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The Effect of the Earned Income Tax Credit (EITC) on Savings: Do Low-Income Households with Multiple Children Save More EITC Money?

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Recommended Citation

Dao, Vinh, "The Effect of the Earned Income Tax Credit (EITC) on Savings: Do Low-Income Households with Multiple Children Save More EITC Money?" (2020). *MPA/MPP/MPFM Capstone Projects*. 405. https://uknowledge.uky.edu/mpampp_etds/405

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THE EFFECT OF THE EARNED INCOME TAX CREDIT (EITC) ON SAVINGS:

Do Low-Income Households with Multiple Children Save More EITC Money?

Abstract

EITC provides monetary assistance to lower-income families with children, and it has expanded to become the largest cash-transfer program in the United States. Understanding how EITC affects savings is crucial, since they are closely linked to financial stability of millions of American families. Do these families put the tax credit in their savings accounts, or do they just spend it? To tackle this question, this paper examines the effects of an increase in EITC's generosity in 2009 on investment income of eligible households with 3 children. I perform a difference-in-differences using 2005-2014 CPS data and find no statistical evidence of the reform's impact on savings.

University of Kentucky
Master in Public Policy (MPP) Capstone Project
April 16, 2020
Presented by: Vinh Dao

Executive Summary

This paper examines the effect of EITC on savings, specifically looking at the EITC threshold expansion included in the 2009 Obama's American Recovery and Reinvestment Act (ARRA) stimulus package. I conduct a difference-in-differences analysis and derive the results from a "mainline" regression model using 2005-2014 Current Population Survey household-level data. The mainline model includes year and household fixed effects while controlling for demographic characteristics and whether the household receives income from other federal social welfare programs and unemployment benefits. The results suggest that the EITC threshold-expansion policy from the 2009 ARRA stimulus package does not affect savings behavior of EITC-eligible families with three children. Furthermore, I find that welfare benefits such as SNAP, Medicaid, and unemployment benefits matter regarding the policy's effect on investment income, whereas demographic characteristics such as age and race can potentially influence savings decisions. Perhaps, in order to single out genuine behavioral responses of targeted low-income households to tax incentives, a tax credit expansion policy should consider the relevance of demographic factors plus how the families' interactions with other ongoing welfare benefits can alter their saving behaviors.

Introduction

The earned income tax credit (EITC) is a refundable tax credit for low- and middle-income working individuals/couples who have children, where the amount of the benefit depends on the recipient's earned income and number of children. The key takeaway is that the credit is equal to a portion of income and keeps increasing until it reaches a maximum amount. After that, the credit "plateaus" and then eventually decreases with each additional dollar of income until it reaches zero – i.e., the household no longer receives monetary assistance if its earned income is large enough¹. In general, EITC's generosity primarily targets poorer families and has expanded to become the largest cash-transfer program for lower-income families with children. According to the 2013 federal spending report by the Heritage Foundation, EITC is the third-largest social welfare program in the United States, only after Medicaid and food stamps (SNAP)².

Over the years, EITC has been part of political debates in the United States. The advocates of EITC argue that, unlike traditional welfare, the tax credit helps "promote both the values of family and work" (Eissa and Hoynes, 2004)³. Theoretically, by rewarding working individuals, the tax credit can increase labor supply and thus put downward pressure on wages. So, in the quest of providing adequate assistance to the poor, most economists prefer expanding

¹ See Figure C in the Appendix for example of a detailed EITC schedule. Source: Urban-Brookings Tax Policy Center.

² Almost 27 million American households received more than \$56 billion in EITC benefit, whereas Medicaid (both federal and state) costs \$402 billion, and SNAP costs \$78 billion. Also see Figure D in the Appendix for the overtime growth trend of EITC spending. Source: Falk and Crandall-Hollick (2018).

³ Due to the nature of the earned income tax credit, households do not receive any tax credit if they do not participate in the labor market – i.e., unemployed and not having wage income. The government designs the program to incentivize low-income families to work by giving them a tax credit amount with respect to their income level. The more wage income the filer earns, the higher the tax credit (up to a specific threshold). In fact, empirical evidence consistent with economic theory suggests that the EITC promotes employment among eligible unmarried women with children, as indicated by Eissa and Hoynes (2004).

the EITC program to increasing the minimum wage⁴. Consistent with theory, Sykes *et al.* (2014) study low-wage EITC recipients and indicate that the tax credit is one of the most effective social welfare programs, whereas the Census Bureau statistics show that EITC has lifted 5.4 million Americans above the 2010 poverty line (Greenstein, 2011).

This paper examines the effect of EITC on savings, specifically looking at the EITC threshold expansion included in the 2009 Obama's ARRA stimulus package⁵. After Congress enacted the package, House and Senate each spent \$4.7 billion on expanding the threshold to provide monetary assistance for working households with 3 or more children in the form of tax credit. The Internal Revenue Service (IRS) states that, for tax years 2009 and 2010, the ARRA created a new family category – three or more children – and provide these larger families larger tax credits. The credit phases in at 45 percent of income (up from 40 percent), effectively increasing the maximum credit for these families by roughly \$600⁶. Specifically, the maximum refundable tax credit is \$5,657 for a 3-or-more-children family and \$5,028 for a 2-children family.

It is crucial to understand how the tax credit program affects households' saving behaviors. A positive impact of EITC on savings is equivalent to more available money and a higher degree of financial stability for millions of American families. In this paper, I use investment income as an indicator for savings. The analysis involves Current Population Survey

⁴ A random survey of 568 members of the American Economic Association in 2011 shows that 63% of economists agree that EITC should be expanded (Fuller and Geide-Stevenson, 2014).

⁵ The package expanded EITC for families with at least 3 children and expected to benefit 6.5 million working parents with 15 million children. This policy was then extended for 2 more years by the Tax Relief, Unemployment Insurance Reauthorization, and Job Creation Act of 2010.

⁶ The expansion increases the marriage penalty relief by raising the income threshold at which the EITC begins to phase out for married couples to \$5,000 above the amount for unmarried filers, thereby giving filers a longer plateau. Nonetheless, the combined plateau and phase-out range for married filing jointly is still not double that for single filers, and thus there still is a marriage penalty, just less than before.

(CPS) household-level panel data from 2005 to 2014. I conduct 4 separated difference-in-differences estimates using households with 3 children as the treatment group and households with 2 children as the control group. For all 4 models, I find no statistical evidence of the 2009 EITC threshold expansion affecting investment income of EITC-eligible families with 3 children. In general, the results are inconsistent with findings from several prior studies. For instance, Weber (2016) finds a statistically significant drop in the likelihood that households with 2 children have investment income after the 1996 EITC expansion. The policy analyzed in this paper, however, is a threshold expansion instead of a change in tax rate. Hence, I expect the effect of such a different policy on the study's targeted group – households with 3 children – to also be different. Furthermore, according to the mainline difference-in-differences model's estimate, welfare benefits such as SNAP, Medicaid, and unemployment benefits matter regarding the policy's effect on investment income, and that demographic characteristics such as age and race can influence savings decisions. Perhaps, in order to single out genuine behavioral responses of targeted low-income households to tax incentives, a tax credit expansion policy should consider the relevance of demographic factors plus how the families' interactions with other ongoing welfare benefits can alter their saving behaviors.

Literature Review

This part of the paper reviews the existing literature on the effect of EITC on savings behavior among poorer households with children. EITC primarily targets households with limited access to savings accounts, and therefore a positive impact of the policy on saving behavior means more available money and more stable financial status for millions of low- and middle-income

American families⁷. Duflo *et al.* (2006) state that these families save little money, and in turn, researchers and policymakers have strived to find ways to raise savings among these families.

Overall, empirical research shows mixed evidence of the effects of EITC on savings. For instance, Jones and Michelmore (2018) and Mendenhall *et al.* (2012) find evidence of EITC associated with increases in savings accounts. In contrast, studies such as Despard *et al.* (2015) and Weber (2016) indicate that EITC disincentivizes saving. This is intriguing, since according to theory, the design of the EITC schedule is expected to provide disincentives to save and invest. Nevertheless, such a wide array of results may potentially be a consequence of conducting different methods on different samples under different reform policies.

Jones and Michelmore (2018), for instance, examine how EITC expansions affect household finances over the past two decades using Survey of Income and Program Participation data. The authors measure household finances using several indicators include money held in savings and checking accounts, credit card use, and other unsecured debt (e.g. medical bills not covered by insurance, money owed to private individuals, car loans, mortgages, etc.) Jones and Michelmore (2018) conduct a stimulated instrument (SI) approach that measures policy-induced variation in EITC, looking at average benefit at the state-year-family size level and capturing the differences in policy generosity across states, years, and family sizes. They find an increase of \$1,000 in average EITC is associated with a 3 percentage-point increase in the likelihood of holding money in a savings or checking account, whereas savings balances increase on average

⁷ According to the IRS, in the 2009 tax year, a family reaches the maximum tax credit amount if its earned income hits \$12,550, and then the credit plateaus and starts to decrease as the family's income hits \$21,450. Thus, we can define a low-income family as a family whose earned income is less than \$21,450 per year. On the other hand, defining a middle-income family is trickier since the tax credit's availability differs across family groups. Specifically, the tax credit amount reaches zero if the earned income hits \$40,500 for a family with 1 child, \$45,300 for a family with 2 children, and \$48,279 for a family with 3 or more children. So, a middle-income family should have income level in between \$21,450 and \$48,279 per year. Note: these statistics assume that the households' filing status is the same – "Married Filing Jointly". Source: IRS, Publication 590, Category No. 15173A.

by about \$700. Additionally, the authors find evidence of decreases in unsecured debt holdings, which furtherly supports their claim of EITC helps improve financial stability.

Mendenhall *et al.* (2012) conduct a social service study and provide further evidence on the positive impact of EITC on saving behavior among eligible households. The authors indicate that the tax credit provides families opportunities to pay old bills and build assets. An in-depth interview survey with 194 parents who receive at least \$1,000 in EITC shows that the majority of families (57 percent) plan to allocate a significant refund portion to savings while being able to pay their debt and bills. Mendenhall *et al.* (2012) examine how families plan to use tax refunds prior to their receipt and how they actually spend the refunds after receiving them. Nonetheless, such a study without formal quantitative analysis that uses a small data sample may not provide conclusive policy implications on a broader margin. In contrast to Mendenhall *et al.* (2012)'s findings, Despard *et al.* (2015) use data on EITC recipients by the Refund to Savings initiative and find immense challenges that obstruct saving and asset building among households. Even though it seems that EITC-eligible households want and are able to save, the authors indicate that these households tend to have high levels of unsecured debt relative to their low levels of income. Hence, paying down debt is a primary use of their tax refunds, and thus they are left struggling with retaining savings in the long run.

An important piece of literature on the subject is Weber (2016)'s study on the effect of EITC on low-income households' saving. Weber (2016) finds a statistically significant drop in the likelihood that the treatment group have investment income after the 1996 EITC expansion. Additionally, the author finds a 1 percent increase in the after-tax return to saving causes a 3.05 percent increase in investment income. Weber (2016) recalls the design of the EITC schedule that induces non-labor income as a determinant of the amount of tax credit received to ensure it

goes to the low-wealth individuals. However, the author finds an unintended consequence of such a design being the distortions in non-labor income (particularly investment income). Weber (2016) implies that about 40 percent of the saving decline in income-bearing accounts by EITC recipients is derived from the incentive shifts for saving caused by the EITC schedule. Furthermore, since evidence suggests that a decline in investment income translates to an actual decline in saving, the tax credit schedule design is questionable as it originally intends to encourage low-income households to save more.

This paper's approach looks similar to that of Weber (2016)'s study, but it looks into the subject from a different perspective. I analyze a different type of EITC reform in a different timeline using different data and methodology⁸. Particularly, I examine the on-the-margin effect of a change in the tax credit threshold on households' investment income, using CPS household-level data from 2005 to 2014. In addition, one innovative aspect of this study is that its mainline regression model controls for the interactions between EITC recipients and other welfare programs such as Medicaid, SNAP, and TANF. Weber (2016)'s analysis assumes that these welfare benefits do not matter since individuals around the phase-out region are often not eligible for those benefits anyway⁹. In addition, the author indicates that these programs do not vary by number of dependents in the households and encourage saving, in contrast to EITC. Nonetheless, Weber (2016) does not test the assumption using evidence from Individual Public Use Tax Files. I, on the other hand, believe that these welfare programs can matter to some extent because a household who receives welfare benefits other than tax credit may be less responsive to a policy

⁸ Weber (2016) conducts research using Individual Public Use Tax Files for 1988-2006. The EITC expansion used in the research is the change in tax rate in 1996.

⁹ This phase-out region is also known as the second tax kink where bunching happens, theoretically (see Notation 15 for more detail). My analysis, alternatively, does not specifically look at this region. Therefore, controlling for welfare benefits on all recipients across the whole EITC schedule may be important.

that involves changes in the tax credit such as an EITC threshold expansion. Wagner (2011) implies that individuals who receive welfare benefits tend to hesitate to save EITC dollars because they do not want their other welfare amounts to be reduced. My paper therefore also examines whether if the interactions between EITC recipients and cash-transfer welfare programs matter and thus affect their saving behaviors. Another different strategy that this study implements is that I conduct an event study analysis in the hopes of purging out time-invariant selection bias since it acts as a time fixed effect. The implementation of this approach can help investigate the dynamic effects of the policy on the targeted group's investment income level.

Data

This paper analyzes the effect of the 2009 EITC threshold expansion policy on households' saving behaviors using Current Population Survey (CPS) household-level data from 2005 to 2014. Since the raw data are originally microdata (at individual level), I use CPS household serial ID to collapse them into a household-level data set for further analysis.

I exclude households with no children from the sample since these families usually have much less wage income and investment income compared to those with children, and thus including these families in the model may largely skew the results. And for simplicity, this analysis also does not include households with 4 or more children since these families only take up a very small portion of the sample¹⁰. I also remove households with investment income above \$3,100 from the analysis due to the 2009 EITC's eligibility threshold¹¹. Doing so also helps

¹⁰ Out of 1.9 million observations from the original collapsed data, only about 62,000 are households with 4 or more children – which accounts for only about 3 percent of the sample.

¹¹ IRS publication 596 category No. 15173A “Earned Income Credit (EIC) For use in preparing 2009 Returns”.

avoid upward bias on the results caused by households with much higher investment income levels. Additionally, the sample does not include households with negative investment income because this small number of outliers can induce downward bias on the results¹². I exclude households that earn above \$47,000 wage and salary income annually to develop an upper bound slightly below the end of the phase-out region (at \$48,279 if married filing jointly) where EITC is zero⁵. The reason for such an upper bound is that, at that level, the amount of tax credit is small but may still be noticeable enough and worth claiming. Lastly, I exclude households who do not earn any wage income since they are not eligible for the tax credit and thus are irrelevant to the analysis.

The dependent variable used in this analysis is investment income, measured in dollars. For the EITC variable, I measure it in both dollars term and reciprocity status. Other relevant variables include age, race, and the reciprocity status of TANF, SNAP, Medicaid, and unemployment benefits. A household who receives welfare benefits other than tax credit may be less responsive to a policy that involves changes in the tax credit such as an EITC threshold expansion. An average household in this sample has about \$176 of annual investment income and about \$329 of EITC¹³. A considerable portion of households (about 19.8 percent of the sample) receive EITC benefits, but not many of them are recipients of TANF, SNAP, Medicaid, and unemployment benefits¹⁴.

¹² Out of 1.9 million observations from the original collapsed data, only about 12,000 are households with negative investment income – which accounts for less than 1 percent of the sample.

¹³ To avoid any confusion, note that the EITC and investment income values for any household in table I are the sum amounts of these categories from all individuals in the same household (who are coded the same CPS serial household ID). Therefore, the maximum value of *household* investment income goes up to \$6,158 while I had previously excluded *individuals* with investment income lower than \$3,100.

¹⁴ In the sample, about 2% of households receive TANF benefits, about 4.7% receive Medicaid, about 4.9% receive SNAP, and about 5% receive unemployment benefits.

This paper involves a difference-in-differences framework. Under this approach, it is important to compare the descriptive statistics of both the treatment (3-children households) and control (2-children households) groups to determine whether systematic differences exist among the specified variables between these 2 groups. To proceed with the comparison, I decided to execute one last step of data cleaning: excluding households with only 1 child from the sample. Consequently, this step reduces the number of observations in the analyzed sample to 77,115. I expect this step to also help reduce the selection bias in the results since the characteristics of families with only 1 child can be systematically different from those of families with multiple children.

Henceforth, Table A1¹⁵ summarizes and compares the average values of the listed variables. There are about 22,000 families that have 3 children in the sample, whereas the number of families with 2 children is more than 55,000. But regardless of the difference in the sample sizes between the 2 groups, the data show that these groups of families both earn about the same level of wage-and-salary income while sharing similar age and race profiles. I find these similar statistics promising since they suggest that 2-children households could potentially provide a valid control group to 3-children households. It is also notable that, on average, the treatment group has a larger portion of families who receive benefits from welfare programs (including EITC) and unemployment assistance. In addition, even though an average family with 3 children receives roughly 28 percent more EITC dollars than a family with 2 children, its investment income level is almost 20 percent lower than that of its counterpart. While this relationship potentially suggests inverse effects of the tax credit on savings, it can also mean that,

¹⁵ See Appendix.

on average, families with more children simply have less opportunity to put money into savings accounts.

Methodology

1. Estimate the Reform's Effects on Savings Using Investment Income

First and foremost, in this paper I use investment income as an indicator for savings. In general, I believe that households who invest or care about investing are likely to have savings accounts and strive towards better financial stability. Weber (2016) indicates that about 18 percent of low-income households who claim EITC have some dividend and interest income. Additionally, Wagner (2011) studies EITC and financial capability among Native American households and finds that EITC recipients who have access to financial education are more likely to save.

Furthermore, for EITC-eligible households whose investment income levels are low (must be under \$3,100 in 2009), it is unlikely that they have substantial income sources from rent or dividend payments¹⁶, and thus I expect a large portion of investment income of these households to come from their savings accounts. Overall, I assume that **the effect of the policy on investment income of the treatment group primarily comes from the changes in savings.** This assumption is vital and thus would determine the policy implications of the results of this paper. Since CPS data do not provide a precise measurement of investment income, I assume that investment income of EITC-eligible households is equivalent to the accrued interest from

¹⁶ Since there is an upper bound for investment income, EITC-eligible households have lower levels of investment income. In other words, if the investment income amount exceeds the limit, the households will lose all the tax credits. On top of that, it is abnormal (if not impossible) for households that receive rent and dividend payments from stock and bonds to be eligible for EITC anyway.

savings accounts plus dividend and rent payments (if any). This calculation is consistent with the definition of investment income by American Community Survey (ACS)¹⁷.

Furthermore, it is important to note that using investment income as a proxy for savings can lead to biased results because families can also have other unreported sources of savings such as cash. A household could choose to “misreport” investment income by hiding a portion of it in the form of cash in order to stay below the cap and still be eligible for the tax credit. There is a hypothetical scenario where a household primarily saves its money as cash, meaning that the savings account only shows a portion of the actual overall savings. In this case, investment income would highly underestimate savings, and thus the measurements would become inaccurate. Therefore, for the results of this study to be valid, they must also assume that EITC-eligible families choose to put every dollar of savings into their savings accounts while not having any other sources of savings. Nevertheless, such a possibility is highly unlikely since most households in the analyzed sample have investment income levels substantially below the \$3,100 threshold¹⁸. The incentives for misreporting would have been much more significant and worth considering if the reported investment income levels were near the threshold.

2. Event Study Analysis

First, I conduct an event study to compare the pre- and post-reform effects between 3-children and 2-children families. This framework assumes that there are no systematic changes over time other than the treatment. In other words, the event study eliminates time-invariant selection bias and acts as a time fixed effect, controlling for the unobserved heterogeneity in

¹⁷ American Community Survey database imply that investment income is the sum of interest income, rent income, and dividend income.

¹⁸ In fact, according to Table I, the average investment income for these households is about \$150/year – which is more than 20 times less than how much they are allowed to invest while keeping tax-credit eligibility.

investment income across households. With that assumption in mind, I observe the change in the post- vs. pre-reform investment income levels between the 2 groups over the selected time period from 2005 to 2014. Equation (1), hence, formally expresses the observed change as follows:

$$(1) Y_{i,t} = \alpha + \gamma \sum_{t=-4}^5 (t \neq -1) (\mathit{Treat} \times \mathit{Period})_{i,t} + \beta \mathit{Treat}_i + \delta \sum_{t=-4}^5 (t \neq -1) \mathit{Period}_t + \varepsilon_{i,t};$$

where for household *i* at period *t*, *Y* denotes the amount of investment income, “*Treat*” denotes the treatment group (3-children families) indicator, “*Period*” denotes the event period (where the treatment year 2009 is labeled period “0” and so forth)¹⁹, and “*Treat x Period*” denotes the interaction term between the treatment group indicator and each of the corresponding event periods.

Specifically, the event study consists of 10 time periods (2005-2014) in which each period is a calendar year. The treatment year is when the EITC reform happens – year 2009, hence it is denoted as period 0. Hereafter, the period is equal to the difference between the corresponding year and the treatment year, creating a range from -4 to 5 (e.g. if the year is 2006, then it is equal to period -3). Using the regression model as expressed by equation (1), I plot the results of the sets of γ coefficients over these 10 event periods alongside their corresponding confidence intervals (except for year 2008 – period “-1”).

I omit year 2008 – the period right before the treatment year – from the plot because the difference in the coefficients will then show the differential evolution of savings between the treatment group and the control group relative to their difference in the period immediately prior

¹⁹ The subsequent paragraph will discuss these event periods in detail.

to the reform. In other words, the set of coefficients for all the event years (excluding the year before the EITC reform) would indicate the dynamic changes in the reform's effect on the treatment group's investment income relative to that of the control group. Since the analysis assumes that the reform is the only exogenous event over the time period, I will set the pre-treatment year's γ coefficient²⁰ to be zero and expect the differences in savings between the treatment and control groups to be insignificant prior to period "0" – i.e., year 2009.

One advantage of the event study is that it tests whether the parallel assumption is met, meaning no statistical differences in the outcome variable between treatment and control groups prior to the treatment year. Meanwhile, the results will also help investigate the dynamic effects of the policy on each group's investment income – i.e. whether the changes in investment income of households alter in sign and magnitude over time. Overall, the event study's results represent similar implications to those of a difference-in-differences model. I expect the average difference in the post-treatment period from the event study to be approximately the same as the interested coefficient's magnitude from the basic difference-in-differences regression – both try to compare the effects of the reform on investment income between households with 3 children and households with 2 children.

Nevertheless, the event study as well as the basic difference-in-differences regression lack the ability to control for household-specific characteristics and other omitted factors. The next part of this paper, hence, will expand further on the methodology in an attempt to implement a more rigorous mainline difference-in-differences model that takes into account these potential issues to help this investigation produce more robust results.

²⁰ As discussed above, this coefficient was originally omitted.

3. Difference-in-differences Framework & The Mainline Model

Aside from the event study, this paper estimates the effect of the 2009 EITC threshold expansion primarily using a difference-in-differences framework. Since I will be working with a panel dataset rather than a cross-sectional one, implementing a difference-in-differences model is preferable to using an ordinary least squares regression. Based on the nature of the 2009 EITC threshold expansion, I use households with 3 children as the treatment group and households with 2 children as the control group. I expect both groups of families to share similar traits on average, and thus the control group can be a good counterfactual to the treatment group.

Equation (I) formally represents the difference-in-differences regression model as follows:

$$(I) \quad Y_{it} = \alpha + \delta(Dx Post)_{i,t} \\ + \beta D_i + \theta Post_t + \gamma X_{it} + \varepsilon_{it}$$

where, for individual i at year t , Y denotes the amount of investment income – an indicator for saving for EITC-eligible households; D denotes the treatment policy – the EITC threshold expansion from the 2009 Obama’s ARRA stimulus package – where the treatment group is households with 3 children; $Post$ is a dummy variable which indicates whether the year is post-treatment (after 2009); and X denotes a vector of controls including demographic characteristics alongside other welfare programs and unemployment benefits.

The coefficient of interest, δ , corresponds to the interaction term between the treatment group indicator and the post-reform indicator. With β and θ “purging out” the systematic differences between the treatment group and control group with respect to household and time-variant characteristics, respectively, δ then shows the difference in the average post-treatment vs. pre-treatment investment income between the treatment group and the control group. In other

words, coefficient δ represents the difference-and-differences estimate of the EITC threshold expansion's effect on investment income. ε is an error term which indicates the unobserved factors that the model fails to capture. It is also important to note that a crude difference-in-differences estimate would neglect the vector of controls, leaving the regression model with only the treatment and post-treatment indicators alongside their interaction term.

Recall parameters β and θ being the coefficients that explain the differences in the dependent variable outcomes between 2 groups caused by household characteristics and time-variant variation, I can rewrite equation (I) in a reduced-form regression model that includes household and year fixed effects as follows:

$$(II) \quad \text{“Mainline Model”}: Y_{it} = \alpha + \delta(Dx Post)_{it} \\ + d_i + d_t + \gamma X_{it} + \varepsilon_{it}$$

While all the specified variables remain constant, this new equation instead “shifts” parameters β and θ into the household and year fixed effects terms d_i and d_t , respectively. For simplicity, I will be using this equation (II) as the mainline model hereafter. I prefer this model because the fixed effects sweep out the time-variant variation in investment income differences across households. Controlling for such a variation helps reduce selection bias in the difference-in-differences estimate because saving behaviors may alter over time and differ across households.

Again, the coefficient of interest remains as δ , since I am interested in examining the average difference in the post-treatment vs. pre-treatment investment income between the treatment group and the control group. I hope that overall, with this investigation, the development of the mainline model as discussed above will help produce more robust results

compared to a simple difference-in-differences estimate and will consequently lead us towards more accurate findings and practical policy implications.

4. Rationale & Drawbacks

This study implements a quasi-experimental approach that treats the 2009 EITC threshold expansion policy like a *treatment*, with 2009 as the treatment year. The pre-treatment period is 5 years, from 2004 to 2009. If the pre-treatment period was longer, some household characteristics, including the number of dependents, could systematically change. Nonetheless, the selected pre-treatment period is long enough to provide a reasonable time-trend comparison between the treatment group and the control group. The post-treatment period is also 5 years, from 2009 to 2014. The justification for such a restriction is to capture possible delays in households' saving adjustments with respect to the threshold change. Having a very long post-treatment period would increase the probability of including future economic shocks and other policies that alter households' saving behaviors, and thus the estimate of the average treatment effect will be biased.

Because the reform is not a randomly assigned treatment, potential manipulation in participation is a possibility. The difference-in-differences estimate of this paper does not control for this issue of heterogeneity in treatment effect, where households can choose whether to participate in EITC. An example of manipulation is bunching near the tax kinks where households can either conduct tax evasion or alter their working hours and saving behaviors near the slope changes of the EITC schedule²¹. In fact, Saez (2010) finds clear evidence of bunching

²¹ The EITC schedule has 2 tax kinks. The first kink is where marginal tax credit is zero – i.e., additional wage income no longer increases the tax credit amount but rather keeping it constant. The second kink is where marginal tax credit becomes negative – i.e., additional wage income now decreases tax credit until the tax credit amount becomes zero. Theoretically, bunching should happen near both the tax kinks, assuming households are *rational* and try to maximize their EITC while working the least amount.

around the first kink point of the tax credit schedule²². If manipulation exists, then the results on the treatment effect may involve selection bias.

Additionally, to examine whether the results are spurious, I must consider all possible confounding post-randomization factors and to show that the control group provides a valid counterfactual. It is also not possible to control for every policy change occurring simultaneously with the 2009 EITC threshold expansion. The 2009 ARRA stimulus package alone includes a tremendous number of programs that target multiple aspects of the economy. Therefore, in general, it is reasonable to expect the results of this paper to possess a lower degree of internal validity and thus indicate a less robust causal inference compared to that of an RCT due to the lack of randomness²³.

Results

1. Event Study Results

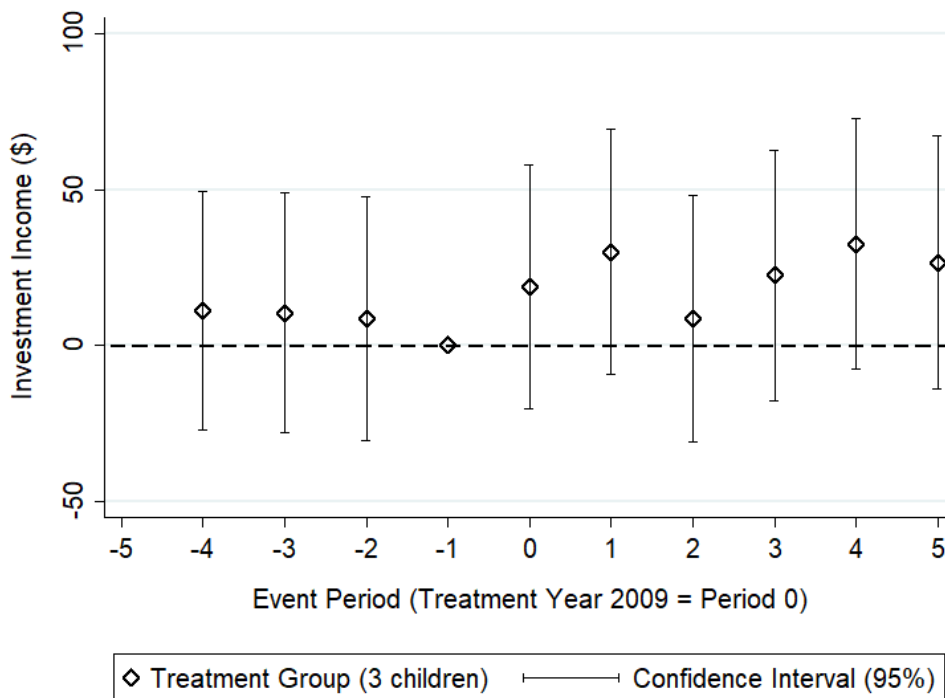
Figure A provides a visualization of the results from the event study. The pre-treatment coefficients are not statistically different from 0 as expected. These results indicate that the parallel trend assumption is met where savings among households in both groups are not systematically different on average. After 2009, when the EITC reform is implemented, there is no conclusive graphical evidence of the reform having significant effects on investment income of households with 3 children. The trend of savings slightly increases, but none of the

²² The author confirms that this bunching behavior concentrates solely among the self-employed recipients and suggests that tax evasion may account for the results.

²³ Recall the RCT example discussed in the previous page, because the treatment is randomly assigned and is unexpected, it would minimize selection bias and prevent manipulation of treatment participation across the observations and thus reduce spurious causality. Therefore, the lottery design to randomize changes in EITC net-of-tax return on eligible households is internally valid and thus provides prevailing evidence of the policy's effect on saving. Another advantage of this particular RCT design is that it already consists of a large sample, while a wide array of taxpayers' characteristics has already been observed and documented.

coefficients is statistically significant. On average, the difference in the change in investment income between the treatment group and the control group is about \$27 per year.

Figure A. Event Study Analysis: Overtime Differences in Savings between Treatment Group and Control Group (2005-2014)



Overall, the results from the event study analysis find little evidence of the 2009 EITC threshold expansion having any effects on savings of households with 3 children. I also do not find significant dynamic changes in the treatment group’s investment income relative to the period right before the reform. It is notable that the size of the average post-reform difference in savings between the two groups is large – about a fifth of the size of an average savings account of a family in the sample.

The event study’s model fails to take into account household-specific characteristics and other omitted factors. Thus, the mainline difference-in-differences regression is intended to address these limitations. Also, the results from several other difference-in-differences models will show the potential improvements that the mainline model brings to this investigation.

2. Difference-in-differences Results

I conduct 4 separate difference-in-differences models, with the last model being the mainline estimate as formally expressed by equation (II)²⁴. In summary, I find no evidence of the effect of the reform on savings, and the findings are inconsistent with the existing literature. The difference-in-differences results suggest no evidence of the 2009 EITC expansion having any effect on households with 3 children's saving behaviors, even after controlling for demographic characteristics and welfare reciprocity covariates while incorporating household and year fixed effects.

The first estimate is the basic difference-in-differences regression and does not control for the specified covariates. From this estimate, I find no statistical difference in the effects of the policy on investment income between households with 3 children and 2 children. Nonetheless, for these results to be valid, I need to test whether 2-children families share the same savings trend as 3-children families prior to the treatment. Figure B provides a visual illustration of the over-time investment income comparison between the 2 groups, while Table 1 summarizes the difference between the groups' average investment income levels.

According to Figure B and Table 1, the difference in the average pre- and post-policy differences between the 2 groups is about \$13.48. The saving's time trends of both groups look similar, especially during the pre-treatment period. This graphical evidence indicates that households with 2 children have indeed satisfied the identifying assumption of pre-treatment parallel trend, and thus this control group may provide a valid counterfactual to households with 3 children. Therefore, I expect the difference-in-differences results to be valid, meaning that the

²⁴ See Methodology.

pre- vs. post-policy difference in investment income between the 2 groups is potentially a consequence of something else rather than of the natural time trends. This study's regression models, henceforth, aim to investigate whether the difference caused by "something else" is the actual effect of the EITC reform in 2009.

Alternatively, column (1) from Table 2 formally represents the main results of the difference-in-differences estimate that controls for both coexisting post-reform and treatment-vs.-control differences but disregards the fixed effects and other covariates. The coefficient δ is not statistically significant and becomes slightly smaller than the first difference-in-differences estimate as shown in Table 1. With the same approach, column (2) indicates the results after controlling for a vector of covariates including age, race, and reciprocity status of SNAP, TANF, Medicaid, and unemployment benefits. The coefficient of interest δ from this estimate nonetheless remains statistically insignificant. But, these first 2 models do not take into account the time-variant variation in investment income differences across households, meaning that saving behaviors may alter over time and differ across households. Consequently, these estimates may involve selection bias that potentially impairs the results.

To control for the potential selection bias as discussed above, I include year and household fixed effects in the next 2 difference-in-differences estimates. Column (3) shows the results of the third difference-in-differences model with the fixed effects but no covariates. The coefficient of interest δ remains positive and statistically insignificant. Controlling for age, race, and reciprocity status of SNAP, TANF, Medicaid, and unemployment benefits, the magnitude of coefficient δ stays the same, and the estimate remains statistically insignificant. Even after taking

Figure B. Difference-in-differences: Overtime Savings between Treatment Group and Control Group (2005-2014)

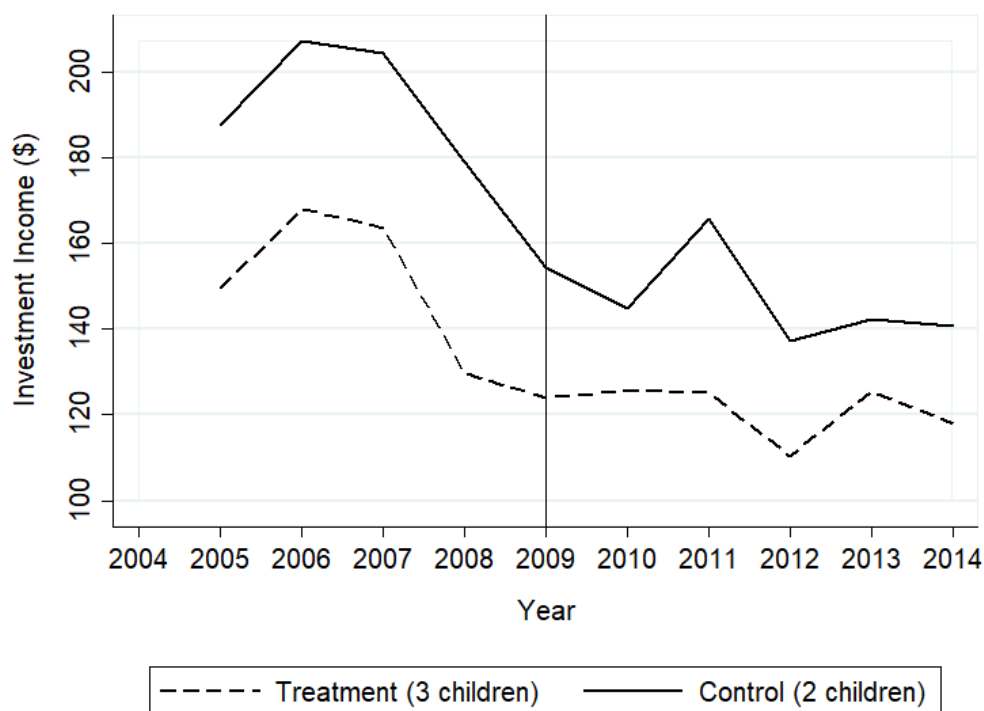


Table 1. Difference-in-differences: Average Savings between Treatment Group and Control Group (2005-2014)

| | (I) | (II) | (III) = (I) – (II) |
|--|--|--|--|
| Treatment year = 2009 | 3-children households – Treatment group (N = 21,846) | 2-children households – Control group (N = 55,269) | Treatment vs. Control Difference in the Period |
| Pre-Reform (2005-2009) – (X) | \$147.29 | \$186.92 | -\$39.63 |
| Post-Reform (2010-2014) – (Y) | \$121.63 | \$147.79 | -\$26.16 |
| Post- vs. Pre-Reform Difference = (Y) – (X) | -\$25.66 | -\$39.14 | \$13.48 (Diff-in-Diff) |

Table 2. Difference-in-differences (D.i.D.) Results – 4 Regression Models²⁵

| Explanatory Variables | Coefficient Estimates* | | | |
|--|------------------------|--------------------------------|------------------------------|---------------------------|
| | (1) D.i.D. | (2) D.i.D. With Controls | (3) With Fixed Effects | (4) Mainline D.i.D. |
| The Effect of 2009 EITC Threshold Expansion on Savings ²⁶ | 10.5 (8.37) | 11.39 (8.33) | 15.74 (14.55) | 15.4 (14.49) |
| Fixed effects | No | No | Yes | Yes |
| Vector of Controls ²⁷ | No | Yes | No | Yes |
| Observations | 77,115 | 77,115 | 77,115 | 77,115 |
| R ² | 0.002 | 0.01 | 0.002 | 0.01 |
| Number of serials | - | | 54,631 | 54,631 |

*Standard errors in parentheses

Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

into account the time variation in investment income across households by incorporating both year and household fixed effects in the mainline model, I still find no evidence of the policy’s impact on investment income of households with 3 children. Furthermore, the coefficient results indicate that the marginal change in investment income corresponding to the EITC threshold-expansion policy for the families in the treatment group is small – under \$16 per year. Thus, even if these results were statistically significant, their actual economic significance would be highly questionable.

In her study, Weber (2016) indicates that Medicaid, TANF, and SNAP are irrelevant because individuals around the phase-out region are often not eligible for these welfare benefits. However, this study does not specifically look at the second tax kink but rather estimates the

²⁵ I have also provided a detailed version of this result table (Table A2) in the Appendix.

²⁶ Equivalent to the Treatment Group and Post-Treatment Indicator’s Interaction.

²⁷ The control variables include age, race, and reciprocity status of Food Stamp, TANF, Medicaid, and unemployment benefits.

effect of the threshold expansion policy by observing all households across the whole EITC schedule. In this context, these welfare programs may matter because, for instance, a family receiving non-tax-credit benefits may have lower responsiveness to a policy that involves changes in the tax credit such as the EITC threshold expansion. Additionally, the author indicates that these programs do not vary by number of dependents in the households and encourage saving, in contrast to EITC, but she does not test the assumption using evidence from tax return data. Hence, I investigate whether the interactions between EITC recipients and other welfare programs are relevant. According to the mainline model's estimate, empirical evidence suggests that all of the reciprocity variables of welfare benefits matter as regards investment income, except for TANF. On average, a family that has been receiving SNAP, Medicaid, or unemployment benefits is expected to invest less money in their savings accounts, holding other things equal. The coefficients of these variables are all negative and statistically significant at the .01 level. These results make intuitive sense since we would expect a household already receiving welfare benefits to have less incentive to save. Nevertheless, the inclusion of reciprocity status of these transfer programs barely makes any difference on the magnitude of the policy's marginal effect on investment income for the treatment group.

Finally, households' demographic characteristics such as age and race seem to influence savings behavior. Both of these explanatory variables from the mainline model show positive coefficients that are statistically significant at the .01 level. An explanation for the coefficient results of the race variable is that it potentially "picks up" the effects on savings caused by other omitted factors, and these effects are noticeable: about \$61 per year – almost half the size of the average investment income amount. On the other hand, the positive coefficient results for the age variable could mean that as household members get older, they are more likely to put more

money in savings, either because their discount rates are expected to decrease over time²⁸ or because they have more incentives to save money²⁹.

These findings suggest that, in order to tackle true behavioral changes of targeted low-income households, a tax credit expansion policy should perhaps consider other ongoing welfare benefits and analyze how the interactions with these benefits may alter saving behaviors. It is important to note that studying the effect of EITC is not equivalent to capturing the full picture of behaviors among eligible households. As Blumenthal *et al.* (2005) imply, the EITC participation rate is only 30.6 to 39 percent; and because the program itself lacks the legal-obligatory component³⁰ while the targeted households generally have lower awareness of the program, these households are less prone to filing for the tax credit. On that subject, Chetty and Saez (2013) state: “the lack of knowledge about the EITC’s structure is striking given that the program parameters have been quite stable since 1996”. Consequently, one may concern whether EITC is less effective than traditional welfare programs in reaching those in need of governmental assistance.

Conclusions

I find no statistical evidence indicating that the 2009 EITC threshold expansion has any effects on savings, particularly investment income of the treatment group – households with 3 children. My paper investigates these effects by analyzing data on low- and moderate-income households with multiple (but under 4) dependents, meaning that the observations in the sample possess characteristics that closely resemble the policy’s target population (mostly households with 3

²⁸ Steinberg *et al.* (2009) find that younger individuals, on average, have significantly higher discount rates than their older counterparts. The authors imply that youngsters are “less concerned about the future and less likely to anticipate the consequences of their decisions”.

²⁹ Not only for their children but also for their future retirement plans.

³⁰ A household that earns lower income is not obligated to file the EITC, even if it qualifies for the benefit.

children). Though, since the EITC reform policy studied in this paper is just one of the legislative changes to the tax credit that have happened since the first EITC's enactment back in 1975³¹, it is hard to conclude that the findings remain valid in different contexts. For instance, I examine the 2009 policy's effect while observing household data over a 10-year time period from 2005 to 2014. Such an investigation, however, may not be generalized to the effect of another reform that will happen years from now, where households' characteristics alongside political influences and economic facets can dynamically change. Another example is that individuals living in areas with higher knowledge on EITC tend to obtain larger tax credit relative to those living in low-knowledge areas. Chetty *et al.* (2013)'s findings suggest that these differences in responses mainly arise from intensive-margin earnings increases in the phase-in region³². Therefore, a concern is whether the results can be generalized across households living in areas with different knowledge levels on EITC. Overall, the study's external validity is robust regarding the choice of target population but is questionable regarding multiple-context generalization.

Another potential drawback of this study is whether I have constructed the dependent variable (investment income) correctly. According to the IRS, the calculation of 2009 investment income includes taxable interest, tax-exempt interest, ordinary dividends, and capital gain distributions. Since CPS data do not include capital gains after 2008 (the year before the 2009 EITC threshold expansion), I am not able to include it in the investment income's calculation. Nevertheless, because the target population of this study is low- or mid-income households, I do not expect them to have significant income from capital gains, especially from stock and bonds.

³¹ Crandall-Hollick (2018) overviews the history of EITC and summarizes the key legislative changes (alongside some of the congressional intentions behind these changes). The author indicates that the program has been evolving through a series of legislative changes for 40 years.

³² Also known as the region before the first tax kink of the EITC schedule, where higher wage income levels bring in higher tax credits (up to the threshold amount at the kink point).

Moreover, I cannot find information on taxable interest or tax-exempt interest from the CPS database. With the current data availability status, I believe that I have come up with the best attempt in constructing an estimate for investment income by following the definition by ACS.

I designed this paper with a quasi-experimental approach to examine the effect of EITC on savings, controlling for an array of covariates such as demographic characteristics and welfare benefits. Even though I have endeavored to develop the methods and model estimates judiciously, my results and implications still undoubtedly encounter limitations. Henceforth, I hope that future studies will be able to analyze the subject using more comprehensive data while furtherly improving the model. In order to achieve more robust estimates with vigorous causal inferences on the policy's effect, researchers should also consider conducting and analyzing experiments with random components (if RCTs are not feasible) to attain higher degrees of internal validity. Furthermore, identifying optimal policymaking through studying behavioral responses is by no means a comprehensive approach. As Duflo *et al.* (2006) suggest, future studies should also explore social elements that influence these behavioral changes and investigate how policymakers explain and publicize the equivalent economic incentives that elicit such responses.

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Appendix

Table A1. Mean Comparisons: Treatment Group vs. Control Group* (2005-2014)

| | 3-children households - Treatment (N = 21,846) | 2-children households - Control (N = 55,269) |
|---|--|--|
| | Mean | Mean |
| Reciency variables** | | |
| TANF | .0031 (.055) | .0017 (.041) |
| Medicaid | .064 (.245) | .049 (.215) |
| SNAP | .081 (.273) | .047 (.212) |
| EITC | .242 (.428) | .206 (.404) |
| Other variables | | |
| Family income | \$39,272.76 (14,859.15) | \$40,022.41 (15,353.48) |
| Investment income | \$134.91 (539.63) | \$167.87 (577.46) |
| EITC amount | \$530.78 (1,143.22) | \$383.29 (923.38) |
| Age of the oldest family member (years) | 40.21 (8.09) | 40.74 (8.93) |
| At least one member who is white (1 = Yes, 0 = No) | .815 (.389) | .821 (.383) |

*Standard deviations in parentheses under mean values

**For all transfer programs' reciency dummy variables, 1 = Yes, 0 = No

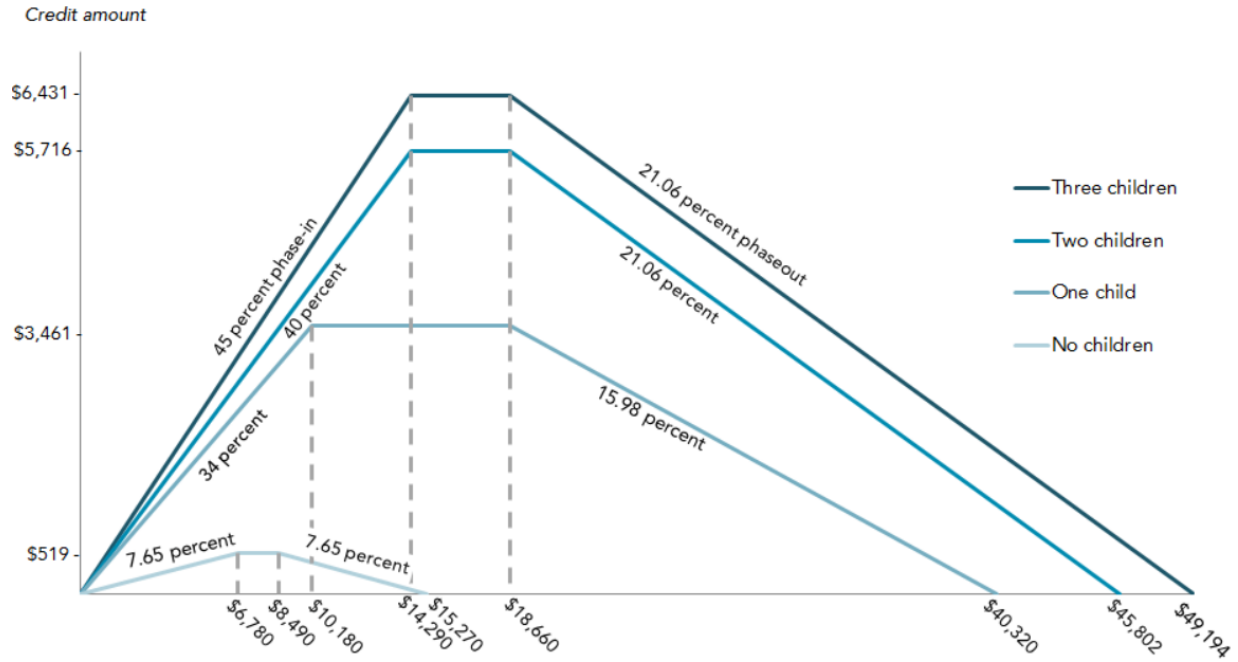
Table A2. Detailed Difference-in-differences (D.i.D.) Results – 4 Regression Models

| Explanatory Variables | Coefficient Estimates* | | | |
|------------------------|------------------------|--------------------------------|------------------------------|---------------------------|
| | (1) D.i.D. | (2) D.i.D. With Controls | (3) With Fixed Effects | (4) Mainline D.i.D. |
| Treat*Post | 10.5 (8.37) | 11.39 (8.33) | 15.74 (14.55) | 15.4 (14.49) |
| Treatment (3 children) | -33.68*** (6.33) | -29.80*** (6.31) | - | - |
| Post | -46.86*** (5.39) | -44.58*** (5.39) | - | - |
| Medicare | - | -61.61*** (9.49) | - | -62.32*** (17.96) |
| TANF | - | 11.25 (45.62) | - | -41.9 (87.82) |
| SNAP | - | -74.30*** (9.17) | - | -82.09*** (16.95) |
| Unemployment benefits | - | -3.43 (9.27) | - | -29.91*** (16.85) |
| Age | - | 4.51*** (0.24) | - | 3.957*** (0.438) |
| White | - | 61.88*** (11.49) | - | 60.98*** (10.29) |
| Fixed effects | No | No | Yes | Yes |
| Observations | 77,115 | 77,115 | 77,115 | 77,115 |
| R ² | 0.002 | 0.01 | 0.002 | 0.01 |
| Number of serials | - | - | 54,631 | 54,631 |

*Standard errors in parentheses

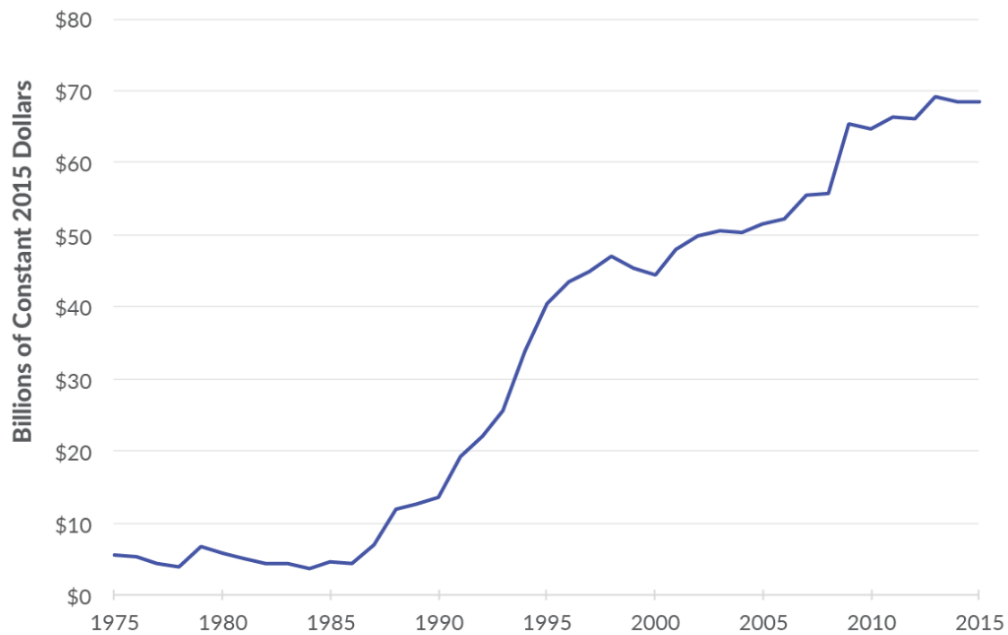
Significance levels *** p < 0.01, ** p < 0.05, * p < 0.1

Figure C. Example of an EITC Schedule: The 2018 EITC Schedule



Source: Urban-Brookings Tax Policy Center (2018). Internal Revenue Procedure 2018-18, Internal Revenue Service.
Notes: Assumes all income comes from earnings. Amounts are for taxpayers filing a single or head-of-household tax return. For married couples filing a joint tax return, the credit begins to phase out at income \$5,690 higher than shown.

Figure D. Overtime Growth of EITC Spending (1975-2015)



Source: Gene Falk and Margot L. Crandall-Hollick, "The Earned Income Tax Credit (EITC): An Overview."