Essays in the Economics of Education

by

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B.A. Economics and Mathematics
University of California, Berkeley, 2002

Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Economics

at the

Massachusetts Institute of Technology

September 2007

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Abstract

This thesis consists of three essays on the economics of education.

The first chapter estimates the effects of participating in the National School Lunch Program in the middle of the 20th century on educational attainment and adult health. My instrumental variables strategy exploits a change of the formula used by the federal government to allocate funding to the states that was phased in beginning in 1963. Identification is achieved by the fact that different birth cohorts were exposed to different degrees to the original formula and the new formula, along with the fact that the change of the formula affected states differentially by per capita income. Participation in the program as a child appears to have few long-run effects on health, but the effects on educational attainment are sizable.

The second chapter studies the issue of racial diversity in higher education. I estimate the effects of college racial diversity on post-college earnings, civic behavior, and satisfaction with the college attended. I use the Beginning Postsecondary Students survey, which allows me to control for exposure to racial diversity prior to college. Moreover, I use two techniques from Altonji, Elder, and Taber (2005) to address the issue of selection on unobservables. Single-equation estimates suggest a positive effect of diversity on voting behavior and on satisfaction with the college attended, but I do not find an effect on other outcomes. Moreover, the estimates are very sensitive to the assumptions made about selection on unobservables.

The third chapter studies university affirmative action bans. I use information on the timing of bans along with data from the Current Population Survey (CPS) and the American Community Survey (ACS) to estimate the effects of such bans on college enrollment and educational attainment. I use a triple difference strategy that uses whites as a comparison group for underrepresented minorities and that exploits variation in the bans over states and across time. I find no adverse impact of bans on overall minority college attendance rates and educational attainment relative to whites, and I find no effect of the bans on minority enrollment in public colleges or four-year colleges.

Acknowledgements

I thank my advisors, Joshua Angrist and David Autor, for their guidance. They have been very accessible and extremely generous with their time. I could always count on them to make suggestions that would improve the quality of my research and my writing.

I also benefited from interactions I had with numerous other faculty members. I would especially like to thank Amy Finkelstein for her assistance with the first chapter of this thesis and Whitney Newey for his assistance with the second chapter. I also thank Dora Costa, Jonathan Gruber, and James Poterba for help with the first chapter and Victor Chernozhukov and Jerry Hausman for help with the second chapter.

I am indebted to my classmate Peter Schnabl for the detailed comments and suggestions he provided on all three chapters.

For Chapter 1, I would also like to thank too many reference librarians, archivists, administrators, and individuals knowledgeable about the National School Lunch Program to list; but I would especially like to mention John Collins at the Harvard Government Documents Library and Wayne Olson at the National Agricultural Library. For Chapter 2, I thank Todd Elder and Christopher Taber for providing assistance with their estimation procedures. For Chapter 3, I thank the numerous individuals with whom I discussed the affirmative action policies of various universities.

On a personal level, I thank Edward Cho and Timothy Watts for their friendship over the past five years. I thank my officemates James Berry, Neil Bhutta, Konrad Menzel, and Christopher Smith for their help with my research and for providing a lighthearted, yet productive, work environment. I also thank the numerous individuals who I regularly played basketball with in my earlier years of graduate school, as well as those I listened to or played music with in my later years of graduate school. Last but not least, I thank my parents, Craig and Marilyn Hinrichs, for their support.

Biographical Note

Peter Laroy Hinrichs was born on June 18, 1980 in La Crosse, Wisconsin. He was raised in Hopkins, Minnesota and graduated from Hopkins High School in 1998. He received a B.A. in Economics and Mathematics from the University of California, Berkeley in 2002. He graduated from UC Berkeley with Highest Honors in Economics and High Distinction in General Scholarship, and he was awarded the Economics Departmental Citation and Earl Rolph Prize. After completing his Ph.D. studies at the Massachusetts Institute of Technology, he begins in Fall 2007 as Assistant Professor of Public Policy at Georgetown University.

Contents

Chapter 1

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

1.1 Introduction

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

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

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

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

Estimating the effects of the NSLP is of interest in its own right as an evaluation of a major government-sponsored nutrition program.[1] Uncovering the effects of the NSLP at its inception may also be relevant for developing countries that have recently adopted or are considering adopting a similar large-scale child nutrition program.[2] Moreover, the research could provide insight for the issue of the effects of health investments as a child on health outcomes as an adult and the issue of trends in health outcomes over time. Thus, this chapter is related to other recent research that has used quasi-experimental methods to study historical health issues, including Almond (2006) on influenza, Bleakley (2007) on hookworm eradication, Bleakley (2006) on malaria eradication, and Ludwig and Miller (2007) on Head Start.

The remainder of this chapter is structured as follows. Section 1.2 discusses the NSLP in more detail, Section 1.3 reviews related literature, Section 1.4 discusses the data, Section 1.5 discusses the identification strategy, Section 1.6 gives the empirical results, and Section 1.7 concludes.

1.2 The National School Lunch Program[3]

The American school lunch has not always been the institution it is today. There were cities such as Boston and Philadelphia that operated their own school lunch

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[1] To give an indication of the size of the program in the time period under consideration, the federal government alone spent roughly $500 million (in 2005 dollars) on the NSLP in 1947 and roughly $1 billion (in 2005 dollars) in 1973.

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

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

programs, often with the help of volunteers or charitable organizations, as early as the late nineteenth century. But it was not until 1932 that the federal government began giving aid for school lunch programs. This aid began on a small scale and originated from New Deal agencies such as the Federal Emergency Relief Administration, the Reconstruction Finance Corporation, and the Civil Works Administration. Federal involvement expanded in 1935 with the creation of the Works Progress Administration and the National Youth Association, both of which operated programs that provided labor for school lunchrooms. In that same year, the Agricultural Adjustment Act was amended with Section 32, which instituted the donation of surplus farm commodities to school lunch programs. By 1943, the New Deal agencies had been dissolved and farm surpluses were not as large as they had previously been, but there was a desire to keep school lunch programs. Thus, federal cash assistance for school lunch programs was appropriated on a year-to-year basis from 1943 to 1946.

The NSLP was made permanent with the passage of the National School Lunch Act in 1946. Under Section 4 of the Act, cash was given from the federal government to the states according to a formula that depended on per capita income and population, and this cash was handed down by states to localities. Schools had the option of participating in the program.[4] If they chose to do so, they would receive cash and commodity aid in exchange for following program requirements, including requirements about the contents of the lunch.[5] A gradual change to a new funding formula began in the 1962-1963 school

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

[5] At the inception of the NSLP, there were three different categories of lunches (Type A, Type B, and Type C), and they had different requirements. The requirements for a Type A lunch were "1) One-half pint of whole milk (which meets the minimum butterfat and sanitation requirements of state and local laws) as a beverage. 2) Two ounces of fresh or processed meat, poultry, cooked or canned fish, or cheese; or one-half cup cooked dry peas, beans, or soybeans; or four tablespoons of peanut butter; or one egg. 3) Six ounces of

year and was fully in place for the 1965-1966 school year. This change forms the basis of my identification strategy. The formulas and the identification strategy are discussed in detail in Section 1.5.

1.3 School Nutrition Programs and Health: Prior Literature

Although this is the first research of which I am aware to estimate the long-run effects of the NSLP and to estimate the effects of the NSLP in the early years of the program, there is some work on the more recent effects of the NSLP and the related School Breakfast Program (SBP). Schanzenbach (2005) studies the effect of the NSLP on obesity. She shows that participants and non-participants enter school with similar rates of obesity but that the obesity rate is higher among participants than non-participants by the spring of first grade. In addition, a regression discontinuity design exploiting a discontinuity in eligibility for a reduced price lunch at an income of 185% of the poverty level gives similar results.[6] Bhattacharya, Currie, and Haider (2006) study the effects of the SBP, which was introduced as a small-scale pilot program in 1966 and made permanent in 1975, with a difference-in-differences strategy that compares outcomes between the school year and the summer for students in schools where the SBP is available and where it is not available. They find beneficial effects of the program on

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raw, cooked, or canned vegetables and/or fruit. 4) One portion of bread, muffins, or other hot bread made of whole-grain or enriched flour. 5) Two teaspoons of butter or fortified margarine." The Type B lunch had to meet requirements 1 and 4, as well as half the portions for the other requirements. The Type C lunch had to meet requirement 1. My data unfortunately do not distinguish between Type A, Type B, and Type C lunches; I return to this issue in the robustness checks.

[6] Also see Anderson and Butcher (2006) for an indirect case that the nutrition policies of schools have an effect on the body mass index of students. Another recent paper on school nutrition policy is Figlio and Winicki (2005), which shows that schools in Virginia were altering the nutritional content of school lunches around the time of high stakes tests and that this was apparently successful in raising test scores.

several outcomes, including the Healthy Eating Index score, the probability of having low serum levels of vitamin C, and the probability of having low fiber intake. [7]

A potential problem with studies of child nutrition programs using recent data is the risk of confounding the effects of different programs with one another. For example, the NSLP and the SBP have similar funding structures and the same income cutoffs for free lunch eligibility (130% of the poverty level) and reduced-price lunch eligibility (185% of the poverty level). In addition, the 130% figure is important for food stamp eligibility, and the 185% figure is important for WIC eligibility.[8] There are also a number of newer child nutrition programs, such as the Summer Food Service, whose effects may be confounded with those of the NSLP or the SBP. Studying a time period before these other programs existed should help isolate the effects of the NSLP. Another distinction is that I focus here on long-run effects.

1.4 Data

I use three data sets. The first contains annual information on NSLP funding, NSLP participation, per capita income, and population aged 5-17 by state for the years 1947-1973.[9] The second pools the five National Health Interview Surveys conducted between 1976 and 1980 (United States Department of Health and Human Services 1976-1980); this data set consists of information on health outcomes and demographic control variables. The third data set is the 5% sample of the 1980 Census (Ruggles et al. 2004).

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

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

[9] Much of the information in this dataset comes from tables showing the exact inputs and output of the NSLP funding formula. When data is unavailable in the funding tables, I use data from other sources or impute the data myself. Details and source citations are provided in the data appendix.

I merge the first data set with the second to estimate the effects of participation in the NSLP on health and the first with the third to estimate the effects of participation on educational attainment. In the remainder of this section, I discuss the three data sources in more detail; additional information about the funding and participation data can be found in the data appendix.

1.4.1 Funding and Participation

Figure 1 shows the national participation rate in the NSLP for each year between 1947 and 1973.[10] The participation rate divides the average number of lunches served in the national "peak month" (generally November or December) by the size of the population aged 5-17.[11] The trend over time is one of increasing participation. Figure 2 shows the amount of Section 4 "general assistance" NSLP funding per child at the national level between 1947 and 1973. Funding per child tends to fall at first but then rises later. Figure 3 is a scatterplot of state participation rates in 1947 and 1973. States with higher participation rates in 1947 also tend to have higher participation rates in 1973, and states with high participation rates tend to be poorer and in the South. Figure 4 is a scatterplot for the cohort born in 1944 of the averages over the 12 years the children are in school of the state participation rate and funding per child; I use the term "exposure" to refer to this average participation rate. This figure reveals that Louisiana

[10] I use the name of a calendar year to refer to the school year or fiscal year ending in that year. In the time period under consideration, the federal government's fiscal year began on July 1 of the previous calendar year and ended on June 30.

[11] Any student at a participating school is eligible to participate. Thus, the data capture full-price as well as free or reduced-price lunches. Uniform national standards for free or reduced-price lunch eligibility were not imposed until 1972 , although Section 11 of the original text of the National School Lunch Act states, "Meals shall be served without cost or at a reduced cost to children who are determined by local school authorities to be unable to pay the full cost of the lunch."

has especially high participation rates. I include state effects in my regressions, but I also drop observations from Louisiana from the sample as a robustness check.

1.4.2 National Health Interview Survey

The health outcome variables and the individual-level control variables used in estimating the effects of the NSLP on health outcomes come from the National Health Interview Survey (NHIS). My NHIS dataset is formed by pooling the five NHIS surveys between 1976 and 1980. I use individuals born between 1941 and 1956 in the continental 48 states, and I drop outliers in height or weight.[12] The individual-level data is matched to the participation and funding data using state of residence, as state of birth is unavailable in the NHIS. For an individual who is a years old in year y, I consider the individual to have been born in year y-a, the first year of school to be y-a+6, and the last year of school to be y-a+17. The top panel of Table 1 reports weighted means and standard deviations by gender and race of variables used to estimate the health models. A substantial percentage of individuals in the sample are underweight (1.4% of men and 8.0% of women), suffer from health limitations (9.4% of men and 7.8% of women), or describe their health as fair or poor (6.8% of men and 9.7% of women).

1.4.3 1980 Census

The data on educational attainment and the individual-level control variables in the education regressions come from the 5% sample of the 1980 Census. I again restrict the sample to individuals born in the continental 48 states between 1941 and 1956,

[12] The dropped observations are men who weighed less than 90 pounds or were less than 58 inches tall and women who weighed less than 80 pounds or were less than 53 inches tall.

excluding those living in group quarters. I match the Census data to the participation data using state of birth and age. The bottom panel of Table 1 reports summary statistics for the Census data.

1.5 Identification Strategy

1.5.1 Estimating Equations and Motivation for IV

I estimate equations of the form

$$y_{isct} = \beta * exposure_{sc} + x'_{isct}\gamma + \alpha_s + \alpha_c + \alpha_t + \varepsilon_{isct} \,. \quad (1)$$

Here y_{isct} is a health or educational outcome variable measured in year t for individual i from state s born in year c. The main righthand side variable is $exposure_{sc}$, the average participation rate over the time the individual was in school measured on a scale of 0-100. The participation rate is calculated for each state in each year by dividing the number of students participating by the size of the population aged 5-17 (and multiplying by 100).[13] The remaining variables in the models are a vector of control variables x_{isct} that contains individual-level data on race and state-level data on per capita income,[14] state dummies α_s, birth cohort dummies α_c, and year dummies α_t.[15] This model is consistent with the theoretical model of Grossman (1972) in which health investments have a cumulative effect on "health capital."

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

[14] Controlling for per capita income has little effect on the instrumental variables estimates but is done to reduce the bias of the least squares estimates.

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

There are several reasons why least squares estimates of equation 1 may be inconsistent. First, NSLP participation should be higher when school enrollment is higher. Thus, the models with educational attainment as the outcome variable may suffer from reverse causality. In the health models, education is an omitted variable and there is the possibility of confounding the effects of NSLP participation with those of education; controlling for education does not necessarily solve the problem, since education is potentially affected by participation.[16] Second, because participation in the NSLP is a choice variable, states that have higher participation rate at a point in time may differ from those with lower participation rates along unobservable dimensions that affect the outcomes.[17] Third, NSLP participation rates may be measured poorly.

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

There are at least three channels through which funding given to states for the NSLP could affect participation within the state. First, a state that receives a larger amount of funding for the NSLP may be able to reimburse schools within the state at a higher rate

[16] If someone enrolls in school in order to participate in the NSLP and enrollment has a direct effect on outcomes, I take that to be an (indirect) effect of the NSLP.
[17] Participation is a two-stage decision. First, a school must choose to participate in the program. Second, children at participating schools must choose whether to participate. Thus, the effects of the program in a least squares regression may be confounded with either unobserved individual-level characteristics or unobserved school-level characteristics that change differentially by state over time.

for lunches, which would tend to increase the number of participating schools. Second, if a state reimbursed schools at a higher rate, this may result in schools charging lower prices to children for lunches, which may increase participation among children in schools already participating in the NSLP. Third, apart from the reimbursement rate to schools, a state that has a large amount of money available under the NSLP may make greater efforts to convince schools to start lunch programs.[18]

1.5.2 The Funding Formulas

The main federal cash aid given to states for the NSLP in the time period under consideration was Section 4 "general assistance" funding. This aid was distributed according to a formula. The original formula was in place from 1947-1962, and a new formula was phased in beginning in 1963. In 1963, 75% of funding was distributed according to the old formula and 25% according to the new formula; in 1964, half of the aid was given according to the old formula and half according to the new; and in 1965, 25% was given according to the old formula and 75% used the new. The new formula was fully in place in 1966 and continuing through the end of the sample period.

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

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

[18] All these channels require there to be a "flypaper effect," whereby targeted aid given to a state 'sticks' to the purpose for which it is intended rather than being reallocated and spent in some other way.

where *population* is the size of the population aged 5-17 and *pci* refers to per capita income.[19] Using $totalfund_t$ to denote the amount of funding nationally in year t, the amount of funding going to state s in year t was then

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

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

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

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

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

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

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

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

[19] The index multiplied population in the numerator by the per capita income of the United States, but that factor cancels in the next step.

where *lunches* is the number of lunches served as part of the program.[20]

1.5.3 Defining the Instrument

The instrument is based on funding levels, but I make two modifications. First, instead of actual funding levels I use "constant characteristics" funding levels, which are funding levels that would be received if states had constant per capita incomes and populations over time. I make this modification because per capita income and population change over time and may have a direct effect on the outcomes; using "constant characteristics" funding levels ensures that the identifying variation comes about due to the formula change rather than from a change in the inputs that go into the formula. Second, I replace $lunches_{s,t-1}$ with $population_{s,t-1}$ for years when the new formula is used. This is done because funding depends on lagged participation under the new formula, and the instrument should be defined in a way such that the variable I am instrumenting for does not have a causal effect on the instrument. With these modifications in mind, the identifying variation in the IV strategy comes from the fact that (1) the formula change affected states differentially (and non-linearly) by per capita income and (2) different birth cohorts were exposed to the two formulas to different degrees.

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

[20] The law does not specify the exact lag structure of the formula's inputs, but it states that the most recent available data is to be used. With the caveat that I take the timing for missing years (1948-1954, 1969) to be the same as that for the nearest non-missing years, in practice the most recent available data income data had a lag of three years in the time period 1947-1961 and a lag of two years in the period 1962-1973. The population data had a lag of three years for every year it was used except for 1962, where the lag was two years. The data on the number of lunches served was from the previous year for every year it was used.

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

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

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

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

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

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

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

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

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

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

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

Here $f(t)$ equals 0 in 1947-1962, .25 in 1963, .5 in 1964, .75 in 1965, and 1 in 1966-1973. The final step in constructing the instrument is to combine constant characteristics funding amounts for the years an individual was in school; this captures the idea that the NSLP is a program individuals could be exposed to throughout their complete stay in elementary and secondary school. For someone born in year c and from state s, the instrument is

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

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

$$exposure_{sc} = \tilde{\beta} * z_{sc} + x'_{isct}\tilde{\gamma} + \tilde{\alpha}_s + \tilde{\alpha}_c + \tilde{\alpha}_t + \tilde{\varepsilon}_{isct}. \qquad (2)$$

Identification comes from the fact that different people were exposed to the two formulas to different degrees according to when they were born,[21] combined with the fact that the change in the formula affects states differentially. In particular, the new formula treats states with an above-average per capita income the same; but under the old formula, increases in income for an already-rich state result in lower funding for that state. Moreover, since the total amount of the funding "pie" is fixed within a given year, a change in the formula that benefits states with higher incomes will be to the detriment of states with lower incomes. Figures 6 and 7 illustrate these points graphically. Figure 6 plots the relationship between constant characteristics funding under the new formula and under the old formula for 1964, the year where half of the funding was appropriated under each formula. Figure 7 displays the difference between new and old constant characteristics funding by per capita income for 1964. Figure 7 reveals that the formula

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

change results in a differential effect on funding by per capita income and that this effect is nonlinear.

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

Table 2 shows that funding affects participation. Column 1 reports a simple bivariate regression of exposure on the instrument, and it shows that there is a positive correlation between the two. This relationship does not change very much when control variables are added to the model in column 2 but drops in column 3 when cohort and year dummies are included. The cohort dummies absorb changes in the total amount of funding at the national level from year to year, so the drop in the coefficient reflects the fact that both participation and funding are generally rising over time. The coefficient also falls when state dummies are included, as shown in column 4. But the positive relationship between funding and participation persists even after including individual-level control variables, cohort effects, year effects, and state effects. Subject to the caveat that the second stage outcome variables are missing for certain observations, column 4 is the first stage used in the IV regressions.

1.6 Results

1.6.1 Main Results for Health Outcome Variables

Height is a measure of long-term nutritional status and is determined primarily prior to reaching adulthood, making it a natural first outcome variable to consider.[22] Table 3 shows the results for height, with the results for men in the top panel and the results for women in the bottom panel. All tables report standard errors corrected for clustering at the state-by-cohort level, and all health regressions use NHIS weights.[23] The least squares estimates in columns 1-4 show a positive relationship between height and NSLP exposure for both men and women; this relationship is significant at the 1% level in the full least squares specification for men in column 4, but the corresponding estimate for women is not even significant at the 5% level. The least squares estimate for men in column 4 suggests that increasing exposure by ten percentage points is associated with an increase in height of .18 inches. This estimate is remarkably similar to the IV estimate in column 6, which suggests that this same increase in exposure results in an increase in height of about .16 inches. However, the IV estimate for men is insignificant due to the

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

[23] The weights within at NHIS dataset for a given year were normalized to sum to 1 before any observations were dropped.

large standard error. The IV estimate for women is also not significantly different from 0, although it is much larger in magnitude than the least squares estimate in column 4. But the general pattern to Table 3 is that I do not uncover a statistically significant impact of the NSLP on the average height. However, focusing on the average may conceal what is happening in the tails, and so Table 4 shows the effects of the NSLP on the cumulative distribution of height. In particular, the NSLP could have reduced the share of the population that is stunted without having a detectable effect on the average height. But this does not seem to be the case, as most of the estimated coefficients in Table 4 are insignificant and there is no clear pattern in the estimates.

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

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

where weight is measured in pounds and height is measured in inches. Whereas height is a measure of long-run nutritional status, BMI is a measure of shorter-run nutritional status. However, there are at least two channels through which school lunch exposure as a child could affect BMI as an adult: (1) the degree of exposure to school lunches as a child could alter eating habits later in life and (2) there could be a physiological effect that carries over from childhood to adulthood.[24] The results for BMI are presented in Table 5. Columns 1, 2, and 3 of both panels actually display a significant negative effect on BMI, but significance is lost in both cases when adding state dummies in column 4. The IV estimates in column 6 are larger in magnitude than the least squares estimates, but

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

they are insignificant and estimated rather imprecisely. So, on the whole, Table 5 does not reveal much of an effect of the NSLP on BMI. But as with the case of height, there could be an effect on extreme values of BMI without there being a statistically detectable effect at the mean. Moreover, whereas it is believed that larger height indicates better nutritional status (all else equal), the relationship between BMI and being in good health is non-monotonic in BMI. In particular, having either an extremely high or extremely low BMI is thought to be unhealthy. Thus, Table 6 shows the results of linear probability models of the effects of school lunch exposure on categorical measures of BMI. According to the Centers for Disease Control, someone is underweight if their BMI is less than 18.5, overweight if their BMI is above 25, and obese if their BMI is above 30. To the extent that the program fed an undernourished population, it may result in a lower probability of being underweight. But if the results of Schanzenbach (2005) held in this earlier time period, the program could increase obesity. However, turning to the results in Table 6, the coefficients are generally small in magnitude and statistically insignificant.

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

1.6.2 Main Results for Education

If time spent in school is more productive for individuals in good nutritional status, then the NSLP could raise the optimal level of education individuals choose. Moreover, the option of receiving a subsidized lunch if a child attends school may directly influence the school participation decision. Table 8 shows the effects of the NSLP on years of completed education using data from the 1980 Census. The least squares estimate in column 1 is significantly negative, which is consistent with the fact that people from poorer areas have both higher exposure and lower educational attainment even in the absence of the program. The estimate in column 4 shows that, after including control variables, there is a significant positive relationship between NSLP exposure and educational attainment. However, the least squares estimates likely suffer from reverse causality: since it is necessary that an individual be enrolled in school in order to receive a lunch, higher school enrollment is likely to result in higher NSLP exposure.

Column 6 displays the IV estimates. The IV point estimates are larger than the least squares estimates, although they are also imprecise.[25] The IV estimate for women suggests than increasing NSLP exposure by ten percentage points results in an average

[25] The IV point estimates for education are significantly different from 0 at the 1% level for both men and women, but the standard errors are large enough that a 95% confidence interval covers values that are more reasonable than the point estimates and does not cover 0.

increase in education of .365 years, and the IV estimate for men suggests that increasing NSLP exposure by ten percentage points increases average education by nearly a year. It is somewhat surprising that the IV estimates are larger than the least squares estimates, since the reverse causality problem that affects the least squares estimates should induce a spurious positive correlation between NSLP exposure and educational attainment. However, one potential explanation for why the IV estimates are larger than the least squares estimates is that the IV estimates are not attenuated by measurement error in NSLP participation that would affect the least squares estimates. This explanation is consistent with the fact that the IV estimates for health outcomes also generally have larger magnitudes than the LS estimates.

1.6.3 Additional Specifications

To investigate the possibility that the NSLP had a differential effect on disadvantaged groups, regressions reported in Table 9 add a righthand side variable for the interaction between NSLP exposure and the percentage of men rejected from service or placed in the limited service class during World War II.[26] The large number of men rejected from military service during World War II played a large role in the passage of the National School Lunch Act, and the results in Table 9 answer the question of whether the program had a larger effect in states where the rejection rate was higher. The equation to be estimated is

$$y_{isct} = \beta_1 * exposure_{sc} + \beta_2 * exposure_{sc} * rrate_s + x'_{isct}\gamma + \alpha_s + \alpha_c + \alpha_t + \varepsilon_{isct}. \quad (3)$$

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

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

To further explore the possibility that the effects of the NSLP were different for different groups, Table 10 shows estimates of the effects on subsamples. I separately estimate the effects of exposure to the NSLP for whites and blacks and for people from Northern states and Southern states.[27] A problem with interpreting the IV results for race arises because I do not have separate data on participation for blacks and whites. Thus, if funding had a different effect on participation for blacks than whites, the IV estimates confound the differential effect of school lunches on health for subpopulations with the differential effect of funding on participation. Even in this case, however, the reduced form shows the effect of increased NSLP funding on the health of blacks compared to whites. The first row of columns 3 and 4 shows that NSLP funding has a larger effect on educational attainment for whites than for blacks. This may suggest that states channeled their NSLP funding toward whites. However, the effects of NSLP funding on health outcomes appear more beneficial for blacks on some outcomes (e.g., height and underweight) and for whites on others (e.g., health limitations), although none of these

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

differences is statistically significant. There is also no clear pattern in the North/South health differences, but it is notable that the education effect is larger in the South. This provides additional evidence that the program may have been more effective in needier states.

In results not reported here, I find that dropping observations from Louisiana, an outlier in participation, does not change the results noticeably. I also perform a robustness check to determine whether any results are driven by changes in the composition of lunches among type A, type B, and type C by state over time.[28] To do this, I drop five states (California, Illinois, Massachusetts, Michigan, and New York) that show a large drop in overall participation around the time that there was a large drop nationally in type C lunch participation.[29] These are large states, and dropping these observations makes the results less precise, but there are no appreciable changes in the conclusions.

1.7 Conclusion

The NSLP appears to have had no long-term effect on health but may have affected educational attainment. The IV estimates on education suggest that increasing NSLP exposure by ten percentage points is associated with increasing education by .365 years among women and nearly one year among men. These estimates are large but imprecise.

The precision of the estimates is limited by the fact that the variation used to identify the effects of the NSLP occurs only at the level of state of birth and birth cohort. But

[28] See footnote 5 for an explanation of type A, type B, and type C lunches.

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

taking the results at face value, there are at least two potential explanations for why I detect an effect on education but not on health. First, there may be beneficial effects of the NSLP on health in the short-term that have faded away by adulthood. Second, the program may have attracted children to school but displaced nutritional inputs coming from elsewhere, including school lunches that were not part of the federal program. [30]

The NSLP today is still broad in its reach, but it has some elements of being targeted toward poorer children. These include codified standards for eligibility for free and reduced-price lunches and also special funding for poorer schools. Had these elements been in place at the inception of the NSLP, the NSLP may have had a detectable effect on health in its early years.

[30] My estimates are effects of participating in the NSLP. To the extent that there are school lunch programs that are not part of the NSLP, my estimates of the effects of the NSLP likely understate the effects of eating a school lunch.

A. Data Appendix

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

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

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

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

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

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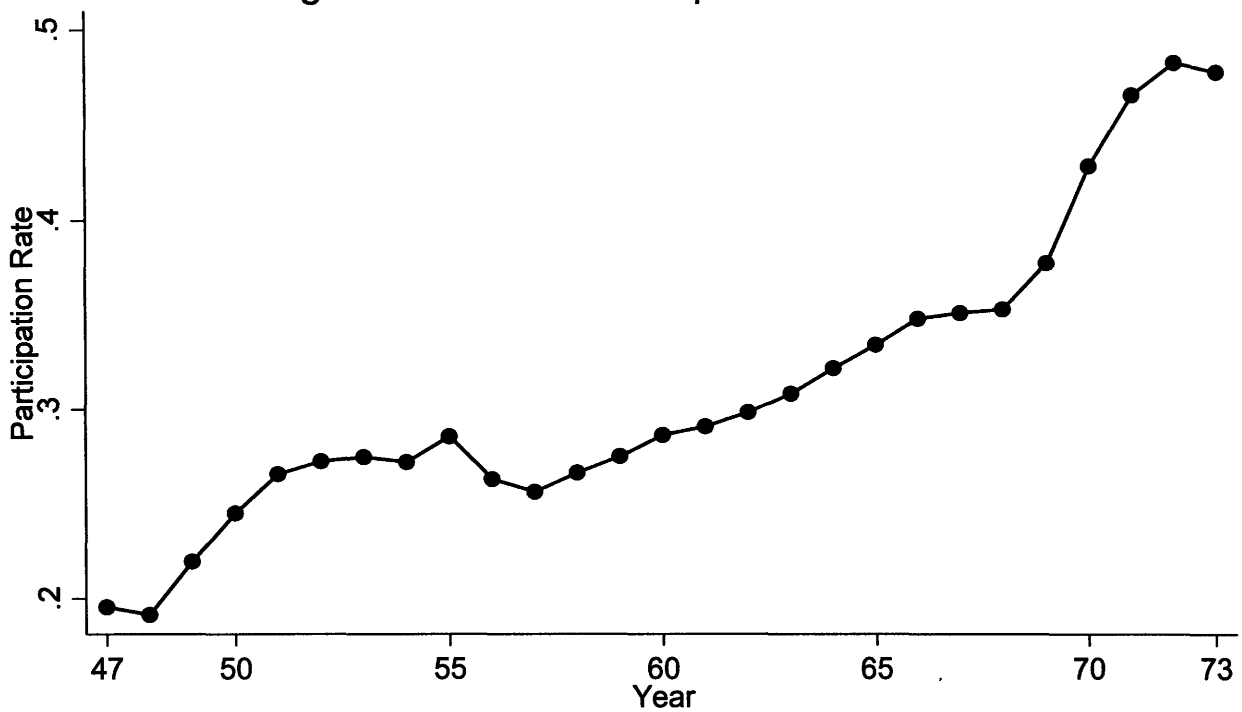
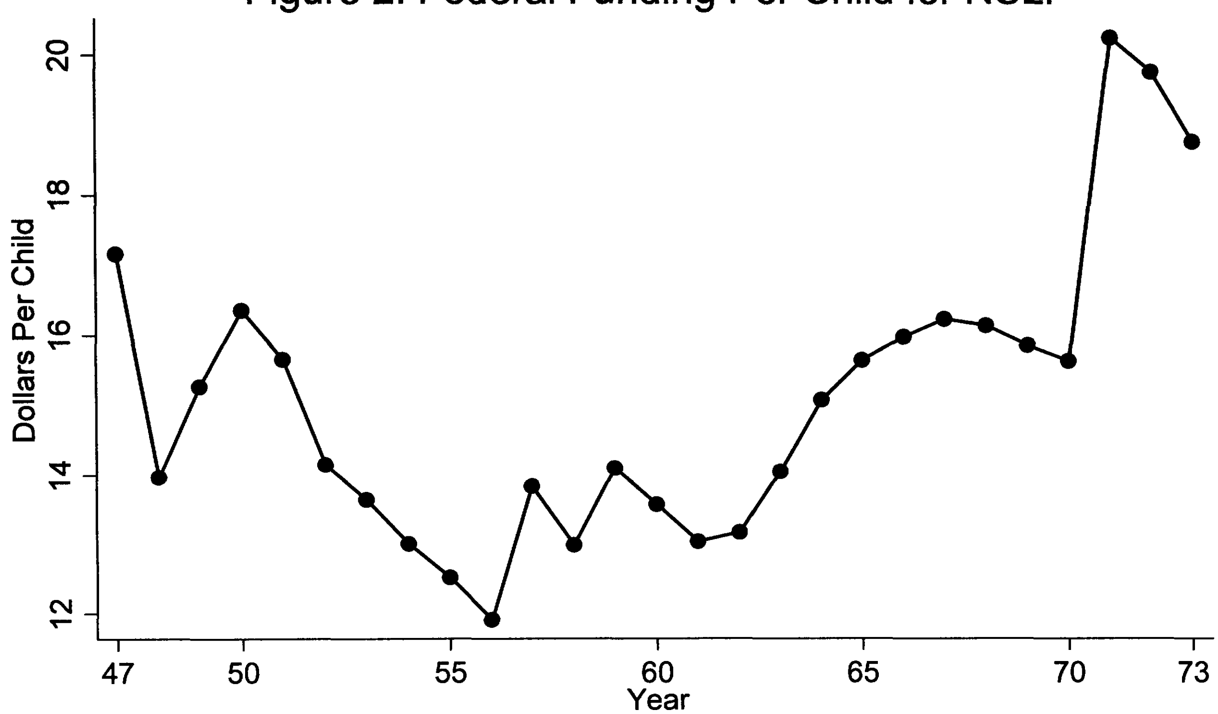
Figure 1: Annual Participation Rate in NSLP



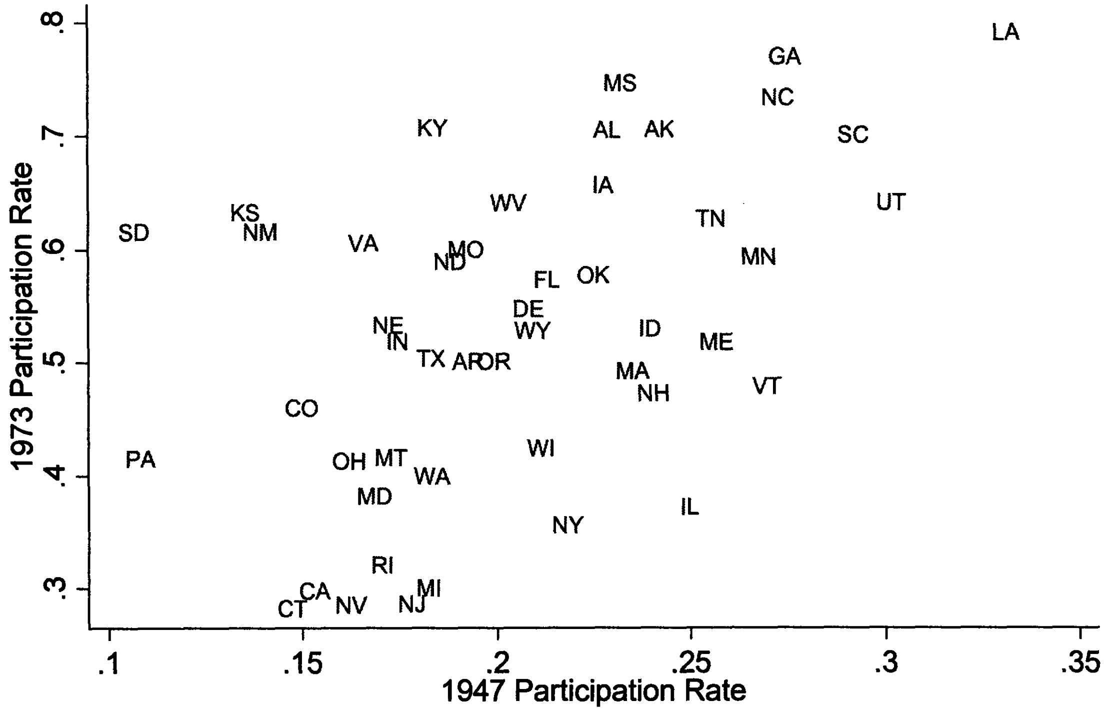
Note: Figure shows average participation nationally in peak month divided by size of population aged 5-17.

Figure 2: Federal Funding Per Child for NSLP



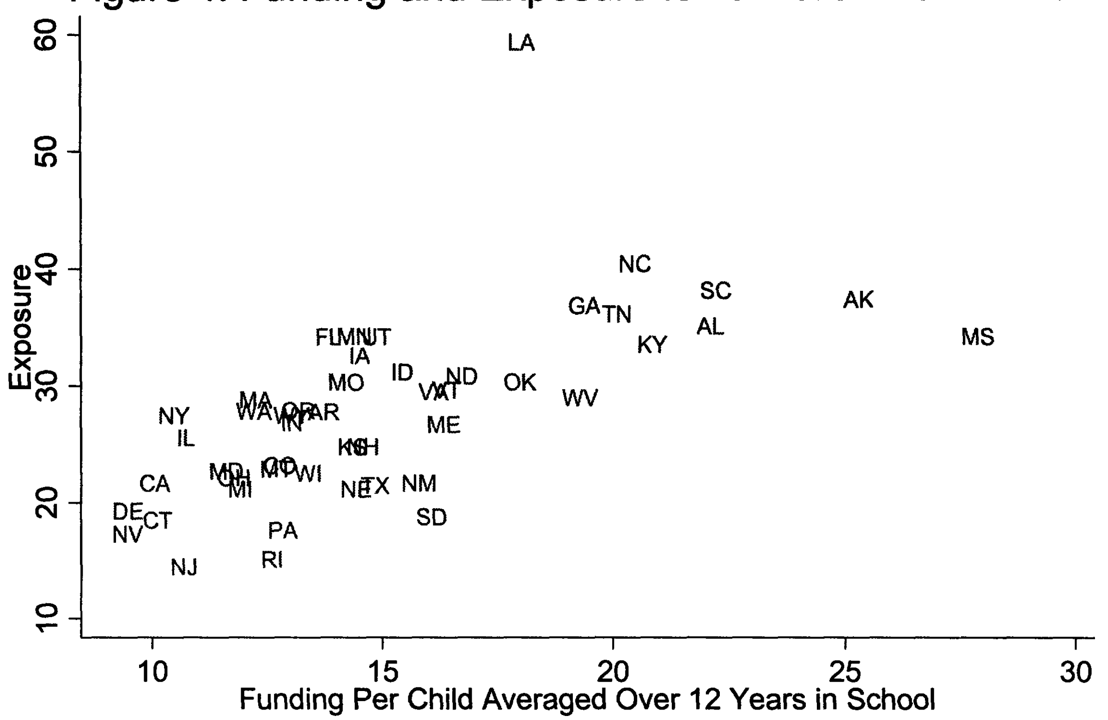
Note: Funding is measured in 2005 dollars.

Figure 3: State Participation Rates



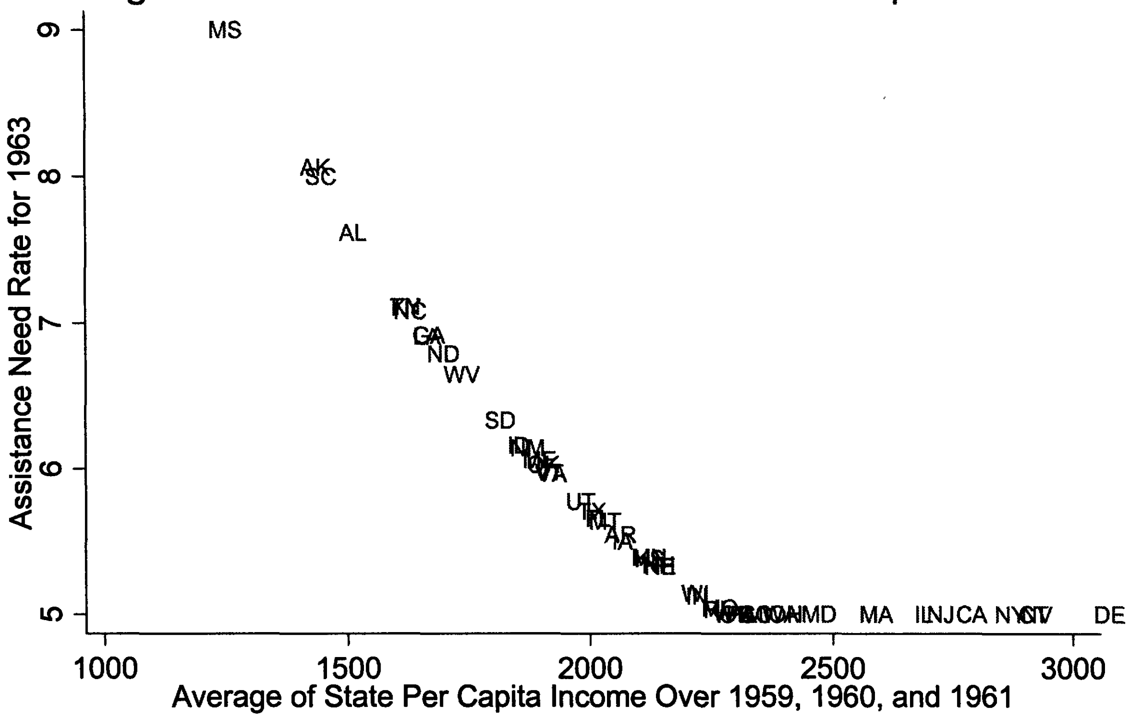
Note: 'Participation rate' is defined in note to Figure 1.

Figure 4: Funding and Exposure for Children Born in 1944



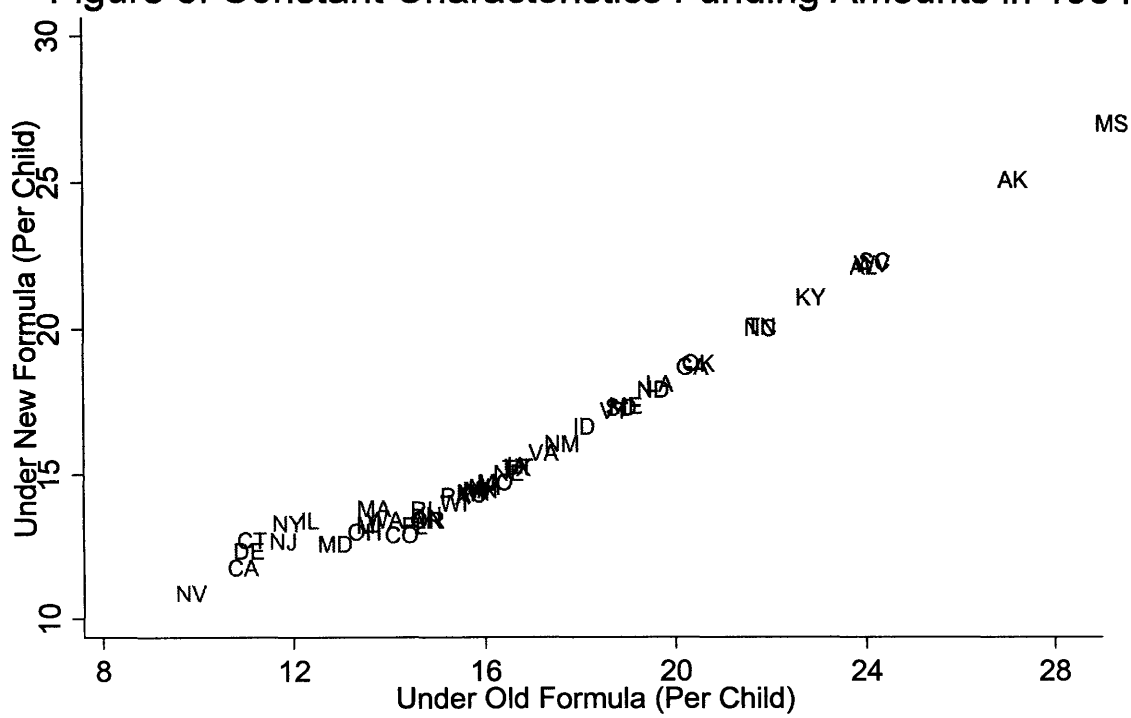
Notes: Funding is measured in 2005 dollars. Exposure is as defined in the text.

Figure 5: Assistance Need Rate and Per Capita Income



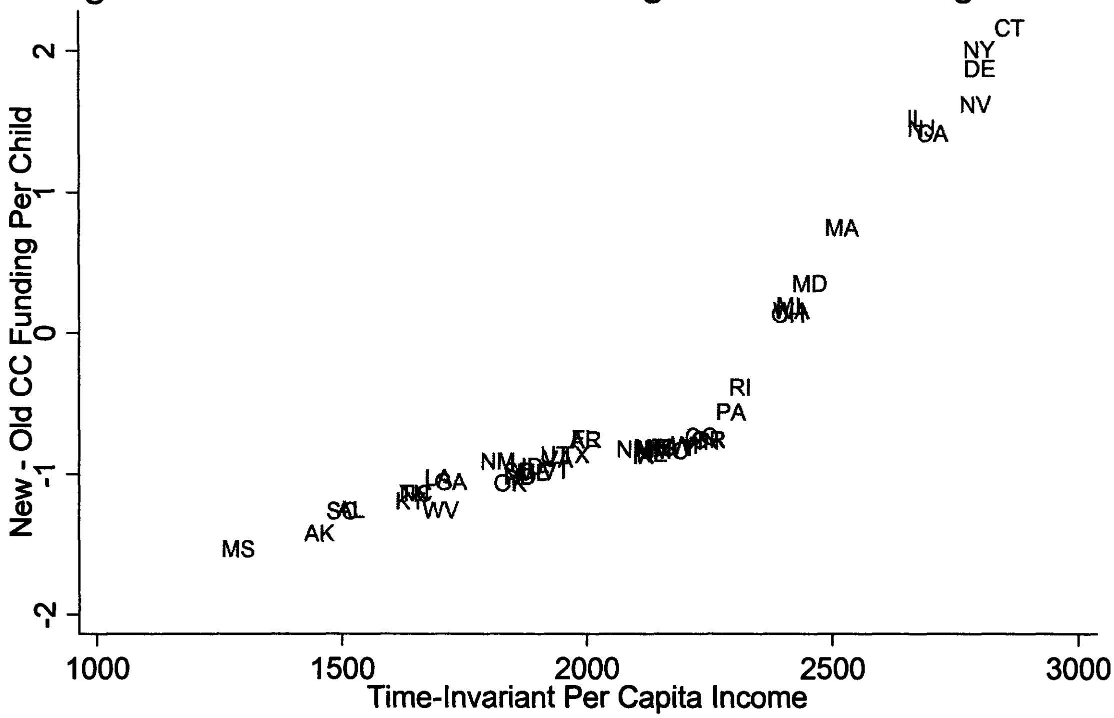
Note: Per capita income is measured in nominal dollars.

Figure 6: Constant Characteristics Funding Amounts in 1964



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

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



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

Table 1: Summary Statistics

A. NHIS Data (Health Outcomes)

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

B. Census Data (Education)

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

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

Table 2: First Stage for Men in NHIS Data

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

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

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

A. Men

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

B. Women

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

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

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

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

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

Table 5: Effect of NSLP Exposure on BMI

A. Men

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

B. Women

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

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

Table 6: Effects of NSLP Exposure on BMI Categories

A. Men

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

B. Women

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

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

Table 7: Effects of NSLP Exposure on Other Outcomes

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

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

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

A. Men

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

B. Women

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

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

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

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

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

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

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

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

Chapter 2

The Effects of Attending a Diverse College

2.1 Introduction

The affirmative action policies of a number of public universities in the United States have been re-examined in recent years. Since 1995, universities in California, Florida, and Texas have all replaced race-based admissions policies with policies that automatically admit any prospective applicant who has achieved a certain high school class rank.[1] In addition, voters in Washington State have passed an affirmative action ban, the University of Georgia has discontinued affirmative action, and the University of Michigan's undergraduate admissions affirmative action policy was struck down by the Supreme Court's 2003 decision in *Gratz v. Bollinger* before Michigan voters ultimately decided in 2006 to end race-conscious admissions policies.

Concomitant with the recent changes in state affirmative action policies has been a debate in the popular media about the merits of affirmative action. Many of the issues involved ultimately come down to matters of value, but there are some issues involved in the debate that could potentially be examined with data. This chapter addresses one such issue: whether there are benefits to be obtained from attending a racially diverse college.[2] This question is not only a major one in the popular debate over affirmative action, but it

[1] However, Texas is now moving back in the direction of race-conscious admissions.

[2] Another important question in the debate that can be examined empirically is whether underrepresented minorities benefit from attending universities they would not have been admitted to in the absence of affirmative action. See Bowen and Bok (1998), Rothstein and Yoon (2006), and Sander (2004) on this topic. See Holzer and Neumark (2000, 2006) for a broader review of affirmative action policy, the arguments involved, and the empirical evidence in a variety of contexts.

is also central to the legal debate. Justice Powell's 1978 landmark opinion in *Regents of the University of California v. Bakke* states that universities can use affirmative action in admissions because there is a compelling state interest in the educational benefits that can be obtained through a diverse student body, a claim which is also advanced by Justice O' Connor in her 2003 decision in *Grutter v. Bollinger*.

I use individual-level data from the nationally-representative Beginning Postsecondary Students survey to estimate the effects of college racial composition on students' earnings, civic behavior, and level of satisfaction with their college. I give results of least squares and probit regressions and also results from estimation procedures developed by Altonji, Elder, and Taber (2005) that account for potential selection on unobservables. Single-equation estimates suggest a positive effect of diversity on voting behavior and on satisfaction with the college attended, but I do not find an effect on other outcomes. Moreover, the estimates from the Altonji, Elder, and Taber (2005) procedures suggest that even a small amount of selection on unobservables can dramatically overturn the results. Section 2.2 discusses previous research on the effects of college diversity, Section 2.3 describes the data as well as the empirical methods and specifications used, Section 2.4 discusses the empirical results, and Section 2.5 concludes.

2.2 The Effects of College Diversity: Prior Literature

Arcidiacono and Vigdor (2006) use the Mellon Foundation's College and Beyond database, which includes students from 30 selective colleges and universities, to estimate the effect on earnings of the underrepresented minority share at a student's college and also within his major at his college. The within-major regressions allow for college fixed effects and are intended to reduce the problem of selection on unobservables, but they could exacerbate the selection problem if there is both (1) selection into majors that varies across colleges and (2) more selection on unobservables into majors within colleges than there is selection on unobservables into colleges. Moreover, using a dataset such as College and Beyond that focuses on selective colleges may result in a larger problem of selection on unobservables than using a representative sample of colleges would. This is because selective colleges have more discretion over whom to admit, and those that choose to admit a more diverse group of students may differ from those that admit a less diverse group along unobservable dimensions that also affect the outcomes.[3] Arcidiacono and Vigdor generally obtain positive but insignificant estimates of the effect of underrepresented minority share at the college on earnings, and they obtain negative but insignificant estimates of the effect of underrepresented minority share in the major on earnings.[4]

Boisjoly, Duncan, Kremer, Levy, and Eccles (2006) utilize a large university's random assignment of roommates and find that whites who were assigned a black roommate were more likely to support affirmative action and feel comfortable interacting with individuals of other races. Although this paper provides a clean test of peer effects from a roommate on attitudes, it is unable to examine 'harder' outcomes such as income or civic participation. Moreover, there are at least two reasons why it is not externally valid for estimating the effects of changing a college's affirmative action policy. First,

[3] On the other hand, if there are heterogeneous treatment effects, then using a sample of individuals at selective institutions may be more relevant for answering questions about the effects of affirmative action policy. Selective institutions may have a greater ability to exercise a strong affirmative action policy and may need to do so in order to achieve an adequate level of minority representation,

[4] In contrast, Daniel, Black, and Smith (2001) use the NLSY and obtain statistically significant least squares estimates showing a positive effect of the fraction of blacks at the institution on the earnings of white men.

changing the affirmative action policy changes the racial composition of the whole university and not just of someone's roommate. It is certainly true that changing a college's affirmative action policy could change the proportion of students who have a roommate of a different race, but it may also bring about other important changes. Second, changing the race of a roommate provides an element of forced interaction with members of another race, but changing the racial composition of the university may not result in increased interaction between students of different races if students socialize and study within racial groups.

2.3 Data and Empirical Methods

2.3.1 Data

The data used in this chapter come from the nationally-representative Beginning Postsecondary Students Longitudinal Study 1996/2001 (BPS). The dataset identifies students who were a part of the National Postsecondary Student Aid Study of 1995-1996 and were beginning their postsecondary schooling in school year. Follow-up surveys were then conducted in 1998 and 2001. The individual-level data is available with a restricted-use license from the National Center for Education Statistics.

The sample I use for the empirical analysis is the set of all whites in the BPS data who were beginning students at a four-year college. I exclude the small number of whites who attended a historically black institution or an institution in Puerto Rico. I also exclude individuals who are coded as coming from either Puerto Rico, Canada, or 'outlying areas.'

As with most longitudinal datasets, there is attrition in the BPS. Thus, I estimate each model using all observations in my sample for which data on all the variables in the model is available, and I use the weights provided in the dataset to account for nonresponse and the complex sampling procedure.[5]

2.3.2 Baseline Least Squares (LS) and Probit Specifications

The empirical analysis in this chapter begins with least squares regressions or probits of the outcome variables on college diversity, individual-level control variables, and institution-level control variables. When the lefthand side variable is continuous I estimate models of the form

$$y_{ij} = diversity_j \alpha + x_i ' \beta + w_j ' \gamma + \varepsilon_{ij}, \qquad (1a)$$

and when it is binary I estimate models of the form

$$P(y_{ij} = 1) = \Phi(diversity_j \alpha + x_i ' \beta + w_j ' \gamma). \qquad (1b)$$

Here y_{ij} is an outcome variable of individual *i* at institution *j*, $diversity_j$ is a measure of the level of diversity at institution *j*, x_i is a vector of individual-level controls, w_j is a vector of institution-level controls, ε_{ij} is a disturbance, and Φ refers to the cumulative distribution function of a standard normal random variable. The parameters to be estimated are α, β, and γ.

I examine six different outcome variables. The first two are earnings variables: the log of income in 2001 for those who are employed, and the log of salary in 2000 for those who are employed. Income is the primary variable in both Daniel, Black, and Smith

[5] In particular, since all righthand side variables are measured in 1996, I use the 1996 cross-section weights when the lefthand side variable is from 1996 and the 1996/2001 longitudinal weights when the lefthand side variable is from 2001.

(2001) and Arcidiacono and Vigdor (2006). I use it as an outcome variable on the grounds that employers may value employees who are skilled at interacting with a diverse group of people and that exposure to a diverse student body in college may help someone acquire these skills. Alternatively, diversity may be associated with lower earnings. A limitation of the BPS earnings data is that, since the income measures are taken only five or six years after the students were beginning college, a fairly large share of the sample may not yet be firmly established in the labor force. The next two outcome variables measure satisfaction with the college attended: a dummy for whether the student is satisfied with the racial climate at the institution at the end of freshman year and a dummy for whether the student is satisfied with the intellectual climate at the institution. The final two outcome variables are measures of civic and community participation. They are a dummy for whether the respondent voted in the 2000 election and a dummy for whether the respondent had done any community service in the past year. These variables are included in the analysis to examine whether college diversity has any effect on the degree to which people participate in their community. Exposure to a racially-diverse group of people may make someone either more or less civic- and community-minded.

I measure the main righthand side variable, diversity, in two different ways. One variable, *URM Share*, measures the fraction of underrepresented minorities at the institution. The second, *Racial Variety*, measures the extent to which different races are equally-represented. In particular, using a_j, b_j, h_j, n_j, and w_j to refer to the fraction of students at institution j who are, respectively, Asian, black, Hispanic, Native American, and white, these two measures are defined as:

1. $URM\ Share \equiv b_j + h_j + n_j$

2. $Racial\ Variety \equiv 1 - [(a_j - .2)^2 + (b_j - .2)^2 + (h_j - .2)^2 + (n_j - .2)^2 + (w_j - .2)^2]$

These two variables, although quite highly correlated in the data, reflect two slightly different views of what is meant by "diversity" and why it might be important.[6] The first measure is based on the idea that there may be effects of interacting with underrepresented minorities, and the second reflects the notion of diversity as variety. Although magnitudes of the second measure may be difficult to interpret, I use this measure to capture the notion that exposure to variety (rather than one racial group in particular) may be what is relevant. Up to the additive constant and the sign of the coefficient on the main righthand side variable, estimating models with the second measure is algebraically equivalent to estimating them using the Herfindahl index of racial shares.

The institution-level variables I use as controls are a dummy for whether the institution is public, the fraction of students who graduate, the log of enrollment, categorical variables measuring selectivity,[7] a full set of state dummies, and seven dummies that characterize how urban or rural the location is. The individual-level control variables include SAT score, a gender dummy, and a full set of dummies for state of residence when beginning college. I also control for the diversity of the individual's

[6] A third way of measuring the racial composition of a university is to measure how closely the racial composition of the university resembles that of the nation as a whole. Results using this measure of racial composition are available from the author.

[7] These variables are a dummy for whether the 25th percentile of SAT scores of students at the institution is at least 1200, a dummy for whether it is greater than 1100 but less than 1200, a dummy for whether it is greater than 1000 but less than 1100, and a dummy for whether the institution doesn't fall into one of those three categories but is deemed to be "selective" according to its 1994 Carnegie Classification.

high school.[8] This is an important control variable because it may affect someone's choice of college while also having a direct effect on the outcomes, and it could also proxy for other omitted variables. Including this as a control allows me to avoid confounding the effects of college diversity with the effects of high school diversity.

2.3.3 Selection on Unobservables

If the unobservable determinants of the outcome variables are uncorrelated with the diversity variable and the other righthand side variables, then the LS estimates are unbiased and consistent. However, the estimates are biased and inconsistent if there are unobservable characteristics of individuals that affect the outcomes directly and are correlated with an individual's college choice. They are also biased and inconsistent if there are unobservable characteristics of colleges that affect the outcomes of students at those colleges and that are correlated with the level of diversity at the college. Although I can control for a number of relevant covariates, including exposure to diversity while in high school, there may be other relevant control variables that are not available in the data. In order to estimate the effects of college diversity while acknowledging this possibility, I employ two techniques, one more informal and the other more formal, developed in Altonji, Elder, and Taber (2005).[9]

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[8] Whenever high school diversity is entered in a regression in this chapter, it is entered in the same way that college diversity is entered. For example, I use the fraction of underrepresented minorities at the high school in the regressions that use the fraction of underrepresented minorities at the college.

[9] Recent papers that use the methods of Altonji, Elder, and Taber include Krauth (2004, 2005) on peer effects in youth smoking behavior and Heinrich, Mueser, and Troske (2006) on the effects of temporary help service employment on later labor market outcomes. The methods have also been employed to study the effects of substance use by youths on suicide attempts (Chatterji, Dave, Kaestner, and Markowitz (2004)), sexual behavior (Grossman, Kaestner, and Markowitz (2004)), and educational attainment (Chatterji (2006)).

The more informal procedure involves estimating a system of two equations, where one equation is the outcome equation and the other is the selection equation, imposing various values on the correlation between the error terms in the two equations. In particular, when the outcome variable is continuous, I jointly estimate the equations

$$\begin{aligned} y_{ij} &= diversity_j \alpha + x_i'\beta + w_j'\gamma + \varepsilon_{ij} \\ diversity_j &= x_i'\delta + w_j'\lambda + u_{ij} \end{aligned} \tag{2a}$$

by maximum likelihood assuming that the errors are bivariate normal and imposing various values on ρ, the correlation between ε_{ij} and u_{ij}. When the outcome variable is binary, using y_{ij}^* to refer to the unobserved latent variable, I estimate the model

$$\begin{aligned} y_{ij} &= 1 \text{ if } y_{ij} \geq 0 \\ y_{ij} &= 0 \text{ if } y_{ij} < 0 \\ y_{ij}^* &= diversity_j \alpha + x_i'\beta + w_j'\gamma + \varepsilon_{ij} \\ diversity_j &= x_i'\delta + w_j'\lambda + u_{ij} \end{aligned} \tag{2b}$$

by maximum likelihood assuming that the errors are bivariate normal and again imposing various values on the correlation between the errors. Single-equation estimates of the outcome equations in these models are consistent when ρ equals zero, and the sensitivity analysis allows one to determine how large the correlation between ε_{ij} and u_{ij} must be in order to overturn the single-equation results. So if, for instance, the single-equation results suggest a significant positive effect of diversity on an outcome variable and the joint maximum likelihood approach also suggests a significant positive effect except for values of ρ that are very large in magnitude, this lends credibility to the claim that there actually is an effect of diversity on the outcome variable.

The more formal procedure attempts to obtain information about selection on unobservables from the amount of selection on observables.[10] Altonji, Elder, and Taber (2005) formalize this idea by estimating models imposing the condition that selection on unobservables and observables are the same, in the sense that the projection of the main righthand side variable on the error term in the outcome equation[11] equals its projection on the index of the other observable righthand side variables in the outcome equation.[12] In other words, the condition is that

$$Proj(diversity_j | x_i\beta + w_j\gamma, \varepsilon_{ij}) = \phi_0 + \phi \cdot (x_i\beta + w_j\gamma) + \phi \cdot \varepsilon_{ij}.$$

However, the assumption of equal selection on observables and unobservables should not necessarily be taken literally. Since many datasets contain variables that are likely to be important in a variety of contexts and because researchers do not choose their righthand side variables at random, in many cases there will be less selection on unobservables than selection on observables. The assumption that there is as much selection on unobservables as there is on observables stands in marked contrast to the assumption that there is no selection on unobservables, which is what is needed in order for LS or probit estimates to be consistent. The two assumptions can be seen as opposite extreme cases. And indeed, Altonji, Elder, and Taber (2002) give conditions under which their

[10] The idea of trying to obtain information about selection on unobservables by examining selection on observables is at work loosely in at least the following three situations: (1) when a researcher argues for an exclusion restriction in an instrumental variables context by showing that the instrument is uncorrelated with observable variables, (2) when a researcher shows there are not significant differences in means of observable variables across two groups when arguing that one group provides a valid control group for the other in a natural experiment, and (3) when a researcher argues that a regression specification is correct by showing that the estimates are not sensitive to including additional control variables.
[11] The error term can be viewed as being an index of unobservables.
[12] Altonji, Elder, and Taber (2005) offer more primitive assumptions that imply this condition. The key assumption is that the observable variables are chosen at random from the full set of variables that influence the outcome. The justification for this assumption is that, for large datasets that are used by many different researchers for many different purposes, the variables that are available may be more or less a random subset of the ones that are relevant for any particular researcher.

procedure and single-equation estimates identify bounds on the true parameter. Thus, the single-equation estimates and the estimates I give from the Altonji, Elder, and Taber procedure are the endpoints of an interval estimate of the true parameter.

2.4 Results

Table 1 displays summary statistics. Columns 1 and 2 give summary statistics for each variable when it is available, and columns 3 and 4 give summary statistics for each variable when it is available but only for those observations that are not missing any data on the righthand side variables used in this chapter. It is apparent from comparing the two sets of columns that, although restricting the sample reduces the number of observations that can be used, the weighted means and standard deviations are similar between the unrestricted sample and the restricted sample. It is also worth noting that a large percentage of respondents report being satisfied with the racial climate (89%) and with the intellectual climate (94%) at their institution. Moreover, 64% of respondents voted and 42% performed community service. Interestingly, the average student in the sample was exposed to a higher fraction of underrepresented minorities in high school than in college, but the mean of the racial variety variable is similar between college and high school. This may be partly because of the increased presence of Asian Americans in higher education and partly because of greater diversity among the underrepresented minorities within a college than within a high school.

Tables 2 and 3 report the results of least squares regressions that examine the relationship between college diversity and earnings. Table 2 uses the natural log of 2001 salary as the lefthand side variable, and Table 3 uses the natural log of 2000 income as the lefthand side variable. Panel A of each table uses the fraction of underrepresented minorities as the measure of diversity, and panel B uses the racial variety measure of diversity. The first column of each panel of these tables shows that there is a positive correlation between college diversity and earnings. This relationship is significant in Tables 2B and 3B but not in Tables 2A and 3A. For example, column 1 of Table 2A suggests that increasing underrepresented minority enrollment by one percentage point is associated with a statistically insignificant increase in salary of roughly one quarter of one percent. Column 1 of Table 2B suggests that increasing the college racial variety measure by .01 is associated with a statistically significant increase in salary of .47%. Thus, for instance, moving from a college that is 90% white and contains equal representation among the other groups to one that is 80% white and contains equal representation among the other groups (which results in increasing the racial variety measure from .3875 to .55) is associated with a large increase in earnings of around 7.6%. However, the general pattern is that adding control variables reduces the estimates in magnitude and renders them insignificant at conventional levels. It is also interesting to note that there is no significant relationship between high school diversity and earnings. The other coefficients are generally of the expected sign. For instance, being male and having a higher SAT score are both associated with higher earnings.

Table 4A gives the results of weighted probits that explore the relationship between the fraction of underrepresented minorities at an institution and satisfaction with the racial climate at the institution. Column 1 gives the results of a simple model with only a single righthand side variable and shows that the relationship between the two variables is positive and significantly different from zero. In particular, at the sample means, increasing the representation of underrepresented minorities by one percentage point is associated with a .0031 increase in the probability of being satisfied with the racial climate of the institution. The relationship between the share of underrepresented minorities and satisfaction with the racial climate is weakened in column 2, which adds a limited set of control variables. The remaining columns, which add additional control variables, show a relationship that becomes successively stronger. Column 6, which includes the full set of controls, demonstrates a positive and statistically significant relationship between the share of underrepresented minorities and satisfaction with the racial climate; the marginal effect at the sample means is similar to that obtained in column 1 and suggests that increasing the representation of underrepresented minorities by one percentage point is associated with a .0027 increase in the probability of being satisfied with the racial climate of the institution. Interestingly, attending a more diverse high school and having a higher SAT score are both associated with a significantly lower propensity to report being satisfied with the racial climate at the institution.

Table 4B, using the "racial variety" measure of diversity, still points to a positive relationship between diversity and satisfaction with the racial climate. Diversity could potentially lead to racial conflict and less satisfaction with the racial climate, but that does not appear to be the case here. However, individuals who are predisposed to report being satisfied with the racial climate may be selecting into diverse colleges. The estimates from the Altonji, Elder, and Taber procedures will determine how robust the estimates are to this possibility.

Table 5 explores the relationship between college racial composition and satisfaction with the intellectual climate at the college. Comparing column 6 to column 1 for each of the panels reveals that an insignificant negative association between diversity and satisfaction with the intellectual climate when no control variables are included in the model becomes an insignificant positive relationship when the full set of control variables is included. Moreover, with the exception of the graduation rate, few of the control variables themselves have significant coefficients in any of the specifications of Table 5.

Tables 6 and 7 turn to the determinants of political and civic behavior. Table 6 uses a dummy for voting in the 2000 election as the lefthand side variable, and Table 7 uses a dummy for participating in community service in the past year.[13] The results from Table 6A show a positive but insignificant relationship between the fraction of underrepresented minorities and voting in column 6, while the results from Table 6B show a positive and significant relationship between racial variety and voting. The coefficients on the diversity variables are generally insignificant in Table 7, but both panels of the table point to a negative relationship between diversity and community service. Being female and having a higher SAT score are associated with a higher propensity to vote and to participate in community service. Interestingly, individuals who attend institutions with higher graduation rates are significantly more likely to participate in community service, although there does not appear to be a relationship between the graduation rate and voting.

Tables 8, 9, and 10 show the results of the sensitivity analyses and the estimates obtained from imposing equality between selection on unobservables and selection on observables. Table 8 focuses on income. The row corresponding to $\rho = 0$ reproduces estimates that are shown in the final columns of Tables 2 and 3. The other rows show

[13] The community service variable was measured in 2001, and the students were beginning college in 1995-1996.

results of estimating system of equations (2a) imposing various values of the correlation between the errors or imposing the condition of equal selection on observables and unobservables. Even a small correlation between the errors can cause the estimates to move quite far in one direction or the other. For example, in column 1, the least squares estimate suggests that increasing minority representation by one percentage point is associated with a statistically insignificant increase of 2001 salary by .147%. But if the correlation between the errors in the outcome equation and the selection equation is -.1, then this same increase in minority representation is associated with a highly significant increase in 2001 salary by 1.39%. If the correlation is .1, then the one percentage point increase in minority representation is associated with a highly significant *decrease* in salary of 1.09%. This same pattern appears in the other columns of Table 8, as well. The pattern is that the least squares estimates do not suggest a significant effect of diversity on earnings, but a large and highly significant relationship appears if there is even a modest amount of selection into diverse colleges based on unobservables. This effect is positive if the selection is negative, and it is negative if the selection is positive. And the relationship between diversity and earnings appears even larger in magnitude the larger the correlation between the errors in the selection equation and outcome equation is in magnitude.

The results shown in Tables 9 and 10 for satisfaction with college and for civic behavior are similar to those in Table 8, in that the estimates are quite sensitive to what value is imposed for the correlation between the errors. For example, the estimates in columns 1 and 2 of Table 9 display a significant positive effect of diversity on satisfaction with the racial climate for $\rho = 0$, a larger and even more significant effect for negative values of ρ, an insignificant effect for $\rho = .1$, and a large and significant negative effect for larger positive values of ρ.

The most natural interpretation of these results is that one should be cautious of the single-equation estimates. It is unlikely that the least squares regressions control for all the relevant covariates, so there is almost certainly at least some selection into diverse colleges based on unobservables. And if imposing a small amount of selection on unobservables causes the estimates to change dramatically, then the original estimates are cast into doubt. However, another possible interpretation of the results is that, if imposing a small amount of correlation between the errors results in estimates that are certainly too extreme, then this may suggest that there is not much correlation between the errors to begin with. But a limitation with this interpretation is that, even if there is not a very large correlation, it does not follow that there is zero correlation or that the least squares estimates are consistent. In fact, if imposing a correlation of .1 results in estimates that are too large to be reasonable, then one should be concerned that imposing values of the correlation that are even smaller will result in estimates that are more reasonable but that still dramatically overturn the least squares results.

The bottom rows of Tables 8, 9, and 10 show the Altonji, Elder, and Taber interval estimates, along with the estimated value of ρ from the model that imposes equal selection on observables and unobservables.[14] The results give some credibility to the claim that the correlation between errors is small and to the claim that we should be cautious about the least squares estimates. For example, in column 1 of Table 8, a very small correlation of .006 results in a modest-sized interval estimate of [.231,.302]. And

[14] Due to computational difficulties, only unweighted estimates are shown. However, the models do include the full set of control variables.

in column 3 of Table 8, a modest correlation of .035 results in a wide interval estimate of [-.574,.208]. Moreover, not only are many of the bounds wide, but sampling error means that our uncertainty about the parameters is even greater than the interval estimates indicate.

2.5 Conclusion

Single-equation estimates suggest a positive effect of diversity on voting behavior and on satisfaction with the college attended, but I do not find an effect on other outcomes. Moreover, the estimates are very sensitive to the assumptions made about selection on unobservables. The sensitivity analyses show that it takes only a small degree of correlation between the unobservable determinants of the diversity of someone's college and the unobservable determinants of the outcomes to dramatically overturn the single-equation results. This casts the single-equation estimates in doubt, although another possibility is that there is not very much correlation between the unobservable determinants of diversity and the unobservable determinants of the outcomes to begin with. The estimates obtained by imposing equal selection on observables and unobservables suggest that both of these points have some validity.

This chapter uses a new method to give some insight into an important policy question, but it by no means provides a final answer. In fact, one of the key findings is that we should be skeptical of least squares estimates of the effects of college diversity on outcomes. This suggests that additional research using different techniques, including instrumental variables, would be fruitful. Recent changes in affirmative action policies may suggest a valid quasi-experiment, although these policy changes also change other characteristics of the student body, including the SAT score distribution and the high school GPA distribution. They may also result in an enrolled student body that has a different attitude towards diversity.[15]

[15] See Long (2004) and Card and Krueger (2005) on the effects of college affirmative action bans on the application behavior of prospective students.

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Table 1: Summary Statistics

	Least-Restricted Sample		Most-Restricted Sample	
	Sample Size (1)	Mean (SD) (2)	Sample Size (3)	Mean(SD) (4)
Outcome Variables				
2001 Log Salary	3237	10.23	2578	10.25
		(0.53)		(0.51)
2000 Log Income	4081	9.74	3291	9.73
		(0.88)		(0.88)
Satisfied with Racial Climate	5184	0.887	4099	0.885
		(0.317)		(0.319)
Satisfied with Intellectual Climate	5212	0.943	4111	0.944
		(0.232)		(0.229)
Voted in 2000 Election	4735	0.642	3807	0.643
		(0.479)		(0.479)
Performed Community Service	4774	0.416	3824	0.420
		(0.493)		(0.494)
Measures of Diversity				
College URM Share	5978	0.0949	4731	0.0945
		(0.0678)		(0.0658)
High School URM Share	5131	0.131	4731	0.133
		(0.169)		(0.169)
College Racial Variety	5978	0.424	4731	0.427
		(0.130)		(0.130)
High School Racial Variety	5131	0.425	4731	0.427
		(0.190)		(0.190)
Individual-Level Controls				
SAT	5657	969	4731	974
		(195)		(194)
Male	5995	0.465	4731	0.455
		(0.499)		(0.498)
Institution-Level Controls				
Public	5996	0.637	4731	0.646
		(0.481)		(0.478)
Log Enrollment	5974	9.19	4731	9.30
		(1.18)		(1.13)
Graduation Rate	5676	0.530	4731	0.548
		(0.175)		(0.172)

Notes: Columns 1 and 2 give summary statistics for each variable when it is available. Columns 3 and 4 give summary statistics for each variable over the set of observations that contain no missing data on any righthand side variables. Means and standard deviations are weighted using the weights provided in the Beginning Postsecondary Students data.

Table 2A: Effect of Underrepresented Minority Share on Log of 2001 Salary

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College URM Share	0.243	0.141	0.353	0.140	0.137	0.147
	(0.139)	(0.156)	(0.188)	(0.215)	(0.245)	(0.248)
High School URM Share			-0.0453	-0.0223	-0.0288	-0.0203
			(0.0661)	(0.0712)	(0.0690)	(0.0724)
SAT (divided by 100)		0.0129	0.0109	0.0120	0.0118	0.0131
		(0.0056)*	(0.0059)	(0.0060)*	(0.0061)	(0.0061)*
Male		0.203	0.191	0.184	0.185	0.184
		(0.019)**	(0.019)**	(0.020)**	(0.020)**	(0.020)**
Public		-0.0401	-0.0325	-0.0402	-0.0665	-0.0611
		(0.0324)	(0.0340)	(0.0366)	(0.0388)	(0.0400)
Log Enrollment		0.0375	0.0335	0.0354	0.0444	0.0447
		(0.0145)**	(0.0152)*	(0.0163)*	(0.0171)**	(0.0174)*
Graduation Rate		0.214	0.260	0.189	0.192	0.195
		(0.096)*	(0.103)*	(0.117)	(0.126)	(0.129)
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	3227	2827	2578	2578	2578	2578

Notes: The table shows estimates of equation (1a) by least squares using 1996/2001 longitudinal weights. Standard errors are in parentheses. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 2B: Effect of Racial Variety on Log of 2001 Salary

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College Racial Variety	0.470	0.139	0.177	0.085	-0.052	-0.023
	(0.078)**	(0.084)	(0.099)	(0.119)	(0.146)	(0.150)
HS Racial Variety			0.0355	0.0487	0.0619	0.0626
			(0.0584)	(0.0652)	(0.0623)	(0.0664)
SAT (divided by 100)		0.0126	0.0104	0.0117	0.0121	0.0132
		(0.0056)*	(0.0059)	(0.0060)	(0.0061)*	(0.0061)*
Male		0.203	0.190	0.183	0.185	0.183
		(0.019)**	(0.019)**	(0.020)**	(0.020)**	(0.020)**
Public		-0.0400	-0.0345	-0.0402	-0.0669	-0.0618
		(0.0323)	(0.0339)	(0.0365)	(0.0387)	(0.0400)
Log Enrollment		0.0362	0.0325	0.0340	0.0448	0.0452
		(0.0144)*	(0.0152)*	(0.0164)*	(0.0171)**	(0.0174)**
Graduation Rate		0.217	0.243	0.182	0.162	0.169
		(0.094)*	(0.100)*	(0.115)	(0.124)	(0.127)
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	3227	2827	2578	2578	2578	2578

Notes: The table shows estimates of equation (1a) by least squares using 1996/2001 longitudinal weights. Standard errors are in parentheses. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 3A: Effect of Underrepresented Minority Share on Log of 2000 Income

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College URM Share	0.111	-0.072	0.014	0.047	0.398	0.321
	(0.202)	(0.242)	(0.285)	(0.326)	(0.372)	(0.375)
High School URM Share			-0.164	-0.153	-0.113	-0.144
			(0.101)	(0.109)	(0.106)	(0.111)
SAT (divided by 100)		-0.0233	-0.0195	-0.0167	-0.0173	-0.0178
		(0.0088)**	(0.0093)*	(0.0095)	(0.0096)	(0.0097)
Male		0.207	0.203	0.195	0.207	0.197
		(0.029)**	(0.031)**	(0.031)**	(0.031)**	(0.031)**
Public		-0.0379	-0.0210	-0.0065	-0.0237	-0.0058
		(0.0507)	(0.0531)	(0.0573)	(0.0611)	(0.0630)
Log Enrollment		0.0168	0.0188	0.0141	0.0214	0.0205
		(0.0224)	(0.0235)	(0.0253)	(0.0264)	(0.0269)
Graduation Rate		0.186	0.327	0.203	0.243	0.243
		(0.152)	(0.162)*	(0.183)	(0.197)	(0.202)
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	4071	3603	3292	3291	3292	3291

Notes: The table shows estimates of equation (1a) by least squares using 1996/2001 longitudinal weights. Standard errors are in parentheses. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 3B: Effect of Racial Variety on Log of 2000 Income

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College Racial Variety	0.228	-0.048	-0.037	0.021	0.255	0.203
	(0.106)*	(0.132)	(0.154)	(0.185)	(0.223)	(0.228)
HS Racial Variety			-0.092	-0.124	-0.041	-0.098
			(0.091)	(0.102)	(0.097)	(0.103)
SAT (divided by 100)		-0.0232	-0.0187	-0.0161	-0.0173	-0.0174
		(0.0089)**	(0.0094)*	(0.0096)	(0.0096)	(0.0097)
Male		0.207	0.203	0.196	0.208	0.198
		(0.029)**	(0.031)**	(0.031)**	(0.031)**	(0.031)**
Public		-0.0373	-0.0232	-0.0089	-0.0299	-0.0112
		(0.0507)	(0.0530)	(0.0572)	(0.0609)	(0.0629)
Log Enrollment		0.0169	0.0199	0.0149	0.0211	0.0207
		(0.0224)	(0.0235)	(0.0253)	(0.0264)	(0.0269)
Graduation Rate		0.190	0.330	0.205	0.232	0.235
		(0.148)	(0.158)*	(0.180)	(0.194)	(0.198)
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	4071	3603	3292	3291	3292	3291

Notes: The table shows estimates of equation (1a) by least squares using 1996/2001 longitudinal weights. Standard errors are in parentheses. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 4A: Effect of Underrepresented Minority Share on Satisfaction with Racial Climate

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College URM Share	1.64	0.32	0.75	1.11	1.62	1.72
	(0.37)**	(0.46)	(0.53)	(0.64)	(0.81)*	(0.83)*
	[0.31]	[0.06]	[0.13]	[0.19]	[0.26]	[0.27]
High School URM Share			-0.197	-0.319	-0.447	-0.545
			(0.176)	(0.194)	(0.190)*	(0.201)**
			[-0.035]	[-0.053]	[-0.072]	[-0.084]
SAT (divided by 100)		-0.0391	-0.0309	-0.0487	-0.0540	-0.0637
		(0.0154)*	(0.0161)	(0.0173)**	(0.0173)**	(0.0180)**
		[-0.007]	[-0.0055]	[-0.0082]	[-0.0087]	[-0.0098]
Male		-0.0107	-0.0021	0.0305	0.0479	0.0642
		(0.0508)	(0.0536)	(0.0557)	(0.0565)	(0.0578)
		[-0.0019]	[-0.0004]	[0.0051]	[0.0077]	[0.0099]
Public		0.153	0.201	0.148	0.173	0.133
		(0.089)	(0.094)*	(0.102)	(0.117)	(0.122)
		[0.028]	[0.037]	[0.025]	[0.029]	[0.021]
Log Enrollment		0.0519	0.0420	0.0556	0.0292	0.0474
		(0.0389)	(0.0409)	(0.0449)	(0.0489)	(0.0505)
		[0.0093]	[0.0075]	[0.0093]	[0.0047]	[0.0073]
Graduation Rate		-0.376	-0.327	-0.324	-0.799	-0.619
		(0.276)	(0.291)	(0.329)	(0.367)*	(0.381)
		[-0.068]	[-0.058]	[-0.054]	[-0.128]	[-0.096]
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	5170	4527	4099	4059	4094	4056

Notes: The table shows probit estimates of equation (1b) using 1996 cross-section weights. Standard errors are in parentheses. The brackets contain marginal effects evaluated at the sample mean for continuous variables and the discrete change in probability at the sample mean for dummy variables. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 4B: Effect of Racial Variety on Satisfaction with Racial Climate

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College Racial Variety	0.27	0.40	0.85	1.46	1.68	1.67
	(0.18)	(0.24)	(0.27)**	(0.33)**	(0.43)**	(0.45)**
	[0.05]	[0.07]	[0.15]	[0.24]	[0.27]	[0.26]
HS Racial Variety			-0.453	-0.516	-0.597	-0.617
			(0.156)**	(0.181)**	(0.173)**	(0.187)**
			[-0.081]	[-0.085]	[-0.095]	[-0.094]
SAT (divided by 100)		-0.0399	-0.0301	-0.0486	-0.0534	-0.0623
		(0.0154)**	(0.0161)	(0.0173)**	(0.0174)**	(0.0181)**
		[-0.0072]	[-0.0054]	[-0.0080]	[-0.0085]	[-0.0095]
Male		-0.0101	-0.0028	0.0325	0.0512	0.0657
		(0.0508)	(0.0536)	(0.0559)	(0.0566)	(0.0580)
		[-0.0018]	[-0.0005]	[0.0054]	[0.0081]	[0.0100]
Public		0.147	0.181	0.115	0.123	0.087
		(0.089)	(0.094)	(0.103)	(0.118)	(0.123)
		[0.027]	[0.033]	[0.019]	[0.020]	[0.014]
Log Enrollment		0.0487	0.0416	0.0483	0.0276	0.0435
		(0.0389)	(0.0409)	(0.0451)	(0.0491)	(0.0506)
		[0.0088]	[0.0074]	[0.0080]	[0.0044]	[0.0067]
Graduation Rate		-0.375	-0.371	-0.329	-0.807	-0.646
		(0.270)	(0.285)	(0.325)	(0.362)*	(0.374)
		[-0.067]	[-0.066]	[-0.054]	[-0.128]	[-0.099]
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	5170	4527	4099	4059	4094	4056

Notes: The table shows probit estimates of equation (1b) using 1996 cross-section weights. Standard errors are in parentheses. The brackets contain marginal effects evaluated at the sample mean for continuous variables and the discrete change in probability at the sample mean for dummy variables. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 5A: Effect of Underrepresented Minority Share on Satisfaction with Intellectual Climate

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College URM Share	-0.379	0.018	-0.357	-0.183	0.623	0.849
	(0.400)	(0.502)	(0.577)	(0.720)	(0.925)	(0.953)
	[-0.043]	[0.002]	[-0.038]	[-0.018]	[0.057]	[0.069]
High School URM Share			0.381	0.226	0.444	0.176
			(0.221)	(0.243)	(0.239)	(0.251)
			[0.040]	[0.022]	[0.041]	[0.014]
SAT (divided by 100)		-0.0205	-0.0277	-0.0222	-0.0291	-0.0226
		(0.0184)	(0.0194)	(0.0205)	(0.0209)	(0.0217)
		[-0.0022]	[-0.0029]	[-0.0022]	[-0.0027]	[-0.0018]
Male		0.0160	-0.0145	-0.0424	-0.0397	-0.0458
		(0.0614)	(0.0653)	(0.0685)	(0.0689)	(0.0718)
		[0.0017]	[-0.0015]	[-0.0042]	[-0.0037]	[-0.0038]
Public		-0.161	-0.099	-0.090	-0.038	-0.055
		(0.114)	(0.120)	(0.136)	(0.146)	(0.156)
		[-0.017]	[-0.010]	[-0.009]	[-0.003]	[-0.004]
Log Enrollment		0.0173	-0.0060	-0.0042	-0.0146	-0.0147
		(0.0491)	(0.0520)	(0.0584)	(0.0615)	(0.0642)
		[0.0019]	[-0.0006]	[-0.0004]	[-0.0013]	[-0.0012]
Graduation Rate		0.58	0.64	1.17	1.27	1.44
		(0.32)	(0.34)	(0.40)**	(0.43)**	(0.45)**
		[0.06]	[0.07]	[0.12]	[0.12]	[0.12]
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	5198	4539	4111	3953	4048	3911

Notes: The table shows probit estimates of equation (1b) using 1996 cross-section weights. Standard errors are in parentheses. The brackets contain marginal effects evaluated at the sample mean for continuous variables and the discrete change in probability at the sample mean for dummy variables. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 5B: Effect of Racial Variety on Satisfaction with Intellectual Climate

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College Racial Variety	-0.103	-0.273	-0.716	-0.366	0.144	0.349
	(0.216)	(0.283)	(0.333)*	(0.421)	(0.529)	(0.555)
	[-0.012]	[-0.030]	[-0.075]	[-0.036]	[0.013]	[0.028]
HS Racial Variety			0.509	0.334	0.613	0.308
			(0.201)*	(0.233)	(0.224)**	(0.243)
			[0.054]	[0.033]	[0.056]	[0.025]
SAT (divided by 100)		-0.0199	-0.0291	-0.0229	-0.0306	-0.0233
		(0.0184)	(0.0195)	(0.0205)	(0.0209)	(0.0217)
		[-0.0022]	[-0.0031]	[-0.0023]	[-0.0028]	[-0.0019]
Male		0.0158	-0.0142	-0.0428	-0.0433	-0.0473
		(0.0614)	(0.0655)	(0.0685)	(0.0691)	(0.0718)
		[0.0017]	[-0.0015]	[-0.0042]	[-0.0040]	[-0.0039]
Public		-0.166	-0.096	-0.087	-0.038	-0.059
		(0.114)	(0.120)	(0.136)	(0.146)	(0.156)
		[-0.017]	[-0.010]	[-0.008]	[-0.003]	[-0.005]
Log Enrollment		0.0245	0.0014	-0.0006	-0.0173	-0.0171
		(0.0493)	(0.0521)	(0.0587)	(0.0621)	(0.0650)
		[0.0027]	[0.0002]	[-0.0001]	[-0.0016]	[-0.0014]
Graduation Rate		0.52	0.58	1.15	1.20	1.39
		(0.32)	(0.34)	(0.39)**	(0.42)**	(0.44)**
		[0.06]	[0.06]	[0.11]	[0.11]	[0.11]
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	5198	4539	4111	3953	4048	3911

Notes: The table shows probit estimates of equation (1b) using 1996 cross-section weights. Standard errors are in parentheses. The brackets contain marginal effects evaluated at the sample mean for continuous variables and the discrete change in probability at the sample mean for dummy variables. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 6A: Effect of Underrepresented Minority Share on Voting

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College URM Share	0.398	0.944	0.526	0.931	0.816	0.813
	(0.274)	(0.336)**	(0.386)	(0.444)*	(0.505)	(0.512)
	[0.149]	[0.349]	[0.196]	[0.345]	[0.301]	[0.299]
High School URM Share			0.336	0.250	0.293	0.238
			(0.143)*	(0.155)	(0.150)	(0.157)
			[0.125]	[0.093]	[0.108]	[0.088]
SAT (divided by 100)		0.0786	0.0851	0.0753	0.0747	0.0753
		(0.0122)**	(0.0128)**	(0.0132)**	(0.0133)**	(0.0135)**
		[0.0291]	[0.0316]	[0.0279]	[0.0275]	[0.0277]
Male		-0.178	-0.188	-0.197	-0.190	-0.207
		(0.041)**	(0.042)**	(0.043)**	(0.043)**	(0.044)**
		[-0.066]	[-0.070]	[-0.073]	[-0.070]	[-0.076]
Public		-0.062	-0.116	-0.107	-0.048	-0.102
		(0.071)	(0.074)	(0.080)	(0.085)	(0.089)
		[-0.023]	[-0.043]	[-0.039]	[-0.018]	[-0.037]
Log Enrollment		-0.0110	0.0256	0.0166	-0.0141	-0.0016
		(0.0313)	(0.0325)	(0.0352)	(0.0367)	(0.0377)
		[-0.0041]	[0.0095]	[0.0061]	[-0.0052]	[-0.0006]
Graduation Rate		-0.225	-0.118	0.104	0.094	-0.018
		(0.212)	(0.224)	(0.256)	(0.275)	(0.283)
		[-0.083]	[-0.044]	[0.038]	[0.034]	[-0.006]
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	4723	4167	3808	3803	3808	3803

Notes: The table shows probit estimates of equation (1b) using 1996/2001 longitudinal weights. Standard errors are in parentheses. The brackets contain marginal effects evaluated at the sample mean for continuous variables and the discrete change in probability at the sample mean for dummy variables. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 6B: Effect of Racial Variety on Voting

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College Racial Variety	0.474	0.628	0.513	0.744	0.756	0.784
	(0.146)**	(0.186)**	(0.215)*	(0.260)**	(0.313)*	(0.321)*
	[0.177]	[0.233]	[0.191]	[0.275]	[0.279]	[0.288]
HS Racial Variety			0.136	0.042	0.080	0.016
			(0.126)	(0.143)	(0.136)	(0.145)
			[0.051]	[0.016]	[0.029]	[0.006]
SAT (divided by 100)		0.0774	0.0828	0.0735	0.0728	0.0736
		(0.0122)**	(0.0128)**	(0.0133)**	(0.0133)**	(0.0135)**
		[0.0287]	[0.0308]	[0.0272]	[0.0268]	[0.027]
Male		-0.179	-0.189	-0.197	-0.190	-0.207
		(0.041)**	(0.042)**	(0.043)**	(0.043)**	(0.044)**
		[-0.066]	[-0.070]	[-0.073]	[-0.070]	[-0.076]
Public		-0.069	-0.118	-0.113	-0.055	-0.109
		(0.071)	(0.073)	(0.080)	(0.085)	(0.088)
		[-0.026]	[-0.044]	[-0.042]	[-0.020]	[-0.040]
Log Enrollment		-0.0124	0.0228	0.0141	-0.0172	-0.0053
		(0.0313)	(0.0325)	(0.0353)	(0.0368)	(0.0378)
		[-0.0046]	[0.0085]	[0.0052]	[-0.0064]	[-0.0019]
Graduation Rate		-0.272	-0.154	0.073	0.084	-0.018
		(0.208)	(0.218)	(0.252)	(0.271)	(0.279)
		[-0.101]	[-0.057]	[0.027]	[0.031]	[-0.007]
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	4723	4167	3808	3803	3808	3803

Notes: The table shows probit estimates of equation (1b) using 1996/2001 longitudinal weights. Standard errors are in parentheses. The brackets contain marginal effects evaluated at the sample mean for continuous variables and the discrete change in probability at the sample mean for dummy variables. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 7A: Effect of Underrepresented Minority Share on Community Service

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College URM Share	-0.830	-0.434	-0.531	-0.760	-0.873	-0.784
	[0.268]**	(0.327)	(0.384)	(0.449)	(0.516)	(0.524)
	[-0.324]	[-0.170]	[-0.207]	[-0.296]	[-0.341]	[-0.306]
High School URM Share			0.0831	0.0095	0.0300	0.0200
			(0.1377)	(0.1501)	(0.1461)	(0.1530)
			[0.0325]	[0.0037]	[0.0117]	[0.0078]
SAT (divided by 100)		0.0855	0.0919	0.0816	0.0824	0.0793
		(0.0119)**	(0.0126)**	(0.0130)**	(0.0131)**	(0.0133)**
		[0.0334]	[0.0359]	[0.0318]	[0.0321]	[0.0309]
Male		-0.199	-0.214	-0.206	-0.219	-0.214
		(0.040)**	(0.042)**	(0.043)**	(0.043)**	(0.043)**
		[-0.077]	[-0.083]	[-0.080]	[-0.085]	[-0.083]
Public		-0.0178	-0.0302	-0.0425	-0.0254	-0.0431
		(0.0693)	(0.0722)	(0.0786)	(0.0845)	(0.0877)
		[-0.0069]	[-0.0118]	[-0.0166]	[-0.0099]	[-0.0168]
Log Enrollment		-0.0195	-0.0122	-0.0338	-0.0506	-0.0417
		(0.0306)	(0.0319)	(0.0345)	(0.0362)	(0.0371)
		[-0.0076]	[-0.0048]	[-0.0132]	[-0.0197]	[-0.0163]
Graduation Rate		0.437	0.493	0.622	0.822	0.882
		(0.209)*	(0.222)*	(0.252)*	(0.273)**	(0.281)**
		[0.171]	[0.193]	[0.243]	[0.321]	[0.344]
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	4762	4191	3825	3823	3825	3823

Notes: The table shows probit estimates of equation (1b) using 1996/2001 longitudinal weights. Standard errors are in parentheses. The brackets contain marginal effects evaluated at the sample mean for continuous variables and the discrete change in probability at the sample mean for dummy variables. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 7B: Effect of Racial Variety on Community Service

Variable	(1)	(2)	(3)	(4)	(5)	(6)
College Racial Variety	-0.201	-0.439	-0.459	-0.640	-0.520	-0.482
	(0.141)	(0.179)*	(0.208)*	(0.253)*	(0.307)	(0.316)
	[-0.078]	[-0.172]	[-0.179]	[-0.250]	[-0.203]	[-0.188]
HS Racial Variety			0.0826	-0.0355	-0.0416	-0.0749
			(0.1235)	(0.1397)	(0.1331)	(0.1425)
			[0.0323]	[-0.0138]	[-0.0162]	[-0.0292]
SAT (divided by 100)		0.0865	0.0925	0.0829	0.0829	0.0800
		(0.0120)**	(0.0126)**	(0.0130)**	(0.0131)**	(0.0133)**
		[0.0338]	[0.0361]	[0.0323]	[0.0323]	[0.0312]
Male		-0.199	-0.215	-0.206	-0.218	-0.213
		(0.040)**	(0.042)**	(0.043)**	(0.043)**	(0.043)**
		[-0.078]	[-0.084]	[-0.080]	[-0.085]	[-0.083]
Public		-0.0126	-0.0214	-0.0336	-0.0137	-0.0329
		(0.0693)	(0.0722)	(0.0786)	(0.0843)	(0.0875)
		[-0.0049]	[-0.0084]	[-0.0131]	[-0.0054]	[-0.0129]
Log Enrollment		-0.0170	-0.0114	-0.0306	-0.0501	-0.0409
		(0.0306)	(0.0318)	(0.0345)	(0.0363)	(0.0372)
		[-0.0067]	[-0.0045]	[-0.0119]	[-0.0195]	[-0.0159]
Graduation Rate		0.437	0.506	0.639	0.863	0.916
		(0.204)*	(0.216)*	(0.248)**	(0.268)**	(0.276)**
		[0.171]	[0.198]	[0.249]	[0.337]	[0.357]
Selectivity Dummies?	no	yes	yes	yes	yes	yes
Urban/Rural Dummies?	no	yes	yes	yes	yes	yes
State of Residence?	no	no	no	yes	no	yes
State of Institution?	no	no	no	no	yes	yes
N	4762	4191	3825	3823	3825	3823

Notes: The table shows probit estimates of equation (1b) using 1996/2001 longitudinal weights. Standard errors are in parentheses. The brackets contain marginal effects evaluated at the sample mean for continuous variables and the discrete change in probability at the sample mean for dummy variables. A single asterisk denotes statistical significance at the 5% level, and a double asterisk denotes significance at the 1% level. All models also include an additive constant.

Table 8: Effect of Diversity on Earnings Accounting for Selection on Unobservables

	2001 Salary		2000 Income	
	URM Share	Racial Variety	URM Share	Racial Variety
rho	(1)	(2)	(3)	(4)
-.4	5.53	3.23	9.55	5.82
	(0.26)	(0.16)	(0.40)	(0.24)
-.3	4.02	2.32	6.97	4.25
	(0.25)	(0.15)	(0.39)	(0.24)
-.2	2.66	1.50	4.64	2.83
	(0.25)	(0.15)	(0.38)	(0.23)
-.1	1.39	0.726	2.45	1.50
	(0.24)	(0.148)	(0.37)	(0.23)
0	0.147	-0.023	0.321	0.203
	(0.248)	(0.150)	(0.375)	(0.228)
.1	-1.09	-0.773	-1.80	-1.09
	(0.24)	(0.148)	(0.37)	(0.23)
.2	-2.37	-1.55	-4.00	-2.42
	(0.25)	(0.15)	(0.38)	(0.23)
.3	-3.73	-2.37	-6.33	-3.84
	(0.25)	(0.15)	(0.39)	(0.24)
.4	-5.23	-3.28	-8.91	-5.41
	(0.26)	(0.16)	(0.40)	(0.24)
Interval Estimate	[.231,.302]	[.078,.651]	[-.574,.208]	[.055,.139]
Estimate of Rho	.006	-.077	.035	.007

Notes: The table shows estimates of system of equations (2a). The final row shows an unweighted interval estimate of the diversity variable in the outcome equation based on (1) single-equation least squares and (2) imposing the condition that selection on unobservables equals selection on observables. The estimate of rho is shown underneath the estimate that imposes the equal selection condition. The other rows use 1996/2001 longitudinal weights to estimate the system imposing the value in the "rho" column as the correlation between the errors. The table displays the coefficient estimate and standard error on the diversity variable in the outcome equation. The outcome variable is 2001 salary in columns 1-2 and 2000 income in columns 3-4. The diversity variables are defined in Section IIIB of the text; columns 1 and 3 use the first measure, and columns 2 and 4 use the second measure. The models include the full set of control variables from column 6 of tables 2-7. Asterisks for significance are omitted, as most coefficient estimates are highly significant.

Table 9: Effect of Diversity on Satisfaction with College Accounting for Selection on Unobservables

	Racial Climate		Intellectual Climate	
	URM Share	Racial Variety	URM Share	Racial Variety
rho	(1)	(2)	(3)	(4)
-.4	11.8	7.90	10.9	6.48
	(0.9)	(0.49)	(1.0)	(0.61)
-.3	9.26	6.32	8.35	4.94
	(0.87)	(0.47)	(1.01)	(0.59)
-.2	6.73	4.76	5.84	3.41
	(0.85)	(0.46)	(0.98)	(0.57)
-.1	4.22	3.20	3.34	1.88
	(0.84)	(0.45)	(0.97)	(0.56)
0	1.72	1.67	0.849	0.349
	(0.83)	(0.45)	(0.953)	(0.555)
.1	-0.775	0.136	-1.64	-1.18
	(0.827)	(0.447)	(0.96)	(0.56)
.2	-3.29	-1.42	-4.14	-2.71
	(0.82)	(0.44)	(0.94)	(0.55)
.3	-5.82	-2.98	-6.65	-4.25
	(0.80)	(0.43)	(0.92)	(0.54)
.4	-8.37	-4.57	-9.17	-5.79
	(0.77)	(0.42)	(0.89)	(0.52)
Interval Estimate	[1.51,2.83]	[.300,1.62]	[-.583,.386]	[-1.79,.095]
Estimate of Rho	-.054	.088	.037	.127

Notes: The table shows estimates of model (2b). The final row shows an unweighted interval estimate of the diversity variable in the outcome equation based on (1) a single-equation probit and (2) imposing the condition that selection on unobservables equals selection on observables. The estimate of rho is shown underneath the estimate that imposes the equal selection condition. The other rows use 1996 cross-section weights to estimate the system imposing the value in the "rho" column as the correlation between the errors. The table displays the coefficient estimate and standard error on the diversity variable in the outcome equation. The outcome variable is a dummy for being satisfied with the racial climate at the institution in columns 1-2 and a dummy for being satisfied with the intellectual climate at the institution in columns 3-4. The diversity variables are defined in Section IIIB of the text; columns 1 and 3 use the first measure, and columns 2 and 4 use the second measure. The models include the full set of control variables from column 6 of tables 2-7. Asterisks for significance are omitted, as most coefficient estimates are highly significant.

Table 10: Effect of Diversity on Political and Civic Behavior Accounting for Selection on Unobservables

	Voted in 2000		Performed Community Service	
	URM Share	Racial Variety	URM Share	Racial Variety
rho	(1)	(2)	(3)	(4)
-.4	10.3	6.82	8.60	5.46
	(0.6)	(0.35)	(0.57)	(0.35)
-.3	7.92	5.30	6.27	3.98
	(0.54)	(0.34)	(0.55)	(0.33)
-.2	5.54	3.79	3.92	2.50
	(0.52)	(0.33)	(0.54)	(0.32)
-.1	3.17	2.28	1.57	1.01
	(0.51)	(0.32)	(0.53)	(0.32)
0	0.813	0.784	-0.784	-0.482
	(0.512)	(0.321)	(0.524)	(0.316)
.1	-1.55	-0.714	-3.14	-1.97
	(0.51)	(0.320)	(0.52)	(0.31)
.2	-3.92	-2.22	-5.49	-3.46
	(0.50)	(0.32)	(0.52)	(0.31)
.3	-6.30	-3.73	-7.83	-4.95
	(0.49)	(0.31)	(0.51)	(0.31)
.4	-8.69	-5.26	-10.2	-6.42
	(0.48)	(0.30)	(0.5)	(0.30)
Interval Estimate	[-.292,.360]	[.316,.399]	[-.916,-.787]	[-.279,.092]
Estimate of Rho	.029	.006	.005	-.025

Notes: The table shows estimates of model (2b). The final row shows an unweighted interval estimate of the diversity variable in the outcome equation based on (1) a single-equation probit and (2) imposing the condition that selection on unobservables equals selection on observables. The estimate of rho is shown underneath the estimate that imposes the equal selection condition. The other rows use 1996/2001 longitudinal weights to estimate the system imposing the value in the "rho" column as the correlation between the errors. The table displays the coefficient estimate and standard error on the diversity variable in the outcome equation. The outcome variable is a dummy for having voted in the 2000 presidential election in columns 1-2 and a dummy for having performed community service in the past year in columns 3-4. The diversity variables are defined in Section IIIB of the text; columns 1 and 3 use the first measure, and columns 2 and 4 use the second measure. The models include the full set of control variables from column 6 of tables 2-7. Asterisks for significance are omitted, as most coefficient estimates are highly significant.

Chapter 3

The Effects of Affirmative Action Bans on College Enrollment and Educational Attainment

3.1 Introduction

Affirmative action in college admissions is one of today's most contentious social policy issues. Its supporters view it as a just response to past or present discrimination and stress the social benefits of producing minority role models and leaders, while its opponents contend that it is an impediment to achieving a race-blind society and may even be harmful to those it is intended to directly benefit. The issue has been in the headlines as affirmative action has been limited in recent years by ballot initiatives in some states and by court decisions in others, and it is likely to remain there as voters in several states decide on the issue in 2008.

This chapter addresses the question of how affirmative action bans affect college enrollment and educational attainment. If affirmative action raises the probability of admission for minorities at particular universities, then it is plausible that eliminating it will reduce minority enrollment. However, there are several factors that may either magnify or diminish the impact of what happens at the admissions stage. First, eliminating affirmative action may have an effect on the behavior of potential students at either the application stage or the enrollment stage. Probabilities of being admitted should affect the number and mix of colleges a student applies to and, moreoever, an affirmative action ban may make minorities feel unwelcome and deter them from attending. Conversely, some individuals may be inclined to attend a university where they know that race played no role in their admission decision. Second, universities may respond to an affirmative action ban by implementing policies that lessen its impact. For example, they may conduct greater outreach, decide to admit a larger number of students, or place greater weight on high school class rank in admissions. Third, even if an affirmative action ban reduces enrollment at a particular selective university, it is not clear what happens to those who are crowded out. Do they attend another selective university, do they "cascade down" to less-selective institutions, or do they prefer to attend no college at all rather than attend their second choice? With all these issues in mind, it is not what clear what the effect of an affirmative action ban on enrollment actually is.

This chapter uses information on which states have affirmative action bans in place in which years along with data on college enrollment from the Current Population Survey (CPS) and data on educational attainment from the American Community Survey (ACS) in order to estimate the effects of affirmative action bans on college enrollment and educational attainment. I use a triple difference strategy that uses whites as a comparison group for underrepresented minorities and that exploits variation in affirmative action bans over states and across time. I detect no adverse impact of affirmative action bans on overall minority college attendance rates and educational attainment relative to whites, and I find no effect of affirmative action bans on minority enrollment in public colleges or four-year colleges.

The rest of this chapter is organized in the following manner: Section 3.2 places this research in the context of previous research, Section 3.3 describes the data and empirical methods I employ, Section 3.4 discusses the results, and Section 3.5 concludes.

3.2 Relation to Previous Research

Conceptually, an individual's college choice can be broken into four stages: the participation stage, the application stage, the admissions stage, and the enrollment stage. The participation stage involves the decision of whether to apply for college or not. If an individual decides to participate, then the next stage is the choice of where to apply. Decisions in the third stage are in the hands of admissions committees and involve choosing which students are admitted among those who apply. In the fourth stage, students make choices about which college, if any, to attend among those to which they have been admitted. Affirmative action bans affect the third stage directly and, as discussed in the introduction, may also have indirect effects at other stages.

Several recent studies have examined how affirmative action bans in California and Texas have affected decisions at the first stage or at the second stage conditional on having reached that stage. Dickson (2006), using a panel of Texas high schools, finds that the percentage of blacks and Hispanics who took the SAT fell when affirmative action was banned and did not recover when Texas implemented a policy to admit those in the top 10% of their high school class to any public university in the state. Moreover, a lower percentage of whites took the test under the 10% plan but not after the initial ban on affirmative action. Card and Krueger (2005) use data on SAT-takers in California and Texas to estimate how score-sending behavior of minorities changed relative to nonminorities over the time period that affirmative action bans went into effect in those states. They find little impact of affirmative action bans on where students send their scores, which suggests that affirmative action does not have an effect on the second stage of the college choice process for those who have reached that stage. In contrast, Long (2004a) finds that the gap between underrepresented minorities and others in sending SAT scores to top quintile colleges widens in California relative to control states when affirmative action was discontinued in California, although he does not find a statistically significant effect for Texas.[1] Although both Card and Krueger (2005) and Long (2004a) make use of high-quality individual-level data to take a focused look at one of the stages of the college choice process, they do share some common limitations. First, the samples in both papers are limited to those who take the SAT, so the results may be misleading if an affirmative action ban also affects whether people take the SAT. Second, although the decision of where to apply and send test scores is a stage in the college choice process, SAT-sending behavior is not the ultimate outcome of interest for policy.

Another set of papers uses data from Texas to focus on the third and fourth stages of the college choice. Tienda et al (2003) show that the odds of admission among applicants fell at the University of Texas at Austin and Texas A&M for minorities relative to whites after affirmative action was banned. They also generally find a negative impact on minority enrollment relative to white enrollment among those who were admitted to the two universities. However, the sample used to produce these results

[1] There are several differences in methodology between Card and Krueger (2005) and Long (2004a) that could lead to differences in results. First, Card and Krueger examine application to a small number of particular schools, whereas Long examines application to schools broadly defined by quintile of standardized test scores. Second, Card and Krueger use a dummy for whether someone applies to a college as their main lefthand side variable, and Long uses a count of the number of colleges an individual applies to. Third, Card and Krueger use data for more years (1994-2001) than Long does (1996 and 1999). Fourth, Card and Krueger use a difference-in-differences approach based on race and year, but Long incorporates a third dimension by using other states as controls.

is a selected one, and the effects in the population as a whole may differ. Kain, O' Brien, and Jargowsky (2005) find that, among underrepresented minorities in Texas who attend a public institution of higher education within the state, the affirmative action ban had a negative effect on the probability of enrolling in selective institutions; moreover, this effect was not reversed by the Top 10% plan. Bucks (2005) also analyzes college choice among high school students from Texas and finds a lower probability among underrepresented minorities and a higher probability among others of enrolling in selective in-state public institutions in the post-affirmative action period. Neither Kain, O'Brien, and Jargowsky (2005) nor Bucks (2005) is able to determine what happens to those who do not attend a public college in Texas. This is a limitation because there would be different implications in the case where an affirmative action ban at public universities causes people to attend out-of-state or private universities than in the case where it deters them from attending any college.

This chapter estimates the effects of affirmative action at the enrollment stage. It differs from previous research in several respects. First, I estimate the effects of affirmative action bans on a random sample of the college-aged population rather than limiting the sample to those who have taken the SAT, applied to a particular college, or chosen to attend a public college in a particular state. Second, I estimate the effects on actual enrollment decisions and educational attainment rather than on SAT-sending. Third, I take a broad look at all states rather than focusing on one particular state. A limitation is the lack of information on the particular college attended.

3.3 Data and Empirical Methods

The data used in this chapter come from Current Population Survey (CPS) October School Enrollment Supplement files and from the 2005 American Community Survey (ACS). My samples consist of whites, blacks, and Hispanics who were 18 years old between 1995 and 2003.

I pool the October CPS for each year between 1995 and 2003 to estimate the relationship between affirmative action bans and contemporaneous school enrollment of 18-year-olds. The CPS data allow me to determine whether someone attends college and, if so, whether that college is public or private and whether it is two-year or four-year. A useful feature of the CPS is that college students who are dependents of their parents are coded as being from the state where their parents live; thus, I am able to examine the effects of an affirmative action ban in the state in which an individual presumably resided while a senior in high school.[2] Table 1A displays summary statistics for the CPS data.

The 2005 ACS public use file is a 1% random sample of the United States population. I use these data to estimate the relationship between whether an affirmative action ban is in place at age 18 and educational attainment. As current state of residence may be an outcome of affirmative action bans and state at age 18 is unavailable, I link the data on affirmative action bans to the individual-level ACS data with state of birth. Thus, there may be some mismatch between what I take to be the relevant state for a person and what the relevant state actually is.[3] Table 1B displays summary statistics for the ACS

[2] This assumes that parents do not move from one state to another after their child graduates from high school and also that the children themselves do not establish residency in another state.

[3] There may also be some mismeasurement in what year someone is 18. The ACS is conducted throughout the year, and the public use data does not contain information on the month the survey is taken. Thus, the age and quarter of birth variables in the data are insufficient to recover the year someone is 18. I assign the year at age 18 through the formula *yearat18*=2005-*age*+*18*. Moreover, even if the year at age 18 were

data. It is noteworthy that a substantially larger proportion of those in the ACS have attended college than are currently attending college in the CPS data. This may be because some people in the CPS data set are still in high school, and it may also indicate that many people do not begin college immediately after finishing high school.

Table 2A shows which states have affirmative action bans in place for the fall admissions cycle for each year between 1995 and 2003. California, Florida, Texas, and Washington had an affirmative action ban in place at some point in this time period. My coding is consistent with previous studies and is based on the year the flagship public university in a state ended affirmative action in admissions.[4] The control states are the remainder of the continental 48 states with the exception of five states (Alabama, Georgia, Louisiana, Michigan, and Mississippi) that are in jurisdictions where there was important affirmative action litigation but that did not have outright bans on affirmative action.[5] It is also noteworthy that California, Florida, and Texas all implemented policies whereby achieving a certain high school class rank guaranteed acceptance at public universities. Table 2B shows which states have "percentage plans" in which years. It is difficult to disentangle the effects of affirmative action bans and these percentage plans, partly because the two are highly collinear and partly because the percentage plans are potentially an outcome of banning affirmative action.[6] And if they are in fact an effect of banning affirmative action, then their effects should be attributed to affirmative action

measured correctly, it is not necessarily the relevant year because some people finish high school and begin college earlier or later than 18.

[4] Some universities in Florida and Texas ended affirmative action one year earlier. As a robustness check, I also estimate models that code the Florida and Texas bans as beginning one year earlier.

[5] Including these five states in the analysis does not cause any substantive changes in the results, although it does have a moderate impact on the number of blacks in the sample.

[6] Long (2004b) writes, "There have been no states that have implemented an x% program without first dropping their affirmative action policies. Thus, these x% programs appear to be devised, implicitly or explicitly, to ameliorate the adverse effects of the elimination of race-based preferences."

when estimating the reduced-form effects of banning affirmative action on enrollment and educational attainment.[7] In any case, previous research has found them to be ineffective in increasing minority enrollment.[8] The appendix contains more information about states' affirmative action policies and percentage plans.

I estimate triple difference linear probability models that exploit variation over time and state in affirmative action bans and use whites as a comparison group for blacks and Hispanics.[9] The models are of the form

$$y_{ist} = ban_{st}URM_i\alpha + (\beta_{1s} + \lambda_{1t})black_i + (\beta_{2s} + \lambda_{2t})Hispanic_i + \theta male_i + \phi_{st} + \varepsilon_{ist} \quad (1)$$

Here y_{ist} is an outcome for individual *i* from state *s* in year *t*, ban_{st} is a dummy for state *s* having an affirmative action ban in place in year *t*, $black_i$ and $Hispanic_i$ are race dummies, and URM_i is the sum of the black and Hispanic dummies.[10] The parameter of interest is α, the effect of an affirmative action ban on outcomes for minorities. The second and third terms of the sum denote a full set of state-specific and year-specific race effects. The variable $male_i$ is a male dummy and θ is its coefficient, ϕ_{st} is a full set of state by year interactions, and ε_{ist} is the error term.[11]

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[7] The same argument could be made about increased recruiting efforts or scholarship programs such as the University of Texas at Austin's Longhorn Scholars program and Texas A&M's Century Scholars program, both of which are targeted toward students at disadvantaged high schools.

[8] See Long (2004b) and Kain, O'Brien, and Jargowsky (2005). One reason for this is that schools are not completely segregated, and so fewer than x% of minorities are in the top x% of their high school class. Another reason is that those that are toward the top of their class would be admitted even in the absence of the policy.

[9] Using whites as a comparison group for minorities is not ideal because whites may be affected by affirmative action bans, albeit likely to a lesser extent that minorities. However, stratifying by race and estimating difference-in-differences models using state and time variation gave unstable estimates. Moreover, both Card and Krueger (2005) and Long (2004a) employ strategies that difference by race. If whites are positively affected by affirmative action bans, then these strategies bias the results in the direction of finding larger negative effects on minorities.

[10] The CPS began allowing multiple races toward the end of the sample period. I code as black all individuals who report being at least part black, and I code as white only those who report being white and no other race.

[11] There are no main effects for race, as the equation is written containing a complete set of state-specific and year-specific race effects. Similarly, the main effects of state and time are accounted for by the full set

3.4 Results

Table 3 explores the relationship between affirmative action bans and college attendance using CPS data. All regression estimates are weighted, and tables report standard errors that are robust to clustering at the state level.[12] The estimate of equation (1) reported in the first column of the first row suggests that, relative to whites, an affirmative action ban is associated with a 3.2 percentage point *higher* rate of college attendance among underrepresented minorities. A scenario under which this could occur is if bans do not affect the decision to attend college among those who would have attended in the absence of the ban but result in outreach efforts that cause additional minorities to attend college. However, the estimate is imprecise and not statistically distinguishable from zero. Column 2 replaces the full set of state*black and state*Hispanic dummies with interactions between a ban state (California, Florida, Texas, and Washington) dummy and the race dummies. The standard error becomes somewhat smaller, and the point estimate becomes much closer to zero. Column 3, which modifies column 1 by replacing the full set of state*year interactions with interactions of a ban state dummy and year, also displays a very small point estimate. Column 4, which makes both modifications, gives further evidence that affirmative action bans do not reduce college attendance rates of minorities relative to whites.

Although affirmative action bans at public universities may not reduce the overall rate of college attendance among minorities, they may affect the type of college attended. For instance, they may cause a shift away from public colleges or from four-year colleges to two-year colleges. However, the remaining rows of Table 3 suggest that this is not the

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of state by year interactions. Moreover, the main effect of ban_{st} is accounted for by the state by year interactions.

[12] CPS weights are normalized to sum to 1 within a year.

case, although I cannot rule out the possibility that affirmative action bans shift minorities from more-selective to less-selective in-state public universities or to out-of-state public universities.[13]

Table 4 turns to the analysis of educational attainment using ACS data. The pattern of results in the first row for having ever attended college is analogous to that in Table 3 for currently attending college, with insignificant positive estimates in the first two columns and insignificant negative estimates in the remaining columns. The second and third rows of Table 4 estimate the effects of affirmative action bans on receiving an associate's degree and on receiving a bachelor's degree or higher. If an affirmative action ban shifts minorities away from four-year colleges, it may increase the proportion who have an associate's degree; however, the estimates in the second row suggest that this is not the case. An affirmative action ban may lower the probability of receiving a bachelor's degree or higher for minorities if it displaces them from four-year colleges or shifts them to colleges that have lower graduation rates. Alternatively, an affirmative action ban may increase minority graduation rates if it reduces "mismatch" between minorities and the type of college they attend. Nonetheless, the results in the third row of Table 4 do not give evidence for either of these possibilities.

Some universities in Florida and Texas ended affirmative action admissions policies one year before the state's flagship campus did.[14] This raises the possibility that my "ban" variable is not a completely accurate measure of the pressure facing public

[13] This raises the possibility that there are general equilibrium effects. For instance, minorities crowded out of a public university in their home state may decide to attend college in another state, thereby affecting the market for higher education in that other state. This would be a limitation for the across-state comparison of the triple difference strategy.

[14] Card and Krueger (2005) raise this point in the context of Texas. See the timeline in Long (2007) for the case of Florida.

universities to discontinue race-conscious admissions policies. To determine whether the results are robust to this possibility, Tables 5 and 6 report specifications that are identical to those in Tables 3 and 4 but that code the bans in Florida and Texas as beginning one year earlier. The estimates in Table 5 are generally smaller in magnitude than the corresponding estimates in Table 3, although the picture that emerges is that affirmative action bans still do not appear to be associated with lower college attendance rates or educational attainment among underrepresented minorities.

3.5 Conclusion

This chapter finds no evidence for an effect of affirmative action bans on overall minority college attendance rates and educational attainment relative to whites. I also find no evidence that the bans affect minority enrollment in public colleges or four-year colleges. However, this does not exclude the possibility that the bans shift minorities into less-selective public colleges.

The affirmative action debate will likely continue in the United States for years to come. As long as there remains racial inequality in income and educational attainment, affirmative action in college admissions will be viewed as a policy lever that can potentially help correct the imbalance. But in recent years, it appears that the tide is beginning to turn against affirmative action. This has prompted concern that racial inequality in education may widen. However, keeping in mind the caveat that I consider only a limited set of outcomes, the results of this chapter do not give support to these concerns.

A. Appendix on Affirmative Action Policies and Percentage Plans

California. California's ban went into effect in 1998 following an earlier decision by that state's Board of Regents and after California voters passed Proposition 209. The Board of Regents' decision has since been overturned and now Proposition 209 is what holds the ban in place. Under California's "Eligibility in the Local Context" policy, those in the top four percent of their high school class are guaranteed admission to at least one campus of the University of California.

Florida. Florida's affirmative action ban is a result of then-Governor Jeb Bush's One Florida plan. Under Florida's Talented 20 Program, those in the top twenty percent of their high school class are guaranteed admission to at least one public university in Florida.

Georgia. The University of Georgia's particular affirmative action policy was struck down by a circuit court ruling, although opinions differ as to whether this banned affirmative action in that circuit or not (Hebel (2001b)). Nonetheless, the University of Georgia eliminated affirmative action beginning in the Fall 2002 admissions cycle (Hebel (2001a)). Alabama is in the same circuit as Georgia and is also dropped from the sample. The other state in that circuit is Florida, which already had its own affirmative action ban.

Michigan. The University of Michigan made major revisions to its affirmative action policy to make it more flexible in the wake the Supreme Court's 2003 *Gratz v. Bollinger* decision (University of Michigan News Service (2003)), but it did not eliminate affirmative action at that time. However, public universities in Michigan are currently not allowed to use affirmative action in admissions as a result of Michigan voters passing Proposal 2 in November 2006.

Texas. Texas' affirmative action ban went into place as a result of a ruling by the Fifth Circuit Court of Appeals in the case of *Hopwood v. State of Texas*. This ruling was overturned by the Supreme Court's 2003 decisions in *Gratz v. Bollinger* and *Grutter v. Bollinger*, and universities in Texas are now permitted to use affirmative action. The University of Texas at Austin reintroduced affirmative action for its Fall 2005 admissions cycle (University of Texas at Austin Office of Admissions (2006)). Under Texas' law HB 588, those in the top ten percent of their high school class are guaranteed admission into any public university in Texas. Louisiana and Mississippi are part of the same circuit as Texas, in which affirmative action was outlawed as a result of the *Hopwood* ruling, but were under federal desegregation orders that pointed them in a conflicting direction (Healy (1998)).

Washington. Washington's affirmative action ban is due to voters passing Initiative 200.

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Table 1: Summary Statistics

A. CPS Sample

Variable	Overall (1)	Black or Hispanic (2)	Black (3)	Hispanic (4)
Attends Any College	0.403	0.289	0.325	0.260
Attends Public College	0.319	0.246	0.270	0.228
Attends Four-Year College	0.268	0.169	0.216	0.131
Attends Four-Year Public College	0.192	0.132	0.166	0.104
Male	0.508	0.503	0.489	0.514
N	12915	3348	1402	1946

B. ACS Sample

Variable	Overall (1)	Black or Hispanic (2)	Black (3)	Hispanic (4)
Has Attended College	0.590	0.471	0.473	0.468
Has Associate's Degree	0.079	0.059	0.056	0.063
Has Bachelor's Degree or Higher	0.197	0.106	0.107	0.104
Male	0.495	0.476	0.455	0.498
N	201945	42706	19352	23354

Notes: Table shows weighted means of variables and sample size by race and ethnicity for the Current Population Survey data and the American Community Survey data. All variables are binary.

Table 2: States with Affirmative Action Bans and Percentage Plans

A. States with Affirmative Action Bans

	95	96	97	98	99	00	01	02	03
California				X	X	X	X	X	X
Florida							X	X	X
Texas			X	X	X	X	X	X	X
Washington					X	X	X	X	X

B. States with Percentage Plans

	95	96	97	98	99	00	01	02	03
California							X	X	X
Florida						X	X	X	X
Texas				X	X	X	X	X	X

Table 3: Effects of Affirmative Action Bans on College Attendance (CPS)

	Full State*Year		Parsimonious State*Year	
	Full State*Race	Parsimonious State*Race	Full State*Race	Parsimonious State*Race
Outcome	(1)	(2)	(3)	(4)
Attends Any College	0.0322	0.0014	-0.0007	-0.0158
	[0.0414]	[0.0357]	[0.0502]	[0.0421]
Attends Public College	0.0575	0.0160	0.0344	0.0071
	[0.0385]	[0.0317]	[0.0432]	[0.0361]
Attends Four-Year College	0.0153	0.0047	-0.0003	-0.0045
	[0.0362]	[0.0301]	[0.0386]	[0.0312]
Attends Four-Year Public College	0.0338	0.0196	0.0303	0.0192
	[0.0355]	[0.0290]	[0.0340]	[0.0262]
N	12915	12915	12915	12915

Notes: Each cell corresponds to a separate (weighted) regression estimate of equation (1). Each row corresponds to an outcome, and each column corresponds to a set of covariates. The table displays estimates for the ban*URM variable, with standard errors corrected for clustering at the state level in brackets. All models include a gender dummy, year dummies, race dummies, state dummies, and interactions of year and race. "Full State*Year" models contain a full set of interactions of state and year, whereas "Parsimonious State*Year" models contain interactions of a ban state dummy and year. "Full State*Race" models contain a full set of interactions of state and race, and "Parsimonious State*Race" models contain interactions of a ban state dummy and race.

Table 4: Effects of Affirmative Action Bans on Educational Attainment (ACS)

	Full State*Year		Parsimonious State*Year	
	Full State*Race	Parsimonious State*Race	Full State*Race	Parsimonious State*Race
Outcome	(1)	(2)	(3)	(4)
Has Attended College	0.0122	0.0118	-0.0062	-0.0033
	[0.0224]	[0.0208]	[0.0215]	[0.0198]
Has Associate's Degree	-0.0003	-0.0039	-0.0008	-0.0041
	[0.0061]	[0.0059]	[0.0056]	[0.0055]
Has Bachelor's Degree or Higher	-0.0091	0.0015	-0.0247	-0.0122
	[0.0168]	[0.0097]	[0.0246]	[0.0164]
N	201945	201945	201945	201945

Notes: Each cell corresponds to a separate (weighted) regression estimate of equation (1). Each row corresponds to an outcome, and each column corresponds to a set of covariates. The table displays estimates for the ban*URM variable, with standard errors corrected for clustering at the state level in brackets. All models include a gender dummy, year dummies, race dummies, state dummies, and interactions of year and race. "Full State*Year" models contain a full set of interactions of state and year, whereas "Parsimonious State*Year" models contain interactions of a ban state dummy and year. "Full State*Race" models contain a full set of interactions of state and race, and "Parsimonious State*Race" models contain interactions of a ban state dummy and race.

Table 5: Effects of Affirmative Action Bans on College Attendance (CPS) - Alternate Coding of "Ban" Variable

	Full State*Year		Parsimonious State*Year	
	Full State*Race	Parsimonious State*Race	Full State*Race	Parsimonious State*Race
Outcome	(1)	(2)	(3)	(4)
Attends Any College	0.0139	0.0168	-0.0076	0.0030
	[0.0532]	[0.0356]	[0.0603]	[0.0421]
Attends Public College	0.0250	0.0148	0.0147	0.0103
	[0.0591]	[0.0371]	[0.0596]	[0.0404]
Attends Four-Year College	0.0108	0.0165	-0.0053	0.0050
	[0.0415]	[0.0255]	[0.0434]	[0.0257]
Attends Four-Year Public College	0.0169	0.0158	0.0133	0.0133
	[0.0470]	[0.0263]	[0.0429]	[0.0233]
N	12915	12915	12915	12915

Notes: Each cell corresponds to a separate (weighted) regression estimate of equation (1). Each row corresponds to an outcome, and each column corresponds to a set of covariates. The table displays estimates for the ban*URM variable, with standard errors corrected for clustering at the state level in brackets. All models include a gender dummy, year dummies, race dummies, state dummies, and interactions of year and race. "Full State*Year" models contain a full set of interactions of state and year, whereas "Parsimonious State*Year" models contain interactions of a ban state dummy and year. "Full State*Race" models contain a full set of interactions of state and race, and "Parsimonious State*Race" models contain interactions of a ban state dummy and race.

Table 6: Effects of Affirmative Action Bans on College Attendance (ACS) - Alternate Coding of "Ban" Variable

	Full State*Year		Parsimonious State*Year	
	Full State*Race	Parsimonious State*Race	Full State*Race	Parsimonious State*Race
Outcome	(1)	(2)	(3)	(4)
Has Attended College	0.0003	-0.0152	-0.0153	-0.0262
	[0.0194]	[0.0125]	[0.0202]	[0.0154]
Has Associate's Degree	-0.0011	-0.0032	-0.0015	-0.0033
	[0.0084]	[0.0073]	[0.0076]	[0.0067]
Has Bachelor's Degree or Higher	0.0001	0.0018	-0.0143	-0.0097
	[0.0150]	[0.0095]	[0.0235]	[0.0165]
N	201945	201945	201945	201945

Notes: Each cell corresponds to a separate (weighted) regression estimate of equation (1). Each row corresponds to an outcome, and each column corresponds to a set of covariates. The table displays estimates for the ban*URM variable, with standard errors corrected for clustering at the state level in brackets. All models include a gender dummy, year dummies, race dummies, state dummies, and interactions of year and race. "Full State*Year" models contain a full set of interactions of state and year, whereas "Parsimonious State*Year" models contain interactions of a ban state dummy and year. "Full State*Race" models contain a full set of interactions of state and race, and "Parsimonious State*Race" models contain interactions of a ban state dummy and race.