INSIDE THE WAR ON POVERTY: THE IMPACT OF FOOD STAMPS ON BIRTH OUTCOMES

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Abstract—This paper evaluates the health impacts of a signature initiative of the War on Poverty: the introduction of the modern Food Stamp Program (FSP). Using variation in the month FSP began operating in each U.S. county, we find that pregnancies exposed to FSP three months prior to birth yielded deliveries with increased birth weight, with the largest gains at the lowest birth weights. We also find small but statistically insignificant improvements in neonatal mortality. We conclude that the sizable increase in income from FSP improved birth outcomes for both whites and African Americans, with larger impacts for African American mothers.

I. Introduction

IN this paper, we evaluate the health consequences of a sizable improvement in the resources available to America's poorest. In particular, we examine the impact of the Food Stamp Program (FSP), which in 2007 provided $34 billion in payments to about 13 million households, on infant health. Our paper makes two distinct contributions. First, although the goal of the FSP is to increase the nutrition of the poor, few papers have examined its impact on health outcomes. Second, building on work by Hoynes and Schanzenbach (2009), we argue that the FSP treatment represents an exogenous increase in income for the poor. Our analysis therefore represents a causal estimate of the impact of income on health, an important topic with little convincing evidence due to concerns about endogeneity and reverse causality (Currie, 2009).

We use the natural experiment afforded by the nationwide rollout of the modern FSP during the 1960s and early 1970s. Our identification strategy uses the sharp timing of the county-by-county rollout of the FSP, which was initially constrained by congressional funding authorizations (and ultimately became available in all counties by 1975). Specifically, we use information on the month the FSP began operating in each of the roughly 3,100 U.S. counties and examine the impact of the FSP rollout on mean birth weight, low birth weight, gestation, and neonatal mortality.

Throughout the history of the FSP, the program parameters have been set by the U.S. Department of Agriculture (USDA) and are uniform across states. In the absence of the state-level variation often leveraged by economists to evaluate transfer programs, previous FSP research has typically resorted to strong assumptions as to the comparability of FSP participants and eligible nonparticipants (Currie, 2003). Not surprisingly, the literature is far from settled as to what casual impact (if any) the FSP has on nutrition and health.

Hoynes and Schanzenbach (2009) use this county rollout to examine the impact of the FSP on food consumption using the PSID. They found that the introduction of the FSP increased total food spending and decreased out-of-pocket food spending. Importantly, consistent with the predictions of canonical microeconomic theory, the magnitude of the increase in food expenditures was similar to an equivalent-sized income transfer, implying that most recipient households were inframarginal (that is, they would spend more on the subsidized good than the face value of the in-kind transfer). As one of the largest antipoverty programs in the United States—comparable in cost to the earned income tax credit (EITC) and substantially larger than Temporary Assistance to Needy Families (TANF)—understanding FSP effects is valuable both in its own right and for what it reveals about the relationship between income and health.[1]

We focus on birth outcomes for several reasons. First, families represent an important subgroup of the food stamp caseload. Over 60% of food stamp households include chil-

[1] The cost of the FSP was $33 billion in 2006 (compared to $24 billion for TANF, $33 billion for the EITC, and $5.4 billion for WIC, the Special Supplemental Nutrition Program for Women, Infants and Children).

dren, and one-third have at least one preschool-age child. Second, birth outcomes improved substantially during the late 1960s and early 1970s. Third, to the extent that the FSP improved birth outcomes, later-life health outcomes of these cohorts may have also benefited (Barker, 1992; Black, Devereux, & Salvanes, 2007). Finally, the vital statistics data used in this project are ideally suited for analyzing FSP rollout: the birth (death) microdata contain the county of birth (death) and the month of birth (death). This, combined with the large sample sizes (for example, more than 1 million birth records per year in the data set), allows us to use the discrete nature of the FSP rollout with significant statistical power.

We find that infant outcomes improve with FSP introduction. Changes in mean birth weight are small, increasing roughly half a percent for blacks and whites who participated in the program (effect of the treatment on the treated). Impacts were larger at the bottom of the birth weight distribution, reducing the incidence of low birth weight among the treated by 7% for whites and between 5% and 11% for blacks. Changes in this part of the birth weight distribution are important because they are closely linked to other newborn health measures. Although not all treatment effects are statistically significant, they point consistently to improvements in birth weight following the introduction of the FSP. We also find that the FSP introduction leads to a reduction in neonatal mortality, although these results rarely reach statistical significance. We find very small (but precisely estimated) impacts of the FSP on fertility, suggesting that the results are not biased by endogenous sample selection. All results are robust to various sets of controls, such as county fixed effects, state-by-year fixed effects, and county-specific linear trends. Moreover, FSP impact estimates are robust to and little changed by county-by-year controls for federal spending on other social programs, suggesting our basic identification strategy is clean. Finally, we present an event study analysis that further supports the validity of the identification strategy.

Food stamps are the fundamental safety net in the United States. Unlike other means-tested programs, there is no additional targeting to specific subpopulations. Current benefits average about $200 per recipient household per month. Our analysis constitutes the first evidence that despite the fact that if did not target pregnant mothers (or even women), introduction of the FSP improved newborn health.

II.   Introduction of the Food Stamp Program

The modern FSP began with President Kennedy's 1961 announcement of a pilot food stamp program that was to be established in 8 impoverished counties. The pilot programs were 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 FSP in their county. As with the current FSP, the pro-

gram was federally funded, and benefits were redeemable at approved retail food stores. In the period following the passage of the FSA, a steady stream of counties initiated such programs, and federal spending on the FSP more than doubled between 1967 and 1969 (from $115 million to $250 million). Support for requiring counties to participate in FSP 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.

Figure 1 plots the percentage of counties with an FSP from 1960 to 1975.[2] During the pilot phase (1961–1964), FSP coverage increased slowly. Beginning in 1964, program growth accelerated, and coverage expanded at a steady pace until all counties were covered in 1974. Furthermore, there was substantial heterogeneity in the timing of adoption of the FSP, both within and across states. The map in figure 2 shades counties according to the date of FSP adoption (darker shading denotes a later start-up date). Our basic identification strategy considers the month of FSP adoption for each county the FSP "treatment."[3]

For our identification strategy to yield causal estimates of the program, it is key to establish that the timing of FSP adoption appears to be exogenous. Prior to the FSP, some counties provided food aid through the Commodity Distribution Program (CDP), which took surplus food purchased by the federal government as part of an agricultural price support policy and distributed those goods to the poor. The 1964 Food Stamp Act allowed counties to voluntarily set up an FSP, but the act also stated that no county could run both the FSP and the CDP. Thus, for counties that previously ran a CDP, adoption of the FSP implies termination of the CDP.[4] The political accounts of the time suggest that debates about adopting the FSP pitted powerful agricultural interests (which favored the CDP) against advocates for the poor (who favored the FSP; see MacDonald, 1977; Berry, 1984).[5] In particular, counties with strong support for farm-

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[2] Counties are weighted by their 1970 population. Note this is not the food stamp caseload, but represents the percentage of the U.S. population that lived in a county with an FSP.

[3] This timing lines up exceptionally well with county-level FSP spending as measured in the Regional Economic Information System data. See online appendix table 3.

[4] This transition in nutritional assistance would tend to bias FSP impact estimates downward, but we do not think this bias is substantial because of the limited scope of the CDP. The CDP was not available in all counties, and recipients often had to travel long distances to pick up the items. Further, the commodities were distributed infrequently and inconsistently, and provided a narrow set of commodities. The most frequently available were flour, cornmeal, rice, dried milk, peanut butter, and rolled wheat (Citizens' Board of Inquiry 1968). In contrast, food stamp benefits can be used to purchase all food items (except hot foods for immediate consumption, alcoholic beverages, and vitamins).

[5] In fact, as Berry (1984) and Ripley (1969) noted, 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.

FIGURE 1.—WEIGHTED PERCENTAGE OF COUNTIES WITH A FOOD STAMP PROGRAM, 1960–1975
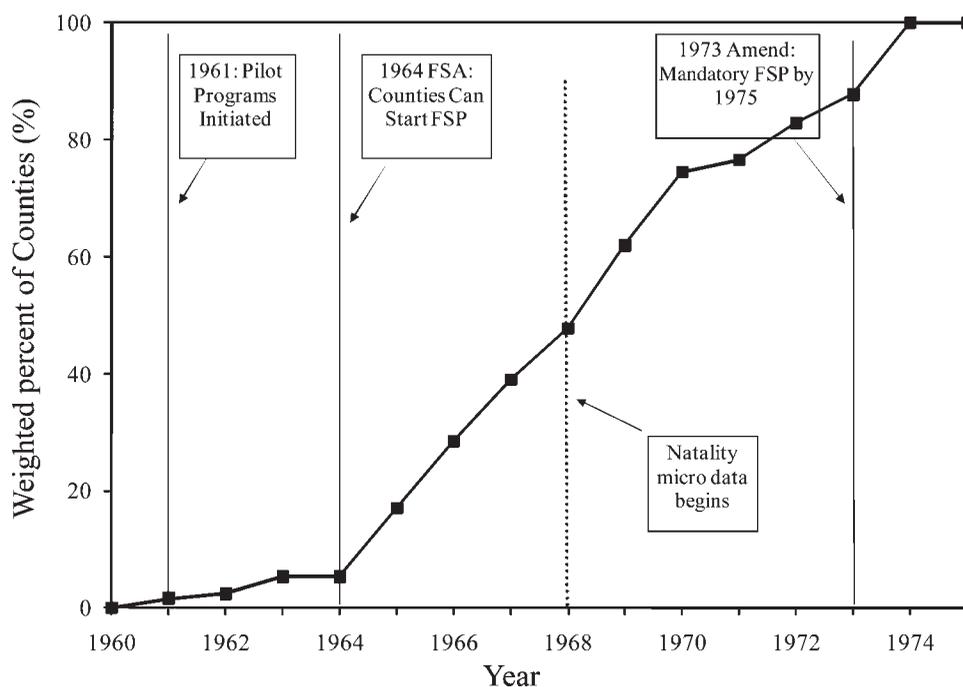
Authors' tabulations of food stamp administrative data (U.S. Department of Agriculture, various years). Counties are weighted by their 1960 population.

FIGURE 2.—FOOD STAMP PROGRAM START DATE BY COUNTY (1961–1975)
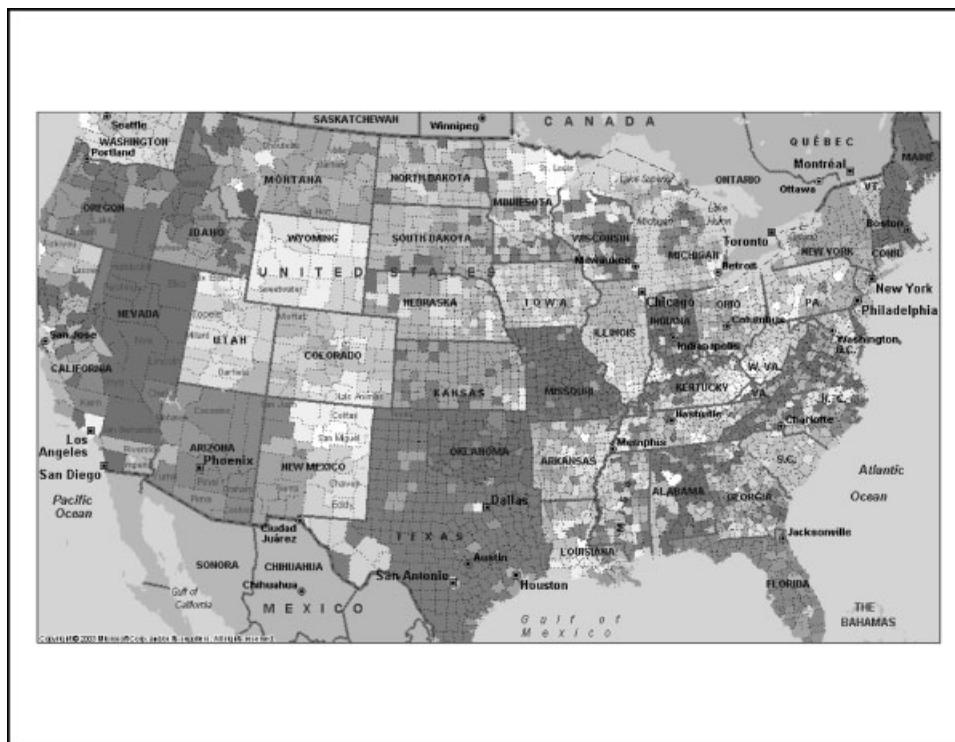
Authors' tabulations of food stamp administrative data (U.S. Department of Agriculture, various years). The shading corresponds to the county FSP start date, where darker shading indicates later county implementation.

ing interests (such as southern or rural counties) may be late adopters of the FSP. Counties with strong support for the low-income population (such as northern, urban counties with large populations of poor) may adopt FSP earlier in the period. 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.

In earlier work (Hoynes & Schanzenbach, 2009), we documented that larger counties with a greater fraction of the population that was urban, black, or low income indeed implemented the FSP earlier, consistent with the historical accounts. We sought to predict FSP adoption date with 1960 county characteristics—those recorded immediately prior to the pilot FSP phase. That analysis showed that larger counties and those with a higher share of black, elderly, young, or low income implemented earlier and those where more of the land was used in farming implement later.[6] Nevertheless, the county characteristics explain very little of the variation in adoption dates (see online appendix figure 1). 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 approachin 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 are spuriously correlated with the county treatment effect, all of our regressions include interactions of these 1960 pretreatment county characteristics with time trends as in Acemoglu, Autor, and Lyle (2004) and Hoynes and Schanzenbach (2009).

FSP introduction took place during a period of tremendous expansion in cash and noncash transfer programs as the War on Poverty and Great Society programs were expanding. To disentangle the FSP from these other programs, the county-by-month variation in FSP rollout is key. Further, given that virtually all means-tested programs are administered at the state level, our controls for state-by-year fixed effects should absorb these program impacts. To be sure, however, our models include controls for per capita real county government (non–food stamp) transfers.[7]

III.   Background Literature

The goal of the FSP is to improve nutrition among the low-income population. As such, many studies have examined the impact of the FSP on nutritional availability and intake, food consumption, food expenditures, and food insecurity (see Currie, 2003, and Fraker, 1990, for reviews of the literature).

Almost all existing studies of the impact of the FSP use research designs that rely on comparisons of program participants to nonparticipants at the individual level. This approach is subject to the usual criticisms regarding selection into the program. For example, a number of researchers (Currie, 2003; Currie & Moretti, 2008; Fraker, 1990) have pointed out that if food stamp recipients are healthier, are more motivated, or have better access to health care than other eligible women, then comparisons between participants and nonparticipants could produce positive program estimates even if the true effect is 0. Conversely, if food stamp participants are more disadvantaged than other families, such comparisons may understate the program's impact. In fact, as Currie (2003) reported, several studies, including Basiotis, Cramer-LeBlanc, and Kennedy (1998) and Butler and Raymond (1996), find that food stamp participation leads to a reduction in nutritional intake. These unexpected results are almost certainly driven by negative selection in participation.

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. A long literature on the effects of cash assistance programs is based on this type of identification strategy (Moffitt, 1992; Blank, 2002). Unfortunately, the FSP is a federal program for which there is very little geographic variation (aside from the variation we use in this paper) or variation in eligibility criteria or benefit levels, so prior researchers have had to employ alternative approaches.

Identification issues aside, it is noteworthy that few FSP studies examine the impact on health outcomes. We are aware of two studies. 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. Our work is closer to that of Currie and Moretti (2008), who use the county rollout of FSP in California to analyze birth outcomes. They find that FSP introduction was associated with a reduction in birth weight, which was driven particularly by first births among teens and by changes for Los Angeles County. As discussed below, this negative effect is possible if the FSP led to fertility changes or increases in the survival of low-birth-weight fetuses. The timing of FSP assignment in Currie and Moretti (2008) differs from ours in that they consider FSP availability at the beginning of pregnancy and its impact on birth weight, whereas we focus on availability toward the end of pregnancy.[8]

The literature (see the review in Currie, 2009) provides few estimates of the causal impact of income on birth

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[6] For more detail, see table 1 in Hoynes and Schanzenbach (2009).

[7] The Special Supplemental Food Program for Women, Infants and Children (WIC), available to low-income pregnant women and children up to age 5 in families, was introduced in 1974. Given the timing of WIC implementation relative to FSP, there is little concern that the introduction of WIC biases our estimates of the introduction of FSP, and results limited to pre-1974 are qualitatively similar.

[8] Table 3 shows the sensitivity of our impact estimates to the timing of FSP assignment.

weight. Cramer (1995) finds that mothers with more income have higher-birth-weight babies, although income is identified cross-sectionally. Kehrer and Wolin (1979) find evidence that the Gary Income Maintenance Experiment may have improved birth weight. However sample sizes are small ($N = 404$ births), and although positive effects were found for woman as being and high risk for low birth weight (young, smokers, short birth interval), perverse effects were found for woman classified as being of low risk low birth weight. Currie and Cole (1993), using IV and mother-fixed effects estimators, find that AFDC income leads to improvements in birth weight. Baker (2008) uses the 1993 expansion in the EITC, which disproportionately benefited families with two or more children, finding a 7 gram increase in the birth weight of subsequent children. In general, the literature has been plagued by imprecise estimates due to small sample sizes as well as a lack of well-identified sources of variation in income. As a result, we argue that our paper provides some of the best evidence to date on the impact of income on birth outcomes.

IV. Food Stamps and Infant Health

The FSP introduction represents an exogenous and sizable increase in income for the poor. Canonical microeconomic theory predicts that in-kind transfers like food stamps will have the same impact on spending as an equivalent cash transfer for consumers who are inframarginal. Hoynes and Schanzenbach (2009) use the same FSP rollout identification approach and data from the PSID to examine the impacts of food stamps on food expenditures; they find that recipients of food stamps behave as if the benefits were paid in cash. Therefore, not only can we think of the FSP introduction as a large income transfer, we can think of it as for the most part the equivalent of a cash income transfer.

With this framing, an increase in income could lead to changes in infant health through many channels. We would expect that spending on all normal goods would increase, therefore leading to increases in food consumption regardless of whether the benefits are paid in cash or in kind. We have little information on how particular subcategories of food demand change with FSP availability: Hoynes and Schanzenbach (2009) are able to measure impacts on total food expenditures, but cannot provide information on the quantity or quality of food consumed (or other goods).

The medical literature on the determinants of birth weight provides a useful structure for thinking about the possible channels for the health effects of the FSP. As Kramer (1987a, 1987b) suggested, birth weight is usefully decomposed into that related to the gestation length (prematurity, or GL) and growth conditional on gestation length (intrauterine growth, or IUG). Of the two, GL is thought to be more difficult to manipulate, though empirically more important than IUG in affecting birth weight in developed countries (Kramer, 1987a, 1987b). Maternal nutrition and cigarette smoking are the two most important determinants

of IUG that are potentially modifiable (Kramer, 1987a, 1987b). Finally, there is evidence that birth weight is generally most responsive to nutritional changes affecting the third trimester of pregnancy.[9] Kramer (1987a) writes, "It is important to analyze additional health measures in addition to birth weight: 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" (p. 510).

We examine impacts on neonatal mortality because it is commonly linked to the health environment during pregnancy; it is therefore plausible that FSP transfers may have been a factor. Estimates from Almond, Chay, and Lee (2005) indicate that a 1 pound increase in birth weight causes neonatal mortality to fall by 7 deaths per 1,000 births, or 24%. Postneonatal mortality, by contrast, is viewed as being more determined by postbirth factors.[10]

This discussion suggests that we would expect FSP to affect birth weight and neonatal mortality but not necessarily gestational length. One obvious channel for food stamp impacts is through improvements in nutrition. The introduction of the FSP transfer increases total family resources and is predicted to increase the quality and quantity of food consumed, thereby leading to improvements in infant health. The increased transfer income could also encourage behaviors that could harm infant health, such as smoking or drinking.[11] Health improvements may work through other channels as well, for instance, reducing stress (such as financial stress) experienced by the mother, which itself may have a direct impact on birth weight. We explore these issues by separately testing for FSP impacts on length of gestation and birth weight and by exploring the sensitivity of our impact estimates to the timing of FSP assignment by pregnancy trimester.

Overall, we expect that access to the FSP should improve infant health. The same forces that improve infant health, however, could also lead to a change in the composition of births. In particular, if improvements in fetal health lead to fewer fetal deaths, there could be a negative compositional effect on birth weight from the improved survivability of marginal fetuses. This could bias downward the estimated

[9] See the literature review of Rush et al. (1980). For example, the cohort exposed to the Dutch famine in the third trimester had lower average birth weight than cohorts exposed earlier in pregnancy (Painter, Rosebooma, & Bleker, 2005).

[10] The initial health at birth is generally much better among infants who die in the postneonatal period than among infants dying in the first month of life. For example, while 72% of all neonatal deaths had a low birth weight (below 2,500 grams), only 20% of all postneonatal deaths were low-birth-weight infants (Starfield, 1985). Postneonatal deaths tend to be caused by negative events after birth, most often by infectious diseases and accidents (Grossman & Jacobowitz, 1981). Further, postneonatal deaths may be more responsive to hospital access than neonatal deaths (see Almond, Chay, & Greenstone, 2007).

[11] Although recipients cannot purchase cigarettes directly with FSP benefits, the increase in resources to the household may increase cigarette consumption, which would work to reduce birth weight.

effects of the FSP on birth weight and infant mortality.[12] In addition, if FSP introduction leads to increases in fertility for disadvantaged women, this could also lead to negative compositional effect and a subsequent downward bias on the estimates.[13] To evaluate such channels, we test for impacts of the FSP on total births (finding no effect).

V.   Data

The data for our analysis are combined from several sources. The key treatment or policy variable is the month and year that each county implemented a food stamp program, which comes from USDA annual reports on county food stamp caseloads (USDA, various years). These administrative FSP data are combined with two microdata sets on births and deaths from the National Center for Health Statistics. In some cases, we augment the core microdata with digitized print vital statistics documents to extend analysis to the years preceding the beginning of the microdata. These data are merged with other county-level data from several sources.

A.   Vital Statistics Natality Data

These data are coded from birth certificates and are available beginning in 1968. Depending on the state-year, these data are either a 100% or 50% sample of births, and there are about 2 million observations per year. Reported birth outcomes include birth weight, gender, plurality, and (in some state-years) gestational 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, including age and race of the mother and (in some states and years) mother's education and marital status. Online appendix table 1 provides information on the availability of these variables over time.

We use the natality data and collapse the data to county-race-quarter cells covering the years 1968 to 1977. We use quarters (rather than months) to keep the sample size manageable. The results are unchanged if we instead use county-race-month cells. We end the sample in 1977, two years after all counties have implemented the FSP and before the program changes enacted in 1978 led to increases in take-up.

Unfortunately, natality microdata are available only beginning in 1968. By 1968, half of the population lived in counties with on FSP in place. In the interest of examining the full FSP rollout, we obtained annual print vital statistics documents and digitized the available data. With these print documents, we augment the microdata with counts of the total number of births by county and year (not available by race) for 1959 to 1967 and counts of births by birth weight ranges by state, race and year (not available by county) for 1959 to 1967.[14]

B.   Vital Statistics Death Data

These data are coded from death certificates and are available beginning in 1959. The data encompass the universe of death certificates (except in 1972, when they are a 50% sample) and report the age and race of the decedent, the cause of death, and the month and county of death. We collapse the data to county-race-quarter cells covering the years 1959 to 1977.

Our mortality measure is the neonatal mortality rate, defined as deaths in the first 28 days of life per 1,000 live births. We focus on deaths from all causes, as this gives us the most power (further cutting of the county-quarter-race cells by detailed cause of death leads to many very thin cells) and is unaffected by changes in the coding of cause of death (conversion from ICD-7 to ICD-8) in 1968. We have attempted to identify causes of death that could be affected by nutritional deficiencies and also present results for these and other deaths.[15] We consider nutritional causes both because the FSP was targeted at those in nutritional risk and widespread concerns about nutritional status among the poor during this period. Online appendix table 2 lists the broad categories for cause of death.

Our main neonatal results use the natality microdata to form the denominator (live births in the same county-race-quarter). This limits the sample to the years 1968 to 1977. In an extension, we use the digitized vital statistics documents and county-year counts of births to construct the denominator for live births and therefore neonatal death rates (for all races) for 1959 to 1977.[16]

C.   County Population Data

The SEER population data are used to construct estimates of the population of women ages 15 to 44 by county-race-year.[17] These are used with the natality data to con-

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[12] The estimates described in table 5 imply an imprecise 1% to 2% increase in the number of births among the treated. If we assume this increase is accounted for by reductions in early prenatal (embryonic) mortality, only to appear as deaths after birth during infancy, this would imply nearly a doubling of the infant mortality rate, which stood close to 2% nationally in 1970. Such an increase is not observed and would obviously overwhelm any reductions in infant mortality among those who would have survived until birth absent the FSP. That said, our data clearly do not allow us to distinguish between births that reflect a prevented embryonic or fetal death versus induced conceptions. But the magnitudes involved suggest that postponement of intrauterine mortality to the first year of life could not have been the norm or the infant mortality rate would have risen substantially. Thus, if we take the table 5 point estimates at face value (despite the large standard errors), either mortality was postponed beyond infancy or the number of conceptions increased.

[13] The existing literature suggests that the elasticity of fertility with respect to additional transfers from income support programs is very small (Moffitt, 1998).

[14] For historical vital statistics documents, see http://www.cdc.gov/nchs/products/pubs/pubd/vsus/1963/1963.htm.

[15] We thank Karen Norberg for helping us identify the cause of death classifications. We are responsible for any classification errors.

[16] We need quarterly births by race-county to match the quarterly deaths in the numerator. We use the distribution of births by quarter for each county in 1968 and assume that quarterly pattern holds for all years 1959–1967. In practice the "seasonality" of births across quarters is minimal.

[17] See National Cancer Institute, http://seer.cancer.gov/popdata/download.html.

struct fertility rates, defined as births per 1,000 women ages 15 to 44. Our main results use fertility rates by county-race-quarter for 1968 to 1977. We also use the digitized annual counts of births by county to construct fertility rates by county-year (not race, not quarter) for the full period 1959 to 1977.

D. County Control Variables

The 1960 City and County Data Book, which compiles data from the 1960 Census of Population and Census of Agriculture, is used to measure economic, demographic, and agricultural variables for the counties' pretreatment (before FSP is rolled out) period. In particular, we use the percentage of the 1960 population that lives in an urban area, is black, is less than 5 years old, is 65 years or over, has income less than $3,000 (in 1959 dollars), the percentage of land in the county that is farmland, and log of the county population. We use the Bureau of Economic Analysis, Regional Economic Information System (REIS) data to construct annual, county real per capita income, and government transfers to individuals, 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.[18] These data are available electronically beginning in 1968. We extended the REIS data to 1959 by hand-entering data from microfiche for 1959, 1962, and 1965 to 1968.[19]

VI. Methodology

We estimate the impact of the introduction of the FSP on county-level birth outcomes, infant mortality, and fertility, separately by race. Specifically, we estimate the following model:

$$Y_{ct} = \alpha + \delta FSP_{ct} + \beta CB60_c \times t + \gamma X_{ct} + \eta_c + \delta_t + \mu_{st} + \varepsilon_{ct}. \quad (1)$$

Y_{ct} (race suppressed) is a measure of infant health or fertility defined in county c at time t. In all specifications, we include unrestricted fixed effects for county η_c and time δ_t.

We examine the sensitivity to including state-by-year fixed effects μ_{st} or county-specific linear time trends, which are not shown in equation (1).

FSP_{ct} is the food stamp treatment variable equal to 1 if the county has a food stamp program in place. The timing of the treatment dummy depends on the particular outcome variable used. For the analysis of births, we assign $FSP = 1$ if there is an FSP in place at the beginning of the quarter prior to birth to proxy for beginning of the third trimester.[20] 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. However, we explore the sensitivity to changing the timing of the FSP treatment. Neonatal deaths are thought to be tied primarily to prenatal conditions, and we therefore use the same FSP timing (we use the age at death and measure the FSP as of three months prior to birth, to proxy for the beginning of the third trimester). We have less guidance for the correct timing for FSP treatment for fertility; we explore FSP availability between three quarters prior to birth (to proxy for conception) and seven quarters prior to birth.

The vector X_{ct} contains the annual county-level controls from the REIS, including real per capita transfers and the log of real annual county per capita income. $CB60_c$ are the 1960 county characteristics, which we interact with a linear time trend to control for differential trends in health outcomes that might be correlated with the timing of FSP adoption.

We consider several outcome variables in our main specifications. First, using the natality data, we measure infant health at birth as continuous mean birth weight in grams and fraction low birth weight (less than 2,500 grams, or about 5.5 pounds). These measures are means within county-race-quarter. Second, using the mortality data, we examine impacts on neonatal mortality rates (per 1,000 live births) for all causes and for those likely to be affected by nutritional deficiencies.

All estimates are weighted using the number of births in the county-race-quarter, and the standard errors are clustered by county. Further, to protect against estimation problems associated with thinness in the data, for the natality (mortality) analysis, we drop all county-race-quarter cells where there are fewer than 25 (50) births.[21] The results are not sensitive to this sample selection. We also drop Alaska because of difficulties in matching FSP service areas with counties.

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[18] Beginning in 1969, the REIS data permit more detailed categories for tabulating government transfers (including the ability to capture Medicaid spending). However, because the natality data begin in 1968 and the mortality data begin in 1959, we have adopted these three categories. In analyses of the data limited to 1969 and after, the results are robust to adding more detailed categories. The REIS data also measure food stamp transfer payments, but for obvious reasons, we do not use this as a control in our model. We have, however, used the REIS data as a check on our USDA-measured county food stamp start dates. REIS-measured per capita spending on FSP sharply increases precisely at the USDA-measured implementation date. In the year prior to FSP introduction, 99% of counties report no spending on FSP; in the year of introduction, this falls to 1.3% and is less than 0.3% in subsequent years (online appendix table 3).

[19] We used linear interpolation to fill in the missing years. We thank Gary Kennedy of the Bureau of Economic Analysis for providing the REIS data microfiche.

[20] To be precise, because we collapse the data to the county-quarter, the FSP variable can sometimes equal something other than a 0 or 1. The natality data are available at the monthly level, and we use that to assign FSP status as of three months prior to birth (proxy for beginning of the third trimester). When the data are collapsed to the county-quarter, this policy variable is averaged among the three months of observations in that cell. Therefore, the policy variable ranges from 0 to 1, with most values at 0 or 1.

[21] Neonatal mortality rates average 12 (19) per 1,000 births for whites (blacks) during our sample period. We use a higher threshold for the mortality analysis because of the low incidence of infant mortality.

TABLE 1.—IMPACTS OF FOOD STAMP INTRODUCTION ON BIRTH OUTCOMES, BY RACE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Birth Weight (in Grams)				Fraction below 2,500 Grams			
	A: Whites							
Average FSP (0/1)	2.039	2.635	2.089	2.175	−0.0006	−0.0006	−0.0006	−0.0006
	(0.947)*	(0.896)**	(1.039)*	(0.975)**	(0.0003)*	(0.0003)*	(0.0003)*	(0.0004)
% impact (coef/mean)	0.06%	0.08%	0.06%	0.06%	−1.02%	−1.02%	−0.97%	−0.97%
Estimate inflated	15.68	20.27	16.07	16.73	−0.0047	−0.0047	−0.0045	−0.0045
% impact inflated	0.47%	0.61%	0.48%	0.50%	−7.82%	−7.82%	−7.44%	−7.44%
Observations	97,785	97,785	97,785	97,785	97,785	97,785	97,785	97,785
R^2	0.54	0.55	0.55	0.56	0.17	0.17	0.18	0.19
Mean of dependent variable	3,350	3,350	3,350	3,350	0.06	0.06	0.06	0.06
	B: Blacks							
Average FSP (0/1)	3.454	4.120	5.466	1.665	−0.0015	−0.0016	−0.0019	−0.0009
	(2.660)	(2.317)	(2.579)*	(2.330)	(0.0010)	(0.0010)	(0.0012)	(0.0012)
% impact (coef/mean)	0.11%	0.13%	0.18%	0.05%	−1.13%	−1.22%	−1.49%	−0.68%
Estimate inflated	26.57	31.69	42.05	12.80	−0.0113	−0.0122	−0.0149	−0.0068
% impact inflated	0.86%	1.02%	1.36%	0.41%	−8.70%	−9.41%	−11.48%	−5.21%
Observations	27,374	27,374	27,374	27,374	27,374	27,374	27,374	27,374
R^2	0.32	0.33	0.34	0.35	0.15	0.15	0.17	0.18
mean of Dependent variable	3,097	3,097	3,097	3,097	0.13	0.13	0.13	0.13
1960 CCDB × linear time	X	X	X		X	X	X	
REIS controls	X	X	X	X	X	X	X	X
County per capita real income	X	X	X	X	X	X	X	X
Year quarter fixed effects	X	X	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X	X	X
State × linear time		X				X		
State × year fixed effects			X				X	
County × linear time				X				X

Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The treatment is assigned as of three months prior to birth (proxy for beginning of the third trimester). The estimation sample includes means by race-county-quarter for years including 1968–1977 where cells with fewer than 25 births are dropped. In addition to the fixed effects, controls include 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban, age below 5, age above 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.

VII. Results for Natality

Table 1 presents the main results for mean birth weight and the fraction of births that are low birth weight (LBW) for 1968 to 1977. Results are presented separately for whites and blacks. For each outcome, we report estimates from four specifications with different controls. Column 1 includes county and time (year × quarter) fixed effects, county per capita income, REIS county-level per capita transfers, and 1960 county characteristics interacted with linear time. The remaining columns control for trends in three ways: column 2 with state-specific linear time trends, column 3 with unrestricted state-by-year fixed effects, and (4) with county-specific linear time trends. In this and all subsequent tables, the number of observations refers to county-quarter cells.[22]

The first four columns in panel A report the impact of having FSP in place in the third trimester of pregnancy on mean 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. The results are extremely robust across specifications, including controlling for county-specific linear time trends. When the estimated coefficient is divided by mean birth

weight, the resulting effect size is a 0.06% to 0.08% increase in birth weight, labeled in this and subsequent tables as "% Impact (coef/mean)". As shown in panel B, the impact of FSP exposure on birth weight is 50% to 150% larger for blacks than whites. That, combined with a smaller average birth weight for blacks, implies an impact between 0.1% and 0.2% 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 are valid estimates of the population impact of FSP, we also want to know the impact among FSP recipients (treatment on the treated). To calculate the implied impact on those who take up the FSP, we divide the parameters by an estimate of the FSP participation rate for this sample.[23] 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 "Estimate, inflated") is between 15 and 20 grams for whites and 13 to 42 grams for blacks. The estimate expressed as a percentage of mean birth weight (labeled "% Impact inflated") is between 0.5% and 0.6% for whites and between 0.4% and 1.4% for blacks.

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[22] Note that with 3,142 counties and 40 quarters of data, the maximum number of observations would be about 125,000. As described above, we drop cells with fewer than 25 births. This reduces the sample of blacks much more than whites because blacks are more geographically concentrated. Despite dropping many counties, this sample represents 98% of white births and 94% of black births.

[23] We do not have information about food stamp participation in the natality data or sufficient data to impute eligibility (for example, income). Instead, we use the 1980 Current Population Survey and calculate FSP participation rates for women with a child under 5 years old. (Participation rates look very similar if we alternatively use the presence of a child below age 1 or 3.) The estimated participation rate for women with young children (under age 5) is 0.13 for whites and 0.41 for blacks.
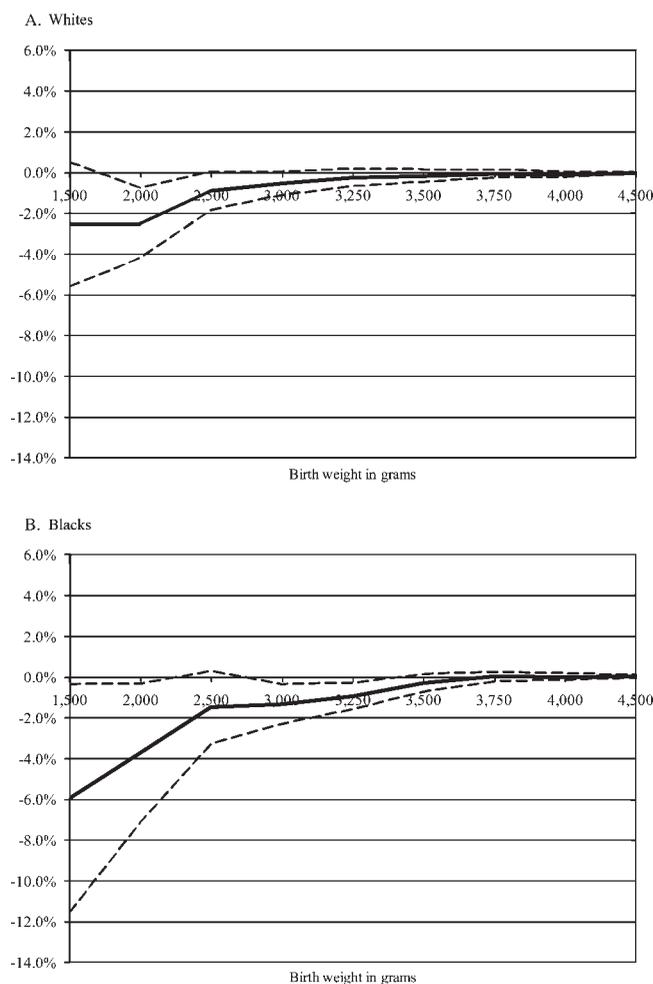
The results for birth weight (and the other outcomes described below) are very robust to adding more controls to the model. We view the specification with state-by-year unrestricted fixed effects as very encouraging, as we have controlled for a whole host of possibly contemporaneous changes to labor markets, government programs, and other things that vary at the state-year level. While not shown here, the county-level variables for government transfers and pretreatment variables do little to change the results. This provides further evidence that the food stamp rollout is exogenous, thereby validating the research design. Finally, we also find the results robust to adding county linear time trends (with some reduction for blacks). On the downside, the poor explanatory power of our control variables in predicting the timing of FSP (described in section II) means that the precision of our impact estimates is not noticeably improved by including these regression controls. For the remainder of the tables, we adopt the specification with state-by-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 5 through 8 repeat the exercise, this time with the fraction low birth weight (less than 2,500 grams) as the dependent variable. Exposure to FSP reduces LBW by a statistically significant 1% for whites (7–8% when inflated by participation rate) and a less precisely estimated 0.7% to 1.5% for blacks (5% to 12% when inflated by participation rate).

To further investigate the impact of the FSP on the distribution of birth weight, we estimated a series of models relating FSP introduction to the probability that birth weight is below a given gram threshold: 1,500; 2,000; 2,500; 3,000; 3,250; 3,500; 3,750; 4,000; 4,500 (Duflo 2001). We use the specification in column 3 with state-by-year fixed effects; the estimates and 95% confidence intervals are presented in Figure 3 (we plot "% Impacts [coef/mean]" not inflated by program participation). Figure 3A displays the results and confidence intervals for whites. We find that the largest percentage reduction in probability of birth weight below a certain threshold comes at very low thresholds of 1,500 and 2,000 grams. The impacts become gradually smaller as the birth weight threshold is increased to 2,500 grams and above, until there is no difference for births below 3,750 grams. Results are larger for blacks (figure 3B), showing a 6% decrease in the probability of a birth less than 1,500 grams, and an impact that declines at higher birth weights.[24]

FIGURE 3.—EFFECTS OF FSP IMPLEMENTATION ON DISTRIBUTION OF BIRTH WEIGHT, PERCENTAGE IMPACTS (COEFFICIENT/MEAN)

A. Whites

B. Blacks

The graph shows estimates and 95% 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 3 in table 1.

Online appendix table 4 presents estimates for three additional outcome variables: the fraction of births that are less than 1,500 grams, have gestation length less than 37 weeks (preterm births), and are female. These results show that FSP leads to a small and statistically insignificant decrease in preterm births and the fraction of births that are female. While small they are and statistically insignificant, this is consistent with recent work finding that prenatal nutritional deprivation tips the sex ratio in favor of girls (Mathews, Johnson, & Neil, 2008).[25]

One limitation of these results is that microdata on births by county are available only starting in 1968, at which point almost half of the population was already covered by the

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[24] In order to gauge the magnitude of these effects, it is useful to compare the estimated effects to those implied by the previous literature. Cramer (1995) finds that a 1% change in the income-to-poverty ratio leads to a 1.05 gram increase in mean birth weight. The Hoynes and Schanzenbach (2009) estimates of the magnitude of food stamp benefits are $1,900 annually for participants (in 2005 dollars). Scaling those to match the units available in the literature (and treating FSP benefits as their face-value cash equivalent) implies that food stamps increased the family income-to-poverty ratio of participants by 15%. The implied treatment-on-treated effect would therefore be approximately 16 grams, which is quite similar to the effects found in table 1.

[25] In results not shown here, we find that birth-weight models are little changed by controlling for gestation (known as an IUG model). We also estimated models where the dependent variable is the fraction of births below a gestation-varying threshold (known as small-for-gestational-age models; Fenton, 2003). These models yielded results very similar to the LBW regressions.

TABLE 2.—IMPACT OF FOOD STAMP INTRODUCTION ON BIRTH OUTCOMES, BY QUARTILE OF POVERTY RATES

	(1) Low-Poverty Counties (Lowest Quartile)	(2)	(3) High-Poverty Counties (Highest Quartile)	(4)
	Birth Weight	Low Birth Weight	Birth Weight	Low Birth Weight
Average FSP (0/1)	1.871	−0.001	3.409*	−0.0012*
	(2.013)	(0.001)	(1.750)	(0.0006)
% impact (coef/mean)	0.06%	−1.23%	0.10%	−1.50%
Observations	8,339	8,339	56,055	56,055
R^2	0.78	0.38	0.56	0.26
Mean of dependent variable	3333	0.07	3303	0.08
Subsample population	0.23	0.23	0.26	0.26
1960 CCDB × linear time	X	X	X	X
REIS controls	X	X	X	X
County per capita real income	X	X	X	X
Year × quarter fixed effects	X	X	X	X
County fixed effects	X	X	X	X
State × year fixed effects	X	X	X	X

Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The treatment is assigned as of three months prior to birth. The estimation sample includes means by county-quarter for years including 1968–1977 where cells with fewer than 25 births are dropped. Controls include county, year × quarter and state × year fixed effects, 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban, under age 5, over 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. Quartiles are assigned using 1970 county poverty rates (weighted using county population).

FSP. In online appendix table 5 we use data from 1959 to 1977 to examine the impact of the FSP rollout on low birth weight and very low birth weight. To push the period back to 1959, we are limited to use of data at the state-race-year level (see the discussion in section V). Controls include state and year fixed effects, REIS variables, and state-specific linear time trends; standard errors are clustered on state.[26] We first present results for 1968 to 1977, where the data are identical to those used in table 1 but are collapsed to the state level. The results show imprecise but qualitatively similar effects of FSP measured with this noisier treatment variable. (For example, the county analysis in table 1 shows a −1.0 percent impact on LBW for whites and −1.5 percent for blacks compared to −0.4 percent for whites and −1.6 percent for blacks for the state and year data in online appendix table 5). We then show the results for the full period (1959–1977) and the post-pilot program period (1964–1977). Whenever estimating models for the full FSP ramp-up period, we look separately at the period from 1964 because the pilot counties were clearly not exogeneously chosen. Using these earlier (but more aggregated) data, we get qualitatively similar (and statistically indistinguishable) results across the different time periods, suggesting that missing the pre-1968 period in our main results may not qualitatively affect our conclusions.

A. Impacts by Likelihood of Treatment

We next explore whether the impacts of the FSP are larger among subsets of the sample that are more likely to be affected by the FSP. The natality data include education of

[26] To construct state-level FSP treatment, we use the 1968 counts of number of births by county-month and calculate (for each state and year using the program variables) the percentage of births in the state that were in counties with FSP in place three months prior to birth.

the mother and presence of the father, but because of missing data (not all states collected this information in earlier years), we lose a substantial fraction of the sample (see, online appendix table 1). Nonetheless, we have estimated models by age of mother, education of mother, and presence of the father (results not shown). Overall, the results showed that the impacts are larger for older mothers (age 25 and over). None of the education results are statistically significant. This analysis did reveal that black mothers 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).

In lieu of detailed demographic variables, in table 2 we break counties into quartiles based on 1970 poverty rates, where we expect larger impacts in high-poverty counties. The results are quite striking: the gains are concentrated in the highest-poverty counties. Large, statistically significant effects are present in the highest-quartile poverty counties, while smaller and insignificant effects are presents in the lowest-poverty counties. (Due to the relatively large standard errors, we cannot reject that they are equal.)

There is some suggestion in the historical accounts that the impact might be different across geographic regions or might differ by race across regions. In particular, participation in the program in the early years (after the county's initial adoption of FSP) was probably higher in urban counties and in the North. Barriers to accessing food stamps might have also differed between North and South and may have interacted with race (Citizens' Board, 1968). Table 3 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. These differences parallel the regional trends: Blacks

TABLE 3.—IMPACTS OF FSP INTRODUCTION ON INFANT OUTCOMES, BY GEOGRAPHY

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	South		Non-South		Urban Counties		Nonurban Counties	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
	A: Whites							
Aevarage FSP (0/1)	2.403	−0.0011	1.771	−0.0003	2.364	−0.0008	0.508	−0.0002
	(1.612)	(0.0005)**	(1.322)	(0.0004)	(1.247)*	(0.0004)**	(1.615)	(0.0006)
% impact (coef/mean)	0.07%	−1.57%	0.05%	−0.48%	0.07%	−1.13%	0.02%	−0.25%
Observations	44,194	44,194	53,591	53,591	32,282	32,282	65,503	65,503
Subsample population	0.29	0.29	0.69	0.69	0.73	0.73	0.25	0.25
	B: Blacks							
Average FSP (0/1)	3.527	−0.0023	7.003	−0.0009	8.371	−0.0034	−0.745	0.0023
	(3.134)	(0.0014)*	(3.992)*	(0.0022)	(2.846)**	(0.0013)**	(5.219)	(0.0023)
% impact (coef/mean)	0.11%	−1.76%	0.23%	−0.69%	0.27%	−2.59%	−0.02%	1.74%
Observations	20,837	20,837	6,537	6,537	13,090	13,090	14,284	14,284
Subsample population	0.49	0.49	0.45	0.45	0.77	0.77	0.17	0.17
1960 CCDB × linear time	X	X	X	X	X	X	X	X
REIS controls	X	X	X	X	X	X	X	X
County per capita real income	X	X	X	X	X	X	X	X
Year × quarter fixed effects	X	X	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X	X	X
State × year fixed effects	X	X	X	X	X	X	X	X

Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The treatment is assigned as of the three months prior to birth. The estimation sample includes means by county-quarter for years including 1968–1977 where cells with fewer than 25 births are dropped. Controls include county, year × quarter and state × year fixed effects, 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban, under age 5, over 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 percentage of total births that are included in the regression. Urban counties are defined as those with greater than 50% of the 1960 population living in an urban area.

saw larger reductions in low birth weight (and neonatal mortality) in the North, while whites saw larger declines in the South. The FSP impacts by South/non-South, however, are less precisely estimated than the results by urban/nonurban.[27]

B. Investigation of the Timing of Impacts

To explore the possible channels for the impacts of the FSP transfer, table 4 reestimates the mean birth weight models varying the timing of the exposure to the FSP. The baseline specification—reprinted from column 3 of table 1—assigns the policy introduction as three months prior to birth, to proxy for beginning of the third trimester. Columns 2 and 3 of table 4 moves assignment of FSP treatment to 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. Furthermore, assigning treatment at three quarters before birth (proxy for conception) yields even smaller and statistically insignificant impacts. The results in columns 4 and 5 show that conditional on third-trimester exposure, additional exposure earlier in the pregnancy has no additional benefits. Similar results are found for fraction low and very low birth weight. Recalling from section IV that the medical literature suggests that nutrition has its greatest impact on birth weight during the third trimester, we view these estimates

as suggestive that nutrition is playing an important channel for the FSP transfer's benefits. In addition, 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.[28]

To further test for spurious trending in the county birth outcomes that might be loading on to FSP, we include a one-year lead of the policy variable for each of the birth outcome variables in online appendix table 6. There is no impact of the policy lead, and the results for the main policy variable are qualitatively unchanged.

As described above, we use the month that the county implemented the FSP to measure food stamp availability during these pregnancies. If there was a lag in ramping up county food stamp programs, then our difference-in-difference estimates will underestimate the true (eventual) program impacts. The administrative ramp-up was aided by the fact that the new FSP offices were often set up in the same building as the county welfare office. To directly evaluate the ramp-up in FSP operations, figure 4 shows food stamp caseloads per capita by year relative to start year (the caseload data are available only by year). The figure separately plots caseloads for counties beginning operations in the first half versus second half of the caseload reporting year. This

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

[28] Note that the reduction in the magnitude of the birth-weight impact may explain the difference between our results and those of Currie and Moretti (2008). Their study of birth outcomes in California assigned the FSP treatment nine months prior to birth and found comparatively limited impacts on birth weight. Another explanation for larger effects in the third trimester is if initial FSP participation is concentrated there (rather than earlier).

TABLE 4.—SENSITIVITY OF BIRTH WEIGHT OUTCOMES TO CHANGING THE TIMING OF THE POLICY INTRODUCTION

Main Policy Effect:	(1) FSP—Beginning of Third Trimester	(2) FSP—Beginning of Second Trimester	(3) FSP—Beginning of First Trimester	(4) FSP—Beginning of Third Trimester	(5) FSP—Beginning of Third Trimester
Second Policy Effect:	–	–	–	FSP—Beginning of Second Trimester	FSP—Beginning of First Trimester
			A: Whites		
Average FSP (0/1)	2.085	1.696	1.288	2.556	2.434
	(1.020)**	(1.024)*	(0.993)	(1.640)	(1.268)*
Average FSP (0/1) Second policy variable	–	–	–	−0.533	−0.454
				(1.650)	(1.232)
Observations	97,785	97,785	97,785	97,785	97,785
R^2	0.55	0.55	0.55	0.55	0.55
Mean of dependent variable	3,350	3,350	3,350	3,350	3,350
			B: Blacks		
Average FSP (0/1)	5.447	4.704	2.071	5.334	8.108
	(2.532)**	(2.464)*	(2.396)	(4.596)	(3.444)**
Average FSP (0/1) Second policy variable	–	–	–	0.130	−3.515
				(4.450)	(3.268)
Observations	27,374	27,374	27,374	27,374	27,374
R^2	0.34	0.34	0.34	0.34	0.34
Mean of dependent variable	3,097	3,097	3,097	3,097	3,097
1960 CCDB × linear time	X	X	X	X	X
REIS controls	X	X	X	X	X
County per capita real income	X	X	X	X	X
Year × quarter fixed effects	X	X	X	X	X
County fixed effects	X	X	X	X	X
State × year fixed effects	X	X	X	X	X

Dependent variable is equal to birth weight in grams. 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 three months prior to the birth (to proxy for beginning of the third trimester). The alternative specifications include timing as of six months (second trimester) or nine months (first trimester) prior to birth. In specifications 4, we estimate jointly the treatment effects at the third and second trimesters, and in column 5, we estimate jointly the impacts measured at the third and first trimesters. All of the other control variables and sample definitions are described in the notes to table 1.

FIGURE 4.—PERCENTAGE OF COUNTY POPULATION ON FOOD STAMPS BY NUMBER OF YEARS SINCE PROGRAM START
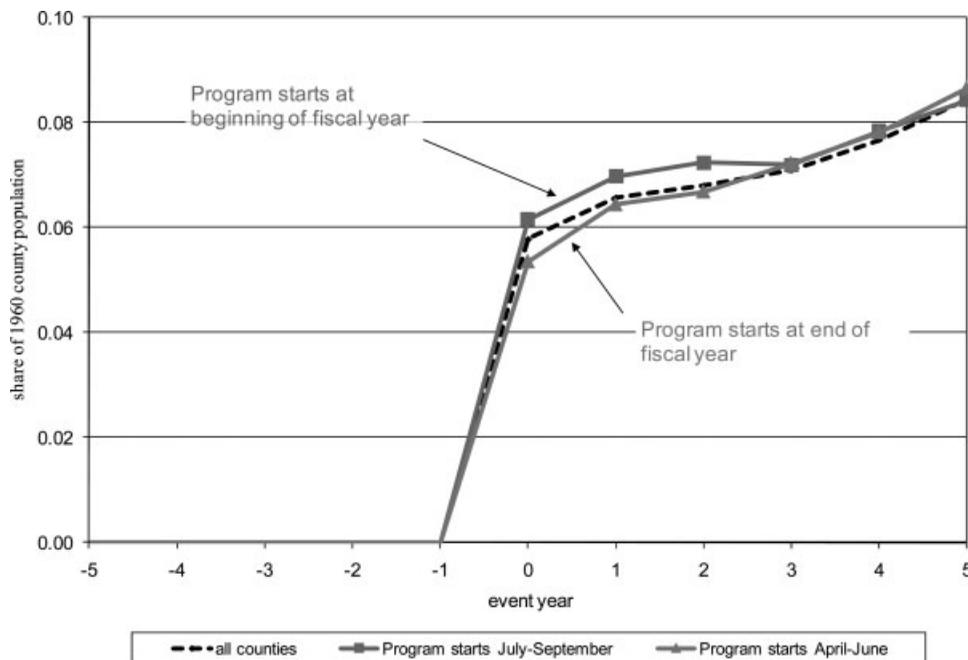
The graph is an unweighted regression of county-year food stamp caseloads on a series of dummy variables tracking year relative to county FSP implementation year. County caseload is expressed as a share of the 1960 population. Source for caseload data is USDA (various years).

figure suggests that rapid ramp-up was achieved and that the ramp-up is only slightly faster in the counties with more lead time (implementation earlier in the year). Further, note that over half of the "steady-state" caseload is achieved in the first year, even for counties that begin operation late in the reporting year.

C. Event Study

The pattern of estimates in table 4 suggests that the FSP treatment effect is identified by the discrete jump in FSP at implementation and its impact on birth weight. In particular, we showed in table 4 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 some other trends in county outcomes that are correlated with FSP start month, then we would expect less sensitivity in the 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 county linear trends do not capture.

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

$$Y_{ct} = \alpha + \sum_{i=-6}^{8} \pi_i 1(\tau_{ct} = i) + \eta_c + \delta_t$$
$$+ \gamma X_{ct} + \phi_c \times t + \varepsilon_{ct}, \qquad (2)$$

where τ_{ct} denotes the event quarter, defined so that $\tau = 0$ for births that occur in the same quarter as the FSP began operation in that county, $\tau = 1$ for births one quarter after the FSP began operation, and so on. For $\tau \leq -1$, pregnancies were untreated by a local program (births were before the program started). The coefficients are measured relative to the omitted coefficient ($\tau = -2$).[29] Our event study model includes fixed effects for county and time, county REIS variables, and county-specific linear time trends.

In order to eliminate potential compositional effects, we restrict the sample to a balanced panel of counties having births for all fifteen event quarters: six quarters before implementation and eight quarters after. Because our natality data begin with January 1968, this means we exclude from the event study analysis all counties with an FSP before July 1969.

Figure 5 plots the event and quarter coefficients from estimating equation (2) on the fraction of low-birth-weight births. The figure also reports the number of county and quarter observations in the balanced sample and the difference-in-difference estimate on this sample.[30] Panel A reports estimates for blacks and panel B for whites. These figures show an absence of a strong pretrend and evidence of a trend break at the quarter the FSP is introduced, implying an improvement in infant outcomes. That such a prompt increase in birth weight is observed with FSP inception indicates that potential confounders would have to mimic the timing of FSP rollout extremely closely. Not shown here, the event study results are nearly identical if we exclude the county controls, providing further evidence of the exogeneity of the treatment. We view this as more evidence of the validity of our identification strategy.[31]

D. Further Robustness Checks

The main results are robust to various additional specification checks. One potential concern is that the FSP introduction is correlated with unobserved county health investments (such as the expansion of access to hospitals in the South as in Almond, Chay, and Greenstone 2007) and our REIS controls fail to pick this up. To test this, we use the natality data to estimate the impact of FSP implementation on the fraction of births in a hospital or attended by a physician. These results indicate very small and statistically insignificant improvements with FSP implementation (online appendix table 7).

Finally, the same forces that improve infant health could also lead to greater survival of low-birth-weight fetuses. In addition, the FSP may lead to increased fertility among disadvantaged women (if children are a normal good). Both factors, through endogenous sample selection, could bias the estimates downward. We consider this by evaluating whether FSP introduction is associated with any change in live births. The dependent variable is the number of births in the race, county, and quarter divided by the number of women aged 15 to 44, and the regressions are weighted by the population of women in each cell. Table 5 presents several estimates, which vary depending on the timing of the FSP treatment: between three quarters prior to birth (proxy for conception) and seven quarters prior to birth (one year prior to conception). Across the table, we find positive but very small and statistically insignificant effects of FSP on births. When these point estimates are inflated by the FSP participation rate, the estimate of the treatment on the treated is about 1% for whites and 2% for blacks. When we stratify the results by quartiles of county poverty rates, we also find small and statistically insignificant impacts among those living in the highest poverty counties (online appendix table 8).

VIII. Mortality Results

Table 6 shows the main results for neonatal mortality rate for 1968 to 1977. We present three outcomes: death rate for all causes, deaths possibly due to nutritional deficiencies,

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[29] Because of the discrete nature of the event study model, the τ's are formed by aggregating months to a quarter. For example, if the FSP started (or birth occurred) in January, February, or March 1970, then the FSP started (or birth occurred) in 1970 quarter 1. Therefore when $\tau = 0$ (birth quarter = policy commencement quarter), the pregnancy could actually have been treated for between zero and three months.

[30] The difference-in-difference estimate is comparable to the results presented in table 1. We present them here because the samples used for the event study differ from the main results (due to balancing of the sample).

[31] Similar patterns are observed when the dependent variable is average birth weight (online appendix figure 2) and the share of births below 1,500 grams (available on request).

FIGURE 5.—EFFECTS OF FSP IMPLEMENTATION ON LOW BIRTH WEIGHT: RESULTS FOR EVENT STUDY ANALYSIS

A. Blacks

B. Whites
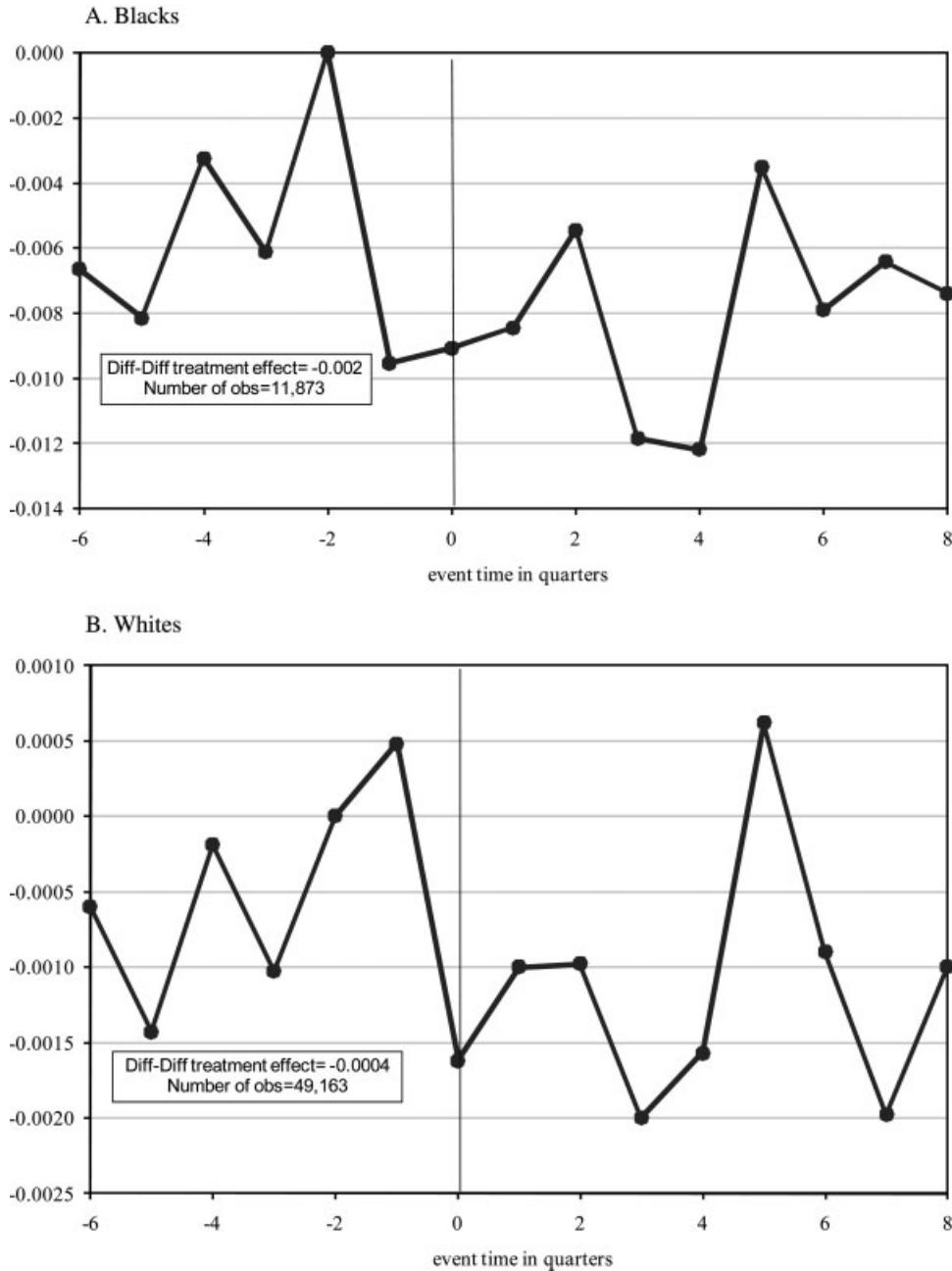
Each figure plots coefficients from an event-study analysis. Coefficients are defined as quarters relative to the quarter the FSP is implemented in the county. The sample is a balanced county sample, where a county is included only if there are six quarters of pre- and eight quarters of post-implementation 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. The "diff-in-diff treatment effect" is comparable to the results presented in table 1. We present them here because the samples used for the event study differ from the main results.

and other deaths (for definition see the data section and online appendix table 2). Because neonatal deaths are thought to be related primarily to prenatal conditions (particularly prior to major technological advances in neonatal care in the 1970s and 1980s), we time the FSP treatment as of a quarter prior to birth (to proxy for the beginning of the third trimester). In these models, we drop any race-county-quarter cell where there are fewer than fifty births. Results are weighted by the number of births in the cell.

The neonatal mortality rate averages about twelve deaths per 1,000 births for whites and nineteen for blacks, with about half of the deaths where the cause of death indicates those possibly affected by nutritional deficiencies. The results for whites and blacks show that the FSP leads to a reduction in infant mortality, with larger impacts for deaths possibly affected by nutritional deficiencies. None of the estimates, however, are statistically significant. Overall, the effect of the treatment on the treated (percentage impact,

TABLE 5.—IMPACT OF FSP INTRODUCTION ON FERTILITY RATE (BIRTHS PER 1,000 WOMEN AGES 15–44)

	(1)	(2)	(3)	(4)	(5)
	FSP Implemented as of X Quarters prior to Birth				
	3 Quarters	4 Quarters	5 Quarters	6 Quarters	7 Quarters
	A: Whites				
Average FSP (0/1)	0.013	−0.004	0.007	0.031	0.035
	(0.078)	(0.074)	(0.071)	(0.074)	(0.070)
% impact (coef/mean)	0.06%	−0.02%	0.04%	0.16%	0.18%
% impact, inflated	0.50%	−0.14%	0.28%	1.22%	1.40%
Observations	120,293	120,293	120,293	120,293	120,293
Mean of dependent variable	19.40	19.40	19.40	19.40	19.40
	B: Blacks				
Average FSP (0/1)	0.211	0.157	0.276	0.307	0.227
	(0.221)	(0.206)	(0.193)	(0.190)	(0.183)
% impact (coef/mean)	0.80%	0.60%	1.05%	1.17%	0.86%
% impact, inflated	1.75%	1.30%	2.29%	2.54%	1.88%
Observations	44,044	44,044	44,044	44,044	44,044
Mean of dependent variable	26.24	26.24	26.24	26.24	26.24
1960 CCDB × linear time	X	X	X	X	X
REIS controls	X	X	X	X	X
County per capita real income	X	X	X	X	X
Year × quarter fixed effects	X	X	X	X	X
County fixed effects	X	X	X	X	X
State × year fixed effects	X	X	X	X	X

Each parameter is from a separate regression of the outcome variable on the food stamp implementation dummy. The columns vary by the timing of the FSP implementation. The estimation sample includes means by race-county-quarter for 1968–1977. Controls include county, year-by-quarter and state-by-year fixed effects, 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban, under age 5, over age 65, and 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 population 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.

TABLE 6.—IMPACT OF FSP ON NEONATAL MORTALITY RATE (DEATHS PER 1,000 LIVE BIRTHS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All Deaths			Deaths Linked to Nutrition			Other Deaths		
	A: Whites								
Average FSP (0/1)	−0.0625	−0.0158	−0.0806	−0.0492	−0.0784	−0.0376	−0.0133	0.0626	−0.0430
	(0.1050)	(0.1194)	(0.1242)	(0.0771)	(0.0839)	(0.0913)	(0.0834)	(0.0936)	(0.0960)
% impact (coef / mean)	−0.52%	−0.13%	−0.67%	−0.79%	−1.25%	−0.60%	−0.23%	1.09%	−0.75%
% impact, inflated	−4.01%	−1.01%	−5.17%	−6.04%	−9.63%	−4.62%	−1.79%	8.39%	−5.76%
Observations	73,577	73,577	73,676	73,577	73,577	73,676	73,577	73,577	73,676
R^2	0.16	0.16	0.18	0.10	0.11	0.13	0.12	0.12	0.15
Mean of dependent variable	12.00	12.00	12.00	6.26	6.26	6.26	5.74	5.74	5.74
	B: Blacks								
Average FSP (0/1)	−0.3898	−0.0067	−0.6551	−0.4128	−0.3098	−0.4233	0.0231	0.3032	−0.2317
	(0.4095)	(0.4610)	(0.4793)	(0.2865)	(0.2953)	(0.3334)	(0.2729)	(0.3348)	(0.2977)
% impact (coef / mean)	−2.06%	−0.04%	−3.46%	−4.58%	−3.43%	−4.69%	0.23%	3.06%	−2.34%
% impact, inflated	−4.47%	−0.08%	−7.52%	−9.95%	−7.47%	−10.20%	0.51%	6.65%	−5.08%
Observations	17,655	17,655	17,695	17,655	17,655	17,695	17,655	17,655	17,695
R^2	0.42	0.44	0.43	0.34	0.36	0.36	0.26	0.29	0.28
Mean of dependent variable	18.94	18.94	18.94	9.02	9.02	9.02	9.91	9.91	9.91
1960 CCDB × linear time	X	X		X	X		X	X	
REIS controls	X	X	X	X	X	X	X	X	X
County per capita real income	X	X	X	X	X	X	X	X	X
Year × quarter fixed effects	X	X	X	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X	X	X	X
State × linear time	X			X			X		
State × year fixed effects		X			X			X	
County × linear time			X			X			X

Each parameter is from a separate regression of the neonatal mortality rate (deaths in first 28 days per 1,000 live births) on the FS implementation. The treatment is assigned as of three months prior to birth (proxy for beginning of the third trimester). The sample includes means by race-county-quarter for years including 1968–1977 where cells with fewer than fifty births are dropped. In addition to the fixed effects, controls include 1960 county variables (log of population, percentage of land in farming, percentage of population black, urban, below age 5, over 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.

inflated) for all causes is about 4% for whites and between 4% and 8% for blacks. These estimates are roughly in line with the birth weight–neonatal mortality rate relationship estimated by Almond et al. (2005): for whites, we estimate a very similar birth weight-mortality relationship, although the relationship between birth weight and mortality we estimate for blacks is substantially stronger than in Almond et al. (2005). Finally, we view the results for "other deaths"

(not affected by nutritional deficiencies), which are opposite signed and much smaller in magnitude (although again statistically significant), as favorable evidence that the mortality estimates are coming from the FSP. Online appendix table 9 separates the mortality effects by quartiles of the county poverty rate, and while imprecisely estimated finds a negative effect in the highest-poverty counties but a positive one in the lowest-poverty counties that were unlikely to experience a substantial FSP treatment.

Online appendix table 10 presents results for all races for the full period from 1959. We are unable to present results by race here because the denominator (live births by county and time) is not available by race prior to 1968. The first three columns replicate the results in table 6 for 1968 to 1977 for all races. In the subsequent columns (for 1959–1977 and 1964–1977), we find results very similar to those for 1968 to 1977. Overall, FSP implementation leads to a reduction in neonatal mortality, although not statistically significantly so.

IX. Interpretation and Conclusion

The uniformity of the FSP was designed to buffer the discretion states exercised in setting rules and benefit levels of other antipoverty 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 that researchers typically use to identify program impacts. As a result, surprisingly little is known about FSP effects.

In contrast to other major U.S. antipoverty programs, the FSP was rolled out county by county. This feature of implementation allows us to separate the introduction of food stamps from the other major policy changes of the late 1960s and early 1970s. Although FSP benefits were (and are) paid in vouchers that themselves could be used only to purchase food, because the voucher typically represented less than households spent on food (covering just the "thrifty food plan"), recipients were inframarginal and benefits were essentially a cash transfer (Hoynes & Schanzenbach, 2009).

Across the board, our point estimates show that this near-cash transfer improved infant outcomes. In particular, we find increases in mean birth weight for whites and blacks, with larger impacts estimated at the bottom of the birth weight distribution (that is, low birth weight and very low birth weight). Consistent with expectations, we find larger birth weight effects for black mothers and those living in high-poverty areas—populations where FSP participation is more common. Consistent with epidemiological studies, FSP availability in the third trimester had the largest birth weight impact. We conclude that despite not targeting pregnant women, the introduction of the FSP increased birth weight. This finding is all the more noteworthy given the

mixed success that randomized interventions have had in raising birth weights (Rush, Stein, & Susser, 1980; Lumley & Donohue, 2006).

While the point estimates for gestation length and neonatal mortality would also suggest improved health at birth, estimated effects are imprecise, despite the large samples from vital statistics data. One interpretation is that statistical power is lost when analyzing gestation length (incomplete reporting by states) and mortality (rare). Leaving the imprecision issue aside, gestation length and mortality appear less affected than the likelihood of low and very low birth weight.

At a minimum, our results indicate that the FSP had an immediate first-stage impact on newborns. Furthermore, these estimated impacts (as reflected by birth weight) are much larger in high-poverty counties. Our findings reveal that an exogenous increase in income during a well-defined period, pregnancy, can improve infant health. Future work should consider whether this FSP-induced birth weight improvement is reflected in subsequent outcomes and how poverty and birth weight may mediate this relationship.

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