Economics of Education Review 84 (2021) 102148

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

# The impact of attending an independent upper secondary school: Evidence from Sweden using school ranking data

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ARTICLE INFO

*JEL-Classification:*
H44
I21
I26
I28

*Key words:*
Private provision
Mixed markets
Voucher school reform
Upper secondary education

ABSTRACT

Since the 1990s, the Swedish education market has gone through a dramatic transformation due to the introduction of voucher-funded independent schools. We make use of data on school applications to condition on student preferences for independent versus public education, and estimate a positive relationship between independent upper secondary school attendance and grades, graduation rates, and post-secondary education. We however also find strong indications of more lenient grading standards in independent schools, especially in schools organized as for-profit entities and in schools with a low share of qualified teachers. Our results suggest that, although independent school attendance seems to benefit the individual students in terms of higher grades and increased transition to post-secondary studies, grade inflation in the Swedish upper secondary independent schools may be a serious problem.

## 1. Introduction[1]

The effectiveness of upper secondary school determines the quality of the academic abilities supplied to universities, the quality of the vocational abilities supplied to the labor market, and individual labor market prospects in general. Allowing for alternative providers, with potentially diverse approaches to learning and educational management, to compete for students, has been discussed as a measure to increase overall educational quality (Friedman, 1955; Le Grand, 1991; Shleifer, 1998; and Hoxby, 2003). This has inspired the introduction of voucher-funded private schools in many countries, among them Sweden, which has since the 1990s seen its education market being dramatically transformed due to the introduction of so called independent schools[2], which now account for about a quarter of the upper secondary education sector.

The empirical evidence on the effects of voucher type schools on educational attainment is mixed.[3] Epple et. al., (2017) provides an interesting overview of the research on voucher school systems, and

This project has benefited from funding from the Swedish Research Council, Project number: 2014-01783, as well as from funding from Swedish Research Council for Health, Working Life and Welfare, Project number: 2018-01573. We are grateful for comments from seminar and conference participants at the Fifth Lisbon Research Workshop on Economics, Statistics and Econometrics of Education; the Swedish Institute for Social Research at Stockholm University (SOFI); the Research Institute of Industrial Economics (IFN); the Institute for Evaluation of Labour Market and Education Policy (IFAU); Maastricht University; Cambridge University; and GRIP seminar participants at Kristianstad University. We are particularly thankful for comments from two anonymous reviewers, as well as from Jan Sauermann, Anders Stenberg, Anna Sjögren, and Jonas Vlachos.

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[1] A snapshot of this project, which was made prior to generating the results presented in this manuscript, is available at https://osf.io/u8r43. After the registration of the snapshot, we encountered and corrected a few data errors, which have been corrected in this version. See the Appendix B, section B4, for details. Note also that the school level analysis in section 5 was not included in the snapshot.

[2] We use *independent* when referring to the Swedish version of publicly voucher funded but privately provided schools, following e.g. Böhlmark and Lindahl (2015), and *voucher* as a catch-all term for international versions. It can be pointed out however that the term *independent* school can be a bit misleading, in the sense that these schools are to a large extent subject to the same rules and regulations as the municipality operated schools (see section A2 in Appendix A for institutional details).

[3] The literature review provided in our paper is by no means exhaustive; we focus on more recent studies, or studies that are more relevant to our paper in terms of method or content relating to Sweden. We refer to e.g. Epple et. al., (2015) and Epple et. al., (2017) for reviews of earlier studies.

https://doi.org/10.1016/j.econedurev.2021.102148
Received 21 January 2021; Received in revised form 17 June 2021; Accepted 1 July 2021
Available online 24 July 2021

conclude that there are signs that voucher systems can have either positive or negative impacts depending on context or subgroup. They suggest that research with a broad focus is needed in order to better understand how voucher systems can be designed to preserve the positive impacts while avoiding the negative consequences.

If we zoom in on the literature on US charter schools, one finding that stands out is that charter schools that adhere to the "No Excuses" approach seem to have positive educational effects (Dobbie and Fryer, 2019; Angrist et. al., 2013; Dobbie and Fryer, 2013; and Abdulkadiroglu et. al., 2011).[4] On the other hand, Epple et. al., (2015) label online "cyber schools" as an apparently failed innovation, and Abdulkadiroglu et. al., (2018) suggest that participation in the Louisiana Scholarship (voucher) Program lowered student achievements.

To mention a few recent studies from other countries, Hahn et. al., (2018) show that high school students in private schools outperform high school students in public schools using data from Seoul, South Korea, whereas Kortelainen and Manninen (2019) report a small positive but statistically insignificant private school effect on matriculation exam scores in Helsinki, Finland.

Most of the existing literature on Sweden analyses the primary and lower secondary education sectors, and the overall finding is that the independent school expansion has improved students' educational attainment. The perhaps most cited reference, Böhlmark and Lindahl (2015), estimate moderate positive impacts on both students' grades and later educational outcomes (upper secondary grades, university attendance and years of schooling). [5] They deem this to be caused mainly by the competition/spillover effects on the local education market of an increased independent school presence, and not primarily by independent school students performing better. Based on a detailed assessment of the patterns of grades and standardized test results, and on a comparison with TIMSS data, they deem it likely that the results are *not* a reflection of inflated grade setting but that they reflect actual productivity gains.

Fewer studies have focused on the upper secondary level, in spite of the fact that the independent school expansion has been much larger there than in lower levels of education.[6] The main reference to this paper, Hinnerich and Vlachos (2017), analyses standardized test results for an approximate 10 percent subset of upper secondary Swedish students in public and independent schools. Their data contain information on tests that were corrected twice – first locally by the students' teachers and then by the Swedish Schools' Inspectorate. The results suggest that attending an independent upper secondary school leads to *lower* educational achievement in terms of externally corrected test scores, but *higher* achievement when measured as teacher-corrected test scores. Contrary to the findings in Böhlmark and Lindahl (2015) for the lower education levels, Hinnerich and Vlachos (2017) thus find that inflated grade setting is a serious concern for the upper secondary Swedish independent school sector.[7]

Given the disparate results from different contexts, we believe that a broad and thorough analysis of the relatively understudied Swedish upper secondary context can provide useful information. We add to Hinnerich and Vlachos (2017) by making use of data on the full population of upper secondary students, and by studying a wide array of outcomes, including educational and earnings outcomes measured after finishing upper secondary school. Another addition, compared to much of the previous literature, is that we have access to information on students' upper secondary school applications, in addition to a broad range of more standard background information. This enables us to estimate value added models (VAMs) of independent school attendance, which, in addition to covariates in the form of student demographics, family characteristics and previous academic achievements, also control for *preferences* for independent/public schools as reflected in the school applications.[8]

For school preferences to matter in the Swedish context, the choice between independent and public schools must be salient to students. One could argue that the choice is only weakly salient since most independent schools have a "general profile" and are by large subject to the same educational regulation as the public schools (see Section 2). On the other hand, public funding of independent schools has been a salient issue since the privatization reforms of the 1990s, and the attitude towards this feature of the Swedish school system is linked to the ideology of both politicians and voters.[9] Preferences linked to ideology could ultimately play a role in the choice of school, as could other general expectations regarding quality (or grade setting) in different types of schools. If commonly used observable characteristics are not sufficient to capture all characterizations of preferences, and if the left-out preference variation is correlated with both school choice *and* students' educational and labour market trajectories, the reliance on only commonly used background variables will cause omitted variable bias in the estimation of the independent school impact. We thus try to capture "residual" preference variation by making use of data from school applications.

Our study provides answers to the following list of issues:

- We document that a positive relationship between Swedish upper secondary independent school attendance and students' school results in terms of final GPA, graduation rates and standardized tests in English and Swedish holds conditional on *preferences* for independent/public education. The positive impact on final GPA is present in all parts of the ability distribution and for students with varying socio-economic background.
- We find that the positive upper secondary independent school impact extends to post-secondary education, and that there is a positive impact both on starting a post-secondary education and on taking university credits. This pattern is present for students in both Vocational and Academic tracks.

[4] Some of these studies are reviewed in Epple et. al., (2015), which provides an interesting overview of the US charter school literature.

[5] Other studies are Sandström and Bergström (2005), Ahlin (2003), Björklund et. al., (2005), and they find zero or positive effects, depending on the outcome variable. Hennerdahl et. al., (2018) estimate an insignificant independent school impact when they replicate Böhlmark and Lindahl but add controls for student composition at the school level (a factor that could be seen as a mediating variable). For references on school choice in general, and segregation within the school system, see e.g. Holmlund et. al., (2014) for an overview.

[6] A recent working paper, Edmark et. al., (2020), which studies the expansion of upper secondary independent schools using a methodology relatively similar to Böhlmark and Lindahl (2015), presents weak, non-robust evidence of a positive overall impact on grades and graduation rates, although the effect can be a consequence of changing educational track composition rather than a direct consequence of increased local private provision.

[7] This difference could reflect that differential grading standards is only a problem for the upper secondary level. It can however be mentioned that Vlachos (2019) finds signs of grade inflation in independent schools at the lower secondary level.

[8] As a complement to the value-added-models, we have also implemented a regression discontinuity (RD) analysis around admission thresholds to independent versus public schools. However, as this analysis resulted in too imprecise estimates to provide much guidance, it is presented briefly in section 4.5 and in more detail in Appendix D.

[9] Elinder and Jordahl (2013) show that right-wing local politicians and voters are more positive towards private provision of welfare services, including independent schools, than left-wing local politicians and voters. Politicians are also found to be overall more positive than voters.

- Although the above results suggest that independent school attendance is associated with more beneficial outcomes in terms of grades, graduation rates, test results, and post-secondary studies, we find indications – in line with Hinnerich and Vlachos (2017) – that grade inflation lies behind at least part of these effects. For instance, we find that students in independent schools are more likely to be "up-graded" on courses relative to their corresponding standardized test result, but no more likely to be "down-graded". This pattern is stronger in schools that are organized as for-profit corporations than in other independent schools.
- We document substantial heterogeneity in the estimated impacts across independent schools, for instance we find that independent schools with a low share of qualified teachers account for larger positive estimated impacts on student grades, but also show larger signs of inflated grade setting. Independent schools with a high share of qualified teachers in contrast give little value-added relative to the public schools in terms of student grades, but also show no or lower indications of inflated grade setting.

Although our results are in the form of value-added models, and can thus be criticized to suffer from omitted variable bias, we believe that the inclusion of student preferences for independent/public provision mitigates this risk. A causal interpretation of the results is furthermore strengthened by the fact that our results are robust to the use of different sampling and matching approaches, and, in particular, to omitted variable bias-correction following Oster (2019). The results are also robust to multiple hypothesis correction of p-values.

## 2. Institutional overview Swedish upper secondary education[10]

Swedish students enter a 3-year long upper secondary education at age sixteen, after ten years of compulsory schooling. Upper secondary school is divided into six academic and twelve vocational tracks, but there is also a 1–2 year long preparatory track for students whose grades do not qualify them to enter directly into any of the regular tracks. Upper secondary education can be provided either by the local governments (the municipalities); *public schools*, or by private entities; *independent schools*. Public and independent schools are both fully funded via school vouchers, which are primarily financed via the local income tax. Additional tuition fees are not allowed.[11]

Entering upper secondary education is associated with making two choices: a choice of school and a choice of educational track. The academic tracks are the more common types of tracks in both the independent and public schools, as can be seen in Table 1, followed by the vocational tracks. The preparatory track is rarely given by the independent schools.[12] Table 1 also shows that public schools tend to be larger than the independent schools, and that they have slightly fewer students per teacher.

Admission to a track and school combination is based on the grade sum, which is calculated as the sum of the grade credits of the 16 highest graded subjects from lower secondary school (GPS9). Students can apply on equal terms to all independent schools in the country, but students in the home admission region are given priority to the public schools in their region.

The regulatory framework for Swedish independent schools stems from a set of reforms implemented in the early 1990s, which greatly expanded the possibilities for independent agents to start schools and obtain full public funding. The result was a steady increase in the independent market share; from 1.7 percent in 1992 to a peak of almost 28 percent in 2013.[13] The reforms provided Sweden with a relatively liberal school system by international standards. For example, independent schools are allowed to be organized as for-profit entities, and in 2013 – the year of the last cohort in our data – 85 percent of independent upper secondary schools were organized as corporations.

The system for vetting and monitoring the independent schools was in the initial years relatively rudimentary, but has over time been transformed into a more comprehensive system, including stricter vetting procedures for new entrants and increased financial oversight. In 2008, the Swedish Schools Inspectorate, which is responsible for the authorization of independent schools and for overseeing all schools, was established. The Swedish Schools Inspectorate can close independent schools if severe violations are detected.[14]

The current government regulation concerning teaching- and instruction-related activities applies to independent and public providers alike: they are obliged to follow the same curriculum; meet the same educational goals; and use the same grading system. At the same time, school providers (or principals) have significant authority over decisions concerning hiring, wage setting, allocation of resources within the school, and allocation of (a minimum total amount of) instruction time between courses and over the school year. Both independent and public schools can profile themselves according to their offering of educational tracks, optional courses, and voluntary special instruction in sports, arts, or in other academic subjects, but only independent schools can have a religious profile.[15]

## 3. Data

Our baseline data set contains information on all individuals in Sweden that applied to upper secondary schools in 2009–2013, what we refer to as the "application register".[16] This data set is merged with a number of different population-wide registers held by Statistics Sweden (SCB) that contain information on students' school attendance, graduation status, grades and test results, parental and student background characteristics, early work life, and post-secondary education. In the sections below we present sample restrictions and describe the data variables.

### 3.1. Sample restrictions

Our sample restrictions are primarily motivated by the aim to obtain more comparable samples of independent and public school students, but we also need to drop observations due to missing information on key variables.[17] For example, we restrict the sample to students who are eligible to the regular educational tracks – meaning that we drop students whose low grades mean that they first need to take a preparatory track – and we also restrict the sample to students who were admitted to

[10] This section provides an overview of the institutional setting. A more detailed review is given in Appendix A.

[11] Students/parents may be asked to cover insignificant and occasional costs, such as public transport or entry to a museum during an excursion. Such costs may however not be imposed at a regular basis.

[12] Our empirical analysis will exclude preparatory track students.

[13] In 2013, the John Bauer (JB) group, containing around 30 independent schools, went bankrupt. In a robustness analysis that is reported in section C2 of Appendix C we find that excluding students affected by the JB bankruptcy has no qualitative impact on the results. See e.g. Sebhatu and Wennberg (2017) for an in-depth analysis of the JB-group.

[14] The Swedish School Inspection can temporarily take over the running of a municipal school. A proposal to expand the possibility to close also municipal schools is currently being investigated.

[15] For more detailed information about regulatory differences, see Section A2 in Appendix A.

[16] 2009 is the first year for which we observe the schools that students applied to – prior years of data show only listed track choices.

[17] Table B1 in section B1 of Appendix B contains detailed information on what variables are dropped and why.

**Table 1**
School characteristics – school year 2013/14.

| | No. school units[a] | School size (No. students) | Academic tracks (student shares) | Vocational tracks (student shares) | Preparatory tracks (student shares) | Students per teacher, adjusted[b] |
|---|---|---|---|---|---|---|
| Independent | 458 | 184 | 0.622 | 0.340 | 0.037 | 12.233 |
| Public | 882 | 273 | 0.562 | 0.320 | 0.118 | 11.160 |

[a] The definition of school units in the national School register changed in 2013. The new code is based on the division of headmaster responsibilities, rather than the physical school units. This has resulted in a large increase in administrative school units for the municipal schools: from 502 in school year 2011/12, to 766 in 2012/13 (after some schools had adopted the new system) and 882 in 2013/14 (when the new system was fully adopted. The number of independent schools was much less affected, and its numbers rather decreased over time; from 499 in 2011 to 484 in 2012 and 458 in 2013.

[b] The 0.5 percent top and bottom observations were excluded in order to eliminate the influence of extreme outliers, and the values presented have been adjusted to account for the shares of students attending Academic, Vocational and Preparatory tracks, as these tend to have different student/teacher ratios, see Table A1 in Appendix A for details. The raw data show a similar, but stronger, pattern of higher student/teacher ratios in independent schools.

one of their two top ranked schools. The missing observations and sample restrictions together shrink the sample size from 575,276 to 296,890 individuals[18]. A detailed exposé over the sample restrictions can be found in Appendix B, Section B1.

When controlling for school preferences, our preferred strategy is to restrict the sample to students who have ranked both an independent and a public school among their top two choices. This means that all students in the sample are unlikely to have strong aversions to either type of school, thus closing one selection channel. This restriction leaves us with a sample of 72 745 observations; our "main sample".

### 3.2. Student background variables and other covariates

The richness of Swedish register data allows us to control for a comprehensive list of covariates on student background characteristics. Table 2 displays the full list, and the averages values, of the covariates for students attending independent and public schools respectively (Columns 1–2).[19] The table also shows the p-values for the differences (Column 3) and normalized differences (à la Imbens and Rubin, 2015) (Column 4). The student background characteristics in independent and public schools come across as remarkably similar. The (normalized) difference is less than 2 percent of the pooled standard deviation for 16 out of 19 variables, including all variables on students' demographic and family background and prior academic achievement.[20] The sample used is the main sample that includes only students who have listed a mix of independent and public schools among the top two choices. As can be seen in Table B.4 in Appendix B, the observed selection is more pronounced when this restriction is not imposed. Preferences for independent/public schools, as reflected by the applications, are thus correlated with student background characteristics, and we take this as an indication that the observed preferences may additionally capture unobserved differences between students, as discussed in the introduction.

The few variables for which the differences between the two samples in Table 2 are more pronounced, relate to geographic aspects and previous independent school attendance. Independent school students are more likely to live in metropolitan municipalities, whereas students attending a public school are more likely to live in urban municipalities, and independent school students are also somewhat more likely to have attended an independent school earlier, in grade 9. Notably, however, independent school attendance in grade 9 is a far from perfect predictor of upper secondary school attendance. Moreover, the correlation between ranking an independent school highly in the application to upper secondary school is only weakly positively correlated with grade 9 independent school attendance (the correlation coefficient between attending independent school in grade 9 and ranking an independent school as 1st (2nd) preference is 0.12 (0.11)).[21] This suggests that the information on independent school attendance in grade 9 is not a perfect

**Table 2**
Student background characteristics in independent/public schools.

| | Independent | Public | P-value | Normalized diff. |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Household disposable income | 246 095 | 243 240 | 0.177 | 0.010 |
| One parent business income | 0.145 | 0.142 | 0.237 | 0.009 |
| One parent unemployed | 0.186 | 0.179 | 0.019 | 0.017 |
| One parent post-sec educ | 0.550 | 0.554 | 0.276 | -0.008 |
| Both parents born in Sweden | 0.730 | 0.729 | 0.739 | 0.002 |
| One parent born in Sweden | 0.124 | 0.123 | 0.545 | 0.004 |
| No parent born in West | 0.082 | 0.082 | 0.729 | 0.003 |
| Born in Sweden | 0.946 | 0.944 | 0.149 | 0.011 |
| Born in West | 0.024 | 0.026 | 0.144 | -0.011 |
| Born in non-West | 0.030 | 0.030 | 0.554 | -0.004 |
| Female | 0.514 | 0.519 | 0.237 | -0.009 |
| Independent9 | 0.186 | 0.172 | 0.000 | 0.037 |
| GPS9 | 226.7 | 226.9 | 0.417 | -0.006 |
| High MA Test9 | 0.117 | 0.118 | 0.661 | -0.003 |
| High SW Test9 | 0.089 | 0.087 | 0.317 | 0.007 |
| High EN Test9 | 0.224 | 0.216 | 0.015 | 0.018 |
| Metropolitan municipality | 0.453 | 0.411 | 0.000 | 0.086 |
| Urban municipality | 0.435 | 0.479 | 0.000 | -0.087 |
| Rural municipality | 0.111 | 0.111 | 0.803 | 0.002 |
| *Observations* | *35,098* | *37,647* | *72,745* | *72,745* |

Table notes: Household income is represented per individual and in year 2016 monetary value. GPS9 refers to the students' final grade sum from lower secondary education, and ranges from 0–320. All other included variables are in the form of dummy variables. High MA Test9 means getting a high grade on the standardized Math test in lower secondary school, and the corresponding variables for Swedish and English are denoted High SW Test9 and High EN Test9. All variables are measured in the year that the students start upper secondary education – the year they turn 16. Missing values are replaced with imputed pooled averages. P-values refer to the raw differences. The normalized difference between samples 1 and 2 for covariate $X$ is calculated as $(\overline{X}_1 - \overline{X}_2)/\sqrt{(S_1^2 + S_2^2)/2}$ (Imbens and Rubin, 2015).

[18] This "original data set" refers to the sample size (575,276) after observations with missing observations on the following variables have been dropped: school ownership, educational track, and personal ID.

[19] See also Table B2 in section B2 of Appendix B for basic summary statistics for all covariates.

[20] We use the unweighted pooled standard deviation, as suggested by Imbens and Rubin (2015). Since sample sizes in the independent and public school samples are relatively similar (35,098 and 37,647), weighting by sample size would not make much of a difference.

[21] This relatively low correlation could indicate that independent/public provision is not a main concern for many parents/students. It could however also reflect different access to independent schools at the upper and lower secondary level, for example due to geographic or academic achievement restrictions (note that access to upper secondary schools and tracks is determined by students grades from lower secondary school).

substitute for preferences as observed in the upper secondary school application register, and thus motivates the inclusion of the latter.

### 3.3. Outcome variables

The cohorts in our data enter upper secondary education in 2009–2013 and are thus expected to graduate in 2012–2016. As 2016 is the last year recorded in our data, all outcomes will be short-term in nature. While we are restricted to short-term outcomes, we have aimed to use the detailed register data to capture a broad range of the options available to students after upper secondary school. Our outcome variables include not only university/college studies, but also other post-secondary educations and labor income. The outcome variables are listed and categorized into three groups in Table 3.[22]

The outcomes in panel A are measured during, or at the end of, upper secondary school. They include: an indicator for switching school type during upper secondary school – from an independent school to a public school, or the reverse; the final 12th grade GPA, measured as the percentile rank by year among all graduating students[23]; a dummy variable for graduating on time, i.e. after three years in upper secondary school; and a dummy variable for remaining in upper secondary school for a 7th term, i.e. after the expected graduation.

In panel B we collect outcomes that are based on standardized tests taken in Mathematics, Swedish, and English throughout upper secondary school. Our data lacks information on the exact test *scores*, but we do have information on the *grades* awarded on the tests. Based on this, we generate one outcome variable indicating whether the student was awarded a "high" grade or not, and one indicating whether the student was awarded a "pass" grade (all grades above fail) or not.[24] The standardized tests are supposed to be a guide for the teachers' assessments of students, but they are not strict determinants of the course grades. We therefore also construct two dummy variables indicating if the test grade is higher or lower, respectively, than the grade on the corresponding course. The timing and the number of tests taken varies across the educational tracks, and students in some tracks are tested in several courses in the same subject, resulting in multiple test observations per student.[25] In our baseline estimations we run the regressions on the student level averages for each outcome, such that each student gets the same weight.[26]

It should be emphasized that all grade-based outcomes, including GPA and test grades, are teacher-set, i.e. not set by evaluators external to the schools. Several authors have found evidence of school-level differences in grading standards (Nordin et. al., 2019; Diamond and Persson, 2017), and moreover, that independent schools overall tend to grade students more generously (Vlachos, 2019; Hinnerich and Vlachos, 2017; Wikström and Wikström, 2005). We will return to how this affects the interpretation of our results when the results are presented and discussed, as well as in the concluding discussion.

Panel C lists our post-graduation outcomes. We measure post-secondary school studies in the fall and create two indicator variables: the first takes on value 1 for all types of post-secondary studies, including both tertiary education (advanced and vocational training), and "complementary" types of studies such as adult complementary education, active labor market educational programs and Swedish for immigrants (see a complete list in section B3.3 in Appendix B). The second dummy variable excludes the "complementary" types of studies. We also capture university studies separately by creating a dummy variable that takes on value 1 for taking university credits equivalent to 50 percent or more of a term of fulltime studies (≥15 Uni cred). Finally, we measure labor market earnings in the form of a dummy variable for earning a "substantial amount" of labor income. We follow Forslund et. al., (2017), and define this amount as yearly earnings of at least half of the median annual work income among 45-year-olds.[27]

We recognize that studying post-graduation outcomes in the same year as graduation is probably premature, since many students choose to take a sabbatical year to work or study abroad, and we will therefore

**Table 3**
Outcome variables in independent/public schools.

| | Independent | Public | P-value | Normalized diff. |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A. Graduation and grades* | | | | |
| Switch independent/public | 0.088 | 0.061 | 0.000 | 0.103 |
| Pctile GPA12 | 57.143 | 53.217 | 0.000 | 0.140 |
| Graduate on time | 0.823 | 0.808 | 0.000 | 0.039 |
| 7th term | 0.093 | 0.103 | 0.000 | -0.034 |
| *Panel B. Standardized tests* | | | | |
| *Mathematics* | | | | |
| High test grade | 0.059 | 0.050 | 0.000 | 0.038 |
| Pass test grade | 0.763 | 0.772 | 0.003 | -0.021 |
| Test grade>Course grade | 0.017 | 0.013 | 0.000 | 0.032 |
| Test grade<Course grade | 0.301 | 0.271 | 0.000 | 0.066 |
| *Swedish* | | | | |
| High test grade | 0.095 | 0.069 | 0.000 | 0.092 |
| Pass test grade | 0.948 | 0.944 | 0.000 | 0.019 |
| Test grade>Course grade | 0.098 | 0.099 | 0.614 | -0.004 |
| Test grade<Course grade | 0.305 | 0.285 | 0.000 | 0.045 |
| *English* | | | | |
| High test grade | 0.128 | 0.103 | 0.000 | 0.076 |
| Pass test grade | 0.978 | 0.977 | 0.812 | 0.002 |
| Test grade>Course grade | 0.108 | 0.111 | 0.195 | -0.010 |
| Test grade<Course grade | 0.183 | 0.148 | 0.000 | 0.093 |
| *Panel C. Post-graduation* | | | | |
| Study | 0.383 | 0.368 | 0.000 | 0.032 |
| Study no-prep | 0.312 | 0.295 | 0.000 | 0.037 |
| Uni cred≥15 | 0.152 | 0.144 | 0.011 | 0.022 |
| Work≥50% | 0.259 | 0.279 | 0.000 | -0.045 |

Table notes: The normalized difference for covariate X is calculated as $(\overline{X}_1 - \overline{X}_2) / \sqrt{(S_1^2 + S_2^2)/2}$ (Imbens and Rubin, 2015).
Post-graduation outcomes are measured in the year following the graduation year, i.e. 4 years after entering upper secondary school.
The pre-registered snapshot version of this table contained an error in this variable. This has been corrected, which is why the variable content for this variable differs from the same table in the snapshot.

[22] Summary statistics for the outcome variables are also available in Tables B5.A and B5.B in section B3 of Appendix B.

[23] As a complement to estimating the impact of the percentile rank of GPA12, Table C3 in section C3 of Appendix C shows the corresponding estimations for the standardized value of the variable.

[24] A high grade is defined as grade value "A" for the grading system introduced for cohorts starting upper secondary education in 2011 onwards, and as grade value "MVG" for the previous system. A pass grade is defined as all values above F for the former grading system and all values above "IG" for the latter. For more details, see section B3.1 in Appendix B.

[25] In order to account for the fact that the exact timing and number of tests taken varies across tracks, and sometimes even across schools within a track, we include fixed effects for the timing in terms of the year, school grade and term, and for the course tested. These are also relevant to include due to the fact that the grading system changed during the time studied, see section B3 of Appendix B for details.

[26] We have also, as a robustness test, estimated the regressions when using each test as the level of observation. The results, which are available upon request, are overall very similar.

[27] According to Forslund et. al., (2017), this corresponds roughly to six months' worth of wages for a full time employed janitor in the municipal (public) sector. A "substantial amount" is redefined as a quarter of the median income among 45-year olds, when we study outcomes in the graduation year, as the students were still in upper secondary education approximately half of that year.

show results when measuring outcomes one year after graduation in our main results tables (4 years after entering upper secondary school). This in effect means that we are excluding the 2013 cohort from the analysis of post-graduation outcomes. The results for the outcomes measured in the expected graduation year and including cohort 2013 are available in Table C4 in Appendix C.

Table 3 shows the outcome variable averages for students attending independent and public schools respectively, as well as the p-values and normalized differences. Similar to Table 2, we use the main sample, which is restricted to students who have listed a combination of independent and public schools as the two top choices. According to the raw differences in Table 3, independent school students are: more likely to switch school type, to have a higher GPA12, and are somewhat more likely to graduate on time. In all test subjects, students in independent schools are more likely to receive the highest grade. The grade awarded on a course is also more often higher than the grade on the corresponding standardized test among independent school students. Among the post-graduation outcomes, the largest difference, with a higher value for students in public schools, is found in the propensity to work at least 50 percent one year after graduation. Finally, students in independent schools are somewhat more likely to be registered in post-secondary education and to take university credits in the year after graduation.

## 4. Empirical methods and results

### 4.1. Overview of the VAM-analysis

We start out by noting that the basic regression equation for our analysis is the following:

$$y_i = \alpha + \beta IND_i + u_i, \quad (1)$$

where $y_i$ denotes some outcome for upper secondary student $i$; $\alpha$ is an intercept; and $IND_i$ is a dummy variable indicating if the students attended an upper secondary independent school instead of a public school as measured at the start of upper secondary education; and $u_i$ is the error term.

If independent and public students were comparable in all aspects apart from what type of school they attended, the $\beta$-coefficient from Equation (1) would capture the average causal effect of attending an independent – instead of a public – upper secondary school. In practice, however, independent and public students may very well differ systematically in ways that are correlated with the outcomes studied. As was explained in the introduction, we deal with this selection problem by first restricting the sample to only include students that have listed a combination of the two school types as the top two choices in their upper secondary school applications. We then address potential remaining student selection by conditioning on observable characteristics by using VAM-regressions.

Although this strategy cannot be thoroughly tested for omitted variable bias, it does have some support in Abdulkadiroglu et. al., (2011), who find that a conditional-on-observables approach yields test score estimates for oversubscribed Boston charter schools that are similar to the estimates obtained when leveraging charter school lotteries.[28] Angrist et. al., (2017) similarly argue that even though there is a bias contained in VAMs, it is small enough to render observational estimates useful from a policy perspective. We take this as suggesting that VAMs yield policy relevant estimates also in the present Swedish case, in particular as we have access to a broad set of student background variables including prior achievement and school preferences.[29] In addition, we shall see below that our results are robust to several robustness tests, including the omitted-variable adjustment suggested by Oster (2019).

Before estimating the VAMs, we implemented coarsened exact matching (CEM) to improve the common support between our two groups of students. This was done by forcing exact matching on the following variables, and keeping only cells containing both independent and public school students before running the regressions[30]: gender, parents' country of birth (three dummies), GPS9 quintile, and, depending on the specification, either the county[31] where the student attends lower secondary school, or the school the student attended in 9th grade.[32]

The two thus generated samples were then alternately used for estimating VAMs; regression models where student background variables and student's prior academic achievements are controlled for in a flexible manner (see the table notes to Tables 4.A–C for the exact covariate specification). This means augmenting equation (1) to the following regression equation:

$$y_{imcp,t+1} = \alpha_t + \beta IND_{it} + \delta \boldsymbol{A}_{i,t-1} + \varphi \boldsymbol{X}_{it} + u_{mt} + u_{c,t-1} + u_{pt} + u_{imcp,t+1} \quad (2)$$

Similarly to equation (1), $y_{imcp,t+1}$ denotes some outcome variable for student $i$, at time $t+1$ ($t+1$ refers to time periods after entering upper secondary school; note that the exact timing of measurement varies across outcomes), and $IND_{it}$ is a dummy variable for attending independent school measured in October of the first year of upper secondary school. Indicator $t$ thus refers to the point in time when the students enter upper secondary education. Furthermore, $\alpha_t$ denotes time (or cohort) fixed effects; $\boldsymbol{A}_{it-1}$ denotes prior academic achievement; and $\boldsymbol{X}_{it}$ denotes the remaining set of student background characteristics. These are, depending on the characteristic, either measured during the year at which the student enters upper secondary education, or are time-invariant (country of birth, gender). Upper secondary school municipality fixed effects are included in $u_{mt}$, 9th grade school fixed effects are included in $u_{c,t-1}$, upper secondary educational track fixed effects are included in $u_{pt}$, and $u_{imcp,t+1}$ is the error term.

Under the assumption that the included covariates and fixed effects successfully capture all systematic background differences between independent and public school students that remain in the restricted and matched samples and that are correlated with the outcome variable, the $\beta$-coefficient in equation (2) corresponds to the average treatment effect (ATE) of attending an independent school measured in the first term of upper secondary education, in the sample population. Or rather, to be more precise, since some students may switch across independent and public schools between the point in time when we measure independent school attendance and upper secondary school graduation, the estimated coefficient will correspond to an intention to treat effect (ITT). It can be noted, however, that there is a strong positive correlation between attending an independent upper secondary school in the first and

[28] Studying the effectiveness of charter schools in New York City, Dobbie and Fryer (2013) also show that observational estimates and lottery estimates can be qualitatively similar, although in their case the observational estimates are somewhat smaller in size. Deming (2014) present observational estimates that are similar to lottery estimates, using data on charter school lotteries in Charlotte-Mecklenburg, North Carolina.

[29] As was commented in the introduction, we complemented this strategy by an RD analysis that compares the outcomes of students around the margin of admission to an independent or a public school, but as these overall yielded very imprecise estimates, they are reported only briefly in section 4.5, and in more detail in Appendix D to this paper.

[30] Note that the CEM-cells are not included in the regressions. Adding them as fixed effects however gives qualitatively similar results, see Tables C12.A–C12.C in section C8 of Appendix C.

[31] There are 21 counties in Sweden, sometimes referred to as "regions".

[32] The variables used for exact matching have been chosen to align ourselves with the previous literature, in particular with Hinnerich and Vlachos (2017), but also Dobbie and Fryer (2019). Another alternative would be to use the 290 municipalities as regional matching variable, instead of 9th grade school or county. This gives very similar results, see Tables C13.A and C13.B in section C8 of Appendix C. When we match on 9th grade school, we also add cohort dummies, since the 9th grade school IDs change over time.

the final year of upper secondary education; running equation (2) with attending an independent school in the final year as outcome variable yields a β-coefficient of around 0.85.[33] This suggests that the ITT is a slight understatement but still a close approximation of the ATE.

It shall be made clear that the β-coefficient of equation (2) measures the effect of independent school attendance on students' observed educational achievements and post-secondary studies and earnings, in relation to similar (in terms of observable characteristics) municipal school students. We will however not be able to identify the exact sources of any estimated performance differences; i.e. whether they are due to higher productivity of independent schools; to more lenient grade setting; or if they are a result of a different peer composition. Our analysis also does not address the question of whether independent school entry affects the productivity of the public schools. This study shall thus be viewed as a complement to studies using other strategies and thus identifying other parameters.[34]

### 4.2. Results of the VAM-analysis

The VAMs are estimated on three different samples, resulting in three 3-column tables – one for each outcome group of Table 3. Results for the outcomes in group A, "Graduation and grades", are shown in Table 4.A. Column 1 shows the results for the most restricted sample, where we enforce the preference restriction (having applied to a combination of independent and public schools), and common support with respect to the interaction of 9th grade school, gender, parents' country of birth (three dummies) and GPS9 quintile. The specification in Column 2 is our preferred specification; here we enforce the preference restriction and common support with respect to upper secondary *school county* instead of *9th grade school*. Enforcing common support with respect to *school county* has little impact on the number of observations; the number of observations before imposing the restriction is 72 745, as we reported in Section 3. In Column 3 we use the full observational sample without adding preference restrictions or preference controls. The difference between Column 3 and the first two columns thus shows the potential importance of utilizing school application data to control for preferences for independent and public schools.

**Table 4.A**
Graduation and grades.

| | (1) | (2) | (3) |
|---|---|---|---|
| Switch independent/public | 0.0279*** | 0.0231*** | 0.0343*** |
| Standard error | (0.0057) | (0.0049) | (0.0042) |
| P-value | [0.0000] | [0.0000] | [0.0000] |
| Observations | 28,837 | 70,623 | 288,762 |
| Mean(Outcome) | 0.0713 | 0.0740 | 0.0437 |
| Pctile GPA12 | 4.4302*** | 4.4906*** | 4.5410*** |
| Standard error | (0.3474) | (0.3080) | (0.2955) |
| P-value | [0.0000] | [0.0000] | [0.0000] |
| Observations | 25,578 | 61,898 | 254,937 |
| Mean(Outcome) | 57.2213 | 55.1276 | 55.2433 |
| Graduate on time | 0.0229*** | 0.0288*** | 0.0200*** |
| Standard error | (0.0051) | (0.0041) | (0.0038) |
| P-value | [0.0000] | [0.0000] | [0.0000] |
| Observations | 29,440 | 72,220 | 294,580 |
| Mean(Outcome) | 0.8337 | 0.8156 | 0.8269 |
| 7th term | -0.0133*** | -0.0153*** | -0.0099*** |
| Standard error | (0.0038) | (0.0028) | (0.0024) |
| P-value | [0.0004] | [0.0000] | [0.0000] |
| Observations | 29,440 | 72,220 | 294,580 |
| Mean(Outcome) | 0.0924 | 0.0981 | 0.0934 |
| Preference restriction | YES | YES | NO |
| CEM on 9th grade school | YES | NO | NO |
| CEM on county | NO | YES | YES |

Table note: All regressions above include the following covariates: upper secondary school municipality dummies, prior achievement as controlled for by a cubic form of GPS9 and GPS9 quintile dummies, as well as 6 dummies representing pass/high test result in Math/Swe/Eng in 9th grade, 9th grade school dummies, track dummies, log household income, income decile dummies; and dummies indicating the following: gender, born in western country (excl. Sweden), born in non-western country, at least one parent post-secondary education, both parents born in Sweden, one parent born in Sweden, both parents born in non-western country, negative or zero household income, at least one parent is self-employed, at least one parent is unemployed, and cohort. Columns 1 and 2 also include a dummy indicating admission to first ranked school. Standard errors are clustered on upper secondary school. *** p<0.005, ** p<0.01, * p<0.05

Although sample sizes vary greatly as a result of alternating these restrictions, the results in Table 4.A are overall very stable across specifications. The results in the first row suggest that independent school students are more likely to switch to another school type (type meaning independent/public) than are public school students. The effect size of 2.3 p.p. in Column 2 (our preferred specification) is quantitatively similar to the raw difference presented in Table 2, and is quite sizeable, given that the average likelihood of switching school type is 7 percent.[35][36]

Further results in Table 4.A suggest that independent school attendance has a positive impact on the percentile rank of the student's GPA in the 12th grade. The effect size of 4.49 percentiles in Column 2 is quantitatively similar to the raw difference in Table 2, and is, in our view, a moderately sized impact. The positive independent school impact on the likelihood of graduating on time of 2.88 p.p. in Column 2 is somewhat larger than the raw difference, while the negative independent school impact of staying behind for a 7th semester at 1.53 p.p. is in line with the raw difference. These effect sizes are moderate to relatively large, given that the sample averages are 82 percent for the graduation rate and 10 percent for the likelihood of staying behind.

Results for standardized test outcomes are shown in Table 4.B. The result that stands out the most is the positive coefficient on the probability of getting a course grade that is higher than the corresponding standardized test grade. The coefficient is statistically significant and/or economically interesting across all samples and subjects. The effect sizes of 4.62 p.p in Mathematics (Column 2), 2.39 p.p in Swedish (Column 5), and 4.01 p.p in English (Column 8), are medium sized compared to the average likelihoods to get a higher course than test grade in the regression samples; 29 percent for Math and Swedish and 16 percent for English. We believe that a plausible interpretation for the independent schools' higher propensity to "up-grade students" relative to the standardized test, is that independent schools overall have more lenient

[33] Results are available in Table C3 in section C3 of Appendix C.
[34] See Urquiola (2016) for a related discussion.
[35] In a robustness analysis, which is presented in section C2 in Appendix C, we show that the estimate for switching school is only somewhat smaller (0.018, compared to 0.023 in Table C2.A) when we exclude observations affected by the 2013 bankruptcy of the corporate JB-schools from the sample. As we lack access to school names, we cannot drop students attending JB-schools. Instead, we have dropped all observations belonging to a track×municipality×year combination where a JB-school was present. The results from the analysis excluding the JB-cases are also very similar to the baseline estimates for the other outcome variables.
[36] Table C3 in section C3 of Appendix C shows corresponding estimations for an outcome dummy variable for switching school irrespectively of the type of school the student switches to. The result suggest that independent school students are, conditional on covariates and fixed effects, 3.5–4 percentage points *less* likely to change school. How does this square with the result in Table 4.A that independent school students are *more* likely to change *type* of school (where type refers to independent/public)? A possible explanation is that this is a mechanical effect of there being more and larger public schools – meaning that the chances of finding an open slot for a student who wants to change schools (irrespectively of whether from an independent or public school) is higher in the public school sector.

**Table 4.B**
Standardized test results.

| | Mathematics | | | Swedish | | | English | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| High test grade | 0.0028 | 0.0034 | 0.0050*** | 0.0190*** | 0.0204*** | 0.0227*** | 0.0161*** | 0.0155*** | 0.0182*** |
| Standard error | (0.0028) | (0.0020) | (0.0017) | (0.0059) | (0.0046) | (0.0043) | (0.0051) | (0.0040) | (0.0036) |
| P-value | [0.3165] | [0.0878] | [0.0028] | [0.0014] | [0.0000] | [0.0000] | [0.0016] | [0.0001] | [0.0000] |
| Observations | 20,062 | 48,106 | 193,412 | 22,052 | 52,515 | 222,275 | 20,798 | 50,202 | 201,527 |
| Mean(Outcome) | 0.0500 | 0.0429 | 0.0447 | 0.0934 | 0.0807 | 0.0825 | 0.1204 | 0.1105 | 0.1062 |
| Pass test grade | -0.0090 | -0.0028 | 0.0003 | 0.0065 | 0.0065* | 0.0055* | 0.0028 | 0.0023 | 0.0039*** |
| Standard error | (0.0066) | (0.0054) | (0.0048) | (0.0034) | (0.0027) | (0.0021) | (0.0022) | (0.0016) | (0.0012) |
| P-value | [0.1733] | [0.6045] | [0.9443] | [0.0593] | [0.0151] | [0.0102] | [0.2056] | [0.1528] | [0.0018] |
| Observations | 20,062 | 48,106 | 193,412 | 22,052 | 52,515 | 222,275 | 20,798 | 50,202 | 201,527 |
| Mean(Outcome) | 0.7543 | 0.7320 | 0.7439 | 0.9446 | 0.9409 | 0.9433 | 0.9747 | 0.9735 | 0.9736 |
| Test grade>Course grade | 0.0042 | 0.0039* | 0.0044*** | -0.0056 | -0.0035 | 0.0004 | 0.0035 | -0.0036 | 0.0011 |
| Standard error | (0.0022) | (0.0017) | (0.0014) | (0.0053) | (0.0040) | (0.0032) | (0.0053) | (0.0044) | (0.0038) |
| P-value | [0.0629] | [0.0228] | [0.0020] | [0.2888] | [0.3735] | [0.8975] | [0.5052] | [0.4134] | [0.7788] |
| Observations | 19,322 | 46,244 | 185,415 | 20,482 | 48,724 | 202,921 | 19,962 | 48,159 | 193,922 |
| Mean(Outcome) | 0.0172 | 0.0166 | 0.0144 | 0.0970 | 0.1000 | 0.0957 | 0.1026 | 0.1110 | 0.1062 |
| Test grade<Course grade | 0.0510*** | 0.0462*** | 0.0533*** | 0.0183* | 0.0239*** | 0.0223*** | 0.0307*** | 0.0401*** | 0.0419*** |
| Standard error | (0.0102) | (0.0088) | (0.0086) | (0.0089) | (0.0069) | (0.0057) | (0.0073) | (0.0058) | (0.0051) |
| P-value | [0.0000] | [0.0000] | [0.0000] | [0.0397] | [0.0005] | [0.0001] | [0.0000] | [0.0000] | [0.0000] |
| Observations | 19,322 | 46,244 | 185,415 | 20,482 | 48,724 | 202,921 | 19,962 | 48,159 | 193,922 |
| Mean(Outcome) | 0.2927 | 0.2902 | 0.2857 | 0.2865 | 0.2866 | 0.2829 | 0.1651 | 0.1641 | 0.1572 |
| Preference restriction | YES | YES | NO | YES | YES | NO | YES | YES | NO |
| CEM on $^{9th}$ grade school | YES | NO | NO | YES | NO | NO | YES | NO | NO |
| CEM on county | NO | YES | YES | NO | YES | YES | NO | YES | YES |

Note: Regressions are performed on individual means within each subject. All regressions above include the following covariates: upper secondary school municipality dummies, prior achievement as controlled for by a cubic form of GPS9 and GPS9 quintile dummies, as well as 6 dummies representing pass/high test result in Math/Swe/Eng in 9th grade, 9th grade school dummies, track dummies, log household income, income decile dummies; and dummies indicating the following: gender, born in western country (excl. Sweden), born in non-western country, at least one parent post-secondary education, both parents born in Sweden, one parent born in Sweden, both parents born in non-western country, negative or zero household income, at least one parent is self-employed, at least one parent is unemployed, and cohort. Regressions on test outcomes also include test specific dummies. Columns 1–2, 4–5, and 7–8 also include a dummy variable indicating whether the student was admitted to the first ranked choice. Standard errors are clustered on upper secondary school. *** p<0.005, ** p<0.01, * p<0.05

grading standards. It is appropriate to remind the reader that not only the course grades, but also the test grades, are set by the teachers, and that earlier research has found that independent schools grade tests more leniently than the public schools (Hinnerich and Vlachos, 2017). This means that using "up-grading" as a measure of grade inflation is probably an underestimation since the anchor it itself (the test grade) is likely subject to more lenient grading.

A second striking result in Table 4.B is the positive coefficient on the probability of getting a high grade on the standardized tests in Swedish and in English. This probability increases with 2.04 p.p. in Swedish, and 1.55 p.p. in English if students attend an independent school, which are sizeable impacts given that the average values in the regression sample are 8 percent for Swedish and 11 percent for English. The coefficient for Mathematics is statistically insignificant and small in columns (1) and (2), but statistically significant in the full sample estimation in column (3).

Finally, Table 4.C shows the results for the post-graduation outcomes. Column 2 suggests that attending an independent school has a positive impact of 1.99 p.p. on the probability of being registered in any type of post-secondary studies one year after graduation, and of 2.44 p.p. if we excluding studies that are of a preparatory/catch-up type. These are reasonably large impacts relative to the corresponding average shares in the regression sample, which are 37 percent for post-secondary education overall, and 30 percent when preparatory/catch-up type educations are excluded. The effect on the probability of earning at least 15 university credits is also positive at 1.42 p.p., which is relatively large compared to the 15 percent in the regression sample that earn this amount of credits in the first year after the expected graduation year.[37][38] The effect on the probability of earning labor income corresponding to at least a half time job is negative at -1.70 p.p., which is to be expected if working and studying are complementary activities. The share in the regression sample that earns this amount of work income is 27 percent. The effects shown in Table 4.C are all of the same sign as the raw differences in Table 2, and the effect sizes are quantitatively similar.

As mentioned in Section 3.3, Table 4.C measures the outcome variables one year after students are expected to graduate from upper secondary school. As a complement, Table C4 in Appendix C shows the corresponding results but measured a year earlier. When comparing Columns 2 the appendix table shows smaller estimates on "Study no prep" and the work income variable, while coefficients for the outcomes "Study" and "UC15" are insignificant.

[37] In section C9 of Appendix C we investigate in more detail what types of post-secondary educations lie behind these effects. The results suggest that the positive independent school coefficients for enrolling in post-secondary studies reflect a positive impact on enrolling in university. Furthermore, we find that the positive impact on taking university credits reflects a positive impact on STEM/Medicine related courses among Vocational track students, and on non-STEM/non-Medicine courses among Academic track students.

[38] This result contrasts to Skolverket (2018), which reports that independent school students are less successful in taking university credits than students from municipal schools. Our study however differs from Skolverket (2018) in terms of both the outcome and the population, and can thus not be compared directly to this report: whereas we study the likelihood to take at least 15 university credits and include students who did not enroll in the reference category, Skolverket's analysis is restricted to students who enrolled in university, and their outcome variable is defined as the number of university credits taken as a share of the number of credits of the courses the students enrolled in.

**Table 4.C**
Post-graduation outcomes.

| | (1) | (2) | (3) |
|---|---|---|---|
| Study | 0.0261*** | 0.0199*** | 0.0165*** |
| Standard error | (0.0068) | (0.0050) | (0.0041) |
| P-value | [0.0001] | [0.0001] | [0.0001] |
| Observations | 22,598 | 55,430 | 230,160 |
| Mean(Outcome) | 0.3531 | 0.3746 | 0.3817 |
| Study no-prep | 0.0265*** | 0.0244*** | 0.0195*** |
| Standard error | (0.0061) | (0.0046) | (0.0039) |
| P-value | [0.0000] | [0.0000] | [0.0000] |
| Observations | 22,598 | 55,430 | 230,160 |
| Mean(Outcome) | 0.2911 | 0.3029 | 0.3118 |
| Uni cred>=15 | 0.0137*** | 0.0142*** | 0.0111*** |
| Standard error | (0.0048) | (0.0034) | (0.0026) |
| P-value | [0.0043] | [0.0000] | [0.0000] |
| Observations | 22,598 | 55,430 | 230,160 |
| Mean(Outcome) | 0.1464 | 0.1479 | 0.1573 |
| Work >=50% | -0.0207*** | -0.0170*** | -0.0233*** |
| Standard error | (0.0067) | (0.0049) | (0.0044) |
| P-value | [0.0021] | [0.0005] | [0.0000] |
| Observations | 22,585 | 55,386 | 229,988 |
| Mean(Outcome) | 0.2825 | 0.2697 | 0.2802 |
| Preference restriction | YES | YES | NO |
| CEM on $^{9}$th grade school | YES | NO | NO |
| CEM on county | NO | YES | YES |

Note: All outcomes are measured one year after graduation. All regressions above include the following covariates: upper secondary school municipality dummies, prior achievement as controlled for by a cubic form of GPS9 and GPS9 quintile dummies, as well as 6 dummies representing pass/high test result in Math/Swe/Eng in 9th grade, 9th grade school dummies, track dummies, log household income, income decile dummies; and dummies indicating the following: gender, born in western country (excl. Sweden), born in non-western country, at least one parent post-secondary education, both parents born in Sweden, one parent born in Sweden, both parents born in non-western country, negative or zero household income, at least one parent is self-employed, at least one parent is unemployed, and cohort. Columns 1 and 2 also include a dummy variable indicating whether the student was admitted to the first ranked choice. Standard errors are clustered on upper secondary school. *** p<0.005, ** p<0.01, * p<0.05

### 4.3. Robustness and specification tests

Our VAM-results are robust to various robustness and specification tests, all of which support the findings of the above presented specifications:

i) Our main regressions do not to include the CEM-cells as fixed effects. Including them gives qualitatively similar results, as can be seen in Section C8 Appendix C.
ii) In a robustness analysis, which was conducted on our preferred VAM-specification (Column 2 in Tables 4.A–4.C), we found that the results are robust to correcting for multiple hypotheses following the procedure proposed in Hochberg (1988) and used by e.g. Banerjee et. al., (2015). (The results are presented in Table C6 in Appendix C.)
iii) We also investigated the robustness of the baseline VAM-estimates to unobservable variables bias following Oster (2019) [39], who in turn builds on (among others) Altonji et. al., (2005). The results, which are reported in Tables C7.A and C7.B in Appendix C, suggest that the main results of this paper are *not* driven by unobserved selection.
iv) Finally, it can be noted that our results are not very sensitive to restricting the sample to individuals with preferences for both types of schools (compare full sample results in Column 3 with other columns). Our results thus provide some support for the conditional on observables analysis in Hinnerich and Vlachos (2017), which does not make use of information on student preferences. In Section C1 in Appendix C we show how the estimates in Tables 4.A–4.C vary with the stepwise inclusion of covariates. In Section C4 in Appendix C we furthermore show results when retaining the full sample and instead controlling for preferences by including dummy variables. The results are qualitatively and quantitatively similar.

### 4.4. Heterogeneity analysis

In this section, we summarize our findings from several heterogeneity analyses. The results referred to below are obtained from running the VAM on subsamples of students within the full-sample specification corresponding to column 3 in Tables 4.A–4.C in order to maximize sample sizes. The results are presented in Tables C9.A–C9.E in Appendix C.[40]

One main finding is that the positive estimated impact on GPA12 is present for students in Academic and Vocational tracks alike; for students with different parental education background and country of birth; and for students with different prior academic achievements. However, the varying coefficient sizes suggest that the positive effect is larger for academic track students than for vocational track students. Interestingly, the estimates furthermore suggest that independent school attendance induces *vocational track* students to pursue higher education instead of entering the labor market; the coefficient on the likelihood to earn a substantial amount of work income is negative and relatively large for vocational track students, and the impact on attending non-preparatory post-secondary studies is relatively large and positive.[41] Additional regressions (available in section C9 in Appendix C) suggest that the larger estimated impact on starting a post-secondary education for vocational students, reflects a higher likelihood to start university studies, in particular in the STEM-related field.

Furthermore, the positive coefficient on GPA12 is largest in the mid (T2) tercile in the prior achievement distribution compared to the low (T1) and high (T3) tercile.[42] Lastly, the positive effect on GPA12 is larger in independent schools that are organized as corporations compared to schools that are not.

The positive impact on being up-graded in Math and English is present and economically interesting in all subsamples except for students in schools that are *not* run as corporations. In other words, the positive effect on the probability of being up-graded seems to be

[39] This analysis was carried out using the STATA command psacalc, see Oster (2019).

[40] The results are by large similar for the preference-restricted specification corresponding to column 2 of Tables 4.A–C, available in Tables C10.A–C10.E, section C7 of Appendix C. We abstained from running the regression on the more restricted sample of column 1 in Tables 4.A–4.C as that would result in very small samples for some subgroups.

[41] The non-preparatory post-secondary educations include university educations as well as non-university post-secondary studies, see section B3.3 in Appendix B for the full list, but exclude education categories that we deemed to be more of repeat or complementary to upper secondary studies in character, such as Adult education, Active Labour Market Education and Swedish for Immigrants.

[42] Groups are constructed based on the distribution of the full cohorts of students, and the number of students in the lower tercile is thus lower than in the higher intervals, since our regression sample excludes the students with the lowest grades who need to attend the preparatory track before moving to regular upper secondary education.

completely driven by up-grading in independent schools that are run as corporations.[43][44] A larger independent school impact on the propensity of getting up-graded is also found in the two lower terciles in the prior achievement distribution compared to the top tercile, which is to be expected since more students in the top tercile are likely to score the top grade on the test, meaning that they cannot be "up-graded" on the course grade.

The estimated coefficients on getting pass- and high test grades in Math are close to zero for all groups of students, including students in corporate schools. Under the assumption that test results in Math are more reliable measures of ability, since correcting Math is more formulaic, the across the board zero-results for Math can be viewed as an indication of zero ability gains (at least in Math) from independent school attendance. We can however neither rule out negative nor positive ability gains, as we cannot be certain that the assumption holds.

### 4.5. Complementary regression discontinuity analysis

As a complement to the VAM-results presented above, we also carried out a Regression Discontinuity based estimation of the impact of being admitted to an independent school instead of a public one (or the reverse).[45] As the results of this analysis did not add much insight, due to large confidence intervals, we have chosen to only give a brief account of the results here, and refer to Appendix D for a more thorough presentation.

Overall, as shown in section D9 Appendix D, it was slightly more common to find statistically significant coefficients that went in the same direction as the VAM-analysis than to find significant estimates in the opposite direction. This holds in particular for the outcomes related to graduation and grades, and for the post-graduation "Study"-variable. For the standardized test outcomes, the statistically significant coefficients in many cases differed in sign and size compared to the generally small VAM coefficients.

It shall be noted, however, that the overwhelming majority of coefficients from the RD-analysis were not statistically significantly different from zero. Although there is thus a slightly larger prevalence of statistically significant estimates that point in the same direction as the VAM-estimates, and this could be viewed as providing some validation for the VAM-estimations, our overwhelming impression is that the RD-estimates are too often statistically insignificant – and when not; overall too messy – to provide any strong insights.

## 5. School-level effects[46]

In this section we investigate if the reported average impacts in Section 4 mask substantial variation across the range of independent schools. We do this by first estimating the school-specific impact for each independent school, using all public schools as the reference group. The regressions are based on the full observational sample without imposing the preference restriction (Column 3 in Tables 4A–4C), so as to maximize the power to identify school level effects.

We then plot the school level estimates against i) the share of qualified teachers in the school[47], and ii) the number of students per teacher.[48][49][50] We restrict the presentation to school level effects with respect to outcomes GPA12 and test results in Math. The reason for why we single out Math is that the correction of the Math test is likely to be less open to teacher discretion than the Swedish and English tests,[51] meaning that the Math test is probably our most unbiased measure of student academic performance – although we acknowledge the obvious limitation that it is restricted to abilities in Math only. The joint pattern for Math and GPA12 can thus provide us with clues on whether better academic achievement or merely more lenient grading standards explains the estimated effects.

As a side note, the mirror images of the below figures for the public schools – based on similar estimations but with the public school level coefficients and all independent schools as reference category – are shown in section C6.3 in Appendix C. The main message from those figures is that the variation of the public school effects, in terms of the standard deviation of the estimated coefficients, is very similar to that of the independent schools (see section C6.4 of Appendix C for a summary).

In Figure 1.A we show that the estimated independent school effects regarding GPA12 are negatively related to the share of qualified teachers. Attending an independent school with a relatively low share of qualified teachers is estimated to give rise to a GPA gain corresponding to on average somewhere between 5–10 percentiles, but the effect approaches zero as we travel up the distribution. There is no discernable pattern regarding the tests results (high/pass test grade) in Math, or the likelihood to be down-graded (get a higher test than course grade), but the relationship is clearly negative regarding the probability of being up-graded on the Math course in relation to the test grade. This suggests that schools with lower shares of qualified teachers are driving the "up-grading" effect that we presented in Section 4.[52]

The correlations using students per teachers as school attribute are weaker in general, see Figure 1.B. The fitted lines are relatively flat for all outcomes, although there is some indication of a smaller independent school impact on GPA12, and a larger impact on the likelihood to up-grade, among independent schools with a lower number of students per teacher.[53]

The most prominent pattern from this school level exercise is that the independent schools with a high percentage of qualified teachers tend to have lower estimated value added in GPA compared to the average public school, but that they also show signs of less inflated grade setting than the independent schools further down in the respective distributions.

[43] It can be noted that the vast majority of upper secondary independent schools are organized as corporate schools: approximately 85% of the independent school students in our estimations samples attend corporate independent schools.

[44] Our indicator of corporate status is based on information from school year of 2013/2014. In order to increase the sample size, we apply this information also to the previous years of data. Although corporate status is likely to be stable over time for most schools, we know that some changes have occurred, for instance when some previously corporate schools were bought by a foundation after the bankruptcy of the JB group in the spring of 2013.

[45] In addition to using RDD, we added an DID-layer to the analysis, and subtracted the "above-below-admission-threshold-difference" in control groups of students around the admission threshold who had the same type of school (in terms of independent/public) as top and fall-back option. Appendix D therefore denotes these estimations as RD/DID.

[46] The analysis in this section was not included in the snapshot on the analysis plan that was registered in October 2019, https://osf.io/u8r43, but is rather an ex-post exploratory exercise.

[47] Qualified here means having a teaching degree.

[48] See section B5 in Appendix B for details on the school level data.

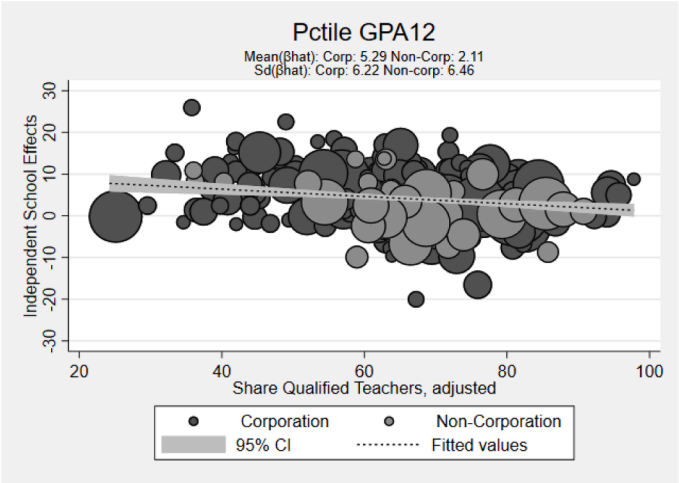
[49] The fitted line is based on a regression weighted by the school size. Unweighted estimation produces similar patterns.

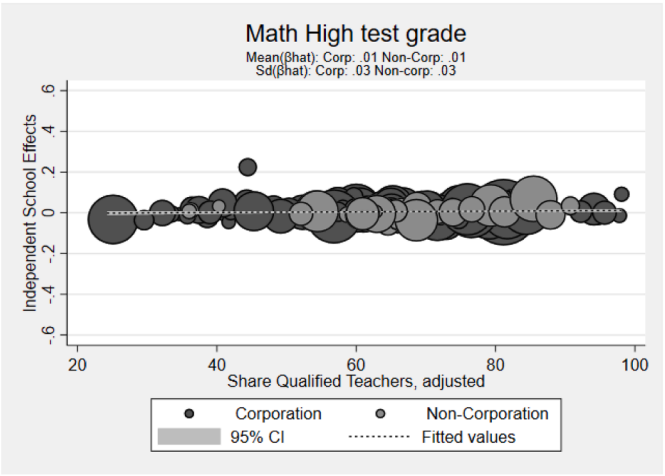
[50] The working paper version of this paper, Edmark and Persson (2020), additionally includes figures based on the school level averages of incoming students' grade sums.

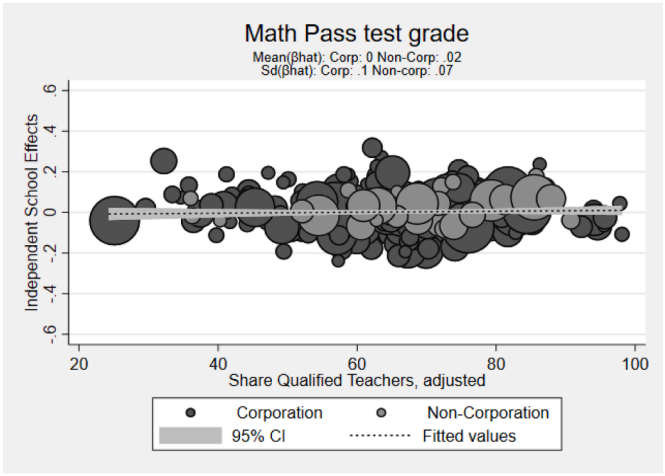
[51] Vlachos (2018) notes that the grades awarded on standardized tests that were externally re-graded deviate less from the internally (teacher) graded tests in Math than in the other tested subjects.

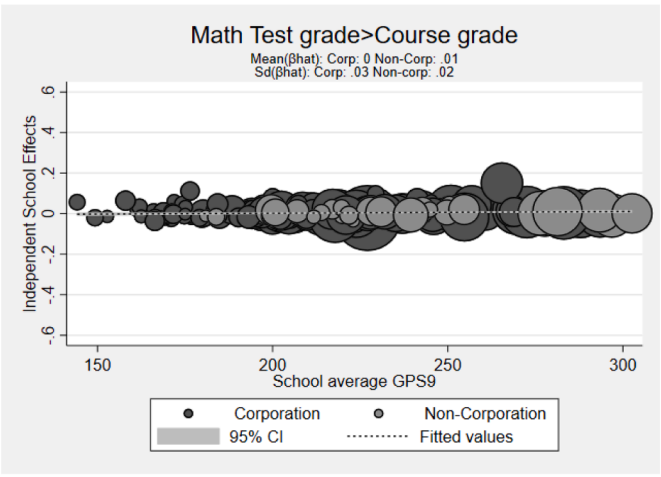
[52] The corresponding figures for English and Swedish are shown in section C6.1 in Appendix C. They diverge somewhat from the above when it comes to the figures on getting a high test grade and getting a lower course than test grade, but show similar patterns for remaining outcomes.

[53] The corresponding figures for English and Swedish (Appendix C, section C6.2) are also mostly relatively flat, although there is some upward slope for the outcome getting a high test grade in Swedish, and getting a lower test and course grade in English.



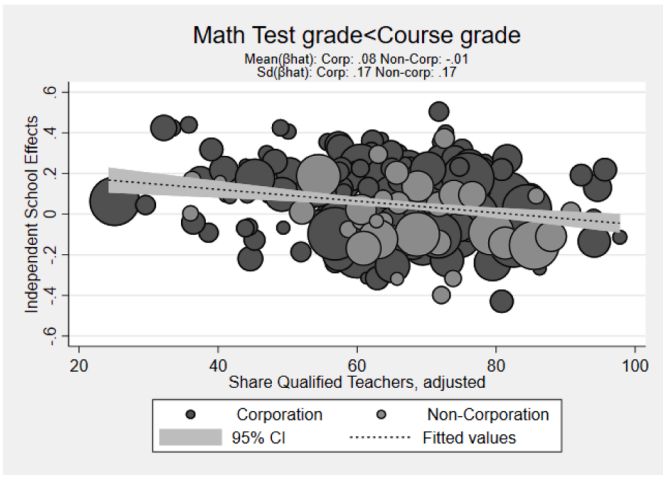
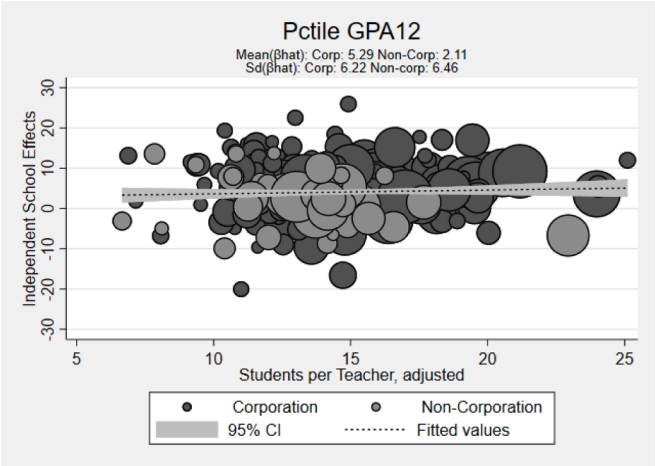
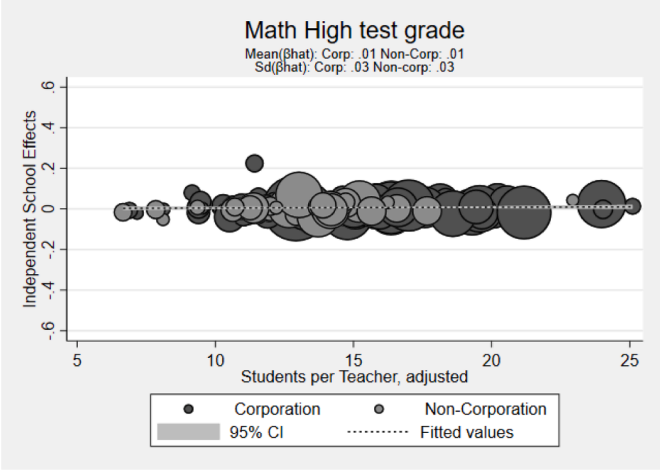
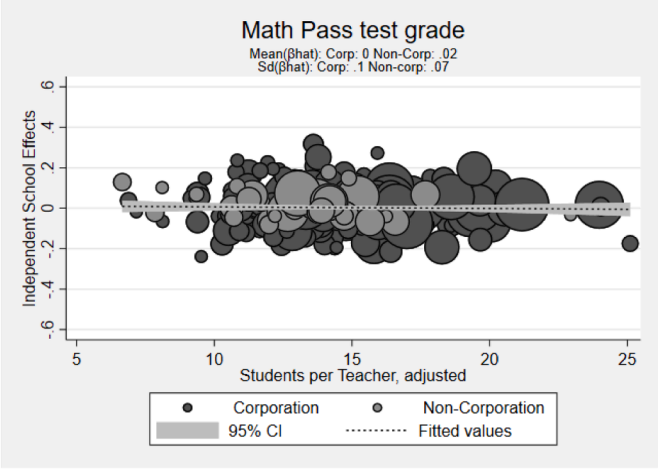



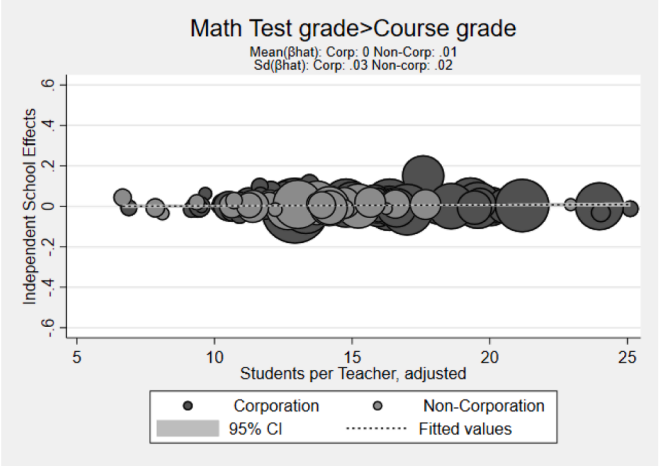

**Fig. 1.A.** the share of qualified teachers

Notes: The figures exclude a small number of coefficients for which the number of student observations in the regression sample fell below 30, and, for the binary outcome variables, a small number of cases where the estimated coefficients exceeded one in absolute value. Markers reflect school size. The fitted line is based on school-size weighted regression. The share of qualified teachers is adjusted for school level differences in educational track shares.[56].



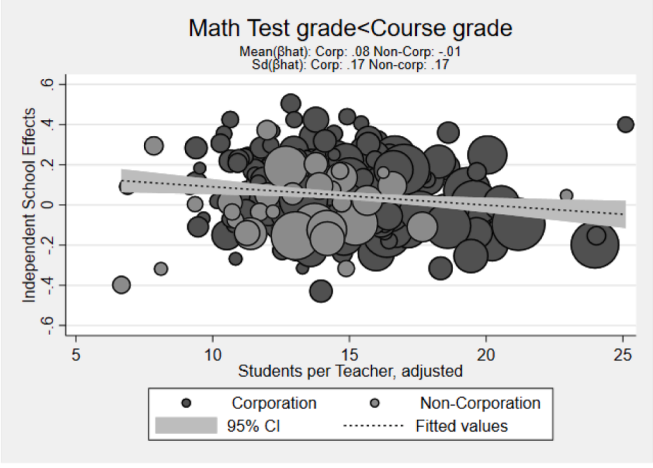




**Fig. 1.B.** students per teacher,
Notes: The figures exclude a small number of coefficients for which the number of student observations in the regression sample fell below 30, and, for the binary outcome variables, a small number of cases where the estimated coefficients exceeded one in absolute value. Markers reflect school size. The fitted line is based on school-size weighted regression. The student/teacher-measure has been adjusted for the influence of different educational track shares between schools.[57].

## Concluding discussion

Evaluation of the educational value added of independent schools in Sweden is a complicated matter: All student achievement indicators that are available for the full population are assessed internally, by the teachers, and the previous literature has indicated that independent schools in general tend to have more generous grading standards than the public schools, meaning that teacher-assessed achievement measures cannot be relied upon to reflect actual educational

achievements.[54] As noted by Vlachos (2019), this is not surprising given the institutional combination of trust and high powered incentives in the Swedish school system. While schools are allowed to grade courses and tests with a substantial amount of freedom, a majority of independent schools are run for-profit on a market where educational achievements are a means to attract students and thereby voucher revenue.

Our results suggest that independent schools have moderate positive added value with respect to teacher-assessed achievements such as final GPA and standardized tests. However, we also, in line with the previous Swedish literature, find support for independent school grade inflation. This is based on our finding that independent students' course grades are more often "up-graded" relative to their test grade, but no more likely to be "down-graded". Our school level analysis suggests that such lenient grading behavior is more prevalent among independent schools with a low share of qualified teachers. Moreover, we find a positive independent school effect on the standardized test grades in Swedish and in English, where teachers have relatively high degrees-of-freedom when grading, but not in the more formulaic subject of Mathematics.

Unfortunately, our data does not allow us to quantify the independent school grade inflation effect, as we lack a credible measure of student achievement – an "anchor" – against which we can compare the teacher-assessed measures. Indeed, Hinnerich and Vlachos (2017) have found that the standardized tests, that we use as a base of comparison for the course grades, are also subject to more lenient grade setting in the independent schools – suggesting that our results likely underestimate the extent of grade inflation.[55] The fact that we cannot quantify grade inflation implies that we can neither rule out negative nor positive "true achievement gains" of attending an independent school.

Our results for post-graduation outcomes suggest that attending an independent school has a positive impact on the likelihood of both enrolling in further studies and of taking university credits. These results suggest that receiving a higher grade – even if it only or partly reflects more generous grade setting – can have positive consequences for students' long term educational trajectories. This is in line with Diamond and Persson (2016), who find that receiving an inflated grade in Swedish lower secondary school has long-term positive consequences on education and earnings. Such impacts may work through motivational and/or signaling effects. Nordin et. al., (2019) also estimate a positive impact on earnings from graduating from grade inflating schools, and suggest that the effect mainly goes through attending better universities and study fields with higher earnings potential. The implication is that individuals are not sorted into higher education according to their innate abilities, which in turn leads to unfair selection into higher education at the individual level, and allocative inefficiency for society as a whole.

In addition to estimating the independent school impact on a broad range of outcomes, including potential indicators of grade inflation, we have evaluated the role of preferences, as reflected in upper secondary applications. Although we found that restricting the sample to students who show no indications of strong preferences either for or against independent schools improved balance with respect to several family background variables, we also found that the overall results were very similar when we made no use of this information. We interpret this as support for the notion that other observable characteristics on student background, as used in previous studies such as Hinnerich and Vlachos (2017), is probably sufficient to capture differences in preferences that are of relevance for academic and labour market outcomes (at least for the type of short run outcomes that we study).

To sum up, the independent school effects that we document in this study consistently indicate that attending an independent school on average benefits the individual in terms of grades, graduation rates and post-secondary studies. Whether or not this also amounts to societal benefits is more difficult to establish. When schools operate on a school market where students bring resources via vouchers, incentives to attract students by showing high achievement gains will be present, and some of our results seem to suggest, in accordance with earlier studies mentioned above, that these incentives can distort the grading standards in Swedish upper secondary school, which is harmful both from a meritocratic and efficiency standpoint. Although these results are estimated for the Swedish setting, it is likely that similar forces can arise in other settings that exhibit the same type of high-powered incentives.

## Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2021.102148.

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[54] Besides Hinnerich and Vlachos (2017), other studies have also documented generous grading standards among independent schools in Sweden. Wikström and Wikström (2005) show that students from upper secondary independent schools have a relatively high final GPA in comparison to their results on the Swedish SATs. Vlachos (2019) finds that independent schools on the compulsory level set higher final course grades, as compared with their students' results on test grades, similarly to our findings for upper secondary school students. He also finds that the discrepancy is larger for the arguably more reliable Math test than for the other tests. (This can be compared to our finding that the course grade discrepancy in Math is larger than in Swedish – but, on the other hand, similar in magnitude to English). Finally, the Swedish National Agency for Education report evidence of more generous grading among independent schools in compulsory school (Skolverket, 2019a), and that students from schools where grades appeared to be more generously set, tend to perform worse in upper secondary school compared to students with a similar grade from a school with stricter grading standards (Skolverket, 2019b).

[55] Hinnerich and Vlachos even find a *negative* value added of independent school attendance based on externally re-graded standardized tests. As these externally graded tests are only available for a subset of schools, we can however not use their results to quantify the exact extent of grade inflation (based on using externally graded tests are an anchor) for our sample.

[56] The adjustment was made in the following manner: First, we generated educational track indicators what were stable over time, taking into account the changes to the track structure that were made during the period (the main adjustment consists of grouping the academic social science tracks into one joint category). We then estimated school level regressions with the share of qualified teachers as outcome variable, and the school level track shares as explanatory variables. Based on the regression output, we calculated the regression equation, using the estimated track share coefficients, and inserting the average values for (the school level) track shares values in the regression sample. We also calculated the residuals within the regression sample. Finally, the track-share adjusted values were generated by adding the abovementioned calculated regression equation to the residual values. This procedure resulted in values exceeding 100 percent for eight of the schools in our sample. These cases were replaced with value 100.

[57] The adjustment was made in the same manner as for the share of qualified teachers, see footnote to Figure 1.A.

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