Technocratic Autocracy, Higher Education and Social Mobility*

María Angélica Bautista Felipe González Luis R. Martínez
Pablo Muñoz Mounu Prem

August 2020

Abstract: We study the capture of higher education by the Pinochet dictatorship following the 1973 military coup in Chile. Echoing the views of their mentors in the Chicago School, the regime's technocrats sharply reduced government funding for universities. This led to a steady reduction in the number of openings for incoming college students and caused a large decrease in enrollment for cohorts reaching college age in the following years. Contrary to the technocrats' claim that universities were bloated and had low returns, we find that the affected cohorts had worse economic outcomes throughout the life cycle and struggled to climb up the socioeconomic ladder. Decades after democratization, children with a parent in these cohorts also have a lower probability of college enrollment. These findings shed light on the long-lasting costs of Pinochet's technocratic reforms and cast doubt on the view that non-democracy may be desirable at early stages of development.

Keywords: Dictatorship, university, technocracy, Chile, Chicago Boys
JEL codes: H52, I23, I24, I25

---

*We thank Daron Acemoglu, Dan Black, José Joaquín Brunner, David Card, Kerwin Charles, José Díaz, Steven Durlauf, Claudio Ferraz, Fred Finan, Mónica Martínez-Bravo, James Robinson, Martín Rossi, Noam Yuchtman and seminar participants at the University of Chicago, the University of California - Berkeley, Universidad del Rosario, Universidad de Chile, Universidad de San Andrés, Universidad Javeriana, Universidad de los Andes, the Latin American Network in Economic History and Political Economy virtual seminar the EH Clio Lab and the 35th Annual Congress of the European Economic Association for comments and suggestions. We also thank Fondecyt (project 11170258) and the Pearson Institute for the Study and Resolution of Global Conflicts for financial support. Azize Engin, Katia Everke, Juan Manuel Monroy, Daniela Guerrero, Maria Paula Tamayo and Piera Sedini provided outstanding research assistance. Bautista and Martínez: University of Chicago, Harris School of Public Policy; González: Pontificia Universidad Católica de Chile, Instituto de Economía; Muñoz: FGV EPGE, Brazilian School of Economics and Finance; Prem: Universidad del Rosario, Department of Economics. First version: May 2020.

1 Introduction

The economic consequences of different political regimes have long interested economists. Previous work has identified redistributive pressures and conflicting interests as a potential hindrance to growth-promoting policies under democracy.[1] This has led some to wonder whether "it is the choices made by the dictators, rather than the constraints on them, that have allowed some poor countries to emerge from poverty" (Glaeser et al., 2004, p.286). Recent economic success stories like those of Singapore, South Korea or China are usually invoked in support of this hypothesis. For instance, Birdsall and Fukuyama (2011, p.51) describe China's authoritarian government as "a tightly managed, top-down policymaking machine that could avoid the delays of a messy democratic process", while Posner (2010) bluntly concludes that "Dictatorship will often be optimal for very poor countries."

In the case of Chile, the technocratic reforms implemented during the dictatorship of Augusto Pinochet (1973-1990) are often credited for the country's economic success over the past 35 years. These reforms were spearheaded by a group of economists known as the 'Chicago Boys', as most had been trained at the University of Chicago (Valdés, 1995). According to Becker (1997), the Chicago Boys "spurred an economic and political revolution... Their teachers are proud of their richly deserved glory."

In this paper, we shed light on the socioeconomic costs of the Pinochet dictatorship's technocratic agenda. We focus on the capture of higher education in the early years of the regime. During this period, the government assumed control of all universities in the country and steadily reduced spending on higher education. This policy responded to the Chicago Boys' belief that high subsidies to universities distorted investment in human capital and led to a bloated university system with weak incentives for thrift and low returns for students. Moreover, these claims echoed the views of their mentors in the Chicago School, most notably Milton Friedman, who argued that high subsidies made college "a pleasant interlude between high school and going to work" (Friedman and Friedman, 1980, p. 175).

We show that the reduction in public subsidies led to a decrease in the number of openings for incoming college students and to a large decline in the enrollment rate, as an ever larger number of graduates from secondary education competed over an ever smaller number of spots in university. We compare cohorts that reached college age in a narrow window

[1] See Alesina and Rodrik (1994); Persson and Tabellini (1994); Acemoglu (2008); Aghion et al. (2008).

around the time of the military coup that brought Pinochet to power in 1973 and document a sharp kink in college enrollment after the coup. Panel (a) in Figure 1 shows that 13% of the cohort reaching age 21 in 1972 went to college, but only 8% of the cohort reaching the same age in 1981 did (38% decrease). We find that women, who had been making large gains in access to higher education and labor force participation in the years before the coup, were affected as much as men. A synthetic-control analysis confirms the existence of a sizable gap in college completion among Chileans reaching college age after the coup.

We exploit the kink in college enrollment after 1973 to examine the consequences of decreased access to higher education. In the spirit of a regression kink design (Card et al., 2015), we provide reduced-form estimates of kinks in cohort-level trends of socioeconomic outcomes that coincide with the coup, as well as instrumental variables (IV) estimates of the effect of college enrollment, using the respective kink as the excluded instrument. The 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. While the military coup brought about other changes to Chilean society, these should have affected contiguous cohorts of young adults similarly. Moreover, panel (b) in Figure 1 shows no evidence of a kink in secondary completion and we use administrative data to show that the drop in college enrollment was entirely supply-driven. Additional tests following Conley et al. (2012) show that violations to the exclusion restriction would have to be very large (85% or more of the reduced-form effects) to explain away our findings.

Our IV estimates capture the average effect of college entry for the set of compliers whose enrollment was affected by the dictatorship's policies. In accordance with its view of university as a privilege for those most deserving, the regime left untouched the existing admissions algorithm based on students' performance in a centralized exam. This meant that students with lower test scores were the ones affected by the drop in openings. These are arguably the students that Friedman and Friedman (1980, p.176) have in mind when claiming that "attending classes, taking examinations... are not the primary reason they are at school." We show that students from less affluent backgrounds were disproportionately affected, but that they represented a small minority and, as a result, the socioeconomic composition of the student body was left mostly unchanged. In fact, the effect of the policy was broad enough that the kink in college enrollment is observed even among siblings. While the regime's interest in political control was reflected in larger cuts to openings in

some fields (e.g., social sciences), hardly any was left untouched and the distribution of students across fields also remained largely unchanged.

Most of the analysis uses individual-level census data from 1992, 2002 and 2017, which we complement with data from several waves of a large household survey (CASEN) between 1990 and 2015. We restrict the sample to individuals with complete secondary education to ensure a relevant counterfactual to college entry. Naturally, a comparison of different cohorts at any single point in time may be confounded by non-linear age effects. To address this problem, we exploit the availability of data from different years to document the presence of kinks in our outcomes of interest at different points in the life cycle. We also estimate a more stringent specification that replaces the pre-coup trend with age fixed effects, thereby allowing the outcome to vary flexibly at each point in the life cycle.

The census data reveals a large negative kink in labor force participation, indicating that the cohorts exposed to reduced access to college were substantially less likely to be economically active during their prime working years. Among those in the workforce, we also document a positive kink in the unemployment rate. The IV estimate of the effect of college enrollment on labor force participation is almost twice as large for women, while the opposite pattern is observed for unemployment. This suggests that men and women respond along different channels to diminished educational opportunities and that college enrollment was a key driver of female participation in the labor market (Goldin, 2006). Reduced access to university also impacted occupational choice. We find a positive kink in the probability of being a domestic worker, which is almost entirely driven by women. There is also a large negative kink in the probability of having a high-skill, white-collar occupation. This effect is at least 50% larger for women than for men.

We also find a negative kink in various measures of self-reported income in the CASEN survey. Our IV estimate from the more conservative specification with age fixed effects indicates that college enrollment increases total income by 23 log points. The return to college is very similar for men and women, consistent with the presence of a gender wage gap (given the heterogeneous effects on other labor market measures). Using information on the distribution of housing wealth from the 1992 census, we find a large negative kink in the probability of being in the top wealth quintile. Equivalently, college enrollment increases the probability of being in the top quintile by 35 percentage points (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 the dictatorship's educational policy hindered social mobility (Acemoglu et al., 2015).

The final part of the analysis examines the intergenerational transmission of college enrollment. For this purpose, we connect parents and children based on household composition in the 2017 census. Even though this naturally introduces some selection, the results are robust to multiple changes to the sample, including different bandwidths for the ages of children. 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 document a negative kink in college enrollment among children with a parent in the affected cohorts. Our IV estimates indicate that having a parent that enrolled in college increases an individual's own probability of doing so by 32 pp (55% of the sample mean). 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.

We contribute to the literature on educational policies in non-democracies.[2] A large body of work studies the political threat posed by more educated people and the trade-offs that authoritarian regimes face in trying to mitigate it (e.g., lower growth), in line with modernization theory (Treisman, 2020).[3] But a separate literature suggests that autocracies may be better able to implement efficient policies, especially at early stages of development.[4] We connect these literatures by documenting the social cost of the capture of higher education by the Pinochet dictatorship and its technocratic reform agenda.

We also add to the large literature on the monetary and non-monetary returns to education (Card, 1999; Oreopoulos and Salvanes, 2011). Our paper relates to previous work on (i) the effects of higher education on social mobility (Chetty et al., 2020), and (ii) the het-

[2]There are mixed findings on the link between democracy and provision of education at lower levels (Baum and Lake, 2003; Avelino et al., 2005; Harding and Stasavage, 2013; Aghion et al., 2018; Paglayan, 2018). Evidence on higher education is scarce and points to a null effect (Stasavage, 2005; Gallego, 2010).

[3]Theoretical work includes Bourguignon and Verdier (2000); Glaeser et al. (2007); Cantoni and Yuchtman (2013); Alesina et al. (2018); Guriev and Treisman (2020). For empirical papers, see Waldinger (2010, 2011); Cantoni et al. (2017); Roland and Yang (2017); Li and Meng (2018); Alesina et al. (2020).

[4]Alesina and Rodrik (1994); Persson and Tabellini (1994); Acemoglu (2008); Aghion et al. (2008). But see Easterly (2013) on 'benevolent autocrats'. This is part of a broader literature studying the effect of democracy on economic growth (e.g., Barro, 1996; Przeworski et al., 2000; Acemoglu et al., 2019).

erogeneous effects of higher education by gender (Goldin, 2006). As Carneiro et al. (2011) point out, IV estimates of the return to education often correspond to compliers that are uninformative for policy analysis. An important feature of our setting is that the Chicago Boys closely followed the policy advice provided by their mentors in the Chicago School, who viewed Chile as a 'laboratory' in which to test their ideas. Another important feature of our setting is that we study a reduction in college enrollment, while most of the existing literature has focused on expanded access, which could have different consequences.

We contribute as well to the literature on the intergenerational transmission of human capital.[5] Previous research has largely focused on parental education at lower levels, often exploiting quasi-random variation in mandatory schooling (Black et al., 2005; Oreopoulos et al., 2006). A few studies have analyzed the relationship between parental college enrollment and children's early-life outcomes or educational attainment at lower levels, but little is known about the causal link between the college enrollment of parents and children.[6] The novelty of our results relates to the unique features of the decision to go to college (increased agency of child, higher price tag and opportunity cost), which set it apart from other junctures in the education process. 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 and by doing so outside of the handful of highly developed countries in Europe and North America usually studied in this literature. Moreover, our findings show that ignoring the college enrollment decision of the child can lead to substantial underestimation of the effects of parental education.

The rest of this paper is organized as follows. Section 2 provides a historical overview of higher education in Chile and describes the policy changes introduced by the military regime. Section 3 presents our empirical strategy to analyze the effects of these changes and our main data sources. Section 4 documents the fall in educational attainment for the cohorts reaching college age after the coup and characterizes the affected population. Section 5 examines socioeconomic outcomes for the affected cohorts, while section 6 looks at educational attainment for children with parents in these cohorts. Section 7 concludes.

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

[6] See Currie and Moretti (2003); Maurin and McNally (2008); Roland and Yang (2017). 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).

2 The Political Economy of Higher Education in Chile (1965-1981)

2.1 Before the Military Coup

There were eight universities in Chile when Salvador Allende took office in 1970.[7] Only two were public, but the entire system was largely financed by the government. Most universities were based in the larger cities of Santiago, Concepción and Valparaiso, but several had smaller campuses throughout the country. Almost 40% of students were female and 67% went to public universities. Faculty mostly had part-time appointments and rarely had graduate degrees (Brunner, 1984).

College enrollment grew rapidly in the 1960s, jumping from 25,000 students in 1960 to 77,000 in 1970. During the Christian-Democrat government of Eduardo Frei (1964-1970), the gross enrollment rate doubled from 4.6% to 9.2% (Figure 2, panel a). The Socialist government of Salvador Allende (1970-1973) oversaw an even larger increase, reaching 146,000 students and an enrollment rate of 16.8% in 1973. This was a time of massive expansion of higher education throughout Latin America (Brunner, 1984). Higher supply through large public subsidies was seen as a means of achieving equality of opportunity and social mobility, especially for the middle class (PIIE, 1984; Brunner, 1987). Allende also tried to improve access to college for working-class students, with mixed results (Castro, 1977; Schiefelbein and Farrell, 1984).

A broad movement for educational reform began in 1965 and reached the universities in 1967. Besides the large increase in enrollment, the reform furthered student and faculty involvement in university governance. Academic structures were modernized and new programs and research centers were created. Differentiated tuition based on family income was introduced, but fees were not very high.

The reform also modified the admissions process. The old baccalaureate exam administered by Universidad de Chile was replaced by a college admissions test called "Prueba de Aptitud Académica – PAA" (Academic Aptitude Test) starting in 1967. College applicants begun ranking university-program combinations and were awarded a score based on their grades in secondary and their PAA results. Each university chose the weight awarded to each component, which could vary by program, and the number of openings per program.

[7] These were Chile, Técnica Estado, Católica, Concepción, Católica Valparaiso, Austral, Federico Santa María, Norte. The oldest was Chile, founded in 1842, while the newest was Norte, founded in 1956.

A deferred-acceptance algorithm then determined admissions. This system remains largely unchanged until the present day.

2.2 After the Military Coup

Amid growing political polarization and deteriorating economic conditions, Allende was overthrown 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. The junta immediately targeted universities as part of its attempt to eliminate all sources of support for left-leaning political views. Two weeks after the coup, the junta named members of the armed forces as rectors of all universities, both public and private. Over the following months, many students, faculty, and staff were expelled or dismissed for their political views, though the exact numbers remain unclear (Castro, 1977; Brunner, 1984). Some were detained, tortured, or killed.[8] Unions and student groups were shut down, political activity was forbidden and teaching materials were censored.

The dictatorship's initial approach to universities, focused on repression and control, soon begun to give way to a technocratic concern about the size of government and the efficiency of public spending (Echeverría, 1980; PIIE, 1984; Velasco, 1994).[9] This was the result of the growing influence over policy of a group of economists known as the 'Chicago Boys', most of whom had studied at the University of Chicago under the likes of Milton Friedman and Arnold Harberger (Valdés, 1995).[10] As early as 1974, the Ministry of Finance begun pushing for a reduction in subsidies to universities and increased self-financing, while the Ministry of Education's program for 1975 called for a more efficient use of resources and set enrollment goals that put an end to the high growth from previous years (PIIE, 1984; Levy, 1986). Naturally, these restrictive measures also helped to reduce the political threat posed by universities. "The regime's penchant for political control

[8] Using detailed individual records from the final report by Comisión Rettig (1996), we count 24 university professors and 252 university students among the roughly 3,200 documented deaths attributed to the Pinochet regime. These death counts correspond to 0.2% of the respective numbers of full-time faculty and students in 1975. The report by the subsequent Comisión Valech (2004) estimates that around 4,100 of the 38,000 victims of detention or torture were students (including lower levels).

[9] Kim (2011) describes a similar transition from an initial wave of intense repression to a phase of technocratic reform during the dictatorship of Park Chung-Hee in South Korea.

[10] The military regime's lack of a coherent plan of government and the economic crisis it soon faced contributed to the rise of the Chicago Boys. Moreover, the Chicago School's aversion to government intervention went well with the regime's fight against Socialism (Velasco, 1994).

meshed conveniently with its penchant for economic conservatism" (Levy, 1986, p.105).

The Chicago Boys' critique of Chilean universities can be found in their manifesto, a lengthy volume known as 'the brick' (CEP, 1992). It was based on two main arguments, both rooted in the Chicago School of economics.[11] First, they argued that an assured stream of government transfers failed to provide universities with incentives for thrift or innovation. Moreover, the high cost per student meant that college, rather than being a right and open to all, should be considered a privilege reserved for those most deserving. In 1979, Pinochet claimed that while the state was obliged to provide basic (i.e. primary) education to everyone, secondary and higher education should be considered an "exception" (PIIE, 1984). In this regard, the regime broke away from the progressive view of higher education as a vehicle for social mobility and embraced a more traditional view of universities as centers of academic excellence and elite training. Underlying this argument was the idea that college enrollment is an investment in human capital, as posited by Becker (1964). Large subsidies distort this investment and turn college into "a pleasant interlude between high school and going to work" (Friedman and Friedman, 1980, p.176).

Secondly, the Chicago Boys also argued that the government's education budget was better spent on lower levels. This argument was made both on grounds of efficiency and equity. Regarding efficiency, the dictatorship's educational plan for 1977 claimed that it had been "scientifically proven that the social return of pre-school and primary education is higher to that of other levels" (PIIE, 1984, p.89 *own translation*). While the source for this claim is unclear, a similar argument would later become strongly associated with the work of another Chicago scholar, James Heckman (2006). Regarding equity, the claim was that higher education serves a minority of students who disproportionately come from the higher classes. "We know of no government program that seems so inequitable in its effects... as the financing of higher education... [We] have conned the poor into subsidizing us on the grand scale" (Friedman and Friedman, 1980, p.183).

These arguments called for lower subsidies and higher tuition. In practice, the regime was more successful at reducing transfers to universities than at making them financially independent. Panel (b) in Figure 2 shows that the share of the education budget devoted to higher education, which had risen during the Allende years to almost 50%, steadily declined after 1974 and returned to its pre-Allende level of 30% in 1980. As government

[11] Regarding Friedman's influence on the Chicago Boys, Valdés (1995, p.192) says that "all of them were strongly influenced by his books" and describes them as "even more Friedmanite than Friedman himself."

subsidies were the main source of funding for universities, equivalent to 77% of total revenue in 1972 (Appendix Figure A1), this was a substantial financial blow. However, the push for higher tuition met with strong resistance and was temporarily abandoned (Levy, 1986). As a result, universities had no choice but to downscale their operation. Figure 3 shows that the yearly number of openings for incoming students, which had risen in tandem with spending under Allende, similarly fell after the military coup.[12] This led to a sharp decline in the college enrollment rate, which also returned to a level comparable to the one before Allende (10.8%), as panel (a) in Figure 2 shows.[13] Importantly, Figure 3 also shows that the number of college applicants generously exceeded the number of openings, especially after the coup, indicating that supply was always the binding constraint.[14]

Panel (b) in Figure 2 shows that public spending on primary education benefited most from the drop in subsidies to higher education, consistent with the regime's spending priorities. However, panel (a) shows that the enrollment rate in primary remained roughly constant, as did the enrollment rate in secondary education. The only level with substantial growth in enrollment after the coup was early education, which almost tripled in size between 1970 and 1980 (from a base rate of 4%).[15] Moreover, the number of primary and secondary schools remained essentially unchanged between 1973 and 1977, while the share of primary students receiving subsidized breakfast or lunch (a proxy for pro-poor policies) decreased after the coup (Appendix Figure A4). While an overall assessment of the dictatorship's educational policies is beyond the scope of this paper, these figures provide *prima facie* evidence that the decline in college enrollment in the early years of the military regime was not offset by large improvements in lower levels of education.

The regime only fully articulated its educational policy in a new directive issued in 1979, which developed many of the initiatives pursued, even if haphazardly, in the previous years. A large reform to higher education followed in 1981. This reform turned the satellite campuses of the public universities into independent institutions and opened the

[12] Appendix Figure A1 shows that payroll as a share of total expenses fell from a peak of 75% in 1972 to 62% in 1980, consistent with dismissals of faculty and staff. Appendix Figure A2 shows that the drop in openings was mostly driven by the public universities, which had also driven most of the growth before.

[13] The unanticipated nature of the drop in enrollment is evidenced by the fact that UNESCO projections placed it at 200,000 students for 1975, while the actual figure fell slightly short of 150,000 (Levy, 1986).

[14] Appendix Figure A3 provides evidence against conscription and student migration as alternative explanations. The number of enlisted soldiers and students abroad both fall after the coup.

[15] However, growth mostly followed the pre-coup trend. Furthermore, the body overseeing early education was the National Council of Kindergartens (JUNJI), established in 1970 during the Allende government.

system to competition by new universities, which were not eligible for government funding. Subsidies to existing universities were further reduced, forcing them to increase tuition.

3 Data and Empirical Strategy

3.1 Data

Our main data source are the individual records from the Chilean household censuses of 1992, 2002 and 2017.[16] In each census, individuals are classified into households, one person per household is identified as the head, and the relationship to this person is reported for all others. The census collects basic demographic information and also asks questions on educational attainment and labor market outcomes. The 1992 dataset includes an additional calculated variable indicating the wealth quintile to which the household belongs, based on observable characteristics of the dwelling and ownership of assets.

We complement the census data with twelve waves of the biennial CASEN household survey between 1990 and 2015.[17] This is a repeated cross-section that is representative at the regional level.[18] Sample size has grown over time, with the latest wave surveying more than 260,000 individuals in over 80,000 households. The CASEN survey includes information on education and economic conditions for all members of each surveyed household. It has several attractive features, including its relatively high frequency (compared to the census) and the availability of information on income, albeit self-reported.

For the synthetic control analysis, we use harmonized census micro-data from IPUMS - International for 57 countries listed in Appendix Table I1. Our main variable of interest is college completion, since harmonized data on enrollment is not available, but we also use other country characteristics in the construction of the synthetic control.

We restrict the analysis to individuals 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 end the sample with the 1981 cohort to mitigate the confounding effect of the university reform implemented by the military regime after that year. We focus on age 21 because administrative data shows that 20.5 is a lower bound for

[16] Supplementary Appendix I provides more detailed information about these sources.
[17] Survey years are 1990, 1992, 1994, 1996, 1998, 2000, 2003, 2006, 2009, 2011, 2013, 2015.
[18] Chile is administratively divided into 16 regions, subdivided into provinces (56) and counties (346).

the average age of first-year college students at the time of the coup.[19] Results are robust to small changes in the age of college entry (i.e. changes in the kink point). We only keep people that report at least four years of secondary (completion is unavailable in all sources except 2017 census). 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 use the information on household composition from the 2017 census, which is arguably the best source on the final education attainment of children with a parent in the affected cohorts.[20] 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).[21] In most cases, we can confidently connect a child to only one parent.[22] 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 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. Parents in this sample must meet the same conditions as in our main sample above: (i) 4+ years of secondary (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 caveat to our analysis of intergenerational effects is that we can only connect parents and children living together in 2017. 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

[19] Appendix Figure A5 shows the age distribution of first-year college students in 1960, 1970 and 1975.

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

[21] An extra 5% corresponds to household heads that have a parent living with them. 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.

[22] We observe both parents for a small number of household heads and pick the oldest parent. Unfortunately, the sample is too small to study potential complementarities in the effects of parental education.

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 reduced access to higher education by comparing changes in trends for cohorts that reached college age around the time of the military coup, in the spirit of a regression kink design (Card et al., 2015). Our identifying assumption is that in the absence of the coup there is no reason to expect a change in the trend of our outcomes of interest for cohorts reaching college age (i.e. age 21) shortly after.[23] We work with the following reduced-form model to estimate the effect of exposure to the dictatorship:

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

where $Y_{i,c}$ is an outcome for individual i belonging to cohort c (which indicates the year in which the cohort reached age twenty-one). X_i is a set of observable characteristics, including gender-specific county-of-birth fixed effects. $\mathbb{1}(c \geq 1973)$ is a dummy equal to one for those individuals (cohorts) that reached age twenty-one in 1973 or later, and $u_{i,c}$ is an error term clustered at the county-of-birth level. To account for clustering by cohort, we also provide p-values from the Wild cluster bootstrap procedure following Cameron et al. (2008). Finally, $f(c)$ and $g(c)$ are smooth functions (polynomials) representing the birth cohort profile of outcome $Y_{i,c}$. 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., $f(c) = g(c) = c$) to avoid over-fitting and provide visual evidence showing that it describes appropriately the evolution of cohort means for most outcomes. Our parameter of interest is π_1, which directly captures the change in trend after 1973:

$$\pi_1 = \frac{\partial Y_{i,c}}{\partial c} \Big|_{\mathbb{1}(c \geq 1973)=1} - \frac{\partial Y_{i,c}}{\partial c} \Big|_{\mathbb{1}(c \geq 1973)=0}.$$

As mentioned above, 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. The discrete nature of the running variable prevents us from applying a non-parametric approach to select an optimal bandwidth,

[23] 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).

but we verify that our results are robust to more conservative bandwidths.

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. To address this problem, we exploit the availability of information from multiple censuses and the CASEN survey to document the presence of kinks in our outcomes of interest at different points in the life cycle. We also use this information to estimate a more stringent specification that replaces the baseline cohort trend with a set of flexible age fixed effects (plus 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.

We can also leverage the cross-cohort variation induced by the dictatorship as an excluded instrument for college enrollment. For this purpose, we estimate the following system of equations:

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

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

where $E_{i,c}$ stands for college enrollment for individual i belonging to the cohort that reached age 21 in year c. 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. Under standard assumptions, the instrumental variables (IV) 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 that failed to attend college because of the reduced supply by the military regime. From the admissions process outlined in section 2, we know that students with the lowest test scores among those that would have been admitted were those affected. These are arguably the students for whom Friedman and Friedman (1980, p.176) argue that "attending classes, taking examinations... are not the primary reason they are at school." Those same students that according to the Chicago Boys' manifesto will "go to university to illustrate their laziness" (CEP, 1992, p.147 *own translation*). Hence, our IV estimates are informative about the socioeconomic implications of the policy advice predicated by the Chicago Boys and their mentors.

The IV analysis also requires an exclusion restriction implying that the change in out-

comes for the cohorts that reached age twenty-one after 1973 is solely driven by reduced college enrollment. Even though the dictatorship brought about other changes to Chilean society, their cohort-level impact would have to take a very specific shape to violate this assumption. For instance, while the disruption caused by the coup and the early wave of repression may have affected the cohorts reaching college age right then, it is in fact later cohorts that we find to be most affected by the contraction of higher education. A similar argument suggests that underlying economic conditions are not driving our results, as our most affected cohorts correspond to the period of sustained economic growth after 1975, "the high point of the military regime" (Valdés, 1995, p.35). Even if these cohorts were more exposed to ideological indoctrination at a young age, the regime's advocacy for economic self-sufficiency should have led, say, to higher labor force participation, which is the opposite of what we find. More generally, any other change besides the reduced supply of college should have affected contiguous cohorts of young adults similarly. The fact that our results are robust to shorter bandwidths (as little as four cohorts on either side), constitutes further evidence against violations of the exclusion restriction. As a final check, we employ the methodology proposed by Conley et al. (2012) to show that violations of the exclusion restriction would have to be very large to explain away our findings.

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 own probability of enrollment. This is also a LATE for those children with a parent whose enrollment was affected by the dictatorship. The exclusion restriction states in this case that the cohort of the parent only affects the child's educational attainment through its effect on the parent's college enrollment (and downstream outcomes). 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: (i) gender by county of birth, (ii) gender by parent's gender, (iii) relationship to household head and (iv) age. The latter alleviates the concern that parents from later cohorts will tend 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.

4 Results: Educational Attainment

4.1 Non-Parametric Analysis

Panel (a) in Figure 1 shows the share of people per cohort that report any college in the 1992 census. In the x-axis, cohorts are organized by the year in which they reached age twenty-one (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 rapid increase in college entry for the cohorts that reached college age before the coup, especially during the Allende government between 1970 and 1973, followed by a large decline for those cohorts that reached the same age after the coup. Enrollment increased 6 percentage points (pp) between the 1964 and 1972 cohorts (86% increase) and decreased 5pp between the 1972 and 1981 cohorts (38% decrease). Panel (b) shows that the share with full secondary increases smoothly throughout, ensuring that the drop in college enrollment is not driven by reduced attainment at lower levels.[24] Panel (c) shows the college enrollment rate among people with full secondary, the relevant counterfactual. It displays a similar pattern of growth and decline around the military coup as in panel (a). This pattern reflects the fact that an ever larger number of graduates from secondary education competed over an ever smaller number of spots in university, in what has been described as the "strangulation" (Levy, 1986, p.102) of higher education. Panel (d) shows similar patterns of growth and decline in college enrollment for both men and women.

4.2 Parametric Analysis

Table 1 presents estimates of equation (1) for various measures of educational attainment in the 1992 census. The sample in columns 1-4 includes all respondents reaching age 21 between 1964 and 1981. Columns 1 and 2 show that the shares of people with any or full secondary education were growing at respective rates of 1 pp and 0.8 pp per cohort before the coup. These rates remain unchanged after the coup and there is no evidence of a kink. Panels (a) and (b) in Figure 4 plot the raw data and the estimated linear trends. The solid lines show the trends before and after the coup, while the dashed line is the counterfactual trend for the post-coup period. The markers correspond to cohort-level averages. The

[24] Appendix Figure A6 shows that the kink in college enrollment is also present in other sources. Appendix Figure A7 shows smooth increases in the shares of people with any primary or secondary.

actual post-coup trends overlap almost perfectly with the counterfactuals in both panels.

Column 3 in Table 1 shows that college enrollment increased on average 0.8 pp per cohort among those reaching age 21 before the coup. This trend *decreased* by 1.3 pp for cohorts reaching the same age after the coup. The difference between the two coefficients indicates a net negative trend of 0.5 pp per year for the affected cohorts. Panel (c) in Figure 4 illustrates this sharp kink. Column 4 shows that the combination of reduced college enrollment and no change in lower levels leads to a slight decline in the trend for years of education (0.17 extra years per cohort before the coup vs 0.11 after). Column 5 replicates the analysis for the restricted sample reporting four or more years of secondary, our proxy for secondary completion. We observe a similar pattern as in column 3. A positive trend of 1.8 pp per cohort before the coup that completely reverses and becomes -1.8pp afterwards.[25] Panel (e) in Figure 4 shows this kink. Column 6 shows that the results are unchanged if we restrict the sample to siblings (96% decrease in sample size) and include family fixed effects.[26] We return to this result in subsection 4.4 below.

Column 7 examines enrollment in any tertiary education, including vocational school. The trend among cohorts reaching college age before the coup is slightly larger than in column 5, indicating that other institutions were also growing, though not as much as universities. The fact that the post-coup trend is smaller at -1.1pp than the one for college enrollment indicates some substitution of college education with vocational schooling.

4.3 Synthetic control analysis

In this section, we present a synthetic control analysis that provides an alternative counterfactual for the evolution of higher education in Chile after 1973 (Abadie and Gardeazabal, 2003). Our baseline estimates use data from Latin American countries to construct the counterfactual, but results are unaffected if we also use data from other countries.[27] We calculate the share of people with complete secondary and college per cohort in each

[25] Appendix Table B1 shows similar results for the other censuses and the CASEN survey. Appendix Table B2 shows that the results are robust to small changes in the location of the kink point. Appendix Table G1 provides disaggregate estimates by gender. In the pre-coup years, female college enrollment was growing 2.2 pp per cohort, while male enrollment had a yearly growth rate of 1.5 pp. After the coup, the net trend for men equals -1.7 pp, while for women it is -1.8 pp, consistent with the graphical evidence in panel (d) of Figure 1.

[26] We classify as siblings people within the same household meeting one of these conditions: (i) two or more 'children of the household head'; (ii) the head and one or more 'siblings of the household head'.

[27] Appendix Figure C1 shows that the results are unaffected if we include all 57 countries with available data or if we exclude all countries with a dictatorship in the years 1950-1990.

census, restricting the sample to individuals over 20 years of age. We focus on college completion because IPUMS does not provide harmonized data across countries on educational enrollment. All estimates use lags of the share of people with complete college to build the synthetic control.[28] 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, and the share with secondary education.

Panel (a) in Figure 5 shows the baseline results. The solid line corresponds to actual college completion by cohort in Chile, while the dashed line shows the prediction from the synthetic control. We observe that the synthetic control tracks the realized time series very closely up to the year of the coup and exceeds it afterwards.[29] The synthetic control keeps growing, while the actual series stagnates and falls.[30] 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 Composition of the Student Body and Student Outcomes after the Military Coup

In this section, we provide additional results helping us to characterize the compliers that were affected by the contraction of higher education after the military coup. We also ex-

[28] We follow Ferman et al. (2019) and use only *odd* years to avoid cherry-picking and overfitting. Appendix Table C1 shows that results are identical if we use *even* or *all* pre-treatment years. The R^2 of a regression of the Chilean data on the synthetic control in the pre-treatment period is always larger than 0.95.

[29] Placebo inference and confidence sets suggest this difference is statistically significant (Abadie et al., 2015; Firpo and Possebom, 2018). See Appendix Table C1 and Appendix Figure C2 for details.

[30] The synthetic control suggests that the share of the population with full college would have been close to 8% in 1981, while in reality it was just 5%. This gap in completion is equivalent to 38% of the counterfactual provided by the synthetic control. Relatedly, panel (c) in Figure 4 shows that the enrollment rate in 1981 would have been 20% had the pre-coup trend remained, but in reality it was just 8.5%. The gap in enrollment is equivalent to 58% of the counterfactual provided by the trend. The ratio of the gaps in completion and enrollment suggests a graduation rate of 66% for the missing students. This number is very close to the college completion rate of the cohort reaching college age in 1973 (69%) shown in Figure 8.

amine potential changes to post-enrollment outcomes, including fields of study, graduation rates and the return to college.

The presence of a kink in college enrollment within families, documented in column 6 of Table 1, indicates that university downsizing had a broad impact.[31] This was arguably the result of the matching algorithm used for admissions, which limited the government's influence. "Notwithstanding scandals involving personal favoritism, the regime's basic policy has been nominally objective" (Levy, 1986, p.102). However, the admissions algorithm also caused the reduction in openings to mechanically affect students with lower PAA test scores. Panel (a) in Figure 6 shows the average score of admitted students in 1976 and 1981, disaggregated by father's occupation.[32] We have normalized to 100 the highest value in each year, corresponding to children of university faculty. We observe that the distribution of test scores compressed as admissions tightened between these years. Admitted students from poorer families (i.e. father in blue-collar occupation) had lower average test scores in 1976 and experienced the largest relative increases in 1981. However, the large reduction in openings meant that it was not just the marginal students barely making the cut before who failed to gain admission. In fact, we see relative improvements in test scores for all parental occupations. Panel (b) shows the distribution of students by father's occupation in these same years. Even though students from less affluent backgrounds were disproportionately affected by the reduction in openings, these students represented a very small share of the student body and the distribution hardly changed as a result.[33]

University downsizing did not affect all fields of study equally, but hardly any was left untouched. Panel (a) in Figure 7 shows the change in openings between 1973 and 1980 by field. Political targeting led to a larger drop in the social sciences and agricultural studies, while the natural sciences were the only to grow. Most fields saw aggregate decreases in openings of 20-30%, including the two fields with the largest shares of students, education and engineering, and the more traditional fields of health and law. As a result, the distribution of students across fields did not change very much, as panel (b) illustrates.

Figure 8 shows that post-enrollment student outcomes improved after the military takeover. Panel (a) focuses on the graduation rate, which we measure as a share of total enroll-

[31] Appendix Table B3 replicates this analysis for the other censuses. Appendix Table B4 shows that the kink in college enrollment is also present within quintiles of housing wealth in 1992.

[32] While these are both post-coup years, they still capture the ongoing contraction of higher education (i.e., 23% reduction in the share with college in Figure 1). Unfortunately, data is unavailable for other years.

[33] Appendix Figure A9 shows that the distribution of father's educational attainment was also unaffected.

ment using UNESCO data (circles) or of the number of people reporting any college in the 1992 census (triangles). Both sources show a similar pattern of declining graduation rates during the Allende years followed by a sharp rebound after the coup (10% increase). This was arguably driven by the tighter admission standards and by the regime's renewed focus on universities as centers for academic excellence.[34] Importantly, the census data also suggests that roughly 70% of the dictatorship's missing students would have graduated.

Panel (b) shows cohort-specific estimates of the effect of college enrollment on log total income using data from the CASEN survey between 1990 and 2015. Additional controls include county of residence by gender, survey year and age fixed effects.[35] Before the coup, the return to college fluctuated around 70 log points, with a weakly decreasing trend. After the coup, we see a sharp jump and a positive trend. The post-coup average of about 80 log points represents a 14% increase.[36] This higher return could again be a reflection of the higher quality of admitted students or their higher graduation rate (panel a). It is also consistent with imperfect substitution between similarly-educated workers in different age groups (e.g., Card and Lemieux, 2001). These results suggest that the contraction of higher education after the coup did not lead to a decline in the quality of education, though repression and censorship likely had a negative impact on the students experience.

5 Results: Socioeconomic Consequences

In this section, we document the downstream effects of the contraction of higher education carried out by the dictatorship. We first examine labor market outcomes and employment characteristics using data from the 1992 and 2002 censuses.[37] We then look at measures of income from the CASEN survey and housing wealth from the 1992 census.

[34] Panel (a) in Appendix Figure A8 shows that average (raw) PAA test scores improved after the coup. This suggests that people with lower expected scores drove the decline in test-taking after 1975 (Figure 3). Panel (b) shows that these people predominantly came from previous graduating cohorts.

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

[36] Appendix Figure A10 shows a similar pattern for other income measures available in CASEN. It also shows that the results are robust to the inclusion of occupation fixed effects.

[37] The 2017 census does not ask questions on occupation. Additionally, the kink in college enrollment roughly coincides with the age of retirement in that year, potentially biasing the results: the cohort reaching age 21 in 1973 is 65 years old in 2017, which is the age of retirement for men (women retire at 60).

5.1 Labor Market Outcomes and Employment Characteristics

Columns 1-4 in Table 2 show results on labor force participation, unemployment and employment characteristics. In each column, we present the reduced-form estimates of equation (1) and IV estimates of the effect of college enrollment from equation (3), using the kink for the cohorts reaching age 21 after 1973 as the excluded instrument. We focus here on the 1992 census, but the results for 2002 are available in Appendix Table F1.

Column 1 in Table 2 shows that cohorts reaching college age before the coup had a positive trend in labor force participation. However, this trend reverses and becomes negative for the cohorts that reached college age after the coup, mirroring the trend in college attainment. This kink is clearly visible in panel (a) of Figure 9. The IV estimate reported at the bottom of the table indicates that college enrollment leads to a 33 pp increase in labor force participation in 1992, equivalent to 43% of the respective sample mean. We observe the opposite pattern when we look at unemployment in column 2.[38] The negative trend for cohorts reaching college age before the coup reverses and becomes positive after 1973, as panel (b) in Figure 9 shows. The IV estimate suggests that college enrollment reduces unemployment by 6 pp, relative to a sample mean of 3%.

Regarding employment characteristics, columns 3 and 4 present results for the probability of being a domestic worker (e.g., maid, driver) and the probability of holding a white-collar high-skill occupation.[39] Before the coup, members of younger cohorts were less likely to be domestic workers and more likely to have a white-collar high-skill occupation, but both trends reverse after the coup. Panels (c) and (d) in Figure 9 show sharp kinks for these outcomes among the cohorts reaching college age after the coup. According to the IV estimates, college enrollment decreases the probability of domestic work by 3 pp, relative to a sample mean of 1%, and increases the probability of a white-collar high-skill occupation by 48 pp, a large effect size over a base of 43%.

The previous results provide strong evidence of worsening labor market outcomes for the cohorts reaching college age after the military coup. We report the Kleibergen-Paap F-statistic at the bottom of each column in Table 2 as evidence of a very strong first stage.

[38] We keep the full sample for this analysis and include a dummy for labor force participation, assigning a zero in the unemployment measure to those out of the workforce. Results are unaffected if we restrict the sample to individuals in the labor force.

[39] These outcomes correspond to separate census questions. Alternatives to domestic worker are business owner, self-employed, wage-earner and unpaid work for relative. Other occupation categories are white-collar low-skill, blue-collar (high and low skill) and military. See Appendix Tables F3 and F4 for full results.

The sharp kinks documented for all outcomes suggest a tight relationship with college enrollment. As further evidence in support of the exclusion restriction, additional tests following Conley et al. (2012) reveal that the direct effect of being in a cohort reaching college age after the coup would have to be equivalent to at least 85% of the reported reduced-form effects to explain away our findings. Moreover, Appendix Figure F1 shows that the previous results are robust to the use of shorter bandwidths (as little as four cohorts on either side of the kink). Appendix Table F1 further shows that similar kinks are present in the 2002 census.[40] The fact that we find the same result in two censuses ten years apart suggests that we are not simply picking up non-linear age effects. Appendix Table F2 shows similar results for the pooled census sample, including a specification with age fixed effects that allows the outcomes to vary flexibly at each point in the life cycle.[41]

5.2 Income and Wealth

Columns 5 and 6 of Table 2 provide results for reported income in the CASEN survey. For this analysis, we pool all twelve survey waves between 1990 and 2015 and focus on total income.[42] Though this data is entirely self-reported, there is no reason to expect differential measurement error across cohorts. Column 5 shows reduced-form and IV estimates from our baseline specifications, while column 6 shows results from the more stringent specification with age fixed effects. The reduced-form results in column 5 show a positive trend in total income of 1.1 log points per cohort before the coup. This trend also reverses after the coup and becomes -0.9 log points per cohort, as panel (e) in Figure 9 shows. The corresponding IV estimate indicates that college enrollment increases yearly income by 76 log points. A similar pattern is observed after adding age fixed effects in column 6. The reduced-form estimate points to a 0.6 log-point decrease in income for each new cohort reaching college age after the coup. The IV estimate indicates that college enrollment raises income by 23 log points. This coefficient is smaller than the one in column 5, but

[40] Appendix Figure F2 displays the raw data.

[41] In this specification, reduced-form estimates capture the trend for the affected cohorts relative to the average for that exact age. The common support across censuses corresponds to ages 42-49. This post-coup trend becomes the excluded instrument for the IV.

[42] Income measures in the CASEN survey include (i) main occupation, (ii) all work, (iii) self-generated, and (iv) total. Appendix Figure F3 shows that the estimates are fairly stable if we run the analysis separately for each wave and income measure. Appendix Figure F4 plots the raw data for each measure.

remains sizable and its magnitude is comparable to previous estimates in the literature.[43]

We complement the analysis of the socioeconomic cost imposed on the cohorts reaching college after the military coup 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 3 shows results using these quintile dummies as dependent variables. We observe that the affected cohorts were increasingly less likely to reach the top of the socioeconomic ladder. While each new cohort before the coup was 0.2 pp less likely to be in the richest 20% in 1992, this trend drops to -1.5 pp per cohort after the coup. Panel (f) in Figure 9 illustrates this kink. The IV estimate in column 1 indicates that college enrollment increases the probability of reaching the top quintile by 35 pp, equivalent to 70% of the sample mean. The remaining columns show a corresponding drop in the probability of being in the second to fourth quintiles of 10-12 pp each and a 2.4 pp decrease in the probability of being in the bottom quintile.[44] Even though we lack information on the individual's family background, there is no reason to expect people born in different years to come from more or less affluent families. Hence, these results provide *prima facie* evidence that the Chicago Boys' technocratic approach to higher education hindered social mobility for an entire generation.

Despite the smaller sample size of the CASEN survey, the first stage remains strong in this part of the analysis. Following Conley et al. (2012), we estimate that the direct effect of being in the affected cohorts would have to be equivalent to at least 87% of the reduced-form coefficients to make our IV estimates on income and wealth insignificant. Appendix Figure F1 further shows that these results are also robust to smaller bandwidths.

5.3 Heterogeneous Effects by Gender

We conclude our analysis of the socioeconomic consequences of the capture of higher education by the Pinochet regime by exploring heterogeneous effects by gender. Figure 10 10 presents separate IV estimates for men and women.[45] Panel (a) shows that the effect of

[43] Zimmerman (2014) provides an RDD estimate for the effect of college admission on income of 22%. Equivalently, our IV estimate in column 6 corresponds to a 7 log-point increase in income per year of education, comparable to the 9% estimate from the meta-analysis by Psacharopoulos and Patrinos (2018).

[44] Appendix Figure F4 plots the raw data for all quintiles.

[45] The interested reader can find the full results disaggregated by gender in Appendix E.

college on labor force participation is around 50% larger for women than for men. This means that college was fundamental for female participation in the labor market, in line with Goldin (2006). However, the effect of college on unemployment (conditional on labor force participation) is larger for men, suggesting that men and women respond along different channels to diminished educational opportunities. Panel (b) shows that the effect of college enrollment on the probability of being a domestic worker or having a white-collar high-skill occupation is also significantly larger for women. Despite these larger impacts on labor market outcomes, panel (c) shows that the return to college is weakly larger for men in the baseline specification, and not different once we include age fixed effects. This pattern is present for all income measures (see Appendix Table G3 for details). Panel (d) similarly shows that college has a larger effect on the probability of being in the richest 20% for men. This differential effect is consistent with the finding by Zimmerman (2019) that elite colleges in Chile help men reach top jobs and incomes, but not women.[46]

6 Results: Intergenerational Transmission of Human Capital

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 were 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 4 shows that college enrollment among the parents of the children in our sample exhibits a pattern essentially identical to the full sample in Table 1, 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 the parent and the college enrollment of the child. 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 cohort. But this trend reverses for those with a parent in the affected cohorts and becomes -0.1 pp per year. This suggests a positive causal relationship between the college enrollment

[46] This result could also 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 vice versa, the marriage market would attenuate the effect of college enrollment on wealth for women.

of parents and children. Using the kink in parental college enrollment as an excluded instrument, panel C shows that having a parent that went to college increases a person's chances of enrolling by 26 pp. This is a large effect, equivalent to 45% of the sample mean of 58%, but 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. Alternatively, while the IV estimate eliminates the selection causing upward bias in OLS, it provides a LATE for complier parents that might have benefited disproportionately from college enrollment before the coup (Card, 1999).

The only controls in column 1 are the gender by county of birth 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. In column 4 we include an additional set of dummies for the relationship of the child to the household head in the 2017 census, which we use to link parents and children. The results change very little across specifications.

In column 5 we introduce age fixed effects for the child. These controls help address the concern that children with parents in later cohorts (i.e. more affected) are themselves likely to be younger, which could bias downwards the estimate of the intergenerational effect if younger people benefited 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. However, the inclusion of age fixed effects also means that we are now comparing children of the same age born to parents from different cohorts (i.e. parents with different age at the time of birth), which could also be an important factor. Hence the importance of showing the robustness of the results to the exclusion of these fixed effects.[47]

Figure 11 provides a non-parametric visualization of the results in column 5. Panel (a) shows the first-stage estimates and their 95% confidence interval. As above, cohorts reaching college age before the coup experience a growing probability of college enrollment, while those reaching the same age after the military take-over see a sharp decline. Panel

[47] Having a parent with college could partly affect the child through the education level of the other parent, which we do not observe. Appendix Table H2 uses information on the spouses of linked parents (household heads) and shows that parents with college are more likely to have a spouse and also more likely to have a spouse with college (conditional on having a spouse). Controlling for the education level of the spouse reduces the effect of the linked parent's college enrollment by 22%.

(b) shows the reduced-form relationship between the cohort of the parent and the college enrollment by the child. We observe a clear decline for children with a parent that reached college age after the coup. In particular, a child with one parent reaching age 21 in 1979 is 5 pp less likely to go to college (9% of sample mean) than a child with a parent born seven years before (1972), but the latter is just as likely to attend college as a third child with a parent born seven years before (1966), despite the fact that all three children were born on the same year and the relevant parents all completed secondary education.

To further understand the stage in the educational process at which children with parents in the affected cohorts lag behind, column 6 of Table 4 includes an additional control indicating whether the child completed secondary education. As expected, this control absorbs some of the variation in college enrollment. However, its inclusion only leads to a 12% reduction in the IV estimate (28 pp), indicating that most of the effect of parental college enrollment materializes after children complete secondary. This result has important implications for the study of the intergenerational transmission of human capital, as studies often focus on children's education outcomes at lower levels. Figure 12 plots IV estimates from our preferred specification using completion of each grade in primary and secondary or 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 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.

We subject the previous results to a battery of robustness tests. Appendix Figure H1 shows that the results are hardly affected if we consider more conservative bandwidths for the ages of parents. We additionally estimate that the direct cohort effect would have to be equivalent to at least 64% of the reduced-form effect to explain away the intergenerational transmission of college enrollment. Appendix Table H3 further shows that the results are robust to different windows of ages for the children included in the sample. For instance, the IV estimate for children with ages in the tighter 25-30 age window is 0.29, similar to the baseline estimates in Table 4. Table H4 shows that the effect of parental college enrollment is stronger for children 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 with a parent 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.

7 Conclusion

Chile's Pinochet dictatorship is often praised for its technocratic reforms and is often hailed, alongside South Korea or Singapore, as a prime example of the benefits of non-democracy in the early stages of economic development. In this paper, we study the capture of higher education by the Pinochet regime following the 1973 military coup. After an initial wave of repression and political cleansing, the early years of the dictatorship were characterized by the implementation of a technocratic reform agenda. Government funding for universities steadily declined as a result of the growing influence over policy of a group of technocrats, the Chicago Boys, who viewed subsidies to higher education as an inefficient use of public funds. These views echoed those of their mentors in the Chicago School of economics, most notably Milton Friedman. In this regard, Chile was a 'laboratory' in which many policy recommendations from prominent members of the Chicago School were put in practice. Our setting provides a unique opportunity to study their socioeconomic consequences.

We show that the cut in funding forced universities to reduce the number of openings for incoming college students, which led to a sharp decline in enrollment for the cohorts that reached college shortly after the coup. Contrary to the Chicago Boys' claim that Chilean universities were bloated and had low returns, we show that the affected cohorts had substantially worse economic outcomes throughout the life cycle and struggled to climb up the socioeconomic ladder. These negative effects were particularly large for women, who had been making rapid progress in the labor market in the years before the coup. While it is beyond the scope of this paper to make an overall assessment of educational policy during the Pinochet dictatorship, our results show that the regime's technocratic approach to higher education was very costly and hindered social mobility for an entire generation. We further show that it is erroneous to write off this cost as the inevitable but transitory price to be paid in the construction of a well-functioning market economy, as children with a parent in the affected cohorts are substantially less likely to enroll in college, even several decades after democratization.

References

Abadie, A., Diamond, A., and Hainmueller, J. (2015). Comparative politics and the synthetic control method. *American Journal of Political Science*, 59(2):495–510.

Abadie, A. and Gardeazabal, J. (2003). The economic costs of conflict: a case study of the Basque country. *American Economic Review*, 93(1):113–132.

Acemoglu, D. (2008). Oligarchic Versus Democratic Societies. *Journal of the European Economic Association*, 6(1):1–44.

Acemoglu, D., Naidu, S., Restrepo, P., and Robinson, J. A. (2015). Democracy, redistribution, and inequality. In Atkinson, A. and Bourguignon, F., editors, *Handbook of Income Distribution*, volume 2, pages 1885 – 1966. Elsevier.

Acemoglu, D., Naidu, S., Restrepo, P., and Robinson, J. A. (2019). Democracy Does Cause Growth. *Journal of Political Economy*, 127(1):47–100.

Aghion, P., Alesina, A., and Trebbi, F. (2008). Democracy, technology and growth. In Helpman, E., editor, *Institutions and Economic Performance*. Harvard University Press.

Aghion, P., Jaravel, X., Persson, T., and Rouzet, D. (2018). Education and Military Rivalry. *Journal of the European Economic Association*, 17(2):376–412.

Alesina, A., Giuliano, P., and Reich, B. (2018). Nation-Building and Education. NBER Working Paper 18839.

Alesina, A. and Rodrik, D. (1994). Distributive Politics and Economic Growth. *Quarterly Journal of Economics*, 109(2):465–490.

Alesina, A., Seror, M., Yang, D., You, Y., and Zeng, W. (2020). Persistence through Revolutions. NBER Working Paper 27053.

Angrist, J. D., Imbens, G. W., and Rubin, D. B. (1996). Identification of causal effects using instrumental variables. *Journal of the American statistical Association*, 91(434):444–455.

Avelino, G., Brown, D. S., and Hunter, W. (2005). The Effects of Capital Mobility, Trade Openness, and Democracy on Social Spending in Latin America, 1980-1999. *American Journal of Political Science*, 49(3):625–641.

Barro, R. J. (1996). Democracy and Growth. *Journal of Economic Growth*, 1(1):1–27.

Baum, M. A. and Lake, D. A. (2003). The Political Economy of Growth: Democracy and Human Capital. *American Journal of Political Science*, 47(2):333–347.

Becker, G. (1964). *Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education*. University of Chicago Press.

Becker, G. (1997). Latin America Owes a Lot to Its 'Chicago Boys'. BusinessWeek. June 9, 1997.

Birdsall, N. and Fukuyama, F. (2011). The Post-Washington Consensus: Development After the Crisis. *Foreign Affairs*, 90(2):45–53.

Björklund, A. and Salvanes, K. G. (2011). Education and family background: Mechanisms and policies. In Hanushek, E. A., Machin, S., and Woessmann, L., editors, *Handbook of the Economics of Education*, volume 3, pages 201 – 247. Elsevier.

Black, S. E. and Devereux, P. J. (2011). Recent developments in intergenerational mobility. In Card, D. and Ashenfelter, O., editors, *Handbook of Labor Economics*, volume 4, pages 1487 – 1541. Elsevier.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2005). Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital. *American Economic Review*, 95(1):437–449.

Bourguignon, F. and Verdier, T. (2000). Oligarchy, Democracy, Inequality and Growth. *Journal of Development Economics*, 62(2):285 – 313.

Brunner, J. J. (1984). Informe Sobre el Desarrollo y el Estado Actual del Sistema Universitario en Chile. Programa Flacso-Santiago de Chile, Documento de Trabajo 227.

Brunner, J. J. (1987). Educación Superior en Chile: Entre el Estado, el Mercado y los Intereses Académicos. Programa Flacso-Santiago de Chile, Documento de Trabajo 357.

Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *Review of Economics and Statistics*, 90(3):414–427.

Cameron, A. C. and Miller, D. L. (2015). A practitioners guide to cluster-robust inference. *Journal of human resources*, 50(2):317–372.

Cantoni, D., Chen, Y., Yang, D. Y., Yuchtman, N., and Zhang, Y. J. (2017). Curriculum and Ideology. *Journal of Political Economy*, 125(2):338–392.

Cantoni, D. and Yuchtman, N. (2013). The Political Economy of Educational Content and Development: Lessons from History. *Journal of Development Economics*, 104:233 – 244.

Card, D. (1999). The Causal Effect of Education on Earnings. In Ashenfelter, O. C. and Card, D., editors, *Handbook of Labor Economics*, volume 3, pages 1801 – 1863. Elsevier.

Card, D., Lee, D. S., Pei, Z., and Weber, A. (2015). Inference on Causal Effects in a Generalized Regression Kink Design. *Econometrica*, 83(6):2453–2483.

Card, D. and Lemieux, T. (2001). Can Falling Supply Explain the Rising Return to College for Younger Men? A Cohort-based Analysis. *Quarterly Journal of Economics*, 116(2):705–746.

Card, D. and Yakovlev, E. (2014). The Causal Effect of Serving in the Army on Health: Evidence from Regression Kink Design and Russian Data. Working Paper.

Carneiro, P., Heckman, J. J., and Vytlacil, E. J. (2011). Estimating Marginal Returns to Education. *American Economic Review*, 101(6):2754–81.

Castro, P. (1977). *La Educacion en Chile de Frei a Pinochet*. Ediciones Sigueme, Salamanca.

CEP (1992). *El Ladrillo: Bases de la Política Económica del Gobierno Militar Chileno*. Centro de Estudios Públicos.

Chetty, R., Friedman, J. N., Saez, E., Turner, N., and Yagan, D. (2020). Income Segregation and Intergenerational Mobility Across Colleges in the United States. *Quarterly Journal of Economics*, 135(3):1567–1633.

Comisión Rettig (1996). *Informe de la Comisión Nacional de Verdad y Reconciliación*. Chile: Ministerio del Interior, Corporación Nacional de Reparación y Reconciliación.

Comisión Valech (2004). *Informe de la Comisión Nacional Sobre Prisión Política Y Tortura*. Chile: Ministerio del Interior, Comisión Nacional sobre Prisión Política y Tortura.

Conley, T., Hansen, C., and Rossi, P. (2012). Plausibly Exogenous. *Review of Economics and Statistics*, 94(1):260–272.

Currie, J. and Moretti, E. (2003). Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings. *Quarterly Journal of Economics*, 118(4):1495–1532.

Díaz, E. and Himmel, E. (1985). Evolución histórica del sistema de selección a las universidades chilenas 1967-1984. Serie Documentos de Trabajo CPU 5/85.

Easterly, W. (2013). *The Tyranny of Experts: Economists, Dictators, and the Forgotten Rights of the Poor*. Basic Books.

Echeverría, R. (1980). La Política Educacional y la Transformación del Sistema de Educación en Chile a Partir de 1973. The Wilson Center, Latin American Program, Working Paper 74.

Echeverría, R. (1982). Evolución de la matrícula en Chile: 1935-1981. Santiago, Programa Interdisciplinario de Investigaciones en Educación.

Ferman, B., Pinto, C., and Possebom, V. (2019). Cherry picking with synthetic controls. *Journal of Policy Analysis and Management*.

Firpo, S. and Possebom, V. (2018). Synthetic control method: Inference, sensitivity analysis and confidence sets. *Journal of Causal Inference*, 6(2).

Friedman, M. and Friedman, R. (1980). *Free to Choose: A Personal Statement*. Harcourt Inc.

Gallego, F. A. (2010). Historical Origins of Schooling: The Role of Democracy and Political Decentralization. *Review of Economics and Statistics*, 92(2):228–243.

Glaeser, E. L., La Porta, R., Lopez-De-Silanes, F., and Shleifer, A. (2004). Do Institutions Cause Growth? *Journal of Economic Growth*, 9(3):271–303.

Glaeser, E. L., Ponzetto, G. A. M., and Shleifer, A. (2007). Why Does Democracy Need Education? *Journal of Economic Growth*, 12(2):77–99.

Goldin, C. (2006). The Quiet Revolution That Transformed Women's Employment, Education, and Family. *American Economic Review*, 96(2):1–21.

Guriev, S. and Treisman, D. (2020). A Theory of Informational Autocracy. *Journal of Public Economics*, 186:104158.

Harding, R. and Stasavage, D. (2013). What Democracy Does (and Doesn't Do) for Basic Services: School Fees, School Inputs, and African Elections. *Journal of Politics*, 76(1):229–245.

Heckman, J. J. (2006). Skill Formation and the Economics of Investing in Disadvantaged Children. *Science*, 312(5782):1900–1902.

Holmlund, H., Lindahl, M., and Plug, E. (2011). The Causal Effect of Parents' Schooling on Children's Schooling: A Comparison of Estimation Methods. *Journal of Economic Literature*, 49(3):615–51.

INE (1965). XIII Censo de Población 1960: resumen país. Instituto Nacional de Estadística, Dirección de Estadísticas y Censos.

Kim, H.-A. (2011). State Building: The Military Juntas Path to Modernity through Administrative Reforms. In Kim, B.-K. and Vogel, E. F., editors, *The Park Chung Hee Era*, pages 85–112. Harvard University Press.

Levy, D. (1986). Chilean universities under the junta: Regime and policy. *Latin American Research Review*, 21(3).

Li, H. and Meng, L. (2018). Scarring Effects of Deprived College Education during China's Cultural Revolution. Working Paper.

Maurin, E. and McNally, S. (2008). Vive la Révolution! LongTerm Educational Returns of 1968 to the Angry Students. *Journal of Labor Economics*, 26(1):1–33.

Oreopoulos, P. (2006). Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter. *American Economic Review*, 96(1):152–175.

Oreopoulos, P., Page, M. E., and Stevens, A. H. (2006). The Intergenerational Effects of Compulsory Schooling. *Journal of Labor Economics*, 24(4):729–760.

Oreopoulos, P. and Salvanes, K. G. (2011). Priceless: The Nonpecuniary Benefits of Schooling. *Journal of Economic Perspectives*, 25(1):159–84.

Paglayan, A. (2018). Democracy and Educational Expansion: Evidence from 200 Years. Working Paper.

Persson, T. and Tabellini, G. (1994). Is Inequality Harmful for Growth? *American Economic Review*, 84(3):600–621.

PIIE (1984). *Las Transformaciones Educacionales Bajo el Régimen Militar, Vols. I y II*. Programa Interdisciplinario de Investigaciones en Educación.

Posner, R. (2010). The Becker-Posner Blog: Autocracy, Democracy and Economic Welfare. `https://tinyurl.com/y5evx7pu`. [Accessed on 08/17/2020].

Przeworski, A., Alvarez, M. E., Cheibub, J. A., and Limongi, F. (2000). *Democracy and Development: Political Institutions and Well-Being in the World, 19501990*. Cambridge University Press.

Psacharopoulos, G. and Patrinos, H. A. (2018). Returns to Investment in Education: A Decennial Review of the Global Literature. *Education Economics*, 26(5):445–458.

Roland, G. and Yang, D. (2017). China's Lost Generation: Changes in Beliefs and their Intergenerational Transmission. NBER Working Paper 23441.

Schiefelbein, E. (1976). Diagnóstico del Sistema Educacional Chileno en 1970. Universidad de Chile, Publicación del Departamento de Economía 31.

Schiefelbein, E. and Farrell, J. P. (1984). Education and Occupational Attainment in Chile: The Effects of Educational Quality. *American Journal of Education*, 92(2).

Stasavage, D. (2005). Democracy and Education Spending in Africa. *American Journal of Political Science*, 49(2):343–358.

Suhonen, T. and Karhunen, H. (2019). The Intergenerational Effects of Parental Higher Education: Evidence from Changes in University Accessibility. *Journal of Public Economics*, 176:195 – 217.

Treisman, D. (2020). Economic Development and Democracy: Predispositions and Triggers. *Annual Review of Political Science*, 23(1):241–257.

Universidad de Chile (2011). Compendio Estadístico - Proceso de Admisión Año Académico 2011. Vicerectoría de Asuntos Académicos.

Valdés, J. G. (1995). *Pinochet's Economists: The Chicago School in Chile*. Cambridge University Press.

Velasco, A. (1994). The State and Economic Policy: Chile 1952-92. In Bosworth, B. P., Dornbusch, R., and Labán, R., editors, *The Chilean Economy: Policy Lessons and Challenges*, pages 379–411. The Brookings Institution.

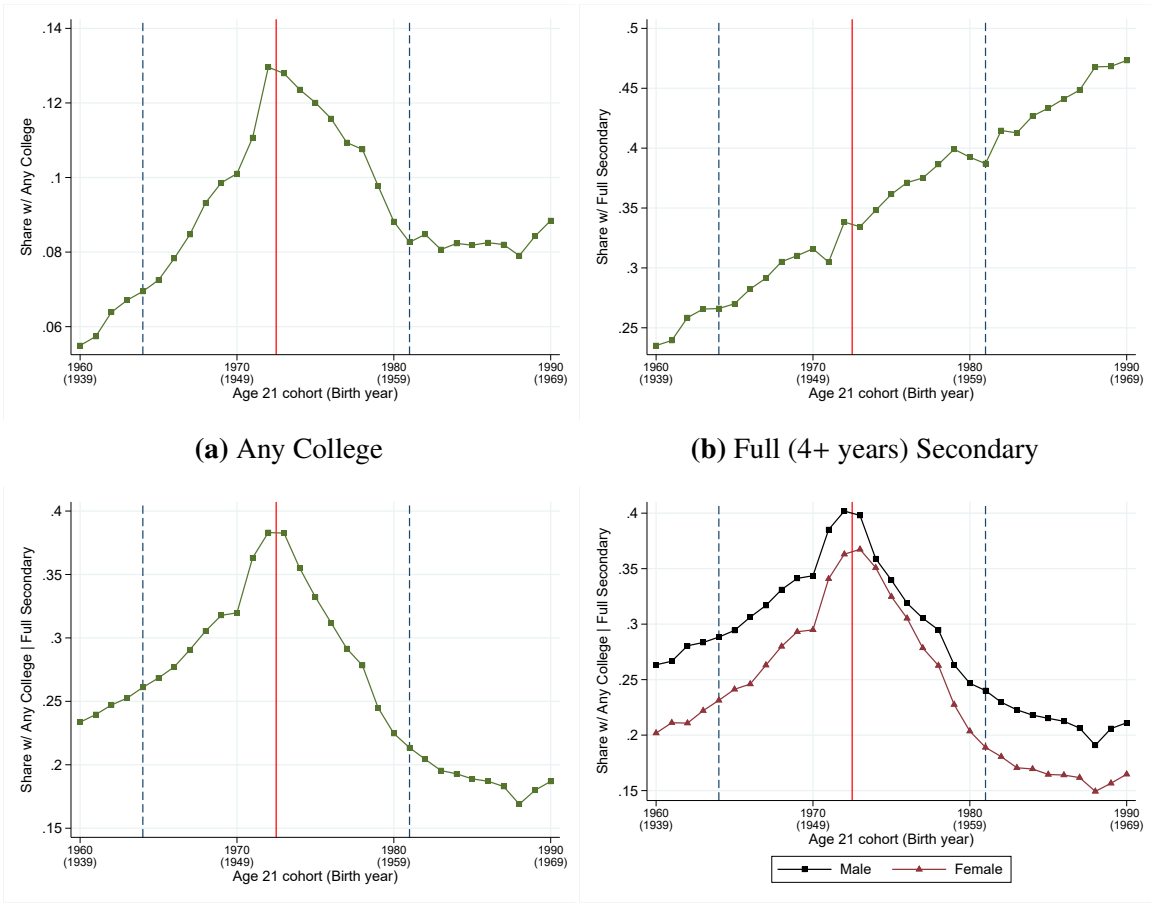
Waldinger, F. (2010). Quality Matters: The Expulsion of Professors and the Consequences for PhD Student Outcomes in Nazi Germany. *Journal of Political Economy*, 118(4):787–831.

Waldinger, F. (2011). Peer Effects in Science: Evidence from the Dismissal of Scientists in Nazi Germany. *Review of Economic Studies*, 79(2):838–861.

Zimmerman, S. D. (2014). The Returns to College Admission for Academically Marginal Students. *Journal of Labor Economics*, 32(4):711–754.

Zimmerman, S. D. (2019). Elite Colleges and Upward Mobility to Top Jobs and Top Incomes. *American Economic Review*, 109(1):1–47.

Figure 1: The Military Coup and College Enrollment



(a) Any College

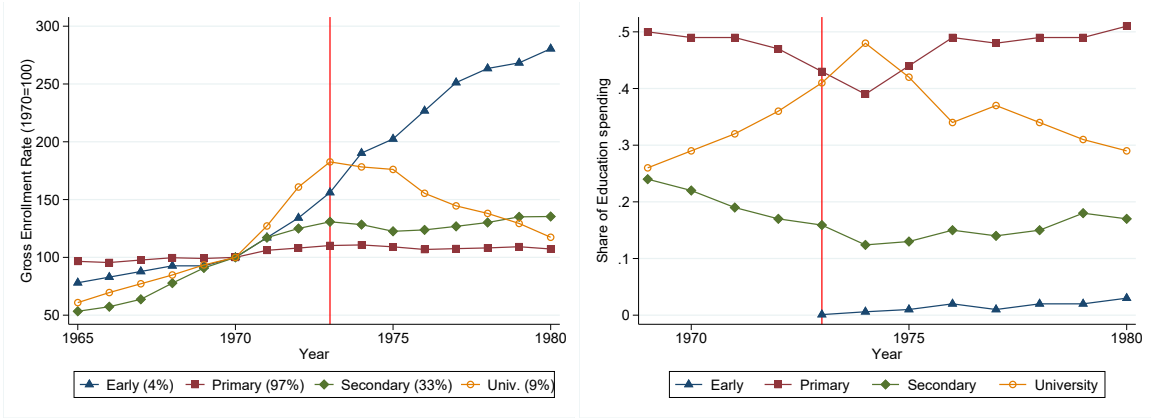
(b) Full (4+ years) Secondary

(c) Any College | Full Secondary

(d) Any College | Full Secondary (gender)

Notes: Panels (a) and (b) show the respective shares of people per cohort (normalized to age 21) in the 1992 census that report any college or 4+ years of secondary. Panel (c) shows the share of people with any college per cohort, among those with 4+ years of secondary. Panel (d) provides the same information disaggregated by gender. The solid red 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.

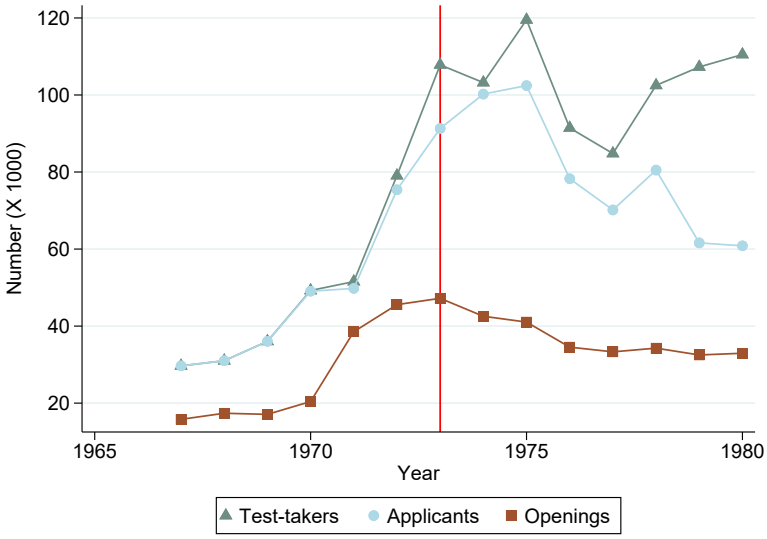
Figure 2: Educational Spending and Enrollment



(a) Gross Enrollment Rate (Index) **(b)** Share of Education Budget

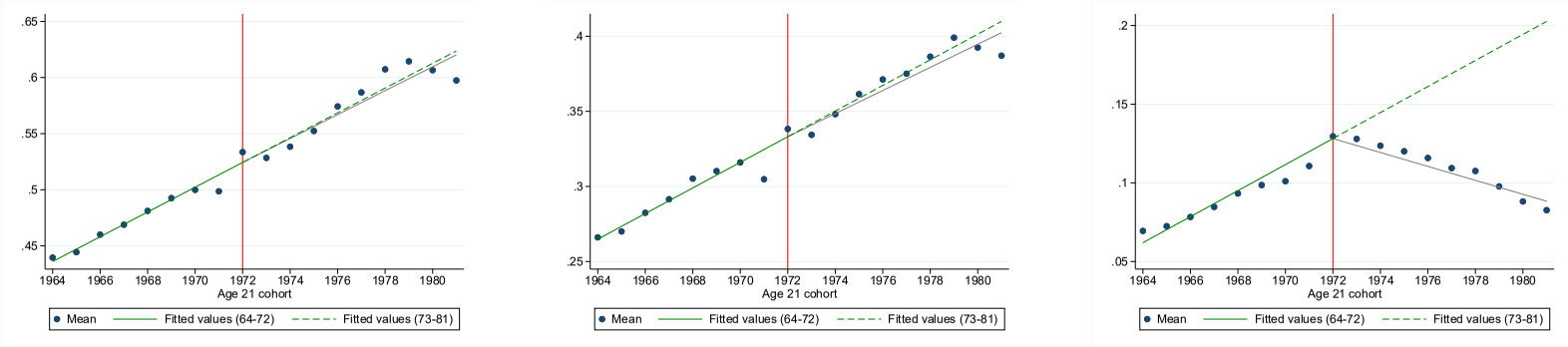
Notes: Panel (a) shows indices for the gross enrollment rates in each level. The respective denominators are population in the 0-5,6-14-15-19,20-24 age groups. Enrollment rates have been normalized to 100 in 1970, number in parenthesis corresponds to enrollment rate in that year. Panel (b) shows the share of public spending on education devoted to early, primary, secondary and higher education. Source: PIIE (1984).

Figure 3: College Applicants and Openings



Notes: Figure shows the yearly number of people that took the PAA test, college applicants, and college openings for incoming students. Source: Universidad de Chile (2011).

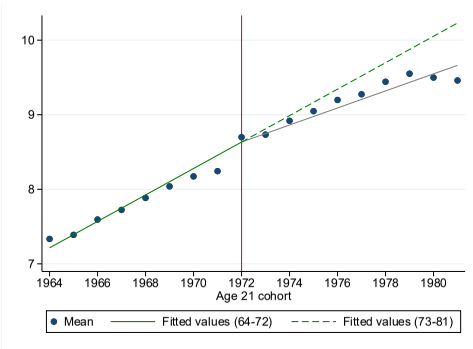
Figure 4: Visualization of kink: Education



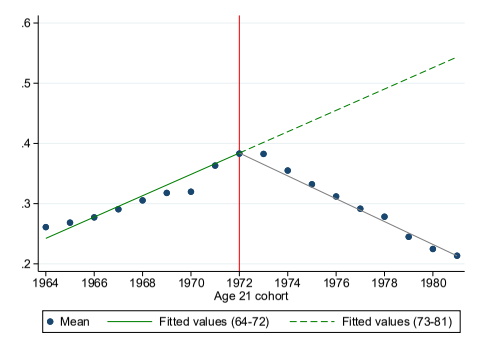
(a) Any Secondary Education

(b) Full Secondary Education

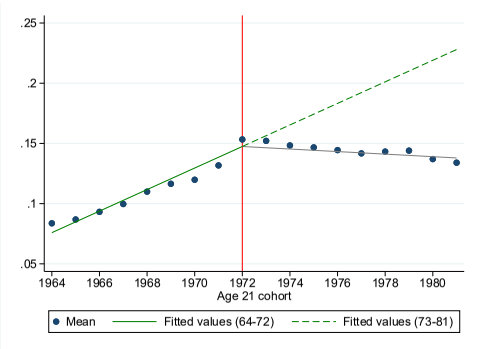
(c) Any College



(d) Years of Education



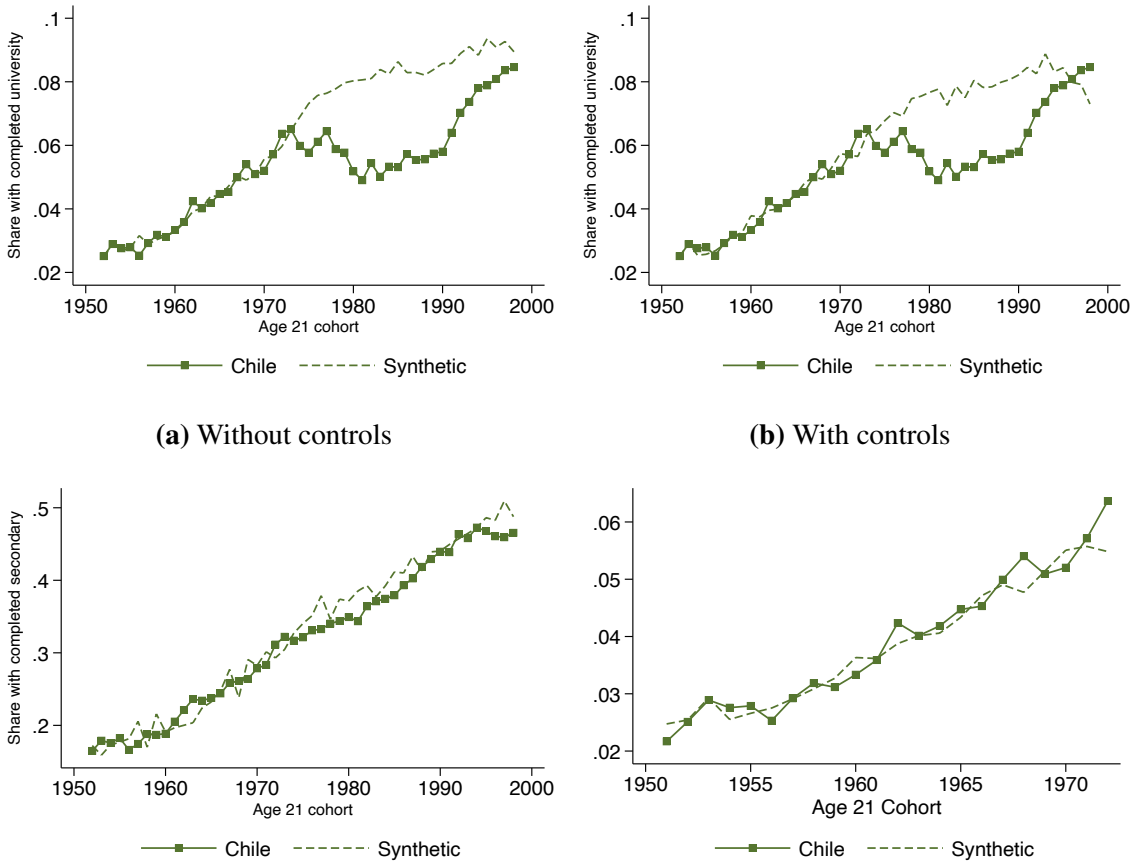
(e) Any College | Full Secondary



(f) Any Higher Education

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.

Figure 5: Synthetic control



(a) Without controls

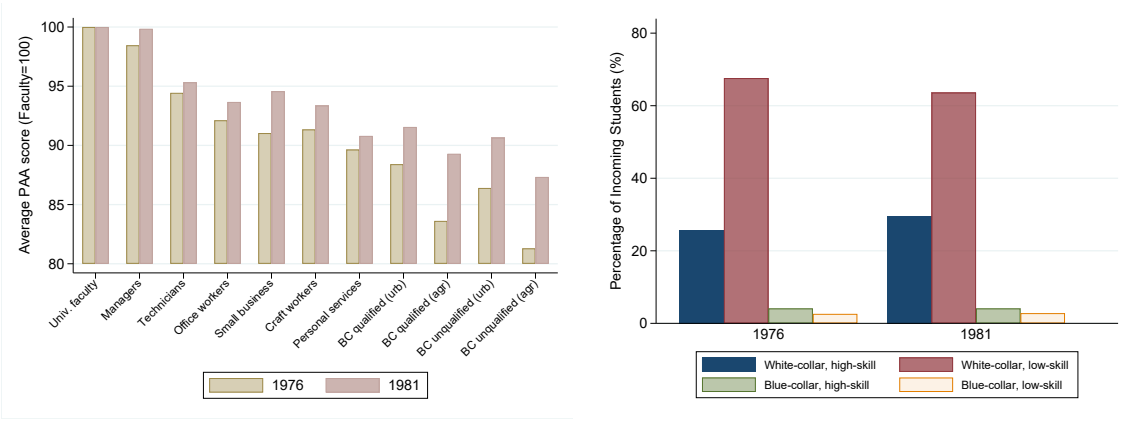
(b) With controls

(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.

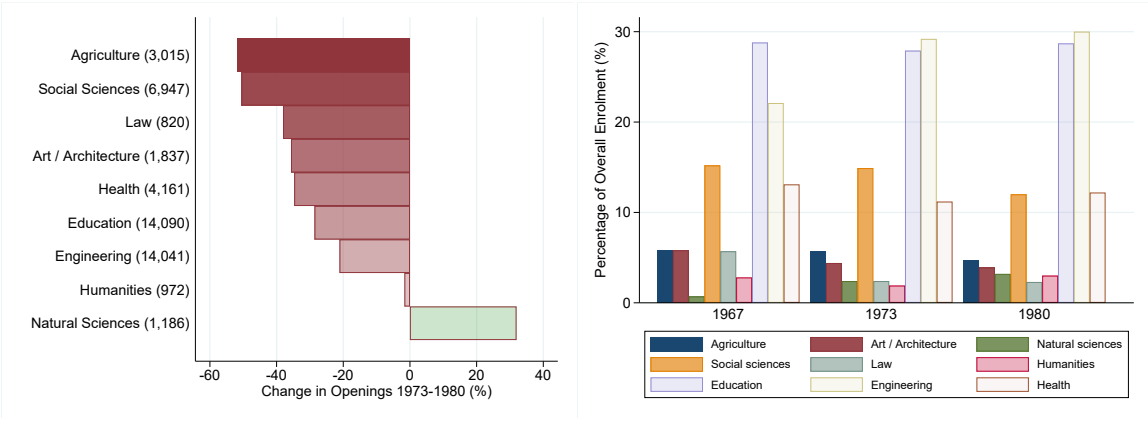
Figure 6: Socioeconomic Background of Admitted Students (1976 and 1981)



(a) Average PAA Test Scores **(b)** Enrollment Shares

Notes: Panel (a) shows the average PAA test scores for admitted college students in 1976 and 1981, classified according to the father's occupation. In both years, the maximum corresponds to children of university faculty, which we have normalized to 100. Panel (b) shows the occupation of the father for admitted students in these same years. Source: PIIE (1984).

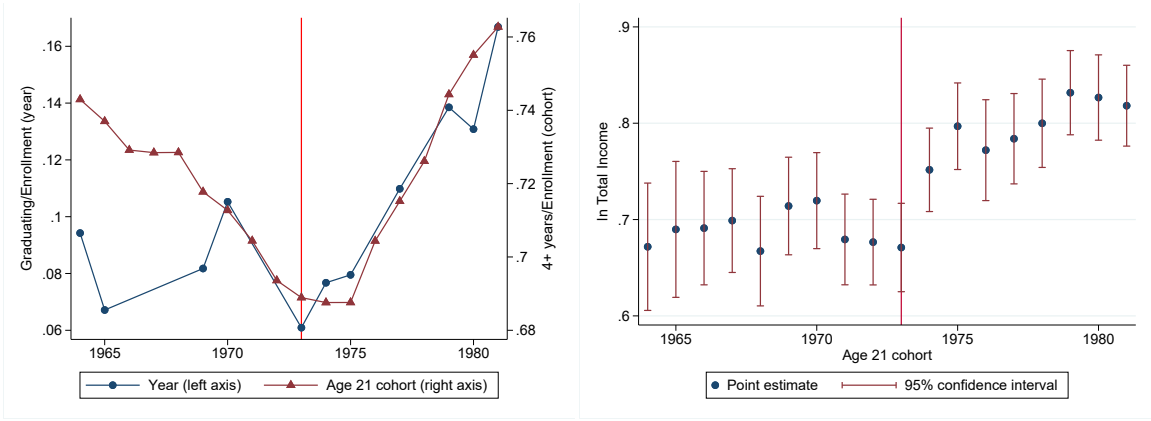
Figure 7: College Openings and Enrollment by Field



(a) Change in Openings by Field (1973-1980) **(b)** Enrollment by Field

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. Sources: PIIE (1984); Brunner (1984).

Figure 8: Post-Enrollment Outcomes

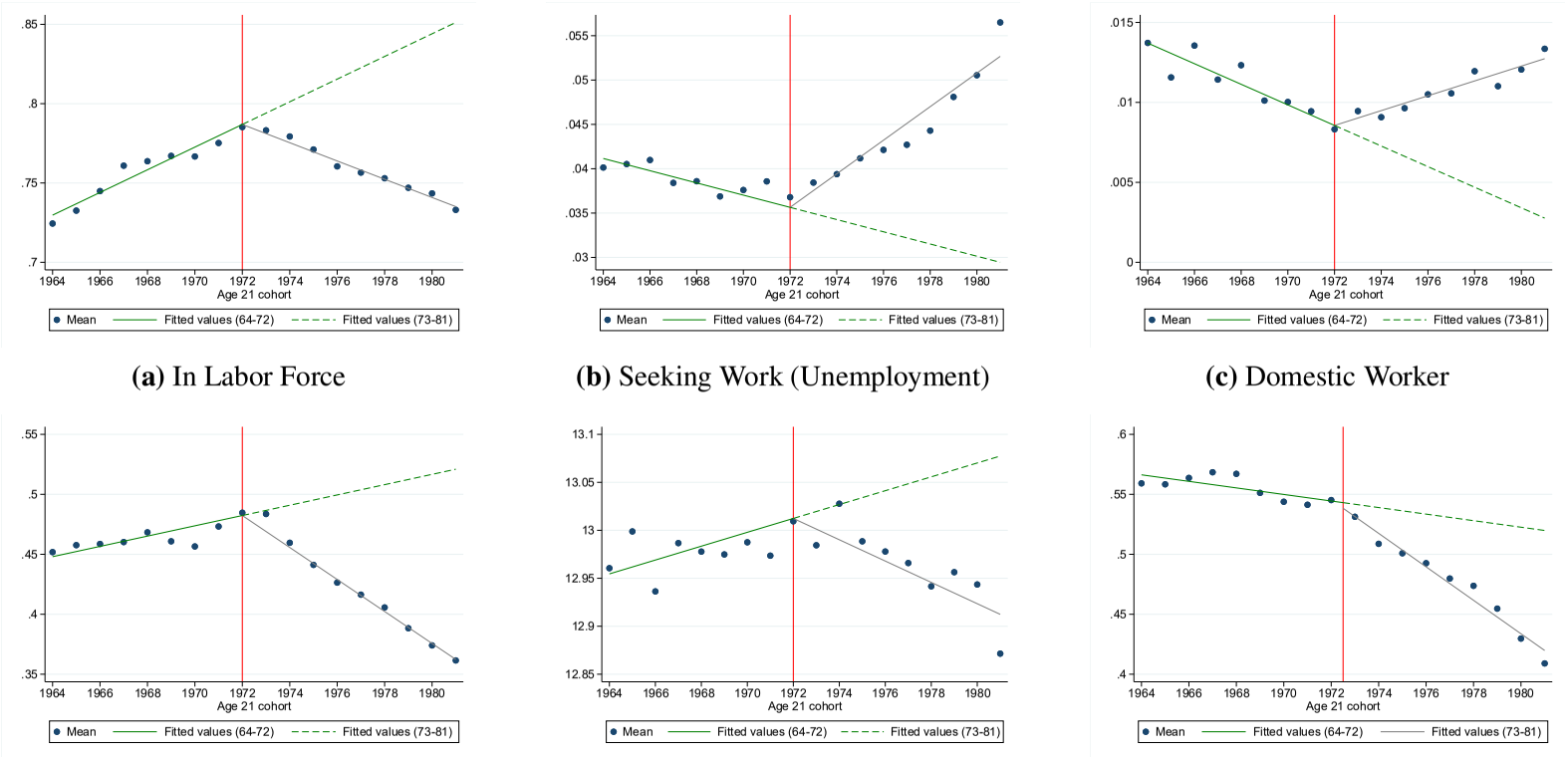


(a) Graduation Rate

(b) Returns to Any College (OLS)

Notes: Panel (a) shows the college graduation rate. Circle markers (left axis) correspond to the number of graduating students as a share of the total number of students per year, based on the UNESCO statistical yearbooks. Triangle markers correspond to the number of people in the 1992 census that report 4+ years of college as a share of the number of people with any college per cohort. Panel (b) shows results from a regression of log real total income on a full set of interactions of a dummy for any college with cohort fixed effects. Sample includes all respondents in the CASEN survey reaching age 21 between 1964 and 1981 and reporting 4+ years of secondary education. Controls include county of residence by gender, survey year and age fixed effects. Standard errors are clustered by county of residence.

Figure 9: Visualization of kink: Labor market and wealth distribution



(a) In Labor Force

(b) Seeking Work (Unemployment)

(c) Domestic Worker

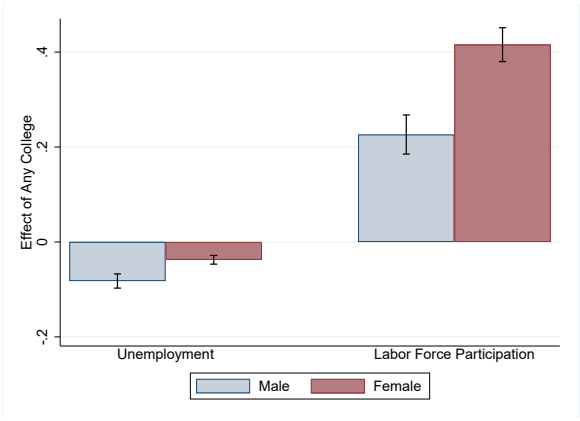
(d) White-collar, high-skilled

(e) Log Total Income

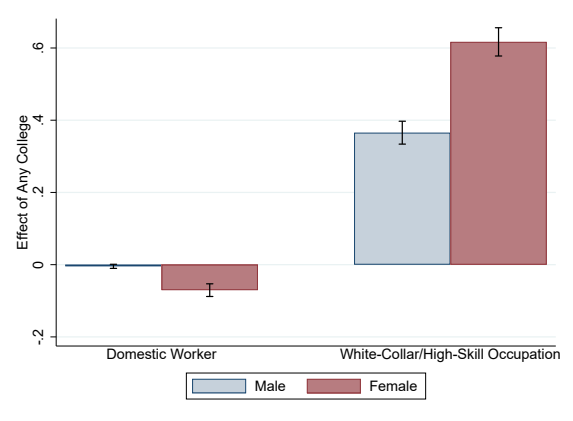
(f) Top wealth quintile

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 e) from CASEN survey, which corresponds to the averages of all generated income across survey years.

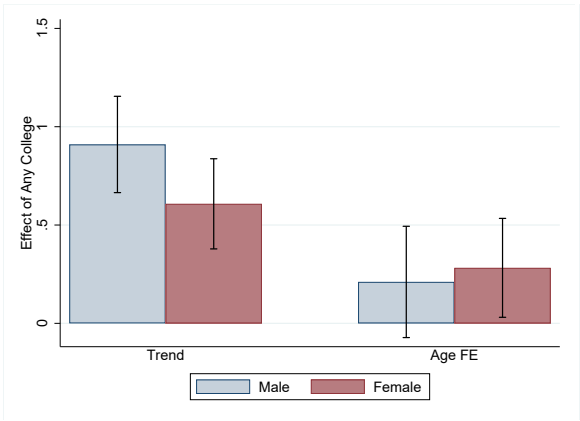
Figure 10: Heterogeneous Labor Market Effects of College Education by Gender



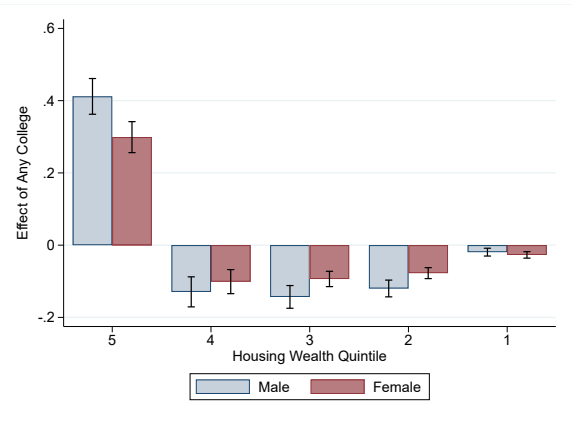
(a) LFP and Unemployment (1992)



(b) Domestic Worker and Occupation (1992)



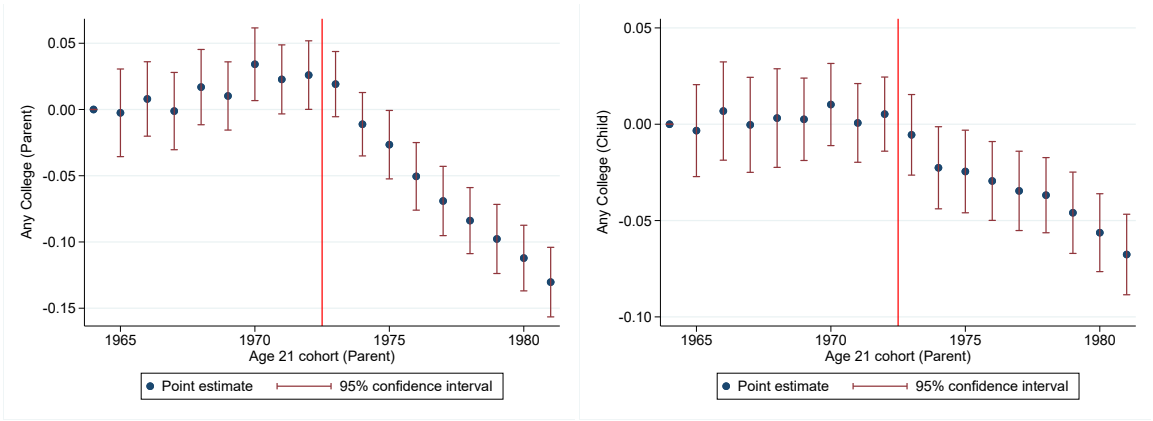
(c) Income (CASEN: 1990-2015)



(d) 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) reaching age 21 between 1964 and 1981 (both inclusive) and 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. Regression for unemployment in Panel (b) includes a dummy for labor force participation (LFP). Panel (c) 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 (county of residence in CASEN). Full results available in the online appendix.

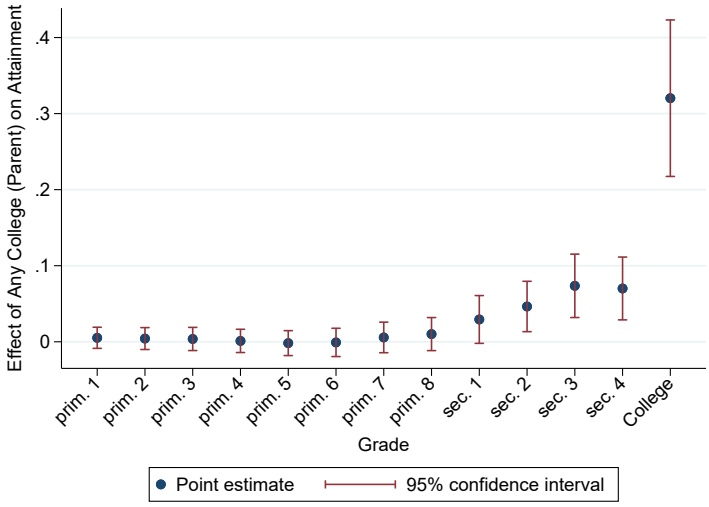
Figure 11: College Enrollment of Linked Parents and Children



(a) Any College (Parent) **(b)** Any College (Child)

Notes: Panel (a) shows results from a regression of parent's college enrollment on parent cohort dummies. Panel (b) uses child's college enrollment as outcome instead. See text for details on sample construction. Controls include county of birth by gender, parent's gender by (child's) gender, age and relationship to household head fixed effects. Standard errors clustered by county of birth.

Figure 12: Effect of Parental College Enrollment on Child's Educational Attainment



Notes: Figure shows IV estimates of the effect of parent's college enrollment on child's completion of each educational level. See text for details on sample construction. Excluded instrument is the interaction of a variable indicating the year at which the parent reached age 21, normalized to zero in 1972, with a dummy equal to one on or after 1973. The baseline trend is an included instrument. Controls include county of birth by gender, parent's gender by (child's) gender, age and relationship to household head fixed effects. Standard errors clustered by county of birth.

Table 1: Educational attainment (Census 1992)

	Any Secondary Education	Completed Secondary Education	Any College Education	Years of Education	Any College Education	Any College Education	Any Tertiary Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
[a] Yr Age 21	0.010***	0.008***	0.008***	0.167***	0.018***	0.020***	0.019***
	(0.0006)	(0.0003)	(0.0004)	(0.0042)	(0.0004)	(0.0021)	(0.0003)
	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.000	-0.001	-0.013***	-0.061***	-0.036***	-0.041***	-0.030***
	(0.0004)	(0.0005)	(0.0007)	(0.0040)	(0.0007)	(0.0030)	(0.0006)
	[0.940]	[0.742]	[0.000]	[0.056]	[0.000]	[0.000]	[0.000]
Sample	Full	Full	Full	Full	Completed Secondary	Siblings w/ Completed Secondary	Completed Secondary
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family FE	No	No	No	No	No	Yes	No
Mean of dependent variable	0.540	0.343	0.101	8.708	0.295	0.293	0.379
Observations	2,982,951	2,982,951	2,982,951	2,982,951	1,024,570	42,649	1,024,570
R-squared	0.119	0.088	0.046	0.149	0.040	0.647	0.034
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Dependent variable in the header. Sample includes all census respondents from cohorts born between 1943 and 1960 (both inclusive).“Yr Age 21” is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. “Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)” is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap procedure for clustering at the cohort level in brackets (see Cameron and Miller (2015) for details). *** p<0.01, ** p<0.05, * p<0.1

Table 2: Labor market outcomes

	Census 1992				CASEN (1990-2015)	
	In Labor Force	Seeking Work	Domestic Worker	High-skill, white-collar	Log Total Income	
	(1)	(2)	(3)	(4)	(5)	(6)
[a] Yr Age 21	0.008***	-0.001***	-0.001***	0.004***	0.011***	-
	(0.0003)	(0.0001)	(0.0001)	(0.0005)	(0.002)	-
	[0.000]	[0.004]	[0.000]	[0.003]	[0.002]	
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.012***	0.002***	0.001***	-0.017***	-0.020***	-0.006**
	(0.0006)	(0.0001)	(0.0002)	(0.0008)	(0.002)	(0.003)
	[0.000]	[0.003]	[0.000]	[0.000]	[0.000]	[0.118]
IV: Any College	0.333***	-0.060***	-0.034***	0.476***	0.761***	0.232**
	(0.017)	(0.004)	(0.005)	(0.015)	(0.090)	(0.099)
	[0.002]	[0.003]	[0.000]	[0.000]	[0.001]	[0.078]
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
In labor force FE	No	Yes	No	No	-	-
Survey year FE	-	-	-	-	Yes	Yes
Age FE	-	-	-	-	No	Yes
Mean of dependent variable	0.758	0.033	0.011	0.431	740,530	740,530
Observations	1,024,570	1,024,570	773,922	770,652	118,301	118,301
R-squared (Reduc. Form)	0.200	0.013	0.024	0.032	0.153	0.160
p-value a+b=0	0.000	0.000	0.000	0.000	0.000	-
First-stage KP F-stat	2733.6	2623.9	2120.2	2094.1	422.3	391.6

Notes: Source of data and dependent variable in the headers. Sample includes individuals born between 1943 and 1960 (both inclusive) that report 4+ years of secondary. Real income in columns 5-6 deflated using yearly CPI. "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). Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap procedure for clustering at the cohort level in brackets (see Cameron and Miller (2015) for details). *** p<0.01, ** p<0.05, * p<0.1

Table 3: Housing wealth quintiles (Census 1992)

	Wealth quintile (dummy)				
	Q5 (highest)	Q4	Q3	Q2	Q1 (lowest)
	(1)	(2)	(3)	(4)	(5)
[a] Yr Age 21	-0.002***	-0.000	0.001***	0.001***	0.000***
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0001)
	[0.015]	[0.378]	[0.024]	[0.021]	[0.001]
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.013***	0.004***	0.004***	0.004***	0.001***
	(0.0007)	(0.0006)	(0.0004)	(0.0003)	(0.0001)
	[0.001]	[0.000]	[0.003]	[0.000]	[0.003]
IV: Any College	0.348***	-0.113***	-0.115***	-0.097***	-0.024***
	(0.021)	(0.017)	(0.010)	(0.008)	(0.004)
	[0.003]	[0.001]	[0.004]	[0.002]	[0.001]
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0.50	0.25	0.15	0.08	0.02
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

Notes: Dependent variable in the header. Sample includes all respondents in the 1992 census born between 1943 and 1960 (both inclusive) that report 4+ 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). Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap procedure for clustering at the cohort level in brackets (see Cameron and Miller (2015) for details). *** p<0.01, ** p<0.05, * p<0.1

Table 4: Educational attainment of children (Census 2017)

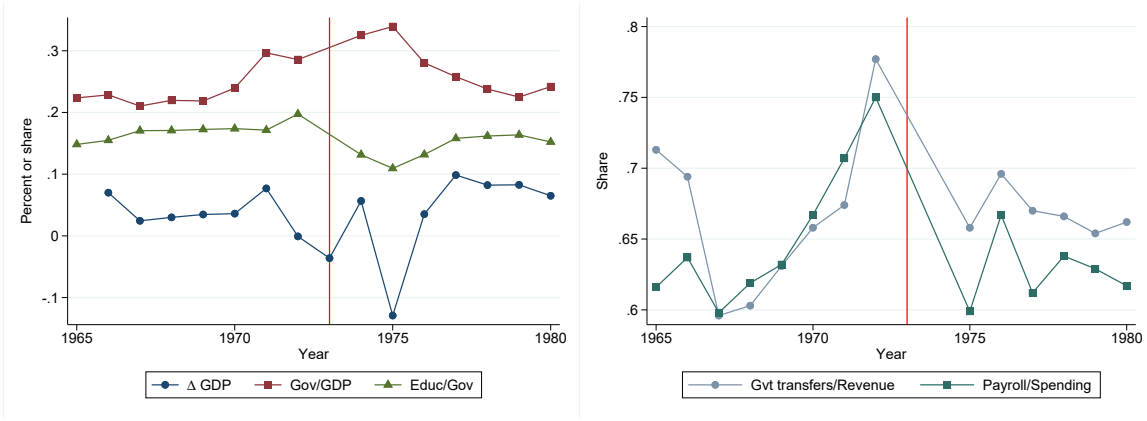
Panel A: First stage	Dependent variable: Any College (Parent)					
	(1)	(2)	(3)	(4)	(5)	(6)
[a] Yr Age 21 Parent	0.006***	0.006***	0.006***	0.006***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.013]	[0.014]
[b] Yr Age 21 Parent	-0.021***	-0.021***	-0.021***	-0.020***	-0.022***	-0.021***
x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: Reduced form	Dependent variable: Any College (Children)					
	(1)	(2)	(3)	(4)	(5)	(6)
[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)
	[0.012]	[0.007]	[0.007]	[0.008]	[0.760]	[0.442]
[b] Yr Age 21 Parent	-0.005***	-0.005***	-0.005***	-0.005***	-0.007***	-0.006***
x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.008]	[0.005]	[0.005]	[0.005]	[0.002]	[0.004]
Panel C: IV	Dependent variable: Any College (Children)					
	(1)	(2)	(3)	(4)	(5)	(6)
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)
	[0.005]	[0.005]	[0.005]	[0.005]	[0.004]	[0.003]
Panel D: OLS	Dependent variable: Any College (Children)					
	(1)	(2)	(3)	(4)	(5)	(6)
Any College (Parent)	0.274***	0.272***	0.272***	0.272***	0.262***	0.243***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
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
Mean of dependent variable (Panel A)	0.309	0.309	0.309	0.309	0.309	0.309
Mean of dependent variable (Panels B-D)	0.582	0.582	0.582	0.582	0.582	0.582
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
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 was born between 1943 and 1960 (both years inclusive) and reported full secondary education. See text for further details on construction of sample. "Yr Age 21 Parent" is a continuous variable indicating the year at which the parent reached age 21, normalized to zero in 1972. "Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)" is the interaction of this variable with a dummy for parents that reached age 21 on or after 1973. In panel C, the interaction term is used as excluded instrument for any college education by the Parent (the trend is an included instrument). Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap procedure for clustering at the cohort level in brackets (see Cameron and Miller (2015) for details). *** p<0.01, ** p<0.05, * p<0.1

APPENDIX (for online publication)

Appendix A Additional background figures

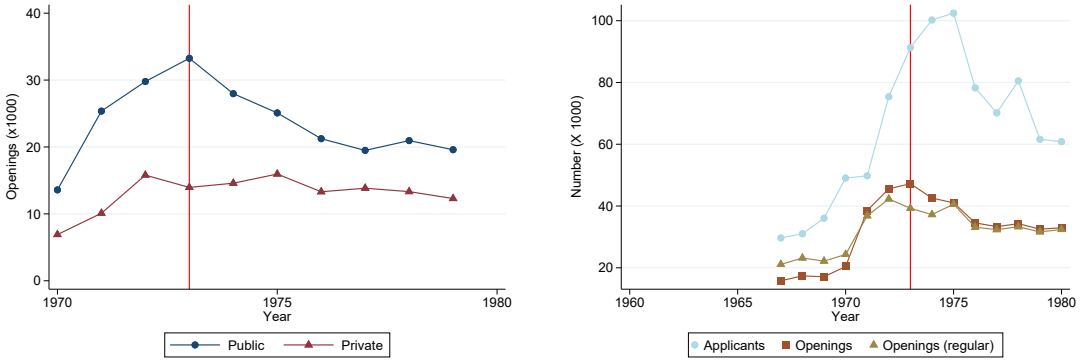
Figure A1: Public Spending on Education



(a) Government Spending **(b)** University Finances

Notes: Panel (a) shows GDP growth, government spending as a share of GDP, and spending on education as a share of government spending. Panel (b) shows fiscal transfers as a share of total revenue in the university system, and payroll as a share of total spending by universities. Source: PIIE (1984).

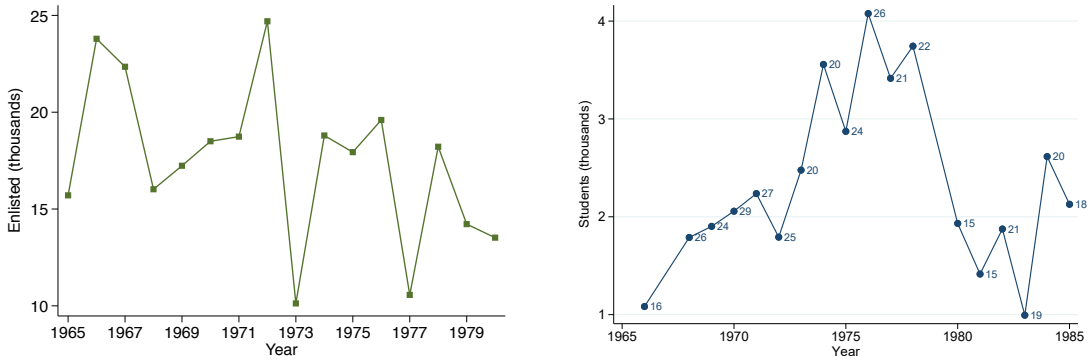
Figure A2: Further Evidence on Supply and Demand for College



(a) Openings: Public and Private **(b)** Alternative measure of Openings

Notes: Panel (a) shows yearly openings in private and public universities. Panel (b) shows the number of applicants and openings per year, but includes an alternative measure of regular openings.

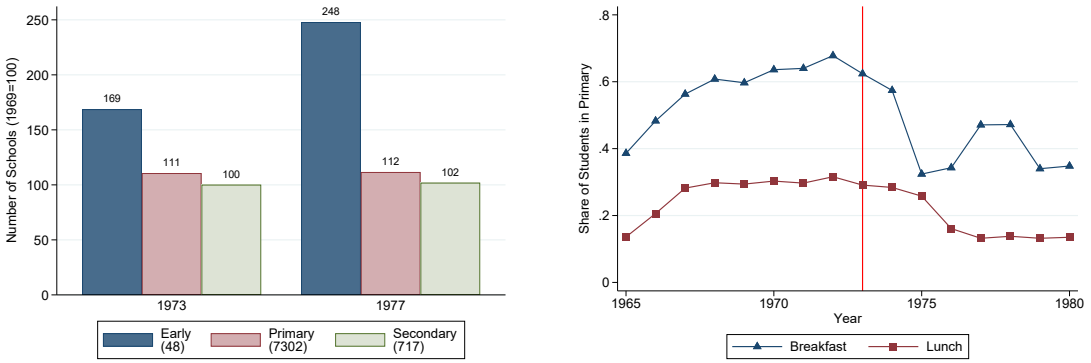
Figure A3: Alternative mechanisms



(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 . Sources: records of conscripts per year were obtained through a Freedom-of-Information request and the number of students abroad from UNESCO statistical yearbooks.

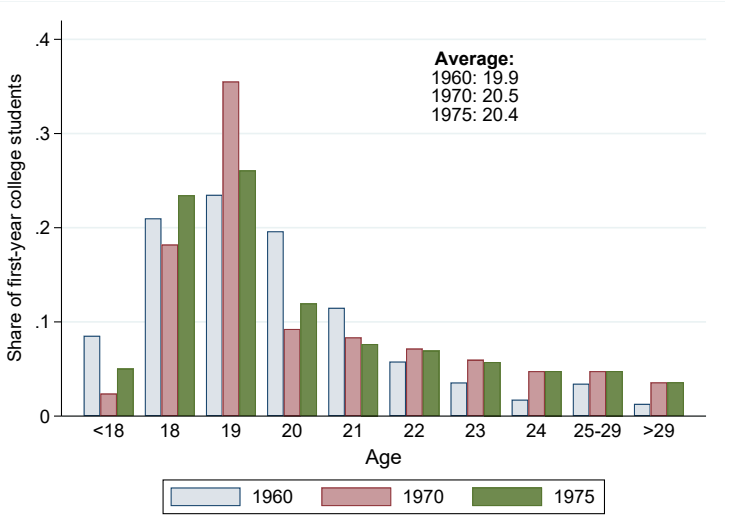
Figure A4: Other outcomes: Lower levels



(a) Number of Schools **(b)** Meals

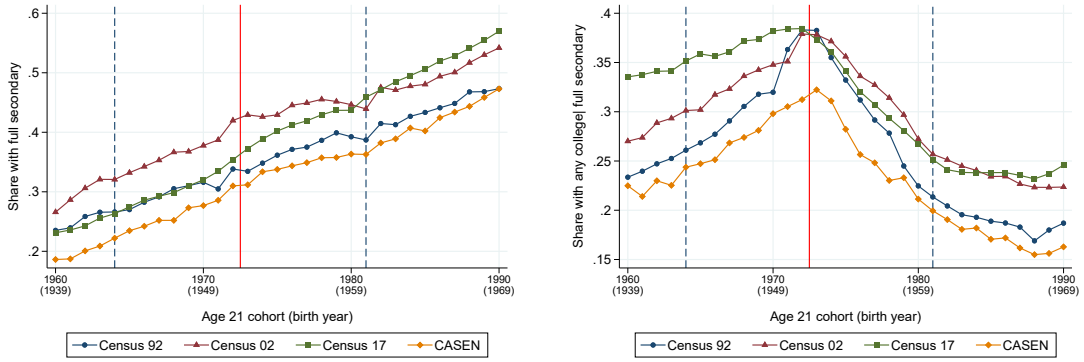
Notes: Panel (a) shows the number of schools per level (early, primary, secondary) in 1973 and 1977, relative to 1969 (normalized to 100). Panel (b) shows the yearly share of primary students receiving either free breakfast (triangle markers) or lunch (square markers). Sources: Echeverría (1980); PIIE (1984).

Figure A5: Age distribution of first-year college students



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 average, we set age at 17, 25 and 30 for the < 18, $25 - 29$ and > 29 age groups respectively, which likely leads to an underestimate.

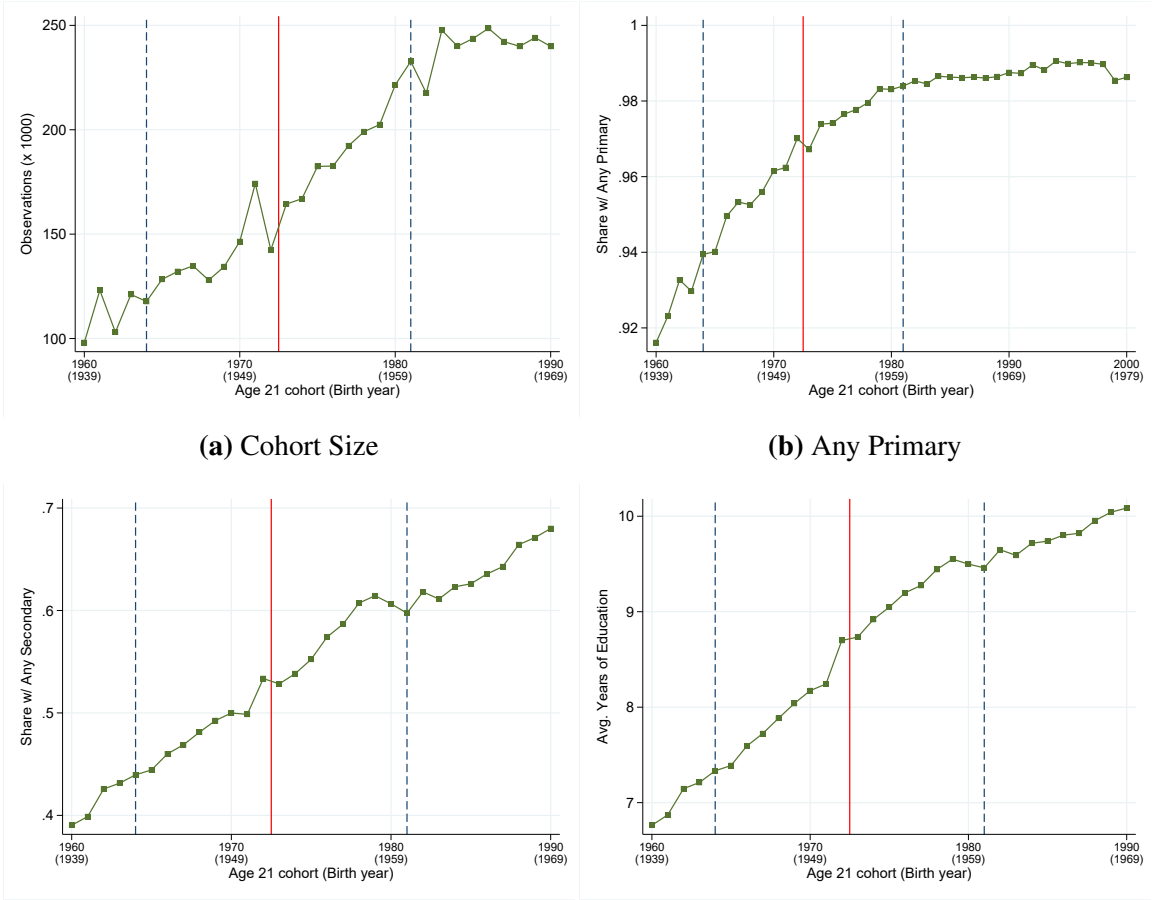
Figure A6: College Enrollment - different sources



(a) Share with 4+ years secondary **(b)** Share with any college | 4+ years secondary

Notes: Panel (a) shows for each source the share of people in each cohort that report at least four years of secondary education. Panel (b) shows the share of people with any college, conditional on having 4+ years of secondary education. The solid red 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 A7: Educational Attainment (Raw data)

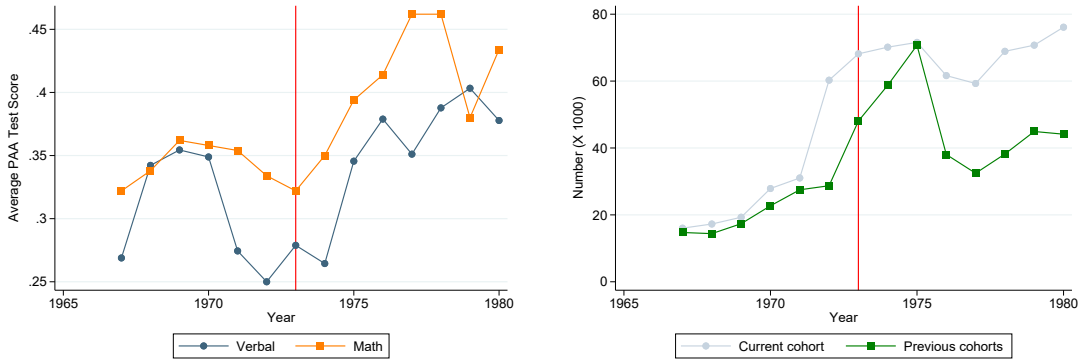


(a) Cohort Size **(b)** Any Primary

(c) Any Secondary **(d)** Years of Education

Notes: Panel (a) shows the total number of people per cohort (normalized to age 21) in the 1992 population census. Panel (b) shows the share of census respondents per cohort that report any primary education. Panel (c) shows the corresponding share that reports any secondary education. Panel (d) shows the average years of education per cohort. The solid red 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.

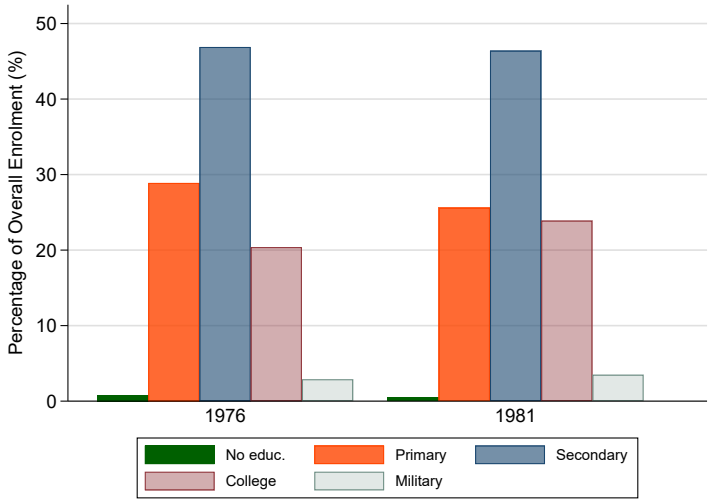
Figure A8: Additional Information on PAA test



(a) Average test scores **(b)** PAA Registration: Old vs Current Cohort

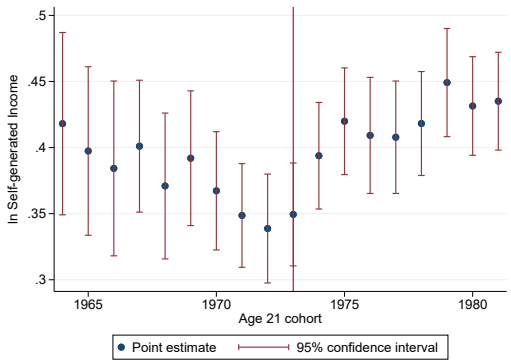
Notes: Panel (a) shows the average (raw) scores in the verbal and math sections of the PAA test. The score is calculated by adding the number of correct answers and subtracting one quarter of the wrong answers. We divide these averages by the number of questions in each section (90 questions in verbal, 50 questions in math) for enhanced comparability. Panel (b) 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. Sources: Díaz and Himmel (1985); Universidad de Chile (2011).

Figure A9: Enrollment Shares by Educational Attainment of Father (1976 and 1981)

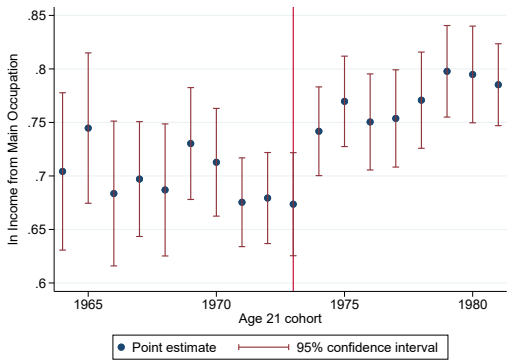


Notes: Figure shows the share of incoming college students in 1976 and 1981, disaggregated by the educational attainment of the father. Source: PIIE (1984).

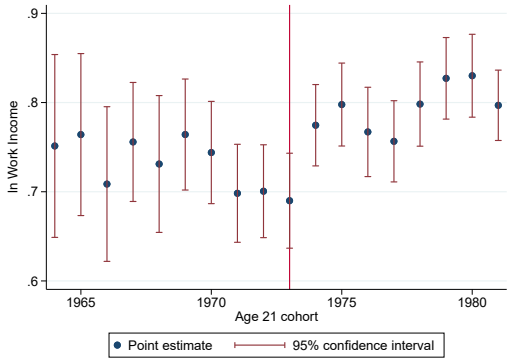
Figure A10: Cohort-specific Estimates of the College Premium



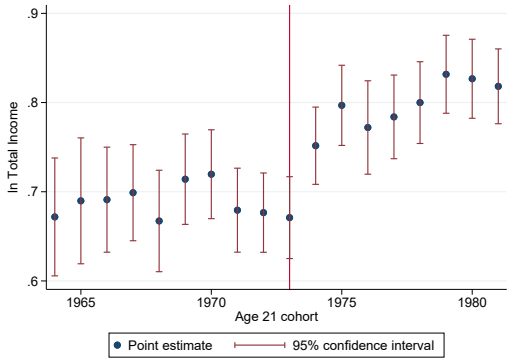
(a) Self-generated (w/ occup. FE)



(b) Primary occupation



(c) Work



(d) Total

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. Panel (a) additionally includes occupation fixed effects. Standard errors are clustered by county of residence.

Appendix B Robustness Checks: Educational Attainment

Table B1: Educational Attainment: Other sources

Source:	Census 2002		Census 2017		CASEN	
	Any College	Any Higher	Any College	Any Higher	Any College	Any Higher
	(1)	(2)	(3)	(4)	(5)	(6)
[a] Yr Age 21	0.012***	0.011***	0.005***	0.005***	0.012***	0.012***
	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0007)	(0.0008)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.025***	-0.013***	-0.019***	-0.016***	-0.024***	-0.019***
	(0.0008)	(0.0007)	(0.0008)	(0.0008)	(0.0012)	(0.0013)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,192,851	1,192,851	962,039	962,039	148,069	148,069
R-squared	0.035	0.030	0.041	0.038	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	0.325	0.452	0.322	0.440	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 $\geq$ 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 B2: 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	0.024***	0.021***	0.018***	0.015***	0.011***
	(0.0006)	(0.0005)	(0.0004)	(0.0004)	(0.0003)
[b] Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ x)	-0.038***	-0.037***	-0.036***	-0.036***	-0.035***
	(0.0008)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes
Observations	1,024,570	1,024,570	1,024,570	1,024,570	1,024,570
R-squared	0.0373	0.0386	0.0396	0.0398	0.0393
p-value a+b=0	0.000	0.000	0.000	0.000	0.000
Mean of dependent variable	0.295	0.295	0.295	0.295	0.295

Notes: Dependent variable in the header. Sample includes all respondents of 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 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 B3: College enrollment: Within household (Census 1992, 2002 and 2017)

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

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

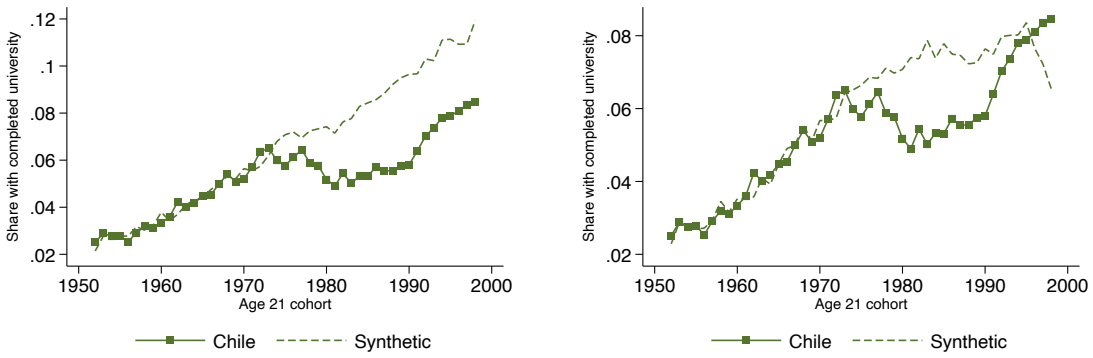
Table B4: College enrollment (Within quintile - 1992)

	Dependent variable: Any college				
Sample (Housing wealth quintile):	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 $\geq$ 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 $\geq$ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of birth x gender and household fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix C Additional Results on Synthetic control

Figure C1: Robustness of synthetic control analysis



(a) Countries without dictatorship **(b)** All countries

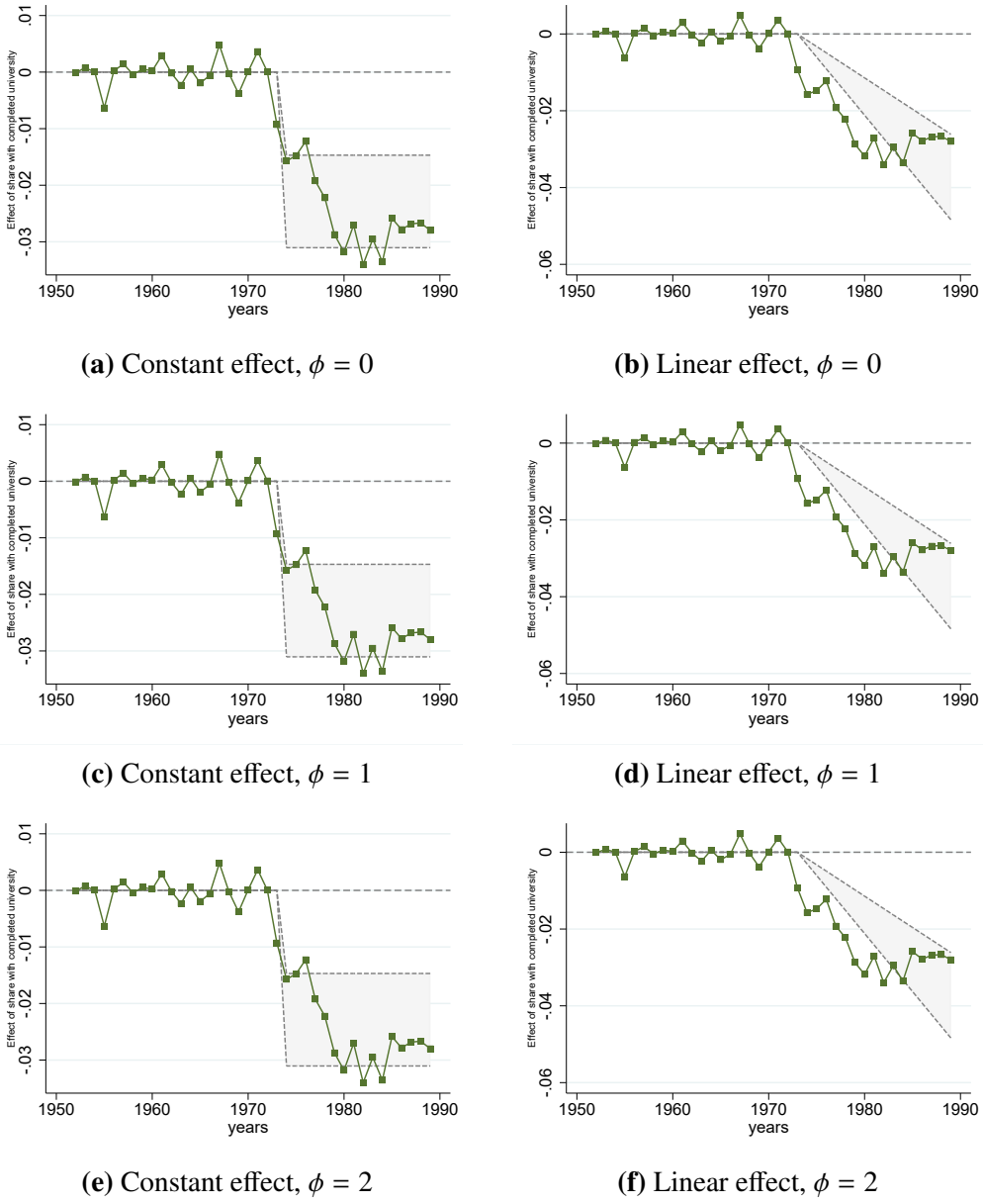
Note: Panel (a) excludes country-year pairs under dictatorship as control units to be potentially used in the synthetic control. Similarly, panel (b) uses all 57 countries with IPUMS data. Both panels use the specification with controls and all countries in the sample of potential controls.

Table C1: 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 matching				
LA without controls	96%	-1.69%	0.00	0.00
LA with controls	94%	-1.27%	0.00	0.00
All countries without controls	96%	-1.23%	0.00	0.00
All countries with controls	95%	-1.02%	0.04	0.04
Exclude dictatorships without controls	88%	-1.11%	0.04	0.04
Exclude dictatorships with controls	94%	-1.20%	0.04	0.04
Panel B: Using all pre-treatment period outcomes for matching				
LA without controls	98%	-1.18%	0.00	0.00
All countries without controls	99%	-0.86%	0.02	0.02
Exclude dictatorships without controls	98%	-0.66%	0.04	0.04

Notes: This table presents the goodness of fit of the matching and the treatment effects for different samples and different sets of matching characteristics. The R^2 comes from a regression between the Chilean data and the synthetic control during the pre-treatment period. The *Average effect* is the average difference between Chile and the synthetic control between 1973 and 1981. 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 (1973-1981) 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, to avoid including as controls countries with a noisy fit.

Figure C2: 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 D Additional results on economic consequences

Table F1: Labor market outcomes (Census 2002)

	In Labor Force	Seeking Work	Domestic Worker	High-skill, white-collar
	(1)	(2)	(3)	(4)
[a] Yr Age 21	0.017***	0.0001	-0.0003***	-0.004***
	(0.0004)	(0.0001)	(0.00009)	(0.0004)
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.014***	0.0004**	0.0005***	-0.006***
	(0.0004)	(0.0002)	(0.00013)	(0.0005)
IV: Any College	0.568***	-0.020**	-0.0193***	0.221***
	(0.021)	(0.008)	(0.00500)	(0.016)
Birth county x gender FE	Yes	Yes	Yes	Yes
In labor force FE	No	Yes	No	No
Observations	1,192,851	1,192,851	907,050	872,783
R-squared (RF)	0.133	0.024	0.027	0.022
p-value a+b=0	0.000	0.000	0.015	0.000
First-stage KP F-stat	1079.8	930.3	761.5	874.9
Mean of dep. var	0.762	0.063	0.0186	0.596

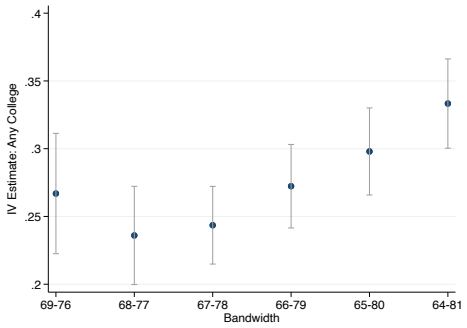
Notes: Dependent variable in the header. Sample includes all census respondents born between 1943 and 1960 (both inclusive) reporting four or more years of secondary. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr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

Table F2: Labor market outcomes (Pooled 1992 and 2002 Censuses)

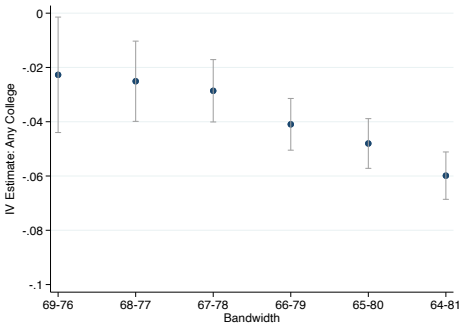
	In Labor Force		Seeking Work (Unemployment)		Domestic Worker		High-skill, white-collar	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[a] Yr Age 21	0.012***		-0.0002*		-0.001***		0.000	
	(0.0002)		(0.0001)		(0.0001)		(0.0003)	
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.013***	-0.006***	0.001***	0.001***	0.001***	0.001***	-0.011***	-0.011***
	(0.0004)	(0.0007)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0005)	(0.0008)
IV: Any College	0.441***	0.199***	-0.043***	-0.042***	-0.028***	-0.028***	0.369***	0.352***
	(0.0156)	(0.0252)	(0.0053)	(0.0071)	(0.0041)	(0.0052)	(0.0123)	(0.0211)
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
Age FE	No	Yes	No	Yes	No	Yes	No	Yes
Mean of dependent variable	0.760	0.760	0.0487	0.0487	0.0151	0.0151	0.519	0.519
Observations	2,217,423	2,217,423	2,217,423	2,217,423	1,680,974	1,680,974	1,643,437	1,643,437
R-squared (Reduc. Form)	0.157	0.160	0.023	0.023	0.025	0.025	0.052	0.052
p-value a+b=0	0.000	-	0.000	-	0.000	-	0.000	-
First-stage KP F-stat	1875.8	1544.1	1694.8	1406.4	1434.0	1127.4	1570.8	1205.4

Notes: Dependent variable in the headers. We consider all 1992 and 2002 census respondents born between 1943 and 1960 (both inclusive) reporting four or more years of secondary. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached 21 years of age, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. In the IV regression, the interaction term is used as excluded instrument for any college education (the trend is an included instrument). All regressions include county of birth x gender fixed effects. Columns 2 and 3 includes labor-force participation fixed effects. Standard errors clustered by county of birth in parentheses. *** p<0.01, ** p<0.05, * p<0.1

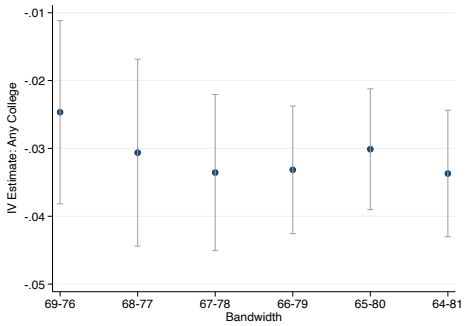
Figure F1: Robustness: Economic Outcomes w/ Different Bandwidths



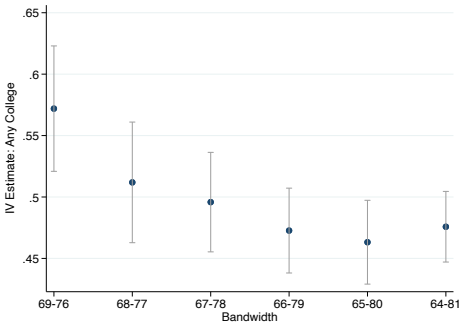
(a) Labor Force Participation



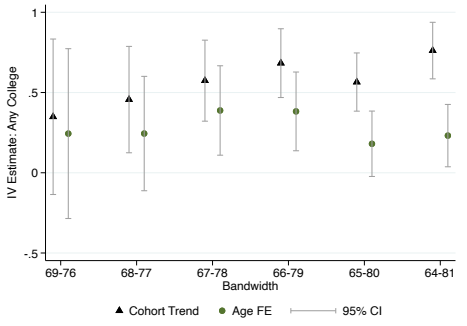
(b) Unemployment



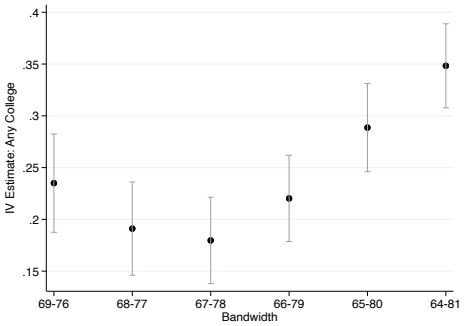
(c) Domestic Worker



(d) White-Collar High-Skill



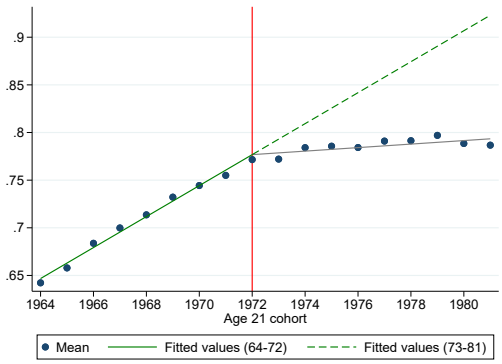
(e) Log Total Income



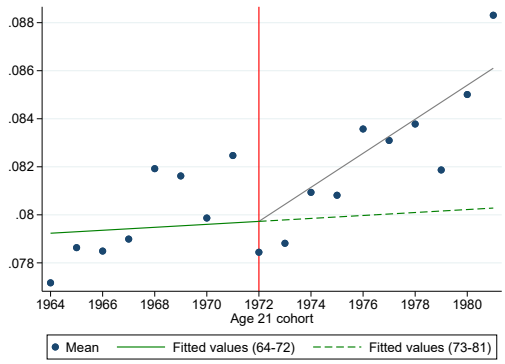
(f) Top quintile

Notes: Each figure replicates the IV analysis for the outcome in the caption for the different bandwidths in the x-axis. Sample includes individuals born between 1943 and 1960 (both inclusive) 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. The latter 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. Panel (b) includes a labor-force participation dummy as well. Panel (e) also includes survey year fixed effects and shows additional estimates from specification with age fixed effects. Standard errors clustered by county of birth in parentheses.

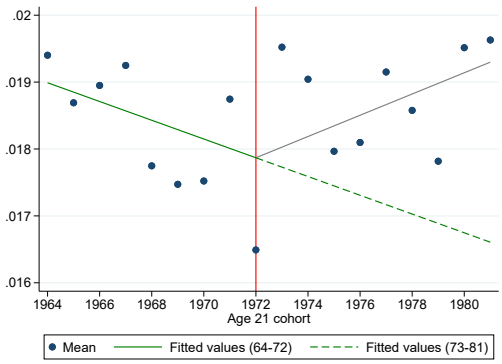
Figure F2: Visualization of Kink: Economic outcomes in 2002



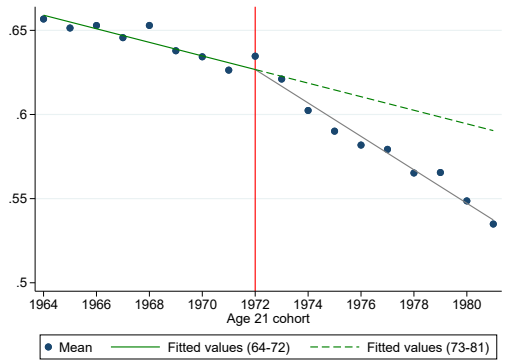
(a) In labor force - 2002



(b) Seeking work - 2002



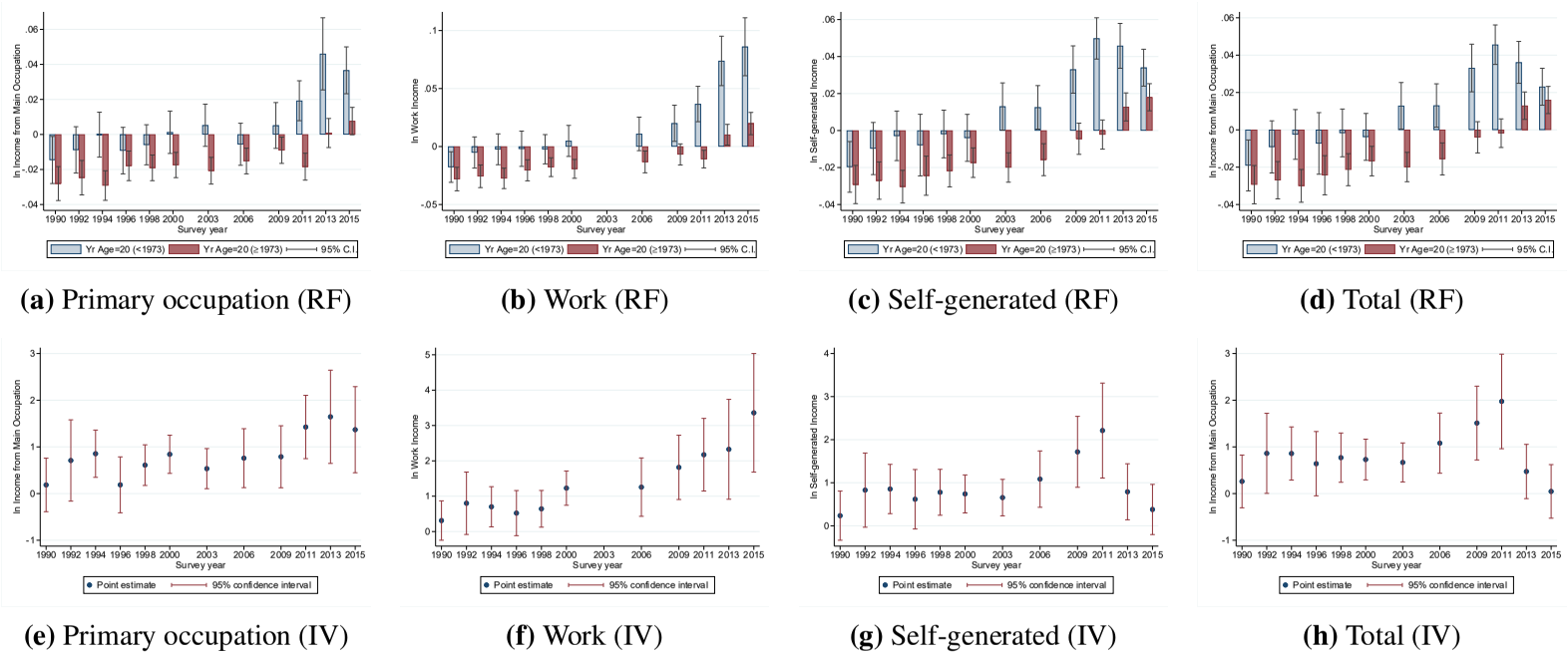
(c) Domestic Worker - 2002



(d) 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 F3: Educational Attainment and Reported Income - Yearly estimate

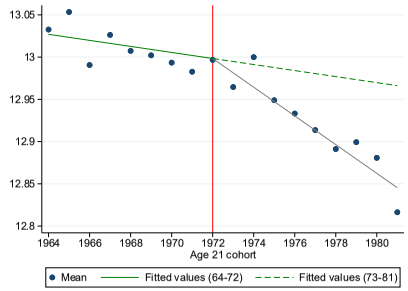


(a) Primary occupation (RF) **(b)** Work (RF) **(c)** Self-generated (RF) **(d)** Total (RF)

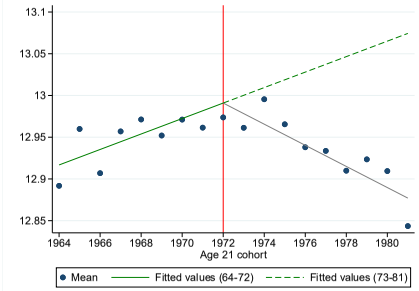
(e) Primary occupation (IV) **(f)** Work (IV) **(g)** Self-generated (IV) **(h)** Total (IV)

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

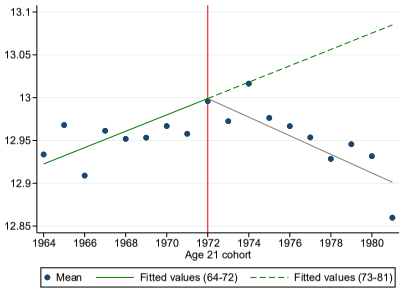
Figure F4: Visualization of Kink: Income and wealth



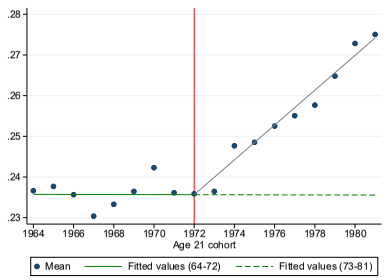
(a) Main Occupation



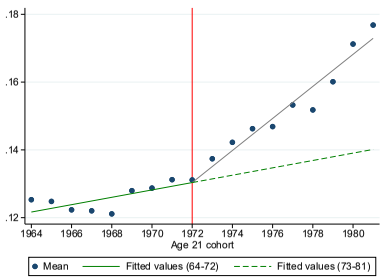
(b) All Work



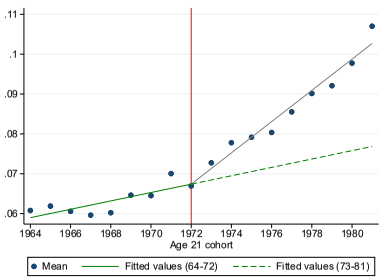
(c) Self-generated



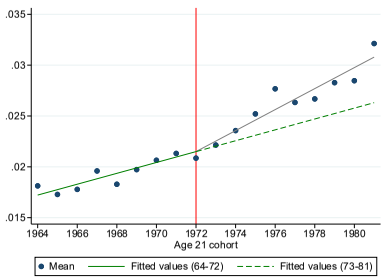
(d) Fourth Quintile - 1992



(e) Third Quintile - 1992



(f) Second Quintile - 1992



(g) First (bottom) Quintile - 1992

Note: Panels (a)-(c) show residualized averages by cohort for the variable in the caption, after controlling for county by gender fixed effects and survey year fixed effects. Panels (d)-(g) 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.

Table F3: Employment category

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

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

Table F4: 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 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 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 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

Appendix E Disaggregated results by gender

Table G1: Educational Attainment (Higher Education) by gender

	Any Secondary Education	Completed Secondary Education	Any College Education	Years of Education	Any College Education	Any College Education	Any Tertiary Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
[a] Yr Age 21 𝟙(Male)	0.0102***	0.0073***	0.0073***	0.1683***	0.0153***	0.0182***	0.0156***
	(0.00053)	(0.00033)	(0.00033)	(0.00459)	(0.00050)	(0.00363)	(0.00042)
[b] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973) x 𝟙(Male)	-0.0027***	-0.0025***	-0.0122***	-0.0884***	-0.0327***	-0.0382***	-0.0264***
	(0.00050)	(0.00058)	(0.00066)	(0.00433)	(0.00075)	(0.00510)	(0.00075)
[c] Yr Age 21 𝟙(Female)	0.0080***	0.0086***	0.1657***	0.0209***	0.0217***	0.0225***	
	(0.00063)	(0.00028)	(0.00048)	(0.00416)	(0.00051)	(0.00227)	(0.00051)
[d] Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973) x 𝟙(Female)	0.0022***	0.0013***	-0.0127***	-0.0340***	-0.0399***	-0.0435***	-0.0336***
	(0.00053)	(0.00048)	(0.00075)	(0.00462)	(0.00083)	(0.00333)	(0.00075)
Sample	Full	Full	Full	Completed High School	Completed High School	Completed High School	Completed High School
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family FE	No	No	No	No	No	Yes	No
Mean of dep. var F	0.531	0.337	0.093	8.580	0.276	0.282	0.364
Mean of dep. var M	0.550	0.351	0.110	8.843	0.315	0.309	0.394
Observations	2,982,951	2,982,951	2,982,951	2,982,951	1,024,570	42,526	1,024,570
R-squared	0.120	0.088	0.046	0.149	0.040	0.646	0.034
p-value a+b=0	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
p-value a=c	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	0.463	0.472

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

Table G2: Economic outcomes by gender (census 1992)

Dependent variable:	In labor force	Seeking work	Domestic worker	White-collar high-skill
	(1)	(2)	(3)	(4)
[a] Yr Age 21 x 𝟙(Male)	0.006***	-0.001***	-0.000	0.001***
	(0.0004)	(0.0002)	(0.0000)	(0.0005)
[b] Yr Age 21 x 𝟙(Male) x 𝟙(Yr Age 21 ≥ 1973)	-0.007***	0.003***	-0.000	-0.012***
	(0.0007)	(0.0002)	(0.0000)	(0.0007)
[c] Yr Age 21 x 𝟙(Female)	0.009***	0.0001	-0.002***	0.010***
	(0.0004)	(0.0001)	(0.0003)	(0.0008)
[d] Yr Age 21 x 𝟙(Female) x 𝟙(Yr Age 21 ≥ 1973)	-0.017***	0.001***	-0.003***	-0.026***
	(0.0007)	(0.0002)	(0.0004)	(0.0013)
[e] IV: Any College (Male)	0.226***	-0.082***	-0.005	0.366***
	(0.021)	(0.008)	(0.003)	(0.016)
[f] IV: Any College (Female)	0.416***	-0.038***	-0.071***	0.617***
	(0.018)	(0.005)	(0.009)	(0.020)
County of birth x gender FE	Yes	Yes	Yes	Yes
In labor force FE	No	Yes	No	No
Observations	1,024,570	1,024,570	773,922	770,652
R-squared	0.200	0.014	0.024	0.032
p-value a+b=0	0.002	0.000	0.025	0.000
p-value c+d=0	0.000	0.000	0.000	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	835.7	827.3
p-value e=f	0.000	0.000	0.000	0.000
Mean of dependent variable (Female)	0.571	0.027	0.026	0.485
Mean of dependent variable (Male)	0.946	0.038	0.002	0.399

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 G3: 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 G4: 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

Appendix F Additional results on IGT of Education

Table H1: Educational attainment of children: Sample Characteristics (Census 2017)

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

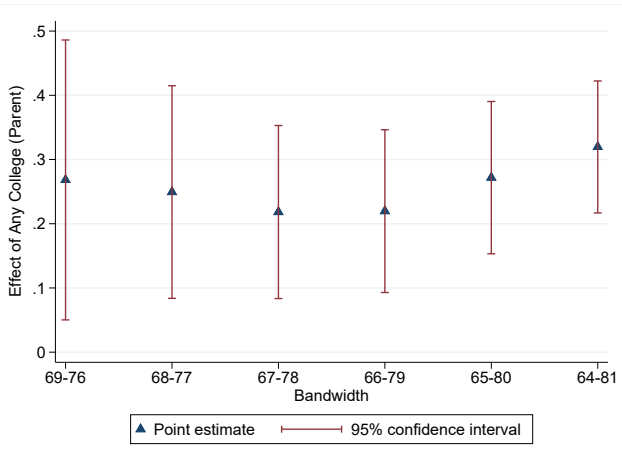
Notes: Table shows averages and standard deviations (in parenthesis) for the characteristic described in the header. Top row shows values for the full sample of people with ages 25-40 in the 2017 population census. Second row shows corresponding statistics for the subsample that cohabits with a parent,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

Table H2: 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
Mean of dependent variable	0.633	0.212	0.585	0.602	0.602
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

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

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 reached age 21 in the relevant bandwidth (both years inclusive) and reported full secondary education. "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. 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 H3: Educational attainment of children: Different bandwidths in child's age

	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 was born between 1943 and 1960 (both years inclusive) and reported full secondary education. "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 H4: Educational attainment of children: Heterogeneous effects by linkage

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

Notes: 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 was born between 1943 and 1960 (both years inclusive) and 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 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 𝟙(Male)	0.002**	-0.000	0.005***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
[b] Yr Age 21 Parent x 𝟙(Male) x 𝟙(Yr Age 21 Parent ≥ 1973)	-0.019***	-0.006***	-0.022***	-0.007***
	(0.002)	(0.001)	(0.001)	(0.002)
[c] Yr Age 21 Parent x 𝟙(Female)	0.006***	-0.000	0.002**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
[d] Yr Age 21 Parent x 𝟙(Female) x 𝟙(Yr Age 21 Parent ≥ 1973)	-0.026***	-0.008***	-0.021***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.001)
Panel B: OLS and IV - Dependent variable: Any College				
	IV	OLS	IV	OLS
[e] Any College Parent (Female)	0.318***	0.258***	0.305***	0.255***
	(0.069)	(0.005)	(0.069)	(0.005)
[f] Any College Parent (Male)	0.311***	0.265***	0.332***	0.268***
	(0.066)	(0.004)	(0.068)	(0.005)
Birth county x gender FE	Yes	Yes	Yes	Yes
Parent gender x gender FE	Yes	Yes	Yes	Yes
Relationship to household head FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Observations	233,123	233,123	233,123	233,123
R-squared [Panel A]	0.095	0.063	0.095	0.063
R-squared [Panel B]	-	0.118	-	0.118
p-value a+b=0 [Panel A]	0.000	0.000	0.000	0.000
p-value c+d=0 [Panel A]	0.000	0.000	0.000	0.000
p-value a=c [Panel A]	0.045	0.663	0.000	0.998
p-value a+b=c+d [Panel A]	0.002	0.010	0.328	0.392
p-value e=f [Panel B]	0.934	0.097	0.754	0.005
Mean of dependent variable (Female) [Panel A]	0.283	0.563	0.305	0.615
Mean of dependent variable (Male) [Panel A]	0.328	0.594	0.314	0.549
Mean of dependent variable (Female) [Panel B]	0.563	0.563	0.615	0.615
Mean of dependent variable (Male) [Panel B]	0.594	0.594	0.549	0.549
First-stage KP F-stat (Panel B)	76.4	-	93.2	-

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

Supplementary material (not for publication)

I Additional information on data sources

i Censuses and surveys

The population censuses of 1992, 2002 and 2017 were *de facto* and took place on days declared as national holidays. We restrict the sample to people born in Chile and we identify the cohort of birth using the respondents' age. 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 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.

ii Other sources

Data from the the Integrated Public Use Micro-data Series (IPUMS) is used for the synthetic control analysis. Harmonized data is available for 57 countries (see Table I1 for details). The Latin American countries (census year) that we use in the baseline analysis 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.

Table I1: Countries and samples in Synthetic Control Analysis

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