

A Service of

ZBW

Leibniz-Informationszentrum Wirtschaft Leibniz Information Centre for Economics

Shirley, Peter

Article

First-time mothers and the labor market effects of the earned income tax credit

IZA Journal of Labor Policy

Provided in Cooperation with: IZA – Institute of Labor Economics

Suggested Citation: Shirley, Peter (2020) : First-time mothers and the labor market effects of the earned income tax credit, IZA Journal of Labor Policy, ISSN 2193-9004, Sciendo, Warsaw, Vol. 10, Iss. 1, pp. 1-53, https://doi.org/10.2478/izajolp-2020-0007

This Version is available at: https://hdl.handle.net/10419/298750

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.



WWW.ECONSTOR.EU

https://creativecommons.org/licenses/by/4.0/

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



Peter Shirley*

First-time mothers and the labor market effects of the earned income tax credit

Abstract

The Earned Income Tax Credit (EITC) is one of the largest anti-poverty programs in the United States, providing over \$67 billion to more than 27 million families for the tax year 2016, an average of \$2,455. By subsidizing the earnings of low-income workers, the EITC reduces poverty both directly through the credit itself and indirectly through labor supply incentives. The two primary determinants of the amount a tax unit receives are earned income and number of children. Many studies define eligibility based on the presence of children in a household and separate analyses by marital status, a reflection of the fundamentally different incentives the EITC poses for single- and dual-earner households. However, as the EITC theoretically encourages fertility and generally discourages marriage, endogenous responses along these two dimensions could bias estimates which rely on them for identification and sample selection. In this paper, I revisit the classic question of the EITC's labor market effects while exploiting a source of arguably exogenous variation in EITC receipt that does not rely on these potentially endogenous characteristics: birth timing around the end of the calendar year. Using the Survey of Income and Program Participation, my results show positive earnings and employment effects for unmarried women in the 12 months following their first/only child's birth, including for those with a high school degree or less (low-ed). Overall, the results are usually small and insignificant for married women, with the exception of negative (and sometimes significant) effects on earnings for low-ed married women. Using a difference-in-discontinuities approach, I separate the income effect of the credit itself from the information effect, which, I argue, occurs when women receive the EITC for the first time. I show that, while the income effect is negative across all groups of women, the information effects are positive for unmarried women and negative for married women, again consistent with theory and the body of evidence on the EITC.

Current version:April 23, 2020Keywords:earned income tax credit; mothers; labor supply; public policyJEL codes:I38; J22; J28; J38Corresponding author:Peter Shirley
peter.shirley@liser.lu

Luxembourg Institute of Socio-Economic Research

[©] The Author(s). 2020 Open Access This article is distributed under the terms of the Creative Commons Attribution 4.0 International License (http://creativecommons.org/licenses/by/4.0/), which permits unrestricted use, distribution, and reproduction in any medium, provided you give appropriate credit to the original author(s) and the source, provide a link to the Creative Commons license, and indicate if changes were made. Cite as: Peter Shirley. *IZA Journal of Labor Policy* (2020) 10:7.

1 Introduction and Motivation

The Earned Income Tax Credit (ETIC) is one of the largest anti-poverty programs in the United States, allocating over \$67 billion to more than 27 million families for the tax year 2016.² The EITC has clear positive extensive margin labor market incentives for non-working single mothers, and many studies of the EITC have focused primarily on this group (e.g., Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001). However, the EITC's theoretical incentives are quite broad, including negative intensive (or extensive) margin labor market effects for women in dual-earner households (Eissa and Hoynes, 2004), fertility incentives (Baughman and Dickert-Conlin, 2009), and impacts on marriage decisions (Eissa and Hoynes, 1999). Many studies exploring the labor supply effects of the EITC use the presence of children, marital status, or education to define eligibility or the sample of interest. Eissa and Liebman (1996), for example, use all three to define treatment and control groups in a difference-in-differences (DiD) framework to estimate the labor supply effects of an EITC expansion in the mid-1980s. Meyer and Rosenbaum (2001) restrict their analysis to single women and estimate across having children, while Eissa and Hoynes (2004) focus on married women. Number of children can also be used as identifying variation (e.g., Grogger, 2003; Hoynes and Patel, 2018) as the value of the credit increases from zero to two children before 2009 and zero to three children from 2009 onwards.

However, such strategies are threatened by the extent to which childbearing, marriage, and education decisions are endogenous to the EITC. Specifically, the EITC encourages childbearing and generally discourages marriage; its effects on education are ambiguous. As a result, using these measures to define treatment groups or divide the sample could yield biased estimates and, in particular, could lead to estimates that overstate the positive labor market incentives of the EITC for single mothers. For example, if the EITC results in some women remaining unmarried, and the group who do so work more (and at higher wages) than the group average,³ this change in group composition could bias the estimates of the EITC's labor market effects upwards. Consequently, it is important to cross-validate the EITC's effects using other sources of identifying variation. Additionally, because the variation used here does not rely on the large 1990s EITC expansion to identify effects, and because my results are in fact much stronger in the second half of my sample period (post-2005), the criticisms levied by Kleven (2019) should not pose a concern.

The variation used in this paper is the timing of a woman's first/only birth around the end of the calendar year.⁴ EITC eligibility, and other Internal Revenue Service, rules state any child born during the calendar year can be claimed on that year's tax return.⁵ For example, a child born on 31 December 2019 could be claimed on their parents' 2019 tax return (filed early in 2020), but a child born a single day later on 1 January 2020 is ineligible; such a child could not be claimed until the tax year 2020, filed in early 2021. Because the difference in maximum

¹ Only Medicaid, SNAP (Food Stamps), and Social Security spend larger amounts.

² Source: https://www.eitc.irs.gov/eitc-central/statistics-for-tax-returns-with-eitc/statistics-for-2016-tax-returns-with-eitc; Viewed 15 April 2020.

³ This could occur if these women are more financially independent and are less likely to rely on a partner's income.

⁴ Although similar to the approach is used in Wingender and LaLumia (2017), there are several differences between their approach and mine, which I detail later in this section.

⁵ In the US, tax years and calendar years coincide.

zero-child and one-child federal credits was \$2,867 in 2016,⁶ this single-day difference in birth timing represents a tremendous potential income shock. In fact, the lowest eligible income for the maximum one-child credit in 2016 was \$9,920; at that income, the difference between the zero-child and one-child credits was \$2,993, or over 30% of the household's annual earned income.⁷

In this paper, I leverage this end-of-year cut-off for EITC eligibility to estimate the EITC's labor market effects for women who have their first/only child. I use the Survey of Income and Program Participation (SIPP) to identify the 12-month period following when women give birth to their first/only child.⁸ I provide regression discontinuity (RD) and difference-indiscontinuities (diff-in-disc) estimates where the month of birth is the running variable from July to June, and the discontinuity occurs at the end of the calendar year, across women with different numbers of children. Using this approach to estimate the EITC's effects on earnings and hours, I find positive employment effects for unmarried women including those with a high school degree or less. I also find some negative earnings effects for less-educated married women, but these effects are less robust. One drawback of the simple RD framework is that it confounds two effects for first-time mothers: the income effect from the credit itself and a knowledge effect from receiving the credit for the first time. However, as these income effects also apply to women having their second or third child, I leverage a diff-in-disc approach to separate the income and knowledge effects.9 In doing so, I show that the income effects are generally negative and significant, consistent with Wingender and LaLumia (2017), but the women having their first child respond in a different manner. The unmarried women, especially the low-ed women, respond positively to this information effect, whereas the married women have a more negative response. Both of these effects are consistent with a scenario in which knowledge gained from receiving the EITC for the first time creates labor market incentives for these mothers in the year following the birth of their first child.

I subject my baseline specification to several robustness checks to explore the validity of my findings as well as to uncover interesting heterogeneity. I explore the timing of these responses, finding that for low-ed unmarried women the effect is concentrated in the first six months following the birth of a child. As the labor market outcomes of the two groups (treatment and control) converge over time, this finding is further consistent with a knowledge effect. As we move forward in time, mothers are more likely to receive information regarding the EITC from some other means (e.g., word of mouth) or, eventually, from receiving it. Thus, it is sensible that my estimation strategy would only find short-run effects as all eligible women will receive the EITC within the first year and a half following the birth of their child. To reinforce this, I restrict the sample to only observations when the mother could have filed her tax return; my results show this time period does drive my findings. Given my crude running

⁶ Depending on the mother or household's earned income, this difference could be as large as the maximum one child credit (\$3,373 in 2016).

⁷ This date-of-birth discontinuity has been used to explore a variety of applied microeconomic topics, including the effects of age on test scores (Smith, 2009), Medicaid expansions on insurance coverage (Card and Shore-Sheppard, 2004), and parental leave on mothers' labor supply (Bass, 2020).

⁸ Alternatively, one could imagine using the child's becoming ineligible based on age (where December births are now controls and January births are treated), but this would be confounded by anticipation effects as well as the age threshold changing based on whether the child is a full-time student (which could itself be endogenous to the EITC (Bastian and Michelmore, 2018)).

⁹ I also show the baseline results for women having their second child as well as a pooled result across women having their first, second, or third child.

variable (month of birth), I use approaches where I vary the months (using only November to February births and excluding December and January births) to ensure that my results stand up to such checks. In the latter case, some results do become weaker, inviting future research with more accurate birth data. Finally, I show that my results are stronger in the second half of my sample period (2006–2016), further distancing this paper from critiques of traditional EITC studies levied by Kleven (2019).

Utilizing an RD research design requires several key assumptions, namely that women are unable to precisely manipulate the timing of their birth (or that they are unaware of the benefits of doing so) and that the mothers on either side of the end-of-year threshold do not differ from each other along other dimensions. I investigate the veracity of these claims in detail in Appendices A and B, but I find it pertinent to discuss the primary takeaways at the forefront. While it is true that the first week of the calendar year has the lowest average number of births during the time period (1969–1988) for which national birth data by date is available, the reduction in below average births per week is only around 8%. Furthermore, LaLumia, Sallee, and Turner (2015) find using the universe of tax returns from 2001 to 2010 that, although some birth timing manipulation occurs around the end-of-year threshold, it is not driven by low-income parents who stand to gain the most from shifting a child's birth from early January into late December. Confirming this as best I can with the available SIPP data, I do not observe any birth movements. In fact, while I observe a similar number of January (719) and December (632) births, the relationship is the opposite of what would be expected if women were manipulating their birth timing.

For mothers to manipulate the timing of their birth, two conditions must simultaneously be fulfilled: they must be aware of the tax-related benefits of doing so and they must have the agency to perform either a Cesarean section or labor induction; either in isolation is insufficient. Further, there are factors including concerns regarding infant's health (Schulkind and Shapiro, 2014) that discourage such behavior. Of course, I also investigate whether the data show any signs of such manipulation. In the Data section, I discuss in detail the observable characteristics of mothers as well as their labor market outcomes leading up to childbirth, as these should endogenously respond to the EITC if mothers know of the credit and anticipate receiving it. For unmarried women, the sample of greatest interest here, I find no evidence that women are endogenously selecting the timing of their children's birth.¹⁰ Overall, the best available evidence does not suggest birth manipulation occurs among either unmarried women or low-ed married women. Some older, more educated married women may respond in such a way, possibly due to better access to health services or greater agency over birth-related decisions, but this is not a group who are likely to be affected by the EITC (or if they do receive it, they typically receive less) and, as a result, they are not the focus of this study.

There are a few notable differences between the approach used in this paper and those used in traditional EITC studies. First, the Local Average Treatment Effect (LATE) identified by the regression discontinuity design is a causal estimate of a treatment's average effect on compliers. Here, I assume perfect compliance and utilize a sharp regression discontinuity design. This is due to the fact that all individuals who had children during a calendar year and filed a tax return are eligible for the EITC, provided their income lies under the end of the phase-out

¹⁰ I also run a specification where I exclude December and January births as this is the only region where such manipulation could occur. Results become slightly weaker but remain generally consistent.

region (\$39,296 for one child in 2016).¹¹ However, the take-up of the EITC is not guaranteed; a taxpayer must claim the EITC on their tax return. Although tax software and tax professionals will claim the credit on behalf of an individual or family if they are eligible, a taxpayer filing on their own may not know to claim the credit. Nevertheless, historical take-up of the EITC conditional on eligibility is quite high, with Scholz (1994) and Dollins and Maynard (2002) estimating that 80–87% of eligible households receive the credit.

Although not explicit, my sharp RD design implicitly assumes that all women having a child are eligible for the EITC, including those with earnings beyond the phase-out region. One potential alternative would be to take earnings for women before the end of the calendar year and use NBER's TAXSIM to simulate the amount each woman would receive from the EITC. However, I am reluctant to do this for several reasons. First, because one outcome is earnings in the 12 months following the child's birth, the treatment would become (partially) a function of an outcome of interest. Second, there is some evidence of bunching around tax thresholds (LaLumia et al., 2015; Gans and Leigh, 2009; Neugart and Ohlsson, 2013), implying that a single EITC parameter for all women (conditional on state and year) is a more parsimonious way of measuring labor market responses to varying degrees of EITC generosity. Third, this would require me to continuously observe women for a longer period before their giving birth than is necessary for the analyses I present. As I would be forced to impute or make assumptions about pre-birth earnings, it would be difficult to accomplish this without my sample sizes being reduced to the point of being unable to detect genuine effects.¹²

As previously noted, the sample used here is restricted to mothers whose labor market outcomes are observed in the 12 months following the birth of their first child. This strategy differs from most research on the EITC, which typically includes all mothers with children in the household and, at most, controls for whether there are any children in the household too young to attend school (e.g., Meyer and Rosenbaum, 2001; Neumark and Shirley, 2020). Consequently, one caveat of this paper is that the population for which the LATE is identified cannot be considered representative of all mothers. At the same time, all women, excluding those whose first birth is also a multiple birth (e.g., twins), belong to this sample at some point in their childbearing history, providing useful information on the effects for women at a particular stage of their childbearing.

As mentioned earlier, there are two potential mechanisms under this identification strategy, and they differ in their hypothetical effects on labor market outcomes for new mothers. The primary mechanism I focus on here comes via an information shock for women as they receive the EITC for the first time.¹³ Both groups of women, those who had children before or after the end-of-year threshold, are eligible for the EITC in the subsequent year. The difference between the two groups is that women who had children before the end of the year were able to claim the child one tax return earlier than those who had their children after. As a result, they observe the credit earlier and are more likely to respond to the credit's incentives.

¹¹ There are a few other technical eligibility criteria, such as a maximum yearly investment income and rules governing care of the child, but earned income is the predominant criterion.

¹² For the sake of transparency, I show that earnings are not meaningfully different across my treatment and control groups in the year prior to childbirth, at least for the subsample of women for whom such information is available in the SIPP.

¹³ Technically, from 1994 onwards there is a small childless EITC. However, due to its small size both in absolute terms and relative to the one-child EITC, as well as the much smaller income range over which it can be claimed, I ignore this zero-child credit. This is common practice in the EITC literature.

This also demonstrates why first-time mothers are of particular interest here. Leveraging differences between first-time mothers and women having a subsequent child allows me to isolate the information effect from the other mechanism, the income effect via the credit itself.

This mechanism closely relates to that of Chetty and Saez (2013), who use a field experiment where tax preparers at H&R Block were randomly assigned whether to go over the EITC schedule in detail with clients. They hypothesize that people do not fully understand the tax and transfer system in the United States, and providing information about that system to individuals and families could affect their decision-making. This mechanism is the same as the one I investigate in this paper with one important wrinkle. Here, I contend that people do not understand parts of the tax and transfer system with which they lack personal experience; upon interacting with a new part of the system (e.g., by having a child and receiving the EITC for the first time), they gain knowledge about the part's mechanisms, at least in a broad sense. Chetty and Saez do not find statistically significant earnings effects from their field experiment, consistent with the evidence I provide here. Under my hypothesis that first-time mothers receive their largest information shock upon initial receipt of the credit, the information gain for the treated individuals and families in Chetty and Saez (2013) would have been minimal; their behavior in response to the EITC has already adapted from previous receipts of the credit on past tax returns.¹⁴

Generally speaking, these information effects should encourage single mothers who are not working to enter the labor force and push married mothers to reduce their hours or leave the labor force entirely. Non-working single mothers effectively receive a large wage subsidy from the EITC whereas married mothers, who are more likely to be in a dual-earner household, face larger effective marginal tax rates along the phase-out region of the EITC, in some cases nearly 50%. My results are consistent with these theoretical incentives; first-time mothers who are unmarried experience more positive labor market incentives from the EITC whereas married mothers are more negatively affected.

The income effect from the credit itself represents the second mechanism present under my research design. Not only do first-time mothers learn about the EITC upon initial receipt, but they also receive a larger-than-anticipated tax return. Given that low-income individuals heavily discount (Green et al., 1994; 1996), these mothers likely respond by using the EITC as a substitution for maternity leave and decrease their hours or leave the labor market entirely. Another factor in this direction is a mother's increased value of time, driving up her reservation wage (Klerman and Leibowitz, 1994), from her desire to be with her child. Wingender and LaLumia (2017) provide evidence of these negative income effects with a similar identification strategy to mine, assigning differences in after-tax incomes calculated from NBER TAXSIM to mothers giving birth around the end of the tax year. Another advantage of their data set is the availability date of birth in the restricted-access American Community Survey, whereas my publicly available SIPP provides only month of birth. Their identification strategy allows them to estimate the effects of four policies in conjunction: the EITC, Child Tax Credit (CTC), additional dependent exemption, and head of household filing status (for unmarried women having their first child). They find that women who give birth in December return to the labor force more slowly than those who give birth in January. The mechanism through which they

¹⁴ Chetty and Saez (2013) do find some effects on reported self-employment income.

propose these findings occur is the income effect on labor supply, specifically that labor supply could be much more elastic following the birth of the child and the larger tax return via these policies encourages women to delay their return to the labor force.

At the surface, their findings and mine may seem to contradict one another, but I believe they are complementary as this paper focuses on a specific group of women, i.e., first-time mothers. As previously discussed, first-time mothers are exposed to both the income and information effects whereas Wingender and LaLumia (2017) focus solely on the income effects, which apply to having any child. My empirical strategy also isolates the effects of the refundable EITC from other non-refundable benefits associated with a December birth.¹⁵ It is plausible that the EITC, which becomes more valuable as a household moves along the phase-in region of the credit, might produce a different labor supply response, due to either the change in generosity or its refundability, than a flat, non-refundable benefit such as the additional dependent deduction. To demonstrate that I can find comparable income effects to Wingender and LaLumia's (2017), I also show an approach pooling all children together; here, my results are nearly universally negative and statistically significant across all samples. From this, I am able to provide evidence validating their overall conclusions while demonstrating heterogeneous labor market effects based on whether a woman is having her first/only child.

2 EITC History

Although the specific parameters of the EITC have evolved over time via several reforms, the basic formula has remained constant. The EITC is designed to encourage work for the lowest earners so an individual or household with no income does not receive a credit. As income increases from zero, for every dollar an individual or household earns, they receive an additional fraction of a dollar in credit. The specific amount they receive is commonly referred to as the phase-in rate.¹⁶ At a certain income level, the maximum credit is reached and a household enters the plateau region where the EITC is flat with respect to income. Finally, the credit declines over the phase-out region where a household loses a fraction of a dollar in credit for each additional dollar of income they earn. These phase-in and phase-out rates as well as the maximum credit vary with the number of children in the household.¹⁷ The full federal EITC schedule for a single taxpayer in tax year 2016 is shown in Figure 1.

The Omnibus Budget Reconciliation Act of 1993 was the single largest expansion of the EITC, increasing the phase-in rate for one child (two children) from 14% (14%) in 1990 to 34% (40%) in 1996. Other notable EITC changes include the introduction of a modest zero-child credit with a phase-in rate of 7.65% in 1994 (also part of the OBRA 1993) and the addition of a three or more children credit in 2009, which phases in at 45%. Starting in 1986, the maximum available credit was also indexed to inflation. Combined with the earlier EITC expansions, the maximum credit for one child rose from \$1,192 in 1991 to \$3,373 in 2016. Given that the minimum income necessary to receive the maximum credit was \$9,920 in 2016, the EITC

¹⁵ Mortenson et al. (2018) find evidence consistent with this, finding no effect of the CTC on labor market outcomes using a similar RD approach. However, as the CTC is worth less than the EITC and until the Tax Cuts and Jobs Act of 2017 was nonrefundable, it is more plausible that mothers respond to the EITC.

¹⁶ Since 2009, the phase-in rates for one, two, and three or more children have been 34, 40, and 45%, respectively.

¹⁷ Additionally, the income level at which the EITC begins to phase-out depends on whether the tax unit is an individual or a married couple. Specifically, married filers see the EITC begin to phase-out at a higher income level.



Figure 1 EITC schedule by number of children.

represents a potentially massive subsidy to low-income families. In addition to expansions by the federal government, 25 states (including the District of Columbia) have state supplements to the EITC as of 2016. In almost every case, the state supplements offer tax filers a flat percentage of the household's federal EITC.^{18,19}

3 EITC Literature

As Figure 1 shows, the two primary determinants of EITC eligibility are children and earned income. Among eligible groups, single mothers are generally the primary sample of interest when investigating the effects of the EITC for two reasons. First, single mothers with children are the largest group benefiting from the EITC, accounting for more than a third of recipients and nearly half of payments (Meyer, 2010). Second, estimating labor market outcomes for single mothers is a straightforward and parsimonious way to select a group of individuals relatively more likely to be affected by the positive extensive margin labor market effects of the EITC. Married mothers, to contrast, are both more likely to be in a dual-earner household and to be the secondary earner. A secondary earner in a dual-earner household likely faces the negative labor market effects from the EITC, which are a result of large effective marginal tax rates for households in the phase-out region²⁰;in some cases, these effective marginal tax rates can be close to 50%. In order to avoid muddling these effects, estimates are often performed separately

¹⁸ Wisconsin, the lone exception, awards a different percentages of the federal credit based on the number of children. As of 2016, these percentages were 4, 11, and 34 for one, two, and three or more children, respectively.

¹⁹ A typical state supplement in 2016 gives a household 15–20% of its federal credit.

²⁰ Two spouses working in 2016 at the US federal minimum wage of \$7.25 per hour would reach the phase-out region if they each work approximately 30 hours per week.

for married and unmarried women. Additionally, single mothers have lower education levels and decreased labor force participation compared to married mothers, again increasing their relative likelihood of being affected by the extensive margin effects of the EITC.

The two most well-known papers in the EITC literature are Eissa and Liebman (1996) and Meyer and Rosenbaum (2001). Each focuses on an EITC expansion in the 1980s and 1990s, respectively, and each estimates the effects of these expansions on the labor market outcomes of single mothers. Both find robust evidence that, relative to women without children, the EITC increased the employment of single mothers. Together, they lay the foundation of the EITC's reputation as a pro-work, anti-poverty policy. Adding to this popularity is the work of Eissa and Hoynes (2004), who focus on married women and find little evidence that these mothers respond to the EITC via their labor market decisions. Although the incentives exist for these mothers to decrease their work, a lack of evidence that such responses actually occur lessens this concern.²¹

To a slightly lesser extent, the number of children and mother's education can also play important roles in the analyses of the EITC's effects. As the credit a household receives is partially determined by the number of children, some (e.g., Grogger, 2003) use this as identifying variation rather than using only a children or no-children distinction. Trying to assign EITC eligibility directly using earned income is problematic, as a mother may choose her earnings endogenously with respect to the EITC and because earnings are often an outcome of interest. To avoid this issue, education level is often used as a proxy for earnings and, thus, eligibility. Mothers with low levels of education, say a high school degree or less, are assumed eligible for the EITC and women with higher levels of education are assumed ineligible or less likely to be eligible.

Many studies estimate these effects in a DiD or generalized DiD framework, which require parallel trends for the treatment and control groups in the absence of the treatment and no compositional change in the two groups as a result of the treatment. Neumark and Shirley (2020) demonstrate that there is little evidence that the parallel trends assumption does not hold in most of the outcome and treatment/control group combinations frequently studied using data from the Panel Study of Income Dynamics, and that this same data replicates the results of earlier studies (specifically Eissa and Liebman (1996) and Meyer and Rosenbaum (2001)) with a reasonable degree of precision. The compositional change assumption requires that women's fertility, marital, and educational decisions are not influenced by the EITC.

However, it is not difficult to imagine a scenario where the EITC does indeed alter these decisions. For example, generally the EITC is seen as a penalty to marriage.²² If women respond to these incentives, the composition of both married and unmarried women changes with some women remaining unmarried who, absent the EITC, would have married. Supposing those who remained unmarried due to the EITC are the most "independent" women, they would likely be employed at higher rates than the group average. Thus, this compositional change in these groups would generate a positive bias in estimates of the EITC's labor market effects for unmarried women. Because estimates for unmarried women are generally positive, it is important to ensure endogeneity biases are not generating a spurious relationship between

²¹ Subsequent work has focused on other potential outcomes, such as infant's health (Hoynes et al., 2015), mothers' health (Evans and Garthwaite, 2014), or children's education (Bastian and Michelmore, 2018).

²² I discuss the specific dynamics of when the EITC is a marriage penalty or subsidy in the following paragraphs.

the EITC and labor market outcomes. The next several paragraphs discuss the potential endogeneity concerns of fertility, marital status, and educational attainment decisions, respectively, in more detail.

The amount of EITC an individual or family is eligible for is strictly non-decreasing in the number of children they have, with the largest jump from zero children to one child. Given the pro-fertility incentives from subsidizing childbearing, the question is whether women respond to the EITC through increased childbearing and, if so, to what degree. Baughman and Dickert-Conlin (2003, 2009) directly explore the relationship between the EITC and fertility, finding mixed evidence. In their baseline specification, they find small reductions in higherorder fertility for white women as well as consistently large and positive fertility effects of state child tax (or child care) credits. They also note that the results they find when estimating their model separately for married and unmarried women may be driven by endogenous marriage decisions in response to the EITC. Duchovny (2001) also finds some evidence of increases in fertility for married white women and unmarried non-white women. More recent evidence that the EITC influences fertility decisions exists, including Bastian (2017), who estimates state EITC expansions increase completed fertility by around 7%. Although not overwhelming, the net of this evidence demonstrates, at a minimum, that care should be exercised when relying on assumptions regarding the lack of fertility effects from the EITC.

Depending on the earnings of two individuals considering marriage, the EITC may act as either a subsidy or penalty. Under a scenario where a non-working person with children considers marrying a working individual without children, the EITC acts as a marriage subsidy if the working individual's income would qualify for the EITC and is marriage-neutral if their income lies beyond the phase-out region. Alternatively, if both individuals work, the EITC will, in most cases, act as a marriage penalty. If the two individuals' combined incomes reach the phase-out region (\$23,740 for one or more children in 2016²³), then the individual with children receives less from the EITC than what they would if they separately filed their taxes. If both individuals have children the incentives are slightly more complicated, but as a general rule-of-thumb the EITC will act as a subsidy if one individual is working and a penalty if both are working.

In addition to subsidizing and penalizing marriage itself, married and unmarried women, on average, face fundamentally different incentives in response to the EITC. A primary objective of the EITC is to encourage work and subsidize earnings for low-income workers with children, and single mothers drive this response. A single mother who is not working would view the EITC as a large wage supplement, greatly increasing her effective wage and encouraging entry into the labor force. On the other hand, married women with a working spouse experience very large effective marginal tax rates if the combined dual-income reaches the phase-out region, in some cases as high as 50%. Taking marital decisions as exogenous to the EITC allows for exploration of differential EITC effects across this dimension, which is particularly interesting given these starkly contrasted predictions. The general consensus is that these theoretical responses are borne out empirically, with the EITC increasing labor force participation for single women with children (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2000, 2001; Neumark and Wascher, 2011) and perhaps having negative intensive margin effects for married women (Eissa and Hoynes, 2004; Meyer, 2010; Neumark and Shirley, 2020).

²³ Again, this income could be reached by two individuals each working about 30 hours per week at the federal minimum wage.

The intersection of these two forces—that the EITC may reward or penalize marriage and the differences in labor market responses for married and unmarried women-is a potential issue for identifying the labor market effects of the EITC. Both Eissa and Hoynes (1999) and Herbst (2011) investigate this relationship and find that although the EITC indeed affects marriage decisions, the effects are relatively modest and not of an economically meaningful magnitude. Ellwood (2000) and Dickert-Conlin and Houser (2002) also explore the EITC's marriage incentives, but find little effect of a response. More recently, however, Michelmore (2016) uses the SIPP and finds that the average EITC-eligible woman would lose approximately \$1,300 in EITC benefits in the year following their marriage. Additionally, Michelmore finds that, relative to single mothers who expect no change or to gain EITC benefits from marriage, single mothers who expect to lose EITC benefits from marriage are less likely to marry and more likely to cohabitate. More recently, Bastian (2017) finds the opposite, with the EITC actually encouraging marriage. Regardless of the direction, such effects could introduce bias into estimates of the EITC's labor market effects. Not all current research finds this, however, with Isaac (2018) using the National Longitudinal Survey of Youth and concluding the EITC affects marriage in line with the theory discussed above.

Taking a long-run perspective, Neumark and Shirley (2020) estimate the effects of the EITC on the proportion of years women spend unmarried or with children from ages 22 to 40. They find highly significant effects of the EITC on both outcomes, increasing the likelihood a woman is unmarried or has children. These effects, however, appear to wash out by the time a woman reaches 40, implying that the EITC marginally delays marriage or accelerates childbirth, rather than permanently changing these decisions. Thus, although not unanimous, there is enough evidence of endogenous marriage and fertility choices with respect to the EITC to warrant alternative identification strategies that estimate the labor market effects of the credit but are not subject to these biases.

Finally, the EITC also theoretically has implications for the educational attainment of mothers. In contrast to fertility and marital status decisions, it is ambiguous how a given mother's educational attainment decisions might be influenced by the EITC. For example, if mothers are constrained by the costs of going back to school due to tuition and fees, foregone earnings, increases in child care costs, or any combination of these, then the EITC could help to offset those and encourage further educational attainment. Women may be able to reduce their hours worked, freeing up time for classes without decreasing their total income. On the other hand, the EITC acting as a wage subsidy decreases the returns to schooling. Depending on the relative size of these competing incentives, the EITC's effects on mothers' education could be positive, negative, or zero. In contrast to the fertility and marriage incentives, no work to my knowledge exists exploring this relationship.²⁴ Because the theoretical effects are unclear, and due to a lack of empirical evidence, I leave this as an open-ended question and possibly the subject of future research.

The literature on the EITC examines its effects on numerous labor market outcomes across a variety of groups most likely to receive its benefits. Almost all of these studies use observable characteristics including having children, marital status, and education level to identify treatment and control groups. However, these categories are all theoretically endogenous to

²⁴ There is a work estimating the educational effects for children of EITC recipients. See Manoli and Turner (2014) and Bastian and Michelmore (2018).

the EITC and there is suggestive evidence that endogenous responses indeed occur. In order to estimate the effects of the EITC without relying on these categorizations, I use a different source of identifying variation and find results broadly consistent with the rest of the literature, an encouraging result for the view of the EITC as an effective anti-poverty program.

4 Data

The data set utilized for the empirical analysis in this paper comes from the SIPP. The SIPP is a nationally representative survey designed to collect detailed data about incomes and labor force participation, as well as eligibility for and take-up of various social programs. To achieve these objectives, the SIPP uses a continuous series of national panels, with durations ranging from 2.5 to 4 years. Interviews are conducted in waves, with each interview utilizing a 4-month recall period.^{25,26} The SIPP provides a number of crucial components for this analysis, including birth timing and high-frequency labor market outcomes in the subsequent year. Other data sets may only provide annual hours or earnings, which would pose a significant issue given my interest in labor market outcomes at a month-by-month level. For my analysis, I use the 1996 through 2014 Panels from the SIPP.²⁷ This allows me to capture both the years immediately following the OBRA 1993 reforms as well as the rapid growth in the number of state supplements throughout the 2000s and early 2010s.

To identify my sample of mothers with newborn children, I first identify whether a family unit has a newborn child in each period. If so, I record the age of the newborn (in months) as well as the identity of the child's mother.²⁸ Thus the main sample consists of women who have a child under the age of 12 months. Next, I record labor market outcomes (employment, inverse hyperbolic sine (IHS) earnings, and log earnings) in each of these months. Employment is defined as monthly earnings of at least \$250 in 2016 dollars, whereas the earnings measures are also in real 2016 dollars. Additionally, I assign treatment status to all women whose children were born from July to December and control status to those with births from January to June. Taking note of birth year and state, the appropriate EITC parameters and other relevant tax policies are merged on to complete the data set.

Restricting the original survey to only women observed in the year following the birth of their first/only child yields a sample containing 59,717 person-month observations. Table 1, which contains descriptive statistics for the sample of women used in the primary analysis, shows that the sample comprises primarily married women, a reflection of the sampling frame of the SIPP. Additionally, we see that married women generally have higher monthly earnings (\$2,079 versus \$1,037) and are more likely to be highly educated. Employment rates and birth timings across marital status are fairly comparable, conditional on education. Although not detailed enough for the types of analyses I perform using the National Vital Statistics System (NVSS) data in Appendix A, births appear relatively uniform across months for all groups of women during our sample period, as shown in Figure 2. Focusing in particular on December

²⁵ This recall period was increased to one year beginning with the 2014 Panel.

²⁶ These interviews are conducted either in person or by phone and are staggered evenly across months.

²⁷ My analysis includes Waves 1 to 4 of the 2014 SIPP, the latest available data, covering calendar years 2013 through 2016.

²⁸ For twin births, I record the birth as usual but note that appropriate number of children for that women. These account for roughly 3% of newborns in the SIPP, similar to the national average.

Variable	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women
Employed	0.48	0.37	0.56	0.37
Monthly earnings (2016 dollars)	1,037	609	2,079	739
Maximum credit (2016 dollars) Jul–Dec birth	3,478	3,457	3,467	3,418
Cohort	2004	2004	2003	2002
Fraction low-ed	0.54	1	0.23	1
Fraction black	0.24	0.22	0.06	0.07
Age	24.7	23.4	28.8	25.7
Fraction with Jan–Jun birth	0.49	0.48	0.50	0.48
Fraction with Jul–Dec birth	0.51	0.52	0.50	0.52
Employed in year prior to birth ¹	0.58	0.46	0.75	0.52
Employed in year prior to birth Jan–Jun birth¹	0.59	0.47	0.75	0.50
Employed in year prior to birth Jul–Dec birth¹	0.56	0.45	0.74	0.54
Monthly earnings in year prior to birth ¹	1,357	793	2,923	1,108
Monthly earnings in year prior to birth Jan–Jun birth¹	1,396	842	3,008	1,041
Monthly earnings in year prior to birth Jul–Dec birth ¹	1,318	746	2,839	1,164
Number of person-month observations	17,883	9,698	41,834	9,803
Number of unique mothers	2,665	1,516	5,378	1,354

Table 1Descriptive statistics for first-time mothers

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

¹These measures are created in a similar method to the primary sample but are calculated over the 12 months prior to a woman's first/only birth.

and January, note that the total number of births across the two months (652 births in December and 723 births in January) are very similar, with more January births than December births the opposite of what would be expected if birth timing manipulation were occurring. This fact aids the validity of my regression discontinuity strategy.

Another issue addressed at the bottom of Table 1 regards earnings in the year prior to women's first births. It is not unreasonable to postulate that unmarried women who are aware of the EITC's benefits but are not yet at the plateau region may, in the year prior to their child's birth, be more likely to work (or to work more) in order to maximize a future EITC payment. On the other hand, those on the phase-out reason (particularly due to a spouse's earnings) may choose to work less. Finding no differences between these groups on labor market outcomes prior to birth would be a useful piece of evidence in justifying my RD design. To explore this concern, Table 1 shows earnings and employment for women in the 12 months prior to their first birth, both aggregated across all women and divided into the treatment and control groups.²⁹ If this potential problem were to exist, it could be reflected in women with July-to-December births having significantly higher earnings in the year prior to their child's birth than January

²⁹ To identify this sample, I "project" the age of the child to periods before its birth where I observe the mother.



Figure 2 First/only birth counts for selected groups of women.

to June births for the unmarried groups or lower outcomes for the married groups. In fact, for three of the four groups, the opposite pattern emerges. Only low-ed married women in the treatment group have higher earnings than their control group counterparts. However, given that the differences are rather small, both in absolute and relative terms, no evidence for birth timing endogeneity exists along this particular dimension.

5 Estimation Strategy and Results

5.1 Baseline Results

The baseline results, which estimate the effects of the EITC on the employment and earnings of women in the year following the birth of their first child, are shown in Table 2. Panel A shows effects on employment rates, where employment is defined as monthly earnings of at least \$250,³⁰ Panel B shows IHS earnings, and Panel C has the log earnings effects (all in 2016 dollars). Estimates are further divided into four samples based on marital status and education level.³¹ In each set of estimates, those in the leftmost column are of the form:

$$Y_{im} = \alpha + \beta D_i + f(X_i) + \varepsilon_{im}$$

$$D_i = \begin{pmatrix} 1 & \text{if } \mathbf{X}_{\mathbf{i}} \leq -0.5 \\ 0 & \text{if } \mathbf{X}_{\mathbf{i}} \geq 0.5 \end{pmatrix}$$

³⁰ Appendix Table A5 uses alternate definitions for each of these outcomes.

³¹ For two of my samples, I use all unmarried or married women unconditional of education level. The other two samples use the same marital status division but also condition on the women having a high school degree or less (low-ed).

Table 2 Effects of the	he ETIC on lal	bor market (outcomes, b	aseline reg	ressions for	women's fi	irst child					
Functional form	All ur	imarried w	omen	Low-ed	unmarried	women	Allm	arried woi	nen	Low-ed	married w	/omen
Panel A: Employme	ent											
Linear	0.002	0.006	0.047	-0.017	-0.001	0.044	-0.045	-0.007	0.002	0.017	0.006	0.007
	(0.032)	(00.0)	(0.036)	(0.043)	(0.012)	(0.054)	(0.026)	(0.008)	(0.024)	(0:050)	(0.015)	(0.056)
Quadratic	0.045	0.024	0.050	0.012	0.010	0.046	-0.027	0.004	0.003	0.102	0.030	0.023
	(0.057)	(0.016)	(0.038)	(0.079)	(0.021)	(0.055)	(0.034)	(0.013)	(0.025)	(960.0)	(0.027)	(0.058)
Panel B: IHS earnin	lgs											
Linear	-0.054	0.039	0.446	-0.244	0.036	0.351	-0.428*	-0.065	0.038	0.069	0.032	0.017
	(0.271)	(0.079)	(0.303)	(0.342)	(960.0)	(0.412)	(0.233)	(0.076)	(0.217)	(0.414)	(0.124)	(0.475)
Quadratic	0.297	0.188	0.473	0.072	0.084	0.376	-0.334	0.031	0.038	0.811	0.243	0.155
	(0.453)	(0.125)	(0.320)	(0.622)	(0.168)	(0.427)	(0.310)	(0.128)	(0.231)	(0.804)	(0.229)	(0.495)
Ν		17,883			9,698			41,834			9,803	
Panel C: Log earnin	Igs											
Linear	0.029	0.023	0.112	0.065	0.037	0.082	-0.062	0.006	-0.078	-0.079	-0.009	-0.226*
	(0.095)	(0.027)	(0.091)	(0.128)	(0.034)	(0.138)	(0.066)	(0.020)	(0.049)	(0.131)	(0.037)	(0.127)
Quadratic	0.050	0.044	0.106	0.083	0.062	0.087	-0.184^{*}	0.005	-0.096*	0.050	0.042	-0.157
	(0.155)	(0.040)	(0.091)	(0.175)	(0.044)	(0.143)	(20.0)	(0.035)	(0.051)	(0.195)	(0.051)	(0.140)
N		8,883			3,805			23,758			3,684	
To 1 Disc.	7	z	z	≻	z	z	≻	z	z	~	z	z
To Max Cr. Disc.	Z	≻	≻	z	≻	≻	z	≻	≻	Z	≻	≻
Controls	N	N	Y	N	N	٢	N	N	Υ	N	N	γ
Data Source: Survey of Inc	come and Progra	am Participatio	on, 1996 to 201	4 Panels.	-			-	-	-	-	:
Each cell shows the paran form regressions also incl	neter of interest lude time square	trom a separat ed and time sq	e regression.⊅ uared*second	ull regressions half birth.	also include ti	me (month of	birth) and tim	e°second-half	of the year biri	th (July–Decer	nber). Quadra	tic functional
Control variables include hold deduction difference	state and cohor e*unmarried)*se	t fixed effects cond half of th	as well as age, ie year birth.	age squared,	and black race	e indicator. Als	so included is (the sum of pe	sonal tax exer	nption, CTC, a	nd single to h	ead of house-

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars. Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

In the above equation, Y_{im} represents the relevant labor market outcome for woman *i* in the month m, β is the parameter of interest, and f (X) is the control function of X (month of birth) on either side of the cut-off. For each set of results, here and moving forward, I estimate versions with linear and quadratic f(X) whenever possible.³² Following Dong (2015), birth month is adjusted to run from -5.5 in July to 5.5 in June the subsequent year; the two months around the cut-off, December and January, take values of -0.5 and 0.5, respectively. This is done because the running variable is discrete and a standard regression discontinuity estimation (e.g., December births as $X_i = -1$ and January births as $X_i = 0$) would lead to inconsistent treatment estimates even if my f(X) function is properly specified. Additionally, the data is an unbalanced panel as women can be observed multiple times, but due to the exact timing of the child's age and the specific reference months a woman is asked about, the total number of months for each woman can vary. For each woman the sample includes all months when her newborn child is between 0 months (born in the same month as the reference month) and 11 months old; this corresponds to the *m* subscript in this and all subsequent equations. For now each month is treated equally, but this dimension is explored further in later tables. Figure 3A-C, shows the regression discontinuities for each sample and outcome using the quadratic functional f(X) form.

The second column within each set of estimates captures the differences in relevant EITC maximum credits for these mothers; variation in this term is driven primarily by the increasing prevalence of state-level supplements over time as the maximum credit was indexed to inflation for the duration of my sample period. Although the end-of-year cut-off always determines whether any particular mother is eligible for the EITC on the previous year's tax return, the generosity of the EITC varies substantially across state and year during the sample period (1995–2016). I am able to capture this with the inclusion of a *Max Credit*, term, yielding:

$Y_{imst} = \alpha + \beta MaxCredit_{st}D_i + f(X_i) + \varepsilon_{imst}$

Finally, the third column includes my full set of control variables. These controls include age, age squared, a black race indicator, state fixed effects, cohort fixed effects, and other tax policies. Cohort effects align with my treatment definition which is based on a July to June period corresponding to a six month period before and after each end-of-year discontinuity, so instead of the traditional January to December year fixed effect, I use a fixed effect defined on 12-month periods ranging from July to June. The control variable for other tax policies is of particular importance because, as previously discussed, the EITC is not the only tax benefit women become eligible for by having their child before the end of the tax year. Namely, the other potential benefits are the CTC, additional personal exemption, and the difference in standard deductions between filing as head of household rather than single.³³ I use the sum of the maximum values of each of these policies as my regressor, in the same way that I parameterize the EITC. The addition of these controls completes my preferred estimation strategy:

$$Y_{imst} = \alpha + \beta MaxCredit_{st}D_i + f(X_i) + \gamma OtherTaxPolicies_{st}D_i + \delta Age_{im} + \zeta Age_{im}^2 + \zeta Age_$$

 $\eta Black_i + S_s + \tau_t + \varepsilon_{imst}$

³² For example, when using only December and January births in Table 6, it is impossible to include the control function.

³³ This last benefit only applies to unmarried women having their first child.



Figure 3A Employment regression discontinutiv plots by sample, 1995–2016.

Figure 3B IHS earnings regression discontinutiy plots by sample, 1995–2016.



Results from this preferred specification are shown in the third column of each sample. Note that this strategy uses variation in EITC rates across state and time in order to separately identify the effects of the EITC from the other policies for which mothers are eligible. In specifications 1 and 2 the effects of the EITC and the other tax policies all load onto the estimate of β , and specification 1 does not take into account the varying levels of generosity in the EITC



Figure 3C Log earnings regression discontinutiv plots by sample, 1995–2016.

that occur across states and the sample period. Although interpretations of coefficients across these specifications are rather different in some cases, it is important to demonstrate all three approaches, at least at the onset of presenting results. Also, note that in the second and third specifications (here and in all subsequent tables), the coefficients are scaled so that they can be interpreted as a \$1,000 increase (2016 dollars) in the maximum EITC credit. I do this for convenience as such an increase in the maximum credit is both generous and within sample for the EITC reforms and state supplements implemented across the sample period.

Overall, the estimated effects of the EITC on the labor market outcomes of first-time mothers in the 12 months following their children's birth are consistent with the previous literature. For low-ed unmarried women, the group the EITC most directly targets, there is suggestive evidence that the EITC increases employment and earnings. A \$1,000 increase in the maximum EITC increases employment probability for low-ed unmarried women by around 4.5 percentage points (pp) using either f(X) specification. This is slightly smaller than, but not statistically different from, other research such as Eissa and Liebman (1996) or Meyer and Rosenbaum (2001). Given that these estimates combine the negative income effect and positive information effect, slightly less positive impacts are to be expected. Notably, the IHS earnings effects are larger, on the order of approximately 35%, but neither is statistically significant. Although it may seem as though these effects are unbelievably large, they must be put into context. Given monthly earnings for this group are, on average, around \$600, a 35% increase is little more than \$200 per month, or \$2,500 per year. Also, given many of these women will likely take some time off from work immediately following the birth of their child there is more "slack" in their labor market outcomes, leaving more room for the EITC to have an effect. This pattern remains largely unchanged when not conditioning on education level, with marginally larger effect sizes. As with the low-ed effects, however, none of the estimates are statistically significant.

For married women, conversely, there is scant evidence that the EITC has employment effects, with a near-zero effect for all married women. Of the twelve estimates from the third specification across the two groups, two are statistically significant; both are log earnings effects, one for all married women and one for low-ed married women. Although the significance is sensitive to functional form, the estimates are at least suggestive of a negative earnings effect. Again, these effects may seem a bit high, but this is not unexpected given these women have all recently given birth and have lower-than-usual earnings.

Recall the two mechanisms that I argue are at work in Table 2: the EITC estimates capture both income and knowledge effects simultaneously, complicating direct comparisons to other parts of the literature. My hypothesis that both forces exist and are important would be reinforced if I could isolate either effect. I expand this analysis to use a diff-in-disc design to try and isolate the informational effects of the EITC. Although imperfect, this strategy will estimate the difference between two regression discontinuities, one for women having their first child and one for women having their second child. Women having their second child become eligible for a more generous EITC (the EITC strictly increases from one to two children over most of its history and all of my sample period) and are exposed to the resulting income effects. At the same time, we can presume they have previously received the EITC from their first child, so women should receive much less information regarding the EITC here.³⁴ Thus, the results shown in Table 3 should help to isolate the information effects for women having their first child.

Table 3 shows these results, with most effects more precisely estimated. The key finding, that unmarried women having their first child experience positive employment and earnings effects from the EITC, including those with a high school degree or less, remains, although the employment effects are much smaller, implying that the main channel is along the intensive margin. Looking at only the unmarried women with a high school degree or less, the employment effects are close to zero but the earnings effects become a bit stronger and, although insignificant, are suggestive of an effect.

For low-ed married women, the evidence also suggests that the EITC has small negative effects on earnings in the year following the birth of their first children, but these are merely suggestive; one result (log earnings, linear functional form) is marginally significant. This result is broadly consistent with the general EITC literature, where effects for married women are generally small (e.g., Eissa and Hoynes, 2004), at least when compared to the positive effects for unmarried women. Critically, this does not imply such effects are not economically meaningful. Although the effect sizes are smaller and are statistically indistinguishable from zero at the individual level, the relative size of these two groups, with married mothers being a much larger group, should be considered when discussing the implications of even small negative effects for married women.

As with any estimation strategy, the diff-in-disc approach used in Table 3 relies on a set of assumptions, namely that women having their second child also do not manipulate the timing of their birth. Although they may now know of these policies, they may not necessarily know that the amount they receive increases with the number of children or, even if they do, they may not have the agency to precisely time their childbirth. Both are required for the validity of

³⁴ Note that the gap from zero children to one child is much larger than the gap between one child and two children, as Figure 1 shows.

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women
Panel A: Employment				
Linear	0.023	0.004	0.006	-0.021
	(0.025)	(0.031)	(0.018)	(0.036)
Quadratic	0.024	0.005	0.008	-0.008
	(0.030)	(0.034)	(0.020)	(0.037)
Panel B: IHS earnings				
Linear	0.281	0.117	0.089	-0.218
	(0.196)	(0.234)	(0.159)	(0.299)
Quadratic	0.280	0.125	0.098	-0.116
	(0.239)	(0.265)	(0.178)	(0.307)
Ν	33,043	19,215	86,875	23,130
Panel C: Log earnings				
Linear	0.107	0.143	-0.009	-0.140*
	(0.077)	(0.097)	(0.043)	(0.077)
Quadratic	0.105	0.144	-0.033	-0.098
	(0.081)	(0.097)	(0.036)	(0.082)
Ν	15,647	7,274	46,496	8,448
To 1 Disc.	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Y	Υ	Y	Y
Controls	Υ	Y	Y	Y

Table 3 Effects of the ETIC on labor market outcomes, one child to two children diff-in-disc results

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

p*<0.1, *p*<0.05, ****p*<0.01.

the diff-in-disc approach to be threatened. Because of the additional precision it provides, and because it isolates the knowledge effects from receiving the EITC for the first time, I elect to use the diff-in-disc approach for most of the remaining analyses.

6 Robustness and Heterogeneity

Before moving on to estimates designed to probe the robustness of my findings or explore avenues of effect heterogeneity, I first show the equivalent of specification 3 from Table 2, but only for the women having their second child. Such an estimate should isolate the income effects of the EITC, which are likely negative, as theory would predict and Wingender and LaLumia (2017) demonstrate. Table 4 appears to confirm this prediction. The information effect from the EITC drives the positive results for unmarried women in Tables 2 and 3 and makes the negative estimates for the married women even more negative. Many more estimates here are statistically significant, including employment and IHS earnings for both unmarried groups, all of which are significant at the 5% level using the linear functional form.³⁵

It need not be the case that the effects of the EITC are symmetrical across the 12 months following the birth of a woman's first child. One reason for this is the age of the child itself and the recovery of the mother after childbirth. At extremely young ages, mothers may have much higher reservation wages (Klerman and Leibowitz, 1994) and are more hesitant to return to work relative to when the child is slightly older. This argument is similar to, but operates on a much shorter time scale than, the distinction of whether a mother's youngest child is school-age or not; this measure has been used in previous EITC studies, including Meyer and Rosenbaum (2001) and Neumark and Shirley (2020). Many women will naturally return to the workforce as their child ages, but the EITC may accelerate or slow down this phenomenon. On the other hand, the aforementioned slack in a new mother's labor market outcomes will be largest early in the 12-month period, yields ambiguous predictions, and provides an opportunity for empirical evaluation.

This discussion touches upon an important and prescient wrinkle: the timing of when the information gain occurs. Individuals or families typically learn about the EITC when filling out their tax returns. That is, the information shock occurs when the tax return is completed and mothers notice that the return is (up to) \$3,000 greater than anticipated. This windfall would obviously come as a pleasant surprise, and knowledge of the EITC is gained as recipients identify the source of the increased return. Because filers have up until 15th April to file their taxes in a normal year, they may not notice the increase until this date. Consequently, although mothers may be eligible to receive the EITC as a result of having a child right before the end of the calendar year, it could be up to four months³⁶ before they file their tax return and acquire knowledge of the credit. The next analyses explore how these effects evolve over the year and discuss the evidence for these potentially competing hypotheses.

The analysis in Table 5 is identical to that of specification 3 in Table 3, but splits the sample into two halves, based on whether the newborn is between 0 and 5 or 6 and 11 months old. The key takeaway here is that although the directions are similar in the first and second six months following the child's birth, the results tend to be stronger in magnitude for all unmarried women during the second six months; for the low-ed married women, the results are rather consistent across the full year. For example, employment effects for all unmarried women are 2.6 and 2.4 pp in the first six months and 2.2 and 2.5 pp in the second six months. A similar pattern holds for earnings effects for this group with positive effects in the first six months and slightly smaller effects in the second six months, although these results are not statistically different from zero (or each other). For low-ed unmarried women, employment effects are positive across the first six months, but are very small and negative in the latter six months, while positive earnings effects also seem more likely at ages 0 to 5 months, with the log earnings effects are negative and relatively consistent, with marginal significance on the two linear functional form log earnings estimates (-16.4 and -15.1%, respectively).

Next, I disaggregate the previous analyses a bit further by estimating for each age

³⁵ In the next subsection I show a similar table, pooling women having their first, second, or third child.

³⁶ This number could be even higher for births in November or earlier. I address this in three ways, as shown in Tables 6–8.

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women
Panel A: Employment				
Linear	-0.020**	-0.022**	-0.005	-0.004
	(0.008)	(0.010)	(0.006)	(0.012)
Quadratic	-0.015	-0.018	0.002	-0.001
	(0.010)	(0.015)	(0.008)	(0.015)
Panel B: IHS earnings				
Linear	-0.160**	-0.166**	-0.019	-0.021
	(0.065)	(0.079)	(0.057)	(0.095)
Quadratic	-0.105	-0.102	0.040	0.006
	(0.079)	(0.117)	(0.069)	(0.125)
Ν	15,160	9,517	45,041	13,327
Panel C: Log earnings				
Linear	0.005	0.006	0.003	-0.041
	(0.029)	(0.031)	(0.019)	(0.029)
Quadratic	0.008	0.014	0.017	-0.030
	(0.029)	(0.031)	(0.023)	(0.038)
Ν	6,764	3,649	22,738	4,764
To 1 Disc.	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Y	Y	Y	Υ
Controls	Y	Y	Υ	Υ

Table 4 Effects of the ETIC on labor market outcomes, results for women's second child

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

*p<0.1, **p<0.05, ***p<0.01.

in quarters. The benefit of this approach, of course, is that it allows me to demonstrate the EITC's labor market effects at a finer level of detail. An additional benefit is that I can visually represent these effects. The obvious disadvantage is that I must effectively divide my baseline sample into fourths to perform this analysis generating further imprecision in my estimates. Given that some of my samples are rather small to begin with, especially the low-ed unmarried women, it could easily be the case that real employment or earnings effects that are detectable at more aggregate levels are impossible to detect. Figure 4A–C are divided by outcome with each bar representing an estimate using a quadratic $f(X_i)$. The estimates are grouped into the four samples and are in order from left to right by the age of the woman's newborn in quarters. These figures largely reflect what is shown in Table 5, albeit in a more visually digestible fashion.

One flaw with the baseline regression discontinuity design employed here is the running

Table 5 Effects of t	he ETIC on labor	market outcomes,	, regressions for w	/omen with first ch	ildren by age of o	child		
Functional form	All unmar	ried women	Low-ed unm	arried women	All marri	ed women	Low-ed mai	rried women
Age of child	0–5 months	6–11 months	0– 5 months	6–11 months	0–5 months	6–11 months	0–5 months	6–11 months
Panel A: Employm	ent							
Linear	0.026	0.022	0.026	-0.010	0.010	0.002	-0.012	-0.028
	(0.032)	(0:030)	(0.044)	(0.037)	(0.018)	(0.024)	(0.037)	(0:050)
Quadratic	0.024	0.025	0.026	-0.006	0.015	0.001	-0.001	-0.011
	(0.034)	(0.036)	(0.045)	(0.042)	(0.019)	(0.026)	(0.035)	(0.052)
Panel B: IHS earnin	lgs							
Linear	0.301	0.277	0.331	-0.036	0.123	0.062	-0.093	-0.308
	(0.254)	(0.238)	(0.344)	(0.295)	(0.163)	(0.205)	(0.309)	(0.418)
Quadratic	0.285	0.287	0.330	-0.011	0.154	0.048	-0.027	-0.167
	(0.279)	(0.282)	(0.349)	(0.334)	(0.170)	(0.231)	(0.294)	(0.439)
Ν	16,129	16,914	9,397	9,818	42,362	44,513	11,005	12,125
Panel C: Log earnir	lgs							
Linear	0.117	0.092	0.249*	0.022	-0.025	-0.016	-0.164^{*}	-0.151^{*}
	(0.107)	(0.075)	(0.134)	(0.112)	(0.046)	(0.050)	(0.95)	(0.088)
Quadratic	0.119	0.087	0.244*	0.035	-0.047	-0.041	-0.098	-0.122
	(0.108)	(0.085)	(0.126)	(0.122)	(0.043)	(0.042)	(0.098)	(0.098)
Ν	7,047	8,600	3,222	4,052	22,020	24,476	3,770	4,678
To 1 Disc.	z	Z	z	Z	z	Z	z	Z
To Max Cr. Disc.	~	7	~	7	7	۲	7	7
Controls	٨	٨	٨	٨	٢	٢	٨	٨
Data Source: Survey of Inc	come and Program P	articipation, 1996 to 2	014 Panels.					
Each cell shows the paran form repressions also incl	neter of interest from Jude time squared an	n a separate regression of time squared *secon	 All regressions also d Alf birth, Each of th 	include time (month o hese parameters is inc	f birth) and time*seco luded twice, each tim	ond-half of the year bir or interacted with an i	rth (July–December). Indicator for one or ty	. Quadratic functional wo children. The coef-
ficient shown is the differ	ence between the di	scontinuity estimates	for one child and two	children.	(
Control variables include	state and cohort fixe	ed effects as well as ag	ye, age squared, and t	olack race indicator. Al	so included is (the su	im of personal tax exe	imption, CTC, and sir	igle to head of house-

Page 23 of 53

hold deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level. * p<0.1, ** p<0.05, ***p<0.01.

variable itself, month of birth; date of birth would provide a much cleaner approach. That is to say, it is likely more convincing to say women giving birth within a few days of each other are more similar than those giving birth a few months from each other. To address the issue as best I can with the available data, I re-estimate my baseline results using all mothers who give birth within two months of the end-of-year cut-off. Thus, Table 6 shows results using births from November to February. Using this approach, the results are reasonably similar to the baseline specification but are, unsurprisingly, less precise. The IHS earnings effects for low-ed unmarried women remain positive, but the other measures become negative, although none are precisely estimated. For the low-ed married women, the effects are consistently negative and large but, again, sample sizes are much smaller than in the full sample. Admittedly, it is difficult to draw clear conclusions using a more restricted sample of women.

Although I have argued throughout the paper and in Appendix A that birth timing is not an issue for the low-ed, unmarried first-time mothers who are the primary sample of interest in this paper, excluding births within a small window on either side of the cut-off is one approach that could be used to directly combat the issue. This approach is similar in spirit to the donut-RD of Barreca et al. (2011) but, again, given that the running variable is birth month, the implementation is a bit crude. Excluding December and January births, as Table 7 shows, does not qualitatively change the results relative to the baseline. The strong positive IHS earnings effects for all unmarried women remain, as well as the negative earnings effects for low-ed married women, but some estimates become sensitive to functional form. While this specification does not refute the discussion so far, it does unfortunately fail to provide much corroborating evidence.

The next two tables will further explore my results in the context of when specifically women will be exposed to the information effects of the EITC. The first, Table 8, excludes the month where women have their child. This is done for two primary reasons. First, regardless of whether a woman learns about the EITC in that month, it is unlikely that she would desire to return to the workforce with such haste. Second, the notion that a woman could even do so in the first place requires a specific set of circumstances. A woman who has a child in late December and files her tax return early in January, i.e., as soon as a person is able to do so, will be exposed to the information effects of the EITC within the first month of her child's birth. At the same time, for women having births in non-December months (July–November), this will not be the case. As a result, I do not exclude this period in the main analyses, but find it pertinent to show how the results are affected. Table 8 shows results quite similar to the baseline.

In Table 9, I take the same idea even further, restricting each treated woman's time period to the months when she has had her child, it is under one year old, and the period is in the subsequent calendar year. The first two are the same sample criteria as every other table thus far, but the third restriction is done to ensure that each woman could have feasibly filled out her tax return and observed the additional EITC she will receive. As an example, the earliest treated group, women with births in July, have a 12-month period that runs from July until the following June in the baseline sample. The treated period for those women runs from January to June in the following year. The July to December period, while corresponding to when the newborn is between 0 and 5 months, does not align with a period when they



Figure 4A Employment effects of EITC by age of child in quarters, 1995–2016.

Figure 4B IHS earnings effects of EITC by age of child in quarters, 1995–2016.



would have been able to file their tax return.³⁷ As Table 9 shows, the results are robust to using these alternate sample selection criteria. This is additional evidence that the women

³⁷ The results are similar to using a slightly more restrictive version where I also exclude January and February in the following year.



Figure 4C Log earnings effects of EITC by age of child in quarters, 1995–2016.

are responding to the information incentive of the EITC and not some other, unobserved, confounding factor.

The last set of robustness results presented are in Table 10. Here, I explore whether women appear to respond differently across the two halves of the overall sample period. Overall receipt of the EITC conditional on eligibility has risen over time and so has general knowledge of the program. As a result, it would be interesting to know whether the women receiving the EITC also seem to react differently along this dimension. Additionally, this allows me to distance my results even further from the critique of Kleven (2019), as responses in the second half of my sample period are more than a decade from the OBRA 1993 EITC expansions and the welfare reform that occurred during the same general time period. Table 10 runs the same analyses as Table 3, but splits the cohorts into two even halves (by number of years), with the first set of results covering the time period 1995-2005 and the second half covering 2006-2016. As the results indicate, the positive effects for unmarried women, especially the least educated, and the negative results for low-ed married women appear to be much stronger during the latter part of the sample window. This is consistent with the theory postulated here and throughout the paper that women learn about the EITC via initial receipt and that this effect is stronger during the period where the amount of knowledge regarding the program in the general public is stronger and there is more access to such resources.

7 Discussion

One important element of this work's contribution relative to Wingender and LaLumia (2017) is my identification of important treatment effect heterogeneity by birth order as well as the separation of the income and knowledge effects of the first-time receipt of the EITC. To further

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women
Panel A: Employm	ent			
Linear	-0.022	-0.006	0.025	-0.089
	(0.061)	(0.065)	(0.041)	(0.091)
Panel B: IHS earni	ngs			
Linear	-0.063	0.094	0.212	-0.851
	(0.511)	(0.546)	(0.357)	(0.748)
Ν	11,469	6,557	28,612	7,876
Panel C: Log earni	ngs			
Linear	0.132	-0.124	0.021	-0.262
	(0.113)	(0.146)	(0.066)	(0.164)
Ν	5,610	2,596	14,960	2,691
To 1 Disc.	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Y	Υ	Υ	Y
Controls	Y	Υ	Y	Y

Table 6Effects of the ETIC on labor market outcomes, regressions for women's first child, November to Februarybirths only

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

p*<0.1, *p*<0.05, ****p*<0.01.

underscore these findings, I provide one final set of analyses in Table 11. Here, the premise is to recreate, as closely as I can using my sample and estimation strategy, a method that allows direct comparisons to Wingender and LaLumia (2017). In practical terms, this combines the simple estimation strategy from Table 2 across women having their first, second, or third child. Note that this will capture the income and knowledge effects for women having their first child as well as the income effects for women having their second or third child. This table clearly shows the treatment effect heterogeneity across birth order. Most estimates for unmarried women, both low-ed and otherwise, are negative and statistically significant. Similarly, all but one of the results for the third specification are negative for both groups of married women. For the all unmarried group, every single estimate is statistically significant at the 1% level save one, which is significant at the 5% level. From this, it is clear that the EITC has an overall negative effect on labor market outcomes in the first year following the birth of a child for most women. However, as the previous tables have argued and shown evidence for, the effects on low-ed unmarried first-time mothers tend to be positive, likely due to the information effect from receiving the EITC for the first time. At the same time, few of the results are statistically significant for these women, making it difficult to draw conclusions beyond suggestive evidence.

Before concluding, I wish to briefly mention the other analyses included in the Appendix. The broad takeaway from these additional tables is that my findings are not qualitatively

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women
Panel A: Employn	nent			
Linear	0.037	-0.005	0.002	-0.015
	(0.030)	(0.041)	(0.017)	(0.041)
Quadratic	0.023	-0.013	-0.008	0.013
	(0.038)	(0.049)	(0.020)	(0.043)
Panel B: IHS earn	ings			
Linear	0.385*	0.054	0.074	-0.121
	(0.221)	(0.315)	(0.149)	(0.338)
Quadratic	0.249	-0.015	-0.036	0.073
	(0.287)	(0.378)	(0.171)	(0.375)
Ν	27,153	15,801	72,052	18,787
Panel C: Log earn	ings			
Linear	0.094	0.105	0.003	-0.114
	(0.090)	(0.111)	(0.049)	(0.085)
Quadratic	0.049	0.058	-0.037	0.010
	(0.104)	(0.110)	(0.052)	(0.088)
Ν	12,714	5,934	38,833	7,012
To 1 Disc.	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Table 7 Effects of the ETIC on labor market outcomes, regressions for women's first child excluding Decemberand January births

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

*p<0.1, **p<0.05, ***p<0.01.

changed, but they show additional approaches a reader may find interesting or pertinent. Appendix Table A1 shows the two children to three children diff-in-disc, finding mostly null effects, with the exception of high-ed married women. This is consistent with the notion that the stronger results for the one child to two children diff-in-disc shown in Table 3 is largely driven by the information effect and not the income effect. Appendix Table A2 takes this a step further, performing a diff-in-diff-in-disc, showing strong positive earnings results for low-ed unmarried women, weaker positive effects for all unmarried women, and negative (but insignificant) effects for both groups of married women. In the other tables, I use the sample weights (Appendix Table A4), alter the employment definitions and trim earnings outliers (Appendix Table A5), parameterize the EITC using the phase-in rate (Appendix Table A6) or the federal credit only (to avoid potential endogenous commuting (Shirley, 2018) behavior) (Appendix

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women
Panel A: Employment				
Linear	0.022	0.006	0.005	-0.020
	(0.024)	(0.031)	(0.019)	(0.038)
Quadratic	0.024	0.008	0.007	-0.004
	(0.030)	(0.035)	(0.021)	(0.039)
Panel B: IHS earnings				
Linear	0.272	0.118	0.088	-0.204
	(0.193)	(0.235)	(0.165)	(0.315)
Quadratic	0.279	0.128	0.091	-0.086
	(0.238)	(0.269)	(0.186)	(0.326)
Ν	30,556	17,790	80,221	21,442
Panel C: Log earnings				
Linear	0.103	0.127	-0.014	-0.147*
	(0.075)	(0.097)	(0.044)	(0.077)
Quadratic	0.101	0.131	-0.039	-0.105
	(0.080)	(0.099)	(0.037)	(0.081)
Ν	14,648	6,844	42,982	7,914
To 1 Disc.	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Υ	Y	Y	Y
Controls	Υ	Υ	Y	Y

 Table 8
 Effects of the ETIC on labor market outcomes, regressions for women's first child excluding age 0 months

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth. Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

*p<0.1, **p<0.05, ***p<0.01.

Table A7), and estimate separate regressions for black mothers (Appendix Table A8). The sum of the evidence from these additional approaches reinforce the conclusions found in the main analyses.

8 Conclusion

The ETIC is one of the largest anti-poverty programs in the United States and has been widely praised for its anti-poverty effects and pro-work incentives. A number of studies have analyzed these effects, particularly for single mothers who are a primary target and recipient of the credit. Most of these studies separate women into treatment and control groups based on having children, marital status, and education level. However, as all of these categories are

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women
Panel A: Employment				
Linear	0.018	0.017	0.011	-0.002
	(0.030)	(0.036)	(0.024)	(0.052)
Quadratic	0.021	0.021	0.010	0.010
	(0.037)	(0.041)	(0.026)	(0.053)
Panel B: IHS earnings				
Linear	0.219	0.177	0.131	-0.064
	(0.243)	(0.281)	(0.204)	(0.427)
Quadratic	0.239	0.209	0.114	0.033
	(0.293)	(0.322)	(0.224)	(0.442)
Ν	25,583	14,862	67,191	17,940
Panel C: Log earnings				
Linear	0.038	0.044	0.008	-0.140
	(0.073)	(0.111)	(0.049)	(0.089)
Quadratic	0.029	0.057	-0.023	-0.129
	(0.079)	(0.116)	(0.043)	(0.098)
Ν	12,369	5,753	36,150	6,650
To 1 Disc.	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Υ	Y	Y	Y
Controls	Υ	Υ	Y	Y

Table 9 Effects of the ETIC on labor market outcomes, regressions for women's first child, post-tax season

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth. Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

*p<0.1, **p<0.05, ***p<0.01.

potentially endogenous to the EITC, it is possible the estimated labor market effects of the policy are biased upwards. This paper has discussed these potential biases, noted some evidence that endogenous responses occur, and implemented a different identification strategy using another source of EITC variation to avoid them: the timing of a woman's first birth around the end of the calendar year.

Utilizing data from the SIPP and a regression discontinuity design around this endof-year eligibility cut-off, I find positive employment and IHS earnings effects of the EITC for unmarried women, including those with a high school degree or less. These effects are consistent with the rest of the EITC literature but are driven here primarily via a knowledge effect stemming from receipt of the credit for the first time. When using a diff-in-disc approach to isolate the knowledge effect from the income effect from the credit itself, I find effect sizes roughly similar to the literature, although my earnings effects are larger in magnitude. This

Functional	All unmarri	ied women	Low-ed u	nmarried	All marrie	d women	Low-ed	married
form			wor	nen			wor	nen
Cohorts	1995-2005	2006-2016	1995–2005	2006-2016	1995–2005	2006-2016	1995–2005	2006-2016
Panel A: Em	ployment							
Linear	-0.019	0.063	-0.044	0.070	0.002	-0.002	-0.032	0.008
	(0.039)	(0.046)	(0.046)	(0.061)	(0.024)	(0.036)	(0.047)	(0.077)
Quadratic	-0.024	0.075	-0.045	0.082	0.007	-0.007	-0.013	-0.009
	(0.042)	(0.049)	(0.049)	(0.060)	(0.029)	(0.034)	(0.048)	(0.079)
Panel B: IHS	earnings							
Linear	-0.080	0.629	-0.297	0.643	0.072	-0.038	-0.309	0.114
	(0.316)	(0.382)	(0.366)	(0.501)	(0.220)	(0.311)	(0.403)	(0.616)
Quadratic	-0.116	0.699*	-0.301	0.689	0.103	-0.076	-0.159	-0.048
	(0.343)	(0.402)	(0.381)	(0.487)	(0.261)	(0.298)	(0.412)	(0.634)
Ν	18,365	14,678	11,633	7,582	56,305	30,570	16,757	6,373
Panel C: Log	g earnings							
Linear	0.065	0.070	0.127	0.133	0.042	-0.054	-0.072	-0.237
	(0.083)	(0.161)	(0.121)	(0.157)	(0.053)	(0.044)	(0.089)	(0.215)
Quadratic	0.062	0.094	0.128	0.156	-0.011	-0.042	-0.065	-0.186
	(0.087)	(0.154)	(0.117)	(0.161)	(0.041)	(0.046)	(0.104)	(0.227)
Ν	8,532	7,115	4,458	2,816	29,037	17,459	6,252	2,196
To 1 Disc.	Ν	Ν	Ν	Ν	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y

 Table 10
 Effects of the ETIC on labor market outcomes, regressions for women with first children by cohort

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth. Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

*p<0.1, **p<0.05, ***p<0.01.

is not unexpected, however, as women have particularly low earnings during the period immediately following childbirth and, thus, my effect sizes reflect this lower baseline. Further, I find some evidence of negative earnings effects for low-ed married women, primarily along the intensive margin of employment. I conclude that, although endogenous marital, fertility, and educational responses to the EITC pose a potential threat to identification in traditional studies of the policy and there is sufficient empirical evidence to merit concern, these biases are not sufficient to demonstrably alter the conclusions of these previous studies. At the same time, my running variable (month of birth) is rather crude and further research using higher-quality data could provide additional evidence and further validation of the work here.

Table 11 Ef	ects of the E	TIC on labor	market outc	omes, baseli	ine regressic	ons for all nu	mbers of ch	ildren				
Functional	All ui	nmarried w	omen	Low-ed	unmarried	women	All	married wor	nen	Low-e	d married w	omen
	.											
Panel A: Em	ployment											
Linear	-0.043*	-0.014***	-0.015***	-0.054*	-0.011^{**}	-0.011^{**}	0.0004	-0.009***	-0.012***	0.037	-0.0002	-0.003
	(0.022)	(0.004)	(0.004)	(0.027)	(0.005)	(0.005)	(0.015)	(0.002)	(0.002)	(0.026)	(0.004)	(0.004)
Quadratic	-0.021	-0.015^{***}	-0.010^{*}	-0.049	-0.010^{*}	-0.008	0.020	-0.012***	-0.012***	0.059	-0.003	-0.001
	(0.035)	(0.006)	(0.005)	(0.042)	(0000)	(0.006)	(0.027)	(0.004)	(0.004)	(0.044)	(0.004)	(0.006)
Panel B: IHS	earnings											
Linear	-0.399**	-0.119***	-0.127***	-0.506**	-0.100**	-0.099**	0.004	-0.071***	-0.112***	0.301	0.005	-0.018
	(0.184)	(0.035)	(0.032)	(0.222)	(0.040)	(0.037)	(0.134)	(0.022)	(0.022)	(0.215)	(0:030)	(0.034)
Quadratic	-0.196	-0.125***	-0.085*	-0.376	-0.088*	-0.064	0.143	-0.106***	-0.112^{***}	0.478	-0.016	-0.004
	(0.290)	(0.045)	(0.044)	(0.339)	(0.047)	(0.049)	(0.240)	(0.037)	(0.038)	(0.349)	(0.034)	(0.044)
Ν		42,200			25,285			111,580			32,497	
Panel C: Lo	gearnings											
Linear	0.010	0.002	-0.020**	0.008	0.016	-0.008	-0.018	-0.005	-0.020***	-0.027	0.007	-0.001
	(0.052)	(0.008)	(600.0)	(0.070)	(0.013)	(0.012)	(0.041)	(0.008)	(0.005)	(0.082)	(0.013)	(0.013)
Quadratic	0.011	0.002	-0.020	0.002	0.024	-0.004	-0.035	-0.008	-0.022**	0.012	0.019	0.024
	(660.0)	(0.010)	(0.013)	(0.108)	(0.017)	(0.018)	(090.0)	(0.012)	(0.008)	(0.119)	(0.017)	(0.017)
Ν		19,318			9,304			56,118			11,093	
To 1 Disc.	7	z	Z	≻	z	z	≻	Z	z	7	z	z
To Max Cr.	Z	≻	≻	z	≻	≻	Z	≻	~	z	~	≻
DISC.												
Controls	z	z	>	z	z	~	z	z	~	z	z	>
Data Source: Sur	vey of Income a	finterest from	articipation, 199)6 to 2014 Panel	ls. seione aleo inc	nom) anima (mon	th of hirth) and	d time*corond_t	h reaverthe	irth / Iulv_Doc	ambar) Outadra	tic functional
form regression:	also include tir	me squared and	d time squared*	'second half bir	th.	וממב נווווב (וווסוו		מ נווווב אבנטוומ-ו	ומוו טו נווב אבמו ט	וו נוו (סמוא–מכר	ellinei). Quaulo	ורור ומווררוחוומו
Control variable	s include state á	ind cohort fixed	d effects as well	l as age, age sqı	uared, and blac	ck race indicato	r. Also include	ed is (the sum of	personal tax ex	emption, CTC	, and single to h	ead of house-

Page 32 of 53

`` 5 ź ace Control variables include state and conorcinsed enercies as well as age, age squa hold deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

References

- Barreca, A. I.; M. Guldi; J. M. Lindo; G. R. Waddell (2011): Saving Babies? Revisiting the Effect of Very Low Birth Weight Classification. The Quarterly Journal of Economics 126(4), 2117-2123.
- **Bass, B.** (2020): Does an Introduction of a Paid Parental Leave Policy Affect Maternal Labor Market Outcomes in the Short Run? Evidence from Australia's Paid Parental Leave Scheme, IZA Journal of Labor Policy, 10(1), 20200004.
- Bastian, J. (2017, January): Unintended Consequences? More Marriage, More Children, and the EITC, in: Proceedings. Annual Conference on Taxation and Minutes of the Annual Meeting of the National Tax Association. National Tax Association, Vol. 110, 1-56.
- Bastian, J.; K. Michelmore (2018): The Long-Term Impact of the Earned Income Tax Credit on Children's Education and Employment Outcomes. Journal of Labor Economics 36(4), 1127-1163.
- Baughman, R.; S. Dickert-Conlin (2003): Did Expanding the EITC Promote Motherhood? American Economic Review 93(2), 247-251.
- Baughman, R.; S. Dickert-Conlin (2009): The Earned Income Tax Credit and Fertility. Journal of Population Economics 22(3), 537-563.
- Card, D.; L. D. Shore-Sheppard (2004): Using Discontinuous Eligibility Rules to Identify the Effects of the Federal Medicaid Expansions on Low-Income Children. Review of Economics and Statistics 86(3), 752-766.
- Chetty, R.; E. Saez (2013): Teaching the Tax Code: Earnings Responses to an Experiment with EITC Recipients. American Economic Journal: Applied Economics 5(1), 1-31.
- Dickert-Conlin, S.: A. Chandra (1999): Taxes and the Timing of Births. Journal of Political Economy 107(1), 161-177.
- Dickert-Conlin, S.: S. Houser (2002): EITC and Marriage. National Tax Journal 55(1), 25-42.
- Dollins, D. R.; M. Maynard (2002): Participation in the Earned Income Tax Credit Program for Tax Year 1996. Mimeo. Internal Revenue Service. January 31.
- Dong, Y. (2015): Regression Discontinuity Applications with Rounding Errors in the Running Variable. Journal of Applied Econometrics 30(3), 422-446.
- Duchovny, N. J. (2001): The Earned Income Tax Credit and Fertility. Doctoral dissertation, University of Maryland, College Park.
- Eissa, N.; H. W. Hoynes (1999): Good News for Low-Income Families? Tax-Transfer Schemes and Marriage. University of California, Berkeley. Mimeo.
- Eissa, N.; H. W. Hoynes (2004): Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit. Journal of Public Economics 88(9-10), 1931-1958.
- Eissa, N.; J. B. Liebman (1996): Labor Supply Response to the Earned Income Tax Credit. The Quarterly Journal of Economics 111(2), 605-637.
- Ellwood, D. T. (2000): The Impact of the Earned Income Tax Credit and Social Policy Reforms on Work, Marriage, and Living Arrangements. National Tax Journal 53(4), 1063-1105.
- Evans, W. N.; C. L. Garthwaite (2014). Giving Mom A Break: The Impact of Higher EITC Payments on Maternal Health. American Economic Journal: Economic Policy 6(2), 258-90.
- Gans, J. S.; A. Leigh (2009): Born on the First of July: An (un) Natural Experiment in Birth Timing. Journal of Public Economics 93(1-2), 246-263.
- Green, L.; A. F. Fry; J. Myerson (1994): Discounting of Delayed Rewards: A Life-Span Comparison. Psychological Science 5(1), 33-36.
- Green, L.; J. Myerson; D. Lichtman; S. Rosen; A. Fry (1996): Temporal Discounting in Choice Between Delayed Rewards: The Role of Age and Income. Psychology and Aging 11(1), 79.
- **Grogger, J.** (2003): The Effects of Time Limits, the EITC, and Other Policy Changes on Welfare Use, Work, and Income Among Female-Headed Families. Review of Economics and Statistics 85(2), 394-408.
- Herbst, C. M. (2011): The Impact of the Earned Income Tax Credit on Marriage and Divorce: Evidence from Flow Data. Population Research and Policy Review 30(1), 101-128.
- Hoffman, C. S.; L. C. Messer; P. Mendola; D. A. Savitz; A. H. Herring; K. E. Hartmann (2008): Comparison of Gestational Age at Birth Based on Last Menstrual Period and Ultrasound During the First Trimester. Paediatric and Perinatal Epidemiology 22(6), 587-596.
- Hoynes, H.; D. Miller; D. Simon (2015): Income, the Earned Income Tax Credit, and Infant Health. American Economic Journal: Economic Policy 7(1), 172-211.
- Hoynes, H. W.; A. J. Patel (2018): Effective Policy for Reducing Poverty and Inequality? The Earned Income Tax Credit and the Distribution of Income. Journal of Human Resources 53(4), 859-890.
- Isaac, E. (2018): Marriage, Divorce, and Tax and Transfer Policy. Tulane University, Department of Economics, Working Papers 1810.
- Klerman, J. A.; A. Leibowitz (1994): The Work-Employment Distinction Among New Mothers. Journal of Human Resources 29(2), 277-303.

- Kleven, H. (2019): The EITC and the Extensive Margin: A Reappraisal (No. w26405). National Bureau of Economic Research.
- LaLumia, S.; J. M. Sallee; N. Turner (2015): New Evidence on Taxes and the Timing of Birth. American Economic Journal: Economic Policy 7(2), 258-293.
- Leridon, H. (1977). Human Fertility. University of Chicago Press.
- Manoli, D. S.; N. Turner (2014): Cash-on-Hand & College Enrollment: Evidence from Population Tax Data and Policy Nonlinearities (No. w19836). National Bureau of Economic Research.
- Meyer, B. D. (2010): The Effects of the Earned Income Tax Credit and Recent Reforms. Tax Policy and the Economy 24(1), 153-180.
- Meyer, B. D.; D. T. Rosenbaum (2000): Making Single Mothers Work: Recent Tax and Welfare Policy and Its Effects. National Tax Journal 53(4), 1027-1062.
- Meyer, B. D.; D. T. Rosenbaum (2001). Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers. The Quarterly Journal of Economics 116(3), 1063-1114.
- Michelmore, K. (2016): The Earned Income Tax Credit and Union Formation: The Impact of Expected Spouse Earnings. Review of Economics of the Household 16(2), 377-406.
- Mortenson, J.; H. Schramm; A. Whitten; L. Xu (2018). The Absence of Income Effects at the Onset of Child Tax Benefits. SSRN 3290744.
- Neugart, M.; H. Ohlsson (2013): Economic Incentives and the Timing of Births: Evidence from the German Parental Benefit Reform of 2007. Journal of Population Economics 26(1), 87-108.
- Neumark, D.; P. Shirley (2020): The Long-Run Effects of the Earned Income Tax Credit on Women's Labor Market Outcomes (No. w24114). National Bureau of Economic Research.
- Neumark, D.; W. Wascher (2011): Does a Higher Minimum Wage Enhance the Effectiveness of the Earned Income Tax Credit?. ILR Review 64(4), 712-746.
- Scholz, J. K. (1994): The Earned Income Tax Credit: Participation, Compliance, and Antipoverty Effectiveness. National Tax Journal 47(1), 63-87.
- Schulkind, L., & Shapiro, T. M. (2014): What a Difference a Day Makes: Quantifying the Effects of Birth Timing Manipulation on Infant Health. Journal of Health Economics, 33, 139-158.
- Smith, J. (2009). Can Regression Discontinuity Help Answer an Age-Old Question in Education? The Effect of Age on Elementary and Secondary School Achievement, The B.E. Journal of Economic Analysis & Policy, 9(1).
- Wingender, P.; S. LaLumia (2017): Income Effects on Maternal Labor Supply: Evidence from Child-Related Tax Benefits. National Tax Journal 70(1), 11-52.

Appendix

A Birth Timing Manipulation

Perhaps the single greatest threat to this paper's identification strategy relates to whether EITC-eligible women manipulate their births to take advantage of this and other end-of-year tax policies. If women are aware of the tax benefits associated with having a child before the end of the calendar year, including the EITC, and they can (at least to some extent) manipulate when they have their child, then the validity of the regression discontinuity assumptions are called into question. Without some form of medical intervention it would be extremely difficult, and most likely impossible, to manipulate a birth with any degree of precision A typical gestation period is approximately 280 days (40 weeks), with 80% of births occurring between 37 and 41 weeks (Hoffman et al., 2008). In addition to greatly varying gestation periods, women's ovulation cycles pose an additional challenge for a woman attempting to manipulate her birth. Specifically, women are typically only fertile for approximately 24-48 hours during each menstrual cycle. Issues including variation in menstrual cycle length and ovulation timing differences across cycles can further complicate precise timing of a pregnancy (Leridon, 1977). As a result, the only reliable way for a woman to precisely manipulate when she has her child requires medical intervention, primarily through a cesarean section or labor induction.

This birth manipulation issue has been explored explicitly, most notably by Dickert-Conlin and Chandra (1999) and LaLumia et al. (2015). Both papers seek to answer whether women purposefully shift their birth from early January to late December to take advantage of tax incentives and, if so, to uncover the degree to which this occurs. Dickert-Conlin and Chandra use the NLSY and find substantial tax incentive effects on the probability of a late December birth relative to early January, but LaLumia et al. reach a more nuanced conclusion utilizing the universe of tax returns from 2001 to 2010. LaLumia et al. do indeed find evidence of movements in birth timing, but these effects are quite small for low-income women who are recipients of the EITC. They argue two primary reasons for this: first, many of these women are largely unaware of the benefits involved in timing their births before the end of the calendar year. Second, many of these women are likely insured through Medicaid and, as a result, have less agency over manipulating their birth.

In addition to the evidence provided by LaLumia et al. (2015), I use natality data from the NVSS to analyze birth data over several dimensions in order to explore the degree to which women seemingly manipulate the timing of their births. The natural starting point for this analysis is births by day of the year, shown in Figure A1 in Appendix. Note that the date range for this figure covers 1969–1988, which has next to no overlap with my eventual SIPP sample. In 1989 the NVSS switched from providing day of the year for births to providing month of year and day of the week. This hinders my ability to measure how births by day of the year evolved over the time period with the largest EITC reforms. Nonetheless, even before these large EITC reforms, there is evidence of manipulation around the end of the year.

Figure A2 in Appendix zooms in on the 20-day period around the end of the calendar year, allowing a closer look at the time frame of greatest interest. Unsurprisingly, the two holidays in this period, Christmas Day and New Year's Day, have the lowest average birth counts. Births fall from December 22nd through Christmas, rise from the 26th to the 30th, fall

through the new year, and then normalize soon thereafter.³⁸ If instead of day of the year, the birth data is averaged to week of the year, as shown in Figure A3 in Appendix, things appear slightly less noisy. Figure A3 in Appendix shows that although the first week of January has the lowest average number of births,³⁹ the last week of December is comparable to all of the other surrounding weeks. On the whole, it does not seem that births in the first week of January are being pushed into the last week of December. Finally, if we average birth data (now using NVSS data from 1969 through 2016) all the way up to the month level, which is the level at which estimation is performed, births appear relatively uniform throughout the year; Figure A4 in Appendix shows this distribution.

Figure A4 in Appendix was able to cover all years of NVSS natality data (1969–2016) because month of birth is provided consistently over the entire data set. However, as previously mentioned, the day of the week for each birth is provided in lieu of the day of the year from 1989 onwards. Day of the week is, of course, a less useful piece of information to have than day of the year when analyzing the manipulation of birth timing. Nevertheless, I explore this issue as well as the data will allow, starting with Figure A5 Appendix which shows the share of births from 1989 to 2016 by day of the week. There is a clear drop-off in births on the weekend, clearly indicating that some manipulation in birth timing occurs. However, this pattern is much less dramatic when births are broken out between method of payment. Indeed, Figure A6 in Appendix shows the same births broken into two categories: those paid for by Medicaid and by private insurance. This breakdown shows that, though not uniform, Medicaid deliveries are more evenly spaced throughout the week. This could be driven by a number of factors, including that women on Medicaid have less agency over decisions related to their birth or because they tend to be younger and thus more likely to deliver naturally and more uniformly across the week. Because Medicaid recipients are low-income they will, conditional on working, be eligible for the EITC. This pattern is reassuring for the identification strategy employed in this paper.

Finally, Appendix Tables A7 and A8 show all births and first births, respectively, by method of delivery from 1989 to 2016. As the figure shows, both vaginal and Cesarean births are lower on the weekends, although this phenomenon is not noticeably stronger or weaker for either group.

B Covariate Analysis through Discontinuity

Appendix A covered in detail the first identifying assumption of the regression discontinuity design, namely that EITC-eligible women are unable to precisely time their birth in order to shift their treatment status. I will now address the second identifying assumption regarding whether the jump in treatment status at the end-of-year cut-off is the only discontinuity. To accomplish this, I examine whether there are any discontinuities in observable characteristics through that cut-off. Figures B1–B5 in Appendix present plots of the observables I use to divide the sample (marital status, education level) and use as control variables in my regressions (race and age) by month of birth. Figures B1–B4 in Appendix show age and fraction black

³⁸ This effect is not driven by day-of-the-week effects. Weekends, defined as either Fridays, Saturdays, and Sundays or Saturdays and Sundays are slightly under-represented from 1969 to 1988.

³⁹ The other outlier in Figure A3 of Appendix A shows the week that includes February 29th.



Figure A1 Births by day of year in the United States, 1969–1988.

Figure A2 Births from December 22 to January 10 in the United States, 1969–1988.



by the birth month of first/only children for each of my four samples: all unmarried, low-ed unmarried, all married, and low-ed married women, respectively. The last figure in this set, Figure B5 in Appendix, shows full sample balance tests of marital status and education level.⁴⁰

⁴⁰ I also run these balance tests with a linear functional form, although I do not show them here. There are no qualitative differences across that dimension.



Figure A3 Births by week of year in the United States, 1969–1988.

Figure A4 Births by month of year in the United States, 1969–2016.



For all unmarried women and low-ed unmarried women (Figures B1 and B2 in Appendix), there is no statistically significant evidence that race and age are not balanced across treatment and control groups. In both bottom panels, the age of women declines for those with



Figure A5 Fraction of births by day of week, 1989–2016.



Figure A6 Share of births by day of week and source of payment, 1989–2016.

December births, implying that the December mothers tend to be slightly younger. If birth manipulation were to occur, it could be more likely that older women do so as they are more likely to have birth via labor induction or a Cesarean section. Arguably the most troubling



Figure A7 Births by day of week and method of delivery, 1989–2016.



Figure A8 First births by day of week and method of delivery, 1989–2016.

graph is in the top panel of Figure B3 in Appendix, where some suggestive evidence can be seen that married black women are more likely to have a December birth. There are a few potential explanations, with varying implications for my regression discontinuity design. The worst-case



Figure B1 Covariate balance tests for all unmarried women by month of first birth.

scenario is that this is driven by birth manipulation, with black mothers more likely to push their birth back into December, which would likely negatively bias my estimates of the EITC's labor market effects. Recall in Appendix A, if mothers have less agency over the timing of their births due to income or insurance status, they cannot (or have less ability) to time their birth. Doctors' preferences also play a role here, though, as some part of this pattern could rest with how they schedule Cesareans or labor inductions before the end of the year. However, I would expect that if birth timing manipulation to occur in a manner that would threaten the validity of the RD design, it would have been driven by older, more well-educated, white mothers. These graphs do not align with that hypothesis.



Figure B2 Covariate balance tests for low-ed unmarried women by month of first birth.

Finally, I examine in Figure B5 in Appendix whether any systematic differences in education level or marital status can be seen across the threshold. The relationship between education and birth timing flips sign for unmarried (negative) and married (positive) women. Of course, the size of the effect is not statistically different from zero. The low-ed unmarried group, who are usually of the greatest interest, the fraction of December births is actually slightly lower than January births which is, of course, counter to what I would expect if birth manipulation were an issue. Overall, these figures reassure the validity of the RD design in this context.



Figure B3 Covariate balance tests for all married women by month of first birth.



Figure B4 Covariate balance tests for low-ed married women by month of first birth.



Figure B5 Covariate balance tests for low-ed and marital status by month of first birth.

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women
Panel A: Employm	ient			
Linear	0.004	0.004	0.017***	-0.001
	(0.006)	(0.007)	(0.006)	(0.008)
Quadratic	0.009	0.017	0.016*	0.006
	(0.011)	(0.012)	(0.009)	(0.013)
Panel B: IHS earni	ings			
Linear	0.027	0.022	0.167***	0.002
	(0.055)	(0.059)	(0.050)	(0.066)
Quadratic	0.074	0.0149	0.166**	0.052
	(0.091)	(0.098)	(0.079)	(0.105)
Ν	24,317	15,587	69,746	22,694
Panel C: Log earn	ings			
Linear	0.017	0.013	0.027**	0.002
	(0.016)	(0.027)	(0.013)	(0.022)
Quadratic	0.016	-0.001	0.024	-0.001
	(0.018)	(0.031)	(0.020)	(0.031)
Ν	10,435	5,499	32,360	7,409
To 1 Disc.	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Y	Υ	Y	Y
Controls	Y	Y	Υ	Y

|--|

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for two or three children. The coefficient shown is the difference between the discontinuity estimates for two children and three children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

 ${\sf Employment} \ is \ defined \ as \ having \ monthly \ earnings \ of \ at \ least \ \$250 \ in \ 2016 \ dollars.$

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women	
Panel A: Employm	ient				
Linear	0.029	0.021	-0.012	-0.007	
	(0.026)	(0.035)	(0.018)	(0.038)	
Quadratic	0.021	0.005	-0.012	-0.012	
	(0.033)	(0.039)	(0.018)	(0.036)	
Panel B: IHS earni	ngs				
Linear	0.341	0.243	-0.083	-0.100	
	(0.214)	(0.263)	(0.150)	(0.321)	
Quadratic	0.260	0.094	-0.103	-0.152	
	(0.260)	(0.296)	(0.158)	(0.309)	
Ν	42,200	25,285	111,580	32,497	
Panel C: Log earnings					
Linear	0.103	0.156*	-0.059	-0.040	
	(0.073)	(0.085)	(0.048)	(0.089)	
Quadratic	0.106	0.179**	-0.069	-0.038	
	(0.082)	(0.083)	(0.046)	(0.103)	
Ν	19,318	9,304	56,118	11,093	
To 1 Disc.	Ν	Ν	Ν	Ν	
To Max Cr. Disc.	Y	Υ	Y	Y	
Controls	Υ	Y	Υ	Υ	

Table A2 Effects of the ETIC on labor market outcomes, one to two children and two children to three children difference-diff-in-disc results

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included three times, each time interacted with an indicator for one, two, or three children. The coefficient shown is the difference between the discontinuity estimates for one child and two children minus the difference between the discontinuity estimates for two and three children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women	
Panel A: Employn	nent				
Linear	0.023	0.032	0.011	-0.009	
	(0.031)	(0.044)	(0.018)	(0.037)	
Quadratic	0.023	0.033	0.015	0.006	
	(0.033)	(0.045)	(0.020)	(0.036)	
Panel B: IHS earn	ings				
Linear	0.269	0.351	0.131	-0.053	
	(0.245)	(0.346)	(0.165)	(0.311)	
Quadratic	0.271	0.353	0.153	0.042	
	(0.272)	(0.354)	(0.174)	(0.304)	
Ν	13,642	7,972	35,708	9,317	
Panel C: Log earnings					
Linear	0.109	0.215	-0.035	-0.181*	
	(0.107)	(0.134)	(0.049)	(0.098)	
Quadratic	0.111	0.211	-0.058	-0.116	
	(0.108)	(0.128)	(0.045)	(0.098)	
Ν	6,048	2,792	18,506	3,236	
To 1 Disc.	Ν	Ν	Ν	Ν	
To Max Cr. Disc.	Y	Υ	Y	Y	
Controls	Y	Y	Y	Y	

Table A3 Effects of the ETIC on labor market outcomes, results for women's first child ages 1 to 5 months

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

Functional form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women	
Panel A: Employm	nent				
Linear	0.038	-0.001	0.006	-0.043	
	(0.024)	(0.034)	(0.018)	(0.032)	
Quadratic	0.037	-0.006	0.009	-0.033	
	(0.029)	(0.035)	(0.020)	(0.035)	
Panel B: IHS earni	ings				
Linear	0.386*	0.050	0.092	-0.367	
	(0.200)	(0.261)	(0.154)	(0.272)	
Quadratic	0.375	0.009	0.103	-0.291	
	(0.264)	(0.274)	(0.176)	(0.297)	
Ν	33,043	19,215	86,875	23,130	
Panel C: Log earnings					
Linear	0.106	0.109	-0.051	-0.188***	
	(0.090)	(0.106)	(0.060)	(0.069)	
Quadratic	0.113	0.120	-0.077	-0.134*	
	(0.093)	(0.110)	(0.056)	(0.077)	
Ν	15,647	7,274	46,496	8,448	
To 1 Disc.	Ν	Ν	Ν	Ν	
To Max Cr. Disc.	Y	Υ	Y	Y	
Controls	Y	Υ	Y	Y	

Table A4	Effects of the ETIC on labor market outcomes.	weighted regressions for women's first child

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

Weighting is performed using provided sampling weights.

Functional form	All unmarried Low-ed unmarried All married women women		Low-ed married women				
Panel A: Employment, \$500 monthly earnings							
Linear	0.028	0.012	-0.0001	-0.035			
	(0.022)	(0.027)	(0.018)	(0.034)			
Quadratic	0.030	0.015	0.001	-0.026			
	(0.027)	(0.031)	(0.020)	(0.035)			
Panel B: Monthly	earnings						
Linear	176.76**	53.20	78.36	-118.33			
	(80.38)	(57.53)	(104.52)	(107.47)			
Quadratic	183.67*	58.68	45.68	-75.81			
	(92.26)	(60.56)	(109.20)	(115.01)			
Ν	33,043	19,215	86,875	23,130			
Panel C: Trimmed log earnings							
Linear	0.109	0.139	-0.004	-0.124*			
	(0.078)	(0.101)	(0.038)	(0.065)			
Quadratic	0.106	0.137	-0.024	-0.083			
	(0.080)	(0.102)	(0.034)	(0.069)			
Ν	14,732	7,169	35,657	8,080			
To 1 Disc.	Ν	Ν	Ν	Ν			
To Max Cr. Disc.	Y	Υ	Y	Y			
Controls	Y	Y	Υ	Y			

Table A5 Effects of the ETIC on labor market outcomes, regressions for women's first child with alternate employment definitions and trimmed earnings

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$500 in 2016 dollars in Panel A.

In Panel C, the top 10% of earners from each sample are excluded.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

Functional form	All unmarried Women	Low-ed unmarried women	All married women	Low-ed married women		
Panel A: Employn	nent					
Linear 0.026		0.016	0.008	-0.021		
	(0.022)	(0.027)	(0.015)	(0.032)		
Quadratic	0.023	0.014	0.009	-0.011		
	(0.028)	(0.031)	(0.018)	(0.032)		
Panel B: IHS earn	ings					
Linear	0.301	0.207	0.096	-0.220		
	(0.180)	(0.205)	(0.133)	(0.265)		
Quadratic	0.269	0.179	0.089	-0.153		
	(0.231)	(0.238)	(0.159)	(0.269)		
Ν	33,043	19,215	86,875	23,130		
Panel C: Log earnings						
Linear	0.108*	0.145*	-0.011	-0.131*		
	(0.059)	(0.076)	(0.036)	(0.071)		
Quadratic	0.104	0.143*	-0.036	-0.100		
	(0.064)	(0.077)	(0.032)	(0.078)		
Ν	15,647	7,274	46,496	8,448		
To 1 Disc.	Ν	Ν	Ν	Ν		
to Phase-in Disc.	Y	Y	Y	Y		
Controls	Y	Υ	Y	Υ		

Table A6 Effects of the ETIC on labor market outcomes, regressions using phase-in rate for women's first child

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

Functional Form	All unmarried women	Low-ed unmarried women	All married women	Low-ed married women	
Panel A: Employn	nent				
Linear	-0.026	-0.073	0.024	-0.013	
	(0.043)	(0.049)	(0.021)	(0.045)	
Quadratic	-0.020	-0.070	0.031	0.008	
	(0.050)	(0.056)	(0.023)	(0.047)	
Panel B: IHS earn	ings				
Linear	-0.126	-0.570	0.271	-0.152	
	(0.351)	(0.390)	(0.184)	(0.370)	
Quadratic	-0.088	-0.535	0.317	0.011	
	(0.403)	(0.438)	(0.203)	(0.388)	
Ν	33,043	19,215	86,875	23,130	
Panel C: Log earnings					
Linear	0.208**	0.034	0.018	-0.184**	
	(0.092)	(0.143)	(0.057)	(0.085)	
Quadratic	0.209**	0.034	-0.019	-0.130	
	(0.098)	(0.146)	(0.051)	(0.092)	
Ν	15,647	7,274	46,496	8,448	
To 1 Disc.	Ν	Ν	Ν	Ν	
To Max Cr. Disc.	Υ	Υ	Υ	Y	
Controls	Y	Υ	Υ	Y	

Table A7 Effects of the ETIC on labor market outcomes, using only federal variation for women's first child

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

 ${\sf Employment} \ is \ defined \ as \ having \ monthly \ earnings \ of \ at \ least \ \$250 \ in \ 2016 \ dollars.$

Low-ed is defined as having a high school degree or less.

Standard errors are clustered at the state level.

Functional form	All ur we	nmarried omen	Low-ed unmarried women		All married women		Low-ed married women	
Race	Black	Non-black	Black	Non-black	Black	Non-black	Black	Non-black
Panel A: Employ	ment							
Linear	0.091	-0.007	0.010	-0.002	-0.055	0.012	0.011	0.0003
	(0.062)	(0.031)	(0.046)	(0.040)	(0.080)	(0.018)	(0.112)	(0.036)
Quadratic	0.081	-0.004	0.005	0.002	-0.041	0.014	-0.001	0.014
	(0.071)	(0.036)	(0.048)	(0.045)	(0.086)	(0.020)	(0.109)	(0.036)
Panel B: IHS ear	nings							
Linear	0.800	0.067	0.149	0.075	-0.528	0.155	-0.032	-0.031
	(0.534)	(0.250)	(0.366)	(0.319)	(0.681)	(0.157)	(0.947)	(0.295)
Quadratic	0.734	0.076	0.119	0.101	-0.402	0.156	-0.153	0.073
	(0.610)	(0.289)	(0.392)	(0.355)	(0.740)	(0.175)	(0.912)	(0.301)
Ν	9,088	23,955	5,061	14,154	5,147	81,728	1,541	21,589
Panel C: Log earnings								
Linear	0.089	0.108	0.178	0.080	-0.057	-0.006	0.214	-0.168**
	(0.112)	(0.100)	(0.210)	(0.104)	(0.116)	(0.044)	(0.379)	(0.078)
Quadratic	0.094	0.104	0.136	0.093	-0.046	-0.036	0.062	-0.128
	(0.118)	(0.103)	(0.200)	(0.104)	(0.118)	(0.038)	(0.384)	(0.082)
Ν	4,306	11,341	1,797	5,477	3,005	43,491	571	7,877
To 1 Disc.	Ν	Ν	Ν	Ν	Ν	Ν	Ν	Ν
To Max Cr. Disc.	Υ	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Υ

Table A8 Effects of the ETIC on labor market outcomes, regressions for women with first children by race

Data Source: Survey of Income and Program Participation, 1996 to 2014 Panels.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*secondhalf of the year birth (July–December). Quadratic functional form regressions also include time squared and time squared*second half birth. Each of these parameters is included twice, each time interacted with an indicator for one or two children. The coefficient shown is the difference between the discontinuity estimates for one child and two children.

Control variables include state and cohort fixed effects as well as age, age squared, and black race indicator. Also included is (the sum of personal tax exemption, CTC, and single to head of household deduction difference*unmarried)*second half of the year birth.

Employment is defined as having monthly earnings of at least \$250 in 2016 dollars.

Standard errors are clustered at the state level.