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1 Introduction

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

One example of public program facing resource constraint is schooling service. Given that schools have capacity constraints and good schools are often in excess demand, policy makers need to impose admission rules to assign students to schools. 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, such a change in the admission rule may produce both 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. For a policy maker who cares about both efficiency and equity implications from changing admission rules, we need to understand the effects of attending a good school both for students who gain access to it and for those who lose access.

In this paper, we develop a framework to analyze the effects of implementing a "merit-based" system on the basis of grades obtained in middle school, when high schools have a limited number of slots.[2] We show that the aggregate effect from expanding access to oversubscribed (or "competitive") schools is a weighted average of the average treatment effects of attending one of these competitive schools for students who are targeted by the expansion *and* for those students who are crowded out. In a world where each student has individual-specific gains from attending a competitive high school, there is no reason a priori to believe that the treatment effects for the targeted and the crowded-out students are equal.[3]

[1] The potential gain in productivity may come from improved match between students and schools or improvement in overall school quality by fostering competition between schools (Hoxby, 2000, 2004). When public resources are not assigned to schools on the basis of their performance (such as the Norwegian setting), the latter mechanism might not be important.

[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.

[3] For instance, results from Walters (2018) suggest that the gains from attending charter schools in Boston are unevenly

In order to separately identify these two treatment effects, we exploit the exogenous variation provided by a reform that took place in the early 2000s in Bergen, Norway. The reform changed the way students are assigned to high schools, moving from a system based on residential location to one where there is free school choice. Yet school capacity is fixed and good schools are oversubscribed. Policy makers thus ration students to schools using a serial dictatorship mechanism, where school choice is offered first to students who obtain higher grades in exit exams from middle school.

The serial dictatorship mechanism generates admission cutoffs based on middle school GPA at competitive schools. We combine the reform with these cutoffs to identify the effect of attending competitive schools for students who gain and those who lose access to these schools as a result of the reform. Effectively, we are able to identify the effect for students who gain (lose) access to competitive schools because, conditional on being above (below) the cutoff, the reform weakly increases (decreases) the probability of enrolling in a competitive school. Although the reform does not satisfy the monotonicity condition for an instrument in the overall population (Imbens and Angrist, 1994), the monotonicity condition may hold for subpopulations stratified by the cutoff and GPA. This allows us to construct estimators using the reform as an instrument in those subpopulations, where the subpopulation-specific IV estimates correspond to the treatment effects either for those who gain or for those who lose access.

We find that considering the crowding out effect of this reform is crucial when evaluating its impact. The effect on high school completion and university education among high-ability students who gain access to competitive schools is zero (if not slightly negative). By comparison, being crowded out of the competitive schools among low-ability students has a large and significant negative impact on university enrolment and completion. To put it differently, those crowded-out students would have benefited the most from enrolling in competitive schools. Overall, our evidence points to a small negative impact of the reform on the outcomes of students in aggregate, with certain group of students losing out much more than the others. Although the reform does not lead to efficiency gains, it levels up access to competitive schools by parental background. The previous residence based allocation allowed easier access to competitive schools for students from high socioeconomic status (SES) families due to residential sorting. Although the reform is meant to allocate students based on merits alone, it also pushes up more students from low SES families into competitive schools and pushes down more students from high SES families to non-competitive schools.

distributed along both observable and unobservable dimensions.

An important caveat in interpreting our results, is that we assume that any change in school characteristics following the reform should have no direct impact on students' outcomes. Such an exclusion restriction would be violated if schools react to the reform by improving their quality (for example because of increased competition, as in, e.g. Hoxby (2003); Wondratschek, Edmark, and Frölich (2013)) or if the change in students' composition had primary impacts on the performance of all students (because of peer effects or because of teachers changing their behavior, as in, e.g. Duflo, Dupas, and Kremer, 2011). We argue that, because of the institutional setting, the reform is not likely to foster school competition and we provide evidence that changes in school resources and students' composition may not be empirically important in our context. Although our evidence can rule out certain type of violation to the exclusion restriction, we acknowledge that we cannot completely exclude that changes in students' composition may have a direct impact on students' outcomes.

This paper is directly related to the literature that evaluates the effect of increasing school choice (e.g, Angrist, Bettinger, Bloom, King, and Kremer (2002); Cullen, Jacob, and Levitt (2006); Lavy (2010); Deming, Hastings, Kane, and Staiger (2014)). With school capacity being fixed, an increase in school choice for some groups must come at the cost of a decrease in the availability of places in the more competitive schools for other groups. We contribute to this literature by estimating the effects of increasing school choice separately for those gaining and losing access to competitive schools. 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. Our goal, instead, 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. We believe that these parameters are informative for policy makers who also care about the equity (or inequality) implications of school allocation mechanisms.[4]

[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

A recent paper by Black, Denning, and Rothstein (2023) also examines the winners and losers of a change in the students' allocation system.[5] 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 no negative effects 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. We further provide a formal potential outcomes framework that enables us to identify treatment effects for different subgroups and demonstrate how we can recover these treatment effects without the need to know or explicitly predict students' counterfactual choice. In a different but related context, Otero, Barahona, and Dobbin (2021) estimate the effects of affirmative action regulation in college admission in Brazil, where an expansion of the affirmative action regulation benefits students who belong to the targeted group at the expense of displaced students outside the targeted group. They estimate a joint model of school choice and potential outcomes, allowing them to go further by conducting counterfactual simulations of alternative affirmative action rules in college admission.

The paper is organized as follows. Section 2 gives details of the school admission reform that we exploit to generate exogenous variation in the school allocation system. Section 3 provides a model to analyze the theoretical implications of the reform. In Section 4 we describe the potential outcome framework, assumptions, and identification of the treatment effects separately for those pulled-into and for those crowded-out from the competitive school. Section 5 presents the data and the empirical models to estimate the parameters of interests. Section 6 presents the results. In Section 7 we extend the analysis to multiple types of schools, allowing heterogeneous effects from attending different competitive schools. In Section 8 we provide evidence to validate the main identifying assumptions. Section 9 concludes.

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.

[5]A recent paper by Riehl (2022) estimates the heterogenous effects of a reform of the Colombian college admission exam and interprets these estimates as the effects for students who were "pulled in" and "pushed out" to selective Colombian universities.

2 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. The middle school GPA is the sum of the final-year grades in 11 different subjects and we provide more details on the grading system in Appendix A.1.

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. This mechanism for allocating students is a serial dictatorship, which generally does not give incentives for strategic manipulation by students (see e.g. Pathak and Sönmez, 2013).[7] We provide additional details on the allocation system both before and after the reform in Appendix B. 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.

The reform was effective in changing student composition among academic high schools. We observe five schools being oversubscribed in the post-reform period.[8] In panel (a) of Figure 1 we show that, as a consequence of residential sorting, even before the reform students attending these oversubscribed

[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] The serial dictatorship is strategy-proof when students can submit an unlimited list of schools they would like to attend. Although after the reform students can submit up to 6 schools they would like to attend, in practice this does not matter since students may not realize that in fact only 4 to 5 schools are oversubscribed. As we evaluate the introduction of a merit-based system, it is even more difficult (if not impossible) to predict where the cutoff will be based on observations from previous years. We thank a referee for pointing this out to us. In addition, schools are spread out geographically, so the set of schools which students usually choose from is not large. Students were required to fill their list in April, before they have full knowledge of their final middle school GPA (in June). These factors make it unlikely for students to strategically select the 6 schools among the 16 available schools.

[8] As described in section 5.1, these oversubscribed schools are what we define as "competitive" schools, and are labelled accordingly in the figures. In 2007, the last year of our sample, one of these five competitive schools is not oversubscribed, hence in 2007 we exclude it from the competitive group.

schools had higher middle school GPA on average (the difference was 0.4 of one standard deviation before the reform). However, this difference increased to 0.8 of one standard deviation after the reform. Panel (b) of Figure 1 shows that such a change is the consequence of a shift toward the right of the distribution of middle school GPA of students attending competitive schools after the reform, while the distribution of middle school GPA of students attending other high schools shifted to the left, indicating an overall reduction of the academic ability of students in these schools. In addition, in Figure A1 we show that the reform was effective in changing the composition also in terms of socioeconomic status (SES, proxied by parental education, see Section 5.1), in that the probability of attending an oversubscribed school changed both for students of high and low SES, depending on their middle school GPA.

In addition to enrolling students with relatively high initial achievements (measured by middle school GPA), before the reform students in oversubscribed schools also had fewer days of absence, were more likely to continue to university and to complete it. Instructors in these schools were more likely to hold a master's degree (Figure A2), suggesting better selection of teachers and an estimate of school value added shows that among the four schools with the highest value added before the reform, three are going to be oversubscribed post-reform (Figure A3).[9] 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.

One potential concern is that the reform may affect the choice of high school tracking, for instance by moving students from academic to vocational tracks. In Figure A4 we plot the trends in the proportion of students enrolling in academic high school in Hordaland (Bergen's county) and in the rest of the country both overall and separating students with different parental background, finding no jumps around the reform year. In addition, Figure A5 shows that the numbers of students attending academic high schools are relatively stable in the six years around the reform and the proportion enrolling in one of these five oversubscribed schools remains fixed at around 40% in each year, providing evidence for capacity constraints at the competitive schools at least in the short run.

The reform provides us with exogenous variations to the admission allocation rule across different cohorts. In particular, the reform implies that students who turn 16 during the calendar year 2005 are

[9]School value added is estimated from a regression of final year high school test scores on school fixed effects and students' middle-school GPA (the school fixed effects identify VA), see Section 6.4 for further details.

assigned to high school under the new admission rule. Given the legal school starting age in Norway, these are students born after December 31, 1988.

3 Theoretical Framework

In this section, we provide a model to analyze the theoretical implications of the reform. Our model draws from Abdulkadıroğlu, Angrist, Narita, and Pathak (2022) and Otero, Barahona, and Dobbin (2021), where there is a centralized admission platform allocating a set of individuals indexed by $i \in \{1, \ldots, n\}$ to a finite set of schools $j \in \mathcal{J} = \{1, \ldots, J\}$.

Students differ in terms of their preferences, academic merits and residential location. Denote a student i's type by $\theta_i = (\succ_i, s_i, r_i)$, where $\succ_i$ is the preference over different schools, s_i is the academic merit (middle-school GPA) and r_i indicates the residential location. We define r_i as a discrete variable, where $r_i = j$ if the student resides in the "catchment" area for school j. The set of all student types is defined by $\Theta = \cup_i(\theta_i)$.

Schools have capacity constraints, represented by a strictly positive capacity vector, $q = \{q_1, \ldots, q_J\}$, where $q_j \in (0, 1]$ is defined as the proportion of the population that can be seated at school j. The central admission platform determines the share of school places (ω) that is reserved for those living within the catchment area. The Bergen school admission reform is represented by a large shift in the value of ω. Prior to the reform ($Z = 0$), $\omega = 1$, meaning that all spaces are reserved for students living within the catchment area. Each school j prioritizes students' living in its catchment area (those with $r_i = j$) and we assume that they fill up the school's places up to its capacity. After the reform ($Z = 1$), $\omega = 0$, implying that school places are open to all students regardless of their locations. We assume that $q_J = 1$, meaning that there is at least one school that will never be oversubscribed (equivalent to an outside option). The centralized admission platform then operates a merit-based scheme to allocate all students to schools.

The centralized admission platform uses a serial dictatorship algorithm to determine assignment of students (Θ) to schools ($\mathcal{J}$), given Θ, ω, and q. The single tie-breaker is the middle-school GPA. We are interested in evaluating the impact of changing the admission scheme from $Z = 0$ to $Z = 1$, or, equivalently, a shift from $\omega = 1$ to $\omega = 0$, while holding student characteristics (Θ) and school capacity (q) fixed. In particular, the change of admission scheme implies a *non-monotonic* change in the admission cutoffs for oversubscribed schools after the reform. Let c^Z_{rj} be the market-clearing admission cutoff for an oversubscribed school j and students living in area r under the admission scheme Z. When school allocation is based on students' locations, the implied admission cutoffs for students in the catchment area are always lower than for those outside the catchment areas, i.e., $c^0_{jj} \ll c^0_{kj}, \forall k \neq j$.[10] By switching to a merit-based allocation scheme, the reform implies that (i) $c^1_{rj} = c_j, \forall r$, meaning that there is a common admission cutoff regardless of students' location, and (ii) $c^0_{jj} < c_j < c^0_{kj}, \forall k \neq j$, meaning an effective increase in the admission cutoff after the reform for those students living inside the catchment area and a decrease in the admission cutoff for those living outside it.[11]

Aggregate impact of the reform. Our theoretical model implies that the aggregate impact of the reform depends on the effects for students who are crowded out from a particular school and those who are pulled into a school. As an illustration, suppose there are two schools, one being competitive ($j = 1$) and the other being non-competitive ($j = 2$).[12] Under the merit-based admission scheme, assume that the school $j = 1$ (the competitive school) is oversubscribed, generating a market-clearing admission cutoff c_1. Under the merit-based allocation ($Z = 1$), $D_i = 1$ only if $s_i \geq c_1$ and $1 \succ_i 2$.

As an illustration, Figure 2 plots the potential outcomes by student's middle school GPA, where Y^1_i denotes the potential outcome for student i if he attends the competitive school, and Y^0_i denotes his potential outcome by attending the non-competitive school. $Y^1_i - Y^0_i$ measures the *individual-specific* return from attending the competitive school, where $Y^1_i - Y^0_i$ varies by student's academic merit (s_i). Only students with GPA $s_i \geq c_1$ are able to switch to the competitive school when the admission scheme becomes merit-based, with the total *potential* gain from attending the competitive school equal to area B on the diagram. The actual (or realized) gain will depend on which students switch to the competitive school as a result of the reform. For these students who are above the admission cutoff ($s_i \geq c_1$), provided that they prefer the competitive school over the non-competitive school, the model

[10] In the case of $\omega = 1$, the implicit admission cutoffs are $c^0_{kj} = \infty, \forall k \neq j$ and $c^0_{jj} \leq \underline{s}$ where $\underline{s}$ is the lowest academic merit among students living in the catchment area for school j.

[11] Serial dictatorship is a special case of the deferred acceptance algorithm (DA). Under DA, assignments between students and schools are unique and stable, and have a unique representation in terms of the market-clearing admission cutoffs (e.g., see Gale and Shapley (1962)).

[12] One way to justify this narrative is to there are many schools but we can aggregate them into two types of schools based on their competitiveness.

implies that the fraction of students attending the competitive school should increase after the reform.[13] This is a key insight which we build on in our identification strategy (see Section 4.2). On the contrary, for low-achieving students with $s_i < c_1$, they may be crowded out from the competitive school, leading to a total potential loss labelled by area A on the same diagram. The aggregate impact therefore depends on the relative size of A and B, as well as the selection and proportion of individuals who are pulled in and crowded out from the competitive school as a result of the reform.

4 Econometric Model

In this section, we show how we can identify the effects of attending a competitive high school for students who are pulled into a competitive school and those who are crowded out from a competitive school. We begin by using the potential outcomes framework to describe the compliance patterns induced by the reform and highlight the identification challenge. Combining the reform with additional variations from school assignment rules, we then provide a constructive proof of identification of the two treatment effects parameters characterizing the effects on students who are pulled in and crowded out separately. Our econometric 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. We also do not make any prior restrictions on the returns from attending a competitive school; the gain from attending a competitive school can be individual specific, implying that the average effects of attending a competitive school may be very different for those who are pulled in and those who are crowded out from the competitive school.

In what follows we focus our discussion of identification in the case of two types of schools: competitive schools and non-competitive schools, where a competitive school is in excess demand after the reform and a non-competitive school is not. Therefore, we are implicitly assuming away any heterogeneity within each type of schools. In Section 7, we extend our discussion to the case of multiple-types of schools.

[13]For these high-ability students, this preference ordering is required in order to rule out the case that they might switch to the non-competitive school under the merit-based system (these would be high-ability students living in the catchment area for the competitive school but opt for the non-competitive school under the merit-based system). To justify this assumption, in Sections 2 and 8, we provide comprehensive evidence documenting that competitive schools outperformed non-competitive schools even prior to the reform, in terms of characteristics such as teacher quality, average student outcomes and value-added. Note that we do *not* need to impose any preference ordering for students below the admission cutoff.

4.1 Potential Outcomes and Compliance Types

We begin by demonstrating what we can identify when using the reform as our instrument Z, where $Z = 1$ if a student is allocated to a school by merit, and $Z = 0$ otherwise. Following the definitions used in Section 3, $D = 0$ if the student attends a non-competitive school and $D = 1$ if the same individual student goes to a competitive school.[14] Denote D^z the potential schooling choice for a student given an instrument $Z = z$. We do not observe both D^0 and D^1 for any individual and only the realized schooling choice D is observed. The population of students can be partitioned into four mutually exclusive groups, depending on their values of potential choices with and without the reform:

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

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

Let $Y^{d,z}$ denote the potential outcome when a student attends a school of type $D = d$ given instrument $Z = z$. For each individual there are four potential outcomes. For simplicity, we abstract from any control variables and defer the discussion of covariates (in order to make the reform exogenous) to Section 5.2. We make the standard IV assumptions, where the instrument (the reform) is independent of potential outcomes and affects outcomes only through school choices:

Assumption 1. *Exclusion:* $Y^{d,z} = Y^d \quad \forall\ d \in \{0,1\}, z \in \{0,1\}$,

Assumption 2. *Independence:* Y^0, Y^1, D^0, D^1 *are jointly independent of* Z

[14] For simplicity we drop the individual subscript i throughout this section.

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, where $Y = Y^0 + (Y^1 - Y^0)D$. We do not restrict the heterogeneity in the payoffs to attending a competitive school: the payoff, $Y^1 - Y^0$, may vary across individuals. Potential schooling choice may be correlated with Y^0, Y^1, or $Y^1 - Y^0$, allowing for selection on levels and gains of potential outcomes.

Under Assumptions 1 and 2, the Wald estimator exploiting Z as an instrument for D identifies a weighted difference of the effects for compliers and defiers:

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

where $LATE_Z$ is the causal estimand and the weights are given by $S_{CP} \equiv \frac{P(D^0=0,D^1=1)}{P(D^0=0,D^1=1)-P(D^0=1,D^1=0)}$, $S_{CC} \equiv \frac{P(D^0=1,D^1=0)}{P(D^0=0,D^1=1)-P(D^0=1,D^1=0)}$. Equation (1) demonstrates that the effect estimated using the reform as an instrument for attending a more competitive school is a weighted average of the treatment effect for CP and CC compliers, where the weights are proportional to the fraction of CC and CP in the population and the "treatment" for CC is being excluded from attending the competitive school.[15] The existence of CC poses challenge to identification, because the reform (Z) will not satisfy the monotonicity condition for the population as a whole. Only if the share of CC compliers is zero (i.e., $P(D^0 = 1, D^1 = 0) = 0$), the causal estimand can identify the average treatment effect for the CP compliers, which is equivalent to the Local Average Treatment Effect in Imbens and Angrist (1994). Under some cases, the treatment effect for the CP compliers can be entirely cancelled out by the treatment effect of those CC compliers, leaving the estimated $LATE_Z$ to zero whereas in fact there may be significant treatment effects for both the CP and the CC compliers.

4.2 Identification of Crowded-out and Pulled-in Effects

In order to separately identify the effects of attending a competitive school for different types of compliers, we exploit variations generated from the interaction of students' middle school GPA with the reform. The theoretical model of school allocation described in Section 3 implies that the reform creates

[15]Note that the weights do not necessarily lie between the 0 and 1 interval. In fact, if $P(D^0 = 0, D^1 = 1) < P(D^0 = 1, D^1 = 0)$, namely, the CP share is smaller than the CC share, the weights can be negative.

nonmonotonic change in the implied admission cutoff scores. Building on this idea, we can partition the population into different subgroups according to students' GPAs and the implied admission cutoffs; within each subgroup, the reform will shift students' potential choices monotonically and satisfy the monotonicity assumption of the IV estimator.[16]

To see how it works, let k be the GPA admission cutoff for the competitive schools under the merit-based allocation scheme. Using the admission cutoff and students' middle school GPA (s), we partition the student population into two groups, $C = 0$ if $s < k$, and $C = 1$ if $s \geq k$. We maintain that Assumptions 2 (independence) and 1 (exclusion restriction) continue to hold *conditional on* C.[17] We make one additional identifying assumption, namely, the conditional monotonicity of the reform:

Assumption 3. *Conditional Monotonicity:* $D^1 \geq D^0$ *if* $C = 1$, $D^1 \leq D^0$ *if* $C = 0$

This assumption rules out the possibility of students with grades above the cutoff, who would have attended a competitive school before the reform, enrolling in a noncompetitive school after the reform. It also excludes students with grades below the cutoff who would have attended a non-competitive school before the reform, enrolling in a competitive school after the reform. As discussed in Section 3, Assumption 3 imposes some preference ordering for students who are above the admission cutoff ($C = 1$), namely, that they prefer the competitive school over the non-competitive school. We believe that this assumption is reasonable in our context of evaluating the relatively short-run implications of the reform. As mentioned in Section 2, the competitive (oversubscribed) high school have the best reputation and consistently outperforms non-competitive schools. In addition, admission to university relies on nationally administered exams (see Appendix A), which does not provide sufficient incentive for high-ability students to strategically attend a low quality high school and improve their relative position within the class, as is instead the case, e.g., in Cullen, Long, and Reback (2013). Finally, as we already discussed in Section 2, the serial-dictatorship mechanism means that we can largely rule out any strategic manipulation when submitting their school rankings after the reform.

[16] We prioritize variation from the admission cutoff score because we have better data on middle-school GPA than locations. If we have data on residential location and catchment areas, we can also exploit allocation rules in the pre-reform period based on residential location. For instance, we can partition the population before the reform into two subgroups: those who do not live in catchment area of a competitive school and those who live in those catchment areas. Then, within each subgroup, the reform shifts students' school choice monotonically.

[17] One concern is that middle-school GPA may be endogenous to the reform. Since the teacher grades are given non-anonymously, teachers may have incentive to change their grading practice following the reform. For instance, teachers may inflate the grades for the best students, knowing that they aim to enroll in the best schools. We conduct several empirical test in Section 8, concluding that there is little evidence of manipulation in the middle school GPA. See Section 8 for details.

Assumption 3 guarantees within-group monotonicity of the reform (i.e. only one type of compliers in each group). To see this clearly, Table 1 shows all the possible compliance types based on potential schooling choices once this assumption is in place. As an example, consider the students below the cutoff ($C = 0$, first two columns). In this case, the only type of compliers is CC. Therefore, the admission cutoff combined with the reform allows us to define subsamples of the student population where the monotonicity condition holds. This, in turn, allows separate identification of the treatment effects for compliers who are crowded out (CC) of and those pulled in (CP) to competitive schools, as shown in proposition 1 below.

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

$$LATE_{CC} \equiv E[Y^0 - Y^1|D^0 = 1, D^1 = 0] = \frac{E[Y|Z = 1, C = 0] - E[Y|Z = 0, C = 0]}{P(D = 1|Z = 0, C = 0) - P(D = 1|Z = 1, C = 0)}, \quad (2)$$

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

$$LATE_{CP} \equiv E[Y^1 - Y^0|D^0 = 0, D^1 = 1] = \frac{E[Y|Z = 1, C = 1] - E[Y|Z = 0, C = 1]}{P(D = 1|Z = 1, C = 1) - P(D = 1|Z = 0, C = 1)}. \quad (3)$$

Proof. See Appendix C. □

Equations (2) and (3) correspond to the covariate-specific Wald estimator with a binary endogenous variables and a binary instrument. The denominators in equations (2) and (3) correspond to the shares of CC and CP among students with grades below and above the admission cutoffs, respectively. Note also that equation (2) estimates the opposite of the effect of attending a competitive school for CC, because the reform *excludes* CC from attending a selective school.

A key aspect of our research design is that we observe outcomes and choices in the pre-reform period. As explained in Section 3, the reform leads to unexpected discrete changes to the school admission cutoffs. By comparing individuals above the admission cutoff after the reform to students with the same GPA before the reform, we can identify the effects for individuals shifted into the competitive school. Effectively, we are able to identify the average treatment effect for a subset of students above the admission cutoff because the reform (weakly) increases their probability of being allocated to the competitive school. The same logic applies to students below the cutoff, whose probability of going to the competitive school (weakly) decreases after the reform.[18]

5 Empirical Implementation

5.1 Data, Competitive Schools' Definition and Descriptive Evidence

Data 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, for the cohorts we consider, we can measure outcomes up to 2018, when they are in 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).[19] Because the starting age for high school in Norway is 16, the individuals in our sample were born in the period 1986–1991 and since we observe the exact date of birth of individuals we can place students in the cohort they belong to.

We focus on students choosing academic high school because the vocational track is not affected by the admission reform. In Section 2 we provided evidence showing that the reform likely had no impact on the choice of students to enroll in academic or vocational track. We include in our sample students attending any of the 16 public academic high schools in Bergen (the largest city in the Hordaland county) and three adjacent municipalities, all located in Bergen's county and hence affected by the reform.[20] We exclude a small number of students who attended a middle school that was more than ten kilometres away from *any* of the 16 high schools. The number of students in each cohort ranges between 1,200 and 1,400 (Figure A5).

As our main outcomes we focus on high school completion, university enrollment within six years since the beginning of high school and university completion by age 28.[21] We also examine additional outcomes, including number of days of school absence (averaged over the three years of high school), university enrollment in STEM fields and enrollment in "elite" universities or courses.[22] School absence

[18]If we only have access to post-reform data (i.e., we only have data under the merit-based school allocation), then, by exploiting GPA as the running variable in an RDD specification where predetermined cutoffs serve as instruments for actual admission, we are only able to identify the treatment effect for marginal individuals at the admission cutoff.

[19]Because of data availability, we cannot extend our sample to students completing middle school before year 2002, hence the first cohort we observe is the one for students born in 1986.

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

[21]High school completion is measured four years after initial enrolment, normal completing time is 3 years, but students may be required to retake one year.

[22]These include universities offering education in medicine, one engineering university (Norwegian University of Science

days are only recorded for students who completed high school, and therefore the estimated effects on school absence should be interpreted with caution as it combines the true effect and the effect from sample selection.

Definition of Competitive School and Admission Cutoff 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 the first two years following the reform (not in 2007). The imputed admission cutoffs for the competitive schools range between the 10^{th} and the 75^{th} percentile of the middle school GPA distribution of the respective cohort. Figure A6 shows that the probability of enrollment in an oversubscribed school is indeed low below the imputed cutoffs and increases on average by 15 percentage points at the cutoff in every post-reform year. We define these five schools as competitive schools, and attending any one of these five schools (or four schools in 2007) as $D = 1$ considered in Section 4. In Section 7, we consider the case of multiple school choices by further categorizing the five competitive schools into two groups based on their selectivity.

Post-reform students are defined as being above the cutoff if their middle school GPA is larger than the lowest cutoff among all competitive high schools that are located within eight kilometres of their middle school. In the data, around 80% of the students attend a high school which is situated within 8km from their middle school. We thus consider an admission cutoff to be relevant only if the school is located not too far, effectively putting some restriction on the choice set of schools. In our main analysis, we exclude students who are more than 8km away from any competitive school after the reform because they do not have a relevant admission cutoff.[23]

and Technology) and the national business school (Norwegian School of Economics). We estimate that the returns from attending these universities are about 40% higher than attending an average university (based on the earnings at age 30–40 of cohorts born between 1970 and 1975).

[23]Further details on the mapping of cutoffs to students are in Appendix D. One might be concerned whether the 8km requirement would limit the number of pulled-in compliers we can identify (e.g. there may be students switching to a competitive school from outside 8km). In practice, we find that the estimated share of pulled-in compliers declines if we relax the 8km restriction to 10km, suggesting that expanding the distance coverage further identifies few additional pulled-in compliers (it worsens the first-stage estimates instead).

Descriptive Statistics In Table A1 we provide a list of descriptives, separately for the pre- and post-reform cohorts, for the overall sample (columns (1) and (2)), for the sample of students whose middle school GPA is within the first quintile of their cohort's middle school GPA distribution (columns (3) and (4)) and for the sample of students with middle school GPA in the top quintile of their cohort's middle school GPA distribution (columns (5) and (6)). The estimated cutoffs for admission in the competitive schools post-reform range between the 10^{th} and the 75^{th} percentile of the middle school GPA, thus some of the students in the first quintile are likely below the cutoff in the post reform period, while all those in the top quintile are above the cutoff. Overall, because we are focusing only on individuals attending academic high school, these are positively selected in terms of parental background. For example, the average parental earnings in the pre-reform period (375,873 NOK, column (1)) is larger than the observed average parental earnings of the overall population of high school students (which, in the same period, is 323,270 NOK).[24] Academic high school students are also slightly more likely to be female. As expected, given the usual positive correlation between school grades and parental background, we find that students in the bottom GPA quintile have less educated parents (less than 30% of them has both parents with a university degree, while the same number is about 50% for those in the top quintile) and consequently live in household with lower familiar earnings. In what follows, we base our definition of socio-economic status (SES) on education of parents in order to use a measure which is more permanent as compared to earnings which are more volatile. In terms of gender, female students are much more likely to be in the top quintile of GPA distribution than male students.

Panel (b) of Table A1 reports mean school choice and outcomes for each subgroup of students. It is clear that the reform changed the probability of admission to competitive high schools depending on students' grades. For instance, before the reform 35% of students in the bottom percentile attended a competitive high school, the same proportion is only 9% post-reform, confirming that a large fraction of these students are below the admission cutoff post-reform. For students in the top quintile, instead, this fraction clearly increased (from 44% to 59%). The differences in education outcomes by student's GPA groups are large, but the within group pre- and post-reform differences are mostly small and do not show any clear pattern. For instance, high school completion rate remains at 73% for those in the bottom quintile and at 96% for those in the top one. The proportion of students in the bottom quintile

[24] Parental earnings are the average annual earnings of fathers and mothers during the three years that the student is in high school and are expressed in 1998 NOK, 6NOK $\approx$ 1USD.

enrolling and completing university is instead increasing (from 66% to 70% for university enrolment and from 57% to 62% for university completion). The simple comparison of average outcomes between cohorts affected and not affected by the reform should not be interpreted as the effects of the reform, because of pre-existing trends are not accounted for – for instance, there has been an increase in university enrolment at the national level. Additionally, admission cutoffs vary for different students, hence it is not clear just looking at the position in the grades distribution whether a students gains or loses admission to more competitive high schools. We now turn to the specification of an empirical model which can account for pre-existing trends and obtain estimates of the effect of the reform for different groups of students.

5.2 Empirical Model and Estimation

Following Proposition 1, we estimate the effect of attending a competitive school for CC and CP separately, exploiting the reform as an instrument conditional on being above or below the admission cutoff. Define the random variable $Post_i \in \{1, 0\}$ which takes value 1 if individual i is born on or after January 1^{st}, 1989, since, as mentioned in Section 2, the first cohort of students who are allocated to high school under the merit-based mechanism is the 1989 cohort (hence, $Post_i$ corresponds to Z_i in Section 4.2). We also define two mutually exclusive indicator variables $Below_i$ and $Above_i$; $Below_i$ is 1 if an individual i's GPA is below the admission cutoff for any competitive school in person i's choice set and 0 otherwise, while $Above_i$ is 1 if i's GPA is above the admission cutoff for at least one competitive school in i's choice set and 0 otherwise ($Above_i$ corresponds to C_i in Section 4.2, while $Below_i$ is $1-C_i$). As mentioned, the choice set considered are all competitive schools located within 8km of the students' middle school.

Our identification argument assumed that the instrument Z_i was independent of potential outcomes and therefore abstracted from any covariates. In our empirical analysis, however, $Post_i$ may not be exogenous unconditionally because, for instance, there may be an increasing trend in unobserved cohort quality over time. We use two research designs to purge the exogenous variations in $Post_i$. The first approach is based on a regression discontinuity design (RDD), where the running variable is the week of birth and the causal effect is identified by comparing students born only few weeks apart who have differential exposure to the reform, that is, those born around January 1st, 1989 (see e.g. Clark and

Royer, 2013; Bertrand, Mogstad, and Mountjoy, 2021). At the core of this approach is a set of covariates controlling for differential time trends before and after the date-of-birth discontinuity. As our second approach, we rely on a before-after comparison, assuming that the timing of the reform is exogenous subject to a pre-existing linear trend which may differ by students' GPA.

To recover the parameters of interest, we begin by estimating the reduced-form parameters. Our RDD specification is the following:

$$\begin{aligned} y_i =& \beta_0 + \beta_{CC} Post_i \times Below_i + \beta_{CP} Post_i \times Above_i + \beta_{\mathbf{1}} \mathbf{GPA_i} \\ &+ Week_i \times (\beta_2 + \beta_{\mathbf{3}} \mathbf{GPA_i} + \beta_4 Post_i + \beta_{\mathbf{5}} Post_i \times \mathbf{GPA_i}) + \beta_{\mathbf{6}} \mathbf{X_i} + \varepsilon_i, \end{aligned} \tag{4}$$

where y_i is an outcome of the student, $\mathbf{GPA_i}$ is a vector of controls for student's middle school GPA (such as nonparametric deciles indicators or parametric polynomials in the student's GPA percentile within his/her cohort) and $Week_i$ is the distance (in week) between the week of birth of the student and the first week of January 1989. By including $Week_i$ in the regression we thus control for a linear function of the week of birth, allowed to be different for students affected and not affected by the reform (β_2 and β_4).[25] We also allow the time trends to be heterogeneous for students of different ability, by interacting both pre- and post-reform trends with $\mathbf{GPA_i}$ (β_3 and β_5). To alleviate concerns that students born early in the calendar year are systematically different compared to those born at the end of the year, in $\mathbf{X_i}$ we include 52 dummies for being born in each week of the year.[26] We also include additional location controls in $\mathbf{X_i}$ (see below). We cluster standard errors at the week of birth level.

The control for middle school GPA allows us to compare students with similar grades before and after the reform. Our working assumption is that the pre-reform baseline difference between being above and below the (latent) admission cutoff is excludable conditional on $\mathbf{GPA_i}$.[27] This means that

[25] As robustness checks, we include a quadratic form of the week of birth and use month of birth (instead of week of birth) as the running variable. See Section 6.4 for details.

[26] For instance, students born early in the year are older when they start school (Black, Devereux, and Salvanes, 2011). Note that, in order to identify the week fixed effects, we need have a few years of data in our sample.

[27] In the pre-reform period, schools do not admit students based on their middle-school GPA. Hence, the admission GPA cutoff is not observed prior to the reform. An alternative method is to control for the main effect of being above the admission cutoff directly (i.e., control for $Above_i$). To implement this method we need to first predict admission cutoffs for the pre-reform cohorts based on the observed cutoffs in the post-reform years, which could introduce measurement errors. Controlling for GPA instead of an indicator variable based on the predicted admission cutoff reduces the sensitivity of our results to arbitrary choices of the imputation procedure.

there should be no baseline differences between pre-reform cohorts above and below the latent admission cutoff once $\mathbf{GPA_i}$ is controlled for. As an empirical assessment of this assumption, we run placebo regressions using pre-reform data and assign (or impute) pre-reform GPA cutoffs using the within-cohort rank of admission cutoffs in the first post-reform year (see Appendix Section E for details). Specifically, we regress (pre-reform) outcomes on GPA controls ($\mathbf{GPA_i}$) and $Above_i$ (defined using the imputed cutoff) along with other controls in equation (4). Under our working assumption, we should expect the coefficient on $Above_i$ to be close to zero and statistically insignificant. We report the placebo regression results in Table A2 under a range of parametric and nonparametric GPA controls. We find that controlling for a polynomial function in GPA and GPA decile indicators pass the placebo tests (whereas a linear function in GPA and GPA quintile dummies do not). In what follows, as $\mathbf{GPA_i}$, we use the quadratic GPA function as our baseline and indicators for GPA deciles as robustness check.[28]

Finally, the relevant admission cutoff for each student is defined from schools in the student's choice set, which depends on the distances between the attended middle school and competitive high schools. In addition, before the reform location of the schools was crucial to determine the school attended. To take into account potential differences between students admitted and not admitted to competitive school living in different areas, we control for location indicators in the regression. Specifically, in $\mathbf{X}$, we include five dummies, each taking value 1 if the middle school attended by the student is within three kilometres from each competitive school.[29]

From equation (4), we recover the RDD estimates β_{CC} and β_{CP}, corresponding to the reduced-form parameters in Proposition 1. As it is clear from Proposition 1, in order for these effects to be interpreted as the effect for CC and CP compliers, they need to be scaled by their respective proportion, which can be recovered from a first stage regression on school choices. Specifically, we can estimate the proportion of CP and CC from a first stage regression based on Equation (4) where the outcome

[28]Since we condition on students' GPA, we also need to ensure that student GPA is not affected by the reform (for example, if there is more grade inflation following the reform). We examine this issue empirically in section 8.

[29]We experimented using 8km dummies (the relevant distance to determine the cutoff, see section 5.1) both jointly with 3km dummies and on their own, with little differences in the final estimates. These location controls may not be good controls, if individuals relocate as a consequence of the reform. However, our location controls are based on distance between middle school and competitive high schools (partly driven by that we do not have data on residential location at hand). Relative to residential location controls, controlling for middle-school location alleviates the bad control problem, because we study relatively short-term effects of the reform (3 post-reform cohorts) and the cohorts under study already started middle school at the time of the reform. This makes harder for them to move middle schools and thus our location dummies are less likely to be affected by the reform.

variable is a dummy d_i for attending a competitive school:

$$d_i = \beta_0^d + \beta_{CC}^d Post_i \times Below_i + \beta_{CP}^d Post_i \times Above_i + \beta_{\mathbf{1}}^{\mathbf{d}} \mathbf{GPA_i}$$
$$+ Week_i \times (\beta_2^d + \beta_{\mathbf{3}}^{\mathbf{d}} \mathbf{GPA_i} + \beta_4^d Post_i + \beta_{\mathbf{5}}^{\mathbf{d}} Post_i \times \mathbf{GPA_i}) + \beta_{\mathbf{6}}^{\mathbf{d}} \mathbf{X_i} + \varepsilon_i, \quad (5)$$

The ratios $\frac{\beta_{CC}}{-\beta_{CC}^d}$ and $\frac{\beta_{CP}}{\beta_{CP}^d}$ estimate the effect of attending a competitive school for the CC and CP compliers, respectively. The estimates of these two ratios correspond to estimates of the parameters in Equations (2) and (3) from Proposition 1. Standard errors are computed using the delta method.

The RDD framework exploits birth-date cutoff and identify the parameters by comparing students who have different exposure to the reform near the January 1, 1989 birth date cutoff. As our second approach, we use a before-after design by comparing the overall changes in outcomes before/after the reform. Effectively, we treat $Post_i$ as an instrument and assume that the timing of the reform is exogenous subject to a pre-existing linear trend which may vary by students' GPA.[30] We estimate the following reduced-form equation:

$$y_i = \gamma_0 + \gamma_{CC} Post_i \times Below_i + \gamma_{CP} Post_i \times Above_i + \gamma_{\mathbf{1}} \mathbf{GPA_i} + Week_i \times (\gamma_2 + \gamma_{\mathbf{3}} \mathbf{GPA_i}) + \gamma_{\mathbf{4}} \mathbf{X_i} + \epsilon_i, \quad (6)$$

where the coefficients γ_2 and $\gamma_{\mathbf{3}}$ estimate linear trend in weeks of birth and its interaction with student GPA, capturing changes in potentially unobserved student quality that is orthogonal to the reform. X includes the same controls as in equation 4. From equation (6), we recover γ_{CC} and γ_{CP}. Similar to the RDD approach, we then estimate the first stage regression in order to recover the compliance shares, by replacing the outcome variable with d_i as in equation (6). Dividing the reduced-form effects by their respective first stage, we obtain the treatment effects for CC and CP compliers.

6 Results

Before presenting the effects for CC and CP compliers separately, we first report estimates for the *aggregate* impact of the reform in Section 6.1, by making aggregate comparisons between Bergen and other education markets and by exploiting discontinuity in the exposure to the reform within Bergen.

[30]We experiment using quadratic trends and results are substantially unchanged.

We then report the treatment effects for CC and CP separately in Section 6.2, following the empirical strategy discussed in Section 5.2. In Section 6.3, we characterize the composition of CC and CP compliers using observable characteristics including family background and gender. Finally, we show that our results are robust to alternative specifications of the control variables (Section 6.4).

6.1 Aggregate effect of the reform

We first present evidence on the aggregate effects of the reform. As explained in Equation (1), this aggregate effect is a weighted average of the effect for crowded-out compliers and the effect for pulled-in compliers. We estimate the aggregate effects in two ways (we provide a more analytic description of these alternative empirical specifications in Appendix F). First, we make aggregate comparisons across markets, comparing the evolution of average performance in Bergen with other markets in Norway which did not implement the reform.[31] To implement this comparison, we use a difference-in-difference design, comparing changes in student outcomes in Bergen and closest municipalities relative to changes in other markets including Trondheim, Stavanger, Drammen, and Kristiansand, the biggest cities in Norway excluding Bergen and the capital Oslo.[32] Columns (1)–(3) in Table 2 display the estimated effect, reporting a small decline in the proportion of individuals completing high school (-1.8 percentage points, p.p.), enrolling at university (-1 p.p.) and completing it (-1.4 p.p.). None of these estimated coefficients is precisely estimated, but this result tends to exclude any positive overall effect of the reform.

Because the reform differently affected individuals with different middle school GPA, we also investigate whether we can recover some heterogeneity on the basis of where students stand in the middle school GPA distribution of their cohort. We restimate the difference-in-difference model by allowing for separate effects of the reform on the basis of quintiles of middle school GPA distribution.[33] We report this heterogeneity analysis in columns (4)–(6) of Table 2. Also in this case the s.e. are relatively large,

[31] This comparison is in the spirit of Duflo, Dupas, and Kremer (2011) and Muralidharan and Sundararaman (2015). Duflo, Dupas, and Kremer (2011) analyze how tracking affects "included" and "excluded" children and also examine whether the tracked schools did better or worse than the untracked ones in the aggregate. Muralidharan and Sundararaman (2015) examine how performance compared across markets where choice was increasing or not, in their case via vouchers.

[32] We did not include Oslo as a control because it also changed its high school enrolment system during the period we consider. For the difference-in-difference specification, the main outcomes (high school completion, university enrolment and university completion) are regressed on the interaction between a dummy for attending an academic high school in the Bergen area and a dummy for belonging to a cohort which is affected by the reform, controlling for municipality fixed effects, cohort fixed effects and a municipality-specific linear trend. See equation (F.1) in Appendix F.

[33] See equation (F.2) in Appendix F for details.

but the pattern points to students in the bottom quintiles in Bergen suffering some losses from being affected by the reform if compared to students from the same cohorts in the same quintiles in other municipalities. Individuals in the bottom quintiles are those who are more likely to be *excluded* from competitive high school as a consequence of the reform (Section 5.1). On the contrary, for high-grades students the probability of *attending* more competitive high schools in Bergen is increasing. However, they do not display any positive effect from the reform. On the contrary, the effect for them also seems slightly negative, but the magnitudes are smaller than for low-grades students.

As our second method, we use the RDD framework to analyze the aggregate effects of the reform within Bergen. Figure 3 shows the proportion of students born in each week around January 1^{st}, 1989 completing high school (panel (a)), enrolling at university (panel (b)) and completing university (panel (c)) and a linear trend fitting these averages separately before and after the reform.[34] In all three education outcomes, we observe an increasing trend before the reform and a flattening out after the reform. The decline is more pronounced for university enrolment and completion; with university enrolment declining by about 2.4 p.p. and university completion declining by 2.2 p.p.[35] The standard errors for these estimates are smaller than those in the diff-in-diff specification and the effect for university enrolment is significant at the 10% level.

Finally, in Table 3 we present a descriptive analysis of the heterogeneous impact of the reform by quintiles of middle school GPA of the students, using the same within Bergen RDD framework and interacting the aggregate RDD effect with dummies for middle school GPA quintiles.[36] As expected, the proportion of students within the first quintile of middle school GPA distribution who attend a competitive high school decreases substantially as a consequence of the reform, while the same proportion increases for students with high grades (top two quintiles). The pattern of the effect on the three outcomes (high school completion, university enrolment and university completion) is similar to what we observed when comparing Bergen with other municipalities. It is generally negative for the first quintile, and still negative, but of smaller magnitude for the top quintile.

Both cross market comparison and within Bergen analysis confirm that, if anything, the reform had

[34] To remove the week-of-birth fixed effects, we first obtain the residuals from a regression of the outcome on week of birth fixed effects (52 dummies) and then add the estimated value of the constant term to these residuals.

[35] We recover these estimates from RDD equations similar to Equation (4), where the *Post* dummy appears on its own and without controlling for middle school GPA. See equation (F.3) in Appendix F for details.

[36] See equation (F.4) in Appendix F for details.

a *negative* impact on the outcomes of affected students. The estimated overall impact of the reform are the reduced form counterpart of the LATE when competitive high school attendance is instrumented by the reform, i.e. the numerator of $LATE_Z$ in Section 4. As mentioned, because this parameter does not separate between the effect for CC and CP, it can hide significant heterogeneity, as the analysis by quintile of GPA seems to indicate. The rest of this section exploits the econometric framework described in Sections 4 and 5 to isolate the effect for the different types of compliers and more directly relate the observed changes in outcome to the changes in high school allocation caused by the reform.

6.2 Treatment Effects for Pulled-in and Crowded-out Compliers

RDD framework Table 4 reports estimates under the RDD framework. The first row shows the estimate of the first-stage parameters (equation (5)), corresponding to the proportion of CC and CP compliers. Both CC and CP account for just below 14% of students who are below and above the admission cutoff, respectively. In subsequent rows, we report the estimated reduced form effects (equation (4)) on education achievements and the consequent local average treatment effect, separately for students who are crowded out of a competitive school (CC complier) and those being pulled into a competitive school (CP complier). Among students who are potentially crowded out, we find the reform had little impact on high school completion (1.2 p.p. decrease with a standard error of 2.3 p.p points) but a stronger negative effect on university education. For instance, we find that the reform leads to a reduction of about 5.2 p.p. in the probability of university enrolment, a significant reduction considering that pre-reform about 66% of low-grades students (in the bottom quintile of the middle school GPA distribution) enrolled at university (Table A1). University completion declined even more, by almost 6 p.p., with the baseline for university completion for low-grades student being already low, at 57%. Scaling the reduced-form estimates by the first stage compliance share, we find large effect of being excluded from a competitive school for CC compliers, with university enrolment declining by 39 p.p. and university completion declining by 44 p.p.

The CC compliers have low academic performance and their education achievements can be particularly elastic to school environment. The large effects of attending a competitive school for CC compliers are generally comparable with previous studies. For instance, in evaluating the impact of a reform that expands access to more competitive schools for disadvantaged students, Lavy (2010) find that the reform leads to a 35% decline in high school dropout rate. Similarly, Bleemer (2021) finds that enrolling in more selective universities caused an increase of 30 p.p. in the probability of obtaining a university degree among students with low academic achievement (average SAT scores were at the 12th percentile of other students at University of California). Black, Denning, and Rothstein (2023) find large effects of attending more competitive colleges among underrepresented minority groups with high academic performance.[37]

Turning to the outcomes of students who gain access to competitive high schools, the final two columns of Table 4 show that there is not any improvement for CP compliers either, with the effects on high school completion and university completion being close to zero. Considering that high ability students already had high completion rates of high school and university before the reform (Table A1), one might be concerned that the zero effects we find may be due to a ceiling effect. Table A3 presents results for a range of additional outcomes, including days of absence and enrolment in elite universities and STEM majors. We do not find that the pulled-in compliers gain from attending competitive high schools along any of these additional outcomes (where ceiling effects are less binding).[38] Overall, our results suggest that pulled-in compliers do not benefit from attending competitive high school. By comparison, being excluded from competitive high school leads to worse outcomes for the crowded-out group, both in terms of education attainment as discussed previously and in terms of enrolment in elite universities and STEM fields as shown in Table A3. The negative effects for CC compliers thus drives the small but negative aggregate impact of the reform reported in Section 6.1.

Considering that admission to competitive schools depended on location prior to the reform, we also exploit additional variations from (middle school) locations to identify more clearly the compliance groups and gain further precision in our estimates.[39] Building on the estimating equation (4), we further interact the treatment dummies with a dummy $close_i$, indicating whether the person attended a middle school which is within 3km from at least one of the five competitive high schools, and with a dummy far_i, indicating whether the person attended a middle school which is more than 3km away from any

[37]In Black, Denning, and Rothstein (2023), their reduced-form effect on college completion is smaller than us (3.7 p.p. points vs. 5.2 in our case), but also the effect of their instrument in moving students towards more competitive colleges is smaller (6.6 p.p. points vs. 13.4 in our case).

[38]The result of no effect for high ability students is consistent with findings exploiting marginal admission to elite high schools in Boston and New York (Abdulkadiroğlu, Angrist, and Pathak, 2014; Dobbie and Fryer, 2014), where for high-achieving students the effect of attending more competitive schools on college attendance and college quality is zero.

[39]For example, under our current analysis, a student who has low GPA but lives far from any competitive school is unlikely to attend a competitive school under any policy regime (thus being a never-taker), while a high-GPA student living closeby a competitive school is most likely an always-taker.

competitive high school:[40]

$$
\begin{aligned}
y_i = & \beta_0 + \beta_{CC}^{close} Post_i \times Below_i \times close_i + \beta_{CP}^{close} Post_i \times Above_i \times close_i \\
& + \beta_{CC}^{far} Post_i \times Below_i \times far_i + \beta_{CP}^{far} Post_i \times Above_i \times far_i + \beta_{\mathbf{1}} \mathbf{GPA_i} \\
& + Week_i \times (\beta_2 + \beta_{\mathbf{3}} \mathbf{GPA_i} + \beta_4 Post_i + \beta_{\mathbf{5}} Post_i \times \mathbf{GPA_i}) + \beta_{\mathbf{6}} \mathbf{X_i} + \varepsilon_i, \qquad (7)
\end{aligned}
$$

Panels (a) of Table 5 report the estimates for students who are within 3km (β_{CC}^{close} and β_{CP}^{close} from equation (7)), while panels (b) for those who are further away (β_{CC}^{far} and β_{CP}^{far} from equation (7)). These results confirm the validity of our approach. As expected, the majority of CC come from areas close to the competitive schools, with 34% of low-GPA student living closeby being crowded out. For them, the estimated effect on university enrolment and completion is more precise than what was estimated for the overall sample; both effects are negative for approximately 30 p.p, thus about 10 p.p. less of what was reported in Table 4, with s.e. being about half the size. The fraction of CP who attended a middle school more than 3km away from competitive schools (15.5%) is larger than the same fraction for those from a closeby middle school (11.6%), but this is still positive, suggesting that even students living not too far did not have access to some of the competitive schools before the reform. For both groups the estimated effects are close to what was estimated for the overall sample and confirm a zero effect of attending a more competitive school.

Before-after comparison The results presented so far are based on the RD design with controls, which implies that we estimate the effect for students born around the 1^{st} of January cutoff. As mentioned, this relies on the fact that we can properly control for seasonality in the date of birth. Table 6 reports estimates of the pre-post comparison, which does not rely on such controls for identification and can be seen as an average effect over the post-reform period, conditional on pre-existing trends. Both first stage and reduced form results are very close to the RDD ones in terms of magnitude and precision, suggesting that our RDD results can have an interpretation which goes beyond the local effect.

All in all, our findings suggest that attending a more competitive high school has an heterogeneous

[40]We do not control for the main effects of $close_i$ because, similar to equation (4), in $\mathbf{X}$ we already include dummies for being within 3km from each competitive high school. These dummies control for baseline differences between individuals who are far or close from competitive high schools. Our results are robust to also adding an interaction of the dummy for being within 3km and the GPA control.

impact on different individuals. Of course, such heterogeneity may follow from different characteristics of students in the CC or CP groups, something we explore in Section 6.3. Interestingly, our findings imply that even a small overall negative effect (as shown in Section 6.1) may hide significant penalties for specific groups, thus being able to separate who loses or gains from a change in school assignment becomes extremely relevant. For the Bergen case, students who are crowded out as a consequence of the reform are those who generally gain the most from "better" school environments.

6.3 Characterizing Compliers

The nature of the reform means that students who gain (lose) admission to these types of schools have better (worse) grades. Although grades are correlated with certain student and family characteristics, such correlation is not perfect. In this section, we characterize CC and CP compliers along two additional dimensions: family background (SES) and gender.

Let J be an indicator variable which equals 1 if a student has a specific characteristic (e.g. comes from a high SES family). Using Baye's rule, one can show that the ratio between the share of CC (CP) compliers conditional on $J = 1$ and the overall CC (CP) compliance share is equivalent to the fraction of CC (CP) compliers with $J = 1$ relative to the population average of J:

$$\frac{P(J=1|D^0=1,D^1=0)}{P(J=1)} = \frac{P(D=1|Z=0,C=0,J=1) - P(D=1|Z=1,C=0,J=1)}{P(D=1|Z=0,C=0) - P(D=1|Z=1,C=0)} \tag{8}$$

$$\frac{P(J=1|D^0=0,D^1=1)}{P(J=1)} = \frac{P(D=1|Z=1,C=1,J=1) - P(D=1|Z=0,C=1,J=1)}{P(D=1|Z=1,C=1) - P(D=1|Z=0,C=1)} \tag{9}$$

For instance, if $\frac{P(J=1|D^0=1,D^1=0)}{P(J=1)} > 1$, then the CC compliers are *over-represented* by students with characteristic $J = 1$ (relative to the population average). To obtain the compliance shares conditional on $J = 1$, we extend the first stage regression (equation (5)) by interacting $Post_i \times Below_i$ and $Post_i \times Above_i$ with the indicator variable J and controlling for the main effect of J.

Table 7 shows that, as expected, CC are 2.5 times more likely to having attended a middle school within 3km from a competitive high school relative to an average student in the population, while CP compilers are more likely to attend a middle school that is beyond 3km from a competitive school. There are interesting patterns of asymmetry when it comes to family background and gender. CC are over-represented by students from high SES families and men, whereas CP are over-represented by students from low SES families and women. The results by family SES suggests that the reform, although designed to allocate students based on merits, also levels up access to competitive schools by parental background to certain extent. This could be explained by high SES parents being more able to locate themselves closer to a competitive high school prior to the reform.

We also estimate the reduced-form effects conditional on family SES and gender and the implied gender- and SES-specific local average treatment effects parameters. We report the results in Appendix Table A4. The estimates are not precise, and in general we cannot reject the null hypothesis that there is homogenous treatment effects by SES and gender. Nevertheless, it appears that the CC compliers among low SES students appears to lose more in terms of university education relative to CC compliers of high SES students. In other words, low-SES and low-grades students should benefit more from attending more competitive schools, suggesting that some mechanism which guarantees admission for this group may alleviate the negative impact of the reform.

6.4 Robustness Checks

We perform different checks to show that our results are not sensitive to the choices we made in the empirical specification to guarantee that assumptions 2–3 hold. In Table 8 we report the estimates for the first stage, the reduced form and the LATE, separately for CC and CP, from specifications where we change the functional forms or the definitions of the control variables.

First, we analyze sensitivity of our results with respect to the functional form of the GPA controls. The discussion in Section 5 and in Appendix E justified our choice of using a quadratic function of the middle school GPA percentile as a control. In Table A2 we show that, in fact, conditioning on GPA decile dummies also guarantees that admission cutoff is excludable (although other functional forms of GPA likely do not). Column (1) of Table 8 reports results from a specification where we replace the quadratic GPA with non-parametric GPA decile indicators. The obtained point estimates are substantially unchanged, although the standard errors increase slightly.

Next, we investigate whether adding flexibility to the trend in week of birth (i.e. the running variable of the RDD), or whether specifying it based on a different time metric, has any impact on the estimated effects. Column (2) reports our estimates when using a quadratic function in the week of birth (allowed to differ for cohorts born before and after the December 31^{st}, 1988 cutoff). Column

(3) specifies all time variables (the linear trend and the seasonality dummies) in terms of the month of birth, effectively making month of birth as running variable instead of week of birth. Both specifications confirm the negative and large impact of the reform on the university attendance of CC and generally no effect for CP.

Taken together, the evidence we obtain from our baseline analysis and the one presented in this section broadly confirm the negative effect of the reform on crowded-out students especially on university outcomes. This negative effect implies a decline in the probability of enrolling and completing university for 40–30 p.p. points. On the contrary, we consistently fail to find evidence of a significant return of attending better schools for students who are pulled in more competitive schools as a consequence of the reform.

7 Multiple Types of Schools

Insofar our analysis focuses on two types of schools, effectively imposing homogenous effect from attending at any one of the over-subscribed schools. In this section, we consider the case of multiple types of schools, allowing heterogeneous treatment effects from attending different competitive schools. We disaggregate the five competitive schools into two groups based on their selectivity. We define as the "most selective" schools the two competitive schools which have consistently an admission cutoff above the 50^{th} percentile of the middle school GPA distribution. They are the most selective because their admission cutoff is always at least 20 percentiles above the cutoff for admission at any other competitive schools, implying that they are in particularly high demand. We then classify the remaining three competitive schools as "selective", meaning that they must also be in high demand, given that they are oversubscribed after the reform, but their admission cutoff is significantly lower the one for the most selective schools. We define a multi-level treatment D, where $D = 0$ if an individual attends a non-competitive school, $D = 1$ if the individual attends a selective school, and $D = 2$ if the individual attends a most selective school. In the binary school choice model, attending a competitive school is equivalent to $D = \{1, 2\}$.

Let c_1 and c_2 be the GPA admission cutoffs associated with the selective and most selective schools under the merit-based allocation scheme, respectively, where $c_2 > c_1$. These two admission cutoffs partition the student population into three groups, $C = 0$ if $s < c_1$, $C = 1$ if $c_1 < s < c_2$, and $C = 2$ if

$s > c_2$. We maintain the assumption that the reform is independent of potential outcomes and satisfies the exclusion restriction conditional on $C = c, \forall c$. We extend the conditional monotonicity assumption to the case of multiple treatments:

Assumption 4. *If $C = 2$ and $D^1 \neq D^0$, then $D^1 = 2$; if $C = 0$ and $D^1 \neq D^0$, then $D^1 = 0$; if $C = 1$ and $D^1 \neq D^0$, then $D^1 = 1$.*

This assumption extends the previous conditional monotonicity assumption to a setting with multiple counterfactual treatments. For students with the highest GPA ($s > c_2$), this restriction states that anyone who changes school choice as a result of the reform does so to attend the most selective school. The compliers are thus pulled into the most selective school from either $D^0 = 0$ or $D^0 = 1$. For students with the lowest GPA ($s < c_1$), they only change school choice as a result of the reform by switching to attend the non-competitive school. In this case, the compliers are crowded out from either $D^0 = 1$ or $D^0 = 2$. For students above the cutoff of the selective school but below the cutoff of the most-selective school, the reform induces them to switch to the selective school. This implies that the compliers may either be pulled into the selective school from $D^0 = 0$, or crowded out from the most-selective school with $D^0 = 2$.[41]

Under Assumption 4, the group-specific IV identifies the average treatment effect for compliers relative to the relevant set of counterfactual alternatives:

$$\frac{E[Y|Z=1,C=0]-E[Y|Z=0,C=0]}{P(D=0|Z=1,C=0)-P(D=0|Z=0,C=0)} = E[Y^0 - Y^{D^0}|D^1 = 0, D^0 \neq 0] \equiv LATE_0 \quad (10)$$

$$\frac{E[Y|Z=1,C=2]-E[Y|Z=0,C=2]}{P(D=2|Z=1,C=2)-P(D=2|Z=0,C=2)} = E[Y^2 - Y^{D^0}|D^1 = 2, D^0 \neq 2] \equiv LATE_2 \quad (11)$$

$$\frac{E[Y|Z=1,C=1]-E[Y|Z=0,C=1]}{P(D=1|Z=1,C=1)-P(D=1|Z=0,C=1)} = E[Y^1 - Y^{D^0}|D^1 = 1, D^0 \neq 1] \equiv LATE_1 \quad (12)$$

The first equation (Equation (10)) yields $LATE_0$, the average effect of attending a non-competitive school for compliers who are crowded out from either a selective or a most-selective school. Equation (11) implies that, conditional on $C = 2$, the IV estimator using the reform as an instrument yields the average effect of attending the most-selective school for compliers relative to their own counterfactual

[41] Similar to Assumption 3, Assumption 4 also places certain preference ordering restrictions for students above the admission cutoffs. It implicitly assumes that the top students ($C = 2$) prefer the most selective schools over the rest of the schools (the selective and noncompetitive ones), and that the students with $C = 1$ prefers the selective schools over the noncompetitive schools.

school choice in the absence of the reform (which could be either a non-competitive school or a selective school). We label this quantity by $LATE_2$. Finally, Equation (12) yields $LATE_1$, the average effect of attending a selective school for compliers who are either crowded out from a most selective school or pulled into the selective school from a non-competitive school.[42]

Comparing equation (10) with equation (2), it is easy to see that $LATE_0$ coincides with the average treatment effect for the crowded-out compliers ($LATE_{CC}$) (in Proposition 1 in Section 4.2). Therefore, when we allow for additional heterogeneity, $LATE_{CC}$ can be interpreted as a weighted average of the effects of being excluded from the most selective and selective schools, for compliers who are crowded out to non-competitive schools.

The interpretation of $LATE_{CP}$ is more complex. The average treatment effect for the pulled-in compliers ($LATE_{CP}$ in Proposition 1) is a weighted average of $LATE_1$ and $LATE_2$,

$$LATE_{CP} = S_1 LATE_1 + S_2 LATE_2 \tag{13}$$

with the weights given by

$$S_1 = \frac{(P(D=1|Z=1,C=1) - P(D=1|Z=0,C=1))s_c}{P(D=\{1,2\}|Z=1,C=\{1,2\}) - P(D=\{1,2\}|Z=0,C=\{1,2\})} \tag{14}$$

$$S_2 = \frac{(P(D=2|Z=1,C=2) - P(D=2|Z=0,C=2))(1-s_c)}{P(D=\{1,2\}|Z=1,C=\{1,2\}) - P(D=\{1,2\}|Z=0,C=\{1,2\})} \tag{15}$$

where $s_c = \frac{P(C=1)}{P(C=\{1,2\})}$. While $LATE_2$ measures the effects of attending the most selective school for pulled-in compliers relative to their counterfactual school choices, $LATE_1$ measures the effects of attending the selective school, relative to the relevant set of counterfactual alternatives which may include either the non-competitive school or the most selective school. If the effects of attending the selective and most selective schools are homogeneous (i.e., $Y^2 = Y^1$), then $LATE_{CP}$ measures the effects for pulled-in compliers, namely, those who switch to $D = 1$ (or 2) from $D = 0$ when the instrument is turned on. If there is heterogeneity within the set of competitive schools, then in general

[42] Each of the $LATE_j$'s is a weighted average of the effects of attending school type j for compliers drawn from specific counterfactual alternatives. Without additional data on the students' ranking of their next-best alternatives or putting additional structure on their preferences, we are not able to separately identify the effects for compliers with a specific counterfactual choice. For papers that separately identify the treatment effects for compliers with a specific counterfactual choice, see Kirkeboen, Leuven, and Mogstad (2016) (using ranking data), Kline and Walters (2016) and Chan, Dalla-Zuanna, and Liu (2022) (using a parameterized choice model).

$LATE_{CP}$ can no longer be interpreted as the effects for pulled-in compliers alone, as it will also include the effects for a specific set of crowded-out compliers, those who are crowded out to the selective schools from the most selective schools after the reform.

We can leverage additional variation provided by the cutoff for the most selective schools to identify $LATE_2$, the effects of attending the most selective school for those pulled into the most selective school from selective or non-competitive school. Invoking Assumption 4, the causal estimand for $LATE_2$ is given by Equation (11).[43] Table 9 reports the estimates for this specific type of pulled-in compliers, using both the RDD approach and the before-after comparison. For both approaches, we find that students who gain access to the most competitive schools have worse outcomes in terms of university enrolment and completion. The magnitude of the loss is in fact comparable to the loss experienced by the crowded-out compliers ($LATE_{CC}$). One interpretation of these findings is related to increased mismatch between students and very competitive schools (as shown in other contexts, see e.g. Arcidiacono and Lovenheim, 2016; Dillon and Smith, 2020; Riehl, 2022), where the pulled-in compliers may be underprepared relative to the academic requirement at the top 2 schools in certain dimensions. In Appendix Table A5, we report the effects separately by parental background and gender. We find that the negative effects of attending the top 2 schools for the set of pulled-in compilers are larger for girls and (to some extent) students of high SES background. The negative effect for girls could be explained by women being negatively affected in a highly competitive environment (Niederle and Vesterlund, 2007; Almås, Cappelen, Salvanes, Sørensen, and Tungodden, 2016).

For a social planner caring about aggregate education outcomes, crowded-out students should have enrolled in competitive schools as their gains from attending these schools are large and positive; pulled-in students, on the contrary, should have not moved to the most selective schools because their gains from attending these schools are mostly negative. Therefore, our evidence suggests that the reform creates a misallocation problem between students and schools, at least in the short run.

[43] We estimate these parameters by following our empirical specification discussed in Section 5.2. For instance, in the RDD specification, we estimate equations (4) by redefining $Below_i$ and $Above_i$ based on the cutoff of the top 2 schools. β_{CP} from this regression is the reduced-form effect.

8 Validating Identification Assumptions

The econometric model displayed in Section 4 relies on Assumptions 1–3 (exclusion restriction, independence and conditional monotonicity). In this section, we provide additional discussion and empirical evidence that in the framework we build these assumptions hold.

8.1 Independence

Assumption 2 implies that students affected by the reform are comparable to those not affected by the reform. In the RDD approach, this translates to an identifying assumption that students who are born just after December 31st, 1988 are comparable to those born few days before. We can thus analyze whether students born around this date are similar, by investigating whether the characteristics of individuals born before and after this date are significantly different. A test like this resembles tests which aim at assessing if randomization has been properly conducted in randomized experiments (Lee and Lemieux, 2010). In Figure A7 we plot the average characteristics of students born in each week around December 31st, 1988, in terms of gender and parental background. The figures show no clear jump in the first week of 1989. In addition, we estimate parameters of a regression of all these characteristics on a dummy for being born from 1989 onwards and linear time trends in the week of birth, allowed to differ before and after the last week of 1988. The estimated coefficients for the dummy at the discontinuity are reported on top of each figure, they are small and never statistically significant, providing evidence that the assumption of no differences between students affected and those not affected by the reform holds.

8.2 Exclusion Restriction

The exclusion restriction (Assumption 1) implies that our instrument, the reform, only affects students' outcomes by changing their allocation to schools. However, as we documented in Section 5.1, the reform also leads to changes to student composition, which could impact students' outcomes directly. In this section, we investigate and empirically assess the potential threats to our exclusion restriction.

One concern is that changes to student composition may affect school resources in terms of teacher quality, class size and facilities. In Norway, schools are centrally financed and resources do not depend on the quantity or quality of students, hence the reform did not affect school finances. Class size is also regulated. In addition, other characteristics, such as schools' location, facilities, or reputation, are also unlikely to change in the first few years after the reform. Although teachers can decide to move between schools, in Figure A2 we show that the characteristics of teachers (age and qualifications) in competitive and non-competitive schools did not change systematically with the reform.

Although average within-school teacher quality as measured by their observed qualifications does not change after the reform, this does not exclude the possibility that the same teachers adapt their teaching style to changing student composition. For example, teachers may be more or less effective in their classes when facing more homogeneous classrooms in terms of underlying ability (Duflo, Dupas, and Kremer, 2011). Teaching styles are not observed, and therefore we cannot rule out that teaching style may respond to class composition, violating the exclusion restriction.

Changes in student composition may impact student outcomes through other channels operating via the general peer effects. It is important to note that we do not need to assume that no peer effects is at play, but that changes to peer quality at school level had little direct impact on student outcomes. In what follows we present three pieces of evidence, showing that changing peer quality may not be empirically important in our context.

First, we show that there exists pre-reform differences between competitive and non-competitive schools beyond differences in student composition, hence changes in this composition alone should not fully explain the estimated effect of attending a more competitive school. We estimate high schools' value added (VA) for the years before the reform, by regressing final year high school test scores on school fixed effects and students' middle-school GPA (the school fixed effects identify VA).[44] The VA estimates tell us whether students in the competitive schools have better outcomes once we control for initial student quality and sorting into these schools. We then estimate the same VA model including also controls for peers' background, to see how much of the difference between schools can be explained by the type of students enrolled in the school. In Figure A3 we show that three out of the five competitive schools are also among the ones with the highest VA, something which cannot be explained by peer composition. Although our definition of a competitive school is based upon post-reform admission

[44]Note that here we use only pre-reform data. Post-reform, we would be able only to observe high school GPA for students who complete high school, so any effect of the reform on high school completion would affect the distribution of high school GPA and would then be difficult to separate any change in VA due to the reform and the one due to changes in the probability of completing high school. For the same reason, we do not include high school GPA as one of the outcomes in our analysis.

cutoffs, this additional result reassures that there were prevailing differences in "productivity" between the competitive schools and non-competitive schools, and that these are not fully explained by students' composition.

Second, we show that our results change little when we control for predicted peer composition directly in our estimating equation. This means that the variation in school attendance which is unrelated to (predicted) peer quality explains the larger part of the estimated effect. Following Kirkeboen, Leuven, and Mogstad (2016), predicted peer quality for students assigned to competitive schools is defined as the average middle school GPA of students who actually attend a competitive school, while predicted peer quality for students not assigned to a competitive school is the average middle school GPA for all non-competitive high school students. We assign students to competitive and non-competitive schools on the basis of the allocation rule which was in place when they had to choose their high school (and not on the actual realization). Hence, post-reform, all students above their relevant admission cutoff are assigned to competitive school. Pre-reform assignment depends on the middle school attended.[45] If at least 60% of the students who then move to academic high school from the same middle school enrol in a competitive high school, then we consider *all* students attending that specific middle school as being assigned to a competitive high school. Once we control for the main effects of middle school GPA, location and time, predicted high school is an exogenous variable. We report the estimates of our model (Equation (4)) when adding predicted peer quality as a control in Table A6; similar to baseline results, estimated treatment effects for CC is negative for about 35 p.p. both in university enrolment and completion, while for CP we do not estimate any statistically significant effect. This additional evidence also points to the fact that the effect of the school allocation reform is not explained by changes in peer quality.

Our exclusion restriction is violated if students who do not change their potential school choice, namely the Always Takers and Never Takers, are affected by the reform. As our final piece of evidence, we show that the reform does not affect specific groups of students who are likely to be Always Takers and Never Takers. We consider two subpopulations for which the reform is unlikely to affect their school choice: (1) students who have a middle school GPA above the 75th percentile of the distribution (who are always above the cutoff) and who enrolled at a middle school within 1.5 kilometres of a competitive

[45] Ideally, we would like to use catchment areas to predict peer composition pre-reform,but, as mentioned, we do not have this information and we thus proxy it based on the middle school attended

high school (and thus are likely to be within the catchment area of a competitive high school) have the possibility to attend a competitive high school irrespective of whether or not they are affected by the reform[46]; (2) students who have a middle school GPA below the 15th percentile of the distribution and who enrolled at a middle school that is more than five kilometres away from any competitive high school, instead, are likely to never enroll in a competitive high school. We estimate the aggregate effect of the reform for each subpopulation separately, using the RDD framework.[47] Column (1) of Table 10 shows that, as expected, when we do not restrict the distance from the competitive high schools, the probability of enrolling in one of these schools increases for the first subpopulation and decreases for the second. Column (2) shows that this effect is much smaller and not significant when we impose the distance limitation, hence suggesting that the reform does not impact them by changing their high school decisions. The estimates of the effect of the reform are generally small and never statistically significant, suggesting no clear effects for either group.

8.3 Middle School GPA Distribution

Our empirical regression includes middle school GPA (in percentiles) as controls; middle school GPA also determines whether a student is assigned above or below an admission cutoff. Our strategy therefore requires that the middle school GPA is exogenous to the reform. For instance, if exams are not blindly graded, teachers' grading practice may change in response to the reform.[48] Middle school GPA can also be endogenous to the reform for other reasons, such as families selecting a different middle school to boost their children's grades or certain students exerting more effort to obtain access to their preferred school.

We argue that the institutional setting and the relative short time span we consider alleviate some of these concerns. The cohorts under study already started middle school at the time of the reform, making it unlikely that parents move their children to a different middle school in order to boost their GPA. As explained in Appendix A.1, although part of the GPA consists of teacher-graded exams which

[46]We selected 1.5km because, as shown in section 6 even among students attending a middle school within 3km from a competitive schools there is a significant fraction of pulled-in compliers. Our results thus rely on a small sample of students (419), hence the estimates cannot be very precise and this evidence is to be taken as indicative.

[47]We estimate a linear regression where the outcome variables are regressed on a dummy for being born after December 1988 (and hence attending high school after the reform took place), a linear trend in week of birth, allowed to change before and after the reform, and seasonal dummies.

[48]As an example, a teacher may inflate grades for certain students aiming for competitive schools.

are not blindly graded, GPA is also determined by national exams which are blindly graded externally. Therefore, teachers only had control over part of the final GPA, with the rest determined by externally graded exams. Furthermore, the teacher assessment is given prior to the national exams, so teachers do not know the grade of the students in the blindly assessed part when giving their marks. In addition, the merit based system was not in place before the reform, hence in the first few years of the reform teachers (and students) operate under lots of uncertainty, in terms of where the admission cutoff is and also which schools are going to be oversubscribed. Therefore, the lack of information at the time of teacher assessment means that teachers are very limited (if not impossible) to change their grading practice to affect the admission outcome for certain students.[49]

We perform two empirical tests to further justify our assumption that middle school GPA is exogenous to the reform. As our first test, we compare the distribution of middle school GPA for students in Bergen before and after the reform, relative to the distribution of grades at the national level. If teachers in Bergen manipulate grades, or if students exert more effort and there is an overall improvement in middle school grades, we should observe changes in the GPA distribution in Bergen relative to the national distribution. We thus plot the densities of the nationally-ranked middle school GPA in Bergen before and after 2005 in panel (a) in Figure 4. These distributions overlap and a Kolmogorov–Smirnov test excludes any significant difference. Similarly, in panels (b) and (c), we plot the same distributions for cohorts affected and not affected by the reform in Bergen, separately for individuals of different SES. Plots and formal tests exclude any difference between pre- and post-reform cohorts, suggesting that the reform had no systematic effect in boosting students' middle school GPA.

Our second test provides more direct evidence showing that teachers do not change their grading style following the reform. Considering that grade inflation is only a potential issue with teacher assessment but not the national tests (as the latter is blinded graded), we estimate the relationship between national test scores and middle-school GPA and test whether the relationship between middle-school GPA and national test scores differs systematically after the reform. For instance, if teachers systematically inflate their grades for the best students after the reform, we should expect the gradient of middle-school GPA with respect to national test score to become steeper. To this end, we regress the within-cohort rank of middle-school GPA on national test scores and interact post reform dummy

[49]This stands in contrast to the case in Diamond and Persson (2016), where admission cutoff is known to the teacher at the time when the exams are graded.

with national test scores. The results from this regression is reported in Table A7. The interaction term between post reform dummy and national test score is small and insignificantly different from zero, suggesting that a stable relationship between national test (blinded graded) and middle school GPA (which includes teacher assessment). This additional evidence provides further empirical support that there appears no systematic grade inflation following the reform.

9 Conclusion

This paper analyzes the effect of reforming the high school admission system from a residence based allocation to a merit-based allocation. In a context where the supply of seats in each school is fixed and certain schools are better than others, the merit-based system generates a number of oversubscribed schools, which tend to favor high-ability students at the expense of displacing low-ability ones.

We analyzed this problem using a formal potential outcome framework where the reform is an instrumental variable and the implied admission cutoffs define subpopulations. We exploited the reform as an instrument in different subpopulations to identify parameters describing the effects for those gaining access to competitive schools and those losing access. Importantly, in addition the standard IV assumptions, we clarified the additional conditional monotonicity assumption that is needed for this strategy to identify the different effects. We believe that our framework can be used to identify the effect of changing allocation rules in other contexts with a rival good under fixed capacity, such as allocation of public housing or health treatments.

We find that the implementation of a meritocratic system in assigning students to more competitive schools leads to a small and negative aggregate effect. We show that this is because attending competitive schools has little impact on high-ability students who gain access thanks to the reform, but has a positive effect on crowded-out low-ability students. The latter group of students would in fact benefit from a system which *favours* their access to more competitive schools. Extending our analysis by distinguishing competitive schools by their selectivity, we show that students (especially girls) who are pulled into the most competitive schools (top 2) also have worse education outcomes. Therefore, although the reform levels up access to competitive schools by parental background, it creates a misallocation problem between students and schools at least in the short run. These 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). The evidence we provide confirms that these features may be relevant when deciding whether to implement policies which increase school choice and that such policies may not be as good as "a tide that lift all boats" (Hoxby, 2003).

It is important to note that our analysis provides an assessment on the implications from changes in allocation rule in the short run. In the long run, there may be other adjustment margins at both school and family level which may become more relevant.[50] Another feature of the school system of most European countries (with Norway being no exception) is the fact that these are heavily regulated markets, where there is no real space for competition between institutions. In the Bergen case, not only schools had fixed capacity and hence no incentive to attract more students, but also resources are not allocated on the basis of students' performance. As a consequence, the incentives for schools to improve their quality and attractiveness as a consequence of increased school choice, which might be important in other contexts, are small in our setting.

[50] For instance, knowing a merit-based high school admission in the future, young parents may adjust their parental investment or choose a good middle school for their children.

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	$C=0$		$C=1$	
	$Z=0$	$Z=1$	$Z=0$	$Z=1$
Never Takers (NT)	D=0	D=0	D=0	D=0
Compliers – Crowded-out (CC)	D=1	D=0	D=1	D=1
Compliers – Pulled-in (CP)	D=0	D=0	D=0	D=1
Always Takers (AT)	D=1	D=1	D=1	D=1

Table 1: Compliance types identified by the reform conditional on being above or below admission cutoff, given assumptions (1)–(3)

	(1) School Completion	(2) University Enrolment	(3) University Completion	(4) School Completion	(5) University Enrolment	(6) University Completion
Bergen × Post	-0.018	-0.010	-0.014			
	(0.018)	(0.020)	(0.021)			
Bergen × Post × Quintile 1				-0.039*	-0.036	-0.016
				(0.020)	(0.022)	(0.024)
Bergen × Post × Quintile 2				-0.030	-0.017	-0.034
				(0.019)	(0.021)	(0.023)
Bergen × Post × Quintile 3				-0.009	-0.009	-0.007
				(0.019)	(0.021)	(0.023)
Bergen × Post × Quintile 4				-0.018	-0.013	-0.019
				(0.019)	(0.021)	(0.023)
Bergen × Post × Quintile 5				-0.010	0.002	-0.010
				(0.019)	(0.021)	(0.022)
N	22,427	22,427	22,427	22,379	22,379	22,379

Table 2: Effect of the reform, separately for each middle school GPA quintile

Notes : Estimates of parameters of a diff-in-diff regression of students' outcome, where we compare the difference in outcomes between pre- and post-reform cohorts of academic high school students in Bergen to the same difference in other municipalities (Trondheim, Stavanger, Drammen and Kristiansand) where no changes in the high school allocation system happened. "Bergen" is a dummy for attending high school in Bergen area, "Post" is a dummy for starting high school after 2005, the year of the Bergen reform. The "Quintile" variables refer to quintiles of the middle school GPA distribution. The regression in columns (1) – (3) include controls for municipality fixed effects, cohorts fixed effects and municipality-specific linear trends in the month of birth. The regression in columns (4) – (6) includes the same controls and controls for the baseline effects of being within each quintile of the middle school GPA distribution.* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Competitive School	-0.246***	0.066	0.052	0.151***	0.210***
Enrolment	(0.036)	(0.047)	(0.042)	(0.047)	(0.049)
High School	-0.077*	-0.003	0.004	0.031	-0.006
Completion	(0.041)	(0.029)	(0.022)	(0.020)	(0.018)
University	-0.066	0.022	-0.035	-0.004	-0.031
Enrolment	(0.042)	(0.037)	(0.027)	(0.029)	(0.024)
University	-0.068	-0.018	-0.008	0.005	-0.020
Completion	(0.050)	(0.038)	(0.033)	(0.032)	(0.021)

Table 3: Effect of the reform, separately for each middle school GPA quintile

Notes : Estimates of parameters of an RD regression where students' outcome is regressed on a dummy for being born after December 31^{st}, 1988 (thus affected by the reform) and a linear trend in week of birth, allowed to be different for cohorts affected and not affected by the reform (with weeks of birth normalized to 0 on the first week of 1989). These control variables are interacted with dummies for the student belonging to each of the 5 quintiles of the middle school GPA distribution in the respective cohort. The regressions also include controls for seasonal dummies for the week of birth within the year (52 dummies). S.e. clustered at the week of birth level. The reported numbers are the estimates and the s.e. of the parameters on the interaction between the dummy for being born after December 31st, 1988 and the dummies for each quintile, as indicated in the top row. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Crowded-out Compliers		Pulled-in Compliers	
First Stage	0.134***		0.138***	
	(0.028)		(0.021)	
	R.F.	LATE	R.F.	LATE
School Completion	-0.012	-0.089	-0.004	-0.029
	(0.023)	(0.171)	(0.012)	(0.090)
University Enrolment	-0.052**	-0.391*	-0.021	-0.153
	(0.026)	(0.203)	(0.016)	(0.119)
University Completion	-0.059*	-0.438*	-0.001	-0.007
	(0.031)	(0.254)	(0.018)	(0.130)
N	6,908			

Table 4: First stage, R.F. and LATE for CC and CP

Notes : Local average treatment effects (LATE) are estimated as the ratio between the Reduced Form (R.F.) and the First Stage. Estimates of the Reduced Form (First Stage) come from a regression of the outcome (dummy for attending a competitive school) on two dummies, one for being born after December 31^{st}, 1988 and having middle school GPA below the relevant cutoff for admission at a competitive school and another for being born after December 31^{st}, 1988 and having middle school GPA above the cutoff. In addition, each regression controls for a quadratic function of middle school GPA percentiles, a linear trend in week of birth (with weeks of birth normalized to 0 for those born in the first week of 1989 and the trend allowed to differ before and after the reform) interacted with the quadratic function of GPA percentiles, seasonal (52 weeks) dummies and five dummies indicating whether each competitive school is located within 3km of the attended middle school. S.e. of Reduced Form and First Stage are clustered at the week-of-birth level. LATE s.e. are estimated using the delta method. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Crowded-out Compliers				Pulled-in Compliers			
	(a) Close		(b) Far		(a) Close		(b) Far	
First Stage	0.339***		0.038		0.116***		0.155***	
	(0.039)		(0.026)		(0.024)		(0.024)	
	R.F.	LATE	R.F.	LATE	R.F.	LATE	R.F.	LATE
School Completion	-0.054	-0.160	0.007	0.180	-0.021	-0.184	0.009	0.058
	(0.035)	(0.106)	(0.026)	(0.685)	(0.015)	(0.135)	(0.013)	(0.087)
University Enrolment	-0.104***	-0.306***	-0.027	-0.726	-0.015	-0.132	-0.025	-0.161
	(0.039)	(0.114)	(0.030)	(0.891)	(0.020)	(0.172)	(0.018)	(0.116)
University Completion	-0.095**	-0.282**	-0.041	-1.074	0.007	0.063	-0.007	-0.044
	(0.042)	(0.128)	(0.034)	(1.209)	(0.020)	(0.176)	(0.020)	(0.127)
N				6,908				

Table 5: First stage, R.F. and LATE for CC and CP, interacted with schools distance

Notes : Local average treatment effects (LATE) are estimated as the ratio between the Reduced Form (R.F.) and the First Stage. Estimates of the Reduced Form (First Stage) come from a regression of the outcome (dummy for attending a competitive school) on two dummies, one for being born after December 31^{st}, 1988 and having middle school GPA below the relevant cutoff for admission at competitive school and another for being born after December 31^{st}, 1988 and having middle school GPA above the cutoff. Each of these is interacted with a dummy for having attended a middle school which is within 3 km from a competitive school and with a dummy for having attended a middle school with no competitive schools within 3km. The estimates of the parameters for the interaction with the former are reported in columns (a) (Close), while the estimates of the parameters for the interaction with the latter are in columns (b) (Far). In addition, each regression controls for a quadratic function of middle school GPA percentiles, a linear trend in week of birth (with weeks of birth normalized to 0 for those born in the first week of 1989 and the trend allowed to differ before and after the reform) interacted with the quadratic function of GPA percentiles, seasonal (52 weeks) dummies and five dummies indicating whether each competitive school is located within 3km of the attended middle school. S.e. of Reduced Form and First Stage are clustered at the week-of-birth level. LATE s.e. are estimated using the delta method. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Crowded-out Compliers		Pulled-in Compliers	
First Stage	0.138***		0.134***	
	(0.029)		(0.021)	
	R.F.	LATE	R.F.	LATE
School	-0.014	-0.101	-0.006	-0.043
Completion	(0.023)	(0.167)	(0.012)	(0.093)
University	-0.054**	-0.388**	-0.022	-0.165
Enrolment	(0.027)	(0.198)	(0.016)	(0.123)
University	-0.062**	-0.450*	-0.004	-0.031
Completion	(0.031)	(0.248)	(0.018)	(0.135)
N	6,908			

Table 6: First stage, R.F. and LATE for CC and CP, average pre-post comparison

Notes : Local average treatment effects (LATE) are estimated as the ratio between the Reduced Form (R.F.) and the First Stage. Estimates of the Reduced Form (First Stage) come from a regression of the outcome (dummy for attending a competitive school) on two dummies, one for being born after December 31^{st}, 1988 and having middle school GPA below the relevant cutoff for admission at competitive school and another for being born after December 31^{st}, 1988 and having middle school GPA above the cutoff. In addition, each regression controls for a quadratic function of middle school GPA percentiles, a linear trend in week of birth interacted with the quadratic function of GPA percentiles (but common to all cohorts), seasonal (52 weeks) dummies and five dummies indicating whether each competitive school is located within 3km of the attended middle school. S.e. of Reduced Form and First Stage are clustered at the week-of-birth level. LATE s.e. are estimated using the delta method. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1) $P[J = 1]$	(2) Crowded-out Compliers	(3) Pulled-in Compliers
Close to Competitive HS	0.39	2.52	0.84
High SES	0.42	1.19	0.88
Female	0.53	0.86	1.15

Table 7: Relative likelihood of having different characteristics, for CC and CP.

Notes : We compute the likelihood of CC and CP having characteristics $J = 1$ as $P(J = 1|D^0 = 1, D^1 = 0)/P(J = 1)$ and $P(J = 1|D^0 = 0, D^1 = 1)/P(J = 1)$, respectively. As described in Section 6.3, these corresponds to the ratio of the first stage for students with $J = 1$ to the ratio of the overall first stage. For example, for CC, we compute $\frac{P[D|Z=0,C=0,J=1]-P[D|Z=1,C=0,J=1]}{P[D|Z=0,C=0]-P[D|Z=1,C=0]}$. Column (1) shows the proportion of students with each characteristic in the population. "Close to competitive HS" are students who attended a middle school within 3km from one of the five competitive high schools. "High SES" students have both parents with a university degree.

	(1)		(2)		(3)	
	Non Parametric GPA		Week Quaratic		Months	
(a) Crowded-out Compliers						
First Stage	0.128***		0.133***		0.132***	
	(0.028)		(0.039)		(0.031)	
	R.F.	LATE	R.F.	LATE	R.F.	LATE
School Completion	0.002	0.014	0.016	0.119	-0.013	-0.102
	(0.023)	(0.178)	(0.029)	(0.223)	(0.022)	(0.167)
University Enrolment	-0.040	-0.315	-0.050	-0.379	-0.053*	-0.403*
	(0.028)	(0.219)	(0.031)	(0.246)	(0.027)	(0.215)
University Completion	-0.042	-0.328	-0.026	-0.196	-0.060*	-0.453
	(0.033)	(0.268)	(0.037)	(0.281)	(0.034)	(0.284)
(b) Pulled-in Compliers						
First Stage	0.135***		0.108***		0.133***	
	(0.021)		(0.030)		(0.022)	
	R.F.	LATE	R.F.	LATE	R.F.	LATE
School Completion	-0.009	-0.064	0.004	0.040	-0.004	-0.031
	(0.012)	(0.091)	(0.018)	(0.164)	(0.012)	(0.089)
University Enrolment	-0.024	-0.181	-0.009	-0.082	-0.020	-0.152
	(0.016)	(0.121)	(0.021)	(0.202)	(0.016)	(0.126)
University Completion	-0.005	-0.039	0.029	0.265	0.000	0.001
	(0.018)	(0.135)	(0.024)	(0.231)	(0.018)	(0.136)
N	6,908		6,908		6,908	

Table 8: Robustness checks for the main specification

Notes : Local average treatment effects (LATE) are estimated as the ratio between the Reduced Form (R.F.) and the First Stage. Each column shows estimates when using different specifications of the time trends and controls for GPA compared to the model used in our baseline analysis (Equation 4 and Table 4). Column (1) uses ten dummies for GPA deciles instead of the quadratic function of GPA percentiles. Column (2) uses a quadratic trend in week of birth instead of a linear trend. Column (3) uses controls for month of birth instead of weeks of birth. Controls always include seasonal dummies in the week of birth (52 weeks), apart from column (2), which uses months of birth dummies. S.e. for R.F. and first stage are always clustered at the week-of-birth level, apart from column (1), which uses the month-of-birth level. LATE s.e. are estimated using the delta method. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	(1) RDD Framework		(2) Pre-Post Comparison	
First Stage	0.143***		0.145***	
	(0.025)		(0.024)	
	R.F.	LATE	R.F.	LATE
School Completion	-0.009	-0.063	-0.011	-0.074
	(0.014)	(0.099)	(0.014)	(0.096)
University Enrolment	-0.053***	-0.369**	-0.052***	-0.361**
	(0.020)	(0.155)	(0.019)	(0.149)
University Completion	-0.036*	-0.252*	-0.040**	-0.279**
	(0.019)	(0.141)	(0.019)	(0.139)
N	6,177		6,177	

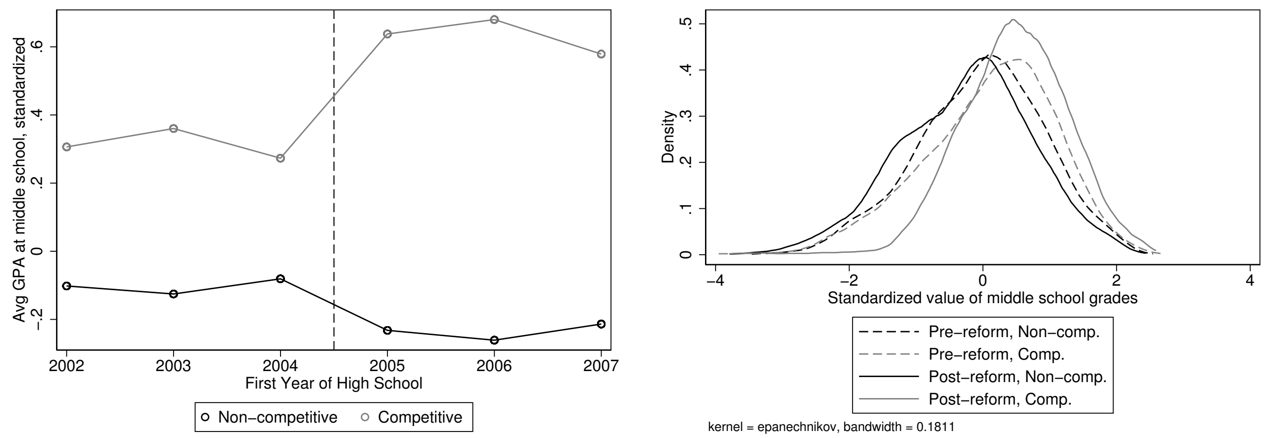
Table 9: First stage, R.F. and LATE for attending the two most selective schools

Notes : Local Average Treatment Effects (LATE) are the ratio between the Reduced Form (R.F.) and the First Stage. The estimates of the R.F. (First Stage) come from a regression of the outcome (dummy for attending one of the two more selective schools) on two dummies, one for being born after December 31^{st}, 1988 and having middle school GPA below the relevant cutoff for admission at one of the two most selective schools and another for being born after December 31^{st}, 1988 and having middle school GPA above the cutoff. In addition, each regression controls for a quadratic function of middle school GPA percentiles, seasonal (52 weeks) dummies and two dummies indicating whether each most selective school is located within 3km of the attended middle school. Columns (1) add the control for a linear trend in week of birth allowed to differ before and after the last week of 1988 (with weeks of birth normalized to 0 for those born in the first week of 1989) interacted with the quadratic function of GPA percentiles. Columns (2) add the control for a linear trend in week of birth interacted with the quadratic function of GPA percentiles. S.e. of Reduced Form and First Stage are clustered at the week-of-birth level. LATE s.e. are estimated using the delta method. The sample only includes post-reform students who attended a middle school which is within 8km from one of the two more selective schools. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

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

Table 10: Estimates of the effect of the reform on specific subgroups of the population.

Notes : Panel (a) shows the estimates for students who have a middle school GPA above the 75th percentile, panel (b) for students who have a middle school GPA below the 15th percentile. “Dist” represents the distance between middle school and the closest of the five competitive high schools. “Post” are estimates of coefficients on a dummy for being born after December 31st, 1988 and thus for being affected by the reform. Regressions also control for linear trends in week of birth, allowed to differ before and after the last week of 1988 (with weeks of birth normalized to 0 for those born in the first week of 1989), and for seasonal (52 weeks) dummies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.



(a) Average GPA

(b) GPA Distribution

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

Notes : The middle school GPA distribution in these figures is normalized to have mean 0 and s.d. 1 at the cohort level. "Competitive" schools are those high schools which we observe being oversubscribed after the reform.

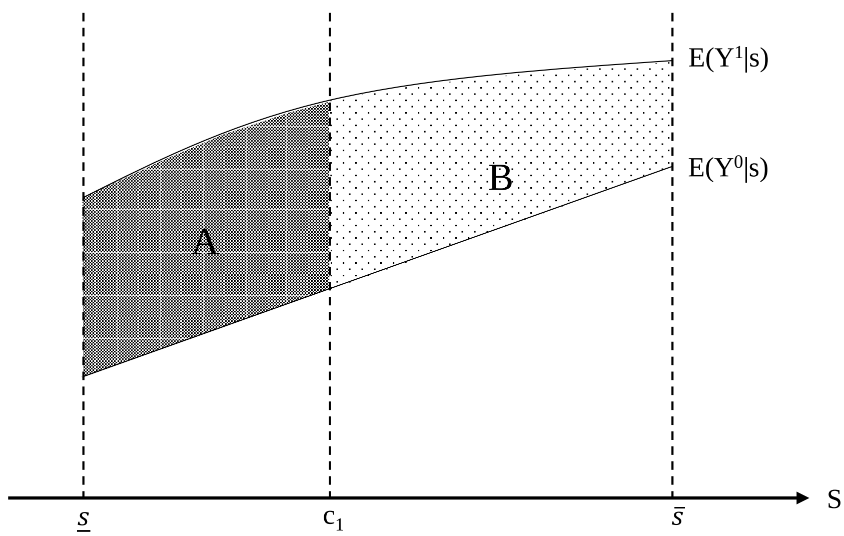
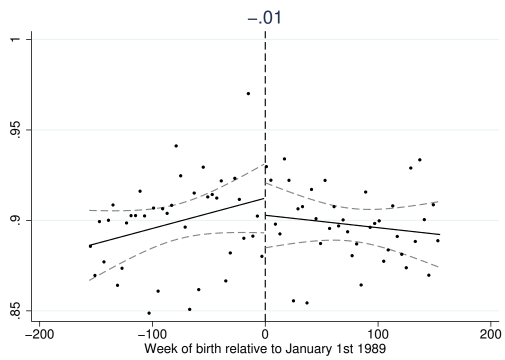
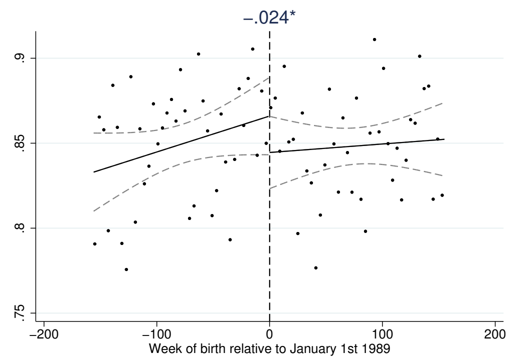


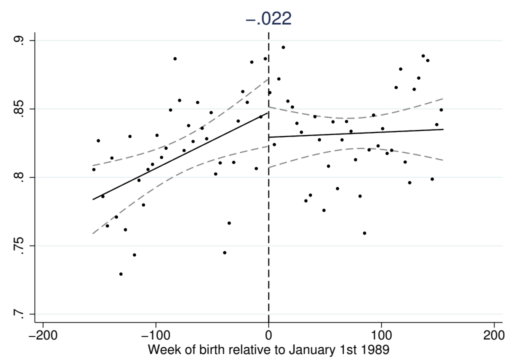
Figure 2: Total potential gains and losses from the reform: a theoretical illustration



(a) High School Completion



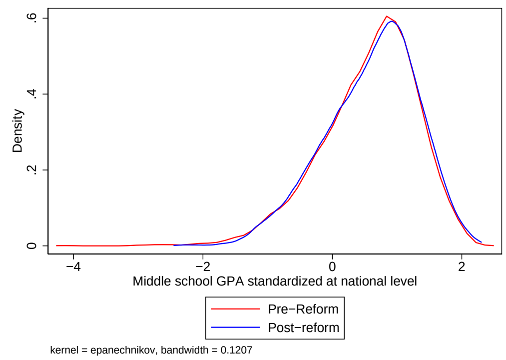
(b) University Enrolment



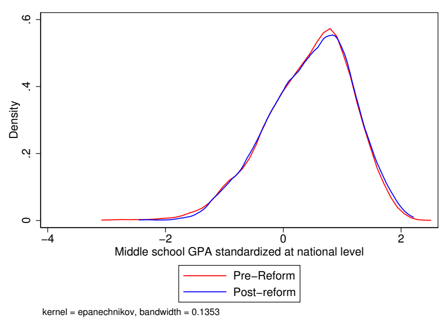
(c) University Completion

Figure 3: Average outcomes for children born in different weeks around January 1^{st}, 1989.

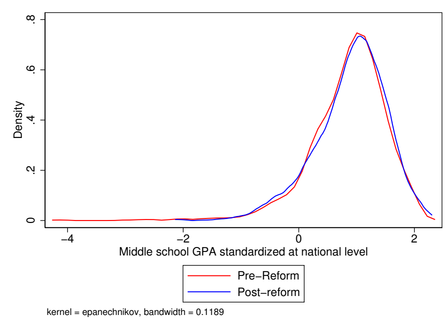
Notes : The plots show the average value of each outcome (completing high school within 4 years from the start (a), enrolling in university after high schools (b) and completing university by age 28 (c)) for students born in the different weeks around the first week of 1989. Each dot represents the average for 4 weeks, while the linear fit (solid line) uses week data. The dashed lines is the 95% confidence interval of such fit. The numbers on top of the figures are the estimates of the parameter on a dummy for being born after the cutoff date (December 31^{st}, 1988), from a regression of the outcome on the dummy for being born after the cutoff, a linear trend in the week of birth (allowed to differ before and after the cutoff, with weeks of birth normalized to 0 on the first week of 1989) and 52 dummies for being born in each week of the year. S.e. are clustered at the week-of-birth level. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.



(a) Overall



(b) Low SES



(c) High SES

Figure 4: Robustness Checks for Middle School GPA Distribution.

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

ONLINE APPENDIX

A The Norwegian education system

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

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

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

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

At the end of middle school, students are evaluated both non-anonymously by their teachers for 11 subjects taught in school, and in addition anonymously in 2 nationally administered exit exams, which are graded by external examiners (who are not students' teachers). The subjects for the national exit exams are randomly selected for each student among the 11 subjects in which they are also evaluated by their teachers. The assessment for Norwegian and English consists of both oral and written exams. For the rest of the subjects, the assessment consists of only written exams. In each assessment, the grade ranges between 1 and 6. The final grade of a subject is determined by a simple average between the grades by the teacher and grades from the national exam in the final year, if present. The middle school GPA is not standardized at the school level so they are not grading to the curve.

The final-year middle school GPA used by post-reform high school admission is the sum of final grades across 11 subjects, for students who had grades in at least 3 subjects, hence it ranges between 3 and 66 (i.e. 6×11). Note that the algorithm for calculating the final-year GPA changed in the school year 2006/2007 (hence the last year we observe). Instead of summing up the grades in 11 subject, the final-year GPA is determined by first taking a simple average across all subjects and then multiplied by 10. This means that the maximum GPA is 60 for the last cohort under study. For this reason when we use middle school GPA throughout the paper we generally refer to within-cohort GPA percentiles, assuming that the ranking of students in percentile bins is invariant to the change in the 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, the final teacher evaluation grade is given in April and is set based on the performance in the final year of middle school. 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 given *before* the grading of national exams, and hence teachers are not aware of the student's national exam score at the time when teacher assessment is given.

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 apllying in the spring of 2005 and starting high school in August 2005. Students are assigned to high schools by the county's school administrative office, which, before the 2005–2006 academic year, practically used to enrol students in the schools which had spare capacity and was closer to their home, to reduce travel time. Hence, although no fixed catchment areas were defined, in practice distance to each school determined the high school a person attended. It was still possible for very high-ability students to request to attend one specific school, but this caused only 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 in middle school, we are comfortable in considering them as always takers (see Section 4.2), hence not affecting our identification strategy.

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

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

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

C Proof of Proposition 1

In this section, we show how we can identify the effect of attending a competitive high school for crowded-out compliers (CC). The treatment effect for the pulled-in compliers (CP) can be identified following the same steps, focusing on students above the admission cutoff. Consider now the set of students below the admission cutoff ($C = 0$). The share of CC (π_{CC}), always takers (π_{AT}) and never takers (π_{NT}) can be identified from the conditional expectation of school choice (D) given Z:[51]

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

Combining the first and the last data moments, we can identify the proportion of CC: $\pi_{CC} = P(D = 1|Z = 0, C = 0) - P(D = 1|Z = 1, C = 0)$.

The second step of the identification argument is to combine the population shares by compliance types with data on outcome for subpopulations defined by the realized Z and D. For instance, the subpopulation with $Z = 0, D = 1$ is a mixture of CC compliers and always takers (AT). Given Assumptions (1)–(3) we can write the moments that characterize the mean outcome conditional on Z and D as follows:

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

[51] Note that these compliance shares are conditional on $C = 0$ and not population shares; $\pi_{CC} + \pi_{AT} + \pi_{NT} = 1$.

Inserting (3) in (1) and re-arranging allows us to identify

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

Similarly, inserting (2) in (4), we obtain

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

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, $(\pi_{NT} + \pi_{CC})$ is the probability that students do not attend a competitive school when they are below the cutoff post-reform, i.e. $P(D = 0|Z = 1, C = 0) = \pi_{NT} + \pi_{CC}$, while π_{AT} is the probability that a student attends a competitive school below the cutoff post-reform i.e. $P(D = 1|Z = 1, C = 0) = \pi_{AT}$. Hence,

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

Similarly, it is easy to show that

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

Using these results, the effect of being crowded out from the competitive school for CC is

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

D Competitive Schools' Cutoffs

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

For every school and cohort, we exclude students who attend a middle school that is more than 30 kilometers away, as they are not likely to attend the school and won't likely contribute to determine the admission cutoff. We experimented different distances, and we found that 10 or 8km changes the estimated cutoff only for one school (out of 16) in every year compared to what we find using 30km. 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 75th percentiles of the middle school GPA in 2005, between the 11th and the 75th percentiles in 2006 and between the 10th and the 55th percentiles in 2007. In Figure A6 we show that the enrolment in competitive high schools exhibits a clear jump around the estimated admission cutoffs in every year (the figure collapses together school admission around all the estimated cutoffs in the same year). Taken together, admission probability increases by 15 percentage points around the estimated cutoffs.

We then need to assign a cutoff for admission at selective schools to each student affected by the reform. We define it as the lowest cutoff that grants access to one of the competitive schools within eight kilometers from the middle school attended, in the year the student turned 16. As mentioned, 80% of the students attended a high school within this distance. Few students (about 2% of the sample) attended a competitive school beyond eight kilometers in the post-reform period, although they do have a competitive school in their eight kilometers neighborhood, hence they are assigned the "wrong" within-eight-kilometers cutoff. We treat them as always takers, assuming they would also have attended a selective high school in the pre-reform period. We experimented other definitions, changing the maximum distance between middle school and competitive high school considered to assign the "relevant" cutoffs. As mentioned in Section 5.1 we generally find that the estimated share of pulled-in compliers declines if, for example, we expand this distance to 10km, which suggests that we do not gain more compliers by allowing individuals to have schools more than 8km away in their choice set.

E Placebo Tests for GPA Control

In this section we empirically assess the validity of the assumption that differences between students with grades below and above admission cutoff are excludable once we properly control for middle school GPA in the regression. In particular, for the assumption to be true, we must have that, absent the reform, two individuals above and below the cutoff must have the same outcome once we properly control for baseline characteristics. Because being admitted to a competitive school post reform only depends on middle school GPA, our assumption relies on properly controlling for GPA. This assumption was laid out in Section 5.2 and is needed for our results to be valid, but it is not needed for the econometric model discussed in Section 4; any other empirical strategy which properly allows to compare students differently affected by the reform while conditioning on being above or below the admission cutoff can in fact be considered to recover estimates of the same model.

We exploit pre-reform cohorts (starting high school in 2002-2004), for whom the admission cutoff should have no effect since admission to competitive schools was not based on those. We assign to each student the cutoff they would have faced for admission at a competitive schools in 2005. Similar to the procedure we use to assign the relevant cutoffs in the post-reform period, for each student, we consider the lowest 2005 cutoff among the competitive schools within 8km from the attended middle school.

We then estimate the parameters of the following regression:

$$y_i = c_0 + c_1 Above_i + \mathbf{c_2 GPA_i} + Week_i \times (c_4 + \mathbf{c_5 GPA_i}) + \mathbf{c_6 X_i} + u_i, \tag{E.1}$$

using different functional forms for GPA_i. $Above_i$ is the dummy for having grades above the assigned cutoff, $Week_i$ is a linear trend in week of birth and, because cutoffs are assigned based on the distance between middle school and high school, in $\mathbf{X_i}$ we also control for dummies for being within 3km from each competitive high school.

In Table A2 we report the OLS estimates of the parameters of this regression. In particular, the null hypothesis of the cutoff being excludable once GPA is controlled for is verified if we cannot reject the hypothesis that parameter c_1 is zero. We find that controlling for a polynomial function in GPA or for GPA decile dummies passes the placebo tests (estimates of c_1 are not statistically significant at the 10% level for any outcome), whereas a linear function in GPA and GPA quintile dummies do not. Based on this result, in our analysis we control for a quadratic function of GPA, and show estimates when controlling for GPA decile dummies as robustness check.

F Empirical Specifications to Estimate the Aggregate Impact of the Reform

We describe the empirical specifications reported in Section 6.1 to estimate the aggregate effect of the reform.

Cross-markets Difference-in-difference Model The first model compares changes in outcomes in Bergen with other education markets, where no reforms took place over the same period, in a difference-in-difference framework. In particular, we include in the sample students who enrolled in academic high schools between 2002 and 2007 in Bergen (and neighbouring municipalities), and the four largest municipalities Trondheim, Stavanger, Drammen and Kristiansand excluding the capital Oslo. We estimate the following regression:

$$y_i = \alpha_0 + \alpha_1 Bergen_i \times Post_i + \alpha_{\mathbf{2}}\mathbf{Munic_i} + \alpha_{\mathbf{3}}\mathbf{Cohort_i} + \alpha_{\mathbf{4}}\mathbf{Munic_i} \times t_i + \varepsilon_i, \tag{F.1}$$

where $Bergen_i$ is a dummy which is 1 if student i started an academic high school in Bergen, $Post_i$ is a dummy for being born after 1988 (affected by the reform if in Bergen), $Munic_i$ is a vector of dummies for enrolling in high school in each of the included municipalities, $Cohort_i$ is a vector of dummies for belonging to each cohort and t_i is a linear function of the month of birth (ranging from 1 to 72). This specification is thus a standard two-way fixed effect model, where we also control for municipality-specific linear trends in the month of birth, to avoid confounding pre-existing differences in the trends of outcomes between municipalities with the impact of the reform. The effect of the reform in Bergen is captured by the estimate of parameter α_1.

Cross-markets Difference-in-difference Model, by GPA Quintiles We also analyze heterogeneity in the impact of the reform according to the grades obtained at middle school. In particular, we define five dummies, each taking value 1 if the student obtained a middle school GPA within each quintile of the middle school GPA distribution of the respective cohort (we rank students within the respective cohort, without separating between municipalities). We then estimate an equation similar to Equation F.1, this time further multiplying the interaction between $Bergen_i$ and $Post_i$ with each of these five dummies, and controlling for quintile dummies main effect. Calling $Quintile_{q,i}$ the dummy for quintile q, we thus estimate the following model

$$y_i = \alpha_0 + \alpha_{\mathbf{q,post}} \mathbf{Quintile_{q,i}} \times Bergen_i \times Post_i + \alpha_{\mathbf{2}} \mathbf{Munic_i} + \alpha_{\mathbf{3}} \mathbf{Cohort_i} + \alpha_{\mathbf{4}} \mathbf{Munic_i} \times t_i + \alpha_{\mathbf{5}} \mathbf{Quintile_{q,i}} + \varepsilon_i. \tag{F.2}$$

RDD Model Our second strategy relies on students attending an academic high school in the Bergen area and exploits an RDD framework which is similar to Equation (4), where the $Post$ dummy appears on its own (not interacted with indicators for being above or below competitive high schools' admission cutoffs) and without controlling for middle school GPA:

$$y_i = \alpha_0 + \alpha_1 Post_i + Week_i \times (\alpha_2 + \alpha_3 Post_i) + \alpha_{\mathbf{4}} \mathbf{X_i} + \varepsilon_i, \tag{F.3}$$

where $Post_i$ is a dummy for being born after December 31^{st}, 1988, $Week_i$ is a linear function of the week of birth (normalized to 0 for those born in the first week of 1989) and X_i is a vector of 52 dummies for being born in each week of the year. Hence, parameter α_1 is the difference between the outcomes of students born just before and after December 31^{st}, 1988 controlling for constant seasonal effects.

RDD Model by GPA Quintiles Also for this specification, we analyze heterogeneity in the effect of the reform by interacting the $Post_i$ dummy with dummies for quintiles of the middle school GPA. For this last piece of analysis we estimate the parameters of the following linear model:

$$y_i = \alpha_0 + \alpha_{\mathbf{q,post}} \mathbf{Quintile_{q,i}} \times Post_i + Week_i \times (\alpha_2 + \alpha_3 Post_i) + \alpha_{\mathbf{4}} \mathbf{X_i} + \alpha_{\mathbf{5}} \mathbf{Quintile_{q,i}} + \varepsilon_i. \quad \text{(F.4)}$$

G Tables and Figures

	Overall		Bottom Quintile of Middle School GPA		Top Quintile of Middle School GPA	
	(1) Pre-Reform	(2) Post-Reform	(3) Pre-Reform	(4) Post-Reform	(5) Pre-Reform	(6) Post-Reform
(a) Students Characteristics						
Parents with University	0.42	0.40	0.29	0.25	0.56	0.56
HH Earnings in 1998 NOK	375,873 (216,036)	411,738 (252,177)	329,446 (187,460)	358,976 (191,265)	425,113 (254,167)	447,744 (244,720)
Female	0.53	0.55	0.36	0.41	0.69	0.65
(b) Academic Outcomes						
Admission at 5 Comp. Schools	0.38	0.36	0.35	0.09	0.44	0.59
High School Completion	0.90	0.90	0.73	0.73	0.96	0.96
University Enrolment	0.85	0.85	0.66	0.70	0.94	0.92
University Completion	0.82	0.83	0.57	0.62	0.95	0.95
Days Absent	6.60 (4.94)	7.98 (6.16)	8.12 (5.55)	10.52 (7.08)	4.89 (3.76)	5.63 (4.26)
Elite Uni Enrolment	0.15	0.15	0.03	0.02	0.38	0.36
Elite Uni Completion	0.11	0.09	0.02	0.01	0.27	0.21
STEM Enrolment	0.16	0.15	0.12	0.12	0.15	0.16
STEM Completion	0.10	0.10	0.07	0.07	0.09	0.12
N	3,675	4,285	711	842	749	880

Table A1: Descriptive statistics and post-high school outcomes.

Notes : Columns (1) and (2) include all students not affected and those affected by the reform, respectively. Columns (3) and (4) include students whose middle school GPA is below the 20^{th} percentile of the middle school GPA distribution of their cohort. Columns (5) and (6) include students whose middle school GPA is above the 80^{th} percentile of the middle school GPA distribution of their cohort. "Parents with university" is the proportion of students whose parents (both) completed a university degree. Parental earnings are expressed in 1998 NOK (6NOK $\approx$ 1USD). S.d. for continuous variables are in parentheses.

	(a) Competitive HS Enrolment				(b) HS Completion				(c) University Enrolment				(d) University Completion			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	GPA^2	GPA 10 bins	GPA 5 bins	GPA linear	GPA^2	GPA 10 bins	GPA 5 bins	GPA linear	GPA^2	GPA 10 bins	GPA 5 bins	GPA linear	GPA^2	GPA 10 bins	GPA 5 bins	GPA linear
Above Predicted	-0.0595	-0.0339	-0.0406	-0.0221	0.0482	0.0157	0.103***	0.150***	0.0409	0.00618	0.0910**	0.136***	0.0176	-0.0262	0.0627	0.125***
Cutoff	(0.0373)	(0.0540)	(0.0505)	(0.0290)	(0.0317)	(0.0387)	(0.0368)	(0.0248)	(0.0360)	(0.0467)	(0.0430)	(0.0273)	(0.0397)	(0.0524)	(0.0481)	(0.0306)
GPA	0.00157			0.00143**	0.00375*			0.000593	0.00398*			0.000972**	0.00647***			0.00249***
	(0.00244)			(0.000595)	(0.00204)			(0.000436)	(0.00208)			(0.000395)	(0.00230)			(0.000507)
GPA^2	1.83e-06				-2.31e-05				-2.21e-05				-3.08e-05			
	(2.22e-05)				(1.69e-05)				(1.88e-05)				(1.98e-05)			
GPA bin 2		-0.113	0.0158			0.116*	0.0206			0.117	0.00239			0.126*	0.0638	
		(0.0855)	(0.0670)			(0.0669)	(0.0471)			(0.0827)	(0.0562)			(0.0753)	(0.0623)	
GPA bin 3		-0.0302	0.0580			0.133*	0.0288			0.103	0.0722			0.131	0.139**	
		(0.105)	(0.0669)			(0.0740)	(0.0480)			(0.0782)	(0.0535)			(0.0891)	(0.0607)	
GPA bin 4		-0.0620	0.101			0.161**	0.0527			0.151*	0.0488			0.272***	0.181***	
		(0.105)	(0.0628)			(0.0720)	(0.0477)			(0.0866)	(0.0458)			(0.0925)	(0.0588)	
GPA bin 5		-0.0360	0.122*			0.159**	0.0808*			0.185**	0.110**			0.244***	0.237***	
		(0.0990)	(0.0638)			(0.0703)	(0.0486)			(0.0820)	(0.0461)			(0.0868)	(0.0566)	
GPA bin 6		0.0335				0.157**				0.217***				0.309***		
		(0.108)				(0.0777)				(0.0719)				(0.0852)		
GPA bin 7		0.0224				0.181**				0.205***				0.288***		
		(0.0902)				(0.0700)				(0.0735)				(0.0852)		
GPA bin 8		0.0566				0.185***				0.152**				0.345***		
		(0.0968)				(0.0704)				(0.0718)				(0.0810)		
GPA bin 9		0.0766				0.209***				0.211***				0.362***		
		(0.104)				(0.0711)				(0.0704)				(0.0796)		
GPA bin 10		0.0482				0.212***				0.261***				0.383***		
		(0.0946)				(0.0698)				(0.0689)				(0.0780)		
Constant	0.256***	0.321***	0.266***	0.240***	0.756***	0.740***	0.785***	0.756***	0.713***	0.704***	0.749***	0.714***	0.618***	0.626***	0.676***	0.628***
	(0.0494)	(0.0638)	(0.0385)	(0.0353)	(0.0450)	(0.0541)	(0.0367)	(0.0322)	(0.0445)	(0.0547)	(0.0356)	(0.0314)	(0.0490)	(0.0589)	(0.0407)	(0.0367)
N	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816	2,816

Table A2: Placebo test for GPA functional form

Notes : Parameter estimates of regressions of four different outcomes (attending a competitive high school (Panel (a)), completing high school (Panel (b)), enrolling at university (Panel (c)) and completing university (Panel (d))) on a dummy for being above the relevant admission cutoff for competitive high schools for cohorts enrolling in high school in the three years before 2005. The relevant cutoff is the lowest 2005 cutoff for competitive high schools which are within 8km from the attended middle school. All regressions control for baseline middle school GPA exploiting different functional forms. For each outcome, the first column uses a quadratic form the GPA percentile (where percentiles are defined as 100 ordered bins of the distribution of middle school GPA within the student's cohort). Second columns use non parametric controls for dummies for 10 bins (deciles) of the distribution of middle school GPA within the student's cohort and third columns controls for dummies for 5 bins (quintiles). Fourth columns control for a linear function of GPA percentiles. Each regression controls also for linear time trends (in weeks of birth), the interaction of linear time trends and the functional form of middle school GPA and five dummies indicating whether each competitive school is located within 3km of the attended middle school (all these controls are the same as the ones in the estimated equations in the main analysis). Similar to our main analysis, s.e. are clustered at the week of birth level. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Crowded-out Compliers		Pulled-in Compliers	
First Stage	0.134***		0.138***	
	(0.028)		(0.021)	
	R.F.	LATE	R.F.	LATE
Avg Days of Absence[a]	0.506	4.905	0.155	1.082
	(0.459)	(4.695)	(0.272)	(1.909)
Elite Uni Enrolment	-0.027	-0.204	0.013	0.093
	(0.017)	(0.133)	(0.016)	(0.116)
Elite Uni Completion	-0.035**	-0.264**	0.004	0.032
	(0.015)	(0.122)	(0.014)	(0.102)
STEM Uni Enrolment	-0.040*	-0.299*	0.000	0.001
	(0.021)	(0.169)	(0.017)	(0.124)
STEM Uni Completion	-0.029	-0.213	-0.009	-0.064
	(0.018)	(0.139)	(0.014)	(0.101)
N	6,908			

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

Table A3: Reduced form and treatment effects for CC and CP, additional outcomes

Notes : "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 5.1). "STEM" refers to attending any course in a STEM field. LATE are estimated as the ratio between the R.F. and the First Stage. Estimates of the Reduced Form (First Stage) come from a regression of the outcome (dummy for attending a competitive school) on two dummies, one for being born after December 31^{st}, 1988 and having middle school GPA below the relevant cutoff for admission at competitive school and another for being born after December 31^{st}, 1988 and having middle school GPA above the cutoff. In addition, each regression controls for a quadratic function of middle school GPA percentiles, a linear trend in week of birth (with weeks of birth normalized to 0 for those born in the first week of 1989 and the trend allowed to differ before and after the reform) interacted with the quadratic function of GPA percentiles, seasonal (52 weeks) dummies and five dummies indicating whether each competitive school is located within 3km of the attended middle school. S.e. of Reduced Form and First Stage are clustered at the week-of-birth level. LATE s.e. are estimated using the delta method. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

Panel 1: Parental Background								
	Crowded-out Compliers				Pulled-in Compliers			
	(a) Low SES		(b) High SES		(a) Low SES		(b) High SES	
First Stage	0.118***		0.160***		0.151***		0.121***	
	(0.028)		(0.038)		(0.022)		(0.024)	
	R.F.	LATE	R.F.	LATE	R.F.	LATE	R.F.	LATE
School Completion	-0.010	-0.084	0.004	0.029	-0.009	-0.054	-0.015	-0.121
	(0.026)	(0.224)	(0.014)	(0.093)	(0.032)	(0.203)	(0.014)	(0.118)
University Enrolment	-0.055*	-0.465*	0.006	0.041	-0.024	-0.152	-0.056***	-0.462***
	(0.031)	(0.268)	(0.019)	(0.126)	(0.036)	(0.229)	(0.017)	(0.172)
University Completion	-0.068**	-0.573*	0.011	0.072	-0.024	-0.149	-0.017	-0.138
	(0.034)	(0.333)	(0.020)	(0.136)	(0.041)	(0.264)	(0.019)	(0.159)
N	6,908							

Panel 2: Gender								
	Crowded-out Compliers				Pulled-in Compliers			
	(a) Men		(b) Women		(a) Men		(b) Women	
First Stage	0.150***		0.115***		0.114***		0.159***	
	(0.032)		(0.031)		(0.024)		(0.023)	
	R.F.	LATE	R.F.	LATE	R.F.	LATE	R.F.	LATE
School Completion	-0.019	-0.128	0.014	0.120	0.005	0.040	-0.018	-0.115
	(0.026)	(0.178)	(0.014)	(0.126)	(0.030)	(0.258)	(0.014)	(0.090)
University Enrolment	-0.063**	-0.417**	0.001	0.007	-0.030	-0.260	-0.039**	-0.244**
	(0.030)	(0.212)	(0.019)	(0.170)	(0.036)	(0.310)	(0.018)	(0.117)
University Completion	-0.044	-0.296	0.017	0.154	-0.074*	-0.638*	-0.017	-0.110
	(0.035)	(0.247)	(0.022)	(0.196)	(0.040)	(0.376)	(0.019)	(0.119)
N	6,908							

Table A4: First stage, R.F. and LATE for CC and CP by Parental Background and by Gender

Notes : Local average treatment effects (LATE) are estimated as the ratio between the Reduced Form (R.F.) and the First Stage. Estimates of the Reduced Form (First Stage) come from a regression of the outcome (dummy for attending a competitive school) on two dummies, one for being born after December 31^{st}, 1988 and having middle school GPA below the relevant cutoff for admission at a competitive school and another for being born after December 31^{st}, 1988 and having middle school GPA above the cutoff. Panel 1 reports the estimates of a regression where we interact each of these with a dummy for having two parents with a university degree (our definition of high SES) and with a dummy for having at least one parent with no university degree (low SES). Panel 2 reports the estimates of a regression where the two treatment dummies are interacted with a dummy for being male and, in addition, with a dummy for being female. Each regression controls for a quadratic function of middle school GPA percentiles, a linear trend in week of birth (with weeks of birth normalized to 0 for those born in the first week of 1989 and the trend allowed to differ before and after the reform) interacted with the quadratic function of GPA percentiles, seasonal (52 weeks) dummies and five dummies indicating whether each competitive school is located within 3km of the attended middle school. S.e. of Reduced Form and First Stage are clustered at the week-of-birth level. Treatment effect s.e. are estimated using the delta method. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Panel 1: Parental Background				Panel 2: Gender			
	(a) Low SES		(b) High SES		(a) Men		(b) Women	
First Stage	0.140***		0.143***		0.133***		0.152***	
	(0.026)		(0.030)		(0.030)		(0.027)	
	R.F.	LATE	R.F.	LATE	R.F.	LATE	R.F.	LATE
School Completion	-0.001	-0.005	-0.018	-0.124	0.014	0.103	-0.025	-0.162
	(0.016)	(0.116)	(0.016)	(0.110)	(0.017)	(0.134)	(0.015)	(0.105)
University Enrolment	-0.027	-0.191	-0.080***	-0.556***	-0.024	-0.179	-0.074***	-0.485***
	(0.023)	(0.168)	(0.022)	(0.204)	(0.024)	(0.188)	(0.022)	(0.169)
University Completion	-0.031	-0.219	-0.041**	-0.289*	-0.010	-0.076	-0.054***	-0.353**
	(0.022)	(0.167)	(0.020)	(0.149)	(0.023)	(0.175)	(0.020)	(0.150)
N	6,177							

Table A5: First stage, R.F. and LATE for attending the two most selective schools, heterogeneity by SES and gender

Notes : LATE are the ratio between the R.F. and the First Stage. The estimates of the R.F. (First Stage) come from a regression of the outcome (dummy for attending one of the two more selective schools) on two dummies, one for being born after December 31st, 1988 and having middle school GPA below the relevant cutoff for admission at one of the two most selective schools and another for being born after December 31st, 1988 and having middle school GPA above the cutoff. In Panel 1 we interact each of these with a dummy for having two parents with a university degree (our definition of high SES) and with a dummy for having at least one parent with no university degree (low SES). In Panel 2 the two treatment dummies are interacted with a dummy for being male and, in addition, with a dummy for being female. Each regression controls for a quadratic function of middle school GPA percentiles, a linear trend in week of birth (with weeks of birth normalized to 0 for those born in the first week of 1989 and the trend allowed to differ before and after the reform) interacted with the quadratic function of GPA percentiles, seasonal (52 weeks) dummies and five dummies indicating whether each most selective school is located within 3km of the attended middle school. S.e. of Reduced Form and First Stage are clustered at the week-of-birth level. LATE s.e. are estimated using the delta method. The sample only includes students who attended a middle school which is within 8km from one of the two most selective schools. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Crowded-out Compliers		Pulled-in Compliers	
First Stage	0.176***		0.093***	
	(0.029)		(0.021)	
	R.F	LATE	R.F.	LATE
School Completion	-0.020	-0.112	-0.012	-0.130
	(0.023)	(0.133)	(0.013)	(0.145)
Predicted Peer GPA	0.005**		0.005**	
	(0.002)		(0.002)	
University Enrolment	-0.059**	-0.333**	-0.028	-0.296
	(0.027)	(0.156)	(0.017)	(0.195)
Predicted Peer GPA	0.004		0.004	
	(0.002)		(0.002)	
University Completion	-0.061*	-0.343*	-0.003	-0.031
	(0.032)	(0.191)	(0.019)	(0.201)
Predicted Peer GPA	0.001		0.001	
	(0.002)		(0.002)	
N	6,908			

Table A6: First stage, R.F. and LATE for attending a competitive school, controlling for average predicted peer quality in the school.

Notes : Estimates of parameters of the baseline equation (Equation 4, see Table 4) including a control for predicted peer quality both in the first stage and in the reduced form. Peer quality is defined on the basis of the type of school (competitive or non-competitive) a student is assigned to. Post-reform, all students above their relevant admission cutoff are assigned to competitive school (irrespective of whether they actually attend it). Pre-reform, assignment depends on the middle school attended. If at least 60% of the students who move to academic high school and who attended the same middle school enrol in a competitive high school, then we consider *all* students attending that specific middle school as being assigned to a competitive high school. Once we assign students to a (non-)competitive high school, we predict their peer quality as the average middle school GPA of all same-cohort students who actually attended a (non-)competitive high school. LATE are the ratio between the R.F. and the First Stage. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

	Middle School GPA
Blind Grade	19.16***
	(0.418)
Blind Grade × Post 2005	0.073
	(0.572)
Year 2003	-3.602***
	(0.948)
Year 2004	-1.049
	(0.921)
Year 2005	-3.630
	(2.477)
Year 2006	-1.616
	(2.453)
Year 2007	-3.109
	(2.479)
Constant	-25.75***
	(1.780)
N	7,913

Table A7: Change in the correlation between middle school GPA and blinded grades, pre- and post-2005

Notes : Estimates of the parameters of a regression of the middle school GPA percentile (with students ranked among other students from the same cohort) on the grades obtained by the same student in the national middle school exam and the interaction between the national exam grades and a dummy taking value 1 if the students takes the exam in or after 2005. The regression controls for year of graduation fixed effects. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

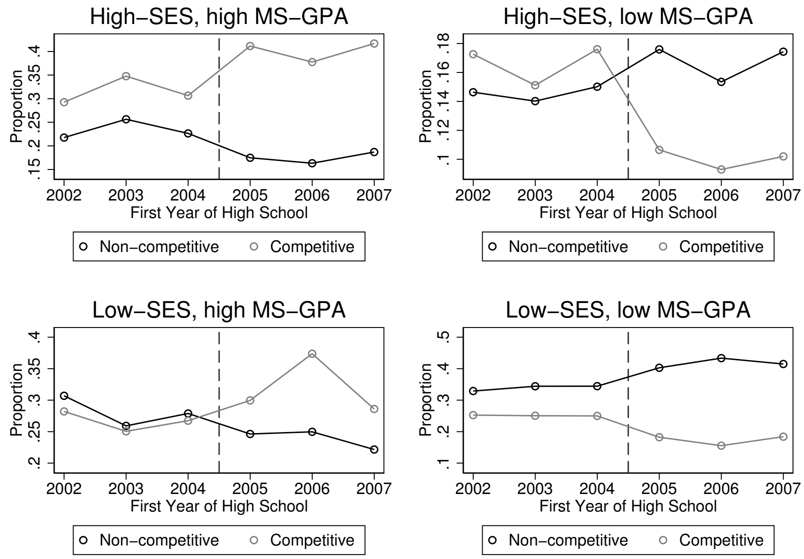


Figure A1: Proportion of students with different middle school GPA (MS-GPA) and different SES attending competitive and non-competitive high schools.

Notes : High-SES students have both parents with a university degree, Low-SES students have at least one parent with no university degree, see Section 5.1. High (Low) middle school-GPA students have a middle school GPA above (below) the median of the middle school GPA distribution in their cohort.

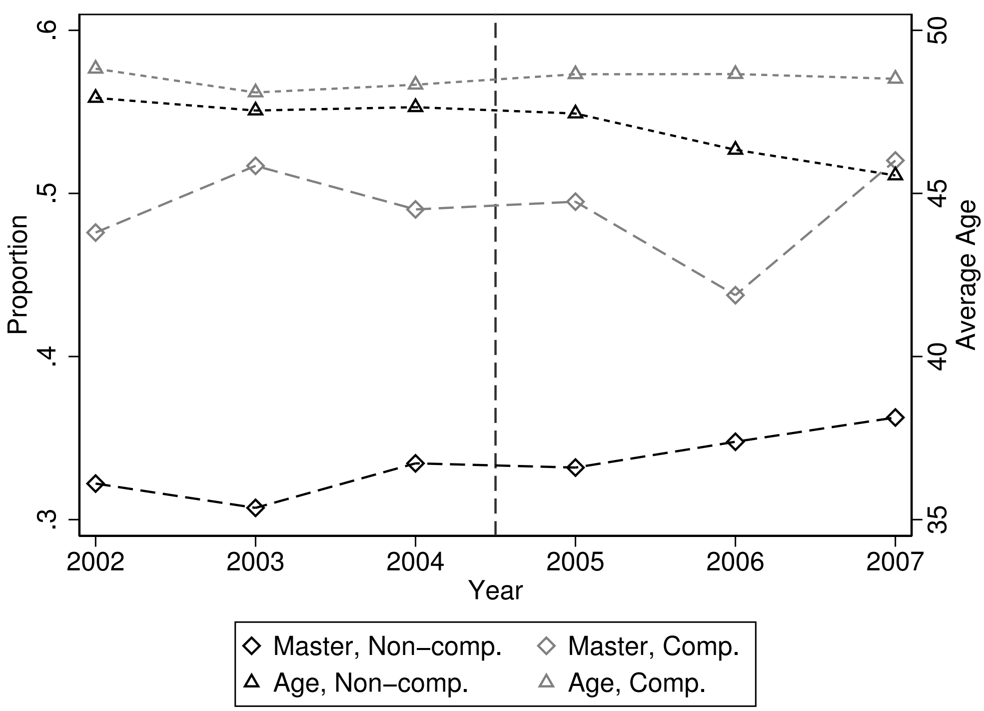


Figure A2: Teachers' average characteristics in every year, competitive and non-competitive schools.

Notes : "Master" is the proportion of teachers holding a master's degree (left axis). "Age" is the average age of teachers (right axis). "Comp." is for competitive schools, "Non-comp." are the non-competitive schools.

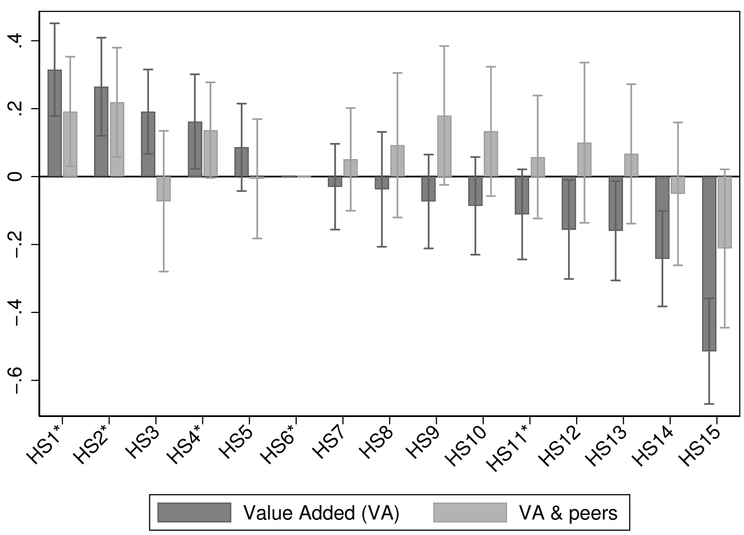


Figure A3: High school FE in Value-Added model for pre-reform cohorts.

Notes : The plotted estimates of the high school fixed effects (FE) parameters are derived from two different school Value Added (VA) models. The dark grey bars are OLS estimates of school FE from an individual-level regression of grades obtained at the end of high schools (in the national exam in Norwegian language) on school FE, middle school GPA (to have VA measures) and cohort FE (we include the three pre-reform cohorts of students). The light grey bars are estimates of the same parameters when we include as controls also the average characteristics of peers in the same school (the proportion of schoolmates' parents in different quartile of the earnings distribution and the average education of schoolmates'parents). Given that we have multiple years available, we have variation in the quality of peers at the school level. The schools FE in this second regression should be an estimate of the effect of the school beyond the impact of peers' composition. We have information on the grades in the national Norwegian exam for about 90% of the sample of pre-reform students. The lines around the bars are the 95% C.I. The starred school (HS1, HS2, HS4, HS6 and HS11) are the competitive schools. Schools HS2 and HS4 are the most selective schools we define in Section 7. Baseline school for the school FE is HS6.

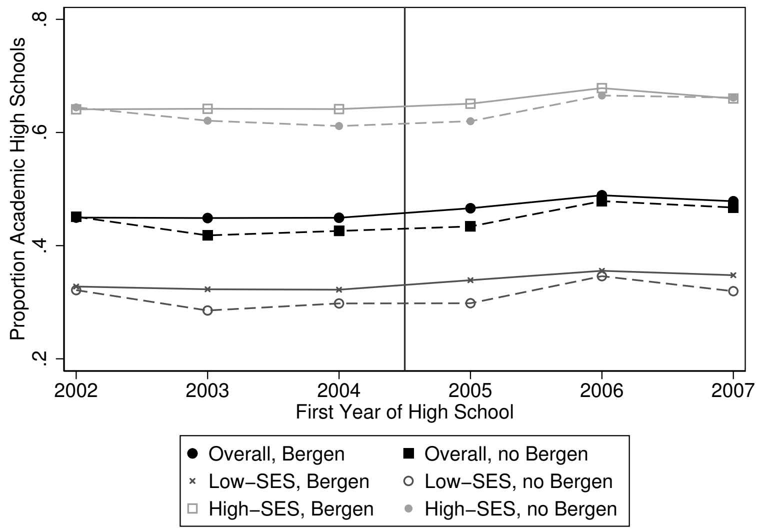


Figure A4: Proportion of students enrolling in academic high school, Bergen vs. rest of Norway.
Notes : The proportions are the ratio between the number of students enrolling in academic high school and the total number of students graduating from middle school in every year. "Bergen county" includes all students graduating in the Hordaland county (where Bergen is located). "Overall, no Bergen" includes students graduating in every county, apart for Hordaland. "High-SES" students' parents (both) graduated from university, while at least one parent of "Low-SES" students did not complete university.

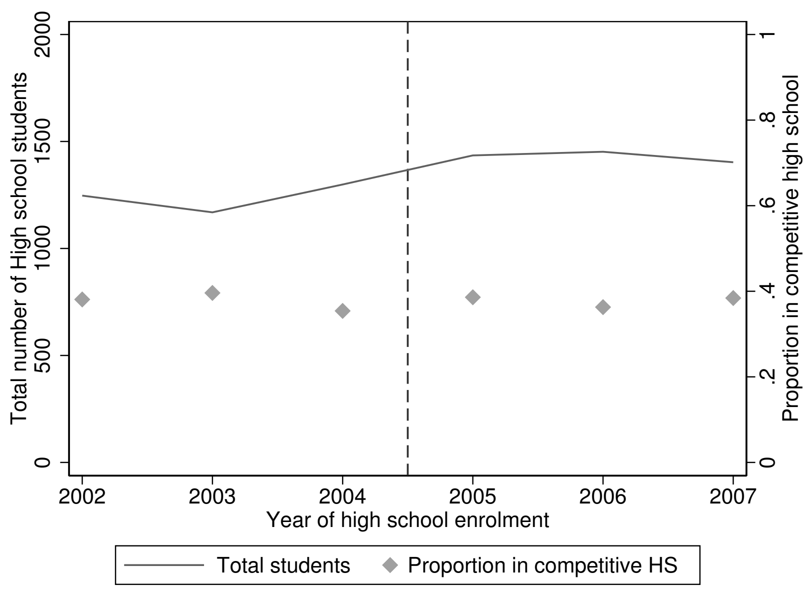
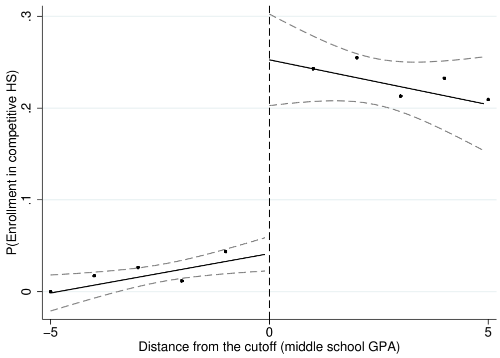
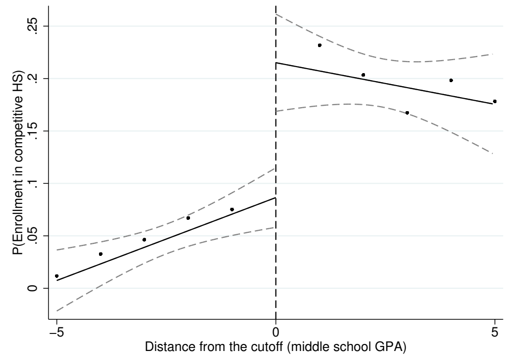


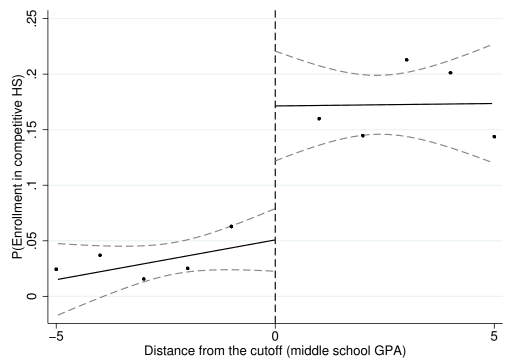
Figure A5: Number of students enrolling in academic high schools and, among these, proportion enrolling in competitive high schools in every year.



(a) 2005



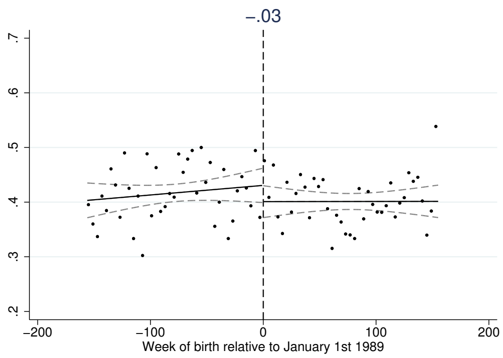
(b) 2006



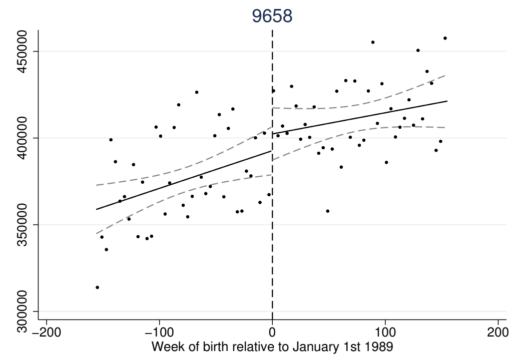
(c) 2007

Figure A6: Probability of enrolling in a competitive high school around GPA admission cutoff in the three post-reform years.

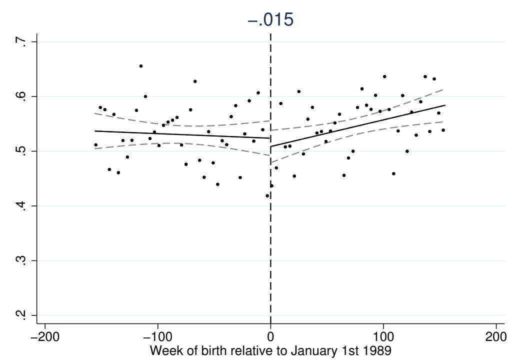
Notes : These figures pull together all the different admission cutoffs in each post-reform year (hence, five admission cutoffs in 2005 and 2006 and four admission cutoffs in 2007, see Section 5.1). The plots show the proportion of students enrolling in a competitive high school for students whose GPAs is between -5 and 5 GPA points from the cutoff (each point is the proportion for all students who are within that distance). Figures also display a linear fit and 95% confidence interval for this fit, allowed to be different above and below the admission cutoff. Details on how the thresholds are defined are provided in Section D.



(a) Parents with University



(b) Household Earnings



(c) Female

Figure A7: Average characteristics of children born in different weeks around January 1^{st}, 1989.

Notes : The plots show the average value of each covariate (the proportion of students with both parents graduating from university (a), average annual parental earnings during the three years in high school (b), proportion of females (c)) for students born in the different weeks around the first week of 1989. Each dot represents the average for 4 weeks, while the linear fit (solid line) uses week data. The dashed lines is the 95% confidence interval of such fit. The numbers on 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 (January 31^{st}, 1988) and a linear trend in the week distance from the threshold (allowed to differ before and after the threshold). Standard errors are clustered at the week-of-birth level and none of the reported coefficients is statistically significant. Parental earnings are expressed in 1998 NOK (6 NOK $\approx$ 1 USD).