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Abstract

This paper examines the effects of student ability on teacher turnover using data from Stockholm high schools and an admission reform that led to the exogenous reshuffling of pupils. The results indicate that a 10-percentile-point increase in student credentials decreases the probability of a separation by up to 10 percentage points. These effects vary across different groups of teachers and are found mainly for mobility between schools rather than out of the profession. Teachers react mostly to direct measures of student ability (grades from compulsory school) rather than to other correlated characteristics (immigrant status, parental income or paternal cognitive skills). Finally, the data do not support the compensating wage differentials hypothesis.

JEL-Codes: I200, J400, J600.

Keywords: teacher labor market, student composition, student ability.

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1 Introduction

Educational interventions such as student busing or school choice change the composition of students in schools. These interventions have been motivated by the idea that certain groups of students might benefit from meeting better peers. However, sometimes the authorities simply want to put disadvantaged students into better schools. Irrespective of the motivation, these policies assume other inputs of the education production function are held constant, and thus rely heavily on their exogeneity with respect to student characteristics (Jackson 2009).[1] It is plausible, however, that changes in student composition affect other factors of input such as teacher composition or school resources (Hanushek 1986), and thus, policies aimed at improving performance may have unintended consequences.

This paper documents how exogenous changes in student composition affect teacher turnover. In particular, I investigate whether teachers who experience an inflow of high achieving students are less likely to quit their jobs compared with teachers who face an inflow of lower aptitude students.[2] Correlational studies suggest that pupil credentials are negatively related to teacher mobility, but we know relatively little about whether this descriptive relationship can be given a causal interpretation.[3][4] For example, teachers with strong preferences for student ability may sort into schools with high performing students that in an ordinary OLS would generate a biased, negative correlation between student ability and teacher turnover. To make matters worse, since aptitude is correlated with a variety of family characteristics, it is impossible to disentangle the "effect" of ability from, say, parental education in such an analysis.

Establishing the causal relationship between student ability and teacher mobility should be of interest for two reasons. First, if students with lower aptitude induce teachers to leave their schools, then the problem of an inflow of less able students may be reinforced by higher teacher turnover and by unfavorable sorting of teachers (Ronfeldt et al. 2013). Second, the potentially positive effects of policies aimed at reshuffling students between schools may be dwarfed by teacher mobility if highly productive teachers leave in response to an inflow of low aptitude pupils.

I explore a major reshuffling of students induced by an admission reform introduced in the municipality of Stockholm, Sweden, in the fall of 2000. Prior to the reform, students only applied for a program, with admission determined by their grades from lower secondary school. While students could state their preferred school, those living closest to a school had priority. Thus, although

[1] Examples of policies that lead to reshuffling of students are: increased freedom in school choice (Cullen et al. 2006); school voucher programs (Hsieh and Urquiola 2006); student busing (Jackson 2009); increased competition from the private sector (Jackson 2012; Hensvik 2012); changes in school admission policies (Söderström and Uusitalo 2010); or court-ordered desegregation (Reber 2005).

[2] Throughout the paper I interchangeably use terms: credentials, aptitude, achievement, performance, ability or GPA. These all refer to outgoing primary school percentiled GPA which is based on examination and teacher assessment.

[3] For example:Hanushek et al. (2004) for Texas; Falch and Strøm (2005) for Norway; Scafidi et al. (2007) for Georgia (US); Karbownik (2014) for Sweden.

[4] To the best of my knowledge there are only four quasi-experimental studies utilizing exogenous changes in school characteristics to study teacher turnover. The closet to this one is Jackson (2009) who uses variation in racial composition of schools due to North Carolina busing policy. Feng et al. (2010), Dizon-Ross (2016) and Gjefsen and Gunnes (2016) all study the effects of changes in school accountability rules.

the program choice included an element of school choice, it essentially limited the possibilities of students living in less affluent neighborhoods, as these students never had a chance at admission into permanently oversubscribed programs in prestigious schools.[5] The 2000 reform abolished all residence-based admission criteria and introduced a system based solely on performance in lower secondary school. The reform was intended to undo the effects of residential segregation and to give all students the option of attending the most elite schools, irrespective of where they lived.

I find that a 10–percentile-point increase in average incoming students' credentials decreases 4-year separation rates by up to 10 percentage points (pp). This effect is driven primarily by teachers switching schools rather than leaving the profession, and it is concentrated at the bottom two-thirds of the student ability distribution. The estimated effect is statistically significant, economically meaningful and heterogeneous across different groups of teachers. Furthermore, teachers seem to react mostly to the direct measures of student aptitude. Once student credentials are taken into account, other characteristics like immigration background become unrelated to teacher mobility. Finally, I do not find any sizable or significant effects on teacher earnings, which suggests that compensating wage differentials was not a major mediator of the ability shock.

2 Background

2.1 Educational institutions in Sweden

The Swedish schooling system starts with voluntary preschool and continues with nine years of compulsory education. Lower secondary school covers grades 7-9. Grades received in 9th grade - a combination of standardized examination and teacher assessment - determine a student's chances to advance to upper secondary (high) school. By law, Swedish municipalities are obliged to provide upper secondary schooling to all students who successfully complete compulsory education. Upper secondary school consists of different programs, lasts three years and provides eligibility for post-secondary education.

Private schooling is growing in Sweden and is encouraged by the government.[6] In 1992, Sweden introduced a school voucher reform that allowed for both non-profit and for-profit independent schools. The municipality must pay the independent schools for each student they can attract, with an amount corresponding roughly to the average per-student cost in the public schools.[7]

The teaching profession in Sweden is regulated and different qualifications are required depending on the type of school and the subject taught. Teaching at the secondary school level requires

[5] Although, Stockholm has a very well developed public transportation system, its housing market is highly regulated. It is much easier to buy or rent a flat in a less affluent neighborhood and commute within the city than it is to get housing in a prestigious location and cut down on transportation costs and time. This feature becomes even more important if the school admission system is, for the most part, residence based.

[6] The fraction of independent high schools has risen from 7.5% in the 1994/1995 school year to 32.0% in the 2004/2005 school year.

[7] An independent school receives around 85-95% of the average per-student cost in public schools and this amount varies from year to year. Some municipalities also have a socioeconomic gradient for the school voucher. Private schooling was effectively introduced at the lower secondary level in 1992, and at the upper secondary level in 1994 (Böhlmark and Lindahl 2007).

completion of special coursework beyond that required of a compulsory school teacher. Individuals from other professions who want to become teachers need to supplement their professional degrees with a minimum of 1.5 years of preparation in pedagogy, didactics and teaching practice. However, uncertified teachers can also be hired on short-term contracts.

Municipalities are the primary employers of public school teachers in Sweden, and thus handle the responsibility of recruiting them.[8] In practice, however, the decisions regarding teacher recruitment, selection and employment are made at the school level by a principal (Böhlmark et al. 2016). Finally, teacher wages are determined at the local level through individual bargaining between a teacher and a principal, given the collective bargaining outcome set at the national level.[9]

2.2 The admission reform

In the fall of 1999, the municipality of Stockholm passed a regulation that changed high school admission rules. Before the 1999-00 school year, students applied only for a program, and their grades from lower secondary school determined admission. While students could state their preferred school, those living closest to a school had priority.[10] This restriction was particularly binding for the two most popular and broadest programs: social sciences (samhällskunskap) and natural sciences (naturvetenskap). Thus, students from low-income, disadvantaged districts had virtually no chance of attending the most popular inner-city schools, even if they had competitive grades.

The student cohort applying to high school in May of 2000 for the 2000-01 school year faced different admission criteria. In line with the new regulation, all residence-based school allocation within the municipality of Stockholm was abolished and replaced by a system based exclusively on GPA from lower-secondary school (9th grade). In this paper, this GPA is the treatment variable of interest. Under the new system, students apply for a specific school and program, and applicants are ranked by schools and programs. If a student's first choice is not accepted, the second choice is considered, and so on.

Most municipalities surrounding Stockholm do not offer all of the programs, and a student has the right to attend their chosen program in another municipality, financed by the municipality in which they reside. Cross-municipality commuting is relatively common in Sweden, and if increased school choice incentivized more students from outside of Stockholm to apply to schools in Stockholm, crowd out of students residing in Stockholm could occur. Furthermore, Stockholm schools could decide to change the number of admitted students in response to higher demand, which would in turn lead to either higher student-teacher ratios or the need for additional hires. I address the

[8] For more information on the reform that shifted responsibility for schooling from the central government to municipalities see Fredriksson and Öckert (2008).

[9] Individualized pay was introduced in 1996 and is discussed in detail by Hensvik (2012), in a survey by Lindholm (2006) and in a report by Skolverket (2009).

[10] For example, if school A, located in downtown Stockholm, excelled in a science program and there were enough students living nearby who subscribed to the program, then students with better grades residing in e.g. Tensta (a relatively poor and disadvantaged district in Stockholm) would be unable to gain admission to the program. Independent high schools were allowed to select students on the basis of GPA also before the reform and there were no geographical restrictions in applying to these schools.

latter issue in Section 4, and my calculations show that the fraction of students living outside of Stockholm but attending Stockholm schools is stable at around 20% over the analyzed period.

Söderström and Uusitalo (2010) found clear evidence that the Stockholm admission reform affected both student mobility and their sorting by ability, as well as ethnic and socioeconomic background. However, the segregation between immigrants and natives increased more than one would expect as a result of increased sorting by GPA.

2.3 Data and descriptive statistics

This paper utilizes multiple Swedish population-wide registries. The main data source is the teacher registry that covers all teaches employed in Swedish schools during the school years from 1991-92 through 2004-05. It contains information on teachers' education, specialization, experience, certification, place of work, type of contract (permanent vs. temporary) and workload. I have matched these data to background information on age, gender, immigration histories, education and employment. The pupil registries for lower and upper secondary schools are used to obtain information on students in a given upper secondary school along with their credentials from lower secondary school. Measures of family background were also obtained by matching students to their parents. Administrative records on earnings provide information on teachers' monetary compensations. The details of the sample construction are discussed in the appendix.

In the pooled sample of all secondary schools in Stockholm prior to the 1999-00 school year, there are 8 private and 21 public schools. However, given the timing of the reform and estimation strategy, I focus on secondary schools that have been in operation in Stockholm for all school years from 1994-95 to 2004-05.[11] Since the reform was only implemented in the municipality of Stockholm, I compare basic descriptive statistics for Stockholm and non-Stockholm schools for the last pre-reform (1999-00) and first post-reform (2000-01) school year in Table A1. Stockholm is more affluent in many dimensions than the rest of Sweden, and schools in Stockholm admit higher-achieving students who come from richer and better educated families. At the same time, due to Stockholm's major concentration of immigrants, these schools admit more minority students. Stockholm schools also employ more teachers with university diplomas, but these teachers have less experience on average.

Since implementation of the reform occurred during the 2000-01 school year, I present descriptive evidence for 1999-00 as the last pre-reform school year and 2000-01 as the first post-reform school year. Table 1 presents descriptive statistics based on the 1994-95 to 2004-05 panel of Stockholm schools for the immediate pre- and post-reform periods, separated by changes in student composition. In particular, for each school j, I calculate the difference between mean-incoming-student GPA in the first post-reform year, 2000-01, and the last pre-reform year, 1999-00. Then, I order these

[11] This restriction corresponds to the full range of years needed to perform analysis in the paper, including placebo regressions. It also addresses potential composition effects related to school openings and closures. In total the data set used in the analysis includes 18 schools, one of which is private. The results are robust to dropping this one private school and also carry over if I use a repeated cross-section of schools or focus on even more restrictive panel sample that only includes 15 schools in uninterrupted operation between 1991/1992 and 2004/2005.

differences from the schools most negatively affected to those most positively affected and divide the rankings into terciles. I call these schools downward-, middle- and upward-shocked schools. The bottom of the table reports the number of schools and teachers in each group.

The reform reshuffled incoming first-grade pupils between schools in Stockholm. In particular, student GPA in the upward-shocked schools increased from 64.6 to 73.3 percentile points, while in the downward-shocked schools, student GPA decreased from 55.3 to 50.0 percentile points. This widened the gap between these groups of schools from less than 10 percentile points to over 20, equivalent to about two-thirds of a standard deviation change in student achievement.

The other student characteristics correlated with student ability, such as parental income or share of minorities, also changed. For example, the gap between schools with the most and least improvement in mean parental income doubled, while the minority students' gap increased by 40 percent. The reform also affected the composition of the teacher stock: On average, there were more teachers with university diplomas in the upward-shocked schools and more teachers on temporary contracts in downward-shocked schools in the post-reform period than in the pre-reform period. The gap in teacher compensation did not change, and interestingly, teachers in schools with higher-achieving students earned less than those in schools with low-performing students, suggesting the presence of compensating wage differentials in a system with a relatively flexible teacher pay scheme.

2.4 Identifying variation

Due to the reform, from one year to another, the same set of teachers experienced changes in incoming students' ability. In particular, some teachers ended up with lower-achieving pupils, while other teachers ended up with higher-achieving pupils than in the pre-reform period. The aim of this paper is to study how teacher labor supply decisions changed in response to this unexpected change in student credentials.

Figure 1 shows the differences in average student credentials for every year (1996-97 to 2004-05) and for upward- and downward-shocked schools relative to average student credentials in the same schools in the 1995-96 school year. The differences are plotted as points, while the whiskers for each year show 95-percent confidence intervals from linear regressions, with the difference in the average student credentials compared to 1995 as the dependent variable and year dummies (one for each year between 1996 and 2004) as independent variables. Figure 1 clearly shows that the reform caused a differential change in average student ability. Prior to the reform, there are no significant differences in average student credentials in upward- and downward-shocked schools. Yet, post-reform, the average credentials for these two groups of schools clearly diverge from one another. For the most part, I do not explore the changes in average characteristics in this paper, but rather, I focus on the changes in incoming student credentials since this is the margin for which the shock induced by the reform was most pronounced. Naturally, the two measures are highly correlated, and Figure A1 confirms that the largest shock in incoming students' ability occurred between the 1999-00 and 2000-01 school years, while the subsequently admitted cohorts mimicked the ability of

the first graders from the 2000-01 school year.[12]

Figures 1 and A1 document that the reform reshuffled students across schools in the municipality of Stockholm. In Figure 2, I provide some initial evidence on how this reshuffling affected the probability of a teacher leaving their current employment. In particular, I start with the pool of teachers in 1995 (pre-reform) and 2000 (post-reform) and plot the fraction of teachers that remained employed in the same school from 1 to 4 years.[13] I plot these percentages separately for upward- and downward-shocked schools, defined in the same manner as in Figures 1 and A1. Although the figure is uninformative about the pre-reform trends in teacher mobility, it shows the mobility differences in levels before and after the reform for the two types of schools. Upward-shocked schools had higher levels of turnover before the reform, but these same schools switched to having lower turnover rates post-reform.

3 Empirical specification

Teacher labor supply decisions can be framed within a job turnover theory (Jovanovic 1979), and I assume that initially observed employment is a result of decisions maximizing a teacher's utility function with respect to job characteristics (Jackson 2013).[14] For simplicity, assume that teachers only value the ability of their students and the monetary compensation that they obtain from employment, and that they weakly prefer higher compensation and better students. Thus, the quality of the match between an individual teacher and a school can be altered either by changes in student composition or by changes in wages. The former factor was exogenously altered starting in school year 2000-01 (Figure 1), and it caused teachers to face a different set of students from one year to the next. Thus, if teachers value working with high achieving students, then they will be less likely to leave schools experiencing an inflow of students with better credentials. Furthermore, if monetary and student ability inputs to a teacher's utility function are jointly determined, compensating wage differentials, then teachers who experience an inflow of students with worse credentials need to experience a rise in monetary compensation. Otherwise, these teachers will leave these schools when presented with an outside option.

[12]Throughout the paper I use the incoming students' ability (Figure A1) as the main treatment variable, however, the results are qualitatively similar if I use all-grades GPA (Figure 1) as student ability measure. In fact, if we compare average student characteristics between school years 1999/2000 and 2000/2001 in a regression framework with year and school fixed effects then we are effectively comparing 3rd grade students in pre-reform period to 1st grade students in post-reform period. If the reform was truly exogenous the results should be invariant to the specification of treatment because the correlation between 1st and 3rd graders in the pre-reform period will be high, while the correlation between 3rd graders in the pre-reform and 1st graders in the post-reform period will be low.

[13]The preferred measure of mobility capturing fully implemented reform requires at least four years of observations, and thus it is not possible to draw a graph akin to Figure 1 for the dependent variable. I discuss the construction of the treatment and outcome variables in Section 3, and I present placebo regressions supporting no differential trends hypothesis in Table 4.

[14]Even in equilibrium the turnover rate will not be zero. First, at any point in time there are also "bad matches" between schools and teachers, and thus there are individuals switching schools from year to year. Second, teachers employed on fixed contracts (e.g. as substitutes for permanent teachers) leave their position once it can be re-filled. Third, teachers retire or pass away, and thus, they drop out of the sample and new teachers need to be hired as replacements. I shut down the retirement channel by limiting the sample to teachers no older than 58 years of age.

The labor supply decisions of teachers interact with school demand for new or existing teachers. Although firing teachers is relatively difficult in Swedish schools, quitting is not. Thus, the principal's role in this optimization problem is related to either manipulating teacher compensation or hiring new teachers when they face a teacher shortage.

Econometrically, the variation induced by the reform can be framed as a difference-in-differences estimator. Since the reform was implemented during the 2000-01 school year, the first difference compares schools before and after this date, while the second difference compares schools that experienced differential changes in student ability. For teacher turnover to result from changes in student aptitude only, I require that students did not select schools based on the underlying trends in teacher turnover and that teachers did not anticipate the inflow of lower-/higher-achieving students. I test this assumption in Table 4.

The nature of the outcome variable (job mobility) requires two years of data to construct a single observation, that is, I need to know a teacher's employment status in periods t and t+1 to generate a mobility indicator. Furthermore, since high school education in Sweden consists of three grades, it took three years for the reform to reach full implementation. During the 2000-01 year, only a third of the student stock had been admitted under the new rules, and the reform did not come into full effect until the 2002-03 school year. Due to this feature, I study changes in teacher mobility up to three years after the reform. Additionally, for the pre-treatment period not to overlap with the post-treatment period, I lag the pre-treatment measure of student ability one year for every additional year that I follow teacher mobility. In terms of outcome variable for the full effect of the reform, I compare the probability that teacher i in school j in 1997-98 had left the school by 2000-01 with the probability that teacher i in school j in 2000-01 had left the school by 2003-04. This can be written as:

$$\begin{aligned}(Y_{ij}^{2000} - Y_{ij}^{2000+k}) - (Y_{ij}^{2000-k} - Y_{ij}^{2000}) \quad &= \quad \alpha + \beta(T_j^{2000} - T_j^{2000-k}) + \\ \gamma(X_{ij}^{2000} - X_{ij}^{2000-k}) + \delta_j + \varphi_{2000} + \varepsilon_{ij} \quad &\end{aligned} \tag{1}$$

where i denotes individual teachers, j denotes schools and k denotes exposure length. The variable Y equals unity if teacher i is observed in school j in a given year and zero otherwise; T represents student credentials or any alternative student characteristic measured at school j in a given year; X denotes individual teacher covariates including gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience; the parameters δ and φ are school and time fixed effects; and ε is a heteroscedasticity-robust standard error. The coefficient of interest is β and it identifies the effect of student ability on teacher mobility.[15]

Equation 1 estimates the causal effect of student credentials on the probability that a teacher separates from their current school, assuming that changes in student composition are not correlated

[15]Each regression uses only one pre- and one post-reform period, although I use multiple years to construct the outcome variables.

with changes in teacher mobility in the reform's absence. One testable implication of this identifying assumption is that post-reform changes in student ability should not be correlated with pre-reform changes in teacher mobility. This is equivalent to the common trends assumption. However, for the placebo analysis to be meaningful, the placebo treatment period must not overlap with the true treatment period. Thus, studying pre-reform teacher mobility over a 3-year period requires lagging the outcome variable by three years. This can be written as:

$$\begin{aligned}(Y_{ij}^{2000-k} - Y_{ij}^{2000}) - (Y_{ij}^{2000-2\cdot k} - Y_{ij}^{2000-k}) &= \alpha + \beta(T_j^{2000} - T_j^{2000-k}) + \\ \gamma(X_{ij}^{2000-k} - X_{ij}^{2000-2\cdot k}) + \delta_j + \varphi_{2000-k} + \varepsilon_{ij}\end{aligned} \tag{2}$$

where Y, T, X, δ, φ and ε are defined as in equation 1.

Equation 2 estimates the placebo effect, but the data also allow me to account for an anticipation effect directly by lagging the dependent variable by one period. In equation 3 this procedure mechanically purges the possibility of a reaction to student ability in advance of the policy implementation but requires following teachers for four years for full implementation of the reform. In other words, the point estimates for one-, two- and three-year mobility estimated by equation 1 should be compared to point estimates for two-, three- and four-year mobility estimated by equation 3. If there is no anticipation effect and the placebo regression specified in equation 2 does not yield any large or significant results, then we should observe a close to zero estimate in a one-period window in this specification. More formally:

$$\begin{aligned}(Y_{ij}^{1999} - Y_{ij}^{1999+k}) - (Y_{ij}^{1999-k} - Y_{ij}^{1999}) &= \alpha + \beta(T_j^{2000} - T_j^{2000-k}) + \\ \gamma(X_{ij}^{1999} - X_{ij}^{1999-k}) + \delta_j + \varphi_{1999} + \varepsilon_{ij}\end{aligned} \tag{3}$$

where Y, T, X, δ, φ and ε are defined as in equation 1. Tables A2 and A3 provide details about the specific school years that are used for outcome and treatment variables of different exposure lengths in regressions defined by equations 1 and 3. In all main regressions I use robust standard errors.[16]

In order to illustrate the logic behind the difference-in-differences strategy used in this paper, Table 2 presents changes in teacher mobility over time for schools that experienced positive or

[16]The common approach in the literature is to assume independence at the level of aggregation where the variation in treatment is present (Bertrand et al. 2004). However, clustered standard errors only have asymptotic properties, and in the regressions with 18 schools, these large sample properties cannot be invoked (Angrist and Pischke 2009). For this reason I have chosen heteroscedasticity robust standard errors for the results reported in the paper, thus imposing the assumption that teachers are independent within schools. In the Appendix I also report standard errors with alternative clustering for the preferred specification – equation 3 (i.e. columns 2 and 3 in Table 6). More specifically in Table A4 I report (i) robust standard errors as a reference (ii) standard errors clustered at the school level; (iii) standard errors clustered at the school×year level, thus allowing interdependence between teachers in a school in a specific year but not across years; (iv) standard errors from regressing the first-differences on the treatment variable using aggregated data. In Table A5 I re-do the analyses from Table A4 but with an unbalanced panel allowing the maximum number of schools in data. Irrespective of the specification the preferred estimate is always statistically significant at least at 5% level.

negative changes in student credentials.[17] I divide schools into three groups based on their changes in incoming student credentials between school years 1999-00 (pre-reform) and 2000-01 (post-reform). In the first column, I show data for the third of schools with the most positive changes in incoming student GPA (one-third upward), while in the second column I show data for the one-third of schools with the least positive (or negative) changes in incoming student GPA (one-third downward). On average, student credentials increased by 13.96 percentile points in the former schools and decreased by 8.27 percentile points in the latter schools.[18] Concurrently, teacher mobility decreased by 19 pp in upward-shocked schools, and there was virtually no change in mobility in downward-shocked schools. The lack of change in teacher mobility in the downward-shocked schools might indicate that principals in these schools increased teacher salaries to compensate for negative shock in student composition and to prevent teachers from leaving.

By calculating the ratio of the two changes (-17 pp divided by 22 percentile points), I obtain the Wald estimate of 3-year teacher mobility on incoming student credentials. The estimate implies that increasing incoming student credentials by 10 percentile points reduces teacher mobility by 7 pp. In the reminder of the paper I investigate whether these results hold up in a more formal regression analysis where the dummy variable for school shock is replaced with a continuous measure of incoming student credentials.

Figure A2 illustrates how I exploit the variation in changes to student ability by plotting the differences in mobility for each school against the differences in incoming students' GPA in that school. It suggests that, on average, the negatively-shocked schools experienced either small increases in teacher mobility or no changes at all. On the other hand, schools that were positively shocked experienced relatively large reductions in teacher mobility. The dashed line in the figure shows a linear fit and points towards a negative relationship between changes in student ability and changes in teacher mobility.[19]

4 Main results

First, I present the results assuming that teachers did not anticipate changes in student credentials. Table 3 reports the estimates for the effects of changes in student ability for one-year (row 1), two-year (row 2) and three-year mobility (row 3). In column (1) I present correlations between GPA

[17] In order to provide better intuition about the timing of the reform and the reshuffling of students I start with the model that does not account for the anticipation effect and does not require a lagged dependent variable, however, the results accounting for the anticipation effects are similar.

[18] This does not indicate that the average student quality in Stockholm increased due to the reform as the comparison excludes the middle tercile schools. However, comparing the ability of incoming students between 1997 and 2000 indeed suggests a 4 percentile points increase. This can be driven by multiple factors: focusing on a panel of more stable schools, differential inflow of high-ability students from outside-of-Stockholm; or differential grade inflation. When analyzing all schools in Stockholm the average incoming students GPA is 54 in 1997, 52 in 1998, 56 in 1999, and 58 in 2000. Furthermore, my calculations show that there is no differential inflow of students residing outside of Stockholm. Thus, it suggests that over time there is some grade inflation at the upper end of the grade distribution, however, it is small in comparison to the magnitude of the shock and should be, at least partially, accounted for by school and time fixed effects.

[19] Graph based on fully implemented reform and accounting for anticipation effect - equation 3 - points to the same conclusion.

and mobility, in column (2) I present difference-in-differences estimates without controlling for any observable teacher characteristics, and in column (3) I condition on a set of teacher controls. The estimates do not change when I control for teacher characteristics, supporting the quasi-experimental nature of student resorting.[20] Since the reform gradually changed the student composition in schools, teachers' responses are stronger the larger the share of students that gained admission under the new rules. The point estimate in row (3) and column (3) indicates that a 10-percentile-point increase in student aptitude reduces the probability of teacher turnover within three years by 7 pp.

Since the GPA of incoming students at the school was made public around May 2000, teachers could have left the school until October 2000; and it is thus important to gauge whether teachers reacted to information about the ability of incoming students or to the realization of incoming students' ability.[21] In an attempt to detect any potential anticipation effects, I estimate Equation 2. The results are presented in Table 4 and support the statement that teachers did not seem to respond to the information on future student sorting, and that students have not sorted based on anticipated teacher mobility.

The results so far focused on teachers' labor supply, but these effects might also be driven by school-level general equilibrium effects, e.g., in response to the reform, schools that experience an inflow of high-achieving students simultaneously grow to accommodate an increase in demand, which mechanically leads to reductions in teacher turnover.[22] Thus, in Table 5 I test whether the reform affected the number of students enrolled, number of teachers in a school and student-teacher ratio. Furthermore, in Panel D, I investigate the effects on teachers' earnings to address the compensating wage differentials hypothesis.[23] Contrary to the mobility analysis, these regressions are based on a static model in which the outcome is determined at a given point in time. Furthermore, since school composition was determined during the pre-period of September 1999, and the reform was not voted into power until later in 1999, there is no need to account for an anticipation effect in this setting.[24] The results in Table 5 show that neither the number of students, the number of teachers nor the student-teacher ratio responded to the reshuffling. In sum, it seems unlikely that post-reform changes in teacher mobility were a mechanical consequence of changes in school size or

[20] Individual control variables do not include teacher earnings or type of contract as these might be an outcome of the reform. The estimates are identical whether I condition on earnings and type of contract or not.

[21] Although teachers could have left within a school year, such situations are rare, and this type of mobility would be captured by comparing two adjacent years.

[22] This would not be consistent with reductions in hires documented in Table 7.

[23] Note that the funding of schools in Sweden is tied to the number of enrolled students. The reform could have forced some students to change schools as a response to changes in peer composition. I address this issue by estimating a model in which I define the outcome as the probability that I do not observe currently enrolled student i in school j in the next school year, and construct the mean of this probability at the school level. The regression framework is identical to Table 5 with mean probabilities as outcomes, and I lag the last pre-reform period by one (to 1998) in order to account for potential anticipation effects by the students. The results are very similar in regressions that do not account for anticipation effects. For each exposure length I find small but significant results on student mobility. Since these estimates do not appear to be quantitatively meaningful and I do not find any effects on the average school size, I conclude that this general equilibrium effect is unlikely to play a major role in a teacher's decision making process.

[24] The results remain unchanged if I account for anticipation effect and treat year 1998 as the last pre-reform period.

school resources.

Panel D of Table 5 further explores the compensating wage differential hypothesis by analyzing the effects of changes in student credentials on teacher earnings. If student credentials and teacher compensations are substitutes then it may be possible to retain teachers in adversely shocked schools by increasing their salaries. This regression takes into account anticipation effects and compares earnings of teachers in school year 1998-99 and 2002-03 to capture full implementation of the reform.[25] All point estimates are small and statistically insignificant, suggesting that teachers did not experience changes in compensation to offset the adverse changes to their students' ability.

The placebo estimates in Table 4 suggest no significant anticipation effects, but in order to further rule out the possibility of a bias, I estimate the effects lagging the dependent variable by one year. Since the reform was not yet announced early in the 1999-00 school year, teachers could not possibly anticipate changes in student composition. The first row of Table 6 compares one-year mobility in 1998-99 to one-year mobility in 1999-00, and supports no anticipation effects.[26] The estimates in column (3) (rows 2-4) should be compared to estimates from column (3) in Table 3, and are modestly larger.

The preferred point estimate - row (4) of column (3) - suggests that, when the reform was fully implemented, a 10-percentile-point increase in student ability reduced the probability of a teacher leaving their school by 10 pp. Alternatively, an increase of one standard deviation (about 16) in incoming student credentials decreased the probability of a separation within four years by 16 pp.[27] Given that the average 4-year separation rate in this sample is 33%, the result implies about 50% reduction in mobility. This effect size may appear large, but there are only two schools in Stockholm in which students improved by more than a standard deviation. The majority of schools improved or deteriorated by up to half of a standard deviation.

Finally, in order to visualize how the effect of changes in student quality evolved as the reform progressed, Figure 3 shows point estimates from column (3) of Table 6 with 95% confidence intervals. The line is clearly downward sloping, starting close to zero, as there are virtually no anticipation effects. The F-test rejects the hypothesis that all four estimates are identical (p=0.018).

The decrease in teacher mobility documented in Tables 3 and 6, paired with no change in school size, suggests that schools should also reduce hiring. Table 7 documents this phenomenon while taking anticipation effects into account. Although less precisely estimated and modestly smaller in terms of effect sizes than the main turnover estimates, these coefficients indeed support the notion of a reduction in the number of teachers hired.

[25] The results are very similar without accounting for anticipation effect, namely using school year 1999/2000 as the last pre-reform year.

[26] Although the OLS point estimate in column (1) is significantly different from zero, it is very small in magnitude and does not point quantitatively towards any substantial bias.

[27] When I include the quadratic in students' credentials in the equation the coefficient on linear part remains negative and significant while the coefficient on quadratic term is positive and significant. Thus, the relationship between student ability and teacher mobility is estimated to be convex i.e. the higher inflow of high achieving students has marginally diminishing effect on teacher separation rates.

5 Correlates of student credentials

The evidence so far suggests that higher student aptitude reduces the probability that teachers leave their employment. To the best of my knowledge, this is the first paper that estimates causal effects of changes in student ability - as measured by academic credentials - on teacher labor supply decisions. Student GPA is, however, correlated with other observable variables such as the fraction of minority students or parental wealth.[28] I thus investigate whether the effects are due to direct measures of student aptitude or to variables correlated with student GPA.

The results presented in Table 8 focus on the preferred specification based on column (3) and row (4) of Table 6. The first row of Table 8 presents estimates in which the treatment is defined as a fraction of first-generation immigrants (a correlation of 0.44 with GPA), the second row presents estimates for mean parental income (a correlation of 0.82 with GPA), the third row presents estimates for mean parental education (a correlation of 0.91 with GPA), and the fourth row presents estimates for mean combined cognitive and non-cognitive assessment of fathers (a correlation of 0.80 with GPA).[29] Column (1) presents the effects of the "alternative" characteristics, while column (2) adds student GPA in a horse race between direct and indirect measures of student ability.

The unconditional estimates in row (1) confirm that the fraction of minorities at a school correlates positively with the probability of job separation (Hanushek et al. 2004; Falch and Strøm 2005; Barbieri et al. 2011; Karbownik 2014).[30] In row (2), the coefficient on mean yearly income in 100 000 SEK is -0.069 with a standard error of 0.035, which is small given that the mean yearly parental income in the studied group of schools is 380 931 SEK. Similarly, rows (3) and (4) indicate significant and robust negative associations of increased parental education as well as paternal aptitude and job separation, which is consistent with the intergenerational transmission of education and cognitive skills (Björklund et al. 2006; Black et al. 2009).

In column (2), where the regression is augmented with student GPA, estimates for the fraction of minorities and paternal military assessments become insignificant and decrease in size, and the coefficient on mean parental income actually turns positive. On the other hand, both parental education and student GPA are negative. Overall, the estimates in column (2) suggest that teachers primarily value student aptitude, but that some of the response to changes in student credentials may be driven by changes in students' socioeconomic backgrounds.[31]

[28] For instance, Jackson (2009) used a similar identification strategy to gauge the causal effect of the reshuffling of minority students on teacher mobility.

[29] These data are available only for some fathers, and the coverage at school level increases from 24 to 51% over the time period used in this analysis. On average, I have information about fathers of 40% of the pupils. This limitation is driven by the fact that the registries are not available for individuals tested before 1970 and immigrants. Nonetheless, I calculate the mean for all fathers with assessment information available in a given school, and in these regressions I also control for the share of students in a given school and year for whom I do not observe paternal cognitive and non-cognitive assessments.

[30] Unlike other researchers I do not find any evidence for the clustering of immigrant teachers and minority students in either specification.

[31] Since direct (student GPA) and indirect (share of immigrants, parental income and education, paternal military test scores) measures are highly correlated, it is plausible that models in column (2) pick up non-linear measures of student ability. When I add the square of student GPA to the estimates in column (2) it is positive and significant in all estimations (similar to the main specification). At the same time, the linear term in student GPA remains highly

6 Heterogeneity

The richness and completeness of Swedish registry data allows me to investigate heterogeneity in the effects of student ability, as the consequences of the admissions reform could be very different for different groups of teachers. Therefore, I analyze how the response to changes in student composition differs by teacher's education, experience, gender, specialization, type of employment, their destination, and the baseline average school-level student ability.

Table 9 presents a range of heterogeneity findings. The table has the following structure: The first column reports the fraction of teachers in each group, the second column reports the mean of 4-year mobility, and the third column reports the point estimate and standard error of the effect of student ability on 4-year teacher mobility. I first consider teacher education and experience, as these are important predictors of student achievement (Boyd et al. 2005; Harris and Sass 2011). More than a quarter of secondary school teachers in Stockholm do not have a formal university degree, and these teachers have substantially higher turnover rates (42% vs. 30%). The coefficient is larger for teachers with a university degree, and the effect size is even bigger because these teachers have lower baseline mobility. Similar conclusions apply to teacher experience - the point estimates for the three groups are similar in magnitude, but the effect sizes range from 2.1% for the least experienced to as high as 4.7% for the most experienced teachers due to differences in average turnover rates between groups.

I also consider whether the estimated effect varies by teacher gender. Female teachers are somewhat less mobile than male teachers, and the estimate for this group is smaller (effect size of 3.4% and 2.5% for males and females, respectively). Science teachers are another group that receives a lot of attention in the media and in research (Edmark and Nordström Skan 2010).[32] From the labor market perspective, these teachers provide important STEM skills (Joensen and Nielsen 2009), but they may also have more favorable outside options. Indeed, science teachers have higher mobility rates, but the effect size estimates for both groups are very similar. Finally, I present estimates separately for teachers on permanent and temporary contracts. The latter are typically employed on fixed-term contracts, often as replacements for teachers on extended leave. Nearly 20% of teachers in Stockholm are employed on a temporary basis, and they have more than twice the rate of job separations. The estimated coefficients indicate, however, that these teachers are less sensitive to changes in average student ability, which may be related to a shorter-run employment perspective at any given school. In sum, the heterogeneity suggests that certain groups of teachers react in a more elastic way to changes in their students' ability.

The models used so far pool all teacher job changes into a single dimension. However, previous research indicates that the correlations with teacher characteristics differ depending on the destination (Lankford et al. 2002). In Table 10, I investigate whether the effects of changes in student credentials are stronger along some mobility margins than others. In particular, I estimate the effect

significant and negative in all cases. Finally, the significant negative coefficient on parental education becomes small and insignificant suggesting that indeed it was picking up non-linearity in student ability. The coefficient on parental income remains positive and significant with a similar point estimate.

[32] Science teachers are defined as these teaching: mathematics, physics, chemistry, biology and computer science.

for mobility within high schools (row (1)), to all levels of education (row (2)), to private schools (row (3)), out of the profession (row (4)) and to high schools with higher baseline achieving students (row (5)). Since policymakers should be particularly interested in whether highly-educated teachers tend to leave the profession in response to such a reform, I also estimate the above specifications separately for the whole population (column (2)) and for teachers with a university degree (column (4)).

The estimates suggest that teachers react mostly in terms of mobility within either high schools or teaching profession in general, and they seek schools with higher achieving pupils. The estimates of the probability of leaving the profession or moving to a private school are smaller and statistically insignificant, which is consistent with Jackson (2013), who argues that teachers will adjust their match quality within the profession rather than outflow to a different occupation. The estimates are also similar in magnitude for both teachers with and without completed university education.

The last two heterogeneity analyses investigate the distributional effects of changes in student credentials. Table 11 shows how teachers initially employed in schools from different parts of the student GPA distribution respond to changes in their pupils' composition, while Table 12 documents how teachers react to changes in the fraction of students from different parts of the GPA distribution.[33] Thus, Table 11 reports heterogeneous responses to the same treatment, while Table 12 documents responses to heterogeneous treatments.

The results in Table 11 indicate that only teachers employed in the bottom two terciles of the distribution react to changes in student credentials, and that the effect size is the largest for schools with the lowest baseline student achievement.[34] In fact, teachers in the bottom tercile of the student achievement distribution are the only ones who are significantly less likely to leave the profession in favor of a different occupation, and a point estimate of -0.007 implies an effect size of 6.0%. On the other hand, middle tercile teachers switch jobs within the profession.

Table 12 largely confirms the findings discussed above. Teachers who experience an inflow of students coming from the bottom tercile of the achievement distribution are more likely to leave their current employment for a different school or a different occupation. On the other hand, teachers who get a positive shock are less likely to terminate their employment.[35]

[33] For this purpose, for every school and year, I calculate the fraction of students admitted from each tercile of the GPA distribution. Then, I use these three variables in separate regressions as a substitute for the average student credentials.

[34] When I split the sample into halves I only find significant estimate for the bottom half. It is -0.017 (0.004) while the estimate for the top half is -0.003 (0.003), implying effect sizes of 6.7 and 1.3 percent, respectively. When I split the sample into quartiles I find significant estimates for the bottom two quartiles and top quartile. These are -0.012 (0.005), -0.025 (0.006), -0.003 (0.003) and -0.015 (0.008) implying effect sizes of 5.3, 8.8, 1.4, 5.9 percent for lowest to highest quartiles, respectively.

[35] These results are similar when I use quartiles of student ability distribution instead. Namely inflow of students from the bottom two quartiles increases the probability of separation while inflow from the top two quartiles decreases the probability of separation. This is true for both total turnover and within profession mobility, while switching to a different occupation appears to be affected only by inflow of students from the bottom quartile of the distribution.

7 Conclusions

A number of educational policies involve placing certain groups of students into more favorable school environments, in the hope that interacting with better performing peers will boost their own academic performance. However, the success of such policies relies on, among other things, how teachers respond to changes in student ability. This paper provides evidence on the causal effect of student aptitude on teacher mobility using data from Stockholm high schools and exogenous changes in incoming students' GPA following an admission reform.

The results show that an increase in student aptitude leads to lower teacher mobility, and a 10-percentile-point increase in incoming students' GPA decreases the probability that a teacher will leave their school by up to 10 pp. I show that this effect is robust to different model specifications, and I account for the fact that changes in student aptitude in different schools might be related to pre-existing trends in teacher mobility. The effects vary by types of teachers and are found mostly for mobility between schools rather than out of the profession. Furthermore, teachers seem to react mostly to direct measures of student ability rather than to characteristics correlated with student aptitude, namely immigrant status, parental income and education or paternal cognitive and non-cognitive skills. Finally, I do not find any significant or sizable effects of changes in student ability on teacher earnings, suggesting that compensating wage differentials do not have a significant mediating effect.

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Figures and Tables

Table 1: Descriptive Statistics

Variables	Pre-reform = 1999/2000			Post-reform = 2000/2001		
	Change in student credentials					
	(1) 1/3 downward	(2) 1/3 middle	(3) 1/3 upward	(4) 1/3 downward	(5) 1/3 middle	(6) 1/3 upward
Outcome variable						
One-year mobility	0.11	0.15	0.11	0.13	0.12	0.08
	(0.32)	(0.35)	(0.31)	(0.33)	(0.32)	(0.78)
Treatment variable						
Incoming students' GPA	55.27	50.30	64.56	49.95	51.69	73.32
	(13.72)	(20.20)	(11.24)	(13.75)	(21.10)	(13.98)
Teacher characteristics						
Female	0.57	0.54	0.47	0.54	0.56	0.46
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Experience (years)	12.90	11.20	11.90	12.89	11.45	11.49
	(6.98)	(6.99)	(7.56)	(7.49)	(7.13)	(7.73)
Has university diploma	0.78	0.65	0.75	0.78	0.68	0.78
	(0.41)	(0.48)	(0.43)	(0.42)	(0.47)	(0.41)
Employed on temporary contract	0.16	0.25	0.24	0.22	0.25	0.24
	(0.37)	(0.43)	(0.43)	(0.41)	(0.44)	(0.43)
Yearly earnings in 1000 SEK	247	219	216	253	232	223
	(86)	(75)	(82)	(89)	(77)	(89)
Student characteristics (alternative treatment variables)						
First generation immigrant	0.20	0.12	0.11	0.22	0.13	0.09
	(0.07)	(0.05)	(0.05)	(0.11)	(0.04)	(0.04)
Yearly parental income in 1000 SEK	352	386	427	339	407	488
	(68)	(97)	(55)	(71)	(95)	(100)
Parental education	12.44	12.85	13.79	12.49	12.93	13.90
	(0.59)	(1.32)	(0.90)	(0.51)	(1.27)	(0.86)
Paternal draft score	55.62	55.42	59.08	55.03	54.04	59.21
	(6.23)	(7.54)	(4.69)	(8.48)	(7.24)	(4.40)
Number of schools	6	6	6	6	6	6
Number of teachers	240	296	316	232	302	350

Note: Means and standard deviations. Columns (1) to (3) present descriptive statistics for the last pre-reform year while columns (4) to (6) present descriptive statistics for the first post-reform year. All descriptive statistics are based on the panel of Stockholm schools in operation between 1994/1995 and 2004/2005 and refer to incoming first year students as far as aggregate school characteristics are concerned. Columns (1) and (4) describe a third of most downward shocked schools. Columns (2) and (5) describe a third of middle tercile schools. Columns (3) and (6) describe a third of most upward shocked schools. Shock is defined as a difference between mean students' credentials measured by primary school 9th grade GPA (only first-grade students who applied to school in the same year) in high school j in the first post-reform year 2000 and mean students' credentials in the last pre-reform year 1999 in these same schools.

Table 2: Wald Estimator

	Schools		
	1/3 upward shocked	1/3 downward shocked	Difference
Treatment: Student credentials - percentile ranked GPA from 9th grade in primary school.			
Year 2000	73.32	49.95	23.37***
	(13.98)	(13.75)	(1.18)
Year 1997	59.36	58.22	1.14
	(14.72)	(13.67)	(1.15)
Difference	13.96***	-8.27***	22.23***
	(1.06)	(1.24)	(1.63)
Dependent variable: 3-year mobility			
Year 2000	0.17	0.25	-0.08**
	(0.38)	(0.43)	(0.03)
Year 1997	0.36	0.27	0.09**
	(0.48)	(0.45)	(0.04)
Difference	-0.19***	-0.02	-0.17***
	(0.03)	(0.04)	(0.05)
Wald estimate			
-0.007***			
(0.002)			

Note: Shock is defined as a difference between mean students' credentials measured by primary school 9th grade GPA (only first-grade students who applied to school in the same year) in high school j in the first post-reform year 2000 and mean students' credentials in the last pre-reform year 1999 in these same schools. Based on the shock schools are divided into these that experience the most positive change (one-third upward shocked schools) and these that experience the least positive change (one-third downward shocked schools). Only schools that are present in the data in each year between 1994/1995 and 2004/2005 are included in the analysis. Dependent variable is defined as probability of leaving school j from school year 1997/1998 to 2000/2001 pre-reform and probability of leaving school j from school year 2000/2001 to 2002/2003 post-reform. Independent (treatment) variable is defined as mean incoming students' credentials in 1997 in pre-period and in 2000 in post-period. Differences report the interaction coefficients from regression of students' credentials or mobility on year dummy, upward shock dummy and their interaction. Wald estimate reports coefficient from instrumental variables regression of the probability that teacher leaves school j on students' credentials, year dummy and upward shock dummy. Students' credentials are instrumented by interaction between year and shock. Robust standard errors and differences rounded to second decimal.

Table 3: Effects of student credentials on teacher turnover

	(1) OLS	(2) DD	(3) DD
1-year mobility	-0.000	-0.002	-0.002
	(0.000)	(0.002)	(0.002)
Mean of Y/Observations		0.115/1,736	
2-year mobility	-0.002***	-0.002	-0.004*
	(0.001)	(0.002)	(0.002)
Mean of Y/Observations		0.237/1,933	
3-year mobility	-0.001**	-0.006***	-0.007***
	(0.001)	(0.002)	(0.002)
Mean of Y/Observations		0.275/1,868	
School and year fixed effects		X	X
Individual controls	X		X

Note: Teacher level regressions. Each estimate comes from a separate regression. Column (1) presents correlations conditional on individual teacher observable characteristics. Column (2) presents difference-in-differences estimates without controlling for any observable teacher characteristics. Column (3) adds individual level controls to column (2). Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience. The dependent variables are defined according to columns (1) and (3) in Table A 2. The independent variables of interest measuring students' credentials are defined according to columns (2) and (4) in Table A 2. Students' credentials measured by primary school 9th grade GPA (only first-grade students who applied to school in the same year) in high school j. This table does not account for potential anticipation effect. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Table 4: Placebo: Effects of post-reform changes in students' credentials on pre-reform changes in teacher turnover

	(1)	(2) Mobility	(3)
	1-year	2-year	3-year
GPA	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)
Mean of Y	0.174	0.237	0.308
Observations	1,901	2,028	1,999

Note: Teacher level regressions. Each estimate comes from a separate regression. All point estimates come from difference-in-differences regressions including school and year fixed effects as well as individual controls (see column (3) in Table 3). The independent variables of interest measuring students' credentials are defined according to columns (2) and (4) in Table A 2. Students' credentials measured by primary school 9th grade GPA (only first-grade students who applied to school in the same year) in high school j. The dependent variables are lagged by one exposure-period in comparison to these described in Table A 2. That is in column (1) I compare one-year mobility in 1998/1999 to one-year mobility in 1999/2000. In column (2) I compare two-year mobility in 1996/1997 to two-year mobility in 1998/1999. In column (3) I compare three-year mobility in 1994/1995 to three-year mobility in 1997/1998. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Table 5: Equilibrium effects and compensating wage differentials

	(1) 1-year	(2) 2-year	(3) 3-year
	Panel A: Number of students		
GPA	1.010	2.298	3.561
	(2.503)	(3.325)	(3.531)
Mean of Y	749.0	758.9	760.0
	Panel B: Number of teachers		
GPA	0.198	0.155	0.237
	(0.346)	(0.486)	(0.747)
Mean of Y	59.5	61.6	63.3
	Panel C: Student-teacher ratio		
GPA	0.029	0.070	0.065
	(0.096)	(0.128)	(0.139)
Mean of Y	12.9	12.6	12.6
Observations	36	36	36
	Panel D: Log teacher earnings		
GPA	0.001	0.003	0.002
	(0.003)	(0.002)	(0.003)
Mean of Y	12.2	12.2	12.3
Observations	1,933	1,975	1,995

Note: School level difference-in-differences in panels A to C and teacher level difference-in-differences in panel D. Regressing number of students (panel A), number of teachers (panel B) and student-teacher ratio (panel C) on students' credentials and school and time fixed effects. In Panel D regressions control for school and year fixed effects as well as individual characteristics. In panels A to C the dependent variables are measured in 1999 in the pre-reform period and in 2000, 2001, 2002 in the post-reform period for one-, two-, and three-year exposure, respectively; while the independent variable is measured in 1999 in pre- and in 2000 in post-period. In panel D the dependent variables are earnings in 1998 in pre-period and earnings in 2000, 2001 and 2002 in post-period for one-, two- and three-year differences, respectively; while the independent variable is defined in year 1999 in pre-period and in 2000 in post-period. Students' credentials measured by primary school 9th grade GPA (only first-grade students who applied to school in the same year) in high school j. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the analysis. Robust standard errors.

Table 6: Effects of student credentials on teacher turnover: Preferred estimates

	(1) OLS	(2) DD	(3) DD
1-year mobility	-0.001*	-0.000	-0.001
	(0.001)	(0.002)	(0.002)
Mean of Y/Observations		0.174/1,901	
2-year mobility	-0.001*	-0.003*	-0.004**
	(0.001)	(0.002)	(0.002)
Mean of Y/Observations		0.229/1,836	
3-year mobility	-0.000	-0.007***	-0.008***
	(0.001)	(0.002)	(0.002)
Mean of Y/Observations		0.285/1,831	
4-year mobility	-0.001	-0.009***	-0.010***
	(0.001)	(0.002)	(0.002)
Mean of Y/Observations		0.333/1,810	
School and year fixed effects		X	X
Individual controls	X		X

Note: Teacher level regressions. Each estimate comes from a separate regression. Column (1) presents correlations conditional on individual teacher observable characteristics. Column (2) presents difference-in-differences estimates without controlling for any observable teacher characteristics. Column (3) adds individual level controls (see Table **??**) to column (2). The dependent variables are defined according to columns (1) and (3) in Table A 3. The independent variables of interest measuring students' credentials are defined according to columns (2) and (4) in Table A 3. This table through one-year lag in outcome variable accounts for potential anticipation effect. Students' credentials measured by primary school 9th grade GPA (only first-grade students who applied to school in the same year) in high school j. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Table 7: Effects of student credentials on teacher hires

	(1)	(2)	(3)	(4)
	1-year	2-years	3-years	4-years
GPA	-0.005	-0.007	-0.007	-0.006
	(0.003)	(0.005)	(0.004)	(0.005)
Mean of Y	0.176	0.317	0.428	0.496
Observations	1,736	1,778	1,798	1,760

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls (see Table 3). The dependent variable in pre-period ends in school year 1999/2000 in each case. That is for one year window I code hired teacher as the one that is present in school j in school year 1999/2000 but was not present in school year 1998/1999. Identical logic applies for longer (2, 3 and 4) exposure lengths, thus for 4-year hire window in the pre-period I code teachers as hired in school year 1999/2000 if they were not present in school j in school year 1995/1996. In the post-reform period I define hires for school years 2000/2001 (1-year), 2001/2002 (2-year), 2002/2003 (3-year) and 2003/2004 (4-year). They correspond to being hired in these years and not being present in school j in school year 1999/2000. The independent variables of interest measuring students' credentials are defined in year 1999 in pre-period and in 2000 in post-period. This table accounts for potential anticipation effect. Students' credentials measured by primary school 9th grade GPA (only first-grade students who applied to school in the same year) in high school j. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Table 8: Correlates of student ability

	(1) Unconditional	(2) Conditional
First generation immigrant students	1.145***	0.127
	(0.417)	(0.492)
GPA		-0.010***
		(0.002)
Parental income in 1000 SEK	-0.001**	0.003***
	(0.000)	(0.001)
GPA		-0.028***
		(0.004)
Parental education	-0.191***	-0.108**
	(0.048)	(0.053)
GPA		-0.008***
		(0.002)
Combined cognitive and non-cognitive	-0.012***	-0.005
paternal skills	(0.004)	(0.005)
GPA		-0.007**
		(0.003)
Mean of Y/Observations	0.333/1,810	

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls (see Table 3). All regressions based on specification from Table 6, row (4) and column (3). In column (1) I substitute students' GPA with other average school-level first grade characteristics: fraction of immigrants (row (1)), parental income (row (2)), parental education (row (3)) and paternal cognitive and non-cognitive military assessments (row (4)). These are correlated with GPA at 0.44, 0.82, 0.91 and 0.80, respectively. In column (2) I keep these alternative measures but also include GPA . Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Table 9: Heterogeneity by teacher characteristics

	Characteristic	Group	(1) Fraction [%]	(2) Mean mobility	(3) Estimate
(1)	University education	Yes	73	0.301	-0.011***
					(0.002)
		No	27	0.419	-0.007*
					(0.004)
(2)	Experience (years)	0-5	24	0.531	-0.011**
					(0.005)
		6-15	36	0.331	-0.009**
					(0.004)
		16+	40	0.215	-0.010***
					(0.003)
(3)	Gender	Male	47	0.353	-0.012***
					(0.003)
		Female	53	0.316	-0.008***
					(0.003)
(4)	Subject taught	Science	10	0.408	-0.013
					(0.008)
		Other	90	0.325	-0.010***
					(0.002)
(5)	Type of contract	Permanent	80	0.277	-0.009***
					(0.002)
		Temporary	20	0.563	-0.012**
					(0.005)

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls (see Table 3). Each row reports estimates from a separate regression. Column (1) reports fraction of individuals in each group while column (2) reports mean 4-year mobility in each group. Column (3) reports point estimates from regression specified in Table 6, row (4) and column (3) for each group separately. In row (1) a university graduate is defined as an individual graduating three, four or five year-long university education or individual with a research degree. Other forms of post-secondary education are not treated as university graduates. Science teacher are defined as these teaching: mathematics, physics, chemistry, biology and computer science. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Table 10: Heterogeneity by teacher destination

	(1)	(2)	(3)	(4)
	All teachers		Teachers with university degree	
	Mean mobility	Estimate	Mean mobility	Estimate
Mobility within high schools	0.094	-0.006***	0.084	-0.005***
		(0.001)		(0.002)
Mobility within schooling	0.170	-0.007***	0.166	-0.008***
		(0.002)		(0.002)
Mobility to private school	0.012	-0.000	0.008	-0.001*
		(0.001)		(0.001)
Out of schooling sector	0.164	-0.003	0.135	-0.003
		(0.002)		(0.002)
To a higher average student ability school	0.049	-0.003***	0.043	-0.002**
		(0.001)		(0.001)
Observations	1,810		1,314	

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls (see Table 3). Each row in columns (2) and (4) reports estimates from a separate regression. Columns (1) and (3) present means of dependent variables. Column (2) presents estimates for all teachers while column (4) presents estimates for teachers with university diploma. Estimates in columns (2) and (4) are based on specification from Table 6, row (4) and column (3). Dependent variables: row (1) equals unity if teacher leaves for another teaching position in high school; row (2) equals unity if teacher leaves for another teaching position within primary or secondary schooling; row (3) equals unity if teacher leaves for another teaching position in a primary or secondary private school; row (4) equals unity if teacher leaves for another occupation outside of teaching; row (5) equals unity if teacher leaves for high school with higher average student GPA than their initial allocation. Rows (2) and (4) add up to total mobility measure used in previous specifications. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Table 11: Heterogeneity by pre-reform average school GPA

Tercile of student GPA	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Mean Mobility			Estimate		
	Fraction	Total	Within	Quit	Total	Within	Quit
Bottom	39	0.227	0.110	0.116	-0.014***	-0.007**	-0.007**
					(0.004)	(0.003)	(0.003)
Middle	28	0.297	0.203	0.093	-0.011**	-0.009**	-0.002
					(0.005)	(0.004)	(0.004)
Top	33	0.217	0.095	0.123	-0.003	0.001	-0.003
					(0.004)	(0.001)	(0.004)
p-value difference					0.114	0.015	0.561

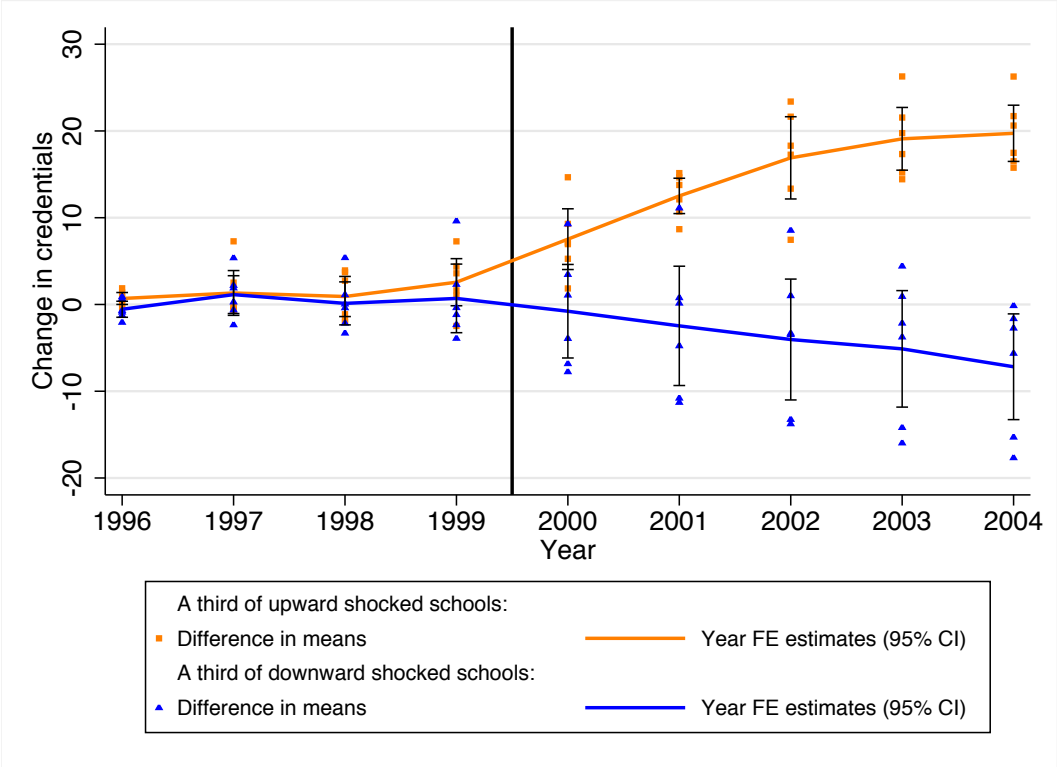
Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls (see Table 3). Each row in columns (5) to (7) report estimates from a separate regression. Column (1) reports fraction of individuals in each group while columns (2) to (4) report means of three measures of 4-year mobility (total, within teaching and out-of teaching) in each group. Column (5) to (7) report point estimates from regressions specified as in Table 6, row (4) and column (3) for each group and outcome separately. Bottom row presents the joint significance tests for the analyzed groups and outcomes. Sample sizes based on 1995 and 1999 comparison are 498, 354 and 423 for rows (1) through (3), respectively. Average student ability is divided into terciles based on the GPA in school year 1996/1997. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Table 12: Heterogeneity by ability of incoming students

Tercile	(1)	(2)	(3)
	Bottom	Middle	Top
		Panel A: Total turnover	
Fraction of students in k-th tercile	1.104***	-0.142	-0.360***
	(0.187)	(0.137)	(0.114)
Mean of Y/Observations		0.333/1,810	
		Panel B: Within teaching mobility	
Fraction of students in k-th tercile	0.693***	0.096	-0.355***
	(0.159)	(0.106)	(0.086)
Mean of Y/Observations		0.170/1,810	
		Panel C: Quits	
Fraction of students in k-th tercile	0.411***	-0.238**	-0.005
	(0.146)	(0.115)	(0.096)
Mean of Y/Observations		0.164/1,810	
Mean fraction (GPA)	0.251	0.318	0.431

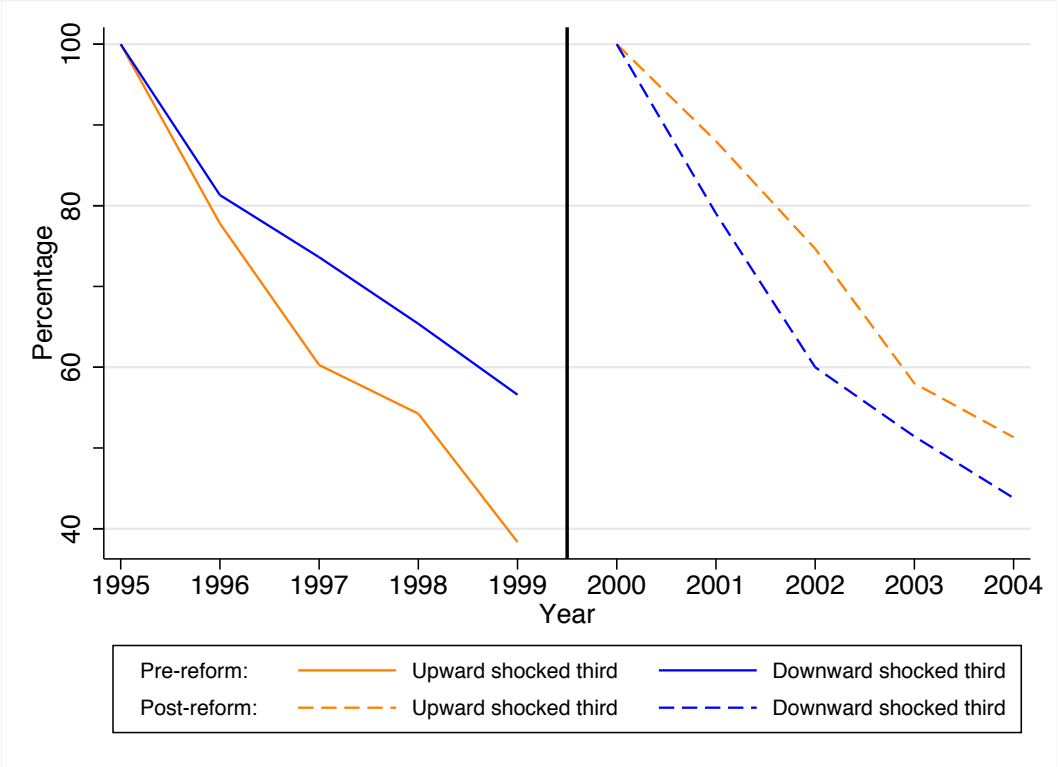
Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls (see Table 3). Each column and row reports estimate from a separate regression. Last row presents the share of students in a given tercile of the ability distribution based on all first grades that applied to schools in the year of graduation. Point estimates based on regression specified as in Table 6, row (4) and column (3) for each group and outcome separately. The outcomes include total, within teaching and out-f teaching mobility. Only schools that are observed in each year between 1994/1995 and 2004/2005 are included in the regressions. Robust standard errors.

Figure 1: Average GPA between 1995 and subsequent years



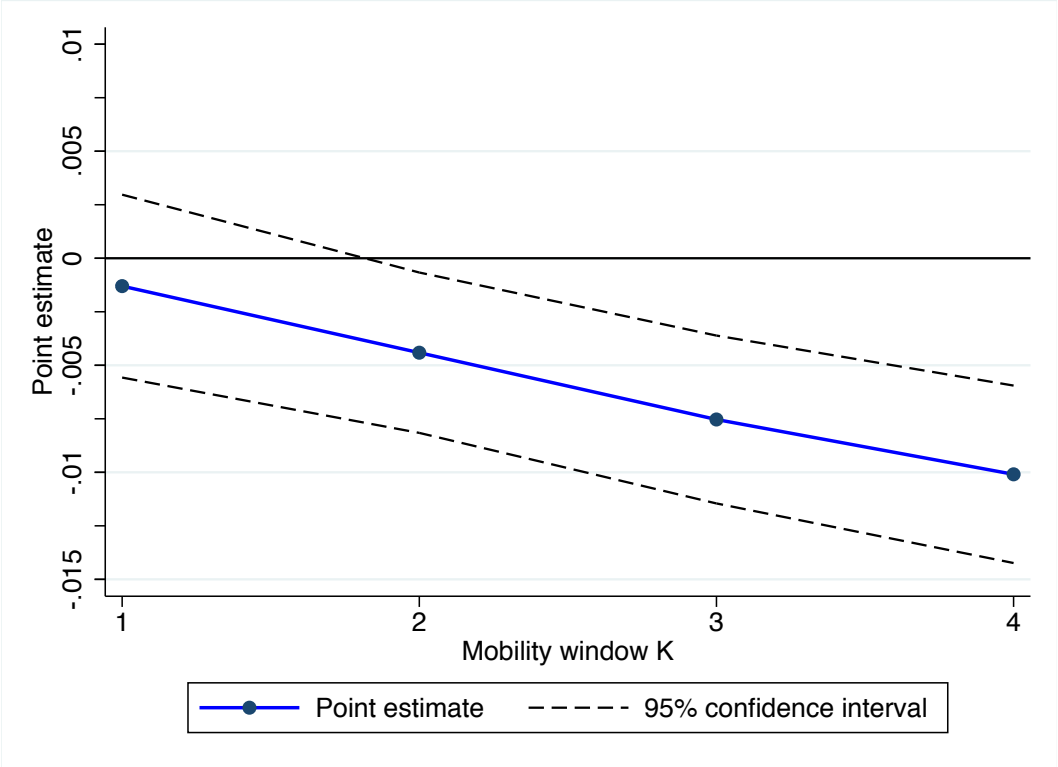
Note: Shock is defined as a difference between mean incoming students' credentials measured by primary school 9th grade GPA in high school j in the first post-reform year 2000 and mean incoming students' credentials in the last pre-reform year 1999 in these same schools. Based on the shock schools are divided into these that experience the most positive change (one-third upward shocked schools) and these that experience the least positive change (one-third downward shocked schools). Each point represents a difference between average all-grades credentials in these schools in a given year (1996 to 2004) and average all-grades credentials in these same schools in 1995. Lines plot coefficients and 95% confidence intervals from regressing these differences on year dummies (one for each year between 1996 and 2004). Robust standard errors. Black solid vertical line depicts reform implementation. Only schools that are present in the data in each year between 1994/1995 and 2004/2005 are included in the analysis.

Figure 2: Fraction of teachers leaving their appointments in 1995/1996 and 2000/2001



Note: Shock is defined as a difference between mean incoming students' credentials measured by primary school 9th grade GPA in high school j in the first post-reform year 2000 and mean incoming students' credentials in the last pre-reform year 1999 in these same schools. Based on the shock schools are divided into these that experience the most positive change (one-third upward shocked schools) and these that experience the least positive change (one-third downward shocked schools). Each point represents percentage of teachers who were teaching in school j in year 1995 (2000) and remain in this same school in year t. Black solid vertical line depicts reform implementation. Only schools that are present in the data in each year between 1994/1995 and 2004/2005 are included in the analysis.

Figure 3: Difference-in-Differences estimates by years of exposure



Note: Estimates based on column (3), row (4) of Table 6 with 95% confidence intervals.

Supplemental Materials (not for publication)

Sample Construction

I construct the sample of high school teachers for the school years 1991/1992 to 2004/2005. The information about teachers comes from the teacher registry and the analysis focuses on teachers working in grades 1 to 3 of secondary education (high school) that were in operation in Stockholm municipality prior to school year 1999/2000. Teachers who are on unpaid leave of absence or whose workloads are zero hours (i.e., they do not perform any pedagogical duties) are excluded from the analysis. Such teachers are treated neutrally in terms of mobility if they come back after the absence period to the same school. Similarly, I exclude teachers who are employed as principals, study counselors etc. In each year if a teacher has multiple entries in the registry, the observation with the highest workload is selected irrespective of whether it is at the same or at different schools.[36] The teacher registry is a high quality data set, that allows recovering information on school location (unique identifier), school ownership and type, teacher certification, workload, employment type (temporary vs. permanent), education and position.

Teacher experience is not available for all years, and therefore, I use predicted experience in the analysis. In particular, since the teacher registries date back to 1979 I explore this feature to construct the "in teaching predicted experience" variable. I create a panel of all teachers between 1979 and 2006 and link it to population enlistment data between 1985 and 2006 in order to obtain teacher's birth date. I then use all this information and tenure data provided in the later registries (since 1999 onward) to construct the predicted measure of experience.

Teachers are then linked (using unique identifier) to population register, which covers all individuals living in Sweden. The register includes information on gender, marital status, age, family composition (using unique family identifier), immigration history and education. The analysis is restricted to teachers aged 25-58 years, to abstract from mobility driven by educational attainment and retirement decisions.

The students' characteristics are based on "school in" and "school out" pupil registries. The secondary school composition is based on all the students that are in a school in a given year. The ability of students in secondary school is measured based on their 9th grade GPA. I percentile rank students for each subject and take the average across all subjects. The average GPA is then percentile ranked again. I match students to their parents using unique family identifier and obtain the family level socioeconomic indicators i.e. mean parental income, mean parental education and the cognitive and non-cognitive skill of the fathers from the military enlistment. Income is measured as a gross salary plus income from business and self-employment plus any work-related allowances. Investment losses are not included, and thus, income is lower-bounded at zero.

The enlistment registry covers period 1969 to 2006 and provides information on cognitive and non-cognitive assessments. All skill measures are percentile ranked by year of draft. The data is linked to students' fathers using the unique personal identifier.

[36] The workload of teachers having multiple positions at the same school is not summed and the highest workload position is selected.

Finally, having a data set with teachers and students I match the two using the unique school identifier. I exclude schools with less than three employed teachers (in full time equivalence) and schools with less than 15 students. I also restrict the analysis to teacher aged 25-58 years. I then select schools that operate within the municipality of Stockholm and were in operation prior to school year 1999/2000. In this paper I focus on a balanced panel of schools, i.e. I restrict the sample to schools present in the data for all years between 1994/1995 and 2004/2005. I also drop teachers from Skärholmens Gymnasium because this school did not admit any new students in school year 1998/1999.

Tables

Table A1: Comparison of Sweden and Stockholm

Variable	(1)	(2)	(3)	(4)
	Pre-period = 1999/2000		Post-period = 2000/2001	
	Sweden	Stockholm	Sweden	Stockholm
One-year mobility	0.13	0.14	0.11	0.15
	(0.34)	(0.35)	(0.31)	(0.36)
Female teacher	0.49	0.55	0.48	0.53
	(0.50)	(0.50)	(0.50)	(0.50)
Teacher experience (years)	12.0	11.05	11.94	10.96
	(7.27)	(7.24)	(7.61)	(7.48)
Teacher with university diploma	0.65	0.72	0.65	0.72
	(0.48)	(0.45)	(0.48)	(0.45)
Teacher employed on temporary contract	0.20	0.21	0.20	0.22
	(0.40)	(0.41)	(0.40)	(0.41)
Yearly teacher earnings in 1000 SEK	224	217	226	226
	(78)	(84)	(79)	(87)
Students' GPA	48.95	56.73	48.99	57.35
	(11.12)	(16.48)	(11.59)	(18.34)
First generation immigrant	0.08	0.13	0.08	0.13
	(0.06)	(0.07)	(0.06)	(0.07)
Yearly parental income in 1000 SEK	338	396	356	423
	(62)	(117)	(65)	(128)
Parental education	11.99	13.05	12.16	13.2
	(0.81)	(1.15)	(0.79)	(1.17)
Paternal draft score	51.60	58.1	51.88	56.92
	(5.86)	(7.86)	(5.78)	(8.91)
Number of teachers	20,793	1,306	21,675	1,365

Note: Means and standard deviations. Columns (1) and (3) present statistics for all high school teachers in Sweden (excluding Stockholm municipality) in years 1999/2000 and 2000/2001 from schools that were in operation prior to school year 1999/2000. Columns (2) and (4) present statistics for all high school teachers in Stockholm municipality in years 1999/2000 and 2000/2001 from schools that were in operation prior to school year 1999/2000.

Table A2: Definitions of treatment and outcomes without anticipation effects

Mobility	(1) Post-period mobility	(2) Post-period GPA	(3) Pre-period mobility	(4) Pre-period GPA
1-year	00/01 to 01/02	2000	99/00 to 00/01	1999
2-year	00/01 to 02/03	2000	98/99 to 00/01	1998
3-year	00/01 to 03/04	2000	97/98 to 00/01	1997

Note: Table presents length of mobility in rows. First row defines mobility as teachers leaving in period t+1, second row in t+2 and third row in t+3. Columns (1) and (2) define the post-reform period dependent and treatment variables. Columns (3) and (4) define the pre-reform period dependent and treatment variables.

Table A3: Definitions of treatment and outcomes with anticipation effects

Mobility	(1) Post-period mobility	(2) Post-period GPA	(3) Pre-period mobility	(4) Pre-period GPA
1-year	99/00 to 00/01	2000	98/99 to 99/00	1999
2-year	99/00 to 01/02	2000	97/98 to 99/00	1998
3-year	99/00 to 02/03	2000	96/97 to 99/00	1997
4-year	99/00 to 03/04	2000	95/96 to 99/00	1996

Note: Table presents length of mobility in rows. First row defines mobility as teachers leaving in period t+1, second row in t+2, third row in t+3 and fourth row in t+4. Columns (1) and define the post-reform period dependent and treatment variables. Columns (3) and (4) define the pre-reform period dependent and treatment variables.

Table A4: Standard errors assumptions: Estimates with anticipation effects

	(1) DD	(2) DD
1-year	-0.000	-0.001
Robust standard errors	(0.002)	(0.002)
Standard errors clustered at the school level	(0.005)	(0.005)
Standard errors clustered at the school×year level	(0.003)	(0.003)
Robust standard errors from first difference with aggregated data	(0.005)	(0.005)
# Teachers	1,901	
# Schools	18	
2-years	-0.003	-0.004
Robust standard errors	(0.002)	(0.002)
Standard errors clustered at the school level	(0.004)	(0.004)
Standard errors clustered at the school×year level	(0.003)	(0.003)
Robust standard errors from first difference with aggregated data	(0.004)	(0.004)
# Teachers	1,836	
# Schools	18	
3-years	-0.007	-0.008
Robust standard errors	(0.002)	(0.002)
Standard errors clustered at the school level	(0.003)	(0.003)
Standard errors clustered at the school×year level	(0.002)	(0.002)
Robust standard errors from first difference with aggregated data	(0.003)	(0.004)
# Teachers	1,831	
# Schools	18	
4-years	-0.009	-0.010
Robust standard errors	(0.002)	(0.002)
Standard errors clustered at the school level	(0.004)	(0.004)
Standard errors clustered at the school×year level	(0.003)	(0.003)
Robust standard errors from first difference with aggregated data	(0.004)	(0.004)
# Teachers	1,810	
# Schools	18	
Individual controls		X

Note: This table replicates columns (2) and (3) of Table 6 but with alternative standard errors. More specifically it reports (i) robust standard errors as a reference (ii) standard errors clustered at the school level; (iii) standard errors clustered at the school×year level, thus allowing interdependence between teachers in a school in a specific year but not across years; (iv) standard errors from regressing the first-differences on the treatment variable using aggregated data. All models, except the first difference analysis, include school and year fixed. Data is the balanced 1994/1995-2004/2005 panel.

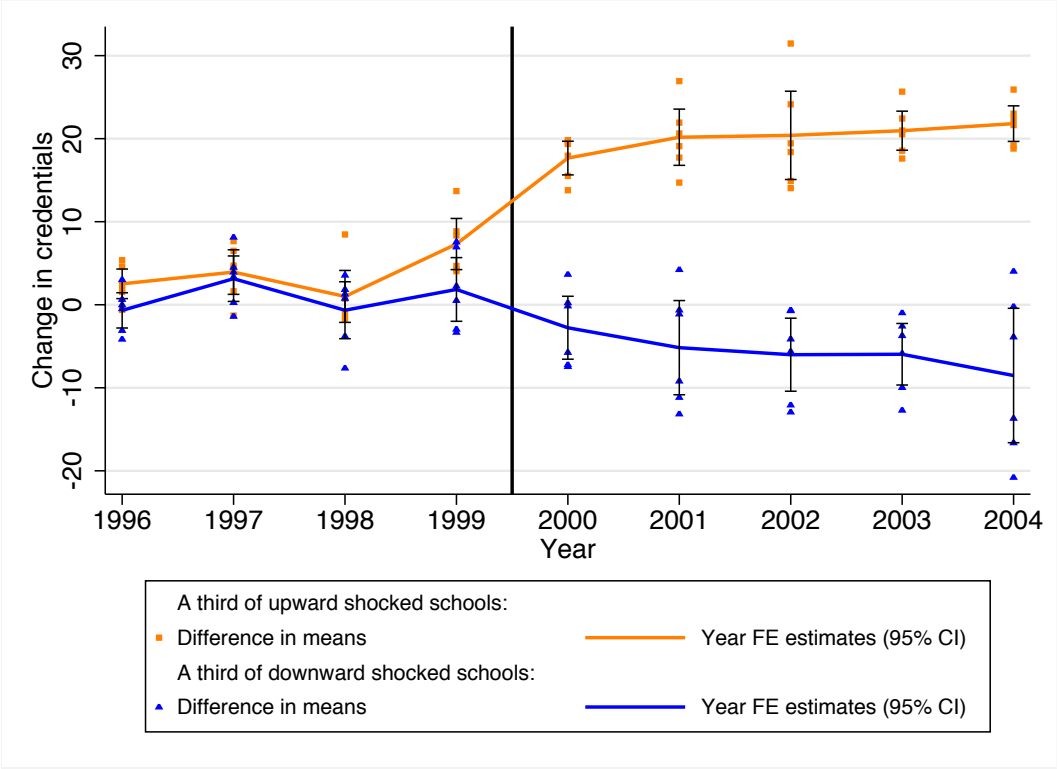
Table A5: Standard errors assumptions: Estimates with anticipation effects. Pooled sample of schools

	(1) DD	(2) DD
1-year	-0.000	-0.001
Robust standard errors	(0.002)	(0.002)
Standard errors clustered at the school level	(0.005)	(0.005)
Standard errors clustered at the school×year level	(0.003)	(0.003)
Robust standard errors from first difference with aggregated data	(0.005)	(0.005)
# Teachers	2,183	
# Schools	21	
2-years	-0.004	-0.005
Robust standard errors	(0.002)	(0.002)
Standard errors clustered at the school level	(0.004)	(0.004)
Standard errors clustered at the school×year level	(0.003)	(0.003)
Robust standard errors from first difference with aggregated data	(0.004)	(0.005)
# Teachers	2,098	
# Schools	20	
3-years	-0.006	-0.007
Robust standard errors	(0.002)	(0.002)
Standard errors clustered at the school level	(0.004)	(0.004)
Standard errors clustered at the school×year level	(0.003)	(0.003)
Robust standard errors from first difference with aggregated data	(0.004)	(0.004)
# Teachers	2,093	
# Schools	20	
4-years	-0.008	-0.009
Robust standard errors	(0.002)	(0.002)
Standard errors clustered at the school level	(0.004)	(0.004)
Standard errors clustered at the school×year level	(0.003)	(0.003)
Robust standard errors from first difference with aggregated data	(0.004)	(0.004)
# Teachers	2,035	
# Schools	19	
Individual controls		X

Note: This table replicates columns (2) and (3) in Table 6 but with alternative standard errors. More specifically it reports (i) robust standard errors as a reference (ii) standard errors clustered at the school level; (iii) standard errors clustered at the school×year level, thus allowing interdependence between teachers in a school in a specific year but not across years; (iv) standard errors from regressing the first-differences on the treatment variable using aggregated data. All models, except the first difference analysis, include school and year fixed. Data is an unbalanced panel of schools between 1995/1996 and 2004/2005.

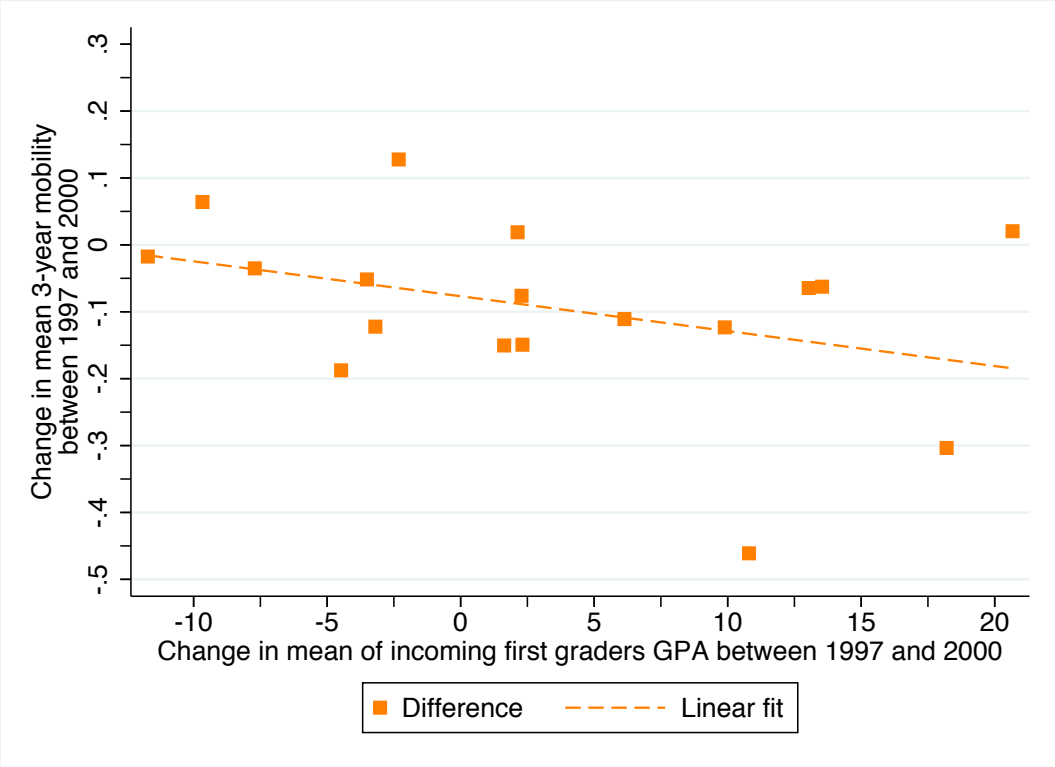
Figures

Figure A1: Average GPA between 1995 and subsequent years. First grade students who applied to high school in the same year



Note: Shock is defined as a difference between mean students' credentials measured by primary school 9th grade GPA (only first-grade students who applied to school in the same year) in high school j in the first post-reform year 2000 and alike defined mean students' credentials in the last pre-reform year 1999 in these same schools. Based on the shock schools are divided into these that experience the most positive change (one-third upward shocked schools) and these that experience the least positive change (one-third downward shocked schools). Each point represents a difference between incoming students' credentials in these schools in a given year (1996 to 2004) and incoming students' credentials in these same schools in 1995. Lines plot coefficients and 95% confidence intervals from regressing these differences on year dummies (one for each year between 1996 and 2004). Robust standard errors. Black solid vertical line depicts reform implementation. Only schools that are present in the data in each year between 1994/1995 and 2004/2005 are included in the analysis.

Figure A2: Difference-in-Differences: Probability of leaving school within 3-years



Note: Values on the vertical axis represent differences in mean 3-year mobility between 1997 (pre-reform) and 2000 (post-reform). Values on the horizontal axis represent changes in mean students' credentials between 2000 and 1997. Student credentials are based on first grade students who applied to high schools in the same year and are measured using primary school 9th grade GPA. This figure does not account for potential anticipation effects. Line represents linear regression fit. Only schools that are present in the data in each year between 1994/1995 and 2004/2005 are included in the analysis.