Income and the Take-Up of Means-Tested Programs

Amanda Eng[*] Kevin Rinz[†]

November 2, 2020

CLICK HERE FOR LATEST VERSION

Abstract

Pro-work policies usually decrease household participation in traditional safety-net programs like the Supplemental Nutrition Assistance Program (SNAP) and the Temporary Assistance for Needy Families (TANF) program. This negative relationship could be driven by newly working households becoming more self-sufficient or by decreased eligibility and higher costs to participate in SNAP and TANF. Understanding which of these factors drives the negative relationship between income and program participation is important for understanding the mechanisms driving take-up decisions and for designing effective policies. However, the design of SNAP and TANF makes it difficult to distinguish these factors. In this paper, we estimate how demand for SNAP and TANF changes with income, holding eligibility and take-up costs constant. We use a discontinuity in child tax benefits, which do not affect program eligibility, to isolate the effect of income on program participation. We additionally show evidence that take-up costs are the same for households on either side of the discontinuity. We find that although eligibility for tax credits decreases households' tax liability by $2,219 on average, the additional income results in no measurable difference in program participation. These findings suggest that the negative correlation between income and program take-up is driven by households losing eligibility or facing greater participation costs and that there could be significant benefits to expanding eligibility for these programs to more working households.

[*]Economics Department, Cornell University.

[†]U.S. Census Bureau.

Any opinions and conclusions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release (Approval IDs: CBDRB-FY20-212 and CBDRB-FY20-418).

1 Introduction

For over three decades, many policy makers in the United States have been focused on the goal of moving low-income families from welfare to work. An implicit assumption underlying this goal is the idea that if individuals work, they will become self-sufficient and no longer need support from government programs. Early evaluations of welfare reforms and expansions of the Earned Income Tax Credit (EITC), a tax credit designed to encourage low-income families to work, concluded that the policies were successful since they increased labor supply and decreased caseloads in the cash welfare and food stamp programs (Council of Economic Advisers, 1999; Grogger, 2003; O'Neill & Hill, 2001). However, more work and falling caseloads are not necessarily a sign of decreased need or demand for government support (Blank & Ellwood, 2001; McKernan & Ratcliffe, 2003; Meyer & Sullivan, 2008; Zedlewski & Brauner, 1999). The negative relationship between work and program participation could also be driven by full or partial loss of eligibility or changing costs of using government programs. Distinguishing these mechanisms is important for understanding the efficiency and welfare effects of pro-work policies.

This paper directly estimates how low-income families' demand for government programs changes with income in the absence of changes to eligibility or costs. We focus on participation in the Supplemental Nutrition Assistance Program (SNAP), formerly known as food stamps, which is one of the major safety net programs in the United States. We also examine participation in traditional cash welfare through the Temporary Assistance for Needy Families (TANF) program, although this program now plays a much smaller role in the current safety net (Bitler & Hoynes, 2010). We estimate how the demand for SNAP and TANF changes in response to income shocks by measuring how differences in the receipt of tax benefits, particularly the EITC, affects households' take-up decisions. While these tax benefits provide often substantial cash income to households when they file their taxes, they do not change the amount of SNAP or TANF benefits that households are eligible to receive in most states, allowing us to isolate income effects in the demand for SNAP and TANF.

Besides providing insight into the factors driving households' take-up decisions, the parameter we estimate is also important for the optimal design of safety-net programs. To maximize social benefit, policy makers aim to target programs at the individuals who will benefit most. However, they usually cannot directly observe households' marginal benefit from those programs and have to resort to proxies like income. A common approach for income-maintenance programs is to set a specific income cutoff above which individuals are ineligible for the program. We show that income effects in take-up are directly related to households' marginal utility of consumption. Intuitively, if households experience positive income shocks but do not change their program participation, this indicates that their marginal utility of consumption remains high enough to overcome the costs of program participation. These high levels of marginal utility indicate that there may be welfare gains to increasing eligibility cutoffs. Thus, income effects in program participation provide insight into who should be eligible for programs.

Despite the importance of estimating how demand for programs changes with income, prior research provides very little guidance on the expected magnitudes or signs of these income effects. Since Moffitt (1983), economists have modeled households' decisions to participate in programs as a trade off between the additional consumption provided by these programs and the costs of participating in the programs. These costs could encompass a wide range of factors including the hassle of applying and recertifying for benefits and difficulties with learning about eligibility. In addition, households may experience more emotional costs like stigma from internal or external sources— particularly as a result of the strong value placed on self-sufficiency in American culture. In general, the model's prediction for the effect of income on participation is ambiguous. Moffitt's own parameterization of the labor supply function in the model predicts that households become more likely to participate when their income increases. However, using more common labor supply functions generally results in the opposite prediction.

Empirically, the pure effect of income on program participation is also difficult to estimate for several reasons. First, for many means-tested programs there is a mechanical relationship between income and benefit eligibility: phase-out rates require that benefits decrease as income increases.[1] In addition, the costs of participation may change as households earn more since working families often have to provide more documentation of their income and may have less time available to apply for benefits (McKernan & Ratcliffe, 2003). We are able to bypass these issues by exploiting variation in a source of income that does not affect benefit eligibility and by comparing households with similar levels of labor force participation on average. To the best of our knowledge, we thus provide the first estimates of how participation in means-tested programs changes with income, holding all else constant.

To do this, we take advantage of a discontinuity in eligibility for child tax benefits that results in sharp differences in households' cash-on-hand. Children born during a calendar year can be claimed as dependents on their parents' tax return when their parents file their taxes for that year. Since the filing season for a given tax year generally runs from February to April of the following year, this rule means that households who look very similar when they file their taxes can have very different tax liability. A child born on December 31 will be eligible for child tax benefits whereas a child born on January 1 will not, creating a discontinuity in families' cash-on-hand in their child's first year of life. However, since tax returns do not count as income for calculating SNAP benefits, these families maintain similar levels of SNAP eligibility. We also show that characteristics associated with participation costs, such as hours of work and residence in a rural area, are balanced on either side of the discontinuity, implying that participation costs are constant across the discontinuity.

We estimate a regression discontinuity design using restricted-access administrative data on SNAP and TANF participation linked to the American Community Survey (ACS) and administrative data on date of birth. These data contain monthly measures of individuals'

[1] Although programs that provide a fixed level of benefits like Medicaid or public housing do not exhibit this type of mechanical correlation, we note that as households work more they may have more access to employer-provided health insurance. Similarly, if landlords require income to be above some multiple of rent, then private-market housing becomes more available as income grows. With these outside options, the government-provided benefits may then implicitly lose value.

receipt of SNAP and TANF benefits, allowing us to precisely observe program participation dynamics around the time of a child's birth and the time when households file taxes. We find that income from tax benefits has no effect on program participation. Eligibility for child tax benefits increases households' cash-on-hand by approximately $2,219 in their child's first year of life, equivalent to about five months of SNAP benefits for the average family that chooses to participate in the program. However, we estimate that the effect of this income on participation in SNAP is indistinguishable from zero. We can rule out a decrease in participation during a child's first year of life of 2.5 percentage points or a decrease in benefits received in the first year of a child's life of $188 at the 95% confidence level. Following the previous discussion on optimal policy design, our findings suggest that there could be significant benefits to expanding eligibility for the SNAP and TANF programs but that such an expansion would also have high budgetary costs.

Our analysis contributes to three main literatures. First, our results provide insights into the large body of literature studying the interaction between labor supply and program participation. Labor economists consistently find that labor supply and policies like welfare reform and the Earned Income Tax Credit, which promote labor supply, have a negative effect on program participation (Bastian & Jones, 2019; Currie & Grogger, 2001; Grogger, 2003). As previously discussed, there are three factors that could drive this result: full or partial loss of eligibility, increased participation costs, or increased self-sufficiency. Generally, studies cannot or do not try to disentangle these effects. In this paper, we identify the final factor and find that it plays no role in determining program participation. Thus, we provide evidence that loss of eligibility or increased participation costs drive the relationship between labor supply and program participation.

Our research also contributes to the literature on incomplete take-up of social programs.[2] Economists continue to rely on the Moffitt (1983) model of incomplete take-up to explain nonparticipation in programs, but the expected role of income in the participation decision

[2] Currie (2004) provides a comprehensive review of this literature.

has not been closely examined. Without additional assumptions, the effect of income on participation is ambiguous. We find empirically that income has no effect on participation and discuss what conditions in the model could explain these findings. We note three factors that— either on their own or jointly— would result in a null effect of income on participation. First, a null income effect is consistent with households not exhibiting quickly diminishing marginal returns to consumption. That is, although tax benefits provide additional consumption, the marginal utility of more consumption from SNAP benefits remains high, so households still find the costs of participation worthwhile. Second, these results could indicate that take-up costs are in fact small. In this case, nearly all households participate in the program, and there is no room for income to have an effect on incomplete take-up. Third, there may be no households on the margin of participating in the program. This could occur, for example, if the distribution of costs in the population is multimodal so that there is a group of people with low costs who all participate and a group of people with high costs who are unlikely to ever participate. This could also occur if most households are unaware of their eligibility for SNAP. While our research design cannot say much to distinguish these potential factors, we discuss the plausibility and policy implications of each.

Finally, our results bolster previous research finding that decreases in caseloads do not indicate greater self-sufficiency. The prior literature focuses on consumption to understand the welfare effects of households leaving the welfare and food stamp programs when they enter the workforce. For example, Meyer & Sullivan (2008) study how single-mother-headed families' reported consumption changed following welfare reform, while Zedlewski & Brauner (1999) examine reported food security of families that left the food stamp program. Both studies find that these households were likely worse off. Meyer & Sullivan (2008) conclude that the small increases in consumption they find do not compensate single mothers for their loss of leisure time, and Zedlewski & Brauner (1999) report that many food stamp leavers still suffer from food insecurity. Our study takes a different approach by asking whether the households would have left welfare or food stamps as their income increased in the absence of additional factors. We find further evidence that falling caseloads are not a good proxy for improvements in well-being or self-sufficiency.

The remainder of the paper is organized as follows. In the next section, we provide intuition for the parameter we estimate using a simplified version of the Moffitt (1983) model of program participation. After presenting this theoretical framework, we turn to the details of our empirical analysis. In Section 3, we provide details on the SNAP and TANF programs and the various child benefits provided through the tax system, and we discuss prior research on the determinants of participation in these programs. Sections 4 and 5 outline our data and identification strategy, respectively. We present our findings in Section 6 and discuss the implications of these findings in Section 7. Section 8 concludes.

2 Conceptual Framework

In this section, we use a model of incomplete program participation to describe the parameter we estimate in this paper and to give intuition on the factors that drive the sign of this parameter. We use a simplified version of the model presented in Moffitt (1983).

First note that in the absence of some participation costs, all households should choose to participate in benefit programs since these programs would essentially provide free consumption. To explain why we do not observe perfect take-up, Moffitt models the decision of eligible households not to participate in a program as the result of two types of costs (1) a fixed cost that decreases utility by a flat amount if households participate and (2) a variable cost where households' disutility from the program increases with the size of the benefit. For ease of presentation, we focus only on the fixed cost here.[3]

Individuals choose consumption c, hours of work h, and whether to participate in a

[3]In estimating his model, Moffitt finds strong evidence for the fixed cost but little evidence for a variable cost. Moffitt's point estimates for the variable cost parameter actually indicate that households value consumption from benefit programs more than they value consumption from nontransfer income, the exact opposite of what would be predicted in the presence of stigma. Moffitt notes several reasons for this surprising finding, including misspecification of the benefits of the program or the return to work. We also note that it is difficult to determine how the parameter is identified, and it could be picking up the fact that households with lower income tend to have larger benefits and higher levels of participation.

program $p \in \{0,1\}$ to maximize utility. We assume that the means-tested program only taxes wage income so that if individuals choose to participate in the program, they receive the benefit guarantee amount G, less some fraction r of their wage income wh. The fixed cost of participation is S, which we assume is distributed in the population according to $F_S(s)$. Households take their wage w, nonlabor income y, and the parameters of the program $\theta = (G, r)$ as given. Their utility maximization problem is then

$$\begin{aligned}
\max_{c,h,p} \quad & u(c,h) - pS \\
\text{s.t.} \quad & c = wh + pb + y \\
& b = G - rwh \\
& h \geq 0
\end{aligned}$$

We characterize the household's optimal choice in two steps. First, we consider their optimal bundles of hours and consumption when they participate and when they do not. Let h^p be the choice of hours for $p \in \{0,1\}$. Let $u^1(w, y, \theta)$ denote the value of the optimal bundle when participating, excluding the stigma cost. Similarly, $u^0(w, y)$ is the value of the optimal bundle when not participating, which we can write as

$$\begin{aligned}
u^1(w, y, \theta) &= u\left(w(1-r)h^1 + G + y,\ h^1\right) \\
u^0(w, y) &= u\left(wh^1 + y,\ h^0\right)
\end{aligned}$$

The household participates if their utility when participating exceeds their utility when not participating:

$$p = \begin{cases} 1 & \text{if} \quad u^1(w, y, \theta) - S > u^0(w, y) \\ 0 & \text{otherwise} \end{cases}$$

We can then consider the cutoff value of stigma p^* above which the household will not participate:

$$p^*(w,y,\theta) = u^1(w,y,\theta) - u^0(w,y).$$

Let $P(w,y,\theta)$ denote the participation rate for individuals with a given income, wage, and set of program parameters, so $P(w,y,\theta) = F_S(p^*(w,y,\theta))$. If p^* increases, then the participation rate increases. We can thus use $P(w,y,\theta)$ to perform comparative statics.[4]

Our analysis estimates how nonlabor income affects participation. We can characterize the parameter of interest as the derivative of $P(w,y,\theta)$ with respect to y:

$$\begin{aligned}\frac{\partial P}{\partial y} =& f_S(p^*)\left(\frac{\partial p^*}{\partial y}\right)\\ =& f_S(p^*)\left(\frac{\partial u^1}{\partial y} - \frac{\partial u^0}{\partial y}\right)\end{aligned}$$

where $f_S(p^*)$ denotes the density of stigma at p^*. Using the envelope theorem, we have

$$\begin{aligned}\frac{\partial u^1}{\partial y} &= u_c\left(w\left(1-r\right)h^1 + G + y, h^1\right)\\ \frac{\partial u^0}{\partial y} &= u_c\left(wh^1 + y, h^0\right)\end{aligned}$$

Thus, the predicted effect of income on participation is the difference between the marginal utility of consumption when participating and the marginal utility of consumption when not participating, scaled by the fraction of the population that is on the margin of participation. The predicted sign of this term is ambiguous. The effect of income on participation will be negative (positive) if the marginal utility of consumption is lower (higher) when participating than when not participating and there are people on the participation margin.

In Appendix B, we discuss a set of assumptions that lead us to predict that this income effect will either be null or negative. Briefly, the expected sign is null if participation costs are zero, there are no households at the margin of participation, or utility is not concave

[4] For ease of presentation, we discuss the participation rate conditional on wage and income level. The marginal participation rate is simply the integral over the joint distribution of nonlabor income and wages.

with respect to consumption. Alternatively, the expected sign is negative if participation costs are nonzero, there are households at the margin, marginal utility of consumption is decreasing, and households consume more when they participate in the program than when they do not. We return to the implications of our findings after presenting our results.

3 Policy Background

We turn next to a description of the policies we study in this paper. Our research question focuses on how receipt of the EITC and other tax benefits for children affects households' use of SNAP and TANF. We first discuss the design of the SNAP and TANF programs. We then describe the EITC and child tax benefits with particular emphasis on the features that will provide income variation in our empirical strategy. Together the EITC and SNAP account for the majority of federal assistance for low-income families.[5]

3.1 The Supplemental Nutrition Assistance Program (SNAP)

In the years since the landmark 1996 welfare reform legislation, SNAP— formerly called food stamps— has become the main safety net program (Bitler & Hoynes, 2010). The program provides households with monthly benefits that can be used to purchase food. In fiscal year (FY) 2019, the program distributed over $55.6 billion in benefits to almost 18 million households. However, participation is cyclical, and during the Great Recession the program peaked at over 47 million households served, distributing $76 billion to those households in fiscal year 2013 (United States Department of Agriculture Food and Nutrition Service, 2020).

Eligibility for SNAP is generally based only on household size, income, and assets, with some work requirements for adults without dependents. As such, it is perhaps the most

[5] Bitler & Hoynes (2010) and Bitler & Hoynes (2016) provide evidence on how the social safety net has transformed to one focused on in-work benefits since welfare reform, with brief descriptions of the major components of the safety net. For recent detailed reviews of the SNAP, TANF, and EITC programs, see Hoynes & Schanzenbach (2015), Ziliak (2015), and Nichols & Rothstein (2015), respectively.

universal assistance program in the US, available regardless of disability, presence of dependent children, or previous work history. There are two main pathways to eligibility. In the traditional eligibility determination, households must first have monthly gross income below 130% of the poverty line and assets below a given threshold—$2,250 for households with no elderly members in FY 2019. Gross income includes earned and unearned income, with few exceptions. Alternatively, households may bypass the gross income and asset tests if they are determined to be categorically eligible based on their eligibility for other safety net programs. Regardless of whether they qualify based on gross income or categorical eligibility, households must additionally have net income below 100% of the poverty line to ultimately receive benefits.[6] Net income is gross income minus items like an earned income exclusion and deductions for child care and shelter costs. Benefit amounts are then calculated as the maximum benefit amount given household size less 30 percent of net income. Importantly for the purposes of this paper, while most types of income directly result in a reduction in benefit eligibility, tax refunds are not counted as income when determining SNAP eligibility or benefit amounts (*Food and Nutrition Act of 2008*, 2019).

Not all households that are eligible for SNAP participate in the program. Since the Great Recession, participation rates among eligible households have been quite high. In FY 2016, the participation rate for individuals was estimated to be 85% (Cunnyngham, 2018). However, participation rates have historically been lower, with rates in the low 50% range as recently as the early 2000s (Leftin et al., 2011). Studies consistently find that participation in SNAP is strongly negatively associated with income and employment (Cunnyngham, 2018; Gleason et al., 1998). Previous research suggests that some of this association may be driven by lack of information about eligibility among households with income (Finkelstein & Notowidigdo, 2019; Ponza et al., 1999). Employed households may also face greater hurdles to applying or recertifying both because they have less time to devote to these tasks and

[6] There is a small exception to this rule: categorically eligible one and two person households are allowed to receive the minimum benefit amount even if their net income would disqualify them from receiving benefits (Aussenberg & Falk, 2019). The minimum benefit for the continental U.S. states is $15 in FY 2019.

because they may have to provide more documentation of their income, potentially at greater frequency (McKernan & Ratcliffe, 2003; Kabbani & Wilde, 2003).

3.2 Temporary Assistance for Needy Families (TANF)

TANF was created in 1996 through the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), also known as welfare reform. PRWORA called on states to replace their traditional cash welfare programs, then under the Aid to Families with Dependent Children (AFDC) program, with programs more focused on work. Prior to welfare reform, AFDC had been criticized for encouraging dependency on welfare by disincentivizing work and the formation of two-parent families. The new TANF programs imposed work requirements and limits on the amount of time households could receive benefits, among other measures to push households to enter the labor force and decrease their use of cash assistance. These reforms, along with a booming economy and expansions of the EITC, are credited with the precipitous drop in participation in cash welfare during the mid to late 1990s. In FY 2018, the average monthly caseload was 3.2 million recipients, compared to 14.2 million in FY 1994 prior to PRWORA (U.S. Department of Health & Human Services Office of Family Assistance, 2019a, 2004). In FY 2018, total TANF expenditures were $31.1 billion although only $6.7 billion of this amount went to the basic cash assistance usually associated with welfare programs while the rest went towards other spending categories (U.S. Department of Health & Human Services Office of Family Assistance, 2019b).[7]

Eligibility for TANF is more restrictive and more complicated than it is for SNAP. Unlike SNAP, which is relatively uniform across states, states have significant leeway in setting their own rules for TANF. Eligibility is usually based on income and household composition although there is wide variance in who must be included in an assistance unit and whose income counts toward eligibility tests. In general, the maximum income that a family can have and receive TANF is well below the income cutoffs for SNAP, but the median state's

[7] Other spending categories include child care subsidies, support for work activities, expansions of state Pre-K programs, state EITCs, and program administration.

maximum TANF benefit is of similar size to the maximum benefit level in SNAP.[8] States also employ policies to divert households from receiving TANF benefits, including paying lump sum amounts if households agree not to apply for TANF for a number of months.

Take up of TANF is quite low. The take-up rate was estimated to be 24.9% in 2016 (Giannarelli, 2019), but again this estimate is complicated by opaque eligibility rules and diversion policies. Similar to SNAP, studies following welfare reform found that participation in TANF was negatively affected the reforms themselves, the economic expansion of the late 1990s, and the EITC.[9] Since the early 2000s, research on TANF has tapered off along with the size of the program (Ziliak, 2015), but recent work shows that participation in the program is much less responsive to downturns than before welfare reform (Bitler & Hoynes, 2010, 2016) and that wide variation in state policies drive disparities in access to support (H. Hahn et al., 2017). Although TANF is a much less important part of the safety net today in terms of expenditures and reach, we include some estimates of how income affects participation in the program to show that our findings are consistent across programs with different levels of take-up costs.

3.3 Tax Benefits for Children

Our analysis focuses on how the receipt of child tax benefits affect households' participation in SNAP and TANF. We first describe the features of these tax benefits and then explain why we interpret these tax benefits as a shock to income.

Several features of the federal tax system provide additional benefits to households with children. These benefits include the Earned Income Tax Credit (EITC) and Child Tax Credit (CTC). In addition, for single parents, the birth of a child makes them eligible for the head

[8] For the median state in 2018, the maximum monthly earnings that a family with one parent and two children could have and be initially eligible was $840 (Goehring et al., 2019). For comparison, the gross income limit for a family of three to qualify for SNAP was $2,213 in FY 2018 (United States Department of Agriculture, 2017). The maximum benefit levels are comparable between SNAP and TANF, on average. The median state's maximum monthly TANF benefit for a family of three with no income was $450, compared to the $504 maximum benefit for SNAP, although some state TANF programs provided as little as $170 or as much $1,039 (Goehring et al., 2019; United States Department of Agriculture, 2017).

[9] See Blank (2002) for a review of this literature.

of household (HOH) filing status. Relative to the single filing status, tax units filing as head of household are allowed a larger standard deduction and wider tax brackets. During the time period studied in this paper, filers could also take exemptions for their dependents, which reduced the amount of their taxable income.[10]

For low-income families, the largest tax benefit for children is the EITC. In tax year 2016— the end of our study period— the EITC could be worth up to $6,269 for a family with three or more children. The credit is fully refundable, so even if a family has no tax liability, they can receive the entire EITC amount for which they are eligible when they file their taxes.

Figure 1 illustrates the decrease in tax liability that a single tax filer would experience if she had a first child in 2017, relative to filing with no children. To create the figure, we assume that the tax filer is unmarried, only has income from wages, and takes the standard deduction. The "All" line shows the total decrease in tax liability from a first child while the dashed lines show how much of that decrease is due to the Earned Income Tax Credit (EITC) and Child Tax Credit (CTC). In total, the decrease in tax liability could be over $5,000 for a single person earning $20,000 in 2017. Most of this decrease in tax liability is from the tax filer becoming eligible for the one child EITC. The CTC further decreases tax liability by up to $1000. The remainder is the benefit from being able to file as a head of household and being able to claim a dependent exemption.

The tax benefit of a child is generally largest for the first child. The difference between the no child and one child EITC is large both due to the maximum credit amounts and the fact that households with children can claim the EITC at higher income levels than childless households. In addition, households are eligible for head of household filing status as long as they have one dependent, and the filing status does not become more advantageous with additional children. Nevertheless, additional children generally do decrease tax liability. The EITC becomes more generous for the first three children that tax units claim, and the Child

[10]However, after the passage of the Tax Cuts and Jobs Act, dependent exemptions were replaced by a larger child tax credit and the credit for other dependents.

Tax Credit and dependent exemptions can be claimed for every child.

An important detail to note is that the amount of EITC and all other child tax benefits that a family qualifies for is based on their earnings and family structure during the tax year. A tax year is coincident with the calendar year, but households do not receive tax credits until they file their taxes in the months following the end of the tax year. Thus, the EITC for the 2016 tax year distributed $66.7 billion to 27 million tax units in the 2017 filing year, $57 billion of which was received as a refund. Throughout the paper we will refer to the *tax year* and the *filing year* in this way. As tax refunds can represent a large proportion of income for the year, low-income taxpayers tend to file to get their refund as early in the year as possible, with most EITC refunds concentrated in February and March (LaLumia, 2013).

In general, taxpayers are allowed to claim their children for the EITC if (1) children meet certain age criteria and (2) the children lived with the taxpayers for more than half of the tax year. To claim the Child Tax Credit, dependent exemption, and head of household filing status, additionally it must be the case that the children did not provide over half of their own support for the tax year.[11] If a child is born in the tax year, instead of applying the half-year criterion, the child must live with the family for more than half of the time that he or she was alive in the tax year. In a child's first year of life, this rule can result in very different tax treatment across similar children. A child born on December 31, 2016 could be claimed as a dependent in the 2017 filing year while a child born on January 1, 2017 could not.

Our research design, which we describe in more detail in section 5, compares the program participation of households on either side of the end of tax year cutoff. Households with children born on or before December 31 will be eligible for child tax benefits in their first year of life while a similar family with a child born on or after January 1 will not. We interpret the difference in tax benefits between these two families as an income shock. As we

[11] There is one additional criterion for the EITC and Child Tax Credit that children also must not file joint returns. Individuals only file joint returns if they are married, so this criterion does not apply to our setting.

already noted, the amount of tax benefits that a family receives when they file their taxes is a function of their income in the previous year, so the receipt of a tax refund does not change the return to work.[12] In this way, we would expect the receipt of the tax refund to only have income effects on behaviors. Furthermore, although households may have a general sense of whether they usually get a refund or even the general magnitude of that refund, they rarely know the exact amount that they will be getting back. LaLumia et al. (2015) also document confusion surrounding whether newborn children can be claimed on taxes and how to get children Social Security numbers in time to be claimed. As a result, we would expect that the average household would respond to their tax return as though it were an unanticipated income shock. Accordingly, in the analysis of our results, we will interpret any changes happening before the February or March following a child's birth as unrelated to differences in tax benefits.

3.4 Intersection of Safety Net Programs

Finally, we briefly discuss the extent to which the EITC, SNAP, TANF, and other important safety net programs serve the same populations. In general, this is a difficult question to answer since there are few instances where researchers have access to accurate information on participation in multiple tax and direct benefit programs. One exception is Maag et al. (2015), who merge Florida SNAP data with tax filing data from the Internal Revenue Service (IRS) and find that about half of individuals in tax units that claimed the EITC in Florida in 2010 also participated in SNAP. The income cutoffs for the EITC and SNAP also suggest that many households that claim the EITC are eligible for SNAP. The lines in Figure 2 plot the EITC schedules for a single adult in tax year 2016. The hollow circles in the figure show the annualized gross income limit cutoffs for SNAP associated with the relevant household size in 2017, the year that the 2016 EITC would be received. It is clear

[12] To the extent that households are unaware of how tax benefits work, the refund amount could cause them to update their understanding of the return to work and thus their labor supply, but previous work suggests that households have a difficult time making this calculation (Chetty & Saez, 2013; Tach & Halpern-Meekin, 2014).

in the figure that SNAP eligibility extends far into the range of EITC eligibility, suggesting that many households are eligible for both programs. The overlap of the EITC and TANF will generally be less since TANF eligibility is more strict than eligibility for SNAP, but the overlap of the programs will vary greatly across states.

Two additional programs that are important to discuss when studying families with young children are the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) and the Medicaid/Children's Health Insurance Program (CHIP). WIC provides food assistance, health care referrals, and nutrition education to low-income pregnant, postpartum, and breastfeeding women; infants; and children up to age five, while Medicaid/CHIP provides health insurance to low-income adults and children. Both have slightly higher eligibility cutoffs than SNAP. Medicaid eligibility varies by state and by whether the individual is a child, parent of a dependent child, or disabled. For dependent children in January 2020, the threshold for family income ranged from 175% of the federal poverty line in North Dakota to 405% in New York (Brooks et al., 2020).[13] Families with gross income at or below 185% of the poverty line are eligible for WIC. Using the American Community Survey (ACS) data and several microsimulation models, Lynch et al. (2017) estimate that nearly all children in the three states they study that were eligible for both SNAP and Medicaid/CHIP in 2015 participated in both programs, with analogous participation rates for adults spanning from 50% to 94%. Both the USDA and the Census Bureau estimate that slightly more than 30% of families that participate in WIC also participate in SNAP (Gray et al., 2019; Valle & Perez-Lopez, 2020). Neither program currently counts EITC payments as income for the purpose of determining income eligibility.[14]

[13] Eligibility thresholds are lower for parents and caretakers of dependent children— ranging from 18% in Alabama to 221% in D.C.— and even lower for other adults when states allow these adults to potentially be eligible.

[14] The *Omnibus Budget Reconciliation Act of 1990* disallowed refunds from the EITC from being counted as income in the AFDC, Medicaid, or Supplemental Security Income (SSI). The Tax Relief, Unemployment Insurance Reauthorization and Job Creation Act of 2010 further disallowed federal or federally-assisted programs from counting federal tax refunds or advanced tax payments resulting from refundable tax credits and received after December 31, 2009 as income for determining eligibility (Mann, 2011). Prior to this law, states could count federal refundable tax credits received as a tax refund as income or resources for determining Medicaid/CHIP eligibility. The legislation still allowed states to count state and local income

Assuming that participation in these programs is positively correlated with participation in SNAP and TANF, our estimates of the effect of tax benefits on SNAP and TANF participation could be biased if participation in WIC or Medicaid is unbalanced between families with December versus January births. We are currently unable to observe WIC participation in our data, and we can only observe Medicaid participation close to the time of birth for a subset of families. However, later in the paper, we show that families with December and January births are similar both in terms of their observable characteristics and their prior participation in SNAP and TANF, which we take as evidence that there are unlikely to be imbalances in participation in WIC or Medicaid prior to the receipt of child tax benefits.

4 Data and Sample Construction

Our analysis centers around the American Community Survey (ACS), a large national household survey administered by the U.S. Census Bureau. The ACS collects information on the demographics, income, and relationships of individuals within households. In the ACS crosswalk file, Census assigns unique protected identification keys (PIKs) to individuals in the ACS based on personally identifiable information. These PIKs allow different datasets housed within Census to be linked without revealing underlying personally identifiable information to researchers.[15] We first link the ACS to the Census Numident, which is a file created from administrative records from the Social Security Administration that contains information on individuals' dates of birth and, if applicable, dates of death.

We use the administrative data on date of birth from the Census Numident to sort households into cohorts based on the birth dates of children in the household. We define birth cohorts as the set of births occurring between July 1 of one year and June 30 of the following year. If households have multiple children when they are surveyed, they may be assigned

tax refunds as part of income. For WIC, States can either use the income definitions for the Free and Reduced Price School Lunch Program or Medicaid/CHIP to determine eligibility (National Research Council, 2003). The School Lunch Program also excludes the refunds from the federal EITC from income (*42 U.S.C. § 1382a*).

[15]For more information on PIKs, see Wagner & Layne (2014).

to multiple birth cohorts. However, if they have more than one child born within a single cohort, we choose the youngest of these children to be the focal child. We then create a variable measuring the focal child's birthday relative to the January 1 that falls within his or her birth cohort. In our main analyses, we analyze all birth cohorts together. We focus on the 2005-2016 birth cohorts, where the cohort is labeled by the year of the relevant January 1. This set of cohorts is chosen to match the years of available SNAP and TANF data.

Before describing the SNAP and TANF data, we note two features of our sample. First, we do not limit our sample to ACS households observed within a year of giving birth. Since our outcome variables come from the SNAP and TANF data, the main function of the ACS is to serve as our universe of potential SNAP and TANF participants and to observe births that occur in our sample period within those households. Second, since we use the Numident as our source for date of birth, we need to account for the fact that not all individuals in the ACS have a match in the Numident.[16] This imperfect match could result in misclassifying whether a household has recently experienced a new birth. To try to mitigate this issue, we drop ACS households whose self-reported youngest member was not assigned a PIK and therefore does not have a date of birth from the administrative data.[17]

Our measures of SNAP and TANF participation come from administrative SNAP and TANF data for sixteen states. The data contain information on all SNAP and TANF benefits distributed to individual cases and clients for each month in the years covered. The main advantage of using administrative program data is that we are able to mitigate the well-known issue of misreporting of program use in survey data (Meyer et al., 2009; Meyer & Goerge, 2011; Meyer et al., 2018). In addition, the data contains richer information than

[16]The Census Numident is constructed from the Social Security Numident, which covers the universe of Social Security numbers and Individual Taxpayer Identification numbers (ITINs) ever issued. The Census Numident in turn serves as the universe of PIKs. Every observation in the Numident is assigned a PIK, so in general, an observation in the ACS that does not have a match in the Numident is one where Census was unable to assign a PIK.

[17]The restricted ACS does include self-reported dates of birth, but there appear to be some issues with these self reports. The ability of the Census to assign PIKs to individuals varies by demographic characteristics (Bond et al., 2014). In supplemental analyses, we reweight our estimates by the inverse probability of the likelihood that a household's youngest child is assigned a PIK. This reweighting does little to change our estimates quantitatively or qualitatively.

what is collected in most household surveys with respect to the amount of benefits received and when benefits were received. The data covers calendar years 2004-2017 although the set of years of available data varies by state. Each state's data differs in the type of information that is collected, but for all states we can use the data to construct measures of the benefits distributed to SNAP and TANF cases and clients every month. For some states, we are also able to observe monthly variables related to the size and composition of SNAP cases as well as income reported to state SNAP agencies to verify eligibility.

We merge this information with the individuals in our ACS data. We only keep matches where individuals' reported state of residence in the ACS is the same as the state where they appear in the administrative benefits data.[18] Similarly, we assume that anyone we observe in the ACS living in a state with administrative data who does not appear in that state's SNAP data did not use SNAP.[19] Based on this linkage, we create measures of SNAP or TANF use at the level of the ACS household, focusing on whether anyone in the household used the benefits in the months or years relative to a child's birth and the total benefits received by the household from each program.[20]

Our main measures of SNAP and TANF participation are an indicator for the household receiving any benefits and the amount of benefits received each month. These variables are indexed by calendar month since our analyses are weighted towards comparing households with children born close to the end of December or the beginning of January. Households with children born close to the end of a month may not be able to update their participation and benefits until the following month and thus, their participation following birth is best

[18]These households are households that moved to a different state between the time of their ACS interview and their participation in SNAP or TANF. We exclude these types of matches since we are unable to observe all movers.

[19]Although not all ACS households' states of residence are observed close to the time of the focal birth, over 96% of families in the ACS with children between the ages of 0 and 5 lived in the same state as the previous year, suggesting that out-of-state migration for our sample is likely low. In addition, we show that for the sample of households observed within one year of a focal birth, out-of-state migration is balanced across December and January births.

[20]Note that the SNAP household and ACS household may not be the same. SNAP benefits are based on groups of individuals who eat together while the ACS household is based on housing units.

measured starting in the month following birth.[21] We also present some analyses of program use at the annual level. We consider whether there was any SNAP or TANF use in a year, the number of months with benefits in a year, and the total amount of benefits received in a year.

Finally, to account for the fact that availability of SNAP and TANF data varies across states and years, we limit our sample to households living in states that have program data available for the calendar year before, the year of, and the year after their focal child's cohort year. For example, if a state has SNAP data available for calendar years 2013-2015, we include households from that state who have a child born in the 2014 cohort. However, all other cohorts of births from that state would be excluded. This limitation allows us to maintain a constant sample of households for analyses of SNAP and TANF use one year before and two years after the focal children's births. As we consider outcomes farther away from the focal child's birth, we drop state-cohorts from our sample to assure that all households in the sample can be observed in their state's data in the relevant time period.

5 Research Design

Our research design exploits discontinuities in the amount of tax benefits that families qualify for based on their child's date of birth. Households file their taxes for each year during the tax filing season, usually between February and April of the following year. For households with newborn children, if their child was born on or before December 31 of the tax year, the child may qualify for child tax benefits, but if the child was born after the end of the tax year, the child will not qualify for child tax benefits until the next tax filing season. As a result, otherwise identical households with children born just one day apart may have

[21] For example, in the extreme case where we want to compare SNAP participation directly following birth for households with a birth on December 31 to those with a birth on January 1, it is clear that it would be better to compare SNAP participation in January for both sets of households as opposed to comparing participation in December for the December 31 births to participation in January for the January 1 births. In supplemental analyses, we present some results by months relative to the child's birth, which make clear why we prefer comparing participation by calendar month.

very different tax returns, with families with December 31 births having as much as \$5,000 more available cash-on-hand for their child's first year of life than families with children born on January 1.[22] We use a regression discontinuity design around the January 1 birth date cutoff to estimate how this additional income impacts households' choice to seek out support through the SNAP and TANF programs.

Our main specification is a reduced form regression discontinuity (RD) design, where we regress measures of household's SNAP and TANF use on an indicator for a child being born on or before December 31. We focus on the reduced form instead of an instrumental variable approach because we only observe the income corresponding to children's first year of life for the subset of households surveyed in the ACS within a year of a child's birth. The reduced form specification allows us to study a larger set of households. Our main specification is:

$$p = \beta\, \mathbb{1}(\text{Birth date before Jan 1}) + h(d) + \varepsilon \tag{1}$$

where p is a measure of a household's SNAP or TANF participation around their youngest child's date of birth; $\mathbb{1}(\text{Birth date before Jan 1})$ is an indicator of whether the household's youngest child was born between July 1 and December 31; and $h(d)$ is a continuous function of the youngest child's date of birth d, which is allowed to differ on either side of the January 1 cutoff. In practice, to avoid potentially disclosing analyses based on small samples, we use a fixed bandwidth of one month around the January 1 cutoff in most of our analyses, so $\mathbb{1}(\text{Birth date before Jan 1})$ can usually be thought of as an indicator for the child being born in December. In our main analyses, we use local linear regressions and a triangular kernel to estimate $h(d)$, but our results are robust to more flexible polynomials and the use of a uniform kernel. In the results below, we report the bias-corrected RD coefficients described in Calonico et al. (2014) and standard errors adjusted for the bias correction. To account for

[22] Wingender & LaLumia (2017) and Shirley (2020) use this variation in eligibility for tax benefits to estimate the income effect of the EITC on new mothers' labor supply. Both find that the income effect is negative, although the findings in Shirley (2020) are more mixed. Broadly, regression discontinuity designs exploiting age-based policies have been used to study various topics including education (Elder, 2010; Benson, 2018) and health (Card & Shore-Sheppard, 2004; Card et al., 2009).

the fact that households with multiple children may appear multiple times in our sample, we cluster standard errors at the household level.

We focus on the reduced form regression discontinuity for our main analyses in order to use our data more effectively. To give an estimate of what the first stage would be in an instrumental variables design, we focus on the subset of households that responded to the ACS within one year of the birth of their youngest child. Since ACS households report their income for the twelve months prior to their interview, this sample restriction allows us to better measure the income that households would report in the first tax filing season after their child's birth. Although for most households, this twelve month period likely does not perfectly align with the tax year for their first tax filing season, it will likely overlap. We use this reported income to impute households' tax liability using NBER's TAXSIM program (Feenberg & Coutts, 1993). We impute two tax liability measures. First, following the rules for claiming dependents, we do not include children born after the end of the tax year as their parents' dependents. This measure gives us an estimate of the actual tax return that households filed in their child's first year of life. Second, we impute the tax liability that households would have had if they were allowed to claim their youngest child, regardless of their child's date of birth. We use this measure as an additional test for underlying differences in income and household composition between households on either side of the January 1 cutoff.

5.1 Validity of the RD Design

The parameter of interest is β in equation (1), which measures the difference in program participation between households with children born on December 31 and those with children born on January 1. We can interpret this parameter as the effect of tax benefit eligibility on participation in SNAP and TANF as long as tax benefits are the only factor affecting program use that changes discontinuously at the January 1 cutoff (J. Hahn et al., 2001). Two common ways to probe this assumption are to test whether there are discontinuities in observable characteristics related to program participation and testing for discontinuities in the distribution of households around the January 1 cutoff (Lee, 2008; McCrary, 2008). The logic behind testing for discontinuities in observables is that discontinuities in these variables would suggest there could be discontinuities in unobservable determinants of program use. For the second test, if households can manipulate the side of the cutoff on which they fall, this would again suggest that households on either side of the cutoff may be fundamentally different populations.

In this setting, potential manipulation of birth date is a major concern. If households know they can benefit financially from moving their child's date of birth from early January to late December, then they may try to do so. When we test for manipulation of births in our sample, we find some evidence of manipulation, but the manipulation is more evident for households that are less likely to participate in SNAP. Figure 3 shows the results of the density test described in McCrary (2008) for the full sample. On the horizontal axis is the running variable, date of birth of the focal child relative to January 1, with January 1 marked by the red line. To the left of the red line are births on or before December 31, while births after January 1 are to the right of the red line. The y-variable is the estimated density of births. The figure shows a slight uptick in births before the end of the year, followed by significantly fewer births at the beginning of the year.However, as we show in Figures 4 and 5, manipulation is concentrated among households with characteristics associated with higher socio-economic status and lower use of SNAP and EITC. For example, while we estimate a significant discontinuity in births for households headed by a married reference person, we find no discontinuity in births for households headed by a single female or a never married reference person. Similarly, for households headed by individuals whose highest education is a high school degree or less, the estimated discontinuity in birth dates is both smaller in magnitude relative to the full sample and not statistically significant. For households where the head is Black or Hispanic, the discontinuity in births is less apparent, relative to households with a White household head, although the discontinuity estimate for households with a Black household head is still statistically significant at the 10% level.

Since the tax benefit of a child is usually largest for the first child, we also test for discontinuities by child parity. We find no discontinuity in births for first children although we do estimate significant discontinuities for higher parity births. This pattern is somewhat unexpected if parents are trying to minimize their tax burdens, but it would be consistent with households learning about tax benefits over time.

Our findings are in line with previous research on whether households respond to tax incentives to manipulate their children's dates of birth. Despite some estimates of large amounts of birth date manipulation in response to taxes (Dickert-Conlin & Chandra, 1999), research using the universe of tax records and national birth records suggests that the effect of taxes on manipulation of birth dates is small. Schulkind & Shapiro (2014) estimate about a 0.7% increase in December births in response to a $1000 increase in child tax benefits using national birth records for 1990 to 2001, while LaLumia et al. (2015) study the universe of tax returns for 2001 to 2010 and find a very similar response.

There are many reasons why, despite the financial incentives, tax-related manipulation of births is low. First, Schulkind & Shapiro (2014) show that most of the birth date manipulation around the end of the year occurs through the use of C-sections and inductions. As LaLumia et al. (2015) note, households with the largest potential benefit from manipulation— those eligible for the EITC— are likely to have less access to C-sections due to doctor supply-side factors.[23] Second, LaLumia et al. (2015) also suggest that households may be unaware of the potential tax benefits from having an earlier birth or not understand the child tax benefit eligibility rules.[24] Finally, inducing earlier births can have negative effects on the health of both children and mothers. Thus, the true benefit of manipulation is not obviously positive. Furthermore, in part because of the known adverse health effects of these proce-

[23]In particular, as LaLumia et al. (2015) describe, EITC recipients are likely to participate in Medicaid. In turn, Medicaid recipients are less likely than privately insured patients to receive C-sections, in part due to lower Medicaid reimbursement rates for C-sections.

[24]This hypothesis comes in part from their finding that first births are less likely to be shifted to December than higher parity births, which is consistent with our results as well.

dures, state Medicaid programs have implemented policies over the last decade to discourage elective early deliveries (Medicaid and CHIP Payment and Access Commission, 2019).

Overall, our findings and previous work in this area suggest that while some households may be able to manipulate their child's date of birth on either side of the January 1 cutoff, this behavior is not widespread. Instead, it is concentrated among higher socio-economic status households and more informed households, as measured by differences in the behavior of parents by birth parity. Higher socio-economic status households are unlikely to be potential SNAP and TANF participants, so manipulation in this population is unlikely to affect our findings. However, if more informed households are more likely to have December births, we expect that these households might have an easier time navigating the process of applying for and maintaining eligibility for benefits. If so, this might cause estimated participation rates among December births to be biased upwards. On the other hand, households that use early C-sections and inductions to manipulate their child's birth date are going against standard medical advice and may be more myopic than the average household. It may therefore be hard to predict how these households would act with respect to program participation. As we discuss next, our other tests of internal validity indicate that bias is unlikely. In addition, in Section 6.3 we present some SNAP participation results where we exclude births within ten days of January 1, which creates a sample that is less likely to be able to manipulate birth dates around the cutoff. Our estimates with the donut sample are less precise than when we use the full sample, but they are qualitatively and quantitatively similar.

To further probe the validity of our research design, we next test for discontinuities in observed characteristics at the cutoff. Again, discontinuities in observed characteristics at the January 1 cutoff would suggest that there may be discontinuities in unobserved determinants of program use. We create a summary measure of covariates by using a large number of predetermined characteristics to predict the likelihood that households will use SNAP in their child's first year. This predicted probability is plotted in Figure 6, where we display the mean for five day bins of the focal child's birth date relative to January 1. It is clear that there is no discontinuity both visually and from the estimated coefficient shown above the graph, which is both small in magnitude and not statistically different from zero.[25] Note that in Figure 6, as in most of the traditional RD graphs shown throughout the paper, the mean of the variable of interest can be approximated visually for comparison with the estimated discontinuity. In this case, the estimated discontinuity of -0.0084 (s.e. = 0.0065), is about 2.8 percent of the mean of the index variable for households with January births.[26]

In Figure 7, we provide evidence that households on either side of the January 1 cutoff face similar costs of participation. We focus on three variables associated with having higher costs of program participation. First, Currie & Grogger (2001) find that rural households and households headed by single parents are particularly sensitive to changes in the frequency at which they need to certify their eligibility for food stamps, suggesting that these households face higher transaction costs. We find no discontinuity in the proportion of households living in an urban area, and although we find a statistically significant negative discontinuity in the probability that a household is headed by a single female, the point estimate is small.[27] Second, the fact that working households have more constraints on their time is another factor that is often cited as a reason for these households' lower participation rates (McKernan & Ratcliffe, 2003). Using weekly hours worked as a measure of working families' time constraints, we estimate a discontinuity that is both small in magnitude and not statistically significant. Thus, when we consider proxies for participation costs, we find very little evidence of imbalances at the January 1 cutoff.

[25]We do, however, see that there is seasonality in the characteristics of births, with a relatively higher proportion of births in the winter occurring in households that are likely to use SNAP. This seasonality is similar to that described in Buckles & Hungerman (2013), and it underscores the importance of comparing households with children born close together during the year as we expect households with children born several months apart to differ in their observable characteristics.

[26]In Figures A.1 and A.2, we present tests for discontinuities in many of the individual covariates underlying our covariate index. For these tests, we do find some statistically significant discontinuities, particularly for the probability that the household head is married or never married and whether the household head's highest level of education is a high school degree or less. However, we see no visible discontinuities and note that in all of these cases, the conventional RD-estimate is usually less than half the magnitude of the bias-corrected estimate presented in the figures, suggesting some sensitivity to the bias-correction procedure.

[27]The bias-corrected coefficient we present is again over twice the size of the conventional discontinuity estimate.

In addition to these covariate balance tests on our entire sample, we also test for discontinuities in income for households that are interviewed within one year of their youngest child's birth. By construction, the income that these households report will include income earned after the birth of their child and may also include income earned after the household filed taxes. Nevertheless, this measure is the closest we can get to testing for whether households differ in their resources prior to filing their taxes. We test for discontinuities in wages, Supplemental Security Income (SSI), welfare income, and poverty status. We also test for a discontinuity in Adjusted Gross Income (AGI), which we calculate using TAXSIM. All tax and income amounts are shown in 2016 dollars. These tests are shown graphically in Figure 8, with the coefficients listed in Table A.1. We find no evidence that households on either side of the January 1 cutoff had different levels of resources prior to filing their taxes. In Section 6.1, we additionally show that households on either side of the cutoff are similar in terms of the tax liabilities they would have had if they all had been able to claim their child in the child's first filing year. These results further support the findings in our analysis of the density of births that sorting across the January 1 threshold is not clearly related to the goal of minimizing taxes.

In Figure 8, we also provide some evidence on household's mobility. Since our sample construction assumes that households remain in the state in which they were observed in the ACS, differential mobility could bias our results. Again using the sample of households interviewed within one year of their child's birth, we test for discontinuities in the probability that households lived in the same state one year ago as their current state of residence. This sample of households is very unlikely to have moved out of state, and we find no discontinuity in mobility at the threshold, suggesting differential mobility is not a concern for our findings.

Finally, a unique feature of our research design and data provides us an additional way to test for differences in unobservable characteristics between households with December and January births. Since the SNAP and TANF data cover several years, we can test for differences in households' program use before a child's birth. Tests for differences in program use before the child's birth serve a similar purpose to testing for imbalance in the outcome variable prior to treatment in a randomized control trial or testing for pre-trends in a difference-in-differences analysis: a finding of significant differences before birth would suggest that there are fundamental differences between our comparison groups that would make causal inference inappropriate. As we show in our presentation of the reduced form results, for all of the program participation outcomes we consider, we find no imbalances between households with December versus January births prior to their children's births. These results imply that households with December and January births have similar underlying propensities to participate in SNAP and TANF prior to receiving their tax refunds. These findings also lend further credence to the argument that to the extent that households decide to manipulate their children's birth around the January 1 cutoff, this manipulation appears to be unrelated to their desire to participate in SNAP and TANF. Given this evidence, we believe it is unlikely that our estimates of the effect of tax benefit eligibility on program participation are biased by unobservable differences in households with December and January births.

6 Results

6.1 First Stage Estimates

Before presenting our main results, we next present our estimates for the discontinuity in tax liability between households with children born in December and households with children born in January. As with our tests for discontinuities in income, we only estimate these discontinuities for households interviewed within one year of their youngest child's birth.

Figure 9 presents the estimated discontinuities in total household tax amounts for the first filing season after the focal child's birth. Tax liability is the total amount of taxes the household is imputed to have owed for the tax year. We estimate that having a child in December instead of January results in a $2,219 (s.e. = 1,417) decrease in tax liability in the child's first year of life. Tax liability is not the same as what households owe or are refunded when they file their taxes. Those amounts will depend on the amount of withholding the household had over the course of the tax year. As we cannot observed withholding, we proxy for the amount of money that households were refunded by considering only negative tax liability and assigning a zero to any household with positive tax liability. Households with negative tax liability would receive this amount back at tax time in addition to any withholding they had, so this measure is a lower bound on the refund the households received. We estimate that having a birth in December decreased this measure of tax liability by $711 (s.e. = 125), an 89 percent increase relative to the mean for January births. As shown in the figure for "Any Negative Tax Liability," households are about 13 percentage points (57 percent) more likely to have any negative tax liability if their child is born in December. Comparing the discontinuities for the EITC only and for total refundable credits, which is the EITC plus the Additional Child Tax Credit, we see that most of the difference in potential refunds is driven by eligibility for a larger EITC.[28] Table 2 again presents the estimated discontinuities shown in Figure 9 along with the mean of each variable for households with births in January.

As an additional test of the validity of our regression discontinuity design, we test for discontinuities in the tax amounts that households would have had if they were allowed to claim their youngest child in their the first filing season after their child's birth. For households with children born on or before the focal December 31, this amount will be the same as what we impute to be their actual tax liability. For households with children born on January 1 or later, this tax amount is calculated as though the households could claim an additional child. The estimated discontinuities in these variables are in Table 3. The estimated discontinuities in the table are much smaller than the true discontinuities and they lose statistical significance. For example the discontinuity in negative tax liability falls

[28]In our context, the only tax benefits that could result in negative tax liability are the EITC and Additional Child Tax Credit. Negative tax liability and our refundable credits measure differ only to the extent that households have some positive tax liability that must first be zeroed out by the refundable credits.

to -\$166 (s.e. = 139). Given that households on either side of the January 1 cutoff have similar levels of income, these results suggest that households in our sample did not sort across the cutoff based on their potential child tax benefits.

6.2 SNAP Use Around a Child's Birth

We now turn to our main estimates of how tax benefits affect SNAP participation in the months following a child's birth. Figure 10 shows how SNAP participation for families in our sample changes around the time of a child's birth. The graph plots the SNAP participation rates and average benefit amounts of households with children born in the last five days of December and the first five days of January. The red lines in the graphs mark the data points corresponding to the January that includes the birth of the January children and directly follows the births of the December children. We can clearly see that there is a jump in both participation rates and benefit amounts at the time of these children's births followed by a steep increase in both variables for the next few months. Between December and May, participation increases by about 19 percent, relative to participation in December, and average benefits increase by about 45 percent. The increase in benefits reflects both the higher participation rates and the fact that having an additional child increases the amount of benefits that families who were already participating are eligible to receive. These graphs show that although many households change their SNAP participation right at the time of their child's birth, a large proportion take a few months to change their SNAP participation decisions. Thus, when households file their taxes— most often in February and March for households expecting a refund— their tax refunds arrive at a time when the households' SNAP participation decisions are likely to be in flux. In these figures we see little evidence of differential changes in SNAP participation for households with December and January births. These descriptive patterns are in line with our main results, which we estimate on a larger window of births to decrease the variance of our estimates.

In our first set of analyses, we focus in on the months when most households that are due a refund file their taxes. If we see differences in the SNAP participation of households with December and January births around this time, this would be strong evidence that tax returns are affecting households' participation decisions. Our primary outcomes are an indicator for whether any SNAP benefits were received in the month and the total amount of benefits that were received in a month. Benefit amounts are adjusted by the maximum benefit amount for a family of three in each month so that they correspond with the benefit amounts received in FY 2017.[29]

Figure 11 shows SNAP participation rates from the October prior to the focal January 1 cutoff to the June following the focal January 1. The points in the graphs are the mean participation rates for households in five day bins of birth dates relative to the January 1 cutoff, and the estimated RD coefficients and standard errors are listed above each figure. It is clear both from visual inspection and from the estimated discontinuities that there are no discontinuities in participation. For example, the estimated discontinuity in participation for February suggests that households with a birth in December have a participation rate 0.10 percentage points higher than that of January births (s.e. = 0.97). The average February participation rate for households with January births is 23.9%, so the point estimate corresponds to a 0.3 percent increase in participation relative to the mean. The estimated discontinuity for participation in March is even smaller in magnitude: -0.08 percentage points (s.e. = 0.98), or a 0.33 percent decrease relative to the mean for households with January births. The confidence intervals are somewhat wide relative to the point estimates, but they allow us to rule out decreases of more than 2.5 percentage points or 10.9 percent relative to the mean at the 95% confidence level in October through June.

We similarly find no discontinuities in the amount of benefits that households receive each month. In Figure 12, we plot means of the amount of benefits that households received, with non-participating households measured as receiving zero dollars in benefits. Once again, there are no visible discontinuities at the January 1 cutoff in any month, and the estimated

[29]Recall that the tax and income variables were presented in CY 2016 dollars, which reflects the fact that 2016 tax refunds were received during FY 2017.

coefficients are all small and not statistically different from zero. The discontinuity in benefits received in February indicates that households with December births received \$1.82 (s.e. = 5.45) less in SNAP benefits than households with January births, or a 1.7% decrease relative to the mean for January households (\$104.7). The estimated coefficient for benefits received in March similarly corresponds to a 1.8% decrease in benefits relative to the mean for households with January births, again not statistically different from zero. The 95% confidence intervals are again somewhat wide but rule out decreases of more than 10 to 13 percent relative to the mean benefits received in each month by households with January births.

Although we do not see any discontinuities in participation or benefits received, the richness of the SNAP data does reveal that household participation is changing around children's births. In Figure 12 we see a step shape that gradually moves from the left side of the graphs to the right as the months progress. This pattern closely follows when births occur.[30] The fact that we estimate no discontinuities in participation or benefit amounts thus is not the result of households maintaining their previous SNAP participation levels but instead the result of households on either side of the cutoff deciding to change their participation in SNAP in similar ways, regardless of differential levels of tax benefits.

Observed benefit amounts reflect both participation and the amount of benefits that families are eligible to receive. As we have already seen that there are no differences in participation rates at the January 1 threshold, the results in Figure 12 indicate that the types of families receiving benefits are similar in terms of income and household composition. This point is further confirmed when we estimate our model only on households participating in SNAP in the given month. Once again, the graphs in Figure 13 show no differences in the benefits received by households with December or January births. The estimated decreases in benefits received in February and March by households with December births versus January births were -\$9.00 (s.e. = 11.95) and -\$6.73 (s.e. = 11.96), respectively, which represent a

[30]For reference to the axis labels, September 23rd is 100 days prior to January 1 while April 11th is usually 100 days after.

2.1 percent and a 1.4 percent decrease relative to the mean benefits received by participating households with births in January. The 95% confidence intervals for October through June now rule out decreases of more than 9 percent relative to the mean.

To summarize the multiple discontinuities we estimate, Figure 14 shows the regression discontinuity coefficients for monthly SNAP participation and benefits received for the calendar year prior to birth and the three calendar years following birth. In the top panel, each point in the graph shows the regression discontinuity estimated for SNAP participation in the month indicated on the x-axis. The bars indicate the 95% confidence intervals for the estimates. Note that the x-axis now indicates the month in which the dependent variable was observed. The red lines in the graphs highlight the discontinuity estimated for participation in the January that includes the births of the January children, noted as "Jan, t=0." This estimate directly corresponds to the graph labeled "January" in Figure 11.

A first thing to note in this figure is that in the months before birth, we do not find significant differences in participation or benefits received between households that will eventually have December and January births. These findings support the validity of the RD design. Similar to balance tests in a randomized control trial, the fact that households with December and January births had similar levels of SNAP participation prior to receiving tax benefits suggests that the participation of households with January births serves as an appropriate counterfactual for the participation of households with December births had they not received tax benefits. In addition, the lack of differences prior to birth implies that households did not sort across the January 1 cutoff based on their preferences for participation in SNAP.

Once households give birth and receive tax benefits, we continue to see no difference in participation rates or benefit amounts. In July, the month with the largest point estimate for participation in the first year following birth, we can rule out a decrease in SNAP participation of more than 2.6 percentage points. Similarly, the largest point estimate for the SNAP benefits analyses in the year following birth occurs in September, and even in this month, we can rule out a decrease in benefits received of more than $17 at the 95% level.

In the second and third year after birth, households with December and January births are both eligible for child tax benefits. We present the estimated discontinuities for these years first to show that there are no long run effects for the households with December births of the receipt of tax benefits. In addition, the results suggest that households with January births do not change their participation decisions in response to the first time they receive child tax benefits for the focal child. In the second and third years after the focal children's births, there is a slight downward trend in the point estimates, indicating that participation of households with December births falls relative to households with January births over time, but these estimates never reach the level of statistical significance.

The estimated discontinuity coefficients for the July before to the December following the focal January 1 are presented in Tables A.2 and A.3, which we discuss more in the next section. Overall, our analyses of monthly SNAP use show that the additional income that households with December births receive from their tax returns has no contemporaneous effect on SNAP participation.

6.3 Robustness Tests

Before probing our estimates further, we note that our analyses are robust to the choice of bandwidth, specification, and various weighting schemes. Our main analyses use a one month bandwidth around the January 1 cutoff in order to limit the risk of disclosing results that create small implicit samples. In Tables A.2 and A.3, we show that our estimates for monthly participation and benefits received are robust to using the bandwidth proposed in Calonico et al. (2014), henceforth CCT. In addition, we show estimates using a two month bandwidth and a one month bandwidth excluding births within ten days of January 1. The column labeled CCT Bandwidth shows that the CCT bandwidth selector usually chooses a bandwidth between one and two months. Thus, in choosing a one month bandwidth, we expect that our estimates will have higher variance but potentially lower bias relative to estimates using the CCT bandwidth. Using the CCT or two-month bandwidths often results in coefficients with a sign opposite to our estimates using a one month bandwidth, but the qualitative interpretation of no significant effect on participation or benefits received remains the same. For the benefit analyses, the finding of no effect is often made stronger when using the CCT or two month bandwidth as the estimated coefficients and standard errors are usually closer to zero. The estimates excluding ten days on either side of the January 1 cutoff are much noisier than our main estimates but again result in the same conclusion of no effect of tax benefits on SNAP participation.

Tables A.4 and A.5 show that our estimates are also robust to the use of a uniform kernel, more flexible local polynomials, or including cohort-by-state fixed effects. We also show the conventional regression discontinuity coefficient and standard errors as a comparison to the bias-corrected coefficient and robust standard errors that we present in the rest of the paper. Again, since many of our estimates are close to zero and none are statistically different from zero, using different specifications often results in estimates with different signs, but the overall finding of no effect persists across all specifications.

Finally, we test whether our estimates are robust to using various weights. Our main estimates are unweighted. In Tables A.6 and A.7 we present results using the ACS household weights normalized to give equal weight to each ACS wave. In addition, we show estimates where we adjust the normalized ACS household weights by the inverse probability that any person in the household has a PIK and the probability that the youngest person in the household has a PIK.[31] Tables A.6 and A.7 show that our estimates are stable across the various weighting schemes. For many of the estimates, the use of weights tends to result in estimates farther away from zero but across all weighting choices the estimates lie within the

[31] The method of reweighting the sample by the inverse probability of anyone in the household having a PIK has been used in previous studies using administrative SNAP data with household surveys housed at the Census (Meyer & Goerge, 2011; Newman & Scherpf, 2013; Cerf, 2014). The weights are intended to adjust for the fact that since we merge the administrative SNAP data to the ACS using PIKs, we cannot measure household SNAP use for households that do not have members with PIKs. The inverse probability weights thus give more weight to households that look like households that do not have any members with PIKs. We alternatively create weights adjusting for the probability that the youngest member of the household has a PIK since we limit our sample based on this criterion. To estimate the probabilities of interest, we run probits with an exhaustive set of household characteristics on the entire ACS sample.

95% confidence intervals of the other estimates.[32]

6.4 Annual SNAP Use

It may be too strong an assumption to expect that households will immediately respond to their tax refund by changing their participation in SNAP. Instead, it could be the case that households who were already participating in SNAP wait to update their SNAP participation until they have to recertify their eligibility. In this case, we might find decreases in SNAP use over a longer time horizon. States have discretion in setting the length of recertification periods, but all non-elderly and non-disabled households can only be certified for a maximum of twelve months (United States Department of Agriculture Food and Nutrition Service, 2018). Thus, we additionally consider measures of SNAP use at the yearly level. As shown in Table 4, we similarly find little evidence to suggest that tax refunds affect SNAP participation. We estimate discontinuities for three measures of yearly SNAP use: any SNAP participation, number of months receiving SNAP, and total SNAP benefits received.

We display the regression discontinuity coefficients for these variables estimated from one year before the focal birth to two years after the focal birth. As with our monthly estimates, we find no evidence of differences in participation prior to the focal birth and also find no differences in participation after the birth. In the year of birth, the point estimates indicate a 0.4 percentage point decrease (1.3 percent decrease relative to the mean for households with January births) in annual participation; a decrease in months receiving SNAP of 0.02 months (0.8 percent); and a $33 (2 percent) decrease in benefits received. We can rule out decreases in any SNAP participation of about 2.5 percentage points, decreases in the number of months receiving benefits of about 0.2 months, and decreases in total yearly benefits of more than $156 at the 95% confidence level. While these amounts are seven to ten percent of the mean, we still conclude that there is no evidence that tax benefits significantly decrease participation in SNAP.

[32]Note that the rdrobust command does not handle weights well, so all coefficients and standard errors presented in the table are the conventional (not bias-corrected) estimates.

6.5 Heterogeneity

Although we find no effects of tax returns on SNAP use in the months following a child's birth for the full sample, we present additional analyses to show that our conclusions are robust across various subgroups of interest.

First, we test whether our results differ across households with different histories of participation in SNAP. As previously noted, it could be the case that households that were already participating in SNAP are unlikely to quickly update their participation decisions when they receive tax benefits. This inertia is less likely to be an issue for households that were not already using SNAP, as they have to make an active decision to participate. Figure 15 displays the coefficients when we estimate regression discontinuities separately by whether or not households used SNAP in the calendar year prior to the focal January 1. The estimated coefficients for households that did not participate in SNAP prior to their most recent birth are always close to zero and precisely estimated, so we find no evidence that tax benefits have stronger effects on households that were not previously participating in SNAP.

Next, we split our sample by households' potential EITC eligibility. Since the child tax benefit that drives most of the difference in tax liability between households with December births and those with January births is the EITC, if tax benefits do have an effect on SNAP participation, we would expect a larger effect for households that receive EITC. We split our sample by households that we impute would have been eligible for some EITC credit in their child's first tax filing season if their child was born before January 1 and those we impute to not receive EITC regardless of when their child was born. As with our first stage estimates, we only calculate tax amounts for households interviewed within one year of their youngest child's birth. Figure 16 presents the estimated coefficients for these groups' monthly SNAP participation and benefits received. With the smaller sample size, these estimates are noisier than our main estimates, but the pattern of the coefficients is consistent with what we found for the full sample.

In an additional set of analyses, we attempt to focus on groups of households we predict to be highly likely to use SNAP. To estimate this propensity to use SNAP, we use self-reported SNAP use of households residing in states outside of our administrative data.[33] We use these responses to calculate a propensity score for the households in our sample and classify households with a propensity score in the top 75th percentile for the sample as households likely to use SNAP.[34] Figure 17 plots the estimated coefficients for the full high propensity sample and the set of high propensity households who did not use SNAP in the year before their focal child's birth. Again, we find no evidence that these households are more responsive to the receipt of child tax benefits.

Finally, Figures A.10 through A.12 plot the coefficients for SNAP participation for specific demographic groups, and Figures A.13 through A.15 show similar graphs for SNAP benefit amount analyses. We show coefficients estimated separately by the ACS reference person's number of own children that are potentially eligible for the EITC; by marital status of the reference person; by race and ethnicity of the reference person; and by education level of the reference person. In all of these analyses, we find no evidence of heterogeneous effects. Taken together, these result show that our finding of no effect of tax benefits on SNAP participation holds even in the populations we expect to be the most responsive to tax benefits.

6.6 Labor Supply Responses

Besides heterogeneous treatment effects, another potential explanation for why we find no effect of tax benefits on participation in SNAP is that households instead use the tax benefits to decrease their labor supply and then continue to use SNAP to supplement their newly lower income. The fact that SNAP benefit amounts are similar for households with December and January births after they receive tax benefits indicates that differential changes in labor supply are unlikely, but we next present analyses that directly test for changes in income in

[33] ACS respondents are asked to report whether anyone in their household used SNAP in the twelve months prior to the interview.

[34] This out of sample prediction performs quite well: 54.19% of households with January births in our high propensity sample participate in SNAP in their child's month of birth compared to 22.72% of households with January births in the full sample.

response to receiving tax benefits.

We approach this question two ways. First, we examine the income amounts that households participating in SNAP report to state SNAP agencies. We use the same regression discontinuity model as in our main analyses but change the dependent variables to SNAP income measures and limit the sample to households participating in SNAP in the relevant time period.[35] In a second approach, we attempt to estimate a model similar to the model used in Wingender & LaLumia (2017), who find that some new mothers decrease their labor supply in the months following receipt of their tax return. Unfortunately, the original code used in the Wingender & LaLumia (2017) paper is no longer available, but we try our best to replicate the procedure as closely as possible based on the published paper.

Figure A.5 presents the results of our analyses of income amounts reported in the administrative SNAP data. We show the estimated discontinuities in reported gross income, net income, earned income, and unearned income. We estimate some significant negative discontinuities in unearned income between households participating in SNAP that have December births versus those with January births, but these negative estimates appear before the December or January births occur and are unlikely to be related to receipt of tax benefits. The earned income estimates do not show evidence that households with December births decrease their labor supply in the months following a birth to a greater degree than households with January births. The estimates are somewhat noisy— ranging from a decrease in earned income of $8 (-1.2 percent) in July to an increase of $100 (15.6 percent) in February— but, if anything, there is more evidence to suggest that households with December births earn *more* than households with January births in the year following birth. We also note that we see no evidence that households mistakenly report their tax return as income since there are no spikes in reported income around the second or third month after the focal birth.

[35]Although requirements for SNAP recipients to report income and employment changes vary by state and case type, households do need to update their income information when they recertify, usually every six to twelve months. Thus, we would expect to observe changes in income for participants. Note also that we found no discontinuities in participation between December and January births, so any differences in participants' reported income is unlikely to reflect differences in the types of participants on either side of the cutoff.

The model estimated in Wingender & LaLumia (2017) differs from our model. We briefly describe the model here before discussing our estimates and leave a more detailed description to Appendix C. Wingender & LaLumia (2017) study how new mothers' labor supply responds to the receipt of child tax benefits. As in this paper, they exploit the fact that women who give birth in December get to claim child tax benefits soon after giving birth while those who give birth in January cannot. Their dataset is the 1999 to 2008 waves of the ACS. In the ACS, respondents report their labor supply at the time they are interviewed. The authors limit their sample to women interviewed within one year of giving birth who gave birth at the end of December or beginning of January. They estimate the following model:

$$Y_i = \sum_{k=1}^{T-1} \alpha_k (MonthsElapsed = k_i) + \sum_{k=1}^{T} \beta_k (MonthsElapsed = k_i \times DecBirth_i) + \gamma X_i + \varepsilon_i$$

where Y_i is a measure of labor supply and $MonthsElapsed = k_i$ is an indicator for the mother being interviewed k_i months after giving birth. The α_k terms account for the fact that women are less likely to be working soon after giving birth and then gradually return to work. The β_k terms capture how the labor supply of mothers with December births differ from those with January births. The authors also estimate instrumental variables regressions where $DecBirth_i$ is replaced in the equation above with the predicted tax savings from having a December birth: $\widehat{TaxValue}_i$, defined as the fitted values from $TaxValue_i = \delta DecBirth_i + \lambda X_i + \nu_i$.

We estimate both models on the set of ACS waves corresponding to our study — 2005 to 2016.[36] The results for employment are shown in Table 5 and results for the probability a woman is working but temporarily not at work are in Table A.8. Unlike Wingender & LaLumia (2017), we find very little evidence that women with December births decrease their labor supply in response to tax benefits. Whereas Wingender & LaLumia (2017)

[36]We attempt to replicate the Wingender & LaLumia (2017) analysis using a set of ACS waves similar to the waves used in their analysis, and these results are presented in Appendix C. Similar to the results presented here, we do not find evidence that households with December births decrease their labor supply in response to receiving child tax benefits.

estimate significant decreases in labor supply around the time of tax filing, the only significant differences we estimate between mothers with December and January births are a slightly *higher* employment rate for December mothers five months after birth and a slightly lower employment rate for December mothers eight months after birth. Thus, we conclude that decreases in labor supply are unlikely to explain the fact that households do not change their SNAP participation in response to tax benefits.

6.7 TANF Use

We briefly turn now to our estimates of changes in TANF participation. Participation rates in TANF are much lower than those in SNAP, and there is ample research on the high participation costs associated with the program. As a result, we might expect that tax benefits would have a larger impact on households' participation decisions. Unfortunately, TANF use in our sample is very low, with 4.1% of households with December or January births participating in the month of their focal child's birth, and although we generally find no effect of tax benefits on TANF use, it is difficult to rule out qualitatively large effects.

Figure A.6 shows how participation and benefit amounts change around the time of a child's birth for December and January births. As with SNAP, there is an increase in participation and benefit amounts received when a child is born, although it appears that the uptick in participation begins several months before birth. There also appears to be a quick change in the participation trend around March, which could be consistent with households responding to tax benefits. However, since we find no differences in participation for households with December and January births, we believe it is unlikely that tax benefits can explain this trend break.

Figure A.7 presents the data underlying our estimates of discontinuities in TANF participation in the months around a child's birth. We see no visible discontinuities in these graphs and all of our discontinuity estimates are quantitatively very small and not statistically significant, with the exception of June where although the coefficient is statistically significant, there is no visible discontinuity.[37] The estimated decreases in participation in February and March are 0.89 percentage points and 0.67 percentage points, respectively. Given the small baseline participation rate, these effects are not small in percentage terms, representing about a 19 percent and 14 percent decrease, respectively, relative to the means for January births. However, we see no evidence that TANF participation is affected by receipt of tax refunds.

We similarly find no effects of tax benefits on the amount of TANF benefits received when we examine the graphs in Figure A.8. Figure A.9 presents the estimated discontinuities in monthly TANF participation and benefits received from one year before the focal birth to three years after the focal birth. We find no evidence of differences in TANF use prior to the focal birth and also do not find strong evidence of differences after the birth. Table 6 shows the estimated discontinuities in annual measures of TANF participation. Again, we see no evidence of discontinuities in TANF use at the January 1 cutoff in any of these analyses.

7 Discussion

7.1 Implications for the Welfare Stigma Model

In this study we find that households do not respond to an influx of income by changing their participation in SNAP and TANF. As discussed in Section 2, this null effect is consistent with three potential— not mutually exclusive— explanations: (1) households face no participation costs, (2) there are no households on the margin of participation, or (3) utility is not concave with respect to consumption. Our research design does not allow us to distinguish which of these factors drives our results, but we believe there is evidence that all three factors play some role.

First, while participation in SNAP and TANF still requires households to apply and re-

[37] The conventional estimate for June is less than half the magnitude of the bias-corrected estimate and not statistically different from zero.

certify for benefits, recent changes to how SNAP is administered have likely significantly decreased the costs of participating in SNAP. Particularly in the last decade, states have simplified the eligibility determination process, increased the length of certification periods, and reduced households' reporting requirements (United States Department of Agriculture Food and Nutrition Service, 2009, 2018). The transition to benefits provided through Electronic Benefit Transfer (EBT) cards that work similarly to debit cards has also decreased the stigma of using SNAP benefits (Schanzenbach, 2009). These changes likely have contributed to the increase in SNAP participation over the past decade, which may in turn have further decreased the costs of participation through network effects (Kroft, 2008).[38] As a result, we may find no effect of income on participation because there simply are very few eligible households that choose not to participate in SNAP. However, this explanation seems unlikely to explain our results for TANF, where take-up rates remain around 25%.

An additional explanation for why we find no effect of income on participation is that there are no households on the margin of participation. Such a situation could occur if the distribution of participation costs in the population were bimodal, with one group of households having low participation costs and another group having very high participation costs. Alternatively, if households do not know they are eligible for SNAP and TANF, then they also are unlikely to be marginal participants. Several studies have found that SNAP nonparticipants often are unaware of their eligibility (Finkelstein & Notowidigdo, 2019; Daponte et al., 1999; Ponza et al., 1999). While lack of information about eligibility cannot explain our results for households that were participating in SNAP prior to their child's birth, it could contribute to why households with January births who were not participating prior to their children's births do not take up SNAP at higher rates.

The final potential reason for why we do not find an effect of income on program participation is that households' utility is not concave with respect to consumption. We might

[38] However, Homonoff & Somerville (in press) find that recertification costs still are significant and that even slight changes in the amount of time households have to recertify can have large impacts on the likelihood of successfully recertifying.

expect households' utility to not have diminishing marginal returns to consumption if their consumption level is very low, which is true of most households participating in SNAP. However, based on calibration exercises, our estimates do not clearly rule out standard estimates of the coefficient of relative risk aversion, which are usually in the range of 1 to 3, with 1 corresponding to log utility and 3 corresponding to higher levels of risk aversion than log utility. The calibration is sensitive to the assumptions made and in particular, varies based on the amount that we use as the change in income. If we use the estimated discontinuity in total federal tax liability ($2,219), then the endpoints of our 95% confidence intervals are inconsistent with a coefficient of relative risk aversion greater than about 0.8. If we instead assume that income increases by the $603 change in EITC, then our estimates are consistent with a coefficient of relative risk aversion of 3. Thus, our estimates are too imprecise to clearly conclude that households' utility is linear. Based on the evidence at hand, it is likely that all three of these factors contribute to our finding that income has no effect on program participation.

7.2 Policy Implications

Our results imply that if households were able to maintain eligibility for SNAP and TANF as their income increased, then they would continue to participate in these programs. As a result, expanding eligibility for SNAP and TANF would clearly increase the utility of working households. What is less clear is whether this policy would be welfare enhancing for society as a whole.

To give insight on this question, we next present a simple optimal policy model that incorporates incomplete program participation. For ease of presentation, we first simplify the household problem to only be a choice of whether to participate in a program that provides a fixed benefit level. Thus, the household problem can be written as

$$\max_p \; u(c + pG) - pS$$
$$\text{s.t } c \leq y + pG$$

where G is now the fixed benefit amount, and all other variables are defined as in Section 2. Again, the household participates if the utility of participation including stigma exceeds the utility of not participating. The participation rate is then

$$P(y, G) = F_s(u(y + G) - u(y))$$

The change in participation as a result of a change in income is now

$$\begin{aligned}\frac{\partial P}{\partial y} =& f_s(u(y + G) - u(y))(u'(y + G) - u'(y)) \\ \approx& f_s(u(y + G) - u(y))(u''(y)(G - y))\end{aligned}$$

where the approximation in the second line comes from a Taylor expansion of both terms in $(u'(y + G) - u'(y))$. Thus, participation changes with income if there is some mass on the margin of participation and utility is not linear.

Suppose the government seeks to maximize social welfare by choosing the maximum income, $\bar{y}$, at which a household can receive the government benefit subject to a budget constraint. Here, we write the budget constraint to reflect the fact that each additional dollar allocated to the benefit program takes money away from alternative government expenditure programs (Salanié, 2003). The penalty term ω below reflects the weight that the government puts on the other expenditure programs or the marginal social benefit of spending an additional dollar on the alternative programs. It can also be thought of as the welfare cost of collecting additional revenue. We assume that the government weights households' utility by $\Psi(\cdot)$, which is increasing and concave. The government problem is:

$$\max_{\bar{y}} \int_0^{\bar{y}} \Psi\left(u\left(y+pG\right)\right) dF(y) + \int_{\bar{y}}^{\infty} \Psi\left(u\left(y\right)\right) dF(y) + \omega \left[R - \int_0^{\bar{y}} p(y) G dF(y)\right]$$

where $F(y)$ is the population distribution of income.

If we assume that the government gives equal weight to households with the same level of income y, then we can split this expression into separate terms for households that participate in the program, households that are eligible nonparticipants, and households that are ineligible for the program:

$$\max_{\bar{y}} \int_0^{\bar{y}} p(y)\Psi\left(u(y+G)\right) + (1-p(y))\Psi\left(u(y)\right) dF(y) + \int_{\bar{y}}^{\infty} \Psi\left(u(y)\right) dF(y)$$
$$+ \omega \left[R - \int_0^{\bar{y}} p(y) G \; dF(y)\right]$$

The first order condition shows that the optimal maximum income for the benefit is characterized by:

$$\Psi\left(u(\bar{y}+G)\right) - \Psi\left(u(\bar{y})\right) = \omega G$$

where we can see that the government sets $\bar{y}$ so that the marginal social benefit of providing the program to households that are just barely eligible is equal to the marginal social benefit of allocating the same amount of money to the alternative public program. If the social welfare function takes the form $\Psi(u(.)) = g(y)u(.)$, then the above expression becomes:

$$g(\bar{y})\left[u(\bar{y}+G) - u(\bar{y})\right] = \omega G$$

and we can see that the magnitude of the term in brackets will be determined by the concavity of households' utility.

The above expression implies that less concave utility will result in a higher optimal $\bar{y}$ as the difference between households' utility if they participate versus their utility if they do not will remain large at higher levels of y. In addition, if a $\bar{y}$ was chosen and it was found that utility was linear at least local to the eligibility cutoff, then the government would likely find it optimal to increase the eligibility cutoff as long as it weighted households' utility smoothly with respect to income. The intuition in this case is that households with income just above $\bar{y}$ have similar marginal utility as households with income $\bar{y}$. Thus, if the government thought it was worthwhile to provide households with income up to $\bar{y}$ with benefits, it is likely to also find it worthwhile to provide benefits to the group of marginally richer households.

Our estimated coefficients, although consistent with linear utility, cannot rule out relatively concave utility either. Thus, the implied welfare effects of expanding eligibility are not completely clear and would require further assumptions on how the government weights the utility of different households and its preferences for spending on other programs.

7.3 Comparison to Previous Estimates

Our finding that receipt of tax benefits has no effect on participation in SNAP or TANF stands in contrast to previous studies finding significant negative effects of expansions of the EITC on use of traditional safety net programs. For example, Hoynes & Patel (2016) estimate that a $1,000 increase in EITC income decreased households' annual cash welfare income by $709 (s.e. = 73) and food stamp income by $213 (s.e. = 43). Similarly, Bastian & Jones (2019) find that a $1,000 increase in the maximum EITC credit decreases average annual public assistance benefits by $516 (s.e.= 96), contributing in part to their conclusion that the EITC "pays for itself". For comparison, if we use our estimated discontinuity in EITC of $603 to scale up our results, we estimate that a $1,000 increase in EITC results in a decrease in annual TANF benefits of $18 (s.e. = 40) and a decrease in SNAP benefits of $54 (s.e. = 103). Although in some cases our 95% confidence intervals overlap with the confidence intervals in these studies, the overall conclusions are quite different. We believe that these differences are explained by differences in the underlying parameters being estimated. Our results do not necessarily conflict with previous studies but instead provide insight into the mechanisms driving the relationship between the EITC and other means-tested programs.

As with the majority of research on the EITC, Hoynes & Patel (2016) and Bastian & Jones (2019) use difference-in-differences methods and variation in the generosity of the credit across years and family sizes due to federal tax reforms. Hoynes & Patel (2016) focus mainly on the 1993 expansion of the EITC while Bastian & Jones (2019) use variation from all reforms.[39] Labor supply and program participation are measured in the tax year that the EITC change goes into effect. In contrast, our study measures outcomes in the filing year when those EITC changes are realized in the tax return.

This timing difference likely drives the differences in our findings. Again, the EITC affects program participation decisions in two ways. First, as a wage subsidy it encourages labor supply, which decreases eligibility for means-tested programs. Second, as an income transfer it can reduce families' need for these programs and decrease their willingness to go through the process of applying for the programs. Difference-in-differences analyses capture a combination of both of these effects. Changes to program participation in the year the EITC change is enacted likely only reflect the wage subsidy mechanism. However, to the extent that the difference-in-differences analyses allow for lagged changes in program participation to be associated with changes in the EITC, whether through the use of a pre-post design or by allowing small year-to-year changes in the EITC due to inflation, these estimates can also capture income effects. In contrast, our estimates only capture the income effect. Households on either side of the January 1 cutoff face the same EITC schedule for the current tax year by the time they file their taxes in their child's first year of life.

Our finding of a zero effect of receiving the EITC thus indicates that previous estimates of a negative effect of EITC on program participation are primarily driven by the wage subsidy effect. Both Hoynes & Patel (2016) and Bastian & Jones (2019) hypothesize that loss of eligibility is the likely cause of their estimates although they do not attempt to separate out these effects. Our results imply that these hypotheses were correct.

[39] Both studies also use variation in the credit to the extent that the indexing and rounding of the credit specified in the tax code does not match their measure of inflation.

8 Conclusion

In this paper, we use a regression discontinuity design to examine how households' decisions to participate in SNAP and TANF change when they receive an influx of income from tax refunds. The benefit structures of the SNAP and TANF programs have in the past made it difficult to isolate the effect of income on participation from the effects of households losing eligibility for these programs. Our focus on tax refunds allows us to estimate this pure effect of income on participation. Using rich administrative data on households' receipt of SNAP and TANF benefits linked to survey and administrative birth data, we find that although the EITC and other child tax benefits increase households' cash-on-hand by an average of $2,219, this income has no effect on households' use of SNAP and TANF: we estimate a decrease in annual participation in SNAP of 0.4 percentage points or about 1.2 percent of the mean participation rate.

These findings are closely tied to previous work studying the effect of pro-work policies like the EITC and welfare reform on participation in the traditional safety net programs. Policy makers often promote pro-work policies as a means to decreasing households' dependence on programs like SNAP and TANF. Our findings in this paper suggest that the additional income households receive from the EITC is not enough on its own to make households choose not to participate in SNAP and TANF. Instead, the previous findings that expansions of the EITC decrease participation in SNAP and TANF are likely driven by households losing eligibility for these programs or facing higher costs of participation as their labor supply increases.

Our finding of no effect of income from tax refunds on households' SNAP and TANF participation decisions are likely the result of three factors. First, the cost of participating in programs may be relatively low. Second, there may be few households truly on the margin of participation. Finally, households eligible for SNAP and TANF may have marginal utilities of consumption that are close to constant, suggesting that households continue to highly value additional consumption as they increase their labor supply. Although our research design does not allow us to disentangle these potential factors, we think this is an interesting avenue for future research. In particular, if the final factor is the main one driving our results, then expanding eligibility for these programs could significantly improve the well-being of working families.

References

42 U.S.C. § 1382a. (n.d.).

Aussenberg, R. A., & Falk, G. (2019, August). *The Supplemental Nutrition Assistance Program (SNAP): Categorical Eligibility.* CRS Report R42054.

Bastian, J. E., & Jones, M. R. (2019, August). *Do EITC Expansions Pay for Themselves? Effects on Tax Revenue and Public Assistance Spending.* Mimeo.

Benson, C. (2018, November). *Is Special Education a Pathway to Supplemental Security Income for Children?* Mimeo.

Bitler, M. P., & Hoynes, H. W. (2010). The state of the social safety net in the post-welfare reform era. *Brookings Papers on Economic Activity*, *Fall*, 71–127.

Bitler, M. P., & Hoynes, H. W. (2016). The more things change, the more the stay the same? The safety net and poverty in the Great Recession. *Journal of Labor Economics*, *34*, S403–S444.

Blank, R. M. (2002, December). Evaluating Welfare Reform in the United States. *Journal of Economic Literature*, *40*(4), 1105-1166.

Blank, R. M., & Ellwood, D. T. (2001, August). *The Clinton Legacy for America's Poor.* NBER Working Paper No. 8437. Cambridge, MA.

Bond, B., Brown, J. D., Luque, A., & O'Hara, A. (2014, April). *The Nature of the Bias When Studying Only Linkable Person Records: Evidence from the American Community Survey.* CARRA Working Paper Series No. 2014-08.

Brooks, T., Roygardner, L., Artiga, S., Pham, O., & Dolan, R. (2020, March). *Medicaid and CHIP Eligibility, Enrollment, and Cost Sharing Policies as of January 2020: Findings from a 50-State Survey.* Kaiser Family Foundation.

Buckles, K. S., & Hungerman, D. M. (2013, July). Season of Birth and Later Outcomes: Old Questions, New Answers. *Review of Economics and Statistics*, *95*(3), 711–724.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014, November). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, *82*(6), 2295–2326.

Card, D., Dobkin, C., & Maestas, N. (2009, May). Does Medicare Save Lives? *Quarterly Journal of Economics*, *124*(2), 597–636.

Card, D., & Shore-Sheppard, L. D. (2004, August). Using Discontinuous Eligibility Rules to Identify the Effects of the Federal Medicaid Expansions on Low-Income Children. *Review of Economics and Statistics*, *86*(3), 752–766.

Cerf, B. (2014, July). *Within and Across County Variation in SNAP Misreporting: Evidence from Linked ACS and Administrative Records.* CARRA Working Paper Series No. 2014-05.

Chetty, R., & Saez, E. (2013, January). Teaching the Tax Code: Earnings Responses to an Experiment with EITC Recipients. *American Economic Journal: Applied Economics*, *5*(1), 1–31.

Council of Economic Advisers. (1999, August). *Technical report: The effects of welfare policy and the economic expansion on welfare caseloads: An update* (Tech. Rep.).

Cunnyngham, K. (2018, July). *Trends in Supplemental Nutrition Assistance Program Participation Rates: Fiscal Year 2010 to Fiscal Year 2016.* Current Perspective on SNAP Participation.

Currie, J. (2004, June). *The take-up of social benefits.* NBER Workingn Paper No. 10488.

Currie, J., & Grogger, J. (2001). Explaining Recent Declines in Food Stamp Program Participation [with comments]. *Brookings-Wharton Papers on Urban Affairs*, 203–244.

Daponte, B. O., Sanders, S., & Taylor, L. (1999). Why Do Low-Income Households Not Use Food Stamps? Evidence from an Experiment. *Journal of Human Resources*, *34*(3), 612–628.

Dickert-Conlin, S., & Chandra, A. (1999). Taxes and the Timing of Births. *Journal of Political Economy*, *107*(1), 161–177.

Elder, T. E. (2010). The importance of relative standards in ADHD diagnoses: Evidence based on exact birth dates. *Journal of Health Economics*, *29*, 641–656.

Feenberg, D., & Coutts, E. (1993). An Introduction to the TAXSIM Model. *Journal of Policy Analysis and Management*, *12*(1), 189–194.

Finkelstein, A., & Notowidigdo, M. J. (2019, August). Take-Up and Targeting: Experimental Evidence from SNAP. *Quarterly Journal of Economics*, *134*(3), 15015–1556.

Food and Nutrition Act of 2008. (2019). 7 U.S.C. § 2014.

Giannarelli, L. (2019, July). *What Was the TANF Participation Rate in 2016?* Urban Institute.

Gleason, P., Schochet, P., & Moffitt, R. (1998, April). *The Dynamics of Food Stamp Program Participation in the Early 1990s.* Current Perspectives on Food Stamp Program Participation.

Goehring, B., Heffernan, C., Minton, S., & Giannarelli, L. (2019, August). *Welfare Rules Databook: State TANF Policies as of July 2018.* OPRE Report 2019-83.

Gray, K., Trippe, C., Tadler, C., Perry, C., Johnson, P., & Betson, D. (2019, December). *National- and State-Level Estimates of WIC Eligibility and WIC Program Reach in 2017.* U.S.D.A. Nutrition Assistance Program Report Series, Food and Nutrition Service, Office of Policy Support.

Grogger, J. (2003, May). The effects of time limits, the EITC, and other policy changes on welfare use, work, and income among female-headed families. *Review of Economics and Statistics*, *85*(2), 394-408.

Hahn, H., Aron, L., Lou, C., Pratt, E., & Okoli, A. (2017, June). *Why Does Cash Welfare Depend on Where You Live? How and Why State TANF Programs Vary.* Urban Institute Research Report.

Hahn, J., Todd, P., & der Klaauw, W. V. (2001, January). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, *69*(1), 201–209.

Homonoff, T., & Somerville, J. (in press). Program Recertication Costs: Evidence from SNAP. *American Economic Journal: Economic Policy*.

Hoynes, H. W., & Patel, A. J. (2016, November). *Effective Policy for Reducing Poverty and Inequality? The Earned Income Tax Credit and the Distribution of Income.* Working paper.

Hoynes, H. W., & Schanzenbach, D. W. (2015, March). *U.S. Food and Nutrition Programs.* NBER Working Paper No. 21057.

Kabbani, N. S., & Wilde, P. E. (2003). Short Recertification Periods in the U.S. Food Stamp Program. *Journal of Human Resources*, *38*(Special Issue on Income Volatility and Implications for Food Assistance Programs), 1112–1138.

Kroft, K. (2008). Takeup, social multipliers and optimal social insurance. *Journal of Public Economics*, *92*, 722–737.

LaLumia, S. (2013, May). The EITC, Tax Refunds, and Unemployment Spells. *American Economic Journal: Economic Policy*, *5*(2), 188-221.

LaLumia, S., Sallee, J. M., & Turner, N. (2015). New evidence on taxes and the timing of birth. *American Economic Journal: Economic Policy*, *7*(2), 258–293.

Lee, D. S. (2008, February). Randomized experiments from non-random selection in U.S. House elections. *Journal of Econometrics*, *142*(2), 675–697.

Leftin, J., Eslami, E., & Strayer, M. (2011, August). *Trends in Supplemental Nutrition Assistance Program Participation Rates: Fiscal Year 2002 to Fiscal Year 2009.* Current Perspectives on SNAP Participation.

Lynch, V., Loprest, P., & Wheaton, L. (2017, August). *Joint Eligibility and Participation in SNAP and Medicaid/CHIP, 2011, 2013, and 2015.* Urban Institute Research Report.

Maag, E., Pergamit, M., Hanson, D., Ratcliffe, C., Edelstein, S., & Minton, S. (2015, September). *Using Supplemental Nutrition Assistance Program Data in Earned Income Tax Credit Administration.* Urban Institute.

Mann, C. (2011, February). *Tax Relief, Unemployment Insurance Reauthorization and Job Creation Act of 2010 - Implications for Medicaid and CHIP.* Center for Medicaid, CHIP and Survey & Certification CMCS Information Bulletin.

McCrary, J. (2008, February). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, *142*(2), 698–714.

McKernan, S.-M., & Ratcliffe, C. (2003, June). *Employment Factors Influencing Food Stamp Program Participation.* The Urban Institute. Washington, DC.

Medicaid and CHIP Payment and Access Commission. (2019, April). *Medicaid Payment Initiatives to Improve Maternal and Birth Outcomes.* Issue Brief.

Meyer, B. D., & Goerge, R. M. (2011, April). *Errors in Survey Reporting and Imputation and Their Effects on Estimates of Food Stamp Program Participation.* Center for Economic Studies Working Paper 11-14.

Meyer, B. D., Mittag, N., & Goerge, R. M. (2018, October). *Errors in Survey Reporting and Imputation and Their Effects on Estimates of Food Stamp Program Participation.* NBER Working Paper 25143.

Meyer, B. D., Mok, W. K. C., & Sullivan, J. X. (2009, July). *The Under-Reporting of Transfers in Household Surveys: Its Nature and Consequences.* NBER Working Paper 15181.

Meyer, B. D., & Sullivan, J. X. (2008). Changes in the Consumption, Income, and Well-Being of Single Mother Headed Families. *American Economic Review*, *98*(5), 2221-2241.

Moffitt, R. A. (1983, December). An Economic Model of Welfare Stigma. *American Economic Review*, *73*(5), 1023–1035.

Moffitt, R. A. (2002). Welfare Programs and Labor Supply. In A. J. Auerbach & M. Feldstein (Eds.), *Handbook of public economics* (Vol. 4, pp. 2393–430). Elsevier Science B.V.

National Research Council. (2003). *Estimating Eligibility and Participation for the WIC Program: Final Report* (M. V. Ploeg & D. M. Betson, Eds.). Washington, DC: The National Academies Press.

Newman, C., & Scherpf, E. (2013, September). *Supplemental Nutrition Assistance Program (SNAP) Access at the State and County Levels: Evidence from Texas SNAP Administrative Records and the American Community Survey.* USDA Economic Research Report Number 156.

Nichols, A., & Rothstein, J. (2015, May). *The Earned Income Tax Credit (EITC).* NBER Working Paper No. 21211.

Omnibus Budget Reconciliation Act of 1990. (n.d.). Pub. L. No. 101-508, 104 Stat. 1388.

O'Neill, J. E., & Hill, M. A. (2001, July). *Gaining Ground? Measuring the Impact of Welfare Reform on Welfare and Work* (Civic Report No. 17). NY: Center for Civic Innovation.

Ponza, M., Ohls, J. C., Moreno, L., Zambrowski, A., & Cohen, R. (1999, July). *Customer service in the Food Stamp Program.* Report. U.S. Department of Agriculture.

Salanié, B. (2003). *The Economics of Taxation.* MIT Press.

Schanzenbach, D. W. (2009, September). *Experimental Estimates of the Barriers to Food Stamp Enrollment.* Institute for Research on Poverty Discussion Paper No. 1367-09.

Schulkind, L., & Shapiro, T. M. (2014). What a difference a day makes: Quantifying the effects of birth timing manipulation on infant health. *Journal of Health Economics*, *33*, 139–158.

Shirley, P. (2020, April). *First-Time Mothers and the Labor Market Effects of the Earned Income Tax Credit.* Mimeo.

Tach, L., & Halpern-Meekin, S. (2014, December). Tax code knowledge and behavioral responses among EITC recipients: Policy insights from qualitative data. *Journal of Policy Analysis and Management*, *33*, 413–439.

United States Department of Agriculture. (2017, July). *SNAP—Fiscal Year 2018 Cost-of-Living Adjustments.*

United States Department of Agriculture Food and Nutrition Service. (2009, June). *Supplemental Nutrition Assistance Program State Options Report.* Eighth Edition.

United States Department of Agriculture Food and Nutrition Service. (2018, May). *Supplemental Nutrition Assistance Program State Options Report.* Fourteenth Edition.

United States Department of Agriculture Food and Nutrition Service. (2020, January). *SNAP Monthly State Participation and Benefit Summary- Public Data, Fiscal Year 2020.*

U.S. Department of Health & Human Services Office of Family Assistance. (2004, December). *Caseload Data 1994 (AFDC Total).* (https://www.acf.hhs.gov/ofa/resource/caseload-data-afdc-1994-total)

U.S. Department of Health & Human Services Office of Family Assistance. (2019a, April). *TANF Caseload Data 2018.*

U.S. Department of Health & Human Services Office of Family Assistance. (2019b, September). *TANF Financial Data - FY 2018.*

Valle, L. C., & Perez-Lopez, D. J. (2020, January). *Family Participation Rates in Nutrition Assistance Programs: 2015.* U.S. Census Bureau Current Population Reports.

Wagner, D., & Layne, M. (2014, July). *The Person Identification Validation System (PVS): Applying the Center for Administrative Records Research and Applications' (CARRA) Record Linkage Software.* CARRA Working Paper No. 2014-01.

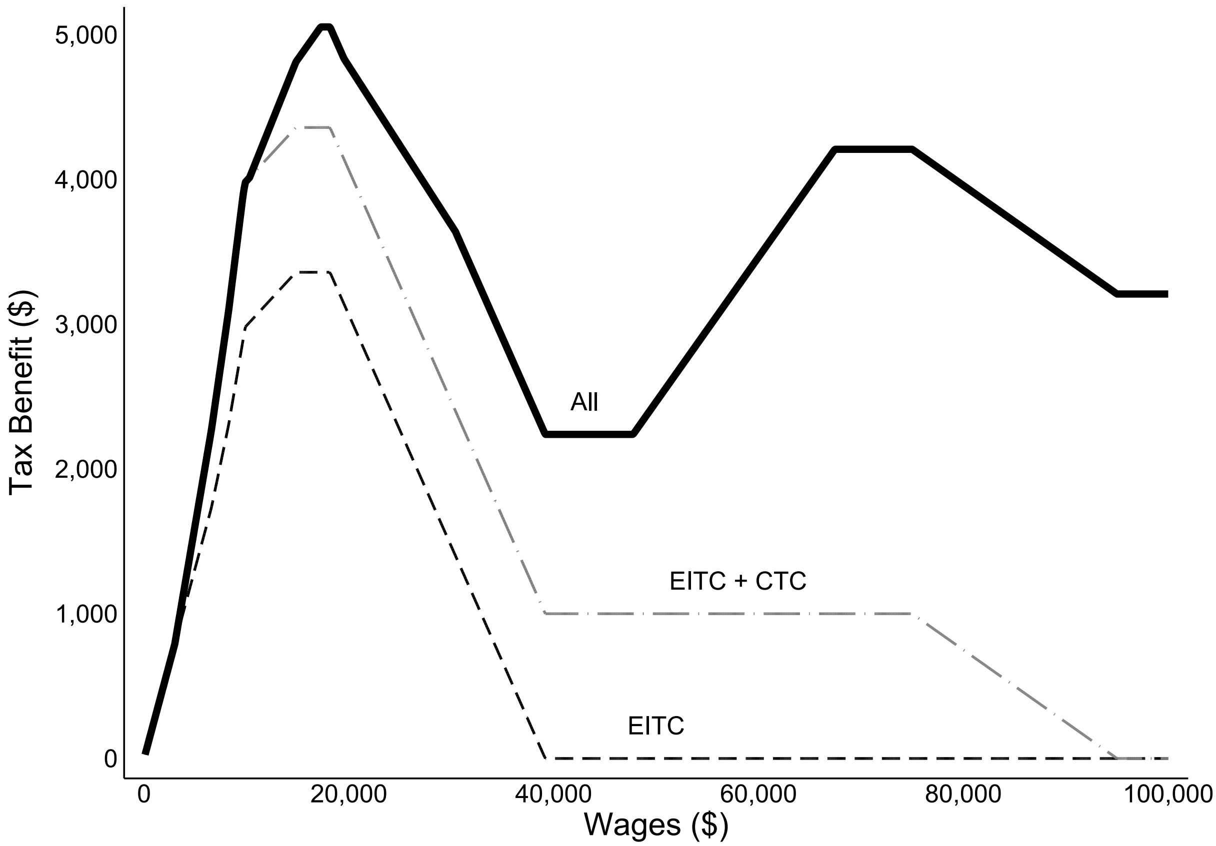
Wingender, P., & LaLumia, S. (2017, March). Income Effects on Maternal Labor Supply: Evidence from Child-Related Tax Benefits. *National Tax Journal*, *70*(1), 11–52.

Zedlewski, S. R., & Brauner, S. (1999). *Declines in Food Stamp and Welfare Participation: Is There a Connection?* Assessing the New Federalism Discussion Papers No. 99-13.

Ziliak, J. P. (2015, March). *Temporary Assistance for Needy Families.* NBER Working Paper No. 21038.

9 Figures

Figure 1: Simulated Decrease in Tax Liability for a Single Parent Having Their First Child in 2017



Note: Figure shows the difference in tax liability for an unmarried tax filer who only has wage income and takes the standard deduction.

Figure 2: SNAP Eligibility and the EITC for a Single Adult in CY 2017

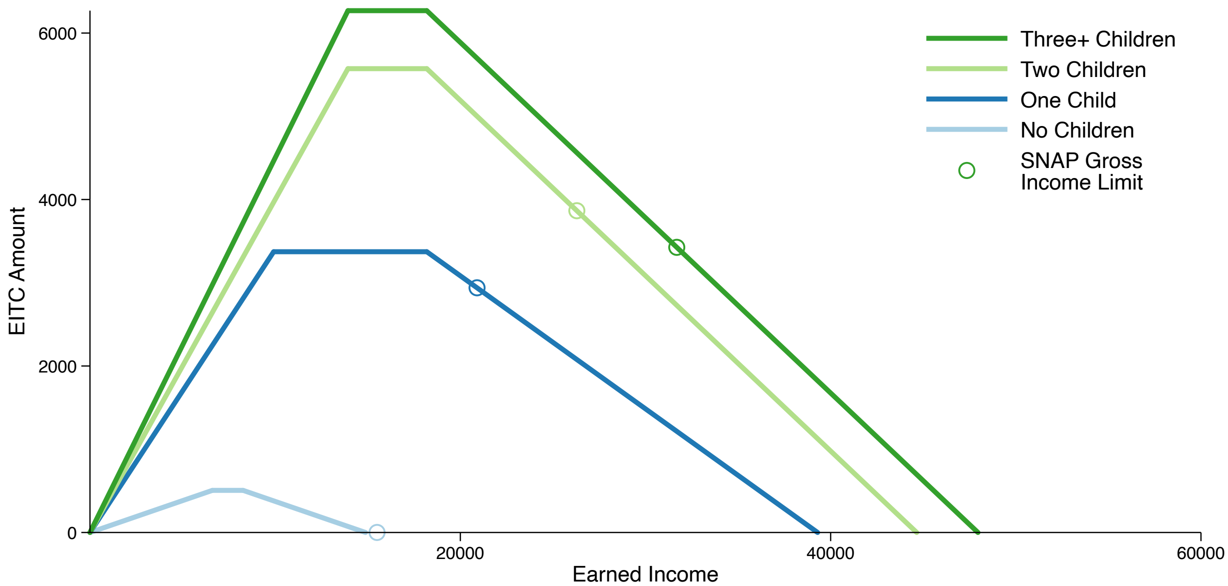


Figure 3: Density of Births Around the January 1 Cutoff, Excluding Births in Year of ACS

Discontinuity: -0.1184 (0.02082)

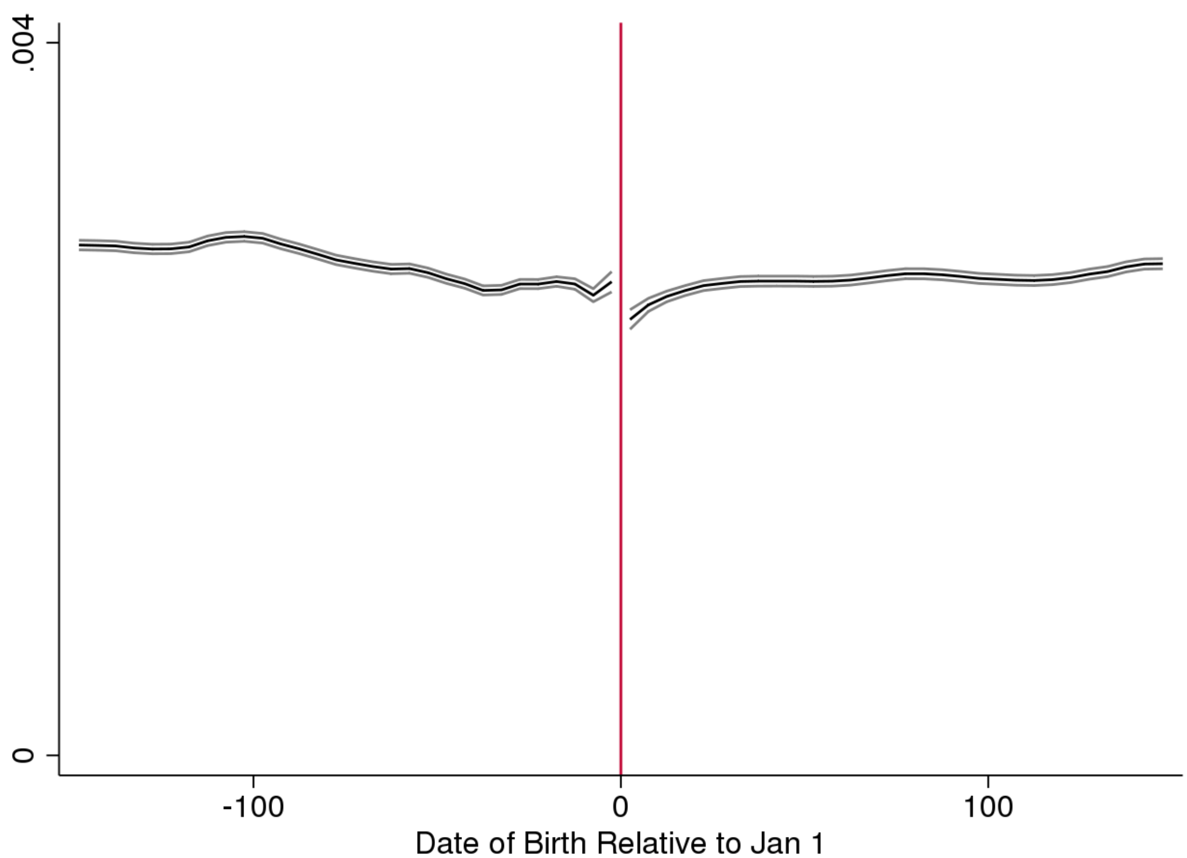


Figure 4: Density of Births Around the January 1 Cutoff, Excluding Births in Year of ACS, By Subgroups

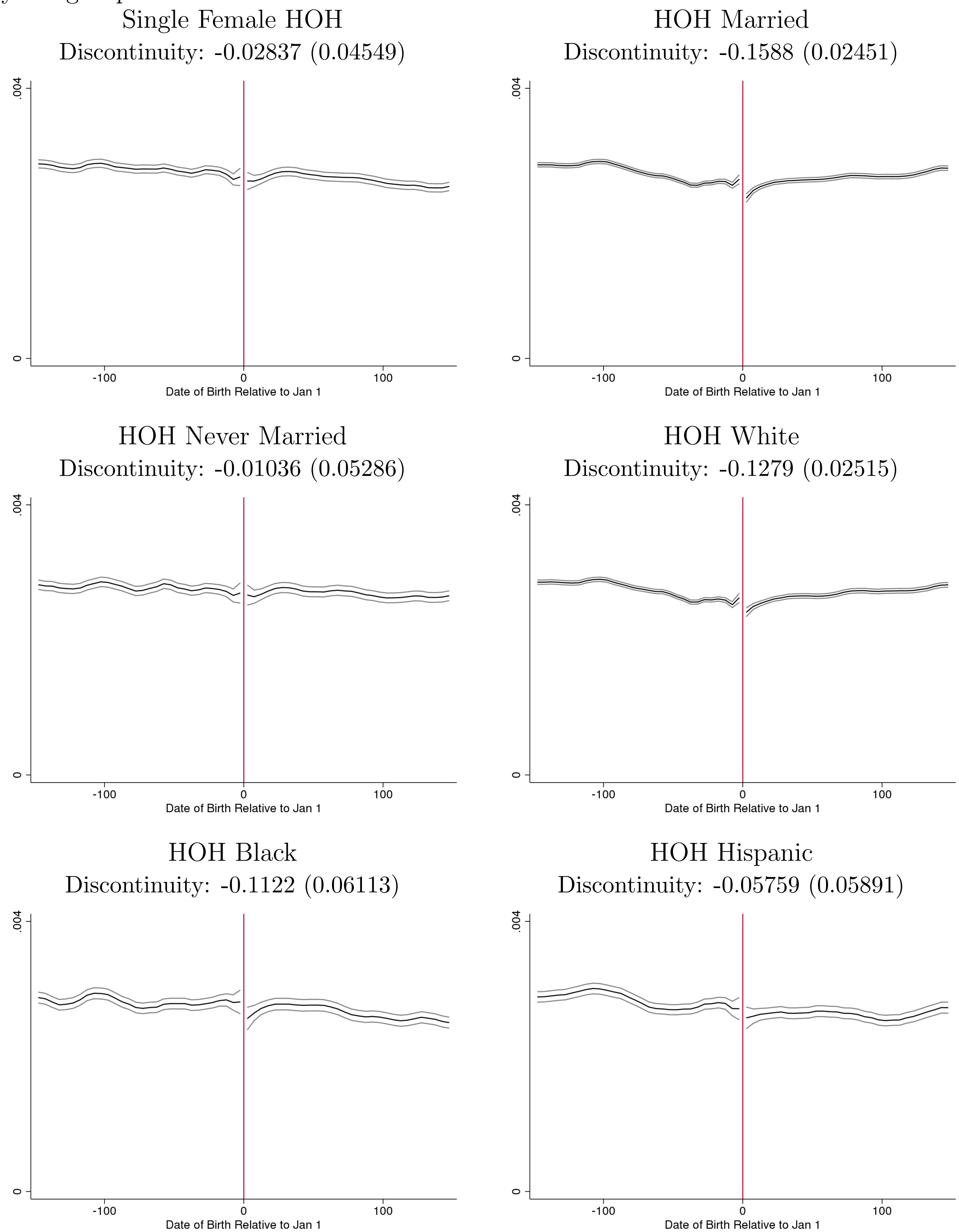
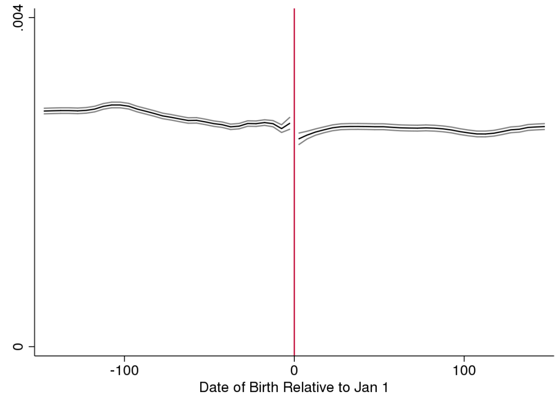


Figure 5: Density of Births Around the January 1 Cutoff, Excluding Births in Year of ACS, By Subgroups, Continued

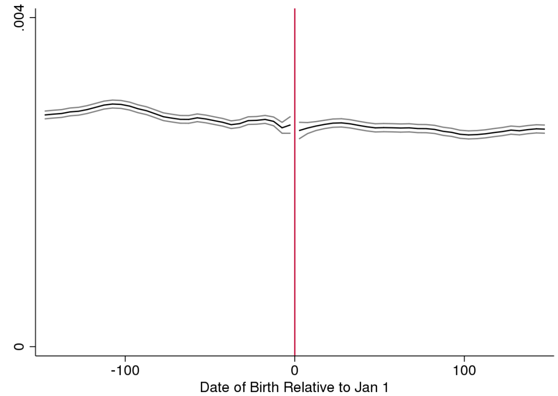
HOH Less Than BA

Discontinuity: -0.0988 (0.02599)



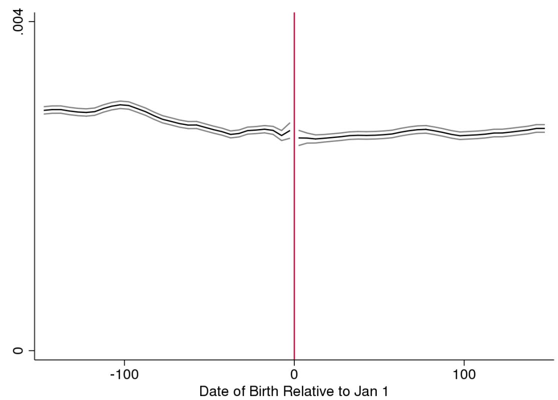
HOH HS Degree or Less

Discontinuity: -0.04124 (0.03624)



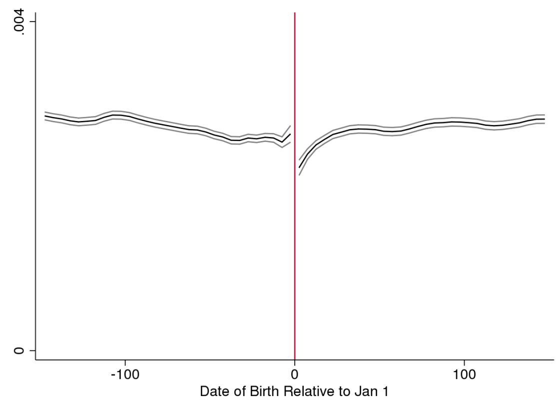
One EITC Child

Discontinuity: -0.04386 (0.03365)



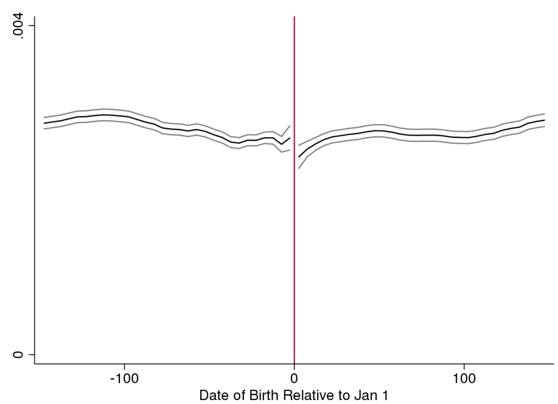
Two EITC Children

Discontinuity: -0.2375 (0.03831)



Three EITC Children

Discontinuity: -0.1315 (0.05487)



Four+ EITC Children

Discontinuity: -0.1598 (0.0735)

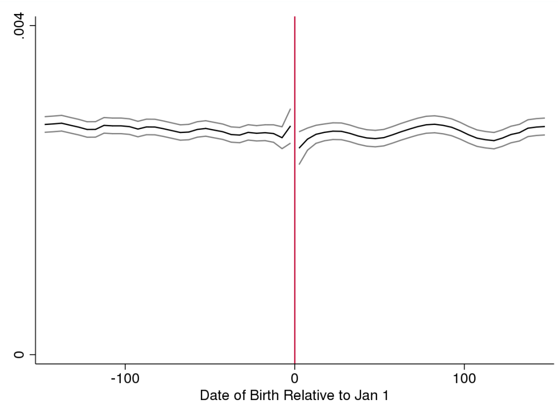


Figure 6: Tests of Smooth Characteristics of Households Around the January 1 Cutoff, All Birth Cohorts

Predicted SNAP Use in First Year of Life

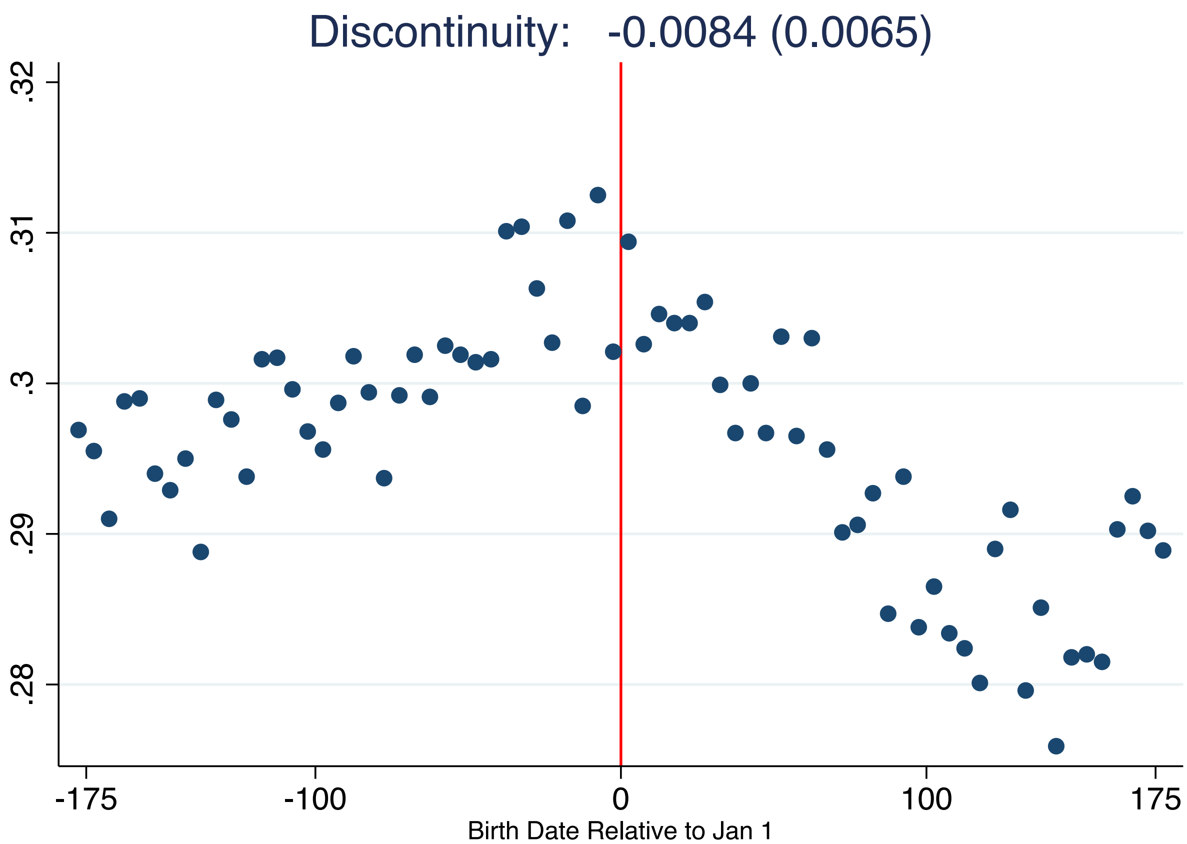
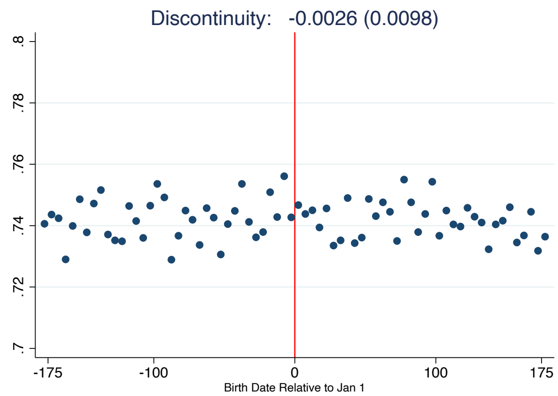
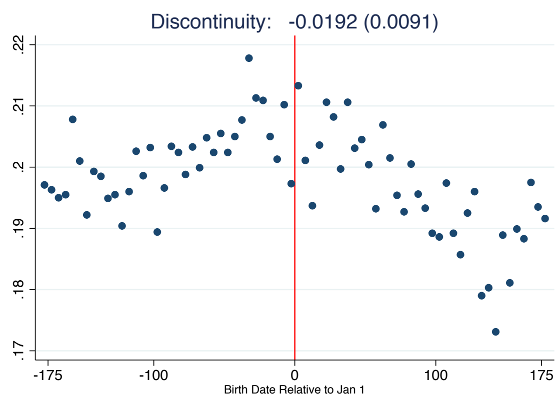


Figure 7: Tests of Smooth Characteristics of Households Around the January 1 Cutoff

Lives in Urban Area



Single Female HOH



HOH Usual Weekly Hours Worked

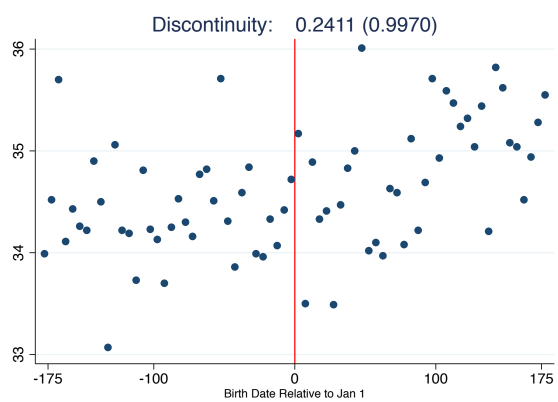


Figure 8: Tests of Smooth Characteristics of Households Around the January 1 Cutoff, Households Interviewed Within One Year of Birth

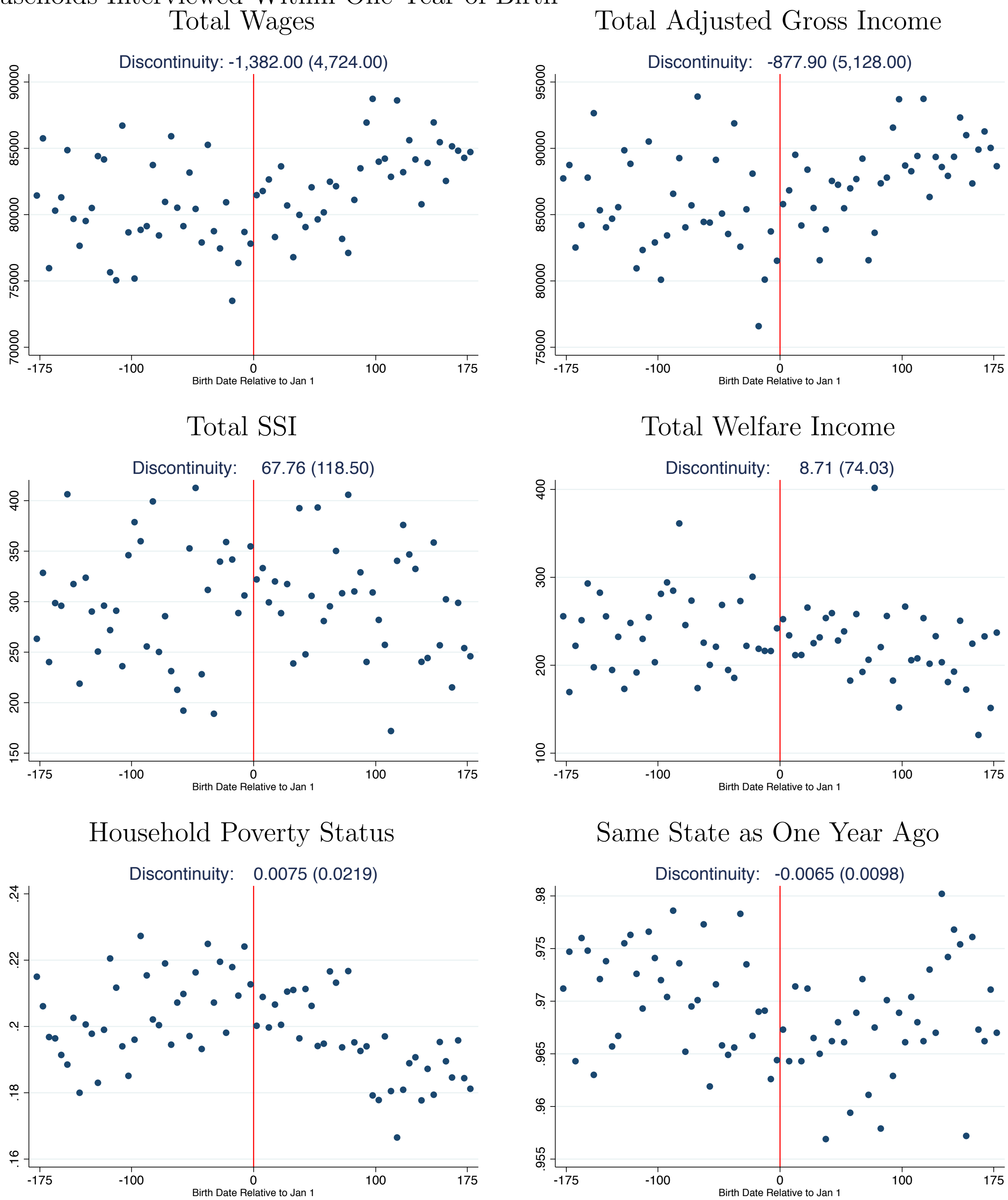
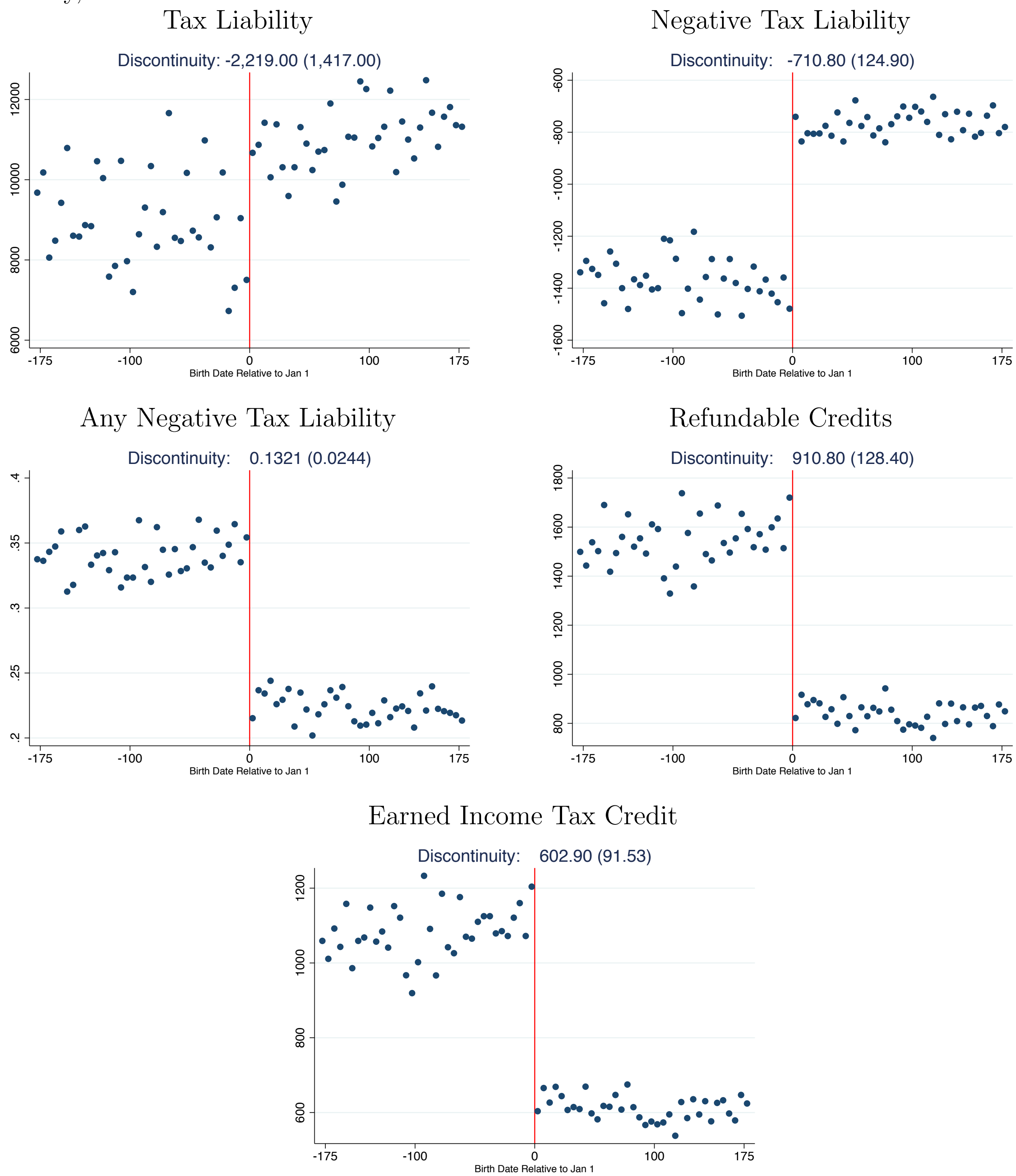


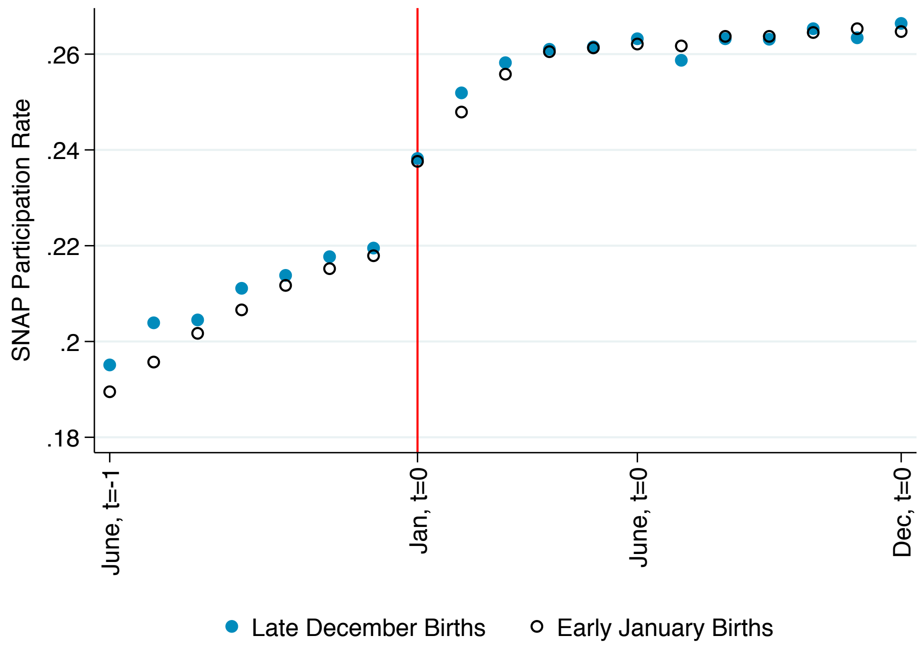
Figure 9: Imputed First Stage: Estimated Discontinuity in Total Household Federal Tax Liability, Households Interviewed Within One Year of Birth



Note: Household tax amounts are the sum of the imputed tax amounts for all tax units in the household.

Figure 10: SNAP Use Around the Time of a Child's Birth

Monthly SNAP Participation



Monthly Household SNAP Benefits

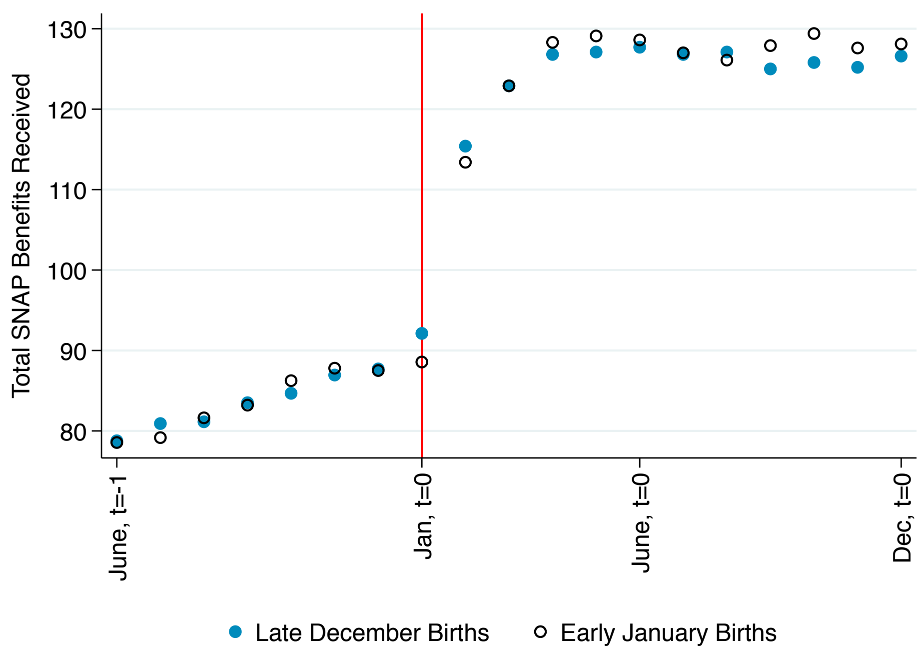


Figure 11: SNAP Participation By Calendar Month, All Birth Cohorts

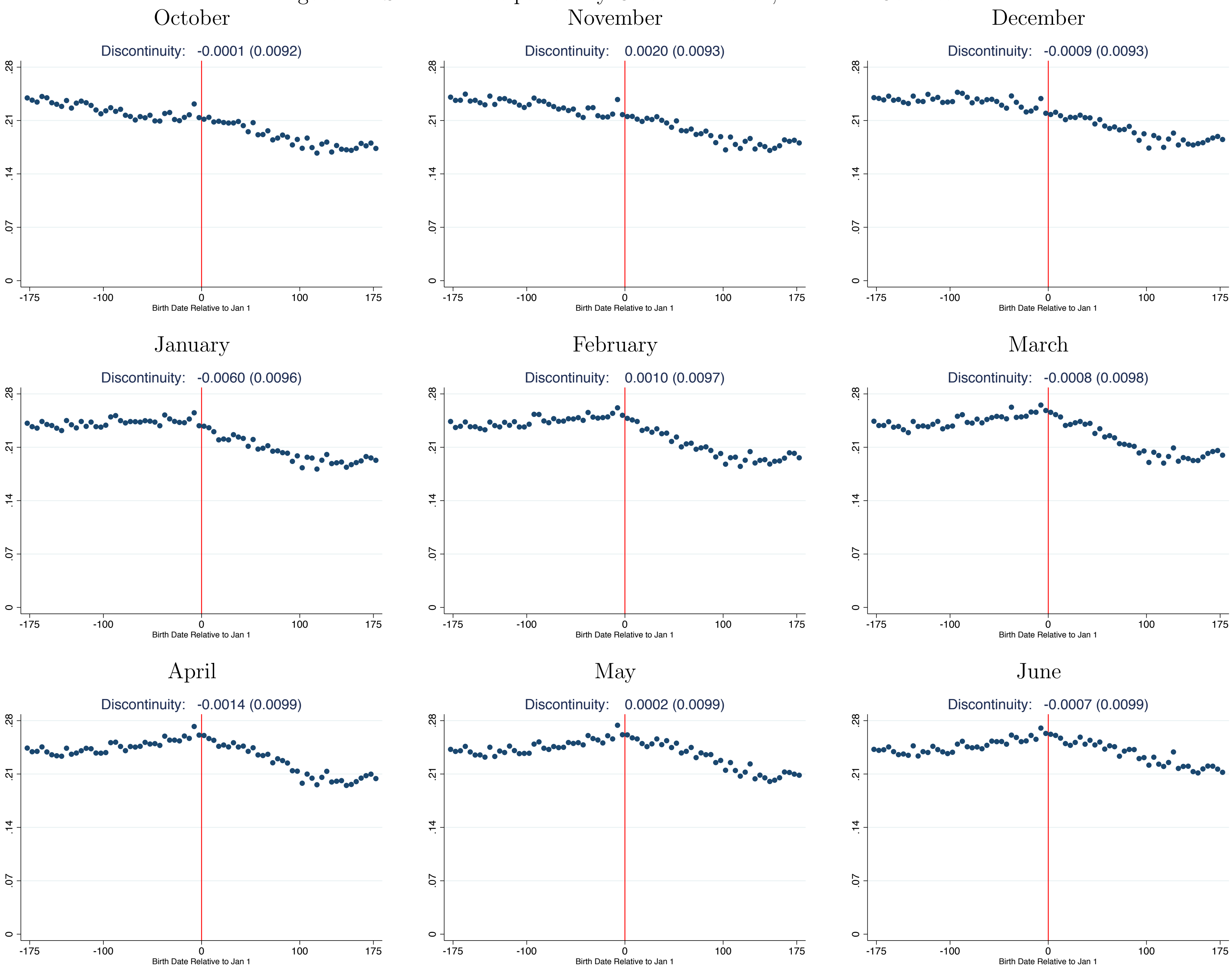


Figure 12: Household SNAP Benefit Amount By Calendar Month, All Birth Cohorts

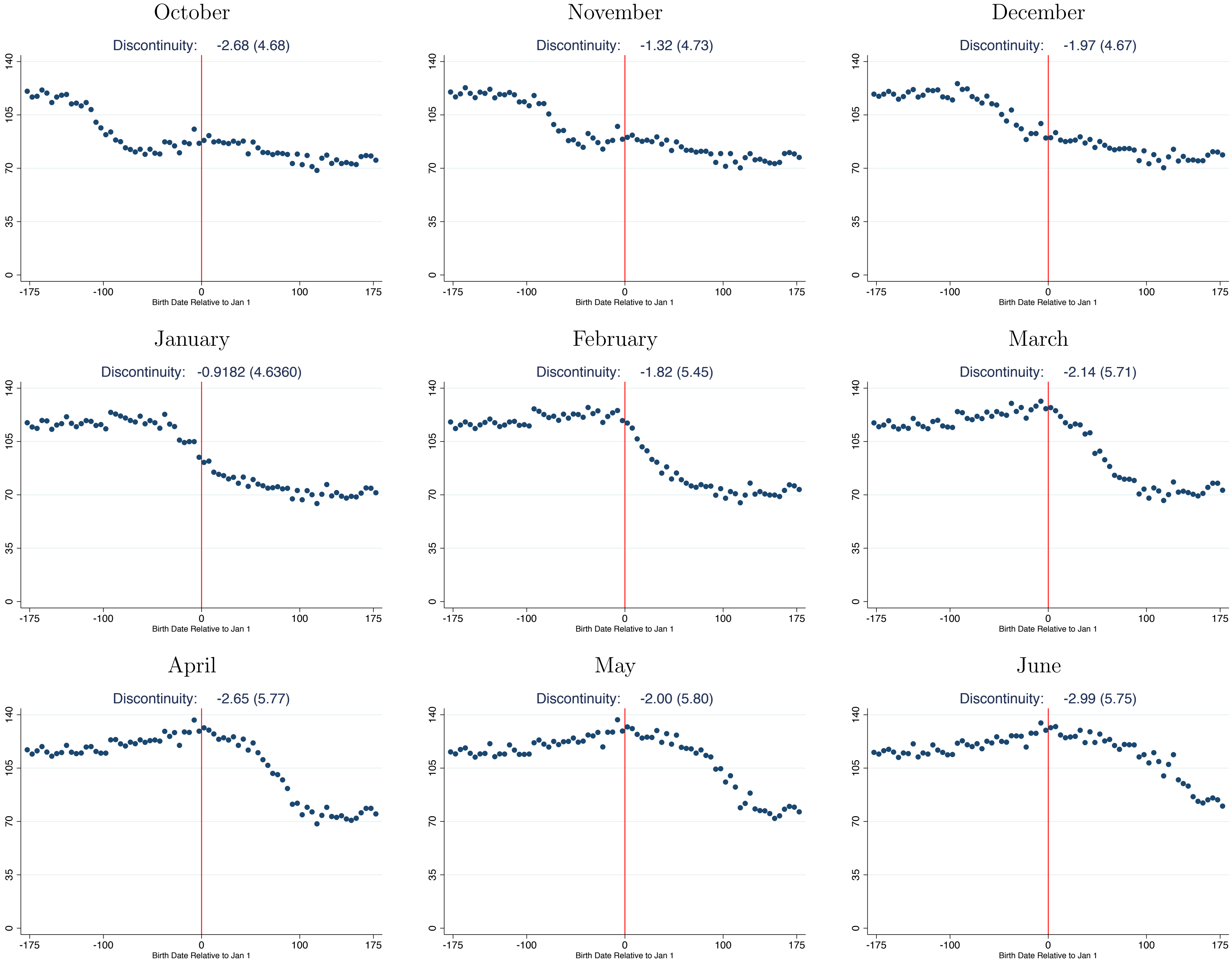


Figure 13: Household SNAP Benefit Amount By Calendar Month, Conditional on Participation, All Birth Cohorts

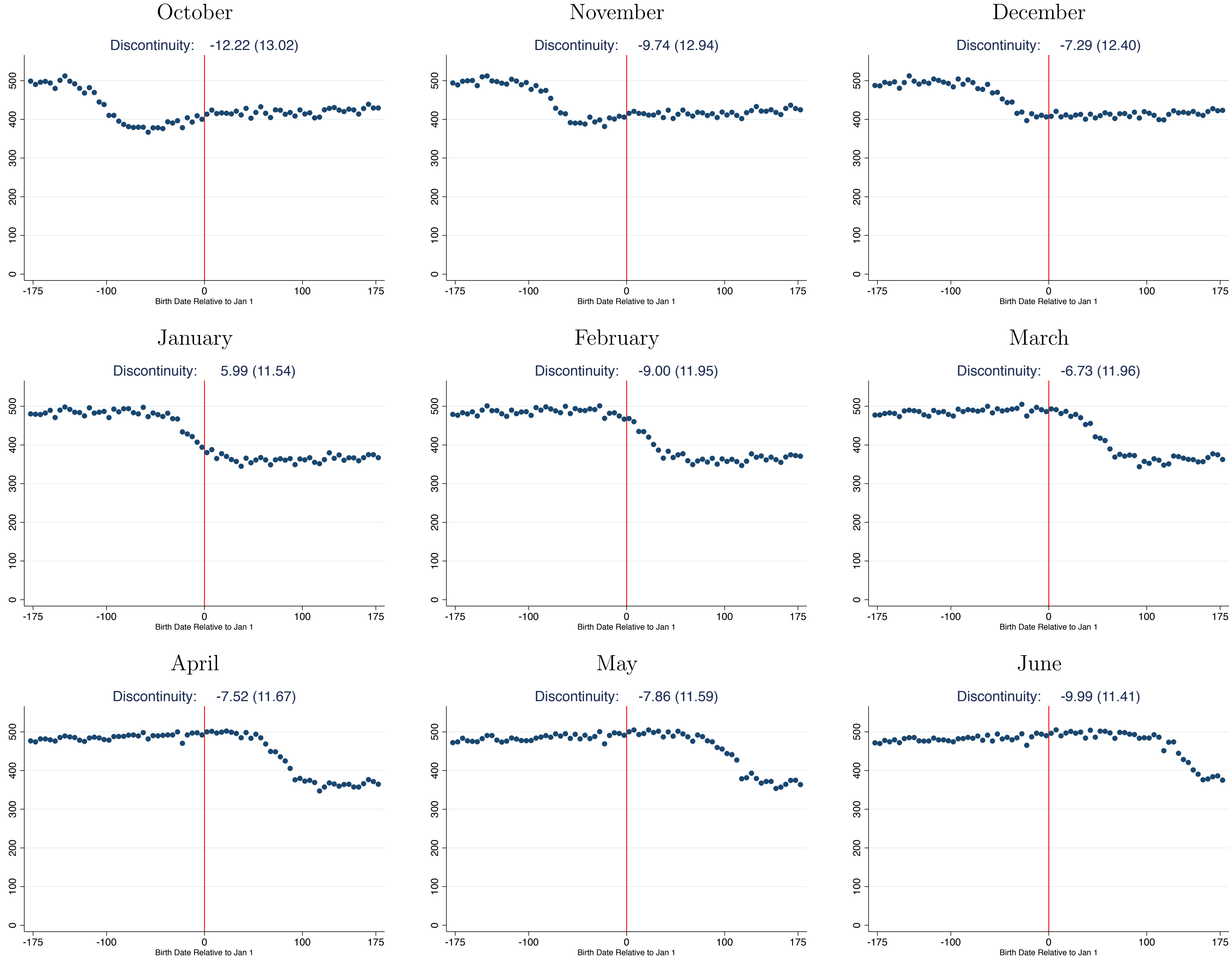
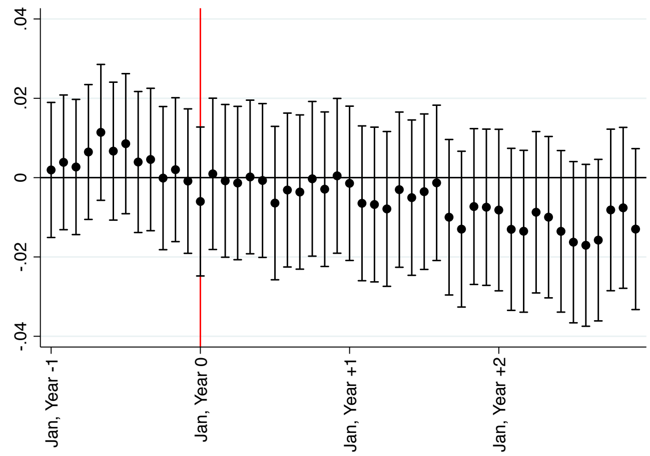
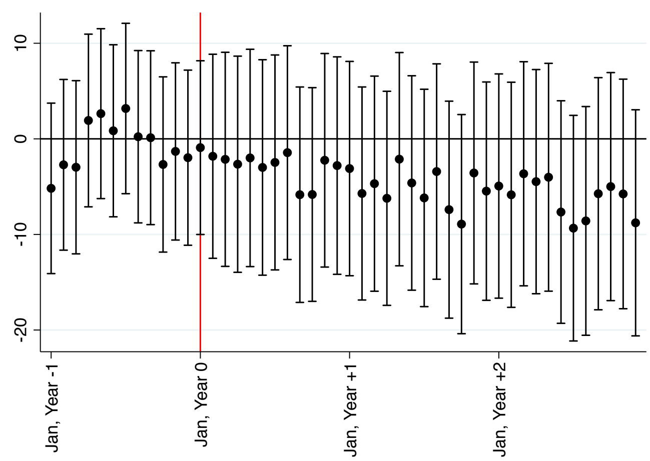


Figure 14: Estimated Discontinuities in Monthly SNAP Use, One Calendar Year Before Birth to Three Calendar Years After Birth, All Birth Cohorts

SNAP Participation

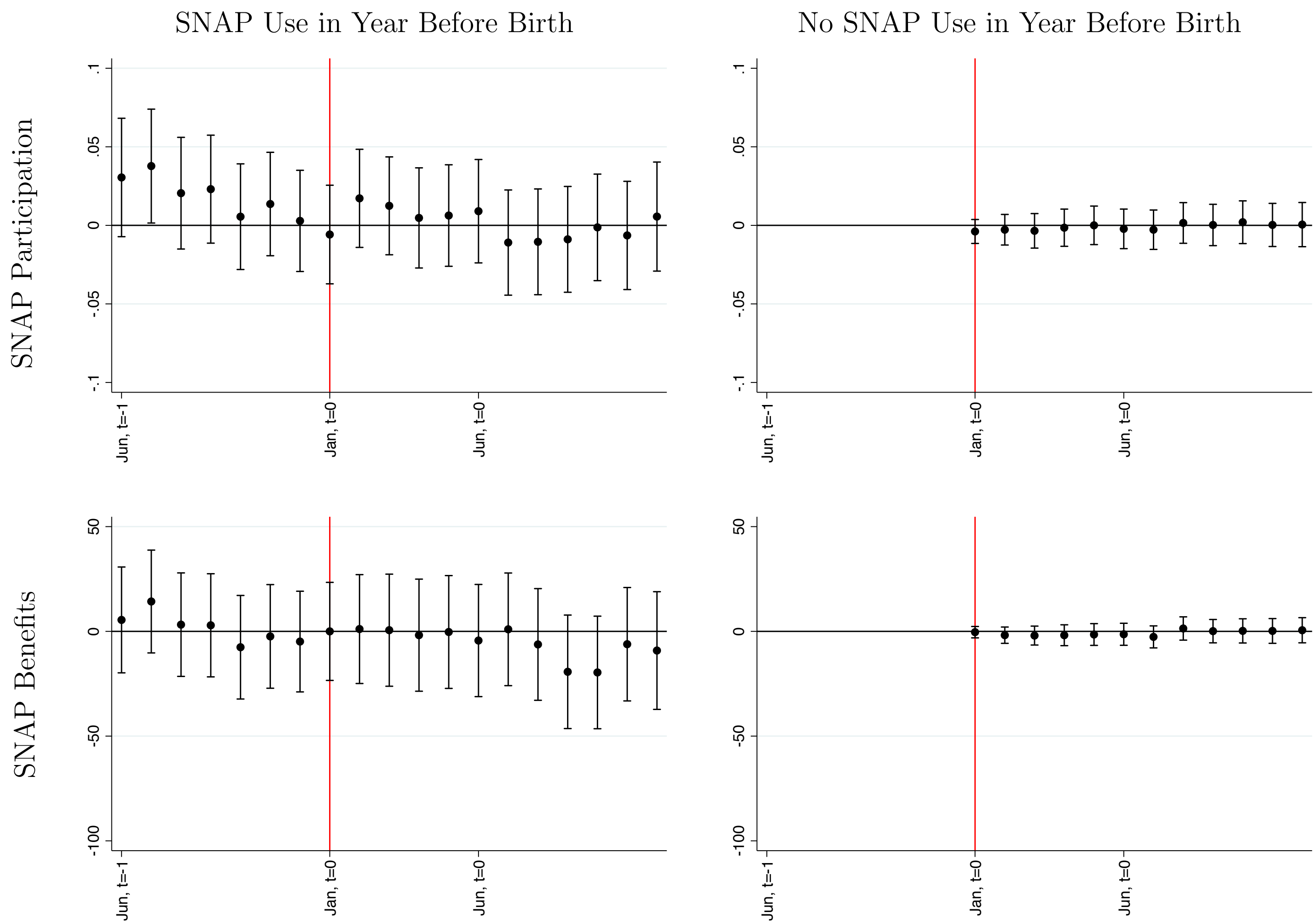


Household SNAP Benefit Amount



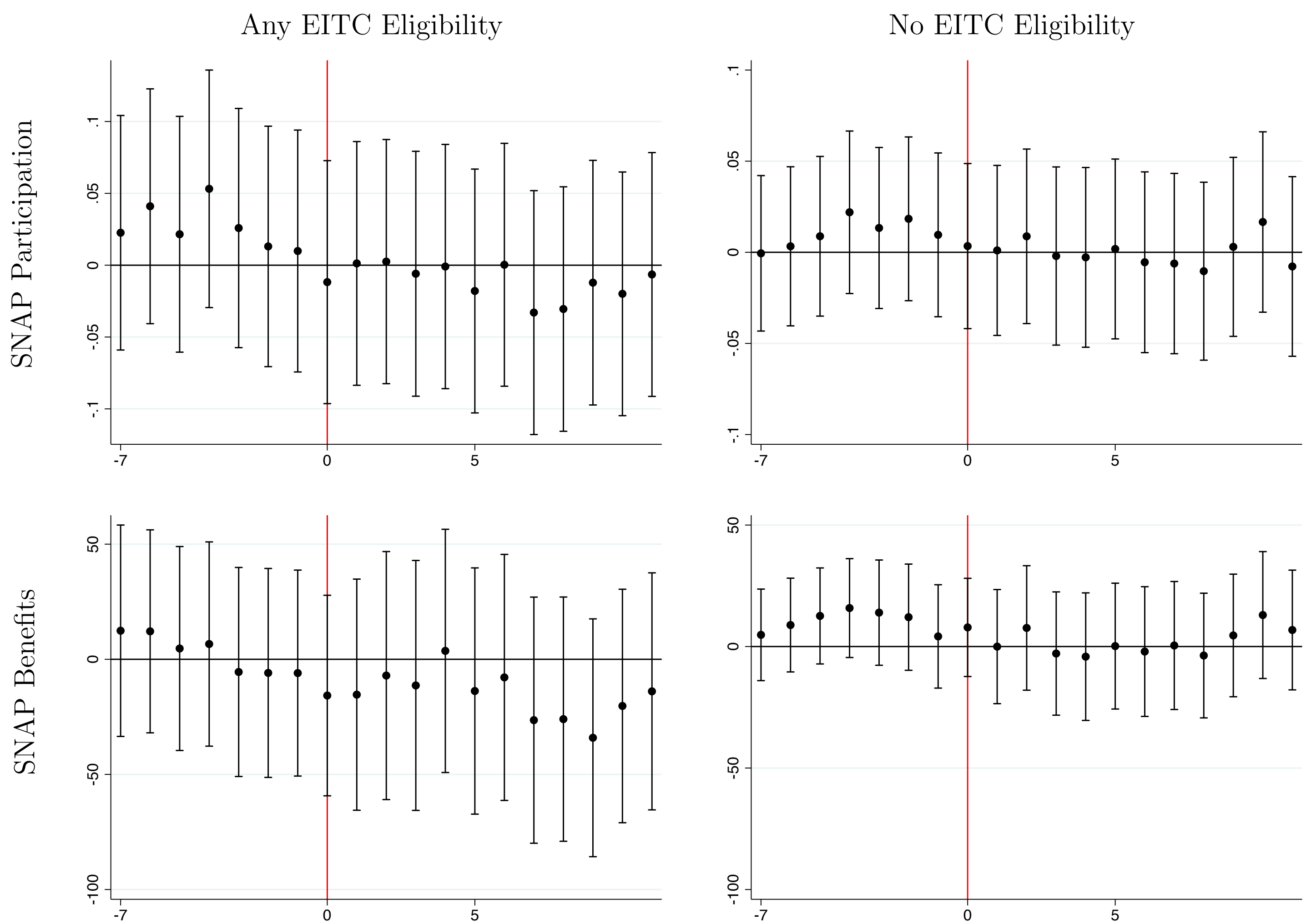
Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure 15: Heterogeneity: Estimated Discontinuities in Monthly SNAP Use, June of Year Before Focal January 1 to December of Year of Focal January 1, All Birth Cohorts, By Household SNAP Participation in Year Before Birth



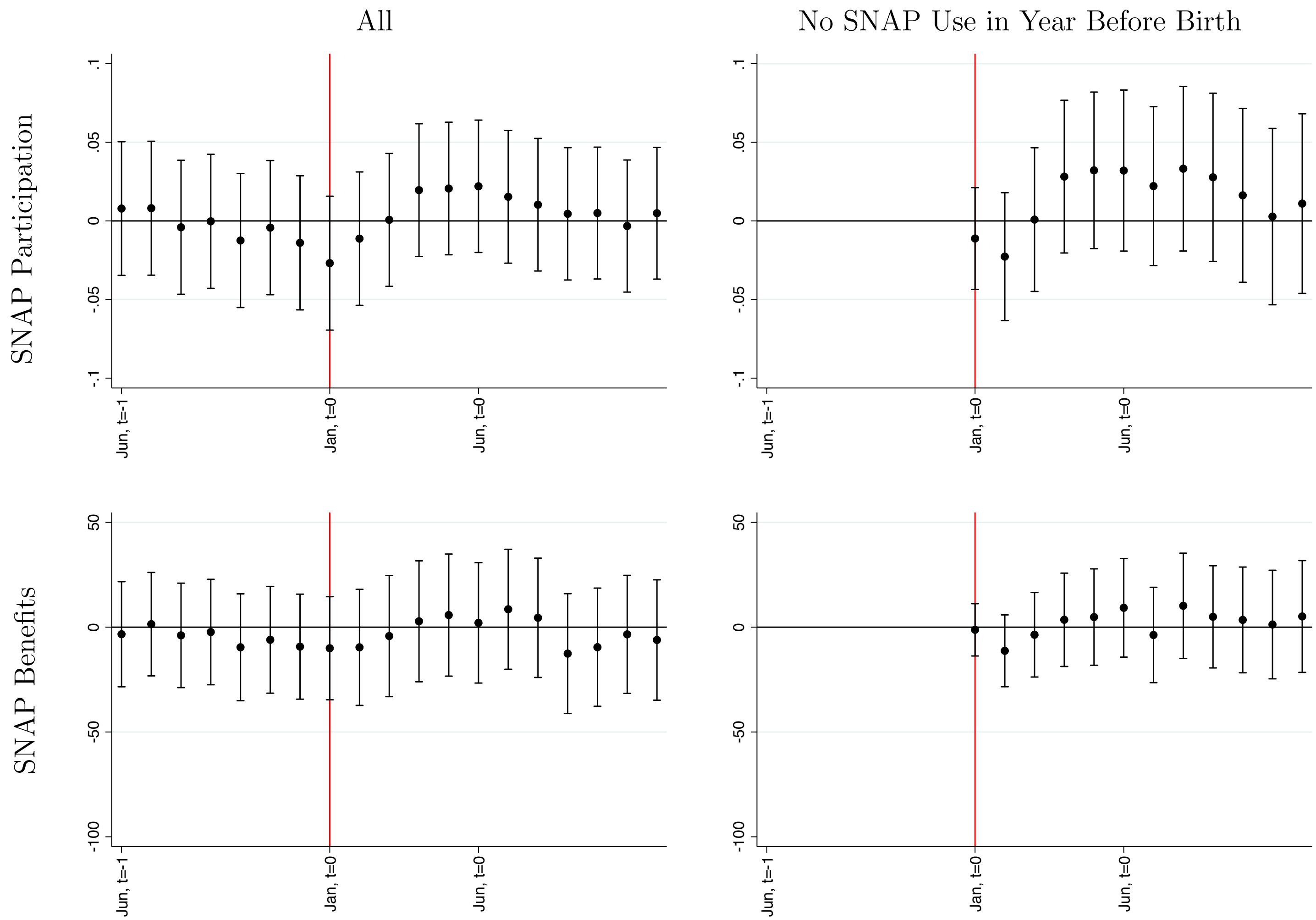
Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure 16: Heterogeneity: Estimated Discontinuities in Monthly SNAP Use, June of Year Before Focal January 1 to December of Year of Focal January 1, Households Interviewed Within One Year of Birth, By Predicted EITC Eligibility If Child Were Eligible for Tax Benefits in First Year of Life



Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure 17: Heterogeneity: Estimated Discontinuities in Monthly SNAP Use, June of Year Before Focal January 1 to December of Year of Focal January 1, Households with High Predicted Probability of Using SNAP, All Birth Cohorts



Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

10 Tables

Table 1: Descriptive Statistics: Characteristics of Reference Person of Households with December and January Births (%)

	December Births	January Births
White	68.19	68.50
	(46.58)	(46.45)
Black	11.69	11.56
	(32.13)	(31.98)
Hispanic	12.58	12.30
	(33.16)	(32.84)
Married	72.73	72.82
	(44.54)	(44.49)
Never Married	15.51	15.59
	(36.20)	(36.28)
Single Female HOH	20.60	20.46
	(40.44)	(40.34)
Less Than BA	63.75	63.28
	(48.07)	(48.20)
HS Degree or Less	32.61	32.82
	(46.88)	(46.96)
Urban	74.34	74.13
	(43.67)	(43.79)
Number of EITC Eligible Children		
One	36.95	36.50
	(48.27)	(48.14)
Two	31.00	30.70
	(46.25)	(46.13)
Three	14.57	14.82
	(35.28)	(35.53)
Four+	8.02	8.18
	(27.16)	(27.40)
Observations	43500	41500

Notes: Source is 2005-2016 waves if the American Community Survey. Sample is households with children born in December or January from December 2004 and January 2016.

Table 2: First Stage: Discontinuities in Imputed Tax Amounts, Households Interviewed Within One Year of Birth

	January Mean	Discontinuity
Total Federal Tax Liability	10,730	-2,219
	(28,010)	(1,417)
Negative Federal Tax Liability	-795.1	-710.8
	(1,930)	(124.9)
Any Negative Tax Liability	.2312	.1321
	(.4216)	(.02442)
Refundable Credits	868.8	910.8
	(1,968)	(128.4)
Earned Income Tax Credit	634.4	602.9
	(1,425)	(91.53)
Observations		14,500

Notes: Source is 2005-2016 waves if the American Community Survey. Sample is households with children born in December or January between December 2004 and January 2016 who responded to the ACS within one year of their youngest child's birth.

Table 3: Falsification Test: Discontinuities in Imputed Tax Amounts If Households Could Claim Youngest Child, Households Interviewed Within One Year of Birth

	January Mean	Discontinuity
Total Federal Tax Liability	9,074	-591
	(28,180)	(1,421)
Negative Federal Tax Liability	-1,360	-165.5
	(2,479)	(138.6)
Any Negative Tax Liability	.3387	.03284
	(.4733)	(.02558)
Refundable Credits	1,527	268.2
	(2,517)	(141.9)
Earned Income Tax Credit	1,072	168.8
	(1,793)	(100.4)
Observations		14,500

Notes: Source is 2005-2016 waves if the American Community Survey. Sample is households with children born in December or January between December 2004 and January 2016 who responded to the ACS within one year of their youngest child's birth. Imputed tax amounts are calculated as though every tax unit was able to claim their youngest child in the child's first year of life, regardless of the child's true date of birth.

Table 4: Estimated Discontinuities in SNAP Use By Calendar Year Relative to Focal January 1

	Year Before		Year Of		Year After		Two Years After	
	Jan. Mean	Disc.	Jan. Mean	Disc.	Jan. Mean	Disc.	Jan. Mean	Disc.
Any SNAP Participation	.2587	-.001958	.3144	-.004044	.3241	-.01163	.3282	-.01615
	(.4379)	(.009915)	(.4643)	(.01052)	(.468)	(.01058)	(.4696)	(.01097)
Number of Months with SNAP	2.313	.05108	3.008	-.02373	3.149	-.07325	3.227	-.1448
	(4.321)	(.09803)	(4.807)	(.1094)	(4.906)	(.1115)	(4.962)	(.1158)
Total SNAP Benefits	987.1	-7.922	1,451	-33.15	1,524	-61.45	1,556	-73.81
	(2,297)	(51.41)	(2,770)	(62.54)	(2,837)	(64.37)	(2,850)	(67.34)
Observations		85,000		85,000		85,000		80,000

Notes: Sources are 2005-2016 waves if the American Community Survey, administrative SNAP data, and the Census Numident. Sample is households with children born in December or January between December 2004 and January 2016. Coefficients are the bias-corrected discontinuity estimates and associated robust standard errors described in Calonico et al. (2014), estimated on a one month bandwidth around January 1.

Table 5: Predicting Whether Mother is Currently Employed and Working, 2005 to 2016 ACS Waves

	OLS Results		IV Results	
	Months Elapsed since Birth	Months Interacted with $DecBirth$	Months Elapsed since Birth	Months Interacted with $TaxValue$
Month 1	-0.319***	0.020	-0.318***	0.012
	(0.024)	(0.016)	(0.023)	(0.009)
Month 2	-0.242***	0.004	-0.242***	0.002
	(0.026)	(0.019)	(0.025)	(0.011)
Month 3	-0.109***	0.004	-0.107***	-0.001
	(0.028)	(0.028)	(0.029)	(0.016)
Month 4	-0.069***	0.025	-0.070***	0.016
	(0.022)	(0.023)	(0.021)	(0.014)
Month 5	-0.041	0.047*	-0.045	0.034**
	(0.028)	(0.025)	(0.027)	(0.015)
Month 6	-0.029	0.025	-0.026	0.011
	(0.020)	(0.019)	(0.021)	(0.011)
Month 7	-0.012	-0.012	-0.011	-0.008
	(0.017)	(0.016)	(0.017)	(0.010)
Month 8	0.005	-0.039**	0.004	-0.023**
	(0.019)	(0.019)	(0.019)	(0.010)
Month 9	0.004	0.026	0.003	0.016
	(0.022)	(0.020)	(0.021)	(0.011)
Month 10	0.019	0.021	0.016	0.016
	(0.020)	(0.018)	(0.020)	(0.010)
Month 11	0.009	0.016	0.010	0.008
	(0.018)	(0.025)	(0.018)	(0.015)
Month 12		0.025		0.015
		(0.021)		(0.014)
Observations		30,500		30,500

Notes: Sample is women interviewed in the ACS 2005 to 2016 waves within one year of giving birth who are between the ages of 20 and 40, gave birth between December 18th and January 14th, and worked within the past five years. Columns 1 and 2 report unweighted OLS coefficients and standard errors. Columns 3 and 4 report second-stage results from an IV regression. Standard errors are clustered at the state level. Each regression also controls for maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own children under age 19 in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, and dummies for being white, having some college education, having completed a college degree, and being married. *** Significant at the 1% level. ** Significant at the 5% level * Significant at the 10% level.

Table 6: Estimated Discontinuities in TANF Use By Calendar Year Relative to Focal January 1

	Year Before		Year Of		Year After		Two Years After	
	Jan. Mean	Disc.	Jan. Mean	Disc.	Jan. Mean	Disc.		
Any TANF Participation	.05556	-.0057	.0781	-.01073	.07097	-.005322	.06723	-.006634
	(.2291)	(.005637)	(.2683)	(.006554)	(.2568)	(.006152)	(.2504)	(.006219)
Number of Months with TANF	.3721	-.03558	.5714	-.06779	.5146	-.01369	.4882	-.01219
	(1.801)	(.04358)	(2.227)	(.05407)	(2.137)	(.05037)	(2.088)	(.05106)
Total TANF Benefits	135.1	-5.057	202.7	-10.87	190.4	.926	184.5	8.581
	(814.4)	(17.59)	(993)	(23.86)	(1,005)	(22.41)	(997.4)	(24.25)
Observations		72,500		72,500		72,500		70,500

Notes: Sources are 2005-2016 waves if the American Community Survey, administrative TANF data, and the Census Numident. Sample is households with children born in December or January between December 2004 and January 2016. Coefficients are the bias-corrected discontinuity estimates and associated robust standard errors described in Calonico et al. (2014), estimated on a one month bandwidth around January 1.

A Appendix Figures and Tables

A.1 Figures

Figure A.1: Tests of Smooth Characteristics of Households Around the January 1 Cutoff, All Birth Cohorts

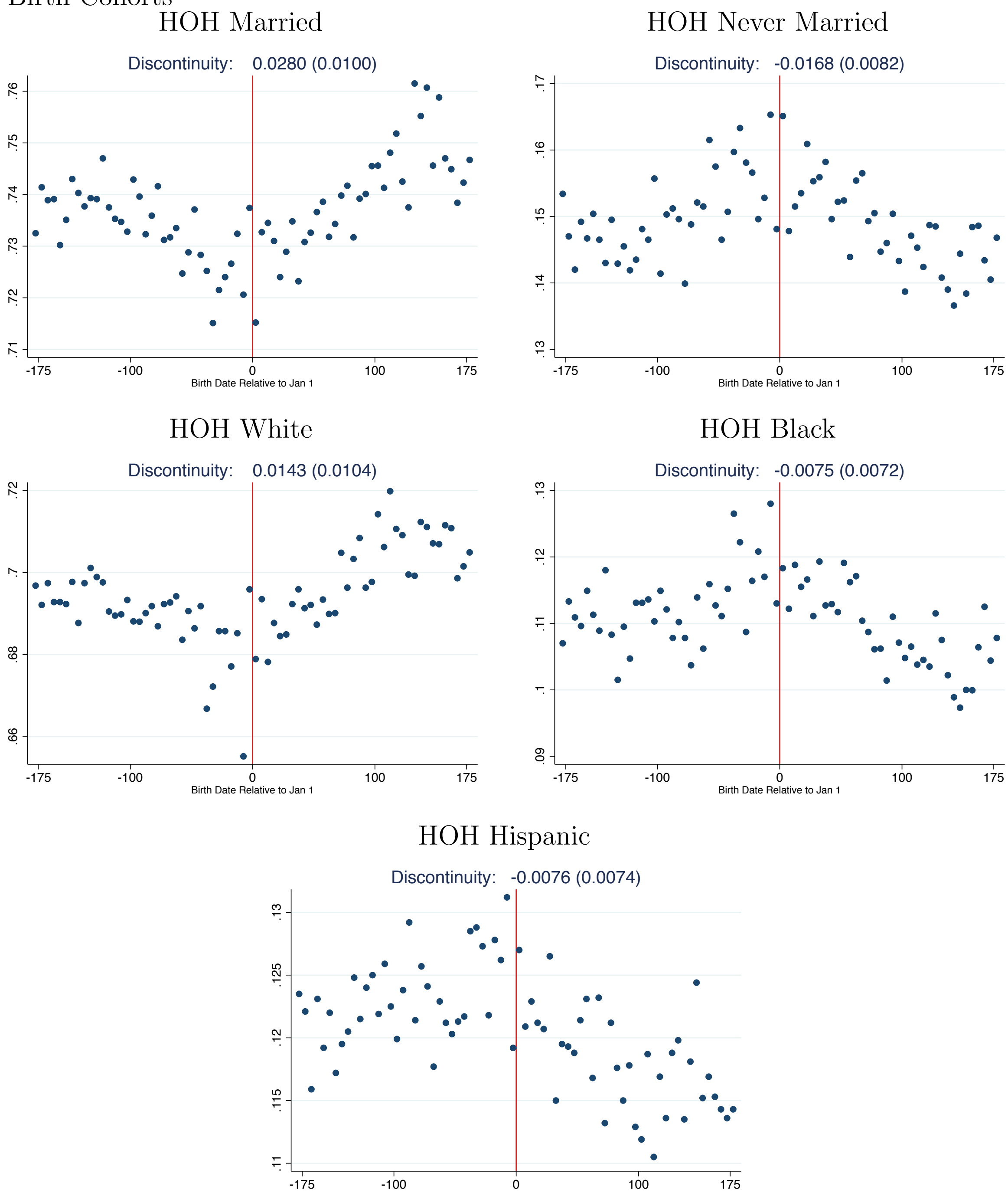


Figure A.2: Tests of Smooth Characteristics of Households Around the January 1 Cutoff, All Birth Cohorts

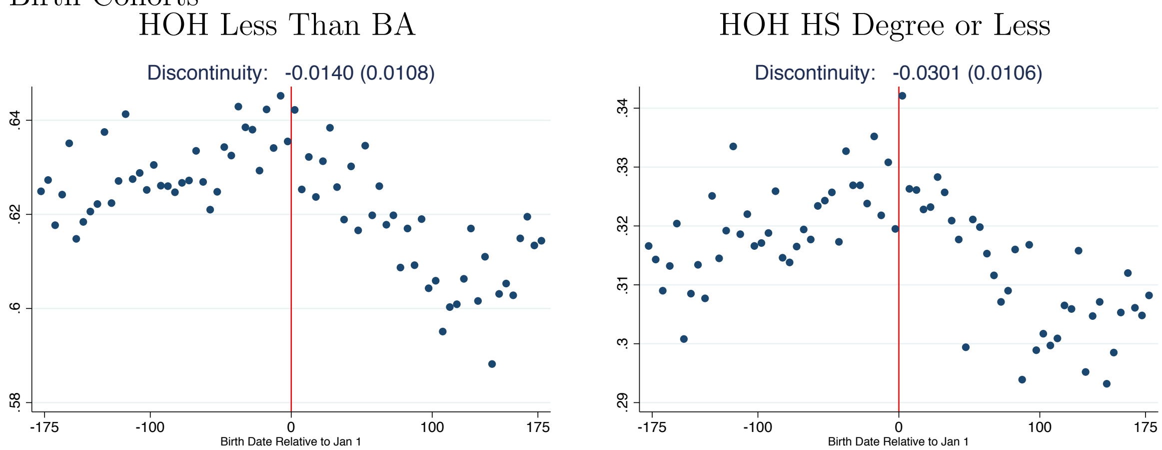
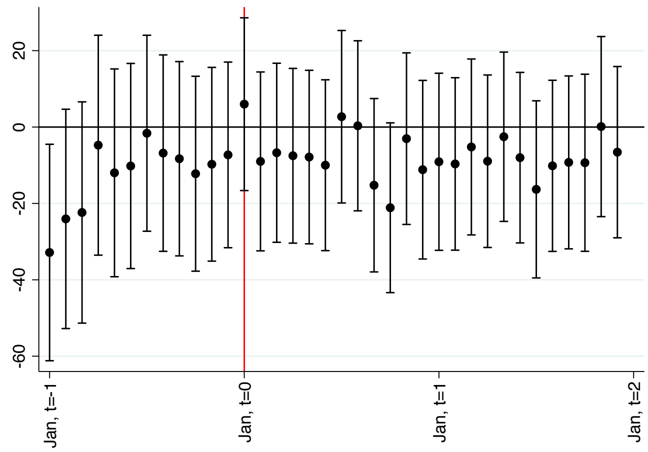
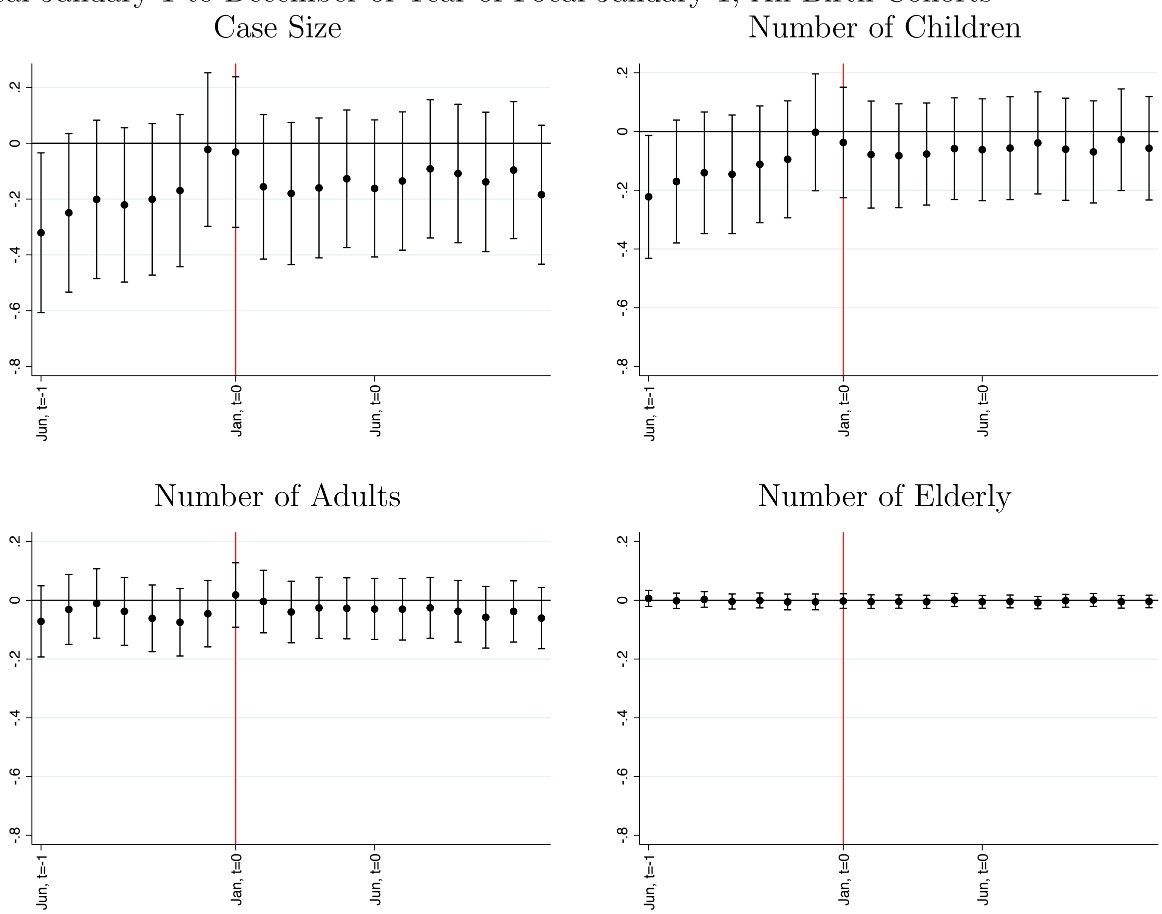


Figure A.3: Estimated Discontinuities in Monthly SNAP Benefits, Conditional on Participation, One Calendar Year Before Birth to Two Calendar Years After Birth, All Birth Cohorts



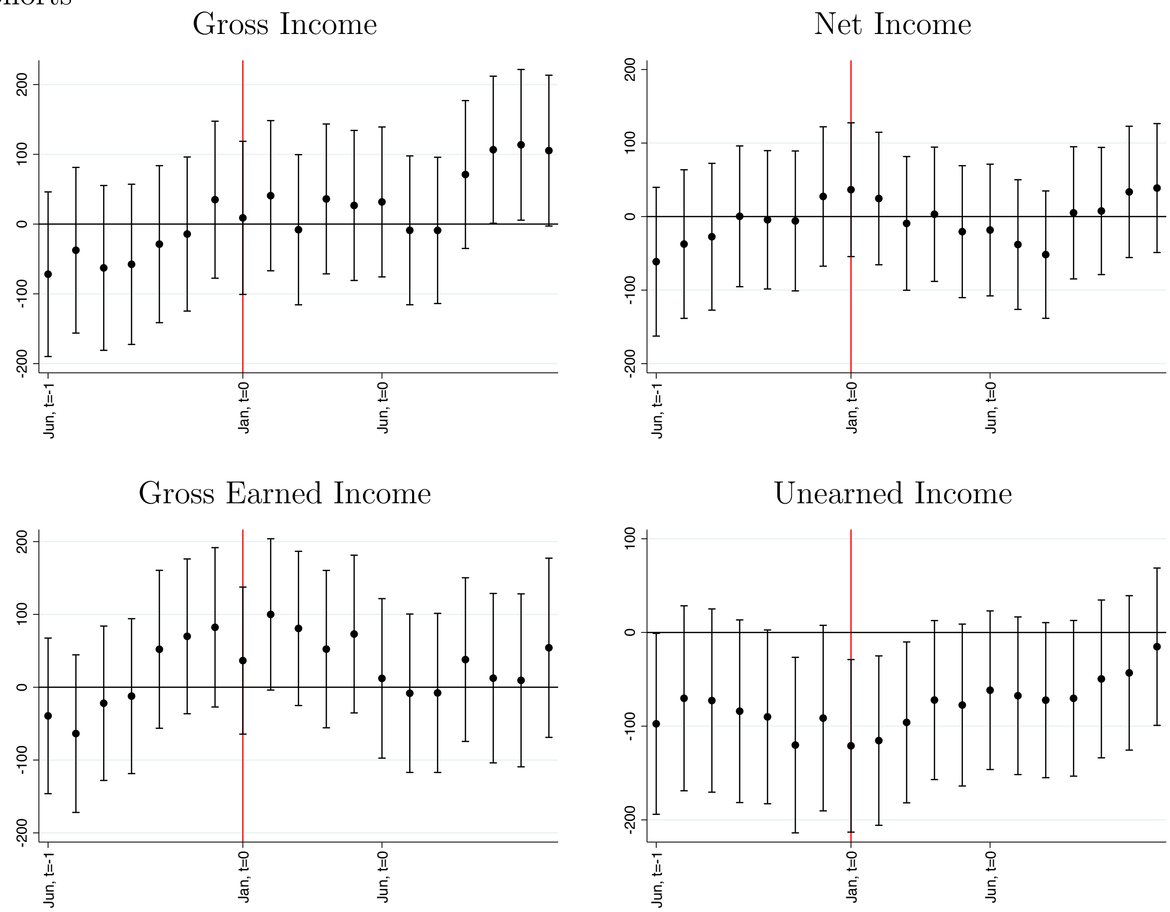
Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure A.4: Estimated Discontinuities in SNAP Case Composition, June of Year Before Focal January 1 to December of Year of Focal January 1, All Birth Cohorts



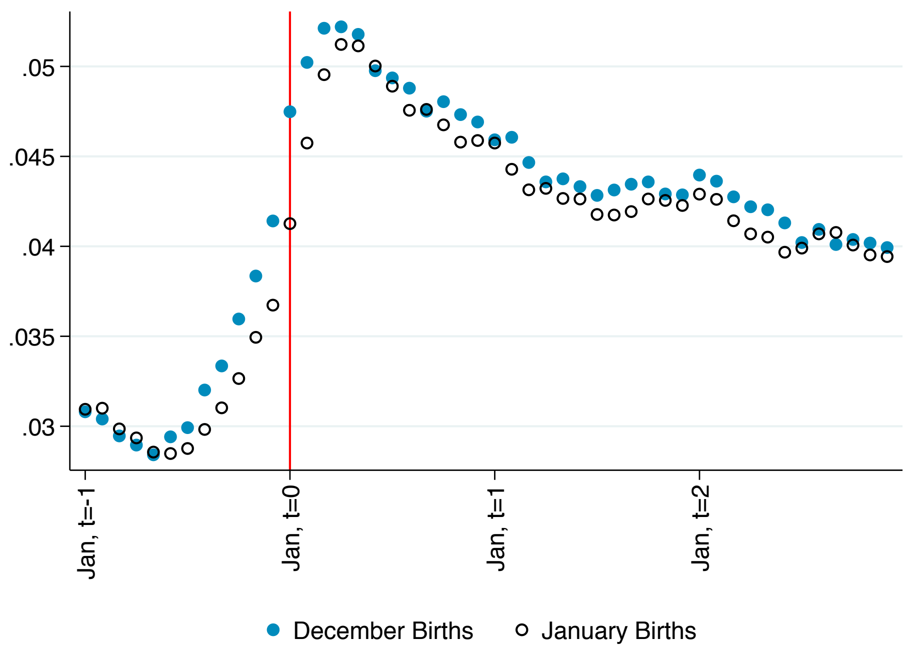
Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure A.5: Estimated Discontinuities in Income Reported in Administrative SNAP Data, June of Year Before Focal January 1 to December of Year of Focal January 1, All Birth Cohorts



Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure A.6: TANF Use Relative to a Child's Birth
Monthly TANF Participation



Monthly Household TANF Benefits

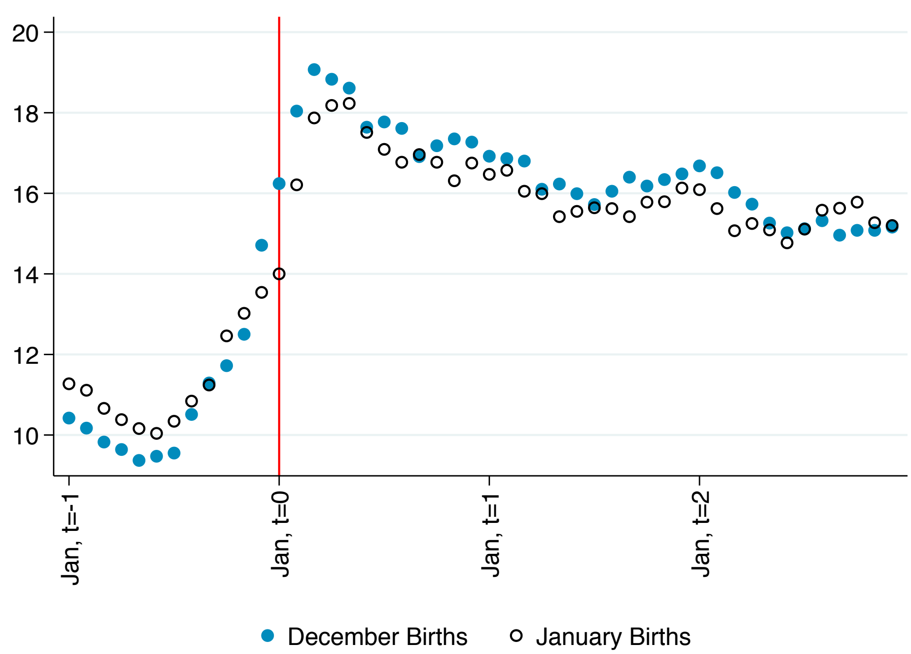


Figure A.7: TANF Participation By Calendar Month, All Birth Cohorts

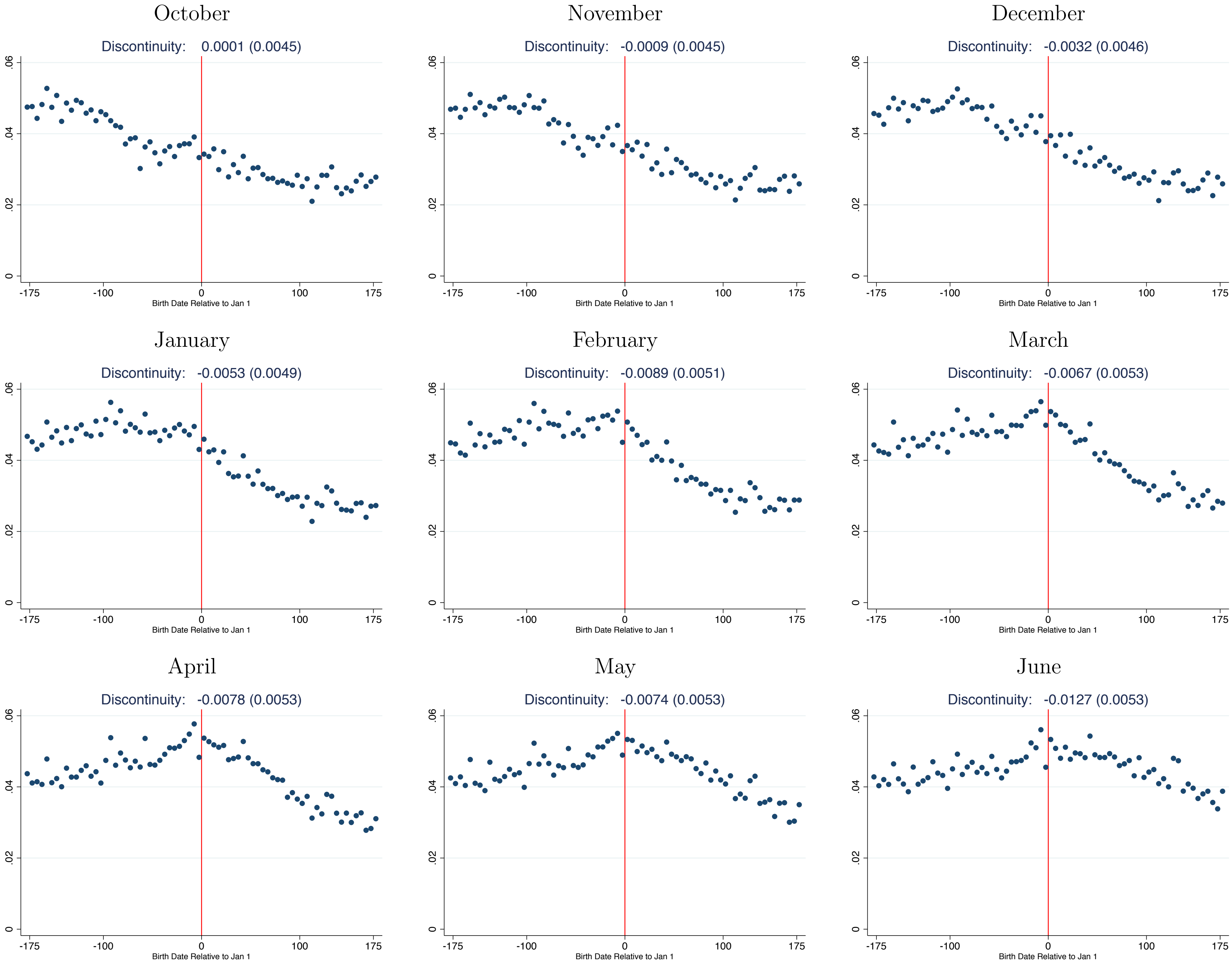


Figure A.8: Household TANF Benefit Amount By Calendar Month, All Birth Cohorts

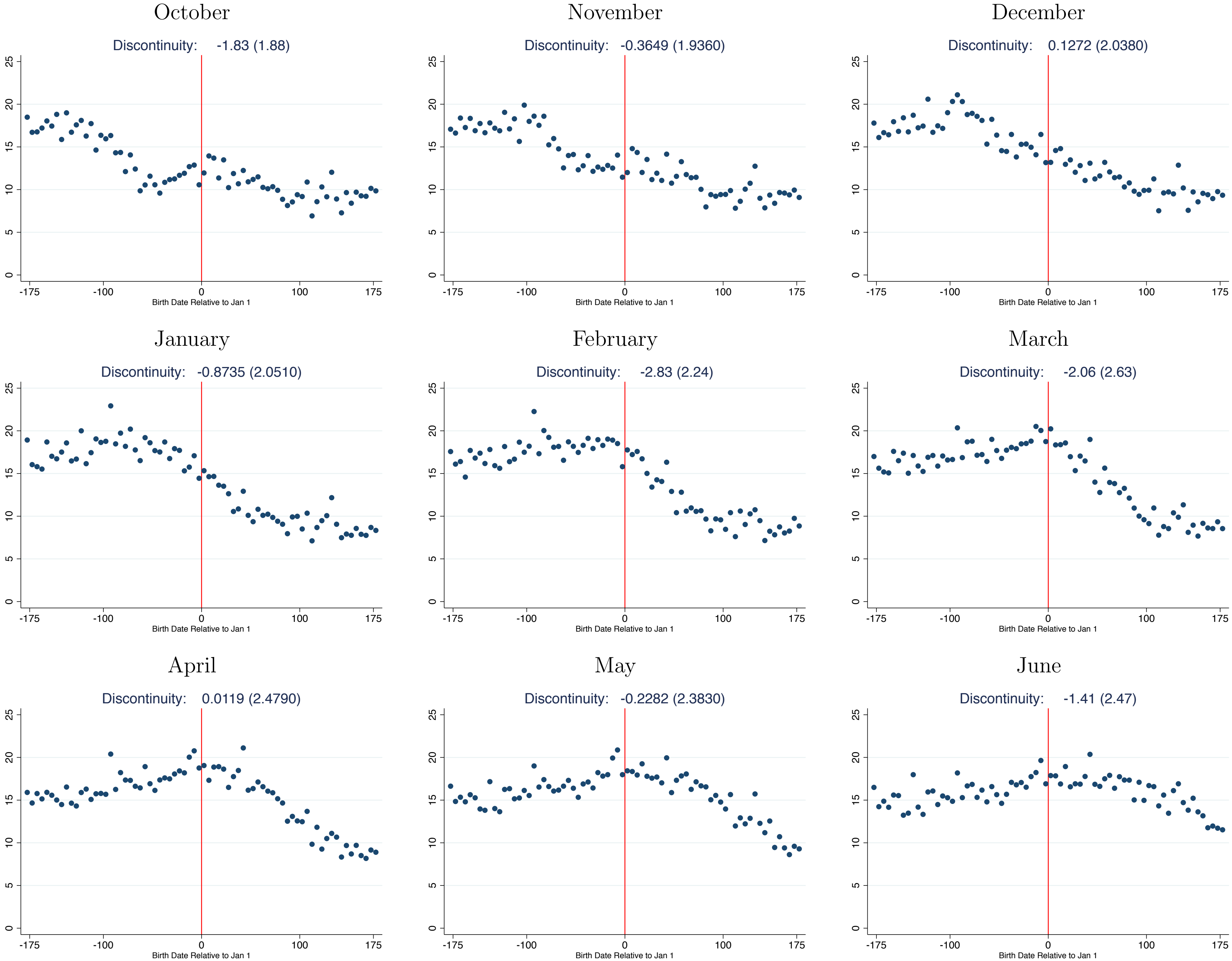
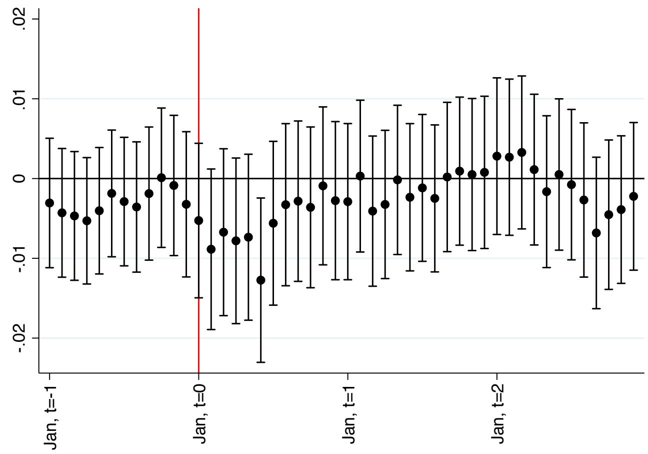
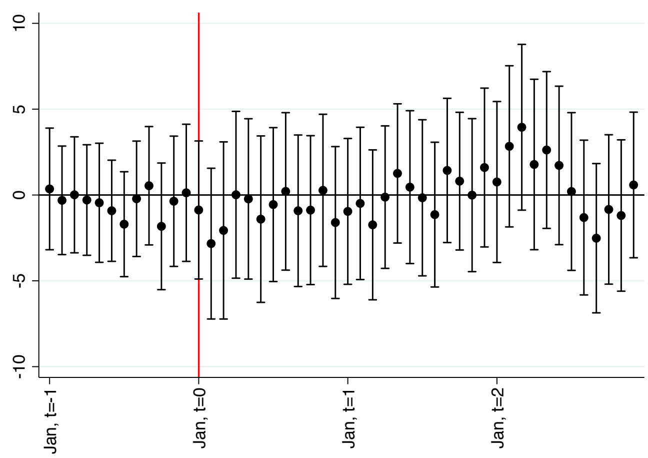


Figure A.9: Estimated Discontinuities in Monthly TANF Use, One Calendar Year Before Birth to Three Calendar Years After Birth, All Birth Cohorts

TANF Participation

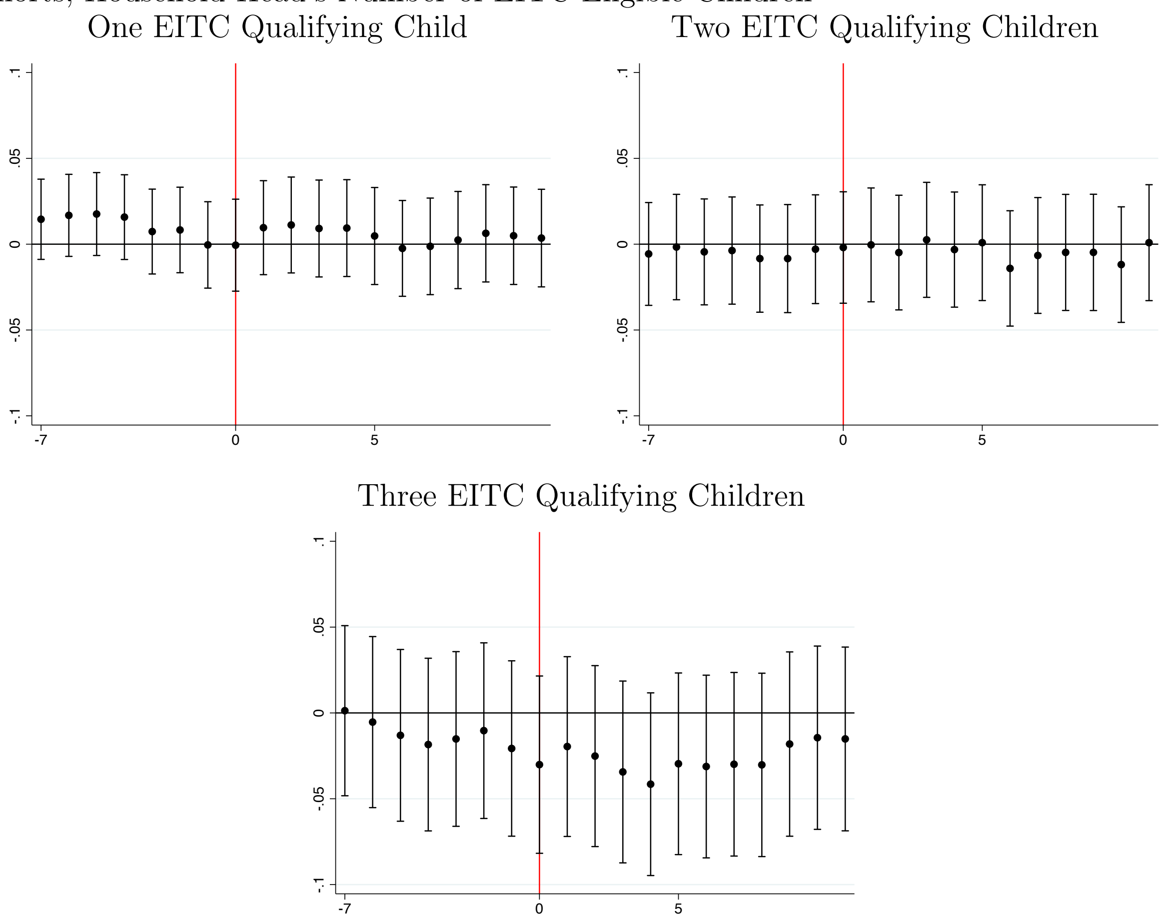


Household TANF Benefit Amount



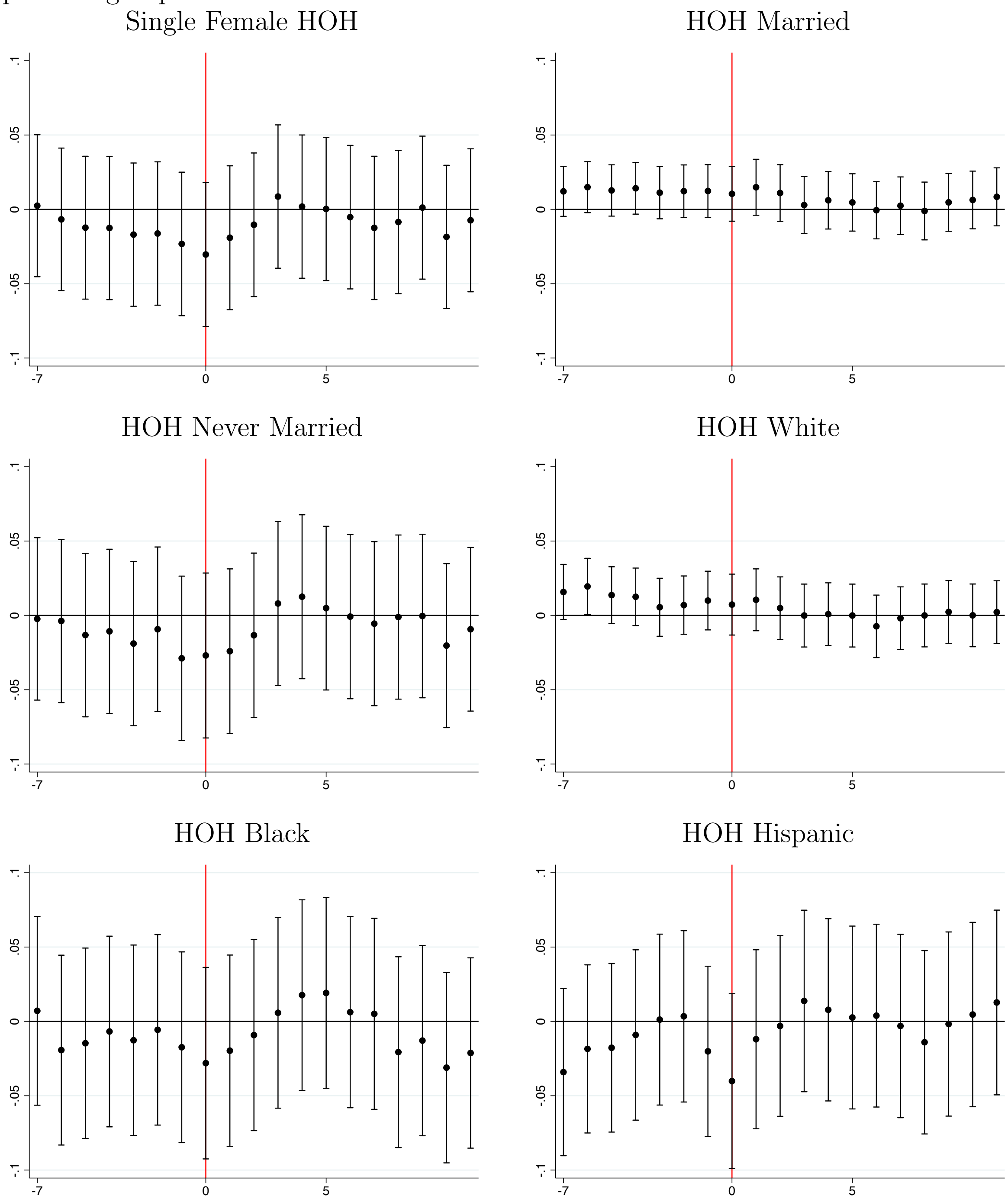
Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure A.10: Heterogeneity: Estimated Discontinuities in Monthly SNAP Participation, June of Year Before Focal January 1 to December of Year of Focal January 1, All Birth Cohorts, Household Head's Number of EITC Eligible Children



Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

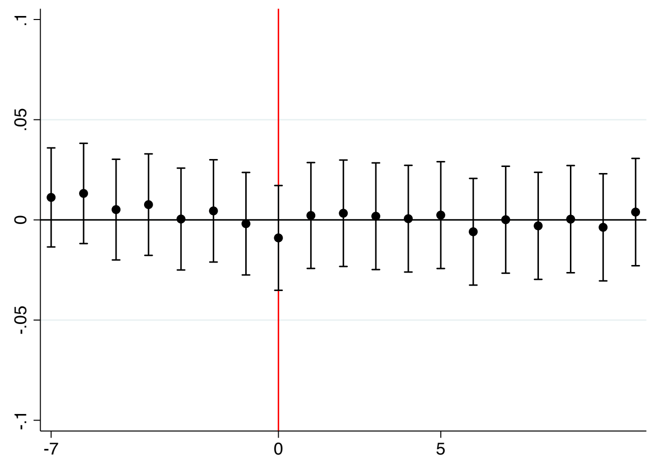
Figure A.11: Heterogeneity: Estimated Discontinuities in Monthly SNAP Participation, June of Year Before Focal January 1 to December of Year of Focal January 1, By Demographic Subgroups



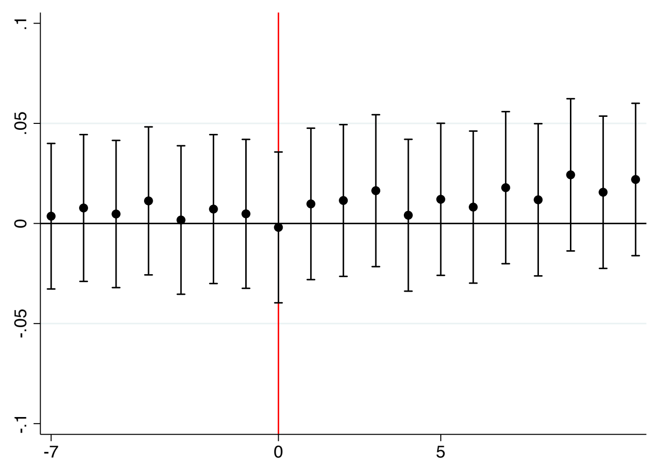
Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure A.12: Heterogeneity: Estimated Discontinuities in Monthly SNAP Participation, June of Year Before Focal January 1 to December of Year of Focal January 1, All Birth Cohorts, By Demographic Subgroups

HOH Less Than BA

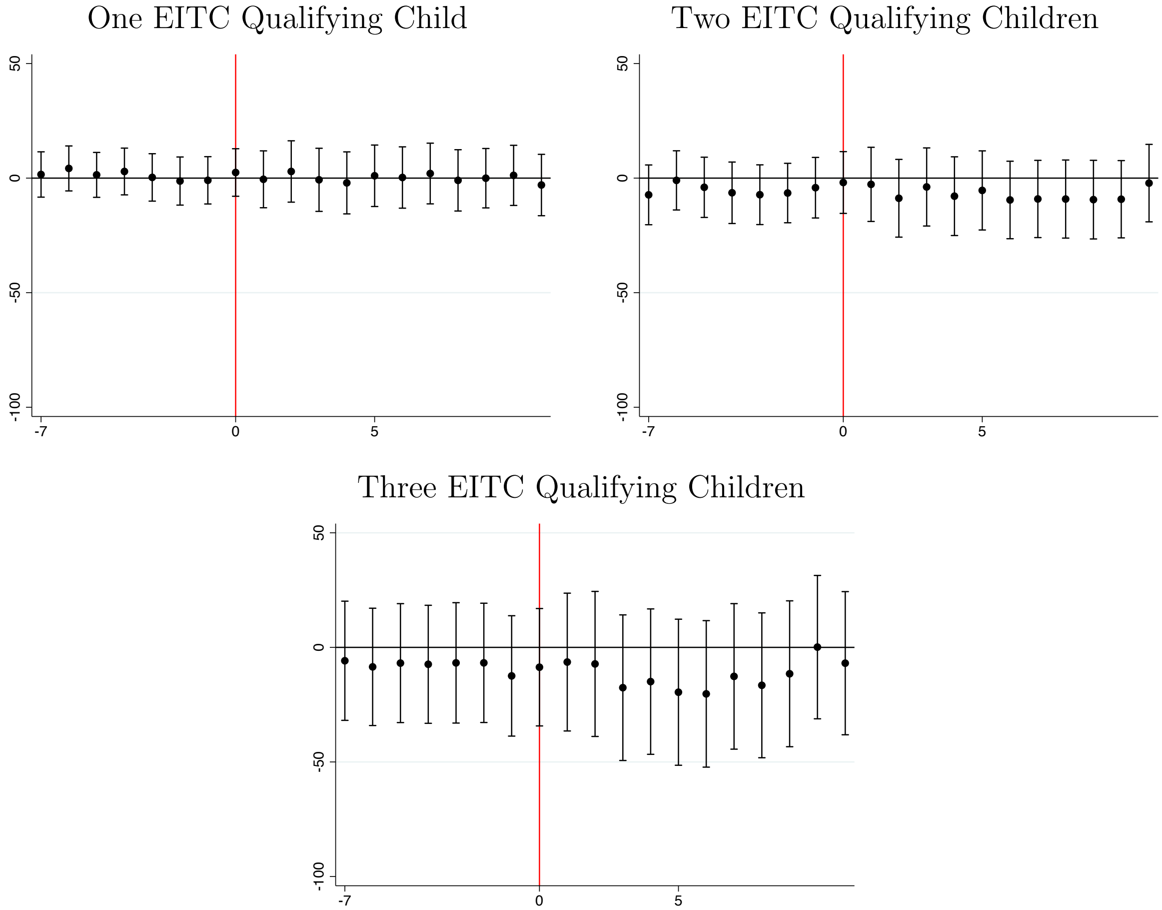


HOH HS or Less



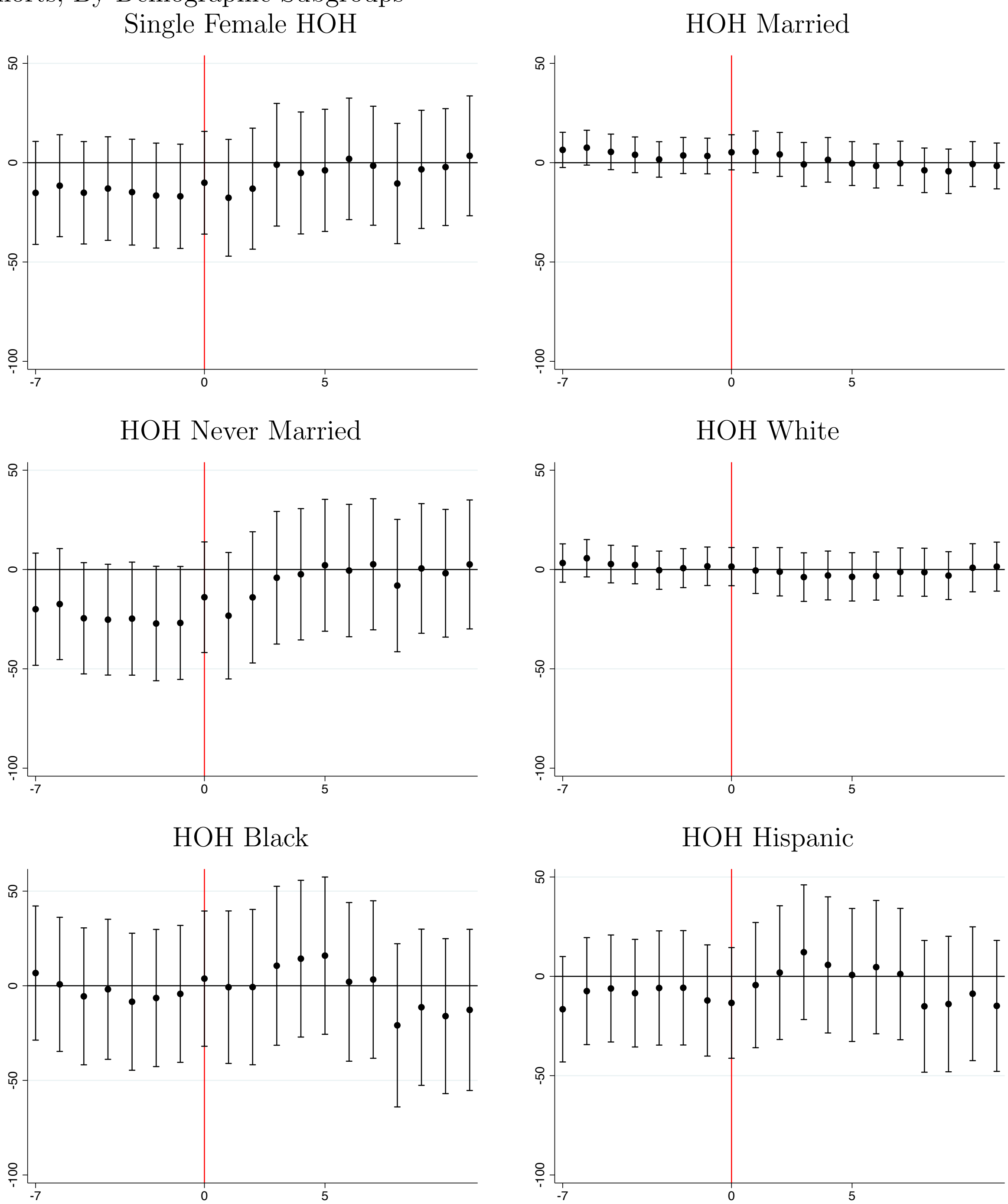
Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure A.13: Heterogeneity: Estimated Discontinuities in Monthly SNAP Benefits Received, June of Year Before Focal January 1 to December of Year of Focal January 1, All Birth Cohorts, Household Head's Number of EITC Eligible Children



Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

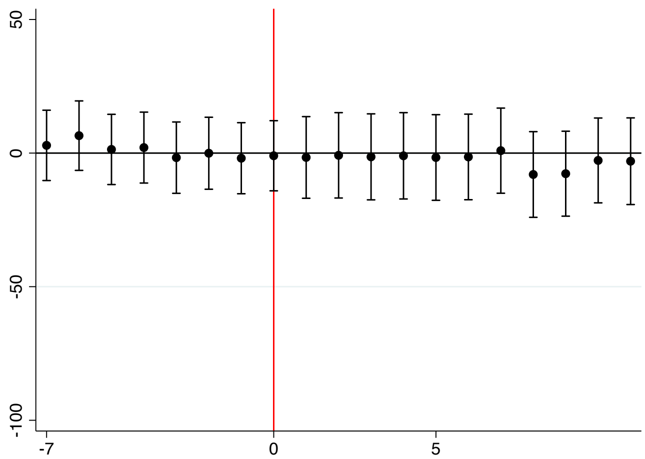
Figure A.14: Heterogeneity: Estimated Discontinuities in Monthly SNAP Benefits Received, June of Year Before Focal January 1 to December of Year of Focal January 1, All Birth Cohorts, By Demographic Subgroups



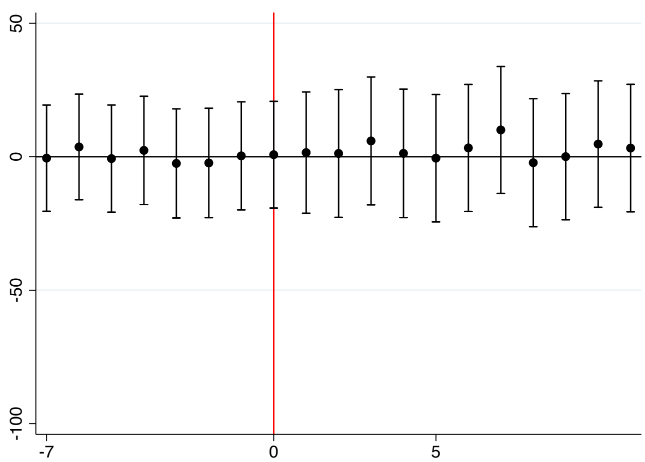
Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

Figure A.15: Heterogeneity: Estimated Discontinuities in Monthly SNAP Benefits Received, June of Year Before Focal January 1 to December of Year of Focal January 1, All Birth Cohorts, By Demographic Subgroups

HOH Less Than BA



HOH HS or Less



Note: Coefficients are from unweighted regressions with no fixed effects. Standard errors are clustered at the ACS household level.

A.2 Tables

Table A.1: Covariate Balance: Discontinuities in Reported Income, Households Interviewed Within One Year of Birth

	January Mean	Discontinuity
Total Wages	81,180	-1,382
	(92,340)	(4,724)
Total Adjusted Gross Income	86,450	-877.9
	(98,710)	(5,128)
Total SSI	309.3	67.76
	(1,972)	(118.5)
Total Welfare Income	232.9	8.713
	(1,423)	(74.03)
Household Poverty Status	.2047	.007547
	(.4035)	(.02194)
Observations		14,500

Notes: Source is 2005-2016 waves if the American Community Survey. Sample is households with children born in December or January between December 2004 and January 2016 who responded to the ACS within one year of their youngest child's birth.

Table A.2: Bandwidth Sensitivity: Estimated Discontinuities in SNAP Participation

	January	CCT	Bandwidth			
Month	Mean	Bandwidth	One Month	CCT	Two Months	10 Day Donut
Calendar Year Before Focal Jan. 1						
Jul	.1928	62.53	.008555	.01024	.01099	.002644
	(.3945)		(.009008)	(.005039)	(.0065)	(.04716)
Aug	.1992	59.92	.003934	.009302	.009242	.02006
	(.3994)		(.00907)	(.005109)	(.006547)	(.04759)
Sep	.2045	56.89	.00457	.009467	.009345	.0334
	(.4033)		(.00916)	(.005294)	(.006609)	(.04803)
Oct	.2094	52.42	-.0001228	.007705	.006682	.03135
	(.4069)		(.009208)	(.005594)	(.006655)	(.04837)
Nov	.2126	42.07	.002003	.009506	.008915	.009477
	(.4092)		(.009255)	(.006325)	(.006694)	(.04856)
Dec	.2159	51.68	-.000875	.005569	.005135	.01407
	(.4114)		(.009289)	(.005856)	(.006726)	(.04904)
Calendar Year of Focal Jan. 1						
Jan	.2272	49.62	-.006015	.00592	.00198	-.000361
	(.419)		(.009578)	(.006028)	(.006919)	(.05033)
Feb	.2388	52.67	.0009508	.006456	.00536	-.003682
	(.4263)		(.009731)	(.006086)	(.007026)	(.05108)
Mar	.2466	52.96	-.0008102	.006217	.004715	-.04295
	(.431)		(.009824)	(.006122)	(.007087)	(.05135)
Apr	.252	46.28	-.001363	.004754	.003494	-.02875
	(.4341)		(.009864)	(.006584)	(.007117)	(.05164)
May	.2534	45.58	.0001662	.005162	.004202	-.03355
	(.435)		(.009881)	(.00665)	(.007125)	(.05161)
Jun	.2549	49.48	-.0007242	.003432	.00263	-.03783
	(.4358)		(.009893)	(.006384)	(.007139)	(.05157)
Jul	.2541	51.02	-.006415	.003103	.0007769	-.01845
	(.4353)		(.009866)	(.006262)	(.007119)	(.05147)
Aug	.2551	45.81	-.003129	.00339	.002643	.009251
	(.4359)		(.009898)	(.006567)	(.007146)	(.05158)
Sep	.2547	46.76	-.003634	.003387	.001668	-.02015
	(.4357)		(.00992)	(.006449)	(.007151)	(.05158)
Oct	.2565	50.27	-.0002977	.00672	.005512	-.004462
	(.4367)		(.009948)	(.006355)	(.007163)	(.05171)
Nov	.2574	51.43	-.002918	.004827	.003396	.01134
	(.4372)		(.009939)	(.00628)	(.007161)	(.0517)
Dec	.2574	47.01	.0004549	.006387	.005452	-.008216
	(.4372)		(.009954)	(.006584)	(.007174)	(.05151)

Table A.3: Bandwidth Sensitivity: Estimated Discontinuities in SNAP Benefit Amount

Month	January Mean	CCT Bandwidth	Bandwidth: One Month	CCT	Two Months	10 Day Donut
Calendar Year Before Focal Jan. 1						
Jul	82.04	65.23	3.175	2.658	2.942	-2.648
	(206.3)		(4.547)	(2.524)	(3.327)	(23.8)
Aug	84.13	53.82	.2211	1.803	1.933	-.9061
	(207.1)		(4.599)	(2.842)	(3.359)	(23.88)
Sep	85.53	45.07	.1193	2.414	2.291	6.887
	(208.2)		(4.639)	(3.198)	(3.398)	(24.08)
Oct	87.83	38.29	-2.681	-.2921	-.4773	5.135
	(211.3)		(4.676)	(3.494)	(3.416)	(24.22)
Nov	88.96	41.3	-1.317	1.428	1.205	6.967
	(212.2)		(4.729)	(3.326)	(3.454)	(24.56)
Dec	89.32	33.85	-1.97	1.604	1.227	3.945
	(210.2)		(4.674)	(3.617)	(3.434)	(24.73)
Calendar Year of Focal Jan. 1						
Jan	85.67	33.74	-.9182	.2966	-.6973	17.36
	(199.9)		(4.636)	(3.307)	(3.408)	(25.59)
Feb	104.7	36.33	-1.823	-.2429	-.7992	17.48
	(229.2)		(5.446)	(3.827)	(3.962)	(28.71)
Mar	120.4	39.42	-2.144	1.234	.9372	-4.706
	(249.9)		(5.714)	(4.159)	(4.15)	(30.19)
Apr	126.8	47.81	-2.655	.9408	-.04433	-6.943
	(257.1)		(5.769)	(3.798)	(4.196)	(30.7)
May	127.6	38.64	-1.996	.7404	.4847	2.754
	(257.5)		(5.8)	(4.271)	(4.203)	(30.32)
Jun	127.9	47.07	-2.993	.3188	-.02795	3.217
	(257.5)		(5.75)	(3.859)	(4.186)	(30.51)
Jul	126.4	51.61	-2.469	1.288	.2701	-13.21
	(255.2)		(5.736)	(3.67)	(4.17)	(30.3)
Aug	126.4	47	-1.443	1.705	1.289	8.098
	(255.1)		(5.705)	(3.82)	(4.148)	(30.37)
Sep	125.8	39.39	-5.844	-2.356	-2.752	-18.84
	(254.7)		(5.749)	(4.044)	(4.16)	(30.32)
Oct	126.4	45.41	-5.823	-.5183	-1.145	-4.767
	(254.8)		(5.703)	(3.837)	(4.136)	(30.37)
Nov	127.2	57.46	-2.242	.02231	-.5375	1.227
	(257.7)		(5.699)	(3.456)	(4.154)	(30.28)
Dec	126.1	46.16	-2.798	.7355	.3675	6.776
	(254.8)		(5.803)	(3.885)	(4.187)	(30.05)

Table A.4: Sensitivity to Specification: Estimated Discontinuities in SNAP Participation

Month	Jan. Mean	(1)	(2)	(3)	(4)	(5)	(6)
Calendar Year Before Focal Jan. 1							
Jul	.1928	.008555	.01452	.01123	.008732	.003444	.003701
	(.3945)	(.009008)	(.008419)	(.006121)	(.006025)	(.01218)	(.01593)
Aug	.1992	.003934	.01181	.009576	.007107	-.005113	-.004044
	(.3994)	(.00907)	(.008478)	(.006164)	(.006072)	(.01226)	(.01602)
Sep	.2045	.00457	.01119	.009502	.006902	-.005249	-.005373
	(.4033)	(.00916)	(.008561)	(.006224)	(.006132)	(.01238)	(.01617)
Oct	.2094	-.0001228	.007698	.006959	.004337	-.01164	-.01534
	(.4069)	(.009208)	(.008615)	(.006267)	(.006173)	(.01243)	(.01623)
Nov	.2126	.002003	.01034	.008821	.00607	-.01133	-.01838
	(.4092)	(.009255)	(.008666)	(.006303)	(.006207)	(.01249)	(.01631)
Dec	.2159	-.000875	.005145	.005505	.002766	-.01168	-.01783
	(.4114)	(.009289)	(.008702)	(.006331)	(.00624)	(.01254)	(.01638)
Calendar Year of Focal Jan. 1							
Jan	.2272	-.006015	-.00159	.002653	-.0004381	-.01516	-.02349
	(.419)	(.009578)	(.008956)	(.006516)	(.006425)	(.01292)	(.01684)
Feb	.2388	.0009508	.003665	.005349	.002379	-.005061	-.009945
	(.4263)	(.009731)	(.009094)	(.006618)	(.006528)	(.01313)	(.01712)
Mar	.2466	-.0008102	-.0008477	.003862	.001023	-.003541	-.009991
	(.431)	(.009824)	(.009175)	(.006676)	(.006588)	(.01327)	(.01732)
Apr	.252	-.001363	.0009028	.003454	.0006235	-.01022	-.02326
	(.4341)	(.009864)	(.009215)	(.006704)	(.006612)	(.01333)	(.01739)
May	.2534	.0001662	.002966	.004199	.001522	-.007673	-.01799
	(.435)	(.009881)	(.009226)	(.006711)	(.006621)	(.01336)	(.01745)
Jun	.2549	-.0007242	.000771	.002465	-.00007705	-.006461	-.02036
	(.4358)	(.009893)	(.009242)	(.006724)	(.00663)	(.01336)	(.01741)
Jul	.2541	-.006415	-.001613	.001356	-.001153	-.01552	-.02673
	(.4353)	(.009866)	(.009219)	(.006706)	(.006618)	(.01332)	(.01736)
Aug	.2551	-.003129	.002316	.003702	.001194	-.01263	-.01951
	(.4359)	(.009898)	(.009252)	(.006731)	(.006644)	(.01336)	(.01743)
Sep	.2547	-.003634	.0006327	.002697	-.00001093	-.01144	-.01794
	(.4357)	(.00992)	(.009262)	(.006736)	(.006651)	(.01341)	(.0175)
Oct	.2565	-.0002977	.003465	.00572	.002992	-.006637	-.008057
	(.4367)	(.009948)	(.00928)	(.006749)	(.006666)	(.01346)	(.01759)
Nov	.2574	-.002918	.0003248	.003847	.001283	-.01161	-.01629
	(.4372)	(.009939)	(.009276)	(.006747)	(.006669)	(.01345)	(.01756)
Dec	.2574	.0004549	.003239	.005685	.003309	-.007708	-.01489
	(.4372)	(.009954)	(.009288)	(.006759)	(.006674)	(.01348)	(.01762)
Kernel		Tri	Uni	Tri	Tri	Tri	Tri
Local Poly		Lin	Lin	Lin	Lin	Quad	Cub
Bias Correction		X	X			X	X
Cohort-State FEs					X		

Table A.5: Sensitivity to Specification: Estimated Discontinuities in SNAP Benefit Amount

Month	Jan. Mean	(1)	(2)	(3)	(4)	(5)	(6)
Calendar Year Before Focal Jan. 1							
Jul	82.04	3.175	5.218	3.063	1.995	2.346	5.07
	(206.3)	(4.547)	(4.288)	(3.134)	(3.085)	(6.079)	(7.854)
Aug	84.13	.2211	3.73	2.154	1.052	-2.895	-1.125
	(207.1)	(4.599)	(4.331)	(3.165)	(3.12)	(6.145)	(7.946)
Sep	85.53	.1193	3.431	2.466	1.322	-4.939	-4.948
	(208.2)	(4.639)	(4.369)	(3.202)	(3.157)	(6.18)	(7.946)
Oct	87.83	-2.681	1.09	-.08809	-1.349	-7.97	-8.944
	(211.3)	(4.676)	(4.4)	(3.219)	(3.173)	(6.215)	(7.955)
Nov	88.96	-1.317	2.434	1.2	-.08357	-7.045	-8.731
	(212.2)	(4.729)	(4.45)	(3.255)	(3.207)	(6.283)	(8.042)
Dec	89.32	-1.97	.7957	.5196	-.8146	-6.398	-6.87
	(210.2)	(4.674)	(4.421)	(3.231)	(3.186)	(6.218)	(7.99)
Calendar Year of Focal Jan. 1							
Jan	85.67	-.9182	1.111	1.568	.1217	-4.531	-4.26
	(199.9)	(4.636)	(4.395)	(3.201)	(3.162)	(6.173)	(7.926)
Feb	104.7	-1.823	.5284	.9811	-.6967	-5.58	-2.488
	(229.2)	(5.446)	(5.11)	(3.737)	(3.684)	(7.254)	(9.331)
Mar	120.4	-2.144	-.7816	.1746	-1.591	-4.351	-1.867
	(249.9)	(5.714)	(5.361)	(3.913)	(3.857)	(7.642)	(9.875)
Apr	126.8	-2.655	.1972	.1755	-1.528	-7.763	-7.686
	(257.1)	(5.769)	(5.416)	(3.955)	(3.893)	(7.727)	(9.996)
May	127.6	-1.996	.7586	.5015	-1.087	-5.283	-1.956
	(257.5)	(5.8)	(5.432)	(3.961)	(3.9)	(7.806)	(10.15)
Jun	127.9	-2.993	-.9142	-.08461	-1.592	-6.464	-6.147
	(257.5)	(5.75)	(5.4)	(3.944)	(3.884)	(7.693)	(9.913)
Jul	126.4	-2.469	.01115	.7357	-.8561	-6.196	-6.92
	(255.2)	(5.736)	(5.381)	(3.929)	(3.869)	(7.689)	(9.945)
Aug	126.4	-1.443	1.786	1.831	.3144	-6.359	-5.706
	(255.1)	(5.705)	(5.353)	(3.907)	(3.85)	(7.659)	(9.884)
Sep	125.8	-5.844	-3.626	-2.078	-3.689	-8.826	-7.216
	(254.7)	(5.749)	(5.373)	(3.919)	(3.863)	(7.757)	(10.05)
Oct	126.4	-5.823	-3.184	-.8884	-2.383	-8.676	-3.452
	(254.8)	(5.703)	(5.339)	(3.895)	(3.84)	(7.676)	(9.936)
Nov	127.2	-2.242	-1.175	-.02713	-1.394	-4.411	-.9131
	(257.7)	(5.699)	(5.354)	(3.915)	(3.863)	(7.665)	(9.927)
Dec	126.1	-2.798	-.1739	.6168	-.7555	-7.013	-4.747
	(254.8)	(5.803)	(5.403)	(3.946)	(3.898)	(7.843)	(10.15)
Kernel		Tri	Uni	Tri	Tri	Tri	Tri
Local Poly		Lin	Lin	Lin	Lin	Quad	Cub
Bias Correction		X	X			X	X
Cohort-State FEs					X		

Table A.6: Sensitivity to Weights: Estimated Discontinuities in SNAP Participation

Month	January Mean	Conventional Unweighted	ACS Household Weights	Weighted by Inverse Pr(Any PIK)	Weighted by Inverse Pr(Youngest Has PIK)
Calendar Year Before Focal Jan. 1					
Jul	.1928	.01123	.01189	.01155	.01186
	(.3945)	(.006121)	(.008299)	(.008435)	(.008812)
Aug	.1992	.009576	.008523	.008333	.008126
	(.3994)	(.006164)	(.008363)	(.008497)	(.008869)
Sep	.2045	.009502	.01179	.01164	.01175
	(.4033)	(.006224)	(.008463)	(.008608)	(.008972)
Oct	.2094	.006959	.01225	.01215	.01249
	(.4069)	(.006267)	(.008532)	(.008672)	(.009046)
Nov	.2126	.008821	.009628	.009452	.009985
	(.4092)	(.006303)	(.008472)	(.008602)	(.008958)
Dec	.2159	.005505	.005095	.005046	.004968
	(.4114)	(.006331)	(.008551)	(.008693)	(.009058)
Calendar Year of Focal Jan. 1					
Jan	.2272	.002653	.001607	.001266	-.00009112
	(.419)	(.006516)	(.008759)	(.008907)	(.009278)
Feb	.2388	.005349	.005375	.005119	.004343
	(.4263)	(.006618)	(.008922)	(.009077)	(.009446)
Mar	.2466	.003862	.003869	.003514	.002692
	(.431)	(.006676)	(.008997)	(.009152)	(.009511)
Apr	.252	.003454	.008005	.008047	.007731
	(.4341)	(.006704)	(.009007)	(.009163)	(.009521)
May	.2534	.004199	.009787	.009917	.01031
	(.435)	(.006711)	(.009008)	(.009164)	(.009521)
Jun	.2549	.002465	.007527	.007708	.008047
	(.4358)	(.006724)	(.009004)	(.009159)	(.009524)
Jul	.2541	.001356	.006942	.007359	.007748
	(.4353)	(.006706)	(.009017)	(.009179)	(.009545)
Aug	.2551	.003702	.004945	.005213	.005532
	(.4359)	(.006731)	(.009002)	(.009163)	(.009533)
Sep	.2547	.002697	.004551	.004623	.004904
	(.4357)	(.006736)	(.009042)	(.009205)	(.009583)
Oct	.2565	.00572	.009436	.009771	.0101
	(.4367)	(.006749)	(.009079)	(.009242)	(.009613)
Nov	.2574	.003847	.003689	.003923	.004065
	(.4372)	(.006747)	(.009044)	(.009207)	(.009582)
Dec	.2574	.005685	.008881	.009182	.009349
	(.4372)	(.006759)	(.00905)	(.00921)	(.009585)

Table A.7: Sensitivity to Weights: Estimated Discontinuities in SNAP Benefit Amount

Month	January Mean	Conventional Unweighted	ACS Household Weights	Weighted by Inverse Pr(Any PIK)	Weighted by Inverse Pr(Youngest Has PIK)
Calendar Year Before Focal Jan. 1					
Jul	82.04	3.063	.7337	.4098	.2481
	(206.3)	(3.134)	(4.212)	(4.245)	(4.582)
Aug	84.13	2.154	-.4422	-.7269	-1.052
	(207.1)	(3.165)	(4.236)	(4.269)	(4.61)
Sep	85.53	2.466	1.205	.9265	.8001
	(208.2)	(3.202)	(4.324)	(4.354)	(4.693)
Oct	87.83	-.08809	.4457	.2147	.1111
	(211.3)	(3.219)	(4.311)	(4.347)	(4.714)
Nov	88.96	1.2	-.4552	-.6748	-.7047
	(212.2)	(3.255)	(4.337)	(4.372)	(4.735)
Dec	89.32	.5196	-2.821	-3.054	-3.565
	(210.2)	(3.231)	(4.319)	(4.36)	(4.724)
Calendar Year of Focal Jan. 1					
Jan	85.67	1.568	-.8954	-1.062	-1.829
	(199.9)	(3.201)	(4.241)	(4.26)	(4.55)
Feb	104.7	.9811	-.4711	-.5137	-.9803
	(229.2)	(3.737)	(4.966)	(4.998)	(5.299)
Mar	120.4	.1746	-1.833	-2.098	-2.909
	(249.9)	(3.913)	(5.206)	(5.245)	(5.571)
Apr	126.8	.1755	2.439	2.501	2.105
	(257.1)	(3.955)	(5.269)	(5.307)	(5.617)
May	127.6	.5015	3.488	3.506	3.532
	(257.5)	(3.961)	(5.217)	(5.256)	(5.556)
Jun	127.9	-.08461	3.183	3.3	3.104
	(257.5)	(3.944)	(5.242)	(5.285)	(5.579)
Jul	126.4	.7357	3.679	3.775	3.572
	(255.2)	(3.929)	(5.242)	(5.29)	(5.611)
Aug	126.4	1.831	2.823	2.907	2.554
	(255.1)	(3.907)	(5.153)	(5.201)	(5.524)
Sep	125.8	-2.078	-.5151	-.5116	-.8426
	(254.7)	(3.919)	(5.207)	(5.247)	(5.583)
Oct	126.4	-.8884	1.391	1.507	1.328
	(254.8)	(3.895)	(5.159)	(5.203)	(5.538)
Nov	127.2	-.02713	.4523	.4161	.1477
	(257.7)	(3.915)	(5.124)	(5.156)	(5.487)
Dec	126.1	.6168	2.23	2.293	1.901
	(254.8)	(3.946)	(5.214)	(5.257)	(5.588)

Table A.8: Predicting Whether Mother is Employed and Temporarily Not Working, 2005 to 2016 ACS Waves

	OLS Results		IV Results	
	Months Elapsed since Birth	Months Interacted with $DecBirth$	Months Elapsed since Birth	Months Interacted with $TaxValue$
Month 1	0.502***	-0.025	0.496***	-0.007
	(0.019)	(0.018)	(0.019)	(0.009)
Month 2	0.377***	0.007	0.374***	0.007
	(0.021)	(0.020)	(0.020)	(0.012)
Month 3	0.138***	0.021	0.145***	0.005
	(0.024)	(0.024)	(0.025)	(0.012)
Month 4	0.063***	0.007	0.068***	-0.002
	(0.014)	(0.019)	(0.015)	(0.010)
Month 5	0.039***	-0.019	0.040***	-0.012
	(0.008)	(0.013)	(0.009)	(0.008)
Month 6	0.039***	-0.016	0.039***	-0.009
	(0.008)	(0.015)	(0.008)	(0.008)
Month 7-12		0.006		0.003
		(0.009)		(0.005)
Observations		20,000		20,000

Notes: Sample is women interviewed in the ACS 2005 to 2016 waves within one year of giving birth who are employed, between the ages of 20 and 40, gave birth between December 18th and January 14th, and worked within the past five years. Columns 1 and 2 report unweighted OLS coefficients and standard errors. Columns 3 and 4 report second-stage results from an IV regression. Standard errors are clustered at the state level. Each regression also controls for maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own children under age 19 in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, and dummies for being white, having some college education, having completed a college degree, and being married. *** Significant at the 1% level. ** Significant at the 5% level * Significant at the 10% level.

Table A.9: Predicting Whether Mother is Currently Employed and Working, 2001 to 2008 ACS Waves

	OLS Results		IV Results	
	Months Elapsed since Birth	Months Interacted with $DecBirth$	Months Elapsed since Birth	Months Interacted with $TaxValue$
Month 1	-0.331***	0.023	-0.332***	0.010
	(0.036)	(0.032)	(0.035)	(0.020)
Month 2	-0.240***	0.026	-0.241***	0.012
	(0.037)	(0.035)	(0.034)	(0.020)
Month 3	-0.071	-0.009	-0.071*	-0.010
	(0.045)	(0.032)	(0.042)	(0.019)
Month 4	-0.042	0.008	-0.050	0.009
	(0.041)	(0.029)	(0.038)	(0.019)
Month 5	0.024	0.006	0.016	0.008
	(0.038)	(0.029)	(0.036)	(0.020)
Month 6	0.012	0.018	0.015	0.002
	(0.028)	(0.033)	(0.028)	(0.019)
Month 7	-0.013	0.007	-0.019	0.008
	(0.034)	(0.034)	(0.032)	(0.021)
Month 8	0.021	-0.027	0.015	-0.013
	(0.031)	(0.031)	(0.029)	(0.016)
Month 9	0.023	0.033	0.018	0.021
	(0.031)	(0.029)	(0.030)	(0.017)
Month 10	0.049	-0.003	0.040	0.004
	(0.041)	(0.028)	(0.040)	(0.019)
Month 11	0.017	0.053	0.011	0.035
	(0.034)	(0.036)	(0.033)	(0.023)
Month 12		0.034		0.016
		(0.049)		(0.028)
Observations		13,500		13,500

Notes: Sample is women interviewed in the ACS 2001 to 2008 waves within one year of giving birth who are between the ages of 20 and 40, gave birth between December 18th and January 14th, and worked within the past five years. Columns 1 and 2 report unweighted OLS coefficients and standard errors. Columns 3 and 4 report second-stage results from an IV regression. Standard errors are clustered at the state level. Each regression also controls for maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own children under age 19 in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, and dummies for being white, having some college education, having completed a college degree, and being married. *** Significant at the 1% level. ** Significant at the 5% level * Significant at the 10% level.

Table A.10: Predicting Whether Mother is Employed and Temporarily Not Working, 2001 to 2008 ACS Waves

	OLS Results		IV Results	
	Months Elapsed since Birth	Months Interacted with $DecBirth$	Months Elapsed since Birth	Months Interacted with $TaxValue$
Month 1	0.582***	-0.014	0.579***	-0.006
	(0.027)	(0.040)	(0.028)	(0.022)
Month 2	0.412***	-0.004	0.409***	0.000
	(0.034)	(0.040)	(0.033)	(0.023)
Month 3	0.116***	0.019	0.112***	0.014
	(0.020)	(0.033)	(0.020)	(0.019)
Month 4	0.053**	0.050*	0.060**	0.021
	(0.024)	(0.027)	(0.026)	(0.014)
Month 5	0.018*	-0.006	0.017*	-0.002
	(0.009)	(0.020)	(0.010)	(0.011)
Month 6	0.023**	0.015	0.023**	0.008
	(0.010)	(0.019)	(0.010)	(0.011)
Month 7-12		0.018		0.010
		(0.016)		(0.009)
Observations		8,100		8,100

Notes: Sample is women interviewed in the ACS 2001 to 2008 waves within one year of giving birth who are employed, between the ages of 20 and 40, gave birth between December 18th and January 14th, and worked within the past five years. Columns 1 and 2 report unweighted OLS coefficients and standard errors. Columns 3 and 4 report second-stage results from an IV regression. Standard errors are clustered at the state level. Each regression also controls for maternal age and age squared, income earned by a male spouse or partner (set equal to zero if there is no male partner), the number of own children under age 19 in the household, state fixed effects, year fixed effects (where a year is defined as an adjacent December/January pair), the number of days elapsed between December 1 and the birth, day-of-week dummies for the date of birth, and dummies for being white, having some college education, having completed a college degree, and being married. *** Significant at the 1% level. ** Significant at the 5% level * Significant at the 10% level.

B Predicting the Effect of Income on Participation in the Welfare Stigma Model

In this section, we discuss conditions under which the expected response to an increase in income would be an increase, decrease, or no change in participation.

First note that for the expected effect to be nonzero, there must be some population of individuals on the margin of participation. Otherwise, the effect of income on participation is zero regardless of any other factors. If there are people at the margin of participation, we then need to consider the relative sizes of the marginal utility of consumption when participating and not participating. To arrive at clearer predictions, we make some simplifying but reasonable assumptions.

We first assume that utility is at least weakly concave in consumption: $\frac{\partial^2 u}{\partial c^2} \leq 0$. If we further assume that labor supply does not affect the marginal utility of consumption, so $\frac{\partial^2 c}{\partial c \partial h} = 0$, then the expected effect of income on participation simplifies down to a question of how consumption when participating differs from consumption when not participating.[40] If consumption when participating is higher (lower) when participating than when not participating, the effect of income on participation will be negative (positive). If consumption is the same in either state or if utility is linear in consumption, then the effect of income on participation will be zero.

How do we expect consumption when participating to compare to consumption when not participating? Consumption when participating will exceed consumption when not participating when income from the benefit program does not fully crowd out labor income. Previous literature and the institutional context suggest that less than complete crowd out is likely. Many studies find that food stamps do not have large work disincentive effects (Moffitt, 2002), and TANF's strong work requirements likely dampen participating individ-

[40] The assumption that labor supply does not affect the marginal utility of consumption rules out the possibility that the act of working more *ceteris paribus* decreases the marginal utility of consumption. This assumption would be satisfied if utility is additively separable in consumption and labor.

uals' ability to decrease their labor supply. In our setting, it is also important to note that low-income workers generally have less control over their hours of work. Thus, we conclude that income will either have a negative or null effect on participation.[41]

C Wingender & LaLumia (2017) Replication Analysis

Wingender & LaLumia (2017) estimate the following model:

$$Y_i = \sum_{k=1}^{T-1} \alpha_k (MonthsElapsed = k_i) + \sum_{k=1}^{T} \beta_k (MonthsElapsed = k_i \times DecBirth_i) + \gamma X_i + \varepsilon_i$$

where Y_i is a measure of labor supply and $MonthsElapsed = k_i$ is an indicator for the mother being interviewed k_i months after giving birth. The α_k terms account for the fact that women are less likely to be working soon after giving birth and then gradually return to work. The β_k terms capture how the labor supply of mothers with December births differ from those with January births. The authors also estimate instrumental variables regressions where $DecBirth_i$ is replaced in the equation above with the predicted tax savings from having a December birth: $\widehat{TaxValue}_i$, defined as the fitted values from $TaxValue_i = \delta DecBirth_i + \lambda X_i + \nu_i$. The X_i matrix includes controls for demographic characteristics.[42]

The two dependent variables the authors consider are an indicator for a woman being employed and working and an indicator for a woman being employed and temporarily not working. For the temporarily not working analysis, the sample is limited to women who are employed. Wingender & LaLumia (2017) find evidence that women with December births have significantly lower employment rates than women with January births in the third, fifth, and eighth months following birth and are more likely to be temporarily not working in the

[41]The prediction of a negative effect will also hold if the assumption that labor supply does not affect the marginal utility of consumption is loosened to allow work to increase the marginal utility of consumption. Work could increase the marginal utility of consumption if, for example, working more reduces leisure time and thereby increases the utility of time-saving consumption goods like transportation and meals away from home.

[42]More details of the sample and control variables are in the table notes for our Tables 5, A.8, A.9, and A.10.

third and fifth months following birth. The third and fifth month following a December birth roughly line up with the tax filing season and thus, the results are consistent with women decreasing their labor supply in response to receiving tax benefits.

We present our estimates of the Wingender & LaLumia (2017) models in Tables 5, A.8, A.9, and A.10. We do not have access to the same set of ACS waves as Wingender & LaLumia (2017), but we first try to replicate their model on a similar set of years, 2001 to 2008. These results are shown in Tables A.9 and A.10. Although our estimated coefficients in most months are similar to those in Wingender & LaLumia (2017), we do not estimate significant differences between the employment of mothers with December and January births. We do estimate that mothers with December births are slightly more likely to be temporarily away from work five months after giving birth, but the general conclusion from our replication attempt is that we do not find similar decreases in labor supply as a result of receiving tax benefits.