PRELIMINARY AND INCOMPLETE

The Impact of the Food Stamp Program on Infant Outcomes

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Abstract

This paper evaluates the health impact of a signature initiative of the War on Poverty: the roll out of the modern Food Stamp Program (FSP) during the 1960s and early 1970s. Using variation in the month the FSP began operating in each U.S. county, we find that pregnancies exposed to a new FSP three months prior to birth yielded deliveries with increased birth weight. Estimated impacts are robust to inclusion of county fixed effects, measures of other federal transfer spending, and time effects (among them county-specific time trends). We conclude that the sizeable increase in income from Food Stamp benefits improved birth outcomes for both Whites and African Americans, with larger impacts for births to African American mothers.

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1. Introduction

Compared to other high-income countries, newborn health in the U.S. is poor. Infant mortality is more than one-third higher than in Portugal, Greece, Ireland, and Britain, and double that in Japan and the Nordic countries. Low birth weight is X times more common in the U.S. than the OECD average. Because the relationship between health and income is concave, health at the bottom of the income distribution exert a disproportionate effect on aggregate health measures. As a result, one plausible hypothesis for poor average newborn health in the U.S. is that the bottom tail of the U.S. income distribution extends below (and is fatter than) that in other developed countries.

In this paper, we look at the health consequences of changes in the resources available to America's poorest. We utilize a natural experiment afforded by the county by county roll-out of the modern Food Stamp Program (FSP) during the 1960s and early 1960s. Maternal nutrition is thought to be one of the primary factors affecting birth outcomes, birth weight in particular (see, e.g., Kramer 1987a; Kramer 1987b). Thus, given the poor baseline nutrition of the 1960s, we might observe a direct effect of improved nutrition on newborn health. In addition, to the extent that the Food Stamp benefit was inframarginal to some recipients, the FSP benefit was equivalent to a cash anti-poverty program and could also affect infant health through other channels related to income.

Our approach is to utilize the timing of introduction of the modern Food Stamp Program during the 1960s and early 1970s. This roll-out occurred on a county-by-county basis, and was constrained by congressional funding authorizations. In particular, we utilize information on the month the FSP began operation in each of the roughly 2,600 U.S. counties. We focus on birth outcomes, using national data derived from the universe of birth certificates.[1] (Future work will expand our analysis to include mortality outcomes as well.) This study is not only a program evaluation of the impact of the FSP on birth outcomes, but since FSP benefits are treated as cash it also provides evidence on the impact of a change in income on birth outcomes.

There are three reasons to focus on birth outcomes. First, nutrition is thought to be one of the primary factors affecting birth outcomes, birth weight in particular (see, e.g., Kramer 1987a, Kramer 1987b). Second, the large number of records available with natality data -- more than a million observations per year -- together with information on both the county and month of birth allows us to utilize the discrete nature of the FSP roll out (see Section 5). Finally, the late 1960s and early 1970s were a period of major improvements in birth outcomes. Both infant mortality and fetal mortality fell by one-third from 1965 to 1975. And after increasing from 1955 to 1965, the likelihood of low weight birth (below 2,500 grams) fell 12% from 1965 to 1975. Prematurity also fell.[2] While previous work has addressed the especially large drop in deaths during the post-neonatal period for Blacks (Almond, Chay and Greenstone, 2006), drops in neonatal mortality (that is, deaths within the first month of life) and reductions in low birth weight for Whites and Blacks alike remain largely unexplained.

We find that newborn health improved promptly when the FSP was introduced. The FSP improved birth weight by about a half a percent for blacks and whites who participated in the program. The improvement was especially large among unmarried and non-white mothers. Impacts were largest at the bottom of the birth weight distribution, reducing the incidence of very low birth weight by 2 percent for whites and 6 percent for blacks. Changes in this part of the birthweight distribution are important as they are closely linked to other newborn health measures. Our results are robust to various sets of controls, such as county fixed effects, state by

[1] Prior to 1972, the data are derived from a 50% sample of birth certificates.

year fixed effect, and even county specific linear trends.

Our paper is organized as follows. Section 2 describes earlier work on the FSP, documents the improvement in infant health 1965-75, and discusses determinants of birth weight. Section 3 describes the history and parameters of the FSP, Section 4 the data, Section 5 the methodology, and Sections 6, 7, and 8 present our results. We conclude in Section 9.

2. Literature Review

Our work touches on three distinct literatures. First, we discuss what is known about the large improvement in infant health that occurred during the late 1960s and early 1970s. Second, we discuss previous studies of the impact of the FSP. Here we highlight two recent studies that use the initial roll out the FSP during the 1960s and 1970s. Finally, we discuss previous work on the etiology of low birth weight.

2.1 Improvements in Infant Health 1965-1975

Between 1965 and 1975, the U.S. infant mortality rate fell by over one-third. Health gains were especially large among blacks, whose infant morality rate fell by 40%.[3] Health care expenditures accounted for the largest share of the War on Poverty and Great Society programs (Davis & Shoen, 1987). Assessment of whether these programs caused the health improvement is complicated by the proliferation of federal programs during the late 1960s, including expanded Maternal and Child Health spending, along with advent of the Medicaid and Medicare programs.

Despite the large health improvement, few studies exist of its causes. Almond, Chay and Greenstone (2006) focused on the reduction of infant deaths among African Americans during the post-neonatal period -- defined as deaths between months 1 and 11. Declines in black post-neonatal mortality were especially large in the South, and driven by reductions in mortality from

[2] From 1968 to 1975 (we are in the process of entering gestation data prior to 1968).
[3] Mortality figures presented in this section come from regionally-aggregated data, described in section IIIa of Almond et al. (2006).

two specific causes: gastroenteritis and pneumonia. As hospitals were critical in preventing deaths from gastroenteritis and pneumonia, Almond et al. (2006) argued that expanded hospital access occasioned by desegregation of Southern hospitals had a large health benefit for black infants. In Mississippi, the timing of certification of hospitals for the Medicare program -- which required compliance with the Civil Rights Acts -- corresponded to the timing of local mortality improvements for blacks, but not for whites. While post-neonatal mortality may have responded to increased hospital access, it is less likely that the neonatal deaths (deaths within the first month of life) would have responded, particularly as the diffusion of life-saving neonatal technologies had yet to take place.[4]

In contrast to the post-neonatal improvement, the drop in neonatal and fetal mortality from 1965 to 1975 remains largely unexplained.[5] Neonatal mortality fell by one-third for both blacks and whites: 9 deaths per 1,000 births and 6 deaths respectively. Fetal mortality fell by a similar magnitude -- 41% (11 deaths per 1,000) for non-whites and 31% for whites (4 deaths per 1,000). As perinatal mortality is commonly linked to the health environment during pregnancy, it is plausible that improvements in prenatal nutrition may have been a factor. These mortality improvements aside, nutritional changes early in life are of interest as they may program" health at older ages, as described by the fetal origins hypothesis of Barker. Indeed, such long-term effects appear to exist for women born during the 1960s and giving birth in the 1980s and 1990s, as documented by Almond & Chay (2003). Understanding the source of improvements in early-

[4]Broadly speaking, post-neonatal deaths are more responsive to hospital access than neonatal deaths. The initial health at birth is generally much better among infants who die in the post-neonatal period than among infants dying in the first month of life. For example, while 72 percent of all neonatal deaths had a low birth weight (below 2500 grams), only 20 percent of all post-neonatal deaths were low birth weight infants (Starfield, 1985). Further, post-neonatal deaths tend to be caused by negative events after birth, most often by infectious diseases and accidents (Grossman & Jacobowitz 1981).

[5]For example, Corman & Grossman 1985 estimated the determinants of neonatal mortality in 1977, and extrapolated these estimates to the explain the rapid decline in neonatal mortality since 1964." For both Blacks and Whites, most of the mortality reduction was left unexplained (see Table 4). Similarly, Grossman & Jacobowitz (1981) found that most of the neonatal mortality reduction after 1964 was unexplained (see Table 3).

life health is therefore crucial.

2.2 Previous FSP Studies

The FSP is the most expansive of the U.S. food and nutrition programs. Eligibility requires satisfying income and asset tests, with little additional targeting to specific populations or family types.[6] The goal of the program is to improve nutrition among the low income population. As such, there have been many studies that examine the impact of the FSP on nutritional intake and availability, food consumption, food expenditures and food insecurity (see Currie 2003 and Fraker 1990 for reviews of the literature).

Almost all existing FSP studies use research designs that rely on comparisons of program participants to non-participants. This approach is subject to the usual criticisms regarding selection into the program. For example, a number of researchers (Currie 2003; Currie and Moretti 2007; Fraker 1990) have pointed out that if food stamp recipients are healthier, more motivated, or have better access to health care than other eligible women, comparisons between participants and non-participants could produce positive program estimates even if the true effect is zero. Conversely, if food stamp participants are more disadvantaged than other families, such comparisons may understate the program's impact. In fact, as reported in Currie (2003), several studies, including Basiotis et al. (1998) and Butler & Raymond (1996), actually find that food stamp participation leads to a reduction in nutritional intake. These unexpected results are almost certainly driven by negative selection into the program.

Many researchers who evaluate the impact of other government programs avoid these selection problems by comparing outcomes across individuals living in states with different levels of benefit generosity or other program parameters. There is a long literature on the effects of cash assistance programs, for example, which is based on this type of identification strategy

[6] Households that contain an elderly member have higher asset limits.

(Moffitt 1993; Blank 2002). Unfortunately, FSP is a federal program for which there is very little geographic variation in either eligibility criteria or benefit levels, so researchers have had to employ alternative approaches.

Aside from the issue of research design, it is noteworthy that few very of the many FSP studies examine the impact of the FS program on health outcomes. Currie and Cole (1991) examine the impact of the FSP on birth weight using sibling comparisons and instrumental variable methods and find no significant impacts of the FSP. Currie & Moretti (2007) also examine impacts of the FSP on infant outcomes; we describe their study in detail below.

2.2.1 Studies of FSP Roll Out

Two recent papers have utilized the early FSP roll out by county for identification of FSP impacts. Hoynes & Schanzenbach (2007) use the Panel Study of Income Dynamics (PSID) to evaluate the consumption impacts of the food vouchers provided by the FSP. Canonical microeconomic theory predicts that in-kind transfers have the same impact on spending as an equivalent cash transfer for consumers who are infra-marginal" (that is, who would spend more on the subsidized good than the face value of the in-kind transfer). Hoynes & Schanzenbach (2007) find using the same identification strategy employed in this study that recipients of Food Stamps behave as if the benefits were paid in cash. Although there are only limited consumption data available in the PSID, spending on food increases in response to the introduction of the FSP.

Currie & Moretti (2007) likewise used the county roll out in analyzing birth outcomes in California. As natality microdata are available for California beginning in 1960, the full history FSP roll out can be analyzed. (In contrast, the national natality microdata we use are available beginning in 1968, see Section 4.) They analyzed the occurrence of low birth weight at the individual level; controls included maternal age, parity, and gender of the child. Regressions were run separately for blacks and whites, and included county fixed effects, along with the interaction of maternal age and county effects.

Currie & Moretti (2007) noted that the expected effect of FSP exposure on low birth weight is ambiguous. To the extent that maternal access to FSP reduced fetal deaths, there could be a negative compositional effect on birth weight from improved survivability of marginal" fetuses. (Fetal deaths rates fell nearly 30% in California from 1965 to 1975.) In addition, selective migration of mothers to counties introducing an early FSP could also lead to a negative composition effect (and a measured reduction in birth weights). On the other hand, among pregnancies that would have generated a live birth in a given county in the absence of FSP, one would expect a positive effect on birth weight, to the extent that FSP actually increased the quantity and quality of food consumption (and baseline nutrition was poor). Currie & Moretti (2007) found that the former negative effect was stronger, particularly in Los Angeles county. FSP generated a small increase in the probability of low birth weight with FSP exposure, especially among teenagers having their first child. Moreover, they found that the number of births increased in Los Angeles which further suggests that in-migration or reduced fetal mortality may have reduced birth weights.

2.3 Birth Weight

There is a large literature on the etiology of low birth weight, which we very briefly summarize. As suggested by Kramer (1987a, 1987b), birth weight is usefully decomposed" in to: 1) that related to the gestation length (prematurity, or GL) and, 2) growth conditional on gestation length (intrauterine growth or IUG). Of the two, prematurity is thought to be more difficult to manipulate, though empirically more important than IUGR in affecting birth weight in developed countries (Kramer 1987a, 1987b). In contrast, maternal nutrition and cigarette smoking are the two most important, potentially modifiable determinants of IUGR (Kramer

1987a, 1987b).[7] Finally, there is evidence that birth weight is generally most responsive to nutritional changes affecting the third trimester of pregnancy.[8]

Rush et al (1980) set about to test the hypothesis that nutritional supplementation in the third trimester would improve birth outcomes. In their study population of African-Americans living in New York in the early 1970s, they randomly assigned a nutritional intervention that included increased calories, protein, and vitamins. They found no effects on gestation length, birth weight, or perinatal mortality. Nevertheless, effects were apparent *at age one*, when visual habituation, visual dishabituation, and the mean length of free play episodes" were improved. The authors speculated that the absence of perinatal effects may have been due to good baseline nutrition, attributable to widespread social and food programs introduced in the 1960s" (p. xvii).

2.3.1 Income and Birth Weight

A sizeable literature investigates the relationship between family income and birth weight and finds that mothers with higher incomes have heavier babies. Cramer (1995) finds that mothers with more income have higher birth weight babies, and as a result differences in income can explain part of the black-white difference in birth weights. Kehrer and Wolin (1979) find evidence that the Gary Income Maintenance Experiment improved birth weights, though the sample sizes are small and some of the estimates are imprecise. Currie and Cole (1993) document a negative cross-sectional relationship between AFDC (welfare) receipt and birth weight, but find that increased income from AFDC payments causes improvements in birth weight outcomes when unobservable characteristics are taken into account through the use of instrumental variables or mother-specific fixed effects.

In general the literature has been plagued by imprecise estimates due to small sample

[7] Other public health interventions [besides reduced smoking] likely to improve intrauterine growth in developed countries include increases in caloric intake, especially in those settings where nutrition may be suboptimal for a substantial minority of the population" (p. 509).

sizes as well as a lack of well-identified sources of variation in income. As a result, this paper provides some of the best evidence to date on the impact of income on birth weight outcomes.

To fix ideas, we are working with a model in mind as follows:

$$birthweight_i = f(GL_i, IUG_i)$$

According to the medical literature, GL is a function of factors unlikely to be modified by FSP introduction, such as maternal characteristics such as prior birth history, age, education, exposure to toxins, cigarette smoking and stress levels (although the latter two factors could certainly be impacted by FSP). On the other hand, IUG is a function of both pre-determined characteristics -- such as maternal health stock at the time of conception, race, parity and sex of the fetus – and characteristics likely to be directly impacted by the introduction of FSP. For example, low caloric intake during pregnancy is a major determinant of birth weight, and has importance about twice the magnitude as the black-white birth weight gap (Kramer 1987b). FSP benefits may work through other channels as well, though, for instance reducing stress experienced by the mother which itself has a direct impact on birth weight. It is also worth noting that even though recipients cannot purchase cigarettes directly with FSP benefits, nontheless because resources to the household increase benefits may increase cigarette consumption, which would work to reduce birth weight. Below we separately test for FSP impacts on length of gestation and birth weight.

In sum, the literature suggests that retardation of IUG, manifested as reduced birth weight, may be more responsive to FSP exposure than gestation length, and that this response is largest when nutrition improves in the third trimester. As a result, in our main specifications we will look for program impacts when the FSP is introduced in the third trimester. The literature also suggests that nutritional interventions may have larger effects when baseline nutrition is

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[8]See the literature review of Rush et al. (1980). The cohort exposed to the Dutch Famine in the third trimester had

especially poor. Finally, it is important to analyze additional health measures in addition to birth weight (and gestation length):

> A final reminder concerns the need for future research to keep sight of the truly important outcomes of infant and child mortality, morbidity, and functional performance. After all, birth weight and gestational age are important only insofar as they affect these outcomes. *Kramer (1987), p. 510*

3. Introduction of the Food Stamp Program

The modern Food Stamp Program began with President Kennedy's 1961 announcement of a pilot food stamp program that was to be established in eight impoverished counties. The pilot programs were later expanded to 43 counties in 1962 and 1963. The success with these pilot programs led to the Food Stamp Act of 1964 (FSA), which gave local areas the authority to start up the Food Stamp Program (FSP) in their county. As with the current FSP, the program was federally funded and benefits were redeemable at approved retail food stores. In the period following the passage of the FSA, there was a steady stream of counties initiating food stamp programs and Federal spending on the FSP more than doubled between 1967 and 1969 (from $115 million to $250 million). Support for requiring food stamp programs grew due to a national spotlight on hunger Berry (1984). This interest culminated in passage of 1973 Amendments to the Food Stamp Act, which mandated that all counties offer FSP by 1975.

3.1 The Commodity Distribution Program

The roll out of the FSP was pitted against direct distribution of surplus farm goods through the Commodity Distribution Program (CDP). The CDP took surplus food purchased by the Federal government as part of the price support policy, and distributed those goods to the poor. In 1960, there were only 5 commodities included in the CDP, but by 1967 the list of goods had grown to 15, and in 1968 it grew to its maximum at 22. It was argued that the CDP was

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lower average birth weight than cohorts exposed earlier in pregnancy (Painter et al., 2005).

inadequate because of the limited range of products and infrequent timing of the distribution of goods. More discussion about the CDP is available in Hoynes & Schanzenbach (2007).

Debate about moving from the CDP to the FSP pitted powerful agricultural interests against advocates for the poor (MacDonald 1977; Berry 1984). In fact, as described in Berry (1984) and Ripley (1969), passage of the 1964 Food Stamp Act was achieved through classic legislative logrolling. The farm interest coalition (Southern Democrats, Republicans) wanted to pass an important cotton-wheat subsidy bill while advocates for the poor (Northern Democrats) wanted to pass the FSA. Neither had majorities, yet they made an arrangement", supported each others bills, and both bills passed.

This political history is important because it illustrates that there was significant heterogeneity across the country in support for the FSP. Remember that the 1964 Act allowed for counties to voluntarily set up the FSP. In addition, the Act stated that no county could run both the FSP and the CDP (although in practice it seems that many counties did have both at the same time for a period.) The political economy of the time suggests that counties with strong support for farming interests may have preferred to administer the CDP instead of the FSP, and may be late adopters of the FSP. On the other hand, counties with strong support for the low income population may adopt FSP earlier in the period. Consequently, the food stamp program introduction may not be completely exogenous. We address this in further detail below.

In the early days, the FSP was structured so that participants had to purchase a given amount of stamps at a discounted rate. The total face value of food stamps that a family was able to purchase depended on family size, and the price charged for them varied by family income. For example, in 1970 a family of four could purchase $106 worth of food stamps each month for a price between 2 percent and 77 percent of their face value, depending on the family's income. The stamps were about the size of regular dollar bills and could be used to purchase food at retail food outlets, and at least by 1970 stores were allowed to give back change from purchases in cash if it was less than 50 cents.

Figure 1 summarizes the overall pattern of FSP introduction. In particular, the figure plots the percent of counties offering FSP, where the counties are weighted by their 1970 population. Note this is NOT the food stamp caseload, but represents the percent of the national population that lived in an area offering a FSP. The figure shows that there was a long ramp up period between 1964 and 1975, leading to the eventual universal coverage of the FSP. For example in 1968 about half of the population lived in counties with FSP and by 1972 this rose to over 80 percent. It is this ramp up period that forms the basis of our research design.

There was substantial geographic heterogeneity in timing of adoption of the FSP, as shown in Figure 2. In the figure, the shading of the counties is assigned by county FSP start up date-with darker shading denoting a later start up date. This shows a great deal of variation in FSP introduction within and across states. Our basic identification strategy uses this county level variation in food stamp "treatment."

As discussed above, the 1964 FSA allowed counties to start FSP-but participation was voluntary. Therefore, for our research design to be valid, we need for the assignment of county start up of FSP to be exogenous. The discussion above suggests that northern, urban counties with large poor populations were more likely to adopt food stamp programs earlier while southern, rural counties and those with strong agricultural interests adopted FSP later. This systematic variation in food stamp adoption could lead to spurious estimates of the program impact if those same county characteristics are associated with differential trends in the outcome variables.

3.2 The FSP Roll Out

To explore this relationship we compiled characteristics of counties in 1960, on the eve of the first food stamp pilot programs. We use these "pre" characteristics to predict the date that the county adopted a food stamp program. The data on county characteristics come from the 1960 City and County Data Book, which is based on data from the 1960 Census and the Census of Agriculture. The dependent variable is the month and year of the county's food stamp start date-expressed as an index equal to 1 in January 1961, 2 in February 1961, and so on. In some specifications, we omit from the analysis the initial pilot counties as they were chosen by a different process than the later counties. In those cases, the dependent variable therefore ranges from 25 (January 1963) to 175 (July 1975). The independent variables include the percent of the population that lives in an urban area, is black, is less than 5, is 65 or over, has income less than $3,000 (1959$), the percent of land in the county that is farmland, and population level.

The results are presented in Table 1. We present estimates with (columns 2-4) and without (column 1) state fixed effects. All regressions are weighted by the county population. Focusing on the results with state fixed effects, we find that counties with larger urban, black, and low income populations implement FSPs earlier. Further, those with a larger share of the population that is very young or old also implement earlier, and counties with larger population sizes implement earlier. In contrast, counties where more of the land is used in farming implement later. In the final column we allow the impacts to differ within counties in the South. In general, the impacts of county characteristics are smaller (in absolute value) in counties the South.

While these regression results show statistically significant impacts of these county characteristics on the timing of food stamp implementation, overall most of the variation remains unexplained. To illustrate this, Figure 3 provides scatter plots of each of the six county characteristics against the county implementation date. In each panel in the figure, the 1960 county characteristic is on the x-axis and the food stamp start date is on the y-axis. For guidance, we also provided the univariate linear regression line for each panel. The county observations and regression are weighted by the county population. These figures show that the association between the county characteristics and the food stamp start date is qualitatively not very strong and there is an enormous amount of variation that is not explained by the characteristics. This is consistent with the characterization of funding limits controlling the movement of counties off the waiting list to start up their FSP: The program was quite in demand, as congressmen wanted to reap the good will and publicity that accompanied the opening of a new project. At this time there was always a long waiting list of counties that wanted to join the program. Only funding controlled the growth of the program as it expanded" (Berry 1984, pp. 36-37).

We view the weakness of this model fit as a strength when it comes to our identification approach-in that much of the variation in the implementation of FSP appears to be idiosyncratic. Nonetheless, in order to control for possible differences in trends across counties that is spuriously correlated with the county treatment effect, all of our regressions include interactions of these 1960 pre-treatment county characteristics with time trends as in Acemoglu, Autor and Lyle (2004) and Hoynes & Schanzenbach (2007).

This period of FSP introduction took place as part of the much larger federal “war on poverty.” Another source of bias may be the introduction or expansion during this period of WIC, Medicaid, AFDC, and Head Start. If these programs are mainly varying at the state level then our controls for state by year fixed effects should absorb these program impacts. But if there is substantial variation across counties that is correlated with the introduction of the FSP in that county, state-level controls will not be sufficient. A strong correlation at the county level between increases in spending on other income support programs and the introduction of the FSP would suggest that other factors besides FSP might be driving any observed changes in infant health. Figure 4 shows the relationship between FSP initiation and the change in real per-capita spending on county-level transfer programs from 1969 to 1977. As in Figure 3, the univariate regression line is included, and counties are weighted by population in 1960. These figures show that there is very little relationship between FSP adoption and changes in spending on other social welfare programs as measured by spending on AFDC, Medicaid, Medicare, or retirement and disability programs. To be sure, however, our models include controls for per capita real county transfers.

It is important to note that our data in Figure 3 do not include spending on other nutrition programs such as WIC, the Special Supplemental Food Program for Women, Infants and Children. This is because WIC was introduced near the end of the time period studied here. WIC was first discussed in the conference report of the 1969 White House Conference on Food, Nutrition and Health. The program was conceived in response to the perception that low-income mothers and children suffered from malnutrition that led to various ailments and high levels of post-neonatal mortality (Citizen's Board, 1968).[9] Following this attention, WIC began as a pilot program in 1974, and was made permanent late in 1975. WIC was designed to supplement - but not replace - food stamps, and benefits were made available to pregnant women and children up to age 5 in families with incomes up to the cutoff for free and reduced-price school lunches. The introduction of WIC is further proof that there was a perceived need at this time for nutritional assistance for low-income pregnant women and their children. Since the program was rolled out over a short period of time from 1974-75, there is little concern that the introduction of WIC pollutes our estimates of the introduction of FSP. Nonetheless, we include a specification check that includes only years prior to 1974 that will not be impacted by the introduction of WIC.

4. Data

[9] In the report *Hunger, U.S.A.*, there were graphic descriptions of nutritional deficiencies among pregnant women, including those in the lowest socioeconomic group [that] show a high incidence of clay or starch eating, from 40 to 50 percent in the Negroes and lower numbers in white women".

The data for our analysis is combined from several sources. The core data are two microdatasets from the National Center for Health Statistics. This data is merged with county level data from several sources. The key treatment or policy variable is the month and year that each county implemented a food stamp program. The data on county level food stamp start dates comes from USDA annual reports on county food stamp caseloads (USDA, various years).

We evaluate impacts of FSP initiation on infant health using two microdatasets. First, we utilize the natality microdata from National Center for Health Statistics. These data are derived from the universe of birth certificates, and are available beginning in 1968.[10] Reported birth outcomes include gender, plurality, birth weight, and gestation length. Data on the month and county of birth permit linkage of natality outcomes to the month the FSP was introduced in a given county. There are also limited demographic variables available in the natality data. In particular, the age and race of the mother is available for the full sample. There is also data on the education and marital status of the mother, although this is not available in all state-years. Appendix Table 1 provides information on the availability of these variables over time.

Second, we utilize the microdata on deaths from the National Center on Health Statistics. These data are derived from death certificates and are available beginning in 1959. These data report the age and race of the decedent, the cause of death, and the month and county of death.

We supplement the above with controls drawn from two sources. First, county characteristics are available in the 1960 City and County Data Book, which is based on data from the 1960 Census of Population and Census of Agriculture. These data are used to measure county pre-treatment variables for use as potential determinants of the timing of county FSP adoption. In particular, we construct the percent of the 1960 population that lives in an urban area, is black, is

[10] Prior to 1968, no national microdata on births are available. Instead, aggregated counts at the county-level are available annually in print format from NCHS at http://www.cdc.gov/nchs/products/pubs/pubd/vsus/1963/1963.htm. We have had these data hand-entered for 1959-1967. Future versions of the paper will use this data to extend our analysis to the inception of FSP pilot programs in 1961.

less than 5, is 65 or over, has income less than $3,000 (1959$), the percent of land in the county that is farmland, and log of the county population.

Second, we use data prepared by the Bureau of Economic Analysis, Regional Economic Information System (REIS). This data is used to construct annual, county-level real per capita measures of transfer payments, including cash public assistance benefits (Aid to Families with Dependent Children AFDC, Supplemental Security Income SSI, and General Assistance), medical spending (Medicare and Military health care), and cash retirement and disability payments.[11] Finally, we also use the REIS to construct an annual measure for real county per capita income.

5. Methodology

We estimate the impact of the introduction of the FSP on county-level birth outcomes, separately by mother's race. Specifically, we estimate the following model:

$$(1) \qquad Y_{ct} = \alpha + \delta FSP_{ct} + \beta CB60_c * t + \eta_c + \delta_t + \mu_{st} + \varepsilon_{ct}$$

on a dataset of county-by-quarter outcomes. Here Y_{ct} is a measure of infant health for all births to residents of county *c* at time *t*. FSP_{ct} is equal to one if the FSP was available in county *c* at the beginning of the quarter prior to birth, to proxy for beginning of the third trimester.[12] We assign the treatment at the beginning of the third trimester following the evidence that this period is the most important for determining birth weight. In some results, however, we explore the sensitivity to changing the timing of the FSP treatment.

[11] Beginning in 1969, the REIS data permitted more detailed categories for tabulating government transfers. Some of these categories are shown in Figure 4--which was possible because of the limitation to 1969-1977. However, because the natality data begins in 1968 and the mortality data begins in 1959, we have adopted these less detailed categories. In analyses of the data limited to 1969+, the results are robust to adding more detailed categories.

[12] To be precise, because we collapse the data to the county-by-quarter level the FSP variable can sometimes equal something other than a 0 or 1. The natality data is available at the monthly level and we use that to assign FSP status as of 3 months prior to birth (beginning of the third trimester). When the data is collapsed to the county-quarter this policy variable is averaged among the 3-months of observations in that cell. Therefore the policy variable ranges from 0 to 1, with most values at 0 or 1. The remaining values are concentrated at 1/3 and 2/3 (but do not exactly equal 1/3 and 2/3 because the quarterly means are calculated by weighting by the number of births in each month.

The vector X_{ct} contains the annual county-level controls from the REIS. In particular, it includes real, per-capita transfer spending on other government transfer programs (cash public assistance benefits, medical care, and retirement and disability payments) which are included to control for other expansions in Great Society programs that occurred during this time period. X_{ct} also includes the log of real annual county per capita income to control for any coincident expansions in labor market opportunites or other factors at the county level.

$CB60_c$ are 1960 county characteristics, as described in Section 3.2, interacted with a linear time trend, to control for observable determinants of the timing of a county's FSP adoption. Also included are county fixed effects η_c and time fixed effects λ_t. We estimate additional specifications with state linear time trends or state-year fixed effects captured by μ_{st}, and in some models also include county*year fixed effects.

We consider several outcome variables in our main specifications. First, using the natality data we consider several variables capturing the health of the infant at birth. This includes: continuous mean birth weight in grams, the fraction low birth weight (less than 2500 grams, or about 5.5 pounds), and the fraction very low birth weight (less than 1500 grams, or about 3.3 pounds). Following the literature, we use low birth weight and very low birth weight to capture the apparent nonlinear effects in those ranges (see, e.g. ACL 2005, BDS 2007). Further, we also include the fraction of births that have a gestation below 37 weeks (considered pre-term) and the fraction of births that are female. These measures are means within county-quarters.

Second, in future work using the mortality data, we will construct the neonatal and post-neonatal death rates. Neonatal deaths are those occurring in the first 28 days of life, and post-neonatal deaths are those occurring between 1 and 11 months. We will form cohort mortality *rates* by dividing the number of deaths by the number of births where we obtain the number of births from the natality data. We define cohort mortality rates in the usual way--the total number of deaths among those born in *t* divided by the number of births in *t*. We can construct the month of birth using the information in the mortality data (month of death and age at death). We present death rates for all causes, and also deaths by cause of death where we attempt to separate deaths unlikely to be impacted by nutritional deficiencies to those not likely to be impacted by nutritional deficiencies.

All estimates are weighted using the number of births in the county-quarter as weights and the standard errors are clustered on county. Further, to protect against estimation problems associated with thinness in the data, we drop all county-quarter cells where there are fewer than 25 births. Results are qualitatively similar if the minimum number of births is changed to 15 or 10.

6. Results for Natality

Our main results are reported in Table 2A. For each outcome, we report estimates from six specifications with different controls. Column (1) omits trend variables, and includes only the parsimonious specification of REIS county-level per-capita transfers, year*quarter fixed effects and county fixed effects. Column (2) adds 1960 county characteristics interacted with linear time, then column (3) adds county per-capita real income. Columns (4) and (5) add state*linear time and state*year fixed effects respectively. Finally, column (6) adds county specific linear time trends. The top panel of the table reports results for white mothers and the bottom panel reports results for black mothers. In this and all subsequent tables, the number of observations refers to county-quarter cells. Further, the line labeled “subsample population” shows the fraction of the underlying micro observations (by race) that are used in this regression. This number is 0.98 for whites and 0.94 for blacks (in this table), which reflects the fact cells with less than 25 births are dropped from the analysis.

The first six columns in Panel A report the impact of having FSP in place during the third trimester of pregnancy on birth weight for births to white women. These columns indicate a small statistically significant increase in birth weight for whites caused by exposure to FSP during the third trimester. As more controls are added and we move across columns, the impacts typically get larger and more precisely estimated. When the estimated coefficient is divided by mean birth weight, the resulting effect size is a 0.06 to 0.08 percent increase in birth weight (this is labeled in this and subsequent tables as "% impact (coeff/mean)"). As shown in Panel B, the impact of FSP exposure on birth weight is 50-150 percent larger for blacks than whites. That, combined with a smaller average birth weight for blacks, implies an impact between 0.1 and 0.2 percent on blacks (about twice the impact on whites).

Only a subset of women who give birth are likely to be affected by FSP. While the coefficients reported above are valid estimates of the population impact of FSP, we may also want to know the implied impact among FSP recipients (i.e. the effect of the treatment on the treated). To calculate the implied impact on those who take up the FSP, we need an estimate of the participation rate of FSP benefits among women giving birth. Unfortunately we do not have information about food stamp participation in the natality data, nor do we have very good proxies for participation. Instead, we calculate FSP participation rates for groups "similar" to women giving birth. Specifically, we estimate the participation rate for all women with a young child living in the house. Participation rates look very similar whether we define the presence of a "young child" as any child below age 1, 3 or 5. The participation rates are calculated from the Current Population Survey (CPS), which first started asking about FSP participation in 1980. The estimated takeup rate for women with young children (under age 5) is 0.13 for whites and 0.41 for blacks. We can inflate the estimated effect by these participation rates for an estimate of treatment on the treated. The results indicate that the impact of FSP on participants' birth weight (labeled "Impact, inflated") is between 14 and 21 grams for whites and 10 to 13 grams for blacks. Expressed as a percent of mean birth weight (labeled "% impact, inflated") is between 0.5 and 0.6 percent for whites, and between 0.3 and 0.4 for blacks.

The results for birth weight (and the other outcomes described below) are very robust to adding more controls to the model. Taking the baseline specification as the one including county characteristics interacted with linear time (column 2), incrementally adding controls for county per capita income (column 3) and state*linear time (column 4) or state*year fixed effects (column 5) make little change to the underlying estimates. We view the specification with state*year fixed effects as very encouraging--as we have controlled for a whole host of possibly contemporaneous changes to labor markets, government programs and other things that vary at the state-year level. Finally, we also find the results robust to adding county linear time trends (column 6). For the remainder of the tables, we adopt specification (5) with state*year fixed effects as our base case specification. Results (not presented here) are the same if log of birth weight is used as the dependent variable instead.

Columns (7) through (12) repeat the exercise, this time with fraction low birth weight (less than 2500 grams) as the dependent variable. Exposure to FSP reduces low birth weight by a statistically significant 1 percent for whites (7 percent when inflated by takeup rate), and a less precisely estimated 1 to 1.5 percent for blacks. We further explore the reduction in probability being born under various weight thresholds below in Figure 5.

In order to gauge the magnitude of these effects, it is useful to compare the estimated magnitudes to those implied by the previous literature. Cramer (1995) finds that a 1 percent change in the income-to-poverty ratio leads to a 1.05 gram increase in mean birthweight. The Hoynes and Schanzenbach (2007) estimates of the magnitude of food stamp benefits are $1200 annually for participants. Scaling those to match the units available in the literature (and treating benefits as their face-value cash-equivalent) implies that food stamps increased the family income-to-poverty ratio of participants by 15 percent. The implied treatment-on-treated effect would therefore be approximately 16 grams, which is quite similar to the effects found in columns (1) - (6).[13]

To further investigate the impact of the FSP on the distribution of birth weight, we present the percent impact on the probability that birth weight is less than various cut points (not inflated by program participation). We find larger percentage impacts of FSP on birth weights between 1500 and 2500 grams, and smaller impacts further up the birth weight distribution. Figure 5A displays results and confidence intervals for whites. We find that the largest reduction in probability of birth weight below a certain threshold comes at very low thresholds of 1500 and 2000 grams. The impacts become gradually smaller as the birth weight threshold is increased to 2500 grams and above, until there is no difference for births below 3750 grams. Results are larger for blacks (Figure 5B), showing a six percent decrease in the probability of a birth less than 1500 grams, and an impact that declines at higher birth weights.

Table 2B presents estimates of the same specifications for two additional outcome variables: the fraction of births that have gestation length less than 37 weeks (considered pre-term births) and the fraction of births that are female. These results show that FSP leads to a small but detectable decrease in pre-term births for whites; with statistically insignificant impacts for blacks. We find that the introduction of the FSP leads to a decrease in the fraction of births that are female. While small and statistically insignificant, this is at least consistent with other recent work that finds that nutritional deprivation leads to a sex ratio imbalance favoring girls (because more male fetuses die in utero under such circumstances).

One limitation of these results is that micro-data on births by county is only available starting in 1968. By that point, almost half of the population was already covered by the FSP.

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[13]Assuming that birth weights are normally distributed, impacts of this implied magnitude would reduce the

To take advantage of the earlier program variation, we are able to push back the low birth weight results to 1959, but we can only measure outcomes at the state level. To construct state-level FSP treatment, we calculate the fraction of population in the state living in counties covered by FSP at time *t*. Results are displayed in Appendix Table 2. To illustrate how much important variation is lost using this state-level measure, we first replicate the main results from Table 2 using data from 1968 forward but collapsing it up to the state level. The results show positive but imprecise effects of FSP measured with this noisy treatment variable. Pushing the data back to 1959, we get qualitatively similar but imprecise effects of the FSP.

Most evidence suggests that nutrition has its greatest impact on birth weight during the third trimester. To explore the sensitivity of our results to the timing of the FSP introduction vis-à-vis the birth, Table 3 shows various reclassifications of the timing of exposure to FSP. We adopt the specification from column (5) of Table 2 for all columns, that is we control for 1960 county characteristics*linear time, per-capita transfer program spending, per-capita real income, year*quarter fixed effects, county fixed effects and state*year fixed effects. The baseline specification -- reprinted from Table 2 -- assigns the policy introduction as of the beginning of the quarter prior to birth, to proxy for beginning of the third trimester. Columns (2) and (3) move the policy treatment variable back to FSP's introduction two and three quarters before birth, respectively. Moving the treatment from third to second trimester reduces the impact of FSP substantially, though there is still a statistically significant impact on birth weight for blacks only. Furthermore, treatment at the beginning of the first trimester yields even smaller and statistically significant impacts. Similar results are found for fraction low and very-low birth weight. Finally, in columns (4) and (5) we include FSP during the third trimester as well as during either the second or first trimester. These results show that all of the action is through 3rd

incidence of low birthweight by 5 percent, and reduce the incidence of very low birth weight by 7 percent.

trimester exposure. We view these results as very compelling and important. First, they are consistent with the epidemiological evidence on the importance of 3rd trimester nutrition. Further, however, these results provide evidence that our model is not simply capturing a spurious correlation between FSP introduction and trends in infant outcomes at the county level. The reduction in the magnitude of the birth weight impact is consistent with results in Currie & Moretti (2007). Their study of birth outcomes in California assigned the FSP treatment nine months prior to birth, and found comparatively limited impacts on birth weight.

Next we test for spurious trending in the county birth outcomes that might be loading on to FSP. Our first approach, shown in Table 4, is to include a one-year lead of the policy variables for each of the outcome variables presented in Table 2. There is no impact of the policy lead and the results for the main policy variable are qualitatively unchanged. We also employ an event study analysis, described below.

7. Event Study

The pattern of estimates in Table 3 suggests that δ in equation (1) is identified by the discrete jump in FSP at implementation and its impact on birth weight. In particular, we showed in Table 3 that as the timing of the treatment is shifted earlier in pregnancy, the estimated FSP effect on birth weight decreased substantially in magnitude. If instead identification were coming from comparisons in a counties' FSP status away from the FSP start month, then we would expect less sensitivity in the Table 3 results to the trimester to which the FSP treatment is assigned. However, there remains a concern that our results are driven by trends in county birth outcomes that are correlated with FSP implementation in a way that count linear trends do not capture.

This proposition can be evaluated more directly in an event study study analysis. Specifically, we fit the following equation:

$$(2) \qquad Y_{ct} = \alpha + \sum_{i=-6}^{8} \pi_i 1(\tau_{ct} = i) + \eta_c + \delta_t + \mu_{st} + \phi_c * t + \varepsilon_{ct}$$

where τ_{ct} denotes the event quarter, normalized for each county so that $\tau = -1$ for births that occur in the same quarter as the FSP began operation in that county, $\tau = 0$ for births one quarter after the FSP began operation, and so on. For $\tau \le -2$, pregnancies were untreated by a local program. This specification includes fixed effects for county, time, and county specific linear time trends.

In order to eliminate compositional effects on π_i, we restrict the sample to counties having births for all 15 event quarters (a balanced panel): 6 quarters before implementation and 8 quarters after. As our natality data begins with January 1968, this means we exclude all counties with a FSP before July 1969

7.1 African-American Mothers

Figure 6 plots the event-quarter coefficients from estimating equation (2) for Blacks. Panel (a) reports estimates when the dependent variable is birth weight. There is a clear and sharp break in the trend for births occurring after FSP had been implemented during the third trimester of pregnancy. That is, pregnancies exposed to a local FSP resulted in higher birth weights than pregnancies conceived slightly earlier and just “unexposed”. As births right around $\tau = 0$ were conceived prior to FSP implementation, the likelihood that selection into childbearing accounts for the slope increase would seem remote. That such a <u>prompt</u> increase in birth weight observed with FSP inception indicates that potential confounders would have to mimic the timing of FSP roll out extremely closely. Consequently we view this pattern as extremely compelling evidence of a casual impact of FSP on infant health.

Similar patterns are observed when the dependent variable is the share of births below 2,500 grams (panel (b) of Figure 6) and below 1,500 grams (available upon request). There is a sharp decline in the likelihood of low weight birth around FSP implementation.

7.2 White Mothers

Figure 7 presents the analogous graphs for whites. Again, there is an increase in birth weights for births occurring soon after FSP implementation (panel (a)), although the pattern is noisy. For birth weight below 2,500 grams, there is a sustained decrease following implementation (panel (b)).

8. Robustness Checks

Table 5 breaks out the results by demographic group, to see whether the results are consistent with finding larger impacts on groups - such as single mothers and those with less education - that are more likely to be impacted by FSP. In particular, we present results by age of mother (less than 24, 24 and older), education of mother, (high school education or less, more than a high school education), and for the subsample of women where the father is not present.[14] While age of mother is available in all states and years, as discussed above, information about education and the legitimacy of the birth is more limited. The results in Table 4 include the full sample of all state-years that report the demographics.[15] Overall, the results show that the impacts are larger for older mothers (age 25 and over) and more educated mothers. While this is surprising, few of the age results and none of the education results are statistically significant. The strongest and most consistent results in the table show that black women with no father present experience much larger impacts than all black women. This is consistent with the high participation rates among this group (0.70 compared to 0.50 for all blacks).

There are relatively few individual-level demographic characteristics that we can use to test whether the impact is larger among groups more likely to be treated. As a result, we have to rely on county-level characteristics for further sensitivity tests. In Table 6, we break counties into

[14] Specifically, we code the father as missing if either the birth certificate indicates that the birth is illegitimate OR the variable for father's age is missing.

[15] It is informative to point out that as we stratify the samples further, our drop rule (we drop the cell if it contains less than 25 births) results in more observations lost. Further, many cells are also dropped because of missing data on education or father's presence. For example, the results for education show that the sample consists of 59 percent of micro observations for whites (0.44 + 0.15) and 58 percent of micro observations for blacks (0.52 + 0.06).

quartiles of poverty as measured in the 1960 Census. The impact of FSP on birth weight and percent low and very-low birth weight bounces around a bit, but is typically largest for the counties in the bottom quartile of poverty as well as for the top quartile of poverty.

Table 7 breaks out the impact by parity of birth. Overall, birth weights among firstborns are markedly lower (about 75 grams lower for both blacks and whites), and the impact of FSP is larger on firstborns. Fractions low and very-low birth weight are more similar by parity, and FSP seems to have similar impacts on these measures by birth parity. This is somewhat surprising to us, as we expected that knowledge about food stamps (and therefore take up) would be higher for a second or later birth as many public assistance benefits are limited to families with children.

There is some suggestion in the historical documents that the impact might be different across geographic regions, or might differ by race across regions. In particular, the ramp-up of the program (after the county's initial adoption of FSP) was probably more rapid in urban counties. In addition, the speed of the ramp-up and the program parameters differed somewhat between North and South so that the impact might differ across regions. Barriers to accessing food stamps might have also differed between North and South, and may have interacted with race. Table 8 shows that the impact of FSP is larger and more statistically significant for both blacks and whites in urban counties. Interestingly, blacks appear to have larger effects outside the South, while whites appear to have larger effects in the south. The impacts by South/NonSouth, however, are less precisely estimated than the results by urban/nonurban.[16]

The main results are robust to many additional specification checks. One concern about the impacts of FSP on birth outcomes is that there are other programs that are being expanded at the same time, and so the effects we pick up are not the result of the FSP but may be driven by other changes. In results not shown (but available upon request), we find some evidence that FSP

[16] We define the county as urban if more than 50 percent of the 1960 population in the county lives in an urban area.

is not simply picking up the effect of other programs because the results are little changed whether we include or omit other county per capita transfer spending . Another way to check whether FSP is coincident with other health improvements, such as the expansion of access to hospitals in the South (Almond et al. 2006), is to test whether FSP impacts whether the birth was in a hospital and/or was attended by a physician. Table 9 displays these tests. The impact of FSP on the fraction born in a hospital is small and not significant for whites, and small, insignificant and wrong-signed for blacks. Effects are also small and insignificant for percent born in a hospital or with a physician attending. Although there is no evidence that FSP impacts these outcomes, note that the mean of the dependent variable approaches unity for both blacks and whites.

Finally, we investigate whether FSP is associated with higher fertility in Table 10. If children are a normal good, a program that increases household income might also increase the number of children. Further, this effect may lead to a negative composition bias as we would expect fertility to increase disproportionately among the disadvantaged (who have higher FSP exposure and worse birth outcomes). We assign the FSP policy treatment to be as of 3 quarters prior to the birth quarter, to proxy for the conception date. The dependent variable is number of births in the county-quarter divided by number of women aged 25-44, and the regressions are weighted by the population of women in each cell. We find a small, positive, statistically insignificant effect of FSP on births. When this is scaled up by the participation rate, the treatment on the treated is about 1 percent for whites and 2 percent for blacks. Overall, there is evidence only for very small and statistically insignificant impacts on fertility.

9. Conclusion

The uniformity of the Food Stamp Program was designed to buffer the discretion States exercised in setting rules and benefit levels of other anti-poverty programs. This uniformity was deliberately preserved through the major reforms to welfare under the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (Currie, 2003). An unintended consequence of this regularity has been to circumscribe the policy variation typically used by researchers to identify program impacts. As a result, surprisingly little is known about FSP effects. Although FSP benefits are paid in vouchers that themselves can only be used to purchase food, because recipients can reduce their out-of-pocket spending on food somewhat, the benefits are essentially a cash transfer.

We analyze the inception of the FSP at a time of widespread poverty and nutritional risk. A 1968 report on hunger in the United States found concrete evidence of chronic hunger and malnutrition in every part of the United States where we have conducted hearings or conducted field trips" (Citizens' Board, 1968). 14 million Americans were going hungry (Citizens' Board, 1968) and more than 1,000 starved to death each year.[17]

Although not targeted at pregnant women (or even families with children) we nevertheless find substantial impacts on birth outcomes. These impacts were larger in populations at greater nutritional risk -- among Black infants and where no father was present at delivery. Consistent with epidemiological studies of maternal nutrition, FSP availability for the third trimester had a larger birth weight impact. Moreover, the shift in birth weights was larger at the low end of the birth weight distribution, where additions to birth weight are more closely tied to other health outcomes. Further, we find some evidence of increased gestation length (reduced prematurity) resulting from FSP availability. We conclude the FSP yielded important -- and previously undocumented -- health benefits. Future work will extend the analysis to the first FSP pilot projects in 1961 and expand the outcome measures to include mortality.

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[17] In 1967, there were 1,274 decedents for whom the cause of death was listed as malnutrition, unqualified," ICD-7 Code=286.5 (authors' tabulation of mortality microdata).

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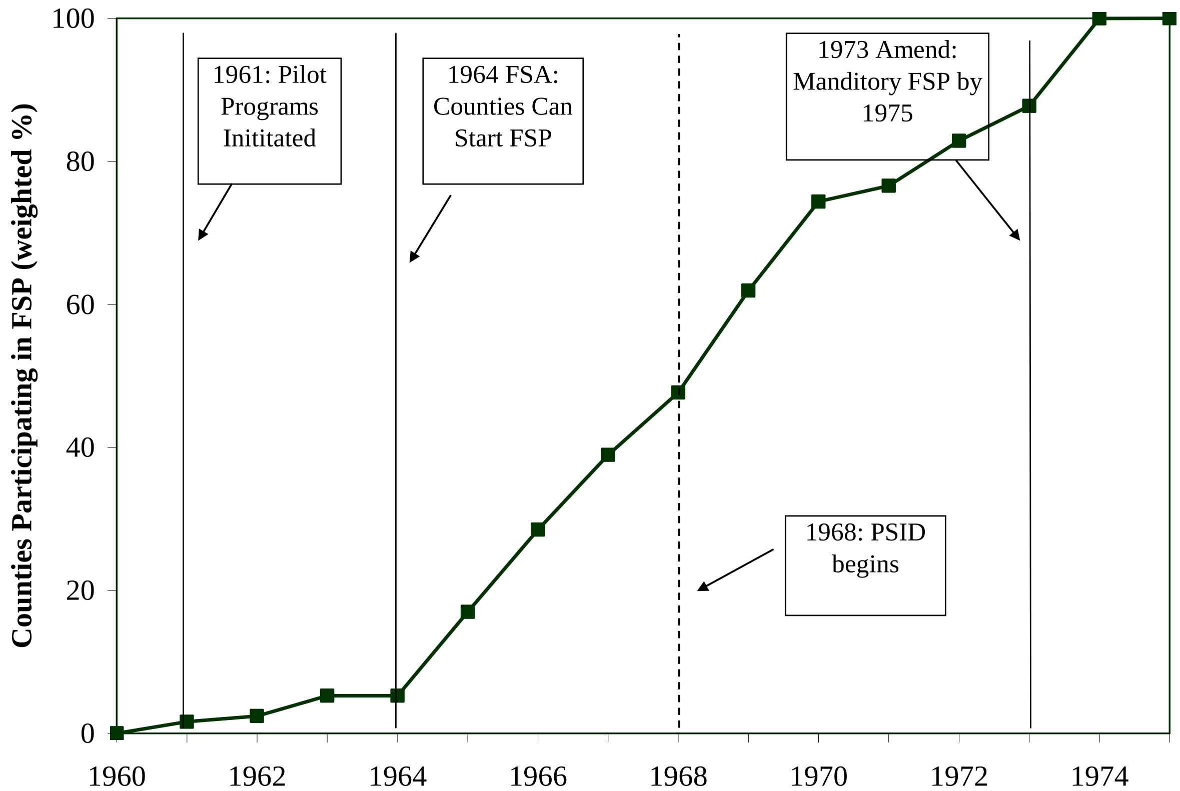
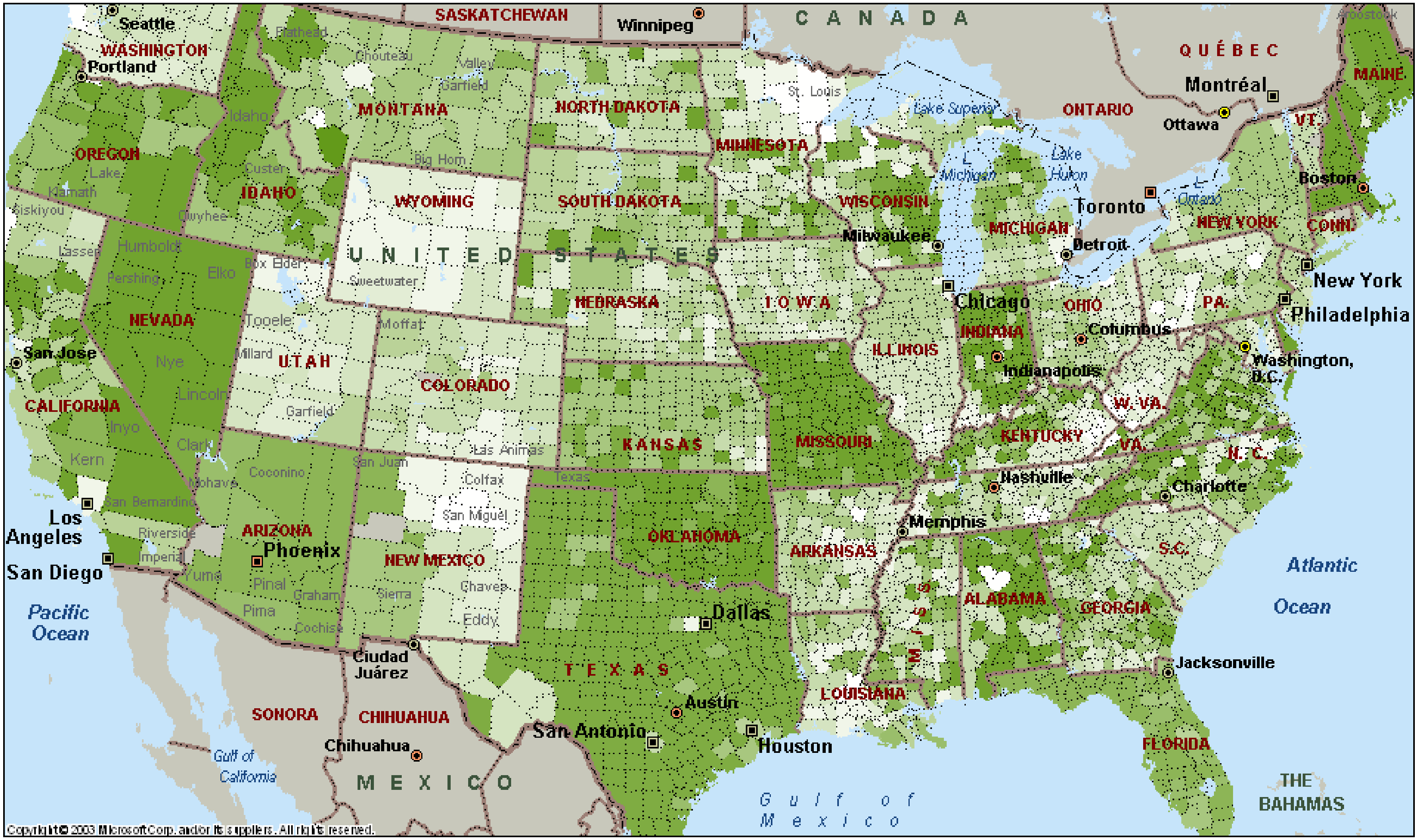
Figure 1: Cumulative Percent of Counties with Food Stamp Program, 1960-1975



Source: Authors’ tabulations of county FSP start dates. Counties are weighted by their 1960 population.

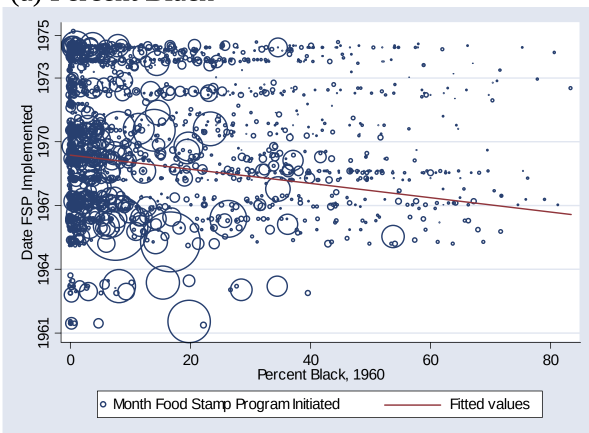
Figure 2: Food Stamp Program Start Date, By County (1961-1975)



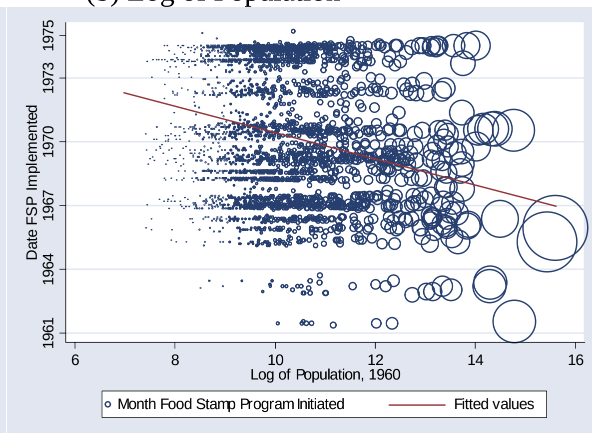
Note: Authors' tabulations of food stamp administrative data (U.S. Department of Agriculture, various years).

Figure 3: 1960 County Characteristics and County Food Stamp Start Date

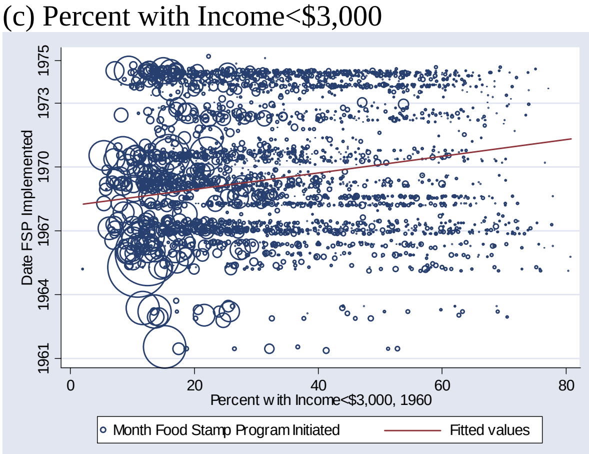
(a) Percent Black



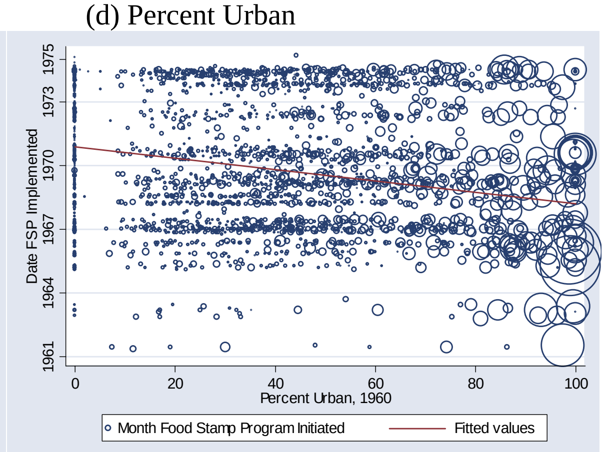
(b) Log of Population



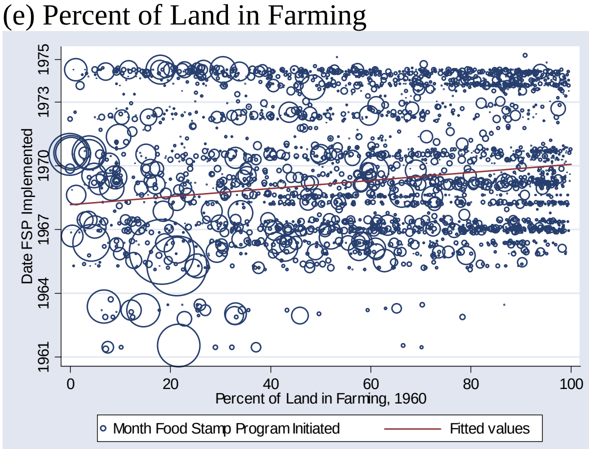
(c) Percent with Income<$3,000



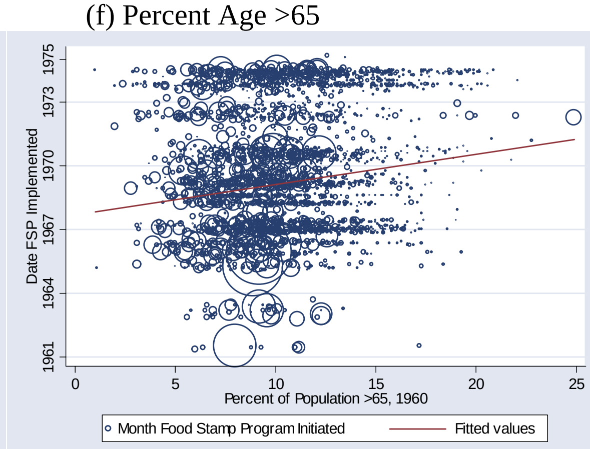
(d) Percent Urban



(e) Percent of Land in Farming



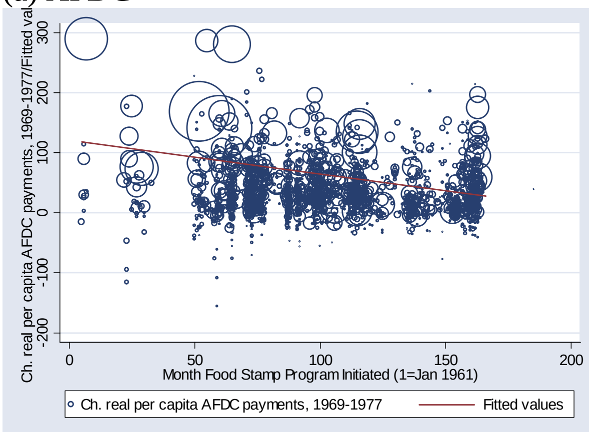
(f) Percent Age >65



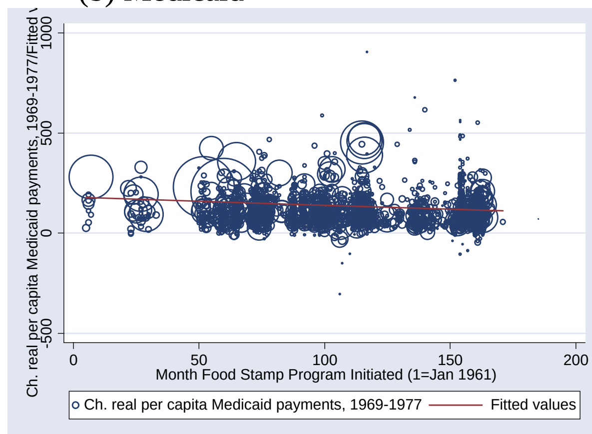
Note: Each graph provides a scatterplot of a 1960 county characteristic (x-axis) against the food stamp start date (y-axis) where the points are weighted by the 1960 county population. The graphs also contain the linear fit where the regression is weighted by 1960 county population. 1960 County characteristics are from the 1960 City and County Databook and the FSP implementation dates are from U.S. Department of Agriculture (various years).

Figure 4: Correlation between County Food Stamp Start Date and Change in Real Per Capita County Transfers 1969-1977

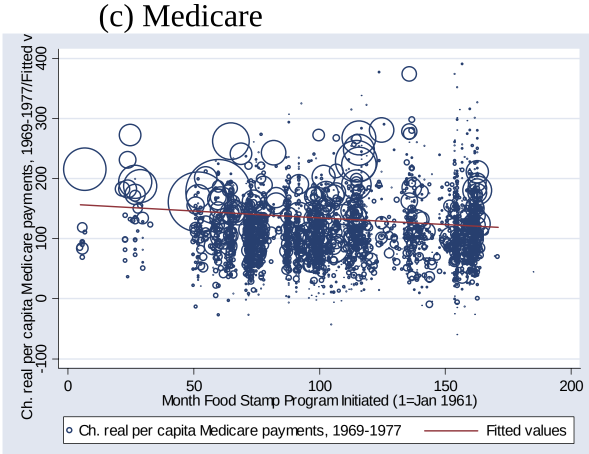
(a) AFDC



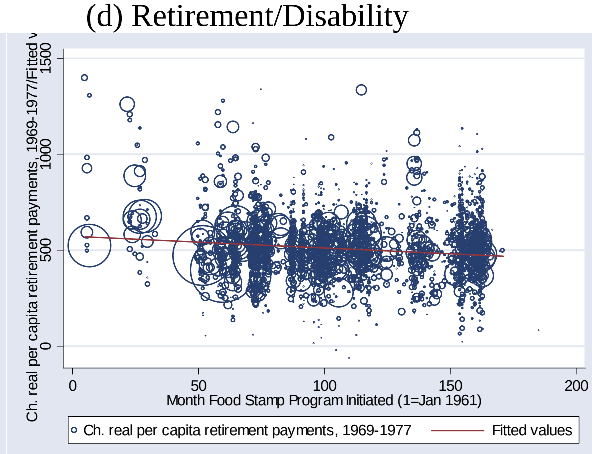
(b) Medicaid



(c) Medicare



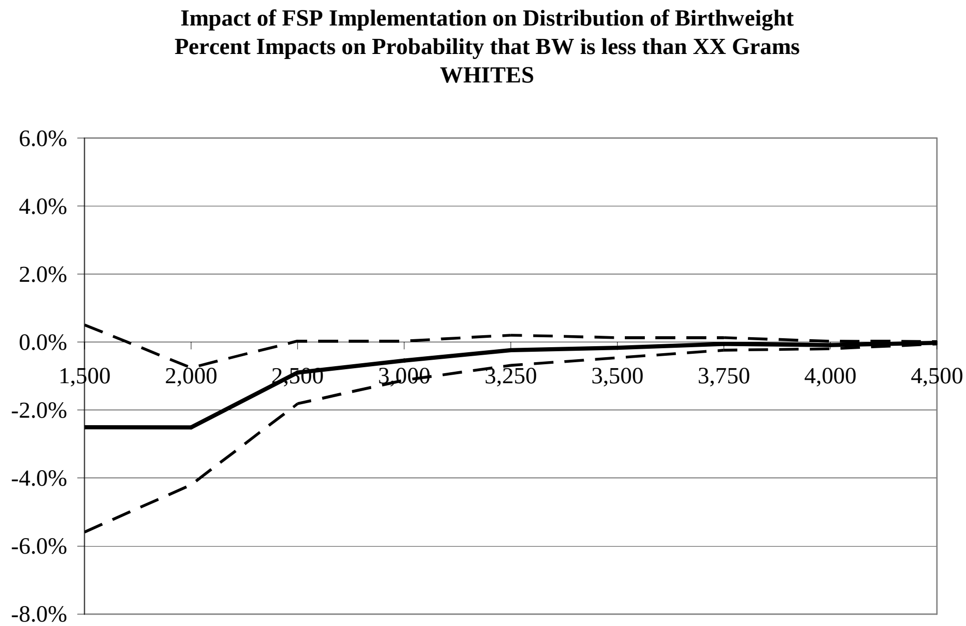
(d) Retirement/Disability



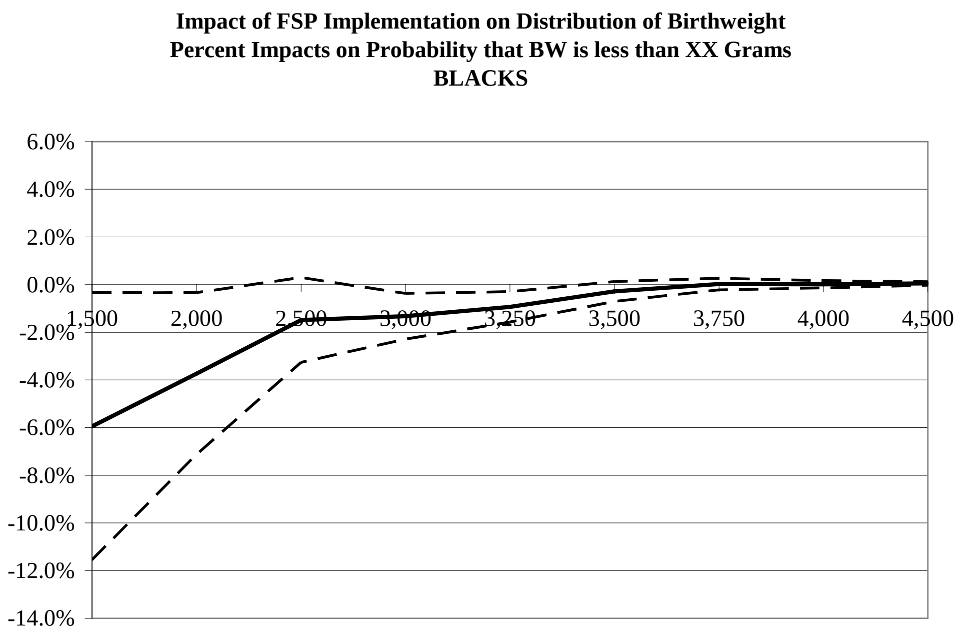
Note: Each graph provides a scatterplot of the 1969-1977 change in the real per capita transfer (y-axis) and the food stamp start date (x-axis) where the points are weighted by the 1960 county population. The graphs also contain the linear fit where the regression is weighted by 1960 county population.

Figure 5: Effects of FSP Implementation on Distribution of Birth Weight
Percent Impacts Relative to the Mean

(a) Whites



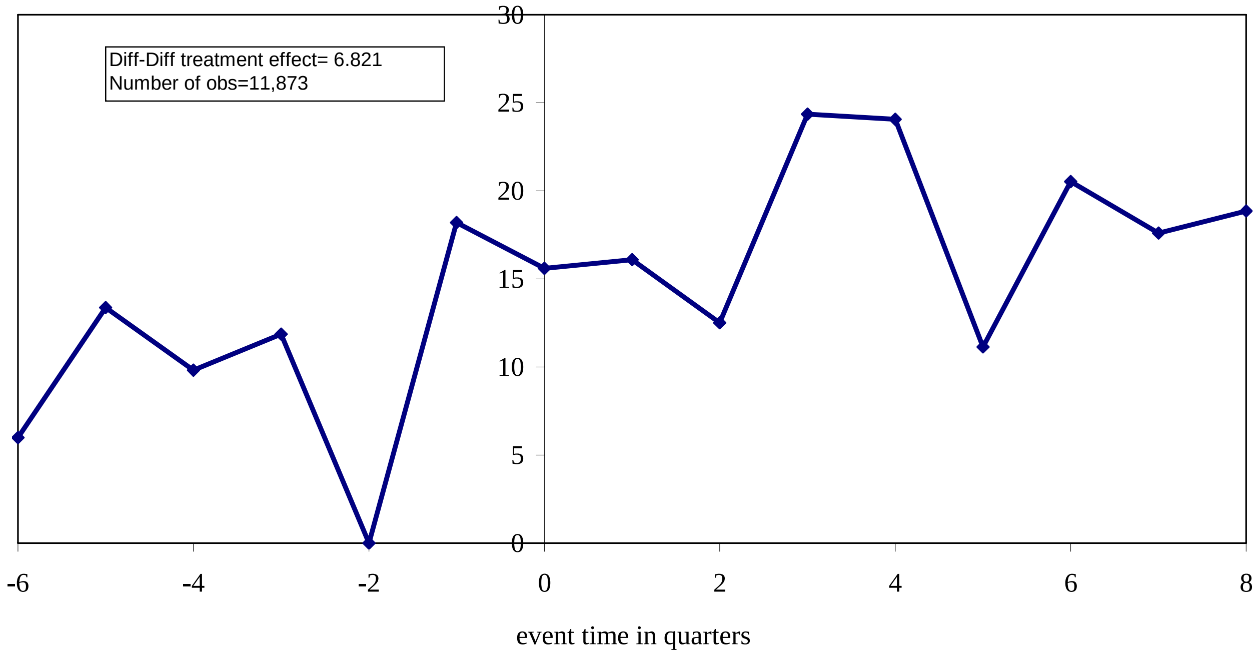
(b) Blacks



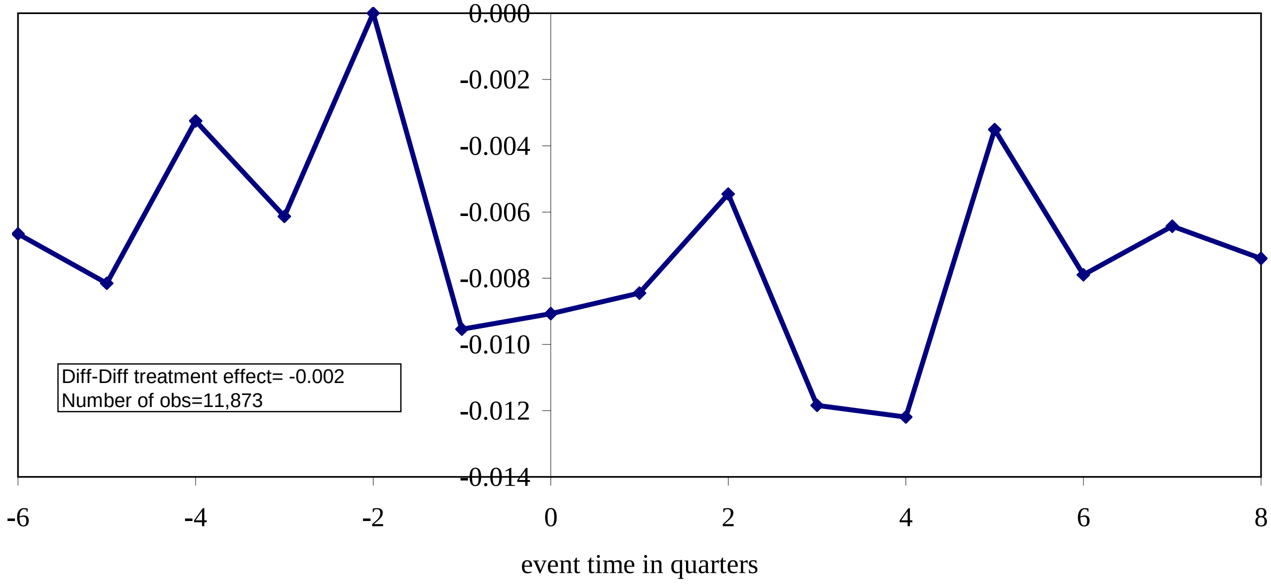
Notes: The graph shows estimates and 90 percent confidence intervals for the estimate of the impact of FSP implementation on the fraction of births in the county-quarter cell that is below each specified number of grams. The specification is given by column (5) in Table 2A.

Figure 6: Event Study Analysis for Blacks,

(a) Birth weight



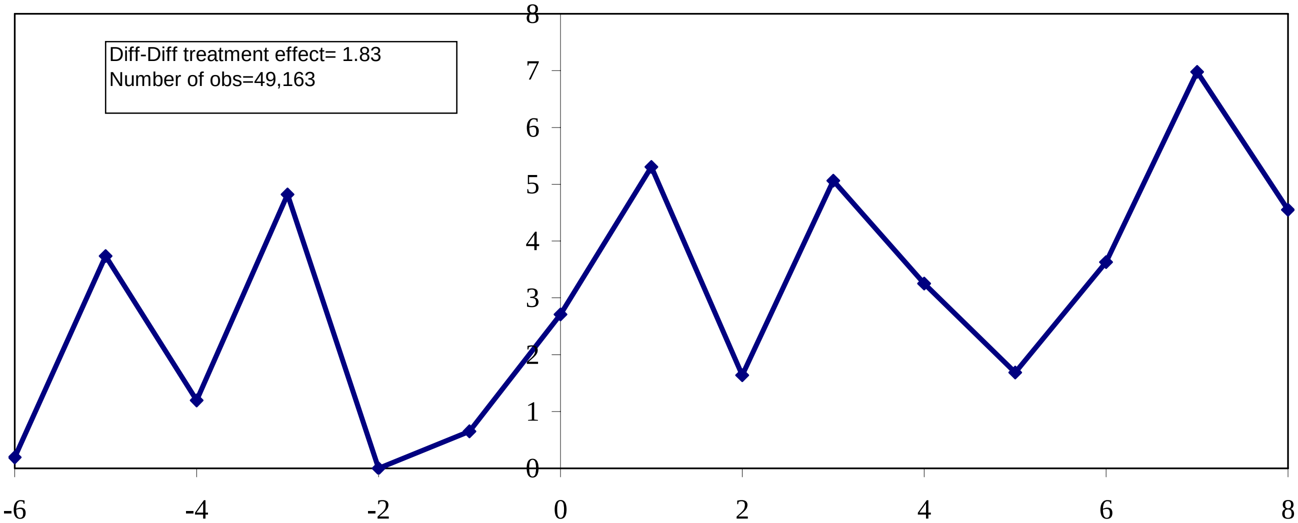
(b) Birth weight < 2,500 grams



Notes: Each figure plots event-study coefficients from a separate regression where the coefficients are defined as quarters relative to the quarter after the Food Stamp Program is implemented in the county. The sample is a balanced county sample, where a county is included only if there are 6 quarters of pre- and 8 quarters of post- implementation data. The specification includes controls for county, county * linear time, quarter, 1960 county controls interacted with time, county per capita transfers and county real per capita income.

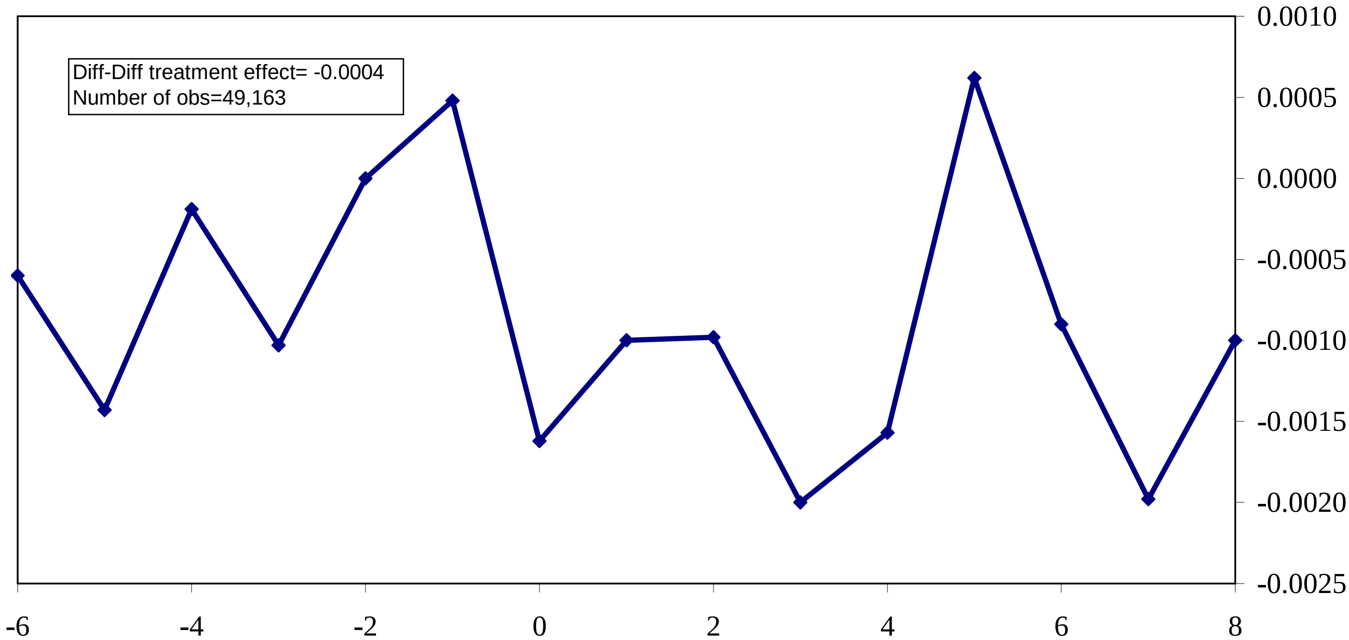
Figure 7: Event Study Analysis for Whites

(a) Birth weight



event time in quarters

(b) Birth weight < 2,500 grams



event time in quarters

Notes: Each figure plots event-study coefficients from a separate regression where the coefficients are defined as quarters relative to the quarter after the Food Stamp Program is implemented in the county. The sample is a balanced county sample, where a county is included only if there are 6 quarters of pre- and 8 quarters of post- implementation data. The specification includes controls for county, county * linear time, quarter, 1960 county controls interacted with time, county per capita transfers and county real per capita income.

Table 1: Determinants of County Level Food Stamp Program Start Date
Analysis Using the 1960 City and County Data Book

	All Counties		Limiting to post-pilot counties	
	(1)	(2)	(3)	(4)
Percent of land in farming	-0.025	0.124	0.114	0.136
	(0.830)	(0.028)***	(0.027)***	(0.033)***
Percent of pop with income<$3000	0.005	-0.544	-0.347	0.085
	(0.050)	(0.092)***	(0.088)***	(0.147)
Percent of pop urban	0.214	-0.068	-0.040	-0.001
	(4.36)**	(0.041)	(0.039)	(0.053)
Percent of pop black	-0.326	-0.208	-0.212	-0.474
	(4.36)**	(0.070)***	(0.067)***	(0.145)***
Percent of pop age < 5	-3.566	-2.329	-2.954	-3.557
	(4.92)**	(0.625)***	(0.593)***	(0.786)***
Percent of pop age > 65	-1.030	-0.982	-1.133	-3.048
	(2.49)*	(0.390)**	(0.371)***	(0.524)***
log population	-11.229	-9.139	-7.819	-7.335
	(13.44)**	(0.752)***	(0.718)***	(0.932)***
South * % of land in farming				-0.125
				(0.058)**
South * % pop with income<$3000				-0.603
				(0.188)***
South * % pop urban				-0.110
				(0.080)
South * % pop black				0.373
				(0.165)**
South * % pop age < 5				0.787
				(1.222)
South * % pop age > 65				3.467
				(0.754)***
South * log population				0.645
				(1.548)
State Fixed Effects		X	X	X
Number of Observations	2,957	2,957	2,939	2,939
R squared	0.14	0.56	0.55	0.56

Notes: The data is at the county level and the dependent variable is equal to the calendar month (normed to 1 in January 1961) that the county began offering the Food Stamp Program. The control variables come from the City and County Databook for 1960. Alaska counties are dropped due to missing data on the food stamp program. Very small counties (with population less than 1,000) are dropped because of missing data on some control variables. A small number of counties are dropped because the variable *percent of land in farming* exceeds 100 percent. Estimates are weighted using the 1960 county population.

Table 2A: Impacts of Food Stamp Introduction on Birth Outcomes, by Race

	Birthweight						Fraction < 2,500 grams					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. WHITES												
Ave FSP (0/1)	0.7952	1.9750	2.0388	2.6351	2.0892	2.1750	-0.0004	-0.0006	-0.0006	-0.0006	-0.0006	-0.0006
	(0.9420)	(0.94922)*	(0.94702)*	(0.89570)**	(1.03890)*	(0.9748)	(0.0003)	(0.00027)*	(0.00027)*	(0.00029)*	(0.0003)	(0.0004)
% impact (coeff/mean)	0.0002	0.0006	0.0006	0.0008	0.0006	0.0006	-0.0065	-0.0098	-0.0102	-0.0102	-0.0097	-0.0097
Impact, inflated	6.1172	15.1925	15.6833	20.2703	16.0705	16.7305	-0.0030	-0.0045	-0.0047	-0.0047	-0.0045	-0.0045
% impact, inflated	0.0018	0.0045	0.0047	0.0061	0.0048	0.0050	-0.0500	-0.0756	-0.0782	-0.0782	-0.0744	-0.0744
B. BLACKS												
Ave FSP (0/1)	5.1119	3.3974	3.4536	4.1202	5.4661	1.6646	-0.0020	-0.0015	-0.0015	-0.0016	-0.0019	-0.0009
	(2.8094)	(2.6496)	(2.6602)	(2.3174)	(2.57946)*	(2.3302)	(0.0011)	(0.0010)	(0.0010)	(0.0010)	(0.0012)	(0.0012)
% impact (coeff/mean)	0.0017	0.0011	0.0011	0.0013	0.0018	0.0005	-0.0157	-0.0113	-0.0113	-0.0122	-0.0149	-0.0068
Impact, inflated	12.4681	8.2863	8.4235	10.0492	13.3319	4.0600	-0.0050	-0.0036	-0.0036	-0.0039	-0.0047	-0.0021
% impact, inflated	0.0040	0.0027	0.0027	0.0032	0.0043	0.0013	-0.0383	-0.0276	-0.0276	-0.0298	-0.0364	-0.0165
1960 CCDB * linear time		x	x	x	x			x	x	x	x	
REIS controls	x	x	x	x	x	x	x	x	x	x	x	x
cty per cap real income			x	x	x	x			x	x	x	x
yr x qtr fixed effects	x	x	x	x	x	x	x	x	x	x	x	x
county fixed effects	x	x	x	x	x	x	x	x	x	x	x	x
state * linear time				x						x		
state * year fixed effects					x						x	
county * linear time						x						x
Observations (whites)	97905	97785	97785	97785	97785	97785	97905	97785	97785	97785	97785	97785
R-squared (whites)	0.54	0.54	0.54	0.55	0.55	0.56	0.17	0.17	0.17	0.17	0.18	0.19
mean of dependant variable (whites)	3349.95	3349.99	3349.99	3349.99	3349.99	3350.29	0.06	0.06	0.06	0.06	0.06	0.06
Subsample Population (whites)	0.99	0.98	0.98	0.98	0.98	0.98	0.99	0.98	0.98	0.98	0.98	0.98
Observations (blacks)	27414	27374	27374	27374	27374	27374	27414	27374	27374	27374	27374	27374
R-squared (blacks)	0.31	0.32	0.32	0.33	0.34	0.35	0.15	0.15	0.15	0.15	0.17	0.18
mean of dependant variable (blacks)	3096.66	3096.71	3096.71	3096.71	3096.71	3097.01	0.13	0.13	0.13	0.13	0.13	0.13
Subsample Population (blacks)	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The treatment is assigned as of the beginning of the 3rd trimester associated with the birth. The estimation sample includes means by county-quarter for years including 1968-1977 where cells with less than 25 births are dropped. In addition to the fixed effects, controls include 1960 county variables (log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Inflated impacts divide the parameter estimate by an estimate of the food stamp participation rate for the regression sample. Subsample population reports the percent of total births that are included in the regression sample (some are dropped due to missing data or due to small cell sizes).

Table 2B: Impacts of Food Stamp Introduction on Birth Outcomes, by Race (Continued)

	GEST < 37					%Female				
		(1)	(2)	(3)	(4)		(1)	(2)	(3)	(4)
A. WHITES										
Ave FSP (0/1)	-0.0010	-0.0009	-0.0009	-0.0010	-0.0012	-0.0003	-0.0002	-0.0002	-0.0005	-0.0004
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0006)	(0.0005)	(0.0005)	(0.0005)	(0.0006)	(0.0006)
B. BLACKS										
Ave FSP (0/1)	-0.0015	-0.0013	-0.0010	-0.0008	-0.0013	-0.0006	-0.0004	-0.0003	-0.0009	-0.0016
	(0.0022)	(0.0022)	(0.0022)	(0.0023)	(0.0025)	(0.0006)	(0.0013)	(0.0013)	(0.0014)	(0.0016)
1960 CCDB * linear time	x	x	x	x	x	x	x	x	x	x
REIS controls		x	x	x	x		x	x	x	x
cty per cap real income			x	x	x			x	x	x
yr x qtr fixed effects	x	x	x	x	x	x	x	x	x	x
county fixed effects	x	x	x	x	x	x	x	x	x	x
state * linear time				x					x	
state * year fixed effects					x					x
county * linear time										
Observations (whites)	66888	66888	66888	66888	66888	97785	97785	97785	97785	97785
R-squared (whites)	0.15	0.15	0.15	0.15	0.15	0.03	0.03	0.03	0.03	0.04
mean of dependant variable (whites)	0.08	0.08	0.08	0.08	0.08	0.49	0.49	0.49	0.49	0.49
Subsample Population (whites)	0.69	0.69	0.69	0.69	0.69	0.98	0.98	0.98	0.98	0.98
Observations (blacks)	15777	15777	15777	15777	15777	27374	27374	27374	27374	27374
R-squared (blacks)	0.22	0.22	0.22	0.23	0.24	0.04	0.04	0.04	0.04	0.05
mean of dependant variable (blacks)	0.18	0.18	0.18	0.18	0.18	0.49	0.49	0.49	0.49	0.49
Subsample Population (blacks)	0.64	0.64	0.64	0.64	0.64	0.94	0.94	0.94	0.94	0.94

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The treatment is assigned as of the beginning of the 3rd trimester associated with the birth. The estimation sample includes means by county-quarter for years including 1968-1977 where cells with less than 25 births are dropped. In addition to the fixed effects, controls include 1960 county variables (log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Inflated impacts divide the parameter estimate by an estimate of the food stamp participation rate for the regression sample. Subsample population reports the percent of total births that are included in the regression sample (some are dropped due to missing data or due to small cell sizes).

Table 3: Sensitivity to Changing Timing of Policy Introduction

	Birthweight				
MAIN POLICY EFFECT:	FSP-beg of 3rd	FSP-beg of 2nd	FSP-beg of 1st	FSP-beg of 3rd	FSP-beg of 3rd
SECOND POLICY EFFECT:	NA	NA	NA	FSP-beg of 2nd	FSP-beg of 1st
A. WHITES					
Ave FSP (0/1)	2.08541	1.69595	1.28799	2.55588	2.43387
[Main Policy Var]	(1.0205)	(1.0239)	(0.9930)	(1.6399)	(1.2683)
Ave FSP (0/1)	--	--	--	-0.5334	-0.4538
[Second Policy Var]				(1.6502)	(1.2316)
Observations	97785	97785	97785	97785	97785
R-squared	0.55	0.55	0.55	0.55	0.55
mean of dependant variable	3350.00	3350.00	3350.00	3350.00	3350.00
Subsample Population	0.98	0.98	0.98	0.98	0.98
B. BLACKS					
Ave FSP (0/1)	5.4466	4.7041	2.0708	5.3341	8.1080
[Main Policy Var]	(2.5318)	(2.4645)	(2.3957)	(4.5955)	(3.4444)
Ave FSP (0/1)	--	--	--	0.1296	-3.5153
[Second Policy Var]				(4.4502)	(3.2683)
Observations	27374	27374	27374	27374	27374
R-squared	0.34	0.34	0.34	0.34	0.34
mean of dependant variable	3096.67	3096.67	3096.67	3096.67	3096.67
Subsample Population	0.94	0.94	0.94	0.94	0.94

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The specifications vary by changing the timing of food stamp implementation. Base case is in column 1 where the timing is as of the beginning of the 3rd trimester. The alternative specifications include timing as of the beginning of the 2nd trimester (2) and the beginning of the 1st trimester (3). In specifications (4) we estimate jointly the treatment effects at the 3rd and 2nd trimesters and in (5) we estimate jointly the impacts measured at the 3rd and 1st trimester. All of the other control variables and sample definitions are described in the notes to table 2.

Table 4: Sensitivity of Estimates to Including 1-year Lead of Policy Variable

	Birthweight	LBW	VLBW	GEST<37	%Female
A. WHITES					
Ave FSP (0/1)	2.1607	-0.0005	-0.0002	-0.0019	-0.0003
	(1.0376)	(0.0004)	(0.0002)	(0.0007)	(0.0007)
One Year Lead of Ave FSP (0/1)	-0.1587	-0.0002	0.0000	0.0015	-0.0002
	(1.2256)	(0.0004)	(0.0002)	(0.0008)	(0.0008)
B. BLACKS					
Ave FSP (0/1)	6.3685	-0.0026	-0.0011	-0.0003	-0.0001
	(2.6810)	(0.0014)	(0.0007)	(0.0027)	(0.0019)
One Year Lead of Ave FSP (0/1)	-1.9802	0.0015	-0.0005	-0.0023	-0.0033
	(3.3561)	(0.0014)	(0.0007)	(0.0033)	(0.0022)
1960 CCDB * linear time	x	x	x	x	x
REIS controls	x	x	x	x	x
cty per cap real income	x	x	x	x	x
yr x qtr fixed effects	x	x	x	x	x
county fixed effects	x	x	x	x	x
state * linear time					
state * year fixed effects	x	x	x	x	x
county * linear time					
Observations (whites)	97785	97785	97785	66888	97785
R-squared (whites)	0.55	0.18	0.06	0.15	0.04
mean of dependant variable (whites)	3350.00	0.06	0.01	0.08	0.49
coeff/mean	0.0006	-0.0082	-0.0230	-0.0235	-0.0007
coeff/mean	0.0000	-0.0030	-0.0010	0.0183	-0.0004
Subsample Population (whites)	0.98	0.98	0.98	0.69	0.98
Observations (blacks)	27374	27374	27374	15777	27374
R-squared (blacks)	0.34	0.17	0.13	0.24	0.05
mean of dependant variable (blacks)	3096.67	0.13	0.02	0.18	0.49
coeff/mean	0.0021	-0.0198	-0.0550	-0.0018	-0.0001
coeff/mean	-0.0006	0.0112	-0.0235	-0.0125	-0.0067
Subsample Population (blacks)	0.94	0.94	0.94	0.64	0.94

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. There are two treatment variables: the baseline treatment is assigned as of the month of birth and a one year lead of that policy variable. The estimation sample includes means by county-quarter for years including 1968-1977 where cells with less than 25 births are dropped. Controls include county, year * quarter and state * year fixed effects, 1960 county variables (log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Inflated impacts divide the parameter estimate by an estimate of the food stamp participation rate for the regression sample. Subsample population reports the percent of total births that are included in the regression sample (some are dropped due to missing data or due to small cell sizes).

Table 5: Impacts of FSP Introduction on Birth Outcomes, by Demographic group

	Age<24				Age 24+				Educ<12				Educ=12				Educ>12			
	Birthweight	LBW	VLBW	GEST<37	Birthweight	LBW	VLBW	GEST<37	Birthweight	LBW	VLBW	GEST<37	Birthweight	LBW	VLBW	GEST<37	Birthweight	LBW	VLBW	GEST<37
A. WHITES																				
Ave FSP (0/1)	1.6005	0.0000	-0.0003	-0.0027	2.1467	-0.0009	-0.0003	-0.0024	2.7399	0.0009	-0.0005	-0.0040	1.8795	0.0001	-0.0003	0.0023	0.7237	-0.0004	-0.0005	-0.0048
	(1.2647)	(0.0005)	(0.0002)	(0.0035)	(1.2269)	(0.0004)	(0.0002)	(0.0023)	(2.6677)	(0.0011)	(0.0005)	(0.0025)	(1.7873)	(0.0007)	(0.0003)	(0.0019)	(2.4561)	(0.0009)	(0.0004)	(0.0047)
coeff/mean	0.0005	0.0001	-0.0250	-0.0302	0.0006	-0.0150	-0.0260	-0.0336	0.0008	0.0094	-0.0500	-0.0365	0.0006	0.0010	-0.0290	0.0329	0.0002	-0.0074	-0.0510	-0.0805
B. BLACKS																				
Ave FSP (0/1)	3.2797	-0.0010	-0.0012	0.0064	10.7279	-0.0051	-0.0019	0.0059	-4.5602	0.0006	-0.0003	0.0014	6.5756	-0.0041	-0.0018	-0.0043	8.5582	-0.0043	-0.0048	-0.0078
	(3.2005)	(0.0017)	(0.0008)	(0.0102)	(4.2345)	(0.0023)	(0.0010)	(0.0074)	(5.6535)	(0.0029)	(0.0012)	(0.0048)	(5.2980)	(0.0030)	(0.0015)	(0.0056)	(12.5336)	(0.0061)	(0.0027)	(0.0080)
coeff/mean	0.0011	-0.0074	-0.0580	0.0338	0.0034	-0.0423	-0.0935	0.0393	-0.0015	0.0046	-0.0150	0.0064	0.0021	-0.0345	-0.0905	-0.0271	0.0027	-0.0388	-0.2380	-0.0602
1960 CCDB * linear time	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
REIS controls	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
cty per cap real income	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
yr x qtr fixed effects	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
county fixed effects	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
state * linear time																				
state * year fixed effects	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
county * linear time																				
Observations (WHITES)	73677	73677	73677	63141	71806	71806	71806	61308	36546	36546	36546	34021	48888	48888	48888	45791	27785	27785	27785	25959
R-squared	0.40	0.12	0.06	0.08	0.51	0.16	0.06	0.10	0.30	0.12	0.07	0.13	0.46	0.12	0.06	0.13	0.48	0.12	0.07	0.15
mean of dependant variable	3308.46	0.07	0.01	0.09	3383.78	0.06	0.01	0.07	3275.68	0.09	0.01	0.11	3376.12	0.06	0.01	0.07	3397.39	0.05	0.01	0.06
Subsample Population	0.43	0.43	0.43	0.39	0.53	0.53	0.53	0.49	0.14	0.14	0.14	0.13	0.27	0.27	0.27	0.26	0.15	0.15	0.15	0.14
Observations (blacks)	20810	20810	20810	16698	13158	13158	13158	10894	12085	12085	12085	11198	8847	8847	8847	8253	3366	3366	3366	3042
R-squared (blacks)	0.27	0.14	0.12	0.25	0.31	0.17	0.14	0.39	0.32	0.17	0.12	0.27	0.23	0.15	0.12	0.31	0.22	0.15	0.15	0.40
mean of dependant variable (blacks)	3043.19	0.14	0.02	0.19	3174.21	0.12	0.02	0.15	3062.93	0.14	0.02	0.21	3105.68	0.12	0.02	0.16	3141.72	0.11	0.02	0.13
Subsample Population (blacks)	0.55	0.55	0.55	0.47	0.33	0.33	0.33	0.29	0.27	0.27	0.27	0.25	0.21	0.21	0.21	0.20	0.06	0.06	0.06	0.06

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The treatment is assigned as of the beginning of the 3rd trimester associated with the birth. The estimation sample includes means by county-quarter for years including 1968-1977 where cells with less than 25 births are dropped. Controls include county, year * quarter and state * year fixed effects, 1960 county variables (log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Inflated impacts divide the parameter estimate by an estimate of the food stamp participation rate for the regression sample. Subsample population reports the percent of total births that are included in the regression sample (some are dropped due to missing data or due to small cell sizes). Note that for some state-years, there is no data on education or presence of father.

Table 6: Impact of FSP Introduction on Infant Outcomes, by Quartiles of Poverty

	Birthweight				LBW			
	quart1	quart2	quart3	quart4	quart1	quart2	quart3	quart4
POOLED WHITES & BLACKS								
Ave FSP (0/1)	1.2070	5.3842	0.3660	1.8311	-0.0010	-0.0006	-0.0002	-0.0008
	(2.1740)	(2.1788)	(1.9353)	(1.6900)	(0.0008)	(0.0008)	(0.0006)	(0.0006)
coeff/mean	0.0004	0.0016	0.0001	0.0005	-0.0137	-0.0083	-0.0024	-0.0098
A. WHITES								
Ave FSP (0/1)	1.6758	4.8780	0.1914	2.3856	-0.0010	0.0001	0.0000	-0.0012
	(2.2587)	(2.1988)	(1.7030)	(1.7944)	(0.0007)	(0.0007)	(0.0006)	(0.0006)
coeff/mean	0.0005	0.0015	0.0001	0.0007	-0.0170	0.0020	-0.0003	-0.0169
B. BLACKS								
Ave FSP (0/1)	4.5768	3.5993	0.2073	1.3051	0.0003	-0.0073	-0.0007	-0.0002
	(7.2241)	(6.5880)	(4.7593)	(3.7190)	(0.0036)	(0.0034)	(0.0021)	(0.0017)
coeff/mean	0.0015	0.0012	0.0001	0.0004	0.0020	-0.0558	-0.0049	-0.0015
1960 CCDB * linear time	x	x	x	x	x	x	x	x
REIS controls	x	x	x	x	x	x	x	x
cty per cap real income	x	x	x	x	x	x	x	x
yr x qtr fixed effects	x	x	x	x	x	x	x	x
county fixed effects	x	x	x	x	x	x	x	x
state * linear time								
state * year fixed effects	x	x	x	x	x	x	x	x
county * linear time								
Observations (POOLED)	6025	11121	26138	54381	6025	11129	26263	59447
R-squared	0.86	0.76	0.56	0.35	0.60	0.60	0.48	0.23
mean of dependant variable	3319.59	3341.76	3352.30	3352.32	0.07	0.07	0.07	0.08
Subsample Population	0.24	0.26	0.25	0.23	0.24	0.25	0.24	0.26
Observations (whites)	6055	11121	26138	54381	6055	11121	26138	54381
R-squared (whites)	0.80	0.76	0.56	0.35	0.38	0.35	0.19	0.11
mean of dependant variable (whites)	3333.16	3341.76	3352.30	3352.32	0.06	0.06	0.06	0.07
Subsample Population (whites)	0.25	0.26	0.25	0.23	0.25	0.26	0.25	0.23
Observations (blacks)	2811	2899	3412	18252	2811	2899	3412	18252
R-squared (blacks)	0.41	0.43	0.41	0.29	0.28	0.26	0.28	0.12
mean of dependant variable (blacks)	3083.61	3069.12	3067.77	3108.52	0.13	0.13	0.14	0.13
Subsample Population (blacks)	0.17	0.22	0.23	0.32	0.17	0.22	0.23	0.32

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The treatment is assigned as of the beginning of the 3rd trimester associated with the birth. The estimation sample includes means by county-quarter for years including 1968-1977 where cells with less than 25 births are dropped. Controls include county, year * quarter and state * year fixed effects, 1960 county variables (log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Inflated impacts divide the parameter estimate by an estimate of the food stamp participation rate for the regression sample. Subsample population reports the percent of total births that are included in the regression sample (some are dropped due to missing data or due to small cell sizes). Quartiles are assigned using 1978 county per capita food stamp payments from the REIS.

Table 7: Impact of FSP Introduction on Birth Outcomes, by Parity of Birth

	FIRST BIRTH				SECOND + BIRTH			
	Birthweight	LBW	VLBW	GEST<37	Birthweight	LBW	VLBW	GEST<37
A. WHITES								
Ave FSP (0/1)	2.9630	-0.0002	-0.0004	-0.0001	0.7042	-0.0005	-0.0002	-0.0020
	(1.4348)	(0.0006)	(0.0003)	(0.0010)	(1.3622)	(0.0004)	(0.0002)	(0.0008)
B. BLACKS								
Ave FSP (0/1)	10.2329	-0.0039	-0.0018	0.0052	3.0579	-0.0005	-0.0019	-0.0014
	(5.4543)	(0.0028)	(0.0013)	(0.0050)	(3.5969)	(0.0018)	(0.0009)	(0.0040)
1960 CCDB * linear time	x	x	x	x	x	x	x	x
REIS controls	x	x	x	x	x	x	x	x
cty per cap real income	x	x	x	x	x	x	x	x
yr x qtr fixed effects	x	x	x	x	x	x	x	x
county fixed effects	x	x	x	x	x	x	x	x
state * linear time								
state * year fixed effects	x	x	x	x	x	x	x	x
county * linear time								
Observations (whites)	57217	57217	57217	39550	71415	71415	71415	49404
R-squared (whites)	0.38	0.11	0.06	0.11	0.52	0.17	0.07	0.15
mean of dependant variable (whites)	3301.72	0.07	0.01	0.08	3383.53	0.06	0.01	0.08
coeff/mean	0.0009	-0.0030	-0.0420	-0.0008	0.0002	-0.0087	-0.0210	-0.0251
Subsample Population (whites)	0.38	0.38	0.38	0.27	0.57	0.57	0.57	0.40
Observations (blacks)	12171	12171	12171	8110	17867	17867	17867	11308
R-squared (blacks)	0.23	0.13	0.12	0.19	0.33	0.18	0.13	0.22
mean of dependant variable (blacks)	3032.64	0.14	0.02	0.17	3133.76	0.13	0.02	0.18
coeff/mean	0.0034	-0.0276	-0.0915	0.0304	0.0010	-0.0038	-0.0935	-0.0075
Subsample Population (blacks)	0.33	0.33	0.33	0.24	0.54	0.54	0.54	0.38

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The treatment is assigned as of the beginning of the 3rd trimester associated with the birth. The estimation sample includes means by county-quarter for years including 1968-1977 where cells with less than 25 births are dropped. Controls include county, year * quarter and state * year fixed effects, 1960 county variables (log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Inflated impacts divide the parameter estimate by an estimate of the food stamp participation rate for the regression sample. Subsample population reports the percent of total births that are included in the regression sample (some are dropped due to missing data or due to small cell sizes).

Table 8: Impacts of FSP Introduction on Birth Outcomes, by Geography

	SOUTH				NONSOUTH				URBAN COUNTIES				NONURBAN COUNTIES			
	Birthweight	LBW	VLBW	GEST<37	Birthweight	LBW	VLBW	GEST<37	Birthweight	LBW	VLBW	GEST<37	Birthweight	LBW	VLBW	GEST<37
A. WHITES																
Ave FSP (0/1)	2.4028	-0.0011	-0.0004	-0.0029	1.7715	-0.0003	-0.0002	-0.0008	2.3642	-0.0008	-0.0004	-0.0016	0.5079	-0.0002	0.0001	-0.0004
	(1.6123)	(0.0005)	(0.0002)	(0.0012)	(1.3223)	(0.0004)	(0.0002)	(0.0006)	(1.2468)	(0.0004)	(0.0002)	(0.0007)	(1.6145)	(0.0006)	(0.0002)	(0.0010)
B. BLACKS																
Ave FSP (0/1)	3.5265	-0.0023	-0.0010	-0.0016	7.0031	-0.0009	-0.0021	-0.0012	8.3711	-0.0034	-0.0019	0.0000	-0.7453	0.0023	0.0005	-0.0045
	(3.1344)	(0.0014)	(0.0008)	(0.0036)	(3.9921)	(0.0022)	(0.0011)	(0.0033)	(2.8456)	(0.0013)	(0.0008)	(0.0029)	(5.2187)	(0.0023)	(0.0009)	(0.0051)
1960 CCDB * linear time	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
REIS controls	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
cty per cap real income	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
yr x qtr fixed effects	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
county fixed effects	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
state * linear time																
state * year fixed effects	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
county * linear time																
Observations (whites)	44194	44194	44194	22754	53591	53591	53591	44134	32282	32282	32282	20427	65503	65503	65503	46461
R-squared (whites)	0.32	0.10	0.06	0.11	0.65	0.23	0.07	0.17	0.71	0.30	0.10	0.26	0.33	0.11	0.05	0.10
mean of dependant variable (whites)	3339.30	0.07	0.01	0.09	3358.82	0.06	0.01	0.08	3330.81	0.07	0.01	0.08	3359.45	0.06	0.01	0.08
coeff/mean	0.0007	-0.0157	-0.0430	-0.0321	0.0005	-0.0048	-0.0160	-0.0100	0.0007	-0.0113	-0.0400	-0.0200	0.0002	-0.0025	0.0140	-0.0053
Subsample Population (whites)	0.29	0.29	0.29	0.13	0.69	0.69	0.69	0.55	0.73	0.73	0.73	0.51	0.25	0.25	0.25	0.18
Observations (blacks)	20837	20837	20837	10411	6537	6537	6537	5366	13090	13090	13090	7429	14284	14284	14284	8348
R-squared (blacks)	0.32	0.14	0.12	0.17	0.38	0.24	0.15	0.28	0.37	0.21	0.17	0.32	0.25	0.11	0.08	0.13
mean of dependant variable (blacks)	3103.39	0.13	0.02	0.19	3075.26	0.13	0.03	0.16	3076.38	0.13	0.02	0.17	3115.27	0.13	0.02	0.19
coeff/mean	0.0011	-0.0176	-0.0485	-0.0086	0.0023	-0.0069	-0.0687	-0.0076	0.0027	-0.0259	-0.0950	0.0001	-0.0002	0.0174	0.0255	-0.0235
Subsample Population (blacks)	0.49	0.49	0.49	0.25	0.45	0.45	0.45	0.39	0.77	0.77	0.77	0.53	0.17	0.17	0.17	0.11

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The treatment is assigned as of the beginning of the 3rd trimester associated with the birth. The estimation sample includes means by county-quarter for years including 1968-1977 where cells with less than 25 births are dropped. Controls include county, year * quarter and state * year fixed effects, 1960 county variables (log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Inflated impacts divide the parameter estimate by an estimate of the food stamp participation rate for the regression sample. Subsample population reports the percent of total births that are included in the regression sample (some are dropped due to missing data or due to small cell sizes). Urban counties are defined as those with greater than 50 percent of the 1960 population lives in an urban area.

Table 9: Impact of FSP Introduction on Birth Location

	Fraction birth in hospital	Fraction birth in hospital or physicial attending
A. WHITES		
Ave FSP (0/1)	0.0012	0.0010
	(0.0009)	(0.0009)
B. BLACKS		
Ave FSP (0/1)	-0.0018	0.0000
	(0.0030)	(0.0030)
1960 CCDB * linear time	x	x
REIS controls	x	x
cty per cap real income	x	x
yr x qtr fixed effects	x	x
county fixed effects	x	x
state * linear time		
state * year fixed effects	x	x
county * linear time		
Observations (whites)	97785	97785
R-squared (whites)	0.73	0.80
mean of dependant variable (whites)	0.99	1.00
coeff/mean	0.0012	0.0010
Subsample Population (whites)	0.98	0.98
Observations (blacks)	27374	27374
R-squared (blacks)	0.77	0.79
mean of dependant variable (blacks)	0.94	0.95
coeff/mean	-0.0019	0.0000
Subsample Population (blacks)	0.94	0.94

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The treatment is assigned as of the month of birth. The estimation sample includes means by county-quarter for years including 1968-1977 where cells with less than 25 births are dropped. Controls include county, year * quarter and state * year fixed effects, 1960 county variables (log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000) each interacted with a linear time trend, per capita county transfer income (public assistance, medical care, and retirement and disability benefits), and county real per capita income. Estimates are weighted using the number of births in the cell and are clustered on county. Standard errors are in parentheses. Inflated impacts divide the parameter estimate by an estimate of the food stamp participation rate for the regression sample. Subsample population reports the percent of total births that are included in the regression sample (some are dropped due to missing data or due to small cell sizes).

Table 10: Fertility

	birthrate= births / pop women 15-44 * 1000				
	FSP implemented as of X quarters prior to birth				
MAIN POLICY EFFECT:	3 qtrs	4 qtrs	4 qtrs	6 qtrs	7 qtrs
A. WHITES	0.013	-0.004	0.007	0.031	0.035
Ave FSP (0/1)	(0.078)	(0.074)	(0.071)	(0.074)	(0.070)
T eff relative to mean	0.001	0.000	0.000	0.002	0.002
B. BLACKS	0.211	0.157	0.276	0.307	0.227
Ave FSP (0/1)	(0.221)	(0.206)	(0.193)	(0.190)	(0.183)
T eff relative to mean	0.008	0.006	0.011	0.012	0.009
1960 CCDB * linear time	x	x	x	x	x
REIS controls	x	x	x	x	x
cty per cap real income	x	x	x	x	x
yr x qtr fixed effects	x	x	x	x	x
county fixed effects	x	x	x	x	x
state * linear time					
state * year fixed effects	x	x	x	x	x
county * linear time					
Observations (whites)	120293	120293	120293	120293	120293
R-squared (whites)	0.82	0.82	0.82	0.82	0.82
mean of dependant variable (whites)	19.40	19.40	19.40	19.40	19.40
Subsample Population (whites)	1.00	1.00	1.00	1.00	1.00
Observations (blacks)	44044	44044	44044	44044	44044
R-squared (blacks)	0.75	0.75	0.75	0.75	0.75
mean of dependant variable (blacks)	26.24	26.24	26.24	26.24	26.24
Subsample Population (blacks)	0.98	0.98	0.98	0.98	0.98

Appendix Table 1: Percent of Observations with Nonmissing Data, by year and race

year	birthweight	parity	gestation	mother's education	legitimacy	attended birth
WHITES						
1968	1.00	1.00	0.83	0.00	0.71	1.00
1969	1.00	0.99	0.60	0.62	0.66	1.00
1970	0.99	0.99	0.60	0.66	0.66	1.00
1971	1.00	0.99	0.60	0.66	0.67	1.00
1972	1.00	0.98	0.60	0.66	0.67	1.00
1973	1.00	0.96	0.61	0.69	0.67	1.00
1974	1.00	0.99	0.61	0.70	0.67	1.00
1975	1.00	0.99	0.61	0.70	0.67	1.00
1976	1.00	0.99	0.64	0.76	0.67	1.00
1977	1.00	0.99	0.65	0.76	0.60	1.00
BLACKS						
year	birthweight	parity	gestation	mother's education	legitimacy	attended birth
1968	1.00	1.00	0.82	0.00	0.75	1.00
1969	0.99	0.99	0.52	0.59	0.70	1.00
1970	0.99	0.99	0.54	0.65	0.70	1.00
1971	1.00	0.98	0.54	0.65	0.71	1.00
1972	1.00	0.95	0.53	0.66	0.72	1.00
1973	1.00	0.92	0.57	0.74	0.72	1.00
1974	1.00	0.98	0.57	0.76	0.71	1.00
1975	1.00	0.98	0.57	0.76	0.71	1.00
1976	1.00	0.98	0.60	0.82	0.71	1.00
1977	1.00	0.98	0.60	0.83	0.64	1.00

Notes: Authors' tabulations of 1968-1977 Natality detail files.

Appendix Table 2
Effect of FSP Implementation on Fraction of Births that are Low Birth Rate
Results using state-year data for 1959+

	Original period 1968+		**Full Period 1959+**		**Post-Pilot county period 1964+**	
Fraction of births less than:	< 2,500 gms	< 1,500 gms	< 2,500 gms	< 1,500 gms	< 2,500 gms	< 1,500 gms
(A) Whites						
FSP (1 qtr before birth quarter)	-0.0003	-0.0002	-0.0009	0.0003	-0.0003	0.0003
	(0.0007)	(0.0003)	(0.0005)	(0.0002)	(0.0006)	(0.0002)
Observations	500	500	947	947	700	700
R-squared	0.96	0.57	0.95	0.58	0.96	0.56
mean of dependant variable	0.065	0.009	0.068	0.010	0.067	0.010
% impact (coeff/mean)	-0.441%	-2.571%	-1.280%	2.795%	-0.516%	2.869%
(B) Blacks						
FSP (1 qtr before birth quarter)	-0.0021	-0.0002	-0.0019	-0.0003	-0.0014	-0.0002
	(0.0019)	(0.0013)	(0.0016)	(0.0008)	(0.0018)	(0.0009)
Observations	500	500	946	946	699	699
R-squared	0.94	0.79	0.93	0.81	0.93	0.79
mean of dependant variable	0.128	0.022	0.131	0.022	0.131	0.023
% impact (coeff/mean)	-1.621%	-0.963%	-1.486%	-1.509%	-1.093%	-0.801%
state & year fixed effects	x	x	x	x	x	x
REIS controls, per cap income	x	x	x	x	x	x
state linear time trends	x	x	x	x	x	x

Note: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy. The estimation sample includes the fraction of births by state-year. In addition to the fixed effects, controls include, per capita state transfer income (public assistance, medical care, and retirement and disability benefits), and state real per capita income. The treatment is assigned as of one quarter before the birth and is a weighted average of the county FSP implementation using county births by month from 1968. Estimates are weighted using the number of births in the cell and are clustered on state. Standard errors are in parentheses.