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

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ABSTRACT

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

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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), while some work has also studied short term effects of the EITC on poverty (e.g., Neumark and Wascher, 2011). The evidence from this literature establishes that a more generous EITC – using both federal and state variation – increases employment of those most affected, like single mothers, and, via work incentives, reduces poverty even without taking account of the income from the credit. 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 children (e.g., Hoynes et al., 2015), and positive impacts on women's 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). The authors compared 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.

[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.

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) 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).

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 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,

[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 even 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.

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. 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 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

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

the variation in shocks or unobservables across states and years that are correlated with the relevant policy variation – the most important factor that could otherwise lead to bias in the estimate of δ.[8]

As always, we cannot distinguish between a true effect of the EITC on women with children and shocks that vary by state and year *and* children. The identifying assumption is that the shocks are the 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.[9] 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.[10]

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- 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.

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[8] This greater parsimony becomes valuable given the data set we use, which does not lead to large samples.

[9] 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.

[10] 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.

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:

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

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 pre-school 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:

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

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). 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

[11] 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.

of different skill levels.[12] Thus, this specification provides our most compelling identification. Denoting low education by LE, our specification with these distinctions becomes:

$$
\begin{aligned}
(4)\ Y_{ijt} = \alpha &+ \beta^{U}CR_{jt}\cdot U_{ijt}\cdot LE_{ij} + \gamma^{UY}YK_{ijt}\cdot U_{ijt}\cdot LE_{ij} + \gamma^{UO}OK_{ijt}\cdot U_{ijt}\cdot LE_{ij} \\
&+ \delta^{UY}CR_{jt}\cdot YK_{ijt}\cdot U_{ijt}\cdot LE_{ij} + \delta^{UO}CR_{jt}\cdot OK_{ijt}\cdot U_{ijt}\cdot LE_{ij} \\
&+ \beta^{M}CR_{jt}\cdot M_{ijt}\cdot LE_{ij} + \gamma^{MY}YK_{ijt}\cdot M_{ijt}\cdot LE_{ij} + \gamma^{MO}OK_{ijt}\cdot M_{ijt}\cdot LE_{ij} \\
&+ \delta^{MY}CR_{jt}\cdot YK_{ijt}\cdot M_{ijt}\cdot LE_{ij} + \delta^{MO}CR_{jt}\cdot OK_{ijt}\cdot M_{ijt}\cdot LE_{ij} \\
&+ \beta^{U'}CR_{jt}\cdot U_{ijt} + \gamma^{UY'}YK_{ijt}\cdot U_{ijt} + \gamma^{UO'}OK_{ijt}\cdot U_{ijt} \\
&+ \delta^{UY'}CR_{jt}\cdot YK_{ijt}\cdot U_{ijt} + \delta^{UO'}CR_{jt}\cdot OK_{ijt}\cdot U_{ijt} \\
&+ \beta^{M'}CR_{jt}\cdot M_{ijt} + \gamma^{MY'}YK_{ijt}\cdot M_{ijt} + \gamma^{MO'}OK_{ijt}\cdot M_{ijt} \\
&+ \delta^{MY'}CR_{jt}\cdot YK_{ijt}\cdot M_{ijt} + \delta^{MO'}CR_{jt}\cdot OK_{ijt}\cdot M_{ijt} \\
&+ \omega M_{ijt}\cdot LE_{ij} + \omega' M_{ijt} + \mu LE_{ij} + D_j\theta + D_t\lambda + \varepsilon_{ijt}\ .
\end{aligned}
$$
[13]

In this case, we introduce the interactions with LE, and the coefficients on these interactions are the parameters of interest.[14]

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 define these over ages 22-39, and we compute the averages of the interactions for the policy, childbearing, and marital status variables over these ages.

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

[12] 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.

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

[14] 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).

(5) $\delta^{UY} \cdot \{ \sum_{a=t\text{-}18}^{t\text{-}1} (CR_{ja} \cdot YK_{ija} \cdot U_{ija})/18\} \cdot LE_{ij}$,

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. We substitute these expressions into equation (3) (or (4)) to estimate the effects of these longer-run exposure variables on outcomes at age 40.[15]

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

The spirit of our approach is to apply the quasi-experimental framework so commonly used for policy evaluation to estimating 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 of 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^{UY}, δ^{UO}, δ^{MY}, δ^{MO} in our longer-term version of equation (3) (or (4)). To clarify, as an example, δ^{UY} is the coefficient on the average, over ages 22-39, of the interaction between the two-kid phase-in rate, a dummy variable for having young kids, and a dummy variable for being unmarried, in equation (3); in equation (4) it is the

[15] In one of our robustness checks we define the variables over ages 22-34, and look at outcomes at age 35.

coefficient of the interaction between this average and the indicator for low education.

III. Data

PSID Data

The data for this paper come from the Panel Study of Income Dynamics (PSID), using data through the 2015 survey. We need to observe long longitudinal records on women, because their "exposure" to the EITC, as explained in Section II, depends on where they live,[16] as well as their marital and childbearing history. 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 the whole history beginning at age 22.[17,18] 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.[19] We assign full 19-year histories for all the necessary variables: marital status, number of children, age of children, and employment. Additionally, we need information on race and education, but these are not assigned on a year-by-year basis.

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

[16] Below, we consider the possibility that migration is endogenous.
[17] Due to other data restrictions the eventual time range will be all women who are observed at age 40 from 1996 to 2014. For one analysis we study outcomes at age 35, for women observed at age 35 from 1991 to 2014.
[18] 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.
[19] 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.

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. 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 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.[20] 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

[20] Hispanic ethnicity cannot be coded consistently.

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.[21] 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 822 women in our final low-education sample.

Policy Variation

Information on the EITC comes from a database of historical parameters maintained by the Tax Policy Center.[22] 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.

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

[21] 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.
[22] See http://www.taxpolicycenter.org/sites/default/files/legacy/taxfacts/content/PDF/historical_eitc_parameters.pdf (viewed October 11, 2016).

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.[23] 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. 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

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

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. To provide some information on the evolution of the marriage and children variables as women age, we break the sample into means calculated over ages 22-29 and 30-39. We also show these for the low-education and high-education subsamples. For both low-education and high-education women, the proportion of years with kids overall is higher from ages 30-39, although the difference is larger for more-educated women, consistent with later childbearing. Naturally, the proportions of years married is higher in the older age range, and we also see that the proportion of years married is higher for the high-educated women. 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.[24]

The variables highlighted in boldface are the key ones that identify the DDD estimates – the averages of the interactions between EITC variation, the proportions of years married or unmarried, and the proportions of years with or without children (or young versus old children). The means are quite low, reflecting the fact that these variables are averages of triple interactions between a credit rate that is well below one and dummy variables that are frequently zero; this is important to keep in mind when

[24] 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, as we do here, reporting both unweighted and weighted estimates. (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.)

interpreting the regression estimates, as discussed below.

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 22-39, 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 column (1), we obtain positive estimates of the effect of the EITC for women exposed to a more generous EITC when unmarried with young children or older children, and negative estimates for women exposed to a more generous EITC when they are married with either young children or older children – with a larger and marginally statistically significant effect in the former case. The signs of all four estimates are consistent with the predicted effects of the EITC in a dynamic setting; 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).[25] The estimates in column (1) simply reflect the accumulation of these static or contemporaneous effects across many years, and the cumulative effect may be stronger than the often weak evidence of negative short-run labor supply effects for married women (e.g., Eissa and Hoynes, 2004). The evidence of a larger negative effect for married women when they have young kids is consistent with a higher reservation wage for such women (compared to those with older kids), which makes labor market withdrawal in response to an income effect more likely.[26]

The magnitudes of these estimated longer-run effects are tricky to interpret, for a couple of

[25] 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.
[26] 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.

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. These amount to multiplying the coefficients by 0.1, and then dividing by 18 (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 0.044 in square brackets implies that a 10 percentage-point increase in the phase-in rate for one year results in 0.044 years of additional cumulative experience, for hypothetical women who always have young children and are always unmarried. This seems like a large effect. Suppose that 10 percent of women work one additional year because of the higher EITC in place for one year.[27] Then over 18 years, the average effect on cumulative experience would be 0.006 years, or about one-seventh of the 0.044 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.

Column (2) reports the estimated effect on employment at age 40. The estimates are not

[27] 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).

statistically significant, and in this column the sign pattern does 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, with three out of the four estimates for exposure to a more generous EITC when unmarried with children positive (both positive for exposure with young children), and all four of the estimates for exposure when married with children negative. The earnings effects are larger, and the negative estimates for exposure when married (with either younger or older children) are statistically significant.

The differences between the earnings and wage estimates are driven at least in part by the hours effects reported in column (5), which indicate positive effects of hours at age 40 for exposure to a more generous EITC when unmarried with young children, and negative effects for exposure when married with children (regardless of age). 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.

For unmarried women with young children, the implied effect of a one-year, 10 percentage-point, increase in the phase-in rate is 1.6 percent for hourly wages and 3.4 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 kids 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 kids 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 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.).[28] 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 3.6 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 statistically insignificant. However, 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

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

unmarried mothers with young children, relative to women who had children early but were always married (e.g., the 0.267 estimate in the second row of column (2) of Panel B), and relative to women who were always married but had children later (the 0.220 estimate the same column and panel). For earnings, in contrast to wages, the estimates in column (2) for never married women who had children early, relative to always married women regardless of when they had children (the 1.011 and 0.755 estimates), are statistically significant (at the 10-percent level). The estimated effects comparing never married women who had children early versus late are close to zero 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 76 to 101 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), although insignificant, point to positive hours effects for the corresponding comparisons, which would of course boost earnings.[29] Regardless, these estimates are 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 (−0.937) – for earnings, which is also consistent with the estimated negative hours effect.[30] In column (4) we find a positive and significant earnings effects for never married women who have children late versus always married women who have children late.

[29] There is stronger evidence of hours effects in subsequent tables.
[30] We would also expect a negative effect relative to women who have kids 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.

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, have the expected signs – positive for never married women and negative for always married women.

Overall, then, there is a fair amount of 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 – probably mainly from intensive margin effects – for married women.

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 kids/never married and early kids/always married; and the difference between early kids/never married and late kids/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 (2.868), 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.[31] 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.

Additional Analyses

We next turn to analyses intended to probe the robustness and credibility of the results. We go

[31] However, for all the analyses 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.

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. There are some cases where the results become weaker, and some cases where they become stronger. And we also find some evidence that there are some differences between black and non-black women as to which women – distinguished by having children early or late – drive the results.

Table 6 presents the estimates – corresponding to Table 5B – that result from estimating the long-run exposure version of equation (3) instead of equation (4). This is, we drop the high-education observations (and the education interactions). Hence, in this table we no longer rely on differences between less-educated and more-educated women to identify the effects of exposure to the EITC, which lets us gauge the extent to which the estimates are identified just from variation in the behavior of less-educated women. (Of course, the observations on more-educated women may be useful to control for other sources of correlation common to both the EITC and the outcomes we study, to the extent they are common for more-educated and less-educated women.)

The estimates in Table 6 are qualitatively similar to those in Table 5B, although the point estimates tend to be a bit smaller, and the results not statistically significant. Nonetheless, we still find positive estimates for the relative effects on never married mothers who have children early relative to always married mothers who have children early or late (e.g., the 0.680 and 0.440 estimates for earnings in column (2) of Panel C). And we find negative estimates of longer-term exposure to a more generous EITC for always married women who have children early relative to either never married women or always married women who have children late. The implication is that a good part of our estimated effects from the baseline specification, in Table 5B, are driven primarily by less-educated women.

As further evidence that low-education women drive our results, Table 7 instead presents estimates from equation (3) applied to high-education women only. These estimates are often small and near zero. And in the couple of cases where the estimates are larger or, in one case, statistically significant (column (4)), the estimates are the opposite sign and smaller than in Table 5B, indicating that – as comparing Tables 5B and 6 also shows – estimating long-run EITC effects relative to high-education women enhances the estimated effects, but the estimates are not primarily driven by the high-education women. Of course, the results for high-education women can serve as a placebo test, in a sense, because we expect highly-education women to be little affected by the EITC. The small and sometimes opposite-signed effects in Table 7 indicate that we are not estimating spurious effects of the EITC.

We next revert to using both low- and high-education women, using the long-run version of equation (4), but we capture the EITC, childbearing, and marriage histories through age 34 and study outcomes at age 35. We do this to assess the robustness of the findings to measuring "adult" outcomes at different ages,[32] although we might expect somewhat muddier results because a good share of women may still have young children from ages 35-39, and we do not capture these effects.[33] In this case, we report the equivalent estimates to both Tables 5A and 5B, in Tables 8A and 8B. The patterns of estimates are generally similar, but for wages and earnings usually somewhat smaller and less likely to be statistically significant. However, one interesting difference from comparing Tables 5B and 8B is that the hours effects are stronger at age 35, which perhaps indicates that longer-term effects on wages and earnings are not as strongly determined at age 35 because of other differences in behavior in the shorter-term that could manifest in wages and earnings in subsequent years.

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 Table 9, 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. (We revert to measuring outcomes at age 40.) The estimates from this analysis are very similar to those in Table 5B.

[32] We keep the sample the same to focus on the age difference. Using an age older than 40 would shrink the already small sample.
[33] Table 4 shows that, from ages 30-39, 27 percent of years of low-education women are spent with young children.

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 Table 10, are also very similar. Thus, we conclude that migration does not bias our estimated effects.[34] 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.

In Tables 11A and 11B 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). In Table 11A, we interact this with a dummy for whether women have children, and average this over ages 22-39. Thus, this variable effectively picks up the share of years women both had children and were exposed to welfare reform. We introduce this variable, as we do our other policy variables, alone and interacted with a dummy variable for low-education women. The estimates in Table 11A indicate that including the effect of welfare reform in this way has virtually no impact on the estimates, and in fact strengthens the evidence for some of the differences – as a comparison with Table 5B shows.

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 kids and married/unmarried, and then averaged

[34] 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 again focusing on the interactions with the low-education indicator). The results for this specification are reported in Table 11B. The point estimates are qualitatively similar, but the standard errors are a good deal larger, which is not surprising since we soak up the average EITC differences before and after 1996, which as Figure 1 shows are substantial. However, the similarity of the point estimates leads us to conclude that our estimates are not spuriously driven by welfare reform.[35]

Our final analyses concerns 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. Table 12 reports results for the baseline specification and sample, but weighting by the Core sample weights. With the weights, the estimates in column (2) – which compare never married women who had children early to other women, most importantly always married women who had children early or late – are a good deal smaller, no longer statistically significant, and for wages, more likely to be negative where we would expect, and previously found, positive effects. On the other hand, the estimates in columns (3) and (4) for wages, earnings, and hours, are more robust. These compare, for example, always married women who had children early to never married women who had children late (−0.792, for earnings), and never married women who had children late to always married women who had children late (0.608, for earnings). Thus, it is only the results for the never married, early child bearers that are not robust to the weighting.

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 the sensitivity of the weighted estimates could to some extent reflect differences in effects between blacks and non-blacks. This is confirmed by the estimates in Tables 13 and 14 – for non-blacks and blacks

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

separately.[36] The estimates for non-blacks, in Table 13, tend to replicate, qualitatively, the overall and the weighted results for columns (3) and (4) – the comparisons that exclude the never married women who have children early. However, the estimated effects are larger; compare, for example, the estimate of 0.681 in the last row of Panel C, column (4) in Table 5B (for earnings), with the corresponding estimate of 0.771 in Table 13. The results in column (2), based on comparisons for the never married women who have children early, are in the same direction as in Table 5B, but smaller in magnitude and not statistically significant. In contrast, the estimates in Table 14, for blacks, provide evidence more similar to Table 5B, for wages and earnings, for the comparisons that involve never married women who have children early.

To some extent, then, the evidence of positive long-run effects of exposure to a more generous EITC for black women comes from exposure of unmarried women who have children early. In contrast, the effects for non-black women come more from differences between married and unmarried women who have children at later ages. However, the samples become particularly small when we disaggregate by race, so we are cautious about over-interpreting this difference.

VII. Conclusions

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

We find some 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

[36] Given our concerns about the weights, and that the oversampling is strongly reflected in race, we do not weight the separate black and non-black samples. When we did, the differences between the results for blacks and non-blacks were not as strong as reported here, but were in the same direction.

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.[37]

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.

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

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Figure 1: Federal EITC Phase-In Rates (%)

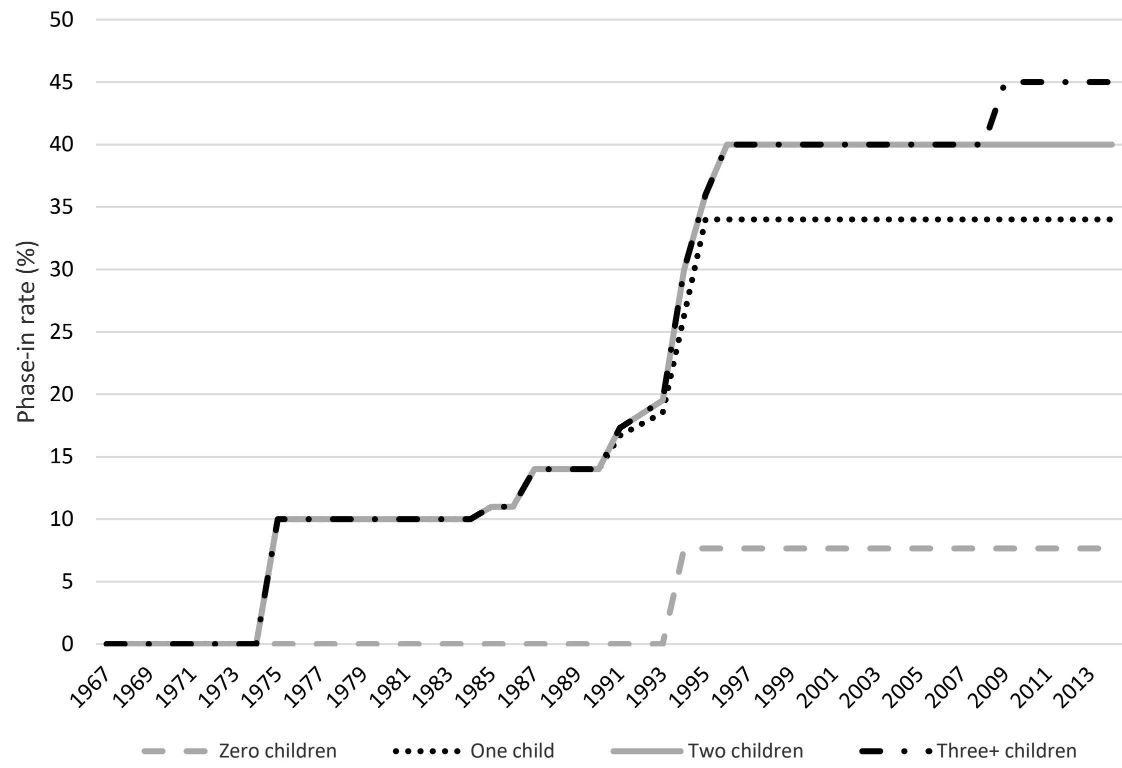

Figure 2: State EITC Supplements (%)

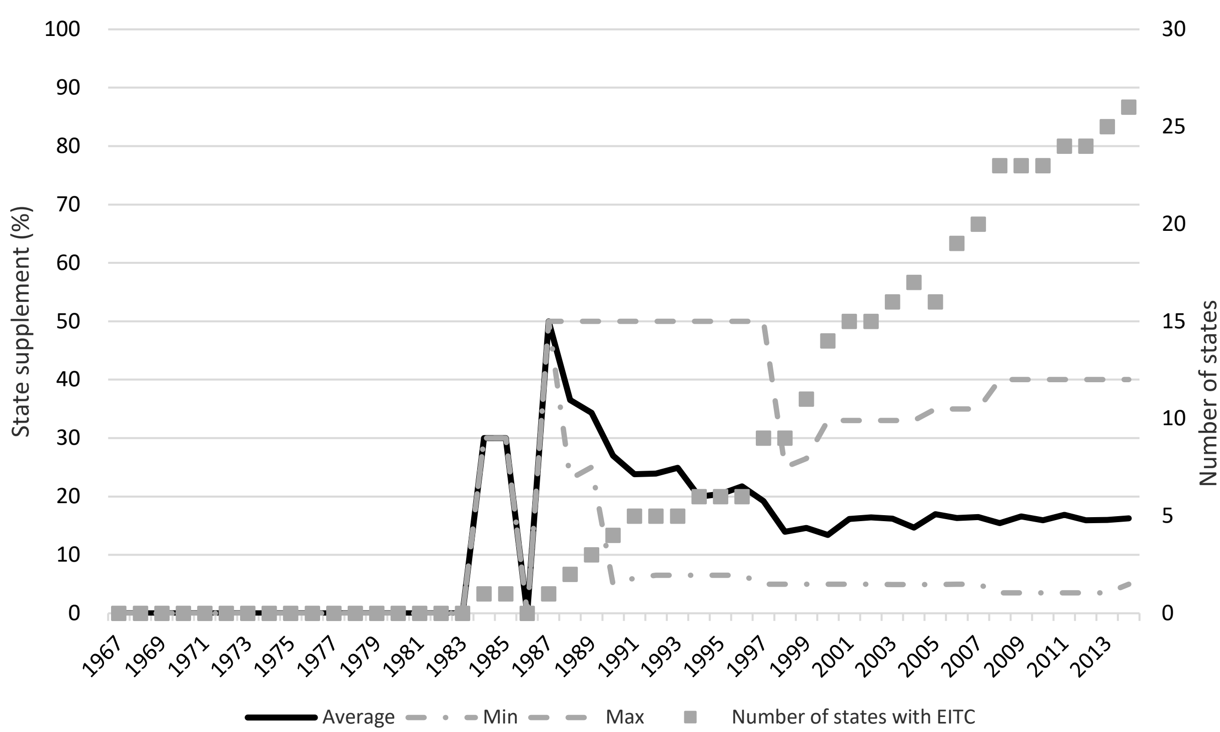

Table 1: Sample Construction Description

	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 kids history	2,291
Number of women in D with full 19-year age of kids 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
F. Number of women in D who fit all the above criteria simultaneously (final sample)	1,901
G. Number of low-educ. (LTHS or HS) women who fit all the above criteria simultaneously	822
H. Number of high-educ. (beyond HS) women who fit all the above criteria simultaneously	1,079

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
Calendar year at age 40	N/A	N/A	N/A	2003	N/A
Federal EITC phase-in rate, 2 kids	0.27	0.19	0.33	0.40	0.30
State EITC supplement percentage, 2 kids	0.02	0.02	0.03	0.04	0.03
Combined EITC phase-in rate, 2 kids	0.28	0.19	0.34	0.42	0.31
Prop. years with young kids	0.38	0.52	0.27	0.09	0.39
Prop. years with older kids	0.46	0.25	0.63	0.83	0.26
Prop. years unmarried	0.38	0.43	0.34	0.35	0.34
Prop. years married	0.62	0.57	0.66	0.65	0.66
Black	N/A	N/A	N/A	0.46	0.28
Avg. (2-kid phase-in rate x young kids x unmarried)	0.03	0.04	0.03	0.01	0.02
Avg. (2-kid phase-in rate x older kids (only) x unmarried)	0.06	0.03	0.08	0.12	0.03
Avg. (2-kid phase-in rate x young kids x married)	0.06	0.06	0.06	0.02	0.10
Avg. (2-kid phase-in rate x older kids (only) x married)	0.09	0.03	0.14	0.23	0.06
Avg. (2-kid phase-in rate x unmarried)	0.11	0.09	0.12	0.15	0.10
Avg. (2-kid phase-in rate x married)	0.17	0.10	0.22	0.27	0.21
Experience (cumulative years employed)	11.41	4.55	6.83	0.71	13.85
Annual hours at age 40	N/A	N/A	N/A	1394	N/A
Log wage (employed) at age 40	N/A	N/A	N/A	2.52	N/A
Log earnings (employed) at age 40	N/A	N/A	N/A	9.85	N/A

"2-kid 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 Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Low-Education Sample Relative to High-Education Sample

	Cumulative experience	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
Interactions with low-education:	(1)	(2)	(3)	(4)	(5)
Avg. (2-kid phase-in rate x young kids x unmarried, 22-39)	7.883 (23.758) [0.044]	-0.562 (1.511) [-0.003]	2.940 (3.371) [0.016]	6.052 (4.681) [0.034]	2213.584 (3178.121) [12.298]
Avg. (2-kid phase-in rate x older kids (only) x unmarried, 22-39)	8.356 (29.956) [0.046]	0.914 (1.463) [0.005]	-0.275 (3.944) [-0.002]	1.663 (4.916) [0.009]	-67.520 (3812.365) [-0.375]
Avg. (2-kid phase-in rate x young kids x married, 22-39)	-30.194* (17.313) [-0.168]	-0.599 (1.494) [-0.003]	-2.091 (2.960) [-0.012]	-7.427* (3.867) [-0.041]	-6171.05 (3174.377) [-34.284]
Avg. (2-kid phase-in rate x older kids (only) x married, 22-39)	-12.798 (13.861) [-0.071]	1.453 (1.244) [0.008]	-1.062 (1.824) [-0.006]	-5.756* (3.315) [-0.032]	-180.268 (2802.197) [-1.001]
R^2	0.1972	0.0885	0.2743	0.2013	0.1131
N, low-education	646	822	639	640	822
N, high-education	697	1079	905	905	1079

See notes to Table 4. 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 22-39; 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 kids, at age 40, and corresponding main effects;
(3) dummy variable for black;
(4) state and year fixed effects;
(5) all controls in (1)-(3) interacted with low-education indicator; the latter are reported. In addition, the main effect of low-education is included.
Years with young kids are defined as years when the youngest child born to the woman under age 6, while years with older kids 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 0.1/18).
***/**/* Significantly different from zero at 1/5/10 percent level.

Table 5B: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate Implied by Table 5A Estimates
(Age 40, Low-Education Sample, Estimated Relative to High-Education Sample)

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment (Table 5A, col. (2))					
Estimate	0.036 (0.069)	0.026 (0.115)	0.054 (0.122)	-0.015 (0.072)	-0.010 (0.076)
Difference from early kids, always married	N/A	-0.028 (0.184)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.041 (0.065)	0.069 (0.156)	N/A	N/A
Difference from late kids, always married	N/A	0.036 (0.148)	0.065 (0.055)	-0.004 (0.115)	N/A
B. Log hourly wage (employed) (Table 5A, col. (3))					
Estimate	-0.055 (0.123)	0.115 (0.220)	-0.152 (0.217)	0.128 (0.141)	-0.105 (0.147)
Difference from early kids, always married	N/A	0.267 (0.347)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.012 (0.175)	-0.280 (0.282)	N/A	N/A
Difference from late kids, always married	N/A	0.220 (0.288)	-0.047 (0.081)	0.232 (0.224)	N/A
C. Log earnings (employed) (Table 5A, col. (4))					
Estimate	-0.260 (0.168)	0.361 (0.305)	-0.650* (0.331)	0.287 (0.203)	-0.394* (0.200)
Difference from early kids, always married	N/A	1.011* (0.530)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.074 (0.218)	-0.937** (0.401)	N/A	N/A
Difference from late kids, always married	N/A	0.755* (0.415)	-0.256* (0.147)	0.681** (0.294)	N/A
D. Annual Hours (Table 5A, col. (5))					
Estimate	-140.236 (147.628)	94.630 (295.807)	-284.284 (278.372)	97.631 (161.861)	-276.272 (165.964)
Difference from early kids, always married	N/A	378.914 (169.438)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-3.001 (169.438)	-381.915 (372.872)	N/A	N/A
Difference from late kids, always married	N/A	370.902 (386.669)	-8.012 (124.542)	373.903 (274.007)	N/A

See notes to Tables 4 and 5A. These results are based on the long-run exposure version of equation (4).

Table 6: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Estimated Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Low-Education Sample

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Estimate	-0.062 (0.069)	-0.086 (0.108)	-0.064 (0.124)	-0.051 (0.070)	-0.059 (0.067)
Difference from early kids, always married	N/A	-0.021 (0.180)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.035 (0.049)	-0.013 (0.160)	N/A	N/A
Difference from late kids, always married	N/A	-0.026 (0.139)	-0.005 (0.062)	0.008 (0.111)	N/A
B. Log hourly wage (employed)					
Estimate	-0.100 (0.131)	0.098 (0.184)	-0.237 (0.232)	0.031 (0.100)	-0.114 (0.150)
Difference from early kids, always married	N/A	0.335 (0.325)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.067 (0.120)	-0.268 (0.279)	N/A	N/A
Difference from late kids, always married	N/A	0.212 (0.247)	-0.123 (0.092)	0.145 (0.201)	N/A
C. Log earnings (employed)					
Estimate	-0.227 (0.194)	0.170 (0.245)	-0.510 (0.371)	0.033 (0.173)	-0.270 (0.215)
Difference from early kids, always married	N/A	0.680 (0.506)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.137 (0.111)	-0.542 (0.447)	N/A	N/A
Difference from late kids, always married	N/A	0.440 (0.366)	-0.240 (0.168)	0.302 (0.306)	N/A
D. Annual Hours					
Estimate	-255.094 (137.877)	-196.961 (258.352)	-291.112 (255.968)	-126.642 (168.370)	-256.641* (134.772)
Difference from early kids, always married	N/A	94.151 (414.627)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-70.319 (106.352)	-164.470 (350.996)	N/A	N/A
Difference from late kids, always married	N/A	59.680 (324.258)	-34.471 (131.917)	129.998 (247.555)	N/A

See notes to Tables 4, and 5A, and 5B. The only difference is that only low-educated women are included, and the controls are not interacted with a low-education indicator (and the main effect of low-education is dropped); these results are based on the long-run exposure version of equation (3).

Table 7: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Estimated Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, High-Education Sample

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Estimate	-0.079* (0.045)	-0.097* (0.049)	-0.091 (0.079)	-0.027 (0.041)	-0.042 (0.043)
Difference from early kids, always married	N/A	-0.006 (0.097)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.071 (0.036)	-0.064 (0.102)	N/A	N/A
Difference from late kids, always married	N/A	-0.056 (0.068)	-0.049 (0.045)	0.015 (0.068)	N/A
B. Log hourly wage (employed)					
Estimate	0.020 (0.074)	0.040 (0.119)	0.004 (0.121)	-0.073 (0.091)	0.018 (0.069)
Difference from early kids, always married	N/A	0.036 (0.178)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.113 (0.098)	0.076 (0.178)	N/A	N/A
Difference from late kids, always married	N/A	0.021 (0.144)	-0.015 (0.062)	-0.091 (0.130)	N/A
C. Log earnings (employed)					
Estimate	0.097 (0.126)	-0.064 (0.191)	0.188 (0.192)	-0.184 (0.112)	0.147 (0.110)
Difference from early kids, always married	N/A	-0.251 (0.261)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.121 (0.173)	0.372 (0.247)	N/A	N/A
Difference from late kids, always married	N/A	-0.211 (0.212)	0.040 (0.093)	-0.332* (0.170)	N/A
D. Annual Hours					
Estimate	-75.978 (94.312)	-260.624 (158.307)	-6.879 (147.863)	-205.106** (98.288)	17.824 (82.414)
Difference from early kids, always married	N/A	-253.745 (218.925)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-55.517 (117.690)	198.228 (199.770)	N/A	N/A
Difference from late kids, always married	N/A	-278.447 (178.511)	-24.702 (79.559)	-222.930 (137.201)	N/A
See notes to Tables 4, 5A, 5B, and 6.					

Table 8A: Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 35, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Low-Education Sample Relative to High-Education Sample

	Cumulative experience	Employment	Log hourly wage (employed)	Log earnings (employed)	Annual hours
Interactions with low-education:	(1)	(2)	(3)	(4)	(5)
Avg. (2-kid phase-in rate x young kids x unmarried, 22-34)	17.759 (11.771) [0.099]	-0.365 (1.387) [-0.002]	0.609 (2.543) [0.003]	3.027 (3.918) [0.017]	3667.047 (3294.576) [20.372]
Avg. (2-kid phase-in rate x older kids (only) x unmarried, 22-34)	0.421 (16.103) [0.002]	-0.380 (1.577) [-0.002]	0.346 (2.782) [0.002]	-1.661 (3.790) [-0.009]	-1682.000 (3000.001) [-9.344]
Avg. (2-kid phase-in rate x young kids x married, 22-34)	-23.488** (11.255) [-0.130]	-2.396* (1.246) [-0.013]	-0.877 (1.509) [-0.005]	-3.596 (2.469) [-0.020]	-6246.002** (2375.173) [-34.700]
Avg. (2-kid phase-in rate x older kids (only) x married, 22-34)	-11.876 (11.033) [-0.066]	-0.651 (1.109) [-0.004]	-1.167 (1.849) [-0.006]	0.444 (2.678) [0.002]	-1419.320 (2095.444) [-7.885]
R^2	0.2291	0.0818	0.3141	0.2205	0.1557
N, low-education	650	822	629	632	790
N, high-education	699	1079	873	875	1028

See notes to Tables 4 and 5A. The sample is the same, but outcomes are measured at age 35 and variables are accumulated over ages 22-34. Some sample sizes differ slightly from Table 5A because of whether observations are available at ages 35 or 40.

Table 8B: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate Implied by Table 5A Estimates (Age 35, Low-Education Sample, Estimated Relative to High-Education Sample)					
Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment (N = 1,901)					
Estimate	-0.099 (0.069)	-0.027 (0.091)	-0.125 (0.076)	-0.010 (0.039)	-0.067* (0.035)
Difference from early kids, always married	N/A	0.098 (0.115)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.017 (0.059)	-0.114 (0.086)	N/A	N/A
Difference from late kids, always married	N/A	0.040 (0.092)	-0.058 (0.045)	0.056 (0.050)	N/A
B. Log hourly wage (employed, N = 1,502)					
Estimate	-0.040 (0.080)	0.037 (0.158)	-0.071 (0.100)	0.017 (0.071)	-0.024 (0.042)
Difference from early kids, always married	N/A	0.108 (0.213)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.020 (0.101)	-0.088 (0.140)	N/A	N/A
Difference from late kids, always married	N/A	0.061 (0.168)	-0.047 (0.066)	0.041 (0.086)	N/A
C. Log earnings (employed, N = 1,507)					
Estimate	-0.085 (0.133)	0.088 (0.215)	-0.147 (0.162)	0.084 (0.109)	-0.100 (0.069)
Difference from early kids, always married	N/A	0.236 (0.291)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.004 (0.131)	-0.232 (0.208)	N/A	N/A
Difference from late kids, always married	N/A	0.188 (0.228)	-0.048 (0.102)	0.184 (0.131)	N/A
D. Annual Hours					
Estimate	-192.891 (123.543)	69.535 (268.146)	-356.451* (198.240)	144.291 (164.586)	-293.370** (120.859)
Difference from early kids, always married	N/A	425.987 (361.914)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-74.756 (133.333)	-500.742* (279.248)	N/A	N/A
Difference from late kids, always married	N/A	362.906 (301.034)	-63.081 (93.131)	437.661** (211.967)	N/A
See notes to Tables 4, 5A, and 5B.					

Table 9: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Estimated Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Fixing State at Age 22, Low-Education Sample Relative to High-Education Sample

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Estimate	-0.832 (0.748)	1.121 (2.257)	-1.975 (1.403)	0.544 (1.214)	-1.439 (0.878)
Difference from early kids, always married	N/A	3.096 (3.193)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.577 (1.234)	-2.520 (2.287)	N/A	N/A
Difference from late kids, always married	N/A	2.560 (2.832)	-0.536 (0.636)	1.983 (1.868)	N/A
B. Log hourly wage (employed)					
Estimate	-0.041 (0.124)	0.152 (0.216)	-0.145 (0.218)	0.148 (0.138)	-0.100 (0.147)
Difference from early kids, always married	N/A	0.297 (0.343)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.004 (0.170)	-0.293 (0.284)	N/A	N/A
Difference from late kids, always married	N/A	0.252 (0.284)	-0.046 (0.080)	0.248 (0.226)	N/A
C. Log earnings (employed)					
Estimate	-0.230 (0.177)	0.370 (0.301)	-0.599* (0.340)	0.289 (0.204)	-0.366 (0.204)
Difference from early kids, always married	N/A	0.969* (0.527)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.081 (0.207)	-0.888** (0.414)	N/A	N/A
Difference from late kids, always married	N/A	0.736* (0.409)	-0.232 (0.153)	0.655** (0.306)	N/A
D. Annual Hours					
Estimate	-134.660 (153.568)	9.709 (305.720)	-228.628 (274.340)	50.771 (16.757)	-244.153 (165.037)
Difference from early kids, always married	N/A	238.337 (465.966)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-41.062 (175.856)	-279.399 (369.919)	N/A	N/A
Difference from late kids, always married	N/A	253.862 (387.624)	15.525 (121.887)	294.924 (275.130)	N/A

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

Table 10: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Only Federal 2-Child EITC Phase-In Rate Based on Age of Kids, Low-Education Sample Relative to High-Education Sample

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Estimate	0.047 (0.069)	0.024 (0.124)	0.077 (0.126)	-0.011 (0.078)	0.001 (0.078)
Difference from early kids, always married	N/A	-0.053 (0.201)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.035 (0.068)	0.089 (0.168)	N/A	N/A
Difference from late kids, always married	N/A	0.024 (0.163)	0.077 (0.059)	-0.012 (0.126)	N/A
B. Log hourly wage (employed)					
Estimate	-0.054 (0.113)	0.108 (0.214)	-0.149 (0.201)	0.113 (0.130)	-0.088 (0.140)
Difference from early kids, always married	N/A	0.257 (0.335)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.005 (0.184)	-0.262 (0.269)	N/A	N/A
Difference from late kids, always married	N/A	0.196 (0.283)	-0.061 (0.073)	0.201 (0.217)	N/A
C. Log earnings (employed)					
Estimate	-0.208 (0.180)	0.366 (0.290)	-0.558 (0.350)	0.286 (0.175)	-0.337 (0.212)
Difference from early kids, always married	N/A	0.925* (0.530)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.080 (0.235)	-0.844** (0.399)	N/A	N/A
Difference from late kids, always married	N/A	0.704* (0.415)	-0.221 (0.154)	0.623** (0.291)	N/A
D. Annual Hours					
Estimate	-95.859 (150.990)	64.188 (317.268)	-185.737 (291.703)	81.598 (173.467)	-230.728 (157.282)
Difference from early kids, always married	N/A	249.925 (511.100)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-17.410 (191.359)	-267.335 (402.821)	N/A	N/A
Difference from late kids, always married	N/A	294.916 (421.005)	44.991 (129.427)	312.326 (299.953)	N/A

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

Table 11A: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Estimated Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Low-Education Sample Relative to High-Education Sample, Adding Post-Welfare Reform Control

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Estimate	0.106 (0.124)	0.119 (0.153)	0.132 (0.177)	0.041 (0.090)	0.038 (0.107)
Difference from early kids, always married	N/A	-0.013 (0.180)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.078 (0.081)	0.091 (0.167)	N/A	N/A
Difference from late kids, always married	N/A	0.081 (0.147)	0.094 (0.076)	0.003 (0.114)	N/A
B. Log hourly wage (employed)					
Estimate	0.145 (0.217)	0.379 (0.368)	0.072 (0.266)	0.283 (0.218)	0.034 (0.177)
Difference from early kids, always married	N/A	0.307 (0.355)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.096 (0.217)	-0.211 (0.267)	N/A	N/A
Difference from late kids, always married	N/A	0.345 (0.337)	0.038 (0.098)	0.250 (0.224)	N/A
C. Log earnings (employed)					
Estimate	0.085 (0.329)	0.820 (0.534)	-0.268 (0.426)	0.560* (0.322)	-0.155 (0.261)
Difference from early kids, always married	N/A	1.088** (0.536)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.260 (0.288)	-0.828** (0.384)	N/A	N/A
Difference from late kids, always married	N/A	0.976** (0.484)	-0.112 (0.179)	0.716** (0.293)	N/A
D. Annual Hours					
Estimate	74.241 (294.180)	380.952 (419.368)	-49.378 (409.416)	270.480 (232.068)	-128.203 (243.763)
Difference from early kids, always married	N/A	430.331 (471.023)	N/A	N/A	N/A
Difference from late kids, never married	N/A	110.472 (214.623)	-319.859 (393.260)	N/A	N/A
Difference from late kids, always married	N/A	509.155 (405.534)	78.824 (174.213)	398.683 (273.493)	N/A

See notes to Tables 4, 5A, and 5B. The post-welfare reform control is the average, over ages 22-39, of the interaction between a post-1996 dummy variable and a dummy variable for kids; this is included alone, and interacted with low-education.

Table 11B: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Estimated Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Low-Education Sample Relative to High-Education Sample, Adding Post-Welfare Reform Control Separately for Each Kids x Marriage Interaction

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
			A. Employment		
Estimate	0.078 (0.123)	-0.038 (0.190)	0.167 (0.198)	-0.021 (0.131)	0.061 (0.115)
Difference from early kids, always married	N/A	-0.205 (0.278)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.017 (0.096)	0.188 (0.263)	N/A	N/A
Difference from late kids, always married	N/A	-0.099 (0.224)	0.106 (0.101)	-0.082 (0.194)	N/A
			B. Log hourly wage (employed)		
Estimate	0.265 (0.248)	0.329 (0.514)	0.323 (0.311)	0.282 (0.350)	0.138 (0.177)
Difference from early kids, always married	N/A	0.006 (0.556)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.047 (0.344)	0.041 (0.451)	N/A	N/A
Difference from late kids, always married	N/A	0.191 (0.500)	0.185 (0.169)	0.144 (0.387)	N/A
			C. Log earnings (employed)		
Estimate	0.265 (0.335)	0.814 (0.702)	0.087 (0.500)	0.631 (0.474)	-0.070 (0.263)
Difference from early kids, always married	N/A	0.726 (0.885)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.183 (0.405)	-0.544 (0.730)	N/A	N/A
Difference from late kids, always married	N/A	0.884 (0.713)	0.158 (0.282)	0.701 (0.538)	N/A
			D. Annual Hours		
Estimate	112.246 (371.799)	-4.870 (578.932)	237.571 (532.690)	74.090 (306.222)	-27.405 (269.597)
Difference from early kids, always married	N/A	-242.441 (690.555)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-78.961 (332.148)	163.481 (600.146)	N/A	N/A
Difference from late kids, always married	N/A	22.535 (594.405)	264.976 (288.644)	101.495 (408.307)	N/A

See notes to Tables 4, 5A, and 5B. In contrast to Table 11A, in this table the post-welfare reform control (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 four variables capturing years with young kids/old kids and married/unmarried, with and without the low-education interaction.

Table 12: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Estimated Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Low-Education Sample Relative to High-Education Sample, Weighted

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Estimate	0.076 (0.069)	0.142 (0.141)	0.073 (0.110)	0.110 (0.077)	-0.005 (0.071)
Difference from early kids, always married	N/A	0.069 (0.192)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.032 (0.099)	-0.037 (0.141)	N/A	N/A
Difference from late kids, always married	N/A	0.147 (0.165)	0.078 (0.055)	0.115 (0.108)	N/A
B. Log hourly wage (employed)					
Estimate	-0.138 (0.119)	-0.224 (0.270)	-0.108 (0.185)	0.073 (0.175)	-0.096 (0.122)
Difference from early kids, always married	N/A	-0.116 (0.353)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.297* (0.170)	-0.180 (0.273)	N/A	N/A
Difference from late kids, always married	N/A	-0.128 (0.311)	-0.012 (0.080)	0.168 (0.232)	N/A
C. Log earnings (employed)					
Estimate	-0.451** (0.181)	-0.257 (0.397)	-0.638 (0.322)	0.154 (0.238)	-0.454** (0.187)
Difference from early kids, always married	N/A	0.382 (0.580)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.410 (0.291)	-0.792* (0.409)	N/A	N/A
Difference from late kids, always married	N/A	0.198 (0.476)	-0.184 (0.164)	0.608* (0.319)	N/A
D. Annual Hours					
Estimate	-137.928 (156.269)	217.581 (342.716)	-328.823 (292.975)	269.908 (188.250)	-353.299* (176.376)
Difference from early kids, always married	N/A	546.404 (526.104)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-52.327 (220.593)	-598.731 (376.673)	N/A	N/A
Difference from late kids, always married	N/A	570.880 (427.902)	24.476 (136.720)	623.207** (272.892)	N/A

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

Table 13: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Estimated Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Low-Education Sample Relative to High-Education Sample and Unweighted, Non-Black Only

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Estimate	0.037 (0.078)	0.132 (0.191)	0.003 (0.134)	0.092 (0.127)	-0.038 (0.086)
Difference from early kids, always married	N/A	0.128 (0.264)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.040 (0.099)	-0.089 (0.208)	N/A	N/A
Difference from late kids, always married	N/A	0.170 (0.233)	0.042 (0.058)	0.131 (0.172)	N/A
B. Log hourly wage (employed)					
Estimate	-0.077 (0.149)	-0.059 (0.343)	-0.086 (0.191)	0.184 (0.260)	-0.058 (0.129)
Difference from early kids, always married	N/A	0.027 (0.387)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.243 (0.196)	-0.271 (0.320)	N/A	N/A
Difference from late kids, always married	N/A	-0.001 (0.353)	-0.028 (0.082)	0.242 (0.284)	N/A
C. Log earnings (employed)					
Estimate	-0.418* (0.226)	-0.080 (0.521)	-0.669** (0.321)	0.342 (0.364)	-0.429** (0.202)
Difference from early kids, always married	N/A	0.589 (0.629)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-0.423 (0.323)	-1.011** (0.464)	N/A	N/A
Difference from late kids, always married	N/A	0.349 (0.552)	-0.240 (0.150)	0.771* (0.394)	N/A
D. Annual Hours					
Estimate	-198.641 (156.228)	151.354 (409.299)	-410.137 (296.967)	204.739 (255.940)	-372.550** (184.547)
Difference from early kids, always married	N/A	561.491 (600.775)	N/A	N/A	N/A
Difference from late kids, never married	N/A	-53.384 (235.848)	-614.876 (447.607)	N/A	N/A
Difference from late kids, always married	N/A	523.904 (508.973)	-37.587 (129.570)	577.288 (351.668)	N/A

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

Table 14: Differences from Permanent 10 Percentage-Point Increase in Phase-In Rate, Implied by Estimated Long-Run Effects of EITC on Women's Employment, Wages, Earnings, and Hours at Age 40, Using Combined 2-Child Federal and State EITC Phase-In Rates Based on Age of Kids, Low-Education Sample Relative to High-Education Sample and Unweighted, Black Only

Evaluated at/for:	Sample averages	Early kids (22, 24), never married	Early kids (22,24), always married	Late kids (30, 32), never married	Late kids (30, 32), always married
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Estimate	-0.015 (0.192)	-0.061 (0.170)	0.013 (0.350)	-0.075 (0.100)	-0.075 (0.196)
Difference from early kids, always married	N/A	-0.073 (0.412)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.014 (0.095)	0.088 (0.394)	N/A	N/A
Difference from late kids, always married	N/A	0.014 (0.282)	0.087 (0.163)	-0.0003 (0.249)	N/A
B. Log hourly wage (employed)					
Estimate	-0.220 (0.423)	0.007 (0.246)	-0.407 (0.749)	-0.091 (0.228)	-0.267 (0.464)
Difference from early kids, always married	N/A	0.414 (0.778)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.098 (0.099)	-0.316 (0.729)	N/A	N/A
Difference from late kids, always married	N/A	0.275 (0.517)	-0.139 (0.297)	0.176 (0.470)	N/A
C. Log earnings (employed)					
Estimate	-0.284 (0.679)	0.341 (0.352)	-0.712 (1.239)	0.048 (0.287)	-0.372 (0.752)
Difference from early kids, always married	N/A	1.053 (1.332)	N/A	N/A	N/A
Difference from late kids, never married	N/A	0.293* (0.151)	-0.760 (1.253)	N/A	N/A
Difference from late kids, always married	N/A	0.712 (0.866)	-0.341 (0.498)	0.420 (0.780)	N/A
D. Annual Hours					
Estimate	-410.622 (336.691)	132.867 (415.221)	-802.273 (493.795)	65.549 (229.298)	-560.015** (272.529)
Difference from early kids, always married	N/A	935.140* (515.232)	N/A	N/A	N/A
Difference from late kids, never married	N/A	67.318 (228.316)	-867.822* (489.355)	N/A	N/A
Difference from late kids, always married	N/A	692.882 (425.086)	-242.258 (243.884)	625.563* (331.068)	N/A

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 Kids and Marriage

	Difference between early kids (22, 24)/never married, and early kids/always married	Difference between: early kids (22, 24)/never married and late kids (30, 32)/always married
Corresponds to:	(1)	(2)
Table 5B (base specification, differences relative to high-education women)	2.868 (3.136)	2.299 (2.787)
Table 6 (low-education women only)	3.216 (3.216)	2.511 (2.695)
Table 7 (high-education women only)	0.848 (1.593)	0.748 (1.227)
Table 8 (base specification, age 35)	2.516 (1.726)	1.989 (1.489)
Table 9 (base specification, fixing state at age 22)	3.096 (3.193)	2.560 (2.832)
Table 10 (base specification, federal EITC only)	1.720 (3.210)	1.499 (2.869)
Table 11A (base specification, adding post-welfare reform control)	2.652 (3.083)	1.630 (2.712)
Table 11B (base specification, adding more complete post-welfare reform control)	5.120 (4.237)	2.552 (3.821)
Table 12 (base specification, weighted)	2.278 (3.542)	1.868 (3.204)
Table 13 (base specification unweighted, non-black)	4.996 (3.453)	3.908 (2.970)
Table 14 (base specification unweighted, black)	7.102 (5.640)	4.877 (4.079)

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 for column (2), for the “Difference from” entries. And the two columns in this table correspond to the second and fourth rows of column (2) in the earlier tables.