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Ability, Not Heritage: Why Expanding University Access Often Fails to Narrow Intergenerational Educational Gaps

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Ability, Not Heritage: Why Expanding University Access Often Fails to Narrow Intergenerational Educational Gaps*

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ABSTRACT

Many countries have established new local colleges to increase access to education for disadvantaged populations. However, many of these expansions have not reduced educational inequality. Drawing on evidence from a large-scale college expansion initiative, we find that increased college availability did not lead to a differential increase in attendance among students from parents with less education. Rather, the expanded access primarily benefited students with marginal academic ability. These results suggest that higher education enrollment is largely determined by inherent scholastic ability and that the expansion of higher education tends to attract students at the upper margin of this ability distribution.

Keywords: Intergenerational correlations, university expansion, access to education, higher education
JEL Codes: I23, I24, I28, J24, J62.

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1 Introduction

How does family background shape a child's future opportunities? Chetty, Hendren, Kline and Saez (2014) find that in the United States, a 10-percentile increase in parental income is associated with a 3.4-percentile increase in a child's income. Similarly, children with at least one highly educated parent are more than twice as likely to attain tertiary education as children whose parents lack higher education (Verweij and Keizer, 2022). These strong intergenerational correlations are often viewed as indicators of a society's inability to provide equal opportunity across family backgrounds. One way to break these inequalities is to provide youth with greater access to higher education.

Indeed, access to higher education has expanded drastically in the last 50 years. For example, Turkey created 41 new public universities and experienced a 60 percent increase in the number of students between 2006 and 2008 (Caner, Derebasoglu and Okten, 2024). In South Korea college enrollment rates increased from just over thirty percent to over eighty percent over a fifteen years period starting in the 1990s (Choi, 2015). Ernst, Langot, Merola and Gonzales Pulgarin (2024) document a rise in upward mobility—the likelihood of moving from the bottom to the top of the education and income distribution—since the 1980s in the United States. They attribute this trend to the expansion of university education. Despite the massive scale of educational expansions, policy evaluations have produced mixed evidence on their effectiveness in promoting equal educational opportunities.

Expanding local educational opportunities, particularly for students from low-income backgrounds, has been a widely used policy tool to reduce intergenerational inequality. The premise rests on evidence that educational attainment is a function of commuting distance to college, and that those living further away from college typically have less wealthy and less educated parents (Card, 1993). It further relies on the premise that genetic factors play a minimal role in intergenerational transmission. However, recent research suggests that genetic endowments account for approximately 21 percent of the observed correlation in educational attainment, whereas paternal involvement—a key environmental factor—explains only around 4 percent (Verweij and Keizer, 2022). And while the intergenerational correlation in schooling has declined over time, and distance to colleges have been reduced substantially in many countries, parental education continues to explain as much of the variance in children's educational attainment as it did in the past (Hertz, Jayasundera, Piraino, Selcuk, Smith and Verashchagina, 2008). These findings raise questions about the extent to which increased access to higher education can mitigate educational inequality, as genetic influences and other structural barriers may impose constraints on upward mobility.

This study provides causal evidence that the 1977 expansion of local colleges in Sweden did not increase college attainment among students from less-educated families during the first decade following the reform. Instead, the gains were concentrated among students of middling academic ability residing in municipalities where the new colleges were established. Importantly, these increases in attainment were not mediated by parental education and did not extend beyond the local areas directly affected by the reform.

We exploit the expansion in 1977 as a natural experiment to assess the impact of local college access on educational attainment. Using detailed registry data on high school Grade Point Average (GPA), parental education, and college attainment, we examine high school graduates who were differentially exposed to this expansion in a differences-in-differences framework. Our analysis estimates both average and conditional treatment effects, focusing on the interaction between college access, parental education and cognitive ability. By leveraging exogenous variation in college access, we provide new insights into the role of cognitive ability in driving college attainment while finding no differential causal effect of parental education on college enrollment or completion. Our findings contribute to the broader policy discussion on reducing educational inequality.

2 Background

The relationship between college proximity and educational attainment is well established. Card (1993) shows that reduced distance to a college significantly increases educational completion even after accounting for parental education, regional factors, and GPA. Similar effects have been documented in Germany (Spiess and Wrohlich, 2010) and Uruguay (Katzkowicz, Lavy, Querejeta and Rosá, 2023) where a 10-km decrease in distance raises university attendance by 2–3 percentage points in Germany and 0.9 percentage points in

Uruguay.

This evidence has spurred policies aimed at expanding higher education by building new colleges in closer proximity to students' homes rather than merely enlarging existing institutions. Evaluations of such expansions have been conducted in Finland, Norway, Sweden, Turkey, Germany, France, Uruguay, and China. Although these reforms typically increase overall college enrollment (see, e.g., Markus (2023) and Nimier-David (2022)), their effects on upward educational mobility remain inconclusive. In Finland, the expansion disproportionately benefited women but increased intergenerational persistence in education, as the children of affected cohorts became more likely to attend university themselves (Suhonen and Karhunen, 2019). In Norway, college expansion contributed to greater gender stratification, making girls more likely to choose typically female dominated (e.g. nursing) and boys to choose more male dominated college educations (e.g. engineering) (Knutsen, Modalsli and Rønning, 2020). The Turkish reform did not reduce the gender gap but did lower the intergenerational correlation in education for girls (Caner et al., 2024). In China, higher college graduation rates were accompanied by increased intergenerational educational persistence and social stratification (Ou and Hou, 2019). In Uruguay, Méndez (2020) find that college expansion from 2008 to 2013 primarily benefited students with well-educated parents. However, Katzkowicz et al. (2023) suggest that the same program successfully increased university enrollment among first-generation college students.

One explanation for these mixed effects is the critical role of ability in educational attainment. Card (2001) finds that GPA explains approximately 25 percent of the variation in schooling among men, with a one standard deviation increase in GPA associated with an additional year of education. If ability is primarily determined by genetic factors, as suggested by Verweij and Keizer (2022), and if the effects of college proximity are endogenous to parental location choices, simply constructing new colleges may not substantially improve opportunities for disadvantaged youth. For example, families with lower income may tend to reside in rural areas with lower living costs, so that new urban colleges predominantly benefit already advantaged students.

A further complication is the possibility of a diversion effect. Mountjoy (2022) finds that in Texas, greater access to two-year community colleges diverted some students away from four-year institutions. Although disadvantaged students may benefit from the short-term mobility afforded by two-year programs, such diversion might limit long-term upward mobility. Similar patterns have been observed in Norway, where increased college proximity alters enrollment patterns without necessarily improving long-term outcomes.

In Sweden, the data align with one well-established pattern but not with another. The intergenerational correlation in higher education attainment is 0.31, consistent with findings from the U.S. and other countries. However, there is no correlation between parental higher education and proximity to traditional university locations prior to the reform. That is, while educational attainment is strongly linked across generations, it is unrelated to geographic access to universities—even though Sweden is the third-largest country in the European Union and the fifth largest in Europe by area. Many children that completed tertiary education thus grew up having highly educated parents but living far from any university.

Collectively, these findings underscore the complexity of the relationship between college expansion and intergenerational educational mobility. While expanding higher education access generally increases enrollment, its impact on reducing educational inequality is far from straightforward. The intergenerational transmission of education is influenced not only by structural factors but also by ability and family background.

3 The Reform

Sweden implemented a major higher education reform in 1977. Before the reform, Sweden's traditional universities were concentrated in seven locations: Stockholm, Gothenburg, Lund, Uppsala, Linköping, Luleå and Umeå. By the late 1960s, university enrollment at these locations had been rising sharply, creating challenges for educational planning, as many programs operated with open admissions and no fixed capacity. At the same time, and as in many other countries, Sweden's booming economy generated a strong demand for skilled labor, intensifying pressure to expand higher education. The policy debate centered on whether to accommodate this demand by enlarging existing universities or by establishing new institutions. Ultimately, Sweden opted for the latter approach, prioritizing regional expansion to broaden access to tertiary education.[1]

Released in 1973 (SOU 1973:2), a government-commissioned report informed a 1975 parliamentary decision to expand higher education, leading to the establishment of 12 new colleges in 16 locations 1977. The report proposed granting independence to former university branches in Örebro, Karlstad, and Växjö, while also creating twelve new institutions in Borås, Eskilstuna, Västerås, Falun, Borlänge, Halmstad, Jönköping, Kalmar, Kristianstad, Sundsvall, Härnösand, and Östersund. When implemented, the proposal was largely followed, with minor adjustments: Gävle and Sandviken were added in 1977, while Halmstad's university was established in 1983, and is therefore excluded from our analysis. All colleges established in 1977 initially focused on offering one-, two-, and four-year degrees without research opportunities. During the 1990s and 2000s, the new colleges were gradually granted university status, allowing their students and professors to also conduct research.

The two-year gap between the policy announcement and its implementation, coupled with minimal changes to university locations, raises the possibility of anticipatory effects, which we account for in our analysis. Additionally, nearly all new colleges were established in locations that had previously offered some form of post-secondary education (see Andersson, Quigley and Wilhelmson (2004)). To mitigate concerns that the municipalities were on different trajectories already prior to the reform, we show that pre-trends were parallell in our treatment and control groups. Figure 1 maps the locations of both new and pre-existing institutions. Most new colleges were concentrated along Sweden's southern and coastal regions, where the majority of the population reside.

[1] A more detailed discussion of the debates leading up to the reform, as well as its specific design and implementation, can be found in Bergh, Hällerfors, Tåg and Åstebro (2024).

4 Data and Empirical Approach

4.1 Data

We use Swedish register data from Statistics Sweden's LISA database to track whether high school graduates obtain a certified college or university degree by age 40, regardless of where the degree was earned. Degrees from "folkhögskolor" (vocational post-secondary schools) are excluded. The registry data are nearly complete, with our high school graduation cohorts in 1973-1987 only having 4.5 percent missing data on educational attainment, 0.0076 percent on high school graduation municipality, zero percent on Grade Point Average (GPA), and 15 percent on father's education and 6 percent on mother's education.[2] Additionally, because our data are not survey-based, we face no attrition. Our data cover high school graduating cohorts from 1973 to 1987.

Similar to Card (1993), we define treatment exposure as a binary variable equal to one if a student graduates from high school in a municipality where a new college was established in 1977, and zero otherwise. We follow Mountjoy (2022) in using high school location to determine distance to colleges, ensuring contemporary identification of access at the time of college application decisions. Swedish municipalities are typically small, with coordinated public transportation enabling students in rural areas to commute easily to central cities where colleges are located. In the less populated northern municipalities, most residents live along the coast near the main city of the municipality. Only students that graduate from high school with a completed degree are eligible for treatment.

We exclude seven municipalities containing the old established universities from analyses (Stockholm, Göteborg, Uppsala, Lund, Umeå, Luleå, Linköping). Municipalities that receive a new university later than 1977 but before 1988 are also excluded (Skövde and Halmstad). Municipalities that receive new universities in 1988 or later are part of the control group. Since universities in the latter municipalities were created after the end of our observation period they are valid as control locations. In robustness analysis we examine the

[2]By comparison, the NLSY, a widely used dataset for studying returns to education in the U.S. (e.g., Card (2001)), has 22 percent missing data on father's education and 11 percent on mother's education.

effects of these sampling choices on coefficient estimates.

4.2 Sample Selection and Control Group Construction

We use 1-1 exact matching without replacement to compare an individual from a reform municipality with a similar individual randomly chosen from a pre-defined cell in the control group. A graduating boy/girl is matched with a graduating boy/girl within the same cohort on 10 quantiles of their high school GPA, and with parents' level of education with two groups: none, or at least one parent with a college/university degree.

Educational outcomes are measured at the age of 40. The proportion of high school graduates having obtained a college or university degree by the age of 40 in our matched sample increased only marginally from 35 percent for the 1973 cohort to 36 percent for the 1987 cohort. Since the expansion of higher education in Sweden began before 1973, the share of high school graduates with at least one university-educated parent, however, grew substantially from just above 14 percent in 1973 to nearly 26 percent in 1987.[3]

Table 1 presents descriptive statistics for individuals in treatment and control group, as well as a balancing test. Matched individuals compare well on the three criteria—gender, high school GPA, and parental education—as expected. The treated and control groups also have similar distance to the nearest of the seven established universities and the taxable income of their high school graduation municipality. The municipality average taxable income is about 2 percent higher for treated individuals compared to controls. The difference in distance to the nearest traditional university is 8 kilometers (approximately 5 miles), equivalent to a bus ride of about 10 minutes. Given these small differences, we disregard the need for additional identification assumptions to adjust these remaining differences between the treated and control groups.[4]

[3]defined as the maximum observed education level at any point during the observation period.

[4]See Mountjoy (2022) for an analysis addressing concerns related to the distance to the outside option.

4.3 Empirical Specification

We use a dynamic difference-in-difference model to examine the evolution of treatment effects over fourteen years. The regression model

$$y_{icm} = \sum_{k \neq 1973}^{1987} \delta_k \times \mathbf{1}\{c = k\} \times \mathbf{1}[D_{im} = 1] + \pi_c + \sigma_m + \varepsilon_{icm} \tag{1}$$

estimates the differential intention-to-treat effect of the establishment of new colleges in 1977 on each individual's educational status by the age of 40 for each graduating high-school cohort between 1973 and 1987 between treated and untreated municipalities. y_{icm} takes the value 1 if individual i from cohort c and municipality m has a tertiary education at age 40, zero otherwise, and δ_k captures the treatment effect. The treatment indicator D_{im} takes the value 1 if individual i resides in a municipality that received a new college in 1977, and 0 in the municipalities that did not. π_c are cohort fixed effects and σ_m are municipality fixed effects. Standard errors are clustered at the municipality level to account for within-municipality correlation in the error term, as treatment is assigned at the municipality level.

To summarize results compactly we then pool all the post-treatment years (1976-1987) and all pre-treatment years together, comparing the pre- and post- periods between the treated and control groups according to equation 2:

$$y_{icm} = \gamma \times \mathbf{1}\{c \geq 1976\} \times \mathbf{1}[D_{im} = 1] + \pi_c + \sigma_m + \varepsilon_{icm} \tag{2}$$

We allow for that some parents may relocate their families in 1976 to take advantage of the 1975 parliamentary announcement of the colleges that would be built in 1977 by using 1976 as the first treatment year. This provides a more conservative estimate of the post-period effect.

The key identifying assumption is that, conditional on matching, the outcome variable's trend would have been identical in both the treatment and control groups absent the treatment. A common approach to assess this assumption is to present pre-treatment trends across these groups, which we will do throughout the analysis.

Identifying the interaction effect with parental education requires a different and more nuanced assumption. For causal identification, we assume conditional independence of the interaction term, given the control variables and matching vector. This implies that graduating high school students with the same parental background in treated municipalities would have behaved similarly to their counterparts in control municipalities had those municipalities not received a new university in 1977. While this assumption is more complex, it may also be more plausible, as it relies on conditional comparability rather than strict exogeneity.

5 Results

The annual cohort treatment effects shown in Panel A of Figure 2 suggest that the reform had a small but positive effect on educational attainment in treated municipalities relative to the control group. The difference-in-differences pooled estimate is a 2.4 percentage point treatment increase, statistically significant at $p < 0.01$ (Table A1). Panel B shows no differential effect over time when disaggregating the treatment effect by parental education, (see Table A2 for the corresponding pooled estimate). Improving access to universities increased enrollment, but did not do so differentially by parental educational background.

We next analyze the effects of the reform by student high-school GPAs. Figure 3 presents two panels displaying annual cohort treatment effects. Panel A shows treatment effects across GPA groups, categorized into low (quantile 1–3), medium (quantile 4–7), and high (quantile 8–10) GPA. The rightmost panel illustrates treatment effects where GPA is interacted with parental college or university education. The left-side figures suggest a positive treatment effect for individuals with middling GPA levels, while the right-side figures indicate that these effects do not vary by parental education.

We also present estimates from the pooled difference-in-difference model, interacting treatment with post-reform status and GPA (Table A3). The results indicate a statistically significant 2.1 percentage point increase in college attainment for individuals with medium GPA, relative to those with low GPA. Additionally, there is a smaller effect for individuals with high GPA, estimated at 1.2 percentage points, but not significant at conventional levels. The confidence intervals for the medium and high GPA estimates overlap, suggesting similar treatment effects for these groups. As an additional analysis we also presents the estimates from a three-way interaction model, incorporating parental education as an additional moderator (Table A5). While the coefficients for both medium and high GPA groups remain positive, they are not statistically distinguishable from zero. This suggests that the observed increase in college enrollment among students with medium or high GPA is not contingent on parental education.

6 Robustness analysis

We have examined the robustness of our results in several ways. First, we exclude municipalities that neighbor the treatment municipalities from the control group, with the idea that these may exhibit spillover treatments. The results are similar in that enrollment increases significantly only for medium GPA individuals in the treated municipalities (Table A6). The point estimates for the middle GPA group are in both cases statistically significantly different from zero, but cannot be distinguished from each other at conventional levels. This suggests that neighboring municipalities did not differ much from other control group locations. Second, we investigate the possibility of local spillovers by only examining the neighboring municipalities as an alternate treatment group. The results are displayed in Table A8. In this case the treatment effect on any of the GPA groups is zero or slightly negative, confirming the presupposition that there appears to be limited local spillovers beyond the treated municipalities. Third, we include municipalities with established universities in the control group (municipalities marked blue in Figure 1). Doing so lowers the size and significance of the treatment effect. One possible interpretation of this result is that individuals graduating from high school in always-treated municipalities may have slightly different educational access and family conditions. That is the reason for why these municipalities were indeed excluded from the rest of the analysis in the first place. Nevertheless, the estimates indicate larger enrollment effects for medium and high GPA individuals in the treatment group when including the municipalities with established universities in the control group (Table A7).

7 Additional Effects

Given that increased access to higher education primarily benefited individuals with middling GPAs, an important question is whether this translated into meaningful long-term differences in their lives. Did these individuals ultimately earn higher incomes, experience different labor market outcomes, relocate, or form families at different rates than their counterparts who were not exposed to the new colleges? To explore these potential consequences, we examine several life outcomes at age 40 without imposing any specific theoretical priors on how improved access to higher education might influence them. Specifically, we analyze marital status, residential mobility (whether individuals live in the municipality where they graduated high school), employment status, and total income (including earnings, capital income, and transfers, expressed in logs).

Results, presented in Appendix Table A4, are based on the same compact difference-in-differences specification used in the main analysis, with a three-way interaction between treatment, post-reform period, and GPA category. Across all outcomes, we find no or only marginally statistically significant effects. In other words, individuals with middling GPAs who were more likely to complete college as a result of the reform did not, by age 40, exhibit higher incomes, better labor market attachment, or different marriage patterns compared to similar individuals in municipalities that did not receive new colleges. However, the results may indicate that they were more likely to exhibit greater geographic mobility, as the propensity to live in the municipality where they graduated high school is reduced by about 2 percentage points.

8 Conclusions

The expansion of more distributed, local universities and colleges across numerous countries has been partly motivated by an effort to reduce inequality in educational access among youth from lower-income and less-educated families. However, these relocation policies have yielded surprisingly modest effects in enhancing educational equality, and in several cases they have increased inequality.

Our findings indicate that the primary beneficiaries of increased college access are youth with medium level GPAs, most likely those who were just below the pre-reform admission cutoff. These results fit well with the pattern that country-level enrollment increases with average income, and that enrollment increases from the top of the ability distribution (Bergh and Fink, 2009, 2008). Under such circumstances policies that aim to promote equality by making higher education more affordable may lead to more high-ability individuals enrolling, and thus to increased wage inequality (Hendel, Shapiro and Willen, 2005).

Notably, local access to the new colleges plays a central role—those graduating from high school in the treated municipalities are 2.4 percentage points more likely to have a college degree at age 40, while those in neighboring municipalities are unaffected, compared to a matched control group. In other words, for high school graduates with middling academic credentials, even a relatively short commute has a substantial impact on the enrollment decision. This "local proximity effect" appears particularly strong for those who just missed the pre-reform admission cutoff.

Prior to the reform, universities were concentrated in a small number of traditional academic centers, such as Lund and Uppsala. The relatively long distance to these established universities did not appear to substantially affect college attainment, likely because the distance was too great for daily commuting; prospective students had to relocate. Those who ultimately enrolled at these institutions were plausibly from more advantaged backgrounds or had stronger academic qualifications, making them less sensitive to geographic barriers. In contrast, the establishment of new local colleges appears to have had the greatest impact on marginal students, whose enrollment decisions were more responsive to the reductions in travel distance.

Moreover, subsequent college enrollment following the policy shock did not vary significantly with parental education—a finding that is reassuring from a social policy perspective. However, the reform did not lead to increased enrollment among youth from non-college-educated families. This absence of an effect on educational inequality suggests that the policy did not meaningfully lower the barriers it aimed to address. It appears that policymakers may have underestimated the role of academic preparedness when asserting that the reform would democratize access to higher education. More broadly, the reform does not appear to have affected any major life outcomes beyond educational attainment.

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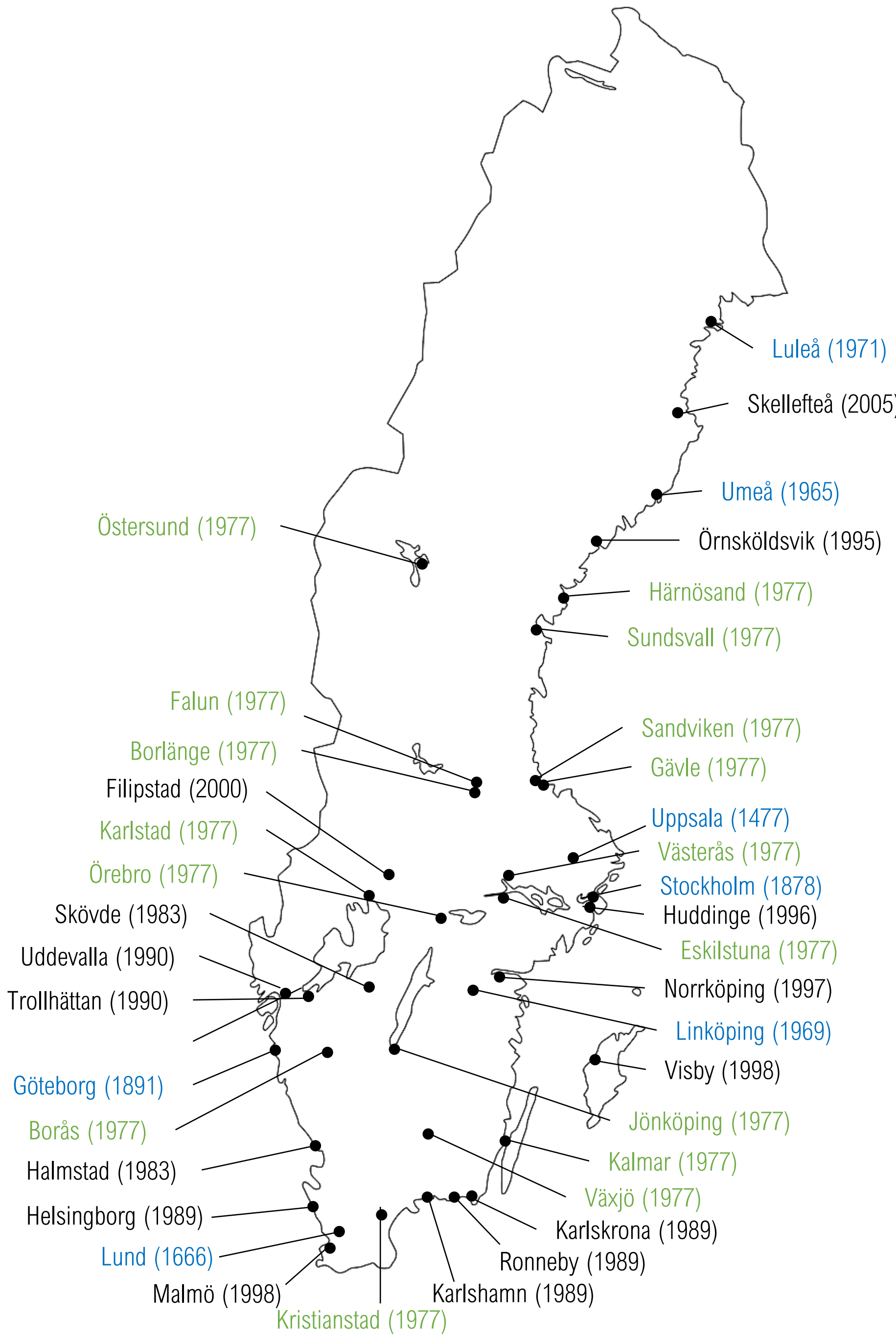

Figure 1: Municipalities with Colleges

The figure displays the locations of the new established colleges. The blue municipalities are those with pre-existing colleges, the green those that get new colleges in 1977, and the red are those with colleges established after the reform in 1977.

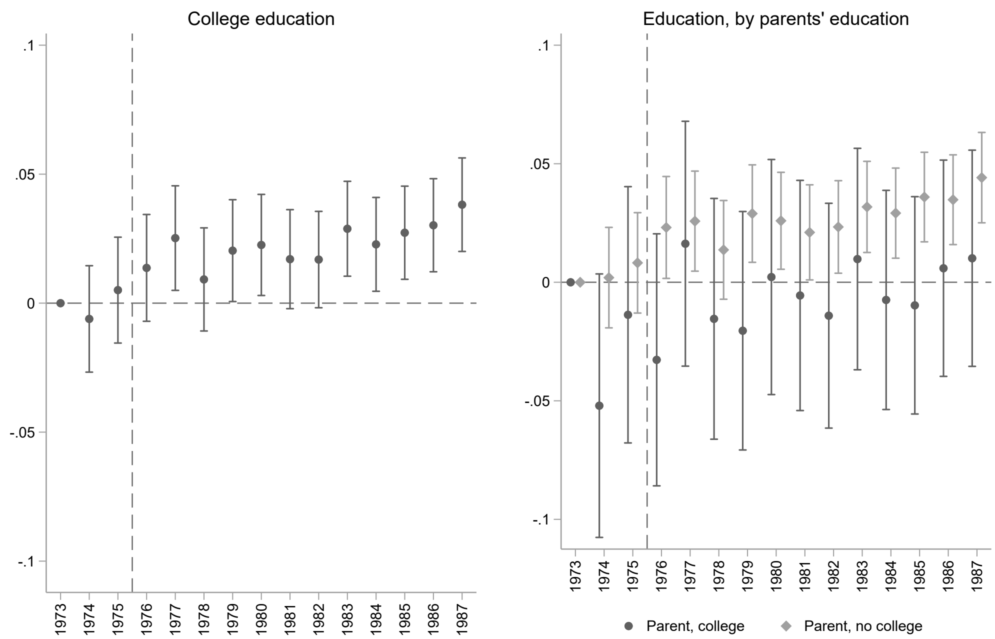

Figure 2: College Education

The figure displays yearly estimates corresponding to Equation 1 where the difference between treated and controls is normalized to zero at 1973. The control group consists of individuals from any municipality which is not previously treated and not treated in 1983. Panel A pools all individuals, while panel B shows the estimates separate for whether at least one parent had a college education or not. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.

Panel A: Pooled **Panel B: By Parents' Education**

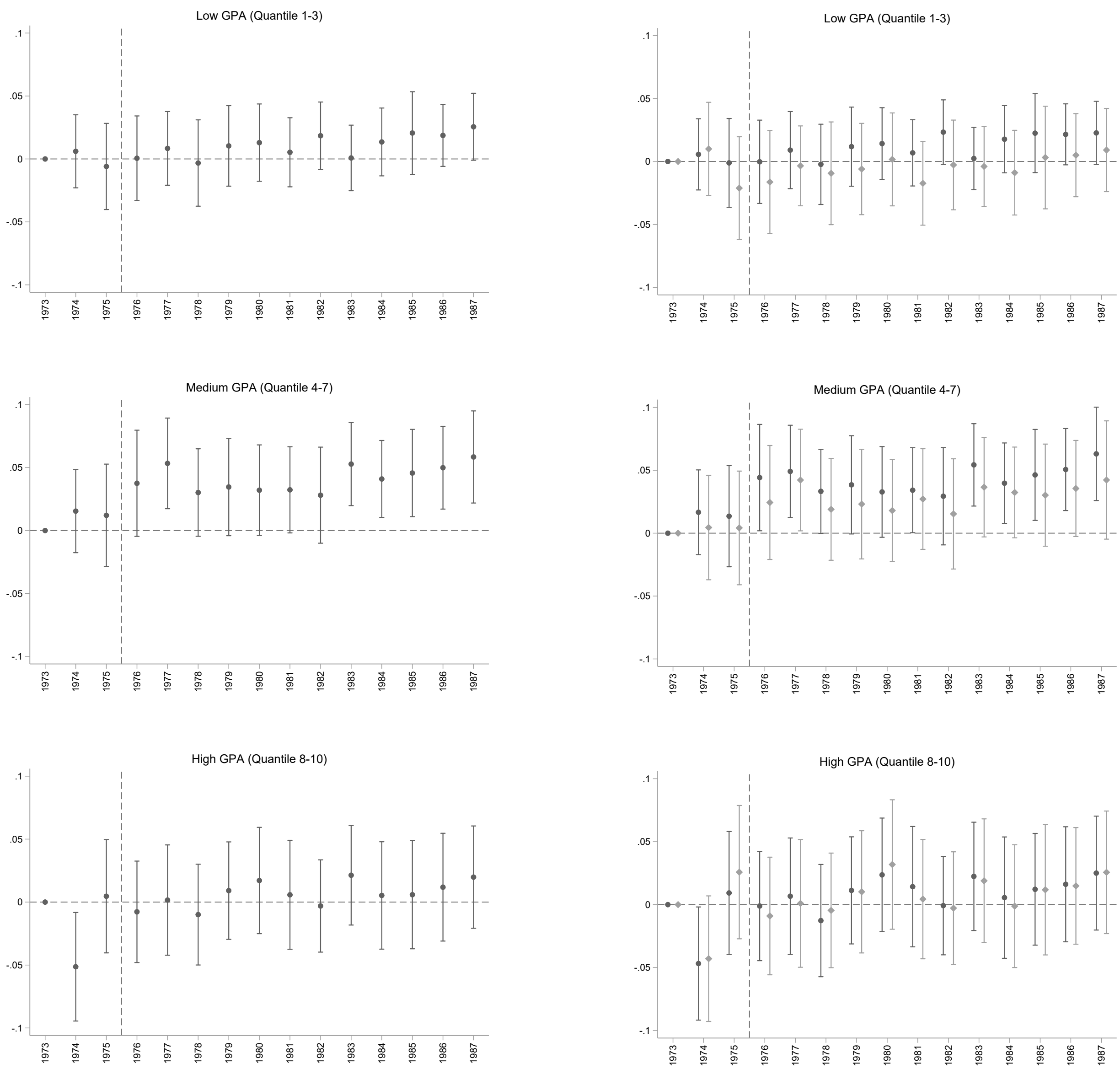

Figure 3: College Education

The figure displays the dynamic estimates from the DiD specification in Equation 1, where the difference between treated and controls is normalized to zero at 1973. The control group consists of individuals from any municipality which is not previously treated and not treated in 1983. Panel A pools all individuals, separated by three groups of GPA quantiles (1-3, 4-7, and 8-10), while panel B shows the corresponding estimates separate for whether at least one parent has a college education or not. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.

Table 1: Descriptive statistics and balancing tests

	Treated			Controls				
	Mean	SD	N	Mean	SD	N	Difference (%)	Norm. Difference
High School GPA	-0.04	0.96	177839	-0.04	0.97	177839	-1.99	0.001
Parent College Education	0.22	0.41	177839	0.22	0.41	177839	0.00	0.000
Taxable Income per Capita (kSEK)	1079.98	96.58	177839	1055.17	177.37	177839	2.35	0.123
Female(%)	0.47	0.50	177839	0.47	0.50	177839	0.00	0.000
Distance to old university (km)	158.23	92.71	177839	150.74	182.99	177839	4.97	0.037

The table shows the mean and standard deviation of the three matching variables, and two other variables of interest. An old university is defined as Stockholm, Lund, or Uppsala. The treated and control sample have been matched on gender, cohort, 10 quantiles of high school GPA (normalized by cohort), and an indicator for whether at least one parent has completed a university education. The final column shows the normalized differences between the covariates, as in Imbens and Wooldridge (2008), where the difference in means is divided by the difference in variance.

Supplemental Appendix for

"Ability, Not Heritage: Why Expanding University Access Often Fails to Narrow Intergenerational Educational Gaps"

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Table A1: College Education by Treatment
Pooled Difference-in-Difference Model

	College Education
Treated	0.00768
	(0.00927)
Post	-0.0287
	(0.00441)
Treated × Post	0.0242
	(0.00542)
Mean	0.346
Standard deviation	0.476
Cluster	Municipality
Individuals	327,262
Observations	355,678

Standard errors in parentheses

The table shows the results from the regression specified in Equation 2. The control group consists of individuals from any municipality which is not previously treated and not treated in 1983. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.

Table A2: College Education by Treatment and Parents' Education
Pooled Difference-in-Difference Model

	College Education
Treated $\times$ Post $\times$ Parent, university	-0.00672
	(0.0141)
Mean	0.346
Standard deviation	0.476
Cluster	Municipality
Individuals	327,262
Observations	355,678

Standard errors in parentheses

The table shows the results from the regression specified in Equation 2, run separately by parental education. The control group consists of individuals from any municipality which is not previously treated and not treated in 1983. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.

Table A3: College Education by Treatment and GPA Groups
Pooled Difference-in-Difference Model

	College Education
Treated × Post × GPA (4-7)	0.0209
	(0.0102)
Treated × Post × GPA (8-10)	0.0117
	(0.0112)
Mean	0.346
Standard deviation	0.476
Cluster	Municipality
Individuals	327,262
Observations	355,678

Standard errors in parentheses

The table shows the results from the regression specified in Equation 2, run separately by groups of quantiles of GPA (1-3, 4-7, and 8-10). The control group consists of individuals from any municipality which is not previously treated and not treated in 1983. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.

Table A4: Life Outcomes by Treatment and GPA Groups
Pooled Difference-in-Difference Model

	Married	Employed	Lives in old municipality	Log total income
Treated × Post × GPA (4-7)	-0.00748	0.00297	-0.0208	-0.0184
	(0.00903)	(0.00568)	(0.0113)	(0.0202)
Treated × Post × GPA (8-10)	-0.0180	-0.00306	-0.0223	0.00745
	(0.00993)	(0.00624)	(0.0124)	(0.0222)
Mean	0.208	0.935	0.434	7.647
Standard deviation	0.406	0.247	0.496	0.881
Cluster	Municipality	Municipality	Municipality	Municipality
Individuals	327,262	327,262	327,262	327,262
Observations	355,678	355,678	355,678	353,634

Standard errors in parentheses

The table shows the results from the regression specified in Equation 2, run separately by groups of quantiles of GPA (1-3, 4-7, and 8-10). The control group consists of individuals from any municipality which is not previously treated and not treated in 1983. The outcome variable in column 1 for being married or not, column 2 is a dummy for employment, column 3 is a dummy for whether the individual is living in the municipality where they graduated high school, and column 4 is the average log total income (labor earnings + capital income). The first three columns are measured at age 40, while the total income is measured as an average between ages 37-40, to mitigate the influence of temporary fluctuations.

Table A5: College Education by Treatment, GPA Groups, and Parents' Education
Pooled Difference-in-Difference Model

	College Education
Treated × Post × GPA Quantile (4-7) × Parent, university	-0.00498
	(0.0347)
Treated × Post × GPA Quantile (8-10) × Parent, university	-0.00922
	(0.0288)
Mean	0.346
Standard deviation	0.476
Cluster	Municipality
Individuals	327,262
Observations	355,678

Standard errors in parentheses

The table shows the results from the regression specified in Equation 2 interacted with groups of GPA (1-3, 4-7, and 8-10) as well as the level of parental education. The control group consists of individuals from any municipality which is not previously treated and not treated in 1983. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.

Table A6: Excluding Neighbouring Municipalities
Pooled Difference-in-Difference Model

	College Education
Treated × Post × GPA (4-7)	0.0197
	(0.0102)
Treated × Post × GPA (8-10)	0.00702
	(0.0112)
Mean	0.344
Standard deviation	0.475
Cluster	Municipality
Individuals	327,416
Observations	355,678

Standard errors in parentheses

The table shows the results from the regression specified in Equation 2 interacted with groups of GPA (1-3, 4-7, and 8-10). The control group consists of individuals from any municipality which is not previously treated and not treated in 1983 and which are not neighbouring the treated municipalities. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.

Table A7: Including Always Treated Municipalities
Pooled Difference-in-Difference Model

	College Education
Treated × Post × GPA (4-7)	0.0273
	(0.0101)
Treated × Post × GPA (8-10)	0.0199
	(0.0112)
Mean	0.346
Standard deviation	0.476
Cluster	Municipality
Individuals	333,566
Observations	355,678

Standard errors in parentheses

The table shows the results from the regression specified in Equation 2 interacted with groups of GPA (1-3, 4-7, and 8-10). The control group consists of individuals from any municipality which were not treated in 1983. In other terms, those treated before 1977 are also included in the control group. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.

Table A8: Only Neighbouring Municipalities Treated
Pooled Difference-in-Difference Model

	College Education
Treated × Post × GPA (4-7)	-0.0132
	(0.0121)
Treated × Post × GPA (8-10)	-0.0261
	(0.0135)
Mean	0.316
Standard deviation	0.465
Cluster	Municipality
Individuals	208,330
Observations	244,962

Standard errors in parentheses

The table shows the results from the regression specified in Equation 2 interacted with groups of GPA (1-3, 4-7, and 8-10). The treatment group now consists of individuals from all municipalities neighbouring the municipalities treated in 1977. The control group consists of any municipality which is not previously treated, not treated in 1977 or 1983, and not a neighbor to any treated municipality in 1977 or 1983. The outcome variable is a dummy for whether an individual has a college education or not at the age of 40.