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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 indicate 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 that longer-run exposure of unmarried mothers to a more generous EITC increases cumulative labor market experience. Additionally, we find evidence that exposure to a more generous EITC when women had children but were married leads to lower earnings and hours in the longer-run. These longer-run effects are consistent with what we would expect 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 lead to greater labor market experience in the longer-run, boosting earnings via greater human capital accumulation; other types of investment, including more intensive search for better paying jobs with stronger prospects for earnings growth, could also be spurred by a more generous EITC that has positive short-term effects on employment. 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] Much of the work finds modest negative labor supply effects (e.g., Eissa and Hoynes, 2004) or no effect at all (Eissa and Liebman, 1996), although Hoffman and Seidman (2003) suggest that there are sizable disemployment effects for married women in the phase-out range, and some decrease in hours for married

[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] On the "plateau" when the EITC is fixed and on the phase-out range there are negative income effects, and on the phase-out range there is also a higher implicit marginal tax rate, so standard theory would predict labor supply disincentives in both regimes, which are strongest in the phase-out range (assuming that substitution effects dominate).

women and married men. Short-run effects, even if they are small, could potentially accumulate into larger negative effects over the longer run.

We test for evidence of longer-run effects of the EITC, adopting a very long-run perspective. Given that EITC payments depend on number of children (directly) and marital status (indirectly, via the spouse's income), in order to capture the long-run effects of the EITC we must be able to observe a woman's childbearing and marital history. The need to capture this history, combined with the requirement to capture state variation in the EITC based on state of residence, necessitates our use of the Panel Study of Income Dynamics (PSID). Specifically, we use longitudinal data on marriage and children from the PSID to characterize women's exposure to the federal and state Earned Income Tax Credit (EITC) from ages 22 to 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 as mature adults, defined here as age 40.

We find evidence indicating 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 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 that exposure to a more generous EITC when women had children but were married leads to lower earnings and hours in the longer-run.[3]

Our analysis is based on a difference-in-difference-in-differences specification measuring the presence of children and marital status across ages 22 to 39 and exposure to the EITC. We subject this specification to a number of checks meant to account for endogenous behavior or policy, a placebo test, and a number of

[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 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.

robustness/sensitivity analyses. Among these checks, we include alternative parameterizations of the effects of the EITC and checking whether the results reflect changes in other anti-poverty policies. These analyses show that the findings are robust, and bolster a causal interpretation of the evidence.

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

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, with the primary goal of subsidizing the earnings of low-income families with children. In the federal program, the phase-in credit rate – the amount a family receives in credit as earnings rise above zero – is based on the number of children, with rates of 34, 40, and 45 percent for families with one, two, or three or more children, respectively. Figure 1 shows the evolution of these credits for a single taxpayer as earned income increases for tax year 2016 (the last year in our sample).[4] 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 a rate around half the rate at which it phased-in until a family is no longer eligible. Notice that Figure 1 also shows a meager credit for families with no qualifying children, with a 7.65 percent credit rate and a $506 maximum credit; the phase-out rate for the childless credit is also 7.65 percent.

Empirical work that parameterizes the EITC effect typically uses a single dimension of the EITC; the two key parameters 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. Conversely, in principle one could have a high phase-in rate but a low maximum credit, which is a possible argument for

[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.

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]

Importantly, the positive extensive-margin employment effects are not predicted for all EITC-eligible women. Those who are second earners, including many married women, may have predicted negative intensive-margin employment effects, depending on the model of labor supply (e.g., Eissa and Hoynes, 2004); higher-earning single mothers also potentially face such effects. These effects stem from the income effect along the plateau, and even more so from the phase-out region, where women could face effective marginal tax rates of nearly 50 percent. Thus, the maximum credit should also capture potential negative intensive-margin effects for these mothers (and, like the phase-in rate, the phase-out rate is closely related to the maximum credit). There could also be negative extensive margin effects for some secondary earners.

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 effects of the EITC on contemporaneous outcomes, paralleling the analysis of short-term employment effects in other papers (e.g., Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001). This allows us to show how we build on these past studies in an intuitive fashion to derive our longer-run estimation strategy.

First, define Y_{ijt} as log earnings (one of the outcomes we consider) for person i in state j at period t,[6] 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

[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, 2006). Others (e.g., Eissa and Hoynes, 2004) try to parameterize the tax effects of the EITC more fully.

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

parameter, CR_{jt}, is the EITC maximum credit for state j in period t, as discussed above. We treat the maximum credit for childless women as effectively zero. In addition, the specification ignores variation across number of children, conditioning only on whether a woman has any children (in practice, we elect to use the two-child maximum credit); this ensures that the policy parameter is exogenous to the number of children and exploits the greatest variation in EITC generosity (children vs. no children).[7] 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.[8] Thus, equation (1) below yields a simple difference-in-difference-in-differences (DDD) specification for estimating the effect of the EITC on Y:

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

In this equation, 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.[9,10]

As always, we cannot distinguish between a true effect of the EITC on women with children and shocks that vary by state and year *and* children. The identifying assumption is that the shocks are the same for women with or without children. Thus, the estimate of δ in equation (1) is typically interpreted as a DDD

[7] Moreover, variation in the maximum credit based on number of children cannot be readily incorporated into our longer-run specification. As explained below, this specification captures the history of the EITC women faced, as well as their childbearing history. Making the EITC variation dependent on the number of children therefore confounds the two separate effects of policy variation and childbearing history.
[8] We relax these restrictions later, but for ease of exposition start simply and gradually add complexity.
[9] This greater parsimony becomes valuable given that the PSID does not yield a large sample with long-term longitudinal data on women.
[10] 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 assume the childless EITC is irrelevant, in which case β captures only common shocks and δ captures the effect of the EITC.

estimator – identified from the difference between the change in employment associated with a more generous EITC for women with children and women without children (the difference between two DD estimators).

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

$$(2)\ Y_{ijt} = \alpha + \beta^{U}CR_{jt}\cdot U_{ijt} + \gamma^{U}K_{ijt}\cdot U_{ijt} + \delta^{U}CR_{jt}\cdot K_{ijt}\cdot U_{ijt}$$
$$+ \beta^{M}CR_{jt}\cdot M_{ijt} + \gamma^{M}K_{ijt}\cdot M_{ijt} + \delta^{M}CR_{jt}\cdot K_{ijt}\cdot M_{ijt} + \eta M_{ijt} + D_{j}\theta + D_{t}\lambda + \varepsilon_{ijt}\ .$$ [11]

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 can expand equation (2) in a straightforward manner to estimate the long-run effects of the EITC. We explain this in a slightly simpler setting than the one we actually use, for clarity, and then explain how our approach deviates from this.

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 to 39 in our baseline specification). Then, we calculate the interactions of these values at each age and use as our regressors the averages of these interactions over ages 22 to 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:

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

For a woman who never has children while unmarried from 22 to 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 to 39. We can construct similar averages for the other terms corresponding to equation (2). We

[11] Note that in equation (3) 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.

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. Our approach will capture, for example, the difference between two women who had the same childbearing history and faced the same EITC in each year, but who were married in different periods.[12] We can then substitute these expressions into equation (2) to estimate the effects of these longer-run exposure variables on outcomes at age 40.[13]

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 facilitate comparisons between the shorter-term and longer-term results. Furthermore, the intuition is relatively straightforward, building naturally on the types of difference 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.

We also include as controls the full set of variables for marital status, children, EITC, etc., at age 40, to ensure that we do not confound the effects of past marriage, childbearing, and the EITC with effects of contemporaneous variables.[14] Following this strategy, our baseline equation would take the form:

$$(4)\; Y_{ijt} = \alpha + \beta^U\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot U_{ija})/18\} + \gamma^U\{\sum_{a=t-18}^{t-1}(K_{ija}\cdot U_{ija})/18\} + \boldsymbol{\delta^U\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot K_{ija}\cdot U_{ija})/18\}}$$

$$+ \beta^M\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot M_{ija})/18\} + \gamma^M\{\sum_{a=t-18}^{t-1}(K_{ija}\cdot M_{ija})/18\} + \boldsymbol{\delta^M\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot K_{ija}\cdot M_{ija})/18\}}$$

$$+ \eta\{\textstyle\sum_{a=t-18}^{t-1}M_{ija}/18\} + \beta^{U,40}CR_{ijt}\cdot U_{ijt} + \gamma^{U,40}K_{ijt}\cdot U_{ijt} + \delta^{U,40}CR_{ijt}\cdot K_{ijt}\cdot U_{ijt}$$

[12] We also report results from specifications that allow even more flexibility with respect to timing of children.
[13] We vary this age in analyses reported below.
[14] In this case, the marital status and children variables are dummy variables. These variables are not included in our models for cumulative experience, as we measure that outcome over ages 22 to 39.

$+ \beta^{M,40} CR_{ijt} \cdot M_{ijt} + \gamma^{M,40} K_{ijt} \cdot M_{ijt} + \delta^{M,40} CR_{ijt} \cdot K_{ijt} \cdot M_{ijt} + \eta^{40} M_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}$.

Although equation (4) looks complicated, the parallel to equation (2) is clear, and we have retained the same notation for the key parameters of interest that we discussed above, 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.

In light of this more complex, long-run specification, it is useful to consider again how we identify the effects of the EITC. Paralleling our earlier discussion of δ^U and δ^M in reference to equation (2), where we consider the short-run effects of the EITC for both unmarried and married women, 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*. Of course, this raises the question of 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. Thus, δ^U 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.

What do the estimated coefficients mean? Consider 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*. In particular, we measure the maximum credit in \$1,000 units (2016 dollars).[15] Thus, given

[15] 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) – one can replace

that we condition on $\sum_{a=t\text{-}18}^{t\text{-}1}(K_{ija}\cdot U_{ija})/18\}$ – the marital status and childbearing histories, and their interactions – a one-unit increase in the variable corresponding to δ^U ({ $\sum_{a=t\text{-}18}^{t\text{-}1}(CR_{ja}\cdot K_{ija}\cdot U_{ija})/18\}$) 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 well 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, this implied effect is for a woman who is unmarried and has children over the entire age range we use (22 to 39). Thus, we alternatively consider the effect of a $1,000 one-year increase in the maximum credit when unmarried and with children, which would be $\delta^U/18$.[16]

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

Finally, because the predicted effects of the EITC are different for unmarried women with children and married women with children, we are interested in the differences between the effects we estimate based on marital status. Given that we estimate a joint model for unmarried and married women, we can easily estimate the effect and do inference for the differences ($\delta^U - \delta^M$) or (($\delta^U - \delta^M$)/18).

Finally, we introduce one additional complication, which was useful to defer discussing until this point, so that we could explain our longer-run specification in a simpler context. Specifically, we introduce

CR with state-year interactions, and also include interactions between *K* and the state-year interactions, fully absorbing the federal variation in the EITC. However, in the models we estimate – like equation (4) – we have summed terms that can capture the full 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) – and the same is true of the longer-term analyses discussed in the Introduction.

[16] 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.

differential effects of longer-run exposure to the EITC when women have young children (i.e., not of school age, defined as ages 5 and under), as opposed to only school-age children. Our a priori reasoning about this difference was that the short-run labor supply effects of the EITC could be different 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. Given that our preliminary analysis bore out this expectation, we present our results for the specification distinguishing effects based on age of children.[17] 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; and we use subscripts *Y* and *O* to denote younger and older children. 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’) $\delta^{MY}\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot YK_{ija}\cdot M_{ija})/18\} + \delta^{MO}\{\sum_{a=t-18}^{t-1}(CR_{ja}\cdot OK_{ija}\cdot M_{ija})/18\}$.

In other words, the longer-run exposure measures like equation (3) are expanded to capture exposure based on marital status and having children – but with the presence of children distinguished by their ages.

[17] 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.

This augmentation of the specification adds a slight complication to interpreting the magnitudes of the estimates of our δ coefficients, of which there are now four – δ^{UY}, δ^{UO}, δ^{MY}, and δ^{MO}. For example, the estimate of δ^{UY} captures the longer-run effect of exposure to a more generous EITC for unmarried (low-skilled) women with young children. Unlike before, though, it does not make sense to think about these women being exposed to a permanently higher maximum credit; given that children age, a woman who has two children would have both young and older (only) children across ages 22-39. Thus, when we calculate the effect of a permanent $1,000 increase in the maximum credit, we instead do this for a hypothetical woman who has one child at age 22 and one at age 24. We then plug the implied childbearing history captured in *YK* and *OK* for each age from 22-39, in the terms in equations (5) and (5'), and compute the longer-term effects implied by these histories and the estimated coefficients. Taking unmarried women as an example, the calculation would be:

(6) $[\widehat{\delta^{UY}} \sum_{a=t-18}^{t-10} (CR_{ja} \cdot YK_{ija} \cdot U_{ija})\} + \widehat{\delta^{UO}} \sum_{a=t-9}^{t-1} (CR_{ja} \cdot OK_{ija} \cdot U_{ija})\}]/18,$

where the first term is computed and averaged over ages 22-29 (when there are young children in the household), and the second over ages 30-39 (when both children are school-aged). For a woman who is unmarried and has children at ages 22 and 24, and faces a $1,000 higher maximum credit (measured in real (2016) $1,000 dollar increments), this is simply:

(6') $[\{\widehat{\delta^{UY}} \cdot 8\} + \{\widehat{\delta^{UO}} \cdot 10\}]/18.$[18]

The implied effect of a one-year change in the maximum credit does not entail these complications, since we do no accumulate the effect over many years. Instead, we simply report the implied effect depending on whether the woman has younger kids or older kids. The calculation is the same as before, just using the estimate of the corresponding parameter – δ^{UY}, δ^{UO}, δ^{MY}, or δ^{MO} .

III. 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

[18] It is straightforward to compute the standard error of this expression, based on the variances and covariance of the two estimates.

EITC, as explained in Section II, depends on their marital and childbearing history, as well as their (state) residential history.[19] We also use the longitudinal data to construct a cumulative measures of years of experience.

The PSID began in 1968 with a nationally representative sample of 18,000 individuals belonging to 5,000 families. Since 1968, the PSID has followed these individuals and their descendants, interviewing them on an annual basis (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.[20]

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.[21] We assign full 19-year histories for all the necessary variables: marital status, number of children, age of children, and employment.[22,23] 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. Our selection of age 40 as the age at which we estimate these effects is similarly motivated. Examining the long-term effects of the EITC requires us to select a late enough age where women have completed the vast majority of their childbearing, while at the same time balancing the sample size shrinkage that occurs when we increase the age at which we measure outcomes and, by extension, increase the length of the history we must observe.[24]

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

[20] 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, as described below.

[21] 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.

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

[23] Note also that 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).

[24] We show later that our results are robust across nearby ages.

Our analysis focuses on the cumulative effects of exposure to a more generous EITC on outcomes at age 40. We would like to test for 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, 1978 is the first year in which employment status for all individuals of working age (16 years and older) is captured. Before 1979, employment status was only available for heads of household and their spouses. Thus, we would need to discard more than a decade's worth of data if we required all the women in our baseline sample to have this measure. However, the PSID does not provide a large sample that meets our other criteria, and we therefore do not rely on this measure to capture cumulative experience. Instead, we use the full panel, defining employment status at each age based on whether there are positive earnings. The limitation of this measure is that while it is available for all years, it is only available for heads of household and their 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. In part, this motivates our selection of age 22 (as opposed to 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.

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 generosities.[25] 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. The cohorts who are age 40 from 1986 to 2015 experience EITCs anywhere in between these extremes.

[25] And, per the discussion above about measuring cumulative experience, our method ensures that we capture this wide range of variation.

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

To assign number of children by age, we use birth history information. Specifically, women report the birth timing of up to five children, allowing us to assign a detailed child history over a woman's primary childbearing years.[26] 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.[27] (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.) 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 using the CPI-U. We count a woman as employed if she had positive earnings in the past year. As previously mentioned, we construct our cumulative experience measures using the same definition at each age from 22 to 39. As we can only observe this measure for women who are a head or spouse in each year, our sample sizes are smaller when estimating cumulative experience effects.

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.[28] 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.

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[26] A woman's birth history includes her number of live births and the birth month and year of up to five children. We therefore exclude a very small number of women who have more than five live births, because we cannot assign ages to each child.
[27] As eligibility requirements for the EITC are based on actual care of the child, we are also implicitly assuming that all women in our sample care for each of their birth children more than half the year.
[28] For example, we cannot consistently code Hispanic ethnicity.

Table 1 shows the sample construction, and how the sample restrictions we impose based on the need for long-term longitudinal data restrict the number of available observations. Offspring of original sample members (and some additional families) are added over time, and the last available survey is in 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,[29] which is why the available observations drop so sharply from row A to row E.[30] 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, or race (black/non-black) being coded consistently over time). 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.[31] We depict this policy variation visually in Figures 2 and 3. Figure 2 shows the federal EITC maximum credit depending on number of children. The figure illustrates that, as noted in the previous section, 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 measure – the two-child maximum credit.

Figure 3 depicts information on supplemental state EITCs; with one exception (Wisconsin), states calculate their supplements as a fixed percentage of the family's federal credit.[32] 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 then show the average, minimum, and maximum state supplement rates over time. As the figure shows, the average state supplement rate was quite high in the late 1980s and early 1990s for the handful of

[29] 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.
[30] 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.
[31] See http://www.taxpolicycenter.org/sites/default/files/legacy/taxfacts/content/PDF/historical_eitc_parameters.pdf (viewed August 16, 2018).
[32] 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.

states that had state supplements. 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, even as the number of states offering supplements to the federal credit increased over time.

IV. Results

Descriptive Statistics

Table 2 reports descriptive statistics for our core PSID analysis sample of less-educated women. The first column shows averages for our baseline sample across ages 22-39, and the second column as of age 40, when we measure longer-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. The share black is quite high, reflecting oversampling of low-income families in the PSID. For most of our analyses, we do not weight our estimates, because the variation provided by oversampling of a population that is underrepresented in the target population is useful, increasing variation in the independent variables, which leads to estimates that are more precise.[33] (We show in the appendix that the results are not sensitive to weighting.) Finally, the last rows report descriptive statistics for the outcomes measured at age 40.

Results from Baseline Specification[34]

Table 3 presents estimates of the regression models used in our core analysis – equation (4), modified

[33] 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.) Regardless, as we show later, the estimates are very similar, but more precise without weighting – per our argument.

[34] 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 A. In short, we show that we generally can replicate the results from these papers with the PSID data.

to distinguish between ages of children – of the effects of long-run exposure to the EITC. Panel A of Table 3 reports the estimates and standard errors of our four coefficients of interest – δ^{UY} , δ^{UO}, δ^{MY}, and δ^{MO} – which are the coefficients on the summed triple interactions between the EITC maximum credit and the kids and marital status variables.[35] We show results for the following outcomes at age 40: employment, log hourly wage (employed), log earnings (employed), and annual hours. We discuss these results in detail for earnings – which we regard as the most important outcome – and then having explained what the table shows, we discuss the results for the other outcomes.

As shown in the first two rows of column (3), we obtain positive estimates of the effect of the EITC for women exposed to a more generous EITC when unmarried with children. A positive effect is consistent with the notion that the short-run positive extensive labor market effects of the EITC for unmarried women with children may translate into higher earnings in the longer run, likely in part through the accumulation of experience. The estimated effect is insignificant for exposure when children are younger than school age (0.020), but over ten times as large (0.246), and significant, for exposure when children are of school age. In the third and fourth rows, we find negative estimates for married women exposed to a more generous EITC when they have children. The estimated magnitudes are not very different depending on the age of the children, but only the estimate for school-age children is significant (at the 10-percent level). The estimated absolute magnitude is similar to the effect of exposure to a more generous EITC for unmarried women with school-age children, but opposite-signed.

We view these estimates are largely consistent with expectations: theory predicts, and existing evidence establishes, that the contemporaneous effect of the EITC is to boost employment of women with children who are unmarried (as they are likely to have lower family income). Additionally, the EITC is more likely to reduce employment among married women with children (although this evidence in the existing literature is weaker). Of course, in this case we test for these predicted effects of the EITC in a dynamic setting. The estimates in column (3) presumably reflect the accumulation of these static or contemporaneous employment effects into their impacts on earnings over many years. The apparent evidence of cumulative

[35] We show the estimated coefficients of the triple-interactions, from which causal inferences can be drawn. The full model estimates are available upon request.

negative effects of married women is thus not inconsistent with the often weak evidence of negative short-run labor supply effects for married women (e.g., Eissa and Hoynes, 2004).

Next, the table reports on the two ways we explained earlier of translating these estimated coefficients into more meaningful quantities. First, in panel B we compute the implied effect of a permanent $1,000 real increase in the maximum credit. Recall that to do this we have to assume a particular childbearing history (births at ages 22 and 24). As the first two rows in panel B of Table 3 show, for unmarried women the implied effect for unmarried women with children is approximately 14.5 percent higher earnings, and the implied effect for married women with children is approximately 21.6 percent lower earnings (only the latter is significant, at the 10-percent level).

The difference in the effect between unmarried and married women with children is shown in the next row. This difference is the sum of the separate effects – approximately 36.1 percent higher earnings for unmarried women (significant at the 10-percent level). There is a potential argument for viewing this estimate as the causal effect for unmarried women. In particular, one might argue that based on the literature on short-run employment effects there is reason to be skeptical that the EITC has much effect on married women. If so, then the estimated effect for married women is not causal, but instead reflects other changes in labor market outcomes for low-education women that happen to be correlated with variation in the EITC. In this case, the married women might serve as a control group, and the difference for unmarried women relative to married women might provide a better causal estimate. As noted, the estimate is of the same sign, but larger, because of the negative estimate for married women.

We also report coefficient estimates in terms of the implied effect of a one-year $1,000 increase in the maximum credit (panel C). As column (3) shows, we get qualitatively similar, albeit smaller, implied effects. For unmarried women with school-age children, the implied effect is 1.4 percent higher earnings, significant at the 5-percent level; for married women, the estimated effect is negative regardless of age of children, but larger (and significant at the 10-percent level) only for school-age children (1.4 percent lower earnings). The unmarried minus married estimate for school-age children is positive regardless of age of children, but larger (2.7 percent higher earnings) and significant only for school-age children.

Having explained the estimates in each row, we now turn to the evidence for the outcomes covered in the other columns. For employment, in column (1), none of the estimated coefficients, reported in the first four rows, are statistically significant, and the sign pattern – unlike for earnings – 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. As the remaining rows indicate, most of the implied effects for the policy changes we consider are very small.

In contrast, the results for wages, reported in column (2), are quite consistent with the results for earnings (column (3)) that we previously discussed. Women exposed to a more generous EITC when unmarried with children have higher wages at age 40. The estimated effect is larger for unmarried women exposed when they had school-age children (about 9.4 percent, vs. 3.1 percent), although both estimates are insignificant. Similarly, the estimated effects are negative for women exposed to a more generous EITC when they were married with children. Again, the estimated effects is larger for exposure when women had school-age children, and again, the estimates are not significant. The translation of these estimates into more meaningful magnitudes is reported in the remaining rows of the table. For example, although insignificant, the estimates imply that unmarried women with children (at ages 22 and 24) exposed to a $1,000 higher maximum credit, over the ages 22-39, earn 6.6 percent higher wages at age 40, and because of the negative effect for married women, the estimated different between unmarried and married women is approximately 14.6 percent.[36]

The effects on hours (without conditioning on employment) are reported 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 young children – a significant hours differential of 280 hours. Translating this into the effect of long-term exposure to a $1,000 higher maximum credit, the effect falls to 159 hours, and the effect of one year of exposure, when children were young, is 15.6 hours; all of these estimates are statistically significant. For

[36] As with all work showing beneficial effects of the EITC for groups like low-skilled mothers, it is possible that the relative and absolute effects on women with children differ if the EITC worsens outcomes for low-skilled, unmarried women without children, owing to an outward labor supply shift among those with children lowering market wages of women who get no (or meager) benefits (Leigh, 2000). There is some evidence of adverse effects of the EITC on wages and employment of low-skilled childless individuals, and female teenagers (Neumark and Wascher, 2011). Thus, the beneficial longer-run effects of the EITC that we estimate here, and that other studies estimate, may somewhat overstate the absolute beneficial effects.

unmarried women with school-age children, the estimated hours coefficient is an insignificant but positive 68 hours.[37] This hours effect does not condition on employment, but the contemporaneous employment effects are negligible, implying that the hours effect 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.[38]

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 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 – generating a non-causal relationship between later earnings and our measure of exposure to a more generous EITC when unmarried with children.

With respect to marriage, the mechanics of the EITC might generate endogenous selection in this direction. A woman who earns enough to put her on the phase-out range might be expected to lose her EITC payment if she marries, as long as the spouse's earnings push her beyond eligibility for the EITC. Similarly, a low-earning woman who earns enough to obtain the maximum EITC credit (i.e., is on the plateau) also may face a marriage disincentive, since marriage could push her onto the phase-out range where the EITC payment is lower. In contrast, a very low-earning woman whose EITC payment is well below the maximum credit could face a higher EITC payment as a result of marriage (as long as combined earnings do not put her far

[37] Estimated hours effects conditional on employment were similar. These results, and others we discuss but do not report in the tables, are available from the authors upon request.

[38] We noted above that we do not define the maximum credit based on number of children. For unmarried mothers, the fact that some of the "young kids" years likely include more children suggests that our estimated positive effects of the EITC on labor supply measures during these years may be downward biased (because the additional children deter labor supply). And for married mothers, the bias is presumably in the same direction. Thus, if we could fully incorporate this information, we might expect the positive unmarried effects to be, if anything, larger, and the negative married effects to be closer to zero. This would reinforce evidence of positive effects on unmarried women and weaken the evidence of negative effects on married women.

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 this, we estimate models like those reported in Table 3, but defining as dependent variables the fraction of years from ages 22-39 that a woman spent with any kids, with young kids, or with older kids (only). Our right-hand-side variables become simpler: we include the exposure to EITC variables, but without any interactions with children. Thus, our estimating equation becomes:

$$(7)\ Y_{ijt} = \alpha + \beta^{U}\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot U_{ija})/18\} + \beta^{M}\{\textstyle\sum_{a=t-18}^{t-1}(CR_{ja}\cdot M_{ija})/18\} + \eta\{\textstyle\sum_{a=t-18}^{t-1}M_{ija}/18\}$$

$$+ \beta^{U,40}CR_{ijt}\cdot U_{ijt} + \beta^{M,40}CR_{ijt}\cdot M_{ijt} + \eta^{40}M_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .$$

We report the estimates from these models in part I of Table 4. In no case do we find that exposure to a more generous EITC increased childbearing – measured as the fraction of years with children. Indeed, all six estimates are negative, with four of the six statistically significant (one at the 10-percent level). In panels B and C of part I of the table, we translate these coefficients into implied differences associated with a permanent $1,000 or one-year real increase in the maximum credit, as in previous tables, comparing women with different marital histories, and we can see that the estimated effects are small.[39] The effects on total years with kids are driven primarily by the number of years with older (only) kids, implying that women are not having fewer children in total, which would necessitate spending fewer years with younger children, but are merely marginally reducing their gaps between children. Although the estimated effects are significant in some specifications, the effects are very small.

It may seem more plausible that marriage responds. After all, being married or not may have trivial economic consequences since one can cohabit, so the incentive effects of the EITC may be stronger for marriage than for childbearing. Estimating these effects in a similar fashion, we modify equation (7). We now estimate it for the fraction of years married, but we re-introduce the interactions between the EITC and the childbearing variables (fractions of years with children) in place of the interactions with marital status.

Part II of Table 4 reports these estimates. They suggest that low-education women with young children spend more years unmarried when the EITC is more generous – the estimate in this case is −0.050, and is significant at the 10-percent level. Panel B translates these estimates into implied effects of a one-year $1,000 real increase in the maximum credit.[40] The estimates in this table are negative for women with young 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.[41]

Thus, our evidence does not point to any substantive evidence of endogeneity bias that could generate spurious support for what we regard as our key finding – that unmarried women with children exposed to a higher EITC have higher earnings in the longer run, 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 above

[39] Note that, unlike in Table 3, panel B does not report separate effects for unmarried and married women. That is because, for this specification, these are simply the estimates in panel A.
[40] In this case, we do not report the effect of a permanent (i.e., age 22-39) increase in the maximum credit, since it does not make sense to think of age of children as fixed (young or school-aged) over this period.
[41] To see this why we say the effect is small, consider the larger estimate in panel B of part II of the table – for the effect of a one-year $1,000 increase in the maximum credit, for unmarried women with young children. Even if a woman faced this higher credit in every year, and managed to have the full 18 years with young children (which is fairly unrealistic), the implied effect on the fraction of years married would only be 0.054 ($18 \times \{-0.003\}$).

but do not pursue in this paper.

Next, we explore the possibility that endogenous migration could influence our findings. In principle, lower-skilled women potentially eligible for the EITC who are more interested in working, who – as suggested by our evidence thus far – accumulate more human capital and eventually earn higher wages and earnings, could migrate to states with more generous EITCs, generating spurious evidence of the positive effects of exposure to a more generous EITC like those we find. Our first check, in panel A of Table 5, is simply to apply EITC policy from the state of residence at age 22 for all the years for which we accumulate effects, rather than letting women's EITC exposure be determined by the states to which they migrate. The estimates from this analysis are very similar to the baseline estimates.

A second check is to use only federal EITC variation, which provides important variation but is unaffected by inter-state migration. These estimates, reported in panel B of Table 5, are also very similar. Thus, we conclude that migration does not bias our estimated effects. The analysis using only federal variation is also potentially useful to address concerns that state variation in EITC policy responds endogenously to labor market behavior of the women who are affected (or the controls). However, given that we are looking at long-term cumulative effects of EITC policy, we doubt this is much of a concern – consistent with the similarity of the estimates.

Endogenous Policy Variation

In our next analysis, 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 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 women with higher education levels, create a dummy variable for the less-educated women, and include all the variables in the baseline 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 *LE* with the year dummy variables, to allow for possible changes over time in differences in the outcomes we study between lower- and higher-education women, which could be correlated with changes in the generosity of the EITC over time.[42] 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, with the interactions between the EITC, marriage, and fertility variables for the latter 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 6, are qualitatively very similar to those in Table 3. We find positive effects on earnings for unmarried mothers exposed to a more generous EITC over the longer run, and negative effects for married mothers. In this case, the statistical significance is stronger for the latter, and weaker for the former. On the other hand, there is also a larger estimated positive hours effect for unmarried mothers. Overall, though, the point estimates are similar. 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 A3, confirm that this is the case.

The previous analysis can be viewed as controlling for a source of non-exogenous variation in the EITC that threatens the interpretation of our baseline estimates as causal – specifically, the possibility that EITC variation is correlated with other shocks or factors affecting longer-run outcomes for different kinds of women. 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

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

problems they identify are much less likely to apply. The results are reported in Table 7. They are often a bit less precise than the baseline estimates in Table 3, 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.[43] To assess whether the effects of these other policies could be confounded with longer-run effects of the EITC, in Table 8 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 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 to be one adult and two dependent children.[44] 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

[43] 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.

[44] 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.

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 interactions with kids (by age) and marital status.

The estimates incorporating these two explicit welfare and welfare reform measures are reported in panel A of Table 8. The estimates are quite similar to their Table 3 counterparts, 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.[45]

In panel B, we instead add controls for the minimum wage. We enter the minimum wage in the same way as the EITC, with the interactions with kids (by age) and marital status. We use the average real minimum wage (using the higher of the state or federal minimum) over ages 22-39, the same age range used for our EITC, childbearing, and marital status measures. A comparison of the estimates in panel B with those in Table 3 shows that adding the minimum wage controls has virtually no impact on the estimates.[46,47]

Of course, we cannot decisively rule out the concern about confounding policies, as other policies that

[45] 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. These estimates were far less precise, although the pattern of estimates was the same overall. The much greater imprecision 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 policy effects.

[46] 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.

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

changed simultaneously with the EITC could exist. However, the policy changes that would have to be relevant are those that differentially affect women based on income (proxied by marital status) and presence of children.[48]

Robustness Checks

We next turn to additional analyses intended to probe the robustness of the results. It is useful to provide the punchline first: The qualitative results are robust, and the statistical strength of the evidence generally does not vary much.

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. Figure 4 summarizes the results, reporting the results in terms of the effects of a one-year, $1,000 real increase in the maximum credit (like in panel C of Table 3); we report the estimates and the 95-percent confidence intervals. Recall that our strongest and most consistent results were for earnings and hours. Here, we see that the results are often consistent regardless of which age we use, and some results are very consistent – like for earnings and hours for married women, whether exposed when children and young or older. Cases where the results are a bit less consistent – including wages and earnings effects from exposure of unmarried mothers with older children to a more generous EITC – indicate a pattern of rising effects with age, suggesting that these longer-run effects become more apparent at the older ages we consider. Furthermore, the figure indicates there is nothing unique about the age of 40 for which we have presented most of our analyses.

In addition, in Appendix A we discuss results and report estimates for three additional robustness analyses. First, we show that the results are robust to specifying the effect of the EITC in terms of the two-

[48] Other potential policies which 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 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).

child phase-in rate rather than the maximum credit (Appendix Table A4). Second, we show that the results are robust to weighting (Appendix Table A5). And third (related to the weighting), we show results that are quite similar for black and non-black women (Appendix Table A6), with the one exception 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.

Mechanism

The estimates to this point indicate that when unmarried mothers are exposed to a more generous EITC, the combination of higher wages and higher hours contributes to a longer-run positive effect on earnings. 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. 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.[49] Together, these effects would boost wages and, via labor supply effects, hours and earnings. 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 EITC, presumably because of similar effects in the opposite direction from the EITC discouraging work.

Our final table looks for evidence of these cumulative effects on experience. We estimate the same specifications as used previously, 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).[50]

The results for our baseline analysis, using only less-educated women, are reported in columns (1) and

[49] This could include more investment in education, although examining this would require a different identification strategy than the one we use, which stratifies on education.
[50] Recall that these are measured for heads and spouses only.

(2) of Table 9. For the cumulative years of work measure, we find significant positive effects of exposure of unmarried mothers to a more generous EITC. In panel B, for example, we find that exposure over ages 22-39 to a $1,000 higher maximum credit boosts years of employment by 1.49 years, and exposure to this higher maximum credit for one year adds a bit more than 0.07 years of employment for women with older (only) children. We can compare these estimated cumulative experience effects for unmarried mothers to the estimated wage and earnings effects from Table 3. For example, the permanently higher maximum credit was estimated to increase hourly wages by about 6.9 percent. If the return to experience is about 3 percent, then this is a bit higher than the implied wage effect (which is not estimated too precisely, and indeed is not significant); but it is in the ballpark. And as we noted, greater labor force attachment over the longer-run may have other effects on wages and earnings that are not necessarily reflected in cumulative experience.

The cumulative hours estimates in column (2) are of the same sign for unmarried mothers, but smaller – and also less precise – which is not surprising given the hours variation. The fact that we do not find larger effects for hours suggests that the response of unmarried mothers is on the extensive margin of employment. In contrast, for married mothers the long-run exposure effects now become more consistently larger and negative, consistent with intensive-margin adjustments, although the estimated effects for married mothers are not statistically significant.

Finally, columns (3) and (4) report the same estimates where, as in Table 8, we add data on more-educated women as additional controls, and estimate effects relative to them. The estimates are qualitatively similar, with the results we just discussed for unmarried women becoming a bit stronger.[51]

V. 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 indicating that exposure to a more generous EITC when women were unmarried and

[51] 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. As before, this serves as a placebo test.

had older (school-age) children leads to higher earnings in the longer-run. We also find corresponding evidence 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 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.

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.

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Figure 1: Federal EITC (2016)

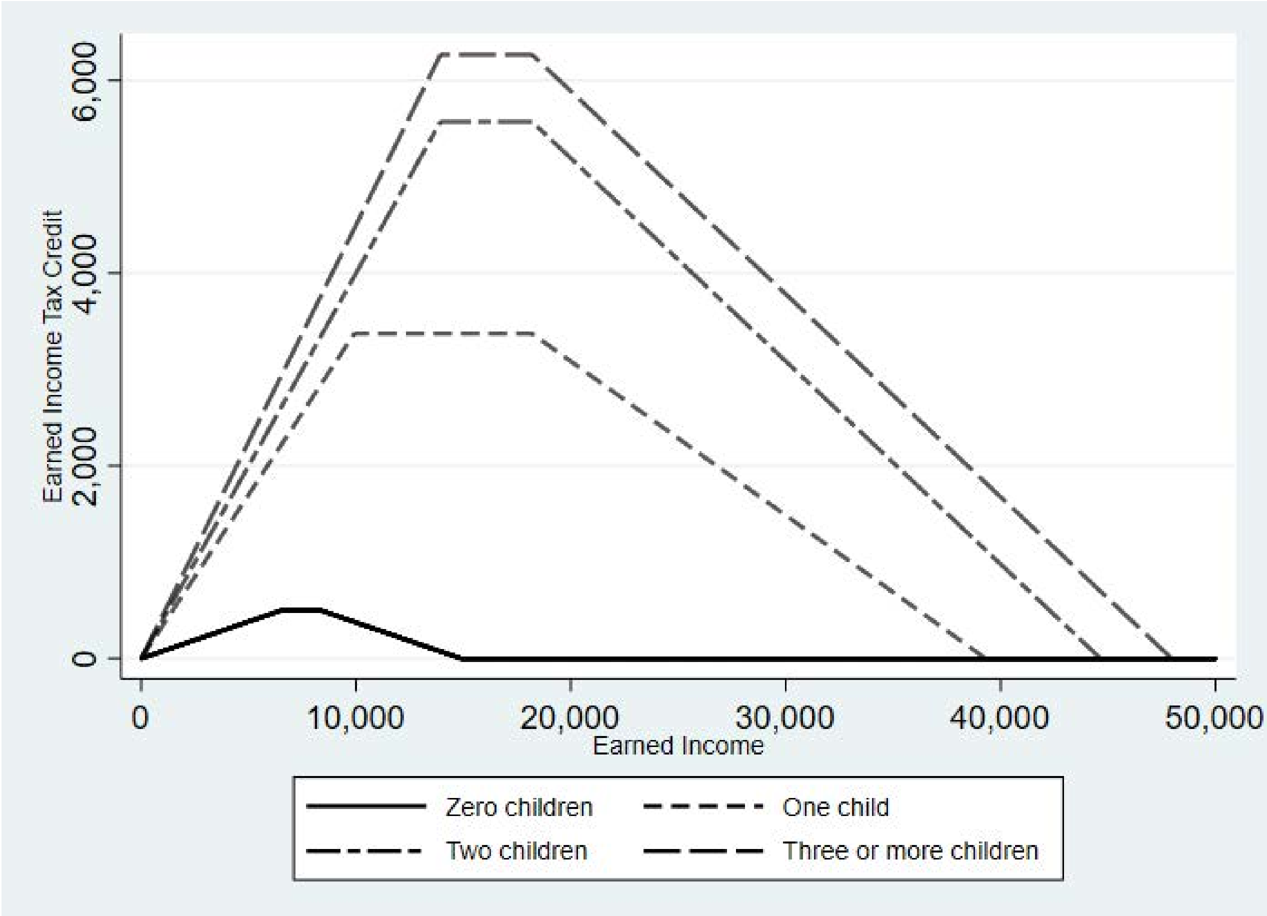


Figure 2: Federal EITC Maximum Credit

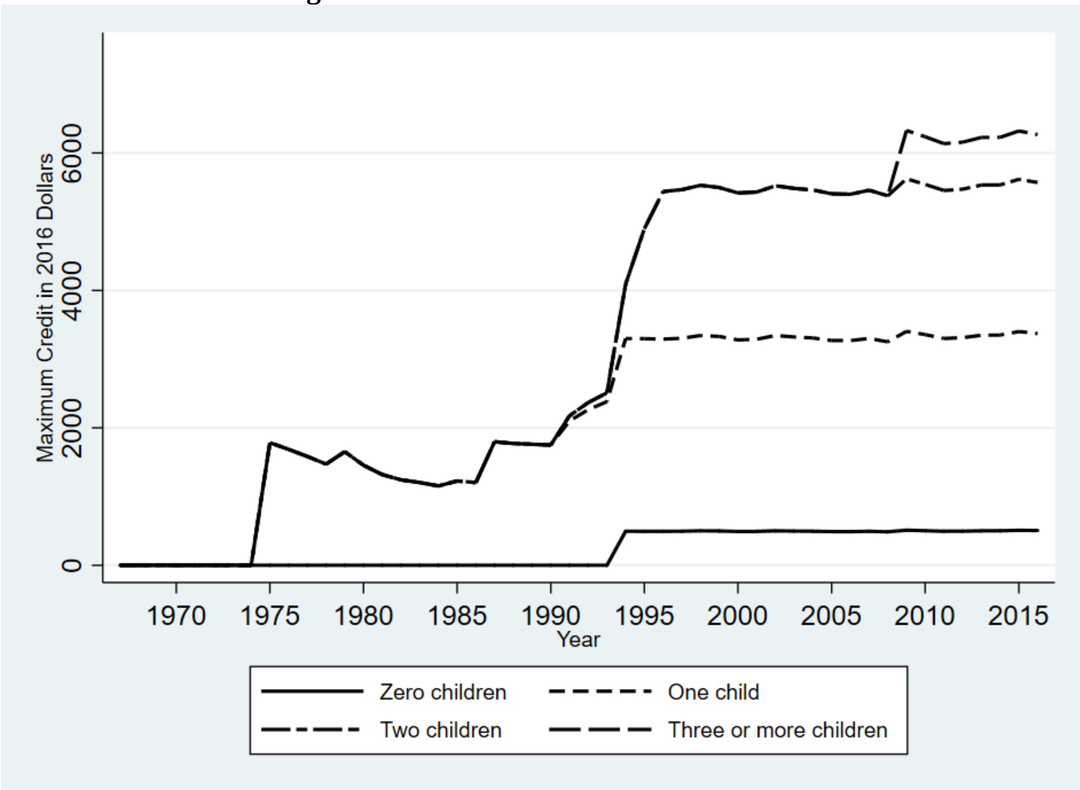


Figure 3: State EITC Supplements (%)

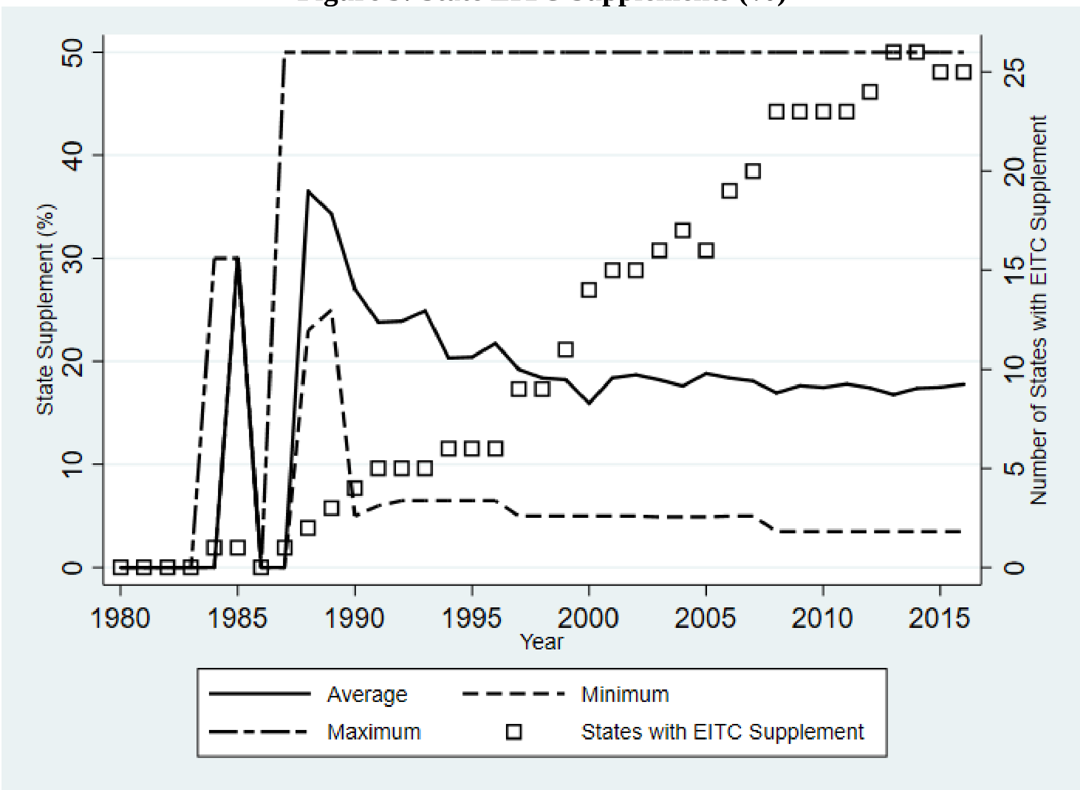
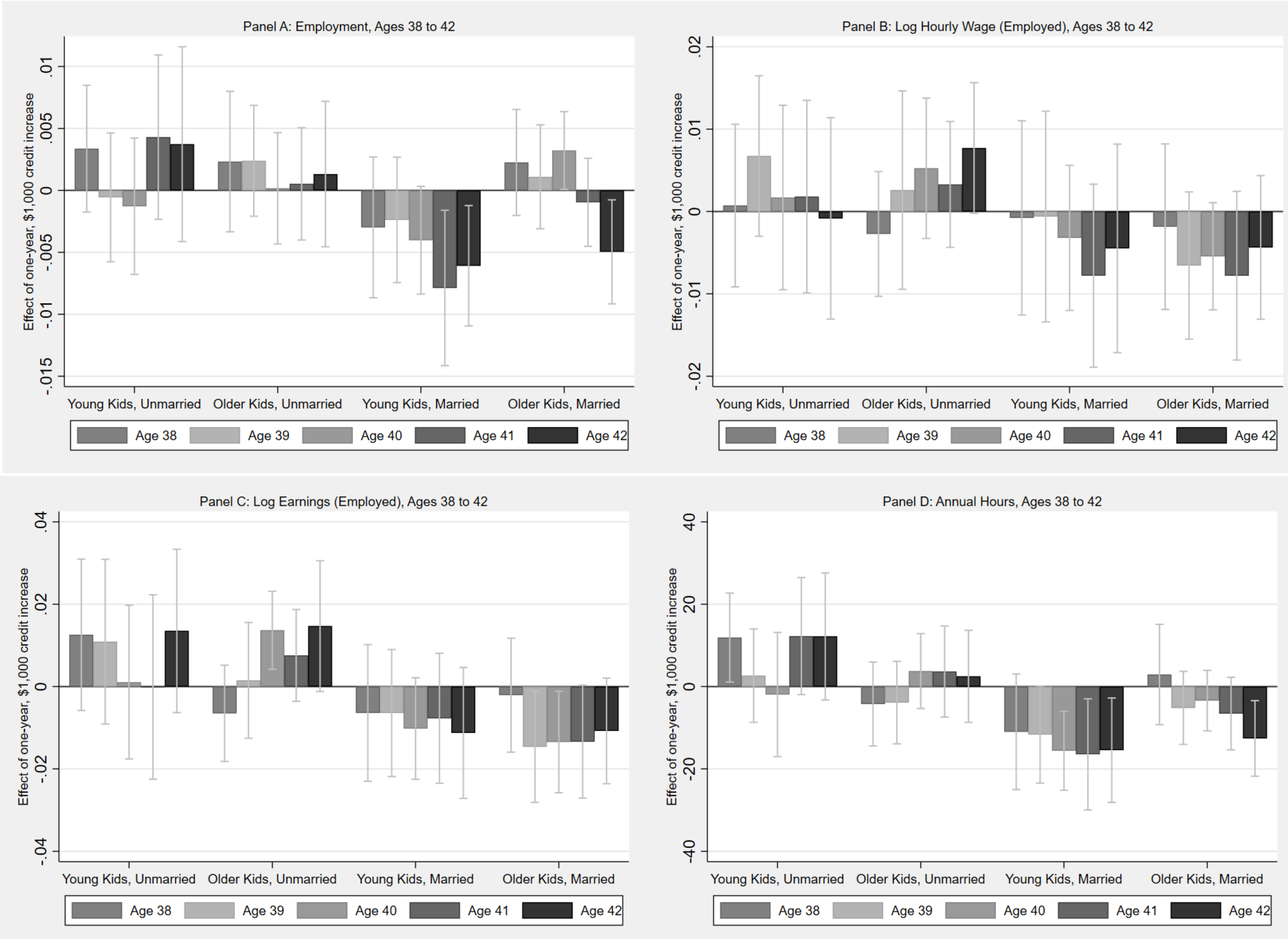


Figure 4: Estimated Effects of Long-Run Exposure to the EITC by Age, Baseline Specification, Ages 38-42



Estimates correspond to Table 3, using different ages at which to measure longer-run outcomes. We create a separate sample at each age using the same approach described in the data section for the age 40 sample, but for ages 38, 39, 41, and 42. The age 40 estimates match Table 3. 95-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-Term Analysis (Means)

Ages	22-39	40
Calendar year at age 40	N/A	1998
Federal EITC two-child maximum credit	3.15	4.03
State EITC two-child maximum credit	0.10	0.22
Combined EITC two-child maximum credit	3.25	4.25
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
Black	N/A	0.41
Experience (cumulative years employed)	13.16	0.78
Annual hours at age 40	N/A	1,368
Log wage (employed) at age 40	N/A	2.51
Log earnings (employed) at age 40	N/A	9.82

Descriptive statistics are for the low-education sample (Row F, Table 1). The maximum credit is measured in $1,000s (indexed to 2016). It is the combined federal plus state EITC. We define employment as having positive earnings in the previous year. (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	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
A. Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	-0.023 (0.059)	0.031 (0.120)	0.020 (0.200)	-34.90 (161.68)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	0.003 (0.048)	0.094 (0.091)	0.246** (0.102)	67.79 (97.39)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.072 (0.047)	-0.058 (0.094)	-0.183 (0.132)	-280.46*** (102.82)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.058* (0.034)	-0.098 (0.070)	-0.242* (0.132)	-61.36 (78.75)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	-0.009 (0.047)	0.066 (0.067)	0.145 (0.115)	22.15 (113.02)
Always married with children at ages 22 and 24	-0.000003 (0.031)	-0.080 (0.074)	-0.216* (0.125)	-158.74** (72.32)
Unmarried – married	-0.009 (0.059)	0.146 (0.105)	0.361* (0.209)	180.89 (160.92)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	-0.001	0.002	0.001	-1.94
Unmarried with older children	0.0002	0.005	0.014**	3.77
Married with young children	-0.004	-0.003	-0.010	-15.58***
Married with older children	0.003*	-0.005	-0.013*	-3.41
Unmarried – married (young children)	0.003 (0.004)	0.005 (0.009)	0.011 (0.016)	13.64 (11.15)
Unmarried – married (older children)	-0.003 (0.003)	0.011 (0.006)	0.027** (0.010)	7.18 (8.47)
R^2	0.09	0.16	0.16	0.09
N	1,505	1,176	1,177	1,505

See notes to Table 2. These results are based on equations (4), (5), and (5'). The calculations in panels B and C are based on equations (6) and (6'). Other controls include:
(1) averages of two-way interactions between the EITC variable, dummy variables for marital status, and dummy variables for young and older 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 dummy variables for young and older 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 Women's Fertility and Marital Status from Ages 22 to 40, Using Combined Federal and State EITC Two-Child Maximum Credit, Treating Childbearing as Potentially Endogenous Conditional on Childbearing

	Fraction of years with any kids	Fraction of years with young kids	Fraction of years with older (only) kids	Fraction of years married
	(1)	(2)	(3)	(4)
I. Treating Childbearing as Potentially Endogenous, Conditional on Marital Status				
A. Coefficient estimates (δ^{UY} , δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **unmarried**), 22-39	-0.107* (0.061)	-0.020 (0.038)	-0.087** (0.035)	…
Avg. (two-child maximum credit × **married**), 22-39	-0.174*** (0.062)	-0.046 (0.036)	-0.128*** (0.036)	…
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Unmarried – married	0.066*** (0.016)	0.026*** (0.009)	0.041*** (0.013)	…
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried	-0.006*	-0.001	-0.005**	…
Married	-0.010***	-0.003	-0.007***	…
Unmarried – married	0.004*** (0.001)	0.001*** (0.001)	0.002*** (0.001)	…
II. Treating Marital Status as Potentially Endogenous, Conditional on Childbearing				
A. Coefficient estimates (δ^{UY} , δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children**), 22-39	…	…	…	-0.050* (0.027)
Avg. (two-child maximum credit × **older children**), 22-39	…	…	…	-0.009 (0.024)
B. Implied effect of one year, $1,000 increase in maximum credit (%)				
With young children	…	…	…	-0.003*
With older children	…	…	…	-0.001
R^2	0.17	0.13	0.17	0.36
N, low-education	1,505	1,505	1,505	1,505

See notes to Tables 2 and 3, and modifications of equation (4) explained in the text.

Table 5: 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	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
I. Fixing State at Age 22				
A. Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	0.005 (0.076)	-0.019 (0.146)	-0.057 (0.229)	-12.94 (207.47)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	0.004 (0.057)	0.110 (0.099)	0.273** (0.118)	66.31 (116.16)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.076 (0.055)	-0.042 (0.124)	-0.163 (0.167)	-282.46** (139.99)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.051 (0.039)	-0.083 (0.086)	-0.239 (0.155)	-81.00 (89.97)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	0.005 (0.059)	0.053 (0.081)	0.126 (0.137)	31.09 (140.58)
Always married with children at ages 22 and 24	-0.005 (0.036)	-0.065 (0.092)	-0.205 (0.146)	-170.54* (90.43)
Unmarried – married	0.010 (0.072)	0.117 (0.127)	0.332 (0.248)	201.63 (195.01)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	0.0003	-0.001	-0.003	-0.72
Unmarried with older children	0.0002	0.006	0.015	3.68
Married with young children	-0.004	-0.002	-0.009	-15.69**
Married with older children	0.003	-0.005	-0.013	-4.50
Unmarried – married (young children)	0.005 (0.005)	0.001 (0.011)	0.006 (0.018)	14.97 (13.71)
Unmarried – married (older children)	-0.003 (0.004)	0.011 (0.008)	0.028 (0.012)	8.18 (10.00)
R^2	0.08	0.16	0.16	0.08
II. Using Only Federal EITC Variation				
A. Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	-0.032 (0.069)	0.013 (0.140)	0.003 (0.222)	-44.40 (200.56)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	0.003 (0.051)	0.118 (0.105)	0.274** (0.115)	54.42 (105.30)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.071 (0.053)	-0.061 (0.114)	-0.169 (0.160)	-267.45** (113.77)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.070* (0.037)	-0.111 (0.085)	-0.263 (0.164)	-49.93 (90.07)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	-0.012 (0.052)	0.071 (0.072)	0.154 (0.125)	10.50 (130.59)
Always married with children at ages 22 and 24	0.007 (0.035)	-0.089 (0.091)	-0.222 (0.155)	-146.61* (80.53)
Unmarried – married	-0.020 (0.062)	0.160 (0.122)	0.375 (0.244)	157.11 (180.87)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	-0.002	0.001	0.0002	-2.47
Unmarried with older children	0.0002	0.007	0.015**	3.02
Married with young children	-0.004	-0.003	-0.009	-14.86**
Married with older children	0.004*	-0.006	-0.015	-2.77
Unmarried – married (young children)	0.002 (0.004)	0.004 (0.011)	0.010 (0.019)	12.39 (13.14)
Unmarried – married (older children)	-0.004 (0.004)	0.013* (0.007)	0.030** (0.012)	5.80 (9.32)
R^2	0.09	0.16	0.16	0.08
N	1,505	1,176	1,177	1,505

See notes to Tables 2 and 3. Only the definitions of the EITC variables differ, as explained in the headings of parts I and II.

Table 6: 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, Including High-Education Women as an Additional Level of Differencing

	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
A. Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39) × low-ed	0.052 (0.068)	0.090 (0.150)	0.171 (0.189)	196.62 (195.12)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39) × low-ed	0.051 (0.053)	0.005 (0.106)	0.156 (0.143)	199.77 (121.40)
Avg. (two-child maximum credit × **young children** × **married**, 22-39) × low-ed	-0.001 (0.052)	-0.104 (0.095)	-0.334* (0.172)	-210.59 (126.37)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39) × low-ed	0.101** (0.039)	-0.091 (0.071)	-0.341** (0.152)	-23.16 (89.77)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	0.051 (0.049)	0.043 (0.077)	0.163 (0.107)	198.37 (122.07)
Always married with children at ages 22 and 24	0.056 (0.036)	-0.097 (0.069)	-0.338** (0.149)	-106.46 (88.74)
Unmarried – married	-0.004 (0.069)	0.139 (0.116)	0.501** (0.226)	304.83* (174.43)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	0.003	0.005	0.009	10.92
Unmarried with older children	0.003	0.0003	0.009	11.10
Married with young children	-0.00005	-0.006	-0.019*	-11.70
Married with older children	0.006**	-0.005	-0.019**	-1.29
Unmarried – married (young children)	0.003 (0.004)	0.011 (0.011)	0.028 (0.017)	22.62 (13.52)
Unmarried – married (older children)	-0.003 (0.004)	0.005 (0.008)	0.028** (0.013)	12.39 (10.08)
R^2	0.08	0.22	0.16	0.11
N	3,358	2,757	2,760	3,358

See notes to Tables 2 and 3. The difference is that high-education women are added to the sample, and variables are entered on their own, and interacted with a low-education dummy variable (as explained in the text). The estimates in the table are based on the latter interactions. (The main effect of low education is also included.)

Table 7: 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, Including State-Specific Linear Trends

	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
A. Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	-0.047 (0.065)	0.011 (0.135)	0.036 (0.209)	-68.38 (178.41)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	0.006 (0.054)	0.080 (0.109)	0.207 (0.127)	61.44 (109.10)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.088 (0.055)	-0.019 (0.109)	-0.126 (0.141)	-328.24*** (111.02)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.057 (0.037)	-0.064 (0.077)	-0.241* (0.139)	-83.69 (89.42)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	-0.018 (0.052)	0.050 (0.081)	0.131 (0.128)	3.74 (126.27)
Always married with children at ages 22 and 24	-0.007 (0.037)	-0.044 (0.085)	-0.190 (0.132)	-192.38** (81.39)
Unmarried – married	-0.010 (0.065)	0.093 (0.127)	0.321 (0.225)	196.12 (175.83)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	-0.003	0.001	0.002	-3.80
Unmarried with older children	0.0003	0.004	0.012	3.41
Married with young children	-0.005	-0.001	-0.007	-18.24***
Married with older children	0.003	-0.004	-0.013*	-4.65
Unmarried – married (young children)	0.002 (0.004)	0.002 (0.011)	0.009 (0.017)	14.44 (12.08)
Unmarried – married (older children)	-0.003 (0.004)	0.008 (0.007)	0.025** (0.011)	8.06 (9.32)
R^2	0.11	0.18	0.20	0.11
N	1,505	1,176	1,177	1,505

See notes to Tables 2 and 3. The difference is the inclusion of state-specific linear trends.

Table 8: 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	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
I. Including Parametric Welfare Reform Control				
A. Coefficient estimates (δ^{UY} , δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	-0.074 (0.072)	0.060 (0.155)	-0.028 (0.275)	-142.59 (181.30)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	0.035 (0.057)	0.071 (0.096)	0.286** (0.131)	178.37 (117.65)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.052 (0.050)	-0.064 (0.098)	-0.194 (0.147)	-254.08** (111.37)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.069* (0.037)	-0.105 (0.066)	-0.230* (0.127)	-28.75 (79.11)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	-0.013 (0.055)	0.066 (0.091)	0.146 (0.167)	35.72 (131.77)
Always married with children at ages 22 and 24	0.015 (0.033)	-0.087 (0.070)	-0.214* (0.124)	-128.90* (75.50)
Unmarried – married	-0.029 (0.065)	0.153 (0.117)	0.360 (0.255)	164.62 (177.39)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	-0.004	0.003	-0.002	-7.92
Unmarried with older children	0.002	0.004	0.016**	9.91
Married with young children	-0.003	-0.004	-0.011	-14.12
Married with older children	0.004*	-0.006	-0.013*	-1.60
Unmarried – married (young children)	-0.001 (0.004)	0.007 (0.011)	0.009 (0.020)	6.19 (12.18)
Unmarried – married (older children)	-0.002 (0.004)	0.010 (0.006)	0.029** (0.012)	11.51 (9.62)
R^2	0.10	0.18	0.18	0.10
II. Including Real Minimum Wage Control				
A. Coefficient estimates (δ^{UY} , δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	-0.049 (0.060)	0.144 (0.135)	0.067 (0.227)	-166.33 (169.49)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	0.0002 (0.058)	0.148 (0.133)	0.254* (0.134)	15.66 (115.31)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.071 (0.051)	-0.067 (0.098)	-0.212 (0.141)	-292.49*** (103.14)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.078 (0.047)	-0.153* (0.085)	-0.306* (0.179)	-61.67 (117.23)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	-0.022 (0.052)	0.146 (0.094)	0.171 (0.130)	-65.23 (118.92)
Always married with children at ages 22 and 24	0.012 (0.039)	-0.115 (0.081)	-0.264* (0.151)	-164.26* (89.26)
Unmarried – married	-0.033 (0.064)	0.261** (0.127)	0.435* (0.243)	99.03 (175.39)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	-0.003	0.008	0.004	-9.24
Unmarried with older children	0.0001	0.008	0.014*	0.87
Married with young children	-0.004	-0.004	-0.012	-16.25***
Married with older children	0.004	-0.009	-0.017*	-3.43
Unmarried – married (young children)	0.001 (0.004)	0.012 (0.010)	0.016 (0.018)	7.01 (11.61)
Unmarried – married (older children)	-0.004 (0.004)	0.017* (0.009)	0.031** (0.013)	4.30 (10.01)
R^2	0.09	0.16	0.16	0.09
N	1,505	1,176	1,177	1,505

See notes to Tables 2 and 3. The modifications to the equations estimated are explained in the text.

Table 9: Long-Run Effects of EITC on Less-Educated Women's Cumulative Experience at Age 40, Using Combined Federal and State EITC Two-Child Maximum Credit

	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
	Baseline analysis		Including high-education women as an additional level of differencing	
	(1)	(2)	(3)	(4)
A. Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39) × low-ed	1.853** (0.755)	1.764** (0.844)	1.633** (0.787)	1.383 (1.034)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39) × low-ed	1.102** (0.459)	0.403 (0.690)	1.438** (0.558)	1.216 (0.870)
Avg. (two-child maximum credit × **young children** × **married**, 22-39) × low-ed	0.123 (0.590)	-0.051 (0.667)	0.033 (0.718)	0.055 (0.811)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39) × low-ed	-0.187 (0.480)	-0.611 (0.563)	-0.170 (0.638)	-0.206 (0.872)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	1.436*** (0.398)	1.008 (0.600)	1.525*** (0.473)	1.290* (0.649)
Always married with children at ages 22 and 24	-0.050 (0.477)	-0.362 (0.518)	-0.080 (0.592)	-0.090 (0.713)
Unmarried – married	1.485*** (0.535)	1.370* (0.749)	1.605** (0.711)	1.380 (0.902)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	0.103**	0.098**	0.091**	0.077
Unmarried with older children	0.061**	0.022	0.080**	0.068
Married with young children	0.007	-0.003	0.002	0.003
Married with older children	-0.010	-0.034	-0.009	-0.011
Unmarried – married (young children)	0.096* (0.049)	0.101* (0.054)	0.089* (0.052)	0.074 (0.052)
Unmarried – married (older children)	0.072** (0.030)	0.056 (0.046)	0.089* (0.045)	0.079 (0.071)
R^2	0.16	0.20	0.21	0.27
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 3.

Appendix A: 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.[52] 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 A1 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 A2, they find clear evidence that the difference in employment rates – with much lower

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

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

Robustness and Sensitivity Analysis

For all the results presented in the main text, we have used the two-child maximum credit to capture the generosity of the EITC. In Appendix Table A4, we instead use the two-child phase-in rate; this is the slope of the upward-sloping graph (for two children).[53] We use a policy simulation that amounts to about the same percentage increase in EITC generosity as the $1,000 increase in the maximum credit we used in the preceding tables – in this case, a 7.5-percentage point increase in the phase-in rate.[54] Note, though, that the regression coefficients in panel A correspond to a one-unit increase in the phase-in rate (i.e., 100 percentage points). Hence these are divided by 13.33 (1/0.075) before doing the calculations in panels B and C. Comparing the results to those in Table 3 shows that the estimates and conclusions are very similar; in fact, the differences we estimate are in some cases more strongly significant using the phase-in rate.

We next 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 A5

[53] Appendix Table A3 was discussed in the main text.

[54] These are approximately equal in relative terms. We have been using an increase of $1,000 2016 dollars. A 0.075 phase-in (7.5 percentage points) rate increase is a 37.1 percent increase in the two-child EITC phase-in rate, based on a weighted average of observations in our sample. The equivalent percentage increase in the two-child EITC maximum credit is very close to $1,000.

reports results for the baseline specification and sample when we weight by the PSID Core sample weights for the age 40 observations. With the weights, 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, and for the unmarried vs. married differences, are frequently significant (with the latter pointing to higher employment and higher hours for exposure when children are young, and higher earnings for exposure when children are school-age). While the exact estimates clearly are sensitive to weighting, we view Appendix Table A5 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 A6. 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 (a result we discuss in the main text).

Appendix Table A1: 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 A2: 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 A1. 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 A3: Estimated Effects for More-Educated Women Corresponding to Table 6's Analysis of 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, Including More-Educated Women as an Additional Level of Differencing (Placebo Test)

	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
A. Coefficient estimates (δ^{UY} , δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	-0.070 (0.042)	-0.051 (0.099)	-0.126 (0.123)	-212.55* (121.09)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	-0.047* (0.028)	0.072 (0.062)	0.068 (0.092)	-125.06 (84.85)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.075** (0.028)	0.060 (0.059)	0.176 (0.107)	-66.92 (73.01)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	-0.045* (0.026)	0.009 (0.052)	0.111 (0.089)	-39.48 (53.18)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	-0.058*** (0.017)	0.017 (0.050)	-0.018 (0.063)	-163.94** (68.72)
Always married with children at ages 22 and 24	-0.059*** (0.021)	0.032 (0.045)	0.140 (0.084)	-51.67 (49.54)
Unmarried – married	0.001 (0.026)	-0.015 (0.075)	-0.158 (0.104)	-112.27 (84.54)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	-0.004	-0.003	-0.007	-11.81*
Unmarried with older children	-0.003*	0.004	0.004	6.95
Married with young children	-0.004**	0.003	0.010	-3.72
Married with older children	-0.003*	0.001	0.006	-2.19
Unmarried – married (young children)	0.0003 (0.002)	-0.006 (0.006)	-0.017* (0.008)	-8.09 (7.41)
Unmarried – married (older children)	-0.0001 (0.002)	0.003 (0.005)	-0.002 (0.007)	-4.75 (5.54)
R^2	0.08	0.22	0.16	0.11
N	3,358	2,757	2,760	3,358

See notes to Tables 2 and 3. The difference 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 this table – in contrast to Table 5 – are for the high-educated women.

Appendix Table A4: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined Federal EITC Two-Child Phase-in Rate as Policy Variation

	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
A. Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child phase-in rate × **young children** × **unmarried**, 22-39)	-0.164 (0.786)	0.357 (1.671)	0.712 (2.779)	147.59 (2300.39)
Avg. (two-child phase-in rate × **older (only) children** × **unmarried**, 22-39)	-0.010 (0.596)	1.320 (1.291)	3.518** (1.421)	935.03 (1179.69)
Avg. (two-child phase-in rate × **young children** × **married**, 22-39)	-1.100* (0.632)	-0.849 (1.252)	-2.487 (1.718)	-3995.68*** (1419.29)
Avg. (two-child phase-in rate × **older (only) children** × **married**, 22-39)	0.709 (0.435)	-1.328 (0.895)	-3.460** (1.702)	-907.34 (1040.39)
B. Implied effect of 10 percentage-point increase in phase-in rate, 22-39 (%)				
Always unmarried with children at ages 22 and 24	-0.006 (0.045)	0.067 (0.063)	0.170 (0.113)	43.88 (112.11)
Always married with children at ages 22 and 24	-0.007 (0.030)	-0.084 (0.071)	-0.227* (0.118)	-171.00** (71.40)
Unmarried – married	0.001 (0.055)	0.151 (0.092)	0.397** (0.193)	214.87 (155.47)
C. Implied effect of one year, 10 percentage-point increase in phase-in rate (%)				
Unmarried with young children	-0.001	0.001	0.003	0.61
Unmarried with older children	-0.0004	0.005	0.015**	3.90
Married with young children	-0.005*	-0.004	-0.010	-16.65***
Married with older children	0.003	-0.006	-0.014**	-3.78
Unmarried – married (young children)	0.004 (0.004)	0.005 (0.009)	0.013 (0.016)	17.26 (11.46)
Unmarried – married (older children)	-0.003 (0.003)	0.011* (0.006)	0.029*** (0.010)	7.68 (7.74)
R^2	0.09	0.16	0.16	0.09
N	1,505	1,176	1,177	1,505

See notes to Tables 2 and 3. The difference is that the two-child phase-in rate is used, instead of the two-child maximum credit. Note that the regression coefficients in panel A correspond to a one-unit increase in the phase-in rate (i.e., 100 percentage points). Hence these are divided by 13.33 (1/0.075) before doing the calculations in panels B and C – which are otherwise that same as the calculations when we use the maximum credit.

Appendix Table A5: 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, Weighted

	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
A. Coefficient estimates (δ^{UY}, δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	0.135 (0.081)	0.150 (0.174)	0.120 (0.211)	233.73 (212.13)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	0.009 (0.069)	-0.025 (0.097)	0.158 (0.111)	130.09 (136.04)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.121** (0.057)	-0.079 (0.127)	-0.248 (0.152)	-445.44*** (116.70)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.007 (0.050)	-0.133 (0.100)	-0.300* (0.165)	-170.32* (94.44)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	0.065 (0.064)	0.053 (0.090)	0.141 (0.108)	176.15 (143.77)
Always married with children at ages 22 and 24	-0.050 (0.044)	-0.109 (0.102)	-0.277* (0.146)	-292.60*** (84.47)
Unmarried – married	0.115 (0.074)	0.162 (0.151)	0.418* (0.211)	468.75** (189.25)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	0.008	0.008	0.007	12.98
Unmarried with older children	0.0001	-0.001	0.009	7.23
Married with young children	-0.007**	-0.004	-0.014	-24.75***
Married with older children	0.0004	-0.007	-0.017*	-9.46*
Unmarried – married (young children)	0.014*** (0.005)	0.013 (0.012)	0.020 (0.015)	37.73*** (12.68)
Unmarried – married (older children)	0.0001 (0.005)	0.006 (0.009)	0.025* (0.013)	16.69 (11.47)
R^2	0.14	0.22	0.20	0.15
N	1,505	1,176	1,177	1,505

See notes to Tables 2 and 3. The difference is the estimates are weighted.

Appendix Table A6: Long-Run Effects of EITC on Less-Educated Women's Employment, Wages, Earnings, and Hours at Age 40, Separate Regressions by Race

	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
	(1)	(2)	(3)	(4)
I. Black Only				
A. Coefficient estimates (δ^{UY} , δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	-0.116 (0.095)	-0.089 (0.196)	0.063 (0.310)	-118.83 (187.84)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	-0.055 (0.059)	0.100 (0.126)	0.339* (0.186)	-20.07 (135.12)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	0.076 (0.170)	0.129 (0.197)	0.332 (0.384)	223.77 (399.44)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.155 (0.118)	0.137 (0.159)	0.150 (0.320)	255.74 (277.03)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	-0.082 (0.064)	0.016 (0.112)	0.216 (0.208)	-63.96 (136.66)
Always married with children at ages 22 and 24	0.120 (0.130)	0.134 (0.161)	0.230 (0.342)	241.53 (317.89)
Unmarried – married	-0.203 (0.149)	-0.118 (0.214)	-0.014 (0.408)	-305.49 (359.27)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	-0.006	-0.005	0.003	-6.60
Unmarried with older children	-0.003	0.006	0.019*	-1.11
Married with young children	0.004	0.007	0.018	12.43
Married with older children	0.009	0.008	0.008	14.21
Unmarried – married (young children)	-0.011 (0.009)	-0.012 (0.015)	-0.015 (0.028)	-19.03 (23.62)
Unmarried – married (older children)	-0.012 (0.009)	-0.002 (0.012)	0.011 (0.021)	-15.32 (18.60)
R^2	0.11	0.23	0.26	0.12
N	622	458	458	622
II. Non-Black Only				
A. Coefficient estimates (δ^{UY} , δ^{UO}, δ^{MY}, δ^{MO})				
Avg. (two-child maximum credit × **young children** × **unmarried**, 22-39)	0.131 (0.100)	0.148 (0.231)	0.147 (0.232)	177.92 (188.09)
Avg. (two-child maximum credit × **older (only) children** × **unmarried**, 22-39)	-0.037 (0.094)	0.040 (0.118)	0.218* (0.126)	-5.31 (183.35)
Avg. (two-child maximum credit × **young children** × **married**, 22-39)	-0.083 (0.066)	-0.035 (0.109)	-0.189 (0.142)	-339.37** (134.19)
Avg. (two-child maximum credit × **older (only) children** × **married**, 22-39)	0.045 (0.051)	-0.144* (0.085)	-0.245 (0.147)	-72.79 (100.56)
B. Implied effect of $1,000 increase in maximum credit, 22-39 (%)				
Always unmarried with children at ages 22 and 24	0.038 (0.075)	0.088 (0.108)	0.187 (0.122)	76.12 (140.73)
Always married with children at ages 22 and 24	-0.012 (0.048)	-0.096 (0.089)	-0.220 (0.132)	-191.27* (96.70)
Unmarried – married	0.050 (0.089)	0.184 (0.139)	0.407** (0.186)	267.40 (201.16)
C. Implied effect of one year, $1,000 increase in maximum credit (%)				
Unmarried with young children	0.007	0.008	0.009	9.88
Unmarried with older children	-0.002	0.002	0.012*	-0.30
Married with young children	-0.005	-0.002	-0.010	-18.85**
Married with older children	0.003	-0.008	-0.014	-4.04
Unmarried – married (young children)	0.012* (0.006)	0.010 (0.014)	0.019 (0.015)	28.74** (12.66)
Unmarried – married (older children)	-0.005 (0.006)	0.010 (0.009)	0.026** (0.012)	3.75 (14.03)
R^2	0.15	0.23	0.20	0.14
N	883	718	719	883

See notes to Table 2 and 3.