2000

The US School Breakfast Program: Short- and Long-Term Academic Effects

Chris Rohlfs
Oberlin College

The US School Breakfast Program: Short- and Long-Term Academic Effects

Chris Rohlfs
Oberlin College
Undergraduate Honors Thesis
Final Draft
Revised: May 23, 2000

ABSTRACT

This paper evaluates short- and long-term academic effects of the US School Breakfast Program (SBP). The paper divides into four sections: an introduction (page 4), a literature review (page 11), a statistical model (page 31), and an empirical model (page 38). In the first section, we cover general facts and details about the SBP. In the second section, we first review literature relevant to the SBP (supply, demand, and short-term effects studies). Next, we explore studies of the long-term effects of schooling and of school quality. Many of the techniques and information from these studies relate to our discussion of long-term effects of the SBP. In the third section, we formalize our argument that the SBP exercises short- and long-term effects on students' performance. Our discussion of the statistical model follows the format of the flow chart on page 86. In the fourth section, we empirically test the hypotheses that the SBP improves students' attendance and expected education levels. We use pooled statewide aggregate data to measure attendance rates, and we use cross-sectional longitudinal data to measure education levels. We find that the SBP does raise attendance and education levels. We are able to quantify the attendance effect and decompose it into two separate effects. We are not able to quantify the effect on educational attainment, but we do find a lower bound for the SBP's effect on high school graduation. Given this lower bound, we are able to calculate a lower bound for the income effect of the SBP. We compare our calculated income effect with two possible alternatives. We find that a dollar spent on the SBP yields a substantially higher return than the 10-year Treasury Bill interest rate. We also find that, dollar for dollar, the SBP's income effect rivals Card and Krueger's estimated income effect for classroom size reduction. We do not find convincing evidence that this return can be achieved by indiscriminately increasing the number of SBP participants. These findings indicate that recent efforts to expand the SBP through universal free breakfasts could be better directed. Indiscriminate expansion of the SBP does earn a fairly high return. Nevertheless, our research suggests that selective expansion of the SBP could capitalize on significantly higher economic returns.

TABLE OF CONTENTS

ABSTRACT......1

INTRODUCTION......4

SBP Background......5

Program History......5

Availability and Eligibility......9

Provisions......11

LITERATURE REVIEW......11

Review of Breakfast and SBP Literature......12

Supply and Demand for School Meals and for the SBP......12

School SBP Participation......12

The Decision to Eat Breakfast......13

School Meal and NSLP Demand......15

SBP Demand......16

The Effects of Breakfast and the SBP on Students' Nutrient Intake......18

Breakfast Foods......18

SBP Effects on 24-Hour Nutrient Intake......19

The Effects of Breakfast and the SBP on Students' Performance......21

Physical and Mental Performance Effects......21

Behavioral and Academic Performance Effects......23

Economic Returns to Schooling and to School Quality......24

Returns to Years of Schooling......25

School Quality and Earnings......26

School Quality and Earnings, Holding Education Level Constant......26

Long-Term Effects of School Quality......27

Further School Quality Research and Long-Term Effects Studies......31

STATISTICAL MODEL......31

Student SBP Participation......32

Short-Term Effects......33

Nutrient Intake......33

Physical and Emotional Health......34

Attendance......35

Cognition......35

Long-Term Effects......36
School Performance......36
Educational Attainment......36
Future Earnings......37
EMPIRICAL MODEL......38
Data......38
USDA Data......38
NLSY79 Geocode......42
Composition of Respondents......42
Equations and Variables......43
Model 1: Attendance in SBP Schools......43
Model 2: Educational Attainment and High School Graduation......48
Model 2a: The SBP and Educational Attainment......49
Restrictions Placed on the Sample Population......51
Education Level......51
SBP Variables......53
Control Variables......58
Model 2b: The SBP and Labor Market Experience......61
Further Restrictions Placed on the Sample Population......63
Results......64
Model 1......64
Model 2......67
Model 2a......67
Model 2b......72
CONCLUSION......77
GLOSSARY OF VARIABLES FOR EMPIRICAL MODELS......102
Model 1......102
Model 2......105
BIBLIOGRAPHY......107

GRAPHS, FIGURES AND TABLES

Graph 1: US Schools and SBP Schools, 1967-98......81
Graph 2: Daily Breakfasts as a Percentage of Total Enrollment, 1967-98......82

Figure 1: Return to Years of Education by Level of Educational Quality......85
Figure 2: Proposed Cause and Effect Relationship for the SBP......86
Figure 3: Venn Diagram of Enrollment, ADA, and SBP Variables......88
Figure 4: Conversion of Coefficients from Equation (17) to Equation (16)......88
Figure 5: Mathematical Rationale Behind BNP_i and BMP_i Evaluated at the Mean......89

Table 1: FY2000 Reimbursement Rates for 48 Contiguous States......83
Table 2: Mean Enrollment by Grade and SBP Availability, 1978 US GAO Sample......83
Table 3: Significant Variables in NSLP Demand by Age and Eligibility Status......84
Table 4: Breakfast Composition by Source......85
Table 5: Variables and Years Available for USDA Data Set, State-Level Observations......87
Table 6: Composition of 716-Respondent NLSY79 Sub-Sample......89
Table 7: USDA Data Serial Correlation Regression......90
Table 8: USDA Data Year-Dummy Regression......90
Table 9: USDA State-Dummy Regression......90
Table 10: Regression Results for Model 1......91
Table 11: Descriptive Statistics for Variables in Model 1......92
Table 12: Descriptive Statistics for Variables in Model 2a......92
Table 13: Descriptive Statistics for Variables in Model 2b......93
Table 14: Regression Results for Model 2, Equations (19) - (21)......94
Table 15: Regression Results for Model 2: Log Formulation, Equations (18) – (21)......95
Table 16: Regression Results for Model 2: Equations (22) – (24)......96
Table 17: Year- and State-Dummy Coefficients for Equation (14)......97
Table 18: State-Dummy Coefficients for Equation (19)......98
Table 19: Year-, State-of-Residence-, and Birth-State-Dummy Coefficients for Equation (22)......99
Table 20: Year-, State-of-Residence-, and Birth-State-Dummy Coefficients for Equation (24)......100
Table 21: Rates of Return on 10-Year T-Bills Versus SBP, 1968-1994......101

INTRODUCTION

In 1998, 17% of US students participated in the US School Breakfast Program (SBP). In the same year, 57% of US schools participated, and the federal government spent $1.3 billion on the program. Last year, Congress passed a pilot program to provide free breakfasts to all interested students living in certain school districts. Recently, lawmakers introduced a bill to Congress to provide free breakfasts to all students in US schools K-12. The program would cost $400 million a year.[1]

The SBP is gaining political momentum despite relatively little feedback about the nationwide impacts of the program. The SBP purports to improve students' nutrition, eating habits, attendance, and learning abilities. School-wide studies have demonstrated that the SBP improves children's health, behavior, attendance, and some forms of academic performance. No studies have demonstrated lasting effects of the program past a single semester. A recent study of US schools showed that the SBP increases the probability that low-income children eat healthful breakfasts.[2] No nationwide studies have demonstrated academic effects of the program. Economists have demonstrated that school quality variables significantly affect student's years of education and their earnings. Our empirical model draws from these studies.

This paper consists of four sections. First, we include an introduction to provide a general background of understanding about the SBP. Second, we review literature written on the SBP to establish what aspects of the SBP researchers have studied and what they have shown. Third, we construct a statistical model to formalize our argument that the SBP exercises long-term effects on students' academic and labor market performance. Fourth, we construct and test two empirical models. Our first empirical model tests the hypothesis that SBP participation improves student attendance. Our second empirical model tests the hypothesis that SBP in the elementary school years raises students' expected education levels. We find that the SBP does raise attendance and education levels. We are able to quantify the attendance effect and we decompose it into two separate effects. We are not able to quantify the effect on educational attainment, but we do find a lower bound for the SBP's effect on high school graduation. Given this lower bound, we calculate a lower bound for the income effect of the SBP. We compare our calculated income effect with two possible alternatives. We find that a dollar spent on the SBP yields a

[1] Associated Press, "Government Plans . . ."
[2] Devaney and Stuart. 1998.

substantially higher return than the 10-year Treasury Bill interest rate. We also find that, dollar for dollar, the SBP's income effect rivals Card and Krueger's estimated income effect for classroom size reduction. We do not find convincing evidence that this return can be achieved by indiscriminately increasing the number of SBP participants. These findings indicate that recent efforts to expand the SBP through universal free breakfasts could be better directed. Indiscriminate expansion of the SBP does earn a fairly high return. Nevertheless, our research suggests that selective expansion of the SBP could capitalize on significantly higher economic returns.

Long-term student effects of the SBP rely on a cause-and-effect chain. Schools may or may not offer the SBP to students. Given school SBP availability, students may or may not participate. SBP participants eat different breakfasts than they would eat at home. The SBP raises participants' morning and daily nutrient intakes. Because SBP participants eat better, their brains and bodies function more efficiently, they do not get distracted by hunger. The SBP gives students an incentive to come to school; participants are less likely to arrive late or to skip school. To the extent that the SBP improves participants' health, participants are less likely to miss school due to illness. As the SBP improves participants' health, participants' cognitive functions and emotional health may improve. Participants' academic performance may improve. These academic improvements may lead SBP participants to attend school longer. Because they may learn better in school and they may attend school longer, SBP participants may earn higher incomes because of the program. To better understand the chain of student effects, consult Figure 2 on page 86.

SBP Background

All US schools, K-12, public or private, may receive federal reimbursements for providing school breakfasts. In order to receive reimbursement, schools must offer breakfast to every interested student. USDA reimbursement rates depend on the income of the student who purchases the breakfast. This section briefly describes the SBP's history, program goals, levels of availability and eligibility, and the provisions of the program.

Program History

Federally sponsored school lunches began during the Great Depression. The government sought to feed children, to provide a market for US agricultural products, and to create cafeteria jobs. World War II revived legislative interest in school lunches for two reasons. First, WWII draft offices had turned away a number of malnourished young men. This phenomenon brought malnutrition to national attention. Second, women's employment opportunities improved during the war, and the opportunity cost of preparing home lunches rose.[3] Congress passed the National School Lunch Act (NSLA), instating the National School Lunch Program (NSLP), in 1946. In the Act, Congress asserted two goals:

"To safeguard the health and well-being of the nation's children, and to expand the market for nutritious agricultural commodities."[4]

Throughout the 1950's, many lower income students and schools did not participate in the NSLP because they could not afford the lunches. In the early 1960's, as part of Johnson's War on Poverty, Congress passed special assistance legislation for schools to offer free and reduced-price lunches to low income students.[5] Congress changed this special assistance to permanent funding in 1972.[6]

The Child Nutrition Act (CNA) of 1966 restated the goals of the NSLA and set out to better achieve those goals. In order to do so, the CNA expanded the NSLP and created a number of new pilot programs, including the SBP. Congress renewed the temporary program in 1968, 1971, and 1972, and they voted in 1975 to make the SBP permanent.

In the early years of the program, policymakers noted that many farm children ate breakfast early and worked before school. These children were hungry again by the time they arrived at school.[7] Unlike the NSLP, the SBP targets both "low income areas and areas where children have to travel a great distance to school."[8] The 1975 amendment to the CNA added the goal that the SBP "be made available in all schools where it is needed to provide adequate nutrition for children in attendance."[9]

---

[3] 103rd Congress Pages 63-64.
[4] 103rd Congress. Page 3.
[5] 103rd Congress. Page 64.
[6] 103rd Congress. Page 3.
[7] Devaney and Stuart.
[8] Burghardt and Devaney. Page 178(S) (Quotation not cited). 103rd Congress. Pages 84-85.
[9] 103rd Congress. Page 86.

The USDA has consistently provided in-kind food donations as a part of school nutrition funding. In 1972, the USDA estimated that food donations constituted 7% of its total school nutrition budget. Beginning in 1973, Congress took steps to guarantee this average level of additional support, requiring that the USDA offer extra cash during times of food shortage.[10]

In 1977, Congress established higher Federal reimbursement rates for schools located in severe need areas, such as inner cities. In 1978, Congress established eligibility guidelines whereby schools qualified for severe need status.[11]

In the early 1980's, the new Congress cut back on SBP and school nutrition spending in an attempt to balance the budget. Under the Omnibus Budget Reconciliation Act (OBRA) of 1981, Congress tightened eligibility for reduced price breakfasts from 195% of the poverty level to 185% of the poverty level, where it stands today. In addition, the federal government lowered the rates at which it reimbursed schools for meals served. The Reagan administration attempted to cut some other costs in the school nutrition programs. St. Pierre & Puma (1992) show that reforms in the early 1980's to reduce fraud in the NSLP were misguided. Many of the NSLP's eligibility errors can be attributed to income fluctuations.

We can observe from Graph 1 on page 81 that the number of schools in the SBP declined between 1981 and 1983. We can also observe that the number of SBP schools grew slowly for the remainder of the 1980's. We can observe from Graph 2 on page 82 that SBP breakfasts served per student dropped sharply between 1981 and 1982. Breakfasts per student did not reach the 1981 level again until 1990.

Within a few years of the budget cuts, Congress revived its efforts to expand the SBP. In the late 1980's Congress voted for higher reimbursement rates and higher nutrition requirements for school meals. In 1989, Congress authorized the "School Breakfast Start-Up" program. This program provides grants to individual schools to begin to offer the SBP. The Start-Up program generated a quick rise in SBP participation. From 1988 to 1993, SBP coverage increased from 38,800 to 55,400 schools.[12] Over the same period, US schools K-12 increased only slightly from 117,000 to 118,000.[13]

[10] 103rd Congress. Pages 117-124.
[11] 103rd Congress. Pages 86-87.
[12] 103rd Congress. Page 88. USDA Data Set.
[13] Digest of Education Statistics.

The SBP served 70,000 (57%) out of a total 123,000 US schools K-12 in 1998.[14] In the same year, 96,000 (78%) schools served lunches. Other school-based child nutrition programs include Afterschool Snacks (1998), Special Milk (1955), Summer Food Service (1975), Child & Adult [Day] Care (1978), and Nutrition Education & Training Programs (1966).[15] All public and private schools are eligible to participate in such programs.[16]

The SBP is not nearly as widespread as the NSLP is. In 1992, schools served a daily average of 4.92 million breakfasts (0.6 million full price) and 24.6 million lunches (11.7 million full price). Only 22% of students attending NSLP schools are certified to receive free/reduced price meals. On the other hand, 42% of students attending SBP schools are certified to receive free/reduced price meals.[17] In 1995, the NSLP was available to 92% of all grade students and 56% of those students participated on a given day.[18] In 1995, the SBP was available to 69% of all grade students, and 19% participated on a given day.[19]

Availability and Eligibility

The USDA uses the same reimbursement schedule for the 48 contiguous states and for Guam and other territories. The USDA assigns Hawaii and Alaska each their own reimbursement schedules with higher rates. Reimbursement schedules vary according to students' eligibility statuses and schools' severe need statuses. The USDA reimburses schools more for each free breakfast served than it does for each reduced-price breakfast served. The USDA reimburses schools more for each reduced-price breakfast served than it does for each full price breakfast served. Students are eligible for free breakfasts if their families live at 130% of the national poverty line or below. Students are eligible for reduced-price breakfasts if their families live at 185% of the poverty line or below. All other students pay the regular subsidized rate. Schools that fall within severe need areas receive higher reimbursements in each category. Table 1 on page 83 shows the reimbursement rates for the continental US for the fiscal year 2000 (FY2000). In addition to cash assistance, the USDA provides commodity assistance. For FY2000, commodity assistance

[14] FRAC. USDA FNS, FACTS. USDA Data Set. Digest of Education Statistics. Total schools figures represent public schools. NSLP and SBP school figures include private schools that participate.
[15] Numbers in parentheses indicate year of inception. USDA FNS Child Nutrition. 103rd Congress. Pages 32, 97, 106, 125-127.
[16] Burghardt and Devaney. Page 179(S).
[17] USDA FNS, FACTS.
[18] Gleason. Page 214(S).

averages $0.1475 per meal. As of 1989, the USDA also provides School Breakfast Start-Up funding to encourage schools to begin to offer the program.[20] In order to receive reimbursement, each school sends the number of breakfasts it served to its school district office. Next, the school district office sends its totals to the State Department of Education (DofEd). The State DofEd sends its totals to the USDA. The USDA writes checks to the states, the states write checks to the school districts, and the school districts write checks to the schools. In some cases, schools serve satellite meals from school district kitchens, so that the district receives the final payment.

Full meal prices vary by school. The USDA sets a nationwide maximum price for reduced-price meals. For the past five years or more, the maximum reduced price has been 30 cents.[21] Individual schools or school districts set the full price to cover remaining program costs. Let us consider the example of Non-Severe need breakfasts in the continental US this year. If the total cost of providing a breakfast exceeds $1.09, then USDA reimbursements will not cover program costs. In this case, schools might charge a high full price to subsidize the provision of free or reduced-price breakfasts.

In 1980, the US General Accounting Office (GAO) published a study researching reasons for the SBP's slow expansion rate.[22] The authors found that financial burden significantly inhibited expansion of the program. Many schools lost money by participating in the SBP. The study described average losses per breakfast in different areas of the country. In 1994, Glantz and others found that reimbursement rates for school lunches typically exceed program costs. Schools often use the extra money to subsidize non-reimbursable meals and the SBP, which typically operates at a loss. Glantz and others and the GAO both cite labor and non-food costs as major difficulties. Glantz and others examined unreported costs of the program, including unreported administrative costs. The authors found that in SY1992, the full costs for the SBP exceeded the reimbursement rates in 93% of SBP school districts. In 82% of SBP school districts, the full costs for the SBP exceeded the severe need reimbursement rates. Moreover, 89% of school breakfasts were served at a loss to the institution providing the meal. The authors estimated a mean total cost of $1.67 and a median total cost of $1.38 per meal.

---

[19] Gleason. Page 217(S).
[20] USDA FNS. "SY99-SY00 Reimbursement Rates."
[21] Telephone interview with Terry Dougherty, Virginia Department of Education, 12/99
[22] GAO, "Major Factors. . ."

Provisions[23]

The USDA requires that SBPs conform to a specific meal pattern of two entrees, one fruit, vegetable, or juice, and one serving of fluid milk. An SBP entree consists of a meat or bread item or a meat or bread alternative. Meals frequently consist of cereal, juice, and toast. Sometimes SBP meals include eggs, pancakes, french toast, or sausage. We describe the foods served through the SBP on page 18 and on Table 4 on page 85. The USDA will only provide reimbursement for a meal if the student takes at least three of the four items served.

Typically, responsibility falls upon individual schools to plan their own menus. Some school districts use one kitchen for multiple schools. We call meals from these kitchens satellite meals. Schools may use a variety of menu-planning methods to meet USDA guidelines and the USDA publishes a wide range of menu-planning materials.[24]

Burghardt and others (1995a) examined meals from a representative sample of US schools through the School Nutrition Dietary Assessment Study (SNDAS) in 1993.[25] SBP meals generally meet and in some cases exceed the USDA's goal of ¼ RDA for both vitamins and protein. The meals did not meet the USDA's goal of ¼ of the RDA for calories. The USDA encourages food providers to vary portion sizes according to different students' caloric needs. The USDA does not require SBP meals to include a meat or meat alternative for SBP breakfasts; both entrees may be bread items or bread alternatives. Consequently, SBPs include a meat or meat alternate about half the time.

Dietary Guidelines for Americans (DGA) recommends that Americans eat 30% or less of their calories from fat and 10% or less of their calories from saturated fat. A 1995 study found that 44% of SBP meals meet the total fat goal. Only 4% of SBP meals meet DGA's saturated fat goal.[26]

LITERATURE REVIEW

[23] Burghardt and others (1995a).
[24] USDA FNS, *Start the Day. . ., Healthy School. . .*
[25] See page 10 for a brief description of the SNDAS.
[26] Burghardt and others.

The literature review divides into two sections. The first section consists of a discussion of literature regarding breakfast and the SBP. In the second section, we discuss studies that measure the economic returns to schooling and to school quality. This second section of the review serves two purposes. First, the studies support our premise that provisions like the SBP or school quality can significantly affect students' long-term performance. Second, when we discuss our empirical results later in the paper, we will compare our own results with results from these studies.

Review of Breakfast and SBP Literature

This section of the literature review divides into three subsections. First, we review economists' and nutritionists' work describing supply and demand for school meals and for the SBP. Second, we explore economists' and nutritionists' studies regarding breakfast and the SBP and how they affect students' nutrient intake. Third, we examine educators' and medical researchers' studies describing students' physical, academic, and behavioral improvements that result from breakfast consumption and SBP participation.

Supply and Demand for School Meals and for the SBP

Philip Gleason (1995) divides SBP demand into two categories. First, he asks whether or not students eat breakfast at all. Second, he asks whether or not students eat SBP breakfasts. We will examine the literature regarding SBP supply and demand in four parts. First, we will examine studies about schools' decisions to participate in the program. Second, we will examine findings from the literature about breakfast consumption patterns. Third, we will examine school meal and NSLP demand models. More students participate in the NSLP than do in the SBP, and some authors have compared the two programs' demand functions. Fourth, we will directly address demand models for the SBP.

School SBP Participation

In 1977, the USDA Food and Nutrition Service (FNS) sampled 625 schools to explore school SBP participation.[27] The study found that economic need, grade level, enrollment size, and the presence of snack bars significantly affected school participation. For a school to qualify as economically needy, 40% or more of the students qualify to receive free or reduced-price meals. Economically needy schools were more likely to participate, and schools at lower grade levels were more likely to participate. Larger schools were also more likely to participate. Table 2 on page 83 shows the variation in school enrollment by grade level and school SBP availability. We might expect that larger schools enjoy economies of scale, which would lower per capita costs of the SBP. SBP availability probably does not affect enrollment size. Schools with snack bars were more likely to participate in the SBP. The authors suggest that schools could serve the SBP through the snack bar.

Transportation, kitchen facilities, and required supervisory time did not significantly affect school participation. Students who did not participate in the SBP were more likely to ride to schools in cars. Students in the SBP were more likely to ride the bus. Nonetheless, the availability of bus service did not correlate with SBP availability. We might explain this relationship with the income disparity between the average SBP participant and the average non-SBP participant. Imagine that car rides to school are normal goods. Non-SBP participants, who are more affluent, will ride to school in the car more often than will SBP participants, who are less affluent. Schools that participated were more likely to have kitchen facilities and cafeterias, but the relationship was not significant. Supervisory time, defined as the length of time between teachers' arrival and the start of classes, did not affect school SBP participation.

The Decision to Eat Breakfast

Gleason splits SBP demand into two functions to compensate for contradictory income effects. Poorer students are more likely to skip breakfast, but poorer students face a lower price for SBP participation. Gleason (1995) and many other studies indicate that the SBP does not affect the probability that a student will eat breakfast. We conclude that the SBP exercises no effect on poor breakfast skippers. Hence, we can distinguish income's positive effect on breakfast consumption from income's negative effect on SBP eligibility. The two effects apply to different groups of people.

Age, sex, income, and race play important roles in the decision to eat breakfast.[28] In 1998, Siega-Riz and others examined the demographics of breakfast skipping. The authors analyzed data

[27] USDA FNS, 1978

[28] Gleason. Siega-Riz and others.

from the Nationwide Food Consumption Survey (NFCS) over 1965, 1977-8, and 1989-91. The number of people who ate breakfast declined dramatically over the period in question. Black children and adolescents and Hispanic adolescents skipped breakfast more often than did white students of the same age and sex. This difference held across all three survey periods. Beginning in 1977, lower-income students skipped breakfast more often than did higher income students of the same age, sex, and race. These gaps widened over the periods in question. From 1965 to 1991, the percentage of adolescent girls who skipped breakfast jumped from 15.6% to 35.3%. The authors did not find significant correlations between the decision to skip breakfast and mother's employment, family size, or location. The authors also found no relationship between breakfast skipping and school SBP availability.

Over the same period from 1965-1991, breakfast fat content decreased, and breakfast eaters consumed more whole grains, fruits, and cold cereals.[29] The study indicated that breakfast skipping increased over the period in question. Fewer people eat breakfast than did before, but those who eat breakfast eat more healthful breakfasts than they did before. Hence, the increase in the percentage of healthful breakfasts might simply indicate a decline in breakfasts consumed. Morgan and others found that children who eat non-sweetened cold cereals are less likely to skip breakfast than other students are.

Hanes and others (1984) examined data from the National Evaluation of School Nutrition Programs (NESNP). The NESNP consisted of a nationwide sample of 6,600 sets of students and parents and 1,900 schools. The surveys suffered very low response rates, and the study used participation on the day of the interview to proxy for SBP participation.[30] The researchers determined that students in school districts offering the SBP were more likely to eat breakfast. They found that 85% of students in SBP school districts ate breakfast, compared with 81.5% of students in school districts not offering the SBP.

Many economists and biostatisticians have examined the question, and no other researchers have found a significant relationship between SBP availability and breakfast skipping. Studies include Devaney and Stuart (1998),[31] Siega-Riz and others (1998), Gleason (1995),[32] Nicklas and others (1993b), Long (1990), and Devaney and Fraker (1989). These authors have tested, through

[29] Siega-Riz and others.
[30] Wellisch and Jordan. Rush, "... Editor's Technical Notes."
[31] Devaney & Stuart. Pages 14-17.
[32] Gleason. Page 218(S).

various methods, whether or not SBP availability affects the probability that a child will eat breakfast. The authors use three different definitions for breakfast. Some define breakfast as any food intake between 5:00 and 10:00 AM. Others define breakfast as any food or beverage[33] intake between waking up and 45 minutes after school begins. Still others define breakfast as any food intake between these times containing a minimum number of calories. Devaney and Stuart (1998) studied the effect of SBP availability on breakfast consumption using each of these definitions.

For example, Gleason used 1992 data from the School Nutrition Dietary Assessment Study (SNDAS) to analyze national NSLP and SBP demand. The SNDAS included interviews with food providers and 3350 students and their parents/guardians in 329 schools nationwide. Researchers considered different students' eating behavior over different one-week periods between February and May 1992.[34] On a given day, 11% of the students in the study skipped breakfast. When the researchers limited their sample to students attending SBP schools, they found that 12% of the students skipped breakfast.[35]

School Meal and NSLP Demand

Akin and others (1983a) took three days of data from the 1977-1978 NFCS and modeled the demand for school lunches. The authors found different demand functions depending on each child's age and free or reduced-price eligibility status. Note that the older students attend different schools than the younger students do. Middle schools and high schools might allow students to leave campus. The signs (+) and (-) in Table 3 on page 84 indicate positive or negative correlations.

Black students were more likely to participate in the NSLP than white students were in all four groups. Urban students were more likely to participate than rural students were in all cases except for full price-payers aged 12-18. The authors speculate that urban areas provide these particular students with more alternative places to eat lunch. The free and reduced-price equations do not include price as a regressor. Note that income and price are confounded in the Free/Reduced category.

[33] Not containing caffeine.
[34] Burghardt (1995), ". . . Study Design."

In 1995, Gleason found that 74% of the eligible students in the SNDAS registered for free or reduced price meals. The main reason that eligible students did not register was that they did not realize that they were eligible. Free/reduced meal certification (requires application) figured prominently in the demand function for school lunch. Given certification status, income did not exert a significant effect on NSLP demand. For full-price payers, Gleason (1995) found a price elasticity of .25, very comparable to .19 in Akin and others' model (1983a). Hence, demand for school lunches is very inelastic. Other variables that affect NSLP demand include access to other sources for lunch,[36] sex (males more likely to participate), age (negative correlation), and race.[37] Students also bought fewer lunches when the calories from fat dropped below 32%. The average school lunch provides 38% of its calories in fat.

McDonald notes the frequency with which eligible households combine benefits from multiple public assistance programs. Fraker and Moffitt found that many households received both AFDC and FSP payments. As we see from Akin and others' demand model for school lunches, FSP participation exerts some influence over school lunch decisions. Many school districts automatically grant free meal certification status to children of TANF or FSP recipients. Hence, families of these students need not report their income when applying for free meals.[38] This provision allows such students quicker and easier access to the programs. We expect that the demand functions for the SBP and other public assistance programs are mutually dependent.

SBP Demand

Gleason's 1995 study shows that, as with the NSLP, certification status, sex, and age figure prominently into SBP demand. Students certified to receive free breakfast were particularly likely to participate. Given that a student will eat breakfast, certification status, full price, age, sex, race, family income (negative), and urban/rural location determine SBP participation. Young, male, black, and rural students are more likely to participate than are other students. Students who are certified to receive free breakfast are considerably more likely to participate than are students who are not certified. Unlike with NSLP demand, though, students registered to receive reduced-price breakfasts are no more likely to participate than are full price-payers.

---

[35] Gleason.
[36] Though in-school carts and vending machines do not exercise a significant effect.
[37] The grouping of Asians, Native Americans, and Pacific Islanders was statistically more likely to purchase a school lunch than were other students.

Among full price-payers, price exerts a marginally significant ($.05 < P < .10$) effect on SBP participation, with an elasticity of 0.81. Hence, full price SBP demand is inelastic with respect to prices, but not nearly as much as NSLP demand is. Fat content and other taste considerations did not figure significantly into the SBP demand equation.[39]

Using survey data from the Bogalusa Heart Study, Nicklas and others observed a broad cross-section of children at two different periods in the mid-1980's. The group used parents' educational level as a proxy for income. The researchers noted that white children of more highly educated parents were more likely to eat breakfast at home than were white children of less educated parents. This relationship between home breakfast and parents' education level did not hold for black students. A majority (59%) of the black students ate breakfast at school, while a majority (56%) of the white students ate breakfast at home. Males were also more likely to participate in the SBP than females were.[40]

Sharon Long found that, while the NSLP only supplemented food expenditures by about 50%, the SBP totally supplemented food expenditures. Using data from household food expenditure surveys, Long analyzed families' spending patterns and preference functions for school meals. Long's model showed that a dollar of NSLP benefits reduced family food expenditure by about 50 cents. Alternatively, a dollar of SBP benefits did not reduce family food expenditure. Long's study indicates that all of the money spent on the SBP increases children's food intake. On the other hand, Long's study also indicates that half of the money spent on the NSLP does not increase children's food intake. This money simply increases the incomes of families in the NSLP. Recall that most NSLP participants do not qualify to free or reduced-price lunches.

In 1985, Akin and others showed that the decisive factor for FSP participation was knowledge of the four food groups.[41] Similarly, food group knowledge and general nutrition education may factor heavily into SBP demand. Nayga (1997) shows that black, female, higher educated, urban, and southern household heads exhibit a higher demand for nutritious food. Similarly, heads of households who are not employed outside of the home exhibit greater nutrition preference than do other household heads.

---

[38] Oberlin City School District. MSDE official Carol Fettweis says that the waiver is relatively new in Maryland.
[39] Gleason.
[40] Nicklas and others. "Nutrient Contribution of Breakfast. . ." Nicklas and others. "Breakfast Consumption Affects. . ."

The Effects of Breakfast and the SBP Effects on Students' Nutrient Intake

This subsection divides into two parts. First, we explore what foods students eat for breakfast. These include home breakfast foods and SBP menus. Second, we review literature describing the SBP's effects on students' 24-hour nutrient intakes. These studies rely on student and parent surveys.

Breakfast Foods

In 1998, Devaney and Stuart performed a USDA-funded evaluation of the SBP. The authors examined whether or not students ate complete breakfasts according to two different standards. The first standard for a complete breakfast included 10% RDA of five essential vitamins. The second standard for a complete breakfast included those vitamins plus representation from two of five major food groups. The authors found that, while SBP availability does not increase the probability that students eat breakfast, it does increase the probability that lower income students eat complete breakfasts by both definitions.

Students do not necessarily eat all the food offered in school meals. Senator Tom Harkin notes, anecdotally, that students waste much less food in the SBP than in the NSLP.[42] A school will only receive federal reimbursement for a school breakfast if the student takes at least 3 of the 4 items served.[43] In 1983, Akin and others (1983a) noted that elementary school students were required to take all five items with lunch.[44] In more recent years, however, the USDA has allowed schools to provide Offer Versus Serve (OVS) options in high schools to reduce food waste. More recently, the USDA has required that schools provide OVS options. Through OVS, students may reject one out of four breakfast items or one out of five lunch items.[45]

School breakfasts differ significantly from home breakfasts. Again using survey data from the Bogalusa Heart Study, Nicklas and others analyzed children's diets depending on where they ate breakfast. The authors observed a variety of differences in the breakfast food and nutrient

---

[41] Akin and others. "The Impact of . . ."
[42] 105th Congress. Page 3.
[43] Oberlin City School District. USDA FNS Pamphlet.
[44] Akin and others. "The School Lunch. . . Regression Analysis." Pages 478-479.
[45] USDA FNS Pamphlet.

compositions between 10-year-olds eating at home and 10-year-olds eating at school. The authors examined breakfast intake in detail and tracked basic 24-hour nutrient intake variables. In addition to those foods listed in Table 4 on page 85, home breakfasts were more likely to contain added sugar, salt, and fat.[46] Students who drank milk at home were more likely to drink whole milk, whereas students who drank milk at school were more likely to drink lowfat chocolate milk.

Compared with home breakfasts, breakfasts eaten at school contained a higher percentage of the day's calories, protein, carbohydrates, sodium, and sugars.[47] School breakfasts also contained slightly more cholesterol.[48] As mentioned earlier, Morgan and others compared the breakfast-eating habits of 5-12-year-olds as a function of non-sweetened cold cereal consumption. The authors found that non-sweetened cold cereal eaters ate more healthfully at breakfast and over the entire day than did other breakfast eaters. In 1998, Nicklas and others suggested that the introduction of cold and hot cereals and lower sodium entrees would improve the nutrient content of school breakfasts.[49]

SBP Effects on 24-Hour Nutrient Intake

In 1989, Devaney and Fraker performed the first analysis of the Dietary Impacts of the SBP. Their study showed that participation in the SBP positively affected calcium and magnesium intakes and negatively affected cholesterol and iron intakes. SBP did not significantly affect vitamin A or B_6 intake either way. The researchers expressed concern about SBP iron content, noting the prevalence of anemia in low-income children. The researchers examined 5-10-year-olds' and 11-21-year-olds' 24-hour nutrient intakes through survey data. All else equal, young SBP participants showed significantly lower vitamin A and iron intakes than did young non-SBP participants. Older SBP participants consumed significantly less vitamin B_6 than did older non-SBP participants. SBP participants showed significantly higher calcium intakes and significantly lower cholesterol intakes for both age groups. Other factors affecting nutrient intake included sex, female employment outside of the home (negative), education of female head (positive, particularly for older students), and geography. Males tended to consume more nutrients, as did younger students from the north-central or western United States.

[46] In the form of butter, margarine, or mayonnaise.
[47] School breakfasts contained more lactose and fructose, whereas home breakfasts contained more sucrose.
[48] Nicklas and others. "Nutrient Contribution of the Breakfast Meal. . ." Gleason
[49] Nicklas and others. "Nutrient Contribution of the Breakfast Meal. . ."

In 1993 and again in 1998, Nicklas and others showed that a student's degree of breakfast consumption affects his/her total daily nutrient intake. Children who skip breakfast compensate by eating more at lunch and dinner. Nevertheless, these children net fewer calories and nutrients over the day than do children who eat breakfast. Students who eat school breakfasts consume more nutrients and calories over the whole day than do students who eat breakfasts at home. Both groups net higher amounts of nutrients and calories than do students who do not eat breakfast.[50]

Akin and others (1983c) use NFCS data to examine nutrient effects of NSLP participation for children of different ages and income levels. The authors show that NSLP participation affects daily nutrient intake significantly more for younger children than for older children. The authors also show that NSLP participation affects nutrient intake significantly more for needy children than for non-needy children. The authors assume that the NSLP's affect on 24-hour nutrient intake is a simple function of an age dummy and an income dummy. Hence, all children below a certain age and income experience the same, higher nutrient effect. Similarly, all children above that cut-off age living above that cut-off income experience the same, somewhat smaller nutrient effect from the SBP. The authors estimate impact curves for a variety of nutrients for each of four different age and income groups. At the same time, the authors estimate the cut-off income, or switching point. At incomes above this point, the NSLP's nutrient effect drops from the higher constant effect to the lower constant effect. The authors construct impact curves for different nutrients. For each nutrient, they constructed four nutrient impact curves according to age and income groups. Their results show that free and reduced-price eligibility levels (130% and 185% poverty) fall well above the switching points for nutrient impact. The switching-point income occurred below 100% poverty in all cases, falling below 60% poverty for three nutrients for older children. Hence, extremely poor children stand to benefit significantly more from the NSLP than do less poor children who still qualify for free meals. Moreover, full price-payers stand to benefit just as much from the NSLP as do free and reduced-price eligible students at higher incomes.

The authors found a positive correlation between NSLP participation and 24-hour nutrient intake in virtually all categories. Moreover, needier children who ate non-NSLP lunches consumed

[50] Nicklas and others. "Nutrient Contribution of Breakfast. . ."
Nicklas and others. "Nutrient Contribution of the Breakfast Meal. . ."

lower amounts of most nutrients over the day than did needier children who skipped lunch.[51] We might conclude that eating home lunches actually hurts students by crowding out more nutritious consumption at other points in the day. Alternatively, home lunch-eaters might come from different types of families than do lunch-skippers. Lunch-skippers' families might expect the children to eat school lunches every day. If so, then lunch-skippers' families might exhibit the same nutrition preferences as NSLP participants exhibit. These switching-points might not carry over to the SBP, where we observe less food waste. In the first empirical model in this paper, we consider short-term effects for all children attending SBP schools. In the second empirical model in this paper, we consider long-term effects only for those children living below 100% poverty.

The Effects of Breakfast and the SBP on Students' Performance

We divide this subsection into types of effects. Medical researchers and some educators have studied ways in which breakfast and the SBP affect participants' physical, mental, and behavioral and academic performance.

Physical and Mental Performance Effects

Studies have demonstrated inconsistent correlations between breakfast and cognition.[52] The results of these studies differ considerably depending on the age and nutritional status[53] of the child and the type of cognition tested.

Tuttle and others (1954) tested the physical effects of eating cold cereal versus no breakfast on seven Iowa boys aged 12 to 14. The study turned up mixed results. Dickie and Bender (1982) did not find that eating breakfast on the day of a memory test affected London 12- and 15-year-olds' performance. An Ohio study by Cromer and others (1990) did not find that eating an SBP breakfast affected 12- to 14-year-olds' cognitive test performance. These tests evaluated memory, attention, and visual and auditory learning. Cromer and others suggest that breakfast may not affect older children's cognition as much as it affects 9-11-year-olds' cognition. Older children's metabolism and intellectual demands differ considerably from those of younger children.

[51] Akin and others. "The School Lunch. . . Regression Analysis."
[52] Pollitt (1995). Page. 1134.

Simeon and McGregor (1989) found that the Jamaican school breakfast improved normal 9-10½ year-olds' performance in math, language, and number memory tests. The breakfast negatively affected normal students' visual memory and wasted and stunted[54] students' language and number memory. Powell and others (1983) found that the Jamaican SBP improved malnourished adolescents' math performance and did not affect spelling or weight.[55] Lopez and others (1993) found that breakfast improved normal Chilean 8½- to 11-year-olds' short-term memory and problem solving ability. They did not find that breakfast improved normal children's attention. The authors also did not find that breakfast improved stunted or wasted children's inferior performance.[56]

Pollitt and others' (1996) results disagree with the study described above. Pollitt and others also found that breakfast skipping affected 9- to 11-year-old Peruvian boys differently, depending on nutritional status. In 1996, Pollitt and others performed a study of fourth and fifth grade (9 to 11 year-old) boys in the Peruvian Andes. The authors classified some children nutritionally at-risk and others not at-risk. The at-risk children did not perform as well in stimulus discrimination and memory search tests if they had skipped breakfast that morning. The same difference did not apply to students who were not at-risk. Students who were not at-risk discriminated visual stimuli more rapidly when they had missed breakfast. At-risk students did not display the same effect. Breakfast did not affect students' performance on number discrimination, picture vocabulary, shape and pattern recognition, or reaction time tests.

The studies above describe the SBP's cognitive effects over a single day. Pollitt and others went on to test the effect of one month of participation in the Peruvian SBP. Stunted children performed better on the vocabulary test if they had been eating school breakfast that month, but the effect was not significant. Other tests also showed no significant difference.[57] Attendance rates increased significantly during periods when the students were receiving school breakfast.[58] Pollitt (1998) has considered his results along with a number of studies conducted in Jamaica and in the US. Pollitt has concluded from these studies that a morning fast adversely affects short-term memory and problem-solving abilities among 9- to 11- year-olds. Pollitt's studies have also

---

[53] Normal, wasted, and stunted.

[54] Wasting (low weight/height) indicates recent malnourishment, while stunting (low height/weight) indicates past malnourishment.

[55] Powell and others. Pages 381-386.

[56] Pollitt and others (1996). Page (S)22-(S)23.

[57] Number discrimination, picture vocabulary, shape and pattern recognition, and reaction time.

[58] Pollitt and others (1996). Page (S)23-(S)25.

shown that long-term nutrient deficiencies impair cognitive abilities. He draws particular attention to the positive effect that SBPs exercise on school attendance.[59]

Behavioral and Academic Performance Effects

Remarkably few studies have addressed behavioral and academic performance effects of the existing US SBP. Tuttle and others anecdotally noted that breakfast improved students' attitudes and scholastic attainments. Work by Murphy and others confirms some of this anecdotal evidence. Meyers and others observed performance effects of the introduction of an SBP in a Massachusetts school district. More recent studies have evaluated the effects of universal breakfast programs.

Recently, Murphy and others found that hunger plays a large part in low-income American children's behavior and academic performance. The authors conducted parent and child surveys and analyzed the responses. The authors found that hungry children were more prone to hyperactive behavior and impaired mental functions than other children were. Hungry children were also more likely than other children were to exhibit tardiness and absenteeism.[60]

The Lawrence, Massachusetts school district began to offer the SBP halfway through the school year in January 1987. The households of 7.6% of the eligible students declined free or reduced-price breakfasts. Using these 7.6% as a control, Meyers and others (1989) observed a change in 3rd and 6th graders'[61] performance related to SBP participation. Over the semester, participants' mathematics, language, and reading test scores and attendance improved as compared with non-participants. Most significantly, tardiness dropped dramatically among SBP participants. The researchers speculate that tests improved due to some combination of morning intake and overall health gains. These improvements might not entirely reflect results of the SBP. The variables that influenced student participation may have influenced student performance.

Over the 1997-1998 school year, Murphy and others worked to apply their breakfast research results to the SBP. Murphy and others offered free breakfast to all students in two Baltimore schools and in one Philadelphia school. The authors surveyed students, parents, and cafeteria staff. From the onset, Murphy and others found that SBP participants were less likely to report

[59] Pollitt and others. "Does Breakfast . . ."
[60] Murphy and others (1998).

depression or anxiety than were other students. Moreover, SBP participants earned higher math grades on average than other students did.[62]

Once they implemented the program, Murphy and others observed a number of positive effects. Over the four-month study period, SBP participation rose from 15% to 27% of the total students. Universal free breakfast raised students' math grades, increased their attentiveness, and lowered the incidence of in-school behavioral problems.[63] Students' behavior at home and grades in other subjects did not change significantly. Kleinman[64] explained that, while serious starving is uncommon in the US, the child hunger that does exist seriously affects children's academic performance.

The Maryland State Department of Education (MSDE) expanded this study over the 1998-1999 school year to include four other schools in four other Maryland counties. Murphy and others continue to collect data from participating schools. The recent pilot program offers free breakfast to all students, in the classroom. MSDE officials speculate that universal in-class breakfast eliminates the stigma cost of the need-based SBP.[65]

Similarly, the state of Minnesota has provided funds to provide a universal free breakfast program in 41 schools. Recently, state and local authorities have been funding studies of the program.[66]

Bro and others (1994) examined the effects of universal in-class breakfast on high-risk 14- to 18-year olds in a Spokane, Washington vocational (welding) high school.[67] The study recorded an increase in on-task behavior on days when breakfast was served. The study recorded no significant increase in attendance.

Economic Returns to Schooling and to School Quality

In this next section, we discuss some studies relating school to earnings. Many studies have attempted to explain children's attainments as functions of various inputs, including government

[61] 8½ to 9 and 11½ to 12 years of age.
[62] McGreevey.
[63] Sacks. McGreevey.
[64] Another researcher in the study
[65] Peiffer and Kerry, MSDE Bulletin.
[66] Peiffer and Kerry. Begale.
[67] High-risk denotes single parent households, adoption, teen parenthood, or behavior problems.

spending on different programs. Haveman and Wolfe (1995) provide a extensive review of such studies, including a discussion of relevant theory, data, and empirical methods. In this paper, we empirically evaluate academic effects of the SBP. We would like to extend our results to draw conclusions about the SBP's economic effects. To do so, we use results from human capital literature. Researchers have also empirically tested the economic benefits of improving school quality. Using these results, we compare the marginal cost-effectiveness of the SBP with the marginal cost-effectiveness of hiring new teachers.

Returns to Years of Schooling

Card and Krueger (1992a) use US Census data to estimate the returns to schooling for white males born in the 1920's, 1930's, and 1940's. The authors estimate average returns by state of birth. Returns to education ranged from 3.6%/yr. to 7.1%/yr. in the 1920's, and the returns ranged from 5.8%/yr. to 8.3%/yr. in the 1940's. The authors measure returns to education as percentage wage increase per year of schooling.

Some studies in recent years have focused on economic returns to schooling holding family background and genetic endowments constant. Ashenfelter and Krueger (1992) and Ashenfelter and Rouse (1998) examine samples of twins with differing education levels. In both studies, the authors collected data from twins attending the Annual Twins Days Festival in Twinsburg, Ohio. These twins were more highly educated, more highly paid, younger, and more likely to be white or female than were average Americans. Moreover, the authors speculate that twins attending the festival exhibit more intra-pair similarity than do average twins. Among twins from the 1991 festival, Ashenfelter and Krueger measured an average 16%/yr. wage increase per year of schooling. Ashenfelter and Rouse use a larger data set, collected from 1991-1993, involving two or three observations for a number of pairs. Ashenfelter and Rouse obtain a more conservative estimate of a 9%/yr. wage increase resulting from a year of schooling. The authors estimate different returns to schooling based on intra-pair averages for educational attainment. They find that the returns range from 11%/yr. at 9 years of education to 8%/yr. at 18 years of education.

Ashenfelter and Zimmerman compared father/son pairs and pairs of brothers from the National Longitudinal Survey (NLS). The authors estimated an average return to schooling between 4.6%/yr. and 8.4%/yr. The researchers' estimates changed depending on the expected measurement error. Fathers and younger brothers experienced significantly higher returns to schooling relative to sons and older brothers, respectively.

School Quality and Earnings

A number of economists have examined whether or not school quality affects children's educational attainment (in years) or future earnings. These economists' findings and techniques will help us to understand long-term effects of the SBP. In this section, we review recent literature that considers the association between school quality and education and earnings. We also briefly discuss similar studies of the long-term effects of other policies affecting children and adolescents.

School Quality and Earnings, Holding Education Level Constant

School quality affects both students' educational attainment and their earnings. First, higher-quality schools motivate some students to attend school longer. By attending school longer, students acquire more relevant skills, information, and credentials. Consequently, these students end up earning more, later in life. As Card and Krueger argue, the correlation between school quality and earnings may become negative when we hold years of education constant.

The most talented, ambitious students respond to high-quality schools by attending school longer. The least motivated students will drop out of school, no matter what. Some talented, ambitious students do not have access to high-quality education. These students may feel stifled in school, and they may find better ways to spend their time.

Let us compare 10^{th} grade dropouts from a high quality school with 10^{th} grade dropouts from a low-quality school. The set of dropouts from high-quality schools *will not* contain the most talented and ambitious students. The set of dropouts from low-quality schools *may* contain some of the most talented and ambitious students. Consequently, dropouts from high-quality schools earn less, on average, than do dropouts from low-quality schools. Holding years of education constant at lower levels of educational attainment, we observe a negative relationship between school quality and earnings.

We do not expect to see this negative relationship at the highest levels of educational attainment. Only the most talented and ambitious students rise to the levels of doctor or lawyer. At the highest levels of educational attainment, holding years of education constant, school quality does not exercise a negative effect on earnings.

Students at higher-quality schools acquire more relevant skills and information each year than do students at lower-quality schools. A year of high-quality education better prepares a student for the workplace than does a year of low-quality education. Hold attainment constant at the highest levels, where the negative effect does not hold, and school quality exercises a positive effect on earnings. Among doctors and lawyers, graduates from high-quality elementary and secondary schools earn more than do graduates from low-quality elementary and secondary schools. Figure 1 on page 85, copied from Card and Krueger (1998), illustrates this positive effect on earnings.

Note the negative relationship between earnings and quality at low levels of education. The high and low quality education-earnings curves intersect each other at a point above the origin. Card and Krueger locate this intersection around high school graduation.[68]

A selection process occurs around high school graduation, so that the most talented and ambitious students begin to attend the best colleges. Hence, the college admission process creates a positive correlation between school quality and student ability independent of the effects of the education process. Consequently, the actual functions for the curves shown in Figure 1 may not be as continuous as Figure 1 suggests.

Long-Term Effects of School Quality

Recent studies of school quality returns have focused on teacher-pupil ratios and relative teacher salaries. Hanushek (1986, 1989) and Hanushek and Taylor (1989) have evaluated the effectiveness of a number of different school quality measures. First, we review these discussions of school quality measures. Next, we examine empirical studies of the effects of school quality. In 1992, David Card and Alan Krueger published two studies observing the effects of school quality on students' future earnings. In one study, the authors examine the role of school quality in closing the black/white wage gap for southern-born men born between 1900 and 1949. The authors pursued this concept further in 1996 by studying racially segregated schools in North and

South Carolina from 1900-1960. Note that these studies estimate positive earnings effects of school quality, because they do not hold education level constant.

Eric Hanushek finds a number of problems with traditional measures of school quality. In particular, he argues that expenditure per pupil does not provide an accurate measure. If we use expediture per pupil as a proxy for school quality, then we assume that all schools spend money with equal efficiency. Schools do not all spend money with equal efficiency, and schools generally allocate money inefficiently. Moreover, if we use expenditure per pupil as a proxy for school quality, our estimates capture variation in students' family backgrounds. Students at more affluent schools tend to come from more affluent families. Hanushek argues that much of the variation in school quality derives from variation in unmeasurable teacher quality. Many of the school quality variables that Hanushek (1986, 1989) and Hanushek and Taylor (1989) discuss exhibit reverse causation. When the average test scores decline in a school, the school district may increase funding to remedy the problem. This reverse causation causes an even greater measurement problem when we do not know the expected lag structure of the independent variables. Hanushek and Taylor note that a number of school quality studies use SAT scores as their dependent variable. Students decide to take the SAT based on their expected performance. Consequently, variation in SAT scores would not measure differences among marginal test-takers. Moreover, SAT-takers and non-SAT-takers may exhibit different returns to school quality. These test-taking differences could bias estimates that use SAT scores as a dependent variable.

Card and Krueger use pupil-teacher ratios to proxy for school quality because they measure specific services provided to the students. In order to examine the effect of school quality, Card and Krueger assume each respondent attended school in his state of birth.[69] They use state averages for school quality measures from the *Biennial Survey of Education* (1920-1958) and the *Digest of Education Statistics* (1960-present). Rather than examine school quality's effect on test-scores, Card and Krueger examine long-term performance measures. They use earnings data from the 1980 Census Public-Use A Samples. These samples link together individuals' responses for a number of survey questions. Variables include income, educational attainment (in years), state of birth, and state of residence. The samples include 5% of the US population.

---

[68] Card and Krueger (1996). Page 38.

[69] During the schooling periods in question, about 90% of students attended school in the states of their birth. When they account for this as a probability, rates of return to schooling increase by 5-15%, and the standard deviations rise by a corresponding amount. Card and Krueger (1992a). Pages 28-29.

Card and Krueger studied racially segregated schools in North and South Carolina from 1900-1960. Schools for black students in North Carolina enjoyed better resources than did schools for black students in South Carolina. Schools for white students in South Carolina enjoyed better resources than did schools for white students in North Carolina. The school qualities for the Carolinas converged over the period in question. The authors find from the Carolina data that a 10% reduction in class size led to a 0.4% to 1.1% increase in a child's future earnings. Figures that the authors examined in previous studies indicated that a 10% increase in school spending led to a 1-2% increase in students' future earnings.

In another 1992 study (1992a), the two authors consider school quality as it affects the incomes of white males born between 1920 and 1949.[70] Card and Krueger tested relative earnings as a function of education level, state of residence, urban versus rural residence, and state of birth. They found that the rate of return to years of schooling varied significantly by state of birth.

The authors then linked each state to a variety of school quality measures, controlling for state- and cohort-effects. These measures included average pupil-teacher ratio, average length of school term, and average teacher salary (relative to other professions) by state. They found significant effects for pupil-teacher ratio and for teacher salary.[71] Controlling for education, the authors found no significant correlation between parents' incomes or years of education and earnings.[72]

Julian Betts (1995) contrasted his study of the returns to school quality with Card and Krueger (1992a). Betts used data from the 1979 National Longitudinal Survey of Youth (NLSY79) to examine high school quality's effect on white men's later earnings. With the NLSY79 data, Betts measured school quality more precisely by isolating the exact schools that respondents attended.

Over three fourths of the schools in the sample contained more than one NLSY79 respondent. Betts found that respondents' incomes varied significantly according to the high school that each respondent had attended.[73] Betts tested for the income effects of library access, parents' education levels, and family income, none of which exercised significant effects.

[70] Card and Krueger (1992a).
[71] Card and Krueger (1992a). Page 19.
[72] Card and Krueger (1992a). Page 3.
[73] $p < 0.000005$

Next, Betts examined income effects of three school quality variables: teacher/pupil ratio, relative teacher salary, and percentage of teachers with graduate degrees. None of these variables exercised significant effects on students' earnings. Alternatively, when Betts used statewide averages for teacher/pupil ratio and for relative teacher salaries, he found that the teacher/pupil ratio affected income significantly.

Betts suggests a number of possible reasons for this discrepancy. Statewide effects include effects for grades K-8, while NLSY79 school quality data only measure high school quality. Earlier school quality may affect earnings more significantly than high school quality does. Betts also addresses the young age at which respondents reported their incomes. Card and Krueger's subjects ranged in ages from 30 to 59. Julian Betts's subjects, on the other hand, ranged in ages from 17 to 32. Card and Krueger's older sample benefits from more highly educated respondents with more serious jobs. Older respondents are less likely to hold temporary jobs.

Most significantly, Betts does not use pooled data. Because he only uses cross-sectional data, Betts is unable to control for school-specific effects. Schools vary considerably not only in resources, but in the types of students that attend. Betts's study finds that rates of return to education vary significantly according to the type of student who attends the school. The percentage of disabled students at a school negatively affects high school dropouts' earnings and positively affects high school graduates' earnings. Betts does not propose an explanation for this changing relationship. Betts also finds that white males from bigger high schools earn slightly higher incomes than do white males from smaller high schools. School and student population differences account for a large part of Betts's variation. These and many other school-specific variables add to Betts's error terms. Consequently, Betts' error terms may drown out school quality effects. When he uses state averages for school quality, Betts finds significant effects. State school quality averages are likely to reflect educational policy, whereas school-specific measures may simply reflect school-specific demands.

In 1986, Hanushek found no significant correlation between school quality variables and students' educational attainment. Hanushek used pupil-teacher ratios, teacher salaries, and school expenditures as quality measures. Hanushek measured educational attainment trends by year for the US and for the state of Iowa. Hanushek did not measure cross-sectional differences. Consequently, Hanushek's study may suffer from similar problems as Betts's study. A number of exogenous variables may influence year-to-year variation in educational attainment. These variables might include family income, immigration, racial composition, and family structure.

Further School Quality Research and Long-Term Effects Studies

In October 1999, Krueger and Whitmore published a study on Tennessee's Project STAR. Project STAR randomly assigned 12,000 elementary school students to particularly small classes for grades K-3. Krueger and Whitmore examined the program's effects on test scores and college test-taking. The authors account for self-selected test-taking by examining test-taking behavior in addition to considering test scores. The authors communicated with the Tennessee DofEd and with Educational Testing Services (ETS) to combine data. Krueger and Whitmore found that Project STAR significantly improved the probability that students, particularly black students and lower income students, would take the ACT or SAT. Project STAR also improved students' performance on the tests by about 0.10 standard deviations. STAR improved black students' ACT and SAT performance by 0.20 to 0.26 standard deviations.

Economists have used Card and Krueger's methodologies to measure long-term effects of other policies related to youths and education. Angrist and Krueger (1991) examine compulsory attendance laws and their effects on long-term student performance. The authors plot educational attainment and earnings as functions of a student's season of birth, and they find significant effects. Students who are young for their grades stay in school longer and earn more later in life due to a minimum dropout age. Angrist and Evans (1999) examine the ways in which abortion reforms in the 1970's affect female educational attainment. White women did not show sufficient variation in behavior to measure the effects. Exposure to abortion reforms positively affected black women's educational attainments. Evans and Dee (1997) examine educational attainment as a function of minimum drinking age. Dee and Evans find that lower minimum drinking ages increase the probability that a teenager will drink. Using PUMS data, the authors find that drinking age does not exercise a significant effect on educational attainment.

STATISTICAL MODEL

In this section, we develop a statistical model to describe short- and long-term effects of the SBP. We construct this model to formalize our argument that the SBP exercises long-term effects on students' academic and labor market performance. The equations in the model below correspond to the ovals in the flow chart on page 86. We do not model school SBP availability, below, but recall that we discussed SBP availability in the literature review on page 12. We expect that all the equations in the statistical model exhibit serial correlation. We omit auto-correlation terms from Equations (1) through (9) for simplicity.

Student SBP Participation

As mentioned on page 16, certification status, family income, age, sex, race, and urban/rural location weigh heavily into SBP demand. Given that a student's school participates in the SBP, we might model SBP demand as follows:

$$(1) \qquad SBP_{i,t} = \alpha_1 + \alpha_2{*}YFam_{i,t} + \alpha_3{*}Price_{i,t} + \alpha_4{*}FElig_{i,t} + \alpha_5{*}RElig_{i,t} + \alpha_6{*}X_{1,i,t} + u_{1,i,t}$$

Our universe includes all students K-12 who attend SBP schools. $SBP_{i,t}$ represents the fraction of mornings in the school year t in which student i eats a school breakfast. $YFam_{i,t}$ represents the student's family income that year. Given eligibility status, this effect may be positive or negative, depending on whether or not school breakfast is a normal good. The free SBP is not a normal good, because a higher income does not reduce the cost of participation. $Price_{i,t}$ represents the price that the pupil faces for school breakfasts. $Price_{i,t}$ varies from student to student depending on eligibility status and the school that the student attends. For instance, $Price_{i,t}$ returns a zero if student i lives at 130% poverty or below. We expect the price effect to be negative. $FElig_{i,t}$ returns a one if the student is eligible for free meals and a zero otherwise. $RElig_{i,t}$ returns a one if the student is eligible for reduced-price meals and a zero otherwise. We expect that both eligibility dummies exert positive effects on SBP demand. We expect α_4 to exceed α_5. Students eligible for free meals face a lower cost for SBP participation than do students eligible for reduced-price meals. $X_{i,t}$ represents a vector of control variables. We expect that $X_{i,t}$ would include the student's age in year t and dummies for the student's sex and race. $X_{i,t}$ would also include a dummy to represent whether the student lived in an urban or rural location in year t. As Gleason (1995) notes, younger students are more likely to participate in the SBP than are older students. Black students are more likely to participate than are non-black students. Male students are more likely to participate than are female students, and rural students are more likely to participate than are urban students.

Gleason (1995) found that price and income affected student participation differently depending on each student's certification status. To estimate these different effects for income, we can multiply the eligibility dummies each by $YFam_{i,t}$. Note that, for all students eligible for free meals, $Price_{i,t}$ returns a zero. Hence, we would not estimate a free meal price effect. To estimate the different effects for the price, we would multiply the reduced-price eligibility dummy and the non-eligibility dummy each by $Price_{i,t}$. Hence, we create five different regressors in place of $YFam_{i,t}$ and by $Price_{i,t}$. With these specifications, Equation (1) becomes Equation (1a), below:

$$(1a.)\quad SBP_{i,t} = a_1 + a_2*(FElig_{i,t})*YFam_{i,t} + a_3*(RElig_{i,t})*YFam_{i,t} + a_4*(NElig_{i,t})*YFam_{i,t} + a_5*(RElig_{i,t})*Price_{i,t} + a_6*(NElig_{i,t})*Price_{i,t} + a_7*FElig_{i,t} + a_8*RElig_{i,t} + a_9*X_{1a,i,t} + u_{1a,i,t}$$

$NElig_{i,t}$ represents $(1 - FElig_{i,t})*(1 - RElig_{i,t})$, and returns a one if the student is not eligible for free or reduced-price meals and a zero otherwise. We expect a_2 to be negative. Among students eligible for free meals, as income rises, family income comes closer to the eligibility threshold. Hence, as a family's income rises, the family is less likely to know its eligibility status. Hence, given free meal eligibility, families with higher incomes are less likely to register to receive free meals. Moreover, because free meals have a price zero, we expect that free meals are inferior goods. We do not predict signs for a_3 or a_4, because we do not know whether or not reduced-price or full-price school breakfasts are normal goods. We expect negative signs for both price effects.

Short-Term Effects

For the purposes of this paper, short-term effects last less than one school year. Short-term effects include nutrient intake, health, attendance, and cognition improvements resulting from the SBP. For all the short-term effects we consider below, our universe includes all US students K-12. For the short-term effects we consider in our empirical model, our universe includes all US students K-12 attending SBP schools.

Nutrient Intake

As described in the literature review, researchers have found that the SBP increases a student's expected intake for many nutrients. We are most interested in nutrient intake adjusted for each student's nutrient needs. Hence, we divide nutrient intake by each student's body weight in pounds.

(2) $$Ntr_{i,t,n} = \beta_{1,n} + \beta_{2,n} * SBP_{i,t} + \beta_{3,n} * X_{2,i,t} + u_{2,i,t}$$

$Ntr_{i,t,n}$ represents student i's intakes per pound body weight in year t for a vector of nutrients n. $X_{2,i,t}$ represents a vector of controls by which nutrient intake might vary. These controls might include the student's age, income, and geographic area in year t as well as the student's age and sex. We might also include variables to represent parents' education levels and the female household head's degree of employment outside of the home. As noted in the literature review, the SBP may exert a negative effect on a student's intake for some nutrients, such as iron. For most nutrients n, however, we expect that the SBP exerts a positive effect on nutrient intake and that $\beta_{2,n}$ is positive.

Physical and Emotional Health

Murphy (1998) has recently shown that universal free SBPs reduce the number of school nurse visits and reported emotional and behavioral problems. Hence, we expect that SBP participation positively influences a student's physical and emotional health.

(3) $$Health_{i,t} = \gamma_1 + \gamma_2 * Ntr_{i,t,n} + \gamma_3 * X_{3,i,t} + u_{3,i,t}$$

(4) $$EHealth_{i,t} = \delta_1 + \delta_2 * Ntr_{i,t,n} + \delta_3 * Health_{i,t} + \delta_4 * X_{4,i,t} + u_{4,i,t}$$

$Health_{i,t}$ and $EHealth_{i,t}$ represent abstract measures of physical and emotional health, respectively. Measures might include number of school days divided by the number of nurse visits or the number of disruptions in class, respectively. We describe emotional health as happiness, attitude toward school, and motivation. We expect that nutrient intake positively affects both physical and emotional health, so that $\gamma_2 > 0$ and $\delta_2 > 0$. We also expect that physical health positively affects a student's attitude so that $\delta_3 > 0$. $X_{3,i,t}$ and $X_{4,i,t}$ represent control vectors. We might wish to control for age, sex, race, urban/rural location, family income, and parent's education levels. In $X_{4,i,t}$, we might also control for family size and whether or not the child lives with both biological parents.

Attendance

We would like to measure the SBP's effect on students' attendance. As shown in Figure 2, this effect works through a number of different channels. The SBP affects attendance directly by providing an incentive for children to attend school. The SBP also improves students' nutrient intake, which, in turn, improves students' physical and emotional health. Less healthy students are more likely than other students are to miss school because of illness. As a student becomes better adjusted or more motivated, we expect the student to feign illness less often or to skip school less often.

$$(5) \qquad Attend_{i,t} = \varepsilon_1 + \varepsilon_2{*}Health_{i,t} + \varepsilon_3{*}EHealth_{i,t} + \varepsilon_4{*}SBP_{i,t} + \varepsilon_5{*}X_{5,i,t} + u_{5,i,t}$$

$Attend_{i,t}$ represents the proportion of school days in school year t in which student i attends school. We expect ε_2, ε_3, and ε_4 all to be positive. $X_{5,i,t}$ represents a vector of controls which might include sex, age, race, school quality, family income, and parent's education level.

Cognition

A number of studies have shown that children can improve their performance on standardized tests by eating healthful breakfasts. We expect that nutrition positively affects test performance. We also expect that better adjusted or more motivated students perform better on tests.

$$(6) \qquad Cog_{i,t} = \zeta_1 + \zeta_2{*}Ntr_{i,t,n} + \zeta_3{*}EHealth_{i,t} + \zeta_4{*}X_{6,i,t} + u_{6,i,t}$$

$Cog_{i,t}$ denotes cognitive ability. We might measure $Cog_{i,t}$ with a test score or with a vector of different test scores. In $X_{6,i,t}$, we might control for sex, race, age, urban/rural location, family income, parents' education levels, and attendance.

Hanushek (1986) notes that researchers have not found convincing evidence linking school quality and standardized test performance. Few researchers would doubt that cognitive abilities influence wages. See Heckman (1995) and Murnane and others (1995) for discussions of the importance of cognitive skills in wage determination.[74] Card and Krueger (1992a) find that school quality significantly affects students' earnings, even if it does not significantly affect

[74] Murnane and others (1995). Card and Krueger (1998).

students' cognitive test performance. As proposed in Figure 2, cognition is not the only channel through which the SBP can affect students long-term performance or earnings.

Long-Term Effects

We define long-term effects to last one school year or longer. These effects include school performance and future earnings. The SBP does not directly affect either of these long-term variables. Consequently, we do not include SBP as a regressor in either equation. Nevertheless, the independent variables in the equations below trace back to SBP participation through a cause-and-effect chain.

School Performance

A number of factors affect a student's performance in school. Among the variables that we have already defined, we expect cognition, emotional health, and attendance to affect school performance. For whatever reasons, some students perform better than others do in controlled settings. We expect differences in school performance to reflect these differences. Cognitive tests also capture some of these differences. Emotional health may affect school performance in ways that cognition does not capture. For instance, students may test well but act disruptive in classroom settings. Students who attend school more often expose themselves to the material more. Hence, we expect attendance to improve school performance.

$$(7) \qquad Perf_{i,t} = \eta_1 + \eta_2{*}Cog_{i,t} + \eta_3{*}EHealth_{i,t} + \eta_4{*}Attend_{i,t} + \eta_5{*}X_{7,i,t} + u_{7,i,t}$$

$Perf_{i,t}$ represents an abstract measure of student i's performance in school year t. We could measure this variable with class rank, grade-point average adjusted for class difficulty, or a vector of class grades, also adjusted for class difficulty. We expect η_2, η_3, and η_4 to be positive. We might include race, sex, family income, and parents' education levels in $X_{7,i,t}$.

Educational Attainment

Once we understand students' performance in school, we can begin to model students' educational attainment (in years). Given school performance, variables like cognition and attendance become irrelevant. We are not interested in the methods that lead to the student's success. We are only interested in the outcome. If a student performs better in school, then that student can expect to earn a higher grade and to learn more from the next year of schooling. A student who performs better in school in year t has a greater probability of passing the next grade in year $t+1$. Hence, the student who performs better in school faces a smaller risk in attending another year of school. We also expect that students who are better adjusted or more motivated stay in school longer.

(8) $\quad Attain_i = \theta_1 + \theta_2{*}Perf_{i,t} + \theta_3{*}EHealth_{i,t} + \theta_4{*}X_{8,i,t} + u_{8,i,t}$

$Attain_i$ represents the (former) student's highest grade completed. We measure this variable in years of education not including failed grades. We expect θ_2 and θ_3 to be positive. $X_{8,i,t}$ denotes a vector of control variables which might include sex, race, school quality, family income, and parents' education levels.

Future Earnings

In the equation below, we address earnings as an indirect function of the SBP. Earnings do not precisely measure the quality of a job or a subject's total welfare. A number of other variables figure into any given individual's utility function. Although earnings do not correspond precisely with utility, earnings data do provide us with important information about each (former) student's welfare. The SBP's earnings effect is particularly interesting because we are evaluating the effects of a program designed in part to reduce poverty.

(9) $\quad Y_{i,t} = \iota_1 + \iota_2{*}Attain_i + \iota_3{*}Perf_{i,t} + \iota_4{*}Cog_{i,t} + \iota_5{*}X_{9,i,t} + u_{9,i,t}$

$Y_{i,t}$ denotes (former) student i's future earnings for year t. Persons with higher education levels earn higher wages. Similarly, persons who performed better as students are likely to perform better at their jobs. We expect persons who perform better at work to earn higher wages. For control variables, we might include sex, race, school quality, and parents' incomes and education levels.

We have constructed a long chain of relationships. By substitution, this chain traces earnings and educational attainment back to a multivariate function that includes SBP participation on the right-hand side. In the empirical model below, we ignore many of the intermediate steps, and we regress attendance and educational attainment directly onto SBP participation. Having done so, we regress employment and wages onto educational attainment to help us to interpret our results.

EMPIRICAL MODEL

We test academic effects of the SBP with two empirical models. In our first model, we examine the SBP's statewide aggregate attendance rates. In our second model, we use longitudinal data to estimate the SBP's effects on educational attainment and earnings. We begin by describing the available data. Second, we describe the variables and equations that we use, and how they apply to our hypotheses. Finally, we examine our results and draw conclusions. On pages 101-105, we include glossaries of variables for the empirical models.

Data

Our models make use of data from the USDA and from the NLSY79 Geocode file. The USDA has provided us with statewide aggregate data about the SBP. The USDA data set also includes some statewide aggregate statistics about schools that participate in the SBP, such as attendance and enrollment. The NLSY79 provides us with a variety of information about a 12,686 of individuals born between 1957 and 1965. This information includes variables describing these individuals' demographics, family backgrounds, education levels, and labor market experiences.

In addition the data described below, we also use statewide attendance and enrollment figures from the NCES's annual *Digest of Education Statistics*. These data measure enrollment annually by state from 1962-1997 and attendance annually by state from 1962-1995. When we explore labor market effects in the second section of Model 2, we use CPI and employment and population data obtained from the Bureau of Labor Statistics (BLS). In Model 2b, we also make use of labor market and interest rate data from the 1995 *Economic Report of the President*.

USDA Data

The USDA has provided us with SBP data both annually by state and nationwide by month for a number of variables. These data cover 32 years, beginning in 1967, when the pilot SBP began. The data cover 58 states and territories, but we will only consider the 50 states and Washington, DC. Table 5 on page 87 lists the variables available through the USDA SBP data set.

The different SBP variables provide information about different aspects of SBP availability and participation. If a school receives any federal reimbursement for the SBP, we call that school an SBP school. According to the provisions of the program, any child attending an SBP school may participate in the SBP. We use the *Enrollment* and *Average Daily Attendance (ADA) in SBP schools* variables in our first empirical model. These variables tell us how many children had the opportunity to participate in the SBP in a given state and year.

SBP participants do not necessarily eat school breakfasts every morning. The *Participants* variable tells us how many students participated in the SBP at all. Only students enrolled in the school may participate in the program, so *Participants* will necessarily be smaller than *Enrollment in SBP schools*. As explained on page 9, many schools offer the NSLP but not the SBP. The next two variables tell us how many students were registered to receive free or reduced-price meals in the state that year. These students include lunch buyers at non-SBP schools. For the USDA to approve a student to receive free or reduced-price meals, the student must both apply and meet income eligibility requirements. If a student attends an SBP school, then that student faces a greater incentive to register for free or reduced-price meals. Unfortunately, the USDA does not differentiate between eligible students at SBP schools and eligible students at non-SBP schools.

The USDA also provides information about the number of school breakfasts served in each state each year. The *Total Breakfasts*, *Free Breakfasts*, and *Reduced-Price Breakfasts* variables give the numbers of meals served over the entire year. *Average Daily Breakfasts* represents the average number of breakfasts served in the state that year during the peak month nationwide.

Average Indemnity Rate denotes the percentage of schools with more than 40% of the students living at 130% poverty or below. These schools qualify for special need status, which, as explained on page 8, grants them higher reimbursement rates per breakfast.

Federal Reimbursement includes Start-Up funding and reimbursements per meal, which depend on each student's eligibility status and each school's special need status. During the earliest years of the program, the USDA tracked more specific information about the SBP budget.

In order to better understand the SBP data, we examine Equations (10), (11), and (12) as illustrated in Tables 7, 8, and 9:

$$(10) \quad BPA_{s,t} = \kappa_1 + \kappa_2 * u_{10,s,t-1} + \kappa_3 * u_{10,s,t-2} + u_{10,s,t}$$

$$(11) \quad BPA_{s,t} = \lambda_1 + \lambda_2 * Year_t + \lambda_3 * u_{11,s,t-1} + \lambda_4 * u_{11,s,t-2} + u_{11,s,t}$$

$$(12) \quad BPA_{s,t} = \mu_1 + \mu_2 * State_s + \mu_3 * u_{12,s,t-1} + \mu_4 * u_{12,s,t-2} + u_{12,s,t}$$

$BPA_{s,t}$ represents total breakfasts served in state *s* and year *t* divided by state *s*'s ADA in year *t*. Our sample includes observations from 51 states (including DC) for the years from 1967 to 1995. $Year_t$ represents a vector of dummies for each year from 1970 to 1995; we omit the years 1967-1969 for comparison. $State_s$ represents a vector of 50 dummy variables, one for each state (including DC); we omit New York for comparison.

In Equation (10), we examine $BPA_{s,t}$ as a simple function of a constant and two auto-correlation terms. In Equation (11), we regress $BPA_{s,t}$ on the two auto-correlation terms and 26 year dummies. In Equation (12), we regress $BPA_{s,t}$ on the two auto-correlation terms and 50 state dummies.

We see from Table 7 that the data are highly serially correlated, and that these two regressors together with a constant explain 98% of the variance in $BPA_{s,t}$. When we omit $\kappa_3 * u_{10,s,t-2}$, our Durbin-Watson statistic drops from 2.04 to 1.37. This change indicates that $\kappa_3 * u_{10,s,t-2}$ controls for a significant amount of serial correlation in the model. When we do omit $\kappa_3 * u_{10,s,t-2}$, κ_2 drops from 1.34 to 1.02. We can offer no explanation for κ_3's negative value. For some reason, given serial correlation for one year, an overestimate in year *t* leads to an underestimate in year *t* +2. Nevertheless, this anomaly does not affect our models. The dependent variables in our empirical models do not exhibit serial correlation past a single year.

In Equation (11), evaluated in Table 8, we include fixed year-effects to capture nationwide annual changes in the program. We omit the first three year-dummies, so we can consider our year-dummy coefficients in comparison to the first three years of the program. Individually, none of the year dummies show significant effects. Nevertheless, adding the year-dummies together reduces the sum of squared residuals by 20%. Using a chi-square test, we can reject the null hypothesis that the year-dummy coefficients equal zero with greater than 99% certainty. We can compare the change in $BPA_{s,t}$ in Table 8 to the change in 100*Breakfasts/Enrollment shown in Graph 2 on page 82. The auto-correlation terms catch much of the upward time trend in $BPA_{s,t}$. Once we control for serial correlation, our steady rises become more erratic. Nonetheless, our coefficients coincide with some of our initial observations about Graph 2. We observe a sharp drop in $BPA_{i,t}$ in 1982, which coincides with the drop in 100*Breakfasts/Enrollment in 1982 in Graph 2. This drop coincides with cost-cutting efforts during the Reagan administration. We also observe that the coefficients for the years after 1981 all fall below zero. We observe a similar relationship in Graph 2. The slope in Graph 2 for 100*Breakfasts/Enrollment over the first 15 years of the program generally exceeds the slope for 100*Breakfasts/Enrollment over the 1980's.

Equation (12), which we evaluate in Table 9, includes fixed state effects. Through this model, we understand general state-level differences in SBP participation. We have omitted New York so that the coefficient for state-dummy s denotes the average difference in $BPA_{s,t}$ between New York and state s. As with the year-dummies, many of the individual state-dummies do not show significant effects, but considered together, they pass a chi-square test with greater than 99% certainty. The states with the most significant and positive effects include Mississippi, Kentucky, Louisiana, and West Virginia. These four states have ranked in the poorest half of states for the past 25 years, usually ranking in the poorest quintile.[75] The states with the most significant and negative effects include Wisconsin, Utah, Alaska, and Wyoming. In 1990, Alaska and Wyoming ranked as the two least densely populated states, and Utah ranked 10th.[76] Over the past 25 years, Alaska has ranked among the 7 states with the highest median 4-person family income, often ranking number 1. Wisconsin typically ranks in the second quartile along the same distribution. We expect poorer states to participate more in the program. If more poor children live in a given state, then more children in that state qualify to receive free or reduced-price meals. Moreover, a state with more poor children might also contain more schools that qualify for special need status. Schools that qualify for special need status receive higher reimbursement rates. Hence, these schools face lower costs for SBP participation. For similar reasons, we might expect affluent states to participate less in the program. As mentioned on page 7, the SBP was designed in part to feed children from rural areas. As noted in the literature review on page 12, bigger schools are

[75] US Census, "Median Income for 4-Person Families, by State."
[76] US Census, "Land Area, Population, and Density for States and Counties: 1990."

more likely to join the SBP. For these reasons, we might expect that schools in non-agricultural states with lower population density would be less likely to join the program.

NLSY79 Geocode

Our longitudinal data come from the 1979 National Longitudinal Survey of Youth (NLSY79), conducted by the National Opinion Research Center (NORC). Our SBP data appear in aggregate form; yet, we consider long-term performance data for individuals. We know participation rates by state and by school district. To combine these figures with individual performance data, we estimate probabilities. We use each individual's state of birth and residence and family background to estimate the probability that the student participated in the SBP. This probability is only an estimate. Hence, our regressor represents the number of SBP breakfasts that the student ate plus some random error of unknown magnitude. This error term will bias our estimated effect toward zero. School breakfasts might affect long-term performance, but the random error does not. Our estimated effect represents a weighted average of the breakfast effect and the random error effect.

The NLSY79 includes responses from a random sample of 12,686 individuals aged 14-21 in 1979. The NLSY79 includes responses from the initial 1979 survey and from seventeen follow-up surveys from 1980 to 1998.[77] Respondents answer questions on a variety of topics including family background and academic performance. With the Geocode file, we can connect respondents' answers to those respondents' states and counties of birth and residence. We will evaluate respondents' education levels as a function of our SBP proxy as well as a number of control variables.

Composition of Respondents

The NLSY79 respondent population includes a representative sample of about 6,111 members of the US population born between 1957 and 1965. In addition to this representative sample, the NLSY79 contains a supplemental sample and a military sample. The supplemental sample includes 5,295 black, Hispanic, and poor non-black non-Hispanic Americans born between 1957 and 1965. The military sample includes 1,280 military personnel born between 1957 and 1961. In 1985, NLSY79 dropped the majority of the respondents from the military supplement, leaving

201 (15.7%) eligible respondents from the original military sample. In 1991, the NLSY79 dropped the poor non-black, non-Hispanic respondents from the supplemental sample, leaving 3,652 (69.0%) eligible respondents from the original supplemental sample.

Of the 9,964 (78.5%) remaining eligible respondents, 8,399 (84.3%) completed the survey in 1998. Of those not interviewed in 1998, 731 (7.3%) declined to be interviewed, and 295 (3.0%) died. Additionally, the NORC marked 725 (7.3%) difficult or impossible to locate, and the NORC did not interview 136 (1.4%) for reasons not disclosed.

Equations and Variables

In this section, we describe the equations and the variables that we use in our two empirical models. In our first model, we explore the SBP's effect on attendance in the current year. We use statewide aggregate figures for both attendance and SBP participation. In our second model, we explore SBP participation in kindergarten through 2nd grade as it affects individual respondents' future education levels.

Model 1: Attendance in SBP Schools

Our hypothesis suggests that, all else equal, students who participate in the SBP attend school more often. In order to test the SBP's attendance effect, we use state-level attendance and enrollment variables from the USDA SBP data set. In order to create our performance variable, $APE_{s,t}$, we divide ADA in SBP schools by enrollment in SBP schools. This quotient tells us the fraction of enrolled students who come to school in a given state on a given day. We multiply this fraction by 180, the length of a typical school year. By doing so, we can more easily compare our performance variable with our independent SBP variable, $BPE_{s,t}$.

(13) $APE_{s,t} = \nu_1 + \nu_2*BPE_{s,t} + \nu_3*TPENR_{s,t} + \nu_{4,d}*State_{s,d} + \nu_{5,d}*Year_{t,d} + \nu_6*u_{13,s,t-1} + u_{13,s,t}$

(14) $APE_{s,t} = \xi_1 + \xi_2*BPE_{s,t} + \xi_3*PPE_{s,t} + \xi_4*TPENR_{s,t} + \xi_{5,d}*State_{s,d} + \xi_{6,d}*Year_{t,d} + \xi_7*u_{14,s,t-1} + u_{14,s,t}$

(15) $LAPE_{s,t} = o_1 + o_2*LBPE_{s,t} + o_3*LTPENR_{s,t} + o_{4,d}*State_{s,d} + o_{5,d}*Year_{t,d} + o_6*u_{15,s,t-1} + u_{15,s,t}$

[77] Annual, excluding 1995 and 1997.

$$(16)\quad LAPE_{s,t} = \pi_1 + \pi_2{*}LBPE_{s,t} + \pi_3{*}LPPE_{s,t} + \pi_4{*}LTPENR_{s,1t} + \pi_{5,d}{*}State_{s,d} + \pi_{6,d}{*}Year_{t,d} + \pi_7{*}u_{16,s,t-1} + u_{16,s,t}$$

$$(17)\quad LAPE_{s,t} = \rho_1 + \rho_2{*}LBNP_{s,t} + \rho_3{*}LBMP_{s,t} + \rho_4{*}LTPENR_{s,t} + \rho_{5,d}{*}State_{s,d} + \rho_{6,d}{*}Year_{t,d} + \rho_7{*}u_{17,s,t-1} + u_{17,s,t}$$

$APE_{s,t}$ represents 180 times ADA in SBP schools divided by enrollment in SBP schools for a given state and year. $BPE_{s,t}$ represents the total number of breakfasts served divided by the enrollment in SBP schools in a given state and year. $PPE_{s,t}$ represents 180 times the number of participants in the SBP divided by enrollment in SBP schools in the state. $TPENR_{s,t}$ represents the total number of public school teachers K-12 in the state divided by total enrollment in the state. $LBNP_{s,t}$ represents the sum of the natural logs of $BPE_{s,t}$ and $PPE_{s,t}$. $LBMP_{s,t}$ represents the difference of the natural logs of $BPE_{s,t}$ and $PPE_{s,t}$. A variable preceded by "*L*" denotes the natural log of that variable. Finally, we consider state dummies, year dummies, and a serial correlation term. Subscript *d* indicates vectors of dummies.

Our model covers the years from 1974 to 1995. Our data set is bounded from below by USDA enrollment figures; the USDA did not begin to track enrollment in SBP schools until 1974. Our data set is bounded from above by NCES teacher and enrollment figures. The NCES has not published data for public K-12 teachers and enrollment that are more recent than 1995. We use pooled least squares estimation for our regressions.

Our dependent variable and our first two independent variables range from 0 to 180. In order to obtain comparable coefficients, we multiplied *ADA* and *SBP Participants* each by 180 over *Enrollment* to obtain $APE_{s,t}$ and $PPE_{s,t}$. Intuitively, we explain $APE_{s,t}$ as the average number of days that an enrolled student attends school in a given state and year. We interpret $PPE_{s,t}$ as the number of SBP "participant days" divided by enrollment in SBP schools. If every participant ate a school breakfast every school morning, then $BPE_{s,t}$ would equal $PPE_{s,t}$. Figure 3 on page 88 (not to scale) illustrates some of our variables in a Venn-Diagram. Figure 3 describes enrollment, ADA, and SBP participation for a single day. In our model, we consider the sums of many of these variables over the entire year.

The variables, $BPE_{s,t}$ and $PPE_{s,t}$ tell us information about students' behavior. We would like to measure the difference in attendance between two otherwise equal students who consume different numbers of school breakfasts. Students who choose participate in the SBP might already be different from students who choose not to participate. Our dependent variable may capture some of these preexisting differences among students. The type of student who participates in the SBP might already be the type who attends school more often. For instance, early risers might participate in the SBP and attend school more often than average students do. Our state dummies should capture much of this effect. If a state contains more motivated students or more students who wake up early, we expect this difference to persist over time. In this case, state effects would suffice to capture this variation. Generally, $APE_{s,t}$ increased from 1974 to 1995. Our year effects and serial correlation should capture the differences in children's behavior over the time period. To the extent that our fixed effects do not capture the differences in students, our model might bias the coefficients for $BPE_{s,t}$ and $PPE_{s,t}$ upward. For instance, we might observe some systematic correlation between SBP participation and school attendance policies. In this case, we would capture existing differences among the student population and we would attribute them to SBP participation. Our model controls for fixed-effects. Our model assumes that any correlation between $APE_{s,t}$ and either $BPE_{s,t}$ or $PPE_{s,t}$ results from the SBP's attendance effect.

In Equation (13), the coefficient for $BPE_{s,t}$ tells us roughly how many more children show up with each breakfast served. For instance, we might multiply the coefficient by 10, and we would know how many more children attend if we serve 10 additional breakfasts. Once we include $PPE_{s,t}$ in our model, in Equation (14), our SBP effect becomes less intuitive. The two variables represent different changes in the SBP. In Equation (14), when we increase $BPE_{s,t}$, we increase the number of breakfasts served while holding the number of participants constant. Any child who eats one school breakfast in the school year counts as an SBP participant. Not every SBP participant eats 180 school breakfasts in a school year. We see from the data in our empirical model that, on average, participants from the average state from 1974-1995 consumed 166 breakfasts. When we increase $PPE_{s,t}$, we increase the number of SBP participants while holding the number of breakfasts served constant. Including $PPE_{s,t}$ gives us two SBP variables measuring different effects. Our first independent variable, $BPE_{s,t}$, tells us how much the SBP affects existing participants. If $BPE_{s,t}$ increases, existing participants consume more breakfasts, and we see whether or not attendance increases. If so, we conclude that some SBP participants would not have come to school if not for the SBP.

Our second independent variable, $PPE_{s,t}$, tells us how much the SBP affects new participants. If we increase $PPE_{s,t}$ but hold $BPE_{s,t}$ constant, we measure two simultaneous effects. We increase the number of participants in the SBP, but we lower the number of meals per participant. Hence, we take some meals from our existing participants, and we give them to our new participants. Our statistical models do not predict an effect for $PPE_{s,t}$ holding $BPE_{s,t}$ constant. Previous studies have not differentiated between existing participants and new participants. Hence, we cannot make definite predictions about the coefficient for $PPE_{s,t}$.

We would like to know the effect of adding participants (increasing $PPE_{s,t}$) while holding meals per participant constant. We do not consider meals per participant in our model because the number is not scaled correctly; it contains participants in the denominator. In order to understand this relationship better, we consider the log estimations in Equations (15) through (17), where the coefficients represent elasticities. The numerator for the coefficient represents percentage change in our dependent variable. The denominator represents percent change in the independent variable in question.

The coefficient for $LBPE_{s,t}$ in Equation (15) tells us the elasticity of the total attendance effect of the SBP. This coefficient gives us the percentage increase in attendance divided by the percentage increase in breakfasts served. In order to understand the effect of $PPE_{s,t}$ on $APE_{s,t}$, we consider Equation (16). When we increase the number of participant days by 10%, let us increase $BPE_{s,t}$ by 10%, as well. We have 10% more participants. If each one eats the average number of meals per participant, then the number of meals divided by enrollment also increases by 10%. To consider the combined effect of this change, we simply add together the two coefficients for $LBPE_{s,t}$ and $LPPE_{s,t}$. We expect this combined effect to be positive. We can compare the coefficient for $LBPE_{s,t}$ with the sum of the coefficients for $LBPE_{s,t}$ and $LPPE_{s,t}$ in order to compare these two different aspects of the SBP.

Equation (17) produces the same sum of squared residuals as Equation (16) produces. The two models are mathematically equivalent, but the coefficients and standard errors provide different sorts of information in the two models. The sum of the coefficients, ρ_2 and ρ_3, in Equation (17) should equal π_2 from Equation (16). The difference of the coefficients, ρ_2 and ρ_3, in Equation (17) should equal π_3 from Equation (16). We observe this conversion in Figure 3, above. By substitution, $\pi_2 + \pi_3$, the coefficient for the combined effect described in the above paragraph, is equal to $(\rho_2 + \rho_3) + (\rho_2 - \rho_3) = 2*\rho_2$. Hence, we can compute the standard error of our combined effect by halving the standard error for ρ_2.

We expect a positive coefficient for $TPENR_{s,t}$. All else equal, we expect that a school with more teachers per student offers more attention and support to its students. If a school offers more attention and support to its students, we expect that children will show up more often. Teacher-pupil ratios proxy for school quality. $TPENR_{s,t}$ might also capture variation in the income or education level in state *s* and year *t* that fixed effects do not capture. We expect that children from more affluent or more highly educated families attend school more often. These children's families might place greater emphasis on education. This effect, along with the incentive effect for school quality, might lead to higher average attendance in schools with more teachers per pupil.

$TPENR_{s,t}$ ranges from 0.039 to 0.079. When we divide the mean of $TEACHERS_{s,t}$ by the mean of $ENROLL_{s,t}$, we obtain 0.054. Hence, yearly state averages for pupil-teacher ratios range from 13 to 27, with the observations averaging to 18 pupils per teacher. Note that we use a different denominator for $TPENR_{s,t}$ than we use for the SBP variables. Our dependent variable and our two SBP variables contain information about SBP schools by state. We would like to know teacher-pupil ratios for the SBP schools in a state in a given year. The USDA does not keep track of that information. Consequently, we proxy for that variable with teacher-pupil ratios across the entire state in a given state and year. Hence, $TPENR_{s,t}$ describes teacher-pupil ratios in SBP schools and non-SBP schools alike.

Using the statewide aggregate figures for teacher-pupil ratios creates another errors in variables problem.[78] Our teacher-pupil ratio represents the teacher-pupil ratio in state-year *s,t* plus some error term. This error biases our coefficient toward zero. Let us consider state-year observation A, containing some very affluent school districts with very high teacher-pupil ratios. Imagine too, that the poorer schools in observation A had average levels for teacher-pupil teacher ratios. Let us also consider state-year observation B with average teacher-pupil ratios across the entire state. Observation A shows a higher $TPENR_{s,t}$ than does observation B. Nevertheless, when we consider teacher-pupil ratios among the poorer schools, which are more likely to participate in the SBP, the two observations are identical. Controlling for other variables, we have no reason to expect that observation A will have a higher value for $APE_{s,t}$. $APE_{s,t}$ measures attendance in SBP schools, where the teacher-pupil ratios are identical for observations A and B. In the situation described above, $TPENR_{s,t}$ captured differences in non-SBP schools. When we looked at the SBP schools to measure the attendance effect, we saw no effect. We might conclude that teacher-pupil ratios do not affect attendance. In actuality, we have simply used the wrong variable to measure teacher-pupil ratios in SBP schools.

$TPENR_{s,t}$ works as a rough estimate of teacher-pupil ratios in SBP schools. In many cases, differences in $TPENR_{s,t}$ actually will capture differences in teacher-pupil ratios across SBP schools in different states. In these cases, $TPENR_{s,t}$ effectively measures teacher-pupil ratios as they affect attendance in SBP schools. If teacher-pupil ratios distribute across SBP schools and non-SBP schools equally, then $TPENR_{s,t}$ will really capture variation in teacher-pupil ratios in SBP schools. The more teacher-pupil ratios are distributed equally across SBP schools and non-SBP schools, the better $TPENR_{s,t}$ works as a proxy. Assuming that teacher-pupil ratios among SBP schools and non-SBP schools are not distributed perfectly equally, our coefficients are biased toward zero. Let us consider the difference between teacher-pupil ratios in SBP schools and those in non-SBP schools. As long as the variance for this difference exceeds zero, we have biased our coefficient toward zero. This bias holds regardless of the direction of the inequality, and it even holds if the expected value of this difference is zero. As mentioned earlier, we encounter a similar problem with our independent SBP variables in our second model, below. In addition to this bias, we expect that the state- and year-dummies duplicate much of the variation in $TPENR_{s,t}$.

In addition to various forms of $BPE_{S,t}$, $PPE_{S,t}$, and $TPENR_{S,t}$, our model also includes fixed state- and year- effects and a serial correlation term. We omit New York State for comparison. Our state effects should capture much of the variation due to race, income, average education level, and other measures of school quality. Because our dummies capture this variation, we expect that a number of these variables would not show significant effects in our fixed-effects model. Future researchers might choose to test this premise by including such variables.

Model 2: Educational Attainment and High School Graduation

[78] We described our first errors-in-variables problem when we described our SBP proxy for Model 2a.

Our second model divides into two sections. In the first section, we evaluate the SBP's effect on students' education levels. In the second section, we use these results to form crude estimates of the SBP's income effect.

Model 2a: The SBP and Educational Attainment

Our hypothesis suggests that, all else equal, students who participate in the SBP complete more grades of school than non-SBP participants do. Equations (18) and (20) explore the effect of the SBP on the highest grade that each respondent completed. In Equations (19) and (21), we explore the effect of the SBP on the probability that a given respondent graduated from high school.

$$(18) \quad HGC_i = \sigma_1{*}FBPA_i + \sigma_2{*}HGCMOM_i + \sigma_3{*}IPF_i + \sigma_4{*}CTMEDINC_i + \sigma_5{*}BLACK_i + \sigma_6{*}MALE_i + \sigma_7{*}RESPONSEAGE\_18_i + \sigma_8{*}RESPAGE_i + \sigma_{9,t}{*}Cohort_{i,t} + \sigma_{10,s}{*}BState_{i,s} + u_{18,i}$$

$$(19) \quad HSDUM_i = \tau_1{*}FBPA_i + \tau_2{*}HGCMOM_i + \tau_3{*}INCOME78_i + \tau_4{*}CTMEDINC_i + \tau_5{*}BLACK_i + \tau_6{*}MALE_i + \tau_7{*}RESPONSEAGE\_18_i + \tau_8{*}RESPAGE_i + \tau_{9,t}{*}Cohort_{i,t} + \tau_{10,s}{*}BState_{i,s} + u_{19,i}$$

$$(20) \quad HGC_i = \upsilon_1{*}BNP_i + \upsilon_2{*}BMP_i + \upsilon_3{*}HGCMOM_i + \upsilon_4{*}IPF_i + \upsilon_5{*}CTMEDINC_i + \upsilon_6{*}BLACK_i + \upsilon_7{*}MALE_i + \upsilon_8{*}RESPONSEAGE\_18_i + \upsilon_9{*}RESPAGE_i + \upsilon_{10,t}{*}Cohort_{i,t} + \upsilon_{11,s}{*}BState_{i,s} + u_{20,i}$$

$$(21) \quad HSDUM_i = \phi_1{*}BNP_i + \phi_2{*}BMP_i + \phi_3{*}HGCMOM_i + \phi_4{*}INCOME78_i + \phi_5{*}CTMEDINC_i + \phi_6{*}BLACK_i + \phi_7{*}MALE_i + \phi_8{*}RESPONSEAGE\_18_i + \phi_9{*}RESPAGE_i + \phi_{10,t}{*}Cohort_{i,t} + \phi_{11,s}{*}BState_{i,s} + u_{21,i}$$

HGC_i represents the highest grade that the respondent completed. $HSDUM_i$ returns a "1" if HGC_i is greater than or equal to 12, and a "0" otherwise. Rather than include a constant term in our regression, we include dummies for all four cohorts. These four dummies add up to a vector of 716 ones. to obtain the constant term for Equations (19) through (21), we can take a weighted average of the coefficients for these four dummies. For Equations (18) and (20), we calculate OLS estimates. For Equations (19) and (21), we use logit models. $FBPA_i$ represents *Free*

Breakfasts Served divided by *ADA* in respondent *i*'s state for the years from kindergarten to second grade. PPA_i (not included above) represents total *SBP Participants* divided by *ADA* in respondent *i*'s state for the years from kindergarten to second grade. BNP_i represents the sum of $FBPA_i$ divided by its own mean and PPA_i divided by its own mean. BMP_i represents the difference of $FBPA_i$ divided by its own mean and PPA_i divided by its own mean.

$HGCMOM_i$ represents the highest grade that respondent *i*'s mother completed. $INCOME78_i$ represents respondent *i*'s family's income in 1978. IPF_i represents respondent *i*'s family income in 1979 divided by respondent *i*'s family size in 1979. $CTMEDINC_i$ represents the median family income in 1969 in the county in which respondent *i* was born. $BLACK_i$ returns a "1" if respondent *i* is black and a "0" if respondent *i* is not black. $MALE_i$ returns a "1" if respondent *i* is male and a "0" if respondent *i* is female.

$RESPONSEAGE\_18_i$ returns a "1" if respondent *i* was 18 at the most recent response to "What is the highest grade that you completed?" $RESPAGE_i$ represents respondent *i*'s age at his/her most recent response to the same question.

Our model also includes fixed cohort- and state-effects. The cohort dummies, AGE_14_i through AGE_17_i, correspond to respondents' ages in 1979. Our model only includes 4 age cohorts, so our 4 cohort-dummies sum to one. Consequently, the weighted average of the cohort-effects represents the constant term in our regression. In order to compute this term, we would first multiply each of the cohort-dummies by the number of respondents in the cohort. Next, we add these products together, and we divide the sum by the total number of respondents. To compute the significance of a single cohort effect, we would first subtract our constant term from the cohort effect.

We also include 41 birth-state dummies. Our sample does not include any respondents from Delaware, Hawaii, Idaho, Maine, Nevada, New Hampshire, North Dakota, Rhode Island, or Wyoming. Furthermore, we omit New York State for the purposes of comparison. State effect *s* represents the difference in the average HGC_i or $HSDUM_i$ between respondents from New York and respondents from state *s*. Our data include one response for each individual; hence, our model does not include a serial correlation term.

Restrictions Placed on the Sample Population

For the purposes of our study, we reduce our 12,686-person sample with a number of constraints. The NLSY79 does not include respondents' parents' incomes. The survey does, on the other hand, include a variable for family income. If the respondent lived in a parental home in the survey year, then family income includes parents' income. For this reason, we include respondent's only if that respondent replied that he/she lived in a parental home in 1979. The NLSY79 does not strictly define "parental home." If a respondent lived with one parent, we expect that the respondent answered that he/she lived in a parental home. If the respondent lived with a legal guardian who was not a biological parent, then the survey leaves the question open for interpretation. Among respondents living in parental homes, we selected those whose family incomes fell below the poverty line in 1978. These restrictions narrowed our sample from 12,686 to 2,465 respondents. We use this poverty restriction to proxy for free meal eligibility 6-17 year earlier.[79] As mentioned on page 20, Akin and others' research suggests that school lunches affect these children more than they affect non-poor children. This difference may or may not carry over to the SBP. If it does, then our estimates only reflect the SBP's effect on poor children.

We place a variety of other restrictions on the sample population. We describe the reasons for these restrictions in the next few paragraphs. We restrict the sample population to include only those respondents who lived in their respective states of birth at age 14. We include only those respondents who were aged 14-17 in 1979. We remove every respondent who did not disclose the highest grade that he/she completed sometime after the age of 17. We remove respondents from the sample if they did not answer every question used in our model. We also remove 8 respondents from our model because they did not answer "Did you live with both biological parents from birth until age 18?" We do not end up using this variable in our final specification because it did not significantly affect our dependent variables. Nevertheless, we should not expect that the omission of these 8 respondents biases our estimates in any way. Our final sample includes 716 respondents.

Education Level

[79] Once we restrict the age-cohorts in our model, this restriction proxies form free meal eligibility 6-11 years earlier.

For the purposes of our model, we use two dependent variables, HGC_i and $HSDUM_i$. HGC_i denotes the highest grade that respondent *i* completed. $HSDUM_i$ returns a one if respondent *i* completed 12^{th} grade or higher and a zero if the respondent did not complete 12^{th} grade.

The NLSY79 includes responses to "Highest Grade Completed" for every survey year. As described on page 43, respondents dropped off from the survey for various reasons. We remove every respondent who did not answer this question after the age of 17. We observe much lower mean values for HGC_i and $HSDUM_i$ for this younger subset of the sample population. We expect that a large proportion of these younger respondents completed more years of schooling shortly after they answered the survey question.

Our remaining respondents disclosed their education levels anytime between the ages of 18 and 33. Even after eliminating the youngest responses, we find that HGC_i and $HSDUM_i$ vary according to the age of the respondent. Hence, we include the two variables, $RESPONSEAGE\_18_i$ and $RESPAGE_i$ to control for this variation. $RESPONSEAGE\_18_i$ returns a one if respondent *i* answered "Highest Grade Completed" at age 18 and a zero otherwise. This variable controls for 18-year-olds who eventually graduated high school but completed the survey before they graduated. $RESPAGE_i$ denotes the age, between 18 and 36, of respondent *i* when he/she reported his/her education level.

$RESPAGE_i$ operates largely independently from the age-cohort effect. Imagine two respondents, both been born in 1963. One drops off from the survey in 1985, while the other stays in the survey through 1998. $RESPAGE_i$ for the first respondent equals 22, while $RESPAGE_i$ for the second respondent equals 35. Birth years for our age-cohorts have a range of three, while $RESPAGE_i$ has a range of 18.

$RESPAGE_i$ captures two effects. Older respondents have had more of an opportunity to complete more years of schooling. Some respondents may have completed high school or the equivalent some time between the ages of 18 and 33. Others may have attended school at higher or lower levels of education during those years. We also expect that some of the respondents misrepresented their education levels as they aged in order to appear more educated. We do not isolate these two effects. $RESPAGE_i$ controls for both effects. We have no reason to think that either of the $RESPAGE_i$ effects can be attributed to the SBP.

SBP Variables

The NLSY79 survey does not include questions about the SBP. Hence, we estimate the probability that each respondent participated in the SBP using annual statewide aggregate data. In order to estimate this probability, we would like to know the state and years in which each respondent attended school.

We restrict our sample population to include only those respondents who lived in their respective states of birth at age 14. Consequently, our state-level SBP variable more accurately represents breakfasts served in the state in which respondent *i* attended grades K-2. Our SBP data require that we link every respondent to the state in which the respondent attended school. While that particular variable is not available, we can proxy for it with each respondent's state of birth.

We restrict our sample population to include only those respondents who were aged 14-17 in 1979. These students were born between 1962 and 1965, and they attended kindergarten between the years of 1967 and 1970. Respondents took the NLSY79 at different points during the year. We assume that respondents took the survey earlier, rather than later, in the school year. Hence, if a respondent was aged 16 in 1979, we assume that he/she entered kindergarten 11 years earlier in 1968. This assumption creates a bias in our estimates. If a respondent has an early birthday, then our model may assume that the respondent began school one year earlier than he/she actually did. The SBP increased over the period in question, so we assume that these students consumed fewer breakfasts than they actually did. Angrist and Krueger (1991) show that students with birthdays later in the school year are less likely to drop out of school. Laws allow these students to drop out of school early. Hence, this assumption about student ages may bias our estimates upward. We think students drop out of school because they had fewer breakfasts. In fact, these students simply have birthdays early in the school year, which predisposes them to dropping out of school. We do not know the average birthday for respondents in our sample. Hence, we cannot definitely conclude that this assumption creates an upward bias. Some students have early birthdays, but some students begin kindergarten at age 6.

The SBP began halfway through the 1967 school year. Hence, the SBP program and the cohort effects are confounded for all respondents born before 1962. All the respondents in the military sample were 18 years of age or older in 1979. Hence, by eliminating the older age-cohorts, we also eliminate all those respondents from the military sample. Recall that we have already restricted our sample to include only those students living in a parental home in 1979. If our sample included respondents aged 18 or older in 1979, then our sample restrictions would select for specific traits in those respondents. Requiring that these respondents lived in parental homes in 1979 excludes all respondents who moved away to work or to attend college.

We have restricted the sample population to include only those respondents whose parents lived at or below the poverty line in 1978. Consequently, we assume that all the respondents in our population met the income eligibility requirements for free breakfasts.

We do not know precisely whether or not our individual respondents participated in the SBP. Our models use three variables to describe the SBP in respondent's respective states. Equations (18) and (19) use $FBPA_i$. $FBPA_i$ represents free breakfasts served divided by attendance in respondent *i*'s state for the years from kindergarten to second grade. First, we add together SBP statistics from K-2. Second, we add the ADA statistics from K-2. Finally, we divide the sum of SBP statistics by the sum of ADA statistics to obtain $FBPA_i$. Equation (20) and (21) include linear transformations of $FBPA_i$ and PPA_i. PPA_i represents the sum of the number of SBP participants divided by the sum of ADAs for the years from K-2. We use a simple, unweighted sum for the years K-2 for both $FBPA_i$ and PPA_i. Future researchers could work to estimate the appropriate weights for relevant years. The sum, BNP_i, and the difference, BMP_i, help us to evaluate the effect of increasing $FBPA_i$ and PPA_i simultaneously. These transformations are analogous to $LBNP_{s,t}$ and $LBMP_{s,t}$, which help us to understand the effects of a simultaneous change in Model 1.

$FBPA_i$ and PPA_i provide information about the relative size of the SBP in each respondent's state during the years from K-2. Our 716 respondents do not constitute a representative sample of the US, or of their respective states. If they did, then $FBPA_i$ would represent the expected number of free breakfasts that respondent *i* consumed. Similarly, PPA_i would represent the probability that respondent *i* participated in the SBP. With our existing sample, $FBPA_i$ and PPA_i do not pertain as directly to our respondents. The two variables simply provide information about the relative size of the SBP for the state and years in which respondent *i* attended K-2. Our SBP variables do not measure respondents' degree of SBP participation precisely. Our regressor is a linear function of our desired regressor (SBP participation) and an error term. Hence, as explained earlier, the effect that we measure will be biased toward zero.

All of our respondents lived at or below 100% poverty in 1978. We assume that all of our respondents qualified for free meals during grades K-2. Hence, we assume that each of our respondents lived at or below 130% poverty for some specific three-year interval beginning 8-11 years before 1978. Because our respondents are poor, our respondents are more likely than average to come from poor school districts. As noted in the introduction, the SBP is located disproportionately in poor school districts. The US government provides stronger incentives for schools to join the SBP if the schools fall within "special need" areas. We may conclude that our respondents are more likely to come from SBP schools than average students are.

We also assume that all our respondents qualify to receive free breakfasts. Slightly more than half of our respondents are black. As noted on page 16, holding income constant, black students are more likely than non-black students are to participate in the SBP. Our respondents are not only more likely than average to come from SBP schools. Those of our respondents who attended SBP schools were more likely to participate in the SBP than were average SBP school students.

Not all of our respondents have participated in the SBP or have attended SBP schools. Among those who participated, changes in $FBPA_i$ and PPA_i may correspond to changes in HGC_i and $HSDUM_i$. Among those who did not participate in the SBP or attend SBP schools, changes in $FBPA_i$ and PPA_i do not correspond to changes in HGC_i and $HSDUM_i$. We have no reason to suspect that the SBP affects non-participants. Our coefficients fall below what our coefficients would be if every respondent participated in the SBP. Our coefficients represent weighted averages of averages of academic effects of the SBP for those who participated and zeros for those who did not participate. Our coefficient for $FBPA_i$ represents the change in highest grade completed or $HSDUM_i$ divided by the change in the number of breakfasts served. Our coefficient for $FBPA_i$ is a downward-biased estimator of the academic effect to a student of serving a breakfast to that student.

Again, our set of 716 respondents does not constitute a representative sample of students from their respective birth-states over the period in question. If we did use such a representative sample, then our coefficient would represent the SBP's academic effect distributed over the entire population. In this case, we would multiply our coefficient by 180 over PPA_i. We scale by 180 so that our denominator becomes *Participants/ADA* rather than *Participant Days/ADA*. We assume that non-participants experience no academic benefits from the SBP. When we multiply our coefficient by ADA, we obtain the SBP's total academic effect on everyone. If we knew the number of participants in our sample, we could then divide this total effect by the number of persons affected. As we have sufficiently established above, our respondents are more likely to participate in the SBP than average students are. PPA_i over 180 does not accurately measure the proportion of participants in our sample. Even if it did, we still use a representative sample rather than the entire SBP population. Hence, our estimates would still suffer from errors-in-variables, biasing our coefficients toward zero.

We have established that our coefficient for $FBPA_i$ falls below the academic effect that results from giving a student a breakfast. We have also established that our coefficient for $FBPA_i$ probably exceeds the value of that same academic effect distributed over the population. If, however, our errors-in-variables problem exerts sufficient bias in our model, then our estimate might fall below the population effect. Nevertheless, 180* $FBPA_i$ divided by PPA_i probably overshoots the academic effect that results from giving a student a breakfast. Because we do not know the number of SBP participants in our sample, we cannot determine a precise estimate of the academic effect of one breakfast. Our estimates can, however, provide us with confidence intervals for upper and lower bounds for this effect. Our upper bound is still biased downward because of errors in variables. If, at a later date, we choose to relax the restrictions on our sample population, we could estimate the SBP's academic effect directly. We would still have to contend with the errors-in-variables problem, though.

When we described Model 1, we considered the possibility that students were already different before they joined the SBP. For instance, we imagined that early risers participated more in the SBP, and that early risers performed better in school. We speculated that our model would catch these existing differences in students and attribute them to the SBP. In Model 1, we used states as our units of observation, and we used state- and year-effects to control for any such differences. In Model 2, we use individual respondents as our units of observation, but this bias still does not concern us. If such differences in people vary by state, then our state-effects control for these differences. If these differences do not vary by state, then our state-level SBP data do not capture the individual differences at all. Our state-level data do not attribute individual respondents' idiosyncrasies to the SBP, because our state-level data only capture state- and year-level trends.

Our first two models describe educational attainment, measured with HGC_i or $HSDUM_i$, as functions of the number of breakfasts served. Equations (20) and (21) attempt to distinguish two separate effects of the SBP, much like $LBNP_{s,i}$ and $LBMP_{s,i}$ distinguished two separate attendance effects in Model 1. When we model Equations (19) through (21) in log form, we find that our regressions do not fit the data as well. Many of our variables exert significant effects in linear form, but not in log form. We conclude that linear variables more accurately model the effects that we wish to measure. Nevertheless, we include regression results for our log formulations in Table 15.[80]

Our log formulations do not yield significant effects. Nevertheless, we would still like to measure the effects of increasing PPA_i while holding $FBPA_i/PADAKT2T_i$ constant. We explain our reasons for holding breakfasts per participant constant when we describe Model 1. When we consider PPA_i and $FBPA_i$ as separate variables, the coefficient for PPA_i reports the effect of increasing SBP participation holding breakfasts constant. By increasing PPA_i and holding $FBPA_i$ constant, we perform two simultaneous and contradictory operations. We increase the number of SBP participants, but we lower the average number of meals that each participant receives. We have essentially taken breakfasts from old participants and given those breakfasts to new participants. Rather than measure the upshot of these contradictory operations, we wish to give breakfasts to new participants and to leave old participants alone. When we increase participation by 10%, we would like to increase breakfasts by 10%. We would like to give our new participants just as many breakfasts as our old participants have. In doing so, we can see how the SBP affects new and old participants differently. Because our model uses linear, and not log, variables we cannot use the same technique as we used in Model 1.

We would like a linear variable that captures simultaneous changes in $FBPA_i$ and PPA_i that are equal in proportion. This variable does not exist; we cannot capture a geometric operation with an arithmetic variable. We can create a linear variable that captures simultaneous changes in $FBPA_i$ and PPA_i that are equal in proportion at a single point. We model the combined effect of changes in $FBPA_i$ and PPA_i with a similar technique as we used in Model 1. In Model 1, we used logs, so that the coefficient for $LBNP_{s,i}$ captured simultaneous changes equal in proportion at every point. In Equations (20) and (21) we use two independent variables, BNP_i and BMP_i, both defined as functions of $FBPA_i$ and PPA_i. As mentioned earlier, BNP_i represents the sum of

[80] In revising the paper, we discovered that $CTMEDINC_i$ and $RESPAGE_i$ exert more significant effects on HGC_i when logged. Logging the variables slightly increases our estimated coefficient for $FBPA_i$ (from 0.247 to 0.255) and our adjusted R^2 (from 0.192 to 0.200). Logging $RESPAGE_i$ in the $HSDUM_i$ equation adds some slight significance to the $RESPAGE_i$ coefficient and does not affect our other estimates. We use

$FBPA_i$ divided by its own mean and PPA_i divided by its own mean. BMP_i represents the difference of $FBPA_i$ divided by its own mean and PPA_i divided by its own mean. In Equations (20) and (21), we regress HGC_i and $HSDUM_i$ on both BNP_i and BMP_i. These equations are mathematically equivalent to equations in which we regress HGC_i and $HSDUM_i$ on both $FBPA_i$ and PPA_i. We distribute the coefficients in Equations (20) and (21) over the sum, BNP_i, and the difference, BMP_i, and combine terms. Doing so, we can manipulate Equations (20) and (21) to obtain linear regressions of HGC_i and $HSDUM_i$ on $FBPA_i$ and PPA_i.

Figure 5 illustrates the rationale behind our technique. Let $FBPA_i = x_i$, $PPA_i = z_i$, and $Y_i = HGC_i$ or $HSDUM_i$. We may add as many other variables to this equation as we like, so long as they do not contain x_i or z_i. When we take the derivative of both sides with respect to w, $\Delta Y_i/\Delta w_i$ equals $2{*}b_1$. We evaluate Equations (20) and (21) at the mean values for $FBPA_i$ and PPA_i,. Hence, $2{*}b_1$ in Figure 5 corresponds to $2{*}\upsilon_1$ and $2{*}\phi_1$, respectively.

Heuristically, υ_1 and ϕ_1 represent coefficients for the aggregated effected of changing $FBPA_i$ and PPA_i simultaneously. On the same level, we can interpret υ_2 and ϕ_2 as coefficients for the effect of changing the relative sizes of the two variables. Indeed, this interpretation is totally valid for linear changes in $FBPA_i$ and PPA_i. Our hypothesis only applies to the mean for both $FBPA_i$ and PPA_i, though. Only at that point does a simultaneous change of equal proportion only affect BNP_i and not BMP_i.

For Equations (20) and (21), we test the hypothesis that the coefficient for BNP_i exceeds zero at the means for $FBPA_i$ and PPA_i. We would like to increase participation subject to the constraint that elasticity of breakfasts to participation equals one. Our hypothesis states that this change positively affects HGC_i and $HSDUM_i$ when evaluated at the means for $FBPA_i$ and PPA_i. The magnitudes of these effects equal $2{*}\upsilon_1$ and $2{*}\phi_1$, respectively.

Control Variables

$HGCMOM_i$ denotes the highest grade that respondent *i*'s mother completed. T. Kane (1994) finds that, among black students, the mother's educational attainment better predicts high school graduation than does father's attainment. T. Kane also finds that, among black students, the

---

the $HSDUM_i$ specification for our estimates in Model 2b. Hence, this increase does not alter our final estimates.

mother's attainment level predicts college entry better than the father's attainment does. For white students, however, the father's attainment level better predicts college entry than does the mother's attainment. Attainment levels between parents are highly correlated. T. Kane finds that both parents' attainment levels significantly affect black and white students' attainments, alike. J. Kane and Spizman (1994) find that the father's education exerts a larger effect than does the mother's education on the probability that a student enters college.

T. Kane uses a series of eight dummies to represent the different education levels of each parent. These dummies indicate whether each parent had graduated high school, attended some college, completed college, or whether the respondent did not know. In our specification, we only consider the mother's attainment. Consequently, we only include observations in which the respondent reported the mother's education level in 1979.[81] The mother's attainment level can serve as a proxy for both parents' education levels. By only using the mother's education, we need not restrict the model to respondents who know their respective fathers' education levels. By using $HGCMOM_i$, we may exclude those respondents who grew up without mothers. However, we do not systematically exclude those respondents who grew up without fathers.

We use different income measurements for the two equations. $INCOME78_i$ represents respondent i's family's income in 1978. IPF_i represents respondent's family income in 1978 divided by respondent i's family size in 1979. Individually, both variables show significant effects in both equations, but the variables are highly collinear. IPF_i showed a slightly more significant effect in the HGC_i equation, and $INCOME78_i$ showed a slightly more significant effect in the $HSDUM_i$ equation. The two income variables emphasize different aspects of income's effect on attainment. $INCOME78_i$ tells us more about the parents or the income-providers. This variable tells us about respondent i's role models just as $HGCMOM_i$ does. IPF_i, on the other hand, tells us more about the resources available to the respondent. We expect that child with higher IPF_i has more of an opportunity to attend college. The child has access to more resources. Using this logic, we might expect that IPF_i would factor more significantly when our dependent variable contains information about college. This is, in fact, the case, and IPF_i factors more significantly into the HGC_i equation, while $INCOME78_i$ factors more significantly into the $HSDUM_i$ equation.

$CTMEDINC_i$ represents the median family income in 1969 in the county in which the respondent was born. As noted earlier, this county is not necessarily the county in which the respondent lived at age 14. Nevertheless, the variable shows a high degree of explanatory power. Given $INCOME78_i$ or IPF_i, we can explain $CTMEDINC_i$'s negative coefficient by discussing relative income changes. Holding family income constant, as median county income decreases, the child's relative income increases. This relative increase in income could expose the child to more opportunities.

As Table 6 illustrates, the racial composition of our 716-person sample does not reflect the racial composition of the US population. As described earlier, the NLSY79 contains a supplemental sample of black and Hispanic respondents over and above the representative sample of the US. We have further biased the sample by only including respondents who lived below the poverty level in 1978. No respondents in our sub-sample are Asian or Pacific American, and no respondents are both black and Hispanic.

Model 2 does not use pooled data, but our model does include fixed cohort- and birth-state-effects. The cohort-dummies, AGE_14_i through AGE_17_i, correspond to each respondent's age in 1979. Our respondents vary by state and age-cohort so that these fixed effects become relevant, even for ordinary least squares and logit estimation. Our birth-state-effects should capture differences in school quality, income and price levels, demographics, family composition, and average level of education across states. Once we control for birth-state-effects, teacher-pupil ratios do not significantly affect HGC_i or $HSDUM_i$.

Our sample covers a very small range of years. Consequently, we expect the cohort-effects to be relatively small. Nonetheless, the cohort effects may capture some of the variation in the economy and in school quality over the time period. For instance, the high school class of 1980 may have faced a different job market than the high school class of 1983 did. These labor market changes might have affected students' incentives to complete more education. Cohort-effects might also capture nationwide changes in school expenditures, classroom size, and teacher quality over the four years.

Other variables that did not contribute significantly to either equation include $HISPANIC_i$, $BOTHPARENTS_i$, $FSIZE_i$, and $MOMAGE_i$. $HISPANIC_i$ returns a one if respondent *i* is Hispanic and a zero otherwise. $BOTHPARENTS_i$ returns a one if respondent *i* lived with both parents from

[81] The NLSY79 allows respondents to consult with parents in order to answer questions more accurately.

birth until age 18 and a zero otherwise. $FSIZE_i$ represents the size of respondent *i*'s family in 1979, and $MOMAGE_i$ represents the age of respondent *i*'s mother in 1987.

Model 2b: The SBP and Labor Market Experience

In the second section of our long-term effects model, we consider employment and earnings as dependent variables. By estimating the SBP's indirect effects on students' future employment and earnings prospects, we can determine crude cost/benefit ratios for the SBP.

$$(22) \quad EMP_i = \chi_1 + \chi_2 * HSDUM_i + \chi_3 * MALE_i + \chi_{4,t} * WYear_{i,t} + \chi_{6,s} * RState_{i,s} + \chi_{7,s} * BState_{i,s} + u_{22,i}$$

$$(23) \quad EMP_i = \psi_1 + \psi_2 * HSDUM_i + \psi_3 * BLACK_i + \psi_4 * MALE_i + \psi_5 * AGE_i + \psi_6 * (WREMP_i / WRPOP_i) + u_{23,i}$$

$$(24) \quad log(WAGE_i/CPI_i) = \omega_1 + \omega_2 * HSDUM_i + \omega_3 * HGCMOM_i + \omega_4 * IPF_i + \omega_5 * MALE_i + \omega_6 * BLACK_i + \omega_7 * AGE_i + \omega_{8,s} * RState_{i,s} + \omega_{9,s} * BState_{i,s} + u_{24,i}$$

$WAGE_i$ represents the wage rate at respondent *i*'s current or most recent job. EMP_i returns a "1" if respondent *i* had a job in the survey week and a "0" otherwise. The NLSY79 survey contained these questions for every year from 1979 to 1993. Some respondents skipped one or both of the questions for some of the survey years. $WAGE_i$ represents respondent *i*'s most recent response to the wage question. EMP_i represents respondent *i*'s answer to the employment question for the same year as *i*'s most recent answer to the wage question. Equation (24) includes only those respondents for whom EMP_i equals one. Different respondents reported their most recent wage rates in different survey years, from 1981 to 1993. We deflate our wage observations with the Consumer Price Index. Hence, we measure our wage observations in 1983 dollars per hour.[82] Log denotes natural log. AGE_i represents respondent *i*'s age for the wage/employment observation in question. $WREMP_i/WRPOP_i$ represents the employment-population ratio for respondent *i*'s race and age category in his/her state of residence in the survey year.[83] We estimate Equations (22) and (23) with a logit model, and we estimate Equation (24) using OLS. Recall from our discussion of Model 2a that we obtain HGC_i and $HSDUM_i$ from respondent *i*'s

[82] Approximately. CPI for 1983 equals 99.6, and the average CPI for 1982-1984 equals 100.
[83] Race categories include white, black, and Hispanic. Age categories include 16-19, 20-24, and 25-34.

most recent report of his/her education level. Respondents reported their education levels as late as 1998. Hence, a one for $HSDUM_i$ might reflect that respondent i completed high school *after* i's most recent wage observation. In order to preserve consistency, we do not update $HSDUM_i$ to fit our new model. Hence, we may observe wages or employment statuses for non-high school graduates and attribute them to high school graduates. If so, then our model contains another errors-in-variables problem. This problem may bias our $HSDUM_i$ coefficients downward.

We expect positive signs for $HSDUM_i$ and for $MALE_i$ in all three equations. We expect that high school graduates are more likely to be employed than non-graduates are. Among employed persons, we expect that high school graduates earn more than non-graduates do. In fact, in constructing this model, we aim to estimate these positive returns to schooling. All else equal, we expect that men are more likely to be employed than women are. Women who keep house and raise children full-time return a zero for EMP_i. We also expect that, among employed persons, all else equal, men earn higher wages than women do. We can attribute this difference to direct discrimination. We expect negative coefficients for $BLACK_i$ in Equations (23) and (24) because of direct discrimination. We expect positive coefficients for AGE_i in Equations (23) and (24), because older workers have more experience. All else equal, workers with more experience demand higher wages and are more likely to be employed. Traditionally, wages and employment vary quadratically with age. At higher age levels, the returns to a year of age decrease as experience and education loses its relevance. In our sample, however, age ranges from 16 to 31. Our sample does not include any respondents at the higher age levels where wages and employment decrease with age. AGE_i^2 does not show a significant effect on either dependent variable. We conclude that $log(WAGE_i/CPI_i)$ and EMP_i's concavity with respect to AGE_i is negligible and can be sufficiently approximated with a linear relationship. Hence, we do not expect wages or employment to vary quadratically with age for our sample. We expect positive coefficients for $(WREMP_i/WRPOP_i)$ in Equation (23) and $HGCMOM_i$ and IPF_i in Equation (24). Higher employment/population ratios for i's demographic group should increase the likelihood that i is employed. $HGCMOM_i$ and IPF_i represent opportunities available to i as a child. These two variables might also capture i's parents' roles as role models and as standards of performance. We expect that respondents from more educated or affluent families earn more than average and are more likely than average to be employed.

In addition to our descriptive regressors, Equation (22) includes state- and year-dummies and Equation (24) includes state-dummies. The state-dummies in Equations (22) and (24) include birth-state-dummies and dummies for the respondent's state of residence in the survey year. We exclude those dummies that make our models perfectly multicollinear. As before, we do not include dummies for New York State. Because of this perfect multicollinearity, we know that we have controlled for the same variation with other variables in our model. We also excluded all dummies that created sufficient near-multicollinearity as to prohibit estimation of our model. Similarly, we know that we have controlled for nearly all of the same variation with other variables in our model.

In addition to the variables included in Equation (24), $MARRIED_i$ also exerted a significant effect on $WAGE_i$. $MARRIED_i$ returns a "1" if the respondent was married in the survey year and a "0" otherwise. We exclude $MARRIED_i$ because marriage is endogenous. An increase in $HSDUM_i$ raises $WAGE_i$, and this increase in $WAGE_i$ might increase the probability that respondent i marries. Respondent i might be more willing to marry if he/she earns a higher wage, and a higher wage might make respondent i a more attractive spouse. If we control for i's marital status, the coefficient for $HSDUM_i$ does not capture $HSDUM_i$'s total effect. Consequently, we estimate a reduced-form equation where the effect of the exogenous element of $MARRIED_i$ simply increases our error terms.

Further Restrictions Placed on the Sample Population

Before estimating Equations (22) through (24), we impose some restrictions on our sample population. We impose these restrictions in addition to the restrictions we impose on the population when estimating Equations (18) through (21).

First, in Equations (22) through (24), we consider only those respondents who were not enrolled or attending school during the survey year. Attending school imposes a constraint on these respondents' earning potentials. We would like to know what our respondents earn without that constraint. Those respondents attending school earned slightly higher wages than the average respondent did. Hence, excluding them from our model may bias our estimates downward. In Equations (22) and (24), we exclude those respondents who did not disclose their state of residence for the survey year. Our employment/population ratio data contain some holes. In Equation (23), we exclude those respondents for whom employment and population statistics were not available by race, state, and age group. Some of our remaining respondents never answered the $WAGE_i$ question. Others did not answer the EMP_i question in the same year as they answered the $WAGE_i$ question. We could not include those respondents in our regression equations. As mentioned earlier, we also restrict Equation (24) to include only those respondents for whom EMP_i equals one. Some of our wage observations may reflect data errors. We omit the one wage observation of $11,500/hr ($15,000 nominal 1993). We also omit the five wage observations falling below $1/hr in 1983 dollars. Of our 545 remaining respondents, 282 earned less than $5/hr in 1983 dollars. Because we do not include respondents who never disclosed a wage rate, we may bias our employment estimates upward. Some respondents may not reply because they never work, or because they work so infrequently that they cannot remember their last wage rate. If many such respondents exist, then we create an upward bias in our estimates of the economic return to the SBP and to classroom size reduction. We do not consider some respondents who are more likely to have zeros for EMP_i.

Results

The next few pages explain results from the empirical models described above. We do not discuss results for our state-dummies or year-dummies. We discuss the chi-square statistic for adding the variables collectively for each equation. We do not interest ourselves with the academic or labor market performance effects of living in one state or another. Including the information distracts from our statistical analysis. In the pages following our regression results, we provide information about year- and state-dummies for Equations (14), (19), (22), and (24).

Model 1:

Table 10, Column 1 shows regression results from Equation (13) on page 43. Our SBP variable, $BPE_{s,t}$ contributes very significantly (t = 5.40) to our attendance variable, $APE_{s,t}$. We obtain a coefficient of 0.0995. For every 10 breakfasts served, 1 child shows up to school who would not have attended otherwise. Column 4 shows us the same SBP effect separated into a positive and a negative component. When we include both SBP variables, $BPE_{s,t}$ and $PPE_{s,t}$ in a linear model (Column 2), we obtain highly significant effects (t = 13.2 and –11.9). This linear model gives us coefficients of 0.602 and –0.497 for $BPE_{s,t}$ and $PPE_{s,t}$, respectively. We interpret the coefficient for $BPE_{s,t}$ as we did with Column 1. Holding the number of participants constant, for every 10 breakfasts served, 6.02 children show up to school who would not have attended otherwise.

As we discussed when we introduced Model 1, we cannot easily interpret the coefficient for $PPE_{s,t}$ in the linear model. We are more interested in the combined effect of increasing $BPE_{s,t}$ and $PPE_{s,t}$ simultaneously. We can most easily measure this effect using log form, as in Table 10, and adding the coefficients. As with the linear model, the effects for $BPE_{s,t}$ and $PPE_{s,t}$ are highly significant (t = 12.2 and –10.7, respectively). We turn our attention to $LBPE_{s,t}$ in Column 3. Holding the number of participants constant, a 10% increase in the number of breakfasts served leads to a 1.66% increase in attendance.[84] We add the coefficients from $LBPE_{s,t}$ and $LPPE_{s,t}$ and we obtain a coefficient of 0.0221. In order to calculate the standard error, we add the variances and add twice the covariance. The coefficients for $LBPE_{s,t}$ and $LPPE_{s,t}$ have a covariance of –$1.70*10^{-4}$. When we add the squares of the standard errors, add twice the covariance, and square root the sum, we obtain 0.00478. This value represents the standard error for our combined effect.

In order to verify the standard error and t-statistic for the combined effect, we move to Equation (17), shown in Column 5. Column 5 shows essentially the same regression as Column 4 does, with the same sum of squared residuals. We multiply our coefficient for $LBNP_{s,t}$ by two to obtain the effect of a simultaneous change of equal proportion in $BPE_{s,t}$ and $PPE_{s,t}$. We double the coefficient 0.01103, and we obtain 0.0221, as with the previous model. We double the standard errors, and we obtain a standard error of 0.00473, roughly equivalent to our calculated standard error. The t-statistic for this collective effect is 4.62. Our coefficient indicates that, holding meals per participant constant, a 10% increase in participation in SBP schools results in a 0.221% increase in attendance.

The descriptive statistics from Table 11 provide us with information about various transformations of enrollment, ADA, and SBP variables. Rather than use means of ratios, we calculate ratios of means. The mean of breakfasts divided by the mean of participants gives us an average of 166 breakfasts per participant. If we hold this ratio constant, adding a new participant involves adding 166 new meals. The mean of ADA divided by the mean of enrollment gives us 0.924. We multiply by 180, and we obtain an average student's yearly attendance of 166. We divide the average number of participants by average enrollment in SBP schools, and we obtain a mean participation ratio of 0.221. Given these averages, we can calculate a linear interpretation of our log formulation evaluated at the mean.

[84] Assuming constant enrollment, we may express effects on our dependent variable solely as effects on attendance.

Let us imagine an SBP school with 180 students. On a given day, we expect 166 students to show up. Over the year, our yearly attendance is 166*180 = 29,880 student days. Of these students, 0.221*180 = 39.8 participate in the SBP. Imagine that we increase participation by 10%, from 39.8 to 43.8 participants. On average, each of these new participants consumes 166 breakfasts, so we serve 3.98*166 = 661 more breakfasts. This 10% change in participation increases attendance by 0.0211*10% = 0.211%. By serving 661 breakfasts, we observe 0.00211*29,880 = 63.0 more instances in which a student shows up to school. Hence, we conclude that, on average, 100*63/661 = 9.53% of breakfasts given to new participants actually bring new participants to school. We can contrast this result with our result earlier from breakfasts given to current SBP participants. On average, more than half (60.2%) of the breakfasts given to current participants actually bring the participants to school. When we do not separate the two effects, as in Column 1, we find that adding 10 breakfasts increases attendance by 0.995. Our combined SBP effect falls much closer to the effect of adding new participants. hence, most of the difference in SBP participation across states reflects variation in participants per capita, not breakfasts per participant.

As we discussed when we introduced Model 1, our coefficients for $TPENR_{s,t}$ and for $LTPENR_{s,t}$ are biased toward zero, because the variables describe teacher-pupil ratios across SBP and non-SBP schools alike. Our most significant effect for this variable appears in Equation (13), shown in Table 10 in Column 1. From Table 11, we calculate a mean value for $TPENR_{s,t}$ or 0.0547, or 18.3 students per teacher. If we reduce class size by 1 student, from 18.3 to 17.3, $TPENR_{s,t}$ rises from 0.0547 to 0.0578, increasing by 0.00316. We multiply this change by our estimated coefficient of 157.8. We find that a 1-person drop in class size increases the average enrolled student's yearly attendance by 0.499. Next, we multiply by the mean value for SBP enrollment. We find that a 1-person drop in classroom size leads to an average of 160,000 more instances of students showing up to class in a given state, an average of 889 more students each day. Our next most significant effect applies to the log variable, $LTPENR_{s,t}$. We obtain a coefficient that is significant at the 10% level, or at the 5% level if we use a 1-tailed hypothesis test. We obtain a coefficient for $LTPENR_{s,t}$ of 0.0493. Hence, a 10% increase in $TPENR_{s,t}$ results in a 0.493% increase in attendance. At the mean, a 10% increase in $TPENR_{s,t}$ drops the average class size by 1.7 students, from 18.3 to 16.6. At the mean, this drop leads to a 1461-student rise in ADA, 263,000 more instances of children attending school, and a 0.819 increase in $APE_{s,t}$. Our first model finds a slightly bigger effect, as 0.819 / 0.499 = 1.64 < 1.7. Recall that our coefficients for both variables, are biased toward zero. Note also that we have controlled for any fixed state or year differences in $TPENR_{s,t}$.

We run chi-square tests on our state and year dummies to accept or reject them as groups. We accept the state dummies in all cases with greater than 99.5% significance. We accept the year dummies for Equations (13) and (14) with 99% significance. The year dummies in the last three regressions only show 75% significance. When we omit the year dummies from these regressions, our explanatory variables are still highly significant. None of the coefficients for our explanatory variables changes by more than 2%.[85] In all cases, $TPENR_{s,t}$ became highly significant with the omission of the year-dummies.

Model 2:

As before, we consider Model 2 in two sections. We interpret our results from Model 2a in order to determine the SBP's effect on educational attainment. We interpret our results from Model 2b in order to estimate the earnings and employment effects of a high school diploma. We combine the results from our two models to estimate the economic return to a school breakfast. We obtain a crude cost/benefit ratio for the SBP using these estimates. We also calculate a crude cost/benefit ratio for classroom size reduction, using Card and Krueger's estimates. We find that, on the margin, the economic returns to the SBP exceeds by 45% the economic return to hiring new teachers. Our estimates are crude. Nevertheless, our model provides convincing evidence that the economic effect of the SBP at least rivals that of reducing classroom size. We do not find convincing evidence that policymakers can achieve this return by indiscriminately increasing the number of SBP participants. Our research suggests that selective expansion of the SBP could capitalize on substantial economic returns.

Model 2a:

Table 12 provides descriptive statistics for a number of variables relevant to our model. The free breakfasts, ADA, and participation variables all represent sums for the years in which the respondent attended K-2. We represent these variables as $FREEBK_i$, $ADAKT2_i$, and $PART_i$, respectively. Again, we calculate mean ratios by averaging before we divide. As we compare

[85] The coefficient for $\mathrm{LBMP}_{s,t}$ changed by 14% with the omission of the year-dummies, but we only used the variable as a control to examine $\mathrm{LBNP}_{s,t}$'s coefficient.

the averages from Table 12, we understand more about the SBP during the period in question. Recall that each respondent began kindergarten sometime between 1967 and 1970. The SBP began in January 1967 and expanded very rapidly for the first few years. For the period in question, the average annual number of breakfasts divided by mean ADA equals 9.64. Again, our respondents do not constitute a representative sampling of their respective birth-states. If they did, would have eaten, on average, 9.64 free breakfasts per year from K-2. On average, our respondents come from state-year combinations in which SBP participants consumed 105 breakfasts per year. Over the years in question, free breakfasts constituted 74.9% of total breakfasts served in the average state-year observation. On average, our respondents came from state-year combinations in which students attending SBP schools ate 28.6 breakfasts per year over the period in question.

Columns 1 and 2 from Table 14 illustrate results from Equations (18) and (19), respectively. $FBPA_i$ shows effects just barely significant at the 5% level ($p = 0.0503$ and 0.0497, respectively). Our results fall well within 5% significance for a one-tailed t-test, though. In Column 1, our dependent variable, HGC_i, ranges from 0 to 20, and our right hand SBP variable, $FBPA_i$, ranges from 0 to 180. We obtain a coefficient of 0.247 for $FBPA_i$. Hence, if the average number of breakfasts served per year in respondent i's state increased by 4.05 over the period, then the respondent would complete 1 more grade of school. Recall that the mean for $FBPA_i$ equals 9.64. To scale our number, we multiply 4.05 by the mean of $ADAKT2_i$, and then we divide by the mean $PART_i$. We see that, at the mean, increasing $FBPA_i$ by 4.05 results in a 442 increase in breakfasts per participant over the three-year period, or 147 per year.

These figures lead to two interpretations, which serve as upper and lower bounds for our estimates. These figures represent the lowest and highest numbers of breakfasts required to increases the average respondent's education level by one grade. Our errors-in-variables problem biases our coefficient downward. Consequently, we overestimate the number of breakfasts required to increase the average respondent's education level by one grade. Hence, the error term in our variables biases our upper and lower bounds upwards. We could assume that every respondent in our sample is an SBP participant. If so, then we conclude that 147 breakfasts per year from K-2 leads a respondent to finish one more year of school. Alternatively, we could conclude that our respondents constitute a representative sampling from their birth-states and age-cohorts. If so, then we would conclude that giving a respondent 4.05 breakfasts per year from K-2 leads a respondent to finish one more year of school. These two estimates give us upper and lower bounds for the SBP's effect on HGC_i. We can expand this interval by one standard deviation on either side. Our interval for breakfasts per year required for one grade improvement becomes {2.68, 301}. The maximum number of breakfasts a participant can eat in a year is 180. Hence, we obtain a wide range of possible values. If we use our upper bound plus one standard deviation, we predict a relatively small effect. This estimate predicts that no amount of school breakfasts eaten from K-2 can raise the average respondent's expected education level by one grade. According to this estimate, the SBP might still influence the education level of respondents on the border between two expected grade levels. Increasing these students' expected education levels could have substantial economic effects. In Model 2b, we examine the economic impact of the SBP's high school graduation effect, described below.

We interpret results from Column 2 with a similar approach. Our average value for $HSDUM_i$ is 0.753. Since we are using a logit model, the dependent variable in our regression is the log of an odds ratio. Hence, our dependent variable for a logit model evaluated at $HSDUM_i$'s mean is log $(0.753/(1-0.753)) = 1.11$. Again, our mean value for $FBPA_i$ is 9.64. We would like to know the number of breakfasts required to increase by 5% the likelihood that a given respondent will graduate from high school. We imagine an average respondent with an average value of $HSDUM_i = 0.753$. We increase this likelihood by 5%, up to 0.791. The log of our odds ratio becomes log $(0.791/(1-0.791)) = 1.33$. We divide $(1.33-1.11) = 0.22$ by the coefficient for $FBPA_i$, and we obtain a change in $FBPA_i$ of 0.579. At the mean, in order to increase by 5% the likelihood that a respondent will graduate high school, we must increase $FBPA_i$ by 0.579. As before, we can convert this number to obtain a maximum number of required breakfasts. We multiply 0.579 by the mean for ADA and divide by the mean for participation. We find that a 5% increase in likelihood requires 63.2 breakfasts over K-2, or 21.1 breakfasts per year for three years. A 5% increase in the probability of graduating from high school requires between 0.579 and 21.1 breakfasts each year for the years K-2. If we tack on one standard deviation to each side, our interval expands to {0.384, 42.8}. Hence, a 5% increase in the probability of graduating requires somewhere between 0.384 and 42.8 breakfasts per year from years K-2. In Model 2b, we explore the economic effects of the SBP using the upper bound of 21.1 breakfasts/year.

Next, we consider the academic effect of increasing SBP participants and breakfasts proportionately. We turn our attention to Equations (20) and (21). The coefficient for BNP_i, when evaluated at the mean, represents one half the effect of changing $FBPA_i$ and PPA_i simultaneously in equal proportions.

Equations (20) and (21), as described in Columns 3 and 4, show marginally significant effects for BNP_i. In both cases, the variable shows a significant effect at the 10% level and passes a one-tailed t-test at the 5% significance level. When we regress either HGC_i or $HSDUM_i$ on both $FBPA_i$ and PPA_i, we show insignificant effects for both explanatory variables. Both independent variables show significant effects when used separately to explain HGC_i or $HSDUM_i$. The two variables show a correlation of 0.843. This collinearity prevents us from estimating accurate coefficients when we consider the variables together. Considering BNP_i and BMP_i, on the other hand, allows us to separate out the significant effect. BNP_i and BMP_i, still show some collinearity, but we obtain a significant effect for BNP_i. The variable only shows a significant effect at the 10%-level. Using a one-tailed test, however, the variable shows significant effects at the 5%-level.

We begin at the mean respective means for $FBPA_i$ and PPA_i. We increase HGC_i by one grade. Evaluated at the mean, $\Delta HGC_i / \Delta w = 2*0.156$, where 0.156 is the coefficient for BNP_i in Equation (20). The factor, w, acts as the common multiplier for $FBPA_i$ and PPA_i. If we let $\Delta HGC_i = 1$, then $\Delta w = 3.21$. Since we began at the mean, w originally equalled one. In order to change HGC_i by 1, we must increase w by 1. Hence, we multiply $FBPA_i$ and PPA_i by 4.21. At the mean, we serve 15,300,000 breakfasts in the state and increase participants by 145,000 in order to raise the HGC_i by 1 grade. Note that $PPART_i$ represents a sum of SBP participants in respondent i's birth-state for the years K-2. Hence, we add 48,300 participants and serve 5,100,000 breakfasts each year for 3 years in order to raise HGC_i by 1. Only 48,300 people experience the effects of those breakfasts, though. At the mean, these 48,300 people represent 0.979% of a given state's population. We assume that new participants occur in our sample as often as they do in the general population. Hence, we assume that 7.01 of our respondents are new participants. We give each of them 105.6 new breakfasts per year for K-2, and HGC_i increased by 1. Hence, we increase the number of breakfasts given to our respondents by $7.01*105.6 = 740$ per year from K-2 to increase HGC_i by 1. Hence, increasing free breakfasts specifically among new participants does not affect education level as much as does increasing breakfasts generally. Our estimated number of breakfasts per year exceeds our estimated upper bound for the more general HGC_i specification.

We perform similar operations with Equation (21) and Column 4. Let us again imagine that we increase by 5% the likelihood that a given respondent will graduate from high school. This increase requires a 0.22 increase in our dependent variable, the log of the odds ratio. Now suppose that 0.22 / Δw = 2*0.217, where 0.217 is the coefficient for BNP_i in Equation (20). We solve for Δw and we obtain 0.507. Hence, we increase $FBPA_i$ and PPA_i simultaneously by 50.7% to increase by 5% the probability that the average respondent graduates high school. Hence, we serve 2,410,000 breakfasts to 22,900 / 3 = 7,630 participants. These 2,410,000 breakfasts distribute over three years, and they average 803,000 per year. Let us assume, again, that new participants occur in our sample as often as they do in the general population. By assumption, 0.00155%, or 1.11, new participants join the SBP from our sample. Consequently, we serve 117 breakfasts annually for 3 years in order to raise by 5% the likelihood of graduating from high school. Hence, our estimated effect on new participants falls in between our estimated upper and lower bounds for the more general $HSDUM_i$ specification.

In addition to the SBP effects, we note an unexpected effect for one of our dummies. $BLACK_i$ returns a "1" if the respondent is black and a "0" if the respondent is not black. $BLACK_i$ exercises a strong positive effect on $HSDUM_i$. The dummy exerts a significant positive effect ($t = 1.99$) even when we regress $HSDUM_i$ on $BLACK_i$ in a bivariate model. Among our sample of poor NLSY79 respondents, black respondents averaged 12.38 years of schooling, with average $HSDUM_i$ of 0.785. Hispanic respondents averaged 12.50 years of schooling, with average $HSDUM_i$ of 0.703. Non-black non-Hispanic respondents averaged 12.19 years of schooling, with average $HSDUM_i$ of 0.728. We might conclude that black families and communities provide cultural support and incentives to acquire more education.

Other researchers have also found a black dummy to exert a positive effect on educational attainment. J. Kane and Spizman (1994) use data from the National Longitudinal Study of the High School Class of 1972 to examine educational attainment. The authors find that, all else equal, being black increases the probability that a student will attend college or graduate school. The authors suggest three possible explanations. First, affirmative action programs may make it easier for black people to attend college. In particular, the authors find that, all else equal, black students are more likely to receive grants or financial awards to attend college. Second, the SAT may show some cultural bias favoring black students. Third, black students may expect higher returns to a college education than other students do. Kane and Spizman cite a study by Meisenheimer (1990) indicating that black students do, in fact, experience higher than average returns to college. In J. Kane and Spizman's study, black students were less likely to attend for college when the authors did not control for income and family background variables. Our findings show that, among poor students, black students are more likely than average to graduate from high school. Our results hold even when we do not control for income. We could attribute this effect to SATs or to college affirmative action programs. Students who face fewer barriers to entering college also face a greater incentive to graduate from high school. Black students also might experience (and consequently expect) higher than average returns to a high school education.

We have tested the chi-square statistics for adding the 41 state dummies or the 4 age dummies (replacing a constant term). For the $HSDUM_i$ regressions, we could reject the hypothesis that the coefficients for all the state-dummies are zero with 99.5% confidence. For our HGC_i equations, we can reject that same hypothesis with about 85% confidence. For the HGC_i equations, we can reject the hypothesis that the coefficients for the age cohorts are equal to zero with 95% confidence. For the $HSDUM_i$ regression, we can reject the same hypothesis with 99% confidence.

Model 2b:

Table 13 shows descriptive statistics for the variables included in Equations (22) through (24). Note that the number of observations changes from equation to equation. As mentioned when we outlined the model, we impose different restrictions on the sample populations for Equations (22), (23), and (24). These changes in the sample population slightly alter the descriptive statistics for many variables. Equation (23) helps us to understand what sort of variation our state- and year-dummies capture. We do not use Equation (23) to determine our cost/benefit ratio. We do not concern ourselves with slight sample differences between Equation (22) and Equation (23). We impose a further restriction on our sample when we estimate Equation (24). We require that respondents were employed in the survey week. To estimate the total earnings effect of the SBP, we combine $HSDUM_i$'s effect on EMP_i with its effect on $log(WAGE_i/CPI_i)$. Using this combined effect, we calculate a crude estimate of the total economic effect of participating in the SBP.

Column 1 shows us results from Equation (22), described earlier. Our mean value for EMP_i is 0.829, obtained from Table 13. Again, we use a logit model, so our dependent variable is the log of an odds ratio. Our dependent variable for a logit model evaluated at EMP_i's mean is $\log(0.829/(1-0.829)) = 1.58$. In Equation (22), we obtain a coefficient of 0.697 for $HSDUM_i$. Our mean value for $HSDUM_i$ for our sample is 0.748. If we increase the expected value of $HSDUM_i$ by 5% from 0.748 to 0.785, we increase the log odds ratio for EMP_i from 1.58 to 1.58 +

0.697*0.0374 = 1.61. Dividing the inverse log of this number by one plus its inverse log, we obtain 0.833 as our expected value of EMP_i. Hence, at the mean, a 5% increase in the expected value of $HSDUM_i$ leads to a 0.483% increase in the expected value of EMP_i. From Model 2a, serving 63.2 school breakfasts to a child from K-2 will cause such a 5% increase in the expected value of $HSDUM_i$. Hence 63.2 breakfasts cause a 0.483% increase in the expected value of EMP_i, an increase of 0.014 standard deviations.

Column 3 tells us the wage effect of an increase in the expected value of $HSDUM_i$. Among the 542 employed persons in our sample, the mean value for $HSDUM_i$ is 0.788. In Column 3, we estimate a coefficient of 0.226 for $HSDUM_i$, meaning that being a high school graduate raises wages by 22.6%. If, instead, we increase $HSDUM_i$'s expected value by 5%, from 0.788 to 0.827, $log(WAGE_i/CPI_i)$ increases by 0.226*0.0394 = 0.00883. Hence, at the mean value for $log(WAGE_i/CPI_i)$, a 5% increase in mean $HSDUM_i$ raises $log(WAGE_i/CPI_i)$ from 1.6544 to 1.6632. This increase corresponds to an increase in real wages from 1983 $5.2299/hr to 1983 $5.2763/hr, an increase of 0.014 standard deviations.. Hence, among working persons at the mean, serving 63.2 school breakfasts from K-2 leads to a 1983 $0.0464/hr increase in wages. When we replace $HSDUM_i$ with HGC_i in Equation (24) (not shown), we obtain a highly significant coefficient of 0.0810. Ashenfelter estimates the return to a year schooling for his twin samples somewhere between 8% and 11%. We find that, for our sample of respondents, the return to a year of schooling falls closer to Ashenfelter's lower bound of 8%.

Using a chi-square test for Equation (22), we can reject at the 0.5% significance level the null hypothesis that the set of dummies exerts no effect for each set of dummies. When we include all our dummies in Equation (22), very few of our descriptive significantly affect EMP_i. Equation (23) includes descriptive variables rather than dummies. By including the descriptive variables, we can better understand some of the variation that our dummies capture. Using a chi-square test for Equation (24), we can reject the null hypothesis at the 97.5% significance level for the state-of-residence-dummies. We can reject the null hypothesis at the 95% significance level for the birth-state-dummies. We exclude the year dummies from Equation (24) because they exert no significant effect on log real wages. We do not reject the null hypothesis for inclusion of the year-dummies in Equation (24) at the 25% significance level ($p < 0.75$).

The mean value of EMP_i for our sample population not enrolled in school is 0.829. The mean value of EMP_i for our total sample population is 0.815. Of our 695 respondents who disclosed employment and school enrollment information in the survey year, 33 were attending school. Of those 33, 51.5% were employed during the survey week. Let us imagine that, at the mean, a respondent from our sample has a job 81.5% of the time. Employed respondents attending school at the time of the survey earned an average of 1983 \$5.50/hr. Employed respondents not attending school earned a mean wage of 1983 \$5.23/hr.

Now, let us give this average respondent 63.2 breakfasts over K-2. For our estimates, we ignore the SBP's effect on respondents' education levels past high school. We assume that, for ages 18-64, breakfasts eaten do not affect the percentage of respondents who attend school in a given year. In doing so, we underestimate the total wage effect of the SBP. Among our respondents not attending school, expected EMP_i rises from 0.829 to 0.833. Holding employment among respondents in school constant, employment for our total 695 respondent sample increases from 0.815 to 0.818. Hence, we imagine that our respondent is employed 81.8% of the time. Our respondent's expected wage when working has also risen from 1983 \$5.2299/hr to 1983 \$5.2764/hr.

We make the simplifying assumption that an increase in the employment rate does not decrease the mean wage. Hence, we assume that, on average, unemployed persons enter jobs at the mean wage. In reality, on average, unemployed persons probably enter jobs at lower than the real wage. This assumption will bias our estimates upwards. In the US, the average workweek has declined slowly over the past two decades. From Table 13, we see that the average response year for Equations (22) and (24) averages to about 1992. Hence, we choose the 1992 average of 34.4 hours/week, taken from the 1993 *Statistical Abstract of the United States*. We assume that, when working, our respondents work the average number of hours weekly. Respondents from our sample are unemployed more often than the average American is. Consequently, we expect that part-time workers will be disproportionately among respondents in our sample. Hence, assuming the average workweek for our respondents biases our estimates upward. Unlike the average workweek from our sample, the mean US workweek averages people from many age groups.

Let us assume that a given respondent works 81.5% of the time for each week from age 18 through age 64, a total of 2,450 weeks. Hence, this mean respondent works a total of 0.815 * 34.4 hrs/week * 2450 weeks = 68,700 hours in his/her career. At the mean wage, this respondent earns a total of 1983 \$359,000 in his/her career.[86] Now, we give the respondent 63.2 breakfasts as a child. In doing so, we increase the average employment level from 0.815 to 0.818. We also increase the average wage from 1983 \$5.2299/hr to 1983 \$5.2764/hr. At the higher employment level, our mean respondent works a total of 0.818 * 34.4 hrs/week * 2450 weeks = 68,900 hours in his/her career. At the higher mean wage, our respondent now earns a total of \$364,000 in his/her career. Hence, giving the mean respondent 63.2 breakfasts over K-2 has increased his/her expected lifetime earnings by 1983 \$5,000. Alternatively, we could give 351 breakfasts to new SBP participants to achieve the same goal.

Using Glantz and others' estimations, the mean total cost of a breakfast equals 1992 \$1.67 = 1983 \$1.19. Hence, an investment of 1983 \$1.19 * 63.2 breakfasts = 1983 \$75.21 returns a total of 1983 \$5000 over the 47 years of the respondent's working life. Hence, a 1983 \$1 investment in the SBP returns a total of 1983 \$66 over the respondent's working life. We could invest the dollar any time between kindergarten and 2nd grade. Assuming constant annual returns, this investment would return \$1.40 every year for 47 years, beginning 11-13 years after the initial investment. If we restrict our investment to new SBP participants, a 1983 \$1 investment in the SBP returns a total of 1983 \$14 over the respondent's working life. Assuming constant annual returns, this investment returns only 1983 \$0.30 per year for 47 years.

We can compare our estimated rate of return to the rate of return on 10-year US Treasury Bills. Our breakfast calculations use data from 1967-1970, so we imagine purchasing T-Bills beginning in 1968. Let us imagine that we earn the annualized rate of return on T-Bills from 1968 to 1994. As we see from Table 21, if we earned the annualized rate of return for T-Bills every year from 1968 to 1994, our initial 1983 \$1.00 would be worth 1983 \$1.98 at the end of 1994. Let us imagine that, on the other hand, we invested this 1983 \$1.00 in the SBP. If we give the 1983 \$1.00 worth of breakfast to a 1st grader in 1968, we begin to earn returns in 1980. Once we earn the 1983 \$1.40 at the end of each year, we reinvest the money in 10-year T-Bills. By 1994, our initial investment has returned 1983 \$29.20. Alternatively, if we invest 1983 \$1 in new SBP participants, our initial investment returns 1983 \$6.25 by 1994.

Let us consider, alternatively, that we use our money to hire new teachers. Card and Krueger estimate that a 10% reduction in class size led to a 0.4% to 1.1% increase in a child's future earnings. Let us use the higher figure and calculate a cost/benefit ratio. *The Digest of Education*

[86] Recall that our sample includes only respondents from disadvantaged backgrounds.

Statistics provides us with numbers of teachers, ADA, and average teacher salaries by state for the 1969 school year. Using these data, we calculate the average class size, and the cost per pupil of reducing a state's average classroom size by 10%. In doing so, we assume that teacher salaries remain constant while the number of teachers increases. If the demand truly did increase, we expect that teacher salaries would also increase. Nevertheless, we are interested in marginal changes, which should not significantly affect prices. For the total US, we calculate this amount at 1983 $127/yr. For a weighted average of our 716 respondents' states, we calculate this amount at $120/yr. Let us assume that one year of a small classroom size suffices to create the 1.1% earnings increase. Card and Krueger do not calculate teachers' effects on employment. Hence, we will compare Card and Krueger's estimates with our estimates for wage increases from the SBP. We ignore employment effects for this comparison. We must assume that classroom size affects wages in our sample in the same amount as it affects wages in the general population. Since classroom size probably affects our respondents more, this assumption biases our estimate downwards. At the mean, a 1.1% increase in earnings increases wages from 1983 $5.2299 to 1983 $5.2874 for our sample. Let us make the same assumptions as before about employment and hours worked. This classroom size reduction raises lifetime earnings from 1983 $359,000 to 1983 $363,000, a 1983 $4000 increase. Considering a 1983 $120 cost per pupil, spending 1983 $1.00 on a new teacher for one pupil raises the pupil's lifetime earnings by 1983 $33.00.[87]

Now, let us compare this effect with solely the earnings effect of spending a dollar on the SBP. If we multiply the higher wage by the lower number of hours, we obtain 1983 $362,000 lifetime earnings. Hence, considering only earnings, 62.1 breakfasts increase lifetime earnings by $3,000. Hence, spending $1.00 on the SBP raises the pupil's lifetime earnings by $48.00. If we consider the economic effect of adding new SBP participants, 351 breakfasts increase lifetime earnings by $3,000. Hence, spending $1.00 on new SBP participants raises the pupil's lifetime earnings by $7.20. Hence, our estimated SBP effect exceeds Card and Krueger's estimated classroom size reduction effect by 45%. If a 10% classroom size reduction led to a 1.6% wage increase, then the two rates of return would be equal. Dollar for dollar, our lower bound for the SBP's earnings effect rivals our lower bound for the earnings effect of classroom size reduction. We also observe that Card and Krueger's estimated classroom size reduction effect exceeds our new SBP participant effect by nearly 460%. Again, our estimates are crude and downward-biased. Nevertheless, increasing the SBP appears to show a very strong effect. Our results for increasing

[87] Card and Krueger estimate separate school quality effects by state. Future researchers might try the same technique with the SBP.

SBP participants are not so conclusive. Recall that we used the greater bound for the return to classroom size reduction. We simply cannot reject the null hypothesis that the effect of classroom size reduction equals or exceeds the effect of adding new SBP participants.

There exist alternative means of improving education. Angrist notes the option of teacher training programs.[88] Researchers know relatively little about the economic effects of this low-cost means for improving school quality. Nevertheless, not knowing the economic returns to such a policy, the SBP provides an appealing, low-cost means for improving children's educational experiences.

CONCLUSION

In this paper, we explored the SBP and short and long-term academic effects of the program. We first described the mechanics of the SBP. Next, we reviewed relevant studies about breakfast, the SBP, schooling, and school quality. Using results and models from the literature, we presented a formal argument that the SBP influences long-term academic performance. Figure 2 illustrates that formal argument in a flow diagram. We combined USDA state-level SBP data with a variety of other data resources to construct empirical models of the SBP's academic effects. Our two empirical models present strong evidence that the SBP exerts a significant positive effect on both attendance and educational attainment. Evidence from our second empirical model indicates that the economic effect of the SBP rivals that of classroom size reduction. Our results suggest that these returns require increasing the meals given to existing SBP participants, not simply increasing the number of SBP participants.

In our first model, we use pooled, annual, state-level data to examine the SBP's effect on attendance in participating schools. We find that, on average, for every 10 breakfasts served, 0.995 children attend school who would not have otherwise attended. We find stronger effects when we hold the number of participants constant. Among those students who already participate in the SBP, for every 10 breakfasts served, 6.02 children attend school who would not have otherwise attended. Among new participants, for every 10 breakfasts served, 0.953 children attend school who would not have attended otherwise.

---

[88] Lecture. 22 March 2000.

In the first part of our second model, we examined the SBP's effect on educational attainment. We use longitudinal data from the NLSY79. We estimate probabilities of SBP participation using respondents' birth-states and state-level SBP data. Errors in variables prevent us from estimating specific values. We obtain an upper and lower bound for the number of breakfasts required to increase the average student's expected attainment by one grade. We similarly bound the SBP's effect on high school graduation from above and below. In addition, we estimate the attainment effect of adding new participants to the SBP. We use our estimation of the SBP's high school graduation effect to determine the economic returns to the SBP. We find that, on average, consuming 63.2 school breakfasts from K-2 increases a respondent's likelihood of graduating high school by at least 5%. We obtain this upper bound by assuming that all of the respondents in our sample participated in the SBP. Consequently, we distribute the SBP's total effect over all 716 respondents in our sample. The real number of SBP participants in our sample probably falls short of 716. Hence, we probably underestimate the SBP's effect on education levels. We also estimate the educational attainment effect of giving breakfasts to new participants. To increase by 5% the likelihood that a new SBP participant graduates from high school, the new participated consumes 351 school breakfasts.

Given these upper bounds, we extend our empirical model to estimate the economic returns to the SBP. We estimate high school graduation's effect on future employment and earnings. Making a series of heroic assumptions, we obtain a crude cost/benefit ratio for the SBP. We estimate that, at the mean, 1983 $1 spent on the SBP returns 1983 $66 in increased employment and earnings over a respondent's working life. Alternatively, $1 spent on new SBP participants returns 1983 $14 in increased employment and earnings over a respondent's working life. Hence, assuming constant annual returns, 1983 $1 spent on the SBP in 1968 returns 1983 $1.40 every year from 1980 to 2026. Also assuming constant annual returns, 1983 $1 spent on new participants in 1968 returns $0.30 every year from 1980 to 2026. When find that our estimated returns from the SBP exceed Card and Krueger's estimated returns to classroom size reduction by 45%. Because our estimates are crude and rely on a string of heroic assumptions, we do not estimate standard errors for our estimate. Our evidence merely shows that the economic return to the SBP rivals that of classroom size reduction. Nevertheless, our upper bound Card and Krueger's estimated effect far exceeds our downward-biased estimated effect for giving school breakfasts to new participants. Hence, we do not obtain conclusive evidence about the effect of increasing SBP participation.

Recently, the SBP has attracted a good deal of media attention and political momentum. As mentioned earlier, school districts have been experimenting with universal free breakfast programs for the past few years. Some, like the programs in Minnesota and in Maryland, simply offer free breakfast to any student who shows up early. Others, like more recent pilot program in Maryland, offer free breakfast in class. Now, Congress has proposed similar programs to be adopted, nationwide.

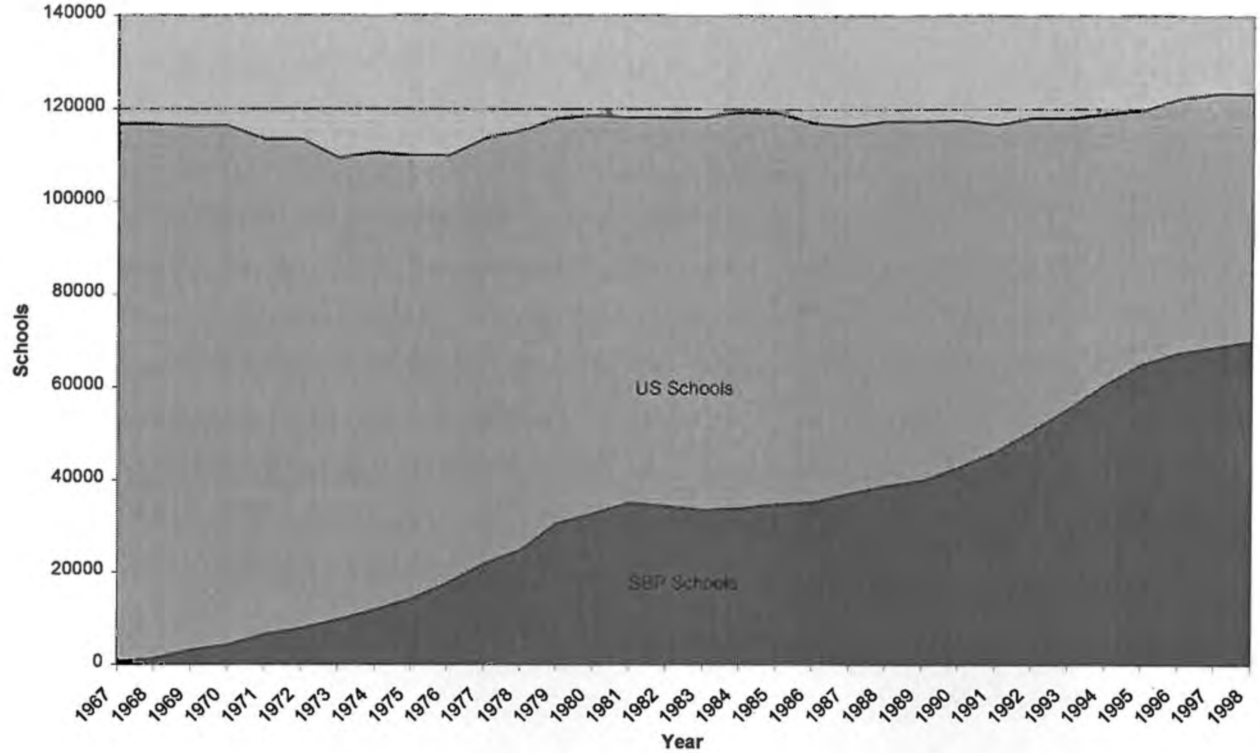
Our estimations shed some light on the recent efforts to expand the SBP. Our research provides substantial evidence to back the claim that the SBP improves children's academic performance. If these universal free breakfast programs affect poor, marginal SBP participants, then the economic return to these programs is probably very high. If the investment only increases SBP participation rates, however, the economic return may not exceed the return to classroom size reduction. Table 21 illustrates the huge returns to be gained through investing in the SBP.

On the other hand, the universal free breakfast programs may affect more students who do not qualify for free or reduced-price meals. Our models do not predict the effects of such a program. Our estimates do suggest, however, that there exist huge economic gains to be realized by expanding the SBP among existing participants. To the extent that universal free breakfast programs reduce the stigma cost for existing participants, these new programs may realize some of these economic gains. For the most part, these programs have primarily focused on increasing the numbers of SBP participants. Our research shows that such a program shows very high returns.

Nevertheless, there exist even larger economic gains to be realized by expanding a different dimension of the SBP. Existing SBP participants stand to experience tremendous gains from increasing breakfasts per participant. In 1998, SBP participants consumed an average of 165 meals per year. Hence, we can only increase the average breakfasts per participant by about 15 breakfasts. With 7.2 million participants, at 1983 $1.19/breakfast, we obtain a maximum investment of 15 breakfasts*7.2 million participants*1983 $1.19 = 1983 $130 million each year.

Not much room exists to expand the SBP among existing participants. Some non-participants may, however, exhibit the same high returns as existing participants do. These non-participants, probably poor, might attend SBP schools, or they might attend non-SBP schools. Such non-participants may exist. If so, then selective expansion of the SBP offers a means to enjoy the same return as we observed in our 716-respondent sample.

Graph 1: US Schools and SBP Schools, 1967-98



Graph 2: Daily Breakfasts as a Percentage of Total Enrollment, 1967-98

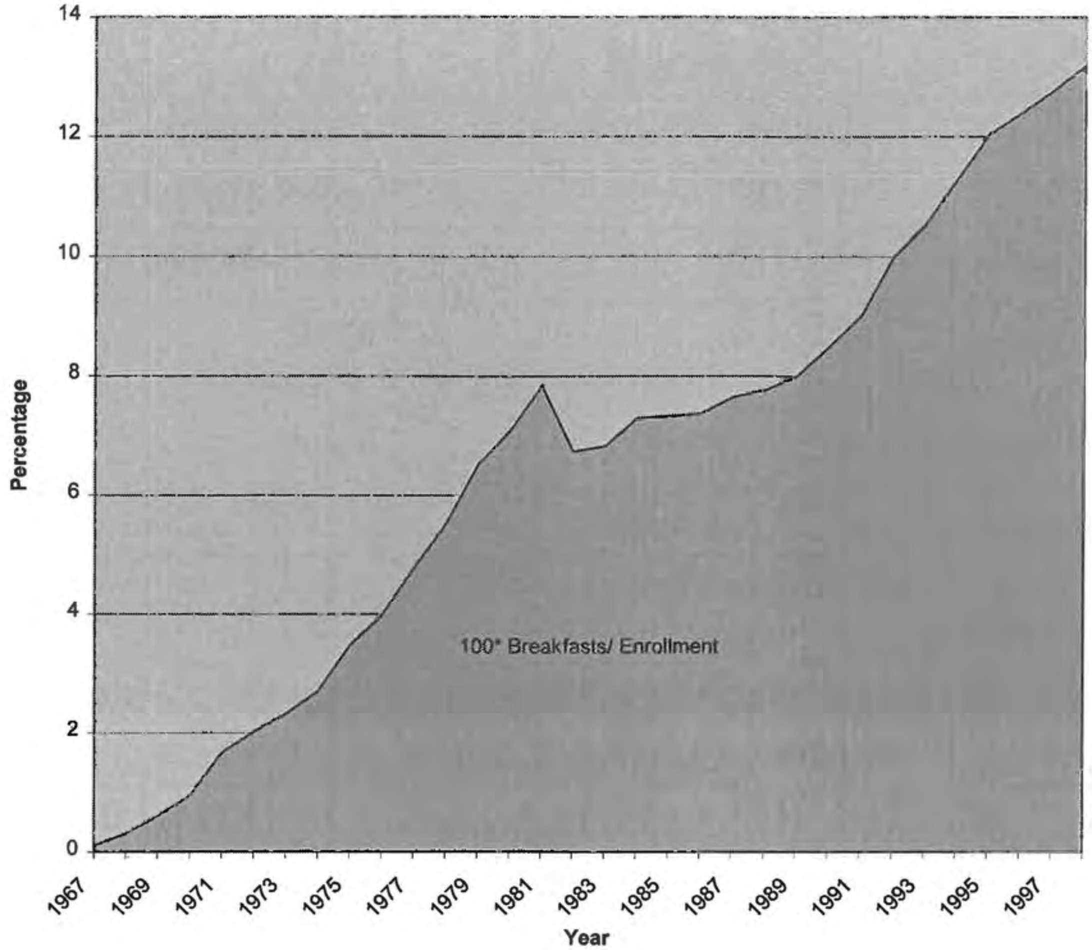


Table 1: FY2000 Reimbursement Rates for 48 Contiguous States		
Eligibility	Non-Severe Need	Severe Need
Full Price	$0.21	$0.21
Reduced-Price	$0.79	$1.00
Free	$1.09	$1.30

Table 2: Mean Enrollment by Grade and SBP Availability, 1978 US GAO Sample			
Grade Level	SBP Schools	Non-SBP Schools	All Schools
Elementary	505	386	406
Junior High	584	443	459
Jr.-Sr. High	1240	861	890
Consolidated	470	446	449
Special	337	477	455
All Grades	585	494	504

Table 3: Significant Variables in NSLP Demand by Age and Eligibility Status		
Age Group	Free or Reduced Price Eligible	Full Price
Ages 6-11	-Food Stamp Program (FSP) Bonus (in $),[89] Urban/Rural, Income, +Household Size, Race, -Calories Consumed at Other Meals	Race, -Price, +Vitamin A, +Age
Ages 12-18	+Extent of Female Head's Employment, Race, +Vitamin A, Urban/Rural, -Age, -Income, -Calories Consumed at Other Meals	-Price, Race, Urban/Rural, -Age

[89] At this time, the FSP still restricted food choices, which likely altered participants' behavior significantly.

Table 4: Breakfast Composition by Source	
Home breakfast	School breakfast
Grits, oatmeal, cold cereal Pancakes Ham, bologna Pop, candy, dessert items	Biscuits, bread, toast, rolls Milk, cheese Bacon, sausage (contains TVP[90]) Fruits

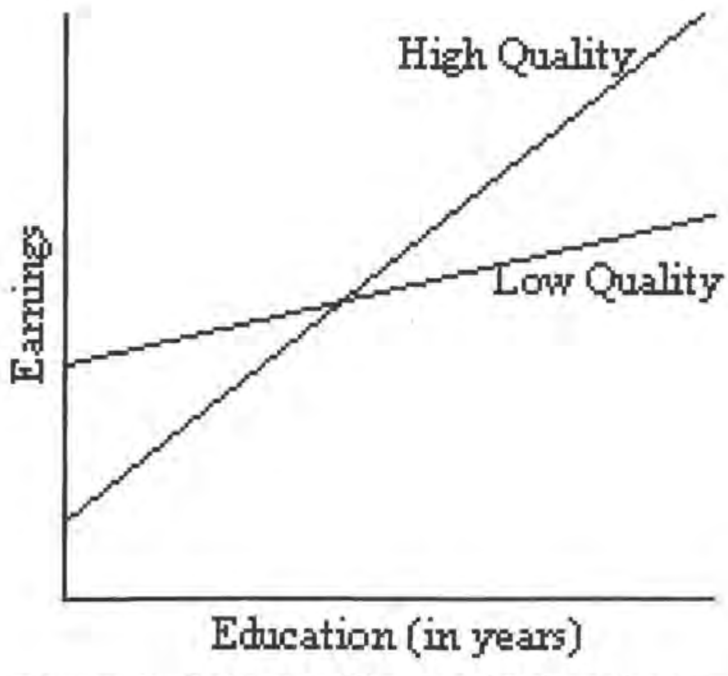


Figure 1: Return to Years of Education by Level of Educational Quality

[90] Texturized vegetable protein (a soy product).

Figure 2: Proposed Cause and Effect Relationship for the SBP

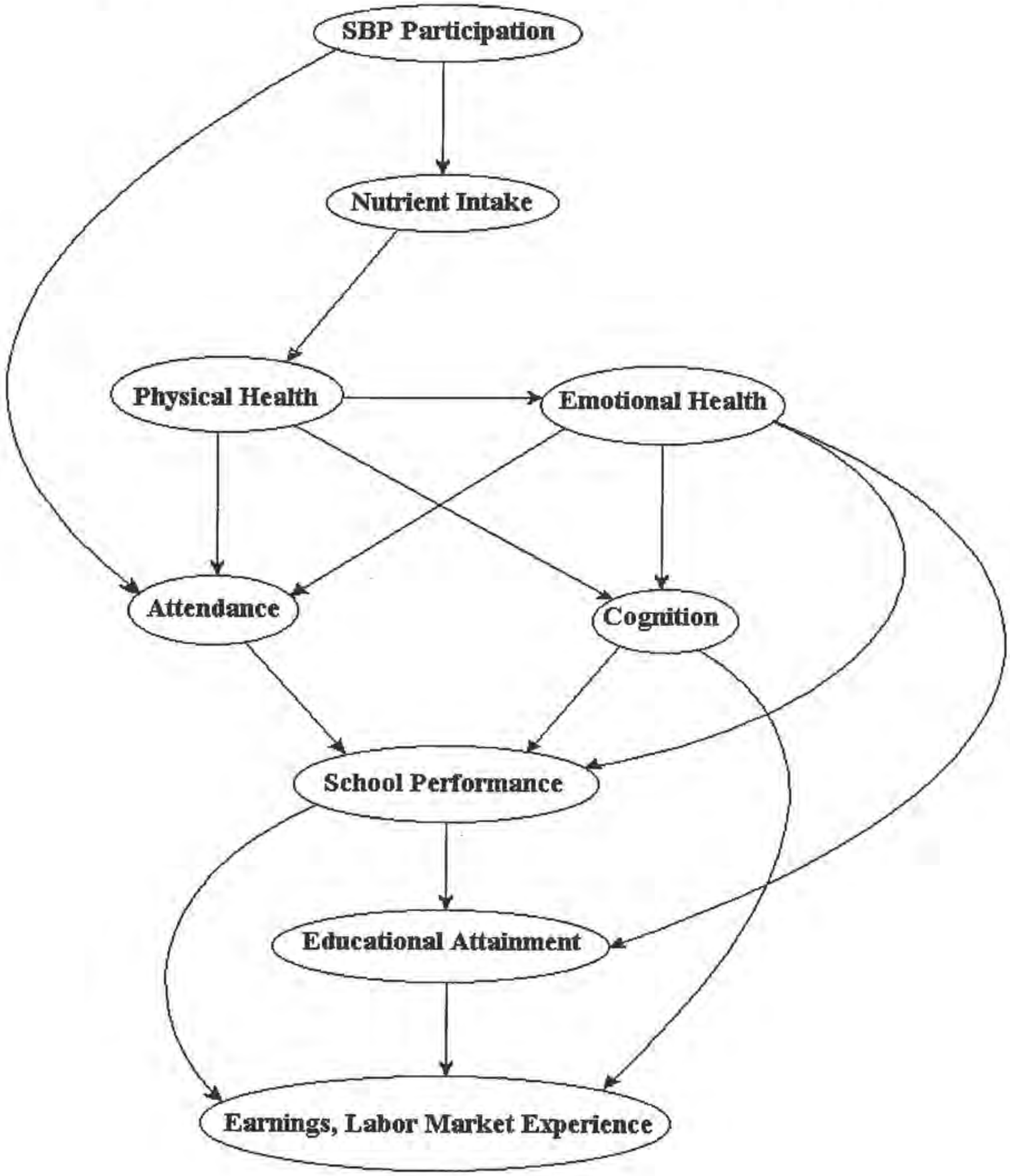


Table 5: Variables and Years Available for USDA SBP Data Set, State-Level Observations		
Variable[91]	Years Available Annually by State	Years Available Nationwide by Month
Schools Participating in the Program	1967-1998, excluding 1976(TQ)[92]	1967-1975
Enrollment in SBP Schools	1974-1998, excluding 1976(TQ)	1974-1975
Average Daily Attendance in SBP Schools	1967-1998, excluding 1976(TQ)	1967-1975, 1982-1998
Participants	1967-1998	1967-1975, 1982-1998
Students Approved to Receive Free Meals	1989-1998	None
Students Approved to Receive Reduced-Price Meals	1989-1998	None
Average Daily Breakfasts	1970-1998	1970-1975, 1982-1998
Total Breakfasts	1967-1998	1967-1975, 1982-1998
Free Breakfasts	1976-1998	1982-1998
Reduced-Price Breakfasts	1976-1998	1982-1998
Free & Reduced-Price Breakfasts	1967-1975	1967-1975
Free & Reduced-Price Breakfasts as a Percentage of Total Breakfasts	1967-1998	1967-1975, 1982-1998
Average Indemnity Rate	1967-1975	1967-1975
Children's Contribution	1967-1970	1967-1970
Cost of Purchased Food	1967-1970	1967-1970
Federal CNA Funds	1969-1973	1969-1973
Federal Section 32 Funds	1969-1973	1969-1973
Total Federal Reimbursement	1967-1998, (1976 Reimbursement expressed as breakfasts/ cents)	1967-1975, 1982-1998

[91] In 1967, the USDA also tracked Other Cash [Income] (besides income from children, states, and the federal government), Donated Labor, Total Income from States, Total Income, Food Expenditure, Paid Labor, Donated Labor, Other Cash [Expenditure], Total Expenditure, all in dollars, and Food as a Percentage of Total

[92] The label, 1976TQ, denotes "1976 Transition Quarter." In 1977, the USDA changed the fiscal year from July-June to October-September. The transition quarter includes July, August, and September 1976. We have omitted the transition quarter from our analysis.

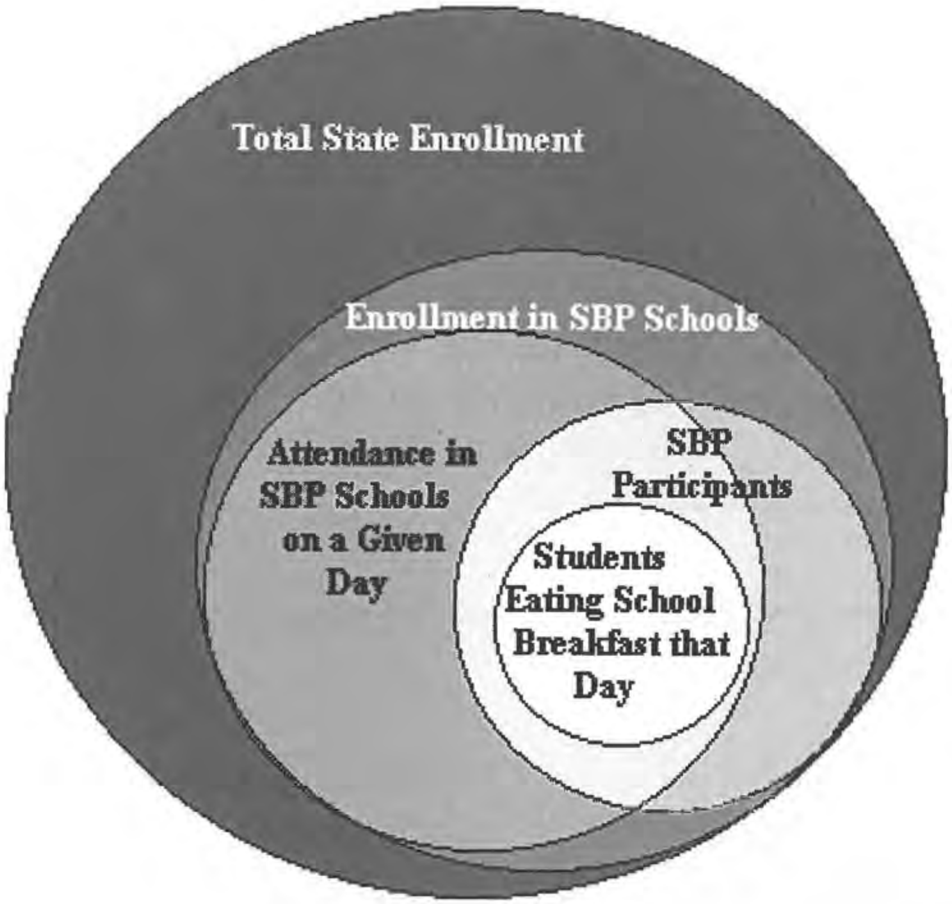


Figure 3: Venn Diagram of Enrollment, ADA, and SBP Variables

$$\begin{gathered}\rho_2{*}LBNP_{s,t} + \rho_3{*}LBMP_{s,t} = \\ \rho_2{*}(LBPE_{s,t} + LPPE_{s,t}) + \rho_3{*}(LBPE_{s,t} - LPPE_{s,t}) = \\ (\rho_2 + \rho_3){*}LBPE_{s,t} + (\rho_2 - \rho_3){*}LPPE_{s,t} = \\ \pi_2{*}LBPE_{s,t} + \pi_3{*}LPPE_{s,t}\end{gathered}$$

Figure 4: Conversion of Coefficients from Equation (17) to Equation (16).

Let $Y_i = b_1*(x_i/\boldsymbol{x} + z_i/\boldsymbol{z}) + b_2*(x_i/\boldsymbol{x} - z_i/\boldsymbol{z})$, where $\boldsymbol{x}$ = mean(x_i) and $\boldsymbol{z}$ = mean(z_i). Imagine we begin at $x_i = \boldsymbol{x}$ and $z_i = \boldsymbol{z}$. Holding $\boldsymbol{x}$ and $\boldsymbol{z}$ constant, we increase x_i and z_i by a factor w. We would like to know the resulting change in our dependent variable, Y_i. We substitute $w\boldsymbol{x}$ and $w\boldsymbol{z}$ for x_i and z_i, add ΔY_i to the left side and solve for ΔY_i.

$$Y_i + \Delta Y_i = b_1*(w\boldsymbol{x}/\boldsymbol{x} + w\boldsymbol{z}/\boldsymbol{z}) + b_2*(w\boldsymbol{x}/\boldsymbol{x} - w\boldsymbol{z}/\boldsymbol{z}).$$

$$Y_i + \Delta Y_i = b_1*(w + w) + b_2*(w - w).$$

$Y_i + \Delta Y_i = 2w*b_1$. Take the derivative of Y_i with respect to w.

$$\Delta Y_i/\Delta w = 2*b_1.$$

Figure 5: Mathematical Rationale Behind BNP_i and BMP_i, Evaluated at the Mean.

Table 6: Composition of 716-Respondent NLSY79 Sub-Sample			
	Male	Female	Total
Black	189	173	362
Hispanic	59	52	111
Non-black, non-Hispanic	116	127	243
Total	364	352	716

Table 7: USDA Data Serial Correlation Regression

Variable	$BPA_{s,t}$
C	-553 (2170)
$u_{s,t-1}$	1.34*** (0.026)
$u_{s,t-2}$	-0.343*** (0.0265)
Adj. R^2	0.979

Tables 6-8 describe regressions using annual data from 51 states (including DC) over 29 years (1967-1995)

Table 8: USDA Data Year-Dummy Regression

Variable	$BPA_{s,t}$	Var. (cnt'd)	$BPA_{s,t}$
C	-114 (93.6)	1984	-2.16 (4.60)
1970	-0.114 (0.289)	1985	-3.14 (4.29)
1971	0.469 (0.575)	1986	-4.09 (5.27)
1972	0.729 (0.869)	1987	-4.96 (5.61)
1973	-0.00405 (1.17)	1988	-5.46 (5.95)
1974	-0.520 (1.46)	1989	-6.28 (6.30)
1975	-0.193 (1.76)	1990	-6.33 (6.66)
1976	0.339 (20.66)	1991	-6.13 (7.02)
1977	1.09 (2.37)	1992	-5.43 (7.38)
1978	1.26 (2.68)	1993	-4.89 (7.75)
1979	2.21 (2.99)	1994	-3.97 (8.13)
1980	2.63 (3.30)	1995	-3.68 (8.51)
1981	2.23 (3.62)	$u_{s,t-1}$	1.29*** (0.0267)
1982	-0.732 (3.94)	$u_{s,t-2}$	-0.285*** (0.0276)
1983	-1.45 (4.27)	Adj. R^2	0.983

Years 1967-1969 omitted for comparison

Table 8: USDA Data State-Dummy Regression

Variable	$BPA_{s,t}$	Var. (cnt'd)	$BPA_{s,t}$	Var. (cnt'd)	$BPA_{s,t}$
C	37.8*** (10.1)	KY	25.9** (13.1)	OK	1.76 (13.0)
AL	6.83 (13.0)	LA	25.4* (13.2)	OR	-15.1 (13.1)
AK	-24.5* (13.3)	ME	-17.7 (13.1)	PN	-19.7 (13.1)
AZ	-3.34 (13.0)	MD	-15.9 (13.1)	RI	-22.1* (13.3)
AR	14.1 (13.1)	MA	-13.9 (13.1)	SC	13.0 (13.0)
CA	-4.12 (13.0)	MI	-20.2 (13.1)	SD	-9.42 (13.0)
CO	-22.9* (13.22)	MN	-22.2* (13.2)	TN	5.66 (13.0)
CT	-21.4 (13.1)	MS	36.9*** (13.4)	TX	8.78 (13.0)
DE	-5.26 (13.0)	MO	-7.56 (13.0)	UT	-27.6** (13.5)
DC	0.946* (13.0)	MT	-20.1 (13.2)	VT	-20.0 (13.1)
FL	14.5 (13.1)	NE	-22.8* (13.2)	VA	-9.22 (13.0)
GA	4.72 (13.0)	NV	-12.8 (13.1)	WA	-17.5 (13.1)
HI	-22.1 (13.1)	NH	-22.7* (13.2)	WV	24.8** (13.1)
ID	-17.0* (13.1)	NJ	-23.0* (13.2)	WI	-28.3** (13.3)
IL	-17.4 (13.1)	NM	3.18 (13.0)	WY	-24.2 (13.2)
IN	-19.8 (13.1)	NC	11.0 (13.0)	$u_{s,t-1}$	1.28*** (0.0264)
IA	-13.4 (13.0)	ND	-20.0 (13.1)	$u_{s,t-2}$	-0.316**
KS	25.9 (13.1)	OH	-16.5 (13.1)	Adj. R^2	0.980

New York omitted for comparison

Table 10: Regression Results for Model 1					
Variable	$APE_{s,t}$ *Column 1*	$APE_{s,t}$ *Column 2*	$LAPE_{s,t}$ *Column 3*	$LAPE_{s,t}$ *Column 4*	$LAPE_{s,t}$ *Column 5*
C	145.37*** (4.46)	152.38*** (4.42)	5.0950*** (0.0893)	5.0941*** (0.0859)	5.0941*** (0.0859)
$BPE_{s,t}$	0.0995*** (0.0184)	0.6017*** (0.0456)			
$LBPE_{s,t}$			0.02934*** (0.00496)	0.1659*** (0.0135)	
$PPE_{s,t}$		-0.497*** (0.0419)			
$LPPE_{s,t}$				-0.1438*** (0.0134)	
$LBNP_{s,t}$					0.01102*** (0.00239)
$LBMP_{s,t}$					0.01549*** (0.0133)
$TPENR_{s,t}$	157.8** (75.4)	95.2 (71.3)			
$LTPENR_{s,t}$			0.0493 (0.0294)	0.0320 (0.0283)	0.0320 (0.0283)
$u_{s,t-1}$	0.2102*** (0.0281)	0.2197*** (0.0284)	0.2139*** (0.0282)	0.2358*** (0.0283)	0.2358*** (0.0283)
Adjusted R^2	0.212	0.309	0.211	0.292	0.292

Data cover 51 states (including DC) and 22 years (1974-1995). Coefficients for State and Year Dummies Not Shown

Table 11: Descriptive Statistics for Variables in Model 1, 1121 Observations					
Variable	Mean	Median	Maximum	Minimum	Std. Dev.
$SBPENROLL_{s,t}$	3.21 E+5	1.59 E+5	3.64 E+6	1760	4.95 E+5
$SBPADA_{s,t}$	2.96 E+5	1.46 E+5	3.37 E+6	1670	4.58 E+5
$BKTOT_{s,t}$	1.18 E+7	5.58 E+6	1.36 E+8	57600	1.75 E+7
$PARTICIP_{s,t}$	71100	33600	7.53 E+5	257	1.04 E+5
$TEACHERS_{s,t}$	44800	32900	2.40 E+5	4090	44800
$ADA_{s,t}$	7.56 E+5	5.50 E+5	5.20 E+6	69100	8.14 E+5
$ENROLL_{s,t}$	8.19 E+5	5.89 E+5	5.54 E+6	79800	8.65 E+5
$APE_{s,t}$	166	167	178	102	6.49
$YATPEN_{s,t}$	166	166	188	147	4.78
$BPE_{s,t}$	38.8	35.9	131	4.38	15.4
$LBPE_{s,t}$	3.58	3.58	4.87	1.48	0.397
$PPE_{s,t}$	41.9	37.4	144	4.14	17.6
$LPPE_{s,t}$	3.65	3.62	4.97	1.42	0.413
$LBNP_{s,t}$	7.24	7.20	9.84	2.90	0.803
$LBMP_{s,t}$	-0.0699	-0.0725	0.460	-0.931	0.109
$TPENR_{s,t}$	0.0566	0.0563	0.0787	0.0395	0.00772
$LTPENR_{s,t}$	-2.88	-2.88	-2.54	-3.23	0.138

Table 12: Descriptive Statistics for Variables in Model 2a, 716 Observations					
Variable	Mean	Median	Maximum	Minimum	Std. Dev.
HGC_i	12.3	12	20	7	1.97
$HSDUM_i$	0.753	1	1	0	0.432
$FREEBK_i$	4.75 E+6	2.81 E+6	2.90 E+7	44313	5.62 E+6
$ADAKT2_i$	4.93 E+6	3.35 E+6	1.41 E+7	218 E+5	3.64 E+6
$PART_i$	45200	27100	2.60 E+5	1000	45800
$SBPADAKT2_i$	1.65 E+5	1.13 E+5	8.68 E+5	2860	1.64 E+5
$FBPA_i$	1.16	0.792	9.05	0.00663	1.25
BPM_i	1.00	0.682	7.79	0.00570	1.08
PPA_i	0.0119	0.00804	0.131	0.000327	0.0136
PPM_i	1.00	0.673	11.0	0.0274	1.14
BNP_i	2.00	1.38	16.1	0.0331	2.13
BMP_i	3.56 E-5	-0.00458	1.65	-6.15	0.624
$HGCMOM_i$	9.65	11	20	0	2.97
$INCOME78_i$	6440	5470	70000	0	5000
$FSIZE_i$	5.86	6	15	2	2.32
IPF_i	1210	1060	12500	0	1050
$CTMEDINC_i$	8320	8440	13800	3090	2120
$BLACK_i$	0.506	1	1	0	0.500
$MALE_i$	0.508	1	1	0	0.500
$RESPAGE_i$	22.9	21	36	18	5.24

Table 13: Descriptive Statistics for Variables in Model 2b						
Variable	Mean	Median	Maximum	Minimum	Std. Dev	Obs.
EMP_i (Eqn. 22)	0.829	1	1	0	0.377	662
EMP_i (Eqn. 23)	0.854	1	1	0	0.354	636
$WAGE_i$ (Eqn. 24)	8.29	7.00	45.70	1.74	4.74	542
CPI_i (Eqn. 24)	1.41	1.45	1.45	0.909	0.0727	542
$WAGE_i/CPI_i$ (Eqn. 24)	5.87	4.84	31.6	1.20	3.29	542
$Log(WAGE_i/CPI_i)$ (Eqn. 24)	1.65	1.58	3.45	0.186	0.457	542
$HSDUM_i$ (Eqn. 22)	0.748	1	1	0	0.435	662
$HSDUM_i$ (Eqn. 23)	0.761	1	1	0	0.427	636
$HSDUM_i$ (Eqn. 24)	0.788	1	1	0	0.409	542
$MALE_i$ (Eqn. 22)	0.515	1	1	0	0.500	662
$MALE_i$ (Eqn. 23)	0.524	1	1	0	0.500	636
$MALE_i$ (Eqn. 24)	0.506	1	1	0	0.500	542
$BLACK_i$ (Eqn. 23)	0.509	1	1	0	0.500	636
$BLACK_i$ (Eqn. 24)	0.506	1	1	0	0.500	542
$HGCMOM_i$ (Eqn. 24)	9.63	11	20	0	3.04	542
$INCOME78_i$ (Eqn. 24)	6,530	5,400	70,000	200	5,291	542
$FSIZE_i$ (Eqn. 24)	5.88	6	15	2	2.34	542
IPF_i (Eqn. 24)	1,230	1,070	12,500	33.3	1,120	542
AGE_i (Eqn. 23)	28.62	29	31	23	1.84	636
AGE_i (Eqn. 24)	28.8	29	31	18	1.92	542
$WREMP_i$ (Eqn. 23)	491,000	254,000	3,680,000	3,730	605,000	636
$WRPOP_i$ (Eqn. 23)	648,000	347,000	472,000	4,240	788,000	636
$WREMP_i/WRPOP_i$ (Eqn. 23)	0.735	0.747	1	0.511	0.0757	636
$WYear_i$ (Eqn. 22)	1991.72	1993	1993	1980	2.07	662
$WYear_i$ (Eqn. 24)	1992.21	1993	1993	1981	1.56	542

Table 14: Regression Results for Model 2, Equations (18) – (21)				
Variable	$HGC_{i\ (OLS)}$ *Column 1*	$HSDUM_{i\ (Logit)}$ *Column 2*	$HGC_{i\ (OLS)}$ *Column 3*	$HSDUM_{i(Logit)}$ *Column 4*
$FBPA_i$	0.247* (0.126)	0.380** (0.194)		
BNP_i			0.156* (0.0813)	0.217* (0.120)
BMP_i			0.0176 (0.351)	0.226 (0.552)
$HGCMOM_i$	0.0929*** (0.0251)	0.0838** (0.0356)	0.0926*** (0.0252)	0.0839** (0.0356)
IPF_i	0.000170** (6.78E-05)		0.000169** (6.79E-05)	
$INCOME78_i$		7.76E-05*** (2.86E-05)		7.76E-05*** (2.86E-05)
$CTMEDINC_i$	-0.000214*** (5.42E-05)	-0.000268*** (7.67E-05)	-0.000213*** (5.43E-05)	-0.000269*** (7.68E-05)
$BLACK_i$	0.231 (0.178)	0.587** (0.250)	0.231 (0.178)	0.588** (0.250)
$MALE_i$	-0.215 (0.137)	-0.627*** (0.198)	-0.218 (0.137)	-0.625*** (0.199)
$RESPONSEAGE\_18_i$	-0.525*** (0.200)	-0.686** (0.273)	-0.523*** (0.200)	-0.687** (0.273)
$RESPAGE_i$	0.1216*** (0.0150)	0.0349 (0.0233)	0.122*** (0.0150)	0.0349 (0.0233)
AGE_14_i	10.114 (0.760)	1.106 (1.12)	10.107 (0.760)	1.10 (1.12)
AGE_15_i	10.227 (0.725)	0.892 (1.071)	10.208 (0.728)	0.896 (1.072)
AGE_16_i	10.788 (0.718)	1.835 (1.06)	10.778 (0.719)	1.84 (1.06)
AGE_17_i	10.591 (0.726)	1.74 (1.07)	10.582 (0.726)	1.74 (1.07)
Adjusted R^2	0.192		0.191	
Log Likelihood	-1397	-336	-1397	-336

Regressions using restricted sample of 716 NLSY79 respondents.
Coefficients for birth-state dummies not shown.

Table 15: Regression Results for Model 2: Log Formulation, Equations (19) - (21)				
Variable	HGC_i (OLS) *Column 1*	$HSDUM_i$ (Logit) *Column 2*	HGC_i (OLS) *Column 3*	$HSDUM_{i(\text{Logit})}$ *Column 4*
$LFBPA_i$	-0.254 (0.154)	-0.424** (0.215)		
$LBNP_i$			-0.0639 (0.135)	-0.102 (0.195)
$LBMP_i$			-0.287 (0.291)	-0.493 (0.432)
$HGCMOM_i$	0.105** (0.0254)	0.985*** (0.359)	0.105*** (0.0254)	0.0984*** (0.0360)
$LIPF_i$	0.125 (0.113)		0.126 (0.113)	
$LINCOME78_i$		0.333** (0.161)		0.334** (0.161)
$LCTMEDINC_i$	-1.70*** (0.420)	-2.13*** (0.611)	--1.70*** (0.420)	-2.11*** (0.610)
$BLACK_i$	0.241 (0.179)	0.572** (0.247)	0.242 (0.179)	0.575*** (0.248)
$MALE_i$	-0.224* (0.138)	-0.643*** (0.199)	-0.223 (0.138)	-0.641*** (0.199)
$RESPONSEAGE\_18_i$	-0.547** (0.200)	-0.716*** (0.271)	-0.551*** (0.201)	-0.724*** (0.272)
$RESPAGE_i$	0.117*** (0.0150)	0.0255 (0.0230)	0.117*** (0.0150)	0.0260 (0.0230)
AGE_14_i	23.54 (3.84)	16.57 (5.74)	24.58 (4.25)	18.29 (6.27)
AGE_15_i	23.30 (3.84)	15.86 (5.73)	24.38 (4.28)	17.65 (6.31)
AGE_16_i	23.42 (3.84)	16.08 (5.74)	24.59 (4.35)	18.01 (6.41)
AGE_17_i	22.87 (3.86)	15.39 (5.74)	24.11 (4.42)	17.43 (6.45)
Adjusted R^2	0.187		0.186	
Log Likelihood	-1397	-338	-1397	-338

Regressions using restricted sample of 716 NLSY79 respondents.
Coefficients for birth-state dummies not shown.
$LBNP_i$ and $LBMP_i$ represent the sum and the difference, respectively, of $FBPA_i$ and PPA_i.

Table 16: Regression Results for Model 2, Equations (22) – (24)			
Variable	EMP_i (Logit) *Column 1*	EMP_i (Logit) *Column 2*	$log(WAGE_i/CPI_i)$ (OLS) *Column 3*
C	-3.55** (1.63)	-18.4*** (2.89)	0.203 (0.340)
$HSDUM_i$	0.697* (0.399)	0.875*** (0.267)	0.224*** (0.0476)
$MALE_i$	0.971** (0.383)	0.685*** (0.256)	0.166*** (0.0385)
$BLACK_i$		-0.694** (0.322)	-0.138*** (0.0503)
$HGCMOM_i$			0.0223*** (0.00671)
IPF_i			2.91 E-5 (1.84 E-5)
$WAGEAGE_i$		0.609*** (0.0807)	0.0378*** (0.0113)
$WREMP_i/WRPOP_i$		3.44* (1.95)	
Adjusted R^2			0.216
Log Likelihood	-122	-216	-232
Observations	662	636	542

Coefficients for year-, state-of-residence-, and birth-state-dummies for Equation (22) not shown. Coefficients for state-of-residence- and birth-state-dummies for Equation (24) not shown. Equation (23) did not include these dummies.

Table 17: Year- and State-Dummy Coefficients For Equation (14)

Years 1974 and 1975 and New York State omitted for comparison

Variable	$APE_{s,t}$
1976	-0.204 (1.01)
1977	0.261 (1.17)
1978	-0.149 (1.19)
1979	1.15 (1.21)
1980	1.32 (1.24)
1981	2.90** (1.23)
1982	3.16** (1.26)
1983	3.46*** (1.28)
1984	3.35** (1.30)
1985	2.64** (1.33)
1986	3.32** (1.33)
1987	3.40** (1.34)
1988	3.07** (1.38)
1989	2.85** (1.41)
1990	3.33** (1.40)
1991	2.96** (1.41)
1992	3.63*** (1.39)

Var. (cnt'd)	$APE_{s,t}$
1993	3.20** (1.40)
1994	3.53** (1.41)
1995	3.73*** (1.41)
AL	4.53** (2.10)
AK	-0.651 (2.00)
AZ	4.38** (2.10)
AR	3.49* (2.05)
CA	4.17* (2.32)
CO	6.18*** (2.05)
CT	-3.70* (2.10)
DE	2.38 (2.00)
DC	1.50 (2.01)
FL	4.12** (2.06)
GA	4.92** (2.09)
HI	6.58*** (2.18)
ID	0.0959 (2.16)
IL	2.37 (2.02)

Var. (cnt'd)	$APE_{s,t}$
IN	3.15 (2.07)
IA	3.83* (1.99)
KS	2.78 (2.00)
KY	3.72* (2.11)
LA	3.83* (1.99)
ME	4.09** (2.00)
MD	4.10** (2.03)
MA	4.46** (2.02)
MI	3.51 (2.17)
MN	4.79** (2.01)
MS	4.28** (2.17)
MO	2.53 (2.01)
MT	-3.42* (1.99)
NE	2.36 (2.01)
NV	4.90** (2.16)
NH	1.36 (1.99)
NJ	1.94 (2.04)
NM	5.53*** (2.06)

Var. (cnt'd)	$APE_{s,t}$
NC	3.75* (2.05)
ND	3.65* (1.99)
OH	1.69 (2.06)
OK	0.831 (2.08)
OR	4.09** (2.07)
PN	2.84 (2.00)
RI	-0.143 (2.00)
SC	4.43** (2.06)
SD	-2.80 (2.04)
TN	5.23** (2.14)
TX	5.25*** (2.00)
UT	5.42** (2.45)
VT	4.25** (2.08)
VA	-0.689 (1.99)
WA	6.29*** (2.21)
WV	2.71 (2.00)
WI	-0.0843 (1.99)
WY	1.19 (2.02)

Table 18: State-Dummy Coefficients For Equation (19)

Variable	$HSDUM_i$	Var. (cnt'd)	$HSDUM_i$	Var. (cnt'd)	$HSDUM_i$
AL	0.966 (0.858)	KY	-1.68 (1.04)	OK	0.822 (1.14)
AK	-36.1 (3.39E+7)	LA	-0.0275 (1.21)	OR	35.7 (4.41E+7)
AZ	-1.35 (1.50)	MD	-1.80 (1.50)	PN	0.521 (0.575)
AR	-0.123 (0.959)	MA	0.689 (0.697)	SC	-0.523 (0.713)
CA	-0.438 (0.445)	MI	-0.0997 (0.601)	SD	34.4 (3.28E+7)
CO	1.79 (1.17)	MN	35.0 (3.48E+7)	TN	-1.10 (0.729)
CT	35.3 (3.97E+7)	MS	-1.62 (1.03)	TX	-0.342 (0.517)
DC	-3.00*** (1.12)	MO	-1.32* (0.679)	UT	0.240 (1.32)
FL	-0.483 (0.624)	MT	-1.39 (1.49)	VA	-1.32 (1.03)
GA	-0.983* (0.560)	NE	35.5 (4.41E+7)	WA	35.4 (3.48E+7)
IL	-0.0941 (0.612)	NJ	0.736 (0.677)	WV	0.134 (1.28)
IN	0.206 (0.920)	NM	35.2 (3.03E+7)	WI	1.36 (0.885)
IA	-0.931 (1.29)	NC	-0.808 (0.611)		
KS	33.4 (3.39E+7)	OH	-1.24* (0.721)		

New York State omitted for comparison. Restricted Sample did not include respondents from Delaware, Hawaii, Idaho, Maine, Nevada, New Hampshire, North Dakota, Rhode Island, or Wyoming.

New York omitted for comparison. All other omitted states left out due to perfect multicollinearity.

Table 19: Year-, State-of-Residence-, and Birth-State-Dummy Coefficients For Equation 22

Variable	EMP_i
1993	7.16 (1.66)
1992	0.530 (1.56)
1991	1.12 (1.60)
1990	2.89 (1.54)
1989	1.74 (1.65)
1988	-0.269 (1.67)
1987	0.412 (1.97)
1985	-70.3 (1.83 E+8)
1984	-0.352 (3.55)
1983	-35.7 (2.20)
1982	-0.0802 (2.20)
1981	37.7 (3.41 E+8)
AL (Residence)	-0.658 (4.84)
AK (Residence)	35.68 (3.41 E+8)
AZ (Residence)	0.669 (3.46)
AR (Residence)	-0.538 (1.87)
CA (Residence)	2.47 (2.86)

Variable	EMP_i
CO (Res)	1.77 (1.33)
CT (Res)	38.3 (3.40 E+8)
DE (Res)	32.1 (3.41 E+8)
DC (Res)	34.0 (1.98 E+8)
FL (Res)	4.32 (2.60)
GA (Res)	0.193 (2.47)
IL (Res)	-3.86 (3.18)
IN (Res)	2.84 (1.68)
IA (Res)	3.04 (3.35)
KS (Res)	-1.30 (3.89)
KY (Res)	1.09 (2.12)
LA (Res)	-1.98 (1.49)
MD (Res)	34.9 (1.85 E+8)
MA (Res)	1.18 (1.15)
MI (Res)	4.02 (13.0)
MN (Res)	44.1 (3.81 E+8)

Var. (cnt'd)	EMP_i
MS (Res)	-1.80 (5.02)
MO (Res)	0.686 (2.18)
MT (Res)	38.0 (2.03 E+8)
NE (Res)	39.4 (3.41 E+8)
NV (Res)	40.8 (2.19 E+8)
NH (Res)	38.3 (3.41 E+8)
NJ (Res)	2.09 (4.82)
NM (Res)	37.1 (1.16 E+8)
NC (Res)	0.938 (2.50)
OH (Res)	1.57 (2.55)
OK (Res)	0.595 (1.06)
OR (Res)	34.0 (3.41 E+8)
PN (Res)	1.22 (1.22)
SC (Res)	-0.660 (8.84)
SD (Res)	1.68 (1.56)
TN (Res)	4.13 (1.40)

Var. (cnt'd)	EMP_i
TX (Res)	3.00 (1.62)
UT (Res)	2.54 (3.82 E+8)
VT (Res)	0.840 (1.14)
VA (Res)	-1.28 (2.64)
WA (Res)	40.5 (1.70 E+8)
WV (Res)	40.5 (8.47 E+7)
WI (Res)	-0.226 (3.42)
AL (Birth)	1.68 (4.82)
AZ (Birth)	37.3 (2.12 E+8)
CA (Birth)	-2.63 (2.80)
CT (Birth)	-4.26 (4.17 E+8)
DC (Birth)	-0.404 (3.16)
FL (Birth)	-3.12 (2.55)
GA (Birth)	1.37 (2.50)
IL (Birth)	2.98 (3.26)
IA (Birth)	-4.46 (3.47)

Var. (cnt'd)	EMP_i
KY (Birth)	0.595 (2.16)
MD (Birth)	-0.0113 (3.04 E+8)
MI (Birth)	-2.91 (-2.91)
MN (Birth)	32.0 (3.46 E+8)
MS (Birth)	2.63 (4.95)
MO (Birth)	0.898 (2.14)
NJ (Birth)	-0.403 (4.79)
NM (Birth)	31.9 (1.01 E+8)
NC (Birth)	0.348 (2.33)
OH (Birth)	-1.45 (2.46)
SC (Birth)	1.89 (8.82)
TX (Birth)	-2.73 (1.46)
UT (Birth)	34.3 (3.43 E+8)
VA (Birth)	3.11 (2.65)
WA (Birth)	-6.48 (2.41 E+8)
WV (Birth)	32.7 (9.44 E+7)
WI (Birth)	3.21 (3.16)

Table 20: State-of-Residence- and Birth-State Dummy Coefficients For Equation 24

New York omitted for comparison. All other omitted states left out due to perfect multicollinearity.

AL (Residence)	-0.265 (0.234)
AK (Residence)	-0.148 (0.416)
AZ (Residence)	-0.298 (0.394)
AR (Residence)	-0.211 (0.255)
CA (Residence)	0.104 (0.198)
CO (Residence)	-0.308 (0.253)
CT (Residence)	-0.202 (0.415)
DE (Residence)	-0.653 (0.483)

Var. (cnt'd)	$Log(WAGE_i/CPI_i)$
DC (Res)	0.242 (0.532)
FL (Res)	-0.0445 (0.193)
GA (Res)	-0.264 (0.224)
IL (Res)	0.289 (0.283)
IN (Res)	-0.326 (0.372)
IA (Res)	-0.586 (0.282)
KS (Res)	0.408 (0.548)
KY (Res)	0.239 (0.343)
LA (Res)	-0.428 (0.393)
MD (Res)	0.0844 (0.396)
MA (Res)	-0.0299 (0.147)
MI (Res)	-0.251 (0.277)
MN (Res)	-0.200 (0.570)
MS (Res)	-0.390 (0.266)
MO (Res)	0.200 (0.228)
MT (Res)	-0.162 (0.298)
NE (Res)	-0.430 (0.416)
NV (Res)	-0.406 (0.358)

Var. (cnt'd)	$Log(WAGE_i/CPI_i)$
NH (Res)	0.438 (0.522)
NJ (Res)	0.767 (0.260)
NM (Res)	-0.352 (0.364)
NC (Res)	-0.193 (0.364)
OH (Res)	0.0240 (0.2530
OK (Res)	-0.602 (0.271)
OR (Res)	-0.116 (0.312)
PN (Res)	0.617 (0.414)
SC (Res)	-0.519 (0.334)
SD (Res)	0.705 (0.2700
TN (Res)	-0.0308 (0.5930
TX (Res)	-0.100 (0.266)
UT (Res)	0.167 (0.203)
VT (Res)	0.0986 (0.694)
VA (Res)	0.237 (0.249)
WA (Res)	0.0550 (0.2920
WV (Res)	0.302 (0.354)
WI (Res)	0.00536 (0.327)

Var. (cnt'd)	$Log(WAGE_i/CPI_i)$
AL (Birth)	0.144 (0.232)
AZ (Birth)	-0.0813 (0.418)
AR (Birth)	-0.0783 (0.228)
CA (Birth)	-0.762 (0.192)
CO (Birth)	0.354 (0.288)
CT (Birth)	0.734 (0.498)
DC (Birth)	-0.417 (0.345)
FL (Birth)	-0.0839 (0.212)
GA (Birth)	0.121 (0.231)
IL (Birth)	-0.305 (0.280)
IN (Birth)	-0.225 (0.310)
IA (Birth)	-0.972 (0.356)
KS (Birth)	-0.689 (0.469)
KY (Birth)	-0.399 (0.319)
LA (Birth)	0.176 (0.401)
MD (Birth)	0.887 (0.4870
MI (Birth)	0.246 (0.286)

Var. (cnt'd)	$Log(WAGE_i/CPI_i)$
MN (Birth)	0.0690 (0.494)
MS (Birth)	0.192 (0.269)
MO (Birth)	-0.399 (0.210)
NJ (Birth)	-0.240 (0.251)
NM (Birth)	0.0833 (0.299)
NC (Birth)	0.168 (0.248)
OH (Birth)	-0.116 (0.265)
OK (Birth)	0.433 (0.282)
PN (Birth)	-0.692 (0.325)
SC (Birth)	0.00803 (0.265)
SD (Birth)	-0.986 (0.543)
TN (Birth)	-0.946 (0.287)
TX (Birth)	-0.0774 (0.301)
UT (Birth)	-0.519 (0.563)
VA (Birth)	-0.132 (0.301)
WA (Birth)	0.136 (0.404)
WV (Birth)	-0.897 (0.328)
WI (Birth)	-0.228 (0.335)

Table 21: Rates of Return on 10-Year T-Bills Versus SBP, 1968-1994						
Year	Annualized return on 10-Year T-Bills (10YTB)	Inflation Rate	10YTB Interest Rate Minus Inflation Divided by 100	Real Value of $1 10YTB Purchased in 1968	Real Cumulative Value of $1 Spent on SBP in 1968	Real Cumulative Value of $1 Spent on New SBP Participants in 1968
1968	5.65	4.2	0.015	1.02	0.0	0.0
1969	6.67	5.5	0.012	1.03	0.0	0.0
1970	7.35	5.7	0.017	1.04	0.0	0.0
1971	6.16	4.4	0.018	1.06	0.0	0.0
1972	6.21	3.2	0.030	1.08	0.0	0.0
1973	6.84	6.2	0.006	1.11	0.0	0.0
1974	7.56	11	-0.034	1.12	0.0	0.0
1975	7.99	9.1	-0.011	1.08	0.0	0.0
1976	7.61	5.8	0.018	1.07	0.0	0.0
1977	7.42	6.5	0.009	1.09	0.0	0.0
1978	8.41	7.6	0.008	1.09	0.0	0.0
1979	9.44	11.3	-0.019	1.10	0.0	0.0
1980	11.46	15.5	-0.040	1.08	1.40	0.30
1981	13.91	10.3	0.036	1.04	2.70	0.59
1982	13.00	6.2	0.068	1.08	4.20	0.91
1983	11.10	3.2	0.079	1.15	5.90	1.27
1984	12.44	4.3	0.081	1.24	7.80	1.67
1985	10.62	3.6	0.070	1.34	9.80	2.11
1986	7.68	1.9	0.058	1.44	11.90	2.55
1987	8.39	3.6	0.048	1.52	14.00	3.00
1988	8.85	4.1	0.048	1.59	16.10	3.45
1989	8.49	4.8	0.037	1.67	18.30	3.91
1990	8.55	5.4	0.032	1.73	20.30	4.36
1991	7.86	4.2	0.037	1.78	22.40	4.80
1992	7.01	3.0	0.040	1.85	24.60	5.27
1993	5.87	3.0	0.029	1.92	27.00	5.78
1994	7.09	2.6	0.045	1.98	29.20	6.25

Glossary of Variables for Empirical Models: Model 1

$BPE_{s,t}$ – Total SBP breakfasts served in the state that year divided by total enrollment in SBP schools. This variable is defined mathematically as $BKTOT_{s,t}$ / $SBPENROLL_{s,t}$.

$BKTOT_{s,t}$ – Total SBP breakfasts served in the state that year

$APE_{s,t}$ – 180[a] times Average Daily Attendance (ADA) in SBP schools in the state that year divided by Enrollment in SBP schools in the state that year. This variable is defined mathematically as $180*SBPADA_{s,t}$ / $SBPENROLL_{s,t}$

$ENROLL_{s,t}$ – Total enrollment in the public schools K-12 in the state that year. This includes SBP schools and non-SBP schools. Because some private schools offer the SBP, it is conceivable that $SBPENROLL_{s,t}$ would exceed $ENROLL_{s,t}$. Nevertheless, $ENROLL_{s,t}$ is always greater than or equal to $SBPENROLL_{s,t}$ in our data set.

$LBPE_{s,t}$ – Natural log of the quotient of total SBP breakfasts divided by enrollment in SBP schools. This variable is the natural log of $BPE_{s,t}$, and is equal to the difference in the natural logs of $BKTOT_{s,t}$ and $SBPENROLL_{s,t}$.

$LAPE_{s,t}$ – Natural log of the quotient of 180*ADA in SBP schools and Enrollment in SBP schools. This variab1le is the natural log of $APE_{s,t}$ and is equal to Log(180) + Log($SBPADA_{s,t}$) – Log($SBPENROLL_{s,t}$).

$LBMP_{s,t}$ – Difference of the natural logs of $BPE_{s,t}$ and $PPE_{s,t}$. When we regress $LAPE_{s,t}$ on both this variable and $LBNP_{s,t}$, the coefficient for $LBMP_{s,t}$ represents the effect of increasing the number of breakfasts served while holding the number of participants constant. Mathematically, this variable represents Log($BKTOT_{s,t}$) – Log(180) – Log($PARTICIP_{s,t}$).

$LBNP_{s,t}$ – Sum of the natural logs of $BPE_{s,t}$ and $PPE_{s,t}$. When we regress $LAPE_{s,t}$ on both this variable and $LBMP_{s,t}$, the coefficient for $LBNP_{s,t}$ represents the effect of increasing the number of participants served while holding the number of breakfasts per participant constant. Mathematically, this variable represents Log($BKTOT_{s,t}$) + Log(180) + Log($PARTICIP_{s,t}$) – 2*Log($SBPENROLL_{s,t}$).

$LPPE_{s,t}$ – The natural log of 180 times participants, or "participant days," divided by enrollment in SBP schools. This number will always be greater than or equal to $LBPE_{s,t}$. Mathematically, we represent this variable as Log(180) + Log($PARTICIP_{s,t}$) – Log($SBPENROLL_{s,t}$).

$LTPENR_{s,t}$ – The natural log of the state teacher-pupil ratio. Mathematically represented as Log($TEACHERS_{s,t}$) – Log($ENROLL_{s,t}$).

[a] The length of a school year varies by school. We select 180 as a rough estimate of the average number of school days per year.

$PARTICIP_{s,t}$ – The number of SBP participants in the state that year. This variable counts students in the state who ate anywhere between 1 and 180 breakfasts that year. This variable will always be less than or equal to $SBPENROLL_{s,t}$. Moreover, 180 times this variable will always be greater than or equal to $BKTOT_{s,t}$.

$PPE_{s,t}$ – 180 times participants, or "participant days," divided by enrollment. This variable counts the number of times when a participant could have eaten breakfast. This variable will always be greater than or equal to $BPE_{s,t}$. Mathematically represented as $180 * PARTICIP_{s,t} / SBPENROLL_{s,t}$.

$SBPADA_{s,t}$ – Average Daily Attendance (ADA) in SBP schools. This variable does not represent ADA over the year. Instead, this variable represents ADA in SBP schools in the state-year observation over the peak month nationally.

$SBPENROLL_{s,t}$ – Total enrollment in SBP schools in the state that year.

$State_{s,d}$ – State dummy. Returns a "1" for an observation in state *s* and a "0" otherwise.

$TEACHERS_{s,t}$ – Total number of teachers in public schools K-12 in the state that year. Part-time teachers are added as fractions of full-time teachers.

$TPENR_{s,t}$ – Teachers divided by enrollment in public schools K-12 in the state that year. We use this variable to proxy for the teacher-pupil ratio in SBP schools in the state that year. As noted in the write-up, the coefficient for $TPENR_{s,t}$ underestimates the effect of changing the teacher-pupil ratio in SBP schools in a given state and year. Mathematically represented as $TEACHERS_{s,t} / ENROLL_{s,t}$.

$Year_{t,d}$ – Year dummy. Returns a "1" for an observation in year *t* and a "0" otherwise.

Glossary of Variables for Empirical Models: Model 2

ADAKT2$_i$ – The sum of Average Daily Attendance (ADA) in public schools in the respondents' state of birth for the years in which the respondent was 5, 6, and 7 years old. We expect that the respondent entered kindergarten at age 5. Hence, these years represent the years when the respondent attended kindergarten, 1st grade, and 2nd grade. Henceforth, we will refer to this period as the years from K-2.

AGE_XX$_i$ – Cohort dummy. Returns a "1" if the respondent was XX years old in 1979 and a "0" otherwise. Our restricted sample includes respondents who were aged 14-17 in 1979.

BLACK$_i$ – Race dummy. Returns a "1" if the respondent is black and a "0" otherwise.

BOTHPARENTS$_i$ – Nuclear family dummy. Returns a "1" if the respondent lived with both parents from birth until age 18 and a "0" otherwise. The original NLSY79 did not include this question. Respondents answered this question in a follow-up survey in 1988.

FBPA$_i$ – The sum of free breakfasts in respondent i's state over the years from K-2 divided by the sum of ADA in respondent i's state over the years from K-2. Mathematically represented as $FREEBK_i$ / $ADAKT2_i$.

BPM$_i$ – $FBPA_i$ divided by its own mean.

BMP$_i$ – The difference of $FBPA_i$ divided by its mean and PPA_i divided by its mean. BMP_i and BNP_i serve the same purpose as do $LBMP_{s,t}$ and $LBNP_{s,t}$ in Model 1. When we regress HGC_i on BNP_i and BMP_i simultaneously, we obtain coefficients υ_1 and υ_2, respectively. υ_2 represents the effect on HGC_i of changing the relative sizes of $FBPA_i$ and PPA_i. The same applies when we regress $HSDUM_i$. Mathematically, we define BMP_i as $FBPA_i$ / Mean($FBPA_i$) – PPA_i / Mean(PPA_i).

BNP$_i$ – The sum of $FBPA_i$ divided by its mean and PPA_i divided by its mean. When we regress HGC_i on BNP_i and BMP_i simultaneously, we obtain coefficients υ_1 and υ_2, respectively. υ_1 represents the combined effect on HGC_i of changing $FBPA_i$ and PPA_i simultaneously. At the mean values for $FBPA_i$ and PPA_i, υ_1 represents the combined effect on HGC_i of changing $FBPA_i$ and PPA_i simultaneously in equal proportions. The same applies when we regress $HSDUM_i$. Mathematically, we define BNP_i as $FBPA_i$ / Mean($FBPA_i$) + PPA_i / Mean(PPA_i).

BState$_{i,s}$ – Birth-state dummy. Returns a "1" if the respondent was born in state s and a "0" otherwise. Our sample only includes those respondents who lived in their respective birth states at age 14.

Cohort$_{i,t}$ – Vector of cohort dummies AGE_14_i through AGE_17_i.

CPI_i – Consumer Price Index for *$WYear_i$*. The average price level from 1992 to 1994 is scaled to equal 100.

$CTMEDINC_i$ – Median family income in respondent *i*'s county of birth in 1966. Note that respondent *i*'s county of birth is not necessarily respondent i's county of residence at age 14.

EMP_i – Returns a one if the respondent was employed in the survey year and a zero otherwise. Survey year determined by most recent response to *$WAGE_i$*.

$FREEBK_i$ – The sum of free SBP breakfasts served in respondent *i*'s state for the years K-2. SBP schools provide free breakfasts to all interested students living at 130% poverty or below.

$FSIZE_i$ – The size of respondent *i*'s family in 1979. Does not exceed 16.

HGC_i – The highest grade (0 –20) that respondent *i* completed. Every follow-up survey in the NLSY79 included this question. We take each respondent's most recent answer to this question.

$HGCMOM_i$ – The highest grade (0-20) that respondent *i*'s mother completed. This variable typically exercises a significant effect on a given student's educational attainment. Father's education level does not typically exercise a significant effect on a student's attainment. Moreover, many of our respondent's did not know their father's education level.

$HISPANIC_i$ – Race dummy. Returns a "1" if the respondent is Hispanic and a "0" otherwise.

$INCOME78_i$ – Respondent *i*'s family income in 1978. Out sample only includes respondents who lived with their parents in 1979. Hence, we assume that family income in 1978 includes parents' income.

IPF_i – Respondent's family income in 1978 divided by family size in 1979. *$INCOME78_i$* tells us more about respondent *i*'s parents, while IPF tells us more about the resources available to the respondent as a child.

$MALE_i$ – Sex dummy. Returns a "1" if the respondent is male and a "0" otherwise.

$MOMAGE_i$ – Age of respondent *i*'s mother in 1987. Less than half of the respondents answered this question. The original NLSY79 survey did not include this question. Respondents answered this question in a follow-up survey in 1987.

$PART_i$ – The sum of the number of participants in the SBP in respondent *i*'s state for the years K-2.

PPA_i – The sum over the years K-2 of the number of SBP participants in respondent *i*'s state divided by the sum over the years K-2 of ADA in respondent *i*'s state. Mathematically represented as $PART_i / ADAKT2_i$.

PPM_i – PPA_i divided by its own mean.

$RESPAGE_i$ – Respondent *i*'s age when he or she most recently reported highest grade completed. This variable ranges from 18 to 36. Our sample omits every respondent who did not report highest grade completed after age 17.

$RESPONSEAGE\_18_i$ – Age 18 dummy. Returns a "1" if the respondent was 18 years old when he or she reported highest grade completed and "0" otherwise.

$RState_{i,s}$ – State-of-residence-dummy. Returns a "1" if respondent *i* lived in state *s* in $WYear_i$ and a "0" otherwise.

$SBPADAKT2_i$ – The sum over the years K-2 of ADA in SBP schools in respondent *i*'s state.

$WAGE_i$ – Respondent *i*'s most recent answer to the question: "What is the wage rate of your current or most recent job?"

$WREMP_i$ – The number of employed persons in respondent *i*'s race and age-group in respondent *i*'s state of residence in $WYear_i$.

$WREMP_i$ – Population in respondent *i*'s race and age-group in respondent *i*'s state of residence in $WYear_i$.

$WYear_i$ – The year of respondent *i*'s most recent wage response.

$WYear_{i,t}$ – Wage year-dummies. Returns a "1" if $WYear_i = t$ and a "0" otherwise.

BIBLIOGRAPHY

103rd Congress, House of Representatives Committee on Education and Labor. Subcommittee on Elementary, Secondary, and Vocational Education. *Child Nutrition Programs: Issues for the 103rd Congress*. US Government Printing Office. Washington, DC. January, 1994.

Akin, John S. and others. "The Demand for School Lunches: An Analysis of Individual Participation in the School Lunch Program." *The Journal of Human Resources*. Volume 17. Number 2. 1983a. Pages 213-220.

Akin, John S. and others. "The Impact of Federal Income Transfer Programs on the Nutrient Intake of Elderly Individuals." *The Journal of Human Resources*. Volume 20. Number 3. Summer 1985. Pages 383-404.

Akin, John S. and others. "The School Lunch Program and Nutrient Intake: A Switching Regression Analysis." *American Journal of Agricultural Economics*. August 1983b. Pages 477-485.

Akin, John S. and others. "The School Lunch Program and Nutrient Intake: A Switching Regression Analysis: Reply" *American Journal of Agricultural Economics*. August 1983c. Pages 513-514.

Angrist, Joshua. "New Evidence on Classroom Computers and Pupil Learning." Lecture. Oberlin College. 22 March, 2000.

Angrist, Joshua and William N. Evans. "Schooling and Labor-Market Consequences of the 1970 State Abortion Reforms." Forthcoming, *Research in Labor Economics*. http://www.bsos.umd.edu/econ/evans/wpapers/teenrev7.pdf

Angrist, Joshua and Alan B. Krueger. "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics*. Volume 106. Number 4. November 1991. Pages 979-1014.

Ashenfelter, Orley, and Alan Krueger. "Estimates of the Economic Return to Schooling From a New Sample of Twins." NBER Working Paper No. 4143. August 1992.

Ashenfelter, Orley, and David J. Zimmerman. "Estimates of the Returns to Schooling From Sibling Data: Fathers, Sons, and Brothers." NBER Working Paper No. 4491. October, 1993.

Ashenfelter, Orley, and Cecilia Rouse. "Income, Schooling, and Ability: Evidence From a New Sample of Identical Twins." *The Quarterly Journal of Economics*. Volume 113. Issue 1. February, 1998.

Associated Press. "Government Plans Free-Breakfast Program to Help Students Learn." *The New York Times*. A9. 15 January 2000.

Begale, Mary. "Minnesota Takes 'Fast Break to Learning.'" *Symposium on Breakfast and Learning in Children*. http://www.usda.gov/cnpp/Seminars/Behavior/breakfast.pdf

Betts, Julian R. "Does School Quality Matter? Evidence From the National Longitudinal Survey of Youth." *The Review of Economics and Statistics*. 1995. Pages 231-247.

Bro, Robert T., and others. "The Effects of an In-Class Breakfast Program on Attendance and On-Task Behavior of High School Students." *Child and Family Behavior Therapy*. Volume 16. Number 3. Pp. 1-8. 1994.

Burghardt, John A. and others. "Meals Offered in the National School Lunch Program and the School Breakfast Program." *American Journal of Clinical Nutrition*. Volume 61 (Supplement). 1995a. Pages 187(S)-198(S).

Burghardt, John A. "School Nutrition Dietary Assessment Study: Overview of the Study Design." *American Journal of Clinical Nutrition*. Volume 61 (Supplement). 1995b. Pages 182(S)-186(S).

Card, David, and Krueger, Alan B. "School Resources and Student Outcomes." *The Annals of the American Academy of Political and Social Science*. Volume 559. September, 1998. Pages 39-53.

Card, David, and Krueger, Alan. "School Resources and Student Outcomes: An Overview of the Literature and New Evidence from North and South Carolina." *Journal of Economic Perspectives*. Volume 10. Number 4. Fall 1996. Pages 31-50.

Card, David, and Krueger, Alan B. "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." *Journal of Political Economy*. Volume 100. Number 1. 1992a. Pages 1-40.

Card, David, and Krueger, Alan B. "School Quality and Black-White Relative Earnings: A Direct Assessment." *The Quarterly Journal of Economics*. Volume 107. Number 1. February, 1992b. Pages 151-200.

Cromer, Barbara A., and others. "The School Breakfast Program and Cognition in Adolescents." *Journal of Developmental and Behavioral Pediatrics*. Volume 11. Pp. 295-300. 1990.

Dee, Thomas and William N. Evans. "Teen Drinking and Educational Attainment: Evidence from Two-Sample Instrumental Variables (TSIV) Estimates." NBER Working Paper No. 6082. July 1997.

Devaney, Barbara, and Fraker, Thomas. "The Dietary Impacts of the School Breakfast Program." *The American Journal of Agricultural Economics*. Pp. 932-948. November 1989.

Devaney, Barbara L. and others. "Dietary Intakes of Students." *American Journal of Clinical Nutrition*. Volume 61 (Supplement). 1995. Pages 205(S)-212(S).

Devaney, Barbara, and Stuart, Elizabeth. *Eating Breakfast: Effects of the School Program*. Mathematica Policy Research, Inc. August 1998.

Dickie, N.H., and A.E. Bender. "Breakfast and Performance in Schoolchildren." *British Journal of Nutrition*. Volume 48. Pp. 483-496. 1982.

Food Research Action Council. "School Breakfast Participation Nearly Doubles in 10 Years." http://www.frac.org/html/news/103097.html

Fraker, Thomas, and Robert Moffitt. "The Effect of Food Stamps on Labor Supply: A Bivariate Selection Model." *Journal of Public Economics*. Volume 35. Number 1. February 1988. Pages 25-56.

Gleason, Philip M. "Participation in the National School Lunch Program and the School Breakfast Program." *American Journal of Clinical Nutrition*. Volume 61 (Supplement). 1995. Pages 213(S)-220(S).

Grantham-McGregor, Sally M. and others. "Evaluation of School Feeding Programs: Some Jamaican Examples." *American Journal of Clinical Nutrition*. 1998. Volume 67(Supplement). Pages 785(S)-789(S).

Hanes, S. and others.. "The National Evaluation of School Nutrition Programs: Program Impact on Dietary Intake." *The American Journal of Clinical Nutrition*. Volume 40. August 1984. Pages 390-413.

Hanushek, Eric A. and Lori L. Taylor. "Alternative Assessments of the Performance of Schools: Measurement of State Variations in Achievement." *The Journal of Human Resources*. Volume 25. Number 2. 1989. Pages 179-201.

Hanushek, Eric A. "The Economics of Schooling: Production and Efficiency in Public Schools." *Journal of Economic Literature*. Volume 24. September 1986. Pages 1141-1177.

Haveman, Robert and Barbara Wolfe. "The Determinants of Children's Attainments: A Review of Methods and Findings." *Journal of Economic Literature*. Volume 33. December 1995. Pages 1829-1878.

Heckman, James L. "Lessons From the Bell Curve." *Journal of Political Economy*. Volume 103. Number 5. 1995. Pages 1091-1120.

Hihn, Jairus M., and Lane, Sylvia. "The School Lunch Program and Nutrient Intake: A Switching Regression Analysis: Comment." *American Journal of Agricultural Economics*. August 1983. Pages 511-512.

Jacoby, Enrique J. and others. "When Science and Politics Listen to Each Other: Good Prospects From a New School Breakfast Program in Peru." *American Journal of Clinical Nutrition*. 1998. Volume 67(Supplement). Pages 795(S)-797(S).

Kane, John, and Lawrence M. Spizman. "Race, Financial Aid Awards, and College Attendance: Parents and Geography Matter." *American Journal of Economics and Sociology*. Volume 53. Number 1. January, 1994.

Kane, Thomas J. "College Entry by Blacks Since 1970: The Role of College Costs, Family Background, and the Returns to Education." *Journal of Political Economy*. Volume 102. Number 5. Pages 878-911. October 1994.

Krueger, Alan B. and Whitmore, Diane M. "The Effect of Attending a Small Class in the Early Grades on College-Test Taking and Middle School Test Results: Evidence From Project STAR." *Working Paper #427*. Princeton University Industrial Relations Section. October 1999.

Long, Sharon K. "Do the School Nutrition Programs Supplement Household Food Expenditures?" *The Journal of Human Resources*. Volume 26. Number 4. December 1990.

Lopez, I., and others. "Breakfast Omission and Cognitive Performance of Normal, Wasted, and Stunted Schoolchildren." *European Journal of Clinical Nutrition*. Volume 47. Pages 533-542. Macmillan Press, Ltd. 1993.

Maryland State Department of Education. *MSDE Bulletin*. Volume 9. Number 13. July 30, 1998. http://www.msde.state.md.us/msde%20bulletin/1998/bulletin9%2D13.html

McDonald, Maurice. "The Role of Multiple Benefits in Maintaining the Social Safety Net: The Case of Food Stamps." *The Journal of Human Resources*. volume 20, Number 3, Summer 1985. Pages 421-436..

McGreevey, Susan. "School Breakfast Participation Leads to Academic, Psychosocial Improvements." Massachusetts General Hospital, Harvard Medical School. *KidSource Online*. http://www.kidource.com/kidsource/content4/school.breakfast.part.html

Meyers, Alan F., and others. "School Breakfast Program and School Performance." *American Journal of Diseases of Children*. Volume 143. Pp. 1234-9. October 1989.

Morgan, Karen J., and others. "The Role of Breakfast in Nutrient Intake of 5- to 12-Year-Old Children." *The American Journal of Clinical Nutrition*. Volume 34. Pp. 1418-1427. USA. July 1981.

Murnane and others. "The Growing Importance of Cognitive Skills in Wage Determination." *The Review of Economics and Statistics*. 1995. Pages 251-266.

Murphy, J. Michael and others. "Relationship Between Hunger and Psychosocial Functioning in Low-Income American Children." *Journal of the American Academy of Child and Adolescent Psychiatry*. Volume 32. Number 2. February 1998. Pages 163-170.

Nayga, Rodolfo M., Jr. "Impact of Sociodemographic Factors on Perceived Importance of Nutrition in Food Shopping." *Journal of Consumer Affairs*. Volume 31. Number 1. Summer 1997. Pages 1-9.

Nicklas, Theresa A., and others. "Breakfast Consumption Affects Adequacy of Total Daily Intake in Children." *Journal of the American Dietetic Association*. Volume 93. Number 8. Pp. 886-891. August 1993a.

Nicklas, Theresa A., and others. "Nutrient Contribution of the Breakfast Meal Classified by Source in 10-Year-Old Children: Home Versus School." *School Food Service Research Review*. Volume 17. Number 2. Pp. 125-132. 1993b.

Nicklas, Theresa A., and others. "Nutrient Contribution of Breakfast, Secular Trends, and the Role of Ready-to-eat Cereals: A Review of Data From the Bogalusa Heart Study." *American Journal of Clinical Nutrition*. 1998. Volume 67(Supplement). Pages 757(S)-763(S).

Oberlin City School District. *Application for Free and Reduced Price Meals*. Includes a letter by Gott, Dianne L., Cafeteria Supervisor.
Oberlin School Board Administrative Offices

153 North Main Street
Oberlin, OH 44074
Tel: (440) 774-1458, Fax: (440) 774-4492

Peiffer, Ron, and Kim Kerry. "Local School Systems and Maryland State Department of Education Team Up to Pilot Classroom Breakfast Program." Maryland State Department of Education. *MSDE Press Release.* September 24, 1998.
http://msde.state.md.us/pressreleases/1998/september/1998%2D0924.html
(410) 767-0473

Pollitt, Ernesto, and Rebecca Matthews. "Breakfast and Cognition: An Integrative Summary." *American Journal of Clinical Nutrition.* 1998. Volume 67(Supplement). Pages 804(S)-813(S).

Pollitt, Ernesto. "Does Breakfast Make a Difference in School?" *Journal of the American Dietetic Association.* Volume 95. Number 10. Pp. 1134-1139. 1995.

Pollitt, Ernesto, and others. "Fasting and Cognition in Well- and Undernourished Schoolchildren: A Review of Three Experimental Studies." *American Journal of Clinical Nutrition.* 1998. Volume 67(Supplement). Pages 779(S)-784(S).

Pollitt, Ernesto, and others. "School Breakfast and Cognition Among Nutritionally At-Risk Children in the Peruvian Andes." *Nutrition Reviews.* Volume 54. Number 4. Pp. S-22-26. April 1996.

Powell, Christine, and others. "An Evaluation of Giving the Jamaican Government School Meal to a Class of Children." *Human Nutrition: Clinical Nutrition.* Volume 37C. Pp. 381-388. 1983.

Radzikowski, Jack, and Stephen Gale. "Requirement for the National Evaluation of School Nutrition Programs." *The American Journal of Clinical Nutrition.* Volume 40. August, 1984. Pages 365-367.

Sacks, Michelle. "New Harvard Research Shows School Breakfast Program May Improve Children's Behavior and Performance." Kellogg Company. *KidSource Online.* March 2, 1998. http://www.kidsource.com/kidsource/content4/breakfast.html

Siega-Riz, Anna and others. "Trends in Breakfast Consumption for Children in the United States from 1965 to 1991." *American Journal of Clinical Nutrition.* 1998. Volume 67(Supplement). Pages 748(S)-756(S).

Simeon, Donald T., and Sally Grantham-McGregor. "Effects of Missing Breakfast on the Cognitive Functions of School Children of Differing Nutritional Status." *American Journal of Clinical Nutrition.* Volume 49. Pp. 646-653. USA. 1989.

St. Pierre, Robert G., and Michael J. Puma. "Controlling Federal Expenditures in the National School Lunch Program: The Relationship Between Changes in Household Eligibility and Federal Policy." *Journal of Policy Analysis and Management.* Volume 11. Number 1. 1992. Pages 42-57.

Stanley, Linda R. and Tschirhart, John. "Hedonic Prices for a Nondurable Good: The Case of Breakfast Cereals." *Review of Economics and Statistics.* Volume 73, Number 3—August 1991—Pages 537-546.

Tuttle, W.W., and others. "Effect on School Boys of Omitting Breakfast: Physiologic Responses, Attitudes, and Scholastic Attainments." *Journal of the American Dietetic Association*. Volume 30. Pp. 674-677. 1954.

USDA Food and Nutrition Service. Child Nutrition Homepage. http://www.fns.usda.gov/cnd/

USDA Food and Nutrition Service. FACTS - Healthy Eating Helps You Make the Grade! http://www.fns.usda.gov/cnd/Breakfast/SchoolBfastCampaign/Facts.htm.

USDA Food and Nutrition Service. *Factors Influencing School and Student Participation in the School Breakfast Program.* 1978.

USDA Food and Nutrition Service. *Healthy School Meals. . .Healthy Kids!*

USDA Food and Nutrition Service. *Start the Day Off Right With School Breakfast!* Pamphlet.

US General Accounting Office. *Major Factors Inhibit Expansion of The School Breakfast Program.* 1977.

Data Sources:

Bureau of Labor Statistics. *Local Area Unemployment Statistics.* 1981-1993.

Center for Human Resources Research. *National Longitudinal Survey of Youth in 1979: GeoCode.*

Council of Economic Advisors. *Economic Report of the President.* 1995. US Government Printing Office.

National Center for Education Statistics. *Digest of Education Statistics.* 1962-1998.

National Center for Education Statistics. "Table 16a: Number of public and private schools, number of students, and average number of students per school in preprimary through secondary schools, by level and country: Various years." *Education in States and Nations: 1991.* http://nces.ed.gov/pubs/esn/n15ca.html

US Census Bureau. "Land Area, Population, and Density for States and Counties: 1990." http://www.census.gov:80/population/censusdata/90den_stco.txt

US Census Bureau. "Median Income for 4-Person Families, by State." http://www.census.gov/hhes/income/4person.html

USDA Food and Nutrition Service. "SY99-SY00 Reimbursement Rates." http://www.fns.usda.gov/cnd/Lunch/BenefitsandRates/ReimburseRates99.htm

USDA Food and Nutrition Service. *School Breakfast Program: State Agency and Regional Office: Program Participation, Number of Breakfasts, and Federal Reimbursement.* FY1967-1998.