Pulled-in and Crowded-out: Heterogeneous Outcomes of Merit-based School Choice *

Antonio Dalla-Zuanna
Bank of Italy

Kai Liu
University of Cambridge

Kjell G. Salvanes
Norwegian School of Economics

Abstract

We study the effects of changing the rule that defines how students are selected into high schools in a context where school capacity is fixed. Schools for which demand exceeds supply must necessarily exclude some students from enrollment. We provide a theoretical framework to analyze the overall effect of policy changes, taking into account the crowding-out effect. By exploiting a reform that implemented merit-based allocation in Norway, we show that we can identify the relevant parameters. The reform had an overall negative effect because of the negative impact on crowded-out students. Different allocation rules would result in higher average outcomes.

*We gratefully acknowledge comments by Debopam Bhattacharya, Sandra Black, Ian Walker, Ellen Greaves, Marco Ovidi and the participants of several workshops and seminars. We thank Fanny Landaud for generously sharing her code used to estimate admission cutoffs.

1 Introduction

Many government programs face resource constraints. Because of these resource constraints, policy makers rely on allocation rules to ration program participation. When considering a change in the allocation rule, policy makers need to know not only the effects for potential beneficiaries, but also the externality on those who may be crowded out. Most program evaluation studies have focused on identifying the effects on the beneficiaries, whereas a complete cost-benefit analysis of changing allocation rules requires one to account for the effects on both the beneficiaries and the crowded-out.

One example of public program facing resource constraint is schooling service. Policy makers often need to consider how to assign different students to different schools of fixed capacity. For example, moving from a system where assignment is rigidly based on residence to one where more choice is offered to students is generally believed to increase the productivity of the education system (Hoxby, 2000; Hsieh and Urquiola, 2006; Lavy, 2010).[1] However, when schools differ in their quality and have capacity constraints, even free choice systems have to impose some allocation rules. Hence, a change in the rule suggests that there are likely to be winners and losers; students who would have attended a good school in the first system may not be allowed to do so in the latter system. As a result, when evaluating the impact of a different system, we need not only to understand the effect of attending a better school for students who gain access to it, but also the effect of being crowded out from the best schools for students who lose access.

In this paper, we develop a framework to identify policy-relevant sufficient statistics to analyze the effects of implementing a "merit-based" system on the basis of grades obtained in middle school, when high schools have a limited number of slots.[2] We show that, when school capacity is fixed, the policy-relevant parameters to evaluate the benefit from expanding access to oversubscribed (or "competitive") schools are the average treatment effects of attending one of these competitive schools for students who are targeted by the expansion *and* for those students who are crowded out. In a world where each student has individual-specific gains from attending a competitive high school, there is no reason a

[1] This potential gain in productivity comes from improved match between students and schools. In some contexts, this policy can also increase school quality by fostering competition between schools (Hoxby, 2000, 2004). However, when competition is low, because public resources are not assigned to schools on the basis of their performance and because school capacity is fixed, this mechanism is not in place. For instance, in our setting, each school has a predetermined number of classes with a given number of students in each class.

[2] In the setting analyzed, the number of slots are limited due to a cap on class-size, and restriction on the number of class rooms.

priori to believe that the treatment effects for the targeted and the crowded-out students are equal.[3]

In order to separately identify these two treatment effects, we exploit the exogenous variation provided by a reform that took place in the early 2000s in Bergen, Norway. The reform changed the way students are assigned to high schools, moving from a system based on catchment areas to one where choice is free. However, because school capacity is fixed and some schools are oversubscribed as a result of the reform, a rule based on merit has been set to allocate students to oversubscribed schools. In particular, school choice is offered first to students who obtain higher grades in exit exams from middle school. This generates cutoffs based on middle school GPA for admission at more competitive schools. We exploit the exogenous variation offered by the reform combined with these cutoffs to identify the effect of attending a competitive school for students who gain and those who lose access to these schools as a result of the implementation of the merit-based allocation system.

We find that considering the crowding out effect of this reform is crucial when evaluating its impact. In fact, the effect on high school completion for high-ability students who gain access to competitive schools is, at best, small and the effect on university outcomes is zero or even negative. On the contrary, being crowded out of the most selective schools for low-ability students has a significant negative impact on both high school completion and university completion and this is particularly true for the low-SES part of the crowded-out population. As a result, the average effect of an expansion of access to competitive schools targeted to high-ability students is negative on any outcome. Our estimates imply that an expansion of access to competitive schools for low-ability students from low SES at the expense of high-ability students from high SES provides the largest increase in average outcomes.

This paper is directly related to the literature that investigates the effect of increasing school choice (Lavy, 2010; Wondratschek, Edmark, and Frölich, 2013; Deming, Hastings, Kane, and Staiger, 2014). As oppose to them, we take into account that school capacity is fixed. This implies that an increase in school choice for some groups must come at the cost of a decrease in the availability of places in the more competitive schools for other groups. Our paper also relates to the rich literature on the effect of attending more selective high schools on students' academic outcomes. Previous studies generally

[3]The heterogeneous effect of attending a "better" school is well emphasized in Walters (2018), who, using the Roy selection framework, shows that the gains from attending charter schools in Boston are unevenly distributed along both observable and unobservable dimensions.

focus on specific margins, often comparing students who are just above the admission cutoff with those who are just below for one of these schools, exploiting regression discontinuity frameworks (Dobbie and Fryer, 2014; Abdulkadiroğlu, Angrist, and Pathak, 2014; Clark and Del Bono, 2016; Pop-Eleches and Urquiola, 2013; Kirabo Jackson, 2010; Clark, 2010; Luflade and Zaiem, 2016; Butikofer, Ginja, Landaud, and Løken, 2020). The estimated causal effects are thus average local effects for the marginal students. The final aim of our study is to analyze the average effect of attending a competitive school separately for students who gain access and for those who lose access to competitive schools when a merit-based system is implemented, hence not only on those students at the cutoff margin. We also show that these causal estimates are the ones needed to build policy-relevant parameters. Note that being able to clearly specify and characterize students for whom we are able to estimate the effect of attending a competitive high school is one of the contributions of this work.[4] For example, our results differ from those in Butikofer, Ginja, Landaud, and Løken (2020) who find positive effects of attending a competitive school in Bergen and Oslo on schooling outcomes, exploiting the discontinuity offered by the admission cutoffs. Their compliers group thus comprises students who are at the cutoff margin, with cutoffs generally in the bottom half of the GPA distribution. Thus, it is likely that their compliers are more similar to the low-ability crowded-out group we consider, for whom we also estimate a positive effect of attending more competitive schools.

A recent paper by Black, Denning, and Rothstein (2020) also examines the winners and losers of a change in the students' allocation system. Using a reform that increases access to selective colleges for high-performing students from disadvantaged high schools, they find a positive effect of the reform on the education of this targeted group and negative on the crowded-out students. They conclude that the policy had an overall positive effect. Our paper is similar to their experiment where access to more selective high schools is generally offered to *any* student with higher grades and we provide a formal potential outcomes framework that enables us to identify treatment effects for specific groups of individuals and conduct policy analysis. We show how this framework allows us to also estimate the effect of alternative allocation systems.

The paper is organized as follows. Section 2 presents a theoretical framework that clarifies the policy-relevant parameters to evaluate the implementation of a merit-based allocation system when

[4]Estimating the effect for different students is generally relevant to evaluate the effect of elite schooling systems on inequality (Burgess, Dickson, and Macmillan, 2020).

capacity is fixed. In Section 3, we specify the potential outcome framework, the reform and discuss the assumptions for identification. Section 4 describes the data and empirical analysis. Section 5 presents the results and robustness checks, and in Section 6 we link these results to the parameters derived in Section 2 to compare the effect of alternative school allocation systems. Section 7 offers conclusions.

2 Evaluating the Effectiveness of Merit-based School Allocation: A Theoretical Policy Analysis

In this section, we provide a theoretical policy analysis highlighting which parameters are policy relevant when school allocation systems become more merit-based. Our model is highly stylized, in that we consider two types of students and two types of schools that differ in terms of their quality. However, our model is flexible in that we do not impose restrictions on why individuals make the school choice that they do, in contrast to the basic Roy model where individuals are assumed to be maximizing an objective function.

There are two types of schools, competitive and noncompetitive. Let D_i be a binary indicator that equals 1 if an individual i attends a competitive school and 0 if he/she attends a noncompetitive school. School capacity is fixed, and the total number of school places is equal to the number of students. For simplicity, assume there are two types of students differing in ability; let $B_i = 1$ if the student is of the low-ability type and $B_i = 0$ if otherwise. As there is excess demand for the competitive schools, the government rations participation in the competitive schools via admission offers R_i, which arrive at random via a lottery. The rate of offer arrival depends on student types. Let δ_h and δ_l denote the offer arrival probabilities for high- and low-ability students, respectively. Note that receiving an admission offer does not necessarily lead to enrolment in a competitive school. Students who have not received an offer can still enroll in a competitive school, by exerting additional effort at a cost. Similarly, students who received an offer from a competitive school may still choose to enroll in a noncompetitive school. Our model allows for both cases.

The policy analysis we consider below focuses on the effects of marginally adjusting the rationing probability for one group of students (for instance, an increase in δ_h), while *maintaining* the number of students enrolled in each type of school. The marginal benefit that we define is the "net" marginal benefit, taking into account the externality to the other group of students who are not directly targeted by the policy change. Our goal is to derive an expression for the policy-relevant sufficient statistics similar to the marginal benefit of a policy change used in the literature (see, e.g. Kline and Walters, 2016; Hendren, 2016). This allows us to link causal estimates of policy effects to normative evaluation of the policies and, for our case, to meaningfully compare different allocation systems. Note that, even though we are considering a marginal change in the rationing probability δ_h, the composition of "compliers" (to be defined formally below) does not change with δ_h due to the assumption that admission offers are random. This, in turn, implies that we can infer the relevant policy parameters using the treatment effects identified from a discrete change in δ_h.

Let D_i^R denote the potential choice for individual i depending on the value of the offer R_i, and $Y_i^{D_i^R}$ denote the student's potential outcome resulting from potential choice D_i^R. The realized and potential school attended by individual i are linked by:

$$D_i = D_i^0 + (D_i^1 - D_i^0)R_i.$$

Policy makers care about the average outcome in the population, $E(Y_i)$ (such as average lifetime earnings), where Y_i is the observed outcome for each student i, after the schooling decision is made. The average outcome of the student population is given by

$$\begin{aligned} E[Y_i] = (1-\pi_B)\left(\delta_h E[Y_i^{D_i^1}|B_i=0] + (1-\delta_h)E[Y_i^{D_i^0}|B_i=0]\right) \\ + \pi_B\left(\delta_l E[Y_i^{D_i^1}|B_i=1] + (1-\delta_l)E[Y_i^{D_i^0}|B_i=1]\right) \end{aligned}$$

where $\pi_B = P(B_i = 1)$ is the proportion of low-ability students in the population.

The proportion of students enrolled in a competitive school is given by

$$\begin{aligned} P(D_i = 1) = \pi_B\left[\delta_l P(D_i^1 = 1|B_i = 1) + (1-\delta_l)P(D_i^0 = 1|B_i = 1)\right] \\ + (1-\pi_B)\left[\delta_h P(D_i^1 = 1|B_i = 0) + (1-\delta_h)P(D_i^0 = 1|B_i = 0)\right] \end{aligned} \quad (1)$$

Given fixed capacity, total differentiation of (1) implies that

$$\frac{d\delta_l}{d\delta_h} = - \underbrace{\frac{(1-\pi_B)}{\pi_B}}_{\text{relative size of ability groups}} \times \underbrace{\frac{P(D_i^1 = 1|B_i = 0) - P(D_i^0 = 1|B_i = 0)}{P(D_i^1 = 1|B_i = 1) - P(D_i^0 = 1|B_i = 1)}}_{\text{relative proportion of compliers}} \tag{2}$$

Equation (2) demonstrates that the policy that expands access to the competitive schools for the high-ability students will reduce access to those schools for the low-ability students. In our model, receiving an admission offer reduces the cost of attending a competitive school, meaning that students are more likely to attend a competitive school ($D_i^1 \geq D_i^0$). This creates a group of "compliers" who only attend a competitive school when an offer arrives. This equation shows that the extent of the crowding out depends on the relative size of ability groups and the relative proportion of compliers within each ability group. The proportion of compliers is obtained by comparing how likely students are to attend a competitive school with and without an offer.

The net marginal benefit of expanding access to the competitive school for high-ability students (a marginal increase in δ_h) is

$$\begin{aligned}\frac{dE(Y_i)}{d\delta_h} &= E[Y_i^{D_i^1} - Y^{D_i^0}|B_i = 0](1-\pi_B) + \pi_B \frac{d\delta_l}{d\delta_h} E[Y_i^{D_i^1} - Y_i^{D_i^0}|B_i = 1] \\ &= \left(E[Y_i^1 - Y_i^0|D_i^0 < D_i^1, B_i = 0] - E[Y_i^1 - Y_i^0|D_i^0 < D_i^1, B_i = 1]\right) P(D_i^0 < D_i^1|B_i = 0)(1-\pi_B)\end{aligned} \tag{3}$$

where the second equality follows from the fact that admission offers affect students' outcomes only via changing their school choice; admission offers do not directly affect students' outcomes.[5] Without a capacity constraint, the aggregate impact of a targeted marginal expansion only depends on the average effects of attending a competitive school for compliers within the targeted group (because $\frac{d\delta_l}{d\delta_h} = 0$). With a capacity constraint, the aggregate impact of a targeted marginal expansion also (negatively) depends on the effects of attending a competitive school for students who are crowded out, or the cost of the "externality," which can be positive or negative. As a result, the net marginal effect of the policy change could be either higher or lower than that when the capacity constraint is not binding.

[5] More specifically, $E[Y_i^{D_i^1} - Y^{D_i^0}|B_i = k] = E[Y_i^1 - Y_i^0|D_i^0 < D_i^1, B_i = k]P(D_i^0 < D_i^1|B_i = k)$, $k = \{0, 1\}$, where $P(D_i^0 < D_i^1|B_i = k)$ denotes the proportion of "compliers" in each group k.

3 Potential Outcomes, Crowding Out, and Identification

Our policy analysis (equation (3)) makes it clear that the policy-relevant parameters include the average effects of attending a competitive school for compliers in the targeted group and the crowded-out group, as well as the proportion of compliers in each group. In the heterogeneous treatment effects framework, where each student has an individual-specific gain from attending a competitive school, the average effects of attending a competitive school may be very different for compliers in the targeted group than those in the crowded-out group. This poses challenges to identification. In this section, we show how we exploit a high school admission reform combined with the assignment rule to identify causal effects of attending competitive high schools for different types of compliers, before linking the theoretical policy analysis with the microeconomic causal estimands.

3.1 The 2005 High School Admission Reform

We focus on a unique natural experiment in Bergen (the second-largest city in Norway). In response to the pressure of different interest groups, in the autumn of 2004, the government of Hordaland county (where Bergen is located) decided to change the intake system for academic high schools.[6] Before the 2005–2006 academic year, students were assigned by the county's school administrative office to the school that was closest to their home. Starting from the academic year 2005–2006, students were instead required to list their six favorite schools and were then assigned to schools based on their preferences and middle school GPA.[7] In practice, the student with the highest middle school GPA was given first choice, while the student with the next-highest score obtained his/her top choice among those schools with remaining capacity. As there was preexisting variation in school quality before the reform, the popular schools filled up quickly. The middle school GPA of the student who is admitted with the lowest GPA identifies the cutoff for admission at each school. Note that students were required to fill their list in April, hence before they knew their final middle school GPA. This mechanism for allocating students is a simple "serial dictatorship" mechanism, which does not give incentives for strategic manipulation

[6] In Norway, students completing middle school (at age 16) can decide to enroll in 'academic' or 'vocational' high schools (lasting three years). Differently from the vocational track, the academic one does not prepare pupils for one specific job and makes them eligible to enroll in university. A full description of the Norwegian education system is in Appendix A. Note that in Figure A1 we show that, in Bergen, there were not systematic changes in the proportion of individuals enrolling in academic high school following the reform.

[7] Grades in middle school are the sum of the final-year grades in different subjects. More details on the grading system are in Appendix A.1.

of preferences by the students (see e.g. Pathak and Sönmez, 2013). We provide additional details on the allocation system both before and after the reform in Appendix B.[8]

The reform was effective in changing student composition. We observe five schools being oversubscribed in the post-reform period. Both the average and all percentiles of the middle school GPA distribution of students of oversubscribed schools increased after the reform (difference in average GPA increased from 0.4 to 0.8 of one standard deviation, Figure 1).[9] In addition, in Figure 2 we show that after the reform there is an increase of students with high GPA at middle school and low socio-economic status (SES) who attended oversubscribed schools, at the expenses of high SES students with low middle school GPA. In fact, before the reform, high-SES students were generally more likely to attend one of these schools which, post-reform, are oversubscribed, because of residential sorting. After the reform, this is true only for the subgroup with high grades at middle school. The opposite pattern is observed for low-SES students, who were generally less likely to enroll in one of these schools before the reform, but this is true only for those with low middle school GPA post-reform. Crucial to our analysis is the assumption that school capacity is fixed. The number of students attending academic high schools changes little in the six years around the reform and the proportion enrolling in one of these five oversubscribed schools remains fixed between 35% and 40% (Figure 3), providing support for this assumption.

A consequence of residential sorting is also that the middle school GPA distribution for students in schools which are oversubscribed after the reform shifted to the right even before the reform. Hence, students in these schools had better peers even before the reform. They also had fewer days of absence, were more likely to enroll in university and to complete it. In addition, instructors in these schools were more likely to hold a master's degree (see Figure A2). These differences are consistent with the widespread perception that these schools had a better reputation, and can explain why they were oversubscribed as a result of the reform.

This reform provides us with exogenous variations to the admission allocation rule across different cohorts. In particular, the reform implies that students who turn 16 during the calendar year 2005 are assigned to high school under the new admission rule. Given the legal school starting age in Norway,

[8] An important feature of the reform is that it applied to all students. Hence, there were no exceptions to accommodate the requests of pupils with specific characteristics, such as siblings of already enrolled students.

[9] As described in section 4.1, these oversubscribed schools are what we define as "competitive" schools, and are labelled accordingly in the figures.

these are students born after December 31, 1988. In our data, we observe the exact date of birth of individuals, hence we can place students in the cohort they belong to. In addition, given that information is available for multiple cohorts around the 1989 cohort, we are able to separate the effect of the reform from secular trends in the outcome of interest (where time trends are to be interpreted in terms of the date of birth of the students) and from seasonal effects (where children born in different periods of the year may have consistently different outcomes).

3.2 Identification

Our identification strategy exploits exogenous variations from the allocation reform combined with additional variations from assignment rules. To understand how it works, it is useful to develop some notations. Define y^d as the potential outcome if an individual student goes to a school of type D, where $D = 0$ if the student attends a noncompetitive school and $D = 1$ if the same individual student goes to a competitive school.[10] Define d^z as the potential schooling choice for a student given an instrument $Z = z$. We do not observe both y^0 and y^1 for any individual; one of these potential outcomes is counterfactual. Only the realized outcome y is observed: $y = y^0 + (y^1 - y^0)d$. Similarly, we do not observe both d^0 and d^1 for any individual and only the realized schooling choice d is observed. In our framework, we do not restrict the heterogeneity in the payoffs to attending a competitive school: the payoff, $y^1 - y^0$, may vary across individuals. Potential schooling choice may be correlated with y^0, y^1, or $y^1 - y^0$, allowing for selection on levels and gains of potential outcomes.

We begin by demonstrating what we can identify when using the reform as our instrument Z, where $Z = 1$ if a student is allocated to a school by merit, and $Z = 0$ otherwise. The population of students can be partitioned into four mutually exclusive groups, depending on their values of potential choices with and without the reform:

- Pulled-in Compliers (CP): $D^0 = 0, D^1 = 1$
- Always Takers (AT): $D^0 = 1, D^1 = 1$
- Never Takers (NT): $D^0 = 0, D^1 = 0$
- Crowded-out Compliers (CC): $D^0 = 1, D^1 = 0$

[10] We use small letters to refer to realizations of random variables and we drop subscript i.

where the Pulled-in Compliers (CP) are those who only attend competitive schools under the reform (but would not do so otherwise), the Crowded-out Compliers (CC) only attend competitive schools in the absence of the reform, and AT and NT attend the same type of schools no matter whether the reform is in place or not. Note that, given capacity constraint in the offer of seats in selective schools, we cannot assume away the Crowded-out Compliers. If some students get access to the competitive schools as a consequence of the reform, others have to lose access, because access to competitive schools is a rival good.[11]

We shall assume that the reform affects outcomes only through school choices and that the reform is randomly assigned. Abstracting from covariates, these assumptions are established by the following two assumptions:[12]

Assumption 1. *Random Assignment:* $y^d,\ d^z\ \perp Z \quad \forall\ d \in \{0,1\}, z \in \{0,1\}$

Assumption 2. *Exclusion Restriction:* $y^{d^1} = y^{d^0} \equiv y^d \quad \forall\ d \in \{0,1\}$

Under these two assumptions, the Wald estimator exploiting Z as an instrument for D identifies a weighted difference of the effects for compliers and defiers:

$$\begin{aligned} LATE_Z &\equiv \frac{E[Y|Z=1] - E[Y|Z=0]}{P(D=1|Z=1) - P(D=1|Z=0)} \\ &= \underbrace{E[Y^1 - Y^0|D^0=0, D^1=1]}_{\text{Treatment effect on CP}} \underbrace{S_{CP}}_{\text{CP weight}} - \underbrace{E[Y^1 - Y^0|D^0=1, D^1=0]}_{\text{Treatment effect on CC}} \underbrace{S_{CC}}_{\text{CC weight}} \end{aligned} \tag{4}$$

where $LATE_Z$ is the causal estimand and the weights are given by $S_{CP} \equiv \frac{P(D^0=0,D^1=1)}{P(D^0=0,D^1=1)-P(D^0=1,D^1=0)}$, $S_{CC} \equiv \frac{P(D^0=1,D^1=0)}{P(D^0=0,D^1=1)-P(D^0=1,D^1=0)}$. Equation (4) demonstrates that the effect estimated using the reform as an instrument for attending a more competitive school is a weighted average of the effect of attending a more competitive school for CP and CC, where the weights are proportional to the fraction of CC and CP in the population. The existence of CC poses challenges to identification, because the reform (Z) will not satisfy the monotonicity condition for the population as a whole (Imbens and Angrist, 1994). Only if the share of CC compliers is zero (i.e., $P(D^0 = 1, D^1 = 0) = 0$), the causal estimand can identify the average treatment effect for the CP compliers. Under some cases, the

[11]For instance, CC could be students who absent the reform, live in the catchment area of a competitive school, when the reform is in place they do not have high enough grades to attend a competitive school.

[12]In section 4.2 we will discuss the covariates that are necessary for the reform to be randomly assigned and conduct validation test for the exclusion restriction.

treatment effect for the CP compliers can be entirely cancelled out by the treatment effect of those CC compliers, leaving the estimated $LATE_Z$ to zero whereas in fact there may be significant treatment effects for both the CP and the CC compliers.[13]

In order to separately identify the effects of attending a competitive school for different types of compliers, we exploit additional variations from assignment rules within an allocation system. The assignment rule facilitates identification because, when combined with the reform, it can partition the population into subgroups–within each subgroup, the reform will shift students' potential choices monotonically, satisfying the monotonicity assumption of the IV estimator. Let c be the allocation rule for a student under the merit-based allocation in the post-reform period. Under the merit-based rule, c is the admission cutoff for the competitive schools.[14] Our instrumental variables are defined from an interaction of the reform and the ability group of the student, where ability group is determined by students' middle school GPA relative to the admission cutoffs for competitive schools after the reform. Specifically, the admission cutoffs partition the students in the post-reform period into two subgroups: $C = 0$ if the student is subject to the merit-based allocation and his/her GPA is below the admission cutoff for competitive schools, and $C = 1$ if the student is subject to the merit-based allocation but his/her GPA is above the admission cutoff. We define an instrument vector, $\tilde{Z} = (Z, C)$, which takes three possible values based on the interaction of Z and C:[15]

- $(0, 0) \rightarrow$ Pre-reform
- $(1, 0) \rightarrow$ Post-reform, grades below the admission cutoff
- $(1, 1) \rightarrow$ Post-reform, grades above the admission cutoff

Based on the new instrument vector, we define potential schooling choice by $d^{\tilde{Z}}$. The realized choice

[13] In our empirical context, $LATE_Z$ corresponds to the estimand of a fuzzy RD design, where the running variable is the week of birth of students and the discontinuity is around the last week of December, 1988. See below for a discussion on the covariates that we need to control for in order for the reform to be exogenous.

[14] Note that for notational simplicity we have suppressed the individual index, although the admission cutoff might potentially differ across the student population.

[15] We can also use allocation rules in the pre-reform period. In this case, c would be a cutoff defined from the distance to the nearest competitive school and we can partition the population before the reform into two subgroups: those who do not live in catchment area of a competitive school and those who live in those catchment areas. Then, within each subgroup, the reform shifts students' school choice monotonically. We do not focus on the variation created from the neighborhood assignment rule in this paper because we don't observe catchment areas in our data.

d is linked to potential choices by:

$$d = d^{0,0} + (d^{1,0} - d^{0,0})\mathbf{1}(\tilde{Z} = (1,0)) + (d^{1,1} - d^{0,0})\mathbf{1}(\tilde{Z} = (1,1)) \tag{5}$$

We maintain Assumptions 1 and 2 to hold for the instrument vector, $\tilde{Z}$.[16] We make one additional identifying assumption, the conditional monotonicity of the reform:

Assumption 3. *Conditional Monotonicity of the Reform:* $d^{1,1} \geq d^{0,0}$, $d^{1,0} \leq d^{0,0}$

This assumption rules out the possibility of students with grades above the cutoff, who would have attended a competitive school before the reform, deciding not to do so after the reform. It also excludes a person with grades below the cutoff who would have attended a noncompetitive school before the reform, deciding to push for admission in a competitive school after the reform.[17]

Assumption 3 also implies choice monotonicity at the cutoff:

Assumption 4. *(Corollary of Assumption 3) Monotonicity at the Cutoff:* $d^{1,1} \geq d^{1,0}$,

Assumption 4 means that, after the reform, a person who attends a competitive school when below the cutoff must also attend a competitive school when above the cutoff. Note also that strategy-proofness of the allocation mechanism guarantees that no student with high enough GPA, who pre-reform would have attended a competitive school, ends up post-reform in a noncompetitive school because of strategic consideration when ranking schools.[18]

[16]Note that, assumptions A.1 and A.2 automatically extend to the instrument vector $\tilde{Z}$, provided that we condition on the main effect of C. In our empirical analysis, one covariate we control for is students' middle school GPA, because whether a student is above or below the cutoff is correlated with students' middle school GPA, which may affect their potential schooling choices (e.g., sorting on ability) and potential outcomes directly. We defer the discussion of covariates when we describe the empirical implementation in Section 4.2.

[17]In our context we show that the competitive high schools have the best reputation and consistently outperforms non-competitive schools. In addition, admission to university relies on nationally administered exams (see Appendix A), which does not provide an incentive to attend a lower-level institution to improve one's relative position within the class, as is instead the case, e.g., in Cullen, Long, and Reback (2013). These factors make it unlikely that a student who would have been assigned to a competitive school before the reform chooses a noncompetitive school after the reform, despite having high enough grades to enroll.

[18]This would not be the case if, for example, a student who would have attended a competitive school pre-reform decides to rank high a worse school in the post-reform period because of fear of not being admitted (hence "skip the impossible", as shown in Fack, Grenet, and He (2019)). If this student had the wrong prediction about where the cutoff is and would have ended up in the preferred school if ranking schools appropriately, then Assumption 4 would be violated. We argue that this type of strategy concerns are not in place in this case because (1) the list of schools is given in April, well before the final exam to compute GPA takes place (in June) and (2) as we evaluate the introduction of the merit-based system, it is not possible to predict where the cutoff will be, based on observations of previous years. In addition, schools are spread out geographically, so the set of schools which students usually choose from is not large. This further reduces the concern that students may strategically select the 6 schools among the 16 available schools.

Assumption 3 guarantees monotonicity of the reform within subgroups (i.e. only one type of compliers in each subgroup), which we can exploit to identify the treatment effects for CP and CC compliers. Table 1 shows all the possible compliance types based on potential schooling choices. The monotonicity assumptions rule out certain compliance types defined by different potential realizations of Z, leaving us with four compliance types: Never Takers (NT), Crowded-out Compliers (CC), Pulled-in Compliers (CP) and Always Takers (AT). From Table 1, it is easy to see that the admission cutoff combined with the reform allows us to define subsamples of the student population where monotonicity holds: conditional on being above or below the cutoff in the post-reform period, the reform changes students' school choice monotonically, either into (for those above the cutoff) or out of (for those below the cutoff) competitive schools. As an example, consider the subsample of students where either $\tilde{Z} = (0, 0)$ or $\tilde{Z} = (1, 0)$. In this case, the only type of compliers is CC because CP is equivalent to NT. This, in turn, allows separate identification of the treatment effects for compliers who are crowded out (CC) of and those pulled in (CP) to competitive schools, as shown in proposition 1 below.

Proposition 1. *Given Assumptions (1)–(3), the effect of being excluded from competitive schools for CC (as defined in Table 1) corresponds to*

$$E[Y^0 - Y^1|CC] = -\frac{E[Y|\tilde{Z} = (0,0)] - E[Y|\tilde{Z} = (1,0)]}{P(D = 1|\tilde{Z} = (0,0)) - P(D = 1|\tilde{Z} = (1,0))}, \tag{6}$$

while the effect of attending a competitive school for CP corresponds to

$$E[Y^1 - Y^0|CP] = \frac{E[Y|\tilde{Z} = (1,1)] - E[Y|\tilde{Z} = (0,0)]}{P(D = 1|\tilde{Z} = (1,1)) - P(D = 1|\tilde{Z} = (0,0))}. \tag{7}$$

Proof. See Appendix C. □

Equations (6) and (7) correspond to the convenient Wald estimator in the IV literature with binary endogenous variables and binary instruments.[19] Note also that equation (6) estimates the opposite of the effect of treatment on CC, i.e. the effect of attending a selective school, because the reform *excludes* CC from attending a selective school.

[19]Note that the denominators in equations (6) and (7) correspond to the shares of CC and CP, respectively.

3.3 Linking the Causal Estimands to Policy Evaluation

We conduct policy analysis by linking the net marginal benefit of a policy change (equation (3) in Section 2) to the microeconomic causal estimands that we have obtained. Under our identifying assumptions, the reform is effectively equivalent to the lottery admission offer modeled above (conditional on covariates). Importantly, the exogenous variation of the reform corresponds to the random offer R only for one group of students (students above a certain admission cutoff). For these students, the reform weakly increased their likelihood of attending a competitive school, generating compliers who attend competitive schools only after the reform. For the remaining students (students below a certain admission cutoff), the reform is equivalent to $1 - R$, i.e., the reform is equivalent to *withdrawing* the lottery offer R. For this group of students, the reform weakly reduced their chance of attending a competitive school. For instance, these could be students who, absent the reform, live in the catchment area of a competitive school, while when the reform is in place they do not have high enough grades to attend a competitive school, and thus enroll in a different school.

The net marginal benefit of expanding access to the high-ability students can be written as follows:

$$\frac{dE(Y)}{d\delta_h} = \{ \underbrace{E[Y^1 - Y^0|CP]}_{\text{Treatment effect on CP}} - \underbrace{E[Y^1 - Y^0|CC]}_{\text{Treatment effect on CC}} \} \underbrace{\pi_{CP}}_{\text{CP share}} (1 - \pi_B) \tag{8}$$

This convenient result follows from the assumption that the reform is random and that it increases the probability of attending competitive schools for those students above the admission cutoff, while it decreases the probability for those below the admission cutoff, which in turn implies that the composition of CP and CC does not change with the marginal change in the offer arrival probability δ_h.

Instead of targeting the high-ability students, we can also use our framework to evaluate the net marginal benefit of a counterfactual policy change that expands access for different groups of students. For instance, consider a counterfactual policy where we reverse the reform by expanding access to competitive schools for low-ability students at the cost of high-ability students. In this case, the net marginal benefit can be evaluated by:

$$\frac{dE(Y)}{d\delta_l} = \{ \underbrace{E[Y^1 - Y^0|CC]}_{\text{Treatment effect on CC}} - \underbrace{E[Y^1 - Y^0|CP]}_{\text{Treatment effect on CP}} \} \underbrace{\pi_{CC}}_{\text{CC share}} \pi_B \tag{9}$$

We can also compute the marginal benefit of a counterfactual policy change where the targeted group and the crowded-out group are defined by combinations of student ability and family background (such as the SES). Consider different groups of students in the population characterized by family background $X \in \{0, 1\}$ and ability $B \in \{l, h\}$, where l and h denote low and high grades, respectively. Let $\delta_{B,X}$ denote the offer arrival probability for students in each group (e.g. offer for high-ability students in group $X = 0$ is δ_{h0}). We then compute, for example, the effect of a marginal change in admission offer arrival rate δ_{h0}, holding δ_{h1} and δ_{l0} constant but allowing δ_{l1} to adjust endogenously to satisfy the capacity constraint. Drawing upon the microeconomic causal estimands for different subgroups of students, the net marginal benefit becomes:

$$\frac{dE(Y)}{d\delta_{h0}} = \{\underbrace{E[Y^1 - Y^0|CP, X = 0]}_{\text{Treatment effect on X=0,CP}} - \underbrace{E[Y^1 - Y^0|CC, X = 1]}_{\text{Treatment effect on X=1,CC}}\} \underbrace{\pi_{CP,X=0}}_{\text{CP share, X=0}} \pi_{h0} \tag{10}$$

4 Empirical Implementation

4.1 Data and Defining the Competitive Schools

We leverage extensive population-wide Norwegian administrative data for students, parents, location and school attendance, where individuals can be identified from primary and middle school, through to high school, and we can measure outcomes up to their late 20s. Our sample consists of students enrolled in an academic high school in the three years before and three years after the reform (2002–2007). Because the starting age for high school in Norway is 16, the individuals we consider in our sample are born in the period 1986–1991. As mentioned in Section 3.1, we focus on academic high school students because these are affected by the reform, while vocational students are not. A potential concern is that the reform may change not only the school attended, but also the *type* of school, for example moving students from academic to vocational tracks. This would imply that pre- and post-reform academic students differ systematically. In Figure A1 we plot the trends in the proportion of students enrolling in academic high school in Hordaland (Bergen's county) and in the rest of the country, finding no jumps around the reform year. In Section 5.2 we provide additional evidence about comparability of students attending academic high schools before and after the reform. We include students attending any of the 16 public academic high schools in Bergen and the three closest municipalities.[20] We exclude a small number of students who attended a middle school that was more than eight kilometres away from any of the 16 high schools.[21] The number of students in each cohort ranges between 1,200 and 1,400 (Figure 3).

In the data, we do not observe the cutoffs for the different schools. We rely on the methodology by Hansen (2000) to recover the cutoff by exploiting information on the grades and school enrollment of the students. A full description of this procedure is in Appendix D. Applying this method, we find that in the three years after the reform, four out of the 16 academic high schools were oversubscribed, while one more school was oversubscribed in two years (not in 2007). Pulling together all the estimated cutoffs, the probability of enrollment in an oversubscribed school is very low below the cutoffs and increases on average by 20 percentage points at the cutoff in every year (Figure 5). We define these five schools as competitive, and attending one of these five schools (four schools in 2007) is the treatment D we consider. Post-reform students are defined as being above the cutoff if their middle school GPA is larger than the lowest cutoff among competitive high schools that are within eight kilometers of their middle school.[22]

We investigate the effect of attending a competitive school on the different educational outcomes of the students, including high school completion, school absence and university enrollment and completion. High school completion is defined as the probability of completing high school within four years. School absence is measured as the number of days a student is absent in one year, averaged over the three years of high school. As these are reported only for students who completed high school, the effects are to be interpreted as the combination of the real effect on days of absence and the effect of changing the sample of students completing high school because of the reform. University enrollment is the probability of enrolling in university within six years since the beginning of high school, while university completion is the probability of completing university by age 28. We further look at enrollment in some specific fields (STEM fields) and in what we define as "elite universities," courses or institutions that provide substantially higher returns than others.[23]

[20]The proportion of students attending private schools is relatively small (between 8 and 9% in every year) and does not change around the reform, thus suggesting that movements from public to private or vice versa was not a consequence of the reform.

[21]These are students who commute to Bergen from other towns and for whom the catchment area rule did not apply.

[22]Further details on the mapping of cutoffs to students and tests for alternative mapping are in Appendix D.

[23]These are universities offering education in medicine, one engineering university (Norwegian University of Science and Technology) and the national business school (Norwegian School of Economics). We estimate that returns to university

A list of descriptives for the pre-reform cohorts is in Table A1. We also provide information on the socioeconomic background of the students, where household earnings are expressed in 1998 Norwegian kroner (6NOK $\approx$ 1USD) and are the average annual earnings of fathers and mothers during the three years that the student is in high school. Recall that we are focusing on the sample of individuals selecting academic high schools, which is likely positively selected: the average parental earnings of this sample (375,369 NOK) are larger than the average earnings of the overall population of high school students (323,270 NOK). When we conduct heterogeneity analysis, we separate high-SES students, whose parents both completed university, from low-SES students, those with at least one parent without university. The average parental earnings of low-SES students according to this definition (318,365 NOK) are below the average parental earnings of the entire population of high school students.[24]

4.2 Empirical Model and Estimation

Proposition 1 implies that we can estimate the effect of attending a competitive school for CC and CP separately, by instrumenting the treatment with two different binary variables Z_1 and Z_2 where $Z_1 = \mathbf{1}[\tilde{Z} = (1, 0)]$ and $Z_2 = \mathbf{1}[\tilde{Z} = (1, 1)]$. In practice, we use a two-step procedure to estimate the population equivalent of the Wald estimators in Equations (6) and (7). In the first step, we estimate the first stage (the denominator of the Wald estimator), which corresponds to the proportion of CC and CP in the population. In particular, these are given by the estimates of parameters $-\theta_{CC}$ and θ_{CP} from a linear probability model where the outcome variable is a dummy for attending one of the five competitive schools:

$$d = \alpha^{fs} + \theta_{CC} z_1 + \theta_{CP} z_2 + f^{fs}(t) + g^{fs}(GPA) + \gamma^{\mathbf{fs}} \mathbf{w} + u, \tag{11}$$

are about 40% higher in these universities than the average Norwegian university (based on the earnings at age 30–40 of cohorts born between 1970 and 1975, and comparing students who attended these universities to those who attended any other university).

[24] We base our definition of SES on education of parents instead of earnings in order to use a measure which is more permanent as compared to earnings which are necessarily volatile. However, if we define SES on the basis of the distribution of earnings, separating those above and those below the median, heterogeneity analysis delivers the same message as the heterogeneity analysis where SES is defined on the basis of parental education.

where

$$-\theta_{CC} = P(D=1|\tilde{Z}=(0,0)) - P(D=1|\tilde{Z}=(1,0)), \tag{12}$$

$$\theta_{CP} = P(D=1|\tilde{Z}=(1,1)) - P(D=1|\tilde{Z}=(0,0)). \tag{13}$$

In the second step, we recover the numerator of the Wald estimators by estimating parameters $-\beta_{CC}$ and β_{CP} from a regression of the outcome of interest on the same independent variables as in Equation (11):

$$y = \alpha + \beta_{CC} z_1 + \beta_{CP} z_2 + f(t) + g(GPA) + \gamma \mathbf{w} + \varepsilon, \tag{14}$$

where

$$-\beta_{CC} = E[Y|\tilde{Z}=(0,0)] - E[Y|\tilde{Z}=(1,0)], \tag{15}$$

$$\beta_{CP} = E[Y|\tilde{Z}=(1,1)] - E[Y|\tilde{Z}=(0,0)]. \tag{16}$$

The ratios $-\frac{(-\beta_{CC})}{(-\theta_{CC})}$ and $\frac{\beta_{CP}}{\theta_{CP}}$ are the indirect least square estimators for the effect of the reform on the two types of compliers (see e.g. Angrist and Pischke, 2008).[25] Note that, despite that our general identification argument does not include any covariates, we condition on trends in the week of birth and GPA to ensure that instrument $\tilde{Z}$ is randomly assigned. In particular, since we define students affected by the reform (hence $Z = 1$) as those born after December 31, 1988, we control for time trends (in terms of the date of birth) to avoid confounding the effect of the reform with secular or seasonal trends in outcomes. We thus include a linear trend in the week of birth ($f(t)$) and we normalize the first week in 1989 to zero. We also allow this trend to be different before and after the reform and in the post reform period we allow it to be different for students whose GPA is above and those for whom it is below the respective admission cutoff. This resembles a fuzzy regression discontinuity framework,

[25] These ratios correspond to Equations (6) and (7) from Proposition 1, thus when we estimate these parameters we are estimating the effect of being excluded from a competitive school for CC and the effect of being admitted for CP. We estimate the standard errors of the indirect least square estimators using the formula for the standard deviation of the ratio of two independent estimators (the estimator for the reduced form and the one for the first stage). In particular, it can be shown that the first order Taylor approximation of the variance of the ratio of two independent random variables is $Var(\frac{\beta}{\theta}) = \frac{E[\beta]^2}{E[\theta]^2}\left[\frac{Var(\beta)}{E[\beta]^2} + \frac{Var(\theta)}{E[\theta]^2}\right]$. The variance of $\beta_{CC}, \beta_{CP}, \theta_{CC}, \theta_{CP}$ is the estimated variance of the OLS estimators, where we cluster s.e. at the week of birth level. Note that this formula collapses to the formula used in Angrist (1990) if the distribution of the estimator for the denominator converges to a constant and not to a nondegenerate distribution.

which estimates the causal effect for students at the discontinuity (see e.g. Clark and Royer, 2013). We also control for dummies for the 52 weeks in the year ($\mathbf{w}$) to control for seasonal effects. In addition, as mentioned in Section 3.2, we control for individuals' middle school GPA ($g(GPA)$) as to control for the main effect of being above or below the cutoff. In our main specification we include a linear trend in GPA. Robustness checks in section 5.2 show that our results are substantively unchanged when we use different functional forms for the time and GPA trends.

The estimates of the reduced form equation ($\hat{\beta}_{CC}$ and $\hat{\beta}_{CP}$) carry interpretation on their own, similar to the interpretation of a difference-in-difference regression. Given assumption (1) (random assignment of the reform), β_{CC} (β_{CP}) identifies the change in outcome y induced by the change in the allocation rule among all individuals who, after the reform, have grades below (above) the relevant cutoff for admission at competitive schools. Hence, β_{CC} is the effect among low ability students, some of whom are crowded out of the competitive schools. Its inverse is an estimate of the change in outcome when least able students are given access to competitive schools.[26] As it is clear from Proposition 1, in order for these effects to be interpreted as the effect for CC and CP, they need to be scaled by their respective first stage coefficients. As an alternative, we can estimate the treatment effects (and their s.e.) from Equations (6) and (7) in one step, using 2SLS. In order to do so, we need to consider two different subsamples; for the effect on CC, we should include in the sample only individuals who either have $\tilde{Z} = (0,0)$ or $\tilde{Z} = (1,0)$ and run 2SLS instrumenting treatment with Z_1, while for CP we can do the same, this time only including individuals for whom $\tilde{Z} = (0,0)$ or $\tilde{Z} = (1,1)$ and using Z_2 as an instrument. However, this procedure would estimate different pre-reform trends when we estimate the effect for CC and for CP, hence we include these estimates only as a robustness in section 5.2.

4.3 Validating Identification Assumptions

Random Assignment of the Reform Assumption 1 implies that students affected by the reform are comparable to those not affected by the reform. We include trends in week of birth and seasonal dummies to control for this, and test for different specifications of these trends (see Section 5.2). As described in Section 4, our preferred specification is similar to a fuzzy RD in the date of birth of students. As a consequence, an identifying assumption is that students who are born just after December 31st,

[26] Similarly, θ_{CC} estimates the fraction of students losing access to competitive schools and it is thus expected to be negative. Hence the fraction of CC is estimated as the inverse of θ_{CC}.

1988 are comparable to those born few days before. We can thus analyze whether students born around this date are similar, by investigating whether the average characteristics of individuals born before and after this date are significantly different. A test like this resembles tests which aim at assessing if randomization has been properly conducted in randomized experiments (Lee and Lemieux, 2010). In Figure 4 we plot the average characteristics of students born in each week around December 31st, 1988, in terms of gender and parental background. The figures show no clear jump in the first week of 1989. In addition, we estimate parameters of a regression of all these characteristics on a dummy for being born from 1989 onwards and flexible time trends in the week of birth, allowed to differ before and after the last week of 1988. The estimated coefficients for the dummy at the discontinuity are reported on top of each figure, they are small and never statistically significant, providing evidence that the assumption of no differences between students affected and those not affected by the reform.

Middle School GPA Distribution One concern with our identification strategy is that the reform may affect middle school GPA directly, because students choose a different middle school to boost grades, because teachers may assign grades differently in order to favour students' high school admissions, or because students exert more effort to obtain access to their preferred school. This makes middle school GPA endogenous to the reform.

First, assignment to middle schools is based on catchment areas and because our cohorts are within three years from the reform, middle school choice is not likely. Second, since the cutoffs are not exogenously determined, but depend on the application and the grades of other students, teachers cannot predict where the cutoffs are and thus do not have the incentive to raise grades when students are just below a cutoff (as, instead, has been shown happening when the cutoff is known, see e.g. Diamond and Persson (2016)). In addition, since the merit based system was not in place before the reform, teachers did not even know precisely whether schools were going to be oversubscribed (although they may have been informed about the preferences of their students over the different schools at the time they grade the final exams). Finally, as explained in Appendix A.1, note that middle school teachers only had control on one part of the final GPA, with the rest depending on externally graded exams.

Our ultimate test to exclude the possibility that middle school GPA is affected by the reform, in order to exclude also the possibility of a change in effort, is to compare the distribution of middle school

GPA for students in Bergen before and after the reform to the distribution of grades at the national level. Since no admission reforms took place elsewhere in the same period, we expect a change in the incentive for teachers and student to manipulate middle school GPA only in Bergen. We thus rank each individual graduating from middle school in Bergen in the national distribution of middle school GPA. If grades in Bergen increase as a consequence of the reform, we expect these ranks to be larger for cohorts starting high school after 2005. We thus plot the distribution of the nationally-ranked middle school GPA in Bergen before and after 2005 in panel (a) in Figure 6. The distributions overlap and a Kolmogorov–Smirnov test excludes any significant difference. Similarly, in panels (b) and (c), we plot the same distributions for cohorts affected and not affected by the reform in Bergen, separately for individuals of different SES. Plots and formal tests exclude any difference between pre- and post-reform cohorts, suggesting that the reform had no effect on students' middle school GPA.

Exclusion Restriction Another concern is that the exclusion restriction assumption may not hold because (1) the reform changes some characteristics of competitive and noncompetitive schools and (2) the outcome may also change for individuals whose potential treatment is the same before and after the reform (that is, Always and Never Takers, see Table 1). This can be a result, for example, of the change in peers induced by the reform or a change in the teaching style given that teachers now face more homogeneous classrooms in terms of underlying ability (as captured by middle school GPA, see e.g. Duflo, Dupas, and Kremer, 2011). If any of this mechanism is in place, we would be mixing the effect on compliers with the teachers' or peer effects for AT and NT.

The first issue is less problematic in the Norwegian context, where schools are centrally financed and resources do not depend on the quantity or quality of students. In addition, in Figure A2 we show that the characteristics of teachers (age and qualifications) in competitive and noncompetitive schools do not change systematically with the reform.

However, this does not guarantee that the second issue is not a problem, if teachers adapt their teaching style to the new class composition. First of all, note that the reform we analyze only matters for academic high school, so both pulled-in and crowded-out groups belong to a group of students with relatively high middle school GPA's. Hence, for example, the average middle school GPA in non-competitive schools post-reform is still around 1.1 s.d. above the average middle school GPA of vocational schools' students from the same cohort. In addition, as shown in Figure 1, after the reform the difference in average GPA between competitive and non-competitive schools increased by approximately 0.4 of a s.d., being 0.8 of a s.d. post-reform. As a comparison, in Duflo, Dupas, and Kremer (2011), where differences in teaching style are found, the difference between classes with high and low ability students in average test scores is 1.6 of a s.d., so 4-times the increase in difference for our case.

In addition, we implement a test to exclude that the reform had any effect on AT and NT, irrespective of what may be the trigger of this effect. We consider a subpopulation for which the reform is unlikely to affect the treatment and we investigate whether post-reform cohorts had different outcomes. If this is the case, it would be a signal that changes in the classroom composition may harm our identification strategy. Students who have a middle school GPA above the 75th percentile of the distribution (who are always above the cutoff) and who enrolled at a middle school within 1.5 kilometres of a competitive high school (and thus are likely to be within the catchment area of a competitive high school) have the possibility to attend a competitive high school irrespective of whether or not they are affected by the reform. Students who have a middle school GPA below the 15th percentile of the distribution and who enrolled at a middle school that is more than five kilometres away from a competitive high school, instead, are likely to never enroll in a competitive high school. We estimate a linear regression where the outcome variables are regressed on a dummy for being born after December 1988 (and hence attending high school after the reform took place), a linear trend in week of birth, allowed to change before and after the reform, and seasonal dummies. Column (1) of Table 2 shows that, as expected, when we do not restrict the distance from the competitive high schools, the probability of enrolling in one of these schools increases for the first subpopulation and decreases for the second. Column (2) shows that this effect is much smaller and not significant when we impose the distance limitation, hence suggesting that the reform does not impact them by changing their high school decisions. The estimates of the effect of the reform are small and never statistically significant, suggesting no clear effects for either group.

5 Results

5.1 Estimated Treatment Effects

In Table 3 we report the reduced form and the indirect least squares estimates for crowded-out and pulled-in compliers. In the first row we report the estimate of the first-stage parameter, i.e. the estimate of the proportion of CC and CP. CC account for about 15% of the population, while CP account for about 18%, which implies that a substantial share of the population responded to a change in the allocation rule, both by being crowded out or pulled in the competitive schools.

Next, we show the estimated reduced form effect of being crowded out of a competitive school for CC ($\hat{\beta}_{CC}$), the consequent treatment effect, and the same estimates for being pulled into a competitive school for CP ($\hat{\beta}_{CP}$). Overall, the reform has a negative impact on school outcomes for students below the cutoff, with the reduced form estimates being negative and significant for high school and university completion and positive for the average days of absence. Rescaling this effect in order to estimate the treatment effect for CC, we observe the high school completion rate falling by 27 percentage points. As a reference to evaluate the magnitude of the effect, in Table A1 we report pre-reform averages overall and for different subgroups. For example, pre-reform, high school completion rate was about 77% among the individuals who attended a competitive high school and had low middle school GPA (below the 25th percentile of their cohort's distribution). The CC are likely to belong to this group (although not all of individuals in this group are CC). If we take this as a reference point, our effect would imply a decline of approximately 35%. Compared to the population average (90%) it would be a decline of 30%, hence still a significant reduction. We also observe an increase in days of absence (5.6 days more, an increase by more than 70%) and a decrease in university enrolment, although not statistically significant. University completion decreases significantly (also in statistical terms) with a reduction of 34 percentage points, between 52 and 40% depending on the baseline. In Table A2, we also look at changes in the type of university enrollment (Elite or STEM majors), but the evidence on this margin is not as clear as the one reported in Table 3, and we do not find any statistically significant effect.

For CP, on the contrary, all estimated effects are much smaller in magnitude and generally not statistically significant. The number of days of absence seem to increase, although by a smaller amount compared to CC. This may also indicate that, in general, when schools are further away, the probability of missing some days of school increases. The decline in university education attainment is large, but not precisely estimated, and similarly there is no clear effect on the type of university attended (Table A2). Overall, this is evidence of small effects for CP who, if anything, are negatively affected by the reform, hence clearly indicating no gains for those who decide to move to a better school. This result is consistent with findings exploiting marginal students admitted to elite high schools in Boston and New York (Abdulkadiroğlu, Angrist, and Pathak, 2014; Dobbie and Fryer, 2014), suggesting that for high-achieving students the effect of attending more competitive schools is likely to be zero.

The large estimated effects for CC and the almost zero estimates for CP can be explained by the fact that the two types of compliers differ not only in terms of their ability, but also along different dimensions. We thus estimate the same effects separately for individuals from high/low SES and for men and women. The ratio between the first stage when we estimate it on individuals with certain characteristics and the overall first stage is informative regarding the relative likelihood of CC and CP to fall in each observationally different subgroup (Angrist and Pischke (2008), the values reported in Table A3). CC are more likely to be from low SES and men, with the first stage for women not even being significant. As a result, estimates for CC women are very imprecise. We report heterogeneity results in terms of SES in Table 4 and for gender in Appendix (Table A4). The negative effect for CC is driven by the pool of low SES students. These results confirm the intuition that disadvantaged students, both in terms of academic ability and socioeconomic background, are those who benefit more from attending better schools. CP, instead, are much more likely to be women and high SES students. The large negative effect for CP estimated for university enrolment is driven by women, for whom this coefficient is negative and significant. This result confirms that for some individuals attending a more competitive school is not necessarily beneficial. Women, for example, are often negatively affected by a more competitive schooling environment, and this may drive the estimated effect (e.g. Niederle and Vesterlund, 2007; Almås, Cappelen, Salvanes, Sørensen, and Tungodden, 2016).

5.2 Robustness Checks for the Empirical Specification

GPA and Time Functional Forms In Table 6, we show the estimate of reduced form and treatment effects under different trends in GPA (quadratic) and different time trends (quadratic and linear but in months). Overall, the way we condition on trends in GPA and time in our main analysis does not seem to have a strong impact on the magnitude and sign of estimates.

Alternative Empirical Specification In section 4 we describe our empirical specification as a two-step procedure where we independently estimate the first stage and the reduced form for the treatment effects defined in Proposition 1. Alternatively, we can estimate the treatment effects using 2SLS exploiting subgroups and instruments for which the standard monotonicity assumption holds. Given assumptions 1– 3, if we compare pre-reform students only to post-reform students who are below the cutoff (controlling for trends which guarantee the reform and the cutoff are randomly assigned), we should be able to recover the treatment effect for CC, given that individuals in this subgroups are monotonically moved out of the competitive schools as a consequence of the reform. In a similar way, in the subsample of individuals who are either in pre-reform cohort or, post-reform, are above the cutoff, monotonicity should hold, in that they are only shifted into competitive schools after the reform. We can thus implement 2SLS separately for these different subgroups using the reform as an instrument for attending a competitive school and be able to recover the treatment effects of interest. The details of the empirical implementation of this strategy are in Appendix E.

This exercise empirically is different to our main specification only in that here we allow for different trends even before the reform among CC and CP and in the way s.e. are estimated. Otherwise, as shown, e.g., in Angrist and Pischke (2008), 2SLS and indirect least square estimators with one treatment and one instrumental variable are the same. In fact, results are very consistent (see Table 7), with coefficients slightly smaller in magnitude, but of the same sign and same statistical significance.

6 Policy Analysis

We conduct policy analysis by linking the net marginal benefit of a policy change to the point estimates that we obtained, as described in Section 3.3. In particular, we focus on predicted lifetime earnings. We convert the different education level obtained by each student into years of education, as described in Appendix F. We then compute the effect of attending a competitive school on years of education estimating the parameters of Equation (14) where we use years of education as the outcome variable. We finally convert this estimate into the effect on lifetime earnings drawing upon causal estimates of education returns in Norway from Bhuller, Mogstad, and Salvanes (2017).[27] Following Equations (8)–(10), we need to compute the share in the population of individuals of different abilities and we do so by computing the proportions of students above and below the cutoff.[28] We then investigate the effect of alternative targeting combining the estimates for individuals of different SES.

The first row in Table 5 shows the effect of a marginal expansion of access to competitive schools toward students with GPA above the cutoff on average lifetime earnings in the population. This is the effect of a marginal expansion toward high ability individuals, hence the effect of a more meritocratic system. As expected, given that the reform had negative impact on education of CC and negligible impact on CP, the effect is negative and relatively large (around 88,000 NOK or 15,000 USD). Instead of targeting high-ability students, we follow Equation (9) to evaluate the net marginal benefit of a counterfactual policy that expands access for low-ability students. As the estimated cutoffs lie around the 25th percentile of the middle school GPA distribution, we can think of this as the effect of extending offers to students with grades in the bottom quartile of the grades distribution, at the expense of students with higher grades. Indeed, our results indicate that this policy would have a positive effect on average lifetime earnings of 39,000 NOK (6,000 USD).

Alternative targeting can be based on a combination of observed characteristics and ability of the student. We thus estimate the effect of attending a competitive school on lifetime earnings of CC and CP from different SES and then consider the effect of a marginal expansion of competitive schools admission toward students of certain ability and background at the expenses of students with different ability and background (keeping the offer to the other categories fixed). For example, from Table 5 we learn that an exercise similar to the one in Black, Denning, and Rothstein (2020) which expands access to high-ability students from low SES at the expense of low-ability students from high SES (without affecting the probabilities for the remaining categories, i.e. low ability-low SES and high ability-high SES students) would have a very small and negative impact, partly because of the small effect the expansion has on low SES CP. The difference between this result and Black, Denning, and Rothstein (2020) may be due to the fact that low SES, in this context (only considering students enrolling in academic high schools) are not as disadvantaged as in other contexts (such as the one analyzed by

[27] Earnings are in 1998 Norwegian Krone (NOK), where 6 NOK $\approx$ 1 USD.

[28] Post reform we observe 64% of individuals being above their relevant cutoff. When we separate according to SES, we observe 35% of the population being above the cutoff from low SES, 29% above the cutoff and high SES, 25% below the cutoff and low SES and 11% below the cutoff and high SES.

Black, Denning, and Rothstein (2020)). Of course, this result does not mean that such a reform is not to be implemented at all, as it may have some positive effects on high-ability/low-SES students (e.g. the point estimate suggests about a 10 percentage point increase in high school completion for them), hence a policy maker with a strong preference for this group may find it optimal to implement it. However, our results suggest that the same reform would have a more positive effect *overall* if the excluded group were high-ability/high-SES students. Table 5 also shows that a policy maker who aims to maximize the average expected income in the population should expand competitive high school access to low-ability students from low-SES backgrounds at the expense of high-ability and high-SES students.

It is important to note that the results from our policy analysis should be interpreted locally, because the compositions of compliers CC and CP are specific to the admission cutoff values and the reform that we consider. For instance, suppose that the targeted student group is defined using a different cutoff. This would then change the distributions of student ability within the targeted group and the crowded-out group, relative to the ones we report. As we consider a marginal policy expansion, the composition of the compilers and defiers induced by the policy expansion would be different and they would have different treatment effects which, in turn, could lead to different policy implications. A second issue with our analysis is that the stylized model we propose assumes that the policy maker cares about the average outcome in the population, and that the potential outcome for an individual only depends on the school choices he/she makes, which is not influenced by the choices from others. This rules out the possibility of any changes in peer compositions affecting individual's outcome indirectly, which may in fact be at play especially when we change the composition within schools on the basis of both ability and SES. Despite the tests performed in Section 4.3 support the view that peer effects are not a primary issue in our empirical setting, the absence of interaction between other students' choices and the individual's potential outcome is a limitation of the model.

7 Conclusion

This paper analyzes the effect of a change in the system that allocates students to schools, in a context where the supply of seats in each school is fixed. This characteristic, together with an allocation rule that incentivizes requesting enrollment in a preferred school, generates a number of oversubscribed schools, for which the enrollment of some students crowds out others. We study this problem within a potential outcome framework that provides us with policy-relevant sufficient statistics. These can be used to compare different allocation systems keeping account of the effect on both included and crowded-out students.

We exploit a reform to high school allocation systems as an instrument to identify the different components of the policy-relevant parameters. We show that the reform generates cutoffs for admission to schools. We combine this with the date-of-birth threshold, which determines whether or not students are affected by the reform, to generate a multivalued instrument. Importantly, we explicitly state the three assumptions needed for the instrument to be able to separately identify the effect for the groups differently affected by the reform. Instruments for which these assumptions hold can be used to identify the effect of changes in allocation systems for any rival good that has fixed capacity. Other examples are in the allocation of public housing or health treatments.

Our conclusion on the specific allocation rule we analyze, the implementation of a meritocratic system in assigning students to more competitive schools, is that its overall effect is negative. This is because in our context competitive schools have little effect on high-ability students (who gain access thanks to merit-based systems) but have a positive effect on crowded-out low-ability students, who would in fact benefit from a system which *favours* their access to more competitive schools. Our results are informative to the policy debate on expansion of selective school systems, such as the grammar school system in the United Kingdom (Burgess, Dickson, and Macmillan, 2020).

It is important to note that our analysis focuses on changes in allocation rule while holding other factors fixed. There may be other adjustment margins, such as parental response, which may become more relevant when one evaluates the long-term effects of the change in allocation rule. In addition, a policy maker whose objective function weighs more heavily the utility of high-ability students may still find it optimal to implement a more meritocratic system. However, the results of this paper challenge the view that policy makers should always reward merit by allowing better students to choose their education, in that this can have negative externalities on less able students and thus not necessarily lead to a Pareto improvement.

References

Abdulkadiroğlu, A., J. Angrist, and P. Pathak (2014): "The Elite Illusion: Achievement Effects at Boston and New York Exam Schools," *Econometrica*, 82(1), 137–196.

Almås, I., A. W. Cappelen, K. G. Salvanes, E. Ø. Sørensen, and B. Tungodden (2016): "Willingness to compete: Family matters," *Management Science*, 62(8), 2149–2162.

Angrist, J. D. (1990): "Lifetime earnings and the Vietnam era draft lottery: evidence from social security administrative records," *The American Economic Review*, pp. 313–336.

Angrist, J. D., and J.-S. Pischke (2008): *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton university press.

Bhuller, M., M. Mogstad, and K. G. Salvanes (2017): "Life-cycle earnings, education premiums, and internal rates of return," *Journal of Labor Economics*, 35(4), 993–1030.

Black, S. E., J. T. Denning, and J. Rothstein (2020): "Winners and losers? the effect of gaining and losing access to selective colleges on education and labor market outcomes," Discussion paper, National Bureau of Economic Research.

Burgess, S., M. Dickson, and L. Macmillan (2020): "Do selective schooling systems increase inequality?," *Oxford Economic Papers*, 72(1), 1–24.

Butikofer, A., R. Ginja, F. Landaud, and K. V. Løken (2020): "School Selectivity, Peers, and Mental Health," *NHH Dept. of Economics Discussion Paper*, (21).

Clark, D. (2010): "Selective Schools and Academic Achievement," *The BE Journal of Economic Analysis & Policy*, 10(1).

Clark, D., and E. Del Bono (2016): "The Long-Run Effects of Attending an Elite School: Evidence from the United Kingdom," *American Economic Journal: Applied Economics*, 8(1), 150–176.

Clark, D., and H. Royer (2013): "The effect of education on adult mortality and health: Evidence from Britain," *American Economic Review*, 103(6), 2087–2120.

Cullen, J. B., M. C. Long, and R. Reback (2013): "Jockeying for position: Strategic high school choice under Texas' top ten percent plan," *Journal of Public Economics*, 97, 32–48.

Deming, D. J., J. S. Hastings, T. J. Kane, and D. O. Staiger (2014): "School Choice, School Quality, and Postsecondary Attainment," *The American economic review*, 104(3), 991–1013.

Diamond, R., and P. Persson (2016): "The long-term consequences of teacher discretion in grading of high-stakes tests," Discussion paper, National Bureau of Economic Research.

Dobbie, W., and R. G. Fryer (2014): "The Impact of Attending a School with High-Achieving Peers: Evidence from the New York City Exam Schools," *American Economic Journal: Applied Economics*, 6(3), 58–75.

Duflo, E., P. Dupas, and M. Kremer (2011): "Peer Effects, Teacher Incentives, and the Impact of Tracking: Evidence from a Randomized Evaluation in Kenya," *The American Economic Review*, 101(5), 1739–1774.

Fack, G., J. Grenet, and Y. He (2019): "Beyond Truth-Telling: Preference Estimation with Centralized School Choice and College Admissions," *American Economic Review*, 109(4), 1486–1529.

Hansen, B. E. (2000): "Sample splitting and threshold estimation," *Econometrica*, 68(3), 575–603.

Hendren, N. (2016): "The policy elasticity," *Tax Policy and the Economy*, 30(1), 51–89.

Hoekstra, M. (2009): "The effect of attending the flagship state university on earnings: A discontinuity-based approach," *The review of economics and statistics*, 91(4), 717–724.

Hoxby, C. (2000): "Does Competition Among Public Schools Benefit Students and Taxpayers?," *The American Economic Review*, 90(5), 1209–1238.

——— (2004): "School Choice and School Competition: Evidence from the United States," *Swedish Economic Policy Review*, 10.2.

Hsieh, C.-T., and M. Urquiola (2006): "The Effects of Generalized School Choice on Achievement and Stratification: Evidence from Chile's Voucher Program," *Journal of public Economics*, 90(8), 1477–1503.

Imbens, G. W., and J. D. Angrist (1994): "Identification and Estimation of Local Average Treatment Effects," *Econometrica: Journal of the Econometric Society*, pp. 467–475.

Kirabo Jackson, C. (2010): "Do Students Benefit from Attending Better Schools? Evidence from Rule-based Student Assignments in Trinidad and Tobago," *The Economic Journal*, 120(549), 1399–1429.

Kline, P., and C. R. Walters (2016): "Evaluating public programs with close substitutes: The case of Head Start," *The Quarterly Journal of Economics*, 131(4), 1795–1848.

Landaud, F., S. T. Ly, and É. Maurin (2020): "Competitive schools and the gender gap in the choice of field of study," *Journal of Human Resources*, 55(1), 278–308.

Lavy, V. (2010): "Effects of Free Choice among Public Schools," *The Review of Economic Studies*, 77(3), 1164–1191.

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

Luflade, M., and M. Zaiem (2016): "Do elite schools improve students performance? Evidence from Tunisia," *Working paper*.

Niederle, M., and L. Vesterlund (2007): "Do women shy away from competition? Do men compete too much?," *The quarterly journal of economics*, 122(3), 1067–1101.

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

Pop-Eleches, C., and M. Urquiola (2013): "Going to a Better School: Effects and Behavioral Responses," *The American Economic Review*, 103(4), 1289–1324.

Walters, C. R. (2018): "The demand for effective charter schools," *Journal of Political Economy*, 126(6), 2179–2223.

Wondratschek, V., K. Edmark, and M. Frölich (2013): "The short-and long-term effects of school choice on student outcomes—evidence from a school choice reform in Sweden," *Annals of Economics and Statistics/ANNALES D'ÉCONOMIE ET DE STATISTIQUE*, pp. 71–101.

	$\tilde{Z} = (0,0)$	$\tilde{Z} = (1,0)$	$\tilde{Z} = (1,1)$
Never Takers (NT)	D=0	D=0	D=0
Compliers – Crowded-out (CC)	D=1	D=0	D=1
Compliers – Pulled-in (CP)	D=0	D=0	D=1
Always Takers (AT)	D=1	D=1	D=1
Dropped (Assumption 4)	D=0	D=1	D=0
Dropped (Assumption 3)	D=0	D=1	D=1
Dropped (Assumption 3)	D=1	D=0	D=0
Dropped (Assumption 4)	D=1	D=1	D=0

Table 1: Compliance types identified by the multivalued instrument $\tilde{Z}$ given assumptions (1)–(4)

	(1)	(2)	(3)	(4)	(5)	(6)
	Competitive HS Enrolment	Competitive HS Enrolment	HS Completion	Avg days Absent	Uni Enrollment	Uni Completion
	(a) Top Quartile Middle School GPA					
	Overall	Dist<1.5km	Dist<1.5km	Dist<1.5km	Dist<1.5km	Dist<1.5km
Post	0.212***	0.0986	-0.001	-0.847	0.0271	-0.0325
	(0.0430)	(0.0701)	(0.0574)	(0.923)	(0.0646)	(0.0441)
N	1,941	419	419	369	419	419
	(b) Bottom 15 Percentiles Middle School GPA					
	Overall	Dist>5km	Dist>5km	Dist>5km	Dist>5km	Dist>5km
Post	-0.252***	0.001	-0.0545	1.845	-0.131	-0.0834
	(0.0402)	(0.0314)	(0.0867)	(1.329)	(0.0858)	(0.0944)
N	1,133	489	489	329	489	489

Table 2: Estimation of the effect of the reform on specific subgroups of the population.

Notes : Panel (a) shows the estimates for students who have a middle school GPA above the 75th percentile, panel (b) for students who have a middle school GPA below the 15th percentile. “Dist” represents the distance between middle school and one of the five competitive high schools. “Post” are estimates of coefficients on a dummy for being born after December 31st, 1988 and thus for being affected by the reform. Regressions also control for linear time trends in week of birth, allowed to change before and after the reform, and for seasonal dummies (52 dummies for weeks of birth within the year). * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

	Crowded-out Compliers		Pulled-in Compliers	
First Stage	0.146***		0.183***	
	(0.024)		(0.023)	
	Reduced Form	Treatment Effect	Reduced Form	Treatment Effect
School	-0.040**	-0.273**	0.003	0.018
Completion	(0.017)	(0.127)	(0.013)	(0.073)
Avg Days of	0.714**	5.618*	0.529*	2.699*
Absence[a]	(0.362)	(3.072)	(0.303)	(1.580)
University	-0.030	-0.204	-0.027	-0.149
Enrolment	(0.019)	(0.133)	(0.017)	(0.095)
University	-0.049**	-0.338**	-0.012	-0.068
Completion	(0.023)	(0.166)	(0.018)	(0.101)
N		7,724		

[a]Average days of absence are observed only for students who completed high school, hence these results are conditional on high school completion.

Table 3: First stage, reduced form and treatment effects for CC and CP

Notes : Treatment effects are estimated as the ratio between the Reduced Form and the First Stage. Estimates of the Reduced Form (First Stage) come from a regression of the outcome (dummy for attending a competitive school) on two dummies, one for being born after December 31st, 1988 and having middle school GPA below the relevant cutoff for admission at competitive school and another for being born after December 31st, 1988 and having middle school GPA above the cutoff. In addition, each regression controls for trends in week of birth and GPA and for seasonal (52 weeks) dummies. s.e. of Reduced Form and First Stage are clustered at the week-of-birth level. Treatment effect s.e. are estimated using the formula for the standard deviation of the ratio of two independent random variables, see section 4. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Crowded-out Compliers				Pulled-in Compliers			
	(a) Low SES		(b) High SES		(a) Low SES		(b) High SES	
First Stage	0.141***		0.132***		0.160***	0.160***	0.206***	
	(0.029)		(0.044)		(0.032)	(0.032)	(0.033)	
	R.F.	Treat.	R.F.	Treat.	R.F.	Treat.	R.F.	Treat.
School Completion	-0.049**	-0.346*	-0.016	-0.118	0.014	0.089	-0.004	-0.021
	(0.025)	(0.189)	(0.027)	(0.211)	(0.020)	(0.125)	(0.018)	(0.089)
Avg Days of Absence[a]	0.787*	6.228*	0.696	5.986	0.489	3.089	0.538	2.307
	(0.430)	(3.758)	(0.640)	(6.035)	(0.360)	(2.374)	(0.451)	(1.966)
University Enrolment	-0.044	-0.311	0.014	0.102	-0.008	-0.050	-0.041*	-0.200*
	(0.028)	(0.208)	(0.033)	(0.250)	(0.027)	(0.168)	(0.023)	(0.117)
University Completion	-0.064**	-0.456**	-0.011	-0.082	-0.006	-0.035	-0.010	-0.051
	(0.029)	(0.225)	(0.037)	(0.284)	(0.026)	(0.163)	(0.023)	(0.114)
N	4,573		3,151		4,573		3,151	

[a] Average days of absence are observed only for students who completed high school, hence these results are conditional on high school completion.

Table 4: Heterogeneity in the effect for CC and CP on the basis of students' SES

Notes : Results for panels (a) ("Low SES") are estimated in a sample which includes all the students who have at least one parent with no university education, while results for panels (b) ("High SES") are estimated using the sample of students whose parents (both) completed university. "R.F." columns show Reduced Form results, while "Treat."columns report treatment effects. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

(a) Expansion targeting ability group only

Pulled in group	**Crowded out group**	
	High Ability	Low ability
High Ability	-	-87,980
Low ability	39,398	-

(b) Expansion targeting ability group and SES group

Pulled in group	**Crowded out group**			
	High Ability		Low ability	
	Low SES	High SES	Low SES	High SES
High Ability, Low SES	-	19,578	-36,851	-11,268
High Ability, High SES	-20,966	-	-60,429	-33,033
Low Ability, Low SES	23,272	35,637	-	16,156
Low Ability, High SES	2,938	8,043	-6,670	-

Table 5: Effect on population average lifetime earnings (in NOK) of marginally expanding access to competitive high school to one pulled-in group at the expense of a crowded-out group.

Notes : Appendix F describes how we translate the effect on different margins of education into lifetime earnings. The different effects are computed following equation (10), based on the estimated effects from Tables 3 and 4. When computing the effect of expansions targeting ability and SES groups jointly, we impose that attendance of competitive high schools does not change for the groups which are neither pulled in nor crowded out.

	(1)		(2)		(3)		(4)	
	Baseline		Grades Quadratic		Week Quadratic		Months	
				(a) Crowded-out Compliers				
First stage	0.141*** (0.023)		0.148*** (0.023)		0.111*** (0.021)		0.138*** (0.025)	
	R.F.	Treat.	R.F.	Treat.	R.F.	Treat.	R.F.	Treat.
School Completion	-0.035**	-0.251*	-0.029*	-0.196	-0.032**	-0.291**	-0.035*	-0.254*
	(0.017)	(0.131)	(0.017)	(0.121)	(0.013)	(0.132)	(0.019)	(0.144)
Avg Days of Absence	0.626*	5.122	0.697*	5.259*	0.401	4.711	0.639*	5.331*
	(0.363)	(3.166)	(0.362)	(2.923)	(0.274)	(3.467)	(0.328)	(2.989)
University Enrolment	-0.025	-0.179	-0.019	-0.129	-0.028*	-0.254*	-0.024	-0.171
	(0.019)	(0.136)	(0.019)	(0.127)	(0.016)	(0.148)	(0.020)	(0.150)
University Completion	-0.043*	-0.306*	-0.039*	-0.260	-0.036**	-0.323*	-0.043	-0.310
	(0.023)	(0.170)	(0.023)	(0.158)	(0.018)	(0.174)	(0.028)	(0.211)
				(b) Pulled-in Compliers				
First stage	0.184*** (0.023)		0.190*** (0.023)		0.165*** (0.021)		0.184*** (0.023)	
	R.F.	Treat.	R.F.	Treat.	R.F.	Treat.	R.F.	Treat.
School Completion	0.006	0.034	0.002	0.011	0.005	0.028	0.006	0.035
	(0.013)	(0.072)	(0.013)	(0.070)	(0.012)	(0.075)	(0.013)	(0.069)
Avg Days of Absence	0.471	2.374	0.417	2.022	0.594**	3.362**	0.476*	2.411*
	(0.301)	(1.547)	(0.300)	(1.474)	(0.258)	(1.522)	(0.266)	(1.383)
University Enrolment	-0.024	-0.130	-0.028*	-0.149	-0.022	-0.133	-0.022	-0.122
	(0.017)	(0.093)	(0.017)	(0.091)	(0.015)	(0.094)	(0.018)	(0.098)
University Completion	-0.008	-0.043	-0.011	-0.059	-0.012	-0.072	-0.005	-0.027
	(0.018)	(0.100)	(0.018)	(0.098)	(0.017)	(0.104)	(0.019)	(0.102)

Table 6: Robustness checks for the functional forms of trends used in estimating the main equation.

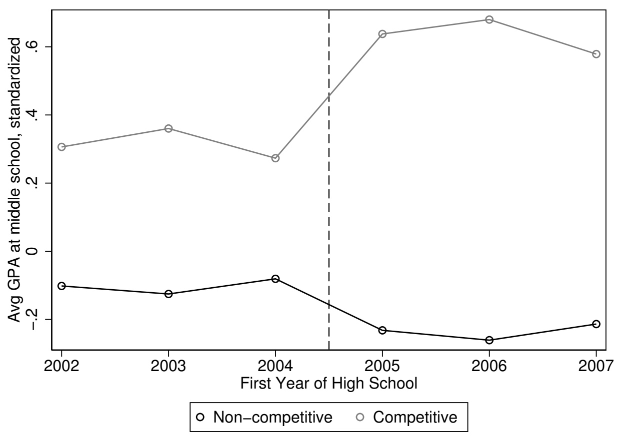
Notes : The table reports the estimate of the effect of the reduced form ("R.F.") and treatment effects ("Treat.") for CC (panel (a)) and CP (panel (b)), when using different specifications of GPA and time trends. Column (1) shows the baseline specification (linear in GPA and in weeks of birth, where the trends in weeks of births are allowed to differ before and after the reform and above and below the cutoff). Column (2) uses a quadratic trend for GPA. Column (3) uses a quadratic trend in week of birth. Column (4) uses a linear trend in month of birth instead of week birth. Controls always include 52 dummies for week-of-birth within the year, apart from column (3), which uses 12 month-of-birth dummies. Standard errors for R.F. and first stage are always clustered at the week-of-birth level, apart from column (3), which uses the month-of-birth level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1) Crowded-Out Compliers	(2) Pulled-In Compliers
First stage	0.156***	0.194***
	(0.024)	(0.023)
School Completion	-0.193*	0.036
	(0.111)	(0.067)
Avg Days of Absence[a]	5.031*	2.341
	(2.771)	(1.469)
University Enrollment	-0.119	-0.125
	(0.120)	(0.088)
University Completion	-0.270*	-0.028
	(0.153)	(0.093)
N	5,178	6,108

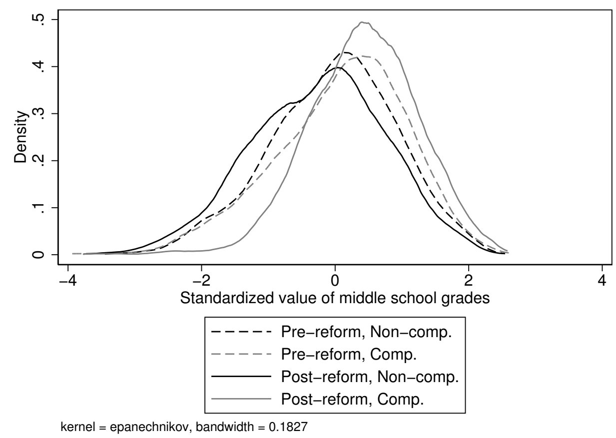
[a] Average days of absence are observed only for students who completed high school, hence these results are conditional on high school completion.

Table 7: 2SLS estimate of the effect of attending a competitive school, separately for CC and CP.

Notes : Each parameter estimate comes from a separate estimation. For CC (CP), only individuals who are either not affected by the reform or below (above) the cutoff after the reform are included in the sample used in the estimation, and a dummy for being below (above) the cutoff after the reform is the instrumental variable used. Each regression controls for time trend and middle school GPA trend. We control using seasonal (52 weeks) dummies and cluster the standard errors at the week-of-birth level. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$



(a) Average GPA



(b) GPA Distribution

Figure 1: Middle school GPA comparison, competitive and noncompetitive high schools.
Notes : The middle school GPA distribution in these figures is normalized to have mean 0 and s.d. 1 at the cohort level within students included in our sample.

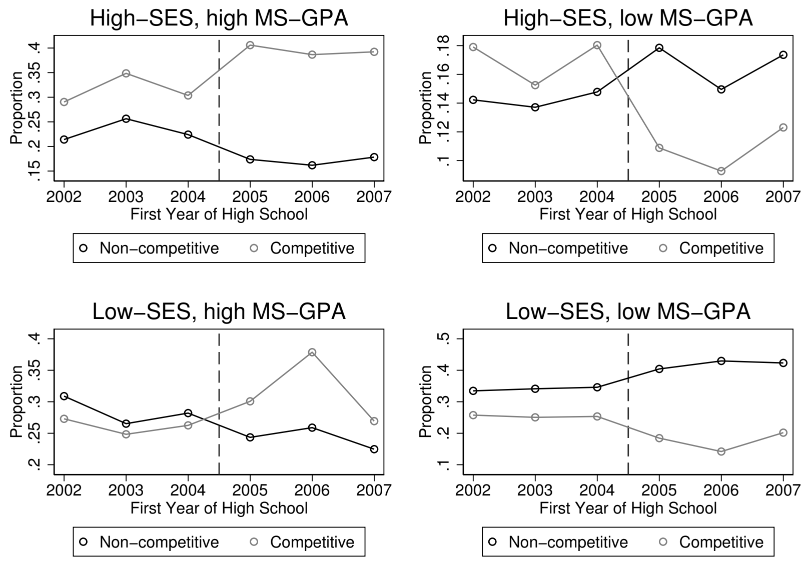


Figure 2: Proportion of students with different middle school GPA (MS-GPA) and different SES attending competitive and noncompetitive high schools.

Notes : These figures show the proportion of students attending competitive and noncompetitive schools belonging to each subgroup. High/Low-SES is defined on the basis of education of parents (High-SES students have both parents having completed university, see Section 4.1. High/Low middle school-GPA is defined based on whether students are above or below the median of the middle school GPA distribution in their cohort).

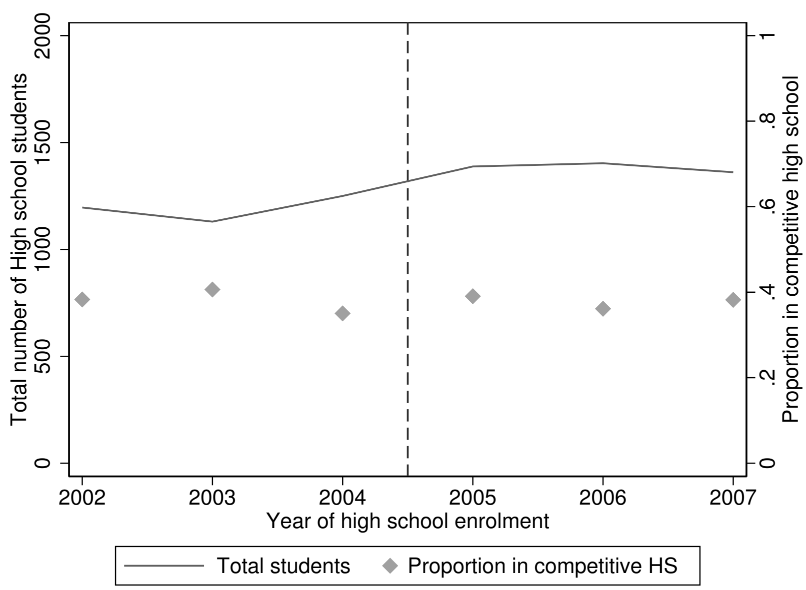
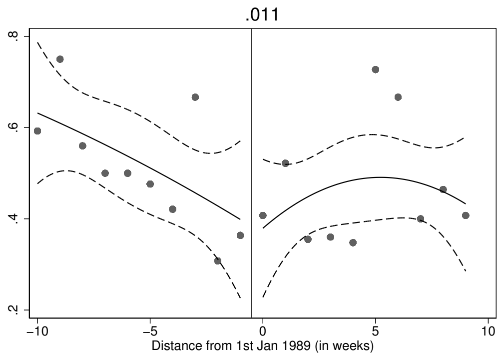
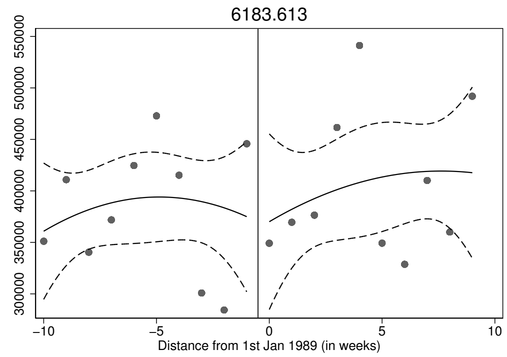


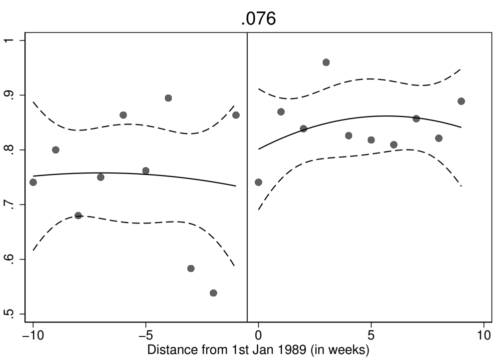
Figure 3: Number of students enrolling in academic HS and, among these, proportion enrolling in competitive HS in every year.



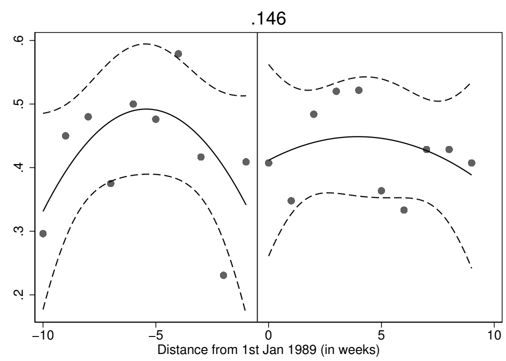
(a) Female



(b) HH Earnings



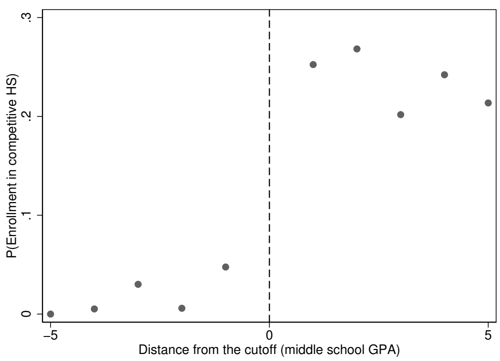
(c) Parents' HS



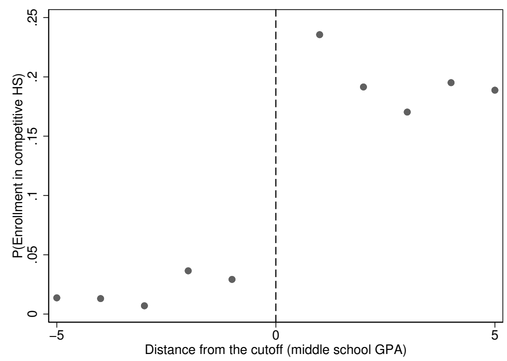
(d) Parents' Uni

Figure 4: Covariates balance around the threshold for being affected by the reform.

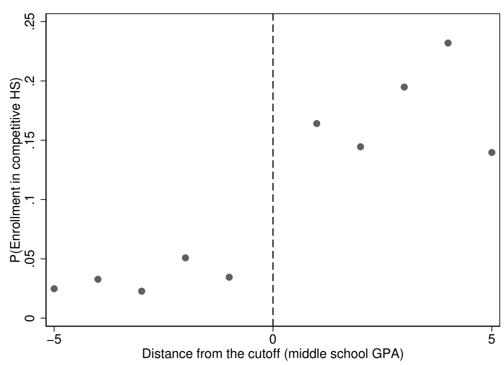
Notes : Students affected by the reform are those born after December 31, 1988. The plots show the average value of each covariate (proportion of females (a), paternal earnings in the three years during high school (b), proportion of students with both parents graduated from high school (c) and from university (d)) for students born in the different weeks around the first week of 1989, for the 10 weeks before and the 10 weeks after. The solid line shows a quadratic fit, while the dashed lines are the 95% confidence interval of such a fit. The numbers at the top of the figures are the estimates of the parameter on a dummy for being born after the threshold date, from a regression of the characteristics on the dummy for being born after the threshold and a quadratic trend in the week distance from the threshold (allowed to differ before and after the cutoff). Standard errors are clustered at the week-of-birth level and none of the reported coefficients are statistically significant.



(a) 2005



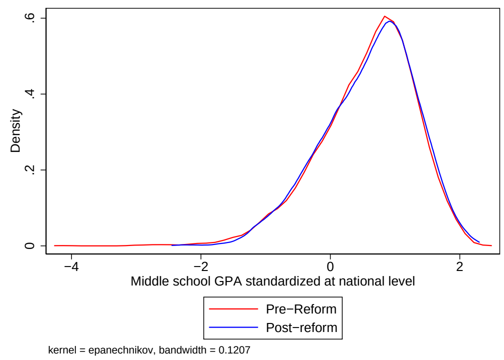
(b) 2006



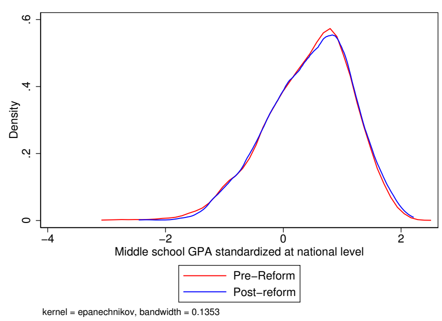
(c) 2007

Figure 5: Probability of enrolling in a competitive high school around grades cutoff.

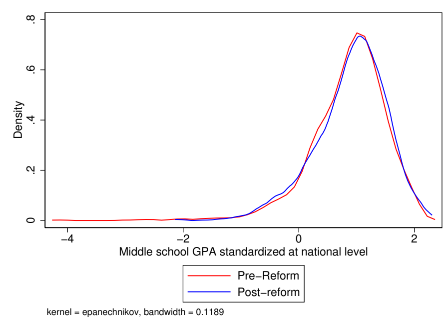
Notes : These figures pull together all different cutoffs in each post-reform year. The plots show the proportion of students enrolling in a competitive high school for students whose GPAs is between -5 and 5 GPA points away from the cutoff (each point is the proportion for all students who are within that distance). Details on how the thresholds are defined are provided in Section D.



(a) Overall



(b) Low SES



(c) High SES

Figure 6: Robustness Checks for Middle School GPA Distribution.

Notes : In panel (a) we test whether the distribution of grades for cohorts pre- and post-reform are statistically different when normalized at the national level (so that the grades of each student reflect the relative position in the national middle school GPA distribution of their cohort). The p-value of the Kolmogorov–Smirnov test for the equality of the distributions is 0.238. In panel (b) and (c) we plot the same distribution, separately for individuals with at least one parent without a university degree (panel (b)) and for individuals whose parents (both) have a university degree (panel (c)). The p-value of the Kolmogorov–Smirnov test for the equality of the distributions is 0.364 (panel (b)) and 0.269 (panel (c)). Cohorts enrolling in high school in 2003 and 2004 are pre-reform, while those enrolling in 2005 and 2006 are post-reform.

ONLINE APPENDIX

A The Norwegian education system

The Norwegian education system consists of four levels, primary school (grades 1–7), middle school or lower secondary school (grades 8–10), high school or upper secondary school (three years), and then higher education. Norwegian compulsory education starts at age six, lasts for 10 years and consists of primary school and lower secondary school. Norwegian municipalities operate schools to provide compulsory education, and the vast majority (98%) of pupils attend public, local schools during compulsory schooling. At the elementary school level, all pupils are allocated to schools based on fixed school catchment areas within municipalities. With the exception of some religious schools and schools using specialized pedagogic principles, parents are not able to choose the school to which their children are sent (except by moving to a different neighborhood). There is a direct link between elementary school attendance and attendance at middle or lower secondary schools (ages 13–16/grades 8–10), in that elementary schools feed directly into lower secondary schools. In many cases, primary and lower secondary schools are also integrated.

The high schools have two main tracks, vocational and academic. High schools are administered at the county level (above the level of municipalities) and attendance is not mandatory, although since the early 1990s everybody graduating from middle schools has been guaranteed a slot in high school. Admissions procedures differ across counties for upper secondary schools. In some counties, pupils can freely choose schools, while in others children are allocated to schools based on well-defined catchment areas, or high school zones. Within schools, there is no systematic sorting of students into classes.

About 95% of students moving into high school enroll in the year they finish compulsory education. About 45% enroll in the academic track, which qualifies for higher education. The rest of the students enroll in the vocational track, and there are several subject fields for this track. There is an option also for students coming from the vocational track to enroll in university, but that requires some extra coursework. Admission at different universities and in different majors at universities is based on high school GPA. This is a combination of nonblind grading by local teachers and the results of the final-year exams, which are prepared centrally by The Directorate for Education (a branch of the Ministry of Education) and is subject to blind grading. The high school GPA is not normalized at the school level.

A.1 Teacher grading and exam grading at middle school (Middle school GPA)

At the end of middle school, students are evaluated both nonanonymously by their teachers for 11 subjects taught in school, and in addition anonymously in 1–2 central exit exams, which are graded by external examiners (hence not students' teachers). The subjects for the central exit exams are randomly selected for each student among the 11 subjects in which they are also evaluated by their teachers. Both grades by teachers and in the final exams are oral and written for Norwegian and English, and are written for the other subjects and range between 1 and 6. In each subject, the final grade is an average between the all the available assessments in that subject (written, oral and centralized exam). The grade point average (GPA) consists of the sum of grades in every subject, for students who had grades in at least 3 subjects, hence it range between 3 and 66 (i.e. 6×11). The middle school GPA is not standardized at the school level.[29]

Grading principles are set by the Education Act of 1998 ("Opplæringslova"). In the Prescript to the Education Act of 1998 (Forskrift til opplæringslova) it is stated that teacher evaluations are to be based on the degree to which students have achieved the competence goals stated by the subject-specific centrally set "Learning goals," which are stated in each topic. For each subject, a grade is given for each semester, and the final teacher evaluation grade is set on the basis both semesters' grades each year. Notably, it is specifically stated that student behavior ("orden og oppførsel") is not to be reflected in grading, and (of course) that student background should not count in grading ("Prescript to Education Act"). Effort is allowed to be included in grading in gymnastics. Teacher grades are to be set before grading of exams.

[29]Note that grading changed in the school year 2006/2007 (hence the last year we observe), when instead of summing up the grades in 11 subject, the average between grades in all subjects multiplied by 10 is taken. This means that the maximum grade is 60. In practice, we observe that, if we add the average grade obtained in one subject (which in the data is 4.5) to the GPA of all students completing middle school in 2007, the distribution of GPA in 2007 overlaps to the distribution for the previous years. Hence, we use this "adjusted" version of the GPA for 2007 in our main analysis: this rests on the assumption that the position of students in the GPA distribution in 2007 would have been the same under the old grading system. Even more explicitly, we can standardize GPA at the year level, so that at the year level the distribution has mean 0 and s.d. 1, preserving the rank of students. When we do it, the results are slightly less precisely estimated, but substantially unchanged in magnitude.

B The 2005 High School Admission Reform in Hordaland County: Details

The reform we analyze was passed by the government of Hordaland county in autumn of 2004, and changed the enrollment system for students starting high school in August 2005. Before the 2005–2006 academic year, students were assigned by the county's school administrative office to one of the schools that was closer to their home, to reduce travel time. Hence, a "catchment area" system was in place, where the place of residence determined the school attended by a student who decided to enroll in an academic track. Some exceptions to this rule were allowed. In particular, it was possible for very high-ability students to request to attend one specific school. In practice, this caused a small number of high-ability students who lived out of the city center to attend high-reputation schools in the city center. As these are students with high grades at middle school, we are comfortable in considering them as always takers (see Section 3.2), hence not affecting our identification strategy.

The reform was approved by the Hordaland county administration as a response to pressure from different interest groups. It established that starting from the following academic year (2005-2006) students were allowed to apply to different schools with no geographical restrictions. They could list up to six schools, and assignment would have been based on preferences and, if a school was oversubscribed, the middle school GPA. Only the county's school administrative office, and not the schools, is then involved in the assignment procedure.

As students are assigned to school cohorts on the basis of year of birth (hence, students born in December are assigned to one cohort, while students born in January are assigned to the next cohort), the reform affected students born in 1989 onward. The reform changed the allocation of students to high school in the whole county, but we focus on the municipality of Bergen and its neighboring municipalities (Os, Øygarden and Askøy) because students who live further away would have a very long commute to reach a high school that is not the one in their municipality, hence this does not happen. In addition, we focus only on the academic track. The reason is that the reform only affected academic-track students, because vocational tracks are specific to one subject and often there is only one school offering that specific subject within the county. Thus, students who were willing to attend one specific vocational course were generally allowed to enroll in the only school offering that course both before and after the reform. In the period we consider, there were 16 academic high schools in the Bergen area.

Parents and pupils are well aware of the quality of each high school in our data period because the school rankings were provided by a publicly available website and extensively reported in the newspapers. Public information about school performance across high schools in Norway (i.e., league tables) became available in 2001.

C Proof of Proposition 1

Combining different realizations of the instrument and of the treatment, we observe six data moments. These are a weighted average of the average potential outcome for the different compliance types, where the weights are given by the proportions of each type (π). First, we show that we can identify the effect for crowded-out compliers (CC). Consider the students who receive either $\tilde{Z} = (0,0)$ or $\tilde{Z} = (1,0)$, i.e. either students not affected by the reform or students below the cutoff after the reform. Given Assumptions (1)–(4) we can write the four data moments that characterize this group as follows:

1. $E[Y|D=1,\tilde{Z}=(0,0)] = \frac{\pi_{CC}}{\pi_{AT}+\pi_{CC}}E[Y^1|CC] + \frac{\pi_{AT}}{\pi_{AT}+\pi_{CC}}E[Y^1|AT]$
2. $E[Y|D=0,\tilde{Z}=(0,0)] = \frac{\pi_{NT}}{\pi_{NT}+\pi_{CP}}E[Y^0|NT] + \frac{\pi_{CP}}{\pi_{NT}+\pi_{CP}}E[Y^0|CP]$
3. $E[Y|D=1,\tilde{Z}=(1,0)] = E[Y^1|AT]$
4. $E[Y|D=0,\tilde{Z}=(1,0)] = \frac{\pi_{NT}}{\pi_{NT}+\pi_{CP}+\pi_{CC}}E[Y^0|NT] + \frac{\pi_{CP}}{\pi_{NT}+\pi_{CP}+\pi_{CC}}E[Y^0|CP] + \frac{\pi_{CC}}{\pi_{NT}+\pi_{CP}+\pi_{CC}}E[Y^0|CC]$

Because of the exclusion restriction, we know that the outcome for AT who are in the pre-reform period is the same as the outcome for AT in the post-reform period, hence we can rewrite

$$E[Y|D=1,\tilde{Z}=(0,0)] = \frac{\pi_{CC}}{\pi_{AT}+\pi_{CC}}E[Y^1|CC] + \frac{\pi_{AT}}{\pi_{AT}+\pi_{CC}}E[Y|D=1,\tilde{Z}=(1,0)]$$

and thus

$$E[Y^1|CC] = \frac{\pi_{AT}+\pi_{CC}}{\pi_{CC}}E[Y|D=1,\tilde{Z}=(0,0)] - \frac{\pi_{AT}}{\pi_{CC}}E[Y|D=1,\tilde{Z}=(1,0)].$$

Similarly, we can rewrite the last data moment as

$$\pi_{CC}E[Y^0|CC] = (\pi_{NT}+\pi_{CP}+\pi_{CC})E[Y|D=0,\tilde{Z}=(1,0)] - \pi_{NT}E[Y^0|NT] - \pi_{CP}E[Y^0|CP]$$

and combining with the second data moment from above

$$\frac{\pi_{CC}}{\pi_{NT} + \pi CP} E[Y^0|CC] = \frac{\pi_{NT} + \pi_{CP} + \pi_{CC}}{\pi_{NT} + \pi_{CP}} E[Y|D = 0, \tilde{Z} = (1,0)] - E[Y|D = 0, \tilde{Z} = (0,0)]$$

which then gives

$$E[Y^0|CC] = \frac{\pi_{NT} + \pi_{CP} + \pi_{CC}}{\pi_{CC}} E[Y|D = 0, \tilde{Z} = (1,0)] - \frac{\pi_{NT} + \pi_{CP}}{\pi_{CC}} E[Y|D = 0, \tilde{Z} = (0,0)].$$

Finally, we can subtract the equations we derived for $E[Y^0|CC]$ and $E[Y^1|CC]$ to obtain the effect of being crowded out from a competitive school for CC in terms of observed data moments. Note that, because the compliance types are mutually exclusive, $(\pi_{NT} + \pi_{CP} + \pi_{CC})$ is the probability that students do not attend a competitive school when they are below the cutoff post-reform, i.e. $P(D = 0, \tilde{Z} = (1,0)) = \pi_{NT} + \pi_{CP} + \pi_{CC}$, while π_{AT} is the probability that a student attends a competitive school below the cutoff post-reform i.e. $P(D = 1, \tilde{Z} = (1,0)) = \pi_{AT}$. Hence,

$$(\pi_{NT} + \pi_{CP} + \pi_{CC}) E[Y|D = 0, \tilde{Z} = (1,0)] + \pi_{AT} E[Y|D = 1, \tilde{Z} = (1,0)] = E[Y|\tilde{Z} = (1,0)].$$

Similarly, it is easy to show that

$$(\pi_{AT} + \pi_{CC}) E[Y|D = 1, \tilde{Z} = (0,0)] + (\pi_{NT} + \pi_{CP}) E[Y|D = 0, \tilde{Z} = (0,0)] = E[Y|\tilde{Z} = (0,0)],$$

so that

$$E[Y^0|CC] - E[Y^1|CC] = \frac{E[Y|\tilde{Z} = (1,0)] - E[Y|\tilde{Z} = (0,0)]}{\pi_{CC}}.$$

In order to derive the proportion of CC, consider the following three data moments observed in the sample only including $\tilde{Z} = (0,0)$ and $\tilde{Z} = (1,0)$:

1. $P(D = 1|\tilde{Z} = (0,0)) = \pi_{CC} + \pi_{AT}$
2. $1 - P(D = 1|\tilde{Z} = (0,0)) = P(D = 0|\tilde{Z} = (0,0)) = \pi_{NT} + \pi_{CP}$
3. $P(D = 1|\tilde{Z} = (1,0)) = \pi_{AT}$

where it is clear that we can identify the proportion of CC combining the first and the last data moments. As a result, the effect of being crowded out for CC is

$$E[Y^0|CC] - E[Y^1|CC] = -\frac{E[Y|\tilde{Z} = (0,0)] - E[Y|\tilde{Z} = (1,0)]}{P(D = 1|\tilde{Z} = (0,0)) - P(D = 1|\tilde{Z} = (1,0))}.$$

The intuition behind this derivation is that CP in this context acts as NT, because they attend a noncompetitive school irrespective of the reform, as they are below the cutoff if affected by the reform. Clearly, we do not have enough data moments to separately identify the effect on NT and on CP in this sample. However, this is not a relevant effect, because deriving the effect for CP in the counterfactual scenario when they are below the cutoff is not required for computing any policy-relevant parameter. Note also that the derivation relies on the assumption of randomness of the reform. As we consider all the students before the reform and only students below the cutoff after the reform, this may appear as a violation of random assignment, because pre-reform students also include students with a higher middle school GPA. Students with a higher GPA are more likely to have higher educational outcomes, and thus Z is no longer random. For this reason, as we explain in Section 3.2, we assume *conditional* randomness of the reform, and in our empirical application we always control for the direct effect of GPA on the final outcome.

All of these arguments hold in the derivation of the effect on the CP. This time, consider the subsample of individuals with either $\tilde{Z} = (0,0)$ or $\tilde{Z} = (1,1)$. We observe four data moments that are combinations of the outcomes of different compliance types:

1. $E[Y|D = 1, \tilde{Z} = (0,0)] = \frac{\pi_{CC}}{\pi_{AT}+\pi_{CC}} E[Y^1|CC] + \frac{\pi_{AT}}{\pi_{AT}+\pi_{CC}} E[Y^1|AT]$
2. $E[Y|D = 0, \tilde{Z} = (0,0)] = \frac{\pi_{NT}}{\pi_{NT}+\pi_{CP}} E[Y^0|NT] + \frac{\pi_{CP}}{\pi_{NT}+\pi_{CP}} E[Y^0|CP]$
3. $E[Y|D = 1, \tilde{Z} = (1,1)] = \frac{\pi^{AT}}{\pi^{AT}+\pi^{CP}+\pi^{CC}} E[Y^{D=1}|AT] + \frac{\pi^{CP}}{\pi^{AT}+\pi^{CP}+\pi^{CC}} E[Y^{D=1}|CP] + \frac{\pi^{CC}}{\pi^{AT}+\pi^{CP}+\pi^{CC}} E[Y^{D=1}|CC]$
4. $E[Y|D = 0, \tilde{Z} = (1,1)] = E[Y^{D=0}|NT]$

This time we combine the second and the last moments to obtain

$$E[Y^0|CP] = \frac{\pi_{NT} + \pi_{CP}}{\pi_{CP}} E[Y|D = 0, \tilde{Z} = (0,0)] - \frac{\pi_{NT}}{\pi_{CP}} E[Y|D = 0, \tilde{Z} = (1,1)]$$

and the first and third moments give us

$$E[Y^1|CP] = \frac{\pi_{AT} + \pi_{CP} + \pi_{CC}}{\pi_{CP}} E[Y|D=1, \tilde{Z} = (1,1)] - \frac{\pi_{AT} + \pi_{CP}}{\pi_{CP}} E[Y|D=1, \tilde{Z} = (0,0)].$$

And again, given that compliance types are mutually exclusive, we get the result

$$E[Y^1|CP] - E[Y^0|CP] = \frac{E[Y|\tilde{Z} = (1,1)] - E[Y|\tilde{Z} = (0,0)]}{\pi_{CP}}.$$

Using the following three data moments observed in the sample only including $\tilde{Z} = (0,0)$ and $\tilde{Z} = (1,1)$:

1. $P(D=1|\tilde{Z} = (0,0)) = \pi_{CC} + \pi_{AT}$
2. $1 - P(D=1|\tilde{Z} = (0,0)) = P(D=0|\tilde{Z} = (0,0)) = \pi_{NT} + \pi_{CP}$
3. $1 - P(D=1|\tilde{Z} = (1,1)) = P(D=0|\tilde{Z} = (1,1)) = \pi_{NT}$

we derive

$$E[Y^1|CP] - E[Y^0|CP] = \frac{E[Y|\tilde{Z} = (1,1)] - E[Y|\tilde{Z} = (0,0)]}{P(D=1|\tilde{Z} = (1,1)) - P(D=1|\tilde{Z} = (1,0))}.$$

D Competitive Schools' Cutoffs

In the data, we do not have information on the cutoff used by oversubscribed schools to admit students in the post-reform period. However, we observe the middle school GPA for every student and we know which school they end up attending. We can identify the cutoff for any school (if it exists) following the threshold estimation literature, as in Hansen (2000). The same approach has been applied to school systems in Hoekstra (2009), Landaud, Ly, and Maurin (2020) and Butikofer, Ginja, Landaud, and Løken (2020).

For every school and cohort, we consider the students who attend a middle school that is at most eight kilometers away as the pool of potential applicants to that school. About 80% of the students attend a high school that is within eight kilometers of their middle school. Note that catchment areas before 2005 were defined at a much smaller distance than eight kilometers. We then consider each school and each year separately.

For every value G_n of the middle school GPA distribution of the pool of potential applicants ($n \in [1, N]$ where N is the total number of values that GPA takes among potential applicants), we define a dummy $g_{n,i}$ that takes value 1 if student i scores above that specific value (i.e. $g_{n,i} = \mathbf{1}[GPA_i \geq G_n] \ \forall\, n \in [1, N]$). Next, we run one bivariate regression for each of these N values, where the dependent variable is a dummy for being admitted at the school ($s_i = 1$ if student i is admitted at the school we consider) and the independent variable is the dummy $g_{n,i}$ defined above:

$$s_i = \alpha + \beta g_{n,i} + \varepsilon_i$$

We select as the admission cutoff for a school in a specific year the value G_n of the GPA distribution for which the regression of the associated dummy has the highest R^2 among all the school-year-specific regressions, under the restriction that it estimates a significantly positive coefficient β. If the coefficient is negative or not significant, then no cutoff is assigned to that school. Using this procedure, we estimate a cutoff for five out of 16 high schools in 2005 and 2006, and four out of 16 high schools in 2007. The estimated cutoffs range between the 10th and the 30th percentiles of the middle school GPA in 2005, between the 15th and the 35th percentiles in 2006 and between the 10th and the 60th percentiles in 2007.

To define the instrument $\tilde{Z} = (Z, C)$, we need to then assign a cutoff for admission at selective schools to each student affected by the reform. We define it as the lowest cutoff that grants access to one of the competitive schools within eight kilometers from the middle school attended, in the year the student turned 16. As mentioned, 80% of the students attended a high school within this distance. As a result of this definition, we treat students who attended middle schools that do not have any competitive high school within eight kilometers as being below any cutoff for admission at a competitive high school. Hence, if they attended a competitive school after the reform, they are considered always takers. Similarly, few students (about 2% of the sample) attended a competitive school beyond eight kilometers in the post-reform period, although they do have a competitive school in their eight kilometers neighborhood, hence they are assigned the "wrong" within-eight-kilometers cutoff. We treat them as always takers, assuming they would also have attended a selective high school in the pre-reform period.

We experiment using 10 instead of eight kilometers as the distance measure to assign the cutoff and Table A5 reproduces the results in Table 3 using 10 kilometers. The results point in the same direction. They are larger in magnitude, but noisier, as one would expect if using 10 kilometers produces a less precise measure of the relevant cutoff than using eight kilometers.

E Empirical Strategy Using 2SLS

Proposition 1 implies that we can eventually recover the effect of attending a competitive school for CC and CP separately, by instrumenting the treatment with two different binary variables using different subsamples. For CC, we should include in the sample only individuals who either have $\tilde{Z} = (0,0)$ or $\tilde{Z} = (1,0)$ and instrument treatment with a binary variable that can take only these two values, while for CP we can do the same, this time only including individuals for whom $\tilde{Z} = (0,0)$ or $\tilde{Z} = (1,1)$. The same arguments as in Section 4.2 extend to this framework and we condition on trends in date of birth and GPA to ensure that instrument $\tilde{Z}$ is randomly assigned. We thus recover the estimates of the treatment effect separately for compliers and defiers estimating the parameters of the following regression, exploiting 2SLS estimator on different samples:

$$y = \alpha_k + \lambda_k d + f_k(t) + g_k(GPA) + \gamma_{\mathbf{k}}\mathbf{w} + \varepsilon \qquad k \in \{CC, CP\} \tag{E.1}$$

where the first stage is given by

$$d = \alpha_k^{fs} + \psi_k z_k + f_k^{fs}(t) + g_k^{fs}(GPA) + \gamma_{\mathbf{k}}^{\mathbf{fs}}\mathbf{w} + u \qquad k \in \{CC, CP\} \tag{E.2}$$

When we estimate the parameters for CC, we use the population of students with either $\tilde{Z} = (0,0)$ or $\tilde{Z} = (1,0)$ and define the binary instrument $\tilde{Z}_{CC} = \mathbf{1}[\tilde{Z} = (1,0)]$. For CP, we use students with either $\tilde{Z} = (0,0)$ or $\tilde{Z} = (1,1)$ and instrument $Z_{CP} = \mathbf{1}[\tilde{Z} = (1,1)]$. The estimate of the parameter λ_{CC} corresponds to the effect of attending competitive schools for crowded-out students, while the estimate of λ_{CP} is the same effect for those who are pulled in. As the reform *excludes* CC from attending a competitive school, the parameter of interest in evaluating the reform is $-\lambda_{CC}$ (see Proposition 1).

F From Education to Lifetime Earnings: Details

We translate the estimated effect on different margins of education into lifetime earnings in the following way. First, we build a measure of the number of years of education at the individual level. In particular, to individuals who do not complete high school we assign 11.5 years of education since in our data, on average, individuals dropping out of high school attend 1.5 years. Those who complete high school but do not enrol in university obtain 13 years of education. Students who drop out of university on average stay 2 years in university and we thus assign them 15 years of education. Students who complete university obtain 16 years of education.

We then estimate the effect of attending a competitive school for CC and CP on this measure, to compute the change in years of education caused by competitive schools. The change in years of education is translated into changes in lifetime earnings exploiting the estimates from Bhuller, Mogstad, and Salvanes (2017). In particular, they estimate that, in Norway, one more year of education causally lead to 12,936 NOK more in every year of working life. We consider 46 years of working life, thus an additional year of education leads to an increase by 12,936*46 NOK during the lifetime, and this is the number we use to translate the change in years of education into lifetime earnings.

G Tables and Figures

	Overall	Men	Women	Low SES	High SES	Low Grades Comp.	High Grades No Comp.
(a) High School Outcomes							
Admission at 5 Comp. Schools	0.37	0.36	0.37	0.32	0.43	1	0
Days Absent during HS	6.58 (4.97)	6.42 (5.20)	6.71 (4.77)	6.61 (4.92)	6.55 (5.03)	7.96 (5.21)	6.17 (4.76)
More than 3 Days Absent	0.77	0.73	0.80	0.78	0.76	0.85	0.74
More than 6 Days Absent	0.47	0.45	0.49	0.48	0.47	0.62	0.44
High School completion	0.90	0.86	0.93	0.88	0.93	0.77	0.94
(b) University Outcomes							
University Enrolment	0.84	0.80	0.88	0.81	0.90	0.72	0.90
University Completion	0.81	0.74	0.87	0.76	0.88	0.63	0.88
Elite Uni Enrolment	0.15	0.17	0.13	0.10	0.22	0.04	0.18
Elite Uni Completion	0.10	0.13	0.08	0.07	0.15	0.02	0.13
STEM Uni Enrolment	0.16	0.22	0.10	0.15	0.17	0.13	0.17
STEM Uni Completion	0.10	0.13	0.07	0.09	0.11	0.07	0.11
(c) Students Characteristics							
HH earnings (in 1998 NOK)	371, 870 (213,080)	370,949 (218,882)	372,665 (213,299)	315,042 (154,693)	454,906 (255,330)	332,243 (185,969)	376,337 (222,607)
Parents with completed HS	0.70	0.71	0.67	0.47	1	0.69	0.68
Parents with University	0.41	0.44	0.38	0	1	0.39	0.40
N	3, 928	1,822	2,106	2,331	1,597	337	1,833

Table A1: Descriptive statistics for cohorts not affected by the reform (1986–1988). Low Grades have grades below the 25th percentile of their cohort middle school GPA distribution, High Grades are above.

	Crowded-out Compliers		Pulled-in Compliers	
First Stage	0.146***		0.183***	
	(0.024)		(0.023)	
	Reduced Form	Treatment Effect	Reduced Form	Treatment Effect
More than 3	0.052**	0.358**	0.012	0.064
Days Absent[a]	(0.023)	(0.171)	(0.020)	(0.110)
More than 6	0.048	0.329	0.016	0.089
Days Absent[a]	(0.031)	(0.218)	(0.025)	(0.136)
Elite Uni	0.014	0.095	-0.005	-0.025
Enrolment	(0.017)	(0.119)	(0.017)	(0.091)
Elite Uni	0.013	0.086	-0.008	-0.042
Completion	(0.014)	(0.099)	(0.016)	(0.085)
STEM Uni	-0.022	-0.153	-0.006	-0.033
Enrolment	(0.018)	(0.123)	(0.018)	(0.100)
STEM Uni	-0.005	-0.034	-0.013	-0.072
Completion	(0.017)	(0.115)	(0.014)	(0.079)
N	7,724			

[a]Average days of absence are observed only for students who completed high school, hence these results are conditional on high school completion.

Table A2: Reduced form and treatment effects for CC and CP, additional outcomes

Notes : Reduced form and treatment effect estimates for attending a selective school on educational outcomes, separately for CC and CP. Three days of absence are approximately the 25th percentile of the distribution of days of absence, while six days of absence are the 50th percentile. "Elite Uni" refers to attending either one of two prestigious institutions (NHH and NTNU) or a medicine degree, which have on average higher returns to completion (see Section 4.1). "STEM" refers to attending any course in a STEM field. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1) $E[X]$	(2) Crowded-Out Compliers	(3) Pulled-In Compliers
High SES	0.41	0.90	1.13
Men	0.46	1.62	0.61

Table A3: Relative likelihood of being from high-SES background and of being male, for CC and CP.

Notes : To compute the likelihood ratio for each binary variable X we follow Angrist and Pischke (2008) who show that, for example, $P(X = 1|CC)/P(X = 1)$ corresponds to
$(E[D|Z = (1, 0), X = 1] - E[D|Z = (0, 0), X = 1])/(E[D|Z = (1, 0)] - E[D|Z = (0, 0)])$, which we compute based on the estimated first stages. Column (1) shows the average of each characteristic in the population.

	Crowded-out Compliers				Pulled-in Compliers			
	(a) Men		(b) Women		(a) Men		(b) Women	
First Stage	0.236***		0.049		0.111***		0.246***	
	(0.031)		(0.035)		(0.032)		(0.032)	
	R.F.	Treat.	R.F.	Treat.	R.F.	Treat.	R.F.	Treat.
School	-0.024	-0.103	-0.053**	-1.079	0.034*	0.306	-0.025	-0.099
Completion	(0.025)	(0.108)	(0.024)	(0.912)	(0.019)	(0.193)	(0.017)	(0.072)
Avg Days of	0.661	3.043	0.679	19.030	0.657	4.734	0.399	1.582
Absence[a]	(0.553)	(2.591)	(0.519)	(24.959)	(0.437)	(3.358)	(0.399)	(1.595)
University	-0.023	-0.098	-0.035	-0.707	-0.002	-0.016	-0.054**	-0.218**
Enrolment	(0.028)	(0.118)	(0.028)	(0.755)	(0.027)	(0.241)	(0.022)	(0.094)
University	-0.027	-0.115	-0.067**	-1.354	0.016	0.144	-0.038*	-0.152*
Completion	(0.034)	(0.143)	(0.029)	(1.126)	(0.030)	(0.276)	(0.021)	(0.086)
N	3,586		4,138		3,586		4,138	

[a]Average days of absence are observed only for students who completed high school, hence these results are conditional on high school completion.

Table A4: Heterogeneity in the effect for CC and CP on the basis of students' gender

Notes : Results for panels (a) ("Men") are estimated in a sample which includes only male students, while results for panels (b) ("Women") are estimated using the sample of female students. "R.F." columns show Reduced Form results, while "Treat."columns report treatment effects. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Crowded-out Compliers		Pulled-in Compliers	
First Stage	0.118***		0.121***	
	(0.025)		(0.022)	
	Reduced Form	Treatment Effect	Reduced Form	Treatment Effect
School Completion	-0.039*	-0.334*	0.000	0.002
	(0.022)	(0.199)	(0.012)	(0.101)
Avg Days of Absence[a]	0.693*	7.869	0.523*	4.010*
	(0.419)	(5.359)	(0.275)	(2.229)
University Enrolment	-0.030	-0.251	-0.024	-0.197
	(0.023)	(0.202)	(0.016)	(0.133)
University Completion	-0.066**	-0.559**	-0.007	-0.060
	(0.026)	(0.251)	(0.018)	(0.149)
N	8,003			

[a]Average days of absence are observed only for students who completed high school, hence these results are conditional on high school completion.

Table A5: Reduced Form and Treatment Effects estimates of attending a competitive school, defining the relevant cutoff using schools within 10 kilometers instead of eight kilometers.

Notes : This table resembles Table 3, but changes the definition of the relevant cutoff for admission at competitive schools for some students. Note that the number of observations used for this analysis is different with respect to the main analysis. This is because, as mentioned in Section 4.1, in our main sample we exclude students whose middle school is more than eight kilometres away from one of the 16 academic high schools, while in this sample we exclude students whose middle school is more than *10* kilometres away.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

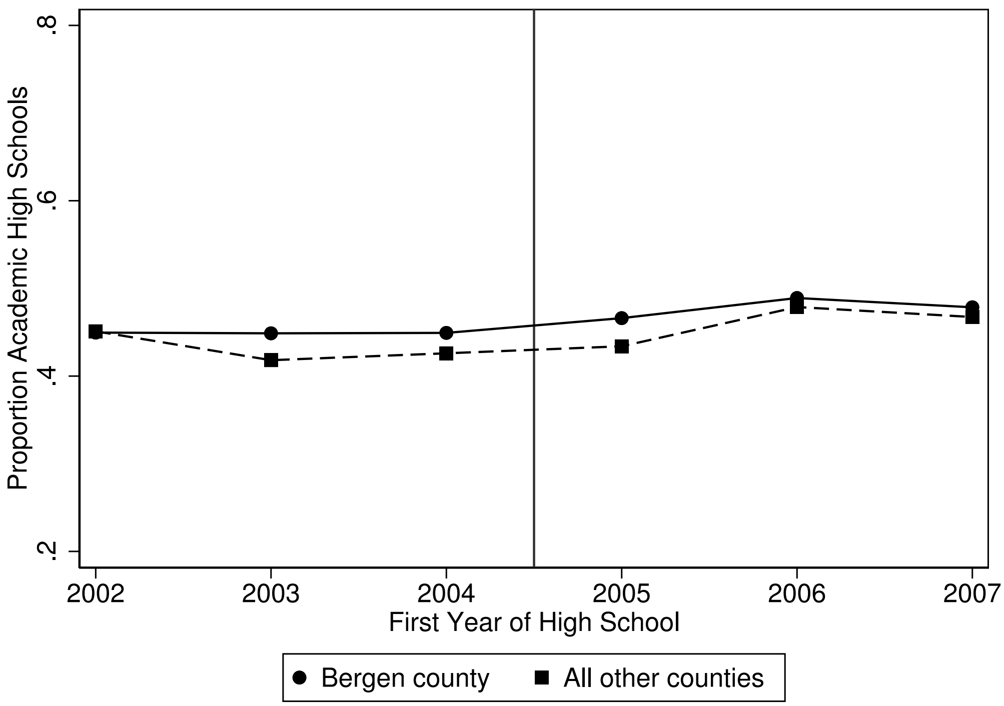


Figure A1: Proportion of students enrolling in academic high school, Bergen vs. all other Norwegian counties.

Notes : The figure shows the proportion of students enrolling in academic high school over the total of students graduating from middle school in every year. “Bergen county” includes all students graduating in the Hordaland county (where Bergen is located). “All other counties” includes students graduating in every county, apart for Hordaland.

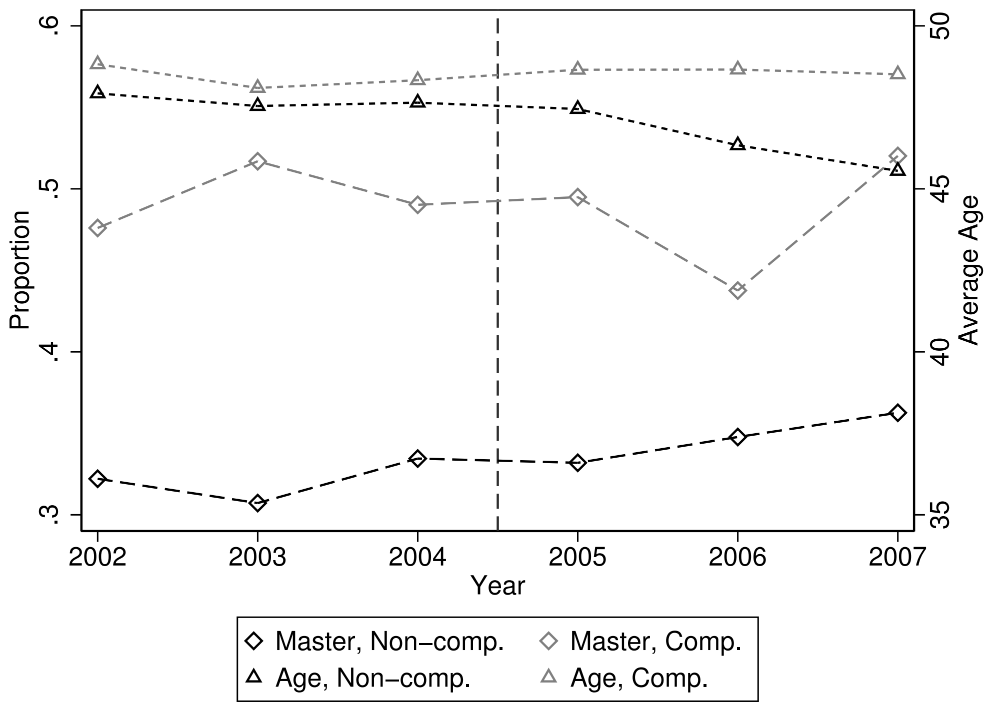


Figure A2: Teachers' average characteristics in every year, competitive and noncompetitive schools.
Notes : The figure shows in every year the average characteristics of instructors teaching in competitive and noncompetitive schools. The characteristics shown are the proportion of teachers with a master degree and the average age of teachers.