Pre-Analysis Plan

The Effects of (Free) College on Earnings and Health Across the

Life Cycle

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Correlational evidence suggests that college graduates outperform their less educated peers in various measures of life success, including earnings and health. However, it is unclear the extent to which the observed correlations are driven by the endogenous selection of individuals into college. We will overcome endogeneity concerns by leveraging the elimination of the Social Security Student Benefit Program. We will use a large sample of administrative Social Security records to precisely identify individuals impacted by the elimination of the program. Linking these individual records to Census Bureau survey data will allow us to estimate the long run effects of college attainment on earnings and health and how these effects vary across the life course.

To improve research transparency, this pre-analysis plan outlines the technical details of the planned study. The plan includes descriptions of the sources of original data and the choice and construction of key and secondary outcome variables. The plan also details the specifications, sets of controls, robustness and internal validity exercises, and heterogeneity analysis that will be conducted. At the time of writing, the analysis dataset has not been created and no analysis has been conducted. Specifying these details in advance allows researchers and others to have a high level of confidence that the reported results reflect true effects.

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1 Project Motivation

Understanding the returns to higher education has relevant implications both for policy-makers seeking to optimize resource allocation for education as well as individuals considering large investments in either their own or their children's education. While there has been significant inquiry into the extent to which higher education enhances labor market outcomes, much of the evidence is for very short-term outcomes, relies on questionable identifying assumptions, or is limited by the context. Even less understood are the effects of higher education on health and health behaviors.[1] While those who attend or complete college exhibit healthier behaviors and have better health, we have little understanding as to whether these relationships are causal. The answer to this question has important implications for optimal investments in and the subsidization of higher education. For example, those with a college degree are significantly less likely to have a chronic disease than those with a high school degree (Choi et al., 2011).[2] If this relationship is causal it suggests enormous social benefits of higher education through reduced healthcare costs and increased resiliency to disease. Measuring the extent of health effects is critical to thinking about how investments in education may translate to downstream improvements in the health and functioning of society as well as understanding the optimal level of public funding for higher education. Unfortunately, we have little understanding of the causal relationship between higher education and these types of non-pecuniary outcomes.

The existing evidence linking higher education and improved health relies almost entirely on correlations between college attainment and non-pecuniary outcomes or twin approaches.[3] While these studies suggest a positive relationship between college attainment and health,

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[1]These outcomes are often associated with the positive externalities generated by education. To the extent that these externalities are causally related to investments in higher education, the market will lead to sub-optimal investments because individuals do not consider the effects of these externalities in their investment decisions. For example, an 18-year-old considering college enrollment may not consider the spillover effects of worsened health (e.g., others getting sicker, higher insurance costs) that might result from not obtaining a college degree.

[2]For example, those with a college degree are roughly 40 percent less likely to have cardiovascular disease than those with a high-school degree.

[3]See Oreopoulos and Salvanes (2011) for one recent overview.

they rely on strong assumptions about selection into schooling to reach these conclusions. It remains unclear whether the positive association between college attainment and health outcomes is a result of college or merely reflects the selection of healthier individuals, even within twin sets, into college. The few studies that attempt to address this selection issue are limited by their contexts, relying heavily on variation in attendance generated by the Vietnam War draft and associated draft avoidance behaviors (MacInnis, 2006a; Buckles et al., 2016; Grimard and Parent, 2007; de Walque, 2007).

In contrast, we will leverage large changes in college attainment generated by the elimination of a large subsidy to higher education, the Student Benefit Program.[4] Under the Student Benefit Program, students age 18-21 who were the children of retired, disabled, or deceased Social Security beneficiaries were eligible to receive monthly payments if they enrolled as full-time college students. At the program's peak in the late-1970s, 12 percent of full-time college students were receiving benefits that averaged roughly $7,500 (2019 dollars). For context, this was roughly equivalent to the student-weighted average total tuition, fees, room, and board charged across all public four-year institutions at the time. It was three times the average tuition and fees charged to in-state students at universities at that time.[5] In 1981, Congress voted to eliminate the program. As demonstrated by Dynarski (2003), the elimination of benefits resulted in a large reduction in college attainment among those who were previously eligible. We will leverage the large decrease in college attainment resulting from the elimination, combined with a unique administrative dataset containing Social Security benefit records linked with administrative and survey data, to examine the impacts of changes in college attainment. As a result of this new and large administrative dataset, will be able to precisely identify those who were impacted by the elimination of the program and estimate the subsequent impacts of reduced college attainment on earnings and

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[4]At the time of its elimination, program expenditures were around $7 billion (2019 dollars), roughly equivalent to annual Pell grant expenditures at the time and roughly three-quarters average annual Pell expenditures throughout the 1980s and 1990s

[5]See Table 306 of the National Center for Education Statistics 1995 Digest of Education Statistics (https://nces.ed.gov/programs/digest/d95/dtab306.asp).

health across the life cycle.

Our unique and large panel of linked administrative and survey data provides several advantages in exploring the relationship between higher education and earnings. First, we have better and more comprehensive measures of income and earnings provided by both the administrative Social Security Administration (SSA) measures of wage income as well as those self-reported by individuals. This overcomes concerns related to out of state migration, lack of coverage, or attrition that are often present with Unemployment Insurance wage data or small panel surveys. Second, we will be able to estimate precise impacts on earnings across the life cycle, including the prime earnings years when earning trajectories are more settled. Our large sample size and ability to estimate effects during prime working ages will allow for more informative overall and subgroup estimates. Third, we can leverage the broader set of outcomes available in the survey data to explore how effects are generated and sustained across the life course. We can estimate effects on health insurance coverage, government benefit receipt, income stability, occupational choice, working hours, family structure, fertility, and a variety of other intermediate outcomes that can help us better understand the pathways through which higher education affects earnings across the life cycle.

Effects on earnings provide an incomplete picture of the returns to education. We bring new evidence to the extremely limited literature on the causal effects of higher education on health. First, our study overcomes concerns related to the endogeneity of college investments that limit prior correlational and twin studies. We provide some of the only evidence on this question that takes advantage of exogenous variation in college attainment. We are able to do so because the elimination of student benefits was a broad policy change that affected a large number of individuals while also being targeted directly at college enrollment. This approach has advantages over prior studies in (1) the extent to which we will be able to interpret the resulting estimates as causal (i.e., internal validity), and (2) the extent to which we believe the estimated effects are generalizable to other contexts (i.e., external

validity). Our policy variation provides compelling identifying variation in a recent and demographically varied cohort of individuals. Second, we take advantage of a unique panel of linked administrative and survey data that supports a deeper examination of our central questions. These data will allow us to explore effects of college attainment on a broad set of primary outcomes (including measures of earnings, health, and health behaviors) as well as explore how these effects are generated across the life course.[6] We combine our estimated effects on health and health behaviors with estimates of the dollar values of the associated costs to provide a more complete quantification of the effects associated with higher education. In addition to our primary outcomes, the data provide a number of secondary outcomes that will allow us to better understand the mechanisms through which any effects may be operating. For example, we can examine effects on food security, welfare use, income stability, family structure, fertility, assortative mating, etc. Finally, the large sample sizes and baseline location information contained in the data will allow us to explore how the effects of college attainment vary across subgroups.

Our pre-analysis plan begins with a very brief discussion of the existing evidence on the returns to college and how our study adds to this literature. We then provide an overview of the Social Security Administration's Student Benefit program and the prior findings of Dynarski (2003). We follow the suggestions of Olken (2015), which discusses the value and limitations of pre-analysis plans, in focusing our discussion on the small number of well-defined hypotheses our research is designed to test. We describe the data used in our analyses, documenting the sources and availability of the key variables across cohorts and ages. We spend a considerable amount of time discussing the construction of our small set of key outcome variables, including our methodology for quantifying the health-related costs that may be affected by higher education. In the following section, we document the correlations between our key outcome measures and educational attainment and how these

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[6]As a result of data limitations, the few earlier studies to address this broad question focus on a handful of outcomes at specific points in time. We can explore effects on both pecuniary and non-pecuniary outcomes across ages.

correlations vary over the life cycle.

We then describe our empirical strategy as well as our preferred specification and robustness checks. Our difference-in-differences empirical strategy assumes that individuals who did not receive SSA benefits at 17 provide a good counterfactual for what would have happened to those who did in the absence of the elimination of the Student Benefit program. We discuss tests for the validity of our identifying assumption as well as checks for a number of anticipated threats to internal validity. We also present calculations of our anticipated minimal detectable effects across key outcomes and incorporate discussion of the extent to which our analyses are powered to identify effects that are substantially smaller than the observed correlations.

Following the suggestions of Olken (2015), we conclude with an initial plan for understanding the observed effects. While it is too complex to map out the full set of tests we might conduct to explore mechanism, we include an initial plan for the estimation of effects within certain subgroups as well as a discussion of how we might use the data to better understand the mechanisms behind any observed effects of college attainment on the outcomes of interest. We leave a more complete exploration of mechanism to the final paper.

2   Brief Overview of the Wage and Health Returns to Higher Education

While we know a great deal about the returns – both pecuniary and non-pecuniary – to compulsory middle and high school education[7], our understanding of the returns to higher education (or financial aid) is more limited.

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[7]For example, a number of studies take advantage of changes to compulsory schooling laws in middle and high school to examine effects on health (Kemptner, Jürges and Reinhold, 2011; Mazumder, 2008; Lleras-Muney, 2005; Oreopoulos, 2007; Fletcher, 2015) civic participation (Milligan, Moretti and Oreopoulos, 2004), occupational prestige (Oreopoulos and Salvanes, 2011), religious views (Hungerman, 2014; Arias-Vasquez, 2012), and fertility (Tequame and Tirivavi, 2015; McCrary and Royer, 2011; Dincer, Kaushal and Grossman, 2014; James and Vujić, 2019)

2.1 Pecuniary Returns

There is an extensive literature estimating the relationship between wages (or log wages) and measures of education (see Oreopoulos and Petronijevic (2013) for an overview). However, much of this evidence relies on questionable identifying assumptions. Only a small subset of the literature makes an attempt to leverage plausibly exogenous variation in college going or attainment to estimate the causal effect on wages. Even among the set of pioneering papers that attempt to leverage exogenous shifters of college going, there are questions about the reliability of the identifying assumptions or the relevance of the estimates to more general or recent populations (Oreopoulos and Petronijevic, 2013). For example, a number of studies take advantage of geographic variation in college proximity to estimate the return to schooling (Card, 1995). However, there is a concern that proximity to a college is endogenous, even after controlling for certain individual characteristics (Card, 2001). Other estimates of the pecuniary return to higher education take advantage of differences in attainment generated by the Vietnam and World War II Era GI Bills (Bound and Turner, 2002; Stanley, 2003; Angrist and Chen, 2011). While interesting and important given the scale of these policies, the extent to which the estimates inform our general understanding of the returns to higher education is severely limited by the context of the policies. Furthermore, all of these studies are focused on individuals who attended college in the 1960s or before.

More recently there have been a number of papers to estimate the labor market effects of higher education (or financial aid programs) using compelling identification strategies and more recent cohorts of individuals. One of the most convincing studies compares high school seniors from Florida who barely qualified to attend one of the state's public colleges with seniors who barely missed the academic cutoff (Zimmerman, 2014). Using students from the late 1990s through early 2000s, Zimmerman finds a return to marginal students of 8.7 percent per year at a four-year college. Other recent and well-identified studies estimate returns to additional attainment induced by financial aid (Denning, 2019; Bettinger et al., 2019). Denning (2019) estimate positive effects of the Pell grant on attainment and short-

term earnings outcomes in Texas, whereas Bettinger et al. (2019) find evidence of modest increases in earnings resulting from Cal Grant eligibility, but the estimates are imprecise and vary across groups. Interestingly, aid has no effect on enrollment in either of these contexts. Both studies provide compelling strategies to estimate the short-term returns to additional financial aid.

We will take advantage of the elimination of a significantly more generous subsidy to study the effects of shifts into higher education in a broad population over the life cycle. This provides several advantages over previous studies. First, our unique and large panel of linked administrative and survey data provides better and more comprehensive measures of income and earnings. We do not have to worry about out of state migration or lack of coverage that is often a concern with Unemployment Insurance wage data. Second, we will be able to estimate precise impacts of college attainment on earnings across the life cycle, including the prime working years when earnings trajectories are more stable. Our larger sample size and ability to estimate effects during prime working ages will allow for more informative overall and subgroup estimates. Third, we can leverage the broader set of outcomes available in the survey data to explore how effects are generated and sustained across the life course. We can estimate effects on occupational choice, working hours, family structure, etc. that can help us to better understand the pathways through which higher education affects earnings across the life cycle.

Finally, earnings and income are incomplete measures of the return to additional education. Higher education may have effects on other important measures such as health. These effects may be driven by increases in earnings and income, but there may also be direct effects of higher education on health behaviors and preferences.

2.2   Health Returns

Our understanding of the non-pecuniary returns to higher education is even more limited. Most studies rely on the strong positive correlation between college attainment and

adult outcomes to suggest that going to or completing college results in better health.[8] To interpret these relationships as causal relies on strong assumptions about selection into schooling. For example, it is likely that children from wealthier families are more likely to attend and complete college. If wealth contributes to health, health behaviors, and longevity as an adult, the positive relationship between college attainment and these outcomes may not be driven by college attendance or completion itself.

A number of studies address this particular type of concern by comparing the health and health behaviors of siblings or twins who complete different amounts of education (Lundborg, Nordin and Rooth, 2018; Lundborg, 2013; Fujiwara and Kawachi, 2009; V.Amin, Behrman and Spector, 2013). This approach "controls" for differences across families, addressing the selection associated with differences in family wealth and upbringing. However, there are still selection concerns related to why one sibling completed more schooling than another. For example, perhaps the twin who was naturally healthier was able to go to college because of her better health. This type of reverse causality could easily explain the positive correlations between college attainment and health, even within families.[9] Putting aside whether sibling education differences are essentially randomly assigned, we might still worry that the effects of education on health outcomes might spillover between siblings, leading us to underestimate the true effect of education.[10] In addition to these fundamental concerns, the small sample sizes underlying these studies severely limit the extent to which researchers can draw strong conclusions about even the sign of the correlations.

A handful of innovative studies have attempted to overcome the limitations of the correlational and siblings studies by taking advantage of natural experiments.[11] For example,

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[8]See Oreopoulos and Salvanes (2011) for a review.

[9]Recent evidence suggests that even if these estimates are internally valid, a more nuanced type of selection into identification may result in misleading parameter estimates that are substantially biased relative to the population average treatment effect (Miller, Shenhav and Grosz, 2019).

[10]For example, one twin going to college might result in improved health behaviors and health for that individual, but it might also improve the health behaviors and health for the other twin via shared information or peer effects (e.g., eating healthier, going to the doctor more, etc.).

[11]We focus here on studies that estimate quasi-experimental effects on health and health-related outcomes and behaviors, but there is also a small literature that estimates quasi-experimental effects on other non-pecuniary outcomes such as civic participation (for example, Dee (2003)) or estimates effects on health

a number of studies use Vietnam War draft and GI Bill-induced increases in college attainment to examine impacts on mortality, health, and smoking (Buckles et al., 2016; MacInnis, 2006b; Grimard and Parent, 2007; de Walque, 2007). While these studies provide evidence of the effect of education for a large and policy-relevant sample, it is not clear the extent to which they would generalize to other populations, including women and/or those attending college under different and perhaps more typical circumstances. Furthermore, the difficulties associated with disentangling the effect of Vietnam War service, avoidance, and GI-bill induced college attainment limit confidence in the interpretation of the resulting effects as causal.

3   Social Security Administration's Student Benefits Program

We contribute to this extremely limited literature by exploiting changes in higher education attainment stemming from the elimination of a large federal student aid program in the United States. The Student Benefit program operated under the umbrella of the Social Security Administration (SSA). In general, SSA benefits were designed to partially offset the loss of income a family experiences when a worker retires, dies, or becomes disabled. This includes increased benefits to support dependent children who have an SSA eligible parent who is retired, disabled, or deceased. Through age 17, dependent children are eligible to receive 75% of the benefits of their parent, although that amount may be reduced to keep the total household benefit level below the household cap.[12] Beginning in 1965, the SSA recognized that children enrolled in school are typically dependent on their parents for support even after the age of 17. To support these students, the SSA expanded the definition of "child" to include individuals age 18-21 who were enrolled in school full time. These extended child benefits are referred to as the SSA's "Student Benefits Program."

As with the child benefits, individuals were only eligible for extended student benefits if they had a social security eligible parent who was retired, disabled, or deceased. Additionally,

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outcomes under structural models (for example, Heckman, Humphries and Veramendi (2018))

[12]Household benefit caps limit the total benefits that can be received by a beneficiary, their spouse and children to 150-180% of the recipient's base benefit eligibility.

they were required to be enrolled in school full-time, unmarried, and under the age of 22.[13] When a child beneficiary was nearing the age of 18 (and each year after) they would receive a form letter from the SSA verifying their continued student status. Additional verification of enrollment was provided annually by schools (Dynarski, 2003). These students would then receive a separate SSA check each month for as long as they stayed enrolled in the student benefits program. The size of the child benefits did not change after the age of 17 but continued to be calculated based on their parent's base eligibility and the total family benefits cap. At the program's peak in the mid-1970s, close to 17 percent of full-time college students were receiving benefits that averaged over $7,500 (2019 dollars) per year (SSA, 1982).[14]

In 1981, as a part of the Omnibus Budget Reconciliation and motivated by a desire to reduce government spending, congress voted to end the SSA's student benefits program. The new rules required that:

1. Benefits to secondary school students older than 18 ended in August 1982.[15]

2. Students who first enrolled in post-secondary education during or after May 1982 were not eligible for any student benefits.

3. Students who were enrolled in post-secondary education prior to May 1982, continued receiving reduced benefits during the phase-out period. During the phase-out:

   - Students did not receive cost of living adjustment (COLA) increases

   - Student benefits were not payable from May through August

   - Student benefits were reduced by 25% (of their August 1981 benefit amount) each year starting in September 1982

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[13]If an undergraduate, benefits ended at the end of the semester/quarter that an individual turned 22 (SSA, 1982).

[14]See Table 174 of https://nces.ed.gov/pubs99/1999036.pdf, which estimates that there were 4,601,000 18-21 year old full time students in 1975. Combined with number of student beneficiaries listed on the SSA website for 1975 (https://www.ssa.gov/history/studentbenefit.html), we estimate that 16.8% of full-time college students in 1975 were receiving benefits (DeWitt, n.d.).

[15]Notably, this preserved benefits for 18-year-old students who had not yet finished high school, which are still available today.

- No post-secondary student benefits were paid after April 1985

Effectively, these rules meant that, for children receiving SSA child benefits, those graduating high school before 1982 were eligible for the student benefits program while those graduating in 1982 or later were not. This change in available benefits, including the reduction in benefits for those graduating between 1979 and 1981, is shown in Figure 1 and Table 1. Eligible students went from having (on average) total cost of four-year public attendance taken care of to not having any assistance over the course of a few years.

4 Data

To answer our research questions, we will make use of a unique panel of linked administrative and survey data. The Annual Social Economic Supplement (ASEC) sample of the Current Population Survey (CPS) for the 1991, 1994, and 1996-2019 survey years serve as our base sample and contain roughly 185,000 individuals per year (Flood et al., 2020). The ASEC contains a number of questions on health, employment, and income, which makes it an ideal dataset to explore the channels through which any long-run effects on outcomes are operating.

The ASEC samples can also be publicly linked to other CPS supplements in adjacent months, which allows us to expand the set of outcomes we are able to examine.[16] Specifically, we use the Food Security Supplement, the Tobacco Use Supplement, the Fertility and Marriage Supplement, and the Un(der)banked Supplement.[17] These additional surveys allow us to examine impacts on health behaviors, housing, food security, welfare use, family structure, fertility, assortative mating, occupational choice, and other non-pecuniary outcomes with the potential to influence earnings and health over the life cycle.

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[16] The CPS follows a 4-8-4 system where households are surveyed for 4 consecutive months, go 8 months without a survey, and are then surveyed for another 4 months. This means that individuals surveyed in March may also have been surveyed in the months of December through June, but the fraction of the base sample surveyed in any month decreases as it becomes farther removed from March.

[17] See Appendix C for a comprehensive list of the variables we use from each supplement as well as the years and sample sizes for which they are available.

Critically, these data can be confidentially linked to individual-level Social Security benefit records, which include the type and amount of benefits received, allowing us to precisely identify individuals who were eligible for and receiving benefits due to the retirement, death, or disability of a parent.[18,19] We will use the receipt of these benefits at age 17 (which was unaffected by the elimination of the Student Benefit program) to assign treatment status. Individuals observed receiving any Social Security Administration benefits at age 17 will be in the treatment group, while all other individuals will be in the control group.

Our identifying variation takes advantage of whether an individual was expected to graduate high school before or after the elimination of the benefits program in 1982. We use exact date of birth information from the SSA records combined with researcher compiled information on age cutoff requirements for kindergarten in each state and year to code an individual's expected year of high school graduation.[20]

Individuals who were receiving social security benefits as a minor (i.e., those in our treatment group) were eligible to continue receiving benefits during college through age 21 as long as they enrolled in college before May 1982. From September 1982 to April 1985 existing beneficiaries were eligible for reduced benefits, as shown in Figure 1 and Table 1. We limit our sample to include 5 years of graduates with expected access to full benefits (1974-1978) and 5 years without any expected benefit access (1982-1986). As a result, our analysis sample will include anyone with an expected year of high school graduation between 1974 and 1986. We also exclude individuals with less than an 11th-grade education level since these individuals are unlikely to be affected by treatment.[21] The resulting sample includes

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[18]The SSA data also allow us to observe administrative records on mortality and long-run earnings streams.

[19]The CPS and SSA data are confidentially linked together using the Census Bureau's Person Identification Validation System (PVS) described in Wagner and Lane (2014). This process links both survey and administrative data to a master reference file. All individuals who are matched are then assigned a Protected Identification Key (PIK), which is an anonymized identification number that we use to link the CPS surveys with the administrative SSA dataset.

[20]The substantial variation in kindergarten start age cutoffs is shown in Appendix Figure A1.

[21]There are interesting questions as to how the promise of financial aid may affect earlier investments but we find it unlikely that this type of investment effect will meaningfully shift the sample of students that reach 11th grade. Section 6 outlines how we will test this assumption. If the results of that test suggest that individuals below grade 11 were affected, then we will expand our sample to include all individuals with a

roughly 70,000 individuals per cohort during this period. Individuals surveyed in the CPS samples listed above with an expected year of high-school graduation outside of this period will be excluded from all analyses.

The merged dataset will contain one observation for each year that an individual appears in the March CPS. That observation will also include any relevant CPS survey questions the individual responded to in the 3 months before or after that year's March survey. Everyone who completes their 4-8-4 rotation in the CPS will have two observations in the resulting dataset in consecutive years.[22,23]

Table 2 presents summary statistics from publicly available CPS data. Just over half the sample is female. Just over 80 percent of the sample is white, 12 percent is black, and 7 percent is of Hispanic Origin. These characteristics vary little between those with an expected year of high-school graduation before or after 1982. The average years of education is 13.76 and higher education is 1.78, with a small increase across cohorts that reflects the modest increase in educational attainment across this period. The average age of observation is 47 in the before cohorts and 40 in the after cohorts.[24] Given the available years of linked SSA-linked CPS data, we will observe most of the pivotal cohorts between 15 and 35 years after the period of expected high school graduation and potential benefit receipt. As shown in Figure 2, this allows us to observe individuals in both the treatment and control group from their late 20s into their 50s, providing a broad window for the observation of income and health.

4.1   Key Outcomes

To address multiple inference concerns, reduce measurement error, and simplify the presentation of information, we focus our inquiry on a set of key outcome variables. This includes:

9th grade education or higher.

[22]As discussed further below, outcomes will be clustered at the individual level to account for the correlation between observations of the same individual.

[23]Regressions using administrative outcomes may have more observations per individual.

[24]Once we access the restricted data, we will calculate similar averages for the treatment and control groups.

- Earnings Measure

    * Admin measures of earnings through age 50[25]

- Health Measures

    – Primary measures from self-reported scale:

        * Poor health (as measured by 4 or 5 on the scale)

        * Estimated health expenditures

    – Other measures:

        * Summary index of health (components below)

            · SSA disability receipt

            · Physical or cognitive difficulty

            · Self-reported health status (1-5)

            · Retired or quit due to health

            · Disability that limits or prevents work

        * Life expectancy[26]

        * Deceased by age 50

- Smoking measures

    * Current smoker

The administrative measure of earnings comes directly from the SSA data. The self-reported health measure comes from a question in the CPS ASEC. We will use the Likert measure, which asks individuals if their health is excellent, very good, good, fair, or poor, to construct a binary indicator for poor health (defined as "fair" or "poor" health) and to monetize effects on health by mapping the individual Likert scale answers to cost measures

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[25]We plan to use the administrative data conditional on access via our census partner. If we are unable to use the admin earnings data then we will use the survey measures of income as a primary outcome. As a robustness check, we will estimate effects on survey measures of earnings even if admin earnings are available.

[26]We will predict life expectancy using a non-parametric forecasting approach that matches individuals on observed demographic and health characteristics (components of the health index) at a particular age to earlier cohorts in which most individuals have already died to predict life expectancy. Because we currently lack access to the mortality data, we cannot present gradients or summary statistics at this point.

from the Medical Expenditures Panel Survey (MEPS).[27] We will also use other summary measures of health that combine information from a larger set of variables in a useful way, including a health index and a measure of life expectancy. And, we will estimate effects on mortality by age 50. Finally, we will estimate effects on whether an individual currently smokes as well as a monetized version of this variable intended to capture the costs of second hand smoke.

4.1.1 Outcome Indices

We construct several summary indices of our outcome measures, grouping components by theme (Kling, Liebman and Katz, 2007).[28] The index of health outcomes, which is a primary outcome of interest, combines measures of self-reported health status (on a scale of 1-5), having a disability that limits or prevents work, any SSA disability receipt, and having ever left a job for health reasons.

We generate z-scores by subtracting the mean and dividing by the standard deviation of each variable. Each index is the average across the standardized z-score measures of each component.[29] For each component of these indices, individual variables are adjusted so that a higher index score reflects a "better" outcome. Due to the rotating panel nature of the CPS and the varied timings of supplemental surveys, some individuals are missing some components of an index. For the construction of the index, missing values are imputed using the mean value of that variable for individuals of the same cohort (expected HS graduation year), treatment group, and gender.[30]

As shown in Table 2, individuals with an expected graduation date after 1982 have

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[27]We may also estimate effects on binary indicators for reporting each individual health response.

[28]As discussed further below, when considering effects on individual components of our indices, we will use the multiple comparisons adjustment approach outlined by Romano and Wolf.

[29]The indices themselves are also standardized using the same procedure.

[30]The current indices, as constructed in Appendix B, include some variables with a relatively high percentage of missing values. We may later exclude these variables. This would allow us to target the imputation groups more narrowly by including an individual's race and age at the time of the survey when calculating means.

slightly higher average values on the index of health.[31] The health differences likely reflect the somewhat younger ages of observation of those born later.

While the indices combine information in a useful way, they do not lend themselves to easy interpretation. We will also produce a measure of life expectancy using a forecasting method that non-parametrically matches observed cohorts of interest to earlier cohorts on demographic and health characteristics (components of the health index), and potentially other intermediate outcomes such as earnings. The selection of variables used to match will be based on a variable's predictive content and the extent to which it appears to satisfy exogeneity conditions required for a valid forecast.[32]

4.1.2 Monetizing and Quantifying Health Effects

To monetize and quantify the health effects of additional college attainment, we also generate variables estimating the direct effect on individual health expenditures as well as the spillover effect due to secondhand smoke. These measures may be used as a quantification of the effect on health to an individual or as a measure of the burden they place on the healthcare system. The role that private and public health insurance play in paying for medical expenses, as well as the obligation of physicians to provide lifesaving care even to patients who are unable to pay, means only a small portion of medical expenses are paid for directly by individuals receiving care. In 2015, only 11% of total health spending was paid for out-of-pocket (HHS, 2016).

While we are unable to directly observe health expenditures of our sample, we are able to estimate expected health expenditures based on individuals reported health level. From 1996 onward, respondents to the ASEC survey have been asked to rate their health as excellent, very good, good, fair, or poor. This type of self-reported health has been extensively used in

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[31]As with all outcomes, we will construct analogous tables for the treatment group once the data are available.

[32]While we focus on these particular summary measures, we may also produce additional measures such as quality adjusted life years (QALYs).

health surveys and there is a wide body of research evaluating its reliability.[33] The evidence suggests that perceived health levels primarily reflect underlying disease burden (Kaplan et al., 1996).

In fact, DeSalvo et al. (2009) find that a simple model of age and self-reported health predicts future health expenditures just as well as a set of more complex health expenditure prediction models using significantly more detailed information. In light of this, we apply the estimates from DeSalvo et al. (2009) for average yearly medical expenses by self-reported health status. [34] Inflation adjusted to 2015, annual averages range from $2,069 for individuals in excellent health to $15,946 for those in poor health. Given that only 11% of medical expenses were paid out of pocket, one might view the remaining 89% of the expenses (whether paid by private insurance, Medicare or Medicaid) to be a cost borne by society more generally.

Some choices also have spillover effects on the health of others.[35] Perhaps the best example of this is the negative health externalities caused by secondhand smoke. The Tobacco Use Supplement of the CPS survey, which is included only for a subset of our sample, asks respondents to report how frequently they smoke cigarettes, if ever. Leveraging existing estimates of the medical expenditures of non-smokers that are attributable to secondhand smoke exposure, we estimate that the average smoker generates $274 of negative health

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[33]See Idler and Benyamini (1997) for a review.

[34]We will update these estimates using the MEPS to be age (and perhaps race and gender) specific. We may also use a similar forecasting method to project health effects and associated health expenditures across the life-cycle. This method may rely on the estimation of dynamic health relationships using the dual measures of health reported by most individuals in the CPS or potentially a more direct mapping to health and expenditure profiles using individuals in the HRS who can be more completely observed over longer time frames.

[35]This may be true for self-reported health as well if sicker individuals are more likely to make others sick. To that extent, we are underestimating the external effects of poor health by focusing on externality that operates through shared insurance costs.

externalities per year.[36,37]

4.2 Secondary Outcomes

In addition to our primary outcomes, the data provide a number of secondary outcomes that will allow us to more comprehensively evaluate the effects of higher education and better understand the mechanisms through which any effects may be operating. For example, we can examine effects on health insurance coverage, food security, welfare use, income stability, family structure, fertility, assortative mating, and a variety of other outcomes that may influence earnings and health over the life cycle. While we describe our initial thoughts on exploration of mechanism in Section 6.5, we define several secondary outcome indices in Appendix B. These summary measures combine information from larger sets of similar variables and include an income index, benefit use index, and financial stability index. The components of each index are described explicitly in Appendix Table A1.

4.3 Education Gradients

As described in Section 1, prior work has illustrated a positive correlation between postsecondary educational attainment and a number of long run outcomes. However, determining whether these relationships are causal has proved challenging. Because we expect individuals of higher socioeconomic status, health, and ability to select into college, we also expect those same individuals to report higher levels of health, longer lifetimes, and improved employment outcomes relative to their peers even in the absence of any causal impacts of

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[36]A number of research papers estimate the yearly cost of secondhand smoke within a set geographical region. Including only medical conditions that are known to be caused by secondhand smoke exposure, they use estimates of the fraction of cases of each disease that are caused by secondhand smoke to calculate the fraction of total expenditures attributable to secondhand smoke exposure. Zollinger et al. (2004) estimate this value for Marion County, Indiana; Waters et al. (2009) estimate it for the state of Minnesota; and Behan, Eriksen and Lin (2005) estimate it for the United States as a whole. For each of these studies, we inflation adjust costs to 2015 dollars and divide the total yearly cost of secondhand smoke by the total number of smokers in the region, to get an estimate of the externality cost per smoker. Despite differences between regions and categories of included health costs, all three estimates are between \$243 and \$306 per year. For simplicity, we assume externality costs are equal to the median of the three (\$274).

[37]Because this value is based on the level of health expenditures and the number of smokers in that same year, it does not directly account for the increased risk of future illness, but includes instead the realized costs of current patients whose conditions are the result of past secondhand smoke exposure. While this method fails to account for changes in rates of smoking or associated illnesses over time, we believe it to be a reasonable estimate of the externalities of secondhand smoke exposure.

higher education. Despite this, correlational results and trends by education level are often misrepresented as, or implied to be, causal effects both within and outside of the academic literature. In light of this, we aim to not only provide accurate estimates of the causal effects of higher education, but also establish what proportion of observed education gradients can be attributed to education itself rather than endogenous selection. In this section, we will outline the relationship between each of our main outcomes and educational attainment, how those gradients were constructed, and how those gradients change across the life course.

It is important to note that our policy variation is variation in the availability of financial aid. While we think that the associated effects on college going and attainment are the most likely drivers of any downstream effects, it is possible that the aid itself has direct effects on student outcomes. For example, for many inframarginal enrollees the Student Benefit Program essentially provides an additional $7,500 in income per year. If this income has direct effects on student choices and behaviors outside of the educational investment decision, this might influence downstream outcomes. Some recent evidence suggests that the effects of this type of windfall income (and even much larger amounts) on health and health behaviors are essentially zero (Cesarini et al., 2016). If this is true, our "instrumental variables" estimates may be unbiased. To the extent that there are effects of additional resources on subsequent health, the almost universal expectation is that the effect will be positive. As a result, we plan to think about the estimated "instrumental variables" effects of additional education induced by the Student Benefit Program as an upper bound for true effects. Of course, we can also think more directly about the reduced form estimated effects of the program as the earnings and health returns to "free college" or generous financial aid.

For the purposes of our first stage estimation and creation of education gradients, we define educational attainment as the years of completed education up to completion of a bachelor's degree.[38] We top code years of education at 16 due to the linear relationship between years of education and our main outcomes of interest. Appendix Figures A2 through

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[38]Degree completion is mapped into years of education such that a high school diploma, associate's degree, or bachelor's degree represent 12, 14, or 16 years of education respectively.

A6 show a relatively linear trend between years of education and our main outcomes, which levels off for advanced degrees. Appendix Figure A7 shows that this pattern of linear effects leveling off at high education levels holds true for almost all outcomes.[39] Our choice to focus on years of college attainment is further motivated by the availability of student benefits, which were only available through age 21, suggesting that most of our identifying variation will occur on the college attainment margin.

We expect that, for most outcomes, selection effects and causal impacts are both playing a role in the observed gradients. Using years of education as defined above and the standardized outcome variables, our main health outcomes all have education gradients between 0.08 and 0.15 (as seen in Figure 5). Using poor health as an example, each additional year of education is associated with an individual being 0.08 standard deviations less likely to be in poor health. Our final paper will report what fraction of each gradient is potentially a causal effect, by comparing the upper bounds of estimated effect sizes to the analogous estimated gradients with years of education. Where possible, we will also provide comparisons with gradients in the literature.

Our large sample size across many survey years will also allow us to estimate how causal effects vary across the life course. Figure 3 plots the correlation between education and each of our key outcomes separately by age group.group The relationship between education and improvements in health rapidly increases with age indicating that causal returns of education on health may not be realized until later in life. On the other hand, the relationship between education and smoking behaviors weakens over time but remains relatively high across ages. The income index shown in Appendix Figure A8 illustrates a relatively weak relationship with education for young adults (less than 0.05 at age 27) but this relationship increases sharply with age with each year of education eventually corresponding to more than a 0.2 standard deviation increase in income for individuals in their 50's. Our final paper will also

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[39]Income and income related outcomes appear to be the exception, showing large gradients at higher education levels. However, it is worth noting that these spikes at higher education levels are driven by 3-6 year increases in educational attainment, implying that the gradients per year of education level off even for those outcomes. We will also present estimates allowing years of education to increase above 16.

map out how the causal effects of education vary across the life course and make comparisons with the observed education gradients.[40]

5   Empirical Strategy

We will implement a difference-in-differences design, comparing the outcomes of SSA benefit recipient children who likely graduated high school before and after the elimination of the Student Benefit Program. The treatment group is defined as all individuals who received any Social Security Administration benefits at age 17. All other individuals are in the control group. The "before" variable indicates all individuals who were expected, based on their exact date of birth and state school age cutoffs, to graduate high school before the end of the student benefits program in 1982. The analysis will not use any sample weights. Standard errors will be clustered at the individual level to account for correlation between multiple observations from the same individual. The basic reduced form specification is as follows:

$$Y_{it} = \lambda_t + \beta_1 * SSA_i + \beta_2(SSA_i * Before_t) + \gamma X_{it} + \epsilon_{it}, \tag{1}$$

where Y_{it} is an outcome measure for individual i in cohort t. Cohort fixed effects are represented by λ_t while X_{it} are individual covariates including state of birth, gender, age, race, and Hispanic origin. SSA_i indicates whether the individual received SSA benefits at age 17 and $Before_t$ indicates whether an individual's expected year of high school graduation was before the program ended in 1982. The parameter of interest is the coefficient on the interaction term $SSA_i * Before_t$, which equals one for individuals who received student SSA benefits at age 17 and whose cohort graduated high school before the program ended in 1982.[41]

All specifications will include birth state and birth cohort (defined by the expected year

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[40]We will also explore whether the gradients are similar among child SSA recipients.

[41]We will discuss robustness to various constructions of the treatment variable in Section 6.2, including defining treatment as a continuous variable or dropping partially treated cohorts.

of HS graduation) fixed effects. Our preferred specification (#5 below, shown in **bold**) will include binary controls for individual gender, age, age-by-gender, race, and Hispanic origin.[42] To illustrate robustness, we will estimate and show results using each of the following sets of controls:

1. No Additional Controls
2. Gender
3. Gender, Age (indicators)
4. Gender, Age (indicators), Race, Hispanic origin
5. **Gender, Age (indicators), Gender by Age (indicators), Race, Hispanic origin**
6. Gender, Age (indicators), Gender by Age(indicators), Race, Hispanic origin, Birth County Characteristics

Because we are interested in understanding the effect of college attainment on non-pecuniary outcomes and benchmarking our effects against the observed correlations, for each reduced form specification we will estimate an analogous "instrumental variables" specifications in which we instrument for years of educational attainment using access to the student benefits program.[43] The instrumental variables specification is as follows:

$$Y_{it} = \lambda_t + \beta_1 * SSA_i + \beta_2(\widehat{Educ_{it}}) + \gamma X_{it} + \epsilon_{it} \tag{2}$$

where $\widehat{Educ_{it}}$ indicates the level of post-secondary educational attainment and is generated

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[42]To maintain consistency across survey years, only two binary race variable were created; White and Black.

[43]Individuals who attended college but received no degree are assumed to have attended one year of college. Associates degree's and Bachelor's degrees are assumed to take 2 and 4 years of additional schooling respectively. Because student benefits were only available through age 21 and the gradient in our outcomes of interest flattens attainment past a Bachelor's degree, years of education is topcoded at 16 years (the level of a bachelor's degree). As mentioned above, we will interpret the resulting estimates as an upper bound for the true effects of higher education across the life cycle. Education gradients are shown in Appendix Figures A2 through A7

by the first stage regression:

$$Educ_{it} = \lambda_t + \gamma_1 * SSA_i + \gamma_2(SSA_i * Before_t) + \gamma X_{it} + u_{it} \tag{3}$$

As above, outcome variables for individual i in cohort t are represented by Y_{it}, while SSA_i and λ_t are treatment group and cohort fixed effects, respectively. $SSA_i * Before_t$ is an interaction term equal to one for individuals who received student SSA benefits at age 17 and whose cohort graduated high school before the program ended in 1982. As mentioned previously, while we think that the associated effects on college going and attainment are the most likely drivers of any downstream effects, it is possible that the aid itself has direct effects on student outcomes. For example, for many inframarginal enrollees the Student Benefit Program essentially provides an additional $7,500 in income per year. If this income has direct effects on student choices and behaviors outside of the educational investment decision, this might influence downstream outcomes. Some recent evidence suggests that the effects of this type of windfall income (and even much larger amounts) on health and health behaviors are essentially zero (Cesarini et al., 2016). If this is true, our "instrumental variables" estimates may be unbiased. To the extent that there are effects of additional resources on subsequent health, the almost universal expectation is that the effect will be positive. As a result, we plan to think about the estimated "instrumental variables" effects of additional education induced by the Student Benefit Program as an upper bound for true effects. Of course, we can also think more directly about the reduced form estimated effects of the program as the earnings and health returns to "free college" or generous financial aid.

5.1 Comparison to Dynarski (2003)

In prior work leveraging the end of the student benefits program Dynarski (2003) found that access to benefits increased educational attainment by 0.754 years. These results were estimated using five cohorts of graduating seniors in the National Longitudinal Survey of Youth (NLSY), focusing on students with deceased fathers. Our first stage estimate described

above will provide an equivalent measure of the programs impact on educational attainment and allow us to directly compare our results to those previously estimated. Our estimates may differ from Dynarski's findings for a variety of reasons, including the fact that we are using a different sample, more cohorts, children of retired, disabled, and deceased parents, or that our window for observing outcomes is farther in the future. Despite these differences between our studies, our ex-ante expectation is that we will estimate first stage effects similar to those previously estimated.

If our first stage estimates are substantially different than those reported in Dynarski (2003), we will also perform a direct replication exercise using the same NLSY sample and approach used there. If proven necessary, the goal of the replication exercise would be to verify the first stage estimates within the NLSY sample, explore the robustness of those results to alternative specification decisions, and reconcile any differences between our first stage estimates and those of Dynarski (2003). Given our overall interest in understanding the effects of education, we may focus on a particular subgroup if we estimate larger first-stage effects for that group.

6   Ancillary Analyses:

This section outlines the additional tests and analyses we will perform to more thoroughly understand the internal and external validity of our results as well as explore the theoretical and policy implications of our findings. This section proceeds as follows. First, we describe potential threats to internal validity and how we will test for evidence of these threats. Second, we outline how we plan to explore the robustness of our findings to alternative specification choices. Third, we discuss the size of effect that we are powered to detect and frame these minimal detectable effects (MDEs) relative to the estimated education gradients. This is followed by motivation for and a description of the heterogeneity analyses that we plan to conduct to explore how effects differ by subgroup. We conclude with a discussion of the potential mechanisms that could drive any observed results and some initial thoughts on how we will test for them.

6.1 Testing Identifying Assumption

The key identifying assumption is that, conditional on cohort and group fixed effects, the likelihood of going to or persisting in college or obtaining better later-life outcomes is orthogonal to one's membership in the group eligible for Student Benefits. In other words, conditional on cohort and group fixed effects, any difference in outcomes among the group eligible for Student Benefits is a result of the benefit availability and not some other factor.

In our context, the primary assumption is that the shift in attainment patterns of non-beneficiaries from before to after effectively proxies for the shift in attainments patterns that would have occurred among beneficiaries absent the elimination of student benefits, the standard difference-in-differences parallel trends assumption. While it is impossible to observe the counterfactual behavior of the treated group, we will conduct several tests to explore whether or not that assumption is reasonable in this setting.

First, we will estimate dynamic treatment effects, allowing the impact of being in the treatment to be different for each cohort. This method allows for treatment effects to change over time and provides a natural test of the parallel trends assumption. We will present the results of these tests in event study figures similar to Figure 4, which was generated using randomly assigned treatment groups. If the treatment and control groups are trending differently prior to the elimination of benefits, we will see a positive or negative slope in the figure. A flat trend of effect sizes prior to treatment indicates that there were no pre-existing differences in trends between the two groups and provides suggestive evidence that the identifying assumption holds.[44]

Given parallel trends prior to the elimination of benefits, our identifying assumption may still fail if the group of people in our treatment and control groups are not consistent across time. For example, one might be concerned that the end of the student benefits program could itself cause a shift in the unobservable characteristics of the treatment group, if

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[44]If parallel trends fails to hold in the full sample, we may refine the sample using matching or look within subsamples to find an appropriate sample in which to estimate effects.

some parents were claiming retirement or disability benefits specifically so that their children were eligible for student benefits. These types of parents would stop claiming retirement or disability after the Student Benefit program ended. In that scenario, those individuals would be assigned to the treatment group in the pre-period and the control group in the post-period, biasing our estimates. This type of strategic behavior would likely show up as a change in the fraction of students in the treatment group or the underlying characteristics of those individuals after the benefit was eliminated. We will plot the fraction of treated students by year of high school graduation to reveal any sharp changes in parent behavior. Additionally, to test whether or not the treatment and control groups have consistent observable characteristics across time, we will estimate our preferred reduced form specification replacing our main outcomes with observable characteristics (gender, race, Hispanic origin, and birth county characteristics) to test for compositional changes. A failure to identify any significant changes in demographic characteristics across the treatment threshold would imply that the treatment and control groups are consistent across time and support the validity of our identifying assumption.[45]

It is possible that behavioral responses could change the unobservable characteristics of the treated group without necessarily changing the demographic makeup of included individuals. In order to avoid this issue, Dynarski (2003) excludes individuals who are receiving benefits due to parental disability or retirement from her analysis, focusing instead on children with a deceased father, which is unlikely to be manipulated in response to the availability of the program. We are unable to distinguish children of deceased parents from

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[45]The identifying assumption may still be violated if spillover treatment affects impact the control group. If spillover effects push outcomes in the same direction as for treated individuals, this will attenuate our results towards zero. However, one might be concerned that increased competition for higher education would reduce enrollment of the control group in equilibrium, biasing our estimates upward. In the most extreme case, if there were a fixed number of available seats for college students, then any improvements in the educational attainment of the treated group would directly reduce the educational attainment of the control group by an equal amount. This would violate our identifying assumption that the control group is a good counterfactual for what would have occurred in the absence of treatment. Given the relatively small implied effects on total enrollment and the fact that the elimination of the Student Benefit program would lead to a decrease in enrollment, we think these types of capacity-driven general equilibrium effects are unlikely to occur in practice. Our paper will include a more rigorous discussion of how likely we are to observe general equilibrium effects in this setting.

children of disabled or retired parents in our data, but we can still address this concern in two ways. First, we will conduct a balance test to determine if the fraction of 17-year-olds receiving benefits was affected by the policy change. If the fraction receiving benefits was unaffected, then it is unlikely that there was any meaningful movement between the treatment and control groups as a result of, or concurrent with, the end of student benefits. Second, we can restrict the treatment group to only include minors who were receiving benefits by age 11. Because our last graduating cohort is 1986, and the law was changed in 1981, every individual in our sample was at least 12 years old when the law changed meaning that their treatment status at age 11 could not have been affected by the elimination of the student benefits program. This strategy will however attenuate our results toward zero because minors who began receiving benefits between ages 12-17 were affected by the loss of student benefits but would be included in the control group.

6.2   Additional Robustness Checks

In addition to testing the validity of our identifying assumptions, we will also provide additional evidence on the sensitivity of our results to minor specification changes and whether the pattern of effects (or lack thereof) matches theoretical predictions. One way that we will demonstrate robustness to specification changes is by estimating our results using the various sets of controls described in Section 5. In addition, we will present results using an alternative treatment definition that more fully captures the cohort level variation in student benefits that occurred at the end of the program. While our main specification treats all cohorts graduating before 1982 as treated, those graduating in high school from 1979 to 1981 were only eligible for a fraction of the benefits accessed by earlier cohorts as shown in Table 1 and Figure 1. These additional specifications will adjust the binary treatment variable to indicate the fraction of total benefits that each cohort was eligible for. Specifically, high school graduates in 1979, 1980, and 1981 were respectively eligible for 87.5%, 70.8%, and 50% of the full four-year benefits available to previous cohorts.[46] We will also explore

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[46]Because benefits were being reduced by 25% each year and only being paid 8 months a year, individuals received only 50% benefits (.75*8/12) for the 1982 academic year, 33% benefits (.5*8/12) for 1983 and 17%

specifications that exclude these partially treated cohorts.

Additionally, our main specifications exclude individuals with less than an 11th grade education. This decision was based on the fact that the end of the student benefits program did not change benefit amounts for any individuals under the age of 18, and the assumption that these individuals were therefore unlikely to be affected by treatment. To support this assumption, we will estimate the impact of the program on the likelihood of completing the 11th grade. We expect to find no effects on this margin.

6.3   Power

In this section we consider whether or not our approach will have sufficient statistical power to answer the research questions we have outlined. To do so, we present the results from a series of power calculations. Estimates of effect sizes and standard errors are approximated using the results of our own placebo analysis on publicly available CPS data supplemented with estimates of the first stage impact on years of educational attainment from Dynarski (2003).

For each index or main outcome, we conducted a placebo analysis using our preferred specification on actual CPS data. But, because we cannot identify Student Benefit recipients in the public data, we randomly assign individuals to placebo treatment groups.[47] We randomly assigned 8.39% of individuals to the treatment group and define graduating cohorts based on their age at the time of the survey.[48] While coefficients from this placebo test are not informative, we use the standard errors from these estimates (as a proxy for the actual standard errors) to estimate the expected minimal detectable effect size for each outcome.

The minimal detectable effect (MDE) describes how small of a true effect we are likely to be able to differentiate from zero at a particular level of statistical significance and power.

(.25*8/12) for 1984. No post-secondary students were eligible to receive benefits after April 1985. See Table 1 and Figure 1 for more details.

[47]Our preferred specification also includes a birth state fixed effect which is unavailable in the public data. Birth state is randomly generated for this analysis.

[48]Because public CPS data does not contain exact date of birth, graduation cohorts are approximately correct but less precisely defined than they will be in the final analysis. 8.39% is an estimate of the fraction eligible for benefits based on the percentage of individuals with a dead farther in the NLSY79 and aggregate statistics on the percentage of beneficiaries with deceased parents.

The confidence interval of an estimate (95% confidence level) will exclude zero only if the estimated effect is 1.96 se or more away from zero. However, if the true effect is 1.96 se, and we expect estimates of that effect to have a normal distribution, then we would only be able to reject the null hypothesis 50% of the time. In order for estimates to be distinguishable from zero at the 95% confidence level 80% of the time, the true effect size would need to be at least 2.8 se from zero. We estimate MDE sizes for each of our main outcomes by multiplying the standard error estimate from our placebo reduced form by 2.8. In order to make these estimates comparable with the education gradients, we inflate the MDE by the first stage estimates on years of completed education from Dynarski (2003). These MDEs and the magnitude of the associated education gradients for key outcomes are displayed in Figure 5.[49] Assuming the first stage from Dynarski (2003), we are well-powered to detect effects that are less than half of the observed gradient for each key outcome we can currently observe.[50]

For any true effect larger than the MDE we can expect to reject the null hypothesis of a zero effect at least 80% of the time. However, in addition to simply distinguishing whether there is a non-zero effect, we are also interested in determining the degree to which each outcome's correlation with post-secondary attainment is driven by a causal relationship. As discussed in Section 4.3, we will compare estimated effect sizes to the observed gradients and determine whether or not we can reject the null hypothesis that the observed education gradient is equivalent to the estimated causal effect. Following the same logic described above, we can expect to reject the education gradient at the 95% confidence level with 80% power if the true effect is more than 2.8 se below the education gradient. For any outcome that has a MDE more than 2.8 se below its gradient we are "doubly powered" in the sense that at any true effect size we can expect to reject either the null hypothesis of a zero effect

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[49]In alternative specifications, we also directly estimated the standard error of the 2SLS IV estimates based on the standard error of a placebo first stage and the size of the first stage in Dynarski (2003). Minimal detectable effect sizes calculated as 2.8 times those 2SLS standard errors are nearly identical to those presented here.

[50]We are also well powered to identify effects on secondary indices as shown in Appendix Figure A9.

or the null hypothesis that the gradient represents the causal effect.

6.4 Heterogeneity Analysis

While our main results estimate the average impact of additional educational attainment, we are also interested in exploring how much this effect size differs for various population subgroups. Because eligibility for student benefits was determined by parental death, disability, or retirement rather than household income or other demographic characteristics, the treated group includes individuals from a broad range of backgrounds.[51] While it is possible that the marginal benefit of additional education is consistent across subgroups, we find it likely that there is a substantial amount of heterogeneity. Recent work that summarizes this literature suggests that this type of evidence is lacking (Galama, Lleras-Muney and Kippersluis, 2018).

We will explore variation in both the first stage impact of benefit eligibility on educational attainment and the long run impacts of that educational attainment by gender, age, race/ethnicity (White, Black, Hispanic) and various birth county characteristics.[52] We will begin by conducting analysis to better understand the composition of compliers. Then we will explore how effects vary over the life cycle. Much of the existing work has focused on males, but patterns of health and the role of education differ significantly between males and females over the life cycle (Galama, Lleras-Muney and Kippersluis, 2018). Some existing estimates of the relationship between higher education and downstream outcomes suggest significantly different correlations across race, particularly for health. Understanding the impacts of the benefit eligibility will help us understand which subpopulations, if any, drive our main results, as well as be informative for policymakers, universities, and charitable organizations seeking to make the largest possible impact per dollar of student financial aid. Given our interest in understanding the downstream effects of post-secondary education, we may focus our analyses on subgroups whose educational attainment is more strongly affected

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[51]We will show basic demographic characteristics of those affected once we access the administrative data.

[52]For example, we may explore heterogeneity based on county poverty levels, baseline college-going rates, and measures of college access.

by the elimination of benefits.

The value of the student benefits was not a fixed amount for all eligible students. Student benefits were calculated based on the base social security eligibility of the parent (which is dependent on past employment income) and were also limited by a cap on total family benefit receipt. This means that students whose parents had higher past earnings or who had few eligible siblings received more benefits each month. We expect that, due to the changing demographics across benefit levels and diminishing marginal returns per dollar of benefits, the impact per dollar of benefit eligibility will be higher at lower benefit levels. To explore this, we will stratify the treated group by the decile of age 17 benefits level (which reflects differences in family income), separately estimating long run outcomes and measures of our key outcomes for each group, scaling by benefit level.

6.5   Exploring Mechanisms

The theoretical relationship between college and pecuniary outcomes is relatively straight-forward and often focuses on the standard human capital investment model, with investments in human capital generating increases in subsequent productivity. The key alternative model instead suggests that investments in human capital cause higher earnings only by allowing individuals to "signal" their higher level of productivity to potential employers. Distinguishing between these models has proven difficult in practice as both result in a causal return to additional education. However, under the latter model we might expect to see shorter-term increases in earnings that are not sustained over the life course as an individual's true productivity is revealed to employers.[53] More generally we can use our rich data to explore how additional attainment at the college level influences the patterns and timing of other choices and outcomes (such as family formation, fertility, labor force participation, occupational choice, assortative mating, etc.) that might influence earnings over the life cycle.

Effects on earnings and income may contribute to effects on health as well, although the theoretical relationship between higher education and health is less clear, with several

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[53]We might also expect fewer health advantages of college attendance if college merely acts as a signal and the relationship between income and health is weak.

channels through which causal relationships could plausibly exist (Grossman, 2006). The productive efficiency model suggests that most effects on health operate as a direct result of additional income, positing that individuals become more productive with their time as a result of their college experience and thus are more likely to have the time and resources to result in improved health. In contrast, there may be non-income channels that drive the education-health relationship. The allocative efficiency model suggests that college imparts improved skills and consequently leads to better decision-making abilities. For example, the negative relationship between schooling and smoking could be explained by this model if more educated individuals smoke less as a result of their greater awareness of the ill effects of smoking or the most successful strategies for quitting. A related possibility is that college makes individuals more patient and forward-thinking by making them think more about their future and the complexities of adult life. As a result, college graduates would be less likely to participate in risky behavior associated with present mindedness. Indeed, Oreopoulos and Salvanes (2011) describe a negative correlation between education and teen pregnancy, crime, and smoking.

Distinguishing between these channels is difficult, as they have many overlapping predictions for increased income and improved health as a result of education. Additionally, it is likely that several mechanisms are occurring simultaneously but to varying degrees. That said, we can combine somewhat different theoretical predictions of the models with empirical analyses in an attempt to disentangle the channels through which any observed effect is operating. We can use our linked survey and administrative data to support such an exploration across a range of outcomes. First and foremost, we will already be estimating effects on earnings across the life cycle. Improvements in income are a necessary condition for the productive efficiency model although, as Cesarini et al. (2016) demonstrate, they may not be a sufficient condition. We can also explore the extent to which increases in income explain any observed improvements in health outcomes. For example, if the magnitude of health improvements relative to increases in income are equivalent to the overall correlation

between income and those health outcomes (or the causal relationship if the literature contains comparable estimates) it would suggest that any observed health improvements are being driven by income gains, supporting the productive efficiency model.[54] Third, the productive efficiency model suggests a delayed timing in effects that is not associated with the allocative efficiency or forward-thinking models. While the latter models imply potentially immediate changes in behavior (for example, healthier choices while in college), the productive efficiency model suggests that these effects might show up later, once an individual can afford to buy different things or make different choices.

Estimated effects on health behaviors, family formation, fertility, and occupation choice may provide some support for the role of improved decision-making abilities or individuals becoming more patient and forward thinking. For example, if improved outcomes are being driven by overall improvements in decision making or increased patience, we expect to see reduced smoking and increased uptake of health insurance. Shifts towards occupations with more delayed benefits may also support these channels for improved health.

More generally, we can directly observe the effect of treatment on decisions regarding a large number of life choices (such as number and timing of children, marriage and divorce, retirement age, savings behavior, etc.). Using the observed correlations (or existing causal estimates) in the data, we can attempt to disentangle the role of different intermediate behaviors and outcomes on long-term health and earnings outcomes. While it is difficult to outline all possible potential mechanisms for all the potential effects that we might observe, in addition to the channels described above, this exploration will include, but is not limited to, determining the extent to which various effects can be explained by changes in family formation and spousal characteristics, fertility, health insurance coverage and geographic mobility.

Finally, we may leverage richer but smaller data from the National Longitudinal Survey

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[54]A related way to test this would be to directly incorporate income into the regressions estimating long-run effects on health outcomes, essentially a mediation analysis. If the effect of education on a health outcome disappears or attenuates substantially once income is controlled for, it would suggest an important role of the productive efficiency model.

of Youth 1979 (NLSY79) to investigate specific mechanisms in greater depth.

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7 Figures and Tables

Table 1: Benefit Eligibility

	Freshman (1)	Sophomore (2)	Junior (3)	Senior (4)	Total (5)
High School Graduation Year					
1977	100%	100%	100%	100%	100%
1978	100%	100%	100%	100%	100%
1979	100%	100%	100%	50%	88%
1980	100%	100%	50%	33%	71%
1981	100%	50%	33%	17%	50%
1982	0%	0%	0%	0%	0%
1983	0%	0%	0%	0%	0%

Note: Individuals who were receiving student benefits before the elimination of the program continued to receive reduced benefits after 1981. These students had their benefits reduced by 25% each year, cost of living adjustments were halted, and they were not eligible for any benefits during summer months (May-August). Because students were being reduced by 25% each year and only being paid 8 months a year, continuing students received 50% benefits (.75*8/12) for the 1982 academic year, 33% benefits (.5*8/12) for 1983 and 17% (.25*8/12) for 1984. No post-secondary students were eligible to receive benefits after April 1985. The cumulative effect of these reductions by cohort are displayed in column (5) above. These calculations do not account for the halt in cost-of-living adjustment after August 1981, which further reduced the benefits in real terms for cohorts graduating high school from 1979-1981.

Table 2: Summary Statistics

|	(1) Full Sample	(2) Before	(3) After
Demographics			
Female	0.51	0.51	0.51
Age	44.48	47.44	40.30
Black	0.12	0.12	0.13
White	0.81	0.82	0.80
Hispanic Origin	0.07	0.06	0.09
Years of Higher Education	1.78	1.75	1.83
Years of Education	13.76	13.73	13.80
Observations	978,285	561,309	416,976
Key Outcomes			
Earnings Measures through Age 50 (SSA Data)	.	.	.
Died by Age 50 (SSA Data)	.	.	.
Health Status is Poor	0.11	0.13	0.09
Estimated Medical Costs	4612.22	4809.66	4334.88
Observations	907,483	518,669	388,814
Current Smoker	0.22	0.23	0.22
Estimated Yearly Secondhand Smoke Costs	61.48	63.07	59.06
Observations	169,597	100,731	68,866
Health Index and Components			
Health Index*	-0.03	-0.11	0.09
Ever Recieved SSA Disability (SSA Data)	.	.	.
Any physical or cognitive difficulty	0.10	0.12	0.08
Health Status (1-5 Scale)	2.23	2.29	2.14
Ever retired or left job for health reasons	0.04	0.05	0.03
Has disability that limits or prevents work	0.08	0.10	0.07

Note: Table presents descriptive statistics for demographic and main outcome variables. Statistics are shown separately for the cohorts expected to graduate high school before 1982 (when the SSA Student Benefit program was still in existence) vs those graduating later. Differences by treatment group will also be presented in the final paper. The functioning, disability , and health index ("health index") is normalized but the full sample average is slightly different from zero because all summary statistics were calculated using ASEC sample weights. All components of the health index were "flipped" to make higher values "better" before constructing the index. Components with missing values are imputed before index construction as described in 4.1.1. Section 4.1.2 outlines how estimated medical costs and secondhand smoke externalities were calculated. Outcome variables only available in the administrative SSA data are left as missing in this table and not included in construction of indices. Statistics presented here are calculated from publicly available data (Flood et al., 2020).
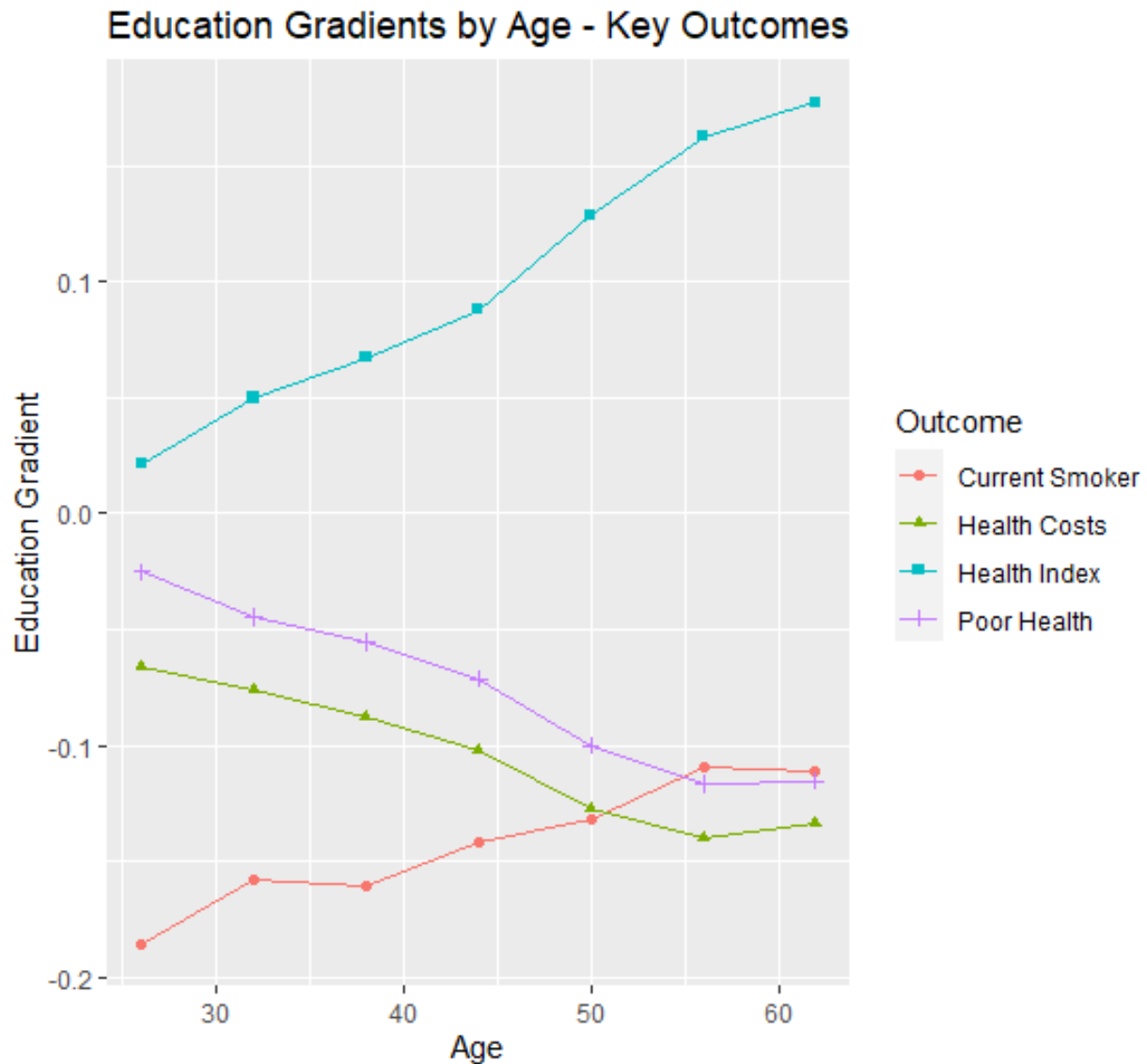
Figure 1
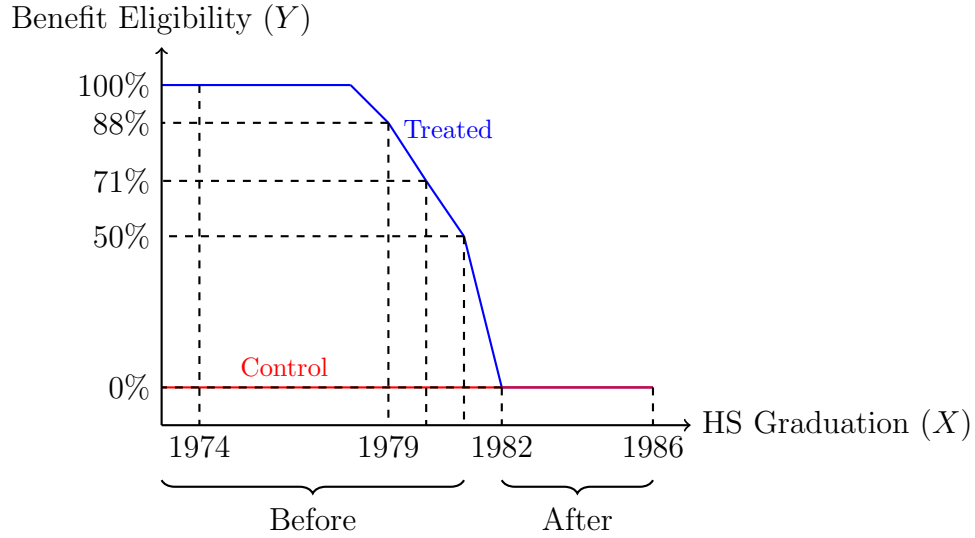
Note: This figure displays the total 4-year SSA student benefits eligibility by high school graduation cohort from Table 1 Column 5. While the control group was never eligible for benefits, access for the treated group dropped off sharply for cohorts graduating high school between 1979 and 1982. These calculations do not account for the halt in cost-of-living adjustment after August 1981, which further reduced the benefits in real terms for cohorts graduating high school from 1979-1981.

Figure 2

Age Distribution
by Expected High School Graduation
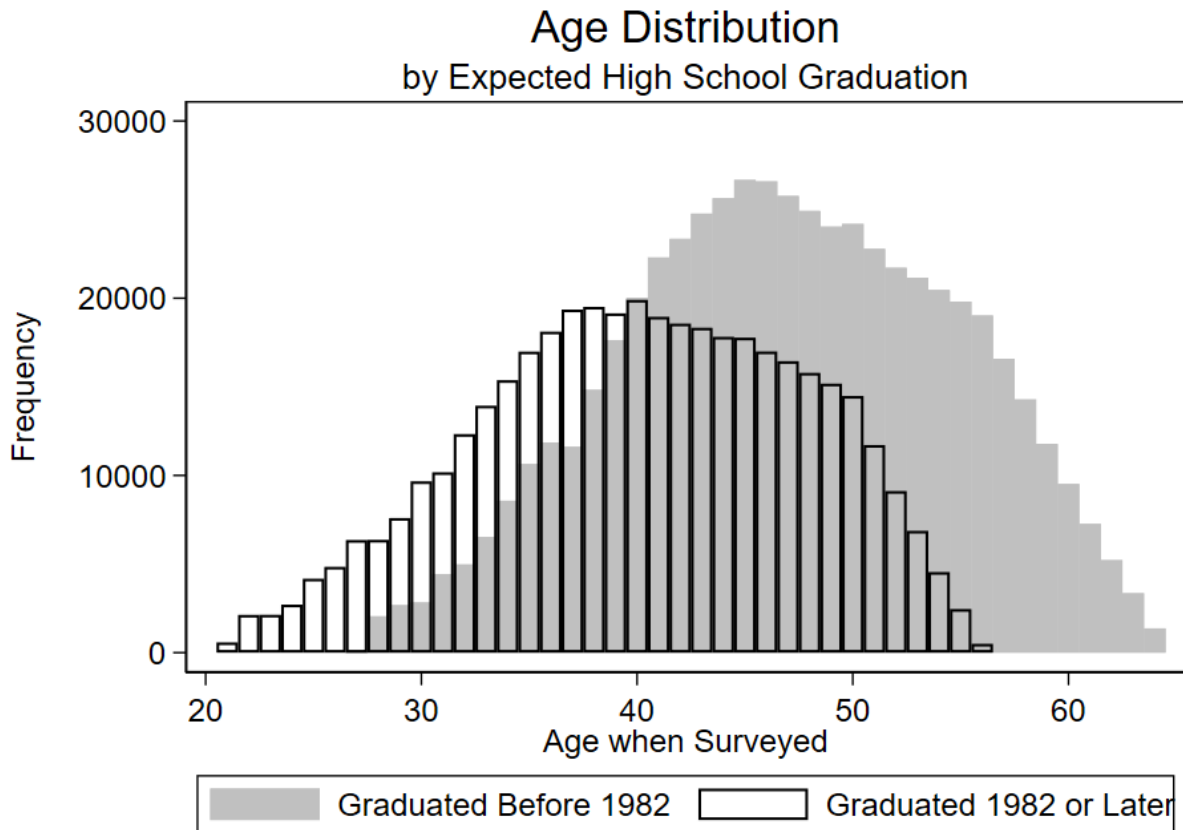
Note: This figure shows the distribution of individuals' age at the time of survey for those who graduated before 1982 (when the SSA Student Benefit program was still in existence) versus those who graduated in 1982 and later. Statistics presented here are calculated from publicly available data.
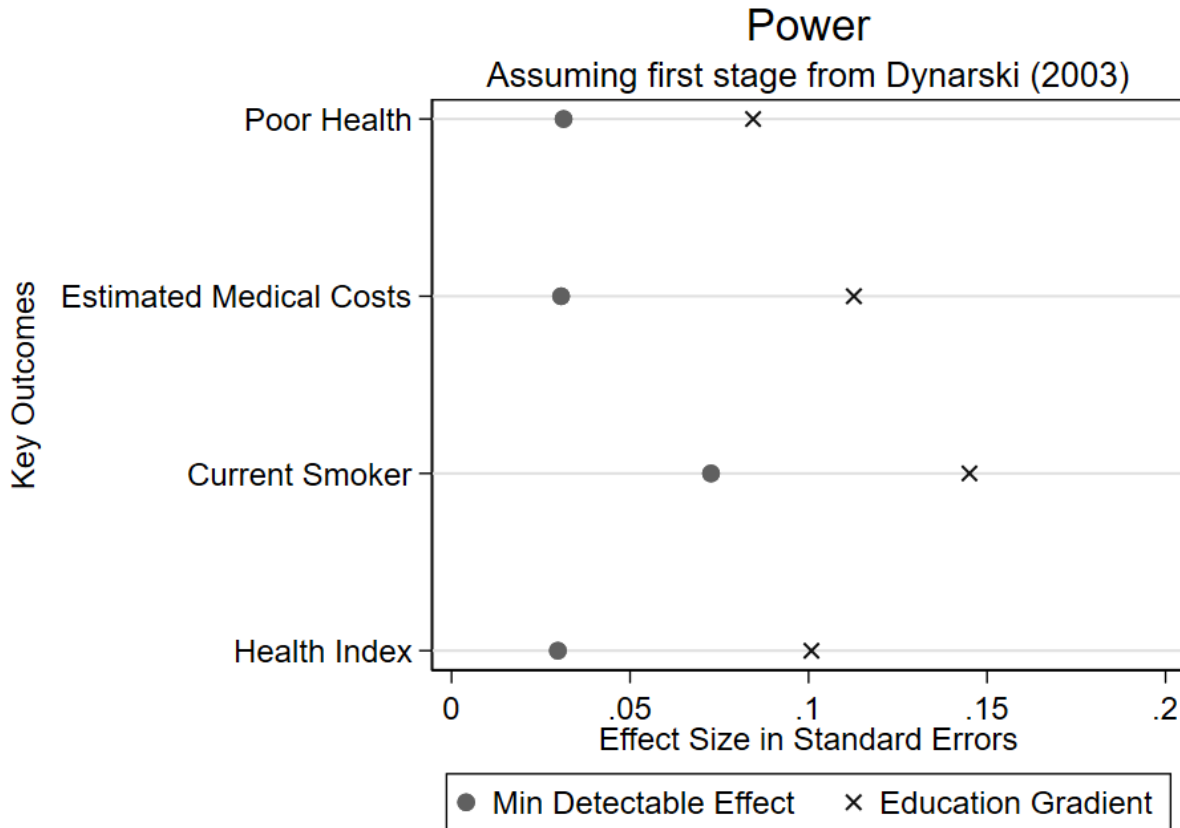
Figure 3

Note: This figure plots the relationship between our primary outcome measures and years of education across ages. Specifically, each dot represents the coefficient from a regression of a primary outcome measure on years of education in six-year bins (i.e., age 23-28, 29-34, 35-40, 41-46, 47-52, 53-58, 59-64). Years of education is defined as years of education through grade 16 (i.e. a bachelor's degree). Other variables are constructed as described in the text. Statistics presented here are calculated from publicly available data.

Figure 4

End of Student Benefits Program

Impact On Health Index - Placebo Treatment Assignment
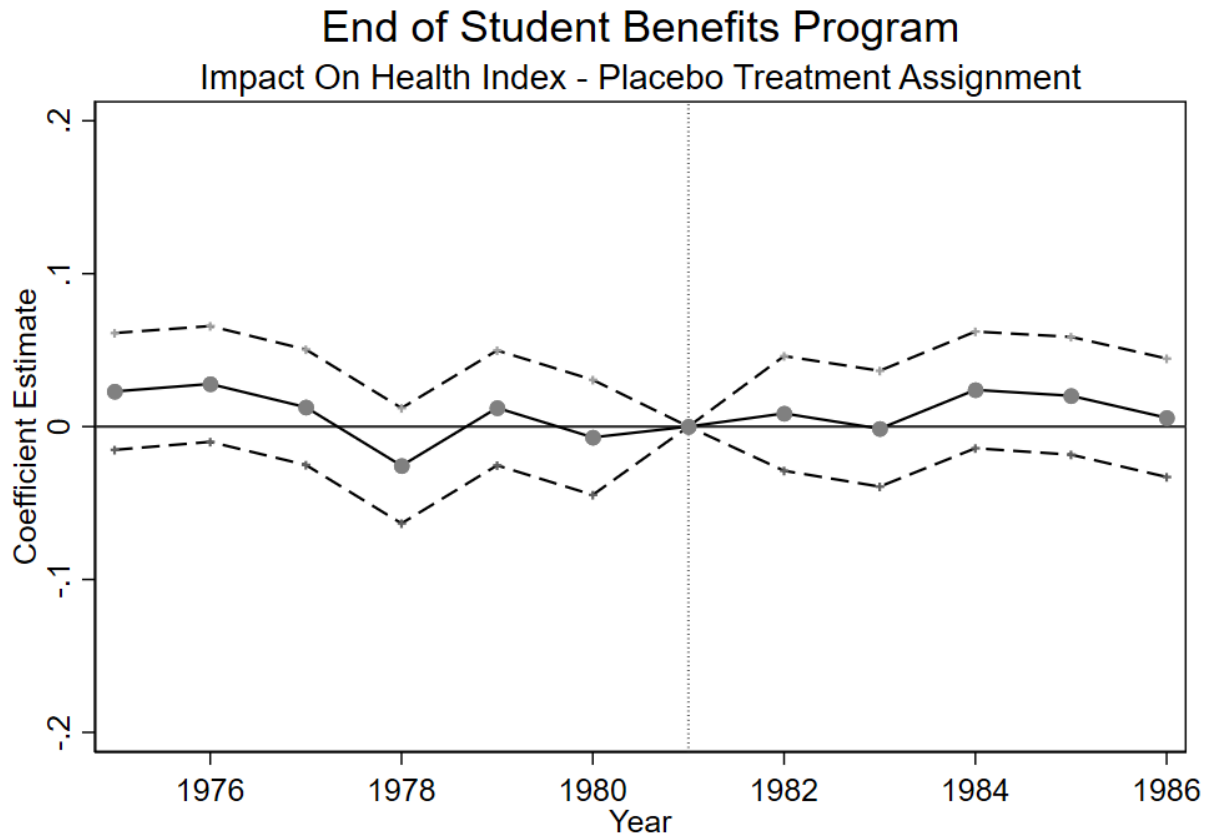
Note: This is an example event study estimated with randomly assigned treatment and control groups (matching the approximate true proportions of treatment and control individuals). Statistics presented here are calculated from publicly available data.
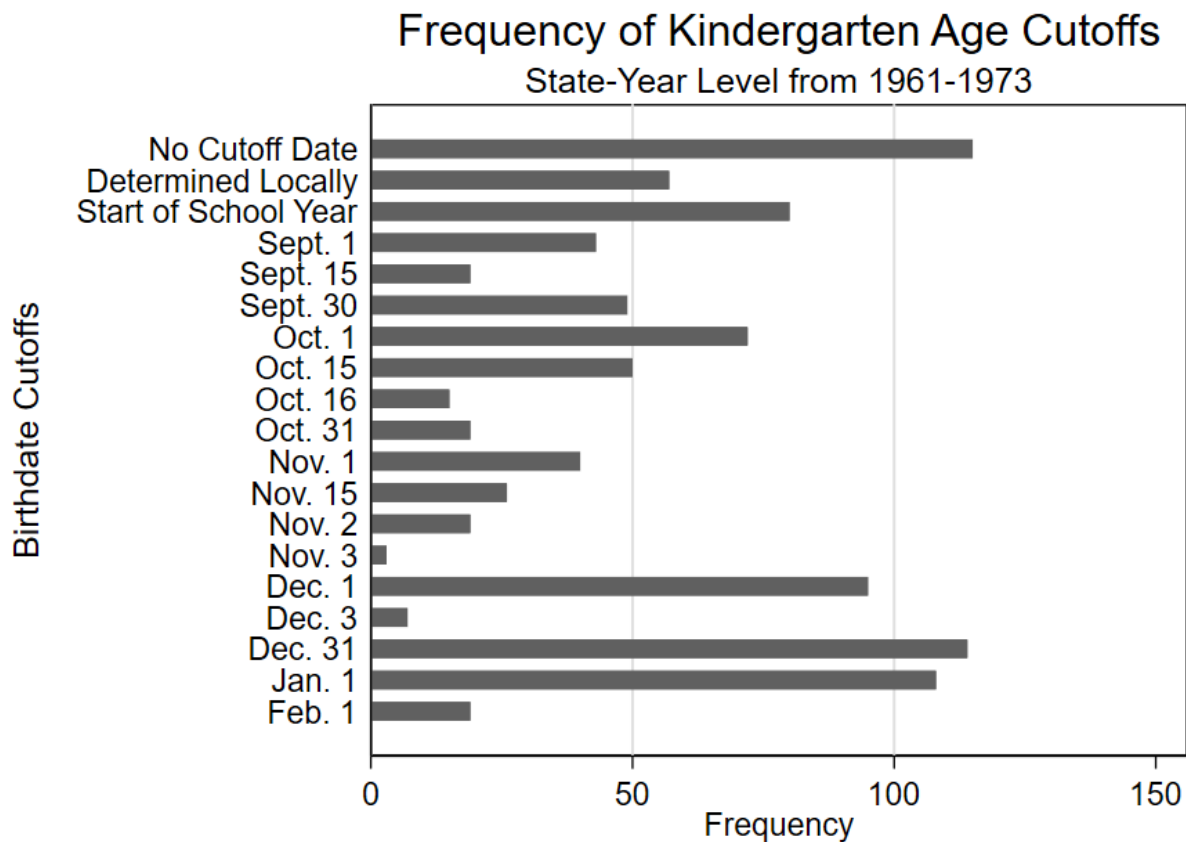
Figure 5

Power
Assuming first stage from Dynarski (2003)

Note: This figure displays the magnitude of both the education gradient and minimal detectable effect sizes for primary outcomes that are available in the public data. Education gradients are the result of regressing each primary outcome measure on years of education, where years of education is defined as years of education through grade 16 (i.e., a bachelor's degree). Minimal detectable effect size is calculated at the 95% confidence level and 80% power based on the standard errors from placebo analysis combined with first stage estimates from Dynarski (2003). This process is described in more detail in Section 6.3. Other variables are constructed as described in the text. Statistics presented here are calculated from publicly available data.

# A   Appendix Tables and Figures

Figure A1

Note: This figure displays the distribution of school start cutoff dates for children entering kindergarten. Observations are at the state-year level. These kindergarten age cutoffs are used in conjunction with exact date of birth to define each CPS survey respondent's expected year of high school graduation. We will approximate the age cutoff date using common school start date(s) for state-years with a "start of school year" cutoff system. For state-years where the cutoff was determined locally, missing, or where there were no age cutoff dates, we will approximate age cutoffs using the most common nationwide cutoffs. These data were compiled by the researchers.

Table A1: Summary Statistics of Secondary Index Components

|	(1) Full Sample	(2) Before	(3) After
Health Behaviors Index	-0.02	-0.09	0.08
Has Health Insurance	0.86	0.87	0.85
Has Smoked 100 Cigarettes*	0.40	0.42	0.35
Frequency of Cigarette Smoking (1-3 Scale)*	1.41	1.42	1.39
Benefit Use Index*	-0.01	0.01	-0.03
In Public or Subsidized Housing	0.09	0.10	0.09
Receiving SNAP/Food Stamps	0.06	0.06	0.07
Received SNAP/Food Stamps Last Year	0.17	0.17	0.18
Recieving WIC	0.01	0.01	0.02
Receiving Free-Reduced Price Lunches	0.27	0.25	0.30
Welfare Income	56.03	49.24	65.60
Any Disability Income	0.01	0.01	0.01
Any SSA Income	0.04	0.05	0.03
Any Welfare Income	0.01	0.01	0.01
Any Unemployment Income	0.04	0.04	0.04
Any Workers Compensation	0.01	0.01	0.01
Covered by Medicaid	0.07	0.07	0.07
Income Index	-0.01	0.01	-0.04
Sum of Earnings through Age 50 (SSA Data)	.	.	.
Household Income	97,597	98,712	96,024
Family Income	92,412	94,225	89,853
Personal Income	49,692	50,882	48,012
Below Poverty Line*	0.09	0.08	0.09
Financial Stability Index	-0.01	0.12	-0.21
Estimated Income Variance (SSA Data)	.	.	.
Owns Home	0.74	0.77	0.70
Has Checking/Savings Account	0.96	0.97	0.95
Food Security Status (1-3 Scale)*	1.14	1.13	1.15
Employment Index	-0.03	-0.06	0.01
In Labor Force	0.82	0.81	0.84
Worked Last Year	0.83	0.82	0.85
Employed	0.78	0.77	0.79
Observations	978,285	561,309	416,976

Note: Table presents summary statistics for each secondary outcome index and each of its components. Statistics are shown separately for the cohorts expected to graduate high school before 1982 (when the SSA Student Benefit program was still in existence) vs those graduating later. Differences by treatment group will also be presented in final paper. Each index is normalized but full sample averages may be slightly different from zero because all summary statistics were calculated using ASEC sample weights. Components with missing values are imputed before index construction as described in 4.1.1. Variables only available in the administrative SSA data are left as missing and excluded from current indices construction. An asterisk (*) indicates that a variable is "flipped" to make higher values "better" before constructing the associated index. Sample size reported here represents the total number of observations. Statistics presented here are calculated from publicly available data (Flood et al., 2020).
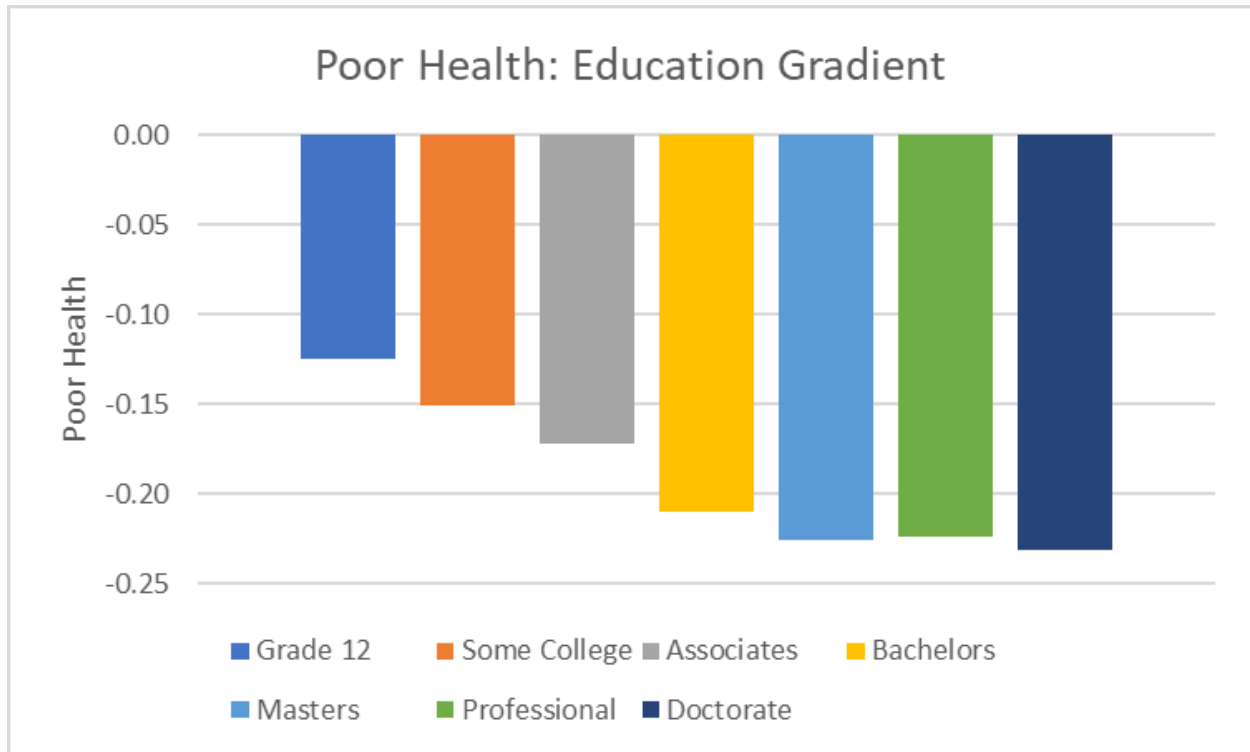
Figure A2
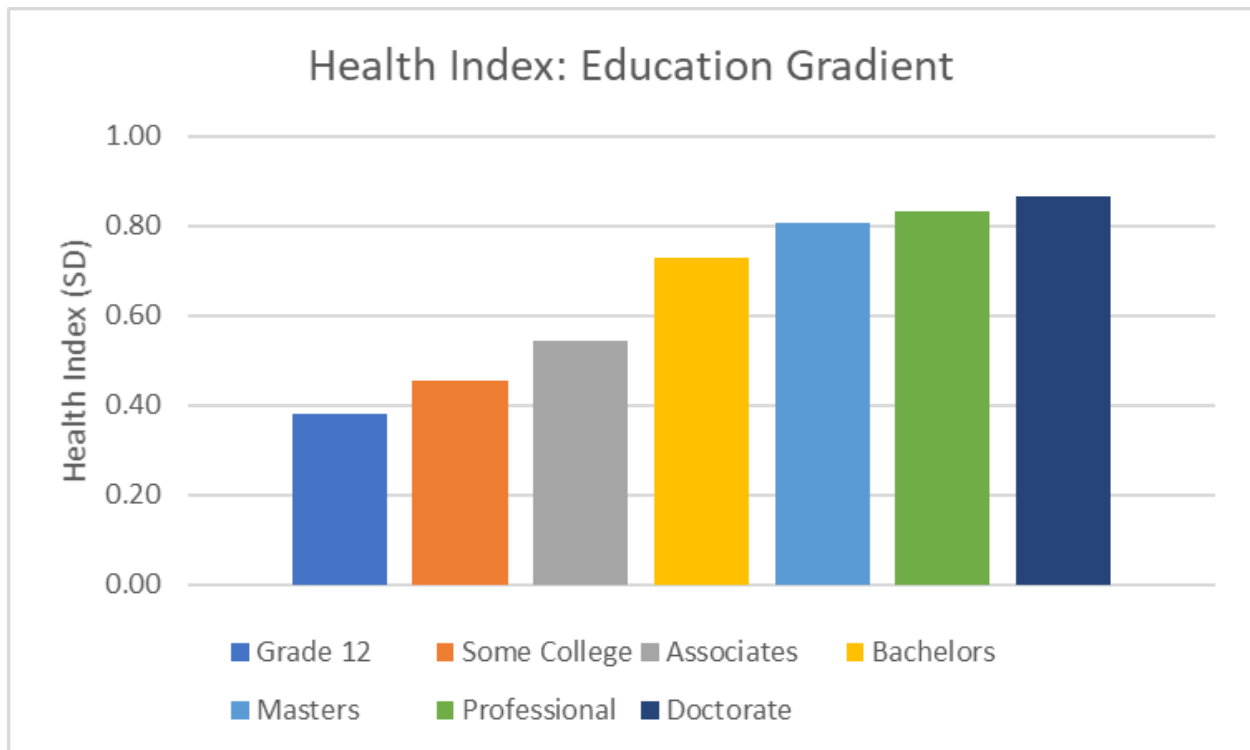
Note: This figure shows the gradient between educational attainment and the health index, a key outcome. The health index was regressed on education indicator variables, one for each bar on the X-axis. Each bar's height shows the estimated relationship between the associated level of educational attainment and the health index, measured in standard deviations. The sample was restricted to individuals with at least an 11th grade education. All estimates are relative to the omitted group, individuals whose highest level of education was 11th grade. Statistics presented here are calculated from publicly available data.
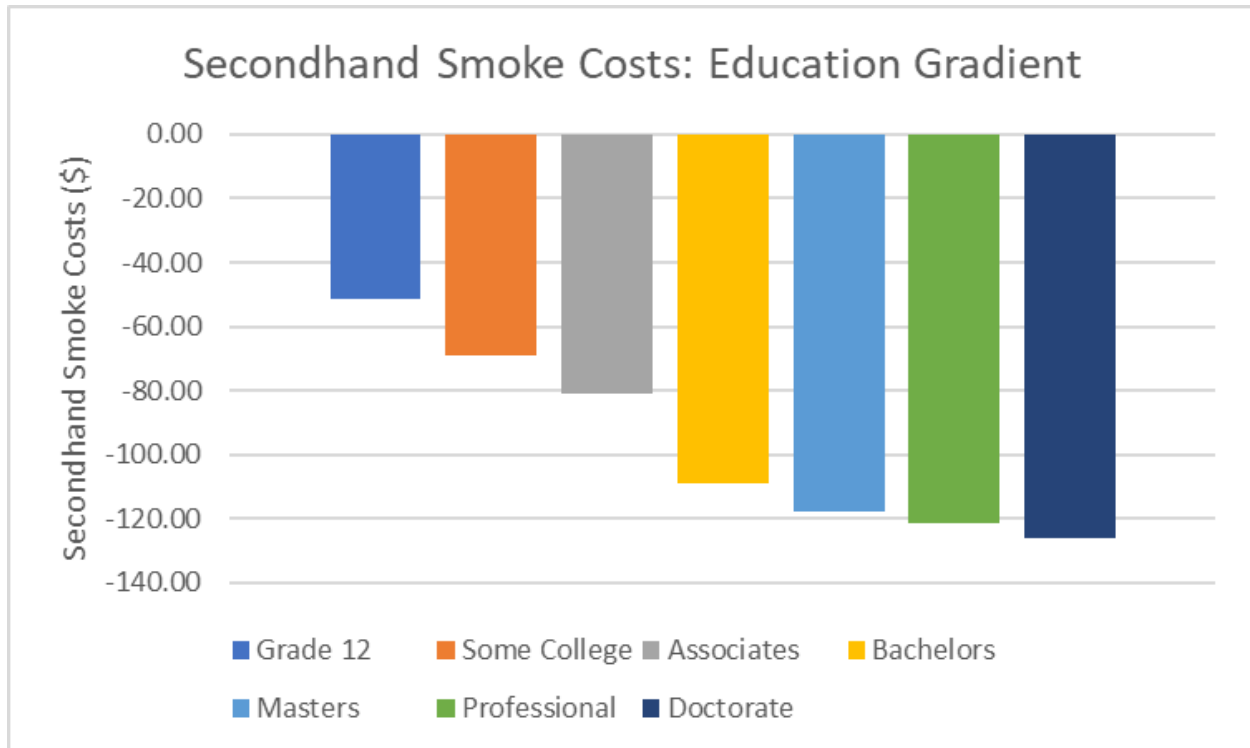
Health Costs: Education Gradient
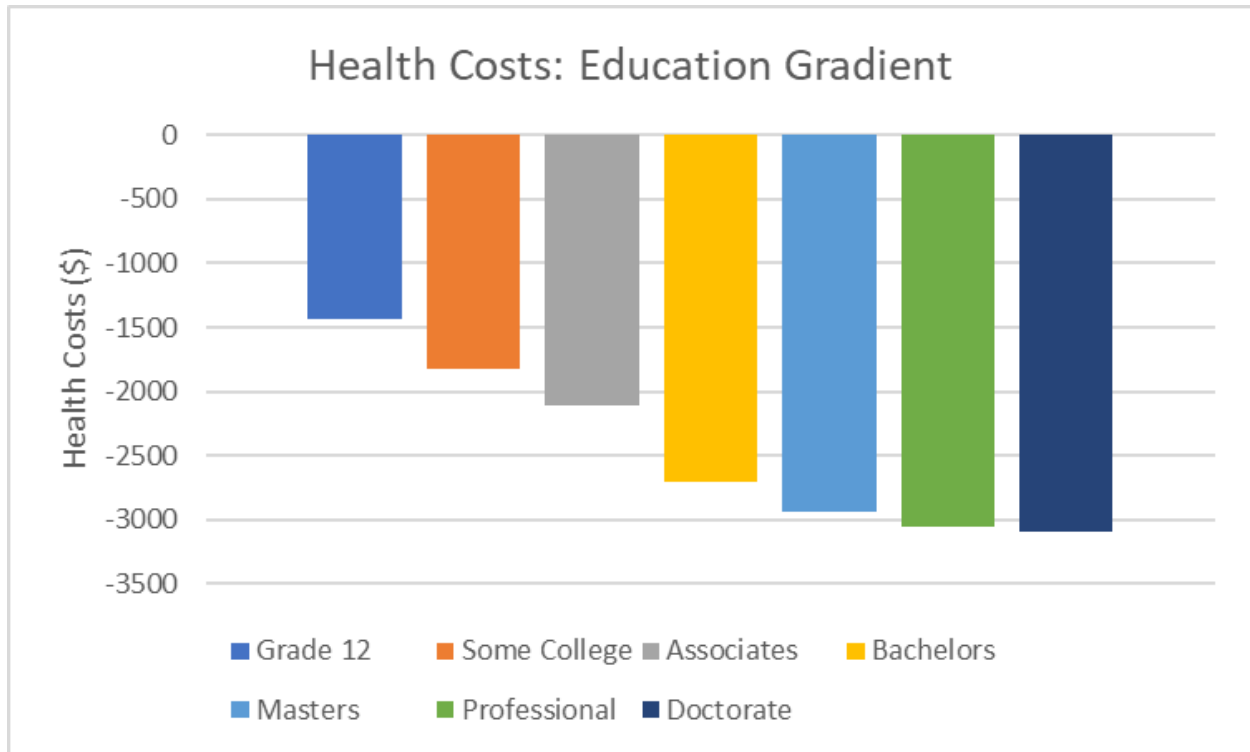
Note: This figure shows the gradient between educational attainment and health costs, a key outcome. Health costs (measured in dollars) were regressed on education indicator variables, one for each bar on the X-axis. Each bar's height shows the estimated relationship between the associated level of educational attainment and health costs, measured in dollars. The sample was restricted to individuals with at least an 11th grade education. All estimates are relative to the omitted group, individuals whose highest level of education was 11th grade. Statistics presented here are calculated from publicly available data.
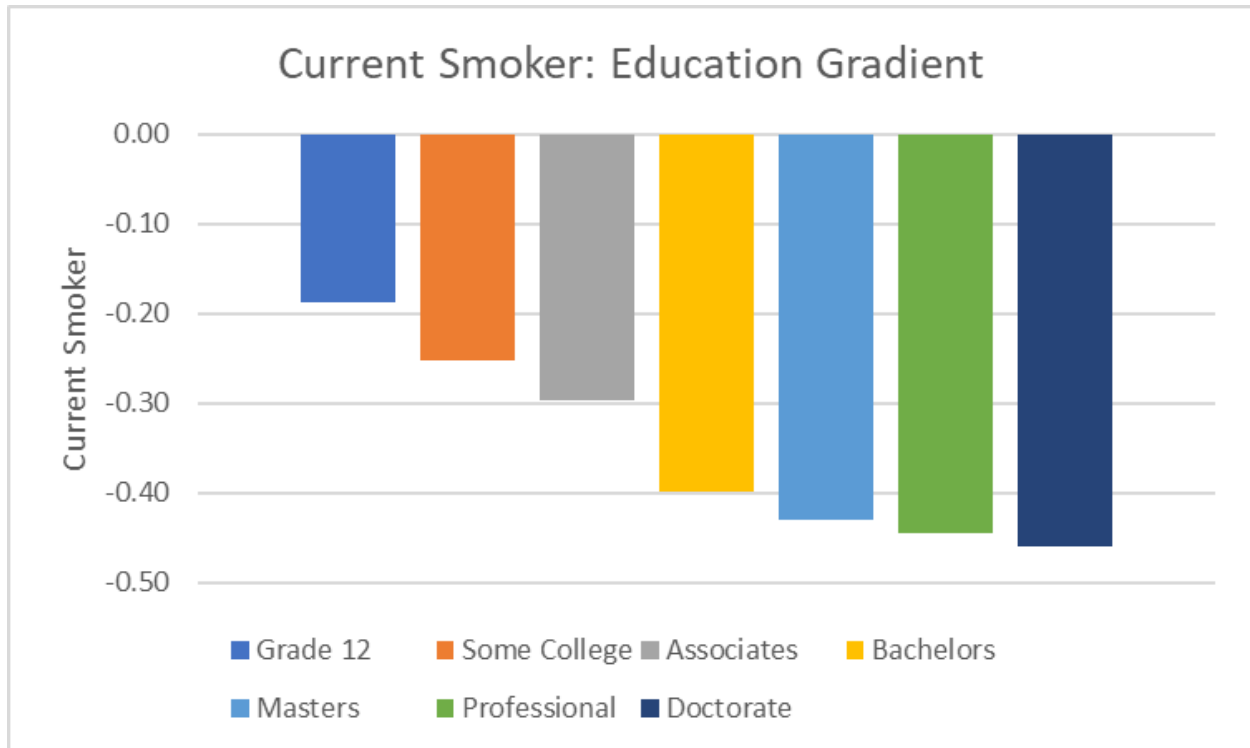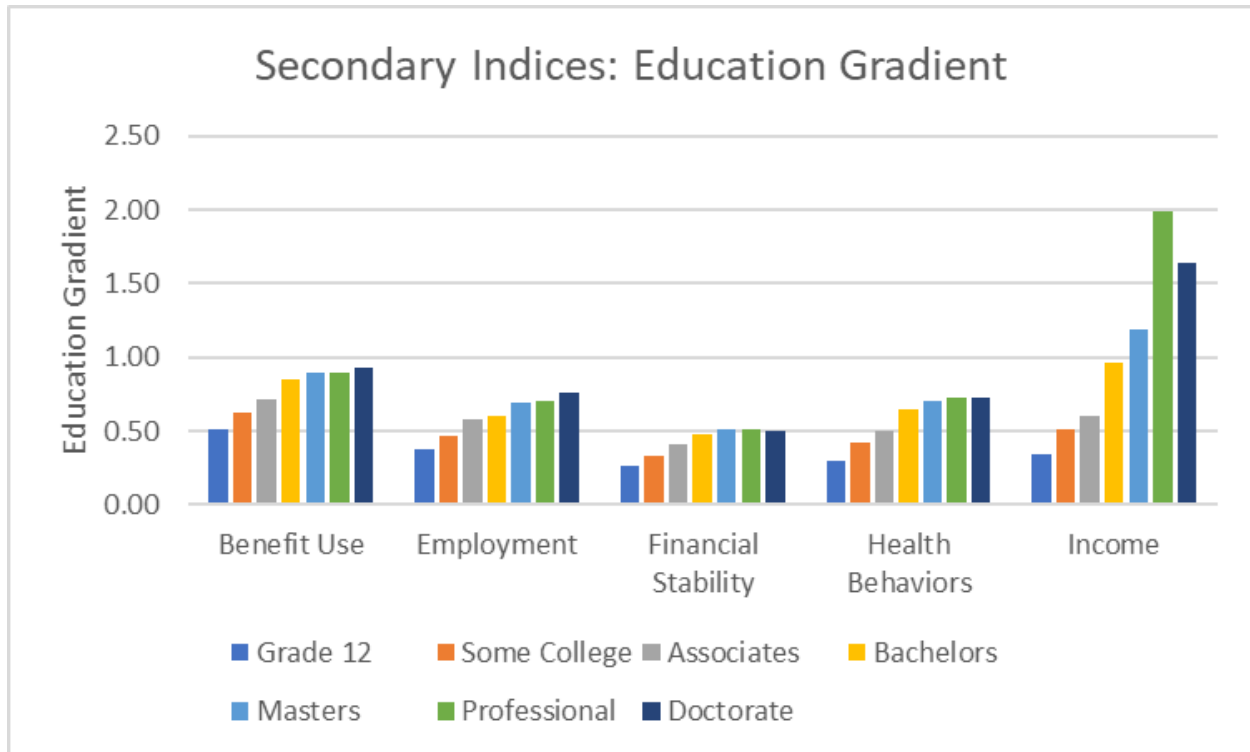
Figure A4

Note: This figure shows the gradient between educational attainment and the poor health, a key outcome. The poor health indicator was regressed on education indicator variables, one for each bar on the X-axis. Each bar's height shows the estimated relationship between the associated level of educational attainment and poor health, which is a binary variable indicating that the individual reported their health as either "fair" or "poor". The sample was restricted to individuals with at least an 11th grade education. All estimates are relative to the omitted group, individuals whose highest level of education was 11th grade. Statistics presented here are calculated from publicly available data.

Note: This figure shows the gradient between educational attainment and the costs of secondhand smoke, a key outcome. Secondhand smoke costs (measured in dollars) were regressed on education indicator variables, one for each bar on the X-axis. Each bar's height shows the estimated relationship between the associated level of educational attainment and this key outcome. The sample was restricted to individuals with at least an 11th grade education. All estimates are relative to the omitted group, individuals whose highest level of education was 11th grade. Statistics presented here are calculated from publicly available data.

**Current Smoker: Education Gradient**

Note: This figure shows the gradient between educational attainment and the smoking, a key outcome. An indicator variable for smoking was regressed on education indicator variables, one for each bar on the X-axis. An individual is defined as a smoker if they reported smoking at least occasionally. Each bar's height shows the estimated relationship between the associated level of educational attainment and the key outcome. The sample was restricted to individuals with at least an 11th grade education. All estimates are relative to the omitted group, individuals whose highest level of education was 11th grade. Statistics presented here are calculated from publicly available data.

Note: This figure shows the gradient between educational attainment and each of the constructed secondary indices; benefit use, employment, financial stability, health behaviors, and income indices. Each index was regressed on education indicator variables, one for each bar on the X-axis. Each bar's height shows the estimated relationship between the associated level of educational attainment and the secondary outcome, measured in standard deviations. The sample was restricted to individuals with at least an 11th grade education. All estimates are relative to the omitted group, individuals whose highest level of education was 11th grade. Statistics presented here are calculated from publicly available data.

Education Gradients by Age
Secondary Indices
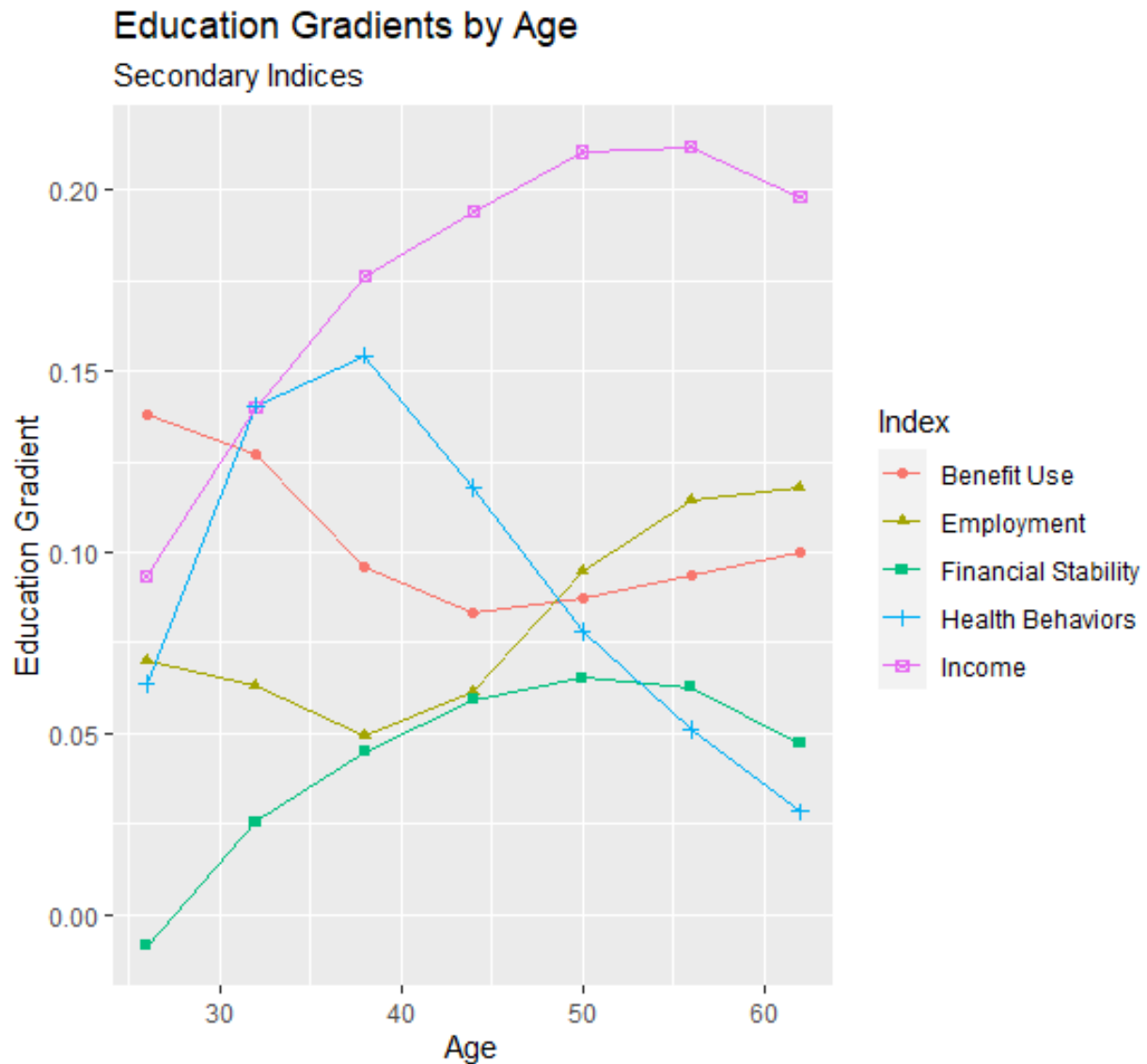
Note: This figure plots the relationship between our on secondary outcome indices and years of education across ages. Specifically, each dot represents the coefficient from a regression of a primary outcome measure on years of education in six-year bins (i.e., age 23-28, 29-34, 35-40, 41-46, 47-52, 53-58, 59-64) . Years of education is defined as years of education through grade 16 (i.e., a bachelor's degree). Other variables are constructed as described in the text. Statistics presented here are calculated from publicly available data.

Figure A9
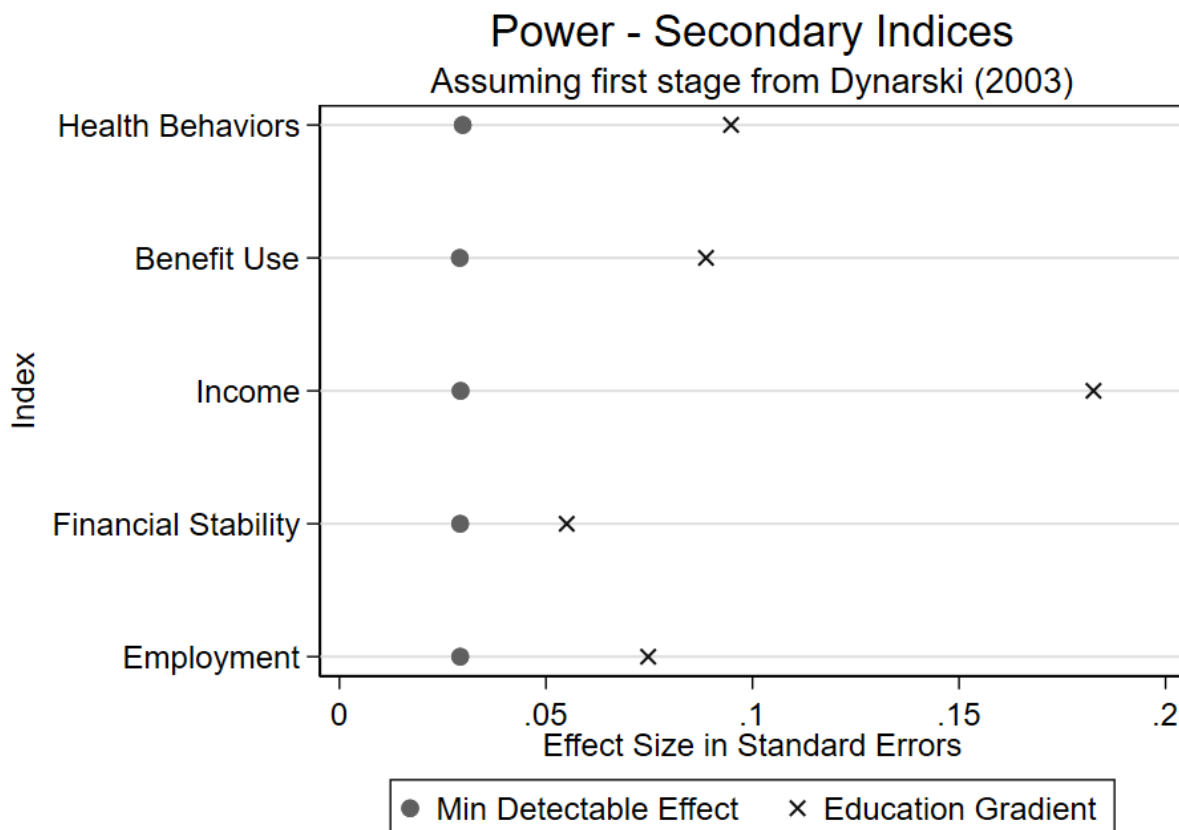
Power - Secondary Indices
Assuming first stage from Dynarski (2003)

Note: This figure displays the magnitude of both the education gradient and minimal detectable effect sizes for secondary outcome indices. Education gradients are the result of regressing each index on years of education, where years of education is defined as years of education through grade 16 (i.e. a bachelor's degree). Minimal detectable effect size is calculated at the 95% confidence level and 80% power based on the standard errors from placebo analysis combined with first stage estimates from Dynarski (2003). This process is described in more detail in Section 6.3. Other variables are constructed as described in the text. Statistics presented here are calculated from publicly available data.

B Indices Appendix

This section lists the original IPUMS-CPS variable(s) used to generate the components of each index and provides a description of each component. All variables have been transformed so that higher values are more preferred outcomes; variables or indices that have been flipped (so that higher values are "better") are indicated with a star*.

Functioning, Disability, and Health Index*:

- (Not yet generated): Ever received SSA disability benefits
- diffany: Any physical or cognitive difficulty
- health: Self-Rated Health status (1 to 5 where 5 is very poor health)
- quitsick: Ever retired or left a job for health reasons
- disabwrk: Disability limits or prevents work

Benefit Use Index*:

- rentsub, pubhous: Living in public or subsidized housing
- foodstmp: Receiving food stamps/SNAP
- lunchsub: Kids receiving free or reduced-price lunch
- incwelfr: Income from welfare programs
- incdisab: Receiving any income from disability
- incss: Receiving any income from social security
- incwelfr: Receiving any income from welfare programs
- incunemp: Receiving any income from unemployment
- incwkcom: Receiving any income from workers comp
- caidly: Covered by Medicaid last year
- gotwic: Household receiving WIC in last year
- fsfdstmp: Received SNAP (or Food Stamps) in the past year

Health Behaviors Index:

- anyhcov: Any health insurance[55]
- tcig100*: Smoked 100 or more cigarettes in entire life
- tfreq*: Frequency of smoking

Income Index:

- hhincome: Household Total Income
- ftotval: Family Total Income
- inctot: Total personal income
- offpov*: Indicator for those below the official poverty line

Employment Index:

- labforce: In the Labor force
- workly: Worked last year
- empstat: Employed

Financial stability Index:

- (Not yet generated)*: Estimated income variance using administrative panel of reported SSA/Tax earnings
- ownhome: Owns home
- bunbanked: Has checking or savings account
- fsstatus*: Food security status (1 is food secure, 3 is very low food secure)

C Current Population Survey: Linkable Supplements

Food security: The food security supplement contains data on household food program participation (SNAP), food expenditures, and food security (adults and children). The

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[55]In 2014 this changes from indicating any insurance last year to indicating any insurance at the time of the survey.

supplement is available and linkable in December (2001-2017) and April (1997, 1999, 2001). Variables selected from this sample include:

- fsstatus: Household food security scale
- fsfdstmp: Anyone in household receiving food stamp benefits in the last year
- fswic: Number of women or children in household receiving WIC in last 30 days

We are able to link 783,849 individuals from the food security supplements to our sample of ASEC survey years. Of those observations, 133,779 are for people who turned 18 between 1974-1986. There may be fewer observations for individual variables that were not asked in each supplement wave.

Tobacco Use: The tobacco use supplement contains data on individuals' personal tobacco use and frequency, whether they are trying, have tried, or want to quit, methods used to quit, opinion on smoking policies. This supplement is available and linkable in January (1996, 1999, 2000, 2007, 2011, 2015), February (2002, 2003), May (1996, 1999, 2000, 2006, 2010, 2011, 2015), and June (2003). Variables selected from this sample include:

- tfreq: Current frequency of cigarette smoking
- tcig100: Smoked more than 100 cigarettes in entire life.
- tagesmk: Age when first started smoking cigarettes fairly regularly

We are able to link 1,027,475 individuals from the tobacco use supplement to our sample of ASEC survey years. Of those observations, 170,504 are for people who turned 18 between 1974-1986. There may be fewer observations for variables that were not asked in each supplement wave.

Fertility and Marriage: The fertility and marriage supplement includes data on marriages and births; number of births, age of mother at last birth, age in months at first marriage, etc. This supplement is available and linkable in June (1994, biannual from 1998 on). Variables selected from this sample include:

- frever: Number of live births ever

- frage1: Age in months of mother at first birth

- mhagemar1: Age at first marriage

We are able to link 355,144 from the fertility and marriage to our sample of ASEC survey years. Of those observations, 70,516 are for people who turned 18 between 1974-1986. There may be fewer observations for variables that were not asked in each supplement wave.

Un(der)banked: The Un(der)banked supplement includes data on bank account ownership (i.e., whether person has a savings or checking account, has ever had one, or plans to get one), where individuals cash checks, which member of household manages finances, and the use of payday loans, pawn shops, rent-to-own, or tax return advance loan. This supplement is available and linkable in January (2009) and June (2011, 2013). The only variable currently selected from this sample is:

- Bunbanked: Household currently has savings or checking account

We are able to link 178,549 from supplement to our sample of ASEC survey years. Of those observations, 28,081 are for people who turned 18 between 1974-1986. There may be fewer observations for variables that were not asked in each supplement wave.