The Long-Run Effects of the Earned Income Tax Credit on Women's Earnings[*]

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Abstract

We use longitudinal data on marriage and children from the Panel Study of Income Dynamics to characterize women's exposure to the federal and state Earned Income Tax Credit (EITC) during their first two decades of adulthood. We then use measures of this exposure to estimate the long-run effects of the EITC on women's labor market outcomes as mature adults. We find evidence indicating that exposure to a more generous EITC when women were unmarried and had children leads to higher wages, earnings, and hours in the longer-run. We also find evidence that exposure to a more generous EITC when women had children but were married leads to lower earnings and hours in the longer-run. These longer-run effects are consistent with what we would expect if the short-run effects of the EITC on employment that are documented in other work, and predicted by theory, are reflected in effects of the EITC on cumulative labor market experience and other consequences of labor market attachment that influence earnings.

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I. Introduction

The extensive literature on the Earned Income Tax Credit (EITC) in the United States – a program that substantially subsidizes earnings in low-income families with children – has focused nearly exclusively on short-term effects. This literature establishes that a more generous EITC increases employment of less-educated, single mothers (e.g., Meyer, 2010), who are important target recipients of the program. Other work shows that these work incentives lead to poverty reductions, even without taking account of the income from the credit (Neumark and Wascher, 2011). Both types of effects are important and establish a strong case for the EITC as a pro-work, anti-poverty policy.[1]

These short-term effects suggest that there may be longer-run benefits of the EITC. Specifically, the positive employment effects should lead to greater labor market experience in the longer-run, boosting earnings via greater human capital accumulation; other types of investment, including more intensive search for better paying jobs with stronger prospects for earnings growth, could also be spurred by a more generous EITC that has positive short-term effects on employment. These kinds of long-run increases in earnings could provide an additional policy rationale for the EITC, as the early expenditures that raise employment in the short-term could lead to greater economic self-sufficiency and reduced dependence on the EITC or other government assistance in the longer term.

Our study takes a very long-run perspective. Given that EITC payments depend on number of children (directly) and marital status (indirectly, via the spouse's income), in order to capture the long-run effects of the EITC we must be able to observe a woman's childbearing and marital history. The need to capture this history, combined with the requirement to capture state variation in the EITC based on state of residence, necessitates our use of the Panel Study of Income Dynamics (PSID). Specifically, we use longitudinal data on marriage and children from the PSID to characterize women's exposure to the federal and state Earned Income Tax Credit (EITC) from ages 22-39 – corresponding roughly to their first two

[1] Some less direct evidence points to beneficial effects of the EITC on infant health (Hoynes et al., 2015) and mothers' health (Evans and Garthwaite, 2014), which presumably lead to better longer-run outcomes. For a review of related work, see Neumark (2016).

decades of adulthood when women bear children as well as a large share of the period when they raise children. We then use measures of this exposure to estimate the long-run effects of the EITC on women's earnings as mature adults (age 40).

We find evidence indicating that exposure to a more generous EITC when women were unmarried and had young (pre-school) children leads to higher earnings and hours, and perhaps wages, in the longer-run. We also find some evidence that exposure to a more generous EITC when women had children but were married leads to lower earnings and hours in the longer-run.[2] We subject our baseline estimates to several robustness checks and placebo tests to ensure our results are not driven by other policies or influences. First, we test the main specifications at other ages and show that our results are robust across a broader age range. Second, we report evidence suggesting that our results are not driven by endogenous migration, or by childbearing or marriage decisions. Third, our qualitative conclusions are robust to controlling for two potentially confounding policies – the minimum wage and welfare reform. Finally, our results are qualitatively similar if we estimate results by race, or reweighting the data.

II. Empirical Approach to Estimating Long-Run Effects of the EITC

The EITC

The federal government enacted the EITC in the 1970s, and around half of states now supplement the federal program. The EITC pays a subsidy to earnings to workers in families with low earnings. In the federal program, the phase-in credit rate – the credit rate that first applies as earnings rise above zero – is based on the number of children, with rates of 34, 40, and 45 percent for families with one, two, or three or more children, respectively. Figure 1 shows the evolution of these credits for each family type as

[2] The only study of which we are aware that looks beyond contemporaneous effects of the EITC on labor market outcomes is Dahl et al. (2009), who look at one-, three-, and five-year growth rates in earnings for single women most strongly affected by the expansion of the federal EITC in the mid-1990s. They do a difference-in-differences analysis focusing on women affected relatively more by changes in the generosity of the EITC in the mid-1990s, and find evidence of positive effects on earnings growth. Our analysis studies the effects of exposure to the EITC over much longer periods. Card and Hyslop (2005) study longer-term effects (up to a bit over six years) of a similar program in Canada (the Self-Sufficiency Project, or SSP). They find that the SSP program in Canada created short-term positive work incentives, but no long-run impact on wages or welfare participation.

earned income increases for tax year 2018. Following the phase-in region over which the subsidy rises, there is a flat “plateau” region – a range of income over which a family receives the maximum EITC based on number of children.[3] After the plateau, the credit phases-out at a rate around half the rate at which it phased-in until a family is no longer eligible. Notice that Figure 1 also shows a meager credit for families with no qualifying children, with a 7.65 percent credit rate and a $519 maximum credit; the phase-out rate for the childless credit is also 7.65 percent.

In empirical work, it is common to focus on a single dimension of the EITC – commonly the phase-in rate (see, e.g., Neumark and Wascher, 2011). It is also common to treat the EITC for families with no qualifying children as effectively zero. We use both of these approaches in our analysis.

Estimating Short-Run Effects of the EITC

To motivate our strategy for estimating longer-run effects, it is instructive to first consider the simpler problem of estimating the effect of the EITC on contemporaneous outcomes, paralleling the analysis of short-term employment effects in other papers (e.g., Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001). As we explain later, our longer-run estimation strategy is an extension of this approach.

Define by Y_{ijt} log earnings (one of the outcomes we consider) of person i, in state j, in period t.[4] As noted above, we estimate the effects of the EITC phase-in rate. Although we could use other EITC parameters (like the maximum credit), higher phase-in rates create unambiguous incentives for single mothers to work (extensive margin effects), and, as a result, the phase-in rate captures the EITC parameter most relevant to extensive margin effects.[5] These extensive margin effects are not predicted for all EITC-eligible women. Women who are second earners, including many married women, may have predicted negative intensive margin employment effects, depending on the model of labor supply (e.g., Eissa and Hoynes, 2004).

[3] The maximum credits for families with one, two, and three or more children are $3,461, $5,716, and $6,431, respectively.
[4] We consider other outcomes as well (log wages, employment, hours, and cumulative employment).
[5] Nonetheless, we obtain robust findings using the maximum EITC instead.

Following what we do in our empirical work, treat the phase-in rate for women without children as zero. In addition, we use the phase-in rate for families with two children, so that the policy parameter is exogenous, and we distinguish women by whether they have children;[6] these restrictions are also reflected in the discussion below.[7] Denote this phase-in rate CR_{jt} (*CR* stands for "credit") and denote by K_{ijt} a dummy variable for whether women have children. Define state dummy variables as D_j and year dummy variables as D_t.

Suppose we are studying low-skilled unmarried women for whom the EITC is predicted to increase employment (ignoring, for now, the potential for quite different effects on married women). Then a simple difference-in-difference-in-differences (DDD) specification for estimating the effect of the EITC on *Y* is:

$$(1)\ Y_{ijt} = \alpha + \beta CR_{jt} + \gamma K_{ijt} + \delta CR_{jt}\cdot K_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .$$

In equation (1), δ captures the effect of the EITC on *Y* for low-skilled, unmarried women with children. *K* and *CR* serve as controls, with γ capturing the effect of children independent of the EITC, and β capturing shocks or other unobservables that vary by state and year that are correlated with variation in both the EITC and *Y*, for all women including those not affected by the EITC. A more flexible way to capture the latter variation is to include a full set of interactions between the state and year dummy variables D_j and D_t, but simply including CR_{jt} is a more parsimonious version of this, as CR_{jt} will capture the variation in shocks or unobservables across states and years that are correlated with the relevant policy variation – the most important factor that could otherwise lead to bias in the estimate of δ.[8]

As always, we cannot distinguish between a true effect of the EITC on women with children and shocks that vary by state and year *and* children. The identifying assumption is that the shocks are the

[6] We also estimate separate effects for younger and older children.
[7] Dahl et al. (2009) who identify effects of the EITC from differences between outcomes for women with one child or two or more children. Little policy variation is lost by ignoring the number of children, conditional on having children; the difference between the one and two child phase-in rates is much smaller than the difference between the zero and one child rates, and the gap between the zero and one child rates becomes more pronounced than the one- to two-child gap over the sample period. Additionally, there is not much independent variation in the phase-in rates for women with one, two, or three or more children.
[8] This greater parsimony becomes valuable given that the PSID does not yield a large sample with long-term longitudinal data on women.

same for women with or without children. Thus, the estimate of δ in equation (1) is typically interpreted as a DDD estimator – identified from the difference between the change in employment associated with a more generous EITC for women with children and women without children (the difference between two DD estimators).

Strictly speaking, δ captures the effect of the EITC only if there is no EITC for childless women (i.e., women without qualifying children). However, because the childless EITC is worth very little, we believe it can be safely ignored and δ will still effectively capture the effect of the EITC, with β capturing common shocks.[9]

We can expand equation (1) to introduce married women, allowing separate effects for married (M) and unmarried (U) women. The expanded equation embeds two DDD estimators – one for unmarried women, and one for married women:

$$(2)\ Y_{ijt} = \alpha + \beta^U CR_{jt}\cdot U_{ijt} + \gamma^U K_{ijt}\cdot U_{ijt} + \delta^U CR_{jt}\cdot K_{ijt}\cdot U_{ijt}$$
$$+ \beta^M CR_{jt}\cdot M_{ijt} + \gamma^M K_{ijt}\cdot M_{ijt} + \delta^M CR_{jt}\cdot K_{ijt}\cdot M_{ijt} + \omega M_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .$$ [10]

We also could augment the specification (and do so in our longer-run analysis) to distinguish women by whether their youngest children were school age (6-17) or younger (5 and under). This specification allows the work incentives of the EITC to differ when women have school-age children, perhaps because of child care costs or women's preferences for being home with children. The "short-run" version of this specification is as follows, replacing K (the indicator for children) with YK and OK, with YK equal to 1 if the woman has a child aged 5 and under, and 0 otherwise, and OK equal to 1 if the woman has children but none aged 5 and under, and 0 otherwise:

[9] It is possible that the relative and absolute effects on women with children differ if the EITC worsens outcomes for low-skilled, unmarried women without children, because the outward labor supply shift from those with children can lower market wages and hence reduce labor supply of women who get no (or meager) benefits (Leigh, 2000). There is some evidence of adverse effects of the EITC on wages and employment of low-skilled childless individuals, and female teenagers (Neumark and Wascher, 2011). Thus, the beneficial longer-run effects of the EITC we estimate may somewhat overstate the absolute beneficial effects, although we have no evidence of adverse longer-run effects on other groups. And, as in the shorter-run literature, we are reluctant to interpret β in equation (1) as capturing EITC effects, rather than shocks common to women with and without children that are correlated with the EITC.

[10] Note that in equation (2) we introduce separate interactions with U and M, and the associated coefficients have the corresponding superscripts. We would obtain the same model fit by retaining the CR and K variables as in equation (1) and introducing interactions only with U (or only with M). But specifying the model this way lets us most easily "read off" the effects for unmarried and married women directly from the regression estimates.

$$(3)\ Y_{ijt} = \alpha + \beta^{U}CR_{jt}\cdot U_{ijt} + \gamma^{UY}YK_{ijt}\cdot U_{ijt} + \gamma^{UO}OK_{ijt}\cdot U_{ijt} + \delta^{UY}CR_{jt}\cdot YK_{ijt}\cdot U_{ijt} + \delta^{UO}CR_{jt}\cdot OK_{ijt}\cdot U_{ijt}$$
$$+ \beta^{M}CR_{jt}\cdot M_{ijt} + \gamma^{MY}YK_{ijt}\cdot M_{ijt} + \gamma^{MO}OK_{ijt}\cdot M_{ijt} + \delta^{MY}CR_{jt}\cdot YK_{ijt}\cdot M_{ijt} + \delta^{MO}CR_{jt}\cdot OK_{ijt}\cdot M_{ijt}$$
$$+ \omega M_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .$$

Equation (3) embeds four different DDD estimators – for unmarried women with younger or older children, and for married women with younger or older children.

Finally, we can incorporate data on more highly-educated women, assume they are not affected by the EITC, and use them to provide an additional level of differencing. This estimator allows us to relax the assumption that there cannot be shocks that vary by state, year, and children, if we are willing to assume that the state-by-year-by-children shocks are similar across women of different skill levels.[11] Thus, this specification provides our most compelling identification. Denoting low education by *LE*, our specification becomes:

$$(4)\ Y_{ijt} = \alpha + \beta^{U}CR_{jt}\cdot U_{ijt}\cdot LE_{ij} + \gamma^{UY}YK_{ijt}\cdot U_{ijt}\cdot LE_{ij} + \gamma^{UO}OK_{ijt}\cdot U_{ijt}\cdot LE_{ij}$$
$$+ \delta^{UY}CR_{jt}\cdot YK_{ijt}\cdot U_{ijt}\cdot LE_{ij} + \delta^{UO}CR_{jt}\cdot OK_{ijt}\cdot U_{ijt}\cdot LE_{ij}$$
$$+ \beta^{M}CR_{jt}\cdot M_{ijt}\cdot LE_{ij} + \gamma^{MY}YK_{ijt}\cdot M_{ijt}\cdot LE_{ij} + \gamma^{MO}OK_{ijt}\cdot M_{ijt}\cdot LE_{ij}$$
$$+ \delta^{MY}CR_{jt}\cdot YK_{ijt}\cdot M_{ijt}\cdot LE_{ij} + \delta^{MO}CR_{jt}\cdot OK_{ijt}\cdot M_{ijt}\cdot LE_{ij}$$
$$+ \beta^{U'}CR_{jt}\cdot U_{ijt} + \gamma^{UY'}YK_{ijt}\cdot U_{ijt} + \gamma^{UO'}OK_{ijt}\cdot U_{ijt} + \delta^{UY'}CR_{jt}\cdot YK_{ijt}\cdot U_{ijt} + \delta^{UO'}CR_{jt}\cdot OK_{ijt}\cdot U_{ijt}$$
$$+ \beta^{M'}CR_{jt}\cdot M_{ijt} + \gamma^{MY'}YK_{ijt}\cdot M_{ijt} + \gamma^{MO'}OK_{ijt}\cdot M_{ijt} + \delta^{MY'}CR_{jt}\cdot YK_{ijt}\cdot M_{ijt} + \delta^{MO'}CR_{jt}\cdot OK_{ijt}\cdot M_{ijt}$$
$$+ \omega M_{ijt}\cdot LE_{ij} + \omega' M_{ijt} + \mu LE_{ij} + D_j\theta + D_t\lambda + D_t\cdot LE_{ij}\lambda' + \varepsilon_{ijt}\ .$$[12]

In this case, we introduce the interactions with *LE*, and the coefficients on these interactions are the parameters of interest. The interactions between the EITC, marriage, and fertility variables that are *not* interacted with *LE* are not interpreted as causal, but rather as control variables for other types of

[11] One could also use this approach *instead* of distinguishing between women with and without children, identifying the effects of the EITC from a DDD estimator for less-educated versus more-educated women with children. This would also potentially avoid the complication that there is a non-zero phase-in rate for women with children. However, given that the EITC is very minor for childless women, the approach of using low-skilled women without children costs little in terms of policy variation we cannot study. Moreover, it seems more plausible to think about common shocks across women of similar skill levels for which the childless low-skilled women provide a control, than to think about common shocks across women of different skill levels.

[12] The specifications also always include a dummy variable for blacks, and an interaction of this with the dummy variable indicating low education.

shocks correlated with EITC changes not picked up in the other controls.[13] Note that we interact *LE* with the year dummy variables, to allow for possible changes over time in differences in the outcomes we study between lower- and higher-education women, which could be correlated with changes in the generosity of the EITC over time.[14]

Adapting the Analysis to Estimate Long-Run Effects of the EITC

We translate this short-run approach to estimating the effects of the EITC to our longer-run approach in a straightforward way. Specifically, we define the variables in equation (4) not as dummy variables (in the case of *U*, *M*, *YK*, and *OK*) or as single-period values (in the case of *CR*). Instead, we compute the averages of the interactions for the policy, childbearing, and marital status variables over the ages prior to when we measure "mature adult" outcomes. In the simplest version of this approach, we compute these over ages 22-39, largely covering the period when women bear children as well as much of the period when they raise children.[15]

Consider, for example, the term $\delta^{UY}CR_{jt}\cdot YK_{ijt}\cdot U_{ijt}\cdot LE_{ij}$ in equation (4), which identifies the effect of the EITC for unmarried, low-education women with young children. For this term, we define *t* as the year in which a woman is observed at age 40, and substitute

$$(5)\ \delta^{UY}_{22-39}\cdot\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot YK_{ija}\cdot U_{ija})/18\}\cdot LE_{ij}\ .$$

We construct similar averages for the other terms in equation (4). We compute averages of the interactions, rather than interactions of averages, to more accurately capture the EITC to which a woman was exposed when she was married or unmarried, had young children, etc. Our approach will capture, for

[13] In our implementation, unlike the variables capturing marriage, children, and the EITC (as explained below), *LE* remains a single dummy variable, defined as "final" education less than or equal to a high school degree – which is why it does not have a *t* subscript in equation (4).

[14] To be symmetric, we might want interactions between *LE* and the state dummy variables as well. We omit these for parsimony, and because the potential correlation over *time* between changes in outcomes for lower- and higher-education women seems more potentially problematic. Nonetheless, results are robust to including these interactions; and they are also robust to omitting the *LE*-year interactions. (Result available upon request.)

[15] Using data from the CDC's National Vital Statistics System, we calculated the ages at which women have children from 1967 to 2009. (See https://www.cdc.gov/nchs/nvss/cohort_fertility_tables.htm, viewed September 18, 2017.) Over this period, the age at which women have children has slowly increased. Women under the age of 20 accounted for around 14 percent of births in 1967, but fewer than 10 percent of births in 2009. Births above age 40 are more stable over time, increasing from 1.5 to 1.75 percent of births over the 1967-2009 period. (We also find that black women tend to have more children and at younger ages than white women, a difference that persists but becomes less prevalent over time.)

example, the difference between two women who had the same marital history and faced the same EITC in each year, but who had young children at different ages. We substitute these expressions into equation (4) to estimate the effects of these longer-run exposure variables on outcomes at age 40.[16]

The effects of children, marriage, and the EITC could also vary depending on a woman's age – in part because her age may be related to income – so in our preferred specification we split the terms like equation (5) into averages computed over younger ages (22-29) and older ages (30-39). In this case, equation (5), for example, is broken into two separate terms covering ages 22-29 and 30-39:

(6) $\delta^{UY}_{22-29}\cdot\{\sum_{a=t\text{-}18}^{t\text{-}11}(CR_{ja}\cdot YK_{ija}\cdot U_{ija})/8\}\cdot LE_{ij}$,

and

(6') $\delta^{UY}_{30-39}\cdot\{\sum_{a=t\text{-}10}^{t\text{-}1}(CR_{ja}\cdot YK_{ija}\cdot U_{ija})/10\}\cdot LE_{ij}$.[17]

Although not included in the above equations, we also include the full set of variables for marital status, children, EITC, etc., variables at age 40. (In this case the marital status and children variables are dummy variables.) We do this to be sure we do not confound the effects of past marriage, childbearing, and the EITC with effects of contemporaneous variables.

The spirit of our approach is to apply the quasi-experimental framework so commonly used for policy evaluation – including for short-run effects of the EITC – to estimate the long-run effects of the EITC. In principle, one could estimate a structural life cycle model and then simulate the long-run effects of alternative policies. We have adopted a non-structural approach in this paper because a structural model would have to embody labor supply as well as marriage and fertility decisions, and we are skeptical of the ability to accurately model all these decisions. Moreover, we think the parallels between our approach and existing short-run analyses of the effects of the EITC facilitates comparison between the shorter-term and longer-term results. Nonetheless, the usual potential limitations of reduced-form, quasi-experimental approaches apply, and ultimately we think both types of evidence could provide valuable and complementary information.

[16] We show that our results are robust to varying the exact age at which we measure these outcomes.
[17] However, we show that the results are robust to using simpler specifications in which we do not distinguish by whether women were in their 20s or 30s. We also show that results are similar for specifications that are simplified so as not to distinguish effects of exposure to the EITC by age of children.

Furthermore, although our final preferred specification seems rather complicated, the intuition is relatively straightforward. The fundamental approach builds naturally on the types of difference estimators based on marital status, children, and education used in, for example, Eissa and Liebman (1996) and Eissa and Hoynes (2004), although adapted to estimate the effects of longer-term exposure to the EITC.

We only report estimates of the key parameters of interest – which, for our full specification, are δ^{UY}_{22-29}, δ^{UY}_{30-39}, δ^{UO}_{22-29}, δ^{UO}_{30-39}, δ^{MY}_{22-29}, δ^{MY}_{30-39}, δ^{MO}_{22-29}, and δ^{MO}_{30-39}; note that we have modified the notation from those equations to include the separate exposure parameters estimated over older and younger ages. To clarify, as an example, δ^{UY}_{20-29} is the coefficient on the average, over ages 22-29, of the interaction between the two-child phase-in rate, a dummy variable for having young children, and a dummy variable for being unmarried, all interacted with the indicator for low education.

Corresponding to what we said about equation (4), the interactions between the variables capturing variation in the EITC and marriage and fertility histories that are *not* interacted with *LE* are not interpreted as causal, but rather as control variables for other types of shocks correlated with these variables. However, we do want to point out that our results are partly driven by differences in estimated coefficients between less-educated and more-educated women. That is, the estimates we report below – of the interactions between the EITC exposure variables and the low-education indicator – are smaller if we simply estimate the model for less-educated women (without the low-education interactions); and the estimates for the more-educated women are in the opposite direction. Thus, the pattern of estimated effects for the low-education only subsample is robust to exclusion of high-education women, but the estimated effects are much larger and become statistically significant when estimated relative to high-education women.

One might view the overall evidence as more compelling if the implied point estimates for high-education women were near zero. But, of course, that is not necessary to draw a causal inference from a differenced estimator. By way of analogy, in a more standard short-run estimate, if we found that an increase in EITC generosity in one state coincided with a decline in employment of high-skilled women but not of low-skilled women, we would interpret the former as reflecting a negative shock to employment in the state raising its EITC, and the *relative* estimate for low-skilled women as reflecting the effect of the EITC.[18] Of course, the identifying assumption is that the shock to high-skilled and low-skilled women is the same. But even if there was no empirical association between the increase in the EITC and employment of high-skilled women, we would still rely on this identifying assumption; otherwise the apparent effect of the EITC could reflect a shock specific to low-skilled women in the state raising its EITC. None of this discussion is to deny, however, that our conclusions would be strengthened if other empirical strategies were to provide corroborating evidence on the long-run effects of exposure to a more generous EITC.

III. Data

PSID Data

Our data come from the Panel Study of Income Dynamics (PSID), using data through the 2015 survey. We need to observe long longitudinal records on women, because their "exposure" to the EITC, as explained in Section II, depends on their marital and childbearing history, as well as their (state) residential history to assign the correct EITC.[19] We also use the longitudinal data to construct cumulative measures of years of experience.

The PSID began in 1968 with a nationally representative sample of 18,000 individuals belonging to 5,000 families. Since 1968, the PSID has followed these individuals and their descendants, interviewing them on an annual basis (biannual since 1997), and collecting detailed economic and demographic information, including employment, wages, earnings, hours, education, marriage, and fertility. This rich information allows us to create full year-by-year histories for women in the PSID.

We limit the sample to women observed at age 40 for whom we also observe their whole history beginning at age 22. To assign histories by age for each woman, we take the year that the woman is

[18] See, e.g., Card and Krueger (1994), who find that when the minimum wage increased in New Jersey but not in Pennsylvania, employment fell in Pennsylvania but was unchanged in New Jersey (Table 3) and conclude that "the increase in the minimum wage increased employment" (p. 792).

[19] Combining SIPP panels can provide data over a long period but would not provide long-term marital, childbearing, or residential histories.

observed at age 40, assign age 39 to the data one year prior, age 38 to the data two years prior, etc.[20] We assign full 19-year histories for all the necessary variables: marital status, number of children, age of children, and employment.[21]

Although the first year of the PSID is 1968, 1979 is the first year in which employment status for all individuals is captured. Thus, this is the first year we use, so that our data cover women who are observed at age 40 from 1996 to 2014. We begin our analysis at age 22 to avoid capturing women when they are more likely to still be in school or living with their parents, periods during which EITC incentives may be much weaker.

We assign marital status based on the Marriage History File. This file contains a series of questions about the timing and status of the respondents' first/only and most recent marriages. Using this information, we assign marital status by age for all women. Note that this will give us a complete marital history for all women who have not been married more than twice.

To assign number of children by age, we use information about the woman's birth history. Specifically, a woman is asked about the birth timing of up to five children, allowing us to assign a detailed child history over a woman's primary childbearing years.[22] If a woman gains a child in a manner other than childbirth, primarily via marriage or adoption, then our measure will miss them; this is relevant to the EITC because step-children, for example, could still affect EITC benefits. We constructed alternative measures using all members of the family unit and their relations to the head, but these measures turn out to be very highly correlated, so the results were qualitatively similar. We similarly assign whether the woman has younger/older children conditional on having children using the age of the youngest child assigned to the woman. Among women with children, we define those with young

[20] These ages may not align perfectly with reported age, due to differences in the timing of PSID interviews. However, there is no other clear way to use the data, and the errors introduced should be inconsequential for our longer-run measures of EITC exposure.

[21] The question about earnings refers to the past year. (For example, the data in the PSID 1968 refer to calendar year 1967.) Because of this, we assign women's ages as the age they report in a year minus one, to align with earnings at that age. We follow the same algorithm in filling in non-survey years once the PSID data become bi-annual.

[22] A woman's birth history includes her number of live births and the birth month and year of up to five children. We therefore exclude a very small number of women who have more than five live births, because we cannot assign ages to each child.

children based on whether the youngest child is aged 5 or under.

Earnings and hours data are available for heads of household and wives. For women who fit either of these relationship categories, earnings are assigned. These earnings are then converted into 2012 dollars using the CPI-U. Employment status, meanwhile, is available at the individual level for all individuals beginning with the 1979 PSID, which excludes the earliest cohorts from the sample for which we can observe a full 19-year employment history. Since many women at younger ages are neither heads of households nor wives, we cannot construct an earnings history, nor track hours worked each year. The only full history we can construct is a cumulative work experience measure based on whether one worked in each year. Whereas the birth and marriage variables do not require a woman to be interviewed every year, constructing cumulative work experience does, so this variable is available for fewer observations.

Additionally, we need information on two measures that are not tied to a 19-year history: race and education. Due to several changes in the PSID's coding of race over the survey's history, only an indicator representing whether a woman identifies as black or not can be coded consistently across time.[23] We assign educational attainment based on the woman's education level at age 40.

Table 1 shows the sample construction, and how the sample restrictions we impose based on the need for long-term longitudinal data restrict the number of available observations. Offspring of original sample members (and some additional families) are added over time, and the last available survey is in 2015. Thus, only a subset of cohorts can be observed as young as 22 and as old as 40, with the labor market and other history observable, which is why the available observations drop so sharply from row A to row D.[24] The five rows after row D document the relatively small number of observations we lose because of other data requirements (e.g., having a full marital history, or race (black/non-black) being coded consistently over time). We end up with 774 women in our final low-education subsample.

Policy Variation

Information on the EITC comes from a database of historical parameters maintained by the Tax

[23] Hispanic ethnicity cannot be coded consistently.
[24] To be sure, there is attrition in the PSID, as documented, for example, in Lemay (2009). This is reflected in the drop in the number of observations between rows C and D of Table 1.

Policy Center.[25] The policy variation we study is depicted in Figures 2 and 3. Figure 2 shows the federal EITC phase-in rate depending on number of children. The figure illustrates that, as noted in the previous section, the zero-child phase-in rate is miniscule. The one-, two-, and three-child phase in rates differ, but there is little independent variation, which is why we simply use one measure – the two-child phase-in rate.

Figure 3 depicts information on supplemental state EITCs; these are, except for Wisconsin, a fixed percentage of the federal EITC payment for which a family/person is eligible.[26] The squares show the number of states with such supplements, rising from zero in 1983 to more than half the states by 2014. We then show the average, minimum, and maximum state supplement rates over time. As the figure shows, the average has settled down to about a 20 percent supplement to the federal EITC.

IV. Replication of Past Results on EITC and Employment

We first explore using the PSID data to see how well we replicate the findings of two of the best-known papers showing that the federal EITC boosted employment of low-skilled women with children (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001). The PSID provides a far smaller sample than the Current Population Survey (CPS) data used in these papers (even before we impose the sample restrictions needed for our longer-term analysis). Thus, prior to trying to answer our more empirically-demanding question with the PSID, we would like to know whether the simpler contemporaneous results from the earlier literature can be replicated. If not, then our analysis might not have a chance to be very informative.

Eissa and Liebman (1996) study federal EITC changes in 1986, which, as Figure 2 shows, increased EITC phase-in rates, although not sharply.[27] They report several difference-in-differences (DD) estimators using treatment groups defined based on having children and, in some cases, lower education, and using control groups of either women without children or women with children but higher education.

[25] See http://www.taxpolicycenter.org/sites/default/files/legacy/taxfacts/content/PDF/historical_eitc_parameters.pdf (viewed August 16, 2018).

[26] While we classify the EITC based on state of residence, technically the EITC may depend on the state of work and not just the state of residence if a person commutes across a state border and the bordering states do not have a tax reciprocity agreement.

[27] There were also increases in the maximum credit, and reductions in the phase-out rate.

The columns labeled "E & L" in Table 2 report their estimates. The second-to-last column reports their DD estimates. All are positive, consistent with a positive effect of the EITC on employment of women (possibly low-skilled) with children. Three of the five estimates are statistically significant.

The columns labeled "Replication" show results using the PSID data for the same years. Despite the much smaller sample sizes, the PSID evidence is broadly consistent. First, most of the employment rates are similar to those in Eissa and Liebman, as the first four columns show. Second, four of the five DD estimates are positive, although standard errors are larger. The one exception is for the estimate using only those with less than a high school education comparing those with children (the treated) and without children (the controls). But as the table shows, the sample size is particularly small for this analysis (175 observations in the control group), and the estimates are, correspondingly, much less precise. For the larger sample of low-skilled women, defined as high school or less, the replication is much more consistent.

Meyer and Rosenbaum (2001) focus on the much larger changes in the EITC in the mid-1990s. They estimate year-by-year differences in the employment rate of women with and without children, controlling for other characteristics. As shown in Table 3, they find clear evidence that the difference in employment rates – with much lower employment rates for women with children initially – shrinks considerably beginning with the changes in the EITC (see the columns labelled "M & R"). Our replication extends the sample further in time. The same effect is clear in the PSID data, and here we can also see that it persists in years beyond the Meyer and Rosenbaum sample period. Moreover, the decline starts a bit earlier, which is more consistent with when the phase-in rate for women with children began increasing (as shown in Figure 2). Thus, it does appear feasible to use the PSID to study the effects of the EITC on women's labor market outcomes – at least with respect to the simpler question of shorter-run effects on employment.

V. Descriptive Statistics

Table 4 reports descriptive statistics for our PSID sample. We break the sample into means calculated over ages 22-29 and 30-39, to correspond to our specifications. We also show descriptive statistics for the low-education and high-education subsamples.

The third through fifth rows report descriptive statistics on the policy variation. The next rows report on the marriage and childbearing histories. For low-education women, we see a higher proportion of years with older children, and a higher proportion of years married, from ages 30-39 compared to ages 22-29, as we would expect. Comparing low-education and high-education women over the entire age range, the lower overall fertility of high-education women is reflected in a smaller proportion of years with (older) children.

The share black is quite high, reflecting oversampling of low-income families in the PSID. For most of our analyses we do not weight our estimates, because the variation provided by oversampling of a population that is underrepresented in the target population is useful, increasing variation in the independent variables which leads to more precise estimates.[28] We show, however, that the results are not sensitive to weighting.

Finally, the last rows report descriptive statistics for the outcomes measured at age 40. We see the expected differences, with higher hours, wages, and earnings of the more-educated subsample.

VI. Results

Baseline Specification Results

Table 5A presents estimates of the regression models used in our core analysis – the long-run exposure version of equation (4) based on equations (6) and (6'). We show the estimates for the averages of the interactions of the EITC, children, and marital history variables interacted with the indicator for low education. To clarify, in equation (4) – the short-term version of the model we estimate – there are four such interaction variables (with their corresponding coefficients): $\delta^{UY}CR_{jt}\cdot YK_{ijt}\cdot U_{ijt}\cdot LE_{ij}$, $\delta^{UO}CR_{jt}\cdot OK_{ijt}\cdot U_{ijt}\cdot LE_{ij}$, $\delta^{MY}CR_{jt}\cdot YK_{ijt}\cdot M_{ijt}\cdot LE_{ij}$, and $\delta^{MO}CR_{jt}\cdot OK_{ijt}\cdot M_{ijt}\cdot LE_{ij}$. We replace these with the averages computed over ages 22-39, split into the ranges 22-29 and 30-39, as in equations (6) and (6').

[28] This follows from the expression for the variance of OLS regression estimates. The issue receives a fuller treatment in Solon et al. (2015), who note that if the oversampling or undersampling is exogenous with respect to the dependent variable, then a correctly specified model should be consistently estimated with or without weighting, but the unweighted estimates can be more precise. Nonetheless, they advocate reporting both unweighted and weighted estimates, which we do below. (Solon et al. also point out that if the oversampling is endogenous with respect to the dependent variable, then weighting by the inverse probability of selection is needed to recover consistent estimates of a regression. In our case, we are generally studying outcomes for offspring of PSID families, at age 40, so the oversampling seems far less likely to be endogenous.)

The eight rows of estimates in Table 5A corresponding to these eight variables, with the eight coefficients referenced earlier, ordered in the table as: δ^{UY}_{22-29}, δ^{UO}_{22-29}, δ^{MY}_{22-29}, δ^{MO}_{22-29}, δ^{UY}_{30-39}, δ^{UO}_{30-39}, , δ^{MY}_{30-39}, and δ^{MO}_{30-39}; the subscript indicates the age range, the first letter of the superscript (*U* or *M*) indicates unmarried and married, and the second letter of the superscript (*Y* or *O*) indicates younger kids or older kids.

The first column of Table 5A shows estimated effects on cumulative labor market experience. As shown in the first row of column (1), we obtain a positive estimate of the effect of the EITC for women exposed to a more generous EITC, at ages 22-29, when unmarried with young children (31.1). We should expect this kind of positive extensive margin effect for unmarried women with children, although we do not find a positive effect for exposure of these women when they have older children (−5.0). In the third and fourth rows, we find negative estimates for women aged 22-29 who are exposed to a more generous EITC when they are married, with either young children or older children – with a larger and statistically significant effect in the latter case (−26.3).

These estimates are largely consistent with expectations: theory predicts, and existing evidence establishes, that the contemporaneous effect of the EITC is to boost employment of women with children who are unmarried (as they are likely to have lower family income), and the EITC is more likely to reduce employment among married women with children (although this evidence in the existing literature is much weaker).[29] Of course, in this case we translate these predicted effects of the EITC into a dynamic setting. The estimates in column (1) simply reflect the accumulation of these static or contemporaneous effects across many years, and the cumulative effect may be stronger than the often-weak evidence of negative short-run labor supply effects for married women (e.g., Eissa and Hoynes, 2004). The next four rows report estimates for the cumulative effects of exposure to a more generous EITC, for women with different marital and childbearing histories, over ages 30-39. These estimates are less clearly consistent with expectations.

However, the magnitudes of these estimated longer-run effects are tricky to interpret, for two

[29] Although the natural interpretation of these latter effects is that they reflect intensive margin effects on hours, there can also be negative extensive margin effects for second earners.

reasons. First, a one-unit increase in the right-hand-side variable is very much an "out-of-sample" prediction and indeed an unreasonable scenario. For example, a one-unit increase in the first variable implies three changes over ages 22-29: a change from zero to 100% in the phase-in rate; changing the marital history from all years married to all years unmarried; and changing the childbearing history from no years with young children to all years with young children. Second, these effects are not interpretable as partial effects, since changes in the marital and childbearing history imply changes in the other variables that also capture these histories.

We address the interpretation issue posed by this second problem below. However, as an initial partial (and imperfect) solution, the numbers in square brackets in Table 5A provide a more sensible scaling of the estimated effects, reporting the effects of a 10 percentage-point increase in the phase-in rate for one year, for someone with – respectively across the rows of the table – all years married/unmarried or with young or with older children, in the corresponding age range. In practice, we multiply the coefficients by 0.1, and then divide by 8 for the 22-29 age range, or 10 for the 30-39 age range. Thus, for example, in the first row the estimate of 0.389 in square brackets implies that a 10 percentage-point increase in the phase-in rate for one year results in 0.389 years of additional cumulative experience, for hypothetical women who always have young children and are always unmarried; this seems like a very large effect. Suppose that 10 percent of women work one additional year because of the 10 percentage-point higher EITC in place for one year.[30] Then over 8 years, the average effect on cumulative experience would be 0.0125 years, or only a small fraction of the 0.389 estimate. However, recall that we are estimating long-run effects, and if short-run increases in employment spur increases in subsequent years, the effects can be larger than what is implied by short-run estimates. Moreover, referring to the second point above about partial effects, this calculation does not account for the fact that more years unmarried implies fewer years married, so it is necessary also to apply the negative coefficients in the

[30] Although this may seem like a large policy change, in Table 3, where we replicate the Meyer and Rosenbaum (2001) estimates, the estimated effect of the more generous EITC – and it is a shorter-term estimate – is to boost the employment rate of single women with children by about 0.1. Their estimates are based on cross-sectional variation, but they estimate effects further and further from the initial policy change, and there is some indication that these effects grow over time (see the third column of Table 3).

third or fourth rows (depending on the fertility history) of Table 5A – a point to which we return below after discussing the other columns in this table.

Column (2) reports the estimated effects on employment at age 40. Only one of the estimates is statistically significant – a positive effect for women exposed to a more generous EITC when they were unmarried with older kids, over ages 30-39 (2.59). In general, the signs of the estimates in this column do not give a clear indication that the potential longer-term effects of exposure to the EITC are reflected in employment at age 40.

Columns (3) and (4) report the estimates for log hourly wages and log earnings at age 40. These outcomes are presumably most reflective of longer-run human capital effects from exposure to a more generous EITC, and hence most important regarding the potential of the EITC to boost economic self-sufficiency over the longer-run. The estimated effects generally point in the same direction in both columns and are consistent with the accumulation of the effects predicted by the static model and confirmed by short-run evidence. For ages 22-29, three of the four estimates in each column (or six of eight) have the predicted sign, with the estimated effect of exposure to a more generous EITC when unmarried with young children positive, and the estimated effect of exposure when married negative regardless of age of children. For ages 30-39, seven of the eight estimates have the predicted signs. The earnings effects are generally larger. However, none of the individual estimates are statistically significant.

Differences between the earnings and wage estimates can be driven by the hours effects reported in column (5), which generally indicate positive effects on hours at age 40 for exposure to a more generous EITC when unmarried with children, and negative effects for exposure when married with children (in the latter case, especially, regardless of age of children).

To return to the magnitudes reported in square brackets, for unmarried women aged 22-29 with young children, the implied effect of a one-year, 10 percentage-point increase in the phase-in rate is 3.0 percent for hourly wages and 2.7 percent for earnings. These are large numbers, perhaps in the range of the return to one year of experience and hence roughly equivalent to what we would expect if all women in this category worked one more year because of the policy change, which is unrealistic; again, though, short-term changes may spur larger longer-term changes. And, as noted above, a single coefficient in this model does not describe a meaningful partial effect.

Therefore, in Table 5B we provide a more satisfactory interpretation of the magnitudes from Table 5A. Specifically, we use our estimates to simulate the effects of a permanent 10 percentage-point increase in the phase-in rate (i.e., from ages 22-39) for three "types" of women – corresponding to different "scenarios" with respect to the timing of marriage and childbearing – and then for comparisons between these three types of women. First, we calculate the effect of this increase in the phase-in rate for women who have children early (two children, at age 22 and 24), but who never marry; we can think of these women as exposed to relatively extreme *extensive* margin effects of the EITC over their 20s and 30s. Second, we calculate the effect for women who have children early (the same ages) but are always married; we can think of these women as exposed to relatively extreme *intensive* margin effects of the EITC over their 20s and 30s. Finally, we calculate the effect for women with the sample average proportions of years unmarried or married and with younger or older children.

Each panel of Table 5B reports results for a different dependent variable. The first row of the panel reports the effect of a permanent 10 percentage-point increase in the phase-in rate from ages 22-39. The second and third rows then report the estimated *differences* in the effects between the three types of women; these latter comparisons are the most informative for inferring how women with different marital and fertility histories are affected by a more generous EITC.[31]

Panel A of Table 5B presents the estimates for employment at age 40. As indicated in the first row of column (3), on average, the effect of the more generous EITC over ages 22-39 on employment at age 40 is estimated to be 6.2 percentage points, although this estimate is not statistically significant. However, the estimate in column (1) shows that the effect of the more generous EITC on those exposed to extreme extensive margin effects – based on a hypothetical history of having kids at 22 and 24 and never marrying – is much larger, with a statistically significant estimate of 24.9 percentage points. These estimates in the first row of each panel are the estimated treatment effects of our assumed policy change

[31] Note that the table lists N/A where there is no comparison (e.g., the "Difference from column (2)" row in column (2)), or where the comparison is covered in another entry (the second row of column (3) in each panel).

for the group in question.

The estimated differences between the effects of exposure to a more generous EITC for women with different marital and childbearing histories are reported in the second and third rows of Panel A. Statistical analysis of these magnitudes allows us to test the statistical significance of the differences between the estimated treatment effects for our three groups of women – and, of course, the signs of these differences are informative about relative effects. All three estimates are positive, indicating that the women exposed to the more extreme extensive margin effects of a more generous EITC through their 20s and 30s are more likely to be employed at age 40. But only the difference relative to the women with histories based on sample averages – an 18.7 percentage point differential – is statistically significant (at the 10-percent level). We first summarize the estimates for the other outcomes measured at age 40, and then come back to discuss the magnitudes.

The estimates in Panels B and C, for wages and earnings, provide stronger findings. Looking first at the estimates in column (1), for those exposed to extreme extensive margin effects longer-term exposure to a more generous EITC boosts wages and earnings. In Panel B, in the first row we see that the estimated effect of a 10-percentage point higher EITC phase-in rate, on wages of women who had children early but never married, is positive (0.367) and significant at the 10-percent level. The estimate for women who had children early but were always married is the opposite sign (−0.182), as expected, although not significant. In the relative comparisons in the second and third rows, the estimated effect on wages of women who had children early but never married, compared to women who had children early but were always married, is 0.549, and the estimate relative to women with average marriage and fertility timing is 0.358; both estimates are significant at the 10-percent level.

For earnings, in Panel C, the estimates for women exposed to strong extensive margin effects, relative to the other two types of women, are also both positive. But the estimates are now larger (1.31 and 0.85) and strongly statistically significant.

Column (2) provides alternative comparisons. We would anticipate negative effects for the third row of each panel, which compares always married women who had children early – women exposed to more extreme intensive margin effects – to women with average marital and fertility histories. The evidence for both wages (Panel B) and earnings (Panel C) is consistent with this prediction, with a larger and statistically significant effect (−0.46) for earnings. We would also expect a negative effect relative to women who have children early and are never married, but that is covered in column (1) – for the comparison in the opposite direction – which is why we report N/A in this cell in column (2).

Finally, Panel D reports estimate for hours. The estimated effects are in the same directions as for wages and earnings – positive for the women exposed to more extreme extensive margin effects, and negative for women exposed to more extreme intensive margin effects. We find statistically significant evidence in the first column for the extensive margin effects, but an insignificant estimate for women exposed to extreme intensive margin effects relative to women with average marital and fertility histories. The hours effects in the same directions as the wage effects help explain why the earnings effects we estimate are larger than the wage effects.

The evidence is consistent with large positive effects on earnings, which is what we would predict from the greater accumulation of human capital associated, in part, with more years of employment. The corresponding estimates for cumulative experience (based on employment each year) are reported in the first row of Appendix Table A1. We do not focus as much on these estimates because we are most interested in mature adult outcomes (for now, at age 40), and because our ability to construct a full work history is limited. We also collect the cumulative experience estimates for the other specifications we estimate, below, in this table. Appendix Table A1 reports the three relative comparisons from Table 5B – between each possible pair of the three types of women we consider for our hypothetical scenarios. For the estimates corresponding to the specification in Table 5B, and indeed nearly every other specification we estimate, the cumulative experience estimates are positive, although rarely statistically significant. The estimates in the first row indicate that cumulative experience is roughly 2 years greater for women exposed to more extreme extensive margin effects.

The magnitudes of the estimated wage and earnings effects are large. The estimated wage effects for never married young mothers versus the other two comparison group are in the range of 0.36 to 0.55 log points (Panel B of Table 5B), and the estimated earnings effects are in the range of 100 log points (Panel C). These magnitudes may seem larger than is credible. For example, if the return to experience averaged 4 percent per year, then 5 additional years of experience would increase wages or earnings by around 20 percent, and our estimated cumulative experience measures are only around 2 years. However, there are a number of other considerations. First, the comparisons between columns (1) and (2), which are reported in the second row of column (1) of each panel, are for two extremes – never married versus always married. The comparisons relative to column (3), which represent sample averages, are more reasonable, and these comparisons yield smaller estimates – e.g., 0.36 instead of 0.55 log points for wages. Second, it is possible that there are factors other than cumulative experience that are influenced by the EITC and lead to higher earnings; the greater labor force attachment spurred by a more generous EITC might boost other human capital investments, increases effort in finding better jobs with prospects for more wage growth, etc.[32] Second, longer-term exposure to a more generous EITC might affect hours – for example, decreasing hours of those exposed to intensive margin effects – so that a cumulative experience measure that weighted by hours worked would reveal much larger effects on cumulative experience. Unfortunately, as explained in Section III, we cannot construct an hours-weighted cumulative experience measure. Third, although the estimated wage and earnings effects are large in percentage terms (log points), these effects are on a small base – of an average hourly wage of about $12.50, and average annual earnings of $19,000. So, the kinds of changes that are implied are not inconceivable.

Overall, then, there is quite strong evidence in Table 5B consistent with positive cumulative effects on wages, earnings, and hours from extensive margin effects of longer-run exposure to a more generous EITC – i.e., effects on women with more years unmarried women and with children. And conversely, there is evidence of negative cumulative effects for those exposed to strong intensive margin effects.

We next turn to analyses intended to probe the robustness and credibility of the results. We go through a long list of such analyses, so it is useful to provide the punchline first: There are some cases where changes in the analysis make the estimates less precise and hence the results become statistically

[32] This could include more investment in education, although examining this would require a different identification strategy than the one we use, which stratifies on education.

insignificant. But for the most part, the qualitative results are robust, and the statistical strength of the evidence generally does not vary much.[33]

Varying the Specification or Sample

We first consider differences in key dimensions of how we specify the model or define the sample. Table 6 presents estimates that result from varying whether we estimate separate effects for younger or older children, or for women distinguished by age 22-29 and 30-39, or, alternatively, estimating effects over the combined 22-39 age range. In the latter case, versions of equation (5), which compute averages of interactions over the entire 22-39 age range, are used, instead of equations (6) and (6').

For this and the additional analyses that follow, we report the three comparisons between each possible pair of the three types of women we consider for our hypothetical scenarios – now in one row.[34] Panel A of Table 6 indicates the same estimates for Table 5B, as a baseline. In Panel B we use cumulative exposure effects distinguishing women by age of children but not their own age; in Panel C we do the opposite, and in Panel D we do neither. The qualitative results are robust to these alternative specifications, although most of the point estimates are smaller when our accumulation of exposure to a more generous EITC is "cruder" in not distinguishing by age of women or age of children. As one example, the positive earnings effect of a more generous EITC for women who have children early and are never married versus women who have children early and are always married falls from 1.307 to 0.974 when we do not distinguish the effects of exposure by either age of children or age of women. Yet the sign pattern and the occurrence of statistically significant effects is virtually identical across the different panels of Table 6.

Table 7 instead varies the age at which we measure labor market outcomes and hence estimate the effects of cumulative exposure to a more generous EITC. In particular, we show results using ages 38, 39, 40 (the baseline), 41, and 42, in each case modifying the sample to be constructed to define outcomes

[33] The strongest deviation from the statistical significance of our estimates was discussed earlier, when we omit the high-education women and hence do not use them as a control for shocks to women that could vary with marital and fertility histories. These results are reported in Appendix Table A2.
[34] These correspond to the three estimates in the second and third rows of each panel of Table 5B.

as of these ages, including changing the range of years over which we accumulate the marriage and fertility histories and exposure to the EITC, from age 30 to one year less than these ages. Comparisons across these different ages indicates that the estimates are robust, although the same estimates are not statistically significant across all panels.

Endogeneity Concerns

Next, we explore the possibility that endogenous migration could influence our findings. In principle, lower-skilled women potentially eligible for the EITC who are more interested in working, who – as suggested by our evidence thus far – accumulate more human capital and eventually earn higher wages and earnings, could migrate to states with more generous EITCs, generating spurious evidence of the positive effects of exposure to a more generous EITC like those we find. Our first check, in Panel B of Table 8, is simply to apply EITC policy from the state of residence at age 22 for all the years for which we accumulate effects, rather than letting women's EITC exposure be determined by the states to which they migrate. The estimates from this analysis are very similar to the baseline estimates.

A second check is to use only federal EITC variation, which provides important variation but is not influenced by inter-state migration. These estimates, reported in Panel C of Table 8, are also very similar. Thus, we conclude that migration does not bias our estimated effects. The analysis using only federal variation is also potentially useful to address concerns that state variation in EITC policy responds endogenously to labor market behavior of the women who are affected (or the controls). However, given that we are looking at long-term cumulative effects of EITC policy, we doubt this is much of a concern – consistent with the similarity of the estimates.

An alternative type of endogeneity that could affect our results is endogeneity of marriage or childbearing. As discussed in several papers, including a recent review by Nichols and Rothstein (2016), in principle the EITC creates incentives to have children, and to remain unmarried if one has children. In terms of our specifications, this implies that a higher EITC can increase the proportion of years spent unmarried, or with young children.

Without knowing how these potentially endogenous responses are associated with the propensity to work, or unobserved determinants of wages, it is unclear whether or how this biases our estimated effects. Given that our results suggest that women who face a more generous EITC when they have children and are unmarried have higher earnings and labor supply at age 40, the concern is that women who would have had higher earnings or labor supply at age 40 are more likely to choose to have children, or to spend more years unmarried if they have children, when the EITC is more generous – generating a non-causal relationship between later earnings and our measure of exposure to a more generous EITC when unmarried with children.[35]

With respect to marriage, the mechanics of the EITC might generate endogenous selection in this direction. A somewhat high-earning woman who earns enough to put her on the phase-out range might be expected to lose her EITC payment if she marries, as long as the spouse's earnings push her beyond eligibility for the EITC. Similarly, a fairly low-earning woman who earns enough to obtain the maximum EITC credit (i.e., is on the plateau) also may face a marriage disincentive, since marriage could push her onto the phase-out range where the EITC payment is lower. In contrast, a very low-earning woman whose EITC payment is well below the maximum credit could face a higher EITC payment as a result of marriage (as long as combined earnings do not put her far enough on the phase-out range to reduce her EITC payment to what would be while single). Finally, a more generous EITC can make marriage more attractive to a non-working woman, because her potential spouse will have higher income (earnings plus EITC). Of course, it is hard to make firm predictions, since they depend on potential spouse earnings. Nonetheless, the endogeneity of marriage could generate a positive correlation between earnings at age 40 and exposure to a more generous EITC when married (and with children).

The mechanics with respect to childbearing are different. Having children (up to two, or up to three beginning in 2009) always increases the value of the EITC (conditional on being eligible). However, there is no clear connection between this incentive and a woman's earnings, and hence no clear reason to expect bias in our estimates one way or the other from endogenous childbearing.

What does the evidence suggest? First, based on existing research, Nichols and Rothstein conclude that there is no clear evidence that the EITC reduces marriage or increases childbearing,

[35] In what follows, we focus on earnings, since this is the more important outcome, and likely drives the labor supply differences.

although some recent simulation evidence points in this direction for marriage (Michelmore, 2018). Recent evidence on childbearing points to negligible overall effects, with increased first births among married women and lower first births among unmarried women, although these differences could be confounded by effects on marriage (Baughman and Dickert-Conlin, 2009). Baughman and Dickert-Conlin (2003) suggest that the endogenous fertility response to the EITC may occur mainly for non-white women.[36]

To assess this issue in our data, we first consider the question of the potential endogeneity of childbearing. To do this, we estimate models like equation (4), but defining as dependent variables the fraction of years from ages 22-39 that a woman spent with any kids, with young kids, or with older kids (only). Our right-hand-side variables become simpler: we include the exposure to EITC variables, but without any interactions with years married, years with children, etc. Thus, our estimating equation becomes:

$$(7)\ Y_{ijt} = \alpha + \beta_{22\text{-}29}CR_{jt}\cdot LE_{ij} + \beta'_{22\text{-}29}CR_{jt} + \gamma_{30\text{-}39}CR_{jt}\cdot LE_{ij} + \gamma'_{30\text{-}39}CR_{jt}$$
$$+ \mu LE_{ij} + D_j\theta + D_t\lambda + D_t\cdot LE_{ij}\lambda' + \varepsilon_{ijt}\ .$$[37]

The estimates from these models are reported in Panel A of Table 9. In no case do we find that exposure to a more generous EITC increased childbearing – measured as the fraction of years with children. Indeed, all six estimates are negative, although none are statistically significant. In Panel A of Table 10, we translate these coefficients into implied differences associated with a permanent 10 percentage point increase in the EITC credit rate, as in previous tables, in this case comparing women with different marital histories. Again, the results are insignificant, and we can see that the estimated effects are small.

It may seem more plausible that marriage responds. After all, being married or not may have trivial economic consequences, since one can cohabit, so the incentive effects of the EITC may be stronger for marriage than for childbearing. Given that we found no evidence of endogenous childbearing,

[36] In the analyses discussed below assessing endogeneity in our data, we did not detect differences in results between blacks and non-blacks.
[37] As before, we omit the other controls from this equation. They are the same as in the earlier models, except we exclude the controls defines at age 40.

we modify equation (7). We now estimate it for the fraction of years married, but we re-introduce the interactions between the EITC and the childbearing variables (fractions of years with children) in equation (4).

These estimates are reported in Panel B of Table 9. The estimates suggest that low-education women with young children spend more years unmarried when the EITC is more generous – the estimate in this case is −1.049, statistically significant at the five-percent level. Moreover, this evidence emerges for ages 22-29, but not ages 30-39, consistent with women endogenously delaying marriage until their 30s. Panel B of Table 10 translates these estimates into implied effects of a permanent 10 percentage point increase in the EITC credit rate, in this case comparing women with different childbearing histories. The estimates in this table are negative for women who have children earlier, consistent with delayed marriage when the EITC is more generous. But interpreted this way, the estimated effects of the EITC on marriage are also small and statistically insignificant.

Thus, our evidence does not point to any substantive evidence of endogeneity bias that could generate spurious support for what we regard as our key finding – that unmarried women with children exposed to a higher EITC have higher earnings in the longer run. As one additional check, we estimated year-by-year earnings regressions for low-education women for each age from 22 to 29 – covering the ages when there was, in Table 9, some evidence suggesting that women with children exposed to a more generous EITC delayed marriage. In our earnings regressions, we include dummy variables for married, children, and their interaction, as well as interactions of each of these with the EITC phase-in rate (as well as the EITC main effect).[38] The question we assess is whether unmarried women with children in states and years with a more generous EITC appear to be high earners – which could generate later evidence of higher earnings associated with more years exposed to a generous EITC when unmarried with children. This analysis is more informative at the younger ages we examine (22-29) than at older ages, since the older women are, the more likely it is that the EITC effects could reflect cumulative effects of past work paralleling our main results, rather than endogenous selection on earnings at the contemporaneous

[38] The specifications also include the controls for race, year, and state.

younger age. However, we find no evidence of such a selection effect; the estimated coefficient of the married-kids-EITC interaction is never statistically significant, and is not consistently of one sign or the other.[39]

Alternative EITC Parameterization

For all the results presented thus far, we have used the two-child phase-in rate to capture the generosity of the EITC. In Table 11 we instead use the maximum credit; this is the credit amount on the flat or "plateau" regions in Figure 1. As explained in the notes to the table, we use a policy simulation that amounts to about the same percentage increase in EITC generosity as the 10-percentage point increase in the phase-in rate we have been using, to make the implied policy change for the maximum credit and the phase-in rate comparable. The table shows that the results are very similar.

Other Policy Changes

There may also be longer-run effects of other policies that affect work or work incentives, most notably, perhaps, the minimum wage and welfare.[40] To assess whether the effects of these other policies could be confounded with longer-run effects of the EITC, in Table 12 we add controls for the longer-run effects of minimum wages and welfare policies.

We take two approaches to incorporating information on welfare, and most importantly the welfare reforms in the same period (the 1990s) as large expansions in the EITC. First, one perspective is that it is infeasible to code up numerous features of welfare, including how they changed when welfare reform transformed Aid to Families with Dependent Children (AFDC) into Temporary Assistance for Needy Families (TANF), and incorporate all of these variables in the kinds of long-term cumulative exposure variables we construct. Fang and Keane (2004) discuss a large array of possible measures of welfare reform that one might use; including many measures would be problematic because of multicollinearity. Thus, one approach we take, instead, is to create two variables meant to capture broad

[39] These results are available from the authors upon request.

[40] For example, Neumark and Nizalova (2007) estimate the effect of exposure to a higher minimum wage as a teenager on earnings of people in their late 20s and find some adverse effects. And Neumark et al. (in progress) estimate the longer-run effects of all three types of policies (including welfare reform) – albeit with a focus on initially disadvantaged areas, rather than individuals.

policy changes associated with welfare reform. We define one indicator for the granting of welfare waivers in the period between 1992 and the TANF rollout (in the states that received waivers), and a second indicator for the rollout of TANF in the state. We identify the month in which either of these occurred, using information from the U.S. Department of Health and Human Services.[41] Given that our data are annual, we define the variables in the years prior to a change to equal zero, and to equal one in the year after the change; for the year of the change we define the variable as the proportion of months the change was in effect. In states with waivers, the waivers remained in effect until TANF rollout, so for these states the waiver "dummy" variable turns on, and then simultaneous with the TANF variable turning on, the waiver variable turns off. For states without waivers, the TANF variable simply turns on in the month of rollout.

Because the value of welfare and the effects of welfare reform depend on marital status and number of children, we use these welfare reform variables in the same was as we do the EITC policy variable – i.e., interacted with the dummy variables for young/old children and married/unmarried, and then averaged over the two age ranges, as in equations (6) and (6'); and again, we focus on the interactions with the low-education indicator. The results are reported in Panel B. The estimates are a good deal less precise, but the pattern of estimates is the same, and the magnitudes of the estimated wage and earnings effects are still quite large. The much larger standard errors are not surprising, given that the timing of welfare reform beginning in 1996 (and the waivers a few years earlier) coincides with sharp increases in the EITC, making it difficult to separately identify the separate policy effects. The similarity of many of the point estimates, and their sign pattern, lead us to conclude that the directions of the effects we estimate are not spuriously driven by welfare reform.[42]

Given that the estimates from this approach using time dummies are quite imprecise, our second approach is to consider focusing on key variables capturing the generosity of welfare, and welfare reform. We include two measures of welfare generosity or stringency. From 1962-1996, the United States joint

[41] See https://aspe.hhs.gov/system/files/pdf/180711/Table_A.PDF (viewed August 13, 2018).

[42] We also note that, as shown in Appendix Table A1, the estimated effects of the EITC on the cumulative experience of low-skilled women exposed to extensive margin effects of the EITC are much stronger for this specification.

federal and state social assistance program was known as Aid to Families with Dependent Children (AFDC). The program was reformed by Congress in 1996 and rebranded as Temporary Assistance for Needy Families (TANF). Our first measure is the maximum payment for a family of three, usually held to be one adult and two dependent children.[43] Second, for the post-welfare reform period, we include a dummy variable for whether tight time limits were imposed. Time limits seem like a good choice to capture the effects of welfare reform. A small but consistent literature has shown that welfare time limits were a significant element of welfare reform distinguishing TANF from AFDC, (Moffitt, 2007) and that they were responsible for decreasing welfare caseloads (e.g., Grogger, 2009). There were no time limits until welfare reform in 1996, after which 10 states adopted limits of less than 60 months (in 2000, ranging from 21-48 months, but generally about two years), and most of the remaining states adopted time limits of 60 months. We use a time limit dummy variable that is equal to zero for all states before welfare reform, and, after welfare reform, switches to one for states that imposed tight time limits (less than 60 months), to capture states that more substantially tightened eligibility for welfare.

The estimates incorporating these two explicit welfare and welfare reform measures are reported in Panel C. A comparison to Panel A indicates that the estimates and the statistical conclusions are very similar. As we might have anticipated, these estimates are far more precise than those in Panel B using the welfare reform dummy variables. On the other hand, the estimates are only slightly less precise than those in Panel A.

It is clearly the case that the welfare controls in Panel C do not capture the gamut of effects of welfare reform. Nonetheless, the robustness of the estimated EITC effects in Panel C of Table 12 does not stem from the welfare controls having no impact. Appendix Table A3 reports the estimated effects of the two controls we use. We report the estimates for the same comparisons as in Table 12. In both cases, we simulate effects of making welfare *less* generous, which might be hypothesized to have effects in the same direction as making the EITC *more* generous; we consider the imposition of tight time limits, and a 10 percent reduction in benefits. The estimates in Appendix Table A3 indicate that the signs of the

[43] We are typically able to measure benefits this way, but in some cases, we can only determine the level of benefits for a family of three. We always use the former when possible.

estimated effects are as would be predicted – increasing employment, wages, earnings, and hours of never married women who have children early vs. always married women who have children early, and relative to women with average marital and childbearing behavior, and decreasing employment, wages, earnings, and hours of always married children who had children young, relative to women with average behavior. (As for the EITC effects, these are the estimated relative effects for low-skilled women.) The estimated effects are statistically significant in some cases for employment and for hours. Together, then, we believe the totality of the evidence from incorporating information on welfare reform undermines the possibility that our results on the EITC are driven by welfare reform.

In Panel D, we instead add controls for the minimum wage. Because the effect of the minimum wage does not depend in a direct way on marital status or the number of children, for the minimum wage our controls are much simpler; we simply average the minimum wage (using the higher of the state or federal minimum) over each of the two age ranges in equations (6) and (6'). A comparison of the estimates in Panel C with those in Panel A shows that adding the minimum wage controls has virtually no impact on the estimates.[44,45]

Weighting

Our final analyses concern weighting. We are quite reticent to put much store in the sample weights, given the sample selection rules imposed to study longer-term effects of the EITC (see Table 1). However, while there is little reason to believe the sample weights are very accurate, they ought to capture broad-brush differences between those oversampled based on the low-income criterion. Panel B of Table 13 reports results for the baseline specification and sample when we weight by the PSID Core sample weights for the age 40 observations. With the weights, the estimated wage and earnings effects are no longer statistically significant, although the positive estimated earnings effects in columns (1) and (2) remain sizable. The estimated hours effects do not change much – especially for the positive effects in columns (1) and (2). Moreover, the employment effects in columns (1) and (2) – where we would

[44] Although this finding contrasts with the results in Neumark and Nizalova (2007), that paper focused on exposure to a higher minimum wage at very young ages.
[45] We generally do not find significant effects of welfare reform or the minimum wage on outcomes at age 40.

expect positive effects – are larger and more strongly significant. Thus, while the exact estimates clearly are sensitive to weighting, we view Panel B as providing additional evidence of the robustness of our estimated effects of longer-run exposure to the EITC – especially for the women exposed to strong positive extensive margin effects.

We know that a principal effect of the oversampling of low-income families in the PSID is a strong overrepresentation of blacks. In our data set, the average weight on blacks is less than one-third that of non-blacks, so the weighted estimates substantially downweight blacks. This suggests that we can also learn about the sensitivity of the estimates to weighting by looking at estimates for blacks and non-blacks, which we do in Panels C and D of Table 13. The point estimates are qualitatively similar in the two panels, although the precision of some of the estimates declines substantially – in part because the samples become especially small when we disaggregate by race, but also likely because of the reduction in variation in the right-hand side variables. Still, the qualitative conclusions are unaffected.

VII. Conclusions

We use longitudinal data on marriage and children from the Panel Study of Income Dynamics to characterize women's exposure to the federal and state Earned Income Tax Credit (EITC) during approximately their first two decades of adulthood. We then estimate the long-run effects of this exposure to the EITC on women's wages and earnings (as well as employment and hours) as mature adults.

We find evidence indicating that exposure to a more generous EITC when women were unmarried and had children leads to higher wages, earnings, and hours in the longer-run. We also find some evidence that exposure to a more generous EITC when women had children but were married leads to lower earnings and hours in the longer-run. The longer-run effects are to some extent consistent with what we would expect if the short-run effects of the EITC on employment that are documented in other work, and predicted by theory, are reflected in cumulative labor market experience that influence earnings. However, the estimated effects of long-run exposure to the EITC on earnings appear to be larger than can be accounted for by differences in labor market experience. The evidence of higher hours may help explain this result, and there might also be a sizable role for impacts on investment aside from that associated with labor market experience, such as training, investment in job search for jobs with greater wage growth prospects, etc.

Overall, the results provide support for concluding that a more generous EITC not only boosts employment of low-skilled, generally single, mothers in the short term – a result established in the existing literature on the labor supply effects of the EITC. Longer-term exposure to a more generous EITC also appears to boost earnings of this group in the longer run, implying that pro-work incentives can have beneficial longer-run effects that can increase economic self-sufficiency.

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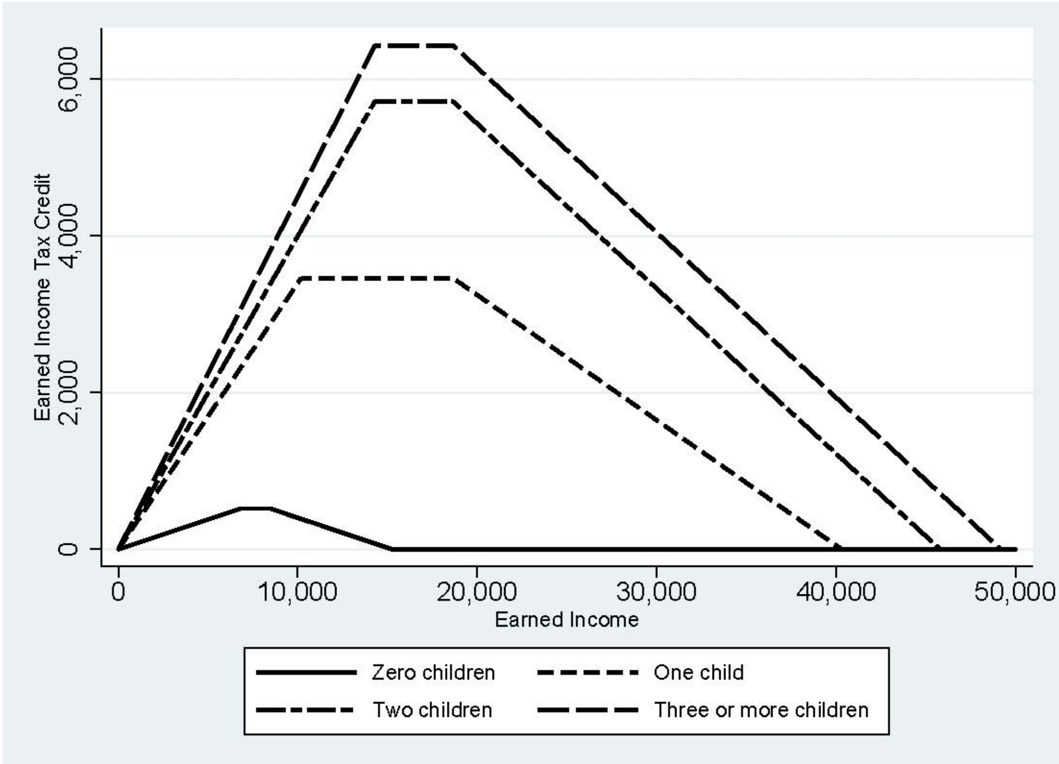
Figure 1: Federal EITC (2018)



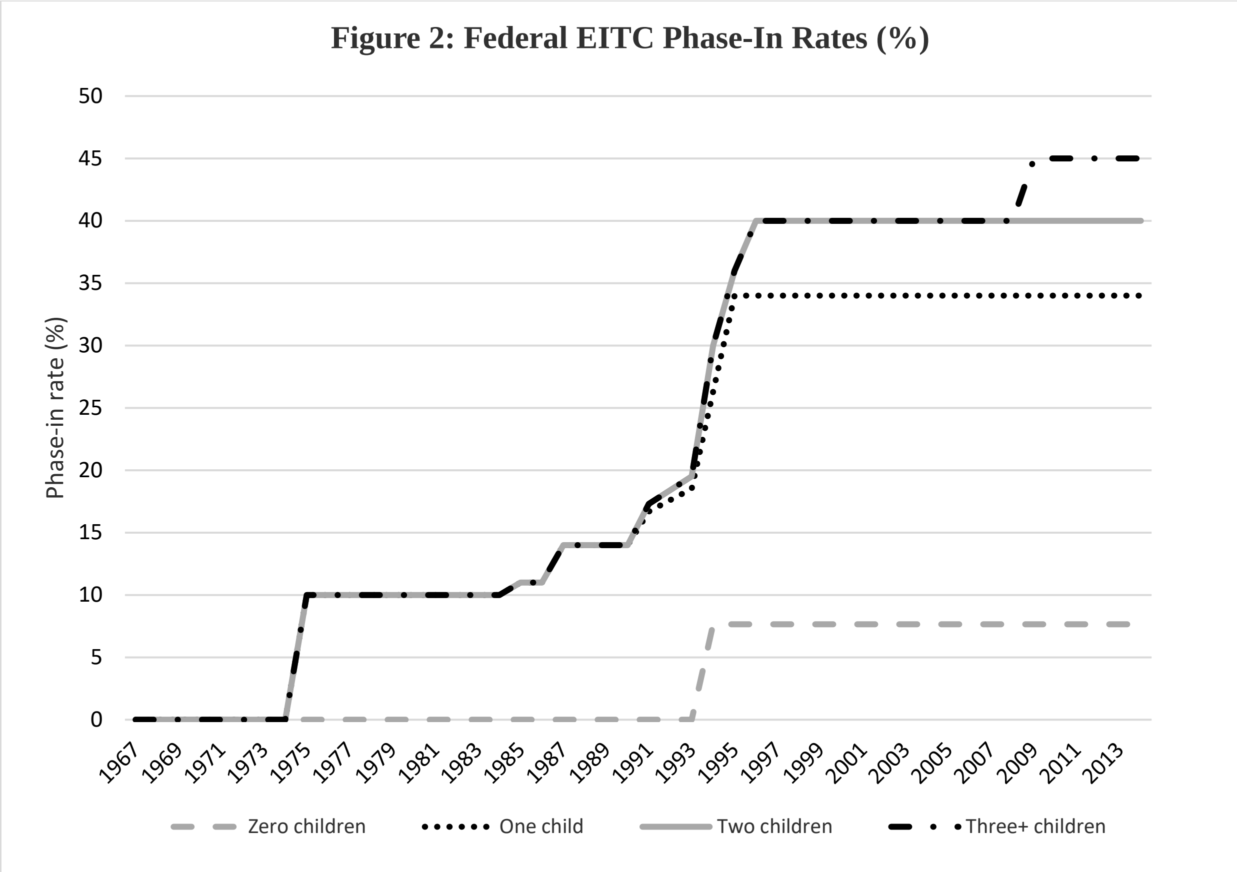


Figure 2: Federal EITC Phase-In Rates (%)
Phase-in rate (%)
Zero children
One child
Two children
Three+ children

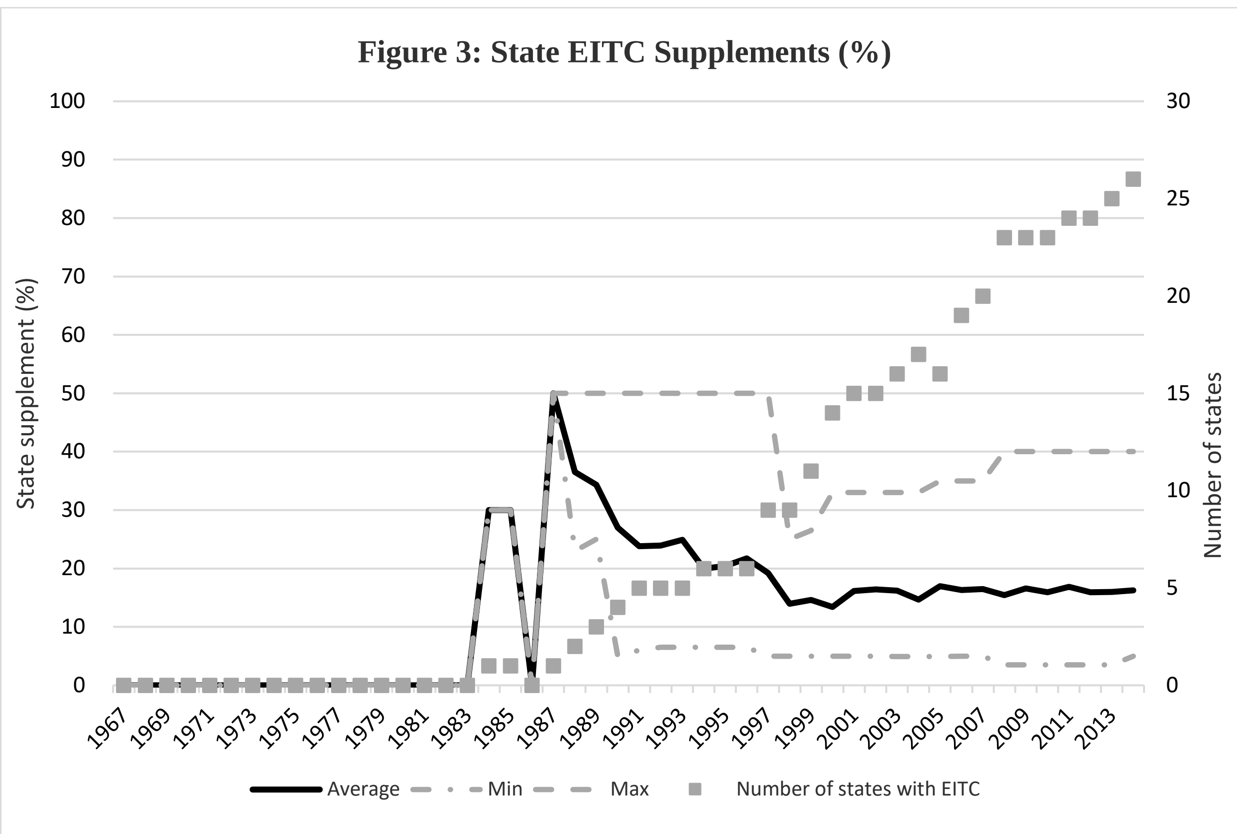


Figure 3: State EITC Supplements (%)
State supplement (%)
Number of states
Average
Min
Max
Number of states with EITC

Table 1: Sample Construction Description	
	Number of Observations
A. All PSID respondents	77,223
B. Number of female PSID respondents	39,012
C. Number of female PSID respondents potentially observed from ages 22-40	4,480
D. Number of female PSID respondents (from row C) potentially observed at age 40 from 1996-2014	3,238
E. Keep only women with a full 19-year state history back to age 22	2,291
Number of women in D with full 19-year marital history	2,089
Number of women in D with full 19-year child history	2,291
Number of women in D with full 19-year age of child history	2,256
Number of women in D with a consistent race categorization	2,180
Number of women in D with non-missing earnings data (including $0 for non-working) at age 40	2,227
Number of women in D with non-missing births data and five or fewer births	2,239
F. Number of women in D who fit all the above criteria simultaneously (final sample)	1,836
G. Number of low-educ. (LTHS or HS) women who fit all the above criteria simultaneously	774
H. Number of high-educ. (beyond HS) women who fit all the above criteria simultaneously	1,062
Row C reports the number of observations we would have for women who were observed at age 22, and could have been observed at age 40, between 1978 (the 1979 survey) and 2014 (the last year covered in our data), in the absence of attrition or missing data – i.e., based only on age and birth year. Row D includes only those observed at age 40.	

Table 2: Replication of Eissa & Liebman (1996) Table 1								
	Pre-TRA 86		Post-TRA 86		Difference		DD	
	E & L	Replication	E & L	Replication	E & L	Replication	E & L	Replication
Treatment group: with children								
Estimates	0.729	0.768	0.753	0.782	0.024	0.015		
	(0.004)	(0.015)	(0.004)	(0.014)	(0.006)	(0.021)		
N (pre and post)	20,810	3,231						
Control group: without children								
Estimates	0.952	0.969	0.952	0.970	0.000	0.001	**0.024**	**0.014**
	(0.001)	(0.005)	(0.001)	(0.006)	(0.002)	(0.008)	**(0.006)**	**(0.022)**
N (pre and post)	46,287	2,265						
Treatment group: less than HS, with children								
Estimates	0.479	0.571	0.497	0.615	0.018	0.044		
	(0.010)	(0.033)	(0.010)	(0.034)	(0.014)	(0.048)		
N (pre and post)	5,396	928						
Control group 1: less than HS, without children								
Estimates	0.784	0.648	0.761	0.819	-0.023	0.171	**0.041**	**-0.127**
	(0.010)	(0.076)	(0.009)	(0.055)	(0.013)	(0.094)	**(0.019)**	**(0.105)**
N (pre and post)	3,958	175						
Control group 2: beyond HS, with children								
Estimates	0.911	0.898	0.920	0.860	0.009	-0.038	**0.009**	**0.082**
	(0.005)	(0.020)	(0.005)	(0.025)	(0.007)	(0.032)	**(0.015)**	**(0.057)**
N (pre and post)	5,712	839						
Treatment group: high school, with children								
Estimates	0.764	0.805	0.787	0.828	0.023	0.023		
	(0.006)	(0.021)	(0.006)	(0.019)	(0.008)	(0.029)		
N (pre and post)	9,702	1,409						
Control group 1: high school, without children								
Estimates	0.945	0.963	0.943	0.958	-0.002	-0.006	**0.025**	**0.028**
	(0.002)	(0.009)	(0.003)	(0.011)	(0.004)	(0.015)	**(0.009)**	**(0.032)**
N (pre and post)	16,527	894						
Control group 2: beyond HS, with children								
Estimates	0.911	0.898	0.920	0.860	0.009	-0.038	**0.014**	**0.060**
	(0.005)	(0.020)	(0.005)	(0.025)	(0.007)	(0.032)	**(0.011)**	**(0.043)**
N (pre and post)	5,712	839						

Eissa and Liebman use the CPS March supplement weights. The PSID results use provided sampling weights to calculate means.

Table 3: Replication of Meyer & Rosenbaum (2001) Table III, Extended				
	M & R		Replication	
Explanatory variable	Marginal effect	Standard error	Marginal effect	Standard error
Any children x 1984	-0.1087	0.0160	-0.0047	0.0413
Any children x 1985	-0.0120	0.0156	-0.0529	0.0552
Any children x 1986	-0.1144	0.0153	-0.0859	0.0764
Any children x 1987	-0.1056	0.0144	-0.0493	0.0617
Any children x 1988	-0.0918	0.0140	-0.1003	0.0493
Any children x 1989	-0.0745	0.0131	-0.0881	0.0726
Any children x 1990	-0.0832	0.0136	-0.0430	0.0470
Any children x 1991	-0.0916	0.0151	-0.0096	0.0364
Any children x 1992	-0.0706	0.0159	-0.0030	0.0405
Any children x 1993	-0.0830	0.0153	0.0095	0.0293
Any children x 1994	-0.0388	0.0145	0.0002	0.0336
Any children x 1995	-0.0154	0.0143	0.0207	0.0249
Any children x 1996	0.0042	0.0140	-0.0128	0.0421
Any children x 1998			0.0120	0.0322
Any children x 2000			0.0289	0.0206
Any children x 2002			0.0457	0.0148
Any children x 2004			0.0427	0.0140
Any children x 2006			0.0465	0.0128
Any children x 2008			0.0498	0.0137
Any children x 2010			0.0431	0.0220
Any children x 2012			0.0388	0.0203
Any children x 2014			0.0490	0.0140
Nonwhite	-0.0727	0.0033	N/A	N/A
Hispanic	-0.0608	0.0033	N/A	N/A
Black	N/A	N/A	-0.0381	0.0130
Age 19-24	-0.0077	0.0055	0.0036	0.0076
Age 25-29	-0.0107	0.0095	-0.0061	0.0077
Age 35-39	0.0008	0.0052	-0.0024	0.0092
Age 40-44	0.0107	0.0116	-0.0161	0.0108
High school dropout	-0.1512	0.0032	-0.1050	0.0191
Some college	0.0989	0.0055	0.0227	0.0102
Bachelors	0.1755	0.0055	0.0659	0.0046
Masters	0.1927	0.0095	0.0638	0.0040
Divorced	0.0620	0.0052	-0.0463	0.0168
Widowed	-0.1218	0.0116	-0.2361	0.0674
Any children x divorced	0.0720	0.0063	0.0462	0.0124
Any children x widowed	0.1148	0.0137	0.0586	0.0074
Number of children under 18	-0.0325	0.0020	-0.0221	0.0042
Number of children under 6	-0.0699	0.0027	-0.0267	0.0098
State unemployment rate	-0.0101	0.0015	-0.0026	0.0029
Any children x state unemployment rate	0.0032	0.0017	-0.0050	0.0037
Number of observations	119,019		23,301	

This sample includes 19-44 year-old single women (divorced, widowed, or never married) who are not in school. Fixed state and year effects are included in the regression (not reported). Employment is defined as having worked in the past year (i.e., annual hours greater than zero). Estimates are weighted using the sampling weights from the corresponding sample. Given the longer sample period, the PSID weighting is more complicated than in Table 2. The PSID introduced new families in the early 1990s, adding around 2,000 immigrant families from Mexico, Puerto Rico, and Cuba. However, because this misses families from other Hispanic/Latino countries as well as all Asian immigrants, and due to a lack of funding, this sample was dropped in 1995. The PSID also added 441 immigrant families in 1997 and an additional 70 families in 1999. We use the Core sample weights, which means that the temporary families added in the early 1990s are not included (as they were never part of the Core sample), but the immigrant families added in 1997 and 1999 are included, as they are representative (with different weights) of families in the Core sample. (There are "Combined weights" that cover the earlier 2,000 immigrant families, but they are not defined for earlier years.)

Table 4: Descriptive Statistics for Long-Term Analysis (Means)								
	Education ≤ HS				Education > HS			
Ages	22-39	22-29	30-39	40	22-39	22-29	30-39	40
Calendar year at age 40	N/A	N/A	N/A	2003	N/A	N/A	N/A	2005
Federal EITC two-child phase-in rate	0.27	0.19	0.33	0.40	0.30	0.23	0.35	0.40
State EITC supplement percentage, two children	0.02	0.02	0.03	0.04	0.03	0.02	0.04	0.05
Combined EITC two-child phase-in rate	0.28	0.19	0.34	0.42	0.31	0.23	0.37	0.42
Prop. years with young children	0.39	0.53	0.27	0.07	0.39	0.36	0.42	0.20
Prop. years with older children	0.43	0.22	0.60	0.65	0.25	0.09	0.37	0.60
Prop. years unmarried	0.37	0.42	0.33	0.35	0.34	0.47	0.24	0.23
Prop. years married	0.63	0.58	0.67	0.65	0.66	0.53	0.76	0.77
Black	N/A	N/A	N/A	0.44	N/A	N/A	N/A	0.28
Experience (cumulative years employed)	11.57	4.66	6.91	0.72	13.89	6.12	7.77	0.80
Annual hours at age 40	N/A	N/A	N/A	1411	N/A	N/A	N/A	1548
Log wage (employed) at age 40	N/A	N/A	N/A	2.53	N/A	N/A	N/A	3.05
Log earnings (employed) at age 40	N/A	N/A	N/A	9.85	N/A	N/A	N/A	10.40

"Two-child phase-in rate" is the combined federal plus state EITC rate. In defining experience and employment, we use a variable asked independently of earnings information, on whether the person worked in the previous year. (Sample sizes appear in the tables that follow.)

Table 5A: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined Federal and State EITC Two-Child Phase-In Rate Based on Ages of Children and Ages of Women

	Cumulative experience	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
Interactions with low-education:	(1)	(2)	(3)	(4)	(5)
Avg. (two-child phase-in rate x young children x unmarried, 22-29)	31.117***	-0.094	2.434	2.131	103.82
	(10.743)	(0.947)	(2.091)	(2.550)	(2521.15)
	[0.389]	[-0.001]	[0.030]	[0.027]	[1.30]
Avg. (two-child phase-in rate x older children (only) x unmarried, 22-29)	-5.035	-1.229	-0.896	-3.017	-1699.61
	(19.751)	(1.474)	(3.207)	(4.213)	(2424.78)
	[-0.063]	[-0.015]	[-0.011]	[-0.038]	[-21.25]
Avg. (two-child phase-in rate x young children x married, 22-29)	-13.902	-0.115	-1.329	-3.466	-1581.76
	(13.429)	(1.132)	(1.924)	(2.461)	(1890.20)
	[-0.174]	[-0.001]	[-0.017]	[-0.043]	[-19.77]
Avg. (two-child phase-in rate x older children (only) x married, 22-29)	-26.252*	-0.350	-1.145	-1.830	-1439.39
	(14.939)	(1.360)	(1.917)	(3.005)	(2871.86)
	[-0.328]	[-0.004]	[-0.014]	[-0.023]	[-17.99]
Avg. (two-child phase-in rate x young children x unmarried, 30-39)	-59.895*	-0.298	3.821	7.336	6339.90
	(32.318)	(1.975)	(2.971)	(4.577)	(4612.84)
	[-0.599]	[-0.003]	[0.038]	[0.073]	[63.40]
Avg. (two-child phase-in rate x older children (only) x unmarried, 30-39)	-22.037	2.586*	1.241	4.447	5815.78
	(28.968)	(1.514)	(2.280)	(3.081)	(4012.98)
	[-0.220]	[0.026]	[0.012]	[0.044]	[58.16]
Avg. (two-child phase-in rate x young children x married, 30-39)	-4.706	-0.751	0.136	-3.566	-6489.27**
	(14.045)	(1.307)	(2.198)	(2.993)	(2648.49)
	[-0.047]	[-0.008]	[0.001]	[-0.036]	[-64.89]
Avg. (two-child phase-in rate x older children (only) x married, 30-39)	3.832	1.268	-0.490	-3.025	-1693.56
	(15.834)	(1.301)	(2.394)	(3.529)	(2347.57)
	[0.038]	[0.013]	[-0.005]	[-0.030]	[-16.94]
R^2	0.2268	0.1229	0.3144	0.2370	0.1484
N, low-education	612	774	610	611	774
N, high-education	683	1062	891	891	1062

See notes to Table 4. These results are based on the long-run exposure version of equation (4), based on equations (6) and (6'). Reported estimates are interactions with indicator for low education. Other controls include:
(1) averages of two-way interactions between the EITC variable, dummy variables for marital status, and dummy variables for young or older children, calculated over ages 22-29 or 30-39; and corresponding main effects;
(2) two-way and three-way interactions between the EITC variable, a dummy for married, and dummy variables for young or older children, at age 40, and corresponding main effects;
(3) dummy variable for black;
(4) state and year fixed effects;
(5) all controls in (1)-(3), plus year fixed effects interacted with low-education indicator; the latter are reported. In addition, the main effect of low-education is included.
Years with young children are defined as years when the youngest child born to the woman under age 6, while years with older children are defined as years when the youngest child born to the woman being age 6-17. The number in square brackets is the implied effect of a 0.1 increase in the phase-in rate for one year (the coefficient x 0.1/8 for the 22-29 variables and x 0.1/10 for the 30-39 variables).
***/**/* Significantly different from zero at 1/5/10-percent level.

Table 5B: Estimated Differences from Permanent 10 Percentage-Point Increase in Two-Child Phase-In Rate Implied by Table 5A Estimates, at Age 40			
Evaluated at/for:	Early children (22, 24), never married	Early children (22, 24), always married	Sample averages
	(1)	(2)	(3)
A. Employment (Table 5A, col. (2))			
Estimate	0.249** (0.106)	0.115 (0.141)	0.062 (0.078)
Difference from column (2)	0.134 (0.152)	N/A	N/A
Difference from column (3)	0.187* (0.103)	0.053 (0.078)	N/A
B. Log hourly wage (employed) (Table 5A, col. (3))			
Estimate	0.367* (0.183)	-0.182 (0.231)	0.009 (0.121)
Difference from column (2)	0.549* (0.315)	N/A	N/A
Difference from column (3)	0.358* (0.191)	-0.191 (0.139)	N/A
C. Log earnings (employed) (Table 5A, col. (4))			
Estimate	0.658** (0.255)	-0.649** (0.306)	-0.188 (0.171)
Difference from column (2)	1.307*** (0.427)	N/A	N/A
Difference from column (3)	0.845*** (0.282)	-0.461*** (0.169)	N/A
D. Annual Hours (Table 5A, col. (5))			
Estimate	591.96* (330.40)	-327.53 (227.93)	-123.26 (164.20)
Difference from column (2)	919.49** (394.99)	N/A	N/A
Difference from column (3)	715.22** (269.15)	-204.28 (147.53)	N/A

See notes to Tables 4 and 5A. These results are based on the exposure variables interacted with the indicator for low education.

Table 6: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Two-Child Phase-In Rate, Alternative Specifications Based on Ages of Children and Ages of Women, at Age 40

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
	(1)	(2)	(3)
A. Baseline (Both Age of Children and Age of Woman)			
Employment	0.134 (0.152)	0.187* (0.103)	0.053 (0.078)
Log hourly wage (employed)	0.549* (0.315)	0.358* (0.191)	-0.191 (0.139)
Log earnings (employed)	1.307*** (0.427)	0.845*** (0.282)	-0.461*** (0.169)
Annual hours	919.49** (394.99)	715.22** (269.15)	-204.28 (147.53)
B. Age of Children Only			
Employment	0.107 (0.177)	0.082 (0.116)	-0.025 (0.063)
Log hourly wage (employed)	0.371 (0.337)	0.236 (0.220)	-0.136 (0.119)
Log earnings (employed)	1.063* (0.532)	0.654* (0.342)	-0.409** (0.192)
Annual hours	785.22* (434.19)	513.19* (287.25)	-272.03* (149.73)
C. Age of Woman Only			
Employment	0.065 (0.173)	0.063 (0.112)	-0.002 (0.065)
Log hourly wage (employed)	0.221 (0.335)	0.124 (0.215)	-0.097 (0.125)
Log earnings (employed)	0.838** (0.397)	0.501* (0.253)	-0.337** (0.151)
Annual hours	933.11** (456.17)	632.63** (300.79)	-300.48* (163.03)
D. Neither Age of Children nor Age of Woman			
Employment	0.080 (0.178)	0.067 (0.115)	-0.013 (0.064)
Log hourly wage (employed)	0.301 (0.306)	0.179 (0.198)	-0.122 (0.111)
Log earnings (employed)	0.974** (0.456)	0.579* (0.289)	-0.395** (0.170)
Annual hours	760.91* (425.10)	491.96* (276.89)	-268.94* (151.27)

See notes to Tables 4, 5A, and 5B. The only differences are that in Panel B women are not distinguished by two age groups, but the exposure variables are computed over ages 22-39, in Panel C the exposure variables are not computed separately for younger and older children, and in Panel D both simplifications are made.

Table 7: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Two-Child Phase-In Rate, at Alternative Ages

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
	(1)	(2)	(3)
A. Age 38 Sample and Outcomes [low-ed N=899; high-ed N=1,252]			
Employment	0.017 (0.129)	0.051 (0.084)	0.034 (0.063)
Log hourly wage (employed)	0.395 (0.415)	0.293 (0.333)	-0.103 (0.129)
Log earnings (employed)	0.856* (0.491)	0.562 (0.385)	-0.294 (0.177)
Annual hours	148.63 (447.62)	131.50 (298.19)	-17.13 (175.63)
B. Age 39 Sample and Outcomes [low-ed N=853; high-ed N=1,152]			
Employment	0.048 (0.143)	0.097 (0.097)	0.048 (0.070)
Log hourly wage (employed)	0.662 (0.424)	0.468 (0.305)	-0.194 (0.152)
Log earnings (employed)	1.023* (0.523)	0.763** (0.356)	-0.259 (0.202)
Annual hours	486.80 (486.00)	438.29 (340.20)	-48.52 (168.42)
C. Baseline (Age 40 Sample and Outcomes) [low-ed N=774; high-ed N=1,062]			
Employment	0.134 (0.152)	0.187* (0.103)	0.053 (0.078)
Log hourly wage (employed)	0.549* (0.315)	0.358* (0.191)	-0.191 (0.139)
Log earnings (employed)	1.307*** (0.427)	0.845*** (0.282)	-0.461*** (0.169)
Annual hours	919.49** (394.99)	715.22** (269.15)	-204.28 (147.53)
D. Age 41 Sample and Outcomes [low-ed N=728; high-ed N=970]			
Employment	0.087 (0.206)	0.161 (0.145)	0.074 (0.077)
Log hourly wage (employed)	0.539* (0.319)	0.362* (0.207)	-0.177 (0.138)
Log earnings (employed)	1.382*** (0.440)	1.029*** (0.269)	-0.353* (0.197)
Annual hours	949.67* (498.16)	861.04** (343.95)	-88.62 (177.70)
E. Age 42 Sample and Outcomes [low-ed N=679; high-ed N=907]			
Employment	0.011 (0.240)	0.009 (0.168)	-0.002 (0.087)
Log hourly wage (employed)	0.511 (0.330)	0.314 (0.232)	-0.197 (0.127)
Log earnings (employed)	1.399** (0.552)	0.961** (0.412)	-0.438** (0.189)
Annual hours	674.00 (639.76)	587.05 (475.57)	-86.96 (199.17)

See notes to Tables 4, 5A, and 5B. Specifications are the same as in Table 5A, except outcomes are defined at different ages, and samples constructed corresponding to the same ages, with the older age range extending to 37, 38, 40, or 41, in Panels B-E, respectively. Maximum sample sizes across the regressions are shown.

Table 8: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, for Alternative Specifications for Eliminating Endogenous Migration or Policy, at Age 40

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
	(1)	(2)	(3)
A. Baseline			
Employment	0.134 (0.152)	0.187* (0.103)	0.053 (0.078)
Log hourly wage (employed)	0.549* (0.315)	0.358* (0.191)	-0.191 (0.139)
Log earnings (employed)	1.307*** (0.427)	0.845*** (0.282)	-0.461*** (0.169)
Annual hours	919.49** (394.99)	715.22** (269.15)	-204.28 (147.53)
B. Fixed State at Age 22			
Employment	0.125 (0.159)	0.165 (0.108)	0.040 (0.077)
Log hourly wage (employed)	0.631** (0.313)	0.402** (0.195)	-0.230* (0.135)
Log earnings (employed)	1.325*** (0.419)	0.845*** (0.278)	-0.480*** (0.166)
Annual hours	865.75** (399.93)	667.33** (269.31)	-198.42 (150.05)
C. Federal Variation Only			
Employment	0.121 (0.172)	0.184 (0.118)	0.063 (0.086)
Log hourly wage (employed)	0.687** (0.316)	0.444** (0.189)	-0.243* (0.141)
Log earnings (employed)	1.443*** (0.479)	0.926*** (0.314)	-0.517*** (0.191)
Annual hours	969.02** (450.45)	769.75** (299.31)	-199.27 (173.93)

See notes to Tables 4, 5A, and 5B. The only difference is how the EITC two-child phase-in rate is defined.

Table 9: Long-Run Effects of EITC on Women's Fertility and Marital Status from Ages 22 to 40, Using Combined Federal and State EITC Two-Child Phase-In Rate

	Fraction of years with any kids	Fraction of years with young kids	Fraction of years with older (only) kids	Fraction of years married
Interactions with low-education:	(1)	(2)	(3)	(4)
A. Treating Childbearing as Potentially Endogenous				
Avg. (two-child phase-in rate, 22-29) x low-ed	-0.815 (0.889)	-0.107 (0.505)	-0.708 (0.728)	…
Avg. (two-child phase-in rate, 30-39) x low-ed	-0.788 (0.866)	-0.393 (0.447)	-0.395 (0.603)	…
R^2	0.1912	0.0712	0.2838	
B. Treating Either Childbearing as Potentially Endogenous, Conditional on Childbearing				
Avg. (two-child phase-in rate x young kids, 22-29) x low-ed	…	…	…	-1.049** (0.478)
Avg. (two-child phase-in rate x older (only) kids, 22-29) x low-ed	…	…	…	0.396 (0.677)
Avg. (two-child phase-in rate x young kids, 30-39) x low-ed	…	…	…	-0.200 (0.580)
Avg. (two-child phase-in rate x older (only) kids, 30-39) x low-ed	…	…	…	1.114 (0.718)
R^2	0.3788	0.2566	0.3888	0.3898
N, low-education	774	774	774	774
N, high-education	1062	1062	1062	1062

See notes to Table 4, and modifications of equation (4) explained in the text.

Table 10: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Two-Child Phase-In Rate on Women's Fertility and Marital Status from Ages 22 to 40			
A. Treating Childbearing as Potentially Endogenous			
Comparisons:	Always married vs. Never married	Always married vs. Married at 30	Never married vs. Married at 30
	(1)	(2)	(3)
Fraction of years with kids	0.004 (0.064)	-0.022 (0.034)	-0.027 (0.068)
Fraction of years with young kids	-0.024 (0.028)	-0.003 (0.025)	0.021 (0.035)
Fraction of years with older (only) kids	0.028 (0.042)	-0.019 (0.024)	-0.047 (0.044)
B. Treating Marriage as Potentially Endogenous, Conditional on Childbearing			
Comparisons:	Early children (22, 24) vs. Late children (30, 32)	Early children (22, 24) vs. Average kids	Late children (30, 32) vs. Average kids
Fraction of years married	-0.009 (0.048)	-0.032 (0.023)	-0.022 (0.032)
See notes to Tables 4, 5A, 5B, and 9.			

Table 11: Selected Estimated Differences from Permanent $1,000 Increase in Maximum Two-Child EITC Credit, at Age 40

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
	(1)	(2)	(3)
A. Baseline			
Employment	0.134 (0.152)	0.187* (0.103)	0.053 (0.078)
Log hourly wage (employed)	0.549* (0.315)	0.358* (0.191)	-0.191 (0.139)
Log earnings (employed)	1.307*** (0.427)	0.845*** (0.282)	-0.461*** (0.169)
Annual hours	919.49** (394.99)	715.22** (269.15)	-204.28 (147.53)
B. Max Credit as Policy			
Employment	0.103 (0.114)	0.139* (0.076)	0.036 (0.059)
Log hourly wage (employed)	0.407* (0.228)	0.263* (0.140)	-0.144 (0.102)
Log earnings (employed)	0.969*** (0.314)	0.624*** (0.213)	-0.345*** (0.124)
Annual hours	721.76** (293.24)	546.20*** (200.77)	-175.57 (109.42)

See notes to Tables 4, 5A, and 5B. The only difference is that we use the maximum credit, instead of the phase-in rate. The policy simulation here is an increase of $1,000 2012 dollars. This is approximate equal, in percentage terms, to the 0.1 phase-in rate increase used in other tables. A 0.1 phase-in rate increase is a 21.2 percent increase in the two-child EITC phase-in rate, based on a weighted average of observations in our sample. The equivalent percentage increase in the two-child EITC maximum credit is $961.7; we round this to $1,000.

Table 12: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, with Alternative Controls for Welfare Reform, and with Minimum Wage, at Age 40

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
	(1)	(2)	(3)
A. Baseline			
Employment	0.134 (0.152)	0.187* (0.103)	0.053 (0.078)
Log hourly wage (employed)	0.549* (0.315)	0.358* (0.191)	-0.191 (0.139)
Log earnings (employed)	1.307*** (0.427)	0.845*** (0.282)	-0.461*** (0.169)
Annual hours	919.49** (394.99)	715.22** (269.15)	-204.28 (147.53)
B. Including Simple Welfare Reform Controls			
Employment	0.187 (0.300)	0.278 (0.215)	0.091 (0.141)
Log hourly wage (employed)	0.324 (0.547)	0.223 (0.440)	-0.101 (0.197)
Log earnings (employed)	0.811 (0.682)	0.535 (0.487)	-0.276 (0.318)
Annual hours	62.44 (577.65)	336.52 (334.45)	274.08 (318.19)
C. Including Parametric Welfare Reform Controls			
Employment	0.096 (0.167)	0.195 (0.127)	0.099 (0.077)
Log hourly wage (employed)	0.610* (0.330)	0.427** (0.211)	-0.183 (0.151)
Log earnings (employed)	1.353*** (0.399)	0.975*** (0.292)	-0.378** (0.159)
Annual hours	864.38** (398.85)	721.19** (277.13)	-143.19 (156.23)
D. Including Real Minimum Wage Controls			
Employment	0.171 (0.150)	0.210** (0.100)	0.040 (0.078)
Log hourly wage (employed)	0.580* (0.323)	0.377* (0.197)	-0.203 (0.142)
Log earnings (employed)	1.318*** (0.440)	0.845*** (0.287)	-0.473** (0.176)
Annual hours	946.82** (389.82)	735.19*** (264.05)	-211.63 (147.89)

See notes to Tables 4, 5A, and 5B. In Panel B, the welfare reform controls are two variables – one for AFDC waivers, and one for TANF rollout. In Panel C, the welfare reform controls are maximum benefits for a family with two children, and welfare time limits of fewer than 60 months. The sets of controls in Panels B and C are treated similarly to the EITC phase-in rate, as explained in the text. In Panel D, the real minimum wage controls are simply the averages to which women were exposed over ages 22-29 and 30-39.

Table 13: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Weighted, and Unweighted by Race, at Age 40			
Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
	(1)	(2)	(3)
A. Baseline			
Employment	0.134 (0.152)	0.187* (0.103)	0.053 (0.078)
Log hourly wage (employed)	0.549* (0.315)	0.358* (0.191)	-0.191 (0.139)
Log earnings (employed)	1.307*** (0.427)	0.845*** (0.282)	-0.461*** (0.169)
Annual hours	919.49** (394.99)	715.22** (269.15)	-204.28 (147.53)
B. Weighted			
Employment	0.381* (0.221)	0.392** (0.154)	0.012 (0.091)
Log hourly wage (employed)	0.345 (0.433)	0.279 (0.312)	-0.066 (0.147)
Log earnings (employed)	0.948 (0.646)	0.638 (0.437)	-0.310 (0.243)
Annual hours	927.61* (491.49)	686.65** (312.74)	-240.96 (199.69)
C. Black Only, Unweighted [low-ed N=341; high-ed N=302][a]			
Employment	0.331 (0.567)	0.279 (0.322)	-0.052 (0.277)
Log hourly wage (employed)	0.075 (1.047)	0.126 (0.596)	0.051 (0.486)
Log earnings (employed)	1.423 (1.818)	0.955 (1.013)	-0.469 (0.855)
Annual hours	1891.41 (1405.05)	1419.70* (802.75)	-471.71 (634.51)
D. Non-black Only, Unweighted [low-ed N=433; high-ed N=760]			
Employment	0.301 (0.324)	0.291 (0.227)	-0.010 (0.123)
Log hourly wage (employed)	0.911 (0.553)	0.788* (0.429)	-0.123 (0.168)
Log earnings (employed)	1.419 (0.880)	0.921 (0.655)	-0.497* (0.283)
Annual hours	825.55 (590.76)	409.06 (413.38)	-416.50* (230.21)

See notes to Tables 4, 5A, and 5B.
[a] This number is the maximum; there are fewer observations in some regressions.

Appendix Table A1: Differences in Cumulative Experience (based on Employment in Each Year) from Permanent 10 Percentage-Point Increase in Phase-In Rate Implied by Estimates in Tables, Estimated Differences between Women Based on Timing of Children and Marriage

	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
Corresponds to:	(1)	(2)	(3)
Table 5B (base specification, differences relative to high-education women)	1.915 (3.523)	2.021 (2.365)	0.105 (1.295)
Table 6 Panel B (age of children only)	2.083 (2.788)	1.343 (1.890)	-0.740 (0.914)
Table 6 Panel C (age of woman only)	-0.892 (3.280)	-1.058 (2.239)	-0.166 (1.068)
Table 6 Panel D (neither age of children nor age of woman)	1.141 (2.398)	0.660 (1.617)	-0.481 (0.800)
Table 7 Panel A (base specification, age 38 sample and outcome)	1.714 (1.712)	1.445 (1.068)	-0.270 (0.813)
Table 7 Panel B (base specification, age 39 sample and outcome)	1.203 (2.120)	1.067 (1.442)	-0.136 (0.887)
Table 7 Panel D (base specification, age 41 sample and outcome)	2.185 (3.891)	2.318 (2.670)	0.133 (1.417)
Table 7 Panel E (base specification, age 42 sample and outcome)	1.545 (4.303)	1.678 (2.941)	0.133 (1.584)
Table 8 Panel B (base specification, fixing state at age 22)	2.376 (3.607)	2.264 (2.441)	-0.112 (1.306)
Table 8 Panel C (base specification, federal EITC only)	1.817 (3.840)	2.178 (2.602)	0.361 (1.381)
Table 11 Panel B (max credit as policy)	1.648 (2.486)	1.577 (1.666)	-0.071 (0.932)
Table 12 Panel B (base specification, adding waiver and TANF control)	9.668** (4.383)	6.772** (2.929)	-2.896 (1.867)
Table 12 Panel C (base specification, adding real minimum wage control)	2.247 (3.517)	2.248 (2.360)	0.001 (1.295)
Table 12 Panel B (base specification, weighted)	1.474 (2.605)	2.081 (2.633)	0.607 (1.157)
Table 12 Panel C (base specification unweighted, black)	0.039 (13.744)	1.448 (8.037)	1.408 (5.927)
Table 12 Panel D (base specification unweighted, non-black)	-0.577 (5.051)	-0.500 (3.761)	0.077 (1.737)

See notes to corresponding tables in the paper. Note that the estimates are arrayed differently than in Table 5B, 6, 7, etc. We report the estimates only two scenario differences reflecting the greatest exposure to the EITC's extensive margin effects.

Appendix Table A2: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Low- and High-Education Women Separately, at Age 40

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
	(1)	(2)	(3)
A. Baseline			
Employment	0.134 (0.152)	0.187* (0.103)	0.053 (0.078)
Log hourly wage (employed)	0.549* (0.315)	0.358* (0.191)	-0.191 (0.139)
Log earnings (employed)	1.307*** (0.427)	0.845*** (0.282)	-0.461*** (0.169)
Annual hours	919.49** (394.99)	715.22** (269.15)	-204.28 (147.53)
B. Low-ed Only			
Employment	0.020 (0.164)	0.012 (0.107)	-0.008 (0.071)
Log hourly wage (employed)	0.145 (0.282)	0.031 (0.179)	-0.113 (0.118)
Log earnings (employed)	0.306 (0.425)	0.171 (0.275)	-0.135 (0.164)
Annual hours	243.04 (410.37)	179.26 (294.70)	-63.77 (139.08)
C. High-ed Only			
Employment	-0.080 (0.152)	-0.194* (0.113)	-0.115** (0.052)
Log hourly wage (employed)	-0.354 (0.225)	-0.314** (0.149)	0.040 (0.096)
Log earnings (employed)	-0.947*** (0.330)	-0.722*** (0.219)	0.225 (0.137)
Annual hours	-630.96** (306.85)	-617.94** (230.96)	13.02 (99.73)

See notes to Tables 4, 5A, and 5B. The only difference is that only low-educated (high-educated) women are included in Panel B (Panel C), the controls are not interacted with a low-education indicator, and the main effect of low-education is dropped.

Appendix Table A3: Selected Estimated Differences from Permanent Welfare Reforms, at Age 40

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Average kids and marital status	Early children (22,24), always married vs. Average kids and marital status
	(1)	(2)	(3)
A. Restrict Time Limits to < 60 Months			
Employment	1.260* (0.643)	0.818* (0.417)	-0.442 (0.338)
Log hourly wage (employed)	0.716 (0.880)	0.273 (0.672)	-0.443 (0.498)
Log earnings (employed)	0.331 (2.095)	0.843 (1.380)	-1.173 (0.982)
Annual hours	1851.05 (2087.20)	1011.21 (1367.52)	-839.84 (883.36)
B. Reduce Maximum Benefits by 10 Percent			
Employment	0.054** (0.025)	0.035 (0.022)	-0.019* (0.011)
Log hourly wage (employed)	0.046 (0.070)	0.026 (0.054)	-0.020 (0.037)
Log earnings (employed)	0.072 (0.114)	0.036 (0.096)	-0.036 (0.052)
Annual hours	147.28** (63.99)	91.97 (59.96)	-55.31** (25.30)

See notes to Tables 4, 5A, 5B, and 12.