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Increasing the admission rate to upper secondary school:

the case of lower secondary school student career guidance

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Abstract
Although several studies investigate the effects of school resources on student performance, these studies tend to focus more on intervention effect sizes than on their cost-effectiveness. Exploiting policy-induced variation in Denmark and using high-quality administrative data, we investigate the effects of a school intervention that introduces structured student career guidance in lower secondary school on upper secondary school admission. Disregarding the sunk-cost of implementation, the reform was cost-neutral. In a difference-in-difference framework we find that the reform increases admission to upper secondary school between 4.0-6.3 percentage points for immigrants, but shows at best small improvements for the native students.

Keywords: policy evaluations; difference-in-differences; career guidance; upper secondary school

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1. Introduction

Ageing populations and concerns about future tax bases are increasingly difficult to ignore in most industrialised countries. As raising the educational level is one means of increasing tax payments, several papers investigate the effects of school interventions, such as decreasing class size, increasing student-teacher ratio, and enhancing teacher quality (e.g. Angrist and Lavy 1999; Hanushek and Rivkin 2006; Krueger and Whitmore 2001; Fredriksson, Öckert and Oosterbeek 2013). However, except for Fredriksson, Öckert and Oosterbeek (2013), these studies tend to focus more on effect sizes than on the cost-effectiveness of the interventions. At a time where most countries are facing budget cuts, the question remains: how can we increase educational attainment without overspending? This paper evaluates a school reform in Denmark designed to increase the quality of student guidance. More specifically, we identify the effects of the intervention on admission to upper secondary school.

Introducing more structure, coherence and quality into the guidance system, in 2004 the Danish Ministry of Education (DME) changed the guidance system in all public schools. The scope was twofold: to improve the admission rate to upper secondary school and reduce the upper secondary school dropout rate.[1] To accomplish this task, career counselling was centralised in regional centres, through which the municipalities expected to gain economies of scale from larger units and knowledge spill-overs between counsellors. Before the reform, school counselling practices were mainly carried out locally, with no national requirements.

Methodologically, this paper follows Machin and McNally's influential study 'The Literacy Hour' (2008), which investigates the effect of a small school intervention. Estimating a difference-in-differences (DiD) model, we use public schools as the treatment group, because the intervention was implemented only in public schools, and use private

schools as the control group. The private schools follow the same curriculum standards as the public schools, and the parents pay only about 25% of the actual cost, with the rest funded by the municipalities. Therefore, we argue that only minor differences exist between Danish public and private schools.

Using high-quality administrative registers on school resources, with detailed information on students and their parents, we follow six cohorts of 9th graders (final year of compulsory schooling) into their first year of upper secondary school.

The literature on student guidance is sparse. Most earlier studies present descriptive measures instead of causal effects (see Hughes and Karp 2004 for an overview), investigate the effects of students' attitudes instead of their actual behaviour (e.g. McKay, Bright and Pryor 2005), or focus on a subsample of the population (e.g. upper secondary school graduates) (e.g. Bettinger et al. 2012; Borghans, Golsteyn and Stenberg 2011). This paper analyses the effect of the reform on complete cohorts.

The theory that students fail to enrol in post-secondary education due to a lack of information about how to succeed has been proven wrong by analyses of information shocks and their effect on student take-up of loans (Booij, Leuven and Oosterbeek 2012) and inclination to apply for post-secondary education funding (Bettinger and Baker 2011). Both studies find that the students acquired the information but failed to apply it. Combining information with individual guidance, however, is apparently proved highly effective (Bettinger et al. 2012). The Danish reform combines general information with in-class and individual student guidance.

The immigrants are of special interest because they have larger drop-out rates (Bratsberg, Raaum and Røed, 2011), they experience larger barriers to education through less information about the application process, and they have larger gains from schooling than the native-born students (Turney and Kao 2009; Perna et al. 2008), especially if they grow up

amongst uneducated adults (Åslund et al. 2011). In Norway, Brinch, Bratsberg and Raaum (2012) find large effects of a non-targeted reform, such as the Danish Guidance Reform, especially on immigrants' school attainment. Thus the immigrants are a vulnerable group that may have potentially high gains from improved career guidance and information.

We find that the new guidance structure improves admission to upper secondary school by 4.0-6.3 percentage points for the immigrants but no robust effect for the native-born, where we find sizes between no effect and 1.0 percentage points. We perform several sensitivity checks to validate our results, and in total we find that except for including school-specific time trends, our results for the immigrants are robust to these checks, whereas our results for the native-born are sensitive to small changes to the model specification.

2. Background

Primary and lower secondary school in Denmark consists of grade zero (ages 5-6) to 9th grade (ages 15-16) and an optional 10th grade. After grade 9 or 10, students either enter upper secondary school or leave the educational system. Students that do not directly enrol in upper secondary school may enter later, with no loss of rights or opportunities for enrolment. For the 2002 grade 9 cohort, 5.1% of the native-born and 10.1% of the immigrants had not enrolled in upper secondary school with five years after leaving grade 9 (DME, 2012). As the upper secondary school consists of several tracks (academic and vocational), career guidance aims at motivating students and helping them to choose between the tracks according to their abilities and wishes. Guidance is mainly carried out in the grades 8-10 in both public and private schools.

2.1 The 2004 change in the student career guidance system

In 2002 an OECD report pointed out three major weaknesses in the Danish guidance system: an inward-looking sectorial guidance system (lower secondary school, upper secondary

school, etc.), with little continuity across sectors; a low educational level of guidance counsellors; and a lack of effective quality-assurance procedures (OECD 2002). On 1 August 2004, DME implemented the Danish Guidance Reform (hereafter DGR) addressing the weaknesses pointed out by the OECD (Jensen and Frederiksen 2004).

Before the DGR – at both public and private schools – the school principal was responsible for career guidance, and the classroom teachers, with a wide range of short-term training courses in counselling, executed the guidance activities. No national or regional requirements existed in terms of the content of the career guidance or the level of counselling qualifications. The municipalities are the local school authorities, and although before the reform the annual grants from the municipalities to the schools included funding for career guidance, the money was not directly earmarked for counselling.

With the DGR, the DME centralised the organisation of the career guidance, making new regional centres responsible for executing the career guidance at primary and upper secondary schools.[2] The guidance counsellors work full-time and are trained through a six-month full-time training programme (courses at the tertiary level). In cooperation with the municipalities, the centres determine the regional activities of the career guidance and, although no specific national requirements exist, the centres are required to document admission, drop-out rates, etc. After the reform the municipalities remain responsible for administering the budget (DME 2004). Table 1 summarises the key features of the career guidance systems before and after the reform.

[Table 1 about here]

In total the DGR constitutes a change from a laissez faire system to a structured and highly professionalised setting focusing on more qualified guidance. As the centres also have the authority to reallocate their resources amongst the schools and the students, the new

guidance targets mainly students at risk of leaving the education system after compulsory schooling or of dropping out of either lower or upper secondary school.

Until 2007, the DGR did not encompass private schools. However, during 2005-2007 private schools could purchase career guidance at the regional guidance centres, or the private schools' counsellors could cooperate with the regional centres without additional costs (The Association of Private Schools (hereafter APS) 2004). Nonetheless, neither the DME nor the APS have data on the number of private schools cooperating with their local student guidance centre before 2007.[3]

According to the DME (2003) the DGR is cost-neutral, as the funds otherwise earmarked for career guidance at the lower and upper secondary schools finance the centres' operating cost. However, because municipalities chose different types of constellations of the new guidance system, the cost of that system also varied between municipalities. Thus we use the guidance program in Copenhagen, the capital of Denmark, to exemplify the start-up expenses of the DGR. In Copenhagen, the total budget for the first year was 232.48 euro per student (calculated on the basis of students in grade 8-10 in public schools and all students in upper secondary school). The sunk costs on recruiting guidance counsellors, establishing offices and other facilities amounted to 4.76 euro per student (2% of total budget), and the government joined in with 19 euro per student (8% of the total budget) first year. All in all the start-up-expenses the first year amounted to 10% of the total spending per student (Copenhagen city council 2004). Generally, as the municipalities were responsible for reallocating resources to implementing the DGR, some municipalities may have financed sunk costs by making the career guidance more efficient; others by cutting expenditures in other areas, such as public administration.

3. Empirical strategy

Given the natural experiment setting in which the DGR exposed some schools to changes in the career guidance system but not others, we use a DiD strategy in which public schools are the treatment group and private schools are the control group. Ninth grade students in 2002-2004 and 2005-2007 define the pre- and post-reform periods, respectively. The standard DiD model for student *i* at school *s* in time *t* is the following:

$$Admis_{ist} = \beta_{0s} + \beta_1(Public_s * DGR_t) + \beta_2 DGR_t + \beta_3 Year_t + X_{ist}\beta_4 + \varepsilon_{ist}, \qquad (1)$$

where $Admis_{ist}$ represents admission to upper secondary school. $Public_s$ is a dummy variable equal to 1 for public schools and 0 for private schools. DGR_t is a dummy variable, where 1 defines the period with the new career guidance system, and 0 the old system. The interaction between $Public_s$ and DGR_t defines the variable of interest: the average treatment effect of the DGR on the treated. $Year_t$ is a set of school year dummies capturing the time trend. X_{ist} represents a vector of individual, parental and school characteristics, and ε_{ist} is the individual-specific error term. We estimate the model as a school-fixed effects model and thereby encompass any time-constant effects common to schools. Therefore, β_{0s} defines the school specific constant and the empirical model the DGR_t dummy drops out. We use a linear probability model (LPM) to estimate equation (1), computing robust standard errors clustered by schools.

3.1 Private schools as the control group

The DGR constitutes a change from laissez faire to structured guidance in public schools. If, and only if, private schools are a valid comparison group, we can identify any causal effects of structured student guidance on admission to upper secondary school. As only minor differ-

ences exist between public and private schools in Denmark, we argue that private schools are indeed a relevant control group for public schools.

Fifteen per cent of all students enrol in private schools, and DME defines the minimum educational standards for both public and private schools. Whilst public schools are fully funded through regional and state taxes, private schools receive funding equivalent to 75% of the cost of the average public school student. Furthermore, Rangvid (2008) finds that the difference between student achievement in private and public schools disappears when she controls for background characteristics. Consequently, public and private schools are alike in Denmark.

However, the *choice* of public or private school is endogenous, and changes in the composition of public and private school students across time could potentially contaminate the estimations (Blundell and Costa-Dias 2009). Some parents choose private schools because they wish their children to be more engaged in religion, music, sports or languages; others, because they are unsatisfied with the local public school; and still others prefer private schools run entirely by a board of parents (Pedersen 2010). As the DGR received little attention in the daily press before it came into force, we find it highly unlikely that private school students would change to public schools due to the reform. Figure 1 shows the number of articles every month mentioning the DGR from January 2002 through June 2007. From January 2002 until July 2004 – before the reform – the DGR is mentioned about 100 times in the local and national newspapers. From August 2004 and onwards, the number of articles mentioning the new guidance system rises steadily predominantly in the local newspapers yet is still mentioned only about 80 times in the national newspapers.

[Figure 1 about here]

Furthermore, we also find that for all four groups (native-born and immigrants in public and private schools, respectively), the percentage of students enrolled at the same

school in grades 8 and 9 is constant around the implementation of the reform. Therefore, we conclude that the DGR did not affect enrolment patterns one year after the reform (appendix figure A1).[4]

Although we do not believe that parents changed schools because of the DGR (as the reform was a minor change), we cannot rule out that some parents did. In section 4.2 we therefore investigate the student composition and enrolment changes concurrent with the reform.

4. Data

4.1 The samples

The data contains most 9th-grade students enrolled at a public or a private lower secondary school during the school years 2002-2007 and combines various high-quality administrative registers from Statistics Denmark and DME. Information on school type and school resources stems from DME and through unique institution and individual identifiers, we merge these school-level records to individual-level information on the students and their parents. We observe all students in the 9th grade and the following two years. Thus the data has a panel structure at the school level but takes the form of repeated cross sections at the individual level.

The outcome variable is a dummy identifying admission to the academic or the vocational track of upper secondary school. As about half of the 9th grade students attend 10th grade, we define the outcome as equal to 1 if the students enrol in upper secondary school in the first or second year after completing 9th grade, and otherwise 0. As some secondary educations take up to four years to complete, we are limited to examining admission rates rather than completion. However, the literature also finds that beginning sooner rather than later also increases completion. Dobkin and Ferreira (2010) find that younger students, despite

lower achievement than their classmates, complete high school at higher rates than their older peers. Furthermore, Bailey, Jeong and Cho (2010) report that older students are more likely to drop out of post-secondary education. If the DGR increased the admission rate, these findings indicate that the students analysed in this study may also have a higher likelihood of completion.

DME (2012) calculates the expected completion rates of upper secondary school five years after 9th grade. As one purpose of the DGR is to allocate extra guidance resources to low-performing students, we analyse not only native-born students (69% completion rate) but also immigrants (55% completion rate). We follow Statistics Denmark in defining 'immigrants' as both first- and second-generation immigrants, i.e. students who were born abroad and whose parents were born abroad (first generation) and parents are born abroad but students born in Denmark (second generation). Students with at least one parent born in Denmark, irrespective of origin at birth, are considered native-born.

Our final dataset consists of 210,546 grade 9 native students enrolled in 998 public (168 private) schools, and 15,013 grade 9 immigrant students enrolled in 262 public (23 private) schools. These samples are equivalent to 77% of all public schools; however, as many private schools offer schooling only to grade 6 or do not offer 9th-grade achievement testing, the samples contain 40% of all private schools. For both the native-born and the immigrants, we restrict the data to schools that for all six years have at least three grade 9 students, aged 14-17, and a minimum of one grade 9 achievement test in the two main subjects: maths and Danish.[5] In addition, as some schools have no immigrants, we restrict schools in the immigrant sample to having a minimum of three immigrants annually. This restriction ensures that we calculate the effects of DGR on a minimum of 18 immigrant students per school.

For the native-born, Figure 2 shows the distribution of 9th-grade enrolment in private and public schools, and illustrates that most (>50%) public schools have 15–30 grade 9 students per year, whereas most private schools have 5–18 students in grade 9.

[Figure 2 about here]

Similarly, for immigrants most public and private schools in our sample have only 3–5 immigrants in grade 9 (figure 3). In addition, demanding at least three immigrant students per school, per year, we exclude about 50% of all the schools that have immigrants.[6]

[Figure 3 about here]

Figure 4 illustrates the annual admission rate to upper secondary school from 1982-2007 for the treatment and control groups, where the vertical line between 2004 and 2005 defines the implementation of the DGR. The figure shows that students from public schools (the solid lines) have a lower admission rate than students from private schools (the dotted lines). The dotted vertical line in 2002 shows the beginning of our period.

[Figure 4 about here]

The native-born students in public schools have an almost constant admission rate around 72% throughout the period. For the native students in private schools the admission rate fluctuates around 85% until the reform and afterwards continues at a high level.

The immigrant students in public schools have a stable admission rate around 60% from 1994 and until the reform. After the reform, the admission starts to increase. For the immigrants in private schools the admission rate fluctuates around 82%, decreases from 82% in 2003 to 79% in 2006, and then increases again in 2007. In the main analysis, we average over three pre-reform and post-reform years to minimise the effects of year-by-year fluctuations. Nonetheless, a dip in the control group is critical for our analysis: in section 5.1 we test whether our results are sensitive to excluding 2006 and in section 5.3 test for differential trends.

4.2 The covariates composition

As discussed in section 3.1, endogenous changes in student composition around the time of the reform may potentially contaminate our results. Therefore, to substantiate a causal interpretation of the results, we investigate whether student characteristics change concurrent with the reform. Table 2 shows means and standard deviations for each covariate for the treatment group (columns 1 and 2) and the control group (columns 3 and 4) before and after the reform, respectively. The means show that the native-born select positively into private schools (e.g. better educated, higher-income parents).

[Table 2 near here]

Column 5 in Table 2 presents the unconditional DID estimator – the trend in public school covariates minus the trend in private school covariates.[7] This estimator shows that despite the differences in levels, most covariates change only marginally (1-3 percentage points) with the reform and only some of these changes are significant. The differences are mainly due to the large sample size in which even little differences are likely to be significant. The most prevalent difference is in grade point average (GPA), where students in private schools have seen an increase in grades from before to after the reform. As GPA is an important indicator for student preparedness for upper secondary school, these strong effects of GPA undermine our parallel trend assumption and are likely to affect our results. However, the effect of GPA is negative, indicating that private schools are better at obtaining high achievements. Thus we expect that excluding GPA will downward bias our estimates. In Table 3 we provide further evidence of how GPA affects our results.

Like Table 2, we estimate summary statistics and the unconditional DiD estimator for the immigrants. For this group we only find that the group of a children, where one of the parents is living with a new partner decreases significantly more for the children in private schools than children in public schools around the time of the reform (significant at the

5% level). For all other covariates, we find no significant differences at the 5% level (appendix table A1). We thus conclude that public and private schools are comparable for the immigrant sample.

5. Results and robustness

5.1 Main results

This section presents the effects of the DGR on admission to upper secondary school for native-born and immigrants. In Table 3, columns 1 and 2 display the effects for the native-born, and columns 3 and 4 show the effects for the immigrants. Whilst columns 1 and 3 present results including only year dummies (hereafter, 'results with no controls'), columns 2 and 4 present results with the full set of controls (see Table 2 for the list of controls).

[Table 3 near here]

The first row of estimates in Table 3 shows the main effects. For the native-born, in the model with no controls, we find that the DGR decreases the probability of admission to upper secondary school by 1.7 percentage points. However, including all controls, the probability of admission to upper secondary school increases by 1 percentage point. For the immigrants, in the model with no controls, the DGR increases the probability of admission to upper secondary school by 4.8 percentage points. Including all controls increases the effect to 6.3 percentage points.[8] This result corresponds with a recent study by Borghans, Golsteyn and Stenberg (2011), who find that career guidance in the Netherlands has the largest effect for immigrants.[9]

The second row of estimates in Table 3 presents results excluding data from 2006, due to the dip in the admission rate for private school students (figure 4). Although this dip appeared mainly for immigrant admission, when we exclude 2006 the significant positive effect of the DGR disappears for the native-born. The effect for the native-born was small to

begin with, and we believe that the effect disappears due to the reduction in sample size. Excluding 2006 from the immigrant sample, the effect remains significant but marginally smaller (5.3 percentage points). This lower effect suggests that our main effect is partly upward biased by the reduction in the admission rate for the untreated.

The third row of estimates in Table 3 presents results, where we exclude GPA. Table 2 showed a significant negative trend in GPA from before to after the reform. The negative trend means that public schools have increased their grades less over the period than the private schools. To test the hypothesis that excluding GPA is likely to downward bias our results, we exclude GPA from our main specification and, as predicted, the estimates become smaller; for the native-born, even insignificant and negative. For the immigrants, however, the DGR still increases the admission rate to upper secondary schooling significantly by 4.0 percentage points.

The effects for the immigrants could be generated by students with the abilities to attend upper secondary school but who before the reform would meet other barriers to entrance. The immigrant students may have lacked not only information about upper secondary schooling but also guidance in how to apply that knowledge. According to Bound, Lowenheim and Turner (2010) admission to higher education in the US has increased but attainment less so. The authors argue that the lack of attainment is due to a lower quality of students entering higher education, a possible explanation for why we find no effects for the native students.

In the next three sections we continue to investigate how sensitive our results are to our model specification.

5.2 Treatment of the untreated

Section 2.1 explained that private schools can enrol in the new student career guidance system if they pay an additional fee. Despite this opportunity we found only few examples of

private schools collaborating with the local counselling centre. Assuming that our results are only weakly biased by this opportunity, we proceed by investigating how sensitive our results are to this type of potential treatment of the untreated. We thus reallocate increasingly more of the private school students to treatment and then plot our new coefficients of interest and their corresponding 99% confidence intervals (CI). For example, we first assume that 1% of private school students were treated. We then randomly sample 1% of private school students, recode them to treatment, and, using model 1, estimate the effect of the DGR on admission to upper secondary school. Then we continue to a 2% random allocation of treatment, etc., until 50% of the private schools students are assumed treated. For each level of reallocation, we make 100 replications of each estimate and then compute the means and the CIs of these estimates.

On the basis of these estimations we find that 30% of the native-born in private school can be treated without the effect of the DGR disappearing, whereas 35% of the immigrants can be treated. Second, for these threshold values the effect of the DGR is 0.7 for the native-born and 4.3 percentage points for the immigrants (appendix figures A2 and A3).

5.3 Test for parallel time trends

Parallel time trends in the treatment and control groups are an important assumption for the validity of the DiD estimator. We test this assumption in two ways: first by estimating a traditional falsification test and, second, by including school-specific trends into model (1).

First, we estimate the traditional pseudo-reform falsification test. If we assume that potential differences in the time trends between the treatment and control groups are constant, we can use a DiD model similar to model (1) using only data before 2005 to test for the effects of differential trends between the two groups. We apply this robustness check using students from 2002 as the pre-reform period and students from 2003 or 2004 as the false post-reform periods.

[Table 4 about here]

Table 4 presents the results of the falsification test. For native-born, column 1 shows the effects using 2003 as the false post-reform period and column 2, the effects of using 2004. For the immigrants, columns 3 and 4 show the equivalent estimated effects. For the identification strategy to be valid, this falsification test should provide small effects, as changes in admission outside the reform are endogenous. For the native-born we find close to zero and insignificant effects. For the immigrants, we also find insignificant effects, but the point estimates suggest that the false reform decreases admission to upper secondary school by 2.7 percentage points, and by 2 percentage points when we use 2003 and 2004 as the false post-reform period, respectively. Whilst we do not find zero effects of this falsification test for immigrants, the negative results suggest that this trend is not upward-biasing our results.

Second, we test the model for its sensitivity to school-specific time trends. We find no significant effects of the DGR for either the native-born or the immigrants. Thus the results suggest that school-specific time trends are potentially generating our main results. However, including school-specific time trends is a very strict test, as in principle it means that a specific trend on the basis of 3 students per year is calculated for some schools (appendix table A4).

6. Conclusion

This paper estimates the effects of student career guidance in lower secondary school on admission to upper secondary school. We exploit a national change, in Denmark, in structure and quality in lower secondary school student career guidance, a change that affected students in public but not in private schools.

For both the native-born and the immigrants, we find that the DGR has a positive significant effect on admission to upper secondary school. However, for the native-born,

the result is small (no effect to 1 percentage point), whereas for the immigrants the effect is larger (4.0-6.3 percentage points).

We perform several sensitivity checks. First, we find only minor changes in the covariates concurrent with the reform, except for GPA. Because GPA changes negatively with the reform, we estimate the model both with and without GPA. Second, we find no changes in the enrolment pattern into private and public schools around the reform. Third, a circular dip in the admission rate for immigrants in private schools exists around 2006, so we expand the analysis to include a model without 2006. Fourth, we test how sensitive our results are to potential treatment of the untreated. Fifth, we estimate how sensitive our results are to changes in time trend through a classic pre-reform falsification test and by including school-specific time trends. From these falsification tests we conclude that our result for the immigrants is fairly robust, except for the inclusion of school-specific time trends. For the native-born the result is sensitive to even small changes in the model specification.

The reform was designed to reallocate resources amongst the students so that mainly those with few resources received face-to-face guidance. As we find that mainly the immigrants benefit from the reform and that the new system, for the average native student, was as good as the old one, our results suggest that the change in the student career guidance system has had the expected impact.

As we cannot investigate the second main purpose of the new student career guidance system – the effect on upper secondary school dropouts – our study examines only half of the intended effects of the structured career guidance system. That effects on dropouts can be found for the average native student is likely.

Notes

1. We cannot investigate the effect on the dropout rate, because this analysis requires data from a minimum of four years after the reform – and three years after the reform the student career guidance system changed again to allow all types of students to benefit.
2. Some centres only administer schools within one municipality, whereas others include schools from several municipalities. Thus the municipalities could decide on different solution frameworks: they could establish new institutions alone or in cooperation with other municipalities, delegate responsibility for the career guidance to established public institutions or to independent institutions or private companies through invitations to tender. As part of the DGR the DME established a national virtual career guidance portal containing information about upper secondary schools. This portal is publicly available for students in both public and private schools and aims at helping students and their parents find relevant information about upper secondary schools. However, several websites provided a similar service throughout the period, and the portal is of minor interest relative to the effect of the DGR.
3. Legally, the municipalities are allowed to change the grants to private schools only three years after the change has been implemented in public schools.
4. Estimating the potential effect of the DGR on enrolment two years after the reform requires information on changes in enrolment in grade 7. Unfortunately, in our period students are not observed until grade 8.
5. Schools with fewer than five grade 9 students live predominantly in rural areas. The age limit excludes adult learners.
6. However, we find that relaxing this restriction and including schools with only one or two immigrants per year, the effects of the DGR are similar in magnitude. Excluding schools with a minimum of four through seven immigrants, the point estimate remains but loses precision presumably from the reduction in sample size (appendix table A3)
7. For each covariate, the following model calculates the unconditional DiD estimator:

$$Unc\_DiD_s = (public_{s,t} - public_{s,t-1}) - (private_{s,t} - private_{s,t-1}), \qquad (2)$$

Where $public_s$ defines the public schools and $private_s$ defines the private schools before (*t-1)* and after *(t)* the reform.
8. Whilst our preferred model is a linear probability model including school fixed-effects, we also try to estimate the effect using a probit model. Comparing the estimates from the linear probability model (without fixed effects) with the marginal effects from the probit model, we find similar effects of the DGR reform for the immigrants but somewhat larger effects in the probit model for the native-born (appendix table A2).
9. We also investigate potential heterogeneous effects of the reform amongst students with different parental backgrounds by estimating triple-difference models. We find no significant effects and thus no not report these results.

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Tables and Figures

Table 1. A comparison of the student guidance system before and after the reform.

	Pre-reform period	Post-reform period
Management	School principal	Counsel centres
Student counsellors	Classroom teachers	Highly trained counsellors
Requirements	No requirements or follow-up	Centres are obligated to document admission and drop-out rates
Funding	School budgets	Centralized at the centres
Counsellors' training	20 different short-term training courses	One course, corresponding to six month full time university training

Source: Jensen and Frederiksen 2004.

Table 2. Summary statistics and the unconditional DiD estimator for the native-born students.

	Public schools		Private schools		
	Pre-reform	Post-reform	Pre-reform	Post-reform	Unconditional DiD estimator
GPA					
A or B	0.02	0.03	0.04	0.05	-0.01 **
C	0.51	0.49	0.59	0.60	-0.03 ***
D	0.28	0.27	0.25	0.26	-0.01 *
E or Failed	0.15	0.18	0.09	0.09	0.03 ***
GPA missing	0.04	0.03	0.03	0.01	0.01 **
Student, gender	0.51	0.51	0.47	0.47	0.01 *
Student, age	15.49	15.58	15.46	15.54	0.00
Mother´s education					
Primary or lower secondary schooling	0.26	0.22	0.19	0.14	0.00
Upper secondary school, academic track	0.04	0.05	0.05	0.06	0.00
Upper secondary school, vocational track	0.37	0.39	0.32	0.34	0.01
Tertiary education	0.31	0.32	0.42	0.45	-0.01 **
Missing education	0.01	0.01	0.01	0.01	0.00 *
Father's education					
Primary or lower secondary schooling	0.23	0.22	0.16	0.14	0.01 ***
Upper secondary school academic track	0.04	0.04	0.06	0.06	0.00
Upper secondary school, vocational track	0.42	0.42	0.35	0.36	-0.01
Tertiary education	0.26	0.27	0.37	0.38	0.00
Missing education	0.03	0.03	0.03	0.03	0.00
Mother's employment					
Permanent social benefits	0.04	0.04	0.04	0.03	0.00 **
Temporary social benefits	0.08	0.06	0.06	0.05	0.00 *
Missing employment	0.02	0.02	0.02	0.02	0.00
Father's employment					
Permanent social benefits	0.04	0.04	0.03	0.03	0.00
Temporary social benefits	0.05	0.04	0.04	0.03	0.00
Missing employment	0.02	0.02	0.03	0.03	0.00
Mother's income quartile					
Income Q1 – lowest	0.22	0.22	0.19	0.17	0.02 ***
Income Q2	0.25	0.25	0.20	0.20	0.00
Income Q3	0.25	0.25	0.23	0.25	-0.02 ***
Income Q4 - highest	0.24	0.24	0.33	0.34	-0.01
Missing income	0.04	0.04	0.05	0.04	0.00

(Continues)

Table 2.

	Public schools		Private schools		
	Pre-reform	Post-reform	Pre-reform	Post-reform	Unconditional DiD estimator
Father's income quartile					
Income Q1 – lowest	0.21	0.20	0.18	0.16	0.02 ***
Income Q2	0.25	0.25	0.18	0.19	0.00
Income Q3	0.25	0.25	0.23	0.24	-0.01 *
Income Q4 - highest	0.24	0.24	0.35	0.35	0.00
Missing income	0.05	0.05	0.06	0.06	0.00
Mother is unknown	0.00	0.00	0.00	0.00	0.00
Father is unknown	0.01	0.01	0.01	0.01	0.00
Mother is deceased	0.01	0.01	0.01	0.01	0.00
Father is deceased	0.02	0.02	0.02	0.02	0.00
Family Structure					
Nuclear	0.67	0.65	0.68	0.68	-0.02 ***
Divorced with new partner	0.12	0.13	0.10	0.11	0.00
Single parent	0.16	0.18	0.16	0.17	0.01 **
Student living alone	0.02	0.02	0.03	0.01	0.01 *
Missing family structure	0.02	0.02	0.03	0.03	0.00
Number of siblings					
One sibling	0.50	0.50	0.50	0.51	-0.01
Two or more siblings	0.45	0.45	0.43	0.42	0.01
Age of mother at time of birth					
Mother's age missing	0.01	0.01	0.01	0.01	0.00
Teenage mother	0.02	0.02	0.01	0.01	0.00
Student, health at birth					
Admitted to hospital up to three years after birth	0.94	0.93	0.92	0.92	0.00
Low birth weight (<2500 grams)	0.05	0.05	0.04	0.04	0.00
Missing birth weight	0.02	0.02	0.03	0.03	0.00
Premature birth (<37th week)	0.08	0.09	0.08	0.08	0.00
Missing, gestation	0.02	0.02	0.03	0.03	0.00 *
Number of observations	106,455	116,394	15,723	16,853	255,425

*Significance at the 10 % level, ** significance at the 5 % level, *** significance at the 1 % level.
Note: The unconditional DiD estimators are calculated according to model (2), i.e. the differences in the means between the treatment and the control group, before and after the intervention. We apply school level fixed effects when identifying the level of significance.

Table 3. The effects of the DGR on admission to upper secondary school.

	Native-born sample		Immigrant sample	
	(1)	(2)	(3)	(4)
	No covariates	All covariates	No covariates	All covariates
Effect of guidance	-0.0168 ***	0.0098 **	0.0478 **	0.0626 ***
Standard errors	0.0053	0.0045	0.0198	0.0237
Number of observations	255,425	255,425	18,235	18,235
2006 excluded	-0.0173 ***	0.0064	0.0277	0.0528 **
Standard errors	0.0056	0.0050	0.0205	0.0249
Number of observations	210,546	210,546	15,013	15,013
GPA excluded	-	-0.0066	-	0.0395 **
Standard errors	-	0.0047	-	0.0189
Number of observations	255,425	255,425	18,235	18,235

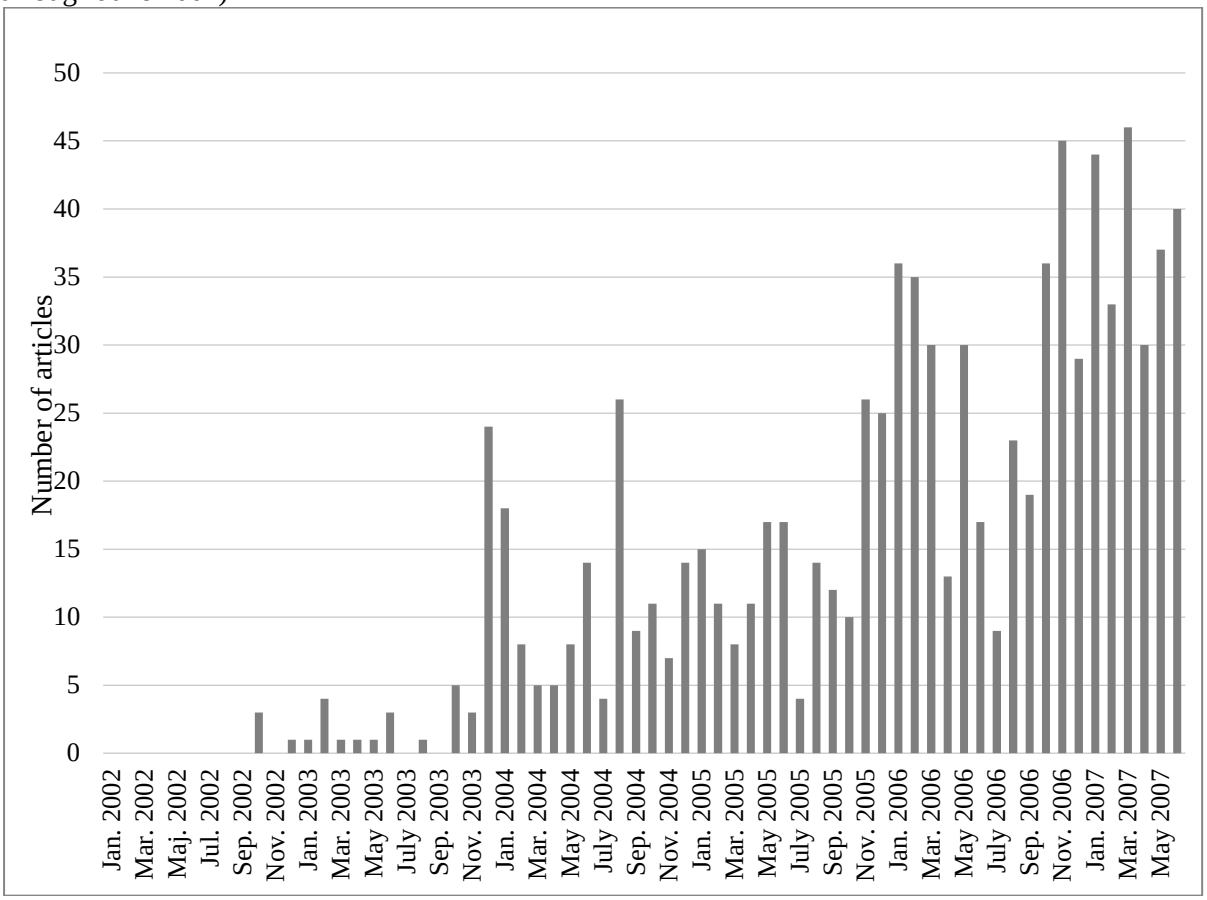
* Significance at the 10 % level, ** significance at the 5 % level, *** significance at the 1 % level.
Note: For both the native and the immigrant sample, we use model (1) to calculate the estimates. Column one and column three represent estimations including year dummies as covariates. Column two and four represent estimations including all covariates in table 2.

Table 4. Pseudo-reform falsification test: The effect of the DGR on admission to upper secondary school using either 2003 or 2004 as the false post-reform year.

	Native-born sample		Immigrant sample	
	(1) Pre-reform:2002, Post-reform:2003	(2) Pre-reform: 2002, Post-reform: 2004	(3) Pre-reform:2002, Post-reform:2003	(4) Pre-reform: 2002, Post-reform: 2004
Pseudo-reform effect	0.0042	0.0006	-0.0265	-0.0201
Standard errors	0.0079	0.0088	0.0455	0.0378
Number of students	80,290	82,167	5,427	5,577
Number of schools	1,166	1,166	285	285
R^2	0.335	0.338	0.406	0.418

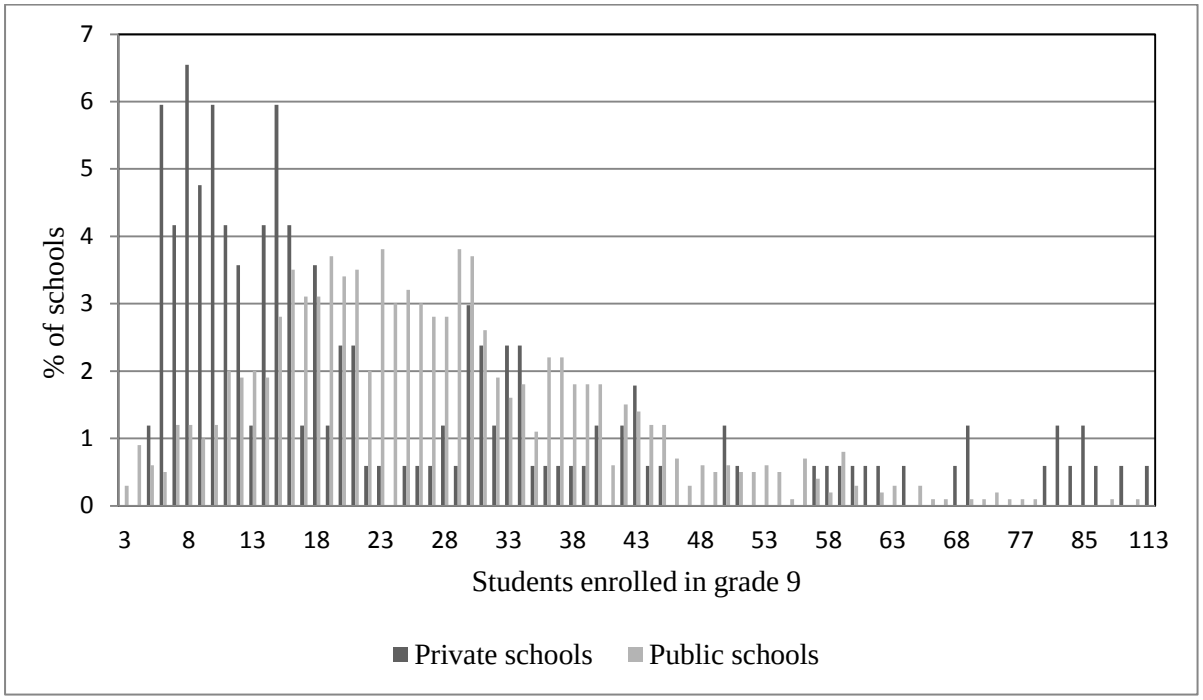
* significance at the 10 % level, ** significance at the 5 % level, *** significance at the 1 % level.
Note: Column one and three uses 2002 as the pre-reform year and 2003 as the false post-reform year. Column two and four uses 2002 as the pre-reform year and 2004 as the false post-reform year. For both the native-born and the immigrants, we use model (1) and include all covariates presented in table 2.

Figure 1: Number of articles mentioning the DGR in the pre- and post-reform period (January 2002 through June 2007)



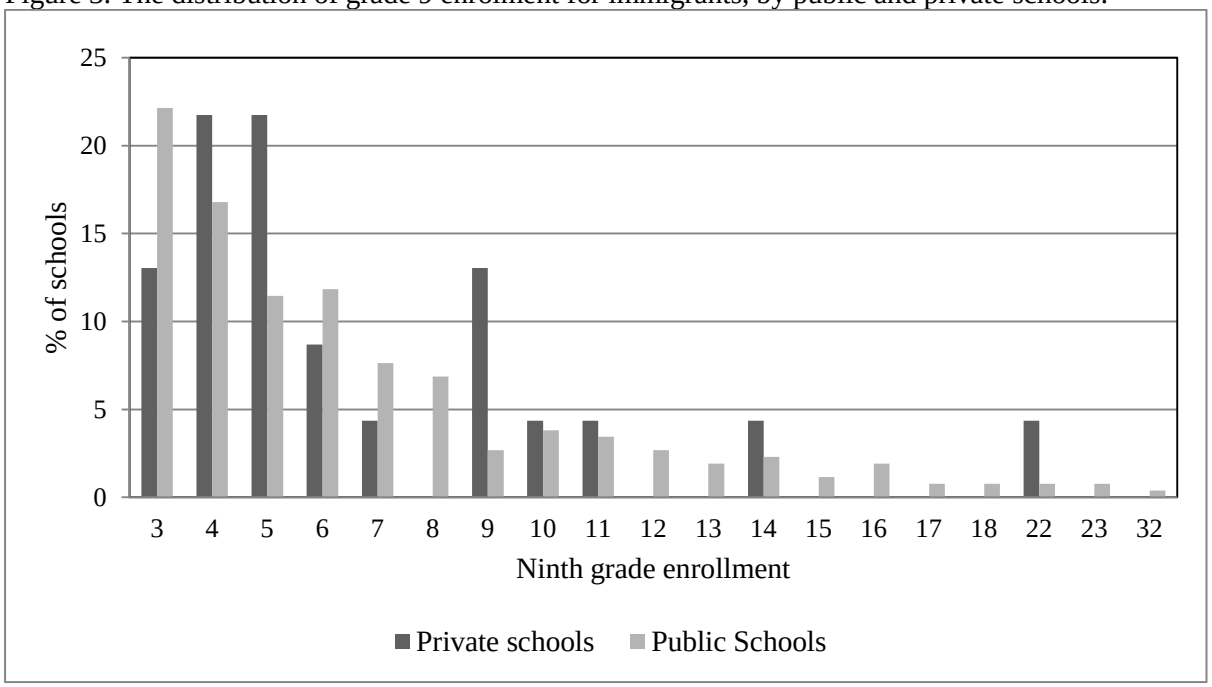
Note: The figure counts the number of articles mentioning the DGR in national or local newspapers per month from January 2002 through June 2007. We use the following search criteria [in Danish] "UU-vejledning" "Ungdommens Uddannelses Vejledning" or "Ungdommens Uddannelsesvejledning". Source: www.Infomedia.dk

Figure 2. The distribution of grade 9 enrolment for the native-born students, by public and private schools.



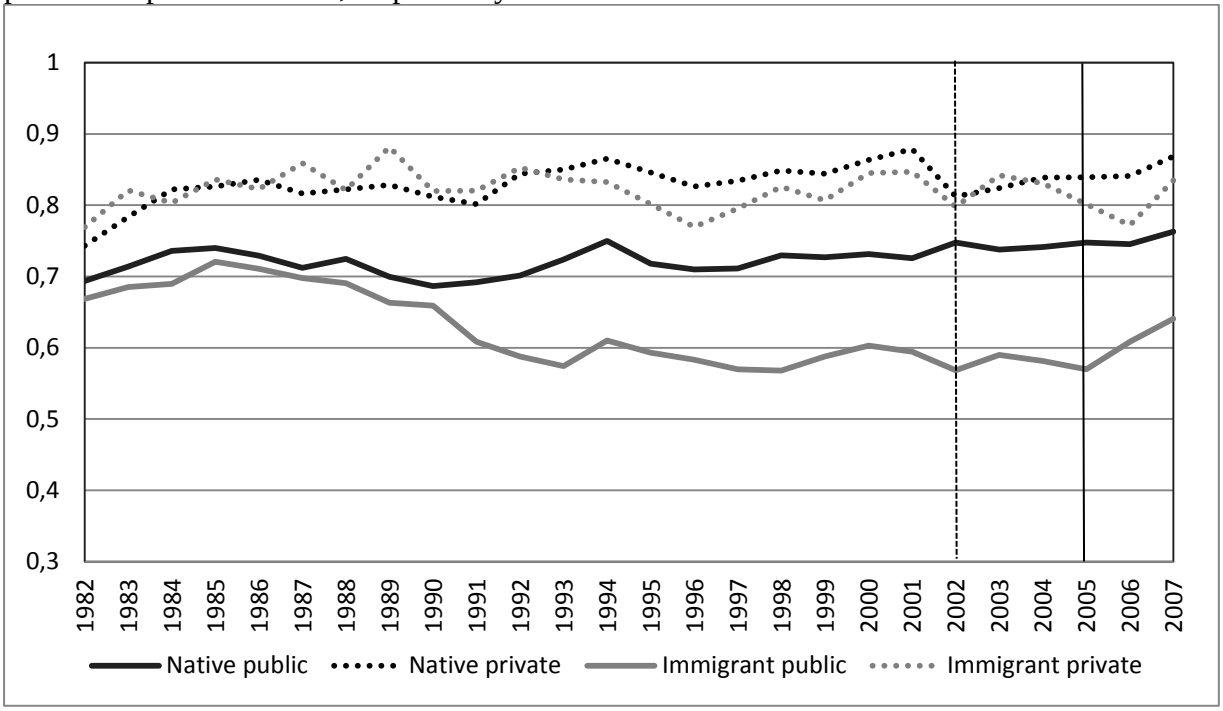
Note: The figure illustrates the distribution of grade 9 enrolments for the native-born students by private and public schools. The vertical axis defines the percentage of schools and the horizontal axis defines the number of students enrolled.

Figure 3. The distribution of grade 9 enrolment for immigrants, by public and private schools.



Note: The figure illustrates the distribution of grade 9 enrolments for immigrant students by private and public schools. The vertical axis defines the percentage of schools and the horizontal axis defines the number of students enrolled.

Figure 4. Admission to upper secondary school by native-born and immigrant students in public and private schools, respectively.



Note: For each grade 9 cohort, the figure shows the proportion of students entering upper secondary school. The vertical dotted line in 2002 defines the start of our data window whereas the vertical solid line at 2005 symbolizes the first cohort after the reform. Over the entire period we measure the same schools, however, the data before 2002 is slightly less restrictive than after 2002. The sample from 2002-2007 equals our main sample in the data where we make the following restrictions: Schools need to have at least three students per year that have a minimum of one exam in maths or Danish, and all schools need to be in the sample in all six years. While we use the same sample of schools in the 1982-2001 periods as the 2002-2007 periods, we cannot be sure that these schools have at least three students taking an exam in maths or Danish, because no data on exams exists before 2002. In addition, all schools are not included in all years from 1982-2007, because some schools started later.

APPENDIX

Table A1.Summary statistics and the unconditional DiD estimator for immigrant students.

	Public schools		Private schools		
	Pre-reform	Post-reform	Pre-reform	Post-reform	Unconditional DiD estimator
GPA					
A or B	0.01	0.01	0.03	0.03	0.01
C	0.27	0.25	0.46	0.49	-0.05*
D	0.29	0.27	0.30	0.26	0.02
E or Failed	0.35	0.40	0.14	0.20	0.00
GPA missing	0.08	0.07	0.06	0.02	0.02
Student, gender	0.49	0.50	0.45	0.46	-0.01
Student, age	15.64	15.72	15.43	15.47	0.04
Mother's education					
Primary or lower secondary schooling	0.34	0.36	0.24	0.31	-0.05
Upper secondary school, academic track	0.08	0.09	0.07	0.06	0.02
Upper secondary school, vocational track	0.19	0.21	0.22	0.22	0.02
Tertiary education	0.14	0.15	0.27	0.25	0.03
Missing education	0.19	0.15	0.18	0.14	-0.01
Father's education					
Primary or lower secondary schooling	0.27	0.27	0.22	0.25	-0.03
Upper secondary school, academic track	0.06	0.06	0.07	0.08	0.00
Upper secondary school, vocational track	0.21	0.22	0.21	0.21	0.00
Tertiary education	0.17	0.18	0.23	0.22	0.02
Missing education	0.16	0.15	0.17	0.15	0.02
Mother's employment					
Permanent social benefits	0.08	0.10	0.05	0.06	0,01
Temporary social benefits	0.45	0.40	0.33	0.26	0,03
Missing employment	0.07	0.07	0.06	0.08	-0,02
Father's employment					
Permanent social benefits	0.15	0.18	0.10	0.10	0.03*
Temporary social benefits	0.25	0.21	0.17	0.13	0.00
Missing employment	0.04	0.04	0.06	0.07	0.00
Mother's income quartile					
Income Q1 – lowest	0.45	0.49	0.41	0.43	0.01
Income Q2	0.21	0.22	0.20	0.21	0.00
Income Q3	0.11	0.10	0.14	0.13	0.00
Income Q4 - highest	0.06	0.05	0.13	0.12	-0.01
Missing income	0.16	0.14	0.12	0.10	0.00

Table A1. *Continues*

	Public schools		Private schools		Uncondtional DiD estimator
	Pre- re-form	Post-reform	Pre- re-form	Post-reform	
Missing income	0.16	0.14	0.12	0.10	0.00
Father's income quartile					
Income Q1 – lowest	0,56	0,59	0,47	0,46	0,04*
Income Q2	0,16	0,16	0,16	0,17	-0,02
Income Q3	0,09	0,08	0,12	0,13	-0,01
Income Q4 – highest	0,04	0,03	0,10	0,11	-0,02
Missing income	0,15	0,14	0,15	0,13	0,00
Mother is unknown	0,04	0,04	0,01	0,01	-0,01
Father is unknown	0,11	0,09	0,09	0,08	-0,01
Mother is deceased	0,01	0,01	0,01	0,01	0,00
Father is deceased	0,02	0,02	0,02	0,01	0,01
Family Structure					
Nuclear	0,69	0,69	0,69	0,71	-0,01
Divorced with new partner	0,02	0,02	0,05	0,03	0,02**
Single parent	0,12	0,15	0,12	0,15	0,00
Student living alone	0,03	0,01	0,01	0,00	-0,01*
Missing family structure	0,14	0,13	0,12	0,11	0,01
Number of siblings					
One sibling	0,20	0,21	0,36	0,32	0,04
Two or more siblings	0,73	0,74	0,55	0,61	-0,05
Age of mother at time of birth					
Teenage mother	0,13	0,11	0,11	0,09	0,00
Mother's age missing	0,01	0,01	0,01	0,01	0,00
Student, health at birth					
Admitted to hospital up to three years after birth	0,48	0,57	0,64	0,72	0,01
Low birth weight (<2500 grams)	0,02	0,03	0,03	0,03	0,01
Missing birth weight	0,54	0,43	0,37	0,28	-0,01
Premature birth (<37th week)	0,05	0,06	0,07	0,08	0,01
Missing, gestation	0,54	0,44	0,38	0,28	-0,01
Number of observations	7,700	9,180	633	722	18,235

*significance at the 10 % level, ** significance at the 5 % level, ***significance at the 1 % level.
Note: The unconditional DiD estimators are calculated according to model (2), i.e. the differences in the means between the treatment and the control group, before and after the intervention. We apply school level fixed effects when identifying the level of significance.

Table A2. The effects of the DGR on admission to upper secondary school, different model specifications

	Native-born sample		Immigrant sample	
	(1)	(2)	(3)	(4)
	No covariates	All covariates	No covariates	All covariates
LPM model	-0,0147 ***	0,0125 ***	0,0469 **	0,0576 **
Standard errors	0,0056	0,0045	0,0190	0,0238
Number of observations	255.425	255.425	18.235	18.235
Probit model	-0,0214 ***	0,0106 **	0,0552 **	0,0630 *
Standard errors	0,0064	0,0064	0,0270	0,0328
Number of observations	255.425	255.425	18.235	18.235

*significance at the 10 % level, ** significance at the 5 % level, ***significance at the 1 % level.
Note: The estimations use variations of model (1) to estimate the effect of the DGR on admission to upper secondary school for the native-born and the immigrants, separately. First row of estimates shows the effect of the DGR using LPM. The second row of estimates shows the average marginal effects of the DGR using a probit model. In both models we exclude the fixed effect restriction and in the probit model we present the average marginal effects to generate comparable effects. Both set of estimations include all covariates presented in table 2.

Table A3. The effect of the DGR on admission to upper secondary school, by the number of immigrant students in grade 9.

	Minimum number of immigrant students per school, per year.					
	1 student	2 students	4 students	5 students	6 students	7 students
Effect of guidance	0.0462**	0.0569***	0.0526**	0.0401*	0.0500*	0.0423
Standard errors	0.0193	0.0215	0.0241	0.0240	0.0275	0.0273
Number of observations	20,454	17,149	13,338	11,667	10,221	8,588
Number of public schools	549	351	204	160	130	99
Number of private schools	57	31	20	15	10	8
R^2	0.36	0.35	0.34	0.34	0.34	0.35

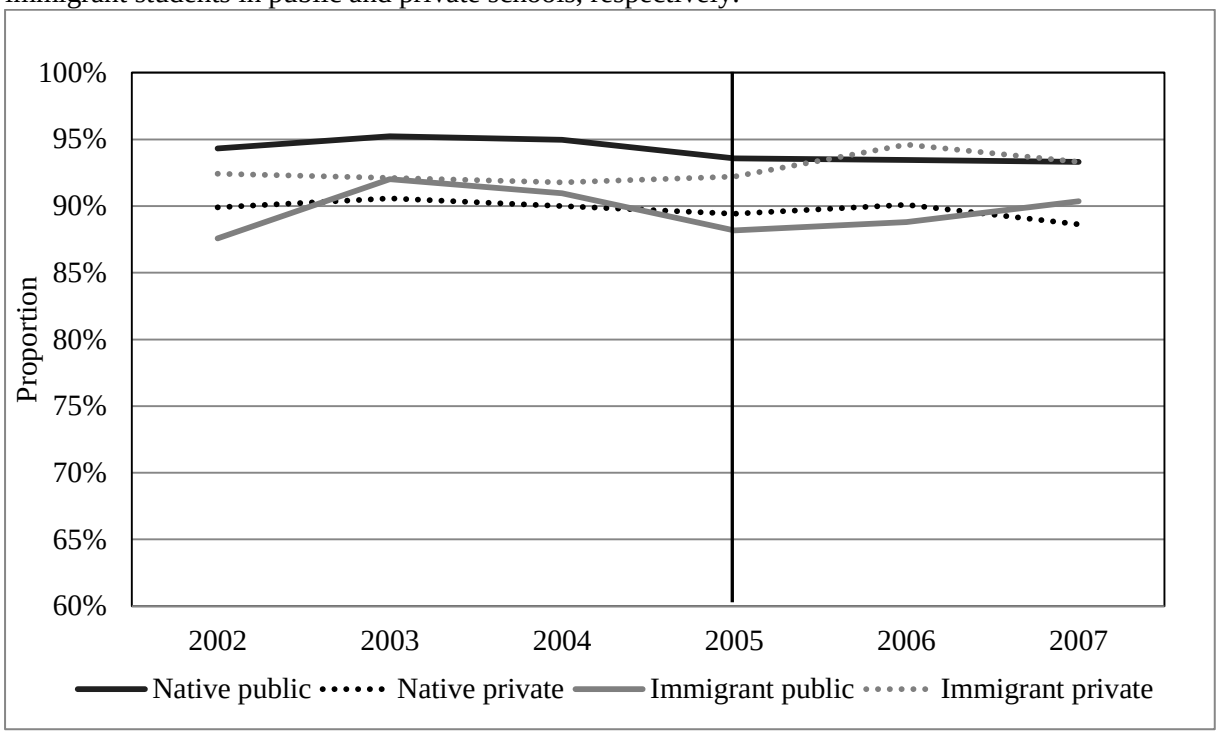
*significance at the 10 % level, ** significance at the 5 % level, ***significance at the 1 % level.
Note: We use model (1) to calculate the estimates and all estimations include all covariates in table 2. Column one presents results where the sample includes schools with only one immigrant student, column two presents the effects where schools with a minimum of two immigrants students are included, and so forth up to a minimum of seven immigrants.

Table A4. The effect of the DGR on admission to upper secondary school, control for school-specific trend.

	Native-born sample		Immigrant sample	
	(1)	(2)	(3)	(4)
	No covariates	All covariates	No covariates	All covariates
Effect of guidance	0.0014	0.0023	-0.0328 *	-0.1074
Standard errors	0.0043	0.0037	0.0170	0.0145
Number of observations	255,425	255,425	18,235	18,235

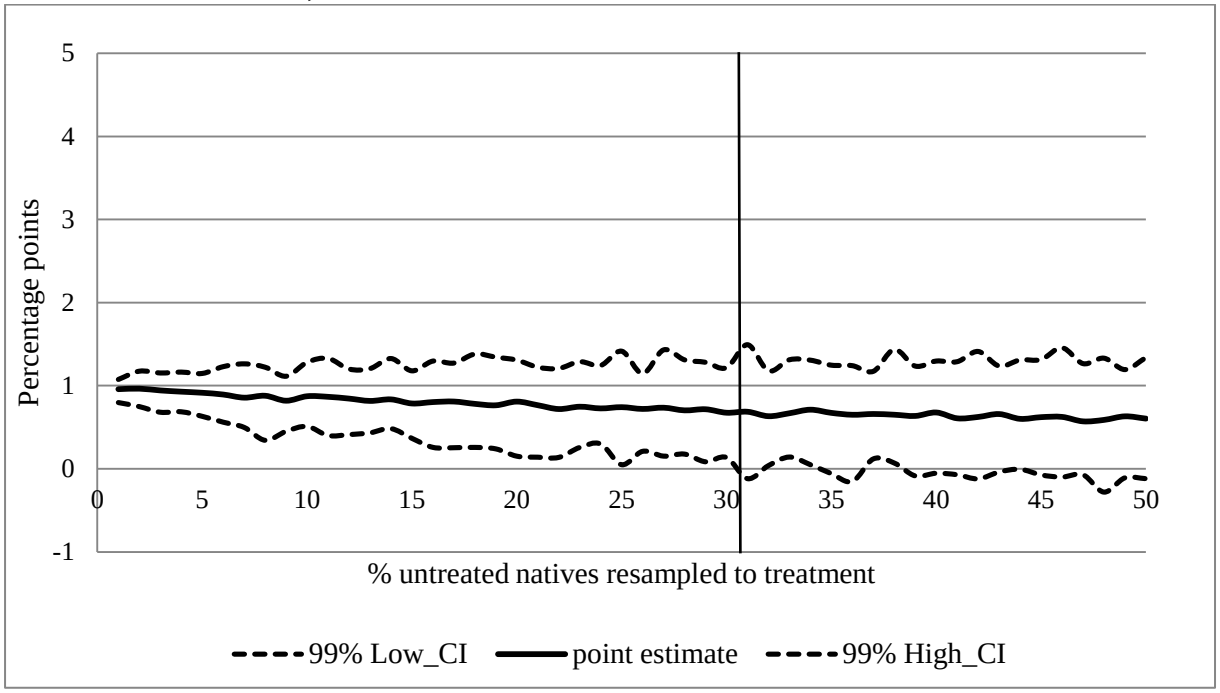
* significance at the 10 % level, ** significance at the 5 % level, *** significance at the 1 % level.
Note: The table presents the effect of the DGR, controlling for a school-specific time trend using model (1). Column one and three include only year dummies and a school specific time-trend. Column two and four include a school-specific time-trend and the covariates listed in table 2.

Figure A1. The proportion of students attending grade 8 and 9 at the same school, by native-born and immigrant students in public and private schools, respectively.



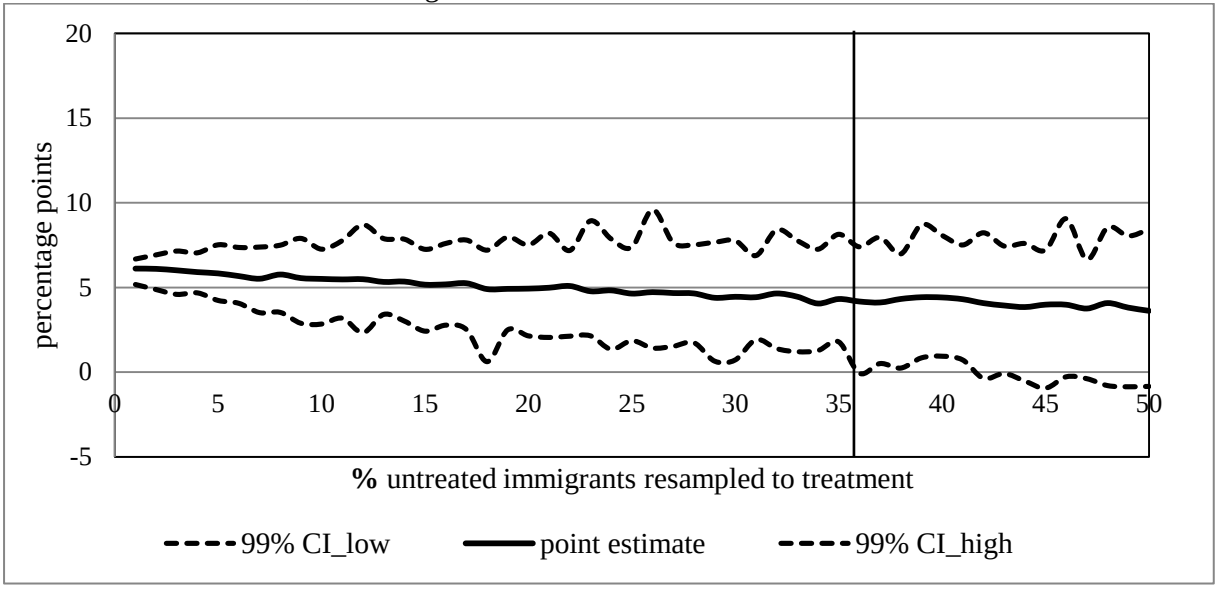
Note: For each grade 9 cohort, the figure shows the proportion of students attending grade 8 at the same school as their grade 9 school. The vertical line at 2005 symbolizes the first cohort after the reform. The solid lines define public schools, the dotted lines private schools. The black lines define native-born students and the grey lines the immigrant students.

Figure A2. The effect of the DGR on admission to upper secondary schooling for different levels of treatment of the untreated, for the native-born students.



Note: The solid line defines the point estimates for the effect of the DGR on admission to secondary schooling whereas the dotted lines define the corresponding 99% confidence interval. The horizontal axis defines the percentage of untreated (pupils in private schools) that we reallocate to treatment. For example, if we reallocate 10% of the untreated students to treatment, the effect of the DGR on admission to secondary school would be 0.9 percentage points and significant at the 99% level. The vertical solid line defines the percentage of untreated reallocated to treatment where our point estimate no longer is significant. For each level of reallocation, we make 100 replications of each estimate and then compute the means and the CIs of these estimates.

Figure A3. The effect of the DGR on admission to upper secondary schooling for different levels of treatment of the untreated, for immigrants



Note: The solid line defines the point estimates for the effect of the DGR on admission to secondary schooling whereas the dotted lines define the corresponding 99% confidence interval. The horizontal axis defines the percentage of untreated (pupils in private schools) that we reallocate to treatment. For example, if we reallocate 10% of the untreated students to treatment, the effect of the DGR on admission to secondary school would be 5.5 percentage points and significant at the 99% level. The vertical solid line defines the percentage of untreated reallocated to treatment where our point estimate no longer is significant. For each level of reallocation, we make 100 replications of each estimate and then compute the means and the CIs of these estimates.