The Effects of the National School Lunch Program on Education and Health*

[JOB MARKET PAPER]

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Abstract: The National School Lunch Act was passed in 1946 "as a measure of national security" because many men had been found unfit for military service in World War II due to nutrition-related health limitations. This paper estimates the effects of participating in the National School Lunch Program in the middle of the 20th century on health outcomes as an adult and on educational attainment. In addition to being of interest in its own right as an evaluation of a major government program, studying the National School Lunch Program may yield more general insight into the long-run effects of health investments as a child and could potentially provide a partial explanation for trends in health outcomes or educational attainment over time. I utilize an instrumental variables strategy that makes use of a change of the formula used by the federal government to allocate funding to the states that was phased in beginning in 1963. Identification is achieved by the fact that different birth cohorts were exposed to different degrees to the original formula and the new formula, along with the fact that the change of the formula affected states differentially by per capita income. Participation in the program as a child appears to have few lasting effects on health, which may suggest that the program was too broadly targeted at its inception and displaced food consumption that would have occurred in the absence of the program. However, the effects on educational attainment are sizable.

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I. Introduction

Section 2 of the National School Lunch Act of 1946 reads,

> It is hereby declared to be the policy of Congress, as a measure of national security, to safeguard the health and well-being of the Nation's children and to encourage the domestic consumption of nutritious agricultural commodities and other food, by assisting the States, through grants-in-aid and other means, in providing an adequate supply of foods and other facilities for the establishment, maintenance, and expansion of nonprofit school-lunch programs.

In the hearings for this Act, Major General Lewis B. Hershey testified to Congress that 16% of Selective Service registrants in World War II were rejected from service or placed in the limited service class and that malnutrition or underfeeding played a likely role in somewhere between 40% and 60% of these cases (U.S. Congress 1945). Congress felt the need to remedy this situation and, thus, the National School Lunch Program (NSLP), under which the federal government provides cash and commodity aid to states for localities to use in serving warm lunches to students, was seen as a "measure of national security." It was not clear in this era that children would receive an adequate amount to eat if they brought a lunch to school or were released from school to eat lunch at home. Therefore, a government-subsidized lunch program could potentially have had a real impact on health and, if nutrition and education are complements, may have also increased learning. On the other hand, the program was broadly-targeted at its inception, and it is not clear that the aid from such a program would find its way to the subset of the population that suffered from malnutrition.

This paper studies the historical effects of participating in the NSLP on health outcomes (such as adult height and body mass index) and educational attainment. In addition to least squares estimates, I present instrumental variables estimates that exploit a change in the funding formula determining the allocation of federal cash assistance across states. The change in the formula affected states differentially (and non-linearly) by per capita income, with wealthier states receiving relatively more funding under the later formula. In order to avoid estimates that are contaminated by changes in per capita income or population, the instrument is based on funding that would be received given a state's average characteristics over the time period. To preview the results, my analysis of data from the National Health Interview Survey uncovers few lasting effects of the NSLP on health, but I find a sizable effect of the NSLP on educational attainment using data from the Census. A potential explanation for these findings is that students would have had a similar diet in the absence of the program but that they attended school in order to purchase food at a subsidized price. An alternative interpretation is that the potential health effects have faded away by the time individuals reach adulthood but that I detect an effect on education because education is a more contemporaneous measure of the impact of the NSLP.

Estimating the effects of the NSLP is of interest in its own right as an evaluation of a major government-sponsored nutrition program. [To give an indication of the size of the program in the time period under consideration in this paper, the federal government alone spent roughly $500 million (in 2005 dollars) on the NSLP in 1947 and roughly $1 billion (in 2005 dollars) in 1973.] Uncovering the effects of the NSLP at its inception may also be relevant for developing countries that have recently adopted or are considering adopting a similar large-scale child nutrition program.[1] Moreover, the research could provide insight for the issue of the effects of health investments as a child

[1] India recently began a nationwide lunch program which, according to at least one journalistic account (Lakshmi 2005), has been successful in increasing school attendance among girls. Vermeersch (2003) reports on a randomized evaluation of a preschool breakfast program in Kenya; the program increased attendance and test scores. Jacoby (2002) shows that school feeding programs in the Philippines increased caloric intake among participants, as opposed to causing households to reallocate calories that would be consumed in the absence of the programs.

on health outcomes as an adult and the issue of trends in health outcomes over time. Thus, this paper is related to other recent papers that have used quasi-experimental methods to study historical health issues, including Almond (2006) on influenza, Bleakley (2006a) on hookworm eradication, Bleakley (2006b) on malaria eradication, and Ludwig and Miller (2006) on Head Start.

The remainder of this paper is structured as follows. Section II discusses the NSLP in more detail. Section III reviews related literature, Section IV discusses the data, Section V discusses the identification strategy, Section VI gives the empirical results, and Section VII concludes.

II. The National School Lunch Program[2]

The American school lunch has not always been the institution it is today. There were cities such as Boston and Philadelphia that operated their own school lunch programs, often with the help of volunteers or charitable organizations, as early as the late nineteenth century. But it was not until 1932 that the federal government began giving aid for school lunch programs. This aid began on a small scale and originated from New Deal agencies such as the Federal Emergency Relief Administration, the Reconstruction Finance Corporation, and the Civil Works Administration. Federal involvement expanded in 1935 with the creation of the Works Progress Administration and the National Youth Association, both of which operated programs that provided labor for school lunchrooms. In that same year, the Agricultural Adjustment Act was amended with Section 32, which instituted the donation of surplus farm commodities to school

[2] This section, as well as other parts of this paper that discuss historical details, draws on Flanagan (1969), Jones (1994), Martin (1999), and *The National School Lunch Act* (1946).

lunch programs. By 1943, the New Deal agencies had been dissolved and farm surpluses were not as large as they had previously been, but there was a desire to keep school lunch programs. Thus, federal cash assistance for school lunch programs was appropriated on a year-to-year basis from 1943 to 1946.

The NSLP was made permanent with the passage of the National School Lunch Act in 1946. Under Section 4 of the Act, cash was given from the federal government to the states according to a formula that depended on per capita income and population, and this cash was handed down by states to localities. Schools had the option of participating in the program.[3] If they chose to do so, they would receive cash and commodity aid in exchange for following program requirements, including requirements about the contents of the lunch.[4] A gradual change to a new funding formula began in the 1962-1963 school year and was fully in place for the 1965-1966 school year. This change forms the basis of my identification strategy. The formulas and the identification strategy are discussed in detail in Section V.

III. School Nutrition Programs and Health: Prior Literature

Although this is the first paper of which I am aware to view the NSLP from a historical perspective and to estimate the effects of the NSLP into adulthood, there is

[3] Not every school participated in the program at its inception. Even today, there is less than full participation among schools.

[4] At the inception of the NSLP, there were three different categories of lunches (Type A, Type B, and Type C), and they had different requirements. The requirements for a Type A lunch were "1) One-half pint of whole milk (which meets the minimum butterfat and sanitation requirements of state and local laws) as a beverage. 2) Two ounces of fresh or processed meat, poultry, cooked or canned fish, or cheese; or one-half cup cooked dry peas, beans, or soybeans; or four tablespoons of peanut butter; or one egg. 3) Six ounces of raw, cooked, or canned vegetables and/or fruit. 4) One portion of bread, muffins, or other hot bread made of whole-grain or enriched flour. 5) Two teaspoons of butter or fortified margarine." The Type B lunch had to meet requirements 1 and 4, as well as half the portions for the other requirements. The Type C lunch had to meet requirement 1. My data unfortunately do not distinguish between Type A, Type B, and Type C lunches; I return to this issue in the robustness checks.

some work on the more recent effects of the NSLP and the related School Breakfast Program (SBP). Schanzenbach (2005) studies the effect of the NSLP on obesity. She shows that participants and non-participants enter school with similar rates of obesity but a gap opens up by the spring of first grade. In addition, a regression discontinuity design exploiting a discontinuity in eligibility for a reduced price lunch at an income of 185% of the poverty level gives similar results.[5] Bhattacharya, Currie, and Haider (2006) study the effect of the SBP, which was introduced as a small-scale pilot program in 1966 and made permanent in 1975, on nutrient intake. Their difference-in-differences strategy compares outcomes between the school year and the summer for students in schools where the SBP is available and where it is not available, and their results show a beneficial impact of the program. Also, Figlio and Winicki (2005) show that schools in Virginia were altering the nutritional content of school lunches around the time of high stakes tests and that this was apparently successful in raising test scores.[6]

A potential problem with studies of child nutrition programs in the United States that use more recent data is that they run the risk of confounding the effects of different programs with one another. For example, the NSLP and the SBP not only have similar funding structures, but they also have the same income cutoffs for free lunch eligibility (130% of the poverty level) and reduced-price lunch eligibility (185% of the poverty level). In addition, the 130% figure is important for food stamp eligibility, and the 185% figure is important for WIC eligibility.[7] There are also a number of newer child nutrition

[5] Also see Anderson and Butcher (2006) for an indirect case that the nutrition policies of schools have an effect on the body mass index of students.

[6] For work regarding other aspects of the NSLP, see St. Pierre and Puma (1992) on the issues of fraud and misclassification in eligibility for free or reduced-price lunches, Gleason and Suitor (2003) on nutrient intake, Long (1991) on the effect on household food expenditures, and Dunifon and Kowaleski-Jones (2003) on factors determining participation in the NSLP.

[7] See p. 80 of Currie (2006).

programs, such as the Summer Food Service, whose effects may be confounded with those of the NSLP or the SBP. Studying a time period before these other programs existed should help isolate the effects of the NSLP.[8] Another difference between my paper and previous ones is that I estimate the longer-run effects of participation in the NSLP, although the cost is that I do not have individual-level participation data.

IV. Data

I use three datasets in this paper. The first is a dataset I assembled from various sources that contains information on funding and participation in the NSLP by state for the years 1947-1973. The second pools the five National Health Interview Surveys conducted between 1976 and 1980 (United States Department of Health and Human Services 1976-1980); this dataset consists of information on health outcomes and demographic control variables. The third dataset is the 5% sample of the 1980 Census (Ruggles et al. 2004). I merge the first dataset with the second to estimate the effects of participation in the NSLP on health; I merge the first dataset with the third to estimate the effects of participation on educational attainment. In the remainder of this section, I discuss the three data sources in more detail; additional information about the funding and participation data can be found in the data appendix.

A. Funding and Participation

The first dataset consists of information on funding and participation in the program that I collected from various sources. It also includes information on per capita income and population aged 5-17 in each state. Much of the information in this dataset comes

[8] However, it is certainly true that the effects of the program may have changed over time.

from tables showing the exact inputs and output of the NSLP funding formula. When data is unavailable in the funding tables, I use data from other sources or impute the data myself. Details are provided in the data appendix.

Two points about the data are in order. First, the participation data give the average number of lunches served in the national "peak month" of the year; this peak month is almost always November or December. Second, any student at a participating school is eligible to participate in the program; thus, my data capture full-price lunches as well as free or reduced-price lunches.[9] I should also note that, throughout this paper, I use the name of a calendar year to refer to the school year or fiscal year ending in that year.[10]

Figures 1-4 plot the funding and participation data in order to give a sense of the magnitudes involved. Figure 1 shows the national participation rate in the NSLP for each year between 1947 and 1973; the trend over time is one of increasing participation. Figure 2 shows the amount of Section 4 "general assistance" NSLP funding per child at the national level between 1947 and 1973. Funding per child tends to fall at first but then rises later. Figure 3 is a scatterplot of state participation rates in 1947 and 1973. States with higher participation rates in 1947 also tend to have higher participation rates in 1973, and states with high participation rates tend to be poorer states in the South. Figure 4 is a scatterplot for the cohort born in 1944 of the averages over the 12 years the children are in school of the state participation rate and funding per child; I use the term "exposure" to refer to this average participation rate. This figure reveals that Louisiana is

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[9] Uniform national standards for free or reduced-price lunch eligibility were not even imposed until 1972 , although Section 11 of the original text of the National School Lunch Act states, "Meals shall be served without cost or at a reduced cost to children who are determined by local school authorities to be unable to pay the full cost of the lunch."

[10] In the time period under consideration, the federal government's fiscal year began on July 1 of the previous calendar year and ended on June 30.

a huge outlier in participation. I include state effects in my regressions, but I also drop observations from Louisiana from the sample as a robustness check.

B. National Health Interview Survey

The health outcome variables and the individual-level control variables used in estimating the effects of the NSLP on health outcomes come from the National Health Interview Survey (NHIS). The NHIS is a large annual survey conducted by the National Center for Health Statistics in which a random sample of Americans is surveyed about health and demographic background information. My NHIS dataset is formed by pooling the five NHIS surveys between 1976 and 1980. I use individuals born between 1941 and 1956 inclusive in the continental 48 states, and I drop outliers in height or weight.[11] The individual-level data is matched to the participation and funding data using state of residence, as state of birth is unavailable in the NHIS. For an individual who is *a* years old in the NHIS survey taken in year *y*, I consider the individual to have been born in year *y-a*, the first year of school to be *y-a+6*, and the last year of school to be *y-a+17*. All health regressions are performed using the weights provided in the NHIS data,[12] and I report standard errors that account for clustering at the state-by-cohort level. The top panel of Table 1 reports weighted means and standard deviations by gender and race of variables used to estimate the health models. A substantial percentage of individuals in the sample are underweight (1.4% of men and 8.0% of women), suffering from health limitations (9.4% of men and 7.8% of women), or in self-reported fair or poor health

[11] The dropped observations are men who weighed less than 90 pounds or were less than 58 inches tall and women who weighed less than 80 pounds or were less than 53 inches tall.
[12] The weights within an NHIS dataset for a given year were normalized to sum to 1 before any observations were dropped.

(6.8% of men and 9.7% of women). Although these variables are measured well after individuals should have completed high school, they reveal that there could have been some room for improvement on health outcomes.

C. 1980 Census

The data on educational attainment and the individual-level control variables I use in the education regressions come from the 5% sample of the 1980 Census. I again restrict the sample to individuals born in the continental 48 states between 1941 and 1956 inclusive, and I exclude individuals living in group quarters. I match the Census data to the participation data using state of birth,[13] and I match an individual's age to the years when in school in the same way as for the 1980 NHIS survey. All regressions report standard errors that account for clustering at the state-by-cohort level. The bottom panel of Table 1 reports summary statistics of the Census data.

V. Identification Strategy

A. Estimating Equations, Potential Inconsistency of OLS, and Motivation for IV

I estimate equations of the form

$$outcome_{iscy} = \beta * exposure_{sc} + X_{iscy}\gamma + \alpha_s + \alpha_c + \alpha_y + \varepsilon_{iscy}. \qquad (1)$$

Here $outcome_{iscy}$ is a health or educational outcome variable measured in year y for individual i from state s born in year c. The main righthand side variable is $exposure_{sc}$, which is the average of the twelve state participation rates over the time the individual

[13] With the NHIS data, I am only able to match the individual-level data to participation data using current state of residence.

was in school.[14] This variable is defined on a scale of 0-100 and is calculated for each state in each year by dividing the number of students participating by the size of the population aged 5-17 (and multiplying by 100).[15] The remaining variables in the models are a vector of control variables X_{iscy} that contains individual-level data on race and state-level data on per capita income, a set of state dummies α_s, a set of birth cohort dummies α_c, and a set of year dummies α_y.[16] This model is consistent with the theoretical model of Grossman (1972), in which health investments have a cumulative effect on "health capital," if NSLP participation is seen as a health investment and the outcomes are proxies for the stock of health capital.

There are several reasons why least squares estimates of these models may be inconsistent. First, since school enrollment is necessary in order to participate in the NSLP, the models with educational attainment as the outcome variable may suffer from reverse causality. And even in the models for health, education is an omitted variable and there is the possibility of confounding the effects of participation with those of education. Controlling for education does not necessarily solve this problem, since education is potentially affected by participation.[17] Second, because participation in the NSLP is a choice variable, states that have higher participation rate at a point in time may differ from those with lower participation rates along dimensions that affect outcomes

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[15] There are two reasons for using the size of the population as the denominator rather than the number of enrolled students. First, the fraction of children who participated is arguably a more useful measure of the degree to which the children are affected by a program than the fraction of enrolled students who participated is. Second, the enrollment rate is potentially endogenous.

[16] Since the 1980 Census is a simple cross-section, the education estimates using the Census data do not allow for year dummies.

[17] If someone enrolls in school in order to participate in the NSLP and enrollment has a direct effect on outcomes, I take that to be an (indirect) effect of the NSLP.

and for which I am not able to control.[18] Third, the NSLP participation data may be measured poorly.

Instrumental variables offer a potential solution to these problems. I use an instrument related to the amount of funding states receive under the program, and it is defined so that the parameters are identified by the *change in the formula* rather than by year-to-year changes in the *inputs to the formula*. Thus, if the instrument is valid, it solves the problems with least squares given above by using variation in participation that originates from the supply side rather than from the demand side. Moreover, since the estimates are driven by a change in the formula, this variation comes about through a large supply side shock.

There are at least three channels through which funding given to states for the NSLP could affect participation within the state. First, a state that receives a larger amount of funding for the NSLP may be able to reimburse schools within the state at a higher rate for lunches, which would tend to increase the number of participating schools. Second, if a state reimbursed schools at a higher rate, this may result in schools charging lower prices to children for lunches, which may increase participation among children in schools already participating in the NSLP. Third, apart from the reimbursement rate to schools, a state that has a large amount of money available under the NSLP may make

[18] Participation is a two-stage decision. First, a school must choose to participate in the program. Second, children at participating schools must choose whether to participate. Thus, the effects of the program in a least squares regression may be confounded with either unobserved individual-level characteristics that, when aggregated up to the state level, are changing differentially by state over time; or they may be confounded with unobserved school-level characteristics that, when aggregated up to the state level, are changing differentially by state over time. Variables that are constant over time will be absorbed by the state effects, and variables that are constant at a point in time across states will be absorbed by the cohort effects.

greater efforts to convince schools to start lunch programs.[19] Unfortunately, data to disentangle the explanations is not available, but all three channels could play a role.

B. The Funding Formulas

In the time period under consideration here, the main federal cash aid given to states, Section 4 "general assistance" funding, was distributed according to a formula. The original formula is written into the original text of the National School Lunch Act and was in place from 1947-1962. A new formula was phased in beginning in 1963. In 1963, 75% of funding was distributed according to the "old formula" and 25% according to the new formula; in 1964, half of the aid was given according to the old formula and half according to the new; and in 1965, 25% was given according to the old formula and 75% the new. The new formula was fully in place in 1966 and continuing through the end of the sample period.

The original funding formula operated as follows: at year t, each state s was given an index defined by

$$index_{st}^{old} = \frac{population_{s,t-3}}{pci_{s,t-3}},$$

where *population* is the size of the population aged 5-17 and *pci* refers to per capita income.[20] Using r to range over the set of U.S. states and $totalfund_t$ to denote the amount of funding nationally in year t, the amount of funding going to state s in year t was then

[19] All these channels require there to be a "flypaper effect," whereby targeted aid given to a state 'sticks' to the purpose for which it is intended rather than being reallocated and spent in some other way.
[20] Technically, the index multiplied population in the numerator by the per capita income of the United States, but this factor cancels out in the next step.

$$fund_{st}^{old} = \frac{index_{st}^{old}}{\sum_r index_{rt}^{old}} * totalfund_t \,.$$

Thus, key features of the original formula are that states with lower per capita incomes and higher populations would receive relatively more funding.

The new funding formula shifted the focus from population-based funding to reimbursement based on past participation, and it also changed the way that funding depended on per capita income (even though it kept the feature that poorer states tended to receive more funding). The new formula can be described as follows: a state's index is

$$index_{st}^{new} = \frac{\overline{pci_{t-2}} + \overline{pci_{t-3}} + \overline{pci_{t-4}}}{pci_{s,t-2} + pci_{s,t-3} + pci_{s,t-4}} \,,$$

where $\overline{pci}$ refers to per capita income in the United States. The "assistance need rate" is defined to be

$$anr_{st} = \min\{9, 5 * \max\{1, index_{st}^{new}\}\} \,.$$

Figure 5 shows the relationship between the assistance need rate and per capita income in 1963. States with per capita incomes that are above average have an assistance need rate of 5, and poorer states have an assistance need rate that rises (up to a maximum of 9) as their income falls. The assistance need rate was used to determine a state's level of funding according to

$$fund_{st}^{new} = \frac{anr_{st} * lunches_{s,t-1}}{\sum_r anr_{rt} * lunches_{r,t-1}} * totalfund_t \,,$$

where *lunches* is the number of lunches served as part of the program.[21]

[21] A point regarding the timing of the variables that go into the funding formula is that the law only states that the most recently available figures are to be used. With the caveats that I take the timing for missing years (1948-1954, 1969) to be the same as that for the nearest non-missing years and that the funding under

C. Defining the Instrument

The instrument is based on funding levels, but I make two modifications. First, instead of actual funding levels I use "constant characteristics" funding levels, which are funding levels that would be received under the appropriate formula if states had constant per capita incomes and constant populations over time.[22] I make this modification because per capita income and population change over time and may have a direct effect on the outcomes; using "constant characteristics" funding levels ensures that the identifying variation comes about due to the formula change rather than from a change in the inputs that go into the formula. Second, I replace participation with population for the years that the new formula is used. Under the new formula, funding depends on lagged participation. Thus, if I used funding in grades 1-12 as the instrument and participation in grades 1-12 as the main explanatory variable, there would be a reverse causality problem in the first stage because participation in grades 1-11 would be a part of the lefthand side and would feed back to affect funding in grades 2-12 on the righthand side. With these modifications in mind, the identifying variation in the IV strategy comes from the fact that (1) the formula change affected states differentially (and non-linearly) by per capita income and (2) different birth cohorts were exposed to the two formulas to different degrees.

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the new formula actually depends on income in the three most recently available years, the most recently available income data had a lag of three years in the time period 1947-1961 and a lag of two years in the period 1962-1973. The population data is with a lag of three years for every year it was used except for 1962, where the lag was two years. The data on the number of lunches served was from the previous year for every year it was used.

[22] The "appropriate formula" is the original formula for 1947-1962, the later formula for 1966-1973, and a combination of the two for 1963-1965.

In particular, the instrument is constructed as follows. For years when the old formula is in place, I define $ccindex_s^{old}$ for state *s* as

$$ccindex_s^{old} = \frac{population_s}{pci_s},$$

where $population_s$ is the average population in state *s* between 1944 and 1971 and pci_s is the average per capita income in state *s* between 1944 and 1971. I define $ccfund_{st}^{old}$ for state *s* as

$$ccfund_{st}^{old} = \frac{ccindex_s^{old}}{\sum_r ccindex_r^{old}} * totalfund_t .$$

Here I have measured total funding in 2005 dollars using the annual CPI. For years when the new formula is in place, I define $ccindex_s^{new}$ by

$$ccindex_s^{new} = \frac{\overline{pci}}{pci_s},$$

where $\overline{pci}$ is the average annual per capita income of the United States over the years 1944-1971. I define $ccanr_s$ by

$$ccanr_s = \min\{9, 5 * \max\{1, ccindex_s^{new}\}\},$$

and $ccfund_{st}^{new}$ by

$$ccfund_{st}^{new} = \frac{ccanr_s * population_s}{\sum_r ccanr_r * population_r} * totalfund_t .$$

For each state *s* and year *t*, I then generate the constant characteristics funding level for the state and year by using the appropriate combination of “old constant characteristics funding” and “new constant characteristics funding.” Stated differently,

$$ccfund_{st} = f(t) * ccfund_{st}^{new} + (1 - f(t)) * ccfund_{st}^{old} .$$

Here *f(t)* equals 0 in 1947-1962, .25 in 1963, .5 in 1964, .75 in 1965, and 1 in 1966-1973. Thus far I have made these two modifications to funding levels: (1) generating them with time-invariant characteristics instead of time-varying characteristics and (2) using population rather than participation in the "new" formula. The final step in constructing the instrument is to combine constant characteristics funding amounts for the years an individual was in school; this captures the idea that the NSLP is a program individuals could be exposed to throughout their complete stay in elementary and secondary school. I do this as follows: for someone born in year *c* and from state *s*, I define the instrument to be

$$instrument_{sc} = \frac{1}{12} \sum_{t=6+c}^{t=17+c} \ln\left(\frac{ccfund_{st}}{population_{st}}\right) .$$

With the equation for the second stage given by equation (1), the first stage then takes the form

$$exposure_{sc} = \tilde{\beta} * instrument_{sc} + X_{iscy}\tilde{\gamma} + \tilde{\alpha}_s + \tilde{\alpha}_c + \tilde{\alpha}_y + \tilde{\varepsilon}_{iscy} . \qquad (2)$$

This specification for the first stage is implied by a model where proportional increases in funding per capita in a given year result in level increases of participation for that year.

Identification comes from the fact that different people were exposed to the two formulas to different degrees according to when they were born,[23] combined with the fact that the change in the formula affects states differentially. In particular, the new formula treats states with an above-average per capita income all the same; but under the old formula, increases in income for an already-rich state will result in lower funding for that

[23] This includes not just a change from one formula to another but also a period when both formulas were in place at the same time.

state. Moreover, since the total amount of the funding "pie" is fixed within a given year, a change in the formula that benefits states with higher incomes will be to the detriment of states with lower incomes. Figures 6 and 7 illustrate these points graphically. Figure 6 is a graph of the relationship between constant characteristics funding under the new formula and under the old formula for 1964, the year where half of the funding was appropriated under each formula. Figure 7 displays the difference between new and old constant characteristics funding by per capita income for 1964. Figure 7 reveals not only that the formula change results in a differential effect on funding by per capita income, but it also reveals that this effect is nonlinear.

To further explain the source of identification, note that the variable $ccfund_{st}$ changes for only three reasons: (1) it varies across states because different states have different time-invariant per capita incomes and populations, (2) it varies over time because of changes in the total amount of funding at the national level, and (3) it varies differentially across states over time due to a change in the formula. The first type of variation is accounted for by including state effects in the models, the second type is accounted for by the cohort effects, and the third type of variation is the identifying variation. Thus, when I combine constant characteristics funding amounts from different years in order to form my instrument, the variation used in estimation comes from the fact that the formula change affects states differentially and that different people were exposed to the two formulas to different degrees. The only other type of variation comes from the fact that I convert funding amounts to per capita terms by dividing by time-varying population. Dividing by time-varying population reflects the fact that it is the actual size of the population at the time that determines how generously a certain level of funding is spread across the population.

Table 2 explores the relationship between the instrument and exposure (using the NHIS data) and shows that funding does indeed affect participation. Column 1 is a simple bivariate regression of exposure on the instrument, and it shows that there is a positive correlation between the two. This relationship does not change very much when control variables are added to the model in column 2, but the coefficient on the instrument does drop precipitously in column 3 when cohort and survey year dummies are included. The cohort dummies absorb changes in the total amount of funding at the national level from year to year, so the drop in the coefficient reflects the fact that both participation and funding are generally rising over time.[24] The coefficient also falls in column 4, which introduces state dummies to soak up cross-state variation in funding at a point in time. Apparently states with higher 'permanent' funding tend to have higher participation rates anyway. But the positive relationship between funding and participation persists even after including individual-level control variables, cohort effects, year effects, and state effects. Subject to the caveat that the second stage outcome variables are missing for certain observations, column 4 is the first stage in the IV regressions.

VI. Results

A. Main Results for Health Outcome Variables

[24] However, in the case of funding, figure 2 reveals that there is a downward trend for the first couple years of the sample but that funding generally rises afterwards.

Height is a measure of long-term nutritional status and is determined primarily prior to reaching adulthood, making it a natural first outcome variable to consider.[25] Table 3 shows the results for height, with the results for men in the top panel and the results for women in the bottom panel. The least squares estimates in columns 1-4 show a positive relationship between height and NSLP exposure for both men and women; this relationship is significant at the 1% level in the full least squares specification for men in column 4, but the corresponding estimate for women is not even significant at the 5% level. The least squares estimate for men in column 4 suggests that increasing exposure by ten percentage points is associated with an increase in height of .18 inches. This estimate is remarkably similar to the IV estimate in column 6, which suggests that this same increase in exposure results in an increase in height of about .16 inches. However, the IV estimate for men is insignificant due to the large standard error. The IV estimate for women is also not significantly different from 0, although it is much larger in magnitude than the least squares estimate in column 4. But the general pattern to Table 3 is that I do not uncover a statistically significant impact of the NSLP on the average height. However, focusing on the average may conceal what is happening in the tails, and so Table 4 shows the effects of the NSLP on the cumulative distribution of height. In particular, the NSLP could have reduced the share of the population that is stunted without having a detectable effect on the average height. But this does not seem to be the case, as most of the estimated coefficients in Table 4 are insignificant and there is no clear pattern in the estimates.

[25] Strauss and Thomas (1998) contains a discussion of various measures of health status. Steckel (1995) is a detailed examination of using height as a measure of individual welfare. To get a sense of the magnitudes involved, Behrman and Hoddinott (2005) find that Mexico's PROGRESA program increased height by .4 inches, and Meng and Qian (2006) find that exposure to famine in China reduced height by 1.3 inches. See Persico, Postlewaite, and Silverman (2004) on the return to height in the labor market.

The next outcome variable I consider is body mass index (BMI). BMI is a measure of weight normalized by height; in particular, the formula for BMI is

$$BMI = 703 * \frac{weight}{height^2},$$

where weight is measured in pounds and height is measured in inches. Whereas height is a measure of long-run nutritional status, BMI is a measure of shorter-run nutritional status. However, there are at least two channels through which school lunch exposure as a child could affect BMI as an adult: (1) the degree of exposure to school lunches as a child could alter eating habits later in life and (2) there could be a physiological effect that carries over from childhood to adulthood.[26] The results for BMI are presented in Table 5. Columns 1, 2, and 3 of both panels actually display a significant negative effect on BMI, but significance is lost in both cases when adding state dummies in column 4. The IV estimates in column 6 are larger in magnitude than the least squares estimates, but they are insignificant and estimated rather imprecisely. So, on the whole, Table 5 does not reveal much of an effect of the NSLP on BMI. But as with the case of height, there could be an effect on extreme values of BMI without there being a statistically detectable effect of the mean. Moreover, whereas it is believed that larger height indicates better nutritional status (all else equal), the relationship between BMI and being in good health is non-monotonic in BMI. In particular, having either an extremely high or extremely low BMI is thought to be unhealthy. Thus, Table 6 shows the results of linear probability models of the effects of school lunch exposure on categorical measures of BMI. According to the Centers for Disease Control, someone is underweight if their BMI is

[26] Although there are many other determinants of BMI as an adult than just exposure to school lunches as a child and these other determinants add noise to the model, they should be orthogonal to the plausibly exogenous variation in school lunch funding that is used by the IV estimator if the IV strategy is correct. Also, see Case, Fertig, and Paxson (2005) on the issue of persistence of childhood health into adulthood.

less than 18.5, overweight if their BMI is above 25, and obese if their BMI is above 30. To the extent that the program fed an undernourished population, it may result in a lower probability of being underweight. But if the results of Schanzenbach (2005) held in this earlier time period, the program could increase obesity. However, turning to the results in Table 6, the coefficients are generally small in magnitude and statistically insignificant.

Table 7 considers three alternative measures of health. They are weight, a dummy for whether someone reports to be in fair or poor health as opposed to good or excellent health, and a dummy for whether someone experiences limitations caused by health problems. The coefficient estimates are not statistically significant, with the exception of the IV estimate of the effect of the NSLP on the 'poor or fair health' variable for men and the least squares estimate of the effect of the NSLP on this variable for women. The IV estimate for men suggests that an increase in NSLP exposure by ten percentage points lowers the probability of being in poor or fair health by 6.6 percentage points. This result could potentially be informative, since self-reported health status is a useful summary of all the various dimensions of health status; moreover, it has been shown to be related to subsequent morbidity and mortality. But on the other hand, this variable is almost certainly measured with error and different individuals may use different scales from one another, making interpretation difficult (Strauss and Thomas 1998).

B. Main Results for Education

If time spent in school is more productive for individuals in good nutritional status, then the NSLP could raise the optimal level of education individuals choose. Moreover, the option of receiving a subsidized lunch if a child attends school may directly influence the school participation decision. Table 8 shows the effects of the NSLP on years of completed education using data from the 1980 Census. The least squares estimates in column 1 is significantly negative, which is consistent with the fact that people from poorer areas have both higher exposure and lower educational attainment even in the absence of the program. One result in the least squares regressions that is somewhat surprising is that the coefficient rises from column 2 to column 3. If people born more recently have acquired more education, then we may expect the coefficient to fall from column 2 to column 3, since column 2 would be confounding the effect of lower participation in the earlier years with the effect of lower education in the earlier years. However, one possibility for the change from column 2 to column 3 is that the younger people in the sample have not had enough time to complete their education. But in any event, the least squares regressions in columns 3 and 4 show that there is a significant positive relationship between NSLP exposure and educational attainment. However, the least squares estimates likely suffer from reverse causality: since it is necessary that an individual be enrolled in school in order to receive a lunch, higher school enrollment is likely to result in higher NSLP exposure.

Column 6 displays the IV estimates. The IV point estimates are larger than the least squares estimates, perhaps implausibly so. The IV estimate for women suggests than increasing NSLP exposure by ten percentage points results in an average increase in education of .365 years, and the IV estimate for men suggests that increasing NSLP exposure by ten percentage points increases average education by nearly a year. However, even though the IV point estimates for education are significantly different from 0 at the 1% level for both men and women, the standard errors are still quite large. Thus, a 95% confidence interval covers values that are more reasonable than the point estimates and does not cover 0. It is somewhat surprising that the IV estimates are larger than the least squares estimates, since the reverse causality problem that affects the least squares estimates should induce a spurious positive correlation between NSLP exposure and educational attainment. However, one potential explanation for why the IV estimates are larger than the least squares estimates is that the IV estimates are not attenuated by measurement error in NSLP participation that would affect the least squares estimates. This explanation is consistent with the fact that the IV estimates for health outcomes also generally have larger magnitudes than the LS estimates.

C. Additional Specifications

To investigate the possibility that the NSLP had a differential effect on disadvantaged groups, regressions reported in Table 9 add a righthand side variable for the interaction between NSLP exposure and the percentage of men rejected from service or placed in the limited service class during World War II.[27] The large number of men rejected from military service during World War II played a large role in the passage of the National School Lunch Act, and the results in Table 9 answer the question of whether the program had a larger effect in states where the rejection rate was higher. The equation to be estimated is

$$outcome_{iscy} = \beta_1 * exposure_{sc} + \beta_2 * exposure_{sc} * rrate_s + X_{iscy}\gamma + \alpha_s + \alpha_c + \alpha_y + \varepsilon_{iscy}\,. \quad (3)$$

[27] The data come from U.S. Congress (1945).

Here $rrate_s$ is the rejection rate on a scale of 0-100. The instrumental variables estimators instrument NSLP exposure and the interaction term with the original instrument and its interaction with the rejection rate. The health results do not give much evidence for a differential effect of the NSLP by World War II rejection rate, but the education results do. Both the least squares estimates and the IV estimates point to a larger effect of the NSLP on education in states where the rejection rate was higher.

To further explore the possibility that the effects of the NSLP were different for different groups, Table 10 shows estimates of the effects on subsamples. I separately estimate the effects of exposure to the NSLP for whites and blacks and for people from Northern states and Southern states.[28] A problem with interpreting the IV results for race arises because I do not have separate data on participation for blacks and whites. Thus, if an increase in funding would have a different effect on participation for blacks than whites, the IV estimates would confound the effect of school lunches on health for subpopulations with the effect of funding on participation of those subpopulations. The correct interpretation of a larger result on BMI for blacks than whites, for instance, then, would be that states that have higher overall participation rates are ones where the BMIs of blacks increase more than those of whites. This issue may not be of much direct interest for policy, but the reduced form may be of interest because it shows what the effect of increasing NSLP funding is on the health of blacks compared to whites. The experiment to imagine is varying the amount of funding given to states, allowing participation of both blacks and whites to adjust as it would, and then estimating what

[28] "Southern states" is defined to mean those states in the Southern Census Region. These states are Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. The Northern states are the remaining 32 continental states.

happens to the BMIs of whites and the BMIs of blacks. But turning to the results, it is noteworthy that the education variable actually shows a greater effect for whites than blacks, which may suggest that funding was channeled toward whites. The results for health appear to be mixed, with there being a more beneficial effect for blacks on some variables (e.g., height and underweight) and a more beneficial effect for whites on others (e.g., health limitations). However, none of the health differences are significant. There is also no clear pattern in the North/South health differences, but it is notable that the education effect is larger in the South.

In results not reported here, I find that dropping observations from Louisiana, a major outlier in participation, does not change the results noticeably. I also perform a robustness check to determine whether any results are driven by changes in the composition of lunches among type A, type B, and type C by state over time.[29] To do this, I drop five states (California, Illinois, Massachusetts, Michigan, and New York) that show a large drop in overall participation around the time that there was a large drop nationally in type C lunch participation.[30] These are large states, and dropping these observations makes the results less precise, but there are no appreciable changes in the conclusions.

VII. Conclusion

This paper finds little evidence that the NSLP had a long-term effect on health but does find evidence that it may have affected educational attainment. The IV point

[29] See footnote 4 for an explanation of type A, type B, and type C lunches.

[30] I have data on type A, type B, and type C lunch participation nationally by year, but I do not have this data by state. The fact that these five states had a large drop in overall participation at a time when participation in type C lunches dropped sharply at the national level may suggest that, for these states, participation in type C lunches may have been a relatively high percentage of total participation.

estimates on education are perhaps implausibly large in magnitude, but a 95% confidence interval covers more reasonable values and does not cover 0.

The precision of the estimates is limited by the fact that, even though my individual-level datasets contain many observations, the variation I use to identify the effects of the NSLP occurs only at the level of state of birth and birth cohort.[31] But taking the results at face value, there are at least two other potential explanations for why I detect an effect on education but not on health. First, there may be beneficial effects of the NSLP in the short-term that do not carry over into adulthood. Thus, the effects on health may have faded away by adulthood, but my education variable measures a more contemporaneous effect. Second, to some extent, the program may just be a transfer to children who would have eaten a healthy lunch even in the absence of the program; in other words, the program may be displacing nutritional inputs coming from elsewhere,[32] including school lunches that are not a part of the federal program.[33] Thus, a potential explanation for the finding that the effects on health are small but the effect on education is large is the NSLP displaced food consumption that would have occurred in the absence of the program but that children attended school in order to purchase food at a lower price.

The NSLP today is still broad in its reach, but it has some elements of being targeted toward poorer children. These include codified standards for eligibility for free and reduced-price lunches and also special funding for poorer schools. If these elements had

[31] Standard errors that do not account for clustering at this level are much smaller.
[32] However, even if school lunches crowd out food consumption from food at home, they may still be valuable. For instance, if there are economies of scale in food production, it may be more efficient to mass-produce meals at school rather than to have them produced in individual households.
[33] My estimates are effects of participating in the NSLP. To the extent that there are school lunch programs that are not part of the NSLP, my estimates of the effects of the NSLP likely understate the effects of eating a school lunch.

been in place earlier, the NSLP may have had a detectable effect on health in its early years.

A. Data Appendix

This appendix gives the sources for the data on participation, funding, population, and per capita income I assembled. It also describes how I impute missing data.

Participation. Data on the number of students participating at the state level from 1947-1949 comes from a USDA publication entitled "School Lunch and Food Distribution Programs Selected Statistics, Fiscal Years 1939-1950" (United States Department of Agriculture 1950). Data from 1949-1973 comes from the edition of the *Statistical Abstract of the United States* for the subsequent year. The participation data from the two sources agrees for the overlapping year.

Population. Estimates of the size of the population aged 5-17 in each state come from editions of *Biennial Survey of Education in the United States*, editions of the *Statistical Abstract of the United States*, and the NSLP funding tables (U.S. Congress, various years). Data from the three sources agrees on overlapping years. This population data is available from the funding tables for 1944, 1952-1958, and 1960-1962. It is available from the *Statistical Abstract of the United States* for 1965-1968, 1970-1971, and 1973. It is available in the *Biennial Survey of Education* for 1944, 1946, 1948, 1950, 1951, 1953, 1955, and 1957. I use a linear interpolation for years in which this variable is not available in any of the three sources (1945, 1947, 1949, 1959, 1963, 1964, 1969, and 1972.)

Funding. Although the instrument in this paper is based on "constant characteristics" funding levels that I generate rather than on the actual funding levels, I do make use of actual funding levels in some of the preliminary graphical analysis. For the years 1947, 1955-1968, and 1970-1973, I take funding amounts from the funding tables. For the years 1948-1950, I take the data from "School Lunch and Food Distribution Programs Selected Statistics, Fiscal Years 1939-50." (Data for 1947 is available from both sources and unfortunately disagrees somewhat between the two sources.) For the years 1951-1954 and 1969, I estimate funding. Due to data limitations, in estimating funding amounts for these years, I excluded the District of Columbia, Alaska, Hawaii, American Samoa, Guam, Puerto Rico, and The Virgin Islands, and applied the appropriate formula (i.e., the old formula for 1951-1954 and the new formula in 1969) to just the continental 48 states using an estimate of the combined amount of funding given to these 48 states. This estimate of the amount of funding given to the continental 48 states is obtained by multiplying the total amount of funding nationally given in those years by the factor .954834, which is the average over the years for which I do have state funding data of the fraction of the total amount of aid going to the continental 48 states. This same factor is also used for the "constant characteristics" funding amounts I use in the regressions; but there the log specification reduces the importance of the particular factor chosen.

Per capita income. Per capita income for 1944 comes from the funding tables and disagrees slightly with the analogous numbers available in the *Statistical Abstract of the United States*. I use the *Statistical Abstract* numbers for 1945-1959 and 1961, and these numbers do agree with the numbers from the funding tables for the years in which the data is available from the funding tables (1952-1958, 1961). For 1960, 1962, 1963, the numbers from the two sources disagree; I use the numbers from the funding tables for those years. For the years 1964-1966 and 1968-1971, the funding tables give the average of per capita income over the past three years by state, which I use along with information from the funding tables for 1962 and 1963 and the *Statistical Abstract* for 1967 in order to obtain per capita income for each year between 1964 and 1971.

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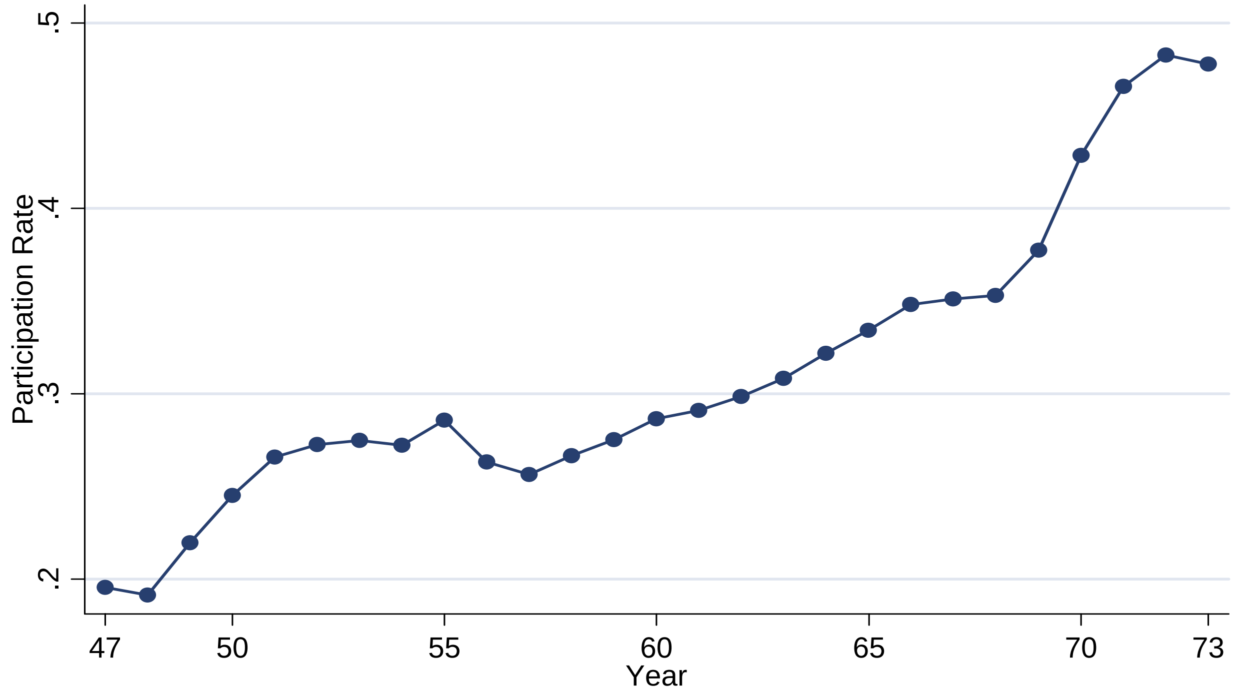
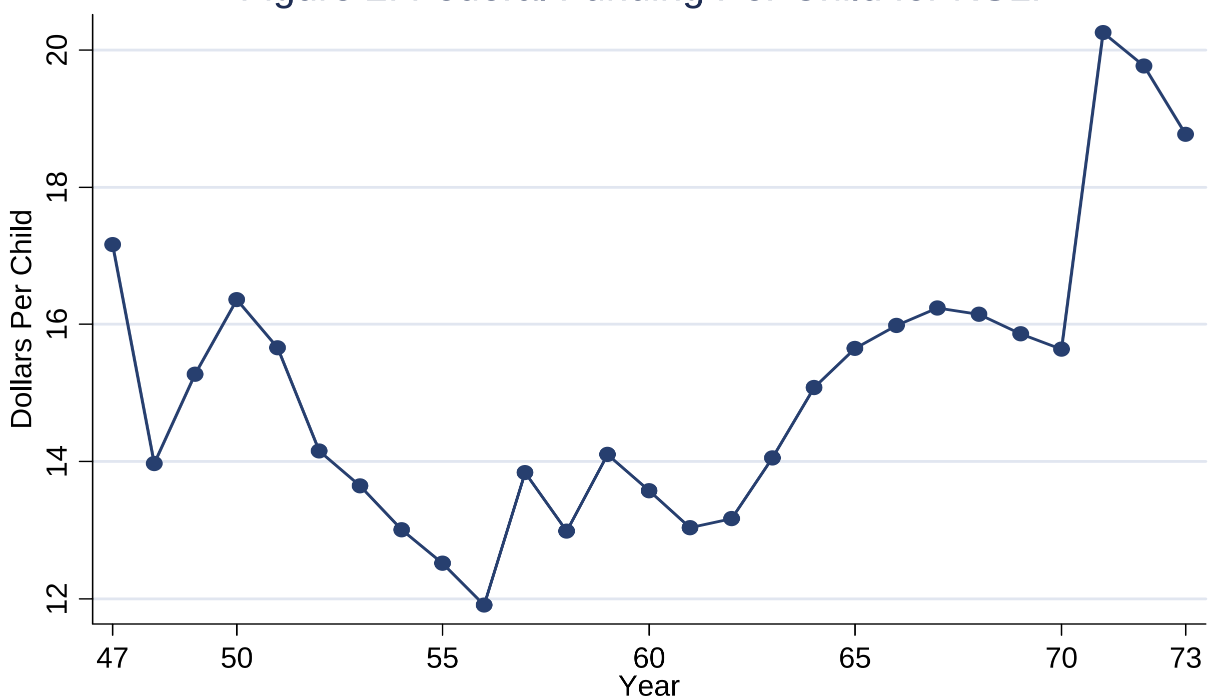
Figure 1: Annual Participation Rate in NSLP



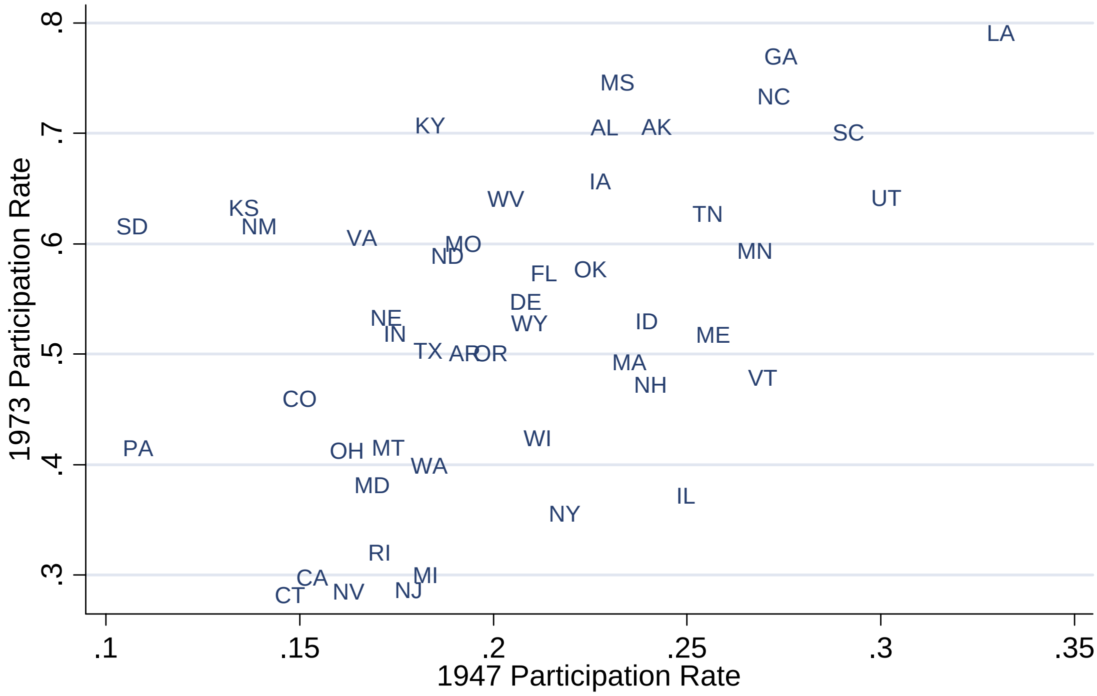
Note: Figure shows average participation nationally in peak month divided by size of population aged 5-17.

Figure 2: Federal Funding Per Child for NSLP



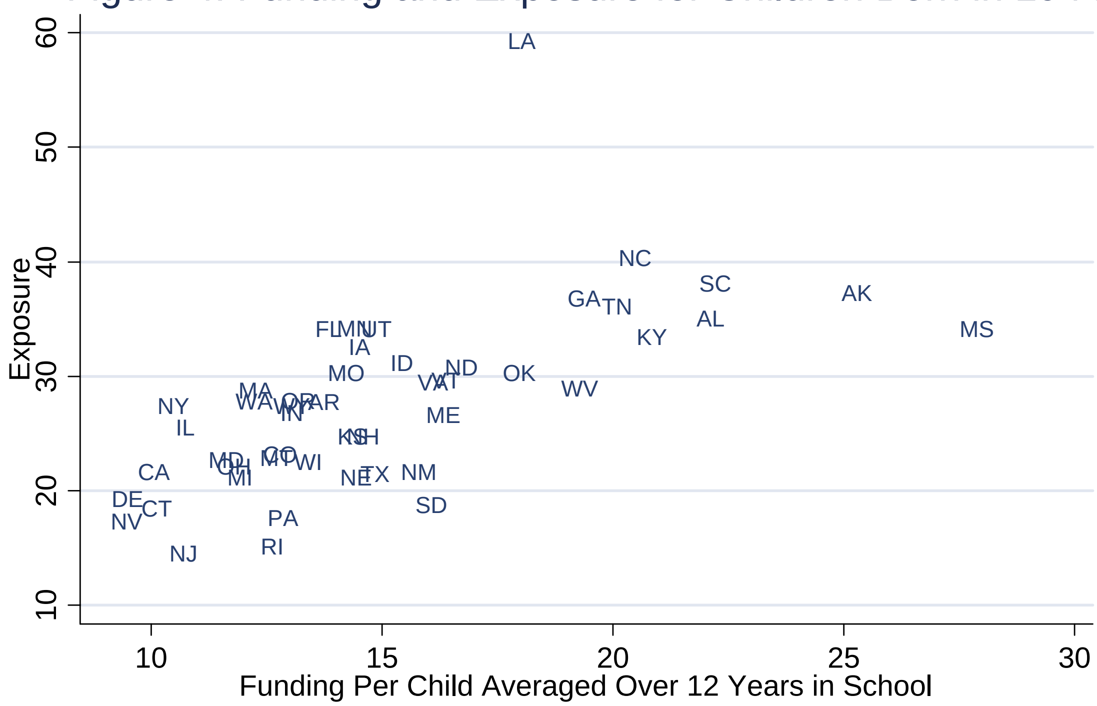
Note: Funding is measured in 2005 dollars.

Figure 3: State Participation Rates



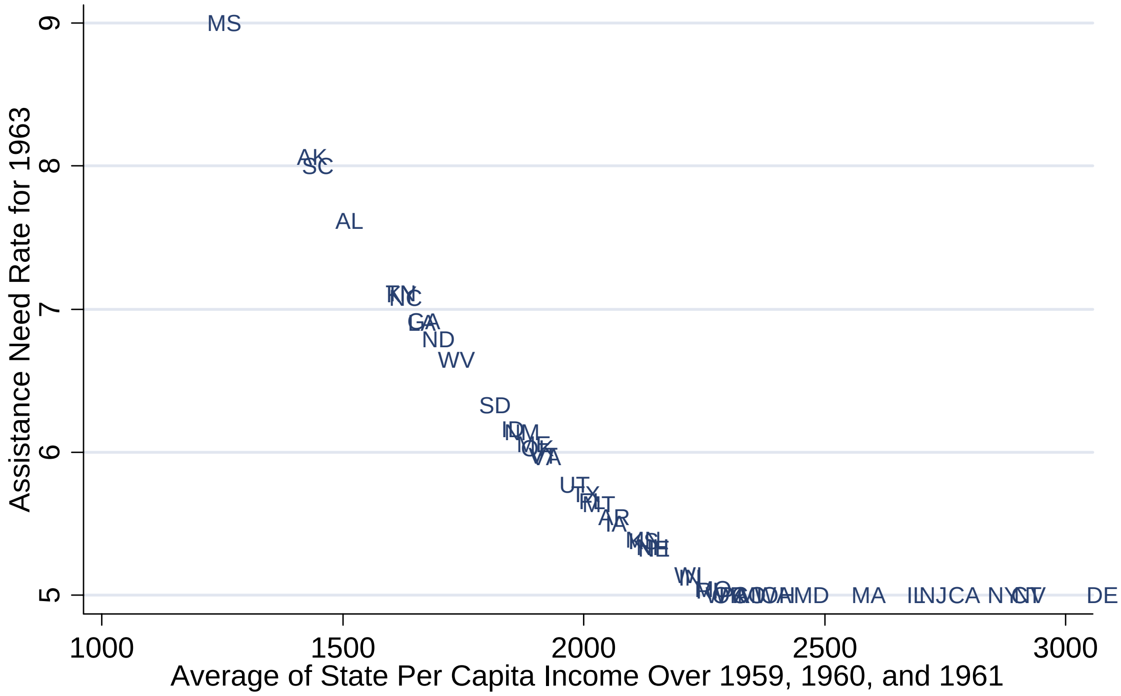
Note: 'Participation rate' is defined in note to Figure 1.

Figure 4: Funding and Exposure for Children Born in 1944



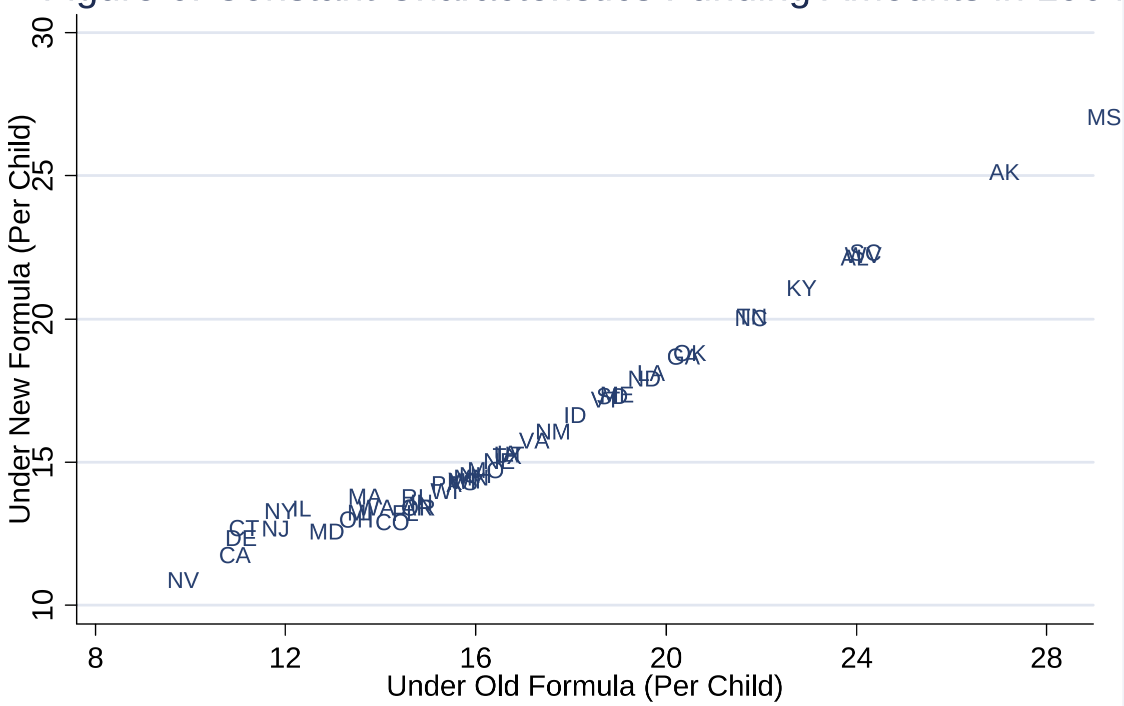
Notes: Funding is measured in 2005 dollars. Exposure is as defined in the text.

Figure 5: Assistance Need Rate and Per Capita Income



Note: Per capita income is measured in nominal dollars.

Figure 6: Constant Characteristics Funding Amounts in 1964



Note: Funding amounts per child are measured with 2005 dollars.

Figure 7: Effect of Formula Change on CC Funding for 1964

Note: Funding amounts on the vertical axis are measured using 2005 dollars.

Table 1: Summary Statistics

A. NHIS Data (Health Outcomes)

	Men			Women		
	All	White	Black	All	White	Black
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Height	70.2	70.3	69.9	64.4	64.5	64.5
	(3.0)	(2.9)	(3.3)	(2.7)	(2.7)	(2.8)
BMI	24.8	24.8	24.8	22.9	22.7	24.7
	(3.7)	(3.6)	(4.0)	(4.4)	(4.3)	(5.2)
Underweight	0.014	0.013	0.019	0.080	0.083	0.052
	(0.118)	(0.113)	(0.136)	(0.272)	(0.275)	(0.221)
Overweight	0.425	0.429	0.418	0.224	0.205	0.374
	(0.494)	(0.495)	(0.493)	(0.417)	(0.403)	(0.484)
Obese	0.080	0.079	0.093	0.074	0.066	0.136
	(0.272)	(0.270)	(0.291)	(0.262)	(0.249)	(0.343)
Weight	174	174	172	135	134	146
	(29)	(29)	(30)	(27)	(26)	(32)
Limitations	0.093	0.092	0.111	0.078	0.076	0.102
	(0.291)	(0.289)	(0.314)	(0.269)	(0.264)	(0.303)
Poor or Fair Health	0.068	0.062	0.121	0.097	0.084	0.183
	(0.251)	(0.240)	(0.327)	(0.296)	(0.278)	(0.387)
Exposure	30.0	29.8	32.9	30.1	29.8	32.6
	(10.7)	(10.4)	(12.5)	(10.8)	(10.5)	(12.4)
Average PCI	2200	2205	2136	2201	2204	2155
	(588)	(584)	(621)	(590)	(585)	(622)
Instrument	2.62	2.62	2.68	2.63	2.62	2.67
	(0.20)	(0.20)	(0.24)	(0.20)	(0.20)	(0.24)
N	61798	55211	5612	68555	59560	7836

B. Census Data (Education)

	Men			Women		
	All	White	Black	All	White	Black
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Education	13.3	13.4	12.1	12.8	13.0	12.1
	(2.9)	(2.9)	(2.8)	(2.5)	(2.5)	(2.5)
Exposure	31.0	30.3	37.3	31.2	30.3	37.4
	(10.8)	(10.3)	(12.8)	(10.9)	(10.3)	(12.7)
Average PCI	2156	2184	1906	2151	2185	1901
	(609)	(598)	(650)	(614)	(600)	(653)
Instrument	2.65	2.64	2.80	2.66	2.64	2.80
	(0.21)	(0.20)	(0.25)	(0.22)	(0.20)	(0.25)
N	1209769	1072230	123280	1260264	1089721	155727

Notes: Panel A shows means and standard deviations of the NHIS data using using sample weights. Panel B shows means and standard deviations of the Census data.

Table 2: First Stage for Men in NHIS Data

Variable	(1)	(2)	(3)	(4)
Instrument	40.7	43.4	9.8	7.8
	[1.5]**	[1.7]**	[4.6]*	[2.2]**
White		-0.207	-0.640	0.025
		[0.309]	[0.303]*	[0.051]
Black		0.527	0.145	0.049
		[0.360]	[0.332]	[0.060]
Average PCI		0.0021	-0.0146	-0.0373
		[0.0006]**	[0.0022]**	[0.0019]**
YoB dummies?	no	no	yes	yes
Age Dummies?	no	no	yes	yes
State Dummies?	no	no	no	yes
N	61798	61798	61798	61798

Notes: The tables shows estimates of equation (2). Standard errors corrected for clustering at the year of birth*state level are in brackets. A single asterisk denotes significance at the 5% level and a double asterisk denotes significance at the 1% level. All models are estimated using NHIS sample weights.

Table 3: Effect of NSLP Exposure on Height (in Inches)

A. Men

	Least Squares				IV	
					First Stage	Second Stage
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	0.0071	0.0063	0.0013	0.0181		0.0155
	[0.0017]**	[0.0016]**	[0.0030]	[0.0063]**		[0.0357]
Instrument					7.78	
					[2.19]**	
White		2.80	2.78	2.74	0.0144	2.74
		[0.11]**	[0.11]**	[0.11]**	[0.0578]	[0.1127]**
Black		2.38	2.36	2.37	0.0268	2.37
		[0.12]**	[0.13]**	[0.13]**	[0.0662]	[0.13]**
Average PCI		-0.00003	-0.0002	0.0007	-0.0372	0.0006
		[0.00003]	[0.0001]*	[0.0003]*	[0.0019]**	[0.0014]
YoB Dummies?	no	no	yes	yes	yes	yes
Age Dummies?	no	no	yes	yes	yes	yes
State Dummies?	no	no	no	yes	yes	yes
N	52224	52224	52224	52224	52224	52224

B. Women

	Least Squares				IV	
					First Stage	Second Stage
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	0.0077	0.0067	0.005	0.0071		0.0348
	[0.0011]**	[0.0011]**	[0.0022]*	[0.0049]		[0.0269]
Instrument					8.05	
					[2.28]**	
White		1.97	1.96	1.98	-0.0197	1.98
		[0.08]**	[0.08]**	[0.08]**	[0.0651]	[0.08]**
Black		1.98	1.97	2.01	0.0041	2.01
		[0.09]**	[0.09]**	[0.09]**	[0.0694]	[0.09]**
Average PCI		0.00001	-.00004	0.0004	-0.0373	0.0015
		[0.00002]	[.00006]	[0.0003]	[0.0020]**	[0.0011]
YoB Dummies?	no	no	yes	yes	yes	yes
Age Dummies?	no	no	yes	yes	yes	yes
State Dummies?	no	no	no	yes	yes	yes
N	58376	58376	58376	58376	58376	58376

Notes: Column 5 shows estimates of equation (2), and other columns show estimates of equation (1). Standard errors corrected for clustering at the year of birth*state level are in brackets. A single asterisk denotes significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models are estimated using NHIS sample weights.

Table 4: Effect of NSLP Exposure on the Cumulative Distribution of Height

	Men			Women		
	Value of CDF	LS	IV	Value of CDF	LS	IV
Height	(1)	(2)	(3)	(4)	(5)	(6)
58	0.0002	-0.00003	-0.0002	0.0074	0.00004	-0.0003
		(0.00004)	(0.0001)		(0.00014)	(0.0008)
60	0.0025	0.0001	-0.0001	0.0748	-0.0002	-0.0021
		(0.0001)	(0.0005)		(0.0005)	(0.0025)
62	0.0090	-0.0002	-0.0017	0.2484	-0.0007	-0.0034
		(0.0002)	(0.0011)		(0.0008)	(0.0042)
64	0.0320	-0.0005	-0.0014	0.5203	-0.0006	-0.0033
		(0.0004)	(0.0020)		(0.0010)	(0.0054)
66	0.1094	-0.0011	0.0014	0.7807	-0.0004	-0.0069
		(0.0006)	(0.0039)		(0.0008)	(0.0043)
68	0.2832	-0.0024	-0.0031	0.9394	-0.0011	-0.0037
		(0.0009)**	(0.0051)		(0.0005)*	(0.0022)
70	0.5119	-0.0017	-0.0039	0.9866	0.0001	-0.0002
		(0.0011)	(0.0056)		(0.0002)	(0.0011)
72	0.7990	-0.0017	0.0021	0.9980	0.0001	-0.0001
		(0.0009)	(0.0048)		(0.0001)	(0.0004)
74	0.9384	-0.0003	0.0019	0.9993	-0.0001	-0.00001
		(0.0005)	(0.0030)		(0.0001)	(0.00030)

Notes: The table shows estimates of equation (1) where the dependent variable is a dummy for having a height less than or equal to the given value. Columns 1 and 4 show values of the cumulative distribution function, columns 2 and 5 show results of least squares regressions, and columns 3 and 6 show results of instrumental variables regressions. Education results are estimated with Census data, and other results are estimated with NHIS data. Control variables are white and black dummies, average per capita income while in school (lagged two years), birth dummies, year dummies (with the NHIS data only), and state dummies. Standard errors corrected for clustering at the year of birth*state level are in parentheses. All models estimated with NHIS data are estimated using sample weights. A single asterisk denotes significance at the 5% level, and a double asterisk denotes significance at the 1% level.

Table 5: Effect of NSLP Exposure on BMI

A. Men

	Least Squares				IV	
					First Stage	Second Stage
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0135	-0.0286	-0.0061	-0.0105		-0.0517
	[0.0033]**	[0.0020]**	[0.0027]*	[0.0081]		[0.0377]
Instrument					7.76	
					[2.19]**	
White		1.46	1.54	1.47	0.0200	1.47
		[0.12]**	[0.12]**	[0.12]**	[0.0582]	[0.12]**
Black		1.50	1.59	1.50	0.0260	1.51
		[0.13]**	[0.13]**	[0.13]**	[0.0664]	[0.13]**
Average PCI		-0.00102	-0.0004	-0.0001	-0.0371	-0.0017
		[0.00003]**	[0.0001]**	[0.0004]	[0.0019]**	[0.0014]
YoB Dummies?	no	no	yes	yes	yes	yes
Age Dummies?	no	no	yes	yes	yes	yes
State Dummies?	no	no	no	yes	yes	yes
N	51975	51975	51975	51975	51975	51975

B. Women

	Least Squares				IV	
					First Stage	Second Stage
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0110	-0.0284	-0.0155	-0.0006		-0.0517
	[0.0031]**	[0.0021]**	[0.0033]**	[0.0091]		[0.0415]
Instrument					8.07	
					[2.27]**	
White		0.777	0.816	0.714	-0.0248	0.713
		[0.131]**	[0.130]**	[0.132]**	[0.0653]	[0.131]**
Black		2.80	2.84	2.77	0.0004	2.77
		[0.14]**	[0.14]**	[0.14]**	[0.0702]	[0.14]**
Average PCI		-0.0009	-0.0005	-0.0002	-0.0372	-0.0022
		[0.0000]**	[0.0001]**	[0.0005]	[0.0020]**	[0.0016]
YoB Dummies?	no	no	yes	yes	yes	yes
Age Dummies?	no	no	yes	yes	yes	yes
State Dummies?	no	no	no	yes	yes	yes
N	57656	57656	57656	57656	57656	57656

Notes: Column 5 shows estimates of equation (2), and other columns show estimates of equation (1). Standard errors corrected for clustering at the year of birth*state level are in brackets. A single asterisk denotes significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models are estimated using NHIS sample weights.

Table 6: Effects of NSLP Exposure on BMI Categories

A. Men

	underweight		overweight/obese		obese	
	LS	IV	LS	IV	LS	IV
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0001	-0.0014	-0.0015	-0.0093	-0.0002	-0.0009
	[0.0002]	[0.0013]	[0.0011]	[0.0057]	[0.0005]	[0.0027]
White	-0.0336	-0.0335	0.165	0.166	0.0282	0.0282
	[0.0065]**	[0.0065]**	[0.016]**	[0.016]**	[0.0073]**	[0.0073]**
Black	-0.0297	-0.0297	0.156	0.156	0.040	0.040
	[0.0066]**	[0.0066]**	[0.017]**	[0.017]**	[0.0082]**	[0.0082]**
Average PCI	-0.00002	-0.00007	-0.00005	-0.0003	-0.00001	-0.00004
	[0.00001]	[0.00005]	[0.00005]	[0.0002]	[0.00003]	[0.00010]
N	51975	51975	51975	51975	51975	51975

B. Women

	underweight		overweight/obese		obese	
	LS	IV	LS	IV	LS	IV
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	0.0008	0.0002	0.0000	-0.0042	0.0002	-0.0010
	[0.0006]	[0.0025]	[0.0008]	[0.0038]	[0.0005]	[0.0025]
White	-0.0625	-0.0625	0.0561	0.0560	0.0181	0.0181
	[0.0111]**	[0.0111]**	[0.0119]**	[0.0119]**	[0.0066]**	[0.0066]**
Black	-0.098	-0.098	0.229	0.229	0.0872	0.0872
	[0.0116]**	[0.0116]**	[0.013]**	[0.013]**	[0.0075]**	[0.0075]**
Average PCI	0.00002	0.00001	-0.00001	-0.0002	0.00000	-0.00005
	[0.00003]	[0.00001]	[0.00004]	[0.0001]	[0.00003]	[0.00010]
N	57656	57656	57656	57656	57656	57656

Notes: The table shows estimates of equation (1). Standard errors corrected for clustering at the year of birth*state level are in brackets. A single asterisk denotes significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models are estimated using NHIS sample weights. All models include year of birth dummies, age dummies, and state dummies.

Table 7: Effects of NSLP Exposure on Other Outcomes

	Men		Women	
	LS	IV	LS	IV
Outcome	(1)	(2)	(3)	(4)
Weight	0.0090	-0.2262	0.0189	-0.1506
	(0.0619)	(0.3126)	(0.0524)	(0.2249)
Health Limitations	0.0009	-0.0005	0.0000	-0.0029
	(0.0007)	(0.0043)	(0.0005)	(0.0031)
Poor or Fair Health	-0.0003	-0.0066	-0.0010	-0.0037
	(0.0004)	(0.0028)*	(0.0005)*	(0.0028)

Notes: The table shows estimates of equation (1). Each entry corresponds to a separate regression. Control variables are white and black dummies, per capita income while in school (lagged two years), year of birth dummies, age dummies, and state dummies. Standard errors corrected for clustering at the year of birth*state level are in parentheses. All models are estimated using NHIS sample weights. A single asterisk denotes significance at the 5% level.

Table 8: Effects of NSLP Exposure on Years of Completed Education

A. Men

	Least Squares				IV	
					First Stage	Second Stage
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0328	-0.0224	0.0107	0.0216		0.0964
	[0.0023]**	[0.0020]**	[0.0017]**	[0.0031]**		[0.0227]**
Instrument					8.70	
					[2.34]**	
White		0.661	0.592	0.661	0.054	0.658
		[0.101]**	[0.093]**	[0.106]**	[0.025]*	[0.108]**
Black		-0.462	-0.476	-0.344	-0.0137	-0.343
		[0.108]**	[0.099]**	[0.110]**	[0.0290]	[0.112]**
Average PCI		0.00030	0.0012	-0.0005	-0.0347	0.0022
		[0.00004]**	[0.0001]**	[0.0001]**	[0.0021]**	[0.0009]*
YoB Dummies?	no	no	yes	yes	yes	yes
State Dummies?	no	no	no	yes	yes	yes
N	1209769	1209769	1209769	1209769	1209769	1209769

B. Women

	Least Squares				IV	
					First Stage	Second Stage
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.0179	-0.0067	0.0118	0.0130		0.0365
	[0.0020]**	[0.0014]**	[0.0017]**	[0.0022]**		[0.0111]**
Instrument					8.67	
					[2.34]**	
White		0.534	0.496	0.516	0.0781	0.514
		[0.100]**	[0.095]**	[0.101]**	[0.0200]**	[0.102]**
Black		-0.0879	-0.0993	-0.0435	0.0112	-0.0439
		[0.1049]	[0.1003]	[0.1063]	[0.0224]	[0.1065]
Average PCI		0.00049	0.00098	-0.00009	-0.0346	0.0008
		[0.00003]**	[0.00005]**	[0.00009]	[0.0021]**	[0.0004]
YoB Dummies?	no	no	yes	yes	yes	yes
State Dummies?	no	no	no	yes	yes	yes
N	1260264	1260264	1260264	1260264	1260264	1260264

Notes: Column 5 shows estimates of equation (2), and other columns show estimates of equation (1). Standard errors corrected for clustering at the year of birth*state level are in brackets. A single asterisk denotes significance at the 5% level, and a double asterisk denotes significance at the 1% level.

Table 9: Differential Effects of NSLP for Men by State World War II Rejection Rate

	Least Squares		Instrumental Variables	
	Main Effect	Interaction	Main Effect	Interaction
Outcome	(1)	(2)	(3)	(4)
Education	-0.0191	0.0024	0.0513	0.0025
	(0.0043)**	(0.0002)**	(0.0242)*	(0.0005)**
Height	0.0236	-0.0003	0.0358	-0.0016
	(0.0109)*	(0.0005)	(0.0362)	(0.0008)
BMI	-0.0167	0.0004	-0.0545	0.0002
	(0.0140)	(0.0007)	(0.0385)	(0.0009)
Underweight	-0.0010	0.00005	-0.0022	0.00007
	(0.0004)*	(0.00002)*	(0.0013)	(0.00003)*
Overweight	-0.0042	0.0002	-0.0117	0.0002
	(0.0019)*	(0.0001)	(0.0057)*	(0.0001)
Obese	0.0004	-0.00004	-0.0002	-0.0001
	(0.0009)	(0.00004)	(0.0029)	(0.0001)
Weight	-0.0057	0.0009	-0.1485	-0.0060
	(0.1119)	(0.0052)	(0.3155)	(0.0075)
Health Limitations	0.0020	-0.0001	0.0012	-0.0001
	(0.0011)	(0.0001)	(0.0044)	(0.0001)
Poor or Fair Health	0.0010	-0.00007	-0.0055	-0.0001
	(0.0008)	(0.00004)	(0.0030)	(0.0001)

Notes: The table shows estimates of equation (3). Each row corresponds to an outcome variable. Within a given row, columns 1 and 2 show the results of the LS regression, and columns 3 and 4 show the results of the IV regression. Columns 1 and 3 show the main effect of NSLP exposure, and columns 2 and 4 show the effect of NSLP exposure interacted with World War II Selective Service rejection rate. Education results are estimated with Census data, and other results are estimated with NHIS data. Control variables are white and black dummies, average per capita income while in school (lagged two years), birth dummies, year dummies (in NHIS data only), and state dummies. Standard errors corrected for clustering at the year of birth*state level are in parentheses. All models estimated with NHIS data are estimated with sample weights. A single asterisk denotes significance at the 5% level, and a double asterisk denotes significance at the 1% level.

Table 10: Differential Effects of NSLP for Men by Race and North/South

	Race		Race Reduced Form		North/South	
	White	Black	White	Black	North	South
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Education	0.107	0.066	0.980	0.287	0.055	0.106
	(0.026)**	(0.041)	(0.115)**	(0.173)	(0.008)**	(0.024)**
Height	0.020	0.101	0.162	0.539	0.050	-0.024
	(0.038)	(0.155)	(0.315)	(0.811)	(0.020)*	(0.058)
BMI	-0.0848	0.211	-0.667	1.13	-0.0622	-0.0437
	(0.0416)*	(0.192)	(0.316)*	(0.96)	(0.0233)**	(0.0559)
Underweight	0.0011	-0.0032	-0.0083	-0.0173	-0.0004	-0.0015
	(0.0015)	(0.0051)	(0.0110)	(0.0270)	(0.0008)	(0.0019)
Overweight	-0.0147	0.0371	-0.116	0.199	-0.0118	-0.0018
	(0.0060)*	(0.0286)	(0.046)*	(0.151)	(0.0035)**	(0.0085)
Obese	-0.0022	0.0040	-0.0174	0.0215	-0.0015	-0.0043
	(0.0030)	(0.0124)	(0.0233)	(0.0682)	(0.0017)	(0.0038)
Weight	-0.437	2.14	-3.43	11.5	-0.158	-0.401
	(0.346)	(1.61)	(2.52)	(7.8)	(0.181)	(0.500)
Health Limitations	-0.0021	0.0129	-0.0167	0.0630	0.0024	-0.0029
	(0.0042)	(0.0212)	(0.0316)	(0.1035)	(0.0023)	(0.0058)
Poor or Fair Health	-0.0072	-0.0086	-0.0563	-0.0466	-0.0019	-0.0062
	(0.0028)**	(0.0166)	(0.0186)**	(0.0838)	(0.0014)	(0.0036)

Notes: The table shows estimates of equation (1) for subsamples. Each row corresponds to an outcome variable. Each entry corresponds to a separate IV regression. Education results are estimated with Census data, and other results are estimated with NHIS data. Control variables are white and black dummies, average per capita income while in school (lagged two years), birth dummies, year dummies (with the NHIS data only), and state dummies. Standard errors corrected for clustering at the year of birth*state level are in parentheses. All models estimated with NHIS data are estimated using sample weights. A single asterisk denotes significance at the 5% level, and a double asterisk denotes significance at the 1% level.