NBER WORKING PAPER SERIES

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

David Neumark
Peter Shirley

Working Paper 24114
http://www.nber.org/papers/w24114

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2017, Revised May 2018

We are grateful to the Laura and John Arnold Foundation and the Smith-Richardson Foundation for support for this research, through grants to the Economic Self-Sufficiency Policy Research Institute (ESSPRI) at UCI. We thank seminar participants at UCI and at Beijing Normal University for helpful comments. Any opinions or conclusions expressed are the authors' own and do not necessarily reflect those of the Laura and John Arnold Foundation or the Smith-Richardson Foundation. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

The Long-Run Effects of the Earned Income Tax Credit on Women's Earnings
David Neumark and Peter Shirley
NBER Working Paper No. 24114
December 2017, Revised May 2018
JEL No. H24,H71,J18,J22,J24

ABSTRACT

We use longitudinal data on marriage and children from the Panel Study of Income Dynamics to characterize women's exposure to the federal and state Earned Income Tax Credit (EITC) during their first two decades of adulthood. We then use measures of this exposure to estimate the long-run effects of the EITC on women's labor market outcomes – especially wages and earnings – as mature adults. We find evidence indicating that exposure to a more generous EITC when women were unmarried and had young (pre-school) children leads to higher earnings and hours, and perhaps wages, in the longer run. We also find evidence that exposure to a more generous EITC when women had young children but were married leads to lower earnings and hours in the longer run. These 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 effects of the EITC on cumulative labor market experience (and other consequences of labor market attachment) that influence earnings.

David Neumark
Department of Economics
University of California, Irvine
3151 Social Science Plaza
Irvine, CA 92697
and NBER
dneumark@uci.edu

Peter Shirley
University of California at Irvine
3151 Social Science Plaza
Irvine, CA 92697
pshirley@uci.edu

I. Introduction

The extensive literature on the Earned Income Tax Credit (EITC) has focused nearly exclusively on short-term effects on employment (e.g., Meyer, 2010). The evidence from this literature establishes that a more generous EITC – using both federal and state variation – increases employment of less-educated, single mothers – who are in important target of the program – and via work incentives, reduces poverty 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. Bolstering this case, some research points to beneficial longer-run effects of the EITC on infant health (Hoynes et al., 2015) and mothers' health (Evans and Garthwaite, 2014).[1]

However, this evidence – and the evidence on labor market effects in particular – ignores a potential longer-run benefit of the EITC. Specifically, the positive employment effects should lead to greater labor market experience in the longer run, boosting earnings via greater human capital accumulation; other types of investment, including more intensive search for better paying jobs with growth prospects, could also be spurred by a more generous EITC that has positive and persistent effects on employment. 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 comparing women with two children to women with one child – because the mid-1990s EITC expansion increased the relative generosity of the federal EITC for women with two or more children – and find some evidence of positive effects on earnings growth.[2,3] However, our study takes a *much* longer-run perspective.

We use longitudinal data on marriage and children from the Panel Study of Income Dynamics to

[1] For a review of related work, see Neumark (2016).
[2] The difference in the phase-in rate expanded, as did the difference in the maximum credit.
[3] Card and Hyslop (2005) study longer-term effects of similar program in Canada (the Self-Sufficiency Project, or SSP). They found that the SSP program in Canada created short-term positive work incentives, but no long-run impact on wages or welfare participation.

characterize women's exposure to the federal and state Earned Income Tax Credit (EITC) from ages 22-39 – corresponding roughly to their first two decades of adulthood, and covering most of the period when women bear children as well as a large share of the period when they raise children.[4] We then use measures of this exposure to estimate the long-run effects of the EITC on women's earnings as mature adults (age 40). We find evidence indicating that exposure to a more generous EITC when women were unmarried and had young (pre-school) children leads to higher earnings and hours, and perhaps wages, in the longer run. We also find some evidence that exposure to a more generous EITC when women had young children but were married leads to lower earnings and hours in the longer run.

The same question about longer-run effects can be asked of other policies, such as the minimum wage or welfare. Neumark and Nizalova (2007) estimated the effect of exposure to a higher minimum wage as a teenager on outcomes in their late 20s, finding some adverse effects. Neumark et al. (in progress) estimate the longer-run effects of all three types of policies (including welfare reform) – albeit with a focus on initially disadvantaged areas, rather than individuals. Studying the effects of the EITC over longer periods is made complicated by the fact that EITC benefits depend on a woman's childbearing history, marital history (because of spouses' incomes), and state of residence.[5,6] The need to observe women over a significant portion of their lives necessitates our use of the Panel Study of Income Dynamics (PSID).

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

To motivate our strategy for estimating longer-run effects, it is instructive to first consider the

---

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

[5] Technically, the EITC may depend on the state of work if they commute across a state border and the bordering states do not have a tax reciprocity agreement.

[6] Neumark and Nizalova study the effects of the minimum wage experienced as a teen, using Current Population Survey (CPS). They only observe the current state of residence and assume no mobility. Given state-level policy variation, it is clearly preferable to observe state of residence when exposed to the policy.

simpler problem of estimating the effect of the EITC on contemporaneous outcomes – like the analysis of employment effects performed in several papers (e.g., Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001). Our longer-run estimation strategy is an extension of this approach.

Define log earnings of person *i*, in state *j*, in period *t*, as Y_{ijt}.[7] We estimate the effects of the EITC phase-in rate. Although we could use other EITC parameters (like the maximum credit), higher phase-in rates create unambiguous incentives for single mothers to work, and, as a result, the phase-in rate captures the EITC parameter most relevant to extensive margin effects.[8] These extensive margin effects are not predicted for all EITC-eligible women. Women who are second earners, including many married women, may have predicted negative intensive margin employment effects, depending on the model of labor supply (e.g., Eissa and Hoynes, 2004).

For simplicity, suppose there is a single phase-in rate for women with children and that the phase-in rate for women without children is zero. (In our empirical work we impose this on the data for simplicity; we use the phase-in rate for families with two children and distinguish women by whether they have children.) Denote this phase-in rate CR_{jt} (*CR* stands for "credit") and denote by K_{ijt} a dummy variable for whether women have children. Define state dummy variables as D_j and year dummy variables as D_t. Suppose we are studying low-skilled unmarried women for whom the EITC is predicted to increase employment (ignoring, for now, the potential for quite different effects on married women). Then a simple difference-in-difference-in-differences (DDD) specification for estimating the effect of the EITC on *Y* is:

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

In equation (1), δ captures the effect of the EITC on *Y* for low-skilled, unmarried women with children. *K* and *CR* serve as controls, with γ capturing the effect of children independent of the EITC, and β capturing shocks or other unobservables that vary by state and year that are correlated with variation in both the EITC and *Y*, for all women including those not affected by the EITC. A more flexible way to

[7] We consider other outcomes as well (log wages, employment, hours, and cumulative employment).
[8] Nonetheless, we obtain robust findings using the maximum EITC credit instead.

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]

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

Strictly speaking, δ captures the effect of the EITC only if there is no EITC for childless women; in fact, there is a very modest EITC for families without children for much of the sample period.[10] However, because the childless EITC is worth very little, we believe it can be safely ignored and δ will still effectively capture the effect of the EITC, with β capturing common shocks.[11]

Note also that we ignore differences between women based on number of children, in contrast to, e.g., Dahl et al. (2009), who identify effects of the EITC from differences between outcomes for women with one child or two or more children. We ignore number of children because the difference between the one and two child phase-in rates are much smaller than the difference between the zero and one child rates. Furthermore, the gap between the zero and one child rates becomes more pronounced than the one-

[9] This greater parsimony becomes valuable given the data set we use, which does not lead to large samples.
[10] Technically, "without children" means they do not have eligible children living in the home for more than six months of a tax year who they claim as a dependent (or a dependent child born into the household during the tax year, who is young enough that they could not have lived in the home for six months).
[11] It is possible that the relative and absolute effects on women with children differ if the EITC worsens outcomes for low-skilled, unmarried women without children, because the outward labor supply shift from those with children can lower market wages and hence reduce labor supply of women who get no (or meager) benefits (Leigh, 2000). There is some evidence of adverse effects of the EITC on wages and employment of low-skilled childless individuals, and female teenagers (Neumark and Wascher, 2011). Thus, the beneficial longer-run effects of the EITC we estimate may somewhat overstate the absolute beneficial effects, although we have no evidence of adverse longer-run effects on other groups. And, as in the shorter-run literature, we are reluctant to interpret β in equation (1) as capturing EITC effects, rather than shocks common to women with and without children that are correlated with the EITC.

to two-child gap over the sample period. Because we focus on the phase-in rate to capture extensive margin effects, we focus only on whether the woman or family has children eligible for the EITC.

We can expand equation (1) to introduce married women into the sample, allowing separate effects for married (M) and unmarried (U) women. This gives us two DDD estimators – one for unmarried women, and one for married women, as in:

$$\text{(2) } Y_{ijt} = \alpha + \beta^U CR_{jt} \cdot U_{ijt} + \gamma^U K_{ijt} \cdot U_{ijt} + \delta^U CR_{jt} \cdot K_{ijt} \cdot U_{ijt}$$
$$+ \beta^M CR_{jt} \cdot M_{ijt} + \gamma^M K_{ijt} \cdot M_{ijt} + \delta^M CR_{jt} \cdot K_{ijt} \cdot M_{ijt} + \omega M_{ijt} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .$$ [12]

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

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

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

Finally, we can add more highly-educated women to the sample, assume they are not affected by the EITC, and use them to provide an additional level of differencing (a fourth difference, in this case).

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

This estimator allows us to relax the assumption that there cannot be shocks that vary by state, year, and children, if we are willing to assume that the state-by-year-by-children shocks are similar across women of different skill levels.[13] Thus, this specification provides our most compelling identification. Denoting low education by *LE*, our specification with these distinctions becomes:

$$
\begin{aligned}
(4)\ Y_{ijt} = {} & \alpha + \beta^{U} CR_{jt} \cdot U_{ijt} \cdot LE_{ij} + \gamma^{UY} YK_{ijt} \cdot U_{ijt} \cdot LE_{ij} + \gamma^{UO} OK_{ijt} \cdot U_{ijt} \cdot LE_{ij} \\
& + \delta^{UY} CR_{jt} \cdot YK_{ijt} \cdot U_{ijt} \cdot LE_{ij} + \delta^{UO} CR_{jt} \cdot OK_{ijt} \cdot U_{ijt} \cdot LE_{ij} \\
& + \beta^{M} CR_{jt} \cdot M_{ijt} \cdot LE_{ij} + \gamma^{MY} YK_{ijt} \cdot M_{ijt} \cdot LE_{ij} + \gamma^{MO} OK_{ijt} \cdot M_{ijt} \cdot LE_{ij} \\
& + \delta^{MY} CR_{jt} \cdot YK_{ijt} \cdot M_{ijt} \cdot LE_{ij} + \delta^{MO} CR_{jt} \cdot OK_{ijt} \cdot M_{ijt} \cdot LE_{ij} \\
& + \beta^{U'} CR_{jt} \cdot U_{ijt} + \gamma^{UY'} YK_{ijt} \cdot U_{ijt} + \gamma^{UO'} OK_{ijt} \cdot U_{ijt} \\
& + \delta^{UY'} CR_{jt} \cdot YK_{ijt} \cdot U_{ijt} + \delta^{UO'} CR_{jt} \cdot OK_{ijt} \cdot U_{ijt} \\
& + \beta^{M'} CR_{jt} \cdot M_{ijt} + \gamma^{MY'} YK_{ijt} \cdot M_{ijt} + \gamma^{MO'} OK_{ijt} \cdot M_{ijt} \\
& + \delta^{MY'} CR_{jt} \cdot YK_{ijt} \cdot M_{ijt} + \delta^{MO'} CR_{jt} \cdot OK_{ijt} \cdot M_{ijt} \\
& + \omega M_{ijt} \cdot LE_{ij} + \omega' M_{ijt} + \mu LE_{ij} + D_j \theta + D_t \lambda + D_t \cdot LE_{ij} \lambda' + \varepsilon_{ijt}\ .
\end{aligned}
$$
[14]

In this case, we introduce the interactions with *LE*, and the coefficients on these interactions are the parameters of interest.[15] The interactions between the EITC, marriage, and fertility that are *not* interacted with *LE* are not interpreted as causal, but rather as control variables for other types of shocks correlated with EITC changes not picked up in the other controls. 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 the change in the generosity

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

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

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

of the EITC over time.[16]

We translate this usual short-run approach to estimating the effects of the EITC to our longer-run approach in a straightforward way. Specifically, we define the variables in equations (1)-(4) not as dummy variables (in the case of *U*, *M*, *YK*, and *OK*) or as single-period values (in the case of *CR*). Instead, we compute the averages of the interactions for the policy, childbearing, and marital status variables over the ages of the women we consider prior to measuring "mature adult" outcomes. Specifically, we compute these over ages 22-39. Moreover, we split these into averages computed over younger ages (22-29) and older ages (30-39), to allow for different effects of children, marriage, and the EITC depending on a woman's age.

Consider, for example, the term $\delta^{UY}CR_{jt}\cdot YK_{ijt}\cdot U_{ijt}\cdot LE_{ij}$. For this term, we define t as the period when a woman is observed at age 40, and substitute two terms

(5) $\delta^{UY}_{30-39}\cdot\{\sum_{a=t\text{-}10}^{t\text{-}1}(CR_{ja}\cdot YK_{ija}\cdot U_{ija})/10\}\cdot LE_{ij}$,

and

(5') $\delta^{UY}_{20-29}\cdot\{\sum_{a=t\text{-}18}^{t\text{-}11}(CR_{ja}\cdot YK_{ija}\cdot U_{ija})/8\}\cdot LE_{ij}$,

and similarly for the other terms in the preceding equations. We compute averages of the interactions, rather than interactions of averages, to more accurately capture the EITC to which a woman was exposed when she was married or unmarried, had young children, etc. Our approach will capture, for example, the difference between two women who had the same marital history and faced the same EITC in each year, but who had young children at different ages.[17] We substitute these expressions into equation (3) (or (4)) to estimate the effects of these longer-run exposure variables on outcomes at age 40.[18]

Although not included in the above equations, we also include the full set of variables for marital

[16] To be symmetric, we might want interactions between *LE* and the state dummy variables as well. We omit these for parsimony, and because the potential correlation over *time* between changes in outcomes for lower- and higher-education women seems more potentially problematic. Nonetheless, results are robust to including these interactions; and they are also robust to omitting the *LE*-year interactions. (Result available upon request.)
[17] We show that the results are by and large robust to computing the average variables like those in equations (5) and (5') over the 22-39 age range, including fewer variables in the model.
[18] In one of our robustness checks we define the variables extending instead to ages 37, 38, 40, and 41, and defining outcomes at ages 38, 39, 41, and 42, respectively, and show that results are similar.

status, children, EITC, etc., variables at age 40. (In this case the marital status and children variables are dummy variables.) We do this to be sure we do not confound the effects of past marriage, childbearing, and the EITC with effects of contemporaneous variables.

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

We only report estimates of the key parameters of interest – which are δ_{20-29}^{UY}, δ_{30-39}^{UY}, δ_{20-29}^{UO}, δ_{30-39}^{UO}, δ_{20-29}^{MY}, δ_{30-39}^{MY}, δ_{20-29}^{MO}, and δ_{30-39}^{MO}, in our longer-term version of equation (3) (or (4)); note that we have modified the notation from those equations to include the separate exposure parameters estimated over older and younger ages. To clarify, as an example, δ_{20-29}^{UY} is the coefficient on the average, over ages 22-29, of the interaction between the two-child phase-in rate, a dummy variable for having young children, and a dummy variable for being unmarried, in equation (3); in equation (4) it is the coefficient of the interaction between this average and the indicator for low education.

Corresponding to what we said about equation (4), the interactions between the variables capturing variation in the EITC and marriage and fertility histories that are *not* interacted with *LE* are not interpreted as causal, but rather as control variables for other types of shocks correlated with these variables. That is the idea underlying additional level of differencing. However, we do want to point out that some of our results are to a fairly large extent driven by differences in estimated coefficients between less-educated and more-educated women. That is, the estimates we report below – of the interactions between the EITC exposure variables and the low-education indicator – are sometimes much smaller if we simply estimate the model for less-educated women (without the low-education interactions); or put differently, in the full model the sums of the exposure variables for the non-interacted variables and the same variables interacted with the low-education indicator are generally in the opposite direction. Overall, the pattern of results for the low-education only subsample is robust to exclusion of high-education women. The longer-run negative effects for whom the EITC creates negative short-term labor supply incentives are similar in both magnitude and significance to the estimates using education differences; this is less true for the longer-run positive effects

One might view the overall evidence as more compelling if the implied point estimates for high-education women were near zero. But, of course, that is not necessary to draw a causal inference from a differenced estimator, and in a sense, violates the idea underlying such as estimator. By way of analogy, in a more standard short-run estimate, if we found that an increase in EITC generosity in one state coincided with a decline in employment of high-skilled women but not of low-skilled women, we would interpret the former as reflecting a shock to employment in the state raising its EITC, and the *relative* estimate for low-skilled women as reflecting the effect of the EITC.[19] Of course, the identifying assumption is that the shock to high-skilled and low-skilled women is the same. But even if there was no empirical association between the increase in the EITC and employment of high-skilled women, we would still rely on this identifying assumption; otherwise the apparent effect of the EITC could reflect a shock specific to low-skilled women in the state raising its EITC. None of this discussion is to deny, however, that it would be useful to obtain evidence on the long-run effects of exposure to a more generous EITC from other empirical strategies.

III. Data

PSID Data

The data for this paper come from the Panel Study of Income Dynamics (PSID), using data

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

through the 2015 survey. We need to observe long longitudinal records on women, because their "exposure" to the EITC, as explained in Section II, depends on where they live,[20] as well as their marital and childbearing history.[21] We also take advantage of the longitudinal data to construct cumulative measures of years of experience.

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

We limit the sample to women observed at age 40 for whom we also observe their whole history beginning at age 22. To assign histories by age for each of these women, 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, etc.[22] We assign full 19-year histories for all the necessary variables: marital status, number of children, age of children, and employment.[23] Additionally, we need information on race and education, but these are not assigned on a year-by-year basis.

Although the first year of the PSID is 1968, 1979 is the first year in which employment status for all individuals is captured. Thus, this is the first year we use, so that our data cover women who are observed at age 40 from 1996 to 2014.[24] We begin our analysis at age 22 to avoid capturing women when they are more likely to still be in school or living with their parents, periods during which EITC incentives may be much weaker.

[20] Below, we consider the possibility that migration is endogenous.
[21] Combining SIPP panels can provide data over a long period but would not provide long-term marital and childbearing histories.
[22] 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.
[23] The question about earnings refers to the past year. (For example, the data in the PSID 1968 refer to calendar year 1967.) Because of this, we assign women's ages as the age they report in a year minus one, to align with earnings at that age. We follow the same algorithm in filling in non-survey years once the PSID data become bi-annual.
[24] We also explore sensitivity of the results to using different ages than 40.

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

To assign number of children by age, we use information about the woman's birth history. Specifically, a woman is asked about the birth timing of up to five children, allowing us to assign a detailed child history over a woman's primary childbearing years.[25] The downside of this approach is that if a woman gains a child in a manner other than childbirth, primarily via marriage or adoption, then this measure will miss them; this is relevant to the EITC because step-children, for example, could still affect EITC benefits. We constructed alternative measures using all members of the family unit and their relations to the head, but these measures turn out to be very highly correlated. We similarly assign whether the woman has younger/older children conditional on having children using the age of the youngest child assigned to the woman. Among women with children, we define those with young children based on whether the youngest child is under the age of 6.

Earnings data are available for heads of household and wives. For women who fit either of these relationship categories, earnings are assigned. These earnings are then converted using the CPI-U into 2012 dollars. Employment status, meanwhile, is available at the individual level for all individuals beginning with the 1979 PSID, which excludes the earliest cohorts from the sample for which we can observe a full 19-year employment history. (We do not construct an earnings history, but we do construct cumulative work experience.) Whereas the birth and marriage variables do not require a woman to be interviewed every year, constructing cumulative work experience does, so this variable is available for fewer observations.

Finally, we include two measures that are not tied to a 19-year history: race and education. Due

[25] 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.

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.[26] We assign educational attainment based on the woman's education level at age 40.

Table 1 shows the sample construction, and how the sample restrictions we impose based on the need for long-term longitudinal data restrict the number of available observations. Offspring of original sample members (and some additional families) are added over time, and the last available survey is in 2015. Thus, only a subset of cohorts can be observed as young as 22 and as old as 40, with the labor market and other history observable, which is why the available observations drop so sharply from row A to row D.[27] The five rows after row D document the relatively small number of observations we lose because of other data requirements (e.g., having a full marital history, or race (black/non-black) being coded consistently over time). We end up with 774 women in our final low-education subsample.

Policy Variation

Information on the EITC comes from a database of historical parameters maintained by the Tax Policy Center.[28] The policy variation we study is depicted in Figures 1 and 2. Figure 1 shows the federal EITC phase-in rate depending on number of children. The figure illustrates that, as noted in the previous section, the zero-child phase-in rate is miniscule. The one-, two-, and three-child phase in rates differ, but there is not much independent variation, which is why we simply use one measure – the two-child phase-in rate.

Figure 2 depicts information on supplemental state EITCs; these are almost always a fixed percentage of the federal EITC payment for which a family/person is eligible. The squares show the number of states with such supplements, rising from zero in 1983 to more than half the states by 2014. We then show the average, minimum, and maximum state supplement rates over time. As the figure shows, the average has settled down to about a 20 percent supplement to the federal EITC.

[26] Hispanic ethnicity cannot be coded consistently.

[27] To be sure, there is attrition in the PSID, as documented, for example, in Lemay (2009). This is reflected in the drop in the number of observations between rows C and D of Table 1.

[28] See http://www.taxpolicycenter.org/sites/default/files/legacy/taxfacts/content/PDF/historical_eitc_parameters.pdf (viewed October 11, 2016).

IV. Replication of Past Results on EITC and Employment

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

Eissa and Liebman (1996) study EITC changes in 1986, which, as Figure 1 shows, increased EITC phase-in rates, although not sharply.[29] They report several difference-in-difference (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 Table 2 report their estimates. The second-to-last column reports their DD estimates. All are positive, consistent with a positive effect of the EITC on employment of women (possibly low-skilled) with children. Three of the five estimates are statistically significant.

The columns labeled "Replication" show results using the PSID data for the same years. Despite the 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 less than high school treatment (with children) and control (without children) groups. But as the table shows, the sample size is particularly small for this analysis (175 observations), 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.

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

They estimate year-by-year differences in the employment rate of women with and without children, controlling for other characteristics. As shown in Table 3, they find clear evidence that the gap (with initially much lower employment rates for women with children) 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; 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 1). Thus, it does appear feasible to use the PSID to study the effects of the EITC – at least with respect to the possible simpler question of shorter-run effects on employment of women (possibly low-skilled) with children.

V. Descriptive Statistics

Table 4 reports descriptive statistics for our PSID sample. We break the sample into means calculated over ages 22-29 and 30-39, to correspond to our specifications; the difference by age also provide some information on the evolution of the marriage and children variables as women age. We also show some descriptive statistics for the low-education and high-education subsamples.

The third through fifth rows report descriptive statistics on the policy variation. The next rows report on the marriage and childbearing histories. For low-education, we see what we would expect, with a higher proportion of years with older children, and a higher proportion of years married, from ages 30-39 compared to ages 20-29. Comparing low-education and high-education women over the entire age range, the lower overall fertility of high-education women is reflected in a smaller proportion of years with (older) children.

We also see that 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 overrepresented in the target population is useful, increasing variation in the independent variables, which in turn results in more precise estimates.[30]

---

[30] 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

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

VI. Results

Baseline Specification Results

Table 5A presents estimates of the regression models used in our core analysis – the long-run exposure version of equation (4) based on equation (5). The first column shows estimates for the effects on cumulative labor market experience of the averages, computed over ages 22-29 and 30-39, of the interactions between the EITC, dummy variables for marital status, and dummy variables for whether women had young children or older children.

As shown in the first row of column (1), we obtain a positive estimate of the effect of the EITC for women exposed to a more generous EITC when unmarried with young children (31.1), when we might expect the strongest positive extensive margin effect, although we do not find a positive effect for exposure of these women when they have older children (−5.0). In the third and fourth rows, we find negative estimates for women aged 20-29 who exposed to a more generous EITC when they are married, with either young children or older children – with a larger and statistically significant effect in the latter case (−26.3). These estimates are largely consistent with expectations: theory predicts, and existing evidence establishes, that the contemporaneous effect of the EITC is to boost employment of women with children who are unmarried (as they are likely to have lower family income), and the EITC is more likely to reduce employment among married women with children (although this evidence in the existing literature is much weaker).[31] Of course, in this case we translate these with the predicted effects of the EITC in a dynamic setting. The estimates in column (1) simply reflect the accumulation of these static or contemporaneous effects across many years, and the cumulative effect may be stronger than the often

estimates, which we do below. (Solon et al. also point out that if the oversampling is endogenous with respect to the dependent variable, then weighting by the inverse probability of selection is needed to recover consistent estimates of a regression. In our case, we are generally studying outcomes for offspring of PSID families, at age 40, so the oversampling seems far less likely to be endogenous.)

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

weak evidence of negative short-run labor supply effects for married women (e.g., Eissa and Hoynes, 2004). The next four rows report estimates for the cumulative effects of exposure to a more generous EITC, for women with different marital and childbearing histories, over age 30-39. These estimates are less clearly consistent with expectations.

However, the magnitudes of these estimated longer-run effects are tricky to interpret, for a couple of reasons. First, a one-unit increase in the right-hand-side variable is very much an "out-of-sample" prediction and indeed an unreasonable scenario. For example, a one-unit increase in the first variable implies a change from zero to 100% in the phase-in rate, and changing the marital and fertility history from all years married to all years unmarried, and no years with young children to all years with young children. Second, these effects are not readily interpretable as partial effects, since changes in the marital and childbearing history imply changes in the other variables that also capture these histories.

We address the interpretation issue posed by this second problem below. However, as a partial (but imperfect) solution, in Table 5A we provide a more sensible scaling, reporting in square brackets the effects of a 10 percentage-point increase in the phase-in rate for one year, for someone with – respectively across the rows of the table – all years married/unmarried or with young or with older children, in the corresponding age range. These amount to multiplying the coefficients by 0.1, and then dividing by 8 for the 22-29 age range, or 10 for the 30-39 age range (the number of years women are studied, excluding the contemporaneous observations at age 40). Thus, as an example, in the first row, the estimate of 0.389 in square brackets implies that a 10 percentage-point increase in the phase-in rate for one year results in 0.389 years of additional cumulative experience, for hypothetical women who always have young children and are always unmarried. This seems like a very large effect. Suppose that 10 percent of women work one additional year because of the higher EITC in place for one year.[32] Then over 8 years, the average effect on cumulative experience would be 0.0125 years, or only a small fraction of the 0.389

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

estimate. However, recall that we are estimating long-run effects, and if short-run increases in employment spur increases in subsequent years, the effects can be larger than what is implied by short-run estimates. Moreover, this calculation does not take account of the fact that more years unmarried implies fewer years married, so it is necessary also to apply the negative coefficients in the third or fourth rows (depending on the fertility history) of Table 5A – a point to which we return below.

Column (2) reports the estimated effects on employment at age 40. Only one of the estimates is statistically significant – a positive effect for women exposed to a more generous EITC when they were unmarried with older kids, over ages 30-39 (2.586). In general, the signs of the estimates in this column do not give a clear indication that the potential longer-term effects of exposure to the EITC are reflected in employment at age 40.

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

Differences between the earnings and wage estimates can be driven by the hours effects reported in column (5), which generally indicate positive effects on hours at age 40 for exposure to a more generous EITC when unmarried with children, and negative effects for exposure when married with children (in the latter case, especially, regardless of age of children). Thus, we do find evidence of longer-term effects on labor supply – but for hours, not employment. For wages, earnings, and hours, we find that the negative estimates for exposure to a more generous EITC when women were married and had children are larger for exposure when children were young. As noted above, the income effects of the

EITC may be most severe when there are young children in the household, and the evidence for hours, for exposure at ages 30-39, is consistent with this.[33]

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

In Table 5B, we therefore provide a more satisfactory interpretation of the magnitudes from Table 5A. Here, we use our estimates to simulate the effects of a permanent 10 percentage-point increase in the phase-in rate (i.e., from ages 22-39) for four "types" of women. First, we calculate this for all women, based on the women with the sample averages of the regressors, shifted only by the change in the EITC (column (1)). More importantly, we then calculate the implied effects for women with different scenarios with respect to the timing of marriage and childbearing. Column (2) is based on having children early (one at age 22 and one at age 24) but never marrying. These women should be, relative to the average, more exposed to high benefits because they have children early, and more likely to reflect the positive extensive margin effects of the EITC on employment because they are unmarried. In contrast, column (3) is based on the same fertility history but being always married from ages 22-39. For these women the labor supply effects are more likely to be negative because they have husbands, and they were likely exposed to these incentives for a long time because of early childbearing. Columns (4) and (5) repeat the marital histories in columns (2) and (3), but for women who have children later (at ages 30 and 32). The estimates in the last row of each panel, in column (4), should be in the same positive direction as in

[33] In the standard labor supply graph with axes for leisure and income spent on consumption, children in the household presumably steepen the indifference curves (a higher marginal utility of "leisure," and a higher reservation wage), making a utility maximum at the kink point more likely when exogenous income is higher.

column (2), but weaker because of shorter exposure to the EITC (and possibly different, also, because the exposure occurs at older ages when earnings may be higher, etc.).[34] In each panel we report the average effect implied for these women, and then in the remaining rows in each panel we report the estimated differences relative to the other marriage and childbearing scenarios; it is these latter comparisons that are the most informative.

Panel A of Table 5B presents the estimates for employment at age 40. As indicated in the first row of column (1), on average, the effect of the more generous EITC over ages 22-39 on employment at age 40 is estimated to be 6.2 percentage points, although this estimate is not statistically significant.

Of more interest are the estimated differences between the effects of exposure to a more generous EITC for women with different marital and childbearing histories, reported in the second through fourth rows. The estimated employment effects in Panel A are small and generally statistically insignificant, with one exception that indicates a positive employment effect for never married women who have children early versus always married women who have children late (0.284), which is consistent with the expected cumulative effect of a positive extensive margin effect of exposure to a more generous EITC for unmarried women with children.

The estimates in Panels B and C, for wages and earnings, provide stronger findings. Longer-term exposure to a more generous EITC boosts the wages and earnings of those who were exposed as young unmarried mothers with young children, relative to women who had children early but were always married (e.g., the 0.549 estimate in the second row of column (2) of Panel B, significant at the 10-percent level), and relative to women who were always married but had children later (the 0.366 estimate the same column and panel). In Panel C, for earnings, the estimates in column (2) for never married women who had children early, relative to always married women regardless of when they had children, are larger than for wages (the 1.31 and 1.00 estimates), and are strongly statistically significant. The estimated effects comparing never married women who had children early versus late are close to zero

[34] There is no comparison reported in column (5), since all the comparisons for the women in this column are covered in columns (2)-(4).

and statistically insignificant.

The estimated magnitudes of the earnings estimates for never married young mothers versus both types of always married mothers are in the range of 100 or more log points – magnitudes that may seem larger than is credible. For example, if the return to experience averaged 4 percent per year, then 5 additional years of experience would increase wages or earnings by around 20 percent. It is possible that there are other factors, however, if the greater labor force attachment spurred by a more generous EITC boosts other human capital investments, increases effort in finding better jobs with prospects for more wage growth, etc. Moreover, the estimates for hours in in Panel D, column (2), point to large positive hours effects for the corresponding comparisons (the 919.5 and 1,145.0 estimates), which would of course boost earnings. These hours estimates also indicate strong cumulative effects on hours of exposure to a more generous EITC when the positive extensive margin effects should have been largest. Overall, the evidence is consistent with large positive effects on earnings, which is what we would predict from the greater accumulation of human capital associated, in part, with more years of employment.

Column (3) provides alternative comparisons. We would anticipate negative effects for the third row of each panel, which compares always married women who had children early to never married women who had children late. The evidence is consistent with this prediction, and again significant – with a large estimate (−1.33) – for earnings, which is also consistent with the estimated negative hours effect.[35] In column (4) we find a positive and significant earnings effect (1.02) for never married women who have children late versus always married women who have children late.

All potential comparisons in column (5) have already been covered; we include this column to report the average effect for the women covered in this column (the row labeled "Estimate"). Note, however, that for wages, earnings, and hours, the estimates in the first row of each panel, which capture the average effect for the corresponding women in each panel, almost always have the expected signs – positive for never married women and negative for always married women.

[35] We would also expect a negative effect relative to women who have children early and are never married, but that is covered in column (2) – for the comparison in the opposite direction – which is why we report N/A in the table.

Overall, then, there is quite strong evidence in Table 5B consistent with positive cumulative effects on wages, earnings, and hours from extensive margin effects on unmarried women with children. And similarly, there is evidence of negative cumulative effects.

We do not focus on similar calculations to those in Table 5B for cumulative experience from ages 22-39, since these estimates are not our primary interest. However, we do collect these calculations for Table 5A, and for the other tables we describe below, in Appendix Table A1. (We report what we regard as the two key estimates capturing extensive margin effects in Table 5B: the difference between early children/never married and early children/always married; and the difference between early children/never married and late children/always married.) As reported in the first row of Appendix Table A1, the implied effect of the EITC on cumulative experience of young never married mothers relative to young always married mothers is positive, but not huge (1.915), and not statistically significant. These results – and they are echoed for later specifications – suggest that the evidence in Table 5B of positive relative effects of long-term exposure to the EITC on wages and earnings of young, unmarried (and low-education) mothers may not be driven primarily by the accumulation of more labor market experience.[36] On the other hand, the hours effects reported in Table 5B – and they are larger in some subsequent tables – suggest that there could be longer-term effects on earnings through working more hours.

We next turn to analyses intended to probe the robustness and credibility of the results. We go through a long list of such analyses, so it is useful to provide the punchline first. For the most part, the qualitative results are robust and survive our different credibility analyses, and the statistical strength of the evidence generally does not vary much, although naturally there are some cases where the results become a bit weaker, and some cases where they become a bit stronger.

Varying the specification or sample

We first consider differences in how to specify the model or how to select the sample. Table 6 presents estimates that result from varying whether we estimate separate effects for younger or older

[36] However, for almost every analysis we present we find a positive effect on cumulative experience for this comparison as well as the second comparison, and many of the estimated effects are larger than those corresponding to Table 5B.

children, or for women distinguished by two age ranges, or simply over the 22-39 age range. In the latter case, for example, equations (5) and (5') are modified to compute a single average over ages 22-39.

For this and the additional analyses that follow, we reported an abbreviated set of estimates corresponding to Table 5B – in particular, the subset of estimates for which there is a clear prediction as to the cumulative effects of static effects. Panel A of Table 6 indicates the same estimates for Table 5B for the exact subset we use, to clarify what this subset is. In particular, we report the estimates for five comparisons: early children/never married versus early children/always married, for which theory and prior evidence predicts positive effects; early children/never married versus late children always married (positive); early children/always married versus late children never married (negative); early children/always married versus late children always married (negative); and late children/never married versus late children/always married (positive).

In Panel B we use cumulative exposure effects distinguishing women by age of children but not their own age, in Panel C we do the opposite, and in Panel D we do neither. The qualitative results are robust to these alternative specifications, although in most cases the point estimates are smaller when our accumulation of exposure to a more generous EITC is "cruder" in not distinguishing by age of women or age of children. As but one example, the positive earnings effect of a more generous EITC for women who have children early and are never married versus women who have children late and are always married falls from 1.00 to 0.67 when we do not distinguish the effects of exposure by either age of children or age of women. Yet the sign pattern and the occurrence of statistically significant effects is virtually identical across the different panels of Table 6.

Table 7 varies the age at which we measure labor market outcomes and hence estimate the effects of cumulative exposure to a more generous EITC. In particular, we show results for the baseline specification (corresponding to Panel A of Table 6), using ages 38, 39, 41, or 42, instead of age 40. Comparisons across these different ages indicates that the estimates are robust, although the same estimates are not always statistically significant across all panels.[37]

Endogeneity concerns

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

A second check is to use only federal EITC variation, which provides important variation but is not influenced by inter-state migration. These estimates, reported in Panel C of Table 8, are also very similar. Thus, we conclude that migration does not bias our estimated effects.[38] 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.

Alternative EITC parameterization

We have used the two-child phase-in rate to capture the generosity of the EITC. In Table 9 we instead use the maximum credit; this is the credit amount at which the EITC payment no longer increases

[37] Appendix Table A2 reports the results – discussed earlier – from separate estimation for the low-education and high-education subsamples.

[38] An alternative type of endogeneity that could affect our results is endogeneity of marriage or childbearing. As discussed in many papers, including a recent review by Nichols and Rothstein (2016), in principle the EITC creates incentives to remain unmarried, and to have children. In terms of our specifications, this implies that a higher EITC can increase the proportion of years spent unmarried, or with young children. However, without knowing how this potentially endogenous response is associated with the propensity to work, or unobserved determinants of wages, it is unclear whether or how this biases our estimated effects. Moreover, Rothstein and Nichols conclude that there is no clear evidence of that the EITC affects reduces marriage or increases childbearing, although some recent evidence points in this direction for marriage (Michelmore, forthcoming). 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.

and stays constant until the phase-out begins. As explained in the notes to the table, we use a policy simulation that amounts to about the same percentage increase in EITC generosity as the 10 percentage point increase in the phase-in rate we have been using. The table shows that the results are very similar.

Confounding with welfare reform

In Table 10, we consider the possibility that our estimates are confounded with the effects of welfare reform. Rather than trying to code up numerous features of welfare and how they changed from AFDC to TANF, we instead create a single variable meant to capture policy changes associated with welfare reform – a post-1996 dummy variable (TANF began in January 1997). For the estimates reported in Panel B, we interact this with a dummy for whether women have children, and average this over the two age ranges used (22-29 and 30-39). Thus, these variables effectively pick up the share of years women both had children and were exposed to welfare reform. We introduce these variables, as we do our other policy variables, alone and interacted with a dummy variable for low-education women. The estimates in Panel A indicate that including the effect of welfare reform in this way has virtually no impact on the estimates.

We then code this up in a richer way, treating it the same way as we do the EITC policy variable – i.e., interacted with the dummy variables for young/old children and married/unmarried, and then averaged over the two age ranges (and again focusing on the interactions with the low-education indicator). The results for this specification are reported in Panel C. A number of the point estimates are qualitatively similar, but with considerably larger standard errors (e.g., the earnings and hours effects in column (2), and the earnings effect in column (4)). In other cases, the estimates are much smaller (e.g., the wage effect in column (1), and the hours effect in column (3)). The much larger standard errors are not surprising, given that we soak up the average EITC differences before and after 1996, which as Figure 1 shows are substantial. Given the larger standard errors, the non-robustness of some estimates could just be attributable to difficulty in identifying the EITC effects in this much more flexible specification. However, the similarity of many of the point estimates leads us to conclude that our estimates are not spuriously driven by welfare reform.[39]

Weighting

Our final analyses concern weighting. We are quite reticent to put much store in the sample weights, given the extensive sample selection rules imposed to study longer-term effects of the EITC (see Table 1). However, while there is little reason to believe the sample weights are very accurate, they ought to capture broad-brush differences between those oversampled based on the low-income criterion. Panel B of Table 11 reports results for the baseline specification and sample, but weighting by the PSID Core sample weights for the age 40 observations. With the weights, the estimated wage and earnings effects are no longer statistically significant, although the positive estimated earnings effects in columns (1) and (2) remain sizable. The estimated hours effects do not change much – especially for the expected positive effects in columns (1), (2), and (5). Moreover, the employment effects in columns (1) and (2) – where we would expect positive effects – are substantially larger and more strongly significant. Thus, while the exact estimates clearly are sensitive to weighting, we view the estimates as providing additional evidence of the robustness of the conclusion that expected short-run effects of exposure to a more generous EITC appear to carry over into longer-run effects – especially for the positive extensive margin effects.

We know that a principal effect of the oversampling of low-income families in the PSID is a strong overrepresentation of blacks. In our data set, the average weight on blacks is less than one-third that of non-blacks, so the weighted estimates substantially downweight blacks. This suggests that we can also learn about the sensitivity of the estimates to weighting by looking at estimates for blacks and non-blacks, which we do in Panels C and D of Table 11. The point estimates are qualitatively similar in the two panels, although precision of the estimates declines substantially, surely in part because the samples become particularly small when we disaggregate by race. Still, the qualitative conclusions are unaffected.

VII. Conclusions

We use longitudinal data on marriage and children from the Panel Study of Income Dynamics to characterize women's exposure to the federal and state Earned Income Tax Credit (EITC) during

[39] We do not find significant effects of exposure to welfare reform on our age 40 outcomes in either specification.

approximately their first two decades of adulthood. We then estimate the long-run effects of this exposure to the EITC on women's wages and earnings (as well as employment and hours) as mature adults.

We find evidence indicating that exposure to a more generous EITC when women were unmarried and had younger children leads to higher earnings and hours, and perhaps higher wages, in the longer run. We also find some evidence that exposure to a more generous EITC when women had young children but were married leads to lower earnings and hours in the longer run. The longer-run effects are to some extent consistent with what we would expect if the short-run effects of the EITC on employment that are documented in other work, and predicted by theory, are reflected in cumulative labor market experience that influence earnings. However, the estimated effects of long-run exposure to the EITC on earnings appear to be larger than can be accounted for by differences in labor market experience. The evidence of higher hours may help explain this result, and there might also be a sizable role for impacts on investment aside from that associated with labor market experience, such as training, investment in job search for jobs with greater wage growth prospects, etc.[40]

Overall, though, the results provide some support for concluding that a more generous EITC not only boosts employment of low-skilled, generally single, mothers in the short term – a result established in the existing literature of the labor supply effects of the EITC. In addition, longer-term exposure to a more generous EITC 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.

[40] We plan to investigate these other channels of long-term effects in future work.

References

Baughman, Reagan, and Stacy Dickert-Conlin. 2009. "The Earned Income Tax Credit and Fertility." Journal of Population Economics 22(3): 537-63.

Baughman, Reagan, and Stacy Dickert-Conlin. 2003. "Did Expanding the EITC Promote Motherhood?" American Economic Review Papers and Proceedings 93(2): 247-51.

Card, David, and Dean R. Hyslop. 2005. "Estimating the Effects of a Time-Limited Earnings Subsidy for Welfare-Leavers." Econometrica 73(6): 1723-70.

Card, David, and Alan B. Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." American Economic Review 84(4): 772-93.

Dahl, Molly, Thomas DeLeire, and Jonathan Schwabish. 2009. "Stepping Stone or Dead End? The Effect of the EITC on Earnings Growth." National Tax Journal 62(2): 329-46.

Eissa, Nada, and Hilary Williamson Hoynes. 2004. "Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit." Journal of Public Economics 88(9-10): 1931-58.

Eissa, Nada, and Jeffrey B. Liebman. 1996. "Labor Supply Response to the Earned Income Tax Credit." Quarterly Journal of Economics 111(2): 605-37.

Evans, William N., and Craig L. Garthwaite. 2014. "Giving Mom a Break: The Impact of Higher EITC Payments on Maternal Health." American Economic Journal: Economic Policy 6(2): 258-290.

Hoynes, Hilary, Doug Miller, and David Simon. 2015. "Income, the Earned Income Tax Credit, and Infant Health." American Economic Journal: Economic Policy 79(1): 172-211.

Lemay, Michael. 2009. "Understanding the Mechanism of Panel Attrition." Ph.D. Dissertation, University of Maryland, College Park, MD.

Meyer, Bruce D. 2010. "The Effects of the Earned Income Tax Credit and Recent Reforms." In J. R. Brown (Ed.) Tax Policy and the Economy, Volume 24. Chicago: University of Chicago Press, pp. 153-80.

Meyer, Bruce D., and Dan T. Rosenbaum. 2001. "Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers." Quarterly Journal of Economics 116(3): 1063-114.

Michelmore, Katherine. Forthcoming. "The Earned Income Tax Credit and Union Formation: The Impact of Expected Spouse Earnings." Review of Economics of the Household.

Neumark, David. 2016. *Inventory of Research on Economic Self-Sufficiency*. Economic Self-Sufficiency Policy Research Institute, UCI. https://www.esspri.uci.edu/researchinventory.php (viewed April 19, 2017).

Neumark, David, Brian Asquith, and Brittany Bass. In progress. "The Long-Run Effects of Minimum Wages and Other Anti-Poverty Policies on Disadvantaged Neighborhoods."

Neumark, David, and Olena Nizalova. 2007. "Minimum Wage Effects in the Longer Run." Journal of Human Resources 42(2): 435-52.

Neumark, David, and William L. Wascher. 2011. "Does a Higher Minimum Wage Enhance the Effectiveness of the Earned Income Tax Credit?" Industrial and Labor Relations Review 64(4): 712-46.

Nichols, Austin, and Jesse Rothstein. 2016. "The Earned Income Tax Credit." In R.A. Moffitt (Ed.) Economics of Means-Tested Transfer Programs in the United States, Volume 1. Chicago: University of Chicago Pres, pp. 137-218.

Solon, Gary, Steven J. Haider, and Jeffrey M. Wooldridge. 2015. "What Are We Weighting For?" Journal of Human Resources 50(2): 301-16.

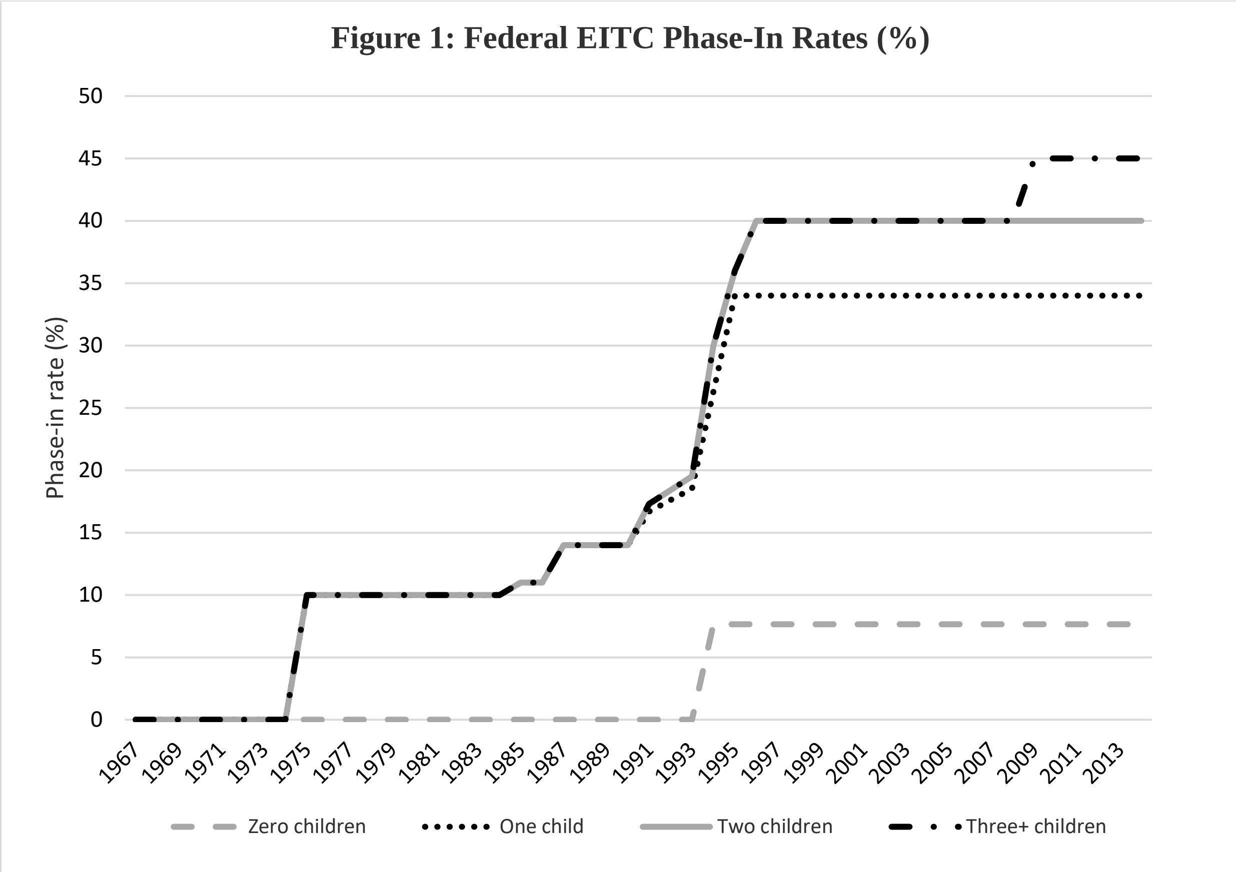

Figure 1: Federal EITC Phase-In Rates (%)
Phase-in rate (%)
50
45
40
35
30
25
20
15
10
5
0
1967 1969 1971 1973 1975 1977 1979 1981 1983 1985 1987 1989 1991 1993 1995 1997 1999 2001 2003 2005 2007 2009 2011 2013
Zero children
One child
Two children
Three+ children

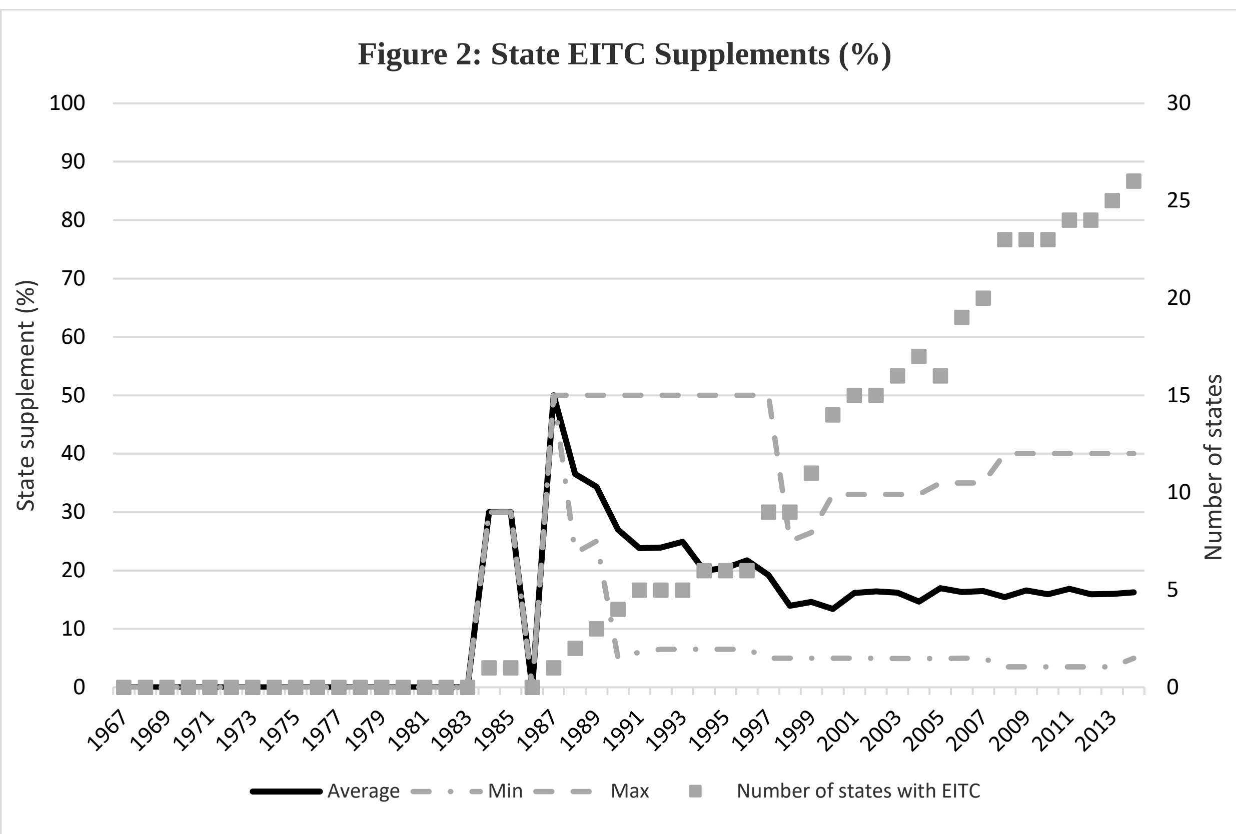

Figure 2: State EITC Supplements (%)
State supplement (%)
100
90
80
70
60
50
40
30
20
10
0
Number of states
30
25
20
15
10
5
0
1967 1969 1971 1973 1975 1977 1979 1981 1983 1985 1987 1989 1991 1993 1995 1997 1999 2001 2003 2005 2007 2009 2011 2013
Average
Min
Max
Number of states with EITC

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

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

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

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

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

Table 4: Descriptive Statistics for Long-Term Analysis (Means)

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

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

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

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

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

Table 5B: Estimated Differences from Permanent 10 Percentage-Point Increase in Two-Childe Phase-In Rate Implied by Table 5A Estimates, at Age 40					
Evaluated at/for:	Sample averages	Early children (22, 24), never married	Early children (22,24), always married	Late children (30, 32), never married	Late children (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment (Table 5A, col. (2))					
Estimate	0.062 (0.078)	0.249** (0.106)	0.115 (0.141)	0.028 (0.173)	-0.035 (0.125)
Difference from early children, always married	N/A	0.134 (0.152)	N/A	N/A	N/A
Difference from late children, never married	N/A	0.221 (0.170)	0.087 (0.211)	N/A	N/A
Difference from late children, always married	N/A	0.284* (0.160)	0.150 (0.105)	0.063 (0.212)	N/A
B. Log hourly wage (employed) (Table 5A, col. (3))					
Estimate	0.008 (0.122)	0.367* (0.183)	-0.182 (0.231)	0.331 (0.251)	0.001 (0.213)
Difference from early children, always married	N/A	0.549* (0.315)	N/A	N/A	N/A
Difference from late children, never married	N/A	0.037 (0.257)	-0.512 (0.353)	N/A	N/A
Difference from late children, always married	N/A	0.366 (0.265)	-0.183 (0.153)	0.329 (0.342)	N/A
C. Log earnings (employed) (Table 5A, col. (4))					
Estimate	-0.188 (0.172)	0.658** (0.255)	-0.649** (0.306)	0.676* (0.387)	-0.346 (0.299)
Difference from early children, always married	N/A	1.307*** (0.427)	N/A	N/A	N/A
Difference from late children, never married	N/A	-0.018 (0.373)	-1.325*** (0.457)	N/A	N/A
Difference from late children, always married	N/A	1.004*** (0.368)	-0.303 (0.205)	1.022** (0.470)	N/A
D. Annual Hours (Table 5A, col. (5))					
Estimate	-123.07 (164.39)	591.96* (330.40)	-327.53 (227.93)	623.51 (423.87)	-553.01** (247.85)
Difference from early children, always married	N/A	919.49** (394.99)	N/A	N/A	N/A
Difference from late children, never married	N/A	-31.55 (342.80)	-951.04** (471.01)	N/A	N/A
Difference from late children, always married	N/A	1144.97*** (369.30)	225.48 (178.65)	1176.52** (502.77)	N/A

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

Table 6: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Two-Child Phase-In Rate, Alternative Specifications Based on Ages of Children and Ages of Women, at Age 40					
Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Late children (30, 32), always married	Early children (22,24), always married vs. Late children (30, 32), never married	Early children (22,24), always married vs. Late children (30, 32), always married	Late children (30, 32), never married vs. Late children (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Baseline (Both Age of Children and Age of Woman)					
Employment	0.134 (0.152)	0.284* (0.160)	0.087 (0.211)	0.150 (0.105)	0.063 (0.212)
Log hourly wage (employed)	0.549* (0.315)	0.366 (0.265)	-0.512 (0.353)	-0.183 (0.153)	0.329 (0.342)
Log earnings (employed)	1.307*** (0.427)	1.003*** (0.368)	-1.325*** (0.457)	-0.303 (0.205)	1.022** (0.470)
Annual hours	919.49** (394.99)	1144.97*** (369.30)	-951.04** (471.01)	225.48 (178.65)	1176.52** (502.77)
B. Age of Children only					
Employment	0.107 (0.177)	0.145 (0.153)	-0.038 (0.133)	0.038 (0.043)	0.076 (0.108)
Log hourly wage (employed)	0.371 (0.337)	0.290 (0.283)	-0.322 (0.254)	-0.082 (0.072)	0.241 (0.209)
Log earnings (employed)	1.063* (0.532)	0.806* (0.427)	-0.912** (0.361)	-0.260* (0.137)	0.655** (0.271)
Annual hours	785.22* (434.19)	739.80* (375.49)	-669.93** (309.63)	-45.42 (93.64)	624.52** (246.58)
C. Age of Woman only					
Employment	0.065 (0.173)	0.124 (0.171)	-0.093 (0.179)	0.060 (0.090)	0.153 (0.191)
Log hourly wage (employed)	0.221 (0.335)	0.044 (0.325)	-0.154 (0.247)	-0.176 (0.148)	-0.022 (0.263)
Log earnings (employed)	0.838** (0.397)	0.527 (0.398)	-0.904*** (0.325)	-0.312 (0.210)	0.592* (0.312)
Annual hours	933.11** (456.17)	961.08** (411.54)	-937.72** (433.22)	27.97 (184.82)	965.69** (403.44)
C. Neither Age of Children nor Age of Woman					
Employment	0.080 (0.178)	0.102 (0.147)	-0.021 (0.136)	0.023 (0.045)	0.044 (0.099)
Log hourly wage (employed)	0.301 (0.306)	0.205 (0.250)	-0.263 (0.236)	-0.096 (0.076)	0.167 (0.170)
Log earnings (employed)	0.974** (0.456)	0.665* (0.358)	-0.851* (0.362)	-0.309** (0.121)	0.541** (0.253)
Annual hours	760.91* (425.10)	617.27* (352.44)	-566.36* (319.76)	-143.64 (98.49)	422.73* (236.17)

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

Table 7: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Two-Child Phase-In Rate, at Alternative Ages					
Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Late children (30, 32), always married	Early children (22,24), always married vs. Late children (30, 32), never married	Early children (22,24), always married vs. Late children (30, 32), always married	Late children (30, 32), never married vs. Late children (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Age 38 Sample and Outcomes [low-ed N=899; high-ed N=1,252]					
Employment	0.017 (0.129)	0.124 (0.133)	0.067 (0.200)	0.107 (0.105)	0.041 (0.216)
Log hourly wage (employed)	0.395 (0.415)	0.406 (0.479)	-0.642 (0.434)	0.011 (0.233)	0.653 (0.497)
Log earnings (employed)	0.856* (0.491)	0.870 (0.587)	-1.125* (0.619)	0.014 (0.285)	1.139* (0.647)
Annual hours	148.63 (447.62)	311.11 (371.61)	-442.77 (524.01)	162.48 (229.37)	605.25 (556.87)
B. Age 39 Sample and Outcomes [low-ed N=853; high-ed N=1,152]					
Employment	0.068 (0.154)	0.204 (0.165)	0.115 (0.194)	0.135 (0.116)	0.020 (0.211)
Log hourly wage (employed)	0.701 (0.462)	0.552 (0.461)	-0.521 (0.462)	-0.148 (0.216)	0.373 (0.492)
Log earnings (employed)	1.114*^ (0.554)	1.113** (0.507)	-1.151** (0.503)	-0.001 (0.309)	1.150** (0.558)
Annual hours	541.54 (533.51)	718.73 (463.13)	-596.32 (456.74)	177.19 (201.55)	773.51 (479.59)
C. Baseline (Age 40 Sample and Outcomes) [low-ed N=774; high-ed N=1,062]					
Employment	0.134 (0.152)	0.284* (0.160)	0.087 (0.211)	0.150 (0.105)	0.063 (0.212)
Log hourly wage (employed)	0.549* (0.315)	0.366 (0.265)	-0.512 (0.353)	-0.183 (0.153)	0.329 (0.342)
Log earnings (employed)	1.307*** (0.427)	1.003*** (0.368)	-1.325*** (0.457)	-0.303 (0.205)	1.022** (0.470)
Annual hours	919.49** (394.99)	1144.97*** (369.30)	-951.04** (471.01)	225.48 (178.65)	1176.52** (502.77)
D. Age 41 Sample and Outcomes [low-ed N=728; high-ed N=970]					
Employment	0.112 (0.195)	0.174 (0.198)	0.384* (0.209)	0.062 (0.118)	-0.322 (0.219)
Log hourly wage (employed)	0.545* (0.302)	0.300 (0.311)	-0.149 (0.333)	-0.245 (0.207)	-0.096 (0.340)
Log earnings (employed)	1.344*** (0.415)	1.082** (0.431)	-0.698 (0.480)	-0.262 (0.269)	0.435 (0.496)
Annual hours	921.96* (460.76)	1263.80** (476.19)	-285.74 (493.72)	341.84* (198.83)	627.58 (509.08)
E. Age 42 Sample and Outcomes [low-ed N=679; high-ed N=907]					
Employment	0.023 (0.218)	0.078 (0.212)	0.182 (0.287)	0.055 (0.111)	-0.127 (0.288)
Log hourly wage (employed)	0.547* (0.293)	0.258 (0.395)	-0.162 (0.427)	-0.289 (0.236)	-0.127 (0.479)
Log earnings (employed)	1.353*** (0.475)	0.965 (0.590)	-0.824 (0.624)	-0.388 (0.301)	0.436 (0.724)
Annual hours	595.14 (552.96)	667.82 (612.84)	-321.17 (622.61)	72.68 (272.16)	393.84 (701.26)
See notes to Tables 4, 5A, and 5B. Specifications are the same as in Table 5A, except outcomes are defined at different ages, and samples constructed corresponding to the same ages, with the older age range extending to 37, 38, 40, or 41, in Panels B-E, respectively. Maximum sample sizes across the regressions are shown.					

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

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Late children (30, 32), always married	Early children (22,24), always married vs. Late children (30, 32), never married	Early children (22,24), always married vs. Late children (30, 32), always married	Late children (30, 32), never married vs. Late children (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Baseline					
Employment	0.134 (0.152)	0.284* (0.160)	0.087 (0.211)	0.150 (0.105)	0.063 (0.212)
Log hourly wage (employed)	0.549* (0.315)	0.366 (0.265)	-0.512 (0.353)	-0.183 (0.153)	0.329 (0.342)
Log earnings (employed)	1.307*** (0.427)	1.003*** (0.368)	-1.325*** (0.457)	-0.303 (0.205)	1.022** (0.470)
Annual hours	919.49** (394.99)	1144.97*** (369.30)	-951.04** (471.01)	225.48 (178.65)	1176.52** (502.77)
B. Fixed State at age 22					
Employment	0.125 (0.159)	0.255 (0.161)	0.057 (0.199)	0.130 (0.108)	0.073 (0.199)
Log hourly wage (employed)	0.631** (0.313)	0.387 (0.258)	-0.597* (0.335)	-0.244 (0.152)	0.353 (0.314)
Log earnings (employed)	1.325*** (0.419)	0.929** (0.348)	-1.274*** (0.429)	-0.395* (0.221)	0.879** (0.419)
Annual hours	865.75** (399.93)	1032.30*** (358.65)	-911.36* (504.87)	166.55 (191.51)	1077.91** (523.42)
C. Federal EITC Variation Only					
Employment	0.121 (0.172)	0.272 (0.175)	0.112 (0.226)	0.151 (0.118)	0.039 (0.228)
Log hourly wage (employed)	0.687** (0.316)	0.433 (0.261)	-0.461 (0.348)	-0.254 (0.157)	0.207 (0.324)
Log earnings (employed)	1.443*** (0.479)	0.985** (0.370)	-1.203*** (0.415)	-0.458* (0.240)	0.745** (0.349)
Annual hours	969.02** (450.45)	1151.85*** (407.15)	-794.76 (542.88)	182.82 (213.34)	977.58* (529.29)

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

Table 9: Selected Estimated Differences from Permanent $1,000 Increase in Maximum Two-Child EITC Credit, at Age 40					
Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Late children (30, 32), always married	Early children (22,24), always married vs. Late children (30, 32), never married	Early children (22,24), always married vs. Late children (30, 32), always married	Late children (30, 32), never married vs. Late children (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Baseline					
Employment	0.134 (0.152)	0.284* (0.160)	0.087 (0.211)	0.150 (0.105)	0.063 (0.212)
Log hourly wage (employed)	0.549* (0.315)	0.366 (0.265)	-0.512 (0.353)	-0.183 (0.153)	0.329 (0.342)
Log earnings (employed)	1.307*** (0.427)	1.003*** (0.368)	-1.325*** (0.457)	-0.303 (0.205)	1.022** (0.470)
Annual hours	919.49** (394.99)	1144.97*** (369.30)	-951.04** (471.01)	225.48 (178.65)	1176.52** (502.77)
B. Max Credit as Policy					
Employment	0.103 (0.114)	0.201* (0.113)	0.049 (0.158)	0.099 (0.080)	0.049 (0.154)
Log hourly wage (employed)	0.407* (0.228)	0.262 (0.189)	-0.386 (0.259)	-0.145 (0.118)	0.241 (0.250)
Log earnings (employed)	0.969*** (0.314)	0.710** (0.273)	-0.945*** (0.328)	-0.258* (0.151)	0.687** (0.329)
Annual hours	721.76** (293.24)	822.59*** (267.81)	-759.66** (328.78)	100.83 (136.01)	860.49** (345.29)

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

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

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Late children (30, 32), always married	Early children (22,24), always married vs. Late children (30, 32), never married	Early children (22,24), always married vs. Late children (30, 32), always married	Late children (30, 32), never married vs. Late children (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Baseline					
Employment	0.134 (0.152)	0.284* (0.160)	0.087 (0.211)	0.150 (0.105)	0.063 (0.212)
Log hourly wage (employed)	0.549* (0.315)	0.366 (0.265)	-0.512 (0.353)	-0.183 (0.153)	0.329 (0.342)
Log earnings (employed)	1.307*** (0.427)	1.003*** (0.368)	-1.325*** (0.457)	-0.303 (0.205)	1.022** (0.470)
Annual hours	919.49** (394.99)	1144.97*** (369.30)	-951.04** (471.01)	225.48 (178.65)	1176.52** (502.77)
B. Simple Welfare Control					
Employment	0.130 (0.153)	0.307 (0.186)	0.126 (0.259)	0.176 (0.141)	0.051 (0.215)
Log hourly wage (employed)	0.577* (0.305)	0.389 (0.314)	-0.545 (0.410)	-0.188 (0.244)	0.357 (0.329)
Log earnings (employed)	1.363*** (0.439)	1.217*** (0.441)	-1.190** (0.532)	-0.146 (0.327)	1.043** (0.462)
Annual hours	897.79** (386.25)	1381.77*** (411.30)	-599.48 (511.15)	483.97** (226.53)	1083.45** (511.86)
C. "Flexible" Welfare Control					
Employment	0.083 (0.288)	0.408 (0.283)	0.236 (0.326)	0.324** (0.157)	0.088 (0.298)
Log hourly wage (employed)	0.026 (0.578)	0.016 (0.536)	0.021 (0.795)	-0.010 (0.358)	-0.031 (0.774)
Log earnings (employed)	0.452 (0.713)	0.723 (0.608)	-0.732 (1.099)	0.271 (0.512)	1.004 (0.969)
Annual hours	232.19 (619.48)	1449.42*** (505.48)	-12.98 (896.32)	1217.23*** (369.11)	1230.21 (865.43)

See notes to Tables 4, 5A, and 5B. In Panel B, the post-welfare reform controls are the averages, over ages 22-29 and 30-39, of the interaction between a post-1996 dummy variable and a dummy variable for children; these are included alone, and interacted with low-education. In Panel C, the post-1996 dummy variable is entered in the same way as the EITC variable – i.e., as the average of interactions for each of the eight variables capturing years with young children/old children and married/unmarried.

Table 11: Selected Estimated Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Weighted, and Unweighted by Race, at Age 40					
Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Late children (30, 32), always married	Early children (22,24), always married vs. Late children (30, 32), never married	Early children (22,24), always married vs. Late children (30, 32), always married	Late children (30, 32), never married vs. Late children (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Baseline					
Employment	0.134 (0.152)	0.284* (0.160)	0.087 (0.211)	0.150 (0.105)	0.063 (0.212)
Log hourly wage (employed)	0.549* (0.315)	0.366 (0.265)	-0.512 (0.353)	-0.183 (0.153)	0.329 (0.342)
Log earnings (employed)	1.307*** (0.427)	1.003*** (0.368)	-1.325*** (0.457)	-0.303 (0.205)	1.022** (0.470)
Annual hours	919.49** (394.99)	1144.97*** (369.30)	-951.04** (471.01)	225.48 (178.65)	1176.52** (502.77)
B. Weighted					
Employment	0.381* (0.221)	0.590** (0.220)	0.131 (0.277)	0.209** (0.103)	0.078 (0.267)
Log hourly wage (employed)	0.345 (0.433)	0.113 (0.409)	0.179 (0.450)	-0.232 (0.147)	-0.411 (0.430)
Log earnings (employed)	0.948 (0.646)	0.629 (0.608)	-0.354 (0.644)	-0.319 (0.229)	0.035 (0.619)
Annual hours	927.61* (491.49)	1189.30** (454.15)	-674.35 (523.48)	261.69 (172.08)	936.04* (528.44)
C. Black only, unweighted [N=643, 341 low-ed, 302 high-ed] This number is maximum, fewer in some regressions					
Employment	0.331 (0.567)	0.428 (0.320)	0.026 (0.656)	0.097 (0.532)	0.071 (0.461)
Log hourly wage (employed)	0.075 (1.047)	0.506 (0.635)	0.885 (1.098)	0.431 (0.851)	-0.454 (0.675)
Log earnings (employed)	1.423 (1.818)	0.769 (0.870)	0.012 (1.694)	-0.655 (1.645)	-0.667 (0.863)
Annual hours	1891.41 (1405.05)	1665.27** (680.75)	-1118.70 (1468.48)	-226.14 (1023.24)	892.56 (854.60)
D. Non-black only, unweighted [N=1193, 433 low-ed, 760 high-ed]					
Employment	0.301 (0.324)	0.467 (0.308)	-0.127 (0.366)	0.167 (0.122)	0.294 (0.347)
Log hourly wage (employed)	0.911 (0.553)	0.789 (0.518)	0.313 (0.604)	-0.122 (0.158)	-0.434 (0.578)
Log earnings (employed)	1.419 (0.880)	1.141 (0.835)	-1.106 (1.236)	-0.277 (0.220)	0.828 (1.237)
Annual hours	825.55 (590.76)	946.02* (524.16)	-1633.04** (765.13)	120.46 (200.51)	1753.50** (746.88)
See notes to Tables 4, 5A, and 5B.					

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

	Difference between early children (22, 24)/never married, and early children/always married	Difference between: early children (22, 24)/never married and late children (30, 32)/always married
Corresponds to:	(1)	(2)
Table 5B (base specification, differences relative to high-education women)	1.915 (3.523)	1.208 (3.034)
Table 6 Panel B (age of children only)	2.083 (2.788)	1.896 (2.556)
Table 6 Panel C (age of woman only)	-0.892 (3.280)	-2.417 (3.067)
Table 6 Panel D (neither age of children nor age of woman)	1.144 (2.398)	0.733 (2.122)
Table 7 Panel A (base specification, age 38 sample and outcome)	1.714 (1.712)	1.284 (1.680)
Table 7 Panel B (base specification, age 39 sample and outcome)	1.194 (2.307)	0.524 (2.239)
Table 7 Panel D (base specification, age 41 sample and outcome)	2.575 (3.644)	1.619 (3.360)
Table 7 Panel E (base specification, age 42 sample and outcome)	2.363 (3.884)	1.221 (3.806)
Table 8 Panel B (base specification, fixing state at age 22)	2.376 (3.607)	1.301 (3.100)
Table 8 Panel C (base specification, federal EITC only)	1.817 (3.840)	1.423 (3.331)
Table 9 Panel B (max credit as policy)	1.648 (2.486)	1.015 (2.135)
Table 10 Panel B (base specification, adding post-welfare reform control)	2.167 (3.581)	2.044 (3.612)
Table 10 Panel C (base specification, adding more complete post-welfare reform control)	5.484 (4.791)	4.656 (4.043)
Table 11 Panel B (base specification, weighted)	1.474 (3.605)	1.648 (3.544)
Table 11 Panel C (base specification unweighted, black)	0.039 (13.744)	5.056 (10.464)
Table 11 Panel D (base specification unweighted, non-black)	-0.577 (5.051)	-1.953 (4.918)

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

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

Comparisons:	Early children (22, 24), never married vs. Early children (22,24), always married	Early children (22, 24), never married vs. Late children (30, 32), always married	Early children (22,24), always married vs. Late children (30, 32), never married	Early children (22,24), always married vs. Late children (30, 32), always married	Late children (30, 32), never married vs. Late children (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Baseline					
Employment	0.134 (0.152)	0.284* (0.160)	0.087 (0.211)	0.150 (0.105)	0.063 (0.212)
Log hourly wage (employed)	0.549* (0.315)	0.366 (0.265)	-0.512 (0.353)	-0.183 (0.153)	0.329 (0.342)
Log earnings (employed)	1.307*** (0.427)	1.003*** (0.368)	-1.325*** (0.457)	-0.303 (0.205)	1.022** (0.470)
Annual hours	919.49** (394.99)	1144.97*** (369.30)	-951.04** (471.01)	225.48 (178.65)	1176.52** (502.77)
B. Low-ed Only					
Employment	0.020 (0.164)	-0.042 (0.148)	-0.079 (0.162)	-0.062 (0.098)	0.017 (0.156)
Log hourly wage (employed)	0.145 (0.282)	-0.096 (0.265)	-0.380 (0.317)	-0.241** (0.117)	0.139 (0.318)
Log earnings (employed)	0.306 (0.425)	-0.045 (0.419)	-0.673 (0.503)	-0.351** (0.153)	0.322 (0.526)
Annual hours	243.04 (410.37)	119.42 (418.59)	-402.47 (435.74)	-123.62 (187.94)	278.85 (470.90)
C. High-ed Only					
Employment	-0.080 (0.152)	-0.269** (0.131)	-0.167 (0.158)	-0.190*** (0.052)	-0.023 (0.144)
Log hourly wage (employed)	-0.354 (0.225)	-0.379** (0.160)	0.188 (0.258)	-0.025 (0.114)	-0.213 (0.202)
Log earnings (employed)	-0.947*** (0.330)	-0.978*** (0.231)	0.690* (0.360)	-0.031 (0.163)	-0.721** (0.287)
Annual hours	-630.96** (306.85)	-960.74*** (238.13)	588.40** (255.09)	-329.78*** (115.39)	-918.18*** (238.41)

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