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

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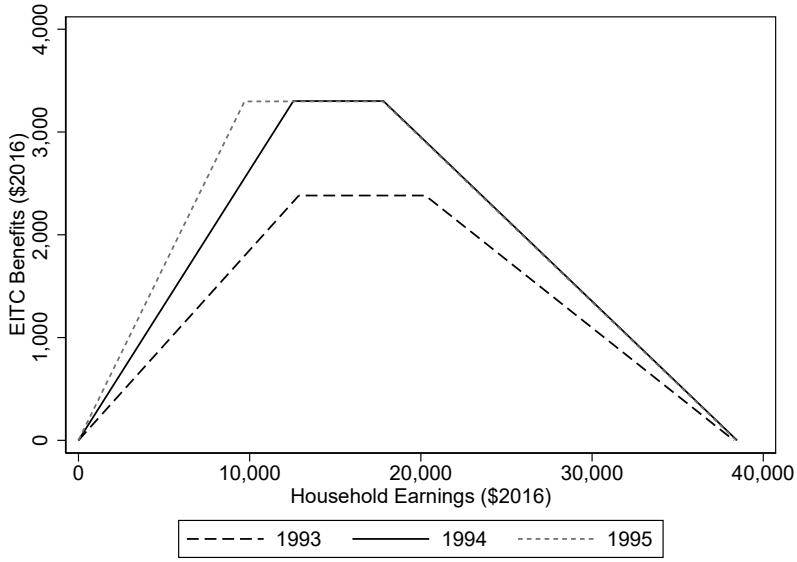
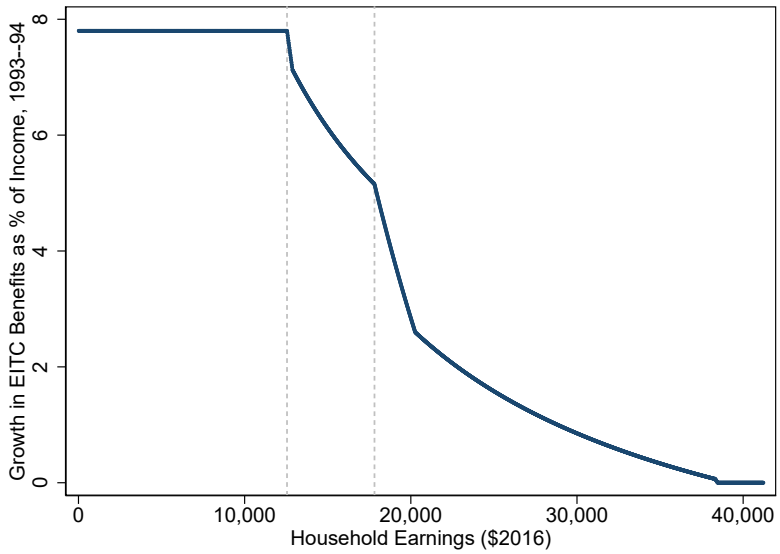
(a) 1993, 1994 and 1995 Benefit Levels

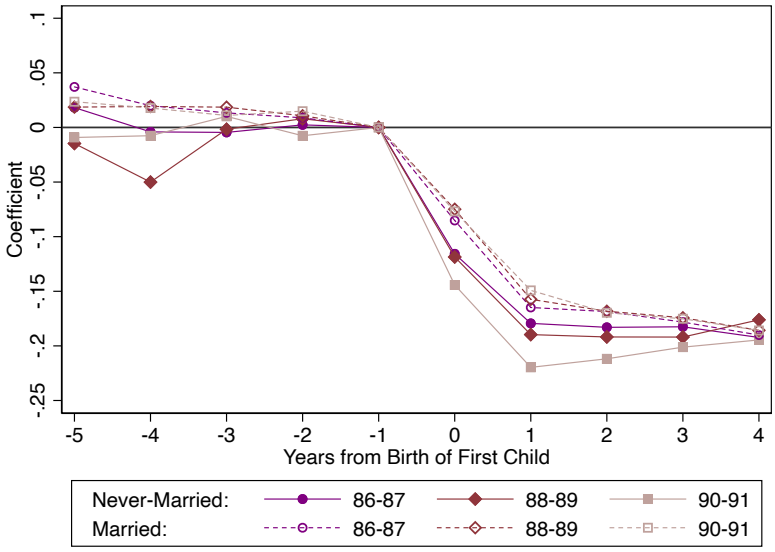


(b) 1993-1994 Change in Benefits as % of Earnings



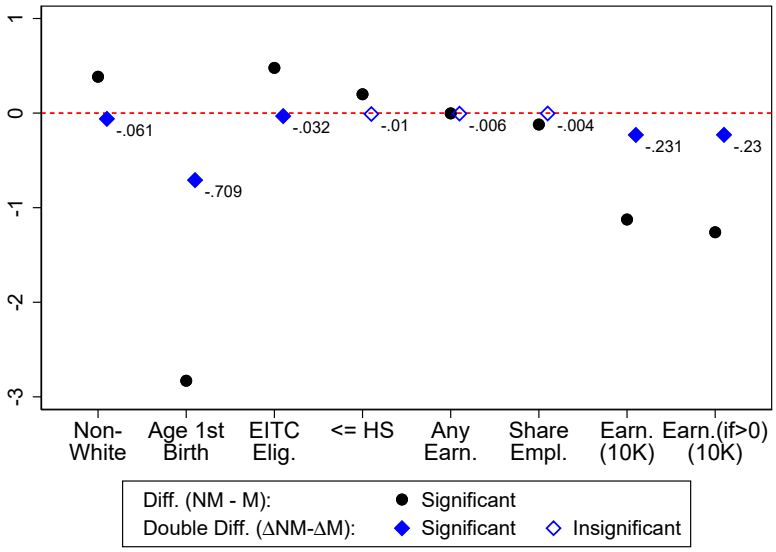
Notes: Panel (a) shows EITC benefits by the level of household earnings for one-child households in 1993, 1994, and 1995. Panel (b) shows the difference between 1994 and 1993 benefits as a share of household income. *Data:* Nominal EITC benefits are published by the Tax Policy Center, and have been converted to 2016 dollars using the CPI from the Bureau of Labor Statistics (Tax Policy Center, 2023).

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



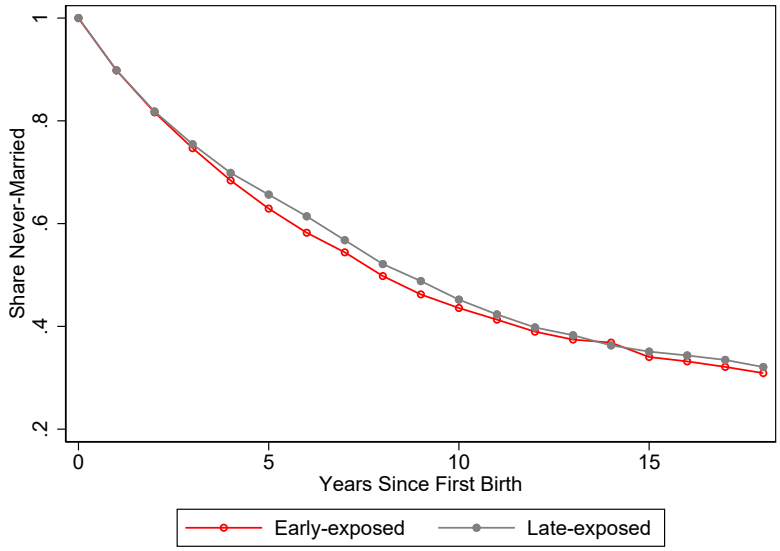
Notes: This figure presents event studies of employment around birth, along with 95% confidence intervals, for never-married and married women who had a first birth prior to the 1993 EITC reform. See the notes of Figure 2 for information on control variables, standard errors, data and sample construction. *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.

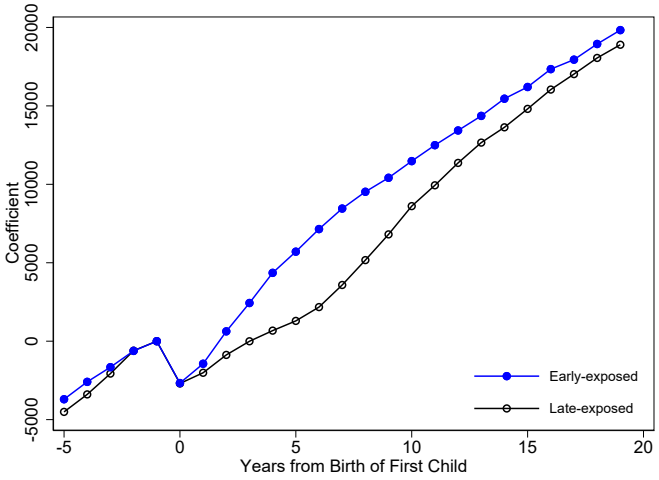
Figure A.4: Share of Mothers Remaining Never-Married in Each Year Since First Birth (SIPP)



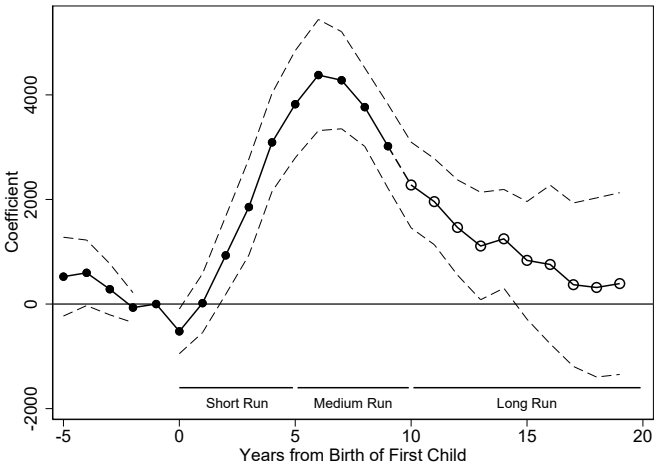
Notes: This figure presents the share of mothers who were never-married at first birth that remain single in each year since first birth. We plot this separately for mothers exposed to work incentives early (in the year of first childbirth) and late (3–6 years after childbirth)). We estimate the gap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



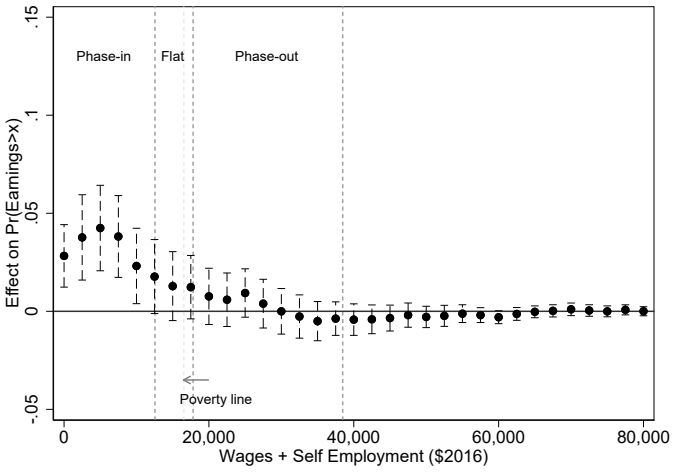
(b) Difference-in-Difference



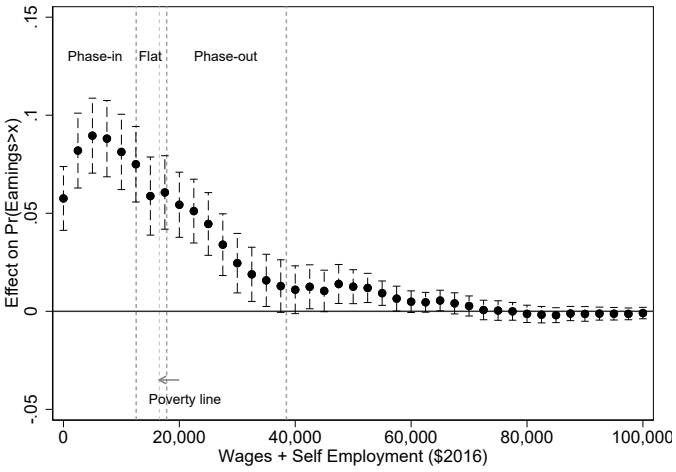
Notes: These figures present the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on earnings in each year from birth. Panel (a) plots event studies of employment around childbirth estimated separately for early- and late-exposed never-married 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

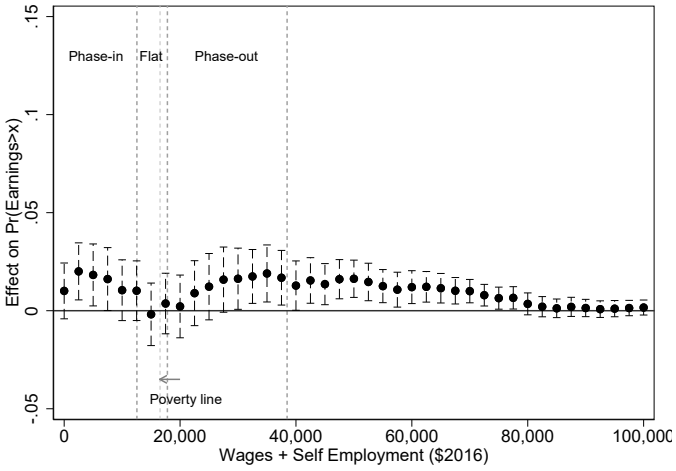
(a) Earnings in Years 0-3



(b) Earnings in Years 4-9

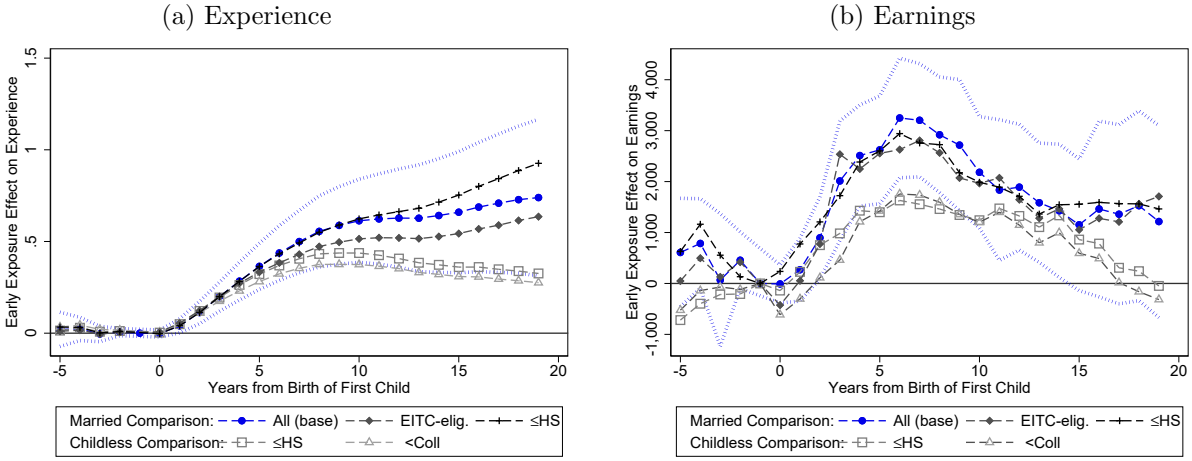


(c) Earnings in Years 10+



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. We use the nominal EITC benefits published by the Tax Policy Center (Tax Policy Center, 2023). *Years:* We include data from 5 years prior to a first birth up to the 4^{th} year after a first birth.

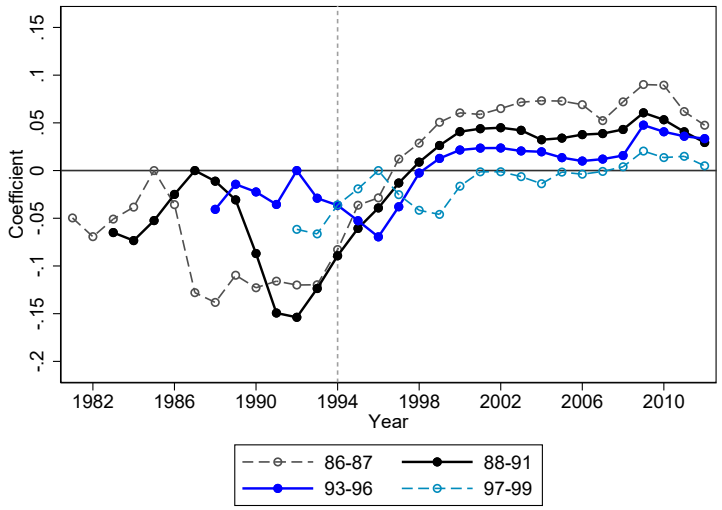
Figure A.7: Long-Run Effect of Early Work Incentives Using Alternative Comparison Groups



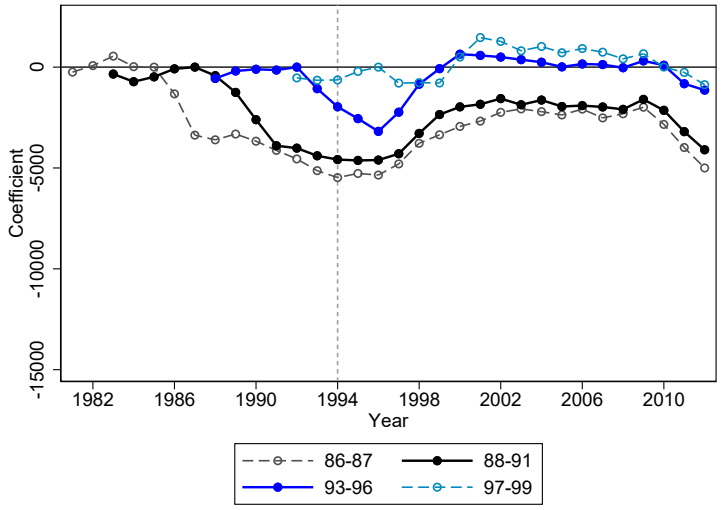
Notes: This figure presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) in each year from birth on years of experience (panel a) or earnings (panel b), along with 95% confidence intervals. Estimates come from the dynamic DDD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



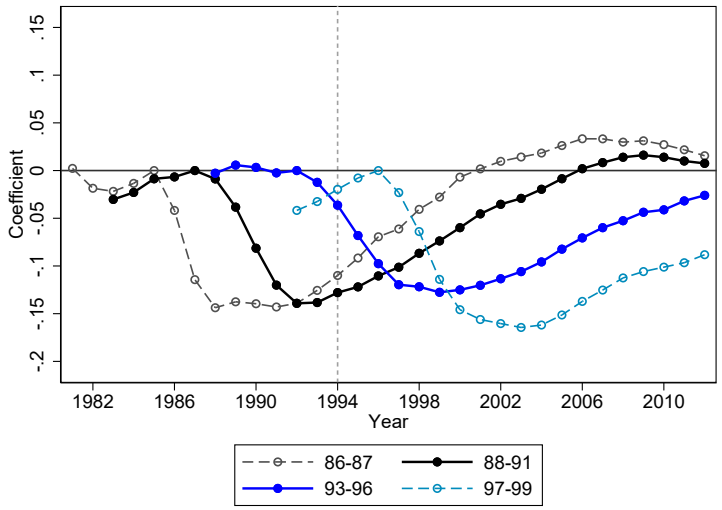
(b) Earnings



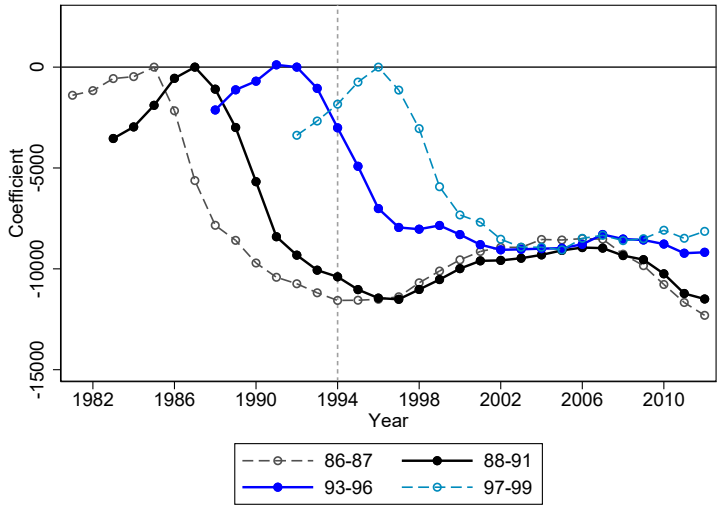
Notes: These figures shows calendar-year event studies of employment (panel a) or earnings (panel b), along with 95% confidence intervals, for groups of never-married mothers who were exposed to work incentives early (first birth: 1993–1996), late (first birth: 1988–1991), very late (first birth: 1986–1987) 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



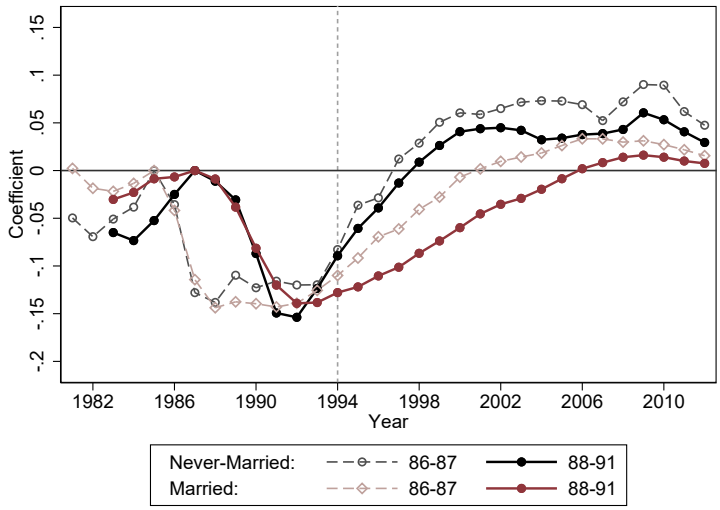
(b) Earnings



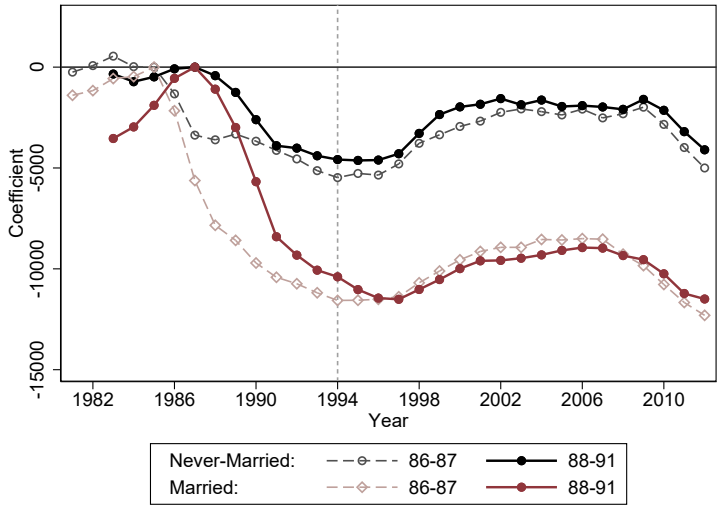
Notes: These figures shows calendar-year event studies of employment (panel a) or earnings (panel b), along with 95% confidence intervals, for groups of married mothers who were exposed to work incentives early (first birth: 1993–1996), late (first birth: 1988–1991), very late (first birth: 1986–1987) 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



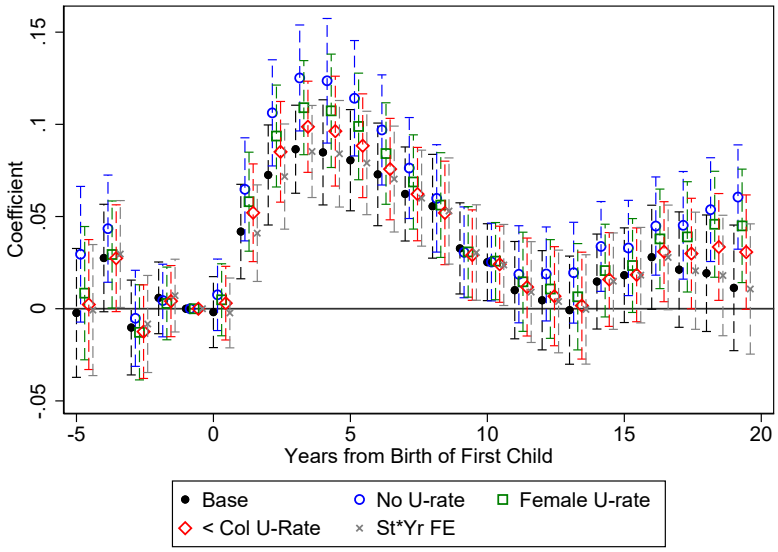
(b) Earnings



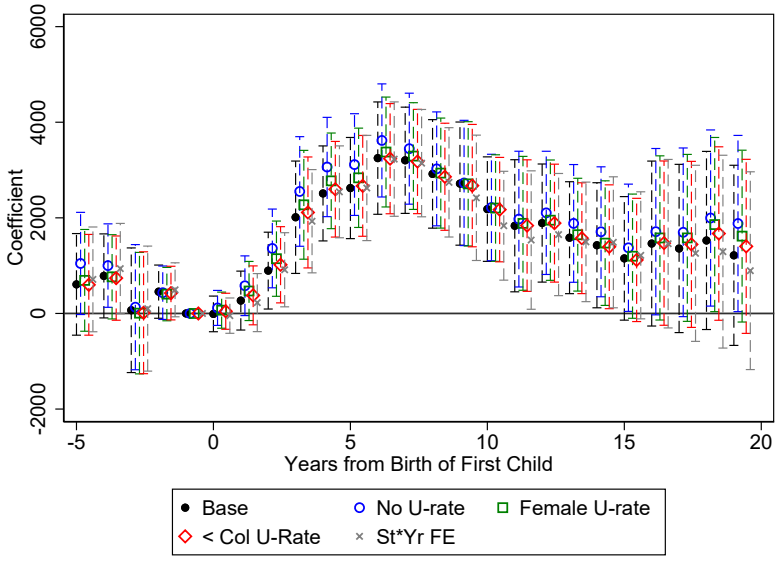
Notes: These figures calendar-year event studies of employment (panel a) or earnings (panel b), along with 95% confidence intervals, for groups of never-married and married mothers who were exposed to work incentives after birth, either late (first birth: 1988–1991) or very late (first birth: 1986–1987). 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

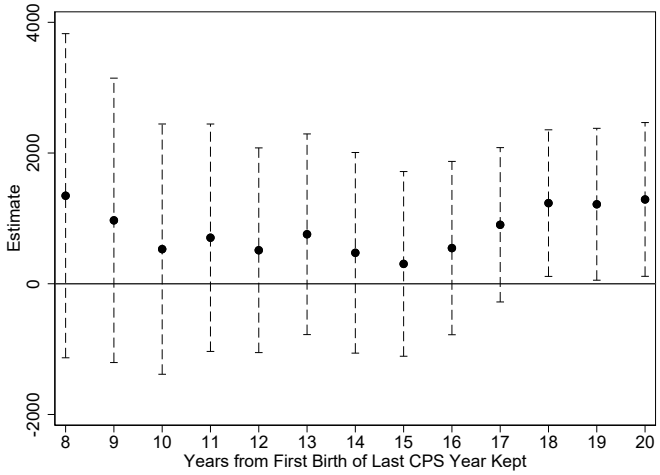


Notes: These figures present the sensitivity of the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on employment (panel a) or earnings (panel b), along with 95% confidence intervals, in each year from first birth. Each panel shows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

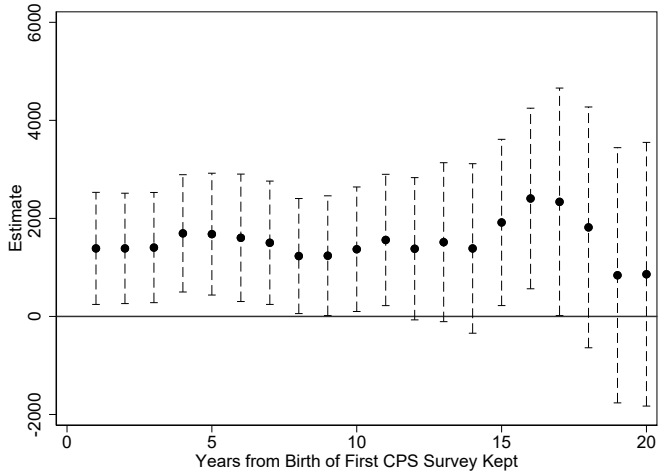
Figure A.12: Effect of Early Work Incentives on Long-Run Earnings, Sensitivity to:

Keeping CPS Surveys

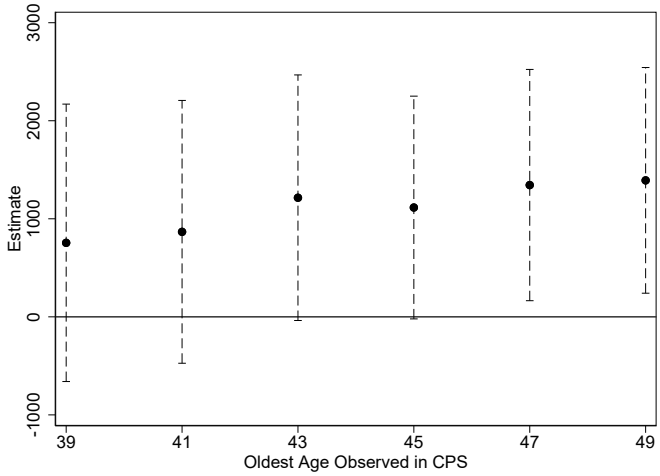
(a) *At most* from 8 to 20 Years of Birth



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

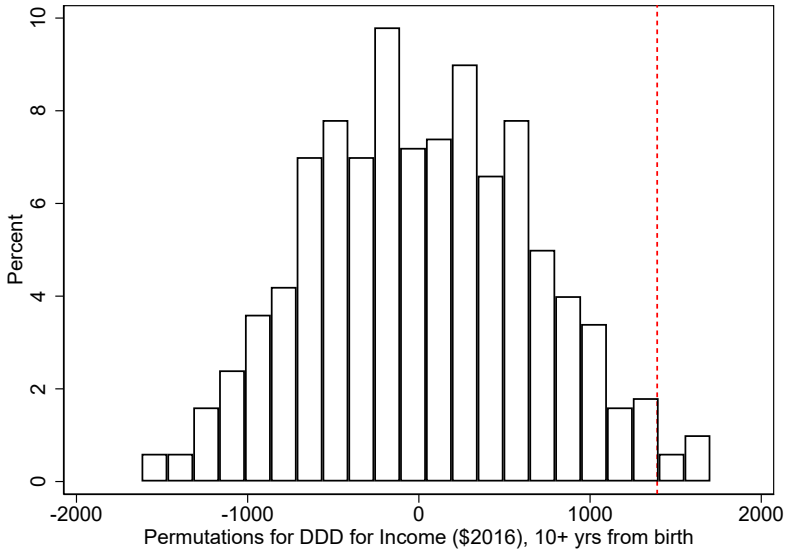


(c) Using Women Interviewed By Age 39 to 49



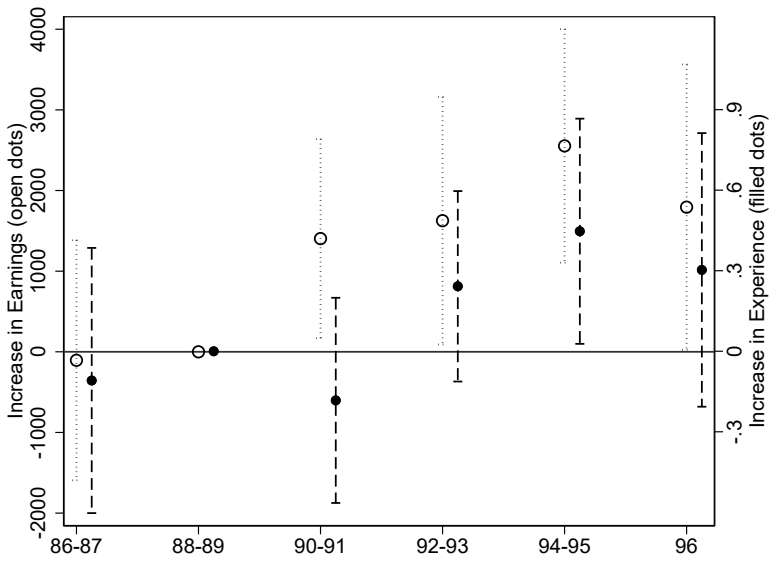
Notes: These figures presents the long-run effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on earnings, along with 95% confidence intervals, as we vary the sample restrictions. Each marker comes from a separate regression where we keep CPS surveys that occurred at most 8, 9, ...20 years from first birth (panel a); occurred at least 1, 2, ...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



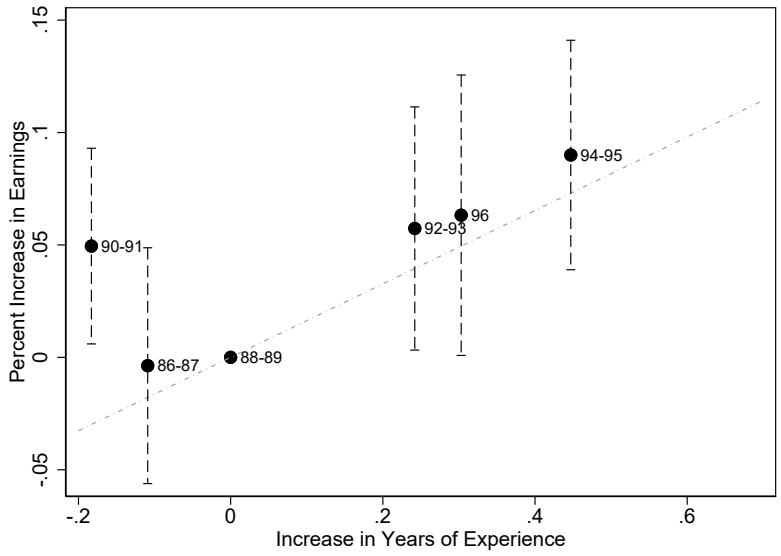
Notes: These figures show the distribution of estimates from 500 placebo experiments of the effect of early exposure to work incentives on long-run earnings (i.e., the coefficient on "10+ Yrs From Birth * EarlyExp * NM", where early and late exposure are randomly assigned. In particular, for each placebo 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.

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 $1(< 35hrs)$	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

	A. Service Occupations							
	Housekeep	Janitor	Food	Child	Beauty	Recreation	Protect	Health services
	(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

	B. Non-Service Occupations						
	Exec./ manager	Prof/tech	Financial sales	Retail sales	Cleric	Agricultural	Mechanic/ construct./ mining
	(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*b*) using information on tasks by occupation from O*NET (Autor and Dorn, 2013*a*). 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*b*) using information on tasks by occupation from O*NET (Deming, 2017*a*). 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.

A.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).[1] 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.

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

A.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.[2]

A.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 (U.S. Census Bureau, 2014).

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

B Appendix to Section 4

]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).[3] 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.
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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.

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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.[4]

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http://data.nber.org/data/survey-of-income-and-program-participation-sipp-data.html,
and were imported using the posted dictionary files (U.S. Census Bureau, 2014).

[4]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).[5]

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.

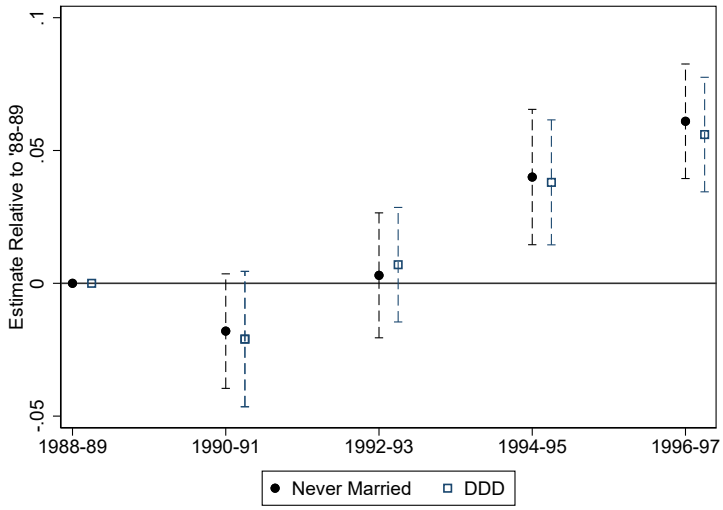
C.1 Pre-reform trend in maternal employment

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.[6]

[5] 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).

[6] 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).[7] 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 ($\frac{5.9}{8.2}$).

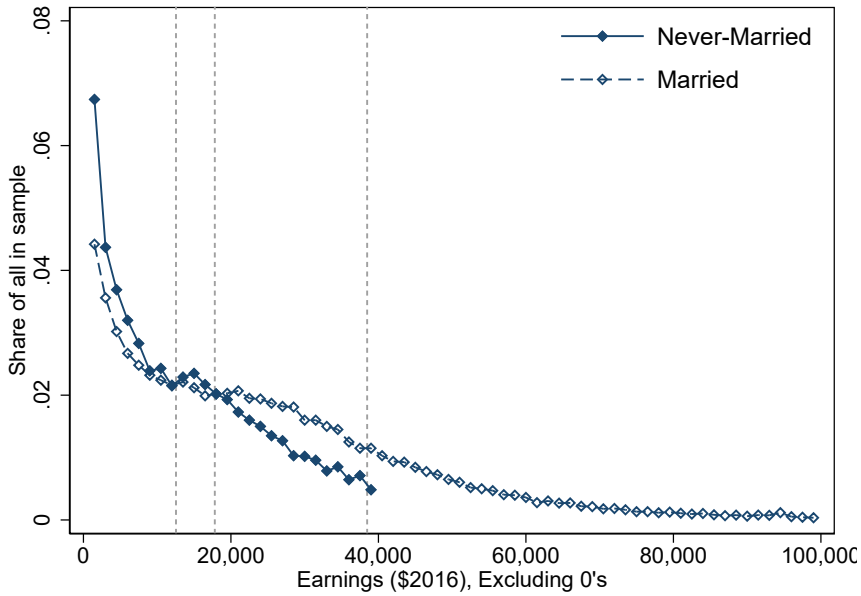
C.3 Are Mothers Responding to EITC Work Incentives in the Short Run?

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

[7] 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.[8]

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).

[8]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).[9] 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).[10]

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.[11] 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.

[9] 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.

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

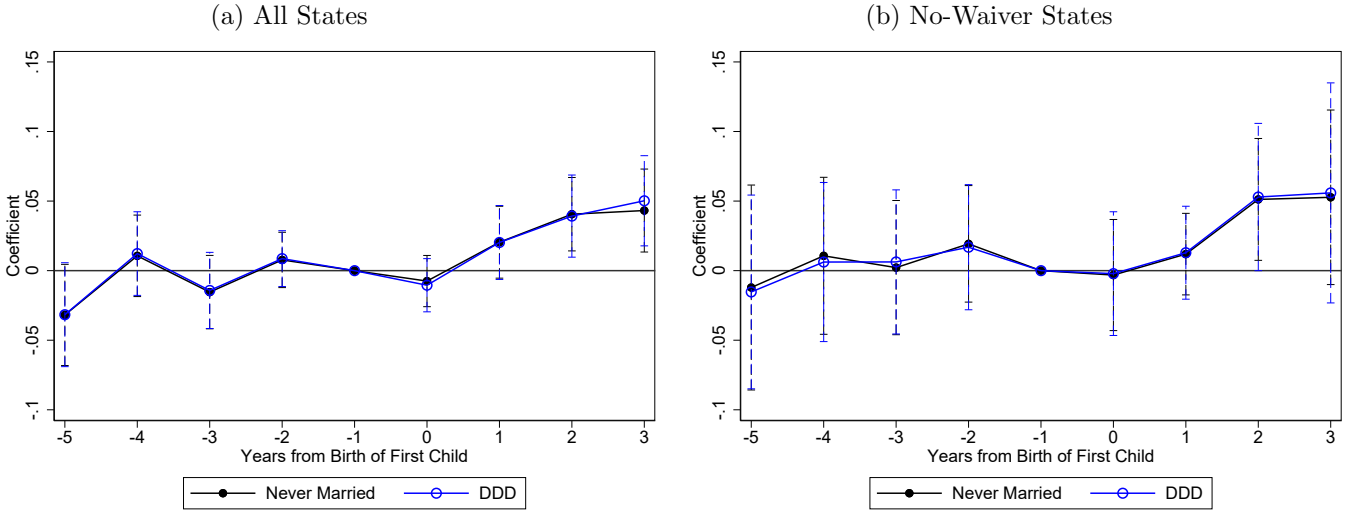
[11] We obtain information on state EITC supplements from https://users.nber.org/~taxsim/state-eitc.html. Supplementary EITC's are typically set as a percentage of the federal EITC; thus, a mother living in a state with a supplement is eligible for a more generous credit, and can expect a larger increase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

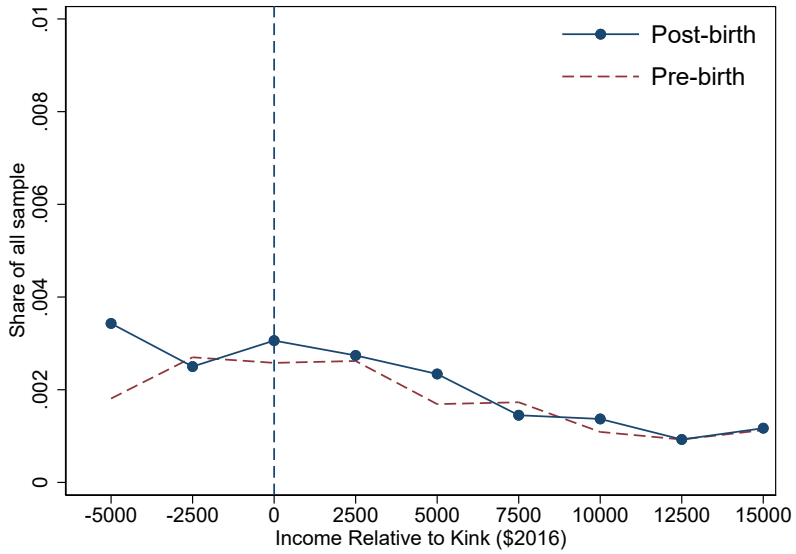
Figure C.3: Effect of Early Work Incentives on Short-Run Employment – Prior to Federal Welfare Reform



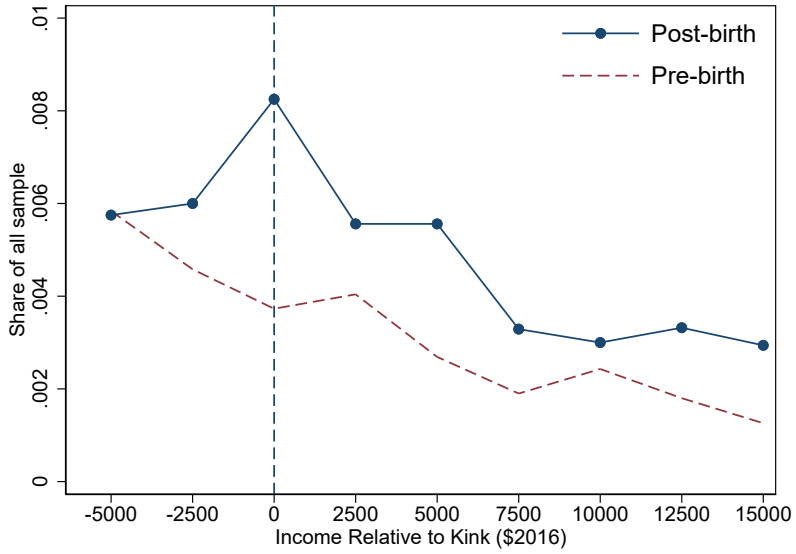
Notes: These figures presents the effect of early exposure to work incentives (in the year of first childbirth rather than 3–6 years after childbirth) on employment in each year after birth, for the years up to 1996. Panel (a) includes all states; panel (b) focuses on states that had not passed a welfare 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.[12]

Brief summary of Kleven (2021) Kleven (2021) analyzes the effect of the 1993 EITC 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. The main analysis is a difference-in-difference design comparing women with kids and without kids, before and after the reform. There are three main results. First, the post-reform increase in employment is increasing in family size and decreasing in the age of one's youngest child. Second, this produces large implied elasticities of employment (participation), e.g. equal to 2.03 (1.79) for mothers with one child. Third, 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.

1. Impacts by number and age of children Different than Kleven (2021), 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.[13] 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 ($\frac{8.5}{68.1}$).[14] 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 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

[12] 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.

[13] 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.

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

mothers.[15] 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.[16] 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.[17]

[15]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.

[16]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.

[17]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 \$7,959 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).[18] 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 \$241, as their earnings begin to surpass the EITC benefits region.

Income taxes As 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). The average rate is 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 \$1,035 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 \$127 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).

[18] 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 $t = 1994\ (1989) + \tau$ applied to household earnings in τ. We assign zero EITC in the years pre-birth. The results do not change if we allow the EITC schedule to vary for each year of first birth.

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).[19] 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 $800, based on the 0.37 year 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 $6,556 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 $2,329 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:

$$MVPF = \frac{\text{WTP}}{\text{Cost + Fiscal Externality}} \leq \underbrace{\frac{\text{WTP}}{\text{Cost + Add'l Taxes}}}_{\text{Our baseline estimate}} \tag{1}$$

Plugging in our estimates, we obtain a long-run MVPF of 1.30 ($\frac{1000}{2{,}329-1{,}559}$), 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 half as large or smaller 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 a lower bound because we are omitting impacts on many non-EITC transfers, particularly cash

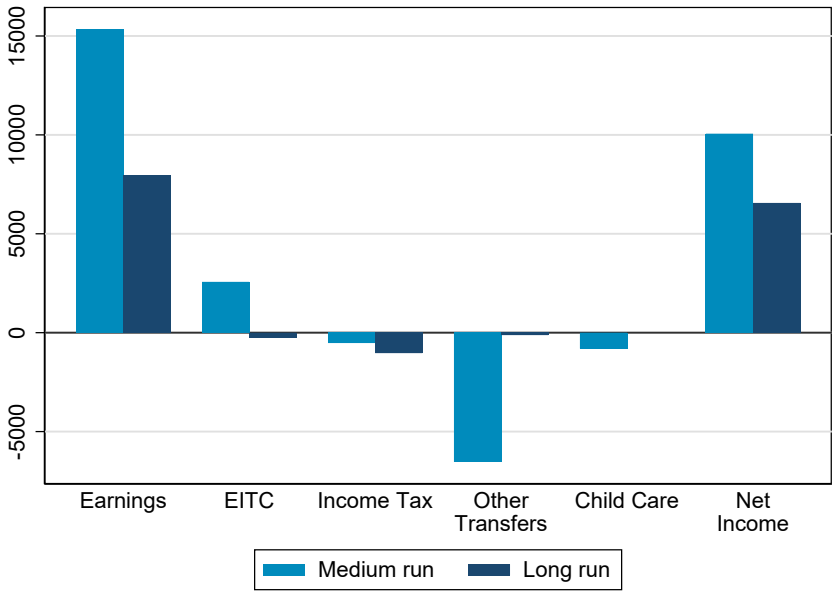
[19] We calculate this as the inflation-adjusted weighted average of the cost of each type of child care, where the weight is the share of unmarried moms that use each type of care times the share that pay anything for care. See the fourth panels of Tables 2 and 3 of Anderson and Levine (2000) for inputs.

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.[20] 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 comprehensive effects of the EITC.

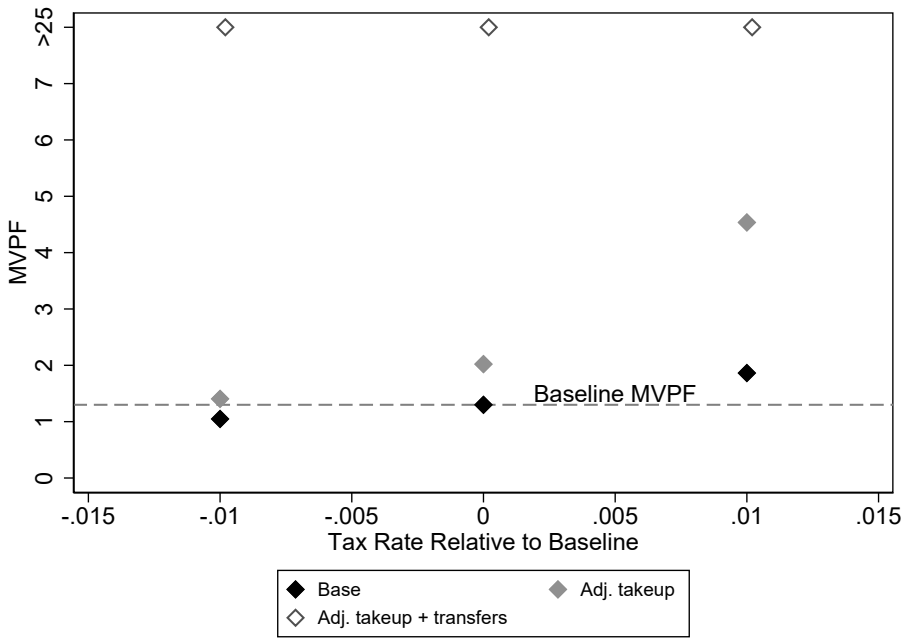
[20] In other respects, our estimates align closely with this prior work. Our estimated "mechanical" share of the EITC increase is identical to Bastian and Jones (2020) (who estimate this to be between 54–72%), and is slightly lower than Hendren and Sprung-Keyser (2019) (who estimate this to be 89.5%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



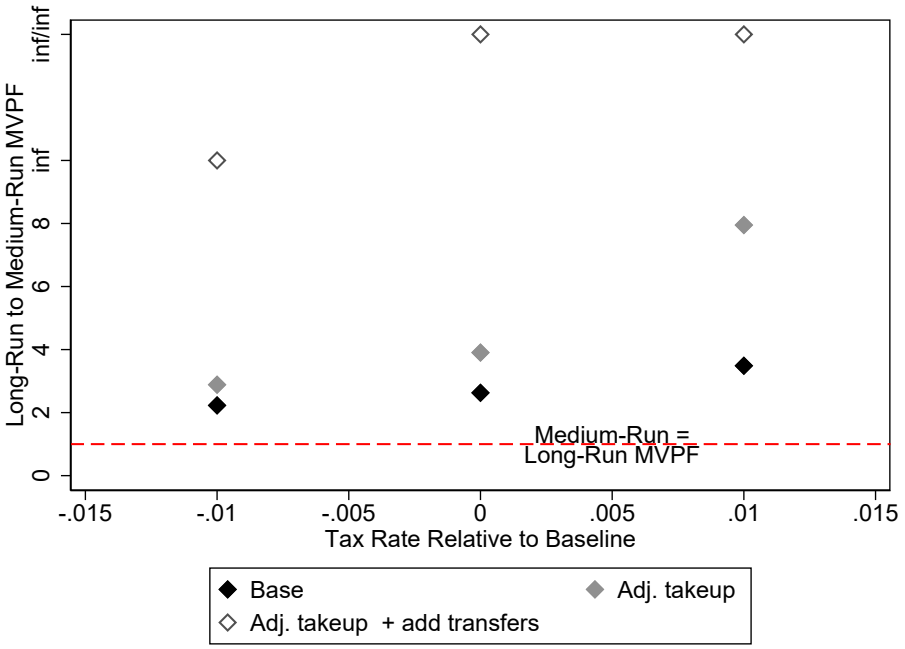
Notes: This figure presents the impact of early exposure to work incentives on the present value of net income in the medium run (years 0 to 9 post-childbirth) and long run (years 10-19 post-childbirth) stemming from changes in (i) earnings, (ii) EITC benefits, (iii) federal income taxes, (iv) other 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.[21]

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.[22] 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 $y = 5000$.[23]

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.[24] We obtain average tax rates that range from 0–0.04, 0.05–0.07, and 0.13–0.14, for the short-, medium-, and long-run, respectively, using the DD and DDD distributional estimates. We use the minimum of the tax rate for each period to calculate tax revenue: 0, 0.05, and 0.13.

[21] 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.

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

[23] E.g.,

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

[24] 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).

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:[25]

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 would receive in each year if its first birth had been in 1994 – hence, it incorporates changes in

[25] 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	Disability	SNAP	Medicaid	Hous Sub	Total
	(1)	(2)	(3)	(4)	(5)	(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.

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	Behavioral	Mechanical
	(1)	(2)	(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.

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