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THE LONG-RUN EFFECTS OF THE EARNED INCOME TAX CREDIT ON WOMEN'S EARNINGS

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ABSTRACT

Using longitudinal data on marriage and children from the Panel Study of Income Dynamics from 1967 to 2016, we characterize women's exposure to the federal and state Earned Income Tax Credit (EITC) during their first two decades of adulthood. We use measures of this exposure to estimate the long-run effects of the EITC on women's labor market outcomes as mature adults, specifically at age 40. Our results suggest that exposure to a more generous EITC when women were unmarried and had older (school-age) children leads to higher earnings in the longer-run, and we find corresponding evidence suggesting that longer-run exposure of unmarried mothers to a more generous EITC increases cumulative labor market experience. Additionally, we find evidence to suggest that exposure to a more generous EITC when women had children while married leads to lower earnings and hours in the longer-run. For both groups, adjustments in hours worked along the intensive margin appear to drive these results. These longer-run effects are consistent with what we would expect from the short-run effects of the EITC on employment and hours predicted by theory and documented in other work.

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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 for less-educated, single mothers (e.g., Meyer, 2010), who are important target recipients of the program. Other research 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]

The presence of such short-run labor market effects suggests the EITC could also affect outcomes in the longer run. Specifically, the positive employment effects for low-skill, single mothers could increase 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. Such long-run increases in earnings would provide an additional policy rationale for the EITC: early expenditures raise short-term employment, and higher earnings in the long-run increase economic self-sufficiency, likely coupled with higher income tax receipts and reduced dependence on the EITC or other government assistance, helping to offset the earlier expenditures.

The predicted short-run effects of the EITC on married (or higher-earning) women are in the opposite direction.[2] The evidence ranges from no or modest negative labor supply effects to more sizable labor supply reductions (e.g., Eissa and Liebman, 1996; Hoffman and Seidman, 2003). Of course, even small short-run effects could potentially accumulate into larger effects over the longer run.

We test for evidence of longer-run effects of the EITC, adopting a very long-run perspective. Given

[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).
[2] Standard theory would predict labor supply disincentives in both the flat "plateau" region of the EITC as well as the phase-out region where women face larger effective marginal tax rates. These effects are likely stronger in the phase-out range (assuming that substitution effects dominate).

that EITC payments depend on number of children (directly) and marital status (indirectly, via the spouse's income), we must be able to observe a woman's childbearing and marital history in order to capture the long-run effects of the EITC. The need to capture this history, combined with the requirement to capture state variation in the EITC based on state of residence, dictates 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 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 and related labor market outcomes as mature adults, defined here as age 40.

We find evidence suggesting that exposure to a more generous EITC when women were unmarried and had older (school-age) children leads to higher earnings in the longer-run. We also find corresponding evidence suggesting that longer-run exposure of unmarried mothers to a more generous EITC increases cumulative labor market experience using a subset of our primary sample for which we can measure this. Finally, we find evidence to suggest that exposure to a more generous EITC when women had children while married leads to lower earnings and hours in the longer-run.[3]

We base our analyses on a difference-in-difference-in-differences specification measuring the presence of children and marital status across ages 22-39 and exposure to the EITC. From this simple specification, we add two elements: one better captures the labor market incentives of the EITC (age of a woman's youngest child); and the second may more cleanly identify policy effects (separate federal and state maximum credits). We subject this preferred specification to a number of checks meant to account for endogenous behavior or policy, a placebo test, and a number of robustness/sensitivity analyses. Among these checks, we include alternative parameterizations of EITC generosity and checking whether the results reflect

[3] 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 federal 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.

changes in other anti-poverty policies. These analyses show that the findings are robust, and bolster a causal interpretation of the evidence, although there are some limitations to how rigorously we can establish causal effects that could be addressed in future research.

II. The EITC: Background, Predictions, and Prior Evidence

The EITC

The federal government enacted the EITC in the 1970s and expanded its scope and generosity under major reforms in the mid-1980s and early 1990s. Today, in addition to the generous federal program, around half of states provide their own supplements. In the federal program, the phase-in credit rate – which determines the amount a family receives as earnings rise above zero – is based on the number of children, with subsidy 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 a single taxpayer as earned income increases, for tax year 2016 (the last year in our sample). 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. In 2016, the maximum credits for families with one, two, and three or more children were $3,373, $5,572, and $6,269, respectively. After the plateau, the credit phases-out at around half the rate at which it phased-in until a family is no longer eligible.[4] Figure 1 also shows a meager credit for families with no qualifying children, with a 7.65 percent credit rate and a $506 maximum credit; the phase-out rate for the childless credit is also 7.65 percent.

The standard labor supply model predicts that the subsidy to earnings along the phase-in range has positive extensive-margin effects, because there is a positive substitution effect but no income effect; the intensive-margin effects are more complicated. Along the phase-in range, the effective wage increases relative to no EITC, generating a positive intensive-margin effect as long as the substitution effect dominates the income effect. Along the plateau, there is only an income effect, which generates negative intensive-margin effects (and could potentially generate negative extensive-margin effects for one earner in a two-earner household). Along the phase-out range, women face a higher effective marginal tax rate, which adds an

[4] For jointly-filing married taxpayers, the phase-in rate, phase-out rate, and maximum credits are the same but the plateau region lasts for an additional $5,550 in earned income.

additional intensive-margin disincentive to work from the substitution effect (assuming it dominates). These predicted effects are static or short-run effects, which inform most empirical work on the EITC.

When estimating the short-run effects of the EITC in empirical work, researchers typically use a single parameter to capture EITC generosity; the two most common parameters used in the literature are the phase-in rate and the maximum credit. A higher phase-in rate generates unambiguous positive extensive-margin work incentives for those least likely to be working absent the EITC, which is why most work on the employment effects of the EITC focuses on single mothers. Of course, the maximum credit is closely related to the phase-in rate, because there are limits to how high the EITC is likely to extend into the income distribution before reaching the plateau and then phasing out. The phase-out rate and maximum credit are similarly related. In principle, one could have a high phase-in rate but a low maximum credit, which is a possible argument for preferring to focus on the maximum credit. As the major federal EITC expansions of the 1980s and 1990s increased both the phase-in rate and maximum credit simultaneously, using a single parameter is a parsimonious way of capturing EITC generosity. Neumark and Wascher (2011) use the phase-in rate. Grogger (2003) uses the maximum credit instead, but notes that the results are very similar to using the phase-in rate; Leigh (2010) also use the maximum credit. We follow the more common approach in the literature and use the maximum credit, although we show that the results are insensitive to using the two-child phase-in rate instead.[5,6]

Potential Long-Run Effects

Our focus, in contrast to most prior work, is on the potential long-run effects of the EITC, which could arise from the cumulative impact of short-run effects. Specifically, the positive employment effects for low-skill, single mothers could 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

[5] Some studies of the EITC study a single event, and hence do not have to parameterize the EITC (e.g., Eissa and Liebman, 1996; Cancian and Levinson, 2005). Others (e.g., Eissa and Hoynes, 2004) try to parameterize the tax effects of the EITC more fully.

[6] The robustness to using the two-child phase-in rate is shown in Appendix Table B5. We also show that our results are not sensitive to using the one-child or three-child maximum credits rather than the two-child maximum credit (Appendix Tables B6 and B7). As the three maximum credits are closely related over time, this robustness is not surprising. Most of the robustness analyses we discuss use Table 5 as a baseline, which we discuss below.

has positive short-term effects on employment. It is also possible that persistently higher employment from long-run exposure to a more generous EITC generates a negative wealth effect on labor supply eventually, although we strongly suspect that this channel is not relevant for the population affected by the EITC. Conversely, for women exposed to a more generous EITC when married, the negative predicted labor supply effects (especially intensive-margin effects) could accumulate into adverse longer-run effects.

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. We review some of that evidence very briefly, and then explain our approach in the next section and how it builds on the short-run literature.

The short-run literature establishes that – as predicted – a more generous EITC increases employment for less-educated, single mothers, who are important target recipients of the program. (Here, we review two key studies, which we discuss in more detail, and replicate using our data set, in Appendix B.) Eissa and Liebman (1996) study federal EITC changes in 1986, which increased EITC phase-in rates, although not sharply.[7] They study only unmarried women, and 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. They find statistically significant evidence that relative employment rates of affected women increased, for a number of different treatment and control groups. 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, also considering only unmarried women. They find clear evidence that the employment shortfall for women with children prior to the policy change shrinks considerably beginning with the changes in the EITC.

The predicted short-run effects of the EITC on married (or higher-earning) women are in the opposite direction, and mainly regard intensive-margin effects. Some work finds modest negative labor supply effects (e.g., Eissa and Hoynes, 2004) or no effect at all (Eissa and Liebman, 1996). In contrast, Hoffman and

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

Seidman (2003) suggest that there are sizable disemployment effects for married women in the phase-out range and some decrease in hours for married women and married men. Additionally, Jones (2013) finds evidence of hours reductions among those near the budget constraint kink where the phase-out rate sets in (where the implicit marginal tax rate from the EITC increases).

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

Empirical Framework for Estimating Short-Run Effects

Our approach parallels the analysis of short-term employment effects in other papers (e.g., Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001). We explain this approach in some detail, to show how we build on these past studies in an intuitive fashion to develop our longer-run estimation strategy.

Define Y_{ijt} as log earnings (one of the outcomes we consider) for person i in state j at period t,[8] K_{ijt} as an indicator for whether a woman has children, and D_j and D_t as state and year fixed effects. Our policy parameter, CR_{jt}, is the EITC maximum credit for state j in period t. We treat the maximum credit for childless women as effectively zero. The specification ignores variation across number of children, conditioning only on whether a woman has any children and using the two-child maximum credit; this ensures that the policy parameter is exogenous to the number of children and exploits the single largest source of variation in EITC generosity (children vs. no children).[9] Finally, in the simplest approach, the sample is restricted to only low-skilled unmarried women to avoid the issues of eligibility for high-skilled women and the potentially differential effects across marital status.[10] Thus, equation (1) below is a difference-in-difference-in-differences

[8] We consider other outcomes as well (cumulative employment, employment, log hourly wages, annual hours, and conditional annual hours).

[9] Variation in the maximum credit based on number of children cannot be readily incorporated. Making the EITC variation dependent on the number of children confounds the two separate effects of policy variation and childbearing. Another way to put this is that we need to include the controls for kids in the specification to capture the effects of kids on labor supply, earnings, etc., independent of the EITC. If we leave in the kids control (K) but define CR as the maximum credit based on number of children, rather than the maximum two-child credit, then we can get extreme multicollinearity between the variables involving K and the variables involved $K \cdot CR$, because for the most part the maximum credit based on number of children is a multiple of the number of children. (To see this in the simple case of equation (1), if CR = a constant c for women with children and zero for women without children, then K and $CR \cdot K$ are perfectly collinear; the same would be true if, for example, K represented dummy variables for different numbers of kids and CR took on a different value for each K.) This same issue carries over to our specification estimating effects of long-run exposure to the EITC; making the EITC variation dependent on the number of children again confounds the two separate effects of policy variation and childbearing history, and if we tie the maximum credit to the number of children there is extreme multicollinearity between the control variables involving number of children and the treatment variation that involves both number of children and the maximum EITC credit.

[10] We relax these restrictions later, but for ease of exposition we start simply and gradually add complexity.

(DDD) specification for estimating the effect of the EITC on Y:

$$(1)\ Y_{ijt} = \alpha + \beta CR_{jt} + \gamma K_{ijt} + \delta CR_{jt} \cdot K_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .$$ [11]

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.[12,13]

We cannot distinguish between a true effect of the EITC on women with children and unmeasured shocks that vary by state and year *and* children. The identifying assumption is that the shocks are the 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 for women without children (the difference between two DD estimators).

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:

$$\begin{aligned}(2)\ Y_{ijt} = {} & \alpha + \beta^U CR_{jt} \cdot U_{ijt} + \gamma^U K_{ijt} \cdot U_{ijt} + \delta^U CR_{jt} \cdot K_{ijt} \cdot U_{ijt} \\ & + \beta^M CR_{jt} \cdot M_{ijt} + \gamma^M K_{ijt} \cdot M_{ijt} + \delta^M CR_{jt} \cdot K_{ijt} \cdot M_{ijt} + \eta M_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .\end{aligned}$$ [14]

[11] Note that a standard generalized DDD specification would also include $K_{ijt} \cdot D_j$ and $K_{ijt} \cdot D_t$. However, we omit these here because, for reasons explained in the context of our model for estimating the long-run effects of exposure to the EITC, the corresponding terms are not included (see footnote 19).

[12] This greater parsimony becomes valuable given that the PSID does not yield a large sample with long-term longitudinal data.

[13] Strictly speaking, δ captures the effect of the EITC only if there is no EITC for childless women (i.e., women without qualifying children). We follow this strategy here, assuming that β captures only common shocks, and that δ captures the effect of the EITC.

[14] 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.

The prior discussion of parameters, identification, etc., carries over fully to equation (2), but now in reference to β^U and δ^U for unmarried women, and β^M and δ^M for married women.

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

We expand equation (2) in a straightforward manner to estimate the long-run effects of the EITC. Instead of using a value at a particular point in time for *CR* or indicator variables for *K*, *U*, and *M*, we measure these variables at each age over a period of time (ages 22-39). Then, we calculate the interactions of these values at each age and use as our regressors the averages of these interactions over ages 22-39, with outcomes for each woman at age 40 as regressands. For example, consider the term $\delta^U CR_{jt} \cdot K_{ijt} \cdot U_{ijt}$ from equation (2). Extending this term to the long run for a woman aged 40 in period *t* yields:

$$(3)\ \delta^U_{22-39} \cdot \{ \textstyle\sum_{a=t-18}^{t-1} (CR_{ja} \cdot K_{ija} \cdot U_{ija})/18 \} \ .$$

For a woman who never has children while unmarried from 22-39, this term will equal zero. A woman who is always unmarried with children will have this term collapse to the average EITC she faced from ages 22-39. We can construct similar averages for the other terms corresponding to equation (2). 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 children, etc. For example, imagine two women who each spend nine years married from ages 22-39; however, one woman spends the first nine years married while the other spends the second nine years married. Further, imagine these women always live in the same state as one another and reach age 40 in the same year, meaning their EITC exposure, conditional on children and marital status, would be the same. Using the interactions of averages would give these women the same value for the measure in equation (3), whereas the average of interactions would be able to capture variation in exposure across these two women, assuming any EITC policy change over these 18 years.[15]

[15] One implication of this this parameterization is that we are unable to differentiate between a more-generous EITC for relatively fewer years versus a less-generous EITC over a longer period of time. However, we do not think this is empirically very important, because there are few "spikes" in the EITC, but rather a gradual evolution to a more generous EITC (see Figures 2 and 3). Arguably the federal increases in the early 1990s could expose a woman in her late 30s to a much more generous EITC than when she was younger, for a short period. However, we now show in a few different ways discussed later in the paper that we get similar (and sometimes stronger) results when we identify the effects of the EITC from state variation. Moreover, we also use specifications distinguishing the effects of exposure with young vs. older children, adding more richness to the marital and childbearing histories.

We then substitute the corresponding expressions into equation (2) to estimate the effects of these longer-run exposure variables on outcomes at age 40.[16] We also include as controls the corresponding variables for marital status, children, EITC, etc., at age 40.[17] We do this to ensure we do not confound the effects of past marriage, childbearing, and the EITC with effects of contemporaneous variables.[18]

Following this strategy, our estimating equation takes the form:

$$
\begin{aligned}
(4)\ Y_{ijt} = {} & \alpha + \beta^U\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot U_{ija})/18\} + \gamma^U\{\textstyle\sum_{a=t-18}^{t-1}(K_{ija}\cdot U_{ija})/18\} + \boldsymbol{\delta^U\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot K_{ija}\cdot U_{ija})/18\}} \\
& + \beta^M\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot M_{ija})/18\} + \gamma^M\{\textstyle\sum_{a=t-18}^{t-1}(K_{ija}\cdot M_{ija})/18\} + \boldsymbol{\delta^M\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot K_{ija}\cdot M_{ija})/18\}} \\
& + \eta\{\textstyle\sum_{a=t-18}^{t-1}M_{ija}/18\} + \beta^{U,40}CR_{ijt}\cdot U_{ijt} + \gamma^{U,40}K_{ijt}\cdot U_{ijt} + \delta^{U,40}CR_{ijt}\cdot K_{ijt}\cdot U_{ijt} \\
& + \beta^{M,40}CR_{ijt}\cdot M_{ijt} + \gamma^{M,40}K_{ijt}\cdot M_{ijt} + \delta^{M,40}CR_{ijt}\cdot K_{ijt}\cdot M_{ijt} + \eta^{40}M_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .
\end{aligned}
$$

Equation (4) looks complicated, but the parallel to equation (2) is clear, and we have retained the same notation for the key parameters, and highlighted in boldface the triple-interaction terms that identify the key coefficients – δ^U and δ^M. These now have a different interpretation, of course, as the effects on outcomes at age 40 of the cumulative history of EITC, kids, and marital status interactions. The state fixed effects are now fixed effects for the state the woman is observed living in at age 40, and the year fixed effects are now cohort effects, shared across all women who are age 40 in a particular year.

In light of this more complex, long-run specification, it is useful to consider how we identify the effects of the EITC. Paralleling our earlier discussion of δ^U and δ^M in reference to equation (2), we focus on δ^U and δ^M in equation (4) as the triple-difference estimators. In contrast to equation (2), we now measure marital status as a proportion of years from zero to one rather than an indicator variable for marital status at a particular point in time, and similarly for K. To explain what δ^U and δ^M capture, consider δ^U, for unmarried women; the discussion will carry over completely to δ^M. The term multiplying γ^U in equation (4), $\{\sum_{a=t-18}^{t-1}(K_{ija}\cdot U_{ija})/18\}$, captures the average number of years the woman was unmarried with children, and the term multiplying β^U, $\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot U_{ija})/18\}$, captures the joint history of the EITC and marital status.

[16] We vary this age in analyses reported below.
[17] We show that the results are robust to controlling for completed fertility in Appendix Table B8.
[18] In this case, the marital status and children variables are dummy variables. These age-40 control variables are not included in our analysis of cumulative experience (discussed below), as we measure that outcome over ages 22-39.

Thus, the term multiplying δ^U, $\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot K_{ija}\cdot U_{ija})/18\}$, captures the independent variation in the history of exposure to the EITC for unmarried women with children. As a result, δ^U can be interpreted in the same way as in equation (2) – but in a longer-run context. That is, it captures the relative effect of the history of exposure to the EITC for unmarried women with children, relative to unmarried women without children. Correspondingly, δ^M in equation (4) captures the relative effect of the history of exposure to the EITC for married women with children, relative to married women without children.[19]

To interpret the coefficient magnitudes, consider, for example, the unmarried women. The independent variation in the variable corresponding to δ^U, given the inclusion of the control $\{\sum_{a=t-18}^{t-1}(K_{ija}\cdot U_{ija})/18\}$, comes from the variation in *CR*. We measure the maximum credit in \$1,000 units (2016 dollars, based on the CPI-U). Thus, a one-unit increase in the variable corresponding to δ^U, $\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot K_{ija}\cdot U_{ija})/18\}$, corresponds to \$1,000 real increase in the maximum credit over the entire age range considered, for a woman who is unmarried and has children over that entire age range. That is a sizable but within-sample policy change to consider. For example, the maximum credit with two children in 1996 was \$3,556 (\$5,440 in 2016 dollars), compared to a nominal maximum credit of \$550 (\$1,204 in 2016 dollars) a decade earlier, or a real increase of more than \$4,000. However, because this implied effect is for a woman who is unmarried and has children over the entire age range we use (22-39), we scale the reported coefficients to reflect the effect of a \$1,000 *one-year* increase in the maximum credit when unmarried and with children; in practice this requires multiplying the estimate of the appropriate δ by 18.[20]

We also discussed, in reference to equation (1), how to interpret the estimates of β – which we now

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[19] One other identification issue to clarify is that we do not fully saturate the model so as to estimate the effects of the EITC only from state variation. If we look back to the short-run model – equation (1) – the standard DDD specification would also include interactions between *K* and the state dummy variables and *K* and the year dummy variables. The latter would fully absorb the federal variation in the EITC. However, in the models we estimate we have summed terms that capture the history of the EITC, marriage, and childbearing, and it is impossible to define and include in the model all the state-year interactions with all the values that the marriage and childbearing variables take on in the sample. The implication is that federal EITC variation continues to play a role in identifying the long-term effects of the EITC that we study. Of course, the key papers in the EITC literature – establishing positive employment effects of low-skilled mothers – also use federal variation (Eissa and Liebman, 1996; and Meyer and Rosenbaum, 2001) – as does the longer-term analysis in Dahl et al. (2009). However, we present other analyses below that isolate the effects of state EITC variation and find qualitatively similar and if anything stronger effects.

[20] Note that this does not change the precision of the estimate in any way, since it is a linear transformation; we are simply scaling the effects for interpretation.

extend to the coefficients β^U and β^M in equations (2) and (4). The analogous interpretation to that of equations (1) or (2) is that the terms multiplying β^U and β^M capture variation in the marital history and the EITC, and the coefficients of these variables capture shocks correlated with the EITC and marital status. Hence, as in the short-run implementation, we focus on the estimates of δ^U and δ^M .

The spirit of our approach is to apply the quasi-experimental framework 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. Furthermore, the intuition is relatively straightforward, building naturally on the types of differencing estimators based on marital status and children used in, for example, Eissa and Liebman (1996) and Eissa and Hoynes (2004), although adapted to our longer-term framework. 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.

IV. Data

PSID Data

Our data come from the Panel Study of Income Dynamics (PSID), using data through the 2017 survey (covering 2016). We need to observe long longitudinal records on women, because their "exposure" to the EITC, as explained in Section III, depends on their marital and childbearing history, as well as their (state) residential history.[21] 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

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

them on an annual basis (biennial 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.[22]

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 observed at age 40, assign age 39 to the data one year prior, age 38 to the data two years prior, and so on.[23] We assign full 19-year histories for all the necessary variables: marital status, number of children, age of children, and employment.[24,25] We begin our analysis at age 22 to avoid capturing women when they were more likely to still be in school or living with their parents, when EITC incentives may be much weaker. We arrived at using age 40 to estimate long-run effects as a balance between using a later age when women have completed the vast majority of their childbearing and the sample size shrinkage from increasing this age owing to the length of the histories we must observe.[26]

A particular strength of the PSID is that, because it spans 1967 to the present day, we are able to observe women exposed to a wide range of EITC variation. For example, the earliest cohort of women in our sample, who are 22 years old in 1967, reach age 40 in 1985. Hence, these women only receive the EITC from 1975 to 1984 when the credit was not very generous; their overall exposure was rather small. On the other hand, the latest cohort in our sample (age 22 in 1998 and age 40 in 2016) are only exposed to the EITC after the large expansion in the 1990s. These women always face a strong federal EITC alongside significant state-level variation. And the intermediate cohorts experience a broad range of EITCs between these extremes.

We assign marital status based on the Marriage History File. This file contains a series of questions

[22] To deal with the biennial nature of the data from the 1997 survey onwards, we use the previous year's state and outcome data to fill in the "missing" year. For information on children and marital status, this is not necessary given how we create those variables (described below).
[23] 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.
[24] The question about earnings refers to the past year. (For example, the data in the PSID 1968 refer to calendar year 1967.) To align age with earnings, we assign women's ages as the age they report in a year minus one. We follow the same algorithm in filling in non-survey years once the PSID data become biennial.
[25] Because we need to observe women for 19 years, we do not use the Immigrant Sample added in 1997/1999, as only a handful of women would meet our age criteria (exactly 22 in the 1997 sample).
[26] We show later that our results are robust across nearby ages.

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. This gives 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 birth history information. Women report birth timing of up to five children, allowing us to assign a detailed child history over a woman's primary childbearing years.[27] 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 stepchildren, for example, could still affect EITC benefits.[28] To assign whether the woman has younger/older children conditional on having children, we use the age of the youngest child assigned to the woman.

Earnings and hours data are available for heads of household and wives. For women who fit either of these relationship categories, we assign earnings and hours; we convert earnings into 2016 dollars. We count a woman as employed if she had positive earnings in the past year.

Additionally, we need information on two measures 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.[29] We assign educational attainment based on the woman's education level at age 40. Our primary sample restricts our analysis to low-educated women, defined as having at most a high school degree.

Finally, our analysis focuses on the cumulative effects of exposure to a more generous EITC on labor market outcomes at age 40. We also want to examine evidence on the mechanism underlying these cumulative effects, and the most obvious mechanism – especially in light of the evidence on short-run labor supply effects of the EITC – is the accumulation of labor market experience during the years of exposure. The data with which to measure cumulative experience pose some limitations. Although the first year of the PSID is 1967,

[27] A woman's birth history includes her number of live births and the birth year and month for up to five children. We therefore exclude a very small number of women (21 from our low-ed sample, 13 from our high-ed sample) who have more than five live births, but otherwise fit our sample criteria, because we cannot assign ages to each child. We are confident this number is small enough that it does not materially affect our findings. Note also that these observations are omitted, rather than including them with a mismeasured childbearing history.

[28] We constructed an alternative measure using all members of the family unit and their relations to the head, but these measures turn out to be very highly correlated, and the results using this alternative measure were qualitatively similar. Also, as eligibility requirements for the EITC are based on actual care of the child, we are implicitly assuming that all women in our sample care for each of their birth children more than half the year.

[29] For example, we cannot consistently code Hispanic ethnicity.

1978 is the first year in which employment status for all individuals of working age (16 years and older) is captured. Because we need the longest possible panel to pin down long-term effects of the EITC, we use the full panel, defining employment status at each age based on whether there are positive earnings, keeping in mind that this measure is only available for heads of household and spouses. As a result, our cumulative experience measures will not capture, for example, employment for a woman who lives with her parents at age 22, implying smaller sample sizes when estimating cumulative experience effects. In part, this motivates our selection of age 22 (as opposed to, say, 18) as our first year of exposure, ensuring women have a reasonable opportunity to establish their own households, either alone or with a partner. Of course, this concern only affects our cumulative experience measures, as our outcomes measured at age 40 only require the woman to be a head or spouse at age 40.

Table 1 shows 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 2017. Thus, only a subset of cohorts can be observed as young as 22 and as old as 40, have low education, and have a full history of state of residence,[30] which is why the available observations drop sharply from row A to row E.[31] The seven rows after row E document the relatively small number of observations we lose because of other data requirements (e.g., having a full marital history). Our final low-education primary sample includes 1,505 women.

Policy Variation

Information on the EITC comes from a database of historical parameters maintained by the Tax Policy Center.[32] Figure 2 shows the federal EITC maximum credit depending on number of children. As noted earlier, the zero-child maximum credit is miniscule. The one-, two-, and three-child maximum credits differ, but there is little independent variation (and in earlier years no independent variation), which is why we simply use one

[30] State of residence is the only variable where we fill in missing data across time. If a woman is missing state of residence in a particular year, but is observed in the previous and proceeding year living in the same state, we fill in the missing year with that state.
[31] 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 D and E of Table 1.
[32] See http://www.taxpolicycenter.org/sites/default/files/legacy/taxfacts/content/PDF/historical_eitc_parameters.pdf (viewed August 16, 2018).

measure – the two-child maximum credit; the simple correlation between the federal one-child and two-children maximum credits from 1967 to 2016 is 0.97. Figure 3 depicts information on supplemental state EITCs, which calculate their supplements as a fixed percentage of the family's federal credit.[33,34] The squares show the number of states with such supplements, rising from zero in 1983 to 25 states (including the District of Columbia) by 2016. We also show the average, minimum, and maximum state supplement rates over time. The average state supplement featured rather dramatic growth from the mid-1980s to early 1990s. However, as the EITC expanded in the mid-1990s, the credit settled down to an average of about a 20 percent supplement to the federal EITC. This has remained consistent since around 2000, although the number of states offering supplements to the federal credit increased sharply.[35]

V. Results

Descriptive Statistics

Table 2 reports descriptive statistics for our core PSID analysis sample of less-educated women. The first column shows averages across ages 22-39, and the second column at age 40 – the age at which we measure long-run outcomes. The second, third, and fourth rows report descriptive statistics on the policy variation. The next rows report on the childbearing and marriage histories, as well as the interactions between the two. Unsurprisingly, the women in our sample spend more years married than unmarried and tend to spend more years married with children than unmarried with children. Married women spend similar amounts of time with young vs. older children, whereas unmarried women spend more years with older children, presumably because they have children earlier on average. Further, by age 40, women are unlikely to still have young children, regardless of marital status. 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 increases variation in the

[33] 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.
[34] Wisconsin, the lone exception, also uses fixed percentages of the federal EITC, but these rates vary by number of children.
[35] Appendix Figures B1 and B2 show the information corresponding to Figures 2 and 3, but for phase-in rates. Comparing the figures, it is clear that these alternative policy measures are highly correlated, which explains why our results are robust to alternative parameterizations of the EITC.

independent variables, which can increase precision of the estimates;[36] but we show that the results are not sensitive to weighting. Finally, the last rows report descriptive statistics for the outcomes measured at age 40. The low-ed women in our primary sample accumulate, on average, around 13 years of experience over ages 22-39. Recall that we define a year of experience as reporting positive earnings for that year. Thus, these women have positive earnings in around 72 percent of years and that number is marginally higher at age 40, when 78 percent of our sample has positive earnings.

Results from Simple Specification[37]

Table 3 presents estimates from the regression models used in the simplest version of our specification for estimating the effects of long-run exposure to the EITC – equation (4). The table reports the estimates and standard errors of the two coefficients of interest in this specification – δ^U and δ^M – the coefficients on the averages of the triple interactions between the EITC maximum credit and the kids and marital status variables from ages 22-39.[38] We show results for employment, log hourly wages (conditional on employment), log earnings (conditional on employment), annual hours, and annual hours conditional on employment – all at age 40. We focus on the results for earnings and hours – which we regard as the key outcomes. Recall that the coefficients in Panel A are scaled to be interpreted as effects of a one-year, $1,000 (in 2016 dollars) increase in the maximum EITC.[39]

As shown in the first row of column (3), the estimated effect of the EITC on earnings (conditional on

[36] 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 – which is based on the prior generation's income – seems far less likely to be endogenous.)

[37] We have explored 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 we could replicate the simpler contemporaneous results from the earlier literature. If not, then our analysis might not have a chance to be very informative. We present and discuss the results in Appendix B. In short, we show that we generally can replicate the results from these papers with the PSID data.

[38] The full model estimates are available upon request, as are any other estimates we discuss but do not report in the paper or the appendix.

[39] Later, we also report the implied effects of long-run exposure to a higher maximum EITC – effects that may better capture the effects of meaningful policy differences.

employment) for women exposed to a more generous EITC when unmarried with children is positive (0.005). A positive effect is consistent with the short-run positive extensive labor market effects of the EITC for unmarried women with children translating into higher earnings in the longer run, likely in part through the accumulation of experience. While positive, the estimated effect is not statistically different from zero. In the second row, we find a negative estimate for married women exposed to a more generous EITC when they have children. The estimated absolute magnitude is larger (0.014) and is statistically significant at the 5-percent level.

We report the effects on annual hours (without conditioning on employment) in column (4). The notable result here is the lower hours at age 40 worked by women exposed to a more generous EITC when they were married with children – a significant hours differential of 7.73 hours. Column (5) also estimates the effects of long-run exposure to the EITC on annual hours, restricting the sample to women with positive hours at age 40. Relative to column (4), both estimates become larger and more precise. In particular, the effect of an additional year of exposure to a $1,000 higher EITC for married women with children yields 9.08 fewer annual hours (1-percent significance), implying that earnings effect captured in column (3) is driven mainly by an intensive-margin hours difference at age 40.

For employment, in column (1) neither of the estimated coefficients reported are statistically significant, and the sign pattern – unlike for earnings and hours – does not suggest that exposure to a more generous EITC when unmarried with children is associated with higher employment at age 40, nor that such exposure when married with children is associated with lower employment at age 40. The negligible contemporaneous employment effects reinforce the conclusion that the hours effect for married women is mainly an intensive-margin effect, in line with what the short-run literature finds for married women when a labor supply effect is detected. The results for wages, reported in column (2), are consistent with the results for conditional earnings (column (3)) that we previously discussed, although statistically insignificant. Women exposed to a more generous EITC when unmarried with children tend to have slightly higher wages at age 40. The estimated effect is for unmarried women is about 0.1 percent per year of exposure, while the estimated effect is more negative for women exposed to a more generous EITC when they were married with children (−0.6 percent per year).

Distinguishing Effects by Age of Children

Next, we refine our specification to more fully capture the incentive effects of the EITC for women with children, separating the indicator for having children into two separate indicators based on whether the youngest child is at least six years old (again defined at each age from 22-39). The short-run labor supply effects of the EITC could differ depending on whether a woman has young children at home. The most obvious difference we would expect is that the positive extensive-margin effects for unmarried women would be stronger once children reach school age, because of how much young children increase the reservation wage.[40] There is an additional rationale for this. The summed terms in equation (4) can take on the same values for different histories of marriage, childbearing, and the EITC. We average because it is infeasible to estimate separate effects for all (or a large number of) different histories. But breaking these terms into those associated with younger vs. older children allows more richness in the histories.

This difference is straightforward to incorporate into our framework. First, we split the terms involving K in equation (4) into two separate terms for having older kids only (OK) or having younger kids (YK). We define the dummy variable YK for each year to equal one if a women has any children age 5 or younger, and define OK to equal one if all children are age 6 or over; we use superscripts Y and O on the δ parameters to denote this difference. With this change, each term involving K in equation (4) becomes two terms. Most importantly, the two triple-difference terms become:

(5) $\delta^{UY}\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot YK_{ija}\cdot U_{ija})/18\} + \delta^{UO}\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot OK_{ija}\cdot U_{ija})/18\}$

and

(5') $\delta^{MY}\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot YK_{ija}\cdot M_{ija})/18\} + \delta^{MO}\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot OK_{ija}\cdot M_{ija})/18\}$.[41]

The δ coefficients, of which there are now four – δ^{UY}, δ^{UO}, δ^{MY}, and δ^{MO} – capture the effects of exposure to the EITC when women are unmarried or married and have children in a particular age range

[40] There is ample evidence younger children affect labor supply of mothers differently, a small portion of which pertains directly to the EITC. As examples, Blau and Tekin (2007) shows that child-care subsidies increase women's employment. Bainbridge et al. (2003) show this explicitly in relation to single mothers with young children – most closely related to the EITC. Less directly, Heinrich (2014) reviews evidence on the effects on young children of mothers' employment, motivated by increased reliance on the EITC. And Gelbach (2002) shows that availability of public schooling for young children increased mothers' labor supply.

[41] More details on the full estimating equation are provided below, after we discuss another extension of our analysis to identify separate effects of state and federal EITC variation.

(either at least one young child or all school-age).

The estimates are reported in Table 4. Focusing first on the results for earnings (conditional on employment), in column (3), the estimates demonstrate the importance of including this distinction. A one-year, $1,000 increase in the maximum EITC a woman faces is associated with only a 0.1 percent increase in conditional earnings when she is unmarried with young children, but a 1.4 percent increase in earnings (significant at the 5-percent level) when she is unmarried and all of her children are school-age. The difference is less stark for married women, with the same EITC increase associated with a 1.0 or 1.3 percent (significant at the 10-percent level in the latter case) decline in earnings for women with young or older (only) children, respectively. In column (4), for annual hours, the negative effect for married women with young children is quite strongly negative (−15.58 annual hours) and highly significant (1-percent level). The conditional hours effects in column (5) also indicated negative effects for exposure when married, although in this case stronger for married women with older children, and a positive (albeit insignificant) effect (5.58) for exposure of unmarried women with older children. These estimates explain the effects we find on earnings but not on wages. Namely, exposure to a more generous EITC is associated with more hours worked at age 40 when unmarried and fewer hours when married. Based on these results, going forward most of our analyses distinguish the effects of EITC exposure by age of children.

Results from Preferred Specification using State and Federal EITC Variation

We next define our EITC exposure variables to estimate the separate effects of federal and state policy variation. One potential critique of the short-run EITC literature is that much of the federal variation in the EITC came during a very short window in the mid-1990s, making it hard to distinguish the effects of federal EITC changes in this period from other policy changes in the same period that could have affected women differently based on marital status of number (and ages) of children, such as welfare reform (e.g., Kleven, 2019). We think this criticism is less relevant to our findings, because we also exploit state EITC variation, as well as rich variation across women with different childbearing and marital histories. We also benefit from the panel nature of the PSID itself and its long history, which allows us to follow women’s childbearing and marital decisions over a significant period before, during, and after this expansion.

We can explore this question more directly, however. As shown in Figure 3, most state credits were enacted post-1996, so the effects identified using state credits come from a period with a stable federal EITC. If we can establish that the effects estimated in Table 4 are as strong or stronger when we focus on the state variation, then it is more likely that we are identifying a causal effect of the EITC.

To do this, we expand our specification to distinguish the effects of federal and state EITC variation. In our estimating equation, each CR_{jt} term in equation (5) now has two versions – $CRFed_{jt}$ and $CRState_{jt}$. The key two terms in boldface from equation (4) now expand to eight terms, reflecting the distinction by age of kids (already discussed) and by federal and state EITC variation, to become:

$$(6) \quad \delta^{UYF}\{\textstyle\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot YK_{ija}\cdot U_{ija})/18\} + \delta^{UOF}\{\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot OK_{ija}\cdot U_{ija})/18\}$$
$$+ \delta^{MYF}\{\textstyle\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot YK_{ija}\cdot M_{ija})/18\} + \delta^{MOF}\{\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot OK_{ija}\cdot M_{ija})/18\}$$
$$+ \delta^{UYS}\{\textstyle\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot YK_{ija}\cdot U_{ija})/18\} + \delta^{UOS}\{\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot OK_{ija}\cdot U_{ija})/18\}$$
$$+ \delta^{MYS}\{\textstyle\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot YK_{ija}\cdot M_{ija})/18\} + \delta^{MOS}\{\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot OK_{ija}\cdot M_{ija})/18\}\ .$$[42]

The results, reported in Table 5, indicate that the effects of state EITC variation tend to be, dollar for dollar, both larger in magnitude and stronger in terms of statistical significance. This leads us to conclude that our estimated effects are not driven by federal variation in the EITC, and certainly not by any particular expansion of the federal credit.

The estimated coefficients in Panel A measure the effects of a one-year, $1,000 increase in the appropriate maximum credit. This is useful for comparing the effects of an additional maximum credit amount coming from one of the two sources (although below we consider different ways to interpret the estimates). Viewed this way, the estimated coefficients indicate two things. First, the overall story is that, for unmarried women, effects tend to be more positive when their children are old enough to attend school and, for married women, having younger children enhances the negative effects of exposure to a more generous EITC. Three of the unmarried coefficients and five of the married coefficients in Panel A of columns (3), (4), and (5) are statistically significant (at the 10-percent level or better).

[42] The full estimating equation is provided in Appendix A. The specification corresponding to equations (5) and (5') and Table 4 would collapse the state and federal credit terms (*CRState* and *CRFed*) to a single credit variable (*CR* in equations (5) and (5')).

Second, the point estimates of the impacts of the state maximum credits are generally larger and/or in the direction predicted by theory tying short-run effects of the EITC to longer-run effects. For the estimates for earnings and hours, this is true in every case. For example, the estimated hours effect of exposure when unmarried with older children is much larger for the state variation, and significant only for the latter; and the negative effect on earnings (conditional on employment) of exposure when married with young children is much larger for the state variation. It may appear inappropriate to directly compare a $1,000 increase in the federal EITC and a $1,000 increase in the state EITC, as they represent different degrees of policy change. Looking back to Figures 2 and 3, the federal EITC has expanded from nothing in the earliest years of our sample to well over $5,000 in 2016; multiple $1,000 increases in the federal maximum credit have occurred across our sample period. On the other hand, the average state supplement since the mid-1990s has been worth around $1,000, so one could think about a $1,000 increase in the state supplement as a state implementing an EITC at an average level of generosity. Within our sample, we have numerous instances of such policy changes – occurring in about half of states.

The federal credit coefficients in Table 5 can be interpreted the effects of a federal EITC expansion in a state without a supplement, whereas the state coefficients capture an expansion of the state maximum credit holding the federal credit fixed (since states supplement the federal EITC by a fixed percentage). The state credit can also increase, however, because of a federal expansion. Given this interaction between the state and federal credits, it is also useful to interpret the implied combined effects in Table 5 in a manner that facilitates comparisons with the estimates in Tables 3 and 4. To do this, in Panel B we translate our estimates into the implied effects of a one-year increase of $1,000 in the federal maximum credit coupled with an additional $200 from the state supplement. This assumes an increase in a state where the supplement is 20 percent of the federal credit; we chose this level because the average supplement across 1967 to 2016, conditional on one existing, is 18.6 percent.[43]

As in Tables 3 and 4, we do not find any significant effects in Panel B from exposure to a more generous EITC on employment or log wages in columns (1) and (2), but we do find effects on earnings and

[43] Visually, one can see this as the ratio between the heights of the two-child state supplement maximum credit (~$1,000 from 1996 onwards) in Figure 3 to the height of the federal two-child EITC (~$5,500 from 1996 onwards) in Figure 2.

hours. Moreover, the effects are larger and more strongly statistically significant. In particular, eight of estimated coefficients in columns (3), (4), and (5) – for conditional log earnings and our two annual hours measures – are significant (five at the 1-percent level and all at the 5-percent level). The differences for unmarried women are most striking. For unmarried women with older (only) children, a one-year increase in the maximum credit is associated with a 2.2 percent increase in earnings and 15.16 more annual hours worked (15.65 conditioning on positive annual hours); this positive intensive-margin effect for unmarried women differs from what the short-run literature usually studies and reports – which is a positive extensive-margin effect of the EITC. We also find larger earnings and hours effects for married women (negative, as before).[44]

We view these estimates as 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). Additionally, the EITC is more likely to reduce hours among married women with children (although this evidence in the existing literature is much weaker). The evidence in Table 5 suggests that these short-run effects are reflected in long-run, cumulative effects on earnings and hours.

Mechanism

The estimates to this point suggest that, for unmarried mothers, exposure to a more generous EITC over ages 22-39 (when kids are older) leads to higher hours and earnings at age 40. This presumably reflects longer-run human capital effects from exposure to a more generous EITC that encourages work in the short term, which should lead to the accumulation of more work experience.[45] These estimated effects are consistent with the accumulation of the effects predicted by the static model and confirmed by short-run evidence. Similarly, the estimates indicate longer-run negative effects on married mothers exposed to a more generous

[44] Appendix Table B9 breaks the EITC variation into federal and state without estimating separate effects by age of kids (and hence more closely parallels Table 3 than Table 4). We find positive hours effects (unconditional and conditional) for unmarried women, which are somewhat smaller than those for unmarried women with older children in Table 5 (reflecting the averaging across women with young and with older children). Similarly, Appendix Table B9 finds negative effects on employment and both hours measures for married women, somewhat smaller than the effects for those with young children in Table 5.

[45] In addition, greater labor force attachment spurred by a more generous EITC might boost other human capital investments or increase effort to find better jobs with prospects for more wage growth, although, admittedly, although our wage results, while consistent with this theory in sign, are not statistically significant. Human capital effects could also include more investment in education, although examining this would require a different identification strategy than the one we use, which stratifies on education.

EITC, presumably because of similar effects in the opposite direction from the EITC discouraging work.

Before testing the robustness of our results to potential identification threats, we first provide evidence for the mechanism we believe underlies our long-run findings. Specifically, Table 6 examines evidence on the effects of EITC exposure on cumulative experience between ages 22 and 39. We estimate the same specification used in Table 5, but now for two different cumulative experience measures: the number of years with positive earnings from age 22-39; and the cumulative hours worked over these ages (divided by 2,000 to obtain a measure in units of full-time years of work).[46]

The results for less-educated women – paralleling our analyses thus far – are reported in columns (1) and (2).[47] Looking at the federal variation, for the cumulative years of work measure we find significant positive effects of exposure of unmarried mothers to a more generous EITC. The cumulative hours estimates in column (2) are of the same sign for unmarried mothers, but are less precise, which is unsurprising given the hours variation. However, for the state variation the estimates are very similar, statistically significant, and larger. For married mothers there is not evidence of effects on cumulative experience. Looking at the combined effect for unmarried mothers, in Panel B, for example, we find that exposure over ages 22-39 to a $1,000 higher maximum federal and $200 higher state credit for one year boosts years of employment by 0.081 years while unmarried with older (only) children.[48] Overall, then, Table 6 suggests that our longer-run evidence for unmarried mothers stems in part from cumulative responses on the extensive margin of employment.

VI. Robustness Analyses

We now turn to analyses intended to probe the robustness of the results along some key dimensions. It is useful to provide the punchline first: The qualitative results are robust, and the statistical strength of the evidence generally varies little. In addition to the analyses described here, in Appendix B we discuss results

[46] Recall that these are measured for heads and spouses only.
[47] We discuss columns (3) and (4) later, in the context of a different alteration to our specification.
[48] We can compare these estimated cumulative experience effects for unmarried mothers with the estimated wage effects from Table 5. For example, for the corresponding estimate, the higher maximum credit was estimated to increase hourly wages by about 0.6 percent. If the return to experience is about 3 percent, then the implied impact on wages of 0.081 more years of experience is 0.24 percent. The smaller impact could indicate other types of investment, including more intensive search for better paying jobs with stronger prospects for earnings growth, are spurred by the short-term positive extensive-margin employment effects of the EITC for unmarried mothers.

and report estimates for two additional robustness analyses (in addition to some of the more minor robustness results we have reported in the footnotes). We show that the results are robust to weighting (Appendix Table B3); and related to the weighting, we show results that are generally quite similar for black and non-black women (Appendix Table B4), with the one exception discussed in Appendix B.

Alternative Ages

We first explore the robustness of the results to altering the age at which we measure longer-run outcomes – using ages 38, 39, 41, and 42 (in addition to age 40). We do not extend far beyond this range because we suspect at younger ages the longer-run effects are less likely to be evident, and using much older ages would sharply reduce the sample. We create a sample for each age, assigning everything as we describe for the age 40 sample, albeit over slightly different age ranges. Next, we estimate the long-run effects of the EITC for each age group on each of our outcomes using the same specification as in Table 5.

Figure 4 summarizes the results, reporting the results in terms of the effects of a one-year, $1,000 real increase in the federal and a $200 increase in the state maximum credits (like in Panel B of Table 5). Our strongest and most consistent results were for earnings (conditional on employment) and hours. For these outcomes, the results are often consistent regardless of which age we use, and in some cases the results are quite consistent, especially for earnings for unmarried women with older children, for hours of unmarried women regardless of children's age, and for both earnings and hours (conditional and unconditional) for married women regardless of children's age.[49] Moreover, the evidence sometimes indicates a pattern of rising effects with age, suggesting that these longer-run effects become more apparent at the older ages we consider (e.g., for annual hours for married women with older kids, and for earnings for unmarried mothers with older kids). Most importantly, the figure indicates there is nothing unique about the age of 40 for which we have presented most of our analyses.

Alternative State Supplement Parameterization

Next, we show an alternative parameterization of state EITC supplements that more cleanly separates their variation from the federal policy. As previously discussed, changes in the federal policy mechanically

[49] The employment and especially the wage results are noisy and likely as a result somewhat inconsistent, but this is not surprising given that we did not find significant results for these outcomes.

alter state credits that are a fixed percentage of the federal credit. We eliminate this channel while keeping the state credits comparable in Table 7, where the state supplements are now calculated as percentages of the federal EITC rather than in dollars. The interpretation of the federal credit coefficients stays the same as in the previous tables, and the state coefficients in Panel A are scaled to be interpreted as the effect of a one-year, 10-percentage point increase in the state supplement. As columns (3), (4), and (5) show, most of the unmarried coefficients are positive (with three statistically significant) and all of the married coefficients are negative (with two statistically significant). Panel B keeps the $1,000 federal maximum credit increase but adjusts the state portion to be a one-year, 20-percentage point increase, as this would generate the same $200 state increase as in Table 5. Each unmarried estimate for log earnings and annual hours is more positive than its Table 5 counterpart and each married estimate is more negative, although the estimates in Table 7 tend to be slightly less precise.[50]

Endogenous Behavior

One type of endogenous behavior 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. Given that our results suggest that women who face a more generous EITC when they have children and are unmarried have higher earnings (conditional on employment) at age 40, the concern is that women who would have had higher earnings 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. This would generate a non-causal relationship between later earnings and our measure of exposure to a more generous EITC when unmarried with children.

[50] In Appendix Table B10, we show a potential alternative to isolating the effects of the state variation, where we project the 1992 federal EITC parameters forward to 2016, only adjusting for inflation, while maintaining the actual state credit percentages. One might be concerned that the state supplements are themselves endogenous to the federal policy. But Appendix Table B10 would represent the lower bound on what state credits would be in the absence of the true federal parameters. The results reported in Appendix Table B10 do not change our conclusions; indeed, consistent with our main analysis showing stronger effects of state EITC variation, we find somewhat stronger results when we "turn off" the federal variation. Thus, we are confident that state credits provide valuable identifying variation independently of the variation driven by federal policy changes.

With respect to marriage, the mechanics of the EITC might also generate endogenous selection. A woman who earns enough to put her on the phase-out range would lose at least part of her EITC payment if she marries, as long as the spouse has positive earnings. Similarly, a 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 receive 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 it 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.

The mechanics with respect to childbearing are simpler. 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, 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.

To assess this issue in our data, we first consider the question of the potential endogeneity of childbearing. To do so, we estimate models like those reported in Table 5, but defining as dependent variables the fraction of years from ages 22-39 that a woman spent with any kids, with young kids, and with older kids (only), as well as completed fertility. Our independent variables become simpler: we include the exposure to EITC variables, but without any interactions with children. Thus, our estimating equation becomes:

(7) $Y_{ijt} = \alpha + \beta^{UF}\{ \sum_{a=t-18}^{t-1}(CRFed_{ja} \cdot U_{ija})/18\} + \beta^{US}\{ \sum_{a=t-18}^{t-1}(CRState_{ja} \cdot U_{ija})/18\}$

$$+ \beta^{MF}\{ \textstyle\sum_{a=t\text{-}18}^{t\text{-}1}(CRFed_{ja}\cdot M_{ija})/18\} + \beta^{MS}\{ \textstyle\sum_{a=t\text{-}18}^{t\text{-}1}(CRState_{ja}\cdot M_{ija})/18\} + \eta\{ \textstyle\sum_{a=t\text{-}18}^{t\text{-}1} M_{ija}/18\}$$

$$+ \beta^{UF,40}CRFed_{ijt}\cdot U_{ijt} + \beta^{US,40}CRState_{ijt}\cdot U_{ijt} + \beta^{MF,40}CRFed_{ijt}\cdot M_{ijt} + \beta^{MS,40}CRState_{ijt}\cdot M_{ijt} + \eta^{40}M_{ijt} + D_j\theta$$

$$+ D_t\lambda + \varepsilon_{ijt} \ .$$

We report the estimates from these models in Part I of Table 8. We do not find exposure to a more generous EITC increased childbearing – measured as completed fertility. The federal credits are positively associated with higher completed fertility while the state credits have an opposite-signed relationship, but the estimates are insignificant. Translating these effects in Panel B, again, there is not a consistent sign pattern and neither estimate is statistically different from zero. While completed fertility shows no clear pattern between EITCs, columns (1)-(3) suggest there may be small effects on timing. Specifically, unmarried women exposed to more generous EITCs tend to have children slightly earlier (increasing the proportion of years with children and with older children), while married women may slightly delay childbirth.

However, a back-of-the-envelope calculation suggests this evidence has minimal implications for our estimates. Using the estimated coefficients in column (1) of Panel B in Part I of the table, along with the observed proportions of years unmarried and married from Table 2, we can calculate that 18 years of exposure to a \$1,000 higher federal and \$200 higher state maximum credits would lead to 0.019 *fewer* years with children.[51] The fact that this number is so small in magnitude and is opposite the predicted sign helps reassure that our results are not driven by endogenous fertility responses to the EITC.

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. Estimating these effects in a similar fashion, we modify equation (7), estimating it for the fraction of years married, but re-introducing the interactions between the EITC and the childbearing variables (fractions of years with children) in place of the interactions with marital status. Part II of Table 8 reports the estimates. Panel B translates them into implied effects of a one-year \$1,000 federal maximum and \$200 state maximum credit increase. The estimates are negative for women with both young

[51] The full calculation is [0.003×(0.32) + (−0.003) ×(0.68)]×18 = −0.019. Inside the square brackets we multiply the coefficients from Table 8, Part I, Panel B by the average proportion of years the low-ed women in our sample are unmarried or married (Table 2, rows 8 and 9). This gives the effect of one year of exposure to a \$1,000 higher federal and \$200 higher state EITC. We then multiply this by 18 years to get exposure across all ages from 22-39.

and older (only) children, consistent with delayed marriage when the EITC is more generous. But interpreted this way, the estimated effects of the EITC on marriage are also very 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 conditional earnings and hours (conditional and unconditional) in the longer run, and the opposite for married women with children. Nonetheless, this evidence should best be viewed as suggestive and indirect, and does fall short of a strategy which fully endogenizes marital and fertility behavior, as in the kind of structural model we alluded to earlier, but do not pursue in this paper.

Next, we explore the possibility that endogenous migration could influence our findings. In principle, lower-skilled women eligible for the EITC who are more interested in working, who – as suggested by our evidence thus far – work more and eventually have higher earnings and hours, could migrate to states with more generous EITCs, generating spurious evidence of the positive effects of exposure to a more generous EITC. Our first check, in Part I of Table 9, is simply to apply the EITC policy from a woman's 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 estimates in Table 5. A second check is to use only federal EITC variation, which is unaffected by interstate migration. These estimates, reported in Part II of Table 9, are also very similar. Thus, we conclude that migration does not bias our estimated effects.[52]

Endogenous Policy Variation

Next, we 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 for the possibility that there are unmeasured shocks that vary by state and year and across women with different marital status and childbearing histories, as long as we are willing to assume that these shocks are similar across women of different education levels. For this analysis, we pool the less-educated women we have studied thus far and

[52] 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.

women with higher education, create a dummy variable for the less-educated women, and include all the variables in the preferred model on their own as well as interacted with the dummy variable for low education; the main effect of low education is also included. Note that we interact our low-education indicator with the year dummy variables, to allow for 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.[53] In this case, the estimated coefficient on the latter interactions are the effects of longer-term exposure to the EITC, but they are now identified relative to more-educated women (a DDDD estimator), with the interactions between the EITC, marriage, and fertility variables for more-educated women potentially serving as control variables for other types of shocks correlated with EITC changes not picked up in the other controls.

The results, reported in Table 10, are qualitatively very similar to those in Table 5. We find positive effects on earnings (conditional on employment) for unmarried mothers exposed to a more generous EITC over the longer run, and negative effects for married mothers. The fact that the estimated EITC effects do not change when they are identified relative to more-educated women suggests that these effects do not reflect other shocks to longer-term labor market outcomes for women distinguished by marital status and children that happen to be correlated with EITC variation. Put differently, the estimates for more-educated women serve as a placebo test; given that the EITC should have little or no impact on more-educated women, we should find little or no evidence of effects on these women if our EITC effects reflect causal effects on less-educated women. The estimates for more-educated women, reported in Appendix Table B11, confirm that this is the case. Finally, going back to Table 6, for cumulative experience, columns (3) and (4) show that when we add data on more-educated women as additional controls, and estimate effects relative to them, the estimates are again qualitatively similar, with the results we for unmarried women becoming if anything a bit stronger.[54]

The previous analysis can be viewed as controlling for a source of non-exogenous variation in the EITC that threatens the interpretation of our estimates in Table 5 as causal – specifically, the possibility that

[53] To be symmetric, we might want interactions between the low-education indicator 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.

[54] Implicit in the similar estimates for less-educated women is that the estimates for more-educated women are small and insignificant, which is indeed the case for cumulative experience, as well.

EITC variation is correlated with other shocks or factors affecting longer-run outcomes for different kinds of women differentiated by their childbearing or marriage histories. As an alternative approach, we allow more explicitly for a relationship between EITC variation and trends in these longer-run outcomes, but reverting to using only the less-educated women and introducing state-specific linear time trends. Recent work (e.g., Meer and West, 2016) has highlighted potential limitations to identifying policy effects using this common augmentation of panel data estimators with state policy variation. However, given that we have a long period prior to the major EITC expansions in the 1990s, the problems Meer and West identify are much less likely to apply. The results are reported in Table 11. They are often a bit less precise than the preferred estimates in Table 5, but the point estimates and the qualitative conclusions are very similar.

Potentially Confounding Changes in Other Policies

There may also be longer-run effects of other policies that affect work or work incentives, perhaps most notably the minimum wage and welfare, given the timing of welfare reform and that many states increased both minimum wages and their EITC in the 2000s.[55] 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. The concern is perhaps most salient for the welfare reforms that occurred in roughly the same period (the 1990s) as large expansions in the EITC.

It is infeasible to code up numerous features of welfare – in particular, how they changed when the 1996 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. We include two measures of welfare generosity or reform that we believe capture key variation in a parsimonious way. Our first measure is the maximum payment for a family of three, usually held

[55] 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. (forthcoming) estimate the longer-run effects of minimum wages, the EITC, and welfare reform – albeit with a focus on initially disadvantaged areas, rather than individuals.

to be one adult and two dependent children.[56] Second, for the post-welfare reform period, we include a dummy variable for whether a state imposed tight time limits. Time limits seem like a good choice to capture the effects of welfare reform, as 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 these limits ranged from 21-48 months, but were generally around 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. We enter these variables in the same way as the EITC, with the average of interactions with kids (by age) and marital status across ages 22-39.

The estimates incorporating these two explicit welfare and welfare reform measures are reported in Part I of Table 12. The estimates are quite similar to their Table 5 counterparts, and in some cases are even stronger, indicating that changes in welfare, including a key element of welfare reform (and changes correlated with it), do not underlie our estimated effects of the EITC.[57]

In Part II, we instead add controls for the minimum wage. We enter the minimum wage in the same

[56] 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 two. We always use the former when possible.

[57] We also experimented with a much less parametric approach using dummy variables that vary by state over time, intended to capture broad policy changes associated with welfare reform. One was for the granting of welfare waivers in the period between 1992 and the TANF rollout (in the states that received waivers), and the other was for the rollout of TANF in the state. We identified the month in which either of these occurred, using information from the U.S. Department of Health and Human Services (see https://aspe.hhs.gov/system/files/pdf/180711/Table_A.PDF, viewed August 13, 2018). 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 used these welfare reform variables in the same was as we do the EITC policy variable – i.e., interacted with the dummy variables for children and married/unmarried. The estimated effect of federal variation were much less precise, which is 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 federal policy effects. However, the estimated effects of the state variation were very similar to those in Table 12 (Part I), and the estimated effects of the combined variation (corresponding to Panel B) were also generally qualitatively similar, with statistically significant positive effects on hours for exposure of unmarried mothers, and statistically significant negative effects on earnings and hours for exposure of married mothers.

way as the EITC, with the average of interactions with kids (by age) and marital status across ages 22-39; in each year, we use the real minimum wage (using the higher of the state or federal minimum). A comparison of the estimates in Panel B with those in Table 5 shows that adding the minimum wage controls has virtually no impact on the estimates.[58,59]

Of course, we cannot decisively rule out the concern about confounding policies, as other policies that changed simultaneously with the EITC could exist. However, the relevant policy changes would have to differentially affect women based on income (proxied by marital status) and presence of children.[60]

Translating Our Estimates into Implied Effects of Longer-Run Policy Change

Finally, we turn our attention to using our results to estimate the long-run effects of persistent changes in the EITC that prevail across the entire 22-39 age range, rather than the one-year effects reported in the preceding tables. The simplest method to translate our effects to the long run might appear to be to multiply each estimated δ by 18, turning a one-year increase into the same increase in each year from ages 22-39. This would not be quite right, however, as most women will not spend all 18 years with young or older (only) children. To deal with this concern in a parsimonious fashion, we create two “scenarios” in which women have two children, at ages 22 and 24, and are always unmarried or married. Thus, the hypothetical woman in each scenario will have children in every year from age 22 to age 39, with young children for 8 of 18 years and older (only) children for the remaining 10 years. At the same time, holding marital status constant will avoid conflating the generally positive unmarried and generally negative married effects or having to assume when women are married. That is, these scenarios let us measure the long-run estimates of strong, yet plausible, exposure to a more generous EITC over a long period.

The results are shown in Table 13, for each of our previous analyses, although for the hours estimates we report only the conditional results; the unconditional estimates were similar. Every estimate in each column

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[58] 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.

[59] We generally do not find significant longer-run effects of welfare reform or the minimum wage on outcomes at age 40.

[60] Other potential policies that might be of concern include changes in tax class (i.e., adding a dependent or a switch from single to head of household for a single mother having her first child) and the Child Tax Credit. However, because these policies are not refundable in the same way as the federal EITC (and many state EITC supplements), we believe they can be safely ignored. The Child Tax Credit did become partially refundable (up to $1,400) under the Tax Cuts and Jobs Act of 2017, but that change occurs occurred after our sample period. Moreover, some recent surveys do not emphasize or point to employment effects of the CTC (Hungerford and Thiess, 2013; Marr et al., 2015).

and panel has the same sign, and many of the estimates are statistically significant. Looking at the estimates for Table 5 as our preferred specification, the estimates imply that, for unmarried women, a permanent $1,000 increase in the federal maximum credit, and $200 increase in the state maximum credit, boosts earnings at age 40 by approximately 24.4 percent, and hours at age 40 by 190. For married women, the corresponding implied effects are approximately 36.1 percent lower earnings, and 277 fewer hours.

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 employment, wages, earnings, and hours as mature adults.

We find evidence suggesting that exposure to a more generous EITC when women were unmarried and had older (school-age) children leads to higher earnings (conditional on employment) in the longer-run. We also find corresponding evidence suggesting that longer-run exposure of unmarried mothers to a more generous EITC increases cumulative labor market experience, using data with somewhat more limitations. Finally, we find evidence to suggest 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, which influences earnings. We present many supplemental analyses that show that the findings are robust, and these bolster a causal interpretation of the evidence. However, some of the evidence in support of a causal interpretation is supportive, but not definitive, and further research could perhaps fruitfully explore treating childbearing, marriage, and/or policy variation as endogenous.

Overall, the results provide support for concluding that a more generous EITC does more than simply boost 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. Indeed, 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. At the same time, the long-run evidence also points to lower earnings and hours for women exposed to a more generous EITC when married with children.

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Figure 1: Federal EITC (2016)

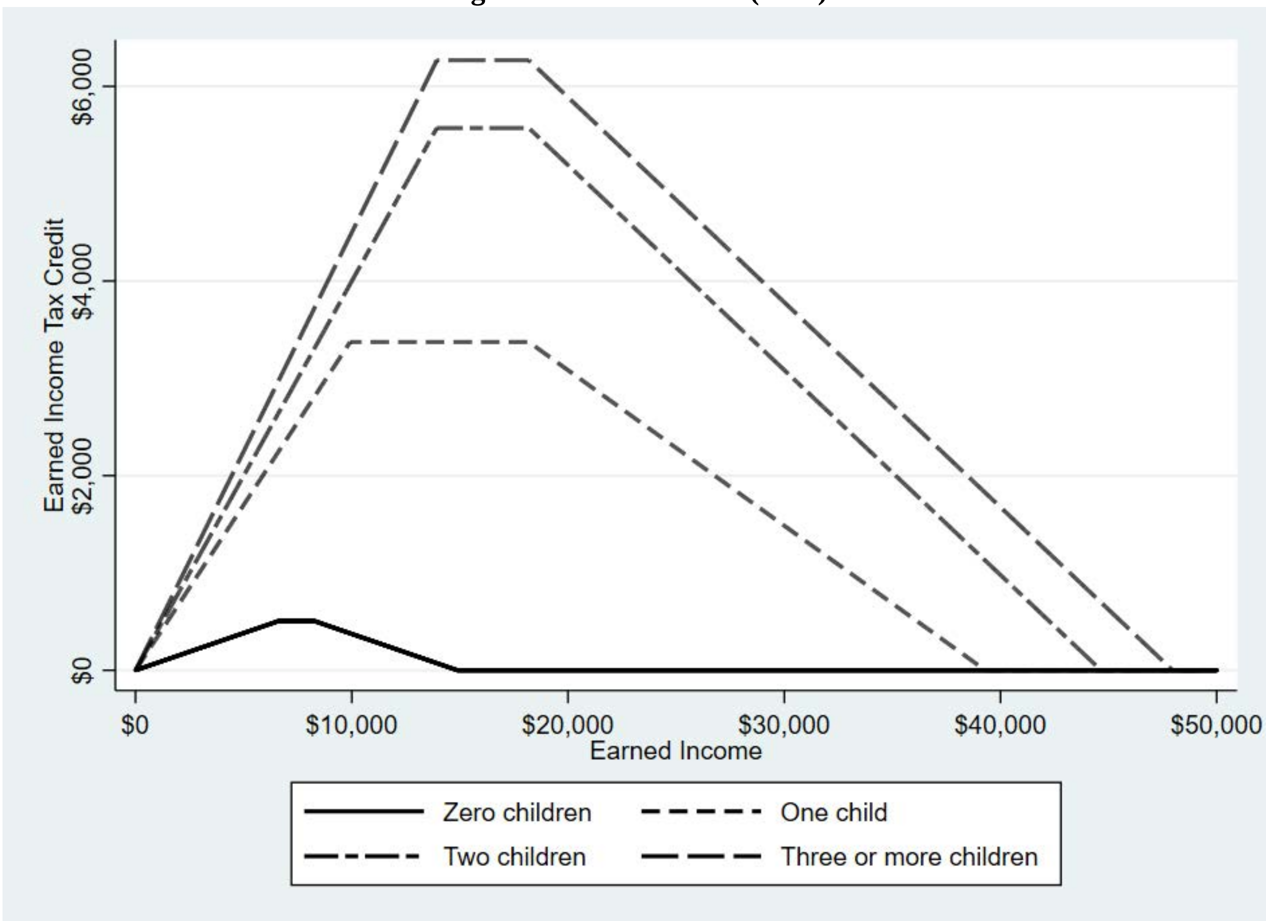


Figure 2: Federal EITC Maximum Credit

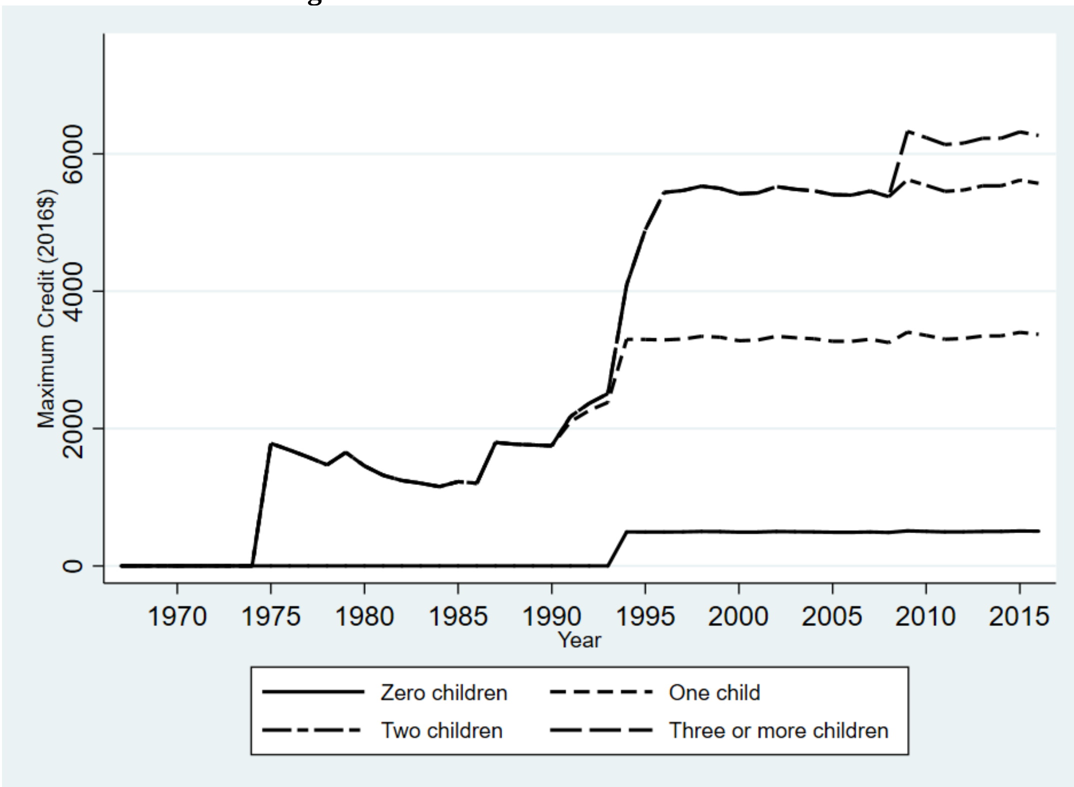


Figure 3: State EITC Supplements (2016 Dollars)

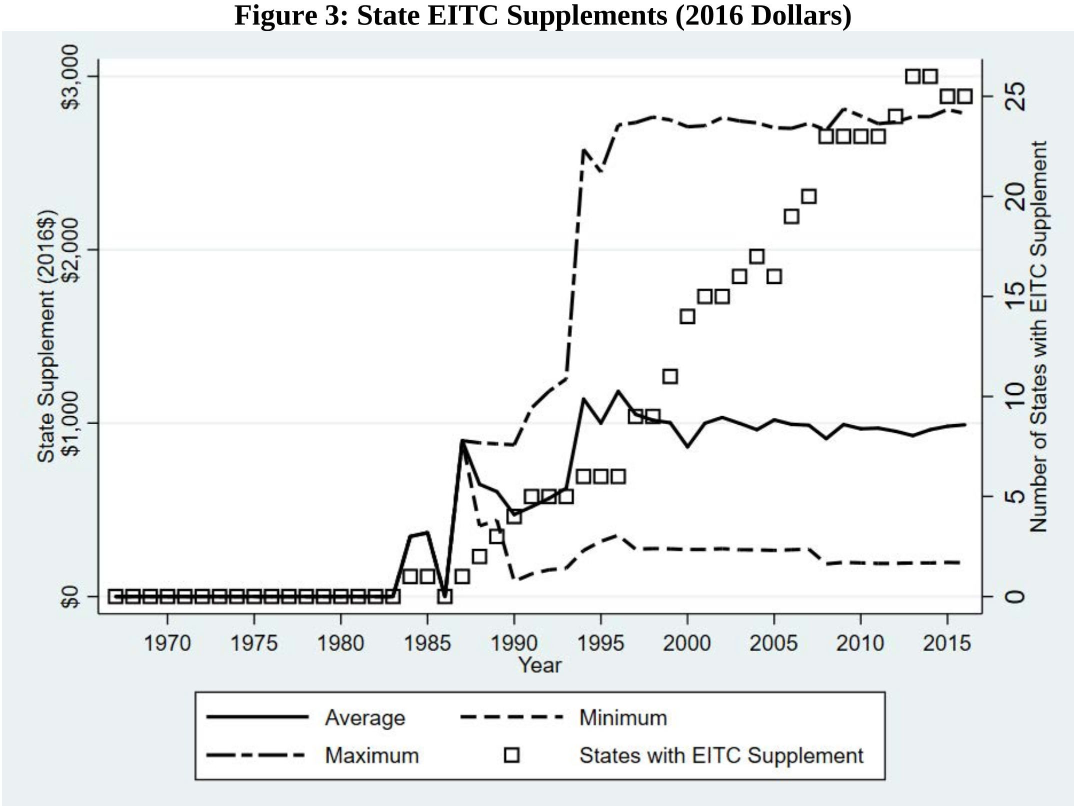
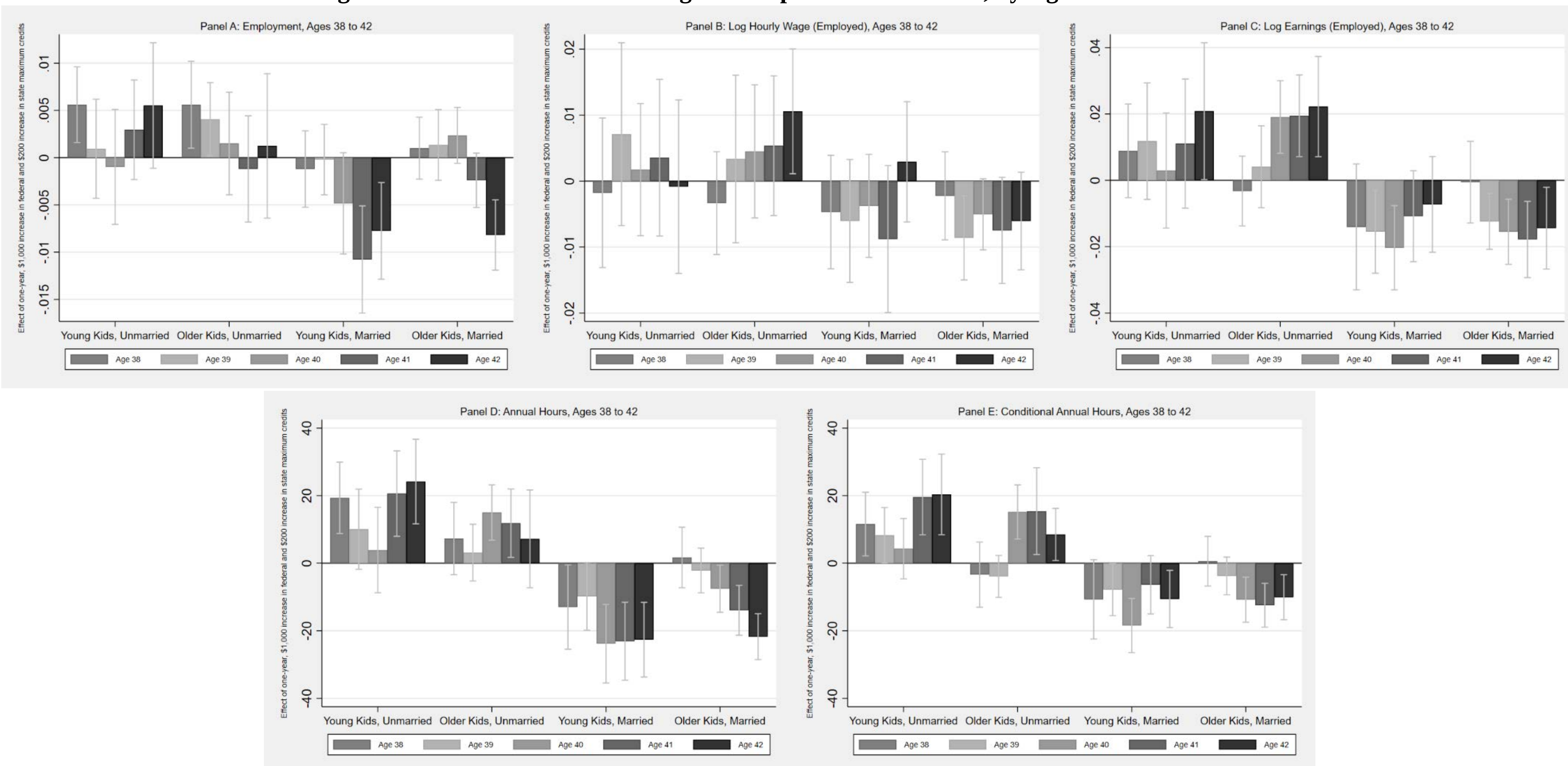


Figure 4: Estimated Effects of Long-Run Exposure to the EITC, by Ages 38-42



Estimates at age 40 correspond to Table 5, Panel B, using different ages at which to measure longer-run outcomes. For ages 38, 39, 41, and 42 we create a separate sample at each age using the same approach described in the data section for the age 40 sample. The 90-percent confidence intervals are shown.

Table 1: Sample Construction Description

	Number of observations
A. All PSID respondents	80,666
B. Number of female PSID respondents	40,681
C. Number of female PSID respondents potentially observed from ages 22-40 from 1985 to 2016	5,652
D. Number of low-educ. (LTHS or HS) women in Row C	2,548
E. Keep only women with a full 19-year state history back to age 22	1,795
Number of women in E with full 19-year marital history	1,613
Number of women in E with full 19-year child history	1,795
Number of women in E with full 19-year age of child history	1,725
Number of women in E with a consistent race categorization	1,772
Number of women in E with non-missing earnings data (including $0 for non-working) at age 40	1,795
Number of women in E with non-missing current employment status at age 40	1,724
Number of women in E with non-missing births data and five or fewer births	1,745
F. Number of women in E who fit all the above criteria simultaneously (final sample)	1,505

Row C reports the number of observations we have for women who were actually observed in the PSID at age 40, and could have been observed back to age 22, between 1967 (the 1968 survey) and 2016 (the last year covered in our data). Explanations for the differences between rows D and F are attrition, missing data, or entering the sample after age 22 (e.g., by marrying into a PSID household).

Table 2: Descriptive Statistics for Long-Run Analysis (Means)

Ages	22-39	40
Calendar year at age 40	N/A	1998
Federal EITC two-child maximum credit	2.61	4.03
State EITC two-child maximum credit	0.08	0.18
Combined EITC two-child maximum credit	2.68	4.20
Prop. years with any children	0.83	0.71
Prop. years with young children	0.39	0.06
Prop. years with older children (only)	0.45	0.65
Prop. years unmarried	0.32	0.30
Prop. years married	0.68	0.70
Prop. years with any children and unmarried	0.23	0.19
Prop. years with young children and unmarried	0.09	0.01
Prop. years with older (only) children and unmarried	0.14	0.18
Prop. years with any children and married	0.60	0.52
Prop. years with young children and married	0.29	0.04
Prop. years with older (only) children and married	0.31	0.48
Black	N/A	0.41
Experience (cumulative years employed, ages 22-39)	13.16	N/A
Employed at age 40	N/A	0.78
Annual hours at age 40	N/A	1,368
Log wage (employed) at age 40	N/A	2.56
Log earnings (employed) at age 40	N/A	9.86

Descriptive statistics are for the low-education sample (Row F, Table 1). The maximum credit is measured in $1,000s (indexed to 2016). We show the combined credit as well as the individual federal and state portions. (Sample sizes appear in the tables that follow.)

Table 3: 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 Maximum Credit

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
Coefficient estimates (δ^U, δ^M)					
Avg. (two-child maximum credit × **children** × **unmarried**, 22-39)	-0.0004	0.001	0.005	0.52	1.97
	(0.002)	(0.003)	(0.005)	(5.45)	(3.53)
Avg. (two-child maximum credit × **children** × **married**, 22-39)	0.001	-0.006	-0.014^{**}	-7.73^{*}	-9.08^{***}
	(0.002)	(0.004)	(0.007)	(3.91)	(3.20)
R^2	0.08	0.14	0.14	0.08	0.09
N	1,505	1,176	1,177	1,505	1,197

See notes to Table 2. These results are based on equation (4). The coefficients can be interpreted as the effect of a one-year, $1,000 increase in the EITC maximum credit. Other independent variables include: (1) averages of two-way interactions between the EITC variable, dummy variables for marital status, and a dummy variable for children, calculated over ages 22-39; and corresponding main effects; (2) two-way and three-way interactions between the EITC variable, a dummy for married, and a dummy variable for children, at age 40, and corresponding main effects; (3) dummy variable for black; (4) state and year fixed effects. $^{***}/^{**}/^{*}$ Significantly different from zero at 1/5/10-percent level. Standard errors are clustered at the state level.

Table 4: 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 Maximum Credit and Age of Children

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})					
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	-0.001	0.002	0.001	-1.94	0.80
	(0.003)	(0.007)	(0.011)	(8.98)	(5.88)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	0.0002	0.005	0.014^{**}	3.77	5.58
	(0.003)	(0.005)	(0.006)	(5.41)	(4.08)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.004	-0.003	-0.010	-15.58^{***}	-7.48
	(0.003)	(0.005)	(0.007)	(5.71)	(4.52)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.003^{*}	-0.005	-0.013^{*}	-3.41	-8.42^{**}
	(0.002)	(0.004)	(0.007)	(4.37)	(3.72)
R^2	0.09	0.16	0.16	0.09	0.10
N	1,505	1,176	1,177	1,505	1,197

See notes to Table 2. These results are based on equations (4), (5), and (5'). The coefficients can be interpreted as the effect of a one-year, $1,000 increase in the EITC maximum credit. Other independent variables include: (1) averages of two-way interactions between the EITC variable, dummy variables for marital status, and dummy variables for having either younger or older (only) children, calculated over ages 22-39; and corresponding main effects; (2) two-way and three-way interactions between each EITC variable, a dummy for married, and a dummy variables for having either younger or older (only) children, at age 40, and corresponding main effects; (3) dummy variable for black; (4) state and year fixed effects.

$^{***}/^{**}/^{*}$ Significantly different from zero at 1/5/10-percent level. Standard errors are clustered at the state level.

Table 5: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Separate Federal and State EITC Two-Child Maximum Credits and Age of Children

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit × **young children** × **unmarried**, 22-39)	-0.002	0.001	-0.0003	-4.28	-0.54
	(0.004)	(0.008)	(0.012)	(11.67)	(7.56)
Avg. (two-child **federal** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.0002	0.009	0.015**	0.82	2.40
	(0.003)	(0.006)	(0.007)	(5.63)	(4.61)
Avg. (two-child **federal** maximum credit × **young children** × **married**, 22-39)	-0.004	-0.003	-0.009	-12.27*	-3.01
	(0.003)	(0.007)	(0.010)	(6.88)	(5.23)
Avg. (two-child **federal** maximum credit × **older (only) children** × **married**, 22-39)	0.003	-0.006	-0.015	-1.66	-8.25
	(0.002)	(0.005)	(0.010)	(5.01)	(4.97)
Avg. (two-child **state** maximum credit × **young children** × **unmarried**, 22-39)	0.004	0.006	0.016	36.59	23.63
	(0.017)	(0.029)	(0.041)	(39.88)	(3.44)
Avg. (two-child **state** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.007	-0.012	0.034	71.71***	66.24***
	(0.012)	(0.017)	(0.026)	(25.07)	(18.25)
Avg. (two-child **state** maximum credit × **young children** × **married**, 22-39)	-0.008	-0.004	-0.067*	-70.01*	-80.16***
	(0.016)	(0.020)	(0.035)	(35.49)	(21.87)
Avg. (two-child **state** maximum credit × **older (only) children** × **married**, 22-39)	-0.002	0.0002	-0.018	-31.14*	-21.00
	(0.008)	(0.011)	(0.022)	(16.95)	(19.01)
B. Implied effect of one-year, \$1,000 increase in federal and \$200 increase in state maximum credits (%)					
Unmarried (young children)	-0.001	0.002	0.003	3.04	4.19
	(0.004)	(0.007)	(0.012)	(9.24)	(6.20)
Unmarried (older children)	0.002	0.006	0.022***	15.16***	15.65***
	(0.004)	(0.007)	(0.008)	(5.57)	(5.52)
Married (young children)	-0.006	-0.005	-0.022**	-26.28***	-19.05***
	(0.004)	(0.006)	(0.009)	(7.71)	(5.52)
Married (older children)	0.003	-0.006	-0.018**	-7.89	-12.45**
	(0.002)	(0.004)	(0.008)	(4.95)	(4.68)
R^2	0.09	0.16	0.17	0.09	0.11
N	1,505	1,176	1,177	1,505	1,197

See notes to Table 2. These results are based on equation (6) (and the full equation in Appendix A). The coefficients can be interpreted as the effect of a one-year, \$1,000 increase in the relevant EITC maximum credit. The calculations in Panel B, from top to bottom in each column, are equal to $\delta^{UYF} + (0.2)\times\delta^{UYS}$, $\delta^{UOF} + (0.2)\times\delta^{UOS}$, $\delta^{MYF} + (0.2)\times\delta^{MYS}$, and $\delta^{MOF} + (0.2)\times\delta^{MOS}$. Other independent variables include: (1) averages of two-way interactions between each EITC variable, dummy variables for marital status, and dummy variables for having either younger or older (only) children, calculated over ages 22-39; and corresponding main effects; (2) two-way and three-way interactions between each EITC variable, a dummy for married, and the dummy variables for having either younger or older (only) children, at age 40, and corresponding main effects; (3) dummy variable for black; (4) state and year fixed effects. ***/**/* Significantly different from zero at 1/5/10-percent level. Standard errors are clustered at the state level.

Table 6: Long-Run Effects of EITC on Less-Educated Women's Cumulative Experience at Age 40

	Total number of years with positive earnings, 22-39	Cumulative hours transformed to FTE, 22-39	Total number of years with positive earnings, 22-39	Cumulative hours transformed to FTE, 22-39
	Preferred specification (Table 5)		Including high-education women as an additional level of differencing	
	(1)	(2)	(3)	(4)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})				
Avg. (two-child **federal** maximum credit × **young children** × **unmarried**, 22-39)	0.094**	0.073	0.057	0.040
	(0.042)	(0.051)	(0.045)	(0.067)
Avg. (two-child **federal** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.070**	0.022	0.090**	0.059
	(0.032)	(0.046)	(0.035)	(0.054)
Avg. (two-child **federal** maximum credit × **young children** × **married**, 22-39)	-0.005	-0.019	-0.023	-0.037
	(0.038)	(0.045)	(0.046)	(0.057)
Avg. (two-child **federal** maximum credit × **older (only) children** × **married**, 22-39)	0.0002	-0.017	0.016	0.010
	(0.031)	(0.039)	(0.040)	(0.058)
Avg. (two-child **state** maximum credit × **young children** × **unmarried**, 22-39)	0.075	0.373	0.398	0.482
	(0.275)	(0.347)	(0.369)	(0.449)
Avg. (two-child **state** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.055	0.234	0.223	0.610*
	(0.226)	(0.607)	(0.245)	(0.330)
Avg. (two-child **state** maximum credit × **young children** × **married**, 22-39)	0.263**	0.262**	0.460**	0.595*
	(0.109)	(0.126)	(0.196)	(0.345)
Avg. (two-child **state** maximum credit × **older (only) children** × **married**, 22-39)	0.016	-0.098	0.082	0.082
	(0.069)	(0.094)	(0.125)	(0.311)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)				
Unmarried (young children)	0.109	0.148**	0.136*	0.136
	(0.067)	(0.069)	(0.080)	(0.088)
Unmarried (older children)	0.081*	0.069	0.134***	0.181***
	(0.041)	(0.055)	(0.047)	(0.064)
Married (young children)	0.048	0.034	0.069	0.082
	(0.040)	(0.043)	(0.047)	(0.072)
Married (older children)	0.003	-0.037	0.032	0.026
	(0.028)	(0.034)	(0.038)	(0.064)
C. Implied effect of $1,000 increase in federal and $200 increase in state maximum credits, 22-39 (%)				
Always unmarried with children at ages 22 and 24	1.680**	1.870*	2.433**	2.899**
	(0.823)	(1.001)	(0.938)	(1.094)
Always married with children at ages 22 and 24	0.416	-0.097	0.876	0.917
	(0.554)	(0.611)	(0.685)	(1.121)
R^2	0.17	0.21	0.19	0.26
N, low-ed	1,047	1,047	1,047	1,047
N, high-ed	N/A	N/A	975	975

See notes to Tables 2 and 5.

Table 7: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Separate Federal Maximum Credit and State EITC Supplement Percentage and Age of Children

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit × **young children** × **unmarried**, 22-39)	-0.002	0.001	0.0001	-3.42	-0.28
	(0.004)	(0.008)	(0.012)	(11.32)	(7.49)
Avg. (two-child **federal** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.0002	0.008	0.015**	1.94	3.47
	(0.003)	(0.006)	(0.007)	(5.56)	(4.44)
Avg. (two-child **federal** maximum credit × **young children** × **married**, 22-39)	-0.004	-0.003	-0.009	-14.13**	-4.62
	(0.003)	(0.007)	(0.010)	(6.63)	(5.17)
Avg. (two-child **federal** maximum credit × **older (only) children** × **married**, 22-39)	0.003*	-0.006	-0.015	-2.52	-8.85*
	(0.002)	(0.005)	(0.010)	(4.94)	(4.82)
Avg. (two-child **state** supplement percentage × **young children** × **unmarried**, 22-39)	0.003	-0.002	0.010	17.72	15.08
	(0.005)	(0.012)	(0.016)	(12.14)	(15.08)
Avg. (two-child **state** supplement percentage × **older (only) children** × **unmarried**, 22-39)	0.003	-0.006	0.009	24.22***	23.90***
	(0.003)	(0.006)	(0.007)	(6.74)	(6.09)
Avg. (two-child **state** supplement percentage × **young children** × **married**, 22-39)	0.0001	-0.003	-0.016	-13.86	-22.08**
	(0.005)	(0.007)	(0.014)	(13.85)	(10.56)
Avg. (two-child **state** supplement percentage × **older (only) children** × **married**, 22-39)	-0.001	0.0005	-0.007	-10.66	-6.65
	(0.003)	(0.005)	(0.010)	(7.62)	(6.66)
B. Implied effect of one-year, $1,000 increase in federal maximum credit and 20pp increase in state supplement (%)					
Unmarried (young children)	0.004	-0.002	0.020	32.01	29.88
	(0.010)	(0.022)	(0.032)	(20.12)	(26.79)
Unmarried (older children)	0.006	-0.005	0.034**	50.39***	51.28***
	(0.007)	(0.014)	(0.016)	(13.76)	(12.69)
Married (young children)	-0.004	-0.009	-0.042	-41.84	-48.78**
	(0.010)	(0.013)	(0.026)	(26.08)	(19.59)
Married (older children)	0.001	-0.005	-0.029*	-23.84	-22.15*
	(0.006)	(0.008)	(0.017)	(14.48)	(12.18)
R^2	0.09	0.16	0.17	0.09	0.11
N	1,505	1,176	1,177	1,505	1,197

See notes to Table 2. In contrast to Table 5, the state supplements are now calculated as percentages of the federal EITC rather than in dollars. The coefficients can be interpreted as the effect of a one-year, $1,000 increase in the federal EITC maximum credit or a one-year, 10-percentage point increase in the state EITC supplement. The calculations in Panel B are equal to $\delta^{UYF}+(2)\times\delta^{UYS}$, $\delta^{MYF}+(2)\times\delta^{MYS}$, $\delta^{UOF}+(2)\times\delta^{UOS}$, and $\delta^{MOF}+(2)\times\delta^{MOS}$, respectively. Other independent variables include: (1) averages of two-way interactions between each EITC variable, dummy variables for marital status, and dummy variables for having either younger or older (only) children, calculated over ages 22-39; and corresponding main effects; (2) two-way and three-way interactions between each EITC variable, a dummy for married, and the dummy variables for having either younger or older (only) children, at age 40, and corresponding main effects; (3) dummy variable for black; (4) state and year fixed effects. ***/**/* Significantly different from zero at 1/5/10-percent level. Standard errors are clustered at the state level.

Table 8: Long-Run Effects of EITC on Women's Fertility and Marital Status from Ages 22 to 40, Examining Endogenous Fertility and Marriage Decisions

	Fraction of years with any kids (1)	Fraction of years with young kids (2)	Fraction of years with older (only) kids (3)	Completed fertility (4)	Fraction of years married (5)
I. Treating Childbearing as Potentially Endogenous, Conditional on Marital Status					
A. Coefficient estimates					
Avg. (two-child **federal** maximum credit × **unmarried**), 22-39	0.003**	0.0001	0.002**	0.007	…
	(0.001)	(0.0001)	(0.001)	(0.007)	
Avg. (two-child **state** maximum credit × **unmarried**), 22-39	0.0003	0.001	-0.001	-0.011	…
	(0.002)	(0.002)	(0.002)	(0.010)	
Avg. (two-child **federal** maximum credit × **married**), 22-39	0.0005	-0.0001	0.001	0.003	…
	(0.001)	(0.001)	(0.001)	(0.006)	
Avg. (two-child **state** maximum credit × **married**), 22-39	-0.017***	-0.005**	-0.011***	-0.046	…
	(0.005)	(0.003)	(0.003)	(0.017)	
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried	0.003***	0.001	0.002**	0.005	…
	(0.001)	(0.001)	(0.001)	(0.007)	
Married	-0.003	-0.001	-0.002	-0.006	…
	(0.002)	(0.001)	(0.001)	(0.007)	
II. Treating Marital Status as Potentially Endogenous, Conditional on Childbearing					
A. Coefficient estimates					
Avg. (two-child **federal** maximum credit × **young children**), 22-39	…	…	…	…	-0.005**
					(0.002)
Avg. (two-child **state** maximum credit × **young children**), 22-39	…	…	…	…	0.013***
					(0.004)
Avg. (two-child **federal** maximum credit × **older children**), 22-39	…	…	…	…	-0.0003
					(0.002)
Avg. (two-child **state** maximum credit × **older children**), 22-39	…	…	…	…	-0.006*
					(0.003)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
With young children	…	…	…	…	-0.002
					(0.002)
With older children	…	…	…	…	-0.001
					(0.002)
R^2	0.18	0.13	0.17	0.14	0.36
N, low-education	1,505	1,505	1,505	1,505	1,505

See notes to Tables 2 and 5, and equation (7) (along with modifications described in the text.

Table 9: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Alternative Specifications for Eliminating Endogenous Migration or Policy

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
I. Fixing State at Age 22					
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit ×	-0.0005	-0.002	-0.003	-1.18	0.22
young children × **unmarried**, 22-39)	(0.005)	(0.009)	(0.014)	(13.89)	(8.74)
Avg. (two-child **federal** maximum credit ×	-0.0004	0.011*	0.019**	-0.44	2.86
older (only) children × **unmarried**, 22-39)	(0.003)	(0.006)	(0.008)	(6.19)	(4.90)
Avg. (two-child **federal** maximum credit ×	-0.002	-0.002	-0.007	-8.87	-0.91
young children × **married**, 22-39)	(0.004)	(0.008)	(0.010)	(8.81)	(6.49)
Avg. (two-child **federal** maximum credit ×	0.003	-0.006	-0.016	-1.87	-9.00*
older (only) children × **married**, 22-39)	(0.002)	(0.006)	(0.012)	(5.69)	(5.32)
Avg. (two-child **state** maximum credit ×	-0.002	-0.007	-0.017	18.52	1.67
young children × **unmarried**, 22-39)	(0.032)	(0.061)	(0.010)	(56.25)	(52.87)
Avg. (two-child **state** maximum credit ×	0.014	-0.052	0.0001	104.36**	85.03***
older (only) children × **unmarried**, 22-39)	(0.018)	(0.040)	(0.057)	(41.40)	(30.00)
Avg. (two-child **state** maximum credit ×	-0.036	-0.006	-0.078	-131.41***	-121.28***
young children × **married**, 22-39)	(0.022)	(0.037)	(0.063)	(44.64)	(39.65)
Avg. (two-child **state** maximum credit ×	-0.014	0.010	-0.024	-60.54**	-31.32
older (only) children × **married**, 22-39)	(0.011)	(0.018)	(0.033)	(27.77)	(22.97)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	-0.001	-0.004	-0.006	2.53	0.55
	(0.007)	(0.012)	(0.019)	(13.74)	(10.47)
Unmarried (older children)	0.002	0.001	0.019*	20.43**	19.86***
	(0.005)	(0.010)	(0.011)	(8.74)	(6.25)
Married (young children)	-0.010*	-0.003	-0.023	-35.15***	-25.17***
	(0.005)	(0.010)	(0.016)	(11.12)	(9.16)
Married (older children)	0.0005	-0.004	-0.020**	-13.98	-15.27***
	(0.003)	(0.005)	(0.009)	(6.46)	(5.05)
R^2	0.09	0.17	0.17	0.09	0.11
II. Using Only Federal EITC Variation					
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF})					
Avg. (two-child **federal** maximum credit ×	-0.002	0.001	0.0002	-2.36	0.63
young children × **unmarried**, 22-39)	(0.004)	(0.007)	(0.012)	(10.66)	(7.00)
Avg. (two-child **federal** maximum credit ×	0.0002	0.006	0.015**	2.89	4.57
older (only) children × **unmarried**, 22-39)	(0.003)	(0.006)	(0.006)	(5.60)	(4.42)
Avg. (two-child **federal** maximum credit ×	-0.004	-0.003	-0.009	-14.21**	-5.69
young children × **married**, 22-39)	(0.003)	(0.006)	(0.008)	(6.05)	(4.60)
Avg. (two-child **federal** maximum credit ×	0.004	-0.006	-0.014	-2.65	-8.55*
older (only) children × **married**, 22-39)	(0.002)	(0.005)	(0.009)	(4.79)	(4.33)
R^2	0.09	0.16	0.16	0.08	0.10
N	1,505	1,176	1,177	1,505	1,197

See notes to Tables 2 and 5. The only differences compared to Table 5 are in the definitions of the EITC variables, as explained in the headings of Parts I and II.

Table 10: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Including High-Education Women as an Additional Level of Differencing

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit ×	0.004	0.001	0.003	9.35	2.80
young children × **unmarried**, 22-39) × low-ed	(0.004)	(0.008)	(0.011)	(13.72)	(9.24)
Avg. (two-child **federal** maximum credit ×	0.001	0.005	0.012	7.84	4.72
older (only) children × **unmarried**, 22-39) × low-ed	(0.003)	(0.006)	(0.009)	(6.99)	(6.13)
Avg. (two-child **federal** maximum credit ×	0.0002	-0.007	-0.018	-9.13	-3.76
young children × **married**, 22-39) × low-ed	(0.003)	(0.008)	(0.013)	(7.96)	(7.61)
Avg. (two-child **federal** maximum credit ×	0.006**	-0.007	-0.021*	1.97	-6.58
older (only) children × **married**, 22-39) × low-ed	(0.003)	(0.006)	(0.011)	(5.61)	(5.60)
Avg. (two-child **state** maximum credit ×	-0.017	0.057	0.094*	32.50	73.69*
young children × **unmarried**, 22-39) × low-ed	(0.021)	(0.044)	(0.055)	(56.57)	(40.63)
Avg. (two-child **state** maximum credit ×	0.022**	-0.033	0.016	90.30***	42.85
older (only) children × **unmarried**, 22-39) × low-ed	(0.010)	(0.029)	(0.040)	(33.68)	(48.04)
Avg. (two-child **state** maximum credit ×	-0.007	0.017	-0.038	-65.90*	-77.87***
young children × **married**, 22-39) × low-ed	(0.014)	(0.047)	(0.046)	(35.85)	(28.54)
Avg. (two-child **state** maximum credit ×	-0.002	0.028	0.003	-46.31	-40.52
older (only) children × **married**, 22-39) × low-ed	(0.013)	(0.051)	(0.074)	(29.73)	(24.57)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	0.0004	0.012	0.022*	15.85	17.54*
	(0.005)	(0.010)	(0.013)	(13.20)	(8.80)
Unmarried (older children)	0.006	-0.002	0.015	25.90***	13.29
	(0.004)	(0.009)	(0.012)	(8.12)	(12.16)
Married (young children)	-0.001	-0.003	-0.026**	-22.31**	-19.33**
	(0.004)	(0.008)	(0.011)	(8.88)	(7.97)
Married (older children)	0.006	-0.002	-0.020	-7.29	-14.68***
	(0.003)	(0.009)	(0.014)	(5.93)	(5.31)
R^2	0.09	0.23	0.17	0.12	0.12
N	3,358	2,757	2,760	3,358	2,804

See notes to Tables 2 and 5. The difference compared to Table 5 is that high-education women are added to the sample, and all variables in the model are entered on their own, and interacted with a low-education dummy variables. The estimates in the table are based on the latter interactions. (The main effect of low education is also included.)

Table 11: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Including State-Specific Linear Trends

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit ×	-0.003	0.001	0.004	-4.46	1.81
young children × **unmarried**, 22-39)	(0.004)	(0.009)	(0.013)	(12.01)	(8.15)
Avg. (two-child **federal** maximum credit ×	-0.0002	0.008	0.014*	-0.26	2.35
older (only) children × **unmarried**, 22-39)	(0.003)	(0.006)	(0.007)	(5.71)	(4.72)
Avg. (two-child **federal** maximum credit ×	-0.005	-0.002	-0.006	-14.07	-1.44
young children × **married**, 22-39)	(0.004)	(0.008)	(0.010)	(7.06)	(5.16)
Avg. (two-child **federal** maximum credit ×	0.003	-0.004	-0.013	-1.83	-6.88
older (only) children × **married**, 22-39)	(0.002)	(0.006)	(0.010)	(5.32)	(4.93)
Avg. (two-child **state** maximum credit ×	-0.003	-0.015	-0.019	22.11	24.69
young children × **unmarried**, 22-39)	(0.018)	(0.037)	(0.054)	(39.43)	(28.77)
Avg. (two-child **state** maximum credit ×	0.013	-0.033	-0.003	75.94***	58.18***
older (only) children × **unmarried**, 22-39)	(0.009)	(0.026)	(0.039)	(24.87)	(17.33)
Avg. (two-child **state** maximum credit ×	-0.007	0.0001	-0.0488	-64.15*	-75.50***
young children × **married**, 22-39)	(0.016)	(0.023)	(0.040)	(37.07)	(21.74)
Avg. (two-child **state** maximum credit ×	-0.004	-0.002	-0.030	-37.58**	-27.82
older (only) children × **married**, 22-39)	(0.008)	(0.013)	(0.024)	(17.74)	(21.27)
B. Implied effect of one-year, \$1,000 increase in federal and \$200 increase in state maximum credits (%)					
Unmarried (young children)	-0.004	-0.002	-0.0002	-0.04	6.75
	(0.004)	(0.009)	(0.013)	(9.60)	(6.68)
Unmarried (older children)	0.002	0.002	0.013	14.92**	13.99***
	(0.004)	(0.009)	(0.010)	(6.18)	(4.65)
Married (young children)	-0.006	-0.002	-0.015	-26.90***	-16.54***
	(0.004)	(0.006)	(0.009)	(8.14)	(5.63)
Married (older children)	0.003	-0.004	-0.019**	-9.35*	-12.44**
	(0.002)	(0.004)	(0.008)	(5.36)	(5.07)
R^2	0.11	0.19	0.21	0.12	0.15
N	1,505	1,176	1,177	1,505	1,197

See notes to Tables 2 and 5. In contrast to Table 5, the models include state-specific linear trends.

Table 12: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Alternative Specifications with Controls for Welfare Reform and Minimum Wages

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
I. Including Parametric Welfare Reform Control					
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit ×	-0.006	0.0004	-0.006	-12.67	-4.68
young children × **unmarried**, 22-39)	(0.004)	(0.011)	(0.016)	(12.41)	(8.41)
Avg. (two-child **federal** maximum credit ×	0.001	0.011	0.020**	5.40	6.19
older (only) children × **unmarried**, 22-39)	(0.003)	(0.007)	(0.009)	(6.51)	(5.27)
Avg. (two-child **federal** maximum credit ×	-0.004	-0.005	-0.010	-12.12*	-2.57
young children × **married**, 22-39)	(0.003)	(0.008)	(0.011)	(6.86)	(4.91)
Avg. (two-child **federal** maximum credit ×	0.004	-0.008	-0.016*	-0.55	-8.06*
older (only) children × **married**, 22-39)	(0.002)	(0.005)	(0.009)	(4.53)	(4.76)
Avg. (two-child **state** maximum credit ×	0.008	0.006	0.019	42.28	25.14
young children × **unmarried**, 22-39)	(0.018)	(0.029)	(0.041)	(41.11)	(33.30)
Avg. (two-child **state** maximum credit ×	0.008	-0.017	0.024	70.07***	62.05***
older (only) children × **unmarried**, 22-39)	(0.011)	(0.017)	(0.027)	(25.72)	(19.04)
Avg. (two-child **state** maximum credit ×	-0.008	-0.001	-0.064*	-71.04**	-81.50***
young children × **married**, 22-39)	(0.016)	(0.020)	(0.034)	(35.21)	(21.43)
Avg. (two-child **state** maximum credit ×	-0.002	0.003	-0.016	-31.46*	-20.86
older (only) children × **married**, 22-39)	(0.008)	(0.011)	(0.021)	(16.29)	(18.87)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	-0.004	0.002	-0.002	-4.22	0.35
	(0.004)	(0.009)	(0.014)	(9.75)	(6.52)
Unmarried (older children)	0.003	0.007	0.025***	19.41***	18.60***
	(0.004)	(0.008)	(0.009)	(6.13)	(5.76)
Married (young children)	-0.005	-0.005	-0.023**	-26.33***	-18.87***
	(0.004)	(0.006)	(0.010)	(7.80)	(5.77)
Married (older children)	0.003	-0.007*	-0.019***	-6.85	-12.23**
	(0.002)	(0.004)	(0.007)	(4.99)	(4.71)
R^2	0.09	0.17	0.18	0.10	0.12
II. Including Real Minimum Wage Control					
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit ×	-0.003	0.008	0.003	-11.34	-5.69
young children × **unmarried**, 22-39)	(0.004)	(0.008)	(0.012)	(11.21)	(8.53)
Avg. (two-child **federal** maximum credit ×	-0.00005	0.013*	0.015*	-2.67	-2.26
older (only) children × **unmarried**, 22-39)	(0.003)	(0.007)	(0.008)	(6.18)	(4.64)
Avg. (two-child **federal** maximum credit ×	-0.004	-0.004	-0.010	-12.63*	-4.41
young children × **married**, 22-39)	(0.003)	(0.007)	(0.011)	(6.34)	(5.62)
Avg. (two-child **federal** maximum credit ×	0.004	-0.008	-0.018	-2.07	-10.01
older (only) children × **married**, 22-39)	(0.003)	(0.006)	(0.013)	(6.72)	(6.94)
Avg. (two-child **state** maximum credit ×	-0.0004	0.006	0.012	26.39	22.17
young children × **unmarried**, 22-39)	(0.015)	(0.033)	(0.043)	(37.66)	(29.55)
Avg. (two-child **state** maximum credit ×	0.006	-0.015	0.029	69.35***	67.01***
older (only) children × **unmarried**, 22-39)	(0.012)	(0.020)	(0.028)	(25.19)	(19.35)
Avg. (two-child **state** maximum credit ×	-0.008	-0.002	-0.068**	-70.93*	-81.07***
young children × **married**, 22-39)	(0.016)	(0.019)	(0.032)	(35.63)	(20.68)
Avg. (two-child **state** maximum credit ×	-0.002	-0.001	-0.019	-30.68*	-19.89
older (only) children × **married**, 22-39)	(0.008)	(0.011)	(0.022)	(16.63)	(18.54)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	-0.003	0.009	0.005	-6.06	-1.25
	(0.004)	(0.008)	(0.014)	(9.04)	(6.60)
Unmarried (older children)	0.001	0.010	0.021**	11.20	11.14*
	(0.004)	(0.009)	(0.010)	(7.42)	(6.18)
Married (young children)	-0.005	-0.004	-0.024**	-26.81***	-20.63***
	(0.004)	(0.006)	(0.010)	(7.89)	(6.01)
Married (older children)	0.004	-0.008	-0.022**	-8.21	-13.99**
	(0.003)	(0.005)	(0.011)	(6.87)	(6.45)
R^2	0.10	0.17	0.17	0.10	0.12
N	1,505	1,176	1,177	1,505	1,197

See notes to Tables 2 and 5. The modifications to the equations estimated are explained in the text.

Table 13: Long-Run Effects of EITC on Less-Educated Women's Earnings and Hours at Age 40, Implied Effect of $1,00 Increase in Federal and $200 in State Maximum Credits from Ages 22-39

	Always unmarried with children at ages 22 and 24 (1)	Always married with children at ages 22 and 24 (2)
A. Log Earnings Estimates From:		
Table 3: Simple specification	0.089	-0.260^{**}
	(0.099)	(0.122)
Table 4: Adding age of kids	0.145	-0.175^{*}
	(0.115)	(0.098)
Table 5: With age of kids and separate state and federal EITC	0.244^{*}	-0.361^{**}
	(0.137)	(0.139)
Table 7: Using state supplement percentage	0.498	-0.629^{**}
	(0.349)	(0.239)
Table 9, Part I: Fixing state at age 22	0.141	-0.388^{*}
	(0.241)	(0.196)
Table 9, Part II: Using only federal EITC variation	0.147	-0.212
	(0.120)	(0.148)
Table 10: Including high-education women as an additional difference	0.328^{*}	-0.410^{*}
	(0.164)	(0.210)
Table 11: Including state-specific linear trends	0.130	-0.307^{**}
	(0.169)	(0.143)
Table 12, Part I: Including parametric welfare reform control	0.234	-0.372^{***}
	(0.166)	(0.132)
Table 12, Part II: Including real minimum wage control	0.251	-0.406^{**}
	(0.155)	(0.169)
B. Annual Hours (Employed) Estimates From:		
Table 3: Simple specification	35.52	-163.50^{***}
	(63.47)	(57.59)
Table 4: Adding age of kids	62.24	-114.12^{**}
	(69.46)	(46.42)
Table 5: With age of kids and separate state and federal EITC	190.00^{**}	-276.88^{***}
	(81.90)	(74.43)
Table 7: Using state supplement percentage	751.88^{***}	-611.75^{**}
	(203.56)	(229.29)
Table 9, Part I: Fixing state at age 22	203.04	-354.01^{***}
	(123.37)	(99.20)
Table 9, Part II: Using only federal EITC variation	50.72	-131.08^{*}
	(76.17)	(67.79)
Table 10: Including high-education women as an additional difference	273.15^{*}	-301.44^{***}
	(160.79)	(102.65)
Table 11: Including state-specific linear trends	193.88^{**}	-256.74^{***}
	(75.23)	(78.73)
Table 12, Part I: Including parametric welfare reform control	188.82^{**}	-273.25^{***}
	(81.82)	(75.90)
Table 12, Part II: Including real minimum wage control	101.34	-304.92^{***}
	(91.58)	(97.89)

See notes to Tales 2 and 5. All coefficients come from the Table specified in each row. These effects are calculated by the following formulas:
Column (1): $(8)\times[\delta^{UYF} + (0.2)\times\delta^{UYS}] + (10)\times[\delta^{UOF} + (0.2)\times\delta^{UOS})]$
Column (2): $(8)\times[\delta^{MYF} + (0.2)\times\delta^{MYS}] + (10)\times[\delta^{MOF} + (0.2)\times\delta^{MOS})]$
For Table 3, because there is neither an age of kids distinction nor separate federal and state credits, the effects are $(18)\times\delta^{U}$ and $(18)\times\delta^{M}$, respectively. For Table 4, the effects are $(8)\times\delta^{UY} + (10)\times\delta^{UO}$ and $(8)\times\delta^{MY} + (10)\times\delta^{MO}$, respectively. For Table 7, the state δ parameters are multiplied by 2, rather than 0.2. For Table 9, Part II, the state portion of the calculation is omitted as that regression does not include state EITC variables. For Table 10, we use the low-ed coefficients for the calculations. The δ parameters are the coefficients of the variables described in equations (4), (5), (5'), and (6).

Appendix A : Full Estimating Equation Corresponding to Equation (6) and Table 5

$$Y_{ijt} = \alpha + \beta^{UF}\{\textstyle\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot U_{ija})/18\} + \beta^{US}\{\textstyle\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot U_{ija})/18\}$$

$$+ \gamma^{UY}\{\textstyle\sum_{a=t-18}^{t-1}(YK_{ija}\cdot U_{ija})/18\} + \gamma^{UO}\{\textstyle\sum_{a=t-18}^{t-1}(OK_{ija}\cdot U_{ija})/18\}$$

$$+ \beta^{MF}\{\textstyle\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot M_{ija})/18\} + \beta^{MS}\{\textstyle\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot M_{ija})/18\}$$

$$+ \gamma^{MY}\{\textstyle\sum_{a=t-18}^{t-1}(YK_{ija}\cdot M_{ija})/18\} + \gamma^{MO}\{\textstyle\sum_{a=t-18}^{t-1}(OK_{ija}\cdot M_{ija})/18\}$$

$$+ \boldsymbol{\delta^{UYF}\{\textstyle\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot YK_{ija}\cdot U_{ija})/18\}} + \boldsymbol{\delta^{UOF}\{\textstyle\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot OK_{ija}\cdot U_{ija})/18\}}$$

$$\boldsymbol{+ \delta^{MYF}\{\textstyle\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot YK_{ija}\cdot M_{ija})/18\}} + \boldsymbol{\delta^{MOF}\{\textstyle\sum_{a=t-18}^{t-1}(CRFed_{ja}\cdot OK_{ija}\cdot M_{ija})/18\}}$$

$$+ \boldsymbol{\delta^{UYS}\{\textstyle\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot YK_{ija}\cdot U_{ija})/18\}} + \boldsymbol{\delta^{UOS}\{\textstyle\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot OK_{ija}\cdot U_{ija})/18\}}$$

$$+ \boldsymbol{\delta^{MYS}\{\textstyle\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot YK_{ija}\cdot M_{ija})/18\}} + \boldsymbol{\delta^{MOS}\{\textstyle\sum_{a=t-18}^{t-1}(CRState_{ja}\cdot OK_{ija}\cdot M_{ija})/18\}}$$

$$+ \eta\{\textstyle\sum_{a=t-18}^{t-1}M_{ija}/18\} + \beta^{UF,40}CRFed_{ijt}\cdot U_{ijt} + \beta^{US,40}CRState_{ijt}\cdot U_{ijt} + \gamma^{UY,40}YK_{ijt}\cdot U_{ijt} + \gamma^{UO,40}OK_{ijt}\cdot U_{ijt}$$

$$+ \delta^{UYF,40}CRFed_{ijt}\cdot YK_{ijt}\cdot U_{ijt} + \delta^{UOF,40}CRFed_{ijt}\cdot OK_{ijt}\cdot U_{ijt} + \delta^{UYS,40}CRState_{ijt}\cdot YK_{ijt}\cdot U_{ijt}$$

$$+ \delta^{UOS,40}CRState_{ijt}\cdot OK_{ijt}\cdot U_{ijt} + \beta^{MF,40}CRFed_{ijt}\cdot M_{ijt} + \beta^{MS,40}CRState_{ijt}\cdot M_{ijt} + \gamma^{MY,\,40}YK_{ijt}\cdot M_{ijt}$$

$$+ \gamma^{MO,\,40}OK_{ijt}\cdot M_{ijt} + \delta^{MYF,40}CRFed_{ijt}\cdot YK_{ijt}\cdot M_{ijt} + {}^{MOF,40}CRState_{ijt}\cdot OK_{ijt}\cdot M_{ijt} + \delta^{MYS,40}CRState_{ijt}\cdot YK_{ijt}\cdot M_{ijt}$$

$$+ \delta^{MOS,40}CRState_{ijt}\cdot OK_{ijt}\cdot M_{ijt} + \eta^{40}M_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .$$

Appendix B: Replication Analysis, and Additional Robustness and Sensitivity Analyses

Replication of Past Results on EITC and Employment

We have explored the replication of the results from key prior papers on the EITC using the PSID data. Eissa and Liebman (1996) study federal EITC changes in 1986, which, as Figure 2 shows, increased EITC phase-in rates, although not sharply.[61] They study only unmarried women, and 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. The columns labeled "E & L" in Appendix Table B1 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). However, 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, also considering only unmarried women. As shown in Appendix Table B2, 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 we can 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

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

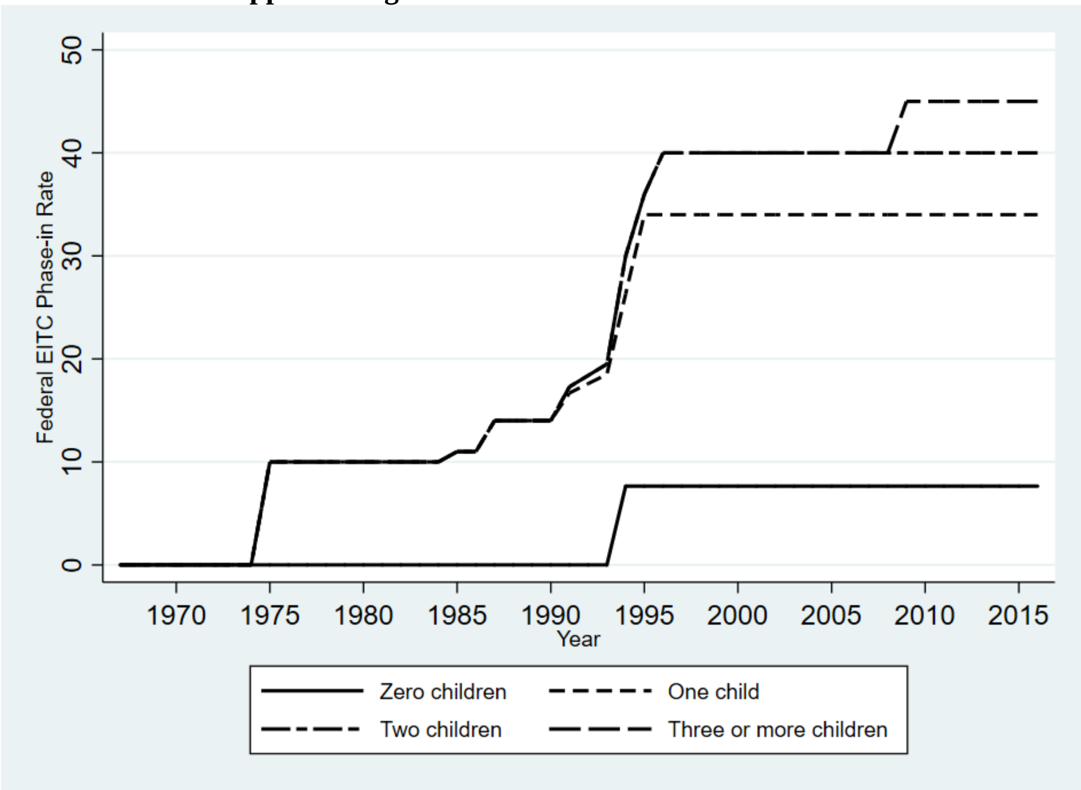
to the simpler question of shorter-run effects on employment.

Robustness and Sensitivity Analysis

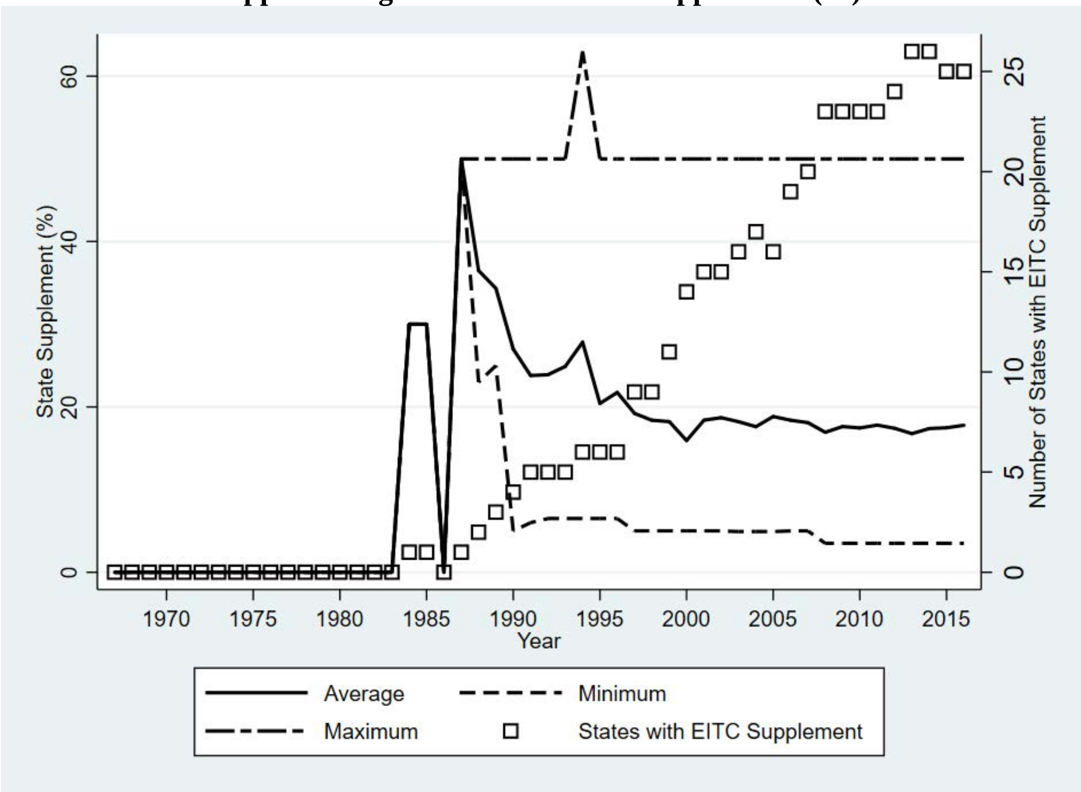
Here, we consider weighting. We are quite reticent to put much store in the sample weights, given the sample selection rules imposed to study longer-term effects of the EITC (see Table 1). However, while there is little reason to believe the sample weights are very accurate, they ought to capture broad-brush differences between those oversampled based on the low-income criterion. Appendix Table B3 reports results for the preferred specification and sample when we weight by the PSID Core sample weights for the age 40 observations. With the weights, fewer of the estimated coefficients (in the top four rows of the table) are significant, but the qualitative results are similar, and the implied impacts for married women are frequently significant. While the exact estimates clearly are sensitive to weighting, we view Appendix Table B3 as providing additional evidence of the robustness of our estimated effects of longer-run exposure to the EITC.

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 Appendix Table B4. The estimates for non-black women are, not surprisingly, very similar to the full sample and the weighted results (since the weighting downweights blacks). Interestingly, the one difference is that for married black women the signs of the estimated effects are not negative, but instead are positive like for unmarried women. One potential explanation is lower black female earnings and lower black male earnings and employment, making the positive extensive-margin effects of a more generous EITC more influential for married black women.

Appendix Figure B1: Federal EITC Phase-in Rate



Appendix Figure B2: State EITC Supplements (%)



Appendix Table B1: 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. The sample, as in Eissa and Liebman (1996), is restricted to unmarried women between the ages of 16 and 44.

Appendix Table B2: Replication of Meyer & Rosenbaum (2001) Table III, Extended

	M & R		Replication	
Explanatory variable	Marginal effect	Standard error	Marginal effect	Standard error
Any children × 1984	-0.1087	0.0160	-0.0047	0.0413
Any children × 1985	-0.0120	0.0156	-0.0529	0.0552
Any children × 1986	-0.1144	0.0153	-0.0859	0.0764
Any children × 1987	-0.1056	0.0144	-0.0493	0.0617
Any children × 1988	-0.0918	0.0140	-0.1003	0.0493
Any children × 1989	-0.0745	0.0131	-0.0881	0.0726
Any children × 1990	-0.0832	0.0136	-0.0430	0.0470
Any children × 1991	-0.0916	0.0151	-0.0096	0.0364
Any children × 1992	-0.0706	0.0159	-0.0030	0.0405
Any children × 1993	-0.0830	0.0153	0.0095	0.0293
Any children × 1994	-0.0388	0.0145	0.0002	0.0336
Any children × 1995	-0.0154	0.0143	0.0207	0.0249
Any children × 1996	0.0042	0.0140	-0.0128	0.0421
Any children × 1998			0.0120	0.0322
Any children × 2000			0.0289	0.0206
Any children × 2002			0.0457	0.0148
Any children × 2004			0.0427	0.0140
Any children × 2006			0.0465	0.0128
Any children × 2008			0.0498	0.0137
Any children × 2010			0.0431	0.0220
Any children × 2012			0.0388	0.0203
Any children × 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 × divorced	0.0720	0.0063	0.0462	0.0124
Any children × 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 × 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 Appendix Table B1. 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.)

Appendix Table B3: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Weighted

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit × **young children** × **unmarried**, 22-39)	0.006	0.006	0.004	10.44	-1.72
	(0.005)	(0.010)	(0.011)	(12.24)	(8.85)
Avg. (two-child **federal** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.0002	-0.001	0.007	4.67	6.42
	(0.004)	(0.006)	(0.007)	(7.99)	(4.20)
Avg. (two-child **federal** maximum credit × **young children** × **married**, 22-39)	-0.005	-0.005	-0.012	-17.12^{**}	-6.84
	(0.004)	(0.009)	(0.011)	(6.99)	(6.25)
Avg. (two-child **federal** maximum credit × **older (only) children** × **married**, 22-39)	0.001	-0.008	-0.018	-7.71	-8.80^{*}
	(0.003)	(0.007)	(0.011)	(4.82)	(4.77)
Avg. (two-child **state** maximum credit × **young children** × **unmarried**, 22-39)	0.017	0.012	-0.006	27.40	7.29
	(0.027)	(0.047)	(0.074)	(44.94)	(35.85)
Avg. (two-child **state** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.011	0.005	0.079^{**}	63.64	73.87^{*}
	(0.020)	(0.030)	(0.038)	(45.97)	(37.31)
Avg. (two-child **state** maximum credit × **young children** × **married**, 22-39)	-0.045^{**}	-0.002	-0.062^{*}	-146.68^{***}	-91.78^{***}
	(0.017)	(0.027)	(0.037)	(39.83)	(26.68)
Avg. (two-child **state** maximum credit × **older (only) children** × **married**, 22-39)	-0.021	-0.001	-0.004	-59.14	-15.77
	(0.016)	(0.023)	(0.034)	(39.36)	(24.40)
B. Implied effect of one-year, \$1,000 increase in federal and \$200 increase in state maximum credits (%)					
Unmarried (young children)	0.009	0.008	0.003	15.92	-0.26
	(0.006)	(0.011)	(0.018)	(10.26)	(9.22)
Unmarried (older children)	0.002	0.0003	0.023^{**}	17.40^{*}	21.19^{**}
	(0.004)	(0.007)	(0.009)	(8.74)	(8.85)
Married (young children)	-0.014^{***}	-0.005	-0.024^{**}	-46.46^{***}	-25.20^{***}
	(0.004)	(0.008)	(0.011)	(9.09)	(6.67)
Married (older children)	-0.004	-0.008	-0.019^{*}	-19.54^{**}	-11.96^{*}
	(0.004)	(0.007)	(0.011)	(8.96)	(6.41)
R^2	0.15	0.23	0.21	0.16	0.16
N	1,499	1,170	1,171	1,499	1,191

See notes to Tables 2 and 5. The difference is the estimates are weighted.

Appendix Table B4: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Separate Regressions by Race

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
I. Black Only					
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit ×	-0.007	-0.002	0.008	-7.38	4.72
young children × **unmarried**, 22-39)	(0.006)	(0.013)	(0.021)	(15.51)	(11.96)
Avg. (two-child **federal** maximum credit ×	-0.002	0.012	0.023*	-4.13	1.07
older (only) children × **unmarried**, 22-39)	(0.004)	(0.009)	(0.013)	(10.49)	(7.96)
Avg. (two-child **federal** maximum credit ×	0.002	0.007	0.028	14.00	24.97
young children × **married**, 22-39)	(0.010)	(0.012)	(0.018)	(25.36)	(15.51)
Avg. (two-child **federal** maximum credit ×	0.007	0.008	0.016	17.52	9.32
older (only) children × **married**, 22-39)	(0.007)	(0.009)	(0.015)	(17.64)	(17.22)
Avg. (two-child **state** maximum credit ×	-0.015	-0.039	-0.045	2.41	32.03
young children × **unmarried**, 22-39)	(0.030)	(0.071)	(0.066)	(116.28)	(94.43)
Avg. (two-child **state** maximum credit ×	-0.024	-0.050	0.003	55.17	122.86*
older (only) children × **unmarried**, 22-39)	(0.036)	(0.072)	(0.074)	(110.91)	(67.07)
Avg. (two-child **state** maximum credit ×	0.029*	-0.212*	-0.935***	9.23	-386.73***
young children × **married**, 22-39)	(0.016)	(0.120)	(0.209)	(57.23)	(139.83)
Avg. (two-child **state** maximum credit ×	0.012	-0.214*	-0.892***	1.81	-327.89**
older (only) children × **married**, 22-39)	(0.016)	(0.121)	(0.204)	(35.41)	(128.50)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	-0.010	-0.010	-0.001	-6.90	11.13
	(0.007)	(0.014)	(0.017)	(19.02)	(14.38)
Unmarried (older children)	-0.007	0.002	0.024	6.91	25.65*
	(0.006)	(0.014)	(0.014)	(16.72)	(13.60)
Married (young children)	0.007	-0.035*	-0.159***	15.85	-52.37
	(0.010)	(0.020)	(0.044)	(24.82)	(32.96)
Married (older children)	0.009	-0.035	-0.162***	17.88	-56.25
	(0.006)	(0.023)	(0.044)	(16.98)	(34.43)
R^2	0.12	0.25	0.29	0.13	0.19
N	622	458	458	622	465
II. Non-Black Only					
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit ×	0.006	0.004	-0.001	4.72	-4.18
young children × **unmarried**, 22-39)	(0.006)	(0.013)	(0.012)	(10.23)	(9.05)
Avg. (two-child **federal** maximum credit ×	-0.003	0.002	0.006	-4.95	0.05
older (only) children × **unmarried**, 22-39)	(0.006)	(0.007)	(0.007)	(10.41)	(5.77)
Avg. (two-child **federal** maximum credit ×	-0.005	-0.003	-0.011	-14.55*	-6.25
young children × **married**, 22-39)	(0.004)	(0.008)	(0.011)	(7.97)	(6.95)
Avg. (two-child **federal** maximum credit ×	0.002	-0.009	-0.018*	-3.86	-7.36*
older (only) children × **married**, 22-39)	(0.003)	(0.006)	(0.010)	(4.50)	(3.95)
Avg. (two-child **state** maximum credit ×	-0.030	0.105	0.388**	108.42	100.66
young children × **unmarried**, 22-39)	(0.064)	(0.116)	(0.180)	(178.65)	(142.76)
Avg. (two-child **state** maximum credit ×	0.035	0.023	0.137**	143.78*	102.08*
older (only) children × **unmarried**, 22-39)	(0.025)	(0.039)	(0.055)	(73.74)	(57.48)
Avg. (two-child **state** maximum credit ×	-0.021	-0.007	-0.069	-106.16**	-86.43***
young children × **married**, 22-39)	(0.021)	(0.028)	(0.043)	(49.12)	(29.56)
Avg. (two-child **state** maximum credit ×	-0.008	-0.012	-0.020	-38.48	-13.07
older (only) children × **married**, 22-39)	(0.013)	(0.022)	(0.031)	(38.23)	(26.23)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	-0.0001	0.026	0.077*	26.40	15.95
	(0.013)	(0.024)	(0.039)	(36.26)	(30.23)
Unmarried (older children)	0.004	0.007	0.033***	23.80	20.47
	(0.006)	(0.010)	(0.011)	(16.45)	(12.86)
Married (young children)	-0.009	-0.005	-0.025**	-35.78***	-23.54***
	(0.005)	(0.008)	(0.012)	(12.29)	(8.26)
Married (older children)	0.001	-0.011*	-0.022**	-11.55	-9.98
	(0.004)	(0.006)	(0.010)	(9.44)	(6.78)
R^2	0.16	0.23	0.22	0.16	0.16
N	883	718	719	883	732

See notes to Table 2 and 5.

Appendix Table B5: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Separate Federal and State EITC Two-Child Phase-in Rates as Policy

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** phase-in rate ×	-0.002	0.002	0.002	-2.85	1.16
young children × **unmarried**, 22-39)	(0.005)	(0.011)	(0.016)	(15.39)	(10.41)
Avg. (two-child **federal** phase-in rate ×	0.001	0.011	0.022**	2.81	5.04
older (only) children × **unmarried**, 22-39)	(0.003)	(0.007)	(0.009)	(6.80)	(6.13)
Avg. (two-child **federal** phase-in rate ×	-0.006	-0.006	-0.013	-17.98*	-5.45
young children × **married**, 22-39)	(0.004)	(0.008)	(0.013)	(9.33)	(6.72)
Avg. (two-child **federal** phase-in rate ×	0.004	-0.008	-0.020	-2.38	-10.88*
older (only) children × **married**, 22-39)	(0.003)	(0.006)	(0.013)	(6.38)	(6.18)
Avg. (two-child **state** phase-in rate ×	0.005	0.003	0.017	47.07	30.11
young children × **unmarried**, 22-39)	(0.022)	(0.038)	(0.052)	(51.16)	(40.37)
Avg. (two-child **state** phase-in rate ×	0.007	-0.019	0.042	91.24***	86.45***
older (only) children × **unmarried**, 22-39)	(0.015)	(0.022)	(0.034)	(32.04)	(22.74)
Avg. (two-child **state** phase-in rate ×	-0.009	-0.006	-0.085*	-86.20*	-101.58***
young children × **married**, 22-39)	(0.020)	(0.024)	(0.044)	(46.93)	(29.30)
Avg. (two-child **state** phase-in rate ×	-0.002	0.001	-0.024	-39.35*	-26.96
older (only) children × **married**, 22-39)	(0.011)	(0.014)	(0.028)	(22.16)	(24.97)
B. Implied effect of one-year, 7.5 percentage-point increase in federal and 1.5pp increase in state phase-in rates					
Unmarried (young children)	-0.001	0.002	0.004	4.92	5.38
	(0.004)	(0.007)	(0.012)	(8.90)	(6.26)
Unmarried (older children)	0.001	0.006	0.022***	15.79***	16.75***
	(0.004)	(0.007)	(0.007)	(5.31)	(5.32)
Married (young children)	-0.006	-0.005	-0.022**	-26.41***	-19.32***
	(0.004)	(0.006)	(0.009)	(7.74)	(5.34)
Married (older children)	0.003	-0.006	-0.018**	-7.69	-12.20**
	(0.002)	(0.004)	(0.007)	(4.84)	(4.58)
R^2	0.09	0.16	0.17	0.09	0.11
N	1,505	1,176	1,177	1,505	1,197

See notes to Tables 2 and 5. The difference relative to Table 5 is that the two-child phase-in rate is used, instead of the two-child maximum credit. We use a policy simulation that amounts to about the same percentage increase in EITC generosity as the $1,000/$200 increases in the maximum credits used in Table 5 – in this case, a 7.5-percentage point increase in the federal and 1.5-percentage point increase in the state phase-in rates. These are approximately equal in relative terms. We have been using an increase of $1,000 2016 dollars, which would represent a 38.3 percent increase over the sample average federal maximum EITC faced ($2,610 in 2016 dollars; Table 2, row 2). A 0.075 phase-in (7.5 percentage points) rate increase is a 38.5 percent increase in the two-child EITC phase-in rate, based on a weighted average of observations in our sample (19.5 percentage points). Thus, the two measures are nearly identical in proportional terms. In addition, as in Table 5, the state increase is 20 percent of the federal increase (0.075×0.2=0.015). Note, though, that the regression coefficients in Panel A correspond to a one-year 10pp increase in the appropriate phase-in rate.

Appendix Table B6: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Separate Federal and State EITC One-Child Maximum Credits and Age of Children

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (one-child **federal** maximum credit ×	-0.002	-0.011	-0.008	2.19	3.06
young children × **unmarried**, 22-39)	(0.008)	(0.016)	(0.026)	(21.11)	(13.93)
Avg. (one-child **federal** maximum credit ×	0.001	0.021**	0.034***	2.47	3.82
older (only) children × **unmarried**, 22-39)	(0.006)	(0.010)	(0.013)	(11.11)	(9.78)
Avg. (one-child **federal** maximum credit ×	-0.009	-0.016	-0.021	-20.97	-2.78
young children × **married**, 22-39)	(0.006)	(0.013)	(0.020)	(13.53)	(9.35)
Avg. (one-child **federal** maximum credit ×	0.009**	-0.014	-0.031	-1.58	-19.71**
older (only) children × **married**, 22-39)	(0.004)	(0.010)	(0.020)	(9.88)	(9.69)
Avg. (one-child **state** maximum credit ×	0.015	0.018	0.044	54.28	45.99
young children × **unmarried**, 22-39)	(0.025)	(0.044)	(0.061)	(58.75)	(48.67)
Avg. (one-child **state** maximum credit ×	0.019	-0.022	0.040	104.07***	96.35***
older (only) children × **unmarried**, 22-39)	(0.017)	(0.028)	(0.039)	(36.69)	(25.43)
Avg. (one-child **state** maximum credit ×	0.002	0.001	-0.065	-73.27	-94.47***
young children × **married**, 22-39)	(0.021)	(0.030)	(0.052)	(53.18)	(34.05)
Avg. (one-child **state** maximum credit ×	-0.005	0.0004	-0.014	-40.89	-16.70
older (only) children × **married**, 22-39)	(0.012)	(0.016)	(0.036)	(26.21)	(25.49)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	0.001	-0.007	0.001	13.04	12.26
	(0.007)	(0.014)	(0.025)	(16.90)	(12.07)
Unmarried (older children)	0.005	0.017	0.042***	23.28**	23.09**
	(0.007)	(0.012)	(0.013)	(10.96)	(10.73)
Married (young children)	-0.009	-0.016	-0.034*	-35.63**	-21.67**
	(0.006)	(0.012)	(0.018)	(13.61)	(9.25)
Married (older children)	0.008*	-0.014	-0.034**	-9.76	-23.05**
	(0.004)	(0.009)	(0.017)	(10.07)	(8.64)
R^2	0.09	0.16	0.17	0.09	0.11
N	1,505	1,176	1,177	1,505	1,197

See notes to Tables 2 and 5. The only difference between this table and Table 5 is that here we use the one-child EITC maximum credit.

Appendix Table B7: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Separate Federal and State EITC Three-Child Maximum Credits and Age of Children

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (three-child **federal** maximum credit × **young children** × **unmarried**, 22-39)	-0.002	0.001	0.0001	-3.94	-0.48
	(0.004)	(0.008)	(0.012)	(11.25)	(7.57)
Avg. (three-child **federal** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.0004	0.008	0.015**	1.53	3.10
	(0.003)	(0.006)	(0.007)	(5.33)	(4.38)
Avg. (three-child **federal** maximum credit × **young children** × **married**, 22-39)	-0.004	-0.004	-0.008	-12.07*	-2.22
	(0.003)	(0.007)	(0.010)	(6.43)	(5.03)
Avg. (three-child **federal** maximum credit × **older (only) children** × **married**, 22-39)	0.003	-0.006	-0.013	-2.04	-7.53
	(0.002)	(0.005)	(0.010)	(4.67)	(4.75)
Avg. (three-child **state** maximum credit × **young children** × **unmarried**, 22-39)	0.004	0.007	0.012	30.58	18.46
	(0.016)	(0.028)	(0.039)	(39.96)	(32.47)
Avg. (three-child **state** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.001	-0.012	0.027	57.95**	59.55***
	(0.014)	(0.014)	(0.022)	(26.87)	(16.72)
Avg. (three-child **state** maximum credit × **young children** × **married**, 22-39)	0.002	0.001	-0.059**	-48.77*	-74.82***
	(0.012)	(0.016)	(0.029)	(26.72)	(17.41)
Avg. (three-child **state** maximum credit × **older (only) children** × **married**, 22-39)	0.001	-0.001	-0.021	-15.58	-16.05
	(0.008)	(0.009)	(0.017)	(18.16)	(13.41)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	-0.001	0.002	0.003	2.18	3.21
	(0.004)	(0.007)	(0.012)	(8.88)	(6.21)
Unmarried (older children)	0.001	0.006	0.021***	13.12**	15.01***
	(0.004)	(0.007)	(0.007)	(5.81)	(5.02)
Married (young children)	-0.004	-0.003	-0.020**	-21.82***	-17.18***
	(0.003)	(0.006)	(0.009)	(6.76)	(5.04)
Married (older children)	0.003	-0.006	-0.018**	-5.15	-10.74**
	(0.002)	(0.004)	(0.008)	(4.60)	(4.06)
R^2	0.09	0.17	0.17	0.09	0.11
N	1,505	1,176	1,177	1,505	1,197

See notes to Tables 2 and 5. The only difference between this table and Table 5 is that here we use the three-child EITC maximum credit.

Appendix Table B8: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Separate Federal and State EITC Two-Child Maximum Credits and Age of Children, Including Completed Fertility Control

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit × **young children** × **unmarried**, 22-39)	-0.002	0.001	-0.001	-3.68	-0.76
	(0.004)	(0.008)	(0.012)	(11.60)	(7.58)
Avg. (two-child **federal** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.000001	0.009	0.015**	0.14	2.31
	(0.003)	(0.005)	(0.007)	(5.64)	(4.65)
Avg. (two-child **federal** maximum credit × **young children** × **married**, 22-39)	-0.004	-0.003	-0.007	-11.63*	-2.66
	(0.003)	(0.007)	(0.010)	(6.81)	(5.20)
Avg. (two-child **federal** maximum credit × **older (only) children** × **married**, 22-39)	0.003	-0.007	-0.015	-2.53	-8.53*
	(0.002)	(0.005)	(0.010)	(5.05)	(5.05)
Avg. (two-child **state** maximum credit × **young children** × **unmarried**, 22-39)	-0.002	0.005	0.015	35.48	23.31
	(0.004)	(0.028)	(0.041)	(39.78)	(32.03)
Avg. (two-child **state** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.007	-0.010	0.038	73.29***	67.82***
	(0.012)	(0.018)	(0.026)	(25.31)	(18.69)
Avg. (two-child **state** maximum credit × **young children** × **married**, 22-39)	-0.008	-0.002	-0.064*	-71.57*	-78.87***
	(0.016)	(0.020)	(0.033)	(35.75)	(20.96)
Avg. (two-child **state** maximum credit × **older (only) children** × **married**, 22-39)	-0.002	-0.0002	-0.019	-31.93*	-21.16
	(0.008)	(0.011)	(0.022)	(16.96)	(18.71)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	-0.001	0.002	0.002	3.41	3.90
	(0.004)	(0.007)	(0.012)	(9.01)	(6.20)
Unmarried (older children)	0.001	0.006	0.023***	14.80**	15.88***
	(0.004)	(0.007)	(0.008)	(5.64)	(5.63)
Married (young children)	-0.006	-0.004	-0.021**	-25.94***	-18.43***
	(0.004)	(0.006)	(0.009)	(7.73)	(5.25)
Married (older children)	0.003	-0.007	-0.019**	-8.92*	-12.76***
	(0.002)	(0.004)	(0.008)	(4.91)	(4.69)
R^2	0.09	0.17	0.18	0.11	0.12
N	1,505	1,176	1,177	1,505	1,197

See notes to Table 2 and 5. The only difference between this table and Table 5 is that we include a control for each woman's total completed fertility.

Appendix Table B9: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Separate Federal and State EITC Two-Child Maximum Credits

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UF}, δ^{US}, δ^{MF}, δ^{MS})					
Avg. (two-child **federal** maximum credit × **children** × **unmarried**, 22-39)	-0.001	0.003	0.006	-2.23	-0.62
	(0.003)	(0.004)	(0.006)	(6.12)	(3.92)
Avg. (two-child **federal** maximum credit × **children** × **married**, 22-39)	0.001	-0.007	-0.015	-5.38	-7.93*
	(0.002)	(0.005)	(0.010)	(4.53)	(4.50)
Avg. (two-child **state** maximum credit × **children** × **unmarried**, 22-39)	0.008	-0.010	0.025	62.66***	60.21***
	(0.009)	(0.014)	(0.023)	(22.62)	(15.86)
Avg. (two-child **state** maximum credit × **children** × **married**, 22-39)	-0.004	0.001	-0.029	-43.29**	-33.58*
	(0.008)	(0.011)	(0.021)	(20.72)	(18.38)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried	0.001	0.001	0.011	10.30*	11.42***
	(0.003)	(0.005)	(0.007)	(5.23)	(3.80)
Married	0.0003	-0.007	-0.021***	-14.04***	-14.64***
	(0.002)	(0.004)	(0.007)	(4.99)	(3.99)
R^2	0.08	0.15	0.15	0.08	0.10
N	1,505	1,176	1,177	1,505	1,197

See notes to Table 2. In contrast to Table 5, this table adds the separate federal and state EITC variation to Table 3, instead of Table 4.

Appendix Table B10: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Projecting 1992 Federal EITC Parameters Forward

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit × **young children** × **unmarried**, 22-39)	-0.001	-0.034	0.006	20.51	21.70
	(0.013)	(0.028)	(0.044)	(29.57)	(26.66)
Avg. (two-child **federal** maximum credit × **older (only) children** × **unmarried**, 22-39)	-0.003	0.016	0.054	12.58	21.13
	(0.012)	(0.024)	(0.034)	(24.56)	(26.40)
Avg. (two-child **federal** maximum credit × **young children** × **married**, 22-39)	-0.004	-0.059***	-0.077**	-21.05	-21.43
	(0.008)	(0.016)	(0.033)	(18.35)	(17.49)
Avg. (two-child **federal** maximum credit × **older (only) children** × **married**, 22-39)	0.020**	-0.032	-0.072*	8.74	-38.55*
	(0.009)	(0.023)	(0.041)	(20.28)	(22.82)
Avg. (two-child **state** maximum credit × **young children** × **unmarried**, 22-39)	0.010	-0.001	0.033	66.16	48.32
	(0.024)	(0.056)	(0.078)	(53.76)	(68.41)
Avg. (two-child **state** maximum credit × **older (only) children** × **unmarried**, 22-39)	0.008	-0.031	0.027	87.67**	98.12***
	(0.014)	(0.034)	(0.042)	(34.15)	(33.44)
Avg. (two-child **state** maximum credit × **young children** × **married**, 22-39)	-0.013	0.004	-0.062	-97.81	-118.07**
	(0.019)	(0.034)	(0.070)	(63.81)	(52.26)
Avg. (two-child **state** maximum credit × **older (only) children** × **married**, 22-39)	-0.002	0.003	-0.050	-62.15	-48.46
	(0.017)	(0.023)	(0.048)	(37.99)	(35.17)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	0.001	-0.034	0.012	33.74	31.37
	(0.012)	(0.025)	(0.045)	(28.99)	(28.96)
Unmarried (older children)	-0.001	0.010	0.059*	30.11	40.76
	(0.014)	(0.023)	(0.032)	(26.03)	(26.18)
Married (young children)	-0.006	-0.058***	-0.089***	-40.61**	-45.04***
	(0.009)	(0.017)	(0.032)	(18.00)	(16.31)
Married (older children)	0.020**	-0.032	-0.082**	-3.69	-48.24**
	(0.008)	(0.023)	(0.039)	(20.95)	(21.67)
R^2	0.08	0.18	0.17	0.09	0.11
N	1,488	1,168	1,169	1,488	1,189

See notes to Tables 2 and 5. The difference here is that, while the state EITC supplement percentages are kept at their actual levels throughout the sample period, the OBRA 1993 expansion is, essentially, assumed to have never happened. Thus, we project the 1992 federal EITC parameters forward to 2016, only adjusting the maximum credit for inflation.

Appendix Table B11: Estimated Effects for More-Educated Women Corresponding to Table 8's Analysis of Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Including More-Educated Women as an Additional Level of Differencing (Placebo Test)

	Employment (1)	Log hourly wage (employed) (2)	Log earnings (employed) (3)	Annual hours (4)	Annual hours (employed) (5)
A. Coefficient estimates (δ^{UYF}, δ^{UOF}, δ^{MYF}, δ^{MOF}, δ^{UYS}, δ^{UOS}, δ^{MYS}, δ^{MOS})					
Avg. (two-child **federal** maximum credit × **young children** × **unmarried**, 22-39)	-0.006**	0.001	-0.002	-12.65*	-2.87
	(0.002)	(0.005)	(0.008)	(7.48)	(6.06)
Avg. (two-child **federal** maximum credit × **older (only) children** × **unmarried**, 22-39)	-0.001	0.003	0.003	-6.52	-1.47
	(0.002)	(0.003)	(0.005)	(4.45)	(3.73)
Avg. (two-child **federal** maximum credit × **young children** × **married**, 22-39)	-0.004**	0.005	0.012	-3.17	1.90
	(0.002)	(0.004)	(0.007)	(4.87)	(4.35)
Avg. (two-child **federal** maximum credit × **older (only) children** × **married**, 22-39)	-0.003	0.002	0.008	-3.86	-0.98
	(0.002)	(0.003)	(0.005)	(3.43)	(2.33)
Avg. (two-child **state** maximum credit × **young children** × **unmarried**, 22-39)	0.020	-0.054	-0.080*	4.84	-44.80*
	(0.012)	(0.033)	(0.040)	(31.80)	(23.53)
Avg. (two-child **state** maximum credit × **older (only) children** × **unmarried**, 22-39)	-0.016**	0.016	0.012	-20.55	24.78
	(0.007)	(0.023)	(0.028)	(40.47)	(38.04)
Avg. (two-child **state** maximum credit × **young children** × **married**, 22-39)	-0.0001	-0.025	-0.033	-2.38	-2.61
	(0.009)	(0.042)	(0.053)	(23.61)	(24.41)
Avg. (two-child **state** maximum credit × **older (only) children** × **married**, 22-39)	0.0003	-0.030	-0.023	17.45	19.47
	(0.008)	(0.049)	(0.067)	(24.38)	(23.90)
B. Implied effect of one-year, $1,000 increase in federal and $200 increase in state maximum credits (%)					
Unmarried (young children)	-0.002	-0.010	-0.018**	-11.68	-11.84*
	(0.003)	(0.008)	(0.009)	(8.37)	(6.21)
Unmarried (older children)	-0.004**	0.006	0.005	-10.63	3.48
	(0.002)	(0.006)	(0.008)	(8.23)	(7.95)
Married (young children)	-0.004**	-0.0003	0.006	-3.65	1.38
	(0.002)	(0.007)	(0.010)	(5.06)	(5.32)
Married (older children)	-0.003	-0.004	0.003	-0.37	2.91
	(0.002)	(0.009)	(0.012)	(4.78)	(4.63)
R^2	0.09	0.23	0.17	0.12	0.12
N	3,358	2,757	2,760	3,358	2,804

See notes to Tables 2, 5, and 10. The estimates in this table – in contrast to Table 10 – are for the high-educated women.