Dictatorship, Higher Education and Social Mobility*

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Abstract: We study the capture of higher education by the Pinochet dictatorship following the 1973 military coup in Chile. We show that the regime's twin aims of political control and fiscal conservatism led to a large contraction of all universities in the country, mostly through a steady reduction in the number of openings for incoming students. As a result, individuals that reached college age in the years immediately after the military coup experienced a sharp decline in college enrollment. These individuals had worse labor market outcomes throughout the life cycle and struggled to climb up the socioeconomic ladder. Children with a parent in the affected cohorts are themselves less likely to enroll in university, even after democratization. These findings illustrate the relationship between political regimes, redistributive policies and social mobility. They also shed light on the long-lasting effects of the reform agenda implemented under Pinochet.

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1 Introduction

The relationship between democracy and inequality has long attracted the attention of economists (Acemoglu et al., 2015). Prominent theoretical models posit a strong link between democratization and the enactment of redistributive policies (Meltzer and Richard, 1981; Acemoglu and Robinson, 2006), but the empirical evidence is mixed, perhaps due to a heavy reliance on cross-country comparisons and a dearth of natural experiments. Educational policy arguably plays a crucial role in this relationship. Education is often seen as the *great equalizer* and educational expansion appears to be the go-to policy to bolster social mobility. However, educational policy in authoritarian regimes remains unclear and may involve a trade-off between the economic loss from lower human capital and the political threat posed by a more educated population (Bourguignon and Verdier, 2000; Cantoni and Yuchtman, 2013). This is especially true in the case of universities, which are typically devoted to critical enquiry and often give origin to opposition movements.

We study the relationship between political regimes, educational policy and social mobility in the context of the Pinochet dictatorship in Chile. Our analysis proceeds in three stages. First, we document the capture of higher education by the Pinochet regime following the coup that brought it to power in 1973. We show that the regime's twin aims of political control and fiscal conservatism led to a large contraction of all universities in the country, mostly through the steady reduction in the number of openings for incoming students. Second, we study the effects of these policies by comparing individuals that reached college age in the years shortly before and after the military coup. We document a sharp downward kink in college enrollment for individuals that reached college age after the coup and show that these individuals had worse labor market outcomes throughout the life cycle and struggled to climb up the socioeconomic ladder. Third, we examine intergenerational effects by studying the educational attainment of children with parents in the affected cohorts. We show that children's college enrollment exhibits an analogous downward kink for those with a parent in the affected cohorts, despite the fact that they reached college age several years after Chile's return to democracy in 1990.

The first part of the paper provides a historical reconstruction of the political economy of higher education in Chile in the years around the 1973 coup that overthrew democratically-elected president Salvador Allende and replaced him with a military junta presided by Augusto Pinochet. In the immediate aftermath of the coup, the junta appointed members of the military as rectors of all universities, claiming that "universities have become centers for Marxist indoctrination" (Brunner, 2008, p.137, *own translation*). As the new government proceeded to eradicate all sources of po-

litical opposition, many students and faculty were dismissed, arrested, tortured or killed for their political views. But the regime's handling of universities soon begun to incorporate a technocratic concern about fiscal prudence and the size of government, under the growing influence over policy of a group of market-friendly economists known as the *Chicago Boys*. In the words of Levy (1986, p.105), "the regime's penchant for political control meshed conveniently with its penchant for economic conservatism." As a result, the government continually reduced public subsidies to universities over the following years, which forced these institutions to downscale, mostly by offering fewer openings for incoming students. Official figures show a sharp downward kink in college enrollment during this period. A comparison of the yearly number of applicants and openings reveals that it was entirely supply-driven.

University downsizing did not affect all fields of study equally, but differential changes in openings (which arguably targeted fields posing a greater political threat) did not substantially affect the distribution of enrollment across fields. More importantly, the regime preserved the pre-existing admissions process that employed a matching algorithm based on applicants' preferences and their performance in an admissions exam. As a result, the reduction in openings mostly affected students with lower test scores, which we show came disproportionately from less affluent family backgrounds. Here, the regime's technocratic goal of having merit-based admissions also meshed conveniently with its political aim "to modify the class composition of university student bodies" (CIA, 1985). We further find that post-enrollment outcomes (graduation rates, college earnings premium) improved for the ever fewer number of admitted students, consistently with the tighter admission standards and the regime's conception of universities as centers of academic excellence rather than vehicles for social mobility.

In the second part of the paper, we study the effects of the contraction of higher education under Pinochet. We exploit quasi-random exposure to diminished educational opportunities across birth cohorts that reached college age in a narrow window around the military coup, in the spirit of a regression kink design (Card et al., 2015). Figure 1 plots educational attainment by cohort in the 1992 census. Panel (a) shows the share per cohort that reports any college education, while panel (b) displays the share with four or more years of secondary (our proxy for secondary completion). We see a sharp negative kink in college enrollment starting with the cohort that reached age 21 in 1973 (the average age of first-year college students at the time), but there is no change in the trend in secondary completion. We conduct a synthetic-control analysis using harmonized census data from the same cohorts in other countries that confirms the existence of a sizable gap in college completion among Chileans reaching college age after the coup.

We then analyze downstream effects on labor market outcomes and social mobility for these

affected cohorts and their children using individual records from the population censuses of 1992, 2002 and 2017 and from 13 waves of a large household survey, CASEN, conducted between 1990 and 2017. Our baseline strategy involves estimating kinks in cohort-level trends of our outcomes of interest for people reaching age 21 between 1964 and 1981, an 18-cohort window centered around 1973. We restrict the sample to individuals with complete secondary education to ensure a relevant counterfactual to college entry, but all results are robust to using the unrestricted sample. Naturally, a comparison of different cohorts at any single point in time may be confounded by non-linear age effects. To address this concern, we use the higher-frequency data from the CASEN survey to estimate a more stringent specification that includes age fixed effects and allows outcomes to vary flexibly at each point in the life cycle.

We document downward kinks in labor force participation, in the probability of having a professional occupation and in self-reported income for individuals in the affected cohorts. These cohorts also exhibit an opposite upward kink in the unemployment rate. These kinks are economically meaningful, clearly visible in raw data from different sources, and are robust to flexibly controlling for age effects. They are also robust to changes in the bandwidth of cohorts included in the analysis. Using information on quintiles of wealth and income at the household level, we also find sizable downward kinks in the probability of being at the top of the respective distributions, as well as upward kinks in the probability of being in the lower quintiles. This is *prima facie* evidence that the dictatorship's educational policy hindered social mobility for the affected cohorts.

The final part of the paper examines potential intergenerational effects. 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 the inclusion of a large battery of controls and to multiple changes in the composition of the sample. We document a negative kink in college enrollment among children with a parent in the affected cohorts, analogous to the one observed for these parents. Looking at lower levels, we find smooth trends in the parents' cohort for children's progress through all levels of primary (which is mandatory in Chile), and downward kinks in all levels of secondary for children with a parent in the affected cohorts. However, the latter are much smaller in size to the one for college enrollment, suggesting that the parent's educational attainment matters more at the critical juncture of the transition into higher education.

We contribute to several strands of literature studying the relationship between political regimes, educational policies and distributional outcomes. Prominent theories of regime change are centered around redistributive pressures (Boix, 2003; Acemoglu and Robinson, 2008), but the evidence on the relationship between democracy and inequality remains inconclusive (Acemoglu et al., 2015). Educational policy has garnered substantial academic attention as a potential mediating mecha-

nism, but most research has focused on lower levels (i.e. primary education) and produced mixed findings (e.g. Mulligan et al., 2004; Harding and Stasavage, 2013; Paglayan, 2020). Evidence on higher education is scarce, limited to cross-country comparisons and suggestive of a null effect (Stasavage, 2005; Gallego, 2010). This may reflect the fact that educational policy in a dictatorship faces a fundamental trade-off between the economic gain from having a more educated population and the political threat it entails (Bourguignon and Verdier, 2000; Cantoni and Yuchtman, 2013).[1]

We make several contributions to this literature. First, we provide historical and quantitative evidence from Chile on the large impact of regime change on the operation and size of the higher education system.[2] Second, we provide within-country evidence that these changes have sizable and long-lasting socio-economic effects for the affected population and their children.[3] We show, in particular, that the contraction of higher education implemented by an incoming dictatorial regime can hinder social mobility for an entire generation. Third, our findings illustrate that the ideological affinity of a dictatorship plays a fundamental role in shaping educational policy, which the existing literature has largely ignored. Specifically, we show that a right-wing dictatorship may be less affected by the trade-off described above, as its aims of fiscal prudence and political control are both served by a reduction in the supply of education.

Regarding this last point, our paper also speaks to a separate strand of literature that has identified redistributive pressures and conflicting interests as a potential hindrance to growth-promoting policies under democracy (Alesina and Rodrik, 1994; Persson and Tabellini, 1994; Acemoglu, 2008; Aghion et al., 2008). Some commentators have more bluntly concluded that "Dictatorship will often be optimal for very poor countries" (Posner, 2010). Chile is an interesting setting in this regard, as the technocratic reforms implemented during the Pinochet dictatorship are often credited for the country's recent economic success (i.e. the *Chilean miracle*). While it is certainly beyond the scope of this paper to provide a comprehensive assessment of the impact of the Pinochet reforms, our results bring to light substantial and persistent socioeconomic costs and distributional consequences of the technocratic reform agenda.

Finally, our paper also relates to the vast body of work studying the returns to education (Card,

[1] Glaeser et al. (2007), Croke et al. (2016) and Larreguy and Marshall (2017) provide theory and empirics on the relationship between education and political engagement. Alesina et al. (2018) study education as a nation-building tool that homogenizes individual preferences, while Guriev and Treisman (2020) analyze a setting in which the size of an informed elite determines the incumbent's ability to manipulate information.

[2] The essays in Connelly and Grüttner (2005) provide historical evidence on the multifaceted functioning of universities in Communist and Fascist regimes. Other aspects of educational policy in dictatorships, such as changes in curricula and purges have been studied by Waldinger (2010, 2011) and Cantoni et al. (2017).

[3] The decade-long closure of Chinese universities amid the Cultural revolution has attracted substantial academic attention (Roland and Yang, 2017; Li and Meng, 2020; Alesina et al., 2020). While this change in policy took place several years into Communist rule, we study a transition from democracy to non-democracy. Also, the dictatorship we study differs sharply in its ideological inclination from Communist China, which we show has an effect on policy.

1999; Oreopoulos and Salvanes, 2011). We contribute to the literature on the intergenerational transmission of human capital (Black and Devereux, 2011; Björklund and Salvanes, 2011). 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, but little is known about the causal link between the college enrollment of parents and children.[4] The novelty of our results relates to the unique features of the decision to go to college (e.g. increased agency of child, higher price tag and opportunity cost), which set it apart from other junctures in the education process. We add to this literature by providing evidence of a positive intergenerational link in college enrollment and by doing so outside of the handful of highly developed countries in Europe and North America usually studied by this literature.

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 introduces our main data sources and presents our empirical strategy to study the effects of these changes. Section 4 documents the fall in educational attainment for the cohorts reaching college age after the military coup, while section 5 examines downstream socioeconomic effects. Section 6 looks at educational attainment for children with parents in these cohorts and section 7 concludes.

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

In this section, we provide an overview of higher education in Chile around the time of the military coup. We summarize the main features of the existing system before the coup and document the changes in policy implemented by the Pinochet regime. We also provide preliminary evidence of the effects of these policies on college enrollment and on the composition of the student body.

2.1 Universities and Public Policy Before the Military Coup

There were eight universities in Chile when Salvador Allende took office in 1970.[5] 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).

[4] Currie and Moretti (2003); Maurin and McNally (2008); Roland and Yang (2017); Suhonen and Karhunen (2019).

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

College enrollment grew from 25,000 students in 1960 to 77,000 by the end of the Christian-Democrat government of Eduardo Frei in 1970. The Allende government oversaw an even larger increase, reaching 146,000 students by 1973. Panel (a) in Figure 2 shows that the gross enrollment rate in higher education was 4.6% in 1960, 9.2% in 1970 and 16.8% in 1973. This was a period of mass expansion of higher education throughout Latin America, which was seen as a means of achieving equality of opportunity and social mobility (Brunner, 1984).

A broad movement for educational reform reached the universities in 1967. Besides bolstering 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 low. The reform also replaced the old baccalaureate exam administered by Universidad de Chile with a college admissions test called "Prueba de Aptitud Académica – PAA" (Academic Aptitude Test) starting in 1967. Under the new system, applicants rank programs while universities rank applicants based on a weighted average of their grades in secondary and their PAA scores. Universities choose the weight awarded to each component and the number of openings per program. A deferred-acceptance algorithm then determines admissions. This system remains largely unchanged until today.

2.2 Changes in Policy After the Military Coup

Amid growing political polarization and worsening 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 goal to eliminate any political opposition. Two weeks after the coup, the junta appointed members of the military as rectors of all universities, public and private. As motivation, the regime claimed that "universities have become centers for Marxist indoctrination" and that "the extremist agitation and hate preaching that almost drove Chile down a tragic abyss originated in these universities" (Brunner, 2008, p.137, *own translation*). 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.[6] Several academic units and most student groups were shut down, political activity was forbidden and teaching materials were censored, as "the regime insisted on depoliticizing student movements and discouraging student self-government" (CIA, 1985).

The dictatorship's initial handling of universities, focused exclusively on repression and polit-

[6]There are 24 professors and 252 students among the 3,200 deaths or disappearances attributed to the Pinochet regime by Comisión Rettig (1996). These correspond to 0.2% of the respective numbers of faculty and students in 1975. Comisión Valech (2004) estimates that about 10% of the 38,000 victims of detention or torture were students.

ical control, soon begun to incorporate a technocratic concern about the size of government and the efficiency of public spending (Echeverría, 1980; PIIE, 1984; Velasco, 1994).[7] This was the result of the growing influence over policy of a group of market-friendly economists known as the *Chicago Boys* (Valdés, 1995). As early as 1974, the Ministry of Finance begun pushing for a reduction in subsidies to universities and increased self-financing. In 1975, the Ministry of Education called for a more efficient use of resources and set enrollment goals for universities that effectively ended the rapid growth seen in previous years (PIIE, 1984; Levy, 1986). The fact that these measures of fiscal austerity further helped to defuse the political threat posed by universities facilitated their implementation by the dictatorship, whose "penchant for political control meshed conveniently with its penchant for economic conservatism" (Levy, 1986, p.105).

The Chicago Boys criticised the *status quo* in Chilean universities on various grounds (CEP, 1992; Valdés, 1995). They argued that an assured stream of government transfers failed to provide incentives for thrift, effort or innovation. The regime saw a connection between a bloated university system, low quality and political opposition: "the mediocrity that prevails in higher education... [is] a source of frustration for students, who become a breeding ground for political agitation" (Brunner, 2008, p.147, *own translation*). The Chicago Boys also argued that education resources were better spent on lower levels. 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*). Regarding college, the high cost per student meant that it should be considered a privilege rather than a right, especially since its benefits mostly accrued to a minority of high-income students. The dictatorship broke away from the previously dominant 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.

2.3 The Contraction: Funding, Enrollment and Composition of the Student Body

The military regime's twin aims of political control and efficiency in resource allocation both called for lower subsidies to universities. Panel (a) 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% by 1980. This was a large financial blow to universities, as government subsidies were their main source of funding, equivalent to 77% of total revenue in 1972 (Appendix Figure A1). However, a push for higher tuition met with strong resistance and was abandoned, forcing universities to downscale their operation.

[7] 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.

Panel (b) in Figure 2 shows the yearly number of openings for incoming college students together with the number of applicants. As expected, the number of college applicants exceeded the number of openings at all points in time, meaning that supply (i.e. openings) was always the binding constraint on first-year enrollment. Openings rose in tandem with spending under Allende and then fell and stagnated after the coup.[8] While universities offered a total of 47,000 openings in 1973, they only offered 33,000 openings in 1980 (30% decline). Even though demand (i.e. applicants) adjusted to the tighter supply after 1975, the number of applicants more than doubled the number of spots in most years until 1981, a process that has been described as "strangulation" (Levy, 1986, p.102). As a result of the early expulsions and the subsequent reduction in openings, college enrollment sharply declined after the military coup.[9] Panel (a) in Figure 2 shows that the gross enrollment rate fell from almost 17% in 1973 to 10.5% in 1981 (38% drop). The fact that UNESCO projections overestimated the number of students in 1975 by about 33% suggests that the drop in enrollment was not anticipated (Levy, 1986).

University downsizing did not affect all fields of study equally, but hardly any was left untouched. Panel (a) in Figure 3 shows the change in openings by field between 1973 and 1980. We see here an interaction between the technocratic imperative to broadly downscale universities and the political objective of targeting those fields deemed more problematic. The fields most affected were agriculture and social sciences, which arguably had more politically active students, while the natural sciences were the only to grow, though from a very small base (3% of openings). Most fields saw aggregate decreases in openings of between 20-30%, including the two fields with the largest shares of openings, education and engineering, which totalled 60% of openings between them in 1973. As a result, the distribution of total enrollment across fields did not change very much between 1973 and 1980, as panel (b) illustrates.

With the admissions process unchanged, the regime had little direct influence over who was admitted to university. Due to the nature of the matching algorithm, applicants with lower scores in the PAA test were mechanically the ones denied admission. Insofar as applicants from less affluent socioeconomic backgrounds had systematically lower test scores, they should have been disproportionately affected. Figure 4 shows the average score of admitted students in 1976 and 1981, disaggregated by father's occupation. While these are both post-coup years, they still capture the ongoing contraction of higher education. The averages are expressed relative to the highest value in each year (=100), which corresponds both times to children of faculty. The plot shows

[8] Appendix Figure A2 shows that the drop in openings was mostly driven by the two public universities, which had also been responsible for most of the growth under Allende.

[9] 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.

that the distribution of test scores of admitted students compressed as admissions tightened over this five-year period. Furthermore, admitted students from poorer families (i.e. father in blue-collar occupations) had lower average test scores in 1976 and experienced the largest relative increase in 1981, suggesting that they were more strongly affected. We observe again how the technocratic goal of merit-based admissions contributed to the regime's political goal of "modifying the class composition of university student bodies" (CIA, 1985).

2.4 Lower Levels and Aftermath

Data from lower levels of education suggests that the decline in college enrollment was not offset by large gains elsewhere in the system. Appendix Figure A5 shows that the enrollment rates in primary and secondary remained roughly constant, despite increases in the respective shares of the education budget. Moreover, the number of 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 (Appendix Figure A6). The only level with substantial growth in enrollment was early education, which almost tripled in size between 1970 and 1980 (from a base rate of 4%), though its growth mostly followed the pre-coup trend.[10]

The dictatorship's early policies towards higher education culminated in a large reform in 1981. This reform had three main elements. First, it turned the satellite campuses of the public universities into independent institutions. Second, it further reduced subsidies to all universities, forcing them to increase tuition. Third, it opened the system to competition by new universities, which were not eligible for government funding. As panel (a) in Figure 1 shows, college enrollment stabilized after the reform, but it would only grow again after the return to democracy in 1990.

3 Data and Empirical Strategy

In this section, we introduce our main data sources and present our empirical strategy to study the socioeconomic effects of the capture of higher education by the Pinochet regime after 1973.

3.1 Data

Our main data sources are the individual records from the population censuses of 1992, 2002 and 2017, which we complement with the thirteen waves of the biennial CASEN household survey

[10]The body overseeing early education was the National Council of Kindergartens (JUNJI), established in 1970 during the Allende government.

between 1990 and 2017.[11] For the synthetic control analysis, we additionally use harmonized census micro-data from IPUMS - International for 57 countries (Appendix Table B1).

The census collects basic demographic information and also asks questions on educational attainment and labor market outcomes. The dataset for 1992 additionally includes a variable calculating the wealth quintile to which the household belongs, which is based on characteristics of the dwelling and reported ownership of durable goods. The CASEN survey (*Encuesta de Caracterización Socioeconómica Nacional*) is a repeated cross-section that is representative at the regional level.[12] Its sample size has grown over time, with the most recent waves surveying more than 200,000 individuals in over 70,000 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, although it is self-reported. Based on this information, the survey datasets include a calculation of the income quintile to which each household belongs.

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-cohort 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 after that year. We focus on the year in which cohorts reached age 21 because administrative data shows that it is a conservative estimate for the average age of first-year college students at the time of the coup (Appendix Figure A7). We verify below that results are robust to tighter cohort bandwidths or to small changes in the age of college entry (i.e. changes in the kink point). For our main analysis, we restrict the sample to individuals that report at least four years of secondary education (completion is unavailable in all sources except the 2017 census). We introduce this restriction to ensure a relevant counterfactual for college enrollment, but we also verify below that the results are robust to using the unrestricted sample.

Our main outcome of interest on educational attainment is college enrollment, which is the margin that was directly affected by the dictatorship's policies. We then analyze downstream labor market outcomes, focusing on three variables that we expect to be directly affected by reduced college enrollment and that we observe in both the censuses and the CASEN survey. These are dichotomous measures of labor force participation, unemployment (if in the labor force), and of having a high-skill, white-collar occupation (i.e. high-ranking public officials, upper management and professionals). We also study reported income in the CASEN survey. To better understand effects on social mobility, we additionally study the household-level quintiles of wealth and income

[11] Appendix B provides further information about the data. Individual-level microdata is not available for earlier censuses. CASEN survey years are 1990, 1992, 1994, 1996, 1998, 2000, 2003, 2006, 2009, 2011, 2013, 2015, 2017.

[12] Chile is administratively divided into 16 regions, subdivided into 56 provinces and 346 counties.

available in the 1992 census and CASEN.

For the study of intergenerational effects, we rely on information on household composition from the 2017 census. We use this census because it is arguably the best source on the final education attainment of children with a parent in the affected cohorts.[13] The census classifies individuals into households, with one person identified as the household head and all others reporting their relationship to this person. We connect children to their parents using several different combinations of positions in the household and, thus, can only connect parents and children living together in 2017.[14] About 90% of our sample is composed of individuals reported as children of the household head.[15] We restrict the sample to children with ages between 25 and 40 in order to improve our chances of observing final college enrollment, while ensuring balance in the distribution of parental cohorts. We verify below that the results are robust to changes in this bandwidth. Parents in this sample must meet the same conditions as in our main sample above (i.e. reached age 21 between 1964 and 1981 and report four or more years of secondary education). We can confidently connect each child to only one parent, though we use information on the spouse of the parent (if observed) as part of our analysis of mechanisms (i.e., assortative matching). Our final sample includes 228,608 individuals (i.e. children), 58% of whom report having enrolled in university.

3.2 Empirical Strategy

Our empirical analysis begins by studying educational attainment among cohorts reaching college age in a small window around the 1973 military coup. To minimize bias from sample attrition, we use the 1992 census for this part of the analysis, but also show that similar patterns are present in all other sources. We first examine whether census respondents report any or full secondary education (i.e. 4+ years). We then look at college enrollment both unconditionally and conditional on our proxy for secondary completion. We work with the following reduced-form model, which exploits the fact that the age of college enrollment cannot be easily manipulated (i.e. cohorts facing reduced access to college could do little about it):

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

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-

[13] In 2002, the youngest cohort of parents was 42 years old, while in 2017 this same cohort is 57 years old.

[14] Appendix Table F1 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.

[15] An extra 5% corresponds to household heads that have a parent living with them. Other categories are much smaller and include siblings of the household head, the spouse of the household head and children of the spouse.

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, while $f(c)$ 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 we provide visual evidence showing that this parsimonious specification appropriately describes the evolution of cohort means for most outcomes. Our parameter of interest is π_1, which directly captures the change in trend (i.e. kink) for cohorts reaching college age after 1973. Finally, $u_{i,c}$ is an error term clustered at the county-of-birth level. To account for correlation of the error term within cohorts, we also provide p-values from the Wild cluster bootstrap procedure following Cameron et al. (2008).

We then study downstream effects on labor market outcomes and income by looking for similar changes in the respective cohort-level trends, in the spirit of the regression kink design (Card et al., 2015). Our identifying assumption is that in the absence of the coup there is no reason to expect kinks in our outcomes of interest for people reaching age 21 after this event.[16] As mentioned above, we use a symmetrical bandwidth of 18 cohorts reaching college age between 1964 and 1981. The discrete nature of the running variable prevents us from applying a non-parametric approach to select an optimal bandwidth, but we verify that our results are robust to changes in the bandwidth.

When using purely cross-sectional data on these outcomes (e.g. 1992 census), we provide estimates of equation (1). However, a valid concern surrounding this approach for outcomes that vary over the life cycle is that a cross-cohort comparison in a single cross-section may be picking up non-linear age effects. To address this concern, we exploit the availability of information from the relatively high-frequency CASEN survey to estimate a more stringent specification that replaces the baseline cohort trend with age and survey year fixed effects. In doing so, we allow the outcome to flexibly vary at each point in the life cycle and we ensure that the comparison across cohorts takes place only among people with the same age. We estimate the following model:

$$Y_{i,c,t} = \alpha + \beta X_i + \gamma_a + \phi_t + \psi \mathbb{1}(c \geq 1973) \times h(c) + e_{i,c} \tag{2}$$

where t now denotes time (i.e., year) and a denotes age, which is determined by the cohort c and the year t. Here, γ_a are age fixed effects while ϕ_t are year fixed effects. Everything else is similar to equation (1): we use a linear polynomial for the running variable (i.e., $h(c) = c$) and also cluster the error term $e_{i,c}$ either by county or by cohort. The coefficient of interest is ψ, which captures any potential kink among the affected cohorts, relative to all individuals observed at the same age.

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

To study intergenerational effects across cohorts, we use a specification analogous to equation (1). The main change is that the cohort trends correspond to the observed parent, while the outcome of interest (e.g. college enrollment) corresponds to the child. We also expand the set of individual controls X_i (i.e. referring to the child) 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.

4 Results: Educational Attainment

In this section, we study the educational attainment of cohorts that reached college age shortly before and after the 1973 military coup. We document a sharp downward kink in college enrollment for cohorts reaching college age after the coup and use a parsimonious parametric model to quantify it. We also show results from a synthetic control analysis that lend support to a causal interpretation of the findings. Finally, we discuss changes in other educational outcomes, including degree completion and the economic return to college.

4.1 Raw Data

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. The enrollment rate increased by 6 percentage points (pp) between the 1964 and 1972 cohorts (86% increase) and decreased by 5 pp between the 1972 and 1981 cohorts (38% decrease). Panel (b) shows that the share with full secondary increased smoothly throughout, ensuring that the drop in college enrollment is not driven by reduced attainment at lower levels.[17]

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 1-3 includes all individuals that reached age 21 between 1964

[17] Appendix Figure C1 shows that the kink in college enrollment is also present in other sources. Appendix Figure C2 shows smooth increases in the shares of people with any primary or secondary.

and 1981. Columns 1 and 2 show that the shares of people with any or full secondary education (i.e. 4+ years) were growing at respective rates of 1 pp and 0.8 pp per cohort before the coup. These rates remain entirely unchanged after the coup. Panel (a) in Figure 5 plots the raw data and the estimated linear trends for the share with completed secondary. 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. Not only do the linear trends fit the data quite accurately, but the post-coup trend overlaps almost perfectly with the counterfactual.

Column 3 shows that college enrollment increased on average 0.8 pp for each cohort that reached age 21 before the coup, but *decreased* by 1.2 pp per cohort for those reaching the same age after the coup. The difference between the two coefficients indicates a net enrollment trend of -0.4 pp per cohort (i.e. decrease) after the coup. Panel (b) in Figure 5 illustrates this sharp kink. Column 4 replicates the analysis for the restricted sample reporting four or more years of secondary, our proxy for secondary completion. We observe a similar pattern to the one in column 3, as expected given the smooth trend in secondary completion. Conditional on completed secondary, which we deem the relevant counterfactual, college enrollment increased by 1.8 pp per cohort before the coup but *decreased* at the exact same rate for those that followed.[18] These results are in line with the yearly pattern in college enrollment shown in Figure 2. What is notable about the cohort-level analysis is that it allows us to identify sharp differences in the incidence of the reduction in access to college across groups of people (i.e., cohorts) that we can track in the censuses and the CASEN survey to study downstream effects.

Columns 5-7 in Table 1 provide additional evidence of a broad impact. In column 5, we expand equation (1) to allow for separate trends in college enrollment by gender. We see that women were making larger gains in enrollment than men in the cohorts that reached college age before the coup, but that they also experienced a sharper decline in the post-coup cohorts. This allows us to explore heterogeneous effects by gender in our analysis of downstream consequences below. Column 6 shows that the results are unchanged despite a 96% decrease in sample size when we restrict the sample to siblings and include family fixed effects.[19] Finally, 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 4, indicating that enrollment in other institutions was also growing, though universities drove almost all of the action. Similarly, the fact that the net post-coup drop in total enrollment is smaller (-1.1 pp) than in college enrollment (-1.8 pp) suggests

[18] Appendix Table C1 shows similar results for the other censuses and the CASEN survey. Appendix Table C2 shows that the results are robust to small changes in the location of the kink point.

[19] We classify as siblings people within a household meeting one of these conditions: (i) two or more children of the household head; (ii) the head and one or more siblings. Appendix Table C3 replicates this analysis for the other censuses. Appendix Table C4 shows similar results within quintiles of housing wealth in 1992.

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 countries in Latin America to construct the counterfactual, but results are unaffected if we also use data from other countries (Appendix Figure D1). We calculate the share of people per cohort with completed secondary and college in each 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. We follow Ferman et al. (2019) and use only *odd* years to avoid cherry-picking and overfitting, but verify that results are unchanged 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.

Panel (a) in Figure 6 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. Placebo inference and confidence sets suggest this difference is statistically significant (Abadie et al., 2015; Firpo and Possebom, 2018).[20] The synthetic control keeps growing, while the actual series displays initially a downward kink and then stagnates. The synthetic control suggests that around 8% of the cohort that reached age 21 in 1981 would have had a college degree in the absence of the coup, while in reality only 5% did. This gap in completion is equivalent to 38% of the counterfactual provided by the synthetic control. The graph further suggests that college enrollment only returned to its counterfactual rate for the cohorts that reached college age in the late 1990s, several years after the return to democracy.

Our baseline estimates do not include controls, but panel (b) shows a very similar pattern if we control for the share of people with ages 18-65, the share of women, and the share with secondary education. 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 for completed secondary education, providing further evidence that the effects on college cannot be attributed to changes in lower levels of education. In panel (d), we restrict attention to the pre-1973 period and create a synthetic control using a placebo coup in 1960, following Abadie et al. (2015). Reassuringly, we observe no meaningful difference between the counterfactual series and its actual realization.

[20] See Appendix Table D1 and Appendix Figure D2 for details.

4.4 Post-Enrollment Outcomes

Before studying the long-term consequences of the reduction in college enrollment for the cohorts that reached college age shortly after the military coup, we examine potential effects of the capture of Chilean universities by the Pinochet dictatorship on some post-enrollment outcomes.

The evidence in Figure 7 suggests that these outcomes improved after the military take-over. Panel (a) focuses on the graduation rate, which we measure either as a share of total enrollment using UNESCO data (circles) or as a share of the number of people reporting any college in the 1992 census (triangles). Both sources show a similar pattern of decline before the coup and recovery afterwards. The census data indicates, though, that the graduation rate hovered around 72% throughout the sample period and never dropped below 69%. Despite the large expansion in enrollment under Allende and the many expulsions after the coup, the graduation rate only declined 4 pp between the cohort that reached age 21 in 1968 and the one that reached the same age in 1975 (5% decrease). Graduations increased sharply in later years, arguably due to the tighter admission standards and the regime's renewed focus on universities as centers for academic excellence.[21]

Panel (b) shows cohort-specific estimates of the correlation between college enrollment and log total income using data from the CASEN survey (1990-2017). Additional controls include county of residence by gender, survey year and age fixed effects.For cohorts reaching college age before the coup, the return to college fluctuated around 75 log points. For the ones that followed, we see a sharp jump and a positive trend. The post-coup average of about 85 log points represents a 13% increase.[22] This higher return could be a reflection of the higher quality of admitted students or their higher graduation rate. It is also consistent with a lower supply of professionals after the coup putting upward pressure on wages if there was 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, at least from the perspective of employers, though repression and censorship likely had a negative impact on the student experience. Hence, any negative socioeconomic effects for the cohorts that reached college age after 1973 can be interpreted as arising from reduced access to college, despite improved outcomes for those that were able to attend.

[21] Appendix Figure A8 shows that fewer people took the PAA test after the coup and that average (raw) test scores improved. This suggests that people with lower expected scores drove the decline in college applications after 1975.

[22] Appendix Figure C3 shows a similar pattern for other income measures. It also shows that the results are robust to the inclusion of occupation fixed effects (i.e. not driven by differential changes in openings across fields).

5 Results: Socioeconomic Consequences

In this section, we document the downstream economic effects of the contraction of higher education carried out by the Pinochet dictatorship. We study labor market outcomes and measures of income and wealth in the cohorts that reached college age shortly before and after the 1973 military coup. We additionally look for heterogeneous effects by gender and, at the end, provide suggestive evidence on the effect of college enrollment using an Instrumental Variables (IV) strategy.

5.1 Labor Market Outcomes

Panels (a)-(c) in Figure 8 plot cohort-level means and estimated trends for binary indicators of being in the labor force, seeking work, or having a white-collar high-skilled occupation, using data from the 1992 census. Panels (d)-(f) plot the same variables from the CASEN survey, averaged across waves between 1990 and 2017. Panel (g) shows an additional plot for log total income, which is only available in CASEN. We observe similar patterns in both sources, indicative of worse labor market outcomes for the cohorts that reached college age after the coup. The graphs provide clear evidence of downward kinks for post-coup cohorts in labor force participation, occupation and income, while there is an upward kink in the probability of being unemployed.

Table 2 quantifies these kinks. Panel A uses the census data and panel B uses CASEN.[23] While the census provides us with a much larger sample, CASEN enables us to observe cohorts repeatedly over time (i.e. at various points in the life cycle). We show standard errors clustered by county in parentheses and p-values from the wild cluster bootstrap procedure at the cohort level in brackets.

Focusing on the results from CASEN in panel B, which include survey year and county by gender fixed effects, we find that the change in trend in labor force participation for the affected cohorts amounts to almost 60% of the observed trend for the pre-coup cohorts (column 1). Similarly, column 2 shows that the upward kink in the unemployment rate is five times as large as the baseline trend, while column 3 indicates that the drop in the probability of having a prestigious professional occupation is twenty-seven times as large as the baseline trend. These results suggest that college enrollment has a much larger effect on occupational choice than on labor force participation or unemployment. Changes in these intermediate labor market outcomes naturally lead to changes in income. Column 4 provides evidence of a downward kink in total income equivalent to 1.4 times the trend observed for the pre-coup cohorts. Importantly, the baseline trends in all columns suggest that before the coup outcomes were generally improving for younger cohorts, the

[23] Similar results for the 2002 census are available in Appendix Table E1. We do not use the 2017 census for this analysis because it does not ask questions on occupation and because the cohort reaching age 21 in 1973 (i.e. kink point) reached age 65 on that year, which is the retirement age for men (women retire at 60).

opposite of what happens after the coup.

Panel C shows estimates of equation (2), an arguably more stringent specification that replaces the baseline trend with age fixed effects. We can only estimate this specification with data from the CASEN survey, as it requires us to observe cohorts repeatedly over time (i.e. exploits variation across cohorts at the same age). We find that people in the affected cohorts experienced sizable negative effects in labor market outcomes, even when we flexibly account for potentially confounding effects associated with the life cycle. Column 1 shows that each new cohort reaching college age after the coup is 0.4 pp less likely to be in the labor force than the average for that age (0.5% decrease per cohort relative to the sample mean). Column 2 shows that the unemployment rate increases by 0.2 pp per post-coup cohort relative to the age-specific average (5% increase relative to sample mean), while column 3 indicates that the probability of a white-collar high skilled occupation decreases at a rate of 1.1 pp per cohort (2.8% of sample mean).[24] Finally, column 4 shows that total income decreases by 0.9% relative to the average for that age for each additional cohort that turned twenty-one between 1973 and 1981.[25]

5.2 Distributional Effects on Wealth and Income

The previous results indicate that people reaching college age after the military coup had worse labor market outcomes, plausibly as a result of diminished educational opportunities. In this section, we provide additional evidence based on distributional measures (i.e. quintiles) of wealth and income available in the 1992 census and CASEN. These measures allow us to better understand the challenges that the affected cohorts faced in climbing up the socioeconomic ladder, insofar as there is no reason to expect people born in different years to come from more or less affluent families. Importantly, these measures of income and wealth are calculated at the household level. As a result, intra-household insurance operating through the marriage market or other mechanisms could potentially attenuate the negative impact of the labor market effects documented above.

Figure 9 plots the cohort-level means and estimated trends. Panels (a)-(e) correspond to dummies for each quintile of wealth based on data from the 1992 census, while panels (f)-(j) show the same information for income quintiles based on CASEN (again, averaging across survey waves). There are sharp downward kinks in the probability of being in the fifth (i.e top) quintile of wealth or income for the affected cohorts, matched by upward kinks in the probability of being in the

[24] Appendix Table E2 shows that the effect on occupation corresponds to a 0.4 pp increase in the probability of a white-collar low-skill occupation, a 0.8 pp increase in the probability of a blue-collar occupation and a 0.1 pp decrease in the probability of being in the military. Looking at types of employment, we also find a downward kink in the probability of being a salaried employee (-0.7 pp per cohort), matched by roughly same-sized increases in the probability of being a business owner or self-employed. We interpret this as further evidence of economic vulnerability.

[25] Appendix table E3 shows similar results for more restrictive measures of income (e.g., main occupation).

bottom three quintiles. While panel (b) shows a clear upward kink in the probability of being in the fourth quintile of wealth, panel (g) suggests a downward kink in the probability of being in the analogous quintile for income, though the latter is exceptionally noisy.

Panels A and B in Table 3 show estimates of equation (1) for these outcomes. The dependent variables in panel A are the dummies for wealth quintiles in the census, while in panel B they are the corresponding dummies for income quintiles in CASEN. Focusing on the top quintiles in column 1, we estimate in panel A a sizable kink in wealth for the post-coup cohorts, equivalent to 6.5 times the baseline trend. For income, in panel B, we estimate an analogous downward kink that is three times as large as the baseline trend. Panel C provides estimates of equation (2) for the income quintiles in CASEN. We find that individuals belonging to each younger cohort reaching college age after the coup are 0.2 pp less likely to be in a household belonging to the top income quintile than the age-specific average (0.6% decrease relative to the sample mean). This reduction in the probability of being at the top of the income distribution is matched by 0.1 pp per-cohort increases in the probability of being in either of the bottom two quintiles (0.8% and 1% increases relative to the respective sample means). These results suggest that diminished educational opportunities for individuals reaching college age after the military coup had sizable negative effects on their relative standing in the income distribution.

5.3 Robustness Checks

We verify that the previous results are robust to several robustness checks. First, Appendix Table E1 provides estimates of equation (2) for labor force participation, unemployment and occupation using pooled census data from 1992 and 2002. The ten-year period between these censuses generates an equivalent gap between the pre- and post-coup cohorts that we are able to observe at the same age, but the results are remarkably similar to the ones using higher-frequency data from CASEN in panel C of Table 2. Secondly, all of the previous results are based on samples that only include individuals that report four or more years of secondary education, our proxy for secondary completion. We consider such individuals to be the relevant ones for studying the impact of reduced access to college, especially after having shown above that the cohort trend in secondary completion does not change after the military coup. Still, we verify that this restriction is not leading to some form of selection bias that is fundamentally shaping the results. Appendix Tables E4 and E5 replicate the analysis for the unrestricted samples that include all individuals belonging to the relevant cohorts. The results are very similar to the ones from the restricted sample. Finally, we examine the robustness of the results to changes in the bandwidth of cohorts in the sample. We focus on the more conservative specification with age fixed effects. Appendix Figures E1 and

E2 plot estimates of ψ in equation (2) for our baseline bandwidth of 18 cohorts, as well as for tighter bandwidths that drop one, two or three cohorts on each side or that add similar numbers. The results are highly stable as we reduce or expand the bandwidth, though some estimates lose precision with the smaller bandwidths due to the reduction in sample size.

5.4 Heterogeneous Effects by Gender

Panel (a) in Figure 10 plots separate point estimates and 95% confidence intervals of ψ in equation (2) by gender for each of the labor market outcomes in Table 2.[26] Next to each bar, we have included the respective sample mean to facilitate the calculation of effect sizes. Hence, even though we observe a similar per-cohort decline of about 0.4 pp in labor force participation for men and women after the coup, 87% of men in the sample are in the workforce while only 53% of women are, meaning that women were more strongly affected. For unemployment, though the sample mean does not differ by gender, the upward kink is much larger and only statistically significant for women. Women also display much larger downward kinks in professional occupation and log income than men. For instance, women in each younger cohort after the coup were 1.6 pp less likely to have a white-collar high skilled occupation than the age-specific average (3.5% of the female sample mean), while men experienced a 0.8 pp decline per cohort (2.2% of the male sample mean). Overall, these results indicate that access to higher education was fundamental to female progress in the labor market, in line with Goldin (2006).

Panel (b) provides analogous results for the household income quintiles included in panel C of Table 3. We estimate a downward kink in women's probability of being in the top wealth quintile of 0.3 pp per cohort relative to the age-specific average (0.9% of the female sample mean), while for men we find an insignificant drop of 0.1 pp per cohort (0.3% of the male sample mean). The drop in women's probability of being at the top of the income distribution is matched by an increase of 0.2 pp in the probability of being in the bottom two quintiles. We conclude that diminished access to higher education had larger negative consequences for women in the affected cohorts.

5.5 Instrumental Variables Estimates of the Effect of College Enrollment

In the online appendix we provide additional results from an instrumental variables (IV) strategy that uses the downward kink for the affected cohorts as an excluded instrument for college enrollment. Under standard assumptions, this IV estimate corresponds to a local average treatment effect (LATE) of the effect of college enrollment for compliers, i.e. those students that failed to attend college because of the contraction of supply by the military regime (Angrist et al., 1996).

[26] Full results disaggregated by gender are available in Appendix Tables E6 and E7.

Importantly, while the previous results strongly suggest that the affected cohorts had worse economic outcomes as a result of their diminished access to higher education, the IV strategy requires a stronger exclusion restriction implying that kinks in downstream outcomes for these cohorts are driven exclusively by reduced college enrollment. Even though the military coup brought about many changes to Chilean society, we find this assumption to be plausible insofar as these changes should have affected cohorts of young adults within a narrow bandwidth in a roughly similar way. Additional tests following Conley et al. (2012) further show that the IV estimates are robust to moderate violations of the exclusion restriction.

Focusing on a specification with age fixed effects (i.e. adaptation of equation 2) with data from CASEN, Appendix Table E8 shows that college enrollment increases labor force participation by 16 pp, reduces unemployment by 6 pp. and increases the probability of having a professional occupation by 41 pp. Moreover, college enrollment leads to a 34% increase in total income, which is slightly higher than the 22% increase estimated by Zimmerman (2014) using a regression discontinuity design. Equivalently, our IV estimate corresponds to a 7% increase in income per year of education, comparable to the estimate of 9% higher income per year from the meta-analysis by Psacharopoulos and Patrinos (2018). Appendix Table E9 further shows that college enrollment increases the probability of being in the top 20% of household income by 9.2 pp and decreases the probability of being in the bottom two quintiles by about 5 pp each. For all outcomes considered, the IV estimates are generally comparable to the partial correlations estimated through OLS and are often smaller, suggesting possible selection bias in the latter.

6 Results: Intergenerational Effects

In this section, we study the educational attainment of children with a parent in the affected cohorts, focusing on their probability of college enrollment. Section 3 discusses the construction of the sample for this analysis, which includes more than 230,000 people in the 2017 census that we can link to a parent meeting our sample inclusion criteria. The children have ages between 25 and 40 in 2017, meaning that the oldest people in our sample reached age 21 in 1998, eight years after Chile's return to democracy. We use here the same specifications as above, with the exception that the cohort-level variables refer to the parent, while the outcomes refer to the child.

6.1 Main Results

To start, panel A of Table 4 provides estimates of equation (1) showing that college enrollment among the parents of the children in our sample exhibits a pattern essentially identical to the full

sample in Table 1. This suggests that these parents are not fundamentally different from the larger population. Panel B provides 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 of parents and children.

The only controls in column 1 are the gender by county of birth fixed effects included in all previous regressions using census data. In column 2 we further control for the combination of parent's and child's gender, ensuring that potential differences in the gender composition of the sample across cohorts do not bias the estimates. In column 3 we include an additional set of dummies for the relationship of the child to the household head in the census, which is what we use to link parents and children. The results change very little across specifications.

In column 4 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 downward-bias the estimate of the intergenerational effect if younger people benefited from a positive secular trend in college enrollment in recent years. Indeed, we find that controlling for the child's age makes the baseline trend negligible and insignificant, while increasing the per-cohort decline after the coup from -0.5 pp to -0.7 pp. The specification in column 4 is our preferred specification for this part of the analysis.[27]

Figure 11 provides a non-parametric visualization of the results in column 4. Panel (a) shows point estimates and 95% confidence intervals for parents' college enrollment. As in our main analysis above, there is an upward trend in college enrollment among cohorts reaching college age before the coup, while those reaching the same age after the military take-over experience a sharp decline. Panel (b) shows the relationship between the cohort of the parent and the college enrollment of the child. We observe a clear decline in the probability of going to college for children with a parent that reached college age after the coup. For example, 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 in 1972. However, the latter is just as likely to attend college as a third child with a parent born seven years before in 1966.

Using the kink for the affected cohorts of parents as an excluded instrument, Appendix Table F2 presents IV estimates of the effect of parental college enrollment on the child's probability of enrollment. As before, these results rely on an additional exclusion restriction requiring the

[27] However, by including age fixed effects we are effectively 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 confound the analysis. Hence the importance of showing the robustness of the results to the exclusion of these fixed effects.

parent's cohort to affect the child's college enrollment exclusively through the parent's diminished access to college. We estimate that having a parent that went to college increases a person's chances of enrolling by 32 pp. This is a large effect, equivalent to 45% of the sample mean of 58%, but is only slightly larger and not statistically different from the corresponding OLS estimate.

6.2 Robustness and Mechanisms

We subject the previous results to a battery of robustness tests. Appendix Figure F1 shows that the results are hardly affected if we consider more conservative bandwidths for the ages of parents. Appendix Table F3 further shows that the results are robust to different windows of ages for the children included in the sample, including a narrow bandwidth with ages 25-30 (the average person in this sample was *born* in 1990, the year in which Chile returned to democracy). Table F4 shows that the downward kink in college enrollment is stronger for affected 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 F5 provides disaggregate results based on gender of the parent or the child. Effects are slightly larger for mothers, consistent with the larger socioeconomic impact of the reduction in college enrollment for women documented above, and for male children.

To better understand the stage at which the educational attainment of children with affected parents lags behind, column 5 in 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, but its inclusion only leads to a small reduction in the magnitude of the estimated kink. Hence, most of the effect of parental college enrollment materializes after children finish secondary. Appendix Table F6 provides additional results using completion of each grade in primary and secondary as dependent variable. The parental cohort trends are smooth for all grades in primary, which is to be expected as primary education is mandatory in Chile and beyond the control of parents. However, we find evidence of a downward kink in the probability of progressing through all grades in secondary for children with a parent in the affected cohorts. The magnitude of these kinks is much smaller than for college enrollment, confirming that the transition into higher education is the critical juncture at which the intergenerational effects mostly manifest.

Our strategy to link parents and children, based on reported relationship to the household head in the census, only allows us to credibly link each child to one of the parents. But having a parent with college plausibly affects the child partly through the educational attainment of the other parent, which we do not observe. To gain further insight, Appendix Table F7 uses information on

the partner or spouse of the linked parent as a proxy. For this part of the analysis, we must restrict the sample to linked parents that are household heads, though these are the bulk of the overall sample. We find evidence of downward kinks for post-coup cohorts in the probability that the parent has a spouse and, if there is a spouse, in the probability that this person attended college (i.e. assortative matching). Controlling for the education level of the spouse reduces the magnitude of the kink in the child's college enrollment from -0.6 pp per cohort to -0.4 (33% drop). This suggests the presence of strong complementarities in the education of both parents (or parents and spouses) in the production of children's human capital.

7 Conclusion

In this paper, we study the capture of higher education by the Pinochet dictatorship following the 1973 military coup in Chile. We show that the regime's twin objectives of political control and fiscal conservatism led to a large contraction of the university system nationwide, mostly through the steady reduction in the number of openings for incoming college students. A comparison of educational outcomes for individuals that reached college age shortly before and after the military coup reveals that the latter experienced a sharp decline in the probability of college enrollment. These individuals had worse economic outcomes throughout the life cycle and struggled to reach the top of the socioeconomic ladder. We further show that the effects of the educational policies of the Pinochet regime extend across generations, as children with a parent in the affected cohorts are also less likely to enroll in university, even several years after the country's return to democracy.

We draw several conclusions from these results. Our findings show that political regime change can have a large impact on the functioning and size of the higher education system. These changes have sizable and long-lasting socioeconomic effects and they hinder social mobility. Educational policy is, thus, an important mediating mechanism through which political regimes affect distributional outcomes. Importantly, the ideological orientation of a dictatorship plays a fundamental role in shaping educational policy. In the case of the right-wing dictatorship we study, the regime's twin aims of fiscal prudence and political control were both served by a contraction in the supply of education. In other settings, the loss in human capital entailed by such policies may be more salient, which could help attenuate the impact of a transition into non-democracy. Finally, our results speak to the hypothesis that dictatorship may be desirable at early stages of development by casting light on the socioeconomic costs and distributional effects of the technocratic reform agenda implemented by the Pinochet regime.

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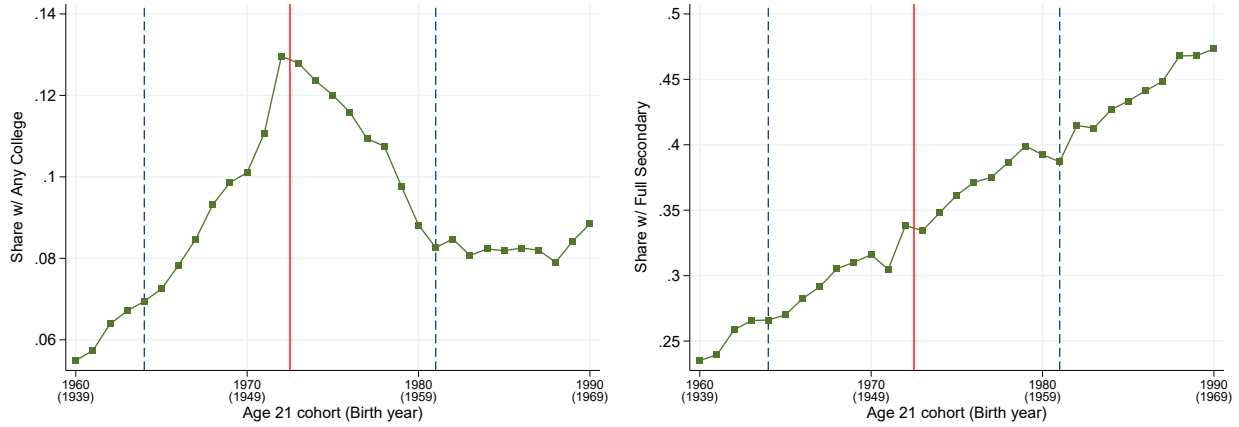
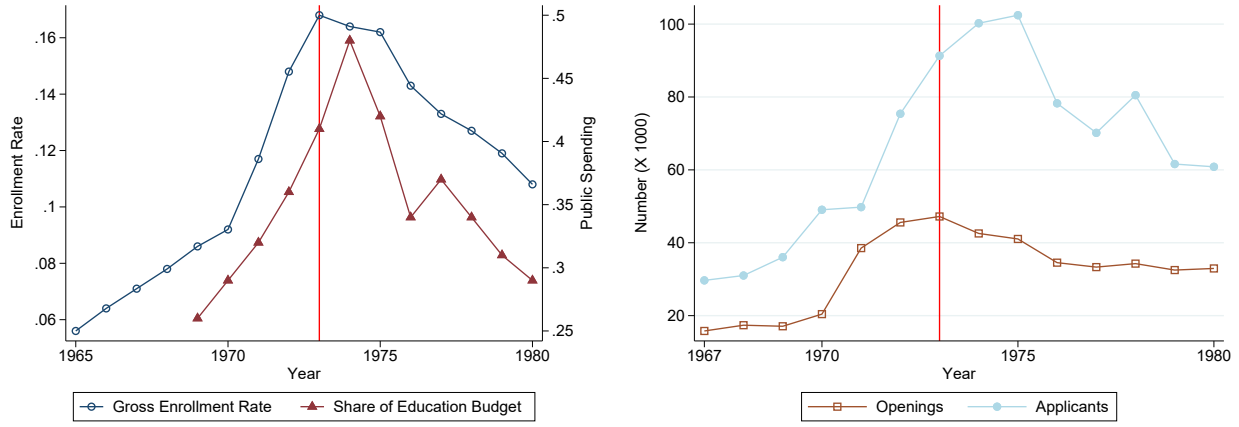
Figure 1: The Military Coup and College Enrollment



(a) Any College **(b)** Full (4+ years) Secondary

Notes: Panels 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. 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.

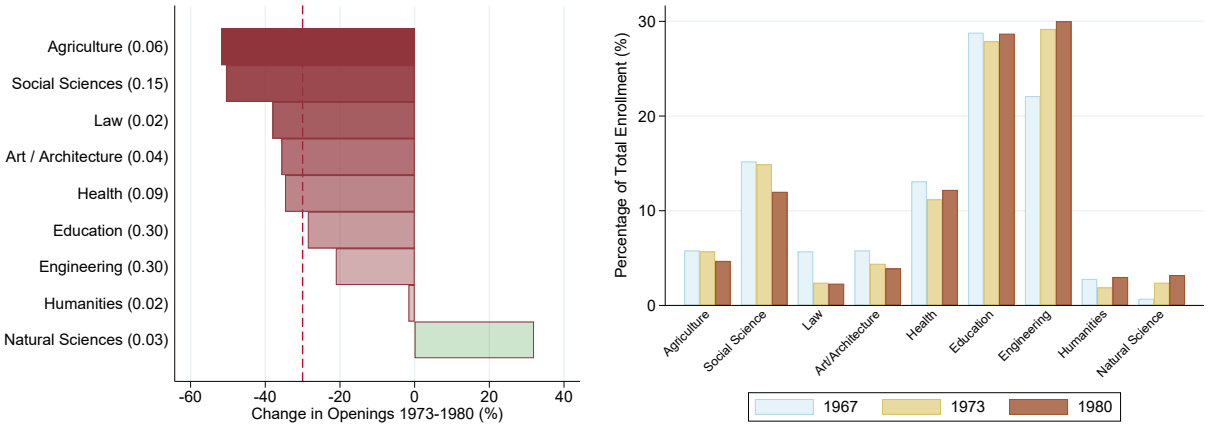
Figure 2: College Funding, Enrollment and Openings



(a) Funding and Enrollment **(b)** Applicants and Openings

Notes: Panel (a) shows the gross enrollment rate in higher education (i.e. share of 20-24 year-old population) and the share of the national government's education budget devoted to universities. Panel (b) shows the yearly number of college applicants and openings. Sources: PIIE (1984); Universidad de Chile (2011).

Figure 3: College Openings and Enrollment by Field

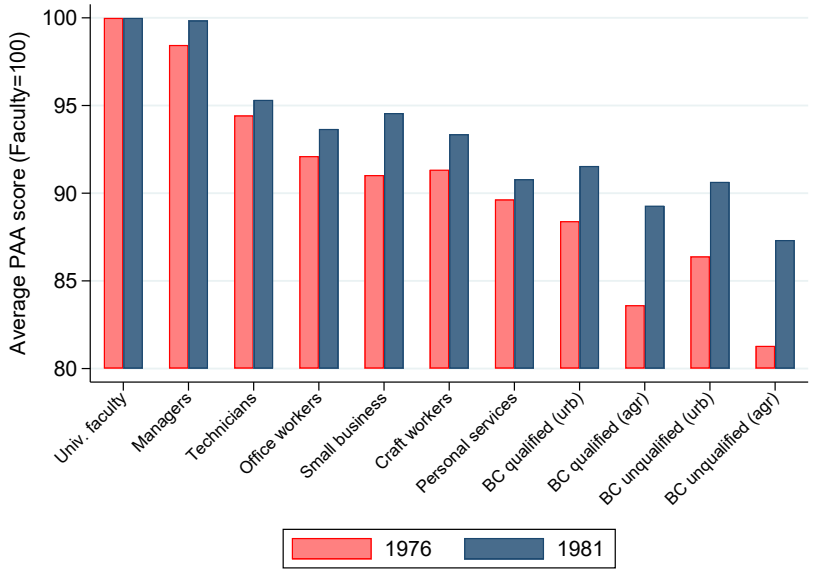


(a) Change in Openings by Field (1973-1980)

(b) Enrollment by Field

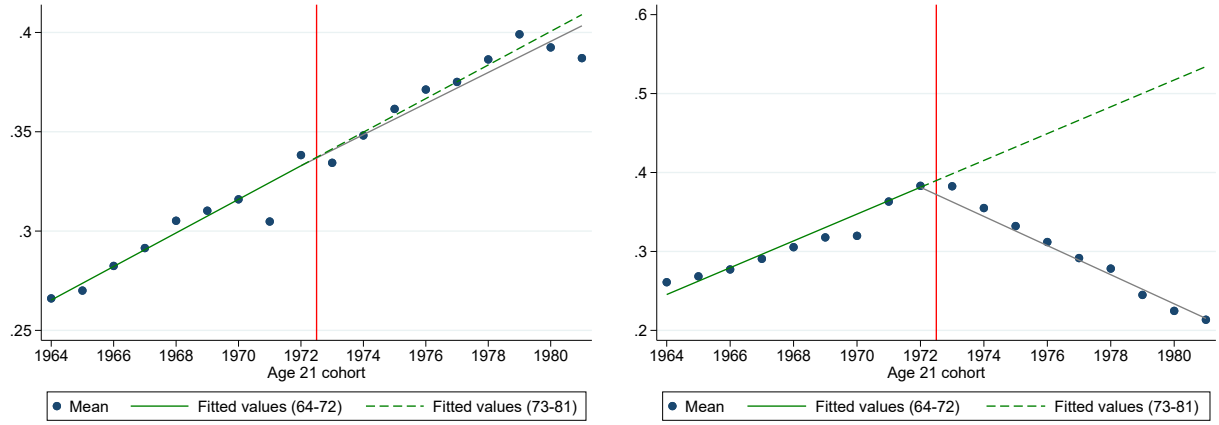
Notes: Panel (a) shows the change in openings by field of study between 1973 and 1980. The number in parenthesis corresponds to the field's share of openings in 1973, while the dashed line indicates the aggregate reduction in openings. Panel (b) shows the share of students enrolled in programs corresponding to different fields of study in 1967, 1973 and 1980. Classification corresponds to UNESCO categories. Sources: PIIE (1984); Brunner (1984).

Figure 4: Average PAA Test Score of Admitted Students by Father's Occupation



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

Figure 5: Visualization of Kink: Educational Attainment

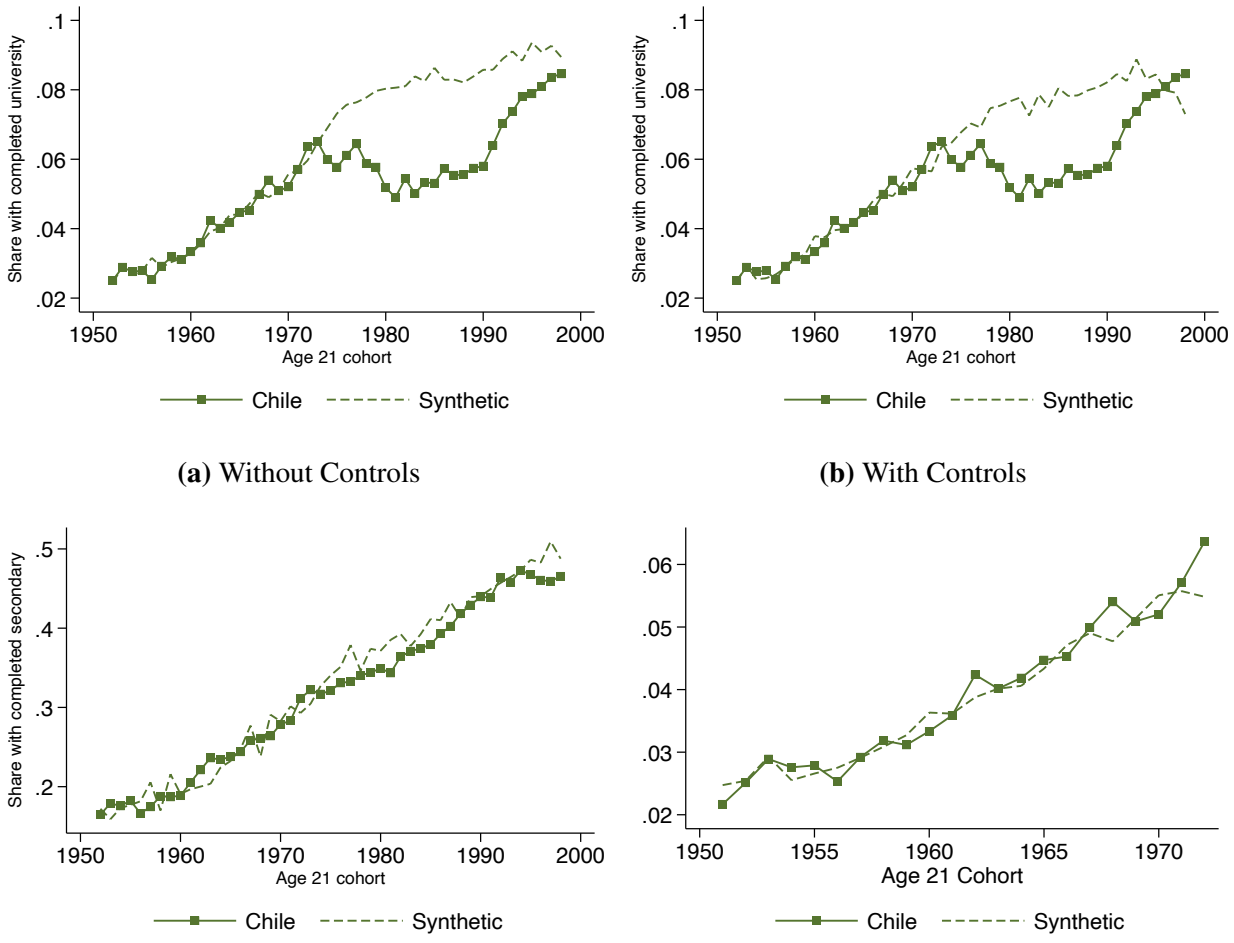


(a) Full Secondary Education

(b) Any College

Notes: 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. Sample in panel (b) is restricted to census respondents with 4+ years of secondary education.

Figure 6: Educational Attainment: Synthetic Control



(a) Without Controls

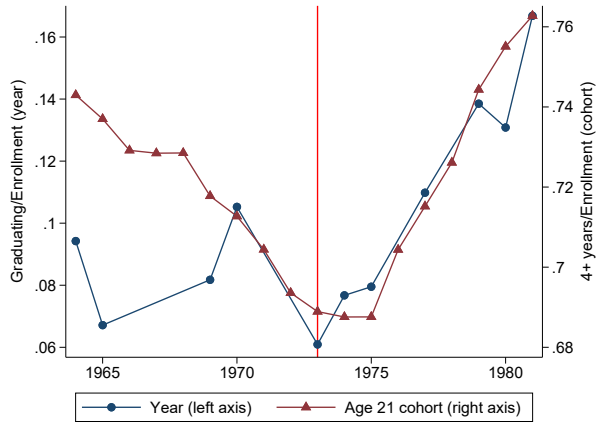
(b) With Controls

(c) Secondary Education

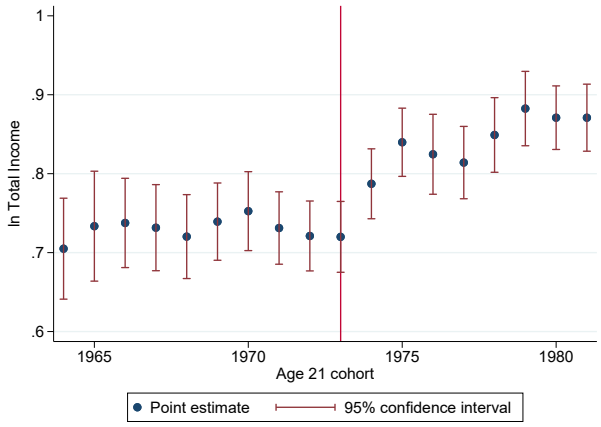
(d) Placebo

Notes: 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 7: Post-Enrollment Outcomes



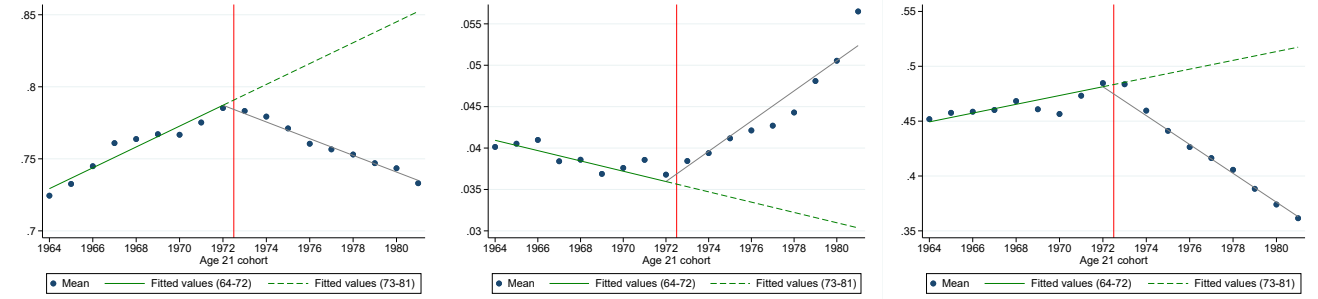
(a) Graduation Rate



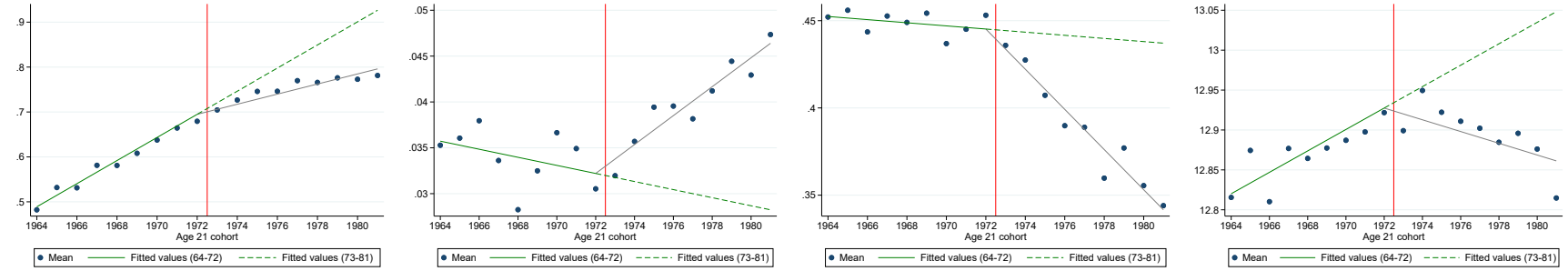
(b) Returns to Any College (OLS)

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 (right axis) show the number of people in the 1992 census that report 4+ years of college as a share of the 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 clustered by county of residence.

Figure 8: Visualization of Kink: Labor Market Outcomes



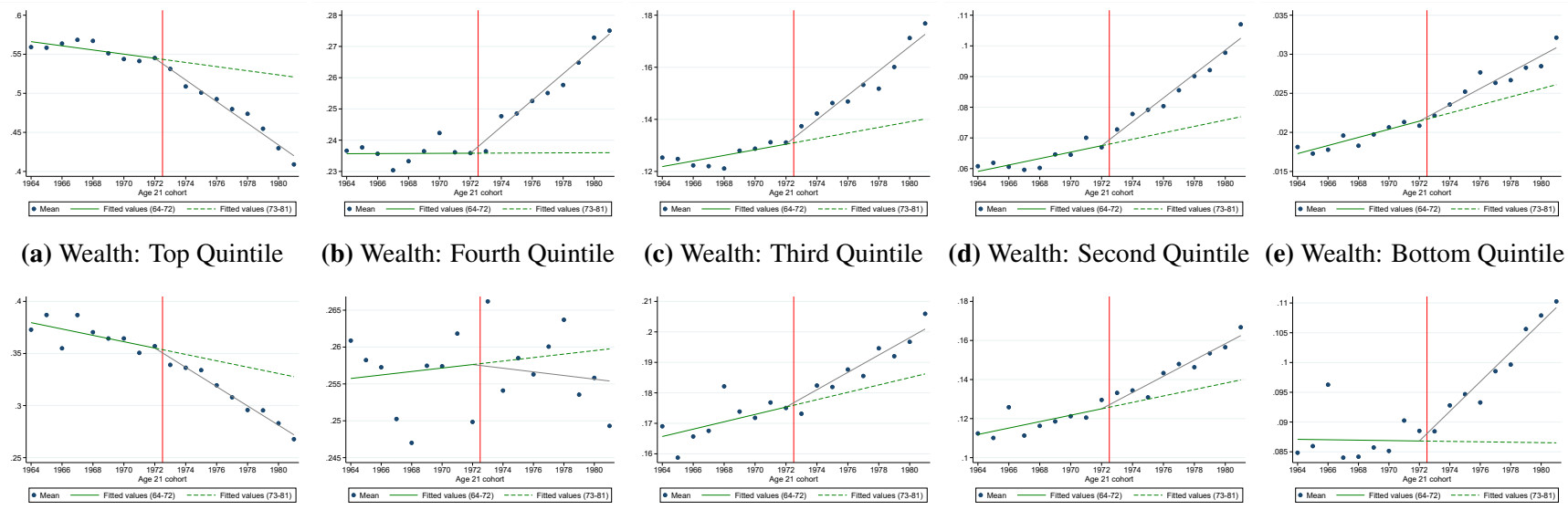
(a) In Labor Force (Census) **(b)** Seeking Work (Census) **(c)** WC-HS occupation (Census)



(d) In Labor Force (CASEN) **(e)** Seeking Work (CASEN) **(f)** WC-HS Occupation (CASEN) **(g)** Log Total Income (CASEN)

Notes: 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. Panels (a)-(c) use data from 1992 population census, while panels (d)-(g) use data from the CASEN survey between 1990 and 2017.

Figure 9: Visualization of Kink: Household Wealth and Income

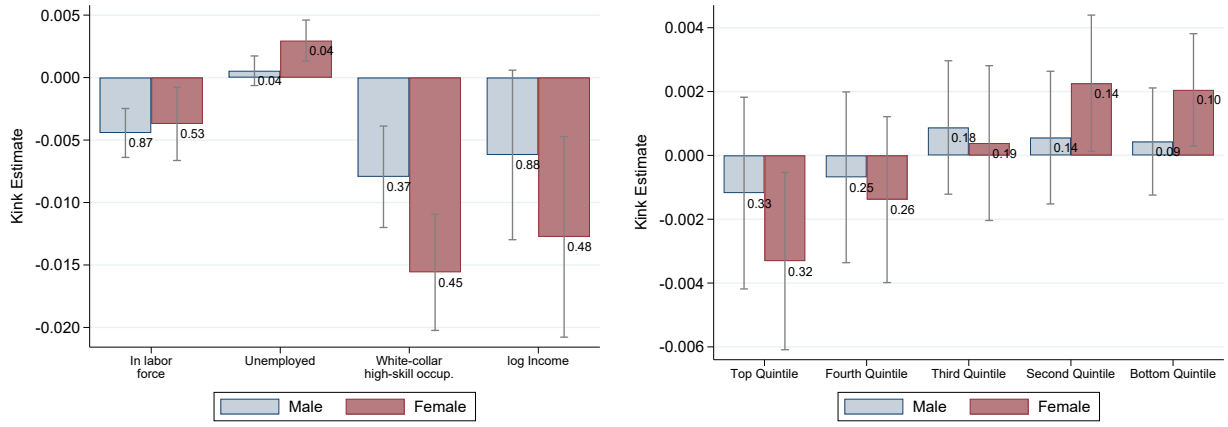


(a) Wealth: Top Quintile **(b)** Wealth: Fourth Quintile **(c)** Wealth: Third Quintile **(d)** Wealth: Second Quintile **(e)** Wealth: Bottom Quintile

(f) Income: Top Quintile **(g)** Income: Fourth Quintile **(h)** Income: Third Quintile **(i)** Income: Second Quintile **(j)** Income: Bottom Quintile

Notes: 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. Panels (a)-(e) use data from 1992 population census, while panels (f)-(j) use data from the CASEN survey between 1990 and 2017.

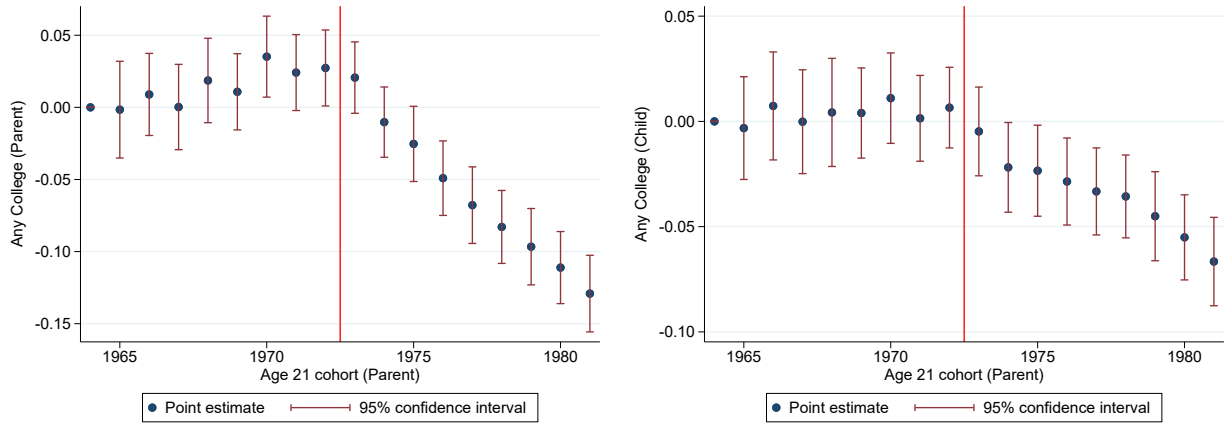
Figure 10: Heterogeneous Effects by Gender



(a) Labor Market Outcomes **(b)** Income Quintiles (Household)

Notes: Graph shows gender-specific estimates and 95% confidence intervals of the kink in the variable in the caption for the cohorts reaching age 21 after 1973. The number next to each bar indicates the sample mean. Sample includes respondents from the CASEN survey reaching age 21 between 1964 and 1981 and reporting 4+ years of secondary education. The gender-specific interaction term 'Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the regressor of interest, where "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. All regressions include gender-specific county, age, and year fixed effects. Standard errors clustered by county of residence. 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 point estimates and 95% confidence intervals 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.

Table 1: Educational Attainment

	Secondary Education		Any College Education				Any Higher Education
	Any	Full					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Yr Age 21	0.010***	0.008***	0.008***	0.018***	0.015***	0.020***	0.019***
	(0.0006)	(0.0003)	(0.0004)	(0.0004)	(0.0005)	(0.0021)	(0.0003)
	[0.001]	[0.002]	[0.000]	[0.001]	[0.001]	[0.000]	[0.001]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.000	-0.001	-0.012***	-0.036***	-0.033***	-0.041***	-0.030***
	(0.0004)	(0.0005)	(0.0007)	(0.0007)	(0.0007)	(0.0030)	(0.0006)
	[0.932]	[0.707]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Yr Age 21 x 𝟙(Female)					0.006***		
					(0.0006)		
					[0.003]		
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973) x 𝟙(Female)					-0.007***		
					(0.0008)		
					[0.002]		
Sample	Full	Full	Full	4+ Years of Secondary Education			
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family FE	No	No	No	No	No	Yes	No
Observations	2,982,951	2,982,951	2,982,951	1,024,570	1,024,570	42,649	1,024,570
R-squared	0.119	0.088	0.046	0.040	0.040	0.647	0.034
Mean DV	0.540	0.343	0.101	0.295	-	0.293	0.379
Mean DV (Male)	-	-	-	-	0.315	-	-
Mean DV (Female)	-	-	-	-	0.276	-	-

Notes: Dependent variable in the header. Sample includes census respondents born between 1943 and 1960. "Yr Age 21" is a continuous variable indicating the year when the cohort reached age 21, normalized to zero in 1972. 𝟙(Yr Age 21 ≥ 1973)" is 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 at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 2: Labor Market Outcomes

	In Labor Force	Seeking Work	White-collar High-skill Occupation	Log Total Income
	(1)	(2)	(3)	(4)
		Panel A: Census 1992		
Yr Age 21	0.008***	-0.001***	0.004***	
	(0.0003)	(0.0001)	(0.0005)	
	[0.000]	[0.004]	[0.004]	
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.012***	0.003***	-0.017***	
	(0.0006)	(0.0002)	(0.0008)	
	[0.000]	[0.003]	[0.000]	
		Panel B: CASEN survey (1990-2017)		
Yr Age 21	0.027***	-0.000	0.000	0.016***
	(0.0008)	(0.0004)	(0.0011)	(0.0017)
	[0.000]	[0.114]	[0.425]	[0.000]
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.016***	0.002***	-0.011***	-0.023***
	(0.0009)	(0.0005)	(0.0015)	(0.0023)
	[0.001]	[0.007]	[0.000]	[0.001]
		Panel C: CASEN survey (1990-2017) w/ Age FE		
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.004***	0.002***	-0.011***	-0.009***
	(0.0009)	(0.0005)	(0.0016)	(0.0027)
	[0.000]	[0.020]	[0.000]	[0.026]
Panel A:				
County of birth x gender FE	Yes	Yes	Yes	-
Observations	1,024,570	776,304	770,652	-
R-squared	0.200	0.004	0.032	-
Mean DV	0.758	0.043	0.431	-
Panels B and C:				
County of residence x gender FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Observations	163,693	114,790	104,061	135,152
R-squared [Panel B]	0.223	0.013	0.064	0.155
R-squared [Panel C]	0.248	0.013	0.065	0.163
Mean DV	0.701	0.039	0.403	709,631

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. Income in column 4 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. Standard errors clustered by county (panel A: birth; B/C: residence) in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Household Wealth and Income

	Household's wealth or income quintile (dummy)				
	Q5 (highest)	Q4	Q3	Q2	Q1 (lowest)
	(1)	(2)	(3)	(4)	(5)
		Panel A: Wealth (Census 1992)			
Yr Age 21	-0.002***	-0.000	0.001***	0.001***	0.000***
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0001)
	[0.010]	[0.392]	[0.017]	[0.014]	[0.004]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.013***	0.004***	0.004***	0.004***	0.001***
	(0.0007)	(0.0006)	(0.0004)	(0.0003)	(0.0001)
	[0.003]	[0.001]	[0.003]	[0.001]	[0.000]
		Panel B: Income (CASEN 1990-2017)			
Yr Age 21	-0.002**	0.000	0.001*	0.001***	-0.000
	(0.0008)	(0.0006)	(0.0006)	(0.0004)	(0.0004)
	[0.077]	[0.833]	[0.096]	[0.073]	[0.419]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.006***	-0.001	0.002*	0.003***	0.003***
	(0.0011)	(0.0010)	(0.0008)	(0.0007)	(0.0006)
	[0.002]	[0.532]	[0.118]	[0.002]	[0.001]
		Panel C: Income (CASEN 1990-2017) w/ Age FE			
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.002**	-0.001	0.001	0.001*	0.001*
	(0.0011)	(0.0010)	(0.0009)	(0.0008)	(0.0007)
	[0.047]	[0.478]	[0.533]	[0.041]	[0.055]
Panel A:					
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes
Observations	1,007,957	1,007,957	1,007,957	1,007,957	1,007,957
R-squared	0.114	0.013	0.032	0.052	0.050
Mean DV	0.500	0.250	0.145	0.080	0.024
Panels B and C:					
County of residence x gender FE	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes
Observations	163,342	163,342	163,342	163,342	163,342
R-squared [Panel B]	0.080	0.012	0.016	0.024	0.028
R-squared [Panel C]	0.084	0.013	0.017	0.026	0.031
Mean DV	0.327	0.257	0.184	0.137	0.096

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. Standard errors clustered by county (panel A: birth; B/C: residence) in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Educational Attainment of Children

	(1)	(2)	(3)	(4)	(5)
			PANEL A: Any College (Parent)		
Yr Age 21 Parent	0.006***	0.006***	0.006***	0.004***	0.004***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
	[0.001]	[0.001]	[0.001]	[0.009]	[0.011]
Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	-0.021***	-0.021***	-0.020***	-0.022***	-0.021***
	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
			PANEL B: Any College (Child)		
Yr Age 21 Parent	0.004***	0.004***	0.004***	-0.000	-0.001
	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0008)
	[0.005]	[0.005]	[0.005]	[0.742]	[0.456]
Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	-0.005***	-0.005***	-0.005***	-0.007***	-0.006***
	(0.0013)	(0.0013)	(0.0013)	(0.0012)	(0.0011)
	[0.006]	[0.005]	[0.005]	[0.004]	[0.003]
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes
Parent's gender x gender FE	No	Yes	Yes	Yes	Yes
Relationship to HH head FE	No	No	Yes	Yes	Yes
Age FE	No	No	No	Yes	Yes
Full secondary FE	No	No	No	No	Yes
Observations	233,136	233,136	233,136	233,136	233,136
R-squared (panel A)	0.085	0.087	0.088	0.095	0.099
R-squared (panel B)	0.044	0.045	0.046	0.063	0.132
Mean DV (Panel A)	0.309	0.309	0.309	0.309	0.309
Mean DV (Panel B)	0.582	0.582	0.582	0.582	0.582

Notes: 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 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, while $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973) is a dummy for parents that reached age 21 on or after 1973. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Dictatorship, Higher Education and Social Mobility: Appendix for online publication

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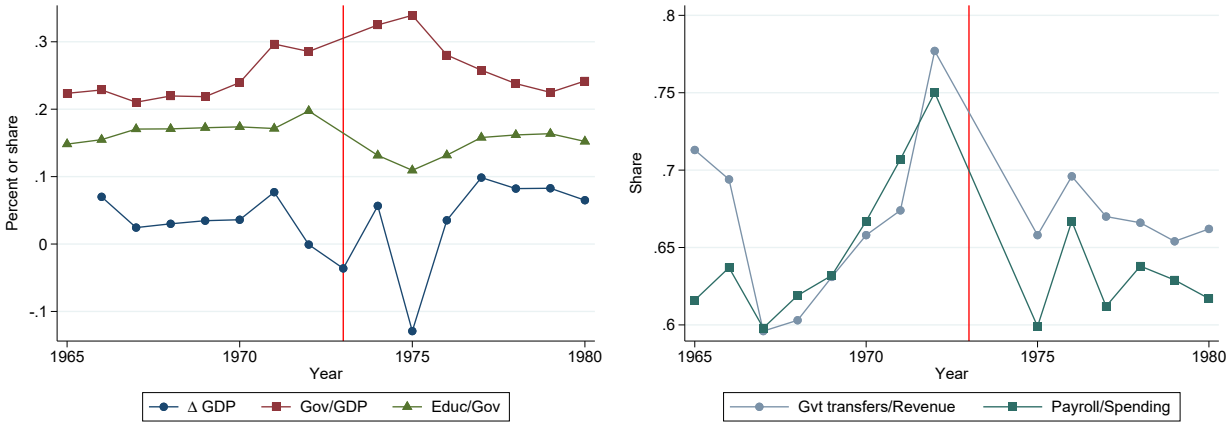
Appendix A Additional Background Figures

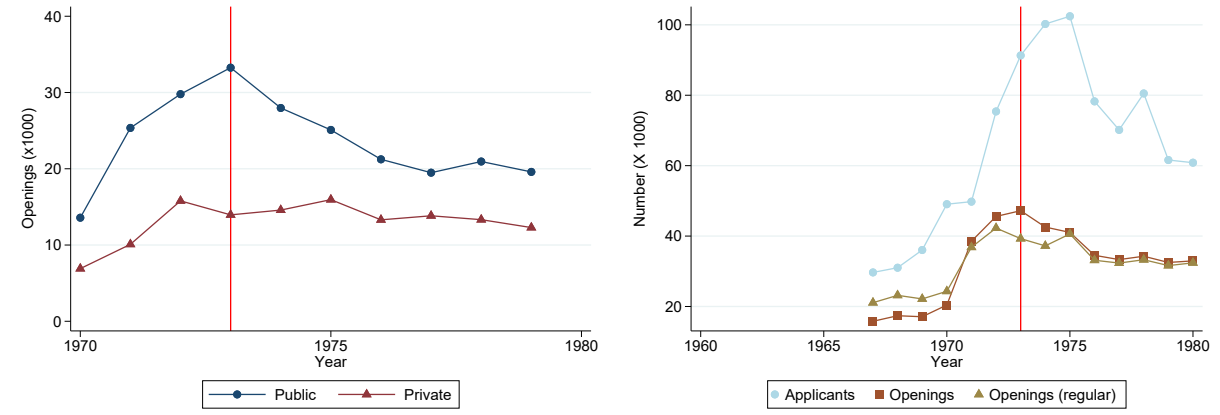
Figure A1: Public Spending on Education



(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).

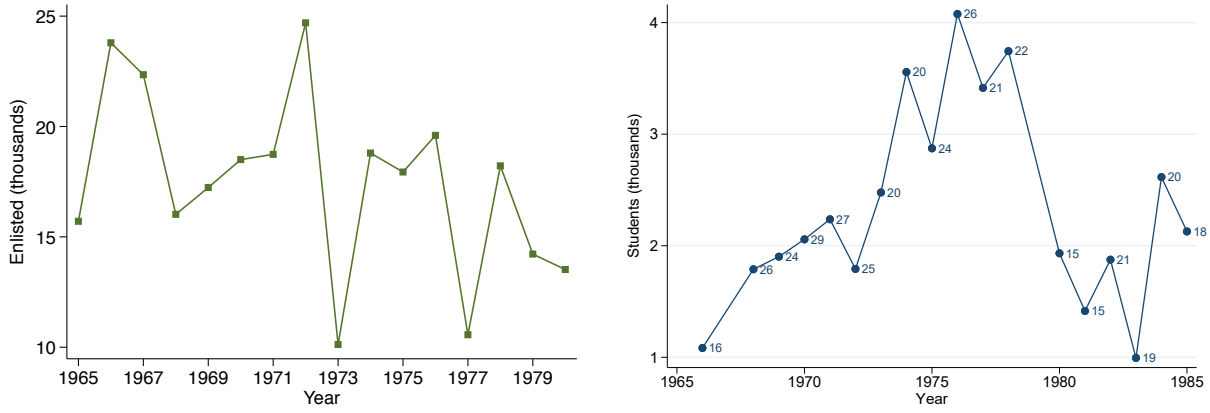
Figure A2: Further Evidence on Supply and Demand for College



(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.

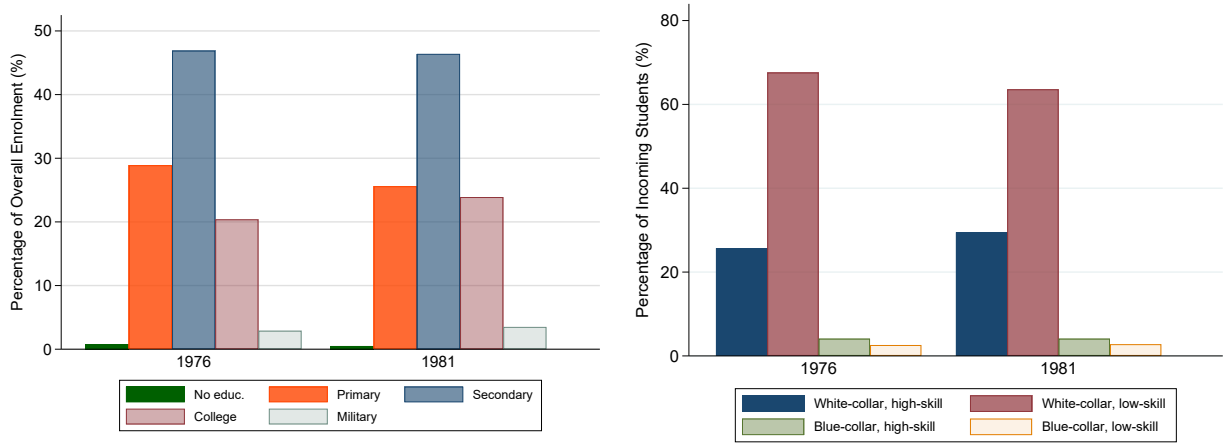
Figure A3: Alternative Mechanisms



(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.

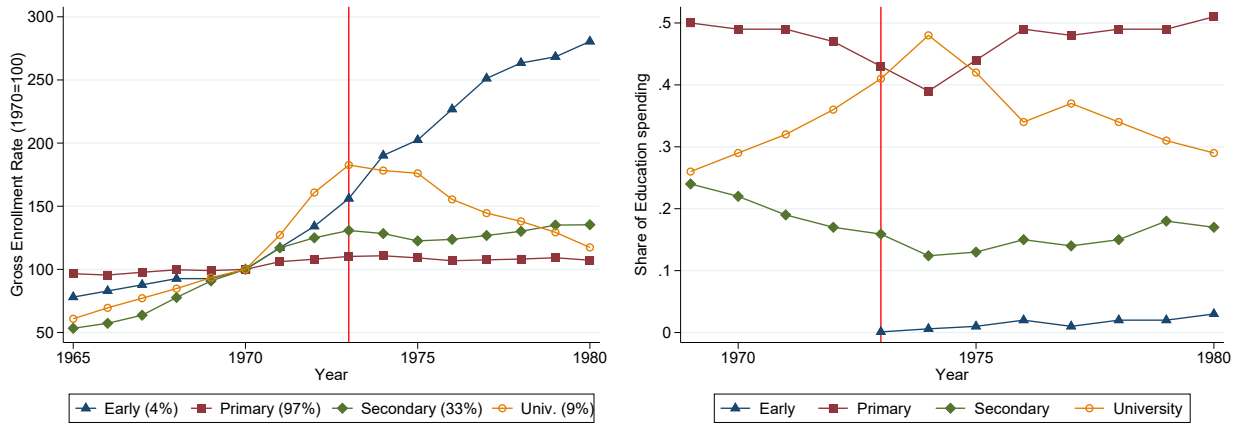
Figure A4: Enrollment Shares by Characteristics of Father



(a) Education **(b)** Occupation

Notes: Panel (a) shows the share of incoming college students in 1976 and 1981, disaggregated by the educational attainment of the father. Panel (b) shows the occupation of the father for admitted students in these years. Source: PIIE (1984).

Figure A5: Educational Spending and Enrollment

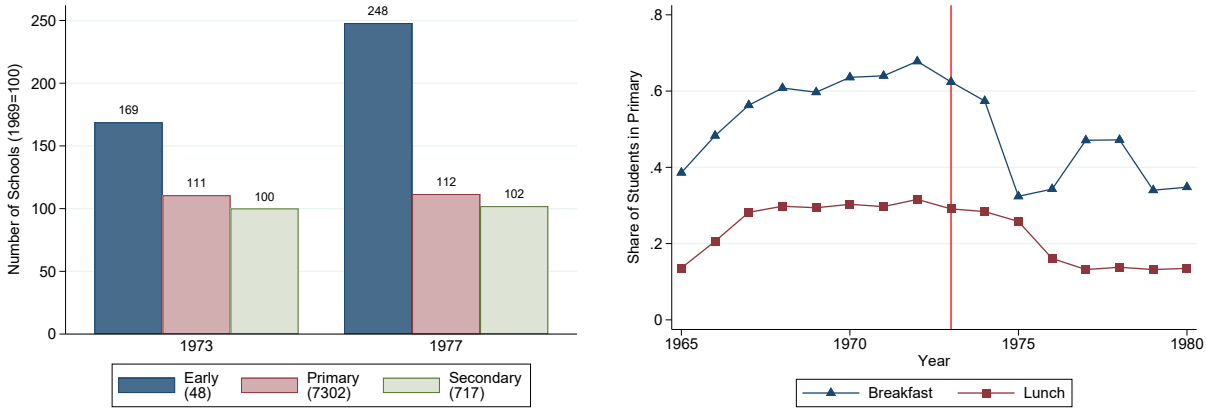


(a) Gross Enrollment Rate (Index)

(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 A6: Other Outcomes: Lower Levels

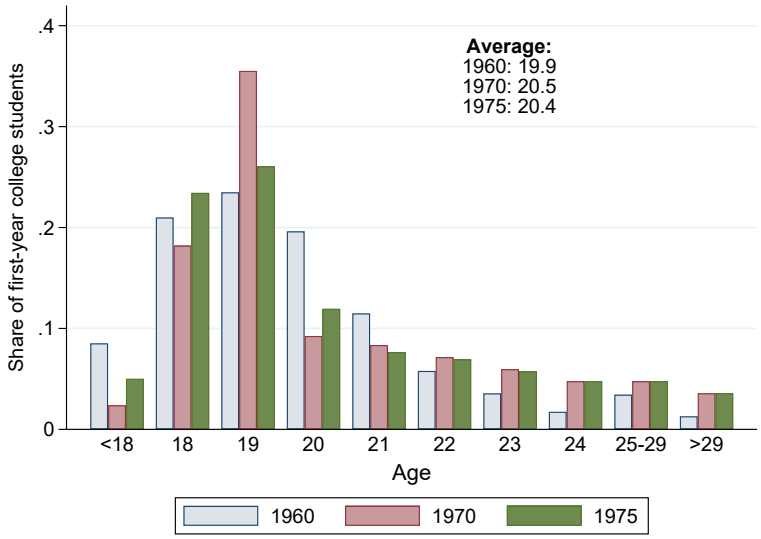


(a) Number of Schools

(b) School Meals

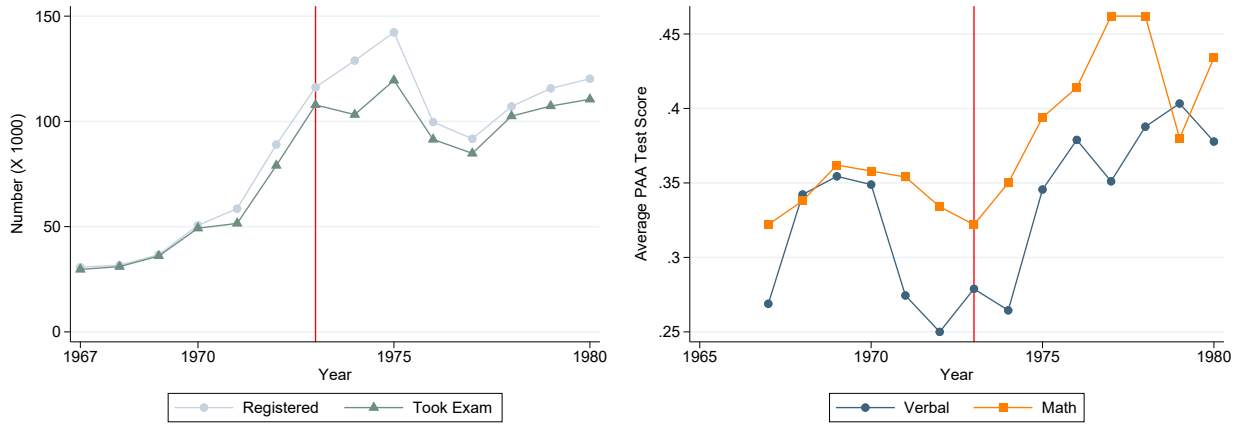
Notes: Panel (a) shows the number of schools per level (early, primary, secondary) in 1973 and 1977, relative to 1969 (normalized to 100). Panel (b) shows the yearly share of primary students receiving either free breakfast (triangle markers) or lunch (square markers). Sources: Echeverría (1980); PIIE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

Figure A7: 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 A8: Additional Information on PAA Test



(a) PAA Test-takers

(b) Average test scores

Notes: Panel (a) shows the yearly number of students that registered for the PAA testand the number that actually took the test. Panel (b) 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. Sources: Díaz and Himmel (1985); Universidad de Chile (2011).

Appendix B Additional Information on Data Sources

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.

Other sources: We use data from the Integrated Public Use Micro-data Series (IPUMS) for the synthetic control analysis. Harmonized data is available for 57 countries (see Table B1 for details). The 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 B1: 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

Appendix C Educational Attainment: Additional Results

Table C1: College Enrollment: Other Sources

Source	Dependent variable: Any College			
	CASEN 1990-2017		Census 2002	Census 2017
	(1)	(2)	(3)	(4)
Yr Age 21	0.011***	0.011***	0.012***	0.007***
	(0.0007)	(0.0007)	(0.0004)	(0.0004)
	[0.001]	[0.001]	[0.001]	[0.001]
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.024***	-0.024***	-0.025***	-0.018***
	(0.0011)	(0.0011)	(0.0008)	(0.0007)
	[0.000]	[0.000]	[0.000]	[0.000]
County x gender FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
Observations	163,693	163,693	1,192,851	1,036,105
R-squared	0.057	0.059	0.035	0.037
Mean DV	0.261	0.261	0.325	0.300

Notes: Sample includes survey/census respondents born between 1943 and 1960 and reporting 4+ years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972, while $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is 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 residence in columns 1-2 and of birth in columns 3-4. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table C2: College Enrollment: Different Kink Points

Kink point (x):	Dependent variable: Any college				
	1971	1972	1973	1974	1975
	(1)	(2)	(3)	(4)	(5)
Yr Age 21	0.024***	0.021***	0.018***	0.015***	0.011***
	(0.0006)	(0.0005)	(0.0004)	(0.0004)	(0.0003)
	[0.006]	[0.002]	[0.001]	[0.000]	[0.000]
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ x)	-0.037***	-0.037***	-0.036***	-0.036***	-0.035***
	(0.0008)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
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	0.037	0.039	0.040	0.040	0.039
Mean DV	0.295	0.295	0.295	0.295	0.295

Notes: Sample includes all respondents of the 1992 census born between 1943 and 1960 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, while $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is 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-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table C3: College Enrollment: Within-Household Estimates

	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)
Yr Age 21	0.021***	0.018***	0.012**	0.010***	0.015	0.007**
	(0.0028)	(0.0034)	(0.0048)	(0.0033)	(0.0108)	(0.0035)
	[0.000]	[0.000]	[0.001]	[0.002]	[0.066]	[0.011]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.043***	-0.038***	-0.029***	-0.022***	-0.034**	-0.020***
	(0.0038)	(0.0050)	(0.0061)	(0.0048)	(0.0143)	(0.0048)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]	[0.001]
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27,518	14,986	14,412	14,133	4,955	20,658
R-squared	0.653	0.667	0.655	0.670	0.705	0.672
Mean DV	0.287	0.304	0.304	0.323	0.289	0.309

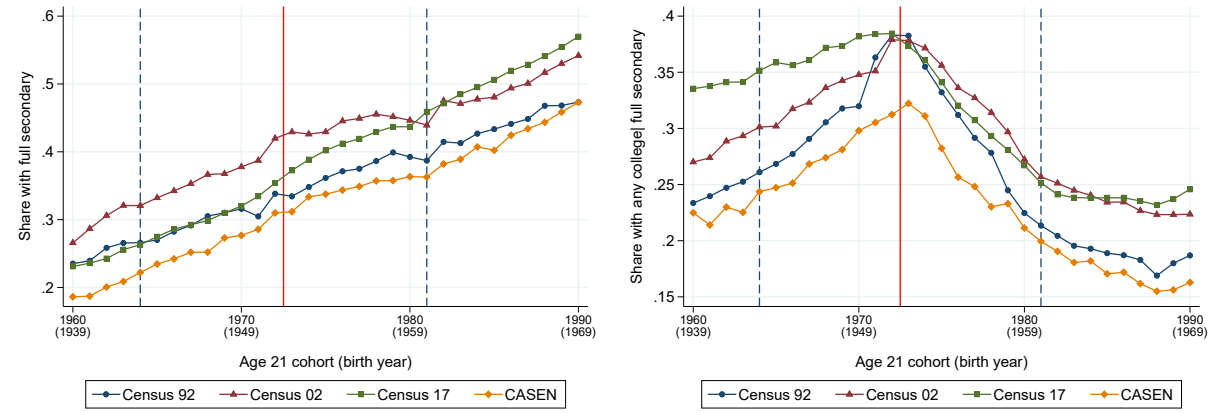
Notes: Sample includes all census respondents from cohorts born between 1943 and 1960, reporting four or more years of secondary education (media). Odd-numbered columns include household heads and respondents classified as siblings. Even-numbered columns include respondents classified as children of 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, while 𝟙(Yr Age 21 ≥ 1973)" is 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-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table C4: College Enrollment: Within-Wealth-Quintile Estimates

	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)
Yr Age 21	0.020***	0.018***	0.017***	0.015***	0.013***
	(0.0005)	(0.0007)	(0.0009)	(0.0008)	(0.0012)
	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.037***	-0.031***	-0.030***	-0.027***	-0.026***
	(0.0009)	(0.0012)	(0.0014)	(0.0013)	(0.0018)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]
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
Mean DV	0.413	0.209	0.165	0.127	0.125

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

Figure C1: College Enrollment: Different Sources

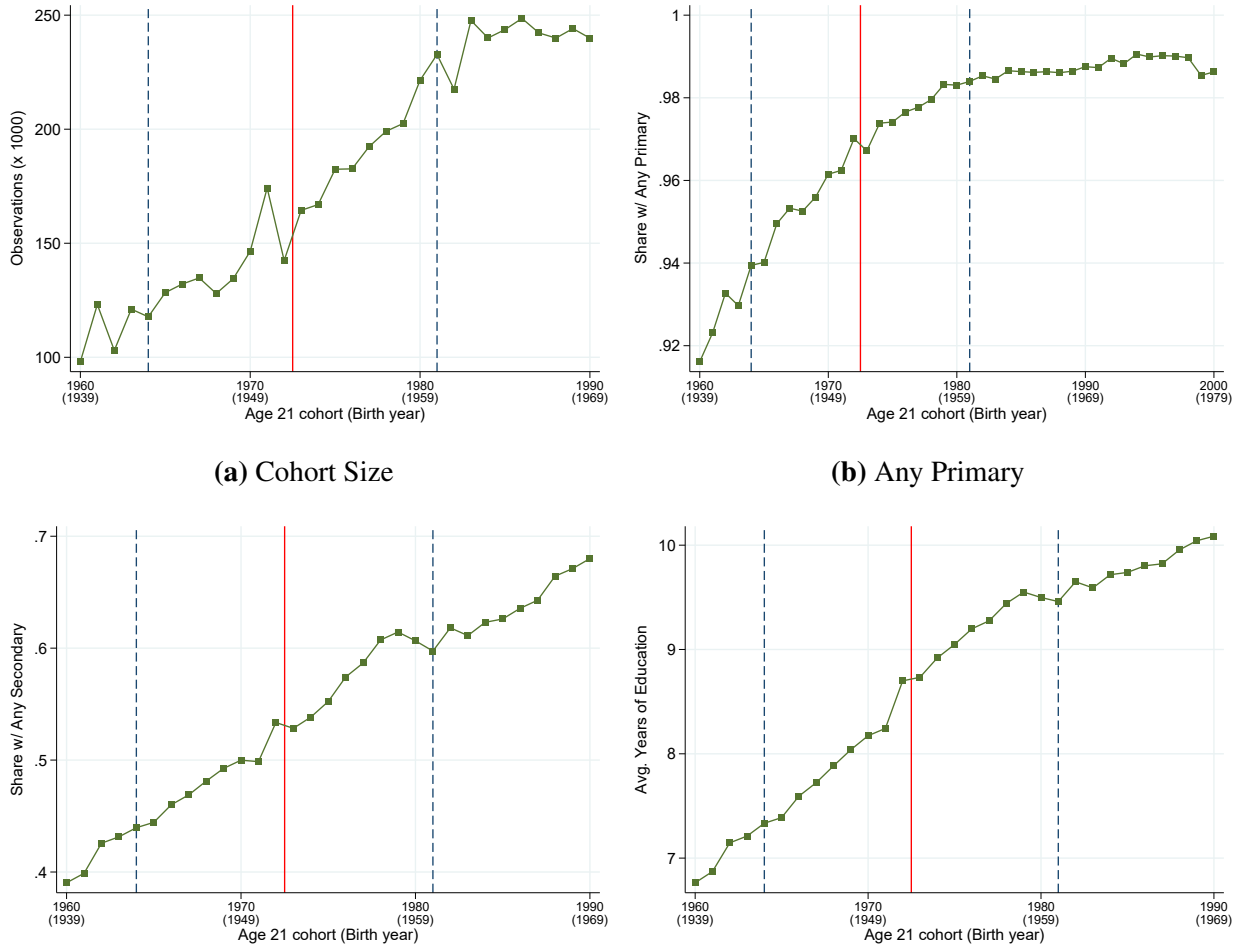


(a) Share with 4+ years secondary

(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 C2: Educational Attainment: Raw Data



(a) Cohort Size

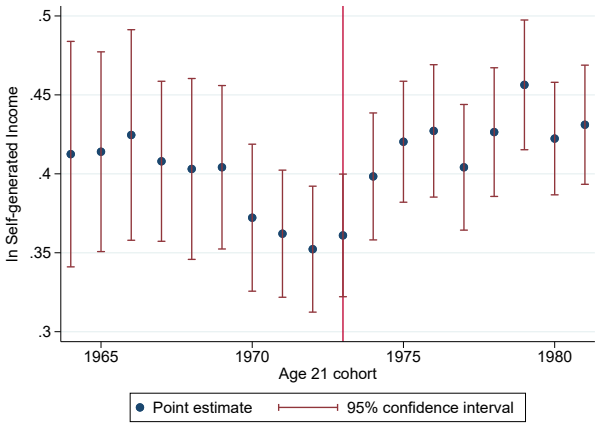
(b) Any Primary

(c) Any Secondary

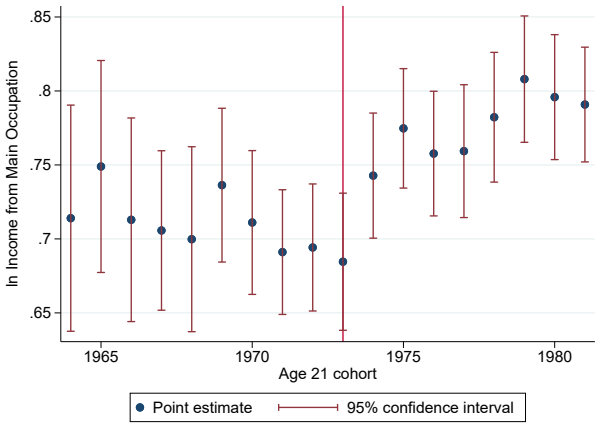
(d) Years of Education

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.

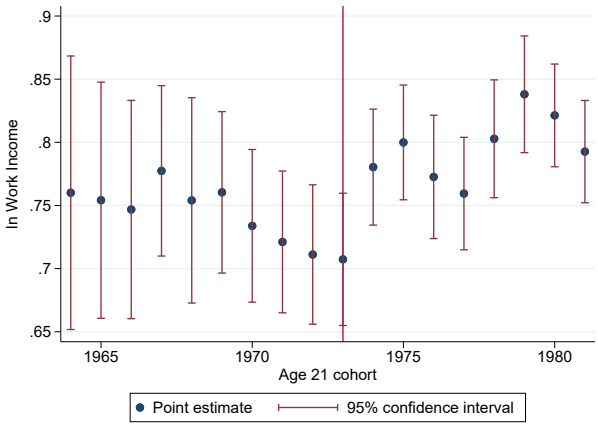
Figure C3: Cohort-specific Estimates of the College Premium



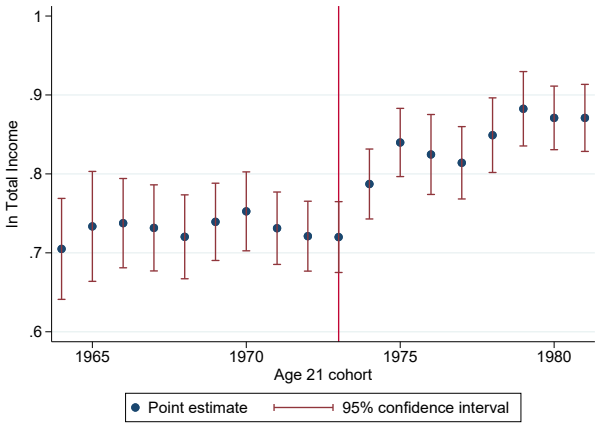
(a) Self-generated (w/ occup. FE)



(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.

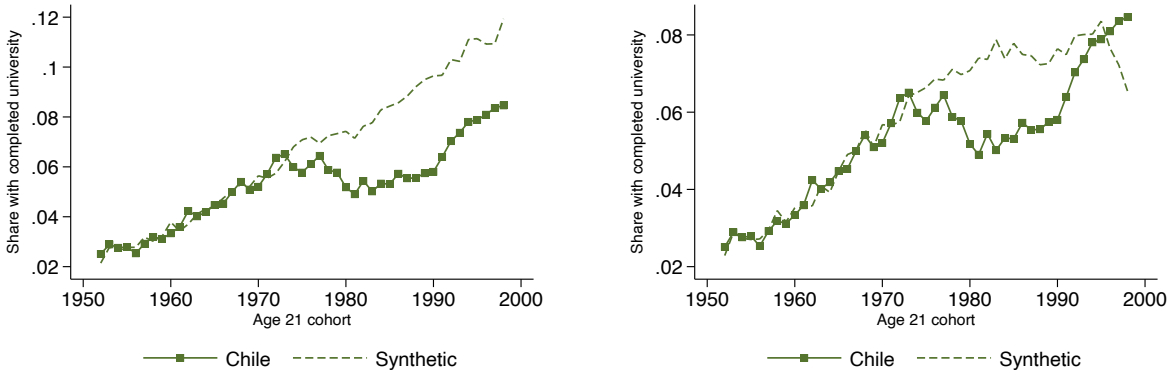
Appendix D Synthetic Control: Additional Results

Table D1: Robustness checks to the synthetic control analysis

Sample:	R^2	Average effect	p-value Unrestricted	p-value Restricted
Panel A: Using even pre-treatment period outcomes for 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.

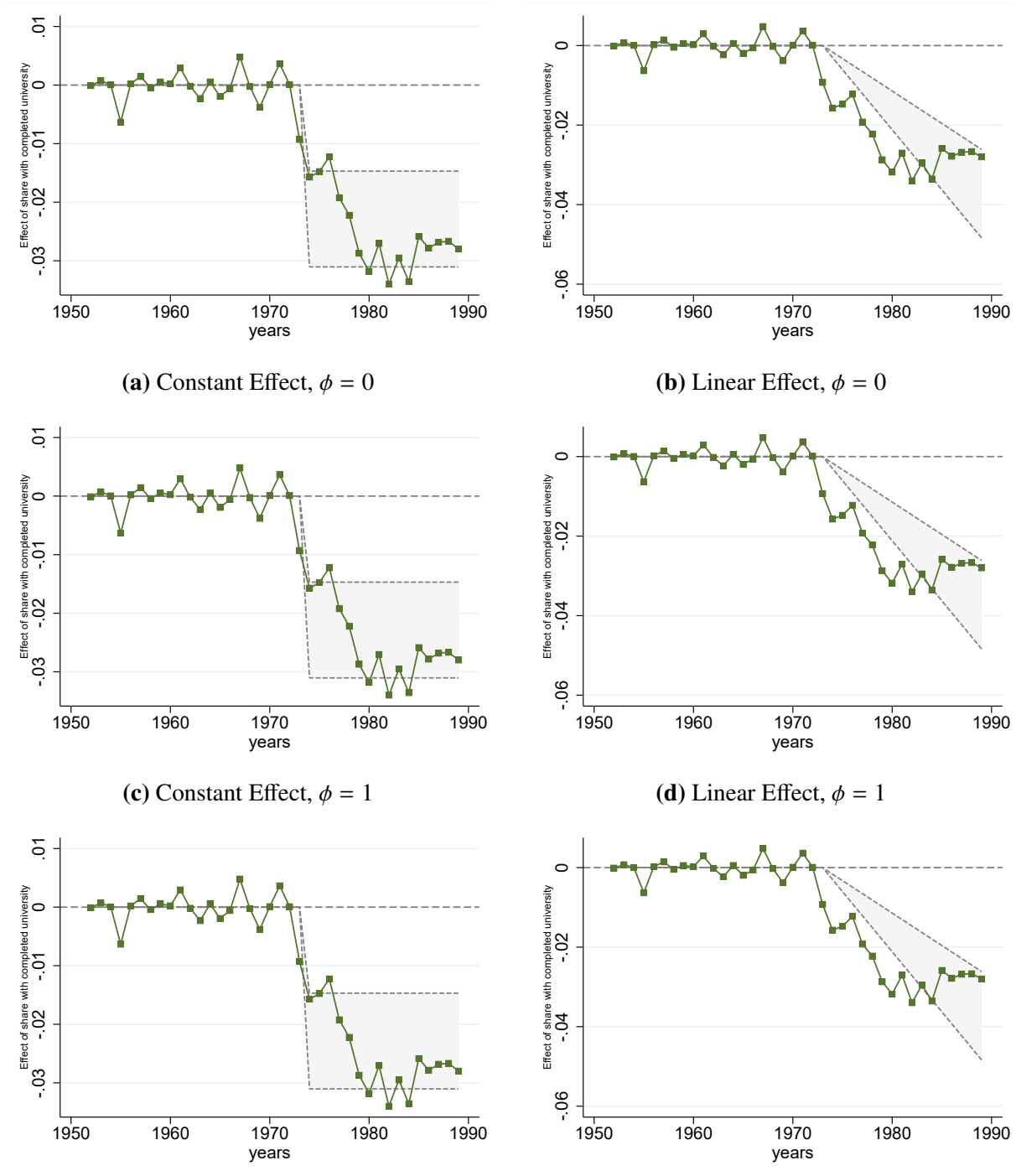
Figure D1: Robustness of Synthetic Control Analysis



(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.

Figure D2: Confidence Sets for Latin America



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

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

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

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

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

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

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

Appendix E Economic Consequences: Additional Results

Table E1: Labor Market Outcomes: Census 2002

	In Labor Force			Seeking Work			White-collar high-skill occupation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Yr Age 21	0.017***	0.012***		0.000	-0.000**		-0.004***	-0.000	
	(0.0004)	(0.0002)		(0.0002)	(0.0001)		(0.0004)	(0.0003)	
	[0.000]	[0.001]		[0.547]	[0.046]		[0.000]	[0.981]	
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.014***	-0.013***	-0.006***	0.000**	0.001***	0.001***	-0.006***	-0.011***	-0.011***
	(0.0004)	(0.0003)	(0.0005)	(0.0002)	(0.0002)	(0.0002)	(0.0005)	(0.0005)	(0.0008)
	[0.001]	[0.001]	[0.002]	[0.213]	[0.002]	[0.001]	[0.001]	[0.000]	[0.002]
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Age FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,192,851	2,217,491	2,217,491	909,204	1,685,569	1,685,569	872,783	1,643,495	1,643,495
R-squared	0.133	0.158	0.160	0.004	0.009	0.009	0.022	0.051	0.052
Sample (census)	02	92/02	92/02	02	92/02	92/02	02	92/02	92/02
Mean DV	0.762	0.760	0.760	0.0822	0.0641	0.0641	0.596	0.519	0.519

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table E2: Occupation and Employment Categories

	White-collar	Blue-collar		Military	Boss/	Salaried	Self-	Domestic	Helping
	Low-skill	High-skill	Low-skill		Owner	Employee	employed	Worker	Relative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	0.004***	0.004***	0.004***	-0.001***	0.004***	-0.007***	0.004***	0.001	0.000
	(0.0012)	(0.0012)	(0.0012)	(0.0003)	(0.0007)	(0.0013)	(0.0013)	(0.0004)	(0.0002)
	[0.000]	[0.008]	[0.066]	[0.002]	[0.001]	[0.002]	[0.019]	[0.267]	[0.639]
County of residence x gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	104,061	104,061	104,061	104,061	110,347	110,347	110,347	110,347	110,347
R-squared	0.064	0.063	0.055	0.044	0.031	0.039	0.038	0.056	0.021
Mean DV	0.227	0.154	0.209	0.00782	0.0662	0.661	0.228	0.0236	0.00710

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972, while 𝟙(Yr Age 21 ≥ 1973)" is a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of residence by gender, survey year and age fixed effects. Standard errors clustered by county of residence in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table E3: Other Income Measures

Dependent variable (log income):	Main Occupation		All Work		Self-generated	
	(1)	(2)	(3)	(4)	(5)	(6)
Yr Age 21	0.002		0.017***		0.014***	
	(0.0019)		(0.0024)		(0.0018)	
	[0.371]		[0.000]		[0.002]	
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.014***	-0.006**	-0.023***	-0.008***	-0.021***	-0.008***
	(0.0024)	(0.0025)	(0.0030)	(0.0031)	(0.0023)	(0.0025)
	[0.001]	[0.039]	[0.001]	[0.029]	[0.001]	[0.044]
County of residence x gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	No	Yes	No	Yes	No	Yes
Observations	107,536	107,536	102,008	102,008	131,133	131,133
R-squared	0.161	0.167	0.143	0.155	0.151	0.159
Mean DV	471,432	471,432	504,077	504,077	526,115	526,115

Notes: Dependent variable in the header. Income deflated using yearly CPI. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972, while $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is a dummy for cohorts that reached age 21 on or after 1973. All regressions include county of residence by gender, survey year and age fixed effects. Standard errors clustered by county of residence in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table E4: Labor Market Outcomes: Unrestricted Sample

	In Labor Force	Seeking Work	White-collar High-skill Occupation	Log Total Income
	(1)	(2)	(3)	(4)
		Panel A: Census 1992		
Yr Age 21	0.007***	-0.001***	0.004***	
	(0.0002)	(0.0001)	(0.0002)	
	[0.000]	[0.004]	[0.003]	
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.009***	0.002***	-0.009***	
	(0.0004)	(0.0002)	(0.0003)	
	[0.001]	[0.003]	[0.000]	
		Panel B: CASEN survey (1990-2017)		
Yr Age 21	0.024***	0.000	0.002***	0.015***
	(0.0005)	(0.0002)	(0.0005)	(0.0011)
	[0.000]	[0.840]	[0.001]	[0.000]
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.013***	0.001***	-0.006***	-0.016***
	(0.0005)	(0.0003)	(0.0007)	(0.0014)
	[0.001]	[0.007]	[0.000]	[0.003]
		Panel C: CASEN survey (1990-2017) w/ Age FE		
Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)	-0.001**	0.001***	-0.007***	-0.008***
	(0.0005)	(0.0003)	(0.0007)	(0.0016)
	[0.120]	[0.028]	[0.000]	[0.017]
Panel A:				
County of birth x gender FE	Yes	Yes	Yes	-
Observations	2,982,951	1,873,045	1,842,799	-
R-squared	0.333	0.004	0.056	-
Mean DV	0.628	0.057	0.208	-
Panels B and C:				
County of residence x gender FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Observations	513,582	308,732	278,032	396,935
R-squared [Panel B]	0.304	0.012	0.066	0.186
R-squared [Panel C]	0.320	0.012	0.067	0.189
Mean DV	0.601	0.048	0.189	404,278

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960. Income in column 4 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. Standard errors clustered by county (panel A: birth; B/C: residence) in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table E5: Household Wealth and Income: Unrestricted Sample

	Household's wealth or income quintile (dummy)				
	Q5 (highest)	Q4	Q3	Q2	Q1 (lowest)
	(1)	(2)	(3)	(4)	(5)
			Panel A: Wealth (Census 1992)		
Yr Age 21	0.002***	-0.001***	-0.001***	-0.000	0.000
	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
	[0.035]	[0.003]	[0.010]	[0.608]	[0.748]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.007***	0.000	0.002***	0.003***	0.003***
	(0.0005)	(0.0004)	(0.0003)	(0.0002)	(0.0003)
	[0.005]	[0.915]	[0.011]	[0.007]	[0.006]
			Panel B: Income (CASEN 1990-2017)		
Yr Age 21	0.001***	0.001***	-0.001*	-0.000	-0.002***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
	[0.018]	[0.001]	[0.128]	[0.508]	[0.038]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.004***	-0.003***	0.000	0.002***	0.005***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
	[0.002]	[0.001]	[0.670]	[0.022]	[0.001]
			Panel C: Income (CASEN 1990-2017) w/ Age FE		
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)	-0.002***	-0.001	0.001**	0.001**	0.000
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
	[0.020]	[0.082]	[0.022]	[0.045]	[0.437]
Panel A:					
County of birth x gender FE	Yes	Yes	Yes	Yes	Yes
Observations	2,938,505	2,938,505	2,938,505	2,938,505	2,938,505
R-squared	0.074	0.021	0.008	0.014	0.069
Mean DV	0.241	0.212	0.193	0.180	0.175
Panels B and C:					
County of residence x gender FE	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes
Observations	511,927	511,927	511,927	511,927	511,927
R-squared [Panel B]	0.080	0.012	0.016	0.024	0.028
R-squared [Panel C]	0.076	0.022	0.008	0.014	0.074
Mean DV	0.148	0.185	0.202	0.223	0.242

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. Standard errors clustered by county (panel A: birth; B/C: residence) in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table E6: Labor Market Outcomes: Heterogeneous Effects by Gender

Dependent variable:	In Labor Force	Seeking Work	White-collar High-skill Occupation	Log Total Income
	(1)	(2)	(3)	(4)
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973) x 𝟙(Male)	-0.004***	0.001	-0.008***	-0.006*
	(0.0010)	(0.0006)	(0.0021)	(0.0035)
	[0.001]	[0.375]	[0.035]	[0.022]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973) x 𝟙(Female)	-0.004**	0.003***	-0.016***	-0.013***
	(0.0015)	(0.0008)	(0.0024)	(0.0041)
	[0.022]	[0.019]	[0.000]	[0.054]
County of residence x gender FE	Yes	Yes	Yes	Yes
Survey year x gender FE	Yes	Yes	Yes	Yes
Age x gender FE	Yes	Yes	Yes	Yes
Observations	163,693	114,790	104,061	135,152
R-squared	0.251	0.014	0.065	0.164
Mean DV (Male)	0.874	0.035	0.372	880,000
Mean DV (Female)	0.533	0.044	0.452	480,000

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. Income in column 4 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 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 residence in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table E7: Household Income: Heterogeneous Effects by Gender

Dependent variable:	Household's income quintile (dummy)				
	Q5 (highest)	Q4	Q3	Q2	Q1 (lowest)
	(1)	(2)	(3)	(4)	(5)
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973) x 𝟙(Male)	-0.001	-0.001	0.001	0.001	0.000
	(0.0015)	(0.0014)	(0.0011)	(0.0011)	(0.0009)
	[0.429]	[0.688]	[0.277]	[0.502]	[0.621]
Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973) x 𝟙(Female)	-0.003**	-0.001	0.000	0.002**	0.002**
	(0.0014)	(0.0013)	(0.0012)	(0.0011)	(0.0009)
	[0.027]	[0.432]	[0.793]	[0.146]	[0.088]
County of residence x gender FE	Yes	Yes	Yes	Yes	Yes
Survey year x gender FE	Yes	Yes	Yes	Yes	Yes
Age x gender FE	Yes	Yes	Yes	Yes	Yes
Observations	163,342	163,342	163,342	163,342	163,342
R-squared	0.085	0.013	0.017	0.026	0.031
Mean DV (M)	0.332	0.255	0.183	0.136	0.0940
Mean DV (F)	0.322	0.258	0.185	0.139	0.0970

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972. "Yr Age 21 x 𝟙(Yr Age 21 ≥ 1973)" is the interaction of this variable with a dummy for cohorts that reached age 21 on or after 1973. Standard errors clustered by county of residence in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table E8: Labor Market Outcomes: Effect of College (IV and OLS)

Dependent variable:	In Labor Force	Seeking Work	White-collar High-skill Occupation	Log Total Income
	(1)	(2)	(3)	(4)
			Panel A: IV	
Any College	0.162***	-0.060***	0.412***	0.338***
	(0.0375)	(0.0199)	(0.0532)	(0.1037)
	[0.000]	[0.022]	[0.000]	[0.017]
			Panel B: OLS	
Any College	0.130***	-0.019***	0.566***	0.792***
	(0.0032)	(0.0012)	(0.0039)	(0.0141)
	[0.000]	[0.000]	[0.000]	[0.000]
County x gender FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Observations	163,693	114,790	104,061	135,152
Kleibergen-Paap F-stat [panel A]	459.5	376.9	343.0	435.7
R-squared [panel B]	0.263	0.015	0.327	0.263
Mean DV	0.701	0.039	0.403	709,631

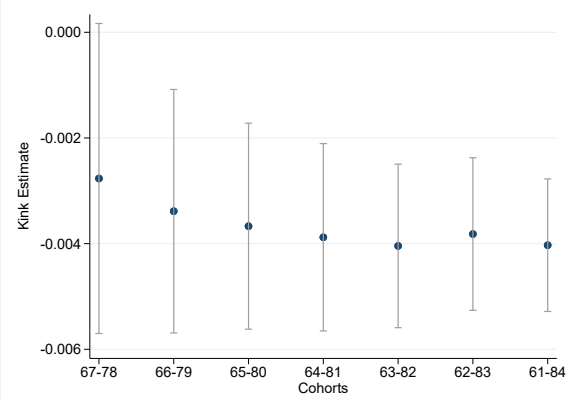
Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. Income in column 4 deflated using yearly CPI. . In panel A, "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the excluded instrument for college enrollment, where "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972 and "$\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is a dummy for cohorts that reached age 21 on or after 1973. Standard errors clustered by county of residence in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table E9: Household Income: Effect of College (IV and OLS)

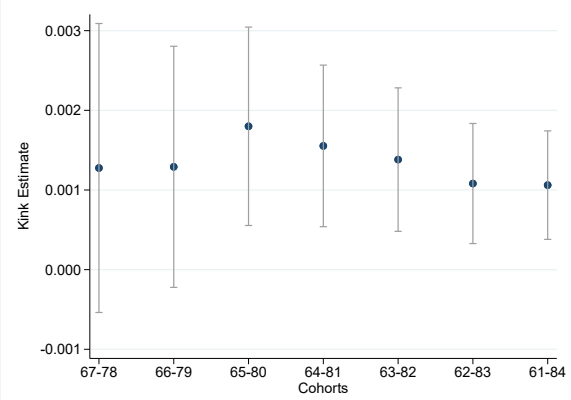
	Income quintile (dummy)				
	Q5 (highest)	Q4	Q3	Q2	Q1 (lowest)
	(1)	(2)	(3)	(4)	(5)
			Panel A: IV		
Any College	0.092**	0.044	-0.027	-0.057*	-0.052*
	(0.0435)	(0.0418)	(0.0370)	(0.0319)	(0.0275)
	[0.049]	[0.473]	[0.529]	[0.042]	[0.047]
			Panel B: OLS		
Any College	0.315***	-0.028***	-0.101***	-0.106***	-0.080***
	(0.0053)	(0.0067)	(0.0024)	(0.0027)	(0.0031)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
County of residence x gender FE	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Observations	163,342	163,342	163,342	163,342	163,342
Kleibergen-Paap F-stat [panel A]	461.8	461.8	461.8	461.8	461.8
R-squared [panel B]	0.166	0.014	0.029	0.043	0.044
Mean DV	0.327	0.257	0.184	0.137	0.096

Notes: Dependent variable in the header. Sample includes individuals born between 1943 and 1960 with 4+ years of secondary education. Income in column 4 deflated using yearly CPI. . In panel A, "Yr Age 21 x $\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is the excluded instrument for college enrollment, where "Yr Age 21" is a continuous variable indicating the year at which the cohort reached age 21, normalized to zero in 1972 and "$\mathbb{1}$(Yr Age 21 $\geq$ 1973)" is a dummy for cohorts that reached age 21 on or after 1973. Standard errors clustered by county of residence in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

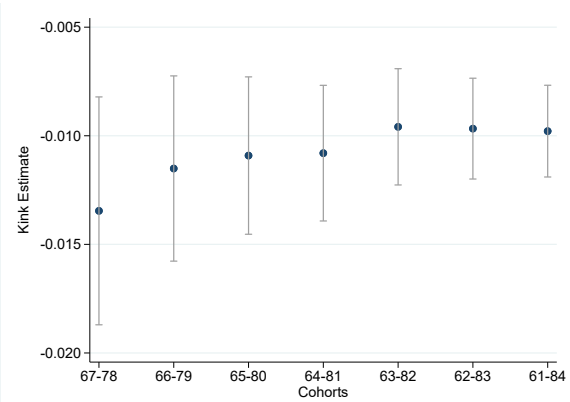
Figure E1: Labor Market Outcomes: Different Bandwidths



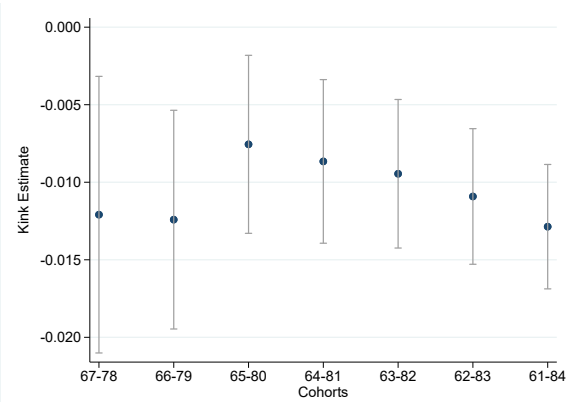
(a) Labor Force Participation



(b) Unemployment



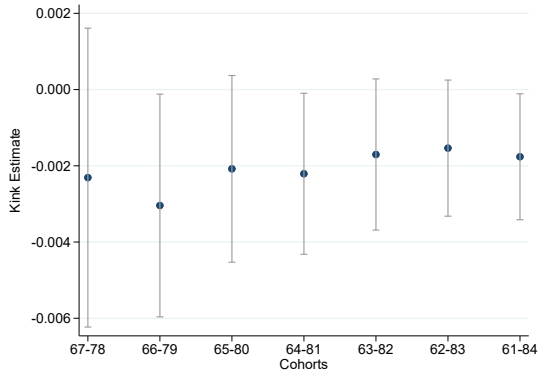
(c) White-Collar High-Skill Occup.



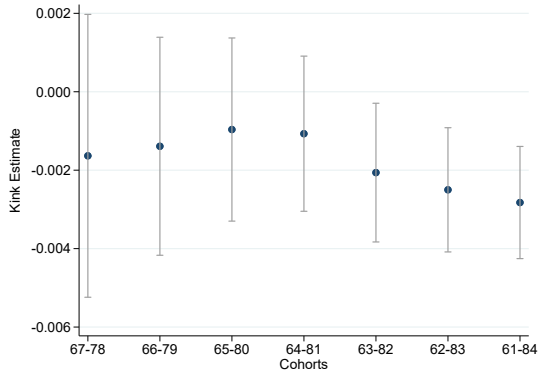
(d) Log Total Income

Notes: Each figure replicates the analysis in panel (c) of Table 2 for the outcome in the caption, using the different bandwidths in the x-axis. Sample includes respondents of the CASEN survey born between the relevant years (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. Plotted coefficients and 95% confidence intervals correspond to this variable. All regressions include county of birth x gender, survey year and age fixed effects. Standard errors clustered by county of birth in parentheses.

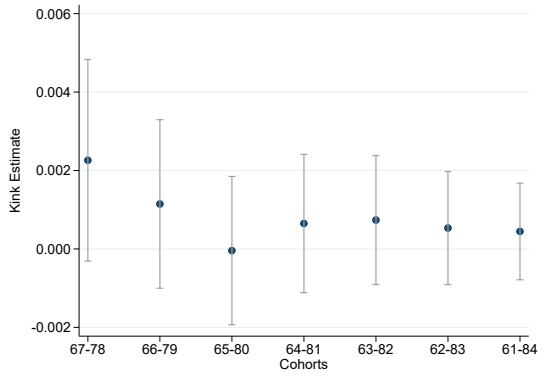
Figure E2: Household Income: Different Bandwidths



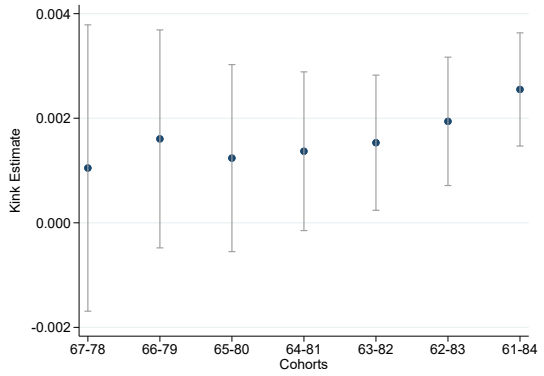
(a) Top Quintile



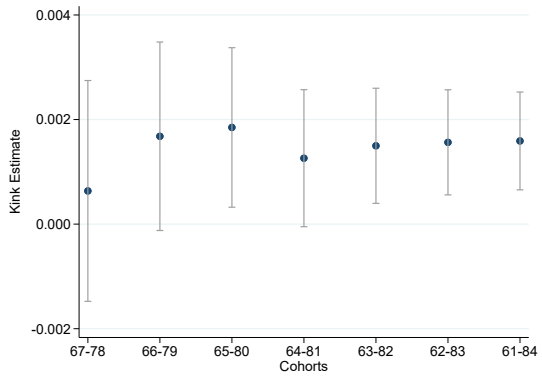
(b) Fourth Quintile



(c) Third Quintile



(d) Second Quintile



(e) Bottom Quintile

Notes: Each figure replicates the analysis in panel (c) of Table 3 for the outcome in the caption, using the different bandwidths in the x-axis. Sample includes respondents of the CASEN survey born between the relevant years (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. Plotted coefficients and 95% confidence intervals correspond to this variable. All regressions include county of birth x gender, survey year and age fixed effects. Standard errors clustered by county of birth in parentheses.

Appendix F Intergenerational Effects: Additional Results

Table F1: Educational attainment of Children: Sample Characteristics

							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 F2: Educational Attainment of Children: Effect of Parent's College (IV and OLS)

	(1)	(2)	(3)	(4)	(5)
			PANEL A: IV		
Any College (Parent)	0.258***	0.258***	0.255***	0.320***	0.284***
	(0.0580)	(0.0578)	(0.0584)	(0.0524)	(0.0500)
	[0.005]	[0.005]	[0.005]	[0.004]	[0.003]
			PANEL B: OLS		
Any College (Parent)	0.274***	0.273***	0.272***	0.262***	0.243***
	(0.0040)	(0.0041)	(0.0041)	(0.0043)	(0.0040)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Birth county x gender FE	Yes	Yes	Yes	Yes	Yes
Parent gender x gender FE	No	Yes	Yes	Yes	Yes
Relationship to HH head FE	No	No	Yes	Yes	Yes
Age FE	No	No	No	Yes	Yes
Full secondary FE	No	No	No	No	Yes
Observations	233,127	233,127	233,127	233,127	233,127
Kleibergen-Paap F-Stat (panel A)	292.3	289.8	282.3	308.8	310.9
R-squared (panel B)	0.104	0.105	0.105	0.117	0.178
Mean DV	0.582	0.582	0.582	0.582	0.582

Notes: 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 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, while $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973) is a dummy for parents that reached age 21 on or after 1973. The interaction of these two variables is the excluded instrument in panel A, with the baseline trend as additional control. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table F3: Educational Attainment of Children: Different Bandwidths (Child's Age)

Ages of children (bandwidth):	Dependent variable: Any College (child)				
	20-40	30-40	25-35	25-45	25-30
	(1)	(2)	(3)	(4)	(5)
Yr Age 21 Parent	-0.000	0.001	0.003***	-0.000	0.004***
	(0.0009)	(0.0011)	(0.0010)	(0.0009)	(0.0013)
	[0.703]	[0.371]	[0.003]	[0.703]	[0.008]
Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	-0.007***	-0.013***	-0.009***	-0.007***	-0.006***
	(0.0012)	(0.0016)	(0.0013)	(0.0012)	(0.0016)
	[0.004]	[0.002]	[0.001]	[0.004]	[0.002]
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
Observations	233,127	131,151	187,156	233,127	118,903
R-squared	0.063	0.057	0.056	0.063	0.054
Mean DV	0.582	0.533	0.608	0.582	0.639

Notes: Dependent variable is a dummy indicating whether child enrolled in college. Original 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 and reported full secondary education. Sample further restricted by age of child as indicated in the header. 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, while $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973) is a dummy for parents that reached age 21 on or after 1973. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table F4: Educational Attainment of Children: Heterogeneous Effects by Link

	Dependent variable: Any College (child)				
Position in household:	Child	Head	Spouse	Child of spouse	Sibling
	(1)	(2)	(3)	(4)	(5)
Yr Age 21 Parent	-0.000	0.001	0.001	-0.005	-0.005
	(0.0010)	(0.0026)	(0.0043)	(0.0100)	(0.0094)
	[0.943]	[0.812]	[0.866]	[0.585]	[0.658]
Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	-0.007***	-0.012***	-0.011*	-0.001	0.007
	(0.0013)	(0.0039)	(0.0060)	(0.0127)	(0.0125)
	[0.004]	[0.029]	[0.137]	[0.913]	[0.669]
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
Observations	213,059	11,617	4,509	1,965	1,521
R-squared	0.067	0.081	0.103	0.200	0.167
Mean DV	0.585	0.565	0.549	0.508	0.499

Notes: Dependent variable is a dummy indicating whether child enrolled in college. Original 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 and reported full secondary education. Sample further restricted by link to household head as indicated in the header. 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, while $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973) is a dummy for parents that reached age 21 on or after 1973. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table F5: Educational Attainment of Children: Heterogeneous Effects by Gender

Estimation Sample:	Dependent variable: Any College (child)			
	Child		Parent	
	Female	Male	Female	Male
	(1)	(2)	(3)	(4)
Yr Age 21 Parent	-0.000	-0.000	0.001	-0.001
	(0.0010)	(0.0012)	(0.0014)	(0.0009)
	[0.599]	[0.957]	[0.656]	[0.483]
Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	-0.006***	-0.008***	-0.008***	-0.006***
	(0.0014)	(0.0015)	(0.0019)	(0.0013)
	[0.004]	[0.003]	[0.001]	[0.012]
Birth county FE	Yes	Yes	No	No
Birth County x Gender FE	No	No	Yes	Yes
Parent Gender x Gender FE	Yes	Yes	No	No
Relationship to HH head FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Observations	114,022	119,105	94,606	138,489
R-squared	0.052	0.066	0.062	0.068
Mean DV	0.615	0.549	0.563	0.594

Notes: Dependent variable is a dummy indicating whether child enrolled in college. 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 and reported full secondary education. Sample further restricted by gender of parent or child as indicated in the header. 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, while $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973) is a dummy for parents that reached age 21 on or after 1973. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table F6: Educational Attainment of Children: Lower Levels

Dependent variable:	Primary education (basic)								Secondary education			
	1st	2nd	3rd	4th	5th	6th	7th	8th	1st	2nd	3rd	4th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Yr Age 21 Parent	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000*	0.001**	0.001***	0.001**	0.001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
	[0.161]	[0.278]	[0.315]	[0.338]	[0.478]	[0.458]	[0.112]	[0.081]	[0.052]	[0.026]	[0.100]	[0.267]
Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	-0.000	-0.000	-0.000	-0.000	0.000	0.000	-0.000	-0.000	-0.001*	-0.001***	-0.002***	-0.002***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0004)	(0.0005)	(0.0005)
	[0.280]	[0.495]	[0.534]	[0.808]	[0.850]	[0.964]	[0.431]	[0.242]	[0.116]	[0.048]	[0.016]	[0.037]
Birth County x Gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent Gender x Gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relationship to HH head FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	233,136	233,136	233,136	233,136	233,136	233,136	233,136	233,136	233,136	233,136	233,136	233,136
R-squared	0.005	0.005	0.005	0.005	0.006	0.006	0.007	0.007	0.010	0.012	0.014	0.015
Mean DV	0.996	0.996	0.996	0.995	0.994	0.993	0.992	0.991	0.981	0.976	0.961	0.950

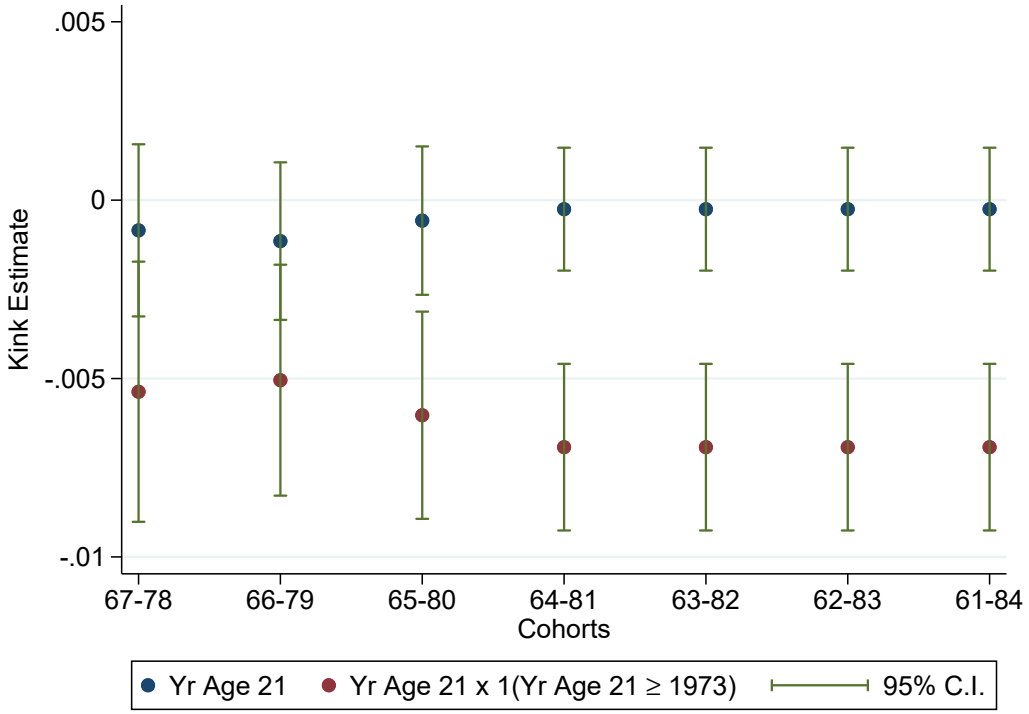
Notes: Dependent variable is a dummy indicating educational attainment at or above the level in the header. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that was born between 1943 and 1960 and reported full secondary education. 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, while $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973) is a dummy for parents that reached age 21 on or after 1973. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table F7: Educational Attainment of Children: Assortative Matching of Parents

Dependent variable:	Parent's spouse		Any College (Child)		
	Observed	Any College			
	(1)	(2)	(3)	(4)	(5)
Yr Age 21 Parent	0.006***	-0.000	-0.000	-0.001	-0.001
	(0.0008)	(0.0010)	(0.0010)	(0.0012)	(0.0011)
	[0.001]	[0.917]	[0.718]	[0.194]	[0.213]
Yr Age 21 Parent x $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)	-0.004***	-0.008***	-0.007***	-0.006***	-0.004**
	(0.0010)	(0.0012)	(0.0013)	(0.0016)	(0.0014)
	[0.009]	[0.000]	[0.004]	[0.019]	[0.065]
Birth County x Gender FE	Yes	Yes	Yes	Yes	Yes
Parent Gender x Gender FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Parent's spouse observed FE	No	No	Yes	No	No
Parent's spouse any college FE	No	No	No	No	Yes
Observations	213,059	133,200	213,059	133,200	133,200
R-squared	0.426	0.086	0.068	0.069	0.110
Mean DV	0.633	0.212	0.585	0.602	0.602

Notes: Dependent variable in the header. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (i) was born between 1943 and 1960, (ii) reported full secondary education, (iii) is a household head. Sample further restricted in columns 2, 4, 5 to children with a parent with an observed partner/spouse. 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, while $\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973) is a dummy for parents that reached age 21 on or after 1973. Standard errors clustered by county of birth in parentheses. P-values from wild cluster bootstrap at the cohort level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Figure F1: Educational Attainment of Children: Different Bandwidths



Notes: Each figure replicates the 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, while "$\mathbb{1}$(Yr Age 21 Parent $\geq$ 1973)" is a dummy for parents that reached age 21 on or after 1973. 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.