The Effect of Food Stamps on Children's Health: Evidence from Immigrants' Changing Eligibility

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Abstract

The Food Stamp program is currently one of the largest safety net programs in the United States and is especially important for families with children: 25% of all children received Food Stamp benefits in 2011. The existing evidence on the effects of Food Stamps on children's and families' outcomes is limited, however, because it is a federal program with little quasi-experimental variation. I utilize a large, recent source of quasi-experimental variation–changes in documented immigrants' eligibility across states and over time from 1996 to 2003–to estimate the effect of Food Stamps on children's health. I study the medium-run health effects of these policy changes on U.S.-born children of immigrants, whose parents were subject to the eligibility variation. I find loss of parental eligibility has large effects on contemporaneous household program participation, and an additional year of parental eligibility, between the time children are in utero to age 4, leads to large improvements in health outcomes at ages 6-16. This provides some of the first evidence that early-life resource shocks impact later-life health as early as school age.

JEL Codes: H5, I1, I3

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"Doctors are warning that if Congress cuts food stamps, the federal government could be socked with bigger health bills. Maybe not immediately, they say, but over time if the poor wind up in doctors' offices or hospitals as a result."

Associated Press, January 9, 2014

1 Introduction

The Food Stamp program is the largest cash or near-cash means-tested safety net program in the United States.[1] Nearly 15% of the total population and 25% of all children received benefits from the program in 2011, up from 6-10% of the population in the 1990s and early 2000s. Among families with children that participate in the program, Food Stamps play a crucial role in their total resources; if benefits were counted at their cash-equivalent value, they would reduce the poverty rate among participators by 16% in 2011.[2] As a result of the growing importance of this program, there has been increased interest among policy-makers and economists about the *costs* of the program, in terms of direct expenditures and labor supply disincentives (Mulligan, 2012; Ganong and Liebman, 2013), as well as the *benefits* of the program, especially the effects of the program on families' nutrition and children's outcomes (Beatty and Tuttle, 2014; Kreider et al., 2012; Schmidt, Shore-Sheppard and Watson, 2015). Concerns over increased spending have resulted in several small cuts to Food Stamp generosity recently, with potentially larger cuts still on the horizon (Grovum, 2014).[3]

Despite all this, very little is known about the effects of the Food Stamp program, because it is a federal program with little variation in eligibility rules or benefit amounts across geographic locations or over time (Currie, 2003), which would typically be used to conduct quasi-experimental analysis. Existing quasi-experimental estimates of the effects of the program on children's and families' outcomes rely on the program's roll-out in the 1960-1970s (Almond, Hoynes and Schanzenbach, 2011; Hoynes, Schanzenbach and Almond, 2016) and the applicability of those estimates to current generations is unclear, as there have been major changes over time to the Food Stamp program and other safety net programs, as well

[1] In 2008 the Food Stamps program was renamed the Supplemental Nutrition Assistance Program (SNAP), but I use the name Food Stamps throughout this paper.

[2] Sources are Moffitt (2013), the The Center on Budget and Policy Priorities (2013*a*), Murray (2011), and the Food Research and Action Center (2012). The calculation of the effect on the poverty rate is static and ignores behavioral responses.

[3] In 2013, Congress allowed the benefit increase from the American Recovery and Reinvestment Act of 2009 to expire (Dean and Rosenbaum, 2013). In 2014, Congress eliminated the "heat and eat loophole" (Chokshi, 2014), which is a procedure by which states give households with no heating bill (e.g. many renters) Low-Income Home Energy Assistance that allows them to receive slightly larger Food Stamp benefit amounts.

as changes in health care technology, average health, and the demographics of the overall population. For more recent cohorts, researchers compare children's outcomes among families that participate to those that do not (Kreider et al., 2012), which may suffer from biases due to endogenous program participation, or they utilize recent state changes in application procedures and eligibility rules as instruments for participation, but these changes had mostly small effects on participation (Ganong and Liebman, 2013; Ziliak, 2015).[4] Therefore, all in all, the effect of Food Stamps on current children's outcomes is still largely unknown.

In this paper, I take advantage of a recent, large change in Food Stamp eligibility for a well-defined and easily identifiable group, to study the effect of access to Food Stamps on children's health. Specifically, I utilize changes in eligibility among documented immigrant families: many foreign-born lost eligibility for the Food Stamp program in 1996 as part of welfare reform (the Personal Responsibility and Work Opportunity Reconciliation Act) and eligibility was subsequently restored to them at different times across different states from 1998 to 2003. Welfare reform caused immigrants' participation in Food Stamps to decline significantly (Fix and Passel, 1999; Haider et al., 2004) and in this paper I examine the "downstream" effects of this loss of eligibility, as well as the restoration of eligibility, on children's health.[5] These policy changes create a very rich source of variation in eligibility to exploit in my empirical strategy: eligibility depends on state and year of residence, country of birth (U.S. or not) and year of entry to the U.S. for foreign-born. Moreover, as eligibility is turned "off" and then back "on", it is very unlikely that trends in children's health would be driving the results. While on the one hand, immigrants are a select group of individuals who are more disadvantaged than natives (Raphael and Smolensky, 2009), on the other hand, in the U.S., children of immigrants currently make up almost 25% of all children and one third of children in poverty. Additionally, prior to welfare reform, 20% of all children receiving Food Stamps were children of immigrants.[6] Thus, estimating the downstream effects of these eligibility changes is very important both to provide new estimates of the impact of Food Stamps on children's health, as well as to understand the consequences of these large eligibility changes.

In the primary analysis I investigate the effects of early-life Food Stamp eligibility on health at school age (6-16), but before turning to these downstream effects, I first examine

[4]The papers using this latter methodology examine a variety of effects of the program including the effects on children's health, and are summarized by Hoynes and Schanzenbach (2015).

[5]Some researchers suggested that the decline in immigrant participation may have been due in part to "chilling effects" from a harsh policy environment in addition to the changes in eligibility rules (Fix and Passel, 1999; Borjas, 2003; Haider et al., 2004).

[6]Children of immigrants defined as children with at least one foreign-born parent. Sources are KidsCount (2014), Addy and Wight (2012) and author's calculation from the Food Stamp Quality Control Data.

the direct effect of the changes in eligibility on participation in the program. Because I am linking early-life changes in Food Stamp eligibility to health in later life, I restrict the sample to U.S.-born children of immigrants to ensure that, other than changes in Food Stamp eligibility, the early life experiences of these children are as similar as possible. This restriction means that all children in my analysis are U.S. citizens and it is their parents who lose eligibility for the program. Despite the fact that children remain eligible, loss of parental eligibility reduces the benefit amount families are eligible to receive, because this amount is a function of the number of eligible household members.[7] This has two potential implications: families continue to receive benefits, but the benefit amount falls substantially, or families no longer participate in the program, because these lower benefits do not outweigh the costs of participating (Daponte, Sanders and Taylor, 1999; Van Hook and Balistreri, 2006). To focus on children most likely to be affected by these changes, my primary sample is U.S.-born children whose mothers have a high school education or less, because this group participated in the program at higher rates than the full population before welfare reform. With the 1995-2007 Annual Social and Economic Supplement (ASEC) to the Current Population Survey (CPS), I find that the changes in parental eligibility led to large changes in participation–loss of parental eligibility reduced participation by 32% in my sample. There is less evidence that these families continued to participate in the program and received smaller benefits.

Building off of these findings, I utilize restricted access data from the National Health Interview Survey (NHIS) to examine the effect of parental eligibility from the time children are in utero to age 4, on their health at ages 6 to 16. These medium-run effects are of interest for two reasons. First, the early years of life are critical for development: poor nutrition and lack of resources during this time can have lasting detrimental impacts on children's health and cognitive ability (Prado and Dewey, 2012). Second, changes in health may occur slowly in response to changes in resources, so examining contemporaneous measures of health may understate the total effect of Food Stamps on health (Grossman, 2000; Currie, 2009). I find that among U.S.-born children of immigrants, whose mothers have a high school education or less, an additional year of parental eligibility in early life reduces the likelihood children are reported in "Poor", "Fair" or "Good" health (relative to "Excellent" or "Very Good" health) by 1.6 percentage points (6%). Moreover, I find suggestive evidence that an additional year of parental eligibility reduces the likelihood of having any overnight hospitalizations, the number of school days missed, and the likelihood of having two or more doctor visits in the past 12 months. The estimates are robust to the inclusion of children of natives as a "control" group in a triple difference model, as well as accounting for changes in the generosity of other

[7]For example, for a family of 3 with two foreign-born parents and one U.S.-born child, loss of parental eligibility reduces the maximum benefits the family can receive by almost 66% ($200 per month in 1998$s).

safety net programs. Falsification tests on children of natives and a subgroup of children of immigrants, who were exempt from the eligibility restrictions, confirm there were no commensurate changes in these "untreated" children's health.

In addition to providing one of the only quasi-experimental evaluations of the modern Food Stamp program on children's health, this paper also contributes to the literature examining the effects of early-life resource shocks on individuals' long-run outcomes in adulthood (summarized by Almond and Currie (2011) and Currie and Almond (2011)).[8] More recently, this literature has also documented the longer-run effects of childhood access to the safety net, including the mother's pension program in the 1910-30s (Aizer et al., 2016), and the expansions to public health insurance programs in the 1980-90s (Brown, Kowalski and Lurie, 2015; Cohodes et al., 2015; Currie, Decker and Lin, 2008; Miller and Wherry, 2014; Wherry et al., 2015; Wherry and Meyer, 2015).[9] Early-life public health insurance leads to medium-run improvements in health in later childhood, and has long-run benefits in adulthood in terms of both health and human capital outcomes. In this paper, I focus on the largest cash or near-cash program in the modern safety net, and my findings illustrate that, like public health insurance, near-cash programs have large beneficial effects on modern children's medium-run health outcomes.[10] Moreover, understanding the effects in the medium-run is important because if benefits are present at this time, this impacts welfare analysis of early-life interventions, as well as provides insight into the mechanisms behind the long-run effects.

The rest of the paper proceeds as follows. In section (2) I describe the Food Stamp program and the policy variation I utilize and review the related literature. I describe the data I use to estimate the effects on participation and children's health in section (3). In section (4) I outline my empirical strategy. I discuss the results on Food Stamp participation and child health in section (5). Section (6) concludes.

[8]Simon (forthcoming), Ludwig and Miller (2007) and Sanders (2012) are exceptions as they all look at the effect of early-life shocks on outcomes during later childhood and early adulthood.

[9]There is also a literature looking at the contemporaneous effects of safety net programs on children's well-being. See, for example, Currie and Cole (1993), who look at the effects of the AFDC program on infant health, as well as Milligan and Stabile (2011); Dahl and Lochner (2012); Hoynes, Miller and Simon (2015) who look at the effects of refundable tax credits on children's well-being on a number of dimensions. My work adds to this literature by extending the analysis to look at the medium-run effects of safety net programs.

[10]Most quasi-experimental and experimental research finds the marginal propensity to consume food out of Food Stamp benefits is similar to that of cash income (Currie, 2003; Schanzenbach, 2007; Hoynes and Schanzenbach, 2009; Bruich, 2014) and currently most eligible families consume more food than their Food Stamp benefits suggesting they will behave infra-marginally (Hoynes, McGranahan and Schanzenbach, 2015). However, Beatty and Tuttle (2014) found that Food Stamp benefits may distort individuals' behavior and cause them to consume more food than they would have with an equivalent cash transfer.

2 Background

The Food Stamp program, renamed the Supplemental Nutrition Assistance Program (SNAP) in 2008, is a federal program whose benefit amounts are determined as a function of family income and family size. The benefits are available to all families with total family income below 130% of the poverty line (the "gross income test"), regardless of their size or household structure, and are intended to allow families to maintain a minimum level of adequate nutrition, assuming the family spends 30% of its income on food.[11] A family's benefit amount is determined by a maximum benefit amount, which is set nationally and is a function of the number of eligible members in the family, minus 30% of (adjusted) family income :
Benefit Amount =

Max Benefit(Number Eligible in Family) - .30*[Family Income]

Typically all members of the family are eligible, but as I describe in more detail below, the immigrant-specific changes to eligibility led to changes in the number of eligible family members and, therefore, changes in the maximum benefit amount. In 1998 the maximum Food Stamp benefit amount for family of three was $321 per month and the average benefits received were roughly $100 below this maximum. These eligibility rules and benefit amounts are set nationally and have varied little since the program began. I describe the non-immigrant-specific program rules in more detail in the Appendix.

A broad literature in economics consistently finds that economic and nutrition shocks in early life have large effects on later life outcomes (Almond and Currie, 2011). This literature is motivated in part by the Fetal Origins Hypothesis, which argues that lack of resources in early life may lead to poor outcomes in adulthood (Barker, 1990). Since Food Stamps make up a large component of families' resources among participators, the loss of eligibility for the program in early life likely has important consequences for children's health as adults. More broadly there may be "sensitive" and "critical" periods in early life during which individuals' outcomes may be greatly influenced or completely determined, respectively (Cunha and Heckman, 2007; Heckman, 2007; Martorell, 1999). Additionally, the effects of deprivation may appear as early as childhood. The "First 1000 Days" Hypothesis asserts that from the time children are in utero to age 2 much crucial brain development occurs that determines children's cognitive, motor, and social skills as they grow older. Nutrition is especially key to this brain development because nutrients are required for the biological processes that are part of this development. The effects of deprivation in the

[11] Families must also meet a "net income test" in which their adjusted income (gross income minus deductions) is less than 100% of the poverty line and in some cases they are also subject to an asset test. I describe these rules in more detail in the Appendix.

first 1000 days can manifest themselves during childhood through weaker immune systems and poor behavioral and cognitive outcomes (Prado and Dewey, 2012; Save the Children, 2012). Therefore, I expect that children who had more access to Food Stamps early in life will have stronger immune systems, and get sick less, than children that did not has early life access. There is strong correlational evidence that malnutrition is related to cognitive functioning (Grantham-McGregor, 1995) and poor immune system functioning (Chandra, 1997), however short term studies of less severe deficiencies in nutrition "seem unable to detect the real influence of nutrition in early life [because] the brain takes a long time to mature" (University of Granada, 2013). So, by looking at the medium-run effects of nutrition in early-life, this paper provides an important test of this hypothesis. Whether or not Food Stamps improves the nutritional content of families' diets remains an open question that I am unable to address in this paper. However, in 1995 15% of all immigrant families were food insecure (Borjas, 2004) suggesting that maintaining an adequate level of nutrition could have been a concern for these families and the loss of Food Stamps may have had severe consequences for the quantity of food consumed even if it did not affect the quality of food.

In addition to changes in food consumption, several other potential mechanisms could explain why access to Food Stamps affects children's health outcomes. First, by increasing families' resources, Food Stamps may reduce stress within the family (Evans and Garthwaite, 2014), which may cause children's stress to decrease, or result in an improved quality of care the children receive. Additionally, the means-tested nature of the Food Stamp program leads to work disincentives (Hoynes and Schanzenbach, 2012; East, 2015), which may result in parents spending more time at home with their children. This could affect children's health by increasing the time parents spend caring for their children's health or by decreasing children's time spent in daycare, which has detrimental impacts on children's health as they are exposed to illnesses in daycare (Ruhm, 2000; Schaller and Zerpa, 2015).

Much of the existing quasi-experimental evidence on the effects of the Food Stamp program utilizes the county by county roll-out of the program in the 1960s and 1970s. Almond, Hoynes and Schanzenbach (2011) find that access to the Food Stamp program in utero decreases the likelihood children are born of low birth weight, and Hoynes, Schanzenbach and Almond (2016) build off of this work by examining how exposure to Food Stamps from the time a child is in utero to age five impacts their adult health and labor market outcomes. They find more Food Stamp exposure in early-life causes statistically significant reductions in "metabolic syndrome" (obesity, high blood pressure and diabetes) and, for women, improvements in labor market and educational outcomes.

Studies on more recent cohorts take several different approaches to circumvent the fact that the modern program provides little quasi-experimental variation (this literature summarized by Currie (2003) and Hoynes and Schanzenbach (2015)). Kreider et al. (2012) use bounding exercises to account for endogeneity in the decision to participate, as well as under-reporting of participation, and they cannot rule out positive or negative effects on children's health. Closer to the approach I take in this paper, Schmeiser (2012) uses changes in state-specific Food Stamp application procedures and vehicle ownership rules, as well as state maximum EITC benefits, as instruments for Food Stamp participation and finds participation in the program reduces child BMI.[12] My work fills in the gap in this literature by examining the effects of a recent, large change in Food Stamp access on modern cohorts of children. Moreover, contemporaneous analysis may understate the total effects on health, so extending into the medium-run may be important.

Also important to my analysis is whether Food Stamp recipients treat the benefits the same as a cash transfer; if they do, then my estimates can be broadly interpreted as the effect of families' cash income on children's health. The existing evidence finds that, at the start of the program, the marginal propensity to consume food out of Food Stamp benefits was similar to the marginal propensity to consume out of cash income (Hoynes and Schanzenbach, 2009). Similarly, evidence from "cash-out" experiments in the 1990s, in which Food Stamp benefits are converted into cash, indicate no difference in behavior for most recipients (Currie, 2003; Schanzenbach, 2007). However, the evidence for today's benefit recipients is more mixed. Bruich (2014) documents the marginal propensity to consume food out of Food Stamp benefits is similar to that of cash income, and similar to that found by Hoynes and Schanzenbach (2009). But, Beatty and Tuttle (2014) find evidence the marginal propensity to consume food out of Food Stamp benefits may be higher than of that of cash income. Therefore my results may speak to the effect of cash transfers on children, in addition to the effect of Food Stamps on children.

2.1 Policy Changes Affecting Immigrants' Eligibility

I take advantage of a mix of federal and state laws governing immigrants' eligibility for Food Stamps for my analysis. Specifically, I make use of the fact that immigrants lost Food Stamp eligibility as part of welfare reform in 1996, and then eligibility was restored to them haphazardly across states and over time between 1998 and 2003. These changes provide a very rich source of variation across states, years, individuals' country of birth (U.S. or not), and year of arrival to the U.S.. Additionally, because these laws turn eligibility "off" and

[12]However, EITC benefits directly affect children's health (Hoynes, Miller and Simon, 2015).

then back "on", it is very unlikely that differential trends will drive the results. I describe these policy changes in detail next.

As part of welfare reform (the Personal Responsibility and Work Opportunity Reconciliation Act or "PRWORA"), eligibility for many safety net programs was drastically restricted for documented immigrants. All documented immigrants who had moved to the U.S. prior to August 22, 1996, whom I call "pre-PRWORA" immigrants, were effectively prevented from receiving Food Stamp benefits. Similarly, all documented immigrants who moved to the U.S. after August 22, 1996, whom I call "post-PRWORA" immigrants, were effectively prevented from receiving Food Stamps, Medicaid, Supplemental Security Income (SSI), and Temporary Assistance for Needy Families (TANF, formerly Aid to Families with Dependent Child, AFDC) for at least their first five years of residence in the U.S.. To isolate the effect of Food Stamps on children's well-being I restrict the analysis to pre-PRWORA immigrants.[13] Several subgroups of pre-PRWORA immigrants were exempt from the restrictions: those who had worked in the U.S. for 40 quarters, those who had served in the military, or those who were refugees, asylees, or naturalized citizens. Most undocumented immigrants and those on temporary visas (such as students) were never eligible for Food Stamps and were therefore unaffected by these policy changes. Empirically I cannot distinguish between documented and undocumented immigrants, so in what follows I simply use the term "immigrants". Additionally I refer to "treated immigrants" as the pre-PRWORA immigrants who were not in any of these exempt groups, and this is the main group of interest.[14]

The first restorative policies were enacted by states using their own funds. I call the states that restored benefits "Fill-In" states, and I define a state as a Fill-In state if it provided benefits to immigrant children and their parents without requirements beyond the federal eligibility requirements for non-immigrants.[15] This is a slightly more restrictive definition than has been used the previous literature, so I test the robustness of my findings to alter-

[13]Eligibility for SSI was limited for non-disabled elderly pre-PRWORA immigrants. However, since my focus is on families with young children this limitation is likely unimportant for these families. Only 3% of these families had an elderly person residing with them in 1996 according to the ASEC. Additionally, the State Children's Health Insurance Program (SCHIP) did not exist until 1997, but all pre-PRWORA immigrants were eligible once it was created.

[14]Due to data restrictions, in the analysis I do not condition the sample based on citizenship, veteran, refugee or asylee status. However, I conduct falsification tests on pre-PRWORA immigrants who likely had 40 quarters of work.

[15]For example, some states required that immigrants apply for citizenship after receiving Food Stamp benefits, and I do not consider these states to be Fill-in states. I define the presence of a fill-in program based on information from the USDA SNAP Policy Database, the California Department of Social Services, and Bitler and Hoynes (2013). For post-PRWORA immigrants, states could provide fill-in programs for TANF, SSI, Food Stamps, and Medicaid and as a robustness check I control for these other fill-in programs.

native definitions. These Fill-In states were California, Connecticut, Maine, Massachusetts, Minnesota, Nebraska, Rhode Island, Washington and Wisconsin. I call the other 41 states and the District of Columbia the "No-Fill-In" states. The nine "Fill-In" states began their "fill-in" programs between in 1998 and 1999, shown in Figure (1).[16] The final policy change occurred in 2002 with the passage of the Farm Security and Rural Investment Act (Farm Bill), which restored eligibility federally to all treated immigrants in April 2003.[17] I show a timeline of these events and how they affected children's eligibility in Figure (2).

I take advantage of all of these changes in eligibility to estimate the effect of Food Stamps on children's health. But, because I am interested in the effects of eligibility in early childhood on outcomes in later childhood, I focus only on U.S.-born children of immigrants to ensure that the children in my sample had experiences that were as similar as possible early in life, except for differences in parental eligibility for Food Stamps. Focusing on these children means their parents lose eligibility, but they themselves remain eligible.[18] When family members become ineligible, the maximum Food Stamp benefit the family can receive falls significantly; for example, for a family of 3, with one citizen child and two treated immigrant parents, benefits could fall by as much as $2400 annually in 1998 dollars (almost 66%).[19]

A potential concern with utilizing this variation to estimate the effects of Food Stamps is that "Fill-In" states are not randomly selected. I test if state observable characteristics predict the decision to provide a fill-in program in section (5) and find no evidence that they do. Additionally, if these policy changes affect the composition of children in different states, either through selective migration or fertility, this could bias my estimates, so I test for these channels directly in section (5) and find no evidence of these mechanisms. In practice, the Fill-In state that is most important for the identification strategy is California, since the other Fill-In states contain relatively small immigrant populations, therefore I test the robustness of my results to dropping observations from California.

Many researchers have examined the consequences of welfare reform on immigrants'

[16]This is based on availability of the program in January-March of a given year.

[17]This discussion drawn primarily from Zimmermann and Tumlin (1999), Capps (2004), and Bitler and Hoynes (2013).

[18]Any foreign-born siblings of U.S.-born children were made eligible as part of the Agriculture, Research Extension and Education Reform Act in 1998. In the ASEC, among families with U.S.-born children and foreign-born parents, more than 90% of the children in the household were U.S.-born.

[19]Additionally, the income of ineligible immigrants was discounted by the share that they represented in the household. Because of this, when eligibility was restored, if the parents' earnings were substantially large the benefit amount could actually decrease. Anecdotal evidence suggests that this was extremely rare: in one Texas region 5% of mixed citizenship households had benefits decline and 6% had benefits stay the same (Swarns, 1997).

program participation. Initial findings indicated that immigrants' participation in several safety net programs fell dramatically relative to natives' participation (Fix and Passel, 1999; Borjas, 2003; Royer, 2005). These large declines caused some researchers to conclude that chilling effects–fear of participation affecting immigration status or confusion about the eligibility rules–rather than just changes in eligibility, were driving the declines in participation. However, once demographic characteristics and state economic conditions were accounted for, the differential decline among immigrants relative to natives fell to zero for all programs except Food Stamps (Borjas, 1999; Haider et al., 2004). While these findings are related to my work, they cannot speak directly to my analysis because they do not take account of the set of policy changes that turn eligibility back on and they focus on all foreign-born rather than only treated immigrants. Therefore, I conduct my own analysis in section (5) to assess whether, among treated immigrants, changes in Food Stamp eligibility led to changes in participation in programs other than Food Stamps. There may have been other responses to these policy changes that are important to consider when interpreting my findings. Both Hungerman (2005) and Royer (2005) find that private charities responded to the implementation of the immigrant-specific provisions of PRWORA, so my estimates will capture the total effect of both the changes in public benefit eligibility, as well as any private responses.

Other studies have investigated the differences between immigrant and native participation in safety net programs more generally. Taking a direct approach to estimating chilling effects on immigrants' participation, Watson (2014) documents that increases in spending on immigration enforcement led to declines in non-citizens' participation in Medicaid (but no effect for other programs). A related literature examines if the participation of children of immigrants in Medicaid and SCHIP responds differently to changes in generosity than participation of children of natives, but the findings are mixed (Currie, 2000; Buchmueller, Lo Sasso and Wong, 2008). With an approach more similar to mine, Bronchetti (2014) utilizes the differences in Medicaid and SCHIP eligibility rules for post-PRWORA immigrants and natives generated by welfare reform, to document that in the post-welfare-reform era, children of immigrants are more responsive to increases in eligibility than children of natives.

A few papers look at the health consequences of welfare reform for immigrants. Kalil and Ziol-Guest (2009) examine the effect of welfare reform on the health of children of immigrants. They find that nationally, non-citizen immigrant children were more likely to be in parent-reported poor health, and more likely to have postponed health care after welfare reform, as compared to natives and naturalized immigrants. Similarly, Kaushal (2007) utilizes the changes due to welfare reform and the state fill-in programs to identify

the impact of Food Stamp eligibility on contemporaneous adult obesity and finds no effect. Additionally, a number of papers have examined other effects of welfare reform on immigrants including food insecurity (Borjas, 2004), and health insurance and labor supply (Borjas, 2003). My work builds upon this literature by taking advantage of a richer source of policy variation and by looking at the longer-run effects of Food Stamp access in critical periods of children's development.

3 Data

The primary data for my analysis is the National Health Interview Survey from 1998-2013, which I use to measure medium-run health outcomes. The NHIS in a nationally representative cross-sectional survey that collects detailed information on 30-40,000 households per year. There are two components of the NHIS: 1) the "person" file which collects information on the demographics and health of each household member, and 2) the "sample child" file which collects more detailed health information about a randomly selected child within each household. Importantly for my analysis, the year of birth, the country or state of birth, and the year of immigration for foreign-born are available for every individual. Location of birth, state of residence at survey, and detailed year of immigration are restricted variables and therefore these data were accessed through the Center for Disease Control's Research Data Center in Maryland.[20]

The health outcomes I focus on are those that measure children's overall health status. As discussed above, loss of parental Food Stamp eligibility may reduce the quality and quantity of food intake and increase parent's labor supply, both of which may lead to improved overall health and reductions in the frequency of illness. To capture these changes, I utilize parent-reported child health and overnight hospitalizations from the "person" file, as well as the number of school days missed, and the number of doctor visits from the "sample child" file. Parent-reported health is reported as a categorical measure (1=excellent, 2=very good, 3=good, 4=fair, 5=poor) and I follow the literature and create a dichotomous variable indicating if the child is in "Poor", "Fair" or "Good" health, which I take as a measure of bad health, because very few parents report their children to be in "Poor" health (Currie and Stabile, 2003; Milligan and Stabile, 2011). Since a change in the number of doctor visits could be the result of changes in access to health care, or changes in health, I follow Simon (forthcoming) and define a variable indicating if the child had two or more doctor visits in the past 12 months. I consider this to be an indicator of poor health because children

[20]Geographic variables including state of birth and state of survey, along with year of birth and year of survey, were used to merge in information about Food Stamp eligibility and other contextual variables.

are only recommended to have one doctor visit per year at these ages. Importantly, while parent-reported health is a subjective measure, Case, Lubotsky and Paxson (2002) find that it is highly correlated with doctor's reports of children's health status. To further validate this as an informative measure of children's health, I also tabulate the mean of the other measures of health among children reported in "Poor", "Fair" or "Good" health and "Very Good" or "Excellent" health, shown in Appendix Table (A.1). On average children reported in "Poor", "Fair" or "Good" health are 200% more likely to have an overnight hospitalization, miss about one additional day of school, and are 16% more likely to go to the doctor two or more times in a year, relative to children reported in "Very Good" or "Excellent" health.[21]

I focus on U.S.-born children born between 1989 and 2005 and observed at ages 6-16, after early-life changes in eligibility, and before they may selectively move out of the household. I further restrict the sample to children of household heads and children whose mothers have a high school education or less, as these families are more likely to be affected by the changes in Food Stamp eligibility.[22] The main sample is composed of two primary groups of children. The first group is "children of treated immigrants" whose mother and father (if present) were born outside of the U.S. and entered the U.S. between 1985 and 1996.[23] The second group of children is "children of natives" whose mother and father (if present) were born in the U.S.. I drop 1% of children who do not have their biological mother present in the household. I pool children of all ages between 6 and 16 in the main analysis to maximize sample size, but I also explore whether the effects differ across age at survey. I use the NHIS-provided weights throughout.

Summary statistics of the NHIS sample are shown in Table (1) for children of treated

[21]The NHIS measures children's BMI, which is likely influenced by the quality and quantity of food intake, however, the NHIS stated concerns about the coding of the variable prior to 2008 and improved their method of coding beginning in 2008. But, because the sample size of children observed between 2008 and 2013 is small, I do not include this outcome variable in my analysis.

[22]According to the ASEC, prior to PRWORA, 38% of immigrant households where the mother had a high school education or less, participated in the Food Stamp program, whereas 8% of similar households where the mother had more than a high school degree did.

[23]I drop children who have one parent born in the U.S. and the other born outside the U.S.–about 5% of all children. I have alternatively defined children of treated immigrants as children whose parents had 15 years of residence in the U.S. at the time of birth and the results are similar. I cannot condition on parents' citizenship at the time of the child's early-life, which will lead to some measurement error, however in the ASEC among young children of treated immigrants in this time period only 10% had a mother who was naturalized citizen. Additionally, less than 0.5% of these children have a parent who reports being a veteran and less than 4% have mothers from countries which sent more than 100 refugees or asylees in 1998 (Department of Homeland Security, 1998). The ASEC and NHIS do not identify whether the foreign-born are documented or undocumented, but Passel (2005) finds that in 2004 about 30% of all foreign-born were undocumented and of those 25% had children who were born in the U.S..

immigrants and natives separately. Children of treated immigrants are more likely to be Hispanic and to have a mother who has less than a high school degree than children of natives. On the other hand, children of treated immigrants are more likely to have a mother who has been married and to have their biological father living with them. Children of treated immigrants are less healthy according to parent-reported health, but they are also less likely to have an overnight hospitalization, or two or more doctor visits, and they miss fewer days of school on average. There are about 8000 children of treated immigrants in the person file, and about 3000 in the sample child file.

The second data set I use is the Annual Social and Economic (ASEC) Supplement to the Current Population Survey (CPS) from 1995 to 2007 to examine the effects on program participation (Ruggles, 2010). The ASEC is a nationally representative cross-sectional survey of 60-90,000 households every year. The survey collects information about the country of birth of all individuals and the year of immigration to the U.S. for foreign-born, however this information was not consistently collected until 1995, so this is the first year in my sample.[24] In addition to this demographic information, the ASEC collects information about participation in many safety net programs in the past 12 months. I mimic the sample definitions described above for the NHIS, and construct a sample of children who are born in the U.S. between 1989 and 2005 and observed between the ages 0 and 4, in order to capture the changes in eligibility faced during early childhood.[25] The primary outcome variables are Food Stamp program participation and the dollar amount of Food Stamp benefits received, which are coded at the household level. The latter measure captures both extensive and intensive margin changes in Food Stamps. I use the CPS-provided individual weights in my analysis.

Changes in economic conditions and other safety net programs are important to control for, as they may also affect children's outcomes (Currie and Gruber, 1996; Dehejia and Lleras-Muney, 2004; Bitler, Gelbach and Hoynes, 2006; Hoynes, Miller and Simon, 2015). Therefore, I merge on to the NHIS and ASEC information about states' unemployment rates, whether the state "filled-in" other programs for post-PRWORA immigrants, whether the state had an EITC or SCHIP program, the timing of welfare reform or waivers in each state, other state Food Stamp policies, and income eligibility cutoffs for Medicaid and SCHIP

[24] 1994 was the first year the ASEC asked about country of birth and year of immigration for all individuals however the weights provided by the CPS were not fully adjusted to account for immigrants until 1996. See Schmidley and Robinson (1998) for more detail about the comparability of information about the foreign-born between 1994 and 1996.

[25] I have also examined a sample of children ages 0 to 16 born in the U.S. and the results are similar. I am unable to identify pregnant women, so any change in participation that occurs for this group will not be captured in my analysis.

for children by state.[26] Attitudes regarding immigration may affect program participation (Watson, 2014), so I follow Bronchetti (2014) and include two measures of state attitudes: 1) the fraction of individuals reporting they would like immigration decreased from the American National Election Studies (ANES), and 2) the number of deportation court cases per foreign-born individual from Transactional Records Access Clearinghouse (TRAC) Immigration Reports.[27]

4 Empirical Strategy

4.1 Medium-Run Effects of Food Stamps Eligibility

To estimate the medium-run effects on children's health, I construct a measure of the number of years that each child's parents were eligible for Food Stamps from the time the child was in utero (as measured by one year before birth year) to their 5th birthday. I implement a double difference model that uses variation in eligibility for Food Stamps among children of treated immigrants depending on the child's year of birth and state of birth (since all children in my sample are born in the U.S.). I estimate the following equation on the sample of children of treated immigrants born between 1989 and 2005 to mothers with a high school education or less and observed at ages 6-16:

$$Y_{isbt} = \alpha + \beta NumYrsElig(IU->4)_{sb} + \gamma_1 X_{isbt} + \gamma_2 Z_{st} + \gamma_3 W_{sb} + \nu_s + \lambda_b + \epsilon_{isbt} \qquad (1)$$

where Y_{isbt} is the outcome of interest for child i born in state s and in year b and observed in survey year t. $NumYrsElig(IU->4)_{sb}$ indicates the number of years of parental eligibility from 0 to 6, and is a function only of the state of birth of the child and the year of birth of the child.[28] I control for demographic characteristics in X_{isbt}, including gender of the child, fixed effects for the age of the child when surveyed, age of the mother at the child's birth, mother's education, number of siblings of the child, number of years the parents had been in the U.S. before having the child, and race of the child (Hispanic white, non-Hispanic white,

[26]The unemployment rates are from the Bureau of Labor Statistics. The EITC information is from the NBER TAXSIM. Information on other Food Stamp program changes–the frequency with which applications must be re-certified, whether in-person applications or re-certifications are required, state spending on outreach, broad based categorical eligibility, vehicle asset rules, and whether benefits are issued on debit cards–are from the USDA's SNAP Policy Database. This database only contains information beginning in 1996, so I assume the policies were the same prior to 1996 as they were in 1996. The SCHIP program start dates are from Rosenbach et al. (2001) and the Medicaid/SCHIP generosity measures come from Hoynes and Luttmer (2011), which I supplemented with information from the National Governor's Association. Finally, information about welfare reform is from Bitler, Gelbach and Hoynes (2005).

[27]The ANES only includes census region identifiers so I assign the same values to all states within the same region. Additionally, the ANES information is only available in "even" years so I linearly interpolate in the missing years.

[28]I do not take account of whether families were "income-eligible" for the program in this measure.

non-Hispanic black, other Hispanic, and other races). I account for fixed characteristics of the child's state of birth with state of birth fixed effects ν_s, and for national shocks to child health with birth year fixed effects λ_b. I also include controls for state characteristics, including the unemployment rate and Medicaid/SCHIP generosity, at the time of birth, W_{sb}, and the time of survey, Z_{st}.[29] Controlling for these characteristics is important because public health insurance generosity and economic conditions directly affect children's health (Currie and Gruber, 1996; Miller and Wherry, 2014; Dehejia and Lleras-Muney, 2004).

The coefficient β indicates how an additional year of parental Food Stamp eligibility for children in early-life affects their medium-run outcomes. Because all health outcomes are "bad" I expect β to be negative. This estimated effect is the Intent to Treat estimate as it captures the effect of parents' eligibility, rather than take-up, however I discuss below several ways of calculating the Treatment on the Treated Effect–the effect on health among those that participated in the program. Figure (3) displays the number of years children of treated immigrants are eligible depending on their state of birth and year of birth. There is variation in eligibility across birth cohorts: children that were born well before and after welfare reform had full eligibility, whereas children born around welfare reform had more limited eligibility. In addition, there is variation across states as the reduction in the number of years of eligibility around welfare reform is much smaller in the Fill-In states than the No-Fill-In states.

I also implement a triple difference model including the sample of children of natives born between 1989 and 2005 with mothers with a high school education or less, and observed at ages 6-16 as a control group as follows:

$$\begin{aligned} Y_{isbtn} = \alpha + \beta NumYrsElig(IU->4)_{sbn} + \gamma_1 X_{isbtn} + \gamma_2 Z_{st} + \gamma_3 W_{sb} + \\ \gamma_4 Z_{st} * \theta_n + \gamma_5 W_{sb} * \theta_n + \nu_s + \lambda_b + \theta_n + \lambda_b * \theta_n + \nu_s * \theta_n + \epsilon_{isbtn} \end{aligned} \tag{2}$$

where n denotes whether the child is born to treated immigrant parents or to native parents. $NumYrsElig(IU->4)_{isbn}$ is equal to 6 for all children of natives. To account for inherent differences in child health due to place of birth of the parents, I include a dummy variable indicating if the child is born to treated immigrant parents, θ_n. Similarly, I control for state of birth by treated immigrant fixed effects, and year of birth by treated immigrant fixed effects. These account for things that vary at the state by treated immigrant level, and the year by treated immigrant level, such as other federal policies that affect immigrants differently than

[29]The survey state and state of birth are the same for roughly 80% of the sample. The measures of Medicaid/SCHIP generosity are the maximum eligibility threshold for Medicaid/SCHIP expressed as a percentage of the poverty line which varies by children's age, state and year. Additionally, I control for whether there was a SCHIP fill-in program in the year of the survey since this changes markedly across my sample period.

natives. In this triple difference model I include controls for state characteristics, Z_{st} and W_{sb}, interacted with whether the child's parents are in the treated immigrant group, because these characteristics may differentially affect immigrants and natives (Haider et al., 2004; Bronchetti, 2014). I also estimate models in which I exclude the state of birth by year of birth controls, W_{sb}, and instead include state of birth by year of birth fixed effects, which will absorb any shocks to children's health that occur across states and over time and affect children of natives and treated immigrants similarly.

The advantage of the triple difference model is that it allows me to control for state-specific changes in children's health that affect children of treated immigrants and natives similarly. However, the disadvantage is that children of natives may not be an ideal control group for children of treated immigrants, and including children of natives in the estimates may bias my estimates rather than differencing out other changes that are occurring across states and over time. Therefore, in a series of robustness checks, I estimate the double difference model including additional time-varying state characteristics that may affect children's outcomes. I cluster the standard errors at the state of birth level and I estimate linear probability models when the dependent variable is dichotomous.

4.2 Contemporaneous Effects

I also test whether the changes in eligibility affect contemporaneous outcomes, including participation in, and income from, the Food Stamp program. I implement a double difference strategy that is analogous to one described above, except here the variation is by the state of *residence* and year of *observation* among children of treated immigrants as follows:

$$Y_{ist} = \alpha + \beta Elig_{st} + \gamma_1 X_{ist} + \gamma_2 Z_{st} + \nu_s + \lambda_t + \epsilon_{ist} \tag{3}$$

Here Y_{ist} is the outcome of interest for child i living in state s and observed in year t and $Elig_{st}$ is equal to one (or zero) if the parents are eligible (or ineligible) for Food Stamps at the time the child is observed. I include state of residence and year of observation fixed effects (ν_s and λ_t), and I control for individual and family demographic characteristics (X_{ist}) and state characteristics at the time of observation (Z_{st}). Here parental eligibility is a function of only the state of residence and the year of observation for each child, and I show the measure of contemporaneous parental eligibility in Figure (4). Therefore β indicates how contemporaneous parental eligibility affects the outcome of interest.

I can also include children of natives as a control group in a triple difference model as follows:

$$Y_{istn} = \alpha + \beta Elig_{stn} + \gamma_1 X_{istn} + \gamma_2 Z_{st} + \gamma_3 Z_{st} * \theta_n + \nu_s + \theta_n + \lambda_t + \lambda_t * \theta_n + \nu_s * \theta_n + \epsilon_{istn} \quad (4)$$

where n denotes whether the child's parents are treated immigrants or natives. $Elig_{stn}$ is equal to one for all children of natives. I include analogous sets of fixed effects to the ones in the medium-run model: an indicator for whether the parents are in the treated immigrant group, θ_n, as well as state of residence by treated immigrant fixed effects, and year of observation by treated immigrant fixed effects. I include controls for state Medicaid/SCHIP generosity and the state unemployment rate at the time of observation interacted with parent's treated immigrant status. Again, I can omit the time-varying state controls and instead include state by year fixed effects that account for changes to contemporaneous outcomes that affect treated immigrants and natives similarly across states and over time. I cluster standard errors at the state of residence level and I estimate linear probability models when the dependent variable is dichotomous.

5 Results

5.1 Effect of Changing Eligibility on Program Participation

Before examining the downstream effects on children's health, it is important to verify whether the changes in eligibility affected annual participation in, and income from, the Food Stamp program. While I utilize sharp changes in parents' eligibility, this essentially amounts to changes in the maximum benefit the family can receive, which may cause participation to fall as there may be costs to participating in safety net programs either because of stigma (Moffitt, 1983) or transaction costs (Currie et al., 2001). Therefore, this analysis is also informative more generally about the responsiveness of program participation to a large change in benefit generosity. Moreover, it is important to understand how the eligibility changes affected receipt of benefits in order to interpret the downstream effects on children's health.

I estimate the double and triple difference models described above on the samples of interest and the results are in Table (2). In column (1) the dependent variable indicates whether the child's household received any Food Stamps, and in column (2) the dependent variable is the value of Food Stamp benefits received (including $0 in benefits for children in households that did not participate). Since all children in my sample are U.S.-born citizens and remain eligible for the program, families may still participate in the program but receive smaller benefit amounts; therefore, I look at the effects on both of these outcome variables. In the double difference model shown in Panel A, there are large and statistically

significant effects on both outcomes. Among children of treated immigrants in the sample, when their parents are eligible for the program, the likelihood of participation increases by 8.0 percentage points ($p<0.01$); an increase of about 32%. Similarly, when parents are eligible the household receives \$185 more annually in Food Stamp benefits in 2009\$s, a 25% increase ($p<0.05$). These estimates are similar in magnitude to previous findings: Haider et al. (2004) found that welfare reform reduced immigrants' participation in the Food Stamp program by 17% nationally relative to natives' participation.[30]

In Panels B and C, I estimate the triple difference model omitting and including the state by year fixed effects, respectively. I find very similar estimates with these models, however the estimated effect on the benefit amount becomes insignificant when state by year fixed effects are included, due to an increase in the standard error. Because of under-reporting of program receipt in the CPS (Meyer, Mok and Sullivan, 2009), I interpret these estimates as a lower bound of the total effect on participation and dollars of benefits received. I return to this issue of under-reporting below, as it is important to interpreting the downstream effects on child health.

In order to interpret the downstream effects, it is also informative to know if the changes in eligibility affect primarily the extensive margin, participation in the program, or the intensive margin, dollars of benefits received. I conduct a back of the envelope calculation and take the average amount of benefits received by those that participate in the program (about \$3000 in 2009\$s) and multiply this by the change in participation, 8 percentage points, to estimate the expected change in the dollar amount of benefits received due only to changes in participation.[31] However, the validity of this calculation relies upon the marginal participant being the same as the average participant, which may not be the case. The expected change in benefits received from this calculation is \$240, slightly larger than my point estimate.[32] Overall participation may be an important margin through which the downstream effects on health operate, but there may be changes on the intensive margin as well, and I discuss this in more detail below.

[30] My estimates are slightly larger, likely due to the fact that I focus on a sample of young children who participate at higher rates than the overall population, and I take account of the state differences in eligibility.

[31] This calculation is similar to the methodology used by McDonald and Moffitt (1980) and Hastings and Washington (2010) to distinguish between extensive and intensive margin responses.

[32] I also estimate the effect of the eligibility changes on the dollar amount of benefits received among participants shown in Appendix Table (A.2). These results should be interpreted with the caveat that the changes in participation I document may lead to selection into participation that affects these estimates. I find a reduction in the dollar amount received: the opposite of what is expected if families continue to participate but receive smaller benefit amounts. This may be due to selection into who changes their participation in response to changes in benefit generosity.

I next implement an "event study" style test of the effect on Food Stamp participation. Since there are multiple "events" I cannot conduct a typical event study, and instead I plot the difference in each outcome between the Fill-In and No-Fill-In states, among treated immigrants and natives. This allows me to examine changes in the difference in Food Stamp participation for treated immigrants, so that I can check for pre-trends and confirm that the changes in participation occurred when they were expected to. Additionally, by examining changes in this difference for natives, I can verify that there were no similar changes in participation for natives over time. Due to data limitations, the earliest year in my sample is 1995, so I plot this difference for 1995 through 2007, where 1997 is the base year, because this was the last year before there was a difference in eligibility across states. I split $Elig_{stn}$ from equation (4) into two components as follows:

$$FS_{istn} = \alpha + \sum_{t=1996}^{2007} \beta_{1t} ImmigDiff_{stn} + \sum_{t=1996}^{2007} \beta_{2t} NativeDiff_{stn} + \gamma X_{istn} + \gamma_2 Z_{st} + \gamma_3 Z_{st} * \theta_n + \nu_s + \theta_n + \lambda_t + \lambda_t * \theta_n + \nu_s * \theta_n + \epsilon_{istn} \tag{5}$$

where $ImmigDiff_{stn}$ is a set of dummy variables equal to one if the child has treated immigrant parents, lives in a Fill-In state, and is observed in year t. Similarly, $NativeDiff_{stn}$ are dummy variables equal to one if the child's parents are natives living in a Fill-In state and observed in year t. The sample used for this analysis is the same as that in the triple difference model in equation (4). The coefficients β_{1t} and β_{2t} indicate the difference in Food Stamp participation in year t between Fill-In states and No-Fill-In states for children of treated immigrants and children of natives, respectively. I expect to observe an increase and then a decrease in this difference for children of treated immigrants over time, and no change in the difference for children of natives over time.

In Figure (5) I plot the coefficients β_{1t} and β_{2t}. The x-axis indicates the year of observation and the y-axis indicates the estimated effect on Food Stamp participation. The dotted gray line indicates the difference in eligibility between the two state groups for children of treated immigrants. The 95% confidence intervals are shown in the dashed lines, and very few individual point estimates are statistically different from zero, but they generally follow the expected pattern. Before 1998, the difference for children of treated immigrants is small and not statistically different from zero. While there are only two "pre" years due to data limitations, this suggests that there were not differential pre-trends across the two states groups prior to the policy changes. This is important because if the trends in participation before the policy changes across the Fill-In states and No-Fill-In states were different, this might lead to biased estimates. In 1998, the first states create their fill-in programs and

the difference in participation becomes large and positive. The difference remains large and positive through 2003, and it is statistically significantly different from zero in several years between 1998 and 2003. After 2003, the Farm Bill takes effect and the difference declines and becomes statistically indistinguishable from zero. Moreover, for children of natives, the differences in Food Stamp participation are close to zero and remain relatively flat over the sample period, except for a few years where they are negative and statistically different from zero, but small in magnitude. Formal falsification tests also show no commensurate changes in participation for children of natives, shown in the appendix. This visual evidence suggests that participation responded to the policy changes, and that only the children of treated immigrants were affected by these changes.[33]

5.1.1 Effects on Other Program Participation

An important potential secondary effect of these policy changes is that they may cause immigrant families to change participation in other safety net programs, in addition to the Food Stamp program. This could happen for several reasons. First, changes in participation in one safety net program may be linked to changes in participation in other programs if the applications for several programs are linked, or the office in which individuals apply is the same (Baicker et al., 2014). In addition, welfare reform may have had "chilling effects" on safety net participation because of confusion about the eligibility rules, complicated application procedures, and fear of participation affecting immigration status (Capps et al., 2004; Watson, 2014). Understanding these potential secondary effects is important for interpreting the downstream effects on health, therefore I test for evidence of changes in participation in other programs directly in columns (2)-(5) of Appendix Table (A.3). Specifically, I examine whether the changes in Food Stamp eligibility caused changes in participation in SSI, TANF/AFDC, Medicaid/SCHIP, and the Free and Reduced Price School Lunch program.[34] The estimated coefficients are all small and none are statistically different from zero, indicating that "chilling effects" on participation in other programs are unlikely to drive the estimated effects on health.[35] However, as discussed above, other responses to the changes in immigrants' Food Stamp eligibility, including increases in private charities and immigrants' labor supply, will be captured in the reduced form effects on health that I estimate next.

[33] I have also tested whether these changes in Food Stamp participation led to changes in food consumption using the Food Security Supplement to the CPS from 2001-2007. I find suggestive evidence of increases in consumption when families become eligible, but the sample size is too small to estimate this effect precisely.

[34] I define participation in these programs at the household level based on whether anyone in the household received benefits from SSI or TANF/AFDC, or whether any child received benefits from Medicaid/SCHIP, or the Free and Reduced Price School Lunch program.

[35] I discuss in detail in the Appendix the differences between my empirical strategy and those used in the "chilling effect" literature that explain the discrepancy between the findings.

5.2 Effect of Changing Eligibility on Children's Health

For my main analysis, I build off the previous findings and I use the National Health Interview Survey to estimate the effect of early-life Food Stamp access–from the time children are in utero to their fifth birthday–on the health of children at ages 6-16. The estimated effects for all health outcomes from the "person" and "sample child" files are shown in Table (3). Recall that all health outcomes in the NHIS are "bad" so that improvements in health will be indicated by negative coefficients. In Panel A, I show the results from the double difference model and in Panels B and C, I show the results from the triple difference models omitting and including state of birth by year of birth fixed effects, respectively. For both the categorical and dichotomous measures of parent-reported health, the coefficients indicate improvements in health at ages 6-16 for more years of Food Stamp access. These estimates are statistically different from zero and shown in the first two columns. Focusing on the dichotomous measure, which is easier to interpret, an additional year of Food Stamp access reduces the likelihood of the child being reported in "Poor", "Fair", or "Good" health by 1.6 percentage points ($p<0.01$). With 29% of all children reported in "Poor", "Fair", or "Good" health, this is a substantial change on the order of a 6% improvement in parent-reported health. The point estimates and standard errors are very similar across the double and triple difference models, indicating no other changes occurring across states and over time are driving the results.

The effects on the other health outcomes are estimated imprecisely and are not statistically different from zero. But, in both the double and triple difference models, the point estimates are negative, suggesting improvements in health for an additional year of eligibility. Moreover, the magnitudes of these coefficients are large; for an additional year of eligibility there is a 0.02 percentage point (20%) reduction in overnight hospitalizations, a reduction in days missed of 0.08 days (3%) and a reduction in the likelihood of two or more doctor visits by 1.3 percentage points (3%).[36]

To examine visually how child health changes over time, I also implement an "event study" analysis. As with the Food Stamp participation analysis, this is not a traditional event study; instead I plot the difference in health between the Fill-In states and No-Fill-In states, for each birth cohort, for children of immigrants and natives. This allows me to test for pre-trends in child health across the state groups, as well as examine whether there were

[36]I have also estimated these models using children of immigrants who likely have 40 quarters of work in the U.S., proxied by their parents' year of entry, as a control group. This yields broadly consistent results, however the sample size is much smaller for this alternative control group so the results are imprecise.

any commensurate changes in health among children of natives. If there are these type of commensurate changes, this would suggest other changes were occurring across states that affected children's health, besides the Food Stamp eligibility changes. Specifically, I estimate the following equation:

$$Y_{isbtn} = \alpha + \sum_{b=1989}^{2005} \beta_{1b} ImmigDiff_{sbn} + \sum_{b=1989}^{2005} \beta_{2b} NativeDiff_{sbn} + \gamma_1 X_{isbtn} + \gamma_2 Z_{st} + \gamma_3 W_{sb} + \gamma_4 Z_{st} * \theta_n + \gamma_5 W_{sb} * \theta_n + \nu_s + \theta_n + \lambda_b + \lambda_b * \theta_n + \nu_s * \theta_n + \epsilon_{isbtn}$$

Similar to above, $ImmigDiff_{sbn}$ is a set of dummy variables equal to one if the child has treated immigrant parents and was born in year b in a Fill-In state. Similarly, $NativeDiff_{sbn}$ are dummy variables equal to one if the child's parents are natives and the child was born in year b in a Fill-In state. The coefficients β_{1b} and β_{2b} indicate the difference in health, for children born in year b, between Fill-In states and No-Fill-In states for children of treated immigrants and children of natives, respectively. Here I omit the 1992 birth year, because this is the last birth cohort for which there are no differences in eligibility for children of treated immigrants across the state groups.

The sample used for this analysis is the same as that in the triple difference model in equation (2). Children of treated immigrants born from 1989 to 1992 had parents who were eligible for Food Stamps at least until the child reached age 5, so there should not be a difference in these cohorts' health across the state groups. For children born from 1993 to 2003, those with treated immigrant parents are all losing Food Stamp eligibility, but they are losing eligibility for more years in the No-Fill-In states compared to the Fill-In states. Therefore, I expect the difference in poor health between the Fill-In states and No-Fill-In states to become large and negative in this period. Children of treated immigrants born in 2004 and 2005 were born after all the policy changes occurred, so there should be no difference in health for these cohorts. Similarly, for children of natives, there should be no difference across the state groups in their health over time.

I plot these coefficients in Figure (6) where the outcome variables are the categorical and dichotomous measures of parent-reported overall health. I focus on these outcomes because I found the most precise evidence of changes in these outcomes above. On the x-axis is the birth year of the child and on the y-axis is the estimated difference in children's health (β_{1b} and β_{2b}). The 95% confidence intervals are shown in the dashed lines. The dotted line indicates the average difference in the number of years of parental eligibility between Fill-In and No-Fill-In states for children of treated immigrants. The sample size for children

of treated immigrants is roughly 8000 across all years for the outcomes in the person file, so the estimates are noisy, but the coefficients follow the expected pattern. The difference in health across the state groups for children born before 1993 is very close to zero for all cohorts, indicating no differential pre-trends in health across the two state groups. As the difference in eligibility between the two state groups is "phased in", the difference in health among children of treated immigrants becomes negative, indicating health is worse in the No-Fill-In states compared to the Fill-In states. Then as the difference in eligibility is "phased out", the difference in health becomes close to zero again. Because of the small sample sizes, the difference is statistically different from zero in only one year for the dichotomous measure and is never statistically different from zero for the categorical measure. For the children of natives, the difference in health is close to zero and remains relatively constant and statistically insignificant except for the last birth cohort. It is important to note that the sample size becomes smaller in the later birth cohorts because my sample window ends in 2013, so there are fewer years to observe children at ages 6-16 for the later cohorts. This is likely part of the reason the differences become "jumpier" for the later birth cohorts for both groups of children. Overall, I find no evidence of differential pre-trends or commensurate changes in children of natives' health, providing support for my empirical strategy.

I next analyze whether these effects are larger for Hispanic immigrants, who are generally more disadvantaged, and are therefore more likely to be affected by changes in eligibility. Additionally, with this analysis I can utilize children of Hispanic natives as an alternative control group. If Hispanic natives are more similar to Hispanic immigrants than children of all natives, they may therefore serve as a better control group. Beginning with the double difference model, I verify in column (1) of Table (4) that Hispanic families were more affected by the eligibility changes than all families. Eligibility increased the likelihood of participating by 11 percentage points (46%) relative to 8 percentage points (32%) for the full sample. Similarly, I find larger effects on medium-run health of Hispanic children with the double difference model. An additional year of Food Stamp access decreases the likelihood that the child is reported in "Poor", "Fair", or "Good" health by 2.0 percentage points ($p<0.01$) relative to 1.6 for the full sample. There is also a statistically significant reduction of 0.16 days of school missed ($p<0.05$), and a marginally statistically significant reduction of 1.7 percentage points in the likelihood of two or more doctor visits ($p<0.10$). Turning to the triple difference models, I find very consistent results; although the addition of state by year fixed effects increases the standard error estimates by more than in the analysis with the full sample, causing some estimates to become statistically insignificant. This is likely due to the relatively small number of children in the Hispanic control group–6,981 compared to

41,467 in the main sample (in the NHIS person file). Overall, with the Hispanic subsample I find further evidence of improvements in health and no evidence that other changes within states explain the results.

All the estimates described above are Intent to Treat (ITT) estimates because they measure the effect of program eligibility, rather than take-up. Assuming that all the effects on health are the result of changes in participation in the program, I can calculate the effect on health among those that participated, the Treatment on the Treated (TOT) effect, by scaling the ITT estimates by the change in participation. To start, I use the estimate from the analysis on Food Stamp participation above, which indicated that participation changed by 8 percentage points in response to parental eligibility, so I divide the ITT estimates by this amount.[37] This gives a TOT effect of 20 percentage points (61%) on the likelihood of being reported in "Poor", "Fair", or "Good" health.[38] But, the effect on participation may be under-estimated because participation is under-reported in the CPS; only about 60% of families in my sample that participate in Food Stamps report receiving benefits (Meyer, Mok and Sullivan, 2009).[39] If the under-reporting is random, this will lead to measurement error and smaller estimates of the effect on Food Stamp participation than the true effect which will cause me to overestimate the true TOT effect (Stephens and Unayama, 2015). Therefore, I scale the estimated effect on participation by this measure of under-reporting and estimate that parental eligibility lead to a 13 percentage point change in participation. This implies a reduction in the likelihood of "Poor", "Fair", or "Good" health of 36% among children in participating families. These calculations are shown in Appendix Table (A.4).

However, if some families continue to participate and receive smaller benefit amounts, the effects on health may be operating not only through changes in participation. Therefore, as an alternative scaling of my estimates, I calculate the "Treatment on the Treated" effect due to receiving an additional $1000 of Food Stamp benefits. Again I start with the estimated change in dollars of benefits received from the ASEC analysis: $185 (2009$s).

[37] Alternatively I could use the estimated pre-PRWORA participation rate in the ASEC to re-scale my estimates, however this would assume that participation falls to zero when treated immigrant parents become ineligible, which may not be the case.

[38] I use the mean health of children of treated immigrants living in households with income below the poverty line to scale the percentage point effect to the percent effect, as these children are the ones most likely affected by changes in Food Stamp participation. Among this group of children, 33% are reported in either "Poor", "Fair", or "Good" health.

[39] Immigrants are more likely to under-report Food Stamp participation than citizens (Meyer and Goerge, 2011), and I account for this here in calculating that only 60% of participants will report receiving benefits. The other measurement issue that may cause me to under-state the effect on participation is that I cannot identify pregnant women in the CPS, who participate in the Food Stamp program at very high rates (Yelowitz, 2002) and therefore may have experienced large changes in participation.

Dividing the point estimates by this amount implies that each $1000 increase in benefits received reduces the probability of being reported in "Poor", "Fair", or "Good" health by 26% (((1.6/185)/33)*1000). However, for the same reasons as discussed above, this may be an under-estimate of the total change in benefits received. Therefore, I conduct the same calculation to adjust for under-reporting and after accounting for this, my estimates imply that for each $1000 increase in benefits received, the probability of being reported in "Poor", "Fair", or "Good" health is reduced by 15%.

The magnitude of the estimated effects are large, however they are in line with similar studies looking at the effects of safety net programs on health. The most directly comparable estimates look at the effects of early-life exposure to the Food Stamp program on adult health for cohorts born when the program was being rolled out in the 1960-70s (Hoynes, Schanzenbach and Almond, 2016). For these cohorts, an additional year of Food Stamp *participation* in early-life increases the likelihood of being in self-reported overall good health by 8%, slightly smaller than my estimated effect.[40] Since the effects I measure may not all be operating through changes in participation, I can also rescale the estimates from (Hoynes, Schanzenbach and Almond, 2016) to the effect per $1000 of benefits received given that participating families in the 1960-70s received about $2300 in Food Stamp benefits annually.[41] This implies that at the time of the roll-out, for each $1000 in benefits a child receives in early life, the likelihood of being in good health increases by 19%, similar to my estimated effect. Another point of reference is the effects of public health insurance on the health of children. Bronchetti (2014) finds that *participation* in public health insurance reduces the contemporaneous likelihood children of immigrants are reported in "Poor", "Fair", or "Good" health by 89%. Looking at the medium-run effects of public health insurance for all children, Currie, Decker and Lin (2008) document that making all children eligible for public health insurance at age 3 would reduce the likelihood children are in poor health at ages 9-17 by 11%. They obtain similar estimates for eligibility at other early-life ages, although the effects are not always statistically different from zero. Finally, cross-sectional evidence suggests that having family income above 200% of the poverty line in early life decreases the likelihood of being in poor health in adulthood by 62% (Duncan, Kalil and Ziol-Guest, 2015). Therefore, while my estimates are large, they are not out of line with similar findings looking at the relationship between safety net programs, family resources, and health outcomes.

[40]The 95% confidence interval of the TOT estimate in Hoynes, Schanzenbach and Almond (2016) on good health is [0.8%, 15%] and the 95% confidence interval of my TOT estimate on poor health, assuming that all the effects on health operate through changes in participation, is [16%, 65%]. Both of these calculations do not account for imprecision in the estimated effect on the dollar amount of benefits received.

[41]Author's own calculation from the Panel Study of Income Dynamics.

5.3 Specification Checks

In the previous analysis, I documented that an additional year of Food Stamp eligibility in early life leads to improvements in medium-run health. I next examine the sensitivity of these estimates to alternative choices of modeling parental eligibility, to including the Fill-In states, and to including controls for other time-varying state characteristics.

5.3.1 Assumptions about the Timing and Linearity of Effects

First, I examine the sensitivity of the results to the choice of modeling the effects of early-life access. The medium-run effects model above makes two assumptions about the timing of the effects: 1) the only ages that Food Stamp access matters is the time in utero to the fifth birthday, and 2) that the effect of an additional year of eligibility is linear–it is the same if it occurred at any age between the in utero to fifth birthday period. I test the validity of these two assumptions next by looking at the results for the categorical and dichotomous parent-reported health variables. As the results across the double and triple difference models above were similar, I focus just on the estimates with the double difference model. I show the baseline estimate in column (1) of Table (5) and in column (2) I include a measure of the number of years the child was eligible from their fifth birthday to the time of the survey.[42] The coefficient on this eligibility measure for older ages is very close to zero and statistically insignificant for both measures, indicating that the choice of modeling eligibility changes only at younger ages is valid, and that an additional year of access at older ages has a negligible effect on medium-run health. Next, in column (3) I test for heterogeneity in the effects by the age at which the changes in eligibility occurred, by separating the primary measure of eligibility into two terms: one that indicates eligibility from the period in utero to the second birthday and one that indicates eligibility from the second to fifth birthday. As predicted by the First 1000 Days Hypotheses, the coefficient on the term indicating access at very young ages is slightly larger than the coefficient on access at older ages, however these coefficients are not statistically significantly different from one another, so I cannot rule out that the effects are the same at all ages.

5.3.2 Sensitivity to Definition and Inclusion of Fill-In States

As discussed previously, I define Fill-In states as those that provided Food Stamp benefits to all adult immigrants, and had no requirements for eligibility beyond those imposed federally. This is a slightly more restrictive definition than that used by other authors (see for example:

[42] The measure of eligibility from the fifth birthday to the time of the survey is a direct function of child's age, however I control for child age fixed effects in all of my models, so variation in this measure due to age will be absorbed.

Zimmermann and Tumlin (1999)), so I test the robustness of my findings to two broader definitions of Fill-In states. The first broader definition includes any state that provided Food Stamps to pre-PRWORA immigrants, regardless of whether the state had eligibility requirements beyond the federal ones. Two states–Illinois and New Jersey–fall into this category, and classifying them as Fill-In states does not substantively change the results as shown in column (4) of Table (5). The second broader definition addresses the fact that foreign-born children under 18 were subject to less harsh restrictions on eligibility than foreign-born adults. In the main analysis, I assume parents were all eligible under the adult rules, rather than the child rules, so I next test the robustness of my results to assuming teen parents were subject to the rules for children under 18. I do not expect this to greatly affect my results because the children in my sample are children of household heads, so teen parents were not dependents and therefore likely not considered "children under 18" when their eligibility was determined. In column (5) of Table (5) I indeed find this change in modeling eligibility has a negligible effect on the results.[43]

While the results are not sensitive to the definition of Fill-In states, a potential concern with my empirical strategy is that the states' decision to fill in is endogenous and that this is driving my results. One piece of evidence against this concern is that there were no differential pre-trends between the two groups of states in the "event study" analysis, however, in this section I consider other direct tests of this potential endogeneity. As mentioned, California is the largest Fill-In state and contains almost 90% of treated immigrant families in Fill-In states, so it is possible that the differences in child health between the Fill-In and No-Fill-In states over this period is being driven by something unique to California. Therefore I check the robustness of the estimates to dropping California from the sample in column (6) of Table (5). The general pattern of results remains the same, but the effect on "Poor", "Fair" or "Good" health is no longer statistically different from zero. As an additional test of the importance of California to the results, I return to the "event study" analysis as above. I restrict the sample to only children of treated immigrants and I separate the Fill-In states into two groups: California and Non-California-Fill-In States. In Appendix Figure (A.1) I plot the differences in parent-reported health between California and the No-Fill-In states, and between the Non-California-Fill-In states and the No-Fill-In states. Both differences follow a similar pattern to those in the main event study graphs, suggesting that the main results are not due to something unique about California.

[43]Several states restored benefits to foreign-born children under age 18 and then federally eligibility was restored in 1998 as part of the Agriculture, Research Extension and Education Reform Act to these children who were also living in the U.S. at the passage of PRWORA. In this alternative measure I take account of both of these policy differences between teens and adults. One case in which teen parents could be considered dependents is if they live with their own parents, but these households are less than 1% of my sample.

I next directly investigate whether states' observable characteristics predict the decision to fill in. Previous work examining this with any fill-in program (for any safety net program and for either pre or post-PRWORA immigrants) suggests that the size of the immigrant population was unrelated to the decision (Zimmermann and Tumlin, 1999), but that the states' safety net generosity and income were correlated with fill-in programs. However, the estimates in Zimmermann and Tumlin (1999) are not directly applicable to my analysis as they focus on a broader set of fill-in programs than I do. If there are state characteristics that are fixed over time and affect the state's decision, these will be captured by the state fixed effects in my regressions, however it is still informative to understand what observable characteristics, if any, predict these fill-in programs. I directly investigate whether state demographic characteristics and in particular the foreign-born population are correlated with the presence a Food Stamp fill-in program in Appendix Table (A.5) and I find little evidence that they are.[44] It is also possible that more politically liberal states, which tend to have generous safety net programs, or states that feel favorably about immigration, chose to create fill-in programs. I test for this directly and again I find no relationship between these characteristics and the fill in decision in Appendix Tables (A.6) and (A.7).[45]

Of greater concern is whether time-varying state characteristics are correlated with state Fill-In programs, so I examine if treated immigrants' eligibility is correlated with the state unemployment rate, the spending per pupil on education, the spending per person on AFDC/TANF, Medicaid, or SSI, the AFDC/TANF max benefit, whether the state had an EITC program, and generosity of the Medicaid and SCHIP programs.[46] As shown in Appendix Table (A.8), there is a marginally statistically significant relationship between Fill-In programs and the unemployment rate as well as Medicaid/SCHIP generosity but these relationships are economically small and none of the other estimates are statistically different from zero.[47]

This analysis indicates that the decision to fill in was not correlated with state demo-

[44]I use the 1990 Census to compile pre-treatment state characteristics. The dependent variable is a dummy variable equal to one if the state enacted a Food Stamp fill-in program.

[45]Governors' political parties comes from Wikipedia and Dave Leip's Atlas of U.S. Presidential Elections. Having an independent governor appears to be correlated with the likelihood of being a Fill-In state however only Maine had an independent governor in 1996 and was also a Fill-In state. In results not shown, I also test whether the *interactions* of state demographic and political characteristics as well as attitudes towards immigrants predict the presence of a fill-in program and find no evidence that they do.

[46]The educational expenditure data come from Kids Count. The data on safety net expenditure are from the BEA Regional Economic Accounts, and the AFDC/TANF benefit data are from Robert Moffitt's website.

[47]I control for the state unemployment rate and generosity of state Medicaid and SCHIP programs in all my regression models, and I investigate the sensitivity of my double difference estimates to the inclusion of additional state characteristics below.

graphic or political characteristics, and changes in states' observable characteristics over time were not correlated with immigrants' eligibility. However, there may be unobservable state characteristics that determine whether a state became a Fill-In state that are also correlated with child health. Therefore, I check whether omitting all Fill-In states (not just California) changes the estimated effects in the triple difference model. This takes advantage of different policy variation than in my primary analysis because I am only using variation over time across treated immigrants relative to natives to identify the effects. I drop the Fill-In states and re-estimate the triple difference model in column (7) of Table (5). The magnitude of the effect becomes smaller with the omission of all Fill-In states and the estimates are no longer statistically different from zero, suggesting that perhaps part of the estimated main effect relies on differential changes across the Fill-In and No-Fill-In states over time.[48]

Another potential threat to identification is that there may be selection into the sample or into living in a Fill-In state if treated immigrants respond to the policy changes by migrating or changing their fertility. This could bias my estimates; if, for example, immigrants who are the most invested in their children move to Fill-In states, the observed children's health in the Fill-In states would be better than in the Non-Fill-In states, but this difference would not be due to the direct effect of access to Food Stamps. Since I use state of birth in the NHIS data to assign Food Stamp eligibility, selective migration that occurred after birth is not a concern. Nevertheless, I test for both migration and fertility responses in Appendix Tables (A.9) and (A.10) and find that neither are impacted by these policy changes.[49]

5.3.3 Accounting for Time-Varying State Characteristics

If children of natives are not a good control group, then the triple difference model will not absorb shocks to children's health over time, so as an alternative test to confirm there were no other changes over time within states that drive my results, I include additional state time-varying characteristics at the time of birth in the double difference model. In column (1) of Table (6) I present the baseline effects, and in columns (2) to (5) I add in controls for other state characteristics at the time of birth that might influence children's health, including other safety net program generosity (AFDC/TANF generosity, welfare reform and waivers, state EITC generosity), whether the state chose to "fill-in" other safety-net programs for post-PRWORA immigrants, state attitudes towards immigrants, and other changes the

[48] I show similar checks for Food Stamp participation in Appendix Table (A.11).

[49] In addition to internal migration within the U.S. it is possible that there was differential *re-migration* in response to these eligibility changes. Specifically, immigrants who lost eligibility might have been more likely to leave the U.S., and return to their country of birth than those who did not. I have tested for this using the American Community Survey and find no evidence of differential re-migration patterns related to eligibility changes.

state made to the Food Stamp program. For most specifications the point estimates are very similar to the baseline estimate. With the addition of other safety net controls, the estimate on the dichotomous measure is no longer statistically different from zero, and with the addition of the other Food Stamp options, neither the categorical or dichotomous measure is statistically different from zero. Finally, in column (6) I include controls for these state characteristics in the *survey* year and *survey* state and the results are very similar to the baseline.

In the previous section, I did not find any relationship between state's observable characteristics and the decision to fill-in, however I did find the results shrink slightly when all Fill-In states were dropped from the sample. Because Zimmermann and Tumlin (1999) suggested that states' safety net generosity and income were correlated with the presence of a Fill-In program, it is possible that states with generous safety nets or high average incomes were experiencing differential trends in children's health, and this is driving my estimated effects. However, this is unlikely to be the case because I do not find differential pre-trends in children's health across Fill-In and No-Fill-In states in the event study analysis above. Nevertheless, I also explore whether including the states' welfare and public health insurance generosity, as well as the unemployment rate in 1990, interacted with state linear year of birth trends changes the estimated effects. As shown in column (7), the estimated effects remain similar. In column (8) I add in state of birth linear birth year trends to flexibly account for the fact that some states may have had different trends in children's health over this time period. Both the categorical and dichotomous measures shrink slightly and become statistically indistinguishable from zero, however this is a very demanding specification.[50]

5.3.4 Falsification Tests

Finally, I conduct falsification tests on groups that should have been unaffected by the Food Stamp eligibility changes. I assign to these groups a measure of the number of years of eligibility as if these "untreated" children were subject to the changes in eligibility faced by the children of treated immigrants. These "untreated" groups are: 1) children of natives whose mothers have a high school education or less, and 2) children of immigrants whose parents came to the U.S. prior to 1985 (and therefore likely met the 40 quarter eligibility requirement) and whose mother had a high school education or less, and 3) children of treated immigrants whose mother had a college education or more. If there were differential trends in child health across states that are driving my results, I would find similar effects on

[50]I show the results of these same checks in Appendix Table (A.11) for the effect on Food Stamp participation.

health for these untreated groups of children. In Table (7) I find the estimated coefficients to be all very close to zero and statistically insignificant.[51]

5.4 Subgroup Analysis

As mentioned, the changes in Food Stamp eligibility did not uniformly affect all demographic groups, so I next test whether the demographic groups that experienced the largest effects on participation also experienced the largest effects on medium-run health. I divide the ASEC and NHIS samples into subgroups and estimate the effect on Food Stamp participation and medium-run health for each subgroup. The subgroups are constructed based on mother's education (less than high school, high school, some college, and college or more), mother's ethnicity (Hispanic or not), mother's age at child's birth (teens, 20s, 30+s), and mother's marital status (never married or ever married).[52] Figure (7) shows the relationship between the effect on participation and the effect on "Poor", "Fair", or "Good" parent-reported health for different demographic subgroups. The x-axis of this scatterplot indicates the effect of Food Stamp participation, β from equation (3) and the y-axis indicates the effect on health, β from equation (1). As expected, the effects are largest for the more disadvantaged groups, and close to zero for the more advantaged groups. Other than for children whose mothers have "some college" education, the results indicate a strong relationship between the effect on participation and the effect on health.

To look for further evidence of this type of pattern, I also explore how the loss of parents' eligibility *in utero* affects children's health at birth and relate this to the effects on heath in the medium run, as later life health may be determined in part by initial health "stock" at birth (Currie, 2009). While other researchers have documented that Food Stamps improves health at birth at the time of the roll out of the program in the 1960-70s, I perform my own analysis of this effect, because I am focusing on a different source of variation in the program as well as a different population and time period. I begin by evaluating the effects on health at birth for all children of treated immigrants, similar to the main NHIS analysis. Then I examine heterogeneity in this effect across demographic subgroups. To implement this, I examine how parental eligibility in the third trimester affects birth weight (in grams) and the likelihood of being born of low birth weight (< 2500 grams), which are common measures of health at birth (Currie, 2011).[53] For this analysis I use a restricted-

[51] Falsification tests for Food Stamp participation are in Appendix Table (A.12).

[52] One caveat with this analysis is that these samples may be overlapping. For example, a child could be in both the Hispanic group and the group where the mother has less than high school education.

[53] The 3rd trimester is the most important for nutrient intake (Rush, Stein and Susser, 1979). And low birth weight is a signal of poor health as it is associated with a higher risk of infant mortality (Mathews and MacDorman, 2011) and increased hospital costs (Almond, Chay and Lee, 2005).

access geocoded version of the Vital Statistics Natality Data. These results also provide an important check on my main analysis with the NHIS, because I am able to estimate the effects of the eligibility changes on the universe of births to foreign-born women (about 8 million births in my sample period), and I can examine the effects of Food Stamps on an objective measure of children's health. I focus only on births between 2000 and 2007 due to data constraints, but otherwise the empirical strategy is the same as in my main analysis.[54] I describe the sample construction in more detail in the Appendix.

First I analyze the effects on infant health for all infants born to immigrant mothers and the results indicate large and statistically significant improvements in health at birth when parents are eligible, shown in Table (8). The likelihood of low birth weight is reduced by 0.01 percentage points ($p<0.05$) and the average birth weight increases by roughly 6.5 grams ($p<0.05$). These effects are relatively consistent across the double and triple difference models, however the estimated effects become insignificant when state by time of birth fixed effects are included due to an increase in the standard error. Given the average likelihood of low birth weight of 7%, and the average birth weight of 3295 grams, Food Stamps reduces the likelihood of low birth weight by 1.4% and increases the average birth weight by 0.2%.[55] To compare my estimates to the previous findings of the effect of Food Stamps on infant health I calculate the TOT effect as above.[56] For an additional $1000 in Food Stamp benefits received in the year before a child's birth, the likelihood of a child being born of low birth weight decreases by 3% and increases average birth weight by 0.5%. These estimates are very similar to those in Almond, Hoynes and Schanzenbach (2011), whose estimates imply for the same $1000 the likelihood of a child being born of low birth weight decreases by 2-3% and increases average birth weight by 0.2%.[57]

Next, I investigate whether the demographic groups that experienced the largest changes

[54]Information about mothers' education is not available consistently in my sample period within the Vital Statistics data so I do not condition the sample on mothers' education.

[55]The larger effect on low birth weight suggests Food Stamps may be more important for infants who are more disadvantaged and would have been born of lower birth weight. In Appendix Figure (A.2) I plot the effect of parental Food Stamp access on the likelihood children are born below various birth weight thresholds from 1500 to 5000 grams (expressed as the percent effect) and the results indicate the largest effects are on the lower end of the birth weight distribution. I also conduct an "event study" style analysis similar to before shown in Appendix Figure (A.3).

[56]For these calculations I mimic the sample construction in the Vital Statistics data within the CPS and restrict the years of analysis in the CPS to 2000-2007 to estimate the effects on the dollars in Food Stamp benefits received among women with children under age 2. (The CPS does not collect information on women's pregnancy status.)

[57]Similarly Hoynes, Page and Stevens (2011) find that WIC participation increases average birth weight by 0.5-1%. Rossin-Slater (2013) finds WIC participation decreases the likelihood of low birth weight by 15%, but this effect is not precisely estimated. Finally, Figlio, Hamersma and Roth (2009) estimate the likelihood of low birth weight is decreased by more than 100% among women who participate in WIC.

in health at birth are also those who experienced the largest effects later in childhood. I divide the Vital Statistics and NHIS samples into the same subgroups as the exercise above, except I omit the educational attainment groups because of data constraints. Figure (8) shows the relationship between the effect on birth weight (x-axis) and the effect on medium-run "Poor", "Fair", or "Good" parent-reported health (y-axis). Overall there appears to be a strong positive relationship between the effects on health at birth and health in the medium-run. While this is suggestive that the effects in these two time periods are related, I cannot conclude that the effects in the medium-run are *caused* by effects at birth. Moreover, it is important to note that the results above indicate that access at later ages of early childhood, beyond just the in utero period, also affect medium-run health, so effects at birth are not the only mechanism behind the medium-run effects.

As previous research found male children to be more sensitive to shocks to family resources than female children, especially in terms of health outcomes, I examine if the same is true for access to Food Stamps (Currie and Almond, 2011; Milligan and Stabile, 2011).[58] I show the effects for boys and girls separately in Panels B and C of Appendix Table (A.13) and indeed all the estimated effects on medium-run health are larger for boys than girls, however not all these differences are statistically significant. Similarly, I estimate the effect separately by the age of the child at observation in Panels D and E to check for differential effects across ages. Because of the small sample sizes, I break the sample into only two groups: ages 6-10 and ages 11-16. Some outcomes appear to be more influenced at younger ages whereas others are influenced at older ages, however, only one of these differences is statistically significant.

5.5 Economic Significance of Effects

To summarize, I find robust evidence that an additional year of parental eligibility improves health in the medium-run and in particular reduces the likelihood of the child being reported in "Poor", "Fair", or "Good" health by 6%. I next convert these estimates into dollar amounts to better understand the economic significance of the effects. While I also find strongly suggestive evidence of declines in the number of hospitalizations, school days missed, and doctor visits, the estimates of the effect on these latter outcomes are not statistically significantly different from zero, so I do not focus on them here.

With the Medical Expenditure Panel Survey, I tabulate that the average health care costs of a child who is in "Poor", "Fair", or "Good" health is $2450 compared to $1462 for

[58] The reasons for this difference by gender are unknown; it is possible that boys are more sensitive to health insults or that parents treat boys and girls differently in response to resource shocks (Case and Paxson, 2005; Nilsson, forthcoming).

children in "Excellent" or "Very Good" health. I assume these health benefits are constant from ages 6 to 16 and I calculate the average present discounted value of these benefits at the time the Food Stamp benefits are being distributed to these families.[59]

Through the outcome of "Poor" "Fair" or "Good" health, an additional year of parental eligibility for Food Stamps in early life leads to about $132 in benefits, due to reductions in health expenditures in the medium-run. The benefits captured through parent-reported health may accrue to different sources: first, a reduction in medical costs directly benefits these children's families, and, as these children participate in Medicaid and SCHIP, the reduction in medical expenditures may also represent government savings. However, there may be other benefits, due to increased learning and higher wages in adulthood through the effects on school days missed or improved health, which are not captured by this estimate.[60]

I next compare the estimate of the total benefits per child per year to the direct costs of making parents eligible for Food Stamps for a year. In 2009, the administrative costs of operating the Food Stamp program were $45 per participating household and I estimate the cost per family of making parents eligible is on average $308 per year (recall this adjusts for under-reporting).[61] This suggests that through just the direct effects on medium-run parent-reported "Poor" "Fair" or "Good" health, 37% of the direct costs are recouped. However, as there may be more benefits (for example, increases in lifetime earnings) as well as additional costs (for example, labor supply disincentives), I am cautious about concluding anything about the total value of the program solely from the numbers I have estimated here.

6 Conclusion

In this paper I estimate the effect of Food Stamps, currently one of the largest safety net programs in the U.S., on children's health outcomes. The Food Stamp program has grown significantly over the past 15 years, but not much is known about its effects because it is a federal program with little quasi-experimental variation in policy parameters to exploit. I take advantage of the loss, and subsequent restoration, in eligibility among immigrant

[59]For each age (-1 to 4) I calculate the present discounted value of these future benefits at ages 6-16. For example, I calculate the present discounted value of an additional year of access to Food Stamps at age 2 on changes in parent-reported health at ages 6-16 and then I sum the effects at ages 6-16. Then I take the average of these estimates for each age of the changes in eligibility (-1 to 4) to obtain an estimate of the present discounted value of the benefits of one year of early-life access on health outcomes at ages 6-16.

[60]For example, Lavy (2012) and Aucejo and Romano (2014) link school attendance to human capital accumulation and I do not account for the effects on school days missed here.

[61]Administrative costs are from the USDA's State Activity Report. Alternatively, I could use administrative data on Food Stamp expenditures by state to calculate the costs. However, this information is not available separately for immigrant and native households.

families to examine how access to the Food Stamp program affects children's health. I find that the loss of parental Food Stamp eligibility has a large effect on household Food Stamp participation. I build off of this finding to examine how Food Stamp access in early-life affects children's medium-run health at ages 6-16 and my estimates indicate large, although not always precisely estimated, improvements in medium-run health outcomes for an additional year of access.

The results are robust to including children of natives as a control group in triple difference models, which allows me to include state of birth by year of birth fixed effects. I implement several other checks to ensure that other things are not changing differentially across states and over time that are biasing my results. First, I take into account various changes to the safety net that occurred around this time by directly controlling for other policy changes in the double difference models. Second, I conduct falsifications tests and find no evidence that the changes in immigrants' eligibility are correlated with changes in the health of children of natives or children of immigrants whose parents were likely unaffected by the policy changes. This suggests that there were not differential changes across states and years, other than the Food Stamp eligibility changes, that are driving my results.

My research also adds to a growing body of work that isolates the *causal* impacts of family resources and safety net programs on children's well-being. My results indicate that near-cash programs have significant health benefits, similar to recent findings on public health insurance programs. Moreover, these benefits appear as early as school-age, and this timing has important implications for welfare analysis of policy changes and resource shocks that take place in early life.

The efficacy of the Food Stamp program is still a contentious issue and in recent years there have been several small cuts to the program. The evidence I present in this paper informs the ongoing debate by providing some of the first estimates of what the effects of a large cut in program generosity would be today. In particular, I find that the elimination of one year of parental eligibility for Food Stamps in early life led to a $130 increase in health expenditures per child, due solely to the effects on health at school age. However, because there are other benefits I am unable to measure directly, this is likely smaller than the total benefits of the program.

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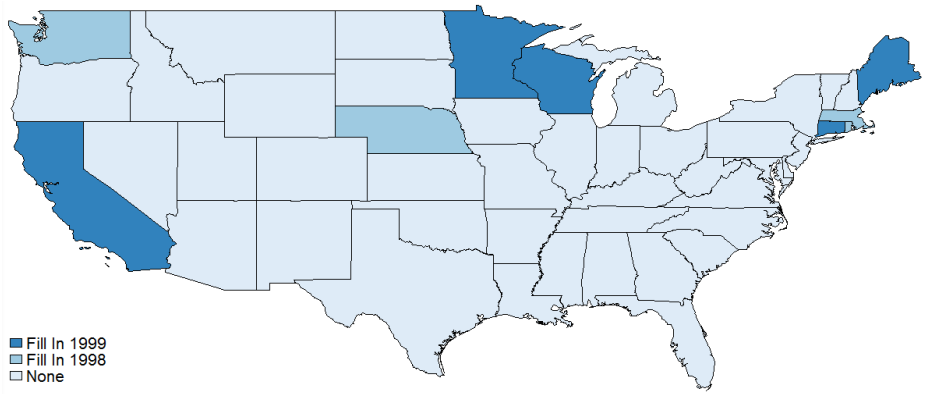
Figure 1: States that Chose to Fill In Food Stamps for Immigrants



Notes: States are classified based on their availability of a Food Stamp fill-in program in January, February or March of a given year. Only fill-in programs that provided benefits to children and their parents are included here and Fill-in programs for the elderly are not included. In addition, states that provided fill-in programs but had additional eligibility requirements beyond the federal ones are not counted as Fill-In states.

Figure 2: US-born Children's Eligibility for Food Stamps

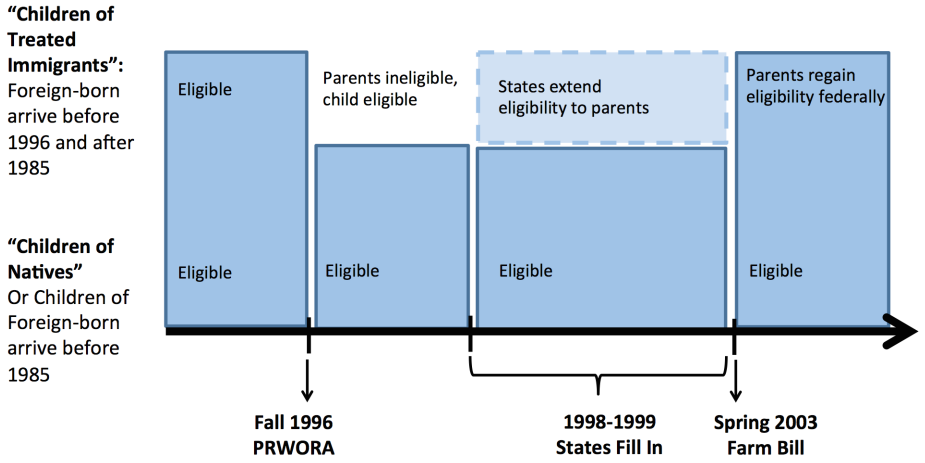
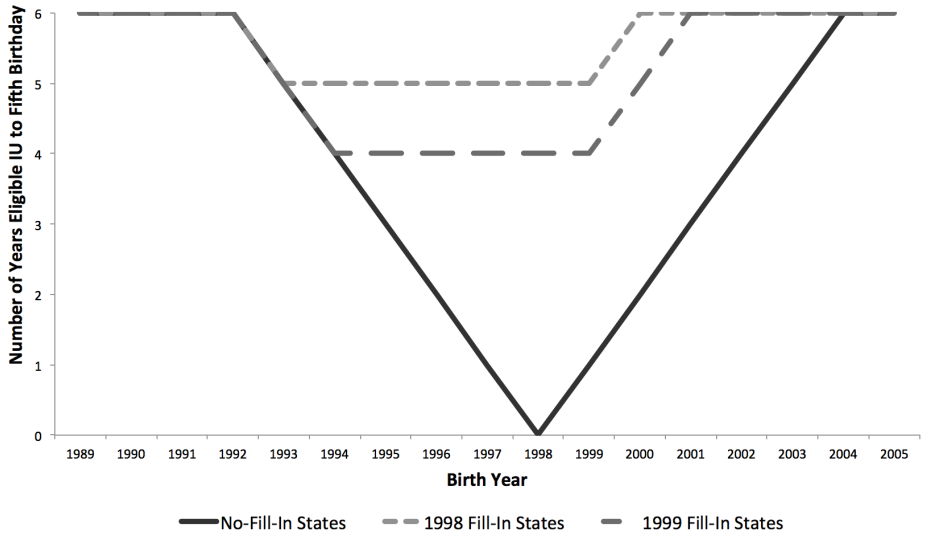
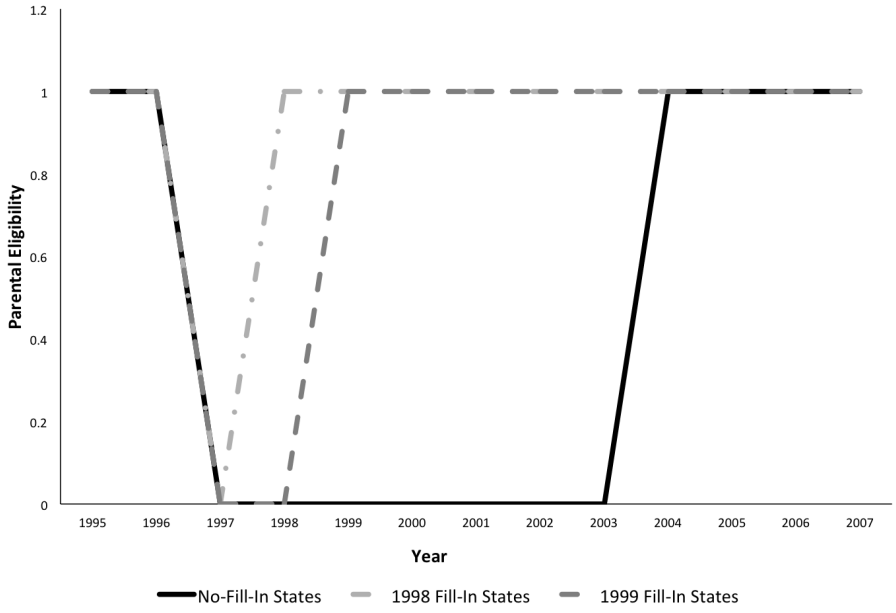


Figure 3: Eligibility for Food Stamps Among Treated Immigrant Parents by Birth Year



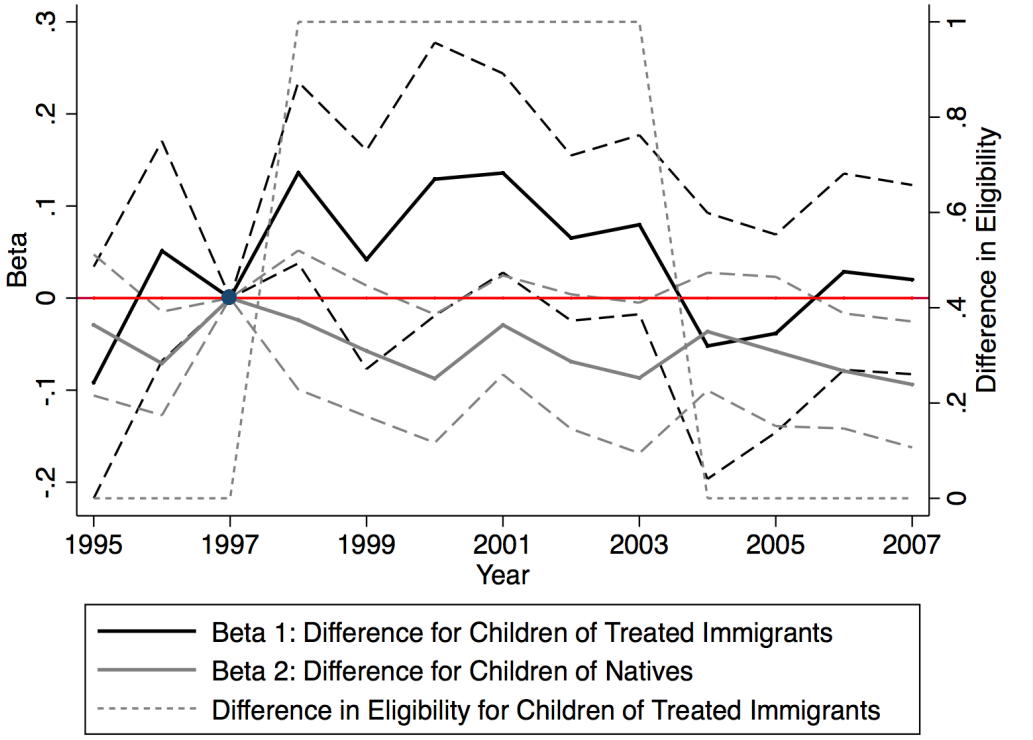
Notes: States are classified based on their availability of a Food Stamp fill-in program in January, February or March of a given year. 1998 Fill-In States are Massachusetts, Nebraska, Rhode Island, and Washington. 1999 Fill-In States are California, Connecticut, Maine, Minnesota, and Wisconsin. The No-Fill-In States are the remaining 41 states and the District of Columbia.

Figure 4: Contemporaneous Eligibility for Food Stamps Among Treated Immigrant Parents by Year



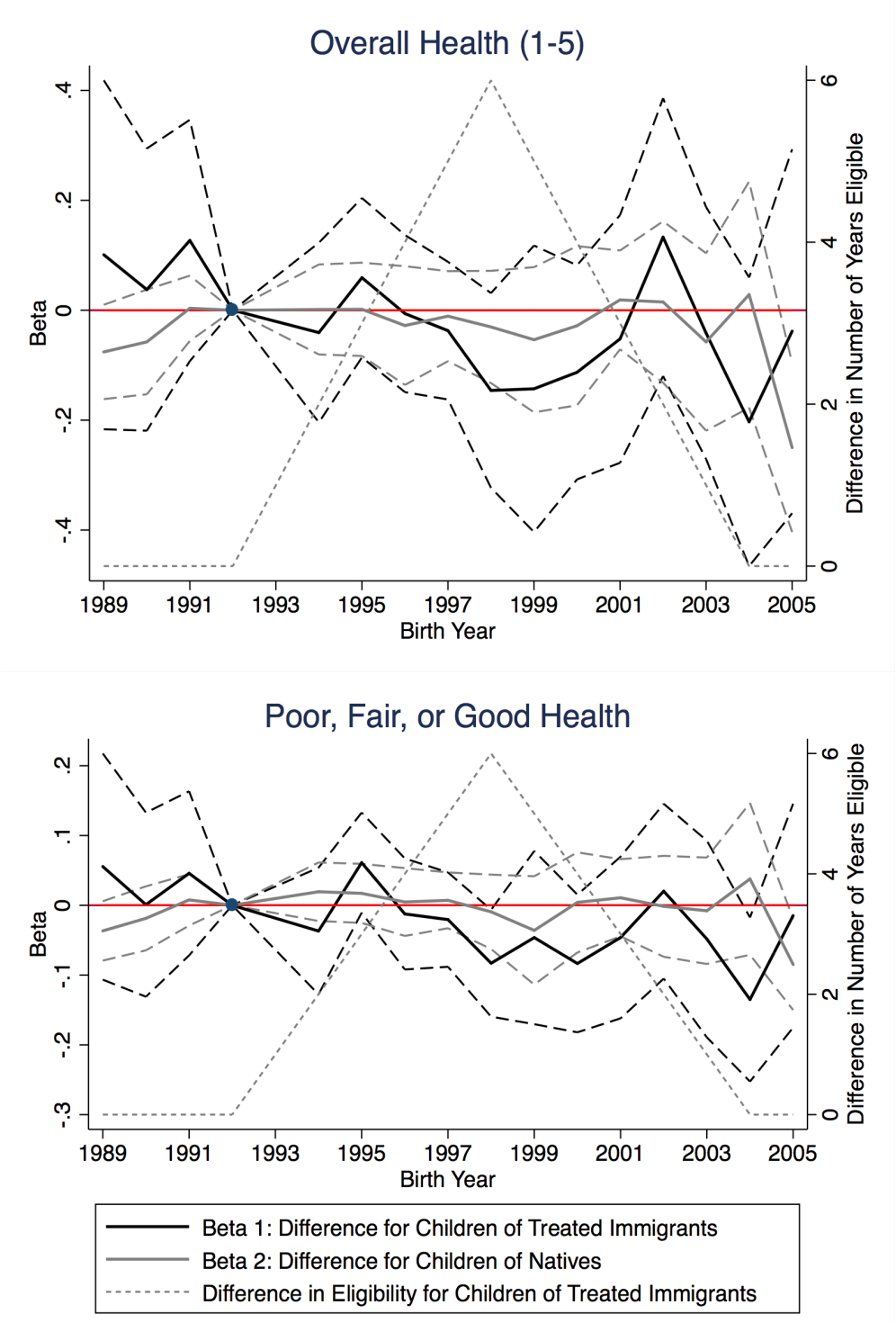
Notes: States are classified based on their availability of a Food Stamp fill-in program in January, February or March of a given year. 1998 Fill-In States are Massachusetts, Nebraska, Rhode Island, and Washington. 1999 Fill-In States are California, Connecticut, Maine, Minnesota, and Wisconsin. The No-Fill-In States are the remaining 41 states and the District of Columbia.

Figure 5: Difference in Participation in Food Stamps



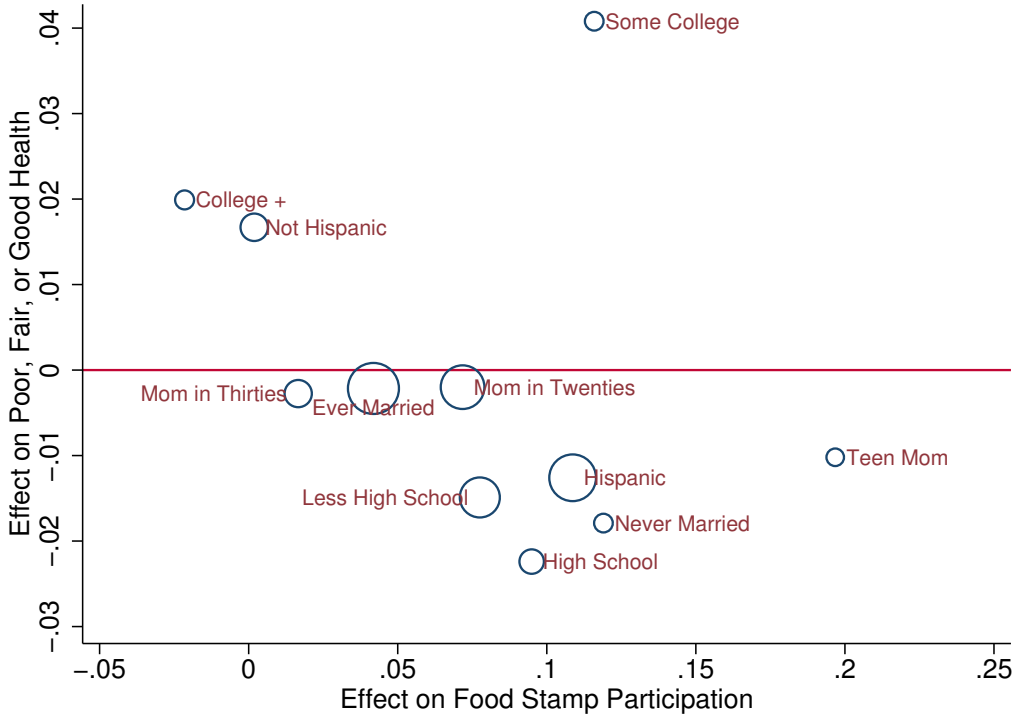
Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. The sample includes children born in the U.S. in 1989-2005 and between the ages of 0 and 4, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. The regression includes state and year fixed effects, and controls for demographic characteristics, the state unemployment rate and Medicaid/SCHIP generosity at the time of the survey, as well as these state by year characteristics interacted with an indicator for whether the parents are treated immigrants. The results are weighted using the CPS-provided weights. The dotted line indicates the difference in eligibility between Fill-In and No-Fill-In states for Children of Treated Immigrants. 95% Confidence Intervals shown in the dashed lines.

Figure 6: Difference in Children's Parent-Reported Overall Health



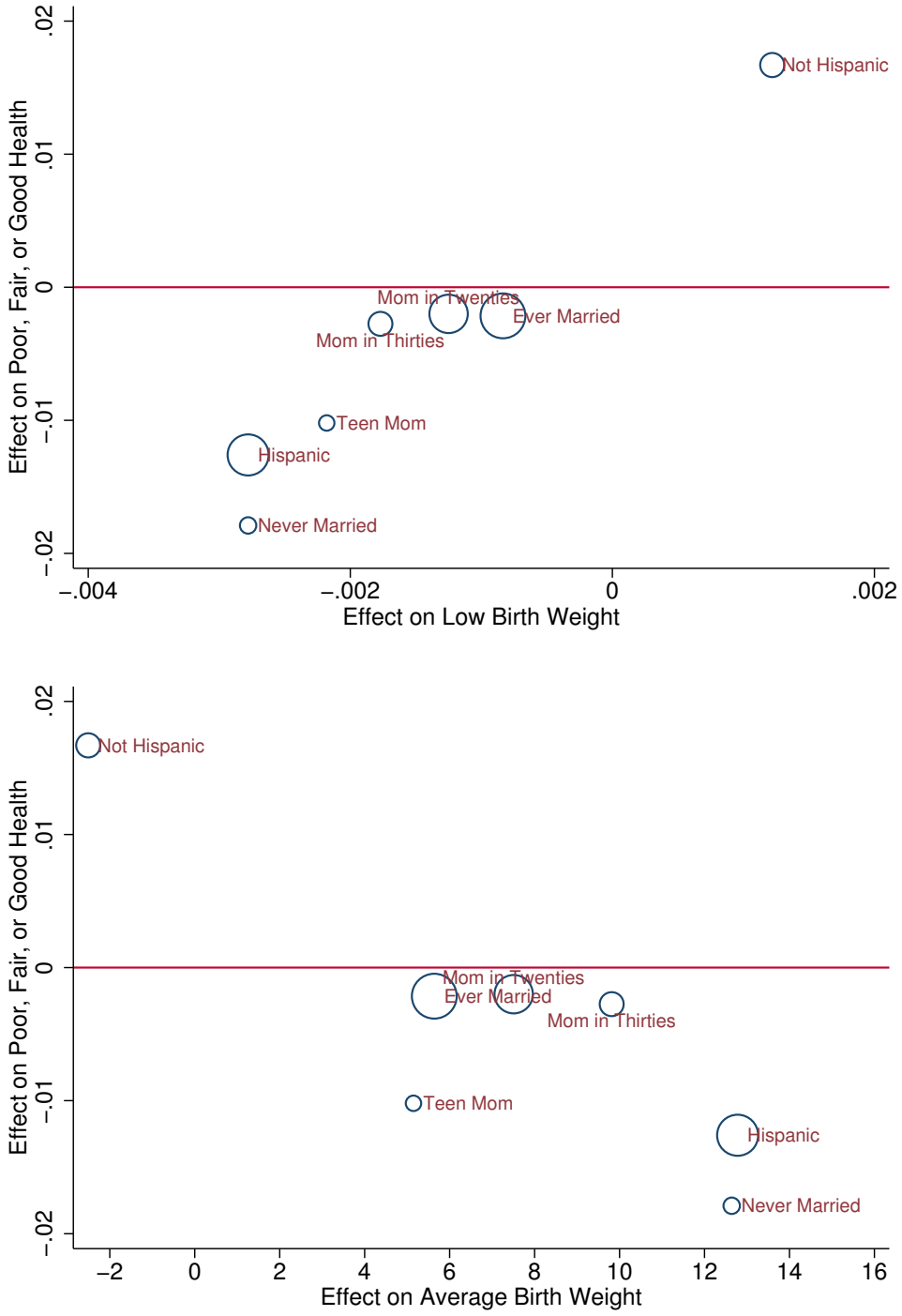
Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. The regression includes state of birth and year of birth fixed effects, controls for the child's demographics, the state unemployment rate and Medicaid/SCHIP generosity at the time of birth and the time of the survey, as well as these state by year and state by birth year controls interacted with an indicator for whether the child was born to treated immigrants. The results are weighted using the NHIS-provided weights. The dotted line indicates the difference in the average number of years the child's treated immigrant parents were eligible for Food Stamps, between Fill-In and No-Fill-In states, from the time the child was in utero to their fifth birthday. 95% confidence intervals shown in the dashed lines.

Figure 7: Subgroup Estimates of Effect on Participation and "Poor", "Fair" or "Good" Health



Notes: Estimates on the y-axis are from the 1998-2013 National Health Interview Survey and the sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16. Estimates on the x-axis are from the 1995-2007 Annual Social and Economic Supplement to the CPS and the sample includes children born in the U.S. between the ages of 0 and 4. Estimates are from the double difference models including only children of treated immigrants–children whose parents were born outside of the U.S. and who immigrated between 1985 and 1996–and are weighted using the NHIS and CPS-provided weights, respectively. The size of each circle indicates the relative sample size of each subgroup in the NHIS.

Figure 8: Subgroup Estimates of Effect on Birth Weight and "Poor", "Fair" or "Good" Health



Notes: Estimates of the effect on "Poor", "Fair" or "Good" health on the y-axis are from the 1998-2013 National Health Interview Survey and the sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16 whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Estimates of the effect on birth weight on the x-axis are from the 2000-2007 National Vital Statistics and the sample includes infants born in the U.S. to foreign-born mothers. Subgroup estimates are from the double difference models. Estimates from the NHIS weighted using the NHIS-provided weights. Results from Vital Statistics weighted by number of births in each state of birth, year and month of birth, and mother's treated immigrant status cell. The size of each circle indicates the relative sample size of each subgroup in the NHIS.

Table 1: Summary Statistics

	Children of Treated Immigrants	Children of Natives
Demographics of Child– Person File		
Male	0.49	0.49
Age	10	10
White	0.50	0.73
Black	0.06	0.22
Hispanic	0.79	0.10
Asian	0.03	0.00
Mom Less than High School	0.69	0.25
Mom Ever Married	0.87	0.83
Mom's Age at Birth	26	25
Mom's Years in U.S.	4.6	.
Dad's Age at Birth	29	28
Dad Less than High School	0.64	0.19
N	8353	41839
Health of Child– Person File		
Overall Health (1=excellent ... 5 =poor)	1.9	1.8
"Poor", "Fair", or "Good" Health	0.29	0.23
Overnight Hospitalizations	0.01	0.02
N	8353	41839
Health of Child– Sample Child File		
Number of School Days Missed	2.5	4.0
Two or More Doctor Visits	0.51	0.61
N	3282	18431

Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. The results are weighted using the NHIS-provided weights.

Table 2: Effect of Parents' Eligibility for Food Stamps on Food Stamp Access

	FS Participation	FS Benefits Received
	(1)	(2)
A: Double Difference, State by Year Controls		
Elig for FS	0.080***	184.781**
	(0.018)	(82.300)
Mean Y	0.25	731.54
N	5949	5949
B: Triple Difference, State by Year Controls		
Elig for FS	0.077***	168.681**
	(0.017)	(74.772)
Mean Y	0.29	962.08
N	45594	45594
C: Triple Difference, State by Year Fixed Effects		
Elig for FS	0.086***	147.131
	(0.023)	(117.944)
Mean Y	0.29	962.08
N	45594	45594

Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. The sample includes children born in the U.S. in 1989-2005 and between the ages of 0 and 4, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include state and year fixed effects, and controls for demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity at the time of the survey. In the triple difference models these state by year controls are also interacted with an indicator for whether the parents are treated immigrants to allow for the fact that these changes may affect immigrants and natives differently. In the triple difference model with state by year fixed effects, the controls that vary by state and year only are omitted. The results are weighted using the CPS-provided weights. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3: Effect of Food Stamps on All NHIS Health Outcomes

	Person File			Sample Child File	
	Overall Health (1=excell ... 5=poor)	"Poor" , "Fair", or "Good" Health	Any Overnight Hospitalizations	School Days Missed	2+ Doctor Visits
A: Double Difference, State by Birth Year Controls					
Num Yrs Elig Ages IU− >4	-0.037**	-0.016***	-0.002	-0.083	-0.013
	(0.014)	(0.005)	(0.002)	(0.097)	(0.010)
Mean Y	1.9	0.29	0.01	2.5	0.51
Observations	8275	8275	8272	3238	3249
B: Triple Difference, State by Birth Year Controls					
Num Yrs Elig Ages IU− >4	-0.034**	-0.015***	-0.002	-0.085	-0.014
	(0.014)	(0.005)	(0.002)	(0.101)	(0.010)
Mean Y	1.8	0.24	0.02	3.9	0.60
Observations	49742	49742	49707	21292	21471
C: Triple Difference, State by Birth Year Fixed Effects					
Num Yrs Elig Ages IU− >4	-0.031**	-0.015**	-0.003	-0.101	-0.008
	(0.015)	(0.006)	(0.003)	(0.108)	(0.013)
Mean Y	1.8	0.24	0.02	3.9	0.60
Observations	49742	49742	49707	21292	21471

Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include state of birth and year of birth fixed effects, and controls for demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity at the time of birth and the time of the survey. In the triple difference models these state by year controls interacted with an indicator for whether the child is born to treated immigrants are included to allow for the fact that these changes may affect immigrants and natives differently. In the triple difference model with state by birth year fixed effects, the controls that vary by state and birth year only are omitted. The results are weighted using the NHIS-provided weights. Standard errors are clustered by state of birth and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Effects on Food Stamp Participation and Health within Hispanic Subsample

	Hispanic Children					
	ASEC	NHIS Person File			NHIS Sample Child File	
	Food Stamp Participation	Overall Health (1=excell ... 5=poor)	"Poor", "Fair", or "Good" Health	Any Overnight Hospitalizations	School Days Missed	2+ Doctor Visits
A: Double Difference, State by Year Controls						
Elig for FS	0.109***					
	(0.019)					
Num Yrs Elig Ages IU− >4		-0.046***	-0.020***	-0.001	-0.160**	-0.017*
		(0.016)	(0.003)	(0.002)	(0.077)	(0.010)
Mean Y	0.24	1.9	0.31	0.02	2.5	0.51
Observations	4793	7050	7050	7049	2731	2725
B: Triple Difference, State by Year Controls						
Elig for FS	0.105***					
	(0.019)					
Num Yrs Elig Ages IU− >4		-0.044***	-0.022***	-0.001	-0.158**	-0.017
		(0.016)	(0.007)	(0.003)	(0.079)	(0.010)
Mean Y	0.31	1.9	0.28	0.02	3.9	0.60
Observations	9590	14031	14031	14022	5382	5424
C: Triple Difference, State by Year Fixed Effects						
Elig for FS	0.105***					
	(0.046)					
Num Yrs Elig Ages IU− >4		-0.036	-0.016*	0.000	-0.464***	-0.010
		(0.023)	(0.009)	(0.005)	(0.146)	(0.015)
Mean Y	0.31	1.9	0.28	0.02	3.9	0.60
Observations	9590	14031	14031	14022	5382	5424

Notes: Data from the 1998-2013 National Health Interview Survey and the 1995-2007 Annual Social and Economic Supplement to the CPS. The sample from the NHIS includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16, whose mothers have a high school education or less and who are Hispanic. The sample from the ASEC includes children born in the U.S. between the ages of 0 and 4 whose mothers have a high school education or less and who are Hispanic. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. The results are weighted using the NHIS and ASEC provided weights, respectively. Standard errors are clustered by state of birth in the NHIS, and state of survey in the ASEC, and are shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 5: Effect on Parent-Reported Overall Health, Specification Checks

	Double Difference						Triple Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A: Overall Health (1=excell ... 5=poor)							
Num Yrs Elig Ages IU− >4	-0.037**	-0.035**		-0.034**	-0.035**	-0.048**	-0.016
	(0.013)	(0.014)		(0.016)	(0.013)	(0.019)	(0.014)
Num Yrs Elig Ages 5− >Survey		-0.007					
		(0.013)					
Num Yrs Elig Ages IU− >1			-0.043**				
			(0.020)				
Num Yrs Elig Ages 2− >4			-0.033*				
			(0.019)				
Mean Y	1.63						
B: "Poor", "Fair" or "Good" Health							
Num Yrs Elig Ages IU− >4	-0.016***	-0.017***		-0.013**	-0.016***	-0.011	-0.008
	(0.005)	(0.005)		(0.006)	(0.005)	(0.008)	(0.007)
Num Yrs Elig Ages 5− >Survey		0.002					
		(0.007)					
Num Yrs Elig Ages IU− >1			-0.024***				
			(0.008)				
Num Yrs Elig Ages 2− >4			-0.011				
			(0.009)				
Mean Y	0.29						
Eligibility at Ages 5 +		X					
Split Eligibility by Ages			X				
Model Illinois & New Jersey as Fill-In				X			
Model Teen Moms as Children for Eligibility					X		
Omit California						X	
Omit All Fill-In							X
Observations	8275	8275	8275	8275	8275	4628	38102

Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include state of birth and year of birth fixed effects, and controls for demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity at the time of birth and the time of the survey. In the triple difference models these state by year controls interacted with an indicator for whether the child is born to treated immigrants are included to allow for the fact that these changes may affect immigrants and natives differently. The results are weighted using the NHIS-provided weights. Standard errors are clustered by state of birth and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Effect on Parent-Reported Overall Health, Adding Birth Year and Survey Year Controls

	Double Difference							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Overall Health (1=excell ... 5=poor)								
Num Yrs Elig Ages IU− >4	-0.037**	-0.033*	-0.031***	-0.034**	-0.028	-0.040***	-0.035**	-0.024
	(0.014)	(0.017)	(0.016)	(0.019)	(0.018)	(0.014)	(0.016)	(0.015)
Mean Y	1.9							
B:"Poor", "Fair" or "Good" Health								
Num Yrs Elig Ages IU− >4	-0.016***	-0.009	-0.013*	-0.014**	-0.009	-0.017***	-0.013*	-0.008
	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
Mean Y	0.29							
Other Safety Net Generosity at Birth		X						
Other State Fill In at Birth			X					
Attitude Towards Immigrants at Birth				X				
Other FS Changes at Birth					X			
Survey State and Year Controls						X		
State 1990 Char * Trends							X	
State of Birth Linear Trends								X
Observations	8275	8275	8275	8275	8275	8275	8275	8275

Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include state of birth and year of birth fixed effects, and controls for demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity at the time of birth and the time of the survey. The results are weighted using the NHIS-provided weights. Standard errors are clustered by state of birth and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 7: Falsification Tests for Children of Untreated Groups

	Double Difference				
	Overall Health (1=excell ... 5=poor)	"Poor", "Fair", or "Good" Health	Any Overnight Hospitalizations	School Days Missed	2+ Doctor Visits
A: Children of Natives, Mother High School or Less					
Num Yrs Immig Elig Ages IU− >4	0.000	-0.001	0.000	0.042	-0.006
	(0.006)	(0.003)	(0.001)	(0.072)	(0.007)
Observations	41467	41467	41435	18054	18222
B: Children of Immigrants Enter U.S. Before 1985, Mother High School or Less					
Num Yrs Immig Elig Ages IU− >4	-0.003	-0.002	0.000	0.001	-0.030
	(0.017)	(0.007)	(0.003)	(0.180)	(0.018)
Observations	5317	5317	5318	2213	2226
C: Children of Treated Immigrants, Mother College +					
Num Yrs Elig Ages IU− >4	0.041	0.020	0.003	-0.007	0.007
	(0.026)	(0.012)	(0.003)	(0.154)	(0.037)
Observations	1349	1349	1351	685	683

Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include state of birth and year of birth fixed effects, and controls for demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity at the time of birth and the time of the survey. The results are weighted using the NHIS-provided weights. Standard errors are clustered by state of birth and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 8: Effect of Food Stamps on Infant Health

	Birth Weight in Grams	Low Birth Weight (<2500 g)
A: Double Difference, State by Birth Time Controls		
Mother Elig 3rd Trimester	6.52**	-0.001**
	(2.96)	(0.001)
Mean Y	3295	0.07
Observations	4896	4896
B: Triple Difference, State by Birth Time Controls		
Mother Elig 3rd Trimester	9.07**	-0.002***
	(3.57)	(0.000)
Mean Y	3286	0.08
Observations	9792	9792
C: Triple Difference, State by Birth Time Fixed Effects		
Mother Elig 3rd Trimester	3.37	-0.001
	(4.76)	(0.001)
Mean Y	3286	0.08
Observations	9792	9792

Notes: Data are from the 2000-2007 National Vital Statistics. The sample includes infants born in the U.S. and is collapsed to the state of birth, year and month of birth, and mother's treated immigrant status cell. Results are weighted by number of births in each cell. Children born to treated immigrants are defined as those whose mother was born outside of the U.S.. Children born to natives are defined as those whose mother was born in the U.S.. All regressions include controls for state of birth and year by month of birth fixed effects, and demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity in the year before birth. In the triple difference models these state by year controls are interacted with whether the mother is a treated immigrant to allow for the fact that these changes may affect immigrants and natives differently. In the triple difference model with state by time of birth fixed effects, the controls that vary by state and year only are omitted. Time of birth indicates the year and month of birth. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

A Appendix: Background

A.1 Food Stamp Program Details

In order to be eligible for Food Stamps, families with children must meet several income and asset tests. First, families' "gross income" must be below 130% of the poverty line. Not all income is counted as gross income. The major components of gross income are earnings, cash income from other safety net programs (e.g. TANF, Unemployment Insurance, Social Security) and child support. The second income test is on "net income", which is gross income minus deductions, and net income must be below 100% of the poverty line to be eligible. There is a standard deduction, as well as a deduction for earnings, child care expenses, medical expenses, child support payments, and excess shelter costs (high rent and utility payments). During the late 1990s and early 2000s most states also had an asset test as part of their eligibility requirements.[62] Alternatively, families are eligible if they received AFDC/TANF benefits, SSI payments, or General Assistance benefits, although these programs often had income eligibility thresholds below the Food Stamp thresholds. Able-bodied adults without dependents (essentially non-disabled working-age adults without children) were subject to new, stricter limits on their eligibility as the result of welfare reform in 1996, however these changes are not likely to play a large role in my context as I focus on families with children.

For families that are eligible, family-level benefit amounts are calculated as follows: families receive the maximum benefit amount minus 30% of the families' "net income". The maximum benefit amount is determined by the Department of Agriculture's Thrifty Food Plan, which is designed to provide adequate nutrition at minimal cost, is indexed to inflation, and varies with family size. Appendix Table (A.14) shows the maximum monthly benefit amount for families based on their size in fiscal year 1998. These amounts are the same in the continental U.S. and are slightly different in Alaska and Hawaii.

A.2 Vital Statistics Data

For the analysis on infant health outcomes, I use the 2000-2007 Vital Statistics Natality Data from the National Center for Health Statistics. Public-use data from 2000-2004 contain state identifiers and I obtained a restricted use version of the 2005-2007 data files that include state identifiers through an application to the National Association of Public Health Statistics and Information Systems (NAPHSIS).[63] This data is well-suited for my analysis because it contains information

[62] This information is from Wilde (2001) and (The Center on Budget and Policy Priorities, 2013*b*).

[63] Specifically the data are the Natality–Limited Geography files for 2005-2007 originally from the National Center for Health Statistics and compiled from data provided by the 57 vital statistics jurisdictions through the

about the birth weight of each infant as well as their mothers' demographic information, including mothers' country of birth (coded as either: U.S., Mexico, or other) for the universe of births in the United States. However, there are several important limitations of this data. First, the data does not contain any information about the year of entry of foreign-born mothers, making analysis of the policy changes harder because I cannot construct a sample of "pre-PRWORA" immigrants. To circumvent this issue, I focus only on births between 2000 and 2007, so as to capture the effects of the restoration of eligibility resulting from the 2002 Farm Bill, but to avoid picking up effects of welfare reform more generally. The second disadvantage is that I do not observe fathers' place of birth, so children of treated immigrants and children of natives are simply defined as those born to foreign-born or U.S.-born mothers, respectively. Finally, over my sample period mother's education is not uniformly reported in the data, so I do not condition my sample on mother's educational attainment. I collapse the data to the month-year of birth, state of birth, and mothers' country of birth level for ease of computation, and weight by the number of births in each cell. The specifications I use to estimate the effects are those in equations (3) and (4) where, instead of indexing by year, I index by the year and month of birth.

B Appendix: Further Results

B.1 Comparison of Food Stamp Program Participation Results with the Previous Literature

As described above, I find no effect of Food Stamp eligibility on participation in programs besides Food Stamps. This appears contradictory to some of the findings from the previous literature, which documented that immigrant participation for many programs declined following welfare reform. However, there are several differences between my study and this literature that explain the discrepancy. I describe these differences next.

Overall, the previous literature argues that a harsh policy climate after welfare reform led to declines in participation, above and beyond changes in participation due to changes in eligibility rules. One of the pioneering papers documenting this "chilling effect" compares mean participation rates of all immigrants to all natives, and of citizens to non-citizens, over time (Fix and Passel, 1999). However, there are several reasons these unadjusted participation rates may not be the best way to compare immigrants and natives. First, natives and immigrants live in different states: immigrants are more concentrated than natives in very few states (along the southern border of the U.S. as well as New York and several other East Coast states). Therefore, it may be important

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to control for state fixed effects to take account of differential patterns in participation occurring across different states. Second, the demographics of these two sets of families are different, as shown in Table (A.15). Mothers in immigrant families have less education, more children, are younger, and are more likely to be poor than native mothers, so it is important to control for observable characteristics of these families as well.[64] Indeed, Haider et al. (2004) find state fixed effects, demographic controls, and controls for state economic conditions explain much of the differential decline in program participation among immigrants relative to natives after welfare reform.

The other major difference between my analysis and the previous literature is the policy variation utilized; I use the variation in Food Stamp eligibility across states and over time for pre-PRWORA immigrants only, whereas most of the chilling effect literature looks at changes in participation for *all* immigrants *nationally* before and after welfare reform.[65] Therefore, the findings in these other papers may not be directly applicable to my setting. However, if there was a fall in participation nationally in programs besides Food Stamps for pre-PRWORA immigrants following welfare reform, I will not pick this up with the state by year identification strategy. I test for these national participation effects directly next.

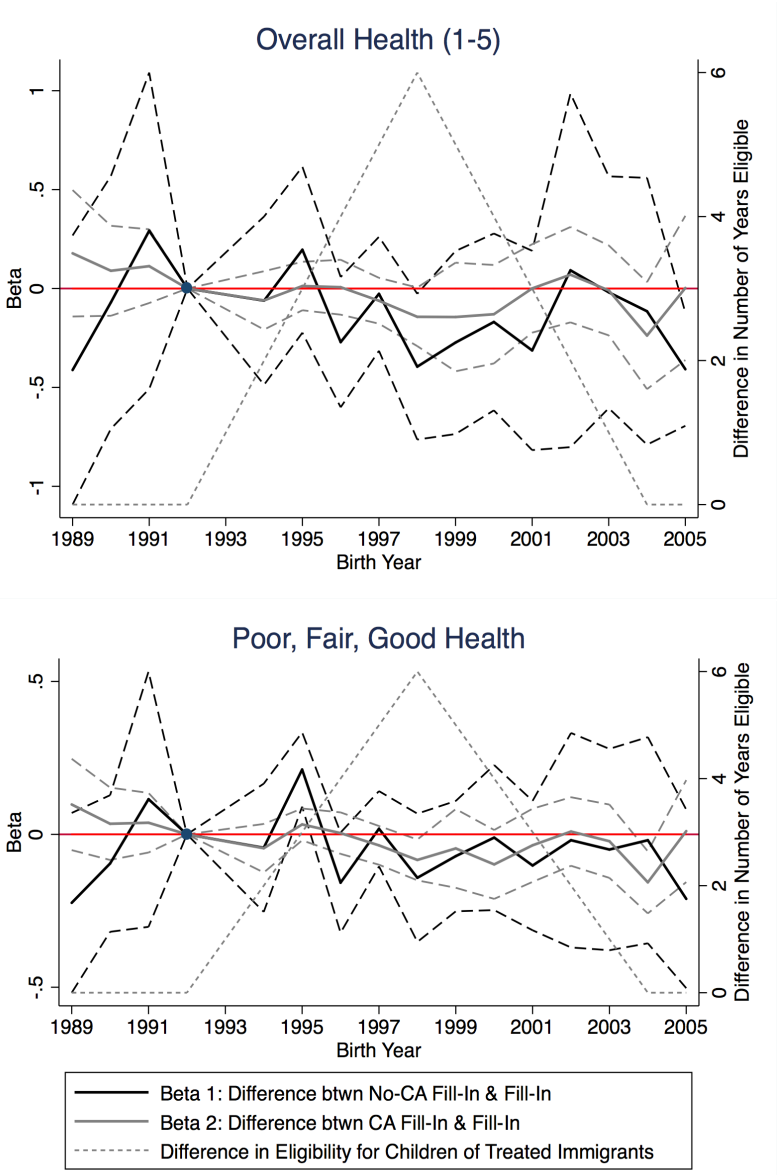
I conduct my own analysis of the chilling effect that accounts for all of the issues described above. I restrict the sample to families where the mother has a high school education or less, and children born in the U.S. to reflect the sample choices made above. I find in Table (A.16) that accounting for differences in demographics between immigrants and natives explains most of the differential decline in program participation among pre-PRWORA immigrants, relative to natives, following welfare reform. After accounting for differential demographics and state of residence, there is no difference in the change in participation in Medicaid/SCHIP between immigrants and natives.[66] There is a marginally significant differential decrease in AFDC/TANF participation, and a significant differential decrease in participation in SSI, however the fraction of families that participate in SSI is small, so this is unlikely to drive the estimated effects in my main analysis. Additionally, there is a differential *increase* in participation in Free and Reduced Price Lunch, but this would cause my estimated effects to be smaller in the main analysis, if participation in these programs improves children's health. Importantly, even after controlling for all of these factors, the effect on Food Stamp participation remains.

[64] I test which of these two factors is more important by controlling for demographics without state fixed effects and vice versa and while both are important, demographics appear to be more so.

[65] Other differences include: 1) not restricting the sample to low-income or low-education households (Borjas, 2003), 2) looking at all programs simultaneously to see if participation in any program changed (Borjas, 2002, 2004), and 3) separating naturalized citizen families from legal permanent resident families (Kandula et al., 2004).

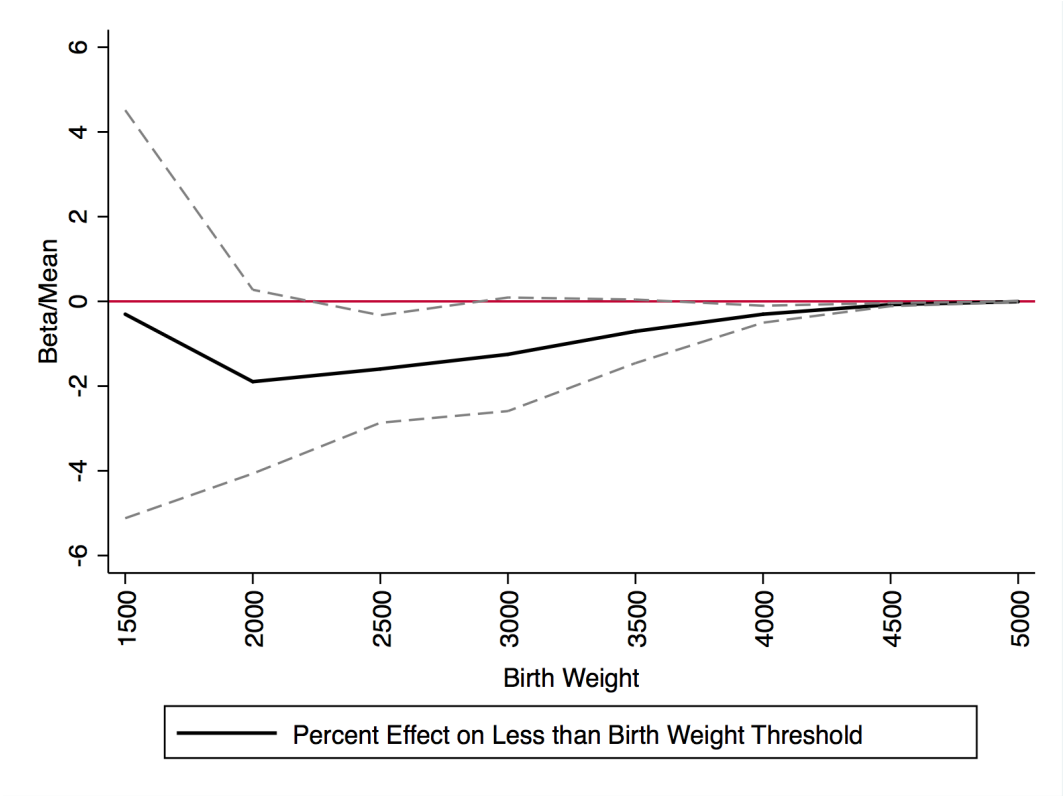
[66] This is similar to the findings in Borjas (1999), which suggest observable characteristics between immigrants and native explain much of the difference in their safety net program participation.

Figure A.1: Difference in Children's Parent-Reported Overall Health: CA and Non-CA Fill-In States Among Children of Treated Immigrants



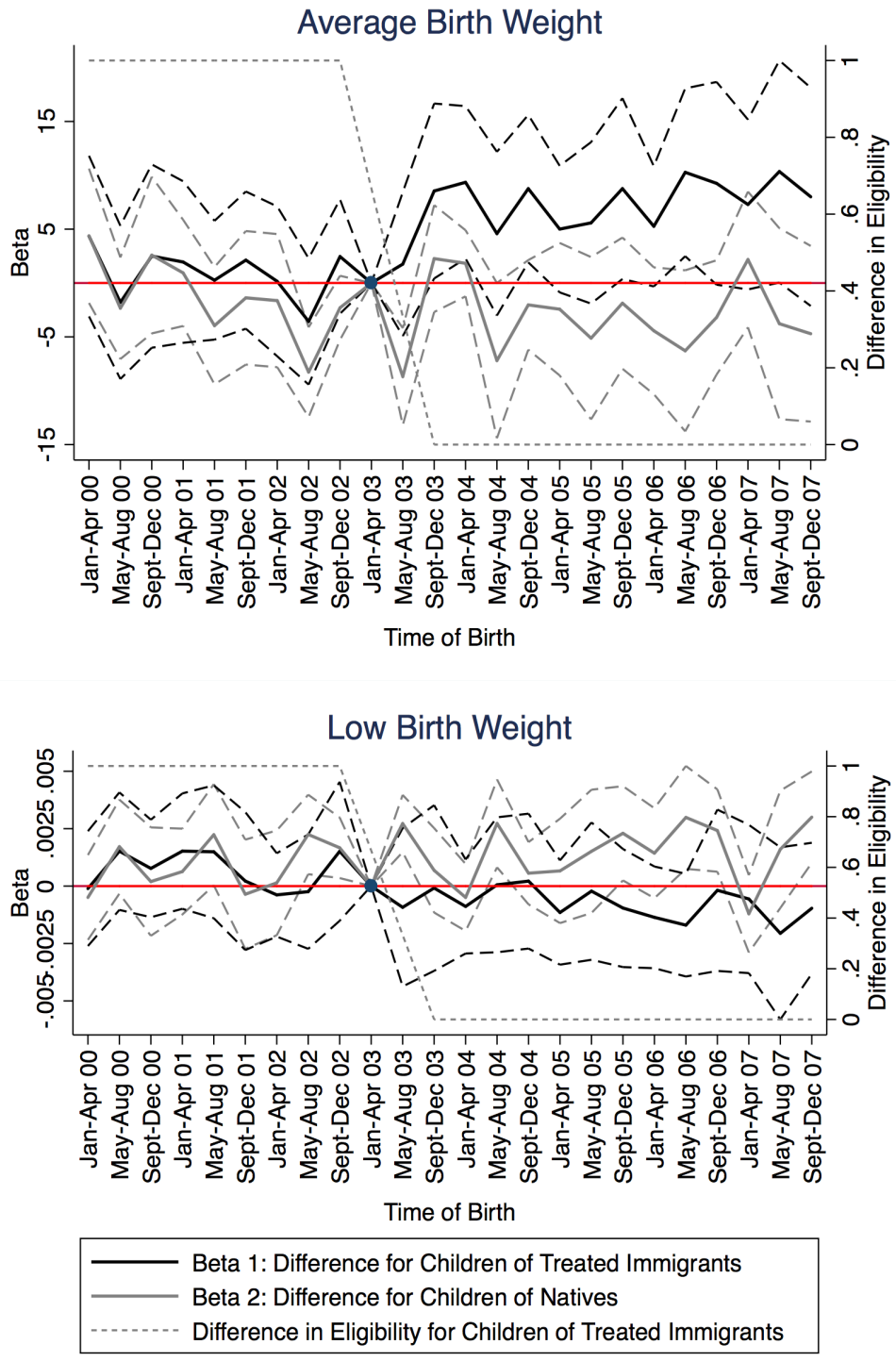
Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16, whose mothers have a high school education or less and who are children of treated immigrants. The regression includes state of birth and year of birth fixed effects, controls for the child's demographics, the state unemployment rate and Medicaid/SCHIP generosity at the time of birth and the time of the survey. The results are weighted using the NHIS-provided weights. The dotted line indicates the difference in the average number of years the child's Treated Immigrant parents were eligible for Food Stamps between Fill-In and No-Fill-In states from the time the child was in utero to their fifth birthday. 95% confidence intervals shown in the dashed lines.

Figure A.2: Effects on Birth Weight Distribution



Notes: Data are from the 2000-2007 National Vital Statistics. The sample includes infants born in the U.S. and is collapsed to the state of birth, year and month of birth, and mother's treated immigrant status cell. Results are weighted by number of births in each cell. Children born to treated immigrants are defined as those whose mother was born outside of the U.S.. All regressions include controls for state of birth and year by month of birth fixed effects, and demographic characteristics. The double difference models include only children born to treated immigrants and controls for the state unemployment rate and Medicaid/SCHIP generosity in the year before birth. The estimates shown take the coefficients from the double difference regression indicating the effect of Food Stamps on the likelihood the child is born below a given birth weight threshold, and divide by the percentage of children born below that threshold. The effects shown can be interpreted as percent changes. The x-axis indicates the birth weight threshold and the y-axis indicates the percent effect. 95% confidence intervals shown in dashed lines. Results are weighted by the number of births in each cell.

Figure A.3: Difference in Children's Birth Weight



Notes: Data are from the 2000-2007 National Vital Statistics. The sample includes infants born in the U.S. and is collapsed to the state of birth, year and month of birth, and mother's treated immigrant status cell. Results are weighted by number of births in each cell. Children born to treated immigrants are defined as those whose mother was born outside of the U.S.. Children born to natives are defined as those whose mother was born in the U.S.. The regression includes controls for state of birth and year by month of birth fixed effects, and demographic characteristics, the state unemployment rate and Medicaid and SCHIP generosity at the time of birth as well as these state by birth year controls interacted with whether the child was born to treated immigrants. The dotted line indicates the difference in mothers' eligibility in the third trimester of pregnancy for children of Treated Immigrants between Fill-In and No-Fill-In states. 95% confidence intervals shown in the dashed lines.

Table A.1: Mean Health Outcomes by Parent-Reported Health

	"Poor", "Fair", or "Good" Health	"Very Good" or "Excellent" Health
Overnight Hospitalizations	0.03	0.01
Number of School Days Missed	3.08	2.14
Two or More Doctor Visits	0.57	0.49

Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children of treated immigrants born in the U.S. in 1989-2005 and between the ages of 6 and 16, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. The results are weighted using the NHIS-provided weights.

Table A.2: Effect of Parents' Eligibility on Amount of Food Stamps Received Among Participants

	(1)	(2)	(3)
Elig for FS	-332.535	-448.330*	-728.604**
	(264.328)	(241.175)	(334.956)
Mean Y	2973.16	3332.79	3332.79
Double Difference	X		
Triple Difference		X	X
State*Year FE			X
N	1403	12591	12591

Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. The sample includes children born in the U.S. in 1989-2005 and between the ages of 0 and 4, whose mothers have a high school education or less who reported participating in Food Stamps. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include state and year fixed effects, and controls for demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity at the time of the survey. In the triple difference models these state by year controls are also interacted with an indicator for whether the parents are treated immigrants to allow for the fact that these changes may affect immigrants and natives differently. In the triple difference model with state by year fixed effects, the controls that vary by state and year only are omitted. The results are weighted using the CPS-provided weights. Standard errors are clustered by state and shown in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.3: Effect of Parents' Eligibility for Food Stamps on Program Participation

	FS Participation	SSI Participation	TANF/AFDC Participation	Medicaid/SCHIP Participation	School Lunch Participation
A: Double Difference, State by Year Controls					
Elig for FS	0.080***	-0.000	0.007	0.010	-0.003
	(0.018)	(0.009)	(0.020)	(0.047)	(0.022)
Mean Y	0.25	0.02	0.14	0.52	0.44
N	5949	5949	5949	5949	5949
B: Triple Difference, State by Year Controls					
Elig for FS	0.077***	-0.002	0.003	0.010	-0.007
	(0.017)	(0.009)	(0.019)	(0.043)	(0.025)
Mean Y	0.29	0.04	0.16	0.43	0.26
N	45594	45594	45594	45594	45594
C: Triple Difference, State by Year Fixed Effects					
Elig for FS	0.086***	-0.002	0.016	0.044	-0.015
	(0.023)	(0.013)	(0.027)	(0.042)	(0.023)
Mean Y	0.29	0.04	0.16	0.43	0.26
N	45594	45594	45594	45594	45594

Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. The sample includes children born in the U.S. in 1989-2005 and between the ages of 0 and 4, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include state and year fixed effects, and controls for demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity at the time of the survey. In the triple difference models these state by year controls are also interacted with an indicator for whether the parents are treated immigrants to allow for the fact that these changes may affect immigrants and natives differently. In the triple difference model with state by year fixed effects, the controls that vary by state and year only are omitted. The results are weighted using the CPS-provided weights. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A.4: Treatment on the Treated Effects

	Effect on FS	ITT as p.p.	TOT= ITT/ΔFS	Mean Y of Poor	TOT as %
Effect on Participants					
Estimated Effect	0.08 p.p.	1.6 p.p.	20 p.p.	33%	61%
Adjusted for Under-Reporting	0.13 p.p.	1.6 p.p.	12 p.p.	33%	36%
Effect Per $1000 Benefits Received					
Estimated Effect	$185	1.6 p.p.	9 p.p.	33%	26%
Adjusted for Under-Reporting	$308	1.6 p.p.	5 p.p.	33%	15%

Notes: See text for detailed descriptions of calculations.

Table A.5: Correlation of State Demographic Characteristics in 1990 with Whether the State is Fill-In State

	(1)	(2)
Frac of Adults with More HS	0.007	0.004
	(0.011)	(0.013)
Frac of Pop Black	-0.007	-0.017**
	(0.005)	(0.007)
Frac of Adults Foreign-Born	-0.068	-0.069*
	(0.040)	(0.037)
Frac Foreign-Born Adults with More HS	-0.003	0.008
	(0.008)	(0.008)
Frac of Pop Age<= 16	-0.064	-0.060
	(0.054)	(0.081)
Frac of Pop Age> 65	0.025	-0.007
	(0.046)	(0.041)
Frac of Kids Foreign-Born	0.180	0.225**
	(0.107)	(0.089)
Frac of Pop Age<= 5	0.104	0.145
	(0.186)	(0.278)
Population Weight		X
N	51	51

Notes: Data are from the 1990 1% Census. The adult population is defined as those aged 25 to 62 and the child population is defined as those aged 0 to 16. The weighted results use the state population in 1990. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A.6: Correlation of State Political Party in 1996 with Whether the State is Fill-In State

	(1)	(2)	(3)	(4)
Governor Republican	0.076	0.076	0.169	0.169
	(0.104)	(0.103)	(0.162)	(0.160)
Governor Independent	0.889***		0.900***	
	(0.076)		(0.081)	
Mean Y	0.18	0.16	0.23	0.22
Population Weights			X	X
Omit Maine		X		X
N	51	50	51	50

Notes: Data are from Wikipedia pages and Dave Leip's Atlas of U.S. Presidential Elections for the results of elections from 1992-1996. See text for more detailed description. The weighted results use the state population in 1996. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Correlation of State Attitude Towards Immigrants in 1996 with Whether the State is Fill-In State

	(1)	(2)
Fraction Foreign-Born Deported	0.024	0.025
	(0.072)	(0.101)
Fraction Population Want Immigration Decreased	-0.038	0.064
	(0.038)	(0.060)
Mean Y	0.18	0.23
Population Weights		X
N	51	51

Notes: Data are from the 1996 American National Election Study and the TRAC database for 1996. See text for more detailed description. The weighted results use the state population in 1996. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A.8: Correlation of State Fill-In Programs with Time-Varying Characteristics

	Spend Sch	Spend Med	Spend Welf	Spend SSI	Max Welf	Urate	Wheth EITC	Med Expand	Med Gen 0	Med Gen 6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Immig Elig for FS	184.78	0.01	-0.00	-0.00	8.82	-0.29*	-0.02	-0.05	23.28*	10.76
	(165.71)	(0.02)	(0.00)	(0.00)	(10.52)	(0.15)	(0.05)	(0.05)	(13.86)	(7.83)
Mean Y	8974.29	0.95	0.08	0.14	411.60	5.09	0.24	0.73	195.73	198.14
N	557	714	714	714	714	714	714	714	714	714

Notes: Data sources are described in the text. All spending is per person except for school which is per pupil and all dollar amounts are in 2009 $s. "Med Expand" indicates the timing of the state's Medicaid/SCHIP expansion after SCHIP was created in 1997, and the other measures of Medicaid and SCHIP generosity are expressed as a percentage of the poverty line. The regressions include state and year fixed effects. The results weighted with the state population. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A.9: Effect of Food Stamp Eligibility on Immigrant Population

	Treat Immig	Frac Pop
	(1)	(2)
Elig for FS	-0.553	-0.010
	(1.115)	(0.008)
Mean Y	15.56	0.09
N	535	535

Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. Sample is mothers aged 16 to 55 who have children under age 16 that were born in the U.S., collapsed to the state and year level. The dependent variables are the number of treated immigrant families in each state and year, and the number of these families, divided by the number of native families in each state and year. The results are weighted using the CPS-provided weights. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A.10: Effect of Food Stamp Eligibility on Fertility

	Number of Children		
	(1)	(2)	(3)
Elig for FS	-0.017	-0.035	-0.086*
	(0.041)	(0.040)	(0.049)
Triple Difference		X	X
State by Year FE			X
Mean Num Kids	1.81	1.37	1.37
N	10542	121554	121554

Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. Sample is women aged 16 to 45 with a high school education or less. The dependent variable is the number of children in the household. The regression includes state and year fixed effects, and demographic characteristics. The double difference models include controls for the state unemployment rate at the time of survey, and controls for Medicaid and SCHIP generosity at the time of the survey. In the triple difference models these state by year controls interacted with an indicator for whether the family is a treated immigrant family are included to allow for the fact that these changes may affect immigrants and natives differently. In the triple difference model with state by year fixed effects, the controls that vary by state and year only are omitted. The results are weighted using the CPS-provided weights. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A.11: Robustness Checks of Effects on Food Stamp Participation

	Double Difference								Triple Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Elig for FS	0.080***	0.056**	0.074***	0.080***	0.057**	0.072***	0.075***	0.030	0.042*
	(0.018)	(0.022)	(0.022)	(0.019)	(0.026)	(0.020)	(0.020)	(0.041)	(0.022)
Mean Y	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.22	0.29
Other Safety Net Generosity		X							
Other Fill-In Programs			X						
Attitudes Towards Immigrants				X					
Other FS Options					X				
State 1990 Char						X			
State Linear Time Trends							X		
Omit California								X	
Omit All Fill-In									X
N	5949	5949	5949	5949	5949	5949	5949	4148	36966

Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. The sample includes children born in the U.S. in 1989-2005 and between the ages of 0 and 4, whose mothers have a high school education or less. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include state and year fixed effects, and controls for demographic characteristics. The double difference models include controls for the state unemployment rate and Medicaid/SCHIP generosity at the time of the survey. In the triple difference models these state by year controls are also interacted with an indicator for whether the parents are treated immigrants to allow for the fact that these changes may affect immigrants and natives differently. The results are weighted using the CPS-provided weights. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A.12: Falsification Tests: Food Stamp Participation

	Parents Treated Immig	Parents Natives	Parents Immig <1985
	(1)	(2)	(3)
Treated Immig Eligible	-0.021	-0.026	0.073*
	(0.018)	(0.022)	(0.042)
Mean Y	0.03	0.29	0.28
Mom College or More	X		
Mom High School or Less		X	X
N	1116	39645	1944

Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. The sample includes children born in the U.S. in 1989-2005 and between the ages of 0 and 4. Children of treated immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. Children of natives defined as those whose parents were born in the U.S.. All regressions include controls for state and year fixed effects, and demographic characteristics. The double difference models include controls for the state unemployment rate at the time of survey, and controls for Medicaid and SCHIP generosity at the time of the survey. The results are weighted using the CPS-provided weights. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A.13: Effects on Food Stamp Participation and Health by Gender and Age

	Double Difference				
	Overall Health (1=excell ... 5=poor)	"Poor", "Fair", or "Good" Health	Any Overnight Hospitalizations	School Days Missed	2+ Doctor Visits
A: All Children					
Num Yrs Elig Ages IU− >4	-0.037**	-0.016***	-0.0018	-0.083	-0.013
	(0.014)	(0.005)	(0.002)	(0.097)	(0.010)
Mean Y	1.9	0.29	0.01	2.5	0.51
Observations	8275	8275	8272	3238	3249
B: Male					
Num Yrs Elig Ages IU− >4	-0.055***	-0.020***	-0.003	-0.202	-0.026*
	(0.013)	(0.007)	(0.002)	(0.142)	(0.014)
Observations	4207	4207	3564	1599	1612
C: Female					
Num Yrs Elig Ages IU− >4	-0.012	-0.011	0.001	0.102	-0.003
	(0.022)	(0.008)	(0.004)	(0.157)	(0.020)
Observations	4068	4068	3485	1639	1637
D: Ages 6-10					
Num Yrs Elig Ages IU− >4	-0.030	-0.008	-0.003**	-0.293**	-0.020
	(0.022)	(0.011)	(0.002)	(0.109)	(0.021)
Observations	4540	4540	3564	1700	1713
E: Ages 11-16					
Num Yrs Elig Ages IU− >4	-0.044**	-0.023***	0.001	0.099	-0.008
	(0.022)	(0.008)	(0.005)	(0.128)	(0.012)
Observations	3735	3735	3735	1538	1536

Notes: Data from the 1998-2013 National Health Interview Survey. The sample includes children born in the U.S. in 1989-2005 and between the ages of 6 and 16 whose mothers have a high school education or less. Children of Treated Immigrants defined as those whose parents were born outside of the U.S. and who immigrated between 1985 and 1996. All regressions include state of birth and year of birth fixed effects, and controls for the child's demographics. The double difference models include controls for the state unemployment rate at the time of birth and the time of survey, and controls for Medicaid and SCHIP generosity at the time of birth and the time of the survey. The results are weighted using the NHIS-provided weights. Standard errors are clustered by state of birth and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A.14: Maximum Food Stamp Benefit in Continental United States in Fiscal Year 1998

Household Size	Benefit Amount
1	$122
2	$224
3	$321
4	$408
5	$485
6	$582
7	$643
8	$735
Each Add'l Member	$92

Notes: Maximum benefit amounts from USDA "CHARACTERISTICS OF FOOD STAMP HOUSEHOLDS FISCAL YEAR 1998". Values are slightly different for Alaska and Hawaii.

Table A.15: Pre-existing Differences in Demographics

	Immigrants	Natives
Food Stamp Participation	0.36	0.22
Married	0.73	0.72
Number of Kids	2.2	1.9
Number of US-Born Kids	1.6	1.9
Number of Elders (65+)	0.03	0.01
Less High School	0.71	0.21
Below Poverty Line	0.53	0.23
Age	30	35

Notes: Data are from the 1995-1996 Annual Social and Economic Supplement to the CPS. Sample is treated immigrant and native mothers who have children under age 16 that were born in the US.

Table A.16: Effect of Welfare Reform on Program Participation

	1995-2007				
	Food Stamps	SSI	AFDC/TANF	Med/SCHIP	Free Lunch
A: No Controls					
Post*Immig	-0.151***	-0.024***	-0.088***	-0.098***	0.082***
	(0.023)	(0.006)	(0.022)	(0.026)	(0.018)
Mean Y	0.20	0.05	0.10	0.31	0.30
N	99337	99337	99337	99337	99337
B: Demographic Controls					
Post*Immig	-0.074***	-0.019***	-0.037**	-0.010	0.085***
	(0.021)	(0.006)	(0.017)	(0.020)	(0.018)
Mean Y	0.20	0.05	0.10	0.31	0.30
N	98790	98790	98790	98790	98790
C: Demographics, State FE					
Post*Immig	-0.073***	-0.018***	-0.032*	-0.007	0.083***
	(0.021)	(0.006)	(0.016)	(0.021)	(0.017)
Mean Y	0.20	0.05	0.10	0.31	0.30
N	98790	98790	98790	98790	98790

Notes: Data are from the 1995-2007 Annual Social and Economic Supplement to the CPS. Sample is mothers who have a high school education or less and who have children that were born in the US and are under age 17. Post is equal to one in 1997 and after. All regressions include survey year fixed effects. Demographic controls include: age and marital status of mother, number of children, number of elderly, number of family members, and race. Standard errors are clustered by state and shown in parentheses. The results are weighted using the CPS-provided weights. * p<0.10, ** p<0.05, *** p<0.01