The Effect of the Earned Income Tax Credit on Household Expenditures for Vulnerable Households*

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Abstract

Prior literature has established clear short- and long-run benefits of the Earned Income Tax Credit (EITC), but the mechanism behind these effects is unclear. This paper provides evidence that these benefits occur through increasing nondurable expenditures. I examine the impact of the EITC on the nondurable expenditures of single female headed households. Using longitudinal data with a dynamic difference-in-differences design and a pooled estimation model, I comprehensively study all EITC policy changes over time. This includes the 1975 introduction, a currently understudied aspect of the program. This paper additionally estimates the marginal propensity to consume out of transfer income, providing new evidence on the elasticity of household expenditures to a large lump-sum transfer. I find that an increase of $1 of EITC benefits leads to $0.39 more of food expenditures, significantly higher than the proportion of disposable income typically spent on food. This research strengthens the literature on how public assistance changes the spending decisions of low-income households and furthers the research on a population not typically the focus of the household finance literature.

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1 Introduction

As one of the largest components of the public safety net in the United States, cash transfer programs have a wide reach to low-income households and impact on their well-being. The Earned Income Tax Credit (EITC) is of particular interest. It is the largest means-tested cash transfer program in the U.S, reaching over one in seven taxpayers. A large body of research has established significant short- and long-run benefits of cash transfer programs, including the EITC. These benefits include poverty reduction (Liebman 1998, Hoynes and Patel 2018), improvements in short- and long-run health (Evans and Garthwaite 2014, Hoynes et al. 2015), and increases in educational attainment (Bastian and Michelmore 2018, Chetty et al. 2011). Some of these studies have found that the impacts of childhood exposure to the EITC persist well into adulthood. Far less is understood about how these benefits are acheived. I focus on how EITC receipt changes household expenditures, which helps unpack the mechanism through which the program might generate the considerable benefits that researchers have documented. Learning more about the direct financial consequences of EITC receipt will provide context for the mechanisms that lead to those long-lasting outcomes.

This paper will estimate the effect of a conditional cash transfer on the economic well-being of vulnerable households. This is the first paper to systematically look at the impact of the EITC on nondurable expenditures. Specifically, I examine the impact of the EITC on the food expenditures of single female headed households. The key outcome variable is household food expenditures, which can be considered a good proxy for household nondurable spending. Understanding the spending response to EITC receipt is fundamental to decomposing the scope and reach of the program's benefits, and provide insights about the economic significance of household budget constraints. Providing an estimate for the amount of spending caused by increases

in EITC benefits can help determine how financial decisions are made after receiving a cash transfer. Furthermore, studying changes in expenditures of low-income households can increase our knowledge of spending decisions in this population. The household finance literature often misses lower income populations. For example, many papers study expenditures using credit card data, but low-income households are less likely to use credit cards or interact with the formal banking sector. This means they would be left out of the analysis. The bias is particularly important in the context of single females, as they tend to be the most economically vulnerable population and have different spending patterns than married families or single males.

I use the Panel Study of Income Dynamics (PSID) to examine the impact of EITC policy changes over time, including the program's 1975 introduction, which has been largely understudied. I utilize two complimentary empirical methods to find an estimate for how the EITC changes nondurable expenditures, each method filling in gaps that the other misses to provide a robust estimate of the outcome of interest. First, I estimate a pooled model, using exogenous variation in EITC benefits from changes in state and federal laws that impact policy generosity over the entire lifetime of the EITC. Using this approach, I estimate that an increase of $1.00 in EITC benefits leads to an additional $0.39 in food expenditures. This estimate is larger than typical estimates of the budget share of food for single female headed households. My results provide evidence that families spend more on food after receiving EITC benefits than before. One reason could be the liquidity of a large lump-sum cash transfer.

I additionally use this empirical method to calculate the Marginal Propensity to Consume (MPC) out of transfer income. There is wide heterogeneity in estimates of the MPC, depending on population, context, and type of income (Jappelli and Pistaferri 2020). Because there are few estimates of elasticities of this type for low-income, single women in a developed country, such as the United States, my estimate is an inherently important addition to the literature. Assessing

the MPC of food expenditures as a consequence of EITC policy changes will allow the results in this paper to be comparable to results in other contexts. I find an MPC of 0.44 among single women eligible to receive any benefits. This is similar in magnitude to my other estimates, and is also higher than other estimates of MPC in the literature in the most comparable contexts.

The second method is a dynamic difference-in-differences approach, which leverages the major changes in federal EITC policy to estimate the causal impact of an increase in EITC benefits on household expenditures. I find suggestive evidence of a increase in food expenditures following EITC benefit increases. I also establish that the size of the increase matters, as well as the broader economic conditions. Overall, both identification strategies are evidence that changes in EITC benefits have a large impact on household food expenditures. This has important implications for the short- and long-term health and well-being of children in single female headed households.

This research will contribute to the literature on how public assistance changes the spending decisions of single female headed households and the literature on women's household finance issues. Estimating the magnitude of the spending response can add to our understanding of the role of the EITC as a means-tested, conditional transfer. Focusing on single female headed households allows for a concentration on vulnerable households that are more sensitive to changes in food expenditures, allowing us to draw conclusions about the economic well-being of a group typically targeted by public safety net measures.

This paper adds to the limited body of household finance research focusing on women, and specifically single women. I additionally contribute to the comparatively sparse literature on the spending response to income shocks, lifetime expenditure profiles, and the impact of transfer programs on financial decision making for single women. A further benefit is to consider these spending responses as a result of a means-tested transfer program. As single women, particularly

single women with children, are most likely to be eligible for and require government assistance programs, it is imperative to know how receiving a cash transfer impacts outcomes for these types of families. Applying estimates calculated for the broader population to the context of low-income families with children will lead to inaccurate conclusions.

The rest of the paper is laid out as follows: section 2 describes the institutional setting and details about the EITC. Section 3 describes the prior literature, and section 4 details the dataset being used and the sample of interest. The empirical models are presented in section 5, and the results from these models are in section 6. Section 7 concludes.

2 Setting

The Earned Income Tax Credit was introduced in 1975 as a tax benefit targeted to low- and moderate-income working adults with children. It has become one of the most effective anti-poverty programs, having lifted around 7.5 million out of poverty in 2019 (U.S. Census Bureau 2020), and has become the most important part of the safety net for households with children (Bitler and Hoynes 2010). The program was created to serve as both cash assistance for families with children and to encourage work. The EITC was implemented as a refundable tax credit in order to link the benefit to wage income and as a means of offsetting Social Security taxes for low wage workers. It was additionally devised particularly as a means to reduce caseloads for the Aid to Families with Dependent Children (AFDC) program, an often discussed policy objective at that time, by incentivizing entry into the labor force.[1] The EITC has become one of the largest means-tested cash assistance programs and an integral part of the safety net for

[1] Additional details about the history and politics surrounding the introduction of the EITC can be found in Ventry (2000).

a large number of families. According to the Internal Revenue Service, as of December 2020, about 25 million families received about $62 billion of EITC refunds, with an average of $2,461 received per household and a maximum $6,660. That means more than 1 in 7 taxpayers received the EITC in 2020.

The EITC began as a sizable benefit and has grown to an increasing portion of a family's total budget. At its enactment in 1975, it provided a 10% subsidy on earned income for salaries up to $4000 (equivalent to about $19,000 in 2020 dollars), as shown in Figure 1a. The EITC has received several expansions over the years, both to generosity and eligibility thresholds. Figure 1b displays the EITC benefit schedule after the latest expansion in 2009, where, when compared to the 1975 benefit schedule in Figure 1a, it can be seen that both benefit amounts and eligible earnings thresholds have increased, and there have additionally been differential changes for families with different numbers of eligible children in the household. Figure 2 shows a household's real maximum federal EITC benefit from 1975 to 2019 based on the number of eligible children in the household. These expansions vary in size and targeted population and provide a wealth of variation to identify the causal impact of receiving a large conditional cash transfer.

The emergence of state-level EITCs beginning in the late 1980s add further variation and an additional expansion to an already sizable credit. Figures 3a through 3c picture the variation in EITC benefit amounts over time by state, varying by the number of EITC eligible children in the household. These figures show both differences in the year of introduction of state-level EITCs between states and variation in the magnitude of the benefits. The earliest was introduced in 1986 by Rhode Island, with other states following through the late 1980s and 1990s.

Evidence on the EITC has shown consistently high take-up among eligible households, leaving

a large population available to study the effects of the program benefits. In tax year 2017, the IRS and Census Bureau estimated that participation among the total eligible population was around 78%.[2] The EITC reaches households with and without children and those with a higher income threshold than other means-tested programs. Comparing this to two other major U.S. government assistance programs, the Temporary Assistance for Needy Families (TANF) program only reaches families with children, and the Supplemental Nutrition Assistance Program (SNAP), formerly Food Stamps, only reaches families with around half of the household income as EITC, plus has an additional asset test not present for the EITC.

EITC benefits are determined through a combination of labor income and demographic characteristics of the eligible household. Both eligibility thresholds and benefit amounts vary depending on the number of children and marital status of the family. At the inception of the credit, only families with children were qualified, regardless of marital status of the parents, as much of the focus at the time was on the welfare eligible population. Further expansions added an additional benefit for families with more than one child. In 1990, the credit was expanded based on family composition for the first time to give a larger benefit to families with 2 or more children compared to only one child. In 1993, there was a small credit introduced for childless workers, and, in 2001, differences in eligibility thresholds were established for married versus single households. Finally, in 2009, a larger benefit was added for families with 3 or more children. The details of these changes can be seen in Table 1 below, adapted from the Congressional Research Service (Crandall-Hollick 2018).

The structure of the EITC benefits has a phase-in and a phase-out region, based on earned income, devised in order to minimize work disincentives. A portion of the literature utilizes variation in these regions, often paired with estimating the exact tax incidence of a household.

[2] https://www.eitc.irs.gov/eitc-central/participation-rate/eitc-participation-rate-by-states

Chetty et al. (2013) is one example, and it provides more institutional details about the exact details of the phase-in, phase-out, and flat regions of the EITC. In this paper, I assume full EITC eligibility for a sample constructed to have a very high level of eligibility based on demographic characteristics. Further details are discussed in the Data section. This avoids several issues with imputing EITC eligibility and exact benefit levels, including measurement error in labor income, especially in a way that could be correlated with EITC eligibility or benefits, and endogeneity of income and imputed benefits with the outcome, food expenditures. Given the high take-up of EITC of around 80%[3] and the construction of the sample to a population most likely to be eligible for the EITC, the treatment assignment in this paper can be considered as an intent-to-treat.

3 Literature

Cash transfer programs are a cornerstone of economic research, both because of their policy relevance and for providing an ideal setting to test a range of economic theories. Not only do these programs provide a policy relevant setting to test economic theories, but understanding economic decision making in the context of these programs strengthens our understanding of household finances in low-income populations.

The EITC has been widely studied due to its large size and broad reach. It is a large portion of the federal safety net budget and can be a sizable amount of money for an eligible household. A large literature on the EITC has found a positive impact on labor supply (Eissa and Hoynes

[3]This estimate is based on a study from the IRS and Census Bureau for tax year 2016 (https://www.taxpolicycenter.org/briefing-book/do-all-people-eligible-eitc-participate). Estimates for earlier years of EITC include 1979 and 1984, of around 70% (Sholz 1990) and 1990, where estimates were between 80 to 86% (Sholz 1994). Another study from the IRS found that between 13 to 18% of eligible individuals did not file taxes, and therefore did not receive benefits (IRS 2002).

2006), income (Grogger 2003), and a wide range of children's short- and long-run outcomes, including health (Evans and Garthwait 2014) and education (Bastian and Michelmore 2018). A recent summary of the economic literature using the EITC policy changes as variation can be found in Hoynes (2019). The magnitude of the EITC refund, which has grown increasingly larger with each expansion, means that the tax credit can be a substantial portion of a family's income. This translates to a substantial impact on a variety of economic outcomes.

Changes in consumer spending are one of these economic outcomes. Understanding the spending response to receiving a lump sum EITC refund can provide insight into household finance issues and economic well-being of families who receive the EITC. There is a large literature establishing that consumption, and by proxy, expenditures, are the most accurate way to understand the welfare of low-income households (Deaton 1992). This research has been applied to survey measures of consumption for low-income households in the United States, finding that consumption measures well-being and hardship better than income (Meyer and Sullivan 2003).

A large literature exists on the relationship between changes in income and expenditures. Focusing on the EITC provides several unique angles to this research topic. First, the EITC is a conditional cash transfer. As we know from the research on labor supply responses to the EITC, there is a substantial influence of EITC benefits on entering the labor market, particularly for single mothers, the focus of this paper. Analysis on changes in spending using changes in EITC benefits has the ability to look at both the impact of a lump-sum payment and increases in wage income. Second, the EITC is means tested. This allows for important research to be done on a population that is not often the focus of the household finance literature. There are both theoretical reasons and empirical evidence that the spending patterns and financial decisions made by lower income populations differ significantly from higher income ones. Better understanding the consumption patterns and household budget constraints of these households

is important both theoretically and as a method to inform policy makers whose objectives are to alleviate economic constraints on lower income households.

Starting with early survey evidence that recipients earmark EITC refunds for specific large purchases (Smeeding et al. 2000) and paying down debt (Romich and Weisner 2000), there has followed a collection of research working to elucidate the response of different types of expenditures to EITC receipt. Past research on the spending response to the EITC has found changes in consumer spending for durables, especially vehicles and transportation spending (Goodman-Bacon and McGranahan 2008). There is also evidence of increased savings and lower debt holdings (Jones and Michelmore 2018). My research focuses on a different type of spending than these papers, focusing on nondurable expenditures. Nondurable expenditures are higher frequency purchases that are spread throughout the course of the year. This paper, instead of analyzing changes in expenditures in the months when EITC receipt is the highest, rather takes a longer-term view by considering annual total nondurable spending.

Another aspect of the literature on EITC and expenditures is the identification strategy used, which determines type of variation used and the interpretation of the results. A common method is to examine changes in expenditure patterns in the months when the EITC refunds are most commonly sent to households. The benefit of this method is the ability to look at spending at the monthly level and to better measure the types of large purchases that could be an immediate consequence of receiving the refund. As described in Barrow and McGranahan (2000), receiving the refund as a lump sum is conducive to making a big-ticket purchase right away. Another finding using differences in outcomes in the months when EITC benefits are most often paid out is in McGranahan and Schanzenbach (2013), which uses food diaries to establish that the types of food purchased are different in high-EITC months than in other months. The drawback of this method is an inability to capture the smaller and more spread-out nondurable expenditures,

such as a permanent or long-term increase in food spending. Using quasi-experimental changes in EITC policy parameters is a method more suited to answering those types of questions. There have been several large expansions in the federal EITC benefit amounts, including variation based on the number of children in the household and marital status. Additionally, there are state EITC benefits in many states, providing an additional source of variation. A typical strategy is to compare households who would be impacted by the EITC changes based on demographic characteristics (i.e. households with or without qualifying children) before and after the policy change, using a difference-in-differences approach. This does not allow for analysis at the monthly level, as in the previous method, but does allow for longer term analysis.

4 Data

My primary source of data is the Panel Study of Income Dynamics (PSID), a large, nationally representative survey panel spanning many decades. The PSID collects data annually from 1968 to 1997, and biannually from 1997 to the present. This covers a longer time period than datasets used in most previous research, which enables coverage of the introduction of the EITC in 1975. This event has been understudied, partly due to the lack of high-quality data in that time period. Another benefit of the PSID is its longitudinal structure. The same households are surveyed every year, and families are tracked throughout the entire span of the PSID. This means that families impacted by EITC policy changes can be followed before and after the change.

This paper estimates the impact of the EITC by choosing a demographic group with a high proportion of households who are both eligible for the EITC and also claim those EITC benefits. The empirical model assumes full eligibility and take-up in the sample population, in order to avoid a different set of constraints imposed by imputing eligibility based on income, the other

main identification strategy in this literature. In the context of this paper, imputing EITC eligibility using reported income is particularly problematic. The outcome variable, household food expenditures, is highly correlated with income. Predicting EITC eligibility using income, and then using that imputed eligibility, based on income, to estimate expenditures, highly correlated with income, introduces a source of endogeneity that will likely bias the results. Therefore, instead I choose a sample to avoids the need to impute eligibility.

The chosen sample is single female headed households in the PSID. A female head is defined as a woman who is unmarried and living without a spouse or cohabiting partner in the household. This captures single women and single mothers without an additional adult providing a source of income within the home. Single mothers are the largest proportion of the population who receives EITC benefits. In 2008, around 60% of all EITC recipients were single, with around 40% being single with children in the household (Athreya et al. 2010). My sample is further limited to have education levels less than a college degree, following the findings in Hoynes and Patel (2018) that approximately 68% of women with some college are eligible for EITC, with a sharp drop-off to 47% of women with a college degree being eligible, based on data from 1996. Finally, the sample contains only women between the ages of 24 to 48, which limits the population to women likely to have children at ages eligible for EITC benefits, be finished with educational attainment, and be at ages to be comparable to one another.

An additional reason for choosing this population is that single mothers are more likely to have incomes below the eligible threshold for EITC, and therefore be able to claim the EITC for a longer period of time than two-parent headed families. Single mothers are more likely to have longer spells of poverty, and less likely to emerge out of a spell of poverty (Stevens 1994). Overall, these characteristics of my sample establish that the sample has a high eligibility for the EITC, is more likely to remain eligible over time, and is the largest proportion of people

receiving EITC benefits.

In order to take advantage of the longitudinal nature of the PSID, I use a balanced panel of households five years before and after each large EITC policy change. In other words, the same families are followed throughout the event window, without attrition. Using the panel structure allows for a cleaner identification of the impact of EITC receipt on food expenditures. One reason is that there are many idiosyncrasies in food purchasing behavior, so using panel data allows me to examine within-household effects of EITC policy changes. The determinants of household spending decisions are often driven by unobservable characteristics that determine behavior, especially for food expenditures. Examining within-household variation eliminates the concern about heterogeneity in preferences for food consumption between households biasing the true results.

Another reason to prefer the panel sample is that the households entering into the cross sectional sample are going to be significantly different than those who remain in the sample throughout the panel. The major difference between the panel sample and the cross sectional sample is a result of changes in marital status. The most significant factor that eliminates women from panel sample is starting out as married and becoming divorced or becoming married within the event window. These women are significantly different, and limiting the analysis to women who remain single mothers is a more cleanly identified sample. Furthermore, there are significant changes in household resources that happen after a marital dissolution, which would bias the estimate downwards (Page and Stevens 2002). A woman with children who just became divorced would enter the treatment sample in the subsequent year, which would also be a year where her household take a large downwards hit. This is further explored in Appendix 1, which presents summary statistics about how key demographic parameters for each sample used in the paper, comparing the households who remain in the panel to those who would be in the cross section.

The sample was constructed as a balanced panel of female headed households most likely to be EITC eligible. As take-up of EITC tends to be high overall (see footnote 3) and especially high in this group of women single filers (Caputo 2010), this provides a good setting to estimate the intent-to-treat in this population. There are, however, some drawbacks that the panel structure imposes on the sample. Both the women and their children are, by definition, aging throughout the sample. Because there is a balanced panel, women are constrained to ages 24 through 48 throughout the entire sample, and therefore at least 24 in the first year of the panel and at most 48 in the last year. As an example, if women are 48 at their oldest in the sample, and the panel is 11 years total, women have to be 37 years or younger in the first year of the panel. One further implication of the age structure of the dataset could be on fertility, as there is a strong relationship between mother's age and fertility decisions. Examining Appendix 1 can give insight into how much these restrictions change the sample composition in the balanced panel, as it shows means of demographic characteristics over the 11 years of the panel with and without sample restrictions.[4]

A piece of the analysis in this paper uses the major expansions in the federal EITC policy, including the 1975 introduction. These changes are described in Table 1. The policy changes include the introduction of the EITC in 1975, and expansions in 1986, 1993, and 2009. One notable exception is the expansion in 1990. Due to missing outcome variables in 1988 and 1989, the fairly small dollar amount of the increase in maximum benefits in 1990,[5] and the proximity to the next policy change in 1993, this event has been left out of the analysis.

After the restrictions described, my sample includes between approximately 130 to 450 obser-

[4]The robustness checks include results using a sample that includes married couples and single men, where treatment status is assigned based on eligibility imputed using reported earnings. These results are discussed at the end of the paper and appear in Appendix 3.

[5]This increase was $330, or around $627 in terms of 2020 dollars.

vations per year, depending on the policy change being examined. Although the food expenditure variables are available for nearly every year in the dataset, there are several years in the PSID where the food spending questions were not asked. These years include 1973, 1988, and 1989 and are left out of the regressions as necessary. Additionally, after 1997, the PSID moved from being asked every year to every other year. This means that data is only available for odd numbered years starting in 1997 onward.

Supplementing the detailed look at the major federal policy changes in the EITC is a big-picture look at the effect of all federal and state variation in EITC benefit generosity. The exact amount of the total federal and state maximum benefit, using eligibility based on demographic characteristics of the household, is used over the entire time period of EITC availability, from 1975 through 2017. The details on benefit amount and eligibility at the federal level are from the Tax Policy Center at the Urban Institute and Brookings Institution and the state level details are from Komro et al. (2020).

Although one of my specifications treats EITC receipt as a binary outcome, the other specification assumes all eligible families in the sample are receiving the maximum possible state plus federal EITC benefit for their demographic group, state of residence, and year. This approach overestimates the amount of EITC income a household is receiving, on average, as most families who receive the EITC are not receiving the maximum possible benefit. Appendix 2 presents figures showing the distribution of earned incomes for households in the main sample used for estimation imposed over the EITC benefit structure for the latest expansion in 2009. This can help to understand how restrictive the assumption is that all households are at the flat part of the EITC benefit schedule, where they would be earning the maximum amount of benefits.

Using the maximum EITC benefit amount for the entire sample has some drawbacks compared

to imputing exact EITC benefit amounts based on reported earnings and family characteristics. First, because I am not using any earnings data to determine eligibility, there must be another way to identify a sample of households likely to be eligible for the EITC for my analysis. The considerations made to construct this sample are discussed in the data section below. There will be households mistakenly identified as receiving EITC, even though their income is too high to qualify them for benefits. This will bias the results downwards, as there will be households in the treatment group that are not receiving any additional income after the year of the policy change.

The key outcome of interest is household food expenditures, a variable that has been defined consistently throughout the PSID. In the early years of the data, food expenditures cannot be separated from Food Stamps spending. As a result, I include dollars spent using Food Stamps with all other measures of food expenditures for all years. Total food expenditures include food used at home, food eaten away from home, and food delivered to home, in addition to food purchased using Food Stamps. There are a small number of observations that have zero food spending through any channel and zero food spending using Food Stamps. This amounts to around a dozen observations throughout the study, so these households are dropped from the analysis.[6]

In using Food Stamps spending, one additional relationship to consider is between EITC receipt and Food Stamps usage. There are several dimensions on which these two programs interact. Although EITC benefits do not impact Food Stamp eligibility or benefit amounts, there are two channels through which they do impact Food Stamp receipt. First, the EITC was designed to have a strong work incentive, and increases in labor income impact Food Stamp

[6]Further analysis is done as robustness checks at the end of the paper to adjust total food expenditures based on the number of members of the household. Results can be found in Appendix 3

eligibility. Second, this same mechanism that incentivized labor force entry, particularly among single mothers, was intended to do so as a means of decreasing AFDC caseloads. As there is automatic eligibility for Food Stamps given welfare participation in many states, leaving welfare could lead to the end of Food Stamp benefits for some families. Using the large reductions in AFDC caseloads due to restructuring of the welfare program in the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, Zedlewski and Brauner (1999) estimate that 62% of households who left AFDC also left the Food Stamp Program, of whom around 50% were likely still eligible for Food Stamps. They estimate a similarly high proportion for families under 50% of the poverty line. If this holds true, especially in the second case, we would expect the results presented in this paper to be biased downwards, as families impacted by EITC policy changes would also be losing Food Stamps benefits. Mikelson and Lerman (2004) examine the interaction between EITC and the Food Stamp Program directly and find both positive and negative impacts of the EITC on Food Stamp receipt in the mid-1990s depending on the empirical model and specification, but find somewhat more evidence that there is a negative relationship between the two.

Additional data used to supplement the analysis include state employment numbers from the Bureau of Economic Analysis's Regional Economic Information System and state population from the U.S. Census Bureau, used to calculate each state's employment-to-population ratio. These data are used as controls in robustness checks. All food expenditures are deflated using the consumer price index (CPI) food and beverage series and EITC benefit amounts are deflated using the base CPI series, both from the Bureau of Labor Statistics. The food and beverage CPI more accurately captures fluctuations in food prices, which tend to be more volatile than all prices on average.

5 Empirical Model

In order to estimate the causal impact of receiving EITC benefits on household food expenditures, I use two separate estimation strategies that complement each other to provide a robust set of results. I begin with a comprehensive look at the history of the EITC, considering both federal and state aspects of the program. This approach leverages all of the policy variation throughout the lifetime of the EITC to estimate the impact of the change in the dollar amount of benefits on the dollar amount of food expenditures. This allows me to estimate the proportion of the total budget spent on food expenditures, which give an idea about if the EITC is significantly impacting the food consumption in the low-income households considered in this paper.

I use the entire period of EITC exposure in the dataset in a single regression, from 1975 through 2018. The sample in this estimation is single female heads of households with some college or less education between the ages of 24 to 48. Each household is assumed to be receiving the maximum possible amount of state plus federal EITC benefits based on the year, state of residence, and demographic composition of the household. This assumption avoids issues of endogeneity from using household income to estimate actual EITC benefits, then using that estimate in the regression to estimate food expenditures. Using maximum possible benefits available to a family based on their demographics captures exogenous policy variation. Because using maximum benefits for a household is an overestimate in aggregate, the overall findings will underestimate the true effect.

The equation below is able to flexibly estimate the change in total food expenditures as a result of a change in EITC benefits over the entire period of close to forty years of EITC variation. The following regression is presented as a straightforward analogue to the equation in the second part of the paper. The empirical model for this pooled strategy is as follows:

$$y_{hdst} = \beta \cdot MaxEITC_{dst} + \psi \cdot ifkids_{ht} + \gamma \cdot X_{ht} + \phi_t + \omega_s + \epsilon_{hdst} \quad (1)$$

The variable $MaxEITC$ is the maximum possible state plus federal EITC benefit for a family, based on their demographic composition, d, state of residence, s, and year, t. The demographic controls, X, in this equation are controls for race, age, and education level of the female head. The variable $ifkids$ is an indicator variable for the presence of any children under the age of 18 present in the household. Although there are multiple possible methods to control for presence of EITC eligible children in the household, this specification most closely matches the current literature, i.e. Schanzenbach and Strain (2020). Standard errors are clustered at the state level.[7] Although households in the PSID remain in the sample year after year, this model uses the PSID as a repeated cross section. The main identifying assumption that this threatens is there is an independent series of cross sections over time. The second set of results will be able to account for this, as they use a balanced panel and do not require this assumption.

This empirical strategy is additionally used to calculate the marginal propensity to consume as a result of expansions in EITC benefits for these households, with one distinct difference. In order to calculate the change in total household expenditures as a response to changes in maximum EITC benefits, I take the log of the outcome variable, y, and the measure of EITC benefits, $MaxEITC$, in equation (1). There are already no households with \$0 in food expenditures[8], but there are many households with \$0 in EITC benefits that serve as the control group in equation (1). In order to calculate MPC using logs, I only keep households that qualify for some EITC benefits.

[7] The identifying variation comes at the state by demographic group level, I cluster at the more aggregate level (state) for the most conservative estimate of the standard errors.

[8] Households with \$0 in total food expenditures are dropped. This amounts to 200 observations out of over 34,000.

I include three separate specifications for equation (1) due to a major change in Food Stamps policy in 1979 that substantively impacts the outcome variable in the regression. Prior to 1979, Food Stamps were required to be purchased in a type of "buy one, get one free" scheme, where the amount a family was required to purchase versus receive for free depended on their income and family composition. This detail, compounded by a quirk in the way the Food Stamps question was asked in the PSID, cause a large, sudden decline in the total dollars spent on Food Stamps in 1980[9], and therefore a large, sudden decline in total food expenditures. Furthermore, in the early years of the PSID, food expenditures using Food Stamps are impossible to separate from food expenditures using cash. It is impossible to tease out how much of this large decline is a consequence of the change in Food Stamp policy, and therefore a real change in family food expenditures, and how much is a result of the PSID questionnaire format. To avoid this issue altogether, I add two additional specifications which drop these earlier years. The first begins in 1980 and is the preferred specification, but an additional specification starting in 1981 is added as a robustness check to make sure that the specific year chosen does not make a significant difference in the estimation.

The second portion of analysis goes in depth into each significant change in federal laws impacting the EITC. Focusing on a single policy change at a time allows for the use of a more sophisticated econometric model, using modern techniques and taking advantage of the panel structure of the data. I use a dynamic difference-in-differences approach to study the changes in spending before and after each of the major federal EITC policy changes, comparing the single women affected and those not affected by the policy change. Using a difference-in-differences design paired with the natural experiments that arise from the expansions in the federal EITC maximum benefits is a commonly used identification strategy. Its strength is in its ability to

[9]Questions about expenditures in the PSID are asked about the previous year's expenditures, as explained earlier in the paper, so 1980 is the year when changes that happened in 1979 show up in the PSID.

estimate the impact of similar groups of people who are and are not impacted by the changes in the size of the credit. I add to this commonly used strategy by incorporating dynamic effects of the EITC policy change. Each policy change is examined separately, meaning there will be a separate estimate for the 1975 introduction and expansions in 1986, 1993, and 2009. As opposed to the first method, this design allows separate analysis for each year of policy change. The benefits are to better understand how the response differs throughout time, to reduce estimation bias by leveraging the longitudinal nature of the data, and to exploit the natural experiments in the federal EITC policy changes for a clean causal identification. The major trade-off is, however, smaller sample sizes, which introduces noise into the analysis and means it will be more difficult to obtain precise results. Because the point estimates will not be statistically significant, this set of results will provide a general pattern of expenditures after the introduction or expansion of the EITC. Furthermore, testing the robustness of this set of results to a number of different specifications can solidify the trust that these patterns are an accurate picture of spending patterns as a result of changes in the EITC.

For most years, this comparison is between single women without children and single mothers. Table 2 describes the exact comparison groups for each regression. In 2009, EITC benefits increased for families with 3 or more children and stayed the same for all others, so in 2009 the comparison is between single mothers with one or two children and single mothers with three or more children. In 1993, there were several changes depending on the number of children in the household. As can be seen in Figure 3 and Table 1, families with only one child received a substantial boost to the benefits, almost doubling the size, and the benefits for families with two or more children was additionally nearly doubled on top of that. 1993 was also the first year that a small credit was introduced for childless workers. Traditionally, in the literature, the comparison when studying the 1993 EITC expansion is between households with one child

and two or more children. Although both groups received an increase to their benefits, the increase for families with two or more children was significantly larger. Using this comparison in the difference-in-difference estimate has one major advantage over comparing childless adults to those with children. Families with one child compared to those with more are more likely to be similar in the underlying ways that could impact the outcome variable. Because all households with children were impacted by the expansion compared to households with no children, I am using both versions of defining the treatment. Examining households with zero versus one or more children will be more comparable to the rest of the results and those with one versus two or more children will be comparable to the rest of the literature. These leads to five different regressions, as laid out in Table 2.

The dynamic difference-in-differences approach differs from a typical difference-in-difference style equation by incorporating time-varying effects of the treatment. Instead of comparing the pre-trend to the post-trend, as in a classical difference-in-difference design, the changes in expenditures are considered for each year separately. The benefit of this is to be able to trace out the dynamics of the change in household expenditures, which could have theoretically different patterns depending on the assumptions we make about the underlying household budget constraints. One point we can visualize in this approach is whether the change in expenditures is persistent or temporary. The largest benefit arises because food expenditures are a continuing expense, and a household is making spending decisions on a monthly or biweekly basis. Compared to an outcome like labor supply, when the EITC changes, a mother would make the decision to change their labor supply once and their decision typically persists for a long time. Additionally, as single mothers have high EITC eligibility, this implies they tend to remain eligible for longer spells of time. This further implies there is a high likelihood they would be receiving additional EITC payments in each of the years post-treatment. For an outcome like labor supply, the change

in EITC policy would impact labor supply decisions one time—the year of the policy change. When considering food expenditures, each additional year of EITC receipt post-treatment would continue to impact expenditures. The specification used in this paper will more accurately allow the comparison of EITC impacts between the treatment and control groups in the period after the policy change.

There are several aspects of this empirical approach that take advantage of the strengths of the PSID data. First, I use individual fixed effects. Being able to focus on changes within households controls for the idiosyncratic nature of expenditures between households. The panel data allows for a more accurate assessment of the increases in expenditures for a family following an increase in the amount of EITC refunds. Focusing on within household variation over time eliminates the bias arising from unobserved heterogeneity between households, which is likely to be present for an outcome variable such as food expenditures. The next advantage of this data is that the PSID spans a much longer time period than most other surveys. The dataset begins before the introduction of the EITC in 1975 and continues through the present day. This allows for a robust examination of all of the EITC variation over time, and especially importantly allows analysis of the introduction of EITC. This is an additional angle to the study of the comparison between introducing a new source of income and increasing the amount families already were eligible for.

The dynamic difference-in-difference equation is the following:

$$y_{hst} = \sum_{k=-5, k\neq -1}^{5} \beta_k (year_{t+k} * ifkids_{ht}) + \sum_{k=-5, k\neq -1}^{5} \phi_k \cdot year_{t+k} + \psi \cdot ifkids_{ht} + \gamma \cdot x_{hst} + \omega_s + \xi_h + \epsilon_{hst} \tag{2}$$

Total food expenditures, y, are the outcome of interest for household h living in state s in year

t. Additional demographic controls, x, include dummies for the age of the female head. Unlike equation (1), controls for race and education level are not needed, as these variables remain unchanged over time for a person and equation (2) includes individual fixed effects. Looking within a balanced panel of households of women between the ages of 24 and 48 with some college or less education, the regressions include individual fixed effects to control for time-invariant unobserved differences that affect food expenditures between households. I include state fixed effects, ω, to account for the mobility between states within households, which happens often over the ten year time span of the estimation. We would be worried if the probability of moving to a new state was influenced by changes in the EITC, but I do not find evidence of that happening. Finally, there is the error term, ϵ. Standard errors are clustered at the individual level.

The group of households impacted by the policy change are in the treated group, indicated with the $ifkids$ variable. Table 2 describes the treatment and control group for each policy change. For 1975, 1986, and 1993, this is single females with children, and single females without children is the control group ($ifkids = 0$). In 2009, the maximum benefit changes for families with three or more children and stays the same for all other families, so the treatment group is the families with three or more eligible children and the control group is families with one or two children. For the 1993 change, I also explore the difference between families with two or more children (in this case $ifkids = 1$ refers to a dummy for if there are two or more children in the household) and families with only one child ($ifkids = 0$). This is because, as described above and as shown in Figure 3, the maximum benefit increased by more for families with two or more children than for those with only one child.

Although the bulk of the literature on fertility decisions in response to the EITC finds a lack of evidence that the EITC induces births or changes fertility in response to policy changes, if it in fact does, there could be an issue with treatment status (based on number of children in

household) being endogenous to the EITC policy changes. In Appendix 6, I assign treatment status based on the family's composition in the year of the policy change to avoid this issue. In this specification, treatment status is static. For example, for the 2009 EITC expansion, treatment status is assigned based on the number of children in the household in the year 2009 and there is no moving between treatment and control in any years. This avoids bias in the post-period if we are concerned about fertility decisions in response to the EITC policy changes, though there is little evidence empirically that this happens. In the figures of Appendix 6, we can see that because some households who would have been assigned to the control are more and more likely to have a child and become eligible for the EITC with each additional year, the treatment effect in the later years is biased downwards.

In a traditional difference-in-difference model, treatment status would always stay static over time. As there are dynamic considerations in this empirical model, a decision needs to be made about how to define treatment status. In equation (2), treatment status is defined based on the each individual year a household in the sample. Based on the number of children in the household, this means a family can move in and out of treatment over time. In Appendix 7, I consider how this has parallels to the emerging literature on staggered adoption in difference-in-difference methodology and how to use the insights from that literature here.

The dynamic effect is found in the year by treatment status interactions. These are included for five years before the policy change and five years after. The reference year is always the year prior to the policy change. For the 1975 introduction, the reference year is defined as the year 1975, because households would receive their first EITC refunds in 1976. The coefficients on the interaction terms describe the average difference in annual household food expenditures between the families impacted by the EITC change and families not impacted in each year prior to and following the policy change, compared to the year before the policy change.

6 Results

The first set of results include estimates from the pooled regression, using all of the variation caused by changes in the state and federal laws concerning the EITC. This broad view of the impact of the variation in the dollar amount of the benefits of the EITC on the amount of spending on food provides us with a large set of policy variation and large sample sizes to produce more precise estimates. The results are presented in Table 3, with my preferred specification in column (1).

These estimates show that an increase of $1.00 of EITC benefits leads to a statistically significant increase of $0.39 of total food expenditures. If the EITC were a pure income shock, this would mean that approximately 40% of benefits are being used for food spending, keeping in mind that because all families are assumed to receive the maximum amount of benefits, this is an underestimate of the true proportion of EITC benefits spent on food. In order to better understand the magnitude of this number, it can be compared to several other estimates taken from other literatures. First, a key perspective is the comparison to the typical budget share of food. The share of food expenditures out of a family's total budget tends to be a good indicator of the economic well-being of a family. Lower income households tend to have a larger share of the budget devoted to food. Paulin and Lee (2002) use data from the Consumer Expenditure Survey in 1998 to 1999 to estimate that single mothers spend around 17% of their total expenditures on food (at home and away from home combined). Lino (1990) breaks this down by type of single mother, and finds that never married single mothers spend around 20% of their budget on food at home, with divorced and widowed parents spending much less as a percentage of the total budget, at 14% and 12% respectively. For the general population, this estimate ranges from around 13.5% around 1975, the beginning of the EITC, and has trended downwards over time

to around 10% today (USDA ERS 2021).

The estimates of budget share of food found in the literature all fall well below the 39% found in this paper. This is evidence that the liquidity shock caused by an increase in the cash benefit from the EITC is purposely being diverted towards food expenditures. The fact that families are spending above the typical proportion of food in the budget suggests that they were significantly constrained in the amount they were able to spend on food before. This provides evidence that the EITC is substantially impacting the food resources of single mothers receiving benefits from the program, providing a mechanism by which the EITC impacts a host of other outcomes, from children's academic success (Bastian and Michelmore 2018) to health outcomes (Baughman 2012). Increased nutrition, especially in childhood and especially in food insecure homes, has the potential for massive benefits, lasting well into adulthood.

Column (2) of Table 3 provides a robustness check to determine whether changing the year where the data begins makes a significant impact on estimation. The difference between the estimates starting in 1980 versus 1981 are 0.011, and the two coefficients are not statistically significantly different from each other. Column (3) displays the results when all of the years of data are used. Using data starting in 1971 includes the large dip in food expenditures caused by changes in Food Stamps policy, as explained earlier. As expected, the results in column (3) are lower than the results in column (1). Further decomposition of the estimates of total food expenditures from 1971 through 1981 can be found in Appendix 8.

A parallel set of results with the calculations for MPC are presented in Table 4. These estimates follow the results shown in Table 3, but instead Table 4 shows the effect of a change in maximum EITC benefits on the change in total food expenditures, by taking the log of both variables. This allows us to see the MPC of food expenditures out of increases in EITC benefits,

which I estimate to be a statistically significant 0.443 in my preferred specification. This is a similar magnitude to the 0.39 found in Table 3. Both numbers imply that households are spending around 40% of their increased EITC benefits on food expenditures. There are not comparable estimates for MPC out of transfer income for single mothers in the United States, so this MPC is an important addition to the literature.

It is important to remember the labor supply effects of the EITC. The EITC additionally increases labor income for a subset of families receiving it, because of its work incentivizing design. These families additionally benefit from extra labor income. To get a sense of the average impact of EITC, we can run a version of equation (1) with earnings as the outcome variable instead of food expenditures. The results of this are reported in Table 5. We see an average increase of \$1.14 for every \$1.00 increase of EITC benefits. This average is misleading, however, as much of the movement in the labor supply decision as a result of changes in the EITC comes on the extensive margin.[10] The much larger change in earnings as a result of entering the labor force are averaged with the more modest change that would occur if on the intensive margin. This evidence gives us some background for how to interpret the final results.[11]

[10] See literature review section for discussion of the evidence on the impact of the EITC on the extensive and intensive margin

[11] Another way to estimate the impact of the EITC on expenditures is by using the natural experiment provided by the federal EITC expansions and using an identification strategy such as was described in equation (2). The results from these regressions are presented in Appendix 9. Overall, we see evidence consistent with other papers. Around 2009, there is no significant impact on earnings for single mothers, consistent with Bitler et al. (2017). In other expansions, we see an increase of around \$2500-\$5000 in pretax earnings, specifically in 1986 and when comparing single mothers with one or more children to single women with no children around 1993. When looking at the difference-in-difference estimation comparing single mothers with two or more children to one child, we see a decrease in earnings following the expansion of the EITC in 1993. This implies that the labor effect impacts all single women eligible for the EITC, and women with only one child are out-earning women with more children as a result of the policy change.

The results of the second specification described by equation (2) are presented in Table 6 and shown visually in graphs as described below. The figures shown graph the coefficients from the interaction terms in the dynamic difference-in-differences estimates. Therefore, the graphs show the average difference in expenditures between the treatment and the control group in each year compared to the reference year, one year before the policy change. All dollar amounts in the graphs and in the following analysis are real 2017 dollars, as 2017 is the last year of data used in the analysis.

The actual year that the policy changes were implemented differ from the years referred to in the text and the figures, as can be seen in Figures 4 to 8. The reference year, which is always the year prior to the first year that a household would be receiving the new, higher EITC benefits after a policy change, does not match the year prior to the year in the title of the law. The policy changes are always referred to by the year in which the legislation was passed, and therefore part of the official title of the law: 1975, 1986, 1990, 1993, and 2009. The law in 1990 was implemented in tax year 1991, and the law in 1993 was implemented in tax year 1994. Furthermore, the effect comes the year after the new policy is implemented. For example, the introduction of the EITC was implemented in 1975, but the benefits were received by households in the subsequent year, making 1976 the first year that household food expenditures would be impacted.

Due to the nature of survey data and human behavioral factors, the true effects are also likely to be spread among two years. This arises because the PSID asks respondents about their expenditures for the previous year, but respondents tend to have an established behavioral bias to report expenditures closer to their current month's spending (Hall and Mishkin 1982). Following the consensus in the literature (see Hoynes and Schanzenbach (2009) and Blundell and Pistaferri (2003) for prominent examples), I assume that reported answers are for the current year. The results in Figures 4 to 8 below provide evidence of heterogeneity in response behaviors.

In some cases, there is a pattern of a ramp up to the full effect size in the first two years after the policy change, with Figure 4 and Figure 7 displaying this most clearly. This is evidence that some households are responding with an accurate expenditure number for the previous year and others with an accurate number for the current year.

There are several conclusions we can draw from the information provided in Figures 4 to 8. First, the average increase in food expenditures following an increase in EITC benefits is higher than the proportion of food spending in the budget normally. Figure 9 presents the coefficients from a simple, static difference-in-difference estimation to do a back-of-the-envelope comparison to the average amount usually spent on food. The figure displays the estimates from a model similar to equation (2), but without single year interaction terms with the treatment indicator, and instead a single indicator for the post-policy change time period, as in a usual difference-in-difference regression. Figure 9 reports the coefficients as a percentage of the total change in maximum EITC benefits, in order to facilitate comparison to statistics on households' usual average food consumption. Estimates range between 19.9% to 78.3% of the total increase in EITC benefits, assuming that all households receive the maximum amount.

It is important to note again that the EITC also provides a strong work incentive and previous research has found a strong association with the EITC policy changes and increases in single women's labor supply, especially along the extensive margin. Therefore, the true increase in family income could be much higher than only the amount from the EITC due to increases in wage income. The way that these estimates are presented as percentages, however, does provide an easy comparison to the first set of estimates from equation (1). The coefficient there, 0.39, is roughly an average of the results presented in Figure 9. The entire range, starting from 19.9% falls above the estimates in the literature for single mother's food expenditure share of total expenditures, suggesting that families are more likely to use their EITC refund money

towards food expenditures. This fits with the fact that the women in the sample have low levels of education and are single mothers, which means they are more likely to be food insecure. Increasing food expenditures in food insecure families is a key policy goal.

Furthermore, we can gain some additional insights from the difference-in-difference coefficients presented as a percentage of the total EITC benefit change. 2009 and 1986 have the highest amounts of food spending as a proportion of the total amount of the EITC benefit increase. Both of these events fall during recessions, when low-income households are even more liquidity constrained than usual, and the EITC benefits might play a larger role in helping families manage expenses like food expenditures. In 2009, we are specifically comparing single mothers with 3 or more children to single mothers with one or two children. This provides evidence that single mothers with more children are spending significantly more of their expenditures on food.

The strength of this estimation strategy comes from the consistent pattern we see across events and in their robustness to many different specifications. Because of the small sample sizes and noisy estimates, the point estimates in these regressions should be taken as secondary to the more important findings of a consistent pattern of increases in total food expenditures after EITC receipt. Furthermore, I do a series of robustness checks and find that results show the same pattern regardless of the decisions made about details within each regression, including controls, different definitions of variables, and changes in measurement of the outcome.

Appendix 3 presents a number of alternative specifications and decisions that all yield similar results. These robustness checks include adding married couples and single men to the sample, no longer restricting the sample based on educational attainment, and instead assigning EITC eligibility based on income. Because a requirement of the balanced panel used in equation (2) is that the household remains eligible for the EITC for the length of the panel, the final sample

used in this estimation is majority single female headed households anyway. This is because single women tend to remain at low incomes for longer periods of time. The second check is adjusting total food expenditures for the number of people in the household. Doing this gives results that are nearly visually indistinguishable from the main results. Third, I account for the possibility of the EITC changing fertility decisions by assigning treatment status based on the number of children in the household in the year prior to the policy change. Because women are more and more likely to move from having zero children, therefore not being eligible for the EITC, to having at least one child as the years progress, the results become more biased at the end of the panel, but results still remain generally similar. The last robustness check is to add state trends, and to further interact state trends with number of children, as women with more children might be more sensitive to business cycle effects. As with the other checks, the results are nearly identical to the main speficiation presented in Figures 4 through 8. The consistency of the overall findings across specifications provides robust evidence of a large increase in food expenditures following expansions to EITC benefits.

Although the point estimates are not statistically significantly different from zero, they show a positive relationship between EITC receipt and additional food spending. These results are a rigorous and cleanly identified analysis of the impact of the EITC on household nondurable expenditures, but due to the small sample size, the point estimates are not as reliable as the overall trend. The first set of results, however, have a statistically significant point estimate for the overall, long-term relationship between food expenditures and EITC policy changes. Together, they provide strong evidence that the EITC significantly impacts average annual food expenditures and comprises a much larger share of the budget than food spending was of the budget before receiving the EITC.

7 Conclusions

Overall, I show that changes in EITC policy that increase benefit amounts lead to a significant increase in household nondurable expenditures for single women. The proportion of the increased benefits spent on food expenditures exceeds calculations of the budget share of food for similar populations in the rest of the literature, indicating that recipients are spending more of their EITC benefits on food than the amount they spend on food in their typical budget. The sample studied in this paper is at high risk of food insecurity, and for children of single mothers, increases in food expenditures can have important implications for the short- and long-term health and well-being. Previous literature has established a clear link between receiving the EITC and a wide range of positive outcomes, and this paper provides a story for the mechanism that causes those outcomes.

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Table 1

	P.L. 94-12 1975	P.L. 99-514 1986	P.L.101-508 1990	P.L. 103-66 1993	P.L. 111-5 2009
Year Enacted	1975	1987	1991	1994	2009
Maximum Real Benefit (2020$)	1,924	1,862	one, 2265 two or more, 2347	none, 534 one, 3,559 two or more, 4,415	0-2, same three or more, 6,824
Change in Maximum Real Benefit from previous year	1,924	609	one, 378 two or more, 460	none, 534 one, 991 two or more, 1,709	three or more, 1,025
Credit adjusted annually for inflation		Y	Y	Y	Y

Table 2

Policy Change	Treatment	Control
1975 Introduction	1+ children	0 children
1986 Expansion	1+ children	0 children
1993 Expansion	1+ children	0 children
1993 Expansion	2+ children	1 child
2009 Expansion	3+ children	1-2 children

Table 3: Coefficients for the Pooled Regression on Total Food Expenditures

	(1)	(2)	(3)
Federal and state EITC	0.394***	0.405***	0.258***
	(0.046)	(0.045)	(0.045)
If EITC eligible children in household	1269.707***	1218.586***	1812.136***
	(163.243)	(161.137)	(142.601)
Race: Black	-450.069**	-462.602**	-342.378*
	(189.553)	(187.355)	(200.396)
Race: other	453.230***	448.533***	450.134***
	(118.203)	(122.462)	(125.754)
HS graduate	-179.424	-176.951	-156.221
	(119.774)	(119.445)	(103.044)
Some college	82.722	81.372	106.479
	(100.928)	(98.072)	(110.970)
Observations	26336	25609	30702
R^2	0.083	0.082	0.109
First year of data	1980	1981	1971

Notes: Standard errors in parentheses. Errors clustered at the person level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Coefficients for the Pooled Regression on Log of Total Food Expenditures

	(1)	(2)	(3)
Log of Max Fed. + State EITC benefits	0.443***	0.444***	0.440***
	(0.033)	(0.033)	(0.032)
If EITC eligible children in household	-0.605***	-0.606***	-0.600***
	(0.072)	(0.072)	(0.071)
Race: Black	-0.064**	-0.065**	-0.053**
	(0.028)	(0.028)	(0.026)
Race: other	0.053*	0.052*	0.048
	(0.031)	(0.031)	(0.031)
HS graduate	-0.012	-0.011	-0.016
	(0.016)	(0.016)	(0.015)
Some college	0.028**	0.029**	0.024**
	(0.011)	(0.012)	(0.012)
Observations	22375	21847	24379
R^2	0.084	0.084	0.099
First year of data	1980	1981	1971

Notes: Sample is all single female headed households eligible for at least $1 of EITC benefits. Standard errors in parentheses. Errors clustered at the person level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Coefficients for the Pooled Regression of EITC benefits on Labor Earnings

	Real Earnings (2017$)
Max. federal + state EITC	1.143***
	(0.212)
1 child in family	-4501.546***
	(630.170)
2 children in family	-8089.051***
	(783.847)
3+ children in family	-11669.469***
	(796.399)
Race: Black	-4628.708***
	(646.583)
Race: other	-5297.490***
	(1089.357)
HS graduate	8820.155***
	(533.982)
Some college	15363.999***
	(656.083)
Observations	33207
R^2	0.196

Notes: Standard errors in parentheses. Errors clustered at the person level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Treatment x Year Coefficients for the Dynamic Difference-in-Difference Regression

	(1)	(2)	(3)	(4)	(5)
	1976	1986	1993 (0 vs 1+)	1993 (1 vs 2+)	2009
Event Time -5	1090.3	-191.6			648.2
	(1030.0)	(586.7)			(1223.7)
Event Time -4	-838.1	37.26			
	(681.7)	(550.5)			
Event Time -3		523.0	390.9	644.4	1460.9
		(550.3)	(510.9)	(621.3)	(1100.2)
Event Time -2	435.5	-837.5	951.8**	515.7	
	(692.2)	(846.8)	(454.8)	(588.8)	
Event Time 0	230.0		580.7	-733.6	
	(676.3)		(483.9)	(707.7)	
Event Time 1	676.8		310.2	329.5	1689.5
	(759.9)		(511.8)	(486.6)	(1167.3)
Event Time 2	1047.6	-336.4	1400.5***	669.2	
	(762.5)	(477.6)	(499.7)	(562.1)	
Event Time 3	895.6	239.7		0	1564.1
	(1008.1)	(465.4)		(.)	(1294.8)
Event Time 4	192.3	162.7	811.0		
	(802.6)	(627.4)	(547.7)		
Event Time 5	-1572.8**	-392.6			2870.7**
	(790.7)	(521.5)			(1420.0)
N	1460	2220	2010	1269	639

Notes: Standard errors in parentheses. Errors clustered at the person level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1a: Earned Income Tax Credit (2020$) benefit schedule at the introduction of the EITC in 1975. Families with incomes between $1 and $38,483 in 2020$ were eligible for some amount of EITC benefits. Only families who had earnings of exactly $19,242 were eligible for the maximum benefit amount in this year. Families needed at least one eligible child in the household to qualify for benefits.

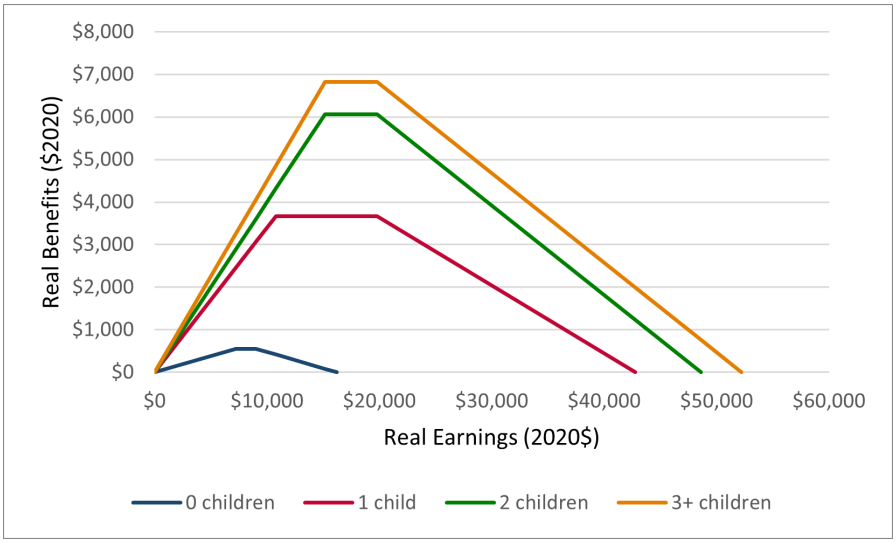

Figure 1b: Earned Income Tax Credit (2020$) benefit schedule for single earners at the expansion of the EITC in 2009. Families with incomes between $1 and $52,217 in 2020$, depending on the number of children in the household, were eligible for some amount of EITC benefits. There is a range of incomes eligible for the maximum EITC benefit, which depends on the number of children in the household. There are different benefit amounts and income thresholds for married couples, but only the numbers for single earners are presented here.

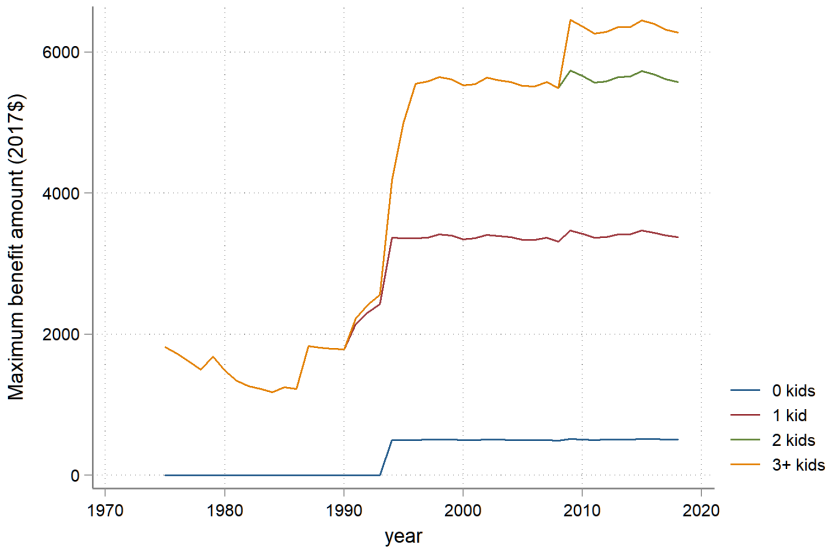

Figure 2: Earned Income Tax Credit (2017$) maximum federal benefit amount based on number of eligible children in household.

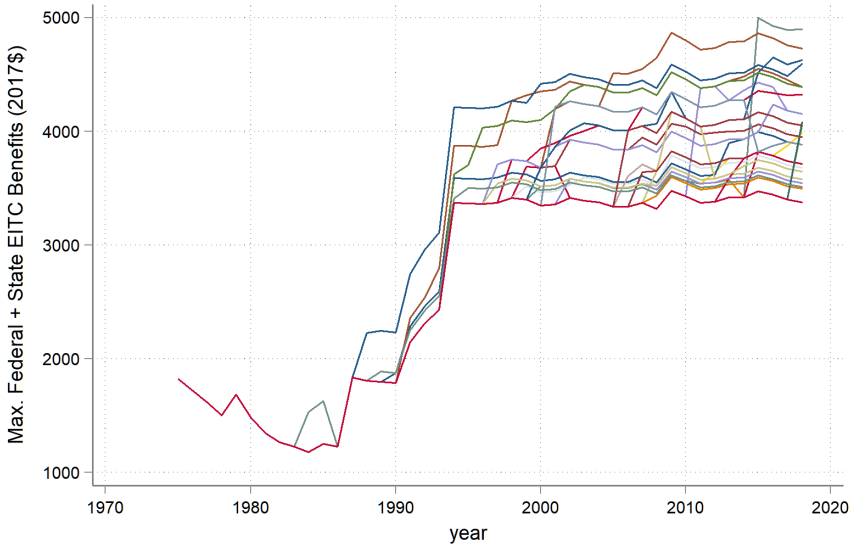

Figure 3a: Maximum state plus federal Earned Income Tax Credit benefit amounts (2017$), by state, for families with exactly one child in their household. Each line represents a different state, and the lowest line represents states with no state EITC credit.

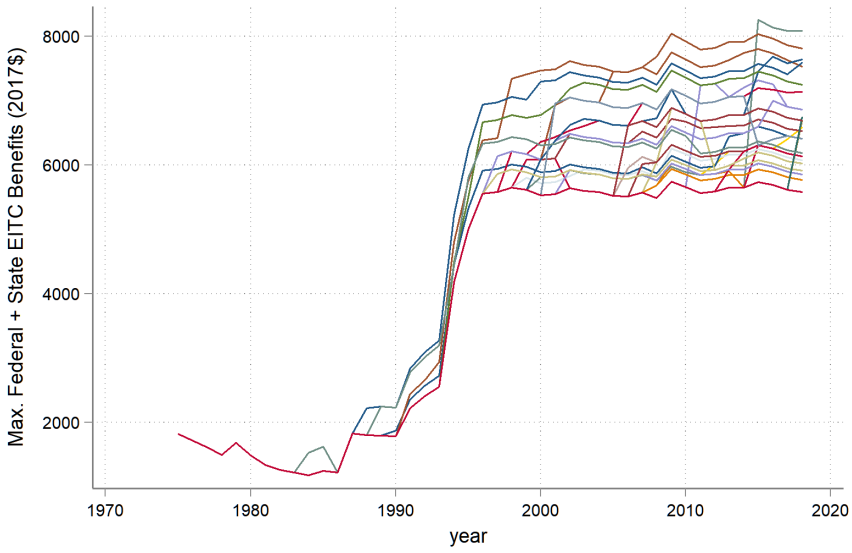

Figure 3b: Maximum state plus federal Earned Income Tax Credit benefit amounts (2017$), by state, for families with exactly two children in their household. Each line represents a different state, and the lowest line represents states with no state EITC credit.

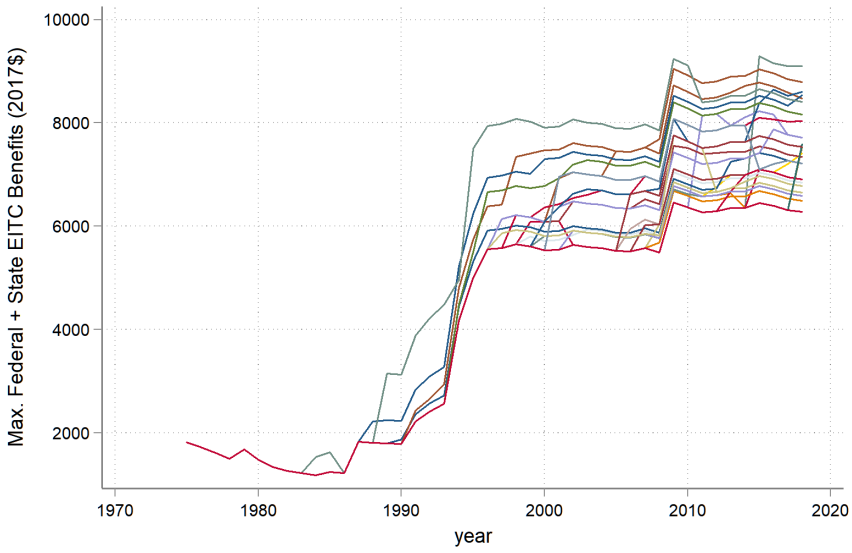

Figure 3c: Maximum state plus federal Earned Income Tax Credit benefit amounts (2017$), by state, for families with three ore more children in their household. Each line represents a different state, and the lowest line represents states with no state EITC credit.

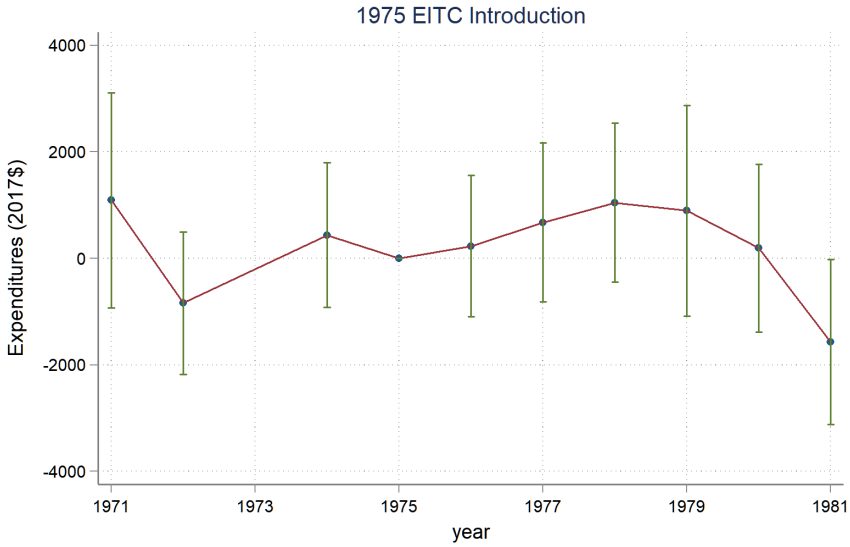

Figure 4: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1975 EITC introduction, effective for the tax year 1976. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

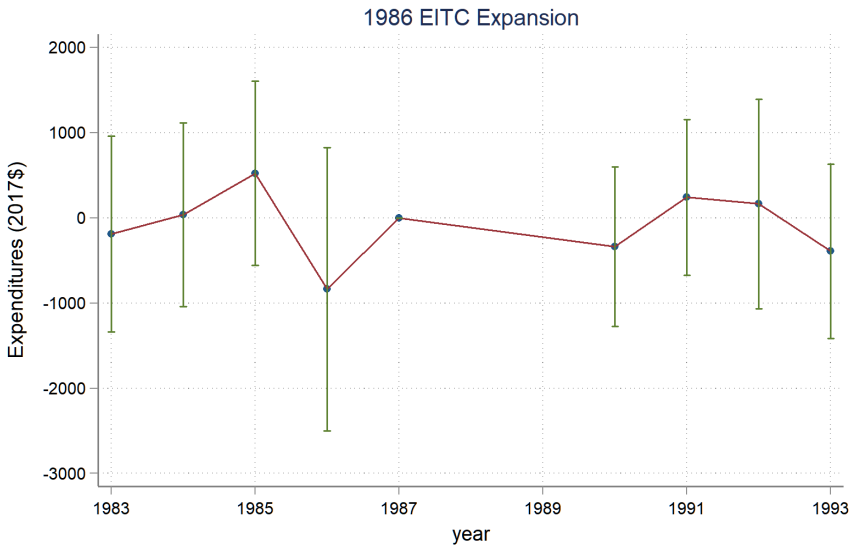

Figure 5: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1986 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

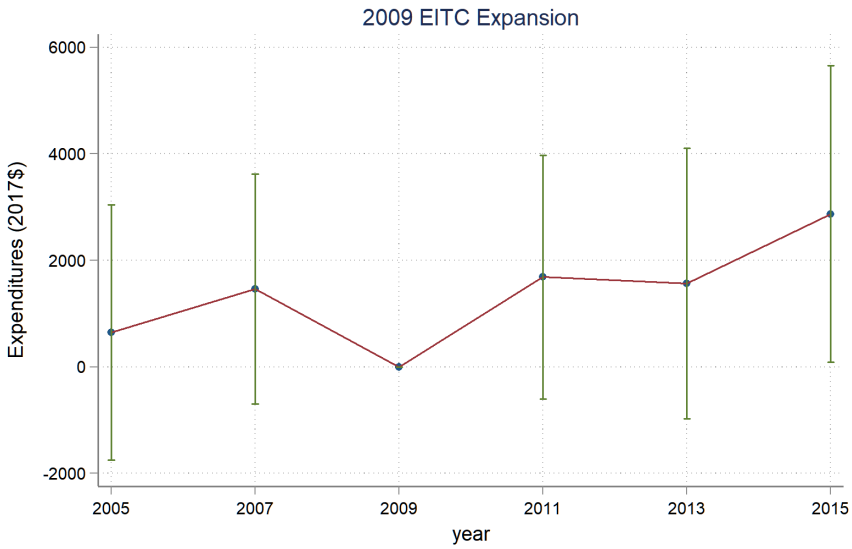

Figure 6: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 2009 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 3 or more children ages 0-18 and the control group is households with 1 or 2 children. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

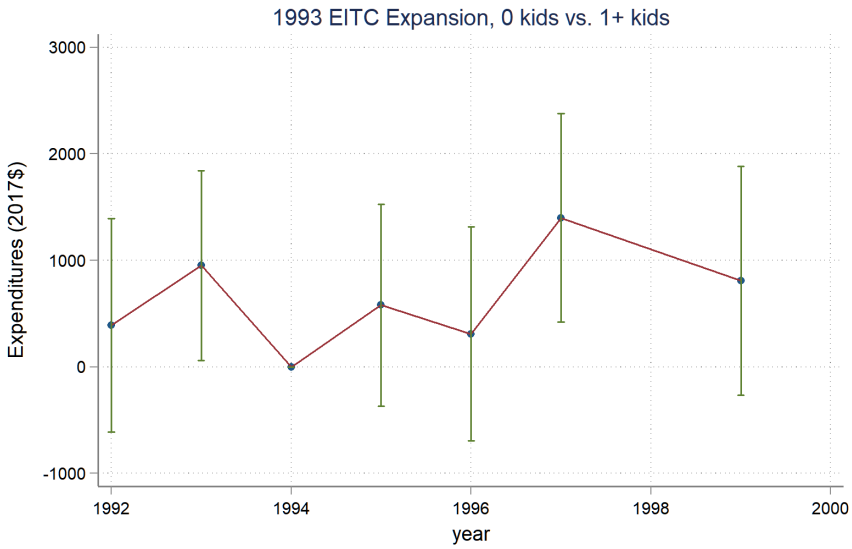

Figure 7: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

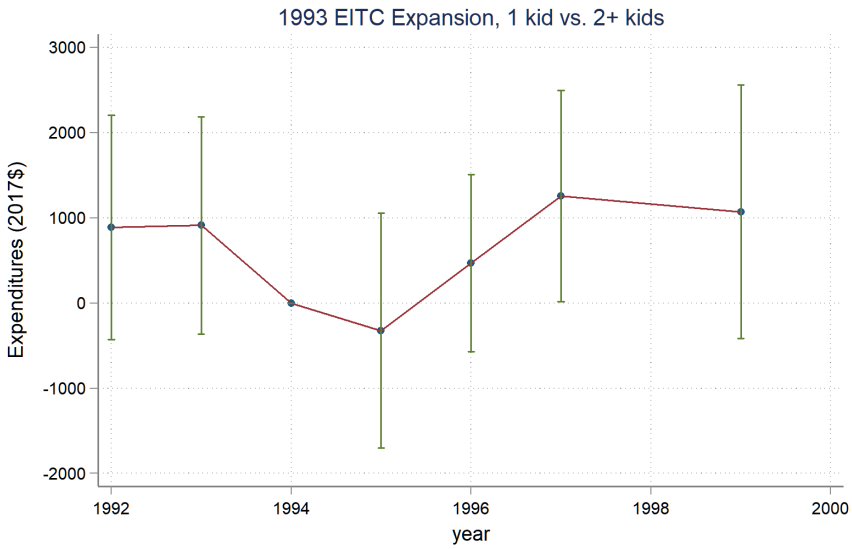

Figure 8: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 2 or more children ages 0-18 and the control group is households with one child. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

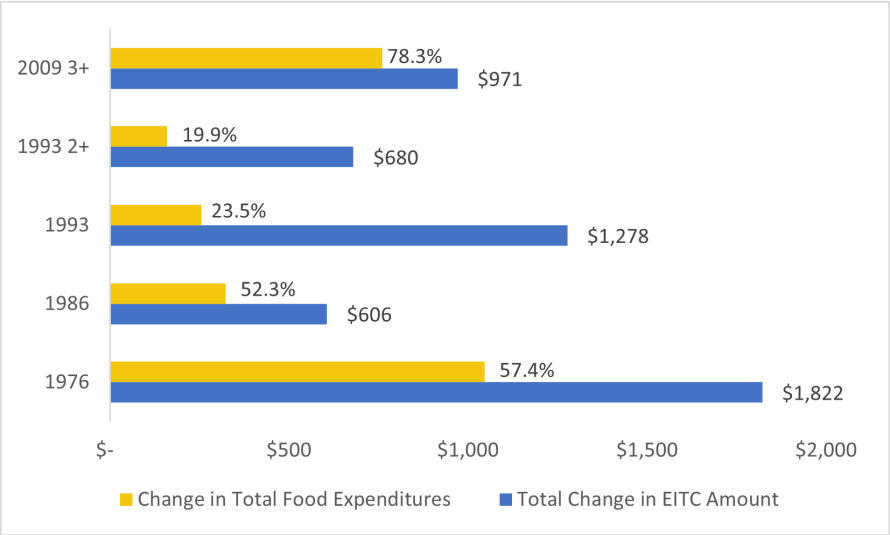

Figure 9: Static difference-in-differences coefficients shown as a percentage of the real total increase in maximum EITC benefits. Coefficients are taken from standard difference-in-difference estimates with the same controls variables and with treatment status defined in the same way as the dynamic difference-in-difference equations. All numbers are reported in terms of 2017$.

Appendix 1

In this appendix, I present how key demographic parameters change over the event period for each of the major federal EITC policy changes studied in this paper, comparing the sample used in the panel estimation to a similarly constructed sample, instead assuming the dataset is a repeated cross section. In other words, the panel sample is as described in the paper, whereas the cross sectional sample is all women who are between the ages of 24-48, have some college or less education, and are single in each individual year, taken separately. This is in contrast to the panel sample, where women need to have all of those characteristics in every year of the event window (5 years before and 5 years after the policy change).

Figures are shown for age, number of children in the household, total family income, and total food expenditures (dollar amounts are always in 2017$). The figures each further separate out the treatment and control samples. Figures are presented separately for each of the EITC policy changes in 1975, 1986, 1993, and 2009. Figures for the cross sectional sample are outlined in green, for ease of reading.

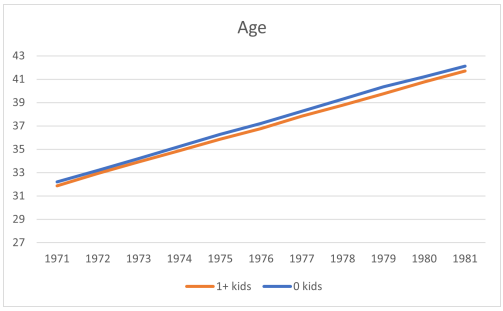

Figure 1: 1975, panel sample

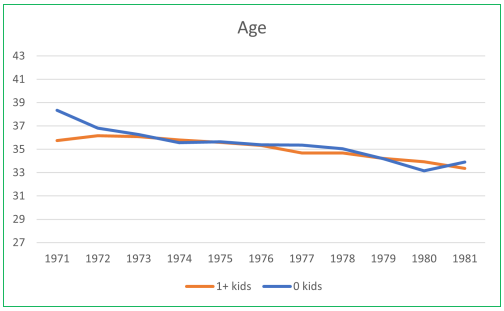

Figure 2: 1975, cross sectional sample

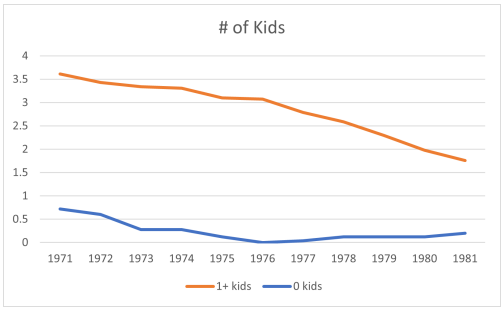

Figure 3: 1975, panel sample

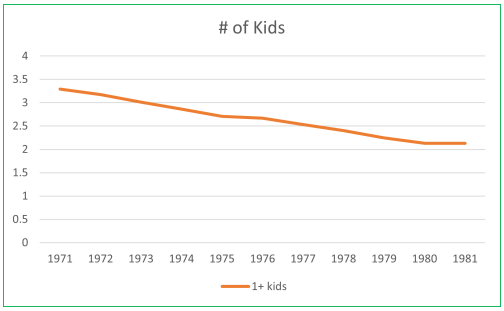

Figure 4: 1975, cross sectional sample

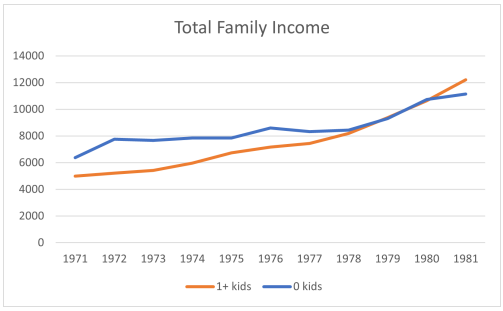

Figure 5: 1975, panel sample

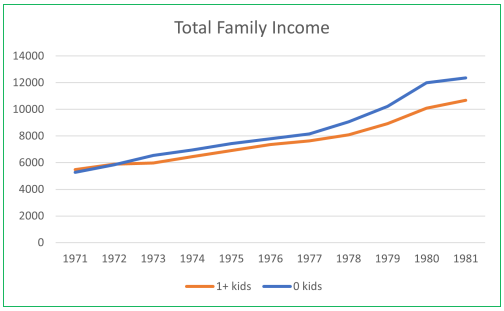

Figure 6: 1975, cross sectional sample

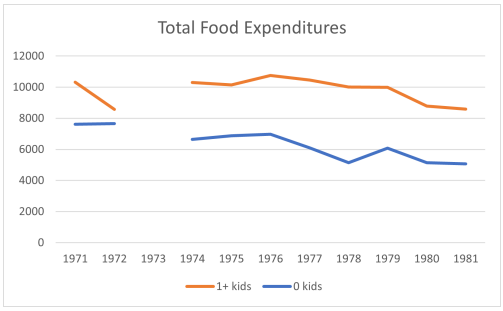

Figure 7: 1975, panel sample

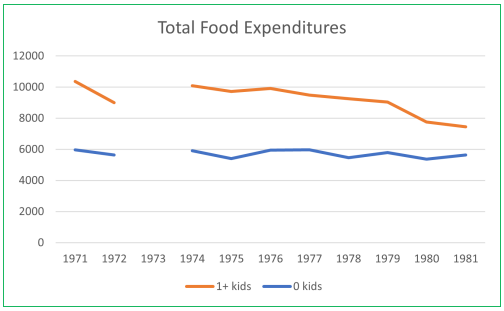

Figure 8: 1975, cross sectional sample

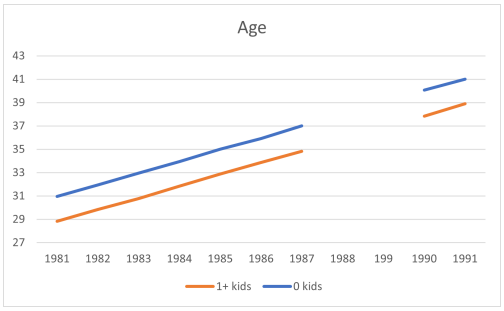

Figure 9: 1986, panel sample

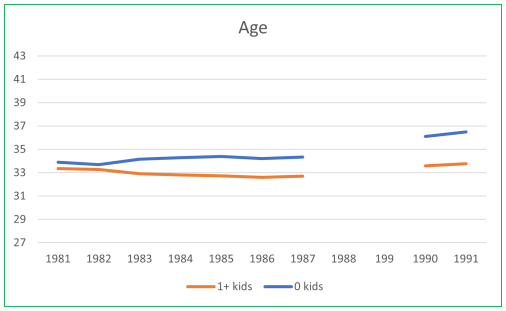

Figure 10: 1986, cross sectional sample

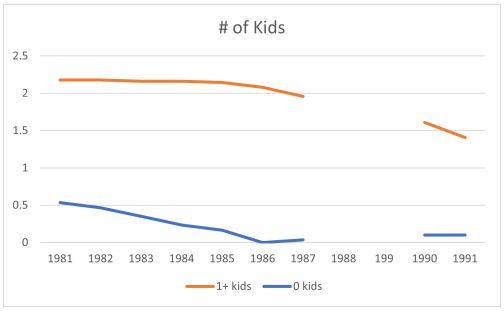

Figure 11: 1986, panel sample

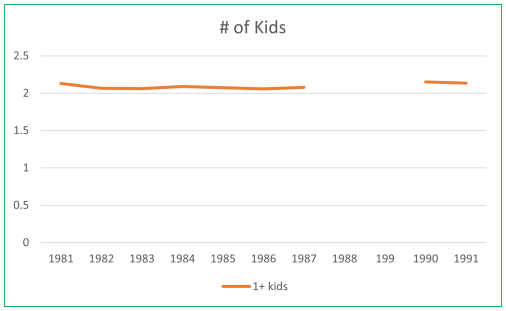

Figure 12: 1986, cross sectional sample

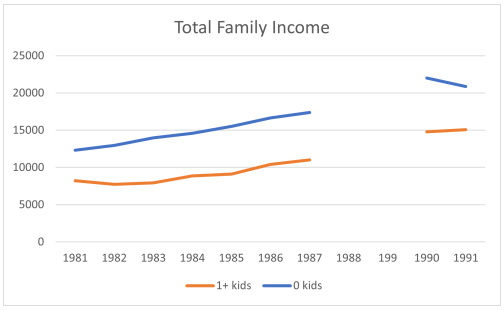

Figure 13: 1986, panel sample

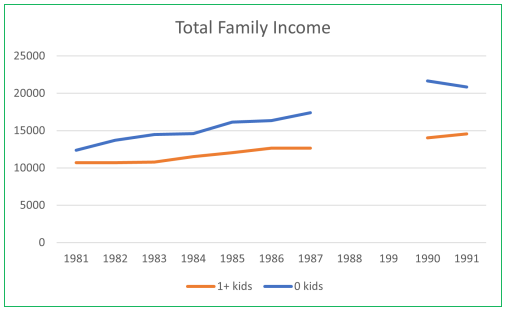

Figure 14: 1986, cross sectional sample

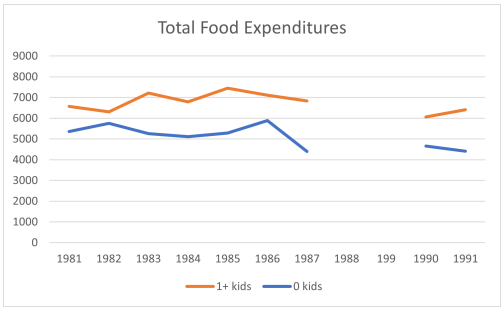

Figure 15: 1986, panel sample

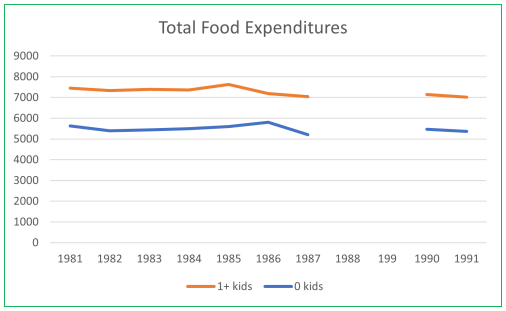

Figure 16: 1986, cross sectional sample

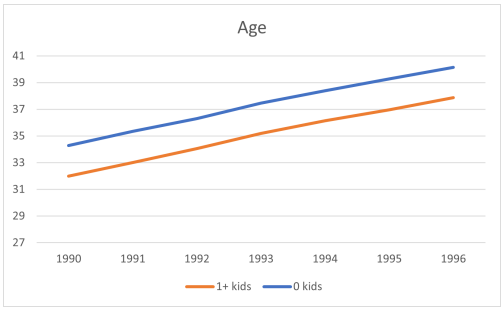

Figure 17: 1993, panel sample

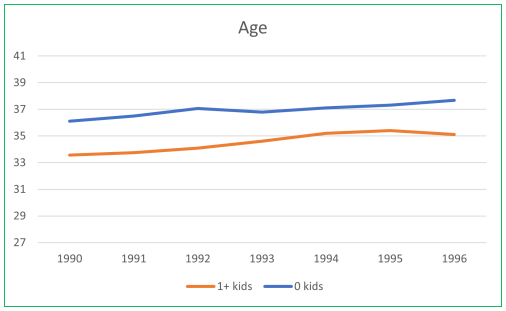

Figure 18: 1993, cross sectional sample

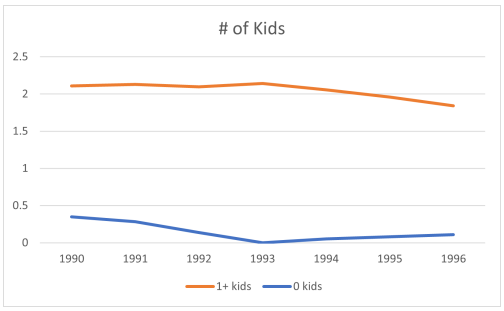

Figure 19: 1993, panel sample

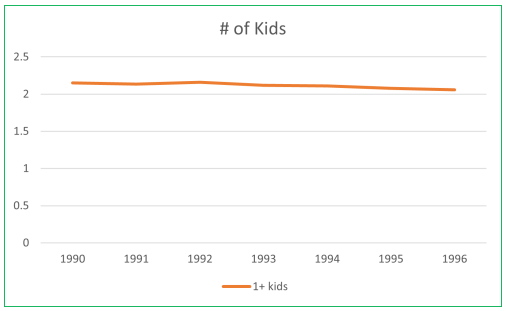

Figure 20: 1993, cross sectional sample

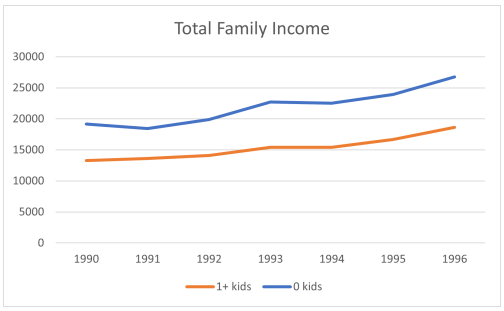

Figure 21: 1993, panel sample

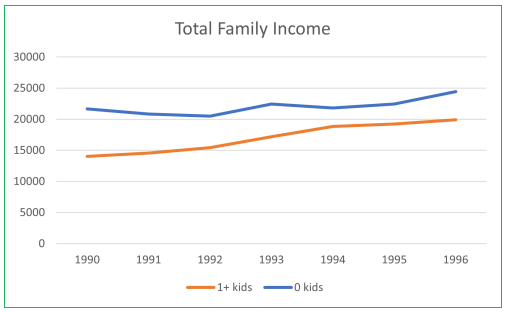

Figure 22: 1993, cross sectional sample

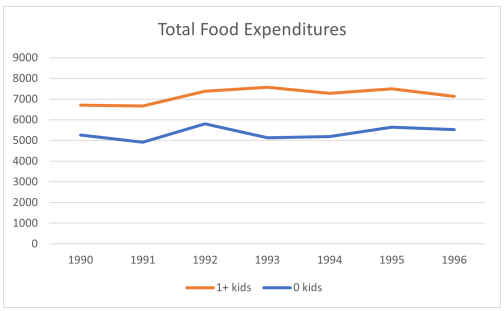

Figure 23: 1993, panel sample

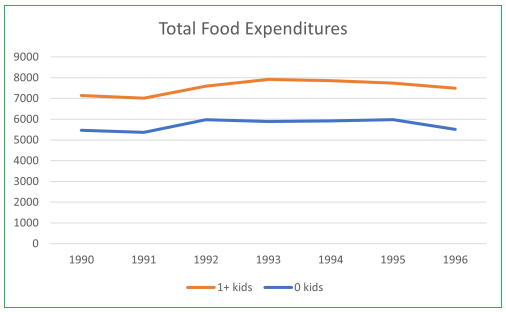

Figure 24: 1993, cross sectional sample

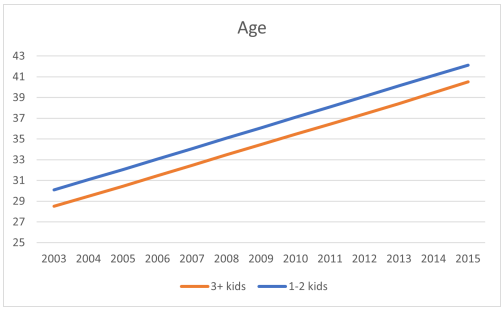

Figure 25: 2009, panel sample

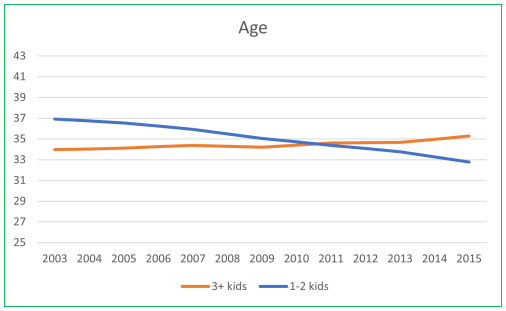

Figure 26: 2009, cross sectional sample

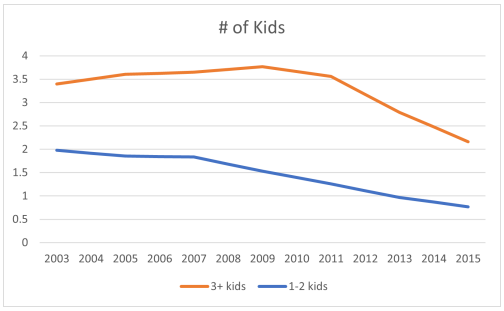

Figure 27: 2009, panel sample

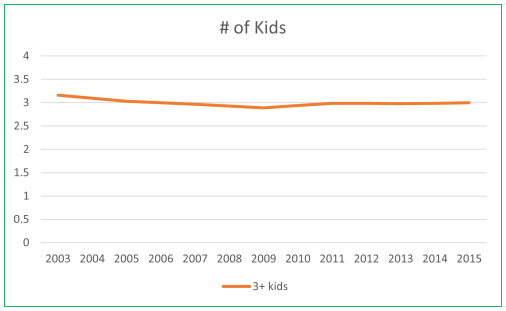

Figure 28: 2009, cross sectional sample

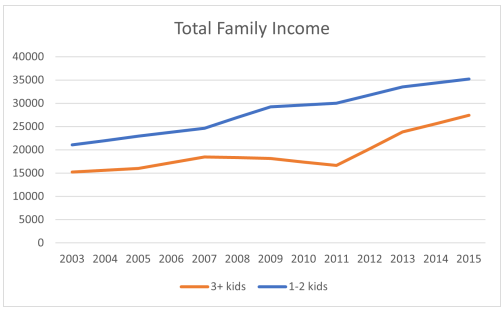

Figure 29: 2009, panel sample

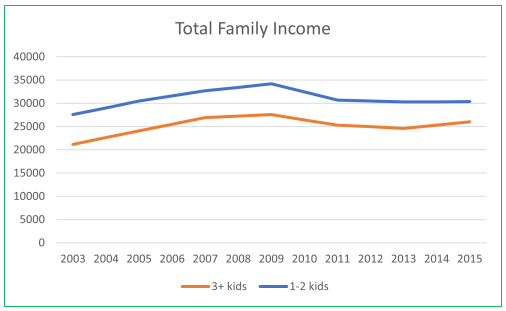

Figure 30: 2009, cross sectional sample

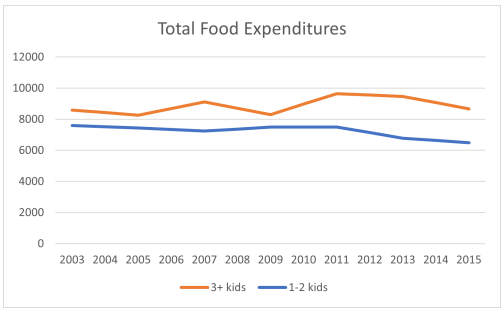

Figure 31: 2009, panel sample

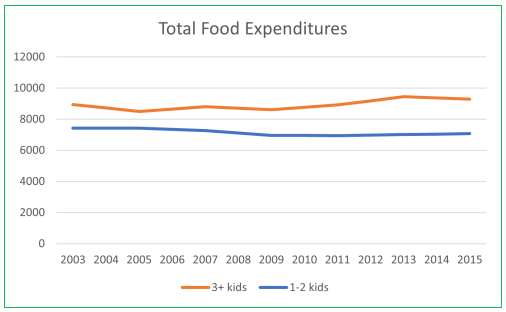

Figure 32: 2009, cross sectional sample

Appendix 2

My sample is single women with some college or less education, which is meant to be a group with high eligibility for the EITC. Although it can vary by year, one estimate from the mid 1990s finds around 68% EITC eligibility for this demographic group (Hoynes and Patel 2018). Program take-up rates are not known for this exact demographic group, but vary between 70 to 80% for the whole population, as described in the main text. Some of the women assigned to the treatment group in my sample are not eligible for the EITC because their income is too high to qualify them.

Furthermore, in my first specification, I make the assumption that any family who would qualify for the EITC based only on their demographic compsition, state of residence, and the year, is reciving the maximum amount of EITC benefits. I do this to simplify the assumptions needed for identification, and because it avoids the issue of endogeneity in imputing the treatment using a variable highly correlated with the outcome.

In order to roughly understand the impact of these restrictions, I present Figures 1 through 3 below. Each shows the distribution of earned incomes for households in the main sample imposed over the EITC benefit structure for the latest expansion in 2009. Not only does it display the proportion of the sample with pretax earnings that fall under the ceiling for receiving EITC benefits, but it also gives an idea of the amount of the sample that would qualify for the maximum amount of EITC earnings, on the flat part of the red line. Figure 1 is for families with exactly 1 child in the household, Figure 2 is for 2 children, and Figure 3 is for families with 3 children.

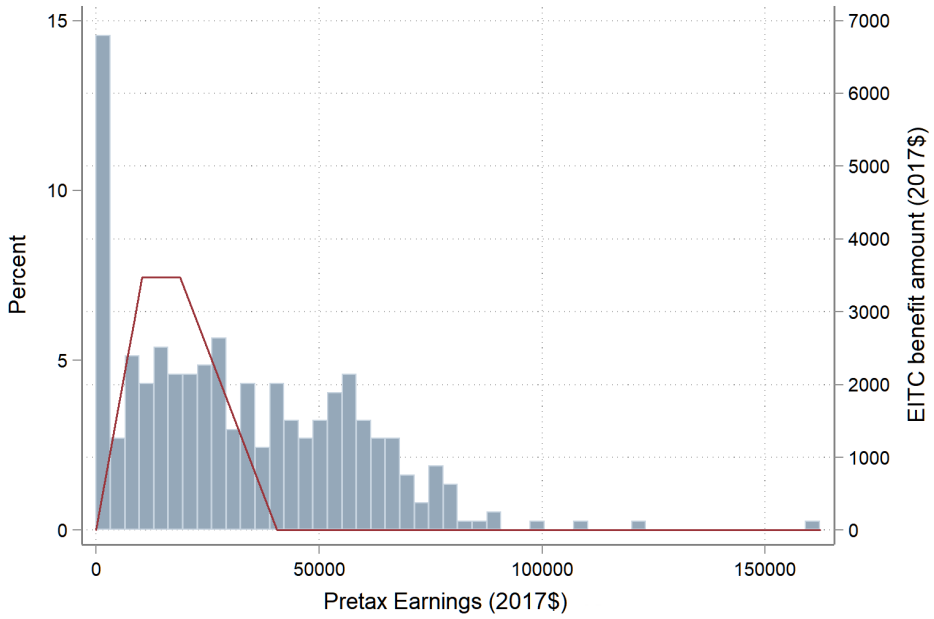

Figure 1: 2009 EITC benefit schedule overlaid on top of distribution of incomes of women in the sample with exactly one child present in the sample in 2009. The left axis shows the percentage of women in each income bin in 2009, and the right axis shows the dollar amount of EITC benefits by family income. All numbers are reported in terms of 2017$.

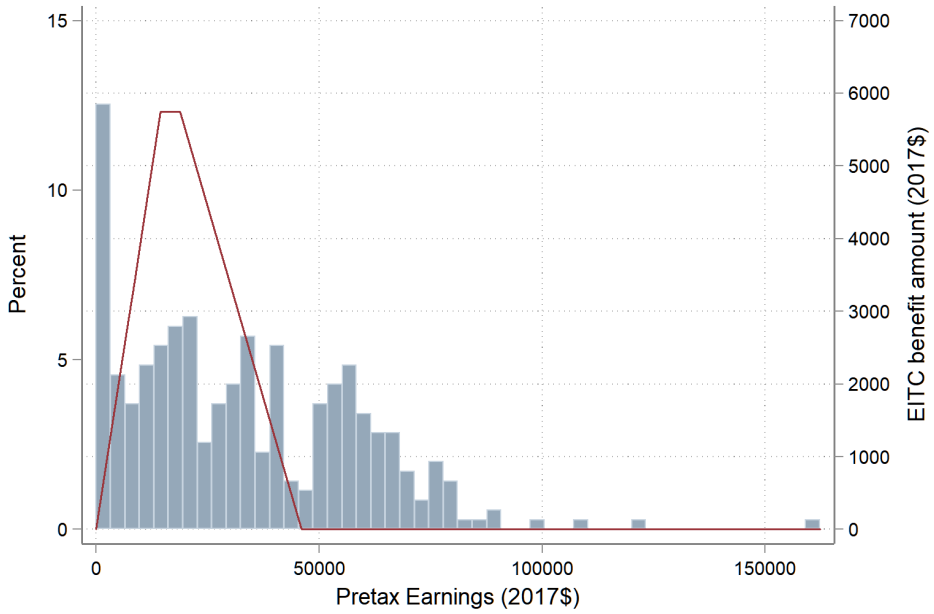

Figure 2: 2009 EITC benefit schedule overlaid on top of distribution of incomes of women in the sample with exactly two children present in the sample in 2009. The left axis shows the percentage of women in each income bin in 2009, and the right axis shows the dollar amount of EITC benefits by family income. All numbers are reported in terms of 2017$.

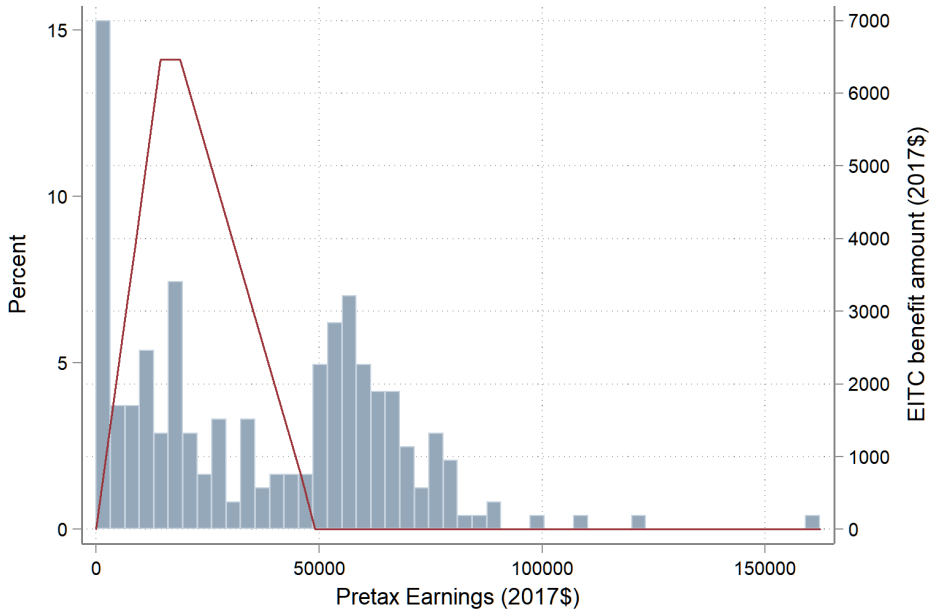

Figure 3: 2009 EITC benefit schedule overlaid on top of distribution of incomes of women in the sample with three or more children present in the sample in 2009. The left axis shows the percentage of women in each income bin in 2009, and the right axis shows the dollar amount of EITC benefits by family income. All numbers are reported in terms of 2017$.

Appendix 3: Robustness Checks

In this appendix, I present a number of different robustness checks to equation (2) from the main text, the dynamic difference-in-differences model. As described in the text, there are numerous strengths to this specification. It incorporates modern difference-in-differences techniques, and the dynamic structure of the model gives more detailed information about expenditure patterns over time in response to EITC receipt. Using a balanced panel allows for within-household analysis of changes in total food expenditures before and after each natural experiment of changes in EITC policy, which reduces bias in the estimates due to heterogeneity in unobservable factors, such as taste, between households. Using a sample with high eligibility for the EITC and high program take-up avoids the issue of imputing EITC eligibility using income, which is highly correlated with the outcome variable. All of these benefits, however, come at the price of restricting sample size and reducing the precision of the point estimates.

One of the ways to feel more comfortable trusting results that are not statistically significant are to try a number of different specifications and see if the patterns in the data hold. Because of the small sample sizes, the estimates from equation (2) are going to be somewhat noisy and mostly not statistically significant than zero. Testing the robustness to a variety of different specifications can solidify the patterns found in the original estimates by ensuring that changes to the sample, measurement of outcome variable, or controls do not change the overall shape of the findings.

This appendix lays out the results for four separate robustness checks. For each of them, I present the same five figures for each of the federal expansions described in Table 2 of the main text.

The first robustness check is changing the sample to include married couples and single men. In this case, treatment status is assigned based on imputed eligibility using reported earnings. It should be noted that although this would increase the cross sectional sample by a large amount, it does not increase the sample in the balanced panel. This new sample includes households headed by both married and single people and has no restrictions based on educational attainment. Relaxing the sample restrictions increases the number of observations in a given year, but married couples and single men are much less likely to remain eligible for the EITC for many years in a row. Many of the new members of the sample are only eligible for the EITC for one or two years based on their earnings. Because I use a balanced panel, households only remain in the sample if they remain eligible for the EITC based on their income for the entire panel.

These results are presented in Figures 1 through 5. The general patterns are similar to the main results. There is an increase following the 1975 introduction and then a dip starting in 1979 as a result of changes in Food Stamps policy described in the main text. There is some evidence of an increase in food expenditures in 1992, likely due to the expansion in 1991, but otherwise no movement around the 1986 expansion, similar to the main results. Figures 3 through 5 show very similar patterns to the main results.

The second robustness check is to adjust total food expenditures based on the number of members in the household. One concern could be that there are large fixed costs to purchasing food, and that each additional person in the family does not have an additive relationship for food expenditures. Here, I adjust total expenditures for the number of members of the household, using the same parameters used in the USDA Thrifty Food Plan.[1] Figures 6 through 10 show these estimates. By using this multiplier, the results are almost visually indistinguishable from the main results.[2]

Although the bulk of the literature on fertility decisions in response to the EITC finds a lack of evidence that the EITC induces births or changes fertility in response to policy changes, if it in fact does, there could be an issue with treatment status (based on number of children in household) being endogenous to the EITC policy changes. In the third robustness check, I assign treatment status based on the family's composition in the year of the policy change to avoid this issue. In this specification, treatment status is static. For example, for the 2009 EITC expansion, treatment status is assigned based on the number of children in the household in the year 2009 and there is no moving between treatment and control in any years. This avoids bias in the post-period if we are concerned about fertility decisions in response to the EITC policy changes, though there is little evidence empirically that this happens. In Figures 11 through 15, we can see that because some households who would have been assigned to the control are more and more likely to have a child and become eligible for the EITC with each additional year, the treatment effect in the later years is biased downwards.

Because food expenditures are especially sensitive to expansions and contractions of the economy, in the fourth and final robustness check, I control for the state employment to population ratio. Furthermore, there are substantial differences in the sensitivity of household spending to business cycles depending on the

[1] This is achieved by using a multiplier based on family size. A family size of 4 is considered to be the baseline, families of one have a multiplier of 1.2 and families of 6 or more have a multiplier of 0.9, with even steps in between.

[2] Adjusting total food expenditures crudely by simply dividing by the number of members in the household (per capita food expenditures) gives qualitatively similar results.

number of children. Households with more children have more cyclical patterns of household expenditures. This is especially troublesome because, as is the consensus in the literature on the labor market impacts of the EITC for single women, there is a large impact of each of the studied EITC policy changes on female labor supply. So the households with children are, on top of being "treated", more likely to enter the labor force as a result of the treatment, and therefore more likely to be impacted by fluctuations in the unemployment rate. In other words, the treatment group is more sensitive to the unemployment rate after being treated. Therefore, I additionally control for the unemployment rate interacted with the presence of children in the household. In the end, results are not sensitive to adding this interaction term.

The results from this specification are presented in Figures 16 through 20. As is seen in the other robustness checks, adding state trends does not change the pattern of the overall results. They remain visually similar to the results found in the main text.

Taken all together, the stability of the results are a good indication that the estimates seen are a reliable measurement of the true effect happening in the population. The point estimates across specifications remain close in magnitude, giving more credibility to the interpretation of the results, even though small sample sizes do add noise and limit statistical significance of each individual estimate.

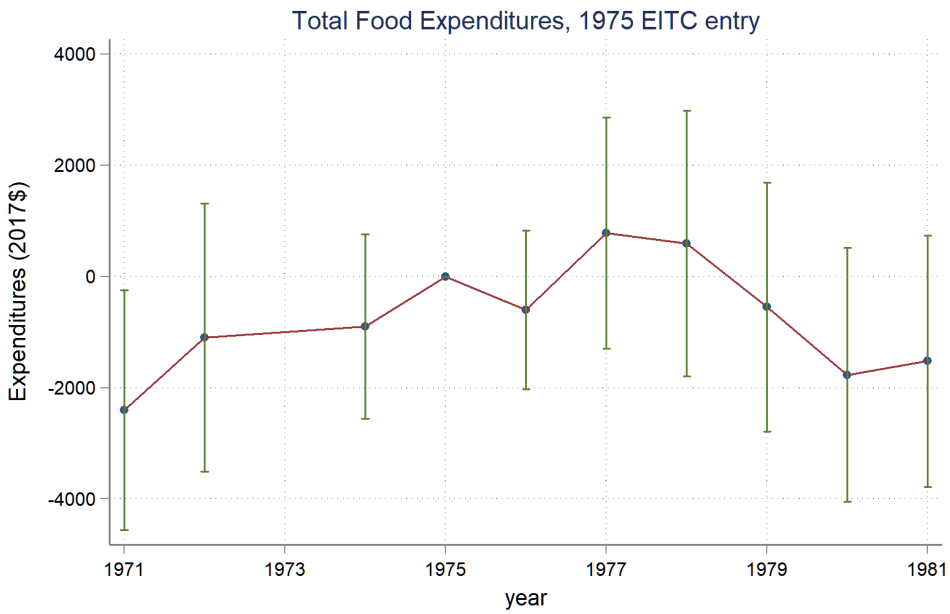

Figure 1: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 1975 EITC introduction, effective for the tax year 1976. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 1 or more eligible children, and state and individual fixed effects.

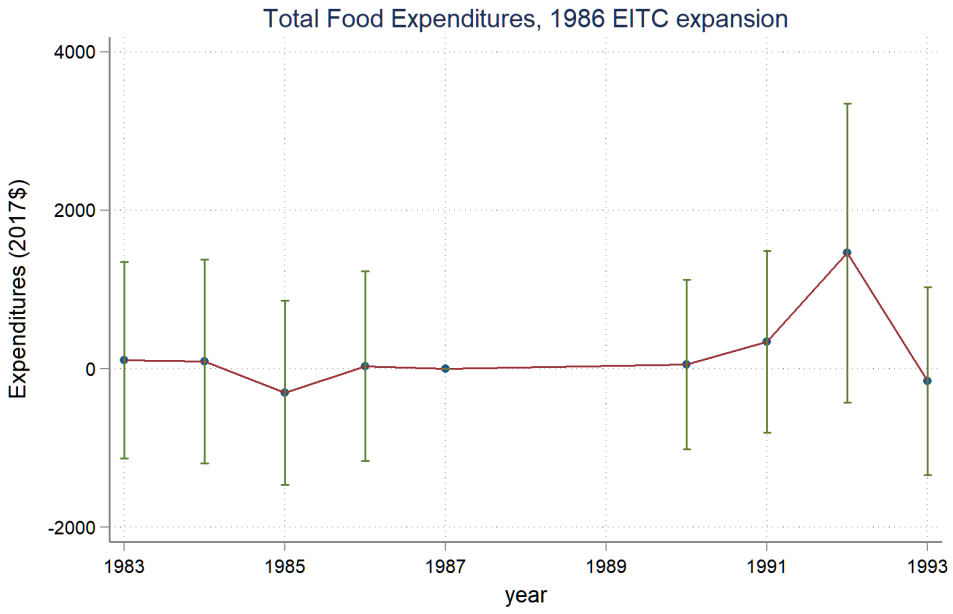

Figure 2: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 1986 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 1 or more eligible children, and state and individual fixed effects.

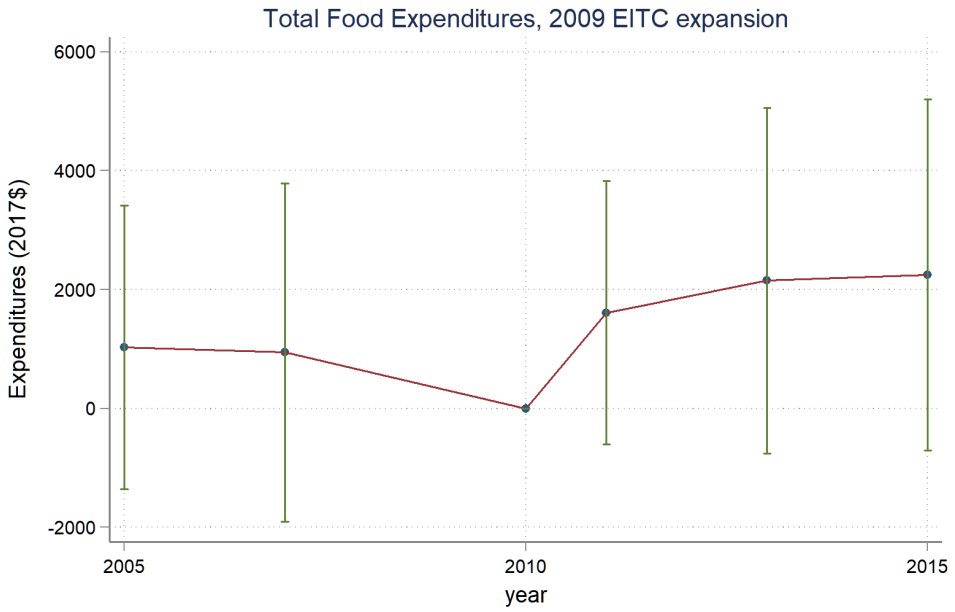

Figure 3: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 2009 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 3 or more children ages 0-18 and the control group is households with 1 or 2 children. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 3 or more eligible children, and state and individual fixed effects.

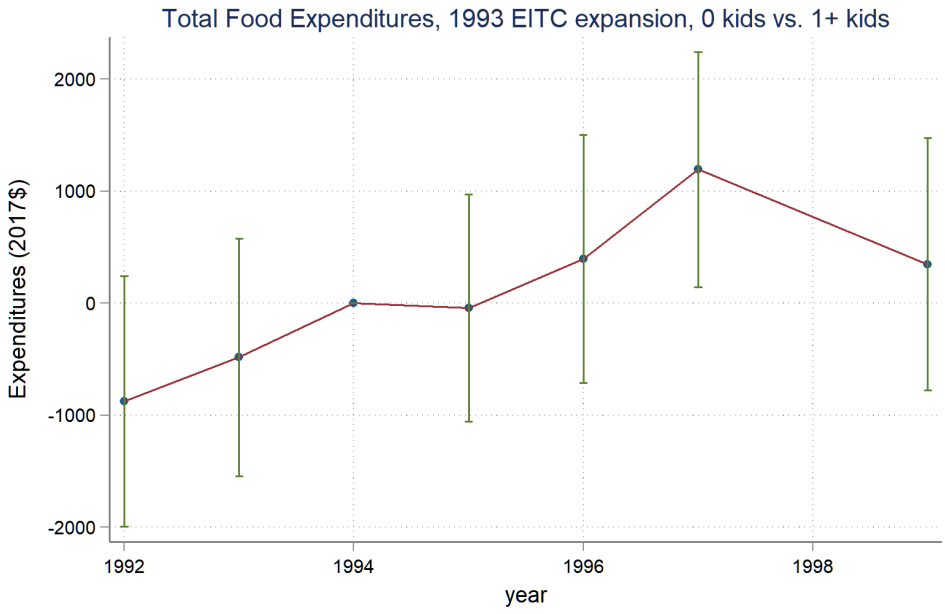

Figure 4: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 1 or more eligible children, and state and individual fixed effects.

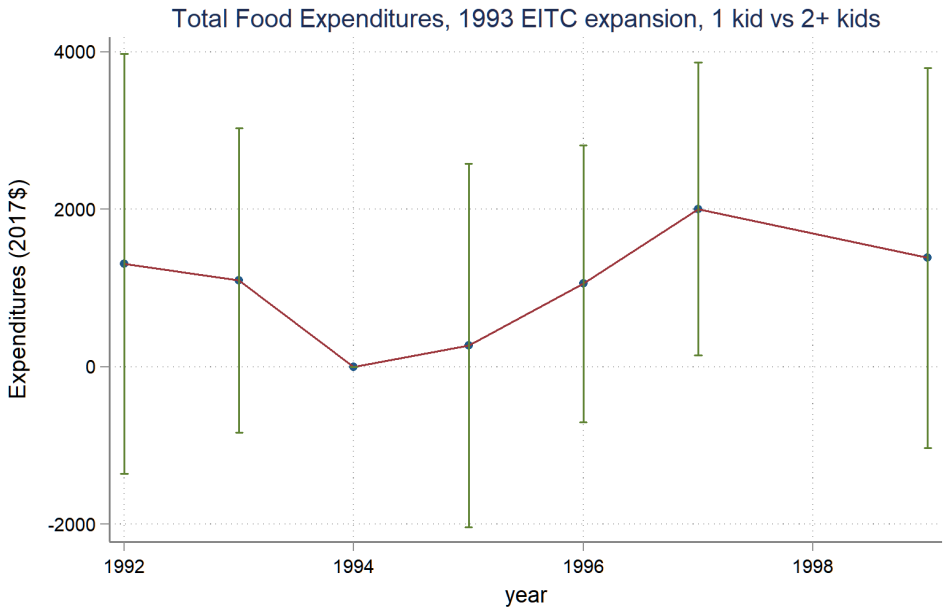

Figure 5: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 2 or more children ages 0-18 and the control group is households with one child. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 2 or more eligible children, and state and individual fixed effects.

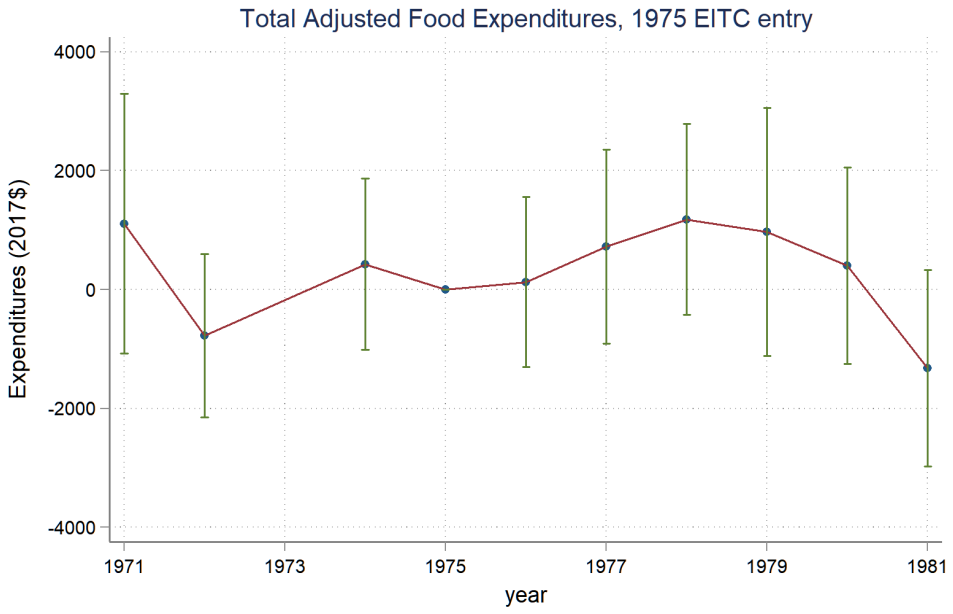

Figure 6: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1975 EITC introduction, effective for the tax year 1976. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

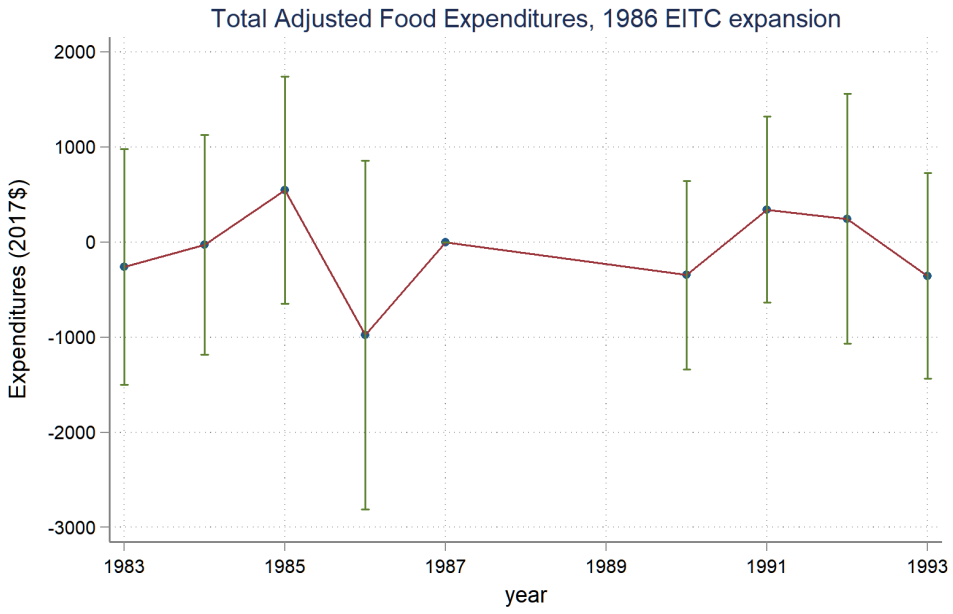

Figure 7: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1986 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

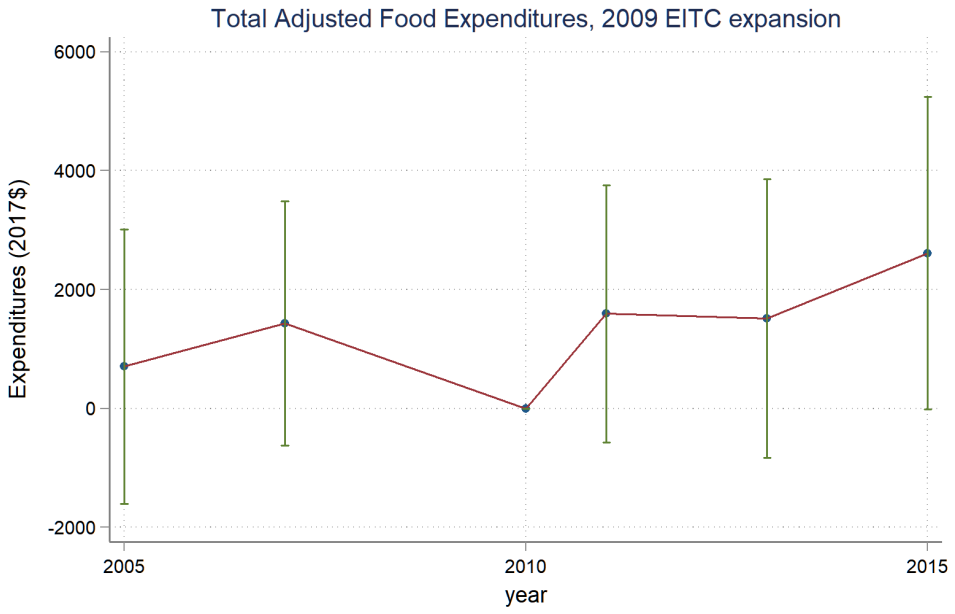

Figure 8: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 2009 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 3 or more children ages 0-18 and the control group is households with 1 or 2 children. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

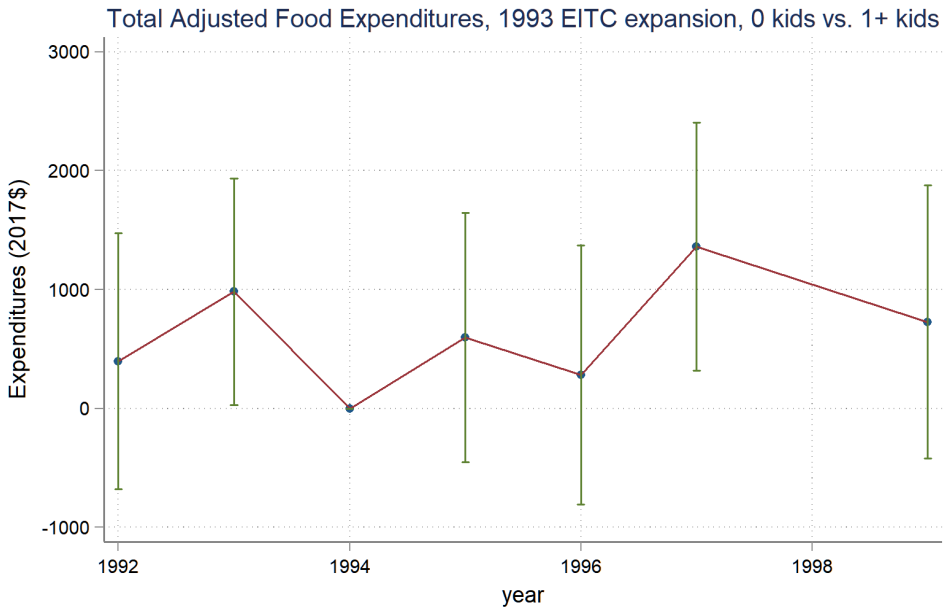

Figure 9: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

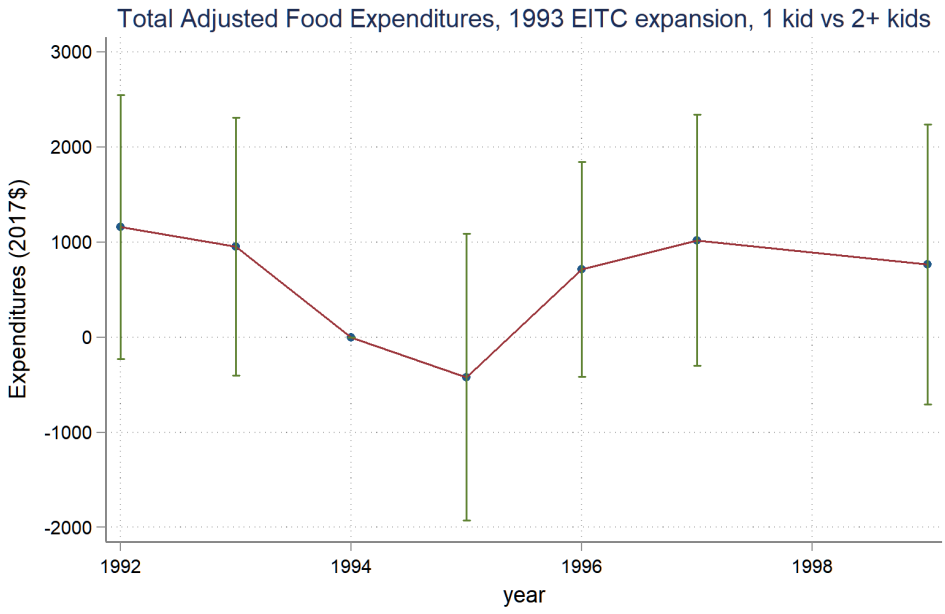

Figure 10: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 2 or more children ages 0-18 and the control group is households with one child. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

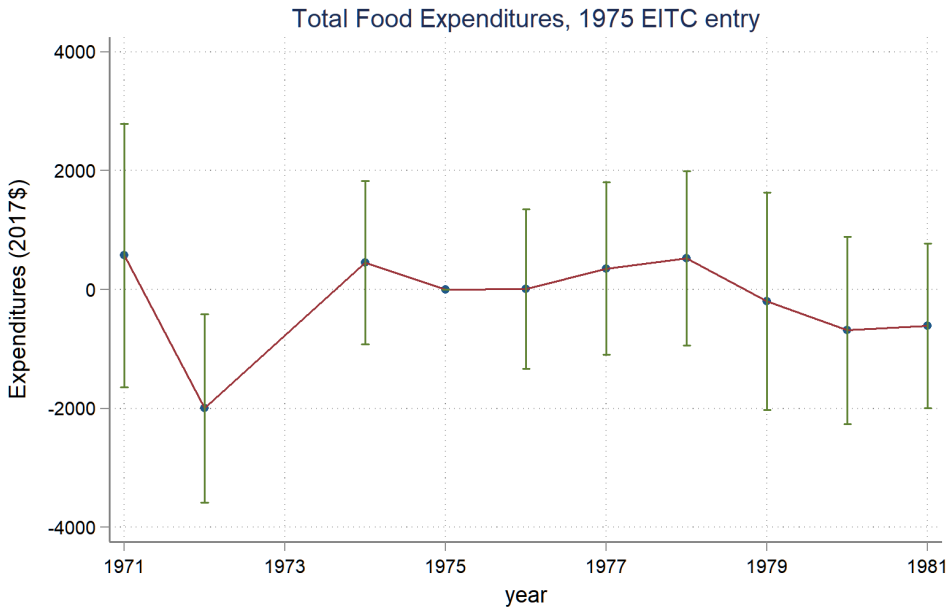

Figure 11: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1975 EITC introduction, effective for the tax year 1976. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 in 1975 and the control group is households with no children in 1975. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

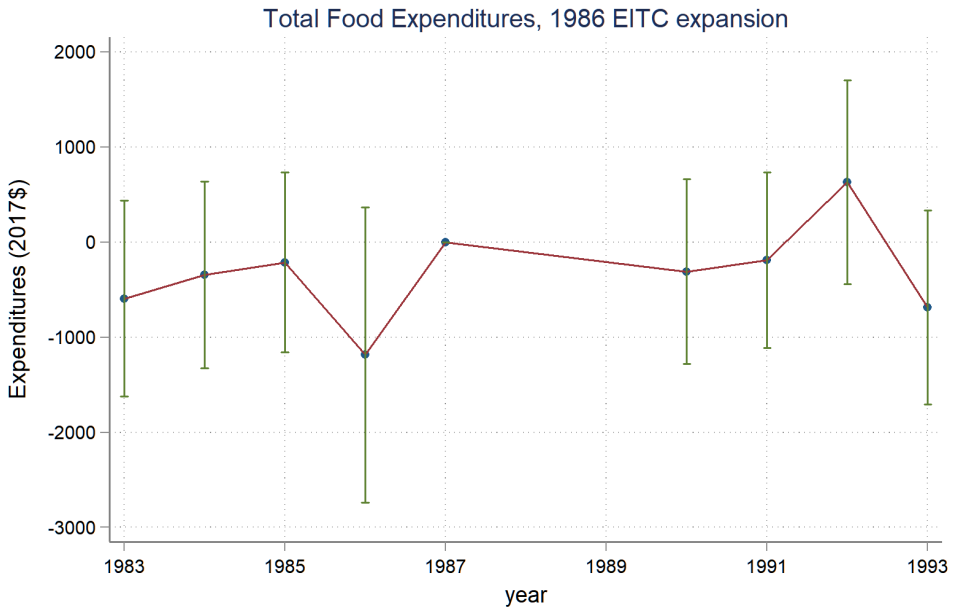

Figure 12: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1986 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 in 1987 and the control group is households with no children in 1987. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

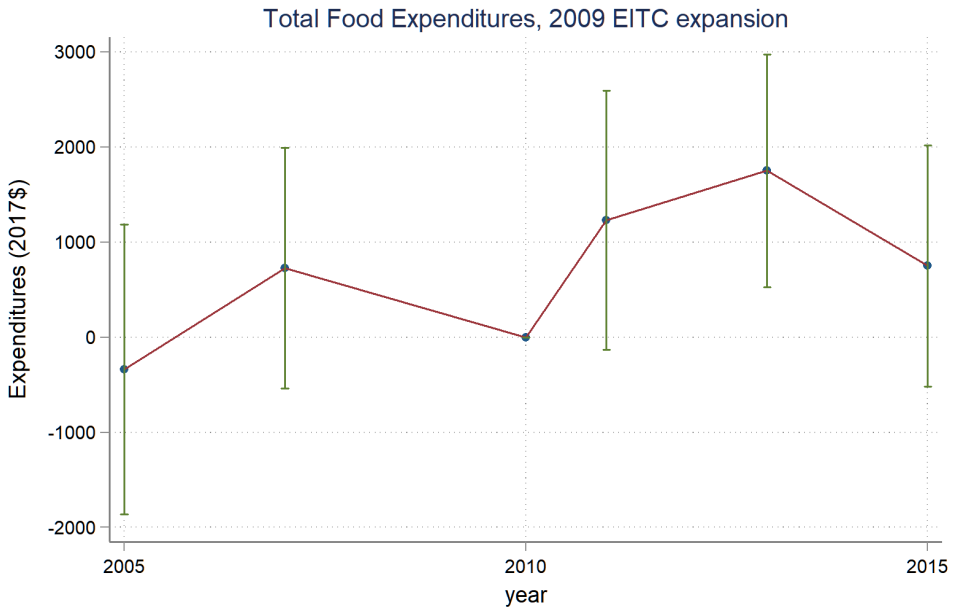

Figure 13: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 2009 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 3 or more children ages 0-18 in 2009 and the control group is households with 1 or 2 children in 2009. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

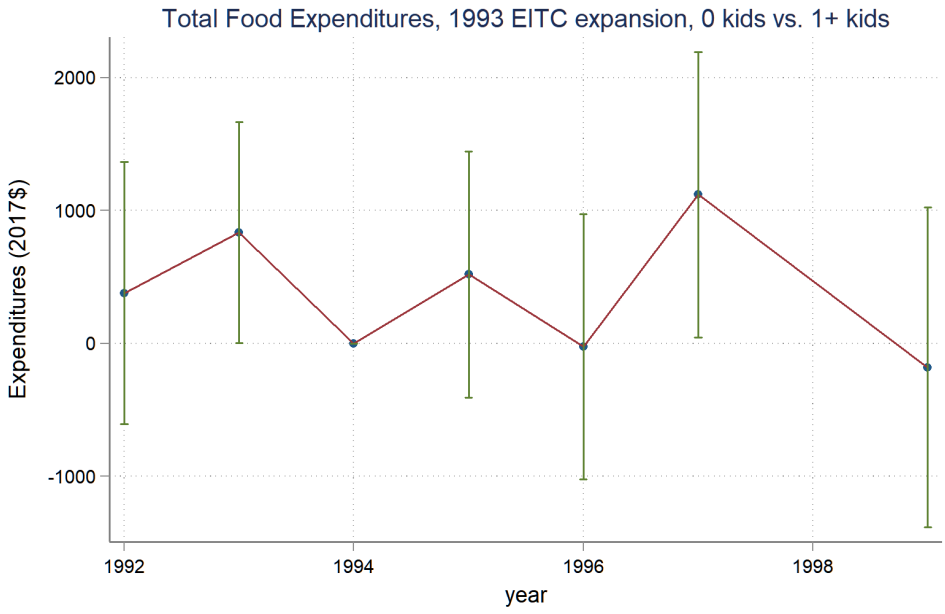

Figure 14: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 in 1994 and the control group is households with no children in 1994. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

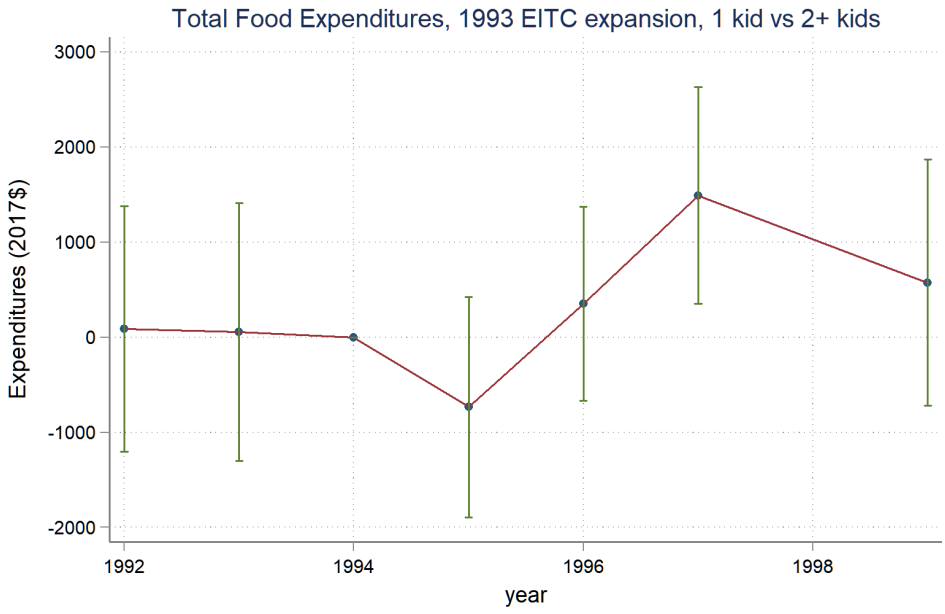

Figure 15: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 2 or more children ages 0-18 in 1994 and the control group is households with one child in 1994. Includes age dummies and state and individual fixed effects. Standard errors are clustered at the individual level.

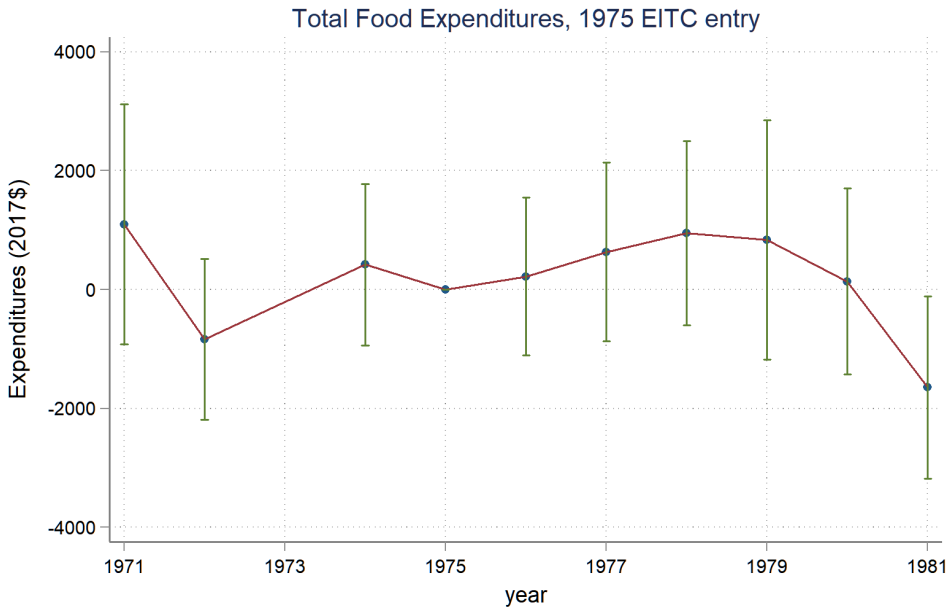

Figure 16: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1975 EITC introduction, effective for the tax year 1976. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 1 or more eligible children, and state and individual fixed effects. Standard errors are clustered at the individual level.

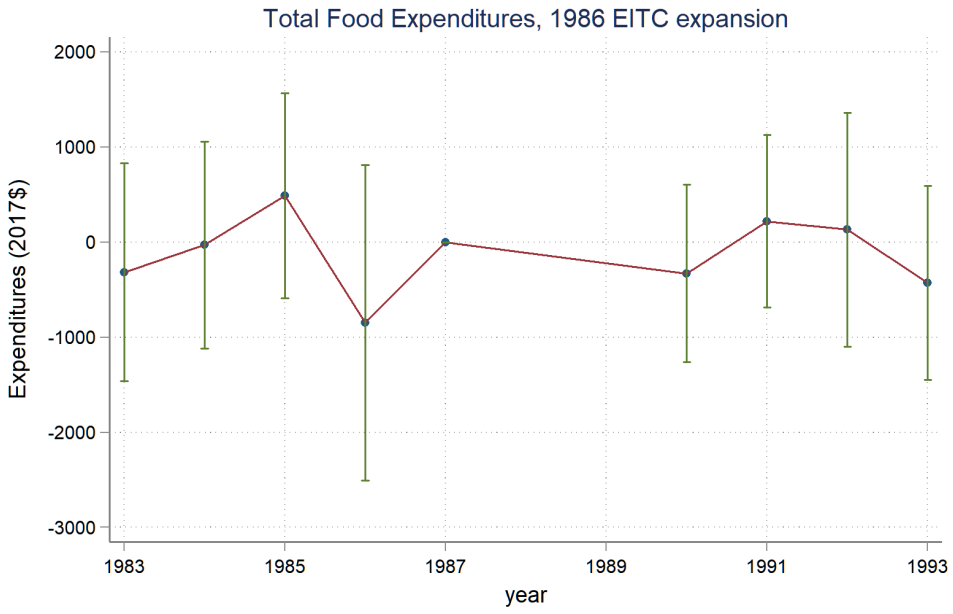

Figure 17: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1986 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 1 or more eligible children, and state and individual fixed effects. Standard errors are clustered at the individual level.

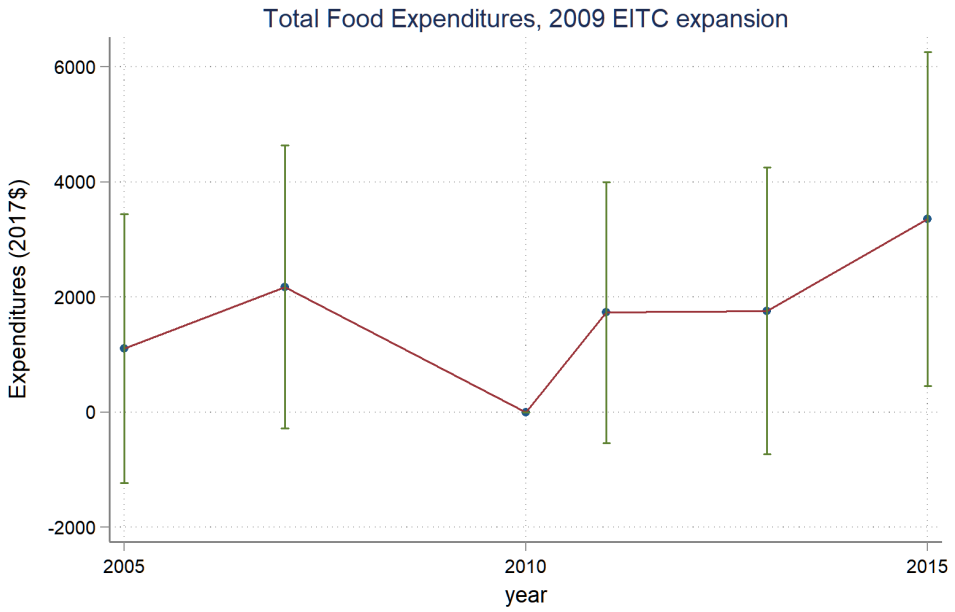

Figure 18: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 2009 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 3 or more children ages 0-18 and the control group is households with 1 or 2 children. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 3 or more eligible children, and state and individual fixed effects. Standard errors are clustered at the individual level.

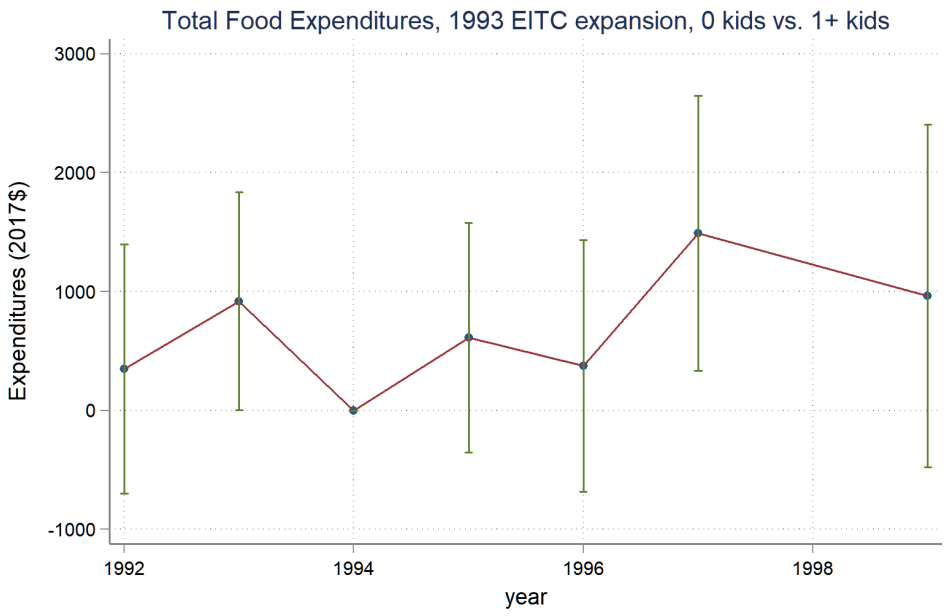

Figure 19: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 1 or more children ages 0-18 and the control group is households with no children. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 1 or more eligible children, and state and individual fixed effects. Standard errors are clustered at the individual level.

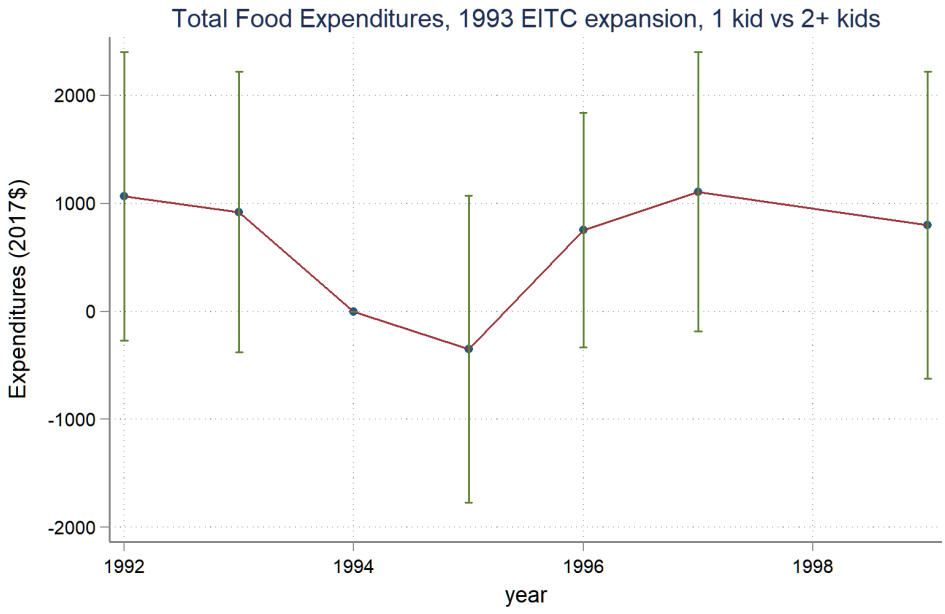

Figure 20: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures at home include food purchased using Food Stamps benefits. Treatment group is households with 2 or more children ages 0-18 and the control group is households with one child. Includes age dummies, controls for state employment to population ratio, state employment to population ratio interacted with dummy for presence of 2 or more eligible children, and state and individual fixed effects. Standard errors are clustered at the individual level.