EITC	0	2	0	0
Meyer and Rosenbaum (2000)	EITC	1	0	0	1
Meyer and Rosenbaum (2001)	EITC	1	0	0	1
Neumark and Shirley (2001)	EITC	6	6	4	6
Total	–	207	141	49	204

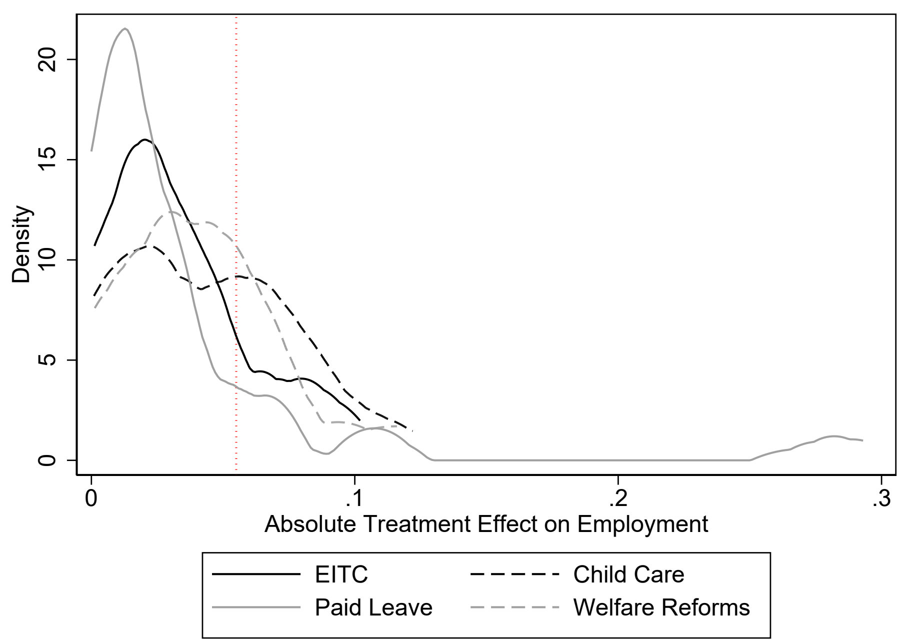
Notes: This table presents descriptive statistics on the policy database described above. Column 1 documents the list of studies; column 2 includes the category of policy (column 2); and columns 3–6 include the number of estimated treatment effects in employment, percent earnings, experience, and imputed experience, respectively.

Figure E.1: Distribution of Policy Impacts on Employment

(a) Treatment Effects on Employment

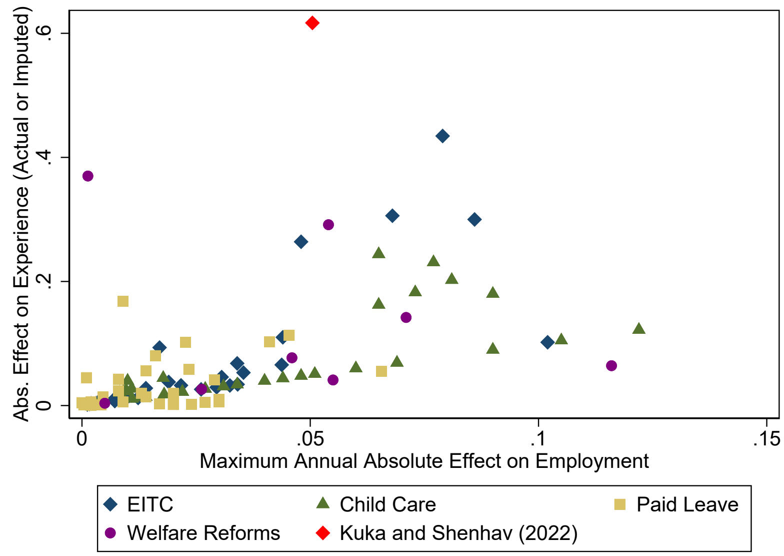


(b) Absolute Treatment Effects on Employment



Notes: This figure presents kernel densities of policy treatment effects on employment (panel a) and the absolute value of treatment effects (panel b) from studies in the policy database with a binary treatment variable. We show separate densities for each category of policy: the EITC (solid dark grey line); child care (dashed dark grey line); paid leave (solid light grey line); and welfare reforms (dashed grey line). For reference, our short-run effect on employment is shown in the vertical dashed red line.

Figure E.2: Larger Impact on Experience not Entirely Explained by Higher Effect on Annual Employment



Notes: This figure presents actual or imputed treatment effects on years of work experience (y-axis) and the maximum (absolute value) impact on an indicator for employment (x-axis) from studies in the policy database described in Section 7. Imputed experience effects are calculated in two steps. First, we obtain the treatment effect on experience for each period of treatment as the running sum of the treatment effect on employment. Second, we obtain the average treatment effect on experience as the total of the treatment effects on experience in each period divided by the duration of treatment. The beige square markers show estimated effects of paid leave; the blue diamond markers show estimated effects of the EITC; the green triangles show estimated effects of child care policies; the purple circles show estimated effects of welfare reforms; and the red diamond marker shows our estimated effects using the SSA administrative data. The vertical distance between markers shows variation in experience due to the duration of policies; while the horizontal distance between markers shows variation in experience due to larger impacts on employment.