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

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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. 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.

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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. Yet when evaluating the cost-benefit of changing the allocation rule for the existing program, one must need 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 school capacity is fixed, 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 schools have a limited number of slots.[2] We show that, when school capacity is fixed, the policy-relevant parameters to evaluate the marginal benefit of such an expansion are the average treatment effects for students with high and low middle school grades separately. In other words, if there is a fixed number of students admitted to each school, we need to be able to separate the effects of attending one of these oversubscribed (or "competitive") schools for students who are targeted by the expansion from

[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.

the effects on 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 priori to believe that the treatment effects for high- and low-grade students are equal.[3]

In order to separate these two 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 focus on marginal benefits allows us to compare different allocation systems by exploiting the estimated parameters. 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). Different from them, we take into account that school capacity is fixed. This implies that an increase

[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.

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 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

[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).

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 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 Section 6 offers conclusions.

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

In this section, our theoretical analysis clarifies 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. Students who have not received an offer can still enroll in a competitive school, by exerting additional effort at a cost. Students who received an offer from a competitive school may still choose to enroll in a noncompetitive school.

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. We now 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). These allow us to link causal estimates of policy effects to normative evaluation of the policies and, for our case, to meaningfully compare different allocation systems. 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.

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

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

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

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

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

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

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.

3 Potential Outcomes, Crowding Out, and Identification

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

[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.

in the crowded-out group. This poses challenges to identification. In this section, we show how we exploit a high school admission reform and the associated new allocation 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 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 the students 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 massively 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

[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

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

[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

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, given the national legal school starting age in Norway, the reform implies that only students born after December 31, 1988, are affected by the reform. For the reform to be exogenous, students born in the last months of 1988 must be similar to those born in early 1989. We perform balance tests of students' characteristics around the birth threshold and find no systematic difference in any of these characteristics (Figure A1). Nevertheless, to ensure that the effect of the reform is not contaminated by seasonal variation in cohort quality, in our empirical analysis we will account for any unobserved seasonal variation in cohort quality by including week-of-birth fixed effects. In addition, the reform creates admission cutoffs for the competitive schools, where only students above a certain cutoff in terms of middle school GPA could access these schools. Below we show how we combine the cutoff with exogenous variations provided by the reform to identify the parameters of interest.

accordingly in the figures.

3.2 Identification

In order to separately identify the effect of attending a competitive school for different types of compliers, we rely on a multi-valued instrument vector, Z. For notational simplicity we suppress the individual index and covariates, although our identifying assumptions can be understood to hold conditional on the appropriate set of covariates. The instrument vector Z consists of two binary instruments, $Z = (Z_1, Z_2)$, where $Z_1 \in \{0, 1\}$ and $Z_2 \in \{0, 1\}$. 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, $Z_1 = 1$ if the student is subject to the merit-based allocation and his/her GPA is below the admission cutoff for competitive schools, whereas $Z_2 = 1$ if the student is subject to the merit-based allocation but his/her GPA is above the admission cutoff. Our instrument vector can take three possible values:

- $(0, 0) \rightarrow$ Prereform
- $(1, 0) \rightarrow$ Postreform, grades below the admission cutoff
- $(0, 1) \rightarrow$ Postreform, grades above the admission cutoff

Define y^0 as the potential outcome if an individual student goes to a noncompetitive school, and y^1 to be the potential outcome if the same individual student goes to a competitive school. Define d^z as the potential schooling choice for a student given the instrument vector z.[10] For any particular individual, we do not observe potential schooling choice d^z. Instead, the realized choice d is observed:

$$d = d^{0,0} + (d^{1,0} - d^{0,0})\mathbf{1}(Z = (1, 0)) + (d^{0,1} - d^{0,0})\mathbf{1}(Z = (0, 1)) \tag{4}$$

In the same fashion, 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:

$$y = y^0 + (y^1 - y^0)d \tag{5}$$

Note that 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,

[10] We use small letters to refer to realizations of random variables.

or $y^1 - y^0$.

We make four identifying assumptions. In the first two, we assume that the instrument vector satisfies the random assignment and the exclusion restriction conditions:

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

Assumption 2. *Exclusion Restriction:* $y^d|Z = y^d$

Note that, for both of the above assumptions to hold, we need to condition on an appropriate set of covariates. For instance, one covariate we will 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.

The remaining two identifying assumptions are monotonicity assumptions restricting potential choice patterns.

Assumption 3. *Monotonicity at the Cutoff:* $d^{0,1} \geq d^{1,0}$,

Assumption 4. *Conditional Monotonicity of the Reform:* $d^{0,1} \geq d^{0,0}$, $d^{1,0} \leq d^{0,0}$

Assumption 3 implies that, after the reform, a person who attends a selective school when below the cutoff must also attend a selective school when above the cutoff. Assumption 4 assumes conditional monotonicity of the reform. This assumption rules out the possibility of students with grades above the cutoff, who would have attended a selective 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.[11] 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.[12]

[11]In practice, we exclude the possibility 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. This is reasonable in a context where the most selective high schools are those with the best reputation. 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).

[12]This would not be the case if, for example, a student who would have attended a competitive school pre-reform decides

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 (see Table 1), leaving us with four compliance types: Never Takers (NT), Crowded-out Compliers (CC), Pulled-in Compliers (CP) and Always Takers (AT). First, note that, without exploiting the additional variation from the admission cutoff, we would not be able to identify the treatment effect for CC and CP separately. From Table 1, it is easy to see that the binary indicator of the reform itself ($\tilde{Z} = \mathbf{1}(Z = \{(1,0),(0,1)\})$) is not a valid instrument because we cannot assume monotonicity: CC act as defiers for the reform instrument. In the presence of defiers, we are only able to identify a weighted average of the effect for CC and CP (Imbens and Angrist, 1994).

Second, 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 postreform 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 $Z = (0,0)$ or $Z = (1,0)$. 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)–(4), the effect of being excluded from competitive schools for CC (as defined in Table 1) corresponds to*

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

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

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

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 spreaded 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.

Proof. See Appendix C. $\square$

Equations (6) and (7) correspond to the convenient Wald estimator in the IV literature with binary endogenous variables and binary instruments.[13] 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.

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

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

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. We include students attending any of the 16 public academic high schools in Bergen and the three closest municipalities.[14] We exclude a small number of students who attended a middle school that was more than eight kilometers away from any of the 16 high schools.[15] 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 4). We define these five schools as competitive, and attending one of these five schools (four schools in 2007) is the treatment D we consider. Postreform 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.[16]

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.[17]

[14]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 viceversa was not a consequence of the reform.

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

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

[17]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 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

A list of descriptives for the prereform 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 equals $\approx$ 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.

4.2 Empirical Model and Estimation

Proposition 1 implies that we can 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 $Z = (0, 0)$ or $Z = (1, 0)$ 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 $Z = (0, 0)$ or $Z = (0, 1)$. Despite our general identification argument does not include any covariates, we condition on trends in date of birth and GPA to ensure that instrument 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:

$$y = \alpha_k + \beta_k d + f_k(t) + g_k(GPA) + \gamma_{\mathbf{k}} \mathbf{w} + \varepsilon \qquad k \in \{CC, CP\} \tag{11}$$

where the first stage is given by

$$d = \alpha_k^{fs} + \delta_k z_k + f_k^{fs}(t) + g_k^{fs}(GPA) + \gamma_{\mathbf{k}}^{\mathbf{fs}} \mathbf{w} + u \qquad k \in \{CC, CP\} \tag{12}$$

other university).

When we estimate the parameters for CC, we use the population of students with either $Z = (0, 0)$ or $Z = (1, 0)$ and define the binary instrument $Z_{CC} = \mathbf{1}[Z = (1, 0)]$. For CP, we use students with either $Z = (0, 0)$ or $Z = (0, 1)$ and instrument $Z_{CP} = \mathbf{1}[Z = (0, 1)]$. The estimate of the parameter β_{CC} corresponds to the effect of attending competitive schools for crowded-out students, while the estimate of β_{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 $-\beta_{CC}$ (see Proposition 1).

We control for time trends 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 define students affected by the reform as those born after December 31, 1988. We normalize the first week in 1989 to zero, and we further allow trends to differ before and after the reform. This resembles a fuzzy regression discontinuity framework, which estimates the causal effect for students at the discontinuity (see e.g. Clark and Royer, 2013). We control for dummies for the 52 weeks in the year ($\mathbf{w}$) to control for seasonal effects.

Finally, we need to control for the individuals' GPA, as instrument Z is correlated with middle school GPA, which in turn may have a direct effect on school choice D and outcome Y (see Section 3.2). GPA is normalized in every year to have a standard deviation equal to 1. In addition, when we estimate the effect for CC (CP), we normalize the GPA to be 0 at the mean of the GPA for students who are below (above) the threshold in the postreform years. This allows us to interpret the constant as the mean value of the outcome for students who, in the prereform years, have a GPA corresponding to the average below- (above-) cutoff GPA after the reform. We test for alternative functional forms for $f(\cdot)$ and $g(\cdot)$ in Section 5.3.

5 Results

5.1 Estimated Treatment Effects

In Table 2 we report the 2SLS estimates for crowded-out and pulled-in compliers. In the first row we report the estimate of the first-stage parameter on the excluded variable Z_k. As is clear from Proposition 1, this is the estimate of the proportion of CC and CP. CC account for about 16% of the population, while CP account for about 19%, 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 effect of being crowded out of a competitive school for CC ($-\hat{\beta}_{CC}$) and the effect of being pulled into a competitive school for CP ($\hat{\beta}_{CP}$). Overall, the reform has a negative impact on school outcomes for CC. The high school completion rate falls by almost 20 percentage points. As a reference to evaluate the magnitude of the effect, in square brackets we report the constant term of the second-stage regression, which corresponds to the average prereform outcome for the average below (above) cutoff student in terms of middle school GPA (see Section 4.2). Hence the fall in the completion rate is around 25%. Average yearly days of absence also increase by about five days, which is quite high in relative terms (the constant is slightly more than 8.5). In Table A2, we show that this is mostly because of an increase in the number of days of absence for students who, before the reform, would have had few such days: the increase in the proportion having more than three days of absence increases more than the proportion having six days of absence. University enrollment also shows a decline, but the estimate (12 percentage points, about 14% of the constant term) is not statistically significant. University completion, instead, shows a sharp decline. This is not conditional on having previously enrolled at university, hence it is the combination of two margins, enrollment and completion upon enrollment. A decline by 27 percentage points is about 40% of the constant, hence a significant decline. In Table A2, we also look at changes in the type of university enrollment, but the estimates are quite imprecise and we find no clear effect on attending prestigious institutions or STEM courses.

For CP, on the contrary, the estimated effect is much smaller and not statistically significant. 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. 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.

We then consider the heterogeneity of the effect on the basis of observed characteristics for CC (Table 3) and CP (Table 4). The ratio between these "conditional" first stages and the overall first stage is informative regarding the relative likelihood of the number of compliers and defiers falling in each observationally different subgroup (Angrist and Pischke (2008), where the values are 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 women are very imprecise. The negative effect for CC is driven by the pool of low-SES students. CP, instead, are much more likely to be women and high-SES students. Interestingly, the effect of attending a competitive school for CP women appears to be negative on all the margins, in particular on university enrollment, where the decline is around 20%. As mentioned, in the literature there is no consensus on whether attending more competitive schools has a positive or negative effect. 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 Policy Analysis

We conduct policy analysis by linking the net marginal benefit of a policy change to the point estimates that we have obtained, as described in Section 3.3. We convert the effects we estimate on the different education margins into the effect of the change in the allocation rule on a unique measure of years of education as described in Appendix E. We then draw upon causal estimates of education returns in terms of lifetime earnings from Bhuller, Mogstad, and Salvanes (2017) to convert this summary measure into lifetime earnings.

Table 5 shows the effect of differently targeted marginal expansions. From the merit-based system introduced in 2005, CC lose on average around 450,000 NOK (75,000 USD) over their lifetime. The reform also has a small negative effect on CP, such that they lose around 140,000 NOK (23,000 USD). About 61% of the students have grades above the cutoff, so following equation (8), a marginal expansion toward students above the cutoffs corresponds to a decrease in the expected value of population lifetime earnings of around 70,000 NOK. 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 37,000 NOK.

Alternative targeting can be based on a combination of observed characteristics and ability of the student. 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 would have a very small and negative impact, partly because of the small effect the expansion has on low-SES CP. This is possibly because low SES in this context, when considering students enrolling in academic high schools, may be not as disadvantaged as in other contexts (such as the one analyzed by 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 low-SES compliers (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 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.

5.3 Robustness Checks

GPA and Time Functional Forms In Table 6, we show the estimate of the parameter $(-)\beta_k$ from equation 11 (the effect of (not) attending a more competitive high school) under different trends in GPA (quadratic and cubic) 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 estimates.

Middle School GPA Distribution One concern with our identification strategy is that the reform may affect middle school GPA directly, either because students choose a different middle school to boost grades, or because they 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, we check that the overall distribution of middle school GPA does not shift after the reform relative to the national distribution. If more effort is exerted in Bergen postreform, the distribution should shift to the right, but the Kolmogorov–Smirnov test finds no difference (Panel (a) in Figure 5). We also test whether the equality in the distribution pre- and postreform is hiding compositional differences, so that in fact pre- and postreform individuals with the same middle school GPA are different. For example, it is possible that while the proportion of individuals with a specific GPA level remains the same pre- and postreform, this is composed of observationally different individuals. We thus consider the distribution of middle school GPA net of the effect of some characteristics, and check whether the distributions pre- and postreform are equivalent. In particular, we regress the standardized middle school GPA on household earnings, education of parents, gender and seasonal dummies (without time effect) and compare the distribution of residuals for cohorts before and after the reform. A formal test on the distributions finds no differences (panel (b) in Figure 5).

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 and implies that we would be mixing the effect on compliers with the peer effect 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.

To test for the effect of the reform on AT and NT, we consider a subpopulation for which the reform is unlikely to affect the treatment and we investigate whether being affected by the reform has any significant effect on their final outcome. 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 two kilometers of a competitive high school (and thus are likely to be within the catchment area of a competitive high school) are likely 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 kilometers 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 (hence after the reform took place), a polynomial time trend, allowed to change before and after the reform, and seasonal dummies. Column (1) of Table 7 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 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 smaller than the effects we find for crowded-out compliers and not statistically significant (partly also because of the small sample size). More importantly, they do not show any clear pattern: for example, in both subgroups the estimate on the coefficient for enrollment at university has the opposite sign to the coefficient for university completion, suggesting no clear effects for either group.

6 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 four 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. 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). These results also 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. A policy maker whose objective function weighs more heavily the utility of high-ability students may still find it optimal to implement such a reform, but this result shows that it does not lead to a Pareto improvement.

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	$Z=(0,0)$	$Z=(1,0)$	$Z=(0,1)$
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 3)	D=0	D=1	D=0
Dropped (Assumption 4)	D=0	D=1	D=1
Dropped (Assumption 4)	D=1	D=0	D=0
Dropped (Assumption 3)	D=1	D=1	D=0

Table 1: Compliance types identified by the multivalued instrument Z given assumptions (1)–(4)

	(1) Crowded-Out Compliers $-\hat{\beta}_{CC}$	(2) Pulled-In Compliers $\hat{\beta}_{CP}$
First stage	0.156***	0.194***
	(0.024)	(0.023)
School Completion	-0.193*	0.036
	(0.111)	(0.067)
	[0.810***]	[0.970***]
Avg Days of Absence[a]	5.031*	2.341
	(2.771)	(1.469)
	[8.634***]	[6.370***]
University Enrollment	-0.119	-0.125
	(0.120)	(0.088)
	[0.828***]	[0.960***]
University Completion	-0.270*	-0.028
	(0.153)	(0.093)
	[0.689***]	[0.852***]
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 2: 2SLS estimate of the effect of attending a competitive school, separately for CC and CP.
Notes : Each parameter estimate comes from a separate regression. 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. The constant of each regression is reported in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1) Low SES $-\hat{\beta}_{CC}$	(2) High SES $-\hat{\beta}_{CC}$	(3) Men $-\hat{\beta}_{CC}$	(4) Women $-\hat{\beta}_{CC}$
First stage	0.156***	0.128***	0.245***	0.057
	(0.029)	(0.045)	(0.030)	(0.036)
School Completion	-0.262*	-0.063	-0.053	-0.746
	(0.156)	(0.211)	(0.103)	(0.592)
	[0.768***]	[0.884***]	[0.877***]	[0.644***]
Avg days of Absence	5.127	6.148	2.856	18.110
	(3.377)	(6.502)	(2.308)	(25.337)
	[7.797***]	[9.970***]	[8.069***]	[11.525**]
University Enrollment	-0.190	0.079	-0.037	-0.445
	(0.181)	(0.259)	(0.108)	(0.545)
	[0.807***]	[0.891***]	[0.901***]	[0.695***]
University Completion	-0.355*	-0.107	-0.051	-1.203
	(0.200)	(0.294)	(0.133)	(0.881)
	[0.652***]	[0.757***]	[0.782***]	[0.422*]
N	3,212	1,966	2,474	2,704

Table 3: Heterogeneity for 2SLS estimate of the effect of attending a competitive school for **CC**.
Notes : "Low SES" are all students whose parents have no university education, "High SES" are students whose parents (both) completed university. Standard errors clustered at the week-of-birth level. The constant of each regression is reported in square brackets. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	(1) Low SES $\hat{\beta}_{CP}$	(2) High SES $\hat{\beta}_{CP}$	(3) Men $\hat{\beta}_{CP}$	(4) Women $\hat{\beta}_{CP}$
First stage	0.172***	0.217***	0.116***	0.258***
	(0.032)	(0.033)	(0.033)	(0.033)
School Completion	0.097	-0.015	0.325*	-0.096
	(0.111)	(0.080)	(0.182)	(0.068)
	[0.953***]	[0.989***]	[0.902***]	[1.000***]
Avg days of Absence	2.679	1.957	3.493	1.642
	(2.160)	(1.791)	(3.049)	(1.516)
	[6.655***]	[6.140***]	[5.789***]	[7.069***]
University Enrollment	-0.042	-0.180	0.013	-0.198**
	(0.152)	(0.110)	(0.229)	(0.087)
	[0.922***]	[0.993***]	[0.948***]	[0.965***]
University Completion	0.005	-0.025	0.202	-0.123
	(0.148)	(0.104)	(0.264)	(0.077)
	[0.836***]	[0.850***]	[0.775***]	[0.898***]
N	3,445	2,663	2,792	3,316

Table 4: Heterogeneity for 2SLS estimate of the effect of attending a competitive school for **CP**.

Notes : "Low SES" are all students whose parents have no university education, "High SES" are students whose parents (both) completed university. Standard errors clustered at the week-of-birth level. The constant of each regression is reported in square brackets. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Crowded out group			
Pulled in group	High Ability		Low Ability	
High Ability	-		-68,277	
Low Ability	36,760		-	
	Low SES	High SES	Low SES	High SES
High Ability, Low SES	-	15,199	-34,113	-4,729
High Ability, High SES	-16,270	-	-52,787	-21,332
Low Ability, Low SES	25,314	36,593	-	21,805
Low Ability, High SES	1,280	5,393	-7,952	-

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 E describes how we translate the effect on different margins of education into lifetime income. The different effects are computed following equation (10), based on the estimated effects from Tables 2, 3 and 4.

	(1)	(2)	(3)	(4)	(5)
	Baseline	Grades Quadratic	Grades Cubic	Week Quadratic	Months
			(a) Crowded-Out Compliers		
First stage	0.156***	0.160***	0.157***	0.151***	0.153***
	(0.024)	(0.024)	(0.024)	(0.024)	(0.025)
School Completion	-0.193*	-0.169	-0.110	-0.207*	-0.197
	(0.111)	(0.107)	(0.107)	(0.115)	(0.129)
Avg days of Absence	5.031*	5.168*	4.864*	5.572*	5.135*
	(2.771)	(2.646)	(2.716)	(2.876)	(2.689)
University Enrollment	-0.119	-0.096	-0.036	-0.128	-0.110
	(0.120)	(0.115)	(0.116)	(0.123)	(0.133)
University Completion	-0.270*	-0.250*	-0.188	-0.300*	-0.256
	(0.153)	(0.148)	(0.149)	(0.158)	-0.204
			(b) Pulled-In Compliers		
First stage	0.194***	0.202***	0.201***	0.195***	0.193***
	(0.023)	(0.023)	(0.024)	(0.023)	(0.023)
School Completion	0.036	0.008	-0.026	0.033	0.039
	(0.067)	(0.065)	(0.063)	(0.067)	(0.061)
Avg days of Absence	2.341	1.923	2.033	2.356	2.455*
	(1.469)	(1.385)	(1.397)	(1.448)	(1.390)
University Enrollment	-0.125	-0.142*	-0.173**	-0.128	-0.116
	(0.088)	(0.086)	(0.086)	(0.088)	(0.092)
University Completion	-0.028	-0.042	-0.082	-0.033	-0.015
	(0.093)	(0.090)	(0.090)	(0.092)	(0.095)

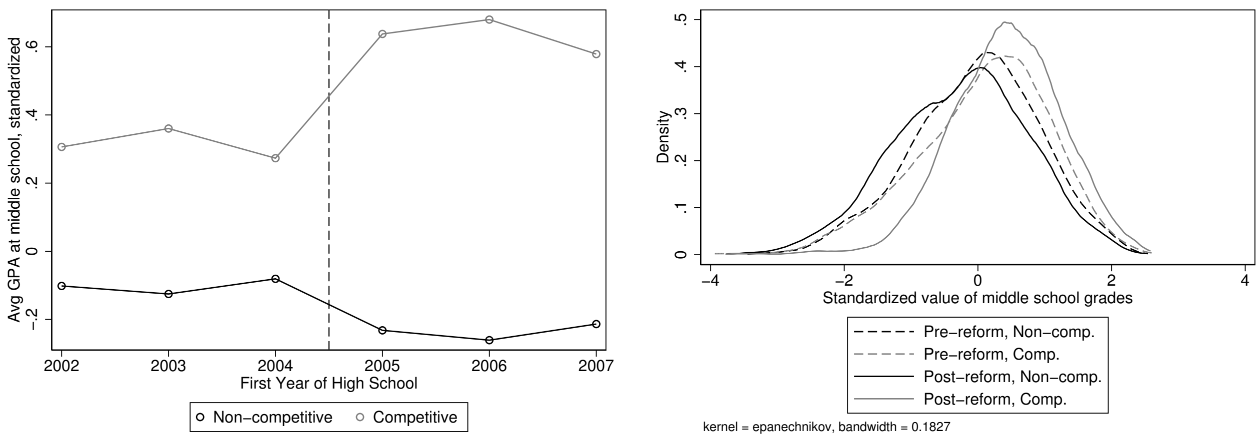
Table 6: Robustness checks for the functional forms of trends used in estimating the main equation.

Notes : The table reports the estimate of the effect of attending a competitive high school using 2SLS. Each column uses a different trend for time and grades. Column (1) shows the baseline estimates. Columns (2) and (3) use different trends in grades: (2) uses a quadratic trend, and (3) a cubic trend. Columns (4) and (5) use different time trends: (4) is a quadratic trend (not allowed to differ before and after the reform), (5) uses a trend in months rather than weeks. Controls always include 52 dummies for week-of-birth within the year, apart from column (5), which uses 12 month-of-birth dummies. Standard errors are always clustered at the week-of-birth level, apart from column (5), which uses the month-of-birth level.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Competitive HS Enrollment	Competitive HS Enrollment	HS Completion	Avg days Absent	Uni Enrollment	Uni Completion
		(a) Top Quartile Middle School GPA				
	Overall	Dist<2km	Dist<2km	Dist<2km	Dist<2km	Dist<2km
Post	0.379***	0.119	-0.0682	1.537	0.0176	-0.0156
	(0.0572)	(0.132)	(0.107)	(1.204)	(0.104)	(0.0456)
Observations	1,941	519	519	460	519	519
		(b) Bottom Decile Middle School GPA				
	Overall	Dist>5km	Dist>5km	Dist>5km	Dist>5km	Dist>5km
Post	-0.247***	0.0460	-0.0558	3.667	-0.275	0.465**
	(0.0659)	(0.0643)	(0.159)	(2.651)	(0.192)	(0.211)
Observations	1,133	579	579	399	579	579

Table 7: 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 of the distribution, while panel (b) shows that 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. In an effort to improve precision in the estimates, the time trend is expressed in terms of months-of-birth rather than weeks-of-birth. The results when using weeks-of-birth are generally slightly larger in magnitude and have larger standard errors. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$



(a) Average GPA

(b) GPA Distribution

Figure 1: Middle school GPA comparison, competitive and noncompetitive high schools.

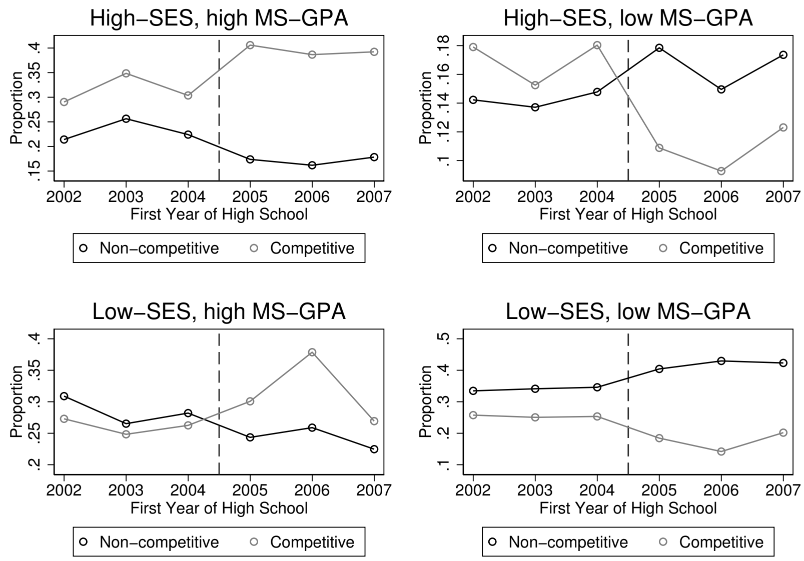


Figure 2: Proportion of students with different middle school GPA (MS-GPA) and different SES attending competitive and noncompetitive high schools.

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).

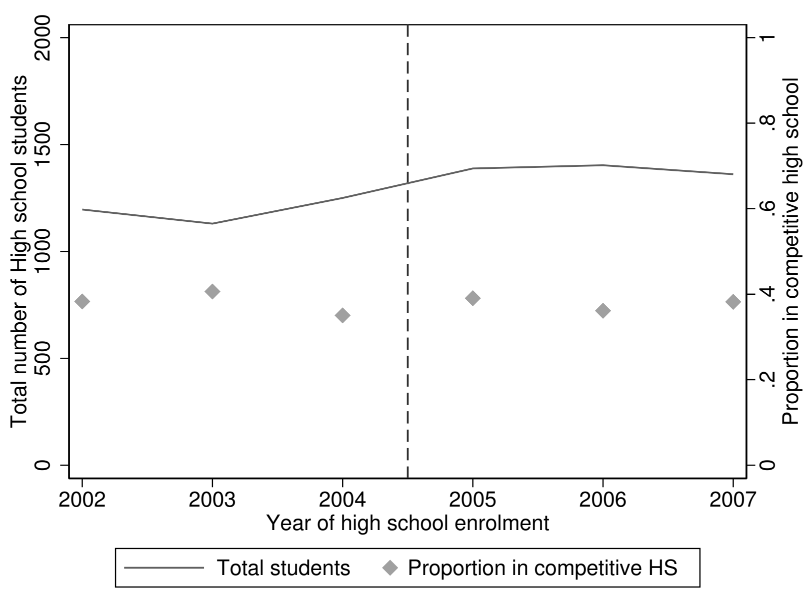
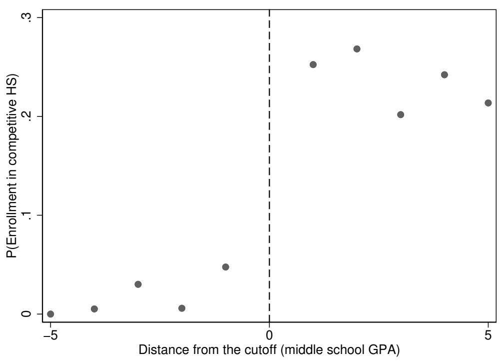
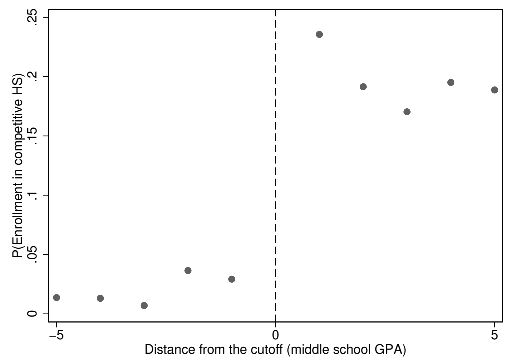


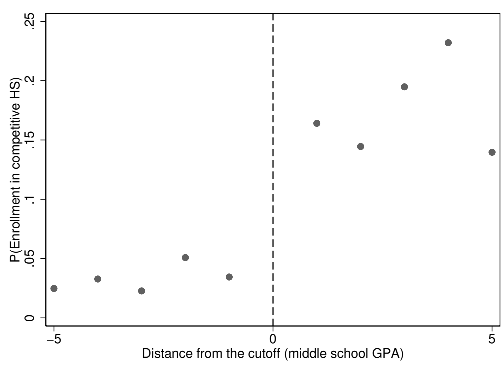
Figure 3: Number of students enrolling in academic HS and, among these, proportion enrolling in competitive HS in every year.



(a) 2005



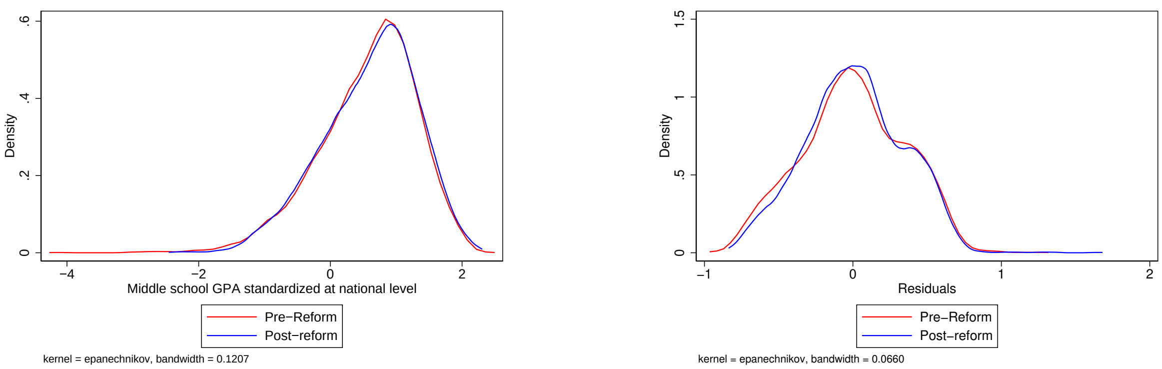
(b) 2006



(c) 2007

Figure 4: Probability of enrolling in a competitive high school around grades cutoff.

Notes : These figures pull together all different cutoffs in each postreform 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) Middle school GPA (national level)

(b) Residuals

Figure 5: Robustness Checks for Middle School GPA Distribution.

Notes : In panel (a) we test whether the distribution of grades for cohorts pre- and postreform 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 and not only within Bergen as in the main analysis when grades are normalized at the local level). The p-value of the Kolmogorov–Smirnov test for the equality of the distributions is 0.238. In panel (b) we plot the estimated distribution of residuals of a regression of middle school GPA (standardized to have mean zero and standard deviation 1 within each Bergen cohort) on individual characteristics, separately for the pre-and postreform cohorts. The p-value of a t-test for the equality of the average pre- and postreform is 0.345. The p-value of the Kolmogorov–Smirnov test for equality of the distributions is 0.142. Cohorts enrolling in high school in 2003 and 2004 are prereform, while those enrolling in 2005 and 2006 are postreform.

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. At the end of middle school, students are evaluated both nonanonymously by their teachers for all subjects taught in school, and in addition anonymously in 1–2 central exit exams. In the period 1999 to 2010 there were 10 teacher-given grades and 1–2 exam grades at middle school. The grade point average (GPA) consists of the mean grade, where both exams and in-school assessments have a grade between 1 and 6. Both oral and written performance is assessed in some subjects, and both oral and written exams are given. If a subject is both oral and written, the mean of these two is used in calculating the final GPA. When an exam is given in a field, it also counts for half of the grade in the GPA.

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.

About 95% of students moving into middle 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.

A.1 Teacher grading and exam grading

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.

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. 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 farther 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.

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. Hence, a fixed "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.

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

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

Because of the exclusion restriction, we know that the outcome for AT who are in the prereform period is the same as the outcome for AT in the postreform period, hence we can rewrite

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

and thus

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

Similarly, we can rewrite the last data moment as

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

and then combining with the second data moment from above

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

which then gives

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

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 postreform, i.e. $P(D=0,Z=(1,0)) = \pi_{NT}+\pi_{CP}+\pi_{CC}$, while π_{AT} is the probability that a student attends a competitive school below the cutoff postreform i.e. $P(D=1, Z=(1,0)) = \pi_{AT}$. Hence,

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

Similarly, it is easy to show that

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

so that

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

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

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

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

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

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 prereform 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 $Z = (0, 0)$ or $Z = (0, 1)$. We observe four data moments that are combinations of the outcomes of different compliance types:

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

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

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

and the first and third moments give us

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

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

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

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

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

we derive

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

D Competitive Schools' Cutoffs

In the data, we do not have information on the cutoff used by oversubscribed schools to admit students in the postreform 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 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 Z, we need to then assign a cutoff for admission at selective schools to each student. 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 postreform 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 prereform period.

We experiment using 10 instead of eight kilometers as the distance measure to assign the cutoff and Table A4 reproduces the results in Table 2 using 10 kilometers. The results are substantially unchanged, but noisier, as one would expect if using 10 kilometers produces a less precise measure of the relevant cutoff than using eight kilometers.

E From Estimated Effects to Lifetime Earnings: Details

We translate the estimated effect on different margins of education into lifetime earnings in the following way. First, we translate changes at the different margins of education into changes in years of schooling. In our data, on average, a student who drops out of high school obtains 1.25 fewer years of school than a student who graduates, hence an increase in high school completion by x percentage points translates to an average increase in years of education by 1.25*(x/100). A student who enrolls in university but does not complete it takes one year fewer than a completing student. If the decrease in university completion exceeds the one in university enrollment, we assign three years of missed schooling to the fraction that did not enroll and one year of missed schooling to the difference between the two. If the decrease in university enrollments exceeds the one in completion, we assign three years of missed schooling to the fraction that did not complete and two years of missed schooling to the difference between the two. When the increase in university enrollment is positive we assign two more years of schooling; when completion is positive we assign one year more.

We then translate the change in years of education into average lifetime income by multiplying it times 12,936*46, where 12,936 NOK is the average per-year causal effect on lifetime earnings of one more year of schooling as computed by Bhuller, Mogstad, and Salvanes (2017) and we assign 46 years as the average working life.

F Tables and Figures

	Overall	Men	Women	Low SES	High SES
		(a) High School Outcomes			
Admission at 5 competitive schools	0.30	0.31	0.29	0.26	0.35
Days of absence during HS	6.61 (4.98)	6.45 (5.22)	6.74 (4.76)	6.62 (4.90)	6.59 (5.08)
More than 3 Days of Absence	0.77	0.73	0.80	0.77	0.76
More than 6 Days of Absence	0.48	0.45	0.50	0.48	0.47
High School completion	0.90	0.86	0.93	0.87	0.93
		(b) University Outcomes			
University Enrolment	0.85	0.81	0.88	0.81	0.90
University Completion	0.81	0.75	0.87	0.77	0.88
Elite Uni Enrolment	0.15	0.17	0.13	0.10	0.22
Elite Uni completion	0.11	0.13	0.09	0.07	0.15
STEM Uni Enrolment	0.16	0.21	0.10	0.14	0.17
STEM Uni Completion	0.10	0.13	0.07	0.09	0.11
		(c) Students' Characteristics			
HH earnings (in 1998 NOK)	375,369 (217,201)	372,481 (214,748)	377,944 (219,389)	318,365 (159,261)	455,804 (258,707)
Parents with completed HS	0.70	0.72	0.67	0.48	1.00
Parents with University	0.41	0.44	0.39	0.00	1.00
N	3,562	1,680	1,882	2,084	1,478

Table A1: Descriptive statistics for cohorts not affected by the reform (1986–1988).

	(1) Crowded-Out Compliers $-\beta_{Def}$	(2) Pulled-In Compliers β_c
First stage	0.156***	0.194***
	(0.024)	(0.023)
More than Three Days of Absence	0.325*	0.070
	(0.181)	(0.097)
	[0.954***]	[0.786***]
More than Six Days of Absence	0.281	0.089
	(0.237)	(0.121)
	[0.606***]	[0.455***]
Elite Uni Enrolment	0.093	-0.038
	(0.115)	(0.088)
	[0.158***]	[0.243***]
Elite Uni Completion	0.089	-0.058
	(0.092)	(0.081)
	[0.115***]	[0.170***]
STEM Uni Enrolment	-0.115	-0.039
	(0.109)	(0.092)
	[0.155***]	[0.161***]
STEM Uni Completion	-0.016	-0.064
	(0.107)	(0.073)
	[0.130***]	[0.119***]
N	5,178	6,108

Table A2: 2SLS estimates of the effect of attending a competitive school, separately for CC and CP, additional outcomes.

Notes : 2SLS estimates of the effect of 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. 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 a linear time trend (allowed to vary between the pre- and the postreform period and normalized to zero for the oldest students affected by the reform) and a linear trend in middle school GPA (normalized to zero for the average GPA of students below (above) the cutoff). We control for seasonal (52 weeks) dummies and cluster the standard errors at the week-of-birth level. The constant of each regression is reported in square brackets. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	(1) $E[X]$	(2) Crowded-Out Compliers	(3) Pulled-In Compliers
High SES	0.41	0.82	1.12
Men	0.46	1.57	0.6

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

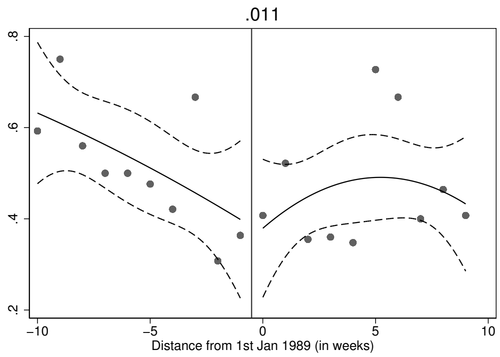
$Notes$: To compute the likelihood ratio for each binary variable X we follow Angrist and Pischke (2008) who show that $P(X=1|CC)/P(X=1)$ corresponds to
$(E[D|Z=(1,0),X=1]-E[D|Z=(0,0),X=1])/(E[D|Z=(1,0)]-E[D|Z=(0,0)])$, which we compute based on the estimated first stages. Column (1) shows the average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

	(1) Crowded-Out Compliers $-\hat{\beta}_{CC}$	(2) Pulled-In Compliers $\hat{\beta}_{CP}$
First stage	0.134***	0.132***
	(0.025)	(0.023)
School Completion	-0.194	0.030
	(0.163)	(0.090)
	[0.808***]	[0.961***]
Avg Days of Absence[a]	6.904	3.334*
	(4.421)	(1.992)
	[9.158***]	[6.073***]
University Enrolment	-0.113	-0.147
	(0.169)	(0.118)
	[0.814***]	[0.971***]
University Completion	-0.395*	-0.005
	(0.211)	(0.132)
	[0.636***]	[0.849***]
N	4,974	6,729

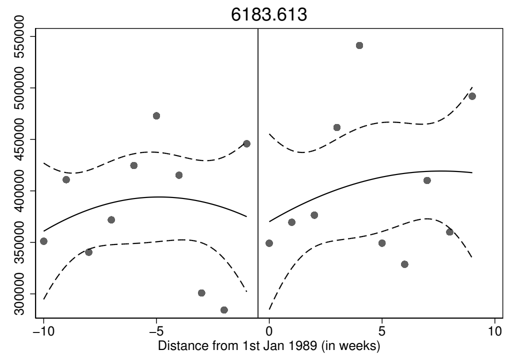
[a]Average days of absence are observed only for students who completed high school, hence these results are conditional on high school completion.

Table A4: 2SLS estimates of the effect of attending a competitive school, defining the relevant cutoff using schools within 10 kilometers instead of eight kilometers.

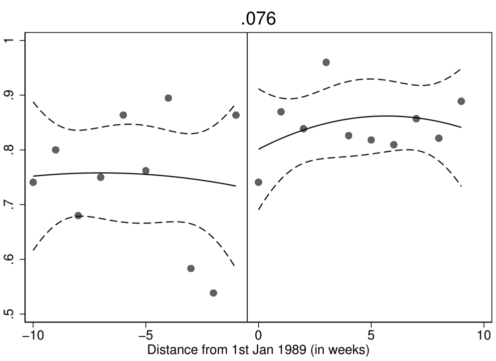
Notes : This table resembles Table 2, but changes the definition of the relevant cutoff for admission at competitive schools for some students. The number of observations used in the analysis for CC and for CP is different with respect to the main analysis because, by changing the definition of neighborhood, we also change the relevant cutoff for some individuals and this affects the sample used in the estimates of parameters for CC and CP.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



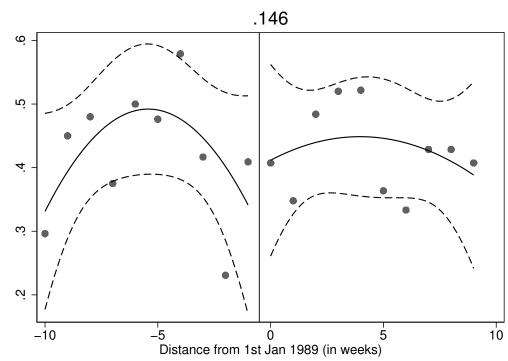
(a) Female



(b) HH Earnings



(c) Parents' HS



(d) Parents' Uni

Figure A1: Covariates balance around the threshold for being affected by the reform.

Notes : Students affected by the reform are those born after January 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.

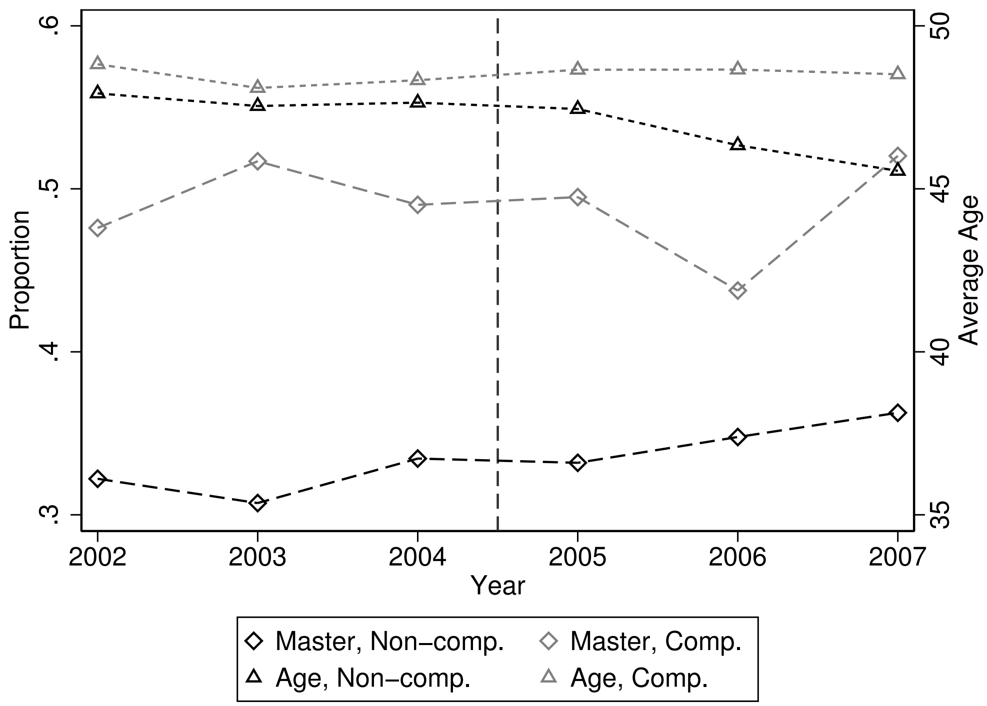


Figure A2: Teachers' average characteristics in every year, competitive and noncompetitive schools.
Notes : The figure shows in every year the average characteristics of instructors teaching in competitive and noncompetitive schools. The characteristics shown are the proportion of teachers with a master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