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Kathryn Anne Edwards, Daniel Schwam, and Melanie Zaber

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Kathryn Anne Edwards
Author1Afiiliation

Daniel Schwam
RAND

Melanie Zaber
RAND

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Michigan Retirement and Disability Research Center, University of Michigan, P.O. Box 1248. Ann Arbor, MI 48104, mrdrc.isr.umich.edu, (734) 615-0422

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The Effect of the Social Security Student Benefit on Lifetime Earnings

Abstract

Between 1965 and 1981, Social Security extended eligibility for dependent benefits from age 18 to age 22 for individuals who were enrolled full-time in school. The "student benefit" ended in 1981, and past research has shown that the benefit's elimination greatly reduced the probability of attending college for individuals who would have been eligible for it. We use the 1979 National Longitudinal Survey on Youth to examine the student benefit's effect on lifetime earnings. We compare the lifetime earnings of individuals who would or would not have been eligible based on their high school graduation year and whether they had a deceased father. Over the study population, we find large differences in lifetime earnings (cumulative over ages 19 to 62), with those ineligible for the benefit earning less over their lifetime. This result is driven by women and elder siblings, as opposed to younger siblings or only children. We interpret what these results mean for understanding the effect of college on earnings, how college is subsidized, and whether cutting the benefit was more costly to Social Security in the long run by lowering earnings.

Citation

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Introduction

Decades ago, the Social Security Administration provided cash support to disabled, deceased, or retired workers' college-going children who were receiving dependent benefits. The student benefit, as it was called, was not means-tested and functioned as a first-dollar scholarship unaffected by any other grant aid the student received. In response to pressure to shore up Social Security's finances, the benefit was ended in 1981. It was a fortuitous timing for research as it split the newly formed 1979 National Longitudinal Survey on Youth (NLSY79) in two — the older birth cohorts included in the survey were eligible for the student benefit, but younger cohorts were not. Prior research has shown that the benefit's cessation reduced the college attendance probabilities of the targeted population by a third (Dynarski 2003).

In this paper, we return to the student benefit and the NLSY79. The panel is now past prime-age and beginning to retire: We evaluate to what extent eligibility for the benefit affected lifetime earnings. Using the same difference-in-difference design as prior research, we estimate the difference in earnings attributable to the subsidized college attendance the student benefit offered by comparing individuals in cohorts before and after the benefit's cessation who had a deceased father.[1]

We find evidence of an increase in lifetime earnings. The increase in earnings is measurable and significant when examining earnings at specific milestone ages. When aggregating over prime-age working years and across subgroups, income effects are

[1] While the benefit was not gender-specific, few mothers would have met the work history in covered occupations requirements. Because we cannot see parental work history, we limit our analysis to those with deceased fathers.

positive-signed but imprecise. Income effects are particularly large (and statistically significant) for female beneficiaries, for college-attending beneficiaries, and for older siblings. These findings suggest that benefit eligibility was an important determinant of human capital investments and, ultimately, economic prospects for young adults with deceased fathers. Effects on secondary outcomes such as family formation and program participation are more sensitive to exact specification.

There are three implications of our question and its findings. The first regards the returns to postsecondary education. Does college increase earnings? A long line of research has shown that college is a good value proposition across demographic backgrounds (e.g., Hubbard 2011, Eide 1997). Despite slow growth in the college premium — the difference in earnings between degree-holders and high school graduates — since the early 2000s, a college degree is still considered a worthwhile investment for individuals (New York Fed 2019). However, as the returns to schooling literature investigates (see Card 1999, 2001; Hendricks and Leukhina 2018), is this the causal effect of education on earnings or the selection effect of individuals with high earnings potential also attending more school? Our paper offers an addition to this research, examining a group whose attendance probability was greatly affected by exogenous changes in aid and, uniquely, those changes were not means-tested.

The second implication regards investments in education made by states and the federal government. The expansion or contraction of financial aid eligibility has a large and robust research literature (of which Dynarski (2003) is part). Given that tuition is a large expense, most aid is targeted by means. However, students do not have to be low-income for the cost of college to seem not worth the potential gains. The student benefit is exceptional in that it was not means tested and very generous. Our paper provides estimates of the return on a nonincome targeting scheme and in a broader population.

Finally, our paper has implications for Social Security itself. It is a straightforward public finance question: Did Social Security lose more in foregone tax revenue and higher relative benefit payouts than they saved in ending the student benefit? Paying for up to four additional years of dependent children's benefits and delaying the entry to full-time work is a cost that is potentially netted out, or even a positive investment, if those dependent children earned more or worked longer over their lifetimes. We do not have enough data to calculate this tradeoff directly. However, we do provide evidence that although ending student benefits may have improved short-term solvency, it might have been a cost-losing endeavor in the long run.

In the sections that follow, we dive more deeply into the student benefit's history, introduce our data and estimation approach, present the results, and contextualize the findings with an analysis of college attendance in the 1980s and 2020s.

Background

Social Security is the shorthand name of Old-Age, Survivors, and Disability Insurance (OASDI), a social insurance program that provides a wage-replacing benefit to individuals who have worked previously and sufficiently in covered (i.e., taxed) employment, and who meet specific conditions of not working: They are retired, disabled, or deceased. For the prior two, workers, their spouse, and any dependent children may receive a benefit. For the latter, a surviving spouse and dependent children may receive a benefit. In 1965, the "student benefit" was created, acknowledging the continued dependence of young adults enrolled in school. This benefit, a form of dependent benefits, allowed recipient children to continue to receive benefits until they turned 22 (rather than ceasing at 19) if they attended school full-time and were unmarried. Individuals who had aged out of the typical dependent benefits but maintained them through this student exception were referred to as student beneficiaries.

The student benefit was substantial. In 1972/1973, the median recipient got 46 months of benefits totaling $4,392, equivalent to more than $32,000 in 2023 (Rosen 1983). Contrast this with an annual cost of tuition, room, board, and fees at a public college of $1,458 in 1972/1973 — the median student benefit covered two-thirds of educational expenses (U.S. Department of Education 2007). When the benefit started sunsetting in 1981, the average annual benefit across student beneficiaries was $3,072, equivalent to a 19-hour per week part-time job at the then minimum wage of $3.10 (Rosen 1983; Table 12).

The exact amount of the benefit depended on the reason for receipt and earnings history of the insured worker. Children of retired or disabled workers received 50 percent of the workers benefit, and children of deceased workers received 75 percent of the spousal survival benefit. Children of deceased parents were the majority (60 percent) of student beneficiaries, with the remainder evenly divided between disabled and retired parents. Because the student benefit was based on workers' prior earnings, it differed from other income supplement or financial aid programs to students because those from lower-income families received *smaller* benefits from Social Security.

As detailed in Rosen (1983), the educational pathways pursued by recipients who had completed high school ranged from vocational education to four-year college. Of all student beneficiaries, a fifth were in high school, half were in four-year colleges or universities, 18 percent were in two-year colleges, with the rest scattered among other kinds of institutions. Compared to the contemporaneous college-going population, student beneficiaries were generally from lower-income backgrounds and families with fewer years of completed education. They were also much more likely to work while in school, with half of student beneficiaries working during the school year and 80 percent working while enrolled (including summers).

In a 1976 survey of student beneficiaries (GAO 1979), part-time work was cited as the most common source of additional school financing, followed by the Pell grant, other grants, and student loans. However, most student beneficiaries did not receive the Pell grant (65 to 75 percent, depending on the source and year),[2] and those that did receive Pell funding typically were not awarded the maximum. Even among dual OASDI-Pell beneficiaries, Pell was typically a smaller amount of support than OASDI. Half of student benefit recipients reported that they weren't sure if they could persist in their college education without the benefit.

In response to pressure to shore up Social Security's finances, on August 13, 1981, President Reagan signed the Omnibus Budget Reconciliation Act, which ended the OASDI student benefit. No new recipients could be added, but existing recipients

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[2] There is some uncertainty as to the low rate of dual receipt. Student beneficiaries could have thought themselves ineligible for Pell, could have been unaware of the relatively-new program, could have had too low of a cost of attendance, or could have been independent students with an income that was too high.

could continue to receive a reduced benefit. Benefits would also be newly counted as income when determining Pell eligibility. In the 1981 fiscal year, Social Security paid out an estimated $2 to $2.4 billion to as many as 1 million students, putting it at a comparable size to Pell in spending ($2.4 billion), reaching fewer students (Pell covered 2.7 million) but giving more money to each (Rosen 1983).

There is a small literature dedicated to identifying the effects of the end of student benefits. Dynarski (2003) finds that eliminating the program reduced college attendance probabilities by 20 to 22 percentage points (more than a third) with a similar, though not statistically significant, reduction in college completion. Groves and Lopoo (2018) explore the impact on family formation, finding a delay (but not a lifetime reduction) in both marriage and childbearing for women who were likely to be eligible, with no effects on men. Ehrenberg and Luzadis (1986) look at how benefit eligibility changed usage of other forms of financial support. They find evidence of complementarities for beneficiaries attending private schools — those with larger benefits receive more parental support and are less likely to work during school.

The remainder of the student benefit literature is descriptive in nature, reporting on the size of the program or the characteristics of beneficiaries; the two primary data sources are Social Security Administration (SSA) administrative data and a beneficiary survey conducted in 1976 (e.g., Springer 1976, Hastings 1978, Huntley 1979, Rosen 1983). Additionally, the Congressional Budget Office (CBO) wrote a background paper on the cessation of benefits and policy alternatives in 1977 (CBO 1977).

The affected student benefit cohorts are now approximately 60 years old, out of the prime age for labor force participation and earnings and beginning to retire. Hence, a study of the student benefit's effect on lifetime earnings is now possible.

Theoretical frame

College attendance is linked with several subsequent life outcomes. It is important to note that the student benefit — essentially a cash grant to students — could have impacts on both the extensive margin (attending college) and the intensive margins (quality of college attended, enrollment intensity, completion trajectory). Those who would never attend college are unaffected by access to the benefit, but those who would always attend are potentially still affected through this intensive margin. Both margins are substantiated by literature on financial aid, which has been shown to increase rates of matriculation (Deming and Dynarski 2010), improve college match (Hoxby and Turner 2013), and speed graduation (Anderson and Zaber 2021; Nguyen et al. 2019).[3] The student benefit acting as financial aid with impacts on rates of college attendance was first identified in Dynarski (2003).

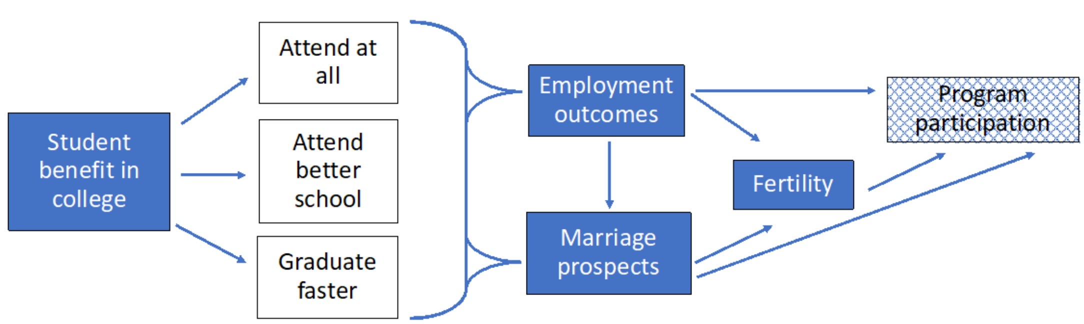
However, it is important to recognize the exceptional circumstances of the group eligible for treatment. In our sample, we are only able to consider potential beneficiaries with deceased fathers. In addition to the emotional and personal challenges stemming from the loss of a parent, fathers were the primary earners for most families in the 1970s and 1980s — in fact, in about half of families, the mothers did not work at all (Rehel and Baxter 2015). This loss of financial resources could lead a child to drop out

[3] Note that for the purposes of this study, these margins are indistinguishable and averaged together in the treatment effect.

of high school to support the family through work or to not pursue college because savings needed to be repurposed: Having a deceased father is the first shock in potentially realigning postsecondary attendance expectations and access, and the student benefit may not have sufficiently ameliorated the associated financial concerns. Because our estimation strategy focuses on variation in access within this population, we temporarily abstract away from these concerns, noting the implications for generalizability.

From the perspective of the individual, these outcomes can shape the returns on time and resources invested in attending college. For a potential beneficiary, these returns would be contrasted with the potential reduction in family benefit (benefits were capped within family unit, so losing individual benefit access by not pursuing college might not have changed the family's resources) and forgone labor income.

From Social Security's perspective, the change in engagement with social programs (through taxes or recipiency) of the beneficiary and their dependents will determine the net benefit or cost of the program. Earnings and household structure (marriage, fertility) will influence tax collection, fertility will determine future taxpayers, and earnings and household structure jointly determine program participation.



Among the general population, research suggests that college attendance improves employment outcomes, increases rates of marriage, and delays (and slightly suppresses) fertility. Depending on the magnitude of these effects, a moderate investment during emerging adulthood may provide a positive return for society. In the rest of the paper, we characterize the direction and magnitude of the effects, and discuss how changes in the labor market, the cost of college, and the U.S.' sociodemographic makeup affect how our findings translate to a modern conceptualization of the student benefit.

Data and methods

Data

The 1979 cohort National Longitudinal Survey of Youth (NLSY79) is a repeated sample of 12,686 individuals born between 1957 and 1964, conducted annually from 1979 through 1994 and biennially starting in 1996.[4] The initial cohort was made up of three subsamples: a cross-sectional sample comprising 48.2 percent of respondents; a supplemental sample that included an oversample of non-Black, non-Hispanic[5] poor individuals and oversamples of Black and Hispanic populations totaling 41.7 percent of respondents; and a military oversample accounting for 10.1 percent of all respondents.[6]

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[4] Survey retention by 2018 was approximately 54.2 percent.

[5] The original race/ethnicity variables collected by the NLSY79 have three mutually exclusive groups: Black; Hispanic; and non-Black, non-Hispanic (non-Hispanic whites are the majority of this third group, but it also includes individuals who may identify as Asian, Native Hawaiian or Pacific Islander, or American Indian or Alaska Native.

[6] The supplemental oversample of non-Black, non-Hispanic poor individuals was suspended after 1990 while 1,079 respondents (84.3 percent) from the military subsample were no longer

The cohort is roughly even in terms of male and female respondents, and is 59.2 percent non-Black, non-Hispanic, 25.0 percent Black, and 15.8 percent Hispanic.

Our identification strategy is to compare student-benefit eligible individuals in cohorts that did and did not have access to student benefits. We proxy for this using an individual's senior year of high school and whether they had a deceased father before turning 18. The year a respondent was a senior in high school is determined using three variables from the NLSY79: enrollment status as of May 1 of the survey year, highest grade attended, and highest grade completed. For example, a respondent is flagged as a high school senior in 1981 if they were enrolled in high school, their highest grade attended was grade 12 as of May 1, 1981, and their highest grade completed was 11.[7]

Father's mortality status is also determined using several variables but is not as straightforward. Most questions in the NLSY79 pertain to a respondent's biological father, complicating responses for individuals who grew up with a stepfather or were adopted and never knew their biological father. Information about the father is also not consistent across waves. In the 1979 and 1980 waves, father's mortality status is determined using the household register and the following criteria:

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interviewed after 1984 (Bureau of Labor Statistics n.d.). See Appendix Table 1 for a summary of the 1979 cohort.

[7] A second version of high school senior year can use similar information from the 1979 initial survey wave on enrollment status, highest grade attended, and highest grade completed to impute a projected high school senior cohort. For instance, we can impute a 1983 high school senior cohort for individuals who were in grade 7 in 1979 and were enrolled in high school in 1983. This second version overlaps with our main method for 98.8 percent of respondents with valid values for both versions and would provide a net gain of 249 respondents. However, this second version assumes all individuals progress to the next grade up every new school year. As a result, we stick with our primary variable for senior year of high school.

1. Father not deceased if respondent never knew their father.
2. Father (biological or father figure) not deceased if listed on household register.
3. Father (biological or father figure) not deceased if not on household register but is known to be still alive.
4. Father (biological or father figure) deceased if known to be deceased (regardless of presence on household register)[8]

If condition 4 is satisfied, then we assign to the Social Security student benefit eligible population the earliest year between 1979 and 1980, depending on when father was flagged as deceased. After 1980, father's mortality is determined using cross-round variables from the NLSY79's health modules fielded at ages 40, 50, and 60. In the health modules, the respondents provide the age when their biological father died. We then impute father's age at mortality using age at death and age during 1979 survey wave. Thus, year father died is calculated as follows:

1. Year father died is 1979 if $FatherAge_{deceased} – FatherAge_{1979} = 0$
2. Year father died is 1980 if $FatherAge_{deceased} – FatherAge_{1979} = 1$
3. Year father died is 1981 if $FatherAge_{deceased} – FatherAge_{1979} = 2$
4. Year father died is 1982 if $FatherAge_{deceased} – FatherAge_{1979} = 3$
5. Year father died is 1983 if $FatherAge_{deceased} – FatherAge_{1979} = 4$

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[8] Respondents who are unaware as to the mortality status of their biological father when responding to the health modules are assigned based on the 1979/1980 household register questions. This means that some individuals born between 1963 and 1964 who, at the time of the health modules, don't know *when* their father died would be coded as father alive at age 18, which may be incorrect. Note that earlier cohorts are immune to this potential miscoding.

After this imputation, we can identify if a father died before the respondent turned 18 using the determined year of father's death and the respondent's age at interview between 1979 and 1983 (the year the youngest survey respondents turned 18).

Table 1 shows, in the left three columns, the NLSY79 respondents who were seniors in the years in which the student benefit was available (1979 to 1981) and unavailable (1982 and 1983). In the right three columns, we show respondents by whether they had a deceased father before turning 18.[9]

[9] Appendix Table 2 displays breakdowns at age 39 to 40 and age 49 to 50 for respondents still in survey at those ages.

Table 1: High school cohort and father's mortality status

Sample	High School Senior Cohort			Father's Mortality Status		
	1979-1981	1982-1983	Undetermined	Deceased Before Turning 18	Not Deceased Before Turning 18	Undetermined
Total cohort	3,081	1,140	8,465	499	11,711	476
	24.3%	9.0%	66.7%	3.9%	92.3%	3.8%
Sex (cross + over)						
Male	1,531	605	4,267	264	5,879	260
	23.9%	9.4%	66.6%	4.1%	91.8%	4.1%
Female	1,550	535	4,198	235	5,832	216
	24.7%	8.5%	66.8%	3.7%	92.8%	3.4%
Race-ethnicity (cross + over)						
Non-Black, non-Hispanic	1,752	650	5,108	202	7,028	280
	23.3%	8.7%	68.0%	2.7%	93.6%	3.7%
Black	871	309	1,994	222	2,846	106
	27.4%	9.7%	62.8%	7.0%	89.7%	3.3%
Hispanic	458	181	1,363	75	1,837	90
	22.9%	9.0%	68.1%	3.7%	91.8%	4.5%
Subsample						
Cross-sectional	1,837	677	3,597	194	5,741	176
	30.1%	11.1%	58.9%	3.2%	93.9%	2.9%
Oversample	1,244	463	4,868	305	5,970	300
	18.9%	7.0%	74.0%	4.6%	90.8%	4.6%

Note(s): Percent of subsample (see Appendix Table 1) included below raw counts. E.g., 1,531 out of 6,403 male respondents, or 23.9 percent, were high school seniors between 1979 and 1981.

Note that the year in which a respondent is a high school senior has more undetermined cases than the year in which a respondent's father died. By the time of the first survey wave in 1979, only 45.3 percent of our sample was enrolled in high school, with the remainder being either not enrolled in school, enrolled in college, or already a high school graduate. This makes these respondents ineligible for consideration in our analysis, thus contributing to the higher rate of undetermined high school senior cohorts compared to father's mortality status.

Table 2 shows descriptive statistics for our sample by high school cohort and father's mortality status, the difference between which is our primary identification. The variables presented are similar to those reported by Dynarski (2003). We find that across both high school senior cohorts, households with a deceased father made less income than those with a father present. Across both cohorts, Black respondents experienced higher father-mortality rates, respondents with a deceased father were less likely for either their mother or father to have attended college if father was deceased and were more likely to come from a single parent household. Note that high school seniors in the 1979-1981 cohort with a deceased father were more likely to have attended college by age 23 than high school seniors (particularly those with a deceased father) in 1982 and 1983 and were also more likely to have completed any college by age 23. Finally, our analytical sample falls in number when limiting to respondents with information about high school and father's status, to the point where the Hispanic subsample is underpowered. We therefore exclude Hispanic respondents from our analysis and consider only non-Black, non-Hispanic and Black non-Hispanic respondents.[10]

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[10] For more see Appendix Table 2 for sample counts and Appendix Table 3 for a replication of Table 3 with Hispanic data included.

Table 2: Descriptive statistics excluding Hispanic respondents

Variable	High School Seniors 1979-1981: Father Not Deceased	High School Seniors 1979-1981: Father Deceased	High School Seniors 1982-1983: Father Not Deceased	High School Seniors 1982-1983: Father Deceased
Household Income (USD2000)	$54,278	$31,826	$47,727	$24,058
Black	13.1%	27.9%	13.3%	34.6%
Father Attended College	37.0%	18.9%	34.3%	16.1%
Mother Attended College	25.8%	13.3%	23.5%	7.3%
Single Parent Household	12.2%	79.4%	14.2%	82.6%
Family Size	5	4	5	4
Age in 1988	25	25	23	23
Female	48.6%	51.4%	46.6%	48.2%
Attend College By 23	56.5%	62.1%	56.0%	37.2%
Complete Any College By 23	49.4%	53.5%	47.9%	29.6%
Years of Schooling At 23	13	13	13	13
Number of Observations	2,400	170	868	71

Note(s): Household income is reported in USD2000 and is conditioned on father's mortality status and cohort (1979-1981 versus 1982-1983). Income adjusted using series CPALTT01USM661S downloaded from the St. Louis Federal Reserve's Economic Database (OECD, 2023). Table structure based on Dynarski (2003). See Appendix Table 1.B for certain variable definitions. Estimates are weighted using 1988 sample weights.

Estimation strategy

The cessation of benefits and the well-defined benefit eligibility criteria provide an opportunity to use difference-in-differences (DiD) to estimate the effect of eligibility for the Social Security student benefit on lifetime earnings, marriage, and fertility. The components that make up our DiD estimator are high school senior cohort and father's mortality status as of age 18.[11] Recall that true eligibility for the Social Security student benefit is based on an individual's parent's mortality, disability, or retirement status and their cumulative earnings during their working life. We proxy eligibility in the NLSY79 using only father's mortality status since parental disability and retirement status are not determined in the survey. Moreover, we do not know father's prior earnings, so it is likely our sample slightly overcounts individuals eligible for the student benefit due to a deceased father, since we cannot confirm their father had sufficient work history to earn survivors' benefits. Disability and timing of retirement claiming may also be endogenous to decisions about college attendance, further justifying their exclusion.

For our estimate to be a causal representation of the effects of receiving the student benefit, we require that in the absence of the benefit, the eligible and ineligible would have had parallel trajectories in terms of earnings, marriage, and fertility. We would additionally hope that there is no migration between eligible and ineligible groups; i.e., individuals do not strategically time their graduation to receive the benefit. There is anecdotal evidence of some strategically timed graduation/enrollment (Mirga 1981), but

[11] Arguably those whose fathers die before age 22 could still take advantage of the benefit (in the oldest cohorts of the NLSY), but our analytic frame focuses on immediate enrollment after high school.

this will mute our estimated effect rather than cause us to claim an effect that does not exist.

Our regression equation is as follows:

$$Y_i = \alpha + \beta_1 FatherMortalityBy18_i + \beta_2 HighSchoolCohort_i + \beta_3 (FatherMortalityBy18_i * HighSchoolCohort_i) + \theta_i + \varepsilon_i$$

where Y_i is one of our outcomes of interest for respondent, *i*; θ_i is a vector of control characteristics for the respondent; and ε_i are residuals. Our three outcomes of interest are cumulative earnings, marriage, and fertility. We consider each of these three outcomes across numerous ages, e.g., cumulative between ages 19 and 62 and between ages 25 and 54 (i.e., prime-working age).

Recall that the NLSY79 becomes a biannual survey starting in 1996. As such, the variables we used to calculate our cumulative outcomes suffer from two types of missingness. The first is simply missingness from nonsurvey years. The second is from nonresponse, which can either come from item nonresponse (e.g., individual chooses not to answer a specific question) or survey nonresponse (the individual does not respond to the survey at all). Certain variables in the NLSY79 are backfilled when an individual misses one survey wave but reappears later. The survey asks respondents about certain variables in the skipped survey years to provide a complete history. However, recall may be biased. We do our best to account for poor recall, particularly in wage/salary income, and discuss how we account for both sources of missingness depending on the outcome.

We impute wage/salary income using a supplemented version of the baseline imputation method described in Nielsen (2023) that imputes wage/salary income in nonsurvey years using linear interpolation from the surrounding survey years (if a valid response is present). For all remaining missing cases (i.e., survey and item nonresponse), wage/salary income is imputed using the 25th percentile of the wage/salary income distribution conditional on year, sex, race, and decile of the Armed Forces Qualification Test (AFQT).[12] We supplement this method by accounting for poor income recall. Specifically, prior to performing Nielsen's imputation steps, if a respondent reported working a nonzero number of weeks but also reported a 0 wage/salary income, these responses are recoded to missing and imputed using the aforementioned steps. Furthermore, wage/salary income is recoded to 0 if respondent reported working 0 weeks but also did not report a valid wage/salary income. After wage/salary income is imputed, we rescale based on the share of weeks worked during the year (inclusive of time in the armed forces). Upon completion of imputation, we sum wage/salary income over the desired age ranges and take the natural log, conditional on the respondent having a full panel of post-imputation wages.[13]

Marital status identifies individuals that were currently married, separated, divorced, remarried, or widowed by ages 25 and 35. As such, we refer to this status as ever married by age 25 and ever married by age 35.

Finally, fertility is measured by the number of births of biological children by ages 25 and 35. There are several variables in the NLSY79 that count each respondent's

[12] Aptitude tests such as the AFQT have well-documented issues with accurately capturing aptitude across sociodemographic groups (e.g., Rodgers III and Spriggs 2002). However, given that labor market compensation often reflects similar biases, we proceed with following Nielsen (2023).

[13] We add 1 before taking the log to avoid dropping respondents with 0 cumulative earnings.

number of children, including the number of births and the number of children in the household (including nonbiological children). We focus on biological births because the roster of children including nonbiological children may suffer from compositional changes from year to year. There is no way to be certain if the set of children listed in one year are the exact same as the set in the following year. The variable that tracks the cumulative number of children born[14] is unaffected by compositional changes, though we acknowledge that this undercounts people who become parents by other means. Although program participation is an important piece of the cost-benefit puzzle, our data are sufficiently imprecise that we do not include the outcome in our analysis.[15]

Results

We begin by analyzing the effect of eligibility for Social Security's student benefit on the rate of college attendance, a loose replication of Dynarski (2003), and a test of what we assume will be a primary mechanism for any substantive effect on lifetime earnings. Our results are shown in Table 3 below. Eligibility for the student benefit increased the rate of college attendance by age 23 in our overall sample and for several subgroups, though the estimates are mostly insignificant. Depending on the model

[14] The NLSY79, prior to 1992, asked female respondents their total number of pregnancy episodes. Starting in 1992, the NLSY79 only asked about pregnancies that ended in live births.

[15] Additionally, the NLSY79 does not separately report income from disability insurance. Veterans' benefits, disability, and worker's compensation are jointly reported for much of the survey. Starting in 2002, the NLSY79 prompted respondents to report disability income, but it was jointly reported with the respondent's spouse. Supplemental Security Income was reported (jointly with other program income) regularly, but Social Security Disability Insurance was not reported separately until 2018. For these reasons, we do not include disability income as an outcome.

specification (no controls versus with controls), we see suggestive, but not statistically significant (*p*>0.1), evidence of eligibility increasing college attendance for most subgroups.

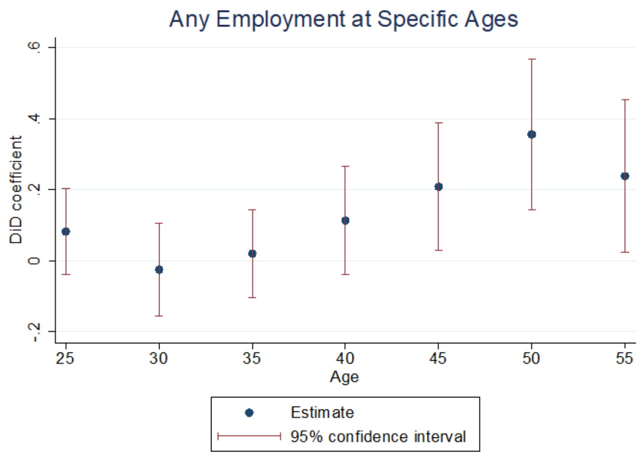
Table 3: Effect of eligibility for student benefit on any college attendance by age 23

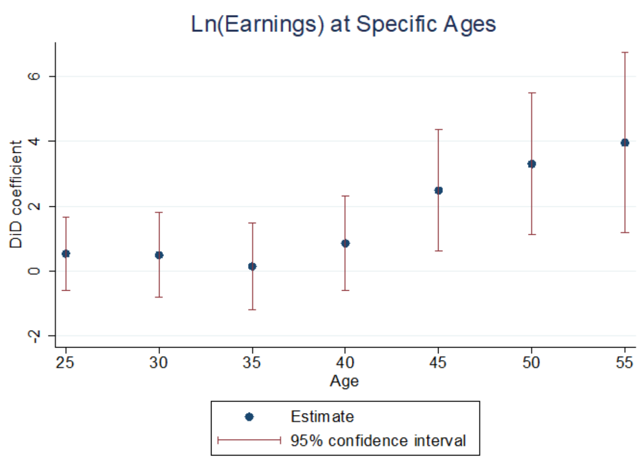
	Without controls		With controls	
Demographic	**Linear probability model**	**Logit marginal effects**	**Linear probability model**	**Logit marginal effects**
Overall	0.1228 (0.0898) [2886/149]	0.1204 (0.0849) [2886/149]	0.0526 (0.1086) [2536/111]	0.0596 (0.1142) [2536/111]
Male	0.0775 (0.1376) [1458/78]	0.0785 (0.1387) [1458/78]	0.1795 (0.178) [1293/58]	0.1866 (0.1676) [1293/58]
Female	0.1847 (0.1194) [1428/71]	0.174* (0.1015) [1428/71]	-0.0016 (0.1373) [1243/53]	-0.0021 (0.1539) [1243/53]
Black	0.1448 (0.1209) [956/73]	0.1464 (0.1159) [956/73]	0.0242 (0.1631) [740/45]	0.022 (0.1845) [740/45]
Non-Black, non-Hispanic	0.1361 (0.1368) [1930/76]	0.1326 (0.1291) [1930/76]	0.0858 (0.141) [1796/66]	0.0871 (0.1506) [1796/66]

Note(s): Estimates presented include the beta coefficient, standard error (in parentheses), and Ns (total/satisfying DiD components) for individual regressions. The demographic is the subset of the population for which the regression was estimated. Outcome is a binary for any college attendance by age 23. Models with controls include regressors for family's income when a senior in high school (scaled to 10,000s); father attended college (binary); mother attended college (binary); single parent household (binary); family size; age in 1988; fixed effects for state when a senior in high school; interactions of each of the aforementioned variables with high school cohort; and an indicator for imputed family income. Significance levels: *=0.10; **=0.05; ***=0.01.

We test two earnings two ways. First, we regress earnings levels and employment probability at different ages on student benefit eligibility. Figure 1 plots the DiD coefficients and 95% confidence intervals for those regressions. Eligibility for the student benefit increased both the rate of any employment and log earnings at almost all ages, but the effect was not statistically significant until age 45.

Figure 1: Effect of eligibility for student benefit on employment and earnings at specific ages





Note(s): Estimates presented include the beta coefficient and 95% confidence intervals for regressions at different ages (Ns included in parentheses by x-axis label). Outcome is a binary for any employment (i.e., worked at least 1 week at that age) and the log of earnings at that age. After 1994, the NLSY79 was conducted every two years. As a result, individuals may not have been surveyed at the appropriate age (e.g., surveyed at ages 49 and 51, but not 50). To increase our sample size, we include cases where the individual was surveyed in the year prior and treat that as data for the considered age. In the previously mentioned example, an individual surveyed at age 49 would be included in the age 50 regression sample. Controls include regressors for race (Black versus non-Black, non-Hispanic); sex (female versus male); father attended college (binary); mother attended college (binary); and fixed effects for state where living at that age. Models are unweighted.

Estimates for lifetime earnings are presented in Table 4, in which we test cumulative (read: added up) lifetime earnings over different age ranges: 19 to 62, a broad measure of total adult earnings bookended by the end of high school on one end and eligibility for early Social Security retirement on the other; and the prime age, 25 to 54. Consistent with the findings of increased earnings at later ages, we find that lifetime earnings are larger for individuals eligible for the student benefit compared to those similarly situated but ineligible. The overall effect is positive but insignificant, with the estimate being somewhat larger for the prime-working wage group compared to the 19 to 62 age range.

To confirm the results by age previously tested, we loosen the ranges for the front end (19 to 54) and the back end (25 to 62) in the final two columns of the table and show the overall increase in lifetime earnings is driven by earnings at middle and later ages. This makes sense, as noncollege attenders are likely to have more years of experience in the labor market at early ages, garnering additional years of earning before a college degree has time to pay off. In addition, the result is driven by women, who see higher lifetime earnings for all four combinations of age ranges. The overall effect for men is negative but very small, and not statistically distinguishable from zero. Our analyses by race trend positive but are imprecise.[16]

[16] For sample counts of included covariates, see Appendix Table 4.

Table 4: Effect of eligibility for student benefit on lifetime earnings

Demographic	19-62	25-54	19-54	25-62
Overall	0.2607	0.3311	0.2581	0.3288
	(0.2006)	(0.234)	(0.2006)	(0.2323)
	[1811/64]	[1811/64]	[1811/64]	[1811/64]
Male	-0.0952	-0.0905	-0.1134	-0.0718
	(0.2516)	(0.274)	(0.25)	(0.276)
	[902/33]	[902/33]	[902/33]	[902/33]
Female	0.6051**	0.7361**	0.6182**	0.7129**
	(0.3011)	(0.3589)	(0.302)	(0.3546)
	[909/31]	[909/31]	[909/31]	[909/31]
Black	0.3396	0.3586	0.3374	0.3611
	(0.2875)	(0.3193)	(0.2865)	(0.3207)
	[569/25]	[569/25]	[569/25]	[569/25]
Non-Black, non-Hispanic	0.1542	0.2952	0.1551	0.2832
	(0.3101)	(0.3882)	(0.313)	(0.3793)
	[1242/39]	[1242/39]	[1242/39]	[1242/39]

Note(s): Significance levels ***=0.01; **=0.05; *=0.10. Estimates presented include the beta coefficient, standard error (in parentheses), and N's (total/satisfying DiD components) for individual regressions. The demographic is the subset of the population for which the regression was estimated. Ages represent the ages over which lifetime earnings are summed. Controls include race (Black versus non-Black, non-Hispanic); sex (female versus male); father attended college (binary); mother attended college (binary). We restrict the sample in this table to individuals with a full set of post-imputed wages for all age ranges. For example, if a respondent had valid data for 19 to 54, but not for 19 to 62, they were excluded from this analysis.

In Table 5, we test the college enrollment mechanism by splitting the two sets of cohorts by their college attendance for lifetime earnings across the two age ranges (never attended, completed less than two years, completed two or more years). Due to the small cell size for the eligible population, we limit these analyses to the overall sample. The effect for up to two years of college attendance is statistically significant and dominates the effect from individuals that completed two or more years of college (as well as nonattenders for whom the effect is positively signed but not statistically significant). This likely signifies that eligibility for the student benefit helped the marginal individual who might not have attended any college if not for receiving the student benefit from Social Security.

Table 5: Effect of eligibility for student benefit on lifetime earnings split by number of years of college completion

	19-62			25-54		
Demographic	**2+ years completion**	**Less than 2 years completed**	**Never attended**	**2+ years completion**	**Less than 2 years completed**	**Never attended**
Overall	-0.1760	1.4548***	0.1294	-0.1191	1.9580***	0.2193
	(0.2509)	(0.3349)	(0.3220)	(0.2736)	(0.2489)	(0.3804)
	[657/23]	[238/14]	[646/21]	[681/25]	[254/15]	[686/24]

Note(s): Significance levels ***=0.01; **=0.05; *=0.10. Estimates presented include the beta coefficient, standard error (in parentheses), and N's (total/satisfying DiD components) for individual regressions. The demographic is the subset of the population for which the regression was estimated. Ages represent the ages over which lifetime earnings are summed. Controls include race (Black versus non-Black, non-Hispanic); sex (female versus male); father attended college (binary); mother attended college (binary). Analyses are limited to the overall sample due to small treated population.

Finally, in Table 6, rather than examine by gender or race, we test lifetime earnings by sibling position. Our motivation in doing so was a benefit issue. Families receiving spousal and dependent benefits were subject to a family maximum, or cap. For large enough families, having one fewer dependent would not change the total amount the family received. But for families with only one or two children, the end of a child benefit could greatly reduce the total amount the family received. This could have resulted in benefits inducing younger or only children to attend college and maintain family benefits. However, when we tested for sibling number and birth order, we consistently found the opposite, as shown in Table 6. The student benefit had a much larger effect on the lifetime earnings of older siblings. Rather than family benefit gaming, the student benefit appears to have enabled elder siblings of families with a deceased father to stay in school. This is consistent with the argument that the benefit was not financial aid, but rather income replacement, allowing older siblings to invest in human capital rather than serve as a replacement breadwinner.

Table 6: Effect of eligibility for student benefit on lifetime earnings split by sibling position

	19-62		25-54	
Demographic	**Youngest**	**Not Youngest**	**Youngest**	**Not Youngest**
Overall	-0.2583	0.4495*	-0.2105	0.5730**
	(0.2753)	(0.2491)	(0.2744)	(0.2904)
	[587/27]	[1164/37]	[618/31]	[1224/40]
Male	-0.5315	0.1010	-0.3903	0.1485
	(0.3926)	(0.3139)	(0.3385)	(0.3297)
	[299/12]	[564/21]	[318/14]	[604/24]
Female	0.0527	0.7483**	-0.0211	0.9761**
	(0.3423)	(0.3704)	(0.4204)	(0.4393)
	[288/15]	[600/16]	[300/17]	[621/16]
Black	0.3385	0.1623	0.2733	0.1960
	(0.2078)	(0.3469)	(0.2223)	(0.3749)
	[141/8]	[413/17]	[153/10]	[443/20]
Non-Black, non-Hispanic	-0.4906	0.8481**	-0.4555	1.1811**
	(0.3710)	(0. 3924)	(0.4048)	(0.5822)
	[446/19]	[751/20]	[465/21]	[781/20]

Note(s): Significance levels ***=0.01; **=0.05; *=0.10. Estimates presented include the beta coefficient, standard error (in parentheses), and Ns (total/satisfying DiD components) for individual regressions. The demographic is the subset of the population for which the regression was estimated. Ages represent the ages over which lifetime earnings are summed. Controls include race (Black versus non-Black, non-Hispanic); sex (female versus male); father attended college (binary); mother attended college (binary). “Youngest sibling” includes only children.

Table 7 presents the effect of eligibility for the student benefit on marriage outcomes by ages 25 and 35. To maintain eligibility, recipients need to be unmarried, potentially delaying marriage by a few years (or suppressing it entirely) for the enrolling population. While overall the coefficients for having ever been married by both 25 and 35 are not statistically significant, the coefficients for by age 35 trend negative, suggesting that eligibility might delay, or potentially even reduce, marriage for individuals receiving the student benefit (perhaps in order to meet the eligibility condition of being unmarried). The negative coefficient is driven by non-Black, non-Hispanic eligible respondents, who are less likely to be ever married by age 25 than those ineligible for the student benefit.

Conversely, Black respondents eligible for the student benefit were *more* likely to have been married by age 25, with no statistically significant longer-term effects, suggesting the benefit (potentially through college) facilitated marriage. College serves as a marriage market, promoting assortative mating among individuals with higher earning capacity (Pestel 2017, Kirkebøen et al. 2021). Earnings prospects also influence marriage markets (e.g., Kearney and Wilson 2018), leading to two potential channels of influence.

Table 7: Effect of eligibility for student benefit on marriage by certain ages

	Ever married by age 25		Ever married by age 35	
DiD coefficient (se) [N/N DiD]	**Linear probability model**	**Logit marginal effects**	**Linear probability model**	**Logit marginal effects**
Overall	0.0263	0.0273	-0.0215	-0.0174
	(0.0923)	(0.0970)	(0.0825)	(0.0792)
	[2658/101]	[2658/101]	[2626/97]	[2626/97]
Male only	0.1751	0.1869	-0.0295	-0.0280
	(0.1270)	(0.1347)	(0.1293)	(0.1221)
	[1376/52]	[1376/52]	[1352/53]	[1352/53]
Female only	-0.1372	-0.1452	-0.0117	-0.0091
	(0.1307)	(0.1442)	(0.1000)	(0.0992)
	[1282/44]	[1282/44]	[1274/39]	[1274/39]
Black only	0.2715**	0.2803**	0.0355	0.0354
	(0.1286)	(0.1275)	(0.1311)	(0.1303)
	[856/50]	[856/50]	[827/45]	[827/45]
Non-Black,	-0.1933	-0.2064	-0.0605	-0.0580
non-Hispanic only	(0.1286)	(0.1383)	(0.1024)	(0.1099)
	[1802/51]	[1802/51]	[1799/52]	[1799/52]
Attended any college by age 23	0.1901	0.1944	0.0132	0.0099
	(0.1526)	(0.1534)	(0.1430)	(0.1047)
	[1273/59]	[1273/59]	[1243/55]	[1243/55]
Did not attend any college	0.0511	0.0535	0.0229	0.0240
by age 23	(0.1486)	(0.1494)	(0.1471)	(0.1187)
	[971/33]	[971/33]	[967/32]	[967/32]

Note(s): Significance levels ***=0.01; **=0.05; *=0.10. Estimates presented include the beta coefficient, standard error (in parentheses), and Ns (total/satisfying DiD components) for individual regressions. The demographic is the subset of the population for which the regression was estimated. Ages represent the ages over which lifetime earnings are summed. Controls include race (Black versus non-Black, non-Hispanic); sex (female versus male); father attended college (binary); mother attended college (binary). Note that the sign change in coefficients for the overall model compared to the stratified college models (the bottom two sets of estimates) can be attributed to 414 respondents dropped from the unstratified model because they did not have a valid college attendance variable.

Table 8: Effect of eligibility for student benefit on fertility by certain ages

	By age 25				By age 35			
	At least one child (binary)		2+ children (binary)		At least one child (binary)		2+ children (binary)	
DiD coefficient (se) [N/N DiD]	**Linear probability model**	**Logit marginal effects**	**Linear probability model**	**Logit marginal effects**	**Linear probability model**	**Logit marginal effects**	**Linear probability model**	**Logit marginal effects**
Overall	0.0427	0.0437	0.0948*	0.1782	0.0791	0.0695	0.1593	0.1568
	(0.0961)	(0.1061)	(0.0539)	(0.1602)	(0.0978)	(0.0752)	(0.1024)	(0.0961)
	[2585/97]	[2585/97]	[2585/97]	[2585/97]	[2419/89]	[2419/89]	[2419/89]	[2419/89]
Male only	0.1718	0.1836	0.0803	0.0920	0.1619	0.1378*	0.1951	0.1929
	(0.1098)	(0.1315)	(0.0739)	(0.1255)	(0.1198)	(0.0814)	(0.1249)	(0.1180)
	[1450/64]	[1450/64]	[1450/64]	[1450/64]	[1352/59]	[1352/59]	[1352/59]	[1352/59]
Female only	-0.0909	-0.1006	0.0821	0.1075	0.0360	0.0345	0.1692	0.1609
	(0.1453)	(0.1389)	(0.0927)	(0.1696)	(0.1319)	(0.1109)	(0.1446)	(0.1271)
	[1240/46]	[1240/46]	[1240/46]	[1240/46]	[1164/41]	[1164/41]	[1164/41]	[1164/41]
Black only	0.1843	0.1808	0.0974	0.1642	0.1896	0.1257*	0.1152	0.1123
	(0.1394)	(0.1280)	(0.0961)	(0.1982)	(0.1379)	(0.0642)	(0.1499)	(0.1383)
	[835/49]	[835/49]	[835/49]	[835/49]	[778/40]	[778/40]	[778/40]	[778/40]
Non-Black, non-Hispanic only	-0.1029	-0.0886	0.0942*	0.9466***	-0.0412	-0.0404	0.1997	0.2028
	(0.1288)	(0.0900)	(0.0480)	(0.0047)	(0.1313)	(0.1369)	(0.1364)	(0.1345)
	[1750/48]	[1750/48]	[1750/48]	[1750/48]	[1641/49]	[1641/49]	[1641/49]	[1641/49]
Attended any college by age 23	0.2451*	0.3008	0.1432***	0.9707***	0.2586	0.1928**	0.2156	0.2136
	(0.1326)	(0.2198)	(0.0490)	(0.0037)	(0.1693)	(0.0875)	(0.1676)	(0.1590)
	[1249/58]	[1249/58]	[1249/58]	[1249/58]	[1144/48]	[1144/48]	[1144/48]	[1144/48]
Did not attend any college by age 23	-0.0015	-0.0006	0.0625	0.0792	0.1067	0.0773	0.0615	0.0615
	(0.1592)	(0.1747)	(0.1180)	(0.1745)	(0.1462)	(0.0774)	(0.1654)	(0.1593)
	[954/30]	[954/30]	[954/30]	[954/30]	[910/31]	[910/31]	[910/31]	[910/31]

Note(s): Significance levels ***=0.01; **=0.05; *=0.10. Estimates presented include the beta coefficient, standard error (in parentheses), and Ns (total/satisfying DiD components) for individual regressions. The demographic is the subset of the population for which the regression was estimated. Ages represent the ages over which lifetime earnings are summed. Controls include race (Black versus non-Black, non-Hispanic); sex (female versus male); father attended college (binary); mother attended college (binary).

Table 8 presents estimates of the effect of eligibility for Social Security's student benefit on fertility outcomes. The results suggest that student benefit eligibility marginally increased the likelihood of having at least two children by age 25 by 9.5 percentage points when using a linear probability model. Moreover, the effect appears to be driven by non-Black, non-Hispanic respondents and individuals who attended some college by the age of 23. By age 35, there is marginal evidence that eligibility increased the likelihood of men having at least one child. Meanwhile, there is consistent evidence across specifications (i.e., linear probability model and logit) that Black respondents are more likely to have at least one child by age 35. One could rationalize this increased total fertility (versus a change in timing of fertility) as further evidence that children are a "normal" good — as the benefit increases lifetime earnings, our increased total fertility findings are consistent with Becker's (1981) contention and results from Black et al. (2013).

Discussion

Labor

Our results suggest that there is a causal relationship between college attendance and lifetime earnings, especially for women. In this interpretation, attendance exogenously varies via the student benefit, which greatly lowered the probability of attendance in a way that was uncorrelated with ability. Individuals with access to college earned more, indicating that college increases earnings. The labor market — both its demographic and industrial composition — has changed since benefit was offered and then ended. Do we have reason to think that results would be similar today? Or put differently, is this a causal relationship that holds in general or one that held at least for this group? We think our findings offer dueling interpretations of applicability.

Recall that our findings showed that the earnings effects of the student benefit didn’t grow significantly until older ages (Figure 1) and were consistently larger for women (Table 4). College education is seen by the labor market as more than a bundle of skills and knowledge. Postsecondary credentials can act as signals in the labor market, conveying future commitment to the labor market, capability of finishing things, and perseverance. Prior evidence has substantiated differential returns to credentials by gender through signaling (e.g., Nielsson and Steingrimsdottir 2018; Baird, Bozick, and Zaber 2021), and signaling labor market commitment may have been especially important for the women of this cohort.

A college degree also insulates workers from economic downturns and layoffs (e.g., Li, Wallace, and Hyde 2019). And prior research has found that a large portion of the college wage premium can be attributed to occupational sorting into careers with high wage growth trajectories over their tenure (rather than to high wages at the outset), another explanation for our results (Deming 2023). Layoff risk varies by occupation, and there is a long literature documenting occupational sorting by gender (e.g., Blau, Brummund, and Liu 2013; Busch 2020), There are a constellation of labor market factors that could substantiate our differential return for women; we are unable to determine the precise mechanism in this study.

Recall that we also found a large sibling prediction: Older siblings had the biggest lifetime earnings gains (Table 6), consistent across subgroups. We hypothesize that older children have pressure or obligation to start earning money to help support their younger siblings as soon as they are able. With a deceased father and mothers who are either unable to work or who earn little, elder siblings may see four years of continued education as too costly given the needs of their family and their ability to earn immediately in the labor market. However, with the offer of highly subsidized education (or a cash benefit able to be shared directly with family), they were able to attend college and earn more. Their higher earnings were not variable or time delayed, but significant across both age ranges overall and for women and non-Black, non-Hispanic individuals.

From the perspective of women, the causal effect of college on earnings could arguably be very cohort specific. From the perspective of older siblings, college attendance's causal effect on earnings is arguably very broad. Additional research is needed to further interrogate these potential hypotheses, and we note that both could be true — they are not mutually exclusive or contradictory in a population.

College finance

College-going today is very different from the late 1970s and early 1980s. To start, the cost of college and how it compares to the way people might pay for it has changed. Across two- and four-year public institutions, annual tuition and fees averaged $635 in 1980/1981, equivalent to $2,067 in 2021/2022 dollars (U.S. Department of Education 2023). In 2021/2022, that had grown almost fourfold to $7,869. At private institutions, tuition has increased from the equivalent of $11,388 to $33,691, tripling after accounting for inflation.

To contextualize these numbers, consider the minimum wage. In 1980, an entry-level worker could expect to earn at least $3.10 per hour, so working just 10 percent time — four hours a week — could cover the year's tuition and fees at a public institution. A 2021/2022 student would need to work 20 hours a week to be able to afford a public institution on a pay-as-you-go model, an intensity likely unsustainable with full-time study. Consequently, the share of students using loans and the average amount of loans has increased over time (Taylor et al., 2011).

Who is attending college has also changed. In 1980, less than 20 percent of Black or Hispanic youth aged 18 to 24 were enrolled in college (Pew Research Center 2011), compared to approximately 30 percent today (National Center for Education Statistics 2023b). The share of women attending and completing college has increased faster than the share of men attending and completing (Pew Research Center 2011). Although the earnings returns on a college degree vary by race and gender, the college wage premium — in terms of the earnings ratio between college degree holders and high school diploma holders — is relatively consistent across race and gender (Geary 2022). The college wage premium rose during this period and has still grown considerably despite recent flattening.

A student benefit today of the current dependent benefit size (an average of $12,800 per year for children of deceased workers; SSA 2023) would likely do more on the intensive margin of college persistence and completion by reducing the need to work (along the lines of Kofoed 2022) than on the extensive margin of enrollment, for two reasons. One, rates of college enrollment are much higher today, having risen 13 percentage points since 1980 (National Center for Education Statistics 2023a; National

Center for Education Statistics 2012). There were particularly large gains in rates of attendance for low- and middle-income students (National Center for Education Statistics 2012). Two, for those eligible for Pell grants, the student benefit and Pell could cover most educational expenses at a public college, but even combined, they would not come close to covering costs at a private institution. A boost to completion rates or speeding of completion timing would likely still increase lifetime earnings, but today's student on the margin of attending college is distinct from the student on the margin in 1980.

Social Security

Our results suggest that the public-finance assessment of ending the student benefit may have been negative. Ending the student benefit shortened the duration of dependent benefits for those attending college by four years. It represents immediate cost savings. However, 40 years removed from that decision, those who could go to college saw higher lifetime earnings, especially in later years. Social Security's tax base is the earnings of workers in covered employment. Benefit calculations are progressive, so that higher earning workers have larger benefits, but those benefits replace a smaller share of prior earnings. Those higher earnings could have translated into higher tax collection for benefits replaced at a lower rate. From our results, we cannot say definitively that the effect was large enough to be a net cost to the program. However, Social Security does have the data to calculate this amount exactly.

Public finance aside, we bring up again the sibling result. Social Security is an insurance benefit, meant to replace the wages of a covered worker in specific circumstances. That benefit was extended to spouses and children in 1939 — before

Social Security had even paid its first benefit — as a recognition that workers are not isolated but exist in families. That eldest siblings had the largest lifetime earnings effects illustrates that point. Without a living, working father, elder siblings enter the workforce. The student benefit enabled them to attend college and earn more. It speaks to symbolic notion of what is lost with a worker's wages and what is insured by the program.

What now?

We have provided evidence that for a subset of late-1970s high school students, access to college through the student benefit improved lifetime earnings, potentially enough to make the program cost-neutral. For the cohort studied in the paper, the largest wage gains were for those with *some* college attendance, rather than isolated to those who completed a degree. Yet, evidence from contemporary financial aid literature suggests that in today's college environment, the larger benefit may come more from persistence than from enrollment. Enrollment has increased more than completion, a dynamic that was less at play for our study population. From a purely economic standpoint, it is unclear if additional tax revenue via wage gains from persistence would be enough to offset the initial outlay of the benefit, should the student benefit be restarted today.

However, the student benefit was never intended as an investment that would yield economic returns. When extending the dependent benefit to enrolled students, the 1965 House Ways and Means Committee stated, "The committee believes that a child over age 18 who is attending school full time is dependent just as a child under 18 or a disabled older child is dependent, and that it is not realistic to stop such a child's benefit at age 18” (DeWitt 2001). By this philosophy, the student benefit was income support to enable emerging adults dependent on Social Security to have choices about their school and career path. This paper shows that that support was not trivial. It affected their choices in the short-term and their outcomes in the long-term. For the first group ineligible for the benefit, that manifested as short-term pullback from college and long-term losses in income. For other groups at other times, those short- and long-term consequences could (and likely would) be different.

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Appendix

Appendix Table 1a: Descriptive statistics of sample as of the first survey wave

Sample	**N**	**% of Total**
Total cohort	12,686	100.00%
Sex (cross + supplemental + military)		
Male	6,403	50.50%
Female	6,283	49.50%
Race-ethnicity (cross + supplemental + military)		
Non-Black, non-Hispanic	7,510	59.20%
Black	3,172	25.00%
Hispanic	2,002	15.80%
Subsample		
Cross-sectional	6,111	48.20%
Supplemental	5,295	41.70%
Military	1,280	10.10%

Source: NLSY79

Appendix Table 1b: Variable definitions

Variable	Definition
Household income	Family's income when respondent is a senior in high school (imputed conditional on father's mortality status and high school cohort if missing)
Single parent household	Defined using the household registers in 1979 and 1980, which considers the presence of parents and stepparents. For respondents that were seniors in high school in either 1979 or 1980, we use the register from that year. If the respondent was a senior between 1981 and 1983, we apply the household register from 1980.
Attend college by 23	Respondent has an enrollment status of "enrolled in college" in at least one year by turning age 23 or responded that they are enrolled at least part time
Complete any college by 23	Respondent has a highest grade completed of "1st year college" or more in at least one year by turning age 23
Years of schooling at 23	Equivalent to highest grade completed

Appendix Table 2: High school senior cohort and father's mortality status for respondents in survey at multiple ages

Sample	High School Senior Cohort			Father's Mortality Status		
	1979-1981	1982-1983	Undetermined	Deceased Before Turning 18	Not Deceased Before Turning 18	Undetermined
Ages 39-40						
Responded to survey	2,273	842	4,861	330	7,498	148
	28.5%	10.6%	60.9%	4.1%	94.0%	1.9%
Did not respond to survey	808	298	3,604	169	4213	328
	17.2%	6.3%	76.5%	3.6%	89.4%	7.0%
Ages 49-50						
Responded to survey	2,132	763	4,536	300	7,013	118
	28.7%	10.3%	61.0%	4.0%	94.4%	1.6%
Did not respond to survey	949	377	3,929	199	4,698	358
	18.1%	7.2%	74.8%	3.8%	89.4%	6.8%

Note(s): Percent of sample present beneath raw counts. For example, for ages 49 to 50, 2,132 respondents from the initial cohort were interviewed at those ages and were a part of the 1979 to 1981 cohort out of 7,431 people that responded to the survey at those ages.

Appendix Table 3: Descriptive statistics of sample by high school cohort and father's mortality status, including Hispanic

Variable	High School Seniors 1979-1981: Father Not Deceased	High School Seniors 1979-1981: Father Deceased	High School Seniors 1982-1983: Father Not Deceased	High School Seniors 1982-1983: Father Deceased
Household Income (USD2000)	$53,907	$31,528	$47,288	$23,912
Black	12.5%	26.3%	12.5%	33.1%
Hispanic	4.9%	5.7%	6.0%	4.3%
Father Attended College	36.2%	19.4%	33.2%	15.8%
Mother Attended College	25.1%	13.7%	22.8%	7.0%
Single Parent Household	12.5%	79.1%	14.7%	82.3%
Family Size	5	4	5	4
Age in 1988	25	25	23	23
Female	48.6%	51.8%	46.8%	47.2%
Attend College By 23	56.6%	61.4%	55.5%	36.6%
Complete Any College By 23	49.2%	52.8%	47.3%	28.4%
Years of Schooling At 23	13	13	13	13
Number of Observations	2,825	192	1,038	81
Observations in Dynarski (2003)	2,745	137	1,050	54

Note(s): Household income is reported in USD2000 and is conditioned on father's mortality status and cohort (1979 to 1981 versus 1982/1983). Income adjusted using series CPALTT01USM661S downloaded from the St. Louis Federal Reserve's Economic Database (OECD 2023).

Appendix Table 4: High school senior cohort and father's mortality by race/ethnicity and sex, overall and conditional of valid responses for additional covariates

Variable	non-Black, non-Hispanic	Black	Hispanic	Male	Female
		Overall			
High school senior cohort					
Senior between 1979-1981	1752	871	458	1531	1550
Senior between 1982-1983	650	309	181	605	535
Undetermined	4157	1743	1285	3443	3742
Father deceased before turning 18					
No	6141	2608	1763	5112	5400
Yes	202	222	75	264	235
Undetermined	216	93	86	203	192
Interaction					
HS Senior 1979-1981 & Father Deceased Before 18	84	86	22	96	96
Only one of HS Senior 1979-1981 or Father Deceased Before 18	2278	1061	605	1995	1949
Undetermined	4197	1776	1297	3488	3782
		Conditional on covariates			
High school senior cohort					

Senior between 1979-1981	1372	562	329	1135	1128
Senior between 1982-1983	401	169	102	346	326
Undetermined	0	0	0	0	0
Father deceased before turning 18					
No	1695	673	409	1405	1372
Yes	78	58	22	76	82
Undetermined	0	0	0	0	0
Interaction					
HS Senior 1979-1981 & Father Deceased Before 18	62	43	18	62	64
Only one of HS Senior 1979-1981 or Father Deceased Before 18	1708	688	413	1419	1390
Undetermined	0	0	0	0	0

Note(s): Covariates considered include any college attendance by age 23, AFQT percentile, father attended college, mother attended college, single parent household family size and geographic region. These covariates are included in at least one regression analysis discussed later and therefore affect the sample included for analysis.

Appendix Table 5: Effect of eligibility for Social Security's student benefit on lifetime earnings split by number of years of college completion

	19-62			25-54			19-54			25-62		
Demographic	**2+ years**	**Less than 2 years**		**2+ years**	**Less than 2 years**	**Never attended**	**2+ years**	**Less than 2 years**	**Never attended**	**2+ years**	**Less than 2 years**	**Never attended**
Overall	-0.1760	1.4548***	0.1294	-0.1191	1.9580***	0.2193	-0.1321	1.4648***	0.1857	-0.1686	1.9424***	0.1547
	(0.2509)	(0.3349)	(0.3220)	(0.2736)	(0.2489)	(0.3804)	(0.2422)	(0.3163)	(0.3199)	(0.2823)	(0.2658)	(0.3778)
	[657/23]	[238/14]	[646/21]	[681/25]	[254/15]	[686/24]	[681/25]	[250/15]	[676/22]	[657/23]	[242/14]	[656/23]
Male	-0.2626		-0.0745	-0.1721		0.0944	-0.1874		0.0274	-0.2573		-0.0010
	(0.2895)		(0.4128)	(0.2917)		(0.4170)	(0.2632)		(0.4015)	(0.3207)		(0.4258)
	[315/13]		[338/13]	[329/14]		[362/16]	[329/14]		[353/14]	[315/13]		[347/15]
Female	-0.0510		0.3286	0.0845		0.3120	0.0569		0.3098	-0.0332		0.3064
	(0.2363)		(0.4999)	(0.2642)		(0.6182)	(0.2466)		(0.4993)	(0.2537)		(0.6135)
	[342/10]		[308/8]	[352/11]		[324/8]	[352/11]		[323/8]	[342/10]		[309/8]
Black	-0.2911		0.0955	-0.2515		0.0879	-0.2264		0.1214	-0.3299		0.0585
	(0.4630)		(0.5861)	(0.4650)		(0.5883)	(0.4073)		(0.5584)	(0.5295)		(0.6117)
	[179/6]		[215/7]	[185/7]		[235/9]	[185/7]		[228/8]	[179/6]		[222/8]
Non-Black, non-Hispanic	-0.1169		0.4440	-0.0809		0.3770	-0.0944		0.2697	-0.1065		0.2989
	(0.1393)		(0.6239)	(0.1435)		(0.5967)	(0.1347)		(0.4498)	(0.1478)		(0.5751)
	[478/17]		[431/14]	[496/18]		[451/15]	[496/18]		[448/14]	[478/17]		[434/15]

Note(s): Significance levels ***=0.01; **=0.05; *=0.10. Estimates presented include the beta coefficient, standard error (in parentheses), and N's (total\satisfying DiD components) for individual regressions. The demographic is the subset of the population for which the regression was estimated. Ages represent the ages over which lifetime earnings are summed. Controls include race (Black versus non-Black, non-Hispanic); sex (female versus male); father attended college (binary); mother attended college (binary). Regressions for less than two years college completed are limited to the overall sample due to small treated population (14 total respondents with deceased fathers in eligible cohorts with less than two years of college). Regressions for non-Black, non-Hispanic, and 2+ years of completion exclude the indicator for father's mortality because it is perfectly collinear with the DiD indicator.