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Long-Run Effects of Incentivizing Work after Childbirth†

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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 percent higher earnings conditional on working. We show that higher earnings are primarily explained by improved wages due to greater work experience. (*JEL* H24, H31, J16, J22, J31)

The substantial and persistent "child penalty" in women's earnings has been widely documented.[1] However, the source of this penalty, particularly for mothers who 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, Rute Cardoso, and Kline 2015). On the other hand, new mothers may obtain a *higher* return to experience if work after childbirth signals commitment to employers (Tô 2018);

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[1] See Angelov, Johansson, and Lindahl (2016); Chung et al. (2017); Kleven, Landais, and Søgaard (2019); Kuziemko et al. (2018); Nix and Andresen (2019); Kleven et al. (2019).

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

leads to greater on-the-job training (Thomas 2019); 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 percent, 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) 23 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 mothers who have never married (at the time of the survey) 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 three to six years after a first birth ("late-exposed").[3] This difference-in-difference comparison captures variation in work 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

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

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 (in real 2016 dollars) more on average in the long run. This is 4.2 percent higher than the average earnings of late-exposed mothers who are employed, or 6 percent 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*.

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 percent) 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, Dustmann, and Stevens 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 in the long run due to investments made in the short run (which could facilitate, e.g., better health); however, we argue that the lack of any long-run impact on employment makes this less likely.

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.[4]

[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.

While our results document earnings gains for women who are incentivized to work after childbirth, it is worth noting that the welfare implications of this policy remain unclear. As one input for this calculation, we provide a back-of-the-envelope estimate that early-exposed mothers' total *net income* is expected to rise by roughly \$16,620 in the long run (70 percent of the effect on total labor earnings), taking into account changes in taxes, transfers, and child costs. We leave a full welfare accounting of this policy for future work.

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 percent 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 nonemployment *beyond* the first year after a first childbirth. Our impact on experience accrues over the first nine years after birth and is at least twice as large as the effect of other maternal employment policies. Third, we can estimate long-run impacts on *wages* because we find convergence in employment and hours (in contrast to 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 on 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 percent 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 percent return using variation in experience across synthetic cohorts of US single mothers; Gladden and Taber (2000), who estimate a 4 to 5 percent return for low-educated US women using an IV approach; Adda, Dustmann, and Stevens (2017) who estimate a 9 to 12 percent 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 United States and estimate an insignificant −3 percent and significant 13 percent return, respectively. However, these estimates

[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, Ruhm, and Waldfogel (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 work experience. Lundborg, Plug, and Wurtz Rasmussen (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.

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, Dustmann, and Stevens 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.

Finally, we show that US 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]

I. 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 nonlinearly with the number of qualifying children and earnings in a household (e.g., see panel A of online 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 percent of EITC dollars (Bitler, Hoynes, and Kuka 2017). Married couples with children make up the second-largest group, and receive 20 percent 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 from $2,381 to $3,300 (in real 2016 dollars), and augmented benefits at every level of eligible earnings.[10] These additional benefits are substantial, representing 8 percent income growth for the lowest-income households, or the equivalent of an additional month's wages (see panel B of online 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 percent growth in benefits.

A. *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

[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, Whitmore Schanzenbach, and Almond. (2016) and Bailey et al. (2020); for Medicaid, see Goodman-Bacon (2021) and Brown, Kowalski, and Lurie. (2019); and for Head Start, see Bailey, Sun, and Timpe (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.

[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.

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 increase when a family has a second child, 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 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 one and two, \$500 to \$800 in years three and four, and zero in year six.[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 three, 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 20 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.

[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., online Appendix Figure A.15.

[12] Early exposure does not change birth spacing. See Section VI.

[13] Seventy-fifth percent 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 noneligible mothers face the same incentives (e.g., the same schools and tax policy).

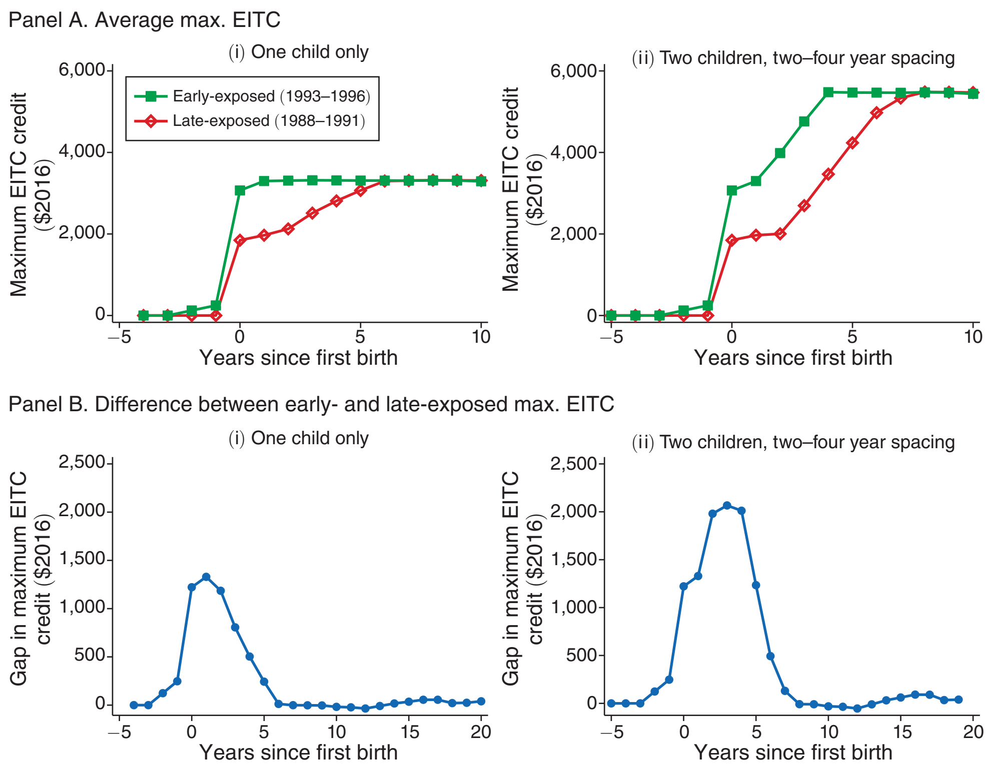

Figure 1. Illustration of the Gap in the Maximum EITC between Early- and Late-Exposed Mothers

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 one child, and subfigure (ii) assumes that mothers have two children spaced with a uniform probability between years two and four (such that the average spacing is three years).

Data: Nominal EITC benefits are published by the Tax Policy Center, and have been converted to 2016 real dollars using the CPI from the Bureau of Labor Statistics (Tax Policy Center 2023).

B. *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 (through the Aid for Families with Dependent Children (AFDC) program) 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 generosity 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 online Appendix Table A.5).[15, 16]

II. 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 has 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 the 1040-SE from. 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 (US Bureau of Labor Statistics 2018). 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.[17] If an individual has positive total earnings, we consider her to be employed for that 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

[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 online Appendix C for details.

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

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

mismeasurement and evidence that this is not empirically relevant for our results in Section IIIA, 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 online Appendix B.1). These outcomes allow us to explore intensive margin employment responses, which is not possible with administrative data. 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 IVA).

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.[18]

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 percent and 80 percent 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 online 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 (based on marital status at survey) 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 percent 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 online 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.

[18] In the few cases where the implied age from the Numident differs by more than five years from the age in the CPS, we instead use the CPS age − year − 1.

For analogous reasons, we identify married mothers as a "low-impact" sample. Based on pre-birth household earnings, 49 percent of married households are likely to be eligible for some EITC benefits. However, 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 percent increase in her earnings post-reform. Incorporating spousal earnings would further reduce the expected increase in benefits. We discuss the advantages and limitations of using married women as a comparison group, and robustness to alternative comparison groups, in Section IIIA.

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 online Appendix Table A.1 for summary statistics.

State-Level Controls.—We obtain annual measures of state-level economic conditions and policy parameters from Kuka (2019) and 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).[19]

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 IIIA). Our sample consists of SIPP mothers who gave birth in the same years as our core sample.

III. 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:

$$Y_{imb\tau} = \alpha + \sum_{k \neq -1} \beta_{k,DDD} \cdot \mathbf{1}\{\tau = k\} \cdot EarlyExposed_b \cdot NM_m$$
$$+ \theta_{b\tau} + \phi_{m\tau} + \lambda_{bm} + \gamma_m \mathbf{X}_{is\tau} + \delta_m \mathbf{P}_{s\tau} + \epsilon_{imb\tau}.$$

The term $Y_{imb\tau}$ is an outcome for mother i with marital status m, whose first child is born 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

[19] 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 noncompliance 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.

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 to the difference in outcomes in the year before childbirth. Importantly, our inclusion of married mothers as an additional comparison allows us to include fixed effects for a full set of two-way interactions, including child-birth-year-by-event-time $(\theta_{b\tau})$, marital-status-by-event-time $(\phi_{m\tau})$, and child-birth-year-by-marital-status (λ_{bm}) fixed effects. The first set of these, $\theta_{b\tau}$, is crucial for identification, as it controls for year- and child-age specific shocks to labor market outcomes that are not due to the timing of exposure to expanded work incentives. 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 $\mathbf{X}_{is\tau}$, and state-level policy covariates, $\mathbf{P}_{s\tau}$. $\mathbf{X}_{is\tau}$ includes fixed effects for a 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. $\mathbf{P}_{s\tau}$ 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:

$$Y_{ib\tau} = \alpha + \sum_{k \neq -1} \beta_k \cdot \mathbf{1}\{\tau = k\} \cdot EarlyExposed_b + \theta_\tau + \chi_b + \gamma \mathbf{X}_{is\tau} + \delta \mathbf{P}_{s\tau} + \epsilon_{ib\tau}. \tag{2}$$

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 V.

A. *Identification Assumptions and Tests*

Our identification relies on the assumption that there are no other factors outside of the change in work incentives that differentially affected the post-birth outcomes of never-married women who were exposed early to the EITC reform. This assumption is inherently untestable; however, we probe its credibility to the best of our ability. First, we use data from years prior to childbirth and prior to the reform to analyze outcomes for women not (yet) exposed to the reform. Second, we provide evidence in favor of using married mothers as a counterfactual for the change in the post-childbirth shock to never-married mothers' outcomes over time.

Pre-reform Outcomes.—We examine pre-reform trends in two ways. First, we test whether the labor market outcomes of early-exposed never married mothers were diverging relative to comparison mothers *prior to* childbirth. Second, we study whether the timing of the improvement in never-married mothers' outcomes aligns with the timing of the reform. Both of these tests pass easily in Section IV and online Appendix C.1, respectively, suggesting that the gains in never-married mothers' outcomes coincided precisely with the expansion in work incentives.

Comparability of Married and Never-Married Mothers.—We provide four empirical facts in favor of using married mothers as a comparison group. First, online 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).[20] This includes a nearly identical "child penalty."[21] Moreover, these employment patterns for these pre-reform mothers continue to track closely even after childbirth (see Section V).

Second, prior work documents that never-married and married women have similar labor supply elasticities (Blau and Kahn 2007; Heim 2007; Bishop, Heim, and Mihaly 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. Online 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).[22]

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

[20] 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 (1986–1987, 1988–1989, 1990–1991) and marital status.

[21] 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.

[22] 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.

particularity of married women. Our results are very similar across these comparisons (see Section V). 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.

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 at the time 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 V).

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, online 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 percentage points) and statistically insignificant.[23]

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. Online 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 V).

[23] 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.

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 V.) Second, we may misassign child birth order since some children may have left home by the time mothers are surveyed. We test for this in Section V by restricting our sample to women surveyed at younger ages and find similar results.

IV. 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, Lange, and Notowidigdo 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 percentage points for both groups, a 20 percent decline from pre-birth levels. In the following year, late-exposed mothers' employment falls 7 percentage points 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 10 to 19 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 percentage points higher employment, which grows to 8 percentage points 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

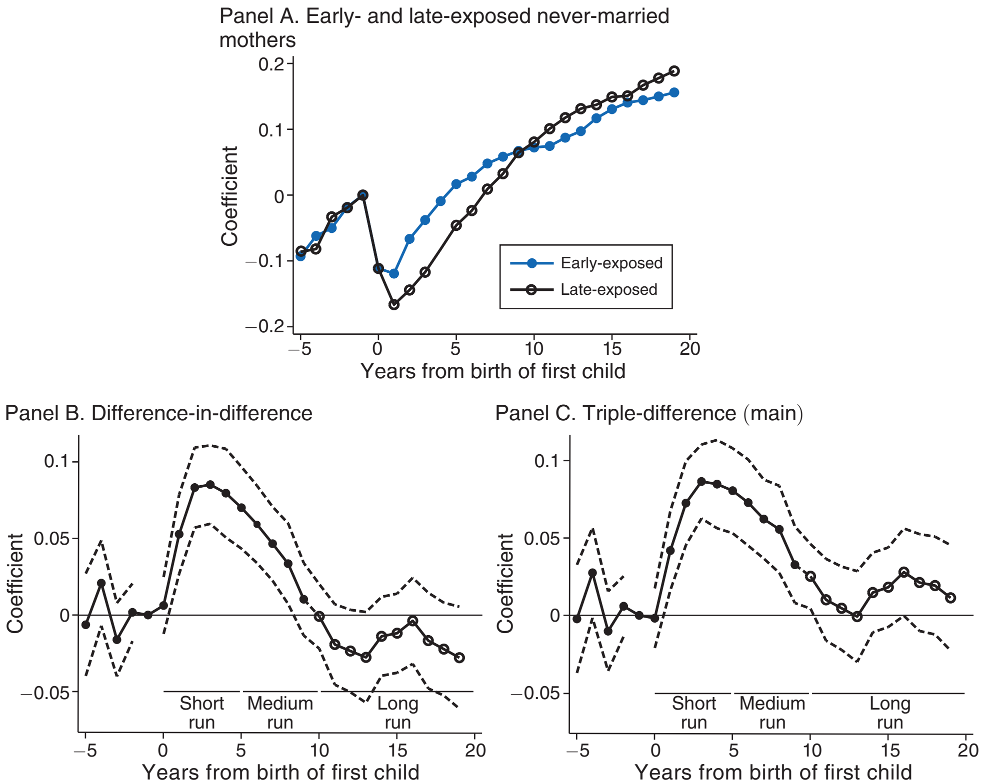

FIGURE 2. EFFECT OF EARLY EXPOSURE TO WORK INCENTIVES ON EMPLOYMENT

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 percent 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 (US Census Bureau and Social Security Administration 2016). All dollar values have been converted to 2016 dollars using the CPI from the Bureau of Labor Statistics. 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 five years prior to a first birth up to the nineteenth year after a first birth.

our expectations. Estimated effects on employment shrink to zero over the medium run, and become slightly positive thereafter.[24] This indicates that early exposure does not have a lasting impact on employment.

[24]We suspect that the modest long-run fluctuations, and the slight difference in results between the DD and 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 V). 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.

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

	Employed (1)	Years of experience (2)	Earnings (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	2,617.6
	(0.010)	(0.070)	(526.6)
10+ *yrs from birth* × *EarlyExp* × *NM*	0.010	0.617	1,392.7
	(0.011)	(0.129)	(587.3)
Mean *Y*	0.765	0.765	23,612.672
Observations	2,714,475	2,714,475	2,714,475

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 (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.

Sources: 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 five years prior to a first birth up to the nineteenth year after a first birth.

Column 1 of Table 1 presents our DDD estimates for employment. Never-married mothers' post-birth employment increases by 5.5 percentage points ($p < 0.01$) in the short run; which represents a 8.7 percent increase relative to late-exposed mothers' employment and a 27 percent recovery relative to the drop in employment in the year after birth.[25] In the medium run, early-exposed women have 5.5 percentage points higher employment rate per year. This difference fades to an insignificant 1 percentage point 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.[26] Panel A of Figure 3 and column 2 of Table 1

[25] The short-run impact on employment is smaller (3.7 percentage points) 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 online Appendix C, we provide additional evidence on the short-run responses to the reform.

[26] 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.

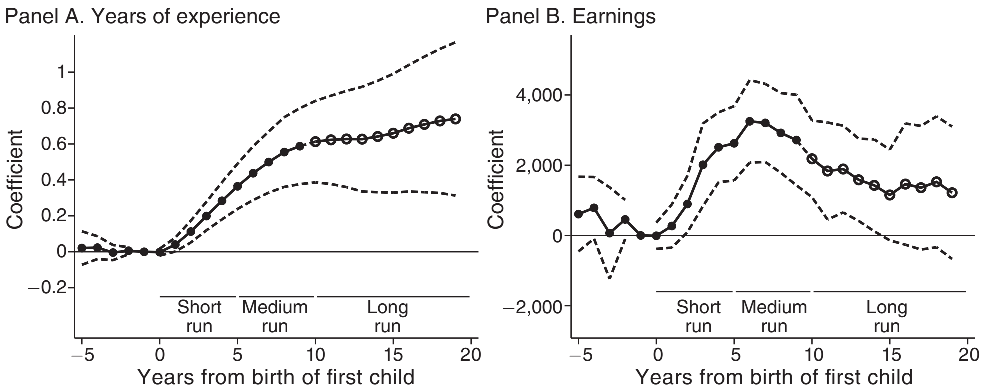

Figure 3. Effect of Early Work Incentives on Experience and Earnings

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 percent 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 five years prior to a first birth up to the nineteenth year after a first birth.

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 percent 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 IVA, 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 nonmonotonicities over time, and remain positive and often statistically significant over the long run.[27,28] Hence, early-exposed mothers have long-lasting 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 percent) is due to increases in pay from employers (see online Appendix Table A.3). Relative to the average annual earnings of late-exposed mothers, these

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

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

estimates imply that early-exposed mothers experience a 17 percent earnings gain in the medium run and a 6 percent 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 online Appendix Table A.3 (columns 1–6). The estimate for earnings conditional on working represents a 4.2 percent 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 percent of log earnings to limit the influence of outliers also makes little difference (columns 7 and 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 online Appendix Figure A.6).[29] 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.

While our goal is not to assess the welfare implications of this policy, a basic question that arises is whether higher earnings amounts to greater *income* over the long run. Cumulatively, we estimate that early-exposed mothers earn \$37,945 more over 20 years after a first birth, or \$23,307 in present value terms if we use a 5 percent discount rate. In online Appendix Section C.5, we calculate expected changes in taxes, transfers from the EITC and other government programs, and child care costs. In total, we project that early-exposed mothers' *net income* increases by \$16,620 in present value terms, 39 percent of which is accrued over the long run.[30] This suggests that early work experience is likely to be financially advantageous, in large part due to higher earnings experienced later in one's career.

A. *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 at each child age relative to married mothers.[31] 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.

[29] The figure shows estimates from regressions where the outcomes are indicators for having earnings above X, with $X = 0, 2{,}500, 5{,}000, \ldots, 80{,}000$, i.e., $1 - \text{CDF}$ (Duflo 2001).

[30] If we further assume that these effects on income are solely a response to the EITC, we can calculate the fiscal impact of the EITC expansion using the Marginal Value of Public Funds (MVPF). See online Appendix C.5.

[31] We estimate the double-difference as: 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.

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

	Hours last week (1)	Annual hours (2)	Cumulative hours (3)	Years FT FY experience (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	1,157.171	0.827
	(1.239)	(63.348)	(476.681)	
10+ *yrs from birth* × *EarlyExp* × *NM*	0.277	6.897	1,277.615	0.913
	(1.545)	(76.599)	(943.107)	
Individuals	94,414	94,414	94,414	

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 1,400 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 five years prior to a first birth up to the nineteenth year after a first birth.

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, Katz, and Kearney 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).

B. *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 mothers earn 4.2 percent higher *wages*.

V. Robustness

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

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 and 42 without any children in the household. To assign a fake year of childbirth, $\hat{b}$, we follow Kleven, Landais, and Sögaard (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 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.[32]

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

[32] 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 percent confidence interval over the estimated effects.

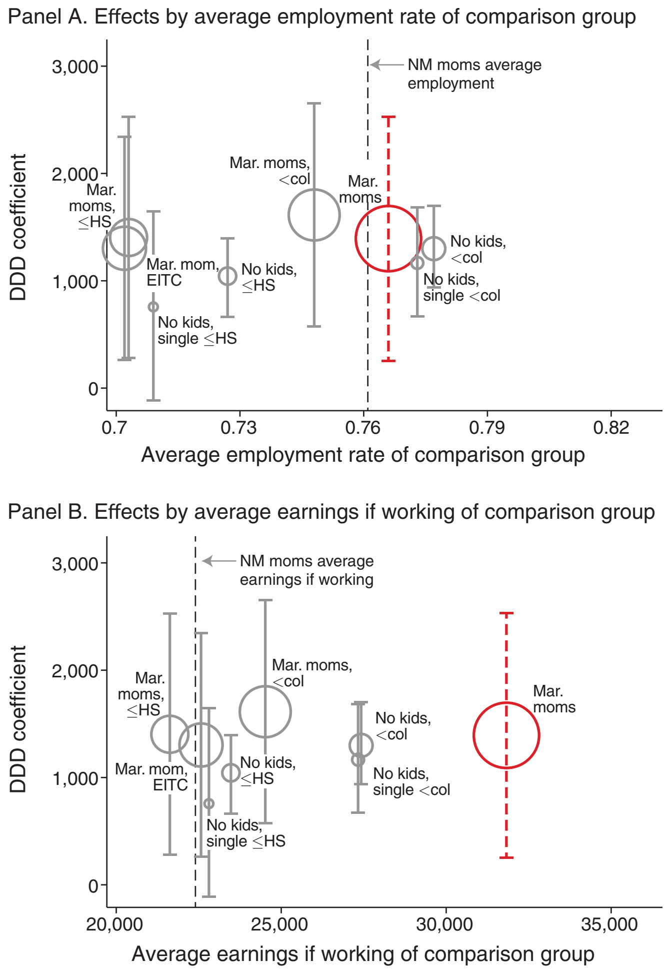

Figure 4. Long-Run Effects on Earnings across Comparison Groups

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 percent confidence intervals, for each DDD comparison group (indicated by the marker label). Specifically, each marker shows the coefficient and 95 percent confidence intervals from the interaction $10+$ *yrs from birth* $\times$ *EarlyExp* $\times$ *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 gray 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 percent 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."

$756 to $1,613, and fit comfortably within the confidence interval of our main estimate (shown in red).[33] Further, there is no systematic relationship between the

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

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.

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 birth in the surrounding years (i.e., 1986–1987 and 1997–1999). Similar to the main analysis, we omit the year prior to the earliest childbirth in each group.

Consistent with our main results, online 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 online Appendix Figure A.9).[34]

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 online 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.

Alternative Controls for Economic Conditions.—Third, while our main specification controls for economics conditions with year effects and local unemployment rates, one could worry that there remain some economic advantages for early-exposed mothers that are not captured by our model. To allay such concerns, we show that our results remain unchanged when we control more flexibly for economics conditions. We allow the impact of unemployment rates and welfare reform to vary with the age of one's child (see online Appendix Table A.5), and either add 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 online 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 online Appendix Table A.6); allow the effect of mother's age to vary with the age she first gave birth; add individual fixed

[34] 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.

effects; or restrict the sample to mothers who are CPS heads of household (see online 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 online 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 online Appendix Figure 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 online 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 online Appendix Figure A.13. The one-sided p-value for long-run earnings is 0.02.

VI. Why Do Early-Exposed Mothers Earn More?

A. *Mechanisms*

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.[35] 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 percent to 5.1 percent between years 10 and 19 after a first birth).

[35] 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 online Appendix Table A.8.

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 percent confidence intervals) on indicators for these four outcomes: having high earnings and high experience, high earnings and low experience, low earnings and high experience, and low experience and low earnings.[36] 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 percent 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 percent 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 to 6 percent), 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.[37]

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 percent/0.91) and 6.8 percent (4.2 percent/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, there are few comparisons for these estimates, particularly for similar populations. However, for context, we briefly review and contrast the features and estimates from the closest papers in Section VIB.

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

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

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

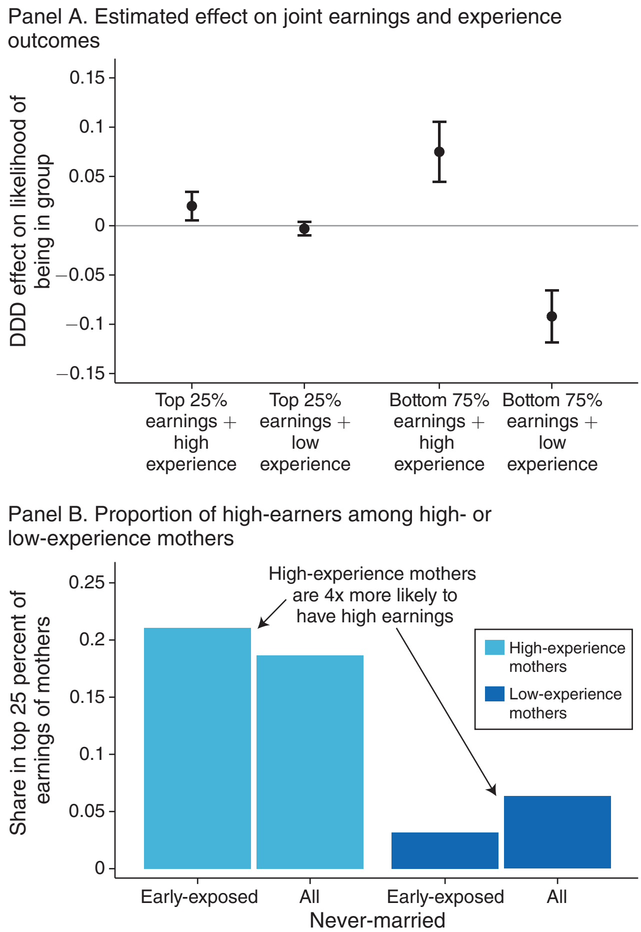

Figure 5. Effect of Early Work Incentives on Jointly Having High Earnings (Top 25 Percent) and High Experience (Worked in Three Years Post-Birth)

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 percent 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 percent or bottom 75 percent 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 and not having worked in each of the three years after a first birth, 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 online 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.

mothers who had a first birth in 1988–1989 (exposed at child age 4–5); 1990–1991 (exposed at child age 2–3); 1992–1993 (exposed at child age 0–1); 1994–1995 (exposed at birth); and 1996 (exposed at birth), relative to mothers who had a first birth in 1986–1987 (exposed at child age 6–7).[38] Online 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, online 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.[39] However, the estimates are imprecise, so we can not reject nonlinear 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, Dustmann, and Stevens (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 online Appendix Table A.10). In the long run, early-exposed mothers are 4 percentage points more likely to be in health occupations ($p < 0.05$) and 5 percentage points less likely to be in clerical occupations ($p < 0.1$). However, we find inconsistently signed and noisily estimated changes across the 13 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.[40,41]

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, Dustmann, and Stevens (2017) find that annual skill depreciation is low (less than 1percent 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 online 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 percent effect).[42]

[38] 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.

[39] Deviations from the best-fit line could suggest that there is a secondary mechanism operating for some cohorts (e.g., mothers with a first birth in 1990–1991) or could reflect noise in the data.

[40] In order to explain the entire increase in long-run earnings, the average earnings in health services would have to be \$34,825 (1,393/0.04) *higher* than in early-exposed mothers' other occupations.

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

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

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, Miller, and Simon 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, Johnston, and Shields 2014). The fact that we do not find any such effects suggests that these improvements are likely to have muted effects on wages.

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

B. *Comparison to Other Policies*

To provide further context for these experience effects, we close by studying how our setting and estimated return compare with three other maternal employment policies that also generate a statistically significant change in experience.[43] Table 3 summarizes the features of these studies, and highlights several respects in which our study is unique relative to earlier work. First, no other paper focuses on low-income women in the United States (columns 2–4). One other paper, Card and Hyslop (2005), studies a similarly low-income population (welfare recipients in Canada), but nonetheless the sample is quite different from ours, with a significantly lower employment rate (19 percent versus 66 percent) and higher average age (32 versus 24 years). Second, our impact on experience is at least twice as large as the effect in any other study, and is precisely measured due to our use of administrative records. This provides us with more power to identify impacts on earnings. Last, our return to experience is much higher than the one other estimate for low-income women (−3.2 percent), shown in the final column. This may be because experience is more valuable for women who are younger or more attached to the labor force (who are more prevalent in our sample), or because working after childbirth provides a costly signal to employers of one's commitment to work (Thomas 2019; To 2018). Our estimate overlaps with the range of returns from paid leave, although the span of these estimates is wide and may incorporate other mechanisms, such as job protection. Overall, the patterns lead us to speculate that the magnitude and timing of experience may be more relevant for the return rather than the particular policy. However, we leave more rigorous investigation of this question to future research.

[43] Specifically, we focus on studies that evaluate the effects of child care, paid leave, welfare reforms, or the EITC on maternal employment using quasi-experimental or experimental methods, report a significant effect on work experience, report effects on earnings conditional on working, and are published in a top field or general interest journal in economics.

Table 3—Return to Experience across Policies

	Policy environment		Data	Population, baseline			Treatment effects [range] (SE)		
	Policy type	United States	Admin. data	Low-income	Working (%)	Mean age	Experience (years)	Earnings if working (%)	Return per year (%)
Lequien (2012)	Paid leave	N	Y	N	100	—	−0.27 (0.04)	−2.8 (1.3)	10.5
Schönberg and Ludsteck (2014)	Paid leave	N	Y	N	100	—	[−0.24, −0.06]	[−5.0, 0.5]	[−24.6, 42.9]
Card and Hyslop (2005)	Welfare reform	N	N	Y	19	31.9	0.37 (0.05)	−1.0 (2.0)	−3.2
This paper	EITC	Y	Y	Y	66	23.6	0.62 (0.13)	4.2 (2.1)	[4.6, 6.8]

Notes: This table summarizes the policy environment, data source, population characteristics, and treatment effects for the selected papers. The four papers in the table meet the following criteria: (i) examine the effects of child care, paid leave, the EITC, or welfare reform on maternal employment; (ii) report significant effects on work experience; and (iii) report effects on earnings conditional on working (or earnings, if there is no impact on employment). Column 1 reports the policy type; column 2 reports whether the policy took place in the United States; column 3 reports whether the study used administrative data on labor market outcomes; column 4 reports whether the policy targets a low-income population; columns 5 and 6 report the baseline employment rate and sample age, respectively; columns 7 and 8 report the estimated treatment effect and standard errors on work experience and earnings, or, if there are multiple policies, a range of estimates in brackets; and column 9 reports the return per year of work experience as reported in the paper or based on the ratio of columns 8 and 7. "Y" indicates that a feature is present; "N" indicates that a feature is not present; and "—" indicates that the information is not available.

VII. 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.

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