Schooling for Profit:
Long-run Effects of Private Providers in Public Education

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Abstract

I estimate the long-run earnings impacts of for-profit and non-profit charter high schools in Sweden. Since the 1990s, privately managed schools have expanded dramatically—driven entirely by for-profit providers—and now enroll nearly half of urban high school students. Unlike in many other settings, there are no schools operating outside of the public system: all schools rely on equal public funding, cannot charge top-up fees, and are subject to the same regulation. Using a combination of value-added and regression discontinuity methods, I find that charter school attendance reduces long-run earnings by 2% on average—comparable to the returns to half a year of schooling in similar settings. For-profits generate these losses by hiring less-educated, lower-paid teachers, consistent with concerns around cost-cutting. By contrast, non-profits reduce earnings by specializing in arts programs: conditional on such specialization, they perform slightly better than public schools. In a discrete choice framework using rank-ordered school applications, I show that students' preferences are weakly related to schools' earnings impacts. Most of the for-profit market share is explained by student demand for attractive locations and study programs, presenting a trade-off between satisfying short-run demand and boosting long-run economic outcomes.

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1 Introduction

Market-based reforms in public education have gained significant traction in recent decades. Policymakers are introducing or expanding charter and voucher programs that allow students to attend privately managed—and sometimes for-profit—schools on public funds. In 2025, for example, the U.S. House of Representatives passed a federal school voucher bill allocating $5 billion to private education providers, including for-profit operators, and similar arrangements have been adopted across a wide range of education systems globally (OECD 2017; Goldstein 2025). A core aim of such policies is to leverage private incentives to improve educational quality (Friedman 1955). Yet whether such mechanisms succeed at scale remains unclear (Cohodes & Parham 2021), since parents may choose schools based on features unrelated to quality, and private providers could face incentives to cut costs at the expense of student outcomes (Holmstrom & Milgrom 1991; Hart et al. 1997)

Evaluating the effectiveness of private providers in public education poses several challenges. First, in many systems, privately operated schools differ from public schools not only in ownership but also in autonomy, resources, and oversight, making it difficult to attribute differences in quality to private management (Angrist et al. 2013). Second, few settings have the data needed to track students into adulthood, limiting what we know about long-run impacts on earnings. Third, evidence from early-stage or small-scale programs does not capture differentiation patterns or productivity improvements that may emerge only at scale (Angrist et al. 2002; Romero et al. 2020).

In this paper, I address all of these challenges by using data from Sweden's charter school system. Introduced in the early 1990s, private providers now serve nearly half of all urban high school students, with about 35% attending for-profit schools and 10% attending non-profit schools. Public and private providers operate under the same institutional constraints: they hire from the same teacher labor market, follow the same national curriculum, and receive the same per-pupil funding. There are no tuition fees or private schools outside the public system, and for-profits may distribute profits freely. The combination of large scale, regulatory comparability, and detailed population-wide register data allows me to study how schools perform and compete for students in a mature, competitive public education market.

I study the high school market of Sweden's largest metropolitan area, covering around 20,000 students enrolling in roughly 160 schools annually. In this setting, high schools are free to offer seats in a menu of government-designed study programs, similar to fields of study in college. I estimate the impact on earnings at age 30 of attending each combination of high schools and study programs in the market, allowing these effects to vary by students' prior achievement, in a value-added framework (Chetty et al. 2014; Dobbie & Fryer 2020).[1] For part of my sample, I have access to data from the mechanism allocating students to school seats. This mechanism (serial dictatorship) ranks students by their GPA from lower secondary school and assigns students in order according to their

[1] Value-added (VA) models typically refer to instances where a lagged measure of the dependent variable, commonly test scores or grades, is observed and controlled for in a selection-on-observables strategy. No such lagged measure is available for earnings, but I will refer to VA as an umbrella term for selection-on-observables models of school effectiveness broadly.

submitted preferences. For oversubscribed seats, this generates cutoffs around which admission is credibly exogenous in a regression discontinuity (RD) design (Abdulkadiroğlu et al. 2017). I follow the approaches of Angrist et al. (2017) and Abdulkadiroğlu et al. (2022), and show that RD estimates of school and program effects are closely approximated by value-added estimates.

I find that, on average, charter schools reduce earnings relative to public schools by 1.11 percentiles of the earnings distribution at age 30. This corresponds to 7,500 SEK ($750) annually—2% relative to a sample mean of 373,000 SEK ($37,300). For comparison, Mincer returns to years of schooling in Sweden is estimated to be around 3–5%. A back-of-the-envelope calculation implies a net present lifetime loss of about 200,000 SEK ($20,000) per student, with forgone tax revenue equivalent to 20% of the cost of providing three years of high school. These losses also vastly exceed the surplus that for-profit providers generate as profits (around 15,000 SEK, or $1,500, per student). However, the negative impact on earnings is not confined to for-profit charters; non-profits reduce earnings by almost twice as much (1.58 percentiles) as for-profits (0.8 percentiles), on average.

While both for- and non-profits are less effective than public schools, the sources of these gaps are entirely distinct. To show this, I decompose the total earnings impacts into separable school and program effects. These are analogous to institution versus field-of-study effects in higher education settings (see, e.g., Kirkeboen et al. 2016). Non-profits are less effective than public schools because they enroll students in study programs associated with low labor market returns, such as arts. In terms of school-level effectiveness, non-profits perform on par with—or even slightly better than—public schools. For-profits, on the other hand, reduce earnings solely by providing low school-level effectiveness. This is, in turn, fully explained by for-profits hiring less educated (and thereby less expensive) teachers, consistent with cost-cutting incentives that are absent for non-profits.

These average impacts mask important heterogeneity by students' academic ability. For-profits serving academically weak students are almost as effective as public schools. This is because they avoid low-return vocational programs more common in public schools. In terms of school effectiveness, though, for-profit charters are less effective than public schools across the entire distribution of students.

The charter sector has expanded dramatically over time, almost entirely due to for-profit schools. If for-profits reduce earnings, why are they growing? Two key hypotheses could explain this puzzle. First, students may simply choose schools based on other features than effectiveness, such as location (Ainsworth et al. 2023). If for-profit charters are effective at catering to this demand, their growth would not be a puzzle but rather a logical outcome of competition. Second, underinvestment in public school capacity may lead students to enroll in charters, despite favoring a public option had one been available.

To investigate this, I turn to the demand side of the market. I estimate a discrete choice model of demand using data on students' school applications. Students can rank any, and as many, schools and programs they want. The assignment mechanism is strategy-proof, so there are no incentives to misreport preferences (Chade & Smith 2006), but students may well omit "irrelevant" alternatives where the chance of admission is very low (Fack et al. 2019). Hence, the estimated model of student demand fulfills two purposes: it allows me to (i) understand the drivers of student choice and (ii) predict which alternatives students would have ranked, had they ranked *all* alternatives available in the market.[2]

Students are more likely to choose more effective schools, but this correlation is very modest in size. According to the model estimates, students in the bottom and top tercile of the GPA distribution are only willing to travel an additional 0.2 and 0.6 kilometers, respectively, to attend a school that increases earnings by 1 additional percentile of the distribution at age 30. For reference, the average travel distance is around 9 kilometers. Program availability and location, rather than earnings impacts, are much stronger predictors of choice.

My model predicts that 23% of students would rank a for-profit charter as their top choice. This is close to the percentage enrolled in for-profits observed in the data (28%). Hence, most of the for-profit market share can be explained by their ability to cater to student demand for programs and location. The remaining market share—as well as the propensity of for-profits to enroll academically weak students—is explained by limited capacity in the public sector. Low-GPA students are admitted to schools further down their rank-ordered lists and these "fallback options" are more likely to be for-profit charters.

Estimating the net effect of the for-profit charter expansion is difficult, but I provide suggestive evidence of an important—and unequally distributed—tradeoff between future earnings and satisfying student demand. Using the model, I predict where for-profit charter students would have enrolled had their chosen school and program not been available, by redistributing for-profit seats proportionally to public schools. On average, students switch to less preferred but more effective public schools—implying higher future earnings at the expense of attending a less preferable school and program. This loss of preferred options is universal, but the gain in earnings is not. Low-GPA students are induced to leave high-return vocational programs that are more commonly available in the for-profit sector, leading to a net earnings loss.

These results provide important insights that extend beyond the Swedish context. The substantial differences between for- and non-profit providers raise concerns about misaligned incentives. In the outsourcing of public education, contracts with private providers are necessarily incomplete, which creates incentives for for-profits to engage in cost-cutting at the expense of effectiveness (Holmstrom & Milgrom 1991; Hart et al. 1997).[3] Importantly, my results suggest that this is not a

[2] This approach is similar to that of Gandil (2021), who estimates a discrete choice model of student choice with the aim of predicting students' rankings over all available colleges and fields-of-study in Denmark. Rather than explicitly trying to capture causal parameters of a structural utility function, this approach frames the problem as one of missing data: we typically only observe student rankings over a set of feasible options, and want to recover a full ordering over all alternatives.

[3] Similar concerns have been raised in the context of higher education. Several studies show that attending for-profit colleges in the U.S. reduces employment and earnings (Deming et al. 2012; Cellini & Turner 2019) and that such colleges may raise prices when eligible for federal student aid programs, pointing to moral hazard (Cellini & Goldin 2014). Outside of education, Knutsson & Tyrefors (2022) show that for-profit ambulance providers perform worse than public providers on contracted measures such as response, but worse for noncontracted outcomes such as mortality.

short-run phenomenon that is eventually corrected by the market: the market share of for-profits has grown in virtually every year since the introduction of the Swedish charter system. A plausible explanation is that students have weak preferences for or little information about school impacts (Hastings & Weinstein 2008; Ainsworth et al. 2023). Instead, for-profit providers attract students by offering other attributes—primarily locations and programs—that students want.

First, this paper contributes foremost to the literature on publicly funded but privately managed schools, such as charter schools in the U.S. (Cohodes & Parham 2021).[4] Very few studies have focused on differences across for- and non-profit charters within a single market, a feature that proves crucial in the Swedish context.[5] Because public, for-profit and non-profit schools operate under similar institutional rules, this setting comes closer to isolating differences in management than is usually possible (Angrist et al. 2013). Most previous research has mostly focused on short-run impacts on test scores, whereas I can track earnings until the age of 30.[6] Finally, the Swedish context provides one of the few examples of a mature charter system, covering the full universe of students, where private providers have served a substantial share of the market over a long period of time.

Second, the paper contributes to the literature on school choice (Burgess et al. 2015; MacLeod & Urquiola 2019; Abdulkadiroğlu et al. 2020; Beuermann et al. 2022). I provide novel evidence on the tradeoff between satisfying student demand, at the time of school choice, and promoting long-run earnings. Theory has long emphasized the potential consequences of low demand for effectiveness in market-based education systems (Friedman 1955; Hoxby 2003), and previous research has found evidence of both weak preferences for and low information about school effectiveness (Walters 2018; Ainsworth et al. 2023). I show that this is not a short-run phenomenon eventually corrected by the market, but one with real consequences for the composition of providers even in a mature, competitive education market.

Finally, I contribute to the methodological literature on observational (value-added) models of school impacts. A growing body of evidence has shown that observational estimates tend to closely approximate quasi-experimental or lottery effects.[7] However, we still know little about the ability of observational models to capture impacts on other outcomes besides test scores. By leveraging variation around admission cutoffs, I show that observational models of school and program impacts closely match regression discontinuity effects on college and labor market outcomes as well.

[4] The evidence on charter effectiveness is mixed, but successful examples include "No Excuses" schools focusing on strict discipline, longer school days, and standardized instruction (Abdulkadiroğlu et al. 2011; Angrist et al. 2013; Dobbie & Fryer 2015; Cohodes & Pineda 2024).

[5] Singleton (2019) studies responses to charter school funding formulas in Florida, which includes both for- and non-profit providers. Further, Dynarski et al. (2018) uses admission lotteries to study the impact of attending schools run by a single, large for-profit provider in Michigan and finds small, positive impacts on test scores.

[6] Only a few studies evaluate school impacts on college outcomes (Angrist et al. 2016; Cohodes & Pineda 2024), and even fewer on early labor market outcomes (Dobbie & Fryer 2020). This is true also in the Swedish setting (Tyrefors & Vlachos 2017; Edmark & Persson 2021).

[7] Evidence showing limited bias in observational value-added models of school and teacher effectiveness covers a wide range of settings, including the U.S. (Kane & Staiger 2008; Deming 2014; Chetty et al. 2014; Angrist et al. 2017; Angrist et al. 2024), Pakistan (Andrabi et al. 2011; Andrabi et al. 2025), India, Peru and Vietnam (Singh 2015; Singh 2020; Berg et al. 2025).

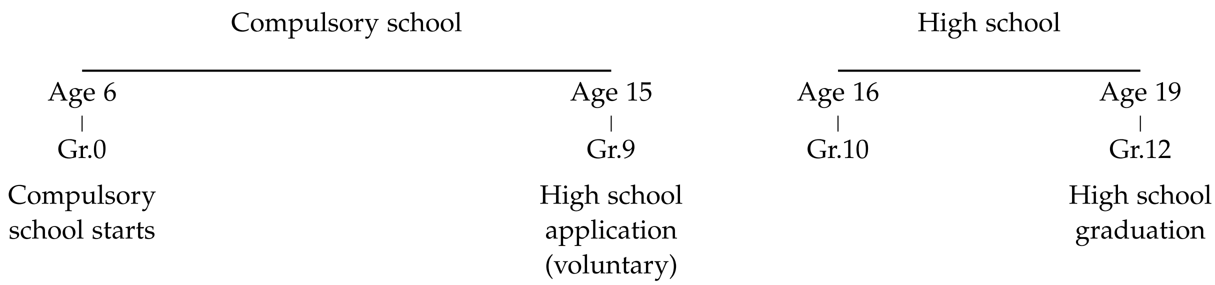
2 Context and Data

In this section, I first provide context on the Swedish educational and charter school system. After, I describe the data sources used in the analysis. Finally, I show descriptive statistics on students, schools and study programs.

2.1 The Swedish Educational System

I provide a timeline of the Swedish educational system in Figure 1. All children are required to complete 10 years of compulsory education, starting in Grade 0 at the age of 6.[8] During compulsory school, virtually all children follow the same national curriculum. Upon graduation from Grade 9—the last grade of compulsory school—students can apply for three years of high school education and are admitted based on their Grade 9 GPA.[9] This GPA is based on all courses taken in lower secondary school (Grades 7-9). While not mandatory, 90% of students enroll in high school education (Statistics Sweden 2023). High school is divided into 18 national study programs, of which 12 are vocational and 6 are academically oriented. Throughout this paper, I group these programs in 7 mutually exclusive categories: social sciences, natural sciences, business, arts and humanities, technology, manufacturing (vocational), and care and services (vocational).[10] An overview of the programs is given in Appendix B.

Figure 1: Timeline of the Swedish education system



The explicit aim of these programs is to either prepare students for higher education, or to start working directly after graduation. Up until 2010, all programs gave students basic college eligibility. This changed in 2011, after which vocational students had to actively choose specific electives in English and Swedish in order to become eligible. Upon graduation, students obtain a Grade 12 GPA—calculated as a weighted average across all high school courses—with which they can apply to college. As a complement to grade-based college admissions, colleges also admit students based on test scores from a national college entry exam (*Högskoleprovet*). This exam is not administered as part of high school education and is open to everyone.

[8] As of 2018, the preschool class of Grade 0 is mandatory. Prior to this, parents were free to choose whether to enroll their child in Grade 0 or, one year later, in Grade 1. Virtually all children did enroll already in Grade 0.

[9] Grade-based admissions were introduced in Stockholm city, the largest municipality in the greater metropolitan area, in 2000. Prior to this, oversubscribed seats were allocated based on proximity within large catchment areas.

[10] In this context, natural sciences is broadly regarded as the main STEM track. The technology program also has focus on mathematics, but is more focused on industrial or IT applications rather than natural sciences broadly (which includes, e.g., biology). The business and humanities programs were only introduced in 2011, and will therefore not be relevant in my long-run earnings sample which ends in 2008. The humanities program is very small, so I merge it with the arts programs after 2011.

2.2 Charter and Public Schools

Since the mid-1990s, schools may be owned and operated by local governments (public schools) or by private providers (charter schools), both at the compulsory and high school level. All schools are funded by the local government based on its number of enrolled students in the form of a voucher, and cannot charge top-up fees.[11] At the high school level, which is the focus of this paper, the voucher amount differs across programs and local governments decide upon a price list for different programs that reflect average costs in the public school sector. In large urban areas, consisting of many different municipal areas, local governments often coordinate on a common price list for school vouchers. Charter providers are allowed to earn profits, be publicly listed and take on debt like a regular firm. While a majority of charters are run as for-profit firms, non-profit providers (typically foundations) are also common. Since donations are very uncommon and tuition is forbidden, virtually all revenue stems from the funding provided by the local government. An overview of the responsibilities of public and charter schools discussed in this section is shown in Table 1.

High schools are free to supply any number of seats in the menu of national programs. However, they are required to follow nationally determined, program-specific curricula. Each high school program is divided into a set of courses, whose contents and educational goals are detailed in the curriculum. The curriculum specifies a number of credits but not an exact number of hours to be spent on each course. By law, all students are guaranteed a minimum level of instructional time over the course of their high school education but schools vary in how they fulfill these requirements; for example, they may count teacher-supervised study time in very large groups without direct instruction (Skolinspektionen 2018).[12] Conditional on following the relevant curricula, schools also enjoy significant discretion in pedagogy, resource allocation within the school, and the design of elective courses.[13]

Teacher hiring is decentralized and relatively unregulated. While teachers are formally employed by school providers, hiring is delegated to individual school units and no centralized teacher assignment exists. As of 2011, only licensed teachers (granted by obtaining a teaching degree at college) can be permanently employed, and unlicensed teachers can only be employed for a duration of two years. However, a recent investigation by the Swedish National Audit Office found that the system of teacher licenses has largely failed to increase the share of teachers with a teaching degree, in part due to loopholes allowing schools to renew temporary employment contracts of unlicensed teachers (Riksrevisionen 2025). Unlike many other settings, teacher contracts specify total working hours rather than a fixed number of instructional hours.

[11] A few fully private schools that charge tuition exist in Sweden. These are limited to explicitly international schools, which do not follow the Swedish curriculum, admit students through separate processes, and primarily enroll foreign nationals. Such schools are not part of this study.

[12] The mandated educational time is 2180 hours in academic programs, and 2430 hours in vocational programs. Each course is associated with a given number of credits, which provides a relative measure of the extent of the course.

[13] For example, the curriculum might mandate that a natural sciences program must teach "Math 101", defined as a set of learning goals (algebra; solving equations, geometry; calculating area and volume, etc.). Schools are free to use, e.g., different textbooks and pedagogical techniques to achieve these goals.

Table 1: Overview of charter and public high school regulations

	Public	For-profit charter	Non-profit charter
Ownership			
Who owns the school?	Local govt.	Private firm	Non-profit organization (e.g., foundation)
Education			
Who sets the curriculum?	National govt.	National govt.	National govt.
Who chooses which programs to offer?	Local govt.	Provider	Provider
Within-school resource allocation?	School (headmaster)	School (headmaster)	School (headmaster)
Inputs			
Where is revenue coming from?	Local govt.	Local govt.	Local govt.
Who owns/rents the building?	Local govt.	Provider	Provider
Who hires teachers?[1]	School	School	School
Who can be hired?[2]	Anyone	Anyone	Anyone
Entry & exit			
Who approves entry?	Local govt.	National govt. authority	National govt. authority
Who approves exit?	Local govt.	Provider	Provider

Notes: [1]Formally, teachers are employed by the school provider (e.g., the local government for public schools), but all decisions on recruitment, terminations, etc., are delegated to the individual school unit.
[2]A teacher license was introduced in 2011, generally granted as a result of obtaining a teaching degree from college, along with regulation stating that unlicensed teachers can only be employed continuously for a period of two years. However, loopholes allowing schools to renew temporary employment contracts of unlicensed teachers exist and are widely used (Riksrevisionen 2025).

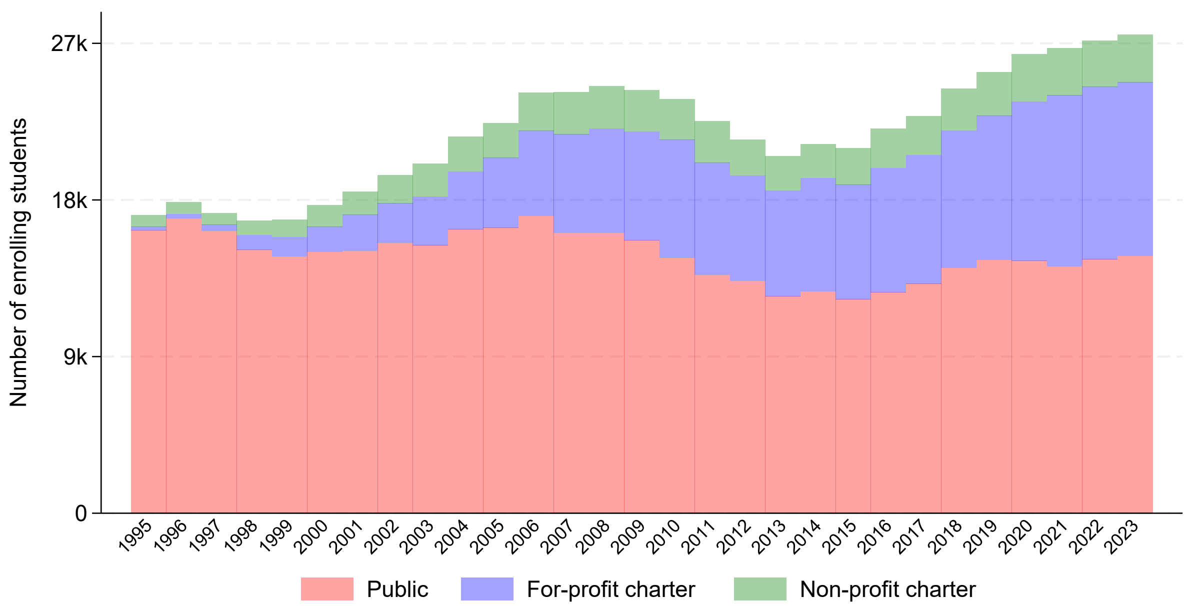
Among teachers with similar credentials, the distribution of pay and non-wage benefits is compressed due to collective bargaining agreements. These agreements regulate a broad set of employment conditions for teachers, including rules on working hours, vacation entitlements, notice periods, and procedures for layoffs (e.g., seniority rules). While the individual salary is typically negotiated between the teacher and the employer, the collective agreements set important minimum standards and frameworks that structure these negotiations, limiting individual variation in pay (Epple et al. 2016). Public schools are always covered by sector-wide agreements whereas most, but not all, charter schools have similar agreements. Survey data indicate that around 85–90% of high school teachers in both public and charter schools were unionized in the early 2000s, a number that reduced only slightly over the subsequent two decades (Kjellberg 2020). However, once a school is covered by an agreement, it extends to all teachers including those that are not members of any union.

New charter schools, and extensions of existing program offerings, must be approved by the School Inspectorate, a national government authority. In addition to plans for the physical building and staffing, prospective charters must provide arguments for why their school would be in demand; typically in the form of student surveys or other supporting data. The local government can and often does provide counterclaims to these applications but have no veto right.[14] Both charter and public schools may exit the market at any time, in which case students must transfer to another school to complete their high school education.

2.3 Data

This paper focuses on the high school market of the greater Stockholm area. Each year, this market admits around 20,000 students to roughly 160 different high schools. The market share of charters in this market has increased steadily since the mid-1990s, and the vast majority of charter schools are run for profit: in 2023, almost 50% of high school students attended charters, and 75% of these attended for-profit institutions (Figure 2). In 2011, the region entered an agreement on centralized assignment based on students' Grade 9 GPA according to a serial dictatorship mechanism. Before this, students could apply to all schools in the region—also on basis of GPA—but public schools prioritized students living in the same municipality in case of oversubscription, while charters did not.

Figure 2: Number of enrolling high school students by sector, 1995–2023



Notes: This figure shows the number of students enrolling in public, non-profit and for-profit charter schools separately for each enrolling cohort between 1995 and 2023. The trend in enrolling cohort sizes closely matches birth cohort sizes over the same time period.

This paper uses several sources of data to investigate school effectiveness. First, I use Swedish administrative data on students' high school enrollment, demographic and academic background, and labor market outcomes. Key variables include where and when students enrolled and graduation from high school, their grades from compulsory school, labor market

[14] The inability of local governments to prevent charter entry is a contentious point of debate. The School Inspectorate can, in principle, deny charter entry on basis of harmful spillover effects on public schools. In practice, this rarely happens (Lindvall & Velizelos 2022).

earnings, as well as parental education, earnings and other demographic characteristics. All of these data are available from 1995 through 2022.

Second, I use publicly available data from the Swedish National Agency for Education to construct a panel of all high schools, and their providers, in the greater Stockholm area. Since school unit codes—the unique identifier of schools in administrative data—often change over time and may refer to different organizational entities within the same school, I manually link them across years using school names and addresses. Charters are categorized as for-profit or non-profit based on what the head owner of the provider is registered as, data that I manually collect from open records on organizational structures. I drop schools that are not open to the general student population, such as those serving students with cognitive disabilities. Further, I omit students enrolled in special study programs designed for students with incomplete grades from compulsory school (introductory programs).

Third, I also use data on students' applications to high school available after 2013. With these data, I construct two main analysis samples.

Long-run earnings sample (1995-2008). In my main analysis sample, I link the universe of high school students in the greater Stockholm area to their future, annual labor earnings at age 30. Labor earnings are measured before taxes and include i) annual wage earnings, ii) income from self-employment and iii) income support for absences due to sickness or parental leave. I complement this with data on high school graduation, college enrollment, field-of-study in college, and labor earnings at age 23. Since most students enroll in high school at age 16, I am restricted to the sample of students enrolling in high school between 1995 and 2008. This sample covers 194,266 students and 158 unique schools.

One concern is that measuring earnings at age 30 may be too early to appropriately capture long-run (or lifetime) earnings (Haider & Solon 2006). In both the U.S. and Nordic countries, previous studies suggest that lifetime earnings is best predicted by earnings around the age of 35–40 (Björklund 1993; Böhlmark & Lindquist 2006; Bhuller et al. 2017).[15] I address this in two ways. First, I use percentile ranks of earnings as my main outcome measure along with absolute earnings, since these tend to stabilize earlier in the lifecycle.[16] Second, I use predicted lifetime earnings as an auxiliary outcome. This prediction is based on a regression of average (absolute) earnings between ages 35–37 on the earnings history, college enrollment and field-of-study up until the age of 30 for students enrolling in high school between 1995–2001. I find that school-by-program effects on earnings at 35–37 are almost fully accounted for by these mediators (see Appendix C for details and tests of predictive ability).

Student applications sample (2013-2022). After 2013, I can observe students' rank-ordered school applications in the administrative data. Students are able to list as many school/program

[15]Estimating impacts on earnings at these ages is infeasible given my setting: I would only be able to estimate school impacts up until 2003, at best, which would severely limit the scope of the analysis.

[16]As an example, Figure A.1 shows the absolute and percentile earnings differences by age between individuals enrolled/not enrolled in college at 21. For earnings ranks, this difference converges to around 12 percentiles already around age 27. The same convergence is significantly slower for absolute earnings.

combinations as they wish in their applications. I combine data on student applications with information on the number of seats offered in each school and program combination obtained from the admissions office of the greater Stockholm area and available from 2004. These data are used to exploit variation in student placements around admission cutoffs for validation purposes, as well as for the estimation of student preferences. One caveat is that I cannot estimate school impacts on earnings at age 30 for this sample. To address this, I predict lifetime earnings as discussed above using only outcomes up until the age of 23. However, given that this prediction is based on outcomes early in the lifecycle, it is best understood as a combined index of early labor market outcomes, college enrollment, and field-of-study.

2.4 Descriptive Statistics

My main analysis sample covers 82 public schools, 58 for-profit charters, and 18 non-profit charters. I provide descriptive statistics on these schools in Table 2.

Charters are, on average, significantly smaller and more specialized in terms of program offerings than public schools. For example, while only 19% of public schools are specialized into a single program type, the corresponding numbers for charters are 34% (non-profits) and 48% (for-profits). Almost 50% of charters are located centrally in the Stockholm region, while public schools are more geographically dispersed.

Table 2: Descriptive statistics on schools, 1995–2008

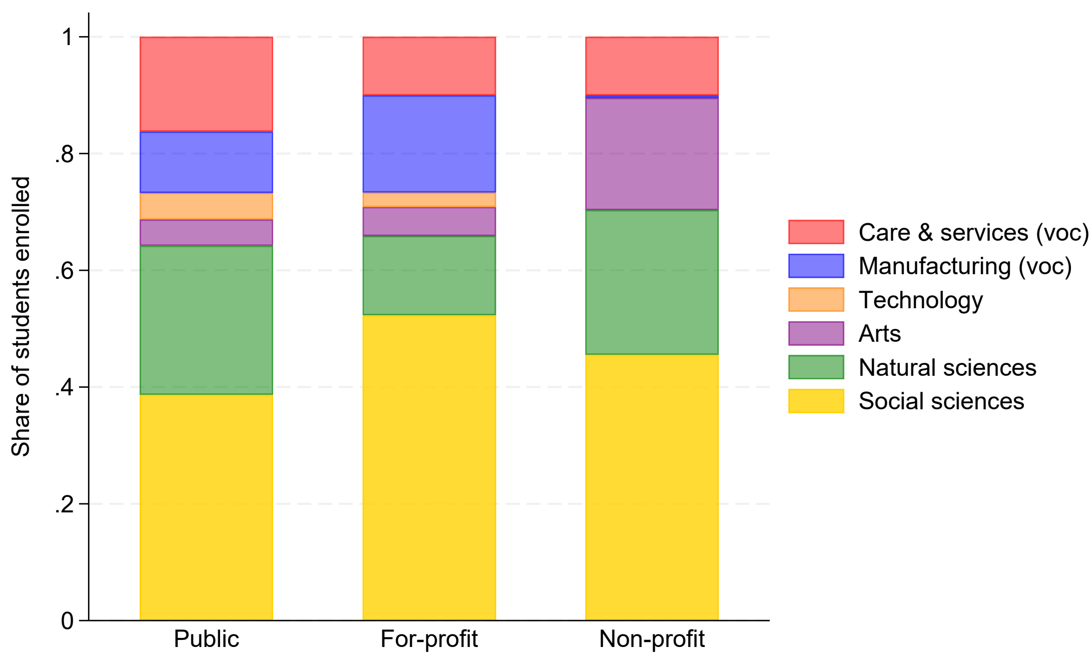
	Sector		
	Public	For-profit	Non-profit
Enrollment, programs and location			
Enrolled students	771	290	310
Number of program types offered (1–7)[1]	2.86	1.78	1.75
Offers academic *and* vocational programs	0.54	0.28	0.08
Offers only one program type	0.19	0.48	0.34
Central location[2]	0.20	0.46	0.49
Teachers			
Students per teacher	13.67	18.48	13.89
Average teacher age	47.57	39.55	42.65
Share with teaching degree	0.79	0.48	0.65
Share full-time employed	0.58	0.58	0.47
Share permanently employed	0.80	0.71	0.85
Log earnings	0.00	-0.14	0.00
Log earnings, resid. (teach. degree)	0.00	-0.09	0.02
Log earnings, resid. (teach. degree, age)	-0.00	-0.02	0.06
Number of unique schools	82	58	18
Number of school-by-year observations	854	290	173

Notes: This table shows means of student- and school-level variables separately for the public, non-profit and for-profit sector using data from 1995 through 2008.
[1]Program types (7 categories) are defined as described in Appendix B.
[2]Central location is defined as being located within 5 kilometers of the Stockholm city center (the royal palace).

The hiring practices of charter schools, and for-profits in particular, are a contentious topic in the Swedish public debate (Letmark 2022; Barkman 2024). For-profit charters have, on average, 35% more students per teacher compared to public schools, and these teachers are i) 31p.p. less likely to hold a teacher's degree from college and ii) 9p.p. less likely to be permanently employed. Teachers in for-profit charters have roughly 14% lower labor earnings than their colleagues in public schools. This difference disappears almost entirely when residualizing earnings on age and a binary measure of having a teacher degree. Hence, the earnings gap by sector is almost entirely driven by for-profit charters hiring younger and less qualified teachers. Since teacher salaries typically account for a substantial share of school expenditures, and charters are only able to make profits by cutting costs conditional on retaining students (and thereby government funding), a significant share of the profits made by charters are likely a result of these hiring practices. However, the hiring of younger and less experienced teachers may not necessarily be indicative of quality; for example, these factors do not seem correlate with effectiveness among U.S. charters. Rather, effective U.S. charters appear to focus on longer school days, stricter disciplinary policies, data-guided instruction and intensive tutoring (Dobbie & Fryer 2013; Angrist et al. 2013).[17]

To provide a more detailed view of program specialization across sectors, Figure 3 shows student enrollment shares by program type and sector. There are important differences across sectors even within the coarse division of academic and vocational programs.

Figure 3: Enrollment shares by program types and sector, 1995–2008



Notes: This figure shows the average share of public, non-profit and for-profit charter students enrolled in each program type between 1995 and 2008. The business program was introduced in 2011, and is therefore absent in this sample (see Appendix B for details on programs).

[17]It is, however, entirely possible that the positive gains of such policies simply outweigh the negative impact of having less qualified teachers.

Among academic programs, for-profit charters enroll a substantially larger share of students in social sciences and a smaller share in natural sciences than public and non-profit charter schools. In vocational programs, for-profit charters enroll nearly twice as many students in manufacturing-oriented programs relative to care and services—the opposite is true for public schools, and non-profits offer virtually no manufacturing-related programs. Instead, non-profits have a much larger share of students studying arts than for-profits and public schools.

The principal challenge in estimating the causal effect of attending different schools and programs is that students may select into them based on characteristics predictive of future earnings. This is evident from Table 3, where I report descriptive statistics on students enrolled in different sectors (Panel A) and programs (Panel B).

Table 3: Student selection into schools and programs, 1995–2008

Panel A: Schools	Public	For-profit	Non-profit
Grade 9 GPA (std)	-0.02	-0.07	0.47
Female	0.49	0.48	0.60
Born in Sweden	0.93	0.95	0.96
Father: college-educated	0.39	0.37	0.53
Mother: college-educated	0.43	0.43	0.58
Father: born in Sweden	0.75	0.75	0.81
Mother: born in Sweden	0.75	0.76	0.80
Father: earnings rank at 50	48.88	47.50	55.28
Mother: earnings rank at 50	48.33	48.49	52.61
Number of students	151596	26501	16169

Panel B: Programs	Social sciences	Natural sciences	Arts	Technology	Manufact. (vocational)	Care & services (vocational)
Grade 9 GPA (std)	0.09	0.78	-0.14	-0.11	-0.91	-0.75
Female	0.58	0.43	0.67	0.11	0.05	0.69
Born in Sweden	0.95	0.91	0.98	0.95	0.95	0.94
Father: college-educated	0.41	0.58	0.41	0.40	0.18	0.19
Mother: college-educated	0.46	0.60	0.50	0.46	0.24	0.23
Father: born in Sweden	0.75	0.73	0.83	0.79	0.76	0.74
Mother: born in Sweden	0.76	0.72	0.84	0.78	0.76	0.76
Father: earnings rank at 50	50.35	56.23	46.74	52.11	40.83	40.42
Mother: earnings rank at 50	50.13	54.13	48.02	50.58	41.58	40.41
Number of students	84909	45123	9575	7306	17958	29395

Notes: This table shows means of student- and school-level variables separately for the public, non-profit and for-profit sector using data from 1995 through 2008.
[1]Program types (7 categories) are defined as described in Appendix B.
[2]Stockholm municipality is the most central of the 26 municipalities constituting the greater Stockholm area.

For-profit charter students tend to be very similar to public school students in terms of gender and immigration status, but have slightly less educated fathers (2p.p.) and weaker GPA from compulsory school (0.05σ). Students in non-profit charters, however, are much more likely to be female and are strongly positively selected both on markers of socio-economic status (SES) and GPA.

The differences in student characteristics across different program types are substantially larger than between school sectors. High-GPA students tend to enroll in natural sciences, whereas students in the middle of the GPA distribution attend social sciences, technology or arts. Vocational students are heavily negatively selected on GPA, parental education and earnings, and segregated by gender: 95% of students in manufacturing programs are male, whereas 70% of students in care & services programs are female.

3 Estimating and Validating School and Program Effects

In this section, I estimate the effect on earnings of attending each school and program combination in the data, allowing for heterogeneity on students' prior GPA. To validate the selection-on-observables assumption underpinning this model, I leverage discontinuous variation in admissions around sharp cutoffs. While I do not have access to long-run earnings in this validation sample, I show that observational estimates of effectiveness on a short-run index of outcomes measured at age 23, that strongly predicts actual earnings impacts, are virtually unbiased.

3.1 Statistical Model

Consider a student i of type g enrolling in school j and program p. I model the earnings Y_i of student i when enrolled in alternative (j, p), denoted as $D_{ijp} = 1$, flexibly as:

$$Y_i = \alpha_g + \sum_{j,p} \beta_{jpg} D_{ijp} + \mathbf{\Gamma} \mathbf{X}_i + \epsilon_i, \tag{1}$$

where β_{jpg} captures the return of enrolling in alternative (j, p) for a student in GPA tercile g, relative to the average return to students in tercile g. $\mathbf{X}_i$ captures predetermined student characteristics, such as demographic characteristics and previous academic achievement, and ϵ_{ijp} reflects unobserved variation in earnings across students. The full set of controls in $\mathbf{X}_i$ includes: a cubic function of standardized Grade 9 GPA, interacted with student gender and the sector of their Grade 9 school (charter vs. public), to reflect differences in the informativeness of GPA on earnings; highest attained paternal and maternal education (seven categories, ranging from nine years of compulsory schooling to a PhD); quintiles of paternal and maternal earnings rank at age 50; indicators for parents and students being in born in Sweden; and fixed effects for student's home municipality, gender, sector of Grade 9 school, and birth year. In practice, I estimate β_{jpg} separately by bins of three years between 1995 and 2008, denoted by t but ignored in this section for brevity. This allows selection patterns into schools and programs to vary flexibly across time. The median number of students in each (j, p, g, t) cell is 104.[18]

I allow for match effects on student GPA for two reasons. First, it makes local comparisons across schools actually relevant to a particular group of students: the experiment of moving an academically weak student to the most selective school is both infeasible in practice, given a

[18]The 25th percentile of the cell size distribution is 25, and the 75th percentile is 181. This is comparable to school-by-year cell sizes in settings such as the NYC public high school matching system, where the average is 109 students (Abdulkadiroğlu et al. 2022).

GPA-based admission system, and very difficult to identify without strong assumptions. Second, whether schools enrolling weak students specialize in programs suited to them, for instance, is an important question in its own right. The inclusion of match effects allows me to investigate this directly. Fixed effects for students' GPA tercile, α_g, ensures that differences in school-by-program effects β_{jpg} are not interpreted as the 'effect' of moving students across GPA terciles, which would require substantially stronger assumptions than invoked here.

The model in Equation (1) allows me to decompose statistical estimates of β_{jpg} into several components of interest. In particular, we can write β_{jpg} as a sum of school-, program- and student-specific treatment effects and their interactions:

$$\underbrace{\beta_{jpg}}_{\text{Total effect}} = \underbrace{\theta_{jg}}_{\text{School effect}} + \underbrace{\gamma_{pg}}_{\text{Program effect}} + \underbrace{\nu_{jpg}}_{\substack{\text{School}\times\text{program}\\ \text{effects}}} . \tag{2}$$

Here, θ_{jg} captures the return to attending school j common to all students in GPA tercile g, irrespective of program enrollment. This *school effectiveness* could reflect, for example, school-wide features such as average teacher quality or the student-teacher ratio, which could differentially benefit students of different types. Correspondingly, γ_{pg} capture market-wide returns to study programs of students in GPA tercile g, referred to as *program effectiveness*. These can be interpreted as school-invariant returns to different high school curricula. Finally, ν_{jpg} capture school- and program specific returns and match effects. For instance, these would pick up potential comparative advantages across schools in providing certain programs.

When comparing groups of schools, such as charter vs. public schools, I weigh β_{jpg} by the number of enrolled students in each (j, p, g) cell. This is equivalent to assigning each student the estimated effect of the school and program s/he enrolled in, and computing averages across charter and public students. For instance, the average difference in effectiveness between charter and public schools is:

$$\frac{1}{N_c} \sum_{i \in \mathcal{C}} D_{ijp} \beta_{jpg} - \frac{1}{N_o} \sum_{i \in \mathcal{O}} D_{ijp} \beta_{jpg}, \tag{3}$$

where $\mathcal{C}$ and $\mathcal{O}$ are sets containing all students enrolled in charter and public schools, respectively, and D_{ijp} is a dummy for enrollment in school j and program p. This is the same difference in means that we would get by replacing the school-by-program indicators in Equation (1) with an indicator for charter vs. public school enrollment.

3.2 Identification

The key challenge in identifying β_{jpg} is that student enrollment might well be correlated with unobserved determinants of future earnings, contained in ϵ_{ijp}. Here, I will rely on the assumption that student enrollment is random with respect to potential outcomes conditional on the vector of predetermined characteristics $\mathbf{X}_i$.

ASSUMPTION 1: *Selection-on-observables*

$$E[\epsilon_i|\mathbf{X}_i, D_{ijp}] = E[\epsilon_i|\mathbf{X}_i] \quad \forall j, p \tag{4}$$

Assumption 1 implies that unobserved determinants of earnings are uncorrelated with student enrollment conditional on the large set of predetermined characteristics contained in $\mathbf{X}_i$. Much work has been devoted to testing this assumption in the context of schooling markets. This literature generally finds that value-added models on test score outcomes, while not perfectly unbiased, strongly predict causal estimates.[19] However, we know substantially less about the performance of observational models on outcomes beyond test scores. To address this, I use credibly exogenous variation around admission cutoffs to validate whether observational estimates provide unbiased impacts on college and labor market outcomes.

3.3 Regression Discontinuity Validation of Observational Estimates

The Swedish setting provides a way to test Assumption 1 directly for a subset of my sample. In particular, I exploit variation generated by admission cutoffs in a regression discontinuity (RD) design and test whether it aligns with the observational variables estimates in an instrumental variables testing regression (Angrist et al. 2017; Angrist et al. 2024). I validate school-by-program effects on predicted lifetime earnings based on graduation, college and labor market outcomes measured at the age of 23.[20] This is because data from the centralized admission system, required for the validation exercise, is only available from 2013 and onward where long-run earnings are not observed. The resulting sample covers three admission rounds between 2013 and 2015.

Credibly exogenous variation in student admissions. Central to this validation strategy is that students are admitted to schools and programs based on sharp admission cutoffs. In the spring before Grade 9 graduation, students submit a rank-ordered list of school and program combinations that they wish to attend, without limits on list length. They are then ranked based on their Grade 9 GPA and admitted to schools and programs in order until capacity is filled. For oversubscribed school and program combinations, this mechanism generates admission cutoffs defined by the GPA of the last admitted student.

In particular, denote the admission status of student i to school j and program p as Z_{ijp}. This variable is not randomly assigned, since students choose what to apply for and are admitted based

[19] This represents a variety of validation efforts spanning a wide range of contexts. These include lottery validations of individual school value-added (Deming 2014; Angrist et al. 2017; Angrist et al. 2024), teacher value-added (Kane & Staiger 2008; Chetty et al. 2014), mover design approaches (Andrabi et al. 2025), dynamic panel data estimators (Andrabi et al. 2011), RD estimates based on enrollment rules (Singh 2020), or comparisons between value-added and experimental estimates (Singh 2015; Muralidharan & Sundararaman 2015). Even in settings where such comparisons are not feasible, bounding exercises in the spirit of Oster (2019) or Cinelli & Hazlett (2019) indicate that the extent of bias required to explain variation in VA is likely implausibly large (see, e.g., Singh et al. 2022; Berg et al. 2025).

[20] I predict students' average earnings rank between 35 and 37 using data on high school graduation, college enrollment, and labor market earnings histories up until the age of 23 (see Appendix C for details). Insofar as my OLS effects on this measure of predicted earnings are unbiased, this lends credibility to the assumption that they are also unbiased for actual earnings impacts. Further unobserved selection would have to stem from factors that predict school and program choice at age 16, as well as earnings at age 30, but do not materialize in college or labor market outcomes at age 23. While this is possible, the strongest omitted factors are likely related to both short- and long-run outcomes.

on their grades. However, Abdulkadiroğlu et al. (2022) show that exogenous variation in Z_{ijp} around the admission cutoff can be isolated by conditioning on the expected probability of admission, denoted by p_{ijp}. This propensity score is a function of the student's preference ranking, GPA, the admission cutoff of (j, p) and an RD bandwidth parameter λ (see Appendix D for a detailed derivation). Under the assumption that students close (λ) to the admission cutoffs are as good as randomly admitted, conditioning on p_{ijp} ensures that I am only comparing students with the same expected chances of admission at a particular school and program.

Balance of admission offers. Admission offers should be unrelated to predetermined student characteristics when conditioning on propensity scores and student GPA (i.e., the RD running variable). In Figure 4, I evaluate this by regressing an index of student background characteristics, weighted by their importance in predicting long-run earnings, on dummies for admission into different ventiles of the distribution of estimated school-by-program effects.[21] Without controls, students admitted to more effective schools and programs are positively selected on predictors of earnings (black markers and line). However, conditioning only on propensity scores and GPA removes this pattern entirely (red).

As a second test for balance, I regress each predetermined student characteristic on the estimated effect of students' admitting alternatives (Table A.2). Students admitted to more effective schools and programs are more likely to be male, born in Sweden, and attend vocational tracks in public schools. However, the differences largely disappear when controlling for students' GPA and expected value-added, in the form of a propensity score-weighted average of the effectiveness of a student's ranked options (Angrist et al. 2024). These tests for balance lend credibility to the identifying assumption that admission is as good as randomly assigned to students who are close to the admission cutoffs.

Validation of observational estimates. I use this exogenous variation around admission cutoffs to test the validity of my observational estimates in an instrumental variables specification (Angrist et al. 2017). The estimated effect of the school and program that student i enrolls in should—if unbiased—predict student i's actual outcomes with a coefficient of 1:

$$Y_i = \alpha + \kappa \hat{\beta}_i + u_i, \tag{5}$$

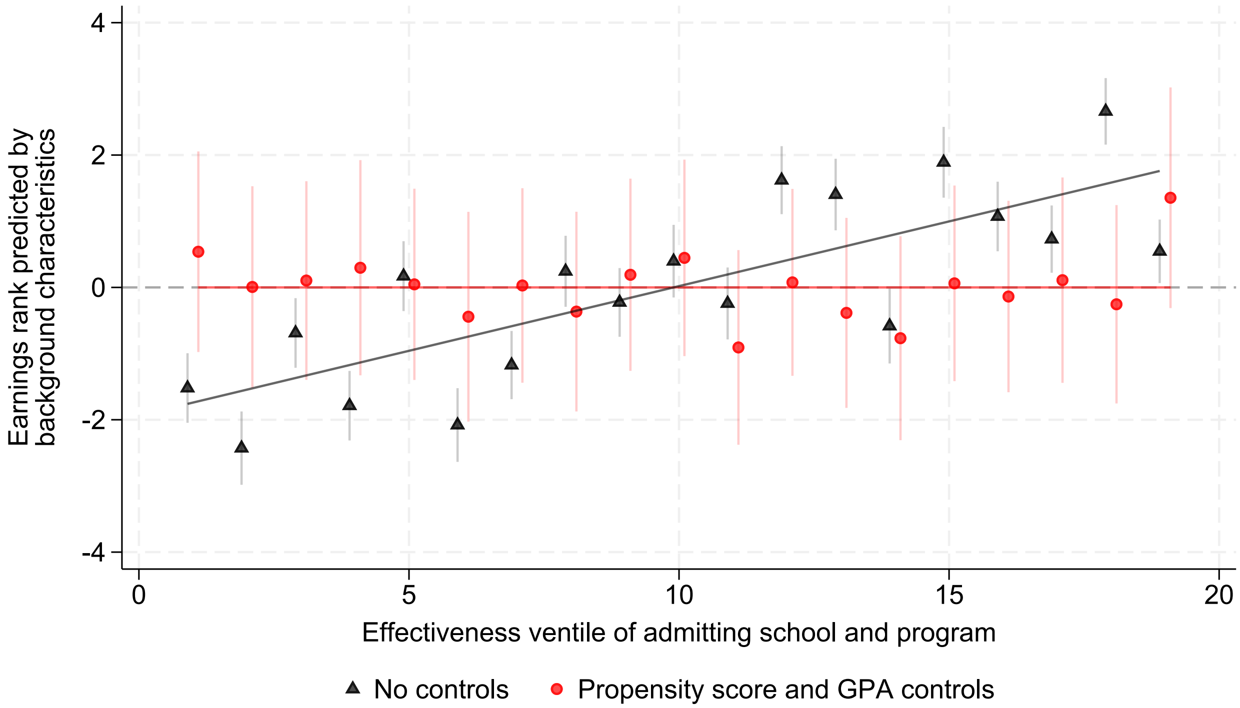
where Y_i is lifetime earnings predicted by outcomes at the age of 23, $\hat{\beta}_i$ is the estimated effect of the school and program that student i actually enrolled in, and κ is referred to as a forecast coefficient.[22] The null hypothesis, $\kappa = 1$, means that a one-unit increase in effectiveness of the school and program that student i is enrolled in increases his or her actual outcomes by the same amount—implying that the observational estimates are unbiased on average.

Admission offers can be used as instruments for $\hat{\beta}_i$ in Equation (5) by providing exogenous variation in where students are admitted. The identifying variation in this framework comes from students

[21] Admission offers and propensity scores for a particular group of schools—such as the least or most effective ventile—are computed simply by summing over all individual schools and programs in that group (Angrist et al. 2024).

[22] Formally, $\hat{\beta}_i = \hat{\beta}_{jpg}$ with j, p s.t. $D_{ijp} = 1$.

Figure 4: Balance of binned admission offers



Notes: This figure shows estimated coefficients and 95% confidence intervals from a regression of an index of predetermined characteristics on the set of bin-level admission dummies Z_{iv}, capturing admission into ventiles of the school and program effect distribution. The index are the fitted values from a regression of earnings rank at 30 on a large set of predetermined student characteristics ($\mathbf{X}_i$ in Equation 1). The black line and markers show estimates from a regression without any controls, except for year fixed effects. The purple line and markers show the corresponding estimates when conditioning on bin-level propensity scores as well as a cubic in Grade 9 GPA, the running variable in the high school admissions procedure.

facing admission risk, meaning those with a probability of admission to some ranked alternative strictly between 0 and 1. In principle, the estimated average effect of attending a particular school and program could differ from the effect among these marginal students. This would move κ away from 1 in Equation (5), conflating heterogeneous treatment effects with selection bias. In Table A.1, I show that the sample of at-risk students is broadly similar to the full sample, limiting this concern. Moreover, all programs and virtually all schools had some students facing admission risk over the three admission rounds I study (2013–2015). Hence, the validation results are not limited to a small subset of schools and programs.

The first stage and reduced form regressions of the IV test is given by:

$$\hat{\beta}_i = a + \sum_v [\pi_v Z_{iv} + f(p_{iv})] + g(GPA_i) + \mathbf{\Omega X_i} + \xi_i \tag{6}$$

$$Y_i = b + \sum_v [\psi_v Z_{iv} + f(p_{iv})] + g(GPA_i) + \mathbf{\Psi X_i} + \epsilon_i, \tag{7}$$

where Z_{iv} and p_{iv} are admission offers and propensity scores aggregated at ventiles of the effect distribution (as in Figure 4), GPA_i is the student's Grade 9 GPA, and $\mathbf{X}_i$ is the same vector of controls as in Equation (1) included to improve precision.[23] Intuitively, π_v tells us how admission to a school–program in ventile v affects the predicted effect for student i, while ψ_v tells us how it affects actual outcomes. Equality of π_v and ψ_v implies that the observational and RD estimates of effectiveness coincide and that the forecast coefficient in Equation (5) is equal to 1.[24]

Figure 5 plots reduced form estimates against the corresponding first stage estimates for each admission instrument Z_{iv}. The left panel shows that, without student background controls, the school-by-program effects are severely biased away from the RD estimates. The forecast coefficient is equal to 0.23 and statistically significantly different from 1 at all conventional levels. The inclusion of controls removes this bias entirely. The right panel shows the equivalent test when using my main estimates from Equation (1), which yields a forecast coefficient of 0.99 that is statistically indistinguishable from 1. This result is not driven by the artifacts of the predicted lifetime earnings measure: the forecast coefficients for its components—high school graduation, college enrollment, and earnings rank at 23—are all close to 1 (Figure D.2).

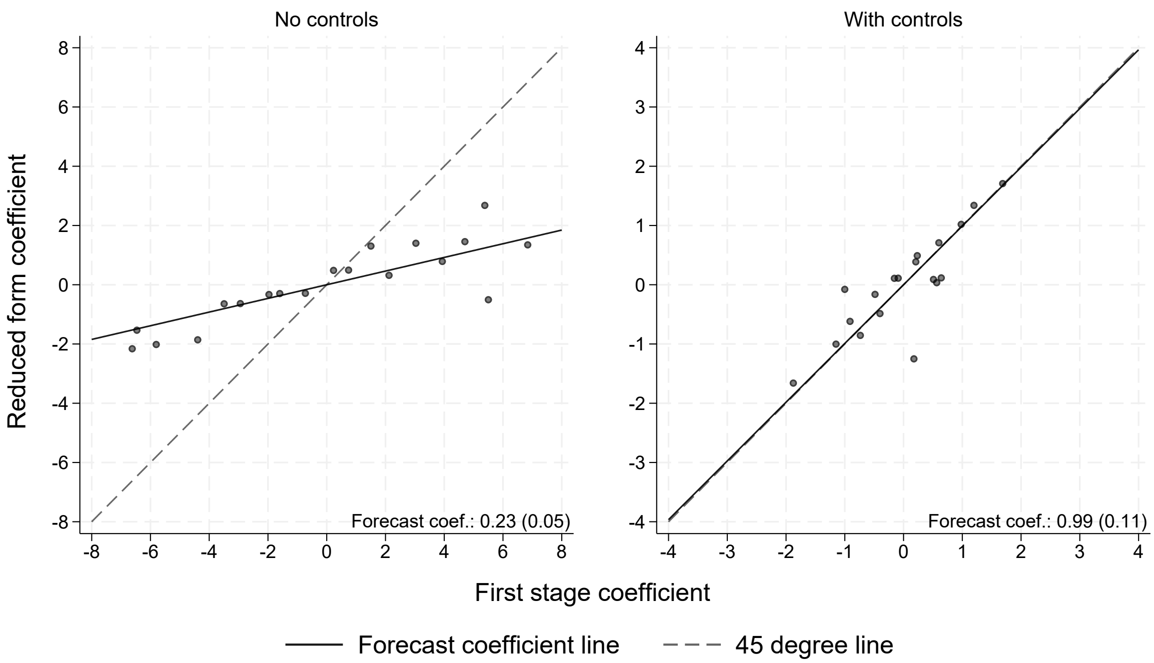
Forecast unbiasedness is still consistent with large biases among groups of schools or programs that cancel out on average. Such biases would appear as points deviating from the 45-degree line in Figure 5. Although there is little visual evidence of this in my controlled estimates, I formally test for it using the "omnibus" test approach of Angrist et al. (2017), which asks if π_v and ψ_v are equal for *all* of the instruments in an overidentification-style test (Table 4). This test strongly rejects unbiasedness of the uncontrolled estimates, but not the controlled estimates (Columns 1-2). When relying on admission offers and propensity score at the school-by-program level, rather than aggregating to ventile bins, I find similar point estimates for the forecast coefficient (Columns 3-4). However, this approach is noisy and produces a weak first stage ($F = 5.89$), leading the omnibus test to reject unbiasedness. This could happen if a non-trivial share of students are on the margin between two relatively similar alternatives in terms of effectiveness. The bin-level IV overcomes this by defining instruments as admission to more or less effective options, which produces a much stronger first stage ($F = 279$).

Together, these results suggest that the rich battery of student background controls addresses most of the selection bias in mean differences in outcomes across schools and programs. In the next section, I use my estimates to investigate differences in earnings impacts between charter and public schools.

[23] Aggregating admissions to bins of the effectiveness distribution improves the power of the test (Angrist et al. 2024). Propensity scores enter linearly in $f(\cdot)$, but I additionally include a set of binary controls for $p_{iv} = 0$ which mostly consists of cases where a student did not rank any school in effectiveness ventile v. $g(\cdot)$ controls for a cubic polynomial in Grade 9 GPA. The validation results are highly robust to using alternative numbers of bins and RD bandwidth parameters λ, controlling non-parametrically for propensity scores and GPA, and restricting the validation exercise to large school-by-program cells effects are more precisely estimated (see Appendix D.4).

[24] The implication that $\kappa = 1$ comes from the fact that the 2SLS second stage equation, implied by Equations (6-7), identifies the impact of $\hat{\beta}_i$ on Y_i when instrumenting with the admission offers. If the first stage and reduced form coefficients on the instruments are equal, the coefficient on $\hat{\beta}_i$ will be equal to 1.

Figure 5: IV test for bias in observational school and program effects, visualized



Notes: These figures visualize the IV test of unbiasedness in Equations (6) and (7), using the aggregated instruments for admission to a school and program in the vth ventile of the distribution of impacts on predicted earnings (Z_{iv}). The vertical axes show the (first-stage) coefficients from a regression of the OLS effectiveness estimate of the alternative that the student enrolled in, on the instruments Z_{iv}, bin-level propensity scores, GPA and student background characteristics to improve precision. The vertical axes show estimates from a (reduced form) regression of actual, predicted earnings on the same set of covariates. In the left plot, I use raw school-by-program outcome means as measures of effectiveness. The right plot uses the estimates from the main model in Equation (1). The solid lines show the estimated forecast coefficient (κ) from a 2SLS regression of predicted earnings on estimated effectiveness, instrumented by admission offers. For further test statistics, see Table 4.

Table 4: IV test for bias in observational school-by-program effects

Instrument type:	Binned		School-by-program	
Effectiveness measure:	Outcome means	OLS	Outcome means	OLS
Forecast coefficient (κ)	0.23	0.99	0.20	0.87
	(0.05)	(0.11)	(0.06)	(0.15)
p-value ($\kappa = 1$)	0.000	0.938	0.000	0.364
p-value (Omnibus test)	0.000	0.799	0.000	0.016
Number of instruments	19	19	352	352
First-stage F-statistic	375.14	278.57	7.35	5.89
Share facing admission risk	0.22	0.22	0.23	0.23
Observations	42792	42792	42792	42792

Notes: This table shows the results of the IV test for validity of school-by-program impacts on predicted earnings (Equations 6 and 7). The first two columns use binned instruments for admission to an alternative in the vth ventile of the distribution of effectiveness, with the first ventile being the omitted category. The last two columns use raw, school-by-program admission dummies as instruments. I test validity of two measures of effectiveness: raw outcome means (columns 1 and 3) and the main observational impacts (columns 2 and 4). A student facing admission risk has a propensity score (as defined in Appendix D.1) that is strictly between 0 and 1 for one of their ranked alternatives.

4 The Effectiveness of For- and Non-profit Charter Schools

In this section, I first compare average earnings impacts between charter and public schools. I then decompose these impacts into school- and program-related components to separate the role of school-level practices from specialization into fields of study. Next, I examine whether charter schools are more or less effective for students with different levels of academic achievement. Finally, I investigate whether differences in school effectiveness can be explained by teacher inputs, such as hiring practices, to shed light on the mechanisms behind the observed gaps.

4.1 Charter School Effectiveness

I begin by investigating average differences in estimated effects ($\hat{\beta}_{jpg}$) between students attending charter and public schools on high school graduation, college enrollment and labor market earnings at age 23 (Table 5).

Differences between charter and public schools are evident already in the short term (Panel A). Attending a charter reduces on-time high school graduation by 2.3 percentage points (p.p.), and college enrollment by 3.2p.p. These effects are meaningful relative to the average graduation and college enrollment rates in public schools (71% and 40%, respectively). Despite lower college attendance among charter students, I find no evidence for a compensatory increase in labor earnings on average.

Table 5: Average effectiveness of charter relative to public schools on short-run outcomes at 23

	High school graduation	College enrollment	Earnings at 23	
	%	%	Rank	Levels
Panel A: All charters				
Charter	-2.27***	-3.20***	-0.37	-17.63
	(0.56)	(0.37)	(0.25)	(12.42)
Panel B: For-/non-profits				
For-profit	-2.94***	-3.63***	0.64**	36.59**
	(0.76)	(0.45)	(0.29)	(14.19)
Non-profit	-1.23**	-2.54***	-1.92***	-100.96***
	(0.56)	(0.49)	(0.34)	(16.35)
For-/non-profit equal (p-value)	0.05	0.07	0.00	0.00
Public outcome mean	71	40	50	1695
SD of OLS impacts	8.36	8.03	5.59	276.90
– bias-corrected‡	7.71	7.33	5.15	257.17
Number of jpg cells	1043	1043	1043	1043
Number of students	194266	194266	194266	194266

Notes: $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *. This table shows mean differences in estimated OLS impacts between for-profit charter schools and public schools, with standard errors clustered at the school-by-year level in parentheses. Columns 1 and 2 show impacts on high school graduation and college enrollment by the age of 23. Columns 3 and 4 show impacts on student earnings i) rank and ii) level at 23.

‡The sample standard deviation of estimated effectiveness will be inflated due to estimation noise. I correct for this using the approach of Kline et al. (2020) (see Appendix E for details).

I distinguish between for- and non-profit charters in Panel B. For-profits, which make up the majority of the charter sector, reduce both high school graduation and college enrollment more than non-profits. The opposite is true for earnings, which is expected if those less likely to attend college are more likely to work. Relative to public school students, though, the increase in labor earnings of for-profit students is modest (0.64 percentiles) given the substantial, negative impact on college enrollment (-3.63p.p. from a public school mean of 40%).

These findings are, in principle, consistent with a range of different effects of charter attendance on long-run earnings—including zero—depending on the size of the high school and college premium.[25] In Table 6, however, I document a sharply negative effect. On average, students attending charter schools score -1.11 percentiles lower in the earnings distribution at age 30 (Panel A, Column 1). In levels, this corresponds to a decrease in absolute earnings of 7500 SEK ($750) annually, or 2% at the sample mean.[26] As a comparison, Mincer returns to years of schooling in Sweden have previously been estimated at around 3–5% (Palme & Wright 1998; Björklund & Kjellström 2002), and similar exercises in my sample yield an estimated return of around 3%. Benchmarked against the distribution of earnings effects, the charter–public gap amounts to around 30% of a standard deviation. In Panel B, I find that non-profits are even less effective at boosting earnings compared to for-profits. On average, attending a for-profit charter reduces earnings by 0.87 percentiles, relative to 1.47 percentiles for non-profits. However, given the fairly imprecise estimate of non-profit effectiveness, the difference is not statistically significant ($p = 0.24$).

I find similar results when turning to a broader measure of long-run outcomes. Columns 3–4 report average effects on predicted lifetime earnings, constructed by combining students' observed earnings and college trajectories up to age 30 to predict average earnings between ages 35 and 37 (see Section 2.3). The charter–public school gap remains large and negative, with point estimates closely tracking those on earnings at age 30. This suggests that the effects are not driven by temporary fluctuations but reflect meaningful differences in long-term economic trajectories.[27]

The negative impact on earnings associated with charter school attendance is meaningful both when benchmarking against for-profit profits—a measure of cost-savings—and the cost of high school provision. Taking the estimates from Table 6, I calculate a back-of-the-envelope lifetime loss of about 360,000 SEK per student over 45 years of work, or 193,000 SEK in present value assuming a 3% discount rate. Official sources report profit margins of around 5% in for-profit charters (SVT

[25] The college premium in Sweden is low in a global comparison. The wage premium of a master's degree is 43% in Sweden, relative to 76% in Germany and 118% in the U.S. (OECD 2025).

[26] Previous studies show that Swedish charter schools are more lenient in grading, leading to inflated scores with which students later apply to university (Tyrefors & Vlachos 2017; Edmark & Persson 2021). Inflated grades have also been associated with higher labor market returns (Nordin et al. 2019). My estimates therefore capture such grading leniency as part of the treatment effect of attending a charter, and should be interpreted as an upper bound on the "true" effectiveness of charters in the absence of grading leniency.

[27] The charter–public gap in effectiveness on predicted lifetime earnings is strikingly similar when relying only on outcomes up until the age of 23—the same measure used in the validation exercise in Section 3.3 (Table A.3). This suggests that the negative charter effects manifests early in life through high school graduation rates, college enrollment, and early labor market outcomes. Combined with the validation evidence showing that the estimated effects on this short-run measure of predicted lifetime earnings are unbiased, this results provide further support that the estimated charter–public differences in long-run earnings are not driven by selection bias.

Table 6: Average effectiveness of charter relative to public schools

	Earnings at 30		Predicted lifetime earnings	
	Rank	Levels	Rank	Levels
Panel A: All charters				
Charter	-1.11***	-74.48***	-1.00***	-79.66***
	(0.26)	(18.72)	(0.18)	(14.94)
Panel B: For-/non-profits				
For-profit	-0.87***	-60.47***	-0.77***	-62.22***
	(0.31)	(21.87)	(0.21)	(17.59)
Non-profit	-1.47***	-96.00***	-1.37***	-106.46***
	(0.43)	(31.20)	(0.29)	(24.38)
For-/non-profit equal (p-value)	0.24	0.34	0.08	0.13
Public outcome mean	50	3730	51	4852
SD of OLS impacts	4.38	297.21	3.01	242.28
– bias-corrected‡	3.83	256.11	2.73	218.04
Number of *jpg* cells	1043	1043	1043	1043
Number of students	194266	194266	194266	194266

Notes: $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *. This table shows mean differences in estimated OLS impacts between for-profit charter schools and public schools, with standard errors clustered at the school-by-year level in parentheses. Columns 1 and 2 show impacts on students' earnings rank and level at age 30. Earnings are measured annually, and expressed in 100 Swedish kronor (SEK). Columns 2 and 3 show impacts on predicted lifetime earnings. This prediction is based on a regression of average earnings between 35 and 37 on students' earnings and educational histories up until the age of 30 (see Appendix C for details).
†The sample standard deviation of estimated effectiveness will be inflated due to estimation noise. I correct for this using the approach of Kline et al. (2020) (see Appendix E for details).

Nyheter 2024; Friskolornas riksförbund 2024), while the average funding is around 300,000 SEK for three years of high school education. This results in a per-student profit on the order of 15,000 SEK ($1,500). As such, students' lifetime earnings losses substantially outweigh the gains in the form of profits. Further, a municipal tax rate of around 32% (SCB 2024) the present value of foregone tax revenue is about 61,000 SEK per student, equivalent to 20% of the cost of three years of high school. Although simplified and disregarding potential general equilibrium effects, these calculations imply that assigning a student to a public rather than a charter seat reduces costs by about 20%.

4.2 The Roles of Schools and Programs

The negative impact on earnings of attending a charter school may be driven by school-level features, such as teacher inputs, or program specialization. For instance, charter schools might offer effective teaching but specialize in low-return fields, or vice versa. Distinguishing between these channels has important policy implications. A social planner may wish to preserve access to programs with high social value but low earnings potential, while prioritizing school-level effectiveness in delivering those programs.

I investigate this by decomposing the estimated earnings impacts into separable school-and program components, as shown in Equation (2):

$$\hat{\beta}_{jpg} = \sum_j \theta_{jg} D_{ij} + \sum_p \gamma_{pg} D_{ip} + \nu_{jpg}, \tag{8}$$

where D_{ij} and D_{ip} equal 1 if student i enrolled in school j and program p, respectively. In this decomposition, the estimates ($\hat{\theta}_{jg}$,$\hat{\gamma}_{pg}$) capture additively separable school and program effects, and $\hat{\nu}_{jpg}$ are the estimated residuals.

Intuitively, $\hat{\theta}_{jg}$ captures the effectiveness of school j for students in GPA tercile g, common across all programs in that school. Likewise, $\hat{\gamma}_{pg}$ reflect market-level returns to enrolling in program p for students in tercile g. These is equivalent to what is commonly referred to as institution and field-of-study effects in higher education (Kirkeboen et al. 2016). Finally, $\hat{\nu}_{jpg}$ picks up more complicated interactions between students, schools and programs—such as comparative advantages in providing certain programs for certain schools. A variance decomposition shows that almost all of the variation in $\hat{\beta}_{jpg}$ is explained by separable school and program effects, indicating that residual interactions play only a minor role (see Appendix E).[28]

While I find that both for- and non-profit charters reduce earnings relative to public schools, the sources of these effects are entirely distinct (Table 7). For non-profit charters, the negative impact on earnings is fully driven by a disproportionate focus on low-return programs. In terms of school-level effectiveness, they are as or even slightly more effective than public schools on average.[29] In contrast, for-profit charters' negative impact on earnings is driven by lower school-level effectiveness rather than program specialization.

This provides stark evidence that for- and non-profits are not only differentiated by ownership, but also by the features that determine their effects on student outcomes. I provide suggestive evidence of what these features are in Section 4.4. Before doing so, however, it is important to recognize that charters may well be effective for some group of students while being less effective overall. Such heterogeneity could arise either because students are attending *different* charter schools, or because a given school may not be equally effective for all types of students. I turn to this in the following section.

4.3 Charter Effects Across the Grade Distribution

The effect of attending a for- or non-profit charter school may differ across students for two primary reasons. First, students attend different schools that could differ from each other in terms of both

[28] To assess the relative importance of schools and programs in explaining variation in total earnings impacts, I adapt the bias-correction method of Kline et al. (2020) to account for estimation noise in both the total impacts and their decomposed school and program components. After correction, separable school and program effects together explain almost all of the variation in total effectiveness: residual interactions account for only about 1.6 percent. At the student level, variation in program returns is larger than variation in school effectiveness, and the two components are negatively correlated.

[29] In particular, non-profit charters are significantly more likely to offer arts programs compared to public schools, which are associated with weak labor market returns (Figure A.3). For-profit charters tend to avoid low-return vocational programs (care & services) in favor of high-return, manufacturing-related tracks. However, this is largely canceled out by a lesser focus on relatively high-return STEM academic tracks (natural sciences and technology) in favor of social sciences.

Table 7: School and program effects on earnings rank at 30 in charter and public schools

	For-profit vs. public	Non-profit vs. public	Difference
School effects (θ_{jg})	-0.98***	0.44*	-1.43
	(0.26)	(0.26)	(p=0.00)
Program effects (γ_{pg})	0.08	-1.97***	2.05
	(0.18)	(0.23)	(p=0.00)
Net (school + program)	-0.90***	-1.52***	0.62
	(0.31)	(0.43)	(p=0.22)
N. *jpg* cells	1043	1043	
N. students	194266	194266	

Notes: $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *. This table shows mean differences in realized school (θ_{pg}) and program effects (γ_{pg}) on earnings rank at age 30 for students in for-profit (column 1) and non-profit (column 2) charter schools, relative to public schools. Column 3 shows the difference between point estimates in columns 1 and 2, with the associated p-value in parentheses. School and program effects are defined as in Equation 8. Standard errors are clustered at the school-by-year level.

school effectiveness and program specialization. Second, students may benefit differently from attending one and the same school or program: for instance, a particular school may be effective at boosting outcomes for low-GPA students, but less so for those with a high GPA. I refer to these concepts as school and program heterogeneity and match effects, respectively.[30] I focus on heterogeneity by students' Grade 9 GPA, a broad measure of academic ability at high school entry that naturally segments the market by determining admissions.

Figure 6 shows the difference in total effects ($\hat{\beta}_{jpg}$), school effects ($\hat{\theta}_{jg}$) and program effects ($\hat{\gamma}_{pg}$) between charter and public schools, separately by GPA tercile g.[31] I also include coefficients from a simpler model that does not allow for match effects on GPA, denoted $\hat{\beta}_{jp}$, $\hat{\theta}_j$ and $\hat{\gamma}_p$. Hence, this figure directly shows i) the extent to which charter effects differ across the GPA distribution and ii) how important match effects are in explaining such differences.

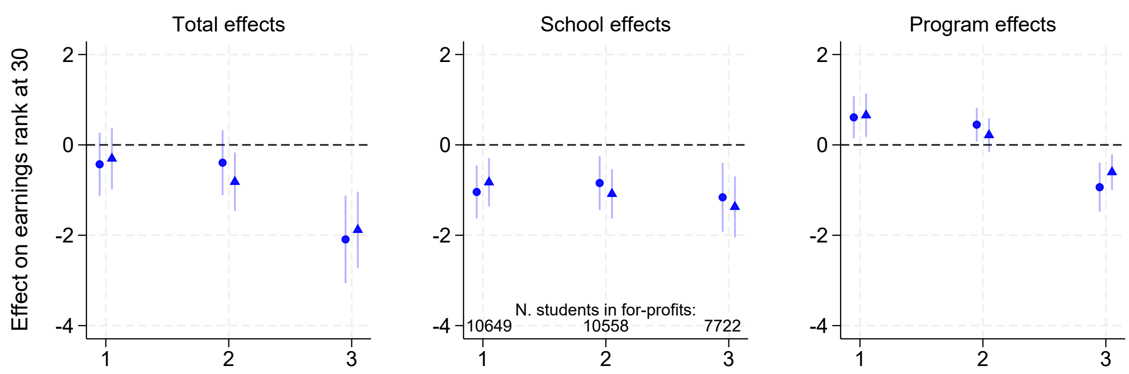
First, I find that charter impacts are indeed heterogeneous across the student GPA distribution. For-profit charters are almost as effective as public schools at boosting long-run earnings for students in the bottom two terciles of the GPA distribution, but significantly worse for the top tercile. This is entirely driven by program specialization; low-GPA students in for-profits attend higher-return programs than their peers in public schools, while the opposite is true for high-GPA students. Notably, the gap in school effectiveness between for-profits and public schools is negative and remarkably stable across the student distribution. In contrast, non-profit charters are more effective for academically strong students. This is driven both by higher school effectiveness *and* specialization into higher-return programs compared to public schools.

[30] This distinction is very similar to that of firm heterogeneity and match quality when trying to, for example, explain wage dispersion within a given industry. Relatedly, it plays an important role in AKM models of wage determination; see, e.g., Abowd et al. (1999) and Card et al. (2018).

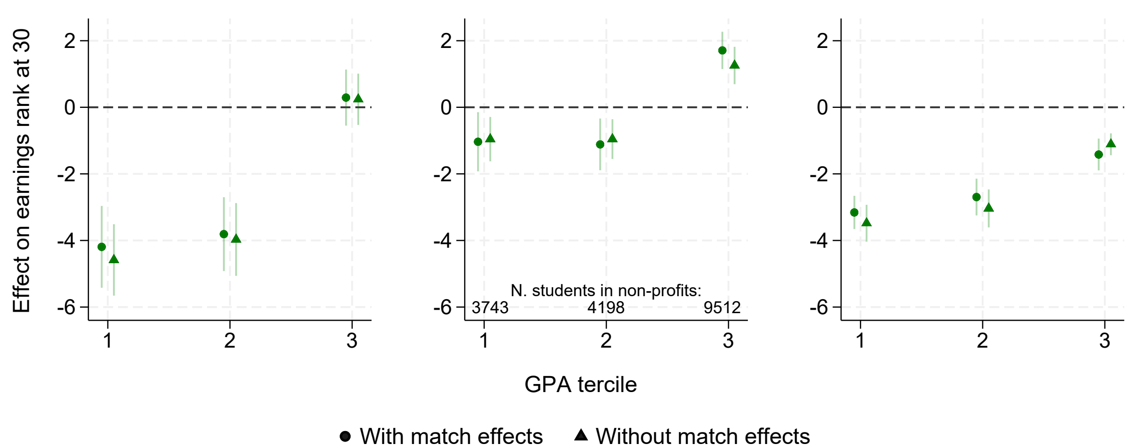
[31] In particular, I am comparing the average difference in estimated effectiveness across charter and public schools, weighted by their enrollment of students in GPA percentile p. As such, this approach makes a local comparison between charter and public schools enrolling similar students in terms of GPA.

Figure 6: Effectiveness by sector and student GPA

Panel A: For-profits vs. public schools



Panel B: Non-profits vs. public schools



Notes: These show the average i) total effect, ii) school effect and iii) program effect in charter relative to public schools, separately by terciles of the student Grade 9 GPA distribution. Each point estimate, and 95% confidence interval, come from a separate regression on total/school/program effects on for-profit and non-profit indicators, restricted to students in each respective GPA tercile. I show estimates of these differences when allowing for match effects (β_{jpg}, θ_{jg} and γ_{pg} for total, school and program effects, respectively), and when relying on a simpler model where the effect is assumed to be homogeneous across students.

Second, these patterns have very little to do with match effects. The estimates change only trivially when relying on a model that restricts all estimated school and program effects to be homogeneous across students. This indicates that heterogeneity in charter effects is mostly driven by school and program heterogeneity—i.e., the fact that students attend different schools and programs—rather than match effects. The finding that match effects play only a minor role in explaining differences in school effects is consistent with recent evidence from other contexts.[32]

[32]See, e.g., Abdulkadiroğlu et al. (2025) for NYC high schools and Mountjoy & Hickman (2021) for universities in Texas.

4.4 Explaining School Effectiveness

The descriptive patterns in Table 2 suggest that differences in school-level effectiveness between for-profit charters and public schools may, in part, be driven by variation in teacher inputs. A common critique of charter schools—particularly for-profits—is their reliance on unlicensed or non-permanently employed teachers to reduce labor costs (Letmark 2022). If teacher qualifications are predictive of school effectiveness, such staffing practices could account for the gap between for-profit charters and public schools documented in Section 4.2.

Interpreting differences in school effectiveness through potential mediators, such as teacher characteristics, is inherently challenging (Imai et al. 2010; Heckman & Pinto 2015). In particular, mediation analyses risk conflating mechanisms with correlated inputs, since both ownership form and staffing decisions are likely endogenous to other organizational decisions that also affect school effectiveness. A first step, though, is testing whether school effectiveness correlates with teacher inputs at all. Table 8 shows that sequentially adding controls for teacher characteristics at the school level fully explains the gap in school effectiveness between for-profits and public schools. Among the included variables, the share of teachers with a college degree appears to play the largest role in closing the gap. This association should not be interpreted as causal, but it suggests that teacher qualifications are closely correlated with the underlying determinants of school effectiveness.

In order to get closer to a causal interpretation, however, I am able to leverage within-school variation in teacher inputs and effectiveness over time using a two-way fixed effects approach (TWFE). I estimate the following regression:

$$\hat{\theta}_{jt} = \mathbf{\Phi T}_{jt} + \lambda_j + \lambda_t + e_{jt}, \tag{9}$$

where $\hat{\theta}_{jt}$ is the estimated earnings effect of school j in year t from the same decomposition of school-by-program effects as in Section 4.2. The vector $\mathbf{T}_{jt}$ collects school-level teacher inputs, λ_j is a school fixed effect, and λ_t is a year fixed effect.

I report results in the last column of Table 8. The coefficient on the share of teachers with a teaching degree remains large and strongly statistically significant at 2.98. Hence, a 10 percentage point increase in degreed teachers is associated with a 0.3 percentile increase in long-run earnings. This closely aligns with the observed gap in effectiveness between for-profit charters and public schools. For-profits have 31 percentage points fewer degreed teachers compared to public schools (Table 2) which would imply a gap in effectiveness equal to $0.31 \cdot 2.98 = 0.92$. This is identical to what I find in Column 1 of Table 8.

5 Why Do Students Choose Charter Schools?

Despite reducing long-run earnings relative to public schools, charter schools—driven entirely by for-profits—have captured a substantial and growing share of the high school market (Figure 2). Two key hypotheses could explain this pattern. First, students may choose

Table 8: School effects and teacher inputs

	Dep. var.: school effectiveness						
							TWFE
For-profit	-0.92***	-1.23***	-0.95***	-0.64**	-0.62**	0.09	
	(0.25)	(0.27)	(0.31)	(0.32)	(0.32)	(0.33)	
Non-profit	0.40	0.41	0.62**	1.09***	0.59*	1.01***	
	(0.27)	(0.27)	(0.30)	(0.31)	(0.31)	(0.30)	
Students per teacher		0.05**	0.06**	0.06**	0.05**	0.04*	0.01
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Average teacher age			0.03	0.05**	0.01	-0.05**	-0.05
			(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Share full-time employed				2.49***	1.65***	1.63***	0.92*
				(0.48)	(0.48)	(0.47)	(0.51)
Share permanently employed					4.80***	3.47***	-0.41
					(0.77)	(0.77)	(0.60)
Share with teaching degree						4.63***	2.98***
						(0.87)	(0.89)
School FE	No	No	No	No	No	No	Yes
Year FE	No	No	No	No	No	No	Yes
Number of unique schools	165	165	165	165	165	165	165
Number of students	190853	190853	190853	190853	190853	190853	190853

Notes: $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *. This table shows estimates from a regression of school effects (θ_{jg}), as defined in Equation (8), on for- and non-profit charters relative to public schools, and a set of teacher-related variables for each school. Data are pooled over 1995–2008, year fixed effects are included in all regressions, and standard errors are clustered at the school-by-year level. The last column (TWFE) additionally controls for school fixed effects, to leverage within-school variation in effectiveness and teacher inputs over time. Since for-/non-profit charter status does not change over time, these are omitted under this specification: the focus lies solely on gauging the relationship between teacher inputs and school effectiveness.

schools based on characteristics other than earnings impacts.[33] If for-profit charter schools are effective at catering to student demand, one would expect them to gain market shares in a competitive market. Moreover, if raising school effectiveness is costly—as suggested by its link to teacher credentials—profit-maximizing schools would have little incentive to do so in the absence of student demand.

Second, even if most students *would* prefer a public option, capacity constraints in the public sector could push students towards charters. As shown in Figure 2, the number of students enrolling in public schools has remained roughly constant over the last three decades. Insofar as this pattern reflects underinvestment into public school capacity relative to demand, rationing of seats may lead

[33] The empirical evidence on preferences over school effectiveness is mixed, with studies finding that students do (Abdulkadiroğlu et al. 2020; Beuermann et al. 2022) and do not (Walters 2018) seem to sort themselves into more effective schools. A related body of research shows that parents and students often have little information both about school characteristics and the school choice mechanism itself (Andrabi et al. 2017; Arteaga et al. 2022; Ainsworth et al. 2023; Borger et al. 2024). See, e.g., MacLeod & Urquiola (2019) for a review of this literature.

students—and low-GPA students in particular—to enroll in less effective charter schools.[34]

To investigate these hypotheses, I use data on students' rank-ordered school applications to estimate a simple discrete choice model of student demand. I first investigate the extent to which students value different school characteristics, including earnings impacts, in the estimated model. Second, I use the model to predict student rankings over all available alternatives in the market—even those that students may omit from their actual applications due to selectivity. This will allow me to disentangle demand from capacity constraints. Finally, I perform counterfactual exercises where all for-profit charter seats are proportionally redistributed to the public sector, in order to investigate how students would move in case their preferred for-profits had not been available.

5.1 Estimating Student Preferences

I model students' preferences over schools and programs in a simple discrete choice framework (McFadden 1977; Train 2009). To estimate this model, I use data on students' rank-ordered lists from 2013–2015 which constitutes the same sample used in the validation exercise presented in Section 3.3.[35] Let $U_i(j,p)$ denote the utility of student i associated with the school and program combination j,p. To allow for heterogeneity in preferences, I divide students into different strata s and characterize utility as:

$$U_i(j,p) = V_s(\pi_s, \mathbf{X}_{ijp}) + \eta_{ijp}, \tag{10}$$

where η_{ijp} are errors following independent type-I extreme value distributions. The deterministic part of the utility function (V_s) is a function of school effectiveness, location, and program availability. Location is defined by three terms. The first term is the Euclidean distance between student i and school j. The second term is the cosine between students' and schools' directions toward the city center: in practice, this measure is equal to -1 if the school is located away from the city center, from the perspective of student i, and 1 if it is located towards the city center. This captures the empirical fact that charter schools are much more likely to be located in or around the inner city, which students may value (Table 2). The third term is the interaction between these measures of proximity and central location, allowing for varying willingness to travel depending on direction. V_s is, then, given by:

$$V_s(\pi_s, \mathbf{X}_{ijp}) = \underbrace{\pi_{1s}\theta_{jg}}_{Effectiveness} + \underbrace{\textstyle\sum_{k\in\mathcal{P}} \pi_{ks}\mathbf{1}[Program_p = k]}_{Programs} + \underbrace{\pi_{2s} f(x_i, y_i, x_j, y_j)}_{Location} \tag{11}$$

where θ_{jg} denotes school effectiveness and x, y denote coordinates of students and schools. Note that since I do not have access to school effects on earnings at 30 in this sample, which covers 2013–2015, I use the same predicted earnings measures as in the validation exercise in Section 3.3.

[34]The idea that weak incentives to invest in capacity lead to rationing is central in public economics and has often been invoked as a rationale for outsourcing public services (Lindsay & Feigenbaum 1984; Hart et al. 1997).

[35]Similar models, estimated from data on revealed preferences or actual school placements, are commonly used in studies of parental or student demand (Burgess et al. 2015; Walters 2018; Abdulkadiroğlu et al. 2020; Beuermann et al. 2022; Ainsworth et al. 2023; Campos & Kearns 2024).

Estimation strata s are defined by the interaction of student gender, GPA tercile, and the program type that the student ultimately enrolled in. The reason for estimating the model separately by program enrollment is to capture the possibility that students are highly committed to only a few programs. This is empirically relevant: in Table A.4, I provide descriptive statistics on students' rank-ordered lists, which show that students rarely rank many different programs but are significantly more flexible when it comes to schools. Failing to capture this rigidity in preferences over programs would overstate the willingness of students to substitute between programs relative to other characteristics.

Under truthful reporting of preferences in students' rank-ordered lists, Equation (10) can be estimated as a rank-ordered logistic model. The assumption on truthful reporting is, in practice, rarely fulfilled even when the mechanism allocating students to seats is strategy-proof. As shown in Fack et al. (2019) and Andersson et al. (2024), students only have incentives to provide a *partially* truthful ranking of schools and programs. For instance, students have few reasons to rank alternatives where their probability of admission is zero. To address this issue, I assume that students rank their most preferred alternatives among a subset of feasible alternatives, where they would have cleared the ex-post admission cutoffs (Fack et al. 2019).

As in most discrete choice models using revealed preference data, the coefficient $\hat{\pi}_{1s}$ should be understood as capturing the role of earnings impacts in explaining school choices rather than a strictly causal valuation. For my purposes, however, this is not a major concern. First, it answers whether students are more likely to choose schools that have higher earnings impacts—regardless of why they do so.[36] Second, the model is used to predict students' rankings over all available alternatives in the market (Gandil 2021), where the causal interpretation of the coefficients is of secondary importance. Throughout the remainder of this section, therefore, preferences should not be interpreted as deep structural parameters, but rather as reduced-form predictors of choice.

Estimates of the parameters in Equation (10), averaged across all strata s and weighted by stratum size, and associated standard errors are shown in Table 9. Conditional on programs and location, students appear to place little weight on earnings impacts in their school choice. For students in the bottom/top tercile of the GPA distribution, attending a school that boosts predicted earnings by 1 percentile (similar to the size of the charter–public gap) is associated with an increase in utility by 0.05 and 0.15 units, respectively. In comparison, the estimated preference for an additional 10 kilometers of distance is equal to around -2.5 units. This implies that students are willing to travel only around 0.2 (bottom tercile) to 0.6 kilometers (top tercile) for an additional percentile in earnings impacts. These numbers are small relative to the average travel distance between students and their admitting alternatives which is around 9 kilometers.

Students have strong preferences over study programs and location. In bottom tercile, students have

[36]It might, for instance, be driven by a preference for qualified teachers (an easily observable school characteristic) which in turn is related to high earnings impacts (that is very difficult to observe). Alternative approaches rely on survey experiments to identify causal parameters (Arcidiacono et al. 2012; Boneva & Rauh 2018; Delavande & Zafar 2019; Singh & Romero 2022), with the potential drawback that stated preferences may not necessarily reflect preferences in real-world choice situations.

Table 9: Model estimates of student preferences

	Student GPA tercile		
	1st	2nd	3rd
School effectiveness			
Estimated effect on predicted earnings	0.05***	0.05***	0.15***
	(0.00)	(0.01)	(0.04)
Programs (baseline: social sciences)			
Natural sciences	-1.84***	-0.35***	1.06***
	(0.03)	(0.03)	(0.04)
Business	-0.14***	0.26***	0.04
	(0.03)	(0.02)	(0.04)
Arts	-2.68***	-1.58***	-1.13***
	(0.46)	(0.04)	(0.29)
Technology	-0.98***	-1.12***	-1.20***
	(0.04)	(0.03)	(0.05)
Manufacturing (voc)	-1.36***	-3.30***	-4.47***
	(0.04)	(0.04)	(0.09)
Care & services (voc)	-1.35***	-2.75***	-2.99***
	(0.04)	(0.28)	(0.06)
Location			
Distance (10km)	-2.16***	-2.64***	-3.22***
	(0.02)	(0.04)	(0.07)
Inner city direction	-0.59***	-0.61***	-0.63***
	(0.02)	(0.03)	(0.06)
Inner city direction × Distance (10km)	1.11***	1.31***	1.84***
	(0.02)	(0.04)	(0.10)
Average distance to admitting option (10km)	0.93	0.90	0.83
Number of strata	14	14	14
Number of students	15580	15365	15450

Notes: $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *. This table shows estimates from the rank-ordered logit model of utility following from Equation (10), using data from 2013–2015. The model is estimated for all 42 combinations of programs, gender and GPA terciles: the table shows the average of the estimated coefficients (with associated standard errors) for students in each tercile, weighing by stratum size. Statistical significance is assessed by testing that the average of the coefficients is equal to zero. The first panel shows the associated utility from attending a school with an additional 1-percentile effect on predicted lifetime earnings rank. The second panel shows coefficients on program indicators, relative to the baseline of social sciences. The third panel shows coefficients on the distance between students and schools, as well as the cosine between their directions toward the city center (equal to 1 if the school is in the direction of the city center, and -1 if it is in the opposite direction). The interaction between the two are also included.

a distaste for all programs relative to the social sciences but prefer vocational programs—especially manufacturing-related tracks—over the math-intensive natural sciences. These vocational programs become unattractive relative to any of the academic tracks already when moving to students in the middle tercile, among whom social sciences and business are the most popular.[37] Natural sciences is the most preferred program only for students in the top tercile. Finally, all students prefer more proximate alternatives, but are willing to travel about twice as far if the school is located in the direction of the city center.[38]

5.2 The Demand for Charter Schools

Demand for a particular school reflects both what students want and what schools offer. Actual enrollment, however, also depends on capacity constraints at competing schools. The large market share of charter schools may therefore not only reflect high demand but also limited availability in more preferred public alternatives. In this subsection, I separate student demand from actual enrollment to investigate i) if and why there is demand for charters and ii) if capacity constraints in public schools play a role in pushing students into the charter sector.

If students truthfully ranked all available alternatives in their school applications, student demand would be fully observed. As discussed previously this is not the case: students typically rank only a few relevant schools and programs. This makes it difficult to tell, for instance, which alternatives the student *would have* preferred if he or she would have had a chance at admission everywhere (Fack et al. 2019). I therefore use the model given by Equations (10) and (11) to predict students' full rankings over all schools and programs in the market. I perform this exercise for all years between 2004–2015, for which I have access to school and program capacities.

Using these predicted rankings, I run the assignment mechanism and compare placements with those observed in the data. In Table A.5, I show that the model can closely replicate the market shares of charters, program enrollment shares, as well as the significantly higher likelihood of low-GPA students to enroll in for-profits observed in the data. Hence, the market share of charter schools can be rationalized by a simple model in which students choose schools primarily based on location and program availability.[39]

I use the model to illustrate the separate roles of student demand and capacity constraints in explaining charter school enrollment. Figure 7 plots the share of for- and non-profit charters in students' predicted rankings, where rank 1 is the most preferred, rank 2 the next, and so on. For readability, I limit myself to comparing the top 10 ranked alternatives for students in the top vs. bottom tercile of GPA: 92.4% of students are admitted to one of their top 10 schools and programs, making alternatives further down the predicted rankings less empirically relevant. There are three main takeaways from Figure 7.

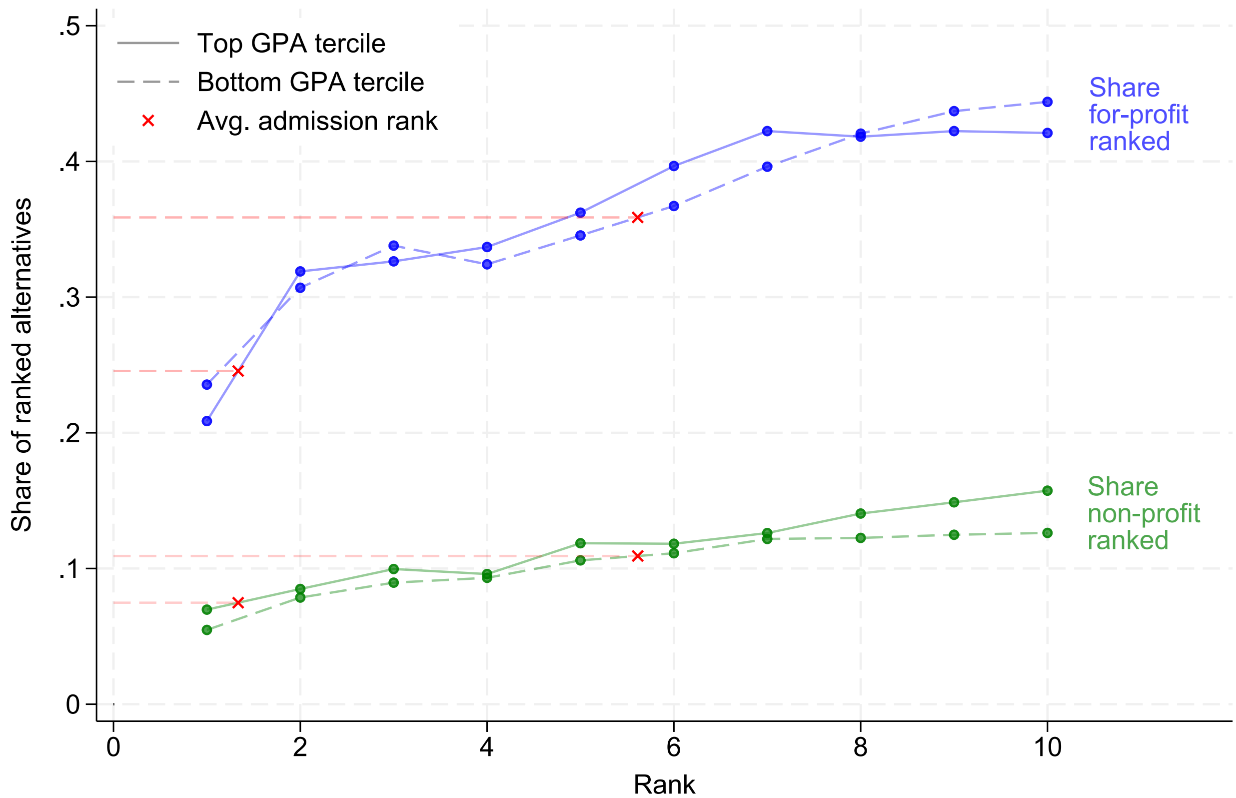
First, there is a high baseline demand for for-profit charters. Around 22% of students rank a for-profit charter at the top of their list, which is fairly close to their market share in the data

[37]The business program launched in 2011 and was therefore not available for students in my main, long-run earnings sample.

[38]Students preferring schools that are located towards the inner city may reflect location as an amenity, or overlap with subway lines that tend to extend from the city center in relatively straight lines. I am ultimately unable to distinguish between these two scenarios: for the purpose of this analysis, I do not probe deeper into why students prefer certain locations over others.

[39]Note that since the preference model is estimated only on part of the sample (2013–2015), this mostly constitutes an out-of-sample validation of model fit. The fit of the model is slightly worse for location: in particular, it predicts more proximate placements than is observed in the data, at slightly less centrally located alternatives.

Figure 7: Charter demand in students' predicted rankings



Notes: This table shows characteristics of the schools and programs that students are placed at by the serial dictatorship mechanism when relying on preferences from the rank-ordered logit model from Equation (10). This is compared with actual enrollment in the data. This exercise uses data from 2004–2015, where I have access to number of seats for each school and program combination, needed to run the serial dictatorship algorithm.

(28%, see Table A.5). This result suggests that even if students would have ranked *all* of the available alternatives in the market, even those that they might omit due to admission being highly unlikely, a substantial share would still prefer charters over public schools. Given the model estimates (Table 9), this demand can be attributed to charters offering attractive combinations of programs and location.

Second, students are even more likely to rank charters further down in their predicted rankings, particularly for-profits. This implies that although many students rank charters at the top of their list, they are even more common as fallback alternatives for students preferring to attend public schools.

Third, there is remarkably little difference in the share of charters ranked by students in the top vs. bottom of the GPA distribution. Instead, the higher propensity of low-GPA students to enroll in for-profits is driven by capacity constraints in popular schools and programs. Most students in the top tercile of GPA are admitted to their first or second ranked alternative, whereas students in the bottom tercile are admitted to their fifth or sixth. Since fallback alternatives are significantly more likely to be for-profit charters, this leads to a heavily negative selection on GPA into for-profits.

These results indicate that the large market share of for-profits can mostly be explained by students demanding the locations and programs that they offer. Had all students been admitted to their top-ranked alternatives, the predicted market share of for-profits would have reduced to around 22%, compared to 28% in the data. The remaining enrollment stems from capacity constraints in the public sector, leading to a disproportionate selection of low-GPA students—who are the last to get assigned to a seat—into for-profit charters.

5.3 Counterfactual Closures of For-profit Charters

Debates over whether to ban or restrict for-profit charter providers have been at the center of Swedish education policy since the inception of the charter system in the 1990s (Henrekson 2025). This has culminated in a broad governmental inquiry—backed by parties across the political spectrum—that is currently underway (Utbildningsdepartementet 2025). Taken together, my results suggest that such policies involves a tradeoff between catering to student demand and improving long-run economic outcomes. However, this depends crucially on where students in for-profit charters would have gone if those schools were not available. For example, Figure 6 showed that low-GPA students in for-profits tend to enroll in higher-return programs than similar students in public schools. The earnings impact of moving these students to public schools depends on whether comparable programs are actually offered in the schools available to them.

A full characterization of the general equilibrium consequences of charters would require a highly elaborate model of competition and the labor market, which is outside the scope of this paper.[40] However, the estimates of school and program effectiveness in combination with the estimated choice model can provide a partial answer. In this section, I investigate how students would move across schools and programs under a counterfactual closure of the for-profit charter sector. The aim of this exercise is not to provide a full-scale policy counterfactual, but rather to investigate the fallback options in the public sector—as currently administered—for students enrolled in for-profit charters.

I conduct this counterfactual exercise by simulating the assignment mechanism using students' predicted rankings, setting the capacity of for-profit charter schools to zero. I then compare the resulting changes in earnings impacts and implied utility for students who were originally enrolled in for-profits. Since for-profits account for a large share of seats in the market, it is necessary to take a stance on how to redistribute these seats in the public sector. To avoid changing the relative capacity in different public sector schools and programs, I redistribute for-profit charters seats proportionally across all public options. Essentially, this makes the very conservative assumption that public sector that is completely unresponsive to local changes in demand.

[40]For instance, public school effectiveness and the stock of teachers may well depend on competitive pressures induced by charters, and future wages may respond due to changes in the composition of workers. A growing literature uses structural methods from industrial organization, or data across many school markets to study a wide range of similar topics, including the effects of competition (Allende 2019; Neilson 2021; Campos & Kearns 2024), strategic supply responses of schools (Singleton 2019), product differentiation (Bau 2022; Carneiro et al. 2024), teacher contracts (Rothstein 2015), residential decisions in relation to school supply (Ferreyra 2007), and student sorting (Epple et al. 2006; Walters 2018).

Table 10: Counterfactual closures of for-profit charters

		GPA tercile		
	All	Bottom	Middle	Top
Effects on earnings rank at 30				
Change in total effectiveness	0.24	-0.24	0.53	0.72
Change in school effectiveness	0.48	0.28	0.57	0.73
Change in program effectiveness	-0.25	-0.52	-0.04	-0.00
Utility				
Change in total utility	-0.16	-0.20	-0.04	-0.24
Change in utility: programs	-0.09	-0.20	-0.01	-0.00
Change in utility: location	-0.07	-0.00	-0.03	-0.24
Changes in program enrollment shares				
Social sciences	0.00	0.00	0.00	0.00
Natural sciences	0.00	0.00	0.00	0.00
Business	0.00	0.00	0.00	0.00
Arts	0.00	0.00	0.00	0.00
Technology	0.03	0.07	0.01	0.00
Manufacturing (voc)	-0.04	-0.09	-0.01	0.00
Care & services (voc)	0.01	0.02	0.00	0.00
Number of students	20023	8878	6084	5061

Notes: This table shows implied changes in total, school and program effects on earnings rank at 30 as well as utility, related to programs and location, among for-profit charter students when simulating the closure of for-profits. This exercise places students using the serial dictatorship mechanism, using predicted rankings over all alternatives in the market as estimated in Section 5.1, while redistributing all for-profit charter seats proportionally to the public sector. I do this for all years between 2004 and 2008, for which I have access to impacts on earnings at age 30, as well as school and program capacities.

On average, reallocating seats from for-profit charters to public schools moves students to alternatives with higher earnings impacts (Table 10). Although students tend to shift into lower-return programs, this loss is more than offset by gains in school-level effectiveness. These earnings gains, however, are unevenly distributed. For students in the top and middle GPA terciles, program choices change little when for-profits close. By contrast, students in the bottom tercile are diverted away from high-return, manufacturing-related vocational programs—enrollment drops by 9p.p. from a base of 33%—that are common in for-profit charters. Instead, they move to technology or care and services tracks, which have lower returns. This suggests that academically stronger students find comparable programs in the public sector, whereas weaker students do not.

Despite the earnings gains, all groups of for-profit charter students move to less-preferred alternatives. This is consistent with the result that most of the market share of for-profits are driven by a demand for the characteristics that they offer (Section 5.2). The loss in utility can be attributed both to programs and location. Low-GPA students are less likely to find the programs they prefer, while high-GPA students lose access to schools with preferable locations. This result is not mechanical, since the counterfactual exercise does not only remove alternatives—in which case students are unambiguously worse off—but also increases capacity to the public sector.

It should be noted, though, that the program offerings of public schools may be endogenous with respect to charters' supply decisions. Without for-profit charters, public schools might expand supply in the programs that for-profits were previously specialized in. In such a scenario, the loss in program returns for academically weak students could potentially be eliminated entirely. At the same time, such responsiveness cannot be taken for granted: competition-based reforms are explicitly intended to create incentives that might otherwise be absent.

6 Discussion

The findings in this paper provide insights relevant to current policy efforts to introduce or expand voucher and charter school systems. These insights center on the potential tension between satisfying student demand and achieving other objectives that policymakers might value—objectives that may or may not align with the choices students make in practice. While my analysis takes no stance on which outcomes should be prioritized, the results illustrate how these goals can diverge even in a mature market.

I find that charters, predominantly driven by for-profits, appear to deliver on student demand. If a central policy goal is to meet revealed preferences—regardless of whether they reflect true preferences, information frictions, or a mix of both—my results suggest that charter systems can achieve this. However, depending on the choices students make, what the market delivers may well be unrelated or even at odds with other outcomes that a social planner would value—for instance, long-run economic outcomes. This is especially the case if investments into promoting such outcomes are costly, in which case cost-cutting incentives may lead to underinvestment at the expense of student outcomes.

Concerns about misaligned incentives likely explain why many charter systems forbid for-profit providers (Cohodes & Parham 2021). However, my results raise yet another potential trade-off with respect to for- and non-profit providers, related to scale. In Sweden, virtually all growth of the charter sector over the past three decades has been driven by for-profit schools, with the non-profit share remaining stable at around 10 percent. This pattern suggests that the profit motive may have been important for large-scale sector expansion. Limiting participation to non-profits could potentially mitigate concerns around cost-cutting, but might also slow the growth of school capacity. This might be important in contexts where relieving pressure on the public sector is a priority.

The possible tension between student demand, provider incentives, and policymaker objectives is unlikely to be unique to Sweden. Many institutional features of Swedish high school education—such as choice-based assignment, centrally designed curricula, and universal public funding—are also common in charter systems globally. Differences, such as the absence of fee-charging outside options, may be less relevant for families relying on publicly funded options: a demographic that charter systems are often targeting. In systems providing even greater scope for incentive misalignment and cost-cutting—through, for instance, less regulatory oversight, less central control over curricula, or more decentralized wage-setting for teachers—these tensions may be even more pronounced.

7 Conclusion

Market-based reforms to public education rest on a central premise: that expanding school choice and introducing private providers can raise quality by aligning supply with demand. If students are informed and choose effective schools, competition should reward the most productive schools and punish the least effective. Yet this mechanism depends critically on whether students value (and are able to identify) school effectiveness. It also raises concerns about the incentives of private providers—particularly for-profit operators—to prioritize cost-cutting over student outcomes.

This paper provides new evidence on these questions from the Swedish high school market, a mature, full-scale charter system where publicly funded for- and non-profit providers compete directly with public schools. I show that, on average, charter schools reduce students' long-run earnings relative to public schools. For-profits are less effective primarily due to lower school-level effectiveness, which appears to be linked to lower investments in teachers. Non-profits also perform worse than public schools, but largely because they specialize in low-return programs. Despite their lower effectiveness, for-profits have expanded substantially and account for a large share of student enrollment. A key explanation is that students place limited weight on earnings impacts when choosing schools, and instead prioritize programs and location.

My results underscore a central tension in market-based school systems: responsiveness to demand does not necessarily translate into improvements in student outcomes. Future research could evaluate whether policy instruments—such as informational campaigns or constraints on provider behavior—can preserve the responsiveness of for-profit schools while aligning their incentives more closely with long-term student outcomes.

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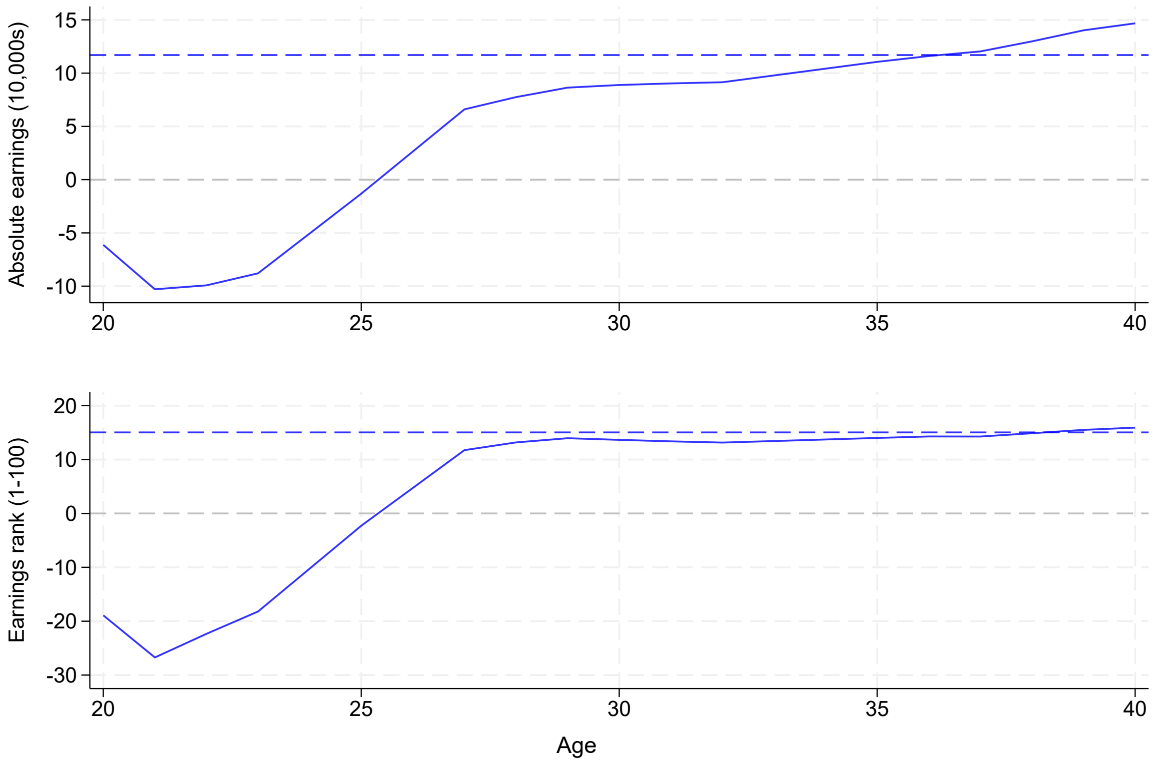
A Further figures and tables

Figure A.1: Age-earnings differences by college enrollment at age 21



Notes: This figure shows the difference in absolute earnings (top) and earnings percentile rank (bottom) across age by individuals enrolled in college by 21, and those not enrolled, using data between 1995-1998. The blue, dashed line shows the average difference between ages 35 and 37.

Table A.1: Descriptive statistics of students facing admission risk

	Sample	
	All students	With admission risk
Student characteristics		
Grade 9 GPA (std)	0.04	0.03
Female	0.48	0.43
Born in Sweden	0.94	0.94
Lives centrally	0.32	0.44
Father: born in Sweden	0.70	0.69
Mother: born in Sweden	0.71	0.70
Mother: college-educated	0.51	0.55
Father: college-educated	0.46	0.50
Mother: earnings rank at 50	51.20	53.16
Father: earnings rank at 50	49.96	51.16
Applied to		
Public	0.93	0.96
Non-profit	0.20	0.24
For-profit	0.55	0.54
Social sciences	0.44	0.49
Natural sciences	0.32	0.35
Business	0.34	0.43
Arts	0.03	0.03
Technology	0.17	0.17
Manufacturing (voc.)	0.13	0.09
Care & services (voc.)	0.15	0.06
Schools	162	155
Programs	7	7
Students	42792	10241

Notes: This table shows descriptive statistics of students in the validation sample (2013–2015). Students with admission risk are defined as having a probability of admission p_{ijp} strictly between zero and one for one of their ranked alternatives. The variables under *Applied to* are equal to one if the student's rank-ordered application included at least one school/program of the corresponding types. I report the number of unique schools and programs that at least one student faced admission risk to, in any of the years between 2013 and 2015, at the bottom of the table.

Table A.2: Balance of admission offers Z_{ijp}

	Uncontrolled	Controlled
Index of earnings determinants†	1.07***	0.03
	(0.12)	(0.22)
Female (%)	-7.33***	-1.12
	(0.74)	(1.33)
Born in Sweden (%)	0.48**	-0.02
	(0.21)	(0.48)
Lives centrally (%)	-4.61***	1.41
	(0.78)	(1.53)
Ranked for-profit first (%)	-1.65	-1.17
	(1.35)	(1.44)
Ranked non-profit first (%)	-2.47***	-1.12
	(0.66)	(0.88)
Ranked academic program first (%)	-3.77***	-1.42
	(1.33)	(0.98)
Father: born in Sweden (%)	1.42**	-0.33
	(0.64)	(1.04)
Mother: born in Sweden (%)	1.30**	-0.10
	(0.64)	(0.96)
Mother: college-educated (%)	-1.75**	0.30
	(0.74)	(0.94)
Father: college-educated (%)	-0.96	-0.72
	(0.76)	(0.89)
Mother: earnings rank at 50	0.64*	-0.28
	(0.36)	(0.52)
Father: earnings rank at 50	1.06***	-0.33
	(0.37)	(0.56)
Observations	42792	42792

Notes: This table shows balance of predetermined characteristics of the school-by-program admission offers Z_{ijp}. Each row corresponds to a regression of a predetermined student characteristic on the impact on predicted earnings of the school and program that the student was admitted to. In the first column, I only include year fixed effects in these regressions, thus showing the degree of selection into more effective schools and programs. In the second column, I control for the *expected* effectiveness for each student, given by the propensity score-weighted average effectiveness of all alternatives ranked by the student, as well as Grade 9 GPA. The table displays the estimated coefficients on the admitting alternative's effectiveness, with robust standard errors in parentheses. †The index of earnings determinants is the predicted earnings at age 30, using the full vector of controls used in the main OLS model (Equation 1).

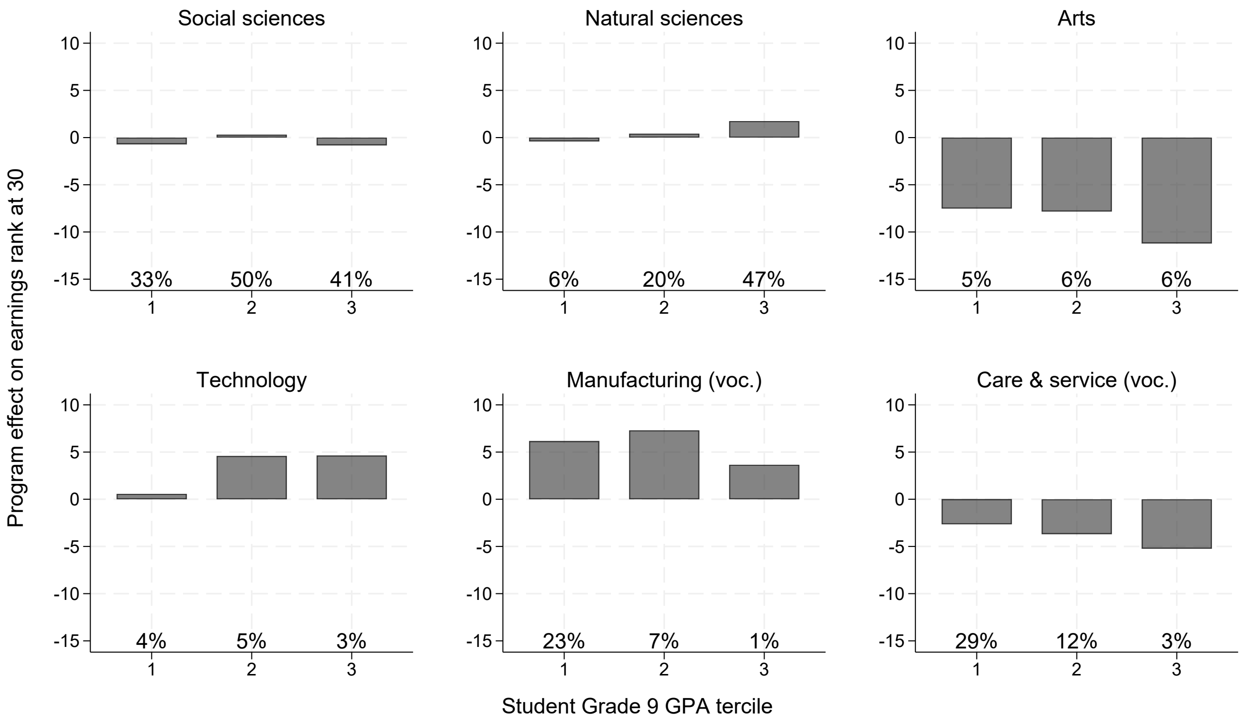
Table A.3: Average effectiveness of charter relative to public schools on short-run prediction of lifetime earnings

	Predicted lifetime earnings (based on outcomes at age 23)	
	Rank	Levels
Panel A: All charters		
Charter	-1.08***	-85.44***
	(0.10)	(8.11)
Panel B: For-/non-profits		
For-profit	-0.93***	-70.90***
	(0.12)	(9.81)
Non-profit	-1.31***	-107.78***
	(0.15)	(12.60)
For-/non-profit equal (*p*-value)	0.04	0.02
Public outcome mean	51	4767
SD of OLS impacts	1.68	140.97
– bias-corrected‡	1.53	129.72
Number of *jpg* cells	1043	1043
Number of students	194266	194266

Notes: $p < 0.01$ = ***, $p < 0.05$ = **, $p < 0.1$ = *. This table shows mean differences in estimated impacts between for-profit charter schools and public schools, with standard errors clustered at the school-by-year level in parentheses. The outcome is predicted average earnings rank (Column 1) and level (Column 2) between ages 35 and 37, based on students' high school graduation, college and labor market histories up until the age of 23 (see Appendix C for details on the construction of this prediction).

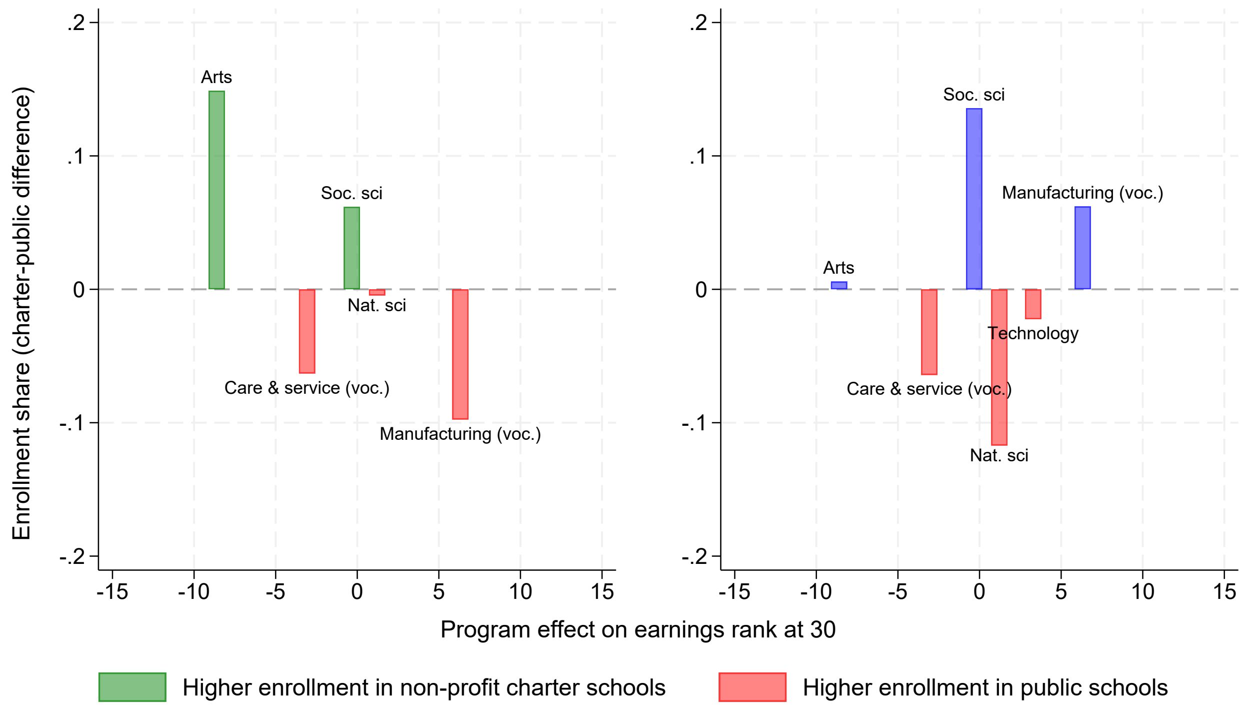
‡The sample standard deviation of estimated effectiveness will be inflated due to estimation noise. I correct for this using the approach of Kline et al. (2020) (see Appendix E for details).

Figure A.2: Estimated program returns by student GPA terciles



Notes: This figure shows the estimated program effect ($\hat{\gamma}_{pg}$, as defined in Equation 8) along the vertical axes, separately for each tercile of the Grade 9 GPA distribution along the horizontal axes. I show the percentage of students in a given GPA tercile enrolled in the program at the bottom of each figure. Program returns are expressed relative to the average (enrollment-weighted) return of students in each GPA tercile.

Figure A.3: Program effects and enrollment



Notes: These figures show the estimated return to each program type ($\hat{\gamma}_{pg}$, as defined in Equation 8) averaged across all students along the horizontal axes, and the difference in enrollment shares between charter and public schools for each program type along the vertical axes.

Table A.4: Students' rank-ordered lists, 2012–2015

	Student GPA tercile		
	1st	2nd	3rd
List length	3.86	4.15	4.06
No. of distinct schools	3.12	3.24	3.26
No. of distinct program types (1–7)	1.54	1.62	1.53
Share admitted to top-ranked choice	0.76	0.77	0.89
Share admitted to top-ranked school	0.79	0.80	0.91
Share admitted to top-ranked program	0.94	0.95	0.98
Top-ranked choice: public	0.63	0.68	0.73
Top-ranked choice: non-profit	0.04	0.06	0.13
Top-ranked choice: for-profit	0.34	0.26	0.14
Number of students	15580	15365	15450

Notes: This table shows descriptive statistics of students' rank-ordered lists, using application data between 2013 and 2015. Students rank school-by-program combinations, with around 170 schools and 18 programs to choose from. I aggregate programs into seven categories, denoted as program types (for details, see Appendix B). Students may rank as many alternatives as they like. Student GPA refers to the Grade 9 GPA with which they apply to high school.

Table A.5: Fit of simulated school and program assignment

	All		GPA tercile: Bottom		Middle		Top	
	Data	Model	Data	Model	Data	Model	Data	Model
Sector								
Public	0.62	0.63	0.59	0.54	0.61	0.64	0.66	0.73
Non-profit	0.10	0.09	0.08	0.10	0.08	0.09	0.15	0.08
For-profit	0.28	0.28	0.33	0.36	0.31	0.27	0.19	0.20
Programs								
Social sciences	0.35	0.34	0.30	0.28	0.40	0.40	0.35	0.35
Natural sciences	0.21	0.21	0.05	0.04	0.16	0.15	0.42	0.43
Business	0.07	0.07	0.05	0.04	0.10	0.09	0.07	0.07
Arts	0.08	0.08	0.07	0.07	0.09	0.09	0.08	0.08
Technology	0.07	0.07	0.07	0.07	0.10	0.11	0.05	0.05
Manufacturing (voc.)	0.11	0.11	0.24	0.25	0.06	0.06	0.01	0.01
Care & services (voc.)	0.12	0.12	0.23	0.25	0.09	0.09	0.03	0.02
Location								
Distance (10km)	0.89	0.55	0.93	0.75	0.90	0.54	0.83	0.35
Central direction (cosine)	0.56	0.35	0.50	0.50	0.56	0.33	0.60	0.20
Number of students	202777	202777	70267	70267	65281	65281	67229	67229

Notes: This table shows characteristics of the schools and programs that students are placed at by the serial dictatorship mechanism when relying on preferences from the rank-ordered logit model from Equation (10). This is compared with actual enrollment in the data. This exercise uses data from 2004–2015, where I have access to number of seats for each school and program combination, needed to run the serial dictatorship algorithm.

B Details on study programs

Table B.1: National study programs

Program category	Representative programs (specialization)	Percentage of students in 2011
Academic programs		
Social sciences	Social sciences (generic)	60%
	Social sciences (psychology)	26%
	Social sciences (media)	13%
Natural sciences	Natural sciences (generic)	82%
	Natural sciences (society, environment)	10%
	Natural sciences (music)	3%
Arts	Arts (music, instruments)	28%
	Arts (media, audio, film)	21%
	Arts (fine art, design)	12%
	Humanities (languages) [started in 2011]	9%
Technology [started in 2000]	Technology (information-, media technology)	36%
	Technology (production technology)	31%
	Technology (product design)	16%
Business [started in 2011]	Business (generic)	80%
	Business (business law)	20%
Vocational programs		
Manufacturing	Electrician	47%
	Construction	27%
	Vehicle and transport	14%
	Plumbing & property maintenance	11%
Care & services	Crafts (hairdresser, stylist, textiles, carpentry)	23%
	Child care	15%
	Animal care	15%
	Restaurant & food	14%

Notes: This table provides an overview of the national study programs available to Swedish high school students. I group these into seven categories, shown in the first column. The second column provides names of the largest programs that make up each group. The third column shows student shares (within the group) in each program in 2011, the first year for which I have granular data on programs and subspecializations.

C Predicting lifetime earnings

I combine data on high school graduation as well as college and earnings trajectories to construct a proxy for lifetime earnings. This approach is closely related to Athey et al. (2019), in the construction of a "surrogacy index" that predict impacts on a (partially unobserved) long-run outcome based on the impacts on its (fully observed) mediators.

For students enrolling in high school between 1995 and 2001, I regress average earnings between ages 35 and 37—providing the best available measure of lifetime earnings (Björklund 1993; Böhlmark & Lindquist 2006; Haider & Solon 2006)—on a set of mediating outcomes $\mathbf{S}_i$ (also referred to as surrogates) as well as the vector of controls used in the main analysis (Equation 1):

$$Y_i = \mu \mathbf{S}_i + \delta \mathbf{X}_i + u_i. \tag{12}$$

The prediction of lifetime earnings is obtained by using the estimated coefficients on the mediators for the rest of the sample, and is given by $\hat{\mu}\mathbf{S}_i$.

I construct two, main predictions of lifetime earnings. For the validation exercise in Section 3.3, I use outcomes measured up until the age of 23. I denote this vector as $\mathbf{S}_i^{23}$. As an auxilliary outcome in the main analysis, I extend this and use outcomes up until the age of 30, denoted $\mathbf{S}_i^{30}$. The mediating outcomes include students' histories of high school graduation, ever enrolling in college, fields of study in college (if enrolled), earnings, and the interaction between earnings and college enrollment. The variables included in $\mathbf{S}_i^{23}$ and $\mathbf{S}_i^{30}$ are shown in Table C.1. Alternative specifications give very similar results.

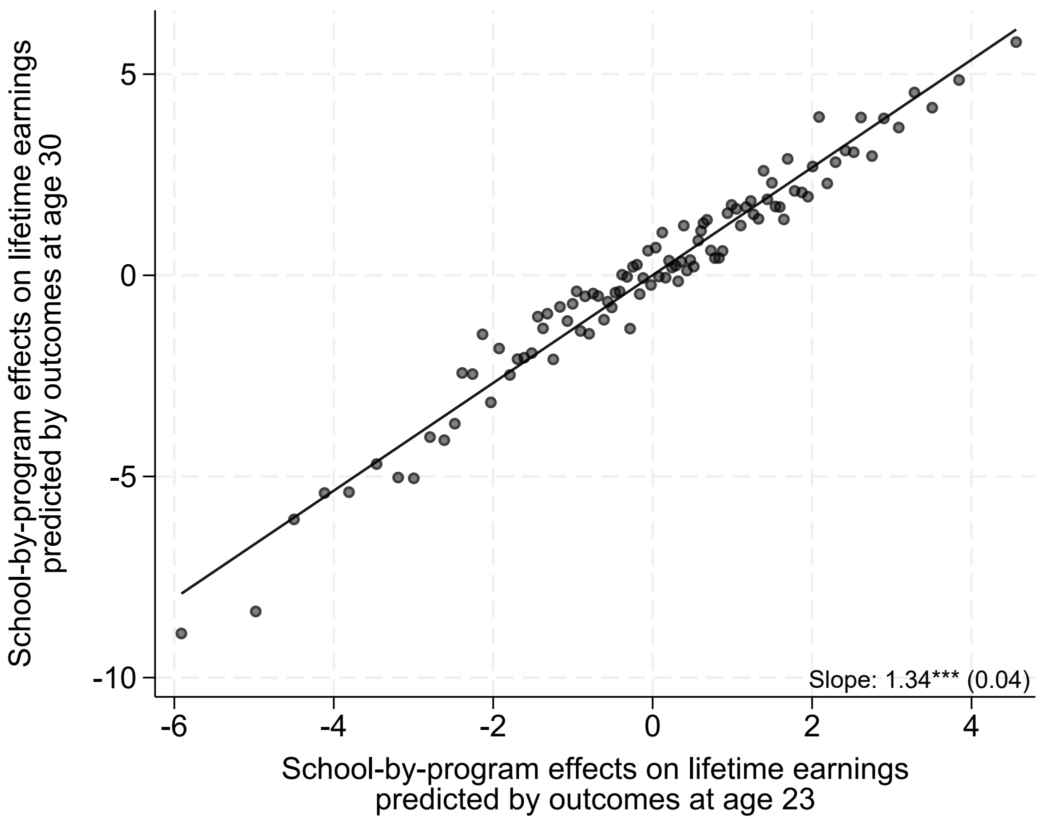
Table C.1: Summary of included mediators in surrogacy indices

		Included variables
Outcomes until age 23	$\mathbf{S}_i^{23}$	High school graduation
		College enrollment by 23
		Field of study at 23 (11 categories)
		Earnings at 20, 21, 22, 23
		College enrollment by 23 × Earnings rank at 20–23
Outcomes until age 30	$\mathbf{S}_i^{30}$	High school graduation
		College enrollment by 23, 25, 27, 30
		Field of study at 30 (11 categories)
		Earnings at 23, 25, 27, 30
		College enrollment by 25 × Earnings rank at 23–30

C.1 Correlation between effects on predicted lifetime earnings

I show a binned scatterplot of school-by-program effects, estimated as described in Section 3.1, on lifetime earnings predicted by outcomes up until the age of 30 ($\mathbf{S}_i^{30}$) and 23 ($\mathbf{S}_i^{23}$) in Figure C.1. Effects on the short-run prediction are shown along the horizontal axis, and effects on the long-run prediction along the vertical. The effects are highly correlated, but the variability in effects on the long-run prediction is larger than those on the short-run prediction. This could, in part, be driven by noise but likely also indicate that part of the effect of schools and programs has not materialized fully at the age of 23.

Figure C.1: Effects on predicted lifetime earnings using outcomes at 23 and 30



Notes: This figure shows a binned scatter plot of school-by-program effects on average earnings at 35–37 predicted by high school graduation, college and labor market outcomes measured i) at the age of 30 (vertical axis) and ii) at the age of 23 (horizontal axis). The data are shown in 100 equally sized bins. The underlying model of school-by-program effects control for a large set of student background characteristics, and is identical to that used to generate the main estimates of this paper (see Section 3.1).

C.2 Diagnostics on lifetime earnings predictions

Regression estimates of Equation 12 are shown in Table C.2, using outcomes up until age 23, and Table C.3 using outcomes up until age 30.

Table C.2: Surrogacy regressions: outcomes until age 23 ($\mathbf{S}_i^{23}$)

Dep. var.: percentile rank of avg. earnings at 35–37 Covariate	Estimate	SE
On-time HS graduation	3.458***	(0.206)
College by 23	22.547***	(1.192)
Earnings rank at 23	0.138***	(0.006)
Earnings rank at 22	0.056***	(0.007)
Earnings rank at 21	0.051***	(0.006)
Earnings rank at 20	0.049***	(0.005)
College by 23 $\times$ Earnings rank at 23	-0.048***	(0.010)
College by 23 $\times$ Earnings rank at 22	-0.036***	(0.011)
College by 23 $\times$ Earnings rank at 21	-0.025**	(0.010)
College by 23 $\times$ Earnings rank at 20	-0.011	(0.008)
College: Pedagogy and teaching	-5.822***	(1.211)
College: Arts	-15.164***	(1.173)
College: Social sciences and business	2.165*	(1.137)
College: Natural sciences, mathematics, data	-2.008	(1.232)
College: Technology and manufacturing	5.245***	(1.153)
College: Agriculture and forestry	-9.178***	(2.151)
College: Healthcare and social work	-1.735	(1.195)
College: Services	-4.858***	(1.498)
Number of students	86541	
Adjusted R^2	0.248	

Notes: This table presents estimates of coefficients from a regression of percentile ranks of average earnings between ages 35 and 37, on a set of outcomes up until the age of 23 (Equation 12). The regression also includes the same set of background variables used in the main analysis (Equation 1). College enrollment equals 1 if the person ever enrolled in, and finished at least some credits, in post-secondary education. Field-of-study in college is measured against the baseline of no college enrollment.

Table C.3: Surrogacy regressions: outcomes until age 30

Dep. var.: percentile rank of avg. earnings at 35–37 Covariate	Estimate	SE
On-time HS graduation	0.822***	(0.174)
College by 30	10.578***	(1.496)
College by 27	-1.008**	(0.493)
College by 25	1.818***	(0.530)
College by 22	0.168	(0.255)
Earnings rank at 30	0.436***	(0.005)
Earnings rank at 27	0.120***	(0.005)
Earnings rank at 25	0.055***	(0.005)
Earnings rank at 22	0.045***	(0.004)
College by 25 $\times$ Earnings rank at 30	-0.007	(0.007)
College by 25 $\times$ Earnings rank at 27	-0.008	(0.007)
College by 25 $\times$ Earnings rank at 25	-0.031***	(0.007)
College by 25 $\times$ Earnings rank at 22	-0.003	(0.006)
College: Pedagogy and teaching	0.854	(1.489)
College: Arts	-6.604***	(1.489)
College: Social sciences and business	3.739**	(1.467)
College: Natural sciences, mathematics, data	2.321	(1.503)
College: Technology and manufacturing	4.951***	(1.477)
College: Agriculture and forestry	-3.268*	(1.853)
College: Healthcare and social work	1.894	(1.482)
College: Services	-3.460**	(1.531)
Number of students	86541	
Adjusted R^2	0.463	

Notes: This table presents estimates of coefficients from a regression of percentile ranks of average earnings between ages 35 and 37, on a set of outcomes up until the age of 23 (Equation 12). The regression also includes the same set of background variables used in the main analysis (Equation 1). College enrollment equals 1 if the person ever enrolled in, and finished at least some credits, in post-secondary education. Field-of-study in college is measured against the baseline of no college enrollment.

Athey et al. (2019) proposes a test of the assumption that the included surrogates are sufficient in capturing the treatment effect on the target, long-run outcome of interest. In my setting, this test regresses Y_i, i.e. long-run earnings, on a set of school-by-program indicators, together with the vector of controls $\mathbf{X}_i$. If, in this regression, controlling for the set of surrogates $\mathbf{S}_i$ eliminates or reduces differences in school-by-program effects, it suggests that their impacts on long-run earnings runs through the included surrogates. The two regressions are given by:

$$Y_i = \beta_{jp} + \Gamma \mathbf{X}_i + \epsilon_i \tag{13}$$

$$Y_i = \tilde{\beta}_{jp} + \Delta \mathbf{S}_i + \Pi \mathbf{X}_i + \nu_i \tag{14}$$

If the variation in β_{jp}, the impact of school j and program p on long-run earnings, is going through the surrogates $\mathbf{S}_i$, then the hypothesis tested can simply be framed in terms of the variances of β_{jp} and $\tilde{\beta}_{jp}$: conditional on the surrogates, the variance of the school-by-program effects should be zero.

$$V(\beta_{jp}) > V(\tilde{\beta}_{jp}) = 0. \tag{15}$$

In practice, the school-by-program effects will be estimated with noise, which results in non-zero variances even if there is no signal. I use the same machinery as in Section E to obtain bias-corrected estimates of the variances of β_{jp} and $\tilde{\beta}_{jp}$:

$$\hat{\sigma}_\beta = \hat{\beta}'_{jp} A \hat{\beta}_{jp} - tr(A\hat{V}), \tag{16}$$

where $A = \frac{1}{1-\sum w^2} diag(w) - ww'$, and w is a vector of weights proportional to the enrollment of students in each of the K school-by-program combinations. Hence, A is a weighting matrix such that the first term on the RHS of Equation (16) is simply the sample variance of the school-by-program effects, weighting by enrollment.[41] $\hat{V}$ is the covariance matrix of the estimated effects.

Table C.4 shows the bias-corrected variance, along with the unadjusted sample variance and the bias-correction term in the RHS of Equation (16), of the school-by-program effects estimated as in Equations (13-14). In the first column, no surrogates are included in the model. In the second and third columns, I control for surrogates $\mathbf{S}_i^{23}$ and $\mathbf{S}_i^{30}$, respectively. The variation in school-by-program effects on long-run earnings decreases sharply as the surrogates are controlled for. Including surrogates measured up to the age of 23 reduces the variance by 40%. It reduces by 88% when including surrogates measured up until the age of 30. This supports the claim that the included college- and labor market outcomes at age 23 captures a substantial share of the impact of schools and programs on lifetime earnings, and almost all of it at age 30.

Table C.4: Variance of school-by-program effects conditional on surrogates

	Included surrogates		
	None	$\mathbf{S}_i^{23}$	$\mathbf{S}_i^{30}$
Sample variance	22.50	13.53	4.24
Correction term	2.69	2.42	1.77
Bias-corrected variance	19.81	11.77	2.48

Notes: This table shows the sample variance, correction term, and bias-corrected variances of school-by-program effects on percentile rank of average earnings at ages 35–37, using data between 1995 and 2001. The first column includes only the controls used in the main specification given by Equation (1). Columns 2 and 3 includes the vectors of surrogates described in Table C.1, using outcomes up until the age of 23 and 30, respectively.

[41] I weigh by enrollment to remain consistent with the main analysis.

D Validation of OLS estimates of effectiveness

D.1 Deriving admission propensity scores

The following exposition provides the details of the validation approach, which follows Abdulkadiroğlu et al. (2022) closely. Denote the cutoff to school and program combination j, p as τ_{jp}. A student is 'conditionally seated' at this alternative if his or her GPA is within some bandwidth λ of its cutoff:

$$C_{ijp} = \mathbf{1}[\tau_{jp} - \lambda < GPA_i < \tau_{jp} + \lambda].$$

Under the assumption that student GPA is drawn from some continuously differentiable distribution, independent but not necessarily identical across students, the unconditional probability of admission to j, p converges to 50% as λ goes toward zero:

$$\lim_{\lambda \to 0} E[Z_{ijp} | C_{ijp} = 1] = 0.5. \tag{17}$$

In practice, the probability of being admitted to a particular alternative, p_{ijp}, will depend on student i's entire application list. A student may, for example, be conditionally seated for multiple alternatives, or far exceed the cutoff of some ranked alternative such that the probability of admission to lower-ranked alternatives is virtually zero. I define p_{ijp} in a way that collapses all of this information into a single probability. Let $p_{ijp} = 0$ if student i i) does not apply to j, p, ii) is below the cutoff by more than λ, or iii) is above the cutoff by more than λ at a more preferred option ($GPA_i > \tau_{ij'p'} + \lambda$ for some $j', p' \succ_i j, p$). If not, then:

$$p_{ijp} = \begin{cases} 0.5^{m_{ijp}}, & \text{if } C_{ijp} = 0 \text{ and } GPA_i > \tau_{jp} + \lambda \\ 0.5 \cdot 0.5^{m_{ijp}}, & \text{if } C_{ijp} = 1, \end{cases} \tag{18}$$

where m_{ijp} is the number of alternatives more preferred than j, p that student i is conditionally seated at.[42] In the first case, the student far exceeds the cutoff τ_{jp} and is guaranteed admission *if* he or she is disqualified from all more preferred conditional seatings. From Equation (17), each such isolated experiment has one-half probability of failure, so a series of m_{ijp} failures happens with probability $0.5^{m_{ijp}}$. In the second case, the student is conditionally seated also at j, p, and so must face m_{ijp} failures and one success, yielding a probability of $0.5 \cdot 0.5^{m_{ijp}}$.

The critical assumption of conditional exogeneity of admission offers Z_{ijp}—given some function of propensity scores p_{ijp} as well as student characteristics $\mathbf{X}_i$ (which includes the running variable GPA_i)—can now be expressed as follows:

ASSUMPTION 2: *Conditional exogeneity of admission offers*

$$E[\epsilon_i | p_{ijp}, Z_{ijp}] = E[\epsilon_i | p_{ijp}] \quad \forall j, p \tag{19}$$

The choice of a bandwidth parameter λ entails a trade-off between bias and precision. Student GPA ranges from 0 to 340 in discrete bins of 2.5 points. Here, I set $\lambda = 12.5$, which effectively includes five GPA bins on both sides of each cutoff. This value corresponds to approximately 7 percentiles of the GPA distribution. Around 18% of applicants to any given school and program combination fall within this bandwidth, making it a fairly conservative choice.

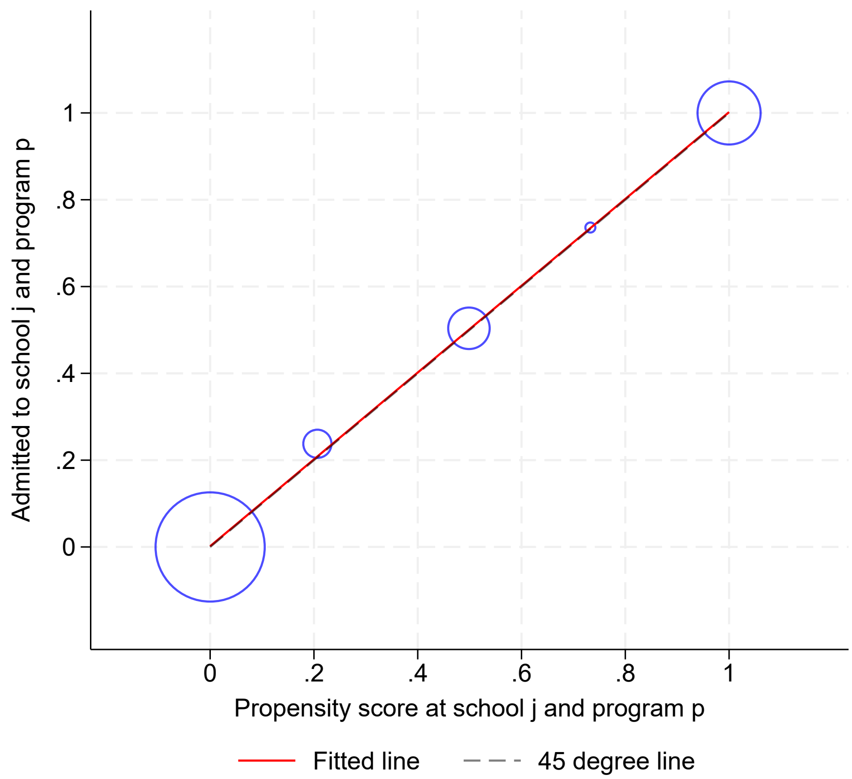
As a comparison, the average standard deviation of admission cutoffs τ_{jp} within a school and program across years is around 25 GPA points. Put differently, even a student 25 points below

[42] Formally, $m_{ijp} = |\{(j', p') \text{ s.t. } C_{ij'p'} = 1 \text{ and } j', p' \succ_i j, p\}|$.

a given cutoff has a fairly good chance of admission based on data from other years. Hence, I only use variation in admissions among students that face significant admission risk. For robustness, I also report results under different choices of λ (Appendix D.4).

In Figure D.1, I plot the correlation of student admissions (Z_{ijp}) and propensity scores (p_{ijp}) for all schools and programs that each student ranked. Propensity scores predict admission with a coefficient of one, which is not surprising given the large influence of the tails of the distribution (0 and 1). However, even for propensity scores strictly between 0 and 1, admission rates match closely. The only exception to this pattern is for low propensity scores where students are slightly more likely to be admitted than expected (23.7% instead of 20.6%). This is potentially a problem for specifications relying on linear propensity score controls. I address this by re-estimating the main validation exercise using fully non-parametric propensity score controls (see Column 3 of Table D.3): this gives results virtually identical to that of the main (linear) specification.

Figure D.1: Correlation between admission and propensity scores



Notes: This figure shows a binned scatter plot of admission of student i to school-by-program alternative j, p, against i's calculated propensity score for the same alternative. The sample only includes alternatives that students actually applied to. Circles are proportional to bin size. The solid line corresponds to the linear fit of admission on propensity scores; the dashed line has a slope equal to 1.

D.2 Statistical tests of unbiasedness

In this section, I provide details of the statistical tests used in Section 3.3. These are developed in Angrist et al. (2017) and Angrist et al. (2024). Denote a particular school and program combination by k, and the outcome of interest as Y_i. I estimate the following model:

$$Y_i = \alpha + \sum_k \beta_k D_{ik} + \mathbf{\Gamma X}_i + \epsilon_i, \tag{20}$$

where β_k are the school-by-program effects of interest, D_{ik} are enrollment indicators, and $\mathbf{X}_i$ is a vector of controls. Let β_k^* denote the true effect of attending k on the outcome Y_i. I am interested in testing the following hypothesis:

$$H_0 : \beta_k = \beta_k^* \quad \forall k \in \{1, ..., K\}. \tag{21}$$

Let's assume that admission to school and program k, denoted Z_{ik}, is as good as random conditional on non-degenerate propensity scores p_{ik} for at least some subset S of the K alternatives. Hence, for S school-by-program combinations, we have access to exogenous variation in admission. The null hypothesis H_0 implies that the residuals from Equation (20) should be uncorrelated with risk-adjusted admission offers for these S alternatives:

$$E[\epsilon_i(Z_{is} - p_{is})] = 0 \quad \forall s = \{1, ..., S\} \tag{22}$$

The intuition behind these S restrictions is that, if the random assignment to some alternative s is correlated with the residuals ϵ_i, then the effect that we estimate (β_k) needs to be different from the true effect (β_k^*).

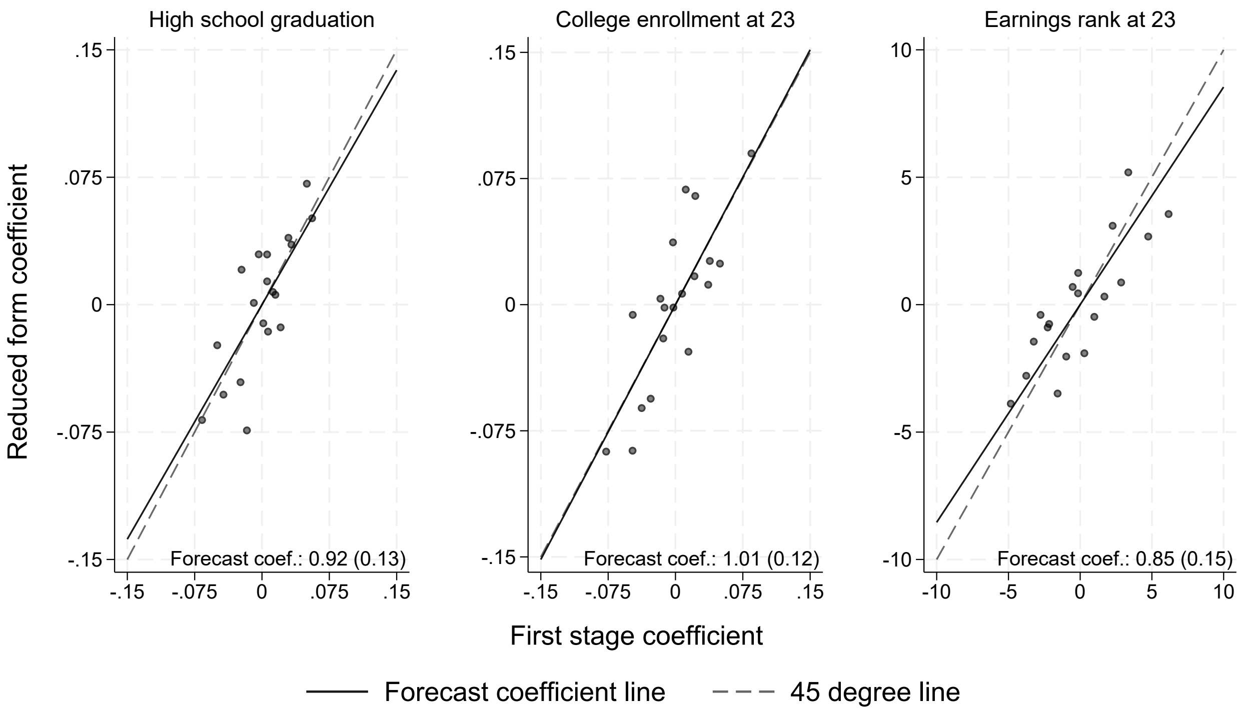
Angrist et al. (2017) show that the restrictions in Equation (22) can be tested using an "omnibus" test statistic, that is the sum of two terms. The first of these is a Wald statistic testing $\kappa = 1$, the forecast coefficient in a 2SLS regression instrumenting the OLS effects β_k of the alternative that students enrolled in with the admission dummies Z_{is}. The second term is the Sargan S-statistic for overidentified IVs (Sargan 1958). Intuitively, $\kappa = 1$ tests whether the RD impacts of admission on Y_i match the observational estimates β_k *on average*, while the S-statistic captures the extent of deviations for individual alternatives that together sum to zero.

D.3 Validation results on raw outcomes at age 23

In the main analysis (shown in Figure 5 and Table 4), I evaluate the unbiasedness of school-by-program effects on predicted lifetime earnings. This prediction is based on students' histories of high school graduation, college enrollment and earnings up until the age of 23. Here, I show the validation exercise using these raw outcomes instead of the more complicated predicted lifetime earnings measure.

Figure D.2 shows the IV test of unbiasedness visually on high school graduation, college enrollment and earnings rank at 23, using the same binned instrument strategy described in Section 3.3. Further test statistics are shown in Table D.1. I estimate forecast coefficients of 0.92 for high school graduation, 1.01 for college enrollment, and 0.85 for earnings rank at 23. None of these are statistically significantly different from 1 (the null hypothesis under unbiasedness of the school-by-program effects). Neither the overidentification (testing for deviations around the solid line) nor omnibus test (testing for deviations around the solid line *and* the slope of the line not being equal to unity) rejects for any of the outcomes.

Figure D.2: IV test for bias in OLS school and program effects: raw outcomes at 23



Notes: These figures provide visualization of the IV test of unbiasedness in Equations (6) and (7), using the aggregated instruments for admission to a school and program in the vth ventile of the distribution of impacts on each respective outcome (Z_{iv}). These outcomes are high school graduation (binary), college enrollment at age 23 (binary) and earnings rank at 23 (percentile rank). The vertical axes show the (first-stage) coefficients from a regression of the OLS effectiveness estimate of the alternative that the student enrolled in, on the instruments Z_{iv}, bin-level propensity scores, and the vector of controls used in the OLS model of effectiveness (Equation 1). The vertical axes show the corresponding estimates from a (reduced form) regression of the actual outcomes on the same set of covariates. The solid lines show the estimated forecast coefficient (κ) from a 2SLS regression of predicted earnings on estimated effectiveness, instrumented by admission offers. For further test statistics, see Table D.1.

Table D.1: Forecast unbiasedness of OLS effectiveness estimates: raw outcomes at 23

	High school graduation	College enrollment at 23	Earnings rank at 23
Forecast coefficient (κ)	0.92	1.01	0.85
	(0.13)	(0.12)	(0.15)
p-value (κ = 1)	0.543	0.929	0.319
p-value (Omnibus test)	0.216	0.256	0.750
Number of instruments	19	19	19
First-stage F-statistic	197.56	337.49	195.42
Share facing admission risk	0.21	0.22	0.22
Observations	43589	42627	42699

Notes: This table shows the results of the IV test for validity of school-by-program impacts on high school graduation, college enrollment at age 23, and earnings rank at age 23 (Equations 6 and 7). As in the main analysis, I use binned instruments for admission to an alternative in the vth ventile of the distribution of effectiveness. Graphical depictions of this test using bin-level instruments are shown in Figure D.2. A student facing admission risk has a propensity score (as defined in Appendix D.1) that is strictly between 0 and 1 for one of their ranked alternatives.

D.4 Robustness of validation results

Table D.2 shows the main validation test (Table 4) under different choices of the bandwidth parameter λ, ranging from 5 to 20 GPA points by steps of 2.5. I find a forecast coefficient very close to 1 in all of these specifications. The main specification uses a bandwidth of 12.5 GPA points.

Table D.2: Robustness of IV validation test: varying RD bandwidths

	Bandwidth around admission cutoffs (λ)						
	5	7.5	10	**12.5**	15	17.5	20
Forecast coefficient (κ)	1.03	1.05	1.02	0.99	0.96	0.91	0.91
	(0.20)	(0.15)	(0.13)	(0.11)	(0.10)	(0.09)	(0.08)
p-value (κ = 1)	0.866	0.755	0.893	0.938	0.700	0.332	0.249
p-value (Omnibus test)	0.162	0.225	0.758	0.799	0.700	0.915	0.733
Number of instruments	19	19	19	19	19	19	19
First-stage F-statistic	89.43	147.51	215.53	278.57	357.15	435.96	513.52
Share facing admission risk	0.10	0.15	0.18	0.22	0.25	0.28	0.31
Observations	42792	42792	42792	42792	42792	42792	42792

Notes: This table shows the results of the IV test for validity of school-by-program impacts on predicted earnings (Equations 6 and 7), using different values of the RD bandwidth parameter λ. The main specification defines being 'close' to an admission cutoff as having a GPA within 12.5 points around the cutoff (GPA ranges from 0 through 340). A student facing admission risk has a propensity score (as defined in Appendix D.1) that is strictly between 0 and 1 for one of their ranked alternatives.

In Table D.3, I run the same validation test, using bin-level instruments Z_{iv}, under a number of different specifications, each corresponding to one column in the table. In the first column, I add additional controls for student i being certain to be admitted to effectiveness ventile v, in the sense of having $p_{iv} = 1$. This amounts to an additional control for nonlinearity of the propensity scores. In column two, I employ the re-centering approach of Borusyak & Hull 2023 which defines instruments as $\tilde{Z}_{iv} = Z_{iv} - p_{iv}$. This approach provides identification regardless of the underlying relationship between propensity scores p_{iv} and potential outcomes. In column three, I fully relax this relationship and include nonparametric controls for each discrete value of p_{iv}, across all ventiles. Columns four and five investigates robustness of the functional form of Grade 9 GPA (the "RD running variable" in this context); in the main specification, it enters as a cubic polynomial. Column four restricts it to enter linearly. Column five, instead, adds nonparametric controls for ventiles of the Grade 9 GPA distribution. Column six restricts the sample to only include school, program and GPA tercile cells (j, p, g) with more than 50 students, excluding around 25% of the sample. Small cells could be problematic since the students driving the RD variation in the validation exercise would have a large weight also in the estimation of the school-by-program effects $\hat{\beta}_{jpg}$. If so, a forecast coefficient close to 1 could indicate correlated noise rather than signal. Finally, columns seven and eight uses instruments and propensity scores aggregated at quintiles or deciles of the effectiveness distribution, rather than ventiles as in the main specification.

Table D.3: Robustness of IV validation test: alternative specifications

	Alternative specification							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forecast coefficient (κ)	1.00	0.97	1.01	1.01	0.98	0.94	0.94	0.91
	(0.11)	(0.13)	(0.11)	(0.11)	(0.11)	(0.12)	(0.12)	(0.14)
p-value (κ = 1)	0.980	0.827	0.930	0.951	0.889	0.626	0.637	0.519
p-value (Omnibus test)	0.829	0.783	0.826	0.740	0.783	0.702	0.726	0.822
Number of instruments	19	19	19	19	19	19	9	4
First-stage F-statistic	275.52	173.19	268.65	279.13	277.56	351.60	425.62	579.80
Share facing admission risk	0.22	0.22	0.22	0.22	0.22	0.23	0.22	0.22
Observations	42792	42792	42792	42792	42792	30230	42792	42792

Notes: This table shows the results of the IV test for validity of school-by-program impacts on predicted earnings (Equations 6 and 7), using a number of alternative specifications and restrictions: **(1)**: controlling for certain admission, i.e. $p_{iv} = 1$ for all ventiles v; **(2)**: re-centering instruments $\tilde{Z}_{iv} = Z_{iv} - p_{iv}$ as in Borusyak & Hull 2023; **(3)**: controlling for discrete values of p_{iv}, for all ventiles v; **(4)**: linear Grade 9 GPA control; **(5)**: fixed effects for ventiles of Grade 9 GPA; **(6)**: restricting to jpg cells with more than 50 students; **(7)**: binning instruments and propensity scores at deciles of the effectiveness distribution; **(8)**: binning at quintiles.

E Bias-corrected variance decomposition

E.1 Econometric approach

The main results of this paper are concerned with the parameters β_{jpg}. These estimates capture the earnings impact of attending a particular school j, program p for a student in GPA tercile g, relative to average return among students in GPA tercile g. Of independent interest is the variability in β_{jpg}: how much does returns vary among schools and programs? This is a question about the variance of the estimated effects. This section details the estimation of this variance, as well as the variance decomposition of β_{jpg} into school and program effects.

Set-up. I obtain estimates $\hat{\beta}_{jpg}$, collected in the vector $\hat{\beta} = (\hat{\beta}_{111}, ..., \hat{\beta}_{JPG})'$. Even if unbiased, these are noisy estimates of the true parameters β:

$$\hat{\beta} = \beta + \epsilon.$$

I assume that this noise has mean zero, consistent with unbiasedness of my estimates, but allow for an arbitrary covariance structure: $E[\epsilon] = 0$ and $E[\epsilon\epsilon'] = \Omega$. While Ω is unknown, an unbiased estimator exists in the form of the sampling covariance matrix of $\hat{\beta}$, denoted $\hat{\Omega}$. This matrix has the squared standard errors of the elements $\hat{\beta}_{jpg}$ on its main diagonal, but may also capture arbitrary correlation in sampling noise in its off-diagonal elements.

Bias-corrected variance estimation. Suppose we compute the enrollment-weighted variance of the estimated parameters $\hat{\beta}$.[43] A reasonable plug-in estimator is the sample variance of $\hat{\beta}$, weighted by enrollment. Let w_{jpg} be the share of students in cell j, p, g, and stack these in vector w:

$$V_{\hat{\beta}} = \frac{1}{\sum_{j,p,g} w_{jpg}^2} \sum_{j,p,g} w_{jpg}(\hat{\beta}_{jpg} - \sum_{j,p,g} w_{jpg}\hat{\beta}_{jpg})^2 = \hat{\beta}' A \hat{\beta}, \tag{23}$$

[43] I investigate enrollment-weighted variances throughout to remain consistent with the main analysis. This enrollment-weighted variance captures the degree of variation in earnings impacts actually experienced by students.

where $A = \frac{1}{1-\sum w^2} diag(w) - ww'$ is a matrix that weighs and demeans the elements of $\hat{\beta}_{jpg}$, along with a degrees of freedom correction. As shown in Kline et al. (2020) and Walters (2024), this sample variance will reflect the true variation in β as well as excess variation due to noise (ϵ):

$$E[V_{\hat{\beta}}] = (\beta + \epsilon)' A (\beta + \epsilon) \tag{24}$$

$$= \beta' A \beta + E[\epsilon' A \epsilon] \tag{25}$$

$$= V_\beta + tr(A\Omega), \tag{26}$$

where the last equality follows from properties of the trace operator. Since an estimator for Ω exists, so does an estimator of the noise term $tr(A\Omega)$. The bias-corrected estimator of the variance of β is then given by:

$$\hat{V}_\beta = \hat{\beta}' A \hat{\beta} - tr(A\hat{\Omega}). \tag{27}$$

Decomposition of variance into school and program components. In Equation 8, I project the estimated school-by-program effects $\hat{\beta}_{jpg}$ into separable school- and program effects, as well as a residual capturing deviations from separability:

$$\hat{\beta}_{jpg} = \hat{\theta}_{jg} + \hat{\gamma}_{pg} + \hat{\nu}_{jpg}. \tag{28}$$

I am ultimately interested in how much of the variation in β is explained by school- and program effects, respectively. I also want to know how important the more complicated school-by-program interactions (ν) are. However, since $\hat{\beta}_{jpg}$ is estimated with noise, this noise will spill over to the components on the RHS of Equation 28. Hence, the sample variances of $\hat{\theta}$, $\hat{\gamma}$ and the estimated residuals $\hat{\nu}$ will not capture their true variances.

Fortunately, the approach introduced above can be extended to provide bias-corrected variance estimates of the projection components in Equation 28, since this projection is a linear function of the underlying estimates $\hat{\beta}$. Let $D = [D_\theta \ D_\gamma]$ be the design matrix collecting column vectors with dummies for schools (D_θ) and programs (D_γ). This matrix has one row for each school-by-program combination observed in the data. The effects $\hat{\beta}$ can be written as the fitted values from a weighted least squares regression on these school and program dummies:

$$\hat{\beta} = P_\theta \hat{\beta} + P_\gamma \hat{\beta} + (I - P)\hat{\beta}, \tag{29}$$

where the projection matrices are given by:

$$P = D(D'WD)^{-1} D'W \tag{30}$$

$$P_\theta = D_\theta [(D'WD)^{-1} D'W]_{1..J,.} \tag{31}$$

$$P_\gamma = D_\gamma [(D'WD)^{-1} D'W]_{J+1..J+P,.}. \tag{32}$$

The notation $X_{1..K,.}$ denotes the block matrix consisting of the first K rows of X, and all of its columns. P is simply the projection matrix from a WLS regression of $\hat{\beta}$ on the school and program dummies contained in D, since $W = diag(w)$ weighs cells by enrollment. P_θ and P_γ takes the J school and P program effects and projects these onto the school-by-program space. This is equivalent to assigning each school-by-program cell its respective school and program effect.

Note here, that e.g. the school effects are contaminated by the noise in $\hat{\beta}$:

$$P_\theta \hat{\beta} = P_\theta(\beta + \epsilon) = P_\theta \beta + P_\theta \epsilon \tag{33}$$

Thus, the sample variance of the school effects will have an additional bias term that is very similar to that of $V_{\hat{\beta}}$:[44]

$$E[V_{\hat{\theta}}] = (P_\theta \beta)' A (P_\theta \beta) + E[(P_\theta \epsilon)' A (P_\theta \epsilon)] \tag{34}$$
$$= (P_\theta \beta)' A (P_\theta \beta) + tr(A P_\theta \Omega P_\theta'), \tag{35}$$

using the same properties of the trace operator as before. This leads to the following bias-corrected estimator of the variance of school effects:

$$\hat{V}_\theta = (P_\theta \hat{\beta})' A (P_\theta \hat{\beta}) - tr(A P_\theta \hat{\Omega} P_\theta') \tag{36}$$

Expressions for the variances of the program effects and residuals use P_γ and $I - P$ in place of P_θ. Using the same derivation, bias-corrected covariances between e.g. school and program effects can also be obtained:

$$\hat{V}_{\theta,\gamma} = (P_\theta \hat{\beta})' A (P_\gamma \hat{\beta}) - tr(A P_\theta \hat{\Omega} P_\gamma'). \tag{37}$$

Finally, the full variance decomposition of the true parameters β involves the variances of the school effects, program effects and residuals, as well as their covariances:

$$V_\beta = V_\theta + V_\gamma + V_\nu + 2V_{\theta,\gamma} + 2V_{\theta,\nu} + 2V_{\gamma,\nu}. \tag{38}$$

The formulas derived in Equations (27), (36) and (37) provide plug-in estimators for all of these elements.

E.2 Results from the variance decomposition

Table E.1 shows the estimated components of Equation (38), both with (column 1) and without (column 2) correcting for estimation noise. As expected, the bias-correction approach reduces the estimated variance of the total effects (β), from 19.2 to 14.7. Hence, a standard deviation of the effect distribution is estimated at $\sqrt{14.7} \approx 3.8$ percentiles of earnings at age 30.

The variance decomposition reveals that both school and program effects are important in explaining the total effects. The estimated variance of the school and program effects are 6.5 and 12.2, respectively. Hence, programs matter more than schools in explaining the distribution of effectiveness. Further, school and program effectiveness appear to be negatively correlated: schools that offer programs with higher returns seem to offer lower school-level effectiveness, and vice versa. Finally, the separable school and program effects explain most of the variation in total effects: the variance of the residuals ν is estimated at only 0.23. This amounts to around 1.6% of the variation in total effectiveness. Hence, we miss very little by simply assuming that school and program effects are additively separable.

[44] Since I am always weighing the variance by the size of each j, p, g cell, the enrollment-weighted variance of the actual school effects $\hat{\theta}$ will be numerically identical to that of the fitted values $P_\theta \hat{\beta}$.

Table E.1: Bias-corrected variance decomposition of earnings impacts

	Bias-corrected		Raw	
	Variance	% of total variance	Variance	% of total variance
Total variance				
Var(β)	14.68		19.22	
Decomposition				
Var(θ)	6.53	44.5	9.07	47.2
Var(γ)	12.19	83.1	12.57	65.4
Var(ν)	0.23	1.6	2.49	13.0
2Cov(θ,γ)	-4.29	-29.2	-4.91	-25.5
2Cov(θ,ν)	0.06	0.4	0.05	0.3
2Cov(γ,ν)	-0.05	-0.3	-0.05	-0.3
Sum	14.68	100	19.22	100

Notes: This table shows the variance decomposition of the total effects on earnings rank at age 30 ($\hat{\beta}_{jpg}$), using the main analysis sample covering 1995-2008. This exercise decomposes the school-by-program effects into separable school and program components (θ_{jg} and γ_{pg}), as well as a residual term capturing deviations from the separability assumption (ν_{jpg}). This decomposition is performed in a weighted least squares of total effects on a set of school and program dummies (Equation 28). Bias-corrected variances and covariances, shown in the first column, are estimated as detailed in Section E.1. The second column shows raw sample variances and covariances, uncorrected for estimation noise. All variances and covariances are weighted by student enrollment in school-program-GPA tercile cells.