HIGHER EDUCATION AND MORTALITY: LEGACIES OF AN AUTHORITARIAN COLLEGE CONTRACTION

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Abstract
We provide new evidence on the causal effect of higher education on mortality. Our empirical strategy exploits the reduction in college openings introduced by the Pinochet regime after the 1973 coup in Chile, which led to a sharp downward kink in college enrollment among those cohorts reaching college age in the following years. Using administrative data from the vital statistics, we document an upward kink in the age-specific yearly mortality rate of individuals in the affected cohorts. We estimate a negative effect of college on mortality between ages 34–74, which is larger for men, but also sizable for women. Individuals in the affected cohorts experience worse labor market outcomes, are more likely to be enrolled in the public health system, and report lower consumption of health services. This suggests that economic disadvantage and limited access to care play an important mediating role in the link between higher education and mortality. (JEL: I12, I23, I26, I28)

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Teaching Slides
A set of Teaching Slides to accompany this article is available online as Supplementary Data.

1. Introduction

The relationship between education and health ranks among the most widely studied in economics. More educated people live longer lives, but research on the causal effect of education on mortality has provided largely null findings (Galama, Lleras-Muney, and van Kippersluis 2018; Xue, Cheng, and Zhang 2021). However, previous work has mostly studied compulsory schooling laws affecting primary or secondary education. Little is known about the impact of *higher* education on mortality (Buckles et al. 2016; Taylor 2017). Insofar as primary, secondary, and tertiary education differentially affect preferences, knowledge, income, or other health inputs (Cutler and Lleras-Muney 2008; Montez, Hummer, and Hayward 2012), the effect of education on mortality is likely to vary across different levels of educational attainment.

We provide new evidence on the causal effect of higher education on mortality. We exploit variation in college enrollment among cohorts reaching college age in a narrow window around the 1973 military coup that brought Augusto Pinochet to power in Chile. The Pinochet regime quickly assumed control of all universities and steadily reduced public subsidies to higher education, which led to a steady reduction in the number of openings offered to incoming students (Bautista et al. 2023a). As a result, college-age cohorts experienced a sharp downward kink in college enrollment after 1973. This kink was entirely supply-driven: The secondary completion rate remained on trend and college applicants greatly exceeded openings throughout this period. The centralized admissions process based on a standardized exam also remained unchanged.

We document a higher age-specific mortality rate among individuals exposed to fewer college opportunities. Our empirical analysis uses individual-level records from the 1992 population census and vital statistics between 1994 and 2017 to calculate yearly mortality rates by cohort, gender, and region of residence. To ensure a relevant counterfactual for college enrollment, we exploit information about education in the vital statistics and restrict attention to individuals who completed secondary education and reached college age between 1964 and 1981. Our baseline sample includes close to 1 million individuals, who we observe between ages 34 and 74.

Before 1973, college enrollment was rising by close to 2 percentage points (pp) per cohort. This trend breaks after the coup and enrollment begins to *decrease* by 2 pp per cohort, equivalent to a 6% drop relative to the sample mean. Accounting for region-by-year and age fixed effects, we find that yearly deaths per 1,000 increase by 0.11 for each additional female cohort reaching college age after 1973. For men, we find that mortality increases by 0.31 deaths per 1,000 for each post-coup cohort. These effects are equivalent to 2.9% and 4.4% of the respective sample means. They are largely driven by deaths from cancer and diseases of the circulatory or digestive systems. The results are similar if we collapse the data to the country level, if we expand the sample to include people without complete secondary, or if we examine smaller windows of cohorts or ages.

We estimate the effect of college enrollment on mortality using as an excluded instrument the downward kink in enrollment for cohorts reaching college age after 1973, in the spirit of a regression kink design (RKD) (Nielsen, Srensen, and Taber 2010; Card, Lee, Pei, and Weber 2015; Card et al. 2016; Dong 2018). In support of our identification strategy, we show that the affected cohorts exhibit no kink in predetermined characteristics such as secondary completion or mortality before reaching college age. There is also no kink in mortality during our study period among individuals without complete secondary education. Our instrumental variables (IV) estimates suggest that college enrollment reduces yearly age-specific mortality by 0.27 pp for women and by 0.91 pp for men. These estimates correspond to local average treatment effects (LATE) (Imbens and Angrist 1994) for those individuals who wanted to attend college but failed to gain admission because of the reduction in openings. We show that this supply-side shock predominantly affected applicants from less affluent backgrounds, whose test scores were close to the cut-off determining admission.

Our analysis of mechanisms indicates that reduced access to college leads to worse labor market outcomes and limited access to medical care. Using data from 13 waves of a large household survey called Encuesta de Caracterización Socioeconómica Nacional (CASEN) between 1990 and 2017, we show that lower college enrollment decreases labor force participation, the quality of occupation, and monthly income. Fewer college opportunities also increase reliance on the more congested public health system and lead to lower consumption of health services, including visits to primary care physicians or specialists, and preventive care procedures (i.e., Pap smear for women). Hospital discharge summaries linked with the mortality files at the individual level further show that hospitalized patients in the affected cohorts exhibit an upward kink in mortality over multiple time horizons, ranging from 6 months to 5 years.

We contribute primarily to the literature studying the effects of education on health (Galama, Lleras-Muney, and van Kippersluis 2018), which is part of a broader research agenda documenting the non-pecuniary consequences of education (Grossman 2006; Lochner 2011; Oreopoulos and Salvanes 2011). An extensive body of work—dating back to Kitagawa and Hauser (1968)—has documented an educational gradient in health status. However, causal evidence remains limited. Previous research on mortality has mostly exploited changes to compulsory schooling laws affecting lower levels of education and has found largely null results.[1] The findings from these studies have limited external validity to higher education, insofar as the latter could plausibly have a differential effect on health behaviors and mediating socioeconomic outcomes (Cutler and Lleras-Muney 2008, 2010). Moreover, this literature has largely focused

1. See Lleras-Muney (2005); Mazumder (2008); Albouy and Lequien (2009); Van Kippersluis et al. (2011); Clark and Royer (2013); Black, Hsu, and Taylor (2015); Meghir, Palme, and Simeonova (2018). Papers studying other health outcomes are similarly inconclusive (Arendt 2005; Oreopoulos 2006; Braakmann 2011; Kemptner, Jürges, and Reinhold 2011).

on European countries with relatively equitable access to high-quality medical care, which could attenuate the impact of education.

Two previous studies examine the effect of higher education on mortality, both focused on white individuals in the United States. Buckles et al. (2016) exploit avoidance of the military draft during the Vietnam war (Card and Lemieux 2001) and find that college reduces male mortality, but face the complication that men exposed to the draft were also more likely to fight in the war. Taylor (2017) leverages variation in the distance to the nearest college at age 17 and finds no average effect of college enrollment on male or female mortality. We contribute to the literature by exploiting a unique setting that allows us to credibly estimate the causal effect of college enrollment on mortality for men and women, both in the population at large and among hospitalized patients. Our setting further enables us to directly combine data from multiple sources to gain insights on the mediating role of labor market outcomes and consumption of health services.

2. Institutional Background

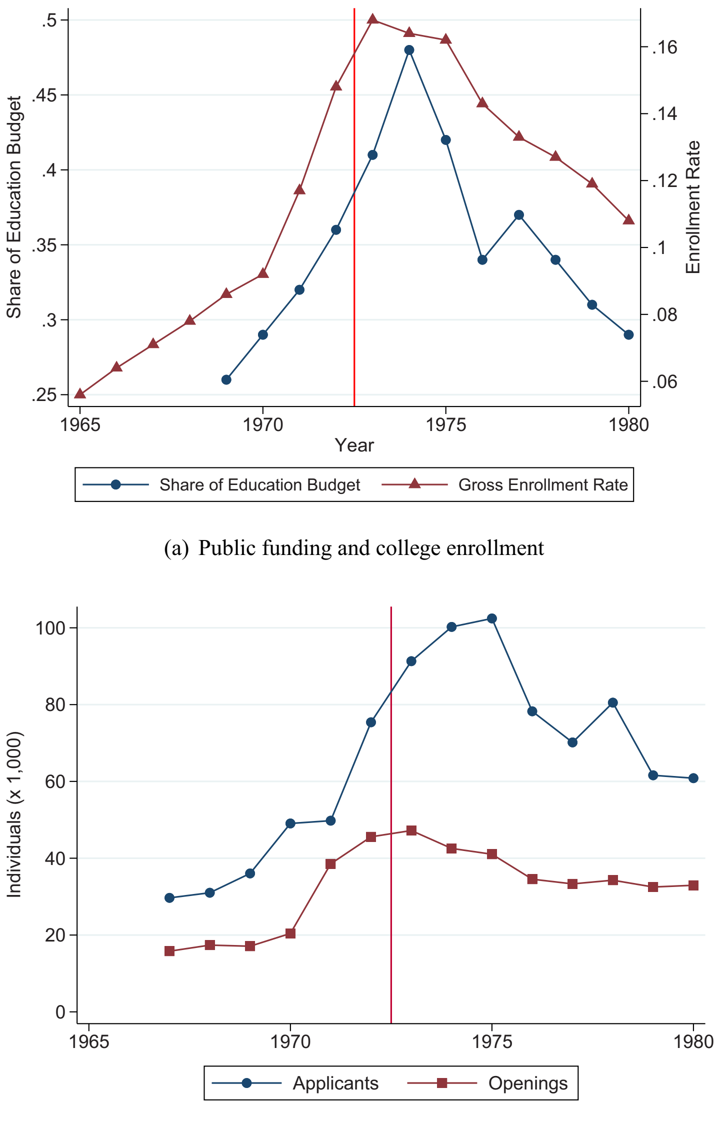
2.1. Regime Change and Higher Education

President Salvador Allende was overthrown by a military coup on September 11, 1973. At the time, there were eight universities in Chile, with presence throughout the country. Only two universities were public, but the entire system depended almost exclusively on government funding, which represented 77% of total revenue in 1972. Differentiated tuition based on family income was charged, but fees were generally low. Starting in 1967, admissions were determined by a deferred acceptance algorithm based on applicants' preferences, their score in a centralized admissions exam and the yearly number of openings that universities made available for each program.[2]

A military junta presided by Augusto Pinochet took control of the government after the coup and remained in power until 1990. The junta quickly appointed members of the armed forces as rectors to all universities and gave them authority over university administration. Soon afterwards, policy began to be influenced by a group of free-market economists known as the *Chicago Boys* (Valdés 1995). The Chicago Boys advocated for a reduction in public subsidies to universities, arguing that these were inefficient and failed to promote effort and thrift. Following their advice, the military government steadily cut back on its contributions to universities over the following years. Panel (a) in Figure 1 shows that the share of the government's education budget allocated to universities fell from a high of 47% in 1974 to 29% in 1980 (38% decrease).[3]

2. Military academies graduated less than 1% of college graduates per year and did not experience any major reform in the 20th century. If anything, their admission criteria became stricter after 1973 (Fuentes 2012).

3. These cuts mostly correspond to a reallocation of public spending on education toward lower levels (Bautista et al. 2023a). Online Appendix Figure A.1 shows that government spending on education and health steadily increased after 1975.



(a) Public funding and college enrollment

(b) College applicants and openings

FIGURE 1. Higher education around the 1973 coup. Panel (a) shows higher education's share of the government's education budget (circle markers, left axis) and the gross enrollment rate in higher education (triangle markers, right axis). Panel (b) shows the yearly number (in thousands) of college applicants (circle markers) and openings on offer by the universities (square markers).

The reduction in funding was not compensated by increases in tuition or revenue from other sources, which forced universities to reduce the number of openings for new students. Panel (b) in Figure 1 shows that college openings peaked at 47,000 in 1973, equivalent to 71% of graduating secondary students on that year, and fell to 34,000 in 1980 (35% of secondary graduates). This led to a dramatic decrease in the gross college enrollment rate, shown in panel (a), which also peaked at 17% in 1973 and fell to 11% in 1980. UNESCO projections for 1975 overestimated college enrollment by 33%, which suggests that the reduction was not expected (Levy 1986a). Panel (b) also shows that the number of applicants exceeded the number of openings throughout this period. Hence, the supply of openings was always the binding constraint on initial college enrollment.

Several important aspects of the higher education system remained constant during this period, including the centralized admissions process. As a result, the reduction in openings mechanically affected students with lower tests scores who were close to the admissions cut-off. We show below that these students predominantly came from less affluent socioeconomic backgrounds. Bautista et al. (2023a) further show that the distribution of students across fields hardly changed and the vast majority of academic units continued functioning. Even though the first months after the coup were characterized by strong repression, political persecution was highly targeted and "most previously enrolled students remained enrolled" (Levy 1986a, p. 101).[4] Online Appendix Figure A.2 shows that over two-thirds of individuals with some college education report at least 4 years of college in all of our study cohorts, which suggests a graduation rate comparable to other settings (e.g., Zimmerman 2014). The number of universities and campuses also remained unchanged until a reform in 1981.

The change in political conditions plausibly affected the college experience after 1973, with a greater emphasis on academic achievement being offset by a limited development of critical thinking and restricted extracurricular activities (Levy 1986b).[5] Online Appendix Figure A.2 shows that the trends for both the college graduation rate and the college premium in earnings exhibit upward kinks for the cohorts that reached college age after 1973. These patterns are consistent with an improvement in the quality of higher education but could also reflect positive selection in admissions or match effects (Arcidiacono and Lovenheim 2016). Focusing on inputs, a back-of-the-envelope calculation suggests that college enrollment declined more than public funding after the coup, such that public spending per student increased 57% in real terms between 1973 and 1981 (Arriagada 1989). Hence, while we cannot conclusively assert that the quality of higher education improved after the coup, there is little evidence to suggest that it worsened.

2.2. *Health System*

Chile experienced rapid economic growth since the mid-1980s, with GDP per capita (in constant 2010 USD) rising from $4,700 in 1985 to $14,700 in 2015. Sustained

4. Twenty-four of the 3,200 documented state-led killings during the Pinochet regime correspond to university professors and 252 to students from all levels (Comisión Rettig 1996). There were 145,000 college students in 1973.

5. Brunner (1984) argues that pedagogic practices remained unchanged but academic curricula became less flexible.

economic growth coincided with improved health: life expectancy rose from 62 to 75 years between 1970 and 1990 and infant mortality plummeted from 67 to 16 deaths per 1,000 live births. Overall, Chile is near the OECD average for several health outcomes (OECD 2019), although it has relatively high rates of smoking (25% of adults), obesity (74%), and chronic disease morbidity (9% of adults with diabetes).[6]

The health insurance system in Chile was created in 1979–1981 and includes private and public providers. The public option is financed with public funds and a payroll tax, has no exclusions, and offers different co-payment based on income and number of dependents. Private insurance companies compete in a regulated market by offering contracts at different prices, and receive payroll tax contributions from their members. The share of the population covered by the public option has risen from 66% in the 1990s to almost 80% in more recent years (MDS 2018), partly due to the rising cost of private insurance (Galetovic and Sanhueza 2013). The public option serves lower-income and higher-risk individuals, while private providers serve a richer, healthier, and younger segment of the population (Pardo and Schott 2012). Switching across sectors is uncommon, though the public option often acts as a safety net (Duarte 2011). Provision of health services (laboratories, clinics, and hospitals) also involves private and public providers. Public providers are more crowded and have longer wait times.[7] Private insurance companies offer flexibility over providers and reduced copay for preferred providers. In contrast, the public insurance only covers services by public providers and additional payment is required to access private providers.

3. Data

3.1. Data Sources

We rely on four main data sources. First, individual death records from the vital statistics for the period 1994–2017. Second, individual records from the 1992 and 2002 population censuses. Third, the universe of hospital discharge summaries for the period 2002–2018. Lastly, individual responses in all 13 waves of the CASEN household survey between 1990 and 2017.

The Department of Health Statistics provides rich individual-level data from death certificates since 1994. This includes year of birth, gender, educational attainment, county of residence, and cause of death. We combine the mortality files with the 1992 census provided by the National Institute of Statistics to calculate yearly mortality

6. Health spending as a percentage of GDP increased from 5% in the 1990s to 9% in 2017 but remains relatively efficient. Chile ranked 8th out of 55 countries in the Bloomberg healthcare efficiency index (Clínicas de Chile 2016).

7. In 2016, 24% of hospitals were private, but employed 55% of doctors (Clínicas de Chile 2016). Online Appendix Figure A.3 shows that the number of doctors per 1,000 beneficiaries in 2014 was 5.3 in the private sector and 1.2 in the public sector. Spending per capita in 2014 was close to $3,000 in the private sector and less than $1,000 in the public option.

rates. The census provides information on basic demographic characteristics and educational attainment. We calculate the mortality rate at the cohort-gender-region level for each year between 1994 and 2017.[8] We also calculate disaggregate rates by cause of death based on the ICD classification. For these calculations, we iteratively adjust the population at risk based on the previous number of deaths per cell, following Clark and Royer (2013). We measure the initial share with college from the 1992 census and similarly adjust it based on previous deaths. We focus on college enrollment, rather than completion, as this was the margin directly affected by the contraction after 1973. The youngest cohort in the panel has age 32 in 1992, so we can confidently assume that everyone in our sample has completed their education.

We also use administrative data for the universe of hospital discharge summaries between 2002 and 2018. This dataset includes almost 5 million hospitalizations and reports basic socioeconomic characteristics of the patient, as well as the hospital of admission, diagnostic, and type of insurance, among others. Unfortunately, educational attainment is not reported. We combine this dataset with the 2002 census to construct yearly hospitalization rates at the cohort-gender-region level. Importantly, we can merge the discharge summaries with the mortality files based on a unique individual identifier, which enables us to track the mortality of hospitalized patients.

Finally, we use all 13 waves of the biennial CASEN household survey between 1990 and 2017. This survey records information on education, health, income, and labor market outcomes. We use CASEN to study mediating outcomes such as labor force participation, occupation, income, and type of insurance. We also examine consumption of health services, including visits to primary care physicians, specialists, or the emergency room, and preventive care procedures such as the Pap smear for women. CASEN's latest waves include more than 260,000 individuals from over 80,000 households. However, sample sizes vary because not every question is asked in every wave.

3.2. *Sample Inclusion Criteria and Descriptive Statistics*

We restrict the sample to individuals born between 1943 and 1960, who reached age 21 between 1964 and 1981. This leaves us with an 18-cohort window centered around 1973. We focus on age 21 because this was the average age of first-year college students in 1970 (Schiefelbein 1976). We end the sample with the 1981 cohort to avoid confounding effects from a subsequent reform of the university system. In our baseline analysis, we observe the study cohorts between ages 34 and 74, but we show below that our results are robust to using alternative windows of ages or cohorts.

To ensure a relevant counterfactual for college enrollment, we further restrict our baseline sample to individuals reporting four or more years of secondary education. Information on secondary completion is mostly unavailable in our sources, but 4 years

8. We rely on region of residence because region of birth is not reported in the vital statistics. We exclude Chileans living abroad and foreign nationals. Chile is administratively divided into 346 counties located in 16 regions.

corresponds to the statutory length. In the absence of this restriction, the comparison group also includes individuals with incomplete secondary or less. This reduces the average number of years of education in the comparison group without college from 12 to 8 (i.e., a reduction from full secondary to full primary). Since people with lower educational attainment have higher mortality, as shown in Online Appendix Figure A.4, this comparison could conflate the effect of college enrollment with that of completing secondary education.

A possible concern about this restriction is that the college contraction may have reduced the incentive for secondary students to complete their degree as a prerequisite to access higher education. If so, the sample restriction will likely bias our estimates downwards, insofar as the students that drop out of secondary plausibly have lower socioeconomic status (SES) and poorer health. However, it seems unlikely that access to higher education is the main driver behind secondary completion if the prospect of college admission is sufficiently uncertain and there are high returns to a secondary degree (Hungerford and Solon 1987).[9] In line with this argument, we show below that secondary completion does not change among the cohorts affected by the college contraction. We also show that the effect of college on mortality is larger in the unrestricted sample, which suggests that the sample restriction to individuals with complete secondary is a conservative choice.

Another potential concern is that the sample restriction to individuals with complete secondary relies crucially on the information on education from the mortality files, which could introduce significant measurement error (Currie and Schwandt 2016). This is not a major issue in our setting, given that the information on education in the death certificates is cross-checked with administrative data using the unique national identification number of the deceased. This process minimizes measurement error and leads to less than 0.1% of missing data. In terms of data quality, Chile's vital statistics rank among the best in the world (Mikkelsen et al. 2015).

Table 1 provides descriptive statistics for our main variables of interest, disaggregated by gender and by exposure to the college contraction (i.e., college age before and after 1973). There are 1,017,162 individuals that meet our sample inclusion criteria in the 1992 census, with ages 32–49. There are 124,745 deaths among these individuals between 1994 and 2017 (ages 34–74), yielding an aggregate mortality rate of 12%. Male mortality is almost twice as high as female mortality (16% vs. 9%), similarly to other settings (Beltrán-Sánchez, Finch, and Crimmins 2015). Cohorts reaching college age after 1973 have higher rates of secondary completion, but lower college enrollment conditional on full secondary. They also have higher labor force participation, but are less likely to have a white-collar high-skill occupation. These differences in averages partly reflect the different ages at which we observe these cohorts. Our empirical strategy, which we introduce next, ensures that we only compare individuals from different cohorts at the same age.

9. Unreported estimates controlling for year, age, gender, any college, and a linear trend in years of education show that CASEN respondents in our study cohorts with full secondary have 38% higher income between ages 25–50.

TABLE 1. Descriptive statistics.

	Female		Male	
	1964–1972 (1)	1973–1981 (2)	1964–1972 (3)	1973–1981 (4)
Panel A: Education (Share per 10)				
Any primary	9.482	9.757	9.614	9.809
Any secondary	4.683	5.744	4.949	5.875
Complete secondary	2.850	3.728	3.137	3.760
Any college	0.826	1.001	1.064	1.127
Any college | complete secondary	2.898	2.686	3.392	2.997
Panel B: Health				
One-year mortality rate (per 1,000)	6.002	2.562	11.189	4.624
Primary care visit in past 3 months (=1)	0.225	0.187	0.158	0.117
Specialist visit in past 3 months (=1)	0.164	0.135	0.109	0.075
One-year hospital visit rate (per 1,000)	80.945	67.401	86.581	59.241
Panel C: Labor Market				
In labor force (=1)	0.423	0.592	0.773	0.932
White-collar high-skill occupation (=1)	0.500	0.434	0.421	0.348
Average monthly income	302.024	302.862	658.650	637.981
Public health insurance (=1)	0.636	0.622	0.562	0.552
Individuals in 1992 census	632,127	889,685	598,778	844,349
Individuals in 1992 census | full secondary	180,167	331,672	187,859	317,464
Deaths in 1994–2017 | full secondary	24,707	19,989	46,080	33,969

Notes: This table presents sample averages of the main variables, disaggregated by gender and exposure to the college contraction. The source for panel A is the 1992 census. The sample in panels B and C is restricted to individuals with complete secondary education. Yearly mortality rate: own calculations for the period 1994–2017 (ages 43–74 and 34–65 for older and younger cohorts), adjusted for previous mortality. Yearly share with hospital visits: own calculations for the period 2002–2018, adjusted for previous mortality. The source for primary care and specialist visits in panel B and all outcomes in panel C is the CASEN survey (1990–2017). Average monthly income is reported in thousands of constant 2015 Chilean pesos.

4. Empirical Strategy

4.1. *Econometric Specification*

If we could connect the 1992 census to the mortality files at the individual level, a natural model to estimate the relationship between college and mortality would be as follows:

$$D_{it} = \tau\, C_i + \delta\, X_{it} + \varepsilon_{it}, \tag{1}$$

where D_{it} is an indicator that takes the value of one if individual i died in period t. The vector X_{it} measures individual-level covariates, and C_i is an indicator that takes the value of one for individuals with some college education. This is a fixed individual characteristic assuming we only observe people with completed education. By restricting the sample to individuals with complete secondary, we can interpret the parameter of interest τ as the average difference in mortality for people with college relative to those that stop their schooling at the level immediately below.

Since we cannot link our data sources at the individual level, we aggregate the data into cohort-gender-region cells, as described above. This unit of observation allows us to account for differences in mortality across genders and regions.[10] Similarly to Lleras-Muney (2005), we derive the aggregate model by averaging equation (1) over individuals in a given cell:

$$\bar{D}_{krt} = \tau\, \bar{C}_{krt} + \delta\, \bar{X}_{krt} + \bar{\varepsilon}_{krt}, \tag{2}$$

where $\bar{D}_{krt}$ is the share of people from cohort k and region r who died in year t, while $\bar{C}_{krt}$ is the share of people with college.[11] By weighting each observation by the number of people in that cell, we obtain an estimate of τ identical to the one provided by the individual microdata (Angrist and Pischke 2009). As mentioned above, we follow Clark and Royer (2013) and iteratively adjust the denominators of $\bar{D}_{krt}$ and $\bar{C}_{krt}$ based on the previous number of deaths per cell.

We can estimate equation (2) using ordinary least squares (OLS), but concerns about omitted variables prevent us from interpreting τ as the causal effect of college enrollment (Card 1999). Examples of omitted variables include unobservable differences in genetic characteristics, parental inputs, or individual preferences (e.g., discount rates) that affect college enrollment and long-run health (Fuchs 1982).[12]

4.2. *Identification Strategy*

To identify the causal impact of college enrollment on mortality, we exploit the kink in enrollment observed for cohorts reaching college age shortly after the 1973 military coup, in the spirit of an RKD (Nielsen, Srensen, and Taber 2010; Card, Lee, Pei, and Weber 2015; Card et al. 2016; Dong 2018). In particular, we examine whether the cohort-level trend in mortality exhibits a kink analogous to the one observed for college enrollment. Although a cross-cohort comparison of this nature could raise concerns about confounding non-linear age effects, our setting allows us to observe the study cohorts repeatedly over 23 years. We leverage the longitudinal nature of the data to include age fixed effects in our regressions, thereby restricting the analysis to individuals of the same age. We estimate the following reduced-form specification:

$$\bar{D}_{krt} = \alpha_{j(k,t)} + \alpha_{rt} + \gamma\, Z_k + \nu_{krt}. \tag{3}$$

10. This aggregation minimizes measurement error caused by internal migration, which could be large in small cells (e.g., counties). Our results are unchanged if we aggregate to the cohort-gender-year level, which eliminates this risk.

11. For simplicity, we are omitting the gender subindex in equation (2), but the collapsed cells are gender-specific.

12. Previous work has shown, for instance, that low birth weight is associated with worse health during childhood and with reduced educational attainment (Behrman and Rosenzweig 2004; Black, Devereux, and Salvanes 2007). Household income is also positively correlated both with children's health and educational attainment (Case, Lubotsky, and Paxson 2002).

We set $k = 0$ for the 1951 birth cohort (which turns 21 in 1972), $k = 1$ for the 1952 birth cohort, and so on ($k \in [-8, 9]$). We focus on age 21 because this was the average age of first-year college students in 1970 (Schiefelbein 1976). The excluded instrument Z_k is defined as $[1(k > 0) \times k]$. That is, $Z_k = 0$ for $k \leq 0$ and $Z_k = k$ for $k > 0$. The parameter γ captures the linear trend break (i.e., kink) of interest. Besides the age fixed effects, $\alpha_{j(k,t)}$, we also include region-by-year fixed effects (α_{rt}) to account for place-based drivers of mortality and time-varying factors that could vary across regions, such as the construction of new healthcare facilities or the occurrence of natural disasters. We allow for spatial correlation in mortality by clustering the error term ν_{krt} at the region-year level. To account for within-cohort correlation in the error term, we also report the p-values from the wild cluster bootstrap procedure (Cameron, Gelbach, and Miller 2008; Canay, Santos, and Shaikh 2021).

In line with equation (3), we can similarly estimate the following first-stage specification:

$$\bar{C}_{krt} = \omega_{j(k,t)} + \omega_{rt} + \theta \, Z_k + \mu_{krt}, \tag{4}$$

where θ captures the kink in college enrollment for the affected cohorts. We expect $\widehat{\theta} < 0$ due to the college contraction. The corresponding second-stage regression is:

$$\bar{D}_{krt} = \varphi_{j(k,t)} + \varphi_{rt} + \beta \, \bar{C}_{krt} + \eta_{krt}. \tag{5}$$

4.3. *Identifying Assumptions and Characterization of Compliers*

The IV estimate of β in equation (5) is numerically equivalent to the fuzzy RKD estimate given by the ratio γ/θ. Under the four assumptions of (i) *relevance*, (ii) *monotonicity*, (iii) *smoothness*, and (iv) *exclusion restriction*, β identifies the LATE of college enrollment on mortality (Imbens and Angrist 1994; Card, Lee, Pei, and Weber 2015; Dong 2018). The relevance assumption states that $\theta \neq 0$, which we corroborate below. The monotonicity assumption states that no individual is more likely to enroll in college than they would have been in the absence of the college contraction. The reduction in college openings that we exploit, which corresponds to a supply-side shock that takes place under an unchanged admissions process (i.e., no targeting), suggests that this assumption is very likely to be satisfied. The smoothness assumption states that the distribution of potential treatment status and potential outcomes is continuously differentiable around the kink point.[13] This assumption is the RKD equivalent of the *independence* assumption in Imbens and Angrist (1994). The exclusion restriction implies that any change in mortality for the affected cohorts is only a consequence of their lower college enrollment. We provide a battery of evidence in support of the smoothness assumption and the exclusion restriction in the next section.

13. Nielsen, Srensen, and Taber (2010) show that γ/θ identifies τ in a constant-effects model under a weaker smoothness assumption.

The LATE identified by β captures the average causal effect of college enrollment on mortality for people that did not attend college because of the contraction after 1973. These *compliers* had test scores close to the admissions threshold and mechanically failed to gain admission when openings fell. Insofar as test scores correlate positively with innate ability and SES, we expect the compliers to be a mix of high-ability/low-SES and low-ability/high-SES applicants.[14] Using microdata from Encuesta de Ocupación y Desocupación (EOD), a large yearly employment survey for the Santiago region conducted since 1958, Online Appendix Table A.1 shows that the share of college students coming from the middle and bottom terciles of household income was increasing in the pre-coup cohorts, but decreases after the coup. There is also a reversal in the share of students with parents that either attended college or have a white-collar high-skill occupation, both of which are markers of high SES. These changes in the composition of the college student body indicate that our compliers mostly correspond to low SES students with high ability and test scores near the admissions cutoff.

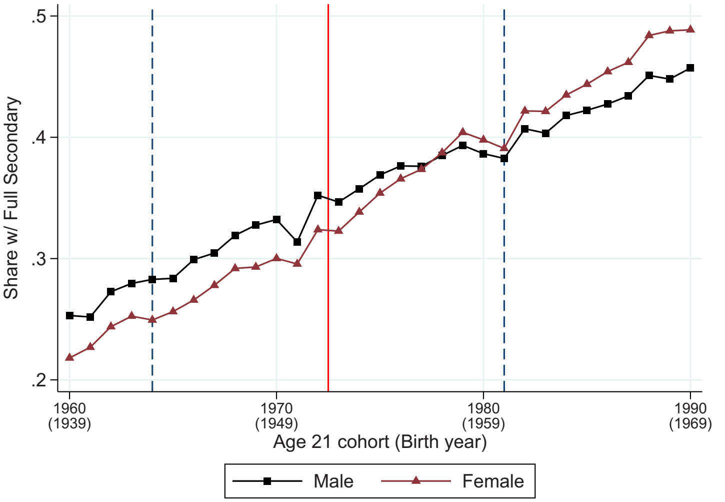
The ratio of applicants to openings in Figure 1 suggests that the compliers in our setting had high returns and wanted to attend college, but were prevented from doing so by the college contraction. In contrast, the changes in compulsory schooling that are often studied in the literature mostly affect low-SES students with low ability and force them to obtain education beyond what they want, which suggests low returns (Domnisoru 2021). These students plausibly exert less effort, which dampens the effect of the extra schooling (Lleras-Muney 2022). A change in compulsory schooling is also likely to have a smaller impact on subsequent economic opportunities than a change in access to higher education. For instance, college enrollment can provide low-SES students with access to high-SES peers, which is usually not the case for additional schooling at lower levels. In this regard, our research design is closer to those leveraging variation in distance to college, with the important difference that we study a supply-side reduction in the probability of admission while keeping the cost of college fixed. Whereas the Chilean college contraction mostly affected low-SES applicants with high ability, changes in the distance to college tend to affect all low-SES applicants, irrespective of ability, due to their high cost sensitivity (Mountjoy 2022).

5. College Enrollment and Mortality

5.1. The Kink in College Enrollment

Figure 2 plots raw data from the 1992 population census. Panel (a) shows the share of men and women per cohort with 4 or more years of secondary education. These shares rise smoothly from approximately 28% for the oldest cohort in our sample,

14. High-ability/high-SES applicants presumably have high test scores and are unaffected by the reduction in openings (*always-takers*) and low-ability/low-SES applicants have low scores and are also unaffected (*never-takers*).



(a) Complete secondary education

(b) College enrollment

FIGURE 2. Educational attainment of cohorts reaching college age around the 1973 coup. Panel (a) shows the share of people per cohort (normalized to age 21) and gender that report four or more years (i.e., statutory length) of secondary education in the 1992 census. Panel (b) shows the respective shares that report having any college education, among those with complete secondary education. Dashed lines show the start (1964) and end (1981) points of the study cohorts included in the analysis.

which reached age 21 in 1964, to 40% for our youngest cohort, which reached this age in 1981. Women overtake men in secondary completion starting with the 1977 cohort due to a steeper secular trend. Online Appendix Table A.2 provides regression estimates showing that these cohort trends in secondary completion do not change after 1973.[15] Online Appendix Figure A.5 plots the smooth estimated trends. Panel (b) in Figure 2 shows the respective shares that report any college education, among those with full secondary. Cohorts reaching age 21 before 1973 exhibit rising college enrollment, especially under Allende after 1970. In contrast, cohorts reaching this age after the military coup experienced a sharp decline in the enrollment rate, which fell from 38% for the 1972 cohort to 22% for the one reaching age 21 in 1980.

Table 2 presents regression estimates of the kink in college enrollment. Columns (1)–(4) show results for women and columns (5)–(8) for men. We report robust standard errors (clustered at the region-year level in columns (3)–(4) and (7)–(8)) in parentheses and p-values clustered by cohort in square brackets. Since the original variation in college enrollment takes place across cohorts, we begin the analysis using data from the 1992 census at the cohort-region level. The dependent variable in columns (1) and (5) is the share with any college education, which we express per ten people to facilitate the interpretation of results below. College enrollment was rising before 1973 by 1–2 pp per cohort, but this trend reverses and *drops* by 3–4 pp after 1973. By adding these coefficients, we obtain a net trend of −2 pp per cohort after 1973 (i.e., 6% of the sample mean). Columns (2) and (6) show a similar downward kink and a net downward trend if we examine average years of college instead. Going back to college enrollment, in columns (3) and (7), we use our longitudinal sample to provide estimates of equation (4) with age and region-year fixed effects, which absorb the baseline cohort trend. We also adjust the college enrollment rate based on previous mortality. The results are very similar to the cross-sectional regression. Columns (4) and (8) replicate this last analysis without the sample restriction based on secondary completion. As expected, including people ineligible for college in the sample leads to a smaller kink, though the estimates remain statistically and economically significant (relative to the smaller sample mean).

Panels (a) and (b) in Figure 3 illustrate the kink in college enrollment.[16] The markers show average college enrollment per cohort, the solid lines indicate the estimated trends before and after the coup, and the dashed line denotes the counterfactual trend for the post-coup cohorts. The parsimonious linear model accurately describes the evolution of the college enrollment rate across cohorts and captures the sharp negative kink for those reaching college age after 1973.

15. The slight drop in secondary completion for the cohorts reaching age 21 in 1971 and 1981 (1950 and 1960 birth years) is a consequence of age heaping in the census (Bachi 1951; Myers 1954; A'Hearn, Baten, and Crayen 2009). We verify below that our results are unaffected if we smooth the population counts based on a linear projection. In contrast, a seemingly downward kink in the share of men with complete secondary in Online Appendix Table A.2 disappears if we drop the cohort reaching age 21 in 1981 or if we use the smoothed population figures that account for age heaping in the census.

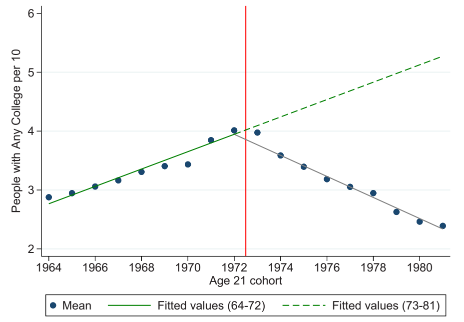
16. Online Appendix Figure A.6 provides analogous plots for the unrestricted sample including people without complete secondary.

TABLE 2. Educational attainment.

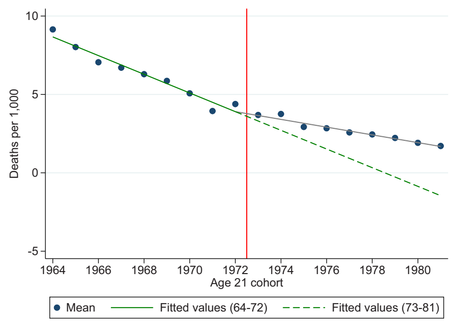
	Female				Male			
Dependent variable:	Share w/ college per 10 people (1)	Average years of college (2)	Share w/ college per 10 people (3)	Share w/ college per 10 people (4)	Share w/ college per 10 people (5)	Average years of college (6)	Share w/ college per 10 people (7)	Share w/ college per 10 people (8)
Cohort trend	0.204***	0.080***			0.150***	0.056***		
	(0.014)	(0.005)			(0.008)	(0.004)		
	[0.000]	[0.000]			[0.000]	[0.000]		
Cohort trend × After 1973	−0.402***	−0.157***	−0.411***	−0.133***	−0.331***	−0.130***	−0.342***	−0.131***
	(0.019)	(0.007)	(0.006)	(0.002)	(0.013)	(0.006)	(0.004)	(0.002)
	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]
Region fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Year-by-region fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Age fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Observations	270	270	6,480	6,480	270	270	6,480	6,480
R-squared	0.893	0.895	0.894	0.878	0.918	0.911	0.917	0.941
Average dependent variable	2.761	1.165	2.784	0.941	3.144	1.393	3.200	1.131

Notes: The unit of analysis is cohort-region in columns (1)–(2) and (5)–(6), and cohort-region-year in columns (3)–(4) and (7)–(8). Observations weighted by cell size. Dependent variable in the header. Sample includes all respondents of the 1992 census who reached age 21 between 1964 and 1981 (both inclusive) and report complete secondary education, except for columns (4) and (8) in which we drop the educational restriction. "Cohort trend" is a continuous variable indicating the year when the cohort reached 21 years of age, normalized to 0 in 1972. "Cohort trend × After 1973" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In columns (3)–(4) and (7)–(8), the share with college is iteratively adjusted based on previous mortality. Robust standard errors (clustered by region-year in columns (3)–(4) and (7)–(8)) reported in parentheses. P-value from wild cluster bootstrap by cohort reported in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

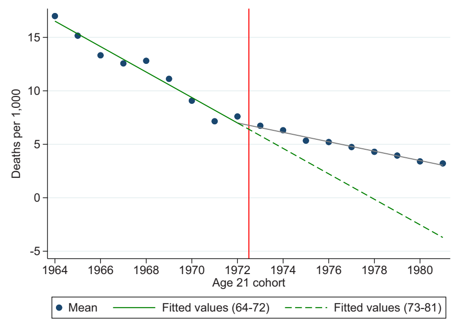
(a) Female: College enrollment



(b) Male: College enrollment



(c) Female: Mortality rate



(d) Male: Mortality rate

FIGURE 3. College enrollment and mortality. Panels (a) and (b) show the share per cohort with any college education, among people with complete secondary education in the 1992 census. Panels (c) and (d) show the average yearly number of deaths (per 1,000) between 1994 and 2017 (ages 34–74), among individuals with complete secondary education. Mortality rate is adjusted for previous deaths before averaging across years. Solid green line corresponds to the line of best fit for cohorts reaching age 21 before 1973, which we extrapolate for later cohorts (dashed line). Gray line corresponds to line of best fit for cohorts reaching age 21 in 1973 or afterwards.

5.2. Impact on Mortality

Panels (c)–(d) in Figure 3 show the average risk-adjusted yearly mortality rate by cohort between 1994 and 2017 (ages 34–74).[17] The simple linear model again yields an accurate representation of the cohort-level trend and provides clear indication of an upward kink for cohorts reaching college age after 1973. Panel A in Table 3 shows estimates of equation (3) with age fixed effects and quantifies this kink. Columns (1)–(4) show results for women and columns (5)–(8) for men.

Column (1) shows that annual deaths per 1,000 increased by 0.11 per cohort among women reaching college age after 1973. We observe a larger increase of 0.31 per cohort

17. Online Appendix Figure A.4 provides descriptive mortality profiles by age, year, and educational attainment.

TABLE 3. College enrollment and mortality.

	Dependent variable: Deaths per 1,000							
	Female				Male			
	Main specification (1)	+ cohort-region region trends (2)	without regional variation (3)	without education restriction (4)	Main specification (5)	+ cohort region trends (6)	without regional variation (7)	without education restriction (8)
Panel A: Reduced form								
Cohort trend × After 1973	0.110***	0.114***	0.107***	0.094***	0.311***	0.316***	0.302***	0.196***
	(0.022)	(0.021)	(0.024)	(0.012)	(0.042)	(0.037)	(0.040)	(0.021)
	[0.071]	[0.062]	[0.079]	[0.244]	[0.001]	[0.001]	[0.001]	[0.081]
Panel B: IV								
Share with college per ten people	−0.268***	−0.278***	−0.258***	−0.710***	−0.911***	−0.923***	−0.882***	−1.491***
	(0.051)	(0.049)	(0.058)	(0.089)	(0.116)	(0.104)	(0.117)	(0.149)
	[0.087]	[0.067]	[0.096]	[0.273]	[0.001]	[0.001]	[0.001]	[0.146]
Panel C: OLS								
Share with college per ten people	−0.197***	−0.229***	−0.221***	−0.426***	−0.799***	−0.809***	−0.853***	−0.857***
	(0.045)	(0.043)	(0.051)	(0.073)	(0.102)	(0.094)	(0.109)	(0.134)
	[0.083]	[0.052]	[0.098]	[0.198]	[0.001]	[0.001]	[0.001]	[0.081]
Year-by-region fixed effects	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Year fixed effects	No	No	Yes	No	No	No	Yes	No
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,480	6,480	432	6,480	6,480	6,480	432	6,480
R-squared (panel A)	0.760	0.765	0.972	0.912	0.853	0.859	0.979	0.928
R-squared (panel C)	0.760	0.765	0.972	0.912	0.853	0.859	0.979	0.927
Average dependent variable	3.751	3.751	3.751	4.265	6.984	6.984	6.984	7.720
Kleibergen-Paap F-stat (panel B)	4231	4435	4369	6236	8548	9102	6594	6622
Exclusion restriction test (% of RF)	70.5	72.4	67.8	77.1	79.6	81.5	80.3	82.9
H_0 : OLS = IV (p-value)	0.164	0.320	0.523	0.002	0.338	0.272	0.802	0.000

Notes: The unit of analysis is cohort-region-year, except columns (3) and (7) (cohort-year). Observations weighted by cell size. Sample period: 1994–2017 (Ages 34–74). Sample includes all respondents of the 1992 census who reached age 21 between 1964 and 1981 (both inclusive) and report complete secondary education, except columns (4) and (8) where we drop the educational restriction. "Cohort trend" is a continuous variable indicating the year when the cohort reached age 21, normalized to 0 in 1972. "Cohort trend × After 1973" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In panel B, this variable is used as excluded instrument for the share with college. Mortality rate and share with college adjusted for previous mortality. Standard errors clustered by region-year (robust in columns (3), (7)) reported in parentheses. P-value from wild cluster bootstrap by cohort in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

for men in column (5). These kinks are equivalent to 2.9% and 4.4% of the respective sample means. Columns (2) and (6) show that the results are the same when we include cohort trends by region, which previous research has shown to be an important predictor of mortality (Mazumder 2008; Black, Hsu, and Taylor 2015). Columns (3) and (7) show that the results also look very similar if we collapse the data to the cohort-year level (i.e., ignore variation across regions). This alleviates concerns about endogenous sorting across regions or measurement error due to internal migration. Columns (4) and (8) show that the results also remain comparable when we expand the sample to include individuals with less than full secondary education, although the estimates lose precision when we cluster by cohort. The stability of the coefficients in the modified sample suggests that the mortality trend for people without complete secondary education experienced little change, as we further verify below.

Panel B presents IV estimates of the effect of college enrollment. The Kleibergen-Paap F-statistics indicate a strong first-stage relationship. Since we express the share with college per ten individuals, we can interpret the estimated effects on deaths per 1,000 as percentage point effects on the probability of dying. Our baseline specification in column (1) shows that college enrollment reduces age-specific mortality by 0.27 pp among women and by 0.91 pp among men.[18] Rescaled to deaths per 1,000, these effects are equivalent to 71% and 130% of the respective sample means. We can reject at the 5% level that the estimates for men and women are equal. The IV estimates remain stable if we add cohort-region trends or if we collapse to the cohort-year level (columns 2–3 and 6–7). Columns (4) and (8) show that we obtain larger effects if we include in the sample individuals without full secondary. Intuitively, this modified sample includes individuals with much lower levels of education in the comparison group for people with college, which magnifies the effect.[19] However, these estimates are again imprecise when we cluster by cohort.

Panel C reveals that our IV estimates are only slightly larger than their OLS counterparts, and we generally fail to reject that the two are equal. Larger IV estimates are common in studies on the effects of education on health (Galama, Lleras-Muney, and van Kippersluis 2018). One explanation is that the returns to college are particularly high for the compliers (Card 1999). As shown above, the marginal applicants who failed to enroll in college after 1973 came from less affluent backgrounds and plausibly had high health returns. Our cohort-level instrument also captures educational spillovers across individuals without college that the OLS estimation fails to incorporate (Grossman 2006).

18. Online Appendix Table A.5 shows that one year of college reduces mortality by 0.07 pp for women and by 0.24 pp for men, equivalent to 19% and 34% of the respective sample means. Buckles et al. (2016) finds that 1 year of college reduces the mortality of white men in the US over 25 years by 19% relative to the sample mean.

19. The fact that the reduced-form coefficients hardly change in the expanded sample, while the first-stage impact on college enrollment is much smaller, suggests that the latter is the main driver behind this change in magnitude.

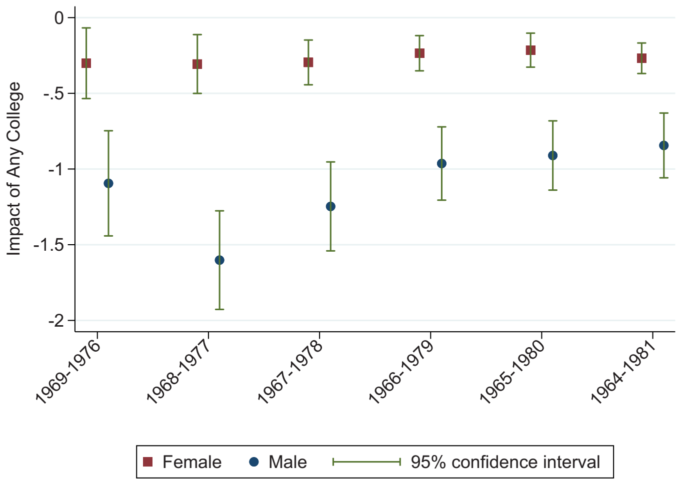
5.3. Validation of the Identifying Assumptions and Robustness Checks

As mentioned in Section 4.3, our identification strategy relies on a *smoothness* assumption. One implication of this assumption is that individuals cannot precisely manipulate the running variable (i.e., no sorting). This condition is likely to be satisfied in our setting because the initial size of all our study cohorts was determined by fertility decisions made many years before the 1973 coup. In support of this claim, Online Appendix Table A.3 shows that there is no kink in cohort size in the 1960 census. We can also expect limited sorting because the age of college enrollment cannot be easily manipulated (i.e., younger cohorts cannot bring forward their secondary completion). The absence of a kink in secondary completion shown in Online Appendix Table A.2 lends support to this claim. The smoothness assumption also implies the absence of kinks in predetermined covariates. Using yearly death counts by cohort for the period 1960–1981 from demographic yearbooks, we show in Online Appendix Table A.3 that the affected cohorts do not exhibit a kink in age-specific mortality before reaching college age.

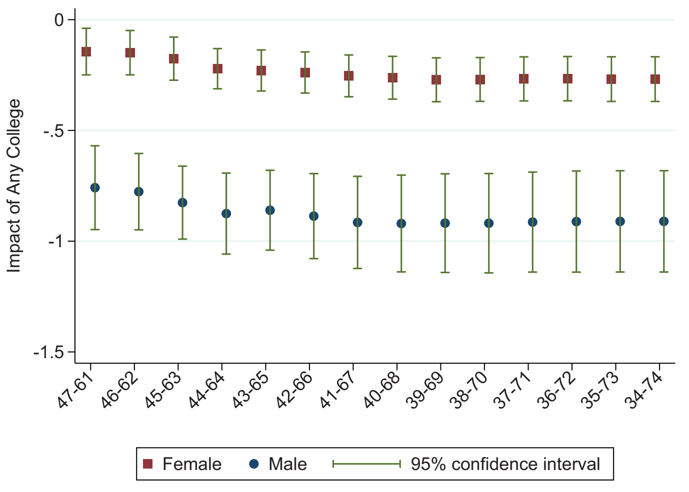
Our identification strategy also requires that the higher mortality in the affected cohorts is only driven by their lower college enrollment (i.e., *exclusion restriction*). While the 1973 coup brought about changes in factors other than access to college, these only compromise the exclusion restriction if they differentially affected younger individuals reaching college age after the coup. Our baseline sample includes 18 contiguous cohorts, who arguably had a comparable exposure to any other changes. Panel (a) in Figure 4 shows that our results are robust to shorter bandwidths of as few as eight cohorts, further enhancing their comparability. The uniqueness of our setting, in which younger cohorts experience a *reduction* in educational attainment, also means that secular improvements in health conditions (e.g., information, technology) will likely attenuate our results.

Additional evidence in support of the exclusion restriction comes from the fact that other relevant outcomes do not exhibit a linear kink after 1973. Online Appendix Figure A.1 shows that economic conditions deteriorated near the coup, but this crisis was short-lived. In fact, the cohorts most affected by the college contraction and exhibiting larger increases in mortality reached college age at a time of rapid economic growth and rising government spending. Online Appendix Figure A.1 also shows that infant mortality and adolescent fertility, which plausibly capture changes in the quality of health care or in major life decisions near college age, evolve smoothly during this period. Using data from the EOD survey, we further show in Online Appendix Table A.4 that there is no kink in youth unemployment in the years after the military coup.[20] Online Appendix Table A.5 verifies that our results are robust to controlling for macroeconomic conditions on the year of college enrollment or

20. Reassuringly, there is a sizable downward kink in the share of EOD respondents with ages 20–30 that report to be currently studying after 1973. We also check that our main results hold if we restrict our analysis sample to the Santiago region covered by EOD.



(a) Cohorts



(b) Ages

FIGURE 4. College enrollment and mortality: alternative bandwidths. Panels show point estimates and 95% confidence intervals of β in equation (5). In panel (a), we consider alternative windows of cohorts. In panel (b), we vary the set of ages at which we allow cohorts to enter the sample. The baseline sample (rightmost estimates) includes people with complete secondary education that reached age 21 between 1964 and 1981, who had ages 34–74 between 1994 and 2017. Dependent variable is the number of deaths per 1,000 individuals, while the endogenous variable is the share with college per ten individuals. The excluded instrument is the interaction of "Cohort trend", a continuous variable indicating the year when the cohort reached 21 years of age (normalized to 0 in 1972) with a dummy for cohorts that reached age 21 on or after 1973. All regressions include region-by-year and age fixed effects. Standard errors clustered by region-year.

restricting the sample to counties lacking a military base in 1970, which were less exposed to repression under Pinochet (Bautista et al. 2023b).[21]

While we cannot fully rule out changes in the quality of higher education after the coup, the evidence on graduation rates and the college earnings premium in Online Appendix Figure A.2 suggests that quality may have increased, which could bias our estimates downwards (i.e., attenuate). In line with this argument, Online Appendix Table A.6 shows that the upward kink in mortality is driven by people with complete secondary that do not go to college. Importantly, Online Appendix Table A.6 also shows that there is no kink in mortality among people with incomplete secondary education, who were ineligible for college but were exposed to other changes. This null result is in line with the negligible impact of the inclusion of people without full secondary on our reduced-form findings in Table 3. Online Appendix Table A.7 lends further support to the exclusion restriction by showing that regions more exposed to the college contraction, as measured either by high rates of secondary completion in 1970 or by the presence of a main university campus, have a larger kink.

Although the exclusion restriction is essentially untestable, this body of evidence suggests that changes in factors other than access to college after 1973 cannot explain our results. Alternatively, at the bottom of Table 3, we report results from sensitivity tests based on Conley, Hansen, and Rossi (2012), which suggest that violations of the exclusion restriction would have to be quite large (70% or more of the reduced-form effects) to change our conclusions.

Another channel through which the 1973 coup may have affected mortality is migration. The analysis at the cohort-year level in Table 3 suggests that internal migration is not affecting our results. Online Appendix Table A.8 further shows that our results are unchanged if we restrict the sample to individuals residing in their region of birth in 1992. Regarding international migration, Online Appendix Figure A.7 documents downward kinks in the share that reports in 1992 to have lived abroad in 1987 and in the share registered to vote outside the country in 2017, as well as an upward kink in the *number* of people per cohort with full secondary in 1992. These patterns suggest lower emigration among the affected cohorts. Official figures for 2003/2004 show that Chilean migrants are positively selected on education (MRE 2005). Hence, the decrease in emigration was plausibly driven by the reduced access to higher education affecting college-age cohorts after 1973.

To assess the influence of international migration on our results, we carry out a bounding exercise in which we construct a counterfactual dataset with fewer individuals in post-coup cohorts (i.e., more emigration) using pre-1973 trends. Based on an average college enrollment rate of 30% in our sample (Table 1), we construct bounds for our estimates by assuming that 10% or 50% of would-be migrants were college educated (i.e., negative or positive selection). We then iteratively adjust the

21. Online Appendix Table A.5 also shows that the results are robust to changes in our econometric specification (i.e., allowing for a discontinuity in 1973 or introducing a quadratic trend), or in the measurement of mortality rates (i.e., not adjusting for previous mortality or smoothing the population counts in the census for age heaping).

number of individuals per cell based on the average mortality rates by educational level (Online Appendix Figure A.4). Online Appendix Table A.8 provides bounds for our IV estimates of [−0.197, −0.179] for women and [−1.503, −0.755] for men. This suggests that emigration is not fundamentally driving our results.

5.4. Heterogeneous Effects by Age and Cause of Death

Our baseline analysis tracks the study cohorts between ages 34 and 74. Panel (b) in Figure 4 shows that our results are robust to smaller age windows that increase the overlap between pre-coup and post-coup cohorts.[22] We observe a slight reduction in effect size for smaller windows, which could indicate that the effect of college on mortality materializes later in the life cycle. To further explore the role of age, we replicate the analysis for the smaller set of cohorts that reached age 21 between 1969 and 1976, which we observe over a larger common support of ages (46–62). We then estimate separate regressions excluding one additional age at a time. Online Appendix Figure A.9 shows that our IV estimates with age fixed effects become smaller for samples that exclude individuals aged 58 or older or that include people aged 52 and below. This suggests that college has a larger effect on mortality after people reach their mid-fifties, in line with evidence by Kaestner, Schiman, and Ward (2020) for the US.

The larger impact of college on mortality for older individuals aligns with additional disaggregate results by cause of death. Online Appendix Table A.9 shows that deaths from cancer and diseases related to the circulatory or digestive systems account for 84% of the total effect of college on female mortality and for 69% of the total effect for men, similarly to what Buckles et al. (2016) find for the US.[23] Relative to the respective sample means, college has a smaller impact on deaths from cancer than on those from diseases of the circulatory or digestive systems. These results are consistent with the notion that environmental or hereditary factors play a larger role in cancer incidence, while diseases of the respiratory or digestive system are more strongly affected by individual behaviors (e.g., exercise). However, these findings must be interpreted with caution because of competing risks across causes of death (Honoré and Lleras-Muney 2006).

6. Mechanisms Linking Higher Education and Mortality

The existing literature has identified many channels linking college and reduced mortality (Cutler and Lleras-Muney 2008, 2010). Following Grossman (2006), we

22. Online Appendix Figure A.8 shows the number of cohorts observed per age, disaggregated by exposure to the college contraction.

23. Online Appendix Table A.10 shows that the reduction in cancer deaths is concentrated in the digestive organs and lymphoid tissue, but also breasts and genital organs in the case of women, and urinary and respiratory organs in the case of men. Men are also less likely to die from external causes, mostly non-traffic accidents and medical complications (Online Appendix Table A.11).

can classify these mechanisms as "market" or "non-market" based. Regarding the former, college education leads to better occupations and higher income (Card 1999), which facilitates access to health insurance, better health care, and different health hazards. Non-market mechanisms include changes in preferences (e.g. risk aversion), beliefs, peer characteristics, and skills that influence health behaviors (Grossman 1972; Becker and Mulligan 1997). In this section, we show that cohorts with lower access to college have worse labor market outcomes and are more likely to be enrolled in the more congested public health system. They are less likely to consume outpatient health services but have similar hospitalization rates. However, they also have higher mortality conditional on being hospitalized.

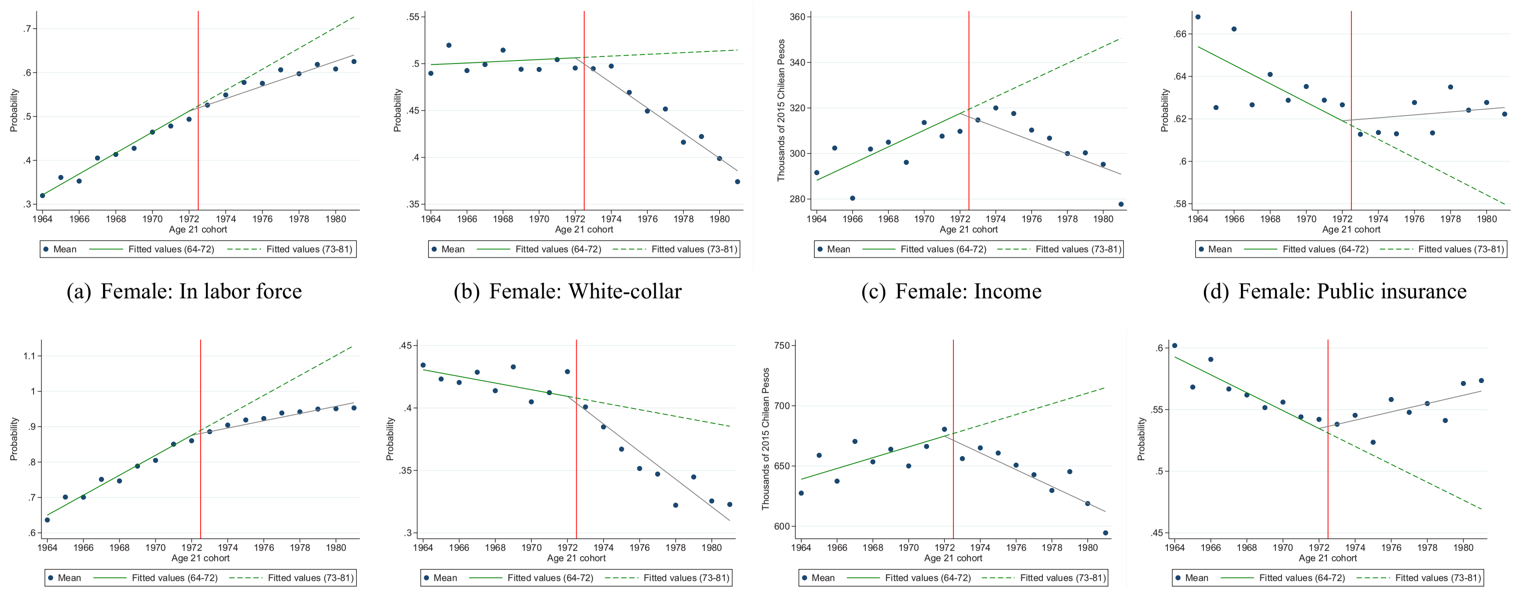
6.1. Labor Market Outcomes

We measure labor market outcomes using individual responses from the CASEN household survey between 1990 and 2017.[24] Since CASEN records information on educational attainment, we can restrict the sample to individuals with complete secondary education and provide IV estimates of the effect of college. Our outcomes of interest are indicators for labor force participation, white-collar high-skill occupation, and enrollment in the public health insurance, as well as monthly income. Averaging across survey waves, Figure 5 shows sizable downward kinks in labor force participation, high-status occupation, and income for the cohorts reaching age 21 after 1973. These kinks are matched by an upward kink in enrollment in the public health insurance.

Table 4 quantifies these kinks and provides IV estimates with age fixed effects. We estimate all regressions at the individual level and include county by year fixed effects that account for a host of spatial and temporal factors. We report standard errors clustered two-way by county and region-year in parentheses (Cameron, Gelbach, and Miller 2011), and *p*-values from the wild cluster bootstrap at the cohort level in brackets. Columns (1) and (5) show that college enrollment increases labor force participation by 14 pp for women and by 21 pp for men. Columns (2) and (6) show that college increases the probability of a white-collar, high-skill occupation (mostly professional occupations—for example, doctor, lawyer—unattainable without a college degree) by 50 pp for women and by 35 pp for men. Columns (3) and (7) show that college increases monthly income by $173,000 (340 USD) for women and by $234,000 (460 USD) for men.[25] These effects correspond to 57% and 36% of the respective sample means. Finally, columns (4) and (8) show that college reduces

24. Information in CASEN is self-reported but we do not expect differential misreporting across cohorts. It also remains unclear whether misreporting varies with education (Cutler and Lleras-Muney 2010).

25. Ffrench-Davis (2018) and Bautista et al. (2023a) show that income inequality increased under Pinochet and that the college contraction hindered social mobility. Higher inequality is associated with worse health outcomes and may have contributed to the increase in mortality (Truesdale and Jencks 2016). Bautista et al. (2023c) further show that children with parents in the affected cohorts are also less likely to attend college, pointing to higher dependency ratios as another potential mechanism.



(a) Female: In labor force (b) Female: White-collar (c) Female: Income (d) Female: Public insurance

(e) Male: In labor force (f) Male: White-collar (g) Male: Income (h) Male: Public insurance

FIGURE 5. Labor market outcomes. Each panel shows cohort averages for the variable in the caption, based on individual responses to the CASEN survey between 1990 and 2017. Panels (a)/(e): labor force participation. Panels (b)/(f): white-collar high-skill occupation. Panels (c)/(g): monthly income, measured in thousands of constant 2015 Chilean pesos. Panels (d)/(h): enrollment in the public health insurance (FONASA). Sample includes all CASEN respondents who reached age 21 between 1964 and 1981 (both inclusive) and report complete secondary education. Solid green line corresponds to the line of best fit for cohorts reaching age 21 before 1973, which we extrapolate for later cohorts (dashed line). Gray line corresponds to line of best fit for cohorts reaching age 21 in 1973 or afterwards.

TABLE 4. Labor market outcomes.

Dependent variable:	Female: In labor force (=1) (1)	Female: White-collar high-skill occupation (=1) (2)	Female: Monthly income (3)	Female: Public health insurance (4)	Male: In labor force (=1) (5)	Male: White-collar high-skill occupation (=1) (6)	Male: Monthly income (7)	Male: Public health insurance (=1) (8)
Panel A: Reduced form								
Cohort trend × After 1973	–0.004**	–0.015***	–4.390***	0.005***	–0.005***	–0.008***	–5.172***	0.009***
	(0.002)	(0.002)	(1.458)	(0.002)	(0.001)	(0.002)	(1.832)	(0.001)
	[0.033]	[0.000]	[0.019]	[0.014]	[0.001]	[0.041]	[0.005]	[0.000]
Panel B: IV								
College enrollment (=1)	0.143**	0.499***	172.673***	–0.200***	0.212***	0.350***	233.941***	–0.410***
	(0.061)	(0.072)	(52.438)	(0.062)	(0.059)	(0.077)	(82.057)	(0.068)
	[0.017]	[0.000]	[0.004]	[0.005]	[0.004]	[0.017]	[0.011]	[0.003]
Year-by-county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,815	39,677	82,815	81,893	80,704	63,978	80,704	79,580
R-squared (panel A)	0.133	0.128	0.129	0.187	0.256	0.139	0.196	0.175
Average dependent variable	0.533	0.452	302.6	0.627	0.874	0.372	645.7	0.555
Kleibergen-Paap F-stat (panel B)	214.0	142.3	214.0	209.2	183.4	153.8	183.4	181.5

Notes: The unit of analysis is an individual respondent in CASEN. Sample includes all survey waves between 1990 and 2017. Sample size varies because not every question is asked every year. Dependent variable in the header: Indicators for labor force participation, white-collar high-skill occupation, and enrollment in public health insurance in columns (1)–(2), (4)–(6), and 8. Monthly income is reported in thousands of 2015 Chilean pesos in columns (3) and (7). Sample includes all CASEN respondents who reached age 21 between 1964 and 1981 (both inclusive) and report complete secondary education. "Cohort trend" is a continuous variable indicating the year when the cohort reached age 21, normalized to 0 in 1972. "Cohort trend × After 1973" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In panel B, this variable is used as excluded instrument for an indicator for college enrollment. Standard errors clustered two-way by county and region-year reported in parentheses. P-value from wild cluster bootstrap by cohort in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

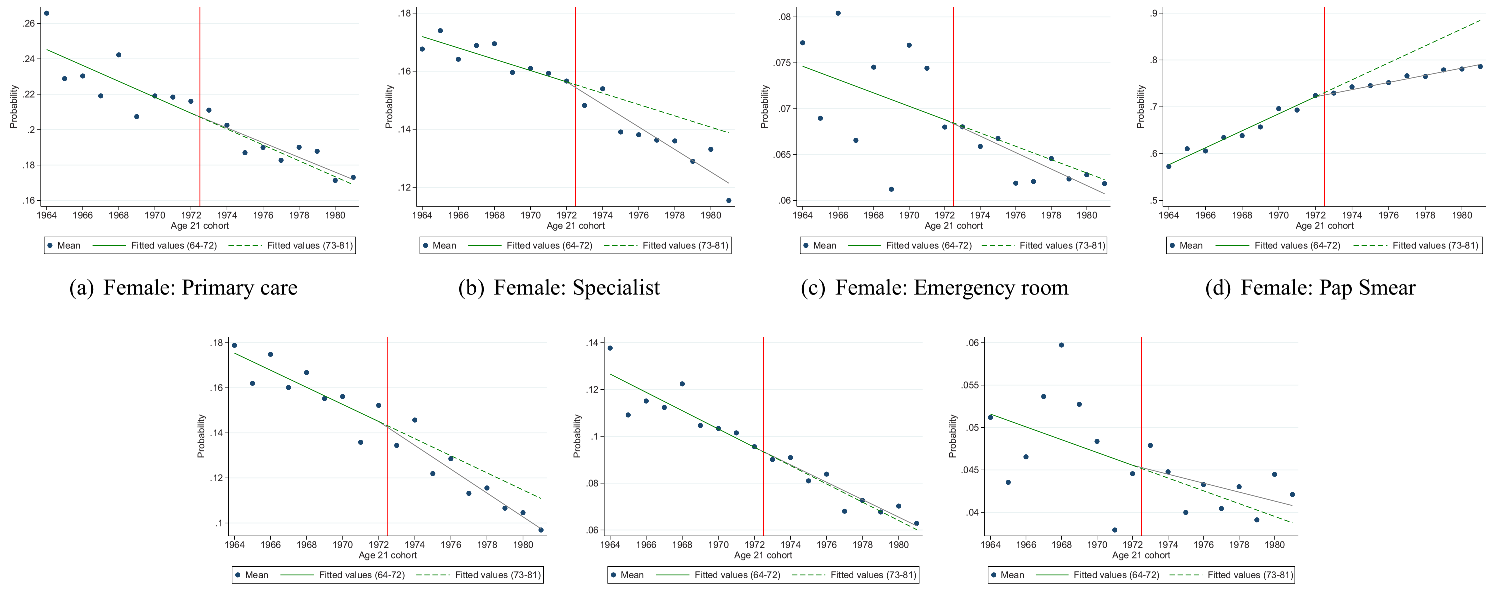
enrollment in the public health insurance by 20 pp for women and by 41 pp for men. The larger effect for men may reflect that women can more easily access private health insurance through their spouse. This difference may partly explain the larger effect of college on male mortality.

6.2. Consumption of Health Services

We measure consumption of health services based on CASEN responses on visits to primary care physicians, specialists, or the emergency room in the past 3 months. The expected effect of college on these outcomes is theoretically ambiguous. On one hand, the cohorts with lower access to college may consume more health services due to their worse health status, as reflected by their higher mortality. On the other hand, economic disadvantage and high reliance on the public health system could limit access to care for these cohorts.

Figure 6 plots the raw data measuring consumption of health services. Women with fewer college opportunities have a lower probability of seeing a specialist (panel b), while men are less likely to see a primary care physician (panel e). There is no clear pattern for emergency room visits (panels c and g), in line with the idea that this service faces fewer barriers to access than outpatient consultations. Table 5 provides the corresponding regression estimates. College increases the probability of having recently seen a primary care physician by 12.8 pp for men, and the probability of having seen a specialist by 10.1 pp for women. These results suggest that more educated individuals make larger investments in health to increase their time available for market and non-market activities (Grossman 1972). They are also consistent with previous research documenting a positive correlation between education and the use of preventive care services (e.g., Fletcher and Frisvold 2009; Cutler and Lleras-Muney 2010; Lange 2011).

As an additional measure of preventive care, column (4) examines the probability that women have had a Pap smear in the past 3 years. This is the main procedure to test for cervical cancer and the Chilean Health Ministry recommends it for all women over the age of 25. Panel (d) in Figure 6 shows a clear downward kink for the affected cohorts, which the regression estimates help to quantify: College enrollment increases the probability of having had this procedure done in a timely fashion by 16.8 pp, equivalent to 23% of the sample mean. This plausibly contributes to the negative impact of college on deaths from female genital cancer documented in Online Appendix Table A.10. Importantly, CASEN asks women who have not had a Pap smear the reason why, which we use to construct additional outcomes in Online Appendix Table A.12. We find that negligence or forgetfulness explains 45% of the college effect, while lack of knowledge or lack of interest explain a further 18%. Although these results are imprecise and should be interpreted with caution, they suggest that college also affects health through non-market channels, including changes in knowledge and preferences.



(a) Female: Primary care (b) Female: Specialist (c) Female: Emergency room (d) Female: Pap Smear

(e) Male: Primary care (f) Male: Specialist (g) Male: Emergency room

FIGURE 6. Consumption of health services. Each panel shows cohort averages for the variable in the caption, based on individual responses to the CASEN survey between 1990 and 2017. Panels (a)/(e): visit to a primary care physician in past 3 months. Panels (b)/(f): visit to a specialist in past 3 months. Panels (c)/(g): visit to emergency room in past 3 months. Panel (d): Pap smear in past 3 years (only women). Sample includes all CASEN respondents who reached age 21 between 1964 and 1981 (both inclusive) and report complete secondary education. Solid green line corresponds to the line of best fit for cohorts reaching age 21 before 1973, which we extrapolate for later cohorts (dashed line). Gray line corresponds to line of best fit for cohorts reaching age 21 in 1973 or afterwards.

TABLE 5. Consumption of health services.

	Female				Male		
Dependent variable:	Primary care visit (=1) (1)	Specialist visit (=1) (2)	Emergency room visit (=1) (3)	Pap smear (=1) (4)	Primary care visit (=1) (5)	Specialist visit (=1) (6)	Emergency room visit (=1) (7)
Panel A: Reduced form							
Cohort trend × After 1973	–0.001	–0.003**	0.001	–0.004**	–0.003**	–0.001	–0.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
	[0.405]	[0.065]	[0.230]	[0.008]	[0.060]	[0.260]	[0.710]
Panel B: IV							
College enrollment (=1)	0.041	0.101**	–0.027	0.168**	0.128**	0.052	0.013
	(0.047)	(0.046)	(0.029)	(0.084)	(0.053)	(0.042)	(0.028)
	[0.406]	[0.040]	[0.236]	[0.004]	[0.055]	[0.254]	[0.709]
Year-by-county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,765	82,801	78,822	58,549	80,671	80,693	76,682
R-squared (panel A)	0.114	0.085	0.067	0.111	0.103	0.083	0.061
Average dependent variable	0.200	0.145	0.067	0.726	0.132	0.088	0.045
Kleibergen-Paap F-stat (panel B)	213.3	214.0	197.6	131.8	183.8	183.7	176.6

Notes: The unit of analysis is an individual respondent in CASEN. Sample includes all survey waves between 1990 and 2017. Sample size varies because not every question is asked every year. Dependent variable in the header: Indicators for visit to a primary care physician in the past 3 months (columns (1) and (5)), visit to a specialist in the past 3 months (columns (2) and (6)), visit to the emergency room in the past 3 months (columns (3) and (7)), or having had a Pap smear in the past 3 years (women only, column (4)). Sample includes all CASEN respondents who reached age 21 between 1964 and 1981 (both inclusive) and report complete secondary education. “Cohort trend” is a continuous variable indicating the year when the cohort reached age 21, normalized to 0 in 1972. “Cohort trend × After 1973” is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In panel B, this variable is used as excluded instrument for an indicator for college enrollment. Standard errors clustered two-way by county and region-year reported in parentheses. P-value from wild cluster bootstrap by cohort in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6.3. Hospitalizations

We study hospital visits as a complementary measure of health care consumption, based on the universe of discharge summaries between 2002 and 2018. We conduct this part of the analysis at the cohort-gender-region-year level and source the initial population counts from the 2002 census.[26] Unfortunately, the discharge summaries do not provide information on educational attainment, so we drop the sample restriction to individuals with complete secondary and the analysis is exclusively reduced form. We then study the mortality of hospitalized patients by combining the discharge summaries with the vital statistics at the individual level.

Panels (a) and (b) in Figure 7 show small upward kinks in the hospitalization rate (per 1,000 individuals) for the cohorts exposed to the college contraction. However, columns (1) and (2) in Table 6 show no evidence of kinks once we account for age effects, irrespective of whether we measure hospitalization events or individual patients.[27] This null result suggests that limited access to college leads to worse health while also limiting access to medical care, such that these two effects tend to cancel out. To explore this possibility, columns (3) and (4) provide estimates by type of insurance. We find evidence of an upward kink in the hospitalization rate using public insurance and a comparable downward kink in the hospitalization rate using private insurance. These results are consistent with the notion that increased reliance on the public health system is a contributing factor in the limited access to care faced by the affected cohorts. We interpret these results with caution because their statistical significance is sensitive to the type of clustering technique.

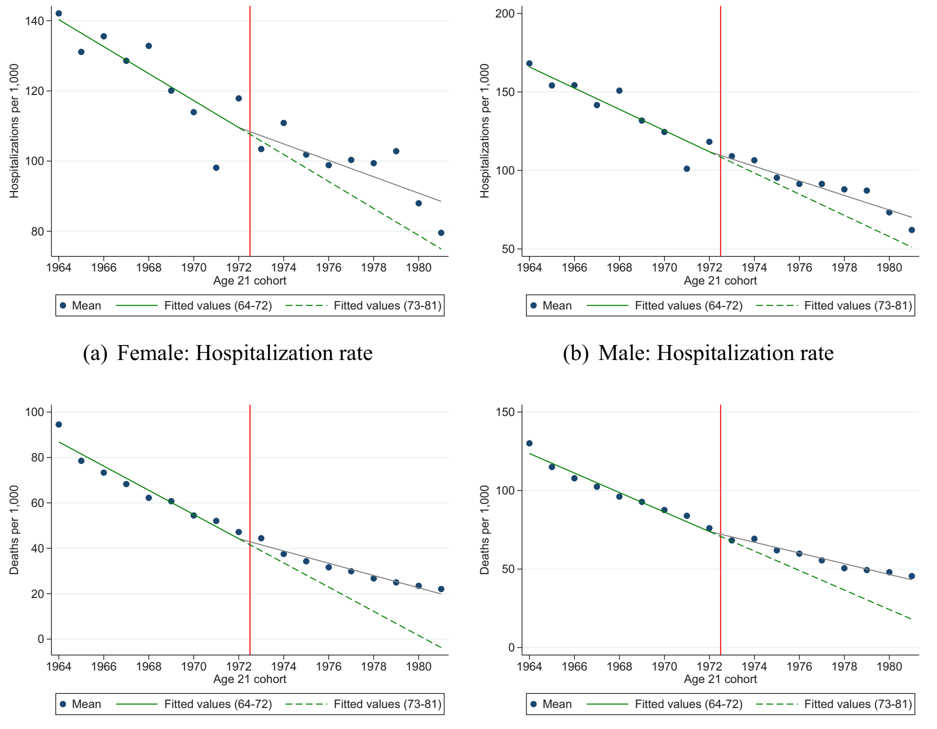
By combining the discharge summaries with the vital statistics, we can establish whether a hospitalized individual dies over different time horizons. Our final estimating sample includes roughly 1.1 million individuals that were admitted to hospital between 2004 and 2012.[28] Our reduced-form specification includes county-by-year and age fixed effects. We report in parentheses standard errors clustered two-way by county and region-year (Cameron, Gelbach, and Miller 2011), and *p*-values from the wild cluster bootstrap at the cohort level in brackets.

Column (5) in Table 6 presents results using the 1-year mortality rate of patients hospitalized in 2004–2012 as dependent variable. The mean of this variable is 45 (per 1,000 patients) for women and 77 for men. These mortality rates are one order of magnitude larger than the averages from our main sample in Table 3 and indicate that hospitalized patients have much higher mortality than the population at large. The

26. Online Appendix Table A.5 shows that our main results hold for the 2002 census and the sample period 2003–2018.

27. First admissions per patient years represent 67% of total admissions and within-year re-admissions correspond to 25% of the total. The remaining 8% of admissions do not have an individual identifier.

28. We study mortality starting in 2004 because important covariates are missing in the discharges for 2002–2003. We finish in 2012 because we study mortality up to 5 years after a discharge and the mortality data ends in 2017.



(a) Female: Hospitalization rate

(b) Male: Hospitalization rate

(c) Female: 1-year mortality rate

(d) Male: 1-year mortality rate

FIGURE 7. Hospitalization rates and mortality rates of hospitalized patients. Each panel shows cohort averages for the variable in the caption. Panels (a)/(b): hospitalizations per 1,000 individuals. Panels (c)/(d): Deaths within 1 year of discharge per 1,000 admitted patients. The sample period is 2002–2018 (ages 44–75) in panels (a)/(b), and 2004–2012 (ages 46–69) in panels (c)/(d). Solid green line corresponds to the line of best fit for cohorts reaching age 21 before 1973, which we extrapolate for later cohorts (dashed line). Gray line corresponds to line of best fit for cohorts reaching age 21 in 1973 or afterwards.

estimates reveal an upward kink of 1.7 deaths for women in the affected cohorts (panel A) and 2.1 deaths for men (panel B). These per-cohort increases in mortality due to reduced access to college correspond to 3.7% and 2.8% of the respective sample means. Panels (c) and (d) in Figure 7 provide visual evidence of this kink.

While we focus on a 1-year horizon to maximize the comparability of these estimates with our main results, Online Appendix Table A.13 shows that the upward kink in mortality becomes larger for longer time horizons of up to 5 years. Online Appendix Figure A.10 further shows that accounting for a rich set of observable characteristics in the discharge summaries (diagnostic, hospital, type of insurance, and admission, indicators for surgery and previous admission in 2002–2003) explains as much as 45% of the kink in mortality for patients in the cohorts with reduced access to college.

TABLE 6. Hospitalization rate and patient mortality.

Dependent variable	Hospital admissions per 1,000 All (1)	Individuals (2)	Public insurance (3)	Private insurance (4)	1-year mortality per 1,000 patients (5)
Panel A: Female					
Cohort trend × After 1973	−0.124	−0.090	0.501***	−0.725***	1.719***
	(0.135)	(0.090)	(0.187)	(0.091)	(0.404)
	[0.942]	[0.928]	[0.690]	[0.140]	[0.002]
Panel B: Male					
Cohort trend × After 1973	0.247	−0.007	1.041***	−0.807***	2.148***
	(0.192)	(0.113)	(0.261)	(0.119)	(0.364)
	[0.891]	[0.996]	[0.495]	[0.131]	[0.000]
Year-by-region fixed effects	Yes	Yes	Yes	Yes	No
Year-by-county fixed effects	No	No	No	No	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes
Observations (panel A)	4,590	4,590	4,590	4,590	611,798
Observations (panel B)	4,590	4,590	4,590	4,590	525,819
R-squared (panel A)	0.851	0.887	0.903	0.902	0.019
R-squared (panel B)	0.927	0.940	0.933	0.904	0.019
Average dependent variable (panel A)	107.4	72.86	77.04	21.70	45.27
Average dependent variable (panel B)	106.2	70.11	74.15	22.48	76.95

Notes: The unit of analysis is cohort-region-year in columns (1)–(4) and a hospitalized patient in column (5). Observations in columns (1)–(4) are weighted by cell size. Dependent variable in the header (all per 1,000): number of hospitalizations in column (1), number of hospitalized individuals in column (2), and number of deaths within 1 year of discharge among hospitalized patients in column (5). We disaggregate hospitalizations into those using public insurance (FONASA) in column (3) and hospitalizations using private insurance (ISAPRE, other private, Armed Forces) in column (4). Hospitalization rates are adjusted for previous mortality within cell. Hospitalization rates are based on the number of respondents in the 2002 census who reached age 21 between 1964 and 1981 (both inclusive). The sample period is 2002–2018 (ages 44–75) in columns (1)–(4) and 2004–2012 (ages 46–69) in column (5). "Cohort trend" is a continuous variable indicating the year when the cohort reached age 21, normalized to 0 in 1972. "Cohort trend × After 1973" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. Standard errors clustered by region-year (two-way clustered also by county in column (5)) reported in parentheses. P-value from wild cluster bootstrap by cohort in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7. Conclusion

We provide novel evidence on the causal effect of higher education on mortality. We document an upward kink in the mortality rate between ages 34 and 74 among Chilean cohorts that reached college age shortly after the 1973 military coup. These cohorts experienced reduced access to higher education and exhibit a downward kink in college enrollment. They also have worse labor market outcomes, are more reliant on the public health system, and consume health services at lower rates, including outpatient consultations and preventive care procedures. These additional results suggest that economic disadvantage and limited access to care play an important mediating role in the causal link between higher education and mortality. Unfortunately, data limitations prevent us from further exploring the underlying mechanisms. This is an exciting avenue for future work.

Our results contribute to the literature on the non-pecuniary effects of education and indicate the presence of a sizable health return to college (Grossman 2006; Lochner 2011; Oreopoulos and Salvanes 2011). These findings stand in contrast to previous work that has largely struggled to find evidence of a causal effect of education on mortality (Galama, Lleras-Muney, and van Kippersluis 2018; Xue, Cheng, and Zhang 2021). Existing research has mostly exploited changes in compulsory schooling laws that take place at lower levels and cause relatively small increases in educational attainment. In our setting, reduced access to higher education has life-changing socioeconomic consequences. The large impact of college enrollment on labor market outcomes and income relative to one additional year of primary or secondary may plausibly explain the positive impact of education on health that we uncover.

Another important difference with respect to previous work is that compulsory schooling laws induce students to acquire additional education that they would have otherwise forgone. Hence, the compliers affected by these policies are likely to exert low effort and to have low returns (Lleras-Muney 2022). Moreover, responsibility over educational decisions predominantly falls on parents at lower levels, such that the compliers in these settings plausibly correspond to families that place a low value on the human capital accumulation of their children (perhaps due to pressing economic needs) and provide limited complementary parental inputs. Delegation of educational decisions to parents may also distort human capital accumulation in the presence of imperfect information about individual ability. In contrast, the compliers in our setting had greater agency over their educational decisions and wanted to attend college—as reflected by the ratio of applications to openings—but failed to gain admission because of the reduction in college openings. These applicants predominantly came from less affluent families but had high enough ability to be close to the test score cut-off determining admission. Our findings show that not being able to attend college proved very costly for their economic and health outcomes.

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Supplementary Material

Supplementary data are available at *JEEA* online.

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