Chile's Missing Students: Dictatorship, Higher Education and Social Mobility*

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Abstract

Hostile policies towards higher education are a prominent feature of authoritarian regimes. We study the capture of higher education by the military dictatorship of Augusto Pinochet in Chile following the 1973 coup. We find three main results: (i) Cohorts that reached college age shortly after the coup experienced a large drop in college enrolment, mostly as a result of the persistent reduction in the number of openings for incoming students decreed by the regime; (ii) these cohorts had worse economic outcomes throughout the life cycle and struggled to climb up the socioeconomic ladder, especially women; (iii) children with parents in the affected cohorts have higher dropout rates in secondary and a lower probability of college enrolment. These results demonstrate that the political capture of higher education in non-democracies hinders social mobility and leads to a persistent reduction in human capital accumulation, even after democratization.

Keywords: Dictatorship, higher education, social mobility, intergenerational transmission
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1 Introduction

Institutions of higher education are typically devoted to critical enquiry and uncompromising debate. As the *Magna Charta Universitatum* states in its fundamental principles, "freedom in research and training is the fundamental principle of university life" (OMCU, 2019).[1] Faculty and students often engage in political discussion, denunciation and mobilization, making universities a thorn in the flesh for governments of all types (Glaeser et al., 2007). While democracies often accommodate the demands of protest movements originating in universities (Maurin and McNally, 2008), autocracies usually respond with repression(Connelly and Grüttner, 2005). One well-documented example involves the dismissal of Jewish faculty and students in Nazi Germany (Waldinger, 2010, 2011). Other examples include Soviet repression against the student-led 'Prague Spring' in 1968, the student massacres in Mexico (Tlatelolco) and China (Tiananmen) in 1968 and 1989, and the arrests and disappearances of students in Iran in 1999. The shutdown of Central European University by Hungarian strongman Viktor Orban in 2018 provides more recent evidence (New York Times, 2018). Figure 1 provides more systematic evidence on the inherent tension between higher education and authoritarian governments. It shows a strong, negative relationship between a country's average level of autocracy and its tertiary enrolment rate over a 45-year period.

Such repression is likely to have long-lasting economic consequences, as universities have been shown to foster economic activity in earlier centuries and in more recent times (Cantoni and Yuchtman, 2014; Valero and Van Reenen, 2019). In this regard, hostile policies towards higher education are a plausible but understudied mechanism connecting political regimes and economic prosperity.[2] A related question concerns the relationship between democracy and inequality. A theoretical literature dating back to Meltzer and Richard (1981) posits a strong equalizing effect of democracy, but the empirical findings are quite mixed (Acemoglu et al., 2015). The possibility that educational repression fosters inequality within non-democracies has seldom been considered before. From a complementary angle, evidence on the relationship between higher education and social mobility is limited and comes mostly from rich, established democracies (e.g., Chetty et al., 2017).

In this paper, we study the effects of the hostile policies towards higher education implemented by the military dictatorship in Chile following the coup of 1973. Shortly after the coup, the ruling military junta, led by Augusto Pinochet, placed members of the armed forces at the top of all

[1] Emperor Frederick I signed on 1155 the *Authentica Habita* granting scholars at the University of Bologna protection from persecution. Commemorating its 900th anniversary in 1988, the university released the *Magna Charta Universitatum*, which had been signed by almost 1,000 universities from around the world as of 2019.

[2] Previous work on the link between democracy and economic growth has mostly relied on cross-country comparisons and has struggled to find credible sources of identification. Prominent contributions include Barro (1996), Przeworski et al. (2000), Doucouliagos and Ulubaşoğlu (2008), Papaioannou and Siourounis (2008) and Acemoglu et al. (2019).

universities in the country, public and private. As part of the regime's attempt to eliminate any source of political opposition, faculty and students suspicious of supporting the deposed left-wing government were routinely dismissed, but in some cases also arrested, tortured, or killed. Severe restrictions on student life and curricula were also implemented. However, the most quantitatively-important component of these measures was the systematic reduction in the number of openings for incoming students that took place over the following several years.

We examine the effects of these policies on affected individuals that reached college age shortly after the coup along several margins: (i) human capital accumulation, (ii) economic and non-economic outcomes, (iii) intergenerational transmission of human capital. Our empirical strategy relies on comparing birth cohorts that reached college age in a narrow window around the time of the military coup in 1973, in the spirit of a regression kink design (Card et al., 2015). In particular, we use the observed trend in the outcomes for cohorts that reached college age shortly before the coup as a counterfactual for the affected cohorts that did so shortly afterwards, under the identifying assumption that in the absence of the coup we should not observe systematic breaks or kinks. Most of the analysis uses individual-level census data from 1992, 2002 and 2017, which we complement with data from a large household survey (CASEN) that is collected roughly every two years in the period 1990-2015.

Naturally, a comparison of members of different cohorts at any single point in time may be confounded by non-linear age effects. We employ multiple strategies to address this problem. First, we exploit the availability of information from multiple years to document the presence of kinks in our outcomes of interest at various different points in the life cycle. Second, we verify that our results are robust to the use of increasingly conservative bandwidths of cohorts reaching college age around the military coup, which are arguably more comparable. Third, when using the relatively high-frequency CASEN survey we estimate a more stringent specification that replaces the pre-coup trend with age fixed effects, thereby allowing the outcome to vary flexibly at each single point in the life cycle.

In the first part of the paper, we study the effect of the incoming dictatorship on the educational attainment of the affected cohorts and provide evidence on the underlying mechanism. We restrict the analysis to high school graduates to have a more relevant counterfactual for college entry and to shut down the potentially confounding effect of disruption at lower levels of education in the aftermath of the coup. We document a sharp kink in college entry and completion that coincides with the military coup. We find that the cohorts reaching age twenty-one between 1964 and 1972 were making yearly gains of 0.5 percentage points (pp) in college enrolment. This trend reverses for the cohorts reaching the same age between 1973 and 1981, who experienced a net yearly decline in the enrolment rate of 1.4 pp. While 38% of people with full secondary that reached age 21 in 1972 went to college, only 25% of those reaching this age in 1981 did. This pattern is present in all data sources available. We find evidence of mild substitution of college education

with technical school. A complementary analysis using different sets of countries to construct a synthetic control for college completion rates in Chile provides further evidence of a sizable gap in tertiary education after the coup.

Using administrative data on the yearly number of college applicants and the number of openings for incoming students, we show that demand was twice as large as supply throughout the sample period. Hence, it was the reduction in the number of openings decreed by the military regime that caused the drop in college entry. The reduction in openings affected all fields except the natural sciences, suggesting a broad effect on most college-age individuals. University admissions in Chile have operated through a deferred-acceptance algorithm since 1967, whereby students are ranked based on their grades in secondary and their performance on a centralized admissions exam. Hence, even though the military regime was able to restrict the supply of higher education, it could not perfectly target the identity of admitted students. As a result, we find that the kink in college entry is present within families as well as within all quintiles of housing wealth, providing further evidence of broad effects. We additionally find that the college premium increases for the affected cohorts. This is consistent with a reduction in the supply of college graduates and suggests that the quality of education did not dramatically decrease after the coup. It is also consistent with the fact that the set of compliers that failed to attend university were marginal students (with lower ability) that were barely making the admissions cut-off before the reduction in openings. The increase in the college premium remains once we control for occupation, indicating that it is not driven by increased targeting of programs more prone to political activism and having arguably lower returns (i.e. social sciences).

In the second part of the paper, we examine the economic consequences of decreased access to higher education. We provide reduced-form estimates of trend-breaks around the time of the military coup and instrumental variables (IV) estimates of the effect of college entry, using the break in the enrolment trend after 1973 as the excluded instrument. The IV estimates capture the average effect of college entry for the set of compliers whose college enrolment was affected by the changes in policy implemented by the military dictatorship. The implied exclusion restriction requires that the change in outcomes for the cohorts that reached college age shortly after the coup is entirely driven by the restricted access to university.

We find large kinks in labor force participation and unemployment throughout the life cycle. Affected cohorts were substantially less likely to be in the labor force during their prime working years in 1992 (late 30s) and 2002 (late 40s). These effects are 50-100% larger for women than for men, indicating that college enrollment was fundamental for female participation in the labor market (Goldin, 2006). Conditional on labor force participation, affected cohorts also had higher rates of unemployment. Access to university also affects occupation along several dimensions. Affected cohorts are less likely to be in salaried employment, with half of the effect coming from increased domestic work and unpaid work with relatives (the other half from business ownership).

This effect is almost entirely driven by women. People in the affected cohorts are also much less likely to have a high-skill, white-collar occupation. This effect is at least 50% larger for women than for men.

Using information from the CASEN survey between 1990 and 2015, we also find a sharp kink in various measures of reported income. The IV estimate of the effect of college entry on log total self-generated income (i.e. excluding government transfers) is 0.85, which comes down to 0.20 in the more conservative specification with age fixed effects. Using information on the distribution of wealth from the 1992 census, we find that college enrollment increases the probability of being in the top quintile by 35 percentage points, relative to a mean of 50% in our sample of high school graduates. This effect is balanced by roughly equal decreases in the probability of being in each of the second, third and fourth quintiles, as well as a smaller but precisely measured decrease in the probability of being in the lowest quintile. These results tell us that restricted access to higher education generated a dramatic impediment to social mobility for the affected cohorts. Results using income quintiles from the CASEN survey paint a similar picture.

We further find that people in the affected cohorts are significantly less likely to be household heads (or spouses of the head) in each of 1992, 2002 and 2017. While this could be explained by the fact the these are younger cohorts, we find that not only are they more likely to report being children of the household head in all three censuses, but also that they are more likely to report being parents of the household head. We interpret these results as further evidence of economic vulnerability. Additionally, members of the affected cohorts are more likely to report being widows (conditional on having ever been married), which suggests a negative relationship between college enrollment and mortality (Buckles et al., 2016).

The final part of the analysis examines whether the drop in educational attainment for the affected cohorts affects the human capital of the next generation. We first show that women in the affected cohorts report having more children. The IV estimates for college enrollment in 2002 and 2017, when fertility is more likely to have been completed are -0.51 and -0.64 respectively. Additionally, women in the affected cohorts report having a smaller share of their children still alive. This effect is present as early as 1992, suggesting it is driven by child deaths in early life.

We then examine whether the drop in parental college enrollment affects the educational attainment of the next generation. We connect parents that finished high school to their children using various combinations of positions in the household in the 2017 census. Most of our sample is made up of individuals that report being children of the household head or of the spouse. We also include household heads, their spouses or their siblings if their parents live with them. Our preferred specification includes (i) county of birth x gender, (ii) parent's gender x gender, (iii) relationship to household head, and (iv) age fixed effects. We estimate that having a parent that enrolled in college increases an individual's own probability of doing so by 32 pp. This effect is equivalent to 55% of the sample mean of 58% for children of high school graduates. Looking at lower levels, we find

that parental college enrolment has no effect on primary education (which is mandatory in Chile), but does reduce dropout at all levels of secondary education. However, high-school dropout only explains 12% of the intergenerational effect on college enrollment. Additional exercises indicate that positive assortative matching and higher maternal age of college students contribute to the intergenerational transmission of higher education.

This paper connects several strands of literature. First, it adds to the empirical literature studying the relationship between democracy, education and inequality. This literature has been largely motivated by the theoretical prediction of a positive relationship between democracy and redistribution (Boix, 2003; Acemoglu and Robinson, 2006). Several papers (relying mostly on country-level data) have documented a positive effect of democracy on educational spending and enrolment at the primary and secondary levels.[3] Higher education has received much less attention and the available evidence actually points to a null effect (Stasavage, 2005; Gallego, 2010). A separate line of work (also reliant on cross-country comparisons) has provided highly inconclusive results on the relationship between democracy and inequality.[4] We make two contributions. First, we provide within-country evidence of a negative effect of dictatorship on the provision of higher education. In our setting, this is a response to the perceived political threat that free universities represent, in line with the predictions of Glaeser et al. (2007).[5] Second, we provide micro evidence showing that the hostile policies towards higher education that are a hallmark of authoritarian regimes hinder social mobility and female progress in the labor market, plausibly contributing to persistent income and gender inequality (Simpser et al., 2018). Our findings bring to light the dark side of the so-called 'Chilean miracle' and help explain the growing levels of social unrest and political protest seen in the last decade.

Second, our paper also adds to an extensive literature on the effects of higher education. More specifically, our work contributes to research on: i) the monetary and non-monetary returns to education (e.g., Card, 1999; Oreopoulos and Salvanes, 2011), ii) the effects of higher education on social mobility (e.g., Torche, 2011; Chetty et al., 2017), and iii) the differential effects of higher education on outcomes for women (e.g., Goldin, 1992, 2006). Particularly related is work by Zimmerman (2019) showing that attending an elite business-oriented program in Chile has large effects on the probability of attaining top jobs and top incomes, but that these effects are concentrated in men graduating from elite high schools. In contrast to these findings, we show that college entry systematically affects economic and non-economic outcomes throughout the life cycle and dra-

[3] See Brown (1999); Baum and Lake (2003); Brown and Hunter (2004); Lindert (2004); Avelino et al. (2005); Ansell (2010); Harding and Stasavage (2013). Aghion et al. (2018) provide opposite findings.

[4] See Rodrik (1999); Li et al. (2001); Mulligan et al. (2004); Scheve and Stasavage (2009, 2017); Haggard and Kaufman (2012); Acemoglu et al. (2015).

[5] A related strand of literature has focused on the manipulation of educational content in autocracies to generate political subservience (Cantoni et al., 2017; Alesina et al., 2018).

matically affects a person's chances of climbing up the socioeconomic ladder.[6] We further show that women were particularly vulnerable to disruption in higher education at a time of structural transformation and female empowerment in the labor market (Goldin, 2006).

The paper also contributes to the literature on the intergenerational transmission of human capital (see Black and Devereux, 2011 or Björklund and Salvanes, 2011 for overviews). Previous research has largely focused either on primary or secondary levels of parental education, often exploiting quasi-random variation in mandatory schooling requirements faced by parents (e.g, Black et al., 2005; Oreopoulos et al., 2006). A few studies have analyzed the relationship between college attainment of parents and early-life outcomes or lower levels of educational attainment in the next generation (e.g., Currie and Moretti, 2003; Maurin and McNally, 2008). But little is known about the causal link between the college enrollment of parents and children.[7] The novelty of our results relates to the unique features of the decision to go to college (increased agency of the student, limited supply, higher cost and foregone earnings, credit constraints), which set it apart from other critical junctures in the process of human capital accumulation. These features make it increasingly likely that variation in family background underlies the intergenerational correlation in college enrollment (Holmlund et al., 2011). We push the literature forward by providing evidence of a positive causal link in intergenerational college enrollment. More generally, we also complement a literature that has largely focused on a handful of developed countries by studying the intergenerational transmission of higher education in a developing-country setting.

2 Historical Background

2.1 Higher Education in Chile Before the Military Coup

There were eight universities in Chile when Salvador Allende took office in 1970 and this number would remain unchanged until the large educational reform implemented by the military government in 1981. The oldest university was Universidad de Chile, founded in 1842, and the most recent one to open was Universidad del Norte, founded in 1956. Formally, only two universities were public (i.e. run by the government), Universidad de Chile and Universidad Técnica del Estado, but the whole university system was mostly financed with government funds. Most universities had their main campuses in the larger cities of Santiago, Concepción and Valparaiso, and several had smaller campuses distributed throughout all the regions of the country. In 1967, 86%

[6] Similarly to us, a few other studies exploit episodes of political disruption to higher education to study the relationship between college enrollment and economic outcomes (Maurin and McNally, 2008; Li and Meng, 2018; Ozturk and Tumen, 2018). These studies largely rely on a single cross-section and do not explore persistent effects throughout the life cycle. They also do not directly examine social mobility or gender inequality.

[7] Suhonen and Karhunen (2019) find that the children of parents that benefited from the geographic expansion of the Finnish university system are more likely to have a higher tertiary degree (i.e. master's).

of faculty had a college degree, 8% had a master's degree and only 3% had a PhD. About a third of faculty had full-time appointments (Brunner, 1984).

College enrolment quickly expanded in the mid-20th century, growing from around 8,000 students in 1940 to 25,000 in 1960 and to 77,000 in 1970. Figure 2 shows that the gross enrolment rate jumped from 4.6% to 9.2% during the administration of Eduardo Frei between 1964 and 1970. The Socialist government of Salvador Allende (1970-1973) would oversee an even more dramatic increase in enrolment, which reached 146,000 students in 1973, corresponding to a gross enrolment rate of 16.8%. In this year, 39% of college students were female and 67% of students were enrolled in public universities.[8] In 1967, education programs held the largest share of students (29%), followed by engineering with 22%, social sciences with 15% and health with 13% (see Appendix Figure A1). Other fields had less than 6% of students each. These numbers were very similar in 1973, except that engineering had become the largest field with 30% of students.

A movement for broad educational reform began in 1965 under the center-left government of Eduardo Frei from the Christian Democratic Party. At the university level, the reform started in 1967, partly in response to gains in political leverage made by the student movement in the previous years. Its main achievements included the large increase in enrolment and greater participation of students and faculty in university governance. Academic structures were modernized, in an effort to resemble the U.S. model, and increased funding allowed for new programs and research centers. During the Allende government, there was a deliberate effort to make access to university more inclusive, with mixed results (Castro, 1977; Schiefelbein and Farrell, 1984, 1985).

From 1850 until 1966, students wishing to enroll in college had to take a baccalaureate exam administered by Universidad de Chile. As part of the educational reforms, this test was replaced by a new one called "Prueba de Aptitud Académica" (Academic Aptitude Test) in 1967. The PAA test had mandatory modules in language and mathematics, as well as optional modules in other subjects. For admission purposes, each student provided a ranking of their preferred program-university combinations. Each student was also awarded a score equal to a weighted average of her grades in secondary and her results in the different components of the PAA test. The weight awarded to each component was determined by each university and could vary by program. Given the number of openings offered by each university in each of its programs, a deferred-acceptance algorithm allocated students to their higher-ranked options until all openings were filled (Koljatic and Silva, 2020). Students could take the PAA test multiple times, but there was only one sitting of the exam per year. Leaving aside some small modifications, the system remains largely unchanged until today.

[8] Brunner (1984) shows that this large increase in enrolment was similar to that seen in several other countries in Latin America during the same time period.

2.2 *Higher Education in Chile After the Military Coup*

As mentioned above, the process of expansion and democratization of the university system accelerated during the government of Salvador Allende starting in 1970. Amid growing political polarization and deteriorating economic conditions, a military coup led by General Augusto Pinochet overthrew Allende on September 11, 1973. A military junta presided by Pinochet assumed all executive and legislative powers and would go on to govern the country until 1990.

In its early days, the military government made it clear that one of its aims was to eliminate all sources of support for left-leaning political views. Universities were immediately targeted and intervened. Only two weeks after the coup, the junta put members of the armed forces at the head of all universities, both public and private. These military delegates had unrestricted power over university governance (Brunner, 1984; Castro, 1977). During the first months after the coup, many students and members of faculty and staff were expelled or fired, though the exact magnitude remains unclear.[9] Some university affiliates with undesirable political views in the eyes of the regime were detained, tortured, exiled or killed.[10] All student groups and teacher unions were monitored and any political activity was forbidden. Teaching materials were tightly controlled.

In the years immediately after the coup, the gains in college enrollment achieved during the period of educational reform were largely undone. Figure 2 shows a steady decline in the gross enrollment rate starting in 1974. By 1980, the enrollment rate was slightly above 10% and was not much different from what it had been at the start of the Allende years. Despite the dismissals and purges, most of the previously-enrolled students remained enrolled (Levy, 1986). Hence, the decline in enrolment was driven by variation in the yearly number of incoming students. To disentangle the role of demand and supply, we use administrative records on the number of people taking the PAA test each year since 1967, the subset of people that effectively apply to college and the total number of openings made available by the universities.[11]

Panel (a) in Figure 3 plots the three series. The data on openings shows positive growth in the late 1960s, followed by a dramatic expansion during the Allende government and a steady

[9] Brunner (1984) cites a study claiming that the total number of expelled students was 20,000 and that at least 25% of faculty had been dismissed by 1984. Castro (1977) claims that 7,000 students had been expelled only from the University of Concepción by 1974. He mentions that 228 researchers in the natural sciences left Chile between 1971 and June 1974, 165 of whom left after the military coup.

[10] Using detailed individual records from the final report by the "National Commission for Truth and Reconciliation", we find that of the roughly 3,200 documented victims 24 were university professors and 252 were university students. The report by the subsequent "National Commission on Political Imprisonment and Torture" estimates that around 4,100 of the 38,000 subjects who experienced such human-rights violations were students from different levels (Comisión Valech, 2004; Comisión Rettig, 1996)

[11] Appendix Figure A2 considers two additional response margins. Panel (a) plots data on conscription, obtained using a Freedom-of-Information request, and shows that the number of enlisted soldiers if anything decreased after the coup. Panel (b) shows that the number of Chilean students abroad increases roughly until the year of the coup and remains relatively flat afterwards. In both cases, the variation is too small to explain the observed drop in college enrollment.

decline and stagnation after the military coup. More specifically, the number of openings grew 30% between 1967 and 1970, jumping from 16,000 to 20,000, and 130% between 1970 and 1973, when it reached its maximum of 47,214. It steadily declined after the coup, dropping to 32,954 by 1980 (a 30% drop relative to 1973). On the other hand, the number of test-takers and applicants steadily increased between 1967 and 1975, when both series exceeded 100,000. Both series similarly drop in the following years, although the number of test-takers picks back up in the last years of the sample period. The graph clearly shows that the number of applicants always exceeds the number of openings and was usually twice as large or more. This tells us that supply was the binding constraint for entry at all points in time. The lagged adjustment in demand suggests that the drop in openings was unexpected. In this regard, projections by UNESCO placed aggregate enrollment at around 200,000 for 1975 while the actual figure fell slightly short of 150,000 (Levy, 1986).

Panel (b) in Figure 3 shows that the drop in openings was larger in public universities, which had experienced faster growth in the years before the coup, though supply in private universities also stopped growing and weakly decreased. This is consistent with government control over all universities. Panel (c) shows the change in openings between 1973 and 1980 by field of study. Programs in agriculture and the social sciences were the ones that were worst affected, experiencing aggregate declines of around 50%, consistent with political targeting. However, all fields with the exception of the natural sciences saw a net decrease in the number of openings in the years after the coup. These include traditional fields such as health or law.

Regarding demand, panel (d) shows that average PAA test scores in the compulsory modules decreased in the years immediately before the coup and improved in the years after it. This suggests that the students driving the aggregate variation in test-taking observed in panel (a) are marginal students with relatively low performance and low chances of entry. Due to the deferred-acceptance algorithm used, we expect that the compliers that fail to gain entry to university when the openings are reduced are also marginal students that would have barely made the cut-off before. We explore this possibility further when we estimate the returns to college below.

2.3 Higher Education after 1981

In 1981 the military government embarked on a program of large reform of higher education. A major feature of the reform was the reorganization of the entire higher education system. The existing set of eight universities was segmented and many new universities were created. This change mostly affected the regional branches of Universidad de Chile, while the other public university (Universidad Técnica del Estado) disappeared and was replaced by seven new ones. The original universities and those that were created from them belong to an elite group known as CRUCH, which dates back to 1964.[12] Eventually, new private universities not belonging to

[12]CRUCH is the Spanish acronym fro Consejo de Rectores de las Universidades Chilenas, which translates to "Council of the Rectors of Chilean Universities"

CRUCH would be created. Currently, there are 58 universities in Chile, 18 of which are private and 30 of which belong to CRUCH. The reform also reorganized a series of smaller institutions providing post-secondary technical training. These institutions were classified into two different categories, known as Institutos Profesionales (IP) and Centros de Formación Técnica (CPT). The main difference between the two is that the former can provide some bachelor degrees in a limited set of areas.

The government also used the 1981 reform to foster the generation of revenue by the universities. Even though the military regime reduced spending on higher education since 1973, it was only mildly successful in reducing the dependence of universities on government transfers [13] The 1981 reform introduced university fees, which would eventually be again eliminated by the government of Michele Bachelet in 2016.

3 Empirical Strategy

3.1 Data

In this section we briefly introduce the different data sources that we use in the paper. Online Appendix A provides more detailed information about these sources. The main data source are the Chilean household censuses of 1992, 2002 and 2017. The census files provide quasi-universal information at the individual level on gender, age, educational attainment, labor force participation, unemployment, occupation, marital status and fertility. In each census, individuals are classified into households and one person is identified as the head within each households. For all other respondents, the census reports how they are related to the household head. The questions in the census and their level of detail vary slightly over time, especially in 2017 relative to the previous two. For example, this most recent census does not ask about employment categories (i.e. business-owner vs salaried employee), but does ask about completion of the highest educational level. Only the 1992 census includes an additional variable indicating the wealth quintile to which the household belongs, based on the observable characteristics of the dwelling and on the answers provided to questions on ownership of various assets.

We complement the information in these censuses using the twelve available waves of the biennial CASEN household survey between 1990 and 2015.[14] This is a repeated cross-section that is representative at the regional level.[15] In the most recent wave we use from 2015, the survey has data on more than 260,000 individuals in over 80,000 households. The CASEN survey includes

[13] Appendix Figure A4 shows that higher education represented almost 50% of government spending on education in 1974. This number dropped to less than 30% in 1980.

[14] Survey years are 1990, 1992, 1994, 1996, 1998, 2000, 2003, 2006, 2009, 2011, 2013, 2015.

[15] Chile is administratively divided into 16 regions, which are subdivided into provinces (56) and counties (346).

information on education, health and economic conditions of all members of each surveyed household. It has several attractive features, including its relatively high frequency and the availability of information on self-reported income.

For the synthetic control analysis, we use harmonized census micro-data from the Integrated Public Use Micro-data Series (IPUMS) - International. We use the most recent census that is available for each of the 57 countries for which harmonized data is available.[16] Our interest is on the internationally-coded variable on educational attainment, but we also use other characteristics of the countries to create a synthetic comparison group for Chile that best reproduces the values of the predictors of the higher education enrollment share in the years before the military coup.

The sample for the analysis of the affected cohorts includes census or survey respondents born between 1943 and 1960. People in these cohorts reached age 21 between 1964 and 1981, creating an 18-year window around the time of the military coup. For all the main outcomes, we verify that the results are robust to more conservative bandwidths. Using administrative data on the age range of first-year college students, we find that 20.5 is a conservative estimate for the average age of first-year students in 1970, the closest year before the coup for which data is available.[17] We show below that the results are robust to changes in the age of college entry (i.e. changes in the kink point). We create a dummy for college entry and set it equal to one for all respondents that report"University" or higher (e.g. "Master's") as highest level of education. We only keep respondents that report having completed secondary in the 2017 census and proxy for secondary completion in other sources (which do not ask about completion) by restricting the sample to respondents reporting at least four years of secondary education. We introduce this restriction to have a valid counterfactual for college enrollment and to shut down any effects arising from disruption of lower levels of education in the aftermath of the military coup.

For the study of intergenerational effects in educational attainment, we exploit the information on household composition contained in the 2017 census. We focus our attention on this census because it is the one that best enables us to observe the final level of education obtained by children of people in the affected cohorts.[18] We connect children to their parents using several different combinations of positions in the household. About 90% of our sample (roughly 213,000 people) is composed of individuals reported as children of the household head (whom we always observe). The second largest category is comprised of heads of households in which at least one individual is reported as a parent of the household head.[19] We observe around 12,000 such cases (5%).

[16] Appendix A provides the list of these countries.

[17] Appendix Figure A6 shows the age distribution of students in the first and last (5+) year of college in these years.

[18] In 2002, the youngest cohort of parents was 42 years old, making it unlikely that their children had finished their education. In 2017, this same cohort of parents is 57 years old.

[19] In a small number of cases, we observe both parents of the household head and pick the oldest parent. Unfortunately, the sample is too small to enable us to use this information to further study potential complementarity or

The other categories are much smaller and include siblings of the household head (if a parent is observed), the spouse of the household head (if a parent-in-law resides in the household) or children of the spouse. We restrict the sample to children with ages between 25 and 40 years. By excluding younger individuals we improve our chances of observing final college enrollment. By excluding older individuals, we ensure balance in the distribution of parental cohorts. We verify that the results are robust to further restricting or expanding the bandwidth of ages of children in the sample. Having linked parents and children, we restrict the sample to parents meeting the same conditions as in the analysis of the affected cohorts: high-school graduates reaching age twenty between 1965 and 1980. Our final sample includes 228,608 individuals (i.e. children), 58% of whom report having enrolled in university.

An important limitation of our analysis of intergenerational effects is that we can only connect parents and children living together at the time of the 2017 census. Appendix Table A18 provides summary statistics of various characteristics for a series of nested samples, starting with the entire population of 25-40 year-olds in the 2017 census and finishing with our estimating sample. Our sample is positively selected in education, primarily because we condition on having a linked parent with full secondary. People in our sample are less likely to report being employed, but are more likely (by roughly the same amount) to report being studying. In this sense, our sample has the desirable feature of including those individuals with non-negligible probability of enrolling in higher education. As mentioned above, the vast majority of our sample is comprised of people reported as children of the household head (90%), while only 26% of entire 35-40 age group has such status. Women in our sample have half as many children as in the broader population.

3.2 Research Design

Our empirical strategy to capture differences in outcomes caused by the capture of higher education by the military regime involves comparing changes in trends for cohorts that reached college age in a narrow window around the time of the military coup in 1973, in the spirit of a regression kink design (Card and Yakovlev, 2014; Card et al., 2015). Our identification assumption is that in the absence of the coup, and within a sufficiently small window, there is no reason to expect a change in the trend of our outcomes of interest for cohorts reaching college age after this event. As mentioned above, we classify cohorts based on the year in which they reached twenty-one years of age, as this is the average age of first-year college students that we observe at different points in time, both before and after the coup. We work with the following reduced-form model to estimate the effect of exposure to the dictatorship:

$$Y_{i,a21} = \alpha + \beta X_i + \pi_0 f(a21) + \pi_1 \mathbb{1}(a21 \geq 1973) \times g(a21) + u_{i,a21} \quad (1)$$

substitution in the effects of paternal and maternal education.

where $Y_{i,a21}$ is the outcome of interest (e.g., enrollment in college) for individual i belonging to a cohort that reached age twenty-one in year $a20$. X_i is a set of observable characteristics, including gender-specific county-of-birth fixed effects, meaning that we restrict our comparison to individuals of the same gender born in the same county. $f(a21)$ and $g(a21)$ are smooth functions (polynomials) representing the birth date profile of outcome Y_{ia21}. We re-scale the running variable in these functions and set it equal to zero for 1972, the last year before the coup. $\mathbb{1}(a21 \geq 1973)$ is a dummy variable equal to one for those individuals (cohorts) that reached age twenty-one in 1973 (year of the coup) or later. Finally, u_{ia21} is an error term clustered at the county-of-birth level.

We are interested in quantifying the break in the trend of Y_{ia21} that was triggered by the dictatorship. To simplify exposition, our baseline specification uses a linear polynomial in $a21$ (i.e., $f(a21) = g(a21) = a21$), such that the parameter characterizing the change in trend before and after 1973 corresponds to π_1:

$$\pi_1 = \frac{\partial Y_{i,a21}}{\partial a21} \mid_{\mathbb{1}(a21 \geq 1973)=1} - \frac{\partial Y_{ia21}}{\partial a21} \mid_{\mathbb{1}(a21 \geq 1973)=0},$$

We use a bandwidth of 18 cohorts reaching college age around the time of the military coup (eight before, eight after). These cohorts have birth years between 1943 and 1960 and reached age 21 between 1964 and 1981. This choice is determined by several factors. Given that our interest is the change in the trend of educational attainment caused by the military regime, rather than an abrupt discontinuity, we need a large enough bandwidth to provide the necessary variation. This need is heightened by the absence of a mandatory minimum or maximum age of college entry, which leads to a fuzzy date of treatment onset. In this regard, we verify below that our results are not sensitive to small changes in the location of the kink. We cut the sample at the 1981 age 21 cohort to mitigate the confounding effect of the large reform of the Chilean university system that was implemented by the military regime in that year. Setting the 1964 age 21 cohort as the start-point ensures a balanced sample centered at 1973. The discrete nature of the running variable prevents us from applying a non-parametric approach to select an optimal bandwidth, but we show the robustness of our results to alternative choices (see Appendix Figures A14-A16).

A valid concern surrounding our empirical strategy is that in any one cross-section our cross-cohort comparison may be picking up non-linearities caused by age effects. We address this concern in three ways. First, we exploit the availability of data at various points over a 25-year period to show that the effects are present in multiple years, corresponding to different stages in the life cycle. Second, the relatively high frequency of the CASEN survey allows us to observe different cohorts (on both sides of the kink) at the same age. This enables to estimate a more stringent specification that replaces the cohort trend with a set of flexible age fixed effects. In this case, the counterfactual for the affected cohorts is constructed using the average of the outcome among unaffected cohorts when they were at the exact same point in the life cycle. Finally, the robustness of our results to shorter bandwidths arguably reduces the confounding effect of age.

We can also leverage the cross-cohort variation in college enrolment triggered by the dictatorship as an instrument to provide Instrumental Variables (IV) estimates of the effect of enrolment. For this purpose, we estimate the following system of equations:

$$C_{i,a21} = \alpha + \beta X_i + \pi_0 f(a21) + \pi_1 \mathbb{1}(a21 \geq 1973) \times g(a21) + u_{ia21}, \quad (2)$$

$$Y_{i,a21} = \phi + \delta C_{i,a21} + \gamma X_i + \rho_0 h(a21) + \epsilon_{ia21} \quad (3)$$

where $C_{i,a21}$ stands for college enrollment for individual i belonging to the cohort that reached age 21 in year $a21$. Similarly to Card and Yakovlev (2014), this approach overcomes the endogeneity of college entry using the break in trend after 1973 to instrument for it. We focus on college enrolment, rather than completion, because this is the margin that is arguably more responsive to the policies implemented by the regime. As a result, our estimates likely provide a lower bound for the effect of a full college education. Under standard assumptions, the 2SLS estimate of δ in equation 3 may be interpreted as a local average treatment effect (LATE) (Angrist et al., 1996). This is an average causal effect of college entry for compliers, i.e. those students who did not attend college because they reached the age of enrollment at a time of reduced supply by the military government. By restricting our sample to people with full secondary education, the IV estimate provides the LATE of college enrollment relative to the relevant counterfactual of having a secondary degree. To use the trend-break after 1973 as a valid instrument for college entry, an additional exclusion restriction must hold. In our setting, this condition implies that the change in outcomes for the cohorts that reached age twenty-one after 1973, relative to those that did so shortly before them, is entirely driven by the lower probability of college enrolment experienced by the former.

To study the intergenerational effects of college enrollment, we use specifications analogous to the ones above. The corresponding IV estimates tell us how does parental college enrollment affect the child's own probability of enrollment. As above, this is a LATE estimate capturing the effect of parental enrollment for children whose parents' enrollment was affected by the decrease in openings when they reached college age. The corresponding exclusion restriction states that the cohort of the parent only affects the child's educational attainment through its effect on the parent's college enrollment. The main change to the previous specifications is that the cohort trends correspond to the parent that we are able to observe. Another difference is that we can expand the set of individual controls, X_i. In our most-preferred specification, we include (i) gender x county of birth, (ii) gender x parent's gender, (iii) relationship to household head and (iv) age fixed effects. By including the gender x parent's gender fixed effects we restrict our comparison to children of the same gender that we can connect to a parent of the same gender. The age fixed effects alleviate the concern that parents from later cohorts are more likely to have children that are younger in 2017, which could have different outcomes due to time trends. However, comparing children of

the same age born to parents from different cohorts means that we are comparing children with parents of varying ages at the time of birth, which could also confound the analysis. In this regard, we follow an agnostic approach and present estimates with and without age fixed effects, as well as replacing them with age-at-birth fixed effects. In this case, we are comparing children with parents from different cohorts but that were of the same age at the time of birth. Results are qualitatively similar across these different specifications and we use the fluctuation in the point estimates to learn about underlying mechanisms (i.e. effect of maternal age).

4 Educational Attainment of the Affected Cohorts

4.1 Non-parametric Analysis

We begin the analysis by examining cross-cohort patterns in educational attainment in the raw data from the 2017 population census. This preliminary inspection does not make any structural assumptions and helps motivate the parametric trend-break analysis that follows. First, panel (a) in Figure 4 shows that the number of people per cohort is relatively smooth around the year of the coup. In the x-axis, cohorts are organized by the year in which they turned 21 years old (year of birth in parenthesis). The vertical lines mark the year of the military coup (solid red) and the window used in the regression analysis below (dashed blue). The smooth population numbers suggest that violent repression at the hands of the military regime and increased out-migration during the dictatorship did not have large differential effects within our sample (i.e. no missing mass). Panel (b) shows the share of census respondents in each cohort that report "Secondary" (Media) as their highest level of education and that report having completed this level. These are the individuals that constitute our baseline sample below, so it is reassuring to observe no large changes around the time of the coup. There is only a minor blip for the cohort that reached age 20 in 1979. This cohort reached age 14, which is the normal age of transition from primary to secondary, in 1973, providing evidence of disruption in lower levels of education in the year of the coup. Panel (c) shows the share of people within the sample with full secondary that report "University" as their highest level of education. Within the 18-cohort window used for the regression analysis, we observe a systematic increase in college attendance for the cohorts that reached age twenty-one before the coup, followed by a large decline for those cohorts that reached this age after the coup. More specifically, college entry increased 4 percentage points (pp) between 1964 and 1972, corresponding to an 11% increase relative to the 35% enrollment rate observed in the initial year. By 1981, this rate was down to 25%, corresponding to an 36% decrease relative to the peak in 1972. Had the previous trend continued, the college entry rate would have been closer to 43-45% by this time. Appendix Figure A8 shows that the patterns above are not exclusive to the 2017 census. Panel (d) plots the same series as in panel (c), but disaggregated by gender. We observe that college enrollment was largely stable among men, but was growing rapidly for women before

the coup. After the coup, enrollment drops sharply for everyone.

4.2 Parametric Analysis

To quantify the break in the trend of college enrollment, Table 1 presents estimates of equation (1). This specification formally extrapolates the upward trend in college enrollment among the 1964-1972 age 21 cohorts to estimate the magnitude of the break for the ones reaching age 21 in the period 1973-1981. Column 1 shows that cohorts in the pre-coup period were experiencing an average *increase* in college enrollment of 0.5 pp per year. This trend experienced a 1.9 pp. *decrease* after the coup. The difference between the two coefficients indicates a net negative trend of 1.4 pp per year for the cohorts reaching college age after the coup. For the cohort that reached age 21 in 1981, these estimates imply that rather than experiencing a 4 pp increase in the college entry rate relative to the 1972 cohort, it faced a 11 pp decline. This reduction is equivalent to about one third of the average rate of college entry among high school graduates during the sample period.

Column 2 provides evidence of mild substitution of college education with technical schooling (lower-level tertiary education). The average yearly increase in the entry rate to these institutions increased from 0.01 pp in the pre-coup period to only 0.4 pp in the post-coup years. As a result, the cohorts affected by the coup experienced a net trend of entry into higher education of -1.1 pp, relative to a pre-coup trend of 0.5 pp per year (column 3). Columns 4-6 examine the change in trend for degree completion. The results in column 4 show that the college graduation rate was also growing in the pre-coup years, but at a lower rate than enrollment, suggesting substantial dropout. This trend becomes -1.0 pp for the affected cohorts.[20] The drop in completion is almost 80% of the size of the drop in enrolment, telling us that most of the people that failed to enroll after the coup would have gone on to complete their degree. For technical schooling, the completion trends roughly coincide with the enrolment trends, suggesting that there was little dropout both before and after the coup. For higher education as a whole, the trend in the graduation rate experienced a net decline and reached -0.7 pp per year after the coup (column 6).

Appendix Table A10 provides disaggregate estimates by gender. In the pre-coup years, we observe a positive trend in college enrolment of 0.8 pp for women and 0.2 pp for men. After the coup, the net trend for men drops to -1.3 pp, while that of women becomes -1.5 pp. This is consistent with the graphical evidence in Figure 4 in showing that men and women experience similar declines in enrollment after the coup.

[20] Appendix Figure A9 plots the college completion rate by cohort among people reporting any college. We observe a 2.5 pp drop for the cohorts that reached age 21 in the period 1971-1973 (relative to a baseline graduation rate of 82% in 1970) and a 7 pp increase between 1974 and 1980. This indicates that the students that were already enrolled in college before the coup experienced a decrease in the probability of completion. On the other hand, those that enrolled afterwards experienced gains in the completion rate, arguably as a result of tighter admissions standards and the increased focus on academic achievement.

Online appendix C shows results from several robustness checks. In Table A2 we verify that the results are not sensitive to changes in the location of the kink point in equation (1). This exercise is motivated by the fact that there is substantial variation in the age of first-year college students (Figure A6), making it impossible to perfectly distinguish between affected and unaffected cohorts very close to the coup. We find that slight changes in the location of the kink do not affect the results, but that our baseline specification has the best fit. Table A3 shows that the same pattern in college enrolment is present in the population censuses of 1992 and 2002, as well as in the CASEN household survey.

4.3 Synthetic control analysis

A synthetic control analysis provides additional evidence for the impact of the military coup on college entry (Abadie and Gardeazabal, 2003; Abadie et al., 2010). We use the most recent population census data available in IPUMS-International for 57 countries. Our baseline estimates use data from Latin American countries to construct the counterfactual for Chile, but results are unaffected if we also use data from other countries.[21] For each cohort in a census, we calculate the share of people with complete college education, complete secondary education, and complete primary education, restricting the sample to individuals over 20 years of age. We use college *completion* instead of *enrollment* as the outcome variable to compare across countries using the harmonized education variables reported in IPUMS. All estimates use lags of the share of people with completed college education to build the synthetic control.[22] Our baseline estimates do not include controls, but results are unaffected if we control for the share of people between 18 and 65 years of age, the share of women, and the share of people with secondary education.

Figure 5 shows the results. The solid line corresponds to the rate of educational attainment by cohort in Chile. The dashed line shows the predicted rate from the synthetic control. In panel (a), the outcome is full college education. We observe that the synthetic control tracks the realized time series very closely up to the year of the coup. College completion increases from 3% in 1950 to slightly more than 6% in 1972. After the coup, both series diverge. The synthetic control keeps growing, reaching 8% around 1980, while the actual series stagnates and falls to around 5%.[23]

[21] The Latin American countries (census year) are: Argentina (2010), Bolivia (2001), Brazil (2010), Colombia (2005), Costa Rica (2011), Dominican Republic (2010), Ecuador (2010), Honduras (2001), Haiti (2003), Mexico (2015), Nicaragua (2005), Panama (2010), Peru (2007), Paraguay (2002), El Salvador (2007), Uruguay (2011). The data for Chile comes from the 2002 census. Figure A10 shows that the results are unaffected if we exclude all countries with a dictatorship in the years 1950-1990, or if we only use countries with a high Human Development Index.

[22] We follow Ferman et al. (2019) and use only *odd* years to avoid cherry picking and overfitting. Table A6 shows that results are identical if we use *even* or *all* pre-treatment years. For reference, the R^2 of a regression between the treatment and the synthetic control in the pre-treatment period is always larger than 0.95.

[23] Placebo inference and confidence sets suggest this difference is statistically significant (Abadie et al., 2015; Firpo and Possebom, 2018). See Table A6 and Figure A11 for details.

The analysis further suggests that it is only after the return to democracy in 1990 that college completion starts growing again and comes closer to the counterfactual. Panel (b) shows a very similar pattern if we include additional controls. Panels (c) and (d) provide some validity checks on the methodology. Panel (c) shows that the synthetic control predicts very well the realized times series of complete secondary education, indicating that the observed effects in college cannot be attributed to changes in lower levels of education. In panel (d), we restrict attention to the pre-treatment period and create a synthetic control using a placebo treatment in 1960, following Abadie et al. (2015). Reassuringly, both groups behave similarly throughout the sample period.

4.4 Targeting and Quality of Education

We have established that it was the reduction in openings for incoming students decreed by the military regime that led to the drop in college entry for the affected cohorts. Still, several related questions remain regarding whether the composition of the student body or the quality of instruction changed as a result of these policies. For example, the regime may have targeted people with specific characteristics, deemed as dangerous, to prevent their entry into college. It could also have happened that people from privileged socioeconomic backgrounds were able to use their influence to crowd out those from less privileged backgrounds from the increasingly limited spots in college. The quality of education could have also changed as a result of the dismissal of faculty and the closure of programs, research centers and academic units. Answering these questions is important in order to have a better sense of potential differences in the characteristics of the average college student before and after the coup, which could affect our interpretation of the IV results below.

In this section, we present results from two exercises aimed at shedding light on these issues. The first one involves examining whether there is a kink in college entry among more tightly-defined sub-groups. In particular, we study entry within wealth quintiles using data from the 1992 census and within families using data from the three censuses available. The second exercise involves estimating the college earnings premium and examining whether it changes for the cohorts that went to college after the military coup.

Table A4 shows estimates of equation (1), estimated separately for each quintile of housing wealth in the 1992 census. As mentioned in section 3, households are classified into quintiles based on characteristics of the dwelling they inhabit and ownership of assets. The main caveat to this analysis is that these quintiles are assigned in 1992 and are quite plausibly affected by the observed changes in college entry. Still, insofar as there is persistence in socioeconomic status independently of educational attainment, these regressions can be informative about the potentially unequal incidence of the reduction in college enrolment. As expected, we find that the positive trend in college entry before the coup is largest for the top quintile and decreases monotonically as we go down the socioeconomic ladder. For people in the top quintile, each additional cohort up to 1972 experienced an increase of 2.1 percentage points (pp) in the college entry rate, while for

those in the bottom quintile this increase was only 1.3 pp (a 39% reduction). After the coup, the people at the very top have a net trend of -1.6 pp, while those at the very bottom have a net trend of -1.3 pp. These results indicate that people across the entire socioeconomic spectrum were affected by the reduction in the supply of college education and that there was limited selective targeting of admissions.

To examine whether the kink in college enrolment is also observed within families, we exploit the information on household composition contained in the population censuses. In a first instance, we only include in the sample groups of two or more people within the same household that report being children of the household head. In an alternative specification, we only include household heads and those that report being their siblings. In both cases, we can be sure that the included individuals within the same household share at least one parent and we include household fixed effects to absorb all common characteristics. This exercise is motivated by previous findings of a strong correlation in educational attainment between siblings across various settings (Björklund and Salvanes, 2011). If the reduction in college entry disproportionately affected certain groups of people based on family-level characteristics, such as political affiliation or pre-coup socioeconomic status, the inclusion of these fixed effects should absorb most of the cross-cohort variation in enrolment. The limitation of this exercise is that it relies on the selected sample of individuals that live with at least one of their siblings in 1992 or later.

Table A5 shows separate estimates of equation (1) for each census year and each way of constructing the sample. This analysis is quite demanding on the data, as the number of observations in all columns is an order of magnitude smaller than that in the full sample. In later years, there is a greater number of cases of siblings living with the household head and a smaller number of cases of children living with the household head. However, in all columns we observe the same trend-break that we found in the overall sample: cohorts that reached college-age in the years immediately before the military coup experienced a positive trend in college enrolment, while those that did so shortly afterwards saw a large reduction and a net decline in this trend. What is new about these results is that this pattern is present even among people that share at least one parent. The magnitude of the estimates is very much comparable to that in the larger sample (e.g., column 6 in Table A5 vs column 1 in Table 1). These results provide further evidence of a widespread impact of the cut in college openings.

The previous results suggest that the entire population of high school graduates was to a large extent equally affected by the restrictive policies towards higher education implemented by the military regime. However, students going to college after the coup may have experienced a different quality of instruction or overall experience. We also have reasons to believe that the average quality of incoming students increased after the coup, as a result of the reduction in openings and the admissions criteria. We use data on earnings from the CASEN survey to shed light on these issues. If the quality of education deteriorated after the coup, we would expect the college graduates

from these later cohorts to have a lower earnings premium over those that did not attend university. This reduction in the premium could be driven by the fact that these college students have less human capital than their peers from previous cohorts or because the signalling value of a college degree is lower in a less challenging environment. On the other hand, if the variation in the quality of education is second-order, relative to the reduction in the supply of college graduates, or if the average quality of college students was higher we would expect the college premium to increase. For this purpose, we estimate the following Mincer equation:

$$\ln \text{income}_{i,a21,j,t} = \kappa + \lambda X_{i,a21,j,t} + \omega_{a21} \mathbb{1}(\text{any college}) + \nu_{i,a21} \tag{4}$$

where $\ln \text{income}_{i,a21,j,t}$ is the natural log of reported real earnings for individual i belonging to age 21 cohort $a21$, that lives in county j and appears in the CASEN survey from year t. In our baseline analysis we use self-generated income as our income measure, but in Appendix Figure A12 we show that the results are robust to using other available income measures. $X_{i,a21,j,t}$ is a set of controls including gender-specific county-of-residence fixed effects, age fixed effects and year (i.e. CASEN survey wave) fixed effects.[24] Hence, we are only comparing people of the same gender that live in the same county, while flexibly allowing for age and time effects. ω_{a21} is a cohort-specific coefficient for the dummy variable $\mathbb{1}$(any college), which equals one for respondents that report university or more as their highest level of education. As before, the sample only includes respondents born between 1943 and 1960 that report having completed four years or more of secondary education.

Panel (a) in Figure 6 shows the results. For the cohorts that reached college age before the coup, we observe a positive effect of college attendance of about 70 log points. After the coup, there is a sharp increase and the college premium rises to about 80 log points. The 10 log point difference between the pre-coup and the post-coup averages is equivalent to an 14% increase over the pre-coup average. This result indicates that college entry became more profitable in the post-coup years and is consistent with both a lower supply of college graduates and a higher quality. Furthermore, it shows that the educational policies of the military regime contributed to increased inequality between those that could and could not enrol in college. Panel (b) replicates the analysis including fixed effects for nine occupational categories. While the overall magnitude of the college premium decreases, consistent with the effect of college on income partly operating through occupational choice, we still observe around an 14% for the cohorts that reached college age after the coup.

[24]CASEN does not specify county of birth. We verify that the results are not sensitive to the exclusion of the county-of-residence control, which could be endogenously affected by educational attainment.

5 Economic Outcomes for the affected cohorts

In this section, we document the effects of reduced college enrolment on the cohorts that reached college age after the military coup in Chile. We first examine several labor market outcomes. We rely on the 1992 and 2002 censuses for this part of the analysis for two reasons. One is that questions on occupation are not asked in the 2017 census. The other is that the kink in college enrollment roughly coincides with the age of retirement in 2017, potentially biasing the results.[25] We then look at measures of income using data from the CASEN household survey. Finally, we provide further evidence on social mobility using novel data on housing wealth from the 1992 census.

5.1 Labor Force Participation and Unemployment

Columns 1 and 2 in Table 2 show reduced-form estimates of equation (1) and IV estimates of equation (3) using an indicator for labor force participation as the dependent variable. Each column uses the sample from a different census. The estimates show that at both points in time the cohorts that reached college age before the coup had a positive trend in labor force participation, meaning that members of each new cohort had higher probability of being in the labor force than those from the cohort immediately preceding it. This could be a reflection of their higher educational attainment (i.e. more likely to go to college) improving their labor market success, but it could also be caused by older cohorts starting to leave the labor market due to disability, early retirement, etc. In both years, we observe a drop and a net reversal in this trend for the cohorts that reached college age after the coup. We observe the opposite pattern for unemployment in columns 3 and 4. Here, the early cohorts had a weakly negative trend, which becomes positive for the affected cohorts. The regressions for unemployment in these columns include labor-force participation as an additional control to ensure that we are only comparing the employment status of individuals interested in having a job. As mentioned above, these kinks could be driven by non-monotonicities in the outcomes associated with different stages in the life-cycle. However, the fact that we observe them at two points in time that are ten years apart suggests that this is unlikely. Our sample cohorts have ages 32-47 in 1992 (with the kink at 39) and ages 42-57 in 2002 (with the kink at 49). Furthermore, we verify in Appendix Figure A14 that the results are robust to additional tightening of the bandwidth, which should reduce the importance of any such non-linearities.

The IV estimates reported at the bottom of the table allow us to quantify the effect of college enrolment that is implied by the previous reduced-form estimates. We find that college enrolment leads to a 33 percentage point (pp) increase in labor force participation in 1992 and to a 57 pp increase in 2002. These are large effects and correspond to 43 and 74% of the respective sample

[25] The retirement age in Chile is 65 years for men and 60 years for women. In 2017, the cohort that reached age 21 in 1973 (i.e. the location of the kink) is precisely 65 years old.

means (approx. 76% in both cases). Similarly, people that enrol in college face an unemployment rate that is 6.0 pp lower in 1992 and 2.0 pp lower in 2002. These are also large effects relative to the respective sample means of 3.3 and 6.3%.

Panels (a) and (b) in Figure 7 show IV estimates disaggregated by gender.[26] We find substantial heterogeneity in the effect of college entry on labor force participation, with the effect for women being 50-100% larger than for men. For women, college enrolment increases labor force participation by 42pp in 1992 and by 66pp in 2002, while the respective estimated effects for men are 23pp and 45 pp. Regarding unemployment, the differences are not so clear. College enrolment has a negative effect for women of -3.8pp in 1992 and -1.9pp in 2002. For men, the equivalent effects are -8.2pp and -1.6pp. All differences in the effects between men and women are statistically significant at the 0.1% level, except for unemployment in 2002 (p=0.47).

Taken together, the evidence on labor force participation and unemployment indicates that the cohorts that were affected by the reduction in the supply of college education implemented by the military regime faced substantially worse opportunities to participate in the labor market and find employment. For women, college enrolment largely determined labor force participation, while for men there was a larger effect on unemployment.

5.2 Occupation

Columns 5 and 6 in Table 2 show results for the probability of reporting being a salaried employee in 1992 or 2002. Other categories include business owners, domestic workers and unpaid workers helping relatives. The pattern in the data are very similar for the two censuses. Focusing on the IV estimates, we find that college enrolment increases the probability of salaried employment by around 9 pp, relative to sample means of about 70%. Appendix Table A7 shows that this gain in wage-earning status comes at the expense of business ownership, but also to a large extent from reductions in domestic work and unpaid work with relatives. Again, we see how reduced educational opportunities substantially worsen the available employment opportunities.

These effects are also highly heterogeneous by gender. Panel (c) in Figure 7 shows that the positive effect of college enrolment on salaried employment is almost entirely driven by women, with an estimated effect size of around 15pp. For men, the effect is a quarter of the size. The full results by gender in Appendix Table A12 show that college enrolment reduced the probability that women engaged in domestic work or unpaid work with relatives by 9.8 pp in 1992, relative to a summed average of 3.7%. The estimates for 2002 show an aggregate reduction of 5.5 pp, relative to an average of 5.7%. Hence, restricted access to college prevented the affected women from accessing paid employment and led them to domestic or unpaid work.

Columns 7 and 8 in Table 2 show that college enrolment has a large positive effect on the

[26] Appendix F provides full results of these estimations.

probability of having a white-collar, high-skill occupation in 1992 and 2002. In 1992, the estimated effect is 48pp, relative to an average of 43%. Appendix Table A8 shows that college entry also increases the probability of being part of the military, but the effect is much smaller. This positive effect could be due to improved screening during the military regime or to the fact that military officers will often also obtain a college degree as part of their formation. The increase in these occupations comes at the expense of low-skill white-collar occupations (i.e. clerical work) and blue-collar occupations to roughly the same extent. The magnitude of the estimates is smaller in 2002, but the pattern is very similar. In sum, college enrolment substantially determines access to the more prestigious occupations.

Panel (d) in Figure 7 shows that the effect of college entry on the probability of having a high-skill, white-collar occupation is almost twice as large for women than for men. The IV estimates for women in 1992 and 2002 are 62pp and 29pp, relative to respective female averages of 49% and 65%. The full results in Appendix Table A13 further show that the increase in job status for college-educated women predominantly comes from reductions in white-collar, low-skill work (i.e. clerical work), while for men the negative effects are larger on blue-collar work. As expected, the association between college education and military status is also substantially larger for men.

5.3 Income and Wealth

To analyze the effects of college enrolment on income, we rely on information from the CASEN household survey. As mentioned in section 3, this is a repeated cross-section collected roughly every two years since 1990. We combine all the survey waves and exploit their relatively high frequency to estimate a more stringent specification that flexibly accounts for age effects in addition to our baseline equations. The CASEN survey includes information on several different measures of income. These include income from the main occupation, total work income (i.e. more than one job), self-generated income (i.e. including non-work income, but excluding government transfers), and total income (i.e. including government transfers). An important limitation of this data is that it is entirely based on self-reports and prone to measurement error. To the extent that measurement error affects the responses from people in different cohorts to the same extent, or is absorbed by the set of controls we discuss below, this is less of a concern.

Panel A in Table 3, shows reduced-form and IV estimates for all four income measures (in logs). The reduced-form results show positive trends for the pre-coup cohorts for all income measures except income from the main occupation in column 1. In columns 2-4, we observe that each new cohort in the pre-coup years has an average income that is roughly 1.2 log points higher than that of the one immediately preceding it. After the coup, this trend reverses and becomes negative: each new cohort has average income that is 1.1-1.9 log points lower than the one immediately before it. The corresponding IV estimates tell us that enrolling in college increases income by 55-92 log points on average. These averages are calculated over a 25-year period between 1990

and 2015. Appendix Figure A13 shows separate estimates for each survey wave. The results are fairly robust, indicating that our average effects are not driven by a small subset of survey years and that the affected cohorts experienced a systematic decline in income throughout the life cycle. Appendix Table A14 shows that these effects are very similar for men and women.

Panel B in Table 3 shows results from a modified specification that replaces the overall trend in income (which we use to build the counterfactual for the affected cohorts in our baseline analysis), with a more stringent set of age fixed effects. These fixed effects allow income to flexibly vary year-on-year at different points in the life cycle. The reduced-form estimates now tell us whether there is a trend in income for the affected cohorts, relative to what we observe for the control ones at the exact same age. The IV estimates rely on this post-coup trend as an excluded instrument for college enrolment. Despite the smaller sample size of CASEN, relative to the censuses, the instrument remains very strong in all specifications (F-stat $\geq$ 280).

The reduced-form results point to a negative trend of 0.6 log points per year. The corresponding IV estimates show that college enrolment has a positive effect on average income of 20-23 log points. These estimates are substantially smaller than those from our baseline specification, but remain quite sizable, especially when considering that our regressor of interest is college enrolment and not college graduation. The estimates are also very precise, with the exception of work income in column 2, which is not statistically significant at conventional levels (p=0.121).

We complement the analysis on income using data on housing wealth from the 1992 population census. Based on the characteristics of the dwelling they occupy and the ownership of assets, households are classified into quintiles of housing wealth. Fifty percent of our sample of individuals with full secondary reaching age 21 between 1964 and 1981 belong to households in the top wealth quintile, 25% to the fourth quintiles, and 15, 8 and 2% to the lower three quintiles in order. Table 4 shows estimates using the resulting quintile dummies as dependent variables. We observe that the affected cohorts are increasingly less-likely to reach the top of the socioeconomic ladder. While the pre-coup cohorts faced a negative trend of -0.2pp per year in the probability of belonging to a household in the top wealth quintile, this trend drops sharply for the cohorts that reach college age after the coup and reaches -1.5pp per year. The IV estimate shows that college enrolment increases the probability of reaching the top quintile by 35pp, equivalent to 70% of the sample mean. We estimate a roughly equal 10pp drop in the probability of being in each of the second, third and fourth wealth quintiles. We also find a quite sizable decrease in the probability of being in the bottom quintile of 2.4pp, equivalent to over 100% of the sample mean.

In Appendix Table A15 we show that the effect of college enrolment on wealth mobility is larger for men. In particular, college entry increases men's probability of belonging to a household in the top quintile by 41pp, while it only increases this probability for women by 30 pp. This result can plausibly be driven by the household-level measurement of wealth, if women that do not go to college are relatively more likely to marry college-educated men than men that do not go to college

are to marry college-educated women. If this is the case, the marriage market would attenuate the effect of college enrolment on female social mobility. We explore the effect of college on marital status below.

6 Non-Economic Outcomes for the affected cohorts

In this section, we explore the effects of reduced college enrollment on non-economic outcomes for the affected cohorts. We first study marital status. We then focus on status within the household as a measure of economic dependence and vulnerability.

6.1 Marital status

Table 5 shows results for marital status. Panel A uses data from the 1992 census, while panel B shows results for 2002. Unfortunately, information on marital status is not available for 2017. The dependent variable in column 1 is a dummy for having ever been married. In columns 2-4, the dependent variables are dummies for being currently married, widowed or divorced, respectively. In these columns, we include an ever-married dummy as an additional control to ensure that we are only comparing post-marriage outcomes among people that have been married at some point in the past.

We find that the affected cohorts are less likely (relative to the pre-coup trend) to have ever been married. The IV estimates indicate that college enrolment increases the probability of marriage by 26pp in 1992 and by 8pp in 2002. We also find that conditional on having been married, the affected cohorts are increasingly likely to report being widows, both in 1992 and 2002. These effects are quite large and the IV results show that college enrolment reduces the probability of being a widow by 5 pp in 1992 and by 12.4 pp in 2002, which correspond in both cases to more than four times the sample average. The fact that we observe increased widowing in younger cohorts goes against any possible age effects. More generally, this result suggests the existence of a negative relationship between college enrolment and mortality (Buckles et al., 2016), if we assume that people plausibly tend to marry people of roughly the same age. However, the data sources employed in this paper are not ideal for studying this topic and we reserve further exploration for a future study.

Appendix Table A17 provides disaggregate results by gender. In both censuses, we observe that the effect of college enrolment on the probability of having ever been married is much larger for men than it is for women. One possible explanation is that women without college are more likely to get married than men without college, which reduces the marital college premium for women. Another explanation is that college enrolment has two opposite effects on women. Even though it makes them more attractive partners in the marriage market, it also increases their leverage and enables them to wait for a better match, thereby partially offsetting the first effect. Given that in both years the share of men without college that have ever been married exceeds the share of

women without college that have ever been married, we find the second explanation to be more plausible.[27] The effect of college enrollment on widowing is 3-4 times larger for women. This suggests that the effect of college on mortality is actually larger for men.

6.2 Status Within the Household

We exploit the information on household composition available in all censuses to study the effects of reduced educational attainment on status within the household. We focus on household heads and their spouses, children and parents.[28]

Table 6 shows results for the probability of being in each of the above categories in each of the census years. One big pattern emerges. At all points in time, the cohorts affected by the reduction in college enrolment after the military coup experience a reduction in the trend for household head or spouse and a corresponding increase in the trend for child or parent of the household head. This constitutes evidence of increased economic vulnerability for the affected cohorts throughout the life cycle, which systematically prevents them from obtaining economic independence. It is striking to note that as early as 1992 we observe a relative increase in the probability of being parents of the household head for these cohorts and that as late as 2017 we observe a relative increase in their probability of being children of the head. These findings make it less likely that we are just capturing age effects (i.e. older people are always more likely to be household heads). The IV estimates tell us that college enrolment increases the probability of being the household head or spouse by 15-39 pp., amounting to 17-45% of the respective sample means.

Appendix Table A16 provides disaggregate results by gender. We observe that while college enrollment predominantly increases the probability of being the household head for men, it increases the probability of being the spouse of the head for women. Also, while college enrolment leads to a reduction in the probability of being a dependent (i.e. child or parent of the head) for both genders at all points in time, the effect on child status is systematically larger for men, while that on parent status is systematically larger for women.

7 Intergenerational Transmission of Human Capital

In this section, we explore whether the reduced educational attainment of the cohorts that reached college age shortly after the military coup affected the next generation. We begin by studying the

[27] The respective averages for men and women are 89% and 81% in 1992. In 2002, these averages are 91% and 83%. In both years, the difference in means is statistically significant at the 0.01% level.

[28] We combine spouses and partners into a single category. We combine own children, stepchildren and grandchildren into a single category. We also combine parents of the household head and parents of the partner/spouse into a single category. A residual category left out of the analysis comprises individuals living with their siblings or other relatives.

fertility of women in the affected cohorts and the survival of their children. We then move on to analyze potential effects on the educational attainment of the children.

7.1 Fertility and Child Survival

Questions on fertility are asked to women in all censuses. Even though later sources provide better estimates of the total number of children per woman, studying earlier ones allows us to learn about the timing of the effects as well. The censuses also ask about the number of children that are still alive, which we use as a coarse proxy for the health status of the children.

Columns 1, 4 and 7 in Table 7 show results using the total number of children as dependent variable. All sources point to a negative trend in fertility among the pre-coup cohorts of -0.03 children per year. In 1992, the trend for the affected cohorts is even more negative and takes a net value of -0.06, but at that time the affected cohorts are still in their thirties and likely to have more children. Starting in 2002, when the youngest cohort in our sample is already 42 years old, the trend-break changes sign and becomes positive. This indicates that fertility dropped at a lower rate for women in the affected cohorts. The results are quite similar in 2017. The IV estimates tell us that college enrolment reduces total fertility by 0.5-0.6 children, which is equivalent to 23% of the sample average of 2.6 children per woman.

The dependent variable in columns 2, 5 and 8 is the share of children that are still alive. The reduced-form results tell us that pre-coup cohorts experienced gains of around 0.2 pp in child survival per year. The fact that the trends are so similar across censuses, despite the large time gap between them suggests that most of the variation in mortality comes from deaths in early life. For women in the affected cohorts, this trend is between 50% smaller, suggesting that their children experienced worse health conditions on average. The IV results are quite stable across censuses and indicate that college enrolment reduces child mortality by about 2pp, a very large effect given the average child mortality of 1.7%.

The fall in the trend of child survival for the women in the affected cohorts is already visible in 1992, when their fertility is still underway. This suggests that the increased mortality of the children with mothers in these cohorts is not entirely driven by a quantity-quality trade-off. To further analyze this possibility, columns 3, 6 and 9 include a full set of fixed effects for the total number of children that each woman has. This way, we are only comparing women from different cohorts that report having the same number of children. In 1992, the IV coefficient is slightly larger conditional on the total number of children (column 3). This suggests that child mortality was disproportionately high for the mothers in the affected cohorts when they were in their thirties, relative both to the trend from previous cohorts and to the total number of children they had at the time. In later years, we find that the effect of college enrolment on child survival is partially attenuated by the inclusion of this additional control, suggesting the presence of a quantity-quality trade-off in the longer-run. The IV estimates drop 40-50%.

7.2 Children's College Enrollment

In this section, we study the educational attainment of children with parents in the cohorts whose college enrollment was affected by the military dictatorship. In particular, we want to know whether people with parents in these cohorts are themselves less likely to go to college. Our sample includes almost 230,000 people between the ages of 25 and 40 that we are able to connect to a parent reaching age 21 between 1964 and 1981. For this analysis, we use the same specifications from the previous section, with the exception that the cohort trend and break refers to the parent, while the main outcome of interest refers to the child. Before that, column 1 in Panel A of Table 8 shows that college enrollment among the parents of the children in our sample exhibits a pattern essentially identical to the full sample in Table 1. This suggests that the smaller sample of parents that we are using is not fundamentally different from the overall population.

Panel B shows reduced-form estimates of the relationship between the birth cohort of parents and the college enrollment of their children. For people with parents that reached college age before the coup, we observe a positive trend in college entry of 0.4 pp per year. However, for people with parents in the later, affected cohorts, the trend reverses and becomes -0.1 pp. This is evidence of a positive relationship between the college enrolment of the parents and the college enrolment of their children. If we use the break in trend for the parents' college entry as an excluded instrument for their own college enrolment, we find in panel C that having a parent that went to college increases a person's chances of enrolling by 26 pp. This is equivalent to 45% of the sample mean of 58%. The IV estimate is only 7% smaller than the corresponding OLS estimate presented in panel D. Arguably, while the IV estimate eliminates the selection bias confounding the OLS estimate, it provides a LATE effect for a complier population that is likely to have high returns to college (Card, 2001). We have a very strong first stage and the Kleibergen-Paap F-statistic takes a value of 292.

In column 1, the only controls are the gender x birth county fixed effects that we also included when analyzing the educational attainment of the affected cohorts. In columns 2 and 3 we further control for the gender of the observed parent and for gender of parent x gender of child, to ensure that differences in the gender composition of the sample across cohorts do not bias the estimates. The results change very little. In column 4 we include an additional set of dummies for the relationship of the individual (i.e. child) to the household head. Each of the ways in which we connect children to parents implies a different relationship of the child to the household head and this set of controls ensures that differences in the composition of the sample between cohorts along this margin do not confound the estimates. Again, we see little change.

In column 5 we introduce age fixed effects for the child. As mentioned above, these controls help address the concern that children with parents in later cohorts are themselves likely to be younger. This could, for instance, downward bias the estimate of the intergenerational effect if younger people benefit from positive trends in college enrollment in recent years. We find evidence

consistent with this possibility, as the IV estimate controlling for age (32 pp) is almost 30% larger than the baseline estimate. The specification in column 5 is our preferred specification for this part of the analysis. However, it is worth noting that the increased comparability gained by the inclusion of these fixed effects comes at a cost, as children of the same age born to parents from different cohorts differ in the age of the parent at the time of birth, which could also be an important factor. We further study this factor below when we focus on the sample of mothers and the broader impact of fertility patterns.

Appendix Figure A17 shows that the results are hardly affected if we consider more conservative bandwidths for the ages of parents in the sample, thereby increasing the comparability around the kink. Appendix Table A21 further shows that the results are also quite similar if we expand or restrict the window of ages of children included in the sample. For instance, the IV estimate of the intergenerational transmission of college for the tighter window of ages 25-30 is 0.29. Table A19 shows that the results are mostly driven by the sample of children of the head and are stronger for people classified as heads or spouses. This is consistent with these statuses being endogenously determined by non-enrollment in college among people with parents in the affected cohorts. Table A20 provides disaggregate estimates of the intergenerational effect of parental college enrollment depending on the gender of the parent or the child. We find little evidence of heterogeneity.

Figure 8 provides a non-parametric visualization of this effect. For these plots, we replace the parametric trends pre-and post-coup with dummies for each parental cohort, leaving 1965 as the omitted category. The set of controls is the same as in column 5 of Table 8. Panel (a) shows the first-stage estimates and their 95% confidence interval. We see increasing college entry of parents by cohort before the coup, followed by a steady decline for those that reached college age after the coup. Panel (b) shows the reduced-form relationship between the cohort of the parent and college enrolment by the child. We observe a clear decline for children with parents that reached college age after the coup. The plot shows that a child with a parent reaching age 21 in 1981 is around 7pp less likely to go to college (roughly 12% of the sample mean) than a child of the same age with a parent born in 1972, eight years before. However, the latter individual is just as likely to attend college as a third individual of the same age with a parent born in 1965, seven years before.

To further understand at what stage in the educational process do children with parents in the affected cohorts lag behind, in column 6 of Table 8 we include an additional dummy capturing whether the respondent (i.e. child) reports having graduated from high school. As expected, this additional control does absorb some of the variation in college enrolment, but its inclusion only leads to a 12% reduction in our IV estimate (28 pp). Hence, most of the effect of parental college enrollment on their children's educational attainment materializes after the children graduate from high school. To better understand the temporal pattern of the effect, Figure 9 plots the IV estimates from a specification analogous to column 5 of Table 8 using achievement beyond each grade in primary and secondary as the dependent variable. We observe no effect up to primary completion,

which is consistent with primary education being mandatory in Chile and beyond the control of parents. Once primary is completed, we find that children with a parent with some college are more likely to move beyond all levels in secondary. In particular, the probability of high school completion is about 7 pp higher for these children.

7.3 Evidence on mechanisms

The results in the previous sections show that college enrollment has positive effects on several economic and non-economic outcomes. Unfortunately, most of these results rely on information from the 1992 and 2002 censuses, preventing us from directly testing for the possible role of these outcomes as mediating factors in the intergenerational transmission on college enrollment. In this section, we study two possible mechanisms that can actually be tested (to some extent) with the data from the 2017 census. The first one is assortative matching by people with a college education. The second one is changes in fertility and caring of children.

To study the role of assortative matching, we focus on the set of children of the household head in our sample of linked parents and children. For these observations we can easily identify the spouse of the parent and his or her educational attainment. The dependent variable in column1 of Appendix Table A22 is a dummy indicating whether there is a person in the household reported as the spouse or partner of the parent (i.e. household head). We find that parents in the affected cohorts are less likely to have a spouse. The IV estimate indicates that college enrollment increases the probability of a spouse in the household by 17 pp. Column 2 then asks whether, conditional on a partner being present, that partner has any college education. We find strong evidence of positive assortative matching. Parents in the affected cohorts are substantially less likely to have a spouse with college and the IV estimate indicates that college enrollment increases the probability of a spouse with any college by 40 pp. This is a very large effect, relative to the sample mean of 21%.

Column 3 re-estimates the intergenerational effect of college enrollment (only for the sample of children of the household head), including an additional control for whether we observe a spouse of the parent. The results change very little compared to the estimates in column 5 of Table 8. In column 4, we restrict the sample to children of heads for which we can observe the spouse. The magnitude of the IV coefficient drops somewhat to 0.29. Finally, column 5 replicates the analysis for the same sample as in column 4, but including as an additional control a dummy for whether the spouse of the parent has any college. The magnitude of the intergenerational correlation in college enrollment drops 22%. This indicates that assortative matching of people with college plays an important role in the propensity of their children to enroll in college.

Finally, in Appendix Table A23 we study the role of fertility. For this part of the analysis, we restrict the sample to those children that we are able to connect to their mother, as detailed information on fertility is only available for women in the census. Column 1 replicates the analysis of total number of children for this smaller sample of mothers. The estimates are almost identical

to the ones reported in Table 7. Column 2 replicates the analysis on child survival and fails to find evidence of change in the trend for the post-coup cohorts in this sample.

Column 3 examines the effect of college enrollment on their age at the time the child was born. For this regression, we drop the age (of child) fixed effects to avoid perfect multicollinearity with the cohort trend. We find that age at birth is lower for women in the affected cohorts, with an IV estimate for the effect of college enrollment of 0.7 years. However, the estimate is imprecise and not statistically significant.

Columns 4-7 introduce additional controls related to fertility to examine their influence on the estimated effect of maternal college enrollment on the enrollment of their children. Controlling for the total number of children by the mother leads to a 7% decrease in the elasticity (i.e. relative to the results in column 1 of table A20). This suggests that the intergenerational transmission of college enrollment is only weakly driven by women without college having more children in a simple quantity-quality trade-off. Column 5 further shows that controlling for whether the mother reports having lost a child is largely inconsequential. In column 6, we replace the age fixed effects from our most-preferred specification with age-at-birth fixed effects. This leads to a 36% reduction in the size of the IV estimate. Column 7 verifies that the results look fairly similar if we replace the cohort trend with age fixed effects. Taken together, these results indicate that lower age-at-birth on the part of mothers without college also plays an important role in the lower propensity to enroll in college of their children.

8 Conclusion

This paper studies the restrictions on access to higher education put in place by the Pinochet regime and its effects on the affected population. Exploiting cross-cohort variation in the age of college enrolment around the time of the military coup that brought Pinochet to power, we show that the cohorts that reached college age shortly after the coup experienced a sharp decline in college enrolment. This reduction in educational attainment had profound effects that chased these cohorts for the rest of their lives. Higher unemployment, worse occupations, and lower incomes are just some of the economic consequences that we document. Our findings show that small variation in birth year, through its effect on college enrolment, substantially affected people's ability to reach the top of the socioeconomic ladder, in terms of both income and wealth. The military coup took place at a time of growing female empowerment in the labor market. Unfortunately, the women who failed to go to college because of the military dictatorship, were substantially less likely to remain in the labor force, to have a paid job (rather than engage in domestic work or unpaid work with a relative), or to have a high-skill white collar occupation than the men who were equally affected.

We draw two main lessons from these findings. The first one concerns the relationship between

political regimes, higher education and economic prosperity and equality. As Figure 1 shows, the negative correlation between non-democracy and enrolment in tertiary education is not a purely Chilean phenomenon. What our findings show is that the political impetus that drives authoritarian regimes to crack down on higher education can have long-lasting economic consequences. On the one hand, doing so reduces the human capital of those affected and their children, thereby plausibly reducing productivity growth and long-term economic growth. On the other hand, insofar as higher education is the 'great equalizer', it impedes social mobility and aggravates inequality. Whether those left behind by lack of opportunities in higher education become increasingly mobilized against the regime is a question that warrants further research.

The second lesson concerns precisely the role of higher education in social mobility. Our findings show that in a developing country, such as the Chile of the 1970s, growing college enrolment served as a platform that propelled people into better jobs and higher incomes. The collapse in vacancies after the military coup effectively prevented an entire generation of Chileans, irrespective of background, from reaching the top of the socioeconomic pyramid. Education is the great equalizer. Our results further show that primary completion is unaffected by parental educational attainment, meaning that the playing field is level up to full primary (leaving aside differences in quality). However, as early as the first year of secondary, parental education starts to make a difference. Hence, policies that keep youth from disadvantaged backgrounds from dropping out before completing secondary education are also still needed.

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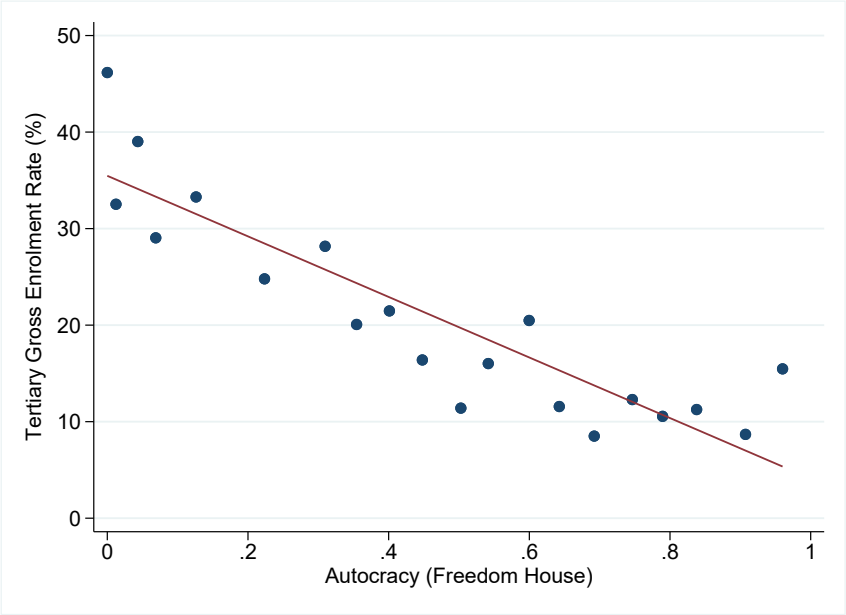
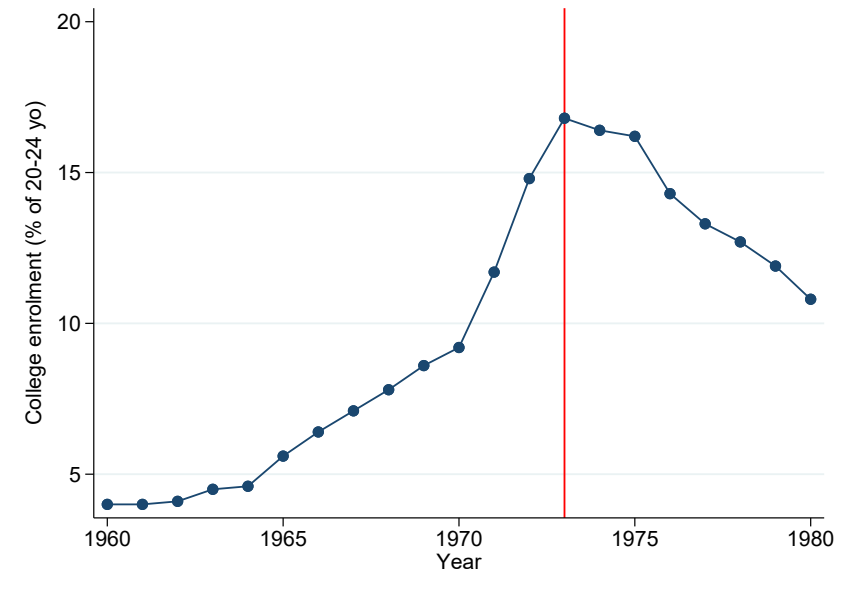
Figure 1: Autocracy and the Tertiary Enrolment Rate



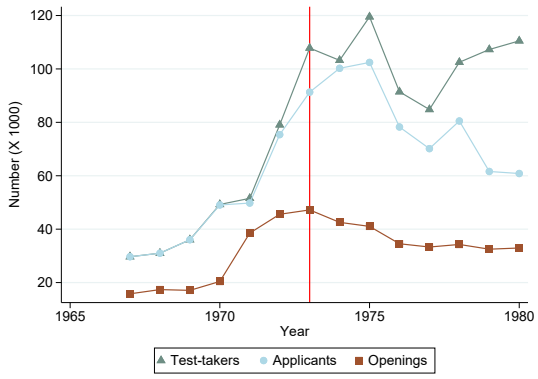
Notes: Figure shows a binned scatterplot of gross enrolment in tertiary education (i.e. number of students in higher education divided by population in the 5-year age group starting from the official secondary school graduation age) against the Freedom in the World index produced by Freedom House (normalized to range from zero to one, with higher values corresponding to more authoritarian regimes). Averages by country for the period 1972-2016.

Figure 2: Gross College Enrolment Rate

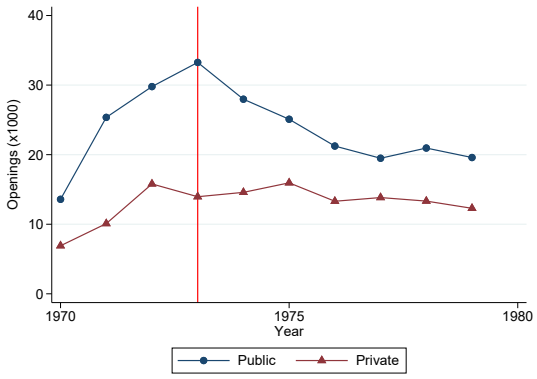


Notes: Figure shows the gross college enrolment rate, defined as the total number of college students divided by the population with ages 20-24. The solid line shows the year of the military coup.

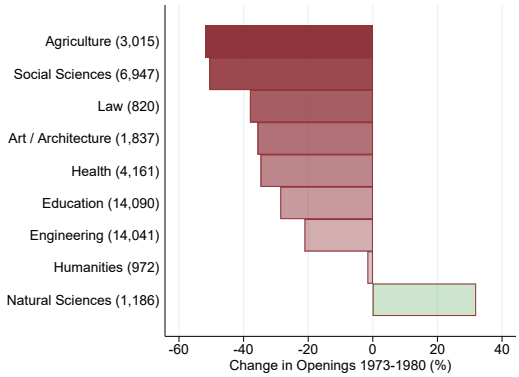
Figure 3: Demand and supply of college openings



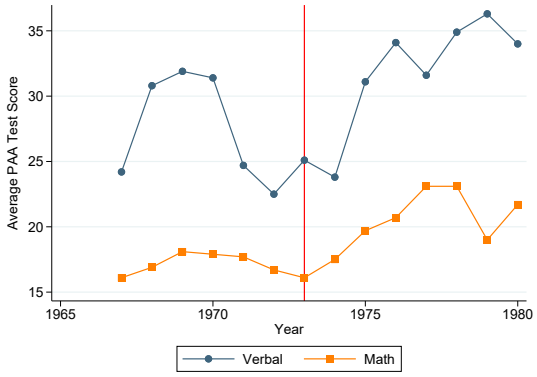
(a) Total Applicants and Openings



(b) Openings: Public vs Private Universities



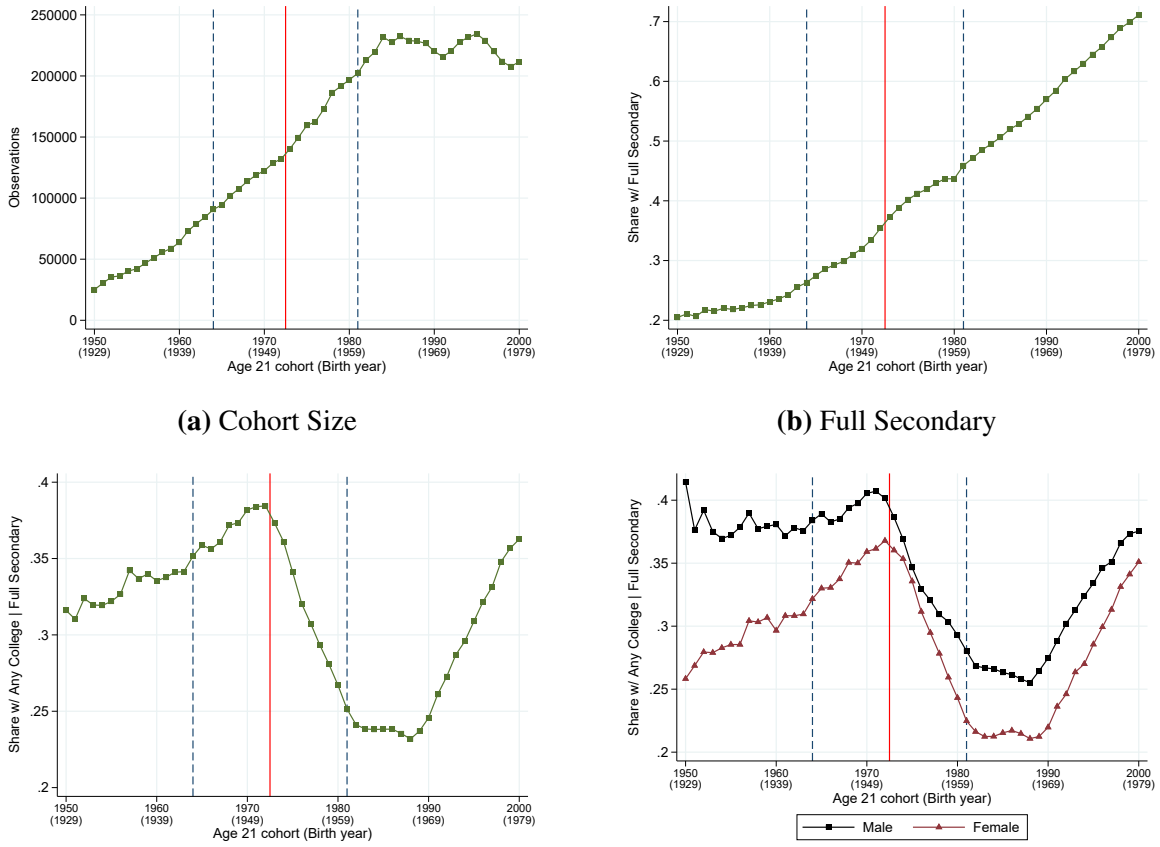
(c) Change in Openings by Field



(d) PAA Test Scores

Notes: Panel (a) shows the yearly number of people that took the PAA test for college admission, the number of applicants to college and the number of openings for incoming students offered by the universities. Panel (b) shows the number of openings separately for the six private universities and for the two public ones. Panel (c) shows the change in openings by field, using UNESCO categories, between 1973 and 1980. The number in parenthesis corresponds to the number of openings per field in 1973. Panel (d) shows the raw average in the verbal and math components of the PAA test. Sources: Universidad de Chile (1972, 2011); PIIE (1984); Brunner (1984); Echeverría (1980); Díaz and Himmel (1985).

Figure 4: The military coup and college enrolment



(a) Cohort Size

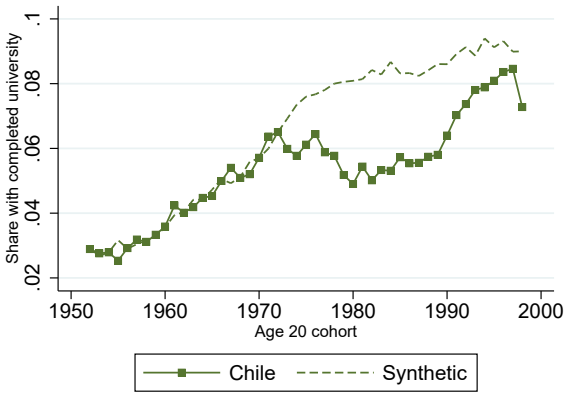
(b) Full Secondary

(c) Any College | Full Secondary

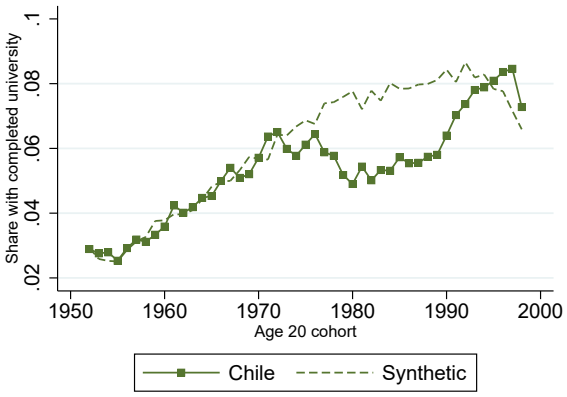
(d) Any College | Full Secondary (gender)

Notes: Panel (a) shows the total number of people per cohort (normalized to age 21) in the 2017 population census. Panel (b) shows the share of census respondents per cohort that report full secondary or higher, while panel (c) shows the share of people with complete secondary that report any college. Panel (d) shows the same information as panel (c), but disaggregated by gender. The solid line shows the year of the military coup. Dashed lines show the start (1964) and end date (1981) of the sample of cohorts used in the analysis.

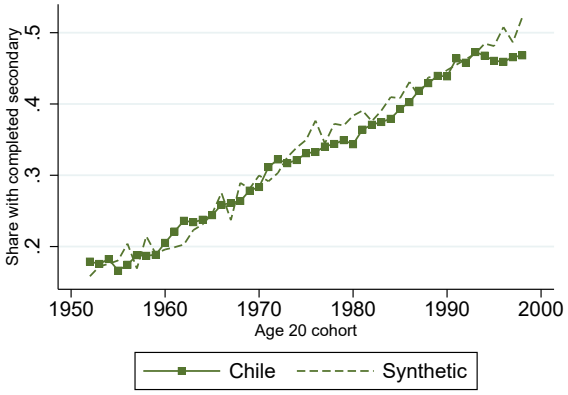
Figure 5: Synthetic control



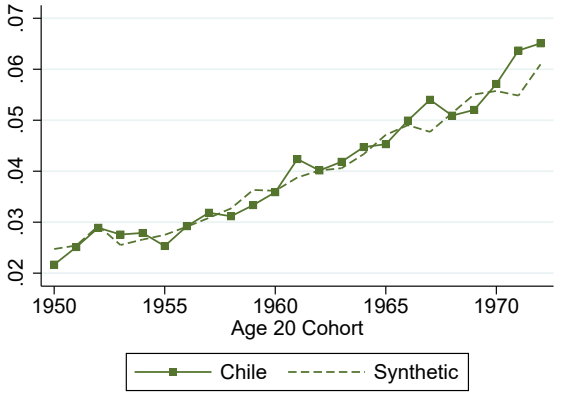
(a) Without controls



(b) With controls



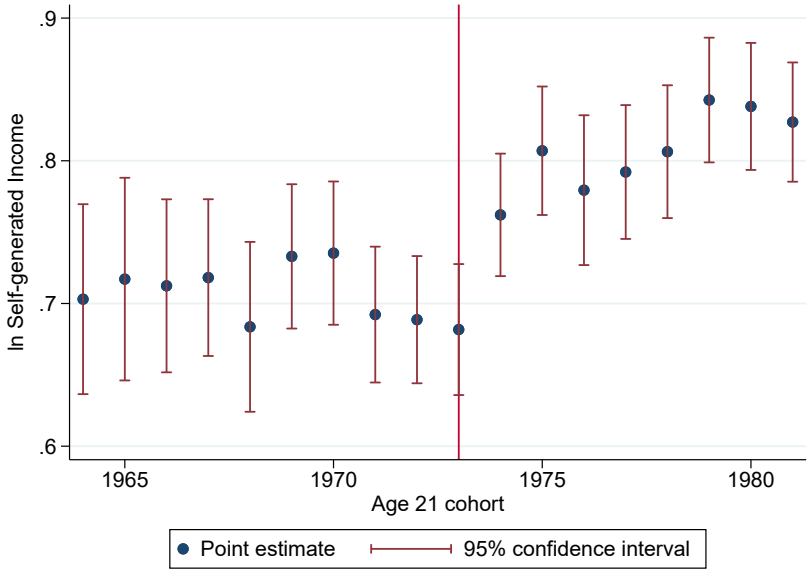
(c) Secondary education



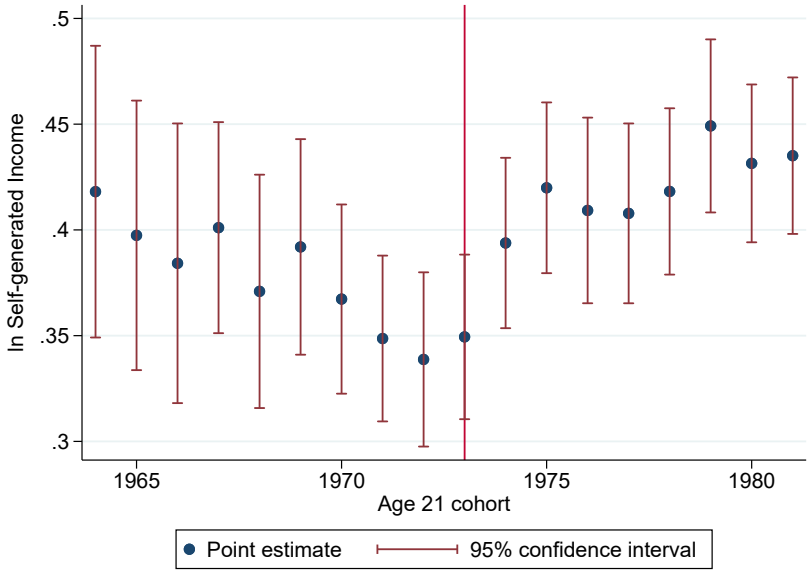
(d) Placebo

Note: Panels show observed rates of educational attainment by cohort in the 2002 population census (solid line) and counterfactuals from a synthetic control (dashed line). See the test for additional information on sample construction and estimation. The outcome in panels (a), (b) and (d) is the share of people with full college education, while in panel (c) is the share of people with full secondary education. Panel (b) includes the share of people with ages 18-65, the share of women and the share of people with secondary education as additional controls. Panel (d) uses 1960 as a placebo treatment date for the military coup.

Figure 6: Cohort-specific Estimates of the College Premium



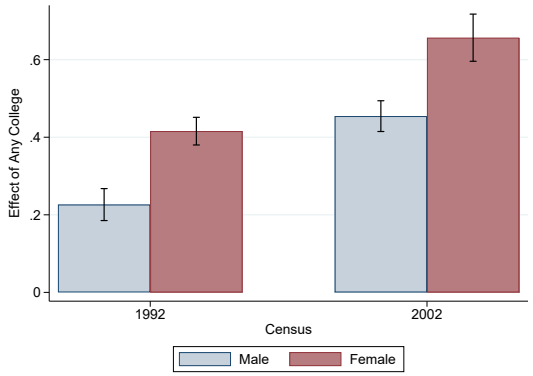
(a) Without occupation fixed effects



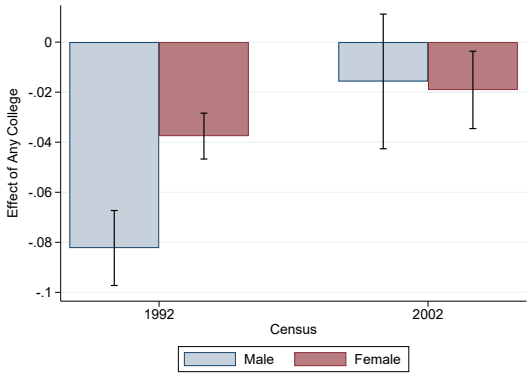
(b) With Occupation fixed effects

Notes: Both panels show results from a regression of log real self-generated income on a full set of interactions of a dummy for any college education with cohort fixed effects. Sample includes all respondents in the CASEN survey from cohorts reaching age 21 between 1964 and 1981 (both inclusive), but is restricted to respondents reporting four or more years of secondary education. Regressions include county of residence x gender, survey year and age fixed effects. Panel (b) also includes occupation fixed effects. Standard errors are clustered by county of residence. N=118,301 and 100,742 respectively.

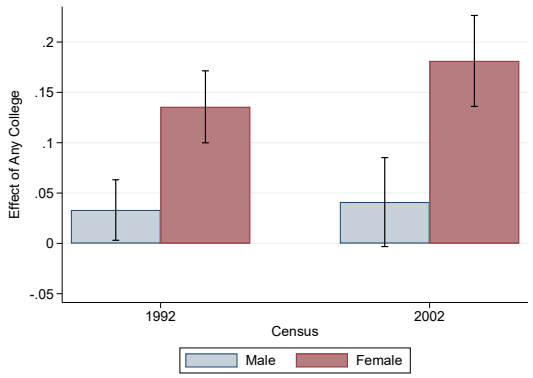
Figure 7: Heterogeneous Labor Market Effects of College Education by gender



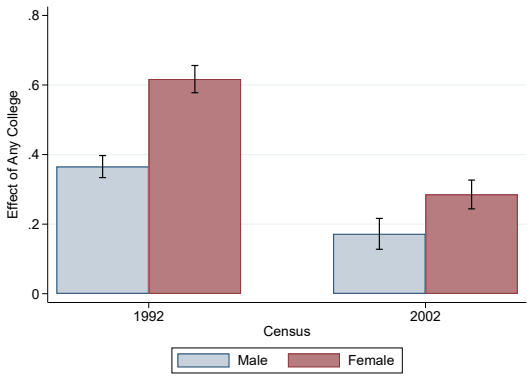
(a) Labor Force Participation



(b) Unemployment



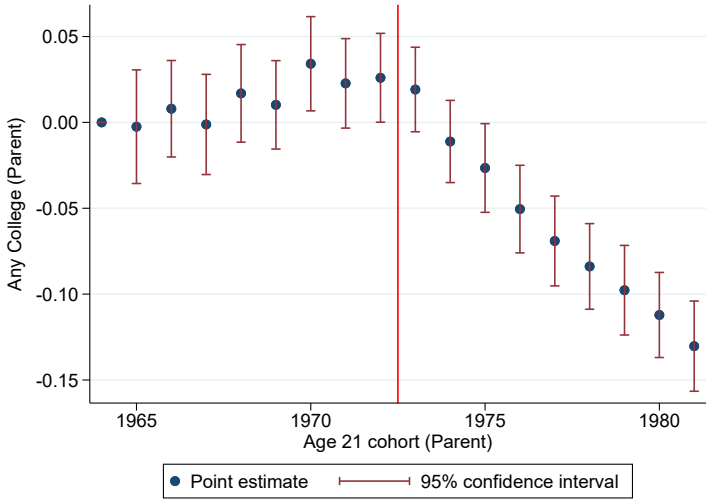
(c) Salaried Employment



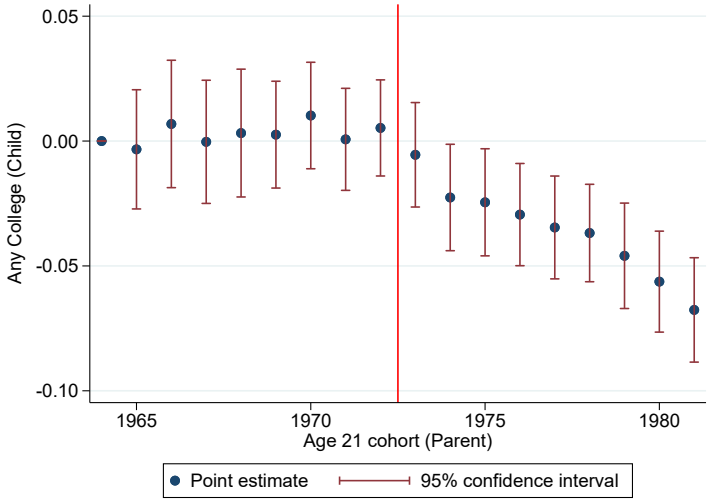
(d) White-Collar, High-Skill Occupation

Notes: Each pair of bars (male and female) shows IV gender-specific estimates of the effect of Any College on the variable in the caption. Sample includes all respondents from the respective census from cohorts reaching age 21 between 1964 and 1981 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). The gender-specific interaction term 'Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is used as the respective excluded instrument for any college education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "$\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is a dummy for cohorts that reached age 21 on or after 1973. The respective cohort-gender trends are included instruments. All regressions include county x gender fixed effects. Panel (b) includes labor-force participation fixed effects. Standard errors clustered by county of birth in parentheses. Full results available in the online appendix.

Figure 8: Non-Parametric Estimates of Inter-generational Transmission of College Education



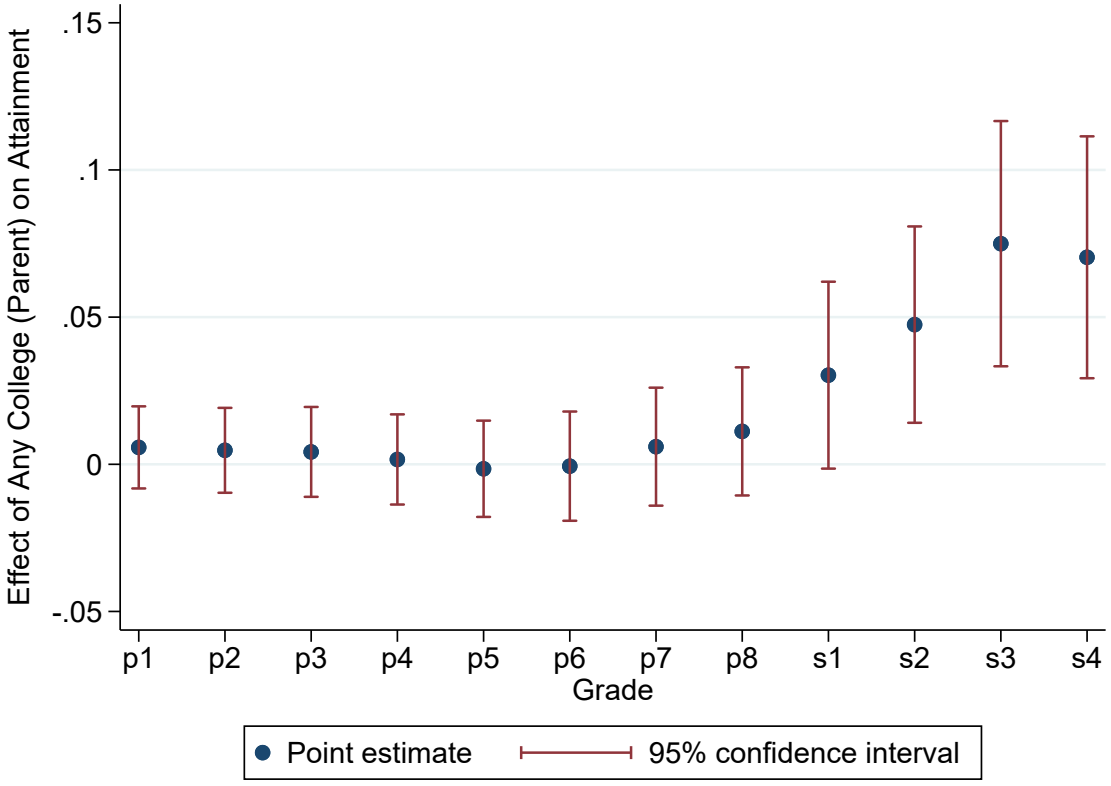
(a) Any College (Parent)



(b) Any College (Child)

Notes: Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (I) reached age 21 between 1964 and 1981 (both years inclusive) and (II) that reported full secondary education. Possible parent-child linkages include: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. Panel (a) shows results from a regression of Any College for the parent on a full set of parent cohort dummies. Panel (b) shows the equivalent regression using Any College for the child as dependent variable. Both regressions include county of birth x gender, parent's gender x (child) gender, age and relationship to household head fixed effects. Standard errors clustered by county of birth.

Figure 9: Educational Attainment of Children by Grade (Census 2017)



Notes: Each marker corresponds to a separate regression. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (I) reached age 21 between 1964 and 1981 (both years inclusive) and (II) that reported full secondary education. Possible parent-child linkages include: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. The dependent variable in each regression is a dummy equal to one if the child has education at or above the respective grade (primary 1-8, secondary 1-4). Each marker is the IV estimate of the effect of any college education by the parent. The excluded instrument is "Age 21 Parent x $\mathbb{1}$(Age 21 Parent $\geq$ 1973)", the interaction of a continuous variable indicating the year at which the HH head reached 21 years of age, normalized to zero in 1972, with a dummy for HH heads that reached age 21 on or after 1973. The first-stage Kleibergen-Paap F-Statistic is 308. The baseline trend is an included instrument in all regressions. All regressions include county of birth x gender, parent's gender x (child) gender, age and relationship to household head fixed effects. Standard errors clustered by county of birth.

Table 1: Educational attainment: Higher education (Census 2017)

Dependent variable:	Enrolment			Completion		
	College	Technical	Higher	College	Technical	Higher
	(1)	(2)	(3)	(4)	(5)	(6)
[a] Yr Age 21	0.005***	0.0001	0.005***	0.002***	-0.0001	0.002***
	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0004)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.019***	0.004***	-0.016***	-0.012***	0.004***	-0.009***
	(0.0008)	(0.0003)	(0.0008)	(0.0006)	(0.0003)	(0.0006)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	962,039	962,039	962,039	962,039	962,039	962,039
R-squared	0.041	0.007	0.038	0.031	0.007	0.027
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000
Mean of dep. var	0.322	0.118	0.440	0.266	0.109	0.375

Notes: Dependent variable in the header. Sample includes all respondents of the 2017 census from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting full secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2: Labor market outcomes (Census 1992, 2002)

Census year:	In Labor Force		Seeking Work		Wage-Earner		High-skill, white-collar	
	1992	2002	1992	2002	1992	2002	1992	2002
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[a] Yr Age 21	0.008***	0.017***	-0.001***	0.0001	0.007***	0.008***	0.004***	-0.004***
	(0.0003)	(0.0004)	(0.0001)	(0.0001)	(0.0004)	(0.0003)	(0.0005)	(0.0004)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.012***	-0.014***	0.002***	0.0004**	-0.003***	-0.003***	-0.017***	-0.006***
	(0.0006)	(0.0004)	(0.0001)	(0.0002)	(0.0005)	(0.0004)	(0.0008)	(0.0005)
IV: Any College	0.333***	0.568***	-0.060***	-0.020**	0.080***	0.107***	0.476***	0.221***
	(0.017)	(0.021)	(0.004)	(0.008)	(0.013)	(0.017)	(0.015)	(0.016)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
In labor force FE	No	No	Yes	Yes	No	No	No	No
Observations	1,024,570	1,192,851	1,024,570	1,192,851	773,922	907,050	770,652	872,783
R-squared (RF)	0.200	0.133	0.013	0.024	0.017	0.016	0.032	0.022
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
First-stage KP F-stat	2733.6	1079.8	2623.9	930.3	2120.2	761.5	2094.1	874.9
Mean of dep. var	0.758	0.762	0.033	0.063	0.750	0.677	0.431	0.596

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Columns 3-4 include labor-force participation fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Reported income (CASEN 1990-2015)

Dependent variable (log income):	Main occupation	All work	Self-generated	Total
	(1)	(2)	(3)	(4)
		Panel A: Linear Trend		
[a] Yr Age 21	0.001	0.015***	0.013***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.015***	-0.024***	-0.022***	-0.020***
	(0.002)	(0.003)	(0.002)	(0.002)
IV: Any College	0.554***	0.920***	0.846***	0.761***
	(0.088)	(0.117)	(0.093)	(0.090)
		Panel B: Age Fixed effects		
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.006**	-0.005	-0.005**	-0.006**
	(0.003)	(0.003)	(0.003)	(0.003)
IV: Any College	0.207**	0.200	0.201**	0.232**
	(0.096)	(0.131)	(0.101)	(0.099)
County x gender FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Age FE (panel B)	Yes	Yes	Yes	Yes
Observations	99,712	93,666	118,301	118,301
R-squared (RF - panel A)	0.165	0.146	0.152	0.153
R-squared (RF - panel B)	0.169	0.156	0.160	0.160
p-value a+b=0 (panel A)	0.000	0.000	0.000	0.000
First-stage KP F-stat (panel A)	374.4	323.9	422.3	422.3
First-stage KP F-stat (panel B)	358.2	287.3	391.6	391.6
Mean of dependent variable (level)	674,304	712,472	737,297	740,530

Notes: Dependent variable in the header. Real income deflated using yearly CPI. Sample includes all respondents in the CASEN survey from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of residence x gender and survey year fixed effects. In panel B, the cohort trend is replace by a full set of age fixed effects. Standard errors clustered by county of residence in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Housing wealth quintiles (Census 1992)

Dependent variable: Housing wealth quintile (dummy)	Q5 (highest)	Q4	Q3	Q2	Q1 (lowest)
	(1)	(2)	(3)	(4)	(5)
[a] Yr Age 21	-0.0017***	-0.0002	0.0007***	0.0008***	0.0004***
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0001)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.0128***	0.0041***	0.0042***	0.0035***	0.0009***
	(0.0007)	(0.0006)	(0.0004)	(0.0003)	(0.0001)
IV: Any College	0.348***	-0.113***	-0.115***	-0.097***	-0.024***
	(0.021)	(0.017)	(0.010)	(0.008)	(0.004)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes
Observations	1,007,957	1,007,957	1,007,957	1,007,957	1,007,957
R-squared	0.114	0.013	0.032	0.052	0.050
p-value a+b=0	0.000	0.000	0.000	0.000	0.000
First-stage KP F-stat	2859.4	2859.4	2859.4	2859.4	2859.4
Mean of dependent variable	0.50	0.25	0.15	0.08	0.02

Notes: Dependent variable in the header. Sample includes all respondents in the 1992 census from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Marital status (Census 1992, 2002)

Source:	Census 1992				Census 2002			
Dependent variable:	Ever married	Current Status			Ever married	Current Status		
		Married	Widowed	Separated		Married	Widowed	Separated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[a] Yr Age 21	-0.0002	0.0045***	-0.0025***	-0.0020***	-0.0008***	0.0045***	-0.0051***	0.0006***
	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.0093***	-0.0011***	0.0018***	-0.0007***	-0.0019***	-0.0006***	0.0031***	-0.0025***
	(0.0003)	(0.0003)	(0.0001)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
IV: Any College	0.257***	0.030***	-0.050***	0.020***	0.076***	0.023***	-0.124***	0.100***
	(0.009)	(0.007)	(0.003)	(0.007)	(0.009)	(0.009)	(0.007)	(0.009)
Observations	1,024,570	1,024,570	1,024,570	1,024,570	1,192,851	1,192,851	1,192,851	1,192,851
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
First-stage KP F-stat	2733.6	2783.3	2783.3	2783.3	1079.8	1083.8	1083.8	1083.8
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ever married FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
R-squared	0.025	0.630	0.013	0.024	0.019	0.444	0.027	0.029
Mean of dependent variable (Panel A)	0.853	0.787	0.009	0.058	0.874	0.749	0.0261	0.0993

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Columns 2-4, 6-8 include an additional dummy for ever married. Standard errors clustered by county of birth in parentheses. N= 958,588 and 1,118,137, respectively. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Position in the household (Census 1992, 2002, 2017)

Source:	Census 1992			Census 2002			Census 2017		
	Head/spouse	Child	Parent	Head/spouse	Child	Parent	Head/spouse	Child	Parent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
[a] Yr Age 21	-0.002***	0.004***	-0.001***	-0.002***	0.003***	-0.002***	0.002***	0.002***	-0.004***
	(0.0003)	(0.0002)	(0.00003)	(0.0004)	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0002)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.014***	0.009***	0.001***	-0.004***	0.002***	0.001***	-0.005***	0.003***	0.002***
	(0.0005)	(0.0003)	(0.00003)	(0.0005)	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0002)
IV: Any College	0.388***	-0.258***	-0.017***	0.149***	-0.092***	-0.048***	0.280***	-0.145***	-0.085***
	(0.014)	(0.010)	(0.001)	(0.017)	(0.009)	(0.003)	(0.015)	(0.008)	(0.008)
R-squared	0.029	0.031	0.004	0.050	0.015	0.009	0.005	0.011	0.015
County x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean of dependent variable	0.800	0.134	0.001	0.858	0.071	0.007	0.878	0.032	0.034
First stage KP F-stat	2732.5	2732.5	2732.5	1079.8	1079.8	1079.8	630.5	630.5	630.5

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. N= 1,023,353 and 1,192,851 and 1,036,105, respectively. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Fertility (Census 1992, 2002, 2017)

	Census 1992			Census 2002			Census 2017		
	Children	Share alive		Children	Share alive		Children	Share alive	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
[a] Yr Age 21	-0.034***	0.002***	0.001***	-0.034***	0.002***	0.001***	-0.029***	0.002***	0.002***
	(0.002)	(0.0001)	(0.0001)	(0.002)	(0.0001)	(0.0001)	(0.002)	(0.0001)	(0.0001)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.022***	-0.001***	-0.001***	0.014***	-0.001***	-0.0003**	0.014***	-0.001***	-0.0004***
	(0.002)	(0.0001)	(0.0001)	(0.002)	(0.0001)	(0.0001)	(0.002)	(0.0001)	(0.0001)
IV: Any College	0.533***	0.020***	0.022***	-0.513***	0.021***	0.010**	-0.642***	0.032***	0.020***
	(0.060)	(0.003)	(0.003)	(0.073)	(0.005)	(0.005)	(0.092)	(0.006)	(0.006)
Observations	429,030	429,030	429,029	503,943	503,943	503,932	470,428	470,428	470,425
R-squared	0.048	0.006	0.029	0.013	0.005	0.097	0.011	0.006	0.043
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean of dep. var	2.420	0.983	0.983	2.614	0.978	0.978	2.618	0.971	0.971
F-stat	2136.7	2136.7	2167.1	976.3	976.3	1022.9	602.1	602.1	620.7
Total children FE	-	No	Yes	-	No	Yes	-	No	Yes

Notes: Dependent variable in the header. Sample includes all female census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Columns 2-3 and 5-6 include total children (births) fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Educational attainment of children (Census 2017)

	(1)	(2)	(3)	(4)	(5)	(6)
	PANEL A: First Stage - Dependent variable: Any College (Parent)					
[a] Yr Age 21 Parent	0.006***	0.006***	0.006***	0.006***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
[b] Yr Age 21 Parent	-0.021***	-0.021***	-0.021***	-0.020***	-0.022***	-0.021***
x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	PANEL B: Reduced form - Dependent variable: Any College					
[a] Yr Age 21 Parent	0.004***	0.004***	0.004***	0.004***	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
[b] Yr Age 21 Parent	-0.005***	-0.005***	-0.005***	-0.005***	-0.007***	-0.006***
x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	PANEL C: IV - Dependent variable: Any College					
Any College (Parent)	0.257***	0.257***	0.258***	0.254***	0.320***	0.283***
	(0.058)	(0.058)	(0.058)	(0.058)	(0.052)	(0.050)
	PANEL D: OLS - Dependent variable: Any College					
Any College (Parent)	0.274***	0.272***	0.272***	0.272***	0.262***	0.243***
	(0.0040)	(0.0041)	(0.0041)	(0.0041)	(0.0043)	(0.0040)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Parent gender FE	No	Yes	No	No	No	No
Parent gender x gender FE	No	No	Yes	Yes	Yes	Yes
Relationship to HH head FE	No	No	No	Yes	Yes	Yes
Age FE	No	No	No	No	Yes	Yes
Full secondary FE	No	No	No	No	No	Yes
Observations	233,123	233,123	233,123	233,123	233,123	233,123
R-squared (panel A)	0.085	0.087	0.087	0.088	0.095	0.099
R-squared (panel B)	0.044	0.045	0.045	0.046	0.063	0.132
R-squared (panel D)	0.104	0.105	0.105	0.105	0.118	0.178
p-value a+b=0 (panel A)	0.000	0.000	0.000	0.000	0.000	0.000
p-value a+b=0 (panel B)	0.000	0.002	0.002	0.001	0.000	0.000
Mean of dep. variable (panel A)	0.309	0.309	0.309	0.309	0.309	0.309
Mean of dep. variable (Panels B-D)	0.582	0.582	0.582	0.582	0.582	0.582
First-stage KP F-stat (Panel C)	291.8	289.0	289.3	281.8	308.1	310.3

Notes: Dependent variable in the header of each panel. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (I) was born between 1943 and 1960 (both years inclusive) and (II) that reported full secondary education. Possible parent-child linkages include: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. "Yr Age 21 Parent" is a continuous variable indicating the year at which the parent reached 21 years of age, normalized to zero in 1972. "Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)" is the interaction of this variable with a dummy for parents that reached age 21 on or after 1973. In panel C, the interaction term is used as excluded instrument for any college education by the Parent (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Columns 2 adds parent's gender fixed effects. Column 3 includes parent's gender x (child) gender fixed effects instead. Column 4 includes fixed effects for each possible relationship to the head of the household, based on the linkages above. Column 5 adds age (of child) fixed effects, and columns 6 adds a dummy for whether the children completed secondary. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX (for online publication)

Appendix A Additional information on data sources

A.1 Censuses and surveys

We rely on the 1992, 2002, and 2017 censuses. These were *de facto population census* that happened in days declared national holidays to allow people to remain in their homes waiting for the enumerators. Among the people present during the census day, we only consider the people who was born in Chile, and we identify the cohort of birth using the respondents' age. We complement the censuses with a repeated cross-section of the National Socioeconomic Characterization Survey CASEN. This survey has been conducted biannually by the Ministry of Planning since 1987, and it includes detailed information on the labor market of the interviewed population.

A.2 Enrolment and seats

In Chile, students apply to institution-degree pairs through a centralized application authority, and admission into degrees is determined based on a deference acceptance algorithm that considers the number of seats available in a given institution-degree pair and the ranking of the student in a national college entrance exam (i.e., similar to SAT in USA).[29] Data on the aggregate number of available seats from 1967 onwards comes from the archival records held at the dependencies of the CRUCH. Data on test takers was digitized from hard copies of published application and wait-list announcements stored in the Biblioteca Nacional de Chile, and it includes all admitted students as well as a list of marginal rejected students that is typically equal in length to the list of admits.

A.3 Other sources

This project uses additional data sources, including: Freedom House, the World Bank, and the Integrated Public Use Micro-data Series (IPUMS). Data from Freedom House and World Bank is used to look at the across-country relationship between enrolment in tertiary education and authoritarianism, while data on IPUMS is used for the synthetic control analysis. In particular, we use data of 57 countries for which harmonized data is available (see Table A1 for details). Finally, we enrich our study with records obtained from Freedom-of-Information requests and previously published research (e.g., conscription, bilateral student flows at the tertiary level, average age at first- and last-year of college, etc.).

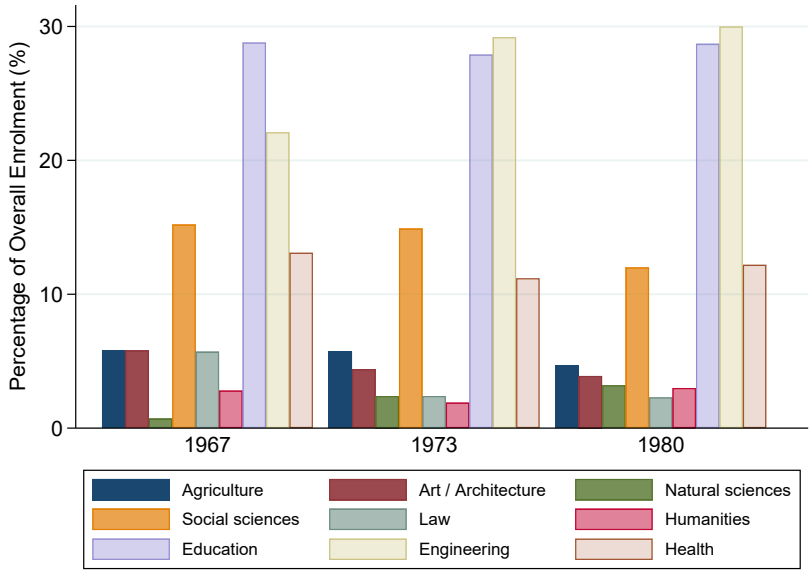
[29] Until the late 1990s, almost all college students in Chile attended one of the 25 (public and private) traditional universities belonging to the Universities of the Rectors' Council (CRUCH). Following their final year of high school, Chilean students take a standardized admissions exam, known as P.A.A. (Prueba de Aptitud Acadmica) before 2003, and as P.S.U. (Prueba de Seleccin Universitaria) afterwards.

Table A1: Countries and samples

Country	Last year of Census
Without dictatorship beetween 1950-1990	
Armenia	2011
Austria	2011
Bangladesh	2011
Benin	2013
Botswana	2011
Cambodia	2008
Canada	2011
China	2000
Costa Rica	2011
El Salvador	2007
Ethiopia	2007
France	2011
India	2009
Ireland	2011
Jamaica	2001
Kenya	2009
Liberia	2008
Malaysia	2000
Mexico	2015
Morocco	2004
Senegal	2002
Switzerland	2000
Ukraine	2001
United States	2015
Vietnam	2009
With dictatorship beetween 1950-1990	
Argentina	2010
Bolivia	2001
Brazil	2010
Burkina Faso	2006
Chile	2002
Colombia	2005
Dominican Republic	2010
Ecuador	2010
Egypt	2006
Fiji	2007
Ghana	2010
Greece	2011
Haiti	2003
Honduras	2001
Hungary	2011
Indonesia	2010
Jordan	2004
Mongolia	2000
Nicaragua	2005
Nigeria	2010
Panama	2010
Paraguay	2002
Peru	2007
Philippines	2010
Poland	2011
Portugal	2011
Romania	2011
South Africa	2011
Spain	2011
Thailand	2000
Turkey	2000
Uruguay	2011

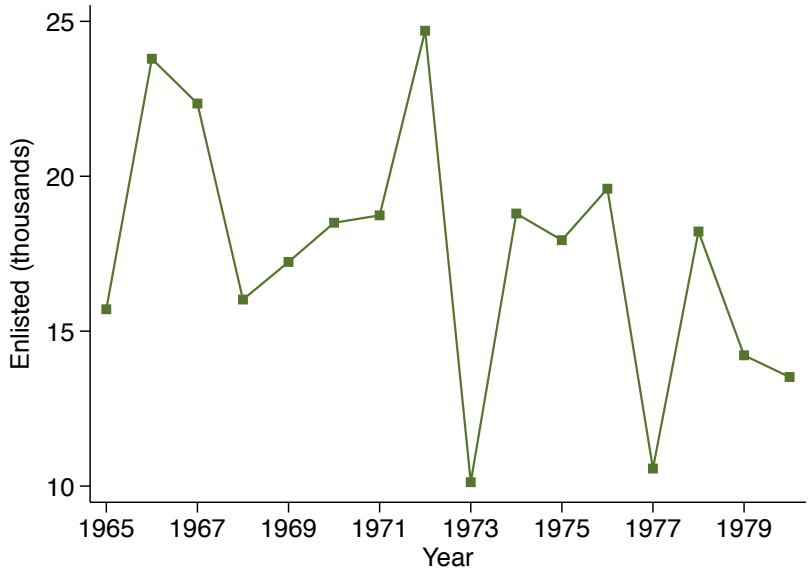
Appendix B Additional background figures

Figure A1: Share of students by Field of Study

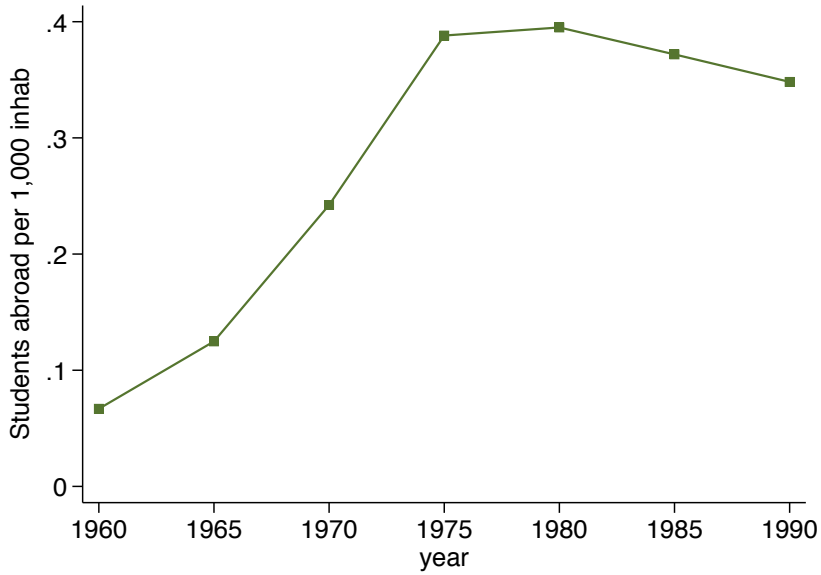


Notes: Figure shows the share of students enrolled in programs corresponding to different fields of study in 1967, 1973 and 1980. Classification corresponds to UNESCO categories.

Figure A2: Alternative mechanisms



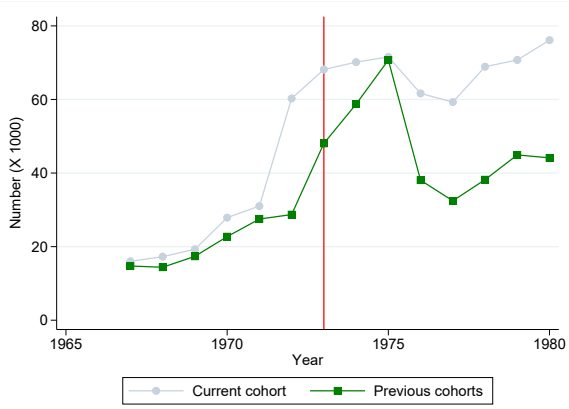
(a) Conscription



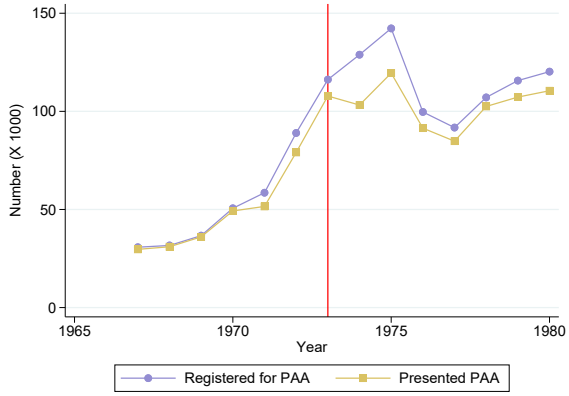
(b) Students abroad

Notes: Panel (a) shows the number of army conscripts per year. Panel (b) shows the number of Chilean students abroad (per 1,000 inhabitants). Sources: records of conscripts per year were obtained using the Chilean equivalent to a Freedom-of-Information-Act request and the number of students abroad come from Spilimbergo (2009).

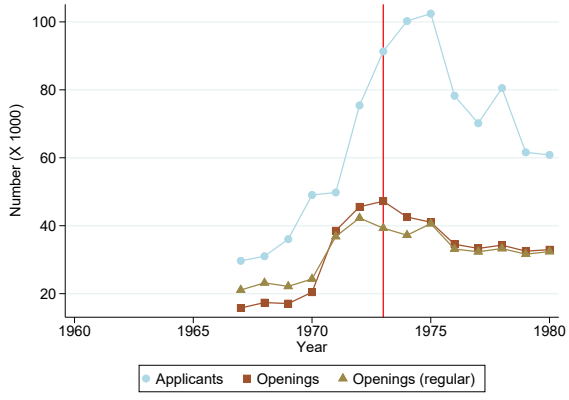
Figure A3: Further Evidence on Supply and Demand for College



(a) PAA Registration: Old vs Current Cohort



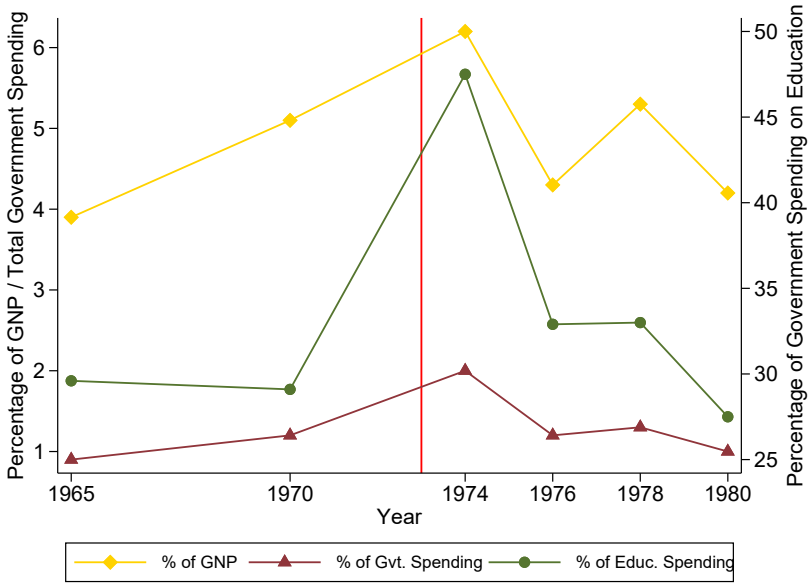
(b) PAA: Registration vs Test-taking



(c) Applications vs Openings: Alternative measure

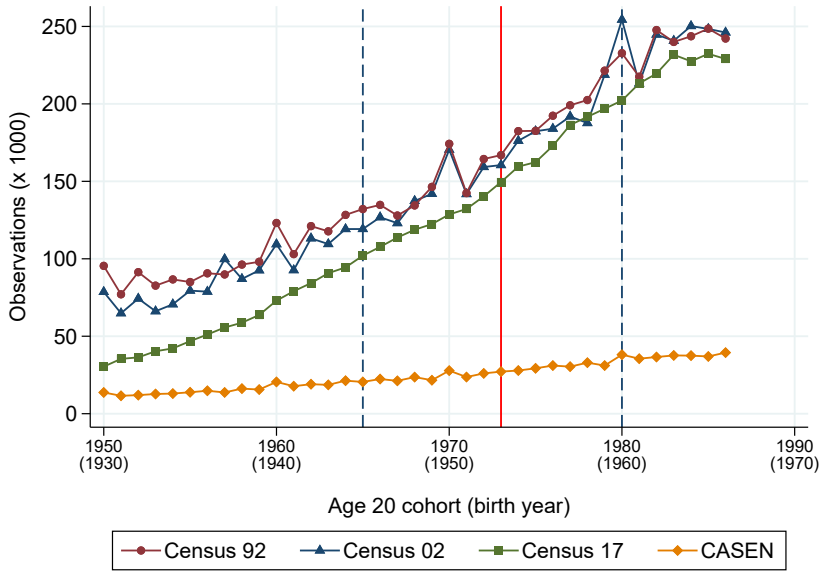
Notes: Panel (a) shows the number of students that registered to take the PAA test every year, disaggregated between graduates from secondary education from the same year and those from previous cohorts. Panel (b) shows the total numbered of students that registered to take the test and the number that effectively presented the exam. Panel (c) shows the number of applicants and openings per year, but includes an alternative measure of regular openings.

Figure A4: Government Spending on Higher Education

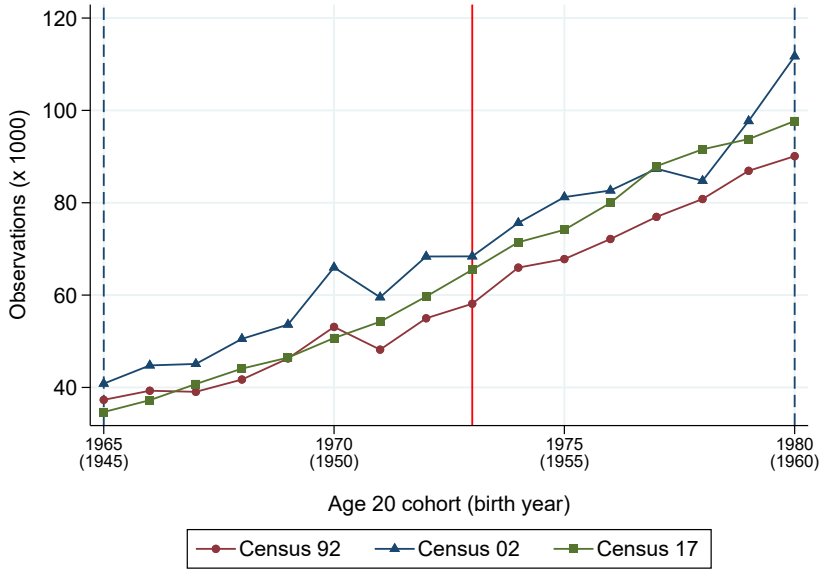


Notes: Figure shows government spending on higher education normalized in three different ways: as a percentage of Gross National Product (yellow line with diamond markers, left axis); as a percentage of total government spending (red line with triangular markers, left axis); as a percentage of government spending on education (green line with round markers, right axis). Source: Levy (1986).

Figure A5: Attrition and Manipulation of Reported Age



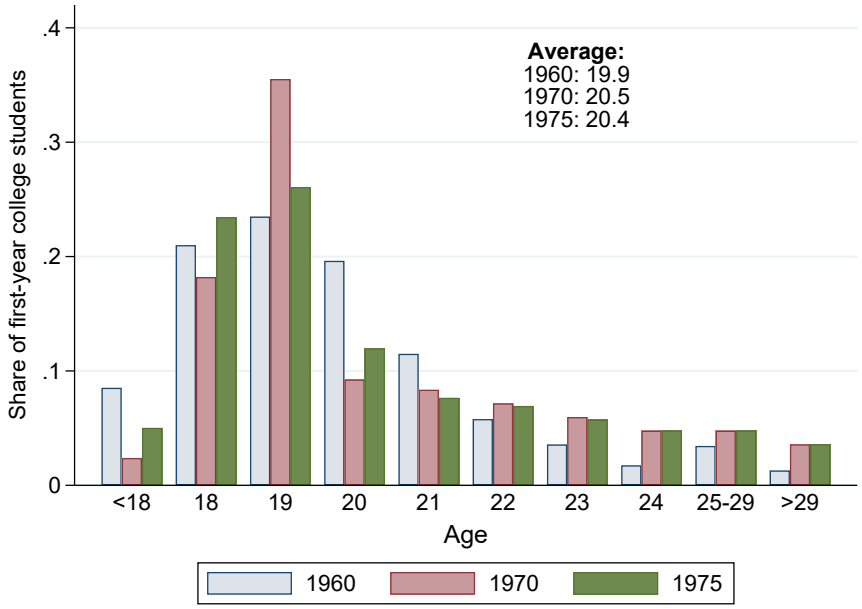
(a) Number of observations per cohort



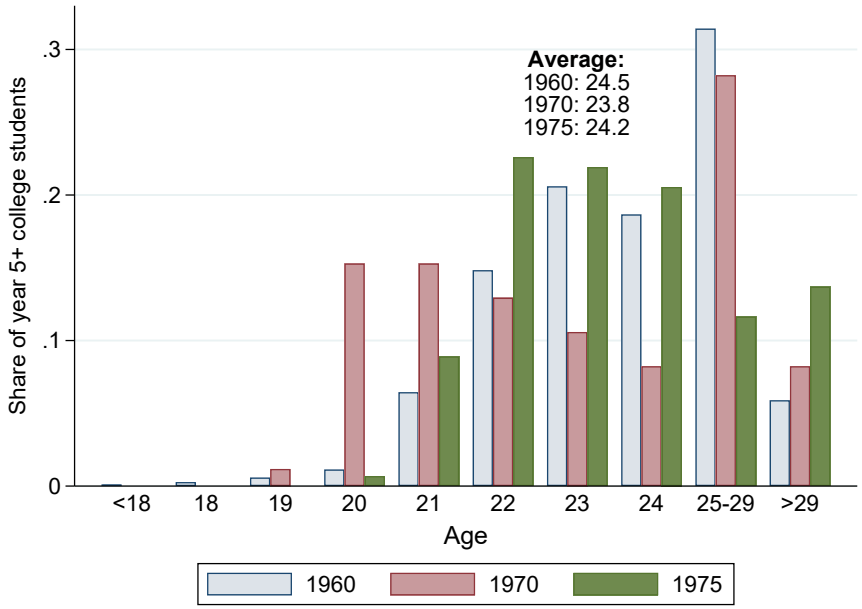
(b) Number of observations per cohort with 4+ years of high school

Notes: Panel (a) shows the number of observations per cohort in each data source, including the 1992, 2002 and 2017 population censuses and the CASEN household survey. Panel (b) shows for each of the censuses the number of people in each cohort that report at least four years of secondary education.

Figure A6: Age distribution of first- and last-year college students



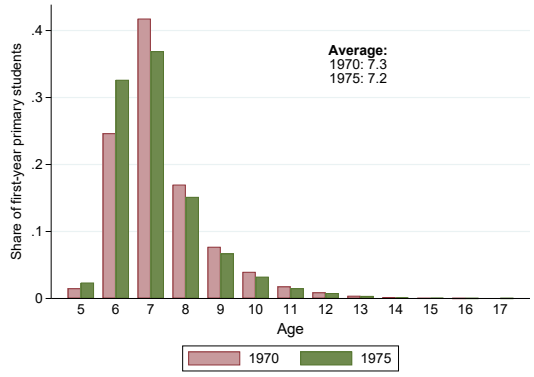
(a) Year 1



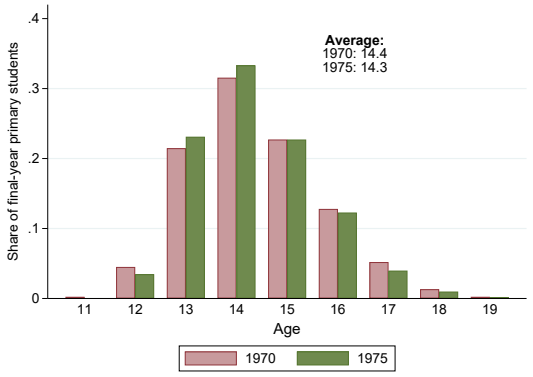
(b) Year 5+

Notes: Information for 1960 comes from the published results from that year's population census (INE, 1965). The respective sources for 1970 and 1975 are Schiefelbein (1976) and Echeverría (1982), based on administrative records and the 1970 population census. Data for 1970 corresponds to entire tertiary sector (i.e., including technical education). For the averages, we set age at 17, 25 and 30 for the < 18, $25 - 29$ and > 29 age groups respectively, which likely leads to an underestimate.

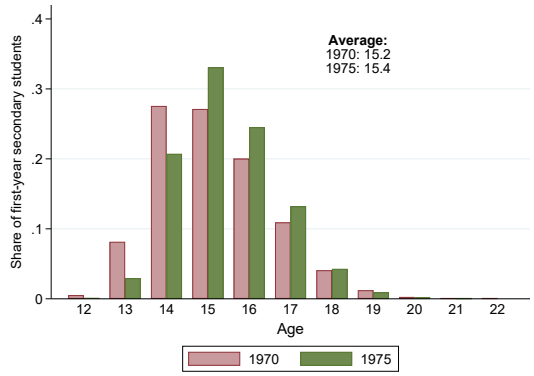
Figure A7: Age distribution of students at start and end of primary and secondary



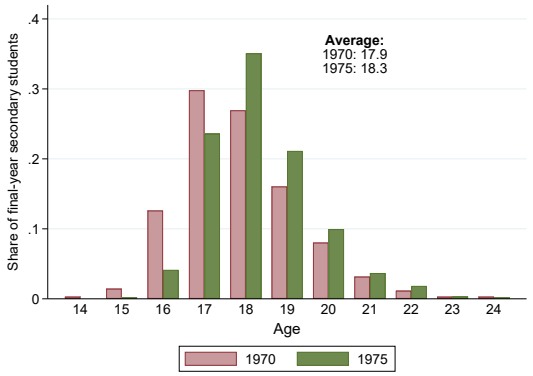
(a) Primary: first year



(b) Primary: final year



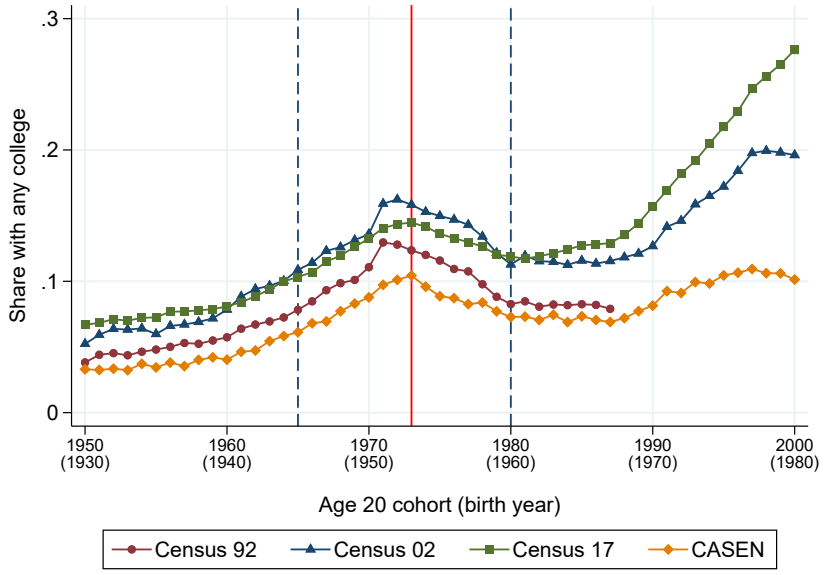
(c) Secondary: first year



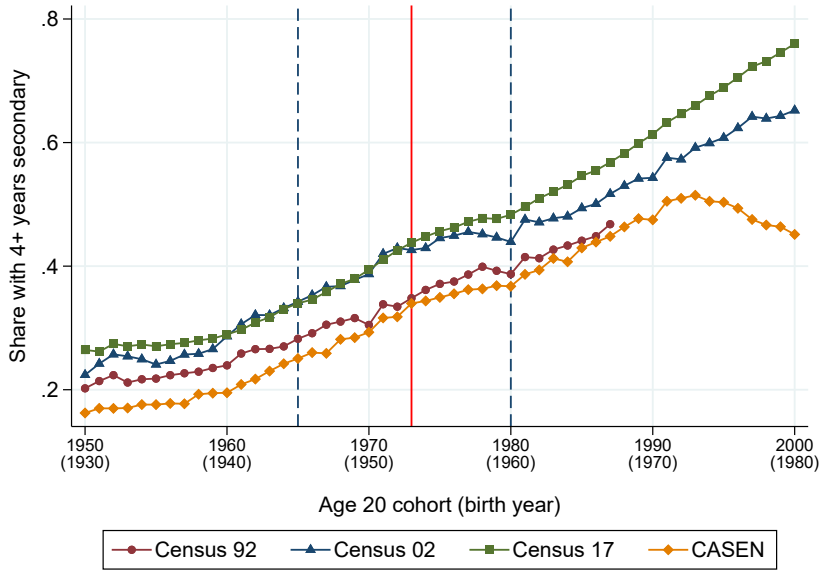
(d) Secondary: final year

Notes: Information for 1960 comes from the published results from that year's population census (INE, 1965). The respective sources for 1970 and 1975 are Schiefelbein (1976) and Echeverría (1982), based on administrative records and the 1970 population census. Data for 1970 corresponds to entire tertiary sector (i.e., including technical education). For the averages, we set age at 17, 25 and 30 for the < 18, $25 - 29$ and > 29 age groups respectively, which likely leads to an underestimate.

Figure A8: Educational Attainment (All sources)



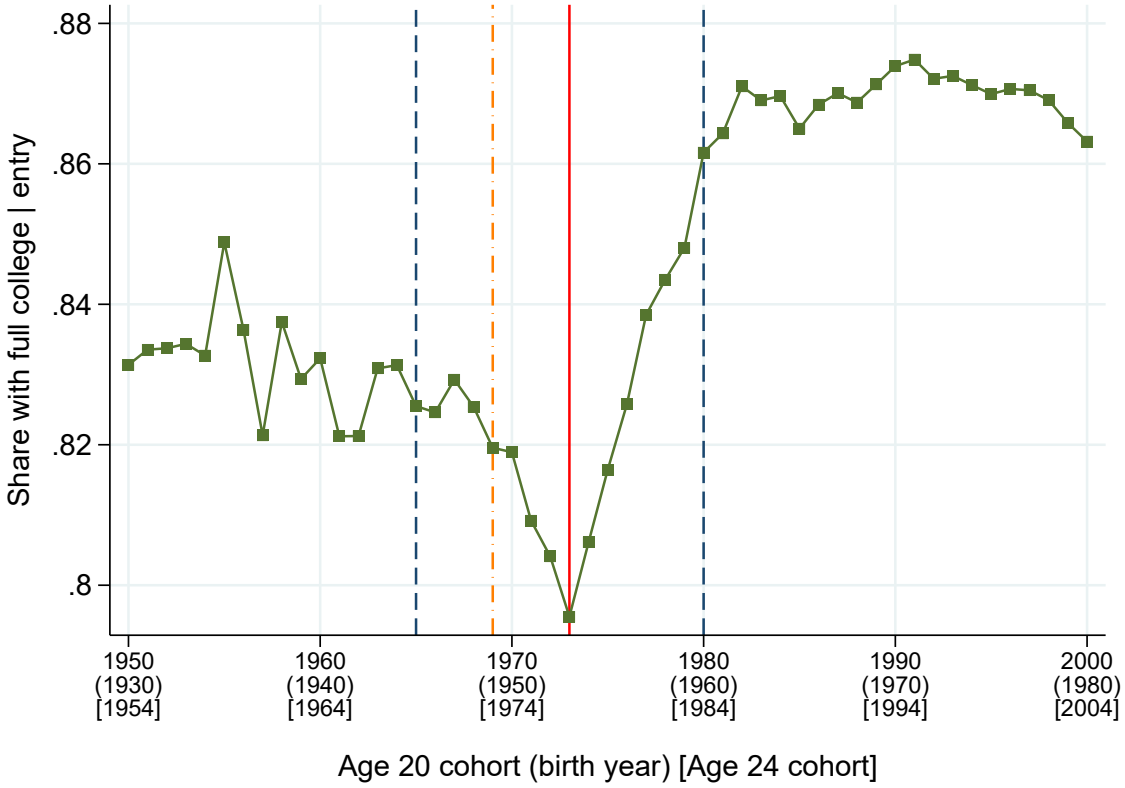
(a) Any College



(b) 4+ years of secondary

Notes: Panel (a) shows the share of respondents per cohort that report college as their highest educational level in each source. Panel (b) shows the share that reports 4+ years of high school. The solid line shows the year of the military coup. Dashed lines show start (1965) and end date (1980) of sample period for the analysis.

Figure A9: College completion conditional on Enrollment



Notes: Figure shows the share of people per cohort in the 2017 census that reports full college among those that report college as their highest educational level. The solid line shows the year of the military coup. Dashed lines show start (1965) and end date (1980) of sample period for the analysis.

Table A2: College entry w/ different kink points (RKD)

Kink point (x):	Dependent variable: Any college				
	1971	1972	1973	1974	1975
	(1)	(2)	(3)	(4)	(5)
[a] Yr Age 21	0.010***	0.007***	0.005***	0.003***	0.001**
	(0.0007)	(0.0005)	(0.0004)	(0.0004)	(0.0003)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.022***	-0.021***	-0.019***	-0.018***	-0.017***
	(0.0010)	(0.0009)	(0.0008)	(0.0007)	(0.0007)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes
Observations	962,039	962,039	962,039	962,039	962,039
R-squared	0.0412	0.0414	0.0415	0.0414	0.0412
p-value a+b=0	0.000	0.000	0.000	0.000	0.000
Mean of dependent variable	0.322	0.322	0.322	0.322	0.322

Notes: Dependent variable in the header. Sample includes all respondents of the 2017 census from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix C Robustness Checks: Educational Attainment

Table A3: Educational Attainment: Other sources

Source:	Census 1992		Census 2002		CASEN	
	Any College	Any Higher	Any College	Any Higher	Any College	Any Higher
	(1)	(2)	(3)	(4)	(5)	(6)
[a] Yr Age 21	0.018***	0.019***	0.012***	0.011***	0.012***	0.012***
	(0.0004)	(0.0003)	(0.0005)	(0.0004)	(0.0007)	(0.0008)
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.036***	-0.030***	-0.025***	-0.013***	-0.024***	-0.019***
	(0.0007)	(0.0006)	(0.0008)	(0.0007)	(0.0012)	(0.0013)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,024,570	1,024,570	1,192,851	1,192,851	148,069	148,069
R-squared	0.040	0.034	0.035	0.030	0.056	0.052
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000
Mean of dependent variable	0.295	0.379	0.325	0.452	0.260	0.352

Notes: Dependent variable in the header. Sample includes all respondents of the respective census or survey from cohorts born between 1943 and 1960 (both inclusive). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A4: College enrollment (Within quintile - 1992)

Sample (Housing wealth quintile):	Dependent variable: Any college				
	5th Quintile (highest)	4th Quintile	3rd Quintile	2nd Quintile	1st Quintile (lowest)
	(1)	(2)	(3)	(4)	(5)
[a] Yr Age 21	0.021***	0.018***	0.018***	0.015***	0.013***
	(0.0005)	(0.0007)	(0.0009)	(0.0008)	(0.0012)
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.037***	-0.031***	-0.031***	-0.027***	-0.026***
	(0.0009)	(0.0012)	(0.0014)	(0.0013)	(0.0018)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes
Observations	504,456	252,358	146,316	80,095	24,493
R-squared	0.042	0.036	0.038	0.035	0.059
p-value a+b=0	0.000	0.000	0.000	0.000	0.000
Mean of dependent variable	0.413	0.209	0.165	0.127	0.125

Notes: Dependent variable in the header. The sample in each column includes all 1992 census respondents from cohorts born between 1943 and 1960 (both inclusive) classified in the respective quintile, but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of birth x gender and household fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

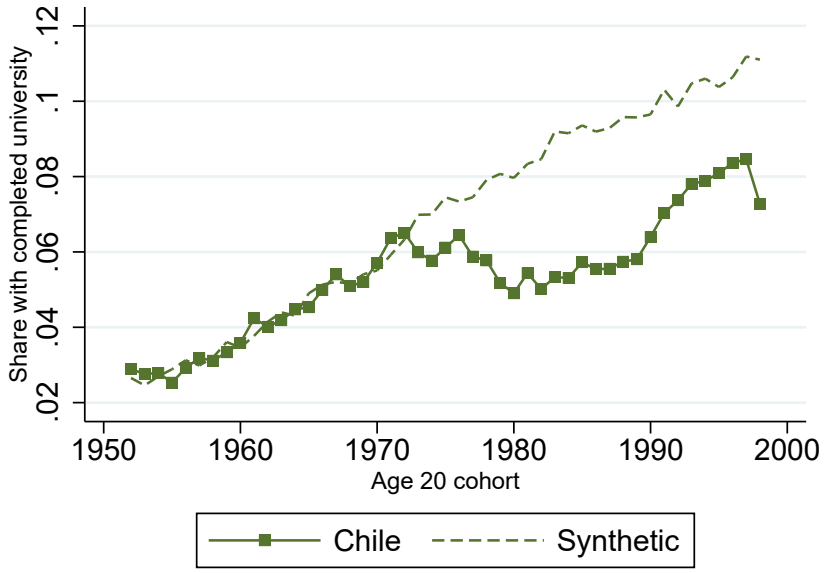
Table A5: College enrollment: Within household (Census 1992, 2002 and 2017)

	Dependent variable: Any College					
Source (Census):	1992		2002		2017	
Relationship to HH head:	Children	Siblings	Children	Siblings	Children	Siblings
	(1)	(2)	(3)	(4)	(5)	(6)
[a] Yr Age 21	0.021***	0.018***	0.012**	0.010***	0.015	0.007**
	(0.0028)	(0.0032)	(0.0047)	(0.0032)	(0.0100)	(0.0034)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.043***	-0.038***	-0.029***	-0.022***	-0.034**	-0.020***
	(0.0038)	(0.0048)	(0.0059)	(0.0046)	(0.0132)	(0.0047)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27,392	14,806	14,291	14,039	4,780	20,552
R-squared	0.651	0.663	0.653	0.668	0.696	0.671
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000
Mean of dependent variable	0.287	0.304	0.305	0.323	0.292	0.310

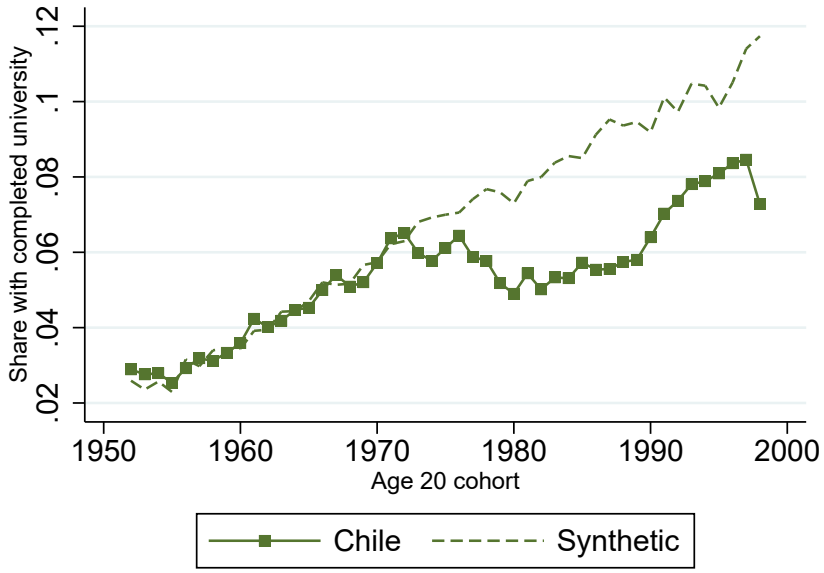
Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). Odd-numbered columns include household heads and respondents classified as siblings. Even-numbered columns include respondents classified as children of the household head. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of birth x gender and household fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix D Additional Results on Synthetic control

Figure A10: Robustness of synthetic control analysis



(a) Countries without dictatorship in the synthetic control



(b) Countries with high HDI in the synthetic control

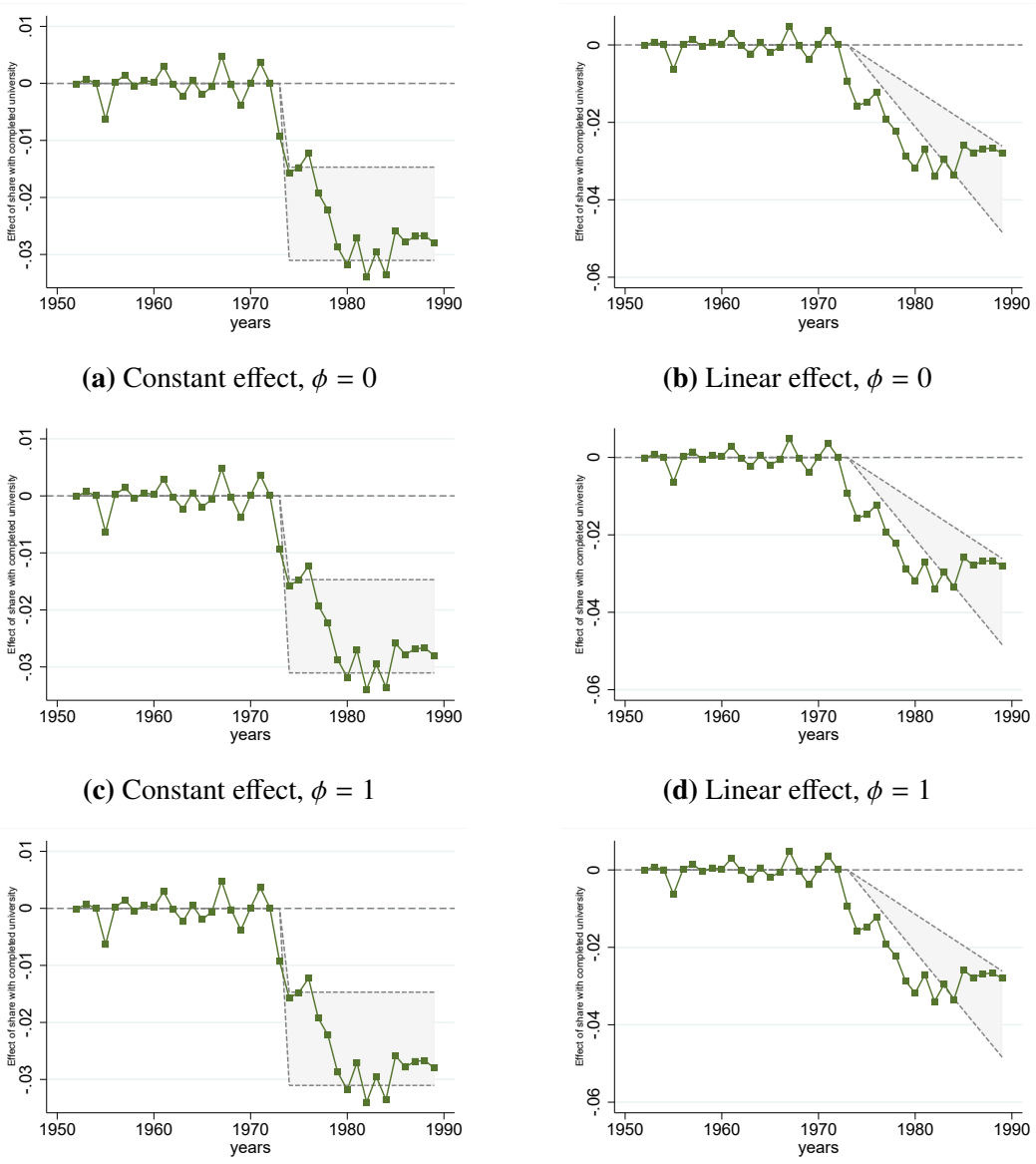
Note: Panel (a) excludes country-year pairs under dictatorship as control units to be potentially used in the synthetic control. Similarly, panel (b) uses countries with a high Human Development Index (HDI larger than 6). For reference Chile is classified as decil 8 in the year 1990. Both panels use the specification with controls and all countries in the sample of potential controls.

Table A6: Robustness checks to the synthetic control analysis

Sample:	R^2	Average effect	p-value Unrestricted	p-value Restricted
Panel A: Using even pre-treatment period outcomes for matchings				
LA without controls	97%	-2,58%	0,00	0,00
LA with controls	95%	-2,00%	0.00	0.00
All countries without controls	95%	-2,32%	0.00	0.00
All countries with controls	96%	-1,83%	0.04	0.04
Exclude dictatorships without controls	95%	-3.05%	0.05	0.05
Exclude dictatorships with controls	96%	-2.91%	0.05	0.05
Panel B: Using all pre-treatment period outcomes for matchings				
LA without controls	97%	-2,34%	0,00	0,00
All countries without controls	96%	-1,67%	0.00	0.00
Exclude dictatorships without controls	95%	-1.75%	0.00	0.00

Notes: This table presents the goodness of fit of the matching and the treatment effects. We present the results for different samples and different set of matching characteristics. The R^2 comes from a regression between the Chilean data and their synthetic control during the pre-treatment period. The *Average effect* is the average difference between Chile and the synthetic control between 1974 and 1990. The *p-value* is computed based on placebo treatments, for each country in the control group we construct their synthetic control and then we create the ratio between the RMSPE in the post (1974-1990) and the RMSPE in the pre-treatment period. Then we see how likely is to find a ratio as large as the one for Chile for the case of a negative effect. The *unrestricted* version uses all the countries, while the *unrestricted* uses only countries with a RMSPE in the pre-treatment period that is smaller than two times the one of Chile.

Figure A11: Confidence sets for Latin America



(a) Constant effect, $\phi = 0$

(b) Linear effect, $\phi = 0$

(c) Constant effect, $\phi = 1$

(d) Linear effect, $\phi = 1$

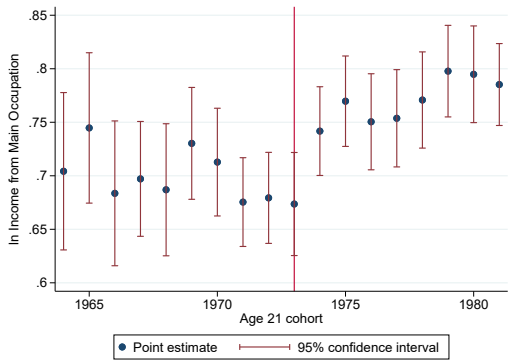
(e) Constant effect, $\phi = 2$

(f) Linear effect, $\phi = 2$

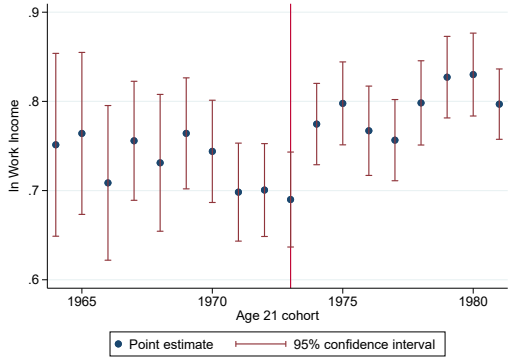
Notes: This figure shows the confidence set proposed by Firpo and Possebom (2018) for a constant and a linear treatment effect. Panels A and B use a sensitivity parameter of 0, while Panels C and D (E and F) use a sensitivity parameter of 1 (2). The sample is all Latin American countries and we use as matching characteristics the even pre-treatment outcomes.

Appendix E Additional results on economic consequences

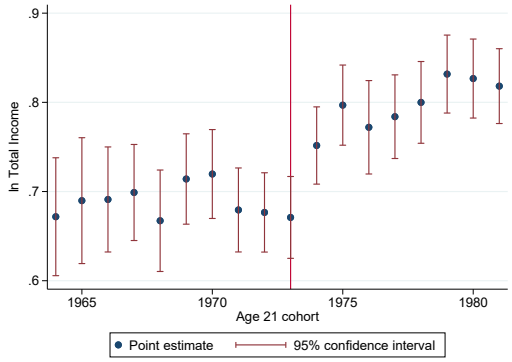
Figure A12: Cohort-specific Estimates of the College Premium



(a) Primary occupation (RF)



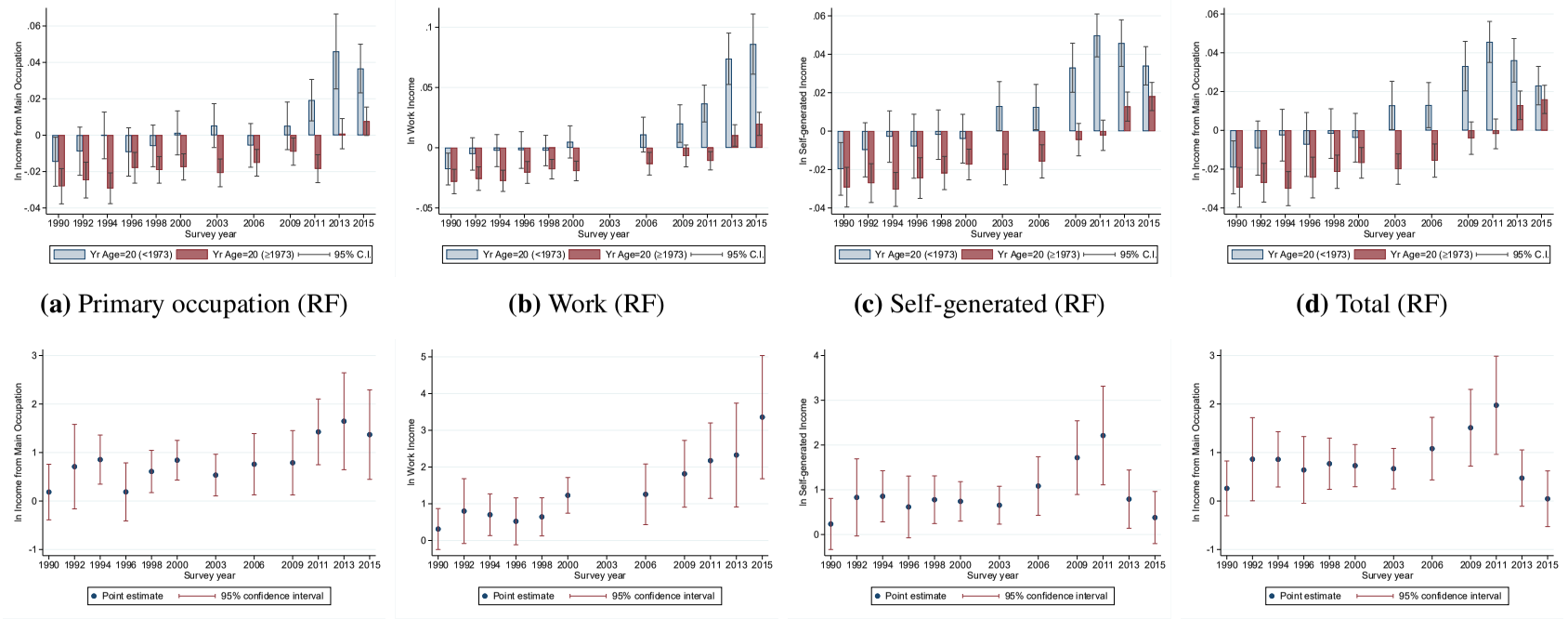
(b) Work (RF)



(c) Total (RF)

Notes: Each panel shows results of a regression of log income from the category in the caption on a full set of interactions of a dummy for any college education with cohort fixed effects. Sample includes all respondents in the CASEN survey from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education. Regression includes county of residence x gender, survey year and age fixed effects. Standard errors are clustered by county of residence.

Figure A13: Educational Attainment and Reported Income - Yearly estimate

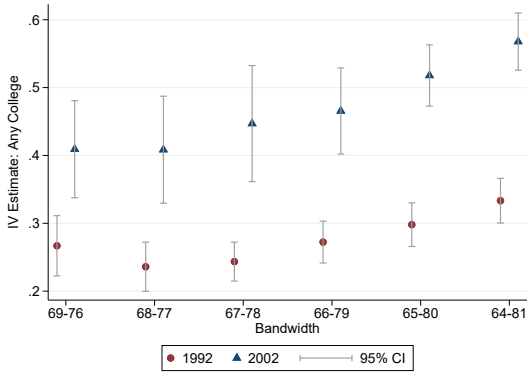


(a) Primary occupation (RF) **(b)** Work (RF) **(c)** Self-generated (RF) **(d)** Total (RF)

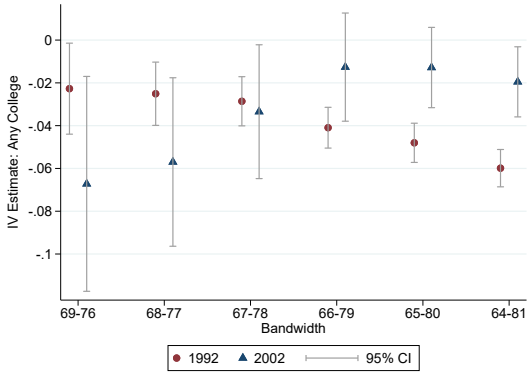
(e) Primary occupation (IV) **(f)** Work (IV) **(g)** Self-generated (IV) **(h)** Total (IV)

Notes: Panels (a)-(d) show results from regressions of log income (for the type in the caption) on separate age trends for birth cohorts that reached age 21 before and after 1973, both normalized to zero in 1972. The sample for each regression includes all respondents in the CASEN survey of the respective year from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education. Panels (e)-(h) show the corresponding IV regressions, in which the trend-break after 1973 (inclusive) is used as excluded instrument for any college education (the baseline trend is an included instrument). All regressions include county of residence x gender fixed effects. Standard errors clustered by county of residence.

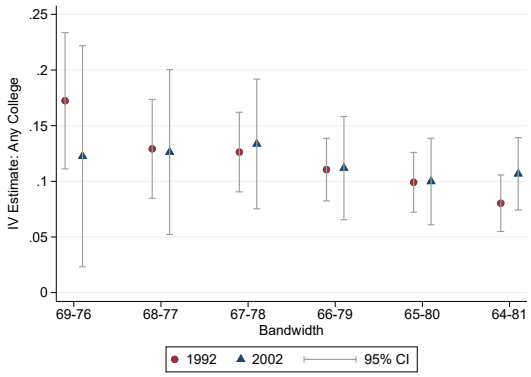
Figure A14: Robustness: Labor Market Outcomes w/ Different Bandwidths



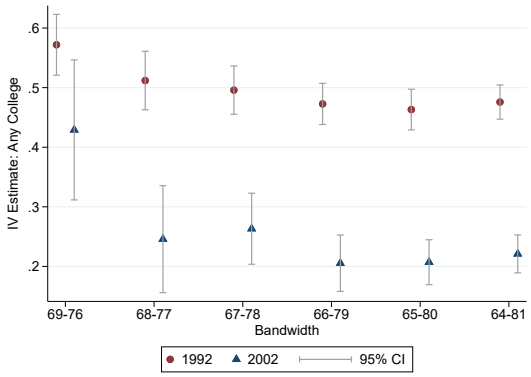
(a) Labor Force Participation



(b) Unemployment



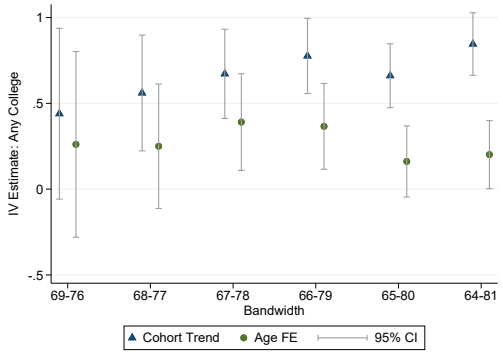
(c) Salaried employment



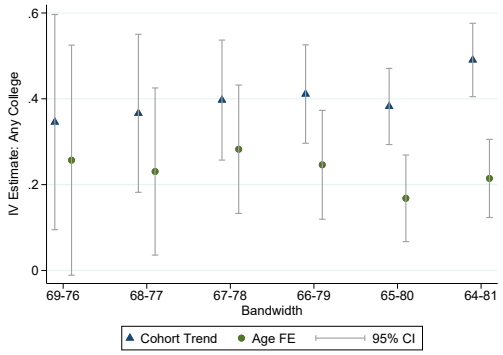
(d) White-Collar High-Skill

Notes: Each figure replicates the IV analysis for the outcome in the caption for the different bandwidths in the x-axis. Sample includes all respondents in the relevant source from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. The regression for unemployment in panel (b) includes a labor-force participation dummy as well. Standard errors clustered by county of birth in parentheses.

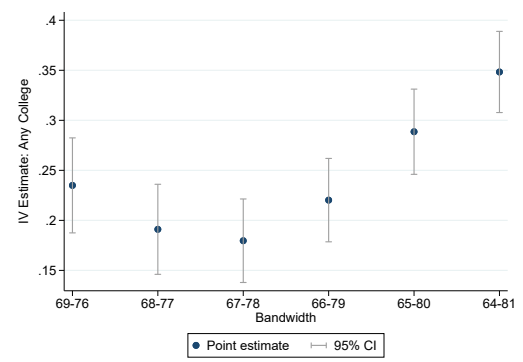
Figure A15: Robustness: Income and Wealth w/ Different Bandwidths



(a) Income



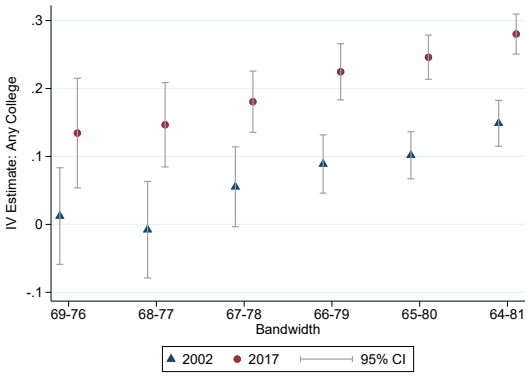
(b) Top income quintile



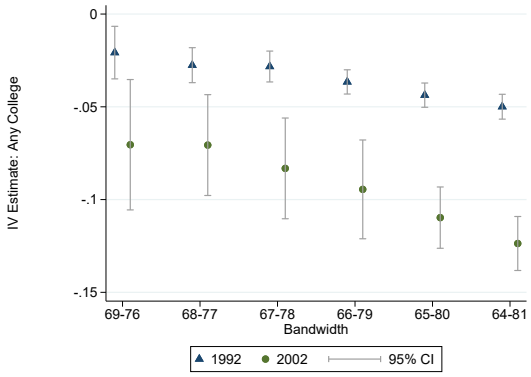
(c) Top wealth quintile

Notes: Each figure replicates the IV analysis for the outcome in the caption for the different bandwidths in the x-axis. Sample includes all respondents in the relevant source (CASEN in panels (a) and (b) and 1992 census in panel (c)) from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Panels (a) and (b) also include survey year fixed effects and show estimates with cohort trend as included instrument or age fixed effects instead. Standard errors clustered by county of birth in parentheses.

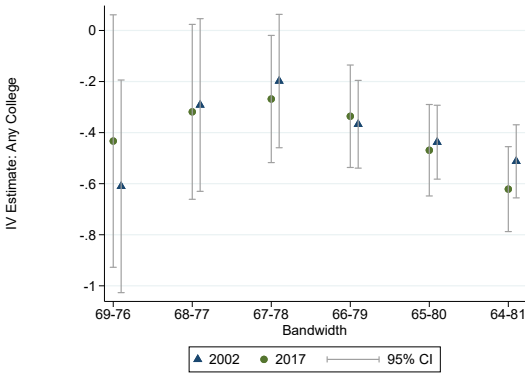
Figure A16: Robustness: Non-economic Outcomes w/ Different Bandwidths



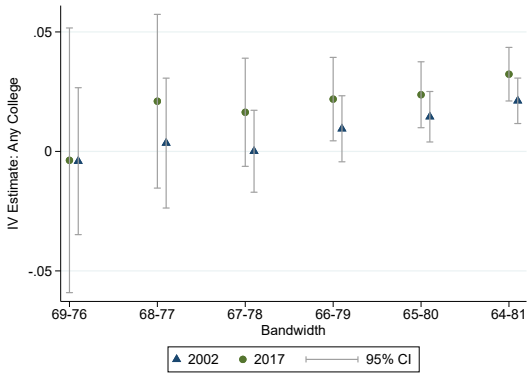
(a) Household Head or Spouse



(b) Widow



(c) Number of children



(d) Share of children alive

Notes: Each figure replicates the IV analysis for the outcome in the caption for the different bandwidths in the x-axis. Sample includes all respondents in the relevant source from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses.

Table A7: Employment category

Dependent variable:	Business owner	Wage earner	Self-employed	Domestic worker	Unpaid w/ relative
	(1)	(2)	(3)	(4)	(5)
			PANEL A: Census 1992		
[a] Yr Age 21	-0.003***	0.007***	-0.003***	-0.001***	-0.0003***
	(0.0002)	(0.0004)	(0.0002)	(0.0001)	(0.0001)
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	0.001**	-0.003***	0.0004	0.001***	0.001***
	(0.0003)	(0.0005)	(0.0003)	(0.0002)	(0.0001)
IV: Any College	-0.017**	0.080***	-0.012	-0.034***	-0.018***
	(0.008)	(0.013)	(0.008)	(0.005)	(0.003)
Observations	773,922	773,922	773,922	773,922	773,922
R-squared	0.014	0.017	0.013	0.024	0.005
p-value a+b=0	0.000	0.000	0.000	0.000	0.000
First-stage KP F-stat	2120.2	2120.2	2120.2	2120.2	2120.2
Mean of dependent variable	0.100	0.750	0.133	0.011	0.007
			PANEL B: Census 2002		
[a] Yr Age 21	-0.002***	0.008***	-0.005***	-0.0003***	-0.001***
	(0.0002)	(0.0003)	(0.0002)	(0.0001)	(0.0001)
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	0.001***	-0.003***	0.001**	0.001***	0.0004***
	(0.0002)	(0.0004)	(0.0003)	(0.0001)	(0.0001)
IV: Any College	-0.039***	0.107***	-0.033**	-0.019***	-0.016***
	(0.008)	(0.017)	(0.013)	(0.005)	(0.004)
Observations	907,050	907,050	907,050	907,050	907,050
R-squared	0.010	0.016	0.016	0.027	0.003
p-value a+b=0	0.000	0.000	0.000	0.015	0.003
First-stage KP F-stat	761.5	761.5	761.5	761.5	761.5
Mean of dependent variable	0.075	0.677	0.219	0.019	0.010
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A8: Occupation

Dependent variable:	White-collar		Blue-collar		Military
	High-skill	Low-skill	High-skill	Low-skill	
	(1)	(2)	(3)	(4)	(5)
			PANEL A: Census 1992		
[a] Yr Age 21	0.004***	-0.004***	-0.002***	-0.003***	0.004***
	(0.0005)	(0.0004)	(0.0002)	(0.0002)	(0.0002)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.017***	0.010***	0.005***	0.007***	-0.004***
	(0.0008)	(0.0005)	(0.0003)	(0.0005)	(0.0003)
IV: Any College	0.476***	-0.270***	-0.126***	-0.178***	0.098***
	(0.015)	(0.012)	(0.008)	(0.011)	(0.009)
Observations	770,652	770,652	770,652	770,652	770,652
R-squared	0.032	0.027	0.049	0.024	0.027
p-value a+b=0	0.000	0.000	0.000	0.000	0.103
First-stage KP F-stat	2094.1	2094.1	2094.1	2094.1	2094.1
Mean of dependent variable	0.431	0.323	0.104	0.109	0.034
			PANEL B: Census 2002		
[a] Yr Age 21	-0.004***	0.002***	0.0002	0.001***	0.001***
	(0.0004)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.006***	0.003***	0.001***	0.001***	-0.0001
	(0.0005)	(0.0003)	(0.0002)	(0.0003)	(0.0001)
IV: Any College	0.221***	-0.126***	-0.053***	-0.045***	0.002
	(0.016)	(0.013)	(0.009)	(0.013)	(0.004)
Observations	872,783	872,783	872,783	872,783	872,783
R-squared	0.022	0.015	0.034	0.017	0.012
p-value a+b=0	0.000	0.000	0.000	0.000	0.000
First-stage KP F-stat	874.9	874.9	874.9	874.9	874.9
Mean of dependent variable	0.596	0.193	0.080	0.121	0.011
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A9: Income Quintiles (CASEN)

Income quintile (dummy)	Dependent variable:				
	5th Quintile (highest)	4th Quintile	3rd Quintile	2nd Quintile	1st Quintile (lowest)
	(1)	(2)	(3)	(4)	(5)
			Panel A: Linear Trend		
[a] Yr Age 21	0.006***	0.002***	0.0002	-0.005***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.013***	0.001	0.004***	0.006***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
IV: Any College	0.491***	-0.055	-0.165***	-0.231***	-0.040
	(0.043)	(0.037)	(0.032)	(0.031)	(0.027)
			Panel B: Age Fixed effects		
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.006***	0.002	0.004***	0.001*	-0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
IV: Any College	0.214***	-0.064	-0.151***	-0.055*	0.055**
	(0.046)	(0.040)	(0.033)	(0.029)	(0.027)
County x gender FE	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes
Age FE (panel B)	Yes	Yes	Yes	Yes	Yes
Observations	118,301	118,301	118,301	118,301	118,301
R-squared (RF - Panel A)	0.111	0.021	0.026	0.040	0.064
R-squared (RF - Panel B)	0.117	0.021	0.026	0.044	0.069
p-value a+b=0 (panel A)	0.000	0.000	0.000	0.002	0.000
First-stage KP F-stat (panel A)	422.3	422.3	422.3	422.3	422.3
First-stage KP F-stat (panel B)	391.6	391.6	391.6	391.6	391.6
Mean of dependent variable	0.464	0.230	0.127	0.0982	0.0803

Notes: Dependent variable in the header. Income quintiles calculated using self-generated income (deflated using yearly CPI) over the entire survey sample. Sample for the regressions includes all respondents in the CASEN survey from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of residence x gender and survey year fixed effects. In panel B, the cohort trend is replace by a full set of age fixed effects. Standard errors clustered by county of residence in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix F Disaggregate results by gender

Table A10: Educational Attainment (Higher Education) by gender

Dependent variable:	Enrolment			Completion		
	College	Technical	Higher	College	Technical	Higher
	(1)	(2)	(3)	(4)	(5)	(6)
[a] Yr Age 21 x 𝟙(Male)	0.002***	-0.001***	0.001*	-0.001**	-0.001***	-0.002***
	(0.0005)	(0.0004)	(0.0005)	(0.0004)	(0.0004)	(0.0004)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	-0.015***	0.003***	-0.012***	-0.008***	0.004***	-0.004***
	(0.0007)	(0.0004)	(0.0008)	(0.0006)	(0.0004)	(0.0007)
[c] Yr Age 21 x 𝟙(Female)	0.008***	0.001***	0.009***	0.006***	0.001***	0.007***
	(0.0005)	(0.0003)	(0.0005)	(0.0004)	(0.0004)	(0.0005)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	-0.023***	0.004***	-0.020***	-0.017***	0.004***	-0.013***
	(0.0009)	(0.0004)	(0.0010)	(0.0008)	(0.0004)	(0.0008)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	962,039	962,039	962,039	962,039	962,039	962,039
R-squared	0.042	0.007	0.038	0.031	0.007	0.027
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000
p-value c+d=0	0.000	0.000	0.000	0.000	0.000	0.000
p-value a=c	0.000	0.000	0.000	0.000	0.000	0.000
p-value a+b=c+d	0.000	0.000	0.757	0.000	0.000	0.678
Mean of dependent variable (Female)	0.303	0.130	0.433	0.251	0.122	0.373
Mean of dependent variable (Male)	0.342	0.106	0.448	0.282	0.095	0.376

Notes: Dependent variable in the header. Sample includes all respondents of the 2017 census from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting full secondary education. " Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. ' Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A11: Labor Force Participation and Unemployment by gender

Source:	Census 1992		Census 2002	
Dependent variable:	In labor force	Seeking work	In labor force	Seeking work
	(1)	(2)	(3)	(4)
[a] Yr Age 21 x 𝟙(Male)	0.006***	-0.001***	0.016***	-0.001***
	(0.0004)	(0.0002)	(0.0004)	(0.0002)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	-0.007***	0.003***	-0.010***	0.0003
	(0.0007)	(0.0002)	(0.0004)	(0.0003)
[c] Yr Age 21 x 𝟙(Female)	0.009***	0.0001	0.017***	0.001***
	(0.0004)	(0.0001)	(0.0007)	(0.0001)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	-0.017***	0.001***	-0.018***	0.001**
	(0.0007)	(0.0002)	(0.0007)	(0.0002)
[e] IV: Any College (Male)	0.226***	-0.082***	0.454***	-0.016
	(0.021)	(0.008)	(0.020)	(0.014)
[f] IV: Any College (Female)	0.416***	-0.038***	0.657***	-0.019**
	(0.018)	(0.005)	(0.031)	(0.008)
County of birth x gender FE	Yes	Yes	Yes	Yes
In labor force FE	No	Yes	No	Yes
Observations	1,024,570	1,024,570	1,192,851	1,192,851
R-squared	0.200	0.014	0.133	0.024
p-value a+b=0	0.002	0.000	0.000	0.072
p-value c+d=0	0.000	0.000	0.014	0.000
p-value a=c	0.000	0.000	0.000	0.000
p-value a+b=c+d	0.000	0.846	0.000	0.000
First-stage KP F-statistic	949.6	957.3	318.5	338.3
p-value e=f	0.000	0.000	0.000	0.474
Mean of dependent variable (Female)	0.571	0.027	0.612	0.048
Mean of dependent variable (Male)	0.946	0.038	0.904	0.077

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). " Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. ' Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term for each gender is used as the respective excluded instrument for any college education (the trends are included instruments). All regressions include county of birth x gender fixed effects. Even-numbered columns include labor-force participation fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A12: Employment category by gender

Source:	Census 1992					Census 2002				
Dependent variable:	Business owner	Wage earner	Self-employed	Domestic worker	Unpaid w/ relative	Business owner	Wage earner	Self-employed	Domestic worker	Unpaid w/ relative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
[a] Yr Age 21 x 𝟙(Male)	-0.0038***	0.0072***	-0.0032***	-0.0001	-0.0002***	-0.0023***	0.0082***	-0.0054***	-0.0002***	-0.0004***
	(0.00028)	(0.00043)	(0.00027)	(0.00006)	(0.00005)	(0.00021)	(0.00037)	(0.00033)	(0.00004)	(0.00008)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	0.0007**	-0.0011**	-0.0001	0.0001	0.0003***	0.0006*	-0.0009*	-0.0001	0.0001**	0.0003**
	(0.00035)	(0.00050)	(0.00037)	(0.00009)	(0.00007)	(0.00032)	(0.00051)	(0.00045)	(0.00006)	(0.00012)
[c] Yr Age 21 x 𝟙(Female)	-0.0019***	0.0071***	-0.0027***	-0.0019***	-0.0006***	-0.0017***	0.0084***	-0.0054***	-0.0006**	-0.0008***
	(0.00023)	(0.00051)	(0.00039)	(0.00026)	(0.00013)	(0.00025)	(0.00051)	(0.00047)	(0.00023)	(0.00012)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	0.0003	-0.0058***	0.0014***	0.0030***	0.0012***	0.0014***	-0.0052***	0.0021***	0.0010***	0.0006***
	(0.00038)	(0.00082)	(0.00051)	(0.00042)	(0.00024)	(0.00035)	(0.00071)	(0.00062)	(0.00033)	(0.00016)
[e] IV: Any College (Male)	-0.0227**	0.0331**	0.0044	-0.0046	-0.0102***	-0.0266*	0.0410*	0.0031	-0.0058**	-0.0116**
	(0.01065)	(0.01530)	(0.01127)	(0.00287)	(0.00231)	(0.01423)	(0.02247)	(0.02009)	(0.00258)	(0.00540)
[f] IV: Any College (Female)	-0.0062	0.1357***	-0.0314***	-0.0706***	-0.0276***	-0.0498***	0.1813***	-0.0747***	-0.0358***	-0.0209***
	(0.00867)	(0.01819)	(0.01198)	(0.00894)	(0.00571)	(0.01316)	(0.02298)	(0.02062)	(0.01111)	(0.00573)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	773,922	773,922	773,922	773,922	773,922	907,050	907,050	907,050	907,050	907,050
R-squared	0.014	0.017	0.013	0.024	0.005	0.010	0.016	0.016	0.027	0.003
p-value a+b=0	0.000	0.000	0.000	0.025	0.000	0.000	0.000	0.000	0.276	0.014
p-value c+d=0	0.000	0.017	0.000	0.000	0.001	0.032	0.000	0.000	0.007	0.014
p-value a=c	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value a+b=c+d	0.000	0.000	0.000	0.000	0.026	0.000	0.000	0.000	0.005	0.358
First-stage KP F-stat	835.7	835.7	835.7	835.7	835.7	306.7	306.7	306.7	306.7	306.7
p-value e=f	0.191	0.000	0.027	0.000	0.006	0.312	0.000	0.013	0.011	0.189
Mean of dependent variable (Female)	0.080	0.785	0.097	0.026	0.011	0.055	0.706	0.181	0.043	0.014
Mean of dependent variable (Male)	0.111	0.728	0.154	0.002	0.005	0.088	0.659	0.244	0.003	0.007

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). " Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. ' Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term for each gender is used as the respective excluded instrument for any college education (the trends are included instruments). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A13: Occupation by gender

Source:	Census 1992					Census 2002				
Dependent variable:	White-collar		Blue-collar		Military	White-collar		Blue-collar		Military
	High-skill	Low-skill	High-skill	Low-skill		High-skill	Low-skill	High-skill	Low-skill	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
[a] Yr Age 21 x 𝟙(Male)	0.001**	-0.002***	-0.002***	-0.002***	0.005***	-0.006***	0.001***	0.001**	0.002***	0.002***
	(0.0005)	(0.0005)	(0.0003)	(0.0003)	(0.0003)	(0.0006)	(0.0003)	(0.0003)	(0.0003)	(0.0001)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	-0.012***	0.004***	0.006***	0.006***	-0.005***	-0.004***	0.002***	0.002***	0.001	0.0001
	(0.0007)	(0.0005)	(0.0004)	(0.0006)	(0.0005)	(0.0006)	(0.0004)	(0.0004)	(0.0004)	(0.0002)
[c] Yr Age 21 x 𝟙(Female)	0.010***	-0.005***	-0.002***	-0.004***	0.001***	-0.002***	0.003***	-0.001***	-0.0001	0.0001***
	(0.0008)	(0.0006)	(0.0002)	(0.0004)	(0.0001)	(0.0004)	(0.0004)	(0.0002)	(0.0003)	(0.00003)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	-0.026***	0.019***	0.002***	0.007***	-0.001***	-0.008***	0.005***	0.001***	0.002***	-0.0001*
	(0.0013)	(0.0010)	(0.0003)	(0.0006)	(0.0002)	(0.0008)	(0.0006)	(0.0002)	(0.0004)	(0.00005)
[e] IV: Any College (Male)	0.366***	-0.130***	-0.191***	-0.193***	0.149***	0.172***	-0.069***	-0.075***	-0.026	-0.002
	(0.016)	(0.015)	(0.014)	(0.015)	(0.015)	(0.023)	(0.017)	(0.015)	(0.018)	(0.008)
[f] IV: Any College (Female)	0.6170***	-0.438***	-0.048***	-0.161***	0.030***	0.285***	-0.186***	-0.029***	-0.073***	0.003*
	(0.020)	(0.017)	(0.006)	(0.012)	(0.004)	(0.021)	(0.018)	(0.008)	(0.014)	(0.002)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	770,652	770,652	770,652	770,652	770,652	872,783	872,783	872,783	872,783	872,783
R-squared	0.032	0.029	0.050	0.024	0.028	0.022	0.016	0.034	0.017	0.013
p-value a+b=0	0.000	0.000	0.000	0.000	0.005	0.000	0.000	0.000	0.000	0.000
p-value c+d=0	0.000	0.000	0.041	0.000	0.000	0.000	0.000	0.433	0.000	0.005
p-value a=c	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value a+b=c+d	0.000	0.000	0.000	0.000	0.000	0.135	0.000	0.000	0.002	0.000
First-stage KP F-stat	827.3	827.3	827.3	827.3	827.3	417.1	417.1	417.1	417.1	417.1
p-value e=f	0.000	0.000	0.000	0.031	0.000	0.000	0.000	0.009	0.030	0.540
Mean of dependent variable (Female)	0.485	0.409	0.034	0.065	0.007	0.649	0.240	0.026	0.084	0.002
Mean of dependent variable (Male)	0.399	0.271	0.146	0.135	0.050	0.563	0.163	0.113	0.145	0.016

Notes: Dependent variable in the header. Sample includes all respondents in the respective census from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. 'Yr Age 21 x 𝟙(Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21on or after 1973. In the IV regression, the interaction term for each gender is used as the respective excluded instrument for any college education (the trends are included instruments). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A14: Reported Income by gender (CASEN)

Dependent variable (log income):	Main occupation	All work	Self-generated	Total
	(1)	(2)	(3)	(4)
[a] Yr Age 21 x 𝟙(Male)	0.000	0.013***	0.011***	0.009***
	(0.002)	(0.003)	(0.002)	(0.002)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	-0.014***	-0.023***	-0.022***	-0.021***
	(0.003)	(0.004)	(0.003)	(0.003)
[c] Yr Age 21 x 𝟙(Female)	0.003	0.020***	0.017***	0.013***
	(0.003)	(0.004)	(0.003)	(0.003)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	-0.017***	-0.027***	-0.022***	-0.018***
	(0.004)	(0.005)	(0.004)	(0.004)
[e] IV: Any College (Male)	0.569***	0.996***	0.962***	0.905***
	(0.128)	(0.156)	(0.128)	(0.124)
[f] IV: Any College (Female)	0.533***	0.858***	0.728***	0.612***
	(0.113)	(0.163)	(0.120)	(0.116)
County x gender FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Observations	99,712	93,666	118,301	118,301
R-squared	0.165	0.146	0.152	0.154
p-value a+b=0	0.000	0.000	0.000	0.000
p-value c+d=0	0.000	0.001	0.014	0.007
p-value a=c	0.694	0.000	0.000	0.000
p-value a+b=c+d	0.536	0.203	0.003	0.003
First-stage KP F-stat	111.2	92.3	125.5	125.5
p-value e=f	0.833	0.529	0.157	0.069
Mean of dependent variable (Female)	486,608	503,336	509,694	512,561
Mean of dependent variable (Male)	789,228	843,026	897,598	901,089

Notes: Dependent variable in the header. Real income deflated using yearly CPI. Sample includes all respondents in the CASEN survey from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term for each gender is used as the respective excluded instrument for any college education (the trends are included instruments). All regressions include county of residence x gender and survey year fixed effects. Standard errors clustered by county of residence in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A15: Housing Wealth Quintiles by gender (1992)

Dependent variable: Housing wealth quintile (dummy)	Q5 (highest)	Q4	Q3	Q2	Q1 (lowest)
	(1)	(2)	(3)	(4)	(5)
[a] Yr Age 21 x 𝟙(Male)	-0.002***	0.0003	0.001***	0.001*	0.001***
	(0.0006)	(0.0004)	(0.0003)	(0.0003)	(0.0002)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	-0.014***	0.004***	0.005***	0.004***	0.001***
	(0.0008)	(0.0007)	(0.0005)	(0.0004)	(0.0002)
[c] Yr Age 21 x 𝟙(Female)	-0.001*	-0.001*	0.001*	0.001***	0.0004***
	(0.0006)	(0.0005)	(0.0003)	(0.0003)	(0.0001)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	-0.012***	0.004***	0.004***	0.003***	0.001***
	(0.0008)	(0.0007)	(0.0004)	(0.0003)	(0.0002)
[e] IV: Any College (Male)	0.412***	-0.129***	-0.143***	-0.120***	-0.019***
	(0.025)	(0.021)	(0.016)	(0.012)	(0.006)
[f] IV: Any College (Female)	0.299***	-0.101***	-0.094***	-0.077***	-0.027***
	(0.022)	(0.017)	(0.011)	(0.008)	(0.005)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes
Observations	1,007,957	1,007,957	1,007,957	1,007,957	1,007,957
R-squared	0.114	0.013	0.032	0.052	0.050
p-value a+b=0	0.000	0.000	0.000	0.000	0.000
p-value c+d=0	0.000	0.000	0.000	0.000	0.000
p-value a=c	0.001	0.032	0.019	0.005	0.004
p-value a+b=c+d	0.000	0.000	0.000	0.069	0.005
First-stage KP F-stat	996.1	996.1	996.1	996.1	996.1
p-value e=f	0.000	0.130	0.004	0.000	0.255
Mean of dependent variable (Female)	0.496	0.253	0.148	0.080	0.023
Mean of dependent variable (Male)	0.505	0.248	0.143	0.079	0.026

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). " Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. ' Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term for each gender is used as the respective excluded instrument for any college education (the trends are included instruments). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A16: Status within the household by gender

Source:	Census 1992				Census 2002				Census 2017			
	HH Head	Spouse	Child	Parent	HH Head	Spouse	Child	Parent	HH Head	Spouse	Child	Parent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
[a] Yr Age 21 x 𝟙(Male)	-0.0022***	-0.0003**	0.0027***	-0.0003***	-0.0033***	0.0003*	0.0029***	-0.0010***	-0.0006	0.0011***	0.0015***	-0.0024***
	(0.00039)	(0.00013)	(0.00018)	(0.00003)	(0.00027)	(0.00019)	(0.00013)	(0.00006)	(0.00037)	(0.00024)	(0.00010)	(0.00016)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	-0.0133***	-0.0003	0.0105***	0.0003***	-0.0018***	-0.0012***	0.0024***	0.0006***	-0.0057***	0.0003	0.0030***	0.0011***
	(0.00058)	(0.00021)	(0.00034)	(0.00004)	(0.00051)	(0.00028)	(0.00022)	(0.00007)	(0.00049)	(0.00035)	(0.00020)	(0.00018)
[c] Yr Age 21 x 𝟙(Female)	-0.0094***	0.0082***	0.0047***	-0.0011***	-0.0065***	0.0064***	0.0041***	-0.0031***	-0.0050***	0.0092***	0.0021***	-0.0051***
	(0.00036)	(0.00040)	(0.00023)	(0.00005)	(0.00043)	(0.00048)	(0.00018)	(0.00013)	(0.00037)	(0.00034)	(0.00010)	(0.00026)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	-0.0011**	-0.0135***	0.0082***	0.0010***	-0.0026***	-0.0021***	0.0021***	0.0019***	0.0011*	-0.0058***	0.0023***	0.0019***
	(0.00050)	(0.00058)	(0.00039)	(0.00006)	(0.00053)	(0.00057)	(0.00032)	(0.00015)	(0.00056)	(0.00042)	(0.00016)	(0.00029)
[e] IV: Any College (Male)	0.408***	0.008	-0.320***	-0.008***	0.078***	0.054***	-0.107***	-0.029***	0.398***	-0.020	-0.208***	-0.079***
	(0.020)	(0.006)	(0.013)	(0.001)	(0.022)	(0.012)	(0.011)	(0.003)	(0.032)	(0.025)	(0.017)	(0.012)
[f] IV: Any College (Female)	0.027**	0.338***	-0.205***	-0.025***	0.092***	0.075***	-0.076***	-0.068***	-0.050*	0.268***	-0.106***	-0.087***
	(0.012)	(0.016)	(0.011)	(0.002)	(0.019)	(0.020)	(0.011)	(0.005)	(0.027)	(0.025)	(0.008)	(0.012)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,023,353	1,023,353	1,023,353	1,023,353	1,192,851	1,192,851	1,192,851	1,192,851	1,036,105	1,036,105	1,036,105	1,036,105
R-squared	0.375	0.374	0.031	0.005	0.234	0.246	0.015	0.010	0.116	0.115	0.012	0.016
p-value a+b=0	0.000	0.000	0.000	0.064	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value c+d=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value a=c	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value a+b=c+d	0.000	0.000	0.320	0.009	0.000	0.000	0.000	0.000	0.000	0.000	0.871	0.000
First-stage KP F-stat	943.7	943.7	943.7	943.7	318.5	318.5	318.5	318.5	191.3	191.3	191.3	191.3
p-value e=f	0.000	0.000	0.000	0.000	0.583	0.344	0.022	0.000	0.000	0.000	0.000	0.665
Mean of dependent variable (Female)	0.187	0.596	0.157	0.002	0.326	0.527	0.083	0.011	0.452	0.410	0.034	0.049
Mean of dependent variable (Male)	0.786	0.031	0.112	0.000	0.784	0.079	0.059	0.003	0.779	0.115	0.029	0.018

Notes: Dependent variable in the header. Sample includes all respondents in the respective census from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. 'Yr Age 21 x 𝟙(Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21on or after 1973. In the IV regression, the interaction term for each gender is used as the respective excluded instrument for any college education (the trends are included instruments). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

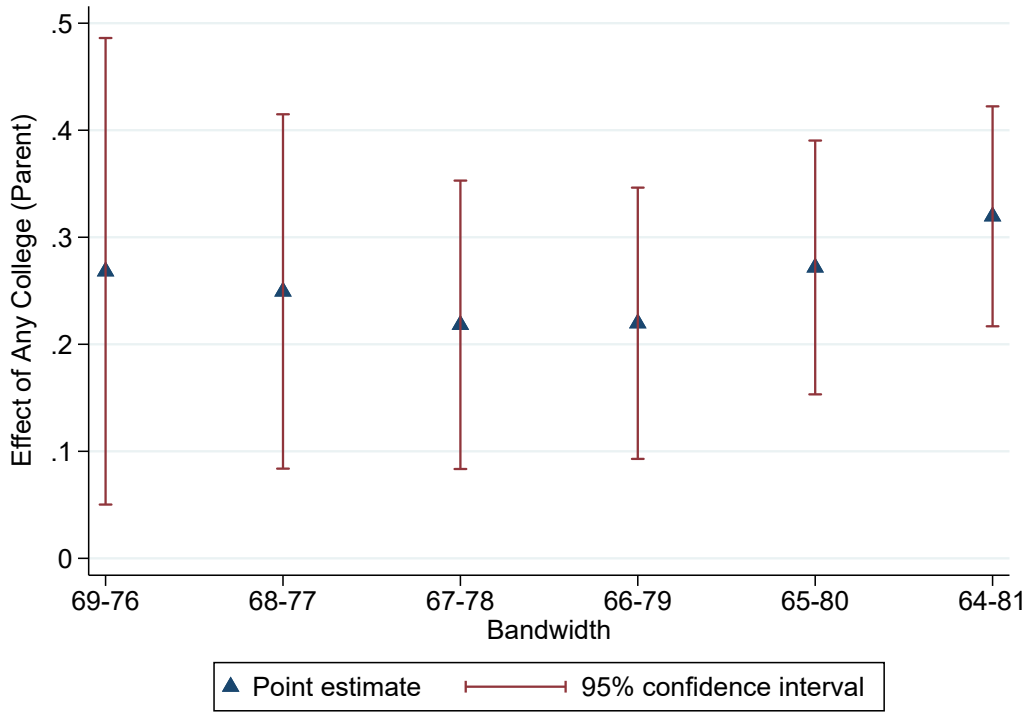
Table A17: Marital Status by gender

Source:	Census 1992				Census 2002			
	Ever	Current Status			Ever	Current Status		
	married	Married	Widowed	Separated	married	Married	Widowed	Separated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[a] Yr Age 21 x 𝟙(Male)	-0.0004*	0.0019***	-0.0008***	-0.0011***	-0.0011***	0.0010***	-0.0020***	0.0010***
	(0.00022)	(0.00018)	(0.00008)	(0.00016)	(0.00024)	(0.00020)	(0.00018)	(0.00020)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	-0.0108***	-0.0005*	0.0007***	-0.0001	-0.0026***	0.0006**	0.0014***	-0.0020***
	(0.00035)	(0.00027)	(0.00009)	(0.00027)	(0.00031)	(0.00027)	(0.00019)	(0.00026)
[c] Yr Age 21 x 𝟙(Female)	0.0001	0.0073***	-0.0043***	-0.0030***	-0.0005**	0.0084***	-0.0085***	0.0001
	(0.00028)	(0.00024)	(0.00019)	(0.00026)	(0.00025)	(0.00029)	(0.00024)	(0.00021)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	-0.0080***	-0.0020***	0.0031***	-0.0011***	-0.0012***	-0.0022***	0.0053***	-0.0030***
	(0.00044)	(0.00036)	(0.00020)	(0.00037)	(0.00042)	(0.00034)	(0.00026)	(0.00029)
[e] IV: Any College (Male)	0.330***	0.016*	-0.020***	0.005	0.117***	-0.027**	-0.060***	0.086***
	(0.013)	(0.008)	(0.003)	(0.008)	(0.015)	(0.012)	(0.010)	(0.013)
[f] IV: Any College (Female)	0.200***	0.050***	-0.078***	0.028***	0.044***	0.081***	-0.190***	0.109***
	(0.012)	(0.009)	(0.005)	(0.009)	(0.015)	(0.012)	(0.010)	(0.011)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ever married FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	1,024,570	1,024,570	1,024,570	1,024,570	1,192,851	1,192,851	1,192,851	1,192,851
R-squared	0.025	0.631	0.016	0.025	0.020	0.445	0.031	0.029
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value c+d=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value a=c	0.225	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value a+b=c+d	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
First-stage KP F-stat	949.6	956.5	956.5	956.5	318.5	318.6	318.6	318.6
p-value e=f	0.000	0.002	0.000	0.011	0.002	0.000	0.000	0.146
Mean of dependent variable (Female)	0.816	0.723	0.017	0.077	0.832	0.664	0.042	0.125
Mean of dependent variable (Male)	0.895	0.850	0.004	0.041	0.915	0.829	0.011	0.075

Notes: Dependent variable in the header. Sample includes all respondents in the respective census from cohorts born between 1943 and 1960 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (media). "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x 𝟙(Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term for each gender is used as the respective excluded instrument for any college education (the trends are included instruments). All regressions include county of birth x gender fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix G Additional results on IGT of Education

Figure A17: Robustness: Child's College Enrolment w/ Different Bandwidths



Notes: Each figure replicates the IV analysis of child's college enrollment for the different bandwidths in the x-axis. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (I) reached age 21 in the relevant bandwidth (both years inclusive) and (II) that reported full secondary education. Possible parent-child linkages include: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. "Yr Age 21 Parent" is a continuous variable indicating the year at which the parent reached 21 years of age, normalized to zero in 1972. "Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)" is the interaction of this variable with a dummy for parents that reached age 21 on or after 1973. The interaction term is used as excluded instrument for any college education by the Parent (the trend is an included instrument). All regressions include county of birth x gender, parent's gender x (child) gender, age and relationship to household head fixed effects. Standard errors clustered by county of birth.

Table A18: Educational attainment of children: Sample Characteristics (Census 2017)

							Position in HH						
	Age	Female	Full primary	Full secondary	Any college	HH size	Head	Spouse	Child	Children (women)	In labor force	Unemployed	Studying
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
I: All 25-40 yo	32.10	0.50	0.95	0.80	0.31	24.82	0.36	0.24	0.26	1.45	0.81	0.06	0.12
N=3,781,382	(4.61)	(0.50)	(0.22)	(0.40)	(0.46)	(242.67)	(0.48)	(0.43)	(0.44)	(1.21)	(0.39)	(0.24)	(0.33)
II: I + linked to parent	30.51	0.48	0.96	0.83	0.35	4.52	0.05	0.02	0.90	0.96	0.81	0.10	0.17
N=1,013,071	(4.48)	(0.50)	(0.20)	(0.38)	(0.48)	(1.84)	(0.22)	(0.14)	(0.30)	(1.05)	(0.39)	(0.30)	(0.37)
III: II + parent w/ full secondary	29.59	0.49	0.99	0.94	0.55	4.30	0.04	0.02	0.92	0.70	0.81	0.10	0.23
N=435,949	(4.14)	(0.50)	(0.09)	(0.24)	(0.50)	(1.65)	(0.20)	(0.12)	(0.27)	(0.93)	(0.40)	(0.31)	(0.42)
IV: III + parent age 21 $\in$ [1964, 1981]	31.06	0.49	0.99	0.94	0.58	4.17	0.05	0.02	0.91	0.74	0.83	0.10	0.19
N=233,134	(4.39)	(0.50)	(0.10)	(0.23)	(0.49)	(1.64)	(0.22)	(0.14)	(0.28)	(0.98)	(0.38)	(0.31)	(0.39)

Notes: Table shows averages and standard deviations (in parenthesis) for the characteristic described in the header. Top row shows values for the full sample of people with ages 25-40 in the 2017 population census. Second row shows corresponding statistics for the subsample that cohabits with a parent, irrespective of any characteristics of the parent. Third row further restricts the sample by only including parents with full secondary. Finally, the bottom row (our estimating sample) limits the sample to parent born between 1943 and 1960.

Table A19: Educational attainment of children: Heterogeneous effects by relationship to HH Head (Census 2017)

	Dependent variable: Any College (child)				
Position in household:	Child	Head	Spouse	Child of spouse	Sibling
	(1)	(2)	(3)	(4)	(5)
IV: Any College (Parent)	0.325***	0.468***	0.494*	0.022	-0.340
	(0.055)	(0.159)	(0.252)	(0.203)	(0.780)
OLS: Any College (Parent)	0.261***	0.267***	0.283***	0.022	0.286***
	(0.004)	(0.010)	(0.017)	(0.203)	(0.028)
Observations	213,059	11,616	4,502	1,965	1,522
R-squared (OLS)	0.121	0.130	0.151	0.207	0.214
Birth County x Gender FE	Yes	Yes	Yes	Yes	Yes
Parent Gender x Gender FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
First-stage KP F-stat	270.3	47.3	14.5	22.5	2.9
Mean of dependent variable	0.585	0.565	0.549	0.508	0.499

Notes: Dependent variable in the header of each panel. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (I) was born between 1943 and 1960 (both years inclusive) and (II) that reported full secondary education. Each column considers a different possible parent-child linkage: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. "Yr Age 21 Parent" is a continuous variable indicating the year at which the parent reached 21 years of age, normalized to zero in 1972. "Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)" is the interaction of this variable with a dummy for parents that reached age 21 on or after 1973. In the IV regressions, the interaction term is used as excluded instrument for any college education by the Parent (the trend is an included instrument). All regressions include county of birth x gender, parent's gender x (child) gender, age and relationship to household head fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A20: Educational attainment of children: Heterogeneous effects by gender (Census 2017)

Source of heterogeneity:	**Parent's gender**		**Child's gender**	
	(1)	(2)	(3)	(4)
Panel A: First Stage and Reduced Form				
Dependent variable:	Any College (Parent)	Any College	Any College (Parent)	Any College
[a] Yr Age 21 Parent x 𝟙(Male)	0.002**	-0.000	0.005***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
[b] Yr Age 21 Parent x 𝟙(Male) x 𝟙(Yr Age 21 Parent ≥ 1973)	-0.019***	-0.006***	-0.022***	-0.007***
	(0.002)	(0.001)	(0.001)	(0.002)
[c] Yr Age 21 Parent x 𝟙(Female)	0.006***	-0.000	0.002**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
[d] Yr Age 21 Parent x 𝟙(Female) x 𝟙(Yr Age 21 Parent ≥ 1973)	-0.026***	-0.008***	-0.021***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.001)
Panel B: OLS and IV - Dependent variable: Any College				
	IV	OLS	IV	OLS
[e] Any College Parent (Female)	0.318***	0.258***	0.305***	0.255***
	(0.069)	(0.005)	(0.069)	(0.005)
[f] Any College Parent (Male)	0.311***	0.265***	0.332***	0.268***
	(0.066)	(0.004)	(0.068)	(0.005)
Birth county x gender FE	Yes	Yes	Yes	Yes
Parent gender x gender FE	Yes	Yes	Yes	Yes
Relationship to household head FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Observations	233,123	233,123	233,123	233,123
R-squared [Panel A]	0.095	0.063	0.095	0.063
R-squared [Panel B]	-	0.118	-	0.118
p-value a+b=0 [Panel A]	0.000	0.000	0.000	0.000
p-value c+d=0 [Panel A]	0.000	0.000	0.000	0.000
p-value a=c [Panel A]	0.045	0.663	0.000	0.998
p-value a+b=c+d [Panel A]	0.002	0.010	0.328	0.392
p-value e=f [Panel B]	0.934	0.097	0.754	0.005
Mean of dependent variable (Female) [Panel A]	0.283	0.563	0.305	0.615
Mean of dependent variable (Male) [Panel A]	0.328	0.594	0.314	0.549
Mean of dependent variable (Female) [Panel B]	0.563	0.563	0.615	0.615
Mean of dependent variable (Male) [Panel B]	0.594	0.594	0.549	0.549
First-stage KP F-stat (Panel B)	76.4	-	93.2	-

Notes: Dependent variable in the header of each column in panel A, and Any College in panel B. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (I) reached age 21 between 1964 and 1981 (both years inclusive) and (II) that reported full secondary education. Possible parent-child linkages include: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. "Yr Age 21 Parent" is a continuous variable indicating the year at which the parent reached 21 years of age, normalized to zero in 1972. "Yr Age 21 Parent x 𝟙(Yr Age 21 Parent ≥ 1973)" is the interaction of this variable with a dummy for parents that reached age 21 on or after 1973. In the columns 1-2, we include separate versions of these variables by gender of the the observed parent. In columns 3-4, we include an analogous disaggregation by gender of the child. In the odd-numbered columns in panel B, the interaction term is used as excluded instrument for "Any College" by the parent (the trend is an included instrument). All regressions include county of birth x gender, parent's gender x (child) gender, age and relationship to household head fixed effects. Standard errors clustered by county of birth of child in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A21: Educational attainment of children: Robustness to different bandwidths in age of children (Census 2017)

	Dependent variable: Any College (child)				
Ages of children (bandwidth):	20-40	30-40	25-35	25-45	25-30
	(1)	(2)	(3)	(4)	(5)
IV: Any College (Parent)	0.230***	0.468***	0.377***	0.171***	0.286***
	(0.052)	(0.057)	(0.051)	(0.060)	(0.071)
OLS: Any College (Parent)	0.255***	0.278***	0.255***	0.264***	0.243***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
Observations	308,121	131,742	187,525	262,711	119,055
R-squared (OLS)	0.115	0.114	0.109	0.125	0.105
Birth County x Gender FE	Yes	Yes	Yes	Yes	Yes
Parent Gender x Gender FE	Yes	Yes	Yes	Yes	Yes
Relationship to HH head FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
First-stage KP F-stat	247.9	432.6	303.6	256.7	131.3
Mean of dependent variable	0.583	0.533	0.608	0.563	0.639

Notes: Sample includes all respondents in the 2017 census with ages in the bandwidth described in the header that we can connect to at least one parent that (I) was born between 1943 and 1960 (both years inclusive) and (II) that reported full secondary education. Possible parent-child linkages include: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. "Yr Age 21 Parent" is a continuous variable indicating the year at which the parent reached 21 years of age, normalized to zero in 1972. "Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)" is the interaction of this variable with a dummy for parents that reached age 21 on or after 1973. The interaction term is used as excluded instrument for any college education by the Parent (the trend is an included instrument). All regressions include county of birth x gender, parent's gender x (child) gender, age and relationship to household head fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A22: Assortative matching and IGT of Education (Census 2017)

Dependent variable:	Parent's spouse		Any College		
	observed	Any College			
	(1)	(2)	(3)	(4)	(5)
[a] Yr Age 21 Parent	0.006***	-0.0001	-0.0003	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
[b] Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	-0.004***	-0.008***	-0.007***	-0.006***	-0.004**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
IV: Any College (Parent)	0.171***	0.395***	0.317***	0.290***	0.228**
	(0.047)	(0.060)	(0.055)	(0.080)	(0.091)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes
Parent gender x gender FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Parent's spouse observed FE	No	No	Yes	No	No
Parent's spouse any college FE	No	No	No	No	Yes
Observations	213,059	133,200	213,059	133,200	133,200
R-squared	0.426	0.086	0.068	0.069	0.110
p-value a+b=0	0.000	0.000	0.000	0.000	0.000
First-stage KP F-stat	270.3	185.1	271.7	185.1	145.9
Mean of dependent variable	0.633	0.212	0.585	0.602	0.602

Notes: Dependent variable in the header of each panel. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that are children of household heads meeting two conditions: (I) born between 1943 and 1960 (both years inclusive) and (II) that reported full secondary education. In columns 2,4,5, sample is further restricted to respondent's for which we observe the spouse of the household head. Spouse includes married, civil union and living together. "Yr Age 21 Parent" is a continuous variable indicating the year at which the parent reached age 21, normalized to zero in 1972. "Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)" is the interaction of this variable with a dummy for parents that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education by the Parent (the trend is an included instrument). All regressions include (a) county of birth x gender (b) parent's gender x (child) gender, (c) age (of child) fixed effects. Column 3 adds a dummy indicating whether the spouse of the household head is observed. Column 5 includes a dummy indicating whether the spouse of the household head enrolled in college. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A23: Fertility and IGT of Education (Census 2017)

Dependent variable:	Total children	Share alive	Mother's age at birth	Any College			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
[a] Yr Age 21 Mother	-0.027***	0.001***	-0.648***	-0.002	-0.002	0.010***	
	(0.0052)	(0.0003)	(0.0113)	(0.0013)	(0.0013)	(0.0015)	
[b] Yr Age 21 Mother	0.014**	-0.0002	-0.018	-0.007***	-0.007***	-0.004**	-0.004**
x $\mathbb{1}$(Yr Age 21 Mother $\geq$ 1973)	(0.0062)	(0.0003)	(0.0151)	(0.0017)	(0.0017)	(0.0019)	(0.0019)
IV: Any College (Mother)	-0.574**	0.006	0.722	0.296***	0.296***	0.195**	0.192**
	(0.251)	(0.013)	(0.620)	(0.066)	(0.066)	(0.080)	(0.080)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relationship to HH head FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Total children (Mother) FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Share children alive < 1 (Mother) FE	No	No	No	No	Yes	No	No
Age at birth FE	No	No	No	No	No	Yes	Yes
Observations	94,166	94,164	94,164	94,164	94,164	94,164	94,164
R-squared	0.030	0.062	0.333	0.079	0.079	0.080	0.080
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000	-
Mean of dependent variable	2.765	0.979	30.48	0.563	0.563	0.563	0.563
First-stage KP F-stat	184.0	174.7	185.3	174.7	175.1	136.6	136.8

Notes: Dependent variable in the header of each column. Sample includes all respondents in the 2017 census that we can connect to their mother, who meets the following conditions: (I) reached age 21 between 1964 and 1981 (both years inclusive) and (II) reported full secondary education. Possible parent-child linkages include: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. "Yr Age 21 Mother" is a continuous variable indicating the year in which the mother reached age 21, normalized to zero in 1972. "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the interaction of this variable with a dummy for mothers that reached age 21 on or after 1973. In the IV estimates, the interaction term is used as excluded instrument for any college education by the mother (the trend is an included instrument). Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1