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EVALUATION

Evaluating institutional changes in education and wage policy

Martin Söderström

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Abstract

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This thesis consists of four self-contained essays.

Essay I (written with Roope Uusitalo) studies the effects of school choice on segregation. We analyse the effect of a reform in Stockholm that changed the admission system of public upper secondary schools. Before the year 2000, students were assigned to their nearest school, but from the fall of 2000 and onwards, the students can apply to any school within Stockholm City and admission decisions are based on grades only. We show that the distribution of students over schools changed dramatically as a response to extending school choice. As expected, the new admission policy increased segregation by ability. However, segregation by family background, as well as, segregation between immigrants and natives also increased significantly. Furthermore, the results show that the increase in school segregation between immigrants and natives is not explained by differences in prior achievement.

Essay II studies the effects of school choice on student achievement by analysing a reform in the Stockholm municipality that changed the admission system of public upper secondary schools. Before 2000, students had priority to the school situated closest to where they lived, but from the fall of 2000 and onwards, admission is based on grades only. Since all schools became open for application from anyone, and funding follows the students, the reform imposed strong incentives for school competition. It is shown that the reform has contributed to increase the between school variance in student outcomes. More importantly, the results indicate that students in Stockholm perform no better with increased choice availability.

Essay III evaluates the introduction of individual wage bargaining for Swedish teachers. A highly centralized bargaining structure with wage scales was in 1996 replaced by a decentralized one, where teachers now negotiate their own wages. The scales induced an increasing age profile of wages and a decreasing age profile of wage dispersion. This paper investigates whether this system was a binding constraint, by studying the earnings structure of teachers during the 1990s. The results indicate reform effects, most pronounced for compulsory school teachers; both the age profile of earnings and earnings dispersion shifted, generating smaller differences over the age distribution. Furthermore, there are no indications that the returns to observable productive teacher characteristics such as education and certification increase after the reform, rather the opposite.

Essay IV (written with Peter Fredriksson) examines the relationship between unemployment benefits and unemployment using Swedish regional data. To estimate the effect of an increase in unemployment insurance (UI) on unemployment we exploit the fact the generosity of UI varies regionally because there is a ceiling on benefits. The *actual generosity of UI* varies within region over time due to, e.g., differences in expected regional wage growth and variations in the benefit ceiling. We find fairly robust evidence suggesting that the actual generosity of UI does matter for regional unemployment. Increases in the actual replacement rate contribute to higher unemployment as suggested by theory. We also show that removing the wage cap in UI benefit receipt would reduce the dispersion of regional unemployment. This result is due to the fact that low unemployment regions tend to be high wage regions where the benefit ceiling has a greater bite. Removing the benefit ceiling thus implies that the actual generosity of UI increases more in low unemployment regions.

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Introduction

One of the key elements of economic theory is that individuals are assumed to respond to incentives. A large fraction of the empirical economic research is devoted to test theoretical predictions, and to find strategies for estimating the impact of incentives on individual outcomes. One such strategy is to use reforms and institutional changes.

This thesis consists of four self-contained essays. They are all empirical, and they all evaluate institutional changes. Essay I and II deal with school choice, Essay III studies wage-setting, and Essay IV concerns unemployment insurance. Even though the four papers share some broad methodological attributes, they are associated with two different subject fields. Therefore, this thesis is best described by first discussing school choice and then the impact of wage formation on labour market outcomes.

School choice and the Stockholm admission reform

In most countries students attend the nearest school. Thus, changing school requires that the student moves to another home. School choice is simply described as the possibility for students and/or parents to choose other alternatives than their nearest school. The economic rationale for increasing choice availability in the educational sector is that choice induces competition, and competition increases school productivity. Not only will those student benefit who exercise choice themselves, all students will gain from competition. Hoxby (2003) writes that school choice could be the rising tide that lifts all boats, arguing that the overall increase in school productivity (the tide) will overwhelm any distributional consequences (the valleys and nests on the surface). However, the opponents to school choice argue that choice merely increases segregation, creating winners and losers among students and schools (Fiske & Ladd, 2000).

In Sweden, students have traditionally been restricted to the school closest to where they live. This was changed in the early 1990s, when private schools became entitled to municipal funding (given some criteria). In 2000, the Stockholm municipality went even further. They drastically increased choice availability among public upper secondary schools, by abolishing the residence-based admission principle for a strict grade procedure. That is, grades from compulsory school became the only admission criteria. The intention was to reduce the effects of residential segregation on school segregation, and to increase efficiency in the public school sector.

In international comparison, two features of the Stockholm choice reform stand out. Most often, the aim of a choice reform is to benefit low-achieving students or troubled schools, as for example the charter or voucher initiatives in the US (see Björklund *et al* (2005) for an overview). In contrast, the Stockholm reform could actually work against low-ability students, who may be forced away from their neighbourhood school due to competition. Further, while the Stockholm reform resembles an open-enrolment reform, it has a much broader scope. Usually open-enrolment reforms do not generate strong incentives for competition since there is targeting of specific students to specific schools, or too weak financial incentives. The Stockholm reform, on the other hand, forcefully induces competition in the public school sector by allowing *all* students to apply for *all* schools, in a system where funding follows the students. Hence, the Stockholm reform is different from most other reforms by not being directed towards low-ability students, and by imposing truly strong incentives for school competition.

The first two essays of this thesis concern the Stockholm admission reform. In both papers surrounding municipalities still using the residence-based principle are used as comparisons. This comparison group approximates what the situation would have been like if the reform would not have taken place. In other words, both papers use a so-called difference-in-differences approach.

Essay I (written with Roope Uusitalo), *School Choice and Segregation: Evidence from an Admission Reform*, investigates what impact this reform had on student sorting. We first present evidence that students actually are exercising choice. For example, we show that student mobility increased; as a consequence of the reform the average commuting distance increased by 1 kilometre.

More importantly, we also show that segregation increased in all observable dimensions. As expected, segregation by ability increased, but also segregation by socio-economic background and segregation between immigrants and natives increased significantly. One of the segregation indices we use, the Duncan index, has the interpretation of how large fraction of one of the two groups that need to be redistributed over schools to achieve a situation that is associated with no segregation. In 1999, 13% of the immigrants had to be redistributed to achieve such an allocation; in 2001 this figure had increased to almost 20%. The difference-in-differences estimates

suggest that this is not reflecting a common trend, since a corresponding increase cannot be seen in the comparison group. The estimated reform effect is neither driven by the evolution of private school alternatives, nor changes in residential segregation.

The reform increased sorting by ability almost by definition. Hence, segregation for characteristics correlated with ability may also be expected to increase. Therefore, we also study segregation by family background and immigrant status for students with similar prior achievement. The results show that segregation increased between immigrants and natives, also when conditioning on ability. This means that the increase in school segregation between immigrants and natives is not explained by differences in prior achievement. This may suggest that there are information differences between groups concerning schooling alternatives and choice opportunities, or that immigrants and natives have different attitudes towards schooling.

Essay II, *School Choice and Student Achievement: Evidence from an Admission Reform,* analyses the impact of the Stockholm reform on student performance. First it is shown that the difference between schools in terms of student outcomes increased. In 1999, school attended explained 21% of the variation in student outcomes; in 2000 this figure had increased to 30%. This was expected, since Essay I showed that sorting on ability increased.

The more important finding is that Stockholm students tend to perform no better with increased choice availability. In a regression analysis, students' upper secondary grades are explained by a set of school, family, and individual background variables, including grades from compulsory schooling. When the outcome measure is based on subjects that are common for all students, the average effect is negatively insignificant with a point estimate of about half a percentile rank. When using the ordinary grade point average, and restricting the sample to include only students completing in the stipulated three years time, the Stockholm students perform about one percentile rank worse, which is significant at the 5%-level. Immigrants tend to be hurt by the reform, but also students from high-income families and students at the higher end of the ability distribution experience losses due to the reform.

One should note that the specification identifies the total effect of the school choice reform. That is, the overall effect could be due to peer effects, sorting, and productivity changes. Moreover, since the outcome measure is based on grades, it may

also be influenced by inconsistent grade setting or grade inflation. However, it seems quite unlikely that peer effects should be negative, and the sensitivity checks do neither suggest grade inflation nor relative grade setting. Maybe the most intuitive explanation is that students base their choices on imperfect information. That is, they only observe crude measures of student performance in terms of grades and test scores. They interpret this as school productivity, but in reality it does not necessarily reflect educational production, it may also reflect educational input and student characteristics. Another, maybe not that intuitive explanation, could be that students are choosing schools by other standards than academic, such as prestige. Finally, one should note that only one post-reform cohort is studied and any competition effects may not be seen at this early stage.

To summarize, the results on the Stockholm reform indicate that segregation increases, but no positive effects on student achievement can be identified, if anything achievement has been hampered by the reform. It is probably too early to draw any policy conclusions, but if these results persist some more years, one must seriously consider whether admission based on grades generate the desired outcome.

Wage formation and labour market outcomes

In the simplest supply and demand framework, individuals' labour supply is determined by maximizing utility (see for example Björklund *et al*, 2000). Labour demand is determined by firms maximizing profits. The equilibrium of the labour market is reached when labour supply equals labour demand. In order to clear the market, it is important that wages, the price on labour, are flexible. Essay III and IV deal with different aspects of this price-setting mechanism, by focusing on the incentives and labour supply decisions generated by different wage-setting regimes.

Essay III, *On the Impact of Individual Wage Bargaining in the Swedish Teachers' Labour Market*, studies differences in earnings structure generated by different wage bargaining schemes. Individual wage bargaining was introduced for Swedish teachers in 1996; until then a very rigid wage scale system had been used. The hypothesis in Essay III is that the wage scales imposed a binding constraint on the wage-setting. If this was the case, the functionality and flexibility of the teacher labour market would have been negatively affected.

It is well documented that centralized bargaining and a high degree of unionization is associated with wage compression, see Katz & Autor (1999). The consequences of too little wage dispersion and non-flexible wage-setting could be that incentives are distorted. In the context of this paper, the incentives for becoming a teacher may have been too weak. In fact, Björklund *et al*, (2005) show that there was a shortage of teacher supply in Sweden during the 1990s. It could well be the case that the rigidity of the scale system contributed to this feature.

An analysis of the earnings structure for Swedish teachers during the 1990s clearly shows that the scale system was a binding constraint. Prior to the reform, wages were determined entirely based on teacher category and experience in the profession. The scales had different entry-wages but the same wage ceiling, inducing an increasing age profile of earnings and a decreasing age profile of earnings dispersion. When removing the scales the age profiles shifted and became more similar to the ones observed in a reference group of public employees. That is, the differences between age-groups became smaller. For example, in 1996, a 50 year old compulsory teacher earned 50% more than a 25 year old, conditional on a set of standard covariates. In 2000, after half a decade of individual bargaining, the difference was only 38%. Further, the earnings dispersion for older compulsory teachers (measured as the standard deviation of log earnings, conditional on working time) increased between 1996 and 2000. For example, in 1996 earnings dispersion was 0.21 for 50 year old compulsory teachers; in 2000 it had increased to 0.25.

Furthermore, the returns to education and certification have decreased over the time period. The intention of the reform was to increase the returns to productive characteristics. However, the earning differentials between educational levels prior to the reform were far larger for teachers than in the reference group. Hence, one can argue that the returns to education have adjusted to be more similar compared to other segments of the labour market. Further, education and certification are the *observed* productive characteristics. It could well be the case that the returns to *unobserved* productive characteristics have increased.

The results in Essay III show that the wage scales were a binding constraint on the wage-setting. After the reform, the earnings structure changed and became more similar

to the ones observed in the reference group. Hence, this paper shows that the removal of the wage scales may contribute to generate a more flexible labour market for teachers.

Essay IV (written with Peter Fredriksson), *Do Unemployment Benefits Increase Unemployment? – New Evidence on an Old Question*, studies the impact of the unemployment insurance (UI) system on unemployment. Two questions are raised in this paper; do UI affect overall unemployment, and does the design of the UI contribute to the observed regional differences in unemployment?

In Sweden, as in most other countries, the unemployment benefit is linked to the foregone income of the unemployed. A UI scheme of this kind can easily be introduced into the simple labour market model described above. The essence of the reasoning is that since UI compensates individuals while unemployed, their reservation wages will increase. That is, individuals' wage claims for supplying their services on the labour market will increase when receiving unemployment benefits. An increase in the average reservation wage will, all else equal, decrease labour supply and increase equilibrium unemployment. This is what theory in general predicts, that is, that the generosity of the UI will increase unemployment. The empirical findings, however, is not that convincing; see Holmlund (1998) for an overview. The first question posted is hence a test of the qualitative prediction of theory. Further, Fredriksson (1999) has shown that regional differences in unemployment are a stable feature of the Swedish labour market. The second question concerns whether the design of the UI may have contributed to this regularity.

To answer these questions, we adapt a strategy making use of the design of the Swedish unemployment insurance system. The level of the benefit is determined by a nominal replacement rate of foregone income, up to a specified benefit ceiling. Hence, there are two components of the insurance system, the nominal replacement rate and the benefit ceiling. Given that your income is high enough, your *actual* replacement rate is below the nominal replacement rate since you are hitting the benefit ceiling.

We use regional data to exploit the variation created by the UI scheme, using the fact that the actual generosity of the UI is lower in high-wage regions than in low-wage regions. Since regional wages are surely endogenous to regional unemployment, we have to take account for this in the econometric modelling. We deal with this issue by creating predicted wages, which are plausibly exogenous to regional unemployment.

Using national rules in the UI system to estimate effects at the regional level is appealing since it should not suffer from policy endogeneity. That is, the changes in the UI are not due to local labour market changes, so there is arguably no problem with reversed causality.

We find fairly robust evidence suggesting that the actual generosity of UI does matter for regional unemployment. Increases in the actual replacement rate contribute to higher unemployment as suggested by theory. Our results show that if the actual replacement rate increases by 1 percentage point, unemployment increases by 5 percent. Furthermore, by using a policy simulation where we remove the benefit ceiling, we show that the spread of the regional unemployment distribution is reduced. This is explained by the fact that the benefit ceiling is more binding in high-wage regions, which also tend to be low-unemployment regions.

To sum up, this paper gives new evidence on the impact of unemployment benefits on unemployment, and also shows that the design of the UI scheme may contribute to regional unemployment differentials.

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Essay I

School Choice and Segregation: Evidence from an Admission Reform[1]

1. Introduction

The debate around school choice is centered on two key questions. The proponents of school choice argue that the competitive forces released by school choice increase efficiency. This increase in efficiency benefits all students, also those students not exercising choice themselves (e.g. Hoxby, 2003). The opponents argue that choice merely increases segregation. According to a typical argument, the students will be increasingly sorted by family background or ability. If peer groups are important to the student outcomes, the students who get into better schools benefit, both because school quality is higher, and because they interact with better peers. On the other hand, the students left behind suffer not only because of lower school quality but also because of the decrease in the average peer quality (e.g. Fiske & Ladd, 2000).

By now, the evidence on the efficiency effects from school choice is accumulating mainly based on various voucher programs and charter schools operating in the United States. In contrast, peer effects and, therefore, the consequences of changes in the way that students are allocated across schools have proven to be hard to estimate. Most promising attempts to evaluate peer effects have been based on small scale controlled experiments (Falk & Ichino, 2003) and on natural experiments randomly assigning individuals to peer groups (Sacerdote, 2001; Katz, Kling & Liebman, 2001).

In this paper we examine how a large scale reform that expands school choice affects sorting of students across schools. We use data from a reform that changed the admission rules to public upper secondary schools, and evaluate the effects of these changes on segregation. We focus on sorting in three dimensions: ability, family background and immigrant status. As we will demonstrate below, the reform increased sorting in all observable dimensions. We will also show that segregation between

[1] Written with Roope Uusitalo.

immigrants and natives increased more than could be expected given differences in previous grades.

Our results are, in general, similar to findings from English data by Burgess, McConnell, Propper & Wilson (2004), who report that sorting according to ability, ethnicity and income, are positively related to the feasibility of school choice, and that different admission systems produce different degrees of segregation. The key difference between their paper and the current study is that while Burgess *et al* examine the relationship between degree of choice and segregation in a cross-section, we study the effects of a reform that extended choice by removing the link between school assignment and the neighbourhood.

In some sense school choice existed in Sweden already since the beginning of the 1990s, long before the 2000 admission reform. The students applied to a certain program and could state their preferences on which school they would like to attend. However, if the schools were oversubscribed the school assignment was based on the place of residence, and those living closest to a school were given first preference. The admission system prior to the recent reform resembled intra-district open-enrolment policies in the US (Cullen, Jacob & Levitt, 2005).

The admission system in Stockholm changed fundamentally in 2000. All residence-based admission criteria were abolished and admission became based on previous grades only. The intention was to reduce the effects of residential segregation, and to open up the option of attending the most prestigious schools in downtown Stockholm for all students, irrespective of where they lived.

The Stockholm reform differs from most other choice reforms. It clearly expanded choice options for the students living further away from the most popular schools. On the other hand, abolishing all residence-based admission criteria actually decreased the choice possibilities for the low-ability students who lived close to these popular schools, but were no longer admitted due to competition from students elsewhere. Still, the reform resembles other choice reforms in the sense that school choices were no longer determined by the place of residence.

As a first step in our analysis we calculate various mobility measures to demonstrate that the reform that opened new options had an impact on the school choices. We then evaluate the effects of the reform on segregation. We analyse data

from the two years immediately before the reform, and compare various measures of segregation to the two years after the reform. To isolate the effect of the reform from other simultaneous changes, we compare the changes in segregation across schools to changes in segregation across residential areas, and we also compare the changes in Stockholm where the admission system changed, to the changes in surrounding communities where the admission system retained residence-based selection rules. In contrast to many previous papers, we also calculate standard errors for the measures of segregation, and adjust the measures so that we compare the observed level of segregation to the expected level under random allocation. This enables us to attach standard errors to our difference-in-differences estimates, and to conclude that the admission reform increased segregation in a statistically significant way.

In the next section, we will describe the school system and the changes due to the admission reform. Section 3 describes data. In section 4, we report measures examining the effects of the reform on student mobility patterns, and after that, in section 5, we discuss measurement issues related to segregation. In section 6, we report the main results on the effects of the reform on segregation, and in section 7 we make some concluding comments.

2. The Swedish school system

The Swedish school system begins with pre-school, and continues with nine years of compulsory schooling. About 90% of the student population complete the ninth grade and are eligible for upper secondary schooling. Of those, 98% do continue. With completed upper secondary schooling, the student can apply for university or post-secondary education.

All children between the ages of 7 and 16 have to attend school. Most schools are public and most children attend the school closest to home. Grades are given from the eighth grade. Grades per subject are set by the teachers, and include one of the following possible grades: Pass (P), Pass with Distinction (PD), and Pass with Special Distinction (PSD). In cases where a student fails to achieve a passing grade in a subject, no grade is given. The grading system was changed in 1995, and those leaving the ninth grade in 1998 were the first cohort with the new system where teachers shall base their assessment according to stated achievement goals. Due to the change in the grading

system, the cohort that finished the ninth grade in 1998 is the oldest cohort used in this paper. In principle, these criteria are absolute, not relative, but there is no guarantee that grading standards are equal across schools. The final certificate from the ninth grade consists of the sum of the 16 best classes, where P earns a student 10 credits, PD 15 credits and PSD 20 credits. A student who has finished the ninth grade, and has passed in Math, Swedish and English is eligible for upper secondary schooling.

All municipalities in Sweden are by law obliged to offer upper secondary schooling to all students that have completed compulsory schooling. The upper secondary school consists of different programs; all of them last for three years and provide eligibility for post-secondary education. Most municipalities do not offer all programs, and the student then has the right to attend such a program in another municipality, financed by the municipality where he or she resides.

Most upper secondary schools are public schools run by the local municipality. At the upper secondary level, there are different types of private schools. In general, private schools offer education corresponding to the public upper secondary schools, and are receiving municipal grants. There are also schools that have tuition fees and selection rules other than grades. These schools are not entitled to municipal funding. In addition, there are schools offering supplementary programs, for example, fine arts and handicraft. In 1998, there were 60 private upper secondary schools located in 35 of the 288 Swedish municipalities. The total number of students in private schools was 8 822, that is about 2.8% of the student population. In Stockholm, there were 13 private schools where 6.5% of the student population attended. The number of private schools is continually increasing. In 2001, there were already 149 private upper secondary schools in Sweden, with a total of 17 887 students.

2.1 The Stockholm admission reform

The design of the local educational system rests in the hands of the municipality. In Stockholm, the centre-right wing coalition carried through a reform of public upper secondary schooling in 2000. Up to 1999 students only applied for a program, with grades deciding admission. Students could state their preferences on which school to go to, but the ones living closest had a priority. In practice, this implied that the Local Admissions Unit first counted the number of places per program in the municipality.

They then ranked the student choices according to grades, and accepted students to a certain program. Given acceptance, the Local Admissions Unit assigned the students to the specific schools based on residence and communication opportunities.

The cohort that applied to upper secondary school in the fall of 2000 was the first cohort of students who applied to both program (including specialization) *and* school. Students were then ranked according to their grades, and those with highest grades among the applicants to each school and program were admitted. If a student was not accepted to his/her first choice, the second was considered and so forth (USK, 2002).

3. Data and descriptive statistics

Our data stem from different registers kept by Statistics Sweden. The sample is constructed using the register of completed compulsory schooling. We select all students who graduated in the spring of 1998, 1999, 2000 or 2001 from a regular compulsory school situated in the Stockholm County. The Stockholm County consists of the Stockholm City and 25 surrounding municipalities. The surrounding municipalities will from here on be labeled as the comparison group. We then follow these students, creating four cohorts of first year students in the upper secondary school.[2] The two first cohorts applied to the upper secondary school prior to the admission reform and the two latter cohorts after the reform.

For these four cohorts we have information on the students' gender, age, immigrant status, parish of residence, compulsory school attended, final grades when leaving compulsory school, upper secondary school attended, parental income, parental education and parents' immigrant status.[3]

3.1 Definition of variables

Table 1 displays descriptive statistics. Since we will use a difference-in-differences analysis, we show the figures separately for Stockholm City and the comparison group.

Grades (GPA) can take the values from 0 (worst) to 320 (best). 1st generation immigrant refers to students that are born outside Sweden, and "1st & 2nd generation immigrant" to those who have at least one parent born outside Sweden. Parental income

[2] We use the register of applicants and admissions to upper secondary schooling.
[3] Information on personal and family characteristics comes from LOUISE and SYS, two registers containing information on education, income and labour market status.

is the sum of the two parents' income. Therefore, parental income captures the effect of having parents that are working or not working, and also the effect of living with one or two parents. Parental education is an indicator for whether the student has at least one parent with a university degree or not. Private compulsory and private upper secondary schools are defined according to the status of schools where the student attended.

Table 1. Descriptive statistics, means and standard deviations.

		1998	1999	2000	2001
GPA	Stockholm	204.03	208.38	211.23	211.92
		(60.05)	(62.86)	(67.84)	(68.86)
	Comparison	201.37	202.69	202.24	205.08
		(58.63)	(62.27)	(64.83)	(65.21)
Female	Stockholm	0.486	0.488	0.489	0.493
		(0.500)	(0.500)	(0.500)	(0.500)
	Comparison	0.486	0.482	0.482	0.481
		(0.500)	(0.500)	(0.500)	(0.500)
Age	Stockholm	16.049	16.060	16.062	16.064
		(0.223)	(0.250)	(0.267)	(0.264)
	Comparison	16.046	16.050	16.050	16.045
		(0.214)	(0.222)	(0.227)	(0.215)
1st generation immigrant	Stockholm	0.138	0.159	0.147	0.158
		(0.345)	(0.366)	(0.355)	(0.365)
	Comparison	0.103	0.116	0.113	0.125
		(0.304)	(0.320)	(0.317)	(0.331)
1st & 2nd generation immigrant	Stockholm	0.332	0.348	0.341	0.347
		(0.471)	(0.476)	(0.474)	(0.476)
	Comparison	0.302	0.313	0.314	0.310
		(0.459)	(0.464)	(0.464)	(0.463)
Parental income (thousands of SEK)	Stockholm	359.9	360.2	389.7	410.4
		(352.0)	(330.6)	(445.7)	(414.6)
	Comparison	364.5	383.0	395.4	420.5
		(300.1)	(330.8)	(365.0)	(387.6)
Parental education (at least one parent with a university degree)	Stockholm	0.530	0.535	0.536	0.529
		(0.499)	(0.499)	(0.499)	(0.499)
	Comparison	0.455	0.447	0.450	0.457
		(0.498)	(0.497)	(0.498)	(0.498)
Share of students in private compulsory schools	Stockholm	0.050	0.065	0.066	0.067
		(0.219)	(0.247)	(0.248)	(0.250)
	Comparison	0.030	0.041	0.041	0.039
		(0.170)	(0.198)	(0.198)	(0.193)
Share of students in private upper secondary schools	Stockholm	0.120	0.150	0.179	0.197
		(0.325)	(0.356)	(0.383)	(0.398)
	Comparison	0.141	0.177	0.204	0.243
		(0.348)	(0.382)	(0.403)	(0.429)
# parishes	Stockholm	28	28	28	28
	Comparison	110	109	109	99
# schools	Stockholm	39	41	47	49
	Comparison	53	58	68	72
# students	Stockholm	5 566	5 826	5 945	6 187
	Comparison	10 784	10 855	11 412	11 710

From Table 1 it can be noted that the student population is rather stable in terms of background variables. Most notable exception is the share of students attending private schools, which is increasing over time. The increase in the number of schools is also driven by the opening of private schools. Another trend worth noting is that the average grades appear to be increasing over time.

In terms of characteristics of the secondary school students displayed in Table 1, the students from outside Stockholm are rather similar to the students within the city. The Stockholm students are slightly more likely to be immigrants, and have more educated parents and better grades, but the differences are small. Hence, the other 25 municipalities in the County should be well suited to be used as a comparison group for the Stockholm students.[4]

Figure A1 in Appendix A shows the 28 parishes of the Stockholm City, and the 47 upper secondary schools present in 2000. The grid indicates intervals of five kilometres. Public upper secondary schools are shown as boxes, and private schools as circles. It can be noted that private schools tend to be more concentrated in the central part. Parishes are the units we are using in measuring student mobility and residential segregation. A parish is also the smallest geographical unit available in our data. The size of the parishes varies substantially. As can be seen from the map, smaller parishes are located in the central part of the city. On average, a parish has about 200 students per cohort, and the inner city parishes are wealthier and more educated.

4. Mobility

The reform broke the link between the place of residence and school attended. A likely effect is an increase in the student mobility. In Table 2 we display measures capturing the mobility patterns in the Stockholm City.

The average commuting distance from home to school is a straightforward measure of mobility across geographical regions. We can locate each school and each student to a certain parish. Based on the map coordinates of the mid-point of each parish, we can calculate the commuting distance for each student. The measure is quite rough, for example, assigning students who go to a school in their home parish implies a

[4] Maybe the best argument for the choice of comparison group is the current discussion of creating one unified upper secondary school area of the entire County.

commuting distance of zero. Even by using this rough measure, it is clear that the commuting distances increase over time, particularly so in the reform year (Table 2). We did suspect that this increase in mobility was partially due to private schools. However, calculating the average commuting distance for students who remain in the public school system produces similar numbers: 4.1 km in 1999 and 4.7 km in 2000. Hence, the increase in student mobility does not seem to be driven by private schools.

The second row of Table 2 calculates the share of students going to school in another area than where they live. The area is defined by the home parish and all adjoining parishes. A sharp increase is observed. In 1998 the fraction of students going to school in another area than where they live was 45%, and in 2001 had increased to 63%.

Table 2. Different mobility measures.

	1998	**1999**	**2000**	**2001**
Average commuting distance (km)	4.1	4.2	4.8	5.2
Share of students going to school in another area than where they live	0.45	0.48	0.55	0.63
Market share of the three most common schools in parish	0.57	0.53	0.44	0.42

Finally, we calculate an index that aim to measure the variation in school choices among students who live in the same parish. It is the "market share" of the three largest schools attended by the students in the same parish. It is calculated by parish, and then averaged over parishes using the number of students in the parish as weights. The measure indicates that the variation in school choices among students who live in the same parish has increased. The increase is rather large. In 1998 the average market share of the three most popular schools in each parish was 57%. By 2001, it has declined by 15 percentage points, to 42%. The steepest decline coincides with the admission reform in 2000. However, part of the increase in dispersion in school choices appears to be unrelated to the reform**.** Most natural explanation is the growth of the private schools, but even this does not fully explain the trend in the dispersion. A similar analysis for public schools only displays larger levels, but very similar changes.

5. Measuring segregation

Finding that students traveled greater distances to schools, and that the dispersion of choices among students from the same parish increased, shows that the reform had its expected effect: the place of residence became less important for school choices after the reform. In what follows, we show that other factors, especially previous grades, have become more important, and that the students will be increasingly sorted or segregated across schools.

The most common measure of segregation is the dissimilarity index, often called the Duncan index according to Duncan & Duncan (1955). The dissimilarity index is defined as

$$D = \frac{1}{2}\sum_{s=1}^{J}\left|\frac{A_s}{A} - \frac{B_s}{B}\right|,$$

where J is the number of categories (e.g. schools), A is the number of individuals belonging to group A (e.g. race) and B the number of individuals belonging to group B. A_s and B_s are the corresponding numbers of individuals belonging to these groups in category s. If the groups are evenly divided across categories, so that the fraction of the group in each category equals its share in the population, the index is zero indicating that there is no segregation. The index reaches its maximum value of one when there is total segregation, so that the student body in each school consists of only a single group.

A major weakness of the dissimilarity index is that it can only measure segregation among dichotomous groupings. Because segregation indices were originally used to measure segregation between the white and minority populations, there was not much need to develop measures that could accommodate more than two groups. More recent developments in the racial patterns, as well as, applications of segregation measures to other problems, have created a need to develop measures that can be applied to multiple groups.

A simple "segregation index", that can also be used with continuous variables, and that is also probably most intuitive for the economists, is the fraction of the total variance that is due to variation across schools (R^2). It reaches the maximum value of 1 when all units within categories are equal, so that across school variance equals total variance, and it is zero when there is no variation across categories, i.e. the means of

each school are equal. A simple way of calculating this index is to regress individual outcomes on the full set of school dummies, and calculate the R^2 from this regression.

5.1 Sampling variation and random segregation

There are two important issues that have to be accounted for when interpreting the segregation indices. First, like all sample statistics also the segregation indices are influenced by sampling variability. This is particularly important when analysing changes in segregation. Second, even if the population were randomly allocated to the different categories, the allocation would not be completely even. The usual segregation indices measure the extent that the allocation deviates from evenness, instead of measuring the deviation from the random allocation. Simulation results by Carrington & Troske (1997) indicate that the most common indices of segregation indicate substantial segregation even when the population is randomly allocated across groups. The deviation from evenness is particularly strong when the categories are small, or when the minority share is small. Furthermore, the dependence of segregation indices on the size distribution of the categories causes problems when comparing the segregation indices calculated over categories of varying size.

Both these problems are important for analysing the change in the segregation after the admission reform in Stockholm. Calculating standard errors or confidence bands for the indices is, of course, necessary if we wish to claim that segregation changed in a statistically significant way due to the reform. We would also like to compare the extent of segregation across the schools to the residential segregation. Both schools and our geographical units are rather small. In 2000, the average cohort size in Stockholm schools was 135, and the average parish had 212 students. Also the size distribution of schools and parishes is different implying that the segregation indices measuring segregation across schools would get different values than indices measuring segregation across parishes even if the student population were randomly allocated both across the schools and across the geographical units. Even more importantly, the number of schools has increased over time, and this increase could change the values of the segregation indices even if no changes in segregation occurred.

In this paper we follow the suggestion of Carrington & Troske (1997) and adjust the segregation indices to measure the deviation from randomness, instead of measuring

the deviation from evenness. We, therefore, first calculate the expected values of each segregation index according to the random allocation, given the school size distribution each year. Since analytical expressions for finite samples and varying category sizes are hard to calculate, we do this by simulation. We reallocate the students randomly to schools keeping the size distribution of schools fixed. We then draw 500 replications from this reshuffled data and take the mean of these random draws as the expected value of the segregation index.

We then calculate the adjusted segregation indices by subtracting the expected value of the segregation index under random allocation from the observed segregation index. For example, the adjusted segregation index in the case of the dissimilarity index is then

$$\hat{D} = \frac{D - D^*}{(1 - D^*)},$$

where D^* is the expected segregation index under random allocation. After dividing by (1-D*), also the adjusted index ranges from 0 to 1, with 0 indicating that segregation equals expected segregation under random allocation, and 1 that there is complete segregation.[5]

In our sample the expected values of the segregation indices under random allocation appear to be only moderate in size. For example, in Stockholm schools, the expected dissimilarity index on segregation along the parental education groups is 0.057 in 1998, and the same index on segregation between natives and immigrants is 0.087 in 1998. The increase in the number of schools and the corresponding decrease in the average school size do not appear to have a major effect. The expected values of segregation indices change only slightly when the number of schools increases. A partial reason for this is that new schools are rather small and their weights on the segregation indices are rather small.

Nordström Skans & Åslund (2005) show that the same procedure that is used to calculate expected segregation under random allocation can be extended to calculating expected levels of segregation conditional on the distribution of other covariates. Also

[5] In principle, it is also possible that there is excess unevenness if the observed segregation is smaller than expected segregation under random allocation. In this case $D < D^*$, and the adjusted segregation index would get negative values.

conditional expectations are easiest to calculate by simulation. We illustrate the method in the end of section 6.

Finally, to evaluate the extent of sampling variation in the adjusted segregation indices, we calculated the bootstrap standard errors for all the segregation measures. We drew with replacement 500 replications of size N from the original sample and calculated the segregation indices for each draw. The standard deviation of these draws provided us with the standard error for each segregation index. Since we adjust each segregation index, we also need to adjust the estimates for the standard error by dividing the bootstrap estimate with (1-D*).

6. Results on segregation

We have measured segregation along three dimensions: ability, immigrant status and family background. For each dimension, we calculate measures of segregation for the Stockholm schools and the comparison schools. We then evaluate the effect of the reform by comparing the change in Stockholm City to the change in the comparison group. We also calculate measures of residential segregation, and compare the changes in school and residential segregation in Stockholm. The entire analysis is conducted for both the Duncan (dissimilarity) index and the R^2-index. In all cases, the two indices produce the same qualitative result: segregation increases. The only difference between the two indices is in the significance level.[6]

In the next three subsections we present the baseline results on the changes in segregation after the admission reform. After showing these results we will discuss the effect of private schools and the effects of schools that closed down or opened up during the period under study. Finally, we will study excess segregation conditional on ability.

6.1 Ability

We use grades when leaving regular compulsory school as a measure of ability. Since the mean and the variance of grades vary over time, we use percentile ranked grades in our calculations for the R^2-index. This does not make a big difference: both the levels and the changes in segregation indices are very similar in the original grades than when

[6] We also calculated the Theil entropy index of segregation, but the qualitative results were very similar. We have chosen to display only the Duncan index and the R^2-index because of the popularity and commonness of these measures. Results with the Theil-index are available from the authors upon request.

using percentiles. When calculating the Duncan index, we compare the highest achieving quartile to the rest but the results appear to be quite robust to other groupings. The results on segregation on ability are presented in Table 3.

Table 3. Segregation by previous grades.

			Segregation between schools	
	1998	1999	2000	2001
R^2				
Stockholm	0.304	0.399	0.537	0.583
	(0.009)	(0.010)	(0.009)	(0.008)
Comparison	0.184	0.212	0.228	0.250
	(0.006)	(0.007)	(0.007)	(0.008)
Duncan				
Stockholm	0.308	0.408	0.541	0.615
	(0.014)	(0.013)	(0.012)	(0.011)
Comparison	0.226	0.287	0.274	0.319
	(0.011)	(0.012)	(0.011)	(0.012)
			Segregation between parishes	
R^2				
Stockholm	0.044	0.059	0.057	0.084
	(0.006)	(0.006)	(0.006)	(0.007)
Comparison	0.055	0.058	0.058	0.058
	(0.005)	(0.005)	(0.005)	(0.005)
Duncan				
Stockholm	0.116	0.112	0.132	0.172
	(0.014)	(0.013)	(0.015)	(0.014)
Comparison	0.131	0.134	0.104	0.140
	(0.011)	(0.011)	(0.010)	(0.010)

	Difference-in-differences					
	Stockholm schools vs comparison schools			Stockholm schools vs Stockholm parishes		
	98/99	99/00	00/01	98/99	99/00	00/01
R^2	0.067***	0.122***	0.025	0.080***	0.140***	0.020
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)
Duncan	0.039	0.146***	0.029	0.104***	0.113***	0.034
	(0.025)	(0.024)	(0.023)	(0.027)	(0.026)	(0.026)

Note: Bootstrapped standard errors are in the parentheses. Both the indices and their standard errors are adjusted so that they measure deviation from random allocation and not from even allocation (see text). We have used the delta method to calculate standard errors for the difference-in-differences estimates. Significance level: *** = 1%, ** = 5%, and * = 10%.

According to the results, there is a sharp increase in segregation by ability in the Stockholm schools. In 1998, 30.4 percent of the variation in the previous grades could be explained by the school attended. This fraction increases to 58.3 percent by 2001. The estimates are precise with small standard errors so that the differences across years are statistically significant. Interestingly, segregation increases already before the

reform. For example, the R^2-index increases by 9.5 percentage points already between 1998 and 1999, a year before the reform. The increase in the reform year, between 1999 and 2000, are still clearly larger than increases before or after the reform.

Part of the observed increase in segregation appears to be unrelated to the reform. Most plausible explanations have to do with the changes in the residential segregation and with the increase of the fraction of students going to private schools. None of these explanations fully explains the observed patterns. First, as can be seen in Table 3, residential segregation has increased in Stockholm, but more so between 2000 and 2001. Around the reform year, between 1999 and 2000, residential segregation was rather stable in Stockholm. As we will show later, the growth of the private school sector or closing of some public schools do not explain the results either.

To isolate the reform effect from other simultaneous changes we calculated difference-in-differences estimates. We compared the changes in segregation across schools in Stockholm in the consecutive years to the corresponding changes in the comparison area. We also made a similar comparison between changes in segregation across schools and segregation across residential areas.

The results indicate a large reform effect. Between 1999 and 2000 the segregation indices increased 12–15 percentage points more in Stockholm than in the comparison group, and segregation across schools increased 11-14 percentage points more than segregation across the residential areas. These estimates are statistically significant and different measures of segregation give similar estimates.

To sum up, we conclude that ability sorting in the Stockholm schools has dramatically increased as a result of the reform. We find it puzzling that segregation increases already before the reform and return to the possible explanations below.

6.2 Immigrant status

Table 4 displays the segregation indices between natives and immigrants. In the table we present results where we count both the first and the second generation immigrants as immigrants. The results indicate that segregation between natives and immigrants increased sharply after the reform in the Stockholm schools. According to the Duncan index, 19.6% of the immigrant students in the Stockholm schools in 2001 would have to be moved to another school to achieve a distribution that corresponds to a random

allocation. The comparable number in 1999 was 13.0%. The point estimates are significantly different at the five percent level. During these years there was a slight upward trend also in residential segregation. The Duncan index calculated across parishes increased from 28.2% to 30.9%, though the increase was not statistically significant. There is no clear pattern in the comparison group.

Table 4. Segregation by immigrant status.

		Segregation between schools		
	1998	1999	2000	2001
R^2				
Stockholm	0.053	0.051	0.067	0.087
	(0.007)	(0.006)	(0.007)	(0.007)
Comparison	0.067	0.079	0.066	0.086
	(0.005)	(0.006)	(0.005)	(0.006)
Duncan				
Stockholm	0.140	0.130	0.162	0.196
	(0.014)	(0.012)	(0.013)	(0.012)
Comparison	0.168	0.181	0.172	0.202
	(0.010)	(0.010)	(0.010)	(0.010)
		Segregation between parishes		
R^2				
Stockholm	0.134	0.145	0.151	0.162
	(0.010)	(0.009)	(0.009)	(0.010)
Comparison	0.111	0.122	0.113	0.127
	(0.006)	(0.006)	(0.006)	(0.006)
Duncan				
Stockholm	0.265	0.282	0.287	0.309
	(0.014)	(0.013)	(0.013)	(0.013)
Comparison	0.235	0.236	0.226	0.247
	(0.011)	(0.010)	(0.010)	(0.009)

			Difference-in-differences			
	Stockholm schools vs comparison schools			Stockholm schools vs Stockholm parishes		
	98/99	99/00	00/01	98/99	99/00	00/01
R^2	-0.013	0.028**	0.001	-0.012	0.010	0.009
	(0.012)	(0.012)	(0.012)	(0.016)	(0.016)	(0.017)
Duncan	-0.023	0.042*	0.003	-0.027	0.026	0.012
	(0.023)	(0.023)	(0.023)	(0.027)	(0.026)	(0.025)

Note: Significance level: *** = 1%, ** = 5%, and * = 10%. Other notes under Table 3.

The difference-in-differences estimates support the view that the admission reform had an effect on segregation. Between 1999 and 2000 the R^2-index increased by 2.8% more in the Stockholm schools than in the comparison schools. The increase in the Stockholm schools was also larger than in the Stockholm parishes during the reform year, but the difference was not statistically significant. Overall there does not seem to be any tight

relationship between segregation across schools and residential areas in Stockholm. For example, between 1998 and 1999 residential segregation increased, while school segregation actually decreased.

When restricting the definition of immigrants to the "1st generation", the segregation levels are lower, but the changes are essentially similar. We also note that the segregation between schools did not change much prior to the reform, but that there is an increase in Stockholm and a decrease in the comparison group after the reform. We have tried different definitions of the immigrant status, such as born outside the Nordic countries, or born outside the OECD countries. These different definitions do not affect the results.

It is worth pointing out that the difference between residential segregation and school segregation in Stockholm decreased after the reform, mainly because segregation across schools increased.

6.3 Family background

We have measured family background with two variables, parents' education and parents' income, but report in Table 5 only the results on parents' education. Also here segregation across schools clearly increased. The R^2-index increases from 10.4% in 1998 to 13.9% in 2001. The point estimates are significantly different at the five percent level. In the comparison group, the segregation is fairly constant; the R^2-index is 10.0% in 1998 and 10.1% in 2001. Also residential segregation is stable in both groups.

The difference-in-differences results indicate a clear reform effect. During the reform year, segregation increased by 2.8 percentage points more in the Stockholm schools than in the comparison schools, when measured with the R^2-index. The Stockholm schools also became significantly more segregated than the Stockholm parishes.

Concerning parental income (not reported in the table), the results were rather similar. As with grades, we percentile ranked the parental income for the R^2-index. There was a sharp increase in school segregation in Stockholm that could not be seen in the comparison group. Residential segregation remained stable over the years in both groups. In the difference-in-differences analysis, Stockholm schools become

significantly more segregated than comparison schools and Stockholm parishes in the reform year. The differences in other years are not statistically significant.[7]

Table 5. Segregation by parental education.

	Segregation between schools			
	1998	1999	2000	2001
R^2				
Stockholm	0.104	0.116	0.138	0.139
	(0.008)	(0.008)	(0.008)	(0.008)
Comparison	0.100	0.108	0.102	0.101
	(0.005)	(0.006)	(0.006)	(0.006)
Duncan				
Stockholm	0.222	0.245	0.275	0.291
	(0.013)	(0.013)	(0.012)	(0.011)
Comparison	0.215	0.233	0.231	0.225
	(0.010)	(0.010)	(0.010)	(0.010)
	Segregation between parishes			
R^2				
Stockholm	0.088	0.089	0.081	0.086
	(0.007)	(0.008)	(0.007)	(0.007)
Comparison	0.092	0.088	0.080	0.080
	(0.006)	(0.005)	(0.005)	(0.006)
Duncan				
Stockholm	0.224	0.216	0.204	0.214
	(0.013)	(0.013)	(0.013)	(0.012)
Comparison	0.201	0.199	0.187	0.191
	(0.009)	(0.010)	(0.009)	(0.009)

	Difference-in-differences					
	Stockholm schools vs comparison schools			Stockholm schools vs Stockholm parishes		
	98/99	99/00	00/01	98/99	99/00	00/01
R^2	0.004	0.028**	0.003	0.011	0.030**	-0.004
	(0.014)	(0.014)	(0.014)	(0.016)	(0.015)	(0.015)
Duncan	0.006	0.032	0.022	0.030	0.042*	0.005
	(0.023)	(0.022)	(0.021)	(0.025)	(0.025)	(0.024)

Note: Significance level: *** = 1%, ** = 5%, and * = 10%. Other notes under Table 3.

According to all indices, the school segregation and residential segregation on family background were at the same level in 1998. After the reform the school segregation in Stockholm sharply increased while residential segregation remained stable. We find the evidence clear; sorting on family background increased with the expansion of school choice.

[7] For example, the increase in the Duncan index (which compares the top quartile of income to the rest) for the Stockholm schools was 5.5% larger than comparison schools, and 6.9% larger than Stockholm parishes. Both differences were statistically significant.

6.4 Possible explanations for the observed patterns

In addition to the admission reform, there were two other important developments that might have had an impact on segregation. First, the fraction of the Stockholm students in the private schools increased from 12 to 20 percent between 1998 and 2001. Second, the number of schools increased from 39 to 49, due to new private schools opening up. In fact, the number of new schools was even larger, because seven schools closed down between 1998 and 2001. Both the increase in the fraction of private school students, and the changes in the school structure may have an effect on student sorting.

To isolate the effect of the admission reform from the effects of changes in the fraction of students in the private schools, we repeated all calculations reported in tables 3 to 5 using only the public school students. We also repeated the calculation using only schools that existed over the whole four-year period.[8]

To our surprise neither the increase in the private schools nor the closing down or opening up schools had a major effect on the results. For example, segregation along ability, measured by the R^2-index, in the Stockholm public schools was 22.9% in 1998, 33.5% in 1999, 51.5% in 2000 and 57.5% in 2001. Comparing these numbers to the corresponding index in the first row of Table 3, reveals that the level of segregation is lower when only public schools are included, but that changes are very similar. Also in the public schools, there is a large increase in the reform year. Concerning segregation along the immigrant status, it increased slightly more in the public schools than in all schools. This makes the difference-in-differences estimates comparing Stockholm schools to Stockholm parishes in Table 4 statistically significant in the reform year. Focusing on surviving schools does not make a large difference in segregation along any dimensions either. If anything, the reform effect stands out more clearly.

6.5 Excess segregation

The final issue that we examined was to what extent segregation along family background and immigrant status are driven by sorting by ability. A grade-based admission system can be expected to increase sorting by ability, and hence any other characteristics that happen to be correlated with ability. To examine this issue, we

[8] Full results on all indices calculated over the sub-sample of the public schools, and schools that existed over the whole four-year period, are available from the authors upon request.

calculated segregation indices that measure segregation in excess to what one should expect given the grade distribution across schools.

To calculate excess segregation between immigrants and natives, we first split the data into sixteen twenty-point intervals according to compulsory school grades. We then calculated the fraction of immigrants in each interval. These fractions can be treated as non-parametric conditional expectations of immigrant status given the observed grade. We then generated random numbers from a uniform (0,1) distribution and assigned a student an immigrant status if this random number was less than the fraction immigrants in his/her grade interval. We calculated segregation indices from this randomized data. Repeating this procedure 500 times and taking an average of the segregation indices from each draw produces an estimate for the conditional expectation of the segregation index. Excess segregation according to family background was calculated similarly.

In Table 6 we adjust the segregation indices by deducting the conditional expectations from the observed indices. We call these measures excess segregation since they measure how much more segregated the schools are than what one could expect given the sorting of students according to ability. For the ease of comparison we also reproduce the earlier unconditional estimates from Tables 4 and 5.

Table 6. Excess segregation conditional on previous grades.

		Immigrant status		
	1998	1999	2000	2001
Stockholm				
R^2	0.053	0.051	0.067	0.087
	(0.007)	(0.006)	(0.007)	(0.007)
R^2 (excess segregation)	0.028	0.022	0.035	0.055
	(0.006)	(0.006)	(0.007)	(0.008)
Duncan	0.140	0.130	0.162	0.196
	(0.014)	(0.012)	(0.013)	(0.012)
Duncan (excess segregation)	0.093	0.064	0.104	0.127
	(0.014)	(0.014)	(0.014)	(0.014)
		Parental education		
	1998	1999	2000	2001
Stockholm				
R^2	0.104	0.116	0.138	0.139
	(0.008)	(0.008)	(0.008)	(0.008)
R^2 (excess segregation)	0.054	0.061	0.062	0.055
	(0.008)	(0.008)	(0.010)	(0.009)
Duncan	0.222	0.245	0.275	0.291
	(0.013)	(0.013)	(0.012)	(0.011)
Duncan (excess segregation)	0.117	0.122	0.135	0.118
	(0.014)	(0.014)	(0.015)	(0.014)

For both immigrant status and the parents' education conditioning on ability decreased the segregation measures. Roughly half of the measured segregation according to both family background and immigrant status can be explained by sorting according to ability. Increased sorting by ability also explains completely the increase in segregation by parents' education after the reform. There is very little change in the excess segregation by family background over time. However, the pattern in segregation along the immigrant status remained similar to that reported in Table 4. Even conditional on ability there was a strong increase in the segregation index after 1999.

7. Conclusions

A key motivation behind the admission reform in Stockholm was that the city is geographically quite segregated. There are large differences in the income and education levels across the residential areas. The immigrants tend to be heavily concentrated to certain neighbourhoods. As a result of residence-based admission criteria, also the schools are quite segregated. The system was considered unjust because those from less advantaged neighbourhoods had little chance of attending the best schools.

The admission reform in 2000 abolished all residence-based admission rules. This benefited those with highest grades as new options became available and school district borders no longer limited their school choices. The losers were those who no longer were accepted to their closest school due to competition from students living further away.

As expected, grade-based admission system increased sorting of students to schools according to their ability. Less expected was that a reform, that was supposed to undo the effects of residential segregation on school segregation, actually increased segregation along all other observable dimensions, particularly along the ethnic and socio-economic lines. All these changes were reasonably large and statistically significant. The increase in segregation by family background was caused by the increased sorting by ability. However, the segregation between immigrants and natives increased more than one would expect as a result of increased sorting by ability. The reason could be that immigrants do not have complete information on choice availability and school opportunities. Another explanation, perhaps not that intuitive, is

that attitudes towards schooling and school preferences are different between natives and immigrants.

The change in the admission system is only one of the important changes that affect segregation of students. Segregation across residential areas has also increased. The increase in the private school sector also increases choice options and might lead into an increase in segregation across schools. However, the quantitative importance of these two changes appears to be minor compared to the effects of the admission reform. This should not be very surprising. Changes in residential segregation are slow compared to sudden changes caused by the change in the admission system. Even though the private school sector has grown rapidly it still represents a rather small fraction of students. For most students, the choice between different public schools is far more important than the choice between the public and the private schools.

Acknowledgements

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Appendix A: Stockholm map

Figure A1 shows the Stockholm City divided into its 28 parishes. The 47 upper secondary schools present in 2000 are located on the map. Public schools are marked with boxes, and private schools with circles. The grid indicates intervals of 5 kilometres.

One can note that the parishes differ in size, where the smaller parishes are located in the city centre. Schools, and particularly the private ones, are concentrated to the central part of the municipality.

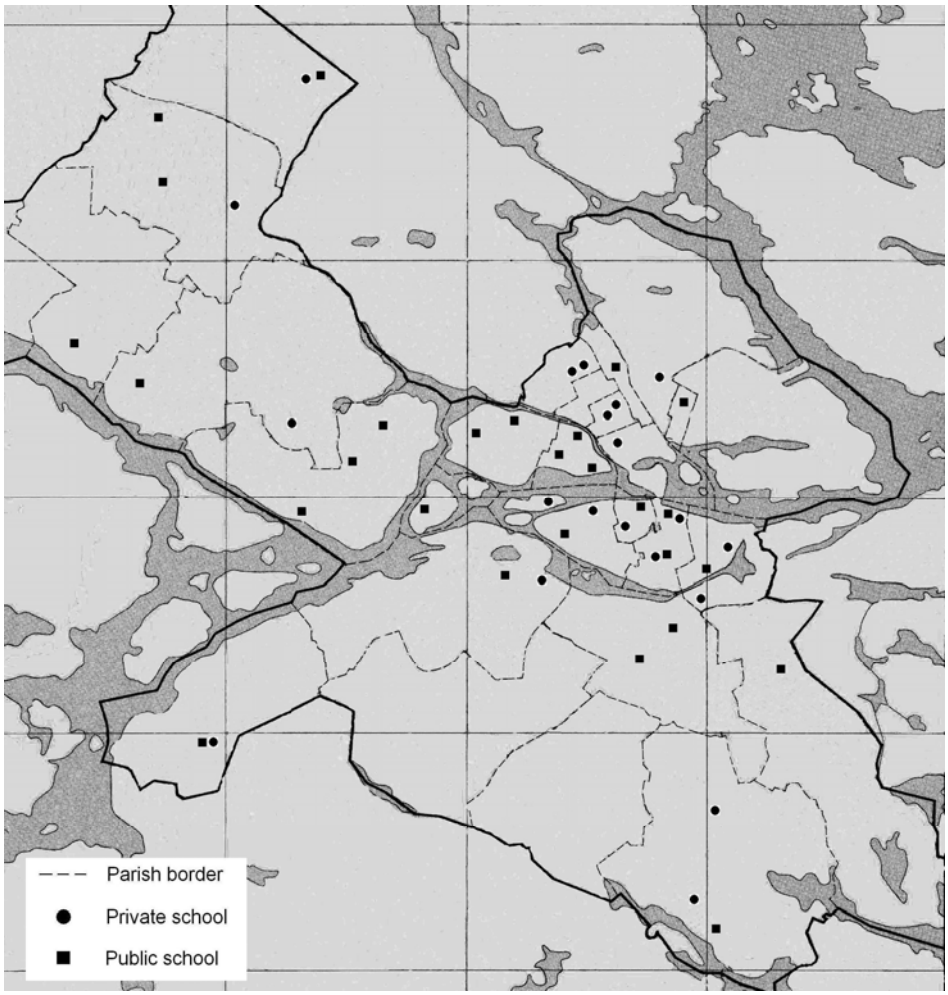

Figure A1. Stockholm City divided into parishes. The distribution of public and private schools corresponds to the year 2000.

Essay II

School Choice and Student Achievement: Evidence from an Admission Reform

1. Introduction

One of the most discussed questions in the field of economics of education concerns the impact of school choice on student performance. The argument for increasing choice availability relies on the idea that students (or parents) prefer better schools. Then, if there are financial incentives for the schools to attract students, competition will increase productivity since it will be profitable to be a better school. This overall increase in productivity is argued to benefit all students, not only those who exercise choice themselves. The opponents to school choice, however, argue that choice only has distributional consequences, creating winners and losers among students and schools.[1]

The empirical evidence on school choice is mostly focused on the availability of private alternatives in the schooling sector. This literature spans large-scale nation-wide reforms to smaller experiments; generally reporting zero or small positive effects of private school competition on student achievement.[2] The evidence on choice availability among public schools is mainly concerned with different reforms in the US. The literature on charter initiatives has produced some reliable evidence by controlling for the selection issues related to these types of reforms.[3] Open-enrolment reforms, on the other hand, have usually failed in creating competitive environments. The reason for this apparent failure is that the choice process often is restricted by targeting specific students to specific schools, or that there are no financial incentives for schools to attract students.[4] This paper aims at shedding new light to the question of public school choice and student achievement by analysing an open-enrolment reform that generated truly strong incentives for competition.

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[1] For a conceptual discussion of school choice, see Hoxby (2003b), and Fiske & Ladd (2000).
[2] A recent review of the literature on school choice can be found in Björklund *et al* (2005).
[3] See for example Hoxby (2003a), Hanushek, Kain, Rivkin & Branch (2005), and Bettinger (2005).
[4] Cullen, Jacob & Levitt (2005) analyse school choice in an open-enrolment setting. They study 9th graders in Chicago who are guaranteed a slot in a pre-assigned neighbourhood high school, but have the possibility to opt for other public schools in the district. The results suggest that there are no academic gains from opting out of neighbourhood schools.

In 2000, the Stockholm municipality changed the admission system of public upper secondary schools. A residence-based principle was abolished for a strict grade procedure. Unlike most reforms, which are designed to help low-ability students, this reform could actually force low-ability students away from their neighbourhood school due to competition. Hence, this reform takes school choice further than most other reforms since *all* students can apply to *all* schools. Furthermore, funding follows the students, creating financial incentives for the schools to attract students. The change in admission procedure has been shown to drastically affect the allocation of students across schools within the city. Söderström & Uusitalo (2005) describe how this reform increased student mobility and segregation, measured along the lines of ability, immigrant status and family background.[5]

To answer the question on school choice and student achievement, this paper utilizes a difference-in-differences strategy. The change in the student performance in Stockholm is compared to students in the surrounding municipalities, where the residence-based admission procedure is still in use. Heterogeneous treatment effects along the lines of gender, ability, immigrant status and socio-economic background is studied, and the impact of private schools is examined. In addition to affecting average academic performance, the reform might also affect the probability of completing schooling; robustness checks are therefore performed conditional on completion. Throughout the paper, school performance is measured by school grades. A priori this is not ideal. Therefore, considerable effort is put into examining whether the results are confounded by issues such as grade inflation and relative grade setting. These sensitivity checks suggest that the results are not distorted by having grades rather than, e.g. test scores, as a measure of student performance.

The results show that the between school variance in student performance has increased in Stockholm after the reform. This result was expected since the reform produced an increased sorting by ability. More importantly, it is shown that the students in Stockholm perform no better compared to the students in the surrounding municipalities after the reform.

[5] Maybe the most interesting result in Söderström & Uusitalo (2005) is that segregation by immigrant status increases also conditional on ability. This suggests that the opportunities of the reform were not conceived by all immigrants, or that immigrants have different attitudes towards schooling.

In fact, the point estimate suggests a decrease of about half a percentile rank on average; this estimate is insignificant however. The interaction estimates suggest some interesting and some surprising results. Immigrants were hurt by the reform. As a consequence of the reform, the school performance of immigrants was reduced by close to two percentile ranks. Given the nature of the reform and the results in Söderström and Uusitalo (2005), this is not surprising, but still of interest. More surprising is that also children from high-income families were hurt by the reform. The performance of these children decreased with a magnitude similar to that of immigrant children. Estimates by ability show a similar pattern, losses due to the reform seems concentrated to the higher end of the ability distribution.

The robustness checks suggest that the difference between groups at least partly can be explained by differences in completion probabilities. High-ability students in Stockholm tend to complete to a lesser extent after the reform, while low-ability students tend to complete to a greater extent. The robustness checks also show that the negative reform effect is larger among those subjects not being common between students.

What can explain these outcomes, that is, why does the reform have a negative impact on students in the higher end of the ability distribution? One should remember that the estimated effect is a catch-all, i.e. is everything that is attributable to the reform. Two potential explanations come to mind. First, it could be that the outcome measure reflects relative grade setting, although an indirect test does not support this conjecture. Second, and maybe the most appealing explanation, could be that students misinterpret school productivities when they choose schools. That is, they only observe raw measures of student performance, such as grades and test scores, but they do not know whether they reflect educational production or student characteristics. Finally, one should keep in mind that the analysis only uses one post-reform cohort, and it may be too early to spot any overall gains of competition.

The outline of the paper is as follows. Section 2 deals with the Swedish school system, and the admission reform is described in Section 3. Section 4 concerns the data, Section 5 presents the results, and Section 6 concludes.

2. The Swedish school system

The Swedish schools are governed by the municipalities within the guidelines and criteria set by the National Agency for Education. Each municipality is given funding from the federal budget, and is then free to determine how much to spend in the educational sector. At the municipal level, a specific amount of money is attached to each student, which follows the student to whatever school he or she attends.

Sweden has nine years of compulsory schooling, between the ages of 7 and 16. Almost all students that complete 9th grade continue to upper secondary schooling, which consists of three-year programs. The programs can be of three different types: national programs (all giving eligibility to post-secondary education), individual programs (intended to prepare the students for national programs later on) or special programs (for example handicraft or art).[6]

These programs were introduced in 1994 along with goal-oriented grades. The programs consist of courses, ranging from 50 to 200 points depending on the extent of the course, and in total a program consists of 2 500 points.[7] Each course is given one of four possible grades; Fail, Pass, Pass with distinction and Pass with special distinction, which earn the student 0, 10, 15 or 20 credits respectively. The final grade point average (GPA) is calculated as a weighted average over courses:

$$GPA = \left(\sum_{i=1}^{n} P_i * C_i \right) / \left(\sum_{i=1}^{n} P_i \right)$$

where P_i denotes points, C_i credits, and subscript i indicates course. Thus, more weight is given to longer courses, and the GPA is ranging from 0 (worst) to 20 (best). The subjects differ between programs, and therefore the GPA will be based on different subjects in different programs. However, there are eight subjects that all students have to take irrespective of program attended, the so called "core subjects".[8] The average of the core subjects (GPA^{core}) is the main outcome measure used in this paper.

[6] There were 16 national programs available until and including 1999, and 17 programs from 2000 and onwards.
[7] This holds for 2000. Before 2000, courses ranged 20-200 points with a total of 2 150 or 2 370.
[8] The core subjects are Mathematics, Swedish, English, Sport, Religion, Art, Natural Science, and Social Science.

All grades are set by the teacher, and should be given according to subject-specific assessment criteria determined by the National Agency for Education.[9] As further guidelines there are national tests in Swedish, Math and English. There is no supervision of the teacher grade setting, and hence there is no guarantee that grading standards are equal across teachers.

Students who began upper secondary schooling prior to 1994 were graded in a norm-based reference system. It is documented that the average grade has been increasing in upper secondary schools in Sweden since the introduction of the goal-oriented grade system (Wikström, 2005). It raises the question if this increase in grades is reflected in higher knowledge, or if it is a result of grade inflation. Wikström & Wikström (2005) study grade inflation and school competition by comparing grades from upper secondary school with performance on the SweSAT national test.[10] They do not find that intra-municipal school competition does induce grade inflation among public schools; however, their results suggest that private schools inflate grades.

Private schools have been very rare in Sweden, but after the school reform in the beginning of the 1990s (which made private schools entitled to municipal funding), the number of private schools in Sweden has been increasing, particularly in the metropolitan areas. Those few private schools that are financed by tuition fees and are using selection rules are not entitled to municipal funding.

3. The Stockholm admission reform

Since the responsibility for the educational system is a case for the municipality, they can freely choose admission procedure to upper secondary schools. The municipality of Stockholm changed this procedure in 2000.[11] The students who began upper secondary schooling in the fall of 2000 were the first cohort in a system where admission is based on grades only. Students apply for a specific program in a specific school. If they are not accepted to their first choice, a second is considered, and so forth. Note that there can only be one first choice, not one first choice per school. Prior to 2000, students applied for a program only. Given acceptance (based on grades), students were

[9] The assessment criteria are specified in broad and general terms for all subjects.
[10] When applying to university there exist two main routes, grades from upper secondary school or the SweSAT test score, a test given twice a year throughout the country open to anyone.
[11] The decision was taken by the centre-right wing coalition of Stockholm on October 18, 1999.

distributed to the school situated closest to where they lived.[12] It should be noted that the reform did not change the way in which students from outside Stockholm could apply for schools within Stockholm.[13]

The Stockholm admission reform fits the criteria of being considered a school choice reform well. The funding follows the student, making the schools financially dependent on how many students they attract. Furthermore, schools have the possibility to take on more students, or lose students. However, in contrast to most other choice reforms - for example voucher initiatives in the US, which often is directed to increase choice availability among low ability students - this reform could actually work against low-ability students if they were no longer admitted to their neighbourhood school.

The argument in favour of school choice is that extending choice increases competition. Hence, the next question is if increased competition can be observed. This is not an easy question to answer. There are two main ways in estimating competition, none of them well-suited for this case. The Herfindahl Index (the sum of the squares of per-unit enrolments over total enrolments) is not appropriate since all schools were not available to all students prior to the reform. The "private school share" is not satisfactory either since it is measuring an aspect of competition that is not capturing the reform effect.

Söderström & Uusitalo (2005) conclude that the mobility of students in Stockholm increased as a response to the reform, and so is also the sorting of students over schools. For example, the average commuting distance for a student increased from 4.2 km in 1999 to 4.8 km in 2000, and the segregation by previous grades increased from 40% to 54% over the same years. This is clear evidence that students are exercising choice, leading to increased school competition. There is also considerable evidence in how schools nowadays are promoting themselves.[14] But perhaps the best argument for increased competition is the fact that some public schools are struggling

[12] The residence-based principle is best described with an example. The Local Admission Unit counted the number of slots in total for each program in the public schools, for example 300 for the program of Natural Science. The students only applied for a program, and they were ranked according to grades. To the program in Natural Science, the 300 with best grades were accepted. Given acceptance, the Local Admission Unit studied every single student, and distributed them to schools by address, minimizing travelling distance to school.

[13] A student from outside Stockholm can apply for a national program within Stockholm if that program is not given in the municipality where the student resides, and if the home-municipality does not have a formal agreement with another municipality.

[14] For example, there is an annual exhibition for ninth-graders, and the largest newspaper in Sweden (Dagens Nyheter) has a supplement of school information at the time of application.

for survival. In recent years, Bromma and Brännkyrka Gymnasium have had severe problems in attracting students, and it was recently decided that Fogelströmska Gymnasiet had to be shut down. Thus, arguably school competition has increased in the Stockholm municipality after the choice reform.

4. Data

The data come from Statistics Sweden. It is constructed by taking all students registered the first semester at an upper secondary school situated in the Stockholm County.[15] Stockholm County consists of 26 municipalities. The Stockholm municipality (hereafter simply denoted Stockholm) is the treatment group and the other 25 municipalities constitute the potential Comparison group. Data are used for six consecutive years, from 1995 to 2000, and give information on which school and program the students are registered at. Note that data only include one post-reform cohort. To this sample, personal, family, compulsory school and upper secondary school information are matched to the students.[16]

The sample is restricted in three ways. First, only those students attending national programs are used. The reason is that individual programs are preparing the student for taking national programs later on, and the special programs are diverse and not easily comparable. In both treatment and comparison groups, about three quarters of the students register at national programs. Second, since grades are only available until 2003, grades are only used for those students completing in the stipulated three years time. Third, a few individuals without information on compulsory school grades are excluded.[17]

Table 1 reports background characteristics for Stockholm and the Comparison group for each year. The students are sorted into the two groups depending on where the upper secondary school they are registered at is located. Immigrant status is defined as those who are born outside Sweden, or has at least one parent who is born outside Sweden. Parental education is an indicator equalling unity if the student has at least one

[15] Data are defined using the register of applicants and admissions to upper secondary school.
[16] Several registers from Statistics Sweden are used. Family information and personal characteristics are given by LOUISE (Longitudinal database on education, income and employment) and SYS (Information about labour market status). The registers of completed elementary schooling and completed upper secondary schooling give additional educational information and grades.
[17] About 1% of the sample is missing information on compulsory school grades.

parent with a university degree. Parental income (in thousands of SEK) is the sum of the parents' income, thereby also capturing the effect of having one or two (working) parents. The compulsory school grade point average, GPA, is percentile ranked over both groups.[18] Compulsory school GPA^{SME} corresponds to the average of the percentile ranked grades in Swedish, Math, and English. The table also includes information on the share of students in private schools, and the number of schools and students.

As can be seen in Table 1, the composition of students in 1995 is fairly similar in the two groups. However, the composition in Stockholm appears to change somewhat over time, that is, it appears to be a trend in Stockholm that cannot be seen in the Comparison group. For example, the share of females in Stockholm increases from 47.7% in 1997, to 51.4% in 2000, while it remains fairly constant in the Comparison group. The share of students from families with an academic background increases from 48.9% to 52.5% in Stockholm over the same period. Stockholm students on average become older, and the average parental income increases by more than in the Comparison group. The share of immigrants, on the other hand, is similar between the two groups, slightly higher than 30% with similar time pattern.

The explanation for the observed pattern seems to be found in the share of students attending private schools. Between 1997 and 2000, this share increased from 5.1% to 20.7% in Stockholm. The numbers of school over the same time period increased from 30 to 41, exclusively due to new private schools. Furthermore, by studying compulsory school grade there is an increase in Stockholm from 1997 to 2000, where the GPA increases from 51.4 to 55.4. Hence, it appears to be an inflow to Stockholm of high ability students attending private schools.[19] It should be noted that the bulk of the increase in the share of private school students in Stockholm happens between 1997 and 1999. For the reform year (between 1999 and 2000), the private school share increases by about the same amount in the Comparison group as in Stockholm.

Data also include information (not shown in tables) on compulsory school attended, residential information and program attended.

[18] Note that students leaving compulsory school prior to 1998 were graded on a norm-based scale from 1-5, and from 1998 and onwards in the goal-oriented system.
[19] The average GPA for public school students increased from 50.4 in 1997, to 52.8 in 2000.

Table 1. Descriptive statistics, standard deviations below means.

		1995	1996	1997	1998	1999	2000
Female	Stockholm	0.488	0.490	0.477	0.502	0.501	0.514
		(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
	Comparison	0.469	0.474	0.468	0.479	0.475	0.462
		(0.494)	(0.499)	(0.499)	(0.500)	(0.499)	(0.499)
Immigrants	Stockholm	0.309	0.309	0.315	0.315	0.369	0.370
		(0.462)	(0.462)	(0.464)	(0.465)	(0.482)	(0.483)
	Comparison	0.302	0.325	0.325	0.327	0.364	0.363
		(0.459)	(0.468)	(0.468)	(0.469)	(0.481)	(0.481)
Age	Stockholm	16.27	16.27	16.28	16.30	16.26	16.32
		(0.69)	(0.74)	(0.75)	(0.90)	(0.80)	(1.03)
	Comparison	16.20	16.19	16.20	16.21	16.22	16.21
		(0.52)	(0.49)	(0.51)	(0.53)	(0.52)	(0.51)
Parental education	Stockholm	0.469	0.479	0.489	0.509	0.519	0.525
		(0.499)	(0.500)	(0.500)	(0.500)	(0.500)	(0.499)
	Comparison	0.415	0.424	0.416	0.442	0.422	0.428
		(0.493)	(0.494)	(0.493)	(0.497)	(0.494)	(0.495)
Parental income	Stockholm	312.5	327.2	342.7	365.5	388.6	408.9
		(292.0)	(284.1)	(308.5)	(342.7)	(338.3)	(411.0)
	Comparison	320.0	337.0	343.5	356.1	366.5	381.1
		(240.7)	(252.7)	(302.2)	(302.0)	(308.0)	(362.9)
Compulsory school GPA	Stockholm	51.10	50.89	51.43	52.37	55.75	55.39
		(29.36)	(29.63)	(29.65)	(28.82)	(28.68)	(28.96)
	Comparison	50.13	50.31	50.01	48.65	46.29	46.69
		(28.47)	(28.33)	(28.30)	(28.99)	(28.62)	(28.16)
Compulsory school GPA^{SME}	Stockholm	51.31	51.36	51.76	52.02	54.32	54.26
		(22.99)	(23.43)	(23.60)	(22.32)	(22.41)	(22.56)
	Comparison	49.90	49.98	49.62	49.27	47.99	47.98
		(22.14)	(22.35)	(22.58)	(21.90)	(21.37)	(21.37)
Share of students in private schools	Stockholm	0.055	0.062	0.051	0.126	0.188	0.207
		(0.228)	(0.242)	(0.219)	(0.332)	(0.391)	(0.405)
	Comparison	0.068	0.065	0.073	0.077	0.095	0.121
		(0.252)	(0.247)	(0.260)	(0.267)	(0.294)	(0.326)
# schools	Stockholm	30	32	30	33	34	41
	Comparison	44	46	48	47	50	56
# observations	Stockholm	6 754	6 823	6 683	5 989	6 245	6 556
	Comparison	10 883	11 439	10 928	9 975	9 477	9 686

Note: Parental education is an indicator equalling unity if the student has at least one parent with a university degree. Parental income is the sum of the parents' income measured in thousands of SEK. Compulsory school GPA is percentile ranked over both groups per year. Compulsory GPA^{SME} is the average of the percentile ranked grades in Swedish, English, and Math. For further information, see text.

Table 2 displays information on student performance at the upper secondary level. Grades from upper secondary school are shown as a weighted average over all grades (GPA), and over core subjects (GPA^{core}). The grade measures are for each year percentile ranked over the full sample. Ranked grades are used throughout this study to minimize potential problems with grade inflation. Educational performance can also be measured by whether a student manages to complete education in the stipulated time.

Upper secondary schooling is intended to last for three years, and a considerable fraction of students fail to get a final grade within these three years. The share of students not completing in the stipulated time corresponds to these students.

Table 2. Grades, core grades, and the share of students completing in three years.

		1995	1996	1997	1998	1999	2000
GPA	Stockholm	52.86	53.03	54.01	53.48	54.42	54.32
		(29.51)	(29.81)	(29.45)	(29.11)	(28.99)	(29.14)
	Comparison	48.34	48.30	47.84	48.06	46.94	47.20
		(28.35)	(28.10)	(28.27)	(28.47)	(28.33)	(28.27)
GPA^{core}	Stockholm	53.88	53.93	54.81	54.37	55.30	55.67
		(29.30)	(29.69)	(29.14)	(28.61)	(28.68)	(28.74)
	Comparison	48.81	48.84	48.37	48.57	47.41	47.03
		(28.43)	(28.15)	(28.40)	(28.72)	(28.51)	(28.28)
Share of students completing in the stipulated time	Stockholm	0.621	0.607	0.626	0.605	0.666	0.663
		(0.485)	(0.488)	(0.484)	(0.489)	(0.472)	(0.473)
	Comparison	0.661	0.639	0.667	0.625	0.633	0.650
		(0.473)	(0.480)	(0.471)	(0.484)	(0.482)	(0.477)

Note: Standard deviations below means. Grades and core grades are percentile ranked over the full sample of completing students for each year. The core subjects are defined in Footnote 8. The share of students completing in the stipulated time corresponds to those students who receive a final grade after three years of study at the upper secondary level.

Grades are higher in Stockholm than in the Comparison group and the difference gets larger over time, as shown by the GPA and the GPA^{core}. The GPA in Stockholm increases from 52.86 in 1995 to 54.32 in 2000, and the same pattern holds for core grades. The increase in the upper secondary grades in Stockholm comes as no surprise since Stockholm students have been shown to have increasingly better grades from compulsory schooling. The share of students completing in three years varies around two thirds, with no clear pattern except for a sharp increase in Stockholm between 1998 and 1999. The figures in Table 2 do not indicate any reform effects.

5. Results

This section begins with a brief descriptive analysis. Then the difference-in-differences model is specified and estimated on the full sample to identify average treatment effects, and on sub-samples to identify heterogeneous treatment effects. Robustness checks include analysing the impact of sorting, relative grade setting, and by conditioning on completion. Finally, there are tests for grade inflation.

5.1 Between school variance in student outcomes

According to the descriptive statistics in Table 2, Stockholm students have higher upper secondary grades than the students in the Comparison group. This difference becomes larger over time but it is hard to spot any indication of a reform effect. In order to examine whether there are any easily depicted effects of the reform on grades, the school level is studied. Since the sorting of students with respect to compulsory school grades changed after the reform, we should also expect the differences between upper secondary schools to increase in terms of student performance.

Table 3 presents ANOVA-results on the between school variance in student outcomes. Percentile ranked core grades are regressed on school dummies, and the results presented are the R^2-adj from these regressions. The upper part of Table 3 shows the result for all schools. It can be noted that the between school variance is larger in Stockholm, and that it is fairly stable for the years prior to the reform. More importantly, the results indicate a reform effect in Stockholm. In 1999, 20.7% of the variation in core grades could be explained by school attended; this figure has increased to 29.7% in 2000. The magnitude of the increase is much larger than in the Comparison group.

The observed pattern could be due to private schools. The lower part of Table 3 presents the same analysis, now using public schools only. The pattern is even stronger, the between school variance in Stockholm increases from 15.5% to 26.1% between 1999 and 2000. Hence, the results do not seem to be driven by private schools.

Table 3. ANOVA. R^2-adj from regression of core grades on school fixed effects.

	Between school variance in student outcomes					
	1995	1996	1997	1998	1999	2000
All schools						
Stockholm	0.116	0.178	0.184	0.174	0.207	0.297
Comparison	0.062	0.061	0.072	0.104	0.107	0.134
Public schools						
Stockholm	0.089	0.145	0.160	0.125	0.155	0.261
Comparison	0.058	0.039	0.055	0.069	0.059	0.077

Due to the selection issue described above, this observed pattern comes as no surprise. An increased sorting of students by ability over schools contributes to the observed pattern. Nonetheless, this is a clear indication that something is going on in the municipality, and the next section studies whether the reform has affected student performance.

5.2 Model specification and estimation

Equation (1) describes the difference-in-differences set up. GPA_{ist}^{core} is the weighted average of core grades (percentile ranked[20]), subscript *i* denotes student, *s* school, and *t* time. Explanatory variables are personal characteristics (X_{ist}), school dummies (γ_s), time dummies (γ_t), and a vector consisting of the products of a Stockholm school indicator with time (SS_{it}). The personal characteristics include information on gender, immigrant status, age, parental education, parental income (percentile ranked), program attended, the municipality where the student resides[21], grades from compulsory school, and compulsory school attended. The reform effect is identified through SS^{2000}, the interaction of an indicator for schools being located in Stockholm and the year 2000.

$$GPA_{ist}^{core} = \alpha + \beta * X_{ist} + \gamma_s + \gamma_t + \varphi * SS_{it} + \varepsilon_{ist} \qquad (1)$$

The results from equation (1) are displayed in column (1) of Table 4.[22] All the personal characteristics have the expected sign, and the explanatory power of the model is about 51%. The interaction-effect of schools situated in Stockholm with 2000 (SS^{2000}) is insignificantly negative, -0.92. Since this is a difference-in-differences approach, the reform indicator may be capturing differences in trends in the outcome measure. By studying the interaction terms for the years prior to the reform, the presence of a descending trend cannot be ruled out. That is, there is an (insignificant) positive effect of 0.74 for Stockholm schools in 1996 that becomes smaller, and eventually negative, over the years.

[20] If a student has not completed upper secondary schooling in three years, (s)he is given the grade 0.
[21] Students living outside the Stockholm County are grouped into one category.
[22] The specification assumes a linear effect of compulsory school grades. More flexible functional forms with dummies per quintile or decile have been tried, and the results are virtually unchanged. The results are also very similar using ordinary grades instead of percentile ranks. The results are available from the author upon request.

Table 4. Specification of baseline model.

	Full sample	Matching; Exclusion restriction 1%	Matching; Exclusion restriction 25%
Constant	46.167*** (2.611)	63.843*** (16.201)	-5.254 (6.519)
Female	0.893*** (0.175)	0.309 (0.324)	0.056 (0.350)
Age	-3.628*** (0.129)	-3.045*** (0.261)	-3.305*** (0.284)
Immigrant	-1.131*** (0.175)	-0.948*** (0.319)	-1.122*** (0.345)
Parental education	2.657*** (0.176)	2.458*** (0.322)	2.310*** (0.346)
Parental income	0.051*** (0.003)	0.054*** (0.006)	0.055*** (0.006)
Compulsory school grades	0.751*** (0.004)	0.790*** (0.007)	0.796*** (0.008)
SS^{1996}	0.738 (0.538)		
SS^{1997}	0.595 (0.540)		
SS^{1998}	-0.107 (0.568)		
SS^{1999}	-0.532 (0.566)		
SS^{2000}	-0.917 (0.568)	-0.568 (0.587)	-0.493 (0.623)
R^2-adj	0.509	0.545	0.545
Time period	1995-2000	1999-2000	1999-2000
N	101 414	28 811	24 797

Note: The outcome variable is percentile ranked core grades. Regressions include controls for time, upper secondary school attended, compulsory school attended, program, and municipality where the student resides. Robust standard errors are in parenthesis. Significance level: * = 10%, ** = 5%, and *** = 1%.

To eliminate potential problems with differences in trends, a strategy is used which matches municipalities with respect to pre-existing trends. Remember that the Comparison group consists of 25 municipalities. The years 1995 to 1999 is used to estimate linear pre-existing municipality-specific trends conditional on all the covariates. Municipalities with trends significantly different from Stockholm at the 1%-

level are excluded.[23] In a second step, a difference-in-differences analysis is performed for the years 1999 and 2000. The results, displayed in column (2), indicate an insignificant negative effect of the reform of 0.57 percentile points.

The result could be sensitive to the level of exclusion, meaning that the procedure does not fully cope with differences in trends. Hence, another specification is used, where a municipality is excluded if it has a pre-existing trend significantly different from Stockholm at the 25%-level. The result, presented in the third column, shows that the reform effect (-0.49) is very similar to the one in column (2). Hence, the model seems not to be sensitive to the exclusion restriction.

With support of the results in Table 4 the model is re-specified. The years 1995 to 1999 is used to estimate pre-existing trends, excluding those municipalities with trends significantly different from Stockholm at the 1%-level. In other words, by applying a common support restriction, four municipalities are excluded from the Comparison group. Hence, the baseline specification is defined to compare 1999 and 2000 in a difference-in-differences analysis according to equation (1).

Another advantage by adapting this strategy is that potential problems with compositional changes are circumvented. In Table 1 it was shown that the share of students in private schools in Stockholm increased sharply in the years prior to 1999, affecting the average student characteristics. Between 1999 and 2000 the share of students in private schools is fairly stable, and by comparing only these two years potential problems with private schools affecting the results are minimized.

Students may be affected differently by the reform. Some students did react to the new opportunities and attend a school which they otherwise would not have had access to. Other students are forced away from their neighbourhood school due to competition. By estimating separate equations by gender, grades from compulsory school, family background and immigrant status, potential heterogeneous treatment effects are examined.

Table 5 displays the results (reform effect estimates) when the sample is restricted to specific groups. Column (2) in the upper part of the table includes only students attending public schools. Compared to the baseline model in column (1) there is a small

[23] This procedure meant that 4 of the 25 municipalities in the Comparison group were excluded. The municipalities are: Järfälla, Huddinge, Sundbyberg and Nynäshamn. In 2000, about 1 500 students attended an upper secondary school in these municipalities.

decrease in the magnitude of the reform effect, both estimates being insignificant though. In column (3) only those students attending theoretical programs[24] are included, and the effect is larger in absolute value, 1.13, but still insignificant. Private schools do not seem to have major impact, and the effect in the theoretical programs, if anything, tends to be larger. Column (4) and (5) presents separate regressions by gender. The result is striking. Females perform significantly worse after the reform with a point estimate of -1.81, while the reform effect for males is insignificantly positive.

The mid-section of Table 5 shows the impact on students with different ability, measured by compulsory school grades. The results are displayed for the four quartiles of ability; Q1 being the lowest achieving and Q4 the highest. Results indicate big differences between groups, even though the coefficients are not significantly different from one another. The lowest achieving quartile has a positive insignificant estimate, 1.15. Quartile 2 and 3 perform significantly worse after the reform with point estimates of -2.00 and -2.47 respectively. The top achieving quartile has an insignificant estimate of -2.29 (p-value of 0.125).

The lower part of Table 5 presents result where student performance is studied according to immigrant status and socio-economic background. Low parental education includes students who do not have a parent with a university degree. Low parental income indicates if the students' parents belong to the lowest quartile in the parental income distribution. Correspondingly, high parental income is the top quartile of the income distribution. Immigrants perform 1.5 percentile ranks worse after the reform, and students with top-earning parents are also performing significantly worse, with a point estimate of -1.75 (both estimates are significant at the 10%-level). The coefficients on less advantageous socio-economic background show insignificant estimates with different signs.

The results seem to be driven by high ability students. This result is surprising. One would have expected that students with high grades from compulsory school should gain from the reform since they can choose school freely. But those who can exercise choice due to good grades do not seem to have benefited from this; their grades have even turned for the worse in some cases. Possible explanations to the observed pattern are discussed below

[24] The programs in natural and social science are academic tracks, denoted theoretical programs.

Table 5. Reform estimates from regressions on subgroups.

	Reform effects for public schools, for theoretical programs, and by gender.				
	All	Public schools	Theoretical programs	Females	Males
SS^{2000}	-0.567 (0.587)	-0.369 (0.624)	-1.126 (0.848)	-1.805** (0.878)	0.858 (0.805)
R^2-adj	0.545	0.542	0.495	0.530	0.551
N	28 811	24 295	17 039	13 951	14 860
	Reform effect by quartiles of the ability distribution				
	Q1 (low)	Q2	Q3	Q4 (high)	
SS^{2000}	1.147 (0.733)	-2.001* (1.067)	-2.473* (1.364)	-2.294 (1.496)	
R^2-adj	0.178	0.162	0.198	0.209	
N	7 441	7 280	7 139	6 951	
	Reform effect by immigrant status and socio-economic background				
	Immigrants	Low parental education	Low parental income	High parental income	
SS^{2000}	-1.546* (0.921)	-1.070 (0.748)	0.710 (1.109)	-1.746* (1.350)	
R^2-adj	0.533	0.519	0.496	0.491	
N	10 421	15 149	7 041	7 038	

Note: The outcome variable is percentile ranked core grades. Regressions include a constant and controls for gender, age, immigrant status, compulsory school grades, parental education and income, time effects, upper secondary school attended, compulsory school attended, program, and municipality where the student resides. Robust standard errors are in parenthesis. Significance level: * = 10%, ** = 5%, and *** = 1%.

5.3 Robustness checks

Different types of robustness checks are performed. These include the potential impact of sorting and relative grade setting, the influence of completion probabilities, and the effect of extending the outcome variable to the ordinary GPA.

The results could be driven by the inclusion of school fixed effects in the model. If school fixed effects are capturing a sorting effect, the school effects will have different meaning before and after the reform. To investigate this issue the estimations are done without school fixed effects. If the hypothesis stated above has an impact, it is expected that the differences between the quartiles of the ability distribution should diminish. The results, however, show similar point estimates (but more precise). For example, the lowest achieving quartile performs 1.1 percentile ranks better after the reform, and Quartile 3 perform 2.2 percentile ranks worse after the reform (both estimates significant at the 5%-level).

Another worry is that teachers are reluctant to give high grades to all the students in a high achieving class, i.e. grade-setting is relative. In principle, this should not be the case since the grading standard is goal-oriented. By including average peer quality in the regressions, it is possible to get a test for the impact of the student composition. The student composition may affect individual outcomes in different ways, e.g. through peer effects or relative grade-setting. Hence, if relative grade-setting has an impact on student outcomes - and drive the estimated reform effects - this can potentially be identified by the inclusion of the average peer quality.

The average compulsory grade per school and program is included additively in the regressions underlying Table 5. In all specifications the reform estimates are totally unchanged, and the direct effect of the peer quality is small and negative (and in most cases insignificant). The conclusion, hence, is that student composition does not drive the results. Even though there may be relative grade setting (the direct effect) it does not seem to affect the estimated reform effects.

The baseline model includes students not completing in the stipulated three years time, giving them the grade 0. Next robustness check is done by conditioning on completion, and the final check is to conduct the analysis with the ordinary GPA instead of core grades, also conditional on completion.

Table 6 displays estimated reform effects from three different specifications; row (1) replicates the baseline results from Table 5, row (2) is conditional on completion, and in row (3) the GPA including all subjects is used as the outcome measure, also conditional on completion. The fourth row shows the size of the restricted sample, and by comparing with the sample sizes in Table 5, one can conclude that on average about one third of the students are excluded.

In general, the results in the second row indicate that completion does matter, at least to some extent. The average treatment is more or less unchanged when conditioning on completion, but the effects in the sub-groups are affected. In general, the differences between treated and comparison group become smaller. The effect in public schools is larger, -0.71, but still insignificant though. For theoretical programs the effect is negatively significant, with a point estimate of -1.02. Unconditional on completion we observed a large difference in the point estimates between genders. This difference do not exist conditional on completion, both gender have an insignificant

effect of about -0.6. By studying the sample sizes, it can be seen that 69% of the females do complete, and only 62% of the males.

The effects over the ability distribution changes as well. All quartiles now have negative point estimates, and the differences between the quartiles are smaller. The largest effect is found in quartile 3 (the 50-75th percentiles of the compulsory grade distribution) with a significant point estimate of -2.19. The expected pattern of completion rates is seen in number of completed students per quartiles. Only 37% of the lowest achieving quartile actually completes, while the corresponding figure for the top achieving quartile is 87%.

Concerning immigrant status the significantly negative effect of the full sample is explained by changes in the probability of completion. Also concerning socio-economic background the negative reform effect estimates get closer to zero conditioning on completion.

The conclusion is that variations in completion rates are driving many of the notable differences between treated and comparison groups. In general, completion rates have gone done in Stockholm among groups of high-achievers after the reform, while the opposite is true among low-achievers The sole exception from this general pattern is immigrants.

The third row of each section in Table 6 displays the results on the ordinary GPA. The average effect, -1.15, is significant at the 5%-level. The pattern remains, the observed effect is largest among those with high compulsory grades. In the theoretical programs the reform effect is -1.97, significant at the 1%-level. In the highest and second highest achieving quartiles the effects are -2.57 and -3.31 percentile ranks respectively, significant at the 1%-level.

When students who do complete in the stipulated time are compared over the ordinary grades instead of the core subjects, the effects are larger and tend to be statistically significant to a greater extent. It seems that the negative reform effect is stronger for those subjects not being core subjects.

Table 6. Reform effects conditional on completion.

	All	Public schools	Theoretical programs	Females	Males
	Reform effects for public schools, for theoretical programs, and by gender.				
Baseline					
GPA^{core}	-0.567	-0.369	-1.126	-1.805**	0.858
	(0.587)	(0.624)	(0.848)	(0.878)	(0.805)
Completed					
GPA^{core}	-0.576	-0.710	-1.024*	-0.573	-0.603
	(0.505)	(0.559)	(0.615)	(0.703)	(0.752)
GPA	-1.145**	-1.376**	-1.967***	-1.541*	-0.835
	(0.575)	(0.636)	(0.714)	(0.796)	(0.853)
N	18 853	15 780	12 382	9 652	9 201
	Reform effect by quartiles of the ability distribution				
	Q1 (low)	Q2	Q3	Q4 (high)	
Baseline					
GPA^{core}	1.147	-2.001*	-2.473*	-2.294	
	(0.733)	(1.067)	(1.364)	(1.496)	
Completed					
GPA^{core}	-1.015	-0.313	-2.191**	-0.773	
	(1.532)	(1.182)	(1.059)	(0.782)	
GPA	-0.824	-0.441	-3.305***	-2.566***	
	(1.698)	(1.310)	(1.175)	(0.922)	
N	2 751	4 465	5 596	6 041	
	Reform effect by immigrant status and socio-economic background				
	Immigrants	Low parental education	Low parental income	High parental income	
Baseline					
GPA^{core}	-1.546*	-1.070	0.710	-1.746*	
	(0.921)	(0.748)	(1.109)	(1.350)	
Completed					
GPA^{core}	-0.088	-0.460	-0.052	-0.803	
	(0.920)	(0.797)	(1.216)	(0.913)	
GPA	-1.047	-0.859	-1.070	-1.692	
	(1.041)	(0.897)	(1.386)	(1.041)	
N	6 299	8 882	3 872	5 407	

Note: Row (1) replicates the baseline results, row (2) restricts the sample to students completing in three years, row (3) uses GPA as the outcome measure also conditional on completion, and row (4) shows the size of the restricted sample. Regressions include a constant and controls for gender, age, immigrant status, compulsory school grades, parental education and income, time effects, upper secondary school attended, compulsory school attended, program, and municipality where the student resides. Robust standard errors are in parenthesis. Significance level: * = 10%, ** = 5%, and *** = 1%.

5.4 Grade inflation

This paper uses grades as the outcome measure, and it also uses grades as a measure of initial ability. As have been stated above, grades are not an unproblematic measure of ability or knowledge. This section aims to answer whether grade inflation is present at the upper secondary level and the compulsory level. Grade inflation in the Comparison group at the upper secondary level, or grade inflation in Stockholm at the compulsory level could explain the observed pattern. The relevant concern is probably grade

inflation at the compulsory level in Stockholm since there are incentives for parents and students to put pressure on teachers to inflate grades after the reform.

Potential grade inflation at the upper secondary level is studied with use of a sample of national test scores. An indirect test for grade inflation at compulsory level is conducted by restricting the compulsory grade variable to only include the subjects English, Math and Swedish. Potential problems with grade inflation are assumed to be smallest in these subjects since the teachers can use national test scores as guidelines.

The National Agency for Education arranges national tests in English, Swedish, and Math, which are used by the teachers as guidelines for the grade setting. For a national sample of around 10% of the upper secondary schools, tests are collected on a yearly basis. These test scores are used in this paper to study grade inflation. Since grades are not a deterministic function of test scores, some divergence between test scores and grades are to be expected. The ratio between grades and test scores is in this paper denoted grade diversion. Grade inflation is said to exist if grade diversion is increasing over time. The question asked is whether potential grade inflation evolves differently in Stockholm and the Comparison group.

Some things should be noted about the test score data. First, it is only a sample of schools on a national basis, making the number of schools in Stockholm and the Comparison group varying over time. Second, test scores cannot be matched at the individual level, only per program and school, making the analysis relying on class averages.[25] Third, there is no information on when the students complete upper secondary schooling, making the analysis relying on the assumption that the students taking national tests complete in the stipulated time. We know that this is an approximation, but it is only a problem if the completion probability varies systematically between groups over time. Fourth, national test results are given a grade on the same scale as the ordinary grades. In this section ordinary grades will be used instead of percentile ranks.

Figure 1 displays clear evidence that grades and test scores in English differs systematically. This pattern holds for all three subjects, that is, the average grade is always above the average test score. It seems that teachers are using national test scores as a lower bound in the grade setting procedure. However, there is no indication that the

[25] Class in this section means program per school.

divergence between grades and test scores varies over time. A second thing to note from Figure 1 is the type of schools available each year, which by no means can be said to be representative for the group. This can be seen by comparing the sample averages with the Stockholm and Comparison group average, shown by the fine lines.

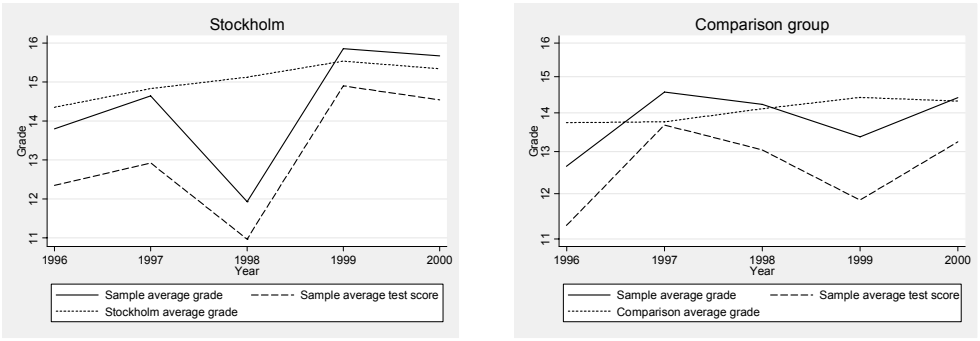

Figure 1. Average grade and average test score in English for the national test samples in Stockholm (left), and the Comparison group (right). The figures are supplemented with the average grade of all individuals per group respectively.

With grade diversion (GD_{pst}, the ratio of grades over test scores) as the outcome variable, equation (2) is estimated at the class level. Subscript p denotes program, s school and t time. Regressors are class averages of background characteristics (X_{pst}), program fixed effects (λ_p), school fixed effects (λ_s), time fixed effects (λ_t), and a vector of products of a Stockholm school indicator and time (SS_{pt}).

$$GD_{pst} = \delta + \phi * X_{pst} + \lambda_p + \lambda_s + \lambda_t + \theta * SS_{pt} + \eta_{pst} \qquad (2)$$

The coefficients of interest are the θ*:s*, which show how grade diversion evolves in Stockholm compared to the Comparison group. The results are shown in Table 7. The coefficients are mostly insignificant, and it is impossible to spot any trends. For Swedish there is a tendency for grade diversion becoming higher in the Comparison group, but that is not supported by results in the other subjects. Hence, the results do not indicate systematic differences in grade inflation between the groups.[26]

[26] Grade inflation has been analysed in two other specifications of equation (2), producing the same result. The first is by defining grade diversion as the difference between grades and test scores, and the second is to include test score as regressor and using grades as outcome variable.

Table 7. Tests for grade inflation at upper secondary level.

	Math	English	Swedish
SS^{1997}	0.222	0.143	0.308**
	(0.328)	(0.118)	(0.129)
SS^{1998}	-0.133	0.285	0.048
	(0.377)	(0.224)	(0.126)
SS^{1999}	-0.341	-0.068	-0.117
	(0.345)	(0.104)	(0.216)
SS^{2000}	0.108	0.017	-0.161
	(0.318)	(0.111)	(0.167)
R^2	0.673	0.552	0.627
N	139	174	136

Note: The outcome variable is grade diversion. Regressions are at the class level, and include a constant and controls for gender, age, immigrant status, compulsory school grades, parental education and income, time effects, school effects, and program. Robust standard errors are in parenthesis. Significance level: * = 10%, ** = 5%, and *** = 1%.

Finally, the impact of grade inflation at the compulsory level is analysed. It is argued that potential grade inflation should be the smallest in subjects where there are national tests. This includes the subjects Swedish, Math and English. In the descriptive statistics the average percentile rank over these three subjects is shown as GPA^{SME}. In this section the same analysis as in section 5.2 is conducted, but now with the compulsory GPA^{SME} instead of ordinary compulsory GPA as a measure of initial ability.

In the first step, six municipalities are dropped according to the 1% exclusion restriction. The model is then estimated in three specifications, corresponding to the first column in the upper part of Table 6, which is reproduced in the upper part of Table 8. That is, core subjects, core subjects conditional on completion, and ordinary grades conditional on completion are used as outcome measures.

The estimates are very similar. By using GPA^{SME} the reform effects are identical when studying the full sample, and somewhat smaller when conditioning on completion. The conclusion is that this indirect test does not support the argument that grade inflation at the compulsory level in Stockholm is driving the results.

Table 8. Reform effects using compulsory school GPA^{SME}.

	Full sample; Outcome variable: GPA^{core}	**Conditional on completion;** Outcome variable: GPA^{core}	**Conditional on completion;** Outcome variable: GPA
Compulsory GPA			
SS^{2000}	-0.567 (0.587)	-0.576 (0.505)	-1.145** (0.575)
R^2-adj	0.545	0.692	0.602
N	28 811	18 853	18 853
Compulsory GPA^{SME}			
SS^{2000}	-0.563 (0.654)	-0.397 (0.547)	-0.939 (0.638)
R^2-adj	0.483	0.654	0.539
N	26 327	17 627	17 627

Note: Regressions include a constant and controls for gender, age, immigrant status, compulsory school grades, parental education and income, time effects, upper secondary school attended, compulsory school attended, program, and municipality where the student resides. Robust standard errors are in parenthesis. Significance level: * = 10%, ** = 5%, and *** = 1%.

6. Conclusions

This paper evaluates a reform of the admission system of public upper secondary schools in the municipality of Stockholm. A residence-based procedure was changed into a grade procedure. This choice reform has earlier been shown to have had great impact on the distribution of students over schools. In this paper it is shown that the Stockholm students perform no better after the introduction of the choice reform. In fact, it seems that the effect is negative for immigrants, students from high-income families, and students with high grades from compulsory schooling. Given the nature of the reform, potential distributional consequences along the ability dimension would have been expected to be the reverse. That is, the losers from the reform should be those who are restricted in the choice process. It should be noted that some of the differences between groups can be explained by differences in completion probabilities. High-ability students in Stockholm tend to complete to a lesser extent, while low-ability students completes to a greater extent. However, some of the surprising distributional effects seem to persist over alternative specifications.

One should remember that the estimated reform effect in this paper is everything that can be attributed as a choice effect. Different mechanisms such as school productivity, peer effects, student sorting, and teacher grade-setting behaviour are all

captured by the reform indicator. However, it is hard to imagine a decrease in school productivity, it is not likely that peer effects are negative, and the results do not indicate grade inflation.

Given the distributional effects it seems reasonable to argue for relative grade-setting since students with high-grades from compulsory schooling to a greater extent were paired with low-achieving students prior to the reform. However, this should not be the case since grades are goal-oriented, and an indirect test does not indicate that relative grade-setting drives the results.

The most plausible explanation seems to be that students misinterpret school productivities when exercising choice. That is, they only observe crude measures of student achievement in terms of grades and test scores, and they have no opportunity to judge whether these outcomes reflect educational production or is a result of student characteristics, i.e. educational input.

One should keep in mind that the analysis in this paper only uses one post-reform cohort, and any competition effects may not be seen at this early stage. That is, the schools may not be capable of reacting to financial incentives so quickly. Therefore, one may conclude that school choice did not immediately improve student performance. The longer run effects are perhaps more favourable to the proponents of school choice.

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Essay III

On the Impact of Individual Wage Bargaining in the Swedish Teachers' Labour Market

1. Introduction

During the 1960s and 1970s, the Swedish labour market was characterized by unionization, centralized bargaining and a "solidaristic" wage policy. The egalitarian ambitions reduced wage differentials, and the wage compression peaked in the early 1980s. Since then, the wage-setting scheme has been transformed in most areas, moving towards decentralized bargaining.[1]

Compared to other segments of the Swedish labour market, teachers were late in the adaptation of decentralized bargaining. In mid 1990s the wage structure was still described by wage scales. A teacher was sorted into one of six wage scales by type of work, and then the wage was determined by experience in the profession. Hence, given teacher category and experience, the wage was fixed. The different scales had different entry-wages but the same wage ceiling. This scheme created a situation with a well-defined, increasing age profile of wages, and a decreasing age profile of wage dispersion. In 1996, this rigid system was replaced by individual bargaining. This was a drastic change, at least on paper, and if the scale system was a binding constraint the wage structure is expected to change.

This paper investigates what impact this reform had on the earnings structure for Swedish teachers. This reform is interesting for two reasons. First, it can add to the literature on wage bargaining and wage dispersion. There is an extensive literature on this topic, and it is well documented that centralized bargaining and a high degree of unionization is associated with wage compression.[2] Most often, collective wage agreements and unionization come together, as it did in Sweden. However, the introduction of individual wage bargaining in Sweden has not given any sharp decline

[1] The Swedish bargaining system is described by Elvander & Holmlund (1997), and Elvander (2003, 2004). Edin & Holmlund (1995), and Gustavsson (2006) describe the Swedish wage structure.
[2] For the impact of labour market institutions on wage dispersion, see Katz & Autor (1999).

in unionization. Hence, this paper can give new evidence on the potential effects of individual wage bargaining in a system with high union coverage.[3]

Second, the teacher labour market is of interest in itself. Björklund *et al* (2005) conclude that the attractiveness of the teaching profession in Sweden has declined over the years, and that those who opt for becoming teachers are on average less able in the 1990s than they were in the decades before. Even though it has been hard to empirically quantify the effect of teachers on student performance, or which teacher characteristics that are of significance, the teaching profession is considered to be of essential impact in the educational production.[4] The teacher unions hoped that individual wage-setting would increase the attractiveness of the teaching profession, and explicitly stated that they wanted the returns to productive teacher characteristics to increase.

The empirical work in this paper consists of two parts, where in the absence of information on wages, the analysis relies on earnings. The first part describes the age-profile of earnings and earnings dispersion. The second part analyses what variables that explain the variation in earnings, and if individual characteristics such as education and certification are valued differently after the reform. Throughout the study, teachers at compulsory level and upper secondary level are studied separately.

Individual data covering the entire Swedish teacher population from 1990 to 2000 are used. One would like to net out the pure reform effect from other simultaneous changes by comparing the outcome to a comparison group; ideally this comparison would approximate what the situation would have been like without a reform. For this study, there is no natural comparison group enabling such a difference-in-differences analysis. Still, to be able to compare the outcome for Swedish teachers to another group over the same time period, a representative sample of public employees is used as a reference group.

The results indicate that the reform affected the earnings structure. In both teacher categories, entry-wages increase and the age-earnings profile becomes less steep, an effect that cannot be seen in the reference group. Furthermore, earnings dispersion

[3] There is some Swedish evidence on the impact of individual wage bargaining on the wage structure. For example, Lundborg (2005) finds increased wage dispersion among white-collar workers, and Säve-Söderbergh (2003) shows that the gender wage gap increased for recent graduates. Granqvist & Regnér (2004) find that graduates negotiating their own wages have higher wages than those who do not.

[4] See for example Rockoff (2004) or Lazear (2003) for studies on the impact of teacher quality and teacher incentives on student performance.

increased for older compulsory teachers. This is in line with expectations since the scale system induced low dispersion for teachers with high experience. In the reference group, on the other hand, dispersion increased for younger individuals.

Observable characteristics do explain the variation in earnings to a lesser extent in the late 1990s compared to the early 1990s, but this is not unique for the teacher labour market. Finally, and perhaps surprisingly, the returns to education and certification do not seem to increase after the reform, rather the opposite. In the reference group, on the other hand, the university premium increases in the second half of the 1990s.

The results suggest that the scale system that existed in the Swedish teachers' labour market was a binding constraint, since individualizing wage-setting clearly affected the earnings structure. Wages is the key mechanism for equalling supply and demand in a labour market. With binding constraints on wage-setting the functionality and flexibility of the market is negatively affected. This may have been the case in the Swedish teachers' labour market prior to 1996.

The paper is organized as follows. Section 2 describes the background and section 3 the data. Section 4 presents results, and section 5 concludes.

2. The Swedish teachers' labour market

This paper focuses on two parts of the Swedish educational system, that is, compulsory and upper secondary schooling. Compulsory schooling lasts for nine years, and the students are usually between the ages of 7 and 16. Students completing compulsory schooling may attend upper secondary schooling, which lasts for three years. Almost all students that completes 9^{th} grade do continue to the upper secondary level.

In the early 1990s, the responsibility for primary and secondary education was transferred from the government to the municipalities. A more goal-oriented, competitive school system was the intention of the decentralization. Until 1993 the municipalities received federal funding which was ear-marked for education, but since then the municipalities are free to determine how much to spend on education. At the time of the decentralization there were discussions on introducing local wage

bargaining, but the economic crisis and the so called Rehnberg-agreement postponed this decision.[5] The wage scales remained until 1996.

There were six different wage scales, defined by type of teacher. The six teacher types were: (1) primary level (grades 1-6), (2) lower secondary level (grades 7-9), (3) music/art/sport (4) general subjects at the upper secondary level, (5) vocational subjects at the upper secondary level, and (6) lectureships. The scales defined an entry-wage, and thereafter when and how wages should rise up to a wage ceiling. Typically, it was raised every 18 months for 15 years, then yearly for 5-8 years. That is, after 20-23 years the wage ceiling was reached.[6]

Figure 1 displays the three wage scales for compulsory school teachers at the 1st of January 1990. The y-axis displays the monthly wage in SEK, and the x-axis displays years of experience. From the graph it is obvious that according to the scale system wages increased with age. Furthermore, according to the scales wage dispersion decreased with age. There is scope for wage dispersion at lower ages (experience), while after 23 years of experience there is by definition zero dispersion. The two teacher categories at upper secondary level (not shown in the figure) had higher entry-wages and reached the common wage ceiling some years earlier.

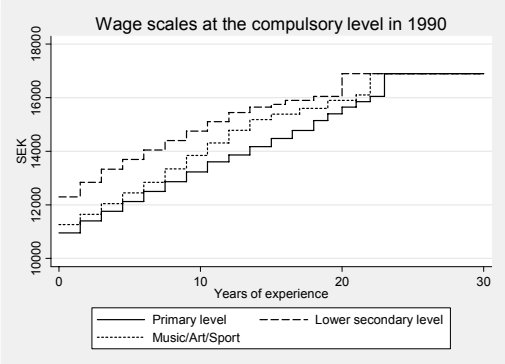

Figure 1. The three different wage scales at the compulsory school level in 1990.

In practice, there were deviations from the expected wage given the wage scales. These deviations were of three main types. First, it was possible to get an addition to the wage for responsibilities outside teaching, for example the school library or the equipment in

[5] Elvander (2003) describes the Rehnberg-agreement. This stabilization agreement covered the period from January 1991 to April 1993 and included among other things a wage freeze and prohibition against local wage bargaining during 1991.
[6] On a regular basis (6-12 months) the entire scale shifted upwards.

the physics department. Second, a mark-up on the wage could be given in fields where it was hard to attract teaching personnel. For example, teachers in the building industry have always been hard to attract since they are better paid in regular industry jobs. Third, teachers missing pedagogical education from university, i.e. were not certified, were typically given 1 800 SEK below the wage given by the scale.

Two things should be noted. Despite these deviations, the wage scales should be considered fairly deterministic. More importantly, the deviations were more frequent at the upper secondary level, due to the more heterogeneous composition of teachers and subject fields.[7]

In February 1996, the two Swedish teacher unions formed an agreement with the teacher employer organization, which abolished the scales and introduced individual wage bargaining in the teacher labour market.[8] The teacher unions are Lärarnas Riksförbund (LR), which hosts about two thirds of the upper secondary school teachers, and Lärarförbundet (LF), hosting about two thirds of the compulsory school teachers. The agreement was set to cover the period from 1st of April 1995 to 31st of March 2000, and the aim was to reach a purely individualistic bargaining system.

The employer organization was the driving force in this negotiation. However, both unions were dissatisfied with the scale system. LF considered the wage scales unfair when compensating for the amount of working time since a full time equivalent meant different hours for different teachers. LR was basically dissatisfied with the level of wages. Actually, LR had already in 1992 internally accepted a transformation towards individual wage-setting. Hence, the union where most of the upper secondary teachers were members seems to have had stronger preferences for individual wage bargaining.

The new agreement contained limited amount of guarantees. A general wage increase was only given retrospectively for 1995 (1% wage increase) and for 1996 (450 SEK, and an additional 350 SEK for those who had reached the wage ceiling). From 1997 and onwards it exists minimum wages after fulfilling one working year, and five working years respectively.

[7] There is no formal documentation on the commonness of these deviations. The claim that they are more common at the upper secondary level comes from discussions with teacher union representatives.
[8] The agreement is described in Lärarförbundet (1996). The teacher employer organization is Svenska Kommunförbundet.

From 1996 and onwards, wages are bargained locally on a yearly basis, using a centrally determined amount of funds.[9] Wages should be differentiated by, e.g., performance, responsibility and power of initiatives.[10] It was explicitly stated that the existing wage dispersion in the public school sector should be kept or increased.

It was realized that one cannot go from wage scales to a freely individual wage-setting overnight. Rather, it was emphasized that the forms for local bargaining need time to develop. Since there were no formal regulations for how to conduct the wage bargaining, differences between regions and schools are to be expected. Hence, the transformation to individual wage bargaining was gradual. During the late 1990s the form for the individual wage-setting was typically either individual wage negotiations between the teacher and the school master, or bargaining between the local union and the school master over individual wages. Even though the forms for the individual wage-setting may differ, the abolishment of wage scales and introducing wage-setting based on individual performance, constitute a sharp change in the wage formation.

3. Data

The data come from the teacher register of Statistics Sweden, which includes all personnel in the schooling sector. All individuals are extracted for eleven consecutive years, from 1990 to 2000. This procedure gives data covering six years prior and five years after the reform. Data contain information on individual and workplace characteristics. To these data, additional information on personal characteristics and earnings are matched using LOUISE, a longitudinal register of education and income kept by Statistics Sweden.

Since the analysis will be done separately for teachers at the compulsory level and the upper secondary level, data are divided into these two categories, leaving out individuals working at preschool, adult education etc.

Data are restricted in four ways. First, all individuals with an appointment other than being a teacher are excluded from the samples. This means that for example school managers and study counsellors are excluded. Furthermore, substitutes and home-

[9] The size of the fund is calculated as a percentage of each union's wage sum. Between 1996 and 2000 it typically varied between 2-4%, see Lärarförbundet (1996, 1998, 2000).

[10] Actually, there was an additional fund created explicitly for rewarding acts that contribute to school and teaching developments. This fund was also set by a percentage of each union's wage sum, see Lärarförbundet (1996).

language teachers are excluded. Second, some teachers have multiple observations in the data because they work in several school areas. Since data include information on working time, the observation per individual where he or she spends most of his or her working time is used. Third, since there is no earnings information on teachers older than 65, they are excluded, and so are those individuals being on leave. For some individuals (about hundred per year) earnings information is missing and they are also excluded. Fourth, to get rid of extreme values, the lowest percentile in the earnings distribution is excluded in both groups.

A sample of individuals employed in the public sector is used as a reference group. All individuals working in the public sector (except for compulsory and upper secondary teachers) between the ages of 18 and 65 are extracted from LINDA, a longitudinal database consisting of a 3.35% representative sample of the Swedish population.[11] This is done for the same eleven years, from 1990 to 2000.[12] From this sample, students and individuals with compulsory schooling as their highest educational attainment are excluded. If an individual receives student benefits or loans during a year, he or she is considered a student and is thereby excluded. The cut along the educational attainment dimension is to make the sample more similar to the teacher groups. Further, the sample is restricted to include individuals with positive earnings and working time. Finally, to get rid of extreme values, the lowest percentile in the earnings distribution is excluded.

A large share of the reference group consists of employees in the health care sector (36.2% in 1992), but also childcare (27.9%), elderly care (14.1%) and public service (11.6%) constitute large fractions of this sample. The fractions of these groups remain fairly stable over the years.

The reference group is not homogenous concerning wage bargaining. In the health sector, for example, individual bargaining is used extensively, while for the police and military a centralized system is used. The choice of reference group is not chosen to approximate a counterfactual wage bargaining regime; rather it is chosen to resemble a labour force which share common labour market characteristics. Those groups who had individual bargaining in the early 1990s were restricted by the Rehnberg-agreement which wiped out the individual bargaining possibilities. The groups who used

[11] For a description, see Edin & Fredriksson (2000)
[12] 1990 and 1991 will be used sparsely, since they do not include information on working time.

centralized bargaining throughout the 1990s (i.e. police and military) are not well suited to be a comparison group due to differences in labour market characteristics and labour force composition.

3.1 Descriptive statistics

Table 1 displays descriptive statistics over key variables for three years; 1992, 1996 and 2000. Data are shown separately for the three groups. The variables are defined as follows. Immigrant is a person born outside Sweden. Highest educational attainment is divided into three categories; upper secondary level, university and doctoral degree (PhD). If there is at most one year since completing education, an individual is defined as an entrant.

Except for differences in sample size, two major differences stand out in data. First, the gender composition differs substantially between upper secondary teachers with a fairly equal gender distribution, and the other groups, where the share of females is above 70%. Second, more than 90% of the teachers have completed university, which comes as no surprise since being a certified teacher implies a university degree. In the reference group, half of the sample has at most completed upper secondary schooling. Furthermore, the time trends show different patterns; the teacher groups become slightly less educated while the reference group becomes more educated.

Some other things are worth noting. The age distribution and the share of entrants show divergent time pattern between the teacher groups and the reference group. Teachers are on average younger in 2000 compared to 1996, while the opposite holds for the reference group. The fraction of individuals labelled as entrants increases in the teacher categories, from 2.4% in 1992 to 10.7% for compulsory teachers. In the reference group the share of entrants lies fairly constant around 4%.

Immigrant and marital status show similar trends in all groups. The probability of being immigrant is highest in the reference group, 7.0% in 1996, compared to 6.5% for compulsory teachers and 6.1% for upper secondary teachers. The share of immigrants is increasing in the late 1990s in all groups, reaching 8.8% in the reference group in 2000. The share of married individuals is sharply decreasing in all groups during the late 1990s. For example, for compulsory teachers, who have the highest marriage rate, it decreases from 66.0% in 1996 to 58.5% in 2000.

Table 1. Descriptive statistics, means and standard deviations.

		Female	Age	Immigrants	Married	N
Compulsory level	1992	0.710	45.451	0.060	0.700	83 853
		(0.454)	(9.431)	(0.238)	(0.458)	
	1996	0.734	45.902	0.065	0.660	82 008
		(0.442)	(9.903)	(0.246)	(0.474)	
	2000	0.735	44.583	0.078	0.585	86 812
		(0.441)	(11.158)	(0.269)	(0.493)	
Upper secondary level	1992	0.457	47.368	0.053	0.674	26 000
		(0.498)	(8.986)	(0.224)	(0.469)	
	1996	0.488	47.693	0.061	0.640	27 671
		(0.500)	(9.269)	(0.240)	(0.480)	
	2000	0.476	47.030	0.073	0.583	26 910
		(0.499)	(10.302)	(0.261)	(0.493)	
Reference group	1992	0.719	41.769	0.075	0.580	30 581
		(0.449)	(10.647)	(0.263)	(0.493)	
	1996	0.728	43.711	0.070	0.591	29 387
		(0.445)	(10.233)	(0.255)	(0.492)	
	2000	0.723	44.049	0.088	0.540	31 873
		(0.447)	(10.634)	(0.283)	(0.498)	
			Educational attainment		**Entrants**	**School areas**
		Secondary	**University**	**PhD**		
Compulsory level	1992	0.044	0.955	0.001	0.024	2 307
		(0.205)	(0.208)	(0.036)	(0.154)	
	1996	0.032	0.966	0.002	0.063	3 277
		(0.176)	(0.180)	(0.040)	(0.244)	
	2000	0.064	0.935	0.001	0.107	3 443
		(0.244)	(0.246)	(0.036)	(0.309)	
Upper secondary level	1992	0.036	0.927	0.037	0.036	664
		(0.186)	(0.260)	(0.189)	(0.187)	
	1996	0.047	0.928	0.024	0.051	566
		(0.213)	(0.258)	(0.154)	(0.220)	
	2000	0.066	0.916	0.018	0.085	609
		(0.249)	(0.277)	(0.132)	(0.279)	
Reference group	1992	0.516	0.466	0.018	0.043	
		(0.500)	(0.499)	(0.134)	(0.204)	
	1996	0.467	0.512	0.020	0.031	
		(0.499)	(0.500)	(0.142)	(0.174)	
	2000	0.472	0.505	0.023	0.045	
		(0.499)	(0.500)	(0.149)	(0.207)	

Note: Immigrants correspond to individuals born outside Sweden, and entrants to those who have completed education within the last year. For further information, see text.

From 1995 and onwards, data include a school identifier, but a school area code is available for the full time period.[13] School area identifies a school at upper secondary level, but at compulsory level several schools can be located in the same area. More precisely, prior to 1992 it was on average more than two compulsory schools per area. During 1992 and 1993 the areas did split up, making the number of areas increasing

[13] School area corresponds to "rektorsområde".

from 1 501 in 1991 to 2 862 in 1993. From 1995 and onwards it is on average 1.5 schools per area.[14]

Figure 2 displays two other characteristics of the two teacher categories. Certified is a variable indicating whether the teacher has a pedagogical education from university or not. Private school is an indicator for whether the teacher is working in a school that is privately run. The share of certified teachers is falling in both groups, but the decline is sharper at the upper secondary level. For compulsory school teachers the major decline is between 1996 (91.9%) and 2000 (83.3%), while for upper secondary school teachers the decline is evenly distributed over the time period. The increase in the share of non-certified teachers seems to reflect a shortage in teacher supply, see Björklund *et al* (2005).

In 1991 private schools became entitled to municipal funding in Sweden.[15] Since then the number of private schools has been continuously increasing, and hence also the number of teachers working in the private sector. Figure 2 shows how the share of teachers in private compulsory schools has increased from below 0.5% in 1991 to 4% in 2000.[16] Hence, the expansion of private school alternatives has clearly increased the labour market opportunities for teachers.

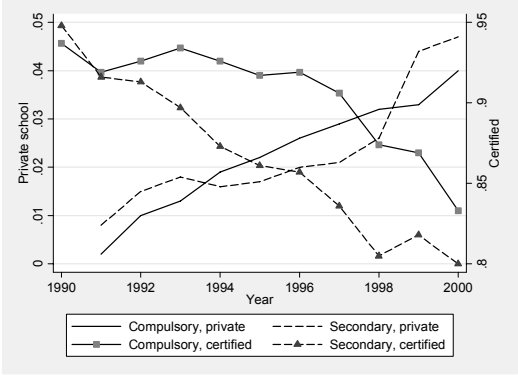

Figure 2. The share of certified teachers and the fraction of teachers working in the private sector, at the compulsory school level and the upper secondary school level during the 1990s.

[14] The number of compulsory schools were 4 998 in 1995, and 5 174 in 2000.
[15] Private schools in Sweden are entitled to municipal funding since 1991 if they fulfil the requirements of the National Agency for Education, and do not charge tuition fees. This type of private school has grown rapidly during the 1990s. There is also a second type of private schools in Sweden. They existed prior to 1991, are using tuition fees, and constitute only a very small fraction of the schools (well below 1%).
[16] Private schools cannot be identified in data in 1990. However, they are very few, see Footnote 15.

Thus, the inflow of teachers during the 1990s tends to be younger and less educated (at least in terms of certification) than the existing stock of teachers. Further, more teachers are working in the private sector. Of course, when evaluating the impact of individual wage bargaining one must take these labour market changes into consideration.

Data also include further background variables not shown in the tables, such as information on the number of children in the household, where the person resides, and working time.

Table 2 shows earnings (in SEK) and log earnings for the three groups. Earnings are higher for upper secondary teachers than for compulsory teachers. This is what to expect given the scales, since they on average are higher for upper secondary teachers. The reference group has the lowest level of earnings initially, but eventually individuals in this group surpass compulsory teachers during the time period.

The standard deviations reported in Table 2 reveal that there is an increase in dispersion for compulsory teachers at the end of the time period, while for upper secondary teachers the increase in dispersion is mainly during the early 1990s. The earnings dispersion is greater in the reference group. This could reflect greater variation in working time, and/or more heterogeneity than in the teacher groups. There is a steady increase in earnings dispersion in the reference group.

The reform concerns wage-setting. Of course, one would like to use wages as the outcome measure. In the absence of wages, this study uses annual variation in earnings. The variation in earnings reflects the variation in wages, but also the variation in working hours. In the data, there is information on working time. Thus, it is possible to handle the variation in working time to some extent, by conditioning on working time. A potential problem with the working time information is that the reliability of this information changes over time. In particular, the quality of the working time variable improves between 1997 and 1998. This is discussed further below, and essentially this issue is dealt with by performing the analysis conditional and unconditional on working time. Generally, the results are very similar. Furthermore, the earnings variable also captures earnings from other sources than teaching. If the teacher has a second job, this is reflected in the earnings variable. However, this is not considered common among Swedish teachers.

Table 2. Earnings and log earnings, means and standard deviations.

	Compulsory school		Upper secondary school		Reference group	
	Earnings	Log earnings	Earnings	Log earnings	Earnings	Log earnings
1990	175 384	12.031	197 988	12.152	158 928	11.906
	(45 776)	(0.322)	(53 474)	(0.319)	(67 481)	(0.362)
1991	180 764	12.064	204 050	12.178	165 117	11.939
	(46 404)	(0.308)	(58 267)	(0.330)	(73 494)	(0.374)
1992	186 487	12.095	207 804	12.192	170 703	11.970
	(46 896)	(0.310)	(60 128)	(0.349)	(74 776)	(0.386)
1993	188 208	12.104	208 971	12.193	173 067	11.980
	(47 236)	(0.313)	(62 514)	(0.371)	(77 025)	(0.397)
1994	189 854	12.111	212 778	12.209	179 799	12.018
	(48 407)	(0.319)	(64 180)	(0.375)	(80 099)	(0.399)
1995	190 339	12.112	212 591	12.211	182 312	12.035
	(49 267)	(0.324)	(63 013)	(0.366)	(79 655)	(0.391)
1996	200 040	12.164	222 122	12.259	194 116	12.098
	(50 706)	(0.318)	(63 234)	(0.354)	(85 098)	(0.392)
1997	205 993	12.188	228 041	12.281	201 799	12.135
	(54 456)	(0.338)	(66 061)	(0.373)	(88 325)	(0.400)
1998	207 376	12.190	232 624	12.301	207 398	12.164
	(57 304)	(0.356)	(67 636)	(0.370)	(89 235)	(0.394)
1999	214 152	12.223	242 989	12.347	215 227	12.196
	(59 102)	(0.353)	(69 335)	(0.360)	(95 987)	(0.411)
2000	216 931	12.235	245 658	12.358	221 763	12.222
	(61 489)	(0.353)	(69 767)	(0.362)	(100 504)	(0.421)

Note: Earnings are measured in SEK. The reference group is restricted to include individuals with positive working time. In 1990 and 1991, when working time information is missing, the 10^{th} lowest percentiles in the earnings distribution are excluded to make the data cross-sectional representative.

4. Results

This section begins with an analysis of the age profile of earnings and earnings dispersion, and continues with discussing how successful background variables are in explaining the variation in earnings. Finally, the returns to individual characteristics are studied.

4.1 The age-earnings profile

Figure 3 shows the age profile of log earnings (conditional on working time[17]) in 1992, 1996, and 2000 for the three groups. The period from 1992 to 1996 represents a period with centralized bargaining, and the period from 1996 to 2000 represents a period with individual bargaining.

[17] This is done by first regressing log earnings on working time. I then subtract the predicted values generated from this regression from log earnings, and finally adjust the mean of the transformed earnings variable such that it corresponds to the original one.

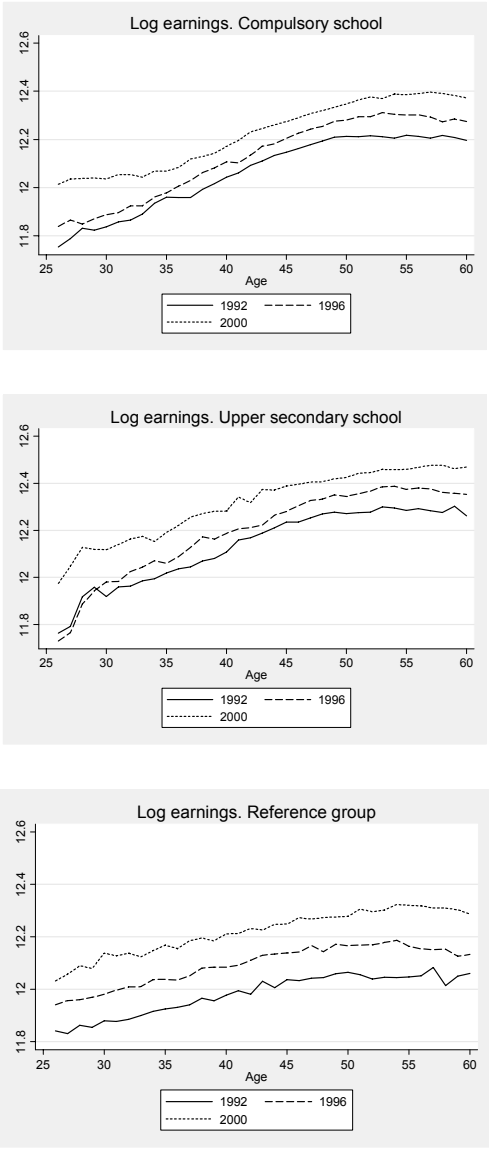

Figure 3. Compulsory teachers (top), upper secondary teachers (middle), and reference group (bottom) age profiles of log earnings in 1992, 1996, and 2000. All profiles are conditional on working time.

As expected, earnings increase with age. However, the teacher profiles in 1992 and 1996 are clearly steeper than in the reference group. In 1992, a 50 year old teacher earns about 50% more than a 26 year old. This holds for both teacher groups. The corresponding figure in the reference group is 20%.

For all groups the profile is more or less unchanged (except for levels; note that earnings are expressed in real values) between 1992 and 1996. If anything, the profile for upper secondary teachers gets somewhat steeper. More importantly, there is an upward shift in earnings for young teachers between 1996 and 2000. This is a striking change in the profile. For example, a 26 year old compulsory school teacher earns on

average 20% more in 2000 compared to 1996, while the corresponding figure for teachers older than 35 is between 5 and 10%. The same pattern is observed for upper secondary teachers. In the reference group, on the other hand, the profile shifts up by about the same amount for all ages.

In 1992 and 1996 the profiles in both teacher categories are flat for individuals above 50. It could be the wage ceiling that kicks in, and it is no longer possible to get into a higher wage scale. In 2000, the profiles are not as flat above 50. However, since this effect can be seen in the reference group as well, it may be something else generating this pattern.

Altogether, the age-earnings profile for the two teacher groups is less steep in 2000 compared to 1996, mainly as an effect of higher earnings among young individuals.[18] In the reference group, if anything, the profile gets steeper.[19]

So what can explain the observed pattern? It could be argued that young individuals are more mobile and can better take advantage of labour market opportunities. In other words, young workers are more apt to benefit from a more competitive market induced by individual bargaining, since labour supply is more elastic at younger ages. If this is the case, the observed pattern can be explained by employers having to bid up wages when facing more elastic labour supply.

4.2 Earnings dispersion

From Table 2 we know that the earnings dispersion increased for compulsory teachers after the reform, while for upper secondary teachers it increased at least as much prior to the reform as after.[20] Figure 4 shows earnings dispersion (standard deviations of log earnings, conditional on working time) over the age distribution in 1992, 1996 and 2000 for the three groups.

[18] The two teacher groups have been divided by gender, and have also been restricted to include only certified teachers, and public school teachers respectively. In all groups there is an upward shift in 2000 for young teachers.

[19] This is in line with the findings in Gustavsson (2006). By studying wage equations he finds that the Swedish age-wage profile is steeper in 2001 than in 1992.

[20] The 90/10 percentile ratio of earnings confirms this pattern. In 1990 it was 2.02 at compulsory level, 1.97 in 1996, and 2.20 in 2000. The corresponding figures for upper secondary teachers were 2.00, 2.20 and 2.20.

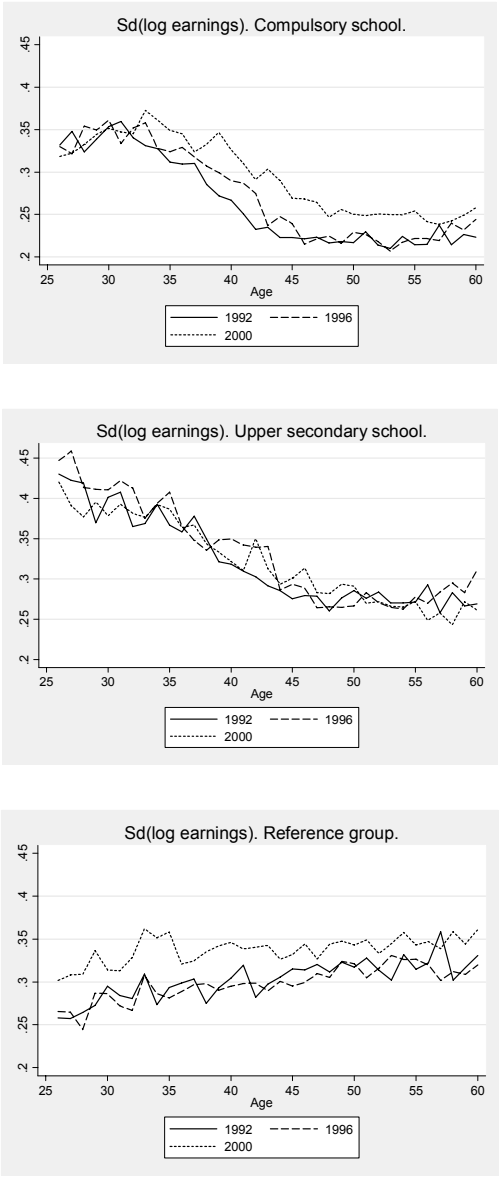

Figure 4. Standard deviation of log earnings in 1992, 1996, and 2000, for compulsory teachers (top), upper secondary teachers (middle), and the reference group (bottom). All profiles are conditional on working time.

For both teacher groups the age profile shows the expected decreasing pattern generated by the wage scales. In 1992, the standard deviation among 30 year old compulsory teachers is 0.353, and for 50 year old teachers it is 0.217. For upper secondary teachers the figures are 0.401 and 0.286 respectively. The reference group has an increasing age-profile of earnings dispersion, where the differences between age groups are substantially smaller.

The profile for compulsory teachers in 1992 and 1996 are very similar, but an upward shift for older teachers is observed between 1996 and 2000. Since the increase

in dispersion only corresponds to those over the mid 30s, the downward sloping profile in 2000 is not as steep as in 1996.[21] This is what one did expect; the wage scales induced dispersion at lower ages, but limited scope for dispersion for those with high experience (i.e. age). When abolishing the scales, dispersion increases where it from the beginning was at a low level.

It should be noted that the quality of the working time variable improves from 1998 and onwards. This means that conditioning on working time reduces the variation in log earnings by more in 2000, compared to 1992 and 1996. In other words, the upward shift in 2000 is underestimated. By studying the dispersion unconditional on working time, one can see that the increase in dispersion in 2000 is even larger.[22] However, Figure 4 clearly shows that the dispersion increases for older compulsory school teachers, and that the downward-sloping age-profile of earnings dispersion becomes less steep.

Upper secondary teachers have prior to the reform slightly higher earnings dispersion than compulsory teachers. It is hard to spot any differences over the years, if anything; the profile is somewhat less steep in 2000 compared to 1996.

As stated above, the age profile in the reference group is drastically different from the teacher groups, and also the changes over the years are different. There is an increase in dispersion between 1996 and 2000, but opposite to compulsory teachers, the increase is largest among young individuals.

It seems that the observed increase in dispersion for compulsory school teachers is a reaction to the abolishment of the wage scales. In order to investigate whether these changes are attributable to the increase in the share of non-certified teachers or private schools, the sample is restricted to public school teachers and certified teachers respectively. The results are presented in Appendix A. It does not seem that private schools are driving the results, since leaving teachers working in the private sector out of the analysis, does not affect the result. Part of the observed increase in earnings dispersion in 2000 seems to be attributable to non-certified teachers. However, the increase in dispersion is still evident for older certified teachers.

[21] The upward shift for older compulsory teachers in 2000 is significant. By regressing the earnings dispersion measures for cohort *a* at time *t* on age dummies, time dummies, and an indicator for cohorts older than 35 in 2000, yields a point estimate of 0.032 for old compulsory teachers in 2000, significant at the 1%-level.

[22] Results are available from the author upon request.

Let me summarize the results of these two sections. Concerning earnings levels, young teachers are positively affected in both teacher categories. That is, entry-wages seem to have gone up. Given that younger teachers are more mobile and better prepared to take advantage of a competitive market; this is what one should expect. Focusing on earnings dispersion, older cohorts at compulsory level are affected. This is also what one should expect. Prior to the reform there was more dispersion in the younger age groups due to differences within and between wage scales. But when the wage ceiling kicks in, there is no longer scope for wage dispersion. Hence, the wage scales compressed the wage structure the most among older cohorts.

It is somewhat surprising that earnings dispersion increases for compulsory teachers, but nothing seems to happen for upper secondary teachers. Focusing on older teachers, one should note that the initial dispersion in 1996 was substantially larger at the upper secondary level. Even though there is no increase in dispersion in 2000, it is still larger at upper secondary level (around 0.28 for a 50 year old upper secondary teacher, compared to 0.25 for a 50 year old compulsory teacher). This suggests that the reason for the differences between the teacher groups is not what happened after 1996; rather that something already had happened. Possible explanations are discussed below.

4.3 Explanatory value of observed characteristics

The reform dissolved the tight connection between experience and wages. It has been shown that the earnings structure over the age distribution has been affected, mostly for compulsory teachers. This section aims to answer what variables that explain the variation in earnings, and how they change over time. Figure 5 shows the R^2 from cross-section regressions from 1990 to 2000 for the three groups. The regressions are described by equation (1), where log earnings are regressed on a set of explanatory variables (X_{it}). The explanatory variables are: gender, age, immigrant status, marital status, educational attainment, number of children, municipality where the individual resides, and working time. The age specification is as flexible as possible, that is, one dummy per age cohort.

$$\ln(y_{it}) = \alpha + \beta * X_{it} + \varepsilon_{it} \qquad (1)$$

Figure 5 shows that the explanatory value of the model is highest in the reference group and lowest for upper secondary school teachers. Given that the wage scales were purely deterministic and information on wages and experience were available, the R^2 in the teacher groups would be equal to one. That the R^2 is around 0.3-0.4 has several explanations. The analysis relies on earnings instead of wages, and the working time variable may not be a perfect measure. Further, age is used instead of experience, and within each teacher category there is no indicator for which wage scale the teacher belong to, and there is no indicator for certification status. Finally, there are some deviations from the expected wage given the wage scales. That the R^2 is lower for upper secondary teachers could reflect that the deviations from the scales are more common at this level.

Further, Figure 5 shows that background variables are less successful in explaining the variation in earnings over time for compulsory teachers and the reference group. For example, in 2000 the explanatory value for compulsory teachers is 36.7%, while the corresponding figure in 1995 is 40.8% and in 1991 43.9%. No clear pattern can be seen for upper secondary teachers.

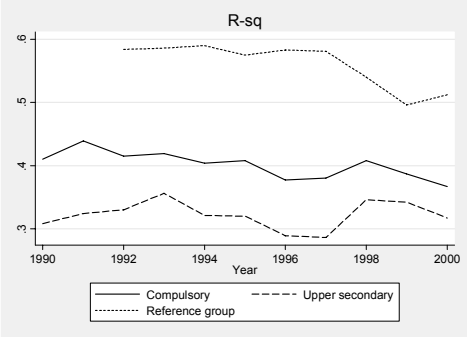

Figure 5. R^2 from regression (1) for compulsory teachers, upper secondary teachers, and the reference group during the 1990s.

In the two teacher groups, R^2 increases between 1997 and 1998. This is due to a better measure of working time. In Appendix B, Figure B1 shows the R^2 from regression (1), now unconditional on working time. The pattern holds; there is a downward trend in the late 1990s, for all groups.

Hence, the explanatory value of standard covariates in the teacher groups decreases, and even if this trend is most pronounced for the later part of the 1990s, it is hard to argue that this is a reform effect, since the same pattern can be observed in the

reference group. The result is in line with the findings in leGrand *et al* (2001) and Gustavsson (2006), that is, human capital variables perform worse in explaining the variation in wages at the end of the 1990s compared to early 1990s.

When introducing individual wage bargaining there is scope for divergence in the wage-setting due to differences in demand by teacher type. For example, it is well known that the shortage of teachers in math and natural science in Sweden is severe. There is also scope for divergence between schools due to different local labour market situations. Hence, there are reasons to believe that subject field and schools to a larger extent can explain the variation in earnings. Hence, first subject field dummies and then school dummies are included additively to the teacher regressions. In each step, the additive value of the R^2 is calculated as a measure of the explanatory value of subject field and school. The results show that the explanatory value of subject field decreases in both teacher categories. For upper secondary teachers, subject field dummies added 6% of explanatory value in 1991, and in 2000 it was only 2%. The explanatory value of school dummies is stable over time, around 3-4% for upper secondary teachers and slightly above 4% for compulsory teachers.

So how do we interpret the results? The explanatory value of the model declines, supporting the argument that unobserved individual characteristics to a larger extent is responsible for the variation in earnings. Unobserved individual characteristics could include motivation, social commitment and bargaining ability. However, there is no indication that the reform made unobserved characteristics more important, since there was a decline already prior to the reform, and further, the decline is similar in the reference group. Furthermore, the argument that individual wage bargaining should increase differences between subject fields or schools are not supported by the results.

4.4 The returns to individual characteristics

In this section, the estimated coefficients associated with age, gender, immigrant status, education and certification from the cross-section regressions are analysed. The regressions are more or less identical to equation (1); the only difference is that an indicator for working in the private school sector and a dummy for being certified have been added to the teacher regressions.

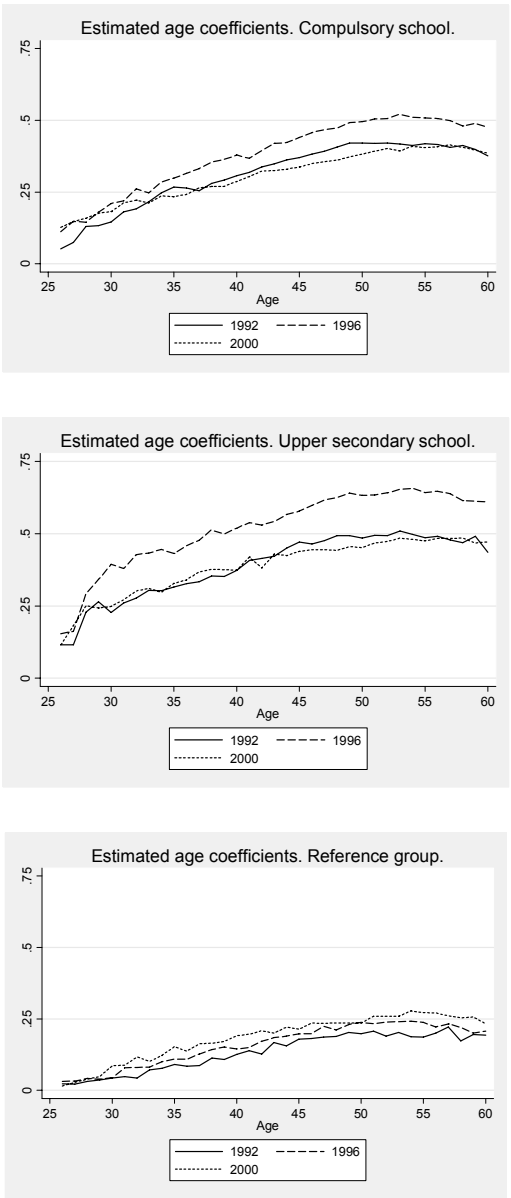

Figure 6. Age coefficients estimated from log earnings regressions in 1992, 1996, and 2000, for compulsory teachers (top), upper secondary teachers (middle), and the reference group (bottom).

The estimated age coefficients are displayed in Figure 6. The excluded category consists of individuals being 25 or younger. The interpretation of the coefficients is hence how much more an individual with given age earns (in log earnings) compared to the excluded category, conditional on the covariates.

The first thing to note is that the age profile in 1992 is clearly steeper in the teacher categories than in the reference group, confirming the pattern of the age-earnings profiles in Figure 3. An upper secondary teacher older than 50 earns about 50% more than a 25 year old, while in the reference group the figure is about 20%.

Between 1992 and 1996 not much changes in the three groups. If anything, there is an indication of the profiles becoming somewhat steeper in all groups.

However, between 1996 and 2000 there are big differences between groups. The teacher profiles become substantially less steep in 2000, while the profile in the reference group becomes even steeper. A 50 year old compulsory teacher earned 50% more than a 25 year old in 1996, and in 2000 the difference was 38%. Hence, the earnings differential between age groups is clearly smaller in 2000 than in 1996.

Also for upper secondary teachers the age profile becomes less steep. Quite a lot of the action seems to occur between the ages of 25 and 30. A 30 year old in 1996 earns 40% more than a 25 year old, while in 2000 it is only a difference of 24%. Returning to the issue of entry-wages below, one can still conclude that the differences between age groups are smaller in 2000 compared to 1996. The difference between a 30 and a 50 year old upper secondary teacher is in 1996 24%, and in 2000 20%.

In the reference group, the profile is getting slightly steeper between 1996 and 2000, indicating greater differences between age groups. However, the changes are small and earnings are quite evenly distributed over the age profile compared to the teacher groups. More importantly, it does not seem that the pattern observed in the teacher groups pick up a general trend. Abolishing the wage scales made the age-earnings profile less steep for teachers, arguably a reform effect since the opposite pattern is observed in the reference group.

Table 3 displays the cross-section coefficients of female, immigrant status, education and certification from the same earnings regressions. For compulsory teachers the female coefficient is -0.138 in 1990, and -0.094 in 2000. Also for upper secondary teachers the gender earnings gap gets smaller, -0.153 in 1990, and -0.093 in 2000. The earnings gap between men and women are, all else equal, not as large in 2000 as in 1990. However, it does not seem to be due to the reform, since it is a trend over the full time period, and is also observed in the reference group, where the female coefficient is -0.234 in 1992, and -0.169 in 2000. These results, diminishing gender differentials in earnings are worth discussing. Gustavsson (2006) has documented a slight increase in the gender wage gap (11.3% in 1992 and 12.8% in 2001) for the same time period with a similar sample of public employees. The difference may be attributable to the samples being slightly different, but it can also reflect that women are working more in 2000

than in 1990. That is, the labour market conditions changed fundamentally during the 1990s. In the recession of the early 1990s women were to a larger extent going in and out of the labour force. Säve-Söderbergh (2003) finds that individual bargaining increased the gender wage gap, a result not consistent with these findings.

The earnings differential between natives and immigrants hovers around 3-7% for all groups during the decade, and does not seem to be affected by the change in bargaining structure. If anything, an increase in earnings differentials is observed in the reference group for the last two years (-5.4% in 1998 and -7.9% in 2000) that can not be seen in the teacher groups.

The excluded educational attainment category is those with at most upper secondary education. In the teacher regressions a control for being certified is included. It should be remembered that being certified implies at least a university degree. Hence, the "ordinary" university premium lies partly in the certification indicator. The reason for including certification is to distinguish between those who are highly educated, but within a field other than teaching, from those who have pedagogical education.

The university premium at the compulsory level is typically around 3-4% from 1991 to 1999, among those not certified. For 2000 it deceases to 1.5%. Being certified implies higher earnings. For most of the time period the extra effect on earnings of being certified at compulsory level is well above 25%, but falls back to 23.2% in 2000. The results indicate that both the returns to education and certification fall at the end of the time period.[23]

The pattern is similar for upper secondary teachers, where the educational premiums fall back during the second half of the decade. In 1997, the university premium is 6.0% and the certification premium is 23.3%. In 2000 the corresponding figures are -0.1% (insignificant) and 19.0%.[24] In the reference group the returns to education falls during the first half of the decade, but increases in the later part. The university premium increases from 19.3% in 1996 to 22.6% in 2000. This result is consistent with Gustavsson (2006), where the university premium is shown to be higher in 2001 than in 1992.

[23] The university premium at compulsory level (without certification in the regression) follows the same pattern as in Table 3. It falls from 35% in 1990 to 26 % in 1992. Then it remains fairly constant until the end of the time period where it drops to 20% in 2000.

[24] The university premium without controlling for certification at upper secondary level drops from 25% in 1997 to 14% in 1998.

Table 3. Cross-section estimates.

	Compulsory school					
	Female	Immigrant	University	PhD	Certified	N
1990	-0.138*	-0.053*	0.069*	0.049	0.301*	83 182
	(0.002)	(0.006)	(0.012)	(0.040)	(0.010)	
1991	-0.134*	-0.033*	0.047*	0.002	0.287*	84 074
	(0.002)	(0.005)	(0.009)	(0.034)	(0.008)	
1992	-0.131*	-0.062*	0.048*	0.119*	0.240*	83 853
	(0.002)	(0.004)	(0.010)	(0.029)	(0.008)	
1993	-0.125*	-0.054*	0.031*	0.062	0.248*	80 627
	(0.002)	(0.004)	(0.011)	(0.035)	(0.008)	
1994	-0.124*	-0.046*	0.040*	0.094*	0.277*	80 807
	(0.002)	(0.005)	(0.011)	(0.034)	(0.008)	
1995	-0.122*	-0.060*	0.032*	0.085*	0.256*	83 138
	(0.002)	(0.004)	(0.010)	(0.036)	(0.007)	
1996	-0.107*	-0.071*	0.021*	0.065	0.246*	82 008
	(0.002)	(0.005)	(0.010)	(0.034)	(0.007)	
1997	-0.102*	-0.057*	0.028*	0.072*	0.270*	82 396
	(0.002)	(0.005)	(0.010)	(0.035)	(0.006)	
1998	-0.098*	-0.057*	0.038*	0.050	0.267*	85 312
	(0.002)	(0.005)	(0.009)	(0.039)	(0.005)	
1999	-0.094*	-0.065*	0.035*	0.070	0.260*	84 254
	(0.002)	(0.005)	(0.008)	(0.040)	(0.006)	
2000	-0.094*	-0.068*	0.015*	0.015	0.232*	86 812
	(0.002)	(0.005)	(0.007)	(0.043)	(0.005)	
	Upper secondary school					
	Female	Immigrant	University	PhD	Certified	N
1990	-0.153*	-0.026*	0.073*	0.255*	0.185*	24 831
	(0.004)	(0.009)	(0.024)	(0.025)	(0.016)	
1991	-0.155*	-0.046*	0.016	0.215*	0.172*	26 048
	(0.004)	(0.009)	(0.018)	(0.020)	(0.013)	
1992	-0.139*	-0.059*	0.079*	0.287*	0.179*	26 000
	(0.004)	(0.009)	(0.020)	(0.022)	(0.013)	
1993	-0.126*	-0.058*	0.042*	0.255*	0.241*	25 787
	(0.004)	(0.009)	(0.019)	(0.021)	(0.013)	
1994	-0.132*	-0.054*	0.019	0.219*	0.275*	27 359
	(0.004)	(0.009)	(0.017)	(0.020)	(0.011)	
1995	-0.120*	-0.041*	0.051*	0.261*	0.207*	28 243
	(0.004)	(0.009)	(0.015)	(0.019)	(0.009)	
1996	-0.119*	-0.054*	0.061*	0.253*	0.213*	27 671
	(0.004)	(0.009)	(0.015)	(0.018)	(0.009)	
1997	-0.113*	-0.045*	0.060*	0.254*	0.233*	28 363
	(0.004)	(0.009)	(0.014)	(0.019)	(0.009)	
1998	-0.099*	-0.039*	-0.001	0.181*	0.195*	29 142
	(0.004)	(0.008)	(0.013)	(0.017)	(0.008)	
1999	-0.091*	-0.063*	0.003	0.177*	0.173*	26 231
	(0.004)	(0.008)	(0.013)	(0.018)	(0.008)	
2000	-0.093*	-0.044*	-0.013	0.150*	0.190*	26 910
	(0.004)	(0.008)	(0.012)	(0.018)	(0.008)	

	Reference group				
	Female	Immigrant	University	PhD	N
1992	-0.234*	-0.038*	0.206*	0.562*	30 581
	(0.004)	(0.007)	(0.003)	(0.017)	
1993	-0.236*	-0.035*	0.202*	0.579*	30 528
	(0.004)	(0.007)	(0.003)	(0.016)	
1994	-0.214*	-0.044*	0.204*	0.575*	29 675
	(0.004)	(0.007)	(0.003)	(0.017)	
1995	-0.215*	-0.038*	0.196*	0.581*	29 913
	(0.004)	(0.007)	(0.003)	(0.018)	
1996	-0.213*	-0.043*	0.193*	0.585*	29 387
	(0.004)	(0.007)	(0.003)	(0.017)	
1997	-0.201*	-0.054*	0.200*	0.590*	29 874
	(0.004)	(0.007)	(0.003)	(0.016)	
1998	-0.205*	-0.054*	0.206*	0.572*	28 499
	(0.004)	(0.008)	(0.003)	(0.016)	
1999	-0.182*	-0.081*	0.224*	0.610*	30 664
	(0.005)	(0.008)	(0.003)	(0.016)	
2000	-0.169*	-0.079*	0.226	0.598*	31 873
	(0.004)	(0.008)	(0.003)	(0.015)	

Note: The outcome variable is log earnings. All regressions include a constant and controls for working time, age, marital status, number of children, and municipality. The teacher regressions also include an indicator for working in a private school. Robust standard errors are in parenthesis. Significance at * = 5%-level.

The results in Table 3 show decreasing returns to education and certification at compulsory level from 1990 until 2000, and at upper secondary level from 1998.[25] It is interesting to note that not only the university premium decreases, also the differences between certified and not certified teachers decreases. It is hard to argue that the observed pattern should be a reform effect. But at least, it indicates that the introduction of individual bargaining has not increased the returns to education and certification.

The reference group is not ideal as a comparison group. A couple of robustness checks have been performed in order to investigate whether the results are sensitive to composition, or changes in composition. First, occupational dummies are included in the reference group regression. The baseline results presented above considers the reference group as a representative cross-section of public employees. Hence, there is no distinction between employees within this group. It could be argued that one should

[25] The result is not driven by the change in the quality of the working time variable. Results from regressions without controlling for working time show the same pattern. That is, decreasing returns to education throughout the time period at the compulsory level, and during the late 1990s at the upper secondary level. Further, the same pattern is observed when restricting the sample to include only full time workers.

correct for the occupational heterogeneity, especially since this composition may change over time. Regressions are performed with occupational dummies included. On average, the explanatory value of the model increases by 1-2 percentage points, and the estimated coefficients are virtually unchanged.[26]

Second, it could be the case that there is coefficient heterogeneity in the model. If there is an interaction in the returns to two explanatory variables, and the composition changes, this is not corrected for unless the regressions are weighted. Since the composition differs most in educational attainment (and also have different time trends) coefficient heterogeneity can be expected to have most impact in this dimension. Therefore, in Appendix C, results are presented with weighted reference group regressions. Weights are constructed as follows. First, each data is divided into six cells by gender and educational attainment. Then the share of individuals in each cell is calculated. The ratio of corresponding cells in compulsory teacher data and reference group data are then used as weights. The main conclusion is that weighting the reference group does not change the main result. For example, the estimated age coefficients are virtually unchanged between 1992 and 2000 (Figure C1), and the university premium increases from 19.1% in 1996 to 22.0% in 2000 (Table C1), as compared to 19.3% and 22.6% in Table 3.

4.5 Entry-wages

With support of Figure 3 one can argue that entry-wages have increased. By defining an "entrant" indicator, one can further investigate whether the entry-wages have changed over time. An individual who has graduated within the last year is defined as an entrant, and this variable is included additively in regression (1). The entrant coefficients are shown in Figure 7.

One can conclude that entry-wages have increased for compulsory teachers since the mid 1990s, an effect that cannot be seen in the reference group. In 1997 being an entrant was associated with around 22.7% lower earnings for compulsory teachers, and in 2000 it was 13.8%, conditional on the covariates. In the reference group, for the same time period, the negative effect associated with being an entrant falls slightly, from 14.7% to 15.2%. Prior to 1997, the two groups had similar trends, but after 1997 they

[26] Results are available from the author upon request.

clearly diverge. Hence, compulsory teacher entry-wages have increased, also conditional on the covariates.[27]

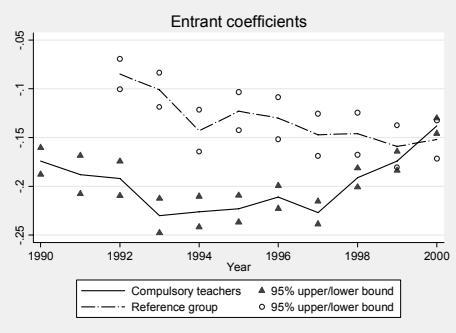

Figure 7. Entrant coefficients estimated from log earnings regressions, for compulsory school teachers and the reference group respectively.

As discussed above, the fact that entry-wages have increased due to individual bargaining probably does not mean that young teachers are better negotiators. It is more likely that it reflects that young individuals have a more elastic labour supply.

5. Conclusions

In Sweden, a country with a high degree of unionization, wage compression peaked in the early 1980s, after decades of centralized bargaining and equality ambitions. Since then, the Swedish labour market has undergone rapid changes, moving towards local wage bargaining. Teachers were one of the last groups adapting individual wage-setting, and a very rigid wage scale system existed until 1996. This paper studies what impact the introduction of individual bargaining had on the earnings structure in the Swedish teachers' labour market. One should note that decentralized bargaining usually is associated with a decrease in union coverage. However, unionization among Swedish teachers has remained high.

The wage scales are shown to have induced a structure with earnings sharply increasing in age. The age-earnings profile prior to the reform was clearly steeper than in a reference group of public employees. It seems that this structure was forced onto the market, because after the reform, entry-wages increased and the age-earnings profile

[27] A corresponding figure for upper secondary teachers shows the same pattern as for compulsory teachers, the level being somewhat lower. For example, being an entrant is associated with a negative effect on earnings with -29.6% in 1990, -29.7% in 1996, and -21.4% in 2000.

became less steep for both compulsory school teachers and upper secondary school teachers.

Further, the scales generated a decreasing age profile of earnings dispersion, since the scales had different entry-wages but the same ceiling. After the reform, dispersion increased for older compulsory teachers, as expected. For upper secondary teachers, on the other hand, no reform effect is observed concerning earnings dispersion. There may be several reasons for this. Three explanations come to mind, all sharing the feature that earnings dispersion to a greater extent already existed for upper secondary teachers. First, responsibilities outside teaching were given extra pay, and extra pay arrangements were arguably more common at the upper secondary level. Second, wage additions existed in order to attract teaching personnel, also a feature more common at the upper secondary level. Third, LR, the union hosting most of the upper secondary teachers, did already in 1992 accept individual wage bargaining. Given their preference for individual wage-setting, deviations from the scales already prior to the reform seem possible.

The explanatory value of ordinary background variables on earnings are decreasing during the 1990s in both teacher categories, as well as in the reference group. This study fails to identify any characteristics that gain in importance. It could be argued that the variation in earnings should increase between subject fields and schools; however, the results do not support this conjecture. The conclusion, hence, is that individual unobserved characteristics gained in importance. These characteristics could include bargaining and social ability, which probably should be rewarded differently with the new scheme.

Finally, the returns to education and certification have gone down among both teachers groups. The aim of the reform was to increase the returns to productive teacher characteristics. However, one should note that the educational premium in the teacher labour market prior to the reform was much higher than in the reference group. Hence, the return to education is after the reform more similar to the reference group. Further, it is also possible that the returns to unobserved productive characteristics have increased.

Two other major changes occurred during the 1990s in the Swedish teachers' labour market. The number of private schools and the share of non-certified teachers increased sharply. However, restricting the analysis of the earnings structure to only

public schools teachers, and certified teachers respectively, produce the same pattern. Hence, the findings in this paper seem not to be driven by these simultaneous changes.

This study clearly shows that the wage scales of the centralized bargaining system imposed a binding constraint on the wage-setting. Teacher type and experience were the only determinants of wages in the scale system, which contributed to the non-flexible labour market of the early 1990s. With individual bargaining, other characteristics constitute the foundation for the wage-setting, and the results suggest that the earnings structure of teachers have become more similar to other parts of the labour market.

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Appendix A: More on earnings dispersion

Figure A1 describes earnings dispersion for compulsory teachers by restricting the sample to include only teachers in the public school sector, and certified teachers respectively.

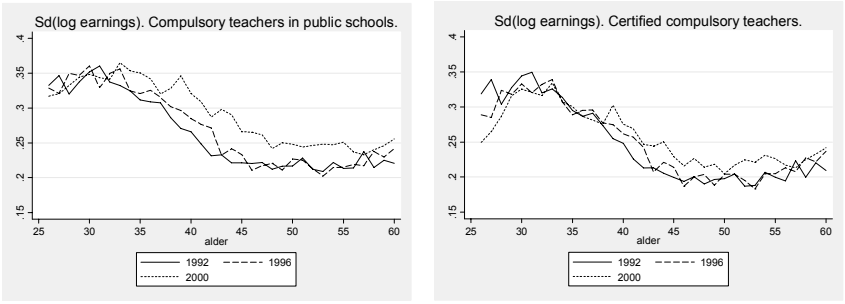

Figure A1. Standard deviation of log earnings (conditional on working time) in 1992, 1996 and 2000 for public school (left), and certified (right) compulsory teachers.

According to Figure A1, it does not seem that the increasing share of teachers working in the private sector is responsible for the increase in dispersion. Excluding teacher in the private sector does not change the result. Excluding non-certified teachers decreases the dispersion in 2000 slightly, which was expected since they are a more heterogeneous group. However, the increase in 2000 is still evident. It should also be remembered that the dispersion in 2000 is underestimated compared to 1992 and 1996, since the quality of the working time variable improves between 1997 and 1998.

Appendix B: R^2 unconditional on working time

Figure B1 shows the R^2 from cross-section regressions described by equation (1). The only difference compared to Figure 5 is that working time is not included as an explanatory variable. By excluding working time, the explanatory value falls, on average, with approximately 20% in the reference group, and around 5% in the teacher categories. There is a clear downward trend at the end of the 1990s. The upward jump in explanatory value in 1998 for the teacher categories observed in Figure 5, is not seen unconditional on working time. This implies that the increases in the explanatory power of the teacher regressions in 1998 is solely due to a better measure of working time, not reflecting that human capital variables perform better in explaining the variation in earnings.

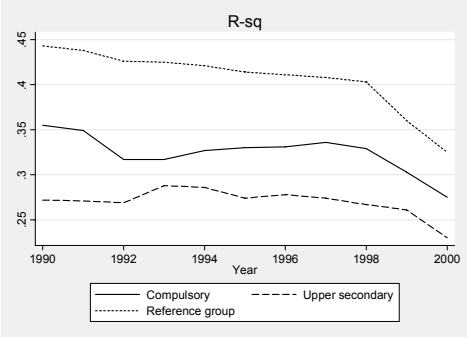

Figure B1. R^2 from regression (1), unconditional on working time, for the three groups from 1990 to 2000.

Appendix C: Weighted reference group regressions

In order to investigate whether the model suffers from coefficient heterogeneity, the reference group regressions are weighted by a vector constructed as follows. Data are divided into six cells constructed by gender and the three educational categories. The proportion of individuals in each cell is calculated. The same procedure is done for compulsory teacher. The ratio of the proportion of individuals in each corresponding cell is used as weight. For example, in 1992 the proportion of female compulsory teachers with a university degree is 96.2%. In the reference group, females with university degree constitute a fraction of 35.8%. That gives a ratio of 0.962/0.358 as weight for females with university degree in the reference group regression in 1992.

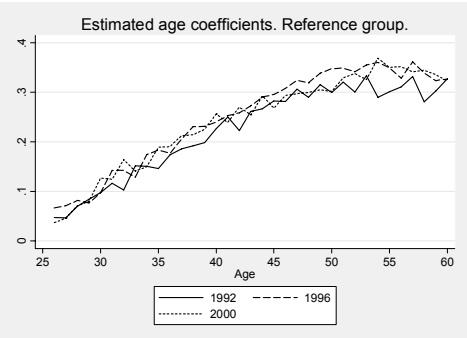

Figure C1. Age coefficients estimated from weighted log earnings regressions in the reference group in 1992, 1996, and 2000.

Figure C1 and Table C1 correspond to the reference group results in Figure 6 and Table 3. The estimated age coefficients in Figure C1 are more or less unchanged between 1992 and 2000. The profiles differ somewhat to the non-weighted version, where the earnings differential between a 50 and 25 year old individual was 25% in 2000. In the

weighted version it is 35%. Still it is substantially smaller than the teacher coefficients, and more importantly, it does not indicate any changes over the years.

The coefficients displayed in Table C1 are very similar to those in Table 3. The female coefficients are slightly larger in magnitude, while the other coefficients are somewhat smaller. Overall, the differences are very small. For example, the female coefficient decreases from -0.247 in 1990 to -0.175 in 2000, compared to -0.234 in 1990 and -0.169 in 2000 for the non-weighted estimation (Table 3).

The overall conclusion is that the model does not suffer from coefficient heterogeneity, at least not in the gender/education dimension.

Table C1. Estimates from weighted reference group regressions.

	Female	Immigrant	University	PhD	N
1992	-0.247*	-0.031*	0.196*	0.534*	30 581
	(0.005)	(0.010)	(0.004)	(0.017)	
1993	-0.248*	-0.022*	0.197*	0.558*	30 528
	(0.005)	(0.010)	(0.004)	(0.017)	
1994	-0.228*	-0.037*	0.202*	0.557*	29 675
	(0.005)	(0.011)	(0.004)	(0.017)	
1995	-0.226*	-0.026*	0.191*	0.564*	29 913
	(0.005)	(0.010)	(0.004)	(0.018)	
1996	-0.226*	-0.034*	0.191*	0.572*	29 382
	(0.005)	(0.010)	(0.004)	(0.017)	
1997	-0.209*	-0.048*	0.198*	0.579*	29 874
	(0.005)	(0.011)	(0.004)	(0.017)	
1998	-0.210*	-0.050*	0.199*	0.549*	28 499
	(0.005)	(0.011)	(0.004)	(0.017)	
1999	-0.186*	-0.072*	0.219*	0.594*	30 664
	(0.006)	(0.010)	(0.004)	(0.018)	
2000	-0.175*	-0.075*	0.220*	0.578*	31 873
	(0.006)	(0.011)	(0.004)	(0.016)	

Note: The outcome variable is log earnings. The weighting procedure is done by the command "aweight" in Stata, see StataCorp. (2005). The regressions include a constant and controls for working time, age, marital status, number of children, and municipality. Robust standard errors are in parenthesis. Significance at *=5%-level.

Essay IV

Do Unemployment Benefits Increase Unemployment? - New Evidence on an Old Question[1]

1. Introduction

Whether the provision and generosity of unemployment insurance (UI) increase unemployment has been the subject of much research.[2] Theory generally predicts that unemployment will rise in response to an increase in UI generosity. However, the empirical evidence is not as unequivocal as the theory suggests.

There are a number of studies using micro data to identify the effects of UI generosity for those already unemployed; Meyer (1995) surveys the most convincing experimental evidence. But the provision of UI affects other margins as well. In addition to affecting search behavior, UI may affect, e.g., wage-setting and quitting behavior. In other words, we are most interested in the general equilibrium effects of variations in the generosity of UI.

Aggregate time series data have the potential of capturing general equilibrium effects of benefit generosity. However, the use of aggregate data creates severe identification problems. This may be part of the explanation for the fact that the estimated effects are much smaller than one would think based on theory. Now, what "one would think based on theory" is usually based on models where UI is equivalent to the "wage" during unemployment. Most empirical specifications are also derived from this simple model. Of course, real-world UI systems are much more complex and modeling their institutional features may yield different conclusions – a point forcefully made by Atkinson & Micklewright (1991).

The use of data over countries or regions, observed at different points in time, is presumably a more promising way to estimate the equilibrium effects of variations in UI benefit generosity. The prototypical US study in this vein (e.g. Katz & Meyer, 1990) uses policy changes at the state level to identify the effects. However, this approach can

[1] Written with Peter Fredriksson.
[2] See Holmlund (1998), Krueger & Meyer (2002), and Fredriksson & Holmlund (2005) for recent reviews of the literature.

be criticized because policy changes at the state level are endogenous with respect to the local cycle; see Card & Levine (2000), and Lalive & Zweimüller (2004).

We also use regional panel data. However, the approach to identification is different and, to our knowledge, novel. The source of variation comes from a nationally determined policy. We exploit the fact that in most real-world UI systems there is a ceiling on the amount of benefits received.[3] This ceiling comes from the fact that there is a cap on income which is used to calculate the actual benefit received; increases in income above the cap produce no increase in the actual benefit.[4] Coupled with the fact that there are well-known regional wage differentials within countries, this implies that the *actual generosity* of UI varies regionally. More importantly, it will vary within region over time because changes in the ceiling produce regional variations in generosity depending on whether the region is above and/or below the ceiling before and after the policy change; moreover, differences in regional wage growth yield regional variation in actual generosity for a given national ceiling.

The fact that the level and changes in the regional wage may produce changes in the actual generosity of UI is, as such, not that useful. Regional wages and wage growth are endogenous with respect to regional unemployment. The challenge is therefore to find a strategy for constructing measures of predicted wages which are plausibly exogenous to local unemployment. Given an exogenous predicted wage, variations in the ceiling will produce differential changes in the actual generosity of UI depending on whether the region is predicted to be above or below the wage cap.

This empirical strategy is implemented using Swedish data during 1974-2002. To generate predicted wages we exploit individual data. For each individual and time point we estimate what the wage would be if his or her characteristics were priced on the national labor market. We then calculate the UI benefit and the actual replacement rate (given the estimated wage) should this individual become unemployed. Finally, the measures of UI generosity are aggregated to the regional level and related to regional unemployment. Notice that the non-linearity of the benefit schedule – induced by the benefit ceiling – implies that the unemployment effect of changes in the actual

[3] In the US, the maximum benefit amount even varies by state (Krueger & Meyer, 2002).
[4] Carling *et al* (2001) use a similar approach, albeit applied to micro data, when examining whether unemployment duration is affected by variations in UI generosity. They use the fact that because of the benefit ceiling some benefit recipients are treated with a policy change while others are not.

generosity of UI is identified even if we hold predicted wages and other labor force characteristics constant.[5]

Whether unemployment responds to changes in UI benefit generosity is one of the classic questions in labor economics that dates back to, e.g., Pigou (1932). The policy relevance of this question should thus be clear. But there is an additional reason to re-examine the issue: the design of the national unemployment insurance system has implications for the regional unemployment distribution because of the ceiling in benefit receipt.

It is an empirical fact that regional unemployment differentials are very stable in Europe. Figure 1 illustrates this for regional labor markets in Sweden. It is clear that regions which were high unemployment regions in the mid 1970s are also high unemployment regions in the beginning of the 2000s, and vice versa; the regression line has a slope of 0.91 with at t-value of 5.3. The benefit ceiling implies that UI is more generous in high unemployment/low wage regions, a fact that further increases the spread of regional unemployment differentials. Therefore, it is interesting to examine whether (and how much) a more "neutral" design of the UI system – one that has no benefit ceiling – would reduce the dispersion of unemployment across regions.

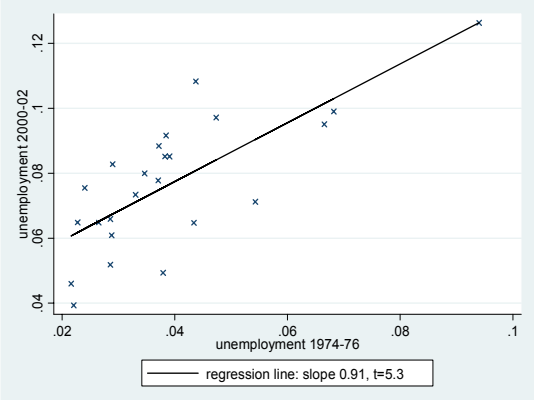

Figure 1. Regional unemployment persistence. "Unemployment" is defined as the sum of the openly unemployed and participants in active labor market programs as a share of the labor force. Sources: Labor Force Surveys and National Labor Market Board

The remainder of the paper is outlined as follows. Section 2 presents a simple model of regional unemployment that we use for specification and interpretation purposes.

[5] Later on we will illustrate that the aggregate movements in the benefit ceiling are more or less idiosyncratic.

Section 3 describes the Swedish institutional setting. Section 4 presents the data and our empirical strategy. We use individual data to calculate measures of the composition of the regional labor force. We also use the individual data to estimate earnings regressions which are used to generate individual expected wages and measures of UI generosity at the individual level. These measures are then used to generate a measure of the actual generosity of UI which is independent of the regional state of the labor market. Section 5 illustrates the identification strategy further. In particular we ask the question: What variation identifies the actual replacement rate? Section 6 presents the estimation results. In Section 7 we conduct two policy experiments to simulate the effects of UI policies on aggregate unemployment and the distribution of unemployment across regions. First we remove the benefit ceiling while holding the nominal replacement rate fixed. Then we raise the nominal replacement rate with the wage cap still in place. Section 8 concludes.

2. A simple model

We want to use this model as a guide for thinking about how a national UI policy may affect regional unemployment and how this is useful for identification purposes.

To model local wage determination we opt for a model involving search frictions and individualistic wage bargaining.[6] Assuming risk neutrality on the part of workers and firms, most kinds of decentralized bargaining models yield a wage equation of the following kind:

$$w_{ijt}(x) = \beta y_{jt}(x) + (1-\beta) O_{jt}(x) \qquad (1)$$

where x denotes the (exogenous) characteristics of the worker involved in the bargain, y labor productivity, and O the outside option, i.e., the flow value of unemployment; see Pissarides (2000) for instance. The weighting parameter, β, reflects worker bargaining power, i indexes the bargaining unit, j the regional labor market, and t time. Thus, according to eq. (1), the bargained wage is a weighted average of inside (y) and outside opportunities (O). We take outside opportunities to be given by:

[6] We could equally well have modeled local wage determination as the outcome between a local union and a firm, but it is more convenient to have a model where we can think of firms as having only one job slot.

$$O_{jt}(x) = \mu[n_{jt}w_{jt}(x) + u_{jt}b_{jt}(x)] + (1-\mu)[n_{-j,t}w_{-j,t} + u_{-j,t}b_{-j,t}(x)] \qquad (2)$$

where u (n) denotes the un(employment) rate, and b the unemployment benefit; notice, that the unemployment benefit depends on the characteristics of the worker because there is a ceiling on benefit receipt. The index "*-j*" denotes aggregates over regions excluding j and μ is a measure of the allocation of search across regions. In equation (2) we have invoked the simplifying assumption that worker search intensity is fixed; coupled with an assumption that the separation rate is independent of x this implies that the un(employment) rate is independent of x.[7] Thus, the opportunities outside the firm are given by a weighted average of the opportunities inside and outside the region respectively. If the regions are small we can equally well write equation (2) as:

$$O_{jt}(x) = \mu[n_{jt}w_{jt}(x) + u_{jt}b_{jt}(x)] + (1-\mu)[n_t w_t(x) + u_t b_t(x)] \qquad (3)$$

where the absence of a regional subscript signifies national aggregates. Conditional on the characteristics of the bargaining pair, the outcome of the wage-bargain is symmetric. Hence, $w_{ijt}(x) = w_{jt}(x)$. Inserting (3) into (1) and imposing symmetry we get

$$w_{jt}(x) = \frac{\beta y_{jt}(x) + \mu(1-\beta)u_{jt}b_{jt}(x) + (1-\beta)(1-\mu)O_t(x)}{1-\mu(1-\beta)(1-u_{jt})} \qquad (4)$$

where we have introduced the notation $O_t(x) = n_t w_t(x) + u_t b_t(x)$ and used $n_{jt} \equiv 1 - u_{jt}$. This is the regional wage equation for a worker-firm pair where workers have characteristics x.

There is undirected search on the part of workers and firms. Hence, when posting a vacancy firms do not know what kind of worker they will meet – the decision to enter the market is based on the average productivity and the average wage in the region. Firms must make an up-front capital investment in order to open up a vacancy. This capital investment commands a flow cost of K. Upon making this capital investment the

[7] We acknowledge that having search intensities fixed, μ constant, and separation rates independent of x, may be short-cuts for which there is little justification except tractability. Notice that our empirical work does not rely on these assumptions.

firm opens a vacancy, which is filled with probability $q_{jt} = q(\theta_{jt})$ where $\theta_{jt} \equiv v_{jt} / u_{jt}$ and $q'(\cdot) < 0$. Thus, vacancy posting behavior satisfies a zero-profit condition

$$\frac{y_{jt} - w_{jt}}{\varphi} = \frac{K}{q(\theta_{jt})} \qquad (5)$$

where $1/\varphi$ is the expected duration of the match. In equation (5), we do not index, e.g., the wage by x since the relevant quantity for a firm's entry decision is the wage averaged over the distribution of observed characteristics in the region: $w_{jt} = \int w_{jt}(x) dG_{jt}(x)$, where $G_{jt}(x)$ denotes the distribution of x in region j at time t. Equation (5) then says that the expected present value of the match (the left-hand side) equals the expected present value of the set-up cost (the right-hand side).[8]

To proceed, let us assume that realized productivity (i.e. the productivity realized after the match) is given by

$$y_{jt}(x) = \gamma_t x + \lambda_j + \lambda_t + \varepsilon_{jt} \qquad (6)$$

where λ_j is a region-specific effect, λ_t a time-specific effect, and ε_{jt} a region-specific shock. Ex ante productivity (i.e. prior to the match) is given by

$$y_{jt} = \gamma_t \int x dG_{jt}(x) + \lambda_j + \lambda_t + \varepsilon_{jt} \qquad (7)$$

Finally, we note that the unemployment benefit for a worker of type x is given by

$$b_{jt}(x) = \rho_t^n \left[w_{jt}(x) I(w_{jt}(x) \le w_t^{cap}) + w_t^{cap} I(w_{jt}(x) > w_t^{cap}) \right] \qquad (8)$$

where ρ_t^n is the nominal replacement rate, $I(\cdot)$ the indicator function, and w_t^{cap} the cap on earnings used to calculate UI benefits.

Let us consider the average unemployment benefit received by workers in region j at time t upon unemployment. This equals

[8] Notice that we have implicitly assumed that there is no discounting when specifying equation (5). This assumption can also help rationalizing equation (2).

$$b_{jt} = \rho_t^n[(1-\psi_{jt}) + \psi_{jt}\frac{w_t^{cap}}{w_{jt}}]w_{jt} = \rho_{jt}^a(\rho_t^n, \psi_{jt}, w_t^{cap}, w_{jt})w_{jt} \qquad (9)$$

where ψ_{jt} is the fraction of workers above the earnings cap and $\rho_{jt}^a(\cdot)$ is *the actual replacement rate*.

Notice that here we define the actual replacement rate as the ratio between average benefits and average regional wages. From an institutional point of view, it would have been more accurate to calculate the actual replacement rate for an individual worker, since unemployment benefits are usually tied to the wage of the individual worker.[9] However, this added realism would have come at substantial loss of tractability. This approach is simpler and we do not think it affects any qualitative conclusion.

Before solving the model, it is instructive to consider the determinants of the actual replacement rate. Since

$$\rho_{jt}^a = \rho_t^n[(1-\psi_{jt}) + \psi_{jt}\frac{w_t^{cap}}{w_{jt}}]$$

it is straightforward to verify that

$$\frac{\partial \rho_{jt}^a}{\partial \rho_t^n} = [(1-\psi_{jt}) + \psi_{jt}\frac{w_t^{cap}}{w_{jt}}] > 0$$

$$\frac{\partial \rho_{jt}^a}{\partial w_t^{cap}} = \rho_t^n\{\psi_{jt}\frac{w_t^{cap}}{w_{jt}} + \frac{\partial \psi_{jt}}{\partial w_t^{cap}}[(w_t^{cap}/w_{jt}) - 1]\} \geq 0$$

where $\partial\psi_{jt}/\partial w_t^{cap} < 0$, i.e., if the wage cap increases the share above the ceiling is reduced. Consider comparing the magnitudes of these derivatives in two extreme regions: one where everyone has wages below the wage cap - $\psi = 0$ - and another

[9] In our empirical work we will calculate the actual replacement rate at the individual level and aggregate this measure to the regional level.

where every wage is above the ceiling, i.e., $\psi = 1$. Evaluating the derivatives at these two extreme points we have

$$\left.\frac{\partial \rho_{jt}^{a}}{\partial \rho_{t}^{n}}\right|_{\psi=0} = 1 > \left.\frac{\partial \rho_{jt}^{a}}{\partial \rho_{t}^{n}}\right|_{\psi=1} = \frac{w_{t}^{cap}}{w_{jt}}$$

$$\left.\frac{\partial \rho_{jt}^{a}}{\partial w_{t}^{cap}}\right|_{\psi=1} = \rho_{t}^{n}\frac{w_{t}^{cap}}{w_{jt}} > \left.\frac{\partial \rho_{jt}^{a}}{\partial w_{t}^{cap}}\right|_{\psi=0} = 0$$

In other words, there are interaction effects in the model. Changes in the ceiling will increase generosity more in high-wage regions than in low-wage regions, while a change in nominal replacement rate will have the opposite effect.

Now, let us solve the model. Equations (6)-(9) imply that we can write the average regional wage as

$$w_{jt} = \frac{\beta y_{jt} + (1-\beta)(1-\mu)O_{t}}{1-\mu(1-\beta)(1-u_{jt}(1-\rho_{jt}^{a}(\cdot)))} \qquad (10)$$

To complete the model we need an equation characterizing the flow equilibrium in the regional labor market. Equating the outflow from employment ($\varphi(1-u_{jt})$) with the inflow into employment ($\mu\alpha(\theta_{jt})u_{jt} + (1-\mu)\alpha(\theta_{t})u_{jt}$) yields the relationship $u_{jt} = u(\theta_{jt})$.[10] Unemployment is decreasing in market tightness (θ) since the job offer arrival rate (α) increases in tightness (i.e. $\alpha'(\cdot) > 0$). It is convenient to invert the flow equilibrium condition (i.e. $\theta_{jt} = \theta(u_{jt})$) and use it to eliminate θ in equation (5). We get

$$\frac{y_{jt} - w_{jt}}{\varphi} = \frac{K}{q(\theta(u_{jt}))} \qquad (5')$$

[10] This flow equilibrium is consistent with the assumption that mobility occurs only when a job has been found. In flow equilibrium, the inflow into employment from other regions must be balanced by an equal-sized outflow from the region under consideration (which motivates the second term in the expression for the inflow into employment).

Conditional on the state of the national market, equation (5') and equation (10) yield two equations in two unknowns: w_{jt} and u_{jt}.

The comparative statics with respect to the parameters of the UI system are fairly straightforward. An increase in the generosity of UI raises regional wage pressure (holding unemployment constant) and eventually increases unemployment by virtue of the zero-profit condition. Hence, we have $(\partial u_{jt} / \partial \rho_t^n) \geq 0$ and $(\partial u_{jt} / \partial w_t^{cap}) \geq 0$. From an empirical point of view, however, these predictions are not that helpful. If we control flexibly for time (by introducing time dummies in the empirical specification) it will not be possible to identify these effects. For empirical work, it is more useful to note the sign of two interaction effects. First of all, the effect of an increase in the nominal replacement rate will be greater in a low-wage region than in a high-wage region; in particular $(\partial u_{jt} / \partial \rho_t^n)\big|_{\psi=0} > (\partial u_{jt} / \partial \rho_t^n)\big|_{\psi=1} > 0$. In other words, the variation in the statutory replacement rate is less relevant in a region where the wage is higher (i.e. the share above the ceiling is higher). Second of all, the effect of increase in the benefit ceiling will be greater in a high-wage region than in a low-wage region; in particular $(\partial u_{jt} / \partial w_t^{cap})\big|_{\psi=1} > (\partial u_{jt} / \partial w_t^{cap})\big|_{\psi=0} = 0$. The sign of these two interaction effects follows from the properties of the actual replacement rate derived above.

To make full use of these predictions we must, of course, take account of the fact that wages (and hence the share above the benefit ceiling) are endogenous to unemployment. More specifically, the concern is that the region-specific shock in labor productivity (ε_{jt}) will spill-over onto unemployment as well as wages. Since the shock has an effect on the regional wage, it will have an effect on the actual replacement rate. In section 4, we outline how we try to eliminate this simultaneity problem.

3. The Swedish institutional setting

The "Swedish model" is a frequently used term for describing institutions in the Swedish labor market. The Swedish model featured centralized collective wage bargaining and extensive use of active labor market policy.

Given the (historical) reliance on centralized bargaining one might ask if the preceding model is a relevant characterization of the Swedish labor market. However,

even during the heydays of the Swedish model, there was bargaining at different layers. There has always been additional wage drift at the local level, which constitutes a substantial fraction of the aggregate wage increase. Historically, wage drift at the local level accounted for 45 percent of total wage increases (Nilsson, 1993); between 1997 and 2002 wage drift amounted to 31 percent of the total increase.[11]

Wage-setting institutions have changed rather drastically over the past couple of decades.[12] Centralized bargaining started to crumble in the beginning of the 1980s (Edin & Holmlund, 1995). During the 1990s, there was also a substantial move towards decentralization of wage negotiations. This started in the beginning of the 1990s, when some central agreements for white-collar workers in the private sector neither contained total wage increases nor minimum wage increases. It was entirely up to the employer and the employee to determine the wage; see Lindgren (2005). This trend towards decentralization has resulted in only 7 percent of the employed having their wages completely determined by the central industry bargain in 2004; moreover, the norm in the public sector is individualistic wage determination (Fredriksson & Topel, 2006).[13]

3.1 Unemployment benefits in Sweden

Receipt of unemployment insurance benefits requires the fulfilment of an employment requirement and a membership requirement.[14] The duration of UI receipt is formally 60 weeks. As explained above, UI benefits replace a fixed fraction (currently 80 %) of previous earnings up to a ceiling.

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[11] This figure comes from the business cycle statistics reported by Statistics Sweden. Incidentally, it is not obvious how one should define wage drift since the early 1990s. During the 1990s, decentralized or individualistic bargaining has become increasingly common; see below.

[12] Despite these changes, unions figure as prominently in the Swedish labor market as they did during the beginning of the 1980s. The unionization rate in Sweden has hovered around 80 percent over the past couple of decades (OECD, 2004).

[13] At the same time as there has been decentralization of the wage bargain, a new coordination regime has emerged. In 1997, the so-called Industrial Agreement (IA) was struck between unions and employers in the manufacturing sector. This agreement involves a set of procedural rules, similar in many ways to the laws governing collective bargaining in the US. It stipulates, inter alia, time-tables for negotiations, rules for conflict resolution, and gives a prominent role for mediators. The IA-model may have delivered incentives for wage restraint at the aggregate level. But it is reasonable to think that it has had a minor influence on the regional wage structure, since the main function of the IA is to establish a set of procedural rules of the game.

[14] The information on the UI rules in this section comes from Olli Segendorf (2003). The employment requirement stipulates that the individual must have worked for a certain number of days during the year immediately preceding unemployment. Sweden is one of the few countries were UI is voluntary, hence the receipt of UI also requires the membership in a UI fund for at least 12 months and the payment of a small fee.

For those who do not fulfil the membership requirement there is an Unemployment Assistance (UA) system. Compensation on UA is unrelated to previous earnings and the generosity of UA is much lower than UI; on average it replaces roughly 40 percent of previous earnings.

Since the key aspect of our model is the effect of unemployment insurance on wage-setting, we will simply ignore the UA-system in the sequel. To us, this seems like an innocuous omission: the relevant issue in the wage bargaining framework is the level of benefit entitlement for an average *employed worker* upon unemployment entry.[15]

Another feature of unemployment benefits in Sweden is more relevant in that respect. All collective agreements provide additional compensation for (some) workers in the case of redundancies. Despite their relevance, it is very hard to get the full picture of the conditions and payments involved (Sebardt, 2005, provides very useful information, however).[16]

The redundancy payments regulated by collective agreement may come in two forms: either as a lump-sum severance payment or as a supplementary unemployment benefit. Although lump-sum severance payments may be non-negligible and should affect incentives in the wage bargain, we choose to ignore them here. The main reason for this omission is that eligibility is a function of tenure – which is information that we do not have. Furthermore, for the biggest group having a lump-sum severance payment – public sector workers – the lump-sum is proportional to the previous wage with no ceiling imposed. With this construction, the severance payment does not contribute to identification.[17]

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[15] This is partly the reason for also ignoring the duration of benefit receipt. More importantly, however, benefit duration is unrelated to previous wages and hence do not contribute to identification.

[16] Indeed, Wadensjö (1993) adequately refers to the additional compensation provided by collective agreement as the "unknown part of the social insurance system". The information in the rest of this section relies heavily on Sebardt (2005).

[17] The main agreements providing lump-sum severance pay concern public sector employees and private sector blue-collar workers. For local public sector employees, such constructions have existed since 1984. The severance pay is proportional to the previous wage (with no ceiling). At most the employee can be paid half of their annual earnings. This happens in the case of employment for 18 years in the local public sector. For each year of "tenure" less than 18 years there is a proportional reduction in the lump-sum payment. For blue-collar workers, the severance payment is only a function of tenure and age. A rough description is that only individuals above age 50 qualify; in addition, the worker should have at least 10 years of tenure. The payment is proportional to tenure, but increasing with age for given tenure; see Sebardt (2005). Of course, the existence of severance payments raises the nominal replacement rates for the workers affected by them. Notice that we can to some extent control for the incidence of severance payments by controlling for age and industry composition.

For our whole study period, 1974-2002, there are no supplementary unemployment benefits for the vast majority of workers. Thus, in terms of the periodic unemployment benefit payments, the rules of the public unemployment insurance system apply. There are some notable exceptions, however. Starting in 1990, all central government employees got additional insurance via a collective agreement. Given that the employee has an open-ended contract, or has been on fixed-term contracts for at least three years, there is no benefit ceiling. That is, the employee gets the statutory replacement rate independent of the previous wage.

The oldest collective agreement offering supplementary benefits applies to white-collar workers in the private sector. This has been in place for the entirety of our study period. The supplementary benefit structure is more complex than for government employees. Supplementary benefits are only offered for workers above age 40 who have at least 5 years of tenure. Their basic structure is that workers should be offered an actual replacement rate which is no less than 70 percent. A simple way to think about these payments is thus that they kick-in at a wage equaling the benefit ceiling divided by 0.7.[18]

The final collective agreement offering supplementary unemployment benefits refers to local public sector employees. This agreement was struck in 1984. It features a strict eligibility requirement. It is given only to redundant employees over 45 satisfying a "tenure" requirement. For 45 year-olds the tenure requirement is that they should have worked in the public sector for 17.5 years.[19] Should they qualify for supplementary benefits, they are given a benefit equaling the nominal replacement rate times the previous wage with no ceiling imposed.[20]

[18] This is almost how the system works at present; the complication that we have not mentioned is that for wages above 20 price base amounts the slope of the benefit-wage schedule becomes 0.25. Further, relative to the system that existed during 1974-2002, it is a slight simplification at the bottom end. Those below the wage cap implied by a replacement rate of 70 percent were given a relatively small nominal amount as well; this nominal amount raises the nominal wage replacement rate for those below the wage cap in the public UI system.

[19] The tenure requirement decreases with age: at age 60, 10 years of tenure is required.

[20] On top of all this, some UI funds offer their members the option of purchasing private unemployment insurance. However, this possibility is very recent and hence does not concern us.

4. Data and construction of key variables

We use three principal data sources: (i) LINDA – an individual (register) data base (see Edin & Fredriksson, 2000, for a description); (ii) regional (open) unemployment from the Labor Force Surveys; and (iii) regional active labor market program rates from the Labor Market Board.

The LINDA data set is based on a combination of income tax registers, population censuses, wage data, and other sources. Unfortunately, the wage data are not available for our entire study period. We only have access to wages for a representative sample of workers from 1998 and onwards. Apart from wages, the individual variables we use in our analysis are based on register information. Earnings and some other characteristics (gender, age, education, marital status, and industry affiliation) are obtained from the income tax registers, which also contain information on region of residence and country of birth from the population registers. The earnings information and most of the other individual characteristics are available throughout the time period; see appendix for more information on data availability.

The individual data are used to calculate measures of the composition of the regional labor force and to run individual earnings regressions. The estimated parameters from the earnings regressions are used to generate expected wages had the characteristics of the individual worker been priced at the national labor market. We use this strategy to free the estimates from the simultaneity bias caused by local shocks affecting both regional unemployment and wages. Having generated these expected wages we calculate the average of these wages at the regional level and the actual generosity of UI at the regional level.

4.1 Construction of key independent variables

We start by estimating individual earnings regressions separately by year. These equations have the following structure

$$\ln y_{ijt} = \alpha_t + \alpha_{jt} + \beta_t X_{it} + \varepsilon_{ijt} \qquad (11)$$

where i indexes individuals, j regions, and t time. In equation (11), y denotes earnings, α_{jt} is a region-fixed effect – normalized such that $\sum_j \alpha_{jt} = 0$ – and X denotes the vector

of covariates. The covariate vector includes information on gender, age (separate dummies for each five-year age category) educational attainment, marital status, country of birth, and industry. We run these equations for each year between 1970 and 1998 including only individuals who are 16-59 years of age.[21] We control flexibly for region at the estimation stage to avoid sorting bias in the coefficient vector, β_t; such a bias might arise if high-skilled individuals cluster in regions hit by positive wage shocks. When estimating these equations we exclude the lowest quintile of the earnings distribution. The rationale for this is that we want the parameter estimates to resemble what one gets when estimating traditional wage equations; see Antelius & Björklund (2000).

Using the estimates of the parameters in (11) we want to generate an expected wage – the wage that each individual would obtain if his/her characteristics were priced on the national labor market. Our main strategy to compute such a wage is as follows

$$w_{it}^{e,1} = \delta_{it} - \overline{\delta} + \overline{w}_t \qquad (12)$$

where $\delta_{it} = \exp(\hat{\beta}_{t-4} X_{it})$, $\overline{\delta}$ denotes the mean of δ_{it}, and $\overline{w}_t$ denotes the average wage in the country.[22] Thus, the individual gets assigned the same wage independently of where s(he) is located. We lag the "national price vector", $\hat{\beta}$, four years in order to ensure that the expected wage is independent of any region-specific shocks. Big regions, such has Stockholm, are likely to be very influential in the estimation of $\hat{\beta}_t$. If we would have used $\hat{\beta}_t$ rather than $\hat{\beta}_{t-4}$ a potential worry is that the wage predictions would not have been independent of shocks to unemployment in Stockholm. Another reason for not using $\hat{\beta}_t$ concerns skilled-biased technical change. Suppose there is skilled-bias technical change. This will presumably raise the return to education and will represent a favorable employment shock in regions rich on observed and unobserved human capital. This scenario will induce a negative correlation between the wage prediction and the error-term in the unemployment equation.

[21] The upper age limit is due to the fact that the information on education is only consistently available for individuals less than 60 years-of-age. See appendix for more details.
[22] According to equation (12) we adjust the predictions such that they are mean zero and center them on the mean national wage.

Given a measure of the expected wage, we proceed to define an individual indicator variable for having predicted wages above the wage cap. Moreover, we calculate the actual replacement rate at the individual level as

$$\rho_{it}^{a} = \rho_{t}^{n}\left[I(w_{it}^{e} \leq w_{t}^{cap}) + \left(w_{t}^{cap} / w_{it}^{e}\right) I(w_{it}^{e} > w_{t}^{cap})\right] \qquad (13)$$

We then average over all individuals residing in the region which gives us

$$\rho_{jt}^{a} = \frac{\sum_{i \in j} \rho_{it}^{a}}{N_{jt}}, \; w_{jt}^{e} = \frac{\sum_{i \in j} w_{it}^{e}}{N_{jt}} \qquad (14)$$

where N_{jt} is the number of individuals residing in region *j* at time *t*. ρ_{jt}^{a} is the key independent variable in the empirical analysis.

In equations (13) and (14) we have calculated the actual replacement rates as if only the public UI system is relevant. Obviously, we would also like to take the existence of supplementary unemployment benefits into account. In the next section, we outline how we try to accommodate this feature.

4.2 Supplementary unemployment benefits

Since we do not have adequate information in the data, taking supplementary unemployment benefits into account is bound to involve some approximations. In the data, we observe in what sector the individual works but we do not observe whether the individual is a blue-collar or a white-collar worker. Further, we do not observe tenure for the individual worker.

The supplementary unemployment benefit in the central government sector is fairly straightforward to approximate. Historically, the vast majority of workers in the public sector were on open-ended contracts. Therefore, we simply assume that all workers are eligible for this system from 1990 and onwards. Since this agreement implies that there is no benefit ceiling, we set the actual replacement rate equal to the nominal one from 1990 and onwards for central government workers.

White-collar worker status in the private sector is proxied with workers in the private sector having at least three years of (theoretical) upper-secondary education. The

supplementary benefit was paid to individuals who were at least 40 years-of-age with at least 5 years of tenure in the firm. The question then is: What does the tenure structure look like for white-collar workers in the private sector above 40? To examine this question we used survey data from the Swedish Level of Livings Survey (LNU) in 2000; Erikson & Åberg (1987) describe the LNU data. It turned out that 75 percent of workers in the private sector with at least 3 years of upper-secondary education had tenure of at least 5 years. Therefore, as an approximation we assume that all workers that we classify as private sector white-collar workers are eligible for supplementary benefits if they satisfy the age constraint. The workers that qualify for this supplementary benefit are given the benefit structure outlined in section 3.1, i.e. the actual replacement rate never falls below 0.7.

The final supplementary benefit agreement concerns local government employees. In this case the age constraint is 45 and the "tenure" requirement is almost 18 years. Since this requirement appears very stringent, we have chosen to ignore this agreement altogether.[23]

4.3 Data

There are many steps involved in creating these regional panel data. The full detail of our data collection effort is presented in Appendix A. Here we describe the main steps and present the main characteristics of the data.

We begin by creating a data set involving individual characteristics and earnings from 1970 to 2002. The included individual characteristics are fairly standard. We have information on gender, age, marital status, region of residence (at the county level), educational attainment, industry affiliation (2-digit ISIC), and country of birth. With respect to country of birth we distinguish between individuals of native, Nordic, OECD,

[23] Also, in this case we had a brief look at the LNU data. The "tenure" requirement in the agreement pertains to the total number of years worked in the local public sector. This is not observed in the LNU data. If we look at tenure with the current employer – a reasonable approximation of the number of years of continuous employment in the local public sector – we find that a quarter of those aged 45 are eligible. Eligibility increases with age. At age 50, half of the relevant population is eligible and at age 59 around three quarters are eligible. At any rate, a small share of the population is eligible for this supplementary unemployment benefit and, therefore, we ignore it.

or non-OECD origin.[24] In terms of education, we distinguish between compulsory school (or less), upper secondary school, and tertiary education.

We first utilize these data for estimating individual earnings and wage regressions. On the basis of the estimated equations we generate an expected "wage" for each individual as described above. The mean of the predictions is adjusted such that it corresponds to the national average wage for each point in time. We are implicitly assuming that the estimates of the slope parameters in the earnings regressions are the same as they would be in the wage regressions. This may be a questionable assumption since earnings variations are also due to variations in hours worked. But notice that we trim the lower tail of the earnings distribution to minimize this problem.

Then we also need information on the relevant parameters of the UI system: the benefit ceiling and the nominal replacement rate. The benefit ceiling is specified in nominal terms, so it comes as no surprise that it has been changed frequently. On 20 occasions the ceiling was changed during the time period. One would expect the ceiling to be adjusted according to the rate of wage inflation such that the "insurance value" is left unchanged. However, during most of the time period, the ceiling is changed on the discretion of the legislator and, as we illustrate later, there is a good deal of hap-hazardness introduced by these discretionary changes. The nominal replacement rates have been changed more infrequently. There have been four changes in the nominal replacement rate between 1974 and 2002.

In Figure 2 we plot the evolution of the nominal replacement rate and the wage cap (divided by mean wages) over time at the national level. Along with these two series, we also plot the evolution of the actual replacement rate – unadjusted as well as adjusted for the incidence of supplementary unemployment benefits.

Figure 2 shows that there is a good deal of idiosyncratic variation in the wage cap and that this variation contributes to most of the variation in the actual replacement rate (we substantiate this claim more in the next section). Figure 2 also shows that benefit generosity was scaled back following the unemployment crisis in the beginning of the 1990s.

[24] Individuals are generally classified as being of OECD origin if they were born in a country which was a member of the OECD in 1985. The only exceptions from this rule are Turkey – which is included among the non-OECD countries – and the Nordic countries.

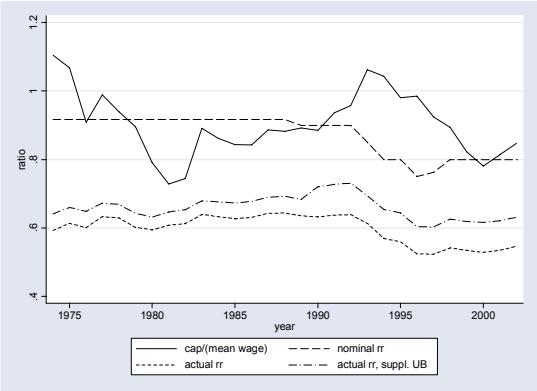

Figure 2. The evolution of nominal and actual replacement rates and the wage cap, 1974-2002. Sources: See data appendix

Our key outcome measure is defined as the sum of open unemployment and participants in labor market programs as a share of the labor force. With some abuse of language we refer to this sum as "unemployment" in the sequel. Figure 3 shows the development of mean unemployment along with the evolution of the extremes in the distribution (the min. and max. values) to give a sense about the regional variation in the data. The most striking event in this figure is the adverse shock that hit Sweden in the beginning of the 1990s. In just three years unemployment shot up from around three percent in 1990 to roughly 13 percent in 1993. The aggregate unemployment rate was stable at this high level until 1997. In some regions, however, unemployment continued to rise to reach 22 percent in 1997. The period since then has seen substantial fall in unemployment.

Figure 3. Unemployment, mean and spread, 1974-2002. Unemployment is defined as the sum of the openly unemployment and participants in active labor market programs as a share of the labor force. Sources: Labor Force Surveys and the Labor Market Board

Figure 4. The actual replacement rate, mean and spread, 1974-2002. The actual replacement rate have been generated using coefficients estimated on earnings data from 1970-1998. The actual replacement rate takes supplementary unemployment benefits into account.

Figure 4 gives a sense about the regional variation in our key measure of the generosity of the UI system. It shows the variation in the actual replacement rates over time and across regions when supplementary unemployment benefits have been taken into account. The actual replacement rate stood at a high in the early 1990s when it equalled 73 percent. Since then it has fallen quite rapidly to 63 percent in 2002. The variation across regions was particularly high around 2000. It is evident that there is a good deal of variation across regions as well as time, which we can potentially utilize in the following sections.

5. What variation identifies the actual replacement rate?

This section is devoted to illustrating in more detail where the identifying variation in the actual replacement rate comes from. To fix ideas we begin, in section 5.1, with a simple graphical example where we ignore the existence of supplementary unemployment benefits. In section 5.2 we turn to the data and examine the empirical importance of the determinants of the actual replacement rate.

5.1 A simple graphical example

Figure 5 provides a simple graphical illustration, where supplementary unemployment benefits are ignored. The bold (solid) line depicts the benefit schedule. According to this schedule, benefits increase linearly with wages for all wages below the cap (w^{cap}); the rate of increase in benefits is given by the nominal replacement rate (nominal rr). For

wages above the cap there is no increase in benefits as indicated by the flat segment of the benefit schedule.

Suppose, for simplicity, that there is no dispersion of wages within region. Then the actual replacement rate (actual rr) in the high-wage (w_h) region is given by the slope of the dashed line, while the nominal and actual replacement rates coincide in the low-wage region (w_l). Now, suppose that the benefit ceiling increases. Then such a change has no effect in the low-wage region. But it has a substantial effect in the high-wage region, as indicated by the thinner dashed line in Figure 5.

It is easy to see that if there is wage growth in the high-wage region – i.e., w_h is pushed further to the right in the figure – then this will lower the actual replacement rate. It is also straightforward to verify that if the nominal replacement rate increases this will have the biggest effect on the generosity of UI in the low-wage region.

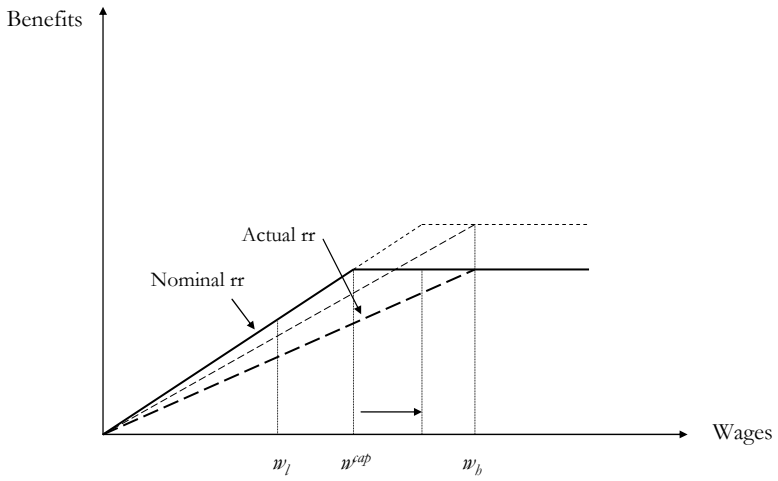

Figure 5. The effects of variations in the benefit ceiling.

Other possible sources of variation are more subtle, however. Consider wage dispersion within regions. Let us focus on two regions where mean wages are the same and coinciding with w^{cap}. Suppose, further, that in the two regions the wages are symmetrically distributed around the mean. Then in the region with the greater variation in wages, the top end of the distribution will have a lower actual replacement rate on average. Thus, the standard deviation of the wage distribution should be negatively associated with the actual replacement rate.

5.2 A look at the data

Above we argued that the variations in expected wages, the benefit ceiling, the nominal replacement rate, as well as the variation in the spread of the expected wage distribution all contribute to the variation in the actual replacement rate. Here we illustrate the importance of each source of variation.

To facilitate the interpretation of the independent variables we standardize these variables with their standard deviations. Table 1 presents the results. In panel A) we show the results when not taking the existence of supplementary unemployment benefits into account. All the estimates have signs which are consistent with the discussion above. So, for instance, if expected wages increases by a standard deviation this yields a reduction of the actual replacement rate by half a percentage point; see column (2). It is also interesting to note that the variation in the wage cap is such a powerful predictor of the actual replacement rate; this confirms the impression given already in Figure 2. An increase in the cap has the effect of increasing the actual generosity of UI more in regions which are expected to be high-wage.[25]

In panel B) we consider the variation in the generosity of UI when supplementary unemployment benefits are taken into account. The evidence presented in panel B) is not as clean as the estimates presented in the previous panel. For instance, the statutory replacement rate no longer has a greater effect in regions that are predicted to be low-wage (which should be the case according to the simple benefit formula). And the standard deviation of the expected wage distribution ceases to be a significant predictor of the actual replacement rate. Nevertheless, the estimates again suggest that the wage cap is the most significant contributor to the explained variance of the actual replacement rate.

Another aspect of the results in Table 1 is also worth noting. With the four variables we do not account fully for the variation in the actual replacement rate. In other words, there is residual variation, since the explained variance does not equal unity. There are a number of reasons for this. At the individual level, the benefit schedule depicted in Figure 5 is deterministic. This is not the case at the aggregate regional level. To explain the variation in the actual replacement rate fully at the regional level, we would have to include all moments of the expected wage distribution;

[25] Notice that it is only the interaction effect which is identified. The main effect of the wage cap is "swamped" by the time fixed effects.

obviously, this is not feasible. Further, supplementary unemployment benefits introduce additional noise, which is evidenced by the fact that explained variance is lower in panel B) than in panel A).[26]

Table 1. What explains the variation in the actual replacement rate?

	(1)	(2)	(3)
A)			
No account for supplementary UB			
Expected wage	-0.392**	-0.502**	-0.701**
	(0.033)	(0.177)	(0.178)
Expected wage interacted with wage cap		0.466**	0.550**
		(0.056)	(0.064)
Expected wage interacted with nominal replacement rate		-0.321**	-0.197
		(0.132)	(0.130)
Standard deviation of expected wage			-0.062**
			(0.014)
# observations	696	696	696
Within R^2	0.63	0.80	0.81
B)			
With account for supplementary UB			
Expected wage	-0.281**	-1.30**	-1.32**
	(0.042)	(0.437)	(0.442)
Expected wage interacted with cap		0.601**	0.611**
		(0.103)	(0.113)
Expected wage interacted with nominal replacement rate		0.463	0.477
		(0.362)	(0.361)
Standard deviation of expected wage			-0.007
			(0.022)
# observations	696	696	696
Within R^2	0.29	0.43	0.43

Note: Dependent variable in percent. The table reports standardized coefficients. An individual coefficient has the interpretation of percentage point change in response to a standard deviation increase in one of the independent variables. The regressions also control for regional fixed effects, region-specific trends, fixed time effects, and exogenous labor force characteristics. Within R^2 reports the share of the variance explained by the four variables in the table after having controlled for other covariates, region-specific FEs and trends, as well as time effects. Other covariates include age, education, immigrant status, gender, industry affiliation, and the share covered by supplementary UB (only panel B). Regressions are weighted by population. Standard errors, reported in parentheses, allow for clustering at the county level. Significance levels: * = 10%, ** = 5%

In summary, the most important finding in this section is that a substantial fraction of the variation in the actual replacement rate at the regional level is due to variations in

[26] A final reason is that we are not using the functional form implied by Figure 5. Since this is not the right function at the regional level, we have no reason to impose it.

the national wage cap. An increase in the wage cap has a greater positive effect on UI generosity in regions which are expected to be high-wage. Thus it should be possible to identify the effect of the actual replacement rate on regional unemployment using only the variation in the wage cap. This identification strategy is the one that we will mainly pursue in the next section.

6. What is the effect of increases in the actual replacement rate?

With the exercise in section 5 as a background we now proceed to examine the relevance of the UI system for regional unemployment. We begin with a very basic question. Do the parameters of the UI system have any impact on regional unemployment? This is a relevant question given that many collective agreements supplement unemployment benefits. To investigate this issue, we first estimate the equation

$$\ln u_{jt} = \kappa^{w} w_{jt}^{e} + \kappa^{cap}(w_{t}^{cap} \times w_{jt}^{e}) + \kappa^{\rho}(\rho_{t}^{n} \times w_{jt}^{e}) + \kappa^{\sigma}\sigma_{jt} + \kappa^{X} X_{jt} + \mu_{j}^{1} + \mu_{t}^{1} + \delta_{j}^{1} t + \omega_{jt}^{1} \quad (15)$$

where w_{jt}^{e} denotes the expected wage and σ_{jt} the standard deviation of the expected wage distribution. The vector of characteristics, *X*, includes the same components as in the individual earnings regressions since any exclusion restriction with respect to the components of *X* is bound to be arbitrary. Furthermore, *X* includes a control for supplementary unemployment benefits. The specification of equation (15) also takes region-specific effects, time fixed effects, as well as region-specific trends into account.

The idea behind equation (15) is that the first four components conceptually drive the variation in the actual replacement rate at the regional level. One can potentially make the argument that the expected wage and the standard deviation capture omitted variables in the unemployment equation. But it is very hard to see that this is a relevant argument for the interaction terms. In particular, if we find that $\kappa^{cap} > 0$ this strongly suggests that the design of the national UI system has implications for regional unemployment. The same line of argument goes for the interaction with the nominal replacement rate where we would expect $\kappa^{\rho} < 0$.

Table 2 reports the results. We mainly focus on the specification where the dependent variable is the log of unemployment; see column (1). But in column (2) we

also report the results of a specification where we use the unemployment rate as the dependent variable. Again, we standardize the key independent variables to facilitate the interpretation of the coefficients of these variables.

In column (1) the interaction between the expected wage and the wage cap enters significantly with a positive sign. Thus, changes in the wage cap produce a greater increase in unemployment in regions which are expected to have a high wage. A standard deviation increase in this interaction term raises unemployment by almost 4 percent. The remaining interaction variable is not significant and does not have the predicted negative sign; this result is consistent with the estimates reported in panel B) of Table 1.

Table 2. Basic estimates

	(1) ln(unemployment)	(2) Unemployment (percent)
Expected wage	-0.069 (0.054)	1.41** (0.628)
Expected wage interacted with wage cap	0.036** (0.015)	-0.090 (0.158)
Expected wage interacted with nominal replacement rate	0.044 (0.046)	-1.30** (0.514)
Standard deviation of expected wage distribution	0.008* (0.005)	-0.021 (0.031)
Other covariates	Yes	Yes
Region-specific FEs	Yes	Yes
Region-specific trends	Yes	Yes
Time effects	Yes	Yes
Overall R^2	0.981	0.982
Within R^2	0.045	0.045
# observations	696	696

Note: Key independent variables are standardized and have the interpretation of the effect on the dependent variable in response to a standard deviation increase in the independent variable. All regressions are estimated using a within-estimator and include controls for gender, age, marital status, educational attainment, immigrant status, industry affiliation, and the share of individuals covered by collective agreements with supplementary unemployment benefits. Within R^2 reports the share of the variance explained by the four variables in the table after having controlled for other covariates, region-specific FEs and trends, as well as time effects. Regressions are weighted by population. Standard errors, reported in parentheses, are clustered by county. Significance levels: * = 10%, ** = 5%.

The estimates in column (2) tell a slightly different story. In this case, the interaction with the wage cap is not significant; but the interaction with the nominal replacement rate enters significantly with the predicted negative sign.

Theory provides little guidance to the question of whether the dependent variable should be specified in logs or as the rate of unemployment. In the sequel, however, we focus on the estimates where log unemployment is the dependent variable. Given the results presented in Table 1, the estimates in column (1) is consistent with the hypothesis that the actual replacement rate drives the evolution of log unemployment. The estimates in column (2) are consistent with this hypothesis to a lesser extent.

The most important result contained in Table 2, however, is that parameters of the national UI system do affect regional unemployment. Having established this we proceed to estimating equations imposing more structure.

The specification in equation (16) imposes more structure. In this case we relate unemployment directly to the actual replacement rate. Thus

$$\ln u_{jt} = \varphi^{\rho}\rho^{a}_{jt} + \varphi^{w}w^{e}_{jt} + \varphi^{\sigma}\sigma_{jt} + \varphi^{X}X_{jt} + \mu^{2}_{j} + \mu^{2}_{t} + \delta^{2}_{j}t + \omega_{jt} \qquad (16)$$

There are two potential ways to estimate (16). The first is akin to a control function approach. Controlling for expected wages and the standard deviation of the expected wage distribution, the remaining variation in the actual replacement rate has two components: one source of variation is due to the interaction terms between the expected wage and the wage cap as well as the nominal replacement rate respectively; the other source of variation is the residual variation in the actual replacement rate. Given the substantial difficulties involved in measuring the actual generosity of UI, the residual variation is likely to contain a lot of noise; this implies that the control function approach will generate estimates that are biased downwards due to attenuation.

The other approach to estimating (16) is to just utilize the predicted variation in the actual generosity of UI stemming from the variation in the interaction terms – the most important of these interactions being the variation stemming from the changes in the wage cap. Implementing this strategy is straightforward; it amounts to estimating equation (16) using standard IV methods.

Table 3 reports estimates of equation (16). The dependent variable is the log of unemployment. In columns (1) and (2), the actual replacement rate does not account for

supplementary unemployment benefits; in column (3) it does. In column (1) we use the control function approach; columns (2)-(3) are based on the IV-approach. The equations are all estimated using a traditional within-estimator.

Table 3. The effect of the actual replacement rate on unemployment

	ln(unemployment)		
	(1)	(2)	(3)
Actual replacement rate (percent) (No account for supplementary UB)	0.018 (0.018)	0.045** (0.023)	
Actual replacement rate (percent) (Account for supplementary UB)			0.054** (0.023)
Expected wage/1000	0.023* (0.012)	0.040** (0.016)	0.036** (0.013)
Standard deviation of expected wage/1000	0.032** (0.012)	0.030** (0.012)	0.023* (0.012)
Other covariates	Yes	Yes	Yes
Region-specific FEs	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes
Time effects	Yes	Yes	Yes
Overall R^2	0.981	0.981	0.980
Estimation approach	Control function	IV	IV
# observations	696	696	696

Notes: Columns (2)-(3) use the interactions between the expected wage and the wage cap as well as the nominal replacement rate respectively to identify the coefficient on the actual replacement rate. All regressions are estimated using a within-estimator and include controls for gender, age, marital status, educational attainment, immigrant status, industry affiliation, and the share of individuals covered by collective agreements with supplementary unemployment benefits. Table B1 in Appendix B reports the coefficient estimates on the majority of the remaining covariates for the specification reported in column (3). Regressions are weighted by population. Standard errors, reported in parentheses, are clustered by county. Significance levels: * = 10%, ** = 5%.

Table 3 suggests that the estimation approach matters a great deal for the results. Column (1) – which is based on the control function approach – reports an insignificant estimate on the actual replacement rate. However, if we use only the variation induced by the interactions terms, the estimate is significant. We are inclined to interpret the divergence in the results as being due to bias because of measurement error. As illustrated in Table 1, the noise is substantial, particularly when the measure of benefit generosity attempts to account for supplementary unemployment benefits.

Our preferred estimate is shown in column (3). The coefficient estimate suggests that unemployment rises by 5 percent (i.e. the unemployment rate increases from, say, 6

to 6.3 percent) in response to increase in the actual replacement rate of 1 percentage point.[27] The elasticity of unemployment with respect to benefit generosity implied by this estimate is remarkably high. Evaluated at the mean actual replacement rate in 2002 (63 %), the elasticity equals 3.4.

The estimates reported in Table 3 are higher than we have found elsewhere in the literature. The estimates in columns (2) and (3) are roughly four times higher than Nickell (1998) obtained in his study of a cross-section of OECD countries. Krueger & Meyer (2002) report a benefit elasticity of one when taking the effect on the incidence as well as duration of unemployment into account.

Of course, it is hard to pinpoint why we get higher estimates than those available elsewhere in the literature. Relative to Nickell (1998), we would argue that effects that we estimate are more credibly identified than in his cross-country regression. The estimate reported in the Krueger & Meyer (2002) is obtained by surveying micro studies mostly pertaining to the US. Here we cannot argue that our estimate is more credibly identified. But clearly the parameter we estimate is different in the sense that it takes equilibrium adjustments into account to a greater extent than in micro studies. Also, we obtain this estimate using Swedish data where unemployment benefits are substantially more generous than in the US. This is a relevant issue since, e.g., standard matching models suggest that the general equilibrium effect on unemployment of a given variation in UI generosity is greater the higher is UI benefits from the outset; some illustrative simulations on this theme are reported in Holmlund (1998), and Hornstein *et al* (2005).

We have subjected the specification in column (3) to some specification checks. First we used the unemployment rate as the dependent variable. The estimate is substantially weaker. A percentage point increase in UI generosity causes unemployment to rise by 0.090 percentage points; the standard error of this estimate is 0.062. Second, we introduced a lag of the actual replacement rate. This virtually had no effect on the estimate and the coefficient on the lag was not significant. Third, we transformed the model by taking first differences. This reduced the size as well as the precision of the estimate. The coefficient on the actual replacement rate was reduced to

[27] This change in benefit generosity roughly corresponds to the weighted standard deviation of the actual replacement rate within regions and time.

0.024 with a standard error of 0.015. Despite the fact that the estimates sometimes become less precise, we view them as fairly robust to specification changes.

7. Policy interventions

The purpose of this section is to conduct two policy simulations. In particular we are interested in the effect of removing the benefit ceiling and the effect of increasing the nominal replacement rate. These two policy changes have obvious implications for aggregate unemployment – i.e. aggregate unemployment increases. The more interesting effects are those on the regional distribution of unemployment. In almost all countries, regional unemployment differentials are very stable over time; see Figure 1 and, e.g., Fredriksson (1999) for a collection of evidence. Perhaps the design of the social insurance system contributes to this feature?

The starting point for these experiments is the regional distribution of unemployment and actual replacement rates in 2002. In contrast to the previous analysis we actually have wage data for 2002 and hence we can calculate the "true" actual replacement rate. In Figure 6 we show the correlation between regional unemployment and the actual replacement rate accounting for supplementary unemployment benefits. As shown by the slope of the regression line, a percentage point increase in the actual replacement rate is associated with 14 percent higher unemployment. This just illustrates that high-wage regions tend to be low-unemployment regions. Since unemployment benefits replace a lower fraction of previous wages in high-wage regions they also tend to have a lower actual replacement rate. This simultaneity bias thus inflates the estimate of the relationship between benefit generosity and unemployment.

In 2002, the aggregate unemployment rate stood at 6.8 percent. To generate the situation after a policy change we use the estimate on the actual replacement rate reported in column (3) of Table 3. We set the coefficient on the actual replacement rate to 0.05.

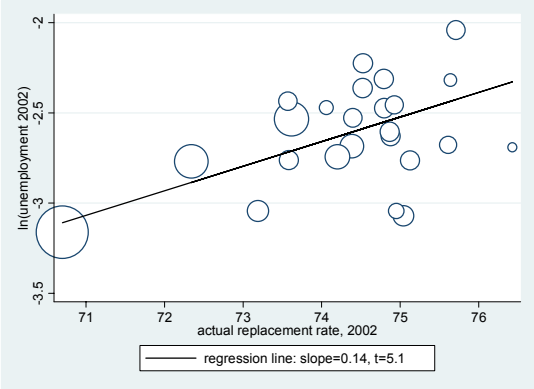

Figure 6: The correlation between unemployment and the actual replacement rate

What happens if we remove the benefit ceiling? Obviously this has the effect of making the system more generous – the actual replacement rate rises by 6.5 percentage points, on average. As a consequence, there is an increase in overall unemployment from 6.8 percent to 9.1 percent. What is more the spread of the regional unemployment distribution is reduced. This is illustrated in Figure 7, which relates the change in log unemployment – induced by the reform – to the log of the unemployment rate prior to the change. As the graph shows, there is a greater change in regions where unemployment was low initially; the slope of the regression line is negative with a *t*-ratio of 5. The intuition for this result is that the proposed policy change has a bigger effect in high-wage regions, which also tend to be low-unemployment regions. Thus, the policy change contributes to reducing unemployment differentials.

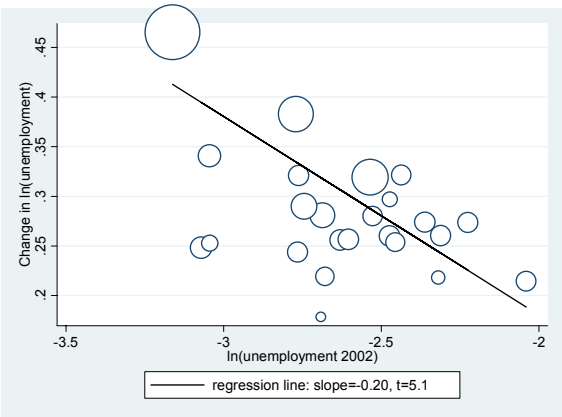

Figure 7. Policy simulation – Removing the cap. This graph is based on a hypothetical policy experiment where the benefit ceiling is removed. The implied change in the actual replacement rate is calculated using the regional distribution of (true) actual replacement rates in 2002. The implied change in unemployment is calculated using a coefficient on the actual replacement of 0.05. The regression line is based on a weighted regression using regional population as weights.

Now, what about raising the nominal replacement rate to 85 %? Again, this makes the system more generous and the actual replacement rate rises by 3.8 percentage points. Consequently, the aggregate unemployment rate rises to almost 8.3 percent. What is more, this policy change has the opposite effect on the regional distribution of unemployment in comparison to the change in the benefit ceiling. As the figure shows, the unemployment rate increases more in regions which were high-unemployment locations initially. The intuition is analogous to the previous case. High-unemployment regions tend to be low-wage regions. Consequently, changes in the statutory replacement rate have a bigger impact on the actual generosity of the UI system in these locations. Comparing the slope of the regression lines in Figures 7 and 8, we see that removing the benefit ceiling has a stronger differential impact across regions.

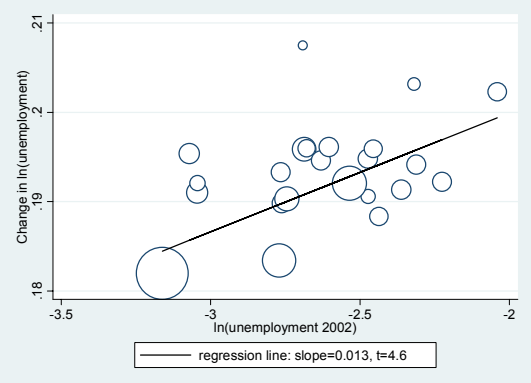

Figure 8. Policy simulation – Increasing the nominal replacement rate. This graph is based on a hypothetical policy experiment where the nominal replacement rate is raised to 85 %. The implied change in the actual replacement rate is calculated using the regional distribution of (true) actual replacement rates in 2002. The implied change in unemployment is calculated using a coefficient on the actual replacement of 0.05. The regression line is based on a weighted regression using regional population as weights.

To sum up, the results of these policy simulations show that the design of the national UI system has repercussions on the regional labor market. Moreover, they concur with the simple model in section 2. The impact on the regional distribution of unemployment differs across the policy experiments. If UI is made more generous by raising the benefit ceiling this will compress unemployment differentials, while if generosity increases because of an increase in the statutory replacement rate this will exacerbate regional unemployment differences.

8. Conclusions

We have presented new evidence on the unemployment effects of increasing UI benefit generosity. The empirical strategy has been to utilize the fact that the nationally imposed benefit ceiling causes actual UI generosity to vary regionally. This paper has thus used variations in the national UI rules to estimate the effects at the regional level. Hence, the estimates should thus not suffer from the potential policy endogeneity hampering studies using regional policy changes for identification.

The evidence suggests that benefit generosity increases unemployment. We view this evidence as fairly robust since the estimates are similar across alternative specifications. The magnitudes involved are rather substantial and appear to be relatively high compared to estimates available elsewhere in the literature. The estimates suggest that an increase in the (actual) replacement rate of 5 percentage points contributes to increasing unemployment by 25 percent.

We have also shown that the benefit ceiling may contribute to exacerbating regional unemployment differentials. Lowering the ceiling reduces benefit generosity more in high-wage regions. Since high-wage regions also tend to be low-unemployment regions, the result follows. Moreover, a reduction in the statutory replacement rate has the opposite effect. Given that a benefit ceiling exists, a reduction in the statutory rate will reduce benefit generosity more in high-unemployment regions. Thus, these simple policy experiments illustrate that national rules in social protection systems can have (perhaps unintended) repercussions at the regional level.

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Appendix A: Creating regional panel data

This appendix describes the construction of the regional panel data. Regional labor force composition, predicted wages and replacement rates are calculated from individual data. We use LINDA, a 3.35% representative sample of the Swedish population; see Edin & Fredriksson (2000). From this register, we select all individuals between the ages of 16-59, from 1970 to 2002.[28] In the early 1970s data contain roughly 130 000 individuals per year; in 2002 about 150 000. LINDA has a panel dimension which is very useful when constructing the data. If information is missing in one year, we can check if this information is available at another time point. This panel structure of the data is extremely valuable when comes to impute missing information on educational attainment as discussed below.

The regions correspond to the counties of Sweden. Between 1970 and 1996 there were 24 counties in Sweden. In 1996 two counties were merged, and in 1997 another three counties were merged.[29] Hence, from 1997 and onwards, there are 21 counties in Sweden. Since we also have data at the municipality level we can reconstruct the original 24 counties. We have thus used the municipality data to split the merging counties – thus creating 24 regions for the full time period.

The individual characteristics used in this paper are standard. Gender is identified by a female dummy. We define a set of age-group dummies for each five-year interval; the youngest group thus contains individuals aged 16 to 20, and the oldest group contains the ages 56 to 59. Marital status identifies married individuals. Immigrants are divided into three groups depending on the country of birth. We identify three groups: Nordic, Oecd, and non-Oecd immigrants. The definition of Nordic ancestry is obvious; the categorization into Oecd and non-Oecd immigrants is perhaps less obvious. We have used the following rule: individuals are classified as being of OECD origin if they were born in a country which was a member of the OECD in 1985. The only exceptions from this rule are Turkey – which is included among the non-OECD countries – and, of course, the Nordic countries.

[28] We have to restrict the analysis to individuals younger than 60, since educational information is not available for those older than 59 for the full time period.

[29] In 1996, the county of Skåne was created by merging the counties of Malmöhus and Kristianstad. In 1997, the county of Västra Götaland was created by a merger of the counties of Älvsborg, Göteborg och Bohuslän, and Skaraborg.

Industry affiliation is defined by two-digit ISIC-codes, generating 33 industry dummies. The coding changed in 1993, but at the two-digit level it is possible to link the two coding systems. However, information on industry affiliation is missing for four years: 1974, 1976, 1977, and 1979. To deal with this issue we use the following simple rule: the information observed in 1975 is used for the individual also in 1974 and 1976; analogously, the information observed in 1978 is used also in 1977 and 1979.

Educational attainment is divided into three categories: compulsory, secondary, and tertiary schooling. Starting in 1991 educational information is available each year. Prior to 1991 we only observe educational attainment at two time points: in 1970 and 1990. We have used the following procedure to attach educational information to the individuals during 1971-1989. If an individual is at least 25 years-of-age in 1970, education is assumed to be completed and the observation from 1970 is used to fill out the missing information during 1971-1989. If the individual is younger than 25 in 1970, we use data from 1990. Different rules are used depending on educational attainment in 1990 and age at the time point of observation. For an individual who has completed tertiary education, we assign the level of attainment should this individual turn 25 during 1971-1989. Should this individual turn 21 during this time period he or she assigned secondary schooling and when the individual is below age 21 he or she is assigned compulsory schooling. For an individual who has completed secondary schooling in 1990, we use this attainment level from the point when the individual turns 21 and onwards. Prior to turning 21, compulsory schooling is used has the highest attainment level. An individual who had completed compulsory schooling in 1990 is classified as having attained compulsory schooling from the time point when he or she enters our data.

Our key measures (expected wages and actual replacement rates) are constructed using earnings and wage information as described in the text, as well as the UI rules described below.

Finally, the regional panel is constructed by averaging over all individuals residing in a particular region. This gives us annual information on the composition of the regional population as well as the key explanatory variables of interest. Ideally, we would have liked to calculate the characteristics of the regional labor force. But this was

not possible since there was no indicator of labor force status in our data. However, the measurement error involved is likely to be small.

To these regional panel data we match information on unemployment. Regional unemployment data are defined for the age-category 16-64; they are collected from the Labor Force Surveys and the Labor Market Board. As the measure of unemployment we use the sum of open unemployment and participants in labor market programs as a share of the labor force.

Unemployment insurance

The design of the public unemployment insurance system has varied somewhat over time. There are two distinct time-periods – the first covers the period from 1974 to 1988, and the second the period 1989-2002.

Between 1974 and 1988, individuals were sorted into different benefit levels depending primarily on how much they earned. The various UI funds used different benefit ceilings. There was a national benefit ceiling, however, and the replacement rate could never exceed 91.7 % of previous income. This implies that the maximum benefit level varied between individuals, depending on which particular UI fund the individuals were members of. Since we cannot observe membership in a particular UI fund, we use the "average maximum benefit level" as a proxy for the maximum level. This measure is reported in the Annual Financial Report of the Labor Market Board; it is calculated as a weighted average over individuals, where the weights are based on the number of members in a particular UI fund.

From 1989 and onwards, the construction of the UI system is more straightforward. An unemployed individual then receives a certain amount (in percent) of the previous wage, up to a maximum level.

Even though the design of the system has varied somewhat over time, we implement the rules in essentially the same way. An individual receives a benefit equal to the nominal replacement rate multiplied by foregone income, but the benefit can never exceed the ceiling. The ceiling is here defined from 1974-1988 by the "average maximum benefit level" and from 1989-2002 as the "maximum benefit".

Table A1 displays the benefit levels (in SEK per day), and the nominal replacement rates from 1974 to 2002 as observed on December 31st each year.[30] Column (1) displays the national benefit ceiling. Remember that this variable is only used as a measure of the benefit ceiling from 1989 to 2002.[31] As described above, the average maximum benefit level in column (2) is used between 1974 and 1988. Note that prior to 1977, the difference between the average maximum benefit level and the national benefit ceiling is substantial. But from then on the differences across UI funds become smaller, to eventually disappear completely.

Column (3) reports the date when the benefit ceiling was changed. Typically, this was not at the beginning of a calendar year. Hence, the annual benefit ceiling used in this paper is calculated in column (4) using the information in columns (1) to (3). For example, there is a change in the maximum benefit on July 1st 1979, and then the calendar year benefit ceiling (179.39), is simply calculated as the mean of the average benefit values observed on December 31st 1978 (171.16) and December 31st 1979 (187.62).

Column (5) shows the nominal replacement rate, column (6) reports when it was changed, and column (7) the annual average of the nominal replacement rate used in the analysis. Hence, the bold figures in column (4) and (7) are the primary input in our analysis. From these data, we calculate the wage cap by dividing the benefit ceiling with the nominal replacement rate. The wage cap and the nominal replacement rate are shown in Figure 2.

[30] Information is taken from the annual reports of the Labor Market Board.
[31] From 2001 and onwards, the benefit ceiling decreases after 100 days of unemployment, as shown by the figures in the brackets. In this paper, we use only the maximum during the first 100 days.

Table A1. The unemployment insurance system, 1974-2002.

Year	National benefit ceiling SEK/day (Dec 31) (1)	Average benefit ceiling SEK/day (Dec 31) (2)	Date of change in ceiling (3)	**Benefit ceiling SEK/day (calendar year) (4)**	Nominal repl. rate (Dec 31) (5)	Date of change in nominal repl. rate (6)	**Nominal repl. rate (calendar year) (7)**
1974	130	98,07		**98.07**	0.917		**0.917**
1975	130	116,58		**116.58**	0.917		**0.917**
1976	160	122,22	July 1st	**119.4**	0.917		**0.917**
1977	160	151,76		**151.76**	0.917		**0.917**
1978	180	171,16	July 1st	**161.46**	0.917		**0.917**
1979	195	187,62	July 1st	**179.39**	0.917		**0.917**
1980	195	192,19		**192.19**	0.917		**0.917**
1981	210	206,80	April 1st	**203.1475**	0.917		**0.917**
1982	230	227,66	July 1st	**217.23**	0.917		**0.917**
1983	280	278,80	Jan 1st	**278.8**	0.917		**0.917**
1984	300	298,87	July 1st	**288.835**	0.917		**0.917**
1985	315	314,48	July 1st	**306.675**	0.917		**0.917**
1986	360	359,20	July 1st	**336.84**	0.917		**0.917**
1987	400	400	July 1st	**379.6**	0.917		**0.917**
1988	425	425	July 4th	**412.5**	0.917		**0.917**
1989	450		Jan 2nd	**450**	0.9	Jan 2nd ; 90%	**0.9**
1990	495		Jan 1st	**495**	0.9		**0.9**
1991	543		Jan 7th	**543**	0.9		**0.9**
1992	564		Jan 6th	**564**	0.9		**0.9**
1993	564		Jan 4th ; 598 July 5th ; 564	**581**	0.8	July 5th ; 80%	**0.85**
1994	564			**564**	0.8		**0.8**
1995	564			**564**	0.8		**0.8**
1996	564			**564**	0.75	Jan 1st ; 75%	**0.75**
1997	580		Dec 29th	**564**	0.8	Sep 29th ; 80%	**0.7625**
1998	580			**580**	0.8		**0.8**
1999	580			**580**	0.8		**0.8**
2000	580			**580**	0.8		**0.8**
2001	680 (580)		July 2nd	**630**	0.8		**0.8**
2002	730 (680)		July 1st	**705**	0.8		**0.8**

Note: Column (4) is based on cols. (2) and (3) during 1974-88, and cols. (1) and (3) during 1989-2002. Column (7) is based on columns (5) and (6) throughout the time period.

Appendix B: Coefficient estimates from baseline specification

Table B1 shows coefficient estimates corresponding to our preferred specification, that is, column (3) in Table 3.

Table B1. Estimates on a selection of observed regional control variables.

	Coefficient estimate (standard error)
Covered by collective agreement with supplementary UB	1.74** (0.637)
Female	2.58 (2.01)
Married	-1.99* (1.12)
Nordic	-0.784 (3.45)
Oecd	-3.57 (7.51)
non-Oecd	-4.79 (3.20)
Secondary schooling	-5.06** (1.84)
Tertiary schooling	-3.85* (2.13)
Age 21-25	3.26 (2.24)
Age 26-30	4.26 (2.84)
Age 31-35	6.89** (3.16)
Age 36-40	8.03** (3.16)
Age 41-45	8.68** (2.58)
Age 46-50	9.05** (3.02)
Age 51-55	11.63* (3.31)
Age 56-59	10.51* (3.04)
Region-specific fixed effects	Yes
Region-specific trends	Yes
Time effects	Yes
Overall R^2	0.981
# observations	696

Note: The regressions also include a constant and industry employment shares. Regressions are weighted by population. Standard errors, reported in parentheses, are clustered by county. Significance levels: * = 10%, and ** = 5%.

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