The EITC and Employment Transitions: Labor Force Attachment and Annual Exit

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Abstract

Many low-income households experience high frequency labor market transitions. It is unclear how the Earned Income Tax Credit (EITC) work incentives affect these frequent entry and exit decisions. Exploiting the panel nature of the Current Population Survey, I show that EITC expansions induce less-educated single women with previous work experience to work more months, leading to more annual weeks worked and less annual exit, suggesting the EITC operates in part by keeping previously employed single women in the labor force. Employment responds to changes in the previous year's EITC, consistent with people learning about how the returns to work have changed, and then responding.

Keywords: EITC, labor supply, turnover, employment stability
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I Introduction

The Earned Income Tax Credit (EITC) is one of the largest components of the US social safety net, transferring over $67 billion to 27 million households (IRS, 2017) and lifting over 6 million people out of poverty each year (Short, 2011; Hoynes & Patel, 2016). The EITC unambiguously incentivizes households to work at some point during the year, and the literature has consistently found evidence that the EITC increases annual employment rates of less-educated single women, a group often eligible for the program (Nichols & Rothstein, 2015).

Despite the annual incentives of the EITC, both longitudinal data and ethnographic evidence suggest single women with a high school degree or less make extensive margin employment decisions at a much more frequent interval, multiple times a year (Schochet & Rangarajan, 2004; Edin & Lein, 1997). In this paper, I evaluate how increases in EITC generosity affect less educated single women's within and across year labor force attachment and transitions out of the labor force. Although the literature has established the effect of the EITC on annual employment rates, it is unclear whether a change in inflows or outflows have led to the rise. We also do not know how the EITC affects the probability and frequency of employment transitions. Understanding these entry and exit transitions can help us better understand the channels through which the EITC affects annual employment rates, who responds to the EITC, and the implications this has as an anti-poverty policy.

For a less-educated single woman (high school degree or less), the returns to a low-wage job are often offset by monetary and psychic employment costs as well as forgone means-tested benefits. When the net returns to work are low, even small week-to-week cost fluctuations, such as the need to stay home with a sick child or a minor car repair, can induce a woman to reduce labor supply. If faced with labor market rigidities that prevent adjustment at the intensive margin, this can result in volatile labor force attachment. For low levels of income the EITC operates as a wage subsidy, essentially raising the opportunity cost of dropping out of the labor force. If the substitution effects outweigh the income effects, this should increase the probability of staying in the labor force and mechanically increase the length of employment spells leading to less annual level exit. In this paper I empirically explore these hypotheses.

The annual, repeated cross-section data used by most of the EITC literature cannot inform us about within year decisions. I exploit the short panel nature of the CPS by linking individuals across months and years (following the methods outlined in Lefgren and Madrian (2000) and Rivera Drew, Flood, & Warren, (2014)) to estimate how year-to-year increases in federal EITC generosity between 1989 and 1995 affect these high frequency employment decisions. This allows me to observe how single women with one or more eligible children changed their behavior from one year to the next after an increase in EITC generosity, relative to single women without children who were unaffected by the change.

Unlike the previous work, this strategy allows me to evaluate within year measures and exploit within person differences through an individual fixed effect strategy. By linking individuals' monthly CPS surveys across years I observe a four month snap-shot of within year employment decisions and can estimate the impact of the EITC on within year employment transitions. The individual fixed effect strategy allows me to control for time invariant individual characteristics, and separate the extensive margin effect from the intensive margin. Estimating the effect of the EITC on employment transitions and duration is not possible without a panel structure. For a one hundred dollar (2010$) increase in the lagged maximum EITC (the EITC a woman is eligible to receive), the probability of being employed during a four month period increased by one percentage point. The share of months working also increases by one percentage point. These estimates would suggest the large expansion in 1994 increased the share of months employed by 10 percentage points (12 percent). This response is only observed among women with labor force participation reported during the initial year. Despite this increase in the share of months employed, many women still eventually dropped out during the sample period. However, the incidence of multiple exits fell, suggesting that within year exit was reduced but not eliminated. These women still transitioned between employment and non-employment within the year, but less frequently than before. These patterns are robust and not present among more-educated single women who generally fall outside the EITC region or similarly educated single women that report household incomes above the EITC region.

Given this increased labor force attachment within the year, I next use the linked annual ASEC March CPS supplement, to see if this within year increase in attachment affects annual level transitions out of the labor force. Consistent with the monthly analysis, less-educated single women responded to an increase in EITC generosity by working more weeks during the year. This response is concentrated among women who worked before the increase in EITC generosity, and results in more women staying in the labor force from year to year, effectively reducing annual level exit. No increase was detected among women who were not initially working, suggesting the EITC expansions during this period increased labor force participation largely by reducing exit among women with previous labor force attachment, rather than pulling in new participants.

In the data, employment decisions respond to changes in lagged EITC policy, which affects the maximum credit eligible to *receive* in the current year rather than changes in the current EITC policy, which affects the maximum credit eligible to *earn* in the current year. The data are consistent with Nichols & Rothstein's (2015) hypothesis that individuals learn about the program and respond *ex post*, while alternative explanations, such as the EITC relaxing liquidity constraints and allowing women to continue working by increasing cash on hand (e.g., they can continue paying for childcare) are less supported by the data. This response to changes in lagged policy, might help explain why the previous work has found that it takes several years for women to fully respond to the EITC (Eissa & Liebman, 1996; Meyer & Rosenbaum, 2001; Hoynes & Patel, 2017; Bastian, 2018), and is consistent with people learning about the program as they interact with it.

Although previous work documents that the EITC increases annual employment, this work sheds light on how it occurs. The EITC increases labor force attachment among less educated single women with previous labor force participation, in part by delaying exit from the labor force. This increase in employment stability might generate potential benefits of the EITC not captured by the previous annual level analysis. These results suggest that the EITC increases employment in the short run by keeping individuals at the margin in the labor market, rather than bringing in new entrants. This is important to keep in mind when evaluating the EITC as an anti-poverty policy, which is often advertised as a way to bring new people into the labor force. My estimates from the data suggest rather, that the EITC is effective at keeping people in the labor force who would have otherwise dropped out. As such, expansion of the EITC to populations disconnected from the labor force might not result in the same labor supply responses.

II The EITC and Labor Force Attachment of Single Women

A large literature explores the structure and impacts of the EITC. For brevity I highlight the related work exploring labor supply impacts and refer the reader to Hotz and Scholz (2003) or Nichols and Rothstein (2015) for a more complete review. The EITC was introduced in 1975 as a refundable annual tax credit and is structured to reward work.[1] A household with no earned income does not receive a credit, but for each additional dollar of income the credit increases, creating large negative tax rates for a short phase-in region, followed by a zero marginal tax rate plateau, and a gradual phase-out region (see Figure 1 for sample parameters in 1989, 1991, and 1994). The program has expanded several times since 1975, with the largest expansions occurring in 1994, 1995 and 1996, as seen in Figure 2.

Eissa and Liebman's (1996) pioneering work on the EITC exploits variation from the 1986 expansion and compares single mothers with a high school degree or less to similarly educated single women without children and to more educated single mothers, to show that

[1] The Advance EITC allowed recipients to receive their credit in advance with their paycheck throughout the year. However, take-up was extremely low (2-3%) and this provision was removed in 2011.

this early expansion increased labor force participation by 2.8 percentage points. Later expansions between 1993 and 1996 differentially affected households with one child and multiple children and also introduced a small credit for households without children. Much of the work exploring the EITC focuses on these expansions and compared single mothers with one child to single mothers with multiple children before and after the expansion, finding that annual employment increased by approximately 3-6 percentage points (Meyer & Rosenbaum, 2001; Hotz, Mullin, & Scholz, 2005). Most of the work relies on repeated cross-sections from the March ASEC CPS supplement (Eissa & Liebman, 1996; Meyer & Rosenbaum, 2001; Eissa & Hoynes, 2004), although some make use of longitudinal administrative data (Hotz, et al., 2005), and the National Longitudinal Survey of Youth (NLSY) in related work exploring annual transitions when households become no longer EITC eligible (Moulton, Graddy-Reed, & Lanahan, 2016).[2] During the 1990s, welfare policy and the economy were also changing and might differentially affect the single mothers with one child and multiple children (Looney & Manoli, 2013). This might lead to differential trends by family status and must be accounted for in the estimation (Meyer & Rosenbaum, 2001).

Although there is ample evidence of a strong, extensive margin effect, there is less conclusive evidence on how individuals adjust at the intensive margin. Nichols and Rothstein (2015) highlight several reasons why this might be. Because the EITC induces participation, the composition of workers changes and repeated cross-sectional data cannot separately identify behavioral responses by those already working and compositional changes due to new entrants. Separating these channels requires an individual's work history both before and after the policy, which has not been available for most of the previous work. Labor market frictions also make it difficult to adjust at the intensive margin. Presumably, many low wage workers face fixed work schedules and cannot flexibly adjust the number of hours worked.

[2] To the best of my knowledge, only one concurrent working paper uses the longitudinal nature of the CPS to explore the EITC. Yucong (wp, 2016) links individuals in the March ASEC supplements to capture employment in the previous year and then estimates a repeated cross-section difference-in-differences by previous employment status to explore the effect of welfare reform and the 1993 EITC expansion on annual entry and exit. These results are difficult to interpret because the sample is conditioned on previous employment (the lagged outcome), which is potentially responding to the policy change.

This rigidity might prevent them from adjusting at the intensive margin, even if they would prefer to do so. Finally, the EITC is a function of annual income, which might be difficult for individuals with volatile labor force attachment to predict. Uncertainty about annual income can lead to uncertainty about marginal incentives, which might lead to less adjustment at the intensive margin.

Recent work exploits detailed administrative data to understand intensive margin responses to the EITC. Saez (2010) looks at kink points in the EITC schedule and finds no evidence of bunching or intensive margin responses among wage workers, but substantial bunching among self-employed workers at the first kink in the EITC schedule. Chetty and Saez (2013) randomly assign H&R Block clients to receive individualized information from the tax preparer about where they fall on the EITC schedule and how additional work would affect their credit in the subsequent year. This information did not change earnings in the following year on average, but there is significant heterogeneity across tax preparers. Using tax data, Chetty, Friedman, and Saez (2013) find that when people move to areas that have more "knowledge" of the EITC schedule they report incomes closer to the refund-maximizing level. This is even true among wage workers. Overall the previous work suggests that either intensive margin elasticities are small or highly dependent on the information people have access to.

Despite the EITC's annual incentives, the empirical patterns suggest that less-educated single women make extensive margin employment decisions at a much more frequent interval. From the CPS in the 1990s, 16-28 percent of single women with a high school degree or less who ever reported employment during a four month period, also entered or exited employment (see Table 1). In comparison, among single women with a college degree, this rate is less than 10 percent. From the 1996 SIPP, 38 percent of women in low-wage jobs had left the job by four months, and 53 percent had left by eight months (Schochet & Rangarajan, 2004).[3] Over half of these exits were to non-employment. Edin and Lein (1997)

[3]For reference, these rates are only 1/2-2/3 as large for women in high wage jobs.

present ethnographic evidence from the early 1990s that single mothers frequently transition between welfare and employment, and cycled through multiple jobs in a year. They conclude that when deciding to work or claim welfare, these single mothers often weighed the costs and benefit of each option and, "made reasonable assessments of how much they would need to earn to offset the added costs of work (p. 63)."[4]

Given the empirical evidence it seems likely that many less educated single women make frequent (monthly or weekly) employment decisions. To understand how the work incentives of the EITC might affect these within year labor supply decisions, consider the following scenario (please see Appendix B for a complete, stylized model). Suppose a high school dropout, single mother is trying to work and support her family. However, for her, working is relatively costly. If she chooses to work she faces low wage pay, might face childcare and transportation costs, and potentially forgoes transfer income from government programs such as Aid to Families with Dependent Children (AFDC), Temporary Assistance for Needy Families (TANF), or Supplemental Nutritional Assistance Program (SNAP). The net benefits of working might be small and as Edin and Lein (1997) suggest, working might be a "financial wash". She is also likely to face labor market rigidities (such as inflexible work schedules) that make it infeasible to adjust labor supply at the intensive margin.

If suddenly the woman encounters an unexpected cost –such as a child becoming ill and unable to attend daycare, or an unexpected car repair– the value of going in to work that week might turn negative prompting her to quit her job or not show up and run the risk of being fired. Because the net benefit of working is already small, these unexpected events do not even have to be extremely costly to induce the woman to exit her job and return to the labor force when the situation is resolved. Empirically, this would result in more transitions in and out of the labor force.

For low levels of income, the EITC operates as a wage subsidy, essentially increasing the benefit of working and the opportunity cost of exiting the labor force. Now, when an unex-

[4] Sometimes this comparison was crude or approximate, while other mothers were able to recall or provide exact calculations on scraps of paper or the back of envelopes.

pected event occurs, it is more costly to exit the labor force, because she is not only forgoing her wage earnings, but the associated EITC credit that will be given in the future. This is likely to induce a substitution effect, pushing her towards more stable employment. On the other hand, increases in EITC generosity might increase future income, inducing income effects that work against the substitution effects. In general, the net effect is ambiguous and remains an empirical question. However, at low levels of annual earnings substitution effects are likely to dominate income effects. This case seems particularly relevant for many less educated single women; in the CPS over one-third of them have household incomes in the phase-in or plateau region of the EITC.

For some fraction of women, the increase in EITC generosity will likely increase the threshold of unexpected costs they are willing to incur to maintain their jobs. At the margin we would expect some women to become more attached to the labor force when the EITC becomes more generous. Increased attachment will lead to fewer exits over time, and more stable employment.

These labor supply responses will depend on whether people consider average or marginal tax rates. As seen in Figure 3, the increase in EITC generosity between 1993 and 1994 unambiguously increased the average EITC subsidy rate, leading to a lower average tax rate for EITC eligible households with two children. However, the effect on the EITC component of the marginal tax rates varied across the EITC schedule. The marginal tax rate became more negative in the phase-in region, the marginal tax rate increased in the phase-out region (became more positive), and the plateau region shortened, with some households experiencing higher, lower, or the same marginal tax rate.[5] Previous work suggests that people facing a multipart tax schedule, such as the EITC, respond to average tax rates rather than marginal tax rates, a phenomenon referred to as "ironing" in the public finance literature (Liebman & Zeckhauser, 2004). If people consider average tax rates, the incentives

[5]For reference, from the 1994 CPS ASEC, approximately 30.5 percent of single women with children had family income in the phase-in region where both average and marginal tax rates were negative. An additional 10.3 percent were in the plateau region where the average tax rate was negative and the marginal tax rate was zero.

described above will be at play. However if people consider marginal tax rates, the average effects will be ambiguous and remains an empirical question.[6]

Mead (2014) raises the concern that many potential recipients lacked information about the program during this time period, casting doubt that it was the EITC driving employment rates during the 1990s. If women did not know about the EITC and never learn about it, we would expect no employment response. However, because the EITC is designed as a tax credit, annual tax filing provides an automatic mechanism for informing potential recipients about the program (Nichols & Rothstein, 2015). Even if potential recipients were not aware of the credit *a priori*, they might become aware of the program when filing taxes and adjust labor supply behavior *ex post* after perceiving that the returns to work are larger than initially believed.[7] This might update her expectations for the future about the value of work and induce the behavior described above. If she did not work in the previous year, she will not receive an EITC when she files her taxes, and might not adjust her expectations about the value of work. As such, we might expect employment responses to the EITC to be concentrated among those who were previously employed and potentially learned about the program, and might occur with a year lag, after households observe the increased EITC generosity and update their perceptions of the returns to work.

As noted earlier, there is an established literature evaluating the role of information in the context of labor supply responses to the EITC (Chetty & Saez, 2013; Chetty, Friedman, & Saez, 2013). The type of information considered in this paper differs from the previous work in several important ways. First, the previous work focuses on intensive margin responses to information during the mid-2000s, when the overall economic climate and knowledge of

[6]One explanation for this is that it might be difficult for households with volatile labor force attachment to accurately predict their location on the schedule to identify the marginal tax rate, whereas average tax rates might be more salient. This logic has been used to explain the strong extensive margin response and weaker evidence of intensive margin responses (Liebman, 1998). During the 1990s and even through the 2000s, only the amount of the EITC credit was reported on the IRS Form 1040. As such, a filer using a tax preparer would only know the amount of the credit in relation to her earned income. Even self-prepared filers using Schedule 596 EITC instructions would only see a table similar to a tax table, which does not report marginal tax rates, although they could be calculated.

[7]A majority of EITC recipients used a tax preparer (Nichols & Rothstein, 2015), who was likely aware of the EITC and shared some level of information with the filer.

the EITC is markedly different than in the 1990s. Unlike the previous work, which focuses on information about an individual's location on the EITC schedule, this work explores the role of information and learning associated with changes in EITC policy. Optimal responses are also likely to change in response to the policy changes. The information exploited here reflects variation in the timing of learning while the previous work addresses salience about where a person is on the EITC schedule, which might affect decisions differently.

III Data

Testing these predictions requires frequent, within-year observations of labor supply. Tax data only provides annual measures, while administrative unemployment insurance data are only quarterly. To approximate these high frequency decisions, I exploit the monthly panel nature of the CPS obtained through IPUMS (Flood et al., 2015). Much of the previous work relies on annual repeated cross-sections of the CPS and ignores the short panel nature of the survey. Since 1953, the CPS has conducted repeated interviews with households (Rivera Drew et. al, 2014). Each household is interviewed for four consecutive months (a survey wave), rotated out of the sample for eight months, and then re-enters the survey for four consecutive months.[8] Households that do not change addresses will be interviewed for the same four consecutive months two years in a row (e.g., January-April in both 1993 and 1994). During each monthly survey round, participants are asked about hours worked and employment status in the previous week, making it possible to create a four month employment history for each participant at the same point in two consecutive years.[9] In the ASEC March supplement, participants are also asked about the total number of weeks worked during the previous calendar year, allowing researchers to look at annual outcomes.

[8]For an in-depth description of the design, panel nature, and linking methods of the CPS, see Lefgren & Madrian (2000) or Rivera Drew et. al, (2014).

[9]People are allowed to report the following employment statuses: in the Armed Forced; At work; Has job, not at work last week; Unemployed, experienced worker; Unemployed, new worker; Not in labor force (NILF), unable to work; NILF other; NILF retired. To help separate labor supply decisions from local labor demand, people reporting "At work" and "Has job, not at work last week" are considered employed.

Previous work documents concerns and constraints associated with linking the CPS (Lefgren & Madrian, 2000) as well as identifies more accurate ways of linking individuals (Rivera Drew et al., 2014). The rotating structure of the CPS means that during any month, only a subset of the sample will be observed in the following month (approximately 75 percent) or following year (approximately 50 percent). Also, since the CPS tracks residential addresses, individuals might be erroneously linked if they move to or away from the residence between survey waves. I follow the practices explained in these papers and use the IPUMS created CPS person IDs to link participants across months and across ASEC supplements from year to year. I then validate these matches by comparing demographic characteristics as suggested by Lefgren and Madrian (2000) and Rivera Drew et al., (2014).[10] As seen in Table 1, the linked sample of single women is similar to the full sample, although slightly more positively selected. The linked sample is slightly more educated, more likely to be Non-Hispanic white, and more attached to the labor force. In Appendix Table A.1 I reweight the linked sample to look like the full sample, and show that the pattern of results is similar and the coefficients are only slightly smaller.

I use the household roster to determine how many EITC eligible children each woman has in the household during any month. Current college enrollment status is not available in the monthly survey, so only children ages 0-18 are counted as EITC eligible children. For each individual I define the number of EITC eligible children as the maximum number of eligible children reported at any time during the sample period.[11] I then restrict my sample to single mothers with a high school degree or less (during both years) who were 19 or older and less than 45 during the survey months.[12] Estimates are consistent if I limit my sample

[10]Following Lefgren and Madrian (2000) I preserve matches across months as valid if the individual's sex, race, and age (within two years) is consistent across all months. To further improve match quality, I then use detailed industry information, education, and number of children to see if invalid matches are the same along these characteristics. If these characteristics are exactly the same, I keep these matches as any difference in sex, race, or age is likely a coding error. The results are virtually the same if these additional matches are dropped.

[11]This removes endogenous changes in the maximum EITC that are due to changes in household composition, such as births or children ageing out. In my sample only 2.4 percent of women have a baby between years and 1 percent have a child age-out. If I exclude these women, the estimates are essentially unchanged.

[12]Previous work also excludes women who are enrolled as full time students, ill or disabled, or had positive

to mothers 25 and older, to avoid concerns that younger women with and without children might be less comparable. Using the survey year and number of eligible children, I combine the CPS data with historic federal EITC parameters from the Tax Policy Center (2015), which vary with the number of children and across years to determine the maximum credit each woman could receive. This is converted to 2010 dollars using the personal consumption index from the Bureau of Economic Analysis. This EITC measure captures the height of the plateau, and is a function of the number of qualifying children and current program parameters, not individual household income.

Monthly Data. In the monthly data, women who are first surveyed between October and December will potentially face different EITC generosity across months within a survey wave. As such, both years might be affected by the same change in EITC generosity. Rather than make assumptions about how this affects decisions, I limit the sample to single women that entered the CPS between January and September so that the EITC schedule is consistent for all months in a survey wave.[13] The sample is restricted to single women with a high school degree or less that entered the CPS from January 1989 to May 1994. In 1994 there was an institutional change in the survey format, making it infeasible to link households across months from the latter half of 1994 and 1995. This leaves monthly data for 14,476 single women. During my analysis period, there was one moderate expansion in 1991 for households

income but zero hours worked. This level of information is not available in the monthly basic interview and I cannot make these restrictions. Not capturing EITC eligible children that are either 19 to 24 and attending college or disabled can potentially lead to misclassification and measurement error. If anything I will be misclassifying women as having fewer children than they do, which would bias my results downward. If I estimate my baseline regression, but exclude women who have 19-24 year old children, the estimates are slightly larger but not statistically different.

[13]Individuals first interviewed in October, November, or December have monthly observations that face three different tax regimes. If there is a change in the second year (end of the first survey wave, beginning of the second) part of the response to the EITC change might also be captured by the employment behavior during the survey months in the second year but during the first survey wave. This would understate the response to the EITC. Similarly if the change was only in the third year (end of the second wave) the individual would also only face incentives to change behavior during part of the second wave, so the response might be understated as well. If I keep the maximum EITC credit measured at the calendar level, this would bias the effects downward, because both periods face some level of "treatment" that isn't captured by the explanatory variable. Consistent with this, the results are smaller when women whose survey wave crossed years are included. If I alternatively measure the EITC variables as the weighted average across the years the coefficients are similar to my baseline estimates and statistically significant (see Appendix Table A.8). However, the interpretation is less straightforward.

with children, small increases for households with children in 1992 and 1993, a very large expansion in 1994 for all households (which increased with the number of children), and a large expansion in 1995 for households with two or more children (these expansions as well as other expansions can be seen in Figure 2).[14]

The month to month employment information in the CPS can capture several of the measures and predictions from the conceptual framework. From the monthly employment reports, I create an indicator for whether or not the woman is employed at any point during the four months surveyed in the year. This captures changes in the probability of being employed (which will arise from changes in the value of employment). As the EITC increases the opportunity cost of exiting the labor market, the duration of employment spells should mechanically increase and employment will become more stable. To capture this empirically, I calculate the share of months during the four month period the woman was employed.[15] As the EITC increases the value of employment, the probability of exiting should also fall. I create a dummy variable that indicates if the woman ever exited employment during the four month period (was employed in one month but was not employed in the next) and an indicator for if she exited multiple times. I will then be comparing to see if these measures change from one year's observation to the next when there is an expansion in the EITC. This can help us understand how an increase in EITC generosity from one period to the next affects the probability of working, the duration of employment, and the probability of exiting the labor force.

Annual Data. I will also make use of the annual number of weeks worked measure

[14]Unfortunately, changes in the CPS methodology prevent linking households between 1995 and 1996, when some of the largest increases in the EITC for single mothers with multiple children occurred. Results are similar in magnitude, but less precise if I exclude the cohort surveyed in 1994 and 1995 that experienced the two largest expansions.

[15]Participants are asked about work in the previous week. As such, it is possible that women have simply found a new job by the next month. During this sample period, participants of the March ASEC who were currently unemployed, were asked about the duration of their unemployment. Among my sample, nearly 75 percent of unemployment spells were four or more weeks. Although this does not directly relate to the number of individuals that would become unemployed and re-employed during a one month period, it does suggest that this is a small fraction of individuals. Also, if I examine employment in the same occupation from one month to the next, the results are virtually the same.

that is only available in the CPS ASEC supplement. As the ASEC was only conducted in March during my sample period, only single women with monthly interviews in March for two consecutive years will be included.[16] For this analysis the sample includes 6,919 single women with a high school degree or less that enter the CPS between 1989 and 1994 and can be linked from one March supplement to the next. The number of weeks reported correspond to the weeks worked during the previous calendar year, meaning that women interviewed in 1993 and 1994 are reporting on employment in 1992 and 1993. I thus lag the data appropriately to see how the maximum credit in years t and $t-1$ affect employment in year t (which is drawn from the report in year $t+1$). Because of this lag, the annual level outcomes will correspond to employment between 1988 and 1994. As such, I am not able to estimate the employment response to the big changes in 1995 in the lagged credit, affecting the maximum credit eligible to receive.

IV Graphical Evidence

Using the CPS, I first descriptively explore the relationship between EITC generosity and labor force attachment among single women with a high school degree or less in Figure 4. Using the four month CPS employment history, I construct the share of months employed during each survey year for each individual in the sample. I then plot the difference in the average share of months employed (during the second survey year) in solid black for women with children relative to women without children. These plots are presented separately for women with any reported work in the first survey year in Panel A and for women without reported work in the first survey year in Panel B. For reference, the lagged maximum EITC is plotted in the background. This measure captures the maximum EITC the woman is elgible to receive in the current year, and is associated with the EITC policy the individual would encounter when they file their taxes in that year. For families with two eligible children

[16] Over time, the CPS has started pulling in more participants for the ASEC, creating a larger sample. However, these people from the over sample cannot be linked from one wave to the other. This only became a large fraction of the total sample after my analysis period.

there was a $365 (2010$) increase in 1992, $164 increase in 1993, $133 increase in 1994, and a $1,312 increase in 1995 (from the 1994 expansion). During this period, households without children were ineligible for the EITC until it was first available in 1995 at $407 (2010$).

When looking at women with any employment during the first survey wave (in Panel A), the gap between women with and without eligible children closes slightly as the lagged EITC becomes more generous. When the largest expansion occurs in 1995, there is a discrete, statistically significant closing of the gap. This is consistent with single women with children responding to the EITC by increasing labor force attachment relative to single women without children. Among women who did not work in the first year, in Panel B, there is no discernible relationship between EITC increases and the share of months employed.

When examining the annual level a similar pattern emerges (see Figure 5). Using the annual weeks worked reported for the previous calendar year from the March Supplement, I construct the individual's employment status for each year. I then plot in black the share of individuals employed in the second survey year for single women with children relative to single women without children in each year. These plots are also presented separately for women with any work in the first survey year in Panel A and for women without work in the first survey year in Panel B with the lagged maximum EITC in the background for reference. Because the weeks worked is reported for the previous calendar year, the sample covers 1989 to 1994, and the largest expansion in 1995 is no longer in the sample. As seen in Panel A, among single women who worked during the first survey year, the share of women with children that were employed during the second year increased relative to women without children when the EITC increased, although this increase is small and insignificant. Even though this comparison of means over time is imprecise, the pattern is consistent with more single women with children staying in the labor force when the EITC expanded, and became less likely to exit at the annual level. Among women who did not participate in the labor force during the first survey wave there is no discernible relationship.

V Identification Strategy

To identify the effect of the EITC on within year employment decisions, I will estimate how changes in EITC generosity affect a given woman's employment behavior. To do this I will exploit the panel nature of the CPS and estimate an individual fixed effects model. For each woman there are four month employment histories for two consecutive years that I collapse to two observations per person. This short, two-period individual-level panel allows me to examine within person changes in employment outcomes from one year to the next after an EITC expansion. It also makes it possible to condition the sample on participation during the initial survey wave, and look at heterogeneous responses by initial labor force attachment in ways that are not possible with repeated cross-sections.

Including individual fixed effects will control for time-invariant individual specific characteristics. For example, if individuals have different underlying and unobservable tastes for work, work related costs, or access to support networks, this could affect a woman's responsiveness to the EITC incentives. The individual fixed effect allows me to abstract from this heterogeneity and make a within person comparison that exploits year-to-year changes in the EITC maximum credit to see how employment behavior responds. Because each individual is observed for two periods, I can estimate an individual fixed effects specification as follows

$$Y_{it} = \beta_1 Max\ Credit_{i,t-1} + \beta_2 Max\ Credit_{it} + X_{it}'\Gamma + \theta_n * t + \delta_i + \phi_t + \varepsilon_{it}. \tag{1}$$

The outcomes of interest in equation (1) are employment measures, constructed from the monthly data such as an indicator for being ever employed, the share of months employed, or an indicator for any exit or multiple exits from employment. The explanatory variables of interest are the maximum credit eligible to *receive* in the current year ($Max\ Credit_{i,t-1}$) as well as the maximum credit eligible to *earn* in the current year ($Max\ Credit_{it}$), both measured in hundreds of dollars (2010$). These variables are a function of the maximum number of eligible children the woman ever reports and year specific parameters of the

EITC schedule. Given the previous discussion on the potential importance of timing and the graphical relationships, it seems relevant to look at employment responses to both the current policy parameters as well as one year lagged policies, which might be the parameters people are aware of or learn about when they file their taxes. The year fixed effects absorb any aggregate shocks or changes faced by everyone.

Although collinearity between the current and lagged maximum credit might be a concern, the EITC does not change every year, so *changes* in the current and lagged maximum credit (the identifying variation being exploited) have a low correlation (0.22 in the monthly data, 0.27 in the annual data).[17] I also show in Appendix Table A.2 that the results are substantively the same if I only include one measure at a time, suggesting this is not driven by collinearity. State-level minimum wage and the presence of a TANF waiver controls are also included.[18] Standard errors are corrected for clustering at the state level in this and all other specifications.[19]

As single mothers with one child and multiple children are treated similarly by the EITC up through 1994, there is not enough variation to only compare single mothers with different numbers of children. For this reason I will exploit variation similar to that used by Eissa and Liebman (1996) and Bastian (2018) by comparing single women with zero, one, or multiple EITC eligible children.[20] As previous researchers have highlighted, single women with zero, one and two children might be differentially affected by other changes over this time period. I therefore include group-specific linear trends in the regression, allowing single women with

[17]It is true that the current and lagged maximum credit in levels are highly correlated, however, because this specification exploits within person changes in the EITC, levels are not the relevant measure.

[18]State TANF waivers and minimum wage data graciously provided by Kearney and Levine (2015).

[19]Standard errors are smaller if I do not correct for clustering, and are similar if I cluster at the family size by income bin level.

[20]If I instead restrict my sample to only single mothers (approximately 45% of the full sample) I am essentially only exploiting the additional 550 dollar increase in 1994 for mothers with two children relative to one child. I find that a one hundred dollar increase in the lagged maximum credit is associated with a 1.2 percentage point (t-stat = 1.21) increase in the share of months employed, a 1.7 percentage point (t-stat = 1.61) reduction in the probability of exiting, and a 0.2 percentage point (t-stat = 1.57) reduction in the probability of multiple exits. The estimates on this smaller sample are not significant, but are similar to the baseline estimates and consistent with the overall pattern.

different numbers of eligible children to have separate trends over the time period ($\theta_n * t$).[21]

As seen in Table 1, some characteristics of less-educated single women with no children, one child, and two or more children are different. Single mothers are slightly older on average, less educated, and less likely to be Non-Hispanic white. Single mothers with multiple eligible children are even less likely to have a high school degree, more likely to be Non-Hispanic black or Hispanic, and more likely to have a child under five. Among single mothers with multiple eligible children, the average number of children is 2.7, with over 85 percent of these mothers having two or three children.[22] Labor force attachment also falls for single women as the number of eligible children increases. These demographic differences across family size are not inherently problematic to identification, but become so if they result in differential trends across eligibility groups. However, the empirical strategy described above allows for potentially different linear trends by the number of eligible children. As such, the identifying assumption is that differential changes in EITC generosity across eligibility groups are uncorrelated with other potential unobserved factors that affect employment behavior. Although this individual fixed effects specification accounts for unobserved individual characteristics, one limitation is that only short-run responses can be measured and this strategy does not speak directly to long run changes in the stock of employed women over the course of the decade.

[21]When looking at heterogeneity by initial labor force attachment there is another reason to include time trends by the number of children. With a 2-period panel and individual fixed effects, I am making a within individual comparison where the time deviation will always be one. When looking within person among those with labor force attachment in the initial wave, the linear trend is capturing how likely single mothers with 0 children, 1 child, and 2 or more children are to change from working in the initial period to not working in the second period. On average, single mothers with children are less likely to be working. So, if I have conditioned on them working in one period, they are empirically more likely to not be working the next period (mean reversion). Because the EITC dollar amount is also a function of the number of children, it will be correlated and pick up this mean reversion. Including the linear trends by the number of children can help control for this, and leaves me with useful variation for identification. Namely, were single women with children less likely to exit after a big EITC increase than single women with children when the increase wasn't as large? The current specification fixes the number of qualifying children from the first survey wave. The results hold if I instead include linear trends by the number of children currently living in the house.

[22]When splitting by employment during the first year, the distribution of the number of children is similar, and excluding mothers with high ordered number of children does not affect the results substantially.

VI Results

VI.A Within Year Employment Duration Response to EITC Increases

To see if an increase in the EITC increases the opportunity cost of leaving the labor force and leads to stronger labor force attachment within a year, I turn to the linked monthly data. As responses to increases in the EITC might vary by initial labor force attachment (as seen in Figure 4), I stratify the estimation by labor force participation during the initial survey wave and report the results in Table 2. For example, women who were not employed during the survey period might face very high employment costs and even with a wage subsidy such as the EITC, the positive returns to work might fall short of these high costs. Alternatively, if information about the policy is only revealed when taxes are filed, women who were not employed previously will not receive the credit or new information. I cannot observe annual employment status for all participants as not all women were surveyed for the ASEC supplement. As such, I measure employment during the first four survey months to capture labor force attachment. Women reported as non-employed might be misclassified if they worked later in the year.[23]

All employment responses are concentrated among women who were previously employed and are associated with changes in the maximum EITC eligible to *receive*, rather than the maximum EITC eligible to *earn.* In other words, only women who reported employment in the initial survey year -and would be more likely to received the credit- responded. This pattern is consistent with both a liquidity constraint framework as well as an information framework where single mothers might be unfamiliar with tax policies that affect them but adjust behavior after information about the current policy is revealed.

Among these women, a one hundred dollar increase in the maximum EITC eligible to

[23]For women surveyed in March both the four month and annual employment are observed. In this sample, 17.2% of people not working during the four month period are employed at some point during the year. This potential misclassification would bias effects for those classified as not previously employed towards the effects for those with previous employment.

receive increased the probability of being employed by 0.01, or 1.0 percentage point (column 1), meaning these women were more likely to be employed during this four month window after an EITC expansion. As seen in column 2, a one hundred dollar increase in the maximum EITC also significantly increased labor force attachment by increasing the share of months worked by one percentage point, or 1.2 percent (.01/.86).[24] As this increase is small, there is not a detectable decrease in the probability of exiting, suggesting many of these women still eventually transitioned out of the labor force. However, the increase in employment duration is associated with a reduction in the frequency of turnover and the probability of exiting multiple times within a survey wave (in, out, in, out), once again indicative of a stronger labor force attachment. I estimate that a one hundred dollar increase in the maximum EITC leads to a 0.1 percentage point reduction in multiple exits (column 4). This represents a large percent effect off of the relative low base of 0.001.[25]

During this period, the average increase in the maximum EITC a household with children was eligible to receive was $277 (2010$). These estimates would imply that this increase in the EITC increased the probability of working in the next year by 2.8 percentage points and the share of months worked by 3.2 percent. In Appendix Table A.3, I also split the sample of women who worked during the first survey wave to look at those that worked the full 4 months and those who worked less than that. The effects are largest, and concentrated among women who were initially less attached, consistent with the predictions of the conceptual framework.[26]

[24]A $100 increase in the maximum credit eligible to receive in the current year is also associated with a significant 0.4 hour increase in the average hours worked during the previous week among women with any employment during the first 4 monthly surveys. However, it is not clear if this is because they are working more weeks, or more hours a week.

[25]Although it would be informative to explore why people exited employment, the CPS only reports exit reason for unemployed individuals. Since over 58 percent of individuals transitioning out of employment also leave the labor force in the following month, this information is not available for most of those who exit.

[26]I also examine several other margins of interest. Previous work suggests self-employed workers are the most able to adjust income to EITC incentives (Saez, 2010), and I find that a hundred dollar increase in the maximum EITC credit increased the probability of being self-employed during the four months by 0.5 percentage points. One might be concerned that this potential job stability is preventing upward mobility. However, the average Occupational Prestige Score (Davis et al, 1991) for occupational changes does not change when the EITC becomes more generous, suggesting the EITC is not preventing upward job moves. The effects are statistically indistinguishable if I exclude mothers with children 5 or under or preschool age

One concern when estimating impacts separately by previous labor force attachment is that selection bias is introduced if the EITC induced women to enter or remain in the labor force at the extensive margin. I do three things to explore this possibility. First, I look at how the composition of observable characteristics changes over time in the sample of women who worked during the first four survey months. Because there might be demographic shifts in the population as a whole, I also look at how the composition of observable characteristics changes for the sample of women who did not work during the first four survey months. As seen in Table A.5, there are slight compositional changes over time, but they do not differ by employment status during the first survey wave, suggesting there is not significant selection into the sample along observable dimensions.

I also estimate the relationship based on predicted employment during the initial wave. Using observations from 1989 (my first year of data), I estimate a logit regression of Reported Any Employment During First 4 Monthly Surveys (the conditioning criterion) on educational attainment, race indicators, age indicators, month indicators, the state minimum wage, and state fixed effects. I then use these estimates to predict the probability of reporting any employment during the first 4 monthly surveys. I then re-run the regressions from Table 2, but condition on whether this predicted probability is above or below 0.5 in Appendix Table A.6. The treatment effects are concentrated among those that have a high predicted probability of being employed during the initial wave, with no impacts for the other group. This would suggest that if we close off selection along observable dimensions and just focus on people who have characteristics of those attached prior to the intervention we still find positive, significant impacts.

Even though the estimated effects do not appear to be driven by selection along observable dimensions, there might be selection along unobservable dimensions that lead to selection bias. Although there is no significant annual entry response to the EITC during the analysis period (see Table 6), there might still be selection that I do not have the precision to

children. The results are also similar when I exploit variation from both the federal and state level EITC expansions or include various individual and state level controls (see Appendix Table A.4).

detect. To evaluate the importance of unobserved selection, I calculate how much of the total effect remains when accounting for selection from entry under various assumptions about its pervasiveness. In these calculations I allow both the scope of selection and the selective entrants' responsiveness to the policy to vary. I consider scenarios where up to 4.41 percentage points (6 percent) of the sample selectively enters when there is a $100 increase in the EITC. This is a very large impact given the estimates from the existing literature exploiting TRA86 and OBRA93 suggest that a $100 increase in the maximum EITC led to a 0.4-0.9 percentage point increase in annual employment rates (Eissa & Liebman, 1996; Grogger, 2003). I find that if selective entrants were to increase their labor supply from one wave to the next by 5 percentage points (corresponding to a labor supply elasticity of 0.36) the treatment effect would still account for at least 74 percent of the total effect across all hypothetical levels of selection I consider (see Appendix Table A.7, and the table notes for detail). If selective entrants were to increase labor supply by 10 percentage points (elasticity of 0.65) at least 42 percent of the total effect would be due to the treatment effect. The total effect is only entirely explained by selection if there are both large levels of selection and if the responsiveness of these selective entrants is very high, too high to be consistent with traditional labor supply elasticities (see Appendix C for elasticity calculations). Even though there might be selection bias, the total effect is still likely to be mostly composed of the causal treatment effect.

VI.B Impacts for Groups with Low and High Expected Impact

Some groups should be unaffected (or less affected) by changes in the EITC. For example, women with a college education are likely high enough in the income distribution that they are not eligible for the EITC. Observing similar responses here might raise concern that this strategy is just capturing some aggregate trend. In Table 3 I re-estimate equation (1) for women with a bachelor's degree or more. Among these women I do not observe any response to the EITC, suggesting this is not just a trend experienced by all single mothers.

Similarly, households with earned income above the EITC schedule should be unaffected by increases in EITC generosity, although households near the end of the phase-out might become eligible. In Table 4, I partition the sample of previously employed women with a high school degree or less into groups based upon whether the reported family income from the previous 12 months falls above or below \$25,000.[27] This is approximately the end of the EITC phase-out region in 1994. This income measure is endogenous and likely exhibits considerable measurement error (households might contain several families or tax filing units) and should therefore be interpreted with caution. As with college educated women, we expect households with high incomes to be a low-impact group, while the EITC is expected to have a high impact among households with income near the EITC range. The employment effects are only observed among women that reported EITC eligible income levels suggesting this is not simply capturing a trend faced by all single women with a high school degree or less. For these women, an additional hundred dollars of EITC credit received increased the probability of being employed during the four month period by 1.6 percentage points, increased the share of months employed by 2 percent (0.016/0.82), and led to a reduction in multiple exits during the four month period. As expected, women with higher income did not significantly adjust the probability of employment, share of months worked or exit behavior.[28] Although the impacts for college educated high and income women are not true "placebo" tests, not observing any response among these groups provides further evidence of the causal nature of these estimates.

VI.C Annual Employment Response to EITC Increases

The monthly data suggest that increased EITC generosity leads to stronger labor force attachment and potentially longer employment duration. However, because these data are

[27]Family income is reported in bins of nominal dollars so I cannot directly convert income bins to real dollars.

[28]We might be concerned that people with incomes just above \$25,000 might still respond to the EITC. If I look at families with income in the first year over \$40,000 (a small sample of only 1,006 women), the effects are still small and insignificant.

identifying the impact of the EITC on with-in year behavior, it does not directly relate to the previous work exploring the impact of the EITC on annual-level employment or weeks worked. By examining the linked CPS ASEC supplement, I can see how this relates to previous work as well as identify if the increase in employment levels from the EITC is driven by increased inflows, reduced outflows, or both. The annual outcomes of interest are the number of weeks worked, and an annual employment indicator (weeks worked greater than zero). Because the ASEC includes the number of weeks worked during the previous year, I can condition the sample on the first year employment status (positive weeks worked), to see if changes in the maximum EITC credit made people who were working one year more likely to stay working the next year (reduce exit) or made people who were not working one year more likely to begin working the next year (increase entry).[29] For example, if an increase in the EITC increases employment among those women who were employed during the initial survey wave, then this represents a reduction in year-to-year exit.

To explore the impact of the EITC on annual outcomes I estimate an equation similar to equation (1), but compare the yearly observations from the CPS ASEC. By including an individual fixed effect I can control for individuals' fixed characteristics and tastes, and see how a given woman's annual employment changes when the EITC she faces becomes more generous. I will first look at how increases in the EITC affect the number of weeks a woman works during the year. I will then examine how increases in the EITC affect a woman's probability of working at all during the year, conditional on employment status during the first year.

Unlike the monthly data, this will yield estimates directly related to the previous work looking at annual outcomes, but unlike the previous work, this will shed light on which employment flows were affected by the EITC. In Table 5 I estimate the impact of the EITC on annual weeks worked. As with the monthly data, I stratify the estimation by reported employment during the first ASEC survey wave. Once again, employment decisions respond

[29]Because I observe weeks worked during the full year, this conditioning does not suffer from the potential misclassification that was present looking at the monthly data.

to changes in the maximum credit eligible to *receive* in the current year, rather than the maximum credit eligible to *earn* in the current year. Women who did not work during the initial survey year did not significantly respond to increases in EITC generosity. However, among women who previously worked, a hundred dollar increase in the maximum EITC credit eligible to receive (the lagged EITC) is associated with 0.83 additional weeks of work a year on average (column 1). This increase in weeks of work leads to a significant 1.5 percentage point increase in the probability of working at least 10 weeks, a 2.1 percentage point increase in the probability of working 30 weeks, and a 2.1 percentage point increase in the probability of working at least 40 weeks (columns 2, 4, and 5).[30] This would suggest that the EITC increased labor force attachment within the year.

To understand how this affects annual labor force transitions, I examine the impact of the EITC on any annual employment in Table 6. The EITC expansions in the early 1990s had a positive, but insignificant 1.1 percentage point effect on the total employment level of less educated single women (see column 1).[31] However, when stratified by employment status in the initial survey year, increases in the maximum EITC eligible to receive increase the probability that those previously employed continue employment in column 2 (i.e., reduced annual exit), but did not induce single women who did not participate in the previous year to enter the labor force (although the standard errors are large). A hundred dollar increase in the maximum EITC credit received reduced year-to-year exit by 2.5 percentage points. During this sample period from 1988 to 1993, the average year-to-year expansion was $104 (2010$), implying that annual exit was reduced by 2.5 percentage points on average. In columns 3 and 4 I separately look at women by labor force attachment during the first survey year. The reduction in annual exit associated with the EITC is concentrated among

[30] Women that worked less than full year and were only weakly attached to the labor force might be more marginal and responsive to changes in the EITC. In Appendix Table A.9 I estimate the impact separately for women who reported 1-51 weeks of work and exactly 52 weeks of work during the initial survey year. These effects are concentrated among women who worked less than full year and were less attached to the labor force. Approximately 35 percent of working women worked less than full year.

[31] For reference, work looking at the EITC expansions later in the 1990s find a 3.6 percentage point increase in annual employment for an additional $1,000 of maximum credit (Grogger, 2003) and a 5 percentage point total increase for women with two or more children (Adirkersombat, 2010).

women who were initially less attached to the labor force (worked less than 52 weeks).[32] This is consistent with women facing lower net benefits from work becoming more attached after the EITC becomes more generous. The reduction in exit provides one channel through which the EITC expansions in the 1990s might have increased the annual employment stock of less-educated single women.

VI.D Information vs. Liquidity Mechanism

Both the monthly and annual data suggest that the EITC increased the labor force attachment of women previously in the labor force, and kept them in the labor force when they might have otherwise dropped out. This is consistent with the information mechanism proposed by Nichols and Rothstein (2015), but actually receiving the EITC transfer could also affect decisions by relaxing liquidity constraints that would otherwise induce women to drop out of employment. Previous work documents that EITC recipients file taxes and receive refunds sooner than other tax payers (Barrow & McGranahan, 2000; LaLumia, 2013; Hoynes, Miller & Simon, 2015). As seen in Figure 6, in 1989 15.6 percent of refund dollars with EITC payments were distributed in February, 40.2 percent in March, and 23.4 percent in April, and 14.4 percent in May, meaning that by May, nearly 94 percent of all refund dollars with an EITC had been distributed. The literature also suggests that EITC recipients spend the money rather quickly, using their refund to pay down bills and fund the purchase of large durable goods, while saving is less common (Smeeding et al., 2000; Goodman-Bacon & McGranahan, 2008). In years when the maximum EITC credit increased, women surveyed during tax season (February through May) might have both new information and extra cash on hand, while women surveyed later in the year might have new information, but were less likely to still have the additional cash on hand. To determine if a response to lagged EITC policy is more likely due to information or liquidity, I test to see if the employment response

[32] 65 percent of women that worked during the initial survey year reported working 52 weeks that year. I use the 52 week cutoff to keep both subsamples reasonably large. The impacts in the "less attached" group become stronger if I restrict the sample to working 45 or 40 weeks.

changes for women surveyed during tax season as follows

$$Y_{it} = \beta_1 Max\ Credit_{i,t-1} + \beta_2 Max\ Credit_{i,t-1} * Share_i + X'_{it}\Gamma + \theta_n * t + \delta_i + \phi_t + \varepsilon_{it} \qquad (2)$$

where $Share_i$ equals the share of months individual i was interviewed during the tax season (February through May). The purpose of this estimation is to see if β_2 is positive and statistically significant. An insignificant β_2 would suggest that women, whose survey period overlapped with tax season, responded similarly to women surveyed throughout the year, suggesting the response is less likely to be due to liquidity. However, as this infusion of income might also introduce income effects, these results are suggestive and should be interpreted with caution. As seen in Table 7 the probability of being employed and the share of months employed increased when the EITC expanded, but the additional effect for those surveyed during the tax season is negative (but insignificant).[33] Rather than observing larger effects during tax season due to liquidity and relaxed constraints, the data suggest that survey participants throughout the year responded to the EITC. This is less consistent with a framework where the EITC refund relaxes liquidity constraints, allowing women to work, but is perhaps suggestive that EITC receipt revealed information about the returns to work.[34]

VI.E Responses to EITC in the 2000s

Since the 1990s, the economy has undergone drastic changes and overall awareness of the EITC has also greatly expanded (Chetty & Saez, 2013; Chetty et al., 2013), making it

[33]The observation that individuals interviewed during tax season have less positive effects would be consistent with on offsetting income effect (as seen by LaLumia & Wingender (2017) and Yang (2015)) or the unemployment spell pattern documented by LaLumia (2013). Results are similar if instead the model includes an indicator that equals one if a woman is surveyed during the tax season rather than the share of months.

[34]I have also explored these relationships using the SIPP. The maximum credit eligible to earn is associated with a positive effect on the probability of being employed for women who were employed the previous year, but it is not statistically significant. For women who were employed during the previous year, a one hundred dollar increase in the maximum credit eligible to earn is associated with a significant 0.7 percentage point increase in the share of months worked. For women who were not employed the total effect (0.007-0.005) is not statistically significantly different from zero. These patterns correspond to the CPS analysis. These results and details of the analysis are presented in Appendix Table A.10.

unclear if we should expect the same behavioral responses in today's economy. Since 1996 there has only been one federal expansion of the EITC, in 2009 as part of the post-recession American Recovery and Reinvestment Act (ARRA), leaving little variation at the federal level. However between 1999 and 2016, 18 states implemented supplemental state EITCs. In most cases, these EITC policies are calculated as a percentage of the federal credit. There is significant heterogeneity across states, with some states distributing as little as 3.5 percent of the federal credit up to 85 percent among households in the phase-in region.[35] Using these state level policies, I construct the total maximum credit eligible to earn and receive in the current year (current and lagged policy), which is composed of both the federal and state level credits. I then estimate equation (1) using the sample of single women with a high school degree or less from the 2000 through 2016 monthly CPS surveys. As before, I estimate the effects separately for women by employment status during the initial survey wave. These estimates are reported in Appendix Table A.11.

During this more recent period, women who were already employed in the initial wave responded by increasing the probability of remaining employed across survey waves and the share of months employed, but this response was to changes in the maximum EITC eligible to *earn* in the current year. These responses are an order of magnitude smaller than the impacts from the 1990s. There are several reasons this might be. People might treat federal and state taxes differently, people might have more information about the EITC, and the identifying variation and treatment are different than the baseline analysis (i.e., state level increases often accompanied by outreach and that require state tax filing). This pattern is consistent with the EITC increasing labor force attachment of single mothers, particularly among women with previous employment.

[35] California has the highest state EITC at 85 percent. However, this only applies to part of the phase-in region, so the actual value is smaller.

VII Conclusion

Although less educated single women make frequent employment decisions, the current literature is silent as to how within year employment decisions respond to the EITC. Exploiting the panel nature of the CPS, I provide evidence that the expansions of the EITC in the early 1990s increased employment levels of less educated single women by increasing employment stability and reducing annual exit from the labor force. Increases in the maximum EITC eligible to receive made less educated single women increase the share of months worked, and exit the labor market less frequently. A hundred dollar increase in the maximum EITC credit increased the number of weeks worked in a year by 0.83 weeks and reduced year-to-year exit among single women that were previously employed by 2.5 percentage points.

These estimates are larger than the previous literature for several reasons. First, the estimates from the monthly data are not directly comparable. These are the estimated impact on the probability of working during a four month period exactly one year apart, whereas the previous literature has looked at the probability of working at all during the year. Because this is a population that transitions in and out of the labor force at a high frequency, the monthly effects should be larger because the 4 month window potentially captures changes among people who might have worked during other parts of the year, regardless of the EITC. For these people the EITC might be increasing within year attachment, but not their annual employment status, leading to smaller annual level estimates. This would suggest that the EITC leads to more stable labor force attachment in addition to the extensive margin impacts documented by the previous literature.

The 2.5 percentage points point reduction in annual exits seems large at first, but it is should be interpreted in context. First, because of data timing, the annual analysis does not include any of the largest expansions. This would yield less precise estimates, as well as larger estimates if the actual response exhibits some concavity. Although significant, the estimate is imprecise and effects on the order of 0.74 percentage points cannot be ruled out. Second, this is the effect for a subgroup, so the total effect would be much smaller. The estimate suggests that among single mothers who worked during the first year, the probability of exiting fell by 2.5 percentage points. Since these women transition in and out of the labor force quite frequently, the impact on exits for all single women is smaller. In my sample, approximately 60 percent of single women with children worked during the first year. So, a \$100 increase is associated with a 2.5*0.60 = 1.5 percentage point decline in exit rates among single women with children. Since single women with children make up about 49 percent of my sample, this would suggest that exit rates among all single women declined by 2.5*0.6*.49 = 0.735 percentage points. As the impact on exit has not been estimated in the previous work, there is no number to compare this to, but the implied annual employment effects fall within the range of previous estimates. These point estimates yield extensive margin labor supply elasticities around 0.47-0.49, which are similar to estimates from previous work (see Appendix C for elasticity calculations).

Employment behavior is responding to changes in the maximum EITC eligible to *receive* rather than the maximum EITC *earned* in the current year and only observed among women who reported employment in the initial year and were therefore more likely to receive the credit. This pattern of behavior is consistent with Nichols and Rothstein's (2015) theory that people lack information and then update behavior after that information is received. Alternative mechanisms, such as relaxed liquidity constraints are less supported by the data as women surveyed after tax season are equally responsive to EITC dollars. This might also help explain the response lag documented in the previous literature. Also, transitioning into employment might take longer, so it would not be captured in this short-term response.

This paper provides evidence that the EITC can increase employment stability of less educated single women, which might have important welfare implications if, for example, labor market experience at this level increases wages or if employment stability leads to positive outcomes for children.[36] However, these employment responses appear to be driven

[36]Consistent with this, there is work suggesting the EITC leads to improved educational outcomes for children (Dahl and Lochner, 2012; Manoli and Turner, 2018; Bastian and Michelmore, 2018).

by women who were already attached to the labor force. This work also sheds light on the dynamics driving the documented effect of the EITC on employment levels. Often the EITC is cited as a tool to bring households into the labor force, but these results would suggest that the EITC operates by keeping individuals with previous labor force attachment in the labor force, rather than by bringing in new entrants. Recent discussions have considered expanding the EITC among different demographic groups (like childless individuals). These expansions are likely to keep marginal participants in the labor force, rather than induce those persistently out of the labor force to return.

DISCLOSURES

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Tables

Table 1: Characteristics and Sample Selection of Linked Single Women with High School or Less in the 1989-1994 CPS

	Single Women 19-44 with High School or Less					
	0 Eligible Children		1 Eligible Child		2+ Eligible Children	
	Full Sample	Linked Sample	Full Sample	Linked Sample	Full Sample	Linked Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Age	26.26	28.49	28.86	30.91	30.17	31.30
Less than High School	0.22	0.22	0.29	0.25	0.40	0.40
Non-Hispanic White	0.66	0.68	0.56	0.55	0.40	0.37
Non-Hispanic Black	0.17	0.17	0.28	0.33	0.42	0.46
Hispanic	0.13	0.11	0.13	0.10	0.15	0.15
Non-Hispanic Other	0.03	0.03	0.02	0.01	0.02	0.02
Number of Eligible Children	0	0	1	1	2.63	2.68
Any Children under 5	0	0	0.49	0.31	0.60	0.54
During Survey Months in Year						
Ever Employed	0.78	0.80	0.65	0.71	0.51	0.53
Ever Enter	0.10	0.08	0.09	0.09	0.09	0.07
Ever Exit	0.10	0.09	0.10	0.08	0.08	0.07
Ever Enter or Exit	0.16	0.13	0.15	0.13	0.14	0.11
Share of Months Continue Employment	0.71	0.74	0.58	0.65	0.44	0.47
Observations	16,423	7,025	6,686	3,293	7,639	4,158

Notes: Data from women who entered the CPS between January 1989 and May 1994. The Full Sample is restricted to the second survey round of single women who were 19-44 and had a high school degree or less, and who entered the CPS between January and September. Sample weighted using population weights provided by the CPS. The Linked Sample is the subset of women who can be linked across waves to their sixth survey round and corresponds to the analysis sample. For reference, the maximum EITC eligible to receive among households with children increased by two hundred dollars (2010$) on average across all years, and by approximately one thousand dollars between 1994 and 1995.

Table 2: Labor Force Attachment Response to EITC Increases

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Panel A.	Reported Any Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	0.010**	0.010**	0.0003	-0.001*
(Credit Eligible to Receive)	(0.005)	(0.004)	(0.005)	(0.0004)
Max Credit$_t$ *($100s)*	-0.000	-0.002	-0.006	-0.0001
(Credit Eligible to Earn)	(0.003)	(0.002)	(0.004)	(0.001)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.09	0.02	0.40	0.42
Second Wave Dependent Mean	0.91	0.86	0.07	0.001
Panel B.	Reported No Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	-0.003	-0.002	-0.002	-0.0002
(Credit Eligible to Receive)	(0.006)	(0.004)	(0.003)	(0.0001)
Max Credit$_t$ *($100s)*	0.003	0.001	0.003	-0.0002
(Credit Eligible to Earn)	(0.005)	(0.004)	(0.002)	(0.0003)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.44	0.64	0.25	0.96
Second Wave Dependent Mean	0.19	0.13	0.06	0.001

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. There are 20,438 observations (10,219 women) included in Panel A (Any employment during first four months), and 8,514 observations (4,257 women) included in Panel B (no employment during first four months). Changes in the maximum credit do not occur every year and the within sample correlation coefficient between changes in the credit currently earning and the credit received this year is 0.22. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table 3: Sensitivity Check: Response of Previously Employed Single Women with a College Degree

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Panel A.	Reported Any Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	0.003	0.002	0.003	0.000
(Credit Eligible to Receive)	(0.003)	(0.005)	(0.008)	(0.001)
Max Credit$_{t}$ *($100s)*	-0.002	-0.001	-0.002	-0.00002
(Credit Eligible to Earn)	(0.003)	(0.004)	(0.006)	(0.0004)
P-value Credit$_{t-1}$ = *Credit*$_{t}$	0.34	0.67	0.64	0.56
Second Wave Dependent Mean	0.97	0.95	0.04	0.001
Panel B.	Reported No Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	-0.013	0.000	-0.0016	-0.007
(Credit Eligible to Receive)	(0.020)	(0.019)	(0.011)	(0.004)
Max Credit$_{t}$ *($100s)*	0.006	0.007	-0.001	-0.0002
(Credit Eligible to Earn)	(0.026)	(0.023)	(0.014)	(0.004)
P-value Credit$_{t-1}$ = *Credit*$_{t}$	0.66	0.86	0.45	0.01
Second Wave Dependent Mean	0.45	0.34	0.11	0.008

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for single women with a college degree between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. There are 12,708 observations (6,354 women) included in Panel A (Any employment during first four months), and 734 observations (367 women) included in Panel B (no employment during first four months). The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table 4: Sensitivity Check: Response by Reported Household Income in Previous Year

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Panel A.	Previously Employed Single Women with High School or Less HH Income <$25K			
Max Credit$_{t-1}$ ($100s)	0.013**	0.017***	-0.006	-0.002**
(Credit Eligible to Receive)	(0.006)	(0.006)	(0.006)	(0.001)
Max Credit$_t$ ($100s)	0.001	-0.003	-0.003	0.001
(Credit Eligible to Earn)	(0.004)	(0.003)	(0.006)	(0.001)
P-value Credit$_{t-1}$ = Credit$_t$	0.12	0.002	0.82	0.04
Second Wave Dependent Mean	0.88	0.82	0.08	0.001
Observations	11,212	11,212	11,212	11,212
Panel B.	Previously Employed Single Women with High School or Less ≥$25K			
Max Credit$_{t-1}$ ($100s)	0.002	-0.001	0.004	0.0001
(Credit Eligible to Receive)	(0.006)	(0.006)	(0.01)	(0.0003)
Max Credit$_t$ ($100s)	0.002	0.001	-0.0004	-0.001
(Credit Eligible to Earn)	(0.005)	(0.006)	(0.004)	(0.001)
P-value Credit$_{t-1}$ = Credit$_t$	0.95	0.85	0.69	0.22
Second Wave Dependent Mean	0.95	0.91	0.07	0.001
Observations	7,348	7,348	7,348	7,348

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Households are sorted by reported household income during the first year. Controls include the state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table 5: Within Individual Annual Labor Supply Response to EITC Increases

	Weeks Worked	Weeks Worked $\geq$ 10	Weeks Worked $\geq$ 20	Weeks Worked $\geq$ 30	Weeks Worked $\geq$ 40	Weeks Worked $\geq$ 50
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Reported Positive Weeks Worked During Initial Survey Year					
Max Credit$_{t-1}$ (\$100s)	0.832***	0.015**	0.009	0.021**	0.021***	0.015
(Credit Eligible to Receive)	(0.264)	(0.007)	(0.008)	(0.008)	(0.006)	(0.010)
Max Credit$_t$ (\$100s)	-0.138	-0.004	-0.001	-0.010*	-0.003	0.003
(Credit Eligible to Earn)	(0.199)	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)
P-value Credit$_{t-1}$ = Credit$_t$	0.01	0.03	0.27	0.003	0.004	0.26
Second Wave Dependent Mean	42.6	0.89	0.86	0.81	0.77	0.69
Panel B.	Reported No Weeks Worked During Initial Survey Year					
Max Credit$_{t-1}$ (\$100s)	0.027	-0.0001	0.001	0.001	-0.0001	-0.006
(Credit Eligible to Receive)	(0.647)	(0.017)	(0.016)	(0.009)	(0.009)	(0.011)
Max Credit$_t$ (\$100s)	0.025	0.002	-0.002	0.002	-0.002	0.003
(Credit Eligible to Earn)	(0.150)	(0.004)	(0.003)	(0.003)	(0.004)	(0.006)
P-value Credit$_{t-1}$ = Credit$_t$	0.99	0.92	0.90	0.98	0.91	0.59
Second Wave Dependent Mean	6.5	0.18	0.14	0.10	0.09	0.07

Notes: Data from the linked 1989-1994 ASEC March CPS supplement. Sample includes one observation for all single women with a high school degree or less between the ages of 19 and 44, who were interviewed for two ASEC supplements. There are 10,052 observations (5,026 women) included in Panel A (any work during initial survey year), and 3,786 observations (1,893 women) included in Panel B (no work during initial survey year). This sample is smaller than the monthly sample (Table 2) because not every woman is surveyed in March. Changes in the maximum credit do not occur every year and the within sample correlation coefficient between the change in the credit currently earning and the credit received this year is 0.27. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the change in the state minimum wages,an indicator for a TANF waiver, linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using household population weights provided by the CPS ASEC. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table 6: Effect of EITC Increases on Annual Labor Market Transitions

	Outcome: Any Employment				
	All	Any Work During Initial Survey Year			No Work During
		Any	< 52 weeks	52 weeks	Initial Survey Year
	(1)	(2)	(3)	(4)	(5)
Max Credit$_{t-1}$ *($100s)*	0.011	0.025***	0.047**	0.007	0.001
(Credit Eligible to Receive)	(0.008)	(0.009)	(0.020)	(0.006)	(0.022)
Max Credit$_{t}$ *($100s)*	0.002	0.002	0.008	-0.002	0.005
(Credit Eligible to Earn)	(0.003)	(0.003)	(0.009)	(0.003)	(0.005)
P-value Credit$_{t-1}$ = *Credit*$_{t}$	0.38	0.02	0.15	0.16	0.88
Second Wave Dependent Mean	0.73	0.91	0.81	0.97	0.23
Observations	13,838	10,052	3,474	6,578	3,786

Notes: Data from the linked 1989-1994 ASEC March CPS supplement. Sample includes one observation for all single women with a high school degree or less between the ages of 19 and 44, who were interviewed for two ASEC supplements. This sample is smaller than the monthly sample (Table 2) because not every woman is surveyed in March. Changes in the maximum credit do not occur every year and the within sample correlation coefficient between the credit currently earnings and the credit received this year is 0.27. The year-to-year change in annual employment captures annual level exit in column (2) and annual level exit in column (3). The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the federal and state minimum wages and an indicator for the introduction of a TANF waiver. Individual and year fixed effects as well as linear trends by the number of qualifying children are included. Regressions weighted using household population weights provided by the CPS ASEC. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table 7: Information vs. Liquidity: Differential Impacts for Previously Employed Women Surveyed During Tax Season

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Max Credit$_{t-1}$ *($100s)*	0.011**	0.012**	-0.0004	-0.0014*
(Credit Eligible to Receive)	(0.005)	(0.005)	(0.006)	(0.001)
Max Credit$_{t-1}$ *	-0.001	-0.003	-0.001	0.001
Share of Surveys Feb-May	(0.002)	(0.003)	(0.003)	(0.001)
Second Wave Dependent Mean	0.91	0.86	0.07	0.001
Observations	20,438	20,438	20,438	20,438

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year, and reported any employment during the initial 4 monthly surveys. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the federal and state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Figures

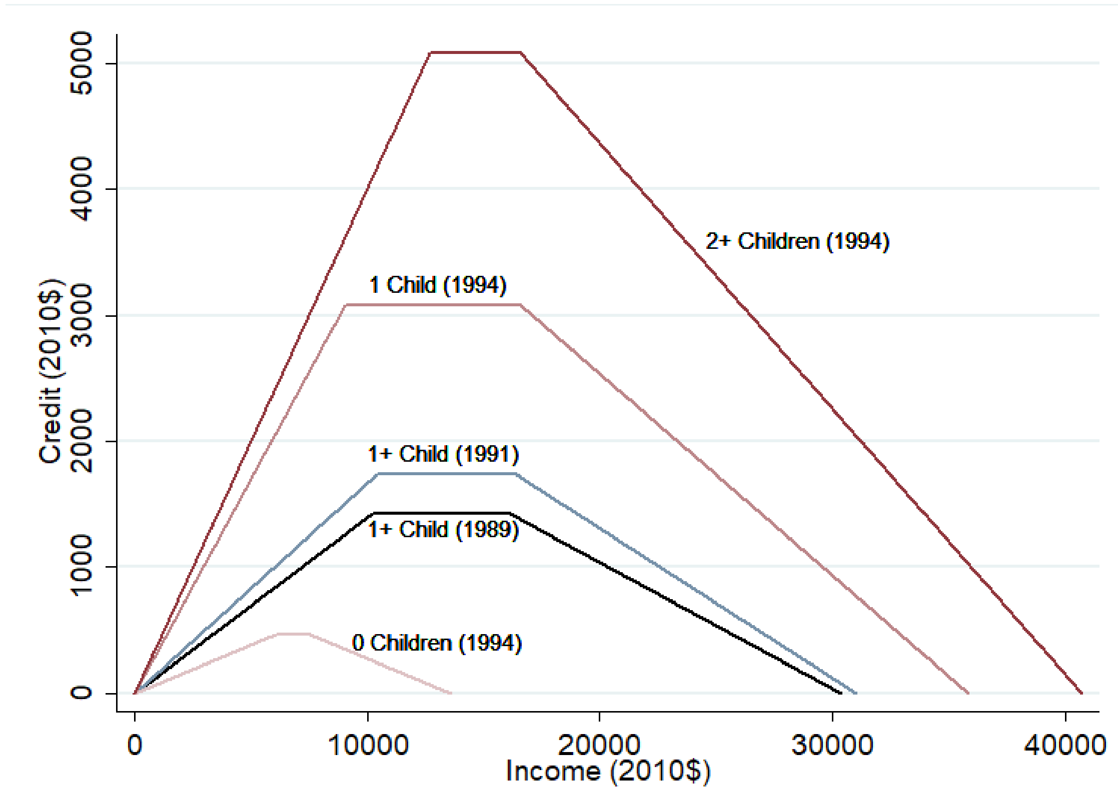

Figure 1: EITC Schedule Parameters over Time for Various Family Sizes

Source: Author's calculations using EITC parameters for 1989, 1991, and 1994 provided by Tax Policy Center.

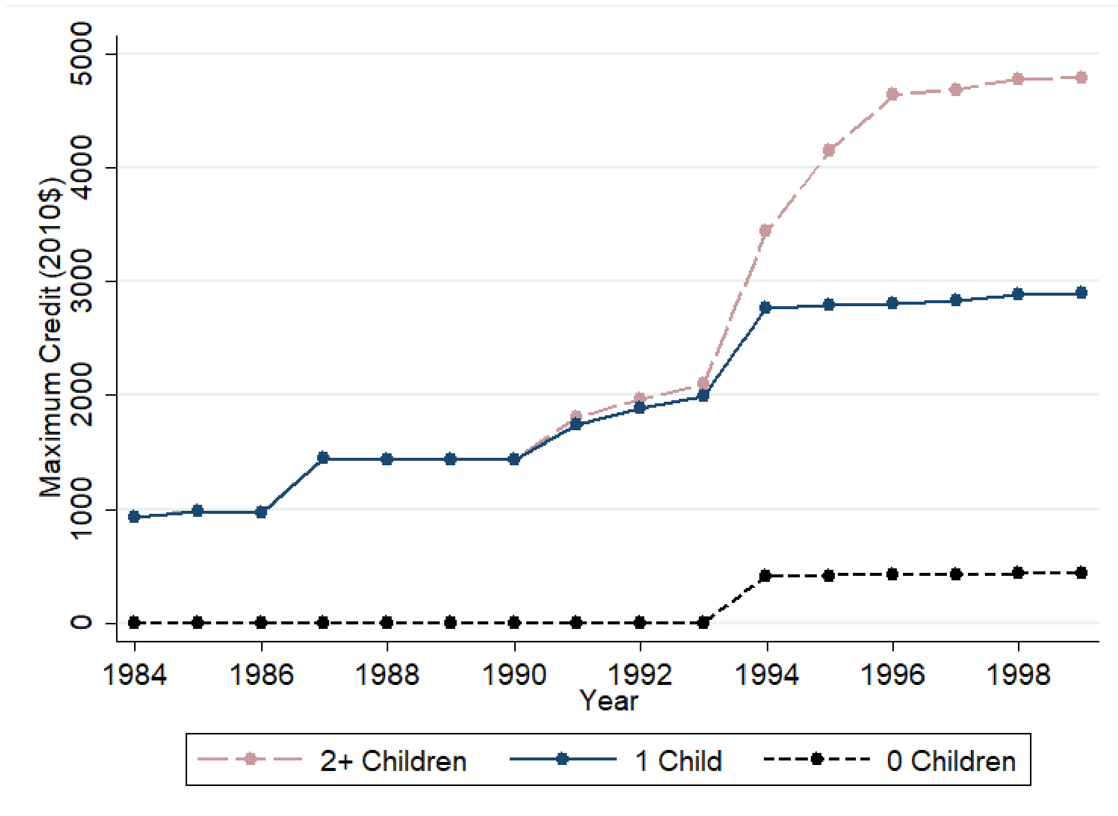

Figure 2: Maximum EITC Credit by Year and Family Size

Source: Author's Calculations using EITC program parameters as provided by the Tax Policy Center.

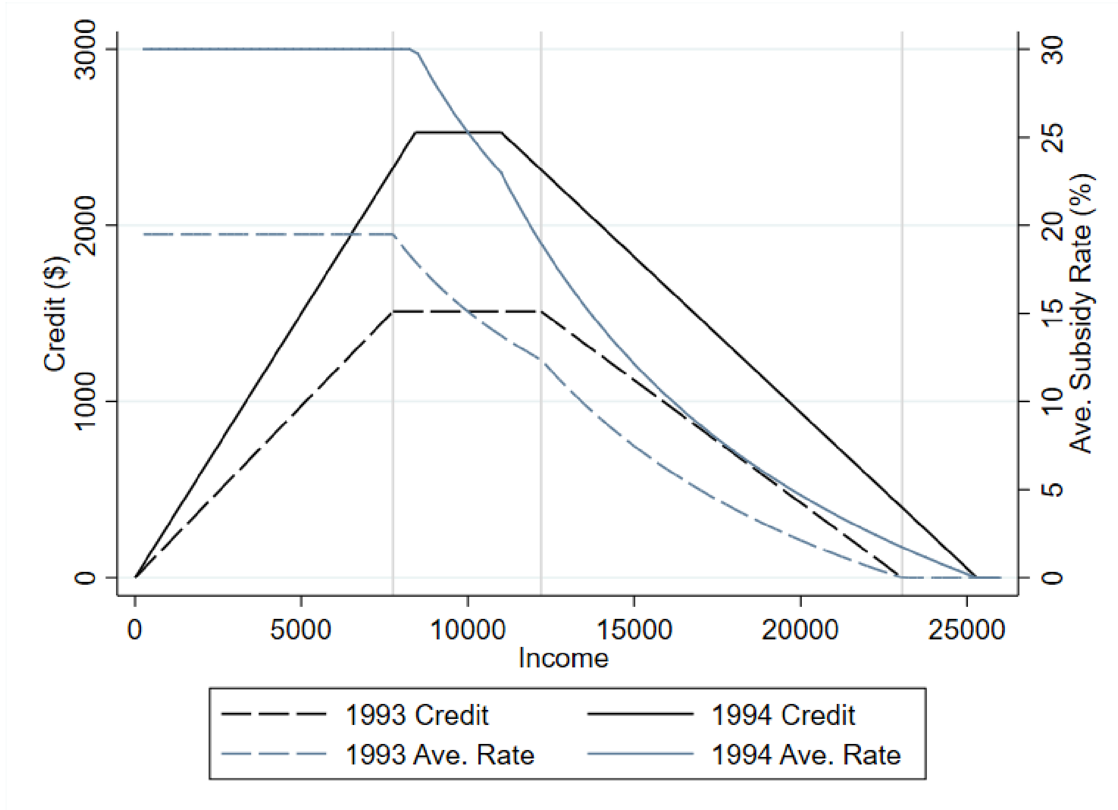

Figure 3: Change in Marginal and Average Subsidy Rates When the EITC Maximum Credit Increases

Notes: The 1994 expansion increased average subsidy rates for households throughout the EITC schedule. The impact on marginal subsidy rates varied across the EITC schedule. For households in the phase-in region the marginal tax rate become more negative, for households in the phase-out region the marginal tax rate became more positive, and for households in the plateau region the marginal tax rates either became larger, smaller or remained constant.

Source: Author's calculations using parameters provided by the Tax Policy Center for a household with two qualifying children from 1993 and 1994.

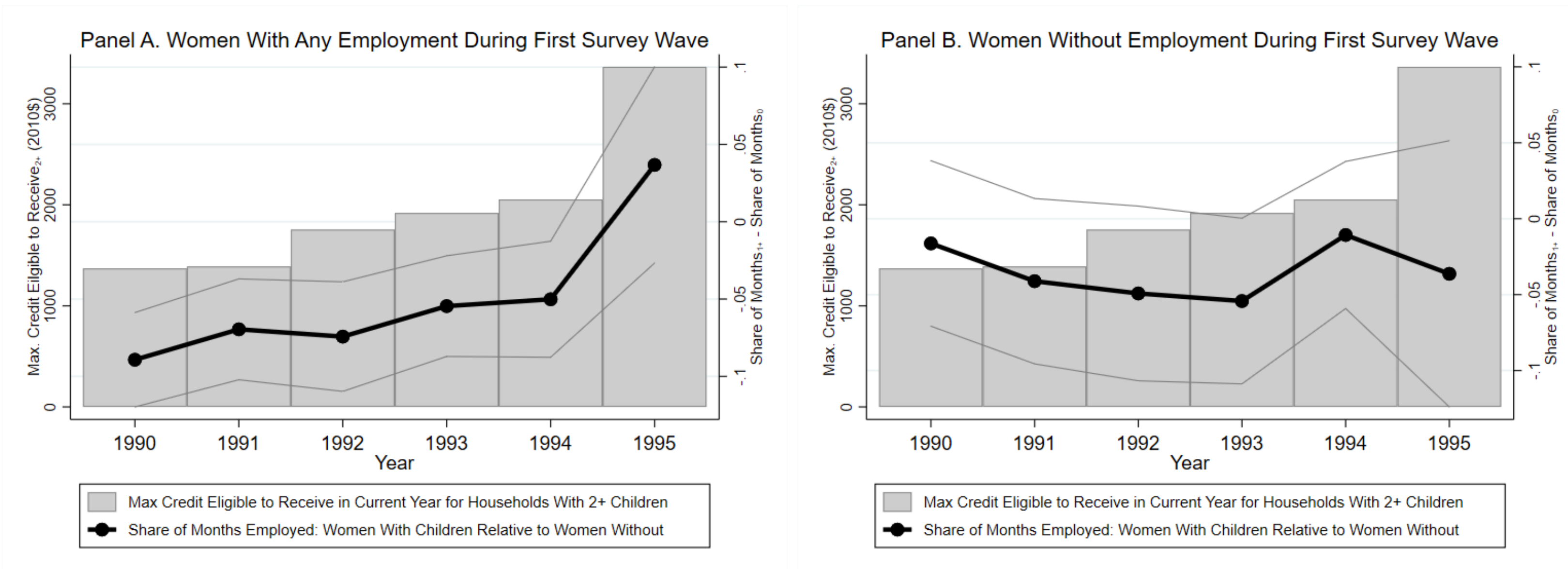

Figure 4: Average Share of Months Employed for Single Women with Children Relative to Single Women without Children

Notes: The solid black line plots the share of months employed (from the second survey year) for single women with a high school degree or less with children relative to women single women with a high school degree or less without children. Averages are weighted using CPS sampling weights. Ninety five percent confidence intervals are also provided. The bar graph depicts the maximum credit eligible to *receive* in the current year (lagged EITC policy) for households with two or more eligible children in 2010 dollars. The maximum credit for households with one eligible child would look similar in most years, but 600 dollars lower in 1995.

Source: Author's calculation using the linked 1989-1995 monthly CPS and EITC program parameters from the Tax Policy Center.

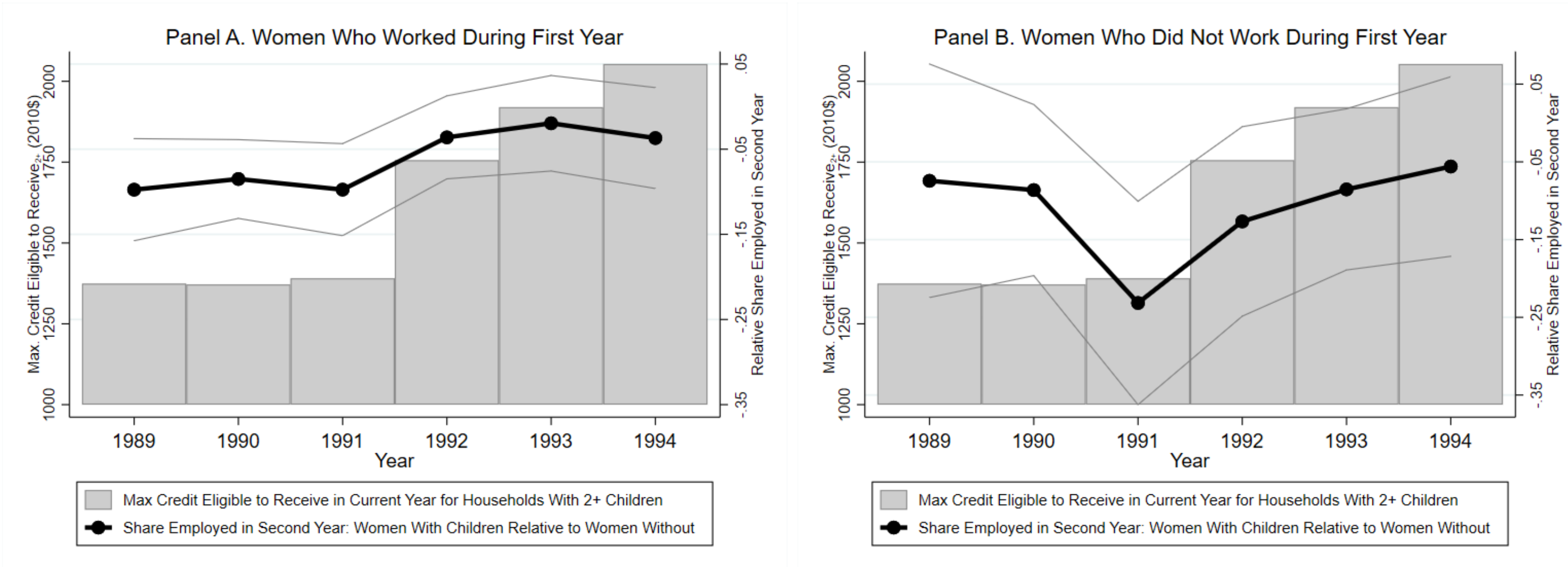

Figure 5: Share Employed During Second Year for Single Women with Children Relative to Single Women without Children

Notes: The solid black line plots the share employed in the second year for single women with a high school degree or less with children relative to women single women with a high school degree or less without children. Averages are weighted using CPS sampling weights. Ninety five percent confidence intervals are also provided. The bar graph depicts the maximum credit eligible to *receive* in the current year (lagged EITC policy) for households with two or more eligible children.

Source: Author's calculation using the linked 1989-1995 ASEC CPS and EITC program parameters from the Tax Policy Center.

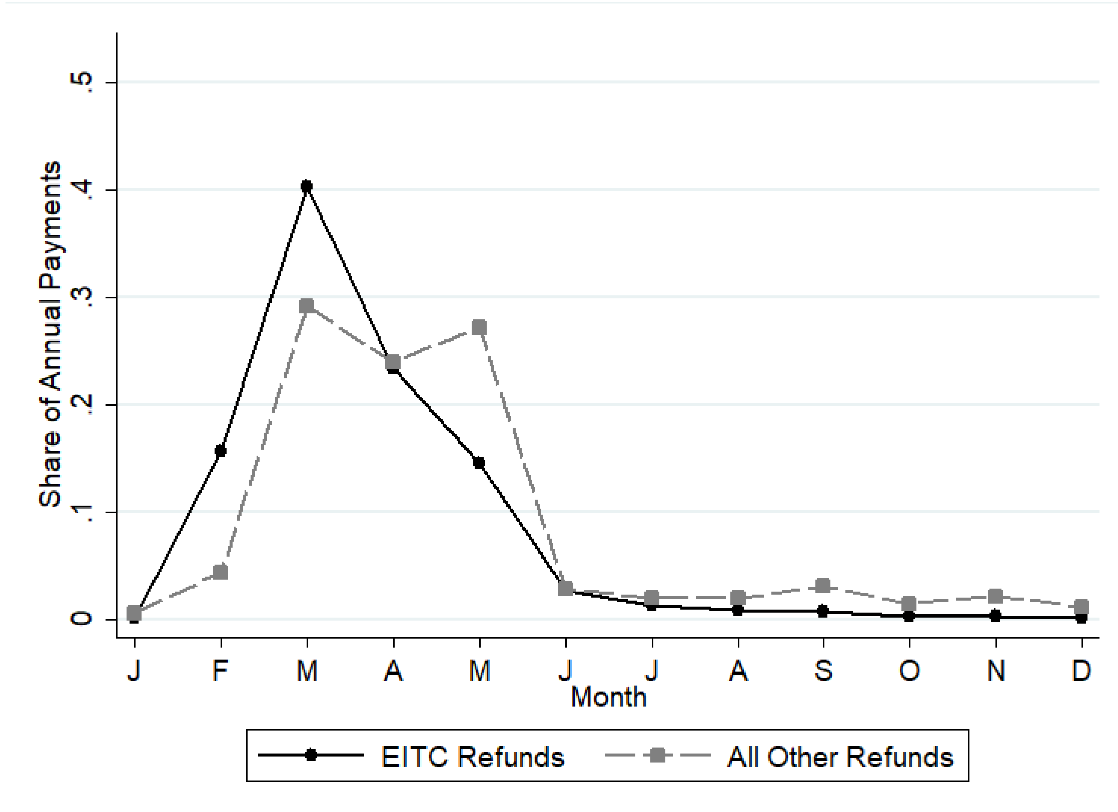

Figure 6: Distribution of Tax Refunds by Calendar Month in 1989

Notes: Plots represent the monthly share of the total tax refund payments made during the year for EITC refunds and all other refunds in 1989.
Source: Author's calculation using Monthly Treasury Statements for January 1989 through December 1989.

VIII Appendix A. Additional Tables and Figures

Table A.1: Re-weighting to Account for Sample Selection in Linked Analysis Sample

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Panel A.	Reported Any Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	0.007*	0.008*	-0.001	-0.0013**
(Credit Eligible to Receive)	(0.004)	(0.004)	(0.004)	(0.0006)
Max Credit$_t$ *($100s)*	0.002	0.001	-0.006	0.0001
(Credit Eligible to Earn)	(0.003)	(0.003)	(0.004)	(0.0004)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.31	0.13	0.44	0.14
Second Wave Dependent Mean	0.91	0.86	0.07	0.001
Panel B.	Reported No Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	-0.001	-0.0001	-0.002	-0.0002
(Credit Eligible to Receive)	(0.005)	(0.004)	(0.003)	(0.0002)
Max Credit$_t$ *($100s)*	0.001	-0.001	0.003	-0.0002
(Credit Eligible to Earn)	(0.005)	(0.004)	(0.002)	(0.0003)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.83	0.88	0.25	0.89
Second Wave Dependent Mean	0.19	0.13	0.06	0.001

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. The analysis sample is reweighted so that the demographic and employment characteristics in the initial year match those of the full sample in Table 1. These weights are created using an entropy weighting approach (see Hainmuller (2012) for details) and re-weights the remaining sample to mach the means for all of the variables in Table 1 of the full sample. Observations given zero weight are excluded from the analysis. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the federal and state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table A.2: Separate Effects of the Credit Currently Earning and the Credit Receiving in the Current Year

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Panel A.	Reported Any Employment During First 4 Monthly Surveys, Credit Currently Receiving			
Max Credit$_{t-1}$ *($100s)*	0.010**	0.009**	-0.001	-0.001*
(Credit Eligible to Receive)	(0.004)	(0.004)	(0.005)	(0.0004)
Panel B.	Reported Any Employment During First 4 Monthly Surveys, Credit Currently Earning			
Max Credit$_{t}$ *($100s)*	0.002	-0.0003	-0.006	-0.0002
(Credit Eligible to Earn)	(0.003)	(0.002)	(0.004)	(0.0005)
Panel C.	Reported No Employment During First 4 Monthly Surveys, Credit Currently Receiving			
Max Credit$_{t-1}$ *($100s)*	-0.002	-0.001	-0.0002	-0.0002
(Credit Eligible to Receive)	(0.007)	(0.005)	(0.003)	(0.0002)
Panel D.	Reported No Employment During First 4 Monthly Surveys, Credit Currently Earning			
Max Credit$_{t}$ *($100s)*	0.003	0.001	0.003	-0.0002
(Credit Eligible to Earn)	(0.005)	(0.004)	(0.002)	(0.0003)

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. There are 20,438 observations (10,219 women) included in Panel A and B (any employed during first four months), and 8,514 observations (4,257 women) included in Panel C and D (no employed during first four months). Changes in the maximum credit do not occur every year and the within sample correlation coefficient between changes in the credit currently earning and the credit received this year is 0.22. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the federal and state minimum wages, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table A.3: Labor Force Attachment Response to EITC Increases, By Attachment in First Survey Wave

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Panel A.	Reported < 4 Months Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	0.018	0.023**	-0.001	-0.004**
(Credit Eligible to Receive)	(0.011)	(0.011)	(0.013)	(0.002)
Max Credit$_t$ *($100s)*	0.003	-0.004	-0.013	0.003
(Credit Eligible to Earn)	(0.009)	(0.008)	(0.011)	(0.002)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.32	0.06	0.54	0.02
Second Wave Dependent Mean	0.70	0.58	0.15	0.002
Panel B.	Reported 4 Months Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	0.008**	0.007	0.001	0.0002
(Credit Eligible to Receive)	(0.003)	(0.004)	(0.004)	(0.0002)
Max Credit$_t$ *($100s)*	-0.001	-0.0004	-0.005**	-0.001*
(Credit Eligible to Earn)	(0.002)	(0.002)	(0.002)	(0.0004)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.03	0.12	0.29	0.08
Second Wave Dependent Mean	0.96	0.92	0.06	0.001

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. There are 3,566 observations (1,783 women) included in Panel A (Less than 4 months employed during first four months), and 16,872 observations (8,436 women) included in Panel B (Four months employed during first four months). Changes in the maximum credit do not occur every year and the within sample correlation coefficient between changes in the credit currently earning and the credit received this year is 0.22. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the federal and state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table A.4: Labor Force Attachment Response to EITC Increases, Impact of Controls

	No Controls (1)	Baseline (2)	State Unemployment (3)	Age Fixed Effects (4)	Welfare Maximum Benefit, Family Cap (5)
Panel A.			Outcome: Ever Employed		
Max Credit$_{t-1}$ *($100s)*	0.010**	0.010**	0.010**	0.012**	0.012**
(Credit Eligible to Receive)	(0.045)	(0.045)	(0.0045)	(0.0046)	(0.0046)
Max Credit$_{t}$ *($100s)*	-0.000	-0.000	0.0001	0.001	0.001
(Credit Eligible to Earn)	(0.0029)	(0.0029)	(0.0029)	(0.0029)	(0.0029)
Panel B.			Outcome: Share of Months Employed		
Max Credit$_{t-1}$ *($100s)*	0.010**	0.010**	0.010**	0.011**	0.011**
(Credit Eligible to Receive)	(0.0042)	(0.0043)	(0.0042)	(0.0044)	(0.0044)
Max Credit$_{t}$ *($100s)*	-0.0019	-0.0019	-0.0018	-0.0013	-0.0013
(Credit Eligible to Earn)	(0.0022)	(0.0022)	(0.0022)	(0.0023)	(0.0023)
Panel C.			Outcome: Ever Exit		
Max Credit$_{t-1}$ *($100s)*	0.0003	0.0003	0.0003	0.0013	0.0013
(Credit Eligible to Receive)	(0.0051)	(0.0051)	(0.0051)	(0.0052)	(0.0052)
Max Credit$_{t}$ *($100s)*	-0.0058	-0.0058	-0.0058	-0.0054	-0.0055
(Credit Eligible to Earn)	(0.0040)	(0.0040)	(0.0040)	(0.0039)	(0.0039)
Panel D.			Outcome: Multiple Exits		
Max Credit$_{t-1}$ *($100s)*	-0.0008*	-0.0008*	-0.0007	-0.0008*	-0.0008*
(Credit Eligible to Receive)	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Max Credit$_{t}$ *($100s)*	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001
(Credit Eligible to Earn)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. Only women who reported any employment during the first four monthly surveys are included (corresponding to Panel A. of Table 2). Changes in the maximum credit do not occur every year and the within sample correlation coefficient between changes in the credit currently earning and the credit received this year is 0.22. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the federal and state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table A.5: Selection on Observables: Changing Composition of Observable Characteristics among Women Who Worked During the First Survey Wave Relative to Women Who Did Not

	White NH (1)	Black NH (2)	Other NH (3)	Hispanic (4)	<HS (5)	Age (6)	Number of Children < 5 (7)	Own Mother in HH (8)
Constant	0.40***	0.43***	0.03**	0.12***	0.48***	29.10***	0.52***	0.36***
(1989 levels)	(0.03)	(0.05)	(0.01)	(0.03)	(0.02)	(0.22)	(0.03)	(0.02)
Employed	0.27***	-0.22***	-0.01	-0.03	-0.32***	0.58**	-0.39***	0.01
	(0.04)	(0.04)	(0.01)	(0.02)	(0.03)	(0.25)	(0.03)	(0.02)
Employed*Year 1990	0.03	-0.03	0.01	-0.01	0.01	0.67	-0.07*	0.05
	(0.03)	(0.04)	(0.01)	(0.01)	(0.03)	(0.43)	(0.04)	(0.03)
Employed*Year 1991	-0.03	0.04	0.01	-0.03	-0.01	0.50	0.05	0.02
	(0.03)	(0.03)	(0.01)	(0.02)	(0.04)	(0.47)	(0.04)	(0.03)
Employed*Year 1992	-0.04	0.03	0.02	-0.01	0.04	1.06***	0.02	-0.06
	(0.03)	(0.04)	(0.02)	(0.01)	(0.03)	(0.36)	(0.05)	(0.04)
Employed*Year 1993	-0.04	0.02	0.02	-0.01	-0.00	0.53*	-0.02	0.00
	(0.03)	(0.04)	(0.01)	(0.02)	(0.03)	(0.29)	(0.05)	(0.03)
Employed*Year 1994	-0.04	0.07	0.01	-0.05*	0.05	0.27	0.02	-0.01
	(0.04)	(0.04)	(0.02)	(0.03)	(0.04)	(0.52)	(0.08)	(0.03)
Year 1990	-0.06**	0.04	-0.00	0.02	0.01	-0.35	0.07*	-0.06**
	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.29)	(0.04)	(0.02)
Year 1991	-0.02	-0.01	-0.00	0.03**	0.05*	-0.15	-0.04	-0.05**
	(0.03)	(0.03)	(0.01)	(0.02)	(0.03)	(0.40)	(0.04)	(0.02)
Year 1992	-0.02	-0.00	-0.01	0.03**	0.01	-0.21	0.01	-0.01
	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.27)	(0.05)	(0.03)
Year 1993	-0.00	-0.00	-0.02	0.03	0.03	0.44**	0.05	-0.05**
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.22)	(0.05)	(0.02)
Year 1994	-0.02	-0.04	-0.01	0.09**	-0.02	0.80**	0.01	-0.04
	(0.04)	(0.04)	(0.02)	(0.04)	(0.03)	(0.32)	(0.07)	(0.03)
Observations	14,476	14,476	14,476	14,476	14,476	14,476	14,476	14,476

Notes: Data from the linked 1989-1995 monthly CPS. Sample restricted to the first year observation for single women with a high school degree or less between the ages of 19 and 44 and for whom survey waves 1-4 were contained in a single year. This allows us to examine if changes in the composition of previously working women over the sample period significantly differ from changes among women that did not work during the first survey wave. Standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Labor Force Attachment Response to EITC Increases Based on Predicted Employment During First 4 Monthly Surveys

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Panel A.	Predicted Reported Any Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	0.0071**	0.0064**	0.0019	-0.0003
(Credit Eligible to Receive)	(0.029)	(0.029)	(0.0033)	(0.0002)
Max Credit$_t$ *($100s)*	0.0027	0.0007	-0.0032	-0.0001
(Credit Eligible to Earn)	(0.0024)	(0.0017)	(0.0026)	(0.0003)
P-value $Credit_{t-1} = Credit_t$	0.25	0.08	0.27	0.63
Second Wave Dependent Mean	0.75	0.70	0.07	0.001
Panel B.	Predicted Reported No Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	-0.0034	-0.0016	-0.0084	-0.0010
(Credit Eligible to Receive)	(0.0137)	(0.0109)	(0.0083)	(0.0010)
Max Credit$_t$ *($100s)*	-0.0085	-0.0082	0.0022	-0.0012
(Credit Eligible to Earn)	(0.0062)	(0.0049)	(0.0051)	(0.0010)
P-value $Credit_{t-1} = Credit_t$	0.72	0.59	0.35	0.90
Second Wave Dependent Mean	0.40	0.34	0.07	0.002

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. There are 24,516 observations (12,258 women) in Panel A, and 4,436 observations (2,218 women) in Panel B. I estimate a logit regression on the 1989 subsample using educational attainment, race indicators, age indicators, month indicators, the state minimum wage and state fixed effects to predict if the woman reported any employment during the first four monthly surveys. Using these coefficients I predict the probability of reporting employment in the first year for full sample and estimate the regression separately for women with a predicted probability above or below 0.5. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table A.7: Estimated Share of Labor Force Response Attributed to Treatment Effect, After Assuming Various Levels of Selection

	Hypothetical Responsiveness Level Among those Selected in by the Policy ($effect_2$)						
Hypothetical Percentage of Workers	0.01	0.02	0.05	0.10	0.25	0.50	1.0
Induced to Work by EITC (p_2)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Annual Effect- Table 6 Column 1							
Point Estimate: 1.1	1.000	0.985	0.939	0.862	0.633	0.250	-0.515
Scaled to 4-month Period: 1.1*0.44=0.48	1.000	0.993	0.973	0.940	0.840	0.673	0.339
Upper Bound: 2.67	1.000	0.962	0.848	0.658	0.089	-0.860	-2.758
Scaled to 4-month Period: 2.67*0.44=1.17	1.000	0.984	0.935	0.853	0.607	0.199	-0.619
Annual Entry Effect- Table 6 Column 5							
Point Estimate: 0.1	1.000	0.999	0.995	0.988	0.967	0.933	0.864
Scaled to 4-month Period: 0.1*0.44=0.044	1.000	0.999	0.998	0.995	0.986	0.970	0.940
Upper Bound: 4.41	1.000	0.936	0.743	0.421	-0.543	-2.150	-5.365
Scaled to 4-month Period: 4.41*0.44=1.94	1.000	0.973	0.891	0.754	0.345	-0.338	-1.703
OBRA93 Effect (Grogger, 2003)							
Point Estimate: 3.6	1.000	0.948	0.793	0.533	-0.245	-1.542	-4.135
Scaled to 4-month Period: 3.6*0.44=1.58	1.000	0.978	0.911	0.800	0.468	-0.087	-1.196

Notes: Values report the share of the total effect that is left over after accounting for selection under various assumptions about the prevalence of selection. A negative value means that selection would explain more than all of the effect. To calculate these shares I use the following computation. Suppose some fraction of the population p is observed to be employed and thus meet the conditioning criterion. This fraction of the population is made up of two groups p_1 who would have worked the first year regardless of the EITC generosity and p_2 which was induced to work because of the EITC generosity, where $p_1 + p_2 = p$. We can decompose the total effect as $\frac{p-p_2}{p} effect_1 + \frac{p_2}{p} effect_2 = \textit{total effect}$. From column 1 of Table 6 we see that 73 percent of the full sample was employed during the year and meet the conditioning criterion (p). From Table 2 we also see that the estimated total effect is 0.01. Given p_2 (the hypothetical percentage of the sample induced to work by the EITC and selecting in) and $effect_2$ (the hypothetical responsiveness among those selectively entering due to the policy), we can solve for $effect_1$ (the impact of the policy on the non-selected sample) as $effect_1 = (0.01 - \frac{p_2}{73} * effect_2) * (\frac{73}{73-p_2})$. I then divide $effect_1$ by the total effect to calculate the share of the total effect that remains after accounting for selection. Across the columns I explore several levels of responsiveness among the selective entrants ($effect_2$). An estimate of 0.05 would suggest that the selective entrants increased their employment probability from one year to the next by 5 percentage points for a $100 increase in the maximum EITC. Given the income and participation levels in this sample this would correspond to an extensive margin labor supply elasticity of 0.36. For reference a 0.10 effect corresponds to an elasticity of 0.65 while a 0.25 effect corresponds to an elasticity of 1.29. Down the rows I explore several hypothetical shares of workers induced to work by the EITC and selecting into the sample, including the insignificant 1.1 percentage point impact from column 1 of Table 6; the 0.1 percentage point impact from column 5 of Table 6 (which solely captures entry); and a 3.6 percentage point impact taken from Grogger (2003) which exploits the OBRA93 EITC expansions. Grogger's (2003) estimate is for a $1000 increase in the maximum EITC, so this is an overestimate of the impact of a $100 increase used here. Since the sample is split by reporting any employment during the first four month surveys (not during the first year –which is not available for everyone) I also scale these levels of selection by the fraction of women who work during the first four month survey waves conditional on working during the year. From the subsample of women interviewed in March, 44% of those who worked at any point during the year worked during the four month window. For my estimates (1.1 and 0.1 percentage points) I also include the calculations evaluated at the 95 percent confidence interval upper bound for completeness. For all levels of selection, the total effect still plays an important role for responsiveness levels consistent with traditional labor supply elasticities (responsiveness at 0.10 or lower).

Table A.8: Labor Force Attachment Response to EITC Increases, Include Women First Interviewed October-December and Use the Average EITC Credit

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Panel A.	Reported Any Employment During First 4 Monthly Surveys			
Ave. Max Credit$_{t-1}$ *($100s)*	0.017***	0.008**	0.004	-0.0002
(Credit Eligible to Receive)	(0.004)	(0.0004)	(0.004)	(0.001)
Ave. Max Credit$_t$ *($100s)*	-0.001	-0.002	-0.004	-0.0005
(Credit Eligible to Earn)	(0.002)	(0.002)	(0.003)	(0.0003)
P-value $Credit_{t-1} = Credit_t$	0.001	0.02	0.14	0.69
Second Wave Dependent Mean	0.92	0.86	0.08	0.002
Panel B.	Reported No Employment During First 4 Monthly Surveys			
Ave. Max Credit$_{t-1}$ *($100s)*	-0.006	-0.004	-0.002	-0.0003
(Credit Eligible to Receive)	(0.004)	(0.003)	(0.002)	(0.0003)
Ave. Max Credit$_t$ *($100s)*	0.001	0.001	0.001	-0.0001
(Credit Eligible to Earn)	(0.004)	(0.003)	(0.002)	(0.0003)
P-value $Credit_{t-1} = Credit_t$	0.13	0.26	0.19	0.56
Second Wave Dependent Mean	0.16	0.11	0.05	0.001

Notes: Data from the linked 1989-1995 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44. There are 27,272 observations (13,636 women) included in Panel A (Any employed during first four months), and 10,878 observations (5,439 women) included in Panel B (no employed during first four months). The maximum credit is averaged over the years in the panel, to account for differences among women whose interviews cross calendar years. The average maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the state minimum wage, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table A.9: Within Individual Annual Labor Supply Response to EITC Increases, By Initial Labor Force Attachment

	Weeks Worked (1)	Weeks Worked ≥ 10 (2)	Weeks Worked ≥ 20 (3)	Weeks Worked ≥ 30 (4)	Weeks Worked ≥ 40 (5)	Weeks Worked ≥ 50 (6)
Panel A.	Reported Between 1 and 51 Weeks Worked During Initial Survey Year					
Max Credit$_{t-1}$ *($100s)*	2.54**	0.040*	0.025	0.056*	0.069**	0.069**
(Credit Eligible to Receive)	(1.06)	(0.024)	(0.021)	(0.031)	(0.031)	(0.029)
Max Credit$_t$ *($100s)*	-0.22	-0.005	-0.003	-0.019**	-0.003	0.007
(Credit Eligible to Earn)	(0.38)	(0.011)	(0.011)	(0.010)	(0.009)	(0.009)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.03	0.13	0.27	0.02	0.03	0.04
Second Wave Dependent Mean	32.9	0.76	0.70	0.61	0.54	0.43
Observations	3,474	3,474	3,474	3,474	3,474	3,474
Panel B.	Reported 52 Weeks Worked During Initial Survey Year					
Max Credit$_{t-1}$ *($100s)*	0.23	0.002	0.003	0.010	0.006	0.003
(Credit Eligible to Receive)	(0.46)	(0.009)	(0.011)	(0.011)	(0.012)	(0.012)
Max Credit$_t$ *($100s)*	-0.01	-0.002	0.002	-0.002	-0.001	0.002
(Credit Eligible to Earn)	(0.20)	(0.004)	(0.004)	(0.004)	(0.006)	(0.008)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.54	0.68	0.85	0.23	0.42	0.95
Second Wave Dependent Mean	47.7	0.96	0.94	0.92	0.89	0.83
Observations	6,578	6,578	6,578	6,578	6,578	6,578

Notes: Data from the linked 1989-1994 ASEC March CPS supplement. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, who were interviewed for two ASEC supplements and reported positive weeks worked in their first ASEC survey. Changes in the maximum credit do not occur every year and the within sample correlation coefficient between the change in the credit currently earning and the credit received this year is 0.27. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the federal and state minimum wages, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table A.10: Response as Measured in the SIPP, 1990-1993 Cohorts

	Ever Employed (1)	Share Months Employed (2)	Ever Exit Employment (3)	Multiple Exits (4)
Max Credit$_{t-1}$ *($100s)*	0.004	0.007***	0.002	-0.0004
(Credit Eligible to Receive)	(0.004)	(0.003)	(0.005)	(0.001)
Max Credit$_{t-1}$**No Emp. Last Year*	-0.005	-0.005**	-0.003	0.0005
	(0.003)	(0.002)	(0.004)	0.001)
Max Credit$_{t}$ *($100s)*	0.0002	-0.001	-0.002	-0.00002
(Credit Eligible to Earn)	(0.003)	(0.002)	(0.003)	(0.001)
Max Credit$_{t}$**No Emp. Last Year*	0.002	0.001	0.002	-0.00002
	(0.003)	(0.001)	(0.004)	(0.001)
P-value Credit$_{t-1}$ = *Credit*$_{t}$	0.58	0.09	0.61	0.87
Dependent Mean	0.68	0.61	0.11	0.009
Observations	14,579	14,579	14,579	14,579

Notes: This analysis used monthly data from the 1990, 1991, 1992, and 1993 cohorts of the SIPP. These cohorts were surveyed every four months for 32 to 40 months and recalled employment status for the previous four months. It is worth noting that previous work has documented considerable recall bias and "seam" effects in the SIPP (Martini, 2002). After 1993, the SIPP was redesigned and the next cohort began in 1996. I construct the probability of employment, the share of months worked, and the incidence of at least one or multiple exits at the annual level. Because I observe individuals for multiple years, conditioning the sample on employment in the previous year would result in an unbalanced sample as individuals transition into and out of employment. To estimate an effect similar to the previous estimates from equation (6), I interact *Max Credit*$_{t-1}$ and *Max Credit*$_{t}$ with an indicator for whether the woman was not employed in the previous year. This allows women who were and were not employed at the time of the policy change to respond differently. Because of these interactions, I also include an indicator for whether the woman was not employed during the previous year. As in equation (6), I control for the federal minimum wage, the state minimum wage, whether or not there is a TANF waiver in place, and include number of eligible children specific linear trends. Sample includes annual observations for all single women with a high school degree or less between the ages of 19 and 44. There are only 3,730 women in the SIPP sample. Changes in the maximum credit do not occur every year and the within sample correlation coefficient between the credit currently earnings and the credit received this year is 0.23. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include an indicator for not being employed last year, state minimum wages, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the SIPP. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Table A.11: Recent Responses to State and Federal EITC Credit Increases, 1999-2016

	Ever Employed	Share Months Employed	Ever Exit Employment	Multiple Exits
	(1)	(2)	(3)	(4)
Panel A.	Reported Any Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	-0.0003	-0.0007	-0.0013	-0.00004
(Credit Eligible to Receive)	(0.0015)	(0.0010)	(0.0018)	(0.0001)
Max Credit$_t$ *($100s)*	0.0024***	0.0017***	0.0007	0.0000
(Credit Eligible to Earn)	(0.0008)	(0.0005)	(0.0011)	(0.0001)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.05	0.02	0.22	0.74
Second Wave Dependent Mean	0.94	0.86	0.11	0.003
	Panel B. Reported No Employment During First 4 Monthly Surveys			
Max Credit$_{t-1}$ *($100s)*	-0.0034	-0.0029	-0.0014	-0.00002
(Credit Eligible to Receive)	(0.0050)	(0.0034)	(0.0019)	(0.0001)
Max Credit$_t$ *($100s)*	-0.00005	-0.0008	-0.0015***	-0.00004
(Credit Eligible to Earn)	(0.0009)	(0.0007)	(0.0004)	(0.0001)
P-value Credit$_{t-1}$ = *Credit*$_t$	0.51	0.54	0.96	0.87
Second Wave Dependent Mean	0.12	0.08	0.03	0.001

Notes: Data from the linked 2000-2016 monthly CPS. Sample includes two observations for all single women with a high school degree or less between the ages of 19 and 44, and for whom survey waves 1-4 were contained within a single year. There are 47,312 observations (23,656 women) included in Panel A (Any employed during first four months), and 21,074 observations (10,537 women) included in Panel B (no employed during first four months). Changes in the maximum credit do not occur every year and the within sample correlation coefficient between the credit currently earnings and the credit received this year is -.05. The maximum credit is converted to real 2010 dollars using the personal consumption index and measured in hundreds of 2010 dollars. Controls include the federal and state minimum wages, an indicator for a TANF waiver, and linear trends by the number of qualifying children during the first survey wave. Individual and year fixed effects are included. Regressions weighted using population weights provided by the CPS. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<.1.

Appendix B. Stylized Model of High Frequency Employment Responses to the EITC

Consider a conceptual model where a less educated single woman is making the decision to work or not in the current week (t) by comparing the lifetime expected utility she could achieve in both cases. Assume labor market rigidities prevent her from adjusting labor supply on the intensive margin and she can only adjust by entering or dropping out of the labor force.

Let $u_t(\cdot)$ be the current period utility where $u' > 0$ and $u'' < 0$. $E_tV_{t+1}(\cdot)$ is the expected future value, which is a function of wealth and will be differentiated by employment status in period t. Allowing $E_tV_{t+1}(\cdot)$ to depend on the employment status accounts for costs or frictions that potential entrants might face as well as potential long run benefits associated with employment (Dahl, Deleire & Schwabish, 2009).[37] I will remain agnostic about the functional form of $E_tV_{t+1}(\cdot)$, but assume that it is increasing and concave ($E_tV'_{t+1} > 0$ and $E_tV''_{t+1} < 0$). These assumptions are fairly standard as the individual's future self faces the same problem, but is endowed with more resources.

Assume there is a tax policy, which gives positive transfers associated with low-income work, similar to the EITC. For simplicity, this income transfer will be defined to be some percentage (τ) of total earnings during the year. Because of potential cost shocks, the woman cannot forecast this transfer perfectly for any period, but expects it to be

$$\tau(X_t + w_t), \ \ where \ \ X_t = E_t\big(\sum_{i \neq t}^{5} 2w_i * 1\{y_i = 1\}\big) \tag{A.1}$$

if she works this period (period t) and $\tau(X_t)$ if she does not work in period t.[38] This subsidy does not increase net wages in the current period, but only enters the value function by providing more wealth in the future.[39] Regardless of her work status this week, she will get a transfer $\tau(X_t)$, so both income and substitution effects will potentially be at play. If she works, her decision can be given by the following Bellman equation

$$V_t^w(a_{t-1}) = \max_{at} u(w_t - c_t + a_{t-1} + a_t) - \phi + \beta E_t V_{t+1}^w(a_t + \tau(X_t + w_t)). \tag{A.2}$$

She receives wages (w_t) and faces both psychic and monetary costs associated with working. The monetary costs (c_t) such as paying for transportation or childcare, can vary from period to period according to a known distribution F, but the current costs are observed by the woman before making her labor supply decision. The psychic utility cost (ϕ_t) associated with working is distributed according to G. This cost can capture things like, a distaste for work, the disutility associated with interacting with difficult managers or coworkers, or the emotional cost of leaving children without adequate supervision as suggested by Edin and Lein (1997) (p. 133-136). For simplicity assume c_t and ϕ_t are independent. The EITC increases the future value of working in this period by increasing the transfer later on.

[37]For example, stable employment might lead to longer job tenure and change the lifetime trajectory of wages.

[38]This transfer system is a simplified version of the EITC. In reality, the refund rate of the EITC is not a constant, but a function of earnings.

[39]There has been a recent literature exploring the incidence of the EITC and resulting impacts on market wages (Rothstein, 2008; Leigh, 2010). However, during this time period many less educated single women were paid wages at or near the minimum wage, constraining general equilibrium adjustments.

If she does not work, her decisions is represented by the following Bellman equation

$$V_t^n(a_{t-1}, y_{t-1}) = \max_{at} u(b_t + a_{t-1} + a_t) + \beta E_t V_{t+1}^n(a_t + \tau(X_t)). \tag{A.3}$$

She receives an outside option benefit (b_t), similar to Aid to Families with Dependent Children (AFDC), Temporary Assistance for Needy Families (TANF), or Supplemental Nutritional Assistance Program (SNAP) benefits. The EITC will increase her future wealth based on her expected work during the rest of the year. In both states, the woman begins period t with wealth a_{t-1}, and can save or borrow from the future. She will decide to work in the current period if the value of working is greater than the value of not working or

$$u_t(w_t - c_t + a_{t-1} - a_t^w) - \phi_t + \beta E_t V_{t+1}^w(a_t^w + \tau(X_t + w_t)) \geq u_t(b_t + a_{t-1} - a_t^n) + \beta E_t V_{t+1}^n(a_t^n + \tau(X_t)) \tag{A.4}$$

where a_t^w is the optimal wealth carried over if she works and a_t^n is the optimal wealth carried over if she does not work. Although this inequality will be nearly always satisfied for a high wage worker, for a single woman facing wages at or near the minimum wage, working might be a "financial wash (p.67, Edin & Lein, 1997)," and even a small psychic cost shock, such as the inability to leave work when a child is sick, might induce her to exit from employment. From equation (A.4), there will be a threshold psychic cost where the woman is indifferent between employment and non-employment in the current period, defined as

$$\phi^* = u_t(w_t - c_t + a_{t-1} - a_t^w) - u_t(b_t + a_{t-1} - a_t^n) + \beta E_t V_{t+1}^w(a_t^w + \tau(X_t + w_t)) - \beta E_t V_{t+1}^n(a_t^n + \tau(X_t)). \tag{A.5}$$

For a given set of w_t, c_t, a_t, τ, and b_t as well as the distribution of ϕ_t the probability the woman works will be equal to the probability that the psychic costs is less than or equal to the cost threshold ϕ^*. Now, consider what happens to this cost threshold, when the subsidy (τ) increases

$$\frac{\partial \phi^*}{\partial \tau} = \beta E_t V_{t+1}^w{}'(a_t^w + \tau(X_t + w_t))\big(\frac{\partial a_t^w}{\partial \tau} + X_t + w_t\big) - \beta E_t V_{t+1}^n{}'(a_t^n + \tau(X_t))\big(\frac{\partial a_t^n}{\partial \tau} + X_t\big). \tag{A.6}$$

Since the value function is increasing, its first derivatives will be positive. The sign of this total effect depends on the sign of $\big(\frac{\partial a_t^w}{\partial \tau} + X_t + w_t\big)$ and $\big(\frac{\partial a_t^n}{\partial \tau} + X_t\big)$. From the first order condition from equation (A.1) and the envelope condition, we see

$$V_t^w{}' = -u_t' + \beta E_t V_{t+1}^w{}' = 0 \rightarrow u_t' = \beta E_t V_{t+1}^w{}' \tag{A.7}$$

$$V_t^w{}' = u_t' \tag{A.8}$$

Combining the first order condition and envelope condition (iterated one period forward) yields the Euler Equation

$$u_t' = \beta E_t u_{t+1}' \tag{A.9}$$

Totally differentiate the Euler Equation with respect to a_t and τ, and rearrange to determine how the optimal choice of a_t responds to changes in w_t

$$-u_t'' da_t = \beta E_t u_{t+1}'' da_t + \beta E_t u_{t+1}''(X_t + w_t) d\tau \tag{A.10}$$

$$\frac{\partial a_t}{\partial \tau} = -\frac{\beta E_t u_{t+1}''}{u_t'' + \beta E_t u_{t+1}''}(X_t + w_t) \tag{A.11}$$

By the concavity of u, $u'' < 0$ so $\frac{\beta E_t u_{t+1}''}{u_t'' + \beta E_t u_{t+1}''} < 1$ and $\frac{\partial a_t}{\partial \tau} + X_{twt} > 0$. Intuitively, the refund increases wealth in the future and reduces the need to save today, but, smoothing motives make this a less than one for one trade off. This implies that

$$\beta E_t {V_{t+1}^{w}}'(a_t^w + \tau(X_t + w_t))\big(\frac{\partial a_t^w}{\partial \tau} + X_t + w_t\big) > 0. \tag{A.12}$$

The first order conditions and envelope condition from equation (A.2), when the woman does not work, will yield a similar result, that $\frac{\beta E_t u''_{t+1}}{u''_t + \beta E_t u''_{t+1}} < 1$ and $\frac{\partial a_t}{\partial \tau} + X_t > 0$, which implies the following relationship

$$\frac{\partial \phi^*}{\partial \tau} = \underbrace{\beta E_t {V_{t+1}^{w}}'(a_t^w + \tau(X_t + w_t))\big(\frac{\partial a_t^w}{\partial \tau} + X_t + w_t\big)}_{>0} - \underbrace{\beta E_t {V_{t+1}^{n}}'(a_t^n + \tau(X_t))\big(\frac{\partial a_t^n}{\partial \tau} + X_t\big)}_{>0} \tag{A.13}$$

The first term –relating to the state of the world where the woman works– is positive, and represents the substitution effect, the additional wage subsidy increases the benefit of working in period t. The second term –relating to the state of the world where the woman does not work– is also positive, and thus affects the cost threshold negatively. This represents the income effect, getting a larger transfer regardless of work status in period t incentivizes her to work less. As such, an increase in the size of the subsidy has an ambiguous effect on the cost threshold, the probability of the woman working in period t, and the probability of exiting the labor force.

There are however several cases when the substitution effect is likely to dominate the income effects, resulting in a higher probability of working and a lower probability of exit. First, the cost threshold unambiguously increases if the woman does not expect to work in any other period ($X_t = 0$). In this case there are only substitution effects, and this collapses to the traditional extensive margin decision that has been focused on by the previous literature.

Another case to consider is if the woman is living hand to mouth ($a_t^w = a_t^n = 0$). If this is the case, the impact of an increase in the subsidy on the cost threshold can be represented as

$$\frac{\partial \phi^*}{\partial \tau} = \beta E_t {V_{t+1}^{w}}'(\tau(X_t + w_t))w_t + X_t\big(\beta E_t {V_{t+1}^{w}}'(\tau(X_t + w_t)) - \beta E_t {V_{t+1}^{n}}'(\tau(X_t))\big). \tag{A.14}$$

Notice that if X_t is small, meaning the woman expects low total earnings, the income effects are likely to be small, and the cost threshold for dropping out is likely to increase. This will likely be a relevant case for many low-income women who have little to no savings, and low expected earnings.[40] Thus, when the EITC becomes more generous, we would expect that for some fraction of women, the drop out cost threshold will increase, meaning the woman is willing to incur a large psychic cost, and still remain working. This results in a higher probability of working in any given period, and a mechanical reduction in the probability of exit, which lengthens employment spells in expectation. For some fraction of women, the work incentives of the EITC likely increase the opportunity cost of dropping out of the labor force in any given period, leading to more weeks of employment, longer employment spells, and more stable employment with less frequent exit. The magnitude of these effects remain an empirical question. Enriching this model to reflect nuances of real-world decisions (such as fixed entry costs or liquidity constraints) generally affect the magnitude, not sign of these predictions.[41]

[40] This would also be the case if marginal value of an additional dollar in the future is similar in both the working and non-working states of the world. The predictions are similar if the woman is not necessarily living hand-to-mouth, but if a_t^w and a_t^n are both small, or close to the same value.

[41] Fixed costs might also play a role (Kleven & Kreiner, 2006; Eissa et al., 2008). Introducing fixed costs associated with entering the labor force increases labor force attachment of those already employed and deters non-working single women from entering employment. The transfer (τ_t) will still generate the same substitution and income effects as before, although they will be muted. If less educated single women

are liquidity constrained and cannot borrow against the future, there will be some probability (γ) that the woman cannot cover the monetary costs of work even if the psychic cost is low and lifetime value is high. However, with probability $1-\gamma$ she can still account for the future benefits of the tax transfer, so an increase in τ_t will still create positive substitution effects, but by a smaller magnitude because there is a positive probability the liquidity constraint will bind.

Appendix C. Extensive Margin Labor Supply Elasticity Calculations

I calculate back of the envelope extensive margin elasticities following the method outlined in Chetty et al. (2012), using the following formula

$$\varepsilon = \frac{log(emp.\ rate + \Delta emp) - log(emp.\ rate)}{log(total\ income + \Delta income) - log(total\ income)} \tag{A.15}$$

However, I must rescale several measures to make it comparable to previous elasticity estimates. As seen in column (1) of Table 2, the average employment rate of women who reported employment during the initial survey wave was 0.91. Also, the impact of the policy was to increase employment by 0.01. From the linked ASEC sample, I find that for this group, average total income (not just wage income) increased from 23,233.4 to 23,664.9, so $\Delta income = 431.5$. This yields an elasticity estimate of

$$\varepsilon = \frac{log(0.91 + 0.01) - log(0.91)}{log(23664.9) - log(23233.4)} = 0.59 \tag{A.16}$$

However, this calculation does not account for the high frequency turnover among this population and captures changes among people who would have worked during other parts of the year, regardless of the EITC. Among my linked ASEC sample of single mothers, the probability of working at all during the year conditional on not working this 4 month period is 0.17. In other words, 83% of the time, single mothers who did not work this 4 month period would not have worked at some point during the year. Thus to get an elasticity that is directly comparable to the annual elasticity estimates, I must scale the 0.01 impact by 0.83, the fraction of women for whom the annual employment status would change.

$$\varepsilon = \frac{log(0.91 + 0.01 * 0.83) - log(0.91)}{log(23664.9) - log(23233.4)} = 0.49 \tag{A.17}$$

This elasticity is slightly larger than those calculated by Eissa & Liebman (1996) and Meyer & Rosenbaum (2001), but inside the confidence interval of their estimates, and the same as the estimate in Bastian (2017).

I can also construct elasticity estimates from the coefficients in Table 6. Among those previously employed we see that the probability of being employed the next year goes up by 2.5 percentage points. Here the probability of being employed during the second wave is also 0.91, thus we get an elasticity of

$$\varepsilon = \frac{log(0.91 + 0.025) - log(0.91)}{log(23664.9) - log(23233.4)} = 1.47 \tag{A.18}$$

This is a very large elasticity and suggests single mothers are very responsive to changes in the EITC. However, this is focusing on a subgroup of mothers who have exhibited recent attachment to the labor force, so we might expect them to be more responsive. To make this directly comparable to previous elasticity estimates, we must scale this for the full population of single mothers. First off, when I do not condition on labor force attachment in the previous year, employment rates are lower, at 0.73 (column (1)). Total income is also lower increasing from 17,036.4 to 17,796.6 from the first year to the second year in the ASEC sample. Additionally, since approximately 60 percent of the sample of single women with children worked during the first year, we should scale the treatment effect by 0.6 to get the treatment effect on the full population. This yields an elasticity estimate of

$$\varepsilon = \frac{log(0.73 + 0.025 * 0.6) - log(0.73)}{log(17796.6) - log(17036.4)} = 0.47 \tag{A.19}$$

Once again, this is not much larger or statistically different than estimates for this same population in the previous literature.

I also estimate extensive margin labor supply elasticities when evaluating the role of selection bias in Appendix Table A.7. For these elasticities I am interested in the response of women who enter the labor force in the first year. In the March monthly sample of single mothers who were not working in the first year, but then work in the second year, second year income was 12,312.4. Income for working single mothers then increases to 23,664.9. Because I only observe individuals for two years, I cannot both identify new annual entrants and measure their employment rates in the year after they entered. This would require three years of data. Instead I use the 19 percent employment rate estimate from Panel B of Table 2 to capture the year-to-year transitions of women less attached to the labor force. With these measures I calculate the following elasticities for 5, 10, 25, and 50 percentage point impacts

$$\varepsilon = \frac{log(0.19 + 0.05) - log(0.19)}{log(23664.9) - log(12312.4)} = 0.36 \tag{A.20}$$

$$\varepsilon = \frac{log(0.19 + 0.10) - log(0.19)}{log(23664.9) - log(12312.4)} = 0.65 \tag{A.21}$$

$$\varepsilon = \frac{log(0.19 + 0.25) - log(0.19)}{log(23664.9) - log(12312.4)} = 1.29 \tag{A.22}$$

$$\varepsilon = \frac{log(0.19 + 0.50) - log(0.19)}{log(23664.9) - log(12312.4)} = 1.97 \tag{A.23}$$

Impacts greater than 10 percentage points are not consistent with traditional micro extensive margin labor supply elasticities which range between 0.1 and 0.5.