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 restricted access to college in the context of the military dictatorship of Augusto Pinochet in Chile following the 1973 coup. We find three main results: (i) cohorts that reached college age in the years immediately after the coup experienced a steady decline in college enrollment as a result of the continual reduction in the number of openings for incoming students decreed by the regime; (ii) these cohorts had worse outcomes along multiple margins throughout the life cycle and struggled to climb up the socioeconomic ladder, especially women; (iii) children with parents in the affected cohorts also have a substantially lower probability of college enrollment. These results demonstrate that restrictions on the supply of higher education in non-democracies hinder social mobility and lead to a persistent reduction in human capital accumulation, even after democratization.

Keywords: Dictatorship, higher education, social mobility, intergenerational transmission
JEL codes: I23, I24, I25, P51

1 Introduction

Institutions of higher education are typically devoted to critical enquiry and uncompromising debate, making them a thorn in the flesh for governments of all types.[1] While democracies often accommodate the demands of protest movements originating within universities, autocracies usually respond with hostile policies of various kinds (Connelly and Grüttner, 2005).[2] These include restrictions on the composition of faculty and the student body (Waldinger, 2010, 2011), and the manipulation of educational content (Cantoni et al., 2017). Broader reductions in the supply of higher education are also common, especially since ideological indoctrination may prove ineffective or even backfire (Fouka, 2020). Prominent examples include the multi-year shutdowns of universities in China and Iran during their respective Cultural Revolutions.[3] A more recent example is the closure of Central European University by Hungarian strongman Viktor Orban in 2018. Several theoretical papers have studied the ruling elite's decision to restrict or expand access to education (Bourguignon and Verdier, 2000; Glaeser et al., 2007; Cantoni and Yuchtman, 2013; Alesina et al., 2018), but the empirical evidence on the socio-economic consequences of such policies and their effect on the intergenerational transmission of human capital remains limited.

In this paper, we study the effects of the capture of higher education by the dictatorship of Augusto Pinochet in Chile following the military coup of 1973. This coup was one of many encouraged by the US government in its fight against Communism during the Cold War (Dube et al., 2011). As part of its attempt to eliminate any source of political opposition, the Pinochet regime quickly took over the administration of all universities in the country and, over the following years, continuously tightened the number of openings for incoming students. As a result, college enrollment dropped by around one third between 1974 and 1980. Using administrative historical records, we confirm that the supply of college openings drove this pattern, as it remained far below the number of college applications throughout this period.

We examine the effects of this reduction in educational attainment on economic and non-economic outcomes throughout the life cycle, and whether it is transmitted across generations. 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, and exploits quasi-random exposure to increasingly fewer college openings, 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 identi-

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[1]"Freedom in research and training is the fundamental principle of university life" says the *Magna Charta Universitatum*, a document published in 1988 to conmemorate the 900th anniversary of the *Authentica Habita* signed by Emperor Frederick I on 1155 protecting scholars at the University of Bologna from persecution (OMCU, 2019).

[2]Maurin and McNally (2008) show that French authorities temporarily reduced the standards for college admission in the aftermath of the 1968 protests to benefit those that had participated.

[3]On China, see Roland and Yang (2017); Li and Meng (2018); Alesina et al. (2020).

fying assumption that in the absence of the coup we should not observe any kinks. Most of the analysis uses individual-level census data from 1992, 2002 and 2017, which we complement with data from a large biennial household survey (CASEN) in the period 1990-2015. We pay particular attention to heterogeneous effects by gender, given that women were making large gains in access to higher education and labor force participation in the years before the military coup.

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 censuses to document the presence of kinks in our outcomes of interest at different points in the life cycle over a 25-year period. Second, we verify that our results hold under increasingly conservative bandwidths of cohorts reaching college age around the military coup, which are arguably less affected by non-linearities. Third, when using the biennial CASEN survey we estimate a more stringent specification that replaces the pre-coup trend with age fixed effects. This way we compare people from different cohorts at the same age, allowing the outcome to vary flexibly at each point in the life cycle.

In the first part of the paper, we study the educational attainment of the cohorts affected by the decrease in college openings. We restrict the analysis to people reporting complete secondary to have a better counterfactual for college enrollment. We document a sharp kink in college entry and completion that coincides with the military coup. Panel (a) in Figure 1 shows that while 38% of the cohort reaching college age in 1972 went to college, only 25% of the one reaching the same age in 1981 did. Panel (b) indicates that women were benefiting most from the expansion in higher education in the pre-coup years, but both genders were equally affected by the drop in openings. These patterns are present in all data sources available. We find evidence of imperfect 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 military coup.

Since 1967, university admissions in Chile have operated through a deferred-acceptance algorithm that ranks students based on their grades in secondary and a centralized admissions exam. Importantly, the military regime preserved the admissions system unmodified. As a result, the reduction in college openings affected all segments of the population. We find that the kink in college enrollment is present within families (i.e. siblings) as well as within all quintiles of housing wealth in 1992. The reduction in openings arguably affected students with relatively lower test scores, who would have been admitted had openings remained constant or kept growing, but who did no longer make the cut when the number of openings decreased. We observe that the college premium increases for the affected cohorts, consistent with an increase in the quality of admitted students or with imperfect substitution between similarly-educated workers in different age groups (Card and Lemieux, 2001). However, we show that both the distribution of students across fields and the

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socio-economic composition of the student body remained largely unchanged.

In the second part of the paper, we examine the economic consequences of decreased access to higher education. We provide reduced-form estimates of kinks in cohort trends around the time of the military coup and instrumental variables (IV) estimates of the effect of college entry, using the kink in the enrollment trend after 1973 as the excluded instrument. The IV estimates capture the average effect of college entry for the set of compliers whose college enrollment was affected by the changes in policy implemented by the military dictatorship (i.e. the marginal students discussed above). 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 and 2002 (ages: 30s and 40s). These effects are at least 50% 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, rather than self-employment, business ownership, domestic work or unpaid work with relatives. 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.

We also find a sharp kink in various measures of self-reported income in the CASEN survey between 1990 and 2015. Our IV estimate indicates that college enrollment increases total income by 76 log points in our baseline specification and by 23 log points in the more conservative specification with age fixed effects. The return to college is weakly larger for men, but the difference disappears once we include age fixed effects. Using information on the distribution of housing wealth from the 1992 census, we find that college enrollment increases the probability of being in the top quintile by 35 percentage points, equivalent to 70% of the sample mean. This effect is balanced by roughly equal decreases in the probability of being in each of the following three quintiles and by a smaller decrease in the probability of being in the bottom quintile. This constitutes *prima facie* evidence that restricted access to higher education hinders social mobility.

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 the 1992, 2002 and 2017 censuses. While this could be explained by the fact that these are younger cohorts, we find that they are more likely to report being either children or parents of the household head in all three censuses. 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 also affects the human capital of their children, who reached college age after democratization. We connect parents (with full secondary) to their children using various combinations of positions in the household in the 2017 census. Most of our sample is comprised of children of the household head or of the spouse. We also include household heads, their spouses or their siblings if parents live with them. The fact that we can only study individuals living with a parent naturally introduces some selection. People in our sample are more likely to have full secondary and to attend college than the average, but this is mostly a result of the restriction that the parent must have full secondary. Our preferred specification includes the following sets of fixed effects: (i) county of birth by gender, (ii) parent's gender by (own) gender, (iii) relationship to household head, (iv) age. 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 slightly more than one half of the sample mean of 58%. Looking at lower levels, we find that parental college enrollment has no effect on primary education (which is mandatory in Chile since 1965), but does reduce dropout at all levels of secondary education (which only became mandatory in 2003). However, dropout in secondary only explains 12% of the intergenerational effect on college enrollment. Positive assortative matching of parents with college explains roughly 20% of the effect.

This paper connects several strands of literature studying the political economy of education. First, it adds to the literature on the relationship between political regimes and education provision by providing causal evidence of a negative effect of dictatorship on the supply of higher education.[4] Second, as mentioned above, the paper documents a channel other than dismissals (Waldinger, 2010, 2011) and curricular reform (Cantoni et al., 2017) through which the capture of higher education in non-democracies takes place. Third, the paper contributes to the literature studying the relationship between democracy and inequality (Acemoglu et al., 2015) by showing that cutbacks to higher education under dictatorship hinder social mobility and female progress in the labor market. In this regard, 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 country over the last decade. Lastly, the paper also speaks to the respective literatures on the effects of (i) regime change (Dube et al., 2011; Berger et al., 2013), (ii) political partisanship (Ferreira and Gyourko, 2009), and (iii) political turnover (Akhtari et al., 2018) on government policies and downstream outcomes by documenting the transformation of higher education caused by the abrupt transition from a radical, far-left government to a reactionary, right-wing one.

We also contribute to a large literature on the monetary and non-monetary returns to education

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[4]Several papers have documented a (mostly cross-country) positive link between democracy and educational spending and enrollment at lower levels (Baum and Lake, 2003; Brown and Hunter, 2004; Avelino et al., 2005; Ansell, 2010; Harding and Stasavage, 2013). See Aghion et al. (2018) or Paglayan (2018) for opposite findings. Evidence on higher education is scarce and points to a null effect (Stasavage, 2005; Gallego, 2010).

([Card](), [1999](); [Oreopoulos and Salvanes](), [2011]()). Our paper relates to previous studies on (i) the effects of higher education on social mobility ([Chetty et al.](), [2017]()), and (ii) the differential effects of higher education on outcomes for women ([Goldin](), [1992](), [2006]()). We show that college entry improves economic and non-economic outcomes throughout the life cycle and dramatically affects a person's chances of climbing up the socioeconomic ladder, especially for women at a time of structural transformation and rapid progress in the labor market.[5]

The paper also contributes to the literature on the intergenerational transmission of human capital.[6] 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 ([Black et al.](), [2005](); [Oreopoulos et al.](), [2006]()). A few studies have analyzed the relationship between parental college and children's early-life outcomes or educational attainment at lower levels ([Currie and Moretti](), [2003](); [Maurin and McNally](), [2008](); [Roland and Yang](), [2017]()). 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 (i.e., increased agency of child, 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 contribute by providing evidence of a positive causal link in intergenerational college enrollment. Regarding external validity, this literature has largely focused on a handful of highly developed countries in Europe and North America and we contribute by studying a middle-income country in Latin America.

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 a large reform in 1981.[8] The oldest university was Universidad de

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[5]A few papers study episodes of political disruption to higher education in other settings ([Maurin and McNally](), [2008](); [Li and Meng](), [2018](); [Ozturk and Tumen](), [2018]()). These studies look at fewer outcomes over shorter time horizons and do not consider heterogeneous effects by gender. Only [Maurin and McNally]() ([2008]()) studies intergenerational transmission of human capital, but only for fathers and at lower levels.

[6][Black and Devereux]() ([2011]()) and [Björklund and Salvanes](), [2011]() provide overviews.

[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).

[8]These universities were Universidad de Chile, Universidad Técnica del Estado, Universidad Católica, Universidad de Concepción, Universidad Católica de Valparaiso, Universidad Austral, Universidad Federico Santa María, Universidad del Norte. The 1981 reform turned satellite campuses of the existing public universities into independent institutions and opened the system to competition by new universities. These were not eligible for government funding, which was also substantially reduced for existing universities, causing an increase in tuition fees. The reform also

Chile, founded in 1842, and the most recent one to open was Universidad del Norte, founded in 1956. Only two universities were public, but the entire system was largely financed by the government. Most universities had their main campuses in the larger cities of Santiago, Concepción and Valparaiso, but several had smaller satellite campuses distributed throughout all the regions of the country. Faculty mostly had part-time appointments and rarely had graduate degrees.[9]

College enrollment quickly expanded in the 1960s, growing from around 25,000 students in 1960 to 77,000 in 1970. Panel (a) in Figure 2 shows that the gross enrollment 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 enrollment, which reached 146,000 students in 1973, corresponding to a gross enrollment rate of 16.8%. Almost 40% of college students were female and 67% were enrolled in public universities. This was a time of massive expansion in access to higher education throughout Latin America (Brunner, 1984).

A movement for educational reform began in 1965 under the center-left government of Eduardo Frei. At the university level, the reform started in 1967 in response to gains in political leverage made by the student movement. Besides the large increase in enrollment, the movement's main achievement was greater student and faculty involvement in university governance. Academic structures were also modernized, in an effort to resemble the U.S. model, and increased funding allowed for new programs and research centers. Differentiated tuition based on family income was introduced, but fees were not very high. The Allende government tried to make access to university more inclusive, with mixed results (Castro, 1977; Schiefelbein and Farrell, 1984).

Between 1850 and 1966, students wishing to enroll in college had to take a baccalaureate exam administered by Universidad de Chile. The reform also replaced this test with a new one called "Prueba de Aptitud Académica – PAA" (Academic Aptitude Test) in 1967. Students could take the PAA test multiple times, but there was only one sitting of the exam per year. Those applying to college provided a ranking of their preferred university-program combinations and were awarded a score based on their grades in secondary and their PAA results. The weight awarded to each component was determined by each university and could vary by program. Each university also determined the number of openings in each of its programs. A deferred-acceptance algorithm then matched students to openings (Koljatic and Silva, 2020). Leaving aside some small modifications, the admissions process remains largely unchanged until today.

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reorganized smaller institutions providing post-secondary technical training.

[9]In 1967, 86% 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).

2.2 Higher Education in Chile After the Military Coup

Amid growing political polarization and deteriorating economic conditions, Allende was over-thrown by a military coup on September 11, 1973. A junta presided by General Augusto Pinochet assumed all executive and legislative powers and would go on to govern the country until 1990. One of the main aims of the military government 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 named members of the armed forces as rectors of all universities, both public and private, claiming that

> "several campuses and universities have become centers for Marxist indoctrination and propaganda... A large part of the extremist agitation and hate preaching that almost drove Chile down a tragic abyss, originated in these universities"
> (Brunner, 2008, p.137, *own translation*).

The military rectors had unrestricted power (Castro, 1977; Brunner, 1984).[10] During the first months after the coup, many students, faculty and staff were expelled or dismissed for their political views, though the exact numbers remain unclear.[11] Some were detained, tortured, or killed.[12] All student groups and faculty or staff unions were shut down, political activity was forbidden and teaching materials were censored.

The military regime also saw a connection between an excessively large university system, low standards of quality and political opposition. In the words of the Secretary of Interior:

> "the mediocrity that prevails in most of our higher education... [is] a source of frustration for thousands of students, who easily become a breeding ground for political agitation" (Brunner, 2008, p.147, *own translation*).

In the following years, the gains in college enrollment achieved during the period of educational reform were largely undone. Panel (a) in Figure 2 shows a steady decline in the gross enrollment rate from 16.4% in 1974 to 10.8% in 1980 (34% drop), only slightly higher than at the start of the Allende government in 1970. That this drop in enrollment was not anticipated is evidenced by the fact that projections by UNESCO placed aggregate enrollment at around 200,000 for 1975, while the actual figure fell slightly short of 150,000 (Levy, 1986).

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[10]Panel (a) in Appendix Figure B1 shows that military control over universities slowly subsided in the mid-1980s.

[11]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 just from Universidad de Concepción by 1974. According to Castro, 228 researchers in the natural sciences left Chile between 1971 and June 1974, 165 after the military coup.

[12]Using detailed individual records from the final report by Comisión Rettig (1996), we find that among the roughly 3,200 documented victims of the Pinochet regime there were 24 university professors and 252 university students. The report by the subsequent Comisión Valech (2004) estimates that around 4,100 of the 38,000 subjects who experienced such human-rights violations were students (from all levels).

Despite the repressive measures outlined above, this decline was mostly driven by a reduction in the yearly number of incoming students rather than by dropout of students already enrolled (Levy, 1986). To disentangle the role of demand and supply, panel (b) plots three series: the yearly number of people taking the PAA test, the subset that effectively applied to college and the number of openings made available by the universities. Openings grew from 16,000 in 1967 to 20,000 in 1970 (30% increase), and reached a maximum of 47,214 in 1973, corresponding to a 130% growth rate during Allende's tenure. But they rapidly declined after the coup and reached 32,954 by 1980 (a 30% drop relative to 1973).[13] On the other hand, the number of test-takers and applicants both increased between 1967 and 1975, when they exceeded 100,000 each. Both series fell in the following years, suggesting staggered adaptation to the drop in openings. Importantly, the number of applicants generously exceeded the number of openings in all post-coup years, indicating that supply was always the binding constraint for college enrollment.[14] Panel (c) further shows that the raw (hence, comparable) average scores in the verbal and math modules of the PAA test increased in the years after the coup. This suggests that the drop in test-taking was driven by people with lower expected scores, who adjusted their expectations as college spots continuously tightened.[15]

The drop in openings was matched by a decrease in government funding. Panel (d) in Figure 2 shows that the share of public spending on education devoted to higher education increased in the years before the coup, reaching a staggering 40% in 1973 (6% of Gross National Product, GNP), but dropped to around 30% in the following years (4% of GNP). In the eyes of the regime, excessive growth led to a bloated and inefficient university system that served a privileged minority of students (Brunner, 2008). As part of its pro-market reforms, the regime floated the idea of universities becoming fully self-sufficient by 1976, but this idea faced strong resistance and was not implemented.[16] As a result, higher education's share of the education budget (or GNP) in 1980 remained at levels similar to the late 1960s. Panel (c) also shows that public spending per student steadily increased after 1975, following a couple of years of high volatility in the immediate aftermath of the coup. This means that enrollment fell disproportionately more than funding and suggests that the quality of university education did not decrease in the early years of the dictatorship. However, the expulsions, faculty dismissals and other forms of repression taking place at this time likely had a negative impact on the student experience.

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[13]Panel (b) in Appendix Figure B1 shows that the drop in openings was more marked in public universities, which had also driven most of the growth in the pre-coup years, while openings in private universities simply stagnated.

[14]Panel (c) in Appendix Figure B1 shows that this conclusion is unchanged if we use an alternative measure of 'regular' openings. Appendix Figure B2 considers two additional response margins. Panel (a) shows that the number of enlisted soldiers remained constant after the coup. Panel (b) shows that the number of Chilean students abroad increased between 1960-1975 and slightly decreases afterwards. In both cases, the variation is too small to explain the observed drop in college enrollment.

[15]Panel (d) in Appendix Figure B1 shows that the drop in test-taking is largely driven by people from older cohorts.

[16]In 1972, 2.6% of revenue at the most selective private university, Universidad Católica, came from tuition fees and 89% from government subsidies. In 1977, these figures were 7.5% and 69.8%, respectively (Brunner, 2008).

While the reduction in openings varied across fields, the composition of the student body remained largely unchanged. Panel (a) in Figure 3 shows the total change in openings between 1973 and 1980 by field of study, using UNESCO categories. With the exception of the natural sciences, all fields saw a net decrease in the number of openings, with magnitudes ranging from -20% to -50%. The fields most affected were agriculture and social sciences, which arguably have more politically active students, but the cuts were not much smaller in more traditional fields like health or law. Panel (b) shows the distribution of enrolled students in three key years: 1967 (early educational reform), 1973 (military coup) and 1980 (end of sample period). The distributions are remarkably similar, especially in 1973 and 1980, suggesting that the impact of the differential reduction in openings across fields was negligible. Panels (c) and (d) show information on the family background of admitted students. Unfortunately, this information is only available for 1976 and 1981, both post-coup years, but it remains useful insofar as openings and enrollment kept falling between these years. Panel (c) shows the educational attainment of the father of admitted students, while panel (d) displays the occupation. The distributions are almost identical in both cases. These plots indicate that the impact of the cutback to higher education implemented by the military regime extended to all segments of the population. We return to this question below when we look for kinks in college enrollment within families or wealth quintiles in 1992.

3   Data and Empirical Strategy

3.1   Data

Our main source of data are the Chilean household censuses of 1992, 2002 and 2017.[17] The census files provide 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 of each household. 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. For example, the 2017 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 calculated variable indicating the wealth quintile to which the household belongs based on the observable characteristics of the dwelling and ownership of various assets.

We complement the information in these censuses using twelve waves of the biennial CASEN household survey between 1990 and 2015.[18] This is a repeated cross-section that is representative

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[17]Online Appendix A provides more detailed information about these sources.

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

at the regional level.[19] The 2015 wave has data on more than 260,000 individuals in over 80,000 households. The CASEN survey includes 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.[20] Our main variable of interest is the harmonized measure of educational attainment, but we also use other characteristics of the countries to create a synthetic comparison group for Chile that best reproduces the evolution of college graduation in the years before the military coup.

Our main sample 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 1973, the year of the military coup. We verify that our 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.[21] We show below that the results are robust to changes in the age of college entry (i.e. changes in the kink point). We only keep respondents that report having completed secondary in the 2017 census and those that report at least four years of secondary in other sources that do not ask about completion. We introduce this restriction to ensure a relevant counterfactual for college enrollment, but the results are qualitatively similar otherwise.

For the study of intergenerational effects, we exploit the information on household composition contained in the 2017 census. This census is the best source of information on the final level of education obtained by children with a parent in the affected cohorts.[22] 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 (who we always observe). The second largest category is comprised of heads of households in which at least one individual reports being a parent of the head.[23] We observe around 12,000 such cases (5%). 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 is observed) or children of the spouse. We restrict the sample to children with ages between 25 and 40. We exclude younger individuals to improve our chances of observing final college enrollment and

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

[20]Appendix Table A1 provides the list of these countries.

[21]Appendix Figure B3 shows the age distribution of first- and last-year college students in 1960, 1970 and 1975.

[22]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.

[23]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 study potential complementarities in the effects of both parents' education.

older individuals to ensure balance in the distribution of parental cohorts. We verify that the results are robust to changes in the ages of children in the sample. Having linked parents and children, we restrict this sample to parents meeting the same conditions as in our main sample above: (i) secondary completion, (ii) reaching age 21 between 1964 and 1981. 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 H1 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 the linked parent having full secondary. People in our sample are less likely to be employed and more likely to be studying. Hence, our sample has the desirable feature of including those individuals with non-negligible probability of enrolling in higher education. Children of the household head make up 90% of our sample, compared to 26% in the population with ages 25-40. Women in our sample have half as many children as in the broader population.

3.2 Empirical Strategy

We measure the effects of the reduced supply of higher education by the military regime by 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.[24] As mentioned above, we classify cohorts based on the year in which they reached twenty-one years of age because this was the average age of first-year college students at the time. We work with the following reduced-form model to estimate the effect of exposure to the dictatorship:

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

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 $a21$. 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. $\mathbb{1}(a21 \geq 1973)$ is a dummy variable equal to one for those individuals (cohorts) that reached age twenty-one in 1973 or later, and u_{ia21} is an error

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[24]Several papers studying the impact of sharp changes to compulsory schooling laws across cohorts make the equivalent assumption in a regression-discontinuity setting (e.g., Oreopoulos, 2006).

term clustered at the county-of-birth level. Finally, $f(a21)$ and $g(a21)$ are smooth functions (polynomials) representing the birth cohort 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. We focus on a linear polynomial (i.e., $f(a21) = g(a21) = a21$) to avoid over-fitting and provide visual evidence and a formal test showing that it describes appropriately the evolution of cohort means for most of our outcomes of interest.[25] Thus, our parameter of interest is π_1, which directly captures the change in trend after 1973:

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

We use a symmetrical bandwidth of 18 cohorts reaching college age around the year of the military coup. 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 regulated age of college entry, which leads to a fuzzy treatment assignment at the kink. We verify below that our results are not sensitive to small changes in the location of the kink. We end the sample with the 1981 cohort to mitigate the confounding effect of the large reform of the Chilean university system that was implemented by the military regime after that year. Starting with the 1964 cohort 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 verify the robustness of our results to alternative choices.

A valid concern surrounding our empirical strategy is that our cross-cohort comparison may be picking up non-linear age effects in any one cross-section. 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 us to estimate a more stringent specification that replaces the baseline cohort trend with a set of flexible age fixed effects (plus survey year fixed effects). In this case, the counterfactual for the affected cohorts is constructed using the average of the outcome among unaffected cohorts when they had the same age. Finally, the robustness of our results to shorter bandwidths reduces the likelihood of confounding non-linear age effects.

We can also leverage the cross-cohort variation in college enrollment triggered by the dictatorship as an excluded instrument to provide Instrumental Variables (IV) estimates of the effect of

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[25]In Appendix E we present a simple test on the null hypothesis of a linear kink in the data. Figure 4 and Appendix Figures E1-E7 plot the raw data for all our outcomes of interest and display the respective p-value from this test.

attending college. For this purpose, we estimate the following system of equations:

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

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

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 as excluded instrument. We focus on college enrollment, rather than completion, because this is the margin that was directly affected by the dictatorship's policies. Hence, our estimates likely provide a lower bound for the effect of a college degree. 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 the average causal effect of college entry for compliers, i.e. those students whose college enrollment was affected because they reached college age in the years 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. The IV analysis requires an additional exclusion restriction implying that the change in outcomes for the cohorts that reached age twenty-one after 1973 is solely driven by the lower probability of college enrollment. In this regard, our use of a short bandwidth ensures that we only compare cohorts facing similar economic and political conditions at the critical juncture of exit from the education system and entry into the labor market (Oreopoulos et al., 2012; Schwandt and von Wachter, 2019).

To study the intergenerational effects of college enrollment, we use specifications analogous to the ones above. The corresponding IV estimate tells us the effect of parental college enrollment on the child's probability of enrollment. This is also a LATE estimate for those children with a parent whose enrollment was affected by the dictatorship. 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 observed parent. We also expand the set of individual controls X_i to include the following sets of fixed effects in our preferred specification: (i) gender by county of birth, (ii) gender by parent's gender, (iii) relationship to household head and (iv) age. The gender by parent's gender fixed effects limits our comparison to children of the same gender linked 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, who may have different outcomes due to time trends. However, comparing children of the same age born to parents from different cohorts implies comparing children with parents of varying ages at the time of birth, which could also confound the analysis. 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 Results: Educational Attainment

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.

Panel (a) in Figure 1 shows the share of people with full secondary that report attending university. 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). 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 enrollment increased 4 percentage points (pp) between the 1964 and 1972 cohorts, corresponding to an 11% increase. Between the 1972 and 1981 cohorts, college enrollment decreased 14 pp, corresponding to a 36% decrease. Had the previous trend continued, the college entry rate would have been almost 45%, rather than 25%, in 1981. Panel (b) disaggregates the data by gender. We observe that college enrollment was largely stable among men before the coup, but growing rapidly for women. After the coup, enrollment drops sharply for men and women.

Panels (a) and (b) in Appendix Figure B4 show that the number of observations and the share with full secondary increase smoothly for younger cohorts, ensuring that the drop in college enrollment is not driven by selective attrition (e.g., repression) or by reduced attainment at lower levels.[26] Panel (c) shows that the probability of college graduation, conditional on enrollment, was stable around 81% for age 21 cohorts up to 1970. The graduation rate drops between 1971 and 1974, reaching 79% for the latter cohort (10% increase in dropout). It then rebounds sharply and reaches 85% in 1981. This pattern likely reflects (i) exits by existing students in the aftermath of the coup (i.e. expulsions), and (ii) the tightening of admission standards caused by the drop in openings and the greater focus on academic achievement imposed by the regime. Importantly, the data shows that the vast majority of college students completed their degrees both before and after the coup.

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[26]Appendix Figure B5 shows that these patterns are also present in previous censuses and the CASEN survey.

4.2 Parametric Analysis

Table 1 presents estimates of equation (1) for college enrollment using data from the 2017 census.[27] This specification formally extrapolates the upward trend in enrollment observed in the cohorts reaching age 21 between 1964-1972 to estimate the magnitude of the change experienced by those reaching this age between 1973-1981. Column 1 shows that college enrollment increased on average 0.5 pp per year for the cohorts reaching college age before the coup. This trend *decreased* by 1.9 pp 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. Panel (a) in Figure 4 provides a visualization of our estimates. The markers show average college enrollment per cohort, while the lines indicate the respective trends before and after the coup. We find that the parsimonious linear trends accurately approximate the break in the data.[28]

Column 2 in Table 1 provides evidence of mild substitution of college education with technical schooling (lower-level tertiary). The average yearly increase in the entry rate to these institutions increased from 0.01 pp in the pre-coup period to 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, albeit at a lower rate than enrollment due to dropout. This trend becomes -1.0 pp for the affected cohorts. The drop in completion is equivalent to 71% of the drop in enrollment, indicating that most of the people that failed to enroll after the coup would have gone on to graduate. For technical schooling, the trends in enrollment and completion roughly coincide, suggesting little dropout before or after the coup. For higher education as a whole, the trend in the graduation rate experienced a net decline of -0.7 pp per year after the coup (column 6).

Appendix Table G1 provides disaggregate estimates by gender. In the pre-coup years, female college enrollment was growing 0.8 pp per cohort, while male enrollment had a yearly growth rate of 0.2 pp. After the coup, the net trend for men equals -1.3 pp, while for women it is -1.5 pp, consistent with the graphical evidence in panel (b) of Figure 1.

4.3 Synthetic control analysis

We conduct a synthetic control analysis to provide additional evidence on the impact of the dictatorship on college entry (Abadie and Gardeazabal, 2003). For this purpose, we use the most recent 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

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[27]Appendix Table C1 shows similar results for the 1992 and 2002 censuses and the CASEN household survey.

[28]Appendix Table C2 shows that the results are not sensitive to small changes in the location of the kink point (i.e. fuzzy onset of exposure to dictatorship), but that our baseline specification has the best fit on the data.

if we also use data from other countries.[29] We calculate the share of people with complete college education, complete secondary education, and complete primary education per cohort in each census, restricting the sample to individuals over 20 years of age. We use college *completion* instead of *enrollment* as the outcome variable because only the former is harmonized across countries in IPUMS. All estimates use lags of the share of people with completed college education to build the synthetic control.[30] Our baseline estimates do not include controls, but results are unaffected if we control for the share of people with ages 18-65, the share of women, or the share of people with secondary education.

Figure 5 shows the results. The solid line shows actual educational attainment by cohort in Chile. The dashed line shows the prediction from the synthetic control. In panel (a), the outcome is complete college education. We observe that the synthetic control tracks the realized time series very closely up to the year of the coup and exceeds it afterwards. The synthetic control keeps growing, while the actual series stagnates and falls.[31] 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   Heterogeneous Effects and College Premium

In this section, we examine whether the drop in college enrollment disproportionately affected certain socio-economic groups and whether it impacted the college earnings premium. Answering these questions helps us identify potential differences in the characteristics of the average college student before and after the coup that could affect the interpretation of our IV results below. We present results from two exercises aimed at shedding light on these issues. The first one involves

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[29]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. Appendix Figure D1 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.

[30]We follow Ferman et al. (2019) and use only *odd* years to avoid cherry picking and overfitting. Appendix Table D1 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.

[31]Placebo inference and confidence sets suggest this difference is statistically significant (Abadie et al., 2015; Firpo and Possebom, 2018). See Appendix Table D1 and Appendix Figure D2 for details.

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examining whether there is a kink in enrollment within more tightly-defined sub-groups: (i) families (i.e. siblings) using data from the three available censuses and (ii) wealth quintiles using data from the 1992 census. The second exercise involves estimating the college earnings premium and examining whether it changes for the cohorts reaching college age after the military coup.

To examine whether the kink in college enrollment is also observed within families, we exploit the information on household composition contained in the population censuses. A first sample includes groups of two or more people that report being children of the household head. A second sample is comprised of household heads and their reported siblings. In both cases, we can be sure that 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 existing evidence of a strong positive correlation in the educational attainment of siblings in various settings (Björklund and Salvanes, 2011). If the reduction in college entry disproportionately affected certain people based on family-level characteristics, such as parental political affiliation or pre-coup socioeconomic status, the inclusion of these fixed effects should absorb most of the cross-cohort variation in enrollment. 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 a census year.

Table 2 shows separate estimates of equation (1) for each census year and each sub-sample. This analysis is quite demanding on the data, as the number of observations in all columns is an order of magnitude smaller than in the full sample. As expected, the size of the sample of household heads and siblings increases in later years while that of children of the household head decreases. In all columns, we observe the same kink in the cohort trend as in the main sample: the enrollment trend was positive for cohorts that reached college age in the years before the military coup and became negative for those that reached this age shortly afterwards. We now learn that this pattern is present even among people that share at least one parent and has a very similar magnitude to that in the larger sample (e.g., column 6 in Table 2 vs column 1 in Table 1).

Appendix Table C3 shows separate estimates of equation (1) for each quintile of housing wealth in the 1992 census. As mentioned in section 3, household wealth is determined based on characteristics of the dwelling and ownership of assets. The main caveat to this analysis is that these quintiles are assigned in 1992 and are plausibly themselves affected by college enrollment. 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 enrollment. 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. Each additional cohort in the top quintile experienced an increase of 2.1 pp in the college entry rate up to 1972, while for those in the bottom quintile this increase was only 1.3 pp (39% smaller). After the coup, the top quintile has a net trend of -1.6 pp, while the bottom quintile has a net trend of

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-1.3 pp. These results indicate that people across the entire socioeconomic spectrum experienced a decline in college enrollment and suggests limited selective targeting of admissions. The regime manipulated the total number of college openings, but could not systematically affect individual admissions due to the algorithm employed.[32]

The previous results provide evidence of a widespread impact of the drop in college enrollment. To further characterize the affected population, we use data on earnings from the CASEN survey. If, as expected, the compliers affected by the drop in enrollment had relatively lower ability, as proxied by admissions scores, and the quality of education remained roughly constant, we should observe an increase in the college premium after the coup. We estimate the following Mincer equation to test this hypothesis:

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

where $\ln \text{income}_{i,a21,j,t}$ is the natural log of reported real earnings for individual i belonging to the cohort turning 21 in year $a21$, that lives in county j and appears in the CASEN survey from year t. Our baseline analysis uses self-generated income, but Appendix Figure F1 shows 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 survey year fixed effects.[33] 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 $\mathbb{1}(\text{any college})$, which equals one for respondents that report attending college. As before, the sample only includes people born between 1943 and 1960 with 4+ years of secondary education.

Panel (a) in Figure 6 shows the results. For the cohorts that reached college age before the coup, we estimate a college premium of about 70 log points. After the coup, the college premium increases to about 80 log points (14% increase). This result indicates that the returns to college increased in the post-coup years, which is consistent with the predicted increase in the ability of admitted college students or with imperfect substitution between similarly-educated workers in different age groups (e.g., Card and Lemieux, 2001). What is certain is that the capture of higher education by the military regime furthered inequality between those that could and could not attend college. Panel (b) replicates the analysis including fixed effects for nine occupational categories. While the overall magnitude of the college premium decreases, consistent with college partly affecting income through occupational choice, we still observe a 14% jump in the premium for the cohorts that reached college age after the coup.

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[32]Appendix Table C4 explores heterogeneous effects based on proximity to college campuses (e.g., Currie and Moretti, 2003). The drop in enrollment was larger among people born in counties farther away from the main college campuses, who plausibly were marginal admitted students with relatively lower test scores before the coup.

[33]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 Results: Socio-economic Outcomes

In this section, we document the downstream effects of reduced college enrollment. We first examine several labor market outcomes. We rely on the 1992 and 2002 censuses for this part of the analysis because of data availability.[34] Also, the kink in college enrollment roughly coincides with the age of retirement in 2017, potentially biasing the results.[35] We then look at measures of income from the CASEN survey and study social mobility using novel data on housing wealth from the 1992 census. Finally, we examine marital status and the position within the household.

5.1 Labor Force Participation and Unemployment

Columns 1 and 2 in Table 3 show reduced-form estimates of equation (1) and IV estimates of equation (3) using labor force participation as the dependent variable. Each column uses data from a different census. The estimates show that the cohorts that reached college age before the coup had a positive trend in labor force participation. This could be a reflection of their higher educational attainment, but could also be caused by older cohorts leaving the labor market due to disability, early retirement, etc. In both years, we observe a large drop in the trend for the cohorts that reached college age after the coup. The fact that we observe kinks at two points in time that are ten years apart suggests that they are not driven by non-linearities related to age.[36] We observe the opposite pattern for unemployment in columns 3 and 4, controlling for labor-force participation. Here, the early cohorts had a weakly negative trend, which becomes positive for the affected cohorts. Panels (b) and (c) in Figure 4 show the kinks in labor force participation and unemployment in 1992.[37]

The IV estimates reported at the bottom of the table allow us to quantify the effect of college enrollment implied by the previous reduced-form estimates. We find that college enrollment leads to a 33 pp increase in labor force participation in 1992 and to a 57 pp increase in 2002. These are large effects, equivalent to 43 and 74% of the respective sample means. Similarly, college enrollment reduces unemployment by 6 pp in 1992 and by 2 pp in 2002. These are also large

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[34]The number of census respondents in the sample cohorts that report complete secondary education (or more) is unexpectedly larger in 2002 than in 1992. Panel (d) in Appendix Figure B5 shows that the difference is driven by higher education rather than secondary education and that it is present for all cohorts and is roughly constant across them (in relative terms). Potential causes include differences in quality between the censuses and additional education or selective migration (i.e. return form exile) in the years in-between. Given this inconsistency in the data, we focus on the robustness of the results to the use of either census, but abstain from drawing conclusions about changing effects of college enrollment throughout the life cycle based on differences in the coefficients.

[35]Men retire at 65 (women at 60), which is the age in 2017 of the cohort that turned 21 in 1973.

[36]The cohorts in the sample have ages 32-47 in 1992 (kink at 39) and ages 42-57 in 2002 (kink at 49).

[37]Appendix Figure E2 plots raw data and trends for all outcomes and years in Table 3.

effects.[38] Panels (a) and (b) in Figure 7 show IV estimates disaggregated by gender.[39] We find that the effect on labor force participation is at least 50% larger for women in both years, while that on unemployment is larger for men in 1992 and there is no difference in 2002. We conclude that college enrollment strongly increases a person's probability of being part of the labor force and that this effect increases with age and is substantially larger for women.

5.2 Occupation

Columns 5 and 6 in Table 3 show results for the probability of salaried employment. Alternatives include business owners and self-employed, domestic or unpaid workers helping relatives. The patterns in the data are very similar for the two censuses: salaried employment grew at a rate of 0.7 pp per cohort before the coup and slows down to 0.4 pp per cohort after the coup. Panel (d) in Figure 4 illustrates this kink in 1992. The IV estimates show that college enrollment increases the probability of salaried employment by around 9 pp, relative to sample means of about 70%. Appendix Table F1 shows that this gain in salaried employment comes at the expense of all other categories, including domestic and unpaid work. Again, we see how reduced educational attainment substantially worsened the available employment opportunities for the affected cohorts.

These effects are also highly heterogeneous by gender and indicate that college enrollment dramatically affected women's chances of engaging in salaried employment. Panel (c) in Figure 7 shows that the effect of college enrollment on salaried employment is three to four times larger for women, with an estimated effect size of around 15 pp. The full results in Appendix Table G3 show that half of this effect came from lower domestic work in 1992 and the other half from self-employment and unpaid work with relatives. In 2002, self-employment and business ownership were the categories most affected by college enrollment.

Columns 7 and 8 in Table 3 show that the affected cohorts also experienced large declines in the probability of having a high-skill, white-collar occupation. Panel (e) in Figure 4 provides evidence of a large kink in 1992. The IV estimates indicate that college enrollment increases the probability of this type of occupation by 48 pp in 1992 and by 22 pp in 2002. Appendix Table F2 shows that college enrollment decreases the probability of having white-collar, low-skill 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 enrollment substantially determines access to the more prestigious occupations.[40]

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[38]Appendix Figure F3 shows that the results on labor force participation remain positive and significant as we increasingly tighten the bandwidth. For 2002, the point estimate stabilizes at around 40 pp in the smaller windows (i.e. 1969-1976 age 21 cohorts), while the one for 1992 remains around 30 pp. For unemployment, the estimate for 1992 stabilizes at around -2pp in the smaller windows, while that for 2002 is more volatile and becomes statistically insignificant in some specifications.

[39]Appendix G provides full results of these estimations.

[40]Panels (c) and (d) in Appendix Figure F3 shows that these results are highly stable across bandwidths.

These effects are also much larger for women, as panel (d) in Figure 7 shows. The IV estimates for women in 1992 and 2002 are 62 pp and 29 pp, while for men they are 37 pp and 17 pp respectively. The full results in Appendix Table G4 further show that the increase in job status for college-educated women predominantly comes from reductions in white-collar, low-skill work, while men with college experience larger reductions in blue-collar work.

5.3 Income and Wealth

To analyze the effects of college enrollment on income, we rely on information from the CASEN household survey. This is a repeated cross-section collected roughly every two years since 1990. We pool all the survey waves for the main analysis and provide disaggregate results in the appendix. Income measures in CASEN include (i) income from the main occupation, (ii) total work income (i.e. more than one job), (iii) self-generated income (i.e. including non-work income, but excluding government transfers), and (iv) 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. This is less of a concern to the extent that measurement error equally affects the responses from people in different cohorts or is absorbed by the set of controls we discuss below.

Panel A in Table 4, shows reduced-form and IV estimates for all four income measures (in logs). The reduced-form results show generally positive trends for the pre-coup cohorts. On average yearly cohort gains amounted to 1.3 log points. After the coup, this trend reverses and becomes negative: each cohort has average income that is at least 1 log point lower than the one before. Panel (f) in Figure 4 shows the kink in self-generated income (averaged across years).[41] The IV estimates tell us that college enrollment increases average income by 50 to 90 log points, depending on the measure.[42] These estimates are comparable to the observational estimates reported by Rodríguez et al. (2015) using administrative data for Chile in 2010.

Panel B in Table 4 shows results from a modified specification that replaces the baseline trend in income with a more stringent set of age fixed effects. These fixed effects allow income to flexibly vary at each point 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 pre-coup cohorts at the exact same age. The IV estimates rely on this post-coup trend as an excluded instrument for college enrollment. We find a negative trend of 0.6 log points per affected cohort. The IV estimates show that college enrollment raises income by around 20 log points. These estimates are smaller than those from our baseline specification, but remain sizable and their magnitude is comparable to previous quasi-experimental evidence for college admissions in the US (Zimmerman, 2014).

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[41]Appendix Figure E3 shows equivalent plots for other income measures.

[42]Appendix Figure F2 shows that the results are fairly stable across survey waves, indicating a systematic effect on income throughout the life cycle. Appendix Figure F4 shows that the IV estimates are robust across bandwidths.

Panel (e) in Figure 7 shows that the return to college is weakly larger for men in the baseline specification, but this difference disappears once we include age fixed effects.[43]

We complement the analysis on income using data on housing wealth from the 1992 population census. In this census, households are classified into quintiles based on characteristics of the dwelling and ownership of assets. Fifty percent of our sample (with full secondary) belongs to households in the top wealth quintile, 25% to the fourth quintile, and 15, 8 and 2% to the lower three quintiles in order. Table 5 shows results using the 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 -0.2 pp per year in the probability of belonging to the top wealth quintile, this trend drops sharply for the cohorts that reach college age after the coup and becomes -1.5 pp per year. Panel (g) in Figure 4 shows this kink. The IV estimate indicates that college enrollment increases the probability of reaching the top quintile by 35 pp, equivalent to 70% of the sample mean, with a corresponding drop in the probability of being in the second to fourth quintiles of roughly 10 pp each.[44] We also find a quite sizable 2.4 pp decrease in the probability of being in the bottom quintile.

Panel (g) in Figure 7 shows that the effect of college enrollment on wealth is 37% larger for men.[45] College increases the probability of being in the top quintile by 41 pp for men but only by 30 pp for women. Zimmerman (2019) similarly finds that elite colleges in Chile help men reach top jobs and incomes, but not women. Our finding can also plausibly be driven by the fact that wealth is measured at the household level. 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, the marriage market would attenuate the effect of college enrollment on female social mobility. We explore the effect of college on marital status next.

5.4 Marital status

Table 6 shows results on marital status in 1992 and 2002, since the 2017 census did not collect this information. The dependent variable in columns 1 and 5 is a dummy for having ever been married. In columns 2-4 and 6-8, the dependent variables are respective dummies for being currently married, widowed or separated. In these columns, include an ever-married dummy to restrict the comparison to people that have been married. Columns 1 and 5 show that the negative trend in marriage sharply accentuates for cohorts reaching college age under dictatorship. Panel (h) in

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[43]Appendix Table G5 provides disaggregate results by gender for all income measures.

[44]Panel (c) in Appendix Figure F4 shows that the IV estimate for the top quintile stabilizes around 20 pp with smaller bandwidths and remains statistically significant. We also calculate quintiles of self-generated income using the information in CASEN. Results in Appendix Table F3 from the specification with age fixed effects show that college enrollment increases the probability of being in the top income quintile also by 21 pp.

[45]Appendix Table G6 provides full results.

Figure 4 illustrates the kink in 1992.[46] The IV estimates indicate that college enrollment increases the probability of previous marriage by 26 pp in 1992 and by 8 pp 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. Panel (i) in Figure 4 shows this kink in 1992. The IV results show that college enrollment reduces the probability of being a widow by 5 pp in 1992 and by 12.4 pp in 2002, more than four times the respective sample averages.[47] Increased widowing in younger cohorts goes against confounding age effects and suggests the existence of a negative relationship between college enrollment and mortality if people plausibly marry within the same age group (Buckles et al., 2016). We leave further exploration of this finding for a future study.

Appendix Table G7 provides disaggregate results by gender. The effect of college enrollment on the probability of having ever been married is at least 50% larger for men than for women in 1992 and 2002. One possible explanation is that women without college are more likely to get married than men without college, but this is not supported by the data.[48] Another explanation is that college enrollment has two opposite effects on women. It makes them more attractive partners in the marriage market, but it also increases their leverage *vis-à-vis* men and allows them to wait for a better match. We also find that the effect of college enrollment on widowing is 3-4 times larger for women, suggesting a larger effect on male mortality.

5.5 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 three composite categories: (i) household heads or spouses, (ii) their children, and (iii) their parents.[49] Table 7 shows the results. At all points in time, the trend in head/spouse status drops for cohorts reaching college age after the military coup, while the trends for dependents (parent or child of the head) increase. Panels (j) and (k) in Figure 4 provide visual evidence for 1992.[50] These results indicate that the affected cohorts experienced increased economic vulnerability throughout the life cycle and struggled to gain economic independence. They had a higher probability of being parents of the household head as early as 1992, which goes against non-linear age effects (i.e. older people increasingly likely to live with their children). Appendix Table G8 provides disaggregate results by gender. College enrollment predominantly increases the probability that men are the household head and that women are the spouse of the head. It decreases the probability of being a

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[46]Appendix Figure E5 plots the raw data and linear trends for all outcomes in Table 6.

[47]Panel (a) in Appendix Figure F5 shows that these estimates remain negative and significant in smaller bandwidths.

[48]The respective averages for men and women (with full secondary but no college) are 89% and 81% in 1992. In 2002, these averages are 91% and 83%. Both differences in means are statistically significant at the 0.01% level.

[49]We combine (i) spouses and partners, (ii) own children, stepchildren and grandchildren, (iii) parents of the household head and parents of the spouse. A residual category includes people living with siblings or other relatives.

[50]Appendix Figure E6 includes plots for all outcomes and dates in Table 7.

dependent (i.e. child or parent) for both genders at all points in time, but the effect on child status is systematically larger for men, while that on parent status is larger for women.

6 Results: Intergenerational Transmission of Human Capital

In this section, we explore potential effects on children with parents in the cohorts reaching college age after the military coup. We begin by studying the fertility of women in the affected cohorts and child survival. We then examine the educational attainment of these children.

6.1 Fertility and Child Survival

All censuses ask women the number of children they have given birth to. Columns 1, 4 and 7 in Table 8 show results for this variable in each census. All sources point to a negative trend in fertility among the pre-coup cohorts of -0.02 children per year. In 1992, the trend for the affected cohorts drops further and takes a net value of -0.07, but women in these cohorts are still in their thirties at the time and likely to have more children. In 2002, when the youngest cohort in our sample is 42 years old, the kink becomes positive, indicating that fertility fell at a lower rate for women in the affected cohorts. The results are very similar in 2017. The IV estimates suggest that college enrollment reduces total fertility by 0.3 children, equivalent to 13% of the sample mean of 2.3. However, panel (c) in Appendix Figure F5 shows that this effect decreases and becomes insignificant for smaller bandwidths.

The dependent variable in the remaining columns is the share of children that are still alive, based on the census question for the number of children that are still alive. The reduced-form results tell us that pre-coup cohorts experienced yearly gains of 0.2 pp in child survival. The fact that the trends are so similar across censuses suggests that most of the variation in child mortality comes from deaths in early life. For women in the affected cohorts, this trend is 0.06-0.08 pp smaller, indicating higher mortality for their children. The IV results suggest that college enrollment reduces child mortality by about 2 pp, a large effect relative to the sample mean of 1.7%, but this result is also sensitive to the bandwidth choice (panel (d) in Appendix Figure F5).[51] Finally, columns 3, 6 and 9 include a full set of fixed effects for the total number of children per woman. This restricts the comparison of child survival to women with the same number of children. The effect of college decreases by 40-50% after the inclusion of this additional control, suggesting the presence of a quantity-quality trade-off (Becker and Lewis, 1974).

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[51]Appendix Figure E7 plots raw data and linear trends for all outcomes and dates in Table 8.

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6.2 Children's Educational Attainment

In this section, we study the educational attainment of children with parents in the affected cohorts. We are particularly interested in their own probability of college enrollment. Section 3 discusses the construction of the sample, which includes more than 230,000 people between the ages of 25 and 40 that we are able to connect to a parent reaching age 21 after the coup. For this analysis, we use the same specifications as above, with the exception that the cohort-level variables refer to the parent, while the outcomes refer to the child.

To start, panel A of Table 9 shows that college enrollment among the parents of the children in our sample exhibits a pattern essentially identical to the full sample (i.e., Table 1), suggesting that these parents are not fundamentally different from the larger 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 a parent that reached college age before the coup, column 1 shows a positive trend in college entry of 0.4 pp per year. But this trend reverses for people with a parent in the affected cohorts and becomes -0.1 pp per year. This is evidence of a positive causal relationship between the college enrollment of parents and children. Panel C shows that having a parent that went to college increases a person's chances of enrolling by 26 pp, using the kink in parental college enrollment as an excluded instrument. This IV estimate is equivalent to 45% of the sample mean (58%), indicating a substantial *nurture* effect. It is only 7% smaller than the corresponding OLS estimate in panel D. This small difference between IV and OLS could indicate limited parental selection into college based on unobservable ability. Another interpretation is that while the IV estimate eliminates the selection causing upward bias in OLS, it provides a LATE effect for complier parents that benefit disproportionately from college enrollment (Card, 1999).

The only controls in column 1 are the gender by birth county fixed effects included in all previous regressions. In columns 2 and 3 we further control for the gender of the observed parent and for the combination of parent and child gender, ensuring that potential 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 child to the household head. Each way of connecting children to parents implies a different relationship of the child to the household head and this set of controls ensures that differences across 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. These controls help address the concern that children with parents in later cohorts are themselves likely to be younger. This could bias downwards the estimate of the intergenerational effect if younger people benefit from positive trends in college enrollment in recent years. Indeed, we find that the IV estimate controlling for age grows to 32 pp, a 26% increase over the baseline estimate. The specification in column 5 is our preferred specification for this part of the analysis. It is worth noting that the increased

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comparability gained by the inclusion of these fixed effects comes at a potential 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. However, Appendix Table H4 shows that the results are robust to controlling for maternal age or other fertility measures.

Appendix Figure H1 shows that the results are hardly affected if we consider more conservative bandwidths for the ages of parents in the sample, while Appendix Table H2 shows that the results are also 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 children with ages in the tighter 25-30 window is 0.29, which remains precisely estimated. Table H3 shows that the effect of parental college enrollment is stronger for individuals that are household heads or spouses than for those classified as children of the head. This is consistent with status within the household being endogenously co-determined with college enrollment (i.e. children of parents in the affected cohorts are both less likely to go to college and more likely to have their parents as dependents). Table H5 provides disaggregate estimates of the intergenerational effect of parental college enrollment by gender (of parent or child). We find little evidence of heterogeneity.

Figure 8 provides a non-parametric visualization of the main result. 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 9. 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 enrollment by the child. We observe a clear decline for children with parents that reached college age after the coup. A child with a parent reaching age 21 in 1980 is 5 pp less likely to go to college (9% of the sample mean) than a child with a parent born in 1972, seven years before, but the latter is just as likely to attend college as a third individual (all 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, column 6 of Table 9 includes an additional control indicating whether the child completed secondary education. As expected, this additional control absorbs some of the variation in college enrollment, but its inclusion only leads to a 12% reduction in our IV estimate (28 pp). Hence, most of the effect of parental college enrollment materializes after children complete secondary. In this regard, panel (c) in Figure 8 plots IV estimates from our preferred specification using completion of each grade in primary and secondary and college enrollment as the dependent variable. We observe no effect through primary, which is mandatory in Chile and beyond the control of parents. However, children with a parent with that went to college are more likely to move beyond all levels in secondary and have a 7 pp higher graduation

rate. Still, this effect is dwarfed by that on college enrollment.

6.3 Assortative Matching as Mechanism

The results in the previous sections show that college enrollment has positive effects on several economic and non-economic outcomes likely to affect the intergenerational transmission of human capital. 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 this section, we study assortative matching by people with college as a potential mechanism.

For this purpose, we focus on children whose linked parent is a household head, as we can identify the spouse of the parent for these individuals and his/her educational attainment. The dependent variable in column 1 of Table 10 is a dummy indicating presence of a spouse/partner of the parent. We find a sizable negative kink for the affected cohorts. The IV estimate indicates that college enrollment increases the probability of a spouse in the household by 17 pp. Column 2 then asks, for the sub-sample with observed spouses, whether the spouse has any college education. We find another negative kink for post-coup cohorts, indicating positive assortative matching. College enrollment increases the probability that the spouse has any college by 40 pp, a very large effect relative to the sample mean of 21%. Column 3 re-estimates the intergenerational effect of parental college for the sub-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 baseline estimates in Table 9. 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 slightly to 0.29. Finally, column 5 uses the same sample and additionally controls for whether the spouse of the parent has any college. The magnitude of the effect of parental college drops to 0.23. This is a 21% drop, indicating that assortative matching of people with college plays an important role in the propensity of their children to enroll in college themselves.

7   Conclusion

This paper studies the capture and decline of higher education in an authoritarian regime and its socio-economic consequences. We exploit variation across cohorts in the supply of college openings following the military coup that brought Augusto Pinochet to power in Chile. Cohorts that reached college age in the years shortly after the coup experienced a sharp decline in enrollment as a result of the reduction in openings decreed by the regime. This decrease in educational attainment had negative consequences that chased these cohorts for the rest of their lives: lower labor force participation, worse occupations, lower incomes and a higher probability of being a dependent of their parents or children, among others. Our findings show that small variation in birth

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year hindered people's ability to climb up the socioeconomic ladder due to reduced educational opportunities. In our setting, college was the great equalizer. Female labor force participation and occupational choice were disproportionately affected, given that the military coup took place at a time of rapid female progress in the labor market. Unfortunately, we also find evidence of persistent effects on the educational attainment of children with parents in these cohorts.

Hostile policies towards higher education are a prominent feature of non-democracies. Whether it is through the dismissal of nonconformist faculty or students, the manipulation of educational content, or modified admission policies like the one we study, authoritarian regimes undoubtedly hinder the functioning and growth of universities. Our findings show that restricted supply of higher education amid dictatorship can have long-lasting economic consequences. It reduces the human capital of those affected and that of their children, potentially also hampering productivity growth and long-term economic growth. Rising levels of democratic backsliding and episodes of repression against universities in places like Hungary or Hong Kong are a bad omen for the future of higher education in various parts of the world.

Our findings are also relevant for the broader debate on the desirability of policies that relax constraints preventing people from accessing higher education. Even though the return to college has been falling in Latin America and may actually be negative for some people (Aedo and Walker, 2012; Rodríguez et al., 2015), our results provide a cautionary tale on the socio-economic consequences of large reductions to the supply of college. Furthermore, our findings provide evidence of the existence of strong intergenerational spillovers. Such spillovers likely increase the net return to college, but may be ignored in individual decision-making and could justify policy interventions.

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Figure 1: The Military Coup and College Enrollment
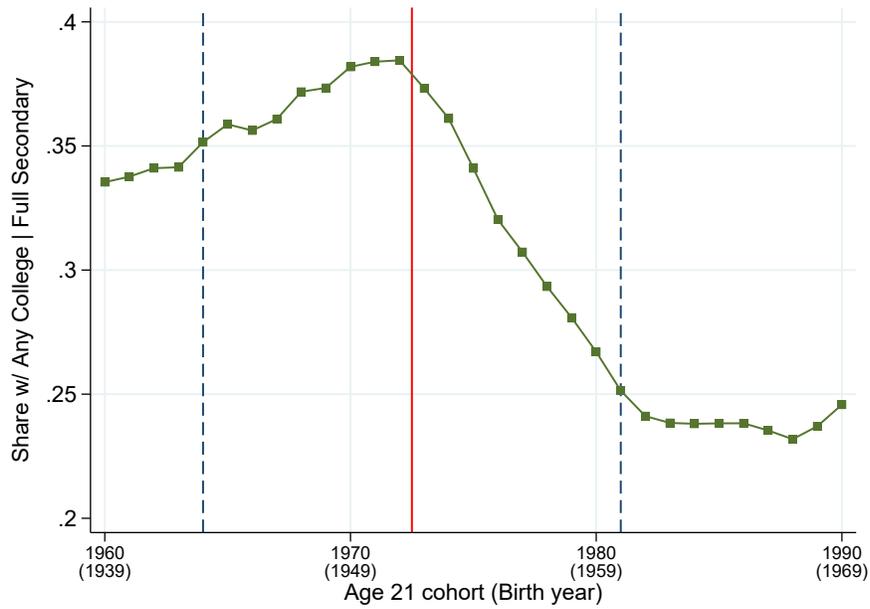
(a) Any College | Full Secondary
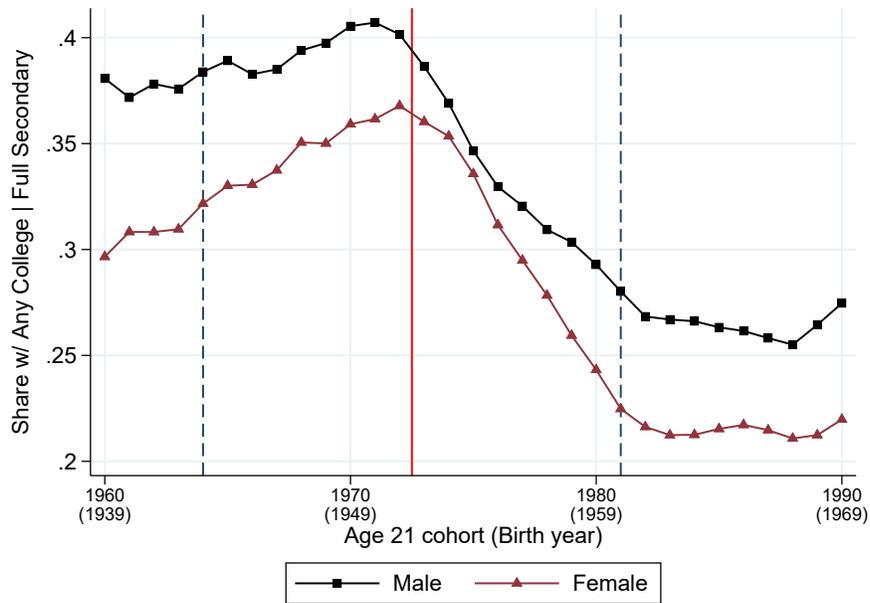
(b) Any College | Full Secondary (by gender)

Notes: Panel (a) shows the share of people per cohort (normalized to age 21) in the 2017 census that report any college, among those with completed secondary education. Panel (b) displays the same information 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 2: College Enrollment: Demand and Supply Factors
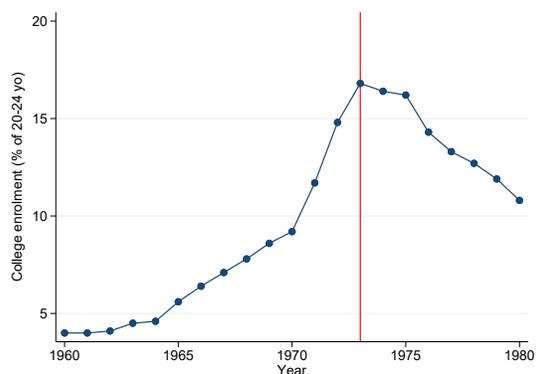
(a) Gross College Enrollment Rate
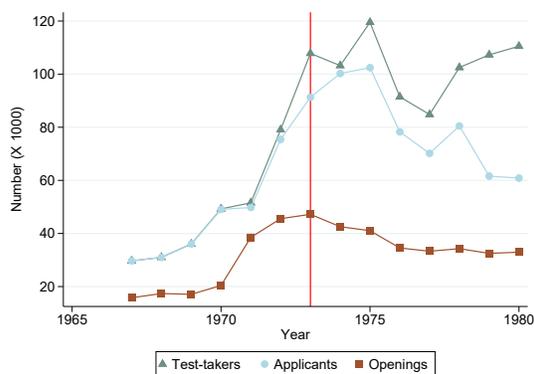
(b) Total Applicants and Openings
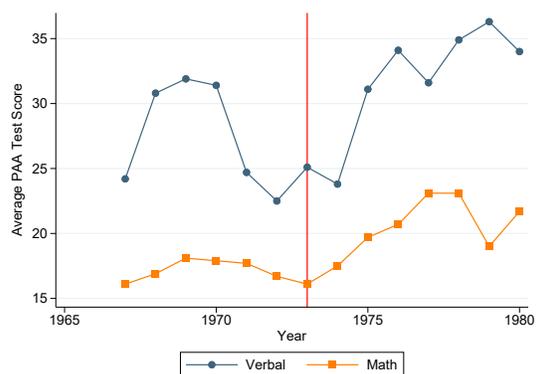
(c) PAA Test Scores
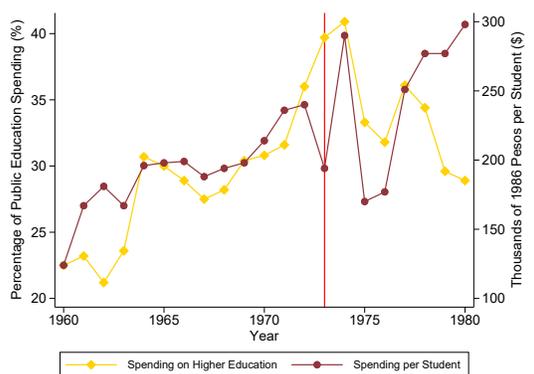
(d) Public Spending

Notes: Panel (a) shows the gross college enrollment rate, defined as the total number of college students divided by the population with ages 20-24. Panel (b) 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 (c) shows the raw (i.e. unadjusted) average in the verbal and math components of the PAA test. Panel (d) shows the percentage of public spending on education devoted to higher education and spending per student in thousands of real 1986 Chilean pesos. The solid line in all graphs shows the year of the military coup. Sources: Universidad de Chile (1972, 2011); PIIE (1984); Brunner (1984); Echeverría (1980); Arriagada (1989); Díaz and Himmel (1985).

Figure 3: Composition of the Student Body
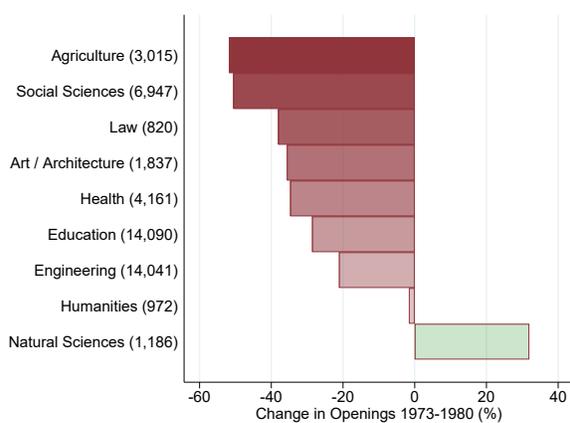
(a) Change in Openings by Field (1973-1980)
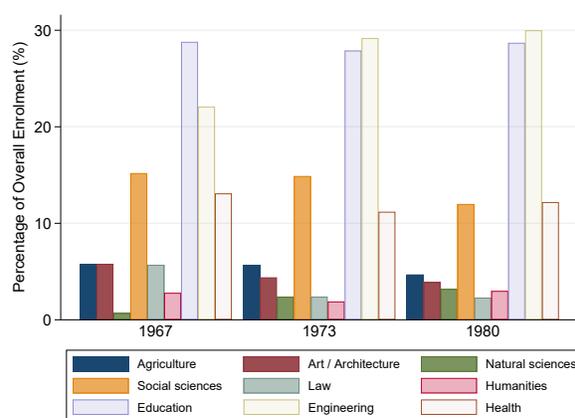
(b) Enrollment by Field (1967, 1973, 1980)
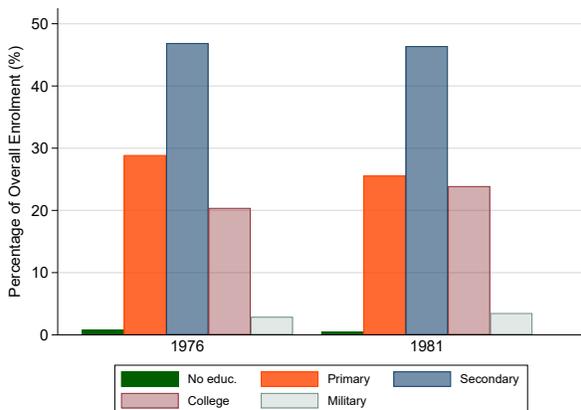
(c) Father's Education (1976, 1981)
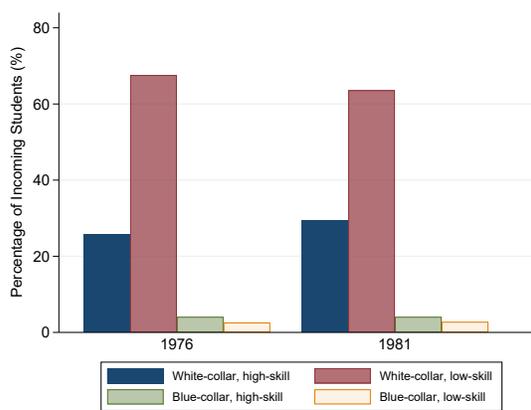
(d) Father's Occupation (1976, 1981)

Notes: Panel (a) 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 (b) shows the share of students enrolled in programs corresponding to different fields of study in 1967, 1973 and 1980. Classification corresponds to UNESCO categories. Panel (c) shows the educational attainment of the father for admitted students in 1976 and 1981. Panel (d) shows the occupation of the father for these same years. Sources: PIIE (1984); Brunner (1984).

Figure 4: Visualization of Kink
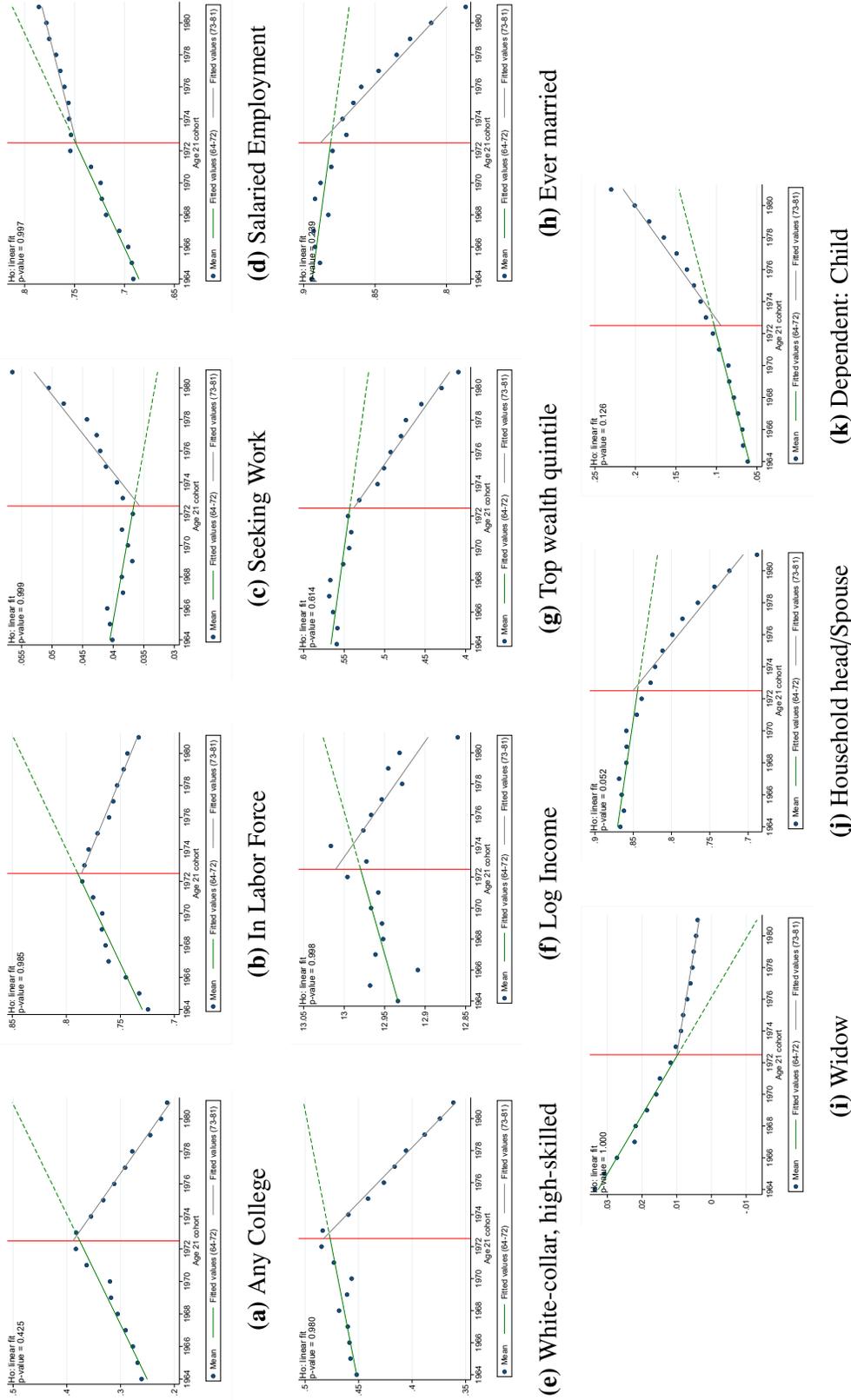
(a) Any College

(b) In Labor Force

(c) Seeking Work

(d) Salaried Employment

(e) White-collar, high-skilled

(f) Log Income

(g) Top wealth quintile

(h) Ever married

(i) Widow

(j) Household head/Spouse

(k) Dependent: Child

Note: Panels show averages by cohort for the variable in the caption. Solid green line corresponds to line of best fit for cohorts reaching college age before 1973. Dashed green line shows extrapolation for later cohorts. Solid grey line corresponds to line of best fit for cohorts reaching college age in 1973 or afterwards. All outcomes from 1992 population census, except income (panel f) from CASEN survey, which corresponds to the averages of self-generated income across survey years. P-value corresponds to a test on the null hypothesis that the raw data fits a linear kink perfectly (see Appendix E for details).

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Figure 5: Synthetic control
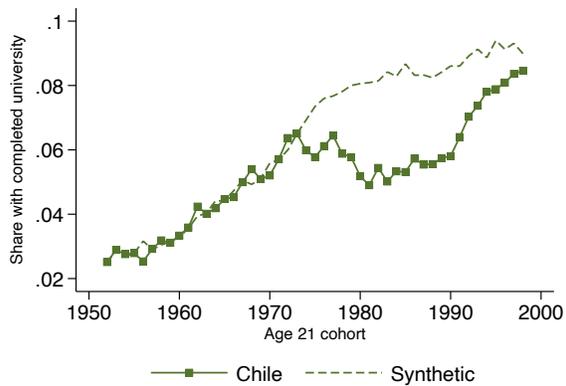
(a) Without controls
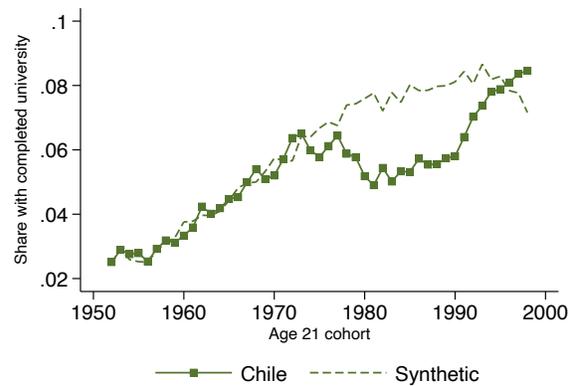
(b) With controls
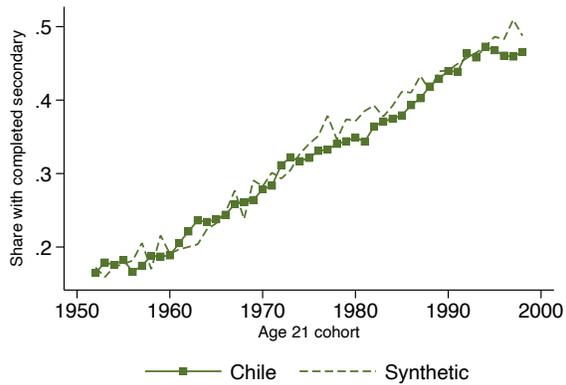
(c) Secondary education
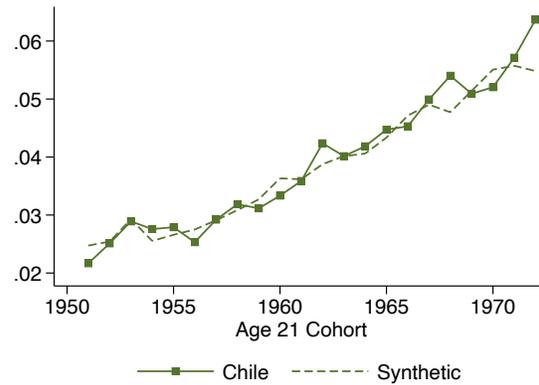
(d) Placebo

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 (OLS)
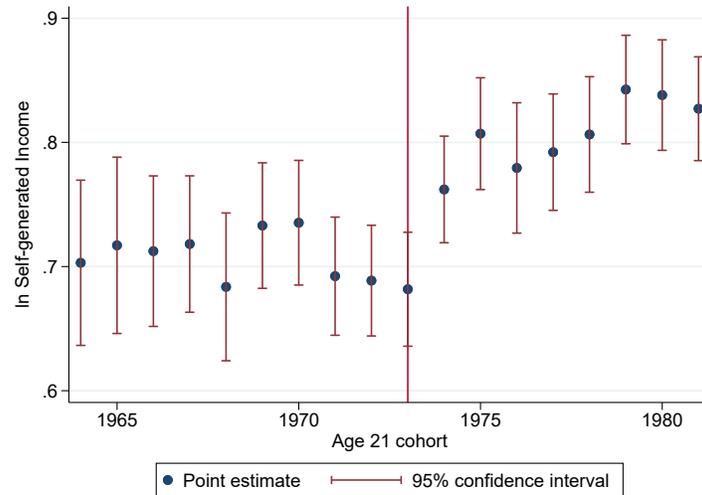
(a) Without occupation fixed effects
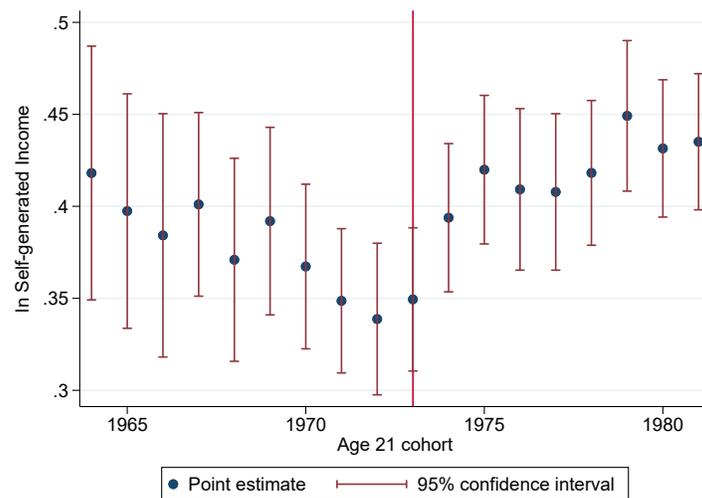
(b) With Occupation fixed effects

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

(e) Income (CASEN: 1990-2015)

(f) Wealth Quintiles (1992)
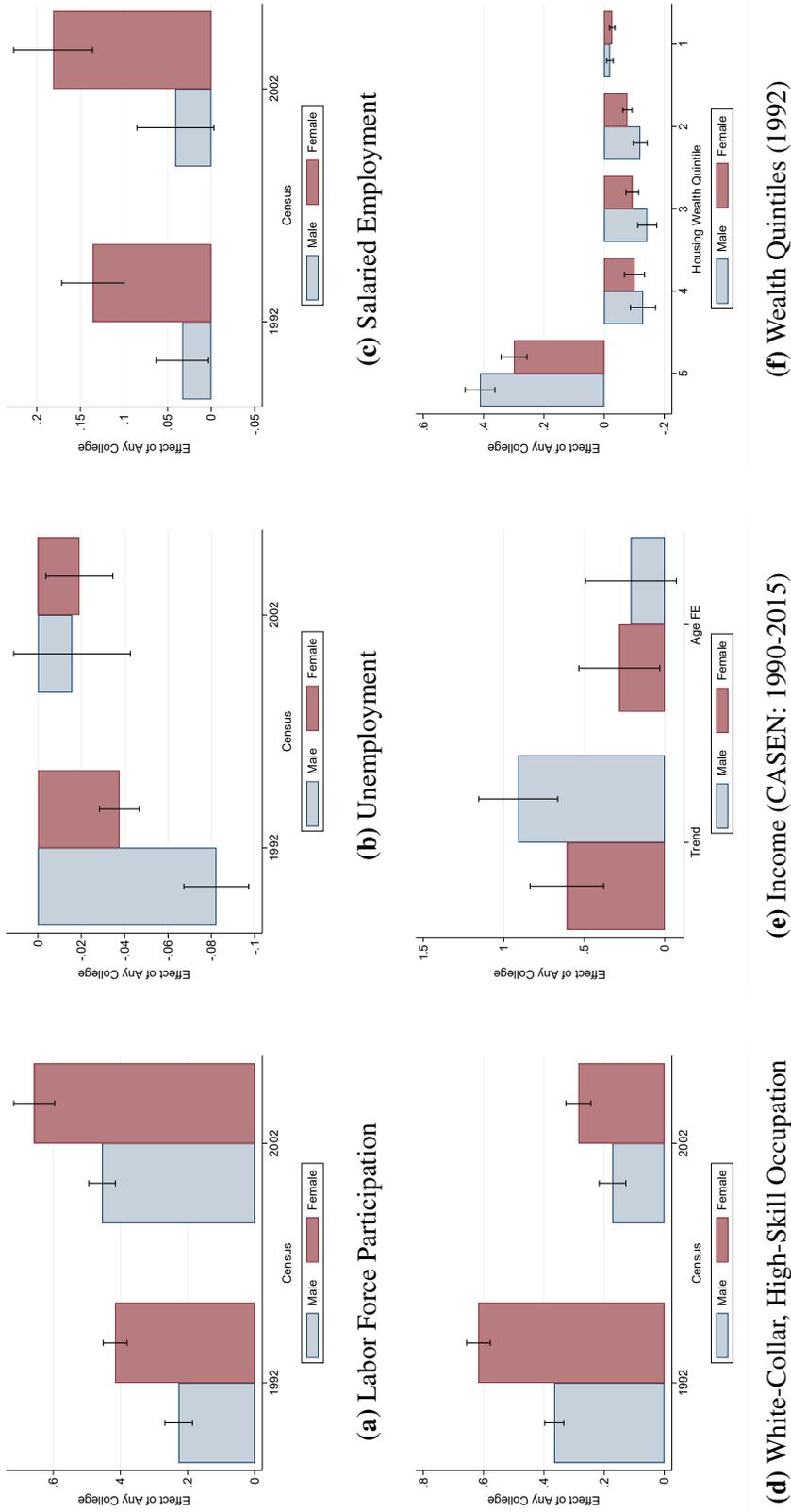
Notes: Each pair of bars (male and female) shows IV gender-specific estimates of the effect of Any College on the variable in the caption. Sample includes all respondents from the respective source (census or CASEN survey) from cohorts reaching age 21 between 1964 and 1981 (both inclusive), but is restricted to respondents reporting four or more years of secondary education (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. Panel (e) also includes year (survey wave) fixed effects and additionally shows results from the specification with age fixed effects. Standard errors clustered by county of birth in parentheses. Full results available in the online appendix.

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Figure 8: Inter-generational Transmission of Education
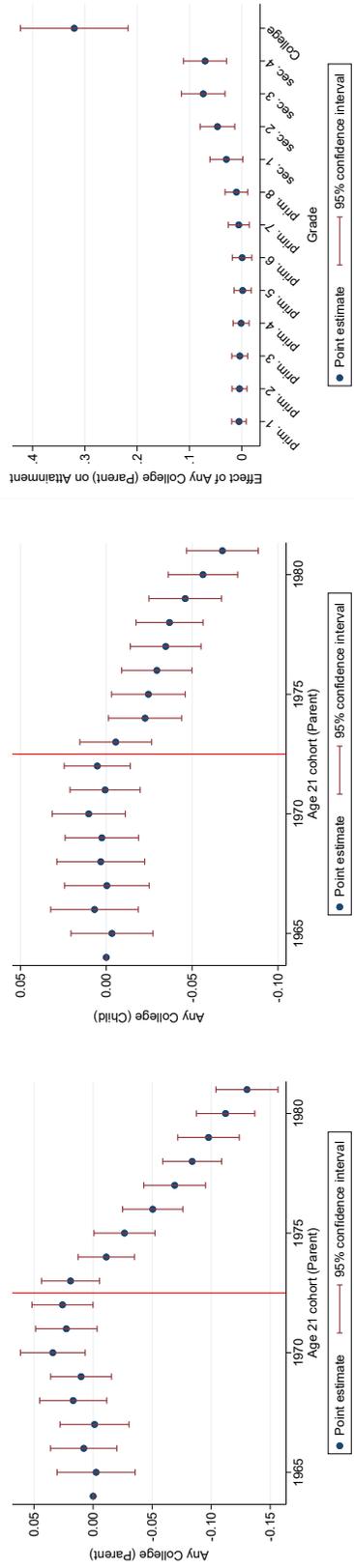
(a) Any College (Parent)

(b) Any College (Child)

(c) Educ. Attainment (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) HH head + parent, (iv) spouse + parent, (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. Panel (c) shows IV estimates of the effect of parental college enrollment on child's educational achievement by grade (primary 1-8, secondary 1-4). 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 baseline trend is an included instrument. The first-stage Kleibergen-Paap F-Statistic is 308. All panels include county of birth fixed effects. Standard errors clustered by county of birth.

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

Dependent variable:	Enrollment			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 ≥ 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 ≥ 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: 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 ≥ 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 ≥ 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 3: Labor market outcomes (Census 1992, 2002)

	In Labor Force		Seeking Work		Wage-Earner		High-skill, white-collar	
Census year:	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 ≥ 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 ≥ 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 4: 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 𝟙(Yr Age 21 ≥ 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 𝟙(Yr Age 21 ≥ 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 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: 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 ≥ 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 ≥ 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 6: Marital status (Census 1992, 2002)

Source:	Census 1992				Census 2002			
	Ever	Current Status			Ever	Current Status		
Dependent variable:	married	Married	Widowed	Separated	married	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)
Birth county 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	0.630	0.013	0.024	0.019	0.444	0.027	0.029
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
Mean of dependent variable	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. *** p<0.01, ** p<0.05, * p<0.1

Table 7: 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)
County x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,023,353	1,023,353	1,023,353	1,192,851	1,192,851	1,192,851	1,036,105	1,036,105	1,036,105
R-squared	0.029	0.031	0.004	0.050	0.015	0.009	0.005	0.011	0.015
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
First stage KP F-stat	2732.5	2732.5	2732.5	1079.8	1079.8	1079.8	630.5	630.5	630.5
Mean of dependent variable	0.800	0.134	0.001	0.858	0.071	0.007	0.878	0.032	0.034

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. *** p<0.01, ** p<0.05, * p<0.1

Table 8: 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	-0.023***	0.002***	0.001***	-0.023***	0.002***	0.001***	-0.017***	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)	-0.043***	-0.0008***	-0.0009***	0.007***	-0.0006***	-0.0003**	0.006***	-0.0007***	-0.0004***
	(0.003)	(0.0001)	(0.0001)	(0.002)	(0.0001)	(0.0001)	(0.002)	(0.0001)	(0.0001)
IV: Any College	1.06***	0.02***	0.02***	-0.26***	0.02***	0.01**	-0.26***	0.03***	0.02***
	(0.069)	(0.003)	(0.003)	(0.063)	(0.005)	(0.005)	(0.087)	(0.006)	(0.006)
Observations	500,535	429,030	429,029	565,974	503,943	503,932	527,917	470,428	470,425
R-squared	0.040	0.006	0.029	0.008	0.005	0.097	0.007	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 dependent variable	2.090	0.983	0.983	2.336	0.978	0.978	2.339	0.971	0.971
First stage KP F-stat	2374.1	2136.7	2167.1	1047.8	976.3	1022.9	602.5	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 9: 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 𝟙(Yr Age 21 Parent ≥ 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 𝟙(Yr Age 21 Parent ≥ 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 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 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

Table 10: 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 𝟙(Yr Age 21 Parent ≥ 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 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 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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   Enrollment and openings

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).[52] Data on the aggregate number of available openings 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 enrollment 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.).

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[52]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 p.2

Appendix B   Additional background figures

Figure B1: Further Evidence on Supply and Demand for College
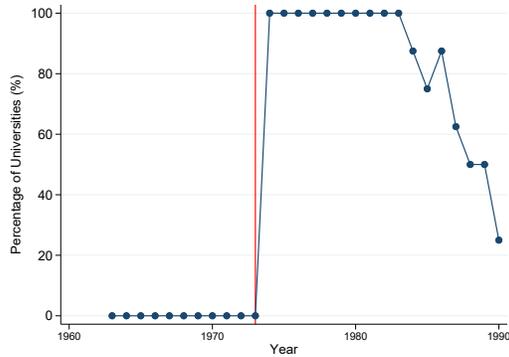
(a) Military Rectors
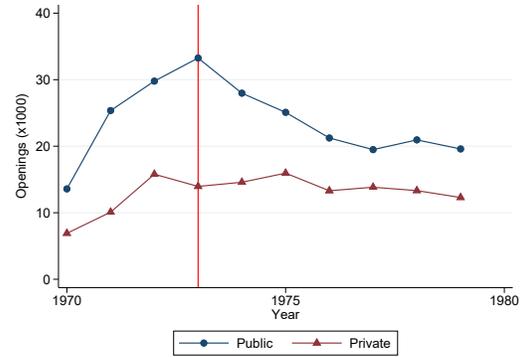
(b) Openings: Public and Private
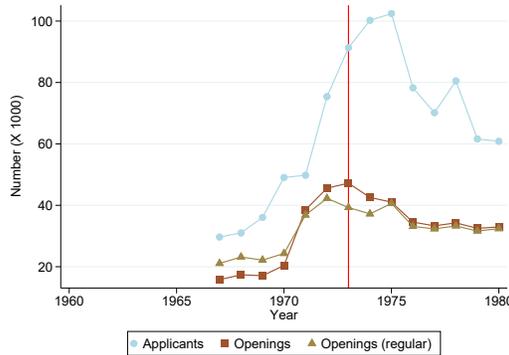
(c) Applications vs Openings: Alternative measure
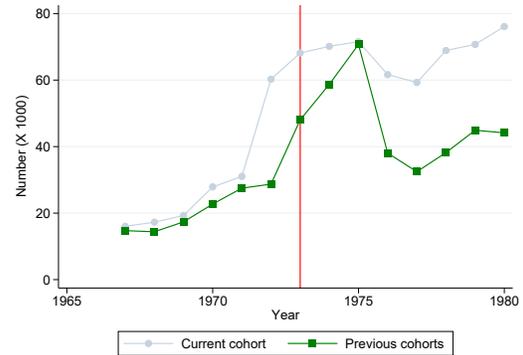
(d) PAA Registration: Old vs Current Cohort

Notes: Panel (a) shows the share of universities with a military rector. Panel (b) shows yearly openings disaggregated between private and public institutions. Panel (c) shows the number of applicants and openings per year, but includes an alternative measure of regular openings. Panel (d) 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.

Figure B2: Alternative mechanisms

(a) Conscription
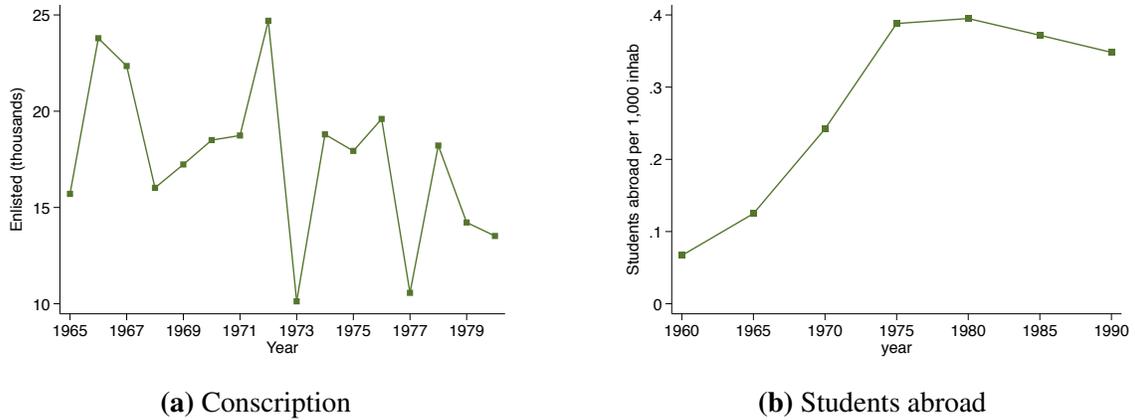
(b) Students abroad

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 through a Freedom-of-Information request and the number of students abroad come from Spilimbergo (2009).

Figure B3: Age distribution of first- and last-year college students

(a) Year 1

(b) Year 5+
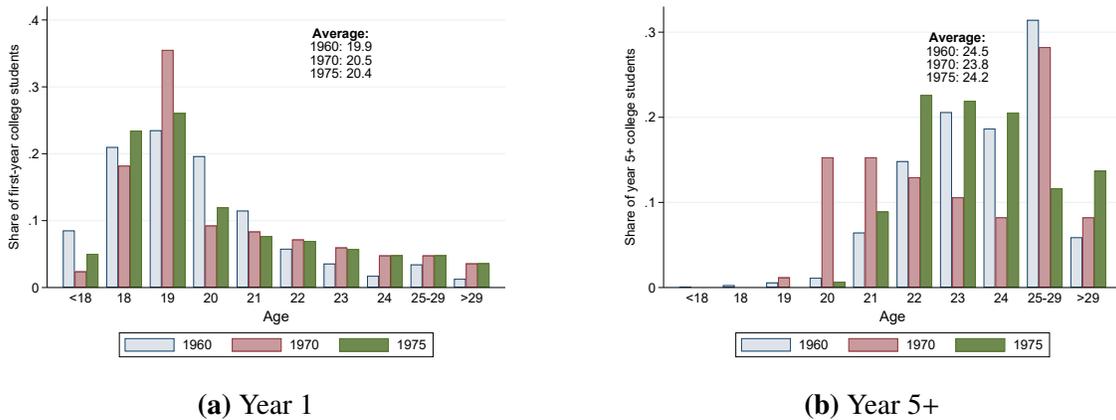
Notes: Information for 1960 comes from the published results from that year's population census (INE, 1965). The respective sources for 1970 and 1975 are Schiefelbein (1976) and Echeverría (1982), based on administrative records and the 1970 population census. Data for 1970 corresponds to entire tertiary 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 B4: Cohort characteristics
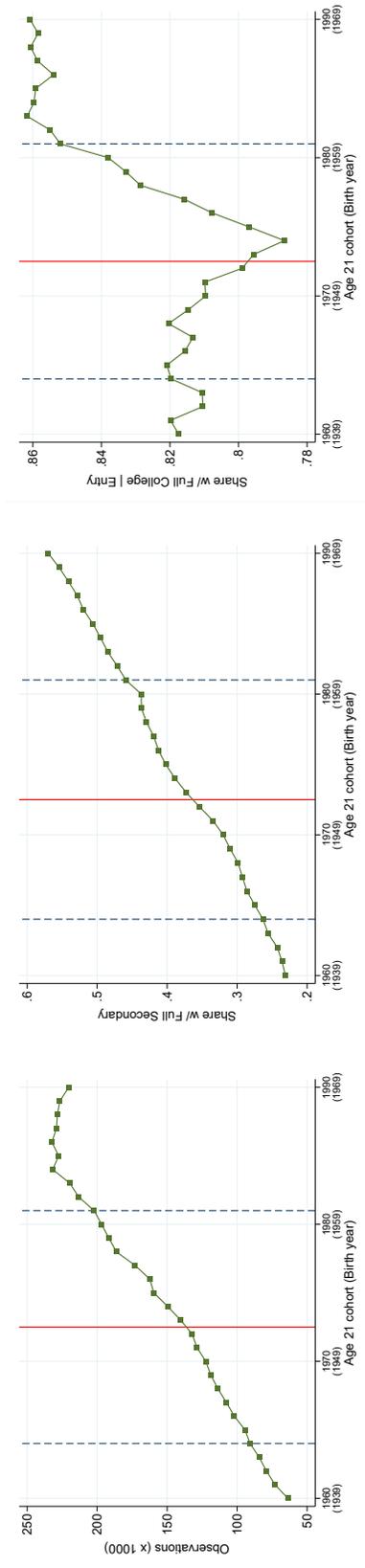
(a) Cohort Size

(b) Full Secondary

(c) Share with full college | entry

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. Panel (c) shows the share of people reporting full college, conditional on entry (information only available for 2017). 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 B5: Cohort size and educational attainment - different sources
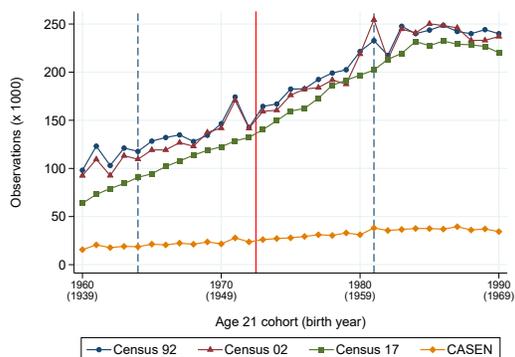
(a) Number of observations
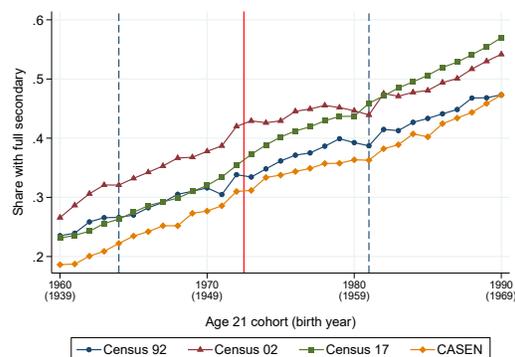
(b) Share with 4+ years secondary
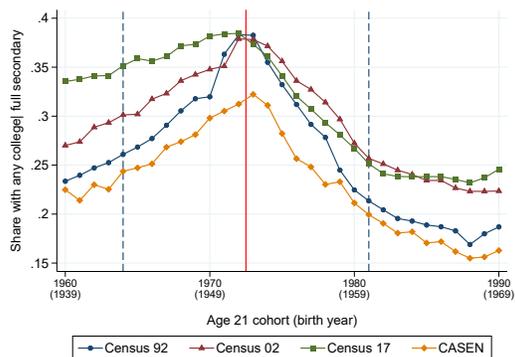
(c) Share with any college | 4+ years secondary
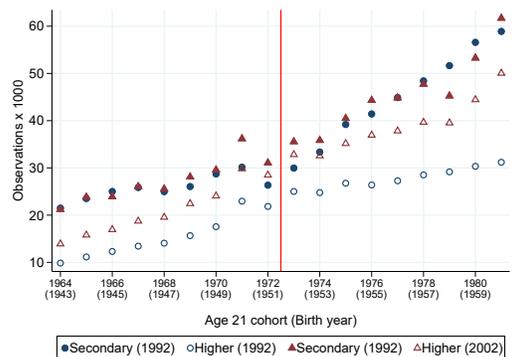
(d) Observations w/ secondary and higher education

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 source the share of people in each cohort that report at least four years of secondary education. Panel (c) shows the share of people with any college, conditional on having 4+ years of secondary education. Panel (d) shows the number of census respondents in 1992 and 2002 that report some secondary or some higher as their highest level of education. Dashed lines show cohorts included in the analysis.

Appendix C   Robustness Checks: Educational Attainment

Table C1: 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 $\mathbb{1}$(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 $\mathbb{1}$(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 C2: College enrollment 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 ≥ x)	-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 ≥ 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 C3: 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 $\mathbb{1}$(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 $\mathbb{1}$(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 C4: College Enrollment: Heterogeneous effects by distance to nearest university (Census 2017)

Distance:	All campuses		Main campus		Main campus (weighted)	
	Log	Discrete	Log	Discrete	Log	Discrete
	(1)	(2)	(3)	(4)	(5)	(6)
[a] Yr Age 21	0.005***	0.005***	0.006***	0.005***	0.005***	0.005***
	(0.0005)	(0.0006)	(0.0005)	(0.0006)	(0.0004)	(0.0006)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 ≥ 1973)	-0.020***	-0.019***	-0.022***	-0.017***	-0.019***	-0.017***
	(0.0008)	(0.0011)	(0.0007)	(0.0010)	(0.0008)	(0.0009)
[c] Yr Age 21 x Distance	-0.0000	0.0001	0.0004*	0.0013*	0.0000	0.0006
	(0.0002)	(0.0008)	(0.0002)	(0.0008)	(0.0002)	(0.0008)
[d] Yr Age 21 x $\mathbb{1}$(Yr Age 21 ≥ 1973) x Distance	-0.0003	-0.0024*	-0.0013***	-0.0058***	-0.0003	-0.0038**
	(0.0004)	(0.0014)	(0.0003)	(0.0012)	(0.0004)	(0.0016)
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	938,783	938,783	938,783	938,783	938,783	938,783
R-squared	0.0415	0.0415	0.0416	0.0416	0.0415	0.0415
p-value a+b+c+d=0	0.000	0.000	0.000	0.000	0.000	0.000
Mean of dep. var	0.324	0.324	0.324	0.324	0.324	0.324

Notes: Dependent variable is enrollment. 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 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. We present heterogeneous effects based on the distance to all the university campuses (columns 1 and 2), to the main university campus (columns 3 and 4), and to the main university campus weighted by the inverse of the total enrollment in 1972 (columns 5 an 6). Odd columns present the results for the demean log of the distance, while even columns present the results for a dummy that takes the value for distances above the median of the empirical distribution. 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 D   Additional Results on Synthetic control

Figure D1: Robustness of synthetic control analysis
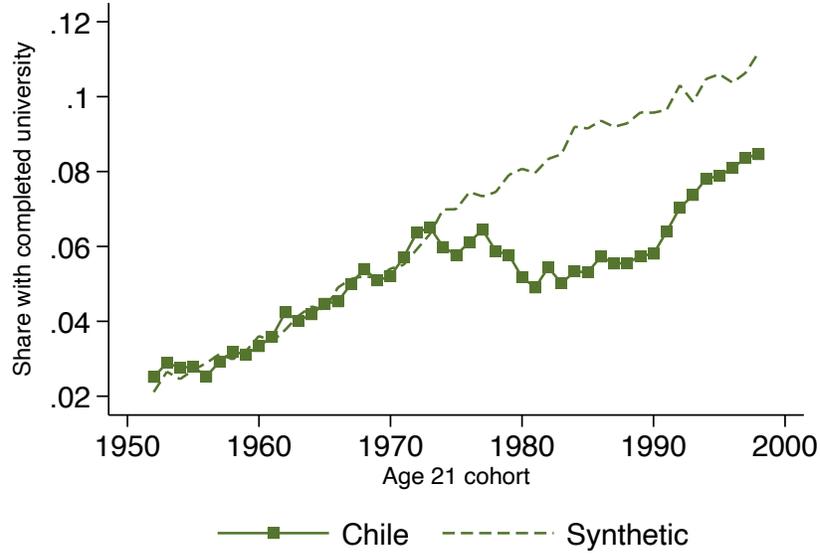
(a) Countries without dictatorship in the synthetic control
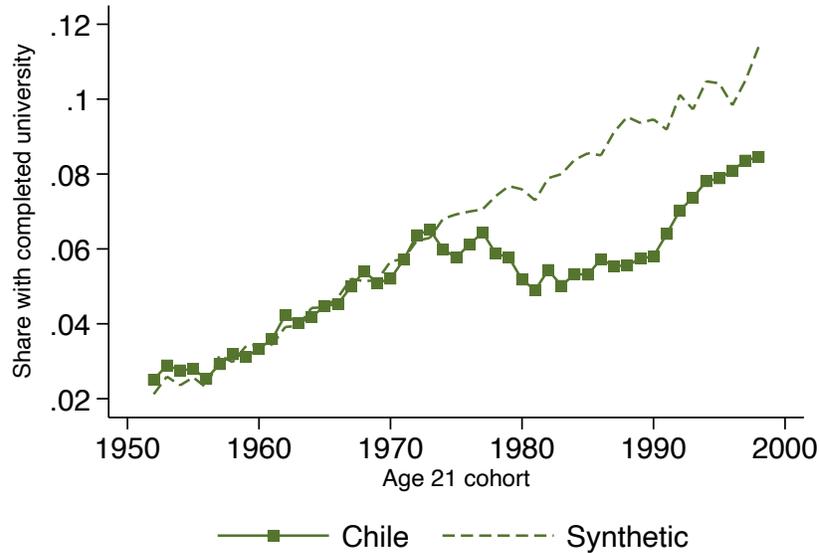
(b) Countries with high HDI in the synthetic control

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 D1: Robustness checks to the synthetic control analysis

			p-value	
Sample:	R^2	Average effect	Unrestricted	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 D2: 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, $\phi = 2$
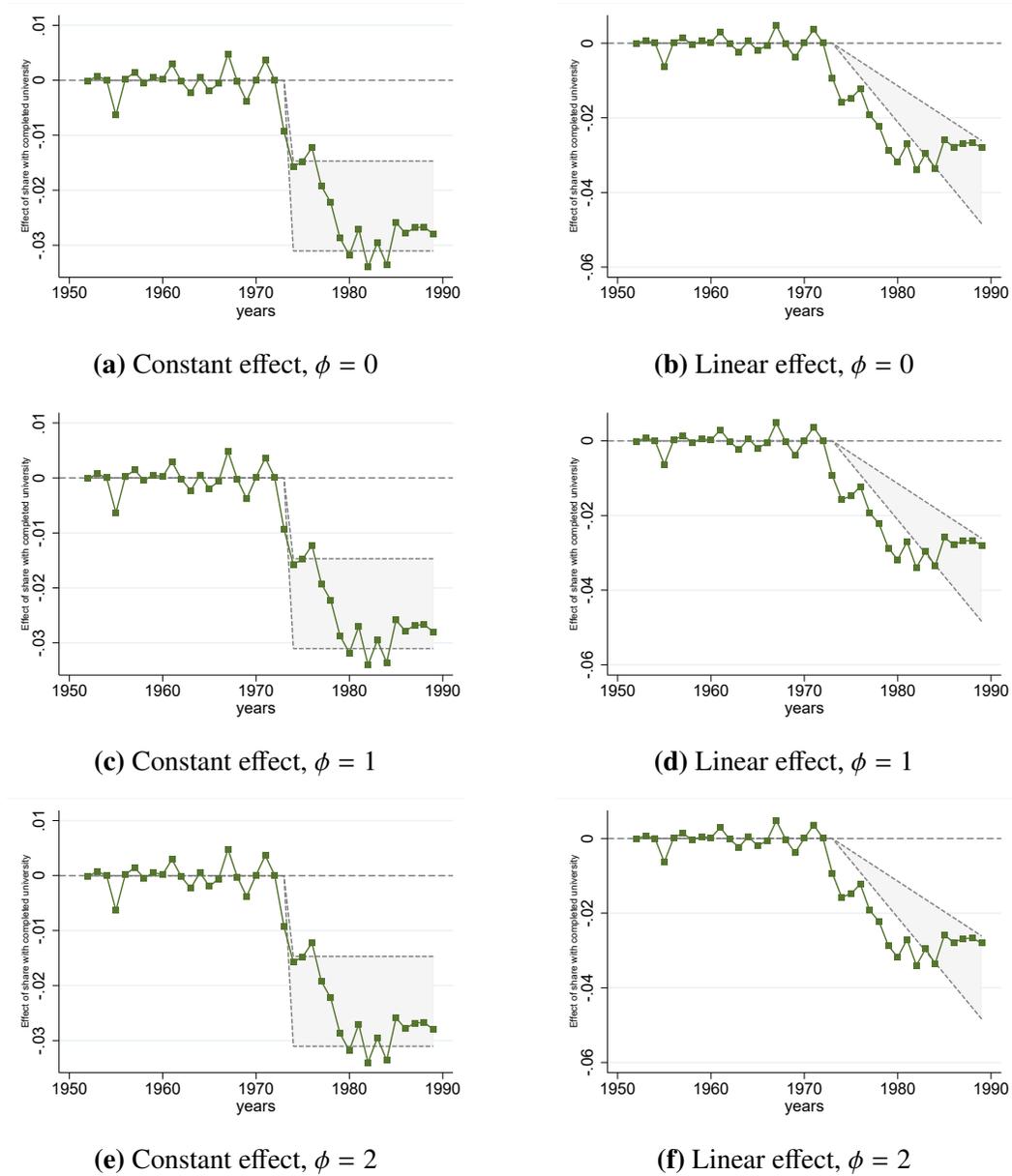
Notes: This figure shows the confidence set proposed by Firpo and Possebom (2018) for a constant and a linear treatment effect. Panels A and B use a sensitivity parameter of 0, while Panels C and D (E and F) use a sensitivity parameter of 1 (2). The sample is all Latin American countries and we use as matching characteristics the even pre-treatment outcomes.

Appendix E Raw data and Visualization of Kink

This section presents the visualization of the kink on different outcome variables. At the top left corner of each figure we report the p-value from a test on the null hypothesis that -apart from the sampling error- the raw data fits a linear kink perfectly. To derive this test, we posit the following data generating process for outcome y_{ict} of individual i of birth cohort c (with $c = 0$ for those who reach age 21 in 1973):

$$y_{ict} = \delta_c + \beta \underbrace{(t - c)}_{\text{Age}} + \epsilon_{ict},$$

where ϵ_{ict} has mean zero and variance σ^2. Then, if the cohort effect is given by: $\delta_c = \pi_0 + \pi_1 c + \pi_2 c \times 1[c \geq 0]$, we would have that *for a given census year t*:

$$y_{ict} = \alpha_0 + \alpha_1 c + \alpha_2 c \times 1[c \geq 0] + \epsilon_{ict},$$

Collapsing to the cohort means, we would get:

$$\bar{y}_{ct} = \alpha_0 + \alpha_1 c + \alpha_2 c \times 1[c \geq 0] + e_{ct}, \tag{5}$$

where e_{ct} is the sample average of ϵ_{ict}. By the law of large numbers, the sample average of ϵ_{ict} would converge to its expected value 0, and the variance of the sample average of ϵ_{ict} would converge to σ^2/N_{ct}, where σ^2 is given by:

$$\sigma^2 = E[(\epsilon_{ict} - E[\epsilon_{ict}])^2] = E[(y_{ict} - \hat{y}_{ict})^2],$$

Thus, we obtain an estimate of the variance of e_{ct} which is given by:

$$\hat{\text{Var}}(e_{ct}) = \frac{1}{N_{ct}} \sum_{i \in c} \frac{1}{N_{ct}} (y_{ict} - \hat{y}_{ict})^2$$

Finally, we can create a quadratic score test statistics as an inference function with asymptotic chi-squared properties (i.e., Lindsay et al., 2003) of the form :

$$\text{Test} = \frac{1}{N_{ct}} \sum_{c} \frac{(\bar{y}_{ct} - r(\bar{y}_{ct}))^2}{\hat{\text{Var}}(e_{ct})},$$

where $r(\bar{y}_{ct}) = \hat{\alpha}_0 + \hat{\alpha}_1 c + \hat{\alpha}_2 c \times 1[c \geq 0]$. Intuitively, this test checks for whether a linear kink model can perfectly explain the evolution of cohort means. In practice, we look at table values from a chi-squared with 14 degrees of freedom, i.e., 18 cohorts, 3 estimated parameters, and the estimated variance.

Figure E1: Visualization of Kink: Higher Education
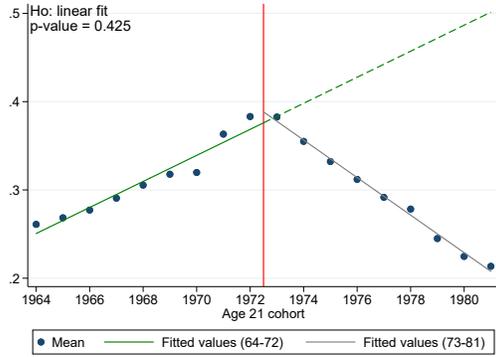
(a) Any College - 1992
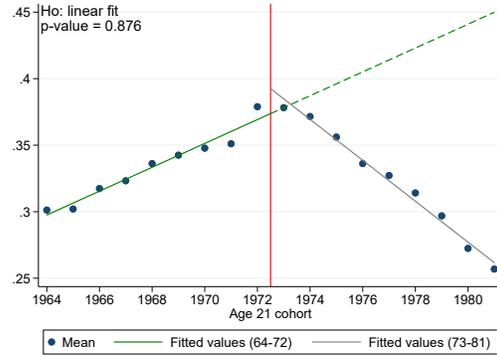
(b) Any College - 2002
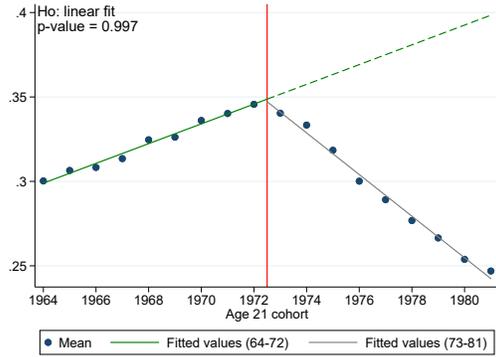
(c) Any College - 2017
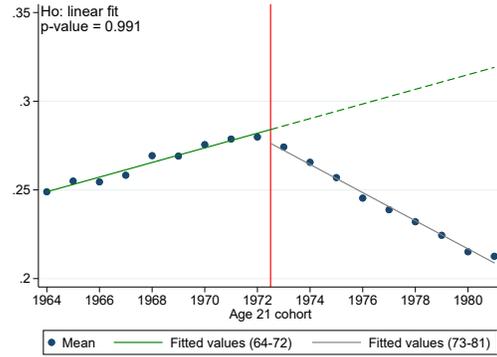
(d) Full College - 2017
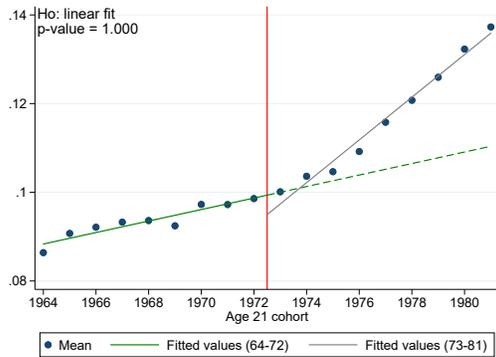
(e) Any Technical - 2017
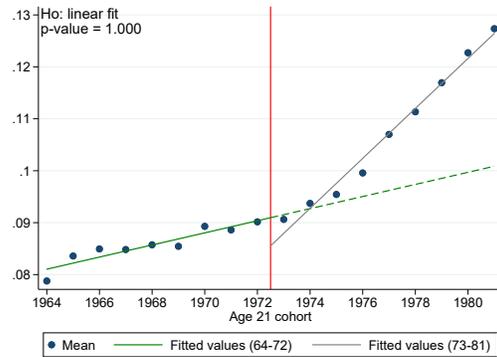
(f) Full Technical - 2017

Note: Panels show averages by cohort for the variable in the caption. Solid green line corresponds to line of best fit for cohorts reaching college age before 1973, which we extrapolate for later cohorts (dashed line). Grey line corresponds to line of best fit for cohorts reaching college age in 1973 or afterwards.

Figure E2: Visualization of Kink: Economic outcomes
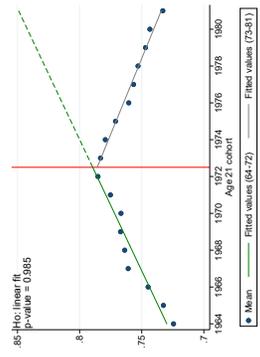
(a) In labor force - 1992
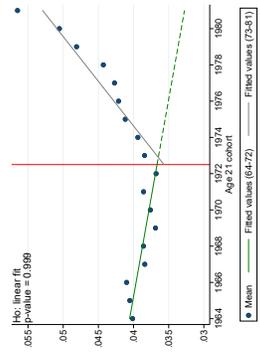
(b) Seeking work - 1992
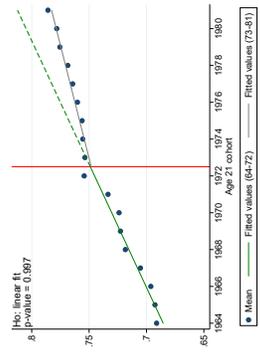
(c) Salaried employment - 1992
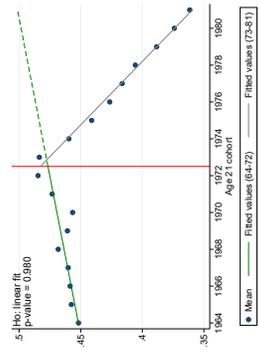
(d) White-collar, high-skill - 1992
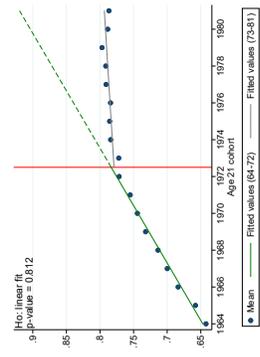
(e) In labor force - 2002
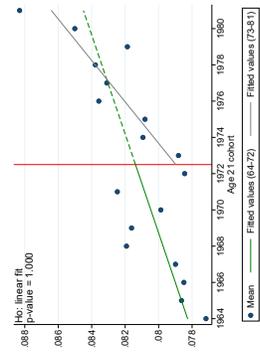
(f) Seeking work - 2002
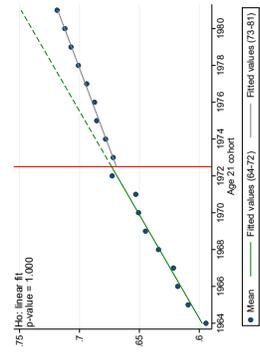
(g) Salaried employment - 2002
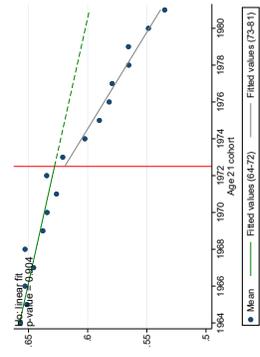
(h) White-collar, high-skill - 2002

Note: Panels show averages by cohort for the variable in the caption. Solid green line corresponds to line of best fit for cohorts reaching college age before 1973, which we extrapolate for later cohorts (dashed line). Grey line corresponds to line of best fit for cohorts reaching college age in 1973 or afterwards.

Figure E3: Visualization of Kink: Log Income
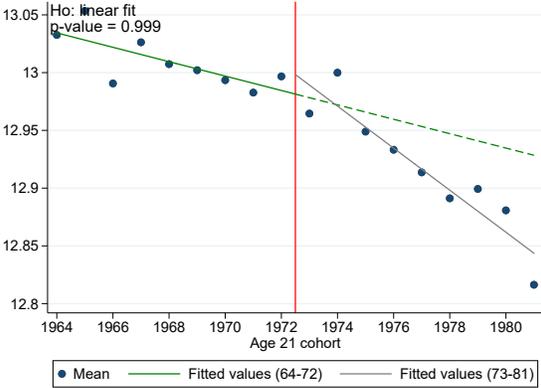
(a) Main Occupation
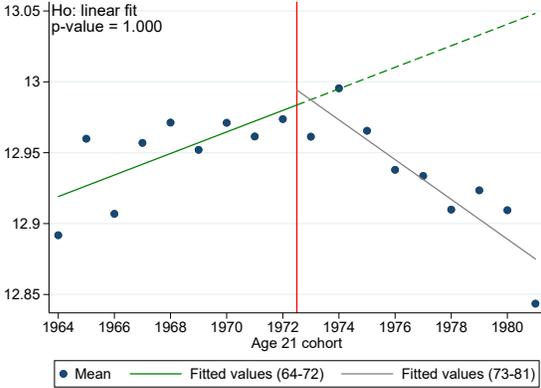
(b) All Work
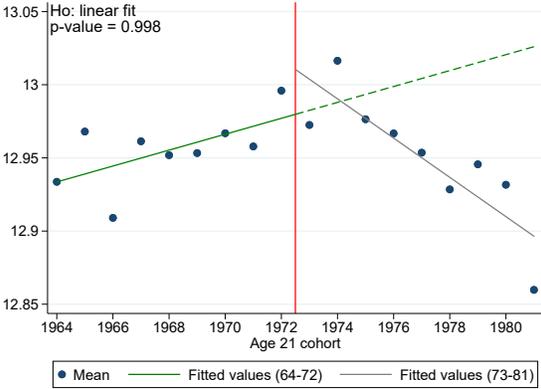
(c) Self-generated
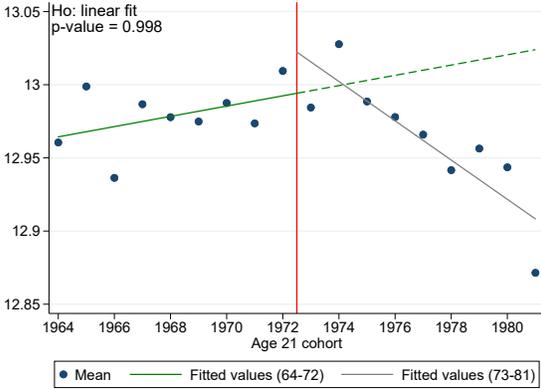
(d) Total

Note: Panels show residualized averages by cohort for the variable in the caption, after controlling for county by gender fixed effects and survey year fixed effects. Solid green line corresponds to line of best fit for cohorts reaching college age before 1973, which we extrapolate for later cohorts (dashed line). Grey line corresponds to line of best fit for cohorts reaching college age in 1973 or afterwards.

Figure E4: Visualization of Kink: Wealth Quintiles
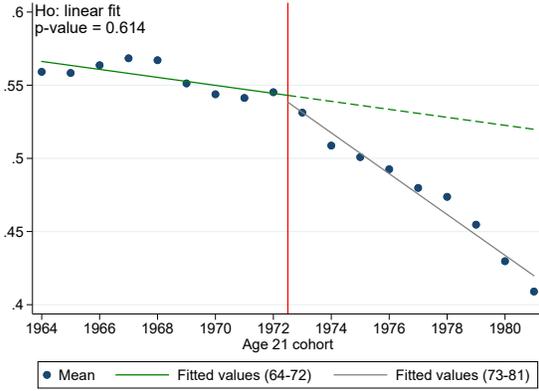
(a) Fifth (top) quintile - 1992
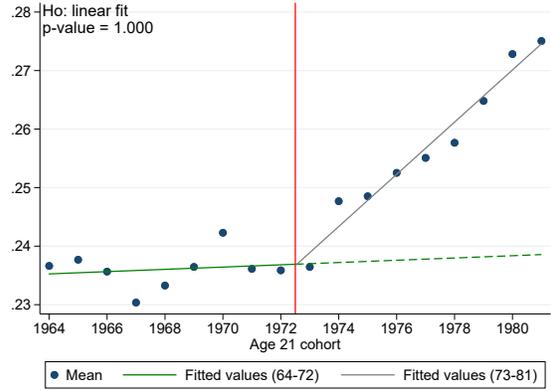
(b) Fourth Quintile - 1992
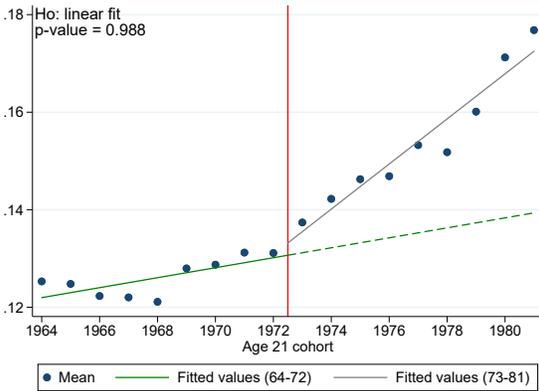
(c) Third Quintile - 1992
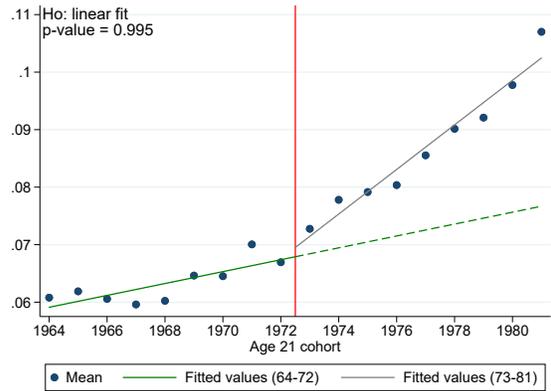
(d) Second Quintile - 1992
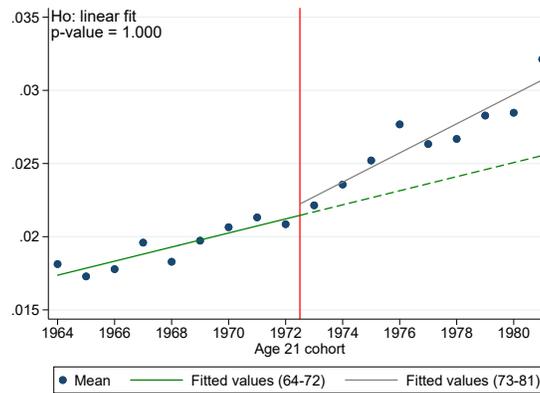
(e) First (bottom) Quintile - 1992

Note: Panels show averages by cohort for the variable in the caption. Solid green line corresponds to line of best fit for cohorts reaching college age before 1973, which we extrapolate for later cohorts (dashed line). Grey line corresponds to line of best fit for cohorts reaching college age in 1973 or afterwards.

Figure E5: Visualization of Kink: marital status
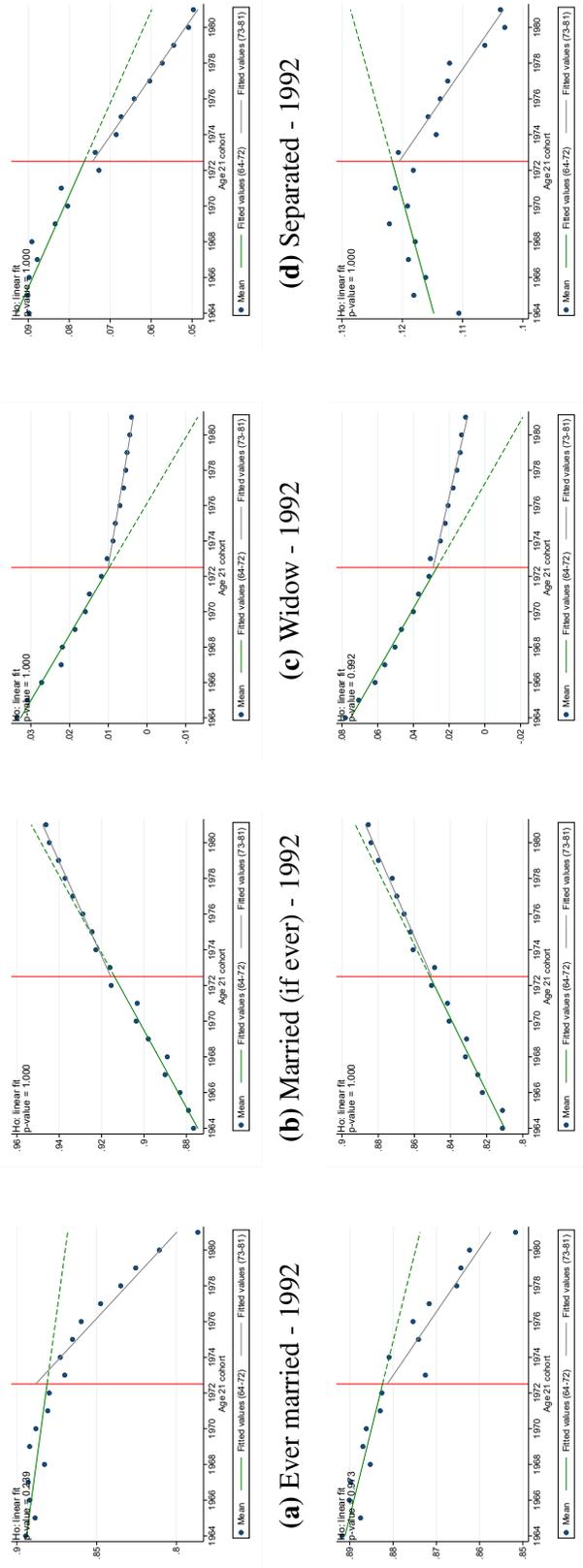
(a) Ever married - 1992

(b) Married (if ever) - 1992

(c) Widow - 1992

(d) Separated - 1992

(e) Ever married - 2002

(f) Married (if ever) - 2002

(g) Widow - 2002

(h) Separated - 2002

Note: Panels show averages by cohort for the variable in the caption. Solid green line corresponds to line of best fit for cohorts reaching college age before 1973, which we extrapolate for later cohorts (dashed line). Grey line corresponds to line of best fit for cohorts reaching college age in 1973 or afterwards.

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Figure E6: Visualization of Kink: Status in Household
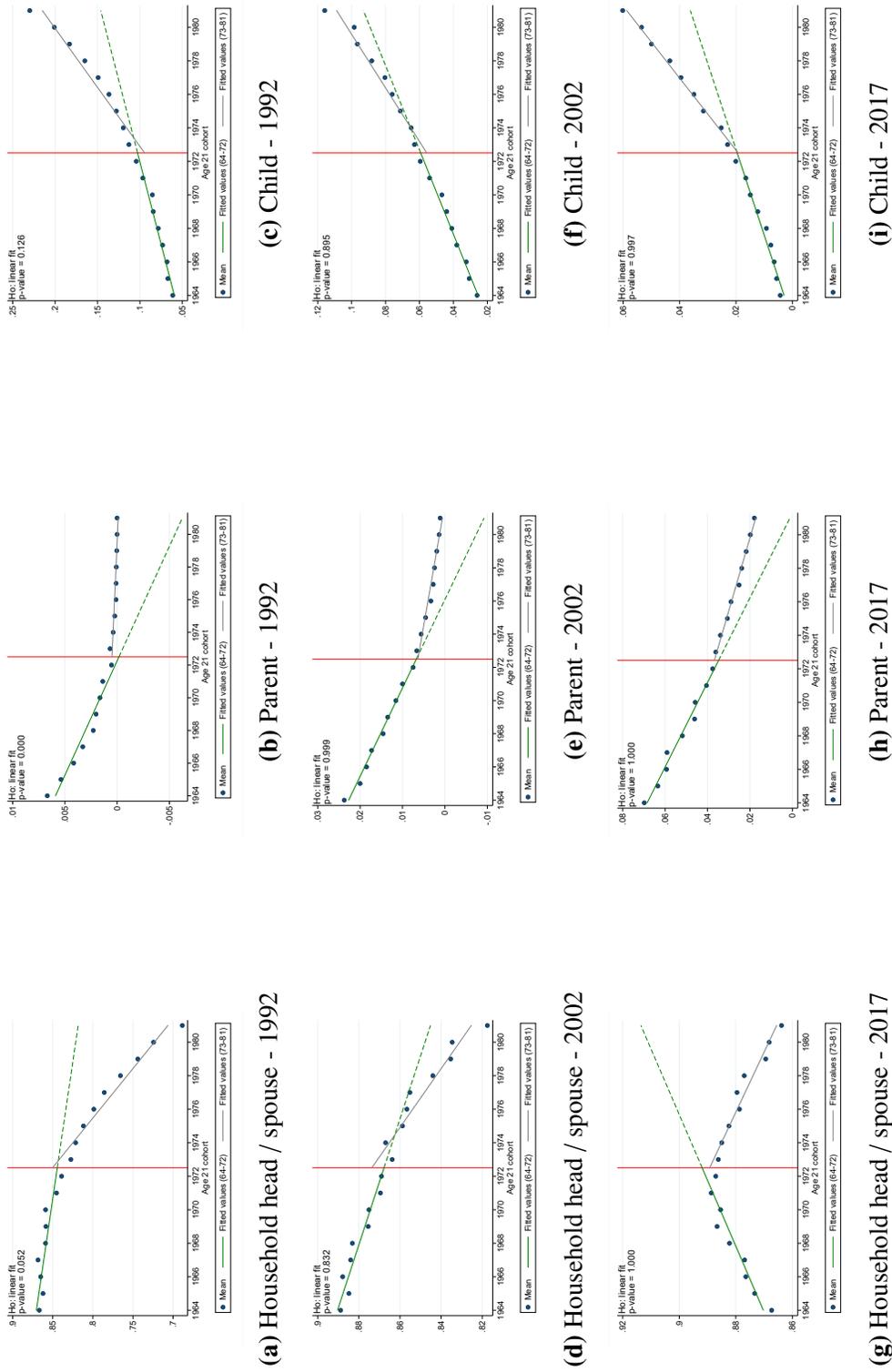
(a) Household head / spouse - 1992

(b) Parent - 1992

(c) Child - 1992

(d) Household head / spouse - 2002

(e) Parent - 2002

(f) Child - 2002

(g) Household head / spouse - 2017

(h) Parent - 2017

(i) Child - 2017

Note: Panels show averages by cohort for the variable in the caption. Solid green line corresponds to line of best fit for cohorts reaching college age before 1973, which we extrapolate for later cohorts (dashed line). Grey line corresponds to line of best fit for cohorts reaching college age in 1973 or afterwards.

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Figure E7: Visualization of Kink: Fertility
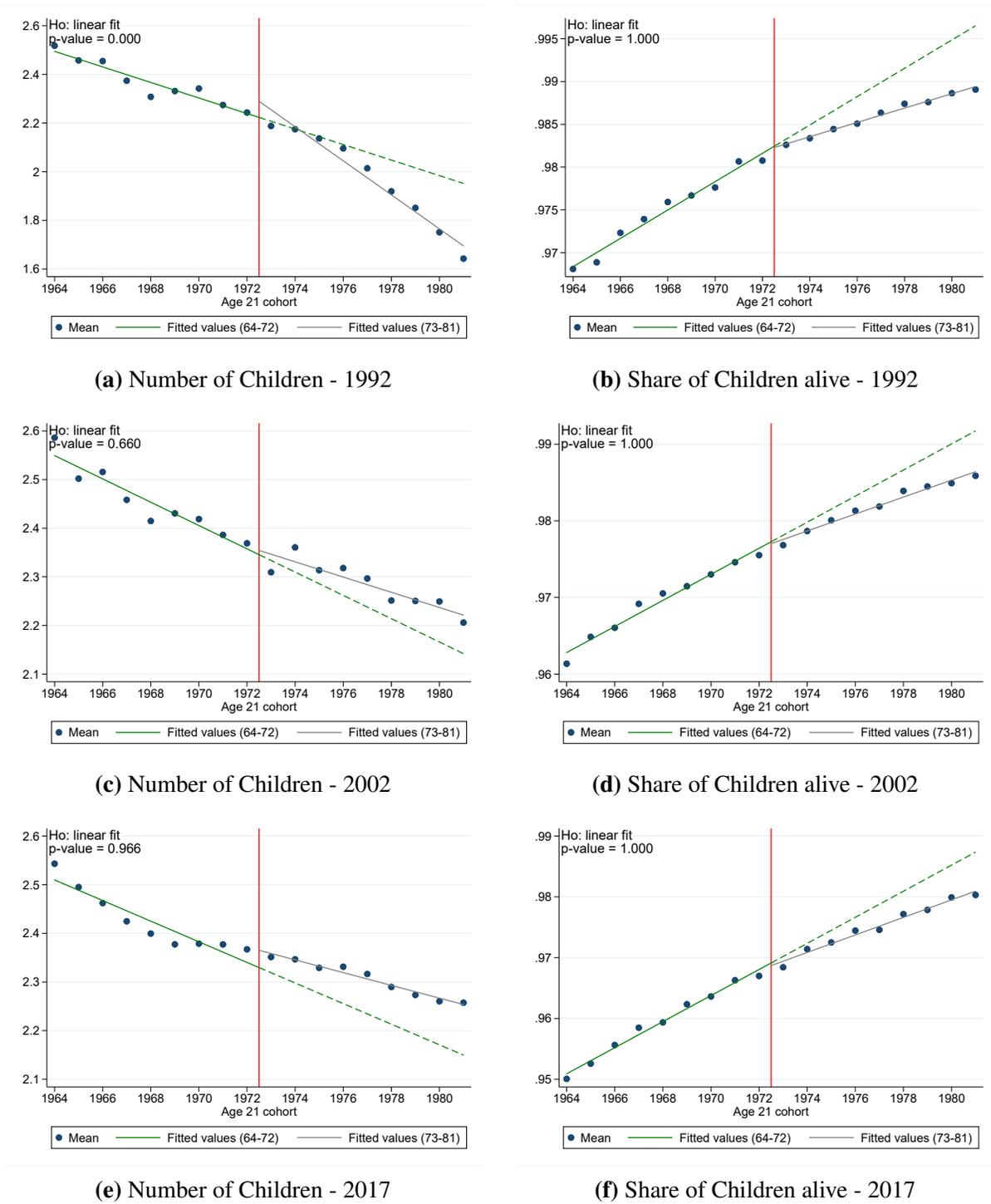
(a) Number of Children - 1992

(b) Share of Children alive - 1992

(c) Number of Children - 2002

(d) Share of Children alive - 2002

(e) Number of Children - 2017

**(f)** Share of Children alive - 2017

Note: Panels show averages by cohort for the variable in the caption. Solid green line corresponds to line of best fit for cohorts reaching college age before 1973, which we extrapolate for later cohorts (dashed line). Grey line corresponds to line of best fit for cohorts reaching college age in 1973 or afterwards.

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Appendix F   Additional results on economic consequences

Figure F1: Cohort-specific Estimates of the College Premium (Other Income Measures)
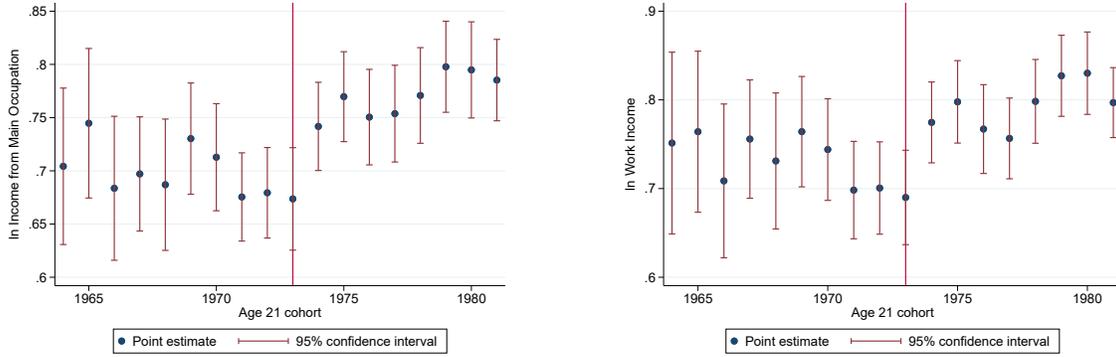
(a) Primary occupation (RF)

(b) Work (RF)
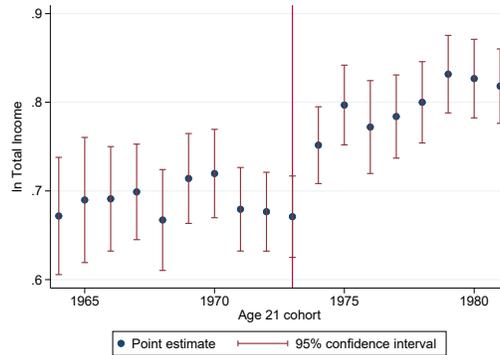
(c) Total (RF)

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 F2: 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)
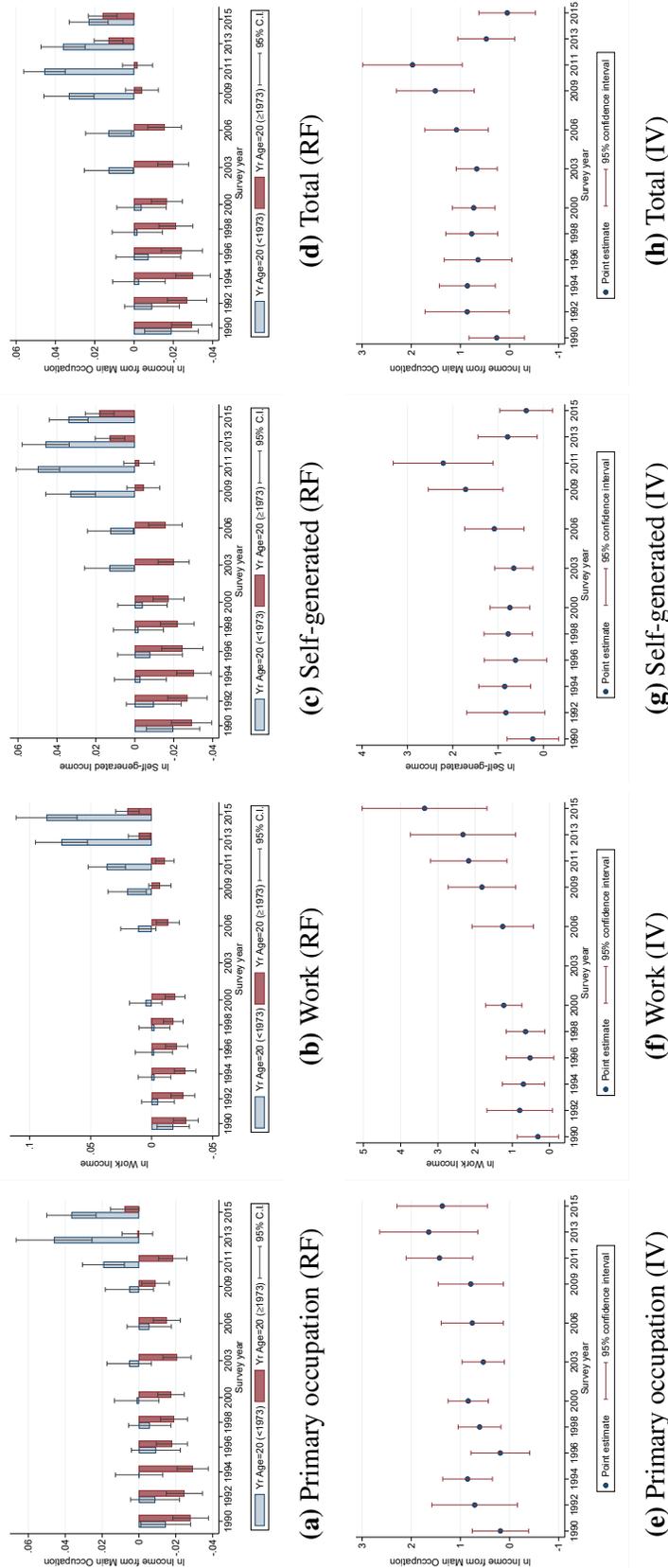
Notes: Panels (a)-(d) show results from regressions of log income (for the type in the caption) on separate age trends for birth cohorts that reached age 21 before and after 1973, both normalized to zero in 1972. The sample for each regression includes all respondents in the CASEN survey of the respective 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.

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Figure F3: Robustness: Labor Market Outcomes w/ Different Bandwidths
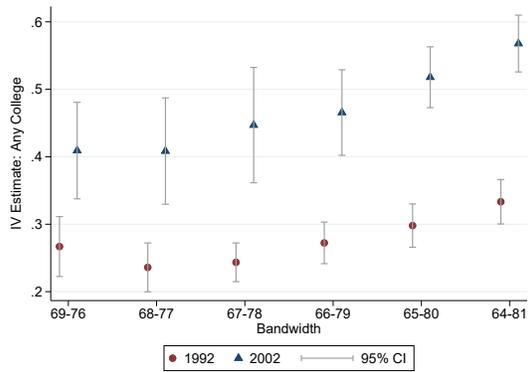
(a) Labor Force Participation
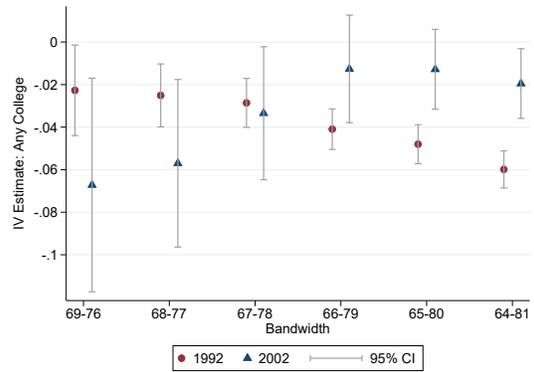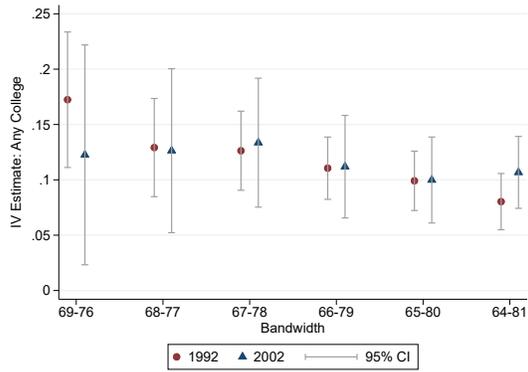
(b) Unemployment

(c) Salaried employment
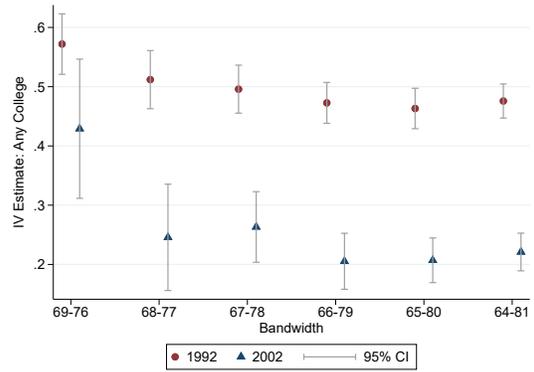
(d) White-Collar High-Skill

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 F4: Robustness: Income and Wealth w/ Different Bandwidths
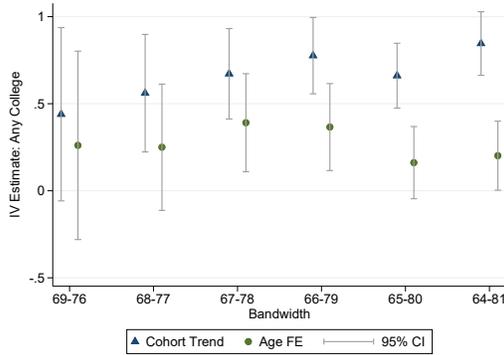
(a) Self-generated Income
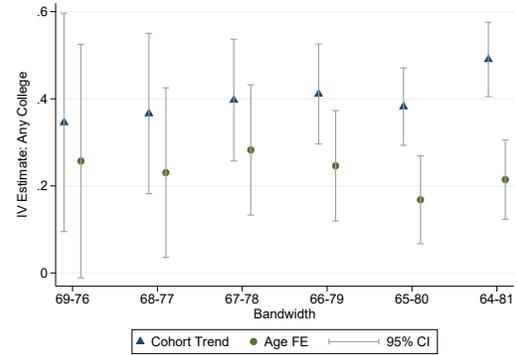
(b) Top income quintile
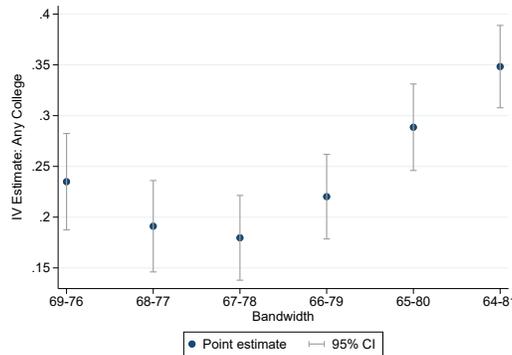
(c) Top wealth quintile

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 F5: Robustness: Non-economic Outcomes w/ Different Bandwidths
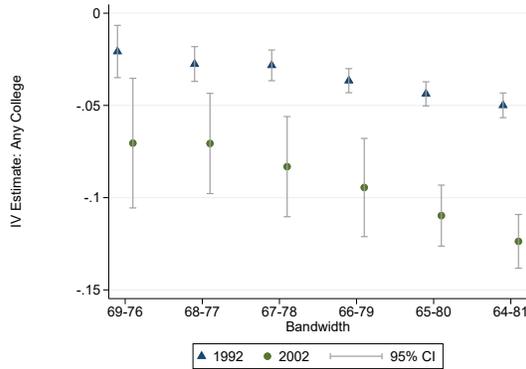
(a) Widow
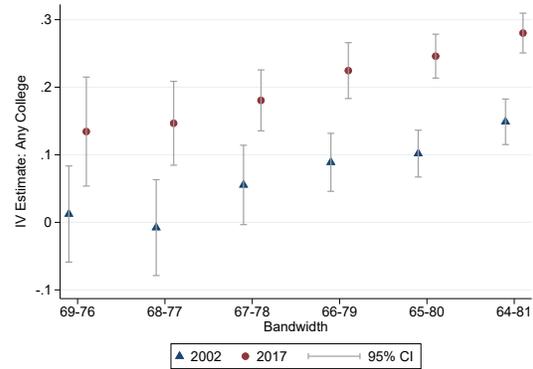
(b) Household Head or Spouse
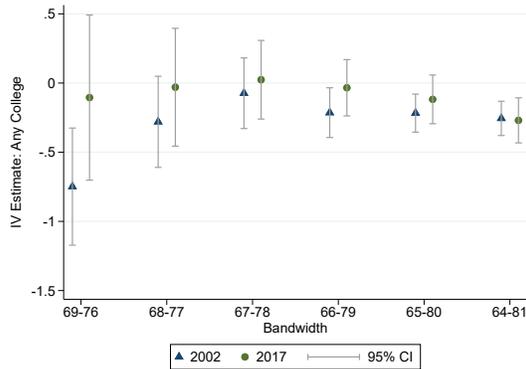
(c) Number of children
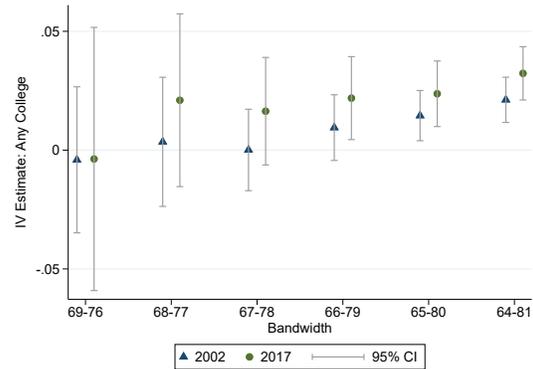
(d) Share of children alive

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 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.

Table F1: 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 $\mathbb{1}$(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 $\mathbb{1}$(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 $\mathbb{1}$(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 F2: 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 ≥ 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 ≥ 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 ≥ 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 F3: Income Quintiles (CASEN)

Income quintile (dummy)	5th Quintile (highest)	4th Quintile	3rd Quintile	2nd Quintile	1st Quintile (lowest)
	(1)	(2)	(3)	(4)	(5)

	Dependent variable:				

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 $\mathbb{1}$(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 $\mathbb{1}$(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 $\mathbb{1}$(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 G Disaggregate results by gender

Table G1: 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 $\mathbb{1}$(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 $\mathbb{1}$(Male) x $\mathbb{1}$(Yr Age 21 $\geq$ 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 $\mathbb{1}$(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 $\mathbb{1}$(Female) x $\mathbb{1}$(Yr Age 21 $\geq$ 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 $\mathbb{1}$(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 G2: 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 $\mathbb{1}$(Male)	0.006***	-0.001***	0.016***	-0.001***
	(0.0004)	(0.0002)	(0.0004)	(0.0002)
[b] Yr Age 21 x $\mathbb{1}$(Male) x $\mathbb{1}$(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 $\mathbb{1}$(Female)	0.009***	0.0001	0.017***	0.001***
	(0.0004)	(0.0001)	(0.0007)	(0.0001)
[d] Yr Age 21 x $\mathbb{1}$(Female) x $\mathbb{1}$(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 $\mathbb{1}$(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 G3: Employment category by gender

	Census 1992					Census 2002				
Source:										
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 G4: Occupation by gender

	Census 1992					Census 2002				
	White-collar		Blue-collar		Military	White-collar		Blue-collar		Military
Dependent variable:	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 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 G5: 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	0.012***	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***	-0.022***	-0.023***	-0.021***
	(0.003)	(0.004)	(0.003)	(0.003)
[c] Yr Age 21 x 𝟙(Female)	0.002	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)	0.576***	0.994***	0.968***	0.910***
	(0.128)	(0.156)	(0.128)	(0.125)
[f] IV: Any College (Female)	0.528***	0.861***	0.725***	0.608***
	(0.112)	(0.162)	(0.121)	(0.117)
[g] IV: Any College (Male) [Age FE]	0.175	0.183	0.176	0.211
	(0.140)	(0.181)	(0.147)	(0.144)
[h] IV: Any College (Female) [Age FE]	0.245**	0.236	0.262**	0.282**
	(0.121)	(0.177)	(0.131)	(0.128)
County x gender FE	Yes	Yes	Yes	Yes
Survey year x gender FE	Yes	Yes	Yes	Yes
Observations	99,712	93,666	118,301	118,301
R-squared	0.165	0.146	0.153	0.154
p-value a+b=0	0.000	0.000	0.000	0.000
p-value c+d=0	0.000	0.002	0.015	0.008
p-value a=c	0.770	0.000	0.000	0.000
p-value a+b=c+d	0.455	0.161	0.003	0.003
First-stage KP F-stat (trend)	109.3	90.5	124.8	124.8
First-stage KP F-stat (age FE)	103.1	78.9	108.5	108.5
p-value e=f	0.779	0.540	0.145	0.063
p-value g=h	0.696	0.827	0.657	0.704
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 or replaced by age fixed effects). All regressions include county of residence x gender and survey year x gender fixed effects. Standard errors clustered by county of residence in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table G6: 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 G7: 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

Table G8: 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)	(0.000051)	(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	0.583	0.344	0.022	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	0.000	0.000	0.000	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 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 H Additional results on IGT of Education

Figure H1: Robustness: Child's College Enrollment w/ Different Bandwidths
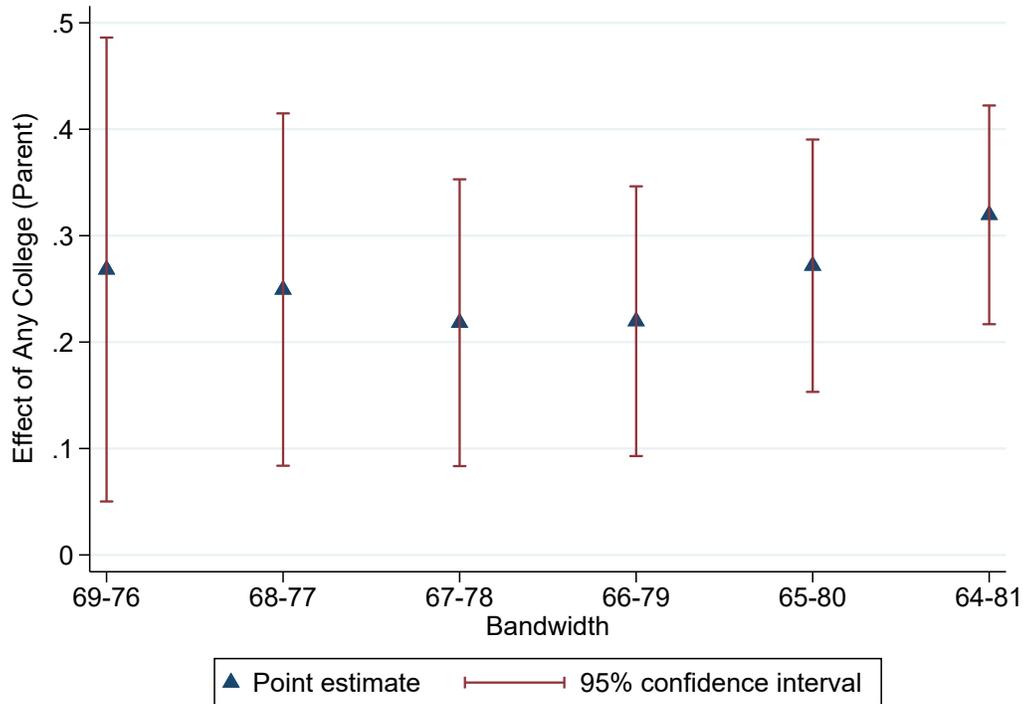
Notes: Each figure replicates the IV analysis of child's college enrollment for the different bandwidths in the x-axis. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (I) reached age 21 in the relevant bandwidth (both years 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 H1: Educational attainment of children: Sample Characteristics (Census 2017)

	Age	Female	Full primary	Full secondary	Any college	HH size	Position in HH 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 ∈ [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.

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Table H2: 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 H3: Educational attainment of children: Heterogeneous effects by relationship to HH Head (Census 2017)

| Position in household: | Dependent variable: Any College (child) | | | | |
| | 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 H4: Fertility and IGT of Education (Census 2017)

Dependent variable:	Mother's characteristics			Any College			
	Total children	Share alive	Age at birth				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
[a] Yr Age 21 Mother	-0.027***	0.001***	-0.653***	-0.002	-0.002	0.010***	
	(0.005)	(0.0003)	(0.011)	(0.001)	(0.001)	(0.001)	
[b] Yr Age 21 Mother	0.014**	-0.0004	-0.013	-0.007***	-0.007***	-0.004**	-0.004**
x $\mathbb{1}$(Yr Age 21 Mother ≥ 1973)	(0.006)	(0.0003)	(0.015)	(0.002)	(0.002)	(0.002)	(0.002)
IV: Any College (Mother)	-0.568**	0.016	0.642	0.297***	0.297***	0.195**	0.192**
	(0.252)	(0.013)	(0.590)	(0.067)	(0.067)	(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	No	No	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	93,690	93,690	93,690	93,688	93,688	93,688	93,688
R-squared	0.030	0.014	0.333	0.079	0.079	0.080	0.081
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	30.49	0.564	0.564	0.564	0.564
First-stage KP F-stat	177.1	168.7	179.2	168.7	169.1	133.2	133.2

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 ≥ 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

Table H5: 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 $\mathbb{1}$(Male)	0.002**	-0.000	0.005***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
[b] Yr Age 21 Parent x $\mathbb{1}$(Male) x $\mathbb{1}$(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 $\mathbb{1}$(Female)	0.006***	-0.000	0.002**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
[d] Yr Age 21 Parent x $\mathbb{1}$(Female) x $\mathbb{1}$(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 $\mathbb{1}$(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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$