The Intergenerational Transmission of College: Evidence from the 1973 Coup in Chile*

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We study the transmission of higher education across generations using the arrival of the Pinochet dictatorship to Chile in 1973 as natural experiment. Pinochet promoted a large contraction in the number of seats opened for new students across all universities. Using census data, we find that parents who reached college age shortly after 1973 experienced a sharp decline in college enrollment. Decades after democratization, we observe that their children are also less likely to enroll in higher education. The results imply large and persistent downstream effects of educational policies implemented half a century ago.

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I. Introduction

Higher education is a key contributor to human capital formation, economic growth, and elite training (Cantoni and Yuchtman, 2014; Zimmerman, 2019). How much of college education transmits itself across generations? The answer to this question is important because it sheds light on the availability of educational opportunities to different members of society with unequal family backgrounds (Chetty et al., 2014). Moreover, the answer can help to guide the design of public policies that aim to increase enrollment rates and social mobility in low and middle-income areas. Unfortunately, it is difficult to construct a rigorous empirical answer because of the hurdle of finding a suitable context. Although previous research has been able to successfully link socioeconomic outcomes of parents and children (e.g. Chetty et al. 2020), it is hard to construct a strategy which leverages *exogenous* variation in parental college enrollment.[1] We study the intergenerational transmission of college education using the arrival of the Pinochet dictatorship as a natural experiment in Chile. We combine historical and contemporary census data spanning more than five decades to provide one of the first causal estimates of the intergenerational transmission of college.

To estimate the transmission of college enrollment from parents to children, we leverage a large and unexpected contraction in the higher educational system in Chile immediately after a military coup in 1973. The steady reduction in the number of openings for incoming students in the years following the coup has been shown to lead to worse labor market outcomes and increased mortality among affected individuals (Bautista et al., 2022; González et al., 2022). In this paper, we examine the long-run downstream effects of this contraction by studying the educational attainment of children with parents in the affected cohorts. By estimating the intergenerational effects of an economic policy on arguably one of the most life-changing decisions (i.e. college enrollment), our analysis also speaks to the role of the environment in shaping socioeconomic outcomes (Chetty et al., 2014). Overall, we show that children with parents exogenously exposed to fewer educational opportunities in the higher education system are also less likely to enroll in college.

Our analysis is centered around the military coup (September 11, 1973) that overthrew president Salvador Allende and put in power a junta led by Augusto Pinochet. Before the coup, college enrollment had been rapidly expanding in response to the growing demand for economic opportunities by the urban middle class. After the coup, universities were quickly targeted as part of the new regime's attempt to eradicate all sources of political opposition. Members of the military were appointed as rectors of all universities and

[1] Exogenous variation in socioeconomic outcomes of parents have been used to estimate the intergenerational transmission of non-college outcomes. The abolition of slavery and its impact on wealth in the U.S. is a notable example (Ager et al., 2021). For college, recent studies have used exogenous variation to document positive spillovers across siblings (Altmejd et al., 2021).

many students and faculty were persecuted. However, the regime's handling of universities soon begun to reflect the growing influence over policy of a group of market-friendly economists known as the *Chicago Boys*. Over the following years, the dictatorship steadily reduced subsidies to universities, which were largely reliant on government funding and were thus forced to downscale. This mostly took place through a reduction in openings for incoming students and led to a sharp fall in college enrollment. As a result of the deferred acceptance algorithm used for college admissions, the students barred from higher education were those with lower scores in an admissions exam, who disproportionately came from less affluent families.

To study the intergenerational transmission of college enrollment, we connect parents and children based on household composition in the 2017 census. We confirm that the sub-sample of linked parents who reached college-age after the coup exhibit a sharp downward kink in college enrollment. This finding is consistent with the results in Bautista et al. (2022) who follows the spirit of a regression kink design (Card et al., 2015) to exploit quasi-random exposure across birth cohorts who reached college age around the military coup. This type of empirical strategy is also similar to previous research exploiting how close to age 21 fathers were at the time of war, quasi-random exposure which has been used to document the intergenerational transmission of wartime service (Campante and Yanagizawa-Drott, 2016).

Our main contribution is to document significantly lower college enrollment in the form of negative kinks among children with parents exogenously exposed to fewer college opportunities in their youth. Remarkably, all of the children we observe in the 2017 census reached college age decades after the country returned to democracy. This type of persistence of socioeconomic outcomes within families after critical junctures is similar to findings in other contexts (Guirkinger et al., 2021; Alesina et al., 2022). Looking at lower levels of education among children of affected parents, we find statistically significant but economically smaller downward kinks in the last four years of secondary education (ages 15-18). This finding is important because it implies that college enrollment decisions of parents also have intergenerational impacts on their children's high-school dropout decisions. Importantly for the validity of the research design, we observe that the educational kink is completely absent in mandatory lower levels of schooling (ages 6-15). To gauge the magnitude of the intergenerational transmission of college, we use the kink among parents as an excluded instrument for their college enrollment decisions and provide instrumental variable estimates. The results show that having a parent who attended a college institution increases the child's probability of enrolling in a college institution by 32 pp (55% of the sample mean).

In terms of mechanisms, we argue that an important part of the intergenerational transmission of college can be explained by the socioeconomic impact on parents. In the context of this study, parents in the affected cohorts experienced lower income, more unemployment, and lower social mobility Bautista et al. (2022), all factors which are likely to have contributed to fewer educational opportunities for their children, who as a consequence exhibit lower levels of educational attainment. This is particularly important in the case of Chile because, after a reform in 1981, the country developed one of the most market-oriented educational systems in the world and as such educational attainment depended heavily on the income-generating capacity of households (Cuesta et al., 2020). In addition to these "income effects," we also find evidence supporting assortative mating and fertility as potential mechanisms. The role of college as a marriage market has been documented in many countries including Chile (Kaufmann et al., 2021; Kirkebøen et al., 2022). The impact of parental college enrollment decreases by approximately one-third after econometrically accounting for the spouse's educational level and the number of children alive. These results suggest that the contraction of higher education affected who married whom, increased the number of children born, and changed parental investment on their children (Becker and Lewis, 1973).

We contribute primarily 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; Amin et al., 2015). The study of educational externalities within families makes our work related to the transmission of college decisions across siblings (Altmejd et al., 2021; Aguirre and Matta, 2021). Other studies estimate the intergenerational persistence of human capital, wealth, and occupational choice using historical data (Clark and Cummins, 2015), with a particular focus on the relative role of inherited human capital versus nepotism (de la Croix and Goñi, 2022). A few studies have analyzed the relationship between parental college enrollment and children's early-life outcomes, beliefs, and attainment at lower schooling levels, but little is known about the link between the college enrollment of parents *and* children (Currie and Moretti, 2003; Maurin and McNally, 2008; Roland and Yang, 2017; Suhonen and Karhunen, 2019) and virtually nothing about the impacts of a contraction of higher education.

Closer to our work, Barrios Fernández et al. (2022) studies individuals in the period between 2002 and 2018 using a regression discontinuity design on the college admissions of their parents. The authors compare the outcomes of people whose parents nearly failed or succeeded in gaining admission to elite college programs in Chile and find that children whose parents attended elite college programs are more likely to attend elite high-schools and colleges, despite being no better in terms of their scores at the college admission exam. Similarly, Kaufmann et al. (2021) also studies the case of Chile and finds evidence that elite college improve assortative mating and educational outcomes for children. While their findings complement ours, their focus is on the consequences of accessing elite versus non-elite colleges in terms of social capital. In contrast to them, we study the intergenerational transmission of higher education more generally.

Our focus on parent-child pairs also makes this paper related to the literature documenting that socioeconomic outcomes persist within families over time. Previous research has documented persistent status and social mobility patterns across different time horizons and in different sub-populations across the world (Barone and Mocetti, 2020; Ager et al., 2021; Abramitzky et al., 2021; Alesina et al., 2022).

Finally, by estimating the intergenerational effects of policies implemented by a dictatorship, we also contribute to a rapidly growing literature documenting the legacies of authoritarian regimes and the consequences for the functioning of young democracies. Previous research has documented how authoritarian policies and institutions, different from educational reforms, have persistent effects and affect the distribution of political and economic power (Martínez Bravo et al., 2018; González et al., 2020, 2021). The link between dictatorship and educational policies also makes our paper related to research studying political regimes and redistribution (Acemoglu et al., 2015). Educational policy has garnered substantial attention, but most studies have focused on lower levels of education, with mixed findings (e.g., Mulligan et al., 2004; Harding and Stasavage, 2013; Paglayan, 2020). We contribute with historical and quantitative evidence on the sizable impact of regime change on college enrollment. Importantly, the Pinochet regime approach to universities shares many features with other authoritarian regimes (Connelly and Grüttner, 2005). These include the capture of the higher education system shortly after regime change and the elimination of democratic structures within universities, as happened in the Soviet Union in 1922 and in Nazi Germany in 1933.[2]

II. Historical background

Chile hosted eight universities in the 1960s, all of which were highly reliant on state funding. College enrollment grew from 25,000 (4.6%) students in 1960 to 146,000 (16.8%) by 1973 (panel (a) of Figure 1). This was a period of mass expansion of higher education throughout Latin America which aimed at improving equality of opportunity and social mobility for the growing urban middle class (Brunner, 1984). Since 1967, all universities in the country have used centralized admissions based on a standardized admissions test. Under this system, applicants rank programs – i.e. college-degree pairs – while universities rank applicants based on a weighted average of their grades in secondary and their admission exam's scores.

[2]They also include the introduction of controls over the student body, as happened in China during the Cultural Revolution and also in Nazi Germany (Waldinger, 2010, 2011; Roland and Yang, 2017; Li and Meng, 2020; Alesina et al., 2022). The shutdown of Central European University by Hungarian strongman Viktor Orban in 2018 provides a more recent example.

Universities choose the weight awarded to each component and the number of openings per program. A deferred-acceptance algorithm then determines admissions.

Amid growing political polarization and worsening economic conditions, socialist president Salvador Allende, democratically elected in 1970, 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. Just two weeks after the coup, the junta appointed members of the military as rectors of all universities claiming 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). 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 focus on repression and political control soon begun to incorporate a technocratic concern about the efficiency of public spending (Echeverría, 1980; PIIE, 1984; Velasco, 1994). This was the result of the growing influence of a group of market-friendly economists known as the *Chicago Boys*, who argued that public subsidies failed to provide incentives for thrift or effort (CEP, 1992; Valdés, 1995).[3] Under their guidance, the Pinochet dictatorship embraced a more traditional concept of universities as centers of academic excellence and elite training. As early as 1974, the Ministry of Finance begun pushing for a reduction in subsidies to universities. The fact that these measures of fiscal austerity further helped to defuse the political threat posed by universities facilitated their implementation: "the regime's penchant for political control meshed conveniently with its penchant for economic conservatism" (Levy, 1986, p.105).

Panel (b) in Figure 1 shows that the share of the education budget devoted to higher education steadily declined after 1974 and returned to its pre-Allende level of 30% only by 1980. This was a large financial blow to universities because subsidies were their main source of funding. A push for higher tuition met with strong resistance and was abandoned, thus forcing universities to downscale their operations. Panel (c) shows that applicants exceeded openings at all points in time, meaning that supply was always the binding constraint on admissions. Openings rose in tandem with spending under Salvador Allende (1970-73), but fell and stagnated after the coup: there were 47,000 openings in 1973 but only 33,000 in 1980. As a result of the contraction in the number of seats available, college enrollment sharply declined from 16.9% in 1973 to 10.5% in 1981 (panel (a) of Figure 1). Applicants with the lowest test scores were the ones who mechanically failed to gain admission as the number of openings fell.

[3] The regime also connected their view on public funding with undesirable activism: "the mediocrity in higher education. . . [is] a source of frustration for students, who become a breeding ground for political agitation" (Brunner, 2008, p.147).

A market-oriented reform in 1981 turned satellite campuses into independent institutions, further reduced subsidies, and opened the system to competition by new universities which were not eligible for government funding. The higher-education system has been institutionally the same since the 1981 reform. Enrollment rates increased after 1981 to become of the largest in the region, mostly driven by private institutions. Perhaps the most important policy between 1981 and 2017 was the introduction of state-guaranteed loans in the early 2000s, which furthered increased college enrollment (Solis, 2017).

The military regime also drafted a new constitution in 1980, which awarded Pinochet an eight-year term as "president." In October 1988, a plebiscite was held to determine whether Pinochet would remained in power. This was the first free election in Chile since 1973 (González and Prem, 2018). The "NO" option won with 55% of votes and triggered the country's democratic transition, with the first presidential election held in 1989 and Pinochet stepping down as president in March 1990.

III. Empirical framework

A. Census and estimating samples

Our main data source are individual records from the 2017 census. The census collects basic demographic information and records both educational attainment and labor market outcomes. We observe a little more than 17.5 million people organized in 5.6 million households. Given our interest in individuals who reached college age around the 1973 coup, we focus on the 2.6 million individuals who 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. We focus on age 21 because it is a conservative estimate for the average age of first-year college students at the time of the coup.[4] The results are robust to tighter bandwidths or small changes in the age of college entry.

Given our interest in the decision to enroll in college, we additionally focus on the sub-population of individuals who report completed secondary education. We do this to ensure a relevant counterfactual for college enrollment, i.e. completed secondary. This restriction leaves us with little more than 1 million individuals who we consider to be the population of interest for the first part of our analysis.

To detect parents and their children, we rely on household composition. The census classifies individuals into households and reports the relationship of all household members to one individual identified as the

[4]The average age of first-year college students in 1970 was 21. See Appendix Figure A.1 for details.

household head. We connect children to their parents using different combinations of within-household relationships, which means that we can only connect parents and children who lived in the same household in 2017. About 90% of our sample is composed of individuals reported as children of the household head.[5]

We restrict the sample to children between 25 and 40 years old to measure 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. We can connect each child to only one parent (the household head), but we use information on the spouse of the parent (if observed) to provide suggestive evidence on assortative mating as a mediating mechanism, and to assess the extent to which fertility patterns could qualify our findings. The final sample includes 228,608 individuals (children), 58% of whom report having enrolled in a university. Appendix Table A.1 compares characteristics of the estimating sample with a series of nested groups in the population to assess differences. Children in our sample are on average more educated than the general population, but this is mostly driven by the restriction that their parents must have completed secondary education. Otherwise, the estimating sample is observationally similar to the rest of the population.

B. Empirical strategy

We begin by studying the educational attainment of parents in a small window around the 1973 military coup. More precisely, we estimate trend breaks in college enrollment conditional on secondary completion by estimating the following reduced-form model:

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

where Y_{ic} is an outcome of individual i. The cohort indicates the year in which individuals turned 21 years old and it is indexed by c. The characteristics X_i include gender-specific county-of-birth fixed effects, $1(c \geq 1973)$ is an indicator equal to one for individuals who reached 21 years old in 1973 or later, while $f(c)$ and $g(c)$ are polynomial functions capturing birth-cohort profiles in Y_{ic}. We set the running variable equal to zero for 1972 and use a linear polynomial $f(c) = g(c) = c$ to avoid over-fitting. Our parameter of interest is π_1, which directly captures the trend change (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 (Cameron et al., 2008).

In this econometric strategy, we study the impact of the contraction of higher education by looking at

[5] 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.

changes in the respective cohort-level trends of our outcomes of interest, in the spirit of the regression kink design (Card et al., 2015). As such, the identifying assumption is that in the absence of the military coup there is little reason to expect kinks in these outcomes for people reaching age 21 after 1973.[6]

When providing estimates of the intergenerational effects, we again use equation (1). The main difference is that the cohort trends correspond to the observed parent, while the outcome of interest corresponds to the child. In our preferred specification, we expand the set of individual controls for the child (X_i) to include the following sets of fixed effects: (i) gender by county of birth, (ii) gender by parent's gender, (iii) relationship to household head and (iv) age. The latter alleviates the concern that parents from later cohorts will tend to have children that are younger in 2017, who may have different outcomes due to secular trends.

IV. Results

A. The coup and college enrollment

Panel (a) in Figure 2 shows the share of people per cohort that report any college in the 2017 census. We focus on the population of individuals who reported at least complete secondary education and who reached college age between 1964 and 1980 (x-axis). These restrictions leave us with little more than one million observations. The vertical line in this figure marks the year of the military coup (1973). The plot clearly shows a rapid increase in college entry for the cohorts who reached college age before the coup, followed by a large decline for people in cohorts who reached the same college age but after the coup. The enrollment rate increased by 6 percentage points (pp) between the 1964 and 1972 cohorts and decreased by 10 pp between the 1972 and 1981 cohorts. Panel (b) shows similar trends, with perhaps an even larger decline, for the sub-sample of the population who we observed as a parent in 2017. Bautista et al. (2022) shows that this kink is *not* driven by an overall decrease in education spending as a whole nor by a reduction in graduation in lower levels of education, i.e. the contraction of enrollment is specific for higher education.

Panels (c) and (d) in Figure 2 present a visual representation of the trend break in the series of college enrollment before and after the coup. The solid lines capture the actual trends in the two periods around 1973, while the dashed line represents our estimate of the counterfactual trend for the post-coup period, i.e. the extrapolation of the pre-coup trend to the post-coup period. Importantly, the linear trends fit the data accurately. In order to calculate the magnitude and statistical significance of the break, panel A in Table 1

[6] We include 18 cohorts reaching college age between 1964 and 1981. The discrete nature of the running variable prevents the application of a non-parametric approach to select an optimal bandwidth. We verify that results are robust to bandwidth changes.

presents our estimates of trend breaks using the micro-data from the census in the sample of linked parents. We report standard errors clustered by county of birth and p-values from wild cluster bootstrap at the cohort level. Column 1-3 in panel A show that, for different combinations of fixed effects, college enrollment increased by 0.6 pp for each new cohort reaching college age before the coup. This trend *decreased* by 2.1 pp per cohort for those reaching the same age after the coup. The difference between the two coefficients indicates a net enrollment trend of -1.5 pp per cohort after the coup.

We focus on the impact of the decreasing enrollment rates on the children of parents affected by the contraction of the higher education system. Bautista et al. (2022) show that the fewer educational opportunities had significant economic consequences for the affected individuals as they had worst labor markets outcomes and struggled to climb up the socioeconomic ladder. The documented negative economic impact the contraction had on parents suggests the existence of downstream effects among their children.

B. Intergenerational effects

We now study the educational attainment of children with a parent in the affected cohorts. The children in our sample are between 25 and 40 years old in 2017, meaning that the oldest ones reached age 21 in 1998, eight years after the end of the Pinochet regime. Panel B in Table 1 provides estimates of the relationship between the cohort of the parent and the college enrollment of the child. For people with a parent who 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, suggesting a positive causal relationship between the college enrollment of parents and children.

The results are robust to a wide range of specification decisions. Column 1 in Table 1 includes gender by county of birth fixed effects. Column 2 further controls for the combination of parent's and child's gender. Column 3 includes an additional set of indicators for the relationship of the child to the household head in the census, which is the information we rely on to link parents and children. Reassuringly, the results change very little across specifications. Column 4 introduces age fixed effects for the child. These controls help to address the concern that children with parents in the affected cohorts are likely to be younger, which could downward-bias the estimate of the intergenerational effect if younger people benefit from a positive secular trend in college enrollment after democratization. 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. We consider column 4 to be our preferred econometric specification.[7]

[7]Results are hardly affected if we consider more conservative bandwidths for the ages of parents (Appendix Figure A.2) or

Figure 3 provides a non-parametric visualization of the results from this specification. 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 their children. 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 who reached age 21 in 1979 is approximately 5 pp less likely to go to college (9% of sample mean) than a child with a parent that reached the same age right before the coup in 1972. This suggests that the contraction of higher education caused by the Pinochet dictatorship had sizable intergenerational effects, even after the country's return to democracy in 1990.

C. Additional results and mechanisms

We begin exploring heterogeneous estimates by household status and gender. Table A.3 shows that the downward kink in college enrollment is stronger for affected children who 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 A.4 provides disaggregate results based on gender of the parent or the child. Effects are slightly larger for mothers, a result consistent with the larger socioeconomic impact of the reduction in college enrollment for women (Bautista et al., 2022) and with recent evidence on the importance of mother's schooling (Amin et al., 2015).

To better understand the stage at which the educational attainment of children with affected parents lags behind, column 5 in Table 1 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 A.5 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. 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

different windows of ages for the children included in the sample (Appendix Table A.2. The latest exercise includes a narrow bandwidth with ages 25-30, i.e. the average person in this sample was *born* in 1990, the year in which Chile returned to democracy.

transition into higher education is the critical juncture with the largest intergenerational effects.

Our strategy to link parents and children 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 explore this possibility, Appendix Table A.6 uses information on the spouse of the linked parent as a proxy.[8] Children with a parent in the affected cohorts exhibit downward kinks in the probability that the parent has a spouse and, if present, in the probability that the spouse attended college, consistent with assortative mating. 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).

Finally, we explore the role of fertility as a mechanism. Previous research has documented the negative impact of college education on fertility (Goldin, 2021). Therefore, children of mothers in the affected cohorts might have received fewer investments from their parents due to a quantity-quality trade-off (Becker and Lewis, 1973). For this analysis, we restrict the sample to children who we are able to connect with their mother, as detailed information on fertility is fortunately available for women in the 2017 census. We present our findings in Appendix Table A.7. Column 1 shows that pre-coup cohorts faced a negative trend in fertility but it weakened for the post-coup cohorts, suggesting a negative association between women's college attainment and fertility, as already shown by previous literature (Goldin et al., 2006). Columns 2 and 3 fail to find post-coup deviations in the trends for the number of children alive and mother's age at birth, although point estimates have the expected sign. To assess the extent to which fertility may explain the intergenerational persistence of college, we study the impact of the kink in mothers' college attainment on the likelihood that their children attend college. Column 4 presents estimates from our baseline specification and columns 5 to 8 add fertility related fixed-effects. Results suggest that controlling for the number of children and age at birth decreases the kink coefficient by 12 and 40%, respectively. We interpret this as suggestive evidence that the quantity-quality trade-off could be a relevant mechanism behind our results.

D. Instrumental variables estimates

Following the econometric strategies of Card and Yakovlev (2014) and Card et al. (2015), we can leverage the cross-cohort variation in college enrollment triggered by the dictatorship as an excluded instrument to provide Instrumental Variables (IV) estimates of the intergenerational effects of attending college. The corresponding exclusion restriction in this case requires that the cohort of parents only affects children's

[8]For this part of the analysis, we must restrict the sample to linked parents that are household heads, as we cannot identify spouses for others. However, household heads are the bulk of linked parents overall.

educational attainment through its effect on the parent's college enrollment. In this regard, our use of a short bandwidth attempts to ensure that we only compare cohorts facing similar economic and political conditions but whose chances of attending college were affected by the regime change. Table 2 provides IV estimates of the effect of parental college enrollment on their children's probability of enrollment.

The results from our preferred specification, including age fixed effects for the child, indicate that having a parent that went to college increases their children's chances of enrolling by 32 pp (55% of the sample mean). In all cases the IV estimates are comparable to the partial correlations estimated through OLS. Nonetheless, this effect must be interpreted as a local average treatment effect (LATE) (Angrist et al., 1996), i.e. the average causal effect for children of parents whose college enrollment was affected because they reached college age in the years of reduced supply by the military government.

V. Conclusion

Does college enrollment transmit itself across generations? We have used the arrival of the Pinochet dictatorship to Chile in 1973 as a natural experiment to provide a positive answer to this question. Children who happened to have parents who were less likely to enroll in college for policy changes beyond their control are also less likely to enroll in college themselves. The impact of parental educational decisions on their children is large and persists across decades, even after critical junctures such as a country's democratization. Our estimates suggest that the returns to college enrollment might be significantly larger than previously thought as positive returns for one generation causes positive spillovers in the following one. Moreover, when combined with previous research suggesting positive externalities across siblings, it is reasonable to conjecture that the benefits of college could be even larger. Finally, inspired by previous literature, we explored and provided tentative evidence of income effects, assortative mating, and fertility as potential mechanisms behind the intergenerational transmission of college.

The link between policy and the persistence of higher education within families over a fifty year period also speaks to the relative role of nature (e.g. genetics) versus nurture (e.g. investments) in driving the socioeconomic paths of individuals (Holmlund et al., 2011). A branch of the previous literature has suggested that our fate could be strongly tied to the status of our parents and even grandparents (Clark, 2014). Under this view, the potential socioeconomic impacts of policies are inevitable constrained (although not determined) by historical circumstances in your family lineage. We have documented that policies implemented fifty years ago can have large consequences on the socioeconomic paths of individuals, affect a variety of life-changing decisions (college, marriage, work), and through these then affect the fate of their children. As such, our evidence lessens the modern importance of nature as driver of the economic life-cycle.

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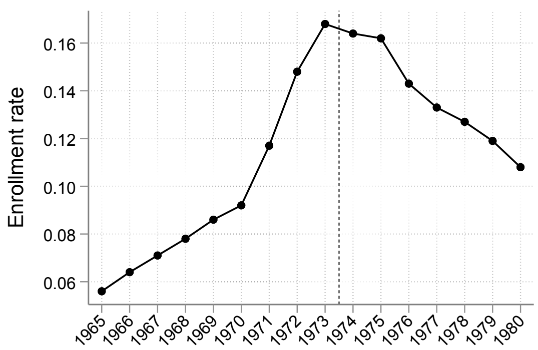
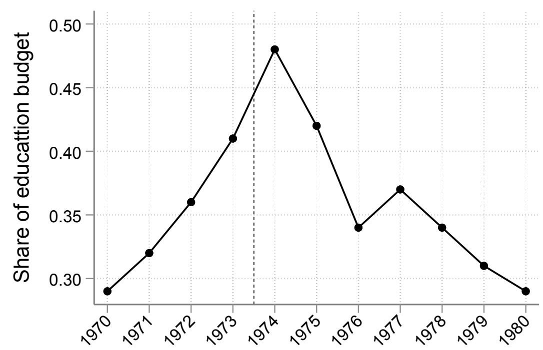
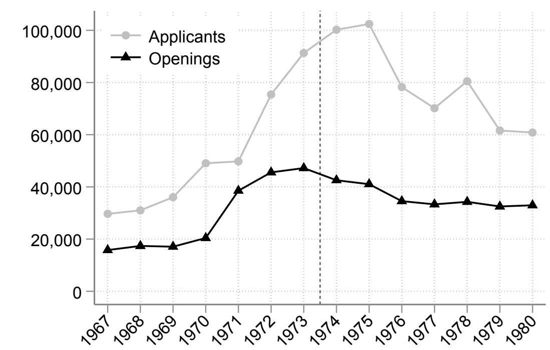
Figure 1: College funding, enrollment, and openings



(a) Enrollment



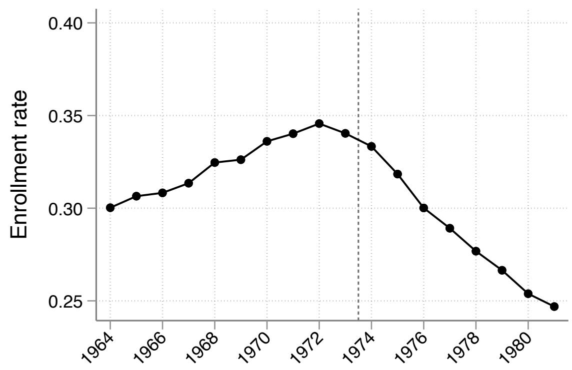
(b) Funding



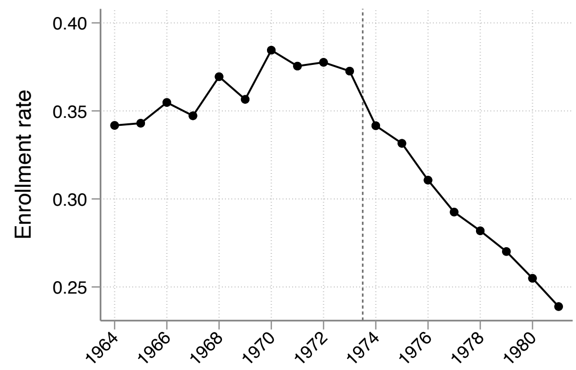
(c) Applicants

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

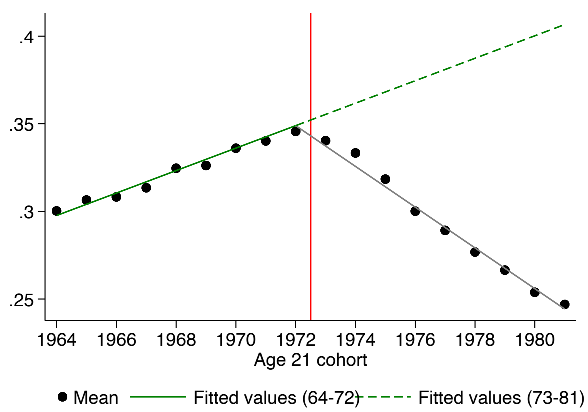
Figure 2: Educational attainment among parents in the 2017 census



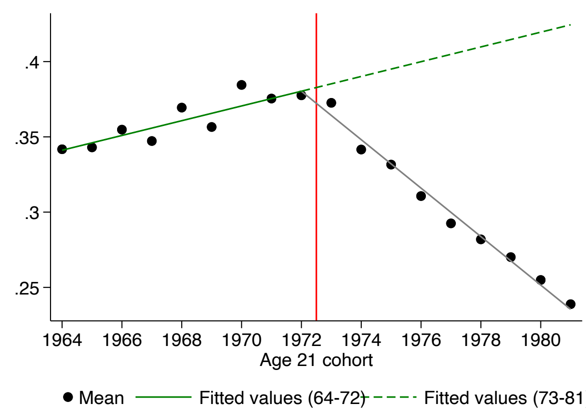
(a) Population (N=1,043,396)



(b) Parents living with their children (N=233,136)



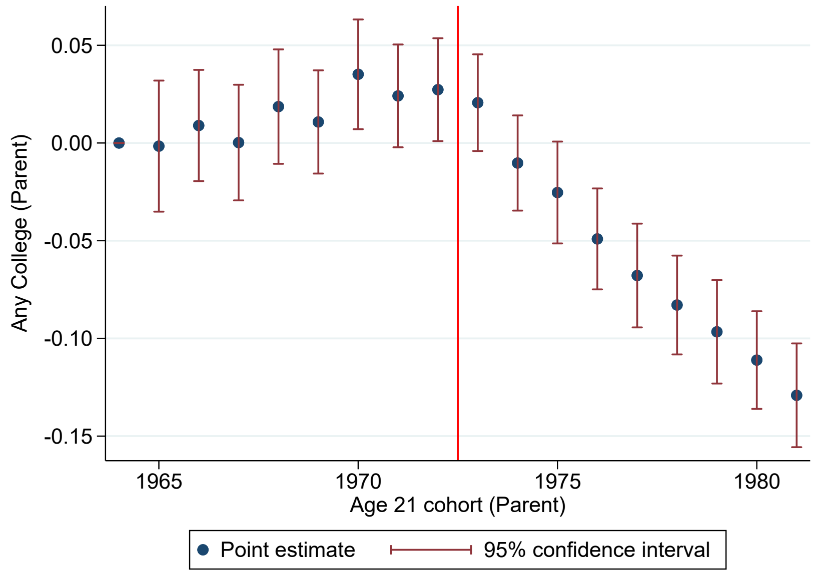
(c) Kink in the population (N=1,043,396)



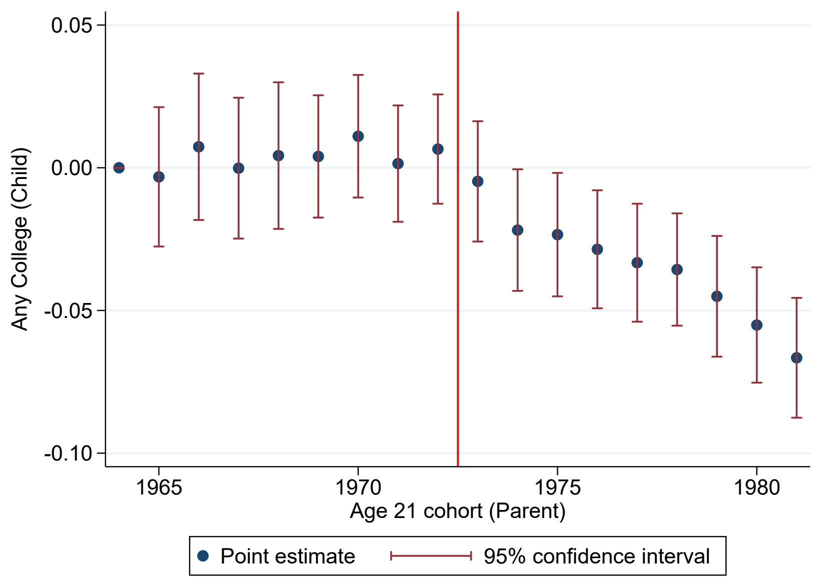
(d) Kink among parents (N=233,136)

Notes: All figures focus on the population of individuals who in 2017 reported at least complete secondary education and who reached college age (21 years old) between 1964 and 1981 (N=1,043,396). Panels (a) and (b) display the share of people per cohort who reported at least one year of higher education in the 2017 census. For example, in the year 1974 we report the share of individuals who reported being enrolled for at least one year in college among the universe of individuals who reached college age (21 yrs old) that year. Vertical lines indicate the year of the military coup. Panels (c) and (d) repeat the analysis but including estimated cohort trends. The solid green line corresponds to the best linear fit for cohorts reaching college age before 1973. The dashed green line shows the linear extrapolation for subsequent cohorts. The solid grey line corresponds to the best linear fit for cohorts reaching college age in 1973 or afterwards.

Figure 3: 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: College enrollment of parents and children

	(1)	(2)	(3)	(4)	(5)
Panel A	Dep variable: Indicator for parents who attended college				
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	-0.021***	-0.021***	-0.020***	-0.022***	-0.021***
$\times$ 1(Yr Age 21 Parent $\geq$ 1973)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B	Dep variable: Indicator for children who attended college				
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	-0.005***	-0.005***	-0.005***	-0.007***	-0.006***
$\times$ 1(Yr Age 21 Parent $\geq$ 1973)	(0.0013)	(0.0013)	(0.0013)	(0.0012)	(0.0011)
	[0.006]	[0.005]	[0.005]	[0.004]	[0.003]
Observations	233,136	233,136	233,136	233,136	233,136
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
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
Avg. dependent variable (panel A)	0.309	0.309	0.309	0.309	0.309
Avg. dependent variable (panel B)	0.582	0.582	0.582	0.582	0.582

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 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 2: The intergenerational tranmission of college

	(1)	(2)	(3)	(4)	(5)
Panel A: IV estimates					
Any College (Parent)	0.258***	0.258***	0.255***	0.320***	0.284***
	(0.058)	(0.058)	(0.058)	(0.052)	(0.050)
	[0.005]	[0.005]	[0.005]	[0.004]	[0.003]
Panel B: OLS estimates					
Any College (Parent)	0.274***	0.273***	0.272***	0.262***	0.243***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	233,127	233,127	233,127	233,127	233,127
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
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
Avg. dependent variable	0.582	0.582	0.582	0.582	0.582

Notes: Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that was born between 1943 and 1960 and reported full secondary education. See text for further details on construction of sample. In panel A, the kink term "1(Yr Age 21 Parent ≥ 1973)" is used as excluded instrument for any college education of the Parent (the trend "Yr Age 21 Parent" is an included instrument). All regressions include county of birth x gender fixed effects. Column 2 adds parent's gender fixed effects. Column 3 includes fixed effects for each possible relationship to the head of the household, based on the linkages described in the main text. Column 4 adds age (of child) fixed effects, and column 5 adds a dummy for whether the children completed secondary. 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

ONLINE APPENDIX

The Intergenerational Transmission of College: Evidence from the 1973 Coup in Chile

María Angélica Bautista, Felipe González, Luis Martínez, Pablo Muñoz, and Mounu Prem

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Table A.1: Educational attainment of Children: Sample Characteristics

	Age	Female	Full primary	Full secondary	Any college	HH size	Position in HH: Head	Position in HH: Spouse	Position in HH: Child	Children (women)	In labor force	Unemployed	Studying
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Sample I													
All 25-40 years old (i.e. children)	32.09	0.50	0.95	0.80	0.31	4.79	0.35	0.24	0.26	1.89	0.81	0.07	0.12
N =3,840,429	(4.61)	(0.50)	(0.22)	(0.40)	0.46)	(6.85)	(0.48)	(0.43)	(0.44)	(0.96)	(0.39)	(0.26)	(0.33)
Sample II													
Sample I + linked to parent	30.51	0.48	0.96	0.83	0.35	4.53	0.05	0.02	0.90	1.59	0.81	0.12	0.17
N =1,019,693	(4.48)	(0.50)	(0.20)	(0.38)	(0.48)	(1.84)	(0.22)	(0.14)	(0.30)	(0.85)	(0.39)	(0.33)	(0.37)
Sample III													
Sample II + parent w/ full secondary	29.59	0.49	0.99	0.94	0.55	4.30	0.04	0.02	0.92	1.47	0.81	0.13	0.23
N =438,237	(4.14)	(0.50)	(0.09)	(0.24)	(0.50)	(1.65)	(0.20)	(0.12)	(0.27)	(0.77)	(0.40)	(0.34)	(0.42)
Sample IV													
Sample 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	1.52	0.83	0.13	0.19
N =234,328	(4.39)	(0.50)	(0.10)	(0.23)	(0.49)	(1.64)	(0.22)	(0.14)	(0.28)	(0.81)	(0.38)	(0.33)	(0.39)

Notes: This table shows averages and standard deviations (in parenthesis) for the characteristic described in the header. The top row shows values for the full sample of people with ages 25-40 in the 2017 population census. The second row shows the same statistics for the subsample that cohabits with a parent, irrespective of any characteristics of the parent. The 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 A.2: Educational Attainment of Children: Different Bandwidths (Child's Age)

	Dependent variable: Any College (child)				
Ages of children (bandwidth):	20-40	30-40	25-35	25-45	25-30
	(1)	(2)	(3)	(4)	(5)
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 1(Yr Age 21 Parent ≥ 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 1(Yr Age 21 Parent ≥ 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 A.3: 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 1(Yr Age 21 Parent ≥ 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 1(Yr Age 21 Parent ≥ 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 A.4: Educational Attainment of Children: Heterogeneous Effects by Gender

	Dependent variable: Any College (child)			
Estimation Sample:	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 1(Yr Age 21 Parent ≥ 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 1(Yr Age 21 Parent ≥ 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 A.5: 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 1(Yr Age 21 Parent ≥ 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 1(Yr Age 21 Parent ≥ 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 A.6: Assortative mating 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 1(Yr Age 21 Parent ≥ 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

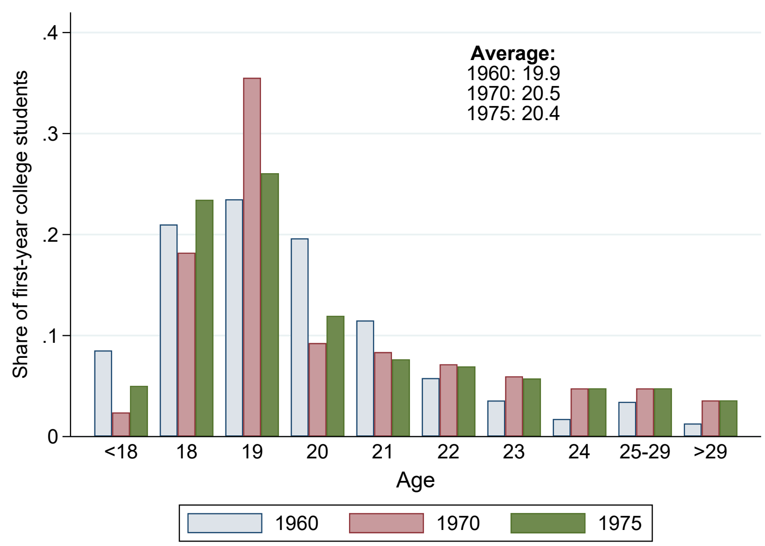
Notes: Dependent variable in the header. Sample includes all respondents in the 2017 census between the ages of 25 and 40 that we can connect to at least one parent that (i) was born between 1943 and 1960, (ii) reported full secondary education, (iii) is a household head. Sample further restricted in 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 1(Yr Age 21 Parent ≥ 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 A.7: Educational Attainment of Children: Fertility (Census 2017)

Dependent variable:	Total children	Share alive	Mother's age at birth	Any College (Child)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[a] Yr Age 21 Mother	-0.027***	0.001***	-0.646***	0.000	-0.002	-0.002	0.010***	
	(0.005)	(0.000)	(0.011)	(0.001)	(0.001)	(0.002)	(0.002)	
	[0.000]	[0.001]	[0.000]	[0.720]	[0.244]	[0.446]	[0.000]	[0.011]
[b] Yr Age 21 Mother x 1(Yr Age 21 Mother ≥ 1973)	0.015**	-0.000	-0.018	-0.008***	-0.007***	-0.007***	-0.004**	-0.004**
	(0.006)	(0.000)	(0.015)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
	[0.003]	[0.639]	[0.352]	[0.000]	[0.000]	[0.022]	[0.008]	[0.011]
Birth county × gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relationship to HH head FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Total children Mother FE	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Share children alive < 1 Mother FE	No	No	No	No	No	Yes	No	No
Age at birth FE	No	No	No	No	No	No	Yes	Yes
Observations	93,727	93,727	93,727	93,727	93,727	23,922	93,727	93,727
R-squared	0.030	0.063	0.333	0.063	0.079	0.070	0.081	0.081
Mean DV	2.765	0.979	30.49	0.564	0.564	0.467	0.564	0.564

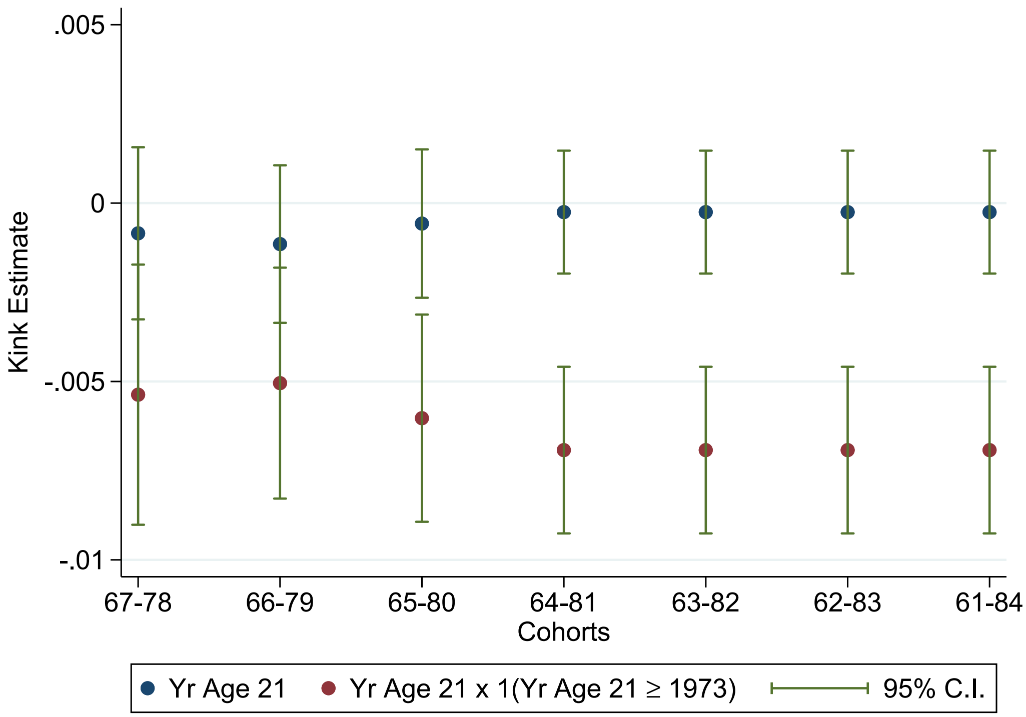
Notes: Dependent variable in the header of each column. Sample includes all respondents in the 2017 census that we can connect to their mother, who meets the following conditions: (i) reached age 21 between 1964 and 1981 (both years inclusive) and (ii) reported full secondary education. Possible mother-child linkages include: (i) HH head + children, (ii) HH head + parent, (iii) spouse + parent, (iv) spouse + children, (v) sibling + parent. "Yr Age 21 Mother" is a continuous variable indicating the year in which the mother reached age 21, normalized to zero in 1972. "Yr Age 21 Mother x 1(Yr Age 21 Mother ≥ 1973)" is the interaction of this variable with a dummy for mothers that reached age 21 on or after 1973. "Share children alive < 1 Mother FE" adds a dummy for whether the share of born alive children of the mother was below one. "Age at birth FE" adds fixed effects based on the age of the mother at birth. 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 A.1: 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 A.2: 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 "1(Yr Age 21 Parent ≥ 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.