Inheritance of fields of study

Adam Altmejd*

February 24, 2023

Click here for the latest version.

Abstract

University graduates are more than three times as likely to hold a degree in the field their parent graduated from. To estimate how much of this association is caused by the educational choices of parents, I exploit admission thresholds to university programs in a regression discontinuity design. I study individuals who applied to Swedish universities between 1977 and 1992 and evaluate how their enrollment in different fields of study increases the probability that their children later study the same topic. I find strong causal influence. At the aggregate level, children become 50% more likely to graduate from a field if their parent has previously enrolled in it. The effect is positive for most fields, but varies substantially in size. Technology, engineering, medicine, business exhibit the largest, significant, effects. For these fields, parental enrollment increases child graduation probability with between 2.0 and 12.8 percentage points. I show that parental labor market experience play an important role in explaining the results, but do not find that parental field enrollment increases subject-specific knowledge or returns to earnings. My findings do not support comparative advantage as the key driver of field inheritance. Rather, parents seem to act as role models, making their own field choice salient. This is indicated by the fact that children become less likely to follow parents with really weak labor market outcomes, and that daughters more often inherit the field of their mothers, and sons the choice of their fathers.

*SOFI, Stockholm University and the Department of Finance, Stockholm School of Economics. adam@altmejd.se. I thank Paul Devereux, Per Engzell, as well as seminar participants at SSE, SOFI, and IFN for valuable comments. Marcos Demetry and Andreas Gustafsson provided outstanding research assistance. This research was supported by grants from Handelsbanken (W18–0005), IFAU (152/2020) and Forte (2016–07099).

1 Introduction

Every society needs an adequate level of social mobility to be considered just. Provision of education is integral in ensuring children are given the opportunity to advance. However, occupations are often inherited across generations. An unrelenting, strong, correlation between parents and their children's occupational and educational trajectories is observed throughout the world. Explaining this persistence and identifying ways to increase class mobility has been a key focus of social science research for decades.[1] While much attention has been given to the topic, we still know little about the causal mechanisms explaining this perceived injustice. Causal estimates are important, since they separate direct parental influence from the effect of norms, or other factors, that influence both generations. In this paper, I identify the causal component of field of study inheritance. To do so, I compare parents who apply to study the same university field but end up either above or below an admission threshold. Parents who enroll in a specific field of study cause their children to become 50% more likely to earn a degree from that field. Among significant field-level estimates, the effect is strongest for technology, medicine, engineering, and business, and negative for almost no field.

The choice of college specialization is one of the most consequential decisions an individual makes. A degree from a university field of study is the start of a distinct career trajectory and a necessary prerequisite for many occupations. Because of the large time-span between the field of study choices of one generation and the next, a likely pathway for the intergenerational transmission of fields is occupational inheritance. I confirm that a positive labor-market experience is indeed a key pathway: it is especially those parents who are predicted to earn well who are followed. But research on occupational inheritance often theorizes that children follow their parents because it gives them a comparative advantage. I find little evidence for this being a key driver. Children do not exhibit subject-specific GPA gains from having parents who enroll in a specific field, and when parental earnings are predicted to be in the first quartile, children become *less* likely to follow. My results are easier to justify if the parent is thought of as a role model. Indeed, daughters are more likely to follow their mothers, and sons their fathers.

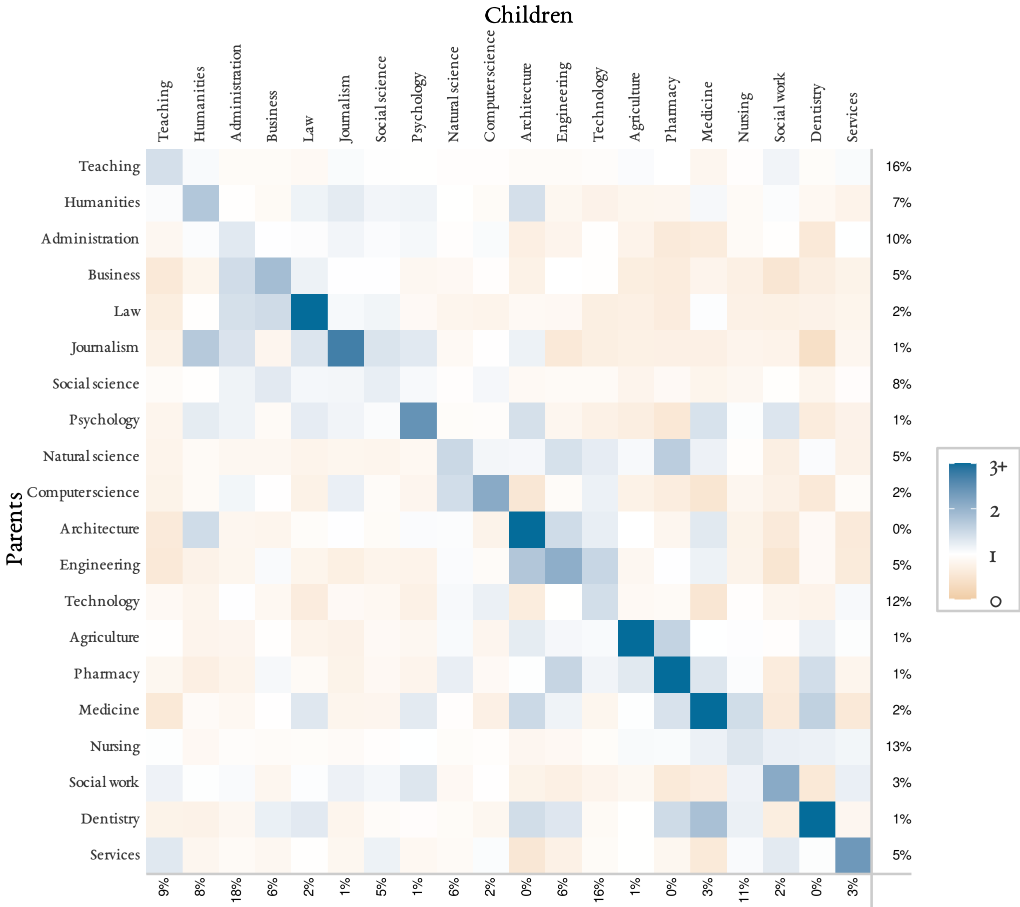
Increasing equality of opportunity is a desired objective in most liberal democracies. To understand how the correlation of educational outcomes across generations is linked to mobility and equality, it is essential to identify and estimate the size of the mechanisms through which these correlations arise. Without deep understanding of these causal pathways, it is hard to design effective policies to improve mobility. My results show that parents exhibit a considerable influence on their children, even in a relatively mobile country like Sweden.

Figure 1 presents a matrix of intergenerational associations for different tertiary degrees. The shade of each cell indicates how much more common a degree is — among children with a parent who holds a certain degree — when compared to the full population of children who graduated from college. While the blue diagonal shows how strong occupational reproduction is, it also visualizes the large variation across fields, with children of dentists earning degrees in dentistry more than 7 times as often as the general population, but children of nurses only being about 1.3 times as likely to become nurses. Importantly, we see no negative relationship on the diagonal. The purpose of this paper is to measure what proportion of this reproduction is actually caused by the educational choices of parents, as opposed to

1. While the empirical study of social mobility began much earlier, intergenerational persistance of education has been researched since at least Becker (1964) and Coleman et al. (1966).

other factors that influence both generations. The causal effects that I find are large, but not nearly as large as the correlations. On the aggregate level, when a parent earns a degree in a specific field it causes the likelihood that their child will do so to increase with about 50%.

Figure 1. Degrees of children and parents



Notes: Grouped by the degree of the parents on the y-axis, the graph shows the relative popularity of different degrees among those parents' children, compared to the baseline frequency of attaining a certain degree. For example, while 4% of the children in the sample earn a degree in law, the rate is about 13% among children with a parent who has a law degree. See table B.2 for the exact values on the diagonal.

To identify this causal effect I study applicants to university programs that are quasi-randomly either above or below cutoffs to different fields, and look at the likelihood that their children also enroll in the same alternative. In other words, I compare parents who all would like to study the same field, but where some end up not being admitted. This estimation framework allows me to identify a causal inheritance effect of parents' education on their children's preferences and outcomes. It does however mean that I identify a local average treatment effect: the estimates are valid for parents who comply with treatment and end up studying something else if they were not admitted.

This paper contributes to several large, but somewhat disparate, strands of literature. Studies of the intergenerational transmission of educational attainment, income, and health, have long attempted at identifying and measuring causal effects.[2] Since social and economic standing permeates generations, this is not a simple task. Families can live in a social stratum where higher education is valued, causing each generation to pursue university education. Such multi-generational, or extended family, human capital associations are even larger than measures across only two generations (Adermon et al. 2021; Lindahl et al. 2015), but are not likely to represent direct causal effects (Braun and Stuhler 2018). Instead, to identify causal effects, many papers exploit policy changes that generate exogenous variation in parental schooling or income[3], or resort to various statistical techniques. Regression control models, instrumental variables methods, or twin relationships have often been used, but it is unlikely that these methods are able to account for all potential sources of bias.[4]

Dahl et al. (2021) is the only other paper that studies causal intergenerational transmission of fields of study specifically. Using a similar econometric design, location, and time period, they estimate causal spillover effects on high school choice across generations and between siblings. The link between high school specializations and occupations is weaker than that from university diplomas, and it is thus not surprising that their intergenerational effects are substantially smaller in magnitude than those presented here. Interestingly, they find similar effects by gender, at least for sons, who also follow their fathers high school choices twice as often. While they find mothers to mainly influence their daughters in fields that are male dominated, Table 12 in this paper shows positive maternal influence on a variety of fields, in most cases stronger for daughters.

The majority of research on intergenerational transmission and social mobility does not attempt to identify causal mechanisms, however. In sociology, measuring and understanding class reproduction is a core objective. Following a body of work that argued that a disaggregated categorization of social class into occupations is needed (Erikson and Goldthorpe 2002; Jonsson et al. 2009; Weeden and Grusky 2005), and since many modern occupations require tertiary diplomas, several recent papers address the intergenerational association of fields of study directly.[5] A common finding is that it is mainly the field of study choices of sons and their fathers that are correlated. Also the causal effects presented in this paper are stronger for fathers and sons. I show, however, that mothers pass their education on to almost the same degree as fathers, a pattern that cannot be explained by assortative mating.

A separate body of research looks at the intergenerational association of occupation choice. An often theorized explanation for the strong correlations illustrated in Figure 1 is that children have a comparative advantage in choosing the same occupation as their parents. They gain this advantage through

2. Surveyed in e.g. Björklund and Salvanes (2011) and Black and Devereux (2011).

3. Oreopoulos et al. (2006) use changes in US compulsory schooling laws to show that a 1-year increase in parental schooling decreases the probability that a child repeats a grade with 2–4 percentage points. Lundborg et al. (2014) make use of a 1950s Swedish compulsory schooling reform to show that maternal schooling improves everything from cognitive skills to health.

4. Some examples include Grönqvist et al. (2017) who show that the heritability of non-cognitive skills is almost as high as that of cognitive skills, and that it is stronger for mothers, and Björklund and Jäntti (2012) who compare the educational correlations of siblings to monozygotic twins to show that the non-genetic role of family background in determining labor market outcomes is substantial. Holmlund et al. (2011) study the causal intergenerational transmission of years of schooling. They compare results from the most common methods to their own and others' IV estimates and show that IV estimates are considerably smaller than the associations identified in control, twin, and adoption studies and argue that this is due to selection issues that have not been accounted for successfully.

5. Van de Werfhorst et al. (2001) find strong associations between fathers' and their children's choice of educational field in the Dutch Family Surveys of 1992 and 1998. Also, the association identified by Hällsten (2010) and Andrade and Thomsen (2017), on Swedish and Danish individuals respectively, is mainly between males. Similarly, Kraaykamp et al. (2013) identify a correlation between parental field of study and the level of education — mainly that sons of parents who study a technical field reach higher educational levels, while daughters to parents with a care field of study attain lower educational level. Hällsten and Thaning (2018) does the opposite, and shows 25% of the variation in field of study choice is explained by a measure of social background that includes the parental level of education.

transfers of occupation-specific resources. Parental human capital can be transmitted actively at dinner table conversations, or when children help their parents with work-related tasks. It can also be passively transmitted through genetic and social endowments. Situations where social endowments are exploited to help a child advance, despite there being better qualified candidates available, are often referred to as nepotism. While all intergenerational persistence could be perceived as unfair, nepotism decreases total welfare. Labor economists have long been interested in studying occupation choice and measuring the degree of nepotism in occupational inheritance.[6] Two studies of particular relevance to this paper address field heterogeneity directly. De la Croix and Goñi (2021) study nepotism in academia throughout history. They estimate intergenerational elasticities and show that nepotism plays a much larger role for legal and medical scholars when compared to researchers in theology and science. Aina and Nicoletti (2018) study intergenerational associations in liberal professions and find especially strong effects for occupations that have high entry barriers because of licensing and compulsory practice periods. This is in contrast to the strongest causal effects identified in this paper, which, except for medicine, are not in fields that yield occupational licenses. Rather than comparative advantage, the patterns identified in this paper speak to understanding field inheritance as a role model effect. For example, Table 9 shows that children are negatively affected by parents enrolling in fields where they are predicted to earn below the 56th percentile.

Finally, this paper contributes to the literature on the relative importance of genetic and environmental effects in explaining schooling outcomes. Any intergenerational association that is not due to genetic endowments is caused by environmental triggers, potentially interacting with genes. Heritability research has found considerable influence on educational outcomes from both genes and environmental factors (Branigan et al. 2013; Polderman et al. 2015). In these studies, all variation that cannot be tied to genetic endowments is attributed to the environment. Using the term "nurture" to describe this residual is somewhat misleading, however, as the studies say nothing about the extent to which these environmental triggers can be controlled. To the contrary, a substantial portion of the residual is likely caused by a multitude of idiosyncratic, random, events (Plomin 2011). To recommend changes to policy or individual behavior, we need to find causal pathways that can be controlled. While recent studies in behavioral genetics have identified the specific genetic markers that are responsible for as much as 10% of the variability of years of schooling (Lee et al. 2018), little progress has so far been made on the environmental side. This paper provides estimates of one such pathway. The paper examines an environmental mechanism that the parent commands, namely how parental field specialization directly influences educational preferences and degree completion of children. While the effect identified is a miniscule part of

6. Important early work includes a number of papers by Lentz and Laband. They show that children of doctors are more likely to be admitted to medical school (Lentz and Laband 1989), that lawyers transfer legal know-how to their children (Laband and Lentz 1992), that farmers tend to be sons of farmers because the experience they gain while growing up gives them a comparative advantage (Laband and Lentz 1983), and argue for an analogous mechanism explaining inheritance of entrepreneurship (Lentz and Laband 1990). Similar findings are presented in a more recent paper by Hvide and Oyer (2018), who show that male entrepreneurs are likely to start a business in an industry in which their fathers are employed — and those who do are likely to outperform other entrepreneurs in that industry, and Bell et al. (2019) who find that growing up in an area with many innovators has a causal impact on the likelihood that an individual registers a patent. Dunn and Holtz-Eakin (2000) argue that the transition to self-employment is better predicted by parental self-employment success than individual or parental financial resources. Additional important papers that identify causal effects are Bennedsen et al. (2007) who exploit the random gender of the first child to show that the appointment of a family CEO has large negative effects on firm performance, Dal Bó et al. (2009) who use discontinuities in election outcomes to show that political success builds dynasties, as well as Mocetti (2016) and Mocetti et al. (2022) who exploit deregulation in Italy to show that a decline in occupation-specific rents together with increased competition, reduces intergenerational persistence.

what heritability studies would ascribe to the "environment", it entails one of the first precisely estimated environmental causal pathways.

To summarize, social scientists have long studied transmission of education from parents to their children. Because of the difficulty to attain experimental data that spans generations, the field is, until recently, void of causal analyzes of these important effects. The contribution of this paper is to estimate the magnitude of the causal transmission of university fields of study and in doing so increase the understanding of how education, and more generally social status, is transmitted over generations. The findings are important for researchers, policy-makers and parents alike. Policymakers who want to increase mobility need to account for this self-perpetuating mechanism by providing children with additional role models to ensure they have enough knowledge about alternative careers. Some university applicants might reconsider their choices knowing how they might impact the education of their children, and parents who do not want their children to follow in their footsteps probably need to give additional attention to alternative pathways.

This paper is organized as follows. I start in Section 2 by presenting the Swedish education system, the data that I use, and how it is processed to identify the admission margins that can be used in a regression discontinuity design. In Section 3, I then describe the identification strategy and the model that I will estimate, after which I outline my main results in Section 4. I show that these results are stable and robust to various placebo checks in Section 5, and explore mechanisms in Section 6. Last, Section 7 concludes by summarizing the results and their relevance.

2 Institutional background and data

Swedish tertiary education is tuition-free and government run. All students are offered stipends and subsidized study-loans. Students apply through a centralized admission system. During the fall semester of 2018, 1817 different programs (at both undergraduate and graduate level) were offered at 37 institutions. Like in many other European countries, individuals apply by submitting a preference ranking of alternatives. Each alternative is a program at a specific institution. If completed, programs award the student with a field-specific bachelor's or master's degree. When a program is oversubscribed, students are sorted by previous academic performance in different admission groups and only those with the highest score are admitted. Importantly, there is no system of legacy admissions, ensuring that children have no mechanical advantage in admission probability if they apply to the same program as their parents studied.

In this paper, I use data on university applications submitted between 1977 and 2021 through the centralized application system in Sweden.[7] For the RDD analysis I include individuals who applied to university between 1977 and 1992. I then match these applicants to their children (if they have any) for which I observe applications until the end of 2021.

I use university application data from three sources. Applications from the current admission system (2008–2021) comes from Universitets- och Högskolerådet (UHR). Older applications are retrieved from the UHÄ (1977–1992) and VHS (1993–2005) archives at the Swedish National Archives

7. It became mandatory for institutions to offer their programs through the centralized system only in 2005. While most universities participated from the start of the sample period in 1977, some joined later or only included a subset of their offered programs. Participation increased monotonically however so the programs applied to by parents will always exist in the data when I study the behavior of their children.

(Riksarkivet).[8] I link the applications using individual identifiers to data from Statistics Sweden (SCB) on enrollment, degrees, high-school performance, socio-economic characteristics, and family connections, recorded up until 2021.[9]

To be eligible for post-secondary education, applicants must have finished high school or, for earlier years, have at least four years of labor market experience. Certain college programs also require passing grades in specific high school subjects. Engineering programs, for example, often require completion of high school classes in science and math. Individuals who have not taken these courses in high school can supplement diplomas with preparatory adult education to become eligible.

Each semester has its own application period, with submission deadlines in mid-April and October. Applicants submit ordered lists of up to 12 (20 after 2005) program-institution combinations, below referred to as choices or alternatives.[10] All applicants to a given alternative are ranked by their score in the admission groups that they are eligible for. The set of available admission groups varies over programs and time. For example, during a transition between high school grading systems, separate groups were used for each system — students with older high school diplomas were only competing against other students with the same kind of grades, while those with newer diplomas were admitted in a separate group. There are specific groups for admission through Högskoleprovet (a standardized non-mandatory admissions exam similar to the SAT). During 1977 to 2005, applicants who had work experience could compete in a group where the number of years they had worked gave bonus points. During part of this period, there was also a group for which one was eligible only for the first three years after graduating high school. The number of spots reserved for each admission group is proportional to the number of eligible applicants in that group. To account for selection into these groups and that admission scores are not always directly comparable, I standardize scores separately for each group and year. In the regressions, I include cutoff fixed-effects, unique for each semester-institution-alternative-admission group combination, and separate polynomials for the running variable in each admission group.

Each application period consists of two rounds. During each round, an allocation mechanism admits students to alternatives until either all slots have been filled or all applicants have been admitted. Applicants are ranked by score in every admission group that they are eligible for and then admitted one by one. Each admission group is attributed a set of slots, decided partly by fixed rules set by national regulation or the institutions, and partly in proportion to the total number of applicants in the group. If an applicant is eligible to be admitted in multiple admission group, they are admitted in the group that has the most slots still open. An applicant that is admitted in one group is removed from the queue in all other groups. After all slots are filled, applicants admitted to higher prioritized alternatives are removed from options they had ranked lower and replaced by the next individual in line from the same admission group. Once no more individuals are being admitted, the process stops and offers are sent out. Applicants then decide whether to accept their offers, and whether they want to stay on the waiting list

8. Data is unfortunately missing for the fall semester of 1992, and there is only partial data available for the years 2006 and 2007.

9. Information on degree completion comes from Utbildningsregistret (UREG), which includes both registered degrees awarded by Swedish institutions and information about highest achieved education collected through surveys and other sources. Family connections are retrieved from Flergenerationsregistret. To ensure I include all potential family members in the same family identifier (used for clustering of standard errors) I count the complete network of individuals connected through children as the same family, but study only biological and adoptive parents when measuring inheritance. If two divorced parents have additional children with new partners, all children are included in the same family identifier.

10. In the current system, in use since 2005, students can apply to both degree programs and individual courses in the same application. Before 2005, only applications to degree programs where handled in the centralized system. Naturally, for parents, I therefor only look at applications to degree programs. In the current system, during which most of the child applications are observed, I also include applications to individual courses for the outcome variables related to applying or enrolling in a field.

for admission to higher prioritized alternatives. The admission procedure is then repeated in a second round.[11]

When applicants are sorted by their admission group scores, ties need to be broken. Because admission scores are coarse, 55% of admission cutoffs have more than one applicant exactly at the threshold.[12] During the period studied in the RDD analysis, two different tie-breakers were used. Applicants with identical scores were first prioritized by the rank of the alternative in their application list (used during 1977–2005), then by a random number.

Disregarding tie-breaking, the allocation mechanism is a truncated multicategory serial dictatorship, a mechanism that is not strategy proof but still minimally manipulable (Balinski and Sönmez 1999; Pathak and Sönmez 2013).

After successful admission, students enroll by simply attending initial lectures. Since students need to complete academic credits each semester to not lose their stipends, enrollment and credit reception is centrally registered at the course level. I use this enrollment data both to instrument for parent admission and as an outcome variable.

Having collected enough academic credits and fulfilled various other requirements (like writing a thesis), the student can apply for a field-specific degree at the Bachelor or Master level. These degrees are registered by SCB in Högskoleregistret. I use child degree completion in the parent's field as the main outcome variable in the paper. It happens, however, that individuals get a job before finishing all requirements to apply for a degree. Moreover, because degrees are completed several years after initial enrollment, children who follow their parents might not have finished their degrees yet. It is therefore likely that the effect on child enrollment is larger than that on degree completion. In combination, these outcome variables yield an interval of inheritance strength.

2.1 Sample construction and description

For the raw application data to be used in a regression discontinuity analysis I first process it in the following way. I build on Kirkebøen et al. (2016) in defining my sample and estimation strategy. First, I identify cutoffs for each admission group, defined as the lowest score among all admitted students. Cutoffs are only defined for those alternatives and admission groups where there are also applicants who were not admitted at the end of the application round. I drop applicants who were admitted in non-standard admission groups and institutions that only offer practical programs, since their admission scores cannot be used for RDD analysis.

I use cutoffs, admission status, and individual scores from the final admission round, but keep individual rankings from the first round. The reason is that second round outcomes are influenced by responses to first round offers. Applicants often drop out of the waiting list for choices that they would have been admitted to if they stayed. Using second round scores to calculate cutoffs increases accuracy of the first stage greatly (because otherwise a much larger share of applicants directly below the cutoff would be admitted eventually), and is not a problem since applicants do not know what the cutoff will be when they apply or when they decide what to do after the first round. It is critical to use first-round preference rankings however, even if this decreases accuracy.[13] The reason is that there is likely selection

11. For a more detailed description of the algorithm governing the admission process see the legal case *T 3897-08* (2009) in Uppsala Tingsrätt.

12. The mean number of applicants at the threshold is 3.5 and at the 99th percentile of applicants at the cutoff there are 27 individuals with exactly the same score.

13. This is the main reason why the first stage for admission, the first plot in Figure 2 does not jump from 0 to 1. A substantial portion of those above the cutoff drop out after not being admitted in the first round.

among those who are not admitted in the first round but decide to stay. Such selection would bias the causal estimates of the RDD analysis.

I collapse admission groups for each choice and use only the group where the applicant performed the best (had the highest relative score). If they are below the cutoff in all groups, this is the group where they would have been admitted if the cutoff was slightly lower. If they were admitted, it is the group that was used for admission. I drop dominated alternatives, where a lower ranked choice has a higher cutoff and where the applicant would thus never be admitted.

I then proceed to create observations of pairs of preferred (j) and counterfactual (k) fields and classify fields into both manually constructed broad categories, and into a more narrow classification that has been created by Statistics Sweden. Furthermore, I collapse consecutively ranked options to the same field, keeping the program where the applicant performed the best (had the highest relative admission score). This could be applications to the same field at different institutions, or to different programs within the same field at one university, or both. Most displays in the paper include results for both broad and narrow fields. Summary statistics for narrow fields are available in Appendix Section C. The broad categorization has the benefit that the difference between categories is normally large. Since the analysis only includes applicants on the margin between different fields, broader categories lead to slightly larger treatment effects. The downside is that the categorization, created by the author, is somewhat arbitrary. The narrow categorization, called SunGrp by Statistics Sweden, is official and created to map educations to related occupations. It is however much more detailed, with e.g., four different fields that map to the broad field technology.

In Appendix Section D, I study additional treatment margins and collapse the individual rankings by, institutions, commuting zones, or institution-field combinations.

The finalized right-hand-side data used for analysis consists of treatment pairs. An observation includes a preferred field j, and a counterfactual field k to which the applicant would be admitted if they are below the cutoff to j. I keep all such combinations for each applicant. For a specific applicant, the sample can contain multiple observations where the applicant is below the cutoff to a preferred alternative j but at most one where he or she is above.[14]

I merge this right-hand-side data of parent field pairs to information about children, allowing each parent's observations to be joined to all their (biological or adoptive) children. In each specification, the outcome variable is set to 1 when the child applies to, enrolls in, or graduates from the field j that their parent preferred, and 0 otherwise. This includes children who do not apply to university at all during the sample period, and parents who have no children.

In the analysis, I focus on parents who apply to university during 1977–1992 and are below the age of 30 when they apply to university. I include both those applicants who have children and those who do not, since removing applicants without children would condition on a post-treatment outcome. Since the application, enrollment, and degree completion data ends in 2021, it is likely some children have yet to follow their parents. This, and the fact that applicants without children are included, means the reported coefficients are conservative and can be seen as lower bounds.

Table 1 shows summary statistics for the main sample of analysis (second column), but also how this data set differs from all applicants (first column). Inside the bandwidth, the sample is further filtered to parents with children born 1998 or earlier (third column) or to observations where the child actually applies to university (fourth column). Differences across the samples are small, except that children in the last two columns are older and have a slightly higher GPA if they apply. Notice also how each parent is

14. This means that for each applicant there is a lowest-ranked pair where being below the cutoff to j means the applicant is not admitted to any option. In total, about 75% of observations have "nothing" as the next-best field. The second most common counterfactual field is Technology with approximately 4% of the sample.

observed on average slightly less than two times (included separately for each child), and how the number of observations is much larger than the number of applicants (separate observations for each threshold the applicant is close to are included). Furthermore, as we will see in the regression results, the share of children who get a degree in the preferred field of their parent (j) is small. There are multiple reasons why almost twice as many children enroll than earn a degree in the field of their parent. But the main one is likely that most children are studying at the very end of the sample period and have yet to complete their studies. Furthermore, many students likely study the field of their parents as minor subjects, never earning a degree. A smaller part is due to dropout.

Table 1. Summary statistics

	All applicants	In bandwidth (1.5 std)	Child born ≤ 1998	Child applies
Application score (std)	0.05	0.16	0.24	0.24
	(1.04)	(1.00)	(1.03)	(1.01)
Parent birthyear	1963.93	1963.96	1961.55	1962.49
	(4.99)	(5.03)	(4.51)	(4.69)
Parent age at treatment	20.86	20.87	21.36	21.07
	(2.56)	(2.56)	(2.91)	(2.68)
Parent female	54.96%	54.38%	58.92%	57.15%
Parent foreign born	3.70%	3.58%	3.59%	3.22%
Grandparents foreign born	6.52%	6.37%	6.13%	5.73%
Grandfather's earnings (kSEK)	372.49	375.44	363.72	376.35
	(281.56)	(281.50)	(249.53)	(259.32)
Grandmother's earnings (kSEK)	185.42	186.69	173.89	180.58
	(117.47)	(119.06)	(106.95)	(109.65)
Grandfather has university education	38.67%	39.39%	34.86%	38.44%
Grandmother has university education	36.59%	37.20%	31.39%	35.29%
Child birthyear	1996.83	1996.90	1991.76	1993.60
	(7.48)	(7.50)	(4.81)	(5.42)
Child female	48.62%	48.66%	48.65%	52.19%
Child high school GPA	0.50	0.52	0.50	0.66
	(0.93)	(0.92)	(0.93)	(0.85)
N. treated applicants	360 500	321 147	179 424	195 324
N. uinique applicant × child	659 866	589 634	350 107	352 274
N. children who rank j first	104 845	91 458	75 381	91 458
N. children who enroll in j	82 823	72 465	60 762	72 165
N. children who earn a degree in j	43 064	37 201	35 017	36 685
Observations	999 252	840 926	443 285	454 339

Notes: The leftmost column includes all applicants to Swedish universities between 1977 and 1992 who apply through the centralized application system and are 30 years or younger at the time of application. The second column filters out those who are within the bandwidth of 1.5 standard deviations from either side of the admission cutoff, and is used for the main analysis in this paper. The third and fourth column focus on those applicants inside the bandwidth who have children. In the third, I summarize observations of applicants with children who were old enough to apply before the end of the sample period in 2021. In the last column, I instead limit the sample to include all children who apply to university before the end of 2021.

Table 2 shows additional results for the main sample of analysis. Here, the data set has been divided by broad fields of study. We see that some subjects are much more common and that the first stage coefficients vary substantially. Both these factors influence the weights of each field in any aggregated results reported. Enrollment below the cutoff happens when applicants reapply and enroll within five years of being treated.

Table 2. Summary statistics by parent field of study

	Observations	Unique parents	Share women	Average age	Share enrolled below cutoff	First stage (parent enrolls)
Teaching	159 007	75 057	78%	20.77	41%	13p.p.***
Humanities	28 338	14 910	72%	20.89	28%	9p.p.***
Administration	32 594	16 700	59%	21.03	32%	11p.p.***
Business	120 387	59 341	46%	20.95	35%	18p.p.***
Law	54 954	27 832	56%	20.58	27%	16p.p.***
Journalism	14 290	7298	64%	21.77	10%	38p.p.***
Social science	44 133	22 446	63%	20.89	15%	15p.p.***
Psychology	9588	4809	66%	23.25	18%	25p.p.***
Natural science	50 460	25 229	45%	20.30	39%	9p.p.***
Computer science	35 127	17 773	38%	21.02	23%	18p.p.***
Architecture	12 460	6158	55%	20.94	18%	35p.p.***
Engineering	123 706	60 671	22%	20.24	48%	15p.p.***
Technology	21 922	11 155	27%	20.41	35%	12p.p.***
Agriculture	14 440	6655	51%	20.97	33%	22p.p.***
Pharmacy	8809	4277	83%	20.42	22%	17p.p.***
Medicine	31 136	13 805	44%	22.01	48%	18p.p.***
Nursing	6820	3388	80%	23.17	30%	16p.p.***
Social work	42 674	20 990	80%	21.50	26%	14p.p.***
Dentistry	10 900	5199	52%	21.45	39%	7p.p.**
Services	19 181	9869	78%	21.17	10%	14p.p.***

Notes: The table shows the main sample of analysis: parents who apply during 1977-1992, before the age of 30 and are within 1 standard deviation of the admission cutoff. The observations are summarized separately for each field of study. The last column shows the disaggregated first stage coefficients, i.e. the increase (in percentage points) of the likelihood that the parent will enroll in their preferred field j if they are above the cutoff. For narrow fields see Appendix Table C.1.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

3 Empirical framework

As we saw in Figure 1 the choices of parents and their children are strongly correlated. But this empirical correlation could be explained by external factors, and should not be understood as causal. In fact, causal transmission effects across generations are very difficult to measure. It is hard to distinguish external influences from effects directly stemming from the parents' behavior. For example, the education and income level of the grandparents or other family members could influence the field of choice of both parents and their children. A family could have a tradition of promoting medical studies going back generations. In addition, the genetic factors that we know strongly influence educational outcomes most likely also have an effect on fields of study choices.

To correctly identify the causal effect of parental education on child preferences I employ a regression discontinuity design (RDD). RDD estimates the causal effect under fairly weak assumptions, but put strong requirements on the data (Lee and Lemieux 2010). As long as treatment assignment is not perfectly manipulable around the cutoff, RDD coefficients can be interpreted as causal effects.

I use the methodology to study individuals who apply to university between the years 1977 and 1992. I compare the behavior of the children of those parents who are above an admission threshold to the children of parents below. If the identifying assumptions hold, each admission cutoff can be seen as a separate natural experiment. I pool a large set of such experiments of admission to different education programs and institutions.

For each parent p, child c, alternative j, and next-best option k, I estimate the reduced form equation

$$\text{Child follows to } j_{pcj} = \alpha \mathbb{1}\left[a_{p\tau} \geq 0\right] + f\left(a_{p\tau}; \theta^g\right) + X_{pj}\gamma + \mu_\tau + \kappa_k + \varepsilon_{pcj}. \tag{1}$$

Admission thresholds are indexed by τ with the score required for admission being $\bar{a}_\tau$. Note that each alternative j has multiple cutoffs τ. On top of each admission group (g) having its own threshold, j can consist of multiple choices as it contains many collapsed alternatives (programs within the same field).

I control for the cutoff-centered running variable $a_{p\tau} = a_{pg} - \bar{a}_\tau$ with the help of a linear polynomial $f\left(a_{p\tau}; \theta^g\right) = \theta_0^g a_{p\tau} + \theta_1^g a_{p\tau}\mathbb{1}\left[a_{p\tau} \geq 0\right]$, that is estimated separately for each admission group g and above and below the cutoff. With 13 admission groups in the main sample of analysis, a total of 26 linear polynomials are included. Estimating the polynomials at the admission group level rather than separately for each cutoff requires assuming unchanging relationships between scores and outcomes across cutoffs within the same admission group. This assumption is relaxed in Table 6, where separate polynomials are included for each cutoff. While the results stay approximately the same, this exercise decreases statistical power substantially.

μ_τ are cutoff fixed effects, and κ_k fixed effects for the next-best alternative k. In total, the main regression controls for 23 687 cutoffs and 21 next-best broad fields.[15] Last, X_{pj} is a matrix of controls and includes fixed effects for parent age and gender as well as the priority ranking of the alternative j in the parent's application.

While the reduced form effect estimated by equation 1 is the most likely to be correctly identified, it is more interesting to understand the effect of actually studying, graduating, or even working in a specific field. To get at these concepts, I employ a fuzzy design and use whether the parent was above the cutoff or not as an instrument:

$$\text{Child follows to } j_{pcj} = \beta \text{Parent enrolls in } j_{pj} + f\left(a_{p\tau}; \psi^g\right) + X_{pj}\delta + \nu_\tau + \xi_k + \upsilon_{pcj}, \tag{2}$$

$$\text{Parent enrolls in } j_{pj} = \pi \mathbb{1}\left[a_{pjg} \geq 0\right] + f\left(a_{p\tau}; \phi^g\right) + X_{pj}\rho + \eta_\tau + \chi_k + u_{pj}, \tag{3}$$

and similar for degree completion. In fact, it would have been even more interesting to know the effect of whether the parent works in an occupation related to the field of study or not. However, as we shall see below, the further in time we get away from treatment (admission), the less likely it is that the assumptions required to interpret the IV coefficient β as a local average treatment effect will hold. Throughout this paper, I will therefore report IV results for both enrollment and degree completion, but focus on the former, since these are more likely to be unbiased.

15. Fort et al. (2022) show that these fixed effects are needed to ensure the pooled estimates can be interpreted as treatment effects.

What are the threats to properly identifying the local average treatment effect (LATE)? The exclusion restriction holds if crossing the threshold only impacts child outcomes through enrollment (or graduation). Since a parent who is admitted but does not enroll learns little about a field, there are not many ways in which exclusion could be violated. One important channel to consider is how treshold-crossing changes the timing of education, and, in turn, later important events. Eager applicants below the cutoff might reapply until admitted, potentially delaying their graduation by several years. If this results in later child-rearing or labor market entry, it could also influence the field of study choices of children. Thankfully Appendix Table A.4 shows no such relationships. To account for reapplication, I include field enrollment and degree completion which happen within 5 and 8 years respectively. This should alleviate concerns that applicants who reapply could invalidate the exclusion restriction.

Instrumenting for degree completion presents additional threats to exclusion, however. Since a degree takes several years to complete, it is possible that also a parent who never earns a degree gains enough knowledge from their studies to impact the education trajectories of their children and voiding exclusion. This threat grows stronger the further into the future and away from treatment we get, and makes instrumenting for e.g., if the parent works in an occupation related to the field, highly problematic.

For IV to estimate the LATE, also the monotonicity assumption needs to hold. Since pairs of preferred and counterfactual fields approximately reflect true relative preferences, crossing the threshold should not make individuals more inclined to enroll in the counterfactual field k. While the admission mechanism is not strategy proof[16], the monotonicity assumption only requires that for any pair of alternatives in the ranked list of options, the applicant prefers the alternative with a higher rank. While there are good reasons for applicants to include safe options in their application, an applicant going against this assumption would be strictly worse off, making it a highly unlikely behavior. In other words, there should not exist any applicants defying treatment, ensuring that the monotonicity assumption holds.

In addition to these classical conditions, the setting studied in this paper requires additional assumptions. First, Kirkebøen et al. (2016) show that another assumption is needed for the IV models to estimate the LATE when there are heterogeneous unordered treatments (fields of study with different next-best fields). The *irrelevance* condition holds if, when crossing the threshold to a specific alternative j does not make the individual enroll or graduate in j, it also does not make them enroll or graduate in another field j'. When paired with fixed effects for the next-best alternative k, this assumption ensures we estimate the LATE. Does the assumption hold? Again, it seems probable that it holds for enrollment, since admission has little effect on an individual than through their possible enrollment. For degree completion, it is possible that admitted applicants who do not complete their studies become more likely to graduate from a related field. For example, someone who almost finishes an engineering degree can count most of their credits towards a degree in the more practical field of technology.[17]

Furthermore, even if exclusions, monotonicity, and irrelevance hold, a recent paper argues that the IV estimator, β, captures the LATE of enrolling in j on child education choices if and only if the specification includes *rich covariates* (Blandhol et al. 2022). Otherwise, the IV estimand will actually contain negatively weighted always-takers. In our case, since admission is quasi-random when comparing those above and below a specific cutoff, inclusion of cutoff fixed effects ensures that the model is saturated.

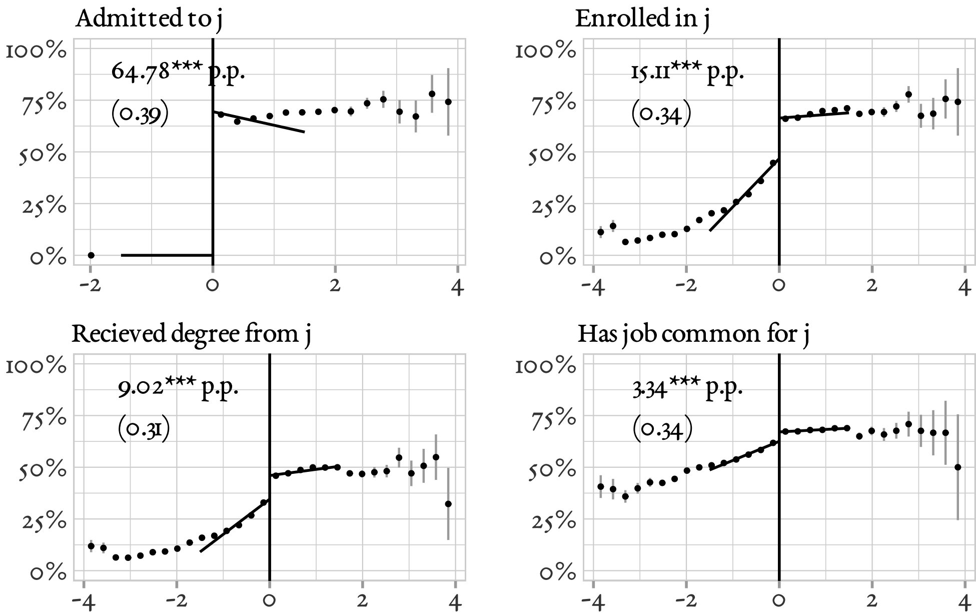
16. Truncation makes it rational for the applicant to add a safe option to the end of their priority ranking. Only 3.6% of applicants submit a full list with 12 ranked alternatives, however. In addition, the priority based tie-breaking creates extra motivation to include safe options, potentially higher in the ranking. However, the main results remain unchanged when looking only at admission to top-ranked options in Table A.3.

17. It should be noted that Kirkebøen et al. (2016), in an estimation strategy that is very similar to the one used by this paper, instrument for degree completion and argue that the irrelevance condition does hold.

To summarize, while it should be quite safe to interprate IV estimates using parental enrollment as LATE, it is not certain all assumptions hold when instrumenting for degree completion. Since obtaining a degree is a central pathway through which any field inheritance must work, I have nonetheless included estimates from such a specification in the paper. These results should be interpreted with caution, however.

Finally, for an IV approach to be meaningful the first stage must have an adequate effect on the instrumented variables. Figure 2 and Table B.1 show clear jumps at the cutoff for parental admission, enrollment, degree completion, and employment in a field-typical job. The paper only includes results for the three first variables, since apart from the exclusion restriction likely not holding, the first stage coefficient for the last variable is small. All results tables in the paper report first stage Wald statistics, which are far above conventional weak instrument thresholds.

Figure 2. Treatment take up around the cutoff



Notes: The plot shows admission, enrollment, degree completion, and employment in the preferred broad field j above and below the cutoff. Admission score is standardized by semester and admission group and centered at the cutoff. Applicants with a score exactly at the cutoff but where a tie-breaking mechanism has ensured they are not admitted have been included in the bin below the cutoff. First stage coefficients from Table B.1 are reported in percentage points within each plot, with standard errors in parentheses.

I include multiple definitions of the outcome variable to assess the strength of the transmission effect. In the first specification, following means that the child ranks the parent's field j highest in their own application (called "Ranks 1st" in the regression tables). The results for this outcome measure are very similar to studying whether the child applies to j at any rank, but defined this way the outcome unambiguously reflects education preferences. I also study if the child enrolls or earns a degree in j.

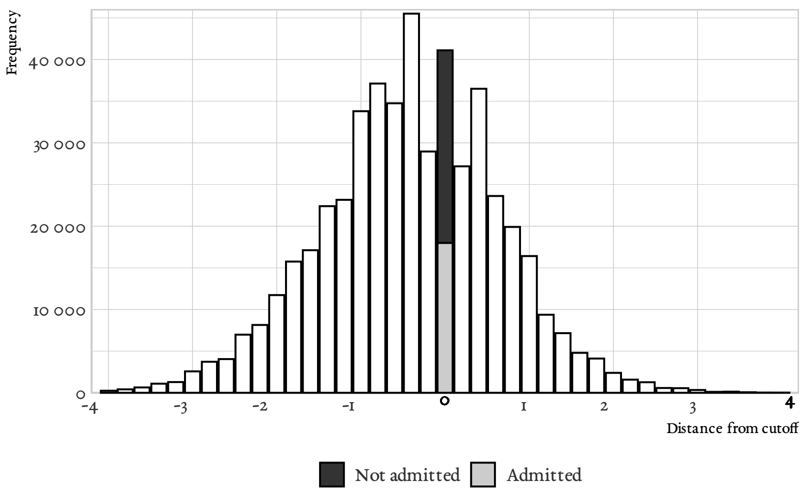
In addition to the aggregate estimates, many results are reported separately for each field j. Such analyses are from joint estimations, where treatment is interacted with the field the parent applies to. This procedure yields smaller standard errors than separately estimating inheritance for each field, since fixed effects and controls can be estimated on the full sample.

I estimate the regressions using OLS and 2SLS by first demeaning the data by the fixed effects using the R package fixest (Bergé 2018). Unless otherwise stated, I include applications with scores at most 1.5 standard deviation away from the cutoff. Since the results are weighted averages of a large set of cutoffs, traditional optimal bandwidth calculations do not apply.[18] Figure 7 shows that the results are robust to the choice of bandwidth size. I use 1.5 standard deviations because it is at this level the aggregate effects in Figure 7 are the lowest. In addition, linear controls for the running variable are likely inadequate for larger bandwidths (see Figure 4). Furthermore, observations are weighted using a triangular kernel, giving linearly decreasing weights to observations further away from the cutoff.

A first validation of the data can be seen in the balance table of Table 3. Here, whether the parent is above the cutoff is regressed on variables that are all defined before treatment. In addition, the regression the same fixed-effects and polynomials as the main specification. A quasi-random admission of applicants should not be statistically related to these outcomes. None of the variables are statistically significant at conventional levels, nor is a joint test of the effect of all variables significant.

Figure 3 provides a second validation, and plots the distribution of the running variable. Applicants exactly at the cutoff (where a tie-breaker has been used) are sorted into a separate bin and their admission status is indicated in shades of gray. In the main analysis, these applicants are counted as below the cutoff whenever the tie-breaking procedure would predict them to not be admitted, and above the cutoff otherwise. The analysis in Table A.2 instead excludes these observations without much change to the estimates, but at a loss of power. In Figure 3, we see no indication of bunching on either side of the cutoff.

Figure 3. Histogram of the running variable



Notes: Histogram of the running variable around the cutoff. Applicants exactly at the cutoff are sorted separately and the shade of the middle bar indicated whether the tie-breaking mechanism admitted them or not.

18. Calonico et al. (2014) optimal bandwidths, calculated on the pooled and cutoff-demeaned data, range from 0.8 to 1.5, depending on the specification.

Table 3. Covariate balance

	Separately estimated	Joint model
Parent female	0.001	0.001
	(0.001)	(0.001)
Parent age	$-0.001^{\dagger}$	$-0.001^{\dagger}$
	(0.000)	(0.000)
Parent born outside of Sweden	0.002	0.006
	(0.003)	(0.005)
Grandfather's age at parent's birth	0.000	0.000
	(0.000)	(0.000)
Grandmother's age at parent's birth	0.000	0.000
	(0.000)	(0.000)
Both grandparents born outside of Sweden	−0.001	−0.005
	(0.002)	(0.004)
Grandparent earnings pt	0.002	0.000
	(0.003)	(0.003)
Uni. educated grandparents	0.002	0.002
	(0.001)	(0.001)
Grandparent degree in j	−0.002	
	(0.003)	
Cognitive skills	0.002	
	(0.002)	
Non-cognitive skills	−0.002	
	(0.001)	
Observations		811 661
Wald statistic		1.025
		[p=0.414]

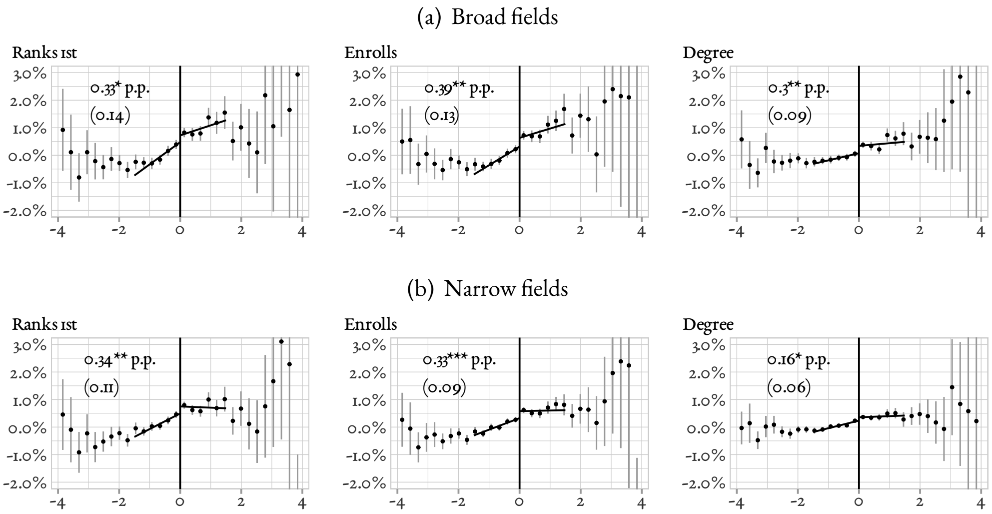
Notes: The table shows coviariate balance tests for a number of parent characteristics that are defined before treatment. The left column reports coefficients from regressions where being above the cutoff is regressed on each covariate separately, while the right column reports results from joint estimation. The regressions are run on the same sample and with the same controls as the main analysis, except that age and gender are included as covariates instead. The three final variables are not included in the joint estimation because they are only available for a limited subset of the full population.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

4 Main results

We start by studying the main aggregate results graphically. Figure 4 plots the three outcome variables: (1) if the child ranks field j first in an application to university, (2) if they enroll in j, and (3) if they earn a degree from j, for broad and narrow fields respectively. The observations are grouped in equally sized bins and plotted as functions of the running variable, demeaned for each cutoff. The sample includes all parent applicants with children born before the end of 1998. Inside each plot, the reduced form regression coefficients from Table 4 are reported. These are estimated using triangular kernels and include 13 separate linear polynomials of the running variable on each side of the threshold; one for each admission group.

Figure 4. Regression discontinuity plots



Notes: The plots show the share of children following their parents above and below the cutoff. Linear polynomials fitted with a triangular kernel on observations within one standard deviation from the cutoff are included. Applicants with a score exactly at the cutoff but where a tie-breaking mechanism has ensured they are not admitted have been included in the bin below the cutoff. Inside the plot, coefficients from Table 4 are reported. These are fitted using separate linear polynomials for each admission group.

Table 4 also includes estimates from IV specifications. Parental enrollment increases the likelihood that a child will earn a degree in the same field by approximately 53% or 2.1 percentage points. We find the largest effects when scaling with degree completion instead of enrollment. When a parent earns a degree in a certain broad field, the likelihood that their child does the same increases with 85% or 3.4 percentage points. For narrow fields, the corresponding numbers are almost identical, at 54% (0.9p.p.) and 87% (1.4p.p.) respectively. The relative effects for enrollment are somewhat smaller at 34% (2.7p.p.) and 55% (4.3p.p.) for broad fields, as well as 50% (1.9p.p.) and 81% (3.0p.p.) for narrow fields. Finally, the relative estimates for ranking the field first are even smaller, at 22% (2.3p.p.) and 36% (3.7p.p.) for broad fields and 34% (2.0p.p.) and 55% (3.2p.p.) for narrow fields. While these aggregate effect are large, they are substantially smaller than many of the correlations displayed in Figure 1.

Table 4. Inheritance of fields of study

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	0.33*	0.39**	0.30**	0.34**	0.33***	0.16*
	(0.14)	(0.13)	(0.09)	(0.11)	(0.09)	(0.06)
Parent enrolls in j	2.27*	2.68**	2.08**	1.96**	1.85***	0.89**
	(0.98)	(0.86)	(0.64)	(0.60)	(0.49)	(0.34)
Parent receives degree in j	3.68*	4.34**	3.36**	3.18***	3.00***	1.44**
	(1.59)	(1.39)	(1.04)	(0.97)	(0.80)	(0.56)
Observations	840 926	840 926	840 926	858 503	858 503	858 503
Control group mean	10.35%	7.85%	3.97%	5.8%	3.7%	1.65%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald (enrolls)	1709	1709	1709	2301	2301	2301
1st stage Wald (degree)	744	744	744	1046	1046	1046

Notes: Each row reports coefficients from different models. Coefficients and standard errors are reported in percentage points. All regressions use triangular kernel weights, and include linear polynomials of the running variables above and below the cutoff to each admission group, as well as fixed-effects for cutoff, next-best field, priority rank, age, and gender. Standard errors are two-way clustered at the cutoff and family level.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

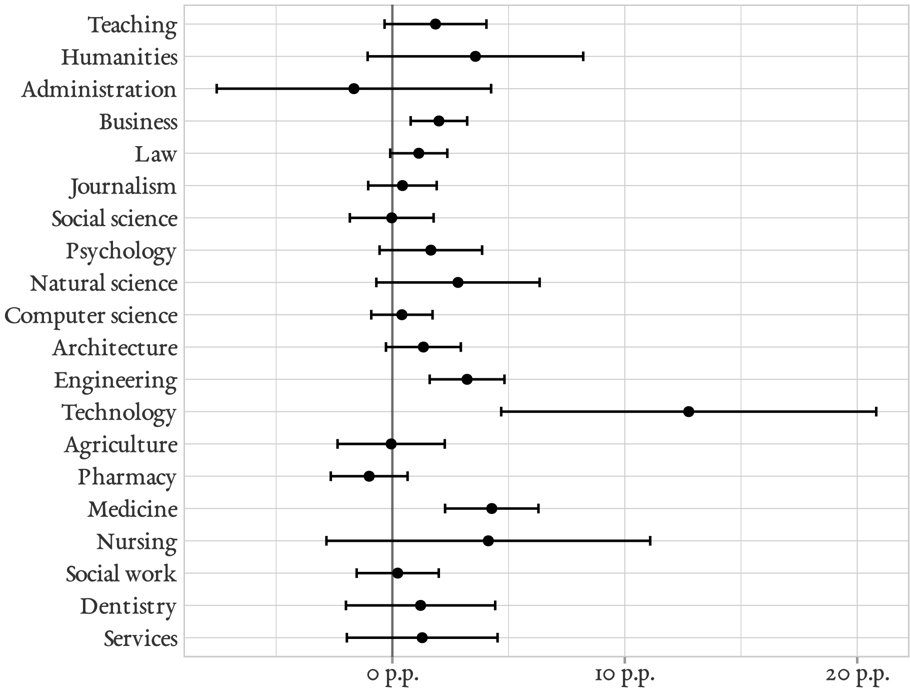
The aggregate effects are weighted averages of heterogeneous treatment effects many different fields of study. Figure 5 displays a coefficient plot of the field-level IV estimates of parental enrollment on child degree completion. The fields are sorted by their corresponding narrow field, to ensure similar fields are grouped together. Appendix Figure C.2 plots the same analysis but for narrow fields.

Several interesting patterns can be seen in this graph. First, there is large variation in the likelihood that children follow their parents. The four significant estimates are technology, medicine, engineering, and business, with point estimates ranked in that order. Teaching and law have p-values below 0.1 and point estimates above 1, while the effects for nursing, humanities, and natural science are all estimated above 2 but with large confidence intervals. Administration and pharmacy are estimated at -1 or lower, but also these effects are insignificant. When compared to the control group means displayed in Table B.2, we see that most of these relative effects are substantially smaller than the correlations reported in Figure 1. The main exception is technology, where the relative effect is almost exactly the same as the correlation.

Appendix Section C presents the same analysis but with applications collapsed by narrow fields. Figure C.2 also shows business and medicine at the top, but the effect for technology (codes 55H to 55K) is harder to discern. The reason is likely that in this specification, most of these applicants end up at margins between different technology subfields, reducing the impact of treatment. In general, the MSc. level fields (mapped to the broad field Engineering) produce larger estimates. Moreover, this categorization results in several significant and negative point estimates: for pre-school teaching and after-school care, as well as for Theology. At -6.94 percentage points, the negative effect for Theology is substantial.

Parental education could also impact the likelihood a child studies closely related fields. Figure 6 reports RDD estimates in a matrix. Here, separate regressions are run for each child field, allowing parents to influence the likelihood the child graduates from any field. The colors are capped at effect sizes

Figure 5. Inheritance of fields of study (broad fields)



Notes: The figure reports coefficients of parental enrollment on child degree completion using the same specification as in Table 4 but with a separate coefficient estimated for each preferred field j. The exact coefficients are reported in Table B.2. Corresponding results for narrow fields are reported in Appendix Figure C.2 and Table C.2.

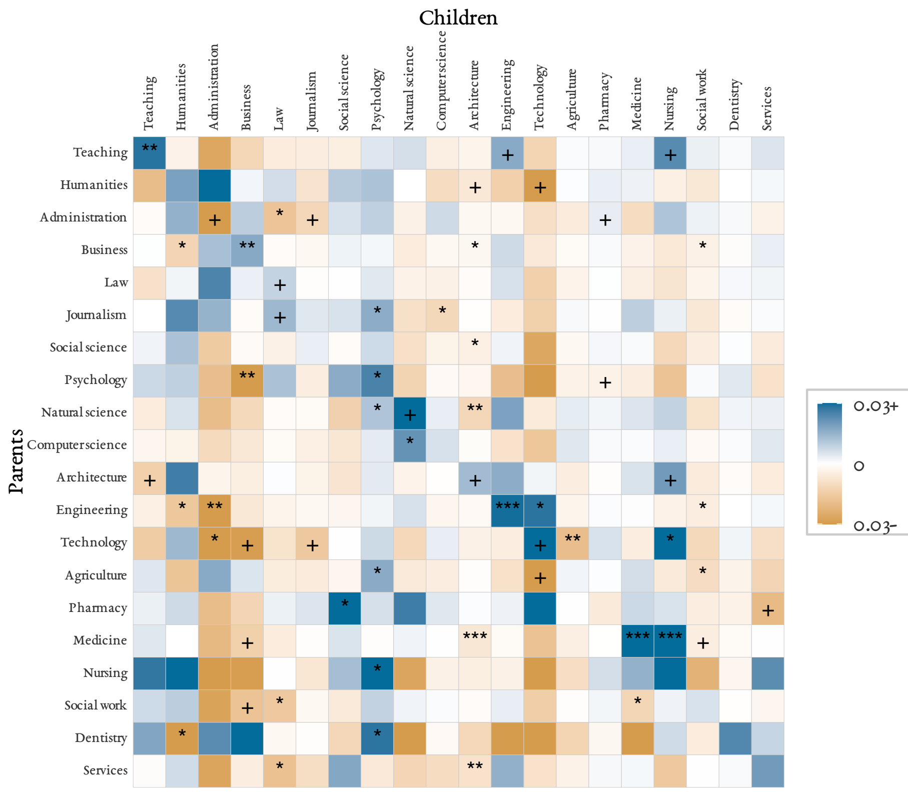
between -0.03 and 0.03 percentage points, since outliers (usually field combinations with almost no observations) would otherwise cause most cells to be indistinguishable from zero. While noisy, the figure still presents some interesting off-diagonal results. We see that parents enrolling in engineering make children more likely to graduate from technology, but not vice versa. In fact, the children of a parent who enrolls in technology are less likely to earn a degree in engineering. The same is true for medicine and nursing. Where medicine makes children more likely to graduate with a nursing degree, but not the opposite. The reason we see these effects is likely because the admission criteria are much stricter for medicine and engineering, prohibiting some children who would like to follow their parents from doing so. The barriers for children who want to follow their parents to technology and nursing are much lower.

Before further scrutinizing these results to identify the mechanisms that drive children to follow their parents in Section 6, the next section evalutes the robustness of RDD analysis and the main results.

5 Robustness

Regression discontinuity designs put strong requirements on the data. The main identifying assumption stipulates that the control function needs to be continuous at the cutoff. In other words, should it not be for admission, nothing would differ between applicants just above and below the cutoff. Since

Figure 6. Cross-field inheritance matrix



Notes: The matrix reports regression coefficients on how quasi-random admission of parents to different fields (y-axis) affects the likelihood of children earning degrees in all different fields. Estimation is done using the same setup as in Table 4. Significance levels are not corrected for multiple comparisons. $+\ p \leq 0.1$, $*\ p \leq 0.05$, $**\ p \leq 0.01$, $***\ p \leq 0.001$.

the exact level of the cutoff changes each year, applicants cannot know with certainty whether they will be admitted before applying, meaning that there is no way to precisely manipulate admission status. By construction, such a system ensures a continuous control function. To confirm that no other, deterministic, allocation has been used, and to verify the validity of the identification strategy, this section includes a number of robustness checks. The section also presents alternative specifications showing that the results are not sensitive to the exact choice of bandwidth or estimation strategy.

We saw in Table 3 that parental admission is not significantly related to characteristics measured before treatment assignment. An additional way to check that parents at the margin are not somehow able to select into the field they prefer is through the placebo analysis presented in Table 5. The estimation uses the same setup as the main analysis, but I instead look at the effect of child admission on parental educational outcomes. Since parents are educated before their children, we should not see any effects. But if the identifying assumptions fail, and applicants are somehow able to manipulate their admission status, the intergenerational field of study correlation should carry over to these estimates and produce significant effects. Thankfully, Table 5 reports no significant results, for none of the three outcomes, across the two field categorizations. This indicates that the RDD estimates do not erroneously capture spurious selection into fields within families.

Table 5. Placebo (parent outcomes regressed on child admission)

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Child above cutoff	−0.02	−0.09	−0.03	−0.15	−0.23	−0.14
	(0.17)	(0.18)	(0.18)	(0.15)	(0.16)	(0.15)
Child enrolls	−0.07	−0.39	−0.15	−0.55	−0.88	−0.52
	(0.72)	(0.77)	(0.76)	(0.57)	(0.59)	(0.57)
Child receives degree	−0.18	−1.10	−0.41	−1.81	−2.92	−1.71
	(2.01)	(2.16)	(2.14)	(1.88)	(1.95)	(1.88)
Observations	550 210	550 210	550 210	542 833	542 833	542 833
Control group mean	8.56%	9.43%	8.79%	6.93%	6.81%	6.07%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald (enrolls)	3242	3242	3242	4379	4379	4379
1st stage Wald (degree)	493	493	493	525	525	525

Notes: The table shows results from a placebo estimation where the admission status of the child is used to study the choices of the parent. Since the parent's application happened long before the child's, we expect to see no pattern. Appendix €

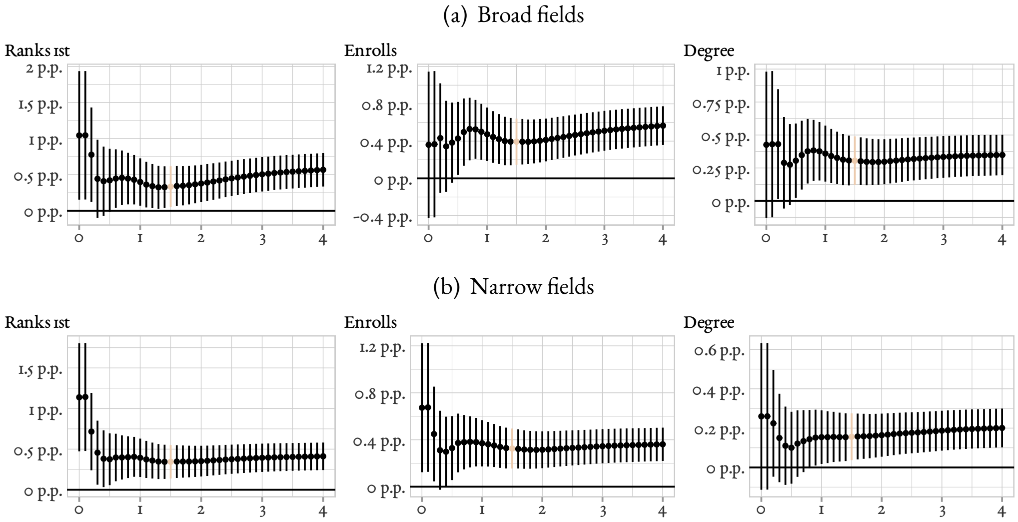
† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Figure 7 shows reduced form results for various bandwidth choices. In choosing the bandwidth, we face the classic bias-variance trade-off, where a larger bandwidth means more statistical power at the cost of potentially increasing bias. In normal RDD analysis, optimal bandwidth procedures yield balanced bandwidth choices. But since I am pooling a large set of quasi-experiments, such calculations couldn't possibly be optimal for all cutoffs. Instead, I use a bandwidth of 1.5 standard deviations for all analyzes (marked in a lighter color in the plot). The choice of a bandwidth of 1.5 yields one of the smaller reduced form effects across all outcomes, making it a conservative choice. Moreover, the figure shows little variation in the size of the effects as the bandwidth changes, except for very small bandwidths.

Applicants select into fields, but also admission groups and programs within fields. This is why I include cutoff fixed effects in all specifications. Since an applicant has one score per admission group it should be sufficient to include linear polynomials for each such group. However, this means that I am not actually estimating distinct RDD models for each quasi-experiment. To do so, the polynomial should be estimated at the cutoff level as well. Table 6 presents results from such an exercise, where a linear polynomial is fitted above and below each of the approximately 24 000 cutoffs. While this exercise is very taxing on statistical power, the estimates barely change.

Additional robustness and validity checks are performed in the appendix. Figure A.1 shows that the effect disappears when the admission cutoff is moved away from zero. Table A.1 adds quadratic polynomials for each admission group with little impact on results. Table A.2 shows that the results stay approximately the same (albeit become more noisy) when applicants exactly at the cutoff are removed. The results in Table A.3 are based on a sample where only those fields that were ranked first by the parent are included, to overcome potential problems with incentive compatibility. These results are again very similar to the main findings, but less precise. Last, Table A.4 shows correlations between the applicant being above the cutoff and the timing of important post-treatment events. Any significant differences

Figure 7. Reduced form results by bandwidth size



Notes: Each plot shows the main reduced form effect of a parent being above the cutoff on their child's application, enrollment, and degree completion. The leftmost bar in each plot has a bandwidth of zero and only includes applicants exactly at the cutoff where different tie-breaking mechanisms were used to allocate students. I use a bandwidth of 1.5 standard deviations for all analyzes, marked in a lighter color in the plot.

Table 6. Separate slopes for each cutoff

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	0.38^{*}	0.45^{**}	0.31^{**}	0.35^{**}	0.29^{**}	$0.12^{\dagger}$
	(0.17)	(0.15)	(0.11)	(0.12)	(0.10)	(0.07)
Parent enrolls in j	2.71^{*}	3.20^{**}	2.25^{**}	2.06^{**}	1.70^{**}	$0.73^{\dagger}$
	(1.21)	(1.06)	(0.79)	(0.72)	(0.60)	(0.42)
Parent receives degree in j	4.34^{*}	5.13^{**}	3.60^{**}	3.36^{**}	2.77^{**}	$1.18^{\dagger}$
	(1.94)	(1.70)	(1.26)	(1.18)	(0.98)	(0.68)
Observations	840 926	840 926	840 926	858 503	858 503	858 503
Control group mean	10.35%	7.85%	3.97%	5.8%	3.7%	1.65%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald (enrolls)	1207	1207	1207	1719	1719	1719
1st stage Wald (degree)	527	527	527	767	767	767

Notes: The table shows the same results as in Table 4 but with distinct linear polynomials of the running variable above and below each cutoff.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

that in turn influence child education could indicate violations of the exclusion restriction. Only the age at first enrollment is affected by admission, indicating that this worry is likely unfounded.

6 Exploring mechanisms

There exists a strong and robust causal relationship between the field of study choices of parents and their children. But why and how are fields of study inherited? In this section, I try to answer this question by studying subsets and correlations across different parts of the data.

Swedes often have their first child after graduating from university. In the studied sample, parents are on average 21 years old when they apply to university for the first time and 51 when their children apply. This empirical fact makes it likely that the identified causal effects work through either the occupational choices or the field-specific knowledge of parents. There are few other pathways through which the treatment effect could persist for so long. For certain fields, a university degree is the only way to earn an occupational license[19], and some other occupations (like engineers and therapists) are clearly linked to university degree programs. Most occupations are not protected, however, and employees can have a number of different degrees, or sometimes no degree at all.

We begin this section by studying the role of field-specific knowledge and labor market experience. Human capital transmission is one of the primary mechanisms thought to drive occupational inheritance. Table 7 presents RDD estimates of parental field enrollment on childrens' standardized elementary school subject grades. Interestingly enough, the results do not show any strong subject-specific transmission. The fields at the top, all in the humanities or social sciences do not improve child skills in the two rightmost columns more. Nor can the corresponding thing be said for the natural science-related middle fields and grades in math and science/technology. Instead, it seems certain fields give children higher grades across all subjects. Parents enrolling in journalism, natural science, and technology have children with higher grades in all subject areas. Also teaching, humanities, social science, and architecture produce significant improvements in several subjects. Medicine and business—two of the most inherited fields—report small and insignificant point estimates. Measured like this, field specific knowledge transmission does not seem to be a first-order effect, neither in explaining field inheritance heterogeneity, nor as a general driver of child subject-level GPA. The fields where parental enrollment improves child GPA seem rather to be theoretical fields where parents perhaps maintain a closer connection to academia also long after graduation.

To get a sense of the relative importance of the academic and labor market experience of the parent in driving field inheritance, we now turn to Tables 8 and 9. These tables report results where the aggregate estimates have been interacted with either the average standardized GPA among enrollees in the field (a measure of academic quality/popularity) or the individual-level predicted earnings percentile of the parent. Here, the estimation is helped by the increased number of fields used in the narrow field classification, enabling more variation in the interaction terms and resulting in smaller standard errors.

By looking at the average GPA among those enrolled in j at the same time as the applicant, we can evaluate how the likelihood that children follow their parents correlates with the popularity of the field. Table 8 reports results from this exercise. We see that most interaction coefficients are positive, but mainly the narrow field classification yields significant resutls. Average GPA is measured in standard deviations. For the included fields, it ranges from -0.1 to 2.1 for broad fields and -0.3 to 2.1. Moving from the

19. For at least a part of the studied period, the following fields allowed graduates to pursue occupational licenses: medicine, nursing, law, architecture, teaching, dentistry, psychology, and pharmacy.

Table 7. Parental field enrollment and child subject-level GPA

Field	Math		Science/technology		Social science		Languages	
Teaching	0.16^{*}	(0.07)	0.10	(0.06)	0.14^{*}	(0.06)	0.08	(0.06)
Humanities	0.15	(0.15)	$0.24^{\dagger}$	(0.15)	0.37^{*}	(0.14)	0.30^{*}	(0.13)
Administration	0.00	(0.12)	0.02	(0.11)	0.08	(0.10)	0.06	(0.10)
Business	0.01	(0.04)	−0.01	(0.04)	0.02	(0.04)	−0.01	(0.04)
Law	−0.09	(0.07)	−0.01	(0.07)	0.06	(0.07)	0.00	(0.06)
Journalism	0.20^{**}	(0.07)	$0.12^{\dagger}$	(0.07)	0.21^{**}	(0.07)	0.18^{**}	(0.06)
Social science	0.11	(0.09)	0.22^{**}	(0.09)	0.19^{*}	(0.08)	$0.13^{\dagger}$	(0.08)
Psychology	−0.10	(0.11)	0.03	(0.09)	0.00	(0.09)	−0.01	(0.09)
Natural science	0.28^{*}	(0.12)	0.30^{*}	(0.12)	0.34^{**}	(0.11)	0.37^{***}	(0.10)
Computer science	0.03	(0.07)	0.01	(0.07)	0.04	(0.06)	$0.11^{\dagger}$	(0.06)
Architecture	$0.14^{\dagger}$	(0.07)	0.10	(0.07)	0.19^{**}	(0.07)	0.15^{*}	(0.07)
Engineering	0.12^{*}	(0.06)	$0.09^{\dagger}$	(0.05)	0.08	(0.05)	0.01	(0.05)
Technology	0.42^{*}	(0.20)	0.42^{*}	(0.18)	$0.30^{\dagger}$	(0.18)	0.29	(0.18)
Agriculture	−0.05	(0.09)	−0.12	(0.09)	−0.08	(0.08)	−0.11	(0.08)
Pharmacy	0.18	(0.18)	−0.02	(0.16)	0.02	(0.16)	0.03	(0.16)
Medicine	−0.07	(0.07)	−0.06	(0.06)	−0.09	(0.05)	−0.03	(0.05)
Nursing	0.27	(0.19)	0.00	(0.16)	0.24	(0.16)	$0.29^{\dagger}$	(0.15)
Social work	−0.12	(0.08)	-0.16^{*}	(0.07)	−0.03	(0.07)	0.00	(0.07)
Dentistry	-0.48^{*}	(0.23)	−0.30	(0.19)	−0.22	(0.19)	-0.37^{*}	(0.18)
Services	0.11	(0.15)	0.06	(0.12)	0.15	(0.12)	0.26^{*}	(0.12)
Aggregate	0.07	(0.04)	0.05	(0.04)	0.08^{*}	(0.04)	$0.06^{\dagger}$	(0.04)

Notes: Each column represents a regression of standardized 9th grade subject GPA on parental field enrollment. All grades are standardized by cohort and subject, and categorized into the four categories above. When children have multiple grades in the same are, an average is calculated. The estimation strategy follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

bottom to the top, the average of the six interaction estimates is an effect of 4.0 percentage points. While several baseline coefficients are negative or close to zero, it is very rare that a field has an average GPA at or below zero, meaning that the total effect is almost always positive according to this categorization.

Results are somewhat different if we instead stratify the analysis by predicted earnings. Table 9 reports an attempt to correlate the parents' labor market experience with field inheritance. It uses a measure of predicted earnings percentile 10-14 years after application instead of actual earnings to avoid selection bias from controlling for post treatment outcomes. The estimation includes an interaction of parental enrollment with the predicted earnings of graduating from j. In the sample, the predicted earnings percentile ranges from 0.25 to 1 for broad fields and 0.23 to 1 for narrow fields. Across the six interaction coefficient, the average difference in child following going from the lowest to the highest predicted earnings percentile in the sample is 5.1 percentage points. For parents with predicted earnings at the lower end of the distribution, most of which are applying to programs in teaching or humanities, the inheritance effect is negative. As an example, parents need to be predicted to earn above the 56th percentile for their children to be more likely to earn a degree in the narrow field they enroll in. The 56th percentile is exactly the first quartile of the distribution of predicted earnings in the sample. In other words, when treatment heterogeneity is analyzed across parent predicted earnings, approximately the quartile predicted to earn the least from enrolling in their preferred field cause their children to be less likely to study said field.

Table 8. Field inheritance by average GPA among enrolled

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	−0.05	1.55	2.20†	−0.47	0.06	−0.52
	(1.69)	(1.51)	(1.21)	(1.01)	(0.83)	(0.58)
× Avg. GPA among enrolled	2.72*	1.32	−0.15	2.75***	2.02***	1.59***
	(1.13)	(1.00)	(0.82)	(0.73)	(0.59)	(0.42)
Observations	840 926	840 926	840 926	858 503	858 503	858 503
Control group mean	10.35%	7.85%	3.97%	5.8%	3.7%	1.65%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald	1027	1027	1027	1432	1432	1432

Notes: Parental enrollment is interacted with the average standardized high school GPA among all students who enroll in j during the application semester. Since average GPA is calculated at the field-semester level, the cutoff fixed effects control for baseline GPA. The estimation follows the same approach as Table 4

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Table 9. Field inheritance by parent predicted earnings percentile

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	−3.02	−1.72	−0.67	−5.06**	−3.94**	−3.23***
	(2.99)	(2.70)	(2.04)	(1.69)	(1.40)	(0.94)
× Predicted earnings (10-14 years, pt.)	6.63†	5.89†	4.04†	9.70***	8.10***	5.75***
	(3.47)	(3.17)	(2.36)	(2.01)	(1.69)	(1.12)
Predicted earnings (10-14 years, pt.)	18.87***	17.03***	7.58***	12.71***	10.73***	3.11**
	(2.65)	(2.39)	(1.68)	(1.94)	(1.59)	(1.06)
Observations	770 533	770 533	770 533	789 177	789 177	789 177
Control group mean	10.3%	7.86%	3.91%	5.77%	3.72%	1.61%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald	539	539	539	760	760	760

Notes: Parental enrollment is here interacted with the predicted earnings percentile of the parent. The predicted earnings percentile is calculated from a logit regression of the full population birth cohort percentile of average yearly non-missing earnings between 10 and 14 years after application on pre-treatment characteristics (gender, high school GPA, immigrant status, parental earnings) as well as age at application, application year, and field fixed-effects. Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

While we observe few cases of negative effects when organizing fields by their academic popularity in Table 8, using parent predicted earnings shows that a considerable share of children become less likely to study the field their parent enrolls in, when the labor market prospects are bad enough. If the only reason children follow their parents was because of comparative advantage we should not see such negative influence.[20] That we do points at the existence of additional mechanisms.

It is not strange that weak labor market prospects make children less likely to follow their parents, considering that field transmission should work through occupations. To better understand the labor market consequences for the child, Table 10 reports how child earnings are affected. The estimates in the second column indicate large differences in earnings between field graduates that have parents with similar degrees and those who don't. For services, the difference is almost 10 percentiles, while for nursing, the change is -2.3 percentiles. Many RDD estimates are even larger, but few are significant. In fact, at conventional levels, there are no significant differences in causal returns for any field. Also the base field returns (column 3) vary considerably, from an increase of 29.8 percentiles for dentists without dentist parents, to a decrease of 13.4 percentiles for natural scientists with parents with other degrees.[21] Looking at the aggregate estimates at the bottom, having a parent with a degree in the same field seems not to matter much for earnings.

Also the correlational estimates in the first two columns of Table 10 provide interesting insights. The first column reports the correlation between parental field degrees and child graduation rates, among those children who are enrolled in the field. The only negative effect is for social science, perhaps because enrolled children of parents with social science degrees are more likely to get a degree in a related field that pays better, like business or law. For teaching, humanities, architecture, agriculture, and services, graduation rates increase with over 10 percentage points. Interestingly, for services and agriculture, access to a parent with a degree is associated with the largest increase in earnings, with 9.8 and 6.4 percentiles respectively. But for teaching, humanities and architceture, the association with earnings is weak or even negative. A possible explanation is that the perceived benefits of following a parent vary across fields. For some fields, like services, agriculture, or dentistry, the child can inherit capital (customers, a farm, a clinic) that improves their earnings. While for other fields, like teaching and humanities, children perhaps follow because they learn to appreciate the knowledge the field brings. A reason why these pecuniary gains do not show up in the fourth column of the table, where the causal returns to enrolling in a field are studied, could be because those who end up below the cutoff to a field from which they could gain from their parent's degree, end up finding other ways to monetize this capital.

So far, the results have not really indicated that fields of study are inherited because children gain a comparative advantage. In terms of knowledge transmission, there are no clear patterns of subject specific transmission in Table 7. And while children are more inclined to follow parents who studied fields that are popular or are expected to yield high earnings, the effects for lower valued fields can be strongly negative. Moreoever, childrens' causal returns to their field of study does not seem to differ by whether or not they have a parent with a degree—at least not at the aggregate level. An alternative hypothesis is that the parent simply acts as a role model, marketing their own study choice and making the consequences of their chosen path more salient. It seems likely that parents with worse labor market experience would not want their children to follow, perhaps explaining the negative estimates in Table 9.

20. As long as enrollment in a field makes the parent more likely to work in a related occupation, which is what the fourth plot in Figure 2 indicated.

21. It is important to note that these are estimates of relative returns, and the control conditions likely vary considerably. The coefficients are LATE measures of earnings differences between those who enrolled in the field and those who enrolled in their next-best alternative. That returns to e.g. business are so small, could be because business applicants all put other high-earning fields as their fallback.

Table 10. Earnings and parental field degrees

Field	Correlations: Parent with degree in field				Returns (earnings pt) to enrolling in field			
	Graduation rate among enrollees		Earnings (pt) among graduates		RDD sample of field applicants (bw = 1.5) Enrolled		× parent degree	
Teaching	11.11^{***}	(0.34)	$0.37^{\dagger}$	(0.20)	−4.30	(3.37)	−0.39	(3.23)
Humanities	11.00^{***}	(0.29)	-1.18^{***}	(0.26)	1.64	(4.28)	8.09	(9.11)
Administration	1.61^{***}	(0.36)	-1.18^{***}	(0.27)	$9.49^{\dagger}$	(5.29)	3.47	(16.93)
Business	3.81^{***}	(0.62)	1.36^{**}	(0.42)	0.87	(1.70)	2.90	(2.79)
Law	3.53^{***}	(0.92)	3.02^{***}	(0.59)	4.09^{*}	(1.92)	$6.44^{\dagger}$	(3.52)
Journalism	3.14	(3.22)	−2.21	(2.00)	10.52^{***}	(2.46)	−16.59	(11.56)
Social science	-1.51^{*}	(0.75)	1.39^{***}	(0.35)	3.45	(2.63)	−1.54	(3.27)
Psychology	6.34^{***}	(1.73)	$-1.84^{\dagger}$	(1.00)	4.61	(4.02)	17.11	(12.59)
Natural science	6.45^{***}	(0.39)	-1.03^{**}	(0.39)	-13.40^{*}	(5.64)	4.38	(7.98)
Computer science	1.54	(2.02)	0.85	(1.44)	2.05	(2.32)	$15.92^{\dagger}$	(9.18)
Architecture	15.26^{***}	(2.37)	1.68	(1.65)	−4.10	(2.63)	5.97	(10.57)
Engineering	5.85^{***}	(0.41)	1.18^{***}	(0.27)	$3.32^{\dagger}$	(1.71)	−0.30	(2.46)
Technology	6.43^{***}	(0.45)	$0.39^{\dagger}$	(0.23)	9.99	(6.16)	−3.78	(7.58)
Agriculture	10.83^{***}	(1.22)	6.42^{***}	(0.80)	4.63	(3.67)	5.84	(7.33)
Pharmacy	7.07^{***}	(2.01)	−0.75	(1.49)	9.37	(6.29)	8.20	(13.02)
Medicine	2.19^{***}	(0.48)	2.53^{***}	(0.44)	16.85^{***}	(1.94)	4.37	(3.25)
Nursing	7.75^{***}	(0.37)	-2.27^{***}	(0.21)	-9.19^{**}	(3.05)	$6.34^{\dagger}$	(3.26)
Social work	2.46^{**}	(0.95)	1.76^{***}	(0.47)	1.10	(2.42)	−1.20	(4.11)
Dentistry	8.45^{***}	(1.64)	5.19^{***}	(1.25)	29.80^{*}	(14.52)	−20.29	(20.92)
Services	15.99^{***}	(1.08)	9.83^{***}	(0.38)	−4.25	(4.62)	12.42	(13.03)
Aggregate	7.03^{***}	(0.12)	0.37^{***}	(0.08)	3.44^{**}	(1.30)	0.12	(0.94)

Notes: The leftmost two columns represent correlations of field degree completion and earnings (at age 30-34), each regressed on if parent have a degree in the field, and based on samples of enrollees and graduates respectively. The two rightmost columns are results from RDD specifications where earnings percentile is regressed on enrollment (column 3) and interacted with parental degree completion (column 4). Earnings (pt) is the birth cohort earnings percentile calculated on average non-missing earnings 10–14 years after application (or at age 30-34). The RDD uses the same setup as Table 4, but is run on a slightly older sample for which earnings data is available. The results in the two leftmost columns are based on regressions with field fixed effects. As an example, the last line shows that enrollees are 7.0p.p. more likely to graduate if they have a parent with a degree in the same field, and have 0.4 percentiles higher earnings. Enrolling in a preferred field increases earnings by 3.4 percentiles (compared to those below the cutoff) among applicants who do not have a parent with a degree in the field and by 3.56 percentiles otherwise—a statistically insignificant difference.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

To further analyze the importance of the labor market experience, we now turn to a type of mediation analysis. Since it conditions on a post-treatment outcome, it should not be interpreted as causal. Table 11 reports coefficients for parental enrollment interacted with if the parent works in a job that is typical for their field of study during the age 40-49.[22]

Table 11. Parent occupation

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	0.61	1.39	1.21	1.61†	1.52*	0.45
	(1.40)	(1.24)	(0.91)	(0.86)	(0.70)	(0.48)
× Parent has job common for j	1.67*	2.40**	1.72**	0.51	0.97*	0.88**
	(0.81)	(0.74)	(0.52)	(0.55)	(0.44)	(0.29)
Parent has job common for j	2.16***	0.69	0.13	1.60***	0.63**	0.19
	(0.54)	(0.48)	(0.35)	(0.29)	(0.24)	(0.16)
Observations	650 733	650 733	650 733	666 688	666 688	666 688
Control group mean	11.67%	8.9%	4.42%	6.54%	4.22%	1.82%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald	3538	3538	3538	3631	3631	3631

Notes: Parental enrollment is interacted with if the parent works in a job that is common for the field during age 40–49. Common jobs are jobs that are representative for the specific field degree. They are marked in gray in Appendix Figure E.1 and described in Table E.2. Since parent occupation is defined after treatment, these results should not be interpreted as causal. Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

The results indicate that children of parents who work in a job that is representative for their degree, children are much more likely to follow. Of course, since parental occupation is defined after treatment, the interaction pick up various selection into occupations. Nonetheless, it seems much of the effect goes through those parents who (for whatever reason) end up working in a job that their education prepared them for.

These findings show how important field of study choices are to understand occupational inheritance. The results underscore the importance of the labor market experience of the parent, but show no clear patterns of children gaining or using their comparative advantage. Fields rarely transmit subject-specific skills to children, and children do not follow parents who are predicted to have relatively bad experiences. A result further strengthening the thesis of parents as role models is reported in Appendix Table B.3. When grouped by the age of the parent at child application, we see little impact of age except if the parent has reached the retirement age of 65. At this age, the effect drops substantially.

Research on educational role models often highlight the importance of self-identification (see e.g. Breda et al. (2021)). Table 12 divides the sample by parent-child gender composition and shows that, indeed, influence is stronger for same-gender pairs. The table reports the effect in levels, the raw intearction coefficients are available in Appendix Table B.4. Like in Dahl et al. (2021) and several correlational studies, fathers exert a stronger influence, especially on sons. However, in contrast to those papers also the choice of mothers matter, especially for their daughters.

22. A common job is defined as those occupations where either more than 3% of all degree-holders from a specific field work, or where more than 30% of workers have a degree from the field. These levels are calibrated to minimize the number of wrongly classified occupations. For medicine, for example, I include medical doctors (221) and health care managers (151). For a Figure of the distribution of occupations per field see Appendix Figure E.1, for a full codebook see Table E.2.

Looking at field-level heterogeneity, the results are more complex, but standard errors are large. For many fields, children are more likely to follow the parent of their own gender. There are exceptions, however. Looking only at significant results, sons follow mothers more often than fathers to technology, engineering, and medicine. Daughters, on the other hand, follow fathers more often to technology. In contrast to Dahl et al. (2021), I do not find much evidence of parents exerting a stronger influence when their field choice is not conforming to gender stereotypes. In stereotypically male fields, like technology and engineering, sons do follow mothers more, but the differences are not statistically significant. For business and law, on the other hand, they follow mothers less. The pattern is less clear for daughters. When it comes to stereotypically female fields, like teaching, nursing, and social work few clear or significant patterns can be discerned.

Table 12. Field inheritance and gender composition

Field	Father - Son		Father - Daughter		Mother - Son		Mother - Daughter	
Teaching	0.43	(1.66)	1.93	(2.26)	1.06	(1.37)	3.78*	(1.86)
Humanities	10.83*	(4.99)	5.23	(5.87)	0.53	(3.69)	6.45	(4.16)
Administration	−1.04	(5.55)	2.02	(5.96)	−5.58	(4.77)	−5.29	(5.53)
Business	3.37***	(1.01)	0.72	(0.98)	2.51*	(0.98)	2.87*	(1.17)
Law	1.05	(0.99)	1.91†	(1.16)	−0.07	(0.84)	2.35*	(1.00)
Journalism	−1.47	(1.19)	3.80	(2.56)	0.39	(0.79)	0.09	(1.45)
Social science	−1.93	(1.89)	0.91	(2.28)	−0.39	(1.33)	1.48	(1.71)
Psychology	−1.56	(2.08)	−0.75	(2.35)	2.12	(1.61)	3.88†	(2.28)
Natural science	2.65	(2.73)	3.50	(2.39)	5.22	(4.05)	1.67	(3.66)
Computer science	2.11†	(1.16)	−0.50	(0.89)	−0.35	(1.39)	0.62	(1.25)
Architecture	0.00	(1.46)	3.72*	(1.88)	1.69	(1.19)	0.93	(1.14)
Engineering	4.06**	(1.26)	2.51*	(1.07)	5.73***	(1.49)	3.23*	(1.39)
Technology	16.08*	(6.39)	14.09**	(4.93)	18.39†	(9.85)	1.30	(8.76)
Agriculture	−0.03	(2.34)	−0.15	(2.65)	−0.71	(1.70)	0.72	(2.17)
Pharmacy	−5.05†	(2.84)	4.58	(4.37)	−1.18	(0.81)	−1.72	(1.48)
Medicine	2.52	(1.98)	5.63**	(1.91)	4.69**	(1.64)	5.57**	(2.02)
Nursing	7.30	(7.70)	18.12	(12.22)	1.05	(4.48)	0.22	(6.84)
Social work	−1.63	(1.46)	1.93	(2.61)	0.03	(0.92)	0.81	(1.63)
Dentistry	6.95*	(3.13)	6.94	(5.48)	−3.43	(2.11)	−5.98†	(3.41)
Services	8.18	(4.99)	−2.48	(4.46)	2.95	(2.91)	0.10	(2.19)
Aggregate	3.13***	(0.80)	1.58*	(0.78)	1.76*	(0.78)	2.72**	(0.87)

Notes: The table reports effects for child degree completion. It shows results from a regression where parent enrollment is interacted with field as well as parent and child gender. Otherwise, the estimation follows the same approach as Table 4. The effects reported are linear combinations of interaction and baseline coefficients with significance levels referring to hypothesis tests against a null of no combined effects. See Table B.4 for the raw interactions.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

A likely explanation for why the effect is often somewhat weaker for mothers, which is echoed in several of the cited studies, is that mothers less often pursue careers in occupations related to the field that they graduated from. A concern could be that the effect we identified for mothers, mainly works through assortative mating.[23] Indeed, Appendix Table B.5 reports that enrolling in a field makes mothers twice as likely as fathers to partner with someone with a degree from that field. While smaller than for fathers

23. Although this concern applies just as much to the previous papers as well

(although the difference is only significant for some specifications), Table 13 shows that also mothers who do not partner with someone with the same degree transmit their field to their children. Again, since partner choice happens after treatment, these differences should not be interpreted as causal, and could be spurious.

Table 13. Field inheritance and assortative mating

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	1.19	1.44	1.71**	1.76**	1.60**	0.79*
	(0.99)	(0.88)	(0.63)	(0.61)	(0.51)	(0.34)
× Parent female	−0.65	−0.34	−0.60	−1.00*	−0.73*	−0.44†
	(0.63)	(0.57)	(0.41)	(0.42)	(0.35)	(0.24)
× Other parent has degree in j	5.22*	7.54***	3.22*	2.03	3.53*	1.33
	(2.06)	(1.85)	(1.37)	(2.11)	(1.78)	(1.30)
× Parent female × other parent has degree in j	1.12	−0.46	1.08	−1.30	−1.81	1.15
	(2.79)	(2.52)	(1.86)	(2.96)	(2.45)	(1.76)
Parent female	1.47***	1.34***	0.78***	0.79***	0.59***	0.36***
	(0.30)	(0.26)	(0.18)	(0.19)	(0.15)	(0.10)
Other parent has degree in j	3.41*	0.27	0.01	4.40**	1.67	0.85
	(1.58)	(1.41)	(1.05)	(1.69)	(1.42)	(1.04)
Parent female × other parent has degree in j	1.25	2.24	0.49	3.05	2.97	0.06
	(2.02)	(1.82)	(1.35)	(2.29)	(1.90)	(1.36)
Observations	840 926	840 926	840 926	858 503	858 503	858 503
Control group mean	10.35%	7.85%	3.97%	5.8%	3.7%	1.65%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald	996	996	996	1360	1360	1360

Notes: This table reports on the effect of assortative mating. Enrollment is interacted with parent gender and if the child's other parent has a degree in the same field. Appendix Table B.5 shows that mothers are about twice as likely to partner with someone with a degree from the same field. Since partnership formation happens after treatment, these results should not be interpreted as causal. Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

One last table about the role of the family is presented in the Appendix. Table B.6 displays the aggregate field inheritance effect by the education level of the grandparents. The differences are not significant, but a weak monotonic relationship indicates that in families with a tradition of more education, children are more likely to follow their parents.

To summarize, inheritance effects are positive also for mothers, and strongest for same-gender parent-child pairs. This result gives additional support to the theory that parents function as role models.

The Appendix contains two sections of additional analysis. First, Appendix Section C includes the results for narrow fields that have not been reported in the main text.

Second, Appendix Section D presents an analysis of how institutions, rather than fields, are inherited. Instead of studying applicants on the margin between different fields, this section looks at those who are on the margin between institutions. Table D.1 reports coefficients that are similar in size to the broad field inheritance effects, and Table D.2 shows that much of this is in fact a result of location persistance, where children become more likely to graduate from any institution within the same commuting zone. To be precise, parental enrollment in a specific institution increases the likelihood that their child earns a degree there with 68% (2.1p.p). At the commuting zone (local labor market) level, the corresponding effect is 61% (2.9p.p.). These relative effects are really close to the main results.

Last, Table D.3 looks at the likelihood for a child to follow their parent to the same field-institution combination. Importantly, the second part of this table shows that also when holding the institution constant, children follow their parents to the same field. The relative effects are actually somewhat larger than the main results. When a parent enrolls in a specific field-institution combination, it increases the likelihood that the child earns a degree there with 128% (0.83p.p.). If one limits the analysis to only cases where the parent's institution is the same both above and below the cutoff, the effect is 84% (0.68p.p.). Because of the large reduction in sample size, this estimate is not significant, however.

7 Conclusion

Children are often 3–5 times more likely than average to graduate from a field that their parents have studied. This well-known pattern of intergenerational association has been shown in previous research to mainly apply to fathers and sons. In this paper, I exploited a quasi-experimental statistical design to investigate how much of this association can be attributed to causal mechanisms.

The field of study choice of a parent strongly impacts the educational trajectory of their children. I have shown that the likelihood that a child graduates from a field increases with 2.1 percentage points or 53% (0.9p.p. or 54% for narrow fields) if the parent enrolls in that field, when compared to parents who apply to the same field but then end up studying something else. The results are robust to alternative specifications and a large set of robustness and placecbo checks. They are also conservative, estimated under several assumptions that likely make them a lower bound of a more general field inheritance effect.

Dissecting these results into heterogeneous effects by field of study shows that few fields see negative parental influence, but some are inherited more often than others. Many of the most inherited fields are in STEM. Parental enrollment increases graduation from technology with 12.8 percentage points (140%) but only with 0.23 percentage points (13%) in social work, and -1.0p.p. (-209%) in pharmacy. Some of these causal effects are close to the correlations. For example, children are 143% as likely to hold a degree in technology if their parent has one. But other results are quite different. The likelihood to earn a degree in social work is 211% higher and in pharmacy the association is no less than 612%. Another interesting example is business, where preferences are so correlated across generations that even though it has one of the larger absolute causal estimates at 2.0 percentage points, the relative effect is only 50% — a fourth of the raw association of 186%.[24]

These variable patterns are the results of a complex set of differences in educational and occupational experiences across fields. It takes on average 28 years between the university application of a parent and their child. Most children are not old enough to directly experience their parents time at university. Instead, the inheritance effect works indirectly, through the knowledge the parent gains from their studies, and the occupational pathways that are opened. Studying the parent's experience, we saw that children are more likely to follow those parents who study popular fields (Table 8), and that it is especially those parents who are predicted to earn well who are followed (Table 9).

Are children who follow their parents better off? Table 10 presents an analysis of how earnings and returns vary with if the parent holds the same degree. The analysis shows that, for certain fields of study, children who have a parent with a degree in the same field as them have substantially larger earnings than those with parents without such a degree. These differences to not translate into higher causal returns,

24. See Table B.2 for a complete list of field level associations and causal effects.

however. Moreover, there are several fields with very weak earnings associations, but where children are still much more likely to earn a degree if their parents have one. In total, it does not seem like earnings improvements are an important driver as to why children follow their parents.

In the mechanisms section, we empirically investigated the drivers of intergenerational university field transmission. Research on occupational inheritance often claims children follow their parents because they have a comparative advantage, either because of human capital transfers or nepotism. While the data used in this study does not allow me to authoritatively refute those claims, it offers suggestions that other mechanisms might be just as important. Several patterns observed in this section instead speak to the importance of the parent as a role model, making their own study choice into a salient alternative for their child. First, I find no evidence for field-specific knowledge transfer in Table 7. It does not seem that parents who study natural science improve their children's grades in quantitative subjects more, and similarly for social science. Second, parents who enroll in fields which are expected to lead to weak labor market outcomes, are much less likely to be followed. For parents in the first quartile of predicted earnings, the effect in Table 9 is in fact negative. Third, children are more likely to follow same-gendered parents, but much less likely to follow retired parents (Appendix Table B.3).

Even in a relatively mobile country like Sweden an individual's choice of field, and, in turn, occupation, is strongly affected by the pathways chosen by their parents. For many fields, the causal findings of this paper go in the same direction as previous correlational estimates, albeit are somewhat weaker. For other fields, the causal effects are very different. Many external elements, like social norms and family traditions contribute to the spurious correlation between intergenerational education choices. This paper accounts for such factors and provides policy-relevant estimates of the direct intergenerational effect of parental education.

In this paper, I have identified an environmental factor influencing educational choices that can be controlled. These results are important to researchers studying intergenerational mobility and to policymakers who are interested in improving equality of opportunity. They are also relevant for parents who want to their children to succees and who will benefit from understanding their importance as role models. The paper underscores the value of parental role models. To increase mobility, children from families with little exposure to tertiary education need additional role models to help them understand what educational and occupational pathways are available to them.

References

Adermon, A., M. Lindahl, and M. Palme. 2021. "Dynastic Human Capital, Inequality, and Intergenerational Mobility." *American Economic Review* 111 (5): 1523–1548.

Aina, C., and C. Nicoletti. 2018. "The Intergenerational Transmission of Liberal Professions." *Labour Economics* 51:108–120.

Altmejd, A., A. Barrios-Fernandez, M. Drlje, et al. 2021. "O Brother, Where Start Thou? Sibling Spillovers on College and Major Choice in Four Countries." *Quarterly Journal of Economics* 136 (3): 1831–1886.

Andrade, S. B., and J.-P. Thomsen. 2017. "Micro-Educational Reproduction." *Social Forces* 96 (2): 717–750.

Balinski, M., and T. Sönmez. 1999. "A Tale of Two Mechanisms: Student Placement." *Journal of Economic Theory* 84 (1): 73–94.

Barrios-Fernández, A., C. Neilson, and S. Zimmerman. 2021. "Elite Universities and the Intergenerational Transmission of Human and Social Capital."

Becker, G. S. 1964. *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education.* NBER.

Bell, A., R. Chetty, X. Jaravel, N. Petkova, and J. Van Reenen. 2019. "Who Becomes an Inventor in America? The Importance of Exposure to Innovation." *The Quarterly Journal of Economics* 134 (2): 647–713.

Bennedsen, M., K. M. Nielsen, F. Perez-Gonzalez, and D. Wolfenzon. 2007. "Inside the Family Firm: The Role of Families in Succession Decisions and Performance." *The Quarterly Journal of Economics* 122 (2): 647–691.

Bergé, L. 2018. *Efficient Estimation of Maximum Likelihood Models with Multiple Fixed-Effects: The R Package FENmlm,* CREA Discussion Papers 13.

Björklund, A., and M. Jäntti. 2012. "How Important Is Family Background for Labor-Economic Outcomes?" *Labour Economics* 19 (4): 465–474.

Björklund, A., and K. G. Salvanes. 2011. "Chapter 3 - Education and Family Background: Mechanisms and Policies." In *Handbook of the Economics of Education,* edited by E. A. Hanushek, S. Machin, and L. Woessmann, 3:201–247. Elsevier.

Black, S. E., and P. J. Devereux. 2011. "Recent Developments in Intergenerational Mobility." In *Handbook of Labor Economics,* edited by D. Card and O. Ashenfelter, 4:1487–1541.

Blandhol, C., J. Bonney, M. Mogstad, and A. Torgovitsky. 2022. "When Is TSLS Actually LATE?"

Branigan, A. R., K. J. McCallum, and J. Freese. 2013. "Variation in the Heritability of Educational Attainment: An International Meta-Analysis." *Social Forces* 92 (1): 109–140.

Braun, S. T., and J. Stuhler. 2018. "The Transmission of Inequality Across Multiple Generations: Testing Recent Theories with Evidence from Germany." *The Economic Journal* 128 (609): 576–611.

Breda, T., J. Grenet, M. Monnet, and C. van Effenterre. 2021. *Do Female Role Models Reduce the Gender Gap in Science? Evidence from French High Schools,* PSE Working Papers halshs-01713068. HAL. Accessed February 23, 2023. https://ideas.repec.org//p/hal/psewpa/halshs-01713068.html.

Calonico, S., M. D. Cattaneo, and R. Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–2326.

Coleman, J. S., E. Q. Campbell, C. J. Hobson, et al. 1966. *Equality of Educational Opportunity.* U.S. Department of Health, Education, and Welfare, Office of Education.

Dahl, G., D.-O. Rooth, and A. Stenberg. 2021. *Intergenerational and Sibling Peer Effects in High School Majors.* Working Paper 27618. National Bureau of Economic Research.

Dal Bó, E., P. Dal Bó, and J. Snyder. 2009. "Political Dynasties." *The Review of Economic Studies* 76 (1): 115–142.

De la Croix, D., and M. Goñi. 2021. "Nepotism vs. Intergenerational Transmission of Human Capital in Academia (1088–1800)." *SSRN Electronic Journal.*

Dunn, T., and D. Holtz-Eakin. 2000. "Financial Capital, Human Capital, and the Transition to Self-Employment: Evidence from Intergenerational Links." *Journal of Labor Economics* 18 (2): 282–305.

Erikson, R., and J. H. Goldthorpe. 2002. "Intergenerational Inequality: A Sociological Perspective." *Journal of Economic Perspectives* 16 (3): 31–44.

Fort, M., A. Ichino, E. Rettore, and G. Zanella. 2022. *Multi-Cutoff Rd Designs with Observations Located at Each Cutoff: Problems and Solutions,* IZA DP 15051. Accessed November 24, 2022. https://www.ssrn.com/abstract=4114595.

Grönqvist, E., B. Öckert, and J. Vlachos. 2017. "The Intergenerational Transmission of Cognitive and Noncognitive Abilities." *Journal of Human Resources* 52 (4): 887–918.

Hällsten, M. 2010. "The Structure of Educational Decision Making and Consequences for Inequality: A Swedish Test Case." *American Journal of Sociology* 116 (3): 806–854.

Hällsten, M., and M. Thaning. 2018. "Multiple Dimensions of Social Background and Horizontal Educational Attainment in Sweden." *Research in Social Stratification and Mobility* 56:40–52.

Holmlund, H., M. Lindahl, and E. Plug. 2011. "The Causal Effect of Parents' Schooling on Children's Schooling: A Comparison of Estimation Methods." *Journal of Economic Literature* 49 (3): 615–651.

Hvide, H. K., and P. Oyer. 2018. *Dinner Table Human Capital and Entrepreneurship.* Working Paper, Working Paper Series 24198. National Bureau of Economic Research.

Jonsson, J. O., D. B. Grusky, M. Di Carlo, R. Pollak, and M. C. Brinton. 2009. "Microclass Mobility: Social Reproduction in Four Countries." *American Journal of Sociology* 114 (4): 977–1036.

Kirkebøen, L. J., E. Leuven, and M. Mogstad. 2016. "Field of Study, Earnings, and Self-Selection." *The Quarterly Journal of Economics* 131 (3): 1057–1111.

Kraaykamp, G., J. Tolsma, and M. H. J. Wolbers. 2013. "Educational Expansion and Field of Study: Trends in the Intergenerational Transmission of Educational Inequality in the Netherlands." *British Journal of Sociology of Education* 34 (5-6): 888–906.

Laband, D. N., and B. F. Lentz. 1983. "Occupational Inheritance in Agriculture." *American Journal of Agricultural Economics* 65 (2): 311–314.

———. 1992. "Self-Recruitment in the Legal Profession." *Journal of Labor Economics* 10 (2): 182–201.

Lee, D. S., and T. Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48 (2): 281–355.

Lee, J. J., R. Wedow, A. Okbay, et al. 2018. "Gene Discovery and Polygenic Prediction from a Genome-Wide Association Study of Educational Attainment in 1.1 Million Individuals." *Nature Genetics* 50, no. 8 (8): 1112–1121.

Lentz, B. F., and D. N. Laband. 1989. "Why So Many Children of Doctors Become Doctors: Nepotism vs. Human Capital Transfers." *The Journal of Human Resources* 24 (3): 396–413.

———. 1990. "Entrepreneurial Success and Occupational Inheritance among Proprietors." *The Canadian Journal of Economics / Revue canadienne d'Economique* 23 (3): 563–579.

Lindahl, M., M. Palme, S. S. Massih, and A. Sjögren. 2015. "Long-Term Intergenerational Persistence of Human Capital: An Empirical Analysis of Four Generations." *The Journal of Human Resources* 50 (1): 1–33.

Lundborg, P., A. Nilsson, and D.-O. Rooth. 2014. "Parental Education and Offspring Outcomes: Evidence from the Swedish Compulsory School Reform." *American Economic Journal: Applied Economics* 6 (1): 253–278.

Mocetti, S. 2016. "Dynasties in Professions and the Role of Rents and Regulation: Evidence from Italian Pharmacies." *Journal of Public Economics* 133:1–10.

Mocetti, S., G. Roma, and E. Rubolino. 2022. "Knocking on Parents' Doors Regulation and Intergenerational Mobility." *Journal of Human Resources* 57 (2): 525–554.

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

Pathak, P. A., and T. Sönmez. 2013. "School Admissions Reform in Chicago and England: Comparing Mechanisms by Their Vulnerability to Manipulation." *American Economic Review* 103 (1): 80–106.

Plomin, R. 2011. "Commentary: Why Are Children in the Same Family so Different? Non-shared Environment Three Decades Later." *International Journal of Epidemiology* 40 (3): 582–592.

Polderman, T. J. C., B. Benyamin, C. A. de Leeuw, et al. 2015. "Meta-Analysis of the Heritability of Human Traits Based on Fifty Years of Twin Studies." *Nature Genetics* 47, no. 7 (7): 702–709.

Van de Werfhorst, H. G., N. D. de Graaf, and G. Kraaykamp. 2001. "Intergenerational Resemblance in Field of Study in the Netherlands." *European Sociological Review* 17 (3): 275–293.

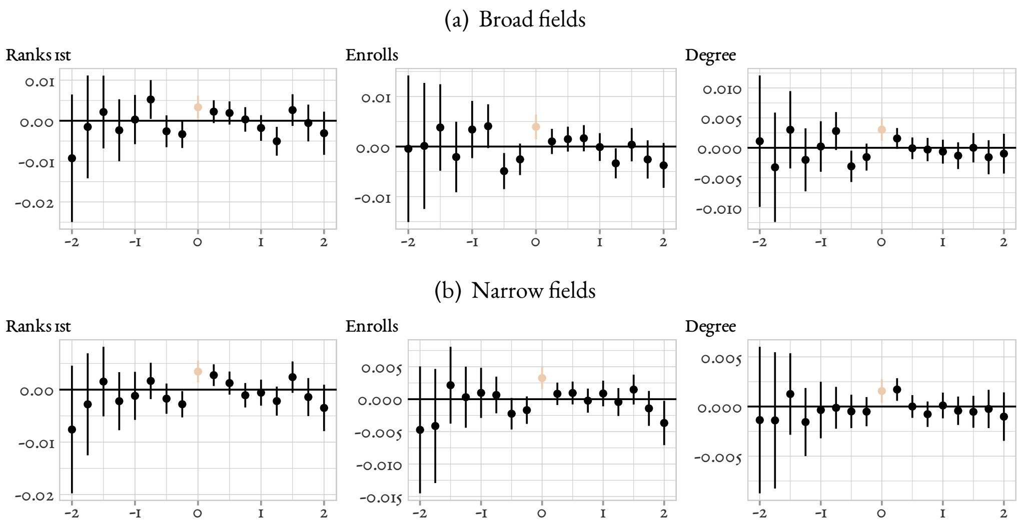
Weeden, K. A., and D. B. Grusky. 2005. "The Case for a New Class Map." *American Journal of Sociology* 111 (1): 141–212.

Appendix

A Additional robustness checks

This section includes additional robustness and validation exercises. We start with Figure A.1 where the main estimation has been conducted using various alternative cutoffs. We see that as soon as the cutoff is moved from its true position, the estimated results disappear. If for example the functional form of the running variable polynomial did not capture the effect of the score on the outcome, moving the cutoff would have had less effect on the estimated coefficients. These results further strengthen the credibility of the RDD analysis.

Figure A.1. Placebo cutoffs



Notes: The plot shows the reduced form effects of the main analysis while the cutoff is changed away from its true position. At $x = -1$ for example, applicants with running variables lower than -1 are counted as below the cutoff, while those with scores at or above -1 are counted as above.

The second display, Table A.1 shows the main results but using quadratic rather than linear polynomials. The effects are very close in size, but with somewhat larger standard errors.

As discussed in Section 2, a tie-breaking mechanism prioritizing those applicants who have ranked the alternative the highest could be a threat to the monotonicity assumption if applicants include safe options relatively high in their ranking. Since I remove dominated options when selecting j, k field pairs, a more preferred field that is included below a safe option will most likely never be included as k. I run a number of robustness checks to ensure this potential threat to the monotonicity assumption does not have significant bearing on the results.

First, Table A.2 removes all applicants exactly at the cutoff from the analysis. In the main analysis, I use the predefined tie-breaking rules to predict admission among applicants at the cutoff. There is no indication that these applicants can manipulate their admission status, but if they could, a donut setup would help avoid the problem. Since I use triangular kernels in all analyses, applicants at the cutoff are important. While the results in Table A.2 for degree completion are somewhat smaller, the estimates for child enrollment are larger. Standard errors are almost twice as large too showing how important the applicants at the cutoff are for statistical power. However, these differences do not change the interpretation of the results in any meaningful way, speaking to the robustness of the estimates.

Table A.1. Quadratic polynomials

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	$0.35^{\dagger}$	0.46^{**}	0.38^{**}	0.38^{**}	0.38^{**}	$0.16^{\dagger}$
	(0.19)	(0.17)	(0.13)	(0.14)	(0.12)	(0.08)
Parent enrolls in j	$2.89^{\dagger}$	3.80^{**}	3.14^{**}	2.51^{**}	2.52^{**}	$1.04^{\dagger}$
	(1.58)	(1.39)	(1.03)	(0.92)	(0.78)	(0.53)
Parent receives degree in j	$4.97^{\dagger}$	6.54^{**}	5.40^{**}	4.24^{**}	4.26^{**}	$1.76^{\dagger}$
	(2.71)	(2.39)	(1.78)	(1.56)	(1.31)	(0.90)
Observations	840 926	840 926	840 926	858 503	858 503	858 503
Control group mean	10.35%	7.85%	3.97%	5.8%	3.7%	1.65%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald (enrolls)	821	821	821	1202	1202	1202
1st stage Wald (degree)	293	293	293	485	485	485

Notes: The admission group polynomials included in the main analysis are here estimated with both linear and quadratic terms. Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Table A.2. Donut

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	0.33^{*}	0.39^{**}	0.30^{**}	0.34^{**}	0.33^{***}	0.16^{*}
	(0.14)	(0.13)	(0.09)	(0.11)	(0.09)	(0.06)
Parent enrolls in j	2.27^{*}	2.68^{**}	2.08^{**}	1.96^{**}	1.85^{***}	0.89^{**}
	(0.98)	(0.86)	(0.64)	(0.60)	(0.49)	(0.34)
Parent receives degree in j	3.68^{*}	4.34^{**}	3.36^{**}	3.18^{***}	3.00^{***}	1.44^{**}
	(1.59)	(1.39)	(1.04)	(0.97)	(0.80)	(0.56)
Observations	840 926	840 926	840 926	858 503	858 503	858 503
Control group mean	10.35%	7.85%	3.97%	5.8%	3.7%	1.65%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald (enrolls)	1709	1709	1709	2301	2301	2301
1st stage Wald (degree)	744	744	744	1046	1046	1046

Notes: In this table, the main estimation is run on a sample where applicants who are exactly at the cutoff are excluded. Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Second, Table A.3 reports results where only those observations where j is the highest ranked field have been included. Clearly, the applicant has no reason to rank a less preferred field first. While these coefficients are only weakly significant, the size of the point estimates does not differ much from the main results.

Table A.3. Only first-ranked j

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	0.37*	0.38*	0.27*	0.40**	0.31*	0.16†
	(0.18)	(0.16)	(0.12)	(0.15)	(0.13)	(0.09)
Parent enrolls in j	2.29*	2.34*	1.71*	2.06**	1.60*	0.84†
	(1.14)	(1.00)	(0.73)	(0.76)	(0.64)	(0.45)
Parent receives degree in j	3.35*	3.42*	2.50*	3.09**	2.40*	1.26†
	(1.66)	(1.46)	(1.07)	(1.14)	(0.96)	(0.67)
Observations	572 782	572 782	572 782	547 910	547 910	547 910
Control group mean	11.02%	8.14%	4.1%	6.66%	4.25%	1.96%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald (enrolls)	1451	1451	1451	1976	1976	1976
1st stage Wald (degree)	713	713	713	924	924	924

Notes: The sample includes all applicants to Swedish universities before 2000 with children who apply to university no later than 2021 where the preferred alternative j is ranked highest in the parent's application. There are no strategic incentives to rank anything but the most preferred alternative first. Coefficients and standard errors are reported in percentage points. All regressions use triangular kernel weights, and include linear polynomials of the running variables above and below the cutoff to each admission group, as well as fixed-effects for cutoff, next-best field, priority rank, age, and gender. Standard errors are two-way clustered at the cutoff and family level.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Table A.4 shows how the timing of important life events is related to admission. Since some applicants who are below the cutoff reapply to their preferred field until they are admitted, assignment risks impacting child outcomes through other ways than field enrollment, violating exclusion. While the first row shows how indeed admitted applicants enroll in their first university program slightly (less than a day) earlier, there is no effect on the timing of degree completion, fertility, or labor market participation.

Table A.4. Treatment assignemnt and threats to exclusion

	Separately estimated	Joint model
Age at first enrollment	-0.002^{***}	-0.003^{***}
	(0.000)	(0.000)
Age at first degree	0.000^{***}	0.000
	(0.000)	(0.000)
Age at first child	0.000	0.000
	(0.000)	(0.000)
Age at first job	0.000	0.000
	(0.000)	(0.000)
Observations		645 347
Wald statistic		41.639
		[p=0]

Notes: This table shows correlations between the age of the applicant at important life events and how it is correlated with treatment assignment (being above the cutoff). Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

B Additional results

This section reports additional results and further subgroup analyses. To begin, Table B.1 reports the first stage regressions also presented in Figure 2.

Table B.1. First stage estimates

	Parent admitted to j	Parent enrolls in j
Parent above cutoff to j	64.78***	15.11***
	(0.39)	(0.34)
Observations	417 656	417 656
Control group mean	0%	32.68%
Bandwidth	1.5	1.5
	Parent receives degree in j	**Parent has job common for j**
Parent above cutoff to j	9.02***	3.34***
	(0.31)	(0.34)
Observations	417 656	288 366
Control group mean	24.36%	56.9%
Bandwidth	1.5	1.5

Notes: Observations are not repeated for each child. Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Second, Table B.2 summarizes results from figure 1 and figure 5 showing both correlations and causal effects for each field of study.

Table B.3 shows inheritance by the age of the parent at the time the child applies to university. While effects are imprecise, there seems to be a strong negative effect on inheritance among parents who have reached the retirement age of 65.

Table B.4 reports the parent-child gender composition interaction terms for the results in Table 12. We see that there aggregate effects are significantly different across both parent and child genders, but few field-level interactions are significant.

Next, Table B.5 shows how the likelihood to end up having a child with a parent with a degree in the preferred field j is affected by enrollment. Not only do we observe strong assortative mating, the effect is more than doubled for mothers. A woman applying to j has a 6.46% likelihood to have a child with a man who holds a degree in j, a share that increases to 21.64% if she enrolls.

Finally, Table **??** reports the results split by the education level of the grandparents. We see very weak, or even negative, effects for families where grandparents have only elementary education, but for any higher level of education, the differences are small.

Table B.2. Associations and causal estimates (child degree completion) by field

Field	Relative popularity	Effect estimate		Control group mean	Relative effect
Teaching	140%	1.86p.p.†	(1.12)	5.99%	31%
Humanities	173%	3.57p.p.	(2.37)	6.58%	54%
Administration	128%	−1.65p.p.	(3.01)	13.56%	−12%
Business	186%	2.00p.p.**	(0.62)	3.97%	50%
Law	343%	1.14p.p.†	(0.63)	1.08%	105%
Journalism	272%	0.43p.p.	(0.75)	1.21%	36%
Social science	122%	−0.03p.p.	(0.92)	2.14%	−1%
Psychology	244%	1.66p.p.	(1.13)	1.32%	125%
Natural science	151%	2.82p.p.	(1.79)	3.61%	78%
Computer science	212%	0.41p.p.	(0.67)	1.34%	31%
Architecture	729%	1.34p.p.	(0.82)	0.55%	245%
Engineering	207%	3.21p.p.***	(0.82)	3.30%	98%
Technology	143%	12.75p.p.**	(4.12)	9.10%	140%
Agriculture	554%	−0.05p.p.	(1.18)	2.53%	−2%
Pharmacy	612%	−1.00p.p.	(0.84)	0.48%	−209%
Medicine	354%	4.28p.p.***	(1.02)	4.39%	97%
Nursing	132%	4.13p.p.	(3.55)	8.70%	47%
Social work	211%	0.23p.p.	(0.90)	1.81%	13%
Dentistry	734%	1.21p.p.	(1.64)	1.17%	104%
Services	236%	1.28p.p.	(1.65)	2.10%	61%
Aggregate	164%	2.08p.p.**	(0.64)	3.97%	52%

Notes: The relative popularity displays the numbers on the diagonal in figure 1 and is the relative share of field degree holders among children of parents with a degree in the field when compared to all children. The estimates are also reported in Figure 5 and follow the same approach as Table 4 but with separate coefficients for each field.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Table B.3. Field inheritance by parent age at child application

	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	2.12	5.80	3.86
	(6.37)	(5.80)	(5.07)
× Parent age 41–50	4.84	2.48	3.37
	(6.27)	(5.72)	(4.97)
× Parent age 51–64	3.01	−1.25	−1.51
	(6.26)	(5.71)	(4.95)
× Parent age 65+	−9.83	-16.27^{*}	−8.72
	(8.70)	(7.70)	(6.50)
Parent age 41–50	−3.43	−2.47	−3.00
	(3.17)	(2.88)	(2.51)
Parent age 51–64	-7.49^{*}	-6.20^{*}	-6.41^{*}
	(3.17)	(2.88)	(2.50)
Parent age 65+	−6.43	−2.22	−5.69
	(5.18)	(4.54)	(3.79)
Observations	454 339	454 339	454 339
Control group mean	19.38%	14.64%	7.33%
Bandwidth	1.5	1.5	1.5
1st stage Wald	200	200	200

Notes: The sample only includes children who have applied to university at least once before the end of the sample period. Otherwise, the estimation follows the same approach as Table 4.

$^{\dagger}\ p \leq 0.1$, $^{*}\ p \leq 0.05$, $^{**}\ p \leq 0.01$, $^{***}\ p \leq 0.001$.

Table B.4. Field inheritance and gender composition (interaction terms)

Field	Earns degree		Interactions					
			× Daughter		× Mother		× Mother × Daughter	
Teaching	0.43	(1.66)	1.50	(2.38)	0.63	(1.85)	1.22	(2.88)
Humanities	10.83*	(4.99)	−5.61	(6.61)	−10.30†	(5.82)	11.53	(8.42)
Administration	−1.04	(5.55)	3.06	(7.69)	−4.54	(6.85)	−2.77	(9.70)
Business	3.37***	(1.01)	−2.65*	(1.25)	−0.85	(1.25)	3.01†	(1.73)
Law	1.05	(0.99)	0.85	(1.27)	−1.12	(1.06)	1.56	(1.58)
Journalism	−1.47	(1.19)	5.27*	(2.29)	1.87	(1.26)	−5.57*	(2.69)
Social science	−1.93	(1.89)	2.84	(2.68)	1.54	(2.15)	−0.97	(3.23)
Psychology	−1.56	(2.08)	0.81	(3.02)	3.69	(2.52)	0.94	(3.97)
Natural science	2.65	(2.73)	0.86	(2.91)	2.58	(4.21)	−4.41	(5.16)
Computer science	2.11†	(1.16)	−2.61*	(1.25)	−2.46	(1.63)	3.58†	(1.91)
Architecture	0.00	(1.46)	3.71†	(2.23)	1.69	(1.60)	−4.48†	(2.42)
Engineering	4.06**	(1.26)	−1.55	(1.10)	1.68	(1.57)	−0.95	(2.00)
Technology	16.08*	(6.39)	−1.99	(5.43)	2.31	(10.13)	−15.10	(11.13)
Agriculture	−0.03	(2.34)	−0.12	(3.41)	−0.68	(2.23)	1.55	(3.98)
Pharmacy	−5.05†	(2.84)	9.63†	(5.73)	3.87	(2.79)	−10.17†	(5.91)
Medicine	2.52	(1.98)	3.11	(2.55)	2.16	(2.47)	−2.22	(3.40)
Nursing	7.30	(7.70)	10.83	(12.96)	−6.25	(8.53)	−11.66	(14.76)
Social work	−1.63	(1.46)	3.56	(2.72)	1.66	(1.47)	−2.78	(3.19)
Dentistry	6.95*	(3.13)	−0.01	(5.94)	−10.37**	(3.45)	−2.54	(6.25)
Services	8.18	(4.99)	−10.66†	(6.08)	−5.23	(5.45)	7.81	(6.94)
Aggregate	3.13***	(0.80)	−1.55**	(0.56)	−1.37*	(0.57)	2.51**	(0.78)

Notes: The table reports results from a regression where parent enrollment is interacted with field as well as parent and child gender. Otherwise, the estimation follows the same approach as Table 4. Table 12 reports linear combinations of the coefficients estimated in this regression.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Table B.5. Assortative mating (first stage)

	Broad fields	Narrow fields
	Other parent has degree in j	Other parent has degree in j
Parent enrolls in j	5.95***	5.58***
	(1.56)	(1.03)
× Parent female	8.32***	5.80***
	(1.04)	(0.79)
Parent female	−2.75***	−0.90*
	(0.52)	(0.36)
Observations	840 926	858 503
Control group mean	10.31%	6.18%
Bandwidth	1.5	1.5
1st stage Wald		947

Notes: The table shows, separately for mothers and fathers, how the likelihood that the other parent has a degree from field j is affected by whether the parent enrolls in j or not. It is a first stage of sorts for Table 13. Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Table B.6. Grandparents' educational level

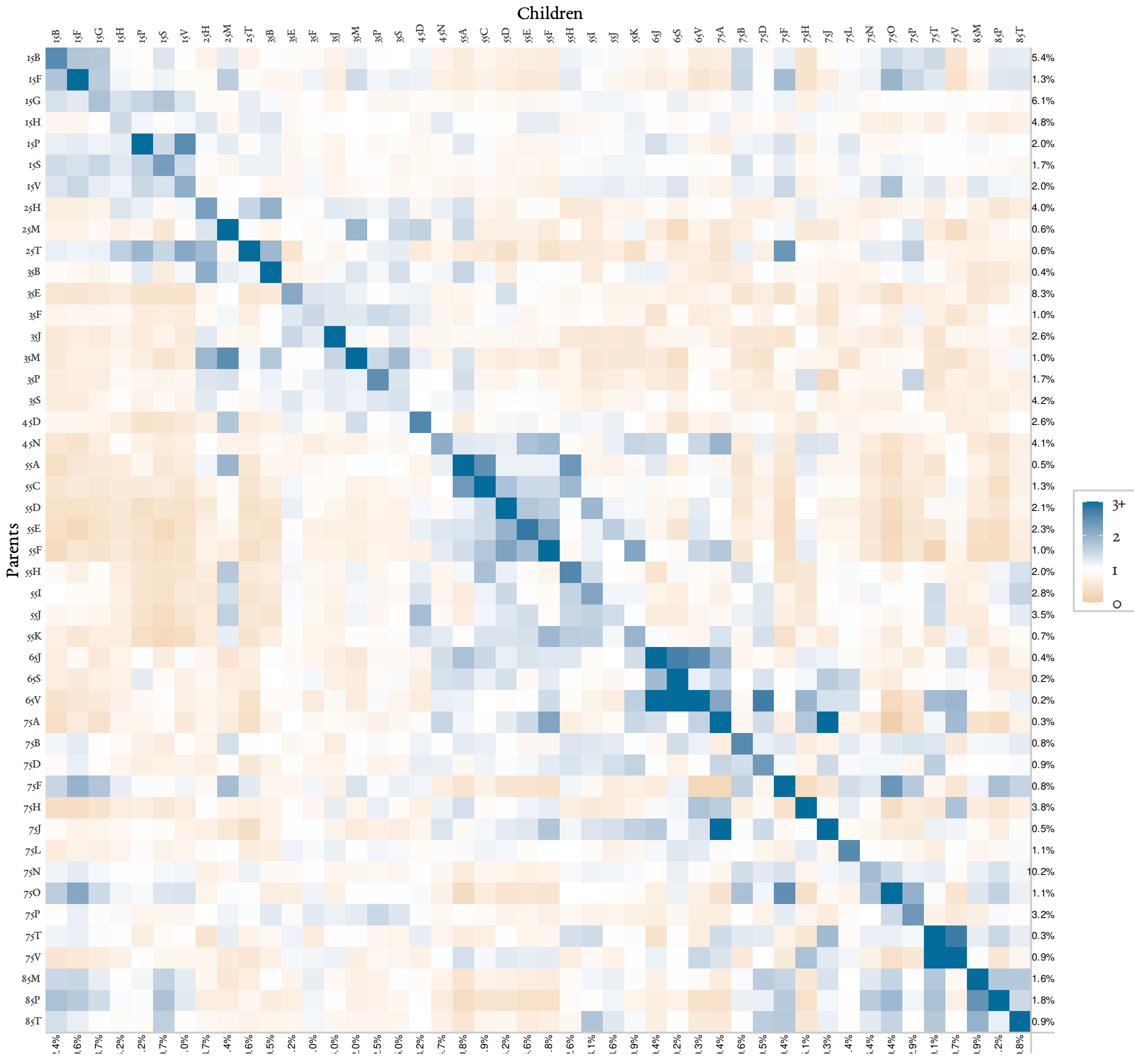
	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	0.94	1.94†	1.80*	1.27†	1.42*	0.60
	(1.22)	(1.07)	(0.82)	(0.75)	(0.62)	(0.45)
× Grandparent high school	1.13	0.27	0.25	0.50	0.03	0.16
	(0.88)	(0.78)	(0.61)	(0.59)	(0.48)	(0.35)
× Grandparent post-secondary	1.25	1.41	−0.37	0.31	0.77	0.33
	(1.06)	(0.95)	(0.71)	(0.74)	(0.60)	(0.41)
× Grandparent tertiary	2.13*	1.34	0.87	1.13†	0.78	0.53
	(0.92)	(0.82)	(0.62)	(0.62)	(0.51)	(0.36)
Grandparent high school	−0.11	0.13	−0.07	−0.04	0.11	0.00
	(0.42)	(0.38)	(0.30)	(0.27)	(0.22)	(0.16)
Grandparent post-secondary	−0.25	−0.45	0.11	0.06	−0.31	−0.16
	(0.52)	(0.46)	(0.34)	(0.34)	(0.27)	(0.18)
Grandparent tertiary	−0.48	−0.20	−0.40	−0.22	−0.15	−0.25
	(0.45)	(0.40)	(0.30)	(0.29)	(0.23)	(0.17)
Observations	840 926	840 926	840 926	858 503	858 503	858 503
Control group mean	10.35%	7.85%	3.97%	5.8%	3.7%	1.65%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald	233	233	233	290	290	290

Notes: Grandparents' educational level is defined as the highest educational level attained by any of an individual's grandparents. The reference group is grandparents with less than high school education. Otherwise, the estimation follows the same approach as Table 4.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

C Additional results for narrow fields

Figure C.1. Degrees of children and parents



Notes: Same as Figure 1 but with the narrow definition of fields of study. For a translation of the SunGrp codes, see Table E.1

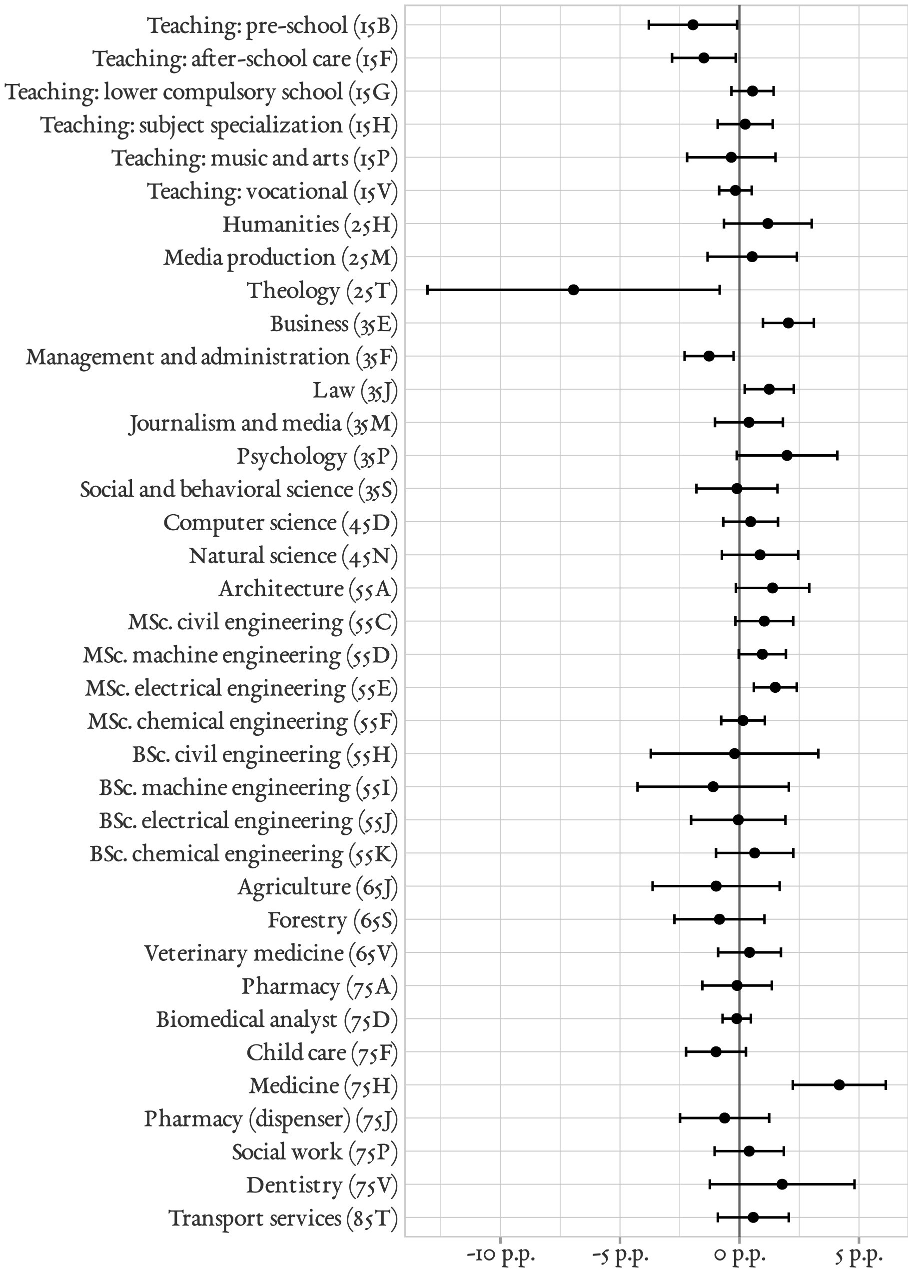
Table C.1. Summary statistics by field of study

	Observations	Unique parents	Share women	Average age	Share enrolled below cutoff	First stage (parent enrolls)
Teaching: pre-school (15B)	61 386	28 737	92%	19.79	39%	9p.p.***
Teaching: after-school care (15F)	30 922	14 873	75%	20.50	22%	13p.p.***
Teaching: lower compulsory school (15G)	50 080	23 091	76%	21.31	22%	27p.p.***
Teaching: subject specialization (15H)	28 260	13 551	59%	21.44	25%	26p.p.***
Teaching: music and arts (15P)	3102	1516	98%	21.93	27%	12p.p.*
Teaching: vocational (15V)	9350	4372	53%	21.78	15%	30p.p.***
Humanities (25H)	25 760	13 682	74%	20.89	19%	11p.p.***
Media production (25M)	1502	776	57%	20.73	9%	14p.p.*
Theology (25T)	2185	1036	58%	20.70	39%	1p.p.
Business (35E)	120 387	59 341	46%	20.95	35%	18p.p.***
Management and administration (35F)	32 594	16 700	59%	21.03	23%	14p.p.***
Law (35J)	54 954	27 832	56%	20.58	27%	16p.p.***
Journalism and media (35M)	14 290	7298	64%	21.77	10%	38p.p.***
Psychology (35P)	9588	4809	66%	23.25	18%	25p.p.***
Social and behavioral science (35S)	31 027	15 794	59%	20.90	15%	13p.p.***
Computer science (45D)	35 127	17 773	38%	21.02	23%	17p.p.***
Natural science (45N)	50 233	25 130	45%	20.30	33%	11p.p.***
Architecture (55A)	12 460	6158	55%	20.94	18%	35p.p.***
MSc. civil engineering (55C)	23 443	11 270	32%	20.27	25%	18p.p.***
MSc. machine engineering (55D)	47 451	22 714	17%	20.29	36%	22p.p.***
MSc. electrical engineering (55E)	57 973	29 200	13%	20.27	38%	28p.p.***
MSc. chemical engineering (55F)	22 537	11 107	41%	19.84	25%	20p.p.***
BSc. civil engineering (55H)	1681	806	28%	20.68	21%	11p.p.
BSc. machine engineering (55I)	4216	2123	25%	20.43	24%	14p.p.***
BSc. electrical engineering (55J)	13 589	7016	13%	20.36	32%	13p.p.***
BSc. chemical engineering (55K)	3248	1659	85%	20.15	14%	13p.p.**
Agriculture (65J)	7058	3234	55%	20.62	34%	12p.p.***
Forestry (65S)	3217	1451	18%	21.22	40%	23p.p.***
Veterinary medicine (65V)	5156	2423	71%	21.19	16%	44p.p.***
Pharmacy (75A)	4744	2301	74%	20.54	22%	19p.p.***
Biomedical analyst (75D)	215	101	80%	20.27	22%	59p.p.***
Child care (75F)	2164	1057	64%	20.81	11%	21p.p.***
Medicine (75H)	31 136	13 805	44%	22.01	48%	18p.p.***
Pharmacy (dispenser) (75J)	4767	2352	93%	20.27	19%	13p.p.***
Social work (75P)	40 746	20 046	81%	21.53	26%	14p.p.***
Dentistry (75V)	10 900	5199	52%	21.45	39%	7p.p.*
Transport services (85T)	1055	544	36%	20.81	13%	22p.p.**

Notes: This table corresponds to Table 2, but the statistics are grouped by narrow fields.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

Figure C.2. Inheritance of narrow fields



Notes: The figure corresponds to Figure 5 but for narrow fields. The point estimates are reported in Table C.2. The regression uses the same specification as the main analysis in Table 4.

Table C.2. Field heterogeneity narrow fields

Field	Relative popularity	Effect estimate		Control group mean	Relative effect
Teaching: pre-school (15B)	253%	−1.95p.p.*	(0.94)	1.05%	−185%
Teaching: after-school care (15F)	323%	−1.49p.p.*	(0.68)	0.28%	−535%
Teaching: lower compulsory school (15G)	180%	0.55p.p.	(0.45)	1.49%	37%
Teaching: subject specialization (15H)	146%	0.24p.p.	(0.59)	1.24%	19%
Teaching: music and arts (15P)	313%	−0.34p.p.	(0.94)	0.29%	−118%
Teaching: vocational (15V)	206%	−0.17p.p.	(0.35)	0.11%	−151%
Humanities (25H)	225%	1.19p.p.	(0.94)	0.97%	122%
Media production (25M)	537%	0.53p.p.	(0.95)	0.19%	287%
Theology (25T)	925%	−6.94p.p.*	(3.12)	0.80%	−871%
Business (35E)	214%	2.05p.p.***	(0.54)	3.97%	52%
Management and administration (35F)	150%	−1.27p.p.*	(0.52)	0.36%	−353%
Law (35J)	341%	1.25p.p.*	(0.52)	1.08%	115%
Journalism and media (35M)	321%	0.40p.p.	(0.73)	1.21%	33%
Psychology (35P)	250%	1.99p.p.†	(1.07)	1.32%	150%
Social and behavioral science (35S)	137%	−0.10p.p.	(0.87)	1.44%	−7%
Computer science (45D)	260%	0.47p.p.	(0.58)	1.34%	35%
Natural science (45N)	207%	0.86p.p.	(0.82)	1.20%	72%
Architecture (55A)	710%	1.39p.p.†	(0.78)	0.55%	254%
MSc. civil engineering (55C)	348%	1.04p.p.†	(0.62)	0.83%	126%
MSc. machine engineering (55D)	306%	0.96p.p.†	(0.50)	1.51%	63%
MSc. electrical engineering (55E)	283%	1.50p.p.**	(0.46)	1.38%	109%
MSc. chemical engineering (55F)	414%	0.15p.p.	(0.47)	0.43%	35%
BSc. civil engineering (55H)	259%	−0.21p.p.	(1.79)	0.51%	−40%
BSc. machine engineering (55I)	220%	−1.10p.p.	(1.61)	0.38%	−290%
BSc. electrical engineering (55J)	144%	−0.05p.p.	(1.01)	0.49%	−10%
BSc. chemical engineering (55K)	198%	0.64p.p.	(0.82)	0.05%	1358%
Agriculture (65J)	961%	−0.98p.p.	(1.36)	0.76%	−128%
Forestry (65S)	1777%	−0.84p.p.	(0.96)	0.63%	−132%
Veterinary medicine (65V)	1314%	0.42p.p.	(0.67)	0.46%	91%
Pharmacy (75A)	818%	−0.10p.p.	(0.74)	0.38%	−27%
Biomedical analyst (75D)	236%	−0.12p.p.	(0.30)	0.00%	
Child care (75F)	478%	−0.98p.p.	(0.64)	0.07%	−1351%
Medicine (75H)	332%	4.18p.p.***	(0.99)	4.39%	95%
Pharmacy (dispenser) (75J)	704%	−0.62p.p.	(0.95)	0.23%	−272%
Social work (75P)	236%	0.41p.p.	(0.74)	1.67%	24%
Dentistry (75V)	853%	1.79p.p.	(1.54)	1.03%	173%
Transport services (85T)	752%	0.58p.p.	(0.76)	0.00%	
Aggregate	276%	0.89p.p.**	(0.34)	1.65%	54%

Notes: This table corresponds to Table B.2, but the statistics are grouped by narrow fields.

† $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

D Inheritance of institution and location preferences

Instead of collapsing alternatives by field of study and looking at treatment margins where applicants are either admitted into one field or deferred to another, we can perform the same exercise but for institutions.[25] This is a useful way to gain an additional measure against which we can benchmark the results. Table D.1 reports the results of this exercise, where the outcome variables take the value 1 if the child follows to the same institution, regardless of what field of study they choose.

Just like with the transmission of education preferences between siblings (Altmejd et al. 2021), the preferences for going to the same institution across generations are a lot stronger, with as many as 18% of children enrolling in the institution that the parent applied to. The absolute effects are often more than twice as large as the ones reported in Table 4, but relative effects are only somewhat larger. The likelihood of earning a degree in a specific field increased with 98% when a parent has enrolled in it, while the corresponding effect for a child earning a degree from a specific institution is 108%.

Table D.1. Inheritance of institutions

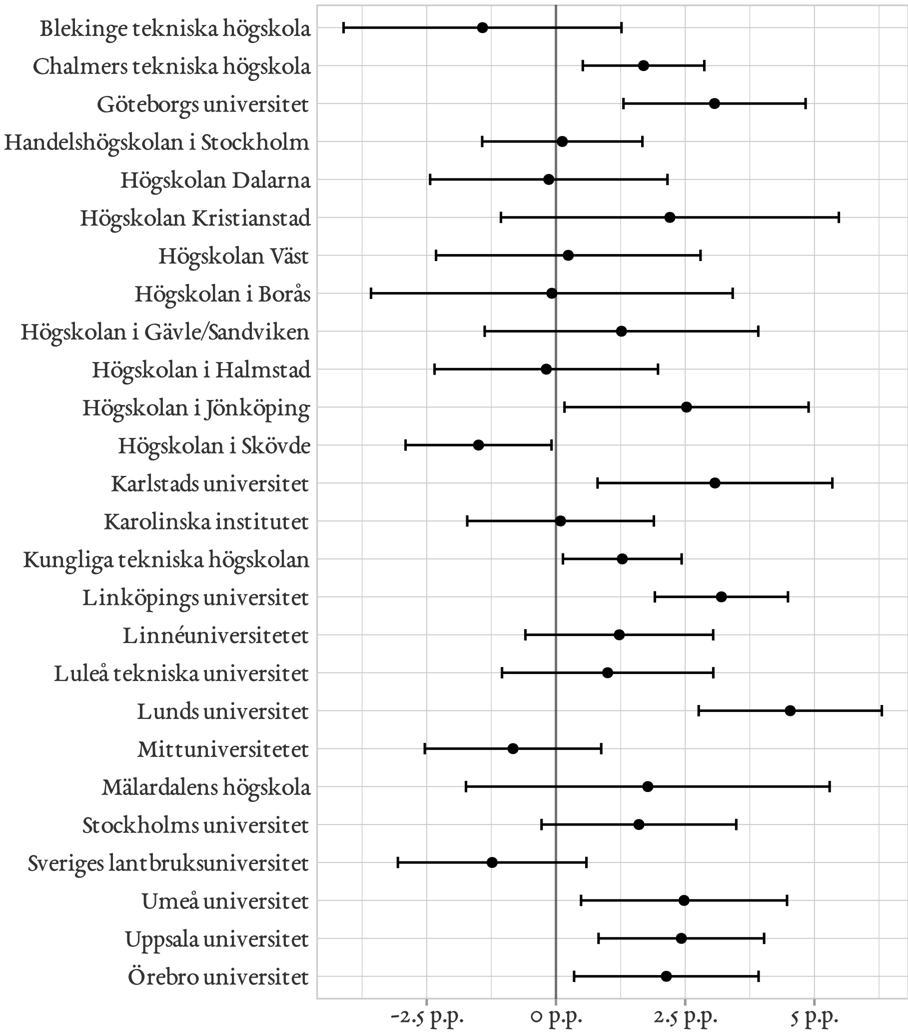
	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	0.93***	0.94***	0.38***
	(0.16)	(0.14)	(0.08)
Parent enrolls in j	5.18***	5.21***	2.10***
	(0.89)	(0.80)	(0.45)
Parent receives degree in j	6.94***	6.98***	2.81***
	(1.19)	(1.07)	(0.60)
Observations	1 059 920	1 059 920	1 059 920
Control group mean	14.57%	10.79%	3.08%
Bandwidth	1.5	1.5	1.5
1st stage Wald (enrolls)	3031	3031	3031
1st stage Wald (degree)	1949	1949	1949

Notes: Instead of collapsing consecutive options by field of study, the sample includes applications collapsed by institution. A child is thus classified as following their parent only if they pick the same institution as their parent, irrespective of what program they chose. Otherwise, the estimation follows the same approach as Table 4.

Figure D.1 presents separate coefficients for each institution. Again, we see few negative effects. The largest and most precise estimates are for big universities that offer a broad range of alternatives. The two most prestigious schools, Stockholm school of economics (Handelshögskolan i Stockholm, SSE) and the Karolinska Institute both exhibit small effects that are not significant. Interestingly, all students at SSE study business which is a field with positive inheritance. A possible reason for this is simply that both school have very high admission requirements ensuring only the most academically successful children will apply there. On the other hand, the point estimates of the two effects are very similar in size to the effect estimated by Barrios-Fernández et al. (2021), who show that children are 2.6 percentage points more likely to attend an elite college if their parents do so.

25. Many institutions have changed their names, merged, or reorganized during the period. I only include institutions that have existed during at least some part of the parent application period (1977–1992) and classify rebranded institutions with the same identifier. For example, Linnéuniversitet is a merger of Kalmar and Växjö universities. A child who goes to Linnéuniversitet is classified as following their parent no matter which of the two schools that parent applied to.

Figure D.1. Inheritance of institutions



Notes: The regression is run on a sample constructed by collapsing consecutive alternatives by institution rather than field. It runs same specification as the main analysis in Table 4 but with next-best fixed effects at the institution level.

Inheriting institutional preferences is likely explained by how institutions are located in different cities. Since a significant share of parents who move to a new city for their university studies stay there, admission also affects what city their children live in. Table D.2 shows results of such an exercise, where alternatives are grouped by commuting zone (2018 local labor market). This means that consecutive applications to schools in the Stockholm-Uppsala region are collapsed, for example. The results are again slightly larger but with larger baseline means, yielding similar relative effects — showing how important location is for university choice.

Table D.2. Inheritance of locations

	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	1.34***	1.20***	0.46***
	(0.18)	(0.16)	(0.10)
Parent enrolls in j	8.34***	7.50***	2.90***
	(1.08)	(1.00)	(0.63)
Parent receives degree in j	10.79***	9.71***	3.75***
	(1.41)	(1.30)	(0.82)
Observations	1 005 529	1 005 529	1 005 529
Control group mean	18.22%	14.87%	4.79%
Bandwidth	1.5	1.5	1.5
1st stage Wald (enrolls)	2517	2517	2517
1st stage Wald (degree)	1605	1605	1605

Notes: Instead of collapsing consecutive options by field of study, the sample includes applications collapsed by local labor market. A child is thus classified as following their parent as long as they choose a program at an institution in the same local labor market (commuting zone) as their parent, irrespective of what program and institution it is. Otherwise, the estimation follows the same approach as Table 4.

Last, as an additional benchmark, we can also group consecutive alternatives by their field-institution combination. Now, only consecutive options to the same field and institution are collapsed. Table D.3 reports these aggregate results. Baselines are of course miniscule here, but also the absolute effects are smaller. Parental enrollment in a field-institution combination increases graduation probability by 2.18 percentage points or 172%. That this relative effect is so much larger indicates that the effect of institution and field are complementary, and that the main results of this paper are not driven by institutions that only offer few fields of study to chose from.

Table D.3. Inheritance of field-institutions

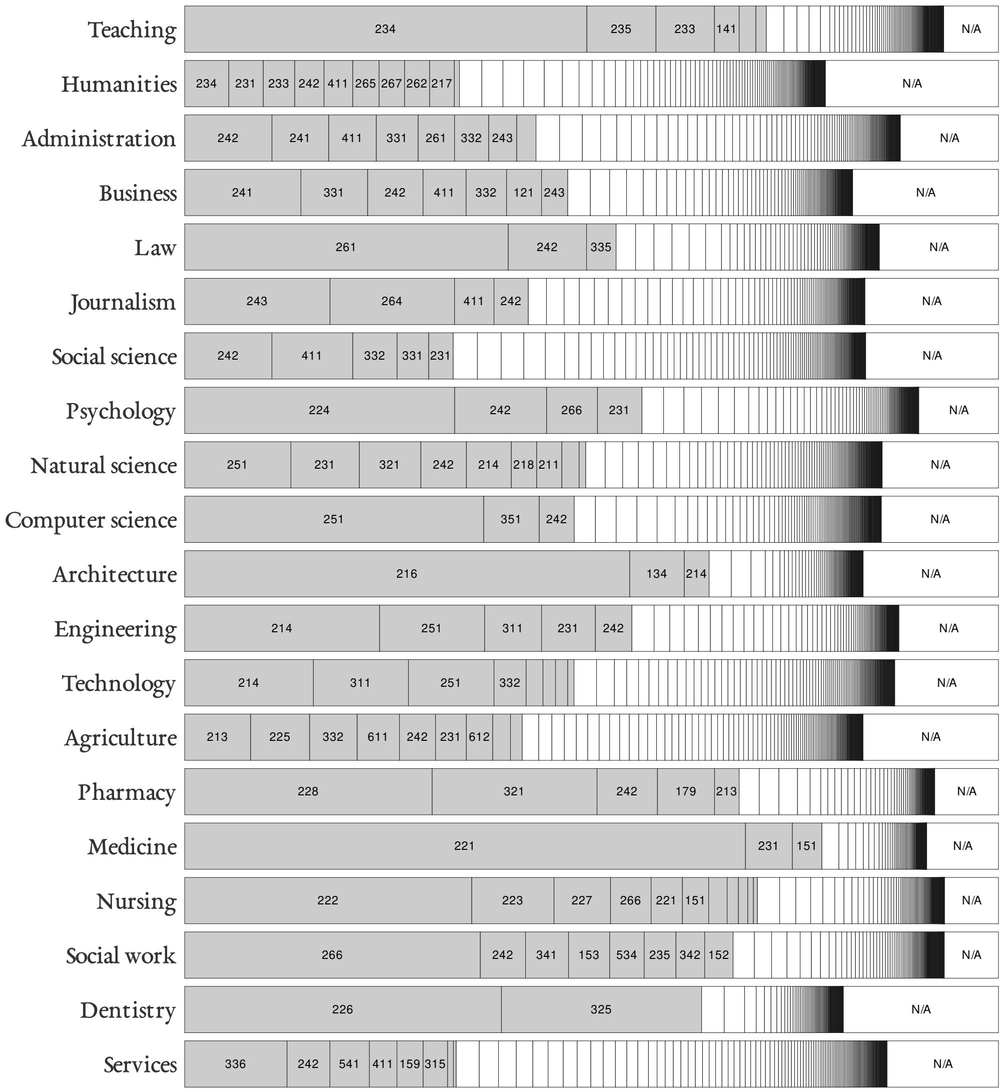
	Field-institution			Field (holding institution constant)		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	0.44***	0.36***	0.18***	0.92**	0.69*	0.20
	(0.08)	(0.07)	(0.04)	(0.35)	(0.28)	(0.16)
Parent enrolls in j	2.00***	1.62***	0.83***	3.12**	2.35*	0.68
	(0.36)	(0.31)	(0.17)	(1.19)	(0.95)	(0.53)
Parent receives degree in j	3.24***	2.63***	1.34***	5.82**	4.37*	1.26
	(0.58)	(0.50)	(0.28)	(2.22)	(1.76)	(0.99)
Observations	1 160 176	1 160 176	1 160 176	86 913	86 913	86 913
Control group mean	3.18%	2.17%	0.65%	4.17%	2.53%	0.81%
Bandwidth	1.5	1.5	1.5	1.5	1.5	1.5
1st stage Wald (enrolls)	4488	4488	4488	810	810	810
1st stage Wald (degree)	2261	2261	2261	249	249	249

Notes: Instead of collapsing consecutive options by field of study, the sample includes applications collapsed by institution-field combinations. A child is thus classified as following their parent only if they pick the same institution as their parent, irrespective of what program they chose. Otherwise, the estimation follows the same approach as Table 4.

E Codebook

Figure E.1 is an attempt at illustrating the complicated relationship between fields of study and occupation. Working age Swedes with university degrees are sorted by their occupation in 2017 using Swedish 3-digit SSYK occupation codes (see Table E.2 for a codebook). Graduates from fields where it is possible to gain an occupational license end up in relatively few different occupations, while e.g. social science leads to a large variety.

Figure E.1. Most common occupations by field



Notes: For each field of study, the figure plots the share of university degree-holders among the full Swedish population who in 2017 work in different 3-digit occupation codes (SSYK 2012). Occupations highlighted in gray are counted as "most common" and include those occupations where either more than 3% of all degree-holders from the field work, or where more than 30% of workers have a degree from the field. This definition of common jobs is used in Table 11.

Table E.1. Narrow field codes and descriptions

Code	Description	Broad field
15B	Teaching: pre-school	Teaching
15F	Teaching: after-school care	Teaching
15G	Teaching: lower compulsory school	Teaching
15H	Teaching: subject specialization	Teaching
15P	Teaching: music and arts	Teaching
15S	Teaching: special needs	Teaching
15V	Teaching: vocational	Teaching
25H	Humanities	Humanities
25M	Media production	Humanities
25T	Theology	Humanities
35B	Library science	Administration
35E	Business	Business
35F	Management and administration	Administration
35J	Law	Law
35M	Journalism and media	Journalism
35P	Psychology	Psychology
35S	Social and behavioral science	Social science
45D	Computer science	Computer science
45N	Natural science	Natural science
55A	Architecture	Architecture
55C	MSc. civil engineering	Engineering
55D	MSc. machine engineering	Engineering
55E	MSc. electrical engineering	Engineering
55F	MSc. chemical engineering	Engineering
55G	MSc. engineering, other	Engineering
55H	BSc. civil engineering	Technology
55I	BSc. machine engineering	Technology
55J	BSc. electrical engineering	Technology
55K	BSc. chemical engineering	Technology
65J	Agriculture	Agriculture
65S	Forestry	Agriculture
65V	Veterinary medicine	Agriculture
75A	Pharmacy	Pharmacy
75B	Occupational therapy	Nursing
75D	Biomedical analyst	Natural science
75F	Child care	Social work
75H	Medicine	Medicine
75J	Pharmacy (dispenser)	Pharmacy
75L	Physiotherapy	Nursing
75N	Nursing	Nursing
75O	Social care	Social work
75P	Social work	Social work
75T	Dental hygiene	Dentistry
75V	Dentistry	Dentistry
85T	Transport services	Services

Table E.2. SSYK 2012 codes of common occupations

SSYK	Occupations	Fields
121	Finance managers	Business
134	Architectural and engineering managers	Architecture
141	Primary and secondary schools and adult education managers	Teaching
151	Health care managers	Medicine, Nursing
152	Managers in social and curative care	Social work
153	Elderly care managers	Social work
159	Other social services managers	Services
179	Other services managers not elsewhere classified	Pharmacy
211	Physicists and chemists	Natural science
213	Biologists, pharmacologists and specialists in agriculture and forestry	Agriculture, Pharmacy
214	Engineering professionals	Architecture, Engineering, Natural science, Technology
216	Architects and surveyors	Architecture
217	Designers	Humanities
218	Specialists within environmental and health protection	Natural science
221	Medical doctors	Medicine, Nursing
222	Nursing professionals	Nursing
223	Nursing professionals (cont.)	Nursing
224	Psychologists and psychotherapists	Psychology
225	Veterinarians	Agriculture
226	Dentists	Dentistry
227	Naprapaths, physiotherapists, occupational therapists	Nursing
228	Specialists in health care not elsewhere classified	Pharmacy
231	University and higher education teachers	Agriculture, Engineering, Humanities, Medicine, Natural science, Psychology, Social science
233	Secondary education teachers	Humanities, Teaching
234	Primary- and pre-school teachers	Humanities, Teaching
235	Teaching professionals not elsewhere classified	Social work, Teaching
241	Accountants, financial analysts and fund managers	Administration, Business
242	Organisation analysts, policy administrators and human resource specialists	Administration, Agriculture, Business, Computer science, Engineering, Humanities, Journalism, Law, Natural science, Pharmacy, Psychology, Services, Social science, Social work
243	Marketing and public relations professionals	Administration, Business, Journalism
251	ICT architects, systems analysts and test managers	Computer science, Engineering, Natural science, Technology
261	Legal professionals	Administration, Law
262	Museum curators and librarians and related professionals	Humanities
264	Authors, journalists and linguists	Journalism
265	Creative and performing artists	Humanities
266	Social work and counselling professionals	Nursing, Psychology, Social work
267	Religious professionals and deacons	Humanities
311	Physical and engineering science technicians	Engineering, Technology
315	Ship and aircraft controllers and technicians	Services
321	Medical and pharmaceutical technicians	Natural science, Pharmacy
325	Dental hygienists	Dentistry
331	Financial and accounting associate professionals	Administration, Business, Social science
332	Insurance advisers, sales and purchasing agents	Administration, Agriculture, Business, Social science, Technology
335	Tax and related government associate professionals	Law
336	Police officers	Services
341	Social work and religious associate professionals	Social work
342	Athletes, fitness instructors and recreational workers	Social work
351	ICT operations and user support technicians	Computer science
411	Office assistants and other secretaries	Administration, Business, Humanities, Journalism, Services, Social science
534	Attendants, personal assistants and related workers	Social work
541	Other surveillance and security workers	Services
611	Market gardeners and crop growers	Agriculture
612	Animal breeders and keepers	Agriculture