Three Essays on the Impact of Government Assistance Programs on Economic Behaviors of Vulnerable Households

by

Jiyoon Kim

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in the University of Michigan
2015

Doctoral Committee:

Associate Professor Martha J. Bailey, Co-Chair
Professor Edward C. Norton, Co-Chair
Professor Charles C. Brown
Associate Professor Luke Shaefer
Professor Jeffrey A. Smith

Acknowledgements

I am very grateful to my dear advisors, Martha Bailey, Edward Norton, Charlie Brown, Jeff Smith and Luke Shaefer for their invaluable time, advice, and support. I learned from their intellectual enthusiasm, energy, insights, as well as humanism. They have encouraged me and guided me through whenever I am lost during a long journey toward Ph.D. I also appreciate Sheldon Danziger for introducing me to the world of poverty. He is the first person who stimulated my curiosity and awakened intellectual passion to study further into this area. I am also grateful to all the other faculty members, researchers, and friends at University of Michigan and other institutions, who made this thesis possible and made my days more joyful and delightful.

I would like to thank my family who believed in me no matter what, and continuously show unconditional love, care, and support from long distance.

I am blessed and thankful to be with Yeonil for the last year of my Ph.D. His love, prayer, encouragement, belief in me made me stronger and stand firm in faith. I look forward to our lives that we will be walking together with common vision and dream.

Lastly but most importantly, I bring all the glory to God, who is the First and the Last, the Beginning and the End. He lifted me up whenever I was about to give up and has been my comfort and shelter. I praise God for his faithfulness to me. Nothing compares to the promise I have in Him.

Contents

List of Tables

List of Figures

Abstract

Introduction

The overarching theme of the dissertation is to examine the impacts of government assistance programs on the economic behaviors of disadvantaged groups such as low-income or single-headed households in the U.S. It is crucial to expand our understanding of how government assistance has helped them to overcome economic barriers to labor force participation or to expand household resources. My dissertation chapters primarily focus on the two largest government safety net programs: Temporary Assistance for Needy Families (TANF) and the Supplemental Nutrition Assistance Program (SNAP). I show that public assistance programs play a pivotal role by interacting with economic choices made by vulnerable households, such as labor supply, health, or household expenditures. Exploiting the variation in specific aspects of welfare program or the changes made to program parameters, I study how the policies have altered the life circumstances and opportunities faced by disadvantaged households.

Chapter 1. Exempt from Welfare Work Requirements? Opposing Effects Before and After Childbirth on Mothers' Work

Beginning in 1996, welfare recipients were required to work in order to receive benefits. Welfare recipients, however, were exempt from work requirements if they are new mothers with infants or at-risk pregnancy. The goal of this paper is to quantify the effects of welfare work exemptions on women's labor force participation. I examine the *pregnancy* exemption and the *age of youngest child (AYC)* exemption, which account for 90 percent of all exemption cases. Considerable between-state and within-state variation

in program rules adopted after 1996 allows me to estimate a triple-difference in the event study — 24 months before and after the event of childbirth. High labor force participation is detected in states with stricter exemption rules. When labor force participation is decomposed into employment and unemployment, however, I find opposing effects by the timing of the exemption relative to birth: mothers who are granted no exemption during pregnancy show more employment and lower welfare dependency after birth. In contrast, short exemptions after birth impose a burden on mothers with infants, resulting in more unemployment and higher welfare dependency. Although simple theory implies mothers would reduce working hours when they are exempt from work requirements, my paper suggests that the effect of work exemptions matters differently by its timing, because mothers face substantially distinct costs of working before and after childbirth.

Chapter 2. Changes in Low-income Households' Spending Patterns in Response to the 2009 SNAP Benefits Increase

In direct response to the Great Recession, Congress passed the American Recovery and Reinvestment Act (ARRA) in 2009. The ARRA included a 14 percent increase in Supplemental Nutrition Assistance Program (SNAP) benefits, which was the largest one-time increase since the program began. In this study, I focus on this plausibly exogenous jump in SNAP benefits to quantify its impact on participants' consumption patterns in the aftermath of the Great Recession. Using data from the Consumer Expenditure Survey, I find a substantial increase in the total expenditures of SNAP household after the SNAP benefit increase compared to those of other low-income non-SNAP households. Increases in total spending are strongly driven by the food and housing categories. Consistent with the theoretical framework, the increased amount in food expenditures for SNAP households is slightly less than the increased amount of SNAP benefit. Instead, I find that SNAP households redirect their expanded resources to other non-food categories. This paper makes two contributions in that it studies the most recent policy change and it investigates all spending categories beyond food expenditures in response to the SNAP benefit increase. Given that consumption is generally favored

over income as a measure of the well-being of the poor (Meyer and Sullivan, 2008), understanding household's consumption response in every spending category to an increase in in-kind transfers helps assess the overall welfare effect of the policy.

Chapter 3. Are Household Food Expenditures Responsive to Entry onto the Supplemental Nutrition Assistance Program?

Direct evidence of SNAP's ameliorative effect should be apparent in the food expenditures of these households, pre- and post-SNAP entry. Do food expenditures rise, fall, or remain stable in the months following program entry? Do SNAP dollars replace or supplement out-of-pocket spending? What other changes happen to a family around the time of SNAP entry? This paper delves into these questions by adopting an event-study framework to draw a month-to-month trend in food expenditures before and after SNAP entry. We use the Panel Study of Income Dynamics (PSID), the only panel dataset with measures of both household food expenditures and SNAP participation by month. Taking advantage of the PSID's panel structure, we create an analysis sample identified by the month relative to the SNAP entry. We then use these observations to track trends in household food expenditures for the twelve months prior to and the twelve months following SNAP entry. Our findings suggest that the SNAP shields new entrants from a substantial fall in food expenditures. Even after controlling for demographic characteristics as well as time and state fixed effects, our event-study estimation reveals that total food expenditures show a stable and smooth trend in the months surrounding SNAP entry. This is found to be largely due to SNAP replacing a great share of households' food budget after program entry. We also demonstrate that there are household shocks that co-occur with SNAP entry, such as new spells of unemployment or marital dissolution, suggesting that these shocks may trigger SNAP entry.

Chapter 1

Exempt from Welfare Work Requirements? Opposing Effects Before and After Childbirth on Mothers' Work

1.1 Introduction

Over the past few decades, one striking trend has drawn people's attention: increased labor force participation of mothers with young children. Figure 1.1 illustrates that labor force participation has been steadily rising for all women with young children, but a drastic jump for single mothers is noticeable, exceeding the corresponding rate of married mothers after 1996. This period coincides with the initiation of welfare reform, when the 60-year-old government cash assistance program underwent a major transformation.

One of the most radical changes made in the 1996 welfare reform is that people are now subject to mandatory work requirements to receive welfare benefits. Only three out of five recipients, however, are required to work (Table 1.1, column 2). Thirty percent of recipients are given exemptions (Table 1.1, column 3-4), the majority of which are granted during pregnancy or immediately after giving birth.[1] Despite a sizable portion

[1] Ybarra (2014) used 2006 administrative data from Wisconsin and found almost half of program entrants are new mothers with infants or at-risk pregnancy, who are not subject to work requirements. 42% of new mothers were pregnant at the time of application, and the rest applied for benefits shortly after giving birth.

of recipients exempt from work, little is known about the effect of these work exemptions provided directly around birth, when a mother's tradeoff between market work and home production is most pronounced.

Theoretical model suggests that mothers who are exempt from work requirements would reduce working hours accordingly. But responses could substantially vary by *when* they are exempt and *how short/long* the exemptions are. Furthermore, exemptions influence not only simple "work or not" decision, but also stability of employment and future welfare dependency.

Hence, the goal of this paper is to examine the effects of welfare work exemptions on women's work and welfare behaviors before and after childbirth. My analysis focuses on two exemption policies: the pregnancy exemption and the age of youngest child (AYC) exemption. These make up 90 percent of all exemption cases. Exploiting considerable across-state and within-state variation in the length of exemption, I run a triple-difference in the event study. The three differences are pre- and post-exemption policy change (variation within state), strict and lenient states (variation between states), and the marital status of mothers (single and married).

A sizable increase in labor force participation is detected in states with tighter exemption rules. I find that a mother not granted an exemption during pregnancy increases her labor force participation by 10 percentage points relative to a mother granted exemption during pregnancy. An AYC exemption shorter than 12 months generates an 11 percentage point drop in labor force participation right before birth, but eventually leads to a 13 percentage point higher labor force participation beginning four months after birth, compared to a mother granted an exemption longer than 12 months after birth.

It is worth noting that a broad range of work-related activities, such as community service or vocational training, also satisfy work requirements. Consequently, looking at an aggregate pattern in labor force participation could mask to what extent the shift is due to actual employment. One possible interpretation could be that the increase in labor force participation in states with limited exemptions is mainly driven by more women searching for work and being unemployed while still being counted in the labor force.

Therefore, I decompose the aggregate labor force participation effects into changes in employment and unemployment. I find that the increase in labor force participation in response to the absence of exemptions during pregnancy is entirely driven by stable employment. In contrast, limited AYC exemptions are associated with greater unemployment among mothers with infants. It indicates that strict pregnancy exemptions successfully increase the number of women *employed*, while strict AYC exemptions raise *unemployment*. This finding adds value to the existing literature by suggesting opposing effects of work exemptions immediately before and after childbirth, and points toward the conclusion that the timing of mandatory work matters substantially to mothers.

I next investigate whether increases in work eventually reduce welfare dependency. Would tighter exemption policies lead more women to leave welfare sooner than would more liberal exemptions? I find a stark difference in monthly welfare receipt trends between the two exemptions. The proportion of single mothers on welfare falls notably after childbirth in states that provide no pregnancy exemption. This implies an accelerated welfare exit enabled by higher labor force attachment in the pre-birth period. Conversely, single mothers with an AYC exemption shorter than 12 months show higher dependency on welfare. Collectively, these findings suggest that mothers face markedly different costs of working before and after childbirth, resulting in opposite behavioral responses to strict work exemptions.

To my knowledge, five published papers examine the AYC exemption (Hofferth et al. 2002, 2005; Washbrook et al. 2011; Hill 2012; Herbst 2014), and none examines the pregnancy exemption. Hofferth et al. (2002; 2005) use the *pre-welfare-reform* period (1989−1996) and evaluate all policies requiring work and their associations with exit behavior. In line with my findings, they discover that a tighter AYC is associated with a higher likelihood of exiting welfare but also a speedier return to welfare. Other studies look at the *post-welfare-reform* period: Washbrook et al. (2011) and Herbst (2014) use 9-month longitudinal data from the Early Childhood Longitudinal Study Birth cohort (ECLS-B) and exploit cross-state differences in the generosity of AYC allotments in 2001, but do not make use of over time variation. Hill (2012) uses the June Fertility Supplement of the Current Population Survey between 1998 and 2008 to employ a

standard difference-in-differences strategy. All of the three papers find a strong impact of the AYC exemption: an AYC shorter than 12 months increases maternal work in the first year after childbirth (Washbrook et al. 2011; Hill 2012), and has negative effects on children's cognitive ability (Herbst 2014). Yet, they are limited in identifying a mother's month-to-month work status prior to and following the childbirth, which is critical in evaluating the AYC that comes into play at different months after birth across states.

I improve upon existing studies by employing a more flexible measure of maternal employment as well as an event study methodology to closely map out the evolution of labor force participation. The Survey of Income and Program Participation (SIPP) allow me to draw a monthly trajectory of maternal work by following the same mothers for as long as 48 months. Moreover, the SIPP provides detailed information about women's work status—whether they are working, unemployed (without work, laid off, or seeking a job), or are out of labor force. This is a new contribution in that most studies explore work vs no work outcome, which may mask substantial dynamics within the labor force. Given that no prior study has examined the pregnancy exemption, another major contribution of this paper is that it is the first to do so.

As a robustness check, I explore whether there is any endogenous response in women's childbearing decisions that could affect their work behavior. A decreasing trend in childbearing is observed after the exemption becomes tightened, however, the effects are not statistically significant. I further investigate whether the pregnancy exemption had any unintended consequences on infants' birth weights. Infants' birth weights declined by 10 grams after the elimination of the pregnancy exemption, but the effects are insignificant.

Taken all together, this paper sheds light on a full range of maternal behavioral responses to welfare work exemptions. It expands our understanding on potential unintended consequences of the new policy interventions, which could arise when the policy fails to consider a mother's different cost of working.

1.2 Institutional Background—Work Exemptions after Welfare Reform

Work requirements are *not* new components: they did exist before welfare reform in the form of Job Opportunities and Basic Skills Training (JOBS) program, under which states were required to engage recipients in education, work, or training. Yet, states' required work participation rate could be as low as 15% of non-exempt caseload. Furthermore, penalty for non-compliance was mild: welfare offices eliminated $65 per month (U.S. Department of Health and Human Services, 1996). Individuals were exempt from JOBS if they had a child under age 3 (or 1 at state option) or if they were in their 2nd or 3rd trimester[2] of pregnancy. In addition, there were many other reasons for not having to work while on welfare.

With the passage of the 1996 welfare reform[3], all states were required to meet higher work participation rate[4] targets by promoting welfare recipients to participate in work activities for at least 30 hours per week during benefit receipt.[5] Meeting the work participation rate has been a primary measure of success of the new welfare program (newly named as Temporary Assistance for Needy Families, TANF) for all states. Because caseload reductions can be used to meet the state work participation requirement, states often have attempted to maximize the measured work rate at the expense of actually helping recipients engage in work— by closing cases, thereby reducing the size of the state's caseloads (Brookings 2002; Kauff and Derr, 2008).

Exemption criteria also shrank substantially after welfare reform. One of the few exemptions remaining is the age of youngest child (AYC) exemption. Although federal law sets forth that single-parent families with a child under age 1 are not counted toward the work participation rate, there exists considerable cross-state variation regarding

[2] Pregnancy is measured in trimesters, totaling 40 weeks. The first trimester is week1 through week 12 (about 3 months), the second trimester is week 13 to week 27, and the third trimester spans from week 28 to the birth.
[3] The official name is the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA).
[4] The work participation rate was set at 25 percent in 1997, which increased to 50 percent in 2002.
[5] 20 hours for a single-parent family with a child under age 6. Higher hours requirements would apply to two-parent families. Bush administration reauthorized the TANF in 2002 and families were required to participate in work activities for a total of 40 hours per week, 24 hours of which should be direct engagement in work (either paid employment or community service jobs), and 16 hours of which in work-related activities (which could be defined by the states).

whether to require parents with a child younger than age 1 to work. Also, families are exempt from work requirements if there is any member with a severe disability or in poor health (e.g. pregnancy). Again, states were given wide discretion to design their own exemption rules and define specific criteria with respect to "young child", "disability" or "poor health".

Some states have made changes to their exemption policies even after welfare reform by further tightening them or by eliminating existing exemptions. Figures 1.2 and 1.3 summarize the variation across states and over time for 1996−2003, the study period of this paper.

In Figure 1.2, it is evident that almost all states allowed pregnant women (including women with their first pregnancy) to collect benefits without fulfilling work requirements around the time of the 1996 welfare reform. The month of pregnancy when the exemption starts varies across states. 29 states eliminated the pregnancy exemption subsequently, which means pregnant women are required to work as long as they are capable. Only 15 states were left with any form of the pregnancy exemption in 2003.

Similarly, in Figure 1.3, AYC exemptions longer than 12 months were the norm in 1996 across the country; however, 22 states shortened their AYC exemptions to be less than 12 months during the 1996−2003 period. By 2003, no states provided such a generous AYC exemption as pre-welfare reform, when a 36-month exemption was the most common allotment.

Exploiting the enormous between-state and within-state variation in work exemption rules over 1996−2003, I investigate whether stricter work exemptions have led to an increase in women's labor force participation, particularly around childbirth, and how they have affected subsequent TANF receipt.

1.3 Conceptual Framework

I present the canonical static model of labor supply with an individual's well-behaved utility defined over consumption (C) and time spent at home (L). I denote utility as $U = U(C, L)$, which is assumed to be convex and monotonic in both arguments. Total time (T) can be allocated to hours at home (L) or working (H). The budget constraint is

$B + (w - c)(T - L) - F = pC = Y$, where B is a transfer from welfare (i.e. TANF), w is the hourly wage rate, p is the price of the composite consumption good, and Y is the total income. After giving birth, a woman incurs a positive childcare cost, c, for every hour she works, as well as a fixed child care cost F.[6] For simplicity, I restrict my model to the *first birth* case. Therefore, c and F are zero in the pre-birth period, and both become positive in the post-birth period.[7] I also ignore positive taxes (e.g. Earned Income Tax Credit) and other government assistances (e.g. Supplemental Nutrition Assistance Program (SNAP, also known as food stamps), Medicaid, etc.).

I focus the following theoretical analysis only on single mothers to derive testable hypotheses. In practice, married mothers account for less than 10 percent of welfare recipients. In the empirical analysis, I use married mothers as a non-eligible comparison group, and indeed find null effects of welfare work requirements on this group.

Figure 1.4 introduces two different periods: pre- and post-birth. Note that there are two budget constraints in each period. The non-welfare budget constraint is $\overline{AH}$ with a slope $-w$, whereas the welfare budget constraint is $\overline{BF}$ with a slope $-w(1-\tau)$ (τ indicates the benefit reduction rate for a dollar increase in income, with $0 < \tau < 1$). The vertical distance $\overline{AB}$ represents the TANF maximum benefit amount for households with zero earned income. Figure 1.4.1 only considers incremental childcare costs, c, for every hour of work. The slope of the budget constraint becomes flatter with a per-hour childcare cost after giving birth. Final budget set is $\overline{ABDH}$ in the pre-birth, and $\overline{ABCG}$ in the post-birth. Figure 1.4.2 considers both fixed and variable costs of childcare. The pre-birth budget constraint is the same as that in Figure 1.4.1, but the post-birth budget constraint shifts down by the amount of the fixed childcare cost ($= BB' = AA'$). This shift creates a discontinuous point when working zero hours.

[6] Here I assume *w>c*. In general, TANF helps welfare recipients pay for child care usually in the form of vouchers that families can use to defray some or all of childcare cost from any legal provider. Sometimes, though, recipients pay part of their childcare costs, and the amount they pay increases with their income (The Urban Institute, 2006).

[7] The differences between first birth and higher-order birth cases would be that 1) F is not zero in the pre-birth period for higher order birth; 2) c will be scaled by the birth order of a child; 3) B will be greater for higher-order birth if the state allows TANF benefits to increase with the number of children.

Finally, I introduce work requirements[8] in Figure 1.5. I assume that mother's utility function changes after the birth such that the mother's preference for hours at home (L) becomes stronger relative to her preference before giving birth. Accordingly, a relatively flatter utility curve ($u_L < u_C$) is observed before childbirth, which becomes steeper ($u_L > u_C$) following childbirth.

The introduction of work requirements removes some portion of the welfare budget constraint. A welfare recipient is eligible for TANF benefits only if she satisfies the minimum number of hours required for work, denoted by $\overline{AH}_{min}$. This creates a new kinked budget constraint, $\overline{ACDEG}$.

Below, I describe two cases: 1) work requirements before birth (Pregnancy exemption); and 2) work requirements after birth (AYC exemption). For each case, I show how single mothers respond to work requirements by shifting their work and welfare behaviors.

1.3.1 Pre-birth work requirements (pregnancy exemption)

Women can choose to either work or stay at home prior to giving birth. I consider each option in Figure 1.5.1 and 1.5.2, respectively, and illustrate how pre-birth work requirements affect maternal labor supply and welfare receipt decisions.

***a) No work before work requirements* (Figure 1.5.1):** A single woman who initially used to work zero hours and fully depend on welfare benefits (u_1) is better off by staying on welfare and supplying minimum hours (u'_1) than leaving welfare ($u"_1$). This implies that work required during pregnancy would increase labor supply at the *extensive* margin, inducing nonworking single women to *enter* the labor force.

***b) Work before work requirements* (Figure 1.5.2):** A single woman u_2 has a flatter indifference curve than u_1 and supplies some amount of working hours (point F) prior to the introduction of work requirements. She will increase her hours to the minimum level required (u'_2, point D) as a result of mandatory work requirements, but no more than that ($u"_2$). This prediction suggests that work required during pregnancy

[8] The model with work requirements adopts elements similar to the model described in Moffitt (2002).

would increase labor supply at the *intensive* margin as well, leading working single mothers to supply *more* hours than they used to.

In the pre-birth period, there is no obligation of child care, which makes the opportunity cost of working relatively lower compared to that in the post-birth period. Therefore, work required during pregnancy is expected to increase women's labor supply—at both the extensive and intensive margins. Furthermore, both cases show that mothers continuously stay on welfare, which subsequently leads to a quicker welfare exit due to the consumption of lifetime limits.

1.3.2 Post-birth work requirements (age of youngest child exemption)

In my model, a mother strictly prefers staying at home after giving birth ($u_L > u_C$). This results in a much steeper indifference curve (u_3, u_4) in the post-birth period, relative to the pre-birth period (u_1, u_2). Therefore, I assume that all single mothers rely entirely on welfare benefits without participating in the labor force after birth if there are no work requirements. I describe two different schemes of childcare costs[9] in Figure 1.5.3 and 1.5.4, respectively, and illustrate how post-birth work requirements affect maternal labor supply and welfare receipt decisions.

a) Variable childcare costs only (Figure 1.5.3): After the introduction of work requirements, a single mother is better off by exiting welfare and not fulfilling the work requirements (u'_3) than she is by working the minimum hours required to qualify for welfare ($u"_3$). She ends up entering the labor force to support her family, but supplies fewer hours than what is required under TANF (u'_3, point F). In other words, a single mother is willing to trade off welfare benefits for non-monetary things such as staying at home and taking care of her newborn baby. To do this, she would seek a part-time job ($H < H_{min}$). Therefore, if work is enforced when there are competing demands of work and motherhood, it could lead to the increase in labor force participation, but not driven by stable employment.

[9] Washbrook et al. (2011) find that the type of child care usage differs by the length of the AYC exemption. They show that long AYC is associated with more parent-only care and less informal non-center-based care providers.

***b) Variable and fixed childcare costs* (Figure 1.5.4):** Adding fixed childcare costs in addition to incremental costs shifts down the entire budget constraint by the amount of the fixed costs, producing a new budget constraint, $\overline{AFCDEG}$. Fixed childcare costs lead to a situation where it is much harder for single mothers to get off of welfare from point B, leading to a higher dependency on welfare. Figure 1.5.4 demonstrates that women will exit welfare and work zero hours when work requirements are imposed after childbirth, revealing the deterrent effects of fixed childcare costs. This implies that a severe burden of child care in the post-birth period makes it more challenging for welfare recipients to transition from welfare to work, generating a "disconnected group", who neither work nor are on welfare (point A).[10]

In the post-birth period, mothers' tradeoff between market work and home production is most pronounced, and the opportunity cost of working is relatively higher compared to that in the pre-birth period. Theoretical model predicts that work enforced shortly after birth is expected to increase women's labor force participation at the extensive margin, mainly driven by part-time employment with an exit from welfare. A greater likelihood of welfare exit for mothers exposed to a restrictive AYC exemption is also found by Hofferth et al. (2002). But these mothers are highly likely to re-enter welfare eventually, as found in Hofferth et al. (2005).

The predictions of the model guide an empirical test of my research question: do we see heterogeneous labor supply responses by the timing of work enforcement? Does the strictness of work requirements affect women's future reliance on welfare? I provide empirical evidence on maternal labor force participation and welfare receipt by studying the pregnancy exemption and the AYC exemption, which apply on either side of the event of birth. The model predicts that single mothers would increase working hours as well as entry into the labor force as a result of their state having no pregnancy exemption. However, a strict AYC exemption would induce single mothers to enter the workforce but work far less hours that what is required at minimum. Accordingly, it is predicted that a strict AYC exemption would extend the period on welfare because the increase in earnings from temporary jobs is unable to offset the loss of welfare benefits.

[10] Perhaps, these mothers may rely on other government transfers, which are outside the model.

1.4 Methodology for Estimating the Effects of Welfare Work Exemptions

I employ an event study framework (Jacobson et al., 1993) to depict a monthly trajectory of maternal labor force participation around the event of childbirth. I run a triple-difference event study estimator by exploiting three of the principal attributes of TANF work exemptions: 1) pre- versus post-implementation of strict exemption policies; 2) strict versus lenient states; and 3) marital status of mothers. By doing so, I obtain a detailed picture of labor force participation pattern across states, policy timing and mother's marital status.

1.4.1 Difference-in-Differences (DD) in the Event Study

I start by estimating a simple difference-in-differences (DD) in the event study where the two differences are 1) pre- versus post-strict exemption policy (identified by the states that tightened the policy), and 2) strict versus lenient states. The DD analysis allows me to see heterogeneous labor supply responses by the strictness of exemption rules and by the timing of work requirements. I estimate equation (1) for single mothers and married mothers, separately:

$$(1)\ LFP_{ist} = \beta_i + \sum_{m=-24}^{m=24} \delta_t \cdot I_{ist}(m = t - B_i) + \sum_{m=-24}^{m=24} \pi_t \cdot I_{ist}(m = t - B_i) \cdot Strict_s \cdot Post_{st} + \alpha \cdot X_{st} + Year_t + State_s + \epsilon_{ist}$$

The outcome of interest is LFP_{ist}, which takes a value of one if a mother, *i*, in state, *s*, is in the labor force during month, *t*. Other outcomes studied in subsequent analyses include indicators for employment, unemployment, and TANF receipt. B_i is the event month of childbirth to a mother *i*. Thus, m counts months relative to the event of birth, which range from −24 to +24. $Strict_s$ is 1 if state, *s*, ever implemented a strict exemption policy and $Post_{st}$ is an indicator of whether month, *t*, occurs after a strict exemption policy has come into effect in state, *s*. Therefore, the interaction between

$Strict_s$ and $Post_{st}$ is 1 if state, *s*, implemented a strict exemption policy during month, *t*. To control for state-specific characteristics and year-specific shocks, I include state ($State_s$) and year ($Year_t$) fixed effects. Also, the time frame of the study was a period of substantial transformation of welfare and other social policies as well as unemployment rates. Therefore, I adjust for state characteristics (X_{st}) that changed differentially over the study period such as the unemployment rate, the maximum TANF and SNAP benefit amounts for a family of three, the maximum refundable EITC credit, and state child support enforcement expenditures. I cluster standard errors at the mother-birth level.[11] The vector of coefficients, π_t, captures the difference in the outcome between a mother living in a state where strict exemption is in effect and a mother exposed to lenient exemption during month *t*. Theory predicts π_t to be positive, implying that a mother would participate in the labor force more if she lives in a state with a stringent exemption policy. Here, an implicit assumption is that strict requirements have their full treatment effect instantly upon being introduced.[12]

1.4.2 Triple Differences (DDD) in the Event Study

The goal of implementing a triple difference (DDD) in the event study is to test whether the DD estimates are meaningful only for women who are receiving TANF. In other words, the DDD analysis allows me to check whether heterogeneous responses by the strictness of exemption rules and by the timing of work requirements are only pronounced by TANF receiving mothers, but not driven by non-recipients.

Actual TANF receipt data would bias the estimates with a selection by excluding mothers who decided not to apply or who decided to leave welfare based on welfare rules, including the exemption policy. Alternatively, marital status is widely used as a proxy for welfare eligibility, and serves to define the treatment group in the broader

[11] As a robustness check, I cluster at the state level, which is the level of the treatment, but the results remain substantially similar.

[12] A reasonable question is whether state TANF administrators are aware of, understand, and enforce the rules upon any changes. Pingle (2003) contacted TANF administrators in 16 states to inquire about these issues. State officials indicated that awareness and enforcement of AYC are widespread. Furthermore, the administrators knew immediately the length of the current AYC, and explained that it is easily applied by caseworkers to exempt mothers from the work requirements (Herbst, 2014).

welfare literature.[13] Hence, the estimates can be interpreted as an intent-to-treat estimate of the exemption policies: I use "eligibility," but not actual "treatment" to estimate the effects of welfare work exemptions. This strategy also avoids the empirically important issue of systematic measurement error of welfare receipt in survey. Therefore, I compare mothers who were *single* when the exemption policy is in effect[14] with *always married* mothers in this study[15].

It is worth noting, however, that some married women in the sample may have received benefits from TANF since married mothers comprise 10 percent of TANF recipients. In other words, marital status does not perfectly predict TANF eligibility. In that sense, estimates from this study may *underestimate* the actual impact of TANF exemption policies and may be thought of as a lower bound (to the extent that married women can still benefit from TANF and single women may not receive benefits from TANF). Figure A1.1 presents TANF participation trend for all women in the analysis sample, and the heterogeneous patterns by marital status and by birth-order of a child.

Keeping this in mind, I estimate a triple-difference estimator in equation (2):

$$(2)\quad LFP_{ist} = \beta_i + \sum_{m=-24}^{m=24} \delta_t \cdot I_{ist}(m = t - B_i) + \sum_{m=-24}^{m=24} \pi_t \cdot I_{ist}(m = t - B_i) \cdot Strict_s \cdot Post_{st} + \sum_{m=-24}^{m=24} \alpha_t \cdot I_{ist}(m = t - B_i) \cdot Single_{ist} + \sum_{m=-24}^{m=24} \theta_t \cdot I_{ist}(m = t - B_i) \cdot Strict_s \cdot Post_{st} \cdot Single_{ist} + \alpha \cdot X_{st} + Year_t + State_s + \epsilon_{ist}$$

The three differences are 1) pre- versus post-strict exemption policy ($Post_{st}$); 2) strict and lenient states ($Policy_s$); and 3) single and married mothers ($Single_{ist}$). The vector of coefficients of interest, θ_t, is the monthly estimate of the treatment effect of strict exemption policies for single mothers relative to married mothers for each month *t* around the event of childbirth. Theory predicts θ_t to be positive during month *t* in which

[13] Married women with children or single women without children often serve as comparison groups. (e.g. Meyer and Rosenbaum 2000, 2001; Kaushal and Kaestner 2001; Kaestner, Korenman and O'Neill 2003; Kaushal, Gao, and Waldfogel 2007; Hill 2012)

[14] I define a single mother *i* using the marital status reported during pregnancy for the pregnancy exemption analysis, and the marital status reported after childbirth for the AYC exemption analysis. This alleviates concerns of endogenous selection to marital status because it is measured simultaneously at the initiation of each exemption.

[15] Results are similar when I define single mothers as those who were always single for all months before and after childbirth, not residing with parents or a partner.

the exemption takes effect, implying that a strict exemption increases labor force participation of single mothers. Furthermore, it will not be significantly different from π_t in equation (1), if there is no heterogeneous response for married mothers. The identification assumption, implicit in the estimator in equation (2), is presented in section 8.

1.5 Data and Measurement

1.5.1 Data: Survey of Income and Program Participation

To measure the monthly evolution of labor supply before and after childbirth, it is essential to know the exact timing of childbirth as well as a fine measurement of the mother's work status. The Survey of Income and Program Participation (SIPP) is the natural choice, since it contains detailed information on all these critical measures. The SIPP is a nationally representative longitudinal data set, following respondents for up to 4 years in each panel.[16] The SIPP also uses a relatively short recall period of 4 months. Moreover, the SIPP has a large sample size and includes a high proportion of low-income households compared to other national panel surveys. Numerous studies have used the SIPP to examine the effects of government assistance programs or specific policy changes targeted to low income households (e.g. Fairlie and London 1997; Barrow 2007; Mazzolari 2007; Blank and Matsudaira 2014).

I use the 1996 and 2001 panels whose reference periods cover 1996 to 2003[17], a period starting from the passage of welfare reform until its aftermath had been stabilized. Five U.S. states (Maine, Vermont, Wyoming, North Dakota, and South Dakota) are not uniquely identifiable in the SIPP, so observations from these states are dropped. For all analyses, I use individual weight measured at the last wave during the panel.

[16] The 1996 panel is approximately 48 months in length (12 waves), whereas the 2001 panel is 36 months in length (9 waves). Response rates shrink over the life of the panel: in the first wave of the 1996 panel, the response rate was 92%, but by the final wave 4 years later, it fell to 65%. The 2001 panel had an 87% response rate in its initial wave, and a 68 % across the full panel.

[17] The reference period of the 1996 Panel is from December 1995 to February 2000, and that of the 2001 Panel is from October 2000 to December 2003. Thus, the 7 months from March 2000 to September 2000 are not covered in the analysis.

Lastly, I obtain state TANF policy data from the Welfare Rules Database of the Urban Institute, which is widely used in the welfare literature (e.g. Washbrook et al. 2011; Hill 2012; Herbst 2014).

1.5.2 Analysis Sample for Women's Labor Force Participation

To construct the study sample, I identify every woman aged 18 to 45[18] who gave birth during the panels. There are a total of 78,888 mother-by-birth-by-month observations in the sample, consisting of 2,463 mothers and 2,570 mother-births (meaning that some mothers had more than one birth during the panel). After identifying mothers with the month of birth[19], I track their labor force participation in the 24 months leading up to and the 24 months following childbirth. All observations more than 24 months away from a birth are dropped. Note that not all women have information for the full 24 lead and 24 lag months because women give birth at different points over the course of the SIPP panel. Whether I can observe $m = t - Birth_i,\ m \in [-24,24]$ for a woman only depends on when the birth falls in the SIPP panels, which is unlikely to be manipulated by women. Therefore, variation in m across mothers is not correlated with unobserved factors that affect women's labor force participation. Thus, the unbalanced panel feature employed in my analysis is not a threat to identification.

Month of birth ($Birth_i$) is available for all observations since it is how women in the sample are identified. Consequently, the sample size is larger for months around the event of birth and gradually declines as the observations move farther away from the month of birth.

1.5.3 Measurement of Labor Force Participation

TANF defines "work" as labor force participation (U.S Census Bureau, 2002). The 1996 law sets forth primary work activities that can count toward the work

[18] Women younger than age 18 are omitted in light of states' stipulations on teen parents: they are deemed engaged in work if they are enrolled in school and work toward the high school degree (Table 1.1 column 5).
[19] The policies that I study here could affect the probability of observing a birth in the data. In Section 8.1, I show that there is no measurable change in fertility in response to different exemption policies across states.

participation rates: 1) paid or unpaid work; 2) vocational training; 3) job search[20]; and 4) providing child care to others. These work activities are broad in the sense that they include not only regular work, but also other types of activities that make individuals "job-ready". By 2002, almost two-thirds of TANF recipients who met their work requirements did so by working full- or part-time jobs in the public or private sectors that are not subsidized by TANF or other public programs. Participation in all other categories was low (The Brookings Institution Policy Brief, 2004).

Therefore, I use an indicator of being *in the labor force* as my main outcome. A woman is counted as *in the labor force* if she worked, was with a job, laid off, on leave, or searching for a job during the reference month[21]. She is *out of the labor force* if she had no job, no time on layoff, and no time looking for work all month. I break labor force participation down into employment (working or with a job *all weeks* during the reference month) and unemployment (reports any time without a job due to layoff or job search).

1.5.4 Descriptive Evidence on Labor Force Participation

To present a descriptive picture of women's labor force participation around childbirth, a simple event study specification is estimated:

$$(3) \quad LFP_{ist} = \beta_i + \sum_{m=-24}^{m=24} \delta_t \cdot I_{ist}(m = t - B_i) \; + \epsilon_{ist}$$

β_i are individual fixed effects and $I_{ist}(m = t - B_i)$ is a set of indicators for months relative to the event of birth (B_i), where *m* ranges from 24 months before to 24 months after. When drawing descriptive plots, I omit the 12th month prior to birth as a reference group so that the δ_t coefficients map out the time path of changes in labor force participation relative to the level a year before the birth.

[20] The act of seeking employment, or preparation to obtain employment, including life-skills training and substance abuse treatment, mental health treatment, or rehabilitation activities (Congressional Research Service, 2012).

[21] In the SIPP, there is an indicator for whether a mother attended schooling or training because social services or a welfare office paid for, referred, or sent her. But, this variable does not distinguish between schooling and training. When I cross-tab this indicator with labor force participation variable, 75 percent of them are recorded as being out-of-labor-force and 25 percent being in the labor force.

Figure 1.6 panel A depicts the overall labor force participation trend for all women in the sample. Labor force participation starts to decline from 9 months prior to childbirth and then rebounds after giving birth. Strikingly heterogeneous patterns are observed by marital status. Panel B reveals that there is a more drastic dip and leap around birth for single mothers, while the rates for married mothers stay relatively stable. The dramatic drop in labor force participation implies that a large share of single mothers withdraw from the work force around childbirth. The path after the birth seems plausible in that single mothers will have to return to work sooner than married mothers, since they are likely to be the only earner of the household[22]. Breaking down by birth order, panel C shows that the fraction of single mothers working right up to birth is pretty much the same across birth orders, around 65 percent. On the other hand, the fraction of their married counterparts who are working right up to the birth is much lower if they already have one child (from 85 percent to 65 percent).

1.6 Results: The Impact of the Pregnancy Exemption

Pregnant women with no child are eligible for TANF, in which case the enrollment is conditional on receipt of medical documentation of pregnancy. Pregnant women can be exempt from work and states vary in their pregnancy exemption rules: some exempt later in the pregnancy (from the third trimester or for the last month), some exempt early in the pregnancy (from the second trimester), and others do not exempt for pregnancy at all. In this section, I define *strict* pregnancy exemption as having *no* pregnancy exemption.

In this section, I explore the heterogeneous effects by the strictness of TANF pregnancy exemption on women's labor force participation. Furthermore, I decompose labor force participation into employment and unemployment in order to assess which is the main factor that drives the changes in the labor force participation patterns. Lastly, I document the effects on welfare receipt as well to uncover any potential consequences on future welfare dependency.

[22] In this analysis, single mothers are defined as women who are unmarried, divorced, widowed, or separated and reside with no senior members and no partner in the household.

1.6.1 The Impact of the Pregnancy Exemption on Labor Force Participation

Figure 1.7 exhibits women's labor force participation response to the pregnancy exemption. As the arrow indicates in panel A, the month in which a divergence in labor force participation occurs coincides precisely with what the policy dictates for the target group. Across all months surrounding childbirth, single mothers who live in states with no pregnancy exemption are more likely to be in the labor force compared to their counterparts who are exempt from the second trimester. The gap in labor force participation starts to open up exactly at the initiation of the second trimester. Interestingly, this trend is not observed for the married mothers in Panel B.

Figure 1.8.1 presents difference-in-differences (DD) event study estimates for single mothers (panel A) and married mothers (panel B). The dashed line with triangle markers represents changes in monthly labor force participation in lenient states, δ_t from equation (1). The solid line with diamond markers shows changes in labor force participation in strict states, $\delta_t + \pi_t$. The difference between these two lines, π_t, is depicted by a dotted line at the bottom. Panel A shows that the DD for single mothers becomes positive from approximately the second trimester of pregnancy. The positive DD estimates are significantly different from zero starting from two months before giving birth and up to 4 months after. In contrast, the DD estimates are essentially zero for married mothers, which ensures that married mothers are a well-defined comparison group.

Figure 1.8.2 plots the DDD estimates, the coefficients on the triple interaction term, θ_t, from equation (2). It is the relative difference between single mothers' DD estimates and married mothers' DD estimates. The DDD estimates resemble the DD estimates for single mothers because those for married mothers are essentially zero. These estimates provide supportive evidence on the effect of the pregnancy exemption policy. If a single mother lives in a state with no pregnancy exemption, she is more likely to participate in the labor force—particularly around childbirth—compared to a single mother who is exempt during pregnancy. Table 1.2 confirms that the DDD estimates are indeed positive and statistically significant starting from the last trimester to

approximately a quarter after birth, consistent with Figure 1.8.2. In addition to showing the magnitude and significance for each month, I implement a joint test of significance in Table 1.3. A joint test of significance for months directly around childbirth has a p-value of 0.10 with an average estimate of 10 percentage points. Taken together, these results suggest that the pregnancy exemption accounts for differences in labor force participation directly around the event of birth between single and married mothers.

1.6.2 The Impact of the Pregnancy Exemption on Employment and Unemployment

Labor force participation combines employment and unemployment. Hence, an aggregate shift in labor force participation masks the extent to which the observed pattern is driven by employment versus unemployment. To uncover the effects, I run the same specifications, with the outcomes of employment or unemployment. Employment is defined as working or being with a job for *all weeks* during the reference month. Unemployment contains everything else, including temporary attachment, layoff, or job search. [23]

Figure 1.9.1 presents the DD estimates for a single mother's employment. If she lives in a state having no pregnancy exemption, she is more likely to be working or continuously attached to a job around birth compared to her counterparts in a state with the pregnancy exemption. The parallel figure for married mothers is not presented here, as the DD estimates for married mothers are indistinguishable from zero for all months along the time line. Figure 1.9.2 presents the DDD estimates. These resemble the DD estimates for single mothers, providing further evidence of zero effect on married mothers. With a significant joint test result, the figure confirms that the absence of the pregnancy exemption leads to a higher rate of working and employment starting from the last trimester. It is worth noting that point estimates are all positive along the time line although they are not precisely estimated except for directly around childbirth.

[23] Out-of-the-labor force is different from unemployment. Unemployment occurs when people are without work and actively seeking work. Out-of-the-labor force group typically is comprised of students, homemakers, or people in military, who are either unwilling or unable to engage in the workforce.

I omit the plots for unemployment because the DD estimates for both single and married mothers are not significantly distinguishable from zero. This suggests that the overall labor force participation changes in response to the pregnancy exemption are indeed driven by employment.

1.6.3 The Impact of the Pregnancy Exemption on TANF Receipt

Using the monthly program participation information available in the SIPP, I also display the monthly TANF receipt pattern by the strictness of the pregnancy exemption. Interestingly, Figure 1.10 reveals that the monthly TANF receipt pattern diverges after childbirth between the two types of states. The difference is zero in the pre-birth period, but a greater increase in welfare receipt after childbirth is evident for single mothers with the pregnancy exemption compared to single mothers with no pregnancy exemption available. Together with the previous finding of increased employment, this finding suggests that work activities enforced before birth actually lead to favorable work outcomes and declining dependence on government assistance afterwards. On the other hand, single mothers who opt out of the labor force during pregnancy with the help of the exemption may suffer from penalties from labor force withdrawals, signaling a lack of commitment. This could trigger a higher reliance on welfare as witnessed in Figure 1.10. The pattern is consistent across all birth orders (results not shown). A flat line at zero for married mothers in Panel B again ensures that the effects decidedly originate from TANF.

1.7 Results: The Impact of the Age of Youngest Child (AYC) Exemption

New mothers represent a large share of TANF entrants (Ybarra, 2014), in which case TANF enrollment is conditional on receipt of a birth certificate. Federal TANF guidelines allow states to provide benefits to mothers with children under one year of age

without imposing work requirements. Because states have flexibility in setting TANF rules, the duration of the work exemption for new mothers varies considerably across states. The age of a mother's youngest child determines the length of the exemption after birth, which varies from 0 months (i.e. no exemption) to 3, 4, 6, 12, 24, 36, 48, and 60 months. The most common AYC exemption is 12 months (about 50%), as can be expected from federal rules, followed by 3−4 months and 36 months[24]. I define an AYC exemption to be strict if it is less than 12 months. In this section, I quantify the heterogeneous effects by the strictness of the AYC exemption on women's labor force participation as well as on employment, unemployment, and TANF receipt.

1.7.1 The Impact of the AYC Exemption on Labor Force Participation

In Figure 1.11, single mothers noticeably respond to the AYC exemption policy while married mothers do not. The month in which labor force participation rates return to the same level of 9 months before birth is exactly when a mother hits the month corresponding to her state's AYC allotment. It is also informative that the pre-birth trends are not distinguishable from each other between the two kinds of states, confirming that the AYC influences mothers' labor supply only after birth, when the exemption becomes effective. This figure contrasts sharply with Figure 1.7, the parallel plot for the pregnancy exemption. In Figure 1.7, labor force participation starts to diverge before birth in response to the pregnancy exemption.

Figure 1.12.1 presents the difference-in-differences (DD) event study estimates for single mothers (panel A) and married mothers (panel B). Labor force participation for single mothers with an AYC exemption shorter than 12 months (solid line with diamond markers) exhibits a sudden dip one month prior to birth and then jumps 4 months after

[24] Concerns may arise that these mothers could also benefit from the Family and Medical Leave Act (FMLA), which guarantees 12 weeks of unpaid, job-protected leave. But, the act has garnered widespread criticism for covering only about half of all women in the workforce (Berger and Waldfogel, 2004), and only a fifth of new mothers (Ruhm, 1997). Acs and Nichols (2007), using 2004 Current Population Survey data, found that those who use FMLA coverage are most likely to be professional, salaried workers with higher earnings and education (U.S. Department of Labor, 2001). Therefore, the uniform consensus is that the FMLA has disproportionately excluded low-income women. Ybarra (2013) discovered that new-mother welfare participants use TANF in a similar way to how other mothers use FMLA or paid leave in Wisconsin, which provides evidence related to a lack of employer-provided paid leave.

birth. This sudden leap at the 4th month is notable given that the most common AYC length in the strict states is 3–4 months (76%). The DD estimates are negative immediately before giving birth, but become positive continuously from the 4th month after birth, although they are not statistically significant. The DD estimates are not distinguishable from zero for married mothers and even become negative after birth, moving in the counterintuitive direction.

Figure 1.12.2 plots the DDD estimates (coefficients on a triple interaction term, θ_t, from equation (2)). The DDD estimates fall right before birth but become positive and statistically significant during the post-birth period—approximately from 4 months after birth. The strong effects in a longer-term post birth are presumably driven by the negative DD estimates for married mothers, making the DDD estimates inflated from a year after childbirth.

Table 1.4 confirms that the DDD estimates are indeed positive and statistically significant from a quarter after birth, consistent with Figure 1.12.2. The drop in labor force participation right before childbirth in strict states could potentially be due to anticipation by single mothers, who change their work behavior and opt out of the work force after learning that they will have to return to work shortly after childbirth. In Table 1.5, a joint test of significance shows that a strict AYC exemption decreases labor force participation right around childbirth by 4 percentage points. The average effect of a strict AYC exemption in the post-birth period is an increase in labor force participation by 13 percentage points. These estimates are well within the range of those from previous studies: Hill (2012) finds that no AYC exemption increases full-time work by single mothers by 23 percentage points compared to an AYC exemption of 12 months or longer. Washbrook et al. (2011) reports that long AYC exemptions reduce maternal work at or before 4 months by 7 percentage points, which is sustained intact to 9 months after birth.

1.7.2 The Impact of the AYC Exemption on Employment and Unemployment

I estimate the same specifications for employment and unemployment outcomes, separately. Figure 1.13.1 reports the DD estimates for single mothers' employment. It

shows the opposite trend from the parallel figure of the pregnancy exemption (Figure 9.1): single mothers are less likely to be working or stably with a job around childbirth when the AYC exemption is short. The DDD estimates in Figure 1.13.2 confirm lower employment in strict AYC states, suggesting that single mothers reduce their amount of work or job attachment even before giving birth, once they realize that they are subject to a quick return to work after giving birth. Figure 1.14 supplements this argument by assessing unemployment changes, suggesting that single mothers are employed unstably or are engaged in activities such as job search, training, or community service.[25] This decomposition exercise, together with the previous finding of a sudden drop in labor force participation before childbirth, implies that it is highly possible that rigid work requirements imposed shortly after birth may bring about a long-term impact on single mothers' unemployment. It could be attributable to the maximized conflict between employment and childrearing for mothers, which enormously increases the opportunity cost of working relative to the pre-birth period.

It is also noteworthy that the statistical significance of single mothers' labor force participation as well as employment and unemployment patterns found above are primarily driven by first births (results by birth order not shown). This is likely due to the fact that many states' AYC exemptions are applicable for first births, but are generally reduced or eliminated for subsequent births (state policy data from the Welfare Rules Database; Herbst 2014).

1.7.3 The Impact of the AYC Exemption on TANF Receipt

The monthly TANF receipt by the strictness of the AYC exemption is presented in Figure 1.15. Although the confidence intervals are overall wide, single mothers living in states with a restrictive AYC exemption exhibit a higher fraction of TANF receipt from pregnancy up to one year after childbirth. This pattern is observed for all birth orders, suggesting that work requirements enforced when mothers are balancing the competing roles of work and motherhood may lead to more dependency on welfare.

[25] For employment and unemployment outcomes I do not present parallel plots for married mothers because the DD estimates are essentially zero for them.

1.8 Robustness Check and Further Analysis

1.8.1 Any Endogenous Response of Women's Fertility?

I assess a potential mechanism through which mothers' labor force participation could be affected by welfare work exemptions: Fertility. Mothers could endogenously make their childbearing decisions in response to the exemptions available to them. For instance, if they are exempt from work for a longer span of time before or after giving birth, this would make having a child less costly within the relevant subpopulation. Moreover, mothers potentially could space the timing of births in a way that they can collect the benefits continuously without having to work for an extended period of time.

Using the National Vital Statistics Birth Data from 1991 to 2002, I collapse all U.S. births by state, year, and marital status in order to compare average general fertility rates[26] by states' exemption policies and by marital status. I specify a triple-difference estimation in the event study framework, where the event is defined as the year of the exemption policy change from lenient to strict in switch states. By presenting the DDD estimates, I test whether any difference in general fertility rates observed between switch and no-switch states after adopting a strict exemption is mainly driven by single mothers.

Figure A1.2 reports the DDD estimates for 5 years before and after the AYC exemption policy change, which are hardly distinguishable from zero. It is worth noting that the point estimates show a decrease by 5 births per year per thousand childbearing women after the AYC exemption became tightened, which is a sizable magnitude, albeit imprecisely estimated. I conclude that there is not enough power to detect the impact of the AYC exemption on general fertility rates of welfare recipients before and after the policy change.

The same story applies to the pregnancy exemption, as shown in Figure A1.3. Similarly, fertility rates decrease by 3−5 births after the pregnancy exemption is eliminated; however, I fail to reject the zero effect due to large confidence intervals. These findings are also consistent with the existing literature, where the dominant

[26] The general fertility rate (GFR) is the number of births per thousand women of childbearing age (15−44). Thus, it is essential to have information on the number of women of childbearing age. I obtained the number of childbearing age women from IPUMS-CPS (Integrated Public Use Microdata Series-Current Population Survey) and counted the number by state, year, and marital status in order to construct population estimates.

conclusion is that the effect of welfare reform on fertility is uncertain (e.g. Joyce et al. 2004; Kearney 2004; Smith 2006). The fertility analysis suggests that we cannot decide whether welfare work exemptions would promote or discourage mothers' subsequent child bearing.

1.8.2 Indirect Consequences on Infants' Birth Weight?

Using birth weight information available in the National Vital Statistics Birth Data, I investigate whether strict enforcement of work during pregnancy has any unintended consequences on infants' health. With the same identification strategy adopted for fertility analysis, I compare relative differences in infants' birth weights between strict and lenient states before and after the elimination of the pregnancy exemption, by mothers' marital status. Figure A1.4 reports the DDD estimates, revealing no measurable impact on infants' birth weight. The point estimates decline by 10 grams 5 years after the removal of the pregnancy exemption, but are not statistically significant. This null finding on infants' health aligns with Washbrook et al. (2011), who find no effects of the AYC exemption on duration of breast feeding nor on the number of well-baby visits, although they do not assess infants' birth weight per se.

1.8.3 Are the Two Exemption Policies Varying Independently?

Any correlation between the two work exemptions could lead to a case in which the effects of these exemption policies cannot be separately estimated. Firstly, since my identification strategy partially relies on the timing of exemption policy changes from lenient to strict, one may be concerned about any overlap in the timing of changes across the two exemptions. I address this issue in Figure A1.5. Switch states identify the cross-year, within-state variation in the exemption policies while no-switch states are in the fixed effects, picking up the aggregate trend. Approximately 80 percent of mothers in the analysis sample live in the 37 states that switched one or both exemption policies, while 20 percent live in the 14 no-switch states. Out of the 80 percent of the sample which identifies my estimates, 76 percent lived in states where only one exemption policy switches with the other exemption policy remains unchanged. Only 6 states out of the 37

switch states change both exemptions simultaneously, and this corresponds to 6.4 percent (80% * 8%) of the study sample. Therefore, the overlap between the two exemption policies is negligible and they vary independently enough that it makes sense to think about them separately.

Secondly, I assess whether there is any interaction between the two exemption policies in a way that the effects of one exemption policy differ by the strictness of the other. To address this issue, I estimate the same triple-difference specification in the event study, equation (2) separately by the strictness of the other exemption policy. I find no evidence of differential effects (results not shown), which ensures that my estimated effects for one exemption are not confounded by the other exemption.

1.8.4 Are Other TANF Policies Changing Simultaneously?

One further difficulty may arise because a state's choices of TANF program parameters may be correlated with one another. The work exemption is only one aspect of TANF. Hence, it is challenging to disentangle the independent impact of provisions in the TANF package and attribute any effects specifically to the work exemptions. Therefore, an additional identifying assumption is needed: between-state and within-state variation in the exemption policies should be orthogonal with variation in other welfare policy parameters. Possible TANF policies established after welfare reform which could also affect labor force participation include the minimum hours required for work, family cap, full sanctions, and welfare lifetime limits.[27]

Indeed, I find that states that adopted strict exemption policies are also strict in other dimensions of welfare policy. For example, a state with a short AYC exemption or no pregnancy exemption is more likely to terminate the benefits as a penalty for non-compliance (i.e. full sanction) rather than to reduce the benefits (i.e. partial sanction), more likely to impose a cap on TANF maximum benefits, and more likely to have a shorter lifetime limit. Hence, estimates of this study could be overestimated to the extent

[27] Currently, about 23 states have implemented some type of a "family cap" or "child exclusion" policy, which denies the increase in welfare benefit amounts after the birth of another child. Under the "full sanction", benefits are terminated as a penalty for failure to engage in work, whereas benefits are reduced under the "partial sanction". Moffitt (2003) provides an excellent review of the rules and structure of the TANF as well as comparison of TANF with the historic AFDC (Aid to Families with Dependent Children) program.

that other TANF policies also affect mothers' labor force participation decisions toward the same direction in conjunction with work exemption.

However, I make use of one particular feature of the exemption policies: that they become effective at a specific month during pregnancy or after giving birth. None of the other policies necessarily come into effect in the months proximate to childbirth. Welfare recipients could alter their work decisions in any month when the rule is likely to bind. A key component of my findings is that the effects visibly appear at two months before childbirth in response to the absence of the pregnancy exemption. The effects emerge at four months after childbirth in response to the short AYC exemption, which is precisely the month when the exemption expires in 80% of strict AYC states. If these results were due to other welfare policies, then we would not see the changes in mothers' work behavior at months when the pregnancy or the AYC entitlements begin or expire. Figure A1.6 shows the triple-difference estimates for four other TANF policies, discussed above, which are expected to have a strong impact on recipients' work. Positive DDD estimates reveal that strict policy is associated with higher labor force participation. Yet the effects do not emerge at certain months surrounding childbirth, nor significantly estimated along the months before and after the childbirth.

1.9 Conclusion

Welfare reform transformed the longstanding cash assistance program, by tying welfare benefits more strongly to work. States were given block grant funding from the federal government to design their own TANF programs and achieve high work participation rates among welfare recipients.

In this paper, I shed light on the effects of the two most prevalent exemption policies which apply to mothers who are expecting a baby, or mothers who just gave birth. These policies vary considerably across states in terms of the length of the exemption. Exploiting not only cross-state but also cross-year variation in the post-welfare reform period, I explore mothers' labor supply responses to the exemption policies, from labor force participation to employment and unemployment. Furthermore, I closely examine the monthly trajectory of welfare receipt by the strictness of exemption

to address whether the policies achieved their goal of moving people off of welfare to work.

I discover a sizable impact on labor force participation, strongly driven by single mothers. The absence of the pregnancy exemption induces more women to work or be attached to the workforce from 2 months before birth, and this impact continues until the first quarter after giving birth. In contrast, work required shortly after birth causes withdrawals from the labor force before childbirth and a higher likelihood of unemployment around childbirth. These exemptions are also found to predict future dependency on the TANF program.

TANF is the only employment program in which getting participants into paid employment is not a key measure of success (Schott and Pavetti, 2013). Therefore, it is unclear whether severe work requirements imposed as a condition for getting cash grants successfully engaged recipients in work. My study suggests that the rigidity of work requirements during a period when family responsibilities are pressing can make vulnerable single mothers further withdraw from the workforce. Or, they seek temporary employment, which hardly continues to a stable attachment to the workforce, thereby triggering more dependency on welfare.

A widely accepted agreement is that work should be a key element of government assistance programs. Yet, prescriptive work requirements imposed on a mother with a young child may give rise to mere labor force "participation", but not actual "employment". By delving into welfare work exemption policies that have been studied little, I quantify a full set of behavioral responses, which is pivotal for understanding the potential costs and benefits of these new interventions. This paper offers useful insights into the optimal structure of TANF work exemption rules to better support vulnerable groups to actively engage in the workforce. Well-established work requirement and exemption policies can have a long-lasting impact, helping these groups to eventually achieve economic independence and self-sufficiency.

References

Acs, Gregory and Austin Nichols, "Low-Income Workers and Their Employers: Characteristics and Challenges," *The Urban Institute* (2007), 1–14

Acs, Gregory and Sandi Nelson, "Changes in Living Arrangements during the Late 1990s: Do Welfare Policies Matter?" *Journal of Policy Analysis and Management* 23(2) (2004), 273–290

Adams, Gina, Halcomb, Pamela, Snyder, Kathleen, Koralek, Robin, and Jeffrey Capizzano, "Child Care Subsidies for TANF Families: The Nexus of Systems and Policies," The Urban Institute, Washington D.C. (2006), 1–95

Barrow, Lisa. "An Analysis of Women's Return-to-Work Decisions Following First Birth," *Economic Inquiry* 37(3) (2007), 432–451

Berger, Lawrence M., and Jane Waldfogel, "Maternity Leave and the Employment of New Mothers in the United States," *Journal of Population Economics* 17(2) (2004), 331–349

Berger, Lawrence M., Hill, Jennifer, and Jane Waldfogel, "Maternity Leave, Early Maternal Employment and Child Health and Development in the US," *The Economic Journal* 115(501) (2005), F29–F47

Berger, Lawrence M., Brooks-Gunn, Jeane, Paxson, Christina and Jane Waldfogel, "First-Year Maternal Employment and Child Outcomes: Differences across Racial and Ethnic Groups," *Children and Youth Services Review* 30(4) (2008), 365–387

Bernal, Raquel and Michael P. Keane, "Child Care Choices and Children's Cognitive Achievement: The Case of Single Mothers," *Journal of Labor Economics* 29 (2011), 459–512

Blank, Rebecca M. and Jordan D. Matsudaira, "The Impact of Earnings Disregards on the Behavior of Low Income Families," *Journal of Policy Analysis and Management* 33(1) (2014), 7–35

Brookings Institution, “Welfare Reform and Beyond: The Future of the Safety Net,” Chapter 3. Problems and Issues for Reauthorization, *Brookings Institution Press* (2002)

Brooks-Gunn, Jeane, Chatterji, Pinka and Sara Markowitz, “Early Maternal Employment and Family Wellbeing,” *NBER Working Paper* 17212 (2011), 1–47

Byker, Tanya, “The Role of Paid Parental Leave in Reducing Women's Career Interruptions: Evidence from Paid Leave Laws in California and New Jersey,” *Working Paper* (2014)

Cancian, Maria, Noyes, Jennifer. L and Marci Ybarra, “The Extended TANF Application Period and Applicant Outcomes: Evidence from Wisconsin,” *Social Work Research* 36(4) (2012), 273–288

Cobb-Clark, Deborah A., Liu, Amy and Deborah Mitchell, “Reassessing the Role of Child Care Costs in the Work and Care Decisions of Australian Families,” *Australian Bulletin of Labour* 26(4) (2000), 279–297

DeBell, Megan, Yi, Hsiao-Ye and Heidi Hartmann, “Single Mothers, Jobs and Welfare: What the Data Tell Us,” Research-in-Brief series, Institute for Women's Policy Research, Washington, DC. (1997), 1–8

Ellwood, David, “The Impact of the Earned Income Tax Credit and Social Policy Reforms on Work, Marriage, and Living Arrangements,” *National Tax Journal* 53(2) (2000), 1063–1105

Fairlie, R.W. and London, R.A, “The Effect of Incremental Benefit Levels on Births to AFDC Recipients,” *Journal of Policy Analysis and Management* 16(4) (1997), 575–597

Falk, Gene, “TANF: Welfare-to-Work Revisited,” *Congressional Research Service* (2012), 1–77

Grogger, Jeffrey and Charles Michalopoulos, "Welfare Dynamics under Time Limits," *Journal of Political Economy* 111(3) (2003), 530–554

Gueron, Judith M., "Work and Welfare: Lessons on Employment Programs," *Journal of Economic Perspectives* 4(1) (1990), 79–98

Haider, Steven J., Jacknowitz, Alison and Robert F. Schoeni, "Welfare Work requirements and Child Well-Being: Evidence from the Effects on Breast-feeding," *Demography* 40(3) (2003), 479–497

Hao Lingxin and Andrew J. Cherlin, "Welfare Reform and Teenage Pregnancy, Childbirth, and School Dropout," *Journal of Marriage and Family* 66(1) (2004), 179–194

Herbst, Chris M., "Are Parental Welfare Work Requirements Good for Disadvantaged Children? Evidence from Age-of-Youngest-Child Exemptions," *IZA discussion paper* No. 8485 (2014), 1–49

Hill, Heather, "Welfare as Maternity Leave? Exemptions from Welfare Work Requirements and Maternal Employment," *Social Service Review* 86(1) (2012), 37–67

Hofferth, Sandra L., Stanhope, Stephen, and Kathleen M. Harris, "Exiting Welfare in the 1990s: Did Public Policy Influence Recipients' Behavior?" *Population Research and Policy Review* 21(5) (2002), 433–472

Hofferth, Sandra L., Stanhope, Stephen and Kathleen M. Harris, "Remaining Off Welfare in the 1990s: The Influence of Public Policy and Economic Conditions" *Social Science Research* 34(2) (2005), 426–453

Jacobson, Louis S., LaLonde, Robert J. and Daniel G. Sullivan, "Earnings Losses of Displaced Workers," *American Economic Review* 83(4) (1993), 685–709

Joyce, T., Kaestner, R., Korenman, S., and S. Henshaw, "Family Cap Provisions and Changes in Births and Abortions," *Population Research and Policy Review* 23 (2004), 475–511

Kaestner, Robert, Korenman, Sanders, and June O'Neill, "Has Welfare Reform Changed Teenage Behaviors?" *Journal of Policy Analysis and Management* 22(2) (2003), 225–248

Kauff , Jacqueline and Michelle Derr, "Achieving Higher TANF Work Participation Rates: Case Studies from Maryland and Utah: Strategies for Increasing TANF Work Participation Rates," Brief #4, Mathematica Policy Research, Princeton, NJ (2008), 1–16

Kaushal, Neeraj, and Robert Kaestner, "From Welfare to Work: Has Welfare Reform Worked?" *Journal of Policy Analysis and Management* 20(4) (2001), 699–719

Kaushal, Neeraj, and Robert Kaestner, "Welfare Reform and Health Insurance of Immigrants," *Health Services Research* 40(3) (2005), 697–721

Kaushal, Neeraj, Gao, Qin and Jane Waldfogel, "Welfare Reform and Family Expenditures: How Are Single Mothers Adapting to the New Welfare and Work Regime?" *Social Service Review* 81(3) (2007), 369–396.

Kearney, Melissa S., "Is There an Effect of Incremental Welfare Benefits on Fertility Behavior? A Look at the Family Cap," *The Journal of Human Resources* 39(2) (2004), 295–325

Mazzolari, Francesca, "Welfare Use when Approaching the Time Limit," *The Journal of Human Resources* 42(3) (2007), 596–618

McKernan, Signe-Mary, Lerman, Robert, Pindus, Nancy and Jesse Valente, "The Relationship Between Metropolitan and Non-Metropolitan Locations, Changing Welfare Policies, and the Employment of Single Mothers," Mimeo, The Urban Institute, Washington D.C. (2001), 1–46

Meyer, Bruce D., "Natural and Quasi-Experiments in Economics," *Journal of Business and Economic Statistics* 13(2) (1995), 151–161

Meyer, Bruce D., and Dan T. Rosenbaum, "Making Single Mothers Work: Recent Tax and Welfare Policy and its Effects," *National Tax Journal* 53(4), Part 2: The Earned Income Tax Credit (2000), 1027–1061

Meyer, Bruce D., and Dan T. Rosenbaum, "Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers," *Quarterly Journal of Economics* 116(3) (2001), 1063–1114

Moffitt, Robert A., "Welfare Programs and Labor Supply," *Handbook of Public Economics* Chapter 34 (2002), 2394−2430

Moffitt, Robert A., "Means-Tested Transfer Programs in the United States," *NBER Books, National Bureau of Economic Research, Inc*, number moff03-1. (2003)

Moffitt, Robert A., "Welfare Work Requirements with Paternalistic Government Preferences," *The Economic Journal* 116(515) (2006), F441−F458

O'Hara, Brian J, "Work and Work-Related Activities of Mothers Receiving Temporary Assistance to Needy Families: 1996, 1998 and 2000," *Current Population Report, U.S. Census Bureau* (2002), 1–12

Parrott, Sharon and Arloc Sherman, "TANF AT 10: Program Results are More Mixed than Often Understood," *Center on Budget and Policy Priorities Report* (2006), 1–16

Pavetti, LaDonna, "The Challenge of Achieving High Work Participation Rates in Welfare Programs," *The Brookings Institution Policy Brief* (2004), 1–8

Peoples-Sheps, Mary, D., Siegel, Earl, Suchindran, Chirayath M., Origasa, Hideki, Ware, Andrea and Ali Barakat, "Characteristics of Maternal Employment during Pregnancy: Effects on Low Birth Weight," *American Journal of Public Health* 81(8) (1991), 1007–1012

Pingle, Jonathan F., "What if Welfare had no Work Requirements? The Age of Youngest Child Exemption and the Rise in Employment of Single Mothers," *Finance and Economics Discussion Series 2003-57,* Board of Governors of the Federal Reserve System (2003), 1–25

Ribar, David C., "A Structural Model of Child Care and the Labor Supply of Married Women," *Journal of Labor Economics* 13(3) (1995), 558–597

Ruhm, Christopher J., "Policy watch: The Family and Medical Leave Act," *Journal of Economic Perspectives* 11(3) (1997), 175–186

Schoeni, Robert, and Rebecca M. Blank, "What has Welfare Reform Accomplished? Impacts on Welfare Participation, Employment, Income, Poverty, and Family Structure," *NBER Working Paper* 7627 (2000), 1–43

Schott, Liz and LaDonna Pavetti, "Changes in TANF Work Requirements Could Make Them More Effective in Promoting Employment," *Center on Budget and Policy Priorities* (2013), 1–9

Smith, Rebekah J., "Family Caps in Welfare Reform: Their Coercive Effects and Damaging Consequences," *Harvard Journal of Law and Gender* 29(1) (2006), 151–200

U.S. Department of Health and Human Services. "Temporary Assistance for Needy Families (TANF) Program – Annual Report to Congress" series (1st to 10th report)

U.S. Department of Labor. "FMLA survey: Balancing the needs of families and employers," Retrieved from http://www.dol.gov/asp/fmla/toc.htm (2001)

Washbrook, Elizbeth, Ruhm, Christopher J., Waldfogel, Jane and Han, Wei-Jui, "Public Policies, Women's Employment after Child-bearing, and Child Well-Being," *Berkeley Electronic Journal of Economic Analysis and Policy* 11(1) (2011), 1–50

Ybarra, Marci, "Implications of Paid Family Leave for Welfare Participants," *Social Work Research* 37(4) (2013), 375–387

Ybarra, Marci, "Work-exempt TANF participants," *University of Wisconsin-Madison, Institute for Research on Poverty, Focus* 31(1) Spring/Summer (2014), 10–14

Yelowitz, Aaron S., "The Medicaid Notch, Labor Supply, and Welfare Participation: Evidence from Eligibility Expansions," *The Quarterly Journal of Economics* 110(4) (1995), 909–939

Table 1.1: Distribution of TANF Recipients by Work Participation Status

(1)	(2)	(3)		(4)	(5)
Fiscal Year	Work Required	Disregarded[1]		Exempt[2]	Deemed engaged in work
		Sanction[3]/ Tribal work[4]	Child under 12 months	Disabled, in poor health or pregnant	Single parents with child under age 6/ Teen parents[5]
FY98	58.3	3.3	8.4	16.8	12.2
FY99	55.2	3.5	9.2	13.9	17.2
FY03	61.7	6.1	8.6	12.9	10.7
FY07	60.6	3.3	11.7	13.5	9.7
FY09	61.6	1.5	11.1	12.6	11.0
FY10	59.9	1.8	10.4	14.6	11.2
FY11	59.1	1.9	9.8	14.9	11.8

Notes : 1) "Disregarded" means that the TANF family is not included in the calculation of the work participation rate; 2) "Exempt" means that the individual will not be penalized for failure to engage in work (i.e., good cause exception); 3) Benefits are reduced or terminated for families under sanction as a penalty, thus they are disregarded from the calculation of work participation rates until the sanction is removed; 4) Work participation rates are not applicable to tribes; 5) Single parents with a child under age 6 engaged in work activities for at least 20 hours per week and teen parents who participate in education are deemed engaged in work

Source: TANF Annual Report to Congress, Administration for Children and Families (ACF) Archives, U.S. Department of Health and Human Services

FY98: 2nd Annual Report to Congress Table 9:19 (http://archive.acf.hhs.gov/programs/ofa/data-reports/annual2/tan19995.pdf)

FY99: 3rd Annual Report to Congress Table 10:19 (http://archive.acf.hhs.gov/programs/ofa/data-reports/annual3/annual3.pdf)

FY03: 7th Annual Report to Congress Table 10:28 (http://archive.acf.hhs.gov/programs/ofa/data-reports/annualreport7/Appendix/TANF_7th_Report_Appendix.pdf)

FY07: 9th Annual Report to Congress Table 10:28 (http://www.acf.hhs.gov/sites/default/files/ofa/ar9appendix.pdf)

FY09: Characteristics and Financial Circumstances of TANF Recipients, Fiscal Year 2009, Table 27 (http://www.acf.hhs.gov/programs/ofa/resource/character/fy2009/tab27)

FY10: Characteristics and Financial Circumstances of TANF Recipients, Fiscal Year 2010, Table 27 (http://www.acf.hhs.gov/sites/default/files/ofa/appendix_ys_final.pdf)

FY11: Characteristics and Financial Circumstances of TANF Recipients, Fiscal Year 2011, Table 27 (http://www.acf.hhs.gov/sites/default/files/ofa/appendix_fy2011_final_amend.pdf)

Table 1.2: Triple-Difference Estimates of the Pregnancy Exemption on Labor Force Participation Around Childbirth

Month-relative-to-birth	7 months before birth	6 months before birth	5 months before birth	4 months before birth	3 months before birth	2 months before birth	1 month before birth	month of birth
Panel A: Months Before Birth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DDD Estimates	-0.005	-0.007	0.025	0.069	0.070	0.104**	0.103**	0.117***
	(0.042)	(0.044)	(0.044)	(0.045)	(0.043)	(0.044)	(0.043)	(0.044)
Mean of Labor Force Participation (%)	66.7	65.0	63.4	61.4	58.8	55.7	52.7	50.7
Month-relative-to-birth	1 month after birth	2 months after birth	3 months after birth	4 months after birth	5 months after birth	6 months after birth	7 months after birth	8 months after birth
Panel B: Months after Birth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DDD Estimates	0.112***	0.079*	0.06	0.098**	0.038	0.038	0.000	0.02
	(0.043)	(0.044)	(0.046)	(0.045)	(0.049)	(0.048)	(0.049)	(0.050)
Mean of Labor Force Participation (%)	51.6	52.6	53.6	55.0	55.6	56.1	56.6	56.8

Notes: These are the monthly DDD estimates in Figure 1.8.2 corresponding to θ_t in equation (2). Each column in Panel A indicates the estimated impact of the pregnancy exemption on women's labor force participation for each month before birth. Each column in Panel B indicates the effects for each month after birth. Year and state fixed effects as well as state-level characteristics are controlled for. The end-of-survey SIPP sampling weights are used for each mother-birth level observation. Standard errors are clustered by mother-birth.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.3: Joint Test of Significance for Impact of the Pregnancy Exemption on Women's Labor Force Participation

	During Pregnancy -9 to -3 (1)	Around Birth -2 to 2 (2)	After Birth 3 to 12 (3)
Average DDD	0.022	0.103*	0.049
p-value of Joint Test	0.261	0.100	0.196

Notes: The table 1.3 summarizes the monthly DDD results in Figure 1.8.2 by the periods around birth.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.4: Triple-Difference Estimates of the AYC Exemption on Labor Force Participation Around Childbirth

Month-relative-to-birth	7 months before birth	6 months before birth	5 months before birth	4 months before birth	3 months before birth	2 months before birth	1 month before birth	month of birth
Panel A: Months Before Birth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DDD Estimates	0.085	0.022	-0.005	-0.015	-0.024	-0.065	-0.110**	-0.085
	(0.061)	(0.058)	(0.058)	0.058	0.057	(0.062)	(0.056)	(0.056)
Mean of Labor Force Participation (%)	66.7	65.0	63.4	61.4	58.8	55.7	52.7	50.7
Month-relative-to-birth	1 month after birth	2 months after birth	3 months after birth	4 months after birth	5 months after birth	6 months after birth	7 months after birth	8 months after birth
Panel B: Months after Birth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DDD Estimates	0.003	0.051	0.069	0.124**	0.150**	0.081	0.125**	0.137**
	(0.054)	(0.054)	(0.055)	(0.057)	(0.063)	0.063	(0.061)	(0.060)
Mean of Labor Force Participation (%)	51.6	52.6	53.6	55.0	55.6	56.1	56.6	56.8

Notes: These are the monthly DDD estimates in Figure 1.12.2 corresponding to θ_t in equation (2). Each column in Panel A indicates the estimated impact of the AYC exemption on women's labor force participation for each month before birth. Each column in Panel B indicates the effects for each month after birth. Year and state fixed effects as well as state-level characteristics are controlled for. The end-of-survey SIPP sampling weights are used for each mother-birth level observation. Standard errors are clustered by mother-birth.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Joint Test of Significance for Impact of the AYC Exemption on Women's Labor Force Participation

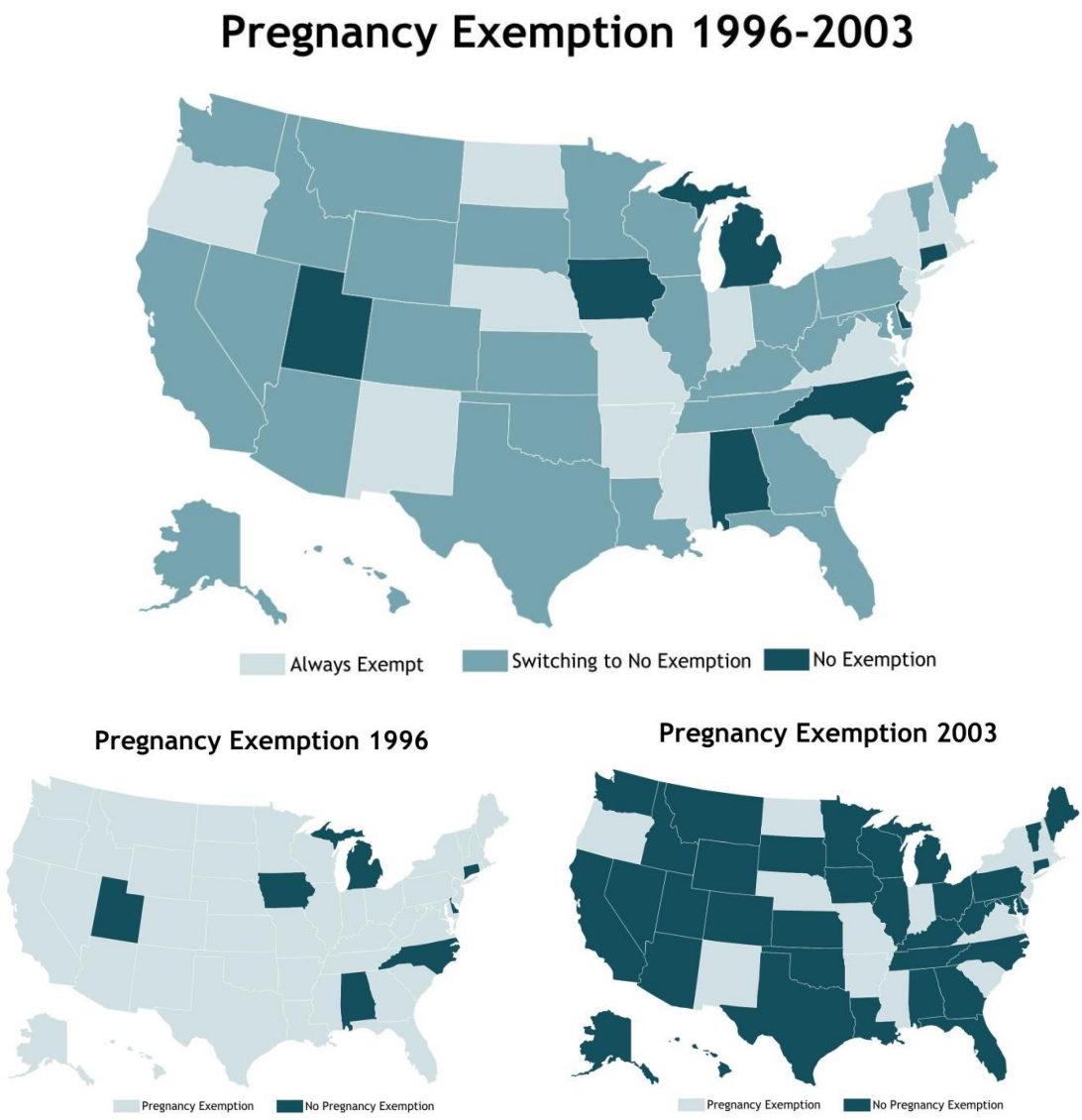
	During Pregnancy -9 to -3 (1)	Around Birth -2 to 2 (2)	After Birth 3 to 12 (3)
Average DDD	0.015	-0.041***	0.128***
p-value of Joint Test	0.430	0.000	0.001

Notes: The table 1.5 summarizes the monthly DDD results in Figure 1.12.2 by the periods around birth.
*** p<0.01, ** p<0.05, * p<0.1

Figure 1.1 Labor Force Participation of Women with Children under 18: 1975−2005

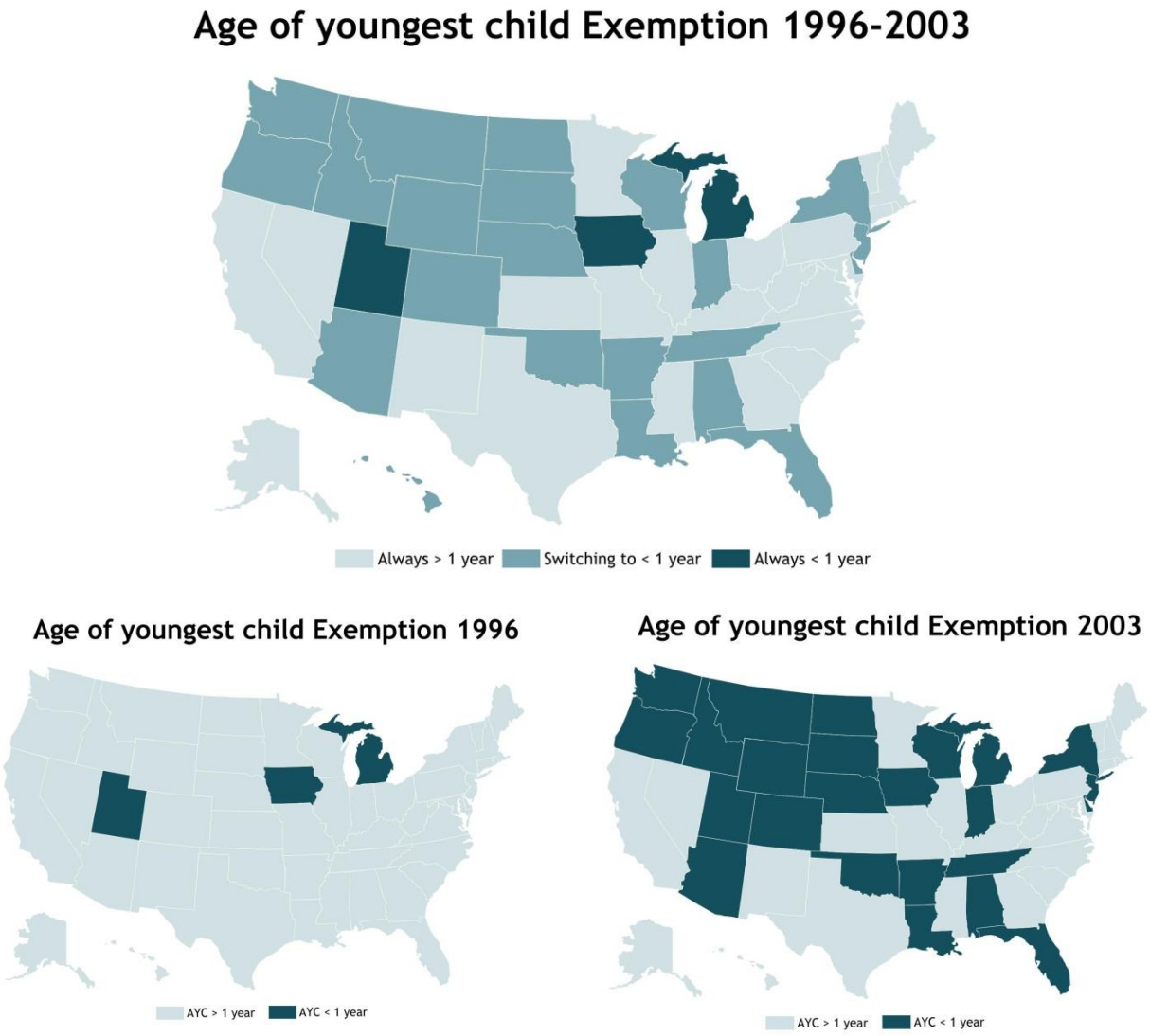
Source: U.S. Department of Labor, Bureau of Labor Statistics, unpublished tabulations from the Current Population Survey, Annual Social and Economic Supplement, 1976−2006.
Notes: This figure is an excerpt from the Annual Report to Congress 2005, chapter 3. Predictors and Risk Factors Associated with Welfare Receipt (http://aspe.hhs.gov/hsp/indicators05/ch3.htm)

Figure 1.2 Cross-State and Within-State Variation in Pregnancy Exemption



Pregnancy exemption 1996-2003	Always lenient (Pregnancy exemption exists)	Always strict (No pregnancy exemption exists)	Lenient to strict (Eliminated pregnancy exemption)
Number of States	15	7	29

Figure 1.3 Cross-State and Within-State Variation in AYC Exemption



AYC Exemption 1996-2003	Always lenient (≥ 12 months)	Always strict (< 12months)	Lenient to strict
Number of States	26	3	22

Figure 1.4 Budget Constraints Prior to Work Requirements: Pre- and Post-Birth

Figure 1.4.1 Childcare costs: Variable cost (*c*) Only

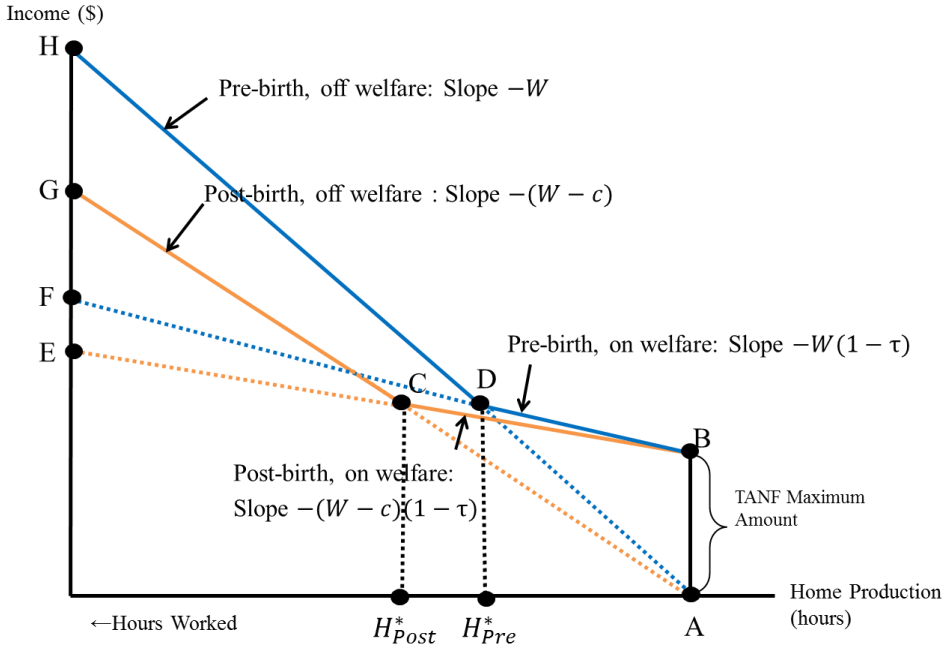


Figure 1.4.2 Childcare costs: Variable cost (*c*) and Fixed cost (*F*)

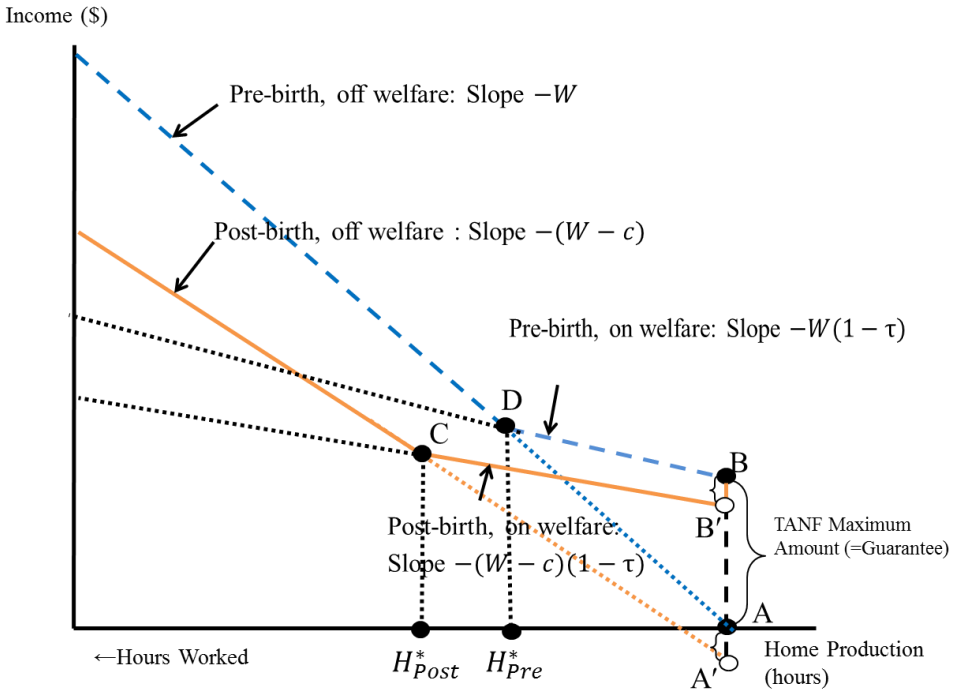


Figure 1.5 Single Mothers' Labor Supply Responses to the Introduction of Work Requirements

Figure 1.5.1 Pre-Birth, no work before work requirements

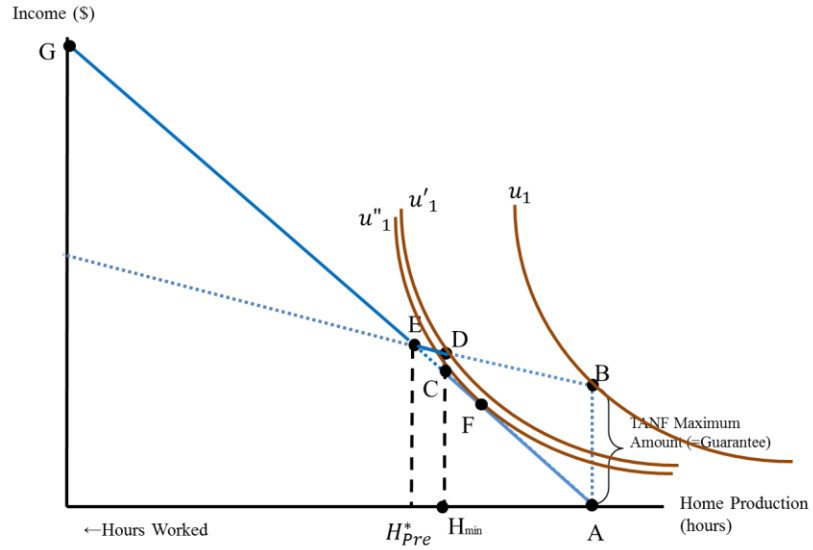


Figure 1.5.2 Pre-Birth, work before work requirements

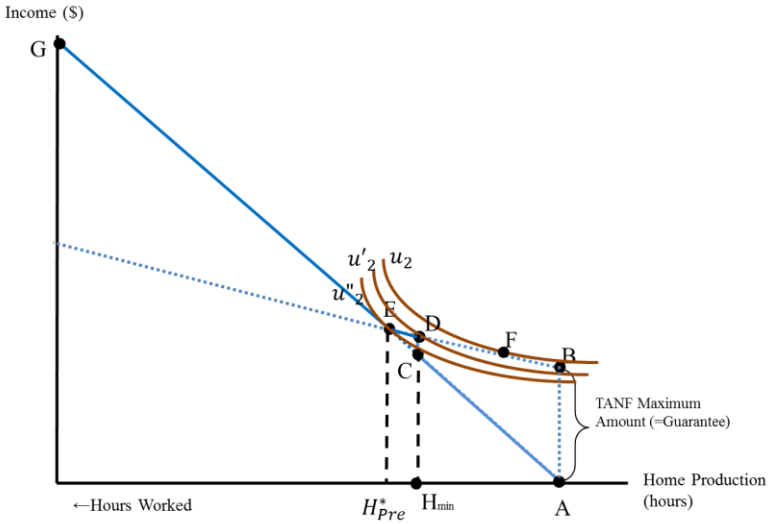


Figure 1.5.3 Post-Birth, no work before work requirements, VC only

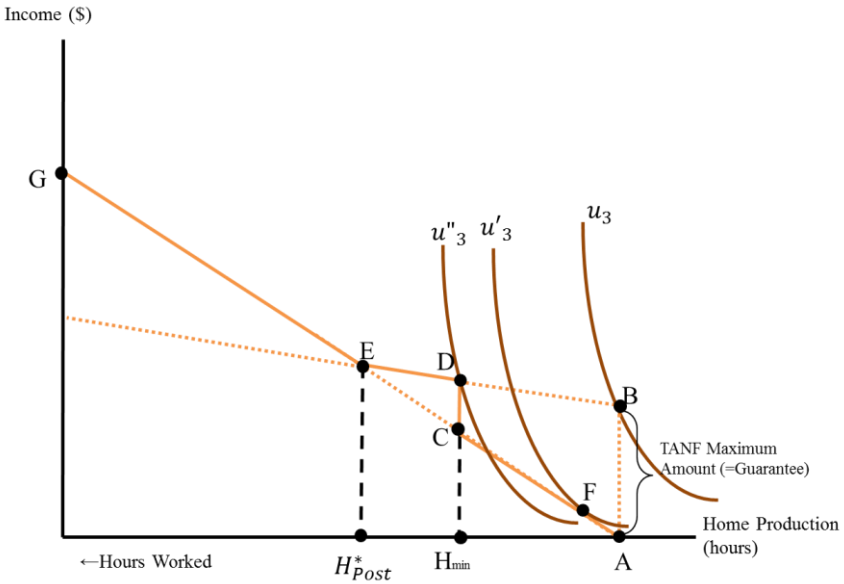


Figure 1.5.4 Post-Birth, no work before work requirements, FC+VC

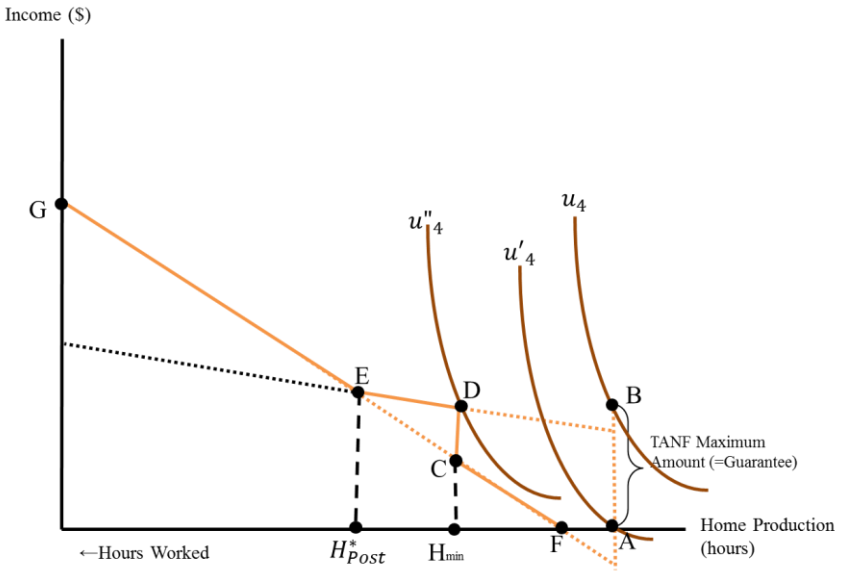
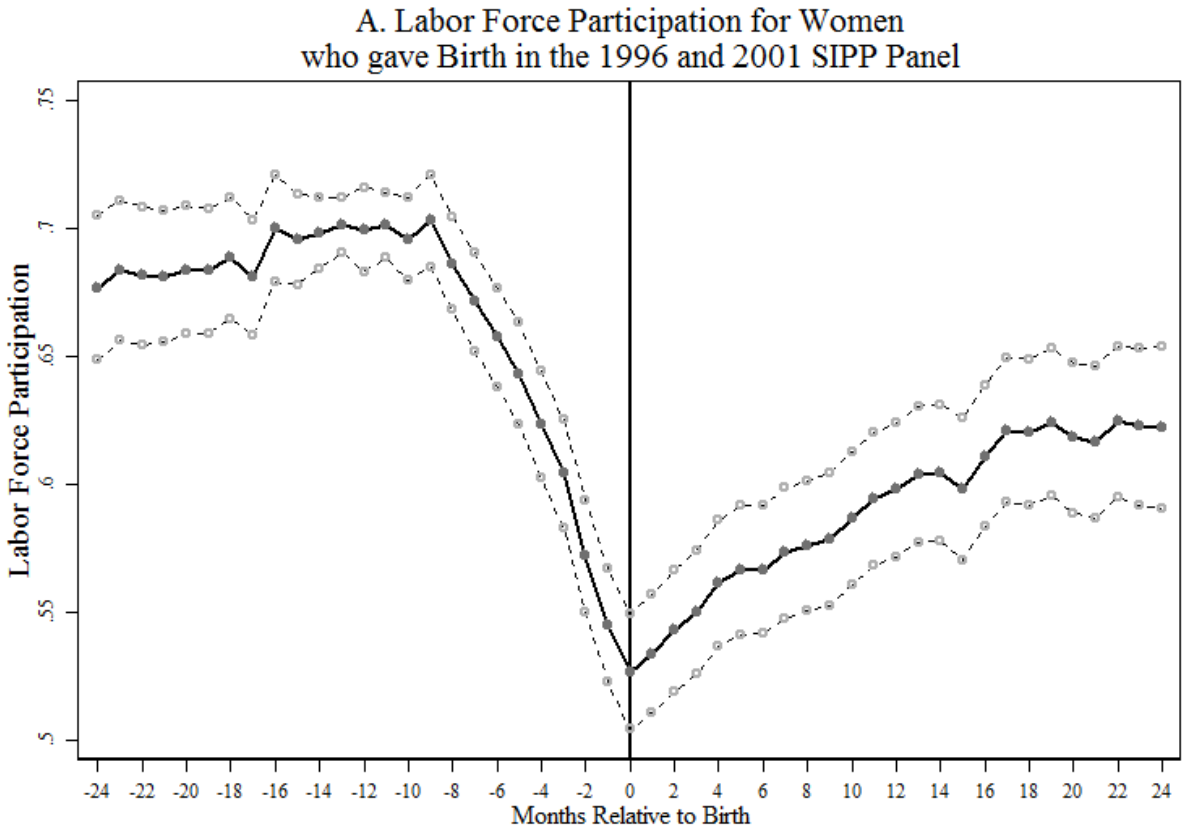
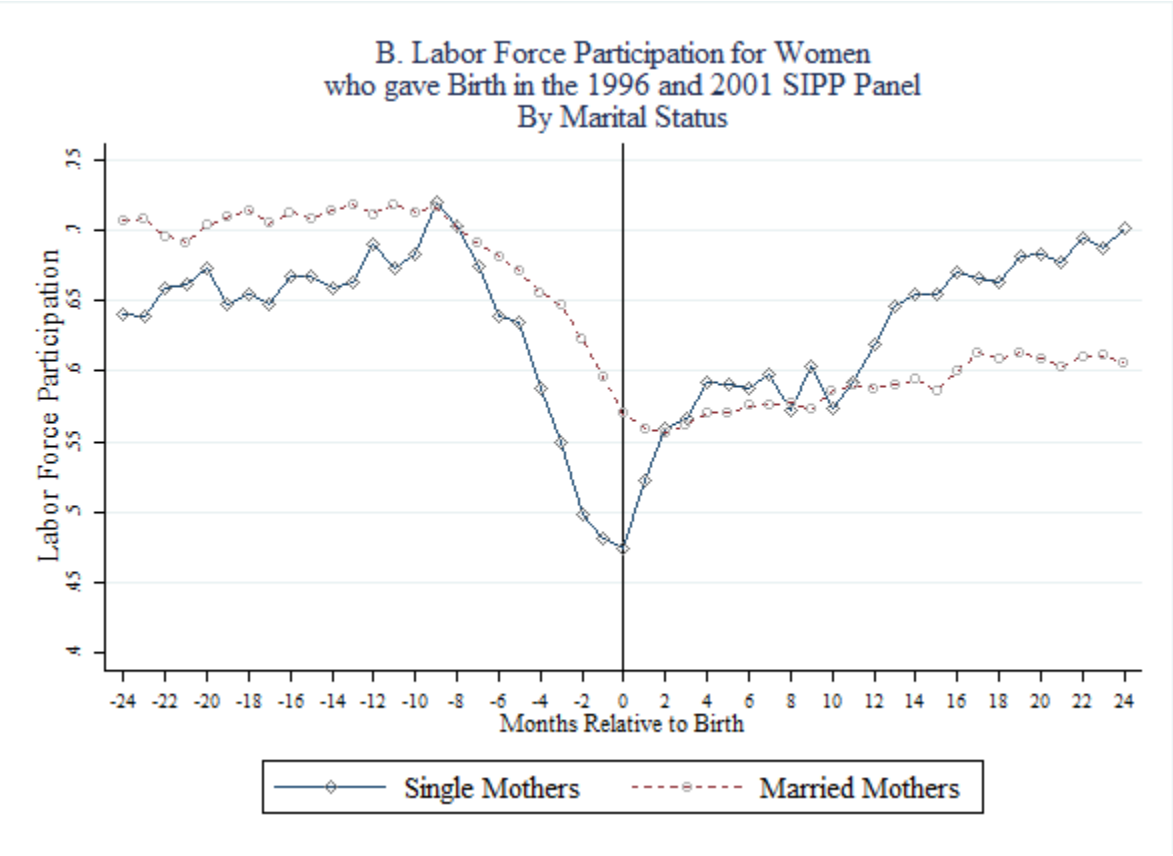


Figure 1.6 Descriptive Patterns in Women's Labor Force Participation



Note: In Figure 1.6 Panels A, B, and C, I plot coefficients on month-relative-to-birth dummies added back to the average level of labor force participation one year prior to birth – these are the δ_t coefficients added back to the level of labor force participation in the omitted period (j= –12) from estimating equation (3) as described in section 1.5.4. The end-of-survey SIPP sampling weights are used for each mother-birth level observation. Standard errors are clustered by mother-birth. Panel A is produced by replicating Byker (2014). In Panel B and C, single mothers are unmarried, divorced, widowed, or separated women for all months before and after childbirth and reside with no senior members or partners in the household. Married mothers are married for all months before and after childbirth.



C. Labor Force Participation for Women who gave Birth in the 1996 and 2001 SIPP Panel by Marital Status and by Birth Order

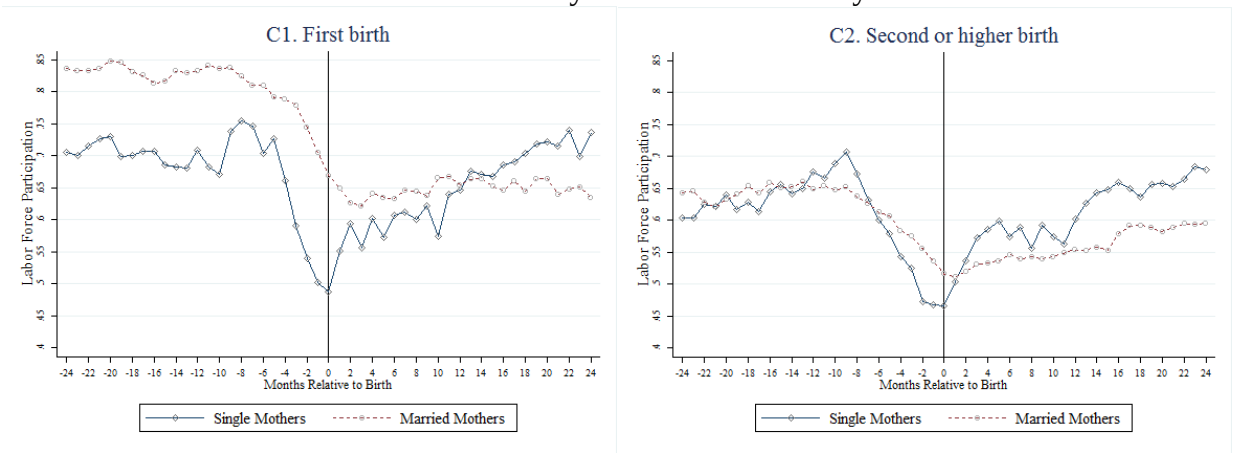
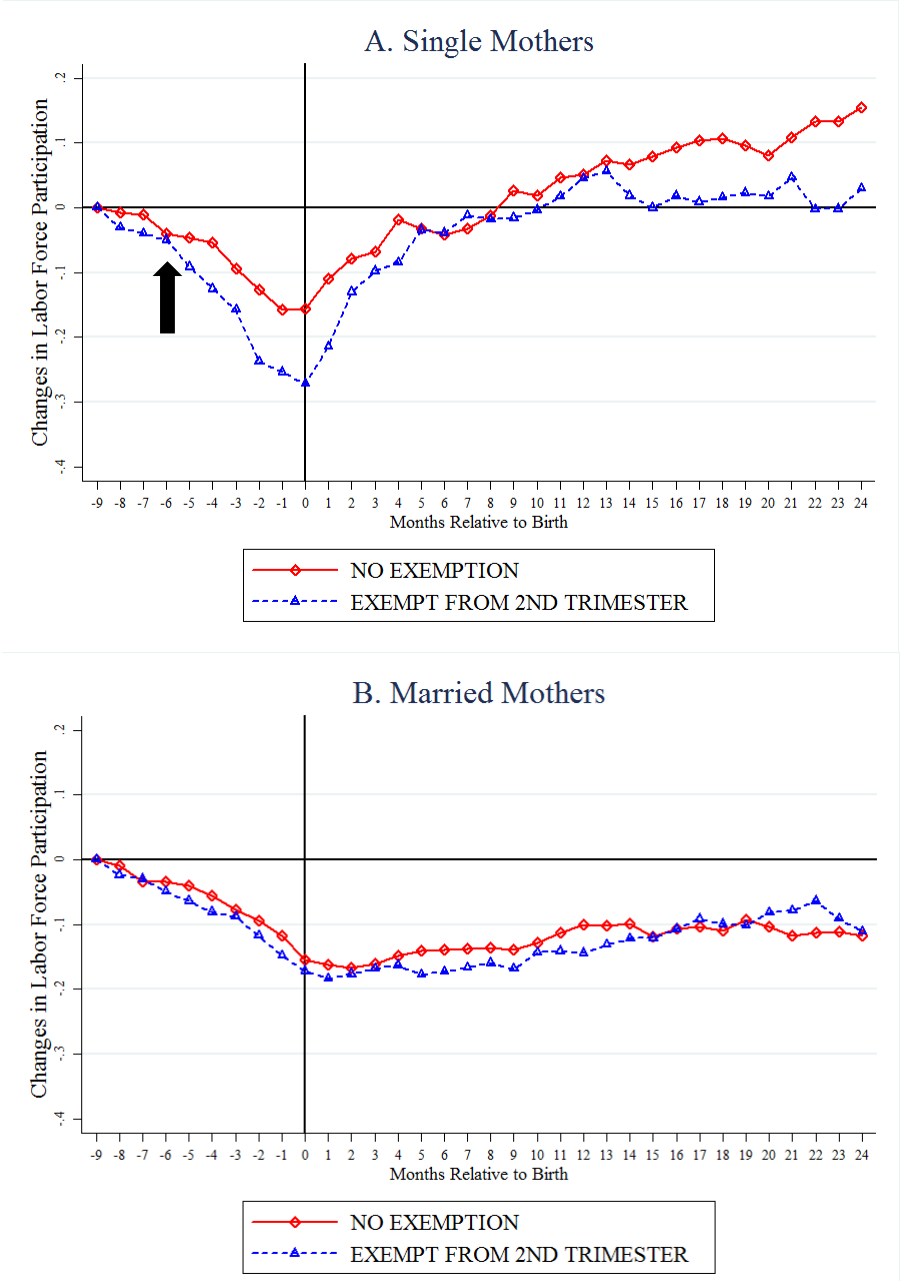


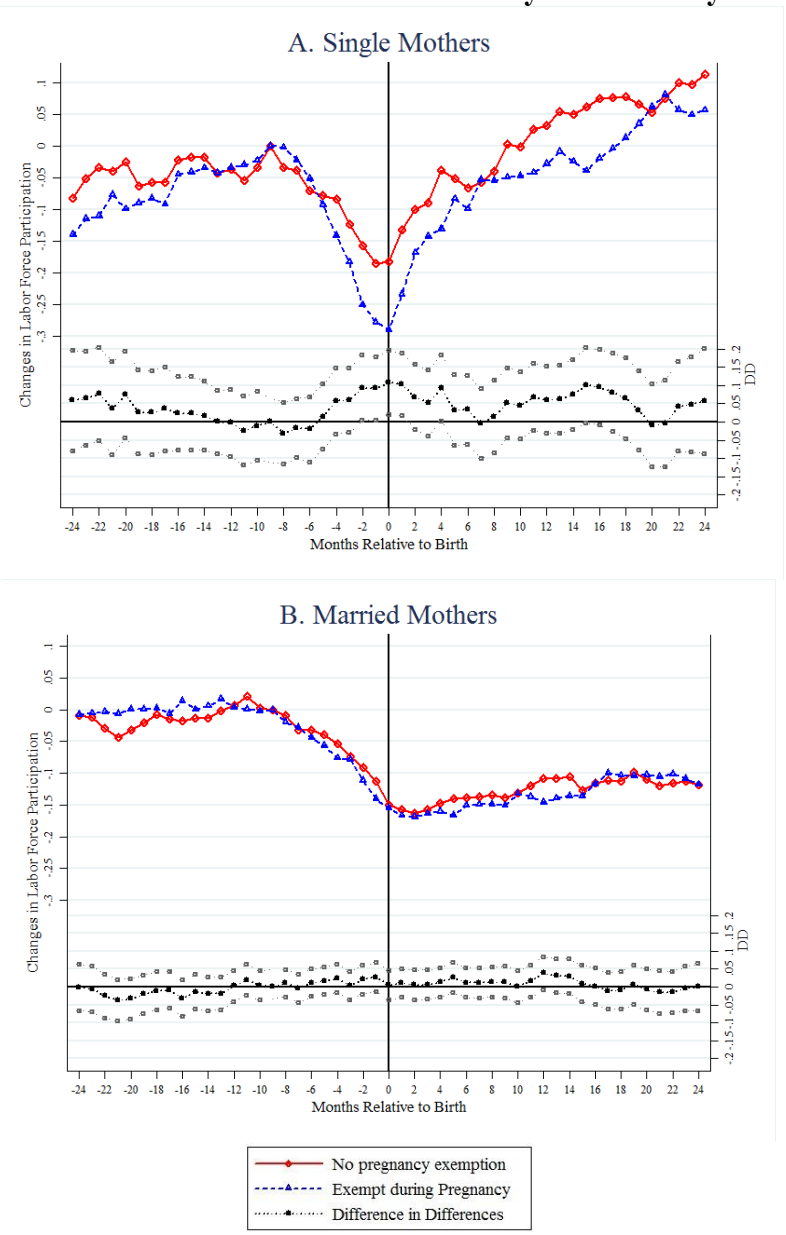
Figure 1.7 Labor Force Participation by the Strictness of Pregnancy Exemption



Note: Panels A and B in Figure 1.7 show the *changes* in labor force participation relative to 9 months prior to birth. With month –9 omitted, the level of labor force participation is normalized to zero at –9. Year and state fixed effects as well as state-level characteristics are controlled for in all specifications. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth.

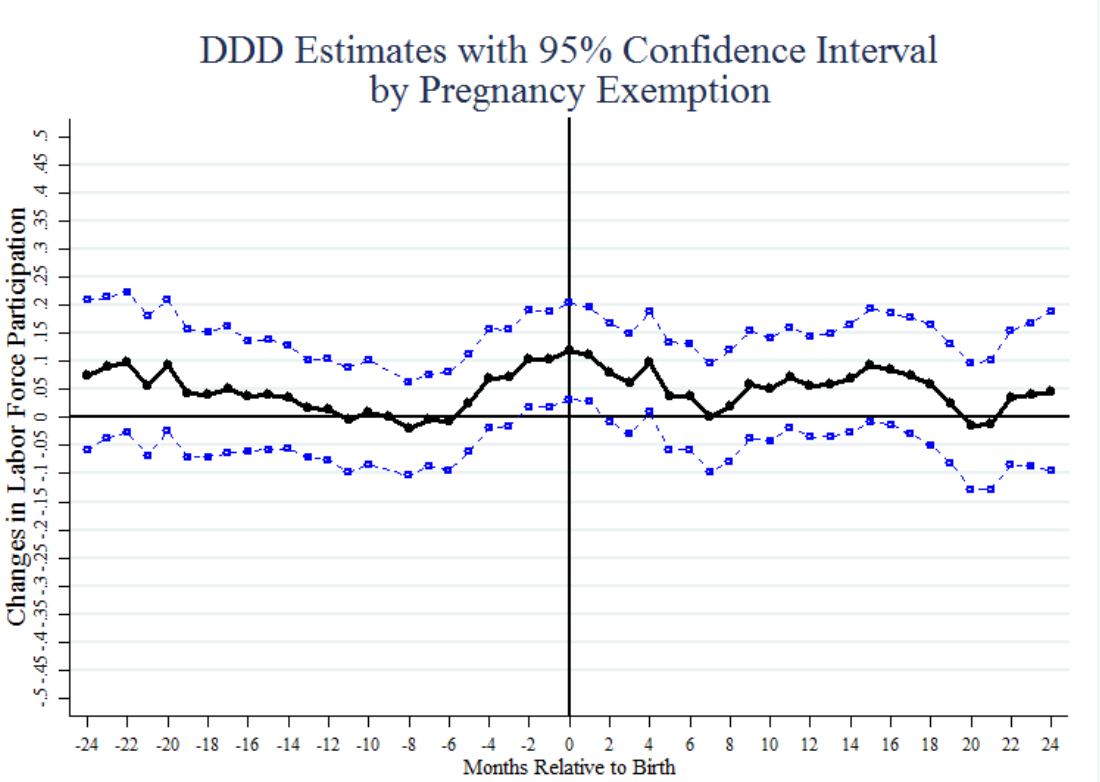
Figure 1.8 Impact of the Pregnancy Exemption on Labor Force Participation

Figure 1.8.1 Difference-in-Differences Event Study Estimates by Marital Status



Note: Panels A and B in Figure 1.8.1 show the *changes* in labor force participation relative to 9 months prior to birth separately by the strictness of the pregnancy exemption. With month –9 omitted, the level of labor force participation is normalized to zero at –9. These are the π_t coefficients from estimating equation (1) with the dependent variable being an indicator for participation in the labor force. The regression includes year and state fixed effects as well as state-level characteristics as described in section 1.4.1. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth. The dotted line at the bottom plots the monthly differences in labor force participation between strict and lenient states. This provides the DD estimates of the impact of no pregnancy exemption on labor force participation.

Figure 1.8.2 Triple-Difference Event Study Estimates for Labor Force Participation



Note: Figure 1.8.2 plots the DDD estimates, θ_t from estimating equation (2), which are the coefficients on the interaction between month-relative-to-birth, an indicator for strict pregnancy policy, and an indicator for single mothers. The dependent variable is an indicator for participation in the labor force. The regression includes year and state fixed effects as well as state-level characteristics as described in section 1.4.2. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth. Table 1.2 provides a summary of these results with the point estimates and significance levels for each month during the 7 months before and 8 months after birth.

Figure 1.9 Impact of the Pregnancy Exemption on Mothers' Employment

Figure 1.9.1 Difference-in-Differences Event Study Estimates for Single Mothers

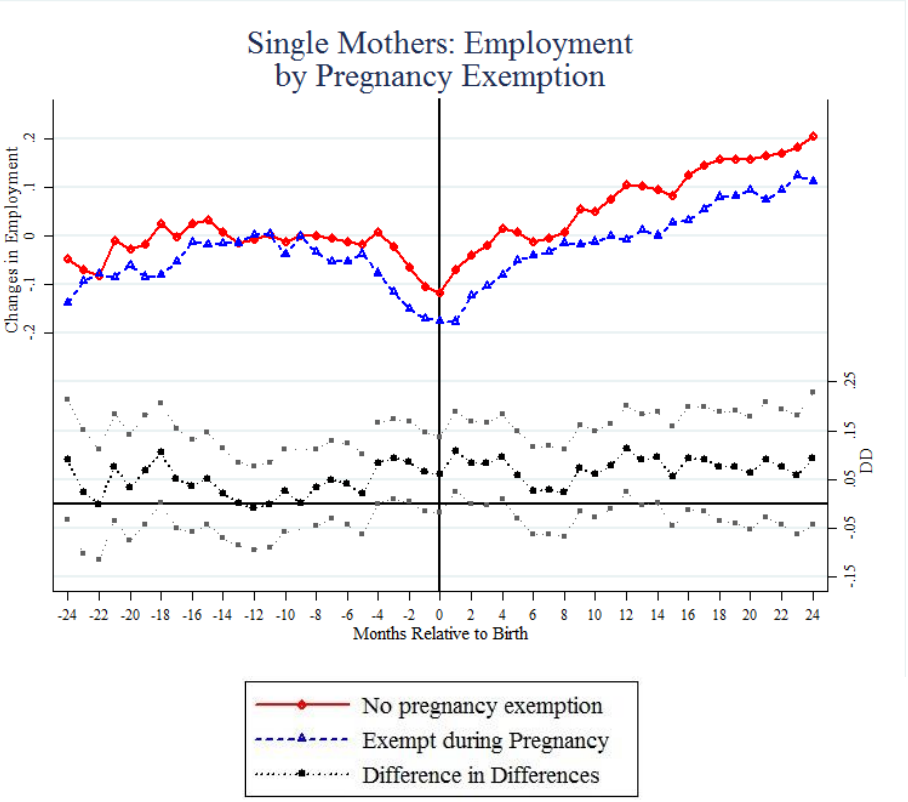


Figure 1.9.2 Triple Difference Event Study Estimates for Employment

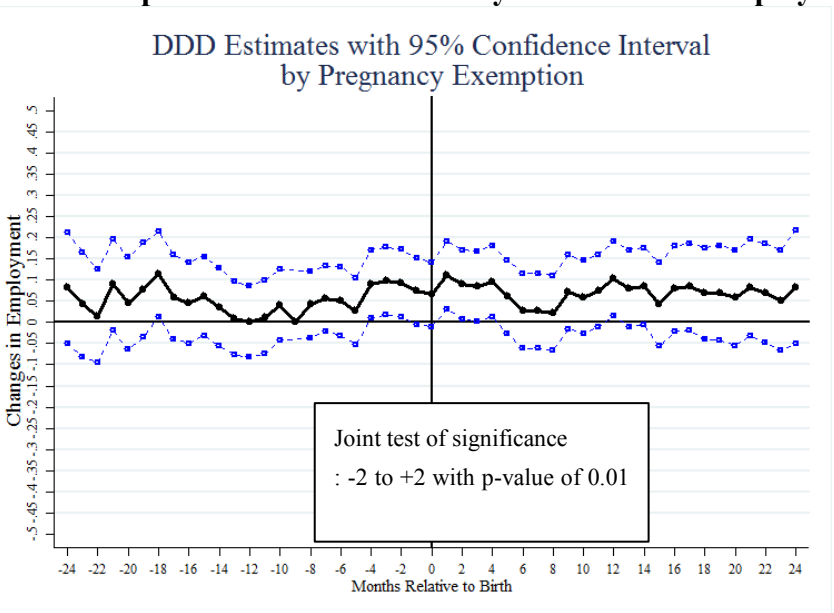
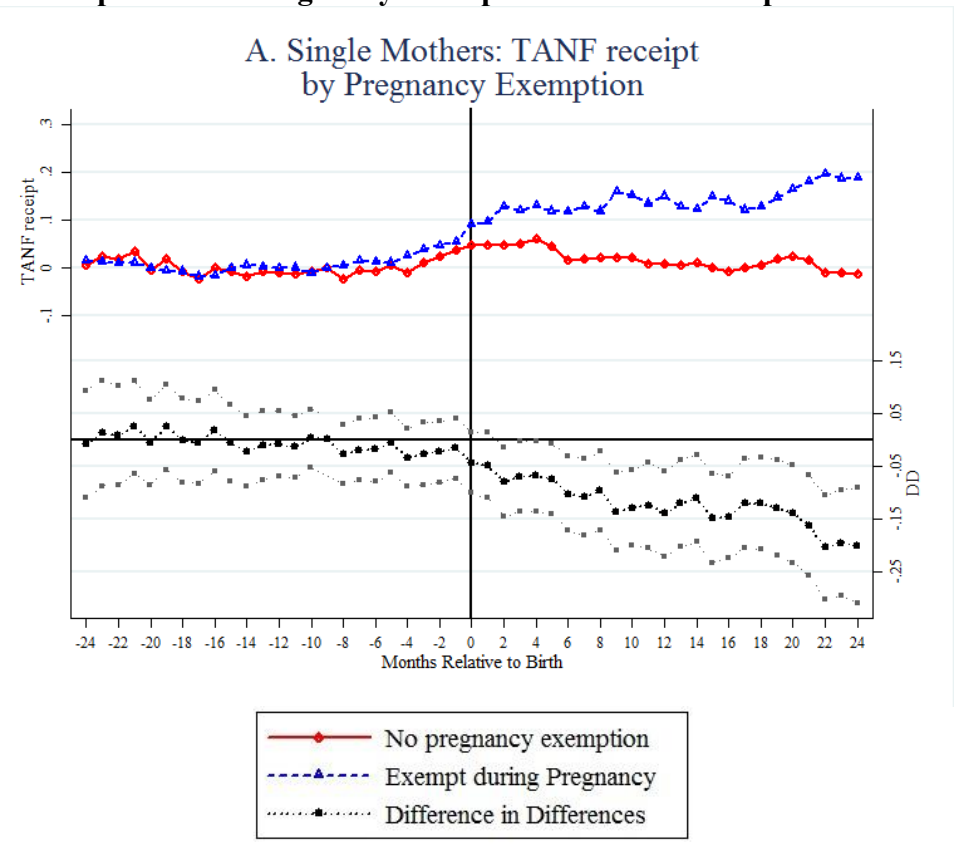


Figure 1.10 Impact of the Pregnancy Exemption on TANF receipt



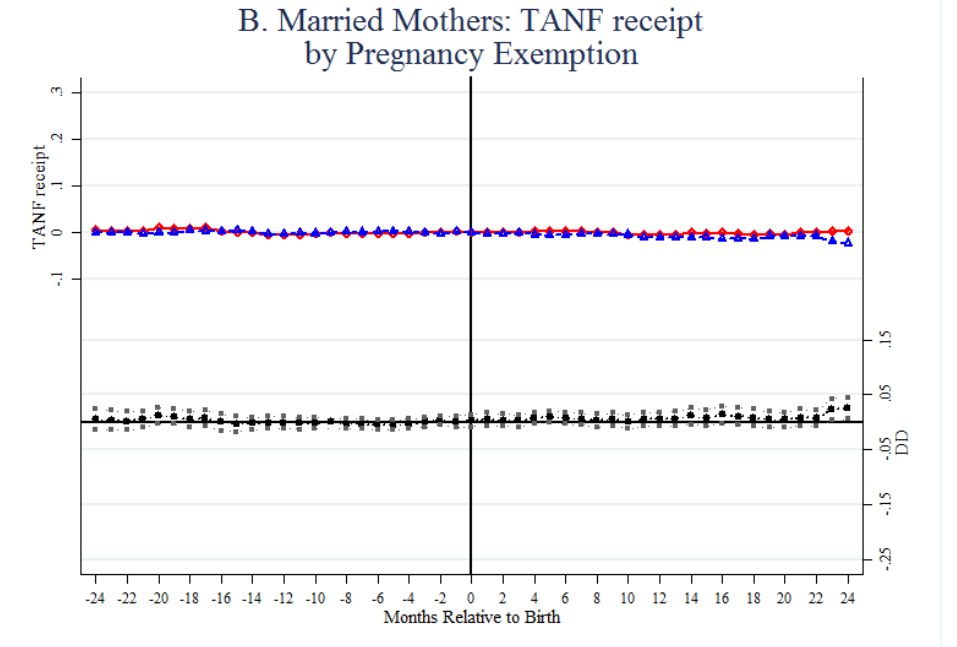
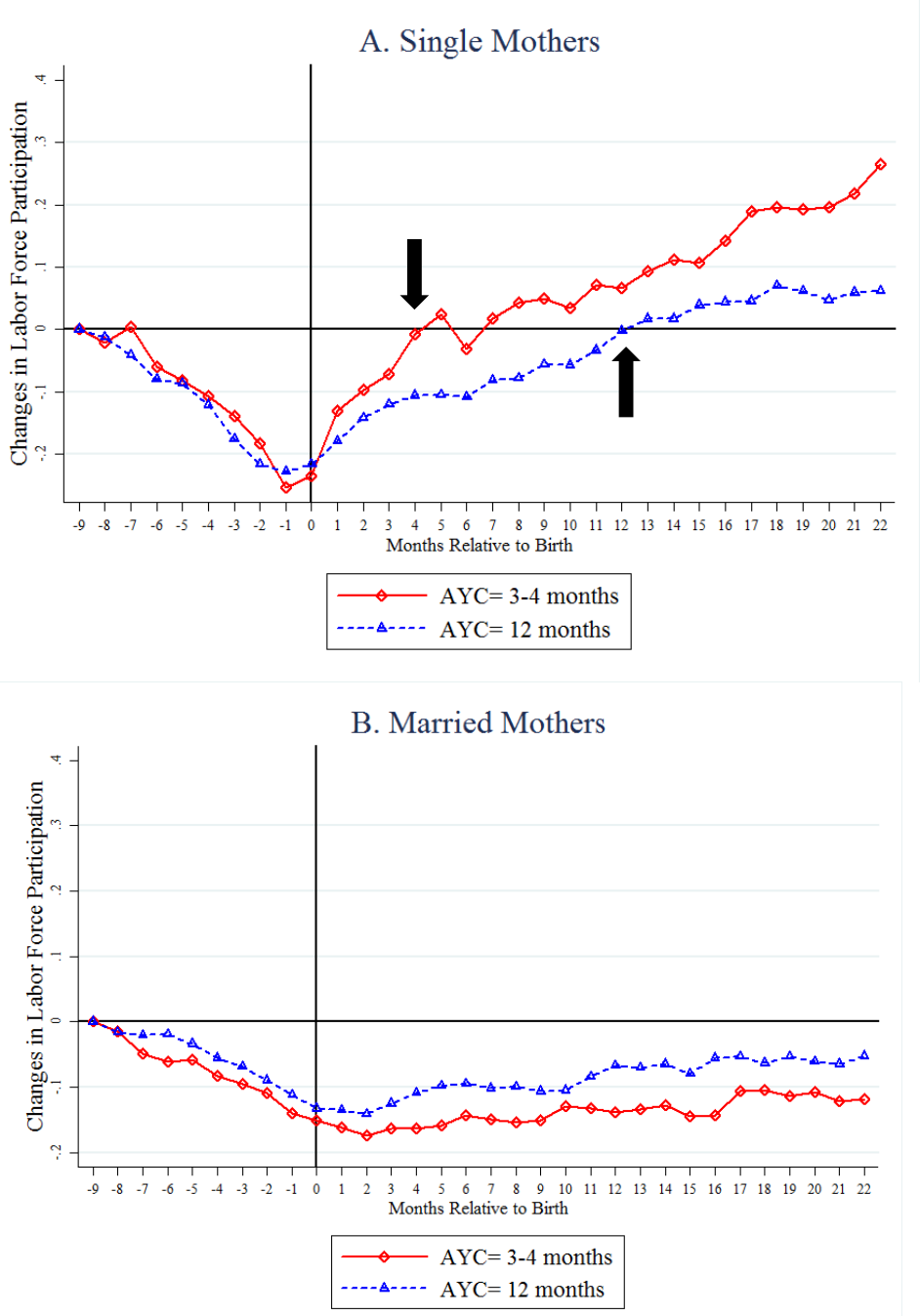


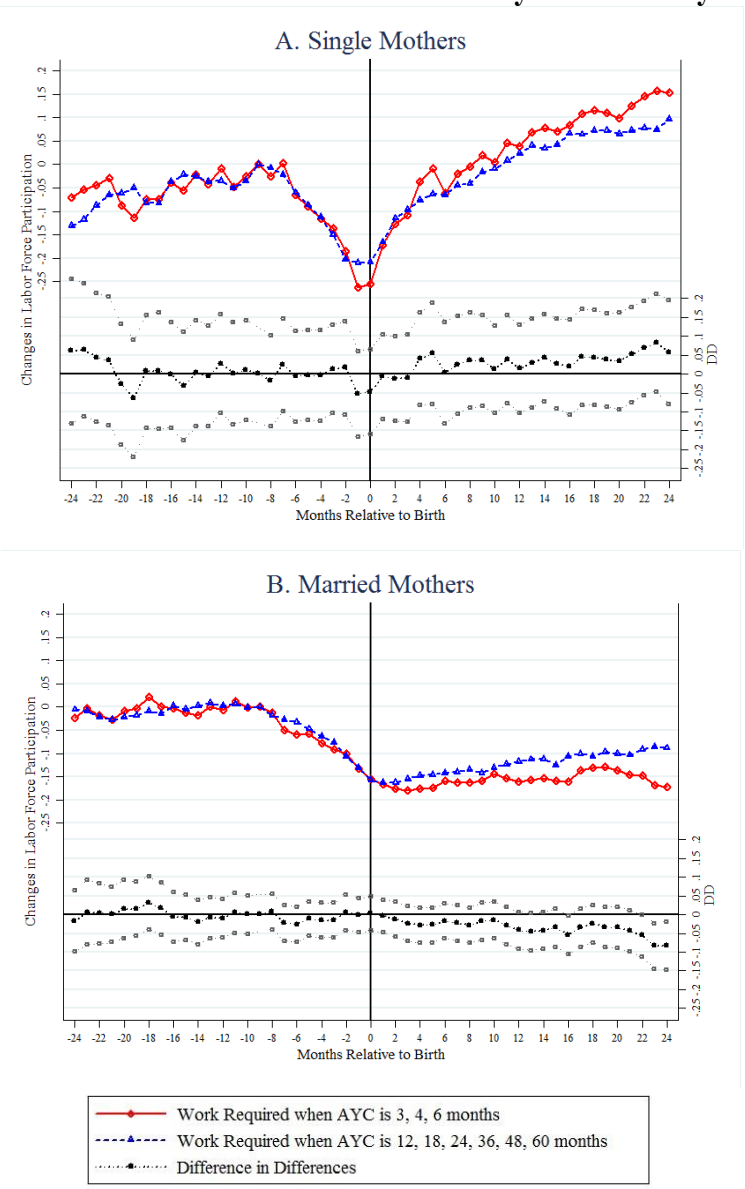
Figure 1.11 Labor Force Participation by the Strictness of AYC Exemption Policy



Note: Panels A and B in Figure 1.11 show the *changes* in labor force participation relative to 9 months prior to birth. With month –9 omitted, the level of labor force participation is normalized to zero at –9. Year and state fixed effects as well as state-level characteristics are controlled for in all specifications. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth.

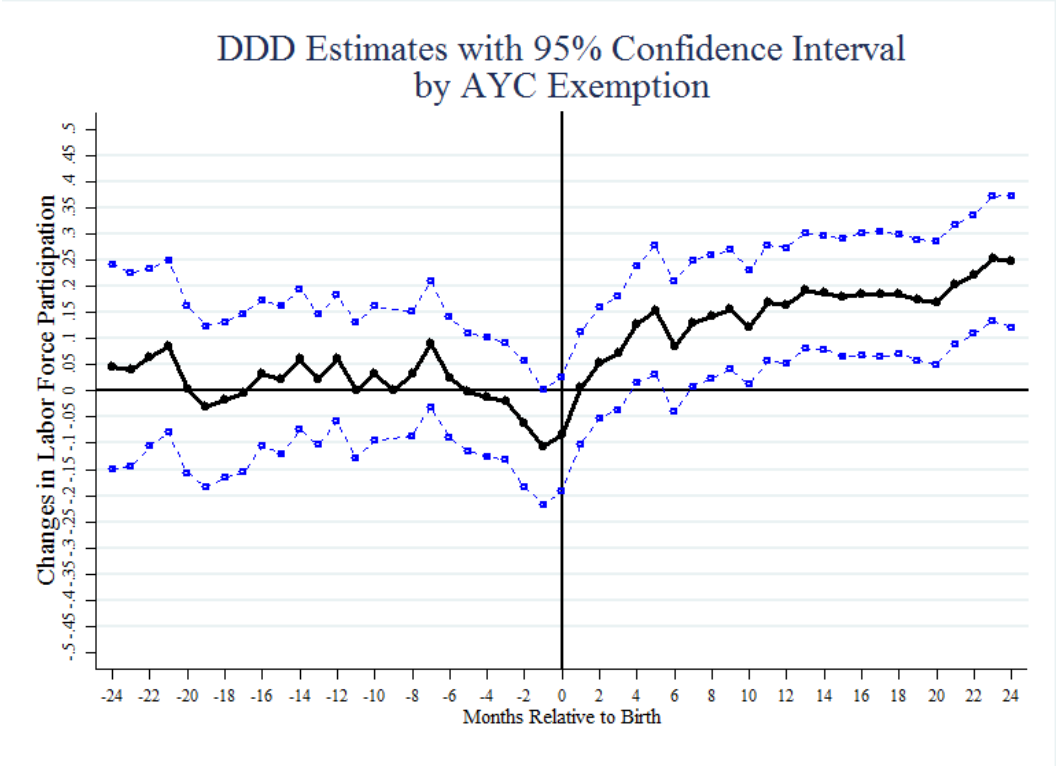
Figure 1.12 Impact of the AYC Exemption on Mothers' Labor Force Participation

Figure 1.12.1 Difference-in-Differences Event Study Estimates by Marital Status



Note: Panels A and B in Figure 1.12.1 show the *changes* in labor force participation relative to 9 months prior to birth, separately by the strictness of the AYC exemption. With month –9 omitted, the level of labor force participation is normalized to zero at –9. These are the π_t coefficients from estimating equation (1) with the dependent variable being an indicator for participation in the labor force. The regression includes year and state fixed effects as well as state-level characteristics as described in section 1.4.1. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth. The dotted line at the bottom plots the monthly differences in labor force participation between strict and lenient states. This provides the DD estimates of the impact of a strict AYC exemption on labor force participation.

Figure 1.12.2 Triple Difference Event Study Estimates for Labor Force Participation



Note: Figure 1.12.2 plots the DDD estimates, θ_t from estimating equation (2), which are coefficients on the interaction between the month-relative-to-birth, an indicator for AYC strict policy, and an indicator for single mothers. The dependent variable is an indicator for participation in the labor force. The regression includes year and state fixed effects as well as state-level characteristics as described in section 1.4.2. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth. Table 1.4 provides a summary of these results with the point estimates and significance levels for each month during the 7 months before and 8 months after birth.

Figure 1.13 Impact of the AYC Exemption on Mothers' Employment

Figure 1.13.1 Difference-in-Differences Event Study Estimates for Single Mothers

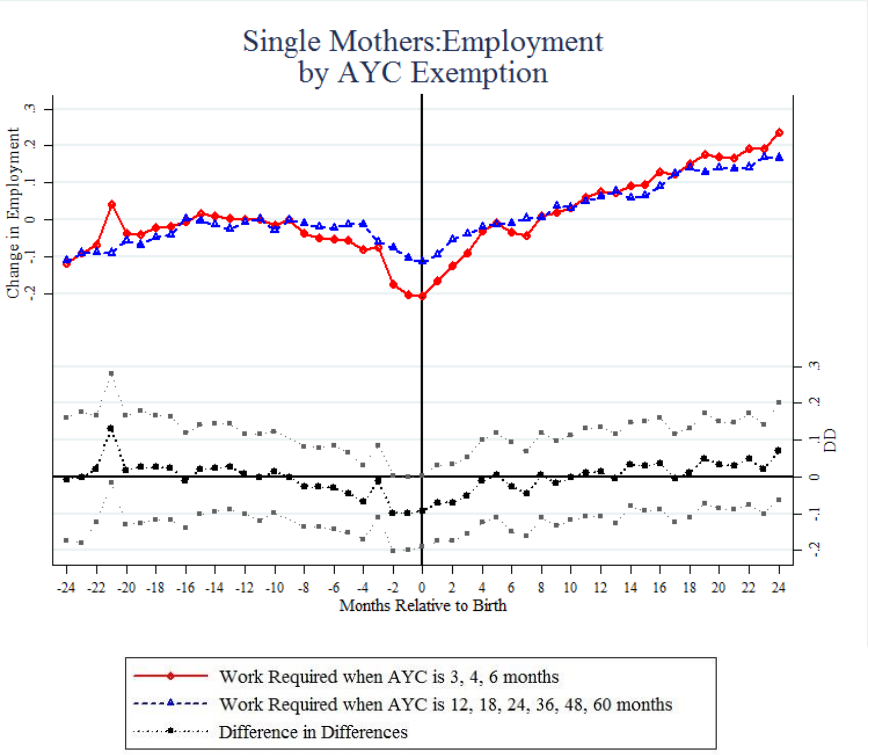


Figure 1.13.2 Triple Difference Event Study Estimates for Employment

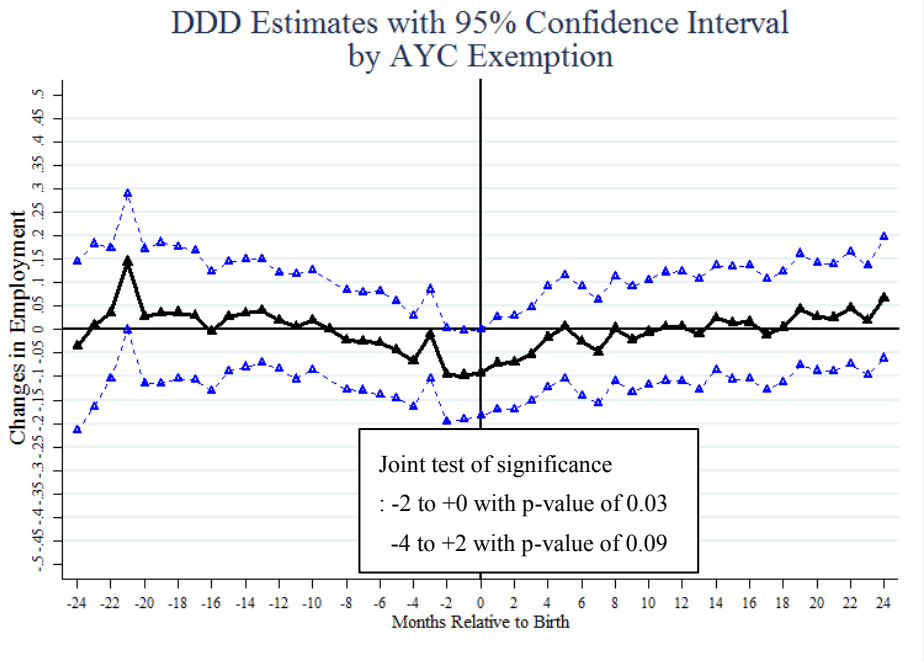


Figure 1.14 Impact of the AYC Exemption on Mothers' Unemployment

Figure 1.14.1 Difference-in-Differences Event Study Estimates for Single Mothers

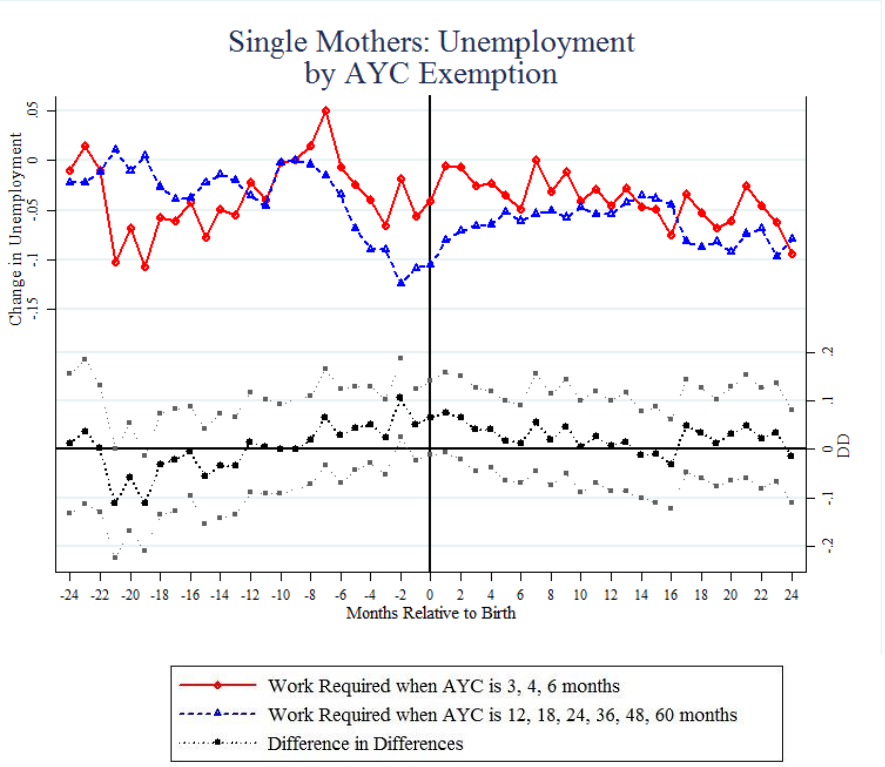


Figure 1.14.2 Triple Difference Event Study Estimates for Unemployment

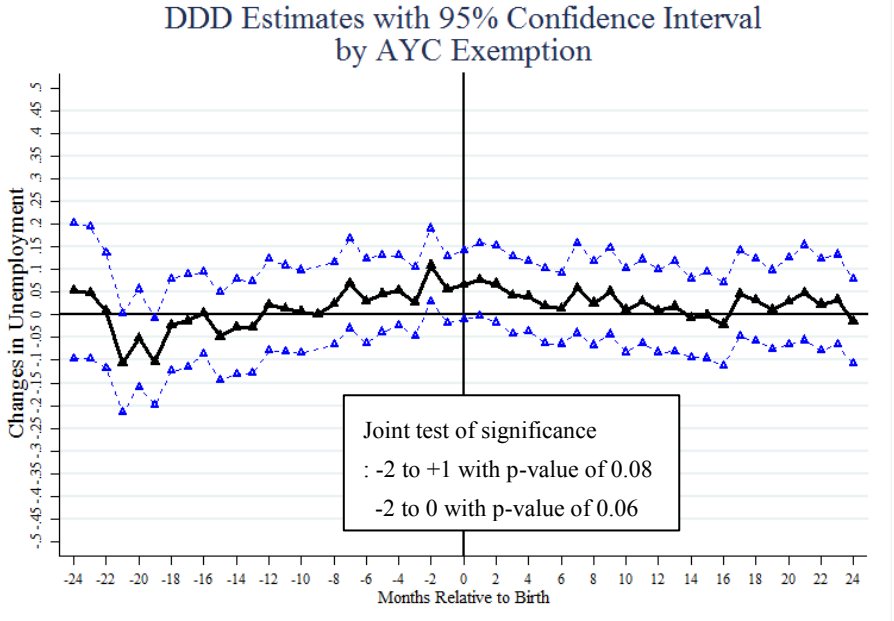
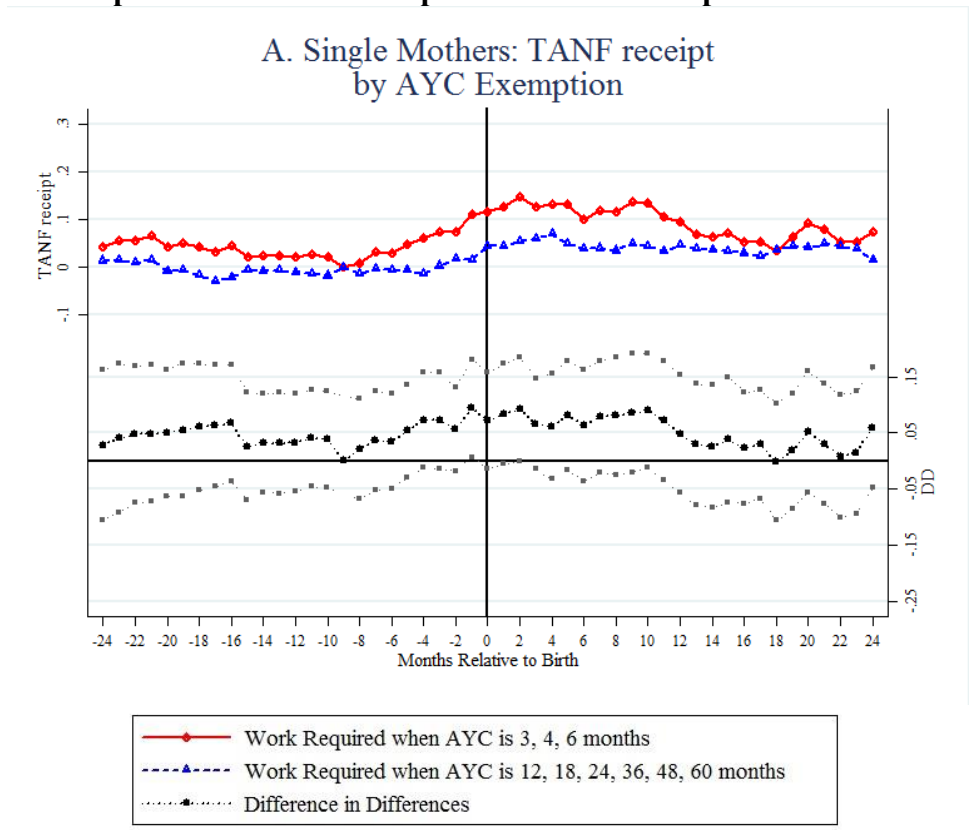


Figure 1.15 Impact of the AYC Exemption on TANF receipt



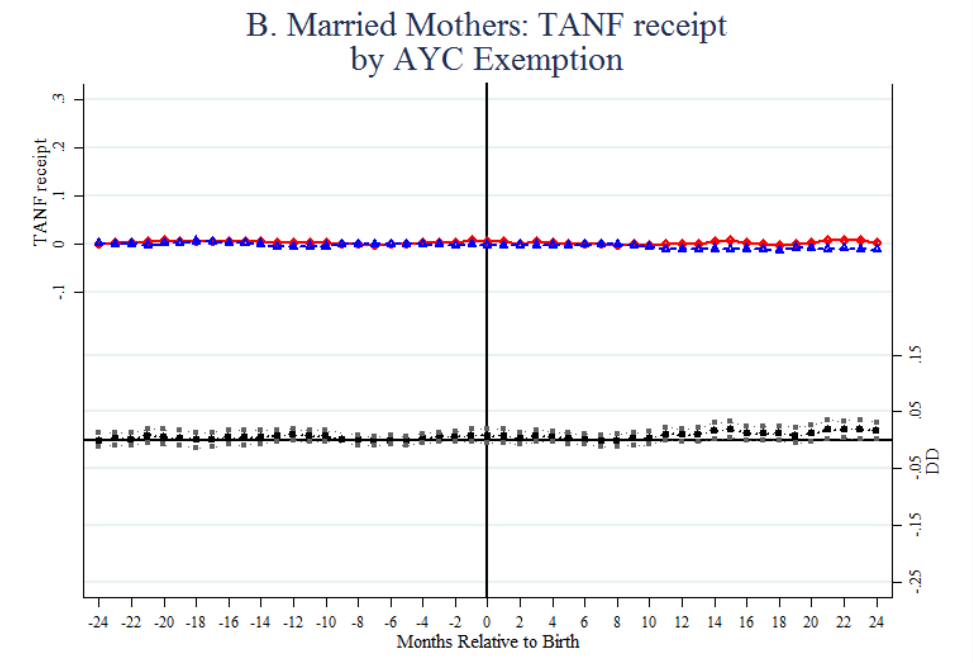
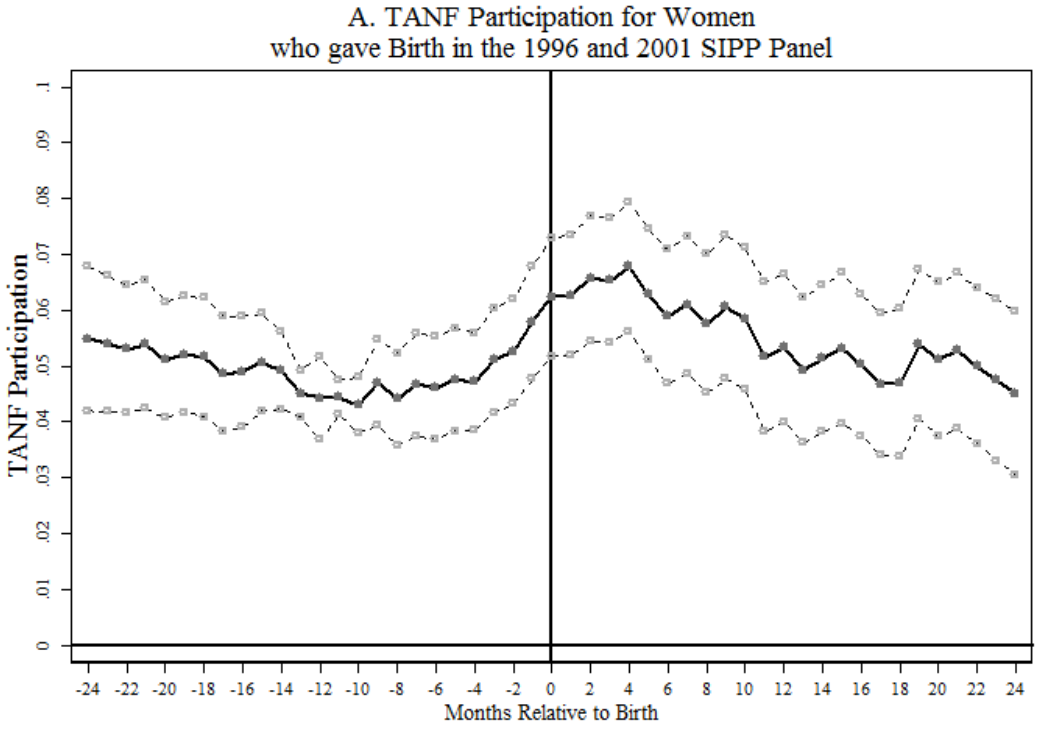
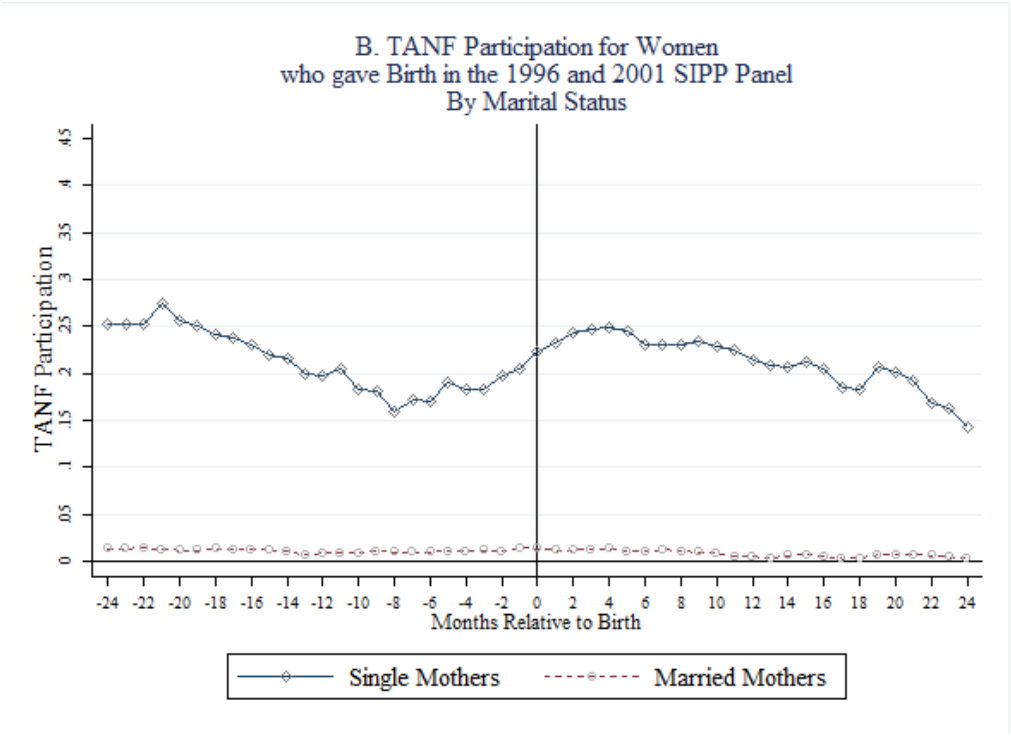
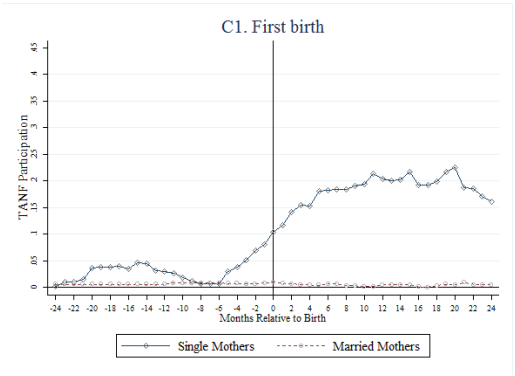


Figure A1.1 TANF Participation Before and After Childbirth



Note: In Figure A1.1 Panels A, B, and C, I plot coefficients on month-relative-to-birth dummies added back to the average level of TANF participation one year prior to birth – these are the δt coefficients added back to the level of TANF participation in the omitted period (j= −12) from estimating equation (3) as described in section 1.5.4. The end-of-survey SIPP sampling weights are used for each mother-birth level observation. Standard errors are clustered by mother-birth. In Panels B and C, single mothers are unmarried, divorced, widowed, or separated women for all months before and after childbirth and reside with no senior members or partners in the household. Married mothers are married for all months before and after childbirth.


B. TANF Participation for Women
who gave Birth in the 1996 and 2001 SIPP Panel
By Marital Status
TANF Participation
0
.05
.1
.15
2
25
3
35
4
45
-24 -22 -20 -18 -16 -14 -12 -10 -8 -6 -4 -2 0 2 4 6 8 10 12 14 16 18 20 22 24
Months Relative to Birth
Single Mothers
Married Mothers


C1. First birth
TANF Participation
Months Relative to Birth
Single Mothers
Married Mothers

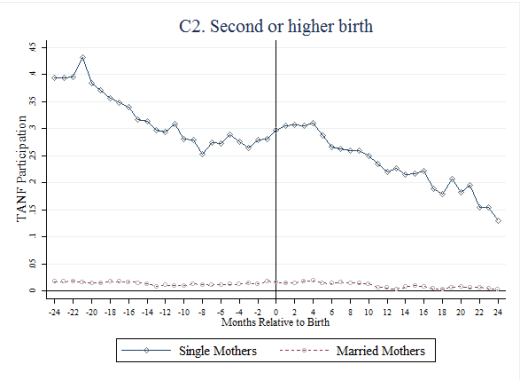
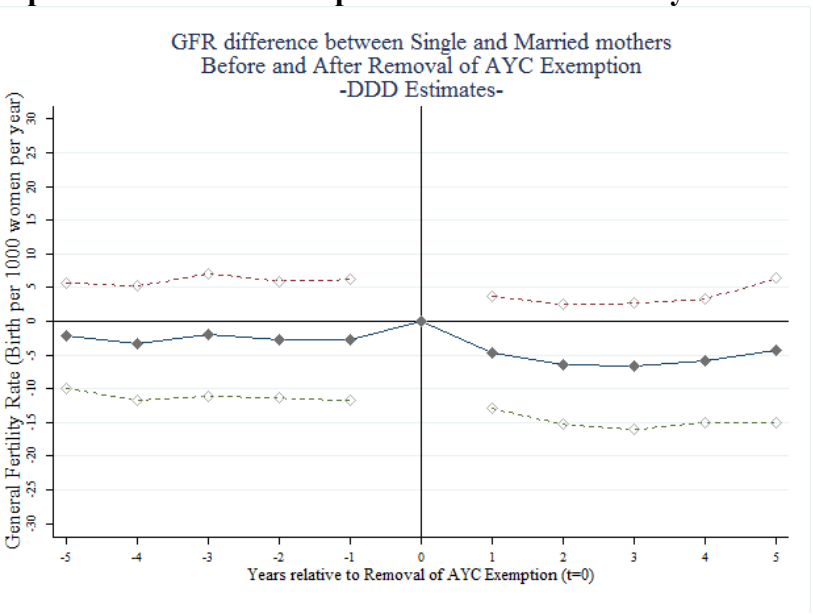
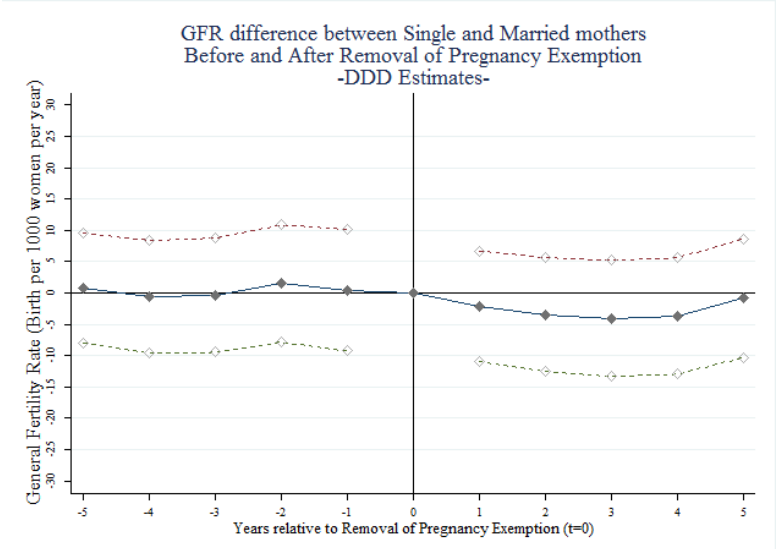

C2. Second or higher birth
TANF Participation
Months Relative to Birth
Single Mothers
Married Mothers

Figure A1.2 Impact of the AYC Exemption on General Fertility Rates



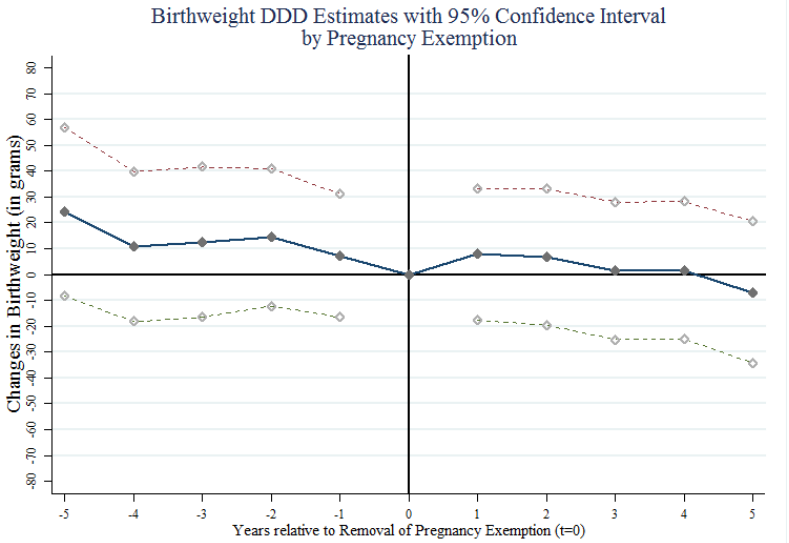
Note: Figure A1.2 plots the DDD estimates, which are coefficients on the interaction between the year-relative-to-AYC exemption policy change, and an indicator for single mothers with the dependent variable being GFR as described in section 1.8.1. The regression includes year and state fixed effects. Standard errors are clustered by state. The dashed lines indicate 95% confidence intervals.

Figure A1.3 Impact of the Pregnancy Exemption on General Fertility Rates



Note: Figure A1.3 plots the DDD estimates, which are coefficients on the interaction between the year-relative-to-pregnancy exemption policy change, and an indicator for single mothers with the dependent variable being GFR as described in section 1.8.1. The regression includes year and state fixed effects. Standard errors are clustered by state. The dashed lines indicate 95% confidence intervals.

Figure A1.4 Impact of the Pregnancy Exemption on Infants' Birth Weight



Note: Figure A1.4 plots the DDD estimates, which are coefficients on the interaction between the year-relative-to-pregnancy exemption policy change, and an indicator for single mothers with the dependent variable being infants' birth weight (in grams) as described in section 1.8.2. The regression includes year and state fixed effects. Standard errors are clustered by state. The dashed lines indicate 95% confidence intervals.

Figure A1.5 Proportion of the Analysis Sample by the Timing of Policy Change in the Two Exemptions

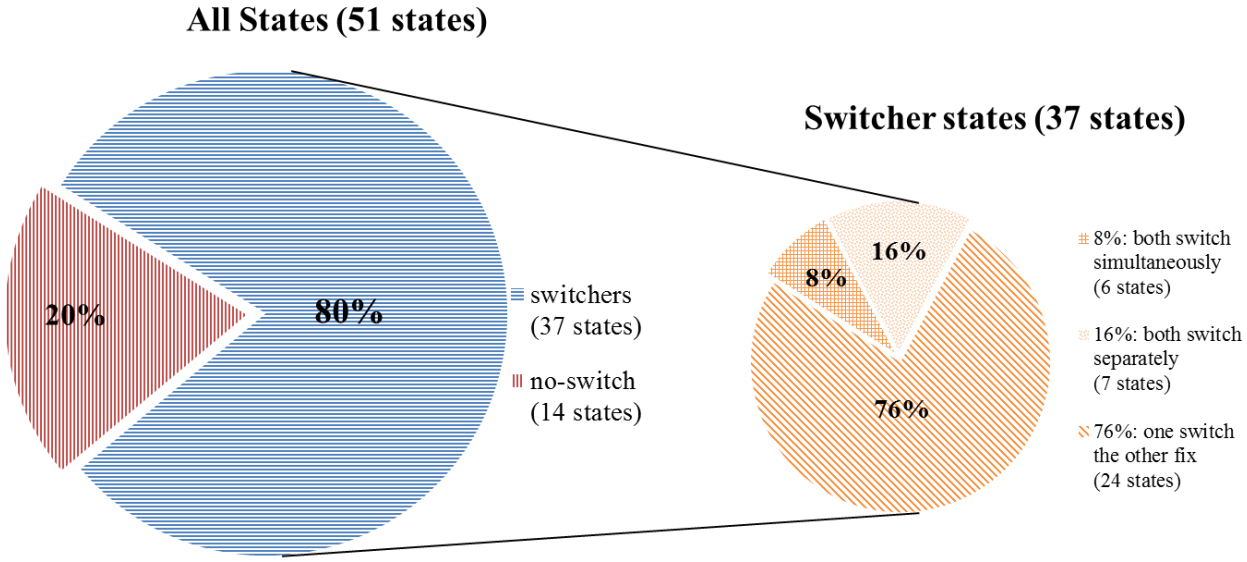
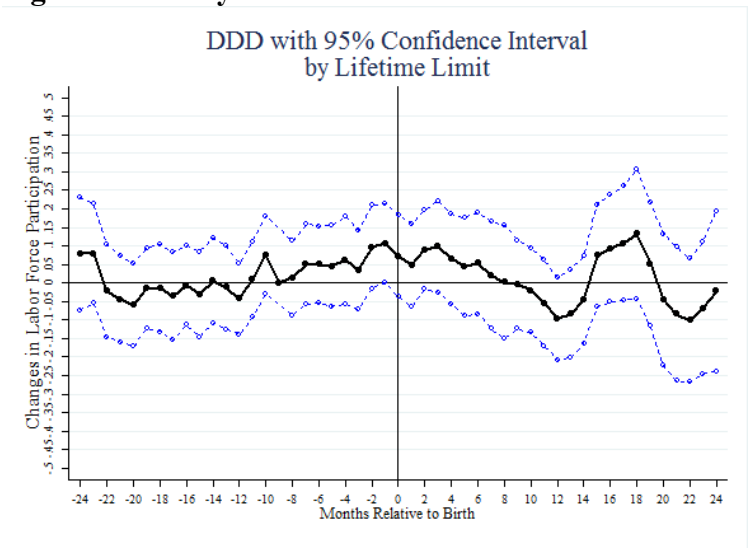


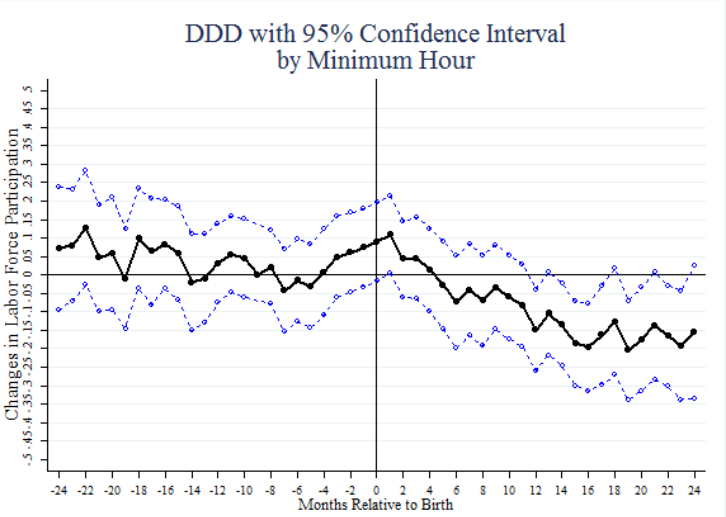
Figure A1.6 Triple Difference Event Study Estimates for Labor Force Participation

Figure A1.6.1 By Lifetime Limit



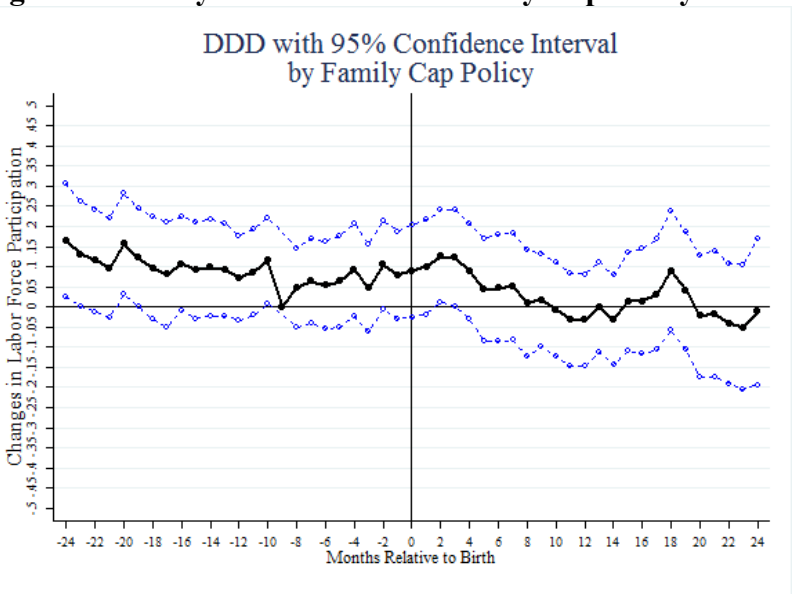
Note: Figure A1.6.1 plots the DDD estimates, θ_t from estimating equation (2), which are coefficients on the interaction between the month-relative-to-birth, an indicator for TANF lifetime limit shorter than 60 months, and an indicator for single mothers. The regression includes year and state fixed effects as well as state-level characteristics as described in section 1.4.2. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth.

Figure A1.6.2 By Weekly Minimum Hours Required for Work



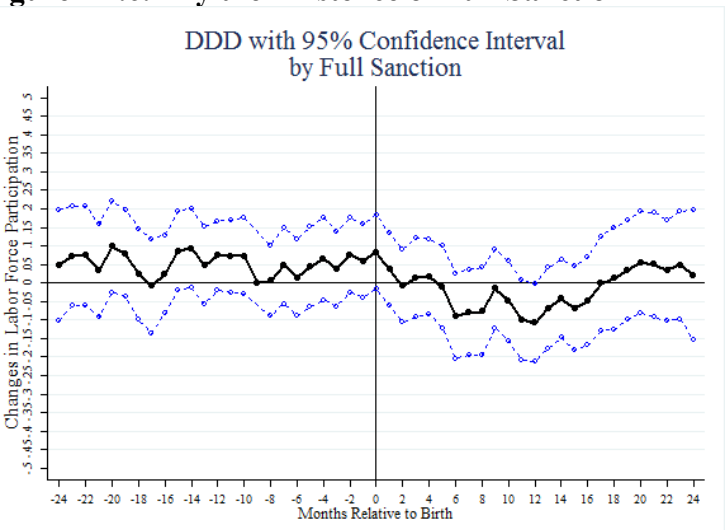
Note: Figure A1.6.2 plots the DDD estimates, θ_t from estimating equation (2), which are coefficients on the interaction between the month-relative-to-birth, an indicator for minimum hour requirement at least 30 hours per week, and an indicator for single mothers. The regression includes year and state fixed effects as well as state-level characteristics as described in section 1.4.2. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth.

Figure A1.6.3 By the Existence of Family Cap Policy



Note: Figure A1.6.3 plots the DDD estimates, θ_t from estimating equation (2), which are coefficients on the interaction between the month-relative-to-birth, an indicator for family cap on benefit increase, and an indicator for single mothers. The regression includes year and state fixed effects as well as state-level characteristics as described in section 1.4.2. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth.

Figure A1.6.4 By the Existence of Full Sanction



Note: Figure A1.6.4 plots the DDD estimates, θ_t from estimating equation (2), which are coefficients on the interaction between the month-relative-to-birth, an indicator for full sanction (termination of benefit for non-compliance of work requirements), and an indicator for single mothers. The regression includes year and state fixed effects as well as state-level characteristics as described in section 1.4.2. The end-of-survey SIPP sampling weights are used. Standard errors are clustered by mother-birth.

Chapter 2

Changes in Low-income Households' Spending Patterns in Response to the 2009 SNAP Benefits Increase

2.1 Introduction

One out of seven U.S. residents is participating in the Supplemental Nutrition Assistance Program (SNAP), formerly known as the Food Stamp Program. SNAP is the federal government's largest nutrition assistance program, providing average monthly benefit of $133 per person, which can only be used to purchase food at participating retailers. The program initiated in the 1960s and participation rate has been dramatically increasing since then. Unlike most means-tested welfare programs with restricted eligibility to particular categories of low-income individuals, SNAP is broadly available to almost all households with low incomes. Furthermore, SNAP eligibility rules and benefit levels are almost uniform across the nation, providing little cross-state variation. Therefore, it has been challenging to identify a causal effect due to selection into program and unobservable differences in participants and non-participants.

Consequently, one way to evaluate the program is to focus on policy changes made at the federal level and observe how the changes have affected SNAP participants differently. There were two notable SNAP policy changes in recent years: In direct response to the Great Recession, the American Recovery and Reinvestment Act (ARRA)

of 2009 increased SNAP maximum monthly benefits by 13.6 percent beginning on April 1, 2009 as a way of stimulating the economy and easing hardship. ARRA provided that SNAP benefit levels would continue until the program's regular annual inflation adjustments to the maximum SNAP allotment exceeded those set by ARRA. However, food price inflation turned out to be lower than expected, accelerating the sunset date of the ARRA benefit increase to October 31, 2013. As a result, SNAP benefit levels were cut beginning on November 1, 2013.

In this study, I quantify the effects of an unprecedentedly large rise in SNAP benefits in 2009 on low-income households' consumption. In the recent literature, consumption has been favored over income as a measure of the well-being for those at the bottom of the income distribution, as it reflects permanent income and the insurance value of government programs as well as private and government transfers (Meyer and Sullivan, 2008). Consumption also likely better captures income from informal jobs. Therefore, this paper addresses to what extent the increase in SNAP benefits provided low-income households with a cushion from a negative shock in household expenditures that they might otherwise have faced resulting from the onset of the Great Recession.

By employing a simple difference-in-differences strategy, I compare consumption of SNAP households with that of low-income non-participants before and after the increase in SNAP benefits. I use the Consumer Expenditure Survey (CEX) data from 2007 to 2011, which covers 2 years before to 2 years after the 2009 SNAP benefit increase.

I find that total household expenditures rose more by $393−422 per quarter for SNAP households compared to that of low-income non-participants after the 2009 ARRA. When I assess each expenditure category separately, the increase is pronounced in the subcategories of food (strongly driven by food at home) and housing (mainly by shelter costs and utility). This provides suggestive evidence that the SNAP benefit increase in 2009 not only helped participating households to expand food expenditures, but also to allocate expanded resources to meet households' other needs.

This paper has two important contributions. A number of papers about SNAP have used either cash-out experiments or the introduction of the Food Stamp Program in the 1960s to estimate the marginal propensity to consume (MPC) on food out of food stamp benefits compared to equivalent cash. However, given that the size and characteristics of population served by SNAP have been changed dramatically over the past years, papers using data from decades ago shed little light on the behavior of contemporary SNAP participants and current

policy issues. Recently, there has been a continuous debate on whether to increase the SNAP benefits to improve the purchasing power of low-income households who are still facing substantial barriers to adequate food consumption. Therefore, first contribution of my paper is that I study the current program to highlight the effectiveness of the SNAP benefit increase in expanding the food budget of needy families.

There is one study (Beatty and Tuttle 2015) that documented the 2009 SNAP benefit increase, but it focuses only on food spending of participant households to estimate MPC out of the SNAP benefit increase. The focus of this paper is not estimating the MPC, but identifying welfare effects of the SNAP benefit increase, which allows low-income households to also expand their non-food consumption. Thus, it is essential to examine the effects of an increase in in-kind transfers on non-targeted spending categories in order to fully assess the policy. Consequently, a second contribution is that I investigate all expenditure categories to identify any important resource allocation across categories in response to the increase in in-kind transfers. Theoretical framework suggests that the increase in SNAP benefits should increase food expenditures, but by less than the increased amount. This difference can be possibly reflected in other categories, providing further evidence of overall welfare improvement of recipients.

The findings of this paper are robust to a host of specification tests. First, to alleviate the concern that the results may have simply picked up the differential trends across groups, I do the same analysis over two different placebo periods, the pre-ARRA period (2007−2008) and post-ARRA period (2010−2011), as a robustness check. I find no evidence of differential effects between the two groups in the years before ARRA and years after ARRA. Second, direct comparison between SNAP and non-SNAP recipients can be confounded by self-selection and under-reporting of SNAP receipt. Accordingly, I use the income-to-poverty ratio as well as the education, employment, and marital status of a household's head as a proxy for SNAP participation. The results remain substantially similar across different treatment definitions.

2.2 Background and Institutional Detail

In direct response to the Great Recession, Congress passed the American Recovery and Reinvestment Act (ARRA) in February 2009. It is commonly referred to as "the Stimulus

Package" or "the Recovery Act". At roughly $800 billion, it was one of the largest fiscal stimulus programs in American history (Wilson 2012).

There has been substantial policy interest in evaluating the effects of the Recovery Act on overall economic activities, particularly of vulnerable groups. A wide range of provisions of the ARRA was targeted toward low-income households to offset a collapse in the economy by protecting jobs and reviving consumer spending, through tax credits and direct transfer payments. In theory, lower income households are expected to spend more of any stimulus money they receive and more quickly (Johnson et al. 2006), thus transfers to these households can push back against rising poverty and economic hardship.

To be specific, unemployment benefits were extended to as long as 99 weeks and increased by $25 a week. Unemployment benefits were excluded from taxation for the first $2,400. In addition, there was a one-time direct cash payment of $250 to Social Security recipients, people on Supplemental Security Income, and veterans receiving disability and pensions. The Earned Income Tax Credit (EITC) was expanded for families with at least three children. Furthermore, billions of dollars were distributed to support welfare payments, such as Temporary Assistance for Needy Family (TANF), Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), free school lunch programs, job training and the employment service. In particular, the monthly maximum benefit of the Supplemental Nutrition Assistance Program (SNAP, commonly known as Food Stamp Program) was increased by about 13.6 percent.

This increase was unprecedentedly large since the initiation of the program. Figure 2.1 depicts the trend of maximum SNAP monthly benefits for a family of 4 in 1994 dollars. A remarkable peak coincides with the 2009 ARRA. Table 2.1 reports the average benefit amount for a family of 4 in each fiscal year, and the percent change in benefit level. The 13.6 percent increase in benefit level in 2009 was the most significant boost compared to any other years.

Table 2.2 presents the increased dollar amount of SNAP from 2009 ARRA by family size. It is extra $18 to $24 per person per month, supplementing low-income households' food purchases and even allowing them to afford other spending. In this study, I find that the SNAP benefit increase certainly provided some firm ground to low-income households by moderating a negative shock after the Great Recession.

2.3 Theoretical Framework

I present a neoclassical model of consumer choice and predict the effect of a SNAP benefit increase on a household's spending. Figure 2.2 provides the overall conceptual framework. It is based on the standard Southworth (1945) model, where consumers allocate a fixed budget between food and all other goods.

Panel A shows the impact of a SNAP benefit increase on the budget constraint. The budget line shifts out by the amount of the increased SNAP benefits ($B_F{}''$), from $\overline{CD}$ to $\overline{EF}$. The slope stays the same as the relative price between food and non-food is not affected by the transfers.

Panel B depicts two types of SNAP recipients—infra-marginal (A) and extra-marginal (B).

(A) Infra-marginal recipient: The infra-marginal type accounts for the vast majority of SNAP recipients (Hoynes et al., 2014). They spend more than SNAP amount on food, thus they use up all of the SNAP benefits and have positive out-of-pocket spending. In other words, their SNAP benefits are treated like cash (Whitmore 2002). After the SNAP benefit rises, infra-marginal recipients move from A_1^* and choose the optimal consumption bundle denoted by A_2^*. This simple static consumer choice theory predicts that their food expenditure goes up with the expanded budget, but the increase is less than the increased amount of SNAP benefit. Instead, their expenditure on non-food goods also goes up as a result of their reduced out-of-pocket spending on food.

(B) Extra-marginal recipient: The extra-marginal type prefers relatively little food consumption. The SNAP allotment is large enough to result in zero dollars of out-of-pocket spending on food. Hence, their food expenditure is equal to the SNAP benefits. In other words, the SNAP benefit amount determines their food expenditures. When the SNAP benefit is increased, extra-marginal recipients move from point B_1^* to point B_2^*, increasing food expenditure by the exact amount of a SNAP benefit increase, while spending on non-food goods is unchanged.

It is empirically well-known fact that most SNAP recipients are infra-marginal, thus this model derives two testable hypothesis: 1) ARRA would induce a majority of SNAP recipients to increase food as well as non-food expenditures; 2) the increase in food expenditures will not be equal to the full amount of the SNAP benefit increase, but to be less.

Figure 2.3 presents total food spending decomposed into cash spending and SNAP spending as a function of household net income for *infra-marginal* group. Maximum SNAP benefit is a horizontal line when family size is fixed. A family with zero income is eligible for the maximum SNAP allotment, which becomes their food expenditures. As income grows, the SNAP benefit decreases proportionally, with the benefit reduction rate currently set at 0.3.[28] I hypothesize that total food spending increases with income.[29] For example, in Panel A, a household participating in SNAP spends total AC on food, among which AB is coming from SNAP and BC is coming out of own pocket. The ARRA raised the maximum benefits to be 113.6 percent of the original. Therefore, the maximum SNAP benefit line is shifted up by 13.6 percent in Panel B. Accordingly the SNAP benefit line is also shifted up with the slope unchanged. Total food spending rises for SNAP households (households whose income is less than 130% of the poverty line[30]), but does not change for non-SNAP households. After the ARRA, the hypothetical household's total spending on food now reaches AC', increased by CC'. This rise is less than the increased SNAP benefit, BB'. In other words, infra-marginal recipients treat the increased amount of SNAP like an increase in cash transfer, leading to a rise in both food and non-food spending.

The predictions of the model guide an empirical test of my research question: How much would food expenditures go up in response to the rise in SNAP benefits? Would the increase in SNAP benefits lead to increase in both food and non-food expenditures? The first question has been asked by many previous studies, but almost all of them compared pre- and post-Food Stamp program to estimate MPC on food. By studying the benefit change occurred in 2009, I provide empirical evidence on the response in food expenditures to the *increase* in SNAP allotments. The second question is fairly new research question as a number of papers about SNAP have been addressing shifts in food spending, but not in non-food spending, which forms another great portion of low-income households' life.

[28] The net monthly income of the household is multiplied by 0.3, and then subtracted from the maximum allotment for the household size to find the household's allotment (i.e. SNAP=Maximum allotment−0.3· [net income]). This is because SNAP households are expected to spend about 30 percent of their resources on food (http://www.fns.usda.gov/snap/eligibility).

[29] It is a well-known Engel's law that the share of income spent on food declines with income level. Although the share declines, the absolute dollar amount spent on food for high-income households will be still greater than the total food expenditures of low-income households.

[30] In the model, I adopt a simple version and assume that every household whose income is less than 130% of the poverty line participates in SNAP, while those with income more than 130% of the poverty line does not. Empirically, SNAP eligibility is not strictly enforced by income threshold – those with high income may be eligible, and those with low income may not when other things are considered such as asset test, broad-based categorical eligibility, and stigma of usage, etc.

2.4 Literature Review

A large body of literature has delved into estimating marginal propensity to consume (MPC) on food out of food stamps in comparison to MPC with cash transfer in order to evaluate the effectiveness of the program on expanding food budget of low-income people. Years during 1960s and 1970s, when the Food Stamp Program was first established, have been examined in order to identify these estimates.

Salathe (1980), for example, used data from the 1972−74 Consumer Expenditure Diary survey and compared Food Stamp recipients with comparably poor non-recipients to estimate that Food Stamps increase at-home food expenditures by 36 cents for each Food Stamp benefit dollar received. In comparison, he reported that a cash transfer would expand at-home food purchases by only 6 cents. A recent study by Hoynes and Schanzenbach (2007) exploits variations in the timing of the implementation of the Food Stamp Program (1963 to 1975) across counties. They show that the introduction of the Food Stamp Program led to an overall increase in household total food expenditures among recipients, but a decreased propensity to eat out, and mixed results for cash food expenditures (i.e. out-of-pocket food expenditures).

Another strand of the literature addresses selection into the program by collecting data from a major cash-out randomized trial, where the Food Stamp benefits were paid out in cash instead of food stamps to a random subset of recipients. However, they have drawn inconsistent conclusions across experiments. Fraker (1990) summarizes that the MPC out of food stamps is two to ten times higher than that out of cash income. Yet, Moffitt (1989), with the experiment in Puerto Rico in 1982, finds no effect of converting food stamps into cash on the food expenditure decisions of recipients. From San Diego cash-out experiment, both Levedahl (1995) and Breunig and Dasgupta (2002, 2005) argue that the MPC out of food stamps is greater than cash, but with different expositions associated with a greater MPC.

All in all, the vast majority of the literature suggests that the Food Stamp Program increases household food expenditures, with a slightly higher or similar MPC on food than that out of direct cash transfers. Yet, most of the studies use data from 20 years ago or earlier. The current SNAP has considerably evolved since its inception. Important changes include the elimination of the purchase requirement[31], the switch to electronic benefit transfer (EBT),

[31] Families were required to make a cash payment upfront to receive their food stamp benefits. For example, if the family was able to afford to spend $50 on food, but the cost of the thrifty food plan was $70, the family could purchase $70 in food stamps for the cash price of $20. Under the current SNAP program, they would receive $20 without outlaying any cash.

and the associated decline in black markets for food stamps. Moreover, the characteristics of SNAP participating households have been markedly changing over time. Participation has nearly quadrupled since the program's introduction, increasing from 4 percent of the U.S. population in 1971 to nearly 15% in 2011 (Beatty and Turtle, 2015). Accordingly, we are greatly in need of studying the current program by estimating the effects that are directly relevant to policymakers: the effect of the *increase* in SNAP benefits.

There are two studies that have looked at the impact of the SNAP benefit increase following 2009 ARRA. Using the Current Population Survey Food Security Supplement (CPS-FSS), Nord and Prell (2011) compare food spending in December 2009 with that for December 2008. To provide an estimate of changes in food expenditures and food security that are attributable to ARRA, they use a difference-in-differences approach to net out the effect of year-to-year changes in food prices and any other factors that affected the SNAP-eligible and non-eligible groups similarly. Their results suggest that the increase in food expenditures of SNAP-eligible households is estimated to be greater by 2.2 percent than non-eligible households. They also compared SNAP participant and non-participant group using the SNAP receipt information in CPS. In this direct comparison, they find a much larger impact: median food expenditures increased by 9.1 percent among SNAP-participant households compared with 3.4 percent among SNAP-eligible non-participating households. This may have overstated the impact, which is attributed to an upward bias, as selection into SNAP is positively correlated with tastes for food consumption (Moffitt 1983; Currie 2004).

Most recently, Beatty and Tuttle (2015), using CEX data, examined several recent SNAP benefit increases over the period of 2007 to 2010 (including the largest increase from 2009 ARRA) and found that they caused households to increase food-at-home expenditure as well as the share allocated to food-at-home. They employ the same methodology, a difference-in-differences, but use the Coarsened Exact Matching approach (CEM) to create a comparison group. A comparison group is generated with a distribution of explanatory variables similar to the treatment group, which improves balance between the two groups, and thereby groups differ only by SNAP participation. Yet, there are several drawbacks with Beatty and Tuttle (2015), which distinguish theirs from my paper: First, they do not solely focus on the SNAP benefit increase from 2009 ARRA, but their analysis spans from 2007 to 2010 and include relatively smaller increases such as 4.6% benefit increase in 2007 and 8.5% increase in 2008. My paper centers around 14% benefit increase in 2009 ARRA to capture the impact of the largest increase in SNAP during the times of recession. Second, Beatty and

Tuttle (2015) focus on food expenditures, disregarding all of the other categories available in the CEX. I make a further contribution by investigating all spending categories to identify any possible allocation across categories after the benefit increase. Lastly, SNAP participation information, except for the first and last interviews, are imputed in the CEX. Beatty and Tuttle use the households that have completed two interviews in a row, one immediately before and the other immediately after the increase. This sample is prone to SNAP imputation issue, and is restricted to families who interviewed at least two consecutive quarters. I improve on this by using only the first quarterly interview where income and program participation are actually reported. Furthermore, I use one interview per consumer unit regardless of the number of quarterly interviews they have completed[32].

Although I do not estimate MPC on food with the SNAP benefit increase in this paper, the findings align with the two previous studies. Results confirm that most SNAP recipients are infra-marginal by showing that the increase in food expenditures is less than the increased amount of SNAP benefits. Furthermore, I show that the increase in transfers induces them to expand not only food spending but also other categories of spending during a time of economic crisis.

2.5 Data and the Empirical Identification Strategy

2.5.1 Data

The Consumer Expenditure Survey (CEX), administered by the Bureau of Labor Statistics, is the only survey in the U.S. that collects a complete record of consumption expenditure data on hundreds of categories of goods and services. In addition to the buying habits of the nation's households, the CEX also reports household characteristics, income, and program participation.

The CEX consists of two surveys, the quarterly interview survey and the diary survey. In the interview survey, each consumer unit[33] is interviewed every three months, providing a short panel of up to five consecutive quarters. In the diary survey, respondents keep track of all of their purchases for two consecutive weeks. In this paper, I use the

[32] I found that economically advantaged households are more likely to complete all five quarterly interviews. Therefore, looking at the first interview only would alleviate this confounding relationship between number of interviews completed and households' SES.

[33] Consumer unit are either members of a household, a person living alone or with others, or two or more persons living together who make joint expenditure decisions. I use consumer unit and household interchangeably to describe the unit of analysis.

interview survey, which is designed to obtain data on the types of expenditures respondents can recall for a period of three months or longer. Relatively large and regular expenditures, such as rent, utilities, health care, major durable goods, as well as food, are reported in the interview survey.

The analysis sample is drawn from the CEX for 2007 to 2011, two years before and two years after the 2009 ARRA SNAP benefit increase, to compare pre- and post-ARRA consumption. I focus on low-income households whose income is below 250% of the poverty line.

In the analysis, I restrict the sample to households with heads at least 20 years old but less than 65 years old to avoid any distinct consumption patterns for households headed by the young or the old. I exclude households whose quarterly expenditures exceed $60,000, which accounts for about 2.4 percent of the entire data. These are outliers that influence the estimators in an unwanted direction. Although each consumer unit is interviewed once per quarter for up to five consecutive quarters, I only use the first interview for each consumer unit. In other words, a household contributes once to the sample no matter how many interviews they have completed. This is again because of income and program participation information in the CEX: expenditure information pertaining to the quarter or to individual months is collected in each quarterly interview, but information about income and program participation for the previous 12 months is collected in only two of the five interviews—the first interview and the last interview[34]. Therefore, income and program participation information in the analysis sample is relatively accurate since none of it is imputed. Finally, the sample size is 11,814 households—5,536 households before 2009 ARRA and 6,278 households after the ARRA.

It is worth noting that the dollar amount of SNAP benefits as well as SNAP receipt could be measured with substantial error in CEX. The number of households reporting SNAP receipt in the CEX is about 60 percent of the SNAP administrative records over the sample period (Hoynes et al. 2014). In that sense, estimates from this study may underestimate the actual impact of the SNAP benefit increase and may be thought of as a lower bound to the extent that those who refuse to report income and program participation are more likely to be from lower end of the income distribution (Keeter et al. 2000 found that surveys with higher response rates underrepresent low-income respondents than those with lower response rates).

[34] Income and program participation data in the middle quarters are imputed, unless there is anyone turning 14 years old or any members who previously did not work but are now working.

2.5.2 Empirical Approach

I employ a difference-in-differences (DID) estimator to examine the effects of the SNAP benefit increase on household consumption:

$$(1) \qquad C_{i.t} = \beta_0 + \beta_1 \cdot SNAP_i + \beta_2 \cdot After_{i,t} + \beta_3 \cdot SNAP_{i,t} \cdot After_{i,t} + \beta_4 \cdot X_{i.t} + \epsilon_{i.t}$$

where $C_{i,t}$ denotes dollar amounts of consumption for household, *i*, in quarter, *t*; *SNAP* is an indicator for SNAP participating households. I carefully design before and after groups: households who have completed their first interview any time before February 2009—the month of ARRA implementation—are included in before group and *After* takes on a value of zero. Those who have done their first interview any time after August 2009 are included in after group, and *After* takes on a value of one. *After* group reports its consumption from May 2009, right after the ARRA SNAP increase. Any particular characteristics of the SNAP households potentially could affect consumption differently, regardless of the policy. Therefore, I control for a vector of demographic characteristics, $X_{i,t}$, such as age of the reference person, age-squared, race, education, marital status, employment status of head, family size, number of children less than 18 years old, proportion of children under 5 years old, proportion of children over 5 years old, proportion of elderly over 65 years old, whether residing in metropolitan statistical area (MSA), and region of residence. Lastly, I also include month (January to December) and year (2007 to 2010) fixed effects to control for seasonality and yearly changes that affect all households. This is a standard difference-in-differences model with β_3 measuring the difference between the change in the SNAP group's consumption pre- to post- ARRA and the change in the non-SNAP group's consumption pre- to post-ARRA. Any changes in consumption common to both groups will be netted out.

Yet, direct comparison between SNAP recipients and non-recipients can be confounded by self-selection and under-reporting of SNAP receipt. Especially, we need to cautious about the selection into SNAP at the time of the ARRA enactment. The increase in SNAP benefits may have changed the self-selection probabilities—some households would not have been motivated to participate with the pre-ARRA benefit level, but now would be induced to participate by a larger benefit. These households are likely to be at the margin of eligibility, better off than the average pre-ARRA participants and worse off than the average pre-ARRA non-participants (Nord and Prell, 2011). Accordingly, their inclusion into SNAP

participants could improve average household consumption status among participants, even in the absence of actual improvement due to the SNAP benefit increase. Thus, I alternatively use income-to-poverty ratio as a proxy for SNAP participation, as done by Nord and Prell (2011).

Therefore, *SNAP* is 1 for 1) households received any SNAP for the past 12 months (0 for households with no SNAP receipt with income below 250% of the poverty line); or 2) households whose income is below 130% of the poverty line (0 for households whose income is above 130% but below 250% of the poverty line[35])

2.6 Findings

2.6.1 SNAP participants vs Non-participants

Main results with difference-in-differences estimators are presented in Table 2.3. First, in Panel A, I use information on whether a household participated in SNAP for the past 12 months to define treatment group.

In each specification, three coefficients are shown. The coefficient on the *SNAP* variable indicates the difference in consumption between the two groups *before* ARRA. As expected, SNAP group has far lower expenditures, showing that they are worse off at baseline. The coefficient on the *After* variable estimates the pre- to post- change for the non-SNAP group. The estimate of primary interest is the coefficient on the interaction term. This is the difference in the pre- to post-ARRA consumption change between the two groups.

The detailed information provided by the CEX allows me to decompose the aggregate household spending into major expenditure categories.[36] Table 2.3 shows the overall change in consumption (column (1)) as well as the contribution to the overall change from various components of consumption (column (2) to (11)). It is important to note that Food at home (column (3)) and Food away (column (4)) are subcategories of Food (column (2)); Shelter (column (6)) and Utility (column (7)) are subcategories of Housing (column (5)). All the other columns are independent categories.

[35] The control group is above the eligibility range for SNAP but far below the national median income.
[36] Appendix 2A describes what is included in each spending category.

Among all households whose income[37] is less than 250% of the poverty line, SNAP households are from the lower end of the consumption distribution—on average, total expenditures are lower than non-participants by $1,400 per quarter in the pre-ARRA period.

Interestingly, the coefficient on the food expenditures interaction term is statistically significant at $121.48, mainly driven by food at home expenditures. This makes sense because the SNAP benefits can only be used for designated food items, and so is mostly used for food at home.[38] The magnitude on the interaction term is $119.42 for food at home. Since it is *quarterly* expenditure, it is equivalent to $40 of monthly expenditure. The mean household size in the SNAP sample is approximately 3 (Table A2.1), so by a back-of-the-envelope calculation, this corresponds to about $13 per person per month. Table 2.2 shows that the average increase in monthly SNAP benefits per person is $18–$24, depending on family size. Thus, food expenditure goes up by less than the full SNAP benefit increase, which is consistent with the earlier discussion of the theoretical framework. This confirms that most SNAP recipients are infra-marginal, who treat an increase in SNAP benefits as an increase in disposable income. Thus, it can be analyzed as a pure income effect, and predicts an increase in both food spending and non-food spending.

This finding aligns with a large body of previous research. Salathe (1980), and Hoynes and Schanzenbach (2007) find that SNAP benefits increase food purchases but by less than the full amount of the benefits. Andrews et al. (2007) argue that estimates of the extra food purchased as a result of a $1 increase in SNAP benefits range from 17 to 47 cents. Since benefits can be used to purchase food only, a typical participating household is expected to cut back on out-of-pocket spending on food as a result of the benefit increase so as to meet other pressing non-food needs, such as housing or transportation that compete for a household's bounded budget.

This is precisely what we see from Table 2.3 Panel A. Expenditure on housing (shelter cost and utility) goes up significantly after the 2009 ARRA compared to non-SNAP participants. This reveals that the majority of recipients is infra-marginal and supports the hypothesis that they shift some cash to meet other needs such as housing fee or utility fee payment.

[37] The income used for SNAP eligibility is a cash, pre-tax measure and does not include in-kind benefits or tax credits. Consequently, I use the before-tax income minus SNAP benefits to determine the treatment and control groups.

[38] The CEX "food at home" concept is the closest match to the items that can be purchased with SNAP benefits. This measure collects spending on food at groceries, convenience stores, farmers markets and home delivery services, minus the cost of paper products, cleaning supplies, pet food, and alcohol (Hoynes 2014).

Another note is that almost all coefficients on the *After* variable are negative, although imprecise. This suggests that there were sizable drops in almost every spending category for all low-income households, yet the drop was much attenuated for SNAP households' food and housing expenditures compared to those of non-participants.

Lastly, given that SNAP receipts in the CEX are fairly underreported, the results using SNAP receipt should be interpreted with caution. To the extent that there are households who rejected to report SNAP participation therefore included in non-SNAP group, the estimates here are underestimated and could be thought as a lower bound of the effect of the SNAP benefit increase.

2.6.2 Households with income ≤ 130% vs Households with 130% <income ≤ 250%

In this section, I define treatment group using the income eligibility criteria of SNAP. SNAP is one of the most universal safety net programs in the U.S. Since there is no targeting to particular groups beyond income and asset eligibility criteria[39], SNAP is broadly available to almost all households with low incomes. Specifically, a household is eligible if its gross monthly income[40] is less than 130% of the poverty line.

In Table 2.3 Panel B, I use this income cutoff to identify SNAP group, with a comparison group whose income is greater than the cutoff, but lower than 250% of the poverty line[41]. However, we should note that income reported in the CEX does not have the same accounting period as the one that is used for SNAP eligibility: amount of income *in past 12 months* is reported in the CEX, while gross *monthly* income is used for eligibility. Hence, the analysis sample consists more of longer-term poor households rather than households whose income was consistently high over the course of year, but a sudden negative shock introduced a plunge in income, pulling it below the SNAP eligibility line.

Interaction terms are statistically significant and positive for almost all quarterly expenditure categories except food away, personal care and tobacco. The low-income group's total expenditure rose after ARRA by $422 per quarter, slightly higher than the results in

[39] Households have to meet both a monthly gross income test and a monthly net income test to be eligible for SNAP benefits unless all members are receiving Supplemental Security Income (SSI) or Temporary Assistance for Needy Families (TANF). Other households with one or more elderly members only have to meet the net income test.
[40] Gross income means a household's total income before any deductions have been made.
[41] The crosstab of two different definitions demonstrates that a third of households with income less than 130% report SNAP receipt. 80% of households with SNAP receipt report income less than 130% of the poverty line.

Panel A. This increase in the total expenditures seems to be strongly driven by the food, housing, transportation, and entertainment categories. The magnitude on the interaction term of food expenditure is quite small here, $83.5, which indicates that the impact of policy change on SNAP recipients may be diluted by the inclusion of non-SNAP low-income households that were not affected by the ARRA SNAP increase. Consequently, Table 2.3 Panel B shows the global effect of the ARRA on low-income households overall.

Again, we can observe some negative coefficients on *After* variable, though not statistically significant. It indicates that the ARRA provisions targeted for low-income households protected them from negative consumption shocks as the increase in their expenditures on food, housing, and transportation offsets the drop in these categories.

The increase in the housing and transportation expenditures for low-income households aligns with findings from Meyer and Sullivan (2008). Using the CEX, they describe the underlying trends in income and consumption for single mother headed families between 1993 and 2003, when dramatic changes in welfare and tax policies targeted poor families, including expansions in the EITC, welfare waivers, and the passage of the welfare reform. They analyze various components of consumption and find that spending on housing and transportation accounts for much of the increase in consumption in the bottom quintiles of income distribution. They report that the increase in housing consumption is mainly driven by increases in out-of-pocket rent and the increase in transportation is associated with trend toward increased work for single mothers during this time.

2.6.3 Robustness Check

1) The Placebo Period

To alleviate concerns that what is picked up might be the differential trends across groups, I estimate the same specification over two placebo periods—the pre-ARRA period (2007−2008) and post-ARRA period (2010−2011). Table 2.4 shows placebo test results using years before ARRA. In Panel A, interaction term on food expenditures is not significant anymore, although there seems to be a large increase in spending on housing for SNAP group between 2007 and 2008. In Panel B, the interaction terms are statistically insignificant or even negative except food expenditures: low-income households' quarterly food expenditures rose by $80 between 2007 and 2008. This is plausible given that there was a small increase in

SNAP allotment by 4.6% in October 2007, followed by an 8.5% increase in October 2008, as reported in Table 2.1. But we see smaller and insignificant impact on food at home.

Table 2.5 presents placebo test results using years after ARRA. In these analyses, I find absolutely no effect and none of the coefficients on the interaction term is statistically significant, regardless of the choice of treatment group. The placebo period results suggest that economic trends over this period are not what the results are capturing. The difference-in-differences estimates are only statistically and economically significant when two years pre-ARRA are compared with two years post-ARRA. The absence of significant results in the placebo periods suggest that the vulnerable households clearly responded to the SNAP benefit increase, which occurred in 2009 by expanding their household's overall budget.

2) Treatment Group Compositional Change Pre- and Post-ARRA

The main issue with the current model specification is a possible compositional shift between treatment and control group over time. For example, suppose that I define the treatment group as households whose income falls under the 130% of the poverty line and the comparison group as households whose income falls under the 250% line, but above the 130% line. The identifying assumption is that the difference between the two groups is fixed over time, and there is no shift across groups. However, a household whose income was above 130% of the poverty line before ARRA could have been in extreme economic hardship and fall below 130% after ARRA. Then, the inclusion of pre-ARRA control group into the post-ARRA treatment group may improve the estimated consumption of the latter group due to their life cycle consumption trend, contaminating the estimator. Similarly, SNAP participants before ARRA could be different from SNAP participants after ARRA in a number of ways.

Ideally, we want to see only one single difference within treatment group across time: SNAP benefit increase. All the other characteristics should stay the same to identify the effect of the 2009 ARRA. To assure that the composition of treatment group is fixed over the period, I report summary statistics of the households included in treatment group to test whether any of their characteristics have changed significantly before and after ARRA. In Table A2.1, I present summary statistics of the households who report SNAP receipt in the past 12 months. There seems to be a slight upward trend in head's education over time, but no notable difference in any other characteristics is observed before and after the ARRA. It is

interesting to discover that these households receive remarkably more unemployment benefits, SNAP benefits and Medicaid after 2009.

In Table A2.2, households with income less than 130% of the poverty line look surprisingly balanced before and after, except age of head, number of earners, and race of head. Again, low-income households receive significantly more unemployment benefits, SNAP benefits, and Medicaid after 2009 ARRA, which reflects the impact of overall provisions of the ARRA on low-income households.

This analysis yields convincing evidence that there is no compositional shift within the treatment group over the study time horizon. Otherwise, it could seriously confound the estimator by reflecting changing attributes of the group over time, failing to capture the effect of the SNAP benefit increase.

3) Treatment Group defined by Education, Employment, and Marital Status of Head

Given that SNAP receipt information in the CEX is incomplete and severely under-reported compared to administrative data, I use demographic characteristics of household head to identify treatment group and control group. Disadvantageous social economic status such as low education, single or unemployed head is known to predict SNAP participation (USDA 2009). Substantially similar results across different definitions of treatment groups assure that there was indeed a positive effect of the ARRA SNAP increase on a wide range of disadvantaged populations. Table 2.6 Panel A reports the results when the treatment group is households with low-educated head. While there is no measurable impact on total expenditures, we still see positive and significant increase in food expenditures, although it seems to be mainly driven by food away. Panel B reports the results when the treatment group is households with single head. Here, we can find a very similar result as we saw in Table 2.3 Panel A: increase in the total expenditures, strongly driven by food and transportation spending. Lastly, in Panel C, I use employment status of household head and reveal that there are sizable increases in the total expenditures, mainly driven by food at home and housing expenditures.

Results that are consistent across a host of different definitions of treatment group highlight that the 2009 ARRA provided a basic floor for protecting disadvantaged families and expanded their limited budget set to insure a basic level of consumption.

2.7 Conclusion

The ARRA was one of the largest fiscal stimulus programs in U.S. history, with total spending accounting for approximately 2.6 percent of GDP over the years of 2009−2010. Given that consumption is generally favored over income as a measure of the well-being of the poor (Meyer and Sullivan, 2008), understanding consumption response to the increase in welfare transfers helps capture the well-being effect of the policy change.

In this study, I focus on a plausibly exogenous and the largest one-time increase in SNAP benefits that started on April 1, 2009. Comparing SNAP households with non-participants, I find sizable increases in food and housing expenditures in response to the SNAP benefit increase. Consistent with the theoretical framework, the increased amount in food expenditure is less than the full amount of the SNAP benefit increase. Instead, SNAP households redirected their expanded resources to other non-food spending such as housing, transportation, and entertainment. Consequently, the rise in SNAP benefits not only resulted in greater food expenditures, but also allowed SNAP recipients to shift resources to non-food purchases.

All in all, I show that spending falls relatively less during recession for the target group relative to another as a result of the increase in government transfers. This is an important finding with the concerns magnified for disadvantaged households with low income and low assets at the time of economic downturn. In light of this, this paper sheds light on policy implications that increased government transfers can be one possible mechanism through which consumption of low income families can be protected. This study provides compelling evidence of SNAP's pivotal role in improving food expenditures for households who would otherwise be at greater risk of food insecurity and lack of food choices, in the absence of the safety net program. In addition, the benefit increase definitely offered a cushion for vulnerable groups by expanding households' budget to afford basic needs.

References

Andrews, Margaret, Frazao, Elizabeth, Prell, Mark, and David Smallwood, "Food Spending Patterns of Low-income Households: Will Increasing Purchasing Power Result in

Healthier Food Choices?" *Economic Information Bulletin* No. 29-4, Economic Research Service, U.S. Department of Agriculture (2007), 1–14

Beatty, Timothy K.M. and Charlotte Tuttle, "Expenditure Response to Increases in In-Kind Transfers: Evidence from the Supplemental Nutrition Assistance Program," *American Journal of Agricultural Economics*, 97 (2) (2015), 390–404

Center on Budget and Policy Priorities, "The Earned Income Tax Credit," *CBPP Policy Basics Report* (2014)

Currie, Janet, "The Take Up of Social Benefits," *NBER Working Paper* No. 10488 (2004), 1–51

Fraker, Thomas M., "The Effects of Food Stamps on Food Expenditures: A Review of the Literature," *Food and Nutrition Service*, U.S. Department of Agriculture (1990)

Fraker, Thomas M., Martini, Alberto P., Ohls, James C., and Michael Ponza, "The Effects of Cashing-Out Food Stamps on Household Food Use and the Costs of Issuing Benefits," *Journal of Policy Analysis and Management, 14(3)* (2007), 372─392

Hoynes, Hilary W. and Diane Schanzenbach, "Consumption Responses to In-Kind Transfers: Evidence from the Introduction of the Food Stamps Program," *American Economic Journal: Applied Economics, 1(4)* (2009), 109─139

Hoynes, Hilary W. and Diane Schanzenbach, "U.S. Food and Nutrition Programs," *NBER Working Paper* No. 21057 (2015)

Hoynes, Hilary W., Schanzenbach, Diane, and Leslie McGranahan, "SNAP and Food Consumption," *University of Kentucky Center for Poverty Research Discussion Paper Series*, DP2014-03 (2014), 1─49

Jappelli, Tullio and Luigi Pistaferri, "The Consumption Response to Income Changes," *The Annual Review of Economics, 2(1)* (2010), 479─506

Johnson, David S., Parker, Jonathan A., and Nicholas S. Souleles, "Household Expenditure and the Income Tax Rebates of 2001," *American Economic Review, 96(5)* (2006), 1589–1610

Keeter, Scott, Miller, Carolyn, Kohut, Andrew, Groves, Robert M., and Stanley Presser, "Consequences of Reducing Nonresponse in a Large National Telephone Survey," *The Public Opinion Quarterly*, 64 (2) (2000), 125–148

Meyer, Bruce D. and James X. Sullivan, "Measuring the Well-Being of the Poor Using Income and Consumption," *Journal of Human Resources*, 38 (Supplement) (2003), 1180─1220

Meyer, Bruce D. and James X. Sullivan, "Consumption, Income, and Material Well-Being after Welfare Reform," *NBER Working Paper* No.11976 (2006), 1─50

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

Moffitt, Robert, "An Economic Model of Welfare Stigma," *American Economic Review* 73(5) (1983), 1023–1035

Nord, Mark and Mark Prell, "Food Security Improved Following the 2009 ARRA Increase in SNAP Benefits," *Economic Research Report No. 116*, Economic Research Service U.S. Department of Agriculture (2011), 1–46

Petev, Ivaylo D. and Luigi Pistaferri, "Consumption in the Great Recession," *A Great Recession Brief*, The Russel Sage Foundation and The Stanford Center on Poverty and Inequality (2012), 1–6

Salathe, Larry E., "The Food Stamp Program and Low-Income Households' Food Purchases," *Agricultural Economics Research*, 32(4) (1980), 33–41

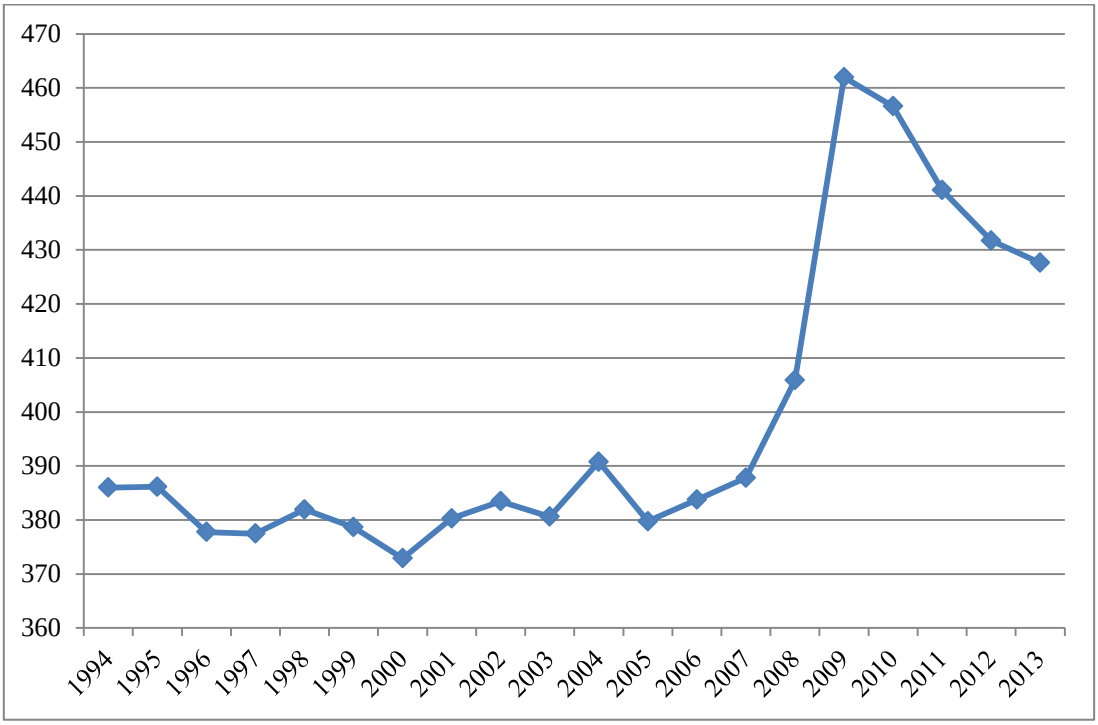
Southworth, Herman M., "The Economics of Public Measures to Subsidize Food Consumption," *Journal of Farm Economics* 27 (1) (1945), 38–66

United States Department of Agriculture, "Trends in Supplemental Nutrition Assistance Program Participation Rates: 2000 to 2007," Food and Nutrition Service, U.S. Department of Agriculture (2009)

Whitmore, Diane, "What Are Food Stamps Worth?" Working Paper No. 468, *Princeton University Industrial Relations Section* (2002), 1–64

Wilson, Daniel J., "Fiscal Spending Jobs Multipliers: Evidence from the 2009 American Recovery and Reinvestment Act," *American Economic Journal: Economic Policy 4(3)* (2012), 251–282

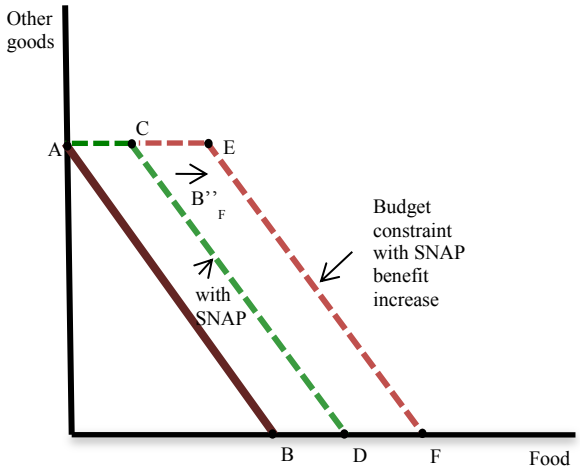
Figure 2.1 Maximum Monthly SNAP Allotment for a Family of 4 ($1994)



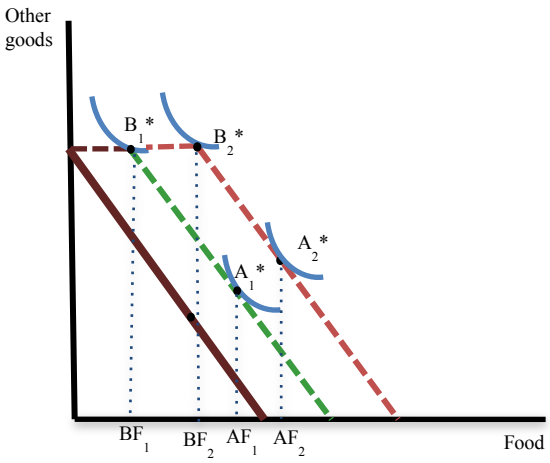
Source: Food and Nutrition Service, United States Department of Agriculture

Figure 2.2 SNAP benefit increase in the 2009 ARRA and a consumer's choice

Panel A: Impact of SNAP Benefit Increase on Budget Constraint



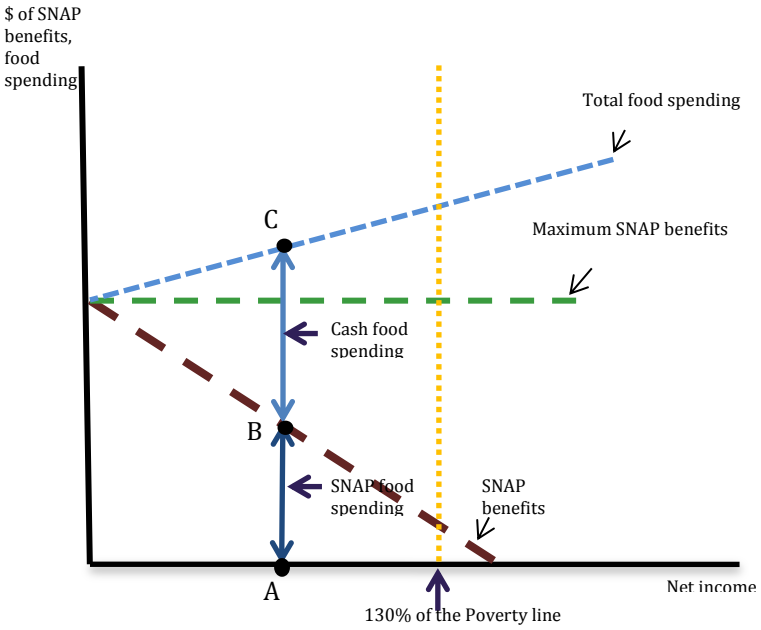
Panel B: Consumption Decisions in Response to SNAP Benefit Increase



Note: Figure 2.2 and Figure 2.3 are directly adopted from Hoynes et al. (2014). Hoynes et al (2014) analyze the introduction of the SNAP program, while I analyze the increase in SNAP benefits.

Figure 2.3 SNAP Benefit Increase in the 2009 ARRA and Allocation of Food Expenditures

Panel A: Food Spending for SNAP-participating Households



Panel B: Changes in Food Spending after the SNAP Benefit Increase

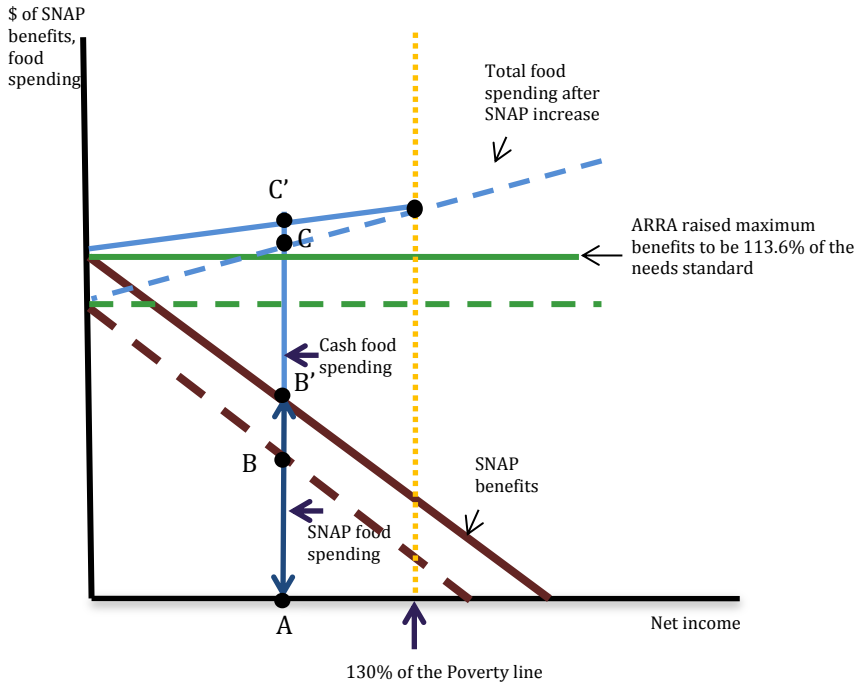


Table 2.1 Monthly SNAP Benefit Amount for a Family of 4

Year	Average Benefit for a family of 4	Percent Change in Benefit level
Oct 1994 - Sept 1995	$386	
Oct 1995 - Sept 1996	$397	2.8%
Oct 1996 - Sept 1997	$400	0.75%
Oct 1997 - Sept 1998	$408	2%
Oct 1998 - Sept 1999	$419	2.7%
Oct 1999 - Sept 2000	$426	1.7%
Oct 2000 - Sept 2001	$434	1.9%
Oct 2001 - Sept 2002	$452	4.1%
Oct 2002 - Sept 2003	$465	2.9%
Oct 2003 - Sept 2004	$471	1.3%
Oct 2004 - Sept 2005	$499	5.9%
Oct 2005 - Sept 2006	$506	1.4%
Oct 2006 – Sept 2007	$518	2.4%
Oct 2007 – Sept 2008	$542	4.6%
Oct 2008 – March 2009	$588	8.5%
April 2009 – Sept 2009	**$668**	**13.6%**
Oct 2009 – Oct 2013	$668	0%
Nov 2013 – Sept 2014	$632	-5.4%

Source: Food and Nutrition Service, U.S. Department of Agriculture

Table 2.2 Monthly SNAP Benefit Increase from the 2009 ARRA

Household size	Increase	Household size	Increase
1	$24	5	$95
2	$44	6	$114
3	$63	7	$126
4	$80	8	$144

* For each extra person, a household receives $18.

Source: Food and Nutrition Service, U.S. Department of Agriculture

Table 2.3 [Main Results] Effects of the 2009 SNAP Benefit Increase on Households' Expenditures: Difference-in-Differences Estimation using CEX 2007−2011

(A) Sample: SNAP participants (Treatment) Compared to Non-participants with Income ≤ 250% of the Poverty Line (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
SNAP	-1,399.89*** [145.871]	-265.69*** [32.257]	-131.62*** [25.242]	-133.92*** [17.266]	-670.83*** [63.041]	-454.77*** [42.569]	-123.02*** [16.500]	-342.82*** [99.413]	-108.55*** [19.523]	-18.15*** [2.346]	61.17*** [10.441]
After ARRA	-369.68 [319.967]	-53.04 [55.273]	-62.71 [39.196]	9.66 [33.872]	-115.68 [134.332]	-16.74 [95.619]	-58.09** [29.290]	-154.62 [213.565]	11.06 [37.320]	-2.73 [5.204]	5.62 [16.704]
SNAP* After ARRA	393.48** [169.504]	121.48*** [38.224]	119.42*** [31.189]	2.07 [19.0033]	165.57** [76.628]	112.41** [55.359]	36.80* [20.391]	39.44 [111.356]	67.69*** [22.604]	5.72* [3.079]	-16.80 [13.651]
Observation	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814
R-squared	0.208	0.254	0.316	0.064	0.210	0.163	0.324	0.040	0.039	0.048	0.044

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used. Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

(B) Sample: Households with Income ≤ 130% of the Poverty Line (Treatment) Compared to those with 130%< Income ≤ 250% (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
Low Income	-1,561.72*** [129.147]	-210.46*** [25.300]	-111.71*** [18.846]	-98.64*** [15.325]	-603.73*** [55.496]	-348.84*** [41.090]	-141.37*** [12.101]	-574.49*** [87.352]	-131.89*** [24.961]	-11.11*** [2.208]	-4.59 [6.666]
After ARRA	-462.63 [327.080]	-65.07 [55.199]	-70.33* [40.122]	5.23 [32.892]	-133.37 [137.934]	-48.40 [96.202]	-50.91* [30.222]	-238.35 [219.668]	-4.44 [40.172]	-0.45 [5.563]	12.83 [17.366]
Low Income * After ARRA	422.13*** [156.569]	83.52*** [32.050]	71.22*** [24.299]	12.31 [18.747]	129.14* [69.457]	121.79** [53.448]	8.30*** [16.034]	211.61** [107.718]	66.13** [27.719]	-1.74 [3.091]	-19.18* [10.073]
Observation	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814
R-squared	0.217	0.255	0.317	0.061	0.214	0.162	0.334	0.044	0.042	0.048	0.039

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used.
Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4 [Placebo Test] Years before ARRA: Households' Expenditures Before (2007) and After (2008)

(A) Sample: SNAP participants (Treatment) Compared to Non-participants with Income ≤ 250% of the Poverty Line (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
SNAP	-1,388.63*** [232.894]	-295.80*** [48.279]	-155.86*** [35.532]	-139.79*** [29.165]	-748.29*** [88.170]	-496.67*** [63.963]	-142.64*** [24.108]	-187.90 [182.271]	-139.77*** [29.122]	-17.56*** [3.585]	48.11*** [13.858]
After	39.69 [204.027]	-12.05 [32.554]	32.26 [23.051]	-44.26** [19.645]	32.88 [84.399]	-1.54 [60.737]	39.87** [16.678]	18.89 [143.580]	-35.88 [34.202]	1.85 [2.979]	0.79 [8.380]
SNAP * After	123.71 [271.935]	86.38 [60.106]	61.81 [46.795]	24.55 [32.933]	235.04** [116.693]	130.38 [81.890]	52.83* [31.120]	-284.79 [191.128]	58.69 [37.071]	1.17 [4.314]	20.54 [19.199]
Observation	5,536	5,536	5,536	5,536	5,536	5,536	5,536	5,536	5,536	5,536	5,536
R-squared	0.198	0.256	0.313	0.070	0.213	0.172	0.324	0.039	0.035	0.051	0.056

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used.
Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

(B) Sample: Households with Income ≤ 130% of the Poverty Line (Treatment) Compared to those with 130%< Income ≤ 250% (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
Low Income	-1,453.88*** [188.587]	-238.55*** [39.142]	-120.94*** [29.738]	-117.50*** [24.557]	-625.23*** [82.188]	-384.90*** [61.481]	-127.72*** [17.686]	-429.10*** [134.300]	-136.61*** [39.141]	-10.98*** [3.363]	-4.14 [8.953]
After	58.12 [247.086]	-39.51 [40.309]	25.01 [29.697]	-64.47*** [23.332]	-3.65 [101.451]	-47.81 [75.066]	49.45** [19.979]	96.78 [180.283]	-33.25 [43.373]	1.54 [3.575]	5.05 [10.614]
Low Income * After	-46.39 [243.715]	80.56* [48.370]	33.40 [36.230]	47.16 [29.031]	139.95 [106.183]	129.67 [80.096]	-5.52 [23.269]	-285.36* [168.035]	11.91 [49.318]	0.36 [4.228]	1.42 [12.024]
Observation	5,536	5,536	5,536	5,536	5,536	5,536	5,536	5,536	5,536	5,536	5,536
R-squared	0.209	0.257	0.314	0.070	0.217	0.172	0.332	0.046	0.037	0.050	0.047

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used. Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5 [Placebo Test] Years <u>after ARRA</u>: Households' Expenditures Before (2010) and After (2011)

(A) Sample: SNAP participants (Treatment) Compared to Non-participants with Income ≤ 250% of the Poverty Line (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
SNAP	-1,154.32*** [154.801]	-186.86*** [34.346]	-34.85 [29.334]	-151.85*** [15.435]	-577.48*** [71.857]	-401.36*** [54.618]	-93.61*** [20.303]	-333.51*** [98.000]	-29.65 [18.275]	-14.97*** [3.334]	40.41*** [13.571]
After	-256.66** [119.772]	-17.54 [25.081]	-5.67 [18.098]	-11.89 [15.629]	-120.95** [57.246]	-111.79** [45.320]	-11.44 [13.061]	-62.13 [80.474]	-14.68 [15.490]	-4.44 [2.815]	-7.02 [9.350]
SNAP * After	180.60 [203.743]	61.70 [47.274]	32.34 [40.406]	29.35 [21.772]	65.58 [97.599]	76.99 [75.625]	-2.00 [26.111]	62.95 [124.780]	-19.26 [25.714]	2.73 [4.402]	12.15 [19.691]
Observation	6,278	6,278	6,278	6,278	6,278	6,278	6,278	6,278	6,278	6,278	6,278
R-squared	0.219	0.257	0.322	0.058	0.212	0.157	0.332	0.041	0.063	0.049	0.041

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used. Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

(B) Sample: Households with Income ≤ 130% of the Poverty Line (Treatment) Compared to those with 130%< Income ≤ 250% (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
Low Income	-1,314.49*** [157.653]	-120.09*** [33.528]	-35.32 [22.448]	-84.66*** [22.832]	-566.01*** [74.691]	-273.53*** [61.160]	-152.84*** [16.945]	-445.61*** [104.144]	-70.58*** [20.999]	-15.43*** [3.671]	-20.07* [12.120]
After	-319.98** [147.403]	20.74 [30.461]	16.83 [23.624]	3.88 [17.110]	-156.81** [68.466]	-112.22** [52.350]	-24.62 [16.648]	-137.93 [105.343]	-21.98 [17.639]	-6.58** [3.340]	0.81 [13.817]
Low Income * After	194.49 [197.822]	-47.36 [42.073]	-28.74 [32.115]	-18.61 [24.349]	93.55 [91.809]	32.12 [72.171]	23.62 [22.210]	172.63 [135.886]	4.84 [24.713]	5.24 [4.462]	-8.84 [16.068]
Observation	6,278	6,278	6,278	6,278	6,278	6,278	6,278	6,278	6,278	6,278	6,278
R-squared	0.227	0.258	0.323	0.054	0.215	0.154	0.343	0.044	0.067	0.051	0.039

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used.
Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6 [Robustness Check] Effects of the 2009 SNAP Benefit Increase on Households' Expenditures: Difference-in-Differences Estimation using CEX 2007−2011

(A) Sample: Households' Head Less than High School Degree (Treatment) Compared to those with High School Degree or more (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
Low-Educ	-2,204.66*** [165.121]	-230.54*** [31.677]	-56.02** [23.099]	-174.37*** [19.478]	-1,008.3*** [76.694]	-634.23*** [56.067]	-128.93*** [16.973]	-627.38*** [105.148]	-227.97*** [30.008]	-24.85*** [3.003]	56.53*** [9.146]
After ARRA	-310.39 [318.453]	-44.34 [55.810]	-41.22 [39.337]	-3.13 [34.236]	-79.54 [131.587]	-5.30 [95.498]	-45.29 [28.998]	-166.86 [212.956]	25.03 [38.270]	-2.44 [5.208]	9.62 [16.630]
Low-Educ * After ARRA	25.84 [174.023]	**63.93*** **[38.770]**	14.17 [30.577]	**49.76**** **[20.899]**	-45.80 [81.957]	28.06 [61.780]	-29.74 [20.558]	69.40 [110.042]	-2.59 [25.304]	3.14 [3.279]	**-30.47**** **[13.391]**
Observation	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814
R-squared	0.198	0.247	0.314	0.055	0.198	0.154	0.318	0.038	0.037	0.044	0.039

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used. Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

(B) Sample: Households with Single Head (Treatment) Compared to those with Married Head (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
Single	-1,288.93*** [155.328]	-240.11*** [33.328]	-166.07*** [26.073]	-74.00*** [18.606]	-387.08*** [66.974]	-206.39*** [49.843]	-88.03*** [14.897]	-550.07*** [104.214]	-117.24*** [30.700]	-4.21* [2.539]	25.67*** [9.387]
After ARRA	-598.20* [346.719]	-85.58 [60.416]	-71.28* [42.358]	-14.31 [38.078]	-130.33 [151.066]	-23.97 [106.829]	-53.84* [31.978]	-303.05 [224.994]	-6.94 [43.120]	1.28 [5.544]	6.71 [17.369]
Single * After ARRA	**461.86***** **[177.481]**	**87.16**** **[37.098]**	**52.22*** **[28.314]**	34.94 [21.373]	63.95 [80.247]	39.16 [61.576]	3.09 [17.759]	**238.48**** **[119.161]**	49.40 [32.786]	-4.76 [3.303]	-6.04 [11.396]
Observation	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814
R-squared	0.199	0.248	0.314	0.055	0.198	0.154	0.318	0.039	0.037	0.044	0.038

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used. Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

(C) Sample: Households with Unemployed Head (Treatment) Compared to those with Employed Head (Control)

	QUARTERLY EXPENDITURE ($2009)										
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
	Total Expenditure	**Food**	**Food at home**	**Food away**	**Housing**	**Shelter**	**Utility**	**Transportation**	**Entertainment**	**Personal care**	**Tobacco**
Single	-894.14*** [129.785]	-112.61*** [26.124]	-31.34* [18.809]	-81.16*** [16.079]	-262.42*** [60.666]	-219.99*** [44.662]	-50.90*** [12.866]	-356.59*** [85.111]	-39.60 [24.412]	-10.85*** [2.271]	8.64 [7.145]
After ARRA	-421.70 [321.756]	-40.95 [56.348]	-50.82 [39.800]	9.87 [34.740]	-138.67 [135.364]	-44.65 [96.159]	-63.09** [29.385]	-169.50 [211.395]	12.56 [38.762]	-3.15 [5.251]	-0.40 [16.925]
Single * After ARRA	**397.15** **[165.374]**	36.51 [33.257]	**43.22*** **[24.858]**	-6.71 [19.463]	**166.22**** **[76.890]**	**154.62***** **[58.095]**	**38.06**** **[17.347]**	61.11 [109.868]	40.38 [26.841]	4.79 [3.355]	11.10 [10.915]
Observation	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814	11,814
R-squared	0.199	0.247	0.314	0.054	0.199	0.154	0.319	0.038	0.037	0.044	0.038

Note: These are coefficients from estimating equation (1). Regressions include demographic variables, month and year dummies. Sampling weight is used. Column (3) and (4) are subcategories of Column (2). Column (6) and (7) are subcategories of Column (5).
*** p<0.01, ** p<0.05, * p<0.1

[Appendix Tables] Summary Statistics: Treatment Group Pre- and Post-ARRA

Table A2.1: Households Who Participated in SNAP Past 12 Months

	Pre-ARRA (N=955)	Post-ARRA (N=1519)	Difference	T-Statistics
Age of Head	38.94	39.00	0.06	0.13
Head, Married	0.26	0.29	0.03	1.54
Own Home	0.20	0.20	0.00	0.10
Family Size	3.27	3.23	-0.04	-0.57
Number of Earners	0.88	0.87	-0.01	-0.28
Number of children less than 18 years old	1.57	1.48	-0.09	-1.46
Head, Less than HS	0.36	0.32	**-0.04****	**-2.24**
Head, HS grad	0.53	0.55	0.01	0.57
Head, College Grad	0.11	0.14	**0.03****	**2.40**
Head, White	0.65	0.67	0.01	0.74
Head, Black	0.30	0.27	-0.03	-1.35
Head, Other race	0.05	0.06	0.01	1.15
Head, Female	0.74	0.72	-0.02	-1.26
Urban	0.92	0.94	**0.02***	**1.75**
Number of hours worked by Headper week	35.25	35.12	-0.13	-0.18
Number of weeks worked by Head	18.46	18.46	0.00	0.00
Social Security Income	1558.56	1757.09	198.53	1.14
Supplemental Security Income	1318.02	1156.61	-161.41	-1.22
Unemployment Compensation	3948.20	6555.10	**2606.9*****	**4.51**
Public Assistance Income	3128.56	2781.00	-347.56	-1.10
Income from Child Support Payment	3226.47	3612.47	386.00	0.84
Income from SNAP	1817.59	2018.86	**201.3****	**2.50**
Medicaid Receipt	0.83	0.88	**0.05*****	**2.80**
Medicare Receipt	0.21	0.20	-0.01	-0.45

Table A2.2: Households with Income ≤ 130% of the Poverty Line

	Pre-ARRA (N=2673)	Post-ARRA (N=3268)	Difference	T-Statistics
Age of Head	37.41	38.10	**0.69****	**1.96**
Head, Married	0.29	0.27	-0.01	-1.08
Own Home	0.25	0.24	-0.01	-1.16
Family Size	2.59	2.60	0.01	0.25
Number of Earners	0.86	0.83	**-0.04***	**-1.88**
Number of children less than 18 years old	1.02	1.00	-0.02	-0.65
Head, Less than HS	0.26	0.25	-0.01	-1.31
Head, HS grad	0.56	0.57	0.01	0.44
Head, College Grad	0.17	0.18	0.01	0.93
Head, White	0.73	0.72	-0.01	-0.83
Head, Black	0.21	0.21	0.00	-0.14
Head, Other race	0.06	0.07	**0.01***	**1.72**
Head, Female	0.61	0.61	-0.01	-0.56
Urban	0.94	0.94	0.00	0.44
Number of hours worked by Head per week	34.61	34.03	-0.58	-1.31
Number of weeks worked by Head	22.61	20.64	**-1.96*****	**-3.33**
Social Security Income	1343.39	1357.19	13.79	0.14
Supplemental Security Income	677.43	633.24	-44.19	-0.71
Unemployment Compensation	3770.75	6575.52	**2804.77*****	**6.14**
Public Assistance Income	2948.96	2910.77	-38.19	-0.12
Income from Child Support Payment	3381.55	3732.46	350.91	0.83
Income from SNAP	539.49	777.05	**237.56*****	**6.09**
Medicaid Receipt	0.75	0.78	**0.04****	**2.19**
Medicare Receipt	0.29	0.26	-0.03	-1.87

Appendix 2A. Items included in Each Expenditure Category in the CEX

A.1 Food

Spending on food includes food consumed at home (mainly through grocery shopping), and food eaten away from home. Alcoholic beverages are reported in a separate category.

a. Food at home
b. Food away from home

A.2 Housing

a. Shelter: For home owners, shelter cost includes mortgage interest, property taxes, maintenance, repairs, and insurance. For home renters, this includes rent for the r ented dwelling.
b. Utility: Fees for natural gas, electricity, fuels, telephone service, and water
c. House operation: Domestic services, babysitting, and child day care
d. House equipment: textiles, furniture, floor coverings, and home appliances

A.3 Transportation

a. Vehicle purchases
b. Vehicle finance charges
c. Gasoline and motor oil
d. Maintenance and repairs
e. Vehicle insurance
f. Vehicle rental, and leases
g. Public transportation

A.4 Health

a. Health insurance
b. Medical services
c. Prescription drugs
d. Medical supplies

A.5 Entertainment

a. Fees and admissions
b. Televisions, radios, and sound equipment
c. Pets, toys, and playground equipment, and others

A.6 Other Expenditures

a. Personal care products and services: products for hair, oral hygiene products, cos metics and bath products, electric personal care appliances, etc.
b. Education and Reading: tuition, fees, textbooks, and supplies
c. Tobacco products
d. Miscellaneous: safety deposit box rental, checking account fees, bank service cha rges
e. Cash contributions

Chapter 3

Are Household Food Expenditures Responsive to Entry onto the Supplemental Nutrition Assistance Program?

3.1 Introduction

The Supplemental Nutrition Assistance Program (SNAP) is the federal government's largest nutrition assistance program. SNAP provides participating low-income families a monthly benefit in the form of Electronic Benefit Transfers (EBT) that can only be used to purchase food at participating retailers. During fiscal year 2013, SNAP spending was a record $82.5 billion, with an average caseload of 47.7 million people per month, about one out of every seven U.S. residents.

Because of growing size of the program and unprecedented number of recipients on SNAP, there has been substantial policy interest in evaluating the effects of program on individual and family outcomes. Among a wide range of outcomes, SNAP's effect on households' food security has been highlighted in the literature. However, self-selection complicates efforts to evaluate the relationship between SNAP receipt and food insecurity because it is likely that households most in need of assistance are also the most likely to apply for benefits. In fact, Gundersen et al. (2008) and other studies have shown that low-income households who receive SNAP are more likely to report food insecurity than similar nonparticipating households (See

Bartfeld & Dunifon, 2006; Gibson-Davis & Foster, 2006; Gundersen & Kreider, 2008; Jensen, 2002; Wilde, 2007; Wilde & Nord, 2005). In an attempt to address this issue, some recent studies use more sophisticated techniques, including instrumental variables approaches, and find a negative relationship between SNAP participation and food insecurity (Nord and Golla 2009; Mykerezi and Mills 2010; Ratcliffe et al. 2011; Shaefer & Gutierrez, 2013). Yet much remains to be learned about the relationship between SNAP and food insecurity.

The mechanism by which SNAP would reduce food insecurity is through expanding recipient households' food budgets. Accordingly, direct evidence of SNAP's ameliorative effect should be apparent in the food expenditures of these households, pre- and post-SNAP entry. Do food expenditures rise, fall, or remain stable in the months following program entry? Do SNAP dollars replace or supplement out-of-pocket spending? What other changes happen to a family around the time of SNAP entry? We delve into these questions by adopting an event-study framework to draw a month-to-month trends in food expenditures, before and after SNAP entry.

We use the Panel Study of Income Dynamics (PSID), the only panel dataset with measures of both household food expenditures and SNAP participation by month. Taking advantage of the PSID's panel structure, we create an event-study analysis sample identified by the month relative to the SNAP entry. We then use these observations to track trends in household food expenditures for the twelve months prior to SNAP entry, and the twelve months 3 following SNAP entry. Our findings suggest that the SNAP shields new entrants from a substantial fall in food expenditures. Even after controlling for demographic characteristics as well as time and state fixed effects, our event-study estimation reveals that total food expenditures show a stable and smooth trend in the months surrounding SNAP entry. This is found to be largely due to SNAP replacing a great share of households' food budget after program entry. We also demonstrate that there are household shocks that co-occur with SNAP entry, such as new spells of unemployment or marital dissolution, suggesting that these shocks may trigger SNAP entry.

3.2 Background and Literature Review

With an average monthly benefit of $159 per person and a maximum monthly benefit of $668 for a family of four, SNAP benefits increase the gross incomes of participating households by 39 percent, and by 45 percent for households with children (CBO report, April 2012). Therefore, we might expect that the SNAP would increase the food expenditures or overall purchasing power of participating households, and that this should reduce their food insecurity.

Using a novel approach, Nord and Golla (2009) find evidence that suggests the food security of households deteriorates considerably in the few months prior to SNAP entry. They match households across two annual Current Population Survey Food Security Supplements (CPS FSS). They exploit food security measures, combined with monthly data on SNAP receipt, to identify two samples: 1) households that entered SNAP during the twelve months before the CPS FSS's food security outcome is measured; and 2) households that entered SNAP during the twelve months after they completed the survey. They find that household food insecurity increases during the few months prior to SNAP entry, and decreases in the months following.

This improved food security after SNAP entry could mainly work through expanded resources available through SNAP dollars. Therefore, understanding the effects of SNAP on household food expenditures would bolster possible mechanisms for ameliorative effects of SNAP that are evident in broad literatures. SNAP benefits should improve food security through increased purchasing power for households' food expenditures. A number of papers, mostly using data from 20 years ago or prior, have examined the impact of SNAP on household food expenditures, focusing on how much SNAP benefits increase household food expenditures compared to what would have occurred with an equal amount of cash. In a review of this literature, Fox et al. (2004) note that 32 studies utilized one of 4 three research approaches: 1) participant versus nonparticipant comparisons; 2) dose-response estimates; and 3) cash-out demonstrations that provide an experimental group with food stamps and a control group with an equivalent cash benefit. These studies consistently find that food stamp participants spend more on food than do comparable non-participants.

Fraker (1990) reports on a major cash-out randomized trial that finds that the marginal propensity to consume food out of food stamps is two to ten times higher than the estimated marginal propensity to consume food out of cash income. A more recent study by Hoynes and Schanzenbach (2007) exploits variation in the timing of the implementation of the Food Stamps Program (from 1963 to 1975) across counties. They find that the introduction of the program led to an overall increase in household total food expenditures among recipients, a decreased propensity to eat out, and mixed results for cash food expenditures (out-of-pocket expenditures).

Yet, none of these studies dynamically examine how food expenditures patterns of households on SNAP change in the months before and after SNAP entry. The current paper employs an event-study methodology that follows month-to-month trends in food expenditures and provides further evidence of the dynamic relationship between SNAP entry and food expenditures.

We may conjecture the trends in food expenditures follow that of food security, given that food expenditures and food security generally move in the same direction. Aligned with findings by Nord and Golla (2009) that food insecurity increased during the few months prior to SNAP entry, household food expenditures may also fall prior to SNAP entry, particularly if economic shocks, such as job loss or marital dissolution, are primary precipitators of new applications for benefits. Mabli & Ohls (2012, p. 5) say that "trigger events" reflect "a series of complex lag structures in both the eligibility and participation equations". New entrants not previously receiving benefits may experience some changes in their circumstances that lead them to apply. Indeed, a number of studies find that SNAP is responsive to economic shocks such as job loss or marital dissolution (Mabli & Ohls, 2012; Hernandez & ZiolGuest, 2009). Thus, such shocks may be a primary cause for falling household food expenditures among new SNAP entrants.

Do households' food expenditures levels fluctuate prior to entering SNAP and recover afterwards? Does SNAP help households maintain constant food expenditures levels, replacing the household's own expenditures on food 5 with public funding during times of need? To provide answers to these questions, we simulate household food

expenditures in the 12-months leading up to SNAP entry, and in the 12-month period after households enter the SNAP program using a nationally representative sample.

3.3 Data and Measures

The PSID is one of the world's longest running household panel surveys. It is among the most important for social science research and for the evaluation of government policies. It is main source for information on changes in income and poverty over time, allowing longitudinal analysis of the income and consumption. Available waves were collected annually from 1968 through 1997, and then biennially after 1997 up until 2011.

The PSID is uniquely suited for our analysis because it is the only nationally representative panel dataset with both measures of household food expenditures and monthly SNAP program participation. We pool data from the four most recent waves of the PSID: 2001, 2003, 2005, and 2007[42]. In each wave, households are asked about average food expenditures at the time of the interview[43], and monthly SNAP participation for the previous 2 calendar years and for the current year. For instance, in the 2001 wave, respondents report average food expenditures at the time of the interview, as well as month-by-month SNAP participation records from January 1999 through the interview month of 2001.

Under-reporting of public transfer receipt in the PSID remains a limitation (Gundersen and Kreider, 2008). Meyer et al. (2009) provides estimates of the extent of under-reporting for ten transfer programs in five major nationally representative surveys, and finds that approximately 80 percent of food stamp dollars are reported in the PSID and the SIPP, while in the other surveys it is closer to 60 percent. For years beginning 2003, a significant improvement is visibly detected in the PSID, with higher reporting rate than that of the SIPP.

[42] We do not use the 2009 and 2011 waves because monthly SNAP participation questions were omitted from the 2009 wave.

[43] Respondents are asked to report the amount they currently spend in an average week, but they are allowed to report annual or monthly amounts. We adjust the reported amount to be representative of monthly food expenditures to be consistent with monthly measures of SNAP participation.

Food expenditures are reported in three different categories in the PSID—food at home, food delivered, and food eaten out. If a household is currently a SNAP recipient, a separate question about the amount of the SNAP benefit is asked.[44] Accordingly, we construct total food expenditures by summing 1) out-of-pocket spending on food at home (i.e. spending not using SNAP benefits), 2) food delivered, 3) food eaten out, and 4) SNAP benefit amount. Li et al. (2010) benchmark the quality of the PSID expenditures data by comparing them with data from Consumer Expenditures Survey (CE). They find that the PSID expenditures compare favorably with those in the CE data, with spending amounts closely aligning in every expenditure category.

The PSID simulates the USDA's food needs standard for each household—an estimate of expected weekly food costs based on the "Low-cost food plan"[45] for a family of the same size and composition. Food needs standard is widely used in order to account for family composition when measuring food expenditures (Zeldes, 1989; Stephen, 2004). The PSID food needs variable, however, is measured in reference to the previous year. Because of different accounting periods of food expenditures and food needs, the food needs variable should not be used at face value when taking ratio between the two. Therefore, we re-constructed the food needs variable with family composition information as of the interview date, using the standard formula taken from USDA Low-Cost Plan (Appendix 3B).

Our main outcome variable is, therefore, the ratio between the food expenditures of the household, over that family's USDA food needs standard, both of which are measured at the time of interview. By dividing a household's food expenditures by its food needs threshold, we account for differences in the family size and composition of heterogeneous households. Moreover, the ratio is easy to interpret– if the ratio is greater than one, a household is spending more than the national average needed for food for the

[44] See Appendix 3A for the question wording.

[45] The USDA provides guides for selecting nutritious diets at home at different cost levels

- Thrifty
- Low
- Moderate
- Liberal

The Thrifty plan, which is the least costly of the four, is the basis for the SNAP benefits. The Low and Moderate programs provide appropriate diets for most people. The Liberal plan, which doubles the cost of the Thrifty plan, permits a greater variety than the other plans. We use the built-in variable in the PSID, food needs standard based on *Low-cost Food Plan*, for the analysis.

low-cost food plan by a household with its composition. If the ratio is less than one, vice versa.

3.4 Methodology and Identification Strategy

3.4.1 Construction of Analysis Sample

We use food expenditures measures, combined with monthly data on SNAP receipt, to identify two samples: 1) households that entered SNAP during the twelve months before food expenditure outcome is measured (food expenditures are treated by SNAP); and 2) households that entered SNAP during the twelve months after they completed the survey (food expenditures are not treated by SNAP).

This sample construction, however, could be complicated by the fact that a family's monthly SNAP participation data is coming from two consecutive PSID waves that are two years apart. For example, if a household was interviewed in April 2001 for the 2001 PSID wave, monthly data on SNAP receipt for 2000 and early 2001 are obtained from the same wave. However, monthly data on SNAP receipt for later 2001 and 2002 are obtained from 2003 PSID wave.

We should note that not all families stay the same over the PSID waves. Some drop from the sample because of attrition.[46] Others change family composition between waves. For example, families can split into two or more sub-families because of marriage or divorce. In order to avoid the results to be confounded by non-random family composition change across waves, we first restrict to households that do not split into multiple families across two waves. This guarantees that monthly SNAP receipt information from two sequential PSID waves corresponds to the same family. It is assuring that the proportion of families that stayed the same with respect to its composition is relatively high across waves: 91% (2001 to 2003), 85% (2003 to 2005), 87.5% (2005 to 2007), and 85.6% (2007 to 2009).

The second restriction that we impose on our sample is that we focus on households that were not on SNAP for at least 12 months prior to the observed SNAP

[46] The proportion of families who drop between waves is the following: 4.9% (2001 to 2003), 6.8% (2003 to 2005), and 4.9% (2005 to 2007).

entry. This restriction provides an estimate of food expenditures, without contamination by households that cycle on and off of SNAP. Because we are interested in SNAP *entry* rather than SNAP *participation*, this restriction leads us to exclude families who use SNAP over long durations. We do not impose any restriction with respect to the duration of a family's SNAP spell, once it starts—some households may have received SNAP for only 1 month, whereas others may have received benefits for 12 months or more.[47]

To understand the effects of the two sample restrictions, we conducted a balance test of demographic variables between households included in our final analysis and households that are excluded due to the two sample restrictions. Table 3.1 presents the result of the test. Our final sample includes almost a third[48] of all families ever reporting SNAP in the PSID waves. In some dimensions such as employment status and race, our final analysis sample is slightly more positively selected, but not considerably so. This is not a surprise since we limit to those who have no SNAP history for at least a year prior to the observed entry, which excludes static SNAP participants, who are more disadvantaged with respect to some observable characteristics. In terms of marital status, home ownership, and education, both groups look substantially similar to each other. Thus, our findings are based on estimates from a sample of relatively new SNAP entrants and may not reflect long-term static SNAP participants. Still, our sample consists of nearly a third of those ever reporting SNAP receipt in the PSID for the years under study. Given these results, it is likely that this study provides lower bounds of the food expenditures pattern, suggesting that the patterns may be more drastic when all SNAP recipients are taken into account.

After the two restrictions imposed, finally, we group households according to the months between SNAP entry and the measurement of food expenditures. Households that entered SNAP 12 months before reporting food expenditures are grouped together; those entering SNAP 11 months before reporting food expenditures are grouped together; continuing until the final group consists of households entering SNAP 12 months after the food expenditures are measured. Thus, the study sample represents a progression of

[47] In sensitivity analyses, we restrict the sample to households whose SNAP spells are at least 6 months or longer. Results remain substantively similar.

[48] About 60 percent of ever-receiving SNAP households are excluded due to 12-month–no-entry restriction, and additional 7 percent are excluded due to no-family composition change restriction.

independent families sorted on the timing of the observation when it reports food expenditures relative to the month of SNAP entry.

Our unit of analysis is the household-SNAP spells. As long as the conditions discussed above are met, households with multiple SNAP spells over the course of the four PSID waves can be included multiple times. The primary analysis sample consists of the 1,764 household-SNAP spells from 1,477 unique households.

3.4.2 Analytic Methods: Event-Study Analysis Before and After SNAP Entry

To explore the underlying trends, we simply plot the average food expenditures-to-food needs ratio across months surrounding SNAP entry in Figure 3.1 as our descriptive analysis.

We evaluate our descriptive trend in an event study specification to test the statistical significance of the association between food expenditures-to-needs ratio and months relative to SNAP entry. Specifically, equation (1) shows the event study model:

$$(1) \qquad \left(\frac{\text{Food Expenditures}}{\text{Food Needs}}\right)_{ht} = \beta_0 + \sum_{i=-12}^{12} \beta_i \cdot I(\tau_{ht} = i) + \pi \cdot X_{ht} + \rho_t + \theta_y + \sigma_s + \sigma_s \cdot y + \varepsilon_{ht}$$

τ_{ht} denotes the number of months relative to the event of SNAP entry for household *h* interviewed in month *t*. It is defined so that $\tau_{ht} = 0$ if food expenditures are measured in the same month as the SNAP entry. For $\tau_{ht} > 0$, food expenditures are treated by SNAP. For instance, $\tau_{ht} = 1$ indicates food expenditures measured 1 month after SNAP entry. For $\tau_{ht} < 0$, food expenditures are untreated by SNAP (i.e. food expenditures are measured before the program starts). When drawing plots, I omit the $\tau_{ht} = 0$ category as a reference group so that the β_i coefficients map out differences relative to the outcome at the time of SNAP entry. Our event study model includes fixed effects for state (σ_s), interview year (θ_y), and interview month (ρ_t) as well as state-specific time trends ($\sigma_s \cdot y$). Given that the study period overlaps with a period of rapid

changes in the SNAP as well as other welfare programs, this set of variables is to absorb any state-specific or time-specific trend that could affect food expenditures differently. Moreover, it captures heterogeneity in SNAP spells as well as food expenditures that could be taking place at different points of time over the study period. Demographic characteristics of household heads[49] such as race, education, sex, marital status, and employment status[50] are also controlled for.

In practice, demographic characteristics could be unbalanced in event-time in a way that interferes with identification. Therefore, we also run the event-study analysis with several fixed characteristics of heads as dependent variables. We show there is no significant trend in such characteristics as race, educational attainment, and age of head across months around SNAP entry. Job loss and marital dissolution are two major forms of household economic shocks, which may trigger SNAP entry and thus may be associated with changes in household food expenditures. With this in mind, we also report event-study analysis in which outcomes are not food expenditures, but are 1) whether a head of household is working, and 2) whether a head of household is married.

3.5 Findings

3.5.1 Descriptive Results

Figure 3.1 simply plots the average ratio of household food expenditures-to-food needs for our analysis sample, in reference to the event of SNAP entry (denoted as 0 on the horizontal axis). Months prior to SNAP entry begin with −12 and progress to −1. Months following SNAP entry progress from +1 to +12. The solid line is total food expenditures (non-SNAP food expenditures plus SNAP benefit amount) as a ratio to food needs. Total food expenditures can be decomposed into two components. The dashed line is non-SNAP food expenditures, which are the sum of out-of-pocket spending on food at

[49] The PSID defines household head as the person with the most financial responsibility and at least 16 years old. If this person is female and she has a husband in the family unit, then he is designated as Head. However, if the husband or boyfriend is incapacitated and unable to fulfill the functions of Head, then the family unit will have a female Head.
[50] Demographic controls include a set of indicators for a head being white, male, married, working, high school graduate, some college or more, and whether s/he owns home.

home, food delivered, and food eaten out. The dotted line indicates the size of SNAP benefits as a ratio to food needs.

Examining first the non-SNAP food expenditures-to-needs ratio trend line, we see stability during the months prior to SNAP entry, with some slight fluctuations. Households a year away from SNAP entry have a ratio more than 1, suggesting that their food expenditures exceeds their USDA Low-cost food needs standard. The ratio holds substantively steady between .82 and 1.04.

The ratio of non-SNAP food expenditures-to-needs is substantially lower after SNAP entry. It is .55 during the month of SNAP entry, and the ratio bottoms out following SNAP entry around .57. This estimate indicates that households in the first month of SNAP participation have non-SNAP food expenditures that are a little over half of their household's basic food needs, substantially lower than households who are a few months away from entering SNAP. This pattern clearly reveals that SNAP recipients reduce their own out-of-pocket spending on food by replacing it with SNAP benefit.

Lastly, the lowest dotted line plots the introduction of SNAP resources into the household's food expenditures, rising from zero[51], to accounting for an average of .36 of the household's basic food needs a month after. This proportion seems reasonable, given the heterogeneity of our sample employed in this study, which includes very different household configurations that would be eligible for greater or smaller SNAP benefits. Note also that some households included in our post-SNAP sample may have cycled off SNAP during the months following entry.

The effect of SNAP benefits on food expenditures are made clear by the wedge that opens up between the total food expenditures and the non-SNAP expenditures. While the non-SNAP food expenditures-to-food needs ratio is different considerably after the entry, overall food expenditures—which incorporate SNAP benefits—change far less, bottoming out at .80 for households three months following SNAP entry. When SNAP dollars are accounted for, the ratio of total food expenditures-to-food needs rises somewhat among households as they are more months past SNAP entry, so that by 8

[51] Even though we limit to the households who were not on SNAP for at least 12 months prior to the observed SNAP entry, there are some positive SNAP expenditure plotted in the months prior to SNAP entry in Figure 1. This is mainly due to reporting errors in the PSID: negligible number of households reported positive SNAP dollar amounts even if they said they did not receive any SNAP benefits.

months following SNAP entry, the ratio is well within the range of pre-SNAP food expenditures. Even then, though, SNAP accounts for a considerable amount of the food expenditures of the average households. In essence, the SNAP benefits visibly appear to act as a "safety net" for households who have recently entered the program, cushioning them from the extent of a possible negative shock in food expenditures that they might otherwise have faced.

3.5.2 Event Study

3.5.2.1 Food Expenditures-to-Food Needs Ratio

Next we test the relationship between our key outcome, the ratio of food expenditures-to-food needs, and months relative to SNAP entry, using a multivariate framework. We estimate using the event-study methodology in an effort to control for various demographic characteristics of household heads. We also include time and state fixed effect so that the analysis is not confounded by seasonal trends or particular state characteristics.

Figure 3.2 plots the regression coefficients β_i from equation (1), where i ranges from −12 to +12 months, separately for three different food expenditures outcomes. Since the month of SNAP entry (i=0) is omitted as the reference month, the β_i coefficients represents the difference in food expenditures-to-needs ratio in month i, relative to the level at the SNAP entry. For example, the coefficient .36 on non-SNAP expenditures a month prior to SNAP entry implies the following: non-SNAP food expenditures account for a higher share of the household's basic food needs in a month prior to SNAP entry than in the month of SNAP entry, by 36 percent. The observed decrease in non-SNAP food expenditures-to-needs ratio that starts from two months prior to SNAP entry is surprisingly consistent with current SNAP application system: In almost all states, SNAP offices make a decision on approval of an applicant's eligibility within 30 days of application (or within 7 days for applicants who qualify for "expedited" SNAP benefits).

This event-study plot affirms that the descriptive trend in Figure 3.1 is also statistically supported even after controlling for households' demographics, time trend and state characteristics. The non-SNAP food expenditures-to-food needs ratio changes

dramatically around the time of SNAP entry and levels off during the following months after SNAP program. The total food expenditures-to-needs ratio also fluctuates across the months surrounding SNAP entry, but the change is much less than that of non-SNAP expenditures. Overall, total food expenditures shows a relatively stable and smooth trend across all event times, suggesting the role of SNAP as a safety-net program for low-income households.

3.5.2.2 Co-Occurrence of Economic Shocks at the Time of SNAP Entry

Estimates presented in Figure 3.3 and Figure 3.4 are consistent with previous research which finds that many households experience economic shocks, prior to SNAP entry. Figure 3.3 plots the proportion of households in which the head is currently working. Note that the level of outcome is normalized to zero at the time of entry, thus estimates in other event months are changes relative to the entry month. Before SNAP participation, the proportion of households with a working head is greater by 10 to 30 percentage point compared to the fraction at the time of SNAP entry. But it starts to lower substantially beginning two months prior to SNAP entry. We see a slow and gradual increase in the proportion of households with a working head in the post-SNAP sample.

A similar pattern is observed for marital status in Figure 3.4. Three months prior to SNAP entry, the proportion of married household heads is higher by 30 percentage points. The fraction of married head hits the lowest point right before SNAP entry. High levels of marital dissolution just before program entry likely lead these families to seek governmental transfer thereafter. Both Figures 3 and 4 suggest that program entry is likely to be preceded by household economic shocks.

Some may be concerned about any possibility of unbalanced nature of pre- and post-SNAP sample, which could possibly confound the results. In Figures A3.1, A3.2 and A3.3, we plot the similar plots but replace the outcomes with fixed characteristics of household heads: indicators for a head being black, years of schooling, and age of a head, respectively. The Appendix Figures show that all estimates are not distinguishable from zero with large standard errors for all months before and after SNAP entry. This reveals that there is no statistically significant underlying trend in any of these fixed

characteristics across pre- and post-SNAP groups, giving us confidence that our findings are not driven by the nature of unbalanced characteristics of two samples.

3.6 Conclusion

Building upon a wide body of empirical literature that evaluates the impact of SNAP on economic, social, and health outcomes, this study provides direct evidence of SNAP, which is immediately reflected on household food expenditures. Taking advantage of the most recent PSID data available, with measures of both food expenditures and monthly SNAP participation, we depict the month-to-month food expenditures trend with an event-study analysis.

Our estimates uncover the dynamic relationship between food expenditures and SNAP entry. First, we start with descriptive analysis, simply plotting the average food expenditures-to-needs ratio for the 12 months leading up to SNAP entry to 12 months after SNAP entry. Then we confirm that this descriptive pattern is supported with statistical significance in the multivariate event-study analyses. Both descriptive and multivariate analyses consistently tell similar story over the course of 2-year time frame and identify a potentially ameliorative effect of SNAP. Specifically, we observe the estimated non-SNAP food expenditures becomes lower substantially at the time of SNAP entry, while changes in total food expenditures are buffered by increased expenditures using SNAP, suggesting a role of SNAP a protection against economic shocks.

Furthermore, we replace food expenditure outcome with economic status of a household head to discover any changes happening to a family around the time of SNAP entry. We find a strong relationship between time to SNAP entrance and the proportion of households with working and married heads. This offers evidence that the timing of SNAP entry often coincides with the occurrence of household economic shocks.

In essence, the SNAP benefits visibly appear to act as a "safety net" for households who have recently entered the program, cushioning them from the extent of a negative shock in food expenditures that they might otherwise have faced. This study offers a credible estimate to what extent SNAP may supplement participants' income and help them expand their food budget accordingly. Because of the program's large size and

prevalence, there is substantial policy interest in evaluating its effectiveness. This study, thus, provides compelling evidence of SNAP's pivotal role in improving food expenditures for households who would otherwise be at greater risk of food insecurity and lack of food choices, in the absence of this safety net program.

References

Almond, Douglas, Hoynes, Hilary W., and Diane Schanzenbach, "Inside the War on Poverty: The Impact of Food Stamps on Birth Outcomes," *The Review of Economics and Statistics, 93(2)* (2011), 387─403

Bartfeld, Judi and Rachel Dunifon, "State-level Predictors of Food Insecurity Among Households with Children," *Journal of Policy Analysis and Management, 25(4)* (2006), 921─942

Borjas, George J., "Food Insecurity and Public Assistance," *Journal of Public Economics, 88(7-8),* (2004), 1421─1443

Dahl, Molly, FitzGerald, Kathleen, Holcombe, Emily, and Jonathan Schwabish, "The Supplemental Nutrition Assistance Program," *Congressional Budget Office Report* (2012), 1─12

Fox, Mary K., Hamilton, William L., and Biing-Hwan Lin, "Effects of Food Assistance and Nutrition Programs on Nutrition and Health: Volume 3, Literature Review," *Food Assistance and Nutrition Research Report No. 19-3.* Economic Research Service, U.S. Department of Agriculture (2004), 1─320

Fraker, Thomas M., "The Effects of Food Stamps on Food Expenditures: A Review of the Literature," *Food and Nutrition Service,* U.S. Department of Agriculture (1990)

Fraker, Thomas M., Martini, Alberto P., Ohls, James C., and Michael Ponza, "The Effects of Cashing-Out Food Stamps on Household Food Use and the Costs of

Issuing Benefits," *Journal of Policy Analysis and Management, 14(3)* (2007), 372─392

Gibson-Davis, Christina M. and E. Michael Foster, "A Cautionary Tale: Using Propensity Scores to Estimate the Effect of Food Stamps on Food Insecurity," *Social Service Review, 80(1)* (2006), 93─126

Grieger, Lloyd D., Danziger, Sheldon H., and Robert F. Schoeni, "Accurately Measuring the Trend in Poverty in the United States Using the Panel Study of Income Dynamics," *Journal of Economic and Social Measurement, 34* (2-3) (2009), 105–117

Gutierrez, Italo A. and H. Luke Shaefer, "The Supplemental Nutrition Assistance Program and Material Hardships Among Low-Income Households with Children," *Social Service Review, 87(4)* (2013), 753–779

Gundersen, Craig, Jolliffe, Dean, and Laura Tiehen, "The Challenge of Program Evaluation: When Increasing Program Participation Decreases the Relative Well-being of Participants," *Food Policy, 34(4)* (2009), 367─376

Gundersen, Craig and Brent Kreider, "Food Stamps and Food Insecurity: What Can Be Learned in the Presence of Nonclassical Measurement Error?" *The Journal of Human Resources, 43(2)* (2008), 352─382

Hernandez, Daphne C. and Kathleen M. Ziol-Guest, "Income Volatility and Family Structure Patterns: Association with Stability and Change in Food Stamp Program Participation," *Journal of Family and Economic Issues, 30(4*) (2009), 357─371

Hoynes, Hilary W. and Diane Schanzenbach, "Consumption Responses to In-Kind Transfers: Evidence from the Introduction of the Food Stamps Program," *American Economic Journal: Applied Economics, 1(4)* (2009), 109─139

Jacobson, Louis S., Lalonde, Robert J., and Daniel G. Sullivan, "Earnings Losses of Displaced Workers," *The American Economic Review, 83(4)* (1993), 685─709

Jensen, Helen H., "Food Insecurity and the Food Stamps Program," *American Journal of Agricultural Economics, 84(5)* (2002), 1215─1228

Li, Geng, Schoeni, Robert F., Danziger, Sheldon H., and Kerwin Kofi Charles, "New Expenditures Data in the PSID: Comparisons with the CE," *Monthly Labor Review, Bureau of Labor Statistics, 133(2)* (2010), 29─39

Mabli, James, Tordella, Stephen, Castner, Laura, Godfrey, Thomas, and Priscilla Foran, "Dynamics of Supplemental Nutrition Assistance Program Participation in the Mid-2000s," *Mathematica Policy Research Reports No. 7136, Mathematica Policy Research* (2011)

Mabli, James and James C. Ohls, "Supplemental Nutrition Assistance Program Dynamics and Employment Transitions: The Role of Employment Instability," *Applied Economic Perspectives and Policy, 34(1)* (2012), 187─213

Meyer, Bruce D., Mok, Wallace K.C., and James X. Sullivan, "The Under-Reporting of Transfers in Household Surveys: Its Nature and Consequences," *NBER Working Papers No. 15181* (2009), 1─94

Mykerezi, Elton and Bradford F. Mills, "The Impact of Food Stamp Program Participation on Household Food Insecurity," *American Journal of Agricultural Economics, 92(5)* (2010), 1379─1391

Nord, Mark and Marie Golla, "Does SNAP Decrease Food Insecurity? Untangling the Self-Selection Effect," *Economic Research Report No. ERR-85*, Economic Research Service, U.S. Department of Agriculture (2009)

Nord, Mark, and Parke Wilde, "The Effects of Food Stamps on Food Security: A Panel Data Approach," *Review of Agricultural Economics, 27(3)* (2005), 425─432

Ratcliffe, Caroline and McKernan Signe-Mary, "How Much Does the Supplemental Nutrition Assistance Program Reduce Food Insecurity?" *American Journal of Agricultural Economics, 93(4)* (2011), 1082─1098

Salathe, Larry E., "The Food Stamp Program and Low-Income Households' Food Purchases," *Agricultural Economics Research, 32(4)* (1980), 33─41

Schmeiser, Max D., "The Impact of Long-term Participation in the Supplemental Nutrition Assistance Program on Child Obesity," *Health Economics, 21(4)* (2011), 386─404

Southworth, Herman M., "The Economics of Public Measures to Subsidize Food Expenditures," *Journal of Farm Economics, 27(1)* (1945), 38─66

Stephen, Melvin Jr., "Job Loss Expectations, Realizations and Household Consumption Behavior," *The Review of Economics and Statistics, 86(1)* (2004), 253─269

Wilde, Parke, "Measuring the Effect of Food Stamps on Food Insecurity and Hunger: Research and Policy Considerations," *Journal of Nutrition, 137(2)* (2007), 307─310

Yen, Steven T., Andrews, Margaret, Chen, Zhuo A., and David B. Eastwood, "Food Stamp Participation and Food Insecurity: An Instrumental Variables Approach," *American Journal of Agricultural Economics, 90(1)* (2008), 117─132

Zeldes, Steven P., "Consumption and Liquidity Constraints: An Empirical Investigation," *Journal of Political Economy, 97(2)* (1989), 305─46

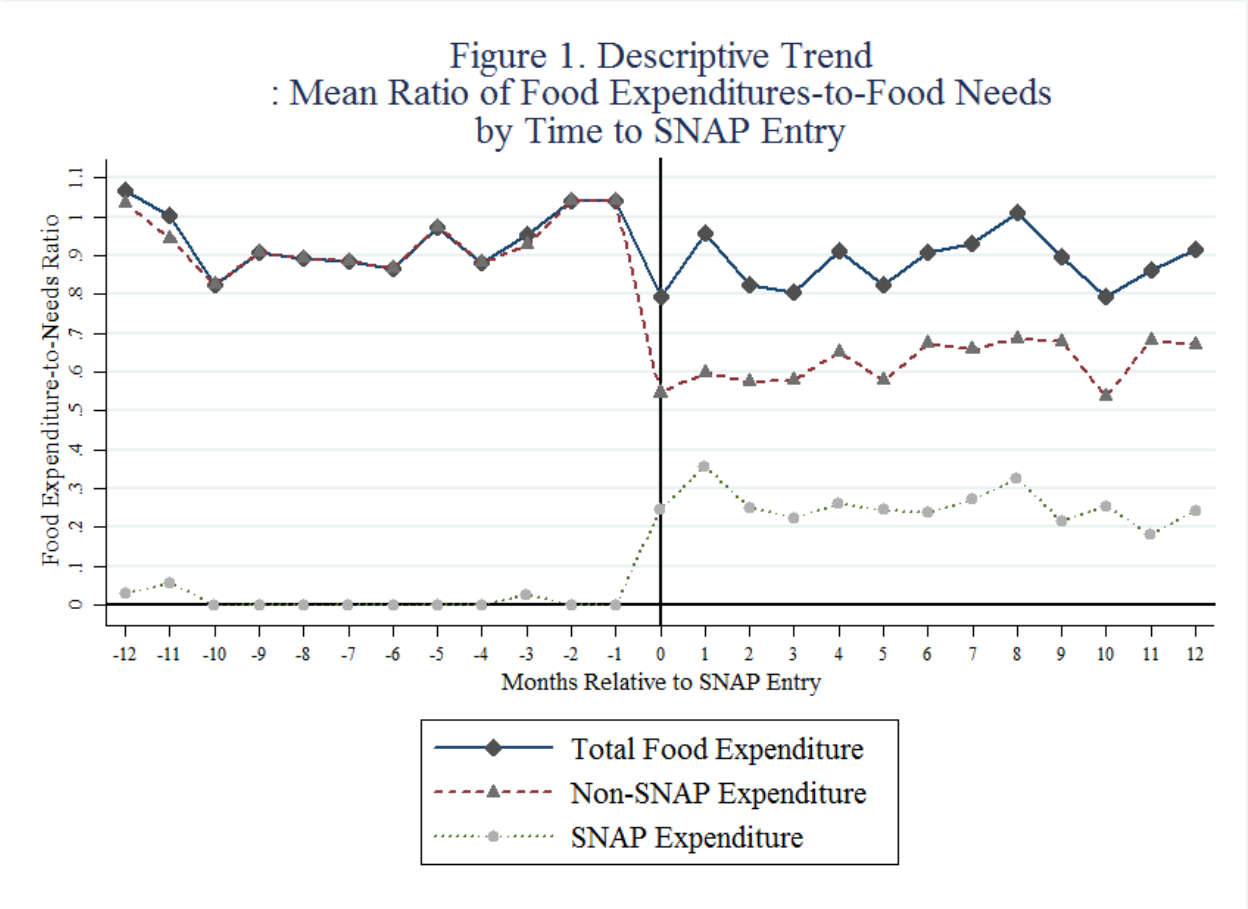
Table 3.1: Comparison of Demographic Characteristics Between Families in Final Sample and Those Excluded

Demographic variables	In Sample	Excluded	Difference	T statistics
Age of head	37.5	39.8	-2.3	-5.3
Head is male	0.53	0.45	0.07	4.6
Number of children	1.3	1.6	-0.3	-7.2
Head is married	0.27	0.26	0.01	0.98
Own home	0.31	0.31	0	0.07
Head working	0.6	0.55	0.05	3.5
Head white	0.33	0.29	0.04	2.7
Head black	0.59	0.64	-0.04	-2.8
Head's education	11.6	11.4	0.18	2.2
Observations	1,477	2,989		

Source: Authors' analysis of the Panel Study of Income Dynamics, 2001, 2003, 2005, and 2007 waves

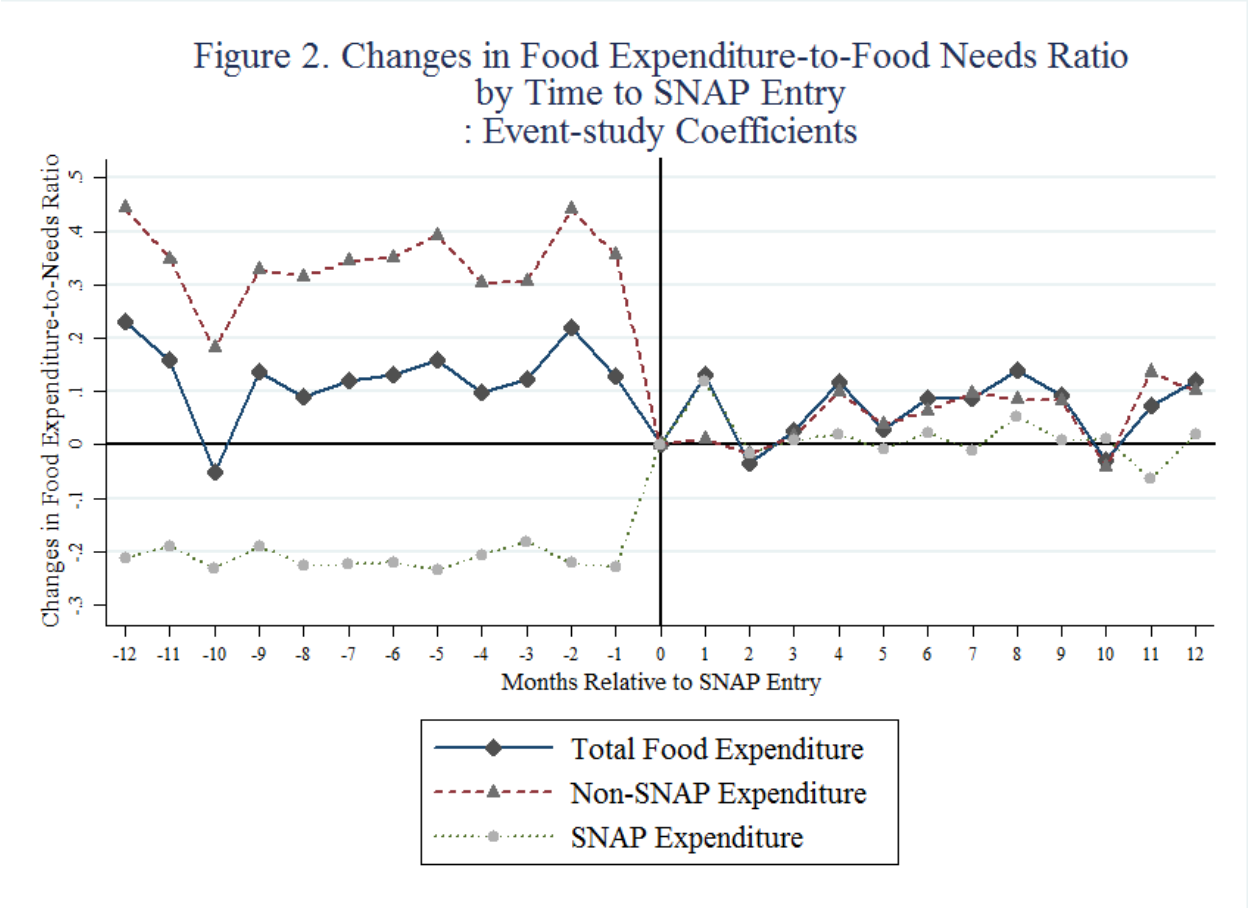
Note: Two sample restrictions, 1) 12-month–no-SNAP-entry, and 2) No family split across two consecutive PSID waves, result in some observations excluded from the analysis sample (2,989 observations). In Table 1, we implement a balance test between our analysis sample (1,477 observations) and those who are excluded (2,989 observations).

Figure 3.1



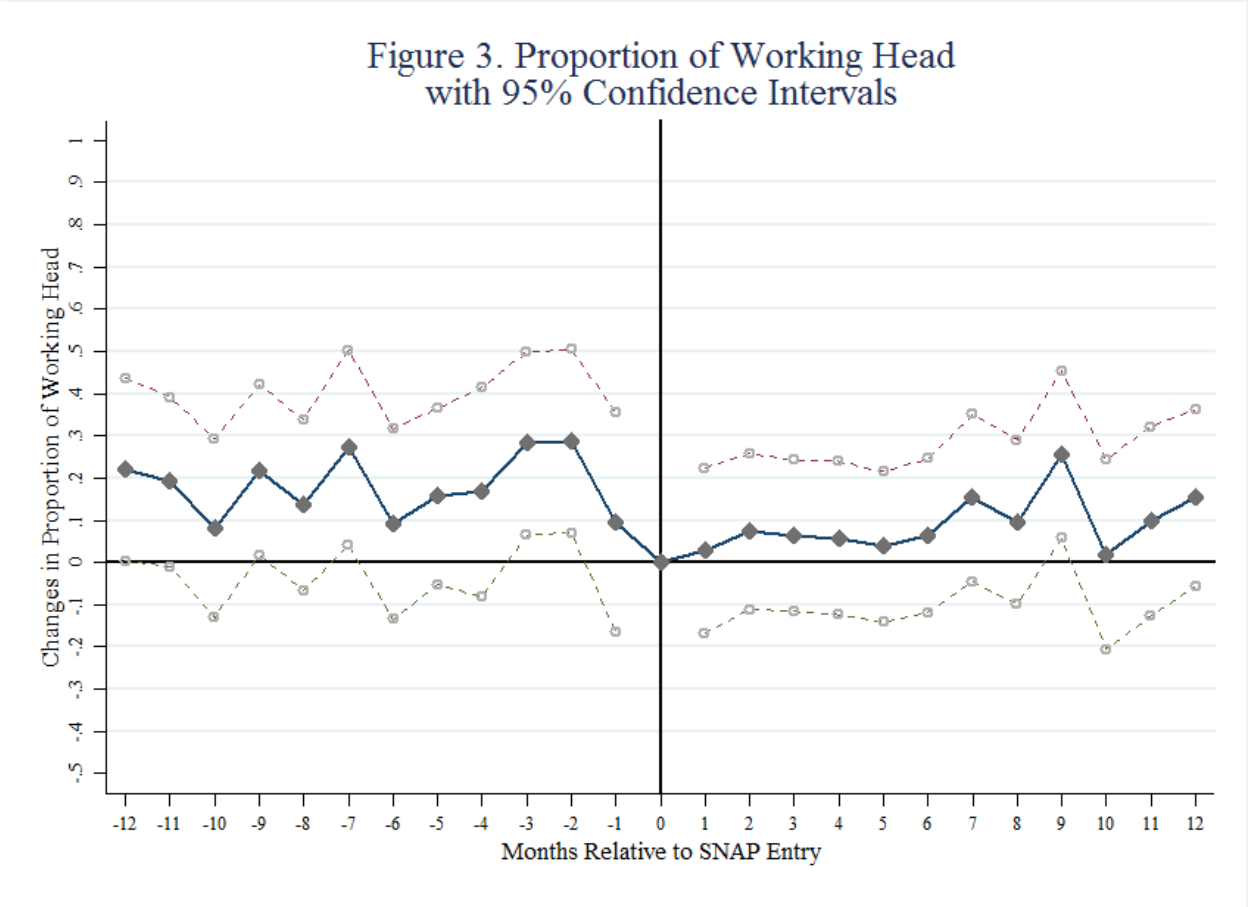
Source: Authors' analysis of the Panel Study of Income Dynamics, 2001, 2003, 2005, and 2007 waves
Note: Estimates are the mean ratio of household food expenditures-to-food needs, broken down into SNAP and non-SNAP expenditures. It plots a mean outcome at each time point, unadjusted for controls.

Figure 3.2



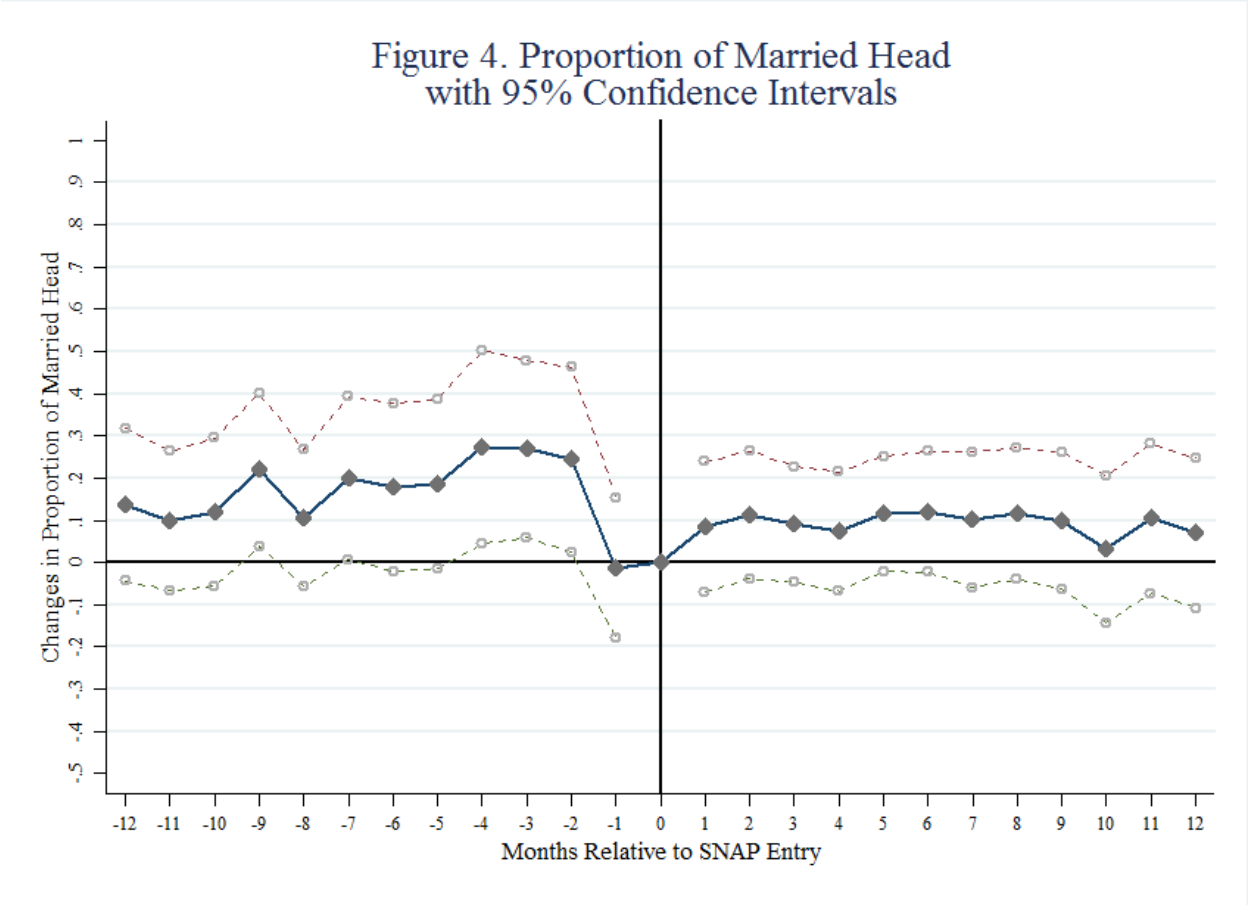
Source: Authors' analysis of the Panel Study of Income Dynamics, 2001, 2003, 2005, and 2007 waves
Note: It plots the regression coefficients β_i from equation (1) with the month of SNAP entry (i=0) omitted as a reference group. The coefficient β_i for each month represents the food expenditures-to-needs ratio relative to the level at the month of SNAP entry, which is normalized to zero. Month, year, and state fixed effects as well as state-specific time trends are included. The regressions control for head's education, race, sex, marital and employment status, and whether the household owns home.

Figure 3.3



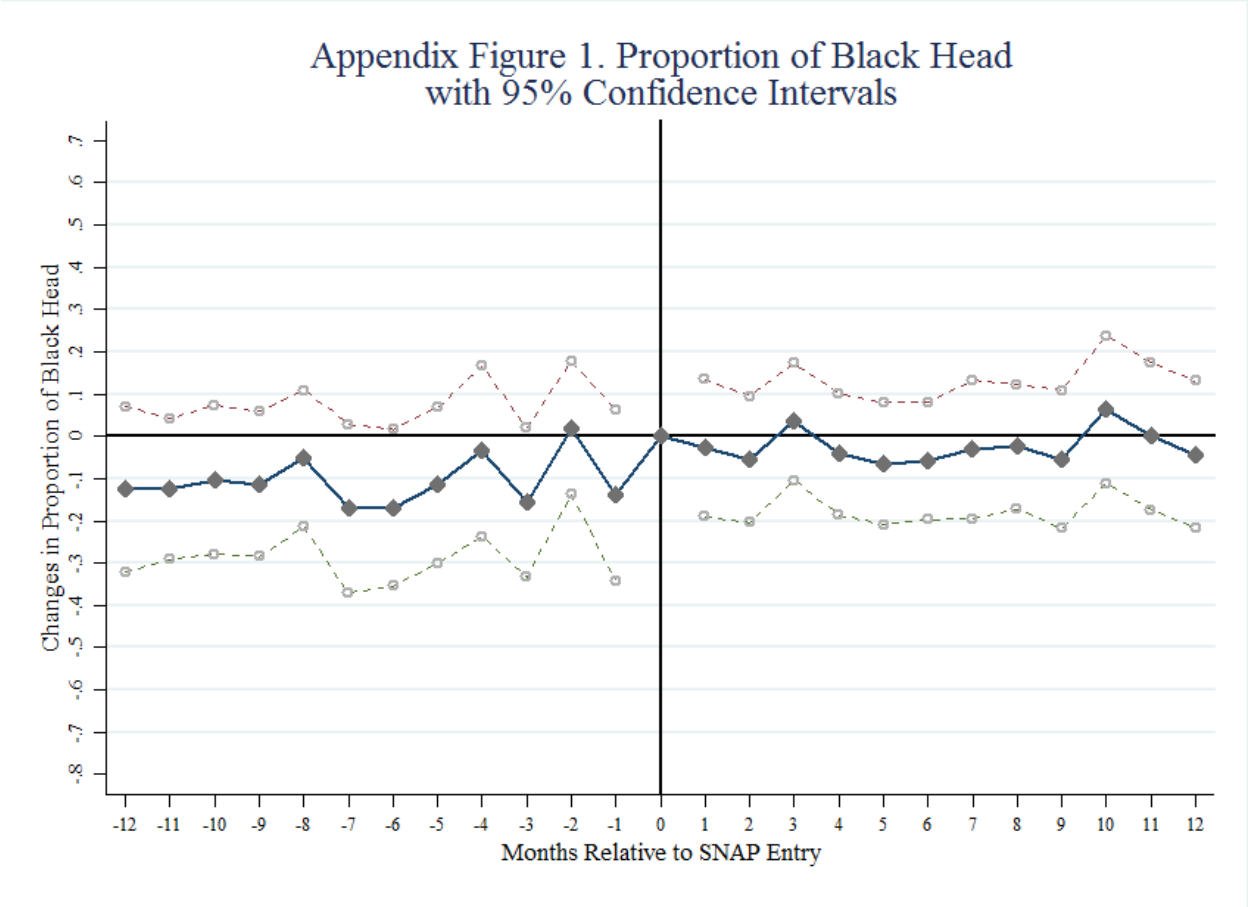
Source: Authors' analysis of the Panel Study of Income Dynamics, 2001, 2003, 2005, and 2007 waves
Note: It plots the regression coefficients β_i from equation (1) with the month of SNAP entry (i=0) omitted as a reference group in the event-study model. Each data point represents the difference in the proportion of working head relative to that at the month of SNAP entry, which is normalized to zero. Month, year, and state fixed effects as well as state-specific time trends are included.

Figure 3.4



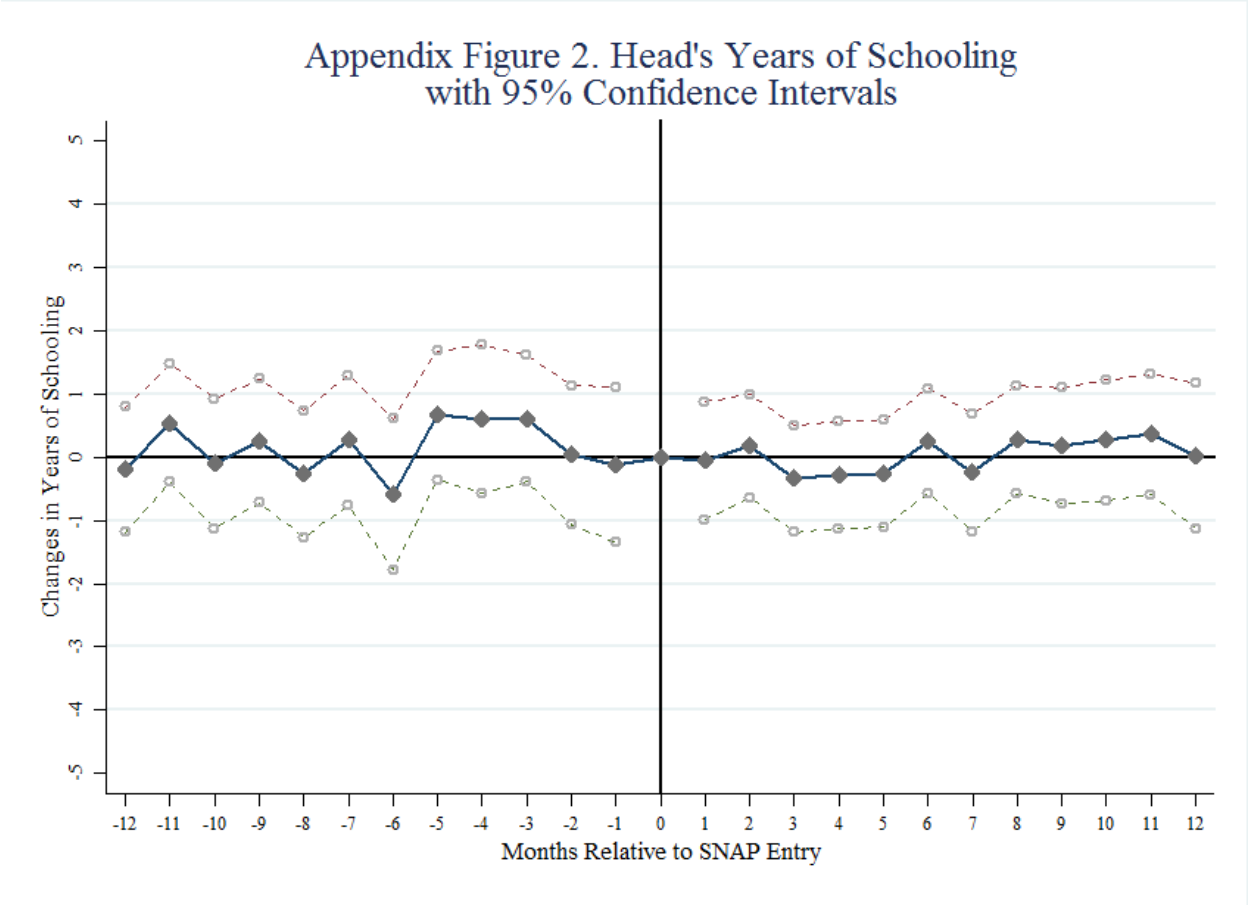
Source: Authors' analysis of the Panel Study of Income Dynamics, 2001, 2003, 2005, and 2007 waves
Note: It plots the regression coefficients β_i from equation (1) with the month of SNAP entry (i=0) omitted as a reference group in the event-study model. Each data point represents the difference in the proportion of married head relative to that at the month of SNAP entry, which is normalized to zero. Month, year, and state fixed effects as well as state-specific time trends are included.

Figure A3.1



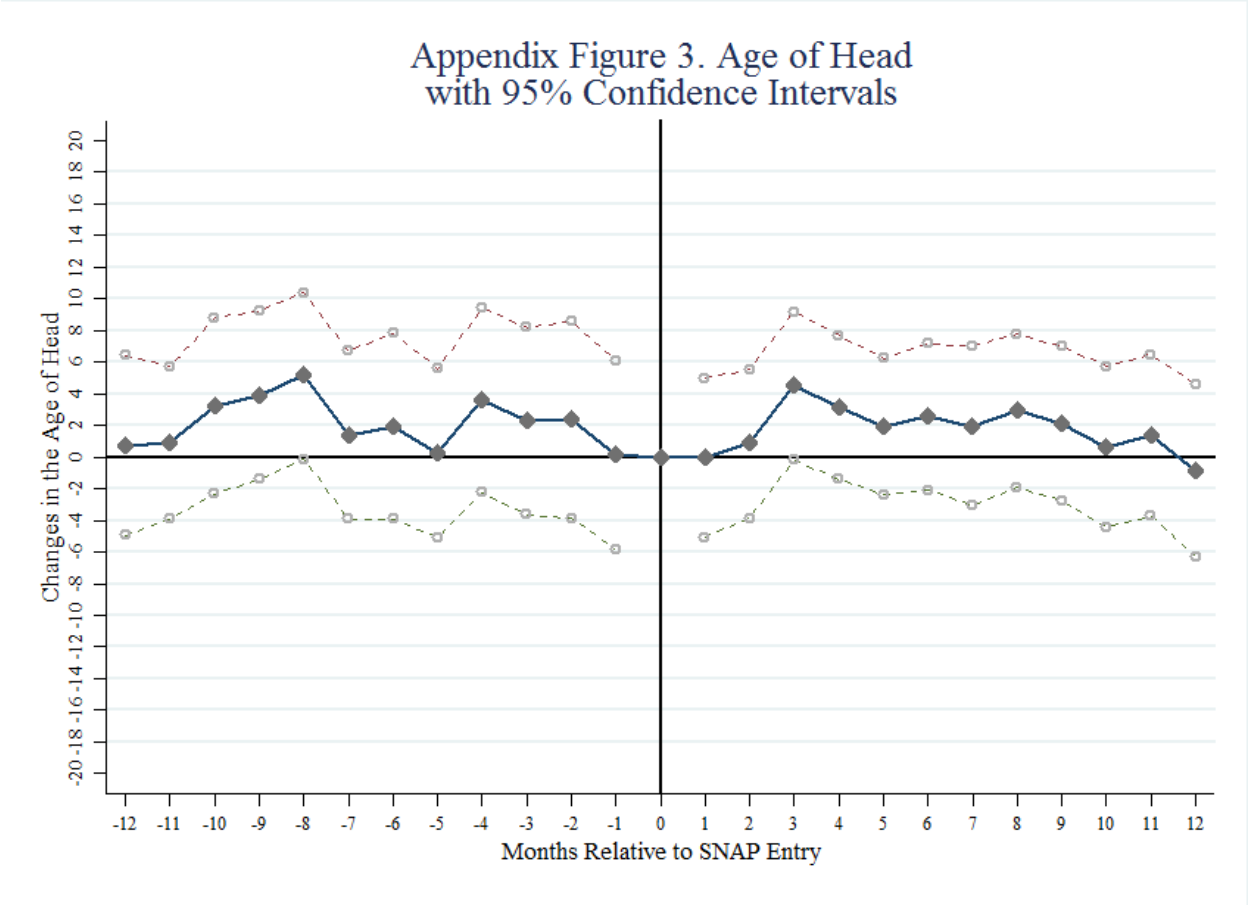
Source: Authors' analysis of the Panel Study of Income Dynamics, 2001, 2003, 2005, and 2007 waves
Note: It plots the regression coefficients β_i from equation (1) with the month of SNAP entry (i=0) omitted as a reference group in the event-study model. Each data point represents the difference in the proportion of black head relative to that at the month of SNAP entry, which is normalized to zero. Month, year, and state fixed effects as well as state-specific time trends are included.

Figure A3.2



Source: Authors' analysis of the Panel Study of Income Dynamics, 2001, 2003, 2005, and 2007 waves
Note: It plots the regression coefficients β_i from equation (1) with the month of SNAP entry (i=0) omitted as a reference group in the event-study model. Each data point represents the difference in years of schooling of head relative to that at the month of SNAP entry, which is normalized to zero. Month, year, and state fixed effects as well as state-specific time trends are included.

Figure A3.3



Source: Authors' analysis of the Panel Study of Income Dynamics, 2001, 2003, 2005, and 2007 waves
Note: It plots the regression coefficients β_i from equation (1) with the month of SNAP entry (i=0) omitted as a reference group in the event-study model. Each data point represents the difference in age of head relative to that at the month of SNAP entry, which is normalized to zero. Month, year, and state fixed effects as well as state-specific time trends are included.

Appendix 3A

SNAP Participation and Benefit Amount Questionnaire in PSID

In PSID interview, they ask about SNAP monthly participation and SNAP benefit amount of the previous two years and current year of the interview (For example, in 2001 PSID, interviewers ask about 1999, 2000, and 2001 SNAP participation status). Here, we show the actual 2001 PSID questionnaire. Same questions are asked in 2003 through 2007 PSID.

- Did you (or anyone in your family) use government food stamps at any time in [year]?

1 YES 5 NO 8 DK 9 NA; refused

- How many dollars' worth of stamps did you get in [year]?—AMOUNT

.01 - 999,996.99	Actual amount
999,997.00	$999,997 or more
999,998.00	DK
999,999.00	NA; refused
.00	Inap: did not receive food stamps in [year]

- How many dollars' worth of stamps did you get in [year]?--TIME UNIT

3 Week
4 Two weeks
5 Month
6 Year
7 Other
8 DK
9 NA; refused
0 Inap.: did not receive food stamps in [year]

- During which months did you receive food stamps?--JANUARY [year]

1 YES 9 NA; DK
0 Inap.: food stamps not used during this month; received no food stamps in [year]

- During which months did you receive food stamps?--FEBRUARY [year]

- During which months did you receive food stamps?--MARCH [year]

...

- During which months did you receive food stamps?--DECEMBER [year]

Appendix 3B

How to Construct Annual USDA Needs Standard Variable

1. Estimate the weekly food cost for each person according to age and sex at 1967 prices. These values are based on USDA Low-Cost Plan estimates of weekly food costs, then are summed for the family as it was at the time of the interview

   Individual Food Standard (Low Cost)

   $3.9 for both males and females under age 4
   $4.6 for both males and females under age 4-6
   $5.5 for both males and females under age 7-9
   $6.4 for males age 10-12
   $6.3 for females age 10-12
   $7.4 for males age 13-15
   $6.9 for females age 13-15
   $8.7 for males age 16-20
   $7.2 for females age 16-20
   $7.5 for males age 21-35
   $6.5 for females age 21-35
   $6.9 for males age 36-55
   $6.3 for females age 36-55
   $6.3 for males age 56 and older
   $5.4 for females age 56 and older

2. Multiply this by 52 to convert to an annual amount and further adjust for economies of scale by USDA rules as follows:

   Single person: add 20%
   Two persons: add 10%
   Three persons: add 5%
   Four persons: no change
   Five persons: deduct 5%
   Six or more persons: deduct 10%

3. An additional adjustment for diseconomies of small households was made as follows:

   4.89 times the food needs for single persons
   3.70 times the food needs for two-person units
   3.00 times the food needs for all other units