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To Jacob, whose unwavering support was a continual source of guidance and inspiration.

Contents

Abstract

The social insurance system in the United States is a broad and varied set of programs, transfers, and tax credits that serve as a social safety net, often through the US tax system. The US spent around 13% of its GDP, or around $2.7 trillion, on social insurance programs in 2019. This dissertation explores how the social insurance system in the US functions as anti-poverty policy. The first two chapters focus on the Earned Income Tax Credit (EITC) and examine the mechanisms through which the program works. These two chapters use different approaches and data to understand household expenditure decisions because of EITC receipt. I focus on food expenditures, as food insecurity and lack of nutrition remain challenges for low-income households in the US. The third chapter explores the role of food and nutrition programs in the US social insurance landscape, pointing out common threads in the literature, established patterns in economic research, as well as the remaining gaps for further analysis.

Chapters 1 and 2 explore the impact of the Earned Income Tax Credit on different measures of household expenditures. 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. These papers provide evidence that these benefits occur by increasing nondurable expenditures for recipients. Both papers consider the same demographic group—low-income, single women—which has technical and empirical advantages for the context of my analysis. Focusing on single women is a critical contribution of these papers, as they tend to be economically vulnerable and have different spending patterns than married families or single males.

In Chapter 1, I examine the impact of the EITC on the food 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.

In Chapter 2, I examine how the Earned Income Tax Credit affects all nondurable expenditures, moving beyond only food expenditures. I use data with a monthly frequency, to better see expenditure patterns in the year following EITC receipt. Using the major expansion of the EITC in 2009, which increased benefits by $1025 (worth about $1250 in 2020) for one particular demographic group. This provides an ideal quasi-experimental setting to explore the effect of increasing the benefit amount for one group, with another unchanged, and very similar, comparison group. Using both a static and dynamic difference-in-differences design, I find an $86 increase in monthly nondurable expenditures, and then further decompose that category into key areas of spending. This includes grocery spending, where there is an increase of about $36 as well as total food spending, with an increase of about $65. These numbers are in line with the findings in Chapter 1. Studying how EITC receipt influences household consumption decisions can strengthen our understanding of the impacts of means tested government transfer programs and provide new evidence on consumption elasticities for low-income populations as an outcome of social insurance policies.

Finally, Chapter 3 summarizes the literature on US public food and nutrition assistance programs. This review focuses on the health and nutrition impacts of food assistance programs. We focus particular attention on the United States, both because of the plethora of types of programs and associated variation and because spending on these programs is a large share of the nonmedical safety net there. We begin by reviewing the theoretical predictions concerning health and nutrition effects of these programs, also paying attention to potential mediators such as education and income. We then discuss program eligibility and size, both as caseload and in terms of spending. We next touch on identifying causal variation and opportunities for further research. The review concludes by discussing the existing literature in five broad areas: take-up and use of the programs; effects on nutrition and food consumption; other immediate effects on short-run health; impacts on other contemporaneous outcomes such as income and labor supply; and longer-run health and nutrition effects.

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CHAPTER 1

How Does the Earned Income Tax Credit Affect Household Expenditures for Single Female Heads of Households?

1.1. 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 (Hoynes and Patel, 2018; Liebman, 1998), improvements in short- and long-run health (Evans and Garthwaite, 2014; Hoynes et al., 2015a), 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.

1.2. 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 (US 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 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.

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

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).

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

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. 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.

1.3. 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, 2006), income (Grogger, 2003), and a wide range of children's short- and long-run outcomes, including health (Evans and Garthwaite, 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

[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% (Scholz, 1990) and 1990, where estimates were between 80 to 86% (Scholz, 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 (Internal Revenue Service, 2002).

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.

1.4. 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, 2004). 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 observations 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.

[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.

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]

[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

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 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.

1.5. 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:

$$y_{hdst} = \beta \cdot MaxEITC_{dst} + \psi \cdot ifkids_{ht} + \gamma \cdot X_{ht} + \phi_t + \omega_s + \epsilon_{hdst} \tag{1.1}$$

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.

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

[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.
[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.

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 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:

$$y_{hst} = \sum_{k=-5,k\neq -1}^{5} \beta_k (year_{t+k} * ifkids_{ht}) + \sum_{k=-5,k\neq -1}^{5} \phi_k \cdot year_{t+k} + \psi \cdot ifkids_{ht} + \gamma \cdot x_{hst} + \omega_s + \xi_h + \epsilon_{hst} \tag{1.2}$$

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.

1.6. 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 Economic Research Service, 2022).

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]

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.

[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 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.

1.7. 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.

1.8. Tables

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) 1976	(2) 1986	(3) 1993 (0 vs 1+)	(4) 1993 (1 vs 2+)	(5) 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.9. Figures

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.

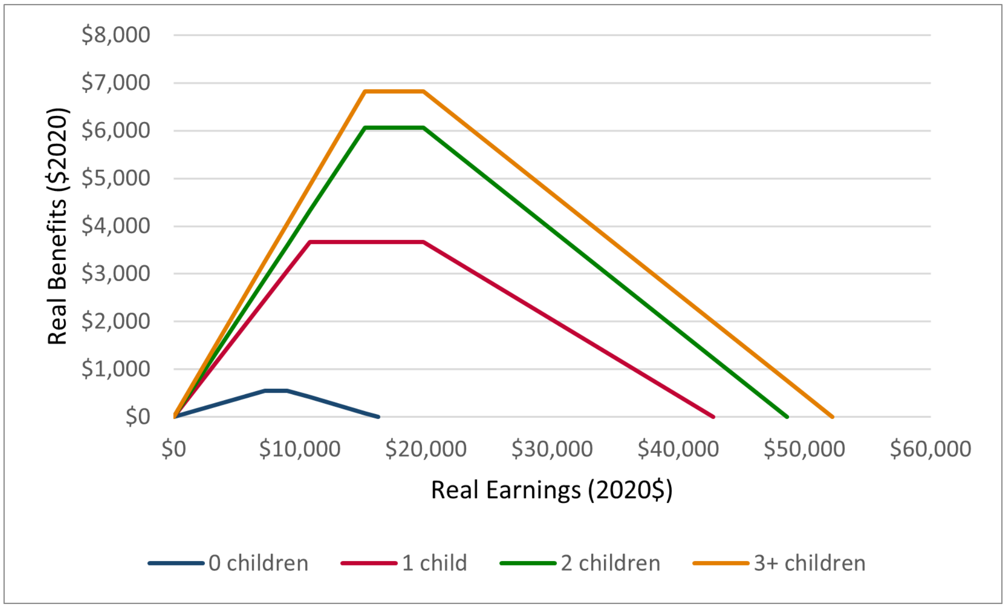

Figure 1b: Earned Income Tax Credit (2020$) benefit schedule for single earners at the expansion of the EITC in 2009. Families with incomes between $1 and $52,217 in 2020$, depending on the number of children in the household, were eligible for some amount of EITC benefits. There is a range of incomes 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.

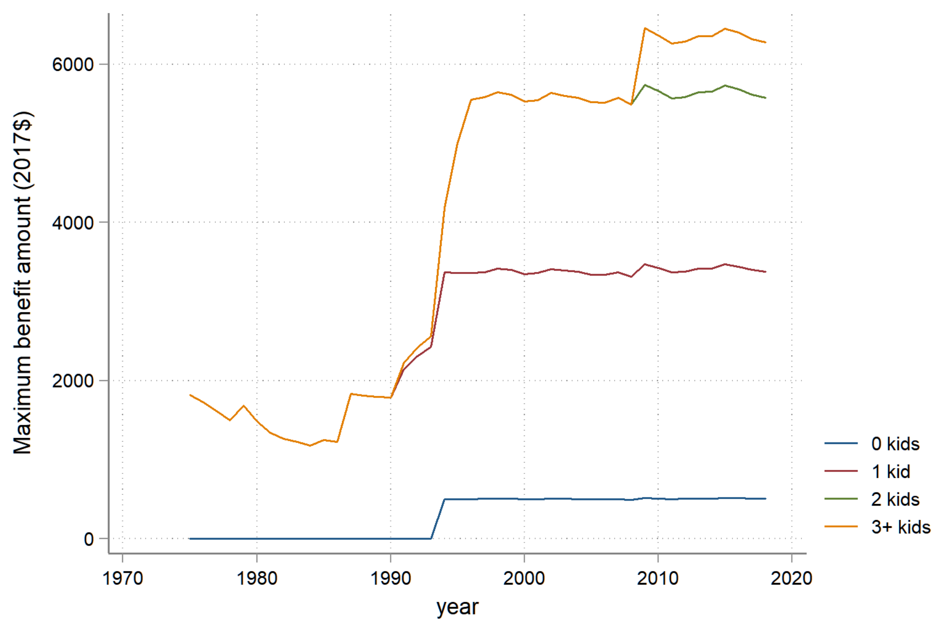

Figure 2: Earned Income Tax Credit (2017$) maximum federal benefit amount based on number of eligible children in household.

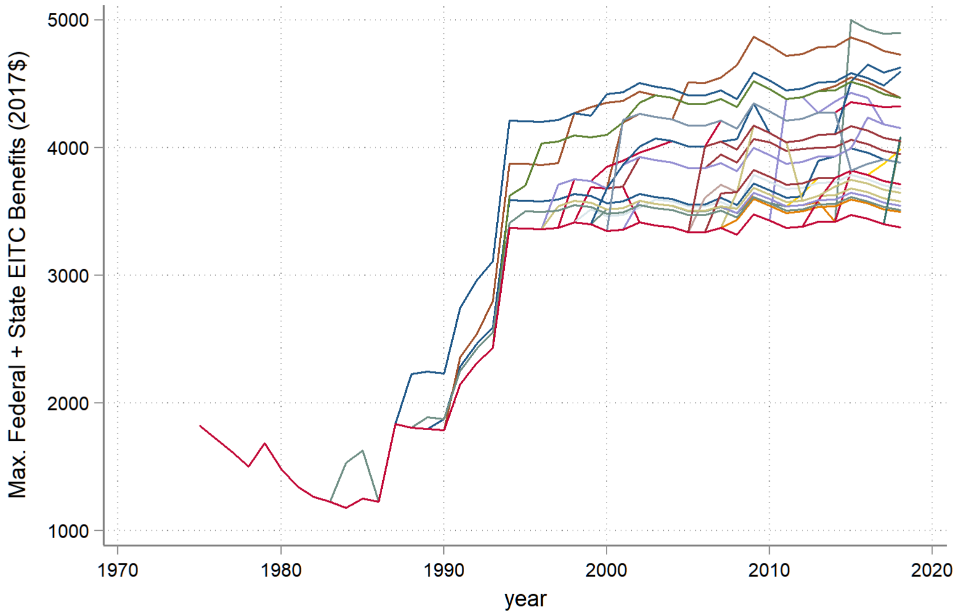

Figure 3a: Maximum state plus federal Earned Income Tax Credit benefit amounts (2017$), by state, for families with exactly one child in their household. Each line represents a different state, and the lowest line represents states with no state EITC credit.

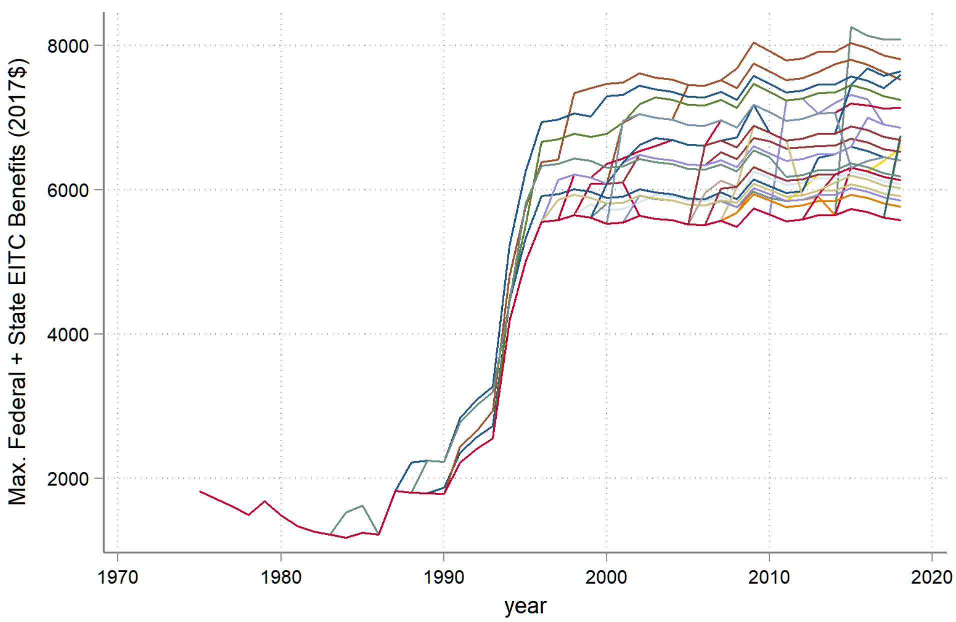

Figure 3b: Maximum state plus federal Earned Income Tax Credit benefit amounts (2017$), by state, for families with exactly two children in their household. Each line represents a different state, and the lowest line represents states with no state EITC credit.

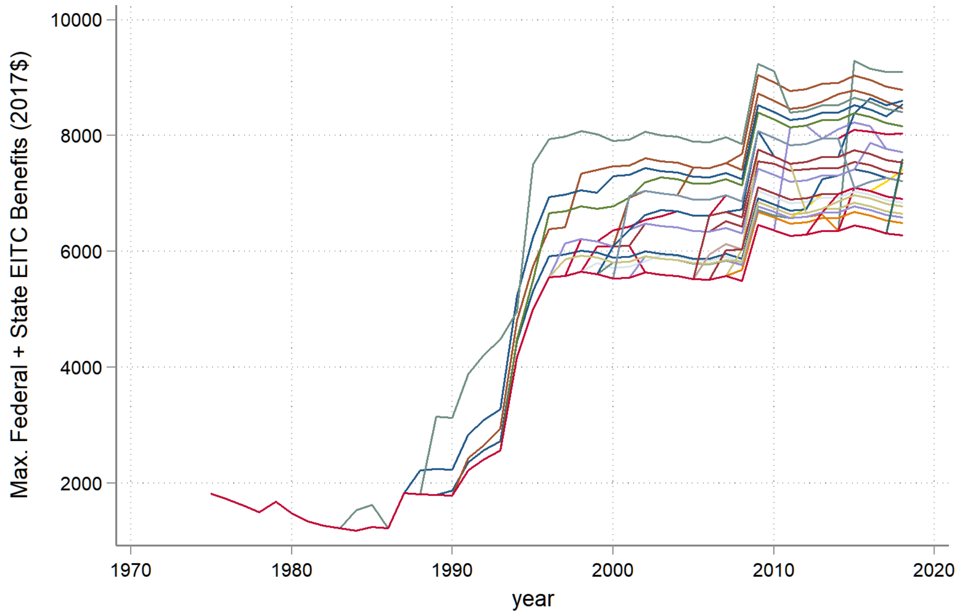

Figure 3c: Maximum state plus federal Earned Income Tax Credit benefit amounts (2017$), by state, for families with three ore more children in their household. Each line represents a different state, and the lowest line represents states with no state EITC credit.

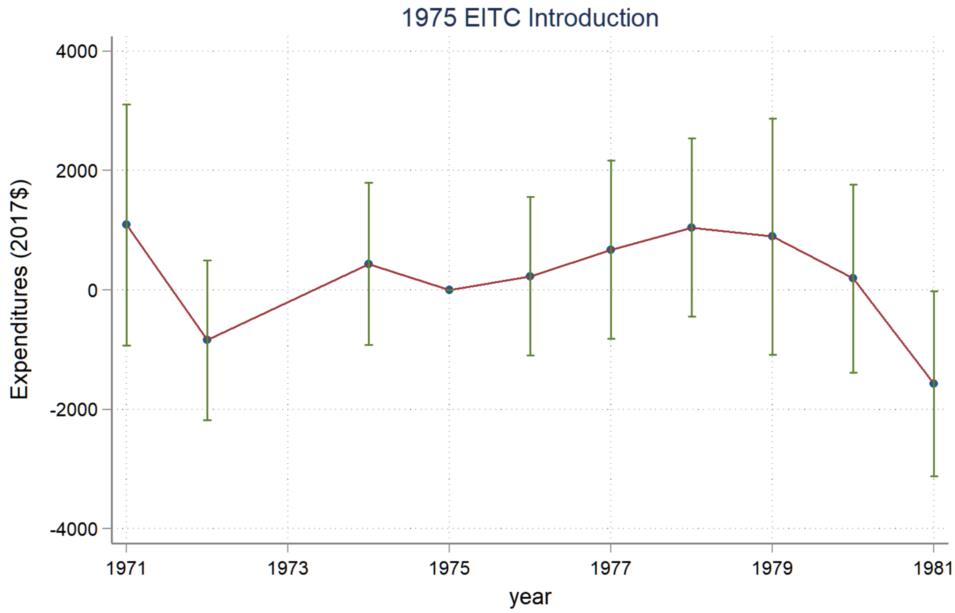

Figure 4: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1975 EITC introduction, effective for the 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.

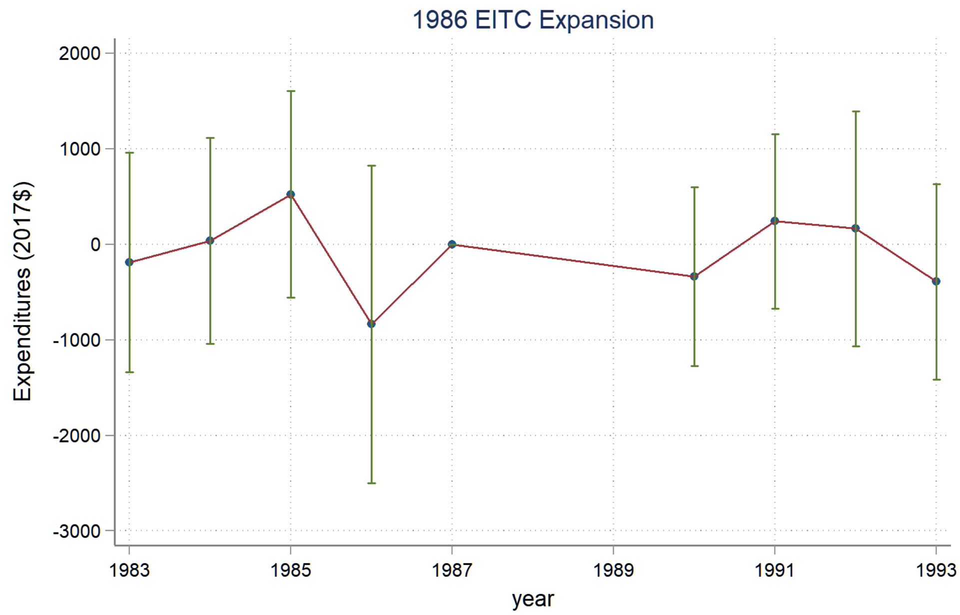

Figure 5: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1986 EITC policy change. Food expenditures 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.

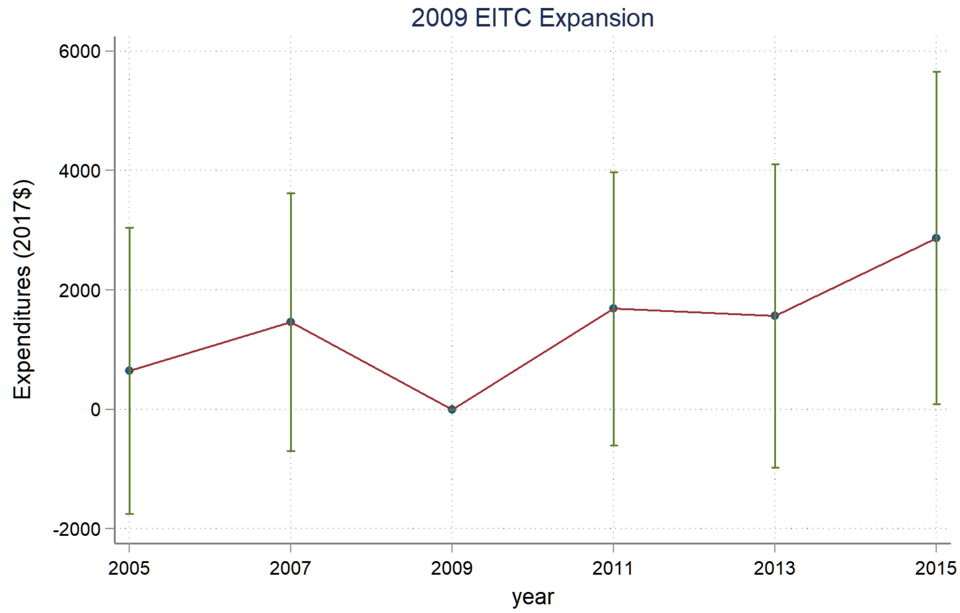

Figure 6: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 2009 EITC policy change. Food expenditures 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.

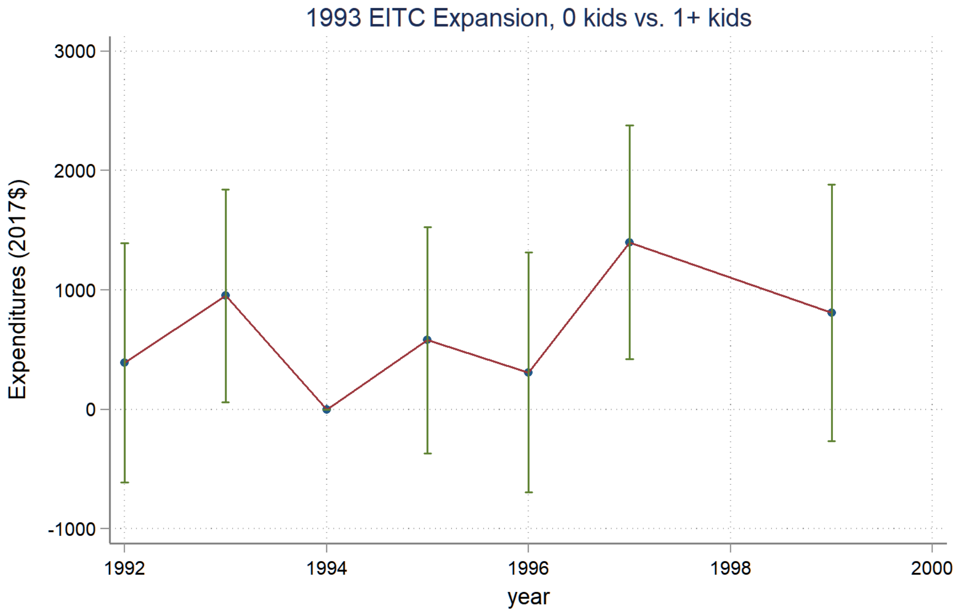

Figure 7: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures 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.

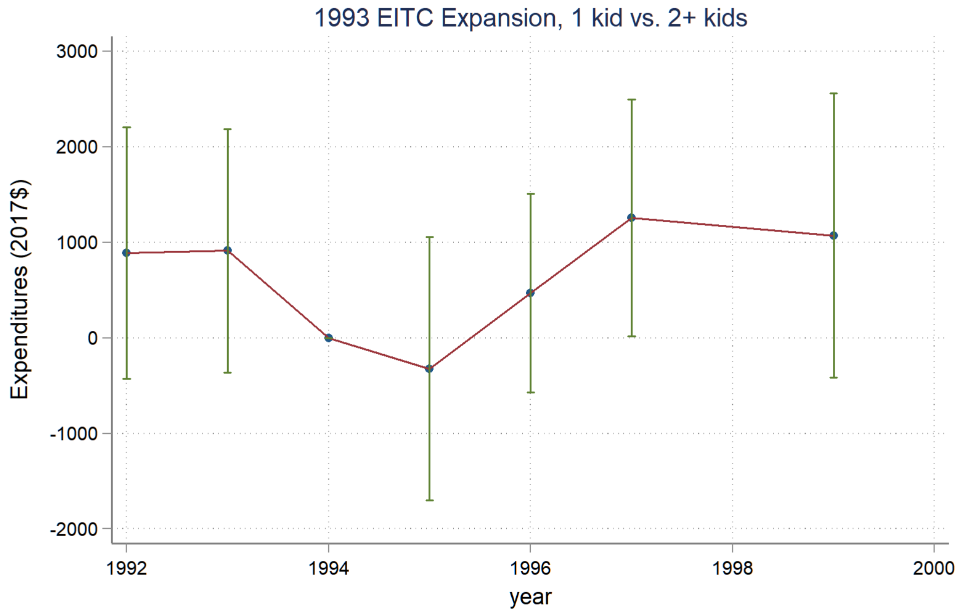

Figure 8: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures 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.

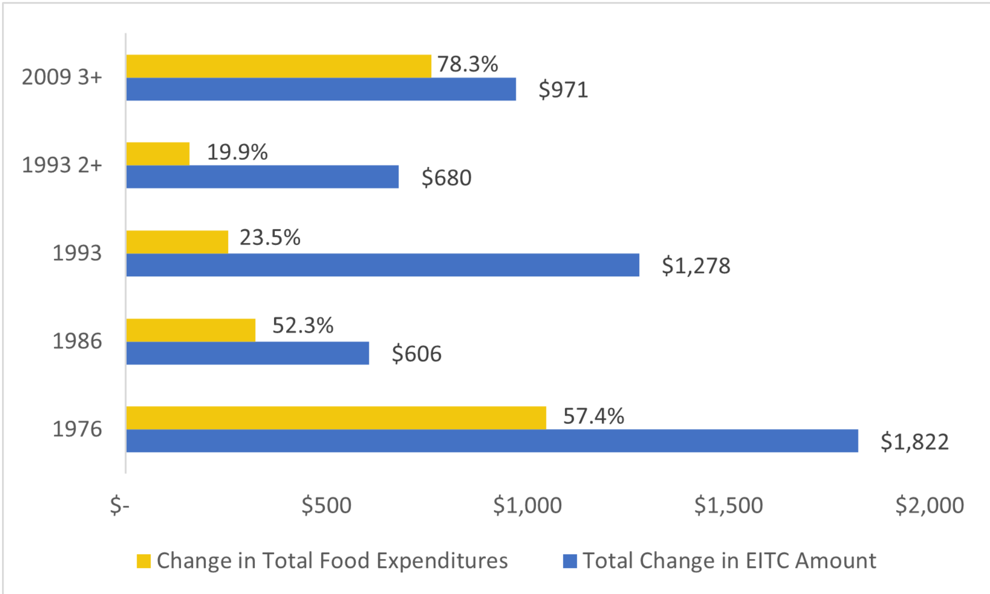

Figure 9: Static difference-in-differences coefficients shown as a percentage of the real total increase in maximum EITC benefits. Coefficients are taken from standard difference-in-difference estimates with the same controls variables and with treatment status defined in the same way as the dynamic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

CHAPTER 2

The Effect of Earned Income Tax Credit Receipt on High Frequency Expenditures

2.1. Introduction

Studying consumption responses is widely agreed to be the best way to understand the well-being of low-income households (Deaton, 2016). High frequency purchases throughout the course of the year are a more reliable measure for family welfare in lower-income families than measures of income. Understanding the consumption responses to government assistance programs, especially cash transfer programs, can provide key insights about whether the program is effective and how it is working to improve the lives of recipients.

In this paper, I study the Earned Income Tax Credit (EITC) and households' expenditure responses as a result of benefit receipt. The EITC is one of the largest pieces of the US safety net, and it is the largest means-tested cash transfer program in the US. Prior literature has estimated robust positive effects of the EITC on a wide array of outcomes, but less is understood about how benefits are actually used.

My research is the first to systematically examine the impact of the EITC on household non-durable expenditures throughout the course of the year. Most papers about the spending response to the EITC primarily identify changes in spending by comparing spending in EITC households to non-EITC households and looking at months when EITC receipt is high compared to months when EITC receipt is low. By doing this, they are estimating the immediate impact of EITC receipt on spending. My paper adds to the literature by studying the change in spending over the course of the year. I am able to fill in knowledge on spending other than right at the time of tax refund receipt. I am further able to provide evidence of consumption smoothing.

Furthermore, I am adding to the literature on an understudied population in the household finance literature in two accounts: women and low-income families. Less is understood about consumption patterns and expenditure responses in these populations. Bolstering that understanding for low-income populations is a key factor in better understanding how increases in resources can alleviate the burdens of poverty. Because many of the families participating in poverty reduction programs are single mothers, who have different spending behaviors and expenditure patterns than single men or married families, focusing on that population is also important to be able to better evaluate government assistance programs.

Because the EITC is such a large percentage of all U.S. safety net spending and because it reaches such a large number of families, understanding the program is integral to the policy landscape. Besides its policy importance, the EITC provides an ideal quasi-experiment to test economic theories in a subsample of the population who are less understood. I specifically focus on the 2009 expansion of the EITC, which sets up a clean identification of the impact of an increase in a lump-sum cash transfer. Families with three or more children received an extra $971 compared to families with one or two children. These two groups are otherwise fairly similar, and the large amount of additional money is a substantial increase in resources for those families.

There are many reasons why the financial decisions of lower income households are understudied, including the difficulty procuring high quality data on financial transactions in this population, in addition to the rarity of opportunities to find exogenous variation that is large enough to produce a visible effect. One major source of data for studying financial transactions, especially those that occur on a higher frequency basis, such as nondurable goods in general, is credit card transaction data (i.e. Pistaferri (2015)). This data inherently leaves out a disproportionate amount of low income households, who are less likely to use credit cards (Mills et al., 2022). In this paper, I use the Consumer Expenditure Survey (CE), which is a long running, nationally representative household survey that has monthly level expenditures on a comprehensive and detailed set of spending categories.

2.2. Setting

The Earned Income Tax Credit is a refundable tax credit targeting lower income working households, particularly those with children. It provides a substantial lump-sum cash amount to families with children and a modest amount to those without children. In 2020, the maximum amount of the federal EITC possible to receive was $6,660, plus more coming from states, depending on the rules in each state. Overall in 2020, around 25 million homes received around $62 billion worth of EITC refunds, with the average amount received about $2,461 (Internal Revenue Service, 2022). This accounts for over one in seven taxpayers, which shows that the EITC has a much wider reach than any other means tested safety net program.

Because of the importance of this program and the way that the policy changes in the program are ideal for identifying an impact, there has been a large literature on the EITC. The literature primarily focuses on labor supply effects, especially because the EITC is structured to have a work incentivizing impact. Some key papers in this area include Eissa and Liebman (1996), Meyer and Rosenbaum (2001), and Bastian (2020). There has been a general consensus that the EITC increases labor market participation, especially among single mothers.

Other work has consistently shown a wide variety of positive benefits from EITC receipt. A large literature exists showing that the EITC, and sometimes other large cash transfer programs, help alleviate poverty (Hoynes and Patel, 2018; Liebman, 1998), improve short-and long-run health (Evans and Garthwaite, 2014; Hoynes et al., 2015a), and increase educational attainment (Bastian and Michelmore, 2018; Chetty et al., 2011). However, less is understood about the mechanisms. Focusing on changes in expenditures after EITC receipt can unpack the channel these benefits work through.

This paper adds to the literature on spending responses to receiving the Earned Income Tax Credit. There are several key papers in this literature that examine the impact of EITC receipt on spending. The first group of papers in this literature examines durable spending, particularly because early qualitative research on EITC recipients identified that they were likely to say that they wanted to purchase cars or large appliances right after receiving their tax refunds. Barrow and McGranahan (2000) find that there is 9% more spending on durables. The find that about a fifth of the total tax benefit is spent right away, which leaves an additional 80% that is spent throughout the rest of the year, providing initial evidence for consumption smoothing.

Goodman-Bacon and McGranahan (2008) use a similar identification strategy but look at a wide range of spending categories. They look at durables, nondurables, and services, including detailed subcategories. Of particular interest to this paper, they find a small but statistically significant effect for all nondurables, with notable increases in spending for food and children's clothing, comparing a high EITC month (February) to a low EITC month (September).

Other research finds impacts for increasing savings (Jones and Michelmore, 2018) and decreasing debt McGranahan (2016).

In this paper, I will be using a natural experiment that arises because of changes in EITC policy that impacts only some demographic groups. This is beneficial for a number of reasons. First, I will be able to study the impact of spending throughout the course of the year. Although it makes sense for large purchases, durable expenditures such as cars or appliances, to be made immediately upon receipt, nondurable purchases are not the same. They are made throughout the course of the year so it would make sense for there to be some amount of consumption smoothing. I would like to test for that possibility. Furthermore, there is an ideal comparison group in the 2009 EITC expansion, because it increased more for certain demographic groups but not others. This way, there is a natural treatment and control group that are otherwise similar, besides the number of children in the household.

The EITC began in 1975 in a much simpler form than how it currently exists today. Figure 1 (a) shows the benefit schedule for the EITC in 1975 when it was first introduced as a tax refund to working households with children. It originally was a 10 percent credit on the first $4,000 (worth around $20,000 today), and then it phased out over the next $4,000 of earnings, with a maximum credit of $400 (around $2000 today).

Figure 1 (b) shows the benefit schedule today. This is the benefit schedule that has existed since the last change in the law in 2009, which introduced an additional higher credit for families with three or more children compared to those with fewer children.

The change in complexity from Figure 1 (a) to (b) is a visual example of the change in policy variation that allows for clean identification of the effect of changes in EITC benefit amounts on spending outcomes. Table 1 shows all of the major policy changes with the maximum benefit amount in the policy change years, as well as the change in benefits from the previous year caused by the policy change.

2.3. Data

The Consumer Expenditure Survey is a household survey with detailed expenditure information for a nationally representative sample of U.S. families. Households are interviewed quarterly and provide monthly level information on a full set of narrowly defined expenditure categories, composed of around 500 different commodities. This data also comes with demographic information about household members, including education and family composition. Because of the level of specificity of the spending variables, the CE is widely used to determine measures of consumption.

I use monthly data on nondurable spending from 2007 through 2011, the years surrounding the 2009 expansion of the EITC. I define nondurable spending based on how the CE is commonly classified in the literature (i.e. Coibion et al. (2021)). Expenditures that fall under the following categories are defined as nondurables: food, alcohol, tobacco, clothing and clothing materials, accessories, gasoline and other energy goods, household products[1], and prescription drugs.

There are several nondurable categories that are of particular interest, which warrants further study. The first is food spending. Food spending is the largest proportion of nondurable spending. It also is closely tied to food insecurity and nutrition in this population, lower educated single mothers. Furthermore, the EITC has a work incentivizing design, and the literature finds a large effect for single mothers entering the workforce as a result of EITC policy (Bastian, 2020), though it has been established that this effect was lower as a result of the 2009 EITC expansion (Schanzenbach and Strain, 2020). Increase in working hours means that single mothers would have less time to cook, which could lead to a much larger increase of restaurant spending than grocery store spending. If the increase in food spending was primarily coming from restaurant spending, we might be less concerned about food insecurity in this population, given that they are able to shift spending to more expensive restaurant foods.

[1]Personal care products, household linens, cleaning products, pets and related products, film and photographic supplies, newspapers and periodicals, flowers and plants, and eyeglasses.

Another category that comes up in policy debates is alcohol and tobacco. The final category to look at separately is children's clothing, with a comparison to all clothing spending. There is evidence from Goodman-Bacon and McGranahan (2008) that there are increases in children's clothing as a result of EITC receipt. Overall, anecdotal evidence tends to say that mother's spend influxes of money in ways that are beneficial to their children, which would include children's clothing.

Figures 2 (a) through (f) show a time series of the means of each of these categories (grocery store, all food, alcohol and tobacco, clothing, children's clothing, and all nondurable spending), comparing the group impacted by the policy change (single women in the sample with three or more children) to those not impacted (those with one or two children). Each figure has a line at February 2010, which is the month when the majority of EITC recipients would be receiving their refunds for the first time after the 2009 expansion.

Although monthly expenditure numbers are noisy, there are some general takeaways from these figures. First, there are generally the same patterns in spending between single mothers with three or more children and those with one to two children. Although the levels are higher for most spending categories for households with more children, the overall patterns tend to look the same. One of the main pieces of information this conveys is a sense of whether to be concerned about parallel trends in spending between the two demographic groups being compared. The unconditional averages in spending between the two categories can give some information about how similar spending patterns are in the two groups before the policy change happens. In Figure 2 (b), which shows grocery store spending, there looks to be similar trends over time. Although there is some noise, the two groups generally move together over time, showing similar seasonal patterns in spending. When adding in all other types of food spending, most of which is restaurant spending, in Figure 2 (c), there is less of a parallel pattern. This could indicate that households with three or more children have different and less comparable restaurant expenditure patterns. For this reason, grocery store spending could be a better category to use to estimate the impact of the policy change on food spending.

Additionally, there is a visual increase in most categories right after February 2010 for those with three or more children compared to those with one or two. This is most visually apparent in the graph for children's clothing. This is initial evidence of an effect, but more sophisticated econometric techniques will reveal more about this effect. Furthermore, there is some evidence of seasonal patterns in spending. One is of more food spending in December, when many families are celebrating holidays, and another is shown as a large spike in August for children's clothing, when most back-to-school shopping is done.

Thinking more about the sample used for this paper, there are several theoretical and economic considerations for how to best construct the sample. There is no measure of actual EITC receipt in the CE, so all analysis needs to be done using some estimate of EITC receipt. By focusing on the natural experiment caused by the increase in EITC benefits for only one demographic group in 2009, I avoid the need to estimate the exact dollar amount of EITC receipt, or even the probability of EITC receipt. This is important for this paper, because the outcome variables of interest are different measures of nondurable spending. There is an issue with endogeneity of the treatment variable, if the treatment is EITC receipt estimating using family income, to the outcome variable, spending, which is highly correlated with family income. Instead, to avoid that source of bias, I choose a demographic group with high EITC receipt and assume that everyone in that demographic group is receiving the EITC.

The demographic group I choose is single women with some college or less education, a group that has high eligibility and participation for the EITC. Although there are no current estimates of exactly the number of people in this group currently receiving the EITC, Hoynes and Patel (2018) estimate that this group has the highest eligibility for the EITC, and women with more education are substantially less eligible. The share of women with exactly some college education, without a degree, that are eligible for the EITC in 1996 is around 68%. This drops off to 47% of women with a college degree with EITC eligibility. For all single women with some college or less education, the average eligibility is around 75%. There is no existing estimate for take-up among those eligible for the EITC by subpopulations. One estimate from the IRS in 2017 shows close to 80% take-up among those eligible for the EITC in the whole population, not just single women. Additionally, around 60% of all EITC recipients were single in 2008, and about 40% were single with children in the household Athreya et al. (2010). So single women with lower levels of education have high eligibility, and single mothers are a large proportion of EITC recipients.

I further limit the sample to women from ages 24 to 48. This is a portion of the population who is most likely to have children under the age of 18 living at home, plus most likely to be finished with school and working. Women at these ages are also most comparable to each other. Overall, this sample is most likely to be both eligible for the EITC and taking up the EITC, plus to have children of age and be working.

Table 2 presents means for a set of variables for the whole sample, then for the control and treatment groups. The sample is as described above, the control group is women with one or two children, and the treatment group is women with three or more children. As is shown in the table, the treatment and control group are very similar in terms of observable characteristics. Women with fewer children tend to be slightly more educated, but otherwise are statistically the same across other characteristics.

Although there are many benefits as described about using this data, there are some drawbacks. I am not able to use geographic identifiers, as many states are suppressed for data confidentiality reasons. The CE has 17% of state codes suppressed, with an additional 4% recoded. Although I am only using federal variation in the EITC in this paper, adding state information to account for state trends could have been a benefit. Another drawback is the way that food stamps spending is accounted for. According to the US Department of Health and Human Services[2] around 76% of single parents are eligible for the Food Stamps Program. This means many households in the sample are likely using Food Stamps to supplement their grocery shopping. In the CE, food stamps spending is reported at the yearly level, so it cannot be used at a monthly frequency.

All spending data is inflation adjusted and presented in terms of 2020 dollars. Food expenditures are deflated using the consumer price index (CPI) food and beverage series (f&b) and all other spending is deflated using base CPI, both obtained from the Bureau of Labor Statistics.

2.4. Empirical Model

There are two main empirical models used in this paper, in order to best use one of the unique benefits of the CE data, which is the monthly frequency of the data. The first is a set of static difference-in-difference regressions and the second is a dynamic version of the same regressions. The

[2]https://www.aspe.hhs.gov/reports/public-assistance-use-among-two-parent-families-analysis-tanf-food-stamp-program-eligibility

static regressions account for the average increase in spending over the rest of the year after EITC receipt and the dynamic versions allow us to better understand the month-by-month spending trends as a result of receiving a large lump sum of cash at one time.

The static difference-in-difference regressions are the following:

$$y_{it} = \beta \cdot (PostExpansion_t * treatment_{it}) + \phi \cdot PostExpansion_t \\ + \psi \cdot treatment_{it} + \gamma \cdot x_{it} + \omega_t + \epsilon_{it} \tag{2.1}$$

Where y is one of the different outcome variables of interest, which are all different categories of nondurable spending. These are grocery store spending, all food spending (including restaurants), alcohol and tobacco, clothing, children's clothing, and all nondurables. The equation is estimated for individual household i in month t. The variable $PostExpansion$ is an indicator for whether the month is after February 2010. Although the expansion passed in 2009 and was in effect for tax year 2009, families received their refunds in 2010. The modal month of EITC receipt is February 2010, so I am assuming that everyone is receiving their refund in February 2010. Goodman-Bacon and McGranahan (2008) show that around 60% of EITC recipients received their refunds in February in 2005, with the second highest month of receipt being March at around 20%. The treatment in this case is based on the parameters of the EITC policy change in 2009. For this policy change, only families with three or more children were impacted, so they are the treatment group. In order to construct a control group that is similar, I use families with one or two children. I also control for demographic characteristics, x_i, which include age and race of the mother, number of children in the household, number of people over 64 in the household, to account for some households that have grandparents living with them, and month and year fixed effects.

The range of these regressions is 11 months before and after February 2010. This is to get the result of the average increase in expenditures over the next year until the next round of refunds, but ending after 11 months to avoid contaminating with the impact of the EITC receipt in the previous and the following year.

The second set of regressions is a dynamic difference-in-difference. It examines the monthly impact of the policy change on the same expenditure categories as the previous estimations, instead of the yearly average. These regressions follow a full year before and after the policy change. The regression equations are the following:

$$y_{it} = \sum_{k=-11,k\neq -1}^{10} \beta_k \cdot (month_{t+k} * treatment_{it}) + \sum_{k=-11,k\neq -1}^{10} \phi_k \cdot month_{t+k} + \psi \cdot treatment_{it} + \gamma \cdot x_{it} + \epsilon_{it} \tag{2.2}$$

The same details that apply to the previous equation also apply to this one.

2.5. Results

One of the main benefits from using the CE data is to have a high frequency series of expenditure data, which can help us better understand the dynamics of a spending response to receiving a large amount of cash. The prior literature has used different methods to establish that spending does increase in the month following EITC receipt, but those methods do not allow further analysis of what happens in the months afterwards. Using the quasi-experimental variation in the 2009 EITC policy expansion of the credit for one particular group of recipients, I can answer the question of how spending dynamics change for impacted households in the following year after an increase in EITC benefits.

Before looking at the dynamic results, it is informative to understand the overall changes in spending over the course of a year following the change in EITC policy. The first set of tables present the results of the difference-in-difference regressions that estimate the impact of the change in EITC policy that increased benefit amounts on different spending categories. Each of these categories represents a portion of nondurable spending of interest. There is a focus on food spending to understand more about how the EITC impacts food insecurity and nutrition, as has been explored in prior literature. Children's clothing is additionally of interest because of findings of increased spending in this category in previous studies. Because the prior work (i.e. Goodman-Bacon and McGranahan (2008)) relies on comparing spending only in the month of February, it misses the month with the highest amount of spending on children's clothes, which is August, prior to the beginning of the school year. Using the yearly average, as in my paper, allows for a more accurate account of spending in this category.

Results are presented in Table 3. There is a large and statistically significant increase in spending in all food categories, including grocery store spending, grocery store plus restaurant spending, and total food spending, and all nondurable spending. Food expenditure are the largest piece of nondurable spending, nondurables as a whole increase by around $11 extra. There is a small, but not significant, increase in alcohol and tobacco spending. There is no evidence of an increase in total clothing spending, but possibly a small, but not statistically significant, increase in children's clothing specifically.

This points to a large increase in food spending over the course of the year for single women with three or more children compared to single women with one or two children. More than half of the increase is coming from grocery store spending, and the rest from food purchased at restaurants, eaten at school, and consumed on trips.

The next set of results are from the dynamic difference-in-difference regressions. They are presented in Figures 3 (a) through (f). The results from these sets of regressions point to no statistically significant monthly effect of the increase of EITC benefits on these spending categories.

There are several conclusions to be drawn from these results. First of all, because there is an effect seen in the static difference-and-difference estimation, this is evidence of a small increase in expenditures smoothed over the course of the year. This is good evidence of consumption smoothing behavior that is too small month-by-month to be picked up in these dynamic regressions but average out to an impact that can be seen over the course of the year. The literature shows that there are increases in savings and checking accounts as a result of EITC receipt (Jones and Michelmore, 2018), which could be a likely mechanism for helping to consumption smooth throughout the course of the year.

The magnitudes of the static estimates can be compared to the magnitudes seen in the dynamic regressions. I will focus on grocery store spending. As described earlier, grocery store spending is shown to have relatively parallel pretrends in the control and treatment group prior to the policy change. The static estimate for grocery spending is around $36. This is an average of the 11 months following EITC receipt compared to the 11 months prior. To compare this to the dynamic estimates, grocery store spending jumps up about $25 right away, there's a small dip and then a jump again to between $45 to $55 per month. Doing a back of the envelope calculation, the dynamic results average out to about the same as the static results. This is good evidence that both estimates are picking up on the same changes in spending, but the dynamic estimate is providing more information about the month-by-month spending patterns. If there is more work done on controlling for seasonal factors that are influencing grocery store spending, this points to the dynamic results as providing more detailed information about spending patterns over the course of the year.

Another conclusion, and direction for further research, is that there are seasonal spending patterns at play that need further disentanglement. Only using one year before and one year after is not enough to fully control for seasonal variation in spending between the two groups. The place where it is most apparent is in Figure 3 (e), children's clothing. There is a spike in spending on children's clothing in August, which, as discussed, is a result of back-to-school shopping patterns. Families with more children are going to spend more money on children's clothing, so the treatment group is spending much more on clothing in August than the control group, which has fewer children by definition. This is visually apparent in the figure.

One last conclusion about these results are that there are many idiosyncrasies in spending between families that might be averaging out to look like zero, when there are actually some families who are distinctly spending more. The ideal data would be able to follow every family over two years, which would enable running the regressions separately for each household to estimate a cleaner effect. The CE is a short panel, so families stay in the survey for one year, and one fourth of the survey population rotates out every quarter. Unfortunately, this means that using that short panel for a two year time period would be impossible.

2.6. Conclusions

This paper establishes that there is an increase in nondurable on average over the course of the year following an increase in EITC benefits. I am unable to make any conclusions about monthly spending patterns, as effects are small and not statistically significant. There are several further directions that research can take to understand more about this topic.

First, 2009 is in the middle of a recession. Families might generally be making fewer expenditures than usual. Using a different policy change during another year could help with this issue. The 1993 expansion of EITC is a popular policy change to use to study the impacts of the EITC.

The next is to use other spending categories, including durables, services, savings and wealth. Other papers have seen effects in these categories, so testing to see whether there is any apparant monthly changes in spending over the course of the year could help fill in more in this literature.

The last major consideration is to use more sophisticated methods for seasonal adjustment. Because there are obvious patterns in seasonal spending, this could be detracting from the overall trend.

Overall, this paper provides evidence about a sustained increase in certain nondurable categories and helps fill in knowledge about how the EITC impacts recipients in the months following benefit receipt.

2.7. Tables and Figures

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

	Full Sample	Control	Treatment
Age	36.09	36.58	34.88
White	0.76	0.77	0.74
Black	0.19	0.19	0.21
Other Race	0.05	0.05	0.05
Less than HS	0.25	0.21	0.32
HS Diploma	0.39	0.40	0.36
Some College	0.37	0.39	0.32
# in Family	4.12	3.53	5.56
# Earners	1.54	1.57	1.48
# Children	2.12	1.53	3.56
# Age 64+	0.03	0.03	0.02
# Observations	38,533	27,367	11,166

Table 3: Coefficients for the Regressions on Various Spending Categories

	Grocery	All Food	All Nondurables	Alcohol and Tobacco	Clothing	Children's Clothing
	36.091***	64.775***	85.972***	6.817	0.534	9.117
	(12.792)	(15.992)	(25.853)	(4.584)	(16.664)	(12.487)
Observations	14494	14600	14600	14600	8612	5473
R^2	0.115	0.095	0.095	0.039	0.053	0.095

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

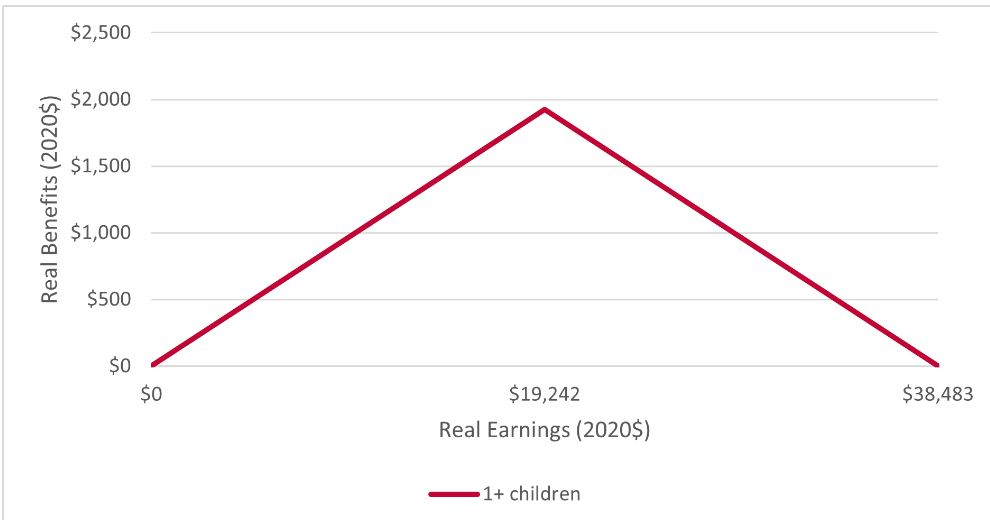

Figure 1a: Earned Income Tax Credit (2020$) benefit schedule at the introduction of the EITC in 1975. Families with incomes between $1 and $38,483 in 2020$ were eligible for some amount of EITC benefits. Only families who had earnings of exactly $19,242 were eligible for the maximum benefit amount in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

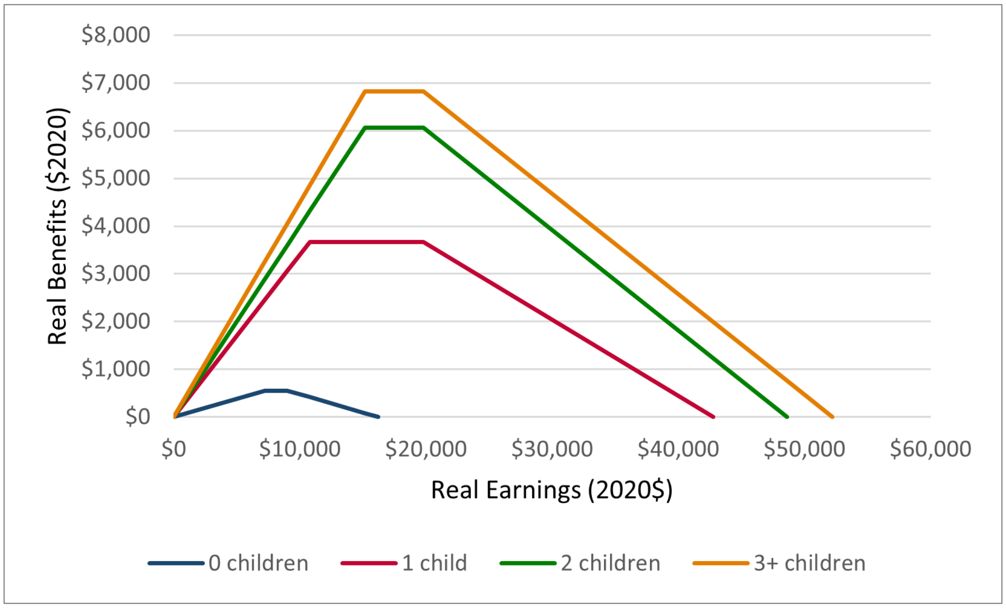

Figure 1b: Earned Income Tax Credit (2020$) benefit schedule for single earners at the expansion of the EITC in 2009. Families with incomes between $1 and $52,217 in 2020$, depending on the number of children in the household, were eligible for some amount of EITC benefits. There is a range of incomes 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.

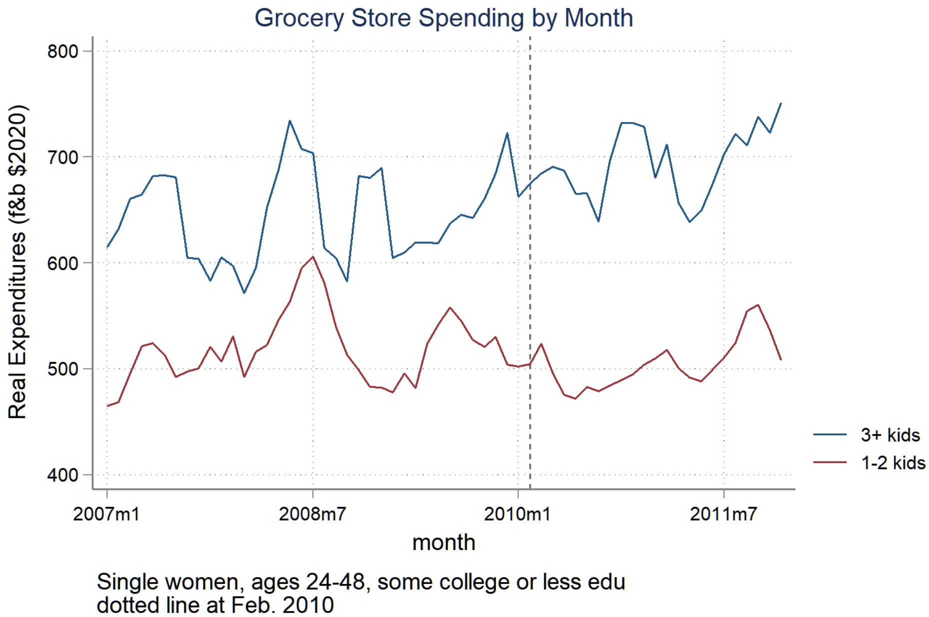

Figure 2a: Mean grocery store expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The group impacted by the 2009 policy change is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

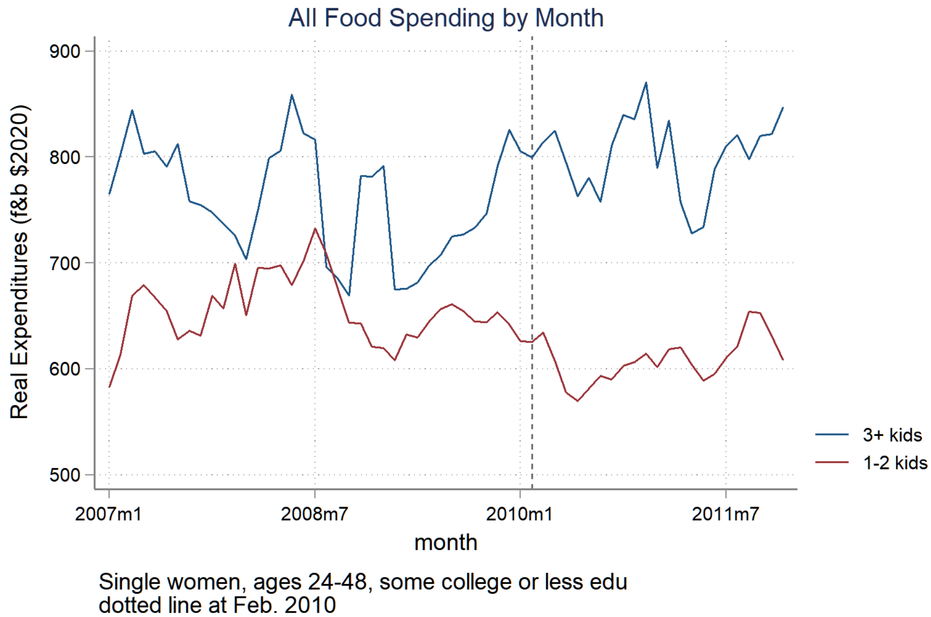

Figure 2b: Mean total food expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The group impacted by the 2009 policy change is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

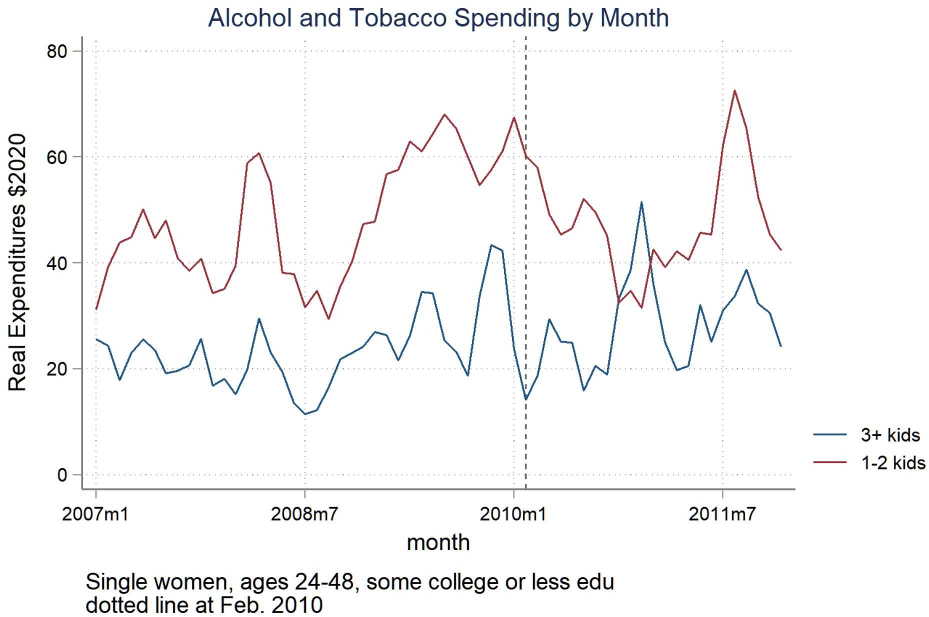

Figure 2c: Mean alcohol and tobacco expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The group impacted by the 2009 policy change is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

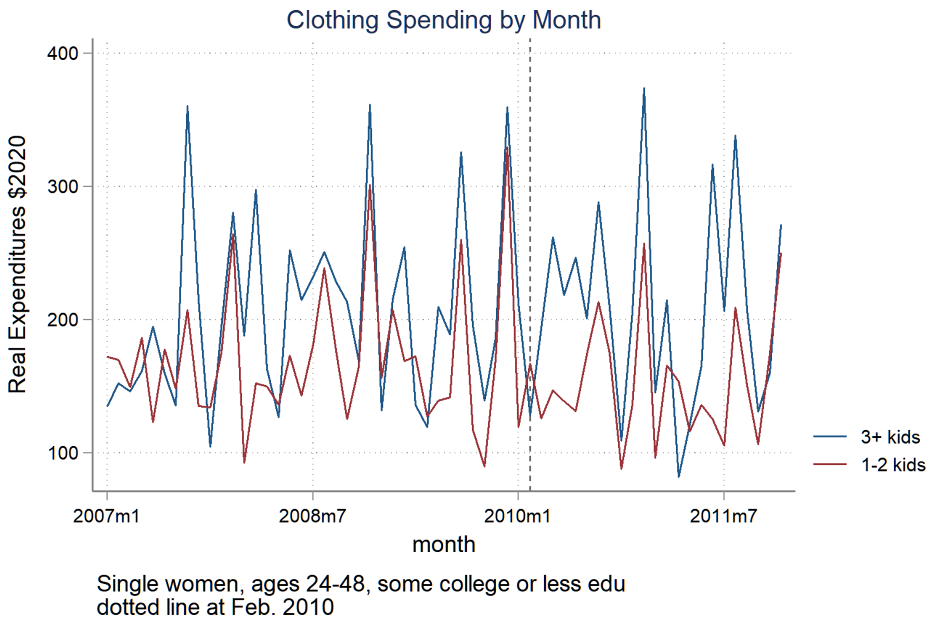

Figure 2d: Mean clothing expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The group impacted by the 2009 policy change is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

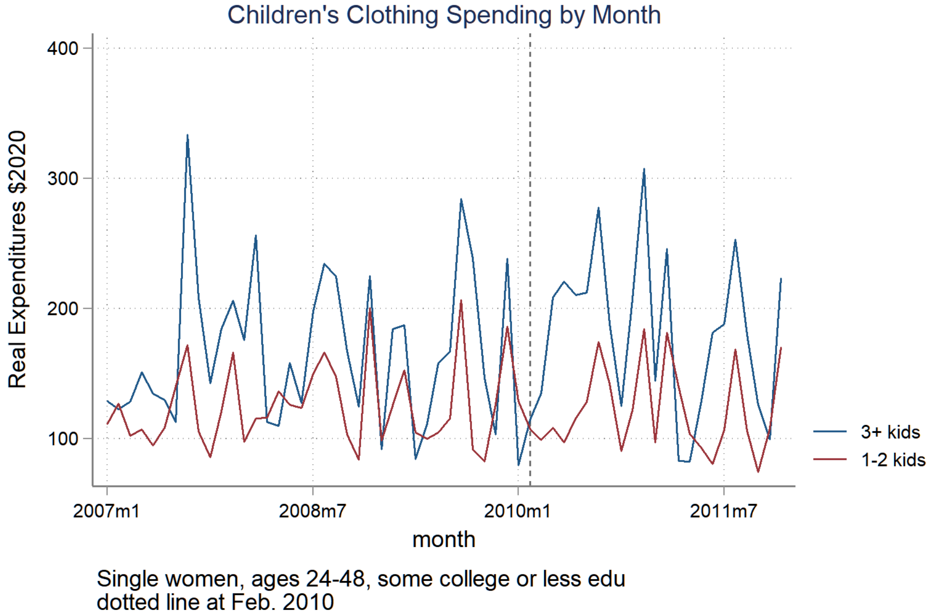

Figure 2e: Mean children's clothing expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The group impacted by the 2009 policy change is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

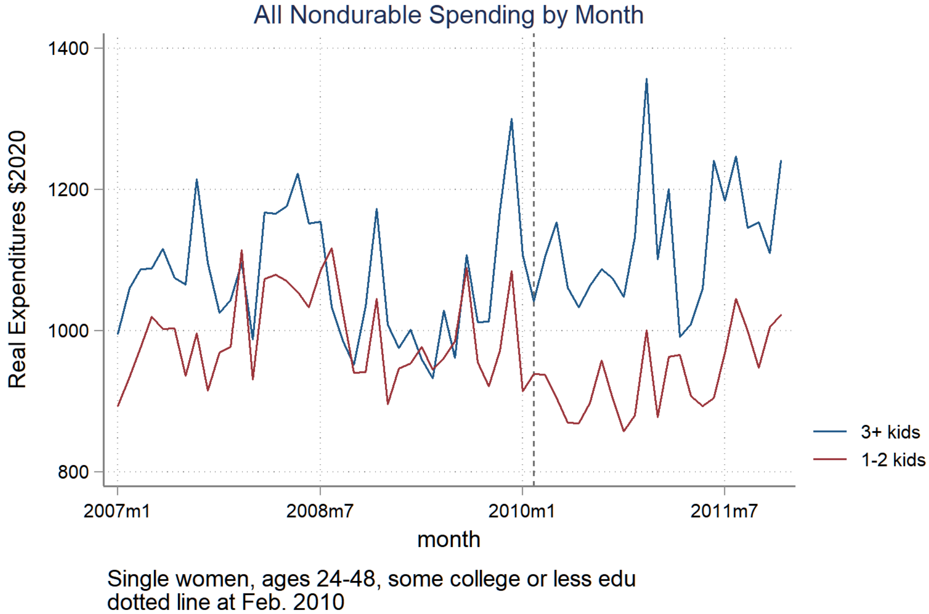

Figure 2f: Mean total nondurable expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The group impacted by the 2009 policy change is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

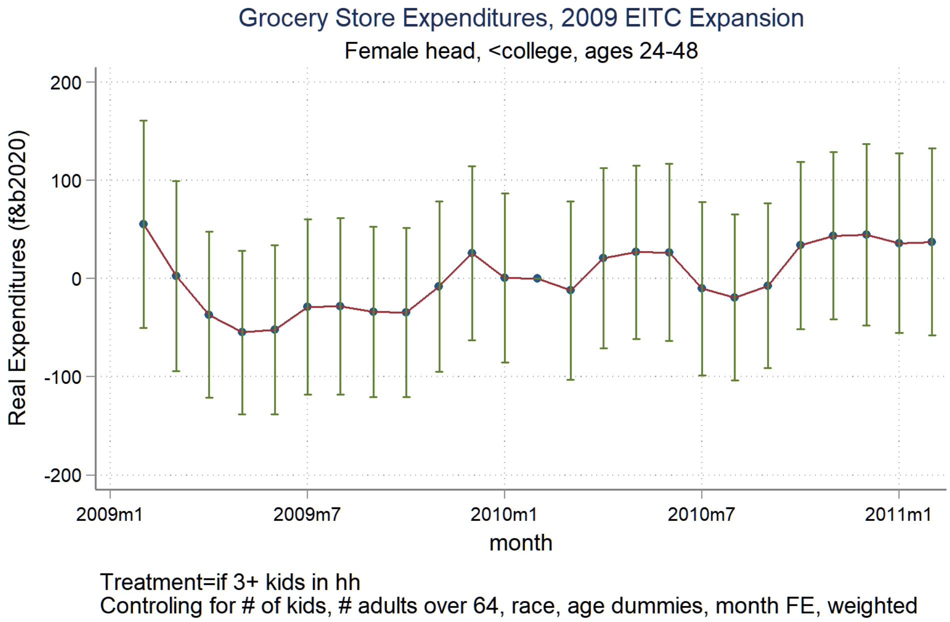

Figure 3a: The results from the dynamic difference-in-difference estimation for grocery store expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The treatment group is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

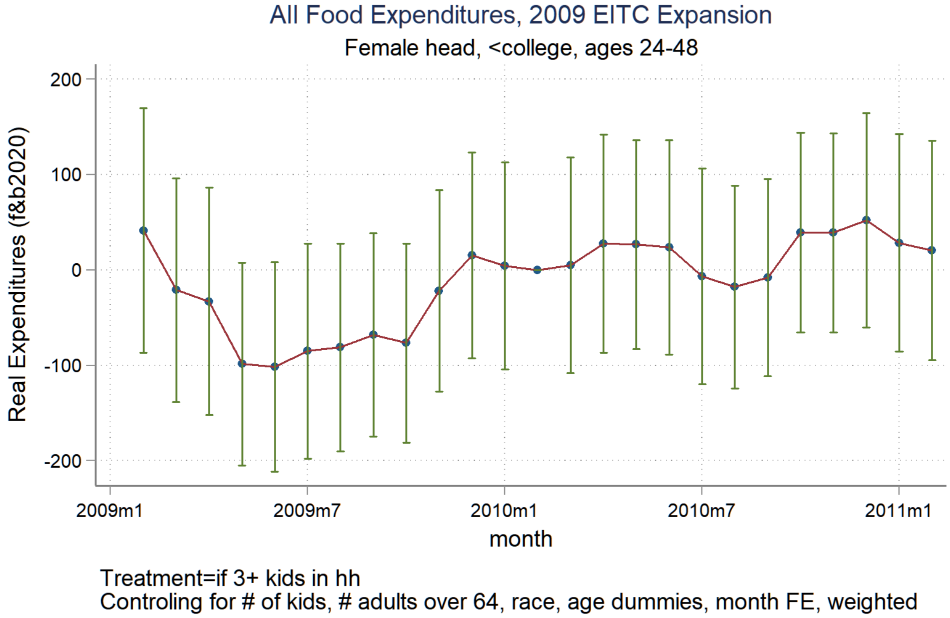

Figure 3b: The results from the dynamic difference-in-difference estimation for total food expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The treatment group is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

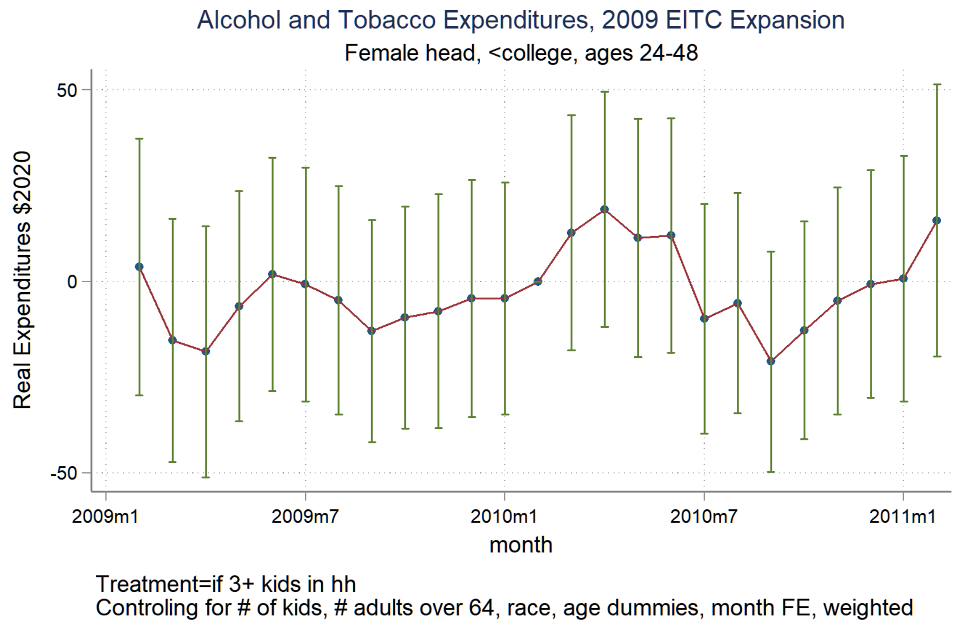

Figure 3c: The results from the dynamic difference-in-difference estimation for alcohol and tobacco expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The treatment group is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

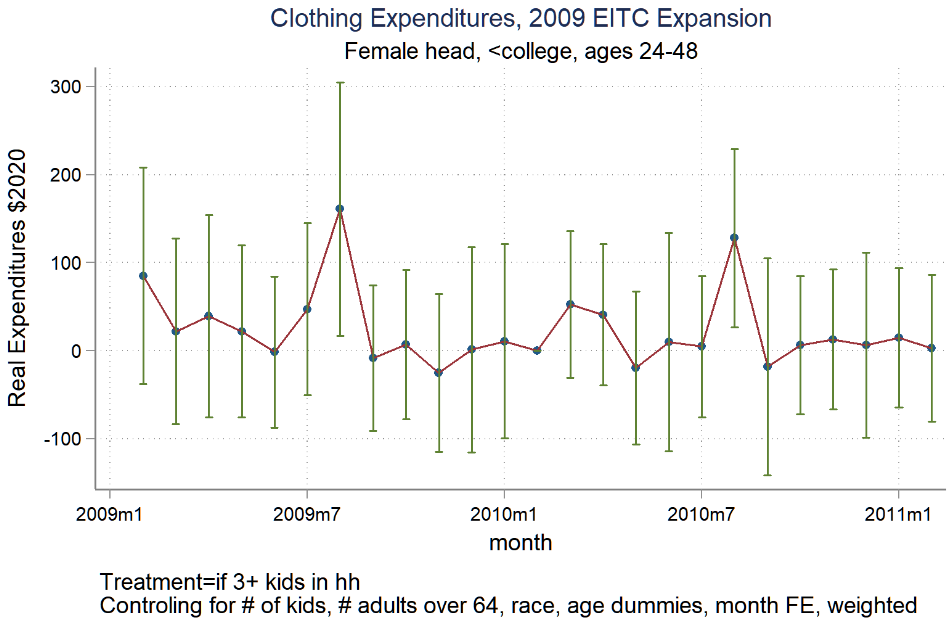

Figure 3d: The results from the dynamic difference-in-difference estimation for clothing expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The treatment group is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

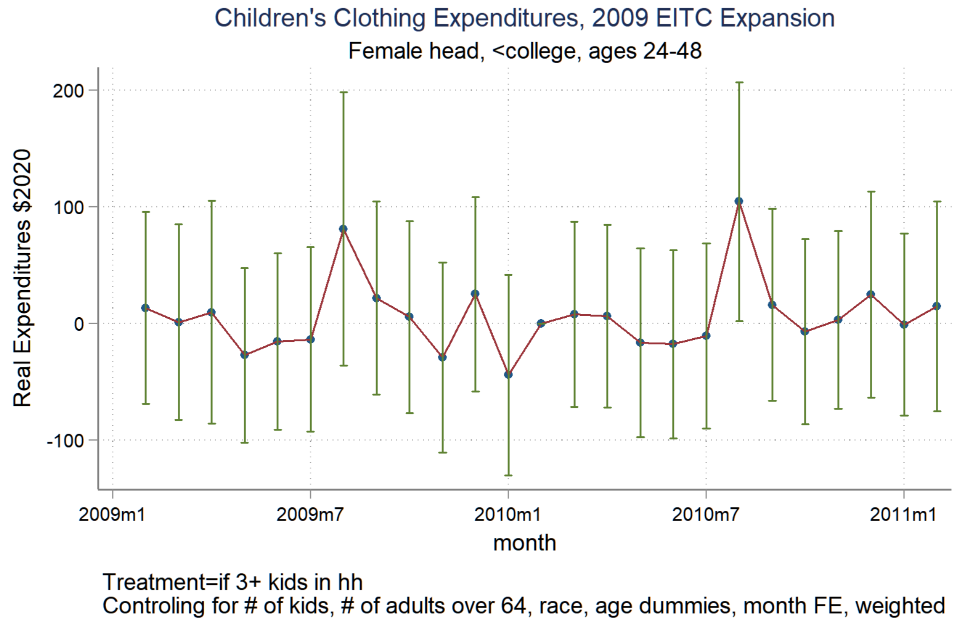

Figure 3e: The results from the dynamic difference-in-difference estimation for children's clothing expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The treatment group is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

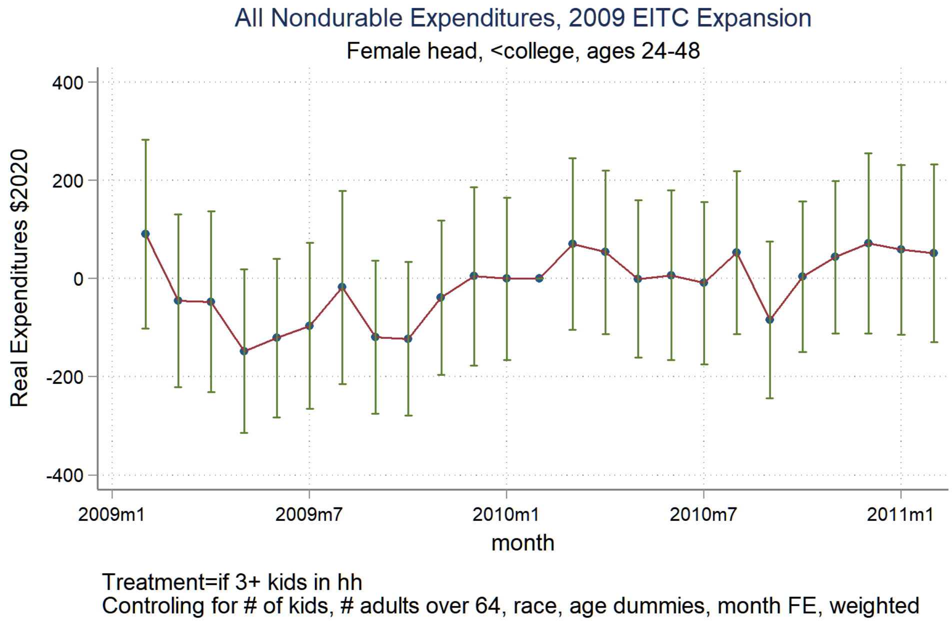

Figure 3f: The results from the dynamic difference-in-difference estimation for total nondurable expenditures for single female headed households (head of household between the ages of 24-48 with some college or less education) around the 2009 EITC expansion. The treatment group is households with three or more children ages 0-18 and the control group is households with one or two children ages 0-18.

CHAPTER 3

Health Impacts of Food Assistance: Evidence from the United States

3.1. Introduction

This review summarizes current research on the health effects of food assistance programs in developed economies, with a focus on US programs. This is in part because the United States has more of them and because geographic program variation provides more plausibly causal variation relative to the typically countrywide programs in Europe. We include information on relevant programs in other developed economies where possible. We start with theoretical predictions, focusing on traditional models and insights from behavioral science, then describe the most studied programs, the approaches used to study them, and what we know about take-up and program participation. We then discuss evidence on the contemporaneous effects of these programs on nutrition and dietary intake–the most proximate health outcome to many of these programs. We follow by laying out the evidence of the effects on other health outcomes as well as outcomes such as education and labor supply, which themselves affect health. We conclude with a discussion on the emerging literature about the effects of these programs on long-term health and other outcomes.[1]

3.2. Food Assistance Programs in General

The primary functions of most food and nutrition programs are to increase food security and alleviate hunger and malnutrition. Secondary goals include improving health, education, and labor market outcomes, with varying focus depending on the target population. In part driven by the goals and motives of the providing authorities, these programs take many different forms. The

[1]There are several other useful reviews, including those by Currie (2007), Hoynes and Schanzenbach (2016) and Meyerhoefer and Tang (2011). Bitler (2015) reviews the literature on SNAP, health, and nutrition; Gundersen (2015) reviews the effects of SNAP on obesity; and Hoynes et al. (2015b) and Gregory et al. (2015) review the effects of SNAP on food consumption.

patchwork nature of US programs, in particular, reflects an idiosyncratic safety net, cobbled together from many programs (e.g., Bitler and Karoly, 2015). The largest amount of US government food spending is for the Supplemental Nutrition Assistance Program (SNAP), but school meals are the most widespread example internationally. Food voucher programs are rarely found in developed countries, but many countries provide either universal free lunches for school-aged children or subsidized meals for students who qualify as low income. In recent times, reducing obesity has been added as an important goal of some programs, both in the United States and Europe, as rates of obesity and related chronic diseases have risen, especially among low-income populations. The focus of food assistance on improving health also explains targeting the elderly, for whom health shocks may be the most dangerous, and children, for whom small investments have been shown to have long-term impacts. Education and informational outreach are key features of many food and nutrition programs. Nutrition education is provided as an optional or mandatory component of many programs. This can include classes and resources meant to teach vulnerable populations about improving diet quality and managing a limited food budget. Education can be a direct way to try to reduce the prevalence of obesity coexisting with malnutrition, especially in low-income communities, although evidence on this is mixed. Eligibility for most food and nutrition programs depends on falling below a certain level of income. These means-tested eligibility rules are usually based on some multiple of the federal poverty guideline and sometimes include limits on assets, such as savings or the value of a car. The few food and nutrition programs (e.g., the Child and Adult Care Food Program or a general subsidy in a school meals program) that are not means tested are tagged (Ackerlof, 1978) in another way, meaning that eligibility is tied to belonging to a certain group, such as school-aged children or the elderly. Additionally, some programs, such as the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), are subject to both categorical screening and a means test. Programs are funded in two distinct ways. Entitlement programs, such as SNAP, provide benefits to every eligible applicant. By contrast, discretionary programs such as WIC are funded with a prespecified amount and have mechanisms for rationing participation if demand exceeds the allocated amount.

3.2.1. Theoretical Predictions. The neoclassical theory of the consumer choice problem is a useful framework for modeling expected effects of government transfer programs. The following equation shows the consumer's utility maximization problem with a food voucher that has a value of $\$V$ that may only be used to purchase goods that belong to category F. The types of food that are eligible for purchase with a voucher vary across programs and may depend on who is administering the program. Out of category F, f refers to the total amount of goods consumed in this category, x is all other goods, Y is total income, and p is the price.

$$
\begin{aligned}
&\max && u(x,f) \\
&\text{s.t.} && p_x x + p_f f = Y + V \\
& && p_x \leq Y \\
&\Longrightarrow && p_f f \geq V
\end{aligned}
\tag{3.1}
$$

Figure 1 graphically shows the budget constraints with and without the food voucher. The x-axis shows the total amount spent on goods in category F and the y-axis shows all other spending. The budget constraint shifts to the right by $\$V$ after receiving the voucher, but because the benefits may only be spent on eligible foods, there is a region that would have been attainable with a cash transfer but cannot be reached using a voucher.

The other major type of food assistance is through food distribution, including provision of free or reduced-price lunches for school-aged children. These are sometimes called "take it or leave it" or no-top-up benefits, because there is only one level of provision that a recipient can either choose to receive all or none of.

The basic neoclassical model helps illustrate the difference between cash and in-kind transfers, as well as some of the reasons that the government chooses to provide food assistance in lieu of cash. Although in-kind transfers provide limitations on consumption, in some cases they are effectively treated as cash. For example, some household receiving \$100 worth of food vouchers would have spent at least \$100 on the same foods the vouchers are used to purchase, leaving an extra \$100 in the household budget that can be used to make non-food purchases. These households are called "inframarginal". Figure 2 depicts the budget set with and without the voucher and shows the utility maximizing amount of consumption with and without the voucher. Here, even though the household depicted spends the entire voucher, total food spending increases by less than the benefit amount and spending on other goods increases as well. If a household is inframarginal, the in-kind transfer is theoretically the same as cash.

The other case is when a household is "extramarginal" and the in-kind transfer does affect the household's choice. Figure 3 depicts the utility maximizing choices of an extramarginal household. Without the voucher, the household would chose f_0 of food. If given a voucher of cash, the household would respond to the new amount via the income effect, and buy more food (now purchasing an amount $f^* > f_0$. However, with the voucher, the household is constrained and can't buy only f^* of food, they must either buy $f_1 > f^*$ of food, or remain on the original budget set. Their utility would be higher with f^*, but it is unattainable. The stricter the requirements on the types of food eligible to purchase with the vouchers, the more likely the household will be extramarginal.

One other important feature of these programs is that they may affect choices about leisure and thus labor supply in the same way that they change other consumption. Figure 4 shows the trade-off between overall consumption on the y-axis, and hours of leisure on the x-axis with the WIC program. If one assumes a family is inframarginal and consumes all of the WIC goods, WIC imposes a notch in the budget set. Thus, there is a set of individuals who should not choose hours in the range shortly above the point at which earned income makes a family ineligible for WIC. Figure 5 shows the analogous graph for leisure/consumption choices under Food Stamps, where there is a 30% benefit reduction rate on earnings. Under Food Stamps, there is also a dominated range.

Even given that cash might lead to Pareto improvements relative to vouchers, there are still reasons a social planner might impose strict requirements. Purposely constraining a recipient's choice can encourage (nutritional) food spending, especially when various constraints may lead the recipient away from making the "best" decision for themselves when unconstrained (Currie and Gahvari (2008) go through many such examples in their excellent JEL review). An example of this is the agency problem between parents and children. Parents make decisions on behalf of their children but may not make the best choices for them, leading to arguments for paternalism.

Another argument for in-kind assistance is that it provides a mechanism for self-targeting, meaning that fewer non-eligible individuals have the incentive to lie about eligibility (Nichols and Zeckhauser, 1982). The government does not have the information to perfectly determine who is in need of assistance and providing aid is costly. Targeting is especially inefficient when the targeted category is difficult to observe, like households unable to meet their basic needs without help. Relying on self-identification creates an incentive compatibility problem, so creating mechanisms that encourage self-targeting will reduce the number of unqualified applicants. In theory, self-targeting imposes barriers causing those who do not need the program to decide that the application is not worth it. It requires that the good only appeals to people who need it. Many would consider food assistance to be worse than cash, reducing the incentive for them to lie about eligibility. Other ways of encouraging self-targeting are to impose unnecessary costs that make it not worth it for non-targeted group, such as restrictions on quantity or quality, or burdens on time (i.e., long applications or lines, (Besley and Coate, 1992). The costs may also come from stigma (Moffitt, 1983). Below, we discuss existing research on determinants of program take-up but note here existing work showing that poverty can cause poor decisions due to a cognitive load imposed by time or money deficits.[2]

3.3. The Food and Nutrition Assistance Program Landscape in the United States

Next, we describe the various US food and nutrition programs, discussing eligibility, program size, and benefit generosity and how these factors have led researchers to various sources of causal variation.

The US food and nutrition assistance landscape, like the overall US safety net, is a varied patchwork system. Different programs are run by various entities and have unique ways of counting income and family size and comparing them to eligibility cutoffs. Most of the programs are categorical, means tested and, with the exception of SNAP, are not top-up programs, and thus provide a fixed amount of benefits or nothing.

Table 1 presents some basic facts about US food assistance programs. This table contains information on categorical eligibility rules as well as other eligibility rules, including income limits, asset rules, citizenship or authorized status rules, and adjunctive eligibility (whether participation

[2]Mani et al. (2013) show that making individuals imagine an expensive outlay resulted in impaired performance for low- but not high-income individuals. This applied to shoppers in New Jersey as well as farmers in Tamil Nadu, India.

in other programs automatically makes persons eligible for this program if otherwise eligible). It also has information on caseloads and expenditures for fiscal year (FY) 2017.

3.3.1. The Food Stamp Program. The largest program by far is SNAP, formerly known as the Food Stamp Program. It is not only the largest food assistance program in the United States in terms of spending, which totaled \$63.6 billion in FY 2017, but also in terms of caseload, with 42 million participants that year. Like all of the food assistance programs in the United States, it is means tested. The food assistance unit (all those in the same household who share food) must have a gross income less than or equal to 130% of the federal poverty guideline. After deductions have been made for working, housing expenses, and other adjustments, net income must be under poverty. The 2017 federal poverty guideline outside of Alaska and Hawaii was \$24,600 for a family of four. There is also an asset limit (\$3,500 in countable resources if elderly or disabled and \$2,250 in countable resources if not). States have leeway to decide if cars are included as resources; some assets such as savings, houses, and retirement savings do not count.

Maximum benefits vary by family size in the lower 48 states and Washington, DC and are somewhat higher in Alaska and Hawaii. In FY 2019, the maximum monthly benefit was \$642 for a unit of size 4. Then, benefits are taxed away as countable income (earnings) increases at a rate of 30%. The maximum allotment level is calculated by the US Department of Agriculture (USDA), using the cost of the Thrifty Food Plan to feed a family of four, with a man and woman aged 20–50, one child aged 6–8, and one child aged 9–11, and adjusted for other unit sizes. Allotments are updated every year in October to account for increasing food prices. Currently, the program is operated with an electronic benefit transfer (EBT) card, and the benefits can be redeemed for unprepared foods (excludes prepared foods and alcohol). Many of the rules are federal, but states have some leeway. For example, states can decide whether they want to cover some groups of authorized immigrants who otherwise would be excluded. States can also waive the higher gross income cutoff by using a concept called Broad-Based Categorical Eligibility. The fact that the program is primarily federal yields limited variation to produce causal estimates of its effects.

3.3.2. Identifying Variation. We discuss four main strategies that have been used to study the effects of the Food Stamp Program or SNAP. First, researchers have leveraged the rollout of the program during the 1960s.The modern Food Stamp Program began as a pilot in 1961 in eight counties (Aussenberg, 2018).

The 1964 Food Stamp Act made the program permanent and included prespecified funding levels for three years. Over time, the program grew rapidly, eventually leading to the end of funding for the predecessor to the Food Stamp Program, the Commodity Distribution Program, which had provided shelf-stable commodities. The original Food Stamp Program had a purchase requirement, where families would purchase their food stamps and receive more than the value they had paid for, with poorer families getting more benefits. In 1971, the federal government set national eligibility and work requirements, among other changes. A further 1973 law required the program to be expanded to every jurisdiction by July 1974. In 1977, Congress eliminated the purchase requirement, set the federal poverty level (FPL) as the income eligibility limit, and otherwise standardized eligibility, while requiring most adult work registrants to look for work.

During this initial rollout period, Congress funded the Food Stamp Program by a set amount each year, limiting the number of counties that could participate. This meant that the timing of implementation was quasi-random, and we discuss below the studies by Hilary Hoynes, Diane Schanzenbach, Doug Almond, and others leveraging this natural experiment. During the early 1980s, the Reagan administration enacted a gross income limit and implemented cuts. From 1988 to 2004, the program eliminated the paper stamps and replaced them with debit cards, with the first pilot taking place in 1984. The next big change to the program was when Congress enacted welfare reform in 1996, replacing the former Aid to Families with Dependent Children (AFDC) program with a block-granted program, Temporary Assistance for Needy Families (TANF). In August 1996, after a period of state experimentation, welfare reform also removed eligibility for most authorized immigrants and limited benefit use for able-bodied adults without dependents to only 3 months out of every 36. Eligibility for elderly, disabled, and child immigrants was restored in 1997 and 1998 (unauthorized immigrants are generally not eligible for federal benefits, with a few exceptions). The 2002 Farm Bill restored benefits for qualified aliens in the United States for at least five years and for all children/disability payment recipients. The name of the program was changed to the Supplemental Nutrition Assistance Program in 2008.

In the post-welfare reform period, most research leverages two sources of variation. The first strand of the literature looks at spillover effects of the welfare reform on the use of SNAP and various outcomes (e.g., Figlio et al., 2000). The old AFDC program conferred automatic eligibility for the Food Stamp Program. So, many families with children on AFDC would also be food stamp participants. Once these families left AFDC, they often left the Food Stamp Program too. (If they satisfied the eligibility rules otherwise, they could have remained on food stamps. Despite this, many families left SNAP and AFDC.) The second strand of the literature leverages changes in eligibility for food stamps for authorized immigrants who entered after August 1996, had been in the United States less than five years, and were not refugees (e.g., East, 2018). Some states chose to make these immigrants eligible for food stamps while others did not. These two strands of research are part of a larger literature that leverages state policy choices about program rules for identifying variation in difference-in-differences designs.

The Great Recession began in 2007, plunging the world economy into a downfall. As a response, Congress passed the American Recovery and Reinvestment Act (ARRA) of 2009 to stimulate the economy. As part of this, maximum benefit levels were increased from April 1, 2009, to October 31, 2013, by approximately $80 a month, and then reduced back to 2009 levels rather unexpectedly on November 1, 2013. This led to a third approach, leveraging of ARRA benefit increases/decreases, and in some years with high food price inflation, allotment increases in October (e.g., **?**).

The last bit of variation on the individual side that has been implemented widely in understanding the role of SNAP is the SNAP benefit cycle. Starting with Wilde and Ranney (2000) and Shapiro (2005), research has used the fact that many SNAP recipients seem to exhaust their benefits quickly once a new month's allotment arrives, and they are left without any benefits approaching the end of the month. Early in this research,most states disbursed benefits at the beginning of the month, which also had ramifications for retailers. More recently,many states, while still always giving each recipient all of their benefits at once, have started to stagger disbursal across the month. This has several advantages for identification. It is even more powerful when the mechanism used to determine disbursal dates can be linked to other data, as then each individual has a series of possible control persons whose benefits are disbursed on other dates.

One underused piece of variation in the program is the rule about store participation in the programs (e.g., Gleason and Pooler, 2011). For example, stocking requirements for stores wishing to participate in SNAP were recently made more comprehensive. This has the power to potentially change local food environments.

Finally, randomized controlled experiments have provided new insights into our understanding of food stamps. This includes the cash-out experiments from several decades back as well as the recent Healthy Incentives Pilot, which tested whether incentivizing food stamp recipients to buy healthful foods changed what they bought and what they ate.

3.3.3. The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) Program. The WIC Program was created in 1972 as a two-year pilot and made permanent in 1974 (Aussenberg and Kortrey, 2015). Although it is also a food voucher program, it is quite different from SNAP. It is a heavily targeted program, providing vouchers for foods high in important nutrients to low-income and nutritionally at-risk pregnant, postpartum, and breastfeeding women and children under age five. The vouchers are quantity vouchers rather than for specific dollar amounts. WIC entitles recipients to a set amount of various foods at participating retailers. In FY 2017, 7.3 million persons obtained WIC benefits worth $5.6 billion, including spending on nutritional education and breastfeeding promotion. WIC spending is not that large compared to SNAP, however. Approximately half of all infants in the United States receive WIC, making it a program with a broad footprint.

WIC is administered by various nonprofits where women and children go to be certified (they must live in the state where they obtain benefits). Doctors assess their nutritional risks, which include both medically based risks (e.g., anemia, poor pregnancy outcomes) and diet-based risks, and they receive mandatory nutritional education and referrals to other programs. After initial certification, families must return periodically to see if they remain income eligible and at nutritional risk. The period between certifications for infants and children can be as large as one year and is shorter for women.

While SNAP provides automatic or adjunctive eligibility only to TANF/AFDC recipients and Supplemental Security Income (SSI) recipients outside California, a number of other programs provide adjunctive eligibility for WIC, including SNAP,TANF/AFDC, and Medicaid. Other categorically eligible individuals with incomes at or below 185% of the federal poverty guideline who are at nutritional risk are eligible. Johnson et al. (2013) report that 71% of WIC participants in 2012 were on Medicaid, 36% were on SNAP, and 9% were on TANF. Families receive the vouchers for three-month periods but, unlike SNAP, each month's benefits expire at the end of that month.Many states are implementing EBT WIC benefits, with all states required to finish by 2020. Despite not having run out of funding recently, WIC is a discretionary program and not an entitlement like SNAP. Given an annual appropriation, benefits are allocated to WIC agencies according to a funding formula. WIC is run by 90 state, tribal, and territorial agencies, who in turn deliver services at the nonprofits described above. The federal government allows states to decide if unauthorized immigrant women can participate in the program (all but Indiana do so).

WIC benefits vary according to the child's age or the woman's pre- or postpartum status, based on nutritional criteria.There are seven specific food packages, along with alternatives for children or women with special dietary needs. The foods include milk, beans, peanut butter, cheese, whole wheat bread, cereal, canned fish, infant formula, and other infant foods; since the new 2009 food package changes, recipients get a small cash voucher for fruits and vegetables. Each state decides what specific products are on the list, as long as they meet broader federal rules (e.g., cereal cannot have more than 6g of sugar per serving). The WIC food package had been mostly unchanged since 1992 until the recent changes implemented in 2009. The 2014 per-person monthly cost of WIC food was approximately $43.65, according to Aussenberg and Kortrey (2015), although this only captures the expense to the government. States are allowed to bargain with infant formula retailers to get rebates for allowing WIC's formula contract to be single sourced, and the rebates are quite large. This makes the value of WIC highest in terms of purchasing power per person for families with infants getting formula. The National Academies of Sciences, Engineering, and Medicine (2017) reports that this compares to total food spending of $184 per week for WIC households.

3.3.4. Identifying Variation. A number of sources of variation have been used to study the effect of the WIC program. As with SNAP, research has looked at the effect of program rollout. Early research, which probably suffers from some selection bias, linked WIC administrative records to Medicaid records, finding that WIC participation was associated with lower spending. (All

Medicaid-recipient children under five are eligible for WIC.) Some work has tried to use state policy variation that induces different participation levels. This has been less promising than academic research considering SNAP (see Bitler and Currie, 2005). Other studies have evaluated between-sibling differences in use of WIC during pregnancy or clinic closures across locations (e.g., Rossin-Slater, 2013).

A few studies have leveraged the fact that generosity changes considerably at specific points, such as at age one, when infant formula is no longer distributed or when children age out entirely from the program. The recent food package change has provided identifying variation in differences in timing of how quickly states adopted the new package. Finally, experiments have shown how families would respond to a subsidy such as the cash value voucher for fruits and vegetables or efforts to encourage breast feeding tested in Oregon.

3.3.5. School Meals and Other Child Nutrition Programs. The USDA's child nutrition programs include the National School Lunch Program (NSLP) and the School Breakfast Program (SBP), which together are known as the school meals programs. They also include the Child and Adult Care Food Program, providing snacks to after school and day care programs; the Summer Food Service Program, filling in for the school meals programs during the summer months; the Special Milk Program, providing milk for schools not in the school meals programs; and the Fresh Fruit and Vegetable Program Billings and Aussenberg (2018).

3.3.5.1. *School Meals Programs.* The National School Lunch Program (NLSP) and School Breakfast Program (SBP) together touch nearly as many persons as the SNAP program. In FY 2017, an average of 22 million free and reduced price students and 8 million paid lunch students participated in the NSLP, at a cost of $12.3 billion. Nearly 15 million students participated in the SBP, with spending reaching $4.3 billion. The NSLP and SBP were in place in nearly 10,000 specific schools and districts in 2017.

The NSLP was started after World War II in 1946, and the SBP began in 1966 as a part of the Johnson administration's War on Poverty and was made permanent in 1975. Both programs are federally run by the USDA Food and Nutrition Service and administered at the state and local levels, leading to variation in program implementation in different states and districts. Benefits are directly delivered by participating public and private schools.

School food authorities receive cash reimbursements and commodities. Reimbursable meals must satisfy federal requirements that they be served to eligible persons and fulfill nutrition requirements. Free meals get the highest reimbursements, followed by reduced price meals. There is a small subsidy for hot meals provided to fully paying children. Some states and localities also contribute toward meals. While the federal government does not require that schools participate, some states do require that some schools and districts participate in the NSLP/SBP.

Eligibility for free meals is means tested. Unlike many other safety net programs, all children—not just authorized immigrants and citizens—are eligible for school meals if meet income limits. Children are typically given forms on which parents list family income sources. Family income is compared to the reduced-price eligibility limits (income $> 130\%$ and income $\leq 185\%$ of the FPL) and free eligibility limit (income $\leq 130\%$ of the FPL).

Again, there is adjunctive eligibility, with children obtaining eligibility automatically if they are in a household where someone is getting SNAP, TANF, or the Food Distribution Program on Indian Reservations;if they are in Head Start or foster care; or if they are homeless. States had the option before 2004 to do direct certification for adjunctive eligibility. States could check whether children were on SNAP or TANF, and then the family would not need to fill out a school meals application. Since 2004, states are required to do direct certification for SNAP.

There are also what are known as eligibility provisions, which allow districts and schools to offer free meals to all enrolled students and then reimburse schools based on a baseline period's share of eligible free, reduced-price, and full-price students. Typically, these were implemented by poor districts. Starting with the Healthy and Hunger-Free Kids Act (HHFKA) of 2010, a new provision was added, which allowed schools to offer free meals school wide if enough students were directly certified. Participating schools then did not need to have anyone complete applications.

Another dimension on which the school meals programs make decisions that could affect children's health is through nutritional standards. The HHFKA specified new nutritional standards, incorporating ideas about nutrition that had changed over the interim 15 years. The new rules increased whole grains, reduced sodium, and allowed only low-fat and skim milk to better align meals with the most recent nutrition science (the standards had been unchanged since 1995).The new standards also required some variety in vegetable subgroups along with a daily fruit serving. There was substantial pushback about these standards, including complaints from high school students that serving sizes were too small and from others that the new standards were leading children to reject large shares of what was offered. The School Nutrition Association also lobbied against these new standards, asking for enhanced flexibility, concerned that declines in participation would hurt their bottom lines. In 2017, Congress relaxed some of these standards, allowing state agencies to let school food authorities not use whole grain options and obtain waivers from the requirement to serve low-fat flavored milks.

There are concerns about both the NSLP and SBP that the regular free and reduced-price programs cause stigma. In particular, participation in the SBP is lower than in the NSLP. Some districts have offered universal free breakfast to offset this, under the provision programs listed above. Another relatively recent innovation is breakfast in the classroom. This provides everyone with an optional breakfast during the beginning of the school day, so families do not have to arrive early and are not singled out.

3.3.5.2. *Identifying Variation.* Rollout is a promising but unused strategy for the SBP. The NSLP changed its financing during the 1960s, and this variation has been leveraged (e.g., Hinrichs, 2010). The school meals programs naturally are not in effect in most districts during the summer and, although there is a summer feeding program, it is lightly used. Studies have leveraged comparisons when kids are and are not in school (e.g., Bhattacharya et al., 2006). For the school lunch program, research has leveraged the reduced-price income eligibility threshold, using a fuzzy regression discontinuity design with income as the running variable (e.g., Schanzenbach, 2009). Some states require that schools participate in the SBP when the share of free and reduced price eligible students exceeds a limit. This provides another regression discontinuity possibility as long as schools are randomly on one side or the other of this threshold. This type of design is also enabled by the Community Eligibility Provision, in which schools with more than 40% of their students directly certified are eligible to offer universal free meals. This might allow for a regression discontinuity, with the share directly certified as the running variable, although some of the existing papers have used difference-in-differences approaches. And there is an additional wrinkle, which is that this provision was phased in over time, allowing for a placebo check in places before the program took effect. To our knowledge, there has been little or no research that tries to leverage state differences in the modern school meals programs. There are again experiments that provide gold standard evidence about breakfast in the classroom and universal free breakfast.

3.3.5.3. *Other Child Nutrition Programs.* The other child nutrition programs are considerably smaller. The Special Milk Program was started in 1954 and made permanent in 1970. Both the Child and Adult Care Food Program (CACFP) and the Summer Food Service Program began as pilots in 1968, separated in 1975, and were made permanent in 1970. Finally, the Fresh Fruit and Vegetable Program was created in 2002.

The CACFP served an average of approximately 4.5 million, primarily children, in July 2017 and spent approximately $3.5 billion in FY 2017. Day care programs and other settings are reimbursed for meals and snacks served. Similar to the school meals programs, free eligibility is associated with income being under 130% of the FPL, while reduced-price eligibility comes with income $> 130\%$ but $\leq 185\%$ of the FPL. These snacks and meals are also required to be of sufficient nutritional quality, and reimbursements per meal are based on the number served and the per-meal or per-snack reimbursements. The vast bulk of those served are at child care centers.

The Summer Food Service Program provides reimbursements to institutions such as summer camps and summer feeding projects where children are that time of year. The reimbursements go to sponsors who provide the meals. In July 2017, approximately 2.6 million children and others were served meals, with a cost of around $483 million in FY 2017. In areas with 50% or more children eligible for free and reduced-price lunch, anyone in the community can get meals. Closed sites also offer snacks to all children, but this is based on 50% or more of their enrolled children being eligible for free and reduced-price lunch. A recent demonstration tested the Summer Electronic Benefit Transfer, which paid EBT benefits to children's families without requiring the children to show up in a community setting.

The remaining child nutrition programs are quite small, although the Special Milk Program was larger in the past. It currently serves about 41 million half pints of milk for a small outlay. The Fresh Fruit and Vegetable program provides grants for the items in its name and had approximately $200 million allocated for FY 2017.

3.3.5.4. *Identifying Variation.* Outside of research that leverages experiments such as the Summer Electronic Benefit Transfer demonstration, there has been little work done on these other child nutrition programs.

3.3.6. Other Smaller Programs. There are a number of other programs. These include the Commodity Supplemental Feeding Program, which provides commodities to adults 60 and older at or below 130% of the FPL and, until 2014, some women, infants, and children. This program reached 630,000 individuals in 2017 (Aussenberg and Colello, 2018).

The Emergency Food Assistance Program provides food to local agencies who frequently pass them on to food banks, soup kitchens, and food pantries. This program was created in 1981 to purchase foods to support food prices and help poor individuals.The Food Distribution Program on Indian Reservations provides commodities to individuals on reservations, serving about 94,000 in 2017, at a cost of $151 million. Finally, there are some other small programs, which have not been extensively studied and are therefore not considered here.

3.3.7. Identifying Variation. To our knowledge, there is no economics literature that discusses these programs. The design of some of these programs would allow for a regression discontinuity design based on age.

3.3.8. Meal Programs in Other Countries. A number of other OECD countries provide school meals programs. We do not survey these in any detail, but will mention interesting papers on this topic below.

3.4. Evidence of Take-Up

We begin by discussing the evidence of take-up of the programs and their effects on nutrition and food consumption. Next, we turn to a discussion of the immediate impacts on health and other short-run factors that affect long-run health and nutrition outcomes. We conclude by discussing the literature on long-run impacts of food and nutrition programs on health and well-being and areas for future research.

3.4.1. Evidence of Take-Up and Use of the Programs. None of the food assistance programs in the United States have universal takeup. Cunnyngham et al. (2017) report take-up rates of 83% for eligibles of SNAP in 2014. Trippe et al. (2018) find that approximately 53% of WIC eligibles are participating in the program in 2015. This varies across subgroups with only about 44% of eligible children but 77% of infants and 89% of postpartum nonbreastfeeding women participating in WIC.

Researchers in the past have speculated that WIC participants are favorably selected. This might provide an explanation for positive effects of the WIC program. Bitler and Currie (2005) find evidence that women on WIC are if anything negatively selected, with eligible WIC participants having worse observable characteristics on average than eligible WIC nonparticipants.

Documenting and modeling behaviors, such as the effect of burdensome applications on take-up rates, is a more recent innovation that has brought economic models closer to observed data. In the context of food assistance, the behavioral literature has provided the most insight into the areas of program take-up and the influence of program design on spending and decision making.

One of the earliest and most influential papers addressing the behavioral aspects of incomplete take-up was by Moffitt (1983), where stigma was modeled as a cost of program participation. This model introduced the idea that a nonpecuniary cost can influence households' participation decisions. Other research documents different psychological costs that change households' maximization decisions. Two major categories are transaction costs and attention costs.

Transaction costs are also referred to as hassle costs, capturing the idea that inconveniences are a real factor in household decisions. Examples of transaction costs are lengthy applications, long lines, travel times to program offices, or hurdles for recertification. Currie and Grogger (2001) show that losing automatic eligibility and adding certification and recertification requirements directly reduce take-up of food stamps by increasing the transaction costs (see also Kabbani and Wilde, 2003). Declines in take-up rates as a result of transaction costs might not be a problem if this improves targeting efficiency by primarily affecting individuals who do not need the benefits. For example,Daponte et al. (1999) show that, although people are sensitive to transaction costs and tend to have poor understanding of eligibility for food stamps, this could be a rational decision to avoid the costly process of learning about the program rules because the perceived benefits of participation are low.

Although imposing these costs may reduce the number of non-needy individuals receiving benefits not intended for them (reducing the rate of type 1 errors), it is at the cost of imposing a disproportionate cost on the neediest (increasing the rate of type 2 errors). Evidence from an experiment conducted by Finkelstein and Notowidigdo (2019) shows that inconveniences in the application process have a larger effect on reducing the take-up rate of the neediest elderly applicants for SNAP.

A problem arises when the probability of participation is not related to the benefits, but rather, as shown by Finkelstein and Notowidigdo (2019), is lowest among the neediest populations. Attention costs capture some of the explanation for low take-up among the populations that would benefit from food assistance programs the most. Research on the relationship between poverty, cognitive load, and the impact of stress on decision making (see Haushofer and Fehr (2014) for a review of some of this literature) has shown that some costs have a larger influence on decisions of the poorest individuals. These include costs associated with awareness, inattention, or lack of information.

Moving beyond take-up, the behavioral literature has also contributed to better understanding the decisions of program participants. Research in this area answers questions about whether and how different types of programs influence a household's spending decisions and food choices.

Food vouchers can influence a household's spending decisions in several different ways. One way is by affecting the voucher recipient's perception of their budget. After receiving a food voucher, the recipient is likely to mentally tag the voucher money to be used for food spending, even though it should theoretically be treated as cash, assuming the recipient is inframarginal. This has been documented in households receiving SNAP benefits in recent years (Beatty and Tuttle, 2015; Hastings and Shapiro, 2018; Kim and Shaefer, 2015), resulting in more food spending than if they had been given the same amount in cash. This phenomenon is referred to as mental accounting, narrow bracketing, or framing.[3] This area still requires further research, as there is still no consensus on household spending responses to receiving food vouchers. In older data, Hoynes and Schanzenbach (2012) find that food stamps at rollout are much like cash, but their time period

[3]One alternative explanation from the literature for why the marginal propensity to spend on food is bigger than one might expect is that it is tied to intrahousehold bargaining. Breunig and Dasgupta (2005)) show that the higher propensity to spend from food stamps compared to cash is only present for households with at least two adults.

includes a different program setup, with the purchase requirement being included. Fraker et al. (1995) use data from the cash-out experiments and find a larger propensity to spend food stamps on food than when the benefits are cashed out.

Another way that decisions can be affected is through the stress and complexity of program participation. Similar to attentional costs of take-up, signing up and managing program benefits can add to the cognitive load of an already-stressed individual. This can lead to inattention toward decisions when purchasing food. Even without the burden of stress, there are high costs to learning about healthy foods, recipes, and habits. Additionally, when people are stressed, they tend to make less healthy food choices.This finding could help provide some understanding on the link between obesity and food insecurity. Hut and Oster (2018) formalize this by modeling the attentional cost of someone changing aspects of their diet. The result is that small or incremental changes are much easier to sustain, matching their empirical results that households who have large overall improvements in dietary quality accomplish that through changing a small number of foods.

Researchers and policymakers have scope to use these behavioral insights to encourage healthy decisions and counteract negative behavioral influences. For example, research has shown that having too many choices leads to worse decisions. Using the idea of framing, policymakers can design programs to frame choices in a way that reduces cognitive burden without actually constraining options.

Understanding whether low take-up rates are driven by stigma versus lack of information is an area that needs more research. An important application of stigma is with school meal takeup. Evidence from experiments and quasi-experiments has shown that program changes such as offering breakfast in the classroom can improve take-up (Leos-Urbel et al., 2013; Schanzenbach and Zaki, 2014).

Another example where reducing hassle increases take-up comes from the Summer Electronic Benefits Transfer demonstrations, where locations experimentally implemented a test of the existing summer feeding program that takes place in so-called congregate feeding areas as compared to electronic benefits modeled on SNAP or WIC. It is well known that participation in the Summer Food Service Program is much lower than in the school meals programs. Collins et al. (2013) report on the demonstration. Children certified for free and reduced meals were randomized to receive the usual summer feeding program (e.g., provided at summer camps and schools) or to receive EBTs of $60 per child per month during the summer. The EBT pilot program led to a reduction in very low food security among children relative to the usual summer feeding program. This was undoubtedly caused in part by increases in take-up with families being more likely to use the EBTs than to attend summer camps or schools to obtain summer meals.

3.4.2. Evidence of Direct Effects on Nutrition and Consumption. Many of the open questions about health, nutrition, and their interaction with food assistance programs are the same ones being asked decades ago, and many still do not have satisfactory answers. The difficulties of accurately measuring participation, food intake, and long-term outcomes are compounded by the added challenge of selection into treatment. Research often finds that program participants have worse health outcomes than eligible nonparticipants, but there is a large body of evidence showing that the two groups are substantially and significantly different, which Bitler (2015) discusses at length. The biggest challenge to overcome in identifying causal effects is that the decision for an eligible household to participate might stem from poor health or nutrition pushing them to seek out assistance.

Although it is difficult to measure both food intake and which aspects of diet are important for health, recent studies use grocery store purchases linked to nutrients in foods (assuming that families eat most of what they buy) to see whether SNAP changes dietary quality. Markers of dietary quality include fruit and vegetable consumption; increased intake of vitamins,minerals, and fiber; and decreased intake of saturated fats, sugar, and sodium. Studies such as Mabli et al. (2010) find that increasing spending on food tends to increase dietary quality. Hastings et al. (2018) study grocery store purchases to show that the amount spent on groceries cannot explain everything, and differences in dietary quality are partly explained by group characteristics.

Other areas of research consider the ways to increase healthy food consumption using program benefits. Hoynes and Schanzenbach (2009) show that food stamp rollout changed where people acquired their food, increasing consumption of food at home and decreasing out-of-pocket spending. One policy proposal that has received a lot of attention but has not been directly tested involves limiting access to certain foods through SNAP. To the extent that encouraging consumption has the same effect as restricting it, one could draw inferences from some existing USDA demonstration projects. One prominent example is the Healthy Incentives Program, which Bartlett et al. (2013) study using a randomized controlled trial (RCT). They find that households given a subsidy for buying fruits and vegetables with their SNAP benefits spend more on them and consume more of them than control households. The opposite approach, using restrictions, may also cause some families—perhaps needy ones—to drop out of the program.

Other research looks at the effect of improving nutritional intake among low-income students. For example, Bhattacharya et al. (2006) find that comparisons of children in and out of school suggest that the SBP has positive effects on levels of nutrients and spills over into the rest of the household. Schanzenbach and Zaki (2014) use RCT data and find that providing school breakfasts decreases spending on food at home.

Several studies leverage changes in the WIC food package (including a new cash value voucher for fruits and vegetables). Herman et al. (2006) evaluate vouchers similar to WIC participant vouchers while randomizing out controls, finding an effect on consumption. Gleason and Pooler (2011) find that the new WIC food packages affect redemptions of WIC foods. The National Academies of Sciences, Engineering, and Medicine (2017) reviews a number of other papers looking at how the new package affect redemptions and consumption and report satisfaction, much of it outside of the field of economics (e.g., Andreveya and Luedicke, 2015).

As noted above, a host of publications looks at how the SNAP cycle affects consumption and nutrition. This includes those by Wilde and Ranney (2000) and Shapiro (2005) (consumption and calories), Todd (2015) (diet quality), and Hamrick and Andrews (2016) (more days with no eating at the end of the month).

This literature does tie many programs to increases in specific nutrients, but large data sets with repeated cross sections and long panels spanning policy changes are rare in the United States and elsewhere.More such data would enhance our understanding of these outcomes. An emerging literature looks at the SNAP disbursal cycle to see whether it affects prices and consumption. Hastings and Washington (2010) identify a puzzle with scanner data. Although evidence shows that SNAP participants disproportionately spend their benefits at the start of the benefit month, stores do not charge them different prices. Goldin et al. (2022) document that this still holds in the current era where disbursals are more evenly spaced in many states. Beatty and Cheng (2016) show, however, that unit prices paid by SNAP participants decrease over the benefit cycle.

3.4.3. Evidence of the Immediate Effects on Health. Food and nutrition programs have been shown to have important health outcomes as well. Using the rollout design, researchers show that both food stamps (Almond et al., 2011) and WIC (Hoynes et al., 2011) have a positive but small effect of rollout on birth weight and other birth outcomes. Currie and Moretti (2008) use data from California and find less robust effects of exposure at birth.

A large nonexperimental literature on WIC shows positive effects of the program. A corresponding literature leverages comparisons across children within the household at different times (e.g., Currie and Rajani, 2015) or uses closings and openings of clinics across children (Rossin-Slater, 2013) to show the positive effects of prenatal WIC participation on birth outcomes. Figlio et al. (2009) compare two sets of women: those with older sibling children who are eligible for reduced price school meals but near the reduced-price eligibility threshold in the pregnancy year (and thus WIC eligible while pregnant) but not in surrounding years and those with older sibling children near that cutoff but who are ineligible for reduced price meals in the pregnancy year but eligible in adjacent years. They also leverage a policy change that interacted with being marginally eligible in the pregnancy year to generate instrumental variable estimates of the effects of WIC on birth outcomes in Florida. Overall, they find that WIC use leads to reductions in low birth weight. Ted Joyce and colleagues (e.g., Joyce et al., 2005, 2008), by contrast, have several papers raising questions about the value of WIC and point out an important caveat: women who have longer pregnancies can be on the program longer. Still, several of their studies find positive effects using fixed-effect types of strategies. Chorniy et al. (2018) use a cross-sibling design with a host of linked administrative data in South Carolina and find that siblings with prenatal exposure to WIC have lower attention-deficit/hyperactivity disorder rates and lower rates of grade repetition.

WIC is most valuable in terms of spending that it displaces for those who would formula feed their infants. Recently, the program has made extensive efforts to focus on encouraging breastfeeding. Among others, Reeder et al. (2014) use an RCT to find that telephone support to postpartum women from peer counselors led to increases in breastfeeding for women who wanted to breastfeed and increases in exclusive breastfeeding among Hispanic women.

Other literature shows that food stamp participation reduces food insecurity, a finding based on program variation across states and time (Gregory and Deb, 2015; Ratcliffe et al., 2011; Schmeiser, 2012; Yen et al., 2008). Bronchetti et al. (2017) leverage differences in the generosity of food stamps driven by food price differences across locations and find evidence from the National Health Interview Survey that children facing higher real benefits get more preventive care and have fewer school absences. Using hospital claims data from California, Seligman et al. (2014) show that for residents of low-income areas, being at the end of the calendar month leads to more hospital admissions for hypoglycemia. Food stamp benefits in California during this period were disbursed during the first 10 days of the calendar month and, thus, the authors attribute this increase in hospitalizations to some combination of the SNAP benefit disbursal cycle and other cycles in paychecks and other social benefits. Heflin et al. (2017) use linked SNAP and Medicaid administrative data to explore a similar question in Missouri. They find no evidence of a SNAP cycle in emergency room visits for hypoglycemia among those on both SNAP and Medicaid, but they also find that emergency room claims for hypoglycemia are less common for families with larger relative to smaller SNAP benefits.

Another set of studies leverages partial identification approaches and finds that the school lunch program has positive effects on health with mild assumptions(Gundersen et al., 2012) and that SNAP is also beneficial (Kreider et al., 2012).

Finally, some studies use regression discontinuity designs. A regression discontinuity using income as the running variable finds that the reduced-price lunch program may lead to more obesity (Schanzenbach, 2009). Regression discontinuities in age show little impact on food consumption of aging out of the WIC program (Bitler et al., 2018; Frisvold et al., 2018; Smith and Valizadeh, 2018).[4] Overall, effects of the various programs on short-run health are mostly positive, but somewhat mixed, with some suggestions that school lunches might be associated with increases in obesity.

3.4.4. Immediate Effects on Other Outcomes. As noted above in the theoretical section, means-tested food assistance programs such as SNAP also affect other margins. First, to the extent that they behave essentially as does cash in changing families' choices, they would be expected to

[4]Arteaga et al. (2016) find an increase in household food insecurity when children age out of WIC if they have not begun kindergarten.

affect income and labor supply as well as savings. Figures 4 and 5 show how labor supply should be reduced as families join WIC or the Food Stamp Program. Most of the other programs have similar effects to WIC assuming that recipients reduce labor supply when given an income shock. One additional complication comes from the fact that, currently,many of the programs do not require families to update their eligibility between recertification periods, which can last up to one year for WIC, for example. So, a family's income could increase as they approached the end of the one-year recertification period for WIC without them actually being forced to leave the program. In this case, it would be as though they had the benefit amount for food stamps set as of the benefit level at the start of the eligibility period and then faced the budget set illustrated in Figure 5 at recertification time. In this case, you might expect either a large change after recertification, as families whose income has exceeded program eligibility limits have their eligibility redetermined, or, if families are forward looking, they might cut their labor supply to remain eligible near recertification. Pei (2007) finds no evidence that families on Medicaid reduce earnings as they approach recertification. Similarly, to our knowledge, there is no evidence of this for the food programs.

Most of the existing literature is on the Food Stamp Program. Hoynes and Schanzenbach (2012) use the rollout design and find that the Food Stamp Program had a small but meaningful effect on labor supply during the 1960s and 1970s using the Panel Study of Income Dynamics (PSID) and the United States Decennial Census of Population.They find that effects on reducing labor supply are concentrated in the high-participation sample—families headed by single women. They find no significant effects on earnings or family income. We note that the program was quite different in the 1960s than it is today. Beatty et al. (2018) find that rollout increased employment in grocery and food stores, perhaps changing the food environment.

East (2018) leverages differences across locations based on how they treated eligibility for documented noncitizen immigrants (states had some flexibility here after welfare reform in 1996 to fill in for federal programs that eliminated immigrants' eligibility for food stamps). Eligibility for these authorized immigrants was later restored at different times across states. Using a difference-in-differences design, East finds that food stamps discourage employment of authorized noncitizen single women by about 6%, whereas unauthorized married men reduced work hours by 5%. Moffitt

(2016) suggests relatively small work disincentives in most of the other recent literature not focused on immigrants.

Some recent work has found immediate human capital effects on test scores, while other studies find no effects. Figlio and Winicki (2005) have found that imminent school accountability tests cause districts to alter their school meal menus, leading to improved scores, which suggests that small changes in food can affect test scores in the short run. Frisvold (2015) leverages state rules requiring some schools to participate in the SBP if enough of their students are free or reduced price eligible in a regression discontinuity design, finding positive effects on test scores. Imberman and Kugler (2014) find that breakfast in the classroom in Houston led to substantive increases in accountability scores but had no effect on grades. Schwartz and Rothbart (2017) conclude that universal free meals in New York City also led to test score improvements. By contrast, Leos-Urbel et al. (2013) find limited evidence that universal school meals led to improvements in test scores, and Schanzenbach and Zaki (2014) show limited evidence that either universal breakfast or breakfast in the classroom impacted test scores. More work is needed in this area to resolve these mixed findings. Ruffini (2018) leverages the rollout of the Community Eligibility Provision, which allows schools or districts with enough students adjunctively eligible through SNAP or TANF to provide free meals to everyone. Using a difference-in-differences design among the set of ever-adopting schools, she finds that offering universal meals leads to an increase in meals served. It also leads to improved math test scores for Hispanic and white students in districts where free and reduced shares were low before adoption of the Community Eligibility Provision. Some of her variation stems from the fact that the USDA rolled this program out in some pilot states and then more universally, so the timing of adoption is in part exogenous to district choices.

Gassman-Pines and Bellows (2015) look at effects of the SNAP cycle on test scores by using linked test score and SNAP data from North Carolina. They can then model differences as a function of the date of food stamp disbursal, which is plausibly unrelated to individual characteristics. They find that test scores peak several weeks after benefit transfer. Gennetian et al. (2015) find that the SNAP cycle affects middle school discipline infractions, with more violations at the end of the benefit month.

Leveraging the community eligibility provision in school meals, Gordon and Ruffini (2018) find a decline in suspensions for elementary and middle school students. Relatedly, **?** show that changes to food stamp disbursement schedules lead to large reductions in theft and crime located at grocery stores. Looking at the SNAP schedule timing shows that reductions are concentrated in weeks two and three after the disbursement, but there are increases in crime in the week before disbursement.

This body of work shows some work disincentives with the more generous programs but that these benefits can affect nonhealth, non-nutrition outcomes such as test scores and crime and noncognitive outcomes such as school suspensions. There is great potential for more study of these sorts of outcomes using merged administrative data.

3.4.5. Long-Run Effects. Next, we turn to a discussion of the long-run effects of these food assistance programs. We start by evaluating the evidence of SNAP, about which there are more existing papers. We note that these are part of a larger literature discussing the rollout design in the United States and Europe.

This literature tends to focus on effects detectable in vital statistics and registry data sets and often (especially in the European context) is unable to consider mechanisms owing to a lack of similarly large and accurate data on the proposed mechanisms at the time of rollout.

First, Hoynes et al. (2016) make use of the now familiar rollout design to consider long-run effects of being exposed to the Food Stamp Program from conception through age five. They use the PSID, and their main results refer to a disadvantaged sample of children (parents had less than a high school education) born between 1956 and 1981. This is linked to the fact that this sample of children whose parents had low levels of education had higher probabilities of being in the program as children (i.e., they used it more intensely). The authors draw on an existing literature about animal models and from quasi-experimental variation in exposure to poor nutrition in-utero in the Dutch winter. Hoynes et al. (2016, p. 905) found that the lack of good nutrition early in life leads to an increase in metabolic syndrome ("a cluster of conditions including obesity, high blood pressure, heart disease, and diabetes"). This warrants research into whether this also affects these individuals once they reach middle age. They interpret these effects as the result of a change in the economic environment, based on their previous work which found that families treated these resources like cash.

Metabolic syndrome is the average of the z-scores of obesity, high blood pressure, diabetes, and heart disease, standardized with the pre-rollout means and standard deviations. Self-sufficiency is also an index, composed of standardized means, including four statuses (not in poverty in adulthood, not on food stamps, not on TANF, being a high school graduate), earnings levels, and family income. Finally, Hoynes et al. (2016) look at self-rated health.

For the high-impact sample, living in a county with food stamps from conception through age five leads to a statistically significant and economically meaningful decline of about 30% of a standard deviation in metabolic syndrome. The associated treatment effect on the treated estimate is even larger, suggesting that exposure to food stamps leads to decline of 0.44 standard deviations in metabolic syndrome. Reduced-form estimates also indicate a statistically significant difference in stunting (it decreases by 6 percentage points). The authors present results stratified by gender, finding slightly larger reductions in metabolic syndrome effects for high participation males (–0.53) compared to females (–0.31). Finally, there is a large statistically significant increase in self-sufficiency for women (0.306, significant at the 10% level for women, but essentially no effect for high-participation men). To put these effects in context, the authors discuss other evidence of large impacts of early interventions.

Several other recent studies and works in progress build on this strategy. Bailey et al. (2020) are conducting ongoing research using Decennial Census data and American Community Survey data linked to the Social Security Administration's NUMIDENT file on place of birth to evaluate how exposure is associated with outcomes in adulthood. They find evidence that childhood exposure to food stamps has important impacts in adulthood, which increases human capital accumulation, reduces mortality, and improves the quality of the neighborhoods in which these individuals live. For nonwhites, they find substantial reductions in incarceration. Bitler and Figinski (2018) leverage the same study design with the Continuous Work History Sample, a large panel data set of Social Security earnings records and involvement with the Social Security Disability Income (SSDI) program as well as data from the NUMIDENT file on place of birth. They use these highly accurate administrative data to examine the effects on earnings and use of SSDI.Using a balanced panel of age cohorts born from 1955 to 1980, they find that for women, exposure to food stamps from conception through age five leads to an approximately 3% increase in earnings at age 32 but has no effect on SSDI. Effects for men are more mixed. Finally,Barr and Smith (2018) use variation from the rollout of the Food Stamp Program to study its effects on crime. In North Carolina data, they show a large decline (about 3%) in the probability of being convicted of a crime by age 23 for each year of exposure to food stamps between conception and age five. These findings are then bolstered by national findings on arrests, using data from the Uniform Crime Reporting program. We end by referring to some interesting findings about the introduction of healthful school meals in Scandinavia and expansion of school meals in the United States. Hinrichs (2010) leverages a strategy similar to the rollout studies discussed earlier, using a change in the funding formula for the NSLP in 1963 to evaluate long-run effects. The funding formula moved from one based on population to one more associated with participation in the program, although both allocated more funding to poorer places. He finds few or no effects on health but finds evidence of increased educational attainment of about one year for men and somewhat smaller effects for women.[5] Bütikofer et al. (2018) use the introduction of nutritious and free breakfasts in Norwegian cities in the 1920s. These meals replaced hot meals at the end of the day, which had a similar number of calories but were less nutritious (had fewer valuable micronutrients). Within a panel of cities in Norway using a difference-in-differences design, they find that earnings in adulthood are 2–3% higher, and schooling attainment is better. There are even intergenerational effects on children of the men. Even more interesting, Alex-Petersen et al. (2017) study the rollout of nutritious and free lunches in Sweden from 1959 to 1969. Again, using a difference-in-differences design, they find that implementation led to a 3% increase in lifetime earnings. Effects are larger for the poorest households. The mediators explored by the authors include educational attainment (from educational registry data) and height (a measure of health from military service records). Taken together, these studies suggest important long-run effects of expanding food assistance or increasing nutrition content of meals on health and income. Future research will likely shed further light on the mechanisms, but Alex-Petersen et al. (2017) is particularly important in showing the effects of food assistance on height (a measure of health) and educational success.

[5]Hinrichs (2010) examines height, health limitations, body mass index (BMI), various BMI cut points, and self-rated assessments of poor or fair health for men or women based on 1976–1980 National Health Interview Survey data on adults born between 1941 and 1956. The results on health outcomes are not significant, although sometimes the coefficients are in a direction indicating positive effects. When he looks at educational attainment using the 1980 Census and an order-of-magnitude-larger samples, Hinrichs finds positive effects that are statistically significant at the 10% level, with state-level clustering on school attainment, centered around 12 and 13 years.

3.5. Conclusion

In this review, we have evaluated the agricultural economics and economics literatures on the effects of US food assistance programs, with a focus on health and nutrition. We have also included a discussion of important findings from the behavioral literature and touched on the impacts on mediators, such as labor supply and human capital (cognitive and noncognitive). Finally, we have discussed both the short- and long-run effects of these programs on health and nutrition in the United States.

Summary Points:

(1) There is evidence of the various programs having short-run intended but sometimes unintended effects on health and other outcomes.

(2) These effects work through nutrition but also through other channels such as increasing both education and self-sufficiency.

(3) Long-run and even intergenerational effects of food assistance programs on health, education, and earnings are economically meaningful.

Future Issues:

(1) There will likely be ongoing tension between understanding what programs do now and capturing all of the costs and benefits of said programs.

(2) Data constraints on the use of administrative data and linking the sources of those data across settings and programs limit our knowledge, but those limitations will hopefully be eased in the future.

3.6. Tables and Figures

Table 1: Summary of program details for all major federal food and nutrition programs in the United States

Program	Targeted population	Eligibility	Caseloads	Expenditures	Type of assistance
Supplemental Nutrition Assistance Program (SNAP)	Nearly universal	≤130% of FPL and asset requirements	42 million	$68.1 billion	Cash to purchase nonprepared meals
National School Lunch Program (NSLP)	Children until age 19	≤185% of FPL or categorical eligibility	30 million	$12.3 billion	Schools receive cash reimbursement and USDA foods
Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)	Pregnant, postpartum, and breastfeeding women; children until age 5	≤185% of FPL or categorical eligibility	7.3 million	$5.6 billion	Credits for specified food items
School Breakfast Program (SBP)	Children until age 19	≤185% of FPL or categorical eligibility	14.7 million	$4.3 billion	Schools receive cash reimbursement and USDA foods
Child and Adult Care Food Program (CACFP)	Universal	≤185% of FPL or categorical eligibility	4.5 million	$3.5 billion	Reimbursement for meals and snacks served at eligible institutions
The Emergency Food Assistance Program (TEFAP)	Nonprofit organizations	NA	NA	$660 million	Emergency nutrition assistance to nonprofits (i.e., food banks, community action agencies)
Congregate Nutrition Services	Ages 60+ and disabled persons	NA	1.6 million	$450 million	Meals and related nutrition services in congregate settings
Summer Food Service Program	Children until age 19	≤185% of FPL or categorical eligibility	2.6 million	$483 million	Reimbursement for free summer meals to children in low-income areas
Home-Delivered Nutrition Services	Ages 60+ and disabled persons	NA	900,000	$227 million	Home-delivered meals
Commodity Supplemental Food Program	60+	≤130% of FPL	630,000	$204 million	Monthly food package with limited commodities
Fresh Fruit and Vegetable Program	Children until age 19	≤185% of FPL or categorical eligibility	NA	$201 million	Reimbursements for fresh fruits and vegetables for students in low-income elementary schools
Food Distribution Program on Indian Reservations (FDPIR)	American Indian households	≤130% of FPL	90,000	$122 million	Provides USDA foods in lieu of SNAP
Special Milk Program	Children until age 19	≤185% of FPL or categorical eligibility	41.3 million half-pints	$8.3 million	Provides milk to children in schools and childcare

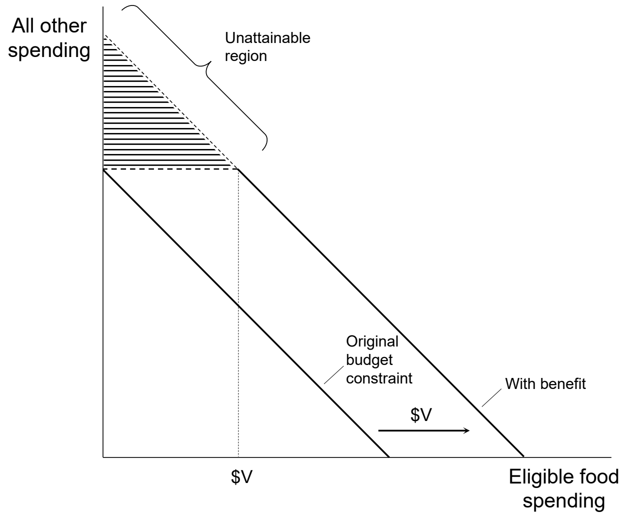

Figure 1
Budget constraint before and after receiving food voucher

Figure 1: Budget constraint before and after receiving a food voucher with benefit equal to $V. The region in gray is unattainable because, unlike cash, the voucher can only be used for eligible food spending.

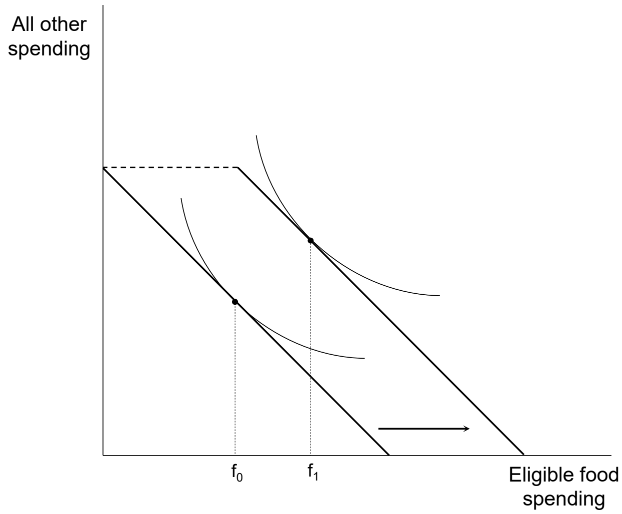

Figure 2
Inframarginal household

Figure 2: Spending decision of an inframarginal household before and after receiving a food voucher. An inframarginal household makes the same decisions whether receiving a food voucher or an equivalent amount of cash. This household originally chooses to spend f_0 on food and increases food spending to f_1 after receiving a food voucher. The inframarginal household moves from indifference curve u to indifference curve u' in either case. The voucher is theoretically equivalent to a cash transfer for inframarginal households.

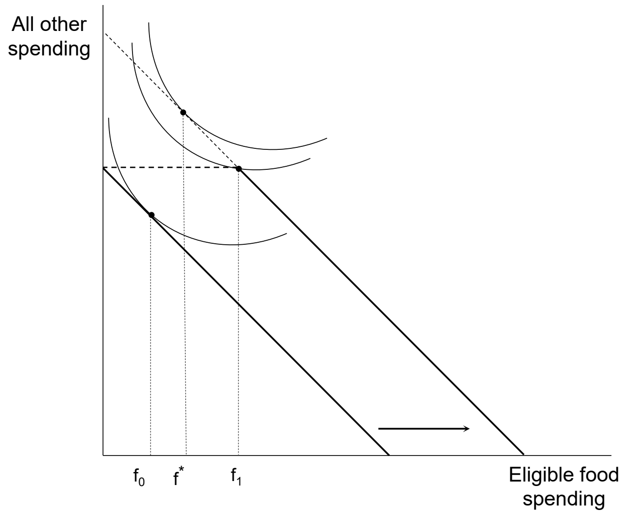

Figure 3
Extramarginal household

Figure 3: Spending decision of an extramarginal household before and after receiving a food voucher. A household is called extramarginal when the receipt of a food voucher constrains them to make a different spending decision than if they had received the same amount in cash. This household originally chooses to spend f_0 on food. A cash transfer would move them from indifference curve u to indifference curve u", where their food spending would be at f*. Instead, the best they can achieve with a food voucher is at f_1 on indifference curve u', where being on indifference curve u' denotes a lower level of utility than being on the indifference curve u". An extramarginal household would receive a larger increase in utility with cash than with a food voucher with equivalent value.

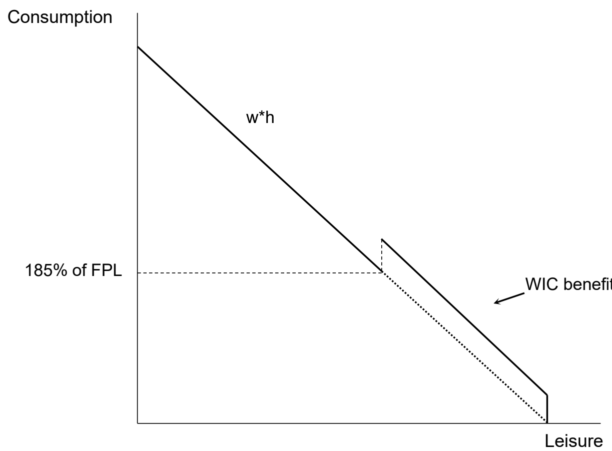

Figure 4
Notch as a result of WIC benefit formula

Figure 4: Notch as a result of Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) benefit formula. The notch occurs when income (measured as hourly wage multiplied by hours worked, or w*h) falls below 185% of the Federal Poverty Level (FPL).

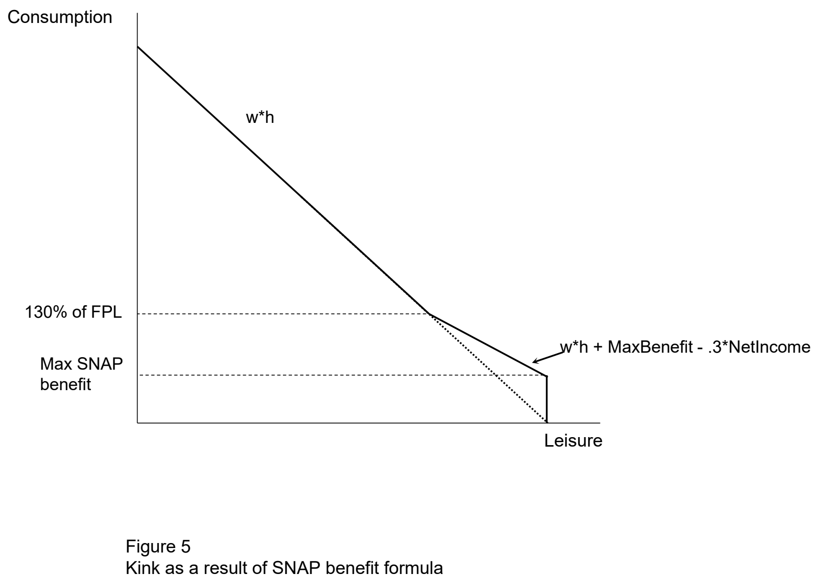

Figure 5: Kink as a result of Supplemental Nutrition Assistance Program (SNAP) benefit formula. The kink occurs when income (measured as hourly wage multiplied by hours worked, or w*h) falls below 130% of the Federal Poverty Level (FPL). At this point, the SNAP benefit is calculated by subtracting 30% of income from the maximum benefit amount, so the benefit is increasing as income is decreasing.

APPENDIX A

Cross Sectional Analysis

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.

A.1. 1975 Figures

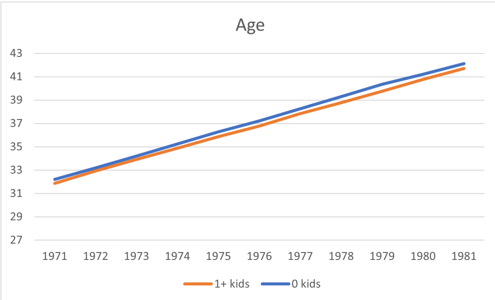

FIGURE A.1. 1975, panel sample

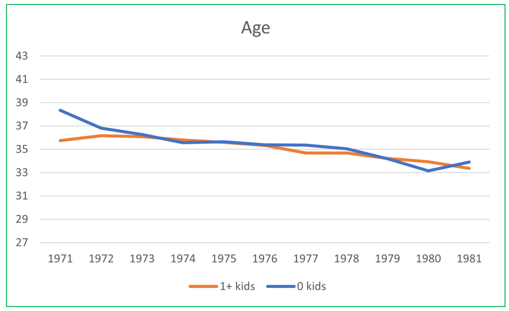

FIGURE A.2. 1975, cross sectional sample

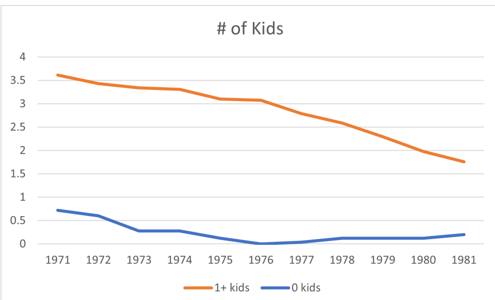

FIGURE A.3. 1975, panel sample

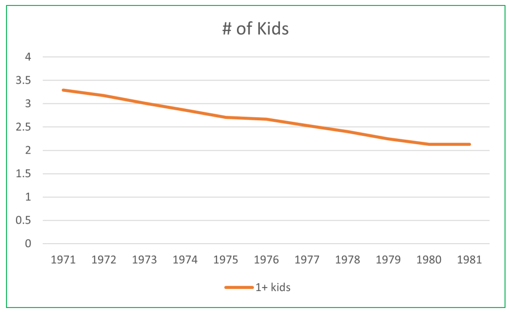

FIGURE A.4. 1975, cross sectional sample

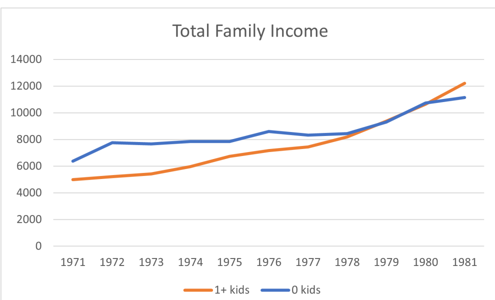

FIGURE A.5. 1975, panel sample

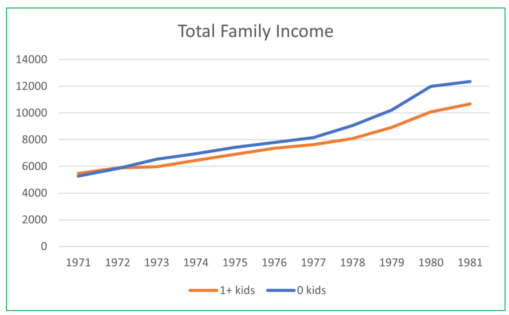

FIGURE A.6. 1975, cross sectional sample

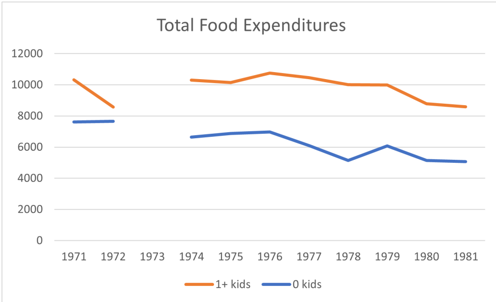

FIGURE A.7. 1975, panel sample

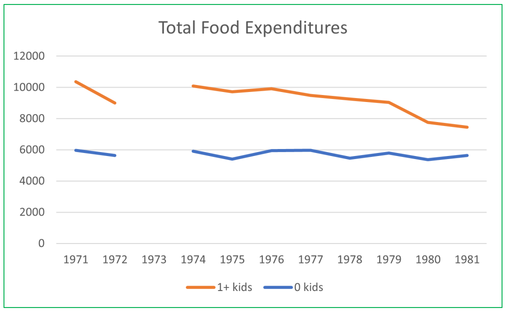

FIGURE A.8. 1975, cross sectional sample

A.2. 1986 Figures

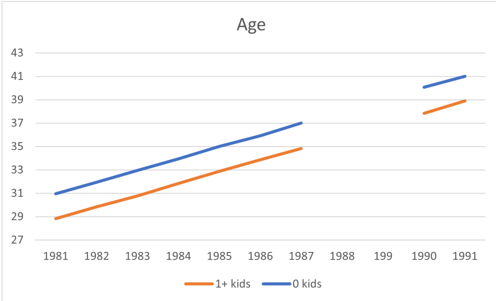

FIGURE A.9. 1986, panel sample

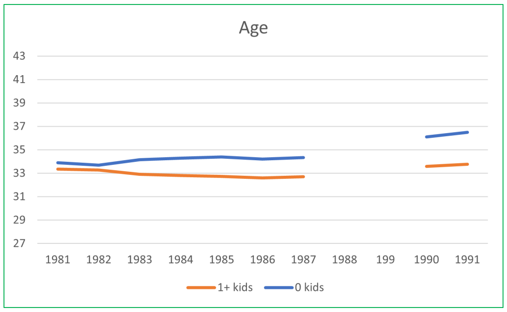

FIGURE A.10. 1986, cross sectional sample

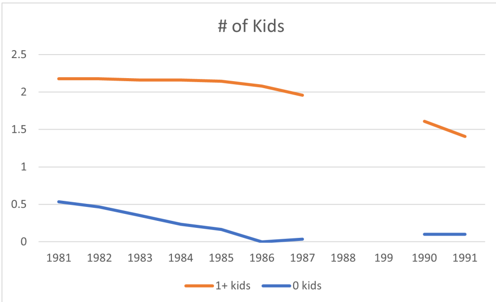

FIGURE A.11. 1986, panel sample

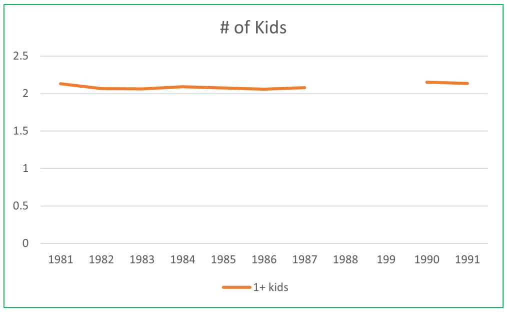

FIGURE A.12. 1986, cross sectional sample

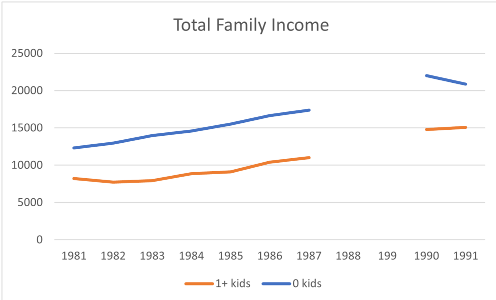

FIGURE A.13. 1986, panel sample

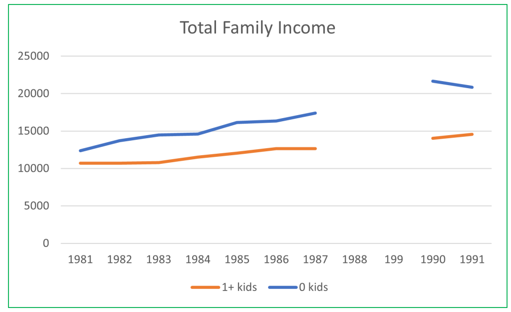

FIGURE A.14. 1986, cross sectional sample

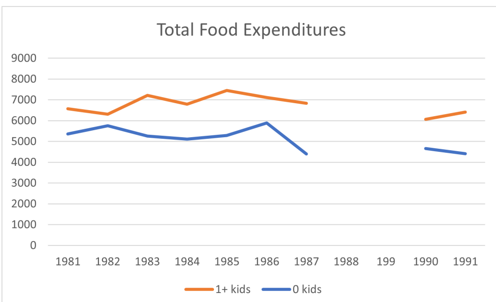

FIGURE A.15. 1986, panel sample

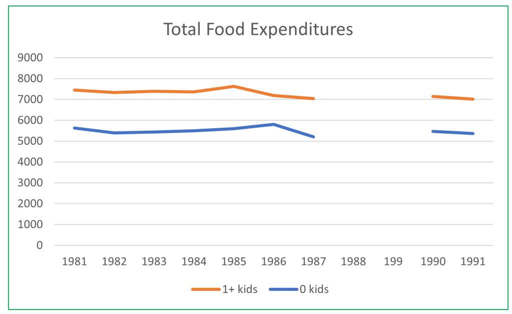

FIGURE A.16. 1986, cross sectional sample

A.3. 1993 Figures

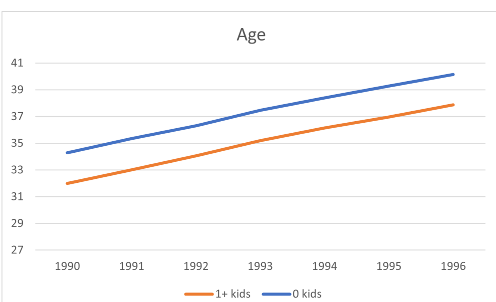

FIGURE A.17. 1993, panel sample

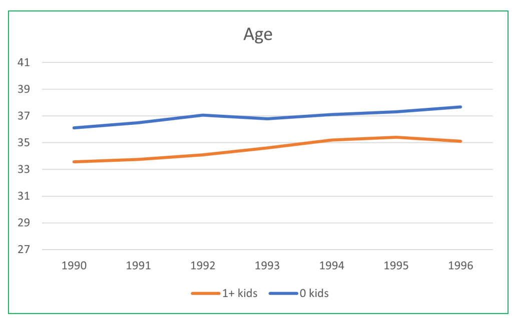

FIGURE A.18. 1993, cross sectional sample

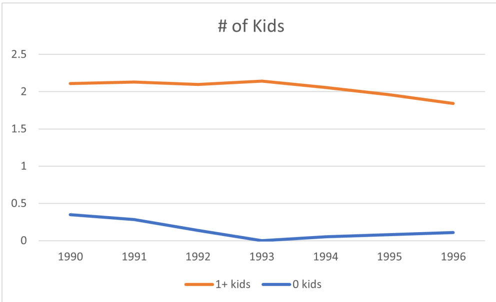

Figure A.19. 1993, panel sample

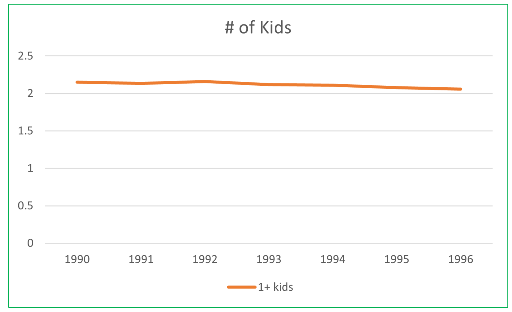

Figure A.20. 1993, cross sectional sample

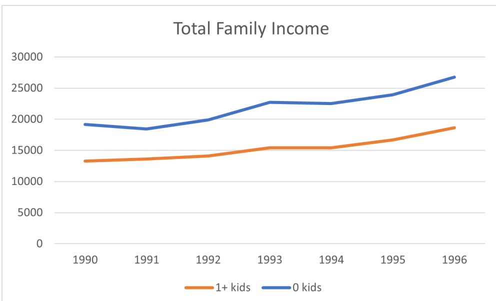

Figure A.21. 1993, panel sample

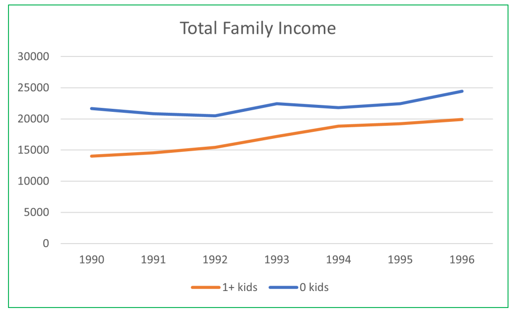

Figure A.22. 1993, cross sectional sample

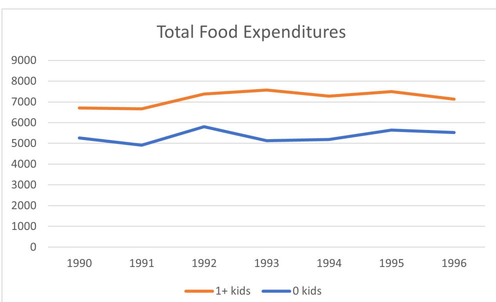

Figure A.23. 1993, panel sample

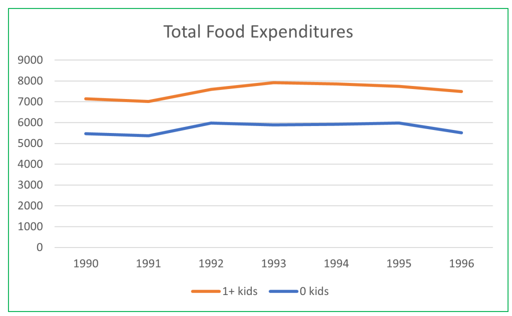

Figure A.24. 1993, cross sectional sample

A.4. 2009 Figures

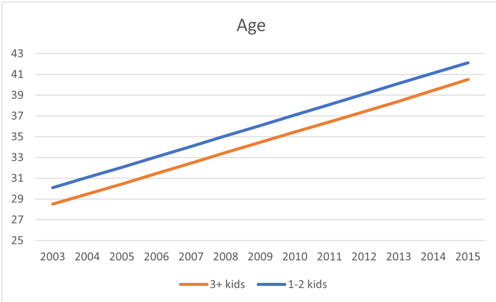

FIGURE A.25. 2009, panel sample

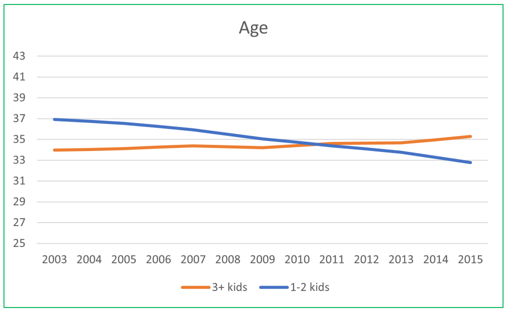

FIGURE A.26. 2009, cross sectional sample

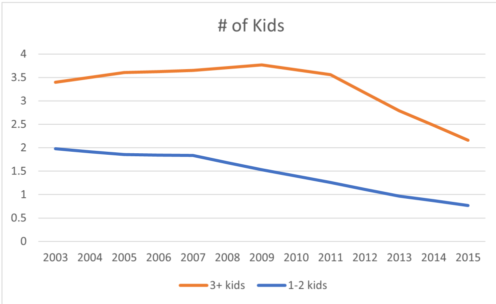

FIGURE A.27. 2009, panel sample

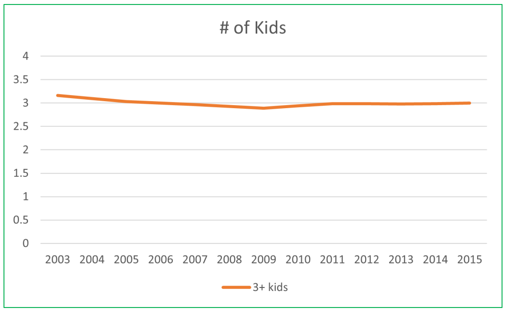

FIGURE A.28. 2009, cross sectional sample

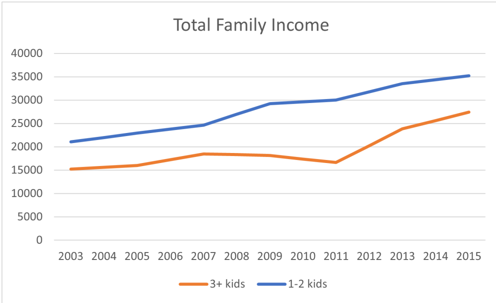

FIGURE A.29. 2009, panel sample

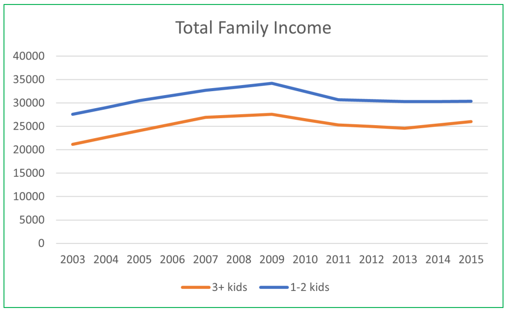

FIGURE A.30. 2009, cross sectional sample

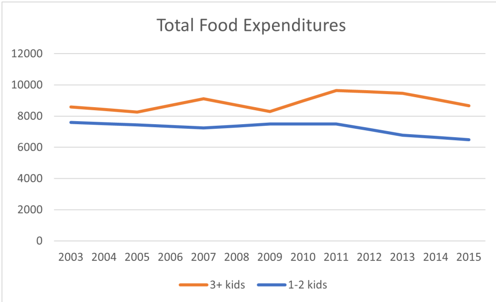

FIGURE A.31. 2009, panel sample

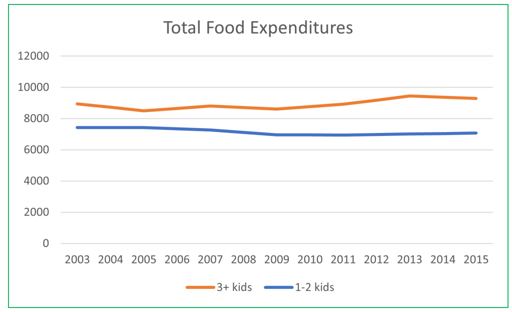

FIGURE A.32. 2009, cross sectional sample

APPENDIX B

Sample Analysis

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 B.1 through B.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 B.1 is for families with exactly 1 child in the household, Figure B.2 is for 2 children, and Figure B.3 is for families with 3 children.

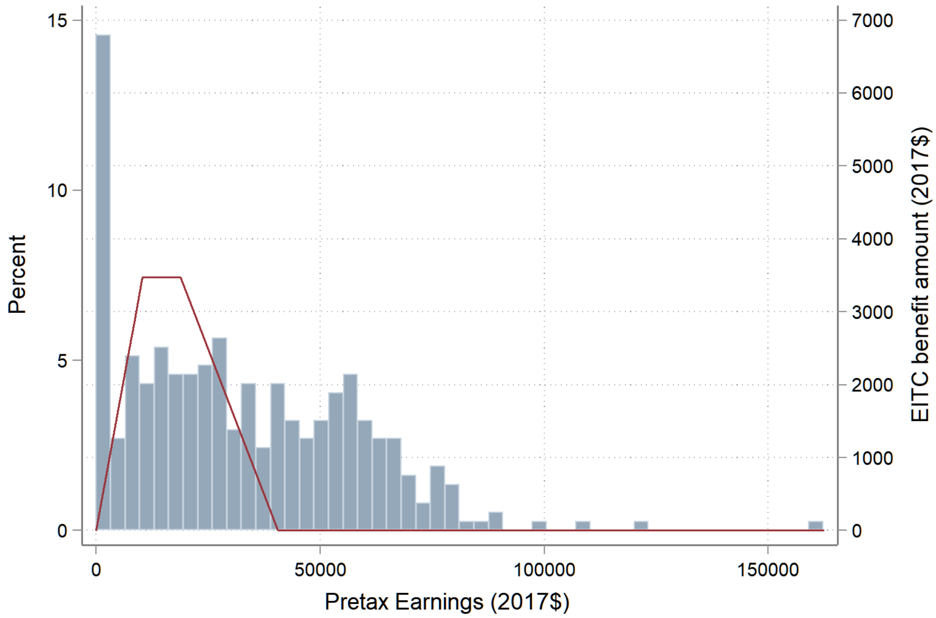

Figure B.1: 2009 EITC benefit schedule overlaid on top of distribution of incomes of women in the sample with exactly one child present in the sample in 2009. The left axis shows the percentage of women in each income bin in 2009, and the right axis shows the dollar amount of EITC benefits by family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

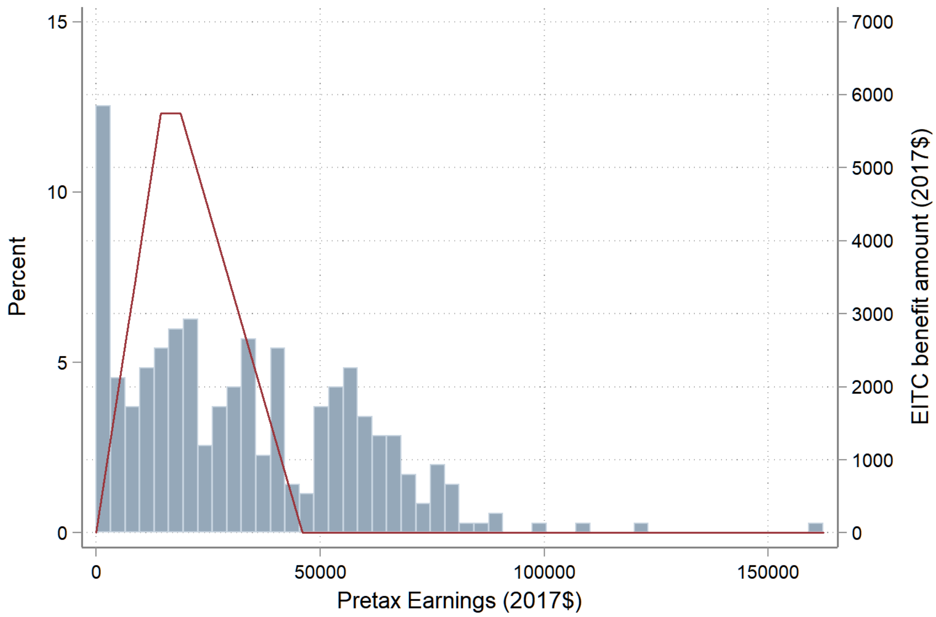

Figure B.2: 2009 EITC benefit schedule overlaid on top of distribution of incomes of women in the sample with exactly two children present in the sample in 2009. The left axis shows the percentage of women in each income bin in 2009, and the right axis shows the dollar amount of EITC benefits by family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

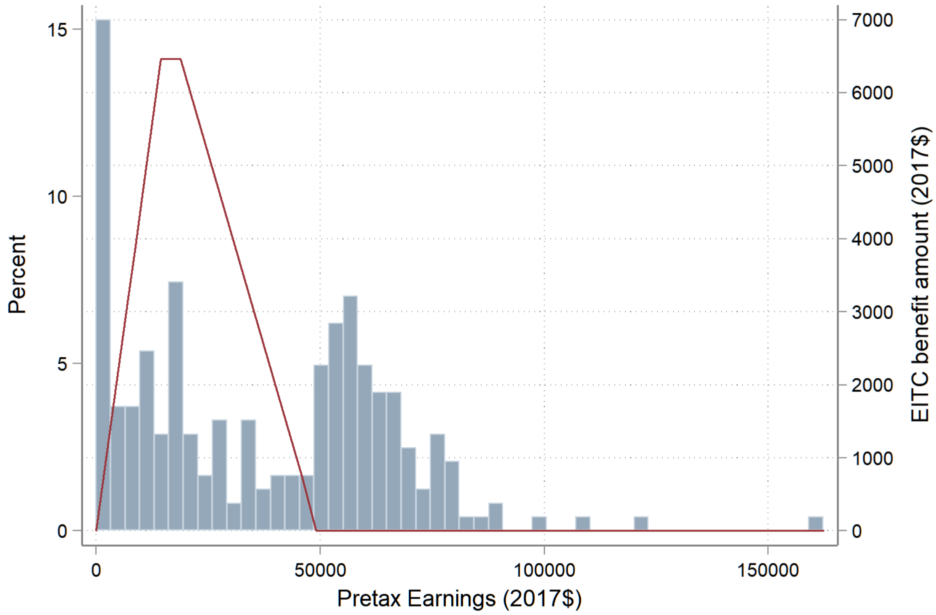

Figure B.3: 2009 EITC benefit schedule overlaid on top of distribution of incomes of women in the sample with three or more children present in the sample in 2009. The left axis shows the percentage of women in each income bin in 2009, and the right axis shows the dollar amount of EITC benefits by family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

APPENDIX C

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

[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.

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 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.

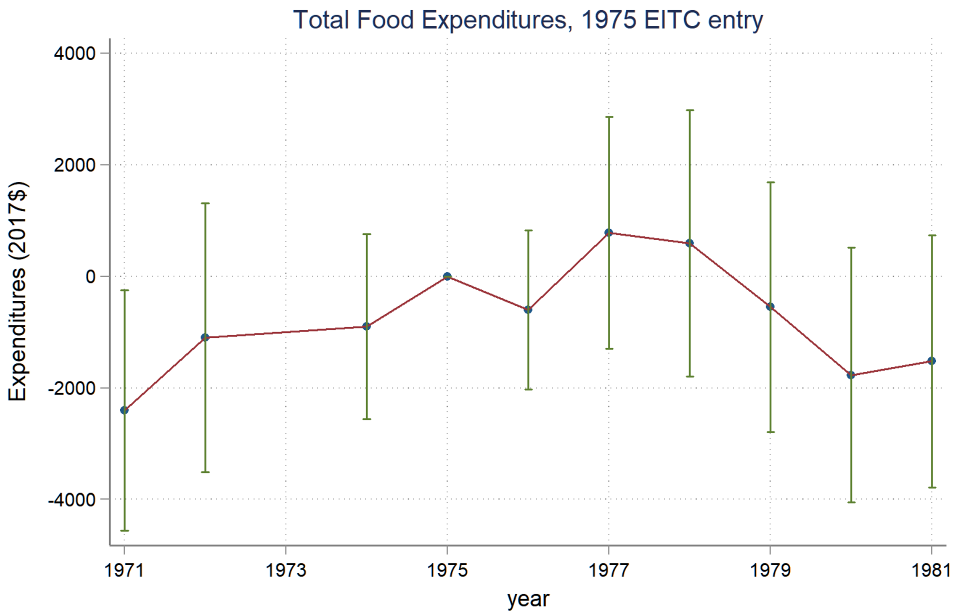

Figure C.1: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 1975 EITC introduction, effective for the tax year 1976. Food expenditures at home 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.

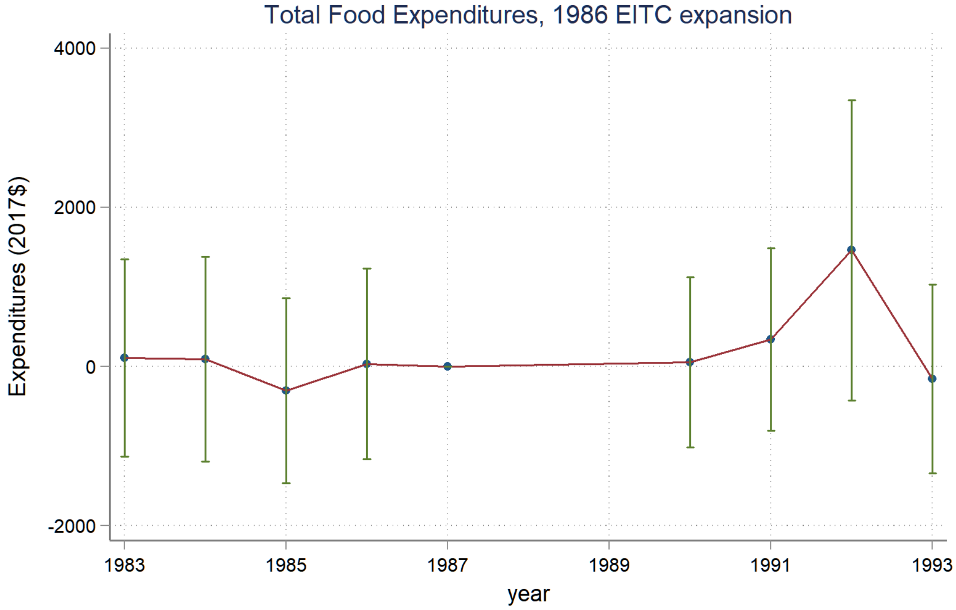

Figure C.2: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 1986 EITC policy change. Food expenditures at home include food purchased using Food 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.

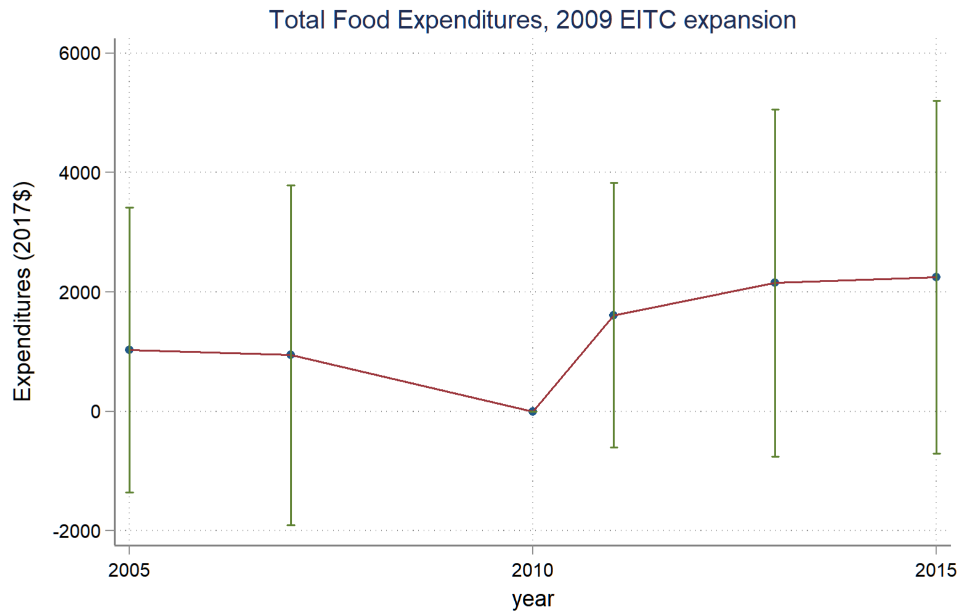

Figure C.3: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 2009 EITC policy change. Food expenditures at home include food purchased using Food 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.

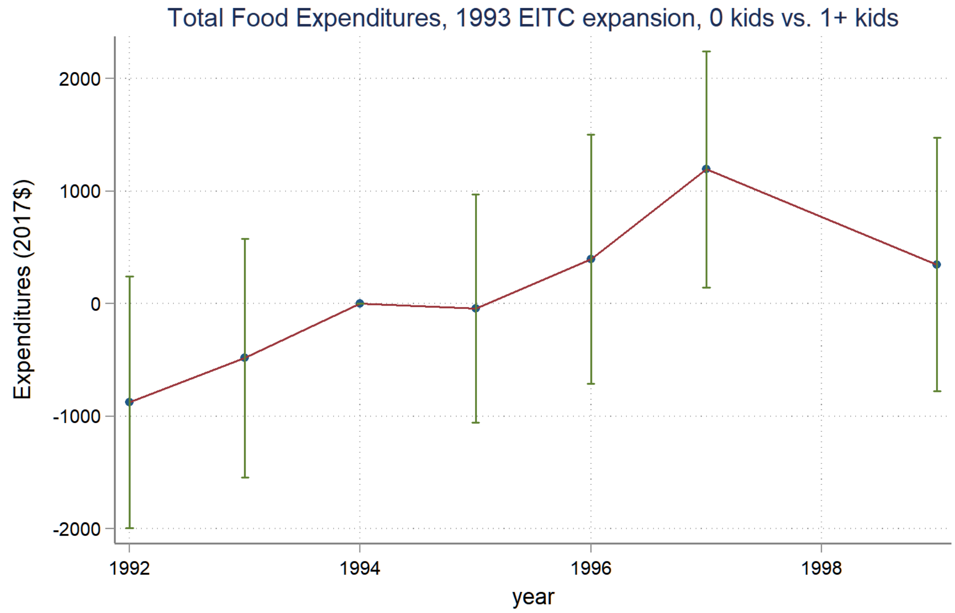

Figure C.4: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 1993 EITC policy change. Food expenditures at home include food purchased using Food 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.

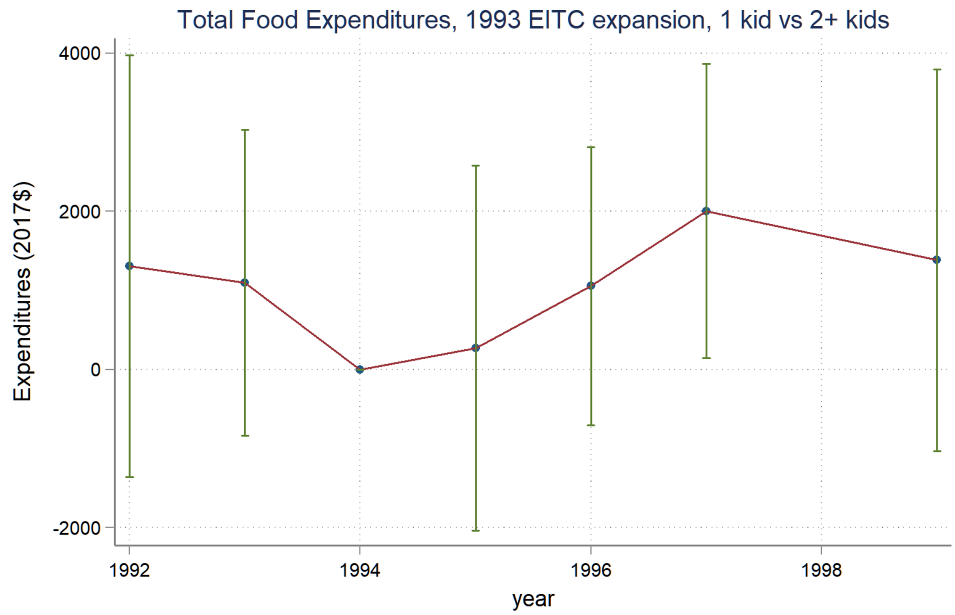

Figure C.5: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single and married households, EITC eligibility imputed based on income, for the 1993 EITC policy change. Food expenditures at home include food purchased using Food 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.

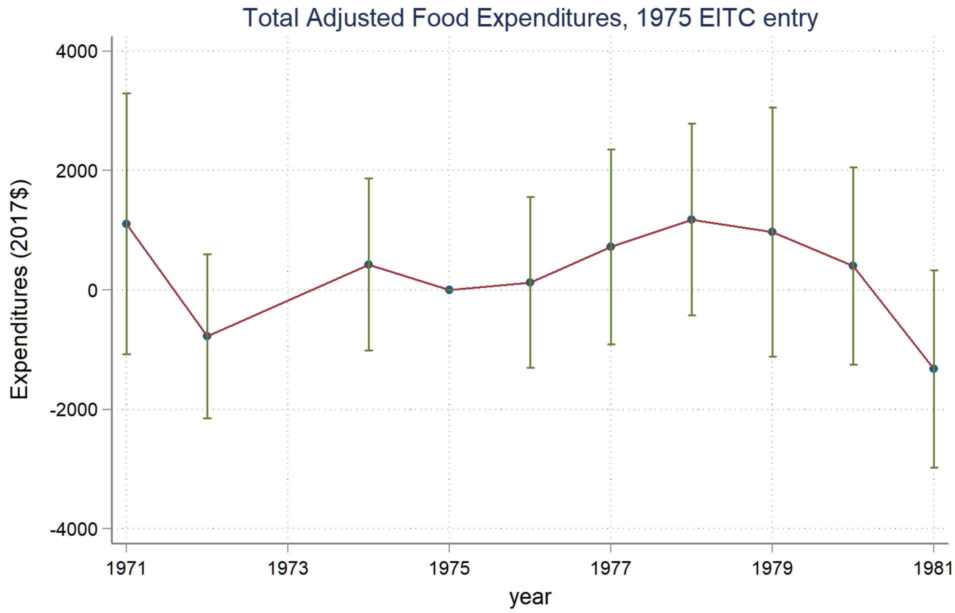

Figure C.6: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1975 EITC introduction, effective 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.

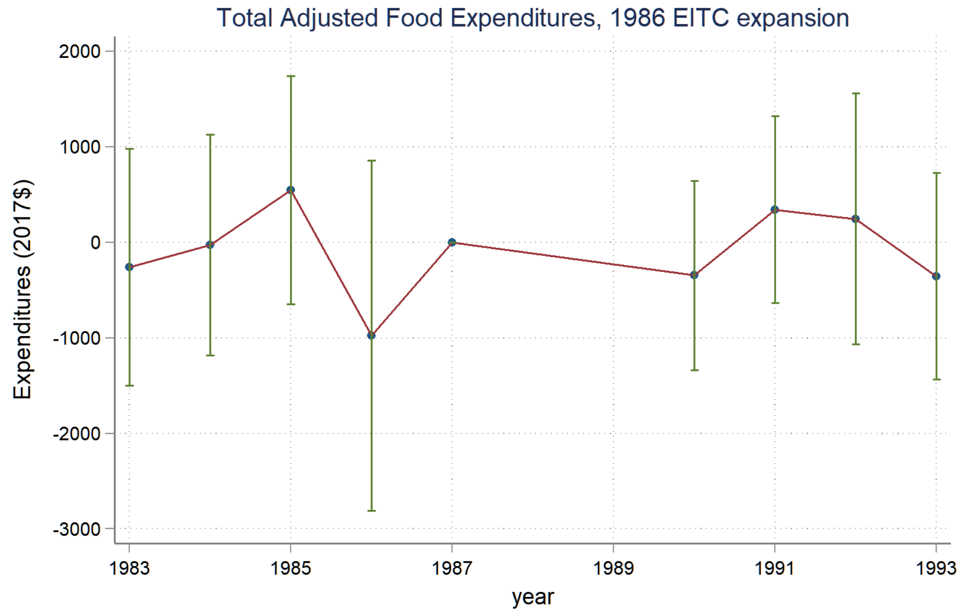

Figure C.7: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1986 EITC policy change. Food expenditures 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.

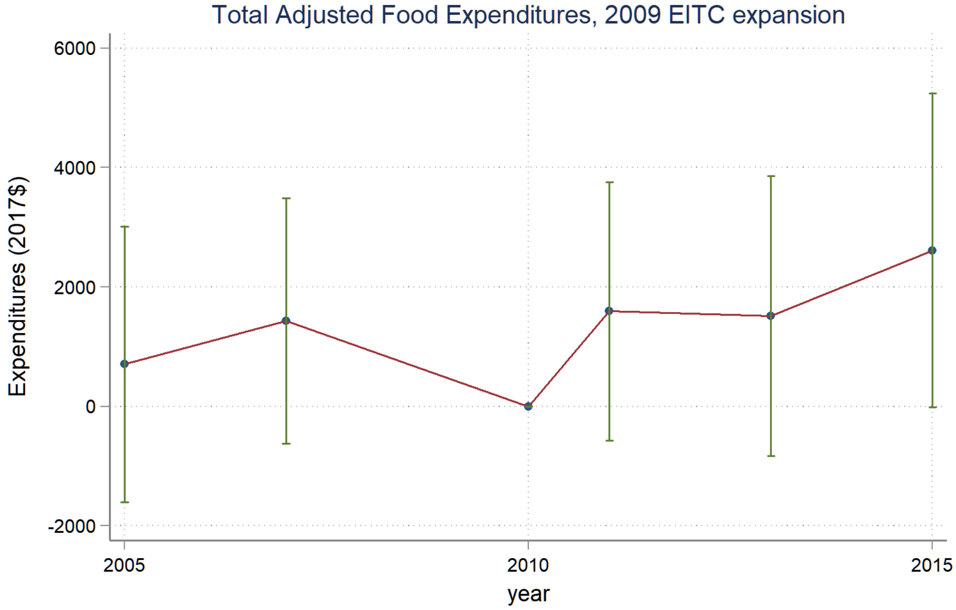

Figure C.8: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 2009 EITC policy change. Food expenditures 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.

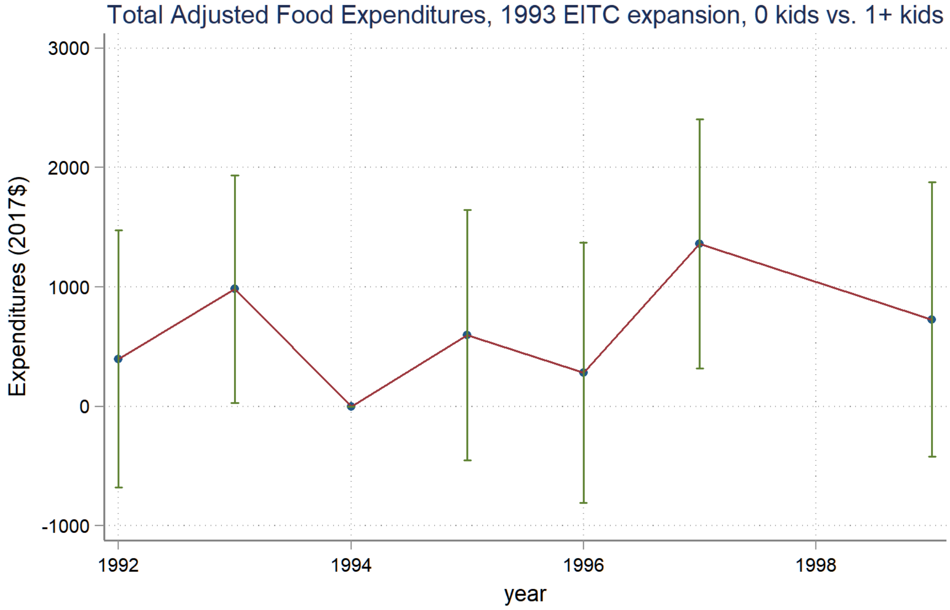

Figure C.9: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures 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.

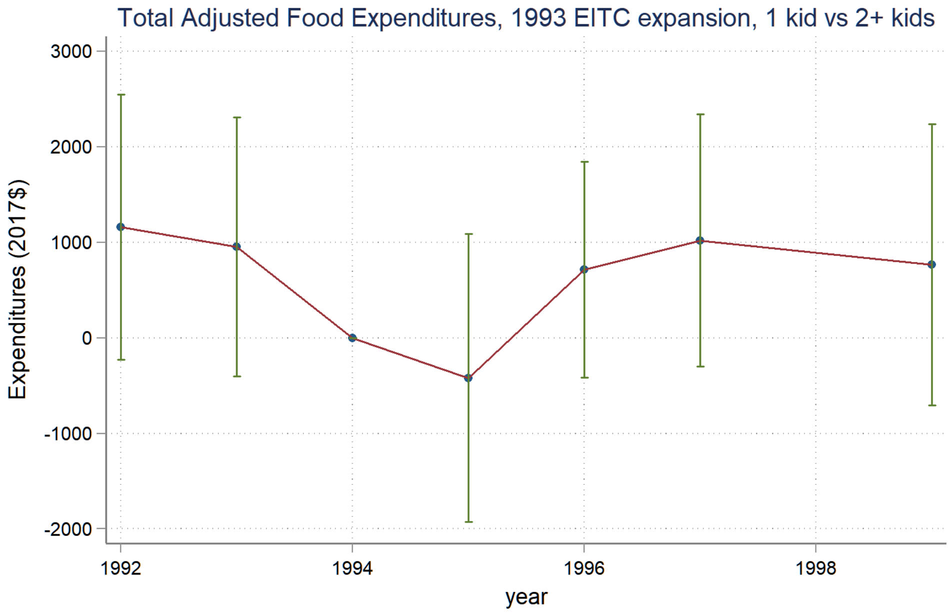

Figure C.10: Dynamic difference-in-differences graph showing adjusted food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food 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.

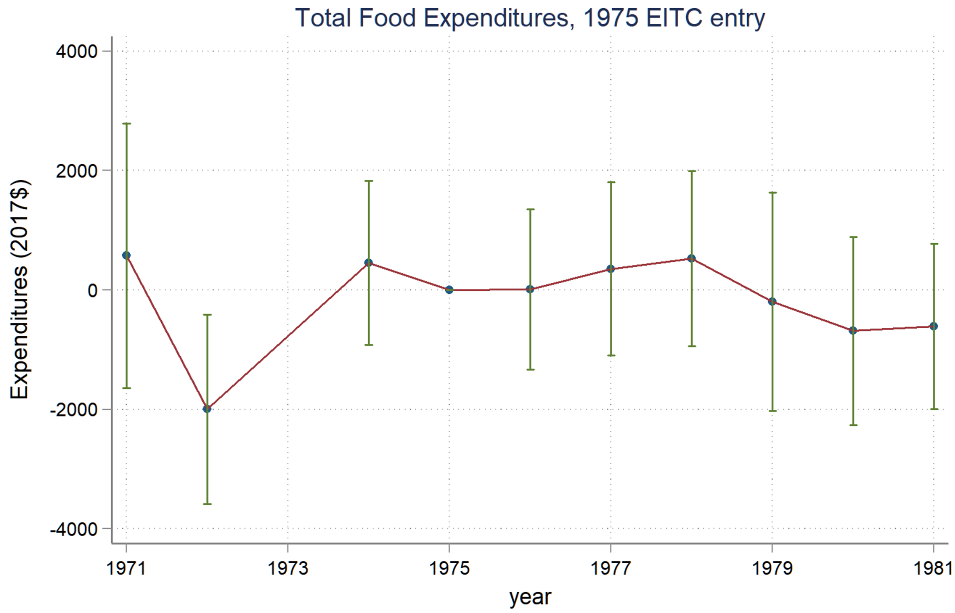

Figure C.11: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1975 EITC introduction, effective for the 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.

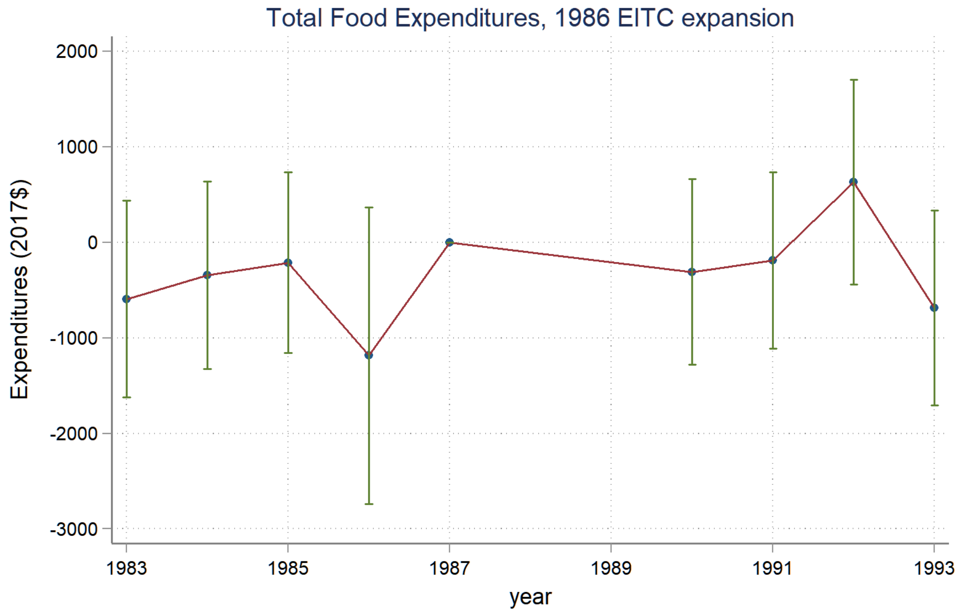

Figure C.12: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1986 EITC policy change. Food expenditures 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.

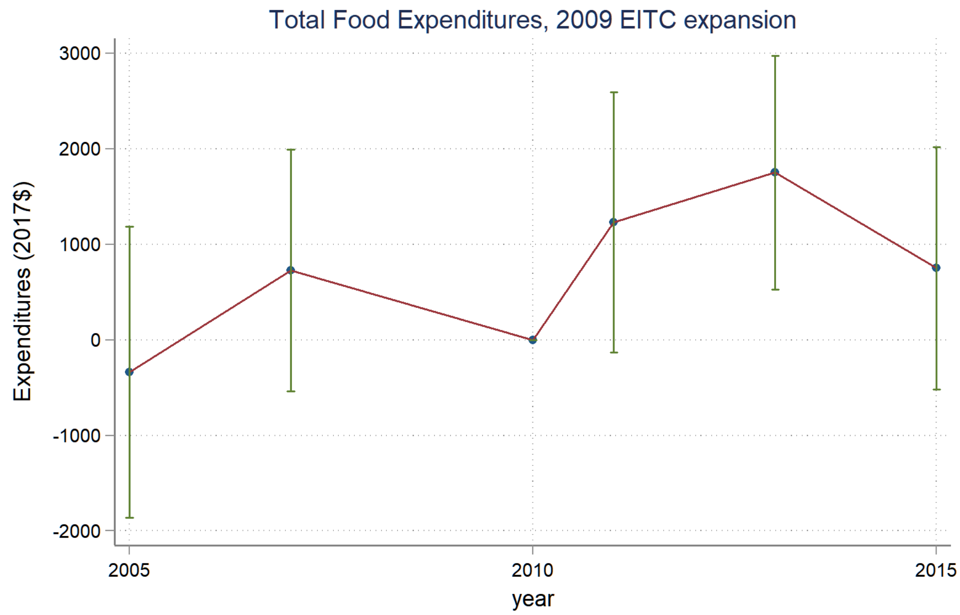

Figure C.13: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 2009 EITC policy change. Food expenditures 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.

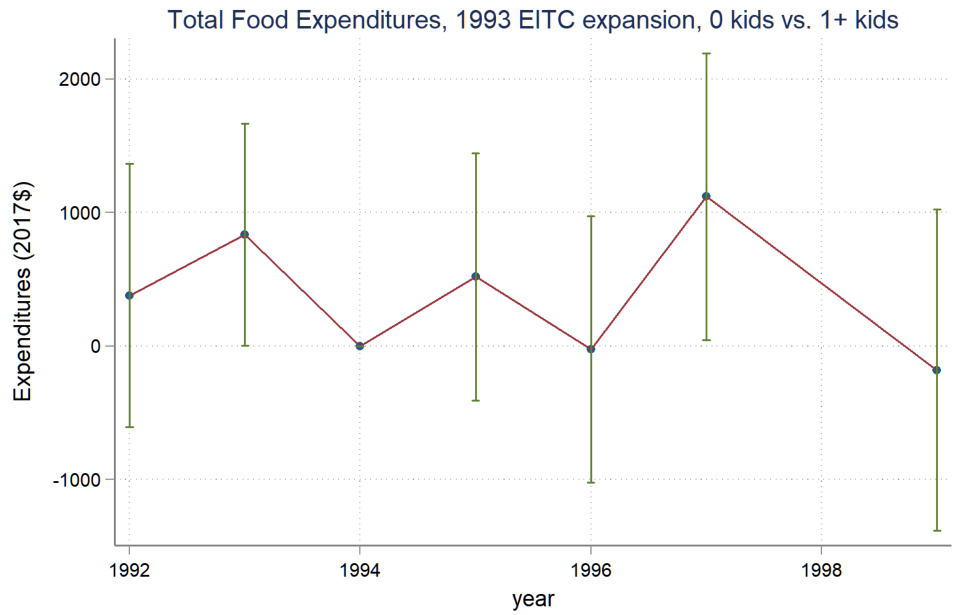

Figure C.14: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures 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.

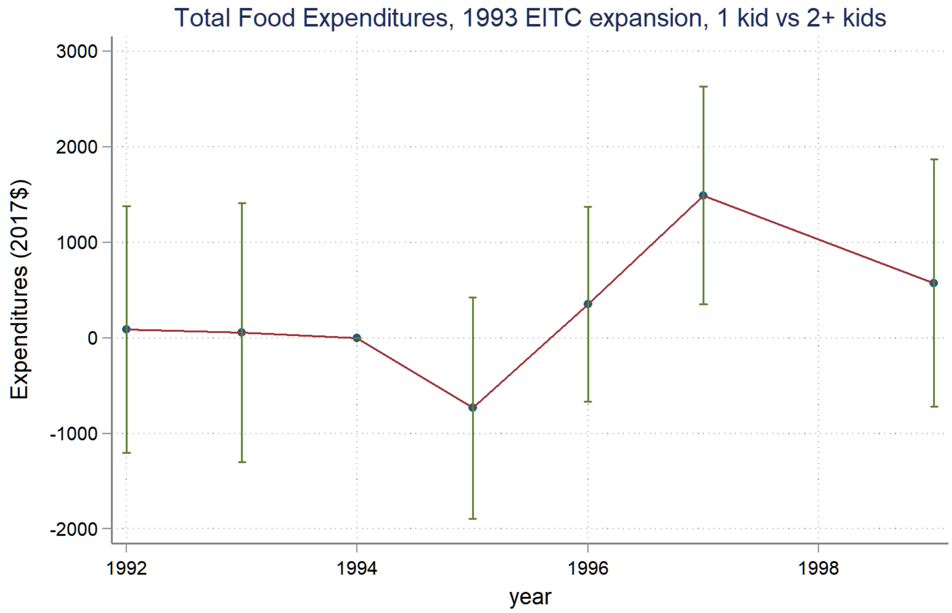

Figure C.15: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures 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.

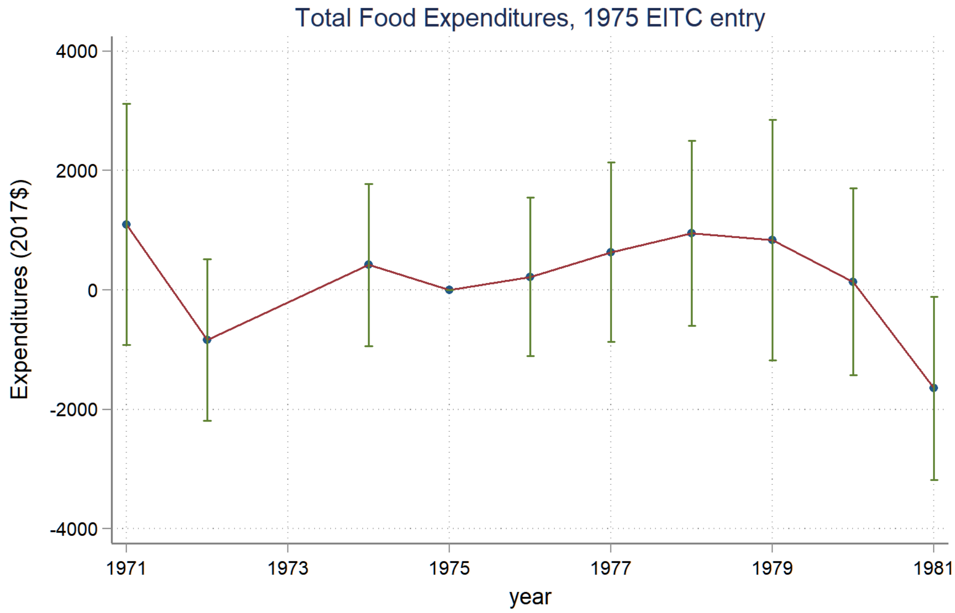

Figure C.16: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1975 EITC introduction, effective for the 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.

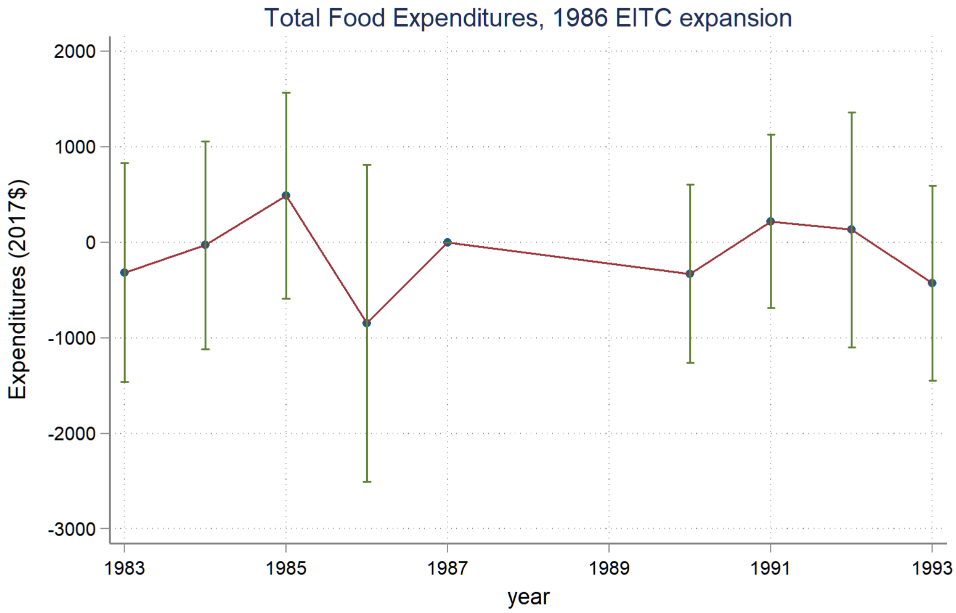

Figure C.17: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1986 EITC policy change. Food expenditures 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.

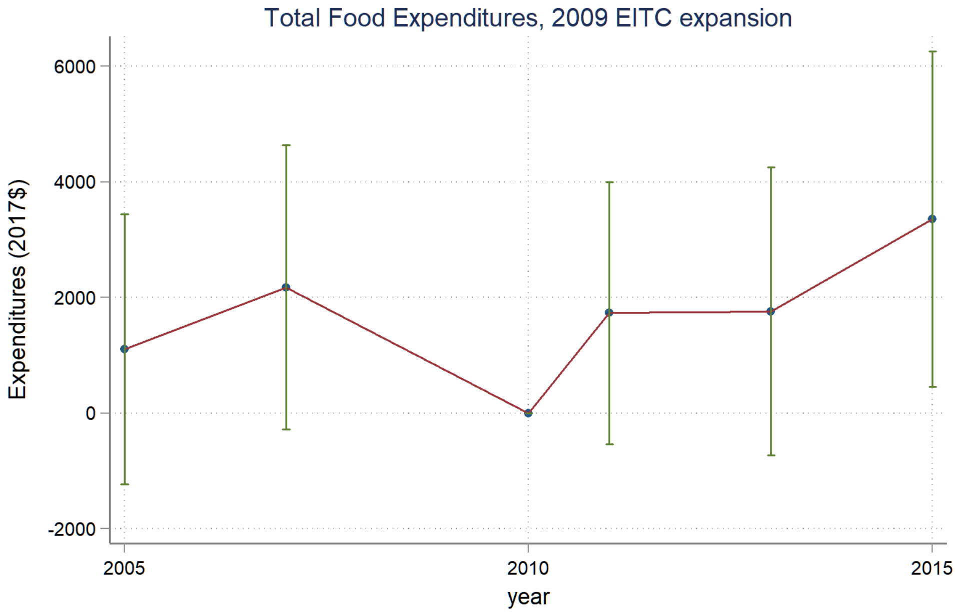

Figure C.18: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 2009 EITC policy change. Food expenditures 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.

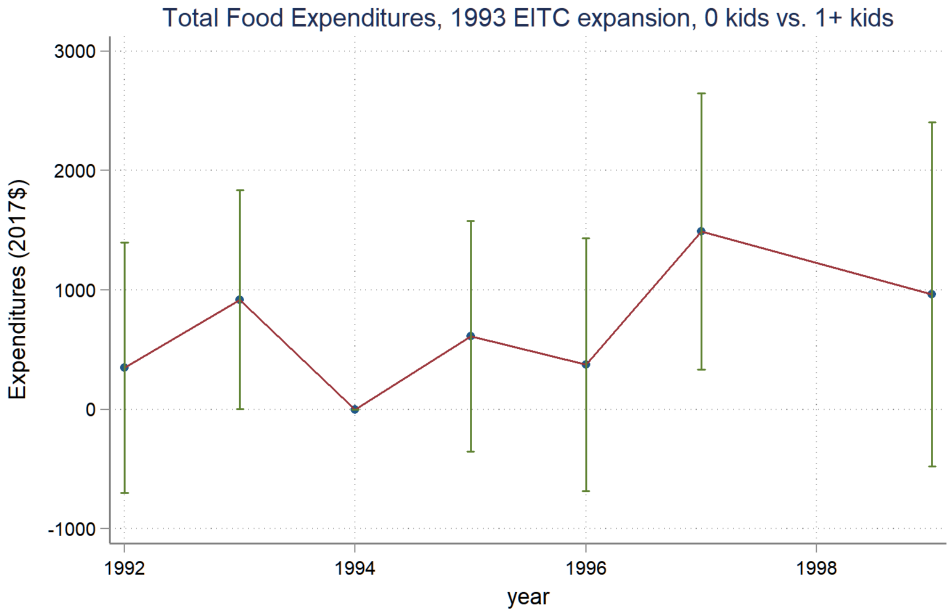

Figure C.19: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures 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.

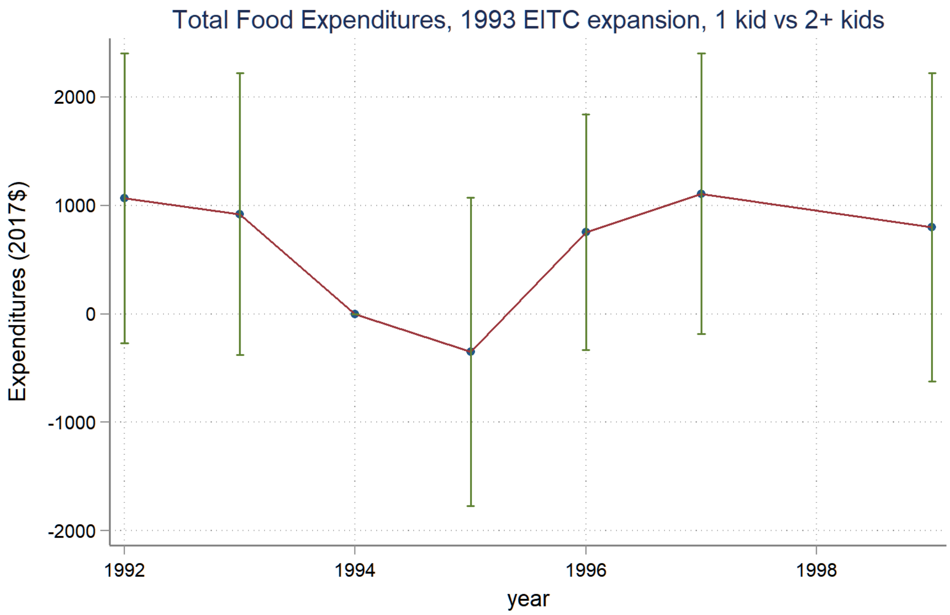

Figure C.20: Dynamic difference-in-differences graph showing food expenditures at home, out (at restaurants), and delivered to the home for single female headed households (head of household between the ages of 24-48 with some college or less education) for the 1993 EITC policy change. Food expenditures 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.

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