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Inheritance of Fields of Study

Abstract

University graduates are two to five times as likely to hold a degree in the field that their parents graduated from. To estimate how much of this association is caused by the education 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 73% more likely to graduate from a field that their parent has quasi-randomly enrolled in. The effect is always positive, but varies in size across fields. Engineering, medicine, social science, and business exhibit the largest effects. For these fields, parental enrollment increases child graduation rates with between 6.0 and 9.5 percentage points. I find little evidence for comparative advantage being the key driver of field inheritance. Parental field enrollment does not increase subject-specific skills in primary school, nor do labor market returns differ by access to parents with the same degree. Rather, parents seem to function as role models, making their own field choice salient. This is indicated by the fact that children are more likely to follow parents with the same gender, and that parental enrollment in gender incongruent fields is more impactful.

JEL-Codes: I240, J620.

Keywords: intergenerational transmission, fields of study.

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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 and 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 socio-economic 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 and other factors whose influence spans 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 73% more likely to earn a degree from the same field. The effect is strongest for engineering, medicine, social science, and business, and negative for 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 test score improvements from having parents who enroll in a specific field, nor are their labor market returns from enrolling in a field higher if they have a parent with the same degree. My results are easier to justify if the parent is thought of as a role model. Indeed, children are more likely to follow parents of the same gender, and parents choosing fields that are incongruent with stereotypes about their gender exert stronger influence.

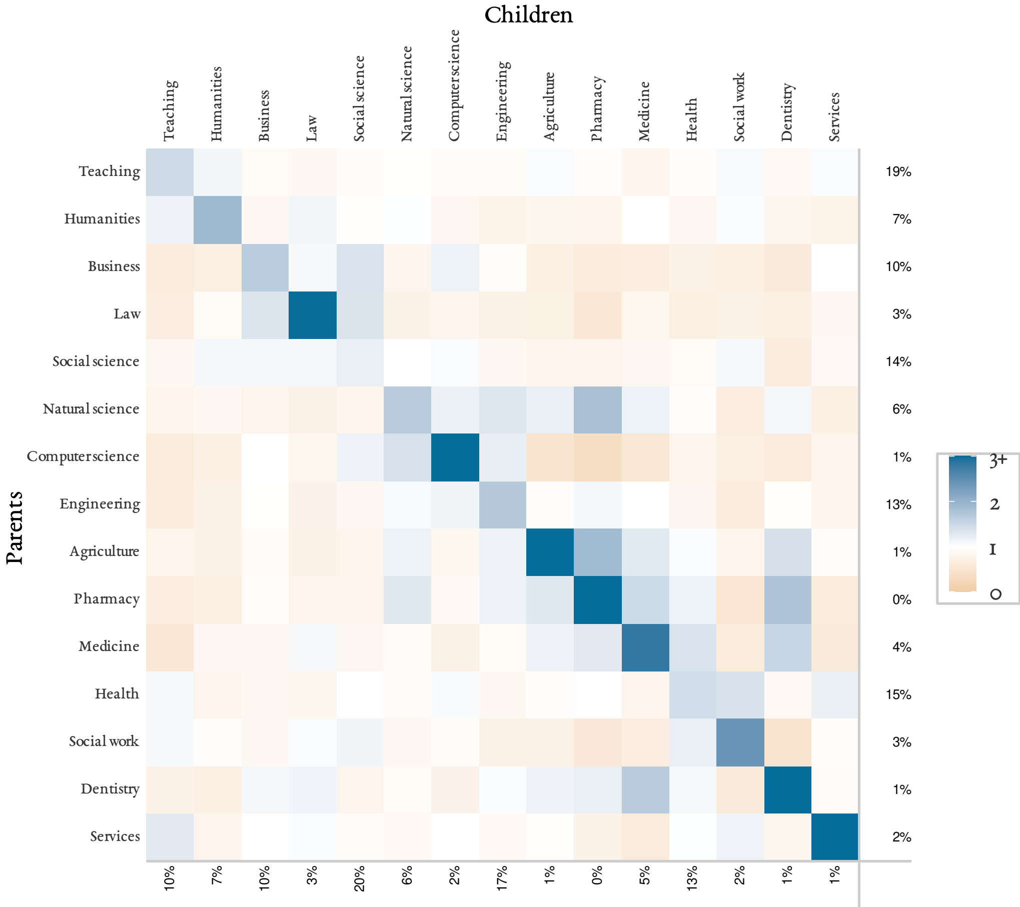
Increasing equality of opportunity is a desired objective in most liberal democracies. To understand how the correlation of educational outcomes across generations is linked to mobility and equality, it is essential to identify and estimate the size of the mechanisms through which these correlations 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 parents with degrees in agriculture being more than 5 times as likely to earn similar degrees, while children of social scientists being no more than 1.2 times as likely. Importantly, we

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

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 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 73%, which can be compared to a relative popularity of 173%.

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 3% of the children in the sample earn a degree in law, the rate is about 9% 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 did not manage to cross the threshold.

This paper contributes to several large, but somewhat disparate, strands of literature. Students 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. (2023) is the only other paper that studies causal intergenerational transmission of fields of study specifically. Using a similar econometric design, 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, Figure 10 in this paper shows positive maternal influence on a variety of fields, in most cases stronger for daughters, but confirms their results on gender incongruent choices.

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. The causal effects presented in this paper are also 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 gender differences in assortative mating.

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.

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 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.

Figure 8 measures subject-specific skill transfers to measure if parents transmit knowledge. This analysis builds on Hanushek et al. (2023) who use Dutch data in a peer-quality-IV and a within-family design to show that a comparative advantage in math is transmitted across generations and then later influences the child's preferences for STEM subjects. Figure 8 does not confirm these results. In my analysis, most STEM subjects do not improve primary school test scores in math or natural science any more than they improve scores in language or social science, and vice versa for the other fields.

Next, the analysis in Figure 9 of this paper relates to the economic returns of following a parent, as a way to quantify comparative advantage. A couple of contemporary papers also take this approach. Staiger (2023) uses linked employer-employee data from the US and finds large and robust improvements in initial earnings from working at a parent's employer. Ventura (2023) exploits lotteries to medical school in the Netherlands and finds returns to be 23% higher for children with parents with a medical degree. Birkelund (2024) looks at all fields but in a family-fixed effects design. Comparing siblings, Birkelund finds returns to reproducing a parent's field of study that are much smaller than Ventura's. At an aggregate level, following a parent is only related to a 2% increase in earnings. While the point estimate for medicine reported in this paper's Figure 9 is almost identical to Ventura's, I estimate an aggregate returns difference that, while not significant, is 36% *lower* for children who follow their parent.

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.

Finally, this paper contributes to the literature on the relative importance of genetic and environmental effects in explaining schooling outcomes. 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—an environmental mechanism that the parent commands—namely how parental field specialization directly influences educational preferences and degree completion. While the effect identified is a miniscule part of 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 analyses 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. Like in many other European countries, individuals apply by submitting a preference ranking of study 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 their score in each of the admission groups that they are eligible for, and only those with the highest score in each group are admitted. Importantly, there is no system of legacy admissions, ensuring that children have no mechanical advantage if they apply to the same program as their parents.

In this paper, I use data on university applications submitted between 1977 and 2023 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 whom I observe applications until the summer of 2023.

I use university application data from three sources. Applications from the current admission system (2008–2023) comes from Universitets- och Högskolerådet (UHR). I have retrieved older applications from the UHÄ (1977–1992) and VHS (1993–2005) archives at the Swedish National Archives (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 2023.[9]

To be eligible for post-secondary education, applicants must have finished high school or 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 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 from 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 admission group. 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.

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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.

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. For grand parents, I get additional information about completed education from the 1968 census. 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.

Each application period consists of two or three 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 for admission to higher prioritized alternatives. The admission procedure is then repeated in a second and, for 1977-1992, third round.[11]

When applicants are sorted by their admission group scores, ties need to be broken. Because admission scores are coarse, applicants often have the same score. In total, 14% of the parents in the study sample compete with a score exactly at a cutoff. Out of these, approximately 60% share their position with at least one other applicant.[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 choice in their application list (used during 1977–2005), and 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 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.

2.1 Sample construction and description

For the raw application data to be used in a regression discontinuity analysis it first needs to be processed. 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 eligible applicants who were not admitted at the end of the application round. I drop applicants who were admitted in non-standard admission groups and to institutions that only offer practical programs, since their admission scores cannot be used for RDD analysis.

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. In the full data, the third quartile of the number of applicants at the threshold is 2 and max is 36 individuals with exactly the same score at the cutoff.

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 final round outcomes are influenced by responses to the offers received in previous rounds. Applicants often drop out of the waiting list for choices that they would have been admitted to if they had stayed. Using second round scores to calculate cutoffs increases accuracy of the first stage greatly, because otherwise a large share of applicants directly below the cutoff would have been admitted. Doing so is not a problem for identification 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 it is not random who stays in the queue after the first round. For example, access to housing makes it less risky to gamble on admission to a preferred program in a different city. Such selection would bias the RDD estimates.

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 the cutoff would have to decrease the least for them to be admitted. 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. Most displays in the paper include results for both broad and narrow fields. 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. To ensure each observation has a well-defined counterfactual field, I drop applicants who only include programs from a single field in their application.

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 but a smaller sample. The downside is that the categorization, created by the author, is somewhat arbitrary. The narrow categorization—called education groups (SUNGrp or *utbildningsgrupper*) by Statistics Sweden—is official and created to cluster fields that map to different occupations. It is however much more detailed, with e.g., more than 8 different fields that map to the broad field engineering, decreasing the statistical power of heterogeneity analyses and making them harder to interpret.

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. To ensure the interpretability, all regression include individual-level weights. I only include observations where k is a well-defined field.

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.

13. This is the main reason why the first stage for admission, the leftmost plots 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.

In the analysis, I focus on parents who apply to university during 1977–1992, are below the age of 30 when they apply to university, and have children born before 2002. Excluding applicants without children conditions on a post-treatment outcome, but Table 4 shows that enrollment has no effect on fertility. Since the application, enrollment, and degree completion data ends in 2023, it is likely some children have yet to follow their parents.[14]

Table 1 shows summary statistics for the main sample of analysis within a bandwidth of 2 standard deviations (third column), but also how this data set differs from all university applicants during 1977–1992 (first column) and all admissible applicants (second column, includes those who have not been filtered out in the data processing described above). Differences across the two rightmost samples are small, but the admissible applicants—who apply to one more field on average—are less likely to come from an immigrant background, and more likely to have university educated parents with higher earnings.

Table 1. Summary statistics

	All applicants	Admissible applicants	Study sample
Birthyear	1961.86	1963.87	1962.61
	(7.30)	(5.26)	(4.96)
Applicant female	55.04%	52.15%	55.26%
	(0.50)	(0.50)	(0.50)
Foreign born	5.42%	3.51%	3.31%
	(0.23)	(0.18)	(0.18)
Parents foreign born	8.13%	6.33%	5.86%
	(0.27)	(0.24)	(0.23)
Parents' earnings (kSEK)	495.70	544.19	540.64
	(303.98)	(327.22)	(283.24)
Parent has uni. degree	0.34	0.45	0.44
	(0.48)	(0.50)	(0.50)
Has children	82.05%	81.88%	100.00%
	(0.38)	(0.39)	(0.00)
N. children	1.84	1.85	2.35
	(1.15)	(1.15)	(0.84)
N. applicants	456 237	72 797	45 200
N. ranked alternatives	2.42	3.68	3.66
	(1.44)	(1.42)	(1.42)
N. ranked broad fields	1.44	2.39	2.39
	(0.70)	(0.64)	(0.64)
N. ranked narrow fields	1.63	2.66	2.65
	(0.86)	(0.83)	(0.83)

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 2 standard deviations from either side of the admission cutoff. The third and fourth column focus on those applicants inside the bandwidth who have children. In the third—the sample used for all analyses in the paper—I summarize observations of applicants with children who were old enough to apply before the end of the sample period in 2023. In the last column, I instead limit the sample to include all children who apply to university before the summer of 2023.

14. The average time from parent treatment to the first time a child applies to university is 30 years.

In the main sample of analysis, several fields have been excluded. I am not able to study the effect of nursing or services because these fields are not university subjects in the parent generation. I exclude computer science because it is a completely different subject in the 80s, mainly including administrative tasks. In addition, dentistry, pharmacy, and humanities are excluded due to weak first stages, caused either by few observations, or by a large supply of undersubscribed programs. Table 2 lists all included and excluded broad fields, while Appendix Table C.1 reports corresponding statistics for narrow fields. All dropped fields are however still included as counterfactual fields. The table also reports key summary statistics by field. We see substantial variation in both the number of observations and the size of the first stage coefficients. Both these factors influence the relative importance 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 broad field of study

	Included in sample	N	Share women	Share enrolled below cutoff	First stage (parent enrolls)
Teaching	Yes	17 590	64.0%	27.7%	28.3p.p.***
Humanities	No	2734	74.9%	21.1%	13.0p.p.*
Business	Yes	21 267	51.3%	32.3%	22.1p.p.***
Law	Yes	7285	59.6%	21.6%	18.2p.p.***
Social science	Yes	13 549	65.9%	17.2%	26.2p.p.***
Natural science	Yes	6541	51.0%	29.6%	13.4p.p.***
Computer science	No	6344	38.7%	21.7%	23.5p.p.***
Engineering	Yes	17 301	39.4%	37.2%	22.7p.p.***
Agriculture	Yes	5322	51.3%	23.5%	35.6p.p.***
Pharmacy	No	2611	83.2%	19.1%	15.7p.p.*
Medicine	Yes	10 932	45.8%	36.8%	26.4p.p.***
Health	No	2166	79.5%	22.8%	20.9p.p.*
Social work	Yes	9934	76.6%	21.4%	18.4p.p.***
Dentistry	No	3206	51.0%	34.2%	9.9p.p.*
Services	No	3744	80.9%	7.3%	12.2p.p.**

Notes: The table shows summary statistics by broad fields for the main sample of analysis. The first column indicates which fields are included in the analysis. 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.

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

In Appendix Section D, I study additional treatment margins and collapse the individual rankings by, STEM status, institutions, commuting zones, and institution-field combinations. I also confirm that there is no effect on the extensive margin of parental university enrollment on the likelihood that the child is educated at the university.

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 parental actions. 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. More broadly, social stratification can lead to different social groups following different sets of norms that influence the admissible career paths of group members, producing preferences that are correlated while not being the direct consequence of parental choices. 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 precisely manipulable around the cutoff, RDD coefficients can be interpreted as causal effects.

I use the RDD 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, using centered and standardized scores to ensure running variables are comparable.

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 (standardized) running variable $a_{p\tau} = a_{pg} - \bar{a}_{\tau}$ with the help of linear functions $f\left(a_{p\tau}; \theta^{g}\right) = \theta_0^{g} a_{p\tau} + \theta_1^{g} a_{p\tau} \mathbb{1}\left[a_{p\tau} \geq 0\right]$, estimated separately for each admission group g above and below the cutoff. Since the shape of the score distributions varies in each admission group, separately estimating the running variable slope of each admission group likely reduces bias. With 13 admission groups in the main sample of analysis, a total of 26 lines are included. Estimating these functions at the admission group level—rather than separately for each cutoff—still requires assuming unchanging relationships between scores and outcomes across cutoffs within the same admission group. This assumption is relaxed in Table A.2, where separate slopes are estimated for each cutoff.

μ_{τ} are cutoff fixed effects, and κ_{k} fixed effects for the next-best alternative k. In total, the main regression controls for 10 965 cutoffs and 15 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.

The reduced form in Equation 1 estimates an intent to treat parameter, but far from all parents turn out to be treated with the field of study they apply to. It is more interesting to understand the effect of actually studying or graduating from a specific field. To get at these concepts, I employ a fuzzy design and use threshold-crossing as an instrument:

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

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

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

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. However, as we shall see below, the further in time we get from instrument activation, the less likely it is that the exclusion restriction holds. 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.

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 for enrollment. One important channel to consider is how threshold-crossing changes the timing of education, and, in turn, later important events. Eager applicants below the cutoff are often always-takers, and 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 Table 4 shows no such relationships. To account for reapplication, I count field enrollment and degree completion which happen within 5 and 8 years respectively. This ensures always- and never-takers are correctly identified and should alleviate concerns that reapplication invalidates 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, thus voiding exclusion. This threat grows stronger the further in time we get from threshold-crossing, 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, applicants have no incentive to defy treatment, ensuring that the monotonicity assumption likely to hold.

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 (individuals are choosing between many fields of study). 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.

16. Truncation makes it rational for the applicant to add a safe option to the end of their priority ranking. Thankfully, only 3.6% of applicants submit a full list with 12 ranked alternatives. In addition, the priority based tie-breaking exactly at the cutoff 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.

For degree completion, it is however likely that admitted applicants who do not complete their studies are more inclined to graduate from a related field. For example, someone who almost finishes business degree can likely count most of their credits towards a degree in social science.[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. A related argument, applied multi-cutoff RD settings like the one used in this paper, is presented by Fort et al. (2022), who show that cutoff fixed effects are required to estimate ATEs.

To summarize, while it should be safe to interpret 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.

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, and degree completion. All results tables in the paper report first stage Wald statistics, which are far above conventional weak instrument thresholds.

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 controls can be fitted 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 2 standard deviation away from the cutoff. Since the results are weighted averages of a large set of cutoffs, traditional optimal bandwidth calculations are not informative.[18] Figure 7 shows that the results are robust to the choice of bandwidth size. I use 2 standard deviations because it gives me enough power to run some disaggregated analyses. But as shown in Figure 7, this choice matters little for the magnitude of aggregate point estimates. Furthermore, observations are weighted using a triangular kernel, giving linearly decreasing weights to observations farther away from the cutoff.

A first validation of the data can be seen in the balance table of Table 3. Here, threshold-crossing is regressed on variables that are all defined before treatment. In addition, the regression includes the same fixed-effects and running variable controls 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.

17. It should be noted, however, 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. I have included IV-estimates for degree completion in most results, leaving it to the reader to judge the validity of the required assumptions.

18. Calonico et al. (2014) optimal bandwidths, calculated on the pooled and cutoff-demeaned data, range from 0.8 to 2, depending on the specification.

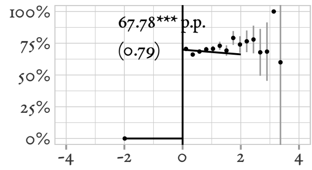
Table 3. Covariate balance

	Separately estimated	Joint model
Parent female	−0.001	−0.001
	(0.004)	(0.004)
Parent age	−0.001	−0.002
	(0.001)	(0.001)
Parent born outside of Sweden	−0.004	−0.011
	(0.010)	(0.016)
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)	(0.001)
Both grandparents born outside of Sweden	−0.004	−0.004
	(0.008)	(0.011)
Grandparent earnings pt	−0.004	−0.008
	(0.009)	(0.010)
Uni. educated grandparents	0.004	0.006
	(0.004)	(0.004)
Grandparent degree in j	−0.003	−0.006
	(0.007)	(0.007)
Cognitive skills	−0.004	
	(0.002)	
Non-cognitive skills	0.000	
	(0.002)	
Observations		107 665
Wald statistic		0.669
		[p=0.738]

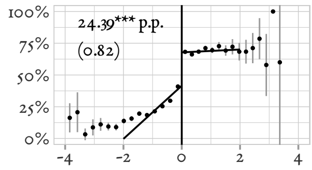
Notes: The table shows coviariate balance tests for a number of parent characteristics that are defined before treatment. The left column reports coefficients from regressions where being above the cutoff is regressed on each covariate separately, while the right column reports results from joint estimation. 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.

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

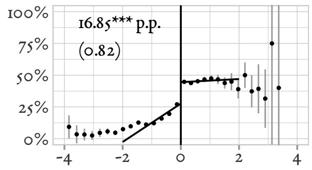
Figure 2. Treatment take up around the cutoff for broad (top) and narrow (bottom) fields



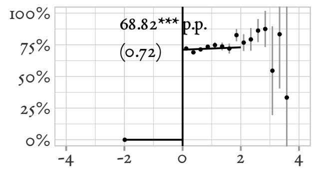
(a) Admitted



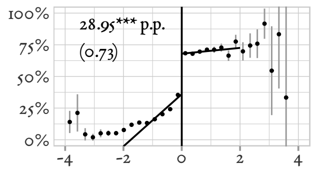
(b) Enrolled



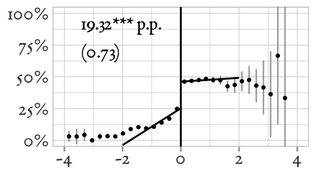
(c) Degree



(d) Admitted



(e) Enrolled



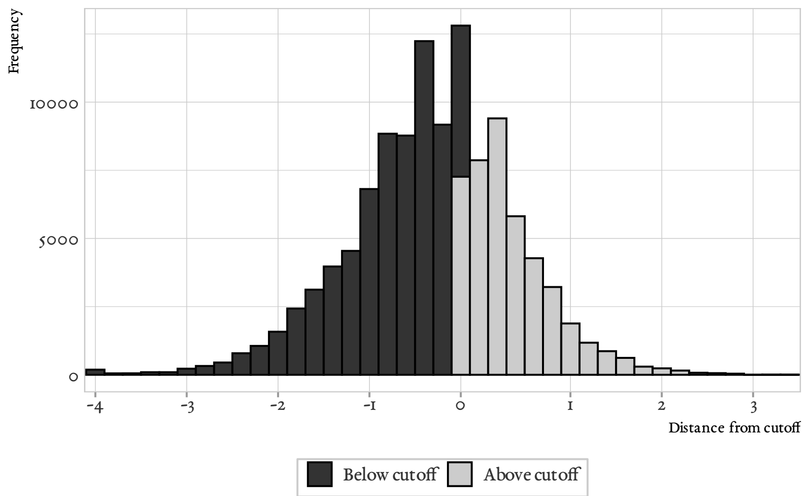
(f) Degree

Notes: The plot shows admission, enrollment, degree completion in the preferred field *j* above and below the cutoff. The top three plots show broad fields, and the bottom three narrow fields. Admission score is standardized by 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. All parents from the main study sample are included, but observations are not repeated for each child. First stage coefficients from Table B.1 are reported in percentage points within each plot, with standard errors in parentheses. The reason threshold-crossing does not increase admission by more than 67 percentage points is mainly because some individuals who would have been admitted withdraw their application after the first round.

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. 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.4 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.

Finally, Figure 4 provides confirmation that treatment does not influence key post-treatment outcomes. Here, threshold-crossing is regressed on variables related to the timing of treatment, fertility and child university application. The main study sample includes children born before 2002, ensuring that they all have had a chance to apply to university. If treatment had an effect on fertility, this conditioning would introduce bias. Thankfully, Figure 4 shows that the treatment only has an effect on the timing of enrollment and degree completion, but that fertility, or the probability to be included in the study sample (row 4) does not change.

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 indicates whether the tie-breaking mechanisms predicts they will be admitted or not.

Table 4. The effect of threshold-crossing on fertility

	Separately estimated	Joint model
Age at first enrollment	-0.004^{***}	-0.006^{***}
	(0.000)	(0.001)
Age at first degree	-0.001^{***}	-0.001^{*}
	(0.000)	(0.000)
Parent has at least one child	0.002	
	(0.003)	
Child born before 2002	0.001	0.005
	(0.003)	(0.004)
Number of children	0.000	−0.002
	(0.001)	(0.003)
Age at first child	0.000	0.000
	(0.000)	(0.000)
Age at first job	−0.001	−0.001
	(0.001)	(0.001)
Child applies to university	−0.001	−0.006
	(0.002)	(0.004)
Observations		118 353
Wald statistic		9.93
		[p=0]

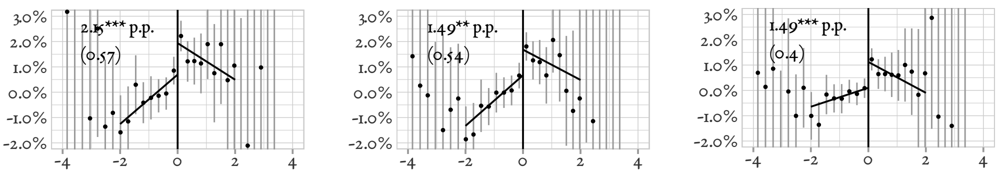
Notes: The table shows balance tests for characteristics defined after treatment. The left column reports coefficients from regressions where being above the cutoff is regressed on each covariate separately, while the right column reports results from a joint estimation. The regressions are run on the full sample of applicants within the bandwidth, including those with no children.

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

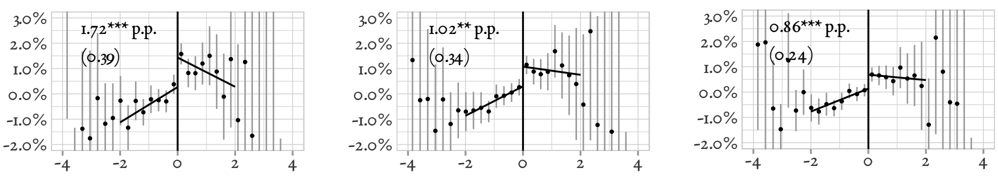
4 Main results

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. Inside each plot, the reduced form regression coefficients from Table 5 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



(a) Ranks 1st (b) Enrolls (c) Earns degree



(d) Ranks 1st (e) Enrolls (f) Earns degree

Notes: The plots shows the demeaned share of children following their parents above and below the cutoff. The outcome variables have been demeaned using the same set of fixed effects as in the main specification. Slopes of the running variable, fitted with a triangular kernel on observations within two standard deviations 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 5 are reported. These are fitted allowing the slope of the running variable to vary also for each admission group.

Table 5 further includes estimates from IV specifications. Parental enrollment increases the likelihood that a child will earn a degree in the same field by approximately 73% or 6.1 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 105% or 8.85 percentage points. For narrow fields, the corresponding numbers are almost identical, at 80% (3.0p.p.) and 120% (4.5p.p.) respectively. The relative effects on child enrollment are somewhat smaller at 35% (6.1p.p.) and 50% (8.8p.p.) for broad fields, as well as 51% (3.5p.p.) and 76% (5.3p.p.) for narrow fields. Finally, the relative estimates for ranking the field first are about the same as those for enrollment, at 41% (2.2p.p.) and 60% (12.7p.p.) for broad fields and 58% (6.0p.p.) and 87% (8.9p.p.) for narrow fields. While these aggregate effect are large, they are substantially smaller than many of the correlations displayed in Figure 1 and Table B.2. At the aggregate level, children are 173% as likely to have a degree in a certain field if one of their parents has one. The corresponding relative causal effect is of 73%, less than half of the correlation.

Table 5. Inheritance of fields of study

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	2.15***	1.49**	1.49***	1.72***	1.02**	0.86***
	(0.57)	(0.54)	(0.40)	(0.39)	(0.34)	(0.24)
Parent enrolls in j	8.80***	6.09**	6.11***	5.96***	3.53**	2.97***
	(2.33)	(2.20)	(1.61)	(1.34)	(1.19)	(0.82)
Parent receives degree in j	12.73***	8.82**	8.85***	8.93***	5.29**	4.45***
	(3.36)	(3.18)	(2.33)	(2.02)	(1.78)	(1.23)
Observations	109 721	109 721	109 721	141 882	141 882	141 882
Control group mean	21.15%	17.48%	8.35%	10.27%	6.96%	3.7%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald (enrolls)	893	893	893	1566	1566	1566
1st stage Wald (degree)	421	421	421	706	706	706

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.

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

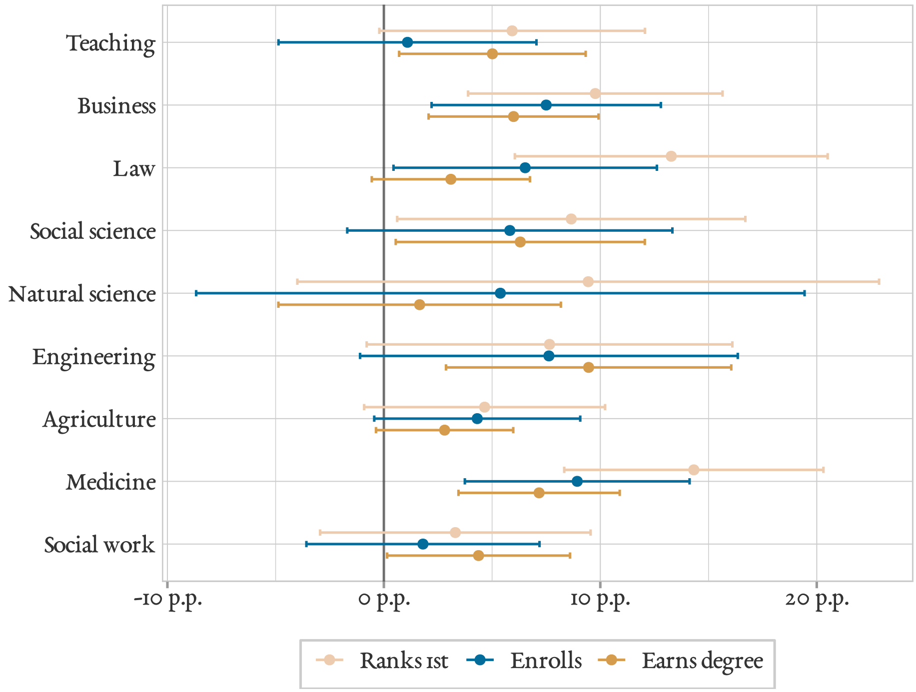
The aggregate effects are weighted averages of heterogeneous treatment effects for many fields of study. Figure 5 displays a coefficient plot of the field-level IV estimates. The fields are sorted by how they map to narrow fields, to make the figure easily comparable to Appendix Figure C.2 which plots the corresponding estimates for narrow fields.

Several interesting patterns can be seen in this graph. First, the effect of parental enrollment varies by field. For degree completion, in decreasing order, engineering, medicine, social science, and business exhibit the largest effects. The effects for teaching and social work are also significant, and only slightly smaller. Looking at the proximate outcomes—ranks first and enrollment—presents similar patterns, except that the effect of law is strongly decreasing from application to degree. Importantly, not a single estimate is negative. When compared to the control group means displayed in Table B.2, we see that all relative effects are substantially smaller than the corresponding associations reported in Figure 1.

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 engineering is harder to discern. The reason is likely that in this specification, most of these applicants end up at margins between different engineering subfields, reducing the impact of treatment. While the plot reports a few negative estimates, none are significant and most are very small.

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. On the diagonal we see the familiar pattern of children following their parents. But the figure also presents some, albeit noisy, interesting off-diagonal findings. We see that parents enrolling in medicine make children more likely to graduate

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

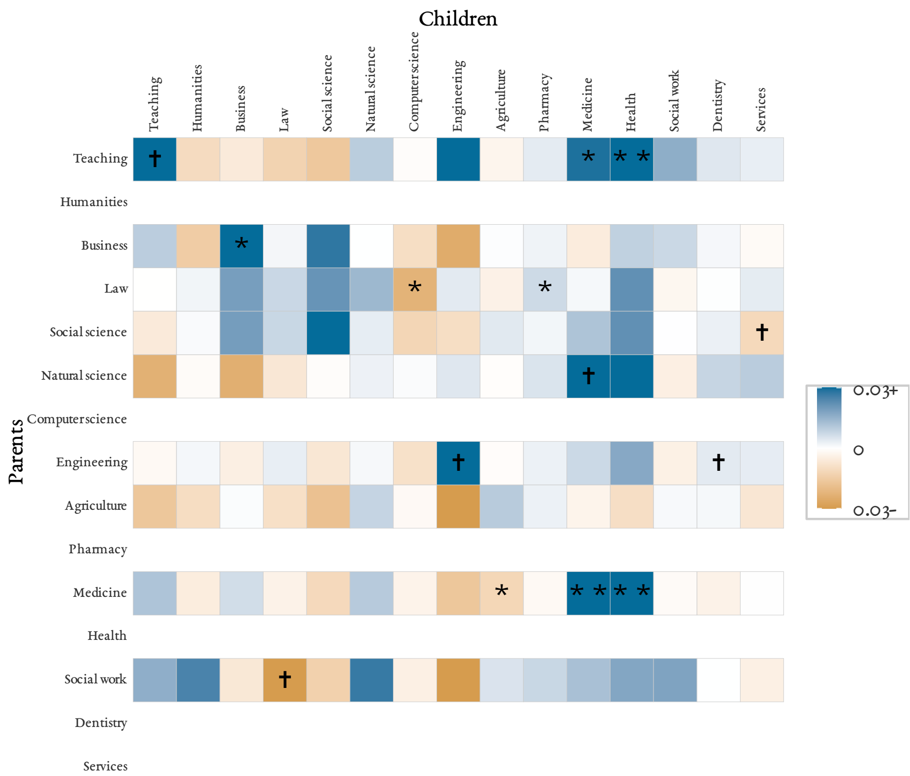


Notes: The figure reports coefficients of parental enrollment on child application, enrollment, and degree completion using the same specification as in Table 5 but with a separate coefficient estimated for each preferred field j. The exact coefficients for degree completion are reported in Table B.2. Corresponding results for narrow fields are reported in Appendix Figure C.2 and Table C.2.

also with a degree in health. Because admission to a medical program requires near-perfect grades, the effect on health is likely in part from children who would like to study medicine but are not able to. This pattern can be discerned at the aggregate level too. The top left and bottom right quadrant have lots of cells in a blue shade. This seems to be true for the top right quadrant too, but not for the bottom left. In other words, parents enrolling in fields related to social science seem to exert a positive influence on the likelihood that children study STEM subjects, but not vice versa. The plot shows that parental enrollment in STEM and health related subjects decreases child degree completion in the social sciences and humanities. As such, the plot points to another important distinction: parental influence seems to mainly operate through horizontal rather than vertical preferences. It is the field of study that is inherited, not the field's social status. For example, children of medical students are more likely to become nurses, but less likely to earn a degree in high-status fields like law and engineering.

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

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 different field degrees. Estimation is done using the same setup as in Table 5. The colors are capped at effect sizes between -3 and 3 percentage points, since outliers (usually field combinations with almost no observations) would otherwise cause most cells to be indistinguishable from zero. Significance levels are not corrected for multiple comparisons. $+\,p \leq 0.1$, $*\,p \leq 0.05$, $**\,p \leq 0.01$, $***\,p \leq 0.001$.

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 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 6. 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 6 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 6. Placebo (parent outcomes regressed on child admission)

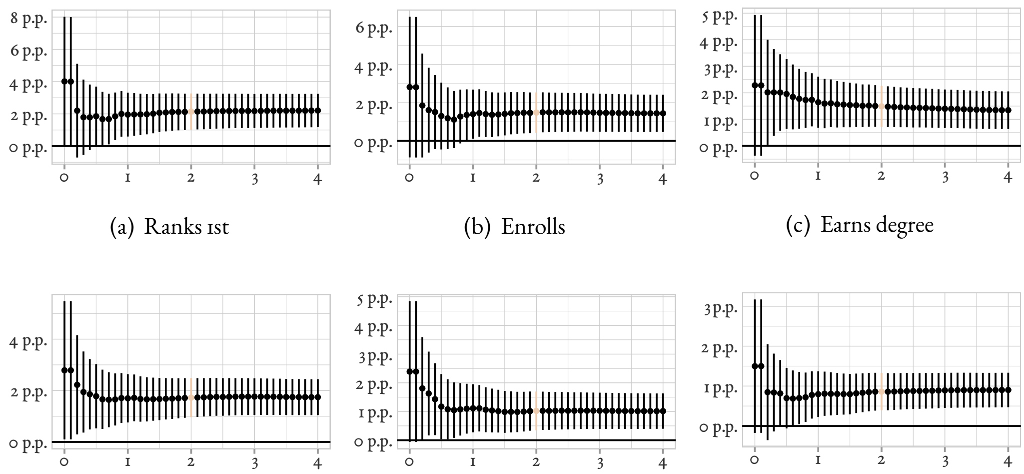
	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Child above cutoff	−0.05	−0.14	−0.06	−0.33	−0.26	−0.02
	(0.37)	(0.39)	(0.35)	(0.28)	(0.27)	(0.27)
Child enrolls	−0.21	−0.59	−0.24	−1.04	−0.81	−0.06
	(1.54)	(1.60)	(1.46)	(0.89)	(0.86)	(0.84)
Child receives degree	−0.30	−0.86	−0.35	−1.80	−1.41	−0.11
	(2.24)	(2.33)	(2.12)	(1.54)	(1.48)	(1.44)
Observations	154 557	154 557	154 557	169 771	169 771	169 771
Control group mean	12.6%	12.76%	8.87%	8.58%	6.77%	6.13%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald (enrolls)	1031	1031	1031	2178	2178	2178
1st stage Wald (degree)	473	473	473	803	803	803

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.

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

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 2 standard deviations for all analyses (marked in a lighter color in the plot). The figure shows little variation in the size of the effects as the bandwidth changes, except for very small bandwidths.

Figure 7. Reduced form results by bandwidth size



(a) Ranks 1st (b) Enrolls (c) Earns degree

(d) Ranks 1st (e) Enrolls (f) Earns degree

Notes: Each plot shows the main reduced form effect of a parent being above the cutoff on their child's first ranked 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 2 standard deviations for all analyses, marked in a lighter color in the plot.

Additional robustness and validity checks are performed in Appendix section A. 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 instead adds separate linear fits for each cutoff. While highly imprecise and fraught with convergence problems in estimation, most results stay economically meaningful in this specification. 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 slightly less precise. Finally, Table A.4 shows that the results stay approximately the same when applicants exactly at the cutoff are removed.

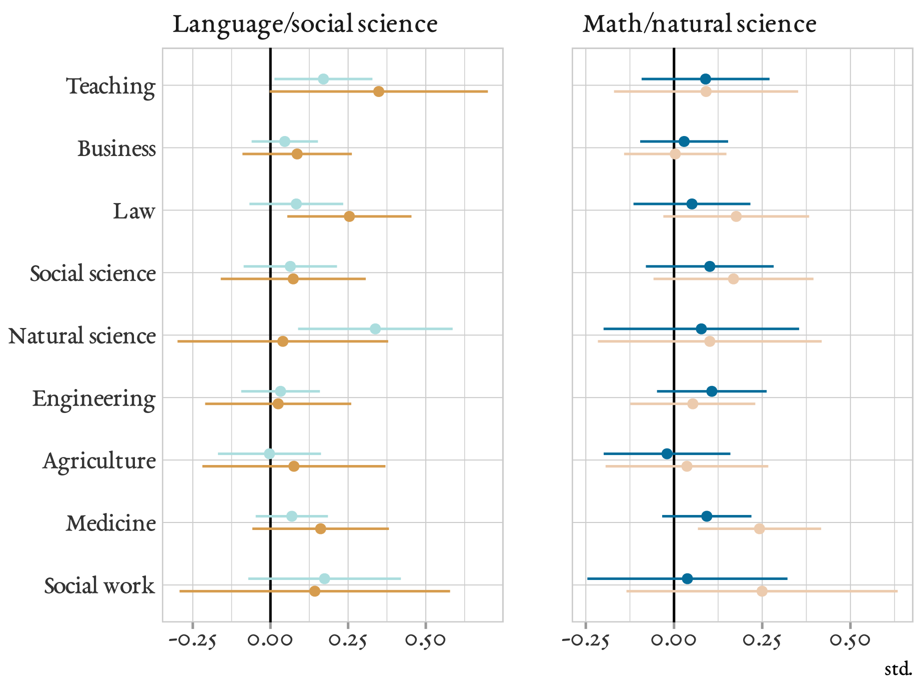
6 Exploring mechanisms

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 discuss two potential reasons why children follow their parents. First, I look for evidence that children gain some kind of comparative advantage by studying the same field as their parents. Second, I evaluate to what extent the parent acts as a role model, providing information and making some fields relatively more salient.

6.1 Does following give children a comparative advantage?

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 the occupational choices of parents. There are few other pathways through which the treatment effect could persist for so long, and while few occupations require licenses, a university diploma is still often the most effective way to gain entry.[19] The strong connection between university fields and occupations makes the literature on occupational inheritance a natural place to look for plausible mechanisms. Economic theories of occupational inheritance often underscore the possibility of gaining a comparative advantage as rationale for why children follow their parents. As discussed in the introduction, comparative advantage can be gained by human capital transfers or through nepotism.

Figure 8. Parental field enrollment and child test scores



Notes: The figure shows the effect of parental field enrollment on child primary school, subject-level, test scores. Each score is an average of the 9th-grade tests in the subject that the student has taken, standardized at the test-cohort level. The left plot shows language in teal and social science in orange, while the right plot shows math in blue and natural science in pink. The estimation strategy follows the same approach as Table 5. Coefficients and standard errors are reported in Table B.3.

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.

We begin this section by studying human capital transfers directly, by looking at the intergenerational transfer of field-specific knowledge. Figure 8 presents RDD estimates of parental field enrollment on child primary school test scores. The results do not show any clear subject-specific transmission. The fields at the top, all in the humanities or social sciences do not improve child skills in the left plot more than in the right plot. Nor can the corresponding thing be said for the lower, natural science-related fields, and the right plot. Rather, some fields—like teaching, law, and medicine—have sizeable, but rarely significant, point estimates in both plots. One of the largest effect is that of natural science on test scores in language. Appendix Table B.4 looks specifically at parents on the margin between STEM and non-STEM fields with similar results. In other words, I find no evidence of subject-level human capital transfers that award the next generation with a comparative advantage.

Test scores from primary school is an imperfect proxy for subject-specific human capital, however. The skills transferred from parents could be specific to their occupation, or perhaps at a higher level than whats tested in a ninth grade exam. In the second analysis, we study comparative advantage directly by looking at labor market returns and parental degrees. Here, higher returns could be caused by nepotism, where parents secure benefits for their offspring instead of higher qualified candidates, or by improvements in subject-specific human capital not captured by the previous analysis. Figure 9 reports RD-results of field enrollment on labor-market returns by the degree of parents. Not that this analysis is run on a different sample, it is the enrollment of children that is quasi-random here, not the degree of the parents.[20] The plot to the left shows returns to total work-related earnings, while the plot to the right presents field-returns to income from self-employment. At the bottom, the pooled estimate shows an increase in earnings of 34 000 SEK from enrolling in a preferred field for those who do not have a parent with a degree in that field, decreasing to 22 000 SEK for those who do. This small positive return to earnings at the aggregate level is evidence that individuals weakly sort into fields by comparative advantage. By field, we observe an increase for medicine, law, agriculture, and social work, but none of the differences by parental degree are significant. Returns to self-employment income from studying medicine is the only significant interaction, confirming the pattern Ventura (2023) observed in the Netherlands. While not significant, the differential return to earnings for medicine is identical to the 23% difference reported by Ventura. Overall, however, the figure shows little support for the theory that children of degree-holders have a comparative advantage when following their parents.

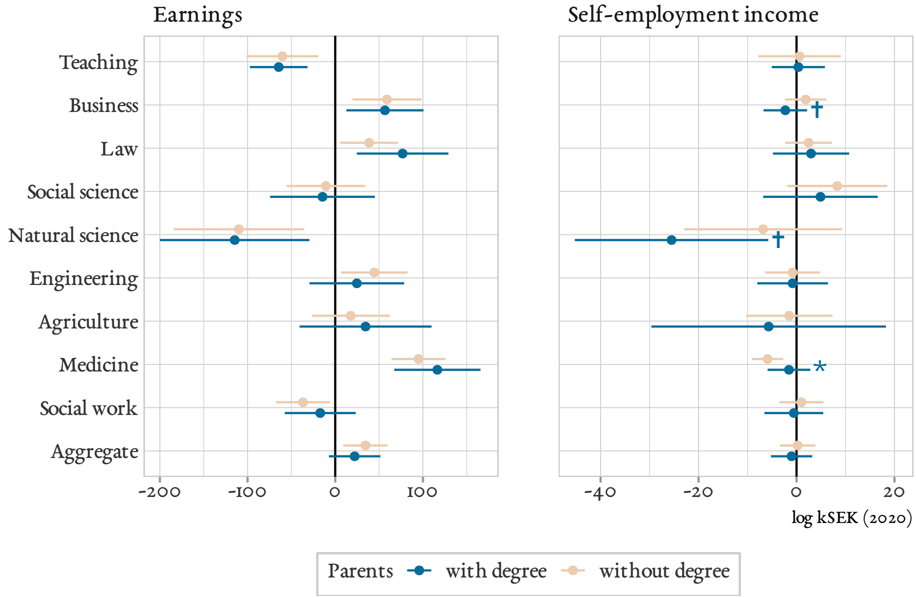
6.2 The parent as a role model

If children do not gain any meaningful labor market advantage from following their parents, why do they do it? A plausible alternative mechanism—that seems to motivate individuals in other settings—is that parents act as role models and provide field-specific information. Role models have been shown to strongly influence education choices (Breda et al. 2023), and the sibling spillover effects in Altmejd et al. (2021) can likely be attributed to a similar mechanism. Moreover, Dahl et al. (2023) find mothers to mainly influence their daughters in fields that are gender-incongruent.

A role model mechanism of this type can be deduced from a simple theoretical framework. In papers like Altonji et al. (2016) and Proctor (2022), field of study choice is modelled as a dynamic choice problem where individuals learn about their field-specific ability. Proctor (2022) specifically lets children form priors about their own ability based on parental signals. Children learn more about their field-specific ability in the fields that their parent has studied. Choosing the same field then becomes a relatively safe alternative, with a lower risk of a bad ability draw and subsequent switching costs.

20. There are not nearly enough cases where both parent and child are quasi-randomly admitted.

Figure 9. Labor market returns by parental field degree



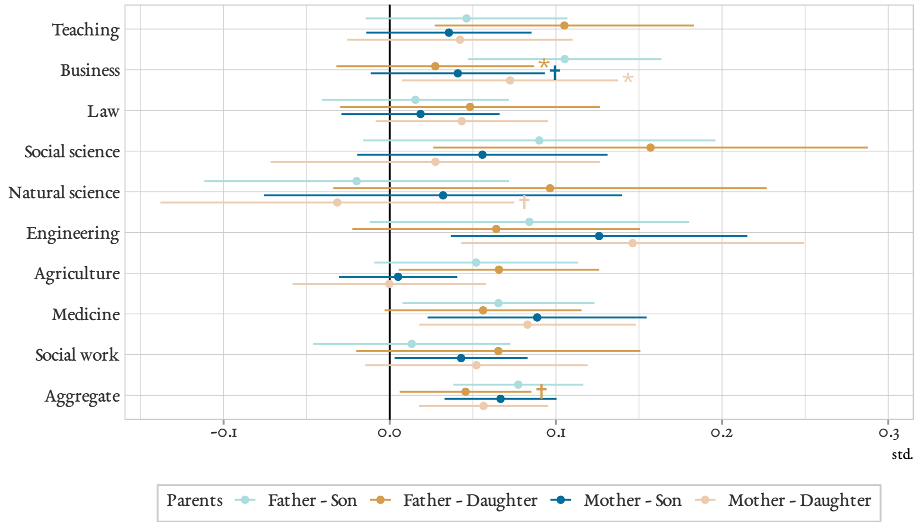
Notes: The figure reports the causal returns to earnings and self-employment income from different fields of study depending on if the applicant has a parent with a degree from the field or not. The sample studied is slightly different from the one used in the main analysis. It includes applications submitted between 1977 and 2010, filtered in the same way as the main study sample. In all other regards, the estimation strategy follows the same approach as Table 5. Enrollment is interacted with an indicator variable for whether at least one of the parents has a degree in field j. The blue estimates are the sum of the base and interaction terms, while significance labels refer to the interaction (a test of difference). Earnings are total labor market earnings, including all taxable income like self-employment, parental leave, and unemployment insurance, but excluding capital income. Both are measured as 5-year averages 10-14 years after treatment, in thousands of 2020 SEK. Coefficients and standard errors are reported in Table B.5.

One way to study role-model effects is to look at field inheritance by gender. Research on educational role models often highlight the importance of self-identification (see e.g., Breda et al. 2023). Figure 10 plots estimates of parental enrollment on child degree completion separately for each parent-child gender combination. Again, the final row reports pooled estimates. Like in Dahl et al. (2023) 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, and the differences between paternal and maternal influence is rarely significant. Stronger same-gender influence speaks to the role-model mechanism. So does the fact that for the three fields most fraught with gender stereotypes—teaching, social work, and engineering—parental influence is stronger when a parent enrolls in a field that is not congruent with their gender.

Building on the results of Altmejd et al. (2021), parental influence should be the strongest for the firstborn child, who does not have any siblings to follow. Table 7 shows exactly this, reporting regression results by the birth order of each child. We see a clear decreasing effect by birth order, with a substantially stronger influence on the firstborn.

A possible explanation for why the effect is often 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

Figure 10. Field inheritance and gender composition



Notes: The figure shows the main effect but split by the gender of parent and child. Significance labels indicate significant differences to the father-son effect. Coefficients and standard errors are reported in Table B.6.

graduated from. A concern could be that the effect we identified for mothers, mainly works through assortative mating. Indeed, Appendix Table B.7 reports that enrolling in a field makes mothers twice as likely as fathers to partner with someone with a degree from that field. However, for most specifications, Table B.8 shows that mothers who partner with someone with the same degree are not more likely to transmit that field than fathers.

A final tables about the role of the family are presented in the Appendix. Table B.9 displays the aggregate field inheritance effect by the education level of the grandparents, with no clear differences.

6.3 Labor market experience

While we have seen little support for comparative advantage as a major driver of field inheritance, the results seem to substantiate the notion of parents as role models. But while siblings are only a few years apart, parents are treated on average 30 years before their children. It is unlikely that parental enrollment would have a direct effect on the education choices of children. As discussed in the introduction, parental occupation is a likely mediator. Table 8 provides strong support for this claim. Here, I have interacted parental enrollment with the full population predicted earnings-by-cohort percentile of the parent 10-14 years after treatment, using a range of predetermined characteristics to predict earnings from enrolling in j.

In the RDD sample, the predicted earnings percentile ranges from 0.3 and 0.9. Across the six interaction coefficients, the average difference in child following going from the lowest to the highest predicted earnings percentile in the sample is 13 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. Depending on the specification, parents need to be above the 37th to 61st percentile of predicted cohort earnings to have a positive influence, corresponding to between the 1st and 25th percentile in the sample.

Table 7. Field inheritance by child birth order

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	6.23^{**}	5.17^{**}	0.38	3.95^{***}	2.27^{**}	−0.10
	(2.00)	(1.71)	(0.59)	(1.06)	(0.83)	(0.31)
× Second-born child	-4.22^{***}	-3.25^{***}	$-0.70^{\dagger}$	-2.48^{***}	-1.91^{***}	−0.23
	(1.14)	(0.97)	(0.37)	(0.66)	(0.52)	(0.20)
× Third-born child	-4.31^{**}	-3.28^{**}	-1.07^{*}	-2.30^{**}	-1.78^{**}	−0.06
	(1.39)	(1.22)	(0.44)	(0.76)	(0.62)	(0.21)
× ≥ Fourth-born child	-6.27^{**}	−1.49	$-1.38^{\dagger}$	-3.45^{**}	-1.83^{*}	−0.01
	(2.04)	(1.81)	(0.71)	(1.11)	(0.81)	(0.41)
Second-born child	-3.21^{***}	-2.92^{***}	-0.79^{***}	-1.65^{***}	-1.31^{***}	-0.43^{***}
	(0.59)	(0.50)	(0.19)	(0.33)	(0.26)	(0.11)
Third-born child	-6.48^{***}	-5.52^{***}	-0.99^{***}	-3.23^{***}	-2.26^{***}	-0.69^{***}
	(0.73)	(0.62)	(0.22)	(0.38)	(0.29)	(0.11)
Fourth-born child	-7.27^{***}	-7.51^{***}	-1.34^{***}	-3.39^{***}	-2.72^{***}	-0.79^{***}
	(1.11)	(0.95)	(0.34)	(0.60)	(0.40)	(0.16)
Observations	103 777	103 777	103 777	134 351	134 351	134 351
Control group mean	8.96%	6.48%	0.93%	4.32%	2.55%	0.44%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald	119	119	119	203	203	203

Notes: The estimation excludes families with only one child. In addition, to not give earlier-born children a longer time (before measurement ends in 2023) to follow their parents, the outcome variables have been redefined to include only cases where the child follows within 30 years of treatment. This is why the estimates on degree completion are so small. The reference group includes firstborn children. Otherwise, the estimation follows the same approach as Table 5.

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

The table illustrates that children often avoid entering fields where their parents are predicted to experience relatively poor labor market outcomes. This observation underscores the role of occupational experience as a mediator, but further challenges comparative advantage as a driver. If children's career choices were solely influenced by comparative advantage, we would not observe any negative influence. That we do points to the relevance of alternative mechanisms.

An additional example of the importance of occupation as a mediator can be found in appendix Table B.10, where treatment is interacted with the age of the parent. While the effects are imprecise, there is a strong negative effect on inheritance among parents who have reached the retirement age of 65.

The Appendix contains two sections of additional analyses. 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 on other margins than fields of study, focusing on how institutions are inherited. Table D.1 reports coefficients that are similar in size to the main results, and Table D.2 shows that much of this is in fact a result of location persistence, where children become more likely to graduate from any institution within the same commuting zone. Table D.3 looks at the likelihood for a child to follow their parent to the same field-institution combination. The second part of this table shows that also when holding the institution constant, children still follow their parent to the same field. The relative effects are actually somewhat larger than the main results, indicating that the main results are not driven by inheritance of institutions. Last, Table D.4 finds no effect on the extensive margin. That is, in this

Table 8. 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	-18.20^{*}	-25.13^{***}	-7.88	$-8.58^{\dagger}$	-8.81^{*}	-2.74
	(7.84)	(7.48)	(5.27)	(4.50)	(3.88)	(2.69)
× Predicted earnings (10-14 years, pt.)	34.56^{***}	41.17^{***}	18.61^{**}	18.27^{**}	16.05^{**}	7.22^{*}
	(10.47)	(10.06)	(7.09)	(5.76)	(5.02)	(3.38)
Predicted earnings (10-14 years, pt.)	$17.92^{\dagger}$	37.54^{***}	15.88^{*}	$12.98^{\dagger}$	22.85^{***}	9.58^{*}
	(10.29)	(9.50)	(6.80)	(7.03)	(6.23)	(4.31)
Observations	97 727	97 727	97 727	127 720	127 720	127 720
Control group mean	21.36%	17.77%	8.48%	10.45%	7.19%	3.74%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald	250	250	250	459	459	459

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 5.

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

sample, parental enrollment in any college education (vs the conterfactual of not enrolling) does not increase the likelihood the child will go to university.

7 Conclusion

Children are two to five times more likely than average to graduate from a field that their parents have studied. This well-known pattern of intergenerational correlation of educational preferences could be a direct consequence of parental choices, or explained by other factors like social stratification or genes. In this paper, I exploited a quasi-experimental statistical design to investigate how much of this association is causal.

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 6.1 percentage points or 73% (3.0p.p. or 80% 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 placebo checks.

Dissecting these results into heterogeneous effects by field of study shows that few fields see negative parental influence, but some fields are inherited more often than others. The most inherited fields often have the best labor market prospects. Parental enrollment increases child graduation probability in engineering with 9.5 percentage points (59%), but only with 1.65 percentage points (37%) in natural science. Some of these causal effects are similar to the relative differences. For example, children are 240% as likely to hold a degree in social work if their parent has one, and the relative causal effect is 203%. But other results are quite different. Agriculture degrees are 550% as common among children of graduates, but parental enrollment only increases graduation rates by 133%.[21]

These variable patterns are the results of a complex set of differences in educational and occupational experiences across fields. It takes on average 30 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 predicted earning, we saw that children are much more likely to follow those parents who are predicted to have a favorable labor market experience (Table 8).

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. We evaluated this claim from two perspectives. First, using primary school test scores in Figure 8, I showed that parental field enrollment does not lead to any subject-specific human capital transfers. In other words, it is not the case that parents who enroll in social science subjects generally have children with better grades in those subjects, or vice versa for natural science. The second test looks at labor market returns directly. Figure 9 shows no clear pattern of higher labor market returns for children with parents with a degree in the same field. For most fields, the differential returns to earnings and self-employment when comparing individuals with and without a parent with the same degree are small an insignificant. An important exception is medicine, where children with same-degree parents have significantly higher income from self-employment.

While there is little support for comparative advantage driving the inheritance of university fields, we did find some support for an alternative mechanism. Like siblings (Altmejd et al. 2021), parents seem to act as role models, providing information and increasing the salience of their choices. This hypothesis is supported by Figure 10, where the sample has been split by the gender of parents and children. Here we observed two facts, both in support of the role model mechanism: children are more likely to follow a parent with the same gender, and parents exert more influence when their field of choice is incongruent with the stereotypes of their gender.

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 are similar in size to previous correlational estimates. For other, the causal effects are very different. Many external elements, like social norms and dynastic traditions contribute to this spurious correlation of intergenerational education choices. This paper accounts for such factors and provides policy-relevant estimates of the direct intergenerational effect of parental choices.

We cannot change our genes, nor do we have any control over most of the factors that are grouped under the umbrella of "nurture". In this paper, I have identified an environmental factor that strongly influences educational choices and that can in fact 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 their children to succeed and who will benefit from better understanding their importance as 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.

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

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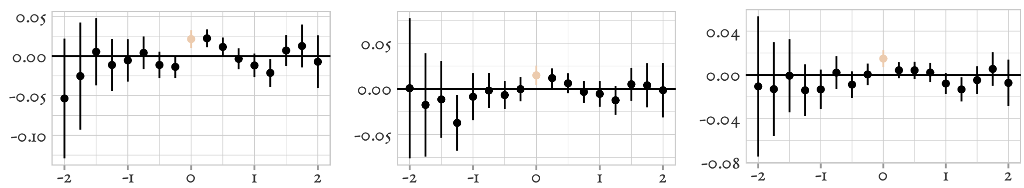
Appendix

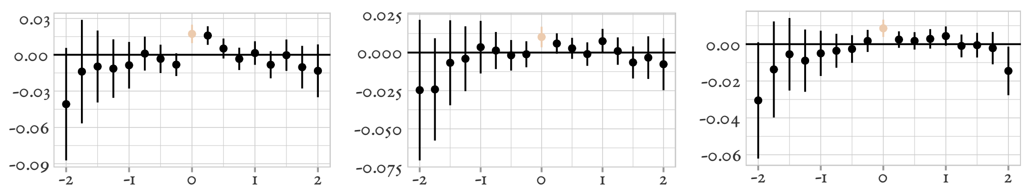
A Additional robustness checks

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, for most outcomes, 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 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



(a) Ranks 1st (b) Enrolls (c) Earns degree



(d) Ranks 1st (e) Enrolls (f) Earns degree

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 fitting quadratic rather than linear functions of the running variable. The effects are very close in size, but with somewhat larger standard errors.

Applicants select into fields, but also admission groups and programs within fields. This is why I include cutoff fixed effects in all specifications. Estimating separate lines for the running variable per admission group accounts for variation in scoring policies. However, it means the main specification is pooling variation across multiple different quasi-experiments when fitting these lines. Table A.2 instead fits separate lines below and above each of the approximately 11 000 cutoffs. This exercise is very taxing on statistical power, and the estimation suffers from convergence problems, but the reported point estimates are still large for all outcomes save enrollment.

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.

Table A.1. Quadratic polynomials

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to *j*	1.76*	1.24†	1.62**	1.64**	1.14*	0.86**
	(0.73)	(0.69)	(0.51)	(0.51)	(0.46)	(0.31)
Parent enrolls in *j*	8.90*	6.26†	8.18**	6.99**	4.86*	3.69**
	(3.69)	(3.51)	(2.59)	(2.19)	(1.95)	(1.32)
Parent receives degree in *j*	13.14*	9.24†	12.07**	10.81**	7.51*	5.71**
	(5.44)	(5.18)	(3.84)	(3.40)	(3.01)	(2.04)
Observations	109 721	109 721	109 721	141 882	141 882	141 882
Control group mean	21.15%	17.48%	8.35%	10.27%	6.96%	3.7%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald (enrolls)	366	366	366	657	657	657
1st stage Wald (degree)	164	164	164	279	279	279

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 5.

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

Table A.2. Separate slopes for each cutoff

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to *j*	1.09	0.16	0.98†	1.02†	0.24	0.60†
	(0.84)	(0.78)	(0.58)	(0.54)	(0.48)	(0.33)
Parent enrolls in *j*	4.83	0.70	4.34†	3.76†	0.88	2.19†
	(3.70)	(3.42)	(2.53)	(1.99)	(1.77)	(1.20)
Parent receives degree in *j*	7.07	1.03	6.36†	5.67†	1.33	3.31†
	(5.38)	(5.00)	(3.67)	(3.00)	(2.66)	(1.80)
Observations	109 721	109 721	109 721	141 882	141 882	141 882
Control group mean	21.15%	17.48%	8.35%	10.27%	6.96%	3.7%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald (enrolls)	371	371	371	788	788	788
1st stage Wald (degree)	169	169	169	343	343	343

Notes: The table shows the same results as in Table 5 but with distinct linear fits of the running variable above and below each cutoff.

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

First, 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. The exclusion of lower ranked options makes little difference for point estimates and only increases standard errors somewhat.

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	1.80**	1.31*	1.59***	1.85***	1.11**	0.96***
	(0.66)	(0.63)	(0.47)	(0.46)	(0.41)	(0.29)
Parent enrolls in j	6.99**	5.08*	6.19***	6.06***	3.62**	3.16***
	(2.55)	(2.45)	(1.80)	(1.50)	(1.35)	(0.93)
Parent receives degree in j	9.84**	7.16*	8.71***	8.86***	5.30**	4.62***
	(3.57)	(3.45)	(2.54)	(2.19)	(1.97)	(1.36)
Observations	81 585	81 585	81 585	103 503	103 503	103 503
Control group mean	21.36%	17.39%	8.53%	10.9%	7.34%	4.02%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald (enrolls)	785	785	785	1397	1397	1397
1st stage Wald (degree)	375	375	375	633	633	633

Notes: The sample includes observations 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.

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

Second, Table A.4 removes all applicants exactly at the cutoff from the analysis. In the main analysis, I use the predefined tie-breaking rules (including lottery numbers) to predict admission among applicants at the cutoff. The results in Table A.4 are very close to the main results.

Table A.4. Donut

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	2.47***	1.55*	1.37**	1.59**	0.79†	0.96***
	(0.73)	(0.67)	(0.48)	(0.50)	(0.43)	(0.29)
Parent enrolls in j	9.77***	6.13*	5.41**	5.15**	2.57†	3.10***
	(2.88)	(2.63)	(1.89)	(1.61)	(1.39)	(0.93)
Parent receives degree in j	14.29***	8.97*	7.91**	7.71**	3.85†	4.64***
	(4.20)	(3.85)	(2.75)	(2.42)	(2.08)	(1.40)
Observations	96 914	96 914	96 914	125 992	125 992	125 992
Control group mean	20.99%	17.32%	8.31%	10.16%	6.89%	3.67%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald (enrolls)	610	610	610	1188	1188	1188
1st stage Wald (degree)	294	294	294	527	527	527

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 5.

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

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 receives degree in j
Parent above cutoff to j	67.78***	24.39***	16.85***
	(0.79)	(0.82)	(0.82)
Observations	54 515	54 515	54 515
Control group mean	0%	28.47%	18.21%
Bandwidth	2.0	2.0	2.0

Notes: Observations are not repeated for each child. Otherwise, the estimation follows the same approach as Table 5.

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

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

Field	Relative popularity	Effect estimate		Control group mean	Relative effect
Teaching	147%	5.01p.p.*	(2.20)	5.62%	89%
Business	165%	5.99p.p.**	(2.00)	8.77%	68%
Law	299%	3.10p.p.†	(1.86)	2.68%	116%
Social science	121%	6.30p.p.*	(2.94)	16.71%	38%
Natural science	167%	1.65p.p.	(3.33)	4.51%	37%
Engineering	170%	9.46p.p.**	(3.36)	16.00%	59%
Agriculture	550%	2.81p.p.†	(1.62)	2.11%	133%
Medicine	282%	7.17p.p.***	(1.90)	6.10%	117%
Social work	240%	4.38p.p.*	(2.16)	2.16%	203%
Aggregate	173%	6.11p.p.***	(1.61)	8.35%	73%

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 5 but with separate coefficients for each field.

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

Following this, Tables B.3 to B.6 are companion tables to the figures in Section 6.

Next, Table B.7 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 much larger for mothers. Table B.8 runs the inheritance regression but with an interaction for if the partner's degree is in the same field. We see stronger inheritance for these cases, but no clear indication this mechanism is gendered. Of course, partner choice is affected by treatment and adding it to the right-hand side of the regression could introduce endogeneity. These results should be interpreted with caution.

Table B.3. Parental field enrollment and child test scores

Field	Languages		Social science		Math		Natural science	
Teaching	0.17*	(0.08)	0.35†	(0.18)	0.09	(0.09)	0.09	(0.13)
Business	0.05	(0.05)	0.09	(0.09)	0.03	(0.06)	0.00	(0.07)
Law	0.08	(0.08)	0.25*	(0.10)	0.05	(0.08)	0.18†	(0.11)
Social science	0.06	(0.08)	0.07	(0.12)	0.10	(0.09)	0.17	(0.12)
Natural science	0.34**	(0.13)	0.04	(0.17)	0.08	(0.14)	0.10	(0.16)
Engineering	0.03	(0.06)	0.02	(0.12)	0.11	(0.08)	0.05	(0.09)
Agriculture	0.00	(0.08)	0.08	(0.15)	−0.02	(0.09)	0.04	(0.12)
Medicine	0.07	(0.06)	0.16	(0.11)	0.09	(0.06)	0.24**	(0.09)
Social work	0.17	(0.13)	0.14	(0.22)	0.04	(0.14)	0.25	(0.20)
Aggregate	0.08†	(0.05)	0.10	(0.08)	0.07	(0.05)	0.09	(0.07)

Notes: Estimates displayed in Figure 8. Each column represents a regression of standardized 9th grade subject test scores on parental field enrollment. Test scores are standardized by test and cohort, and then averaged by the four categories above. The estimation strategy follows the same approach as Table 5.

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

Table B.4. Parental field enrollment and child test scores (STEM margin only)

is STEM	Languages		Social science		Math		Natural science	
FALSE	0.18	(0.20)	0.45	(0.41)	0.34	(0.24)	0.16	(0.30)
TRUE	0.14	(0.11)	0.02	(0.26)	0.16	(0.13)	0.03	(0.16)
Aggregate	0.16	(0.14)	0.28	(0.33)	0.23	(0.16)	0.09	(0.21)

Notes: See Table B.3 for notes.

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

Table B.5. Labor market returns by parental field degree

	Earnings				Self-employment income			
Field			× parent				× parent	
Teaching	−60.31**	(20.89)	−4.14	(17.33)	0.61	(4.29)	−0.23	(3.85)
Business	58.99**	(20.07)	−2.32	(26.54)	1.87	(2.16)	−4.16†	(2.35)
Law	38.69*	(16.87)	38.16	(26.63)	2.47	(2.45)	0.49	(3.76)
Social science	−10.72	(22.89)	−3.82	(26.02)	8.31	(5.21)	−3.43	(5.65)
Natural science	−109.84**	(37.96)	−4.84	(43.95)	−6.82	(8.21)	−18.69†	(11.14)
Engineering	44.52*	(19.35)	−19.87	(21.81)	−0.81	(2.88)	0.01	(3.20)
Agriculture	17.76	(22.70)	16.80	(40.64)	−1.49	(4.50)	−4.20	(11.85)
Medicine	94.98***	(15.70)	21.53	(26.48)	−5.91***	(1.63)	4.36*	(2.22)
Social work	−36.70*	(15.65)	19.57	(20.66)	0.97	(2.30)	−1.51	(3.28)
Aggregate	34.47**	(12.92)	−12.35	(9.05)	0.23	(1.85)	−1.24	(1.22)

Notes: Estimates displayed in Figure 9. See the figure notes for a description of the estimation strategy and sample.

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

Table B.6. Field inheritance and gender composition (interaction terms)

Field	Earns degree		Interactions × Daughter		× Mother		× Mother × Daughter	
Teaching	4.62	(3.10)	5.89	(4.07)	−1.06	(3.45)	−5.22	(5.30)
Business	10.53***	(2.96)	−7.79*	(3.68)	−6.43†	(3.44)	10.94*	(5.21)
Law	1.54	(2.87)	3.29	(4.28)	0.31	(3.37)	−0.81	(5.06)
Social science	9.00†	(5.41)	6.70	(7.73)	−3.42	(6.27)	−9.53	(9.32)
Natural science	−1.99	(4.68)	11.64	(7.29)	5.20	(6.95)	−18.01†	(9.53)
Engineering	8.40†	(4.90)	−2.00	(5.05)	4.20	(5.51)	4.01	(7.37)
Agriculture	5.20†	(3.13)	1.37	(3.59)	−4.69	(3.10)	−1.90	(4.41)
Medicine	6.54*	(2.95)	−0.93	(3.75)	2.33	(4.02)	0.35	(5.15)
Social work	1.32	(3.03)	5.21	(4.49)	2.97	(3.09)	−4.29	(5.47)
Aggregate	7.73***	(2.00)	−3.17†	(1.73)	−1.06	(1.66)	2.15	(2.25)

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 5. Figure 10 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.7. Assortative mating (first stage)

	Broad fields	Narrow fields
	Other parent has degree in j	Other parent has degree in j
Parent enrolls in j	9.31**	7.82***
	(2.87)	(1.68)
× Parent female	7.83***	10.58***
	(2.14)	(1.49)
Parent female	−0.94	−0.99
	(1.11)	(0.71)
Observations	109 721	141 882
Control group mean	11.22%	7.05%
Bandwidth	2.0	2.0
1st stage Wald	317	556

Notes: The table shows the likelihood that the other parent has a degree from field j is affected by whether the parent enrolls in j or not, and how this differs by the gender of the parent. It is a first stage of sorts for Table B.8. Otherwise, the estimation follows the same approach as Table 5.

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

Table B.8. Assortative mating

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	8.25**	5.45*	4.55**	5.34***	2.27†	2.06*
	(2.61)	(2.47)	(1.75)	(1.44)	(1.28)	(0.87)
× Parent female	−2.15	−1.65	−0.93	−1.19	0.24	0.33
	(1.98)	(1.86)	(1.34)	(1.17)	(1.01)	(0.72)
× Other parent has degree in j	5.98	8.27	10.63*	−0.57	3.56	2.78
	(5.76)	(5.40)	(4.22)	(4.88)	(4.36)	(3.10)
× Parent female × other parent has degree in j	−6.30	−11.64	−0.65	1.94	−2.42	0.09
	(7.65)	(7.20)	(5.49)	(7.05)	(6.26)	(4.69)
Parent female	0.72	0.83	0.79	−0.49	−0.68	−0.07
	(0.97)	(0.91)	(0.64)	(0.56)	(0.48)	(0.32)
Other parent has degree in j	3.65	2.00	−4.14	7.99*	3.95	1.35
	(4.59)	(4.28)	(3.30)	(3.99)	(3.52)	(2.44)
Parent female × other parent has degree in j	8.66	11.62*	3.13	0.87	3.43	1.15
	(5.88)	(5.49)	(4.18)	(5.67)	(4.99)	(3.72)
Observations	109 721	109 721	109 721	141 882	141 882	141 882
Control group mean	21.15%	17.48%	8.35%	10.27%	6.96%	3.7%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald	349	349	349	557	557	557

Notes: The table shows field inheritance by whether the partner of the parent has a degree in the same field and the gender of the parent. Otherwise, the estimation follows the same approach as Table 5.

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

Table B.9 reports the results split by the education level of the grandparents with little difference across the strata.

Table B.10 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 for most outcomes among parents who have reached the retirement age of 65.

Table B.9. Grandparents' educational level

	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in *j*	9.30**	6.69*	6.54**	5.77**	2.97†	1.99†
	(2.93)	(2.73)	(2.08)	(1.83)	(1.55)	(1.17)
× Grandparent high school	−2.51	−2.33	−1.27	−0.31	0.09	0.72
	(2.68)	(2.42)	(1.89)	(1.61)	(1.35)	(1.04)
× Grandparent post-secondary	−1.15	−0.72	−1.16	−1.10	−0.42	1.68
	(3.37)	(3.14)	(2.35)	(1.99)	(1.76)	(1.32)
× Grandparent tertiary	0.69	0.26	0.12	0.81	1.23	1.22
	(2.69)	(2.48)	(1.93)	(1.60)	(1.36)	(1.07)
Grandparent high school	−0.48	0.05	0.21	−0.27	−0.52	−0.39
	(1.32)	(1.17)	(0.89)	(0.77)	(0.64)	(0.48)
Grandparent post-secondary	0.25	0.46	1.11	0.62	0.01	−0.62
	(1.67)	(1.54)	(1.13)	(0.94)	(0.83)	(0.58)
Grandparent tertiary	−0.80	0.29	−0.30	0.01	−0.21	−0.78
	(1.36)	(1.24)	(0.93)	(0.76)	(0.65)	(0.50)
Observations	109 721	109 721	109 721	141 882	141 882	141 882
Control group mean	21.15%	17.48%	8.35%	10.27%	6.96%	3.7%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald	105	105	105	183	183	183

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 5.

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

Table B.10. Field inheritance by parent age at child application

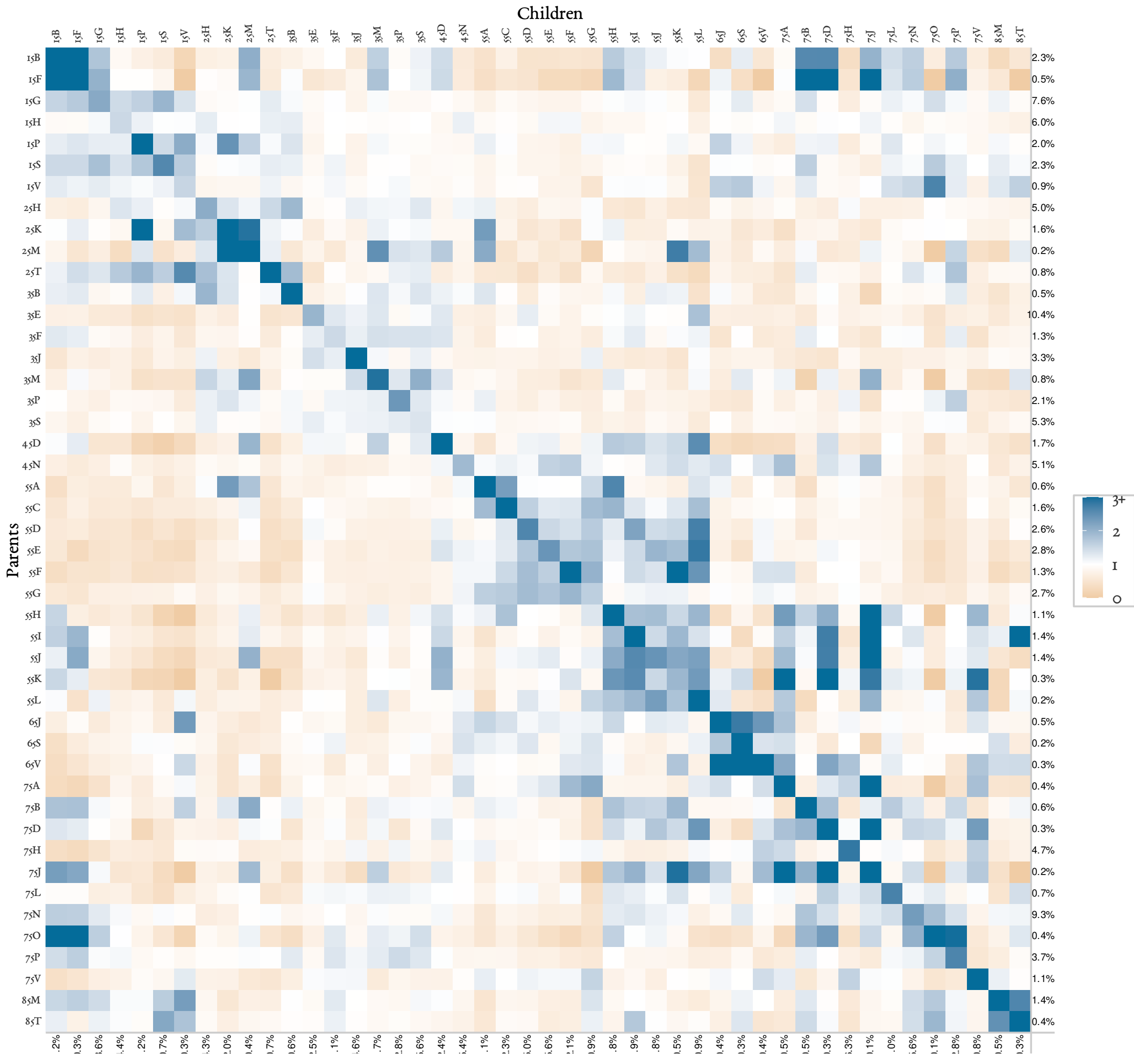
	Broad fields			Narrow fields		
	Ranks 1st	Enrolls	Earns degree	Ranks 1st	Enrolls	Earns degree
Parent enrolls in j	2.13	3.60	5.38	−2.77	2.46	1.11
	(13.69)	(12.39)	(9.85)	(8.63)	(6.40)	(5.14)
× Parent age 41–50	8.00	4.85	3.81	9.94	2.58	3.61
	(13.53)	(12.21)	(9.68)	(8.55)	(6.33)	(5.09)
× Parent age 51–64	7.74	3.03	−0.08	9.18	0.38	1.24
	(13.55)	(12.25)	(9.68)	(8.56)	(6.32)	(5.08)
× Parent age 65+	−11.81	−4.58	1.73	−15.15	−15.34	−4.94
	(17.63)	(15.29)	(12.12)	(13.72)	(10.48)	(5.30)
Parent age 41–50	−2.16	−0.52	1.29	−4.34	1.44	−0.16
	(6.79)	(6.18)	(4.92)	(4.32)	(2.99)	(2.34)
Parent age 51–64	−7.43	−4.08	−4.42	−6.33	0.66	−3.38
	(6.80)	(6.20)	(4.92)	(4.32)	(3.01)	(2.34)
Parent age 65+	−3.97	−8.25	−13.69*	3.34	6.74	−5.81*
	(10.72)	(8.56)	(6.40)	(8.97)	(6.78)	(2.53)
Observations	90 916	90 916	90 916	117 653	117 653	117 653
Control group mean	25.6%	20.98%	10.08%	12.46%	8.39%	4.48%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald	120	120	120	204	204	204

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 5.

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

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.

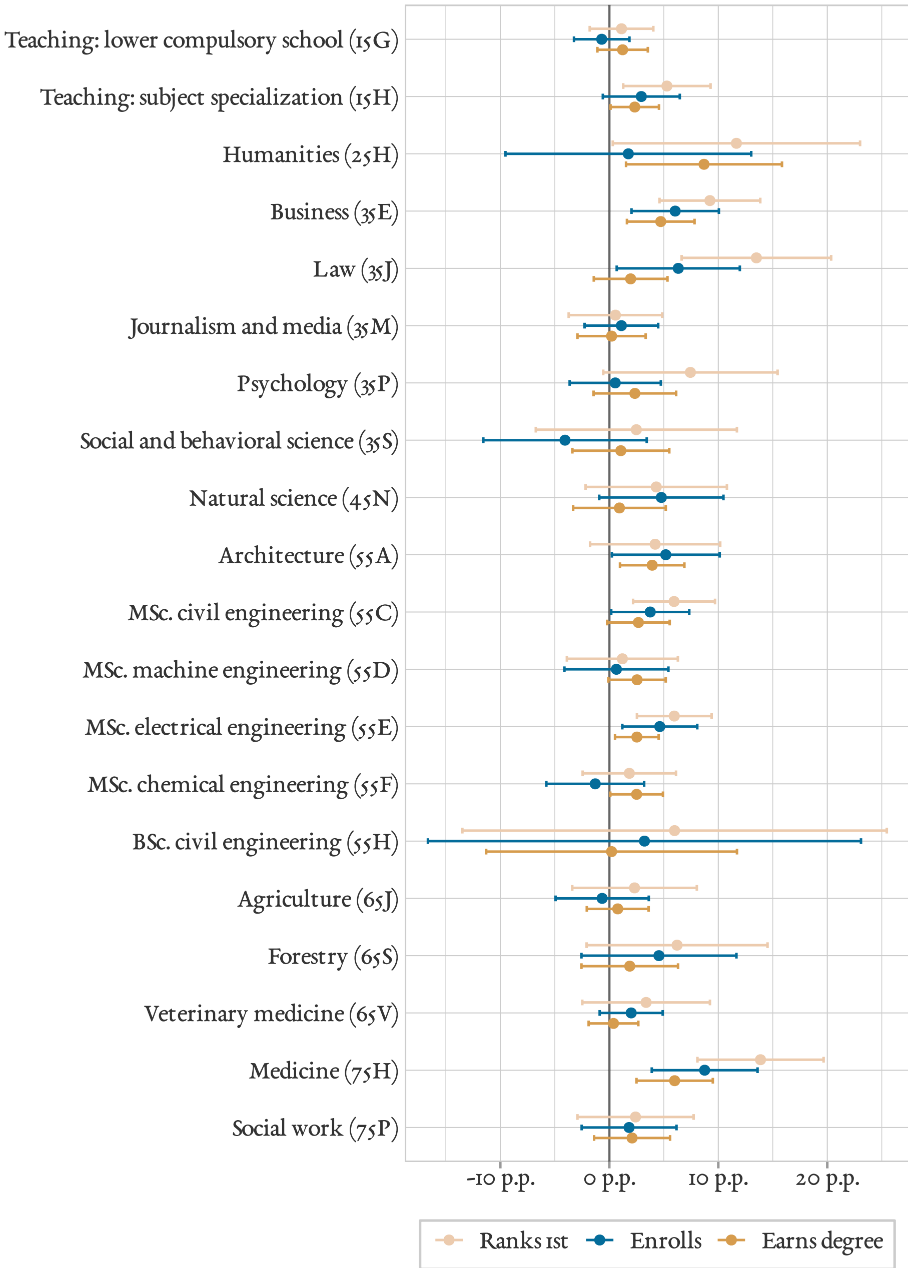
Table C.1. Summary statistics by parent narrow field of study

	Included in sample	N	Share women	Share enrolled below cutoff	First stage (parent enrolls)
Teaching: pre-school (15B)	No	2880	89.7%	32.4%	4.7p.p.
Teaching: after-school care (15F)	No	5293	77.0%	19.6%	26.1p.p.***
Teaching: lower compulsory school (15G)	Yes	13 386	76.6%	15.6%	35.4p.p.***
Teaching: subject specialization (15H)	Yes	9073	58.6%	18.7%	35.8p.p.***
Teaching: music and arts (15P)	No	465	99.1%	21.4%	
Teaching: vocational (15V)	No	3291	55.9%	12.1%	37.3p.p.***
Humanities (25H)	Yes	2967	74.4%	15.4%	19.1p.p.***
Media production (25M)	No	149	68.5%	12.3%	124.7p.p.***
Theology (25T)	No	132	64.4%	12.9%	−22.0p.p.
Business (35E)	Yes	24 235	49.4%	28.7%	23.3p.p.***
Management and administration (35F)	No	3252	63.4%	22.5%	11.3p.p.*
Law (35J)	Yes	7285	59.6%	21.6%	18.2p.p.***
Journalism and media (35M)	Yes	4542	67.1%	9.4%	41.5p.p.***
Psychology (35P)	Yes	3605	67.5%	12.8%	36.5p.p.***
Social and behavioral science (35S)	Yes	4594	58.9%	11.7%	19.6p.p.***
Social science, other (35X)	No	3269	73.6%	10.3%	30.3p.p.***
Computer science (45D)	No	6344	38.7%	21.7%	23.1p.p.***
Natural science (45N)	Yes	6504	51.0%	26.2%	16.2p.p.***
Natural science, other (45X)	No	80	26.2%	20.0%	8.4p.p.
Architecture (55A)	Yes	4019	55.6%	13.8%	40.8p.p.***
MSc. civil engineering (55C)	Yes	3954	38.0%	20.0%	19.2p.p.***
MSc. machine engineering (55D)	Yes	10 234	21.7%	28.0%	32.0p.p.***
MSc. electrical engineering (55E)	Yes	15 476	17.9%	26.9%	37.6p.p.***
MSc. chemical engineering (55F)	Yes	4789	51.9%	24.1%	26.3p.p.***
BSc. civil engineering (55H)	Yes	327	26.0%	10.8%	53.4p.p.**
BSc. machine engineering (55I)	No	587	23.5%	26.7%	5.7p.p.
BSc. electrical engineering (55J)	No	1020	18.6%	25.6%	−7.1p.p.
BSc. chemical engineering (55K)	No	396	86.1%	9.3%	16.7p.p.
BSc. engineering, other (55L)	No	570	11.8%	14.3%	35.7p.p.***
Agriculture (65J)	Yes	2378	54.5%	25.4%	23.2p.p.***
Forestry (65S)	Yes	1294	17.9%	31.7%	39.9p.p.***
Veterinary medicine (65V)	Yes	2354	68.9%	11.0%	52.4p.p.***
Agriculture, other (65X)	No	94	9.6%	4.9%	
Pharmacy (75A)	No	1931	77.4%	20.7%	15.2p.p.
Biomedical analyst (75D)	No	39	84.6%	24.0%	
Child care (75F)	No	427	55.5%	5.1%	3.3p.p.
Medicine (75H)	Yes	10 932	45.8%	36.8%	26.7p.p.***
Pharmacy (dispenser) (75J)	No	1078	93.6%	14.4%	12.4p.p.
Social work (75P)	Yes	9934	76.6%	21.0%	18.7p.p.***
Dentistry (75V)	No	3206	51.0%	34.2%	10.4p.p.*
Care, other (75X)	No	2166	79.5%	10.3%	23.6p.p.**
Transport services (85T)	No	47	34.0%	18.2%	
Transport service, other (85X)	No	3752	81.2%	7.1%	12.3p.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 5.

Table C.2. Field heterogeneity narrow fields

Field	Relative popularity	Effect estimate		Control group mean	Relative effect
Teaching: lower compulsory school (15G)	213%	1.22p.p.	(1.18)	2.06%	59%
Teaching: subject specialization (15H)	150%	2.34p.p.*	(1.13)	1.59%	147%
Humanities (25H)	209%	8.69p.p.*	(3.65)	2.06%	422%
Business (35E)	197%	4.72p.p.**	(1.58)	8.26%	57%
Law (35J)	314%	1.96p.p.	(1.72)	2.68%	73%
Journalism and media (35M)	291%	0.21p.p.	(1.60)	1.93%	11%
Psychology (35P)	238%	2.35p.p.	(1.93)	2.12%	111%
Social and behavioral science (35S)	134%	1.06p.p.	(2.27)	2.88%	37%
Natural science (45N)	190%	0.93p.p.	(2.17)	2.55%	36%
Architecture (55A)	591%	3.94p.p.**	(1.51)	0.88%	446%
MSc. civil engineering (55C)	309%	2.67p.p.†	(1.46)	2.19%	122%
MSc. machine engineering (55D)	262%	2.55p.p.†	(1.34)	3.82%	67%
MSc. electrical engineering (55E)	240%	2.53p.p.*	(1.02)	4.18%	60%
MSc. chemical engineering (55F)	341%	2.51p.p.*	(1.23)	1.02%	245%
BSc. civil engineering (55H)	345%	0.21p.p.	(5.87)	2.23%	9%
Agriculture (65J)	931%	0.77p.p.	(1.45)	0.91%	85%
Forestry (65S)	1757%	1.88p.p.	(2.26)	1.08%	174%
Veterinary medicine (65V)	1192%	0.38p.p.	(1.16)	0.70%	55%
Medicine (75H)	283%	6.00p.p.***	(1.79)	6.10%	98%
Social work (75P)	264%	2.09p.p.	(1.78)	2.13%	98%
Aggregate	273%	2.97p.p.***	(0.82)	3.70%	80%

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 Analysis of additional treatment margins

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.[22] This is a useful way to gain an additional measure against which we can benchmark the results of the main text. 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.

In contrast to the transmission of education preferences between siblings (Altmejd et al. 2021), the baseline preferences for going to the same institution across generations are about the same or even slightly smaller, than for fields of study. Also, the treatment effects are somewhat weaker. In relative terms, parental enrollment improves children's institutional preferences by approximately 60%. Considering the time span between the applications of parents and children this result is not surprising. The inheritance of institutional preferences is not likely mediated by occupation choice, but rather by location.

Table D.1. Inheritance of institutions

	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	2.93***	2.32***	0.84***
	(0.41)	(0.38)	(0.23)
Parent enrolls in j	10.70***	8.44***	3.07***
	(1.47)	(1.36)	(0.85)
Parent receives degree in j	16.28***	12.85***	4.67***
	(2.26)	(2.10)	(1.29)
Observations	236 668	236 668	236 668
Control group mean	19.36%	15.46%	5.08%
Bandwidth	2.0	2.0	2.0
1st stage Wald (enrolls)	2193	2193	2193
1st stage Wald (degree)	1108	1108	1108

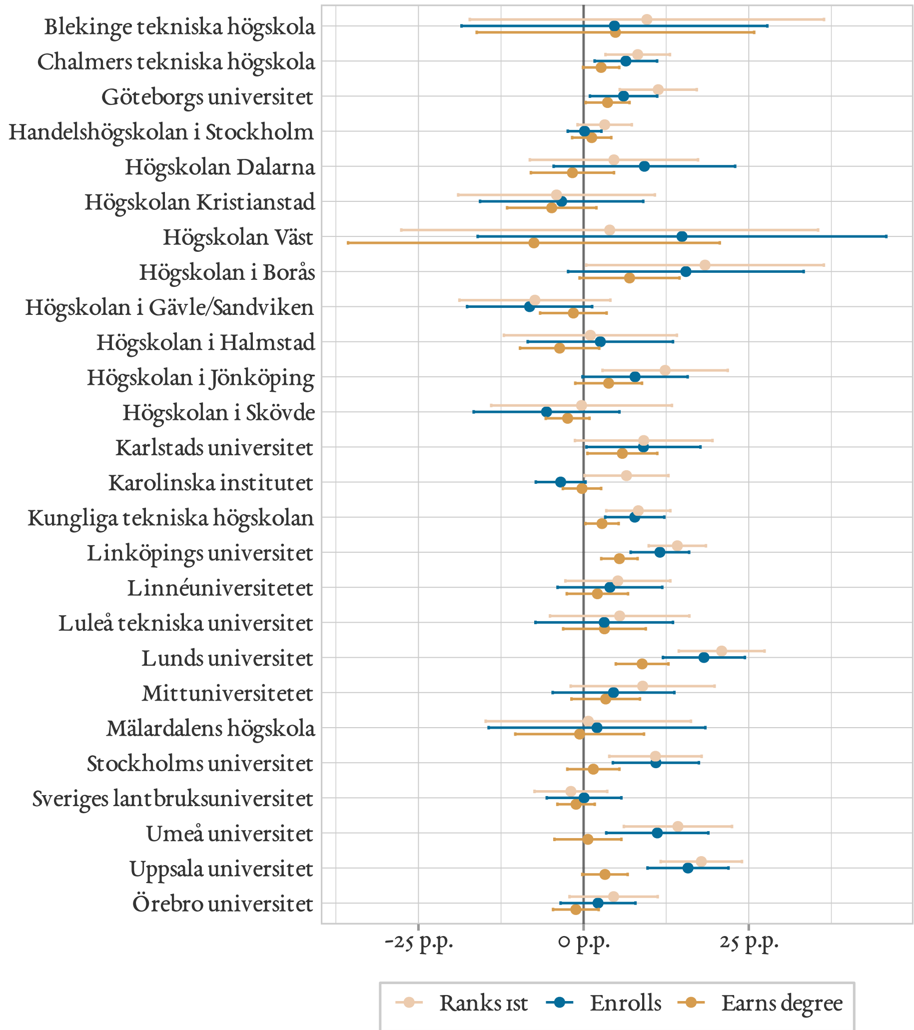
Notes: Instead of collapsing consecutive options by field of study, the sample includes applications collapsed by institution. A child is thus classified as following their parent only if they pick the same institution as their parent, irrespective of what program they chose. Otherwise, the estimation follows the same approach as Table 5.

Figure D.1 presents separate coefficients for each institution. Again, we see very few negative effects, but quite a bit of heterogeneity. The largest and most precise estimates are for big universities that offer a broad range of alternatives and are located in towns with no other higher education institutions (e.g., Lund, Uppsala, Umeå). The two most prestigious schools, Stockholm school of economics (Handelshögskolan i Stockholm) and the Karolinska Institute both exhibit small effects that are not significant. This while specializing on two of the most often inherited fields: business and medicine. 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

22. 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.

two effects are 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.

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 5 but with next-best fixed effects at the institution level.

As noted above, 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 larger than for institutions, but also with higher baselines, yielding similar relative effects, between 62–70% and showing how important location is for university choice.

As a final benchmark, I 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

Table D.2. Inheritance of locations

	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to *j*	3.91***	3.48***	1.43***
	(0.45)	(0.44)	(0.30)
Parent enrolls in *j*	15.13***	13.46***	5.55***
	(1.70)	(1.68)	(1.17)
Parent receives degree in *j*	22.39***	19.92***	8.21***
	(2.55)	(2.52)	(1.74)
Observations	204 766	204 766	204 766
Control group mean	24.03%	21.31%	7.88%
Bandwidth	2.0	2.0	2.0
1st stage Wald (enrolls)	1806	1806	1806
1st stage Wald (degree)	943	943	943

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 5.

results. Not surprising, baseline shares are considerably smaller, but so are the absolute effects. Parental enrollment in a field-institution combination increases graduation probability by 1.96 percentage points or 128%. The rightmost part of the table reports the likelihood that a child follows to the same field when the parent is on the margin between two different fields in the same institution. The relative effect of 95% can be compared to the effect in the main specification at 73%. That field inheritance is even larger when the treatment does not incur a change of institution indicates that the transmission effect of institutions and fields 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	1.14^{***}	0.93^{***}	0.48^{***}	1.54^{*}	$1.03^{\dagger}$	$0.58^{\dagger}$
	(0.15)	(0.14)	(0.09)	(0.68)	(0.60)	(0.33)
Parent enrolls in j	4.66^{***}	3.81^{***}	1.96^{***}	4.89^{*}	$3.26^{\dagger}$	$1.83^{\dagger}$
	(0.62)	(0.56)	(0.35)	(2.14)	(1.89)	(1.06)
Parent receives degree in j	8.56^{***}	7.00^{***}	3.59^{***}	8.81^{*}	$5.88^{\dagger}$	$3.30^{\dagger}$
	(1.14)	(1.02)	(0.64)	(3.82)	(3.38)	(1.88)
Observations	834 542	834 542	834 542	52 495	52 495	52 495
Control group mean	5.78%	4.29%	1.52%	8.47%	5.97%	1.92%
Bandwidth	2.0	2.0	2.0	2.0	2.0	2.0
1st stage Wald (enrolls)	4472	4472	4472	666	666	666
1st stage Wald (degree)	1744	1744	1744	223	223	223

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 5.

Table D.4. Extensive margin inheritance

	Ranks 1st	Enrolls	Earns degree
Parent above cutoff to j	0.02	0.05	−0.39
	(0.28)	(0.30)	(0.31)
Parent enrolls in j	0.51	1.23	−10.05
	(7.18)	(7.74)	(8.19)
Parent receives degree in j	0.55	1.32	−10.81
	(7.73)	(8.33)	(8.88)
Observations	427 240	427 240	427 240
Control group mean	77.15%	69.93%	43.61%
Bandwidth	2.0	2.0	2.0
1st stage Wald (enrolls)	164	164	164
1st stage Wald (degree)	91	91	91

Notes: This table reports results for the extensive margin. Here, all the parents preferences are collapsed and we study enrolling in any program vs not. Consequently outcome dummies are switched on when a child applies, enrolls or graduates from any program. Otherwise, the estimation follows the same approach as Table 5.

E Codebook

Table E.1. Narrow field codes and descriptions

Code	Description	Broad field
15F	Teaching: after-school care	Teaching
15G	Teaching: lower compulsory school	Teaching
15H	Teaching: subject specialization	Teaching
15S	Teaching: special needs	Teaching
15V	Teaching: vocational	Teaching
25H	Humanities	Humanities
35B	Library science	Social science
35E	Business	Business
35J	Law	Law
35M	Journalism and media	Social science
35P	Psychology	Social science
35S	Social and behavioral science	Social science
45N	Natural science	Natural science
55A	Architecture	Engineering
55C	MSc. civil engineering	Engineering
55D	MSc. machine engineering	Engineering
55E	MSc. electrical engineering	Engineering
55F	MSc. chemical engineering	Engineering
55H	BSc. civil engineering	Engineering
65J	Agriculture	Agriculture
65S	Forestry	Agriculture
65V	Veterinary medicine	Agriculture
75B	Occupational therapy	Health
75D	Biomedical analyst	Natural science
75F	Child care	Teaching
75H	Medicine	Medicine
75L	Physiotherapy	Health
75N	Nursing	Health
75O	Social care	Social work
75P	Social work	Social work
75T	Dental hygiene	Dentistry
85M	Officer	Services
85P	Policing	Services
85T	Transport services	Services