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Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion

Susan Dynarski

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Measuring the Effect of Student Aid on College Attendance and Completion

Susan M. Dynarski*

Kennedy School of Government, Harvard University and NBER

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Abstract

Does student financial aid increase college attendance, or simply subsidize costs for infra-marginal students? Settling the question empirically is a challenge, because aid is correlated with many characteristics that influence schooling decisions. A shift in financial aid policy that affects some youth but not others can provide an identifying source of variation in aid. In 1982, Congress eliminated the Social Security student benefit program, which at its peak provided grants totaling $3.9 billion a year (amounts are in constant 2000 dollars) to one out of eight college students. I use difference-in-differences analysis to evaluate the effect of this program on schooling outcomes. Using the death of a parent to proxy for Social Security beneficiary status, I find that the college attendance of the affected group dropped by more than a third, and schooling by two-thirds of a year. Offering $1,000 of grant aid increases the probability of attending college by 3.6 percentage points and years of completed schooling by a tenth of a year. Aid eligibility also appears to have a positive impact on school quality.

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I. Introduction

The United States spends billions of dollars each year on financial aid for college students. During the 1999-2000 school year, the federal government delivered $47 billion in grant aid and subsidized loans to students.[1] State governments spent an additional $59 billion in subsidizing their public universities. Despite the dollars devoted to aid programs such as the Pell Grant, there is little evidence that these subsidies serve their goal of increasing college attendance. The billions spent annually on aid may simply reduce the costs of infra-marginal students.

Determining whether aid expands access to college is an empirical challenge. The traditional approach has been to regress a person's educational attainment against the aid for which he is eligible and interpret the coefficient on aid as its casual effect. However, this is problematic, as aid is correlated with many characteristics that affect schooling. For example, Pell Grants are channeled toward low-income students, who are relatively unlikely to attend college. Omitting a variable such as income from the regression, or including it in the wrong functional form, will produce a biased estimate of the causal effect of aid on education. Further, if there are characteristics that are correlated with aid and education that are unobservable to the econometrician, then this approach has no hope of capturing the causal effect of aid.

In order to identify the effect of aid, we need a source of variation in aid that is plausibly exogenous to unobservable attributes that influence college attendance. A discrete shift in aid policy that affects some students but not others is one such source of exogenous variation. In this paper, I analyze the impact on college attendance and completed schooling of a major shift in federal financial aid policy that occurred in the early 1980s.

From 1965 to 1982, the Social Security Administration paid for millions of students to go to college. Under this program, the 18- to 22-year-old children of deceased, disabled or retired Social Security beneficiaries received monthly payments while enrolled full-time in college. Benefits were

[1] College Board (2000). Unless otherwise stated, all dollar amounts are in constant 2000 dollars.

generous: the average annual payment in 1980 to the child of a deceased parent was $6,700. The program was large: during the 1980-81 school year, the program distributed $3.9 billion to college students, while the largest grant program, the Pell Grant, distributed $5.0 billion. At the program's peak, 12 percent of full-time college students aged 18 to 21 were receiving Social Security student benefits.[2]

In 1981, Congress voted to eliminate the Social Security student benefit program. Program enrollment sank rapidly. By the 1984-85 academic year, program spending had dropped by $3 billion. Except for the introduction of the Pell Grant program in the early 1970s, and the various GI Bills, this is the largest and sharpest change in grant aid for college students that has ever occurred in the United States.

The program's sudden demise provides an opportunity to measure the incentive effects of financial aid. I use differences-in-differences methodology, proxying for benefit eligibility with the death of a parent during an individual's childhood. I find that the elimination of the Social Security student benefit program reduced college attendance probabilities by more than a third. The paper's estimates suggest that an offer of $1,000 in grant aid increases the probability of attending college by about 3.6 percentage points. Aid eligibility also appears to increase completed schooling, though these latter results are less precisely estimated.

The paper is organized as follows. Section II discusses the econometric difficulties of identifying the effect of aid on education. Section III provides background on the Social Security student benefit program and the data set used in the analysis. Section IV discusses the identification strategy. Section V presents results on the effect of aid eligibility on the probability of attending college and completed schooling. Section VI provides some descriptive evidence on how aid affects the type of college students choose to attend. In Section VII, I discuss the estimates' study's external validity. Section VIII concludes.

[2] Statistics in this paragraph are drawn from data in Table 54 in Social Security Administration (1982), Table A in College Board (1998) and Table 174 in National Center for Education Statistics (1998).

II. Background

We are interested in the effect of aid on a person's educational attainment. This relationship can be expressed with the following reduced-form equation:

$$(1) \qquad S_i = \alpha + \beta AID_i + \varepsilon_i$$

Here, S_i is some measure of an individual's schooling, such as college attendance or completion and AID_i is the amount of student aid for which an individual is eligible. If aid is uncorrelated with ε_i, the unobserved determinants of schooling, then β can be interpreted as the effect of the offer of a dollar of aid on educational outcomes.

AID_i would be uncorrelated with ε_i if it were randomly assigned. However, in most cases, aid is offered to students on the basis of characteristics that have their own effect on the probability of college attendance. For example, the federal government uses aid to encourage the college attendance of needy youth. If these students are relatively unlikely to attend college, perhaps because of low levels of parental education or poor secondary schooling, then estimates of β based on this source of variation in aid will be biased downward. Conversely, since many colleges use merit scholarships to attract high-achieving students, the bias on β in Equation (1) may instead be positive.

We can attempt to eliminate this bias by adding to the regression a vector of regressors X_i:

$$(2) \qquad S_i = \alpha + \beta AID_i + \delta X_i + \varepsilon_i$$

Leslie and Brinkman (1988) review several dozen studies that estimate some form of equation (2). A typical study estimates a selection model using data on college students and their aid packages to impute aid offers for students who have not gone to college.[3] Common covariates include measures of financial resources, such as parental income, and measures of individual ability, such as standardized test scores. Whatever the particular strategy, these studies share the common assumption that controlling for covariates can effectively isolate the causal effect of aid. However, under plausible conditions, this approach will fail. First, data on relevant characteristics are frequently unobservable to the

[3] See, for example, Manski and Wise (1983).

econometrician. Examples of variables usually unobserved in survey data that affect both aid and college attendance include financial assets and ease of access to information about college. Second, even if the econometrician has access to a complete set of covariates, the estimate of β will still be biased if the (perhaps non-linear) relationship between covariates and attendance is incorrectly modeled.

In order to obtain an unbiased estimate of the effect of aid, we require a source of variation in aid that is plausibly exogenous to unobservable attributes that influence college attendance. A discrete shift in aid policy that affects some students but not others is one such source of exogenous variation. In the following section, I describe the policy shift that provides this paper's identifying variation in student aid.

III. The Social Security Student Benefit Program

In 1965, Congress established the Social Security student benefit program, which subsidized the college attendance of the children of deceased, disabled and retired Social Security beneficiaries. Prior to 1965, the Social Security Administration ended benefits to the children of Social Security beneficiaries once those children turned 18. With the new legislation, payments were continued while the child remained unmarried and enrolled full-time in school. Proponents of the new program argued that since parents generally subsidize children while they are in college so too should a program intended to replace the lost income of those parents. Note that, in 1965, need-based federal aid for college students was negligible; the large-scale federal aid programs were not established until the Seventies.[4]

As shown in Figure 1, the program grew rapidly. Program enrollment peaked in 1977 at 700,000.[5] In 1980, one in eight full-time college students aged 18 to 21 was a Social Security student beneficiary. Much of this growth was driven by students gaining eligibility through the retirement or disability of a

[4]This section draws on Committee on Ways and Means (1979 and 1982), Luzadis (1983) and Office of the Comptroller General (1979).

[5] Child benefits were converted to student benefits if a beneficiary turned 18 while in high school, and so some student beneficiaries were enrolled in high school rather than college. A 1973 Social Security Administration study showed that 79 percent of student beneficiaries were in post-secondary institutions (Springer, 1976). I apply this 79 percent estimate to the annual figures to obtain the numbers plotted in the figure and discussed in this paragraph.

parent; these students comprised 25 percent of student beneficiaries in 1965 but 40 percent in 1980. The growth in this population was driven by rising disability and retirement rates of prime-age men. By contrast, the number of beneficiaries eligible due to the death of a parent roughly tracked cohort size and overall college attendance rates.[6]

Student benefits were distributed as monthly lump sums, without reference to actual schooling costs, much like GI Bill benefits paid to college-going veterans. The size of the benefit was determined by the earnings history of the parent whose death, disability or retirement triggered Social Security payments to the family. This earnings history was used to determine the Primary Insurance Amount (PIA), a baseline benefit from which dependent and spousal benefits were calculated.[7] Especially when compared to other student aid, these benefits were extremely generous. The average annual benefit in 1980 for the child of a deceased parent was $6,700. By comparison, the average Pell Grant in the same year was $2,000 and the average guaranteed student loan was $4,500.[8] A subsidy of $6,700 is particularly striking when viewed in the context of contemporaneous tuition costs. In 1980, before the rapid increases in college prices of the past two decades, average tuition and fees were just $1,900 at public universities and $7,100 at private, four-year colleges.[9]

Students had to do very little in order to gain access to benefits. Several months before the 18th birthday of a child beneficiary, the Social Security Administration sent her a letter inquiring into her

[6] Beneficiary population figures are from Table 5.F.4 in Social Security Administration (1998).

[7] Each child of a deceased worker was eligible to receive 75 percent of the PIA, while the children of retired and disabled workers were eligible to receive 50 percent. If the benefits of family members summed to more than 175 percent of the PIA, each person's benefit was proportionally reduced until the total was under the family cap. A referee noted that this provides useful identifying variation, in that the number of siblings affects the generosity of a child's benefit. However, I find that the results of using this source of variation are extremely imprecise to be informative.

[8] Student beneficiaries were eligible for these traditional forms of student aid. In 1978, one-quarter received Pell Grants. Because Social Security benefits counted as income, rather than aid, in the federal financial aid formulas, a dollar in student benefits reduced other aid by only a nickel. However, colleges may have treated student benefits less generously in calculating their own scholarships. Such "crowd-out" of institutional aid would bias the paper's estimates toward zero.

[9] Average grant and loan figures are from Table 7 in College Board (1983). College tuition figures are from Table A-9 in College Board (1983).

college plans. A beneficiary who responded that she was already in college, or would be as of her 18th birthday, continued to receive the same benefits she had received as a child. The Social Security Administration asked schools for verification of enrollment on an annual basis. Benefit checks were mailed directly to the student each month, separate from payments delivered to the rest of her family.[10] Benefits continued until the beneficiary left college, married or turned 22.[11]

In 1981, Congress voted to eliminate the program. Opponents of the benefits argued than a now-extensive system of federal aid provided for the needs of needy college students. Under the terms of the program's elimination, anyone not enrolled as of May, 1982 was ineligible for future subsidies, while students enrolled in college had their payments severely reduced. By the 1983-84 academic year, program spending had dropped to $0.38 billion. Except for the introduction of the Pell Grant program in the early 1970s, and the various GI Bills, this is the largest and sharpest change in grant aid for college students that has ever occurred in the United States.

IV. Empirical Strategy and Data

I use difference-in-differences to analyze the effect of eligibility for Social Security student benefits on college attendance and completed schooling. The key estimating equation is the following:

$$(3) \quad y_i = \alpha + \beta (SS_i * Before_i) + \delta SS_i + \theta Before_i + \nu_i$$

where the dependent variable is a measure of college attendance or completion. SS_i is a binary variable that is set to one if a youth is the child of a Social Security beneficiary (the "eligible" group) and zero for all other youth (the "control" group). $Before_i$ is a binary variable that is set to one if a youth is a member of an age cohort potentially eligible for benefits. This specification controls for variance over time in

[10] These rules were operative during the period analyzed in the paper. In the early years of the program, the only verification of college enrollment status was the self-report of the student. And, before 1972, the student benefit was bundled into the family's benefit check.

[11] Benefits were continued until the end of the semester during which a beneficiary turned 22.

average college attendance rates, as well as for the average effect on attendance of being the child of a Social Security beneficiary.

The reduced-form effect of Social Security Student Benefits is identified by β. The identifying assumption is that any relative trend in the attendance of the children of Social Security beneficiaries is attributable to the withdrawal of student benefits. Note that β captures the effect on schooling decisions of aid *eligibility* rather than aid *receipt*. In the language of the experimentalist literature, β captures the effect of the *intention to treat*. Since policy-makers control the offer of aid, but not its take-up, β is the parameter of interest if we wish to predict the effect of altering aid policy.

The analysis focuses on children potentially eligible for student benefits due to the death of a parent. I do not focus on the children of disabled and retired parents because parental disability and retirement may be endogenously determined by the availability of student benefits.[12] That is, student benefits may encourage the retirement or disability of those parents who expect their children to go to college. The retired or disabled parents of college-age children may also re-enter the labor market upon the elimination of student benefits. This endogenous selection into and out of the eligible group will bias upward the difference-in-differences estimator. By contrast, it's doubtful that the lure of student benefits will encourage the parents of college-bound children to die, and the withdrawal of benefits certainly cannot bring them back to life.[13]

[12] Due to data limitations, the children of disabled and retired parents cannot be identified and are, by necessity, in the control group. As will be discussed later in the paper, this misclassification has little impact on the estimates. In analysis that is not shown here but is available in Dynarski (1999), I have used the age of an child's father to proxy for his retirement status and, therefore, the child's eligibility for student benefits. The results are similar to those shown in this paper.

[13] A referee suggests that mothers of college-going children may have made a greater effort to establish paternity of absent fathers, and inform children of the death of these fathers, when student benefits were available. However, financial incentives to establish paternity were still substantial even after 1982, since benefits for children under 18 were not eliminated. The data indicate do not suggest endogenous selection into survivor status. Specifically, the probability of having a deceased father is constant across age cohorts (4.8 and 4.9 percent for the "before" and "after" cohorts, respectively), and the time dummy has no predictive power in a regression of the deceased-father dummy against covariates.

I use the death of a person's father to proxy for Social Security eligibility and estimate the following equation:

$$(4) \quad y_i = \alpha + \beta(Father\ Deceased_i * Before_i) + \delta\, FatherDeceased_i + \theta\, Before_i + \nu_i$$

The variable $Father\ Deceased_i$ indicates whether a child's father died before the child turned 18. I focus on fathers for two related reasons: 90 percent of student beneficiaries were entitled to benefits through their fathers (Social Security Administration, 1982) and fathers are more likely to have a sufficient working history to generate survivor benefits upon death.

The data-set used in the analysis is the National Longitudinal Survey of Youth (NLSY); see the Data Appendix for details. Survey respondents in their senior year of high school in the springs of 1979, 1980 or 1981 form the "before" cohorts. Social Security dependents who are members of these early cohorts were eligible for student benefits upon high school graduation. The "after" cohorts are those who were seniors in the spring of 1982 or 1983. Social Security dependents in these cohorts were not eligible for student benefits upon high school graduation.[14]

Since cohort is defined by the year that one is a high school senior, those who don't complete junior year are excluded from the analysis. If I instead define cohort by the junior year, the results are substantively unchanged.[15] The junior-year approach excludes any bias produced by students dropping out of high school and enrolling in college in order to meet the May, 1982 deadline for benefit eligibility. These students would never appear as seniors in the NLSY. As a result, college-going children of deceased fathers in the "after" cohorts would be disproportionately excluded from the analysis, thereby biasing the estimates upward. Media reports and Congressional testimony of the era suggest that this was a widespread phenomenon, but there is little evidence of such behavior in the data.

[14] The sampling frame of the NLSY precludes expanding the sample to include younger cohorts. A few older cohorts could be added by using retrospective data on high school graduation. The point estimates are similar, and little precision gained, when these older cohorts are included, which suggests that the retrospective schooling measures are noisier than the contemporaneous measures.

[15] This implies that the program did not affect high school completion rates. As suggested by a referee, I have directly examined whether benefit eligibility affected high school graduation rates and found no significant effect.

In the NLSY, about five percent of children had a male parent die before the child turned 18. In order to gauge the plausibility of this figure, I examined the Social Security Administration's mortality tables for men born in 1930, who are roughly the age of the NLSY's fathers. Of those alive in 1960, the midpoint of NLSY births, six percent were dead 18 years later. This deceased-father variable therefore appears to be accurately capturing deaths of fathers during childhood.

Sample means, along with their standard errors, are presented in Table 1. The means are presented separately for the periods before and after the policy change and for the eligible and control groups. Children with deceased fathers grow up in relatively low-income families and are far more likely to live in single-parent households, explaining much of the income differential.[16] They are more likely to be black, due to the higher mortality rate of prime-age black men, and they have lower Armed Forces Qualification Test (AFQT) scores.[17]

But, as the last column shows, except in their schooling outcomes the differences between the two groups are stable across time. In that column are listed the changes in mean characteristics of the control group relative to the eligible group, along with the standard errors of these changes. During the period under analysis, there is no statistically significant change in the differences between children with living and dead fathers. Table 1 therefore provides evidence that the additivity assumption of difference-in-differences holds for this analysis. Nevertheless, in the regression analysis I will test for whether the estimates are sensitive to both fixed and changing differences between the two groups.

I use ordinary least squares (OLS) to estimate equation (4). Probit yields similar marginal effects. Standard errors in the tables are corrected for within-household correlation in error terms due to the presence of sibling pairs in the data, as well as for heteroskedasticity due to the dichotomous dependent

[16] Since some wives remarry after the husband's death, not all children with deceased fathers live in single-parent households. Children continued to be eligible for survivor benefits if their mothers remarry.

[17] Blacks are twice as likely to have a dead father (7.2 percent vs. 3.4 percent). If we assume a constant annual mortality rate for fathers for the eighteen years after a child's birth, the implied annual mortality rate is 0.4 percent for blacks and 0.2 percent for whites. This is consistent with data from the National Longitudinal Mortality Survey (NLMS), which shows that, among men aged 25 to 50, annual mortality rates are two to three times as high for blacks as whites. Thanks to Jeff Liebman for providing these NLMS estimates.

variable.[18] All regressions are weighted by the 1988 NLSY sample weights.[19] The results are unchanged if I drop the poverty over-sample (see Data Appendix) and run un-weighted regressions.

V. The Effect of Aid on Attendance and Completion

The first result is computed from the means in Table 1. The table shows probabilities of having entered college on a full-time basis at any time between the start of the survey and age 23, when everyone has aged out of student benefit eligibility. For the cohort of students who were high school seniors in 1979, 1980 and 1981, those with deceased fathers were more likely to attend college than their classmates: 56.0 percent had attended college by 1996, while 50.2 percent of seniors with living fathers had done so. For the younger cohort of students, seniors in 1982 and 1983, the pattern is reversed: only 35.2 percent of seniors whose fathers had died by the time they were 18 went to college, while 47.6 percent of their classmates attended. The probability of college enrollment dropped by more than a third for the group with deceased fathers (20.8 percentage points), while it barely dropped for other students (2.6 percentage points). The estimated effect of eligibility for Social Security Student Benefits on the probability of attending college is the difference in these two differences: 18.2 percentage points. The estimate is statistically significant at the six percent level.

I next use regression analysis in order to probe the robustness of this result. I include as covariates family size, income, parental education, and marital status of household head, all of which are measured when the youth is a high school senior.[20] AFQT score and state-of-residence dummies are also included; these variables are measured in the first year of the survey. Additional covariates are age (as of

[18] Because the NLSY was initiated as a household survey, there are multiple sibling pairs in the sample. While there are 3,545 people in my sample, they lived in just 2,500 households in the first year of the survey.

[19] I exclude the eight percent of the sample that exited before 1988, when questions about parental death were asked.

[20] Family income is imputed if missing; see the Data Appendix. A dummy indicating these imputed values is included in the regression. Dummies indicating whether either mother's or father's education is missing are also included.

the 1988 survey), race and gender. Further, I include two sets of interaction terms: 1) the interactions of the covariates just discussed with the "before" dummy and 2) their interactions with the deceased-father dummy.[21]

The interaction terms will absorb sources of bias. One potential source of bias is that the effect of covariates may change over time. For example, the effect of race may vary with time. A secular drop in the black college attendance rate coincides with the elimination of student benefits. Since youth with a deceased father are disproportionately black, this will bias upward the estimated effect of aid eligibility in specifications in which the effect of race is constrained to be constant over time. Similarly, if the college attendance of youth from low-income families is particularly sensitive to the business cycle, then we may confound the effect of aid with the effect of the 1981-82 recession on college-going among low-income youth.[22] Both of these examples suggest that the effect of covariates should be allowed to vary over time. Further, the effect of income may differ between the deceased-father group and the control group, which suggests that the effect of covariates should also be allowed to vary between the eligible and control groups.

Table 2 shows the results of a regression which includes all of these covariates and interactions. The results of the difference-in-differences without covariates are included in Column (1) for ease of comparison. The estimated effect of aid eligibility on attendance barely changes with the addition of this rich set of covariates: it is 21.9 percentage points, with a standard error of 10.2 percentage points. However, the explanatory power of the regression rises dramatically, from 0.002 to 0.339. This regression clearly captures many of the key determinants of college attendance; an R^2 of 0.339 is especially high for a linear probability model. The robustness of the point estimate to the inclusion of this extensive set of covariates provides strong support for the identifying assumptions of the paper.

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[21] The variables indicating missing data are also interacted with the before and deceased dummies.

[22] The repeal of the Middle Income Student Assistance Act (MISAA) may also induce an interaction between income and time. I have run a version of the specification that includes dummies corresponding to income eligibility cutoffs for need-based federal aid before and after the repeal of MISAA, along with their interaction with the "before" dummy. The results are unaffected by the inclusion of these variables.

Meyer (1995) points out that difference-in-differences estimates can be sensitive to functional form. In particular, the difference-in-differences estimate can actually change sign if a non-linear transformation, such as a log, is applied to the dependent variable. The present estimates are not vulnerable to this most severe form of functional-form sensitivity. As can be seen in Table 1, the children of deceased fathers were *more* likely that their counterparts to attend college before the policy change but *less* likely to attend college afterward. Under these conditions, linear and nonlinear analysis will produce estimates of the same sign, though their magnitude may vary. In fact, probit produces marginal effects that are quite similar to OLS.

The results in Table 2 indicate that aid eligibility has a strong effect on college attendance. In the next section, I will put the magnitude of this effect in context. Here I turn to examining whether aid eligibility affects college completion in addition to entry. Persuading a student to enter college does not guarantee that he will successfully complete even a year of credits, since the marginal student may be of low ability and unable to complete college-level work. To determine whether this is the case, I examine the effect of aid eligibility on two measures of schooling: the completion of at least one year of college and the number of years of completed schooling.

The estimates, based on the fully-controlled specification of Table 2, are in Table 3. Eligibility for student benefits appears to increase the probability of completing at least a year of college by 14.5 percentage points, though this estimate is not significant. The effect on completed schooling is an increase of about half a year and again, is not significant. These results, while not conclusive, suggest that aid eligibility has some positive effect on completed schooling at age 23.

The NLSY is a longitudinal survey, which provides us the opportunity to repeat the analysis when the sample is older, to see if our conclusions about the effect of aid are affected. In theory, the effect of aid eligibility could dissipate over time: student benefits might induce students to accelerate rather than increase their schooling investments, in which case those not eligible would eventually catch up in their

schooling. To see if this is the case, I examine schooling decisions as of age 28.[23] This exercise will be somewhat complicated by attrition. Of those present in 1988, when the survey questions were asked about age at father's death, 5.3 percent exited the sample by age 28. By making conservative assumptions about the nature of attrition, I calculate a upper bound on its effect on the magnitude of the estimates.

In Table 3 is a set of estimates from regressions in which the outcome is college attendance by age 28. The specification is that of the previous section. That is, covariates along with their interactions with the before and father-deceased dummies are included in the regressions. I start by limiting the sample to those present as of age 28. This approach assumes that the probability of attrition is uncorrelated with the interaction term. The estimated effect of aid eligibility on college attendance by age 28 is 24.8 percentage points, somewhat higher than the effect at age 23.

Since this higher estimate may be driven by attrition, I next add back to the sample all attriters and assign them the last schooling value observed before they exited the sample. This yields an estimate of 25.6 percentage points, quite close to that based on the selected sample. Again, though, this estimate may be contaminated by attrition bias. One option is to parametrically model the selection process using a two-step Heckman selection model. Identification then arises from either the exclusion from the schooling equation of a variable that predicts attrition, or, in the absence of such a variable, the functional form of the selection model.

Lacking a plausible exclusion restriction, I instead estimate an upper bound on the effect of non-random attrition on the estimate. In the worst-case scenario, the coefficient on $FatherDeceased_i * Senior1979-81_i$ is negative among those who exit the sample. In this case, among attriters aid eligibility is associated with relatively low schooling. I impute values to attriters that correspond to this most severe form of attrition bias. Specifically, within the control group I assume that all attriters in the "before" cohort, but none in the "after" cohort, attended college between exiting the

[23] Schooling does increase moderately between age 23 and 28, so this is a reasonable test. In the Current Population Survey, average schooling for the cohort aged 23 in 1986 rises 0.26 years over the next five years. The share that has attended college rises from 53.2 percent to 55.7 percent. To maintain comparability with the NLSY sample, these means are calculated among those who have attained at least 12th grade (author's calculations from 1986 and 1991 CPS Merged Outgoing Rotation Groups).

sample and age 28. This will tend to produce a drop within the control group across age cohorts in the college attendance rate at age 28. For the deceased-father group, I assume the opposite: none in the before cohort, but all in the after cohort, attended college after attrition and by age 28. This will tend to produce a *rise* across cohorts in the attendance rate of the deceased-father group.

The resulting estimate is in the last row of Table 3. The estimate is 22.4 percentage points, and is significant at the five percent level. This estimate is almost exactly the same as the effect at age 23, indicating that student benefits affect not just the timing of schooling but also its optimal level. If we examine the other schooling outcomes, the same pattern emerges: the lower bound of the effect at age 28 is just slightly above the effect at age 23, indicating that the effect of aid eligibility does not dissipate over time. If anything, it appears that the effect on completed schooling rises over time, though the size of the standard errors precludes any strong conclusions on this matter. The effect of eligibility on the probability of completing at least a year of college is 14.5 percentage points at 23 and 17.8 percentage points at 28, while the effect on completed years of schooling is 0.564 at 23 and 0.679 at 28.[24]

VI. Discussion

The set of coefficients in Table 3 is consistent with aid eligibility increasing both college entry and persistence. Aid appears to induce about 24 percent of eligibles who would not otherwise have attempted college to attend. Plausibly, many of those induced into college complete just a few years of college but do not graduate. Further, aid likely induces some eligibles who would have otherwise completed just a few years of college to instead persist and finish their degrees. The effect of movement along these various margins is a relative increase in average schooling of 0.626 years.

[24] I calculate the lower bound on the effect of aid eligibility on completed schooling at age 28 in the following way. Within the control group, I assume that all attriters in the "before" cohort completed a year of college during every year between exiting the sample and age 28, while those in the "after" cohort completed none. For the deceased-father group, I assume the opposite. This will tend to produce a relative rise in the completed schooling of the deceased-father group upon the elimination of student benefits. Note that previous iterations of this paper failed to address attrition bias and therefore concluded that the effect of aid eligibility on completed schooling was larger than 0.679.

While this story is consistent with the results, so too is the interpretation that aid increases the schooling of students who would have gone to college anyway, but has no effect on the completed schooling of marginal entrants. Since we cannot identify those individuals whose behavior is changed by a given treatment, we cannot empirically distinguish between these competing explanations. But since student benefits reduce the cost of every year of college through age 22, it is most plausible that aid eligibility affects the completed schooling of both those who are and are not on the margin of attending college.

As was discussed earlier, individuals are classified into the eligible and control groups with error. Aigner (1973) and Freeman (1984) show that the relationship between the true coefficient (β^*) and that estimated in the presence of classification error (β) is:

$$(5) \qquad \beta^* = \frac{\beta}{(1-\delta_E-\delta_C)}$$

where δ_E and δ_C are the degrees of classification error in the eligible and control groups, respectively. Equation (5) indicates that the estimates of the previous section are biased toward zero by classification error.[25] In the case at hand, δ_E is the share of children *with* deceased fathers who *are not* eligible for Social Security benefits and δ_C is the share of children *without* deceased fathers who *are* eligible for Social Security benefits. The Social Security Admnistration estimates that, in the early Eighties, 95 percent of children under 18 would have been eligible for survivor benefits had a working parent died, suggesting that misclassification of the eligible group is minor. Misclassification of the control group is also minimal. The share of 17-year-olds in 1980 whose fathers were not dead but were eligible for Social

[25] This result may seem obvious, since classical measurement error is known to produce attenuation in regression coefficients. However, measurement error in binary variables is non-classical: if a zero is observed, the measurement error can only be non-negative and if a one is observed the measurement error can only be non-positive. This violates the standard assumption that the measurement error is uncorrelated with the truth. Note that Equation (5) holds for bivariate regressions and in the special multivariate case in which the classification variable and its error are uncorrelated with other regressors. In the general multivariate case the degree of attenuation bias worsens. As I have no information about how classification error correlates with covariates, I am limited to the (conservative) adjustment of Equation (5).

Security due to the disability or retirement of a parent was 5.3 percent.[26] Inserting these figures into Equation (5) indicates that the estimates of Table 3 should be adjusted upward by 11 percent. Table 4 contains these adjusted estimates, which suggest that aid eligibility increases the probabilities of attending college by age 23 by 24.3 percentage points and of completing at least a year of college by 16.1 percentage points. The effect on completed schooling at age 23 is 0.626 years.

These are large effects, but so too was the financial incentive. The average student benefit for the child of a deceased parent was about $6,700 in 1980, more than enough to cover the $1,900 cost of tuition and fees at a public university.[27] If we sum these costs and the opportunity cost of college (proxied by the average wage of young high school graduates, $18,500 in 1980) we obtain an elasticity of attendance with respect to schooling costs of about 1.5.[28] Each $1,000 of student benefits offered induces an increase of 3.6 percentage points in the share of high school graduates attending college and a tenth of a year in completed schooling. These estimates have an instrumental variables interpretation, as Wald estimates. If the effect of aid eligibility is linear and constant across individuals, these estimates are computationally equivalent to those obtained by using $FatherDeceased_i * Senior1979-81_i$ as an instrument in regressions of schooling against aid eligibility.[29]

[26] The number of 17-year-olds is from the 1980 Census and the number of 17-year-olds with parents retired or on disability is from Table 54 in Social Security Administration (1982). Note that the NLSY contains no information about parents' retirement or disability status, so these individuals cannot be identified. The discussion that follows assumes that these individuals would have had the same response to the elimination of benefits as the children of deceased parents. This assumption is not precisely correct, for two reasons. First, the children of disabled and retired parents were eligible for only two-thirds of the benefits of the children of deceased parents. Second, as discussed earlier, the withdrawal of student benefits reduced the incentives for parents of college-bound children to apply for Social Security. This problem has no simple solution, but is unlikely to have a major impact on the error adjustment, since such a small proportion of the control group is incorrectly classified.

[27] Tuition figure is from Table A-9 in College Board (1983).

[28] Average wage weekly wage is calculated for 19-year-old high school graduates in the Merged Outgoing Rotation Group of the Current Population Survey, multiplied by 50 and inflated to 2000 dollars.

[29] I discuss the implications of heterogeneity in the response to aid in Section VIII. It is plausible that the effect of aid is non-linear. For example, in the presence of liquidity constraints, a threshold amount of aid may be needed to affect behavior, leading a large grant to have a larger per-dollar effect than a small grant. It is also plausible that the marginal effect of aid falls as aid rises. Further, a given dollar of aid may have a different effect on a student who is on the margin of entering college as compared to one who is on the margin of completing college.

How do these results compare with previous estimates of the effect of aid on schooling decisions? There are virtually no estimates of how aid affects completed schooling. For this reason, the completion results are worthy of some attention, even though they are not statistically significant at conventional levels. The only studies of completed schooling that deal with biases caused by unobserved heterogeneity are of those of the effect of the GI Bills on military veterans.[30] Bound and Turner (1999) conclude that the World War GI Bill increased the education of veterans by about 0.26 years, while Stanley (2000) concludes that the Korea War GI Bill had a similar effect. The present estimates are somewhat larger, at 0.626 years. Very roughly, the GI Bill benefits were comparable in generosity to the student benefit program, and closely approximate its structure, in that benefits were distributed as a lump sum rather than as a proportion of actual schooling costs. However, since the populations, schooling costs and average levels of education are so different in this study and the GI Bill studies, it is unsurprising that the estimates differ.

A larger literature examines the effect on aid on the probability of attending college. Manski and Wise (1983), using data from the early Seventies, conclude that $1,000 in aid leads to a 3.8 percentage point increase in attendance. Leslie and Brinkman (1988) review several dozen college attendance studies, which vary widely in their method and quality. They conclude that a $1,000 decrease in net price is associated with a three to five percentage point increase in the attendance of 18- to 24-year-olds. However, all of these estimates are vulnerable to the biases discussed in Section II.

A few studies plausibly control for unobservable and observable attributes that are correlated with aid and college attendance.[31] Using within- and cross-state variation in tuition costs at public universities, Kane (1994) concludes that a $1,000 drop in public tuition produces a 3.7 percentage point increase in college attendance rates. Dynarski (2000) finds that Georgia's HOPE Scholarship, a merit-aid program, produced an increase in the attendance rate of 18- to 19-year-olds of four percentage points per $1,000 of

[30]Angrist (1993), Bound and Turner (1999), Stanley (1999) and Turner (2000).

[31] Van der Klaauw uses regression-discontinuity analysis to measure the effect of financial aid on enrollment. However, he examines the effect of a single college's aid offers on enrollment at that college, and so his results are not comparable to the present estimates.

aid offered. By contrast, Hansen (1983) and Kane (1995) find no change in the relative attendance rate of low-income youth upon the introduction of the Pell Grant.[32] Seftor and Turner (forthcoming) do find an effect of Pell Grant eligibility on the attendance rate of adults in their twenties and thirties; they estimate an effect of 0.7 percentage points per $1,000 in aid. While this effect is smaller than the present estimate, it is comparable in magnitude once the estimates are scaled by the baseline share of the relevant population that is in college.

In summary, with the exception of the Pell studies, estimates that do and do not account for unobservable differences across individuals reach similar conclusions: a $1,000 drop in schooling costs increases college attendance by three to four percentage points. This suggests that either the cross-sectional results are unbiased, or, as is the case in the return-to-schooling literature, competing biases cancel in a cross-sectional analysis.

VI. Student Aid and School Choice

Aid likely affects dimensions of the schooling decision other than attainment and completion, such as the choice of college. In this context, it is helpful to think about the effect of student benefits on the budget constraint. Recall that the student benefit was paid as a lump sum. For students on the margin of attending college in a given year, student benefits had a substitution effect, reducing to zero the price of the first unit of schooling. This is the group whose behavior we picked up in the previous sections. For the infra-marginal student who would go to college even in the absence of student benefits, the subsidy had an income effect, possibly altering the type of college attended. For example, aid may shift students from public into private schools or from two-year into four-year colleges.

While the NLSY contains detailed information about individual schooling choices, the sample with deceased fathers is too small to allow for informative examination of school choice. However, administrative data allow insight into the schooling choices of Social Security student beneficiaries. In

[32] Turner (2000) suggests that schools may have "undone" the Pell Grant by lowering the institutional aid they offered low-income students, thereby explaining the zero program effects.

1972, the Social Security Administration commissioned a survey of student beneficiaries, gathering information about their schools, grades and family backgrounds. The survey was conducted only in one year, and was limited to those who actually took up student benefits by attending college. While these limitations preclude using the data in a formal difference-in-differences analysis, the survey provides some insight into the schooling choices made by student beneficiaries.[33]

Student beneficiaries during the 1972-73 academic year looked much like the average young college student. Seventy-four percent of college-going beneficiaries attended four-year institutions, as compared to 72 percent of all college students.[34] A higher share of student beneficiaries than all full-time, young college students attended private schools: 29 vs. 18 percent, respectively.[35] They were similarly successful in their academic endeavors: 50 percent of student beneficiaries maintained a B average or better during their freshmen year, while 54 percent of college freshmen in the National Longitudinal Survey of the High School Class of 1972 did so (Springer, 1976).

Student beneficiaries spent about the same as the average student on tuition, fees and books: $2,500 for those attending public schools and $7,900 for those attending private schools, as compared to $2,200 and $8,000 for all full-time post-secondary students. Adding in room and board brings the average educational cost for all student beneficiaries to $8,200; for all students, the comparable figure is $7,400. Since the average student benefit in this survey was $5,500, beneficiaries (on average) spent more than their benefit on schooling costs. A concern in the policy debate over student benefits was that, since benefits were paid as a lump sum, beneficiaries had an incentive to select undemanding schools with low

[33] The Social Security Administration has been unable to find a copy of this data-set in its archives. However, detailed tabulations of the data were published in Springer (1976) and Hastings (1978). The statistics in this section are drawn from these sources.

[34] Calculated from data in Table 18 in Bureau of the Census (1974) and Chart 1 in Springer (1976). Note that the Census figures for this period do not measure enrollment in non-degree programs, such as secretarial or technical schools. To maintain comparability, I have similarly excluded such students from this figure.

[35] Calculated from data in Table 12 in Bureau of the Census (1974) and Table 9 in Springer (1976). This figure does include those in non-degree programs, since a separate private/public breakdown is not provided for just those in degree programs.

costs. That beneficiaries' schooling costs were higher than their benefits (and higher than the costs of the average college student) is evidence against this type of gaming behavior.

Overall, then, those receiving Social Security student benefits made schooling choices much like those of the typical college student. This is remarkable, since the beneficiary population was disproportionately black and low-income. In 1972, 12.5 percent of college-going beneficiaries were black, as compared to 8.3 percent of all college students of the same age.[36] More than half of college student beneficiaries had family incomes below $39,000, while only 35 percent of all families with a full-time student between the ages of 18 and 24 fell into that income range.[37] That the college choices of this group look much like those of the average student suggests that Social Security student benefits influenced not only the level of education chosen but the quality of school attended, as well. The benefits appear to have played a compensatory function, inducing them to make schooling choices like students of higher socio-economic status.

VII. External Validity

The Social Security student benefit program was unique in its structure and eligible population. I have argued that the paper provides internally valid estimates of the effect on schooling of eligibility for Social Security student benefits. Does it provide externally valid predictions of the effect of other sources of aid? Two issues to explore in answering this question are 1) the characteristics of the population eligible for the student benefit program and 2) the structure of the program itself.

If those eligible for Social Security student benefits are unusual, then their response to subsidies may also be unusual. The youth that are the focus of this paper are special in at least one way: their fathers are dead. This may make their families especially sensitive to the price of college, since they have only one parent's labor supply with which to buffer shocks to schooling costs. On other observable

[36] Beneficiary figure calculated from data in Table 1 and Table 2 in Huntley (1979). Total enrollment figure calculated from data in Table 1 in Bureau of the Census (1974).

[37] Calculated from data in Table 18 of Bureau of the Census (1974) and Table 17 of Springer (1976).

characteristics, the population eligible for student benefits closely resembles that served by need-based programs. Both groups are disproportionately black: 26 percent for deceased-father children and 21 percent for Pell Grant recipients.[38] Both groups have low incomes: in 1973, average family income of student beneficiaries placed them in the bottom income quartile of families with children in college (Bureau of the Census, 1974 and Springer, 1976), while 90 percent of dependent Pell Grant recipients are from families with incomes below $40,000.[39] Given these similarities, it is plausible that the behavior of this population provides insight into how low-income youth respond to aid.

In its structure, the Social Security program is unique in that benefits rise with the earnings of the deceased parent. In traditional programs, the correlation between aid and parents' human capital is negative. This distinction could cause two problems. First, if the population eligible for student benefits is well-off, their response may not predict that of a low-income population. This issue was addressed in the last paragraph. Second, the paper's estimates will provide a biased prediction of the effect of need-based aid if the response to aid is heterogeneous and correlated with parental human capital. The direction of the bias depends on the sign of this correlation. Suppose children from high-human-capital families are most sensitive to cost. In this case, relative to traditional student aid, the Social Security program channels more dollars to high-response individuals. By implication, the paper will over-estimate the effect of traditional aid. The opposite will hold if, instead, children from high-human-capital families are *less* sensitive to cost. Dynarski (2000) shows that, in a simple human capital framework, the sign of the correlation is ambiguous. Kane (1994) presents results that indicate that low-income youth are most sensitive to tuition. By contrast, Stanley (2000) and Turner and Bound (2001) find that the post- Word-War II GI Bill had the greatest impact on veterans from more privileged backgrounds.

A second unique characteristic of the Social Security program is that benefits are unrelated to schooling costs. Traditional aid, such as the Pell Grant, is capped by actual costs, while the Social Security student benefit was not. While this may appear to be a critical distinction, it matters little in

[38] Pell Grant statistics are calculated from data in Table 317 in NCES (2000).

[39] Calculated from data in Table 314 in National Center for Education Statistics (1998).

practice. The Pell Grant maximum is low relative to schooling costs, which are defined by law to include not only tuition, fees and books but also a living allowance. As a result, for few students do actual schooling costs affect the size of their Pell Grant. The key insight is that the Pell Grant and the student benefit have the same effect on the student's budget constraint, shifting it outward rather than altering its slope.[40]

A final concern is whether the predicted effect of a change in aid policy depends on the time horizon under consideration. Note that I have examined the effect of a *withdrawal* of aid. Those eligible for this program knew about the subsidy during the years in which they attended elementary and secondary school. They exerted academic effort, and their families made financial plans, with the assumption that the subsidy would be available. In other words, the effect that is measured in this paper is that of aid on the lifetime behavior of youth and their families. For this reason, the paper's estimates are best understood as the effect of the *withdrawal* of aid in the short-term and the effect of the *introduction* of aid in the long-term, after youth have adjusted their behavior to the availability of aid.

VIII. Conclusion

A key question in the debate over higher education policy is whether student aid increases college attendance or simply subsidizes college for infra-marginal youth. This paper has shown that aid eligibility can affect schooling decisions. I find that eligibility for Social Security student benefits increased college attendance by 24 percentage points, or 3.6 points for each $1,000 of grant aid offered. Aid eligibility also appears to have a positive impact on completed schooling, though these results are not statistically significant: the probability of completing any college increases 15 percentage points and years of schooling by about two-thirds of a year, or a tenth of a year for each $1,000 of aid offered. Administrative data indicates that student beneficiaries, though disproportionately black and from low-

[40] However, it should be noted that Social Security student benefits are much larger than Pell Grants, both in absolute terms and relative to contemporaneous schooling costs. If the effect of a subsidy is non-linear, then the estimates of the paper may not accurately predict the effect of a marginal increase in a less-generous aid program.

income families, attended the same types of schools as the average college student. Together, this set of results suggests that aid plays a compensatory role, increasing college attendance and school quality of eligible youth to the levels of their more well-off peers.

While there are no comparable estimates of aid's effect on completed schooling, the attendance results are roughly consistent with previous estimates. This is somewhat surprising, since we suspect many of the cross-sectional estimates to be biased by correlation between aid and unobserved attributes that affect the probability of attending college. That the cross-sectional estimates are consistent with the present results, as well as with the handful of other studies that control for unobserved heterogeneity, suggests that competing biases are at work in the cross-section. This is quite plausible: cross-sectional estimates of the effect of need-based aid are likely biased downward and estimates of the effect of merit aid are likely biased upward, since eligibility for these types of aid is correlated (negatively and positively, respectively) with the underlying propensity to attend college. Analyses that pool these sources of aid may fortuitously arrive at an unbiased estimate of the causal effect of aid eligibility.

From a student's perspective, this program made college an optimal choice at very low rates of return to schooling. With the student benefit, a year of college would pay for itself with a rate of return as low as 2.5 percent.[41] The program therefore likely induced into college some students with a very low *ex ante* payoff to schooling. But given the rapid rise in the return to schooling since the early Eighties, *ex post* returns for those induced into college by the student benefit were likely considerably higher.

A final question is whether the paper's high estimates of the elasticity of college attendance with respect to aid indicate the presence of liquidity constraints. Since grant aid reduces the cost of schooling and thereby increases its optimal level, a behavioral response does not, *per se*, indicate the presence of capital constraints. The policy experiment examined by this paper, while providing a plausibly-identified estimate of the causal effect of aid, does not allow us to untangle its liquidity and subsidy effects.

[41] This calculation assumes costs consisting of tuition and fees at a public university ($1,900 in 1980-81) and a year of forgone earnings ($18,500). I further assume a work life through age 65, annual real wage growth of one percent , and a real discount rate of four percent.

Table 1: NLSY Summary Statistics

	High School Seniors 1979-81		High School Seniors 1982-83		Difference in Differences
	Father Not Deceased	Father Deceased	Father Not Deceased	Father Deceased	
Household Income (2000$)	54,357	32,875	50,842	32,298	-2,938
	(537)	(1,839)	(788)	(2,958)	(4,816)
AFQT Percentile	60.50	58.18	52.87	44.90	5.65
	(0.51)	(2.36)	(0.91)	(3.92)	(5.33)
Black	0.135	0.235	0.151	0.297	-0.046
	(0.007)	(0.036)	(0.011)	(0.063)	(0.068)
Hispanic	0.051	0.055	0.062	0.059	0.007
	(0.004)	(0.020)	(0.007)	(0.032)	(0.026)
Father Attended College	0.331	0.184	0.299	0.158	-0.006
	(0.009)	(0.033)	(0.014)	(0.050)	(0.079)
Mother Attended College	0.238	0.127	0.203	0.166	-0.074
	(0.008)	(0.029)	(0.012)	(0.050)	(0.085)
Single Parent Household	0.153	0.787	0.194	0.837	-0.009
	(0.007)	(0.035)	(0.012)	(0.051)	(0.071)
Family Size	4.77	4.40	4.71	4.34	0.00
	(0.03)	(0.18)	(0.05)	(0.27)	(0.31)
Age in 1988	25.95	25.92	23.95	23.95	-0.03
	(0.02)	(0.09)	(0.02)	(0.09)	(0.14)
Female	0.483	0.488	0.471	0.485	-0.009
	(0.010)	(0.043)	(0.015)	(0.069)	(0.097)
Attend College by 23	0.502	0.560	0.476	0.352	0.182
	(0.010)	(0.043)	(0.015)	(0.066)	(0.096)
Complete Any College by 23	0.487	0.560	0.459	0.361	0.171
	(0.010)	(0.043)	(0.015)	(0.066)	(0.097)
Years of Schooling at 23	13.41	13.44	13.25	12.90	0.380
	(0.03)	(0.13)	(0.05)	(0.20)	(0.296)
Number of Observations	2,745	137	1,050	54	3,986

Note: Means are of NLSY poverty and random samples, weighted by 1988 sample weights. Income and household composition measured during senior year of high school. AFQT is age-adjusted; see Data Appendix. Standard errors are in parentheses. Standard errors in the difference-in-differences column adjusted for clustering at the household level.

Table 2:
OLS, Effect of Eligibility for Student Benefits on Probability of Attending College by Age 23

	(1) Difference-in-Differences	(2) Add covariates
Deceased father × senior 1979-81	0.182	0.219
	(0.096)	(0.102)
Deceased father	-0.123	Y
	(0.083)	
Senior 1979-81	0.026	Y
	(0.021)	
Senior-year family income/10,000 ($2000)		Y
AFQT score		Y
Black		Y
Hispanic		Y
Father attended college		Y
Mother attended college		Y
Single parent household		Y
Family size		Y
Female		Y
Age in 1988		Y
State dummies		Y
All Covariates × Senior 1979-81		Y
All Covariates × Deceased Father		Y
R^2	0.002	0.339
N	3986	3986

Notes: Regressions weighted by 1988 sample weights. Standard errors adjusted for heteroskedasticity and multiple observations within households.

Table 3:
OLS, Effect of Eligibility for Student Benefits on
Schooling Outcomes by Age 23 and Age 28

	Attended College Full-Time	Completed Any Years of College	Years of Schooling
By Age 23	0.219	0.145	0.564
	(0.102)	(0.123)	(0.379)
By Age 28:			
Lower Bound	0.224	0.178	0.679
	(0.106)	(0.113)	(0.399)
Exclude Attriters	0.248	0.191	0.754
	(0.111)	(0.118)	(0.408)
Assign Last Value	0.256	0.211	0.727
	(0.105)	(0.112)	(0.397)

Notes: Coefficients are those on Deceased father × senior 1979-81 in 12 regressions in which the outcomes are those indicated in the columns. The regression specification is that of Column (2) in Table 3. "Completed Any Years of College" is set to one if a respondent completed thirteen or more years of schooling. See text for explanation.

Table 4:
Estimates Adjusted for Classification Error

	Attended College Full-Time	Completed Any Years of College	Years of Schooling
By Age 23	0.243	0.161	0.626
By Age 28 (lower bound)	0.249	0.198	0.754

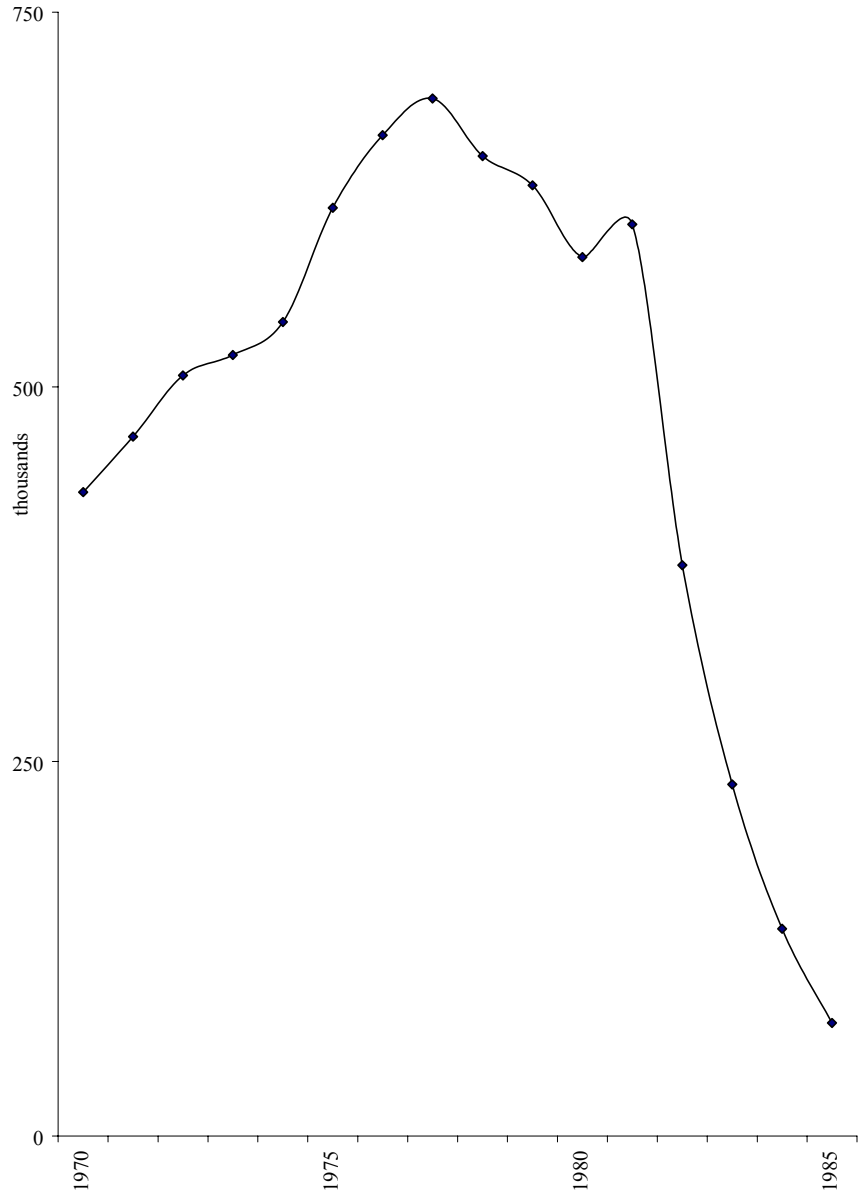
Data Appendix

The National Longitudinal Survey of Youth was initiated in 1979 with a sample of 12,686 youth. I focus on the cross-section and poverty samples, which have been interviewed almost every year since 1979. I use the 1988 sample weights in the analysis, though the point estimates are similar when the regressions are limited to the random sample and not weighted.

The key variables of interest are whether a youth has attended college and how much schooling he has completed by age 23 or 28. In each survey, respondents indicate whether they are currently enrolled in college. I use these responses to code whether a youth attended college full-time since his senior year of high school. The completion variables are obtained from the surveys in which the respondent is 23 or 28. Highest grade completed ranges from the 11^{th} to the 20^{th}. Since cohort is defined by the year one is a high school senior, those who don't complete junior year are excluded from the analysis.

The Armed Forces Qualifications Test (AFQT) was administered to the NLSY sample in 1980. The respondents ranged in age from fourteen to twenty-two when they took this test. Since age has been shown to affect AFQT score, I regress AFQT on age dummies and use within-age percentile scores in the analysis. I measure family income at the time a youth was a high school senior; all values are inflated to 2000 dollars. Family income is missing for about 20 percent of the sample. AFQT is missing for a handful of cases. For both variables, I calculate cohort-specific means separately for those with and without deceased fathers and assign these means to the missing values. A dummy is included in regressions to indicate these imputed values. Parental education, as measured in the 1979 survey, is used to create a set of variables that indicate whether each parent completed any college. Variables indicating whether education is missing for either parent are also included in the analysis.

Figure 1
Number of Social Security College Student Beneficiaries



Source: Social Security Administration (1985, 1986). The moderate drop in the late Seventies is due to a national drop in enrollment rates and slowed growth in the college-age cohort (NCES (1998), Tables 6 and 15).

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