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Do changes in student quality affect teacher mobility?
Evidence from an admission reform

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Do changes in student quality affect teacher mobility? Evidence from an admission reform.[1]

by

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Abstract

This paper examines teachers' mobility in response to exogenous changes in the credentials of their students using data from Stockholm high schools. I explore a major admission reform that lead to the reshuffling of students between schools within the municipality of Stockholm. The results show that a 10-percentile-point increase in student quality decreases the probability of a separation by up to 9 percentage points. These effects are very similar across all types of teachers and are found mainly for mobility between schools rather than out of the profession. They are also present only in the lower half of the student quality distribution. Teachers react mostly to direct measures of student quality (grades from compulsory school) rather than to other characteristics that are correlated with student quality (immigrant status, parental income, paternal cognitive skills). Finally, I do not find any significant effects of changes in student quality on individual teacher's earnings or school hiring policies.

Keywords: Teacher mobility, student quality, school choice
JEL-codes: I21, J44

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1 Introduction

Many educational interventions such as student busing, school choice or changes in admission policies impact the composition of students in schools. The interventions have been motivated by the idea that it could be beneficial for certain groups of students to meet better peers while keeping constant other inputs of the education production function.[3] This hypothesis relies heavily on the assumption that inputs of the educational production function are exogenous to student characteristics. It is quite possible, however, that low-performing students impose a heavier burden on teaching. Thus, if changes in student composition affect other factors of input such as teacher quality or school resources (Hanushek, 1986), then polices aimed at changing the peer group composition in schools may have unintended consequences.

In this paper, I study how exogenous changes in student composition affect teacher mobility. In particular, I investigate whether teachers who experience an inflow of high quality students are less likely to quit their jobs in comparison to teachers who face an inflow of lower quality students. Multiple correlational studies suggest that teacher mobility is negatively related to pupil quality.[4] At the same time, we know relatively little about whether this descriptive relationship can be given a causal interpretation, with the exception of a busing policy study by Jackson (2009). However, due to the nature of the policy, he focuses primarily on racial sorting, which only has a secondary relationship with student quality.[5]

Uncovering the causal relationship between student quality and teacher mobility should be a central priority for policy makers for two reasons. First, if worse quality students induce teachers, particularly of high quality, to leave their schools, then the problem with an inflow of less able students may be reinforced by higher teacher turnover and by unfavorable sorting of teachers. Second, the potential positive effects of policies aimed at reshuffling students between schools may be dwarfed by teacher mobility if high quality teachers leave in response to an inflow of low quality pupils.

[3] Examples of policies that lead to reshuffling of peers 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); and court-ordered desegregation (Reber, 2005).

[4] 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.

[5] A third quasi-experimental study in the relevant literature is Feng et al. (2010), who study the effects of changes in school resources on teacher mobility.

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 applied only for a program and their grades from lower secondary school determined admission. Students could state their preferences for the school they would like to attend, but those living closest to a school had priority. Thus, although the program choice included an element of school choice, it essentially limited the choice of students living in less affluent neighborhoods as they never had a chance to be admitted to permanently oversubscribed programs in prestigious downtown schools.[6] The 2000 reform abolished all residence-based admission criteria and introduced a system that is based solely on lower secondary school performance. The reform was intended to undo the effects of residential segregation and to give the option of attending the most prestigious schools in downtown Stockholm to all students, irrespectively of where they lived.

I make use of rich registry data and a difference-in-differences strategy to identify the effect of student quality on teachers' decisions to leave their current employment. Since the composition of students changed exogenously and teachers faced students of utterly different quality before versus after the reform, the estimate can be treated as teacher preference for student quality, under certain theoretical assumptions on the teacher's utility function. In Section 7, I also consider a broader school-level perspective of the reform. In particular, I investigate whether the reform affected schools' hiring policies and if it changed an individual teacher's monetary compensation.

I find that a 10–percentile-point decline in average incoming student credentials increases 4-year separation rates by up to 9 percentage points (pp). The effect is driven primarily by teachers switching schools rather than teachers leaving the profession, and it is concentrated at the bottom half of the student quality distribution. The estimated effect is statistically and economically significant and similar across groups of teachers whose baseline mobility is very different. Furthermore, teachers seem to react to the direct measures of student quality. Once student credentials are taken into account, other characteristics like immigration background become unrelated to teacher mobility.

[6] 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 low quality neighborhood and commute within the city than it is to get housing in an affluent 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.

Finally, I do not find any significant effects of changes in student quality on an individual teacher's earnings or school hiring policies.

The reminder of the paper is organized as follows. Section 2 gives details regarding educational institutions in Sweden: the reform, data used and identifying variation. Section 3 presents a simple theoretical framework for teacher mobility and sets up the empirical analysis while Section 4 contains main results. Section 5 presents sensitivity analyses, while Section 6 includes heterogeneity analyses and Section 7 extends the analysis to school-level responses to the change in student quality. Finally, Section 8 concludes.

2 Institutions, reform, data and identification

2.1 Educational institutions in Sweden

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

Private schooling is growing in Sweden and is encouraged by the government.[7] In 1992, Sweden introduced a school voucher reform that allowed for both non-profit and for-profit independent schools. The municipality is obliged to 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.[8]

The teaching profession in Sweden is regulated and different qualifications are required depending on the subject taught and on the type of school. Teaching at the secondary school level requires completing special coursework beyond what is required for a compulsory school teacher. Individuals from other professions who want to

[7] 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.

[8] 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).

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 could also be hired on short-term contracts.

Municipalities are the primary employers of teachers in Sweden, and thus, handle the responsibility of recruiting them.[9] In practice, however, the decisions regarding recruitment, selection and employment of a teacher are made at the school level by a principal. 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.[10]

2.2 The admission reform

In the fall of 1999 the municipality of Stockholm passed regulation that changed the high school admission rules. Up to the 1999/2000 school year, students applied only for a program and their grades from lower secondary school determined admission. Students could state their preferences for which school they would like to attend, but those living closest to a school had priority. In practice, the educational administration first counted the number of places per program in any given municipality and then ranked the student choices according to grades, and accepted students to a certain program. Subsequently, they assigned the students to the specific schools based on their residence, and thus, assuming competitive grades, it was possible to get accepted into a better program in a school further away, but only if it was not oversubscribed with students residing in the neighborhood. 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 Tensta (a relatively poor and disadvantaged district in Stockholm) would be unable to gain admission to the program.[11] In particular, the restriction was binding for the two most popular and broadest programs: social sciences (samhällskunskap) and natural sciences (naturvetenskap). Generally, those from low-income, disadvantaged districts had

[9] For more information on the reform that shifted responsibility for schooling from the central government to municipalities see Fredriksson and Öckert (2008).
[10] 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).
[11] 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.

virtually no chance of attending the most popular inner-city schools, even if they had competitive grades.

The cohort applying to high school in May 2000 for the 2000/2001 school year faced utterly 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 grades from the 9th grade in lower-secondary school. In this paper, the grades of incoming high school students is the variable of interest.[12] In the new system, students apply for a specific program in a specific school 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. Importantly, this reform was introduced only in the municipality of Stockholm, and thus, the rest of Stockholm County was not affected.

It is important to note that 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 incentivizes more students from out-of-Stockholm to apply to schools in Stockholm, then they may crowd out students residing in Stockholm. Furthermore, Stockholm schools may decide to change the number of admitted students in response to higher demand for quality, which would in turn lead to either higher student-teacher ratio, and thus, impoverishment of school resources, or to the need for additional hires. I address the latter issue in Section 4. Finally, my calculations show that the fraction of students living outside of Stockholm municipality but attending Stockholm schools is stable around 20% over the analyzed period.

Söderström and Uusitalo (2010) found clear evidence that the Stockholm admission reform affected both student mobility and the sorting of students by quality. In particular, the grade-based admission system increased the sorting of students to schools according to their ability, as well as ethnic and socio-economic background. However, the segregation between immigrants and natives increased more than one would expect as a result of increased sorting by ability. Edin et al. (2011) used the same strategy to evaluate the effects on student outcomes. They find either zero or negative effects on

[12] From here on, I refer to grades as students' credentials, student quality or student GPA.

student performance. The authors conclude that their results do not support the idea that choice and competition improve performance. One possible mechanism behind this finding could be that schools that face inflowing students of poorer quality may also lose their best teachers.[13] Thus, this study evaluates how the resorting of students between schools in Stockholm affected teacher turnover rates.

2.3 Data and descriptive statistics

This paper utilizes Swedish population-wide registries. The main data source is the teacher registry that covers all teaches employed in Swedish schools during the 1991/1992 through 2004/2005 school years. It contains information on teachers' education, specialization, experience, certification, place of work, type of contract (permanent vs. temporary) and workload. I have matched background information on age, gender, immigration histories, education, employment and income to these data. The pupil registers for lower and upper secondary schools are used to obtain information on students in a given upper secondary school and their credentials from lower secondary school. All students have also been matched to their parents to obtain measures of family background. Administrative records on earnings provide information on teachers' monetary compensations. The details of the sample construction are discussed in the appendix.

Given the timing and the geographical implementation of the reform, I focus on secondary schools that have been in operation in Stockholm for all school years from 1991/1992 to 2004/2005. This avoids potential composition effects related to school openings and closures. However, all the results carry over if I use a repeated cross-section of schools. Due to the reform implementation date there are no independent high schools in the 1991/1992 to 2004/2005 panel sample. I can observe, however, if a teacher leaves their current school in favor of a privately run institution. In the pooled sample of all secondary schools in Stockholm prior to the 1999/2000 school year there are 8 private schools out of 29 total schools.

Since the reform was only implemented in the municipality of Stockholm, it is important for potential generalizations of the results to gauge how comparable the Stockholm population is to the overall population of teachers and schools in Sweden.

[13] This assumes that there is a positive interaction effect between student quality and teacher quality in the production of student skills.

Table A 1 compares basic descriptive statistics for Stockholm and non-Stockholm schools for the last pre-reform (1999/2000) and first post-reform school year (2000/2001). It is clear that Stockholm is more affluent in many dimensions than the rest of Sweden. Schools in Stockholm admit students with higher credentials, who come from richer and better educated families, and whose fathers obtain higher cognitive and non-cognitive scores during military assessment. At the same time, these schools admit more minority students, which is not surprising given that Stockholm has a major concentration of immigrants to Sweden. Stockholm schools also have the advantage of employing more teachers with university diplomas; however, the teachers are on average less experienced.

The reform was implemented in the 2000/2001 school year, and thus, as a starting point, I present descriptive evidence for the 1999/2000 school year as the last pre-reform year and the 2000/2001 school year as the first post-reform year.[14] This paper focuses on the exogenous reshuffling of students within the municipality of Stockholm and responses of teachers when they face a different set of pupils. Therefore, in Table 1, I present descriptive statistics from the 1991/1992 to 2004/2005 panel of Stockholm schools for the immediate pre- and post-reform periods, separated by changes in their student composition. In particular, for each school j in the panel, I calculate the difference between mean-incoming-student credentials in the first post-reform year, 2000/2001, and the last pre-reform year, 1999/2000. Then, I order these differences from the schools most negatively affected to those most positively affected and divide the ranking into tertiles. 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.

[14] Later in the paper I discuss, test and account for possible anticipation effects.

Table 1. Descriptive statistics – panel of Stockholm schools. Comparison across treatments

	Pre-reform = 1999			Post reform = 2000		
Variables	**Change in student credentials**					
	(1)	(2)	(3)	(4)	(5)	(6)
	1/3 downward	1/3 middle	1/3 upward	1/3 downward	1/3 middle	1/3 upward
Outcome variable						
One-year mobility	0.10	0.16	0.11	0.10	0.13	0.08
	(0.30)	(0.37)	(0.32)	(0.30)	(0.33)	(0.27)
Treatment variable						
Incoming students' credentials	50.45	54.84	62.31	45.70	57.36	71.92
	(11.82)	(20.06)	(10.36)	(10.40)	(20.09)	(14.19)
Teacher characteristics						
Fraction of female teachers	0.56	0.53	0.45	0.53	0.56	0.46
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Mean teacher experience	13.00	11.13	11.79	13.01	11.21	11.46
	(6.92)	(7.04)	(7.59)	(7.49)	(7.03)	(7.73)
Fraction of teachers with university diploma	0.77	0.66	0.74	0.75	0.68	0.77
	(0.42)	(0.47)	(0.44)	(0.43)	(0.47)	(0.42)
Fraction of teachers employed on temporary contracts	0.18	0.26	0.24	0.22	0.27	0.23
	(0.38)	(0.44)	(0.43)	(0.42)	(0.45)	(0.42)
Mean yearly teacher earnings in 1000 SEK	245	218	216	248	231	223
	(84)	(76)	(80)	(88)	(77)	(88)
Student characteristics (alternative treatment variables)						
Share of immigrants	0.18	0.12	0.10	0.23	0.11	0.09
	(0.08)	(0.05)	(0.05)	(0.09)	(0.03)	(0.04)
Mean yearly parental income in 1000 SEK	346	404	422	330	438	486
	(44)	(100)	(58)	(48)	(81)	(106)
Mean parental education	12.34	13.12	13.66	12.37	13.22	13.77
	(0.55)	(1.33)	(0.90)	(0.43)	(1.28)	(0.83)
Mean paternal draft score*	54.81	57.39	58.08	53.83	55.96	58.34
	(6.12)	(7.13)	(4.20)	(8.10)	(6.93)	(3.84)
Number of schools	5	5	5	5	5	5
Number of teachers	266	238	274	260	240	312

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 sample of Stockholm schools in operation between 1991 and 2004 and refer to incoming first year students as far as aggregate school characteristics are concerned. For each characteristic I report descriptive statistics for teachers and schools affected differently by the reform. In particular, columns (1) and (4) describe a third of most downward shocked schools. Columns (3) and (6) describe a third of most upward shocked schools. Columns (2) and (5) describe a third of middle tertile 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.

As is evident from Table 1, the reform indeed reshuffled incoming first-grade pupils between schools in Stockholm. In particular, student GPA in the top schools increased from 62.3 to 71.9 percentile points while it decreased in the bottom schools from 50.5 to 45.7 percentile points, widening the gap between best and worst schools from less than 12 to over 26 percentile points. This is equivalent to over two-thirds of a standard deviation change in student quality.

At the same time, other student characteristics correlated with student quality, such as parental income or share of minorities, also changed. For example, the gap between the best and worst schools in terms of mean parental income doubled, while the share of minority students increased by 75 percent. As a result of teacher turnover and school hiring decisions, the reform also affected the composition of the teacher stock. For example, there were on average 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 in comparison to the pre-reform period. The gap in teacher compensation did not seem to widen, and it actually decreased from 29000 to 25000 Swedish Kronor. Interestingly, teachers in schools with better students earned less than those in schools with low-quality students, suggesting the presence of compensating wage differentials in a system with fairly flexible teacher pay scheme.

In summary, the descriptive evidence in Table 1 suggests that downward shocked schools attracted lower quality students even prior to the reform, but this gap increased after the reshuffling. Quite the opposite, however, is the relationship between school shock and teacher separation rates. Prior to the reform, the upward shocked schools experienced more one-year separations, but the fraction of separations is higher in downward shocked schools in the school year 2000/2001. Since these two facts are crucial for the identification in this paper I explore them further in Section 2.4.

2.4 Identifying variation

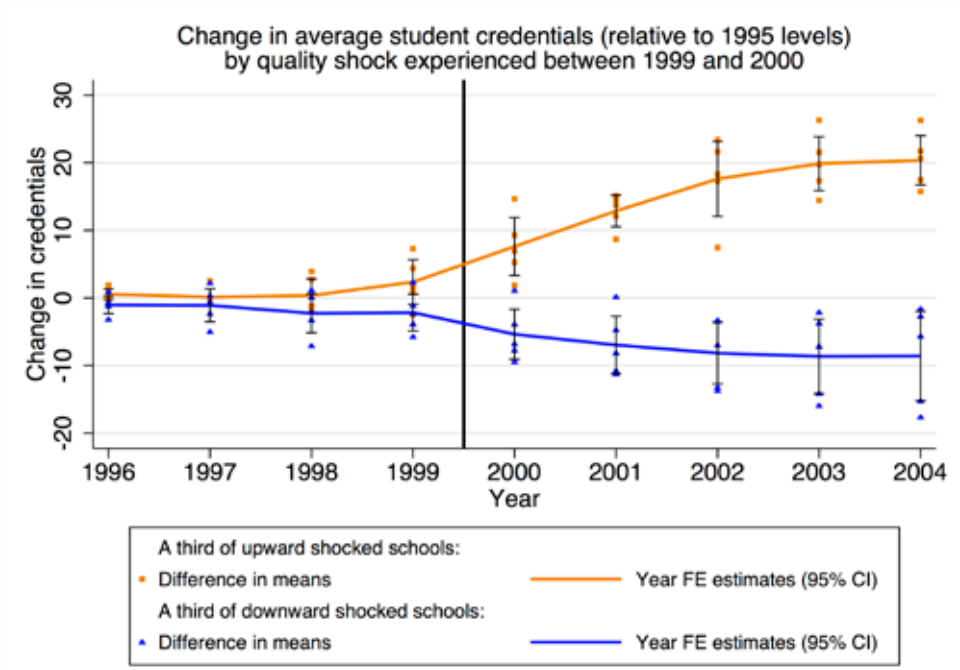
The implementation of the reform lead to abrupt changes in the sorting of students over schools in Stockholm. From one year to another, the same set of teachers experienced radical changes in the quality of the incoming students. In particular, some teachers ended up with lower quality pupils and some other teachers ended up with higher quality pupils than in the pre-reform period. The aim of this paper is to study how teacher mobility changed in response to this unexpected change in student quality. In this section I probe deeper into the changes in students' credentials and the changes in teacher mobility.

Figure 1 shows the differences in average student credentials for every year (1996 to 2004) and for upward and downward shocked schools relative to average student credentials in the same schools during the 1995/1996 school year. In Figure 1, these differences are plotted as points, while the vertical lines at each year show 95-percent

confidence intervals from a linear regressions with the difference in the average first-year student credentials compared to 1996 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 quality. Prior to the reform there are no significant differences in average students' 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 the most pronounced. Naturally, the two measures are highly correlated, and Figure 2 confirms that the largest shock in incoming students quality occurred between the 1999/2000 and 2000/2001 school years, while the subsequently admitted cohorts mimicked the quality of the first graders from the 2000/2001 school year.[15]

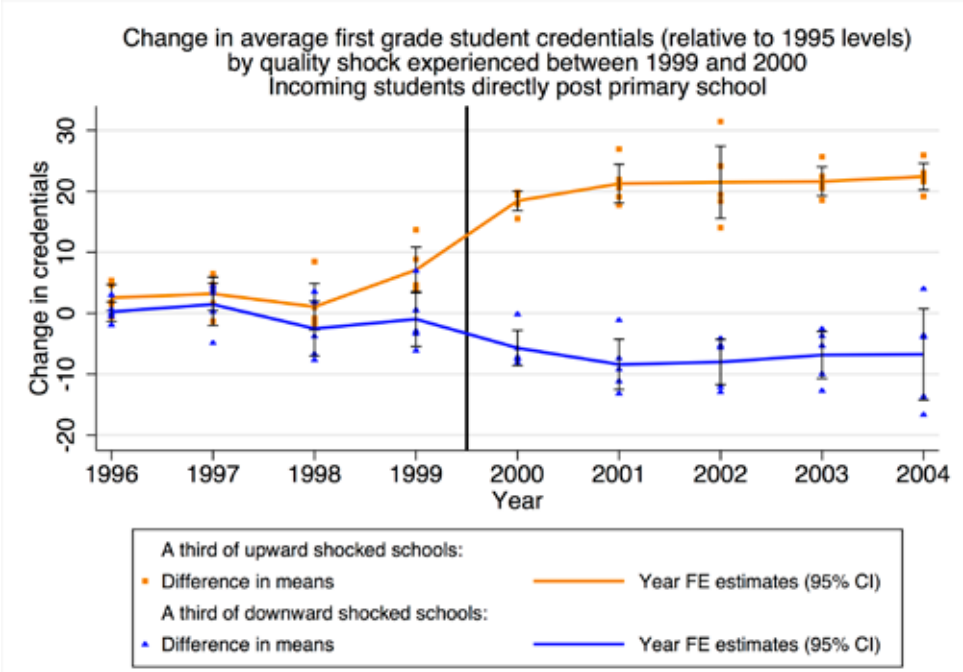
[15] Throughout the paper I use the incoming students' credentials (Figure 2) as the main treatment variable, however, one might also think about using the average student quality from all grades (Figure 1). In fact, if we compare average student characteristics between school years 1999/2000 and 2000/2001 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 is truly exogenous then this should not make much of a difference 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. The results are qualitatively similar irrespectively of the measure used and in fact they are larger quantitatively if I use all-grades GPA as student quality measure.

Figure 1. Variation in treatment: Correlation in mean school quality 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. Each dot is related to a single difference for a single school. 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 1991 and 2004 are included in the analysis.

Figure 2. Variation in treatment: Correlation in mean school quality between 1995 and subsequent years. First grade students who applied to high school in the same year

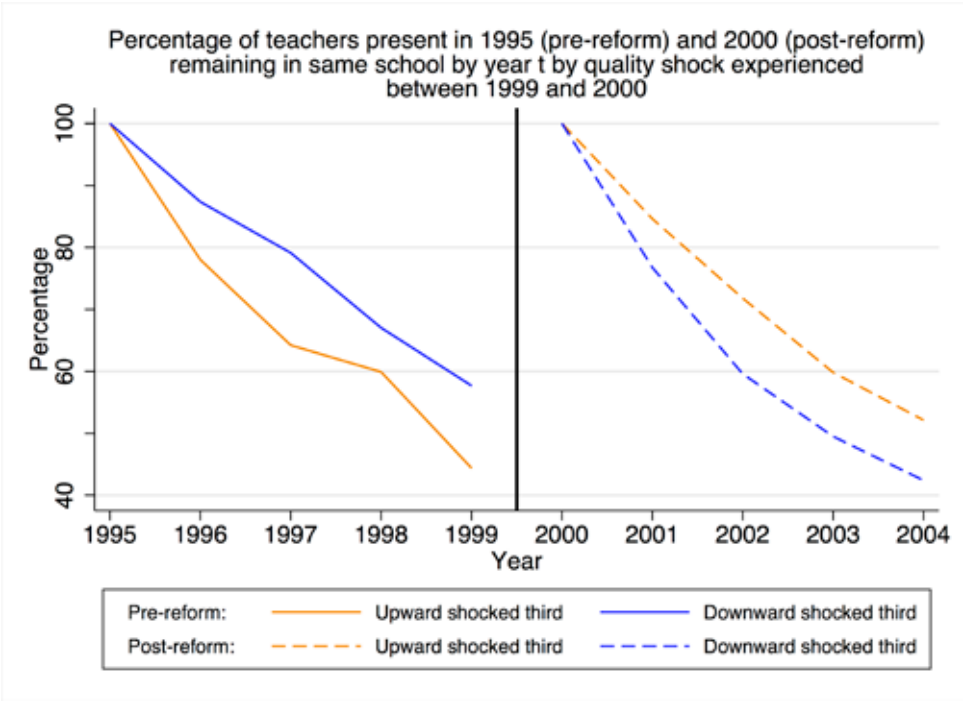


Note: Shock is defined as a difference between mean students' credentials measured by primary school 9th grade GPA (only students who applied to school in the same year) in first grade of high school j in the first post-reform year 2000 and alike defined mean students' credentials in the last pre-reform 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. Each dot is related to a single difference for a single school. 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 1991 and 2004 are included in the analysis.

Figure 1 and Figure 2 documented that the reform abruptly reshuffled students across schools in the municipality of Stockholm. In Figure 3, I provide some first evidence on how this reshuffling affected the probability that a teacher left their current employment. In particular, I start with the pool of teachers in 1995 (pre-reform) and in 2000 (post-reform), and I plot the fraction of teachers that remained employed from one up to four years. I plot these percentages separately for upward and downward shocked schools defined in the same manner as in Figure 1 and Figure 2. Although the figure is uninformative about the pre-reform trends in teacher mobility and thus potentially biased, it shows the mobility differences in levels before and after the reform for the two types of schools. For example, it depicts that upward shocked schools had higher levels of turnover before the reform, and that these same schools switched to having lower turnover rates in comparison to downward shocked schools post-reform. This is of importance as one might be worried that finding a negative effect of increased student

quality on teacher turnover is driven by the fact that upward shocked teachers had lower mobility rates even prior to the reform. Figure 3 clearly shows that this is not the case and, if anything, the opposite is true. Thus, the empirical strategy should provide a lower bound estimate for the rate of mobility.

Figure 3. Variation in dependent variable: Teachers leaving their 1995 or 2000 employment



Note: Shock is defined as a difference between mean students' credentials measured by primary school 9th grade GPA (only students who applied to school in the same year) in first grade of 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 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 1991 and 2004 are included in the analysis.

3 Theoretical framework and empirical specification

3.1 Teachers' decision making process

The decision making process of teachers choosing whether to stay with their current employer or search for a new job can be framed within a turnover theory proposed by Jovanovic (1979). In the first period of time, I observe a teacher employed by a certain school and I assume that the employment decision was made so that it maximizes their utility with respect to the job characteristics (Jackson, 2013). For simplicity, let us assume that teachers only value the quality of their students and the monetary compensation 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 school can potentially be altered either by changes in student composition or by changes in wages.

Since the admission system did not change over time, the expected quality of incoming students was roughly constant prior to the reform. Therefore, teachers did not expect that their match quality with respect to student quality would rapidly change and teachers with good matches were less likely to separate from their schools. Naturally, even without a policy change, teacher mobility is not zero. There are several reasons for this phenomenon. First, since at any point in time there are also poor matches between schools and teachers – formed due to imperfect information or uncertainty about student composition – so there are teachers switching schools in between school years. Second, there are teachers employed on fixed contracts (for example, as substitutes for permanent teachers who are on leaves) who leave their position once it can be filled again. Third, teachers retire or pass away, and thus, they drop out of the sample and new teachers need to be hired as replacements.[16] Having a poor match, however, is specific for a given school but not teaching as a profession, and thus, teachers with low quality matches should rather switch schools than leave the profession. On the other hand, retired or deceased teachers will naturally leave the profession. Finally, it is not clear a priori if teachers employed on fixed contracts are more or less likely to leave for a different occupation or switch schools within the profession.

So far I have discussed an individual teacher's separation decision - supply side. However, the decision naturally interacts with their employer's demand for new or existing teachers. Although firing teachers is relatively hard 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, possibly as a result of increased mobility following the reform. However, the decisions made by principals regarding hires will only be observed after teachers decide whether to stay with his or her current school or separate. Therefore, the reform should not have an immediate influence on hiring policies but rather a delayed effect.

The framework discussed above generates two predictions that can be tested empirically: first, since teachers value working with high quality students, they will be

[16] I shut down the retirement channel by limiting the sample to teacher no older than 58 years of age, however, I cannot exclude any disability pensions.

less likely to leave schools experiencing inflow of students with better credentials; second, if monetary and student quality inputs to a teacher's utility function are jointly determined then non-switchers who experience an inflow of students with worse credentials should expect a rise in monetary compensation.

3.2 Empirical specification

The reform can be described in two stages. First, it generated a change in the composition of incoming students in different schools, but it did not alter the average quality of students in the municipality of Stockholm. Figure 1 and Figure 2 show that the reform indeed altered the student quality in different schools. Second, the change in student composition caused teachers to face a different set of students from one year to the next, and generated a reshuffling of teachers whose match quality had been exogenously altered. For this second stage to be due to changes in student quality only, I require that students did not select schools based on the underlying trends in teacher turnover – I discuss this possibility in the main results.

Since the reform was implemented in the school year 2000/2001, it is natural to first compare schools before and after this date which experienced different changes in student quality. Such a comparison yields a difference-in-differences estimator in which schools are treated to different extents, depending on the change in student quality. Thus, I compare teacher turnover in schools that experienced a sharp increase (or fall) in student quality to teacher turnover in schools where the student composition did not change that much.

Furthermore, since high school education in Sweden consists of three grades, it took up to three years for the reform to be fully implemented. Thus, in school year 2000/2001 only a third of the student stock had been admitted under the new rules and it was not until the school year 2002/2003 that the reform came into effect for the full student stock. Because of this feature of the reform, I study how teacher mobility changes up to three years after the reform. For the pre-treatment period not to overlap with the post-treatment period, I lag the pre-treatment measure of student quality one year for every additional year that I follow teacher mobility. In other words, a one-year teacher mobility analysis compares students in school year 1999/2000 to students in 2000/2001. A two-year mobility analysis compares students in school year 1998/1999

to students in 2000/2001, while a three-year mobility analysis compares students in school year 1997/1998 to students in 2000/2001.

Given the nature of the outcome variable I need at least two years to construct a single observation of the outcome variable, that is, I need to observe a teacher in periods t and t+1 to construct a mobility indicator. Since it took up to three years for the reform to be fully implemented, I construct three mobility measures. In each measure teacher is observed in school j in period t, and then separately in period t+1 (one-year mobility), period t+2 (two-year mobility) or period t+3 (three-year mobility). Thus, if I want to study the full effect of the reform, I compare the probability that teacher i in school j in 1997/1998 had left the school by 2000/2001 with the probability that teacher i in school j in 2000/2001 had left the school by 2003/2004. The treatment is set to the first year in the mobility window and, thus, compares the difference in incoming student quality in school year 1997/1998 to incoming student quality in school year 2000/2001. This can be written as:

$$(Y_{ij}^{2000} - Y_{ij}^{2000+k}) - (Y_{ij}^{2000-k} - Y_{ij}^{2000}) = a + b(T_j^{2000} - T_j^{2000-k}) + g(X_{ij}^{2000} - X_{ij}^{2000-k}) + d_j + j_{2000} + e_{ij} \quad (1)$$

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 quality 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 in this paper is β and it identifies the effect of student quality on teacher mobility.

Equation (1) estimates the causal effect of student quality on the probability that a teacher separates from his or her current school, assuming that changes in student composition are not correlated with changes in teacher mobility in an absence of the reform. One testable implication of the identifying assumption is that post-reform changes in student quality in different schools are not correlated with pre-reform changes in teacher mobility in these schools. This examines if the assumption about a common underlying trends in teacher turnover in the absence of the reform is plausible. For the placebo analysis to be meaningful, however, 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:

$$(Y_{ij}^{2000-k} - Y_{ij}^{2000}) - (Y_{ij}^{2000-2*k} - Y_{ij}^{2000-k}) = a + b(T_j^{2000} - T_j^{2000-k}) + g(X_{ij}^{2000-k} - X_{ij}^{2000-2*k}) + d_j + j_{2000-k} + e_{ij} \quad (2)$$

where Y, T, X, δ, φ and ε are defined as in Equation (1).

Equation (2) directly estimates the possibility of an anticipation effect. However, finding insignificant results in placebo estimates does not prove that the effect is not present as failing to reject a hypothesis does not imply it is true. Furthermore, one should focus not only on the second moment, which could be uninformative in the case of low precision in the estimates, but also on the point estimate which should be as close to zero as possible. Therefore, in order to be on the safe side, and since it is possible to directly account for an anticipation effect, I lag the dependent variable by one period in Equation (3). Such a procedure mechanically purges the possibility of a reaction to student quality in advance of the policy implementation. It requires, however, following teachers for four years for the reform to be fully implemented. 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).

In the specification described by Equation (3) I define the outcome variable as a comparison between the probability that teacher i in school j in 1995/1996 had left the school by 1999/2000 and the probability that teacher i in school j in 1999/2000 had left the school by 2003/2004. At the same time, the treatment compares the difference in incoming student quality between school year 1996/1997 and incoming student quality in school year 2000/2001. 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 close to zero estimates in a one-period window in this specification. More formally, I can write:

$$(Y_{ij}^{1999} - Y_{ij}^{1999+k}) - (Y_{ij}^{1999-k} - Y_{ij}^{1999}) = a + b(T_j^{2000} - T_j^{2000-k}) + g(X_{ij}^{1999} - X_{ij}^{1999-k}) + d_j + j_{1999} + e_{ij} \quad (3)$$

where Y, T, X, δ, φ and ε are defined as in Equation (1). Details about specific school years that I use for outcome and treatment variables of different exposure lengths in regressions defined by Equations (1) and (3) can be found in Table A 2 and Table A 3.

Note that in each regression I use only one pre- and one post-reform period, although I use multiple years to construct the outcome variables.[17]

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 negative changes in student quality, respectively.[18] I divide schools into two groups based on their changes in incoming student credentials between school years 1999/2000 (pre-reform) and 2000/2001 (post-reform). In the first column, I show data for one-third of schools with the most positive changes in incoming student credentials (one-third upward) while in the second column I show data for one-third of schools with the least positive (or negative) changes in incoming student credentials (one-third downward). On average, student quality increased by 15.79 percentile points in upward shocked schools and it decreased by 6.78 percentile points in downward shocked schools.[19] Concurrently, teacher mobility decreased by 20 pp in upward shocked schools and there was virtually no change in mobility in downward shocked schools.

[17] An always-present question in this type of analysis is interdependence across units, that is, whether and how to cluster the standard errors. 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 my setting with only 15 schools, these large sample properties cannot be invoked (see for example 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. But as one could argue that it is more reasonable to assume independence at an aggregated level, in the Appendix I also report standard errors with alternative clustering for the baseline specification – equation 3 (i.e. columns 2 and 3 in Table A 5). Reassuringly, the main results follow through. More specifically in Table A 5 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 A 6 I re-do the analyses from Table A 5 but with an unbalanced panel allowing the maximum number of schools the data allows.

[18] In order to provide better intuition about the timing of the reform and the reshuffling of students I start off with the model that does not account for the anticipation effect and does not require a lagged dependent variable. In Table A4, however, I also present the results for the Wald estimator accounting for the anticipation effects. Thus, Table A4 compares four-year mobility and four-year changes in student quality before and after the reshuffling started. The results are remarkably similar.

[19] This does not indicate that the average student quality in Stockholm increased due to the reform as the comparison excludes the middle quality schools. However, comparing the quality of incoming students between 1997 and 2000 indeed suggests that student quality increased by 6 percentile points. This fact can be driven by multiple factors: focusing on a panel of more stable schools, differential inflow of high-quality students from outside-of-Stockholm; or differential grade inflation. When analyzing all schools in Stockholm the average incoming students GPA is 54 in 1997, 58 in 1999, and 58 in 2000. Furthermore, my calculations show that there is no differential inflow of students residing outside of Stockholm. Thus, given that the averages in 1999 and 2000 are very similar but the average in 1997 is lower I conclude that over time there is some grade inflation at the upper end of the grade distribution. It is, however, small in comparison to the magnitude of the shock and should be purged by school and time fixed effects.

Table 2. Effects of changes in students' credentials and probability of leaving school within 3-years

Effects of 3-year changes in student quality on 3-year teacher mobility			
	Schools		
	1/3 upward shocked	1/3 downward shocked	Difference
Treatment: Student quality - percentile ranked GPA from 9th grade in primary school. Incoming students graduating 9th grade in the same year.			
Year 2000	71.92	45.70	26.22***
	(14.19)	(10.40)	(1.06)
Year 1997	56.13	52.48	3.65***
	(13.41)	(12.68)	(1.03)
Difference	15.79***	-6.78***	**22.57*** **
	(1.09)	(0.99)	**(1.46)**
Dependent variable: Leaving school j from year 1997 to year 2000 (3-year mobility)			
Year 2000	0.17	0.22	-0.06*
	(0.37)	(0.42)	(0.03)
Year 1997	0.36	0.26	0.10***
	(0.48)	(0.44)	(0.04)
Difference	-0.20***	-0.04	**-0.16*** **
	(0.03)	(0.04)	**(0.05)**
	Wald estimate		
	-0.007* **		
	(0.002)		

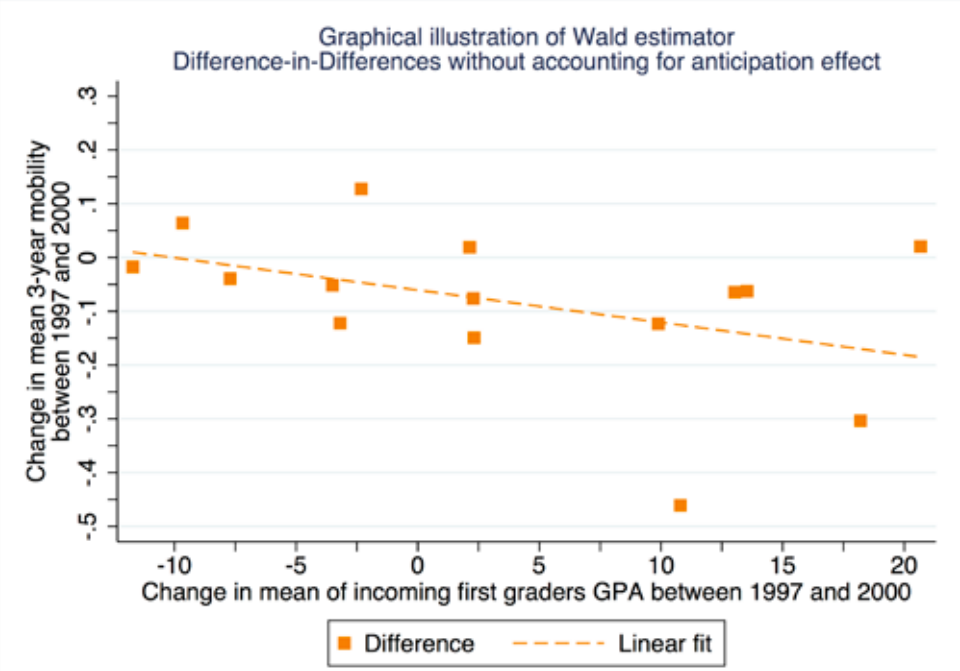
Note: Shock is defined as a difference between mean students' credentials measured by primary school 9th grade GPA (only students who applied to school in the same year) in first grade of high school j in the first post-reform year 2000 and alike defined mean students' credentials in the last pre-reform 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). Only schools that are present in the data in each year between 1991 and 2004 are included in the analysis. It results in a sample of 15 schools. Dependent variable is defined as probability of leaving school j from school year 1997/1998 to school year 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 difference in mean incoming students' credentials between 1997 in pre-period and 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 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.

By calculating the ratio of the two changes (-16 pp divided by 22.57 percentile points) I obtain the Wald estimate of 3-year teacher mobility on incoming students quality. It 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.

Finally, to illustrate how I exploit all of the variation in the changes in student quality, Figure 4 plots the differences in mobility for each school against the differences in the GPA of incoming students. This figure 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 were more likely to have experienced relatively large reductions in mobility. The dashed line in the figure shows a linear fit of the individual school observations and clearly points towards a negative relationship between changes in student quality and changes in teacher mobility.[20]

Figure 4. Difference-in-Differences. Probability of leaving school j in 3-years



Note: Values on the vertical axis represent differences in mean 3-year mobility between 1997 (pre-reform) and 2000 (post-reform). This figure does not account for potential anticipation effects and quits related to rumors or announcement of the 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. Student credentials are measured using primary school 9th grade GPA. Line represents linear regression fit. Only schools that are present in the data in each year between 1991 and 2004 are included in the analysis.

4 Main results

I start by presenting the results for the regressions specified in Equation (1), where I assume that teachers did not anticipate the changes in student quality. In Table 3, I report the estimates for the effects of changes in student quality on one-year mobility (row 1), two-year mobility (row 2) and three-year mobility (row 3). In column (1) I present correlations between GPA and mobility, in column (2) I present difference-in-differences estimates without controlling for any observable teacher characteristics, while in column (3) I condition on a set of teacher controls. The estimates do not change much when I control for teacher characteristics, thus supporting the quasi-experimental

[20] Figure A1 presents the same graph but for fully implemented reform specified by Equation (3). It points to the same conclusion as Figure 4.

nature of student resorting.[21] Since the reform gradually changed the student composition in schools, it is interesting to note that teachers' responses seem stronger the larger the share of students that gained admission under the new rules. The point estimate in row (3) in column (3) indicates that a 10-percentile-point increase in student quality reduces the probability of individual teacher turnover within three years by 7 pp.

Table 3. The effects of student credentials (first grade) on probability of leaving school j. No anticipation effects

Dependent variable: probability of leaving school j within k years	(1) OLS	(2) DD	(3) DD
1-year mobility	0.000	-0.001	-0.002
	(0.001)	(0.002)	(0.002)
Observations		1,590	
2-year mobility	-0.002***	-0.002	-0.004*
	(0.001)	(0.002)	(0.002)
Observations		1,770	
3-year mobility	-0.001*	-0.006***	-0.007***
	(0.001)	(0.002)	(0.002)
Observations		1,710	
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. This table does not account for potential anticipation effect and quits related to rumors or announcement of the reform. 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 students who applied to school in the same year) in first grade of high school j. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

The model underlying the estimates in Table 3 assumes that teachers did not anticipate the changes in student composition that followed the announcement of the reform in the fall of 1999. However, the GPA of incoming students at the school was made public around May 2000 and teachers could have left the school until October 2000.[22] The crucial question is whether teachers react to information about the quality of incoming students or the realization of the quality of incoming students. Thus, in an attempt to detect any potential anticipation effects, I estimate Equation (2), which is a placebo test of the difference-in-differences specification in Equation (1). The results are presented in Table 4. They clearly support the fact that teachers did not seem to respond to the information on future student quality. The estimates are insignificant and relatively small.

[21] 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.

[22] 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 registers.

Table 4. Placebo analysis for regressions in table three. Effects of post reform changes in students' credentials on pre-reform changes in probability of leaving school j

Variable of interest/Difference	(1) 1-year	(2) 2-years	(3) 3-years
1st graders quality	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)
Observations	1,736	1,847	1,839

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 students who applied to school in the same year) in first grade of 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-years mobility in 1998/1999. In column (3) I compare three-year mobility in 1994/1995 to three-years mobility in 1997/1998. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

Thus far I have focused on the supply side of the teacher's labor market. Teachers who faced abrupt positive changes in the quality of their incoming students became less likely to separate from their current school. In this section, I analyze school responses to changes in student quality. In particular, I test whether the reform affected the number of enrolled students, which could mechanically lead to changes in teacher turnover. For example, if schools that experienced a positive shock to student quality also admitted more students after the reform, then it is possible that these schools attempted to retain or hire more teachers. Conversely, if unpopular schools both lost pupils and admitted students of lower quality after the reform they may have been forced to let some teachers go.[23] In addition, I analyze to what extent changes in student quality affected the number of teachers in the school as well as the student-teacher ratio.

Table 5 presents the effects of the reform on changes in the number of students, the number of teachers and the student-teacher ratio. 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, similar to the treatment, but not over multiple time periods as in the

[23] Note that the funding of schools in Sweden is tied to the number of enrolled students. The reform could also force some students to change schools as a response to changes in peer composition. I address these issues 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 probability at the school level. The regression framework is identical to Table 6 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. For each exposure length I find small but highly significant results on student mobility. In the anticipation year the point estimate is 0.001 and in subsequent differences these are -0.001, -0.0008 and -0.0005 for one, two and three year windows, respectively. Given that roughly 12% of students change schools from year to year, these effects are tiny in terms of magnitude, although they are statistically significant. Given an average school size of 825 pupils, the estimate suggests that when a school is shocked by a one-standard-deviation decrease in school quality, for each 91 pupils that would normally leave the school, an extra student will leave due to the changes in student quality. Since this estimate is tiny and I do not find any effects on the average school size, I conclude that general equilibrium effects are unlikely to play a major role in a teacher's decision making process.

case of mobility. 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. Thus, the outcome variable takes the form of a comparison between the last three years prior to the reform and the first three years after the reform, but I set up the treatment as in all other regressions. The results in Table 5 show that neither the number of students, the number of teachers nor the student-teacher ratio responded to changes in student quality. Thus, it is unlikely that the changes in teacher mobility following the reform were a mechanical consequence of changes in school size or school resources.

Table 5. Difference-in-Differences: Effects of the reform on school size and resources

Variables	(1) 1-year	(2) 2-year	(3) 3-year
Panel A: Number of students			
1st graders quality	0.143	1.272	2.749
	(2.742)	(3.298)	(3.608)
Panel B: Number of teachers			
1st graders quality	0.182	0.195	0.230
	(0.367)	(0.466)	(0.755)
Panel C: Student-teacher ratio			
1st graders quality	-0.018	-0.002	-0.002
	(0.068)	(0.062)	(0.089)
Observations	30	30	30

Note: School level difference-in-differences. Regressing number of students attending school (panel A), number of teachers at school (panel B) and student-teacher ratio at school (panel C) on students' credentials and school and time fixed effects. Students' credentials measured by primary school 9th grade GPA (only students who applied to school in the same year) in first grade of high school j. The dependent variables are measured in 1999 in the pre-reform period and in 2000, 2001, 2002 in the post-reform period for 1, 2, and 3-year exposure, respectively. The independent variable is measured in 1999 in pre- and in 2000 in post-period. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

In Table 4 I showed placebo estimates suggesting that there is no significant anticipation effect in teacher turnover decisions. However, in order to further rule out the possibility of a bias, I estimate the effects using the specification from Equation (3) – Table 6. It implies that teachers could not possibly anticipate changes in student quality because I lag the dependent variable by one year and the reform was not even announced yet early in the fall of school year 1999/2000. The first row of Table 6 can also be treated as a test for the anticipation effects as it compares one-year mobility in 1998/1999 to one-year mobility in 1999/2000. If there are no anticipation effects, then I should find an effect that is insignificant and close to zero. This is indeed the case, which should reassure the readers that teachers in Stockholm did not react significantly to the information and expectations, but rather they reacted to realized changes in student quality. 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. The estimates in column (3) for rows (2) through (4) in Table 6 that correspond to estimates from column (3) in Table 3 are slightly larger but in the same general ballpark. Thus far I have shown that there are no anticipation effects on average, yet it may well be the case that some teachers, such as better educated ones, are better at anticipating the effects of the reform. Therefore, instead of presenting placebo estimates for all possible settings studied in this paper, I lag the outcome variables of interest in each case, and thus, mechanically purge the possibility of an anticipation effect.

Table 6. The effects of student credentials (first grade) on probability of leaving school j. Accounting for anticipation effects

Dependent variable: probability of leaving school j within k years	**(1) OLS**	**(2) DD**	**(3) DD**
1-year	-0.001**	-0.000	-0.001
	(0.001)	(0.002)	(0.002)
Observations		1,736	
2-years	-0.001	-0.003	-0.004**
	(0.001)	(0.002)	(0.002)
Observations		1,676	
3-years	-0.000	-0.006***	-0.007***
	(0.001)	(0.002)	(0.002)
Observations		1,667	
4-years	-0.001	-0.008***	-0.009***
	(0.001)	(0.002)	(0.002)
Observations		1,657	
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. This table through one-year lag in outcome variable (with respect to reform timing) accounts for potential anticipation effect and quits related to rumors or announcement of the reform. 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. Students' credentials measured by primary school 9th grade GPA (only students who applied to school in the same year) in first grade of high school j. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

The point estimate in row (4) of column (3) in Table 6 is the most important and most conservative estimate to be taken away from this paper. It suggests that when the reform was fully implemented a 10-percentile-point increase in student quality reduced the probability of a teacher leaving his or her school by 9 pp. Alternatively, a one standard deviation (17.3) increase in incoming student credentials decreased the probability of a separation within four years by 16 pp.[24] Given that the average four-year separation rate

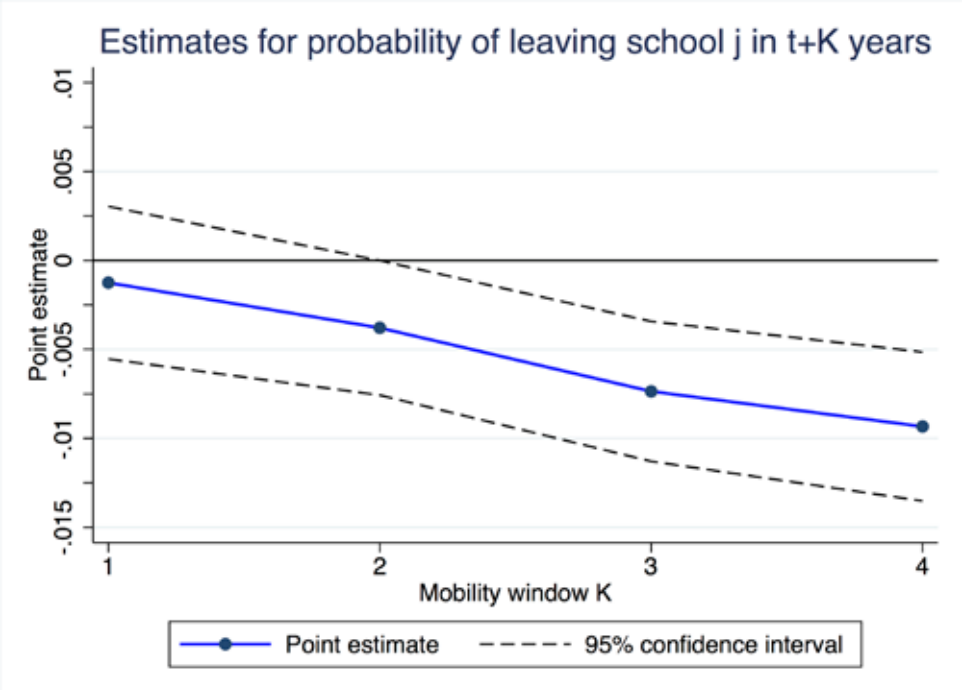
[24] When I include the quadratic in students' credentials in the equation the coefficient on linear part remains negative and significant while the coefficient on quadratic term is positive and significant. Thus, the relationship between

in this sample is 32%, the result implies a 50% reduction in mobility. As can be seen in Figure A 1, there are two schools in Stockholm in which students improved by more than a standard deviation, three schools that improved by roughly three-quarters of a standard deviation, two schools that depreciated by a half of a standard deviation, and three schools that depreciated by roughly a third of a standard deviation. In summary, my findings are not only statistically significant, but also economically large and policy relevant.

Finally, in order to visualize how the effect of the changes in student quality evolved over time as the reform progressed, Figure 5 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=0.030).

student quality and teacher mobility is estimated to be convex i.e., the higher inflow of good students has marginally diminishing effect on teacher separation rates.

Figure 5. Difference-in-Differences estimates for different exposure lengths



Note: Estimates from teacher level regressions controlling for school and year fixed effects as well as individual controls. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Each point comes from a separate regression. Robust standard errors. 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. Students' credentials measured by primary school 9th grade GPA (only students who applied to school in the same year) in first grade of high school j. Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience.

5 Is it really about student quality?

Thus far I have presented evidence that higher student quality reduces the probability that teachers leave their current employment. To the best of my knowledge this is the first paper that estimates causal effects of changes in student quality, as measured by academic credentials, on teacher labor supply decisions. Student quality is, however, correlated with other observable variables such as the fraction of minority students or parental wealth. For instance, Jackson (2009) used a similar identification strategy to gauge the causal effect of the reshuffling of minority students on teacher mobility. It is therefore relevant to ask whether it is direct measures of student quality or variables correlated with student quality that drive teachers' decisions.

My data include a number of background characteristics that may proxy for student quality such as whether a pupil is a first generation immigrant. I also have information on the yearly income and education of a pupil's parents, for which I compute school-level averages. Finally, I use military draft data with information on the cognitive and

non-cognitive assessment of fathers. The results are presented in Table 7 where I focus on the specification of interest based on column (3) and row (4) from Table 6 with the estimate showing the effect for the fully implemented reform and accounting for an anticipation effect. The first row of Table 7 presents estimates in which the treatment is defined as a fraction of first generation immigrants (a correlation of 0.39 with GPA), the second row presents estimates for mean parental income (a correlation of 0.81 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 0.79 with GPA).[25] Column (1) presents the effects of the characteristics from rows (1) to (4) while column (2) adds a student quality measure in a horse race between direct and indirect measures of student quality.

Table 7. Probability of leaving school j. Alternative measures of student composition

Dependent variable: probability of leaving school j within k years	**(1) Unconditional**	**(2) Conditional on credentials**
Share of immigrant students	1.199***	0.209
	(0.447)	(0.532)
GPA		-0.009***
		(0.002)
Mean parental income in 1000 SEK	-0.001**	0.003***
	(0.000)	(0.001)
GPA		-0.026***
		(0.005)
Mean parental education	-0.190***	-0.119**
	(0.049)	(0.053)
GPA		-0.007***
		(0.002)
Mean combined cognitive and non-cognitive paternal IQ	-0.017***	-0.008
	(0.004)	(0.005)
GPA		-0.007***
		(0.003)
Observations	1,657	

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls. Each row and column reports estimates from a separate regression. All regressions based on specification from 6, row (4) and column (3). In column (1) I substitute students' credentials with other mean school-level first grade characteristics, mainly, 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 first grader GPA (only students who applied to school in the same year) at the level of 0.39, 0.81, 0.91 and 0.79, respectively. In column (2) I keep these alternative measures but also include first graders GPA (only students who applied to school in the same year). Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

[25] 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 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.

First, I focus on the unconditional effects. The estimate in row (1) confirms what other researchers have previously found in descriptive analyses, in particular, 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). Furthermore, unlike other researchers I do not find any evidence for the clustering of immigrant teachers and minority students in either specification. In row (2), the coefficient on mean yearly income in 100 000 SEK is -0.074 with a standard error of 0.035. This is a small estimate given that the mean yearly parental income in the studied group of schools is 377 696 SEK and in standard deviation terms it is roughly half of the effect estimated for student GPA. Similarly, rows (3) and (4) indicate significant and robust negative effects of increased parental quality on the probability of job separation. The intergenerational transmission of education has been well documented in the literature so it is not surprising that parental education is a good measure of student quality (Björklund et al., 2006). The last estimate is in line with Black et al. (2009), who document an intergenerational mechanism of cognitive skills transmission, and thus, it is not surprising that teachers favor working with students whose fathers obtained relatively higher cognitive and non-cognitive scores in the military assessment.

In column (2), the estimates for the fraction of minorities and paternal military assessments become insignificant and decrease in size after controlling for students' credentials. The coefficient on mean parental income actually turns positive. On the other hand, the two coefficients in row (3) are negative when I include both parental education and student GPA. Overall, the estimates in column (2) suggest that teachers value primarily student quality, but that some of the response to changes in student quality is driven by changes in the students' socio-economic backgrounds.[26] In particular, teachers may prefer working with poorer students conditional on their high quality.

[26] Since direct (student quality) and indirect (share of immigrants, parental income and education, paternal military test scores) are highly correlated, one might be worried that models in column (2) pick up non-linear measures of student quality. When I add the square of student GPA to the estimates in column (2), however, it turns out to be positive and significant in all estimations (similar to the main specification). At the same time, the linear term in student quality remains highly significant and negative in all cases. Finally, the significant negative coefficient on parental education becomes insignificant suggesting that indeed it was picking up some non-linearity in student quality, however, the coefficient on parental income remains positive and significant with an identical point estimate.

6 Heterogeneity analysis

The richness and completeness of Swedish registry data allows me to investigate heterogeneity in the effects of student quality. It is important from a policy point of view to learn whether the effects of student quality vary by teacher characteristics. In particular, the consequences of the admissions reform could be very different if high-quality teachers are more likely to leave the most disadvantaged schools. Therefore, I analyze how the response to changes in student composition differs by teacher quality, as measured by their formal education and experience. For male teachers born after 1951 I also have information about their cognitive and non-cognitive skills as measured at the military draft. Furthermore, I study how the response to changes in student credentials differs by the teacher's gender, specialization and type of contract. Then, I also divide teacher mobility by their destination. Finally, I split schools into quartiles of student quality distribution measured in the pre-reform period.

Table 8 presents a range of heterogeneity findings. The table has the following structure: the first column reports the fraction of teachers in each group, while the second column reports the mean and a standard deviation of 4-year mobility for the group. The third column reports the point estimate and standard error of the effect of student quality on 4-year teacher mobility for the group. Finally, the fourth column presents a joint significance test for whether the point estimates for different sub-groups of teachers are different from one another.

Table 8. Heterogeneous effects by teachers' characteristics

	Characteristic	Group	(1) Fraction [%]	(2) Mean mobility	(3) Estimate	(4) p-value difference
(1)	University education	Yes	72	0.286	-0.009***	**0.706**
				(0.452)	(0.002)	
		No	28	0.422	-0.007*	
				(0.494)	(0.004)	
(2)	Experience	0-5	24	0.536	-0.011**	**0.917**
				(0.500)	(0.005)	
		6-15	36	0.323	-0.008**	
				(0.468)	(0.004)	
		16+	40	0.195	-0.009***	
				(0.396)	(0.003)	
(3)	Cognitive assessment	High	73	0.381	0.001	**0.207**
				(0.487)	(0.007)	
		Low	27	0.352	-0.017	
				(0.481)	(0.012)	
(4)	Non-cognitive assessment	High	50	0.392	-0.013	**0.323**
				(0.490)	(0.008)	
		Low	50	0.354	-0.001	
				(0.480)	(0.008)	
(5)	Gender	Male	48	0.347	-0.011***	**0.492**
				(0.476)	(0.003)	
		Female	52	0.303	-0.008***	
				(0.460)	(0.003)	
(6)	Subject taught	Science	10	0.405	-0.011	**0.865**
				(0.492)	(0.009)	
		Other	90	0.315	-0.009***	
				(0.465)	(0.002)	
(7)	Type of contract	Permanent	81	0.270	-0.008***	**0.619**
				(0.444)	(0.002)	
		Temporary	19	0.554	-0.011**	
				(0.498)	(0.005)	

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls. Each row reports estimates from a separate regression. Columns (1) and (2) present descriptive statistics for each group. Column (1) reports fraction of individuals in each group while column (2) reports mean and standard deviation of a dependent variable (4-year mobility) in each group. Column (3) reports point estimates from regression specified as in Table 6, row (4) and column (3) for each group separately. Column (4) presents the joint significance test for the analyzed groups in difference-in-differences model from column (3). Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicators and experience. 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. In row (6) science teachers include: mathematics, physics, chemistry, biology and computer science subjects. In rows (3) and (4) sample is restricted to native, males for whom both cognitive and non-cognitive assessment is observed. Cognitive and non-cognitive test scores are available for 89% of Swedish male population born 1951 or later. Low score is defined as below or equal to median in population percentiled draft-year distribution, while high score is defined as above median in population percentiled draft-year distribution. Sample size 1657 based on 1995 and 1999 comparison. Sample size for cognitive and non-cognitive skills regression is 260 based on 1995 and 1999 comparison. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

I first consider the standard teacher quality measures. The first and second panels of Table 8 stratify teachers by their education and experience, which are important predictors of student achievement (Boyd et al., 2005; Harris and Sass, 2011). More than one-quarter of secondary school teachers in Stockholm do not have a formal university degree. Although these teachers have substantially higher turnover rates (42% vs. 29%),

the estimated relationship between student quality and probability of leaving the current employment is remarkably similar (-0.007 vs. -0.009).

The same observation applies to teacher experience. Even though there are large differences in average turnover rates between groups (for example 54% for the least experienced teachers and only 20% for the most experienced ones), the point estimates are virtually identical, suggesting that the effects of student quality are similar across the distribution of teacher quality. It is worth noting, however, that although similar in percentage points and not significantly different from one another the point estimates suggest different relative reductions in mobility percent wise due to large differences in average mobility levels between the groups.

In rows (3) and (4) I further explore the uniqueness of the Swedish registry data and split teachers by their cognitive and non-cognitive skills that are available for all native males born in 1951 or later in Sweden. The sample size in this analysis is reduced dramatically to only 260 observations. The estimated responses to changes in student composition are somewhat different for teachers of different skills, but due to the relatively few observations I fail to reject that the estimates are different from one another.

I also consider whether the estimated effect of student quality varies by teacher gender. While female teachers are somewhat less mobile than male teachers, the difference-in-differences estimates for both groups are virtually identical (-0.011 and -0.008, respectively). Another important group of teachers which often gets a lot of attention in media and research are science teachers (Edmark and Nordström Skans, 2010). On the one hand, providing these skills to students may be important for their chances on the labor market. On the other hand, teachers with this specialization may have favorable outside options. Thus, it is worth learning how changes in student quality affect teachers in mathematics, physics, chemistry, biology, and computer science in comparison to other teachers (row (6)). Even though science teachers have higher mobility rates I fail to find any significant evidence that they respond stronger to changes in student composition than other teachers.

Finally, I present estimates separately for teachers on permanent and temporary contracts. The latter teachers are typically employed on fixed-term contracts, often as replacements for teachers on extended leave, and are exposed to higher probabilities of

job separation. Nearly 20% of teachers in Stockholm are employed on a temporary basis and they have more than twice as high turnover rates as permanently employed teachers. The estimated coefficients indicate, however, that the effects of student quality are virtually identical irrespectively of the type of employment. The evidence suggests that most teachers are affected to the same extent by changes in student quality. This may indicate that schools that end up with lower quality students are likely to lose all types of teachers and not only the best (or the worst) ones.[27]

The models used so far pool all of teacher mobility into one destination. However, previous research indicates that the correlations with teacher characteristics differ depending on the destination (Lankford et al., 2002). In Table 9, I investigate whether the effects of changes in student credentials are stronger along some mobility margins than others. In particular, I estimate the effect of student quality on teacher 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 a higher quality of students (row (5)). Since it should be of particular interest to policy makers if 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 university degree (column (4)).

[27] This statement might not be completely accurate as the groups presented in Table 8 overlap. To purge this confounding factor I use teacher's individual characteristics to predict their 4-year mobility and divide it into 10 mutually exclusive groups. I then run heterogeneity analysis using these groups. Even though the groups range in mean predicted mobility from 12.7% to 68.5%, the estimated effects are very similar and in a range between -0.025 to 0.005, and the slope of the line for 10 estimates is insignificant -0.0002. Thus, I conclude that the estimated effects of student quality are indeed identical for all teachers.

Table 9. Heterogeneity analysis: Effects by teachers' destination

Sample	(1) All teachers	(2) All teachers	(3) Teachers with university degree	(4) Teachers with university degree
Dependent variable: probability of leaving school j within 4 years	Mean	Estimate	Mean	Estimate
Mobility within high schools	0.093	-0.006***	0.083	-0.005***
	(0.290)	(0.001)	(0.276)	(0.002)
Mobility within schooling	0.158	-0.007***	0.153	-0.006***
	(0.365)	(0.002)	(0.360)	(0.002)
Mobility to private school	0.011	-0.000	0.008	-0.001*
	(0.104)	(0.001)	(0.091)	(0.001)
Out of schooling sector	0.166	-0.003	0.133	-0.003
	(0.372)	(0.002)	(0.340)	(0.002)
To a higher quality school	0.048	-0.003***	0.051	-0.002**
	(0.213)	(0.001)	(0.221)	(0.001)
Observations	1,657		1,192	

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls. Each row in columns (2) and (4) reports estimates from a separate regression. Columns (1) and (3) present means and standard deviations 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 variable in row (1) equals unity if teacher leaves for another teaching position in high school. Dependent variable in row (2) equals unity if teacher leaves for another teaching position within primary or secondary schooling. Dependent variable in row (3) equals unity if teacher leaves for another teaching position in a primary or secondary private school. Dependent variable in row (4) equals unity if teacher leaves for another occupation outside of teaching. Dependent variable in row (5) equals unity if teacher leaves for high school with higher student quality than their initial allocation. Rows (2) and (4) add up to total mobility measure used in previous specifications. 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. Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

The estimates in the first two rows in column (2) show that increases in student credentials reduce the probability that a teacher will leave their current school for either a different high school or for any other primary or secondary school to the same extent. These estimates are also virtually identical for teachers with university education. Furthermore, I do not find any significant relationship between student quality and moving to a private school for all teachers, while the estimate is small and significant for teachers with a university diploma. On the contrary, I find significant and negative estimates on the probability of leaving to a school with a higher quality of students. This potentially provides meaningful information about the direct manifestation of teacher preferences. Teachers seem to value the quality of their students, as they flee the adversely shocked schools in favor of schools with higher student quality. Finally, I do not find any effects of student quality on the probability that teachers leave the profession. This is in accordance with Jackson (2013), who argues that teachers will adjust their match quality within the profession rather than through outflow from the profession.

The last aspect of the heterogeneity analysis investigates the distributional effects of changes in student quality. These might be especially important as adversely shocked teachers tend to move to schools that have better a pool of students. I investigate this phenomenon in two ways. First, in Table 10, I study how teachers employed initially in schools from different parts of the student quality distribution respond to changes in their pupils' composition. Second, in Table 11, I study how teachers react to changes in the fraction of students from different parts of the quality distribution. For every school and year, I calculate the fraction of students admitted from each quartile of the quality distribution. Then, I use these four variables in separate regressions as a substitute for the average of student credentials. Thus, Table 10 reports heterogeneous responses to the same treatment, while Table 11 documents reactions to heterogeneous treatments.

Table 10. Heterogeneity analysis: Effects by pre-reform school quality

Quartile of student quality	(1) Fraction [%]	(2) Mean mobility	(3) Estimate	(4) p-value difference
Bottom	27	0.206	-0.012***	
		(0.405)	(0.005)	
Lower middle	22	0.306	-0.028***	
		(0.462)	(0.006)	0.007
Higher middle	29	0.240	-0.003	
		(0.428)	(0.005)	
Top	22	0.137	-0.003	
		(0.344)	(0.005)	

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls. Each row in column (3) reports estimates from a separate regression. Columns (1) and (2) present descriptive statistics for each group. Column (1) reports fraction of individuals in each group while column (2) reports mean and standard deviation of a dependent variable (4-year mobility) in each group. Column (3) reports point estimates from regression specified as in Table 6, row (4) and column (3) for each group separately. Column (4) presents the joint significance test for the analyzed groups in difference-in-differences model from column (3). Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience. Sample sizes based on 1995 and 1999 comparison are 315, 258, 333 and 256 for rows (1) through (4), respectively. Student quality is divided into four quartiles based on the quality in school year 1996/1997 i.e., baseline student quality. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

Table 11. Heterogeneity analysis: Effects by changes in fraction of students in quartiles of quality distribution

	(1)	(2)	(3)	(4)
Quartile	**Bottom**	**Lower middle**	**Higher middle**	**Top**
Mean fraction	0.183	0.235	0.254	0.328
	(0.163)	(0.120)	(0.095)	(0.242)
Fraction of students in k-th quartile	1.511***	0.609***	-0.766***	-0.149
	(0.255)	(0.190)	(0.162)	(0.108)

Note: Teacher level regressions controlling for school and year fixed effects as well as individual controls. Each column in the second row reports estimate from a separate regression. First row presents mean and standard deviation of the share of students in a given quartile of the quality 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 separately. Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience. Sample size based on 1995 and 1999 comparison is 1657 observations. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Robust standard errors.

The results in Table 10 indicate that only teachers employed in the bottom half of the distribution respond to changes in student quality.[28] The coefficients on the upper half are identical for both quartiles at -0.003 with a standard error of 0.005. It should be of interest that most of the turnover occurs in the second quartile of the distribution, suggesting that teachers on the margin that experience a mixture of high and low quality students on a daily basis react most strongly to the reshuffling. In fact, teachers at the bottom of the student quality distribution are the only ones who are significantly more likely to leave the profession in favor of a different occupation. Among the worst performing schools, the point estimate of -0.008 suggests that a 10-percentile-point decrease in student quality increases the probability that a teacher leaves his or her school for a job in a different profession by 8 pp. At the same time, I do not find any significant results for within-teaching mobility for the lowest quality schools, with an estimate of -0.005 and a standard error of 0.004, yet I find strong negative estimates for within-profession mobility for the second lowest quartile – an estimate of -0.023 with a standard error of 0.004. This last piece of evidence suggests that teachers at the bottom of the distribution prefer to leave the profession when facing an adverse shock to student quality. However, their colleagues who are at the margin and who experience a mix of good and bad students seek a higher quality match within the occupation.

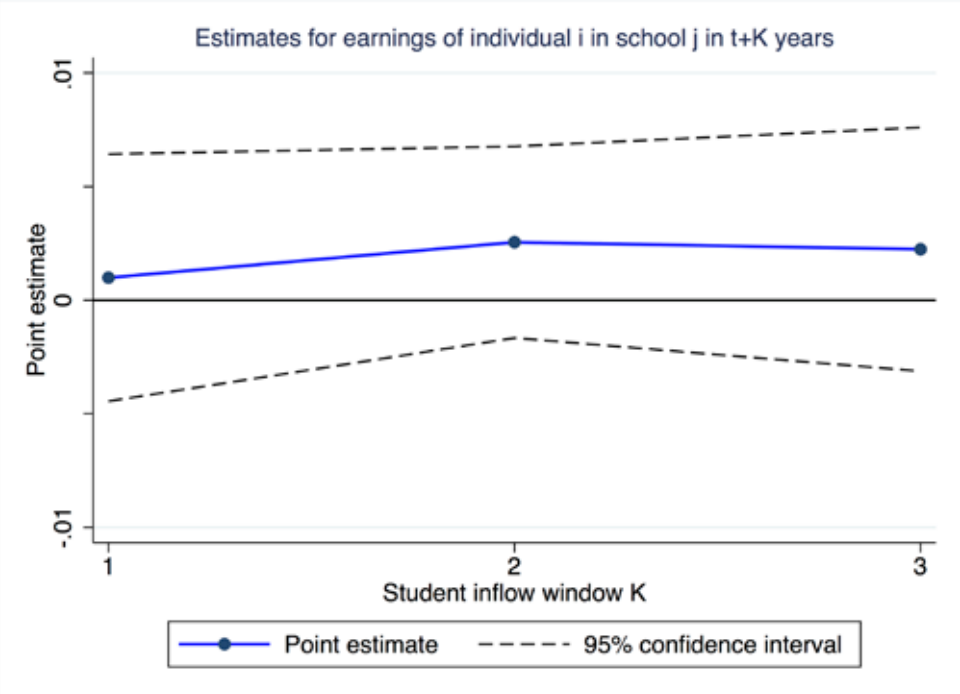
[28] When I split the sample into halves I only find significant estimate for the bottom half. It is -0.018 with SE of 0.004, while the estimate for the top half is -0.003 with SE of 0.003. The two coefficients are different at 1% level. When I split the sample into tertiles I find significant estimates for bottom and middle tertile. These are both -0.014 with SEs of 0.005 and 0.004, respectively. The coefficient for the top tertile is -0.002 with SE of 0.004 and the three coefficients are significantly different from one another at 10% level.

Finally, Table 11 leads to similar conclusions regarding the quality of students. Teachers who experience an inflow of students coming from the bottom half of the quality distribution are more likely to leave their current employment. The point estimate for the lowest quartile is also more than twice the size of the coefficient for the second lowest quartile suggesting that the really bad students have a significantly higher pushing out effect than their moderately more able peers. Here, it is also the case that only an increase in the fraction of students from the bottom quartile induces teachers to leave the profession. One the other hand, an increase in the fraction of students that come from the upper half of the quality distribution actually decreases the probability that teachers separate from their school. It is also interesting to note that this effects is driven by the mediocre students scoring above the median rather than by the very top students.

7 School responses: teacher earnings and hiring policy

Swedish teachers' wages are determined at a national level with some room for individual wage bargaining. Since the reform only affected the admission system in the municipality of Stockholm, any effects on wage bargaining at the national level were likely small. It is thus interesting to investigate whether the principals at Stockholm schools used teacher wages as a way to compensate for the changes in the attributes of the school. Figure 6 shows the point estimates together with 95-percent confidence intervals. Similarly to regressions in Table 5, this analysis is based on the static model in which the earnings are determined at a given point in time and do not require using multiple time periods to construct a single dependent variable. It is plausible however, that if teachers expected changes in student quality they could have renegotiated their monetary compensations in the school year 1999/2000 as an insurance against a potential shock. In fact, wage renegotiation is probably more plausible in this setting than changes in employment. Therefore, when analyzing earnings I account for the anticipation effects and compare the second-to-last year prior to the reform to the three years post-reform. All point estimates are positive, but they never reach statistical significance. If anything, the results indicate that schools with a positive shock to student quality raise wages in an attempt to retain old teachers and attract new ones.

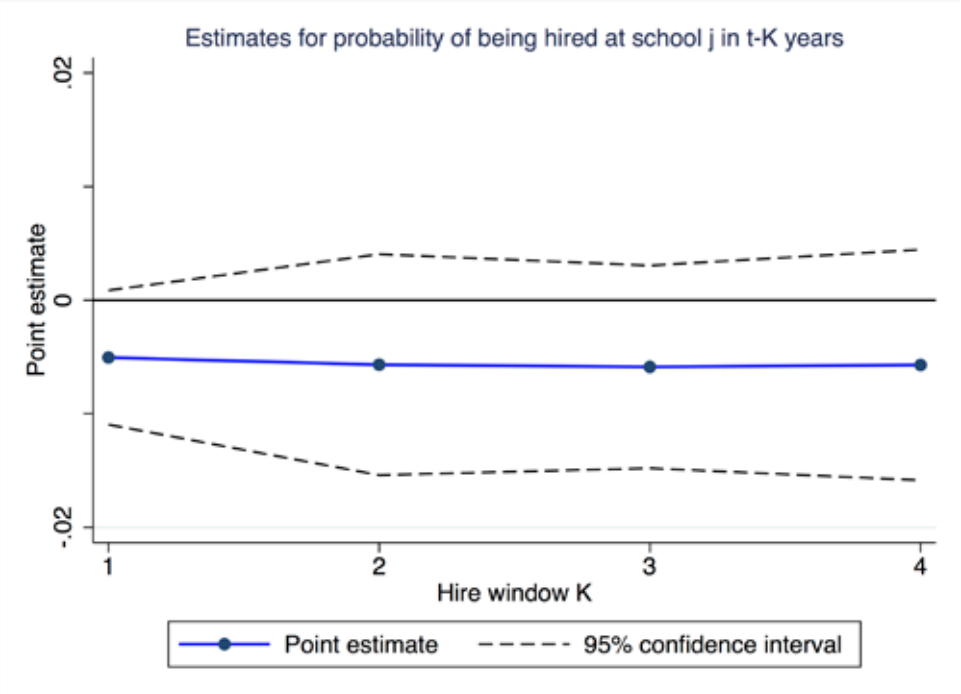
Figure 6. Difference-in-Differences estimates for individual teacher's earnings



Note: Estimates from teacher level regressions controlling for school and year fixed effects as well as individual controls. Only schools that are observed in each year between 1991 and 2004 are included in the analysis. Each point comes from a separate regression. Robust standard errors. 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. The independent variables of interest measuring students' credentials are defined in year 1999 in pre-period and in 2000 in post-period. Students' credentials measured by primary school 9th grade GPA (only students who applied to school in the same year) in first grade of high school j. Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience.

In Figure 7, I present results on teacher hiring. This analysis, akin to the analysis of mobility, is based on a dynamic treatment of the outcome variables. In particular, I need two time periods to define a single outcome variable. The dependent variable is defined as the probability of being hired a year, two, three or four years prior to baseline in pre- and post-treatment period. Here again I need to use four years in order to account for the anticipation effects. Even though I do not find any effects of the reform on changes in the number of teachers, it is plausible that principals might have attempted to contract some extra teachers if they expected their schools to be adversely shocked, resulting in an outflow of their current staff. Although statistically insignificant, all the estimates are negative, which suggests that schools that experience an increase in student quality retain their current teachers, and thus, reduce new hires.

Figure 7. Difference-in-Differences estimates for probability of being a new hire



Note: Estimates from teacher level regressions controlling for school and year fixed effects as well as individual controls. Only schools that are observed in each year between 1991 and 2004 are included in the regressions. Each point comes from a separate regression. Robust standard errors. 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. Students' credentials measured by primary school 9^{th} grade GPA (only students who applied to school in the same year) in first grade of high school j. Individual controls include: gender, marital status, immigration status, specialization (science, vocational, special education), university education indicator and experience.

8 Conclusions

A number of educational policies involve placing certain groups of students in a more favorable school environment, in hopes that interacting with better peers would boost their school performance. However, the success of such policies relies on, among other things, how teachers respond to changes in student quality. This paper provides evidence on the causal effect of student quality on teacher mobility, using abrupt changes in the credentials of the incoming students following an admission reform in Stockholm. I use data on teachers, students and their parents for Swedish high schools covering years 1991/1992 to 2004/2005.

The results show that an increase in student quality leads to lower teacher mobility and that the effect is increasing as the reform progresses. A 10-percentile-point increase

in incoming student credentials decreases the probability that a teacher will leave their school by up to 9 pp. I show that this effect is robust to different model specifications and I account for the fact that the change in student quality in different schools might be related to pre-existing trends in teacher mobility. The effect is very similar across all types of teachers and is found mostly for mobility between schools rather than out of the profession. It is also present only in the lower half of the student quality distribution. Furthermore, teachers seem to react mostly to direct measures of student quality (credentials) rather than to characteristics that are correlated with student quality (immigrant status, parental income and schooling, paternal cognitive and non-cognitive skills). Finally, I do not find any significant effects of changes in student quality on teacher's earnings or school hiring policies.

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Appendix

Section I:

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 irrespectively of whether it is at the same or at different schools.[29] 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 onwards) to construct the predicted measure of experience.

Teachers are then linked (using unique identifier) to population registers, which covers all individuals living in Sweden. The registers include information on gender, marital status, age, family composition (using unique family identifier), immigration history, education and income. 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 analysis is restricted to

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

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 quality of students in secondary school is measured based on their 9^{th} grade grades. 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.

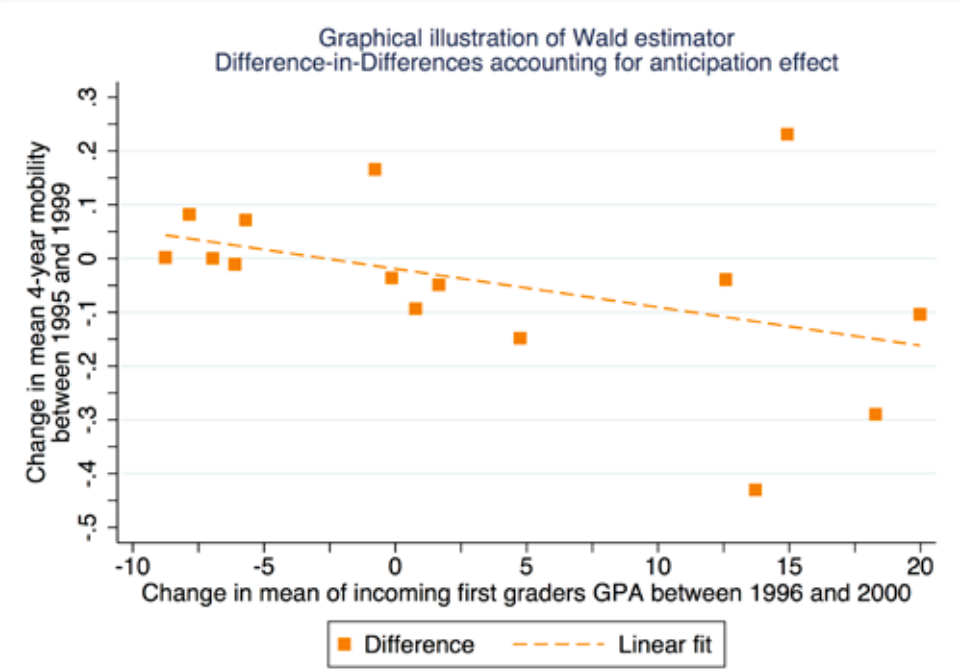
The enlistment registry covers period 1969 to 2006 and provides information on cognitive and non-cognitive assessments. All skill measures are percentile ranked by year of draft. The data is linked to teachers and students' fathers using the unique personal identifier.

Finally, having a dataset 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. This results in a sample of 15,765 teacher-year observations, which is based on 3,621 unique teachers from 29 schools. 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 1991/1992 and 2004/2005. I also drop teachers from Skärholmens Gymnasium because this school did not admit any new students in school year 1998/1999. The final sample consists of 2758 teachers, 15 unique schools and 12 226 person-years.

Section II:

Figures

Figure A 1. Difference-in-Differences. Probability of leaving school j in 4-years



Note: Values on the vertical axis represent differences in mean 4-year mobility between 1995 (pre-reform) and 1999 (post-reform). This one-year lag in outcome variable (with respect to reform timing) accounts for potential anticipation effects and quits related to rumors or announcement of the reform. Values on the horizontal axis represent changes in mean students' credentials between 2000 and 1996. Student credentials are based on first grade students who applied to high schools in the same year. Students' credentials are measured using primary school 9th grade GPA. Line represents linear regression fit. Only schools that are present in the data in each year between 1991 and 2004 are included in the analysis.

Tables

Table A 1. Descriptive statistics – comparison of Sweden and Stockholm

Variable	(1) Pre-period = 1999 Sweden	(2) Pre-period = 1999 Stockholm	(3) Post-period = 2000 Sweden	(4) Post-period = 2000 Stockholm
One-year mobility	0.13 (0.34)	0.14 (0.35)	0.11 (0.31)	0.15 (0.36)
Fraction of female teachers	0.49 (0.50)	0.55 (0.50)	0.48 (0.50)	0.53 (0.50)
Mean teacher experience	12.00 (7.27)	11.05 (7.24)	11.94 (7.61)	10.96 (7.48)
Fraction of teachers with university diploma	0.65 (0.48)	0.72 (0.45)	0.65 (0.48)	0.72 (0.45)
Fraction of teachers employed on temporary contracts	0.20 (0.40)	0.21 (0.41)	0.20 (0.40)	0.22 (0.41)
Mean yearly teacher earnings in 1000 SEK	224 (78)	217 (84)	226 (79)	226 (87)
Students' credentials	48.95 (11.12)	56.73 (16.48)	48.99 (11.58)	57.35 (18.34)
Share of immigrants	0.08 (0.06)	0.13 (0.07)	0.09 (0.06)	0.13 (0.07)
Mean yearly parental income in 1000 SEK	338 (62)	396 (117)	356 (65)	423 (128)
Mean parental education	11.99 (0.82)	13.05 (1.15)	12.16 (0.79)	13.20 (1.17)
Mean paternal draft score*	51.60 (5.86)	58.10 (7.86)	51.88 (5.78)	56.92 (8.91)
Number of teachers	20 795	1304	21 675	1364

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

Table A 2. Definitions of mobility and students' credentials variables. No 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. Column (1) defines the post-reform period dependent variables while column (2) defines post-reform treatment variables. Column (3) defines the pre-reform period dependent variables while column (4) defines pre-reform treatment variables.

Table A 3. Definitions of mobility and students' credentials variables. Anticipation effects present

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. Column (1) defines the post-reform period dependent variables while column (2) defines post-reform treatment variables. Column (3) defines the pre-reform period dependent variables while column (4) defines pre-reform treatment variables.

Table A 4. Effects of changes in students' credentials and probability of leaving school within 4-years. Wald estimator accounting for an anticipation effect

	Schools		
	1/3 upward shocked	1/3 downward shocked	Difference
Effects of 4-year changes in student quality on 4-year teacher mobility			
Treatment: Student quality - percentile ranked GPA from 9th grade in primary school. Incoming students graduating 9th grade in the same year.			
Year 2000	73.90	46.04	27.87***
	(13.23)	(10.19)	(1.02)
Year 1996	56.35	50.05	6.29***
	(13.90)	(10.51)	(1.00)
Difference	17.56***	-4.02***	**21.58*** **
	(1.10)	(0.88)	**(1.41)**
Dependent variable: Leaving school j within 4-years			
Year 1999	0.24	0.30	-0.06
	(0.43)	(0.46)	(0.04)
Year 1995	0.41	0.29	0.11***
	(0.49)	(0.46)	(0.04)
Difference	-0.16***	-0.01	**-0.17*** **
	(0.04)	(0.04)	**(0.05)**
	Wald estimate		
	-0.008* **		
	(0.003)		

Note: Shock is defined as a difference between mean students' credentials measured by primary school 9th grade GPA (only students who applied to school in the same year) in first grade of 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). Only schools that are present in the data in each year between 1991 and 2004 are included in the analysis. It results in a sample of 15 schools. Dependent variable is defined as probability of leaving school j from school year 1995/1996 to school year 1999/2000 pre-reform and probability of leaving school j from school year 1999/2000 to 2003/2004 post-reform. Independent (treatment) variable is defined as difference in mean incoming students' credentials between 1996 in pre-period and 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 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 A 5. The effects of student credentials (first grade) on probability of leaving school j: Accounting for anticipation effects and using the balanced 1991/1992-2004/2005 panel

Dependent variable: probability of leaving school j within k years	**(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	1,736	
# Schools	15	
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	1,676	
# Schools	15	
3-years	-0.006	-0.007
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)	(0.003)
# Teachers	1,667	
# Schools	15	
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	1,657	
# Schools	15	
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 the balanced 1991/1992-2004/2005 panel.

Table A 6. The effects of student credentials (first grade) on probability of leaving school j: Accounting for anticipation effects and using an unbalanced panel of schools 1995/1996-2004/2005

Dependent variable: probability of leaving school j within k years	**(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	2,182	
# 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	2,097	
# 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	2,092	
# 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	2,034	
# 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 1995/1996-2004/2005.

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