ESSAYS ON SKILLS, SCHOOL CHOICE AND THEIR LONG-TERM CONSEQUENCES

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Content

1 Introduction

The three essays in this dissertation are concerned with cognitive and noncognitive skills as a part of human capital, their importance for long-run socio-economic success and the possibility of improving skills and long-term outcomes by allowing more parental freedom in school choice.

The importance of skills over and above formal educational degrees as a major factor to secure individual improvements in income and country-level economic growth is widely recognised today, both among scientists[1] as well as among politicians. The European Union has placed improving education and skills at the heart of its EU 2020 growth strategy. In 2012, the Organisation for Economic Co-operation and Development (OECD) has launched a "Skills Strategy" in order to "[..] help governments build economic resilience, boost employment and reinforce social cohesion [..]" (OECD 2012). Moreover, studies that measure competencies of children and adolescents in a comparative way across countries[2] have received wide public attention in recent years and have fuelled discussions on how to best design the education system. It is thus of high policy relevance to provide evidence on which specific skills play a role in economic success and which education policies are most efficient in fostering the development of these skills.

While the majority of studies focus on assessing cognitive achievements such as literacy or numeracy skills, there is an increasing number of research papers also in the economic literature that acknowledge the multi-dimensionality of skills as a part of human capital. They show that not only cognitive skills, but also noncognitive, social or interpersonal skills are important predictors for economic and social outcomes[3]. Moreover, there is a large body of evidence that points to the importance of (early)

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[1] See for example Hanushek & Woessmann (2008).

[2] Examples of such studies are the OECD Programme of International Student Assessment (PISA), which measures competencies in the areas of reading, math and science among 15-year-olds, the Trends in International Mathematics and Science Study (TIMMS), which focuses on measuring math and science achievements of 4th and 8th graders, and the "Progress in International Reading Literacy Study" (PIRLS), which focuses on reading achievement of 4th graders.

[3] See for example Heckman, Stixrud and Urzua (2006) and Bowles, Gintis and Osborne (2001).

childhood as the optimal period for investments in human capital[4]. Cunha and Heckman (2007) summarise the evidence and present a model that stresses the value of dynamic complementarities between early and late investments in skills and between different facets of skills.

Against this background, this thesis provides empirical evidence on, first, whether cognitive and social skills in childhood factor into socio-economic success later in life, and second, whether changes in the degree of school choice improve these same outcomes and skills. All three essays use microeconometric methods that allow distinguishing among different factors that influence children's outcomes, such as the family and schooling background, and different skills. Moreover, in order to estimate not only short- but also long-term returns, this thesis uses rich longitudinal data that follows individuals throughout their childhood and into young adulthood.

The second chapter studies whether both cognitive and social skills, measured already in childhood, are related to the duration of unemployment in early adulthood. By estimating a proportional hazard rate model, I analyse the probability of making a transition from unemployment to employment during an individual's first unemployment spell. The study is based on British cohort data from the National Child Development Study, which contains information on children and their family and schooling environment from birth to adulthood. The estimates show that higher cognitive and social skills at the age of seven are associated with an increased probability of finding employment, resulting in a shorter duration of unemployment. This result holds also when controlling for individuals' educational attainment, which means that skills measured at the age of seven are important over and above their effect via increasing education. What is more, also holding a broad set of variables on the family background, parenting activities and school characteristics constant does not change the results qualitatively. Lastly, I find that for men, these effects are mostly driven by individuals with low social skills.

The third and fourth chapter are based on joint work with Karin Edmark and Markus Frölich. We evaluate the effects of a major school choice reform in the compulsory

[4] See Cunha, Heckman, Lochner and Masterov (2006) for an overview.

schooling system in Sweden in 1992 on student outcomes. The reform increased school choice and competition among public schools and lead to a large-scale introduction of publicly funded but privately run schools. While proponents of free school choice argue that more choice and the resulting competition among schools may lead to increases in the overall quality of the education system, opponents are concerned about possible adverse effects, both on the overall quality, but also on equality of opportunity within the schooling system.

In order to assess this reform, we make use of detailed Swedish register data containing information on several cohorts of the entire Swedish population. Some of them had already left compulsory education before the enactment of the reform and were therefore not affected, while others were affected at different stages in their educational career. Besides information on the family background, schooling and socio-economic outcomes later in life, we observe geographical locations of school buildings and children's homes. Using this geographical information, we construct measures of the degree of potential choice by counting the number of schools within reach of children's homes. This allows us to capture the effects of choice opportunities and competition also among public schools, whereas previous studies have focused on newly opened private schools. Moreover, since the reform was enacted 20 years ago, we can now measure its long-term effects and look at education and employment outcomes up to age 25.

In the third chapter, we focus on evaluating the average effects of the reform on the entire population. We find that increased school choice had very small but positive effects on marks at the end of compulsory schooling, but virtually zero effects on longer-term outcomes such as university education, employment, criminal activity and health. Moreover, we see that the effects are largest for the youngest cohorts in our dataset, indicating that it took some time for the reform to unfold.

In the fourth chapter, we focus on equity concerns relating to the 1992 Swedish school choice reform and analyse whether it had different effects for students from different socio-economic backgrounds. In addition, we explore effects on the distribution of marks at the end of compulsory schooling. Our results show that students from a socio-economically disadvantaged or immigrant background did not benefit less from more school choice than those from more advantaged backgrounds. The differences between the subgroups are small, but, if anything, students from low-income families benefited slightly more than those from higher-income families.

2 The Importance of Cognitive and Social Skills for the Duration of Unemployment[5]

2.1 Introduction

Understanding which individual factors influence the duration of an unemployment spell is important for designing policy measures that help individuals to find employment. Recent research indicates that noncognitive skills, such as social skills or personality traits, as well as cognitive skills influence wages, employment, educational and social outcomes (see for example Heckman, Stixrud and (2006), Bowles, Gintis and Osborne (2001), Blomeyer, Coneus, Laucht and Pfeiffer et al. (2009)). However, only little is known about the influence of cognitive and noncognitive skills on the duration of an unemployment spell. Existing studies show that certain noncognitive skills are related to the probability of making a transition from unemployment to employment (see Uhlendorff (2004), Uysal and Pohlmeier (2011), Lindqvist and Vestman (2011), DellaVigna and Paserman (2005)). Yet, only Lindqvist and Vestman (2011) and DellaVigna and Paserman (2005) include both cognitive and noncognitive skills in the estimation, finding different results on the effect of cognitive skills. If both dimensions of skills are correlated, not controlling for cognitive skills might overestimate the effect of noncognitive skills.

Concerning the optimal timing of social policy measures, Cunha, Heckman, Lochner and Masterov (2006) summarise evidence showing that interventions targeting human capital early in life are more efficient in ameliorating outcomes of disadvantaged individuals than later attempts to do so. Correspondingly, many papers have pointed out that already early skills predict socio-economic outcomes later in life (see for example Currie and Thomas (2001), Carneiro, Crawford and Goodman (2007)). In particular,

[5] This chapter was published in a very similar version as discussion paper in the ZEW and IFN discussion paper series (Niepel (2010) and Niepel (2011)).

some studies show that cognitive and noncognitive skills measured in childhood are related to different measures of time spend in unemployment in adolescence and early adulthood. For example, Gregg (2001) and Gregg and Machin (2000) show that social skills at the age of 7 are associated with the over- all number of months spent unemployed in early adulthood. However, by aggregating all unemployment episodes into one measure, the authors do not distinguish between the relevance of skills in childhood for the probability of becoming unemployed and the probability of finding a job.

The present paper contributes by studying the importance of human capital in childhood for the duration of an unemployment spell in early adulthood for men and women. In particular, it analyses not only the relevance of cognitive skills, but also explores the importance of social skills, being a further dimension of human capital that might be targeted with policy measures. I estimate a proportional hazard rate model for the probability of making a transition from unemployment to employment during an individual's first unemployment spell experienced before the age of 23. This approach has three main advantages. First, it disentangles how skills in childhood are associated with the probability of leaving unemployment from how they relate to the probability of becoming unemployed. Second, I specifically look at the probability of being successful at finding a job, which should be distinguished from other reasons for leaving unemployment, such as leaving the labour force. Third, since experiencing unemployment might influence noncognitive skills (see e.g. Goldsmith, Veum and Darity (1995)), analysing skills measured before individuals have entered the labour market circumvents reverse causality issues.

The empirical analysis builds on data from the National Child Development Study (NCDS), a cohort study based on all individuals born in Great Britain in a single week in March 1958. As a measure for social skills I rely on teacher ratings of children's behaviour using the Bristol Social Adjustment Guides (BSAG). In addition, four tests administered to the children in school are used to measure cognitive skills at the age of 7.

The results show that higher cognitive and social skills at the age of 7 are associated with an increased probability of making a transition from unemployment to employment during an individual's first unemployment spell. For men, there is a significant negative interaction effect between cognitive and social skills. Therefore, only those in the lower part of the cognitive skill distribution benefit from an increase in social skills and vice versa. For women, this inverse interaction effect is less pronounced. Furthermore, concerning men, the positive effect of an increase in social skills seems to be driven mostly by individuals with low social skills. Including educational attainment in the estimation slightly reduces the estimated hazard ratios but leaves the qualitative results unchanged. The estimates are robust to controlling for parenting activities, family background and school characteristics at the age of 7. Moreover, I find that also skills measured at the age of 11 are related to the probability of finding employment. However, the point estimates for age 11 social skills are smaller and of lower statistical significance than those for age 7 social skills.

The paper is organised as follows: Section 2.2 briefly surveys previous findings in the literature. Section 2.3 introduces the dataset and skill measures used in this study and presents descriptive statistics. Possible effects of skills in childhood on the duration of unemployment are discussed in Section 2.4. Section 2.5 presents the econometric model, followed by estimation results in Section 2.6. Section 2.7 summarizes the main findings and concludes.

2.2 Existing evidence

Recent studies on the relation between noncognitive skills and the duration of an unemployment spell have used German, Swedish and US datasets, measuring skills in late adolescence and adulthood. Uhlendorff (2004) uses the German Socio-Economic Panel (SOEP) to analyse the effect of an individual's locus of control and membership in political associations or clubs on the duration of an unemployment spell for men. He estimates a non-proportional Cox model and uses unemployment spells that occurred after measuring locus of control, thereby avoiding reverse causality issues. His results show that locus of control has a large effect on the duration of an unemployment spell for men living in the western part of Germany, while there is no significant association for those living in the eastern part. Uysal and Pohlmeier (2011) also use the SOEP to investigate the relation between noncognitive skills as measured by the Big Five and the duration of employment and unemployment. They analyse all unemployment spells that occurred between the years 1983 and 2006 for both men and women and show that the personality factors conscientiousness and openness to experience increase the chances of finding a job, while neuroticism decreases them. However, the statistical significance of these results differs for men and women. In contrast to these two studies using German data, Lindqvist and Vestman (2011) jointly include cognitive and noncognitive skills in estimating the duration of unemployment. They use Swedish register data on wages and employment biographies of men in 2006, thereby observing individuals at different stages in their working life. Skills are measured via tests and interviews conducted in the course of the military enlistment process around the age of 18. Lindqvist and Vestman find that higher noncognitive skills significantly decrease the duration of unemployment while cognitive skills have no statistically significant influence. DellaVigna and Paserman (2005) use the National Longitudinal Study of Youth and the Panel Study of Income Dynamics to show that several indicators of impatient behaviour, such as smoking and the interviewer's rating of an individual's impatience, are associated with a decreased exit rate from unemployment. They report that also higher cognitive skills are related to an increased probability of leaving unemployment.

In addition, several studies have investigated the relationship between noncognitive skills and other measures of unemployment, in particular the probability of being unemployed at a certain point in time and cumulative work experience[6], the probability of experiencing unemployment of a certain duration up to a certain age[7] or the overall amount of time spent unemployed in a certain period[8]. Most of these studies do not measure the duration of a single unemployment spell but aggregate an individuals' unemployment experience over a longer time period or, on the other hand, take a snapshot perspective by analysing the employment status at a given day. Thereby, they do not separate between the underlying economic mechanisms of individual unemployment experience, namely between the probability of becoming unemployed

[6] See for example Carneiro et al. (2007) and Heckman et al. (2006).
[7] See for example Feinstein (2000) and Hobcraft (2000).
[8] See for example Gregg and Machin (2000), Gregg (2001) and Caspi et al. (1998).

and the duration of the resulting unemployment spell. Carneiro et al. (2007) use the NCDS and find that the probability of being unemployed at the age of 42 and cumulative work experience between the age of 23 and 42 are influenced by cognitive and social skills in childhood. They show that this effect persists even after controlling for educational attainment. Heckman et al. (2006) use data from the USA and find similar results for cognitive and noncognitive skills measured in adolescence and early adulthood.

Gregg (2001) analyses the scarring effect of the number of months spent unemployed between the age of 16 and 23 for experiencing unemployment later in life using the NCDS. In doing so, he also estimates a Tobit model on the number of months spent unemployed between the age of 16 and 23 and finds that social skills at the age of 7 increase the number of months spent unemployed, while cognitive skills are not a significant predictor. Similarly, Caspi, Wright, Moffitt and Silva (1998) find that intelligence tests and behavioural indicators in childhood are predictors for the number of months spent unemployed between the age of 15 and 21, using a dataset from New Zealand. These studies do not distinguish between the importance of skills in childhood for the probability of becoming unemployed and their relevance for the duration of an unemployment spell, which is what the present paper aims at explaining.

Focusing on the duration of a single unemployment spell, Feinstein (2000) analyses the probability of having an unemployment spell that lasts for more than 4 months before the age of 26 and, for individuals for which this is the case, the probability that their longest spell lasts for more than one year using the 1970 British Cohort Study. The first outcome is thus comprised of the probability of becoming unemployed and the probability of experiencing an unemployment spell of at least 4 months. He finds that the math score and certain noncognitive skills at the age of 10 are related to the probability of being unemployed for more than 4 months. However, for the conditional probability of experiencing a spell of more than 12 months, he finds no predictive power of early skills for men and reports that a different set of noncognitive skills than in the first estimation are significant for women. These mixed results point out that early skills might be of different importance for the probability of becoming unemployed and for the probability of finding new employment. They thus emphasise the relevance of modelling the probability of making a transition from unemployment to employment in a duration model framework in order to achieve a deeper understanding of the effect of early skills.

2.3 Data and descriptive statistics

2.3.1 Data

This analysis uses data from the National Child Development Study[9]. The NCDS builds upon the Perinatal Mortality Survey (PMS) which includes all women who gave birth in Great Britain in a single week in March 1958, resulting in a dataset with information on 17.414 individuals. These form the basis for the following waves of the NCDS that have been carried out when the study members were aged 7, 11, 16, 23, 33, 42, 46 and 50. Across the different waves, information was provided by the children's parents, teachers, the schools' health service and the cohort members themselves. The dataset includes detailed information on the family background of the individuals, skills in childhood, educational attainment, family status and on the employment history.

At the age of 23, individuals were asked to report their main activity since May 1974 in a monthly diary. The diary contains information on periods in unemployment, employment and time spent out of the labour force, including education[10]. Using this information, I construct individual unemployment spells to be used in the analysis. Note that unemployment is defined as a time period in which the individual did not have a job but was willing to start work or was registered as unemployed. It is thus clearly distinguished from being out of the labour force. Furthermore, holidays or vacations during full-time education are not included in this definition. In months with more than one labour market status the dominant one is recorded. Therefore, only periods of unemployment that lasted for more than two weeks show up in the data.

[9] University of London. Institute of Education. Centre for Longitudinal Studies (2008a,b)

[10] The survey was designed in a way to help respondents remember these facts more easily and eliminate recall bias. Interviewers handed out a calendar in which they marked other important dates together with the respondent in order to provide orientation over the years. Nevertheless, when interpreting the results it should be kept in mind that recall bias might still have occurred such that especially short spells might not be recorded. However, by construction of the survey, spells shorter than two weeks are never recorded. Hence, missing such short spells is not correlated to any characteristics of individuals.

The measures for cognitive and social skills in childhood were obtained at the cohort members' schools when they were 7 years old. Cognitive skills were assessed using the following four tests. In the Southgate Group Reading Test (see Southgate (1962)), children were asked to pick one out of five words on a list to describe a drawing or a word that the teacher read out to them. The Copying Designs Test assesses children's perceptuo-motor ability by letting them copy geometric figures and taking a writing sample. In order to assess the children's general perceptual ability, they were asked to draw a man in the Drawing-A-Man Test (see Goodenough (1926)). Lastly, the Problem Arithmetic Test consisted of ten arithmetic exercises (see Pringle, Butler and Davie (1966)). I construct a total score for cognitive skills by summing up the normalized scores on each of the tests and then normalizing the total to have mean zero and variance one.

Social skills are measured using the Bristol Social Adjustment Guides. This instrument assesses children's behaviour in school and is supposed to capture behavioural disturbances in responding to different social situations (see Stott (1974))[11]. For example, a child might act aggressively or rather withdrawn and inhibited when confronted with new situations. The BSAG was measured by handing out a list of 146 phrases that describe behaviour of children to the teachers of the cohort members[12]. They were asked to underline the phrases that best describe the child. The different aspects of behaviour are grouped into twelve syndromes: unforthcomingness, withdrawal, depression, anxiety for acceptance by children, anxiety for acceptance by adults, hostility towards adults, hostility towards children, writing-off adults and adult standards, restlessness, inconsequential behaviour, miscellaneous symptoms and miscellaneous nervous symptoms. By adding up the respective number of underlined phrases, twelve syndrome scores have been constructed by the data providers. Following Carneiro et al. (2007), I use the overall BSAG score, defined as the sum of the syndrome scores, as my measure for social skills and reverse the sign such that a

[11] For further reading on the BSAG see for example Davie (1973) or Ghodsian (1977).

[12] Achenbach, McConaughy and Howell (1987) investigate correlations between assessments of behavioural problems in children that were performed by different informants, such as teachers, parents, health visitors or the children themselves. They carry out a meta-analysis and find that, within the group of informants other than the children themselves, the correlation between the ratings is highest among teachers.

higher value indicates more skills. In addition, I normalize it to have mean zero and variance one in the sample. Figure 1 shows kernel density estimates of the resulting measures for cognitive and social skills. One can see that the density of social skills is strongly skewed to the left, that is many individuals have a high social skills score, while the density of cognitive skills more closely resembles a bell-shape.

The estimation sample includes the first unemployment spell of all individuals that provided sufficient information on their employment history in NCDS 4[13] and for whom there is information on cognitive and social skills at the age of 7. Individuals in the highest and lowest percentile of cognitive skills are dropped from the sample in order to avoid results being driven by outliers. Concerning social skills, only the lowest percentile is dropped since 10% of the sample have zero points on the original BSAG score, which is the highest possible score for social skills. This leaves me with 10,130 cohort members. 4,287 of these had at least one unemployment spell before the age of 23 and form the sample used for the estimations[14]. Unemployment spells lasting longer than 24 months, which is the case for about 2% of all spells, are censored at 24 months. Details on the derivation of covariates are given in Section 2.8.1 in the appendix.

2.3.2 Descriptive statistics

Table 1 displays descriptive statistics on the first unemployment spell, cognitive and social skills and background covariates. The statistics on the covariates refer to the values at the beginning of the spell. One can see that, on average, women have higher age 7 cognitive and social skills than men. Education is measured as highest qualification achieved until the beginning of the unemployment spell: 44 per cent of men and 38 per cent of women have a qualification below O-Levels or the equivalent National Vocational Qualification (NVQ) level 2 while 10 per cent of men and 14 per cent of women are in the highest education category and have achieved a higher qualification or degree, equivalent to NVQ levels 4, 5 and 6. Moreover, 31 per cent of men and 35 per cent of women have not been employed between leaving full-time

[13] Those that did not provide information for more than 6 months of their employment history are dropped from the sample.

[14] Section 2.8.3 in the appendix discusses the probability of experiencing at least one unemployment spell before the age of 23 and thus being in the sample used for the estimations.

education and the beginning of their first unemployment spell. About half of all unemployment spells start between 1974 and 1976, which is when the cohort members who have obtained A-Levels or a lower degree usually leave school.

The mean duration of unemployment is 4.37 months for men and 4.76 for women, the median is 3 months for both. However, not all spells end in a transition to employment: 8 per cent of all male and 6 per cent of all female spells are censored by the interview at the age of 23. Furthermore, one per cent of male and two per cent of female spells are artificially censored after 24 months. The activity following the unemployment spell is unknown for another per cent of spells. Lastly, 7 per cent of male and 12 per cent of female unemployment spells are followed by a period out of the labour force. Thereof, 5 and 4 per cent, respectively, are transitions to education. Overall, 17 per cent of male and 21 per cent of female spells do not end with a transition to employment. Hence, it is important to use an econometric model that allows treating incomplete spells such as transitions into states other than employment differently from unemployment spells that end with a successful transition into a new job. The proportional hazard rate model used in this study is well suited for this.

Figure 2 shows Kaplan-Meier estimates of survival functions for individuals with above and below average cognitive and social skills. One can see that most individuals leave unemployment within the first 12 months. Of all unemployment spells, 93 per cent end in the first year. Furthermore, the estimated survival function for individuals with above average cognitive skills lies slightly to the left of the one for those with lower cognitive skills. This shows that individuals with above average cognitive skills at the age of 7 leave unemployment slightly faster than those with lower cognitive skills. The corresponding graph for social skills is similar; however, the difference between the two survival functions is smaller. Nevertheless, a log-rank test rejects the null hypothesis of equality of the survival functions of individuals with below and above average skills at the 99% significance level for both cognitive and social skills[15].

[15] The test statistic is 19.04 for cognitive skills and 9.21 for social skills.

2.4 Hypotheses

This section discusses potential mechanisms through which cognitive and social skills at the age of 7 may influence the duration of unemployment.

Cognitive and social skills at the age of 7 are likely to influence the duration of unemployment mainly via two channels: First, higher cognitive and social skills at the age of 7 are associated with higher skills in adolescence and adulthood[16], which may affect the length of an unemployment spell. Second, higher skills in childhood are positively related to the likelihood of achieving a higher education[17], which in turn might have an impact on the duration of unemployment (see e.g. Nickell (1979) and Kiefer (1985)). Thus, deriving hypotheses on the influence of early cognitive and social skills on the probability of finding a job requires a discussion of the effects of education and skills in adulthood on this probability. In the following, this is done by making use of predictions from job search theory (see e.g. Mortensen (1986) and Cahuc and Zylberberg (2004)) and evidence from the empirical literature on unemployment duration.

Two important objects in standard job search theory are the wage offer distribution, capturing the size of the wage an individual might earn, and the job offer arrival rate. Adult cognitive and social skills are likely to positively affect both of them by increasing an individual's productivity. The evidence on the association of cognitive and social skills with wages points in this direction[18]. Cognitive skills, such as the capacity to process information, language skills and general reasoning skills, are likely to be important for performance in most tasks in working life. At the same time, social skills may enable individuals to adjust to a new work environment more easily and thereby start focusing on the specific tasks of a job more quickly. Moreover, individuals

[16] See for example Heckman (2008), Borghans, Duckworth, Heckman and ter Weel (2008), Cunha and Heckman (2007), Caspi et al. (2003) and Dennissen, Asendorpf and van Aken (2008). The latter two studies analyse the correlation between behaviour and personality assessed in childhood and in adulthood. Dennissen et al. (2008) report positive correlations between being assessed as undercontroller or overcontroller as opposed to resilient at the age of 4 to 6 and scores in shyness and aggressiveness in early adulthood. Caspi et al. (2003) report correlations between the temperamental types undercontrolled and inhibited, assessed at the age of 3, and the Big Five personality dimensions (see e.g. Costa and McCrae (1992)) assessed at the age of 26. The scale neuroticism is positively related while openness to experience is negatively related to both temperamental types mentioned. Moreover, the scales agreeableness and conscientiousness are negatively related to the type undercontrolled and the type inhibited is negatively related to extraversion.

[17] See for example Heckman et al. (2006) and Carneiro, Crawford and Goodman (2008).

with higher social skills might be more productive in working in teams and interacting with colleagues. Besides being more productive, individuals with higher social skills are likely to have a larger social network, which they can use when searching for employment[19]. They might also incur a lower disutility from working, if they are more able to cope with stressful situations that involve dealing with other people or complex problems.

For these reasons, higher adult cognitive and social skills are likely to increase the number of jobs and the average wage offered to an individual. Holding search effort constant and not considering the indirect effect on the reservation wage, higher cognitive and social skills therefore increase the probability of finding a job. However, both a higher number of job offers and higher wage offers increase an individual's reservation wage. This has a negative effect on the exit rate to employment, rendering the overall effect ambiguous. Yet, findings in the theoretical and empirical literature suggest that the direct positive effects dominate the indirect negative effect in many cases[20]. Higher cognitive and social skills at the beginning of an unemployment spell are thus likely to exert a positive effect on the probability of making a transition from unemployment to employment.

A higher educational degree may increase the number of employment possibilities since individuals can apply also for jobs requiring a lower level of education. Moreover, education may serve as a productivity signal to employers. Assuming that the job search activity stays constant, these factors increase the job offer arrival rate. At the same time, higher educational degrees transfer into higher wages. This leads to an increase in the reservation wage, which is why the effect of a higher educational attainment on the exit rate is ambiguous, too. Previous studies on the duration of unemployment in the UK found mostly positive effects of education on the probability of leaving unemployment,

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[18] See for example Osborne (2000), Heckman et al. (2006), Carneiro et al. (2007).
[19] See literature on the importance of social networks for job search, for example Ioannides and Loury (2004) and Cappellari and Tatsiramos (2010).
[20] Van den Berg (1994) showed in the framework of a basic job search model that a large class of wage offer distributions satisfy conditions such that the effect of a higher offer arrival rate outweighs that of a higher reservation wage. That is, an increasing job offer arrival rate leads to an increase in the exit rate from unemployment. Devine and Kiefer (1991) conclude in their review of the literature that the job offer arrival rate seems to be at least as important as the reservation wage for empirically observed durations of unemployment spells.

although they were not always statistically significant (Kiefer (1985), Nickell (1979), Narendranathan and Stewart (1993) and Arulampalam and Stewart (1995)). Taking this into account and following the argument laid out in the last paragraph, a higher education is likely to increase the probability of leaving unemployment.

Summarising this section, the direction and relevance of the effect of cognitive and social skills in childhood remain an empirical question. Nevertheless, higher skills in childhood are likely to increase the probability of leaving unemployment both via increasing later skills and via increasing the probability of achieving a higher education.

2.5 Econometric model

The empirical analysis uses a proportional hazard rate model for grouped duration data as proposed by Prentice and Gloeckler (1978). This method models the probability of making a transition from unemployment to employment for each point during the unemployment spell in a continuous time framework and adjusts for the monthly structure of the observed data.

Unemployment spells end either in transitions to employment or in transitions to another state, being out of the labour force[21] or an activity for which the state is unknown. The observed duration corresponds to the minimum of the two. In addition, spells may be right censored if the individual is unemployed at the time of the interview. This paper focuses on the duration until an individual finds a job and thus does not aim at modelling transitions into other states. Assuming that, conditional on covariates, the duration until the spell ends with a transition to employment is independent of the duration until a transition into another state takes place, unemployment spells that are not followed by an employment spell are treated as right censored in the estimation[22]. The large vector of covariates in this study, including often unobserved variables like social and cognitive skills and detailed information on the family background, renders this assumption less strict.

[21] Participation in active labour market policy programmes are grouped into this category as well.
[22] Right censored spells can be used to estimate the parameters of interest in the same manner as completed spells: For each observed point in time, they contain the information that no transition into the state of interest occurred. They are, however, not informative on when such a transition takes place.

The continuous time hazard function for individual i, that is the probability of making a transition to employment at time t, is modelled as the product of the baseline hazard $\lambda(t)$, capturing the duration dependence, and a term that captures the effect of covariates:

(1) $$h(t, Z_{it}) = \lambda(t) \cdot \exp\{Z_{it}^{'}\beta\}$$

The covariate vector Z_{it} includes time-varying and time-constant variables and a constant as its first element. The corresponding coefficient vector is denoted by β and the complete time path of covariates until time t by $\{Z_{it}\}$. The baseline hazard is modeled as a piecewise constant function in order to avoid imposing strong functional form assumptions:

(2) $$\lambda(t) = \exp\left\{\sum_{l=2}^{L} I_l(t) \cdot \alpha_l\right\}$$

Each $I_l(t)$, for $l = 2,...,L$, denotes an indicator variable equal to one if t lies in the time period l and zero otherwise. α_l are the corresponding coefficients. I include indicator variables for every month in the first year in unemployment and one indicator for the second year in the estimation[23].

In the data, durations are observed in monthly intervals $\lfloor t_g - 1, t_g)$ for $g = 1,...,24$. In order to adjust for the grouping structure, the covariate vector Z_{it} is assumed to be constant within each month g and denoted by Z_{ig}. The grouped hazard function $h^d(g, Z_{ig})$ denotes the probability of making a transition to employment in the g^{th} interval, that is in the interval $\lfloor t_g - 1, t_g)$, given survival until the beginning of the

g^{th} interval:

(3) $$h^d(g, Z_{ig}) = \Pr ob\left(T \in \left[t_g - 1, t_g\right) \middle| \, T \geq t_g - 1, Z_{ig}\right)$$

The grouped survival function denotes the probability of surviving through the g^{th} interval:

(4) $$S^d\left(g,\{Z_{ig}\}\right) = \Pr ob\left(T \geq t_g \mid \{Z_{ig}\}\right)$$

Following Jenkins (1995), the grouped survival and hazard functions can be expressed in terms of the parameters of the continuous time model in the following way:

(5) $$h^d(g, Z_{ig}) = 1 - \exp\left\{-\exp\left\{Z_{ig}^{'}\beta + \gamma_g\right\}\right\}$$

(6) $$S^d\left(g,\{Z_{ig}\}\right) = \prod_{j=1}^{g}\left(1 - h^d\left(j, Z_{ij}\right)\right)$$

with

(7) $$\gamma_g = \ln \int_{t_g - 1}^{t_g} \lambda(u)du .$$

Letting $\delta_i = 1$ denote a transition to employment and $\delta_i = 0$ a censored spell, the log-likelihood reads

(8) $$\begin{aligned}\log L\left(\beta,\gamma \,\Big|\, \left\{g_i, \{Z_{ig_i}\}\right\}_{\mathrm{i=1,..N}}\right) = \sum_{i=1}^{N} \delta_i \cdot \log\left[h^d\left(g_i, Z_{ig_i}\right) \cdot \prod_{j=1}^{g_i - 1}\left(1 - h^d\left(j, Z_{ij}\right)\right)\right] \\ + \left(1 - \delta_i\right) \cdot \log\left[\prod_{j=1}^{g_i}\left(1 - h^d\left(j, Z_{ij}\right)\right)\right],\end{aligned}$$

where γ denotes the vector containing all γ_g.

As derived in Jenkins (1995), this log-likelihood may also be written and estimated in the form of a sequential binary choice model. To see this, define $y_{ig} = 1$ if there is a transition to employment in interval g and zero otherwise:

(9) $$\log L\left(\beta,\gamma \,\Big|\, \left\{g_i, \{Z_{ig_i}\}\right\}_{\mathrm{i=1,..N}}\right) = \sum_{i=1}^{N}\sum_{j=1}^{g_i} y_{ij} \cdot \log\left[h^d\left(j, Z_{ij}\right)\right] + \left(1 - y_{ij}\right) \cdot \log\left[1 - h^d\left(j, Z_{ij}\right)\right]$$

Given the formula for the grouped hazard in equation (5), this corresponds to the log-likelihood in a generalized linear model with a binary dependent variable and complementary log-log link function (Jenkins, 1995). I estimate the parameter vectors β and γ using maximum likelihood estimation.

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[23] The indicator for the first month is left out of the estimation and used as reference category. Allowing for a more flexible shape for the second year of the spell does not change the results, which may be due to the small number of

2.6 Results

2.6.1 Main specifications

Table 2 presents the estimation results, displaying hazard ratios[24] and corresponding standard errors in parentheses. I estimate the model separately for men and women, as the determinants of the duration of unemployment may differ by gender. The first and third column present results from estimations not controlling for education. The skill variables will therefore capture the overall effect of skills at the age of 7 on the hazard, including the effect that goes through the channel of achieving a higher education. Column 2 and 4 show results controlling for educational attainment.

In all estimations, I control for whether the individual is married or has children at the beginning of the spell, has had a job since leaving full time education and for the socio-economic status (SES) of the father or male head of household when the child was 7 years old. Furthermore, I include the monthly claimant count rate of Great Britain, whether it is autumn or winter as opposed to spring or summer, region of residence at the age of 16[25] , year dummies in order to control for macroeconomic changes in the economy and changes in the social security system[26] and indicator variables capturing the duration dependence.

The first and third column of Table 2 show that social skills at the age of 7 have a significant association with the probability of leaving unemployment both for men and women. An increase in social skills of one standard deviation increases the probability of finding a job at each point during the spell by 6.6 per cent for men and 11.8 per cent for women at the sample mean of cognitive skills. Higher age 7 cognitive skills have a similar effect on the hazard. A one standard deviation increase raises the probability of leaving unemployment by 6.1 per cent for men and 12.7 per cent for women at the

spells that last longer than 12 months.

[24] The hazard ratio measures the change in the hazard rate associated with a change in the corresponding variable by one unit, holding all other variables fixed. It is calculated by exponentiating the estimated coefficient.

[25] If this is not available because the individual did not respond in the corresponding interview, I use the region of residence at the age of 11.

[26] See Clasen (1994) and Mittelstädt and Veil (1975) for a detailed description of the British social security system in the 1970's and 80's. Clasen (1994) reports that there were no changes in the eligibility or entitlement period of unemployment benefits between 1973 and 1981, which is the relevant period for this analysis. However, there were changes in the level of benefits and the benefit rates, which basically eroded the real value of unemployment insurance benefits and the earnings-related supplement over time.

sample mean of social skills. The interaction term of cognitive and social skills at the age of 7 is significantly smaller than one for men, indicating that the two skills are substitutes and can compensate each other to some degree. However, the interaction effect is not significantly different from one for women.

In order to test whether the positive effect of age 7 skills on the hazard is solely due to an increase in the probability of obtaining a higher education, the second and fourth column of Table 2 report results controlling for the level of education. I include indicator variables on having O-Levels/NVQ 2, A-Levels/NVQ 3 or a degree/NVQ 4-6 in the estimation, with having less than O-Levels/NVQ 2 being the reference category. One can see that the hazard ratios for social skills are still highly significant, showing that higher social skills at the age of 7 reduce the duration of unemployment not only via the channel of a higher level of education. Likewise, age 7 cognitive skills increase the probability of finding a job even when controlling for the educational degree. Due to the interaction effect, the hazard ratios presented in the table correspond to the effect of cognitive and social skills at the sample mean of the respective other skill, which is zero by construction. In order to see whether the effect of a one standard deviation increase in skills is relevant at other points of the distribution of the respective other skill, Figure 3 depicts the hazard ratios along the support of the other skill[27]. Moreover, an estimate of the density of the respective other skill gives an impression of the share of individuals that is located at a given point in the support. The graph in the upper right hand corner of Figure 3 shows that cognitive skills significantly increase the hazard rate for men with lower values of social skills, but that the effect is not statistically different from one when social skills are zero or larger. The two lower graphs show the corresponding hazard ratios for women. However, since the interaction term is not statistically different from one, the effect of cognitive and social skills does not differ significantly along the support of the respective other skill[28]. Furthermore, the results

[27] The depicted hazard ratio for cognitive skills, and correspondingly for social skills, is calculated as follows:
$$hazard\ ratio_{\text{cognitive}} = \exp\{\beta_{cognitive} + social\ skill \cdot \beta_{\text{cognitive}\cdot\text{social}}\}$$

[28] The proportional hazard rate model assumes that the baseline hazard and the covariates act proportionally on the hazard. This might be violated if some covariates are more important at certain points during the spell than at others. Additional analyses including interaction terms of the duration and cognitive and social skills in the estimation indicated that cognitive skills might be more relevant in the second half of the first year in unemployment, especially

show that having an educational degree of O-levels/NVQ 2 or more increases the probability of finding a job by 25 to 58 per cent for women. For men, only having O-levels/NVQ 2 significantly increases the hazard while the higher qualification levels show positive but insignificant associations with the probability of making a transition to employment.

In duration models, unobserved heterogeneity may lead to biased coefficients even if it is uncorrelated with the covariates at the beginning of the spell (see for example van den Berg (2001) and Nicoletti and Rondinelli (2010) for a discussion). In order to explore whether the results are subject to this bias, I also estimated the model with a normally distributed unobserved heterogeneity term, but found almost no change in the estimated coefficients[29].

Results across all specifications show that the relation between being married at the beginning of the unemployment spell and the duration of the spell differs for men and women. Being married is associated with a significant increase in the hazard rate by about 40 per cent for men. Married women, however, have an about 30 per cent lower probability of leaving unemployment at any point during the spell. Having children at the beginning of the spell reduces the probability of finding a job for men by about 33 per cent and for women by about 62 per cent. The father's socio-economic status at the age of 7 is of different relevance for men and women. It is significantly associated with the hazard of leaving unemployment only for men. Having a father of medium as opposed to low SES relates to an increase in the hazard rate by about 13 per cent, while the effect of a father of high SES is also positive but not significantly different from zero. In addition, labour market characteristics play a role in explaining the hazard of leaving unemployment: both for men and for women, a one percentage point increase in

for women. However, as the results on a time-varying effect of skills were not very robust, the model without the interaction is being used.

[29] Estimating the model with a Gamma distribution also does not indicate the presence of unobserved heterogeneity, the coefficients converge to almost the same values as in the estimation without frailty. However, the algorithm runs into numerical problems as the variance of the unobserved heterogeneity tends to zero. For men, I find some evidence for unobserved heterogeneity when estimating the model with a discrete frailty distribution with two mass points, but compared to a model without frailty, the estimated coefficients for cognitive and social skills do not change qualitatively. Yet, the estimated coefficients for the baseline hazard differ, pointing out that they should not be interpreted as true duration dependence in the models omitting unobserved heterogeneity. For women, the estimation of the model with a discrete frailty distribution exhibits numerical problems.

the claimant count rate is associated with a decrease in the probability of leaving unemployment by about 20 per cent. Results on the other variables that are included in the estimation are reported in Table 3.

In order to interpret the economic significance of the effects of cognitive and social skills one can compare them to the estimated effects of other variables influencing the duration of unemployment. For example, a decrease in the national claimant count rate by one percentage point is associated with an increase in the probability of leaving unemployment by 21 per cent for women. This corresponds to a shift of the claimant count rate from the 25th to the 75th percentile in its distribution during the observation period of this study. In order to achieve an equally large increase in the hazard rate by changing social or cognitive skills, one would have to increase a woman's skills by 1.71 standard deviations. This corresponds to moving a woman from the 20th to the 75th percentile in the distribution of social skills. Thus, even though this would imply a sizable shift, the effect of cognitive and social skills in childhood is not to be disregarded for women. For men, the economic conditions are more important for the probability of finding employment than skills in childhood. Achieving the same effect as is associated with a decrease in the claimant count rate by one percentage point would require social skills to increase by more than three standard deviations, which spans almost the entire support of the distribution of social skills. However, due to the significant interaction effect, the required shift decreases for lower values of cognitive skills. Moreover, the importance of early skills relative to educational degrees is not as low for men as when compared to macroeconomic conditions. Yet, comparing the effect of skills to that of educational degrees is problematic. Keeping the latter fixed underestimates the relative importance of early skills since they also increase the probability of achieving a higher education (see e.g. Carneiro et al. (2007)). In addition, measuring a latent concept such as cognitive and social skills is likely to be less precise than measuring the national claimant count rate or an educational degree. The coefficients on cognitive and social skills may therefore underestimate the influence of skills in childhood.

2.6.2 Robustness

2.6.2.1 Confounding factors

This section explores whether the results on the importance of skills in childhood are driven solely by the family or school environment of the child. School characteristics and the family background likely affect both early and later skills. Moreover, the family background might have a direct effect on the probability of finding employment. An association between early skills and the duration of unemployment might therefore arise from a correlation between the school and family background and early and later skills. I address this issue in the previous estimations by controlling for the socio-economic status of the father or male head of household at the age of 7. Indeed, having a father with a higher SES is positively related to the probability of leaving unemployment, even though this is only significant for men. Yet, this measure might not capture all confounding factors. For this reason, I repeat the estimations including several further control variables measured at the age of 7 and grouped into the categories family background, parenting activities and school characteristics[30]. The results are shown in Table 4. The first column repeats the baseline results from the first and third column in Table 2. Columns 2 to 4 show results from separately adding the different groups of control variables. In the last column, all variables are included at the same time.

The inclusion of further control variables has no large impact on the hazard ratios and significance levels of cognitive and social skills[31]. Therefore, the findings in this section provide some evidence against concerns that the effect of cognitive and social skills at the age of 7 on the duration of unemployment is solely due to confounding factors such as the family background or schooling characteristics.

2.6.2.2 Heterogeneous effects with respect to education

Until now, the proportional effect of skills at the age of 7 on the hazard of finding a job is restricted to be the same for all individuals. However, the importance of skills

[30] See Section 2.8.2 in the appendix for a list of these variables.

[31] When further including control variables that are measured at the age of 11 and 16, the hazard ratios and corresponding significance levels change only slightly and the qualitative results stay robust.

with respect to the probability of leaving unemployment might differ for individuals with different levels of education. They may search for different kinds of jobs for which cognitive and social skills are more or less relevant. Furthermore, it is possible that employers trust higher education to be a signal for higher cognitive and social skills, while putting more effort into evaluating skills of individuals that have a low educational degree. This effect would increase the importance of skills for low educated individuals. In order to explore whether the effect of skills on the probability of finding a job is heterogeneous with respect to education, I estimate the model including interaction terms of skills and the education variables.

The baseline estimation that restricts skills to have a homogeneous effect is reported in the first and the third column of Table 5 for men and women, respectively. Columns 2 and 4 report the hazard ratios of age 7 skills for the different levels of education from the model with interaction terms. Concerning men, cognitive and social skills only have a significant influence on the probability of leaving unemployment for those with an education below O-levels/ NVQ 2. For women, the same result emerges for cognitive skills, while social skills are significant only for those with A-levels or O-levels respectively NVQ 2-3. However, the standard errors of these effects are large and, in most cases, the effects of skills do not vary statistically significantly across educational degrees. In the estimation for women, the effect of cognitive skills slightly differs between those with a degree/ NVQ 4-6 and those with less than O-levels/ NVQ 2 but does not significantly differ between other education levels. In the estimation for men, the effect of social skills differs significantly only between those with O-levels and those with less than O-levels. The hazard ratio on cognitive skills is marginally significantly different only between those with less than O-levels and those who have A-levels or a degree. Given the small sample size in the higher education categories, this exercise can only point to possibly heterogeneous effects of early skills, suggesting that they might be more important for low than for highly educated men in reducing the duration of unemployment. However, a larger sample would be necessary in order to achieve robust statistical evidence on this issue.

2.6.2.3 Investigating the measure for social skills

Results until now are based on one specific way of aggregating the information contained in the BSAG, namely defining an overall score. This section presents estimations using two alternative measures for social skills that are also derived from the BSAG. The first alternative disentangles the BSAG into two facets, the second uses the overall score to construct indicators for different categories of social skills.

For the first alternative, following Ghodsian (1977), and similar to Stott (1974) and Osborne (2000), I construct two scores from the BSAG syndrome scores, labelled "over- react" and "underreact". The score on overreact is the sum of the scores on the syndromes anxiety for acceptance by adults, hostility towards adults, anxiety for acceptance by children, hostility towards children, restlessness and inconsequential behaviour. According to Ghodsian (1977), this factor captures rather aggressive, restless and anxious behaviour. The score on underreact is generated by summing the syndrome scores for unforthcomingness, withdrawal, depression and miscellaneous nervous symptoms and represents rather withdrawn and inhibited behaviour[32] [33].

In order to be consistent with the main measure for social skills and to facilitate comparability, I reverse the sign of the resulting scores such that a higher value of overreact and underreact symbolizes higher skills in the respective dimension. Moreover, the scores are normalized to have mean zero and variance one in the sample of all unemployed individuals. The means and standard deviations of the resulting scores are displayed in the upper panel of Table 6 for men and women separately. In both dimensions, girls' social skills at the age of 7 are higher than those of boys.

For the second alternative, I follow Stott (1974) and Davie (1973) in defining three categories for individual's social skills. Those that have an overall BSAG-score of less than 10 are termed stable, those that have a score between 10 and 19 are termed un-

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[32] The grouping of syndromes emerges from a principal component analysis using the varimax rotation method. When using other rotation methods or when not rotating the factor loadings, other factors might emerge even though they are often similar to the ones used here.

[33] Several studies have related concepts of behaviour and temperament in childhood to the dimensions of the five-factor model in adulthood (see for example Caspi et al. (2003) and John and Srivastava (1999)). Caspi et al. (2003) find that inhibited behaviour at the age of 3, which might be related to the factor underreact measured here, is negatively related to the Big Five factors extraversion and openness to experience and slightly positively related to neuroticism at the age of 26. Undercontrolled behaviour at the age of 3, which captures rather impulsive, restless and

settled, and those with a score of 20 or higher are termed maladjusted. The lower panel in Table 6 shows the proportion of individuals in each of the categories for men and women separately. This measure also reflects that, in this sample, women have higher social skills at the age of 7 than men. 71% of women but only 55% of men belong to the category stable.

Table 7 shows the hazard ratios from estimations that include the alternative measures for social skills but are otherwise equivalent to the main model as presented in the second and fourth column of Table 2. One can see in the upper panel that less withdrawn behaviour at the age of 7 significantly increases the probability of leaving unemployment by 8.2 per cent for men at the mean of cognitive skills. The significant interaction term of cognitive skills and overreact implies that more aggressive behaviour is more harmful for men with low levels of cognitive skills. It also means that higher cognitive skills significantly increase the probability of finding employment for those who have a lower overreact score, that is those who are more aggressive. For women, a one standard deviation increase in underreact, meaning being less inhibited, is associated with a 7.1 per cent higher probability of finding employment. In addition, less aggressive behaviour is associated with a modestly significant increase in the hazard rate by 5.7%[34].

The lower panel of Table 7 presents results from estimating the model using the indicator variables for social skills described above. Being stable at the age of 7 is used as the reference category. For men, being unsettled as opposed to stable at the age of 7 has no significant effect on the hazard rate. However, being maladjusted as opposed to stable decreases the hazard rate by 16% at the overall mean of cognitive skills. This is comparable in magnitude to the effect of having O-levels in contrast to having a lower education. However, the hazard ratio increases and is no longer significantly different from one for high levels of cognitive skills. A one standard deviation increase in cognitive skills significantly increases the probability of finding employment by almost

negative behaviour, and might thus be comparable to ”overreact”, is found to be negatively related to the scales agreeableness, conscientiousness and openness to experience and positively to neuroticism.

[34] Together with the findings on the relation between childhood behaviour and the Big Five, this is in line with the results in Uysal and Pohlmeier (2011), who report a positive association between the probability of finding

20% for individuals that are in the category maladjusted, but is not significantly different for individuals with higher social skills. The results for women follow a similar pattern, but in addition, being unsettled as opposed to stable also significantly decreases the probability of leaving unemployment. These findings suggest that, especially for men, the results on the importance of cognitive and social skills at the age of 7 are driven to a large extent by individuals that are in the lower distribution of childhood social skills.

2.6.2.4 Do the results change when using skills measured at the age of 11?

According to the hypothesis, when controlling for educational attainment, skills at the age of 7 are important for the duration of the first unemployment spell because they are positively correlated with skills later in life, which in turn influence the duration of an unemployment spell. As a further robustness check I therefore explore whether skills measured after the age of 7 are related to the probability of finding a job.

The NCDS also provides measures of cognitive and social skills at the age of 11. Again, social skills are measured by asking the children's teachers to fill out the BSAG questionnaire. Cognitive skills at the age of 11 are assessed with a math, reading, copying designs and general ability test. In order to be able to compare the results, I use only individuals for whom information on skills at the age of 7 and 11 is available and estimate the model for this sample, once including age 7 skills and once including age 11 skills. The number of observations reduces to 1,893 for men and 1,788 for women, with the mean of cognitive and social skills at the age of 7 being slightly larger in the reduced sample[35]. That is, disproportionately many individuals with lower skills are excluded from the sample, which may attenuate the estimated hazard ratios.

Table 8 presents Pearson correlation coefficients between cognitive and social skills measured at the age of 7 and 11 for women and men separately. The correlation between cognitive skills measured at the age of 7 and 11 is 0.69 both for men and women, which is in line with the finding of a high rank-order stability of cognitive skills

employment and the scales conscientiousness for men and openness to experience for women and a negative effect of neuroticism for men.

early in life (see e.g. Borghans et al. (2008)). Social skills are less correlated between the age of 7 and 11 with a correlation coefficient of 0.34 for men and 0.36 for women. This correlation is somewhat lower than estimates found for various other measures of personality in other studies, but given the time span between the measurements and the assessed measure it is still in a similar range as estimates reported in Roberts and DelVecchio (2000). The reason for the correlation of social skills being lower than that of cognitive skills in this study may be attributed to two factors: First, personality traits are in general found to be less rank-order stable in childhood than cognitive skills (Borghans et al., 2008). Second, the measurement of cognitive skills via tests is less subjective and might therefore be less prone to measurement error than teacher assessments of children's social skills (Borghans et al., 2008).

Table 10 presents estimation results for men in the upper and results for women in the lower panel. Comparing the first and second column, one can see that also social skills at the age of 11 have a positive influence on the probability of finding a job, even though they are no longer statistically significant in the estimation for women. An increase in cognitive skills at the age of 11 is associated with a significant increase in the probability of making a transition to employment for women and, for lower values of social skills, also for men. Moreover, the hazard ratios for cognitive skills at the age of 11 are larger than those for skills at the age of 7. This is reasonable if age 11 cognitive skills are a better proxy for cognitive skills at the beginning of the unemployment spell than age 7 cognitive skills and, at the same time, cognitive skills in adulthood reduce the duration of unemployment.

Columns 3 and 4 of Table 10 report results using the categories stable, unsettled and maladjusted as measures of social skills[36]. For men in the lower part of the age 11 cognitive skills distribution, both being unsettled and being maladjusted as opposed to stable at the age of 11 significantly decreases the probability of finding employment (see Figure 4). In addition, as found for age 7 cognitive skills, an increase in cognitive skills

[35] Note that skills at the age of 7 are standardized in the smaller sample for the estimations.

[36] Table 9 displays the share of individuals in the different categories of social skills measured at the age of 7 and 11. 25% of girls that were maladjusted at the age of 7 are still termed maladjusted at the age of 11, while 42% of them are in the category stable. Concerning boys, a larger share, namely 38%, of those who were in the category maladjusted

at the age of 11 significantly increases the hazard rate for individuals that have low social skills. For women, the lower panel of Table 10 and Figure 4 show that being unsettled or maladjusted as opposed to stable at the age of 11 is negatively associated with the hazard rate. Yet, only the coefficient on being unsettled is significantly different from zero at the 90% confidence level. An increase in cognitive skills significantly increases the hazard rate for women that are in the categories stable or unsettled at the age of 11, but does not seem to have an effect for women in the lowest social skills category.

Summarizing, also higher cognitive and social skills at the age of 11 positively influence the probability of making a transition from unemployment to employment. However, especially for women, the results on social skills are not as statistically significant as those found using measures taken at the age of 7 which points at the need for further research on the channels via which social skills at the age of 7 influence the duration of unemployment[37].

2.7 Conclusion

This paper studies how social and cognitive skills in childhood are related to the duration of an individual's first unemployment spell in adolescence and early adulthood by estimating a flexible proportional hazard rate model.

The results show that higher cognitive and social skills at the age of 7 are associated with an increased probability of finding employment. For men, cognitive and social skills are only relevant for individuals in the lower part of the distribution of the respective other skill. That is, those with below average social skills benefit from an increase in cognitive skills and vice versa. Correspondingly, the effect of social skills seems to be driven by those in the lowest social skills category at the age of 7 for men. For women, the negative interaction effect is less pronounced. Adding education to the estimation slightly reduces the estimated hazard ratios but leaves the qualitative results unchanged. Moreover, the estimates are robust to controlling for parenting activities,

at the age of 7 are still in the category maladjusted at the age of 11. Moreover, only 142 women are termed maladjusted at the age of 11.

family background and school characteristics at the age of 7. In addition, also skills measured at the age of 11 are related to the probability of finding employment. However, the point estimates for social skills are smaller and of lower statistical significance when using skills measured at the later age. These results speak in favour of the hypothesis that the importance of cognitive skills at the age of 7 for the probability of finding employment is due to the importance of later skills. However, at the same time they point at the need for more evidence on the channels via which early social skills are related to the duration of an unemployment spell.

The estimates suggest that the economic significance of the effect of cognitive and social skills at the age of 7 is comparable to that of the national claimant count rate at the beginning of the unemployment spell for women. For men, the relative importance seems to be considerably smaller. However, since measuring skills is likely to be less precise than measuring a conventional economic variable such as the claimant count rate, the relative importance of skills may be underestimated.

The results of this study offer more insights into the finding of Gregg (2001) by showing that the reduced number of months spent in unemployment that is associated with higher social skills is also explained by shorter individual unemployment spells and not purely driven by the propensity to become unemployed. Moreover, the results add to the literature on the relation between the duration of unemployment spells and cognitive and noncognitive skills by providing evidence on the importance of skills in childhood, jointly analysing both dimensions of skills and examining the effect both for men and women. Even though the positive association between early skills and the length of an unemployment spell is not established using exogenous variation, controlling for a large range of background factors does not change the results. This suggests that policy measures aiming at increasing early cognitive and social skills contribute also to reducing the risk of long unemployment episodes. Moreover, in light of recent debates on achievements in international pupil tests that often focus on cognitive skills, this study provides additional evidence showing that one should not

[37] It could be that social skills at the age of 7 mostly work through increasing later cognitive skills, and once these are controlled for, age 7 social skills are no longer relevant. However, when including skills measured at the age of 7 and at the age of 11 at the same time in the estimation, social skills at the age of 7 are still highly significant.

neglect investing in children’s social skills since they are related to later outcomes in a similar way as cognitive skills.

A limitation of this study is that the evidence builds on the experience of one specific cohort in their early years on the labour market. Future research should therefore gather more evidence on the relationship between cognitive and social skills and the duration of unemployment, using different cohorts and measuring skills at different points in the lifecycle.

2.8 Appendix

2.8.1 Construction of dataset

Using the information on education provided in NCDS 4, I derive an individual's qualification at the beginning of the unemployment spell. There are several questions asking the individuals about qualifications obtained during training courses, apprenticeships, or any other education since leaving school. I aggregate the different qualifications according to the description provided by John Bynner in the guide accompanying the documentation for NCDS 5 "NCDS5 - Derived Variables 1" (Smith 2000). Following this, qualifications are grouped into 6 categories: no qualifications, CSE 2-5/ equivalent NVQ 1[38], O-Level/ equivalent NVQ 2, A-Level/ equivalent NVQ 3, higher qualification/ equivalent NVQ 4 and degree/ higher NVQ 5 and NVQ 6. In the empirical analysis I further aggregate these categories and form the following groups of highest achieved qualifications: below O-Levels/ NVQ 0-1, O-Levels/ NVQ 2, A-Levels/ NVQ 3 and higher qualification or degree/ NVQ 4-6. I construct the complete qualification biography for each individual by recording at which point in time she received a qualification. Whenever it is not possible to determine the exact date, I assign the lower qualification level until the higher one is certainly obtained. I include a dummy in the estimation, indicating whether or not the complete qualification history could be constructed in this way in order to control for a potential structural pattern of incompleteness. The coefficients on this dummy are almost never significantly different from zero.

In order to control for family status, I record for each cohort member whether she is married or not and has children or not at each point in time, using information on marital status and biological children which was collected in NCDS 4. Additionally, I control for the cohort member's family background during her own childhood. The social class of the father or male head of the household is reported in the form of an

[38] NVQ stands for National Vocational Qualification level.

index in NCDS 1[39]. In order to generate a measure of socio-economic background I group the social class categories according to the following rule: I assign a high socio-economic status (SES) if the father belongs to social class I or II, a medium SES if the father belongs to social class III and a low SES if the father belongs to social class IV or V. I also create a category for not having a male head of household.

In case information on a variable is missing, I assign a value and create a dummy variable that indicates this. This indicator is included in the estimation in order to control for potential selectivity in missings. This makes it unnecessary to drop an individual because of missing information on one control variable[40]. In case of missing information in a dummy variable I replace the missing with zero. Missings in continuous variables are replaced with the mean and missings in discrete variables are replaced with the median.

In order to control for regional variation in macroeconomic conditions, I include the latest information on the region of residence. This is the region that the cohort members report in NCDS 3, which is at the age of 16. If this is unknown, I use the information provided in NCDS 2. The monthly claimant count rates for Great Britain are taken from the website of the Office for National Statistics.

2.8.2 List of covariates

The following control variables, reported at the age of 7, are additionally included in the estimation in Table 4.

- family background:

  socio-economic status of father or male head of household; whether the child was an only child; number of household members; birth order; whether mother stayed on at school after the minimum school leaving age; father's years of education; mother's and her husband's age at childbirth; whether the father reads a lot of books; whether the mother reads a lot of

[39] Information on the employment and occupational status according to the General Register Office (GRO) 1960 classification was used to construct this index.

[40] For a discussion of the treatment of missing information in a linear estimation framework see Jones (1996).

books; whether the mother speaks mostly English with the child; whether the family has any difficulties as assessed by the health visitor[41]

- parenting activities:

  whether the parents would like the child to stay on at school after the minimum school leaving age; whether the mother reads to/ with the child; whether the father reads to/ with the child; whether the father takes the child outside; whether the father takes an active role in raising the child as seen by the mother; whether the mother started working before the child started school; whether the mother started working after the child started school; whether the mother shows interest in the child's education as seen by the teacher; whether the father shows interest in the child's education as seen by the teacher; whether the parents have actively sought to discuss the child with the teacher in school

- school characteristics:

  whether the child is at an infant as opposed to a junior school or other type of school; number of pupils in the child's class; whether the school has a parent/teacher association; whether the school arranges meetings with the parents on educational matters; whether the school organizes any social functions for parents; whether parents provide substantial help in money, kind or labour to the school

2.8.3 Who experiences unemployment before the age of 23?

This paper analyses the duration of an unemployment spell for individuals who became unemployed for more than two weeks at least once between the age of 16 and 23. Those who did not become unemployed during this time have no observations on the outcome variable and are therefore not included in the sample used in the estimations. Nevertheless, it is likely that the probability of becoming unemployed and being in the sample is not random. This section therefore explores which factors increase the likelihood of being in the sample used for the duration analysis.

[41] Including difficulties in the following areas: housing, financial, physical illness or disability, mental illness or neurosis, mental subnormality, death of child's father, death of child's mother, divorce, separation or desertion,

Table 11 contrasts the mean and standard deviation of explanatory variables for those who have experienced unemployment and those who have not. Average skills at the age of 7 are higher among those who were not unemployed before the age of 23 than among those who experienced unemployment. The latter have an average score of cognitive skills of -0.09 and average social skills of -0.11. The corresponding figures for those who were not unemployed are 0.07 for cognitive skills and 0.08 for social skills. Hence, there is a difference of about 16 to 19 per cent of a standard deviation between the two groups, indicating that these skills are related to an increased probability of becoming unemployed before the age of 23. Furthermore, among those who were not unemployed before the age of 23, the percentage of individuals who stayed on at school after the age of 16 is slightly higher, with 29% as opposed to 26%.

Table 12 presents results from a probit estimation of the probability of experiencing unemployment until the age of 23[42]. This exercise further explores the composition of individuals that are in the sample for the analysis of the duration of the first unemployment spell. Columns 1 and 2 refer to estimations for men, 3 and 4 to those for women. Specification (a) includes skills at the age of 7, whether the individual stayed on at school after the age of 16, father's socioeconomic status and region of residence at the age of 16 as right-hand side variables. The impression from the descriptive statistics is partly confirmed in this estimation. Higher cognitive and social skills are significantly associated with a reduced probability of becoming unemployed. Having stayed on at school after the age of 16, which serves as a proxy for educational attainment, is positively but not significantly correlated with the probability of becoming unemployed. However, the effect of having stayed on at school is hard to interpret. Staying on at school after the age of 16 also reduces the time at risk of becoming unemployed until the age of 23 and implies that individuals entered the labour market in a different year and possibly different economic environment[43]. A higher SES of the father is only significantly negatively related to the probability of becoming unemployed for men.

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domestic tension, "in-law" conflicts, unemployment and alcoholism.

[42] Note that Hobcraft (1998) has done a similar analysis with the NCDS, examining the probability of ever becoming unemployed until the age of 33 for men. He finds that cognitive and social skills in childhood are related to educational achievement, but not directly to the probability of experiencing unemployment until the age of 33.

Specification (b) additionally controls for information on the family background, parenting activities and school characteristics[44]. This decreases the average partial effect of cognitive skills, which is no longer significantly different from zero. However, a one standard deviation increase in social skills is still associated with a significant decrease of 3.4 (2.0) percentage points in the probability of becoming unemployed, and thus in the probability of being in the sample for the duration analysis, for men (women).

[43] The claimant count rate in Great Britain slightly increased from 1974 onwards, which is the year when cohort members turned 16 and finished compulsory education.
[44] A detailed list of the included variables can be found in Section 2.8.2 in the appendix.

2.8.4 Tables and figures

Table 1: Descriptive statistics

	MEN		WOMEN	
	mean	std. dev.	mean	std. dev.
duration of unemployment spell	4.37	4.59	4.76	4.83
spell censored	0.17		0.21	
- by interview	0.08		0.06	
- at 24 months	0.01		0.02	
- following activity unknown	0.01		0.01	
- by transition out of the labour force	0.07		0.12	
-- thereof: transition into education	0.05		0.04	
cognitive skills	-0.05	1.02	0.05	0.98
social skills	-0.17	1.05	0.18	0.91
less than O-Levels/ NVQ 0-1	0.44		0.38	
O-Levels/ NVQ 2	0.28		0.38	
A-Levels/ NVQ 3	0.18		0.11	
degree/ NVQ 4-6	0.10		0.14	
no exact education biography	0.03		0.02	
married	0.08		0.15	
parent	0.05		0.06	
socio-economic status of father at the age of 7				
low SES	0.26		0.24	
high SES	0.16		0.17	
medium SES	0.50		0.52	
no male head of household	0.03		0.02	
SES missing	0.05		0.05	
no previous employment	0.31		0.35	
autumn or winter	0.36		0.36	
claimant count rate	4.78	1.77	4.65	1.6
year spell started in				
1974	0.17		0.15	
1975	0.14		0.18	
1976	0.16		0.2	
1977	0.11		0.11	
1978	0.07		0.08	
1979	0.11		0.10	
1980	0.14		0.12	
1981	0.10		0.07	
region of residence at the age of 16				
Wales	0.07		0.07	
North	0.09		0.10	
North West	0.15		0.15	
E & W.Riding	0.09		0.09	
North Midlands	0.07		0.07	
Midlands	0.10		0.11	
East	0.08		0.06	
South East	0.13		0.12	
South	0.05		0.06	
South West	0.06		0.06	
Scotland	0.11		0.11	
number of observations	2223	2223	2064	2064

Notes: The descriptive statistics of time-varying variables, that is the season dummy, year dummies and the claimant count rate, correspond to the values at the beginning of the unemployment spell.

Table 2: Estimation results of duration model

	MEN		WOMEN	
	without education	with education	without education	with education
social skills	1.066**	1.060**	1.118***	1.105***
	(0.028)	(0.028)	(0.036)	(0.036)
cognitive skills	1.061**	1.042	1.127***	1.078**
	(0.029)	(0.030)	(0.033)	(0.034)
cognitive skills	0.948**	0.948**	0.971	0.967
× social skills	(0.021)	(0.021)	(0.027)	(0.027)
O-Levels/ NVQ 2		1.149**		1.247***
		(0.072)		(0.080)
A-Levels/ NVQ 3		1.096		1.577***
		(0.093)		(0.167)
degree/ NVQ 4-6		1.190		1.498***
		(0.140)		(0.167)
married	1.384***	1.419***	0.676***	0.693***
	(0.161)	(0.167)	(0.061)	(0.063)
parent	0.665***	0.670***	0.358***	0.380***
	(0.103)	(0.104)	(0.059)	(0.063)
high SES	1.129	1.110	1.099	1.033
	(0.090)	(0.090)	(0.094)	(0.089)
medium SES	1.141**	1.139**	1.059	1.047
	(0.064)	(0.064)	(0.065)	(0.064)
no previous employment	0.932	0.898*	1.221***	1.090
	(0.057)	(0.059)	(0.074)	(0.071)
claimant count rate	0.788***	0.783***	0.826***	0.811***
	(0.039)	(0.039)	(0.045)	(0.044)
autumn or winter	0.898**	0.898**	1.048	1.050
	(0.044)	(0.044)	(0.055)	(0.055)
number of observations	2,223	2,223	2,064	2,064

Notes: The table displays hazard ratios and corresponding standard errors in parentheses. Having a father of low SES or not having a male head of household is the base category for father's SES. Missings in the variable on SES were replaced by a zero. A dummy variable indicating this was included in the estimation. Another dummy variable indicating whether there were missings in the information on the qualification level was also included. None of these was significant in the estimations. Further control variables in the estimation are the region of residence, year dummies and the piecewise constant time specification. *** indicates significance at the 99% level, ** at the 95% level and * at the 90% level.

Table 3: Estimation results of duration model - variables not presented in Table 2

	MEN		WOMEN	
	without education	with education	without education	with education
constant	0.673**	0.684**	0.369***	0.385***
	(0.112)	(0.114)	(0.067)	(0.071)
piecewise constant baseline specification				
month 2	1.090	1.093	1.191**	1.200**
	(0.074)	(0.074)	(0.087)	(0.087)
month 3	1.225***	1.231***	1.250***	1.268***
	(0.091)	(0.091)	(0.1)	(0.102)
month 4	0.916	0.922	0.968	0.990
	(0.085)	(0.086)	(0.095)	(0.097)
month 5	1.010	1.019	0.88	0.908
	(0.102)	(0.103)	(0.099)	(0.102
month 6	1.027	1.038	1.005	1.048
	(0.117)	(0.118)	(0.121)	(0.127)
month 7	0.765*	0.774*	0.787	0.824
	(0.111)	(0.112)	(0.116)	(0.122)
month 8	0.839	0.851	0.761*	0.800
	(0.128)	(0.130)	(0.125)	(0.132)
month 9	0.622**	0.632**	0.663**	0.701*
	(0.118)	(0.120)	(0.127)	(0.134)
month 10	0.529***	0.539***	0.802	0.851
	(0.116)	(0.118)	(0.153)	(0.163)
month 11	0.457***	0.466***	0.540**	0.581**
	(0.117)	(0.119)	(0.135)	(0.145)
month 12	0.834	0.852	1.026	1.114
	(0.176)	(0.181)	(0.206)	(0.225)
months 13-24	0.414***	0.427***	0.500***	0.546***
	(0.057)	(0.059)	(0.07)	(0.077)
year dummies				
1975	0.933	0.901	0.813*	0.770**
	(0.104)	(0.101)	(0.097)	(0.092)
1976	1.079	1.044	0.941	0.855
	(0.176)	(0.172)	(0.163)	(0.150)
1977	1.208	1.168	0.948	0.841
	(0.22)	(0.216)	(0.188)	(0.168)
1978	1.180	1.126	0.924	0.824
	(0.215)	(0.208)	(0.182)	(0.164)
1979	1.065	0.986	0.691**	0.591***
	(0.166)	(0.161)	(0.117)	(0.104)

Table 3 continued

	MEN		WOMEN	
	without education	with education	without education	with education
1980	0.895	0.833	0.92	0.780
	(0.186)	(0.178)	(0.204)	(0.178)
1981	1.155	1.086	1.269	1.138
	(0.39)	(0.369)	(0.468)	(0.423)
region at the age of 16 dummies				
North	0.920	0.912	1.137	1.152
	(0.110)	(0.109)	(0.146)	(0.148)
North West	1.006	0.996	1.579***	1.569***
	(0.110)	(0.110)	(0.187)	(0.186)
E & W.Riding	1.127	1.114	1.407***	1.436***
	(0.138)	(0.136)	(0.183)	(0.187)
North Midlands	1.367**	1.340**	1.908***	1.883***
	(0.175)	(0.173)	(0.258)	(0.255)
Midlands	1.086	1.072	1.291**	1.278*
	(0.128)	(0.126)	(0.163)	(0.162)
East	1.284**	1.274**	1.788***	1.808***
	(0.158)	(0.157)	(0.247)	(0.250)
South East	1.312**	1.299**	1.701***	1.655***
	(0.146)	(0.145)	(0.211)	(0.205)
South	1.441***	1.433***	1.724***	1.718***
	(0.200)	(0.20)	(0.243)	(0.243)
South West	0.986	0.968	1.606***	1.599***
	(0.130)	(0.128)	(0.229)	(0.229)
Scotland	0.930	0.911	1.255*	1.188
	(0.107)	(0.105)	(0.157)	(0.150)
missing indicator				
info on region at the age of 16 missing	0.963	0.964	0.822**	0.812***
	(0.087)	(0.087)	(0.077)	(0.077)
SES missing	0.856	0.855	0.951	0.946
	(0.095)	(0.096)	(0.118)	(0.117)
no exact education history		1.125		1.261
		(0.173)		(0.198)
number of observations	2223	2223	2064	2064

Notes: The table displays hazard ratios and corresponding standard errors in parentheses. The first month in unemployment, living in Wales and the year 1974 are the base categories for the respective groups of variables. *** indicates significance at the 99% level, ** at the 95% level and * at the 90% level.

Table 4: Controlling for different sets of covariates

	(1)	(2)	(3)	(4)	(5)
men (N=2223)					
social skills	1.060**	1.053*	1.050*	1.061**	1.049*
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
cognitive skills	1.042	1.045	1.038	1.045	1.047
	(0.030)	(0.030)	(0.030)	(0.030)	(0.031)
cognitive skills	0.948**	0.947**	0.954**	0.947**	0.949**
× social skills	(0.021)	(0.022)	(0.022)	(0.022)	(0.022)
O-Levels/ NVQ 2	1.149**	1.106	1.119*	1.147**	1.093
	(0.072)	(0.071)	(0.071)	(0.072)	(0.071)
A-Levels/ NVQ 3	1.096	1.083	1.076	1.093	1.076
	(0.093)	(0.094)	(0.093)	(0.093)	(0.095)
degree/ NVQ 4-6	1.190	1.162	1.196	1.184	1.176
	(0.140)	(0.139)	(0.143)	(0.140)	(0.143)
women (N=2064)					
social skills	1.105***	1.106***	1.100***	1.106***	1.104***
	(0.036)	(0.036)	(0.037)	(0.036)	(0.037)
cognitive skills	1.078**	1.074**	1.071**	1.079**	1.074**
	(0.034)	(0.034)	(0.034)	(0.034)	(0.035)
cognitive skills	0.967	0.972	0.966	0.966	0.964
× social skills	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)
O-Levels/ NVQ 2	1.247***	1.205***	1.244***	1.246***	1.227***
	(0.080)	(0.079)	(0.081)	(0.080)	(0.082)
A-Levels/ NVQ 3	1.577***	1.514***	1.575***	1.571***	1.551***
	(0.167)	(0.165)	(0.169)	(0.167)	(0.171)
degree/ NVQ 4-6	1.498***	1.436***	1.508***	1.492***	1.457***
	(0.167)	(0.162)	(0.170)	(0.167)	(0.167)
included control variables					
family background		✓			✓
parenting			✓		✓
school characteristics				✓	✓
baseline covariates	✓	✓	✓	✓	✓

Notes: The table displays hazard ratios and corresponding standard errors in parentheses. A list of included control variables can be found in Section 2.8.2 in the appendix. Missings in the variable on SES were replaced by a zero. A dummy variable indicating this was included in the estimation. Another dummy variable indicating whether there were missings in the information on the qualification level was also included. Further control variables in the estimation are the region of residence, year dummies and the piecewise constant time specification. *** indicates significance at the 99% level, ** at the 95% level and * at the 90% level.

Table 5: Heterogeneity with respect to education

	MEN		WOMEN	
	without interaction	with interaction	without interaction	with interaction
social skills	1.060**	-	1.105***	-
	(0.028)		(0.036)	
if less than O-levels	-	1.121***	-	1.074
		(0.043)		(0.053)
if O-levels	-	0.990‡	-	1.104*
		(0.046)		(0.058)
if A-levels	-	1.039	-	1.251**
		(0.077)		(0.140)
if degree	-	1.098	-	1.148
		(0.114)		(0.137)
cognitive skills	1.042	-	1.078**	-
	(0.030)		(0.034)	
if less than O-levels	-	1.111***	-	1.126***
		(0.044)		(0.052)
if O-levels	-	1.020	-	1.075
		(0.053)		(0.056)
if A-levels	-	0.955†	-	1.050
		(0.074)		(0.109)
if degree	-	0.886†	-	0.930†
		(0.099)		(0.091)
education at the mean of skills				
O-levels	1.149***	1.089	1.247***	1.224***
	(0.072)	(0.069)	(0.080)	(0.081)
A-levels	1.096	1.080	1.577***	1.488***
	(0.093)	(0.097)	(0.167)	(0.192)
degree	1.190	1.238	1.498***	1.587***
	(0.140)	(0.165)	(0.167)	(0.210)
number of observations	2223	2223	2064	2064

Notes: The table displays hazard ratios and corresponding standard errors in parentheses. The effect of cognitive (social) skills is assessed at the mean of social (cognitive) skills, which is zero by construction. The same set of control variables as used in the main model presented in Table 2 is included. A dummy variable indicating whether there were missings in the information on the qualification level was included but never significantly different from zero. $***$ indicates significance at the 99% level, $**$ at the 95% level and $*$ at the 90% level. † indicates that the hazard ratios are different from the hazard ratios for the education level "less than O-levels" at the 90% significance level. ‡ denotes the corresponding significance at the 95% level.

Table 6: Means for different measures of social skills

	men	women
two scores for social skills		
overreact	-0.147	0.159
(standard deviation - overreact)	(1.079)	(0.881)
underreact	-0.094	0.102
(standard deviation - underreact)	(1.026)	(0.962)
categories of social skills (in per cent)		
stable at the age of 7	55.24	70.59
unsettled at the age of 7	28.34	19.82
maladjusted at the age of 7	16.42	9.59
number of observations	2223	2064

Table 7: Investigating the measure for social skills

	MEN	WOMEN
using 2 scores for social skills		
overreact	1.002 (0.025)	1.057* (0.035)
underreact	1.082*** (0.029)	1.071** (0.032)
cognitive skills	1.045 (0.030)	1.081** (0.034)
cognitive skills × overreact	0.949** (0.021)	0.971 (0.029)
cognitive skills × underreact	0.986 (0.023)	0.992 (0.026)
using categories of social skills		
unsettled	0.971 (0.056)	0.863** (0.058)
maladjusted	0.839** (0.067)	0.795** (0.083)
cognitive skills if stable	1.014 (0.038)	1.065* (0.040)
cognitive skills if unsettled	1.064 (0.048)	1.040 (0.058)
cognitive skills if maladjusted	1.191*** (0.072)	1.256*** (0.101)
number of observations	2223	2064

Notes: The table displays hazard ratios and corresponding standard errors in parentheses. The same set of control variables as used in the main model presented in Table 2 is included. *** indicates significance at the 99% level, ** at the 95% level and * at the 90% level.

Table 8: Pearson correlation coefficients between skills at the age of 7 and 11

	cognitive 7	social 7	cognitive 11	social 11
men				
cognitive skills 7	1.00	0.41	0.69	0.32
social skills 7 (BSAG)	0.41	1.00	0.38	0.34
women				
cognitive skills 7	1.00	0.37	0.69	0.31
social skills 7 (BSAG)	0.37	1.00	0.36	0.36

Table 9: Transition matrix of social skills at the age of 7 and 11

	stable 11	unsettled 11	maladjusted 11	total
men (N=1893)				
stable 7	67.48	22.80	9.72	56.52
unsettled 7	47.77	28.81	23.42	28.42
maladjusted 7	31.58	30.18	38.25	15.06
total	56.47	25.62	17.91	100
women (N=1788)				
stable 7	79.84	15.84	4.31	71.31
unsettled 7	57.14	29.71	13.14	19.57
maladjusted 7	42.94	31.90	25.15	9.12
total	72.04	20.02	7.94	100

Reading example for the table, upper panel: 56.52% of all men were in the category stable at the age of 7. Of these, 67.48% were also in the category stable at the age of 11, 22.8% were in the category unsettled at the age of 11 and 9.72% were in the category maladjusted at the age of 11. At the age of 11, 56.47% of men were in the category stable, 25.62% were in the category unsettled and 17.91% in the category maladjusted.

Table 10: Comparing results for skills at the age of 7 and 11

	skills at the age of 7	skills at the age of 11	skills at the age of 7	skills at the age of 11
men (N=1893)				
social skills	1.076*** (0.031)	1.054* (0.030)	-	-
cognitive skills	1.009 (0.031)	1.049 (0.034)	-	- -
cognitive skills × social skills	0.946** (0.023)	0.968 (0.025)	-	-
unsettled	-	-	0.952 (0.060)	0.889* (0.056)
maladjusted	-	-	0.817** (0.073)	0.906 (0.078)
cognitive skills if stable	-	-	0.974 (0.039)	1.001 (0.040)
cognitive skills if unsettled	-	-	1.029 (0.050)	1.096* (0.057)
cognitive skills if maladjusted	-	-	1.199*** (0.082)	1.179** (0.084)
women (N=1788)				
social skills	1.099*** (0.038)	1.042 (0.036)	-	-
cognitive skills	1.062* (0.035)	1.137*** (0.043)	-	-
cognitive skills × social skills	0.956 (0.028)	0.984 (0.032)	-	-
unsettled	-	-	0.902 (0.065)	0.879* (0.064)
maladjusted	-	-	0.789** (0.090)	0.835 (0.106)
cognitive skills if stable	-	-	1.046 (0.042)	1.091** (0.046)
cognitive skills if unsettled	-	-	1.003 (0.060)	1.319*** (0.089)
cognitive skills if maladjusted	-	-	1.307*** (0.115)	0.996 (0.108)

Notes: The table displays hazard ratios and corresponding standard errors in parentheses. Further control variables in the estimation are education, the father's SES at the age of 7 and 11, whether the individual is married at the beginning of the spell, has children, was employed before, the season of the year, the claimant count rate, the region of residence, year dummies and the piecewise constant time specification. $***$ indicates significance at the 99% level, $**$ at the 95% level and $*$ at the 90% level.

Table 11: Mean of variables by whether an individual was unemployed before the age of 23 or not

	NEVER UNEMPLOYED		UNEMPLOYED	
	mean	standard deviation	mean	standard deviation
female	0.52		0.48	
cognitive skills	0.07	0.97	-0.09	1.03
social skills	0.08	0.96	-0.11	1.05
stayed on at school after age 16	0.29		0.26	
socio-economic status of father at the age of 7				
low	0.20			0.25
medium	0.53			0.51
high	0.20			0.17
no male head of household	0.02			0.03
missing	0.04			0.05
region of residence at the age of 16				
Wales	0.05			0.07
North	0.06			0.10
North West	0.11			0.15
E & W.Riding	0.08			0.09
North Midlands	0.09			0.07
Midlands	0.10			0.10
East	0.10			0.07
South East	0.16			0.13
South	0.07			0.06
South West	0.07			0.06
Scotland	0.10			0.11
number of observations	5843			4287

Table 12: Probit estimations: Probability of experiencing unemployment of at least two weeks until the age of 23

	MEN		WOMEN	
Specification	(a)	(b)	(a)	(b)
cognitive skills	-0.020**	-0.009	-0.020**	-0.013
	(0.006)	(0.007)	(0.007)	(0.007)
social skills	-0.040***	-0.034***	-0.026***	-0.020***
	(0.006)	(0.006)	(0.007)	(0.007)
stayed on at school after age 16	0.014	0.034*	0.005	0.008
	(0.014)	(0.015)	(0.014)	(0.014)
socio-economic status of father at the age of 7				
high SES	-0.081***	-0.039	-0.024	-0.038
	(0.018)	(0.020)	(0.018)	(0.020)
medium SES	-0.056***	-0.031*	-0.021	-0.022
	(0.014)	(0.015)	(0.014)	(0.015)
number of observations	5,024	5,024	5,106	5,106
included control variables				
region at the age of 16	✓	✓	✓	✓
family background		✓		✓
parenting		✓		✓
school characteristics		✓		✓

Notes: The table displays average partial effects, calculated using the Stata command "margeff" written by Tamás Bartus. Standard errors obtained from the same command are reported in parentheses, they are derived using the delta method. Significance levels correspond to those of the coefficients. Having a father of low SES or not having a male head of household is the base category for father's SES. See Section 2.8.2 in the appendix for a detailed list of control variables. Missings in covariates were replaced by values, as explained in Section 2.8.1 in the appendix, and dummy variables indicating this were included in the estimation. These were partly significant in the estimation. *** indicates significance at the 99% level, ** at the 95% level and * at the 90% level.

Figure 1: Kernel density estimates of the distribution of skills at the age of 7

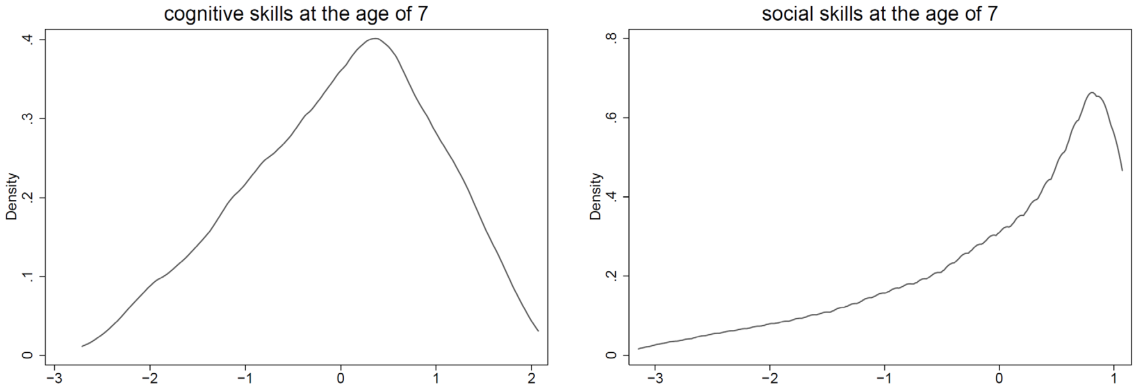


Figure 2: Kaplan-Meier estimates of survival functions for above and below average cognitive, respectively social, skills

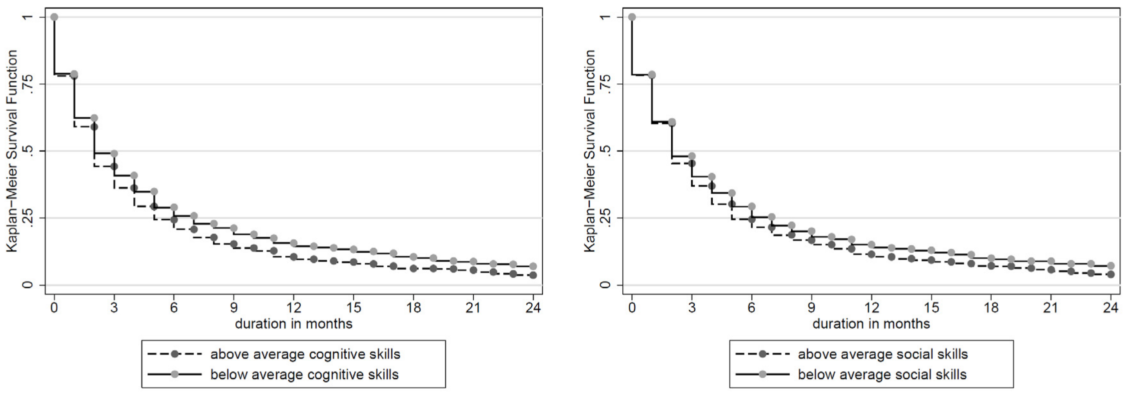
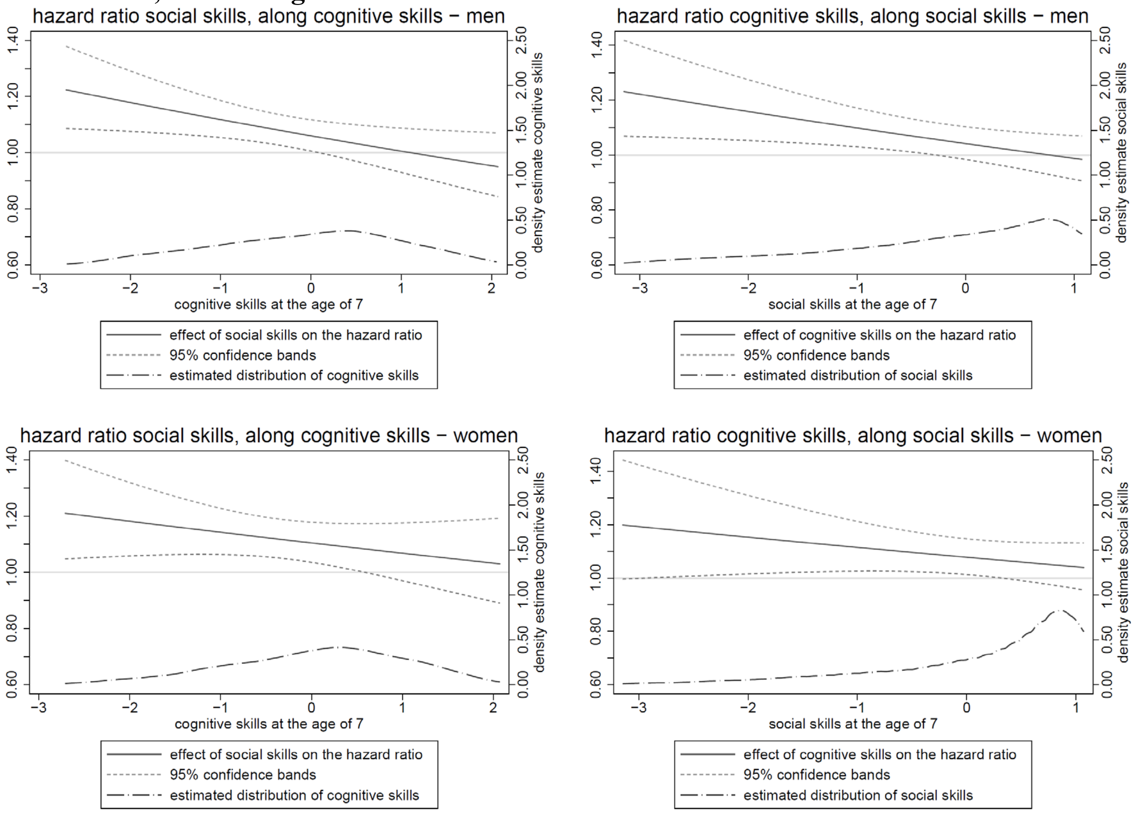


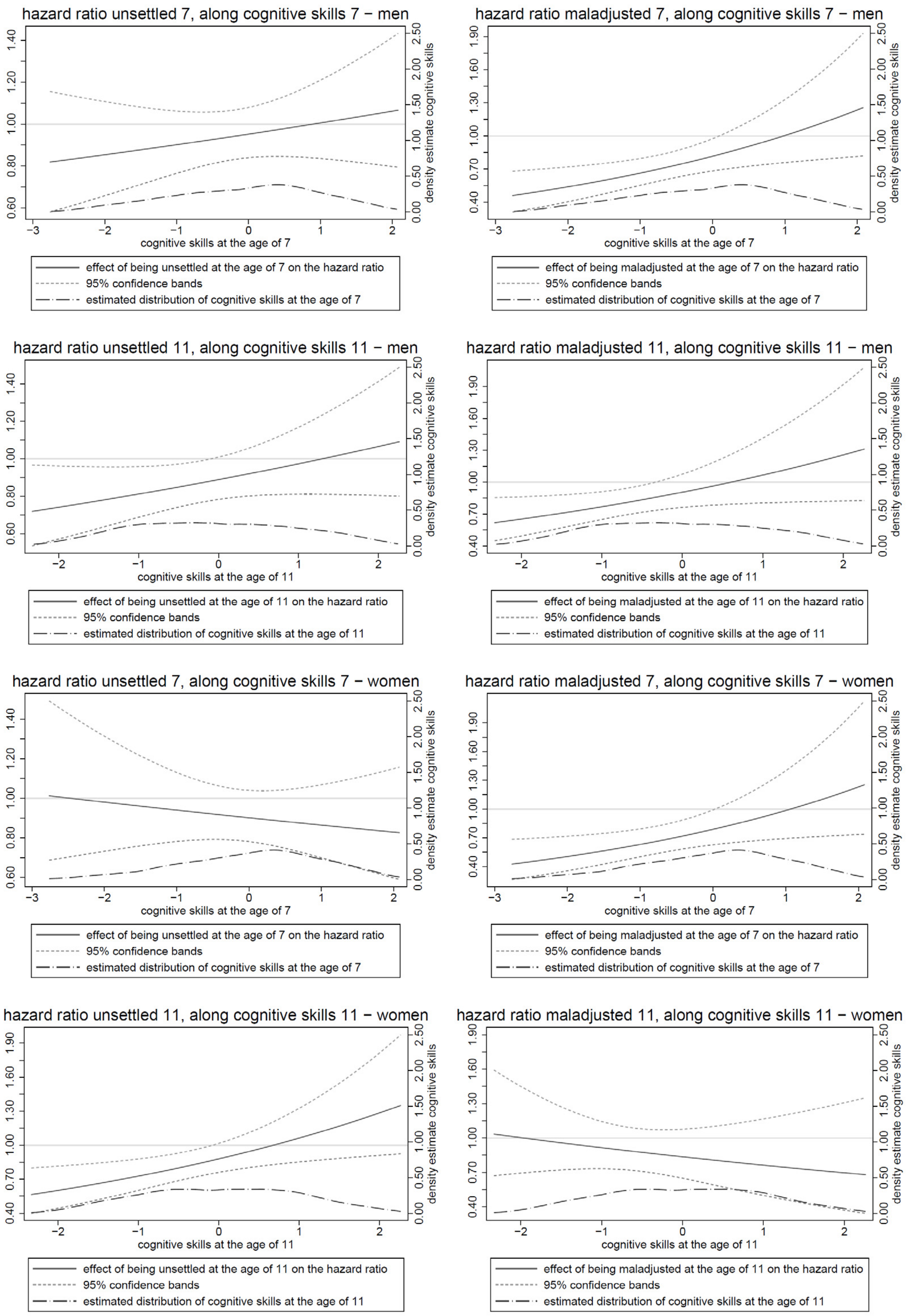
Figure 3: Effect of a one standard deviation increase in cognitive and social skills on the hazard ratio, controlling for education



Notes: The depicted hazard ratio for cognitive skills is calculated using the estimated coefficients in the following way (correspondingly for social skills):

$$hazard\ ratio_{cognitive} = \exp\{\beta_{cognitive} + social\ skill \cdot \beta_{cognitive \cdot social}\}$$

Figure 4: Effect of being unsettled or maladjusted as opposed to stable at the age of 7 and 11 on the hazard ratio, using the smaller sample



Notes: The depicted hazard ratio for being in the category maladjusted as opposed to stable is calculated using the estimated coefficients in the following way (correspondingly for being in category unsettled):

$$hazard\ ratio_{maladjusted} = \exp\{\beta_{maladjusted} + cognitive\ skill \cdot \beta_{\text{cognitive}\cdot maladjusted}\}$$

3 The Short- and Long-term Effects of School Choice on Student Outcomes - Evidence from a School Choice Reform in Sweden[45]

3.1 Introduction

Whether or not students should be allowed to choose their school of attendance is a highly controversial topic in many countries. Whereas some see school choice as a means to improve students' results, others fear that choice and competition will have adverse effects on the school system. Economic theory has no clear predictions on this matter: the aggregate expected effects of school choice and competition on students' outcomes are ambiguous. Empirical evaluations of existing school choice reforms are therefore important as they provide information on the actual effects of school choice policies.

In this paper we evaluate the effects on short-term and long-term student outcomes of a large-scale school choice reform in Sweden. The reform was implemented in 1992 and has significantly increased the amount of school choice in compulsory education. It affected the entire country and profoundly changed the workings of the Swedish school sector. Before the reform, students were assigned to the school in their catchment area. Now, 20 years after the reform, choosing school is a normal phenomenon, especially in more urban communities, and many municipalities encourage active school choice and provide information about the schools available. The reform essentially contained two elements: first, it allowed publicly funded but privately run schools[46] to set up and compete on basically equal terms with the publicly run schools; second, it encouraged choice among the already existing public schools. We believe that this reform, together with the detailed data that we have access to, provides a good opportunity for obtaining empirical evidence of the effects of school choice. Moreover, since the reform was

[45] This chaper is based on joint work with Karin Edmark and Markus Frölich.

[46] The Swedish term is friskolor, i .e. "independent schools", but we will refer to them as private schools throughout the paper.

introduced 20 years ago, we can now assess not only its short- but also the long-term effects.

The first part of the reform, the introduction of privately run, but publicly funded, schools, has been extensively studied (see Ahlin (2003), Sandström and Bergström (2005), Björklund, Edin, Fredriksson and Krueger (2004), Böhlmark and Lindahl (2007) and (2012), and Hensvik (2012)). The overall evidence of the previous studies suggests that competition and choice, in terms of a higher share of students in the municipality attending private schools, has had fairly modest effects on short term school results and basically no effect on long-term results.

Our study differs from those in several ways. First, while previous studies focussed on the effects of *private* schools, we study the overall effects of the choice reform, including in particular the choice among *public* schools. We examine the effects of school choice via permitting more private schools as well as via choice between the existing public schools. The latter could be particularly important since choice between public schools could be exerted immediately after the reform, whereas choice among private schools naturally requires that such new schools be founded, something that may take time and may not happen in all parts of the country. In fact, even in school year 2004/05, private schools existed only in 166 out of the 290 municipalities (National Board of Education, 2005, p.29). Also, a survey conducted by the National Board of Education revealed that, in school year 2000/01, choosing another public school than the nearest one was more common than choosing a private school (National Board of Education, 2003, pp. 48f.). Hence, private schools represent only one facet of the choice options, and the establishment of new private schools might in fact be an endogenous outcome of what is offered by the existing public schools.

Second, whereas the previous studies evaluate the effects of private schools measured by their share of students within the municipality, we use detailed geographical information on the locations of schools and student residences to construct measures of choice and competition that are specific for each student and each school. In particular, we calculate student-specific measures of the number of schools available, and school-specific measures for the competition they face from other schools. Our evaluation method then consists of comparing the outcomes of students with different degrees of school choice and competition, *before* and *after* the reform. The idea is that students with few schools nearby will in practice be unaffected by the introduction of the choice reforms (i.e. they will only have one school to choose from anyway), while for students with many schools nearby, the choice reforms will have a large impact on the actual choice opportunities.

Using identifying variation at the student-level, instead of at the level of the municipality, is potentially important since municipalities vary a lot in size, both in terms of population and area,[47] which means that variation only across municipalities may be too crude to capture the essential variation in choice and competition. Our approach also has the advantage of estimating the effects of choice *opportunities*, whereas the share of students in private schools only measures the degree to which students *exercised* choice to private schools. This is a possibly important distinction, as (potential) choice and competition could affect school quality, even if we only observe few people to actually change their school of attendance. A further advantage of having access to detailed geographical data is that we can construct different measures for the degree of competition facing each school, and the degree of choice facing each student. In supplementary analyses, we will thus distinguish between general effects caused by an increased competition among schools, and individual effects caused by students' possibility of choosing a school that best matches their preferences.

An important methodological issue that we need to deal with is the fact that the location of schools after the reform, in particular of the private schools, is likely to be endogenous with respect to student and community factors (such as student ability and background, or population density), or with respect to the performance of existing schools in the area (demand for private schools could for example be higher where public schools are bad). Moreover, if school choice and competition leads to improved school quality, it might also be that parents who are very concerned about education

[47] The largest municipality in terms of population, Stockholm, had 864,324 inhabitants in 2011, while the smallest, Bjurholm, had 2,431. The largest municipality in terms of area, Kiruna, is 20715 km^2 and the smallest, Sundbyberg, is 9 km^2. (Source: www.scb.se)

may move to regions with many schools.[48] If we knew which factors were important, we could control for them; yet, several factors may be unobserved.

Our empirical strategy is to use the *pre-reform* locations of schools and students' homes to measure choice and competition. That is, for students choosing a school in or after autumn 1992, we will measure choice as present right before the reform, in 1991. As we argue later, the school choice reform came largely as a surprise because of an unexpected federal election outcome. Hence, the location of schools and families was pre-determined to the reform. For students choosing a school under the old system, i.e. before 1992, we will therefore measure choice in the year they make their decision without risking endogeneity with respect to the reform. Using this strategy, we also permit that the establishment of new (private) schools or the closure of schools may be an endogenous outcome of the school choice reform. In our main specification we will thereby estimate the effects of school choice and competition as introduced by the 1992 reform. Our estimates will thus include all effects resulting from the dynamic processes that were triggered by choice and competition as it was present at the outset of the reform, like the opening or closing of schools and parents moving in response to the new options. In additional analyses we will also examine these processes.

Obviously, the location of schools even before 1992 was not random and also school choice was possible to some extent before by moving residence (i.e. Tiebout choice[49]). To deal with this, we control for many observable background characteristics at the individual and regional level and include municipality fixed effects. Moreover, as mentioned before, we also observe unaffected cohorts in our dataset which allows us to control for all time-constant relationships between having many schools nearby and student outcomes. Further, we make use of these unaffected cohorts to test whether pseudo treatment effects are indeed zero and control for pre-reform trends. The intuition for our identification strategy can thus be summarised as follows: The reform of 1992 came as a surprise to the population. Until then, parents had to move homes to exercise choice; afterwards, school choice was much easier. The number of schools available in

[48] Before the reform, we would see parents move as close as possible to a good school, which does not imply that there are many schools in the area.
[49] The term stems from the work of Tiebout (1956).

1992 is pre-determined for those cohorts entering school then. While differences in contextual factors between many-schools and few-schools areas have already existed before, we can control for many observed covariates and make use of the many pre-reform cohorts to additionally control for time-constant unobserved factors. Additionally, we can use unaffected cohorts to test whether pseudo-treatment effects are indeed zero.

We draw on very informative register data on the entire population of students and schools in Sweden, including a broad range of short term and long term student outcomes, ranging from educational results to labour market outcomes and socio-economic indicators. We can hence study the effects on a wide array of outcomes. The data cover a long period and hence enable us to evaluate the effects both immediately after the reform and many years later. This is important since effects on non-cognitive skills may not be fully reflected in school test scores but may become visible only later in labour market outcomes or criminal activity.

Our empirical results reveal that the effects of school choice as well as competition were very small during the period considered. This finding applies to the short-term effects on test scores and grades as well as to the longer-term effects on employment, higher education, criminal activity and health, where there is often no effect. The effects become larger for younger cohorts, i.e. those affected by the reform earlier in life, yet still remain very small. While the effects of choice and competition are hard to disentangle because of a high correlation, choice tends to have a *positive* effect, while competition tends to have a *negative* effect on marks, but almost only for students that were already in school as the reform was enacted. The latter could be due to the reform causing a disruption to the previously stable school system to which the schools eventually adjusted.

The magnitudes of all effects are very small, though. A potential explanation for this is that the previously existing Tiebout choice (i.e. moving homes) may already have delivered sufficient choice options for those families who wanted to choose. Moreover, according to economic theory, the school choice reform is expected to affect students' outcomes in various ways, and it is possible that the very small estimated effects reflect that negative and positive effects in practice cancel each other out.

3.2 The Swedish School System

3.2.1 General information on the Swedish school system

Sweden has nine years of compulsory schooling, starting the year the student turns seven. Throughout these grades, all students follow the same basic curriculum. After the compulsory schooling, the great majority of students continue with voluntary secondary school.[50]

Compulsory education is organised in three stages: grades 1 to 3, grades 4 to 6 and, finally, grades 7 to 9. Grades 1 to 6 are referred to as primary school, whereas secondary school starts with grade 7. Schools usually offer either only grades 1 to 6, or grades 7 to 9, while some offer all grades 1 to 9. Therefore, school choice is particularly relevant for entering school (i.e. grade 1) but also for grade 7, where many students graduate from elementary schools offering only grades 1 to 6.

Compulsory education is organised and provided by the municipalities, and the main source of finance of compulsory education is municipal tax revenues, followed by central government grants. Both the tax base and the grants are adjusted by equalisation formulas that are designed to give municipalities with different population structures roughly equal economic conditions.

3.2.2 The school system before the reform in 1992

The school choice reforms that are studied in this paper took place in the early 1990s. Before that, school choice in Sweden was very limited as students were placed in the school of their catchment area. Privately run schools existed, but they were few, and public funds were restricted to schools with alternative pedagogic profiles.[51] There also existed a few public schools with special profiles, such as music, which accepted students based on their skills in the relevant subject. In general, however, school choice was limited to Tiebout choice, i.e. to moving near the desired school.[52]

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[50] In 2011, 98 per cent of students entered secondary school. The share of students graduating from secondary school in at most 4 years was approximately 75 per cent in years 1999–2011 (see The National Board of Education: www.skolverket.se).

[51] In fact, until 1987, in order to receive public funding, schools were in addition required to prove that the use of these alternative pedagogical methods also benefited the development of the public schools, see The National Board of Education (2003).

[52] The allocation of students to schools was regulated in the compulsory school decree (Grundskoleförordningen 1988:655 Chapter 2 § 23), where it was stated that the allocation shall be based on what is appropriate in terms of

Politically, there were however heated debates on choice and competition in the public sector, including the education system. The right-wing opposition, especially the party Moderaterna, argued in favour of increased school choice and competition throughout the 1980s, but the Social democrats, who were in power for most of the decade, had a much more restrictive attitude. This reluctance started to soften during the late years of the 1980s, but, even then, the idea of the Social Democrats was first and foremost to increase choice and flexibility by increasing the local influence within schools, for example in terms of allowing schools to profile in terms of pedagogical style or special subjects.[53] It can be noted that school choice was tentatively discussed also by the Social Democrats in the late 80s and early 90s, but then mainly in terms of making it easier to choose schools with special profiles, should these become more common.[54] Apart from the few existing private schools and schools with special profiles, school choice only existed at the idea stage. This was however soon to change.

3.2.3 The 1990s school choice reforms

The regime shift in terms of school choice began in the fall of 1991, after a very tight parliamentary election brought a right wing coalition to power.[55] The newly elected government took a series of steps to increase choice and competition in the education sector:

In March 1992, the government proclaimed, in proposition 1991/92:95, that the aim was to "achieve the largest possible freedom for children and parents to choose school". It furthermore stated that "This freedom should apply both to choice between the existing public municipal schools, and to private schools."[56]

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transportation, efficient usage of facilities and other educational resources, and on parents' and students' wishes. While the regulation hence specified parents' and students' wishes as one (out of many) factor(s) to be considered, the general rule was that students were allocated to the nearest school.

[53] See for example Proposition 1988/89:4.

[54] See pp. 56–57 Proposition 1988/89:4.

[55] The right wing coalition (Moderaterna; Folkpartiet; Centerpartiet; and Kristdemokraterna) obtained 46.6% of votes, the socialist block (The Social Democrats and the Left Party (Vänsterpartiet)) 42.2%, and a populist party, New Democracy, which has since then disappeared from politics, obtained 6.7% and hence acquired a power balancing position. The greens, Miljöpartiet, received 3.7% of the votes and were hence only 0.3% away from parliamentary representation. In 1994 the Social Democrats came back to power, but by then the school choice reform was largely accepted, and no attempts were made to reverse it.

[56] See proposition 1991/92:95: "Målet är att åstadkomma största möjliga frihet för barn och föräldrar att välja skola. Denna frihet bör innebära möjlighet att välja mellan det offentliga skolväsendet och fristående skolor men också att välja skola inom det kommunala skolväsendet och att välja också en skola i annan kommun."

In June 1992, the parliament voted in favour of the proposition, and thus opened up for more choice between the existing public schools as well as for publicly funded but privately run compulsory schools to operate under basically the same conditions as the public schools. This new type of privately run school was to receive funding, through a voucher system, from the enrolled students' home municipalities, at a minimum of 85% of the average cost per student in the home municipality. [57] The schools were to be open to all students, and could only charge very limited additional student fees.

In 1994, another change in the school law, following proposition 1992/93:230, opened up for choosing a public school in another municipality than that of residence, something that was previously only allowed for independent schools, or in special cases such as bullying.[58]

In summary, propositions 1991/92:95 and 1992/93:230 established private schools as a publicly funded alternative, and made a strong statement that the central government viewed school choice as important. While the main law changes implemented in these reforms treated the opening up for independent schools, it is clear from the propositions that the aim was to increase overall school choice, both by facilitating for privately run schools to enter, and by encouraging choice between existing publicly run schools. Evidence by the National Board of Education suggests that school choice, both to private and public schools, has increased a lot during the 20 years that the reform has been in place, in particular in more urban areas.[59]

3.2.4 Other education-related reforms

The school choice reforms in the early 1990s were not the only education-related changes taking place in the 1990s, but they were part of a broad decentralisation and choice-enhancing trend in the organisation of the educational sector, as well as in the

[57] The reason for setting the minimum compensation level to less than 100% of the public schools' average cost reflected that the public schools were still ultimately responsible for granting all students in the municipality compulsory education. This, it was argued, could give rise to higher costs for example for administrative costs for ensuring that all students in the municipality attend school and costs from having to offer schooling to children from private schools that stop operating. In addition, public schools have to cater to all students, and cannot select students by, for example, offering only certain profiles (see prop 1991/92:95.) In1994, following the return of the Social Democrats to power, the minimum voucher level was lowered to 75% of the average cost.
[58] Following this proposition, the independent school reform was also expanded to secondary school level (grades 10-12).
[59] See Section 3.8.1.1 in the appendix.

public sector in general. The main changes consisted of making the municipalities, instead of the central government, responsible for the provision and organisation of compulsory education, and of replacing the system of ear-marked central government grants with a system of general central government grants.[60] Since these other reforms increased the municipalities' influence over compulsory education, it is reassuring that our analysis is, in contrast to most other studies of the Swedish choice reform, not conducted at the level of the municipality, which would risk to pick up effects of these other reforms.

3.3 Mechanisms of School Choice and Competition

In pre-reform Sweden, students were in general allocated to schools according to the proximity principle (i.e. to the nearest school), and the only way to change school was by moving. With the reform, choice could be exercised without moving. These enhanced choice options could affect student outcomes through various channels.

First, school choice can improve the matching of students and schools, e.g. regarding the desired pedagogical tools or any other aspect of the student-school match that improves the productivity of education. This should have an unambiguously positive effect on student and school results. In addition, students may increase their effort if they are allowed to attend the school of their liking.

Second, school choice may affect the allocation of students, which in turn gives rise to different peer effects.[61] Theoretically, it is not clear how school choice should affect the composition of students between schools: On the one hand, loosening the link between residential address and school of attendance could in principle decrease segregation[62] with respect to parental background (income, immigrant background etc.) as students are no longer required to attend the school nearest to their home. That is, students from poorer areas can gain access to schools in rich areas, even though they

[60] For a more detailed overview of these reforms, see Section 3.8.1.2 in the appendix.
[61] See for example Epple and Romano (1998) for a theoretical model on school choice where students sort according to ability and where peer effects are modelled. For empirical evidence on peer effects, see for example Zimmerman (2003), Sacerdote (2001), Lefgren (2004), Hanushek, Kain, Markman and Rivkin (2002), Angrist and Lang (2004), Ammermueller and Pischke (2009), Lavy and Schlosser (2007) and Hoxby (2000a).
[62] Segregation may refer to different aspects of student and parental characteristics. Here we deliberately use the term loosely, in the sense of "less mixing" with respect to any characteristic that may be of importance for peer effects and so to the productivity of education.

cannot afford to live there. On the other hand, however, school choice can also lead to more segregation, if parents/students increasingly choose to attend schools with similar peers[63].

It is also a priori unclear how being surrounded by more or less similar peers (with respect to academic ability, parental background etc.) may affect students' educational outcomes. On the one hand, more homogenous classes are easier to teach. On the other hand, weaker students may benefit disproportionally from stronger students, which are only available in more heterogeneous classes. The overall effects are ambiguous.

Third, school choice can put competitive pressure on schools to improve quality in order to attract students.[64] That more competition leads to higher quality, however, hinges on a couple of assumptions: i) that school quality is a determining factor for the choice of school; ii) that parents can observe school quality; iii) that schools have an incentive to attract students. The fact that funding for Swedish schools is, at least partly, based on the number of students,[65] suggests that there is an incentive for schools to at least attract enough students to fill the classes in order to cover the fixed costs for facilities and teachers. Having many applicants may in addition be desirable as it signals high reputation and status, and teachers and headmasters have a clear incentive to avoid a situation where the number of students is so low that the school is forced to shut down.

[63] In a Chapter 4, we show that we do not find evidence for a change in overall segregation at schools in terms of the socio-economic background of the parents, characterised by income, educational level and being born outside of Sweden, after the reform. Our measure of segregation, which is the yearly average of the standard deviation in the share of students with different characteristics across schools in Sweden, does however not take into account changes in residential sorting, i.e. it does not imply that school choice did not change sorting into schools on the local level.

[64] See Hoxby (2003) on school choice and school quality. See also Hanushek (1986) for an early overview of education production functions.

[65] There exists little information on the different resource allocation models used by the municipalities: the first country-wide survey, covering all municipalities, refers to the situation in 2007 (The National Board of Education (2009)). The survey suggests that the vast majority of municipalities base the resource allocation on the number of students (although part of the budget is not per-student-based, but based on, for example, special needs). Only 9 per cent of the municipalities responded that none of the budget was directly volume based, and that the allocation was instead made through an application-procedure (the Swedish term is: äskanden), and through dialogue with the school units. According to the authors of the report, it is however likely that volume was indirectly considered also in these municipalities, although not necessarily through an exact amount per student (p. 39). The survey furthermore suggests that the budget allocation procedures have often been in place for a long time: 52 per cent of the municipalities respond that they have used the same model for the last six budget years or more. 22 per cent respond that the current model has been used for 4–5 budget years, and the remaining 26 per cent respond that the current model has been used for less than four budget years.

The first assumption – that school choice is based on the quality of the school – is complicated by the fact that school quality can be difficult to observe. This means that, even though parents, all else equal, may want to choose the better school, they may in practise not be able to observe this. In the Swedish case, this is a relevant aspect since the only school level results that are publicly available are the final average grades, i.e. grades when students exit compulsory school in grade 9. In addition, if school choice is determined by student grades, schools have an incentive to inflate grades, which naturally devaluates their value as quality indicators.[66]

In addition, there are a number of factors – apart from school results – that potentially influence the choice of school, such as proximity, facilities, peers, extra-curricular activities etc.[67] These factors may or may not be correlated with students' learning. The competitive pressure on schools to attract students can hence in principle even give rise to negative effects on student outcomes by shifting focus from factors that improve teaching and learning to factors that are unrelated to students' learning, but potentially more easily observable, such as peer quality.

In sum, school choice can in theory give rise to various mechanisms, and it is hence a priori unclear which effects we should expect on students' outcomes. This makes an empirical evaluation of school choice reforms all the more important.

It is also worth mentioning that the Swedish school choice reforms are likely to give rise to a *process* of changing incentives. For example, even if competition between schools eventually gives rise to over-all higher quality, this is a process that is likely to take time, and that may in the meantime cause disruptions, as bad schools downsize and better performing schools expand. The effects of the school choice reform may hence take time, and may also look different over time. This is important to take into account in the empirical analysis.

[66] Vlachos (2010) suggests that the competition stemming from the introduction of independent schools has given rise to some, but very modest, grade inflation. His estimations suggest that a ten percentage point increase in the private school share would give rise to a 1–2 unit increase in the average student credit values (which is a measure of students GPA). This is a small effect, considering that student credit values are given at a 0–320 scale, with mean value at 206. We examine grade inflation in Section 3.8.1.5 in the appendix.

[67] For example, Burgess, Greaves, Vignoles and Wilson (2009) suggest that British families choosing school care both about the academic performance and the student composition.

3.4 Data

The study uses Swedish register data for the full population born in the years 1972 to 1990 and contains data from Statistics Sweden, the Swedish National Council for Crime Prevention, the Military Archives and the Swedish Defence Recruitment Agency.

First, as previously mentioned, we have access to detailed information on the geographical location of schools (for years 1988–2006) and students' residences (for 1985–2006), which enable us to construct student- and school-level indicators of choice and competition.[68] How these are constructed will be explained in the following section.

Second, our data contain information on a broad set of short-term and long-term student outcomes: First, we can observe the educational attainment at age 16 in the form of average final grades from compulsory school, i.e. by the end of grade 9 and, for the last 4 cohorts in our sample, the 9th grade test scores in English, Swedish and Maths. Since the latter are only available for a subsample of students, we will note make use of the information on the 9th grade test scores in the main analysis, but only in order to test for grade inflation in Section 3.8.1.5 in the appendix. In addition, for the male students, we have access to cognitive ability test scores from the military draft. These test scores, which are also used in for example Grönqvist, Öckert and Vlachos (2010) and Lindqvist and Vestman (2011), contain the overall scores from four subtests that measure the draftees' verbal, logical, spatial and technical ability, and are used to sort draftees to different assignments in the military service. The draft test scores are available for all cohorts, although for the later cohorts, the share of draftees drops significantly.[69] In terms of longer-term outcomes, we observe whether the individual was employed at the age of 25, as well as the highest educational degree the individual had completed at that age. We choose this age since it allows us to include many cohorts in the analysis – choosing a later age would have the benefits of capturing also older graduates, but would on the other hand decrease the number of cohorts for which we observe the

[68] Specifically, we have access to the midpoint coordinate of 100*100 m squares for student residence and school location, i.e. the coordinates measure the residential location with a maximum error of approximately 70 meters.

[69] Until the late 90s, virtually all 18–year-old males were required to take the test. After that, although the universal draft remains on paper, in practice only a minority of each cohort goes through the military service, see Figure 9 in the appendix. According to anecdotal evidence, the drafting decision can now in practice be influenced by the draftees, which leads to potential selection problems in this variable. We have analysed whether the selection is

outcome. We also observe whether individuals had health problems, indicated by receiving sickness benefits[70], at age 22, and whether the individual had ever been convicted for crime (including all crimes, from pilfering and petty traffic- and drug related crimes, to more serious types of crime, but excluding civil penalty) [71] at the same age.

An important task will be to control for all covariates that could potentially influence the outcomes, while also being correlated with the choice/competition variables. We therefore use a broad set of background covariates at the level of the student (including parental background information) as well as at the level of the local area (parish and/or municipality)[72]. The list of control variables is given in the note to Table 16 and further descriptive statistics are given in Table 15 [73].

Table 13 shows descriptive statistics of students' outcomes for affected and non-affected cohorts. Non-affected cohorts are those that have left 9th grade before autumn 1992, i.e. before the reform was implemented. These are all students born in the years 1972 to 1976. Summary statistics of variables measuring choice and competition will be given in Section 3.5.2.

Comparing the development of outcomes for the two different cohort groups, we see an increase in the share of individuals with a university degree at age 25 from 35% to 41% and a decrease in share of those employed at the same from 71% to 69%. It has to be taken into account, of course, that there are also still students who have not yet finished their studies at this age, which might thus reduce the share of employed individuals. The percentile rank in the grade point average at grade 9, which ranges from 0 to 100, has a mean of 48.21 for the non-treated and 49.40 for the treated cohorts

related to the choice reform (see Section 3.8.1.6 in appendix) but find only a very small association, which we do not believe to have important effects for our results.

[70] This variable is based on the sum of the yearly benefits received as sickness benefits and as benefits for early retirement due to bad health. We define an individual as having health problems if she/he received an amount exceeding the price base amount, which is an amount used in the social welfare legislation, and which varies with the aggregate price level. During the data period of our study, this amount was approximately €4,000.

[71] The Swedish term is "ordningsböter".

[72] The municipality level covariates were downloaded from the webpage of Statistics Sweden (www.scb.se), except for the indicator for urban municipality, which was constructed based on the 2005 year municipality classification by the Swedish Association of Local Authorities and regions (SKL). The parish level covariates were generated from individual level data generously made available from the Institute for Labour Market Policy Evaluation (IFAU). These data, as well as our individual-level covariates, come from the national registers held by Statistics Sweden.

and a standard deviation of 28.6 for both.[74] The cognitive score is a standardised measure that ranges between one and nine, with median value 5, and has a mean of around 5 and a standard deviation of about 1.9 in both cohort groups. The share of those having committed any criminal offense up until age 22 is 16 per cent for the untreated and 14 per cent for the treated cohorts. Note that this also includes small offences, like speeding or petty crimes, which explains why the share is not smaller. Since school choice may affect a student's peer group and the degree of segregation, which in turn could affect the social adjustment of students, we believe that it is important to also include these less serious types of offences.

3.5 Empirical Strategy

3.5.1 Identification

In order to estimate the effect of school choice as introduced by the 1992 reform, we need to address two main empirical challenges. The first is to separate the effect of having more school choice due to the reform from effects of other factors that are related to our choice measure, i.e. the number of schools close-by, also in the absence of a free school choice regime. The second is the potential endogeneity of schools' choice of location and parents' choice of residence after the reform. To deal with the first, i.e. to separate the effect of school choice from background factors that are correlated with living in an area with many schools, we include many regional- and individual-level covariates and municipality fixed effects in our estimation. Moreover, we control for the effect of our school choice measure on student outcomes in a situation without free school choice by including the unaffected cohorts in our dataset. Thus, we estimate the *additional* effect of having more schools nearby for cohorts that chose a school after the reform was implemented, compared to cohorts that chose a school before reform. We thereby control for time-invariant influences of unobserved factors that are correlated with both the choice measure and the outcome variable, conditional on many control variables. Our identifying assumption is that the effect of having many schools nearby

[73] All monetary variables have been deflated to year 2006 monetary value, using the consumer price index (source: Statistics Sweden, www.scb.se).
[74] The reason for the mean rank value not being exactly 50 is that ties in the data were given the same rank.

in the counterfactual situation, i.e. if the 1992-reform had never been enacted, can be estimated by the effect of having many schools nearby for cohorts that left education before the reform was enacted. This is similar to the assumption of common trends in a difference-in-differences design as we assume that the effect of having more schools nearby would have been the same as it was before the reform if there had been no reform.

This assumption is not directly testable, but we can make it more plausible by including a large set of control variables on the individual, municipal and parish level. Whatever is not controlled for is thus assumed to be constant over time. Importantly, we can assess the credibility of the assumption by performing *placebo tests* on the five pre-reform student cohorts. That is, we pretend the reform had happened two years earlier and estimate the effect of this "placebo"-reform. If our control variables successfully capture all correlation between our choice-index and other factors that affect student outcomes, and there is no additional time-varying influence of other factors, we expect the resulting placebo-effects to be zero. Furthermore, we can test for time trends in the effect of having more schools nearby in the pre-reform cohorts. Not finding any such trends can be seen as an indication that the results of our analysis are not due to time trends that are unrelated to the choice reform. Finally, even in cases where we do find evidence of time trends before the reform, we can use the five non-affected cohorts of students to estimate and control for such pre-reform time-trends when we estimate the choice-effect of the reform.

The second empirical challenge stems from the location of new schools, and the residential choice of parents, after the reform. Many new private schools opened up and their chosen locations are certainly not random. Some of them operate as for-profit schools and would base their location decision on expected profits. The many new private not-for-profit schools follow a social mission and would also not choose geographical location randomly. Ignoring such deliberate location choices in a regression analysis would lead to biased results, where the direction of the bias is uncertain. It could be positive if schools locate in areas where students perform well, e.g. in order to cream skim the best students, or to meet a demand for good schools in areas where parents and students are eager to learn and willing to invest time in actively choosing a school. On the other hand, the bias could be negative if schools locate in areas where the educational quality was previously low.

To deal with these problems, our main empirical strategy is to base our measures of choice and competition on the *pre-reform* location of schools and students. Since the school reform should have come as a surprise to the population, due to the tight race in the 1991 national election, we can consider them as pre-determined and thus not endogenously affected by the reform itself. Hence, for cohorts that chose a school after 1991, we will approximate the amount of choice they faced by measuring the amount of schools they had nearby in 1991, just before the reform. For cohorts that chose school before the reform, we use students' actual location of residence in the year they enter 7th grade, or, depending on data restrictions, the information that is closest to that year.[75]

By measuring choice and competition via the pre-reform location of schools and students, we will measure the overall effect of the reform that goes through having more schools nearby at the beginning of the process. This effect will comprise all dynamic processes happening after the reform, such as new schools opening up or schools closing down. In later sections we will also examine how the school choice reform affected the number of public and private schools, that is how our *pre-reform* measures of school choice are related to school choice measured after the reform. We believe that our approach captures the policy-relevant parameter, particularly for a school reform that encourages and supports *non-public* schools, such that the exact placement of these schools is more market-driven and less centrally determined. (In many countries, Tiebout choice with only few private schools is still the status quo.)

Our estimates of the school choice effects are to be interpreted relative to the Tiebout choice that already existed before the reform: Families had always been able to choose schools via changing their place of residence and moving into the catchment area of their preferred school. We imagine that Tiebout-type migration was more frequent in areas with many schools, where merely a short move was sufficient in order to switch

[75] We have information on individuals' residential coordinates starting from year 1985. However, when for example constructing a measure for choice on the grade level 1-3 for cohort 1972, we would need to know their coordinates in the year 1979. In cases like this, we use their coordinates in 1985 instead. For schools, we only have information on coordinates starting from 1988. Therefore, when merging the competition measure to individuals who started a certain grade level before 1988, we merge the school competition measure from 1988 instead.

catchment area. In addition to Tiebout choice, also another potential mechanism existed before the reform through which the number of schools might have affected student outcomes: Having had many schools nearby may have given parents the possibility to compare different schools and thus increase their ability to judge the quality of the school their children go to. This would have enabled them to complain and put pressure on the local education authorities to increase quality. Our estimates will thus reflect the *additional* effect due to being able to choose without moving homes.

One can imagine that the new choice possibilities after the reform may reduce Tiebout-type choice as the reform weakened the link between location of residence and school of attendance. While we cannot thoroughly test that hypothesis, a descriptive analysis in Section 3.8.1.3 in the appendix, however, shows no evidence for it.[76]

3.5.2 Measuring the degree of choice among schools

The degree to which students can exercise school choice crucially depends on the availability of alternative schools in the vicinity of students' homes. Thus, we measure the degree of school *choice* by exploring the distance between a student's home and the schools a student could potentially choose from.[77] Specifically, we count the number of schools within a given radius around a student's home in order to measure her choice possibilities.[78] As Sweden is a geographically diverse country with very rural but also urban areas, our preferred radius is the median commuting distance within each municipality in 1992.[79] This radius takes different local settings into account in a very flexible way and, in our opinion, can be used to approximate the area within which parents might consider different schools for their children. The average median commuting distance across all municipalities is about 5km. In addition to this flexible

[76] It can be added, however, that our graphs only show descriptive statistics starting from 1991. They do hence not rule out that Tiebout choice existed before then. If the degree of Tiebout choice, for some reason, was changing during the years before that, i.e. in the pre-reform period, then this could give rise to pre-reform trends in the outcomes and cause our placebo-test to fail.
[77] See Section 3.2.3 for details on which schools a child could in principle attend.
[78] See also Gibbons, Machin and Silva. (2008), Himmler (2009) and Noailly, Vujic and Aouragh (2009) for other studies using the distance between a student's home and schools.
[79] We are grateful to John Östh for providing information on municipality commuting distances. The distances are measured "as the crow flies", and do not take into account the directions of roads etcetera.

radius, we will also estimate the effects using a 2km radius as a test of the robustness of the results.[80]

Another issue refers to the point in time in a child's schooling career when one should measure the degree of available school choice. In the Swedish compulsory schooling system, it is common not only to choose a school when starting first grade, but to also potentially change school at the beginning of 7th, and sometimes also 4th, grade. For this reason, there are three points in time in the schooling career at which the degree of school choice might potentially be important. We found however, that these measures are very highly correlated, i.e. having more schools in the neighbourhood that offer grades 1-3 is highly correlated with also having more schools that offer grades 7-9. Because of the high correlations we were unable to include these different measures in the same regression. Hence, we will only include choice measured at one grade level at a time in the estimations, and following the previous Swedish studies, which all analyse choice and competition in grade 9, we focus on choice opportunities when choosing a school that offers grades 7-9. Note also that this is a point in a child's educational career at which parents might pay special attention to choosing a school, as the marks at the end of 9th grade are important for admission into high school. In our main specification, we thus measure among how many schools offering grades 7-9 a child may choose from at the age of 13, which is when children enter 7th grade, or, as explained in the last section, in 1991, if the child started seventh grade after the reform.[81]

Table 14 shows descriptive statistics for our choice measures separately for affected and non-affected cohorts. The average number and the standard deviation of the distribution of schools offering grades 7-9 within median commuting distance around a student's home are 3.45 and 4.66 for the non-affected cohorts[82]. With a mean of 5.91, students born after 1976 have on average more schools within their median commuting distance, measured at their place of residence in 1991 and taking into account schools existing in 1991. The reason for this increase is that our choice-measures are computed

[80] We also explored several different other radii and obtained similar empirical results.

[81] We only have geographical information on schools starting from year 1988. Students born in the years 1972-1974, who should be matched to schools' location in the years 1985-87, will be matched to schools' location in the year 1988 instead.

taking into account the 1994 law change (see Section 3.2.3) that enabled students to attend public schools also in other municipalities, something which was previously restricted to special cases or private schools. For the smaller radius of 2 km, this change has less impact, and the average number of schools within 2km around a student's home only increases from 1.24 to 1.35 schools for affected versus non-affected cohorts.

Another fact to note is that the median number of schools within 2 km from students' homes is only one, meaning that for at least 50% of the sample, this measure implies no choice close to home. When using the radius that is endogenous to local circumstances, namely the median commuting distance, the median number of schools is two, thus already capturing some choice also for those in the lower part of the distribution. The measures will thus compare different groups of people and will have a different power in measuring choice in different regions.

3.5.3 Estimation

In a first step, we estimate the effect of choice on student outcomes separately for each cohort and graphically inspect whether we can see a pattern in how the effect evolves over time. This approach has the advantage of being very flexible in identifying how the effect changes over time but comes at the price of not using between-cohort variation to control for time-constant effects, which might help with the identification. In all estimations we use least squares for continuous outcome variables and probit estimation for binary outcome variables and report marginal effects in all tables. We allow for clustering of the error term on the school level[83] as it is likely that error terms of students at the same school will not be independent.

Our first analysis is used mainly to obtain a graphical representation of the correlation between choice and outcomes over time, shown in Figure 6. The following regression (10) is estimated *separately* for each cohort born in {1972,...,1990}:

$$(10) \qquad Y_i = \beta \cdot c_i + \delta \cdot X_i + \lambda_{municipality} + u_i \quad \text{estimated separately for each cohort}$$

[82] The average median commuting distance over all municipalities is 5.8km, with a standard deviation of 4.2km, minimum of almost 1km and a maximum of 26km.

[83] Since we cannot link schools over time in our dataset, the clustering will not be on the school level over time but just within cohorts.

where c_i is the choice measure, X_i is a vector of control variables, $\lambda_{municipality}$ are municipality fixed effects, and u_i is an error term. The list of control variables X is given below Table 16. Descriptive statistics on these variables are given in Table 15.

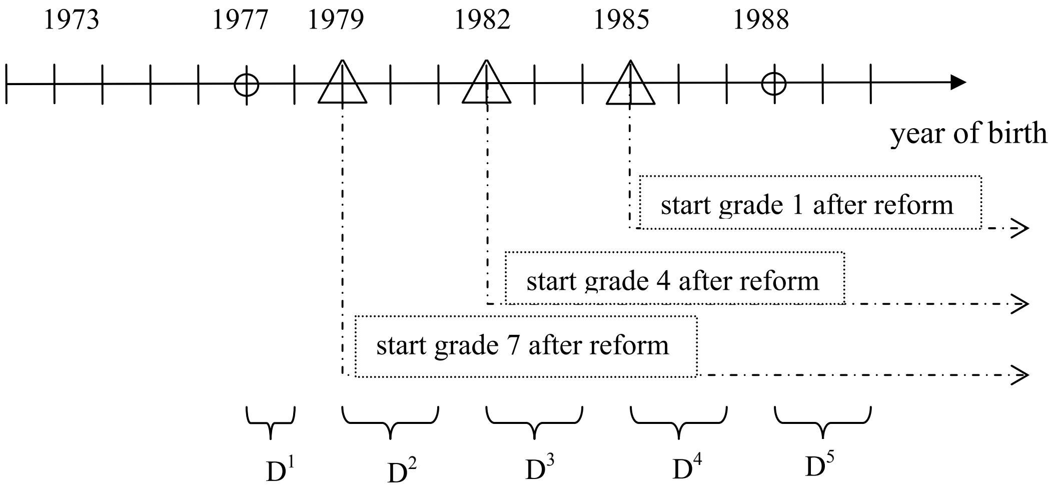
In our main analysis we instead pool the observations from *all cohorts* and estimate the differential effect of choice before and after the reform. In principle, we could permit the effect of choice to vary from year to year, i.e. one cohort happened to be in grade five when the reform was enacted, the next cohort was in grade six etc. For statistical precision and also because choice is usually exercised only at grades 1, 4 or 7 and only very rarely at grades 2, 3, 5, 6, 8 or 9, in our main specification we will however define treatment windows of three years length instead. Therefore, we define the five dummy variables:

$$(11) \qquad \begin{pmatrix} D_i^1 = 1\ if\ born\ in\ 1977\ or\ 1978;\ zero\ otherwise \\ D_i^2 = 1\ if\ born\ in\ 1979\ or\ 1980\ or\ 1981;\ zero\ otherwise \\ D_i^3 = 1\ if\ born\ in\ 1982\ or\ 1983\ or\ 1984;\ zero\ otherwise \\ D_i^4 = 1\ if\ born\ in\ 1985\ or\ 1986\ or\ 1987;\ zero\ otherwise \\ D_i^5 = 1\ if\ born\ in\ 1988\ or\ 1989\ or\ 1990;\ zero\ otherwise \end{pmatrix}$$

and note that all these treatment dummies are zero for the pre-reform cohorts.

The choice of these windows is motivated by considering which cohorts are affected by school choice and competition at which stage in their educational career. Figure 5 displays this, together with the different treatment groups D^1 to D^5 that we define. One can see that the first cohort to be possibly affected by competition at grade level 7–9 is the cohort born in 1977. They went to grade nine in the school year 1992/1993 and could therefore potentially have been affected from an increased competitive pressure. However, they are unlikely to change school one year before graduation and are therefore unlikely to benefit from choice. The first cohort of students to be really affected by choice is born in 1979, as they started grade 7 in fall 1992. Starting with this birth cohort, we could imagine measurable effects of choice on academic outcomes. We nevertheless place cohorts 1977 and 1978 into treatment group D^1 since they are not a clean control group: Even though these two cohorts could not choose the school at which they started grade 7-9, they were still in grades 8-9 as the reform was enacted and could thus potentially have been affected by increased competitive pressure. Only students born *before 1977* were not affected at all by the reform.

Figure 5: Treated cohorts



Using these treatment windows, we estimate

$$Y_i = \beta_1 D_i^1 c_i + \beta_2 D_i^2 c_i + \beta_3 D_i^3 c_i + \beta_4 D_i^4 c_i + \beta_5 D_i^5 c_i + \alpha \cdot c_i + \gamma_{cohort} + \lambda_{municipality} + \delta \cdot X_i + u_i \quad (12)$$

where γ_{cohort} and $\lambda_{municipality}$ are cohort and municipality fixed effects. We use OLS for continuous outcomes and Probit for binary outcomes, and cluster standard errors at the school level as before.

The coefficient α measures the relationship between (Tiebout) school choice and outcomes for the pre-reform cohorts (we do not assume a causal interpretation for α), whereas the β coefficients measure the differential effects of free choice after the reform, i.e. without the need to move residence. Including c_i in the regression nets out all effects our choice-measure might have had also on non-affected cohorts.

In addition to allowing the effect of choice to differ for groups of cohorts after the reform, compared to a constant effect before the reform, as we do in Equation (12), we also run a specification that includes a parametric time trend in the effect of choice on student outcomes. The time trend is defined as t_i = year of birth – 1972. As shown in Equation (13), where the coefficients α^t and β^t refer to the time trends, we allow the effect of choice to exhibit a linear time trend both before the reform (captured by the term $\alpha^t \cdot c_i \cdot t_i$) and, with a different slope in each treatment window group, after the reform. With this specification, we can test whether the effect of having many schools nearby already changed over time before the reform and can control for such a pre-reform trend. In this case, α^t would be significantly different from zero.

$$
\begin{aligned}
Y_i = {} & \beta_1 D_i^1 c_i + \beta_2 D_i^2 c_i + \beta_3 D_i^3 c_i + \beta_4 D_i^4 c_i + \beta_5 D_i^5 c_i \\
& + \beta_1^t D_i^1 c_i t_i + \beta_2^t D_i^2 c_i t_i + \beta_3^t D_i^3 c_i t_i + \beta_4^t D_i^4 c_i t_i + \beta_5^t D_i^5 c_i t_i \\
& + \alpha \cdot c_i + \alpha^t \cdot c_i t_i + \gamma_{cohort} + \lambda_{municipality} + \delta \cdot X_i + u_i
\end{aligned} \tag{13}
$$

where t_i = year of birth – 1972 and coefficients α^t and β^t refer to the time trends.

As discussed in Section 3.5.1, we will use students' residential location and schools' location from right before the reform, that is from year 1991, if the student started grade 7 after the reform. This will exclude endogenous relocation with respect to the choice-reform from the estimation. For the same reason, we will also measure all municipal- and parish-level covariates in 1991 if the choice for grade 7 was taken after 1991. For students who started grade 7 before the reform, we measure all variables at the time when they started grade 7 or, if we do not have data from that year, the most current information.

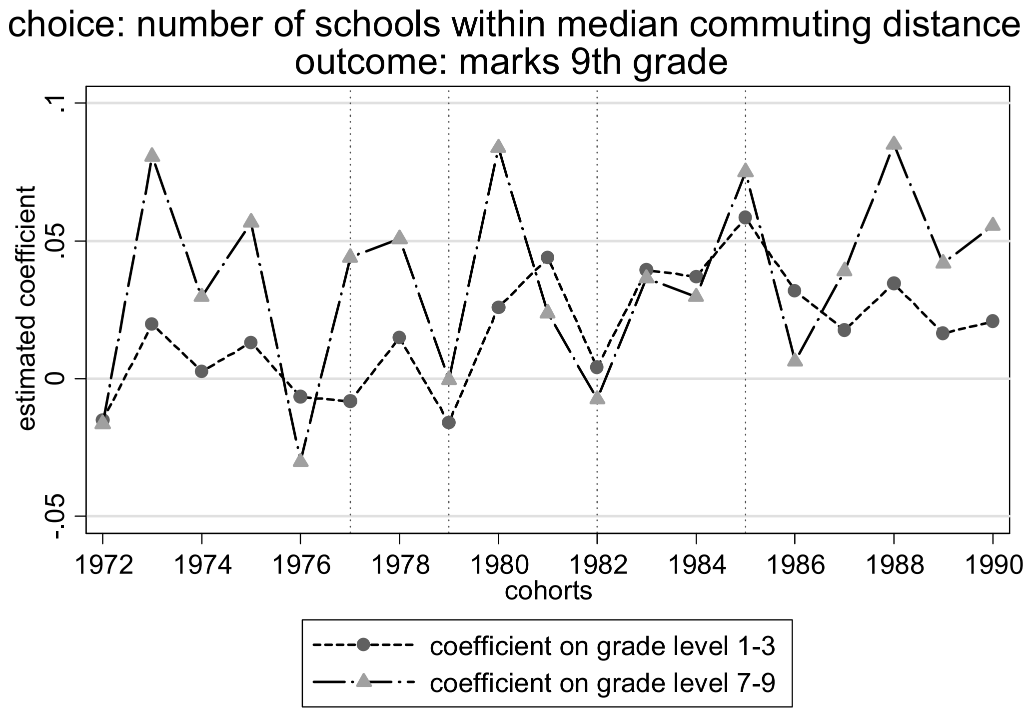
3.6 Results

3.6.1 Main specifications

We start by analysing the effect of having more schools to choose from close to home by regressing different student outcomes on the number of schools within the median commuting distance of the home municipality for each cohort separately, in accordance with Equation (10).

Figure 6 displays the estimation coefficient for the outcome percentile rank in GPA 9, using the choice measure based on the number of schools offering grade 1–3 and 7–9, respectively. For the cohorts that were completely unaffected by the reform, that is those who left primary education before autumn 1992, having more schools close to their home has no significant effect on the percentile rank of their grade point average in grade 9 (cohorts 1972-1976). This supports the hypothesis that having more schools nearby without being able to choose from them should not have any effect on student outcomes. For cohorts born after 1977, one can see a slight positive trend in the effect of choice, but this effect is statistically significant only for the youngest cohorts. Economically, the effect is very small, with an increase of 0.05-0.08 (0.02 to 0.05) percentile ranks in the GPA 9 for every additional school that offers classes on the level 7-9 (1-3) within the median commuting distance of the municipality.

Figure 6: Estimation coefficients of percentile rank in marks in grade 9 on choice



In order to quantify the difference between the importance of having more schools nearby for the GPA in grade 9 before and after the reform, and to test whether there is a statistically significant difference, Table 16 shows regression results from estimating Equation (12) for grade level 7-9. These estimates denote the overall effects of the choice reform that work through having more schools nearby at the place of residence right before the reform.

Column 1 shows results from a specification where a constant treatment effect of the reform is assumed, that is we compare the average effect of having more schools nearby for cohorts that left primary education before and after 1992. To that end, we interact the number of schools within the median commuting distance of the municipality, denoted "Choice" in the table, with a treatment window indicator that captures all cohorts that were potentially affected by the reform, i.e. all individuals born in or after 1977. The resulting point estimate shows that having one more school within the median commuting distance increases the percentile rank in GPA of cohorts that are affected by the reform by 0.06. Taking into account the observed variation in the sample[84], a one standard deviation increase in the choice measure, that is 9.35 more schools, leads to an increase in the percentile rank by about 0.56. The effect is thus very small.

However, as the reform was enacted only gradually over time, allowing for a time-varying effect is potentially important. The second column of Table 16 therefore shows results from estimating Equation (12), with treatment windows as specified in Equation system (11). The estimated effect of choice is not significantly different from zero for students born between and in the years 1977 and 1984, but is positive and significant for cohorts 1985–1990, which started first grade after the choice reform was enacted. Moreover, with an increase in the percentile rank of 0.13 for each additional school within the commuting distance, the effect is largest for the youngest cohorts. It is, however, not increasing in a linear way, which is why we prefer modelling the effect in the piecewise constant fashion rather than with a time trend. In terms of a standard deviation increase in the number of schools within the median commuting distance, the percentile rank in GPA increases on average by 1.2 for cohorts born between 1988 and 1990, and by 0.7 for cohorts born between 1985 and 1987. Thus, the effect of having more schools nearby on the marks in 9th grade is modest also for these later cohorts.

We now turn to analysing the effects of school choice on later outcomes in order to see whether the small effects on marks at the end of 9th grade fade out over time or transform into long-lasting effects on students' adult outcomes. Table 17[85] shows the corresponding results, again using the treatment window specification displayed in

[84] We use the standard deviation for the post-reform cohorts here.

Equation (12). The table shows coefficients and clustered standard errors for the cognitive score, and marginal effects at the mean and corresponding standard errors for all other outcomes[86].

The point estimates for the effect of having more choice among schools on the cognitive score are very small for all cohorts and none of them is significantly different from zero. As mentioned before, although the draft is still mandatory, in practice it has become more voluntary over time, and among the younger cohorts, there is an increasing share of men who did not take the test, which raises issues of selection problems for this variable. Section 3.8.1.6 in the appendix shows that the selection into taking the test is slightly related to our choice measure, but the correlation is very small.

Column three shows marginal effects at the mean for the probability of having a university degree at age 25. For the youngest cohorts, which is the only one for which we find a marginal effect that is significantly different from zero, we estimate an increase of 0.14 percentage points in the probability of having a university degree for each extra school within the commuting distance. This is again a very small effect. However, it shall be noted that the placebo-test for this outcome fails; students born in years 1975-1976 had a disadvantage from having more schools nearby, compared to those born between 1972-1974[87]. This cannot be attributed to the reform as the reform had not been enacted yet while cohorts 1975-1976 where in compulsory education. Since this violates our identifying assumption, the marginal effects for this outcome cannot easily be interpreted, even though the negative placebo effect suggests that the found effect may be a lower bound. Also for the outcome "being employed at age 25", the estimates indicate a placebo effect, although again very small. With a marginal effect of 0.12, it is positive and in the range of the marginal effects we find for the youngest cohorts; that is an increase in the probability of being employed by 0.07

[85] Column one repeats the results for the percentile rank in order to ease comparability. The military test score in column two is a continuous outcome that varies between one and nine. All other outcomes are binary and denote the probability of a certain outcome being true.

[86] See Table 30-Table 32 for coefficient estimates of the Probit models, and coefficients and marginal effects for the placebo-specifications and specifications allowing for a pre-reform trend.

[87] The estimated marginal effect for cohorts 1975-1976 is -0.12 percentage points for each additional school, so again, a very small effect. See Table 31 for detailed results.

percentage points. Even though the placebo test fails, we see that both estimates are very small, which indicates that there is no notable effect on employment at age 25.

We find nearly zero effects for the probability of ever having been convicted for a criminal offence (largest estimate is -0.07 percentage points per extra school, see column five) or having serious health problems at age 22 (only significant above the 95 per cent level for the youngest cohort, with a marginal effect of 0.026 percentage points per extra school, see column six).

Summing up, our main results, using the number of schools within the median commuting distance of the home municipality in 1991, show that more choice leads to marginally higher grades at the end of 9th grade but does not seem to have affected our long-term outcomes in an economically significant way (keeping in mind the identification problems for some of the long-term outcomes).[88] Although we cannot exclude possible grade inflation, additional empirical analysis suggests that it cannot be the main explanation.[89] Hence, choice seems to have (very) small effects on grades, but these fade out as the children grow older.

3.6.2 Alternative measure of choice opportunities

Which radius to take into account, when assessing choice opportunities for students is, as previously discussed, not a priori obvious. This section presents how the magnitude of the results differs with respect to the chosen radius.

Using the median commuting distance of a municipality as the relevant choice area has the advantage of automatically adjusting the radius to the local situation, but it is less transparent and harder to understand than a fixed radius. As an alternative measure, we use a radius of 2 km which is easier to relate to and always within close reach of students' home. The disadvantage of this constant radius is that it does not even comprise the nearest school for many children who live in rather rural areas and may

[88] Additional empirical analysis suggests that the positive and small effects of the choice index on the short term outcomes are limited to more urban regions, and to municipalities that have been more active in promoting school choice. See Section 3.8.1.4 in the appendix for details.

[89] Available data on 9th grade standardised test scores in English, Swedish and Maths, taken in years 2005–2008, have enabled us to partially test for different degrees of grade inflation between high and low choice areas. The results reported in Section 3.8.1.5 in the appendix suggest that there is some grade inflation related to having more schools to choose from, but that its effect on our estimates is probably small.

therefore give a too crude picture of the degree of choice in these areas.[90] At the same time, the 2 km radius might still be too large to distinguish between choice and no-choice areas in larger cities. Nevertheless, it illustrates how the size of the estimated effect varies with the chosen measure[91].

Table 18 presents the results from this exercise, showing only the preferred specification that models the treatment effect as a piecewise constant function of cohorts of Equation (12)[92]. Starting with the effect on the percentile rank in GPA 9, the point estimate on having one more school to choose from within 2 km is now negative but insignificant, or marginally insignificant, for all cohorts that were already in compulsory school when the reform was enacted. For the youngest cohorts, i.e. those born between 1988 and 1990, having one more school within a 2 km radius of their place of residence in 1991 significantly increases the percentile rank by 0.298. This effect is larger than the increase caused by an additional school in the median commuting distance. However, taking the observed variation into account, an increase in this choice measure by one standard deviation, that is 1.69 schools, amounts to a 0.5 percentile rank improvement in the GPA, i.e. rather close to what we find using the median commuting distance as radius.

Turning to the effect on cognitive skills as measured by the military draft test score, we can see that one more school within 2 km distance raises the cognitive score by about 0.01 to 0.02 points for the younger cohorts. With a standard deviation in the outcome of 1.9, this amounts to a very small effect of 0.5-1% of a standard deviation. So again, even though we now find a significant effect, economically, the effect is small. The pattern is similar for the other outcomes: the point estimates are larger when using this measure, but economically, they are still small.

As in Table 17, we find that for the outcomes “university degree at 25” and “employed at 25” there are some unresolved identification problems, as indicated by the

[90] In terms of the exogenous pre-reform measures, 68% of the individuals in the sample have no or only one school offering grades 7-9 within 2 kilometres around their home. This share reduces to 45% when using the median commuting distance of the municipality instead.

[91] Results using a radius of 3km, 4km or 5km lie within the region spanned by results using 2 kilometres and the commuting distance (which is about 5km on average).

[92] See Table 33-Table 35 for coefficient estimates of the Probit models, and coefficients and marginal effects for the placebo-specifications and specifications allowing for a pre-reform trend.

significant placebo tests[93], which is why the results for these variables shall be interpreted with caution. Nevertheless, estimated marginal effects are below 0.6 percentage points for each additional school within 2km for both outcomes, and thus, again, economically very small. We do not find any significant effects on the probability of having been convicted for a criminal offence until age 22 or having health problems at that age.

Summing up, we find that, qualitatively, the results are robust to using a different radius to approximate the area in which parents may consider choosing schools.

3.6.3 Disentangling the effects of choice and competition

As discussed in Section 3.3, the school choice reform might have affected student outcomes through various channels. On the one hand, students have more choice, and on the other hand, schools may compete to attract students. In this section, we try to distinguish these two mechanisms – competition and choice – by adding a competition regressor to the estimations.

In order to measure possible effects of *competition*, we calculate, for each school, the competitive pressure it experiences by taking into account the number of nearby schools as well as the size of their student bodies. The exact formula for the competition measure $comp$ of school j is:

$$comp_j = \sum_{k=1}^{K} \frac{1}{dist_{k,j}} \cdot size_k \quad , \tag{14}$$

where $k = 1,...,K$ indexes potential competitors within a radius of 100 km, $dist_{k,j}$ is the distance between school j and school k and $size_k$ is the number of students visiting school k. We will also use alternative measures of competition where we simply count the number of schools within a certain distance around the school. As our analysis is on the individual and not on the school level, we assign each student the competition measure of the school she attends in 9th grade, or, for post-reform cohorts, of the closest

[93] When estimating the probability of having received a university degree until the age of 25, we left out household income and its squared term to achieve convergence. The results are qualitatively the same when leaving the variables in and stopping the estimation after 25 iterations, and when comparing OLS results including the variables to those that do not. The only difference is that the placebo test for the outcome "university degree at age 25" is not

school offering grades 7–9 measured in year 1991[94]. We note that the appropriate geographical range for the definition of competition is clearly ambiguous since competitive pressure might be felt at different levels. At first sight, it appears that the headmaster should mostly be concerned about being compared to her immediate neighbours. In addition, though, there is a competition for good teachers, particularly when larger numbers of new private schools opened up who need to hire teachers. Yet, teacher labour markets certainly pass beyond the immediate vicinity. We therefore also examined various other definitions of competition in our robustness analyses.

The measures for school competition and school choice are related but still have a number of distinct features. First, a student's choice options are not directly influenced by the relative size of the schools she might attend, while the competitive pressure a school (director) faces from a neighbouring school is strongly influenced by the capacity of the competing school to take up a significant share of its own students. Second, only schools that offer the grade level a student plans to attend in the following school year are relevant for her choice, while schools will also feel competitive pressure from schools that offer other common grade levels. Nevertheless, the measures for school competition and school choice capture similar phenomena and are therefore highly correlated.[95]Since it is not obvious which is the appropriate radius for measuring choice and competition, we will present results for different definitions of choice and competition in order to gauge the sensitivity of the results.[96] Table 20 shows the effect of choice and competition at grade level 7-9 on the percentile rank in GPA in grade 9 for different combinations of our choice and competition measures, when including both measures in the regression. The first two rows of the table display which measure was used for choice and which one for competition. In column one, choice is defined as the number of schools within the median commuting distance and competition as the

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significant when excluding household income and its squared term. We are therefore careful in our interpretation of these results.

[94] The mean and standard deviation for this measure for cohorts that were not affected by the reform are 7.13 and 11.24 respectively. For affected cohorts, i.e. those born between 1977 and 1990, the mean is 14.6 and the standard deviation 17.44, and thus a lot higher than for pre-reform cohorts. This difference is however mostly explained by the 1994 law changed that allowed to choose also among public schools in municipalities other than the one of residence,

[95]See Table 19.

[96] It should also be kept in mind that assigning a competition measure to a student is less accurate than assigning a choice measure as we cannot use the actual school the student went to for affected cohorts.

number of schools within a 100km radius, weighted by their distance and student body size (see Equation (14) for the mathematical formula). Columns two to five show the results for all combinations of choice and competition measures that count the number of schools within a 2km and the median commuting distance radius[97].

The overall pattern that emerges from this exercise is that the positive but small effect of choice that was found in the baseline analysis is in most specifications robust to adding our measures of competition. The size of the choice effects is fairly similar to the baseline result: whereas they vary between the specifications in the table, they are always small, and are often positive and statistically significant.

On the contrary, the effect of the nearest school facing a lot of competitive pressure is negative and significant for the older cohorts. However, it gets more positive for the youngest cohorts and, depending on the choice measure used, even reaches positive significance for the youngest cohorts. Again, all effects are economically small. One interpretation of these results is that the competitive pressure might at first have shaken up the system and caused disruptions[98], resulting in a lower school quality, but that this effect faded out as schools learned to adjust to the new situation. However, as the results on the competition effect depend on the combination of choice and competition measures included in the estimation, and as both measures are highly correlated, it seems difficult to reliably separate between the two effects.

3.6.4 Effects of time-varying post-reform measures of choice

In our empirical analysis so far, we examined the impacts of choice and competition based on the pre-determined location of schools and individuals since these were plausibly not affected by the choice reform.

It is likely that the reform itself started an endogenous process of school development, where some schools, particularly non-public schools, started up, and others were closed. This process may also have affected families' choices of where to live. In order to shed light on these processes, we will study the relation between the pre-determined and the actual availability of schools in this section. Moreover, we will

[97] We do not show the combination: choice 2km with competition 100km since the geographical reach of these measures is too different.

re-run the baseline regression equation using the actual locations of residence and schools to measure choice and competition instead of the pre-reform measures. This allows us to specifically take into account choice opportunities among public and private schools separately. Even though these results will be only suggestive due to the endogenous location choices, they are still interesting since we can already control for many individual- and region-level covariates.

We will label definitions of choice based on the location of schools and individuals before the reform as *pre-reform* measures, whereas the *post-reform* measures (also called *actual* measures in the below sections) will refer to the location at the time when a student potentially chooses school, i.e. when starting grade 7, in our analysis. For example, for students entering first grade in 1995, the post-reform measure will be calculated based on the locations in 1995, for those entering in 1996, the locations in 1996 will be used, etcetera.

3.6.4.1 Number of schools before and after the reform

We start by exploring how the pre-reform and post-reform choice measures differ. The aim is to achieve a better understanding of how the pre-reform situation is related to the choice situation that evolves after the reform. Specifically, we will test if the change over time in the number of available schools as measured by our choice-index is correlated with the choice-index at the time the reform was implemented.

In order to do so, we take the difference between the post-reform and the pre-reform choice measures, and regress this difference on the pre-reform choice measure and on a linear trend that is interacted with the pre-reform choice measure.[99] This specification will show how the change in the number of available schools over time is correlated with the initial choice situation that a student faced in 1991. In a second specification, we add all control variables used in the main estimations and, additionally, the parish-average 9th grade percentile rank of cohort 1972, i.e. of the first cohort for which we have information on educational outcomes and who finished 9th grade in 1988, four

[98] See Waslander, Pater and van der Weide (2010) for a related case study on Stockholm.
[99] The estimations include only data on individuals that started grade seven in or after the year 1992, i.e. birth cohorts 1979-1990, as these are the ones for which we use the pre-reform measures in the main estimations.

years before the choice reform.[100] The idea of controlling for the latter variable is to explore whether the difference in the number of schools is correlated with previous educational outcomes. Keeping the notation used in Section 3.5, the estimation equation for the second specification, including all covariates, reads as follows:

$$(15)\quad c_i^{post-reform} - c_i^{pre-reform} = \alpha \cdot c_i^{pre-reform} + \beta \cdot c_i^{pre-reform} \cdot t_i + \gamma_{cohort} + \lambda_{municipality} + \delta \cdot X_i + u_i$$

where t_i = year of birth – 1972.

Table 21 shows the results of the estimations. We focus on the choice measure "number of schools within median commuting distance" and run separate regressions looking at all schools, only public schools, and only private schools, respectively.[101] Note that the difference between the pre- and post-reform measures is due both to schools opening up and closing down, and to students moving. For the private schools, the difference between the pre-reform and the post-reform measure reflects the growth of the private school sector after the reform as there were almost no private schools in Sweden before the reform.

The upper panel of Table 21 shows the coefficients on the pre-reform choice measure and its interaction with a trend. The resulting marginal effects for each treatment window cohort group are displayed in the lower panel. Note that these are averages of the cohort-specific marginal effects of all cohorts in the respective treatment window. A cohort-specific marginal effect is computed by adding the base coefficient to the product of the interaction coefficient and the value of the trend variable for the specific cohort. Columns two, four and six include additional covariates.

Focusing first on the marginal effects when no additional covariates are included, that is columns one, three and five in the lower panel, we see that having more schools around before the reform is associated with a more positive difference between the actual and the pre-reform number of overall, public and private schools for all cohorts except for those in the first treatment window, cohorts 1979–1981. This holds both when we pool the public and private schools, and when we run separate regressions.

[100] As in the previous estimations, all control variables are again based on the pre-reform location of residence.

[101] Note that we always use the median commuting distance *before* the reform, i.e. the range of the geographical area considered is not permitted to be endogenously changed by the reform.

However, controlling for all covariates and municipality and cohort fixed effects that are also included in the main estimation, we estimate negative marginal effects for all but the youngest three cohorts when we use only the public schools. The estimates on the increase in private schools are mostly unchanged, resulting in somewhat lower correlations between the pre-reform and actual measure when we pool both types of schools.

Since these effects are a combination of schools opening and closing, and families moving homes, it is hard to interpret this in terms of school openings only, especially for the public schools[102]. However, an important fact to note for the interpretation of our results is that the pre-reform measures of choice and competition are positively correlated with their post-reform counterparts in levels, and that having a higher pre-reform choice measure is positively related to the increase in the number of available private schools. This means that our pre-reform measure also captures the opening up of private schools after the reform, and that this dynamic process is thus included in our estimated effects.

Table 21 also reports the effect of the parish level average of the percentile rank in 9th grade GPA of students born in 1972 (that is the class finishing 9th grade in 1988) on the difference between pre- and post-reform choice measures (see the upper panel). It is always negative, though very small and mostly statistically insignificant, suggesting that, conditional on parental and parish level characteristics such as income and education, on average private schools do not seem to sort into areas based on student grades.[103]

[102] To illustrate this, imagine for example a student who will start 7th grade in the year 2000. In 1991, when we measure the pre-reform choice value, her parents might not have thought much about schooling yet, and may thus live in a region that has relatively few schools. By 2000, they may have moved to a region that allows their child to attend a school nearby. So we see an increase between the post- and the pre-reform measure. Another family in the same situation may be living in an area with many schools since long, so once their child starts school they do not need to move, and the difference between their pre-reform measure and the actual measure is zero if no school has opened up or closed. So without any new schools opening up, we see that the difference between the actual measure and the pre-reform measure is negatively related to having many schools nearby before the reform. The second mechanism is the opening up of new schools. Ignoring any moves by families, having more schools nearby before the reform may mean that fewer schools open up in the same area after the reform, and there may even be more potential for schools closing down. Again, this results in a negative correlation between the difference in the actual and the pre-reform choice measure and the number of schools nearby before the reform.

[103] Under the assumption that student grades are a valid indicator of ability, this can be generalised to indicating that the choice of location of private schools is not endogenous with respect to student ability.

3.6.4.2 *Effects of choice when using the actual choice opportunities at 7th grade*

The previous section indicated that, as expected, the choice-index changes somewhat over time. In this section we therefore show the results of our main regression specification when we, instead of using the pre-reform measures, use the actual measures of choice (that is, measured at the time a student starts grade 7). There are a few issues that are worth discussing before we turn to the results. Using the actual measures instead of the pre-reform counterparts means making a different set of assumptions and also gives estimates that have a different interpretation.

First, under the assumption that schools opened up randomly and parents did not move in reaction to the reform, using the actual choice-measure would estimate the pure effect of having more schools to choose from when entering grade 7. This is a different effect from the one we estimate when we use the pre-reform measures, which includes all dynamic processes (including subsequent changes in the number of schools and residential location) associated with having many schools nearby at the time of the implementation of the reform[104].

Second, however, as discussed in Section 3.5.1, there are reasons to believe that those assumptions, i.e. that schools opened up randomly and parents did not move in reaction to the reform, might not hold[105]. By including our extensive set of control variables on regional and individual characteristics we may be able to reduce this endogeneity problem to some extent, but we cannot be certain that it is fully eliminated.

Keeping this in mind, it is still interesting to analyse the association between the number of public and private schools nearby and student outcomes at the time when a choice is made. The first column of Table 22 presents the effect of the actual number of schools in the median commuting distance near a student's home offering grades 7-9 on

[104] Another caveat is that, when we measure the number of schools contemporaneously, we do not know at which point we are in the equilibrium process of schools opening up and closing in response to parental/student demand. It could for example be that having more schools nearby in period t leads to the bad schools closing down or being overtaken in period $t+1$, which leads to having only few schools nearby in period $t+2$. If the market worked perfectly, the schools remaining would be the very good schools, which would lead us to observe a positive association between few schools (low measures of choice) and good outcomes. A low choice index in $t+2$ could hence actually be the result of a highly competitive process. Thus, estimating the effect of more choice and competition in a dynamically evolving environment poses identification problems of a new nature.

[105] For example, schools that work for profit will probably have chosen the location of business such as to maximise profits, and parents who are concerned about their children's outcomes are more likely to move to areas that will increase the chances of educational success.

the percentile rank in GPA 9 (estimated according to Equation (12)). Rather surprisingly, we see that having more schools nearby at the time of making a decision has no significant positive effect for most cohorts, but even a small negative one for birth cohorts 1982-1984. Column three shows that using the number of schools within 2km as a choice measure leads to similar conclusions.

In order to obtain a better understanding of this result, we calculated the choice measures for public and private schools separately. Column two and four show results from estimations including these two measures and their interaction terms with treatment window dummies. For the radius "median commuting distance", the coefficients on the effect of having more public schools to choose from are always small and negative but only statistically significant at the 90% level for cohorts 1982-1984. However, using a 2km radius instead, we find small but significant negative effects of having more public schools nearby on the percentile rank in marks. At the same time, the point estimates on the number of private schools are mostly positive but again very small, economically zero, and almost never statistically significant.

Table 23 shows the results for the outcome "cognitive skills at age 18" which is estimated for men only. Also in this case the specifications using the choice measure based on the median commuting distance yields very small, basically zero, marginal effects. Using instead the 2km-choice radius yields somewhat larger effects, which are negative for the private but positive for the public schools.

In sum, controlling for our broad set of individual and region level covariates, we only find small and often insignificant associations between school choice at the time the individuals make their choice, both concerning public and private schools, and student outcomes.

3.7 Conclusion

In this paper, we analyse the effects of choice and competition caused by the introduction of the Swedish school choice reform in 1992. We find that more school choice, measured by having more schools nearby right before the reform, has small positive effects on marks at the end of compulsory schooling, and, depending on the choice measure used, very small effects on cognitive skills at age 18. We also analyse longer term outcomes such as university education, employment, criminal activity and health, and sometimes find small, but no economically relevant positive effects in these dimensions. Additional analyses to disentangle the effects of choice and competition suggest that competition, as opposed to choice, may have had small negative effects on marks right after the introduction of the reform, though these mostly fade out over time and are no longer present for the youngest cohorts in our sample. Even though we use different methods and identify a slightly different effect than previous studies on the Swedish choice reform, we come to a similar conclusion, namely that the choice reforms did not lead to large changes in average student outcomes, especially in the long run.

3.8 Appendix

3.8.1 Additional information and analyses

This section presents more detailed information on the Swedish school choice, other education-related reforms and additional analyses.

3.8.1.1 School choice in practice

For our study, it is important to know to what extent the reforms actually affected school choice as perceived by parents and students. This section aims to shed light on this issue.

The school choice reforms implemented in the early 1990s give quite some leeway for interpretation for the municipalities, which are in charge of providing compulsory schooling. Sweden's 290 municipalities vary a lot in size, from the small rural municipalities with a few thousand inhabitants and few schools, to the large densely populated urban municipalities with several hundred thousand inhabitants and many schools. It is hence likely that the practical implementation of the reforms differed between municipalities.

While information on the share of students opting for private schools is readily available, there exists relatively little information on the amount of active school choice taking place between public schools, especially for the early years after the reforms. Two surveys from the National Board of Education however provide information on the situation during school years 1994/95 and 2000/01.[106]

The 1994/95 survey, which contains information from the local authorities in ten large and predominantly urban municipalities[107], reports that seven per cent of the students switched to another school than the one they were assigned to before the start of the school year 1994/95. Out of these seven per cent, two per cent switched to a private school and five per cent to a public school, and most of the changes took place

[106] See The National Board of Education 1996 and 2003.

[107] The surveyed municipalities are: Stockholm, Göteborg, Malmö, Uppsala, Linköping, Helsingborg, Södertälje, Botkyrka, Täby and Östersund. The study also contains case studies of 38 schools, out of which eight were private schools, in 12 municipalities.

between grade 3-4 and grade 6-7. In the country as a whole, only 1.5 per cent of students chose another public school than the nearest one, while 1.8 per cent of all students attended private schools.[108] It hence seems that by the mid-nineties, a fairly small share of students chose another school than the one assigned.

By 2000/01, making an active school choice had become much more common. The report by the National Board of Education contains information from a survey to parents in six large urban municipalities where the scope for choice is deemed to be large[109], and in a set of smaller and more rural municipalities.[110] In the survey, 67 per cent of parents in the "high-choice municipalities" and 34 per cent of parents in the "low-choice municipalities" state that they have made an active school choice. About two thirds of these, for both sets of municipalities, were, however, choices to the nearest public school, to which the student was assigned anyway. For the remaining third of those who had made an active choice, again for both sets of municipalities, choosing another public school than the nearest one was a bit more common than choosing a private school. A small share of parents, 1–3 per cent, furthermore states that they made an active choice, but that they were not accepted due to lack of slots. It hence seems that the preferences of parents could be satisfied in the majority of cases. These figures suggest that by 2001, school choice was relatively common, but that there were large differences between municipalities.[111]

The 2003 report also collected information from the local authorities in all municipalities and town districts.[112] According to the estimates of the local authorities, reported by the survey, in school year 2000/01, almost a quarter of all students lived in municipalities and town districts where five per cent or more students attend another public school than the one in their catchment area, and five per cent of students lived in

[108] See p. 57 The National Board of Education (1996) for public schools, and, for private schools, the website of The National Board of Education: Table 1.1.A on http://www.skolverket.se/statistik_och_analys/.
[109] These municipalities are large and urban, and were also covered in the 1996 report, see The National Board of Education (1996).
[110] The high-choice municipalities" are: Botkyrka, Stockholm, Södertälje, Uppsala, Helsingborg and Västerås. The survey info for the "low-choice municipalities" was gathered for a large set of municipalities – and they are not reported by name in the report.
[111] Source: The National Board of Education (2003), pp. 48f.
[112] The larger municipalities are in general divided into town districts, which are responsible for some of the operations of the public sector.

municipalities and town districts where this share was 15 per cent or higher.[113] Regarding the private schools, the National Board of Education (2005) reports that the number of private compulsory schools nationwide grew from a bit over 200 in 1995 to over 500 in 2005. In 2002 the municipality-wise share of students that attended a private school was on average 5 per cent, and was considerably higher, 12 per cent, among the large cities.[114]

The 2003 survey from the National Board of Education also suggests that, by 2000/01, a bit less than 30 per cent of all municipalities and town districts[115], predominantly in urban areas, have a policy to encourage parents/students to make an active school choice, and almost 40 per cent provide parents/students with comprehensive information about the schools available in the municipality. About a quarter of the municipalities and town districts state that they provide school transport also to other schools than the nearest one.

About half of the parents in the 2003-survey also report that they had enough knowledge to make a well-informed choice.

We conclude that school choice has become increasingly common during the almost 20 years that have passed since the choice-reforms of the early 90s, and parents/students choose both to attend another public school than the nearest one and to attend private schools.

3.8.1.2 Other education-related reforms

The school choice reforms in the early 1990s were not the only education-related changes taking place in the 1990s, but they were part of a broad decentralisation and choice-enhancing trend in the organisation of the educational sector, as well as in the public sector in general. This section gives an overview of the other reforms that took place in the 1990s. This is useful both in order to provide a deeper understanding for the

[113] These figures were calculated using the raw data from the survey to the local authorities, which we were generously given access to from the National Board of Education. See also Table 3.8, The National Board of Education (2003)

[114] See the National Board of Education website for education statistics http://www.jmftal.artisan.se.

[115] The survey was directed to officials of the municipalities, or, in the case of larger municipalities which are divided into town districts, officials of the town districts.

environment in which the school choice reforms took place, and in order to discuss other changes that took place in relation to our evaluation method.

One of the major education-related reforms of the 1990s, apart from the choice-reforms, was the 1991 decentralisation of the Swedish compulsory school system.[116] The reform changed the role of the central government from providing detailed regulation for the municipalities and schools to follow, to specifying broad goals on what the students should know at each completed level of education but by large leaving it to the schools and municipalities to decide how to achieve these goals. The evaluation of whether the schools meet the goals specified in the Law and National curriculum was, until the establishment of the Swedish Schools Inspectorate in 2008, by large left to the schools themselves.[117] [118]

After the reform, municipalities and the individual schools were thus given considerable freedom to design the education, as long as they follow the basic curriculum.[119] The reform also made teachers and headmasters, previously state-employed, employees of the municipalities.[120]

Another part of the decentralisation trend of the early 90s was the replacement of the previously earmarked central government grants[121] with a system of general grants in 1991. At first, the grants were sector-specific, but in 1993, the grants were made completely general. Through the reform, the local politicians were hence given more decision power over the use of central government grants, both in terms of how much to allocate to education per se and how much to allocate to different education-related items.

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116 See Proposition 1990/91:18, SOU (2008) (p. 49f), and von Greiff (2009).

117 National standardised grade 9 tests were in addition made mandatory in 2003, and in 2009 for grades 3 and 5. Previously, standardised tests were available but were up to the schools to use or not.

118 See Proposition 2008/09:87, SOU (2007a), SOU (2007b), and Björklund et al (2004).

119 The school law (1985:1100) names the overall goals for the education system, as well as overall guidelines for the overall design of the education. It specifies the minimum requirements that the schools need to fulfil, such as how much time should be devoted to each subject.

120 At first, teacher pay negotiations remained centralised, but in 1995, the responsibility for the negotiations was transferred to the school level, and many schools adopted partly individualised wage schemes (Björklund, Clark, Edin, Fredriksson and Krueger (2005)).

121 Until 1991, central government grants were earmarked for specific educational expenses. These grants were to cover for expenses that were directly related to actual teaching, with teacher salaries being the largest post. According to von Greiff (2009), the system for the allocation of the central government grants was very complex and non-transparent. The municipalities were responsible for financing, through income tax revenues, facilities, school food,

In addition to the reforms described above, there are a couple of changes in the economic regulations for municipalities that took place during the 90s merit mentioning. First, during 1991–1994, municipalities were prevented from raising the local income tax, which constitutes their main source of income. Second, the rules for municipal budget balance have changed over time; the requirement for local budget discipline was relaxed in 1992 in order to become stricter again in 1997.[122]

We can conclude that there is a set of other reforms that are related to the education sector during the period under study. In which sense are they relevant for our study?

First, the decentralisation reforms gave the municipalities more say in how to organise compulsory education and how to allocate resources, while the tax rate cap and the stricter budget discipline are likely to have contributed to making the local education budget more sensitive to the local economic development. One can suspect that this may have given rise to larger variation in the education policy between the municipalities, but there is little guidance available from previous studies on whether this actually happened.[123] Second, the decentralisation reforms mean that schools have more freedom to choose pedagogical style and curriculum, and potentially also over the local budget process.[124]

3.8.1.3 Moving patterns

Figure 7 and Figure 8 show the municipality average for the share of households that moved during the year, separately for households in which the oldest child was aged 0–3, 4–6 etcetera, and in which there were no children aged 0–17[125]. Moving is defined as either moving into the municipality or moving between parishes within the

school transport, school medical care and teaching material. In addition, they were free to add to the central government transfers for all posts except for the teacher salaries.

[122] In 1992, the previous requirement of yearly balanced municipal budgets was changed to a more general statement that the local economy should follow "good economic housekeeping" (the Swedish term is: "god ekonomisk hushållning"). In 1997, the municipality law again was made stricter, stating that at the latest in 2000 all municipalities should follow a balanced budget over a 3-year period.

[123] In fact, the only evaluation study that we are aware of, Ahlin and Mörk (2008), find some evidence of a less disperse distribution of education resources (measured as per student costs and teacher density) between municipalities after the decentralisation reform, and find no correlation between the municipal tax base and per student school resources (excluding costs for facilities), neither before nor after the reforms.

[124] See p. 21 in The National Board of Education (2009).

[125] This analysis was conducted on data that was generously made available from the Institute for Labour Market Policy Evaluation (IFAU). These data contain indicators for number of children living in the household, in the age spans 0–3, 4–6, 7–10, 11–15 and 16–17.

municipality. If Tiebout migration (in terms of moving in order to get into a good school) was affected by the school choice reform, we would expect to see a different moving pattern after the reform for households with school aged children, and probably especially so for households in which the oldest child is about to enter school. Unfortunately, we can only observe the migration patterns for households with and without children starting from year 1991, that is only one year before the reform, which is why it is hard to draw too much on the pre-reform moving patterns from the figures. Still, if Tiebout migration changes in response to the reform, it is likely that the moving pattern changes gradually as there is evidence that the impact of the reform was also gradual (see Section 3.6.1). Section 3.8.1.4 also suggests that school choice is more common in more urban, more densely populated, areas. We therefore show the moving patterns separately for urban and small/rural municipalities. While the share that move varies a bit over time, there is no clear indication in that the households for whom Tiebout migration can be expected to be relevant, i.e. those with children who are just about to start school (i.e. children aged 4–6), or children who have just started school (children aged 7–10) have changed their moving pattern after the choice reform. The figures neither suggest that households with school-aged children in urban municipalities have become relatively less likely to move, compared to the small and rural municipalities, which would be expected if Tiebout migration decreased as a result of increased options to choose school without moving.

Figure 7: Share moving to the municipality or between parishes within the municipality, average for urban municipalities, for households with the oldest child in different age spans

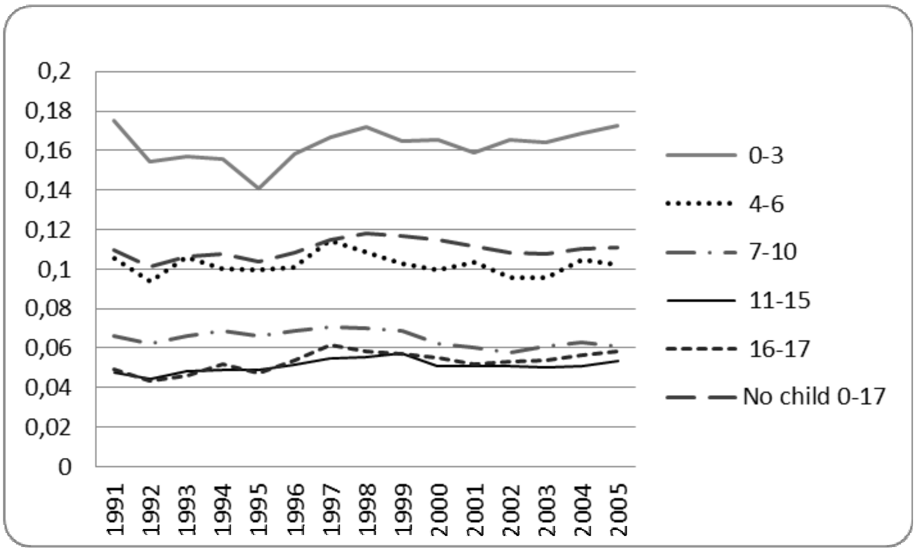
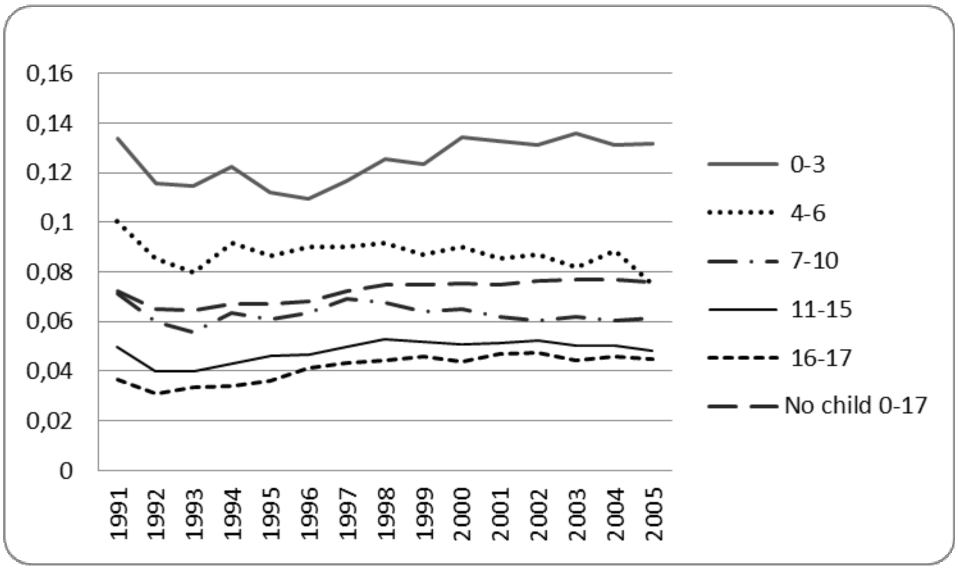


Figure 8: Share moving to the municipality or between parishes within the municipality, average for smaller/rural municipalities, for households with the oldest child in different age spans



3.8.1.4 Heterogeneity of effects with respect to region

The analyses in Section 3.6 explored the average effect of choice and competition introduced by the choice reform. However, it is possible that different types of municipalities have had different policies on school choice, which means that the effects may vary across municipalities. In order to address this issue, in this section, we re-estimate our specifications for 9th grade GPA and military draft test score, when we divide municipalities along the following two dimensions.

First, we analyse the effects separately for individuals living in urban and non-urban municipalities before the reform[126]. The reason for this division is that school choice is likely to be more of an urban phenomenon due to, for example, the higher population density and easier transportation in urban areas.

Second, even for municipalities in the more urban areas, the impact of the school choice reform is likely to differ due to differences in local policy. In order to take this into account, we focus on the municipalities in the county of Stockholm and divide them into municipalities that were early or late adopters of school choice, in terms of actively facilitating and encouraging residents to make active school choices.[127] The early adopters consist of those that actively encouraged school choice in the early or mid 1990s, for example through providing information on schools and how to make a choice in practice, or by having clear student-based systems for allocating resources between schools, whereas the late adopters are those where school choice became more of an issue later on in the 2000s.

Table 24 and Table 25 present separate estimations for the two groups of urban and non-urban municipalities, and Table 26 and Table 27 show the corresponding for the two groups of early and late school choice adopters within Stockholm county.

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[126] The classification of municipalities as urban and rural follows the classification of the Swedish Association of Local Authorities, see http://www.skl.se/kommuner_och_landsting/om_kommuner/kommungruppsindelning. This divides municipalities into nine categories (metropolitan, suburban, large cities, commuter, sparsely populated, manufacturing, and other – divided into population >25,000, 12,500-25,000 and <12,500) on the basis of structural parameters such as population, commuting patterns and economic structure as of Jan 1 2005. We classify a municipality as urban if it is thus defined as a city, suburb or "large town". This results in 68 municipalities being labelled as urban.

[127] In order to make this division of the sample, we contacted the municipalities in the county of Stockholm and asked them if they have a policy to actively facilitate and encourage residents to make an active school choice, and if so, when this policy was implemented. It shall be noted that, even though school choice is today a normal phenomenon in these urban municipalities, it was often not easy to find out exactly when it became common practice. The information gathered is therefore often not very detailed. In addition, we failed to receive answers from 11 out of 26 municipalities, although most of the non-respondents were the smaller municipalities in the county. The respondents were the following municipalities: Danderyd, Tyresö, Sollentuna, Haninge, Nacka, Norrtälje, Vallentuna, Upplands-Bro, Stockholm, Upplands-Väsby, Huddinge, Salem, Ekerö, Botkyrka and Täby. The non-respondents were: Lidingö, Solna, Sundbyberg, Södertälje, Järfälla, Österåker, Värmdö, Nykvarn, Vaxholm, Sigtuna and Nynäshamn. The

Focusing first on the division of municipalities into urban and non-urban, the overall pattern in Table 24 and Table 25 is that school choice is related to more positive outcomes in the urban municipalities, while there is some indication of a negative relation for the non-urban municipalities. Specifically, the regressions on 9th grade GPA (Table 24, Columns 3 and 5) yield positive and statistically significant estimates for the treated cohorts in the urban municipalities, which are similar to the estimates from the pooled baseline specification (see first Columns in Table 17 and Table 18). In contrast, the estimation including only non-urban municipalities yields non-significant effects when the median commuting distance is used, see Column 2, although the empirical identification is problematic for this outcome[128], and negative effects when we use the choice-measure based on the 2-km radius (Table 24, Column 4). With around -0.4 to -0.5 percentile ranks per additional school, the effect is however small, especially when taking into account that the standard deviation of this choice measure in non-urban areas is only 0.85. We conclude that the positive, albeit small, estimated effect of school choice seems to be limited to the urban municipalities, and that there is some, although weak, evidence of negative effects in the non-urban municipalities.

The estimates for the outcome cognitive skills from the military draft test scores in Table 25 show, similarly to the main estimations, little evidence of any significant effect when using the median commuting distance as radius (Columns 1-2), while the radius 2 km yields small positive effects of more choice in both urban and non-urban municipalities, although the estimates are almost only significant for the former.

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following of these were classified as being early adopters of school choice: Tyresö, Stockholm, Vallentuna, Nacka, Danderyd, Täby and Upplands-Bro.

[128] Note that the result in Column 1 suggests statistically significant estimates for the youngest cohorts and those born between 1982 and 1984. However, we also find a significantly negative placebo effect of -0.251 percentile ranks, implying that the negative effects were not necessarily caused by the reform but already there before it was enacted. When we include a pre-reform trend and interact all our treatment windows with the trend (Equation (13)), this placebo-test is no longer significant, though the pre-reform trend also is not. In this specification, shown in the second column, the standard errors are much larger and the point estimates are now positive and no longer significantly different from zero. However, since the pre-reform trend that is estimated on cohorts 1972-1976 is being extrapolated to form a control group as far as 14 years into the future, this specification is very sensitive to the estimated trend. Since the pre-reform trend is not even statistically significant from zero, one should be very cautious in interpreting this result.

Turning to the division between early and late adopters among the municipalities in Stockholm county[129], Table 26 shows that, when we use the choice measure based on median commuting distance (Columns 1–2), students living in the early-choice-adopting municipalities seem to have benefited slightly more in terms of receiving higher grades, while students in the late choice-municipalities sometimes even have a negative effect of having more schools nearby. However, this effect disappears when looking at the measure using a 2km radius: In Columns 3–4, we see that students in both municipalities seem to have benefited from having more schools nearby, even though the standard errors are a bit larger in the smaller sample living in the "late-adopting" municipalities, thus causing the marginal effects to be less often statistically significant.

Table 27 presents results for the outcome cognitive skills. They indicate some significantly negative effects of having more schools nearby in the "late-adopting" municipalities, when we use the larger radius "commuting distance". However, the negative effects and the differences between the two groups vanish when using the 2km radius.

Summarising, we find that having more schools nearby right after the reform seems to have had more positive effects on marks and cognitive skills of students living in urban areas at that time, even though the size of the effect is still small. Looking at Stockholm county only, differences only show up when using the radius "commuting distance" to measure choice but mostly vanish when using the 2km-radius.

3.8.1.5 Grade inflation

It is a major concern that grades have been inflated since competition between schools has increased. If parents care about grades, schools have an incentive to give slightly better grades in order to attract more students. The grade point average at the end of 9th grade determines admission into upper secondary schools in Sweden and will thus be an important and observable scholastic output for parents. In addition, standardised national tests, which can be used by teachers as a check for the grade

[129] Naturally, the sample for this analysis is restricted to students living in municipalities that provided information in our survey, in 1991 or the year they make the decision to start 7th grade, if that was before 1991, respectively.

levels, were not mandatory for schools to use during most of the years we study. The schools' need to be attractive for students should be larger in areas with a lot of competition, such that any potential positive effects of more competition on marks cannot easily be disentangled from grade inflation.

How serious is this concern in the Swedish case? Wikström and Wikström (2005), who compare the final grades from upper secondary school in 1997 to the SweSAT national test scores, find no evidence that competition from private schools, as measured on the municipality level, leads to grade inflation. However, they find that the difference between a standardised test and grades at the end of upper secondary school is much larger in independent schools. Vlachos (2010), who uses data on grades and national standardised test scores, finds no indication of different rates of grade inflation between private and public schools overall but finds some evidence of higher grade inflation in private for-profit schools.

The potential link between school competition and grade inflation should still be taken seriously, and in this study we have addressed this by considering also outcomes determined outside of the school, like the cognitive score in the military test and labour market outcomes. However, it could still be that inflated grades permit admission to better high schools which in turn also improves these "real" outcomes. For that reason, we provide another robustness check by using data on student grades from mandatory standardised national tests in English, Swedish and Maths, taken at the end of grade 9, that we have available for the years 2003-2008[130]. We use these data to test if our school choice measures can predict grades even when we control for the test results, something which would indicate that grade inflation is present. However, it is to be kept in mind that this approach has a potential pitfall: if grades measure something different than just performance in tests, then any additional explanatory power of school choice over and above tests could result from choice positively affecting other skills that are necessary to obtain a good grade.[131]

[130] Previously, these tests were voluntary for the schools, and are not centrally available, which is why we cannot use them for all cohorts.

[131] In Sweden, teachers form grades by assessing the class room performance of students. Noncognitive skills, such as pro-social behaviour, patience and the ability to control ones temperament, might be of higher importance in receiving a good grade in the class room than in performing well in a standardised test.

Since we are interested in whether the effects that we capture with our choice measures are driven by grade inflation, we use the exactly same measures as in the main estimations to test for grade inflation here. As we only observe the test scores for 9th graders in the years 2003-2008, we only use cohorts from our sample that were 16 in these years, i.e. cohorts 1987-1990.

We estimate Equation (16), using the percentile rank in GPA in grade 9 as outcome ($rank\ GPA_i$) and controlling for all three test results that we have information on (Swedish, English, Maths) at the same time. The subject-specific grades are given on an ordinal 1–4 scale (no pass, pass, pass with distinction and pass with special distinction). The test grades are included as dummies for each of the *k=4* pass-categories. We include the same covariates as we did in the main estimation, including the municipality dummies, and our choice measure.

$$(16)\ rank\ GPA_i = \alpha \cdot c_i + \beta_{km} \cdot math9_{ik} + \beta_{ks} \cdot swedish9_{ik} + \beta_{ke} \cdot english9_{ik} + \gamma_{cohort} + \delta \cdot X_i + u_i$$

The idea is that even though these test results only account for some of the grades that make up the grade point average, finding no effects of our choice measure when we include them could be seen as an indication that grade inflation is probably not correlated with our choice-measure.

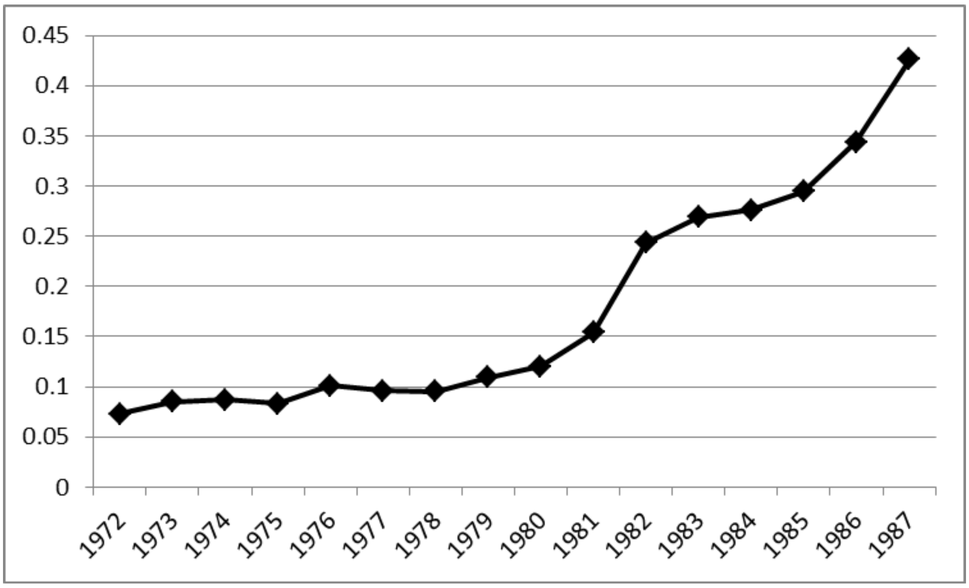
Table 28 shows the results for the choice measures counting the number of schools within the median commuting distance and 2km around a student's home in columns one and two respectively. We find small statistically significant effects of choice on the grades.[132] However, the coefficients are smaller than the marginal effects from our baseline estimation in Sections 3.6.1 and 3.6.2. We can thus conclude that, even if there is some grade inflation related to having more schools to choose from, its effect on our estimates is probably small.

[132] Note for the interpretation of the coefficients that student test score grades are measured at an ordinal 1–4 scale, while our dependent variable for 9th grade GPA is in terms of percentile rank.

3.8.1.6 The military score

As mentioned in Section 3.4, the share of men going through the drafting process declined significantly starting from the late 1990s. This means that for the younger cohorts in our sample, no longer all Swedish men took part in the military test. Figure 9 shows that the share of men not taking part in the draft test rose from around 7 per cent for the cohort born in 1972 to around 43 per cent for cohort 1987. The sharp increase started with men born in the years 1980 and 1981.

Figure 9: Share of men in each cohort for which we do not observe the military test



This raises concerns about potential selection into taking the test and being ready to possibly serve in the military. In order to test whether such selection is likely to bias our results, we run our treatment effect analysis with the same covariates we use for the main estimations on the outcome "not taking part in the test". Results of this analysis are presented in Table 29.

We find that having one more school offering grade 7-9 within the median commuting distance of a student's place of residence in or before 1991 decreases the probability of missing the test by 0.44 percentage points for the cohorts that are most affected by the reform. We find similar results using the number of schools within a 2km radius; however, the placebo-test fails for this specification. Even though this effect is mostly statistically significant, it is relatively small compared to the share of men not taking part in the test in these youngest cohorts, which is 30 to 43 per cent. We thus do not believe that our results on the cognitive skills test score are biased in a quantitatively relevant way.

3.8.2 Tables

The tables are presented in the following order: First, Section 3.8.2.1 presents tables on descriptive statistics and estimation results from main analyses. Second, Section 3.8.2.2 presents tables on additional analyses that are included in Section 3.8.1 in the appendix. Lastly, Section 3.8.2.3 shows, for reporting purpose, more detailed estimation results relating to the analyses in Section 3.6.1 and 3.6.2.

3.8.2.1 Tables from main analyses

Table 13: Descriptive statistics for outcome variables

	PRE-REFORM COHORTS (*cohorts 1972-1976 are not affected*)			POST-REFORM COHORTS (*cohorts 1977-1990 are affected*)		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Percentile rank GPA 9	48.21	28.58	437 953	49.40	28.60	1 277 468
Cognitive score	5.06	1.93	213 145	5.04	1.94	403 161
University degree (at age 25)	0.35	0.48	445 295	0.41	0.49	692 729
Employed (at age 25)	0.71	0.45	446 509	0.69	0.46	698 068
Entry in criminal record (until age 22)	0.16	0.36	449 802	0.14	0.35	990 157
Health problem (at age 22)	0.07	0.26	448 043	0.08	0.27	985 478

Note: Sample contains only observations with full information on all covariates *X* given below Table 16.

Table 14: Descriptive statistics for pre-reform choice measures

	PRE-REFORM COHORTS (*cohorts 1972-1976 are not affected*)				POST-REFORM COHORTS (*cohorts 1977-1990 are affected*)			
	Mean	Std. Dev.	Median	Obs.	Mean	Std. Dev.	Median	Obs.
School choice								
Number of schools within median commuting distance	3.45	4.66	2	449 802	5.91	9.35	2	1 306 879
Number of schools within 2km	1.24	1.50	1	449 802	1.35	1.69	1	1 306 879

Note: The table displays pre-reform measures on grade level 7-9. Sample contains only observations with full information on all covariates *X* given below Table 16.

Table 15: Descriptive statistics on covariates in the estimation

	Mean	Std. Dev.	Median
Municipality level variables			
Population density	392.35	876.36	64.00
Average taxable income in year t-2 in 100 SEK, deflated to 2006	1 079.05	153.39	1 067.89
Urban municipality	0.54	0.50	
Parish level variables			
Share of 16-64 year olds born in Sweden	0.89	0.08	0.92
Average yearly earnings of 20-64 year olds in 100 SEK	1 140.46	224.25	1 150.94
Share of 20-64 year olds with university degree	0.20	0.09	0.18
Share of 20-64 year olds that are employed	0.83	0.04	0.84
Population density of 7-15-year-olds in lower quartile of distribution	0.09	0.28	
Population density of 7-15-year-olds in highest quartile of distribution	0.64	0.48	
Individual level variables			
Household income in 1000 SEK, deflated to 2006	373.77	382.38	350.00
Household received welfare	0.06	0.24	
Age of mother at birth	27.78	5.05	27.00
Single parent household	0.22	0.42	
Number of children	2.23	1.01	2.00
Only child	0.23	0.42	
Child born in Sweden	0.96	0.19	
Mother born in Sweden	0.89	0.32	
Mother born in Scandinavia, outside of Sweden	0.05	0.21	
Mother born in western Europe, North America or Australia	0.01	0.10	
Father born in Sweden	0.88	0.32	
Father born in Scandinavia, outside of Sweden	0.04	0.19	
Father born in western Europe, North America or Australia	0.02	0.13	
Mother has university degree	0.31	0.46	
Mother's highest degree is from secondary education	0.49	0.50	
Father has university degree	0.27	0.44	
Father's highest degree is from secondary education	0.46	0.50	
Number of observations: 1 756 681			

Notes: summary statistics are on individual level, thus, statistics on municipal and parish level variables are weighted with the share of inhabitants. E.g.: this says that 55% per cent of the sample lives in an urban municipality, it does not mean that 55% of municipalities are urban.

Table 16: Results from main estimation for percentile rank in marks in grade 9

Outcome: PercentilePercentile rank GPA Grade 9
Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	Constant treatment effect	Piecewise constant treatment effect
Choice × Cohorts 1988-1990		0.130***
		(0.0183)
Choice × Cohorts 1985-1987		0.0772***
		(0.0186)
Choice × Cohorts 1982-1984		0.0100
		(0.0183)
Choice × Cohorts 1979-1981		0.0231
		(0.0195)
Choice × Cohorts 1977-1978		-0.0139
		(0.0227)
Choice × Cohorts 1977-1990	0.0607***	
	(0.0168)	
Choice	-0.0195	-0.0334*
	(0.0172)	(0.0173)
Constant	25.33***	26.95***
	(5.099)	(5.055)
Observations	1,715,421	1,715,421
R-squared	0.186	0.186

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. The definition of the placebo tests is explained in Section 3.5.1. The following control variables are included in the estimation:
On the municipality level: population density, taxable income and taxable income squared
On the parish level: share of Swedish citizens among the 16-64-year-old, mean earnings of the 20-64-year-olds, share of university graduates among the 20-64-year-olds, share of employed persons among the 20-64-year-olds, indicator variables for whether the population density of 7-15-year-olds is in the lowest or highest quartile across Sweden
On the individual level: household income and household income squared, whether the household received welfare, age of the mother at birth, whether living in a single parent household, number of children in the household, whether child was only child, whether child has Swedish citizenship, indicator variables on mothers and fathers citizenship separately (Swedish, Nordic (=Norwegian, Finnish, Danish), from other western country(=Western Europe, North America, Australia), rest of the world is base category), indicator variables on whether mother and/or father graduated from university or secondary education

Table 17: Results for later outcomes

Choice Measure: Number of schools in median commuting distance
Grade Level: 7-9

	Percentile Percentile Rank Grade 9	Cognitive Draft Score *(Men only)*	University Degree at Age 25 †	Employed Age 25	Any Crime until Age 22	Health Age 22
Cohorts 1988-1990	0.130***					
rel. to untreated	(0.0183)					
Cohorts 1985-1987	0.0772***	0.00203			-0.000560***	0.000262**
rel. to untreated	(0.0186)	(0.00154)			(0.000190)	(0.000118)
Cohorts 1982-1984	0.0100	0.000503	0.00138***	0.00072***	-0.000680***	0.000131
rel. to untreated	(0.0183)	(0.00155)	(0.000292)	(0.000258)	(0.000195)	(0.000120)
Cohorts 1979-1981	0.0231	-0.000367	0.000183	0.000621**	-0.000577***	0.000252*
rel. to untreated	(0.0195)	(0.00162)	(0.000308)	(0.000282)	(0.000209)	(0.000129)
Cohorts 1977-1978	-0.0139	0.00267	-0.000581	0.000012	-0.000022	0.000201
rel. to untreated	(0.0227)	(0.00213)	(0.000395)	(0.000328)	(0.000278)	(0.000165)
Untreated Cohorts	-0.0334*	0.000143	-0.000191	-0.00121***	0.000744***	-0.000333***
(1972-1976)	(0.0173)	(0.00155)	(0.000290)	(0.000265)	(0.000195)	(0.000117)
Placebo test:	Pass	Pass	Fail	Fail	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	1,715,421	610,182	1,120,459	1,120,845	1,409,092	1,402,829
R-squared ‡	0.186	0.146	0.126	0.0300	0.0382	0.0290

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

† For the outcome university degree at age 25, we had to leave out household income and its squared term to achieve convergence. The results are qualitatively the same when leaving the variables in and stopping the estimation after 25 iterations, and when comparing OLS results including the variables to those that do not.

‡Pseudo R-squared for binary outcomes.

Table 18: Estimation results using number of schools within 2 km as distance measure

Choice Measure: Number of schools within 2 km
Grade Level: 7-9

	Percentile Rank Grade 9	Cognitive Score *(Men Only)*	University Degree at Age 25 †	Employed Age 25	Any Crime until Age 22	Health Age 22
Cohorts 1988-1990 *rel. to untreated*	0.298***					
	(0.0570)					
Cohorts 1985-1987 *rel. to untreated*	0.0722	0.0193***			0.000351	0.000690*
	(0.0614)	(0.00507)			(0.000592)	(0.000413)
Cohorts 1982-1984 *rel. to untreated*	-0.115*	0.0103**	0.00565***	0.00229***	0.0000935	-0.000034
	(0.0602)	(0.00519)	(0.00103)	(0.000827)	(0.000628)	(0.000435)
Cohorts 1979-1981 *rel. to untreated*	-0.0729	0.0110**	0.00257**	0.00129	-0.000174	0.000283
	(0.0638)	(0.00534)	(0.00103)	(0.000875)	(0.000658)	(0.000441)
Cohorts 1977-1978 *rel. to untreated*	-0.106	0.0138**	0.000383	0.00141	0.00117	0.000456
	(0.0653)	(0.00615)	(0.00119)	(0.00101)	(0.000784)	(0.000475)
Untreated Cohorts (1972-1976)	0.138***	-0.0122***	0.000259	-0.00549***	0.000524	-0.000465*
	(0.0411)	(0.00371)	(0.000721)	(0.000656)	(0.000464)	(0.000282)
Placebo test	Pass	Pass	Pass †	Fail	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	1,715,421	610,182	1,120,459	1,120,845	1,409,092	1,402,829
R-squared ‡	0.186	0.146	0.126	0.0301	0.0382	0.0290

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

† For the outcome university degree at age 25, we had to leave out household income and its squared term to achieve convergence. The results are qualitatively almost the same when leaving the variables in and stopping the estimation after 25 iterations, and when comparing OLS results including the variables to those that do not. The only difference is that the Placebo test does not pass in these cases.

‡Pseudo R-squared for binary outcomes.

Table 19: Correlation between competition and choice measure on grade level 7-9

Choice Measures: Number of schools within radius…	Radius: Median Commuting Distance	Radius: 2km
Competition Measures		
No. of schools, weighted by distance and size of student body	0.6898	0.5277
No. of schools within median commuting distance	0.9462	0.5016
No. of schools within 2km	0.4482	0.7594

Table 20: Results disentangling effects of choice and competition

Outcome: Percentile rank GPA in Grade 9
Choice Measure: Number of schools within certain radius around students' home
Competition Measure: Number of other schools within certain radius around school
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE			RADIUS: 2KM	
	Weighted by Distance and Student Body Size	Radius: Median Commuting Distance	Radius: 2km	Radius: Median Commuting Distance	Radius: 2km
Choice					
Cohorts 1988-1990 *rel. to untreated*	0.116***	0.237***	0.157***	0.114*	0.236***
	(0.0200)	(0.0330)	(0.0204)	(0.0631)	(0.0751)
Cohorts 1985-1987 *rel. to untreated*	0.0865***	0.212***	0.112***	0.0159	0.106
	(0.0204)	(0.0325)	(0.0208)	(0.0665)	(0.0749)
Cohorts 1982-1984 *rel. to untreated*	0.0315	0.167***	0.0459**	-0.0514	-0.0265
	(0.0200)	(0.0328)	(0.0205)	(0.0667)	(0.0762)
Cohorts 1979-1981 *rel. to untreated*	0.0611***	0.216***	0.0695***	0.0817	0.133*
	(0.0208)	(0.0353)	(0.0212)	(0.0697)	(0.0754)
Cohorts 1977-1978 *rel. to untreated*	0.0343	0.236***	0.0377	0.130*	0.116
	(0.0249)	(0.0416)	(0.0264)	(0.0767)	(0.0819)
Untreated Cohorts (1972-1976)	-0.0492***	-0.179***	-0.0742***	0.112**	0.0342
	(0.0182)	(0.0299)	(0.0196)	(0.0461)	(0.0523)
Competition					
Cohorts 1989-1990 *rel. to untreated*	0.00928	-0.126***	-0.0486	0.0584**	0.0531
	(0.00966)	(0.0388)	(0.0883)	(0.0268)	(0.104)
Cohorts 1986-1988 *rel. to untreated*	-0.0152*	-0.162***	-0.233***	0.0121	-0.135
	(0.00892)	(0.0372)	(0.0876)	(0.0259)	(0.0985)
Cohorts 1983-1985 *rel. to untreated*	-0.0221**	-0.178***	-0.181**	-0.0350	-0.132
	(0.00922)	(0.0375)	(0.0877)	(0.0261)	(0.0993)
Cohorts 1980-1982 *rel. to untreated*	-0.0377***	-0.217***	-0.352***	-0.0676**	-0.373***
	(0.00897)	(0.0387)	(0.0875)	(0.0263)	(0.0980)
Cohorts 1977-1979 *rel. to untreated*	-0.0413***	-0.290***	-0.336***	-0.126***	-0.407***
	(0.00897)	(0.0430)	(0.0904)	(0.0285)	(0.0981)
Untreated Cohorts (1972-1976)	0.0269***	0.179***	0.338***	0.0412	0.250***
	(0.00760)	(0.0352)	(0.0737)	(0.0253)	(0.0804)
Placebo test	Pass	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	1,688,234	1,688,234	1,688,234	1,688,234	1,688,234
R-squared	0.186	0.186	0.186	0.186	0.186

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. The combination (choice 2km, competition 100km) not shown because of very different geographical range. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1.

Table 21: Relation between pre-reform and post-reform number of schools

Outcome: Difference between number of schools before and after the reform
Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	All Schools	All Schools	Public Schools	Public Schools	Private Schools	Private Schools
Coefficients						
Pre-reform No. of schools	-0.534***	-0.638***	-0.161***	-0.217***	-0.373***	-0.420***
	(0.0423)	(0.0318)	(0.0245)	(0.0333)	(0.0261)	(0.0203)
Pre-reform No. of schools ×Linear Trend (cohort-1972)	0.0600***	0.0565***	0.0174***	0.0132***	0.0427***	0.0433***
	(0.00432)	(0.00255)	(0.00282)	(0.00214)	(0.00218)	(0.00145)
Pre-reform parish average percentile rank GPA 9		-0.00587		-0.00424*		-0.00164
		(0.00363)		(0.00234)		(0.00394)
Constant	0.893***	13.60	0.754***	-4.383	0.139**	17.99
	(0.172)	(10.32)	(0.134)	(14.03)	(0.0552)	(13.32)
Marginal Effects†						
Cohorts 1988-1990	0.486***	0.323***	0.134***	0.00757	0.353***	0.316***
	(0.0346)	(0.0410)	(0.0267)	(0.0171)	(0.0158)	(0.0096)
Cohorts 1985-1987	0.306***	0.154***	0.0815***	-0.0321**	0.225***	0.186***
	(0.0227)	(0.0153)	(0.0189)	(0.0156)	(0.0112)	(0.0078)
Cohorts 1982-1984	0.126***	-0.0160	0.0294**	-0.072***	0.0967***	0.0558***
	(0.0132)	(0.0137)	(0.0121)	(0.0167)	(0.00940)	(0.00812)
Cohorts 1979-1981	-0.054***	-0.186***	-0.0226**	-0.112***	-0.031***	-0.074***
	(0.0129)	(0.0160)	(0.00893)	(0.0198)	(0.0117)	(0.0150)
Cohort dummies	No	Yes	No	Yes	No	Yes
Municipality dummies	No	Yes	No	Yes	No	Yes
Full set of covariates	No	Yes	No	Yes	No	Yes
Observations	1,214,130	1,117,774	1,214,130	1,117,774	1,214,130	1,117,774
R-squared	0.207	0.245	0.035	0.083	0.524	0.554

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a list on the full set of covariates see Table 16.

† The marginal effects are averages of the cohort specific marginal effects of all cohorts in the respective treatment window. A cohort specific marginal effect is computed by adding the base coefficient to the product of the interaction coefficient and the value of the trend variable for the specific cohort.

Table 22: Effects of actual choice measures on percentile rank in GPA 9

Outcome: Percentile rank Grades 9
Choice Measure: Number of schools within radius...
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	All Schools	Public And Private Separate	All Schools	Public And Private Separate
Cohorts 1988-1990 *rel. to untreated*	0.0137		-0.0431	
	(0.0185)		(0.0489)	
Cohorts 1985-1987 *rel. to untreated*	-0.00643		-0.0916*	
	(0.0187)		(0.0519)	
Cohorts 1982-1984 *rel. to untreated*	-0.0387**		-0.124**	
	(0.0185)		(0.0533)	
Cohorts 1979-1981 *rel. to untreated*	-0.0145		-0.0417	
	(0.0188)		(0.0584)	
Cohorts 1977-1978 *rel. to untreated*	-0.0184		-0.0596	
	(0.0215)		(0.0624)	
Untreated Cohorts (1972-1976)	0.0190		0.205***	
	(0.0180)		(0.0406)	
Public choice				
Cohorts 1988-1990 *rel. to untreated*		-0.0127		-0.192***
		(0.0264)		(0.0613)
Cohorts 1985-1987 *rel. to untreated*		-0.0230		-0.212***
		(0.0263)		(0.0633)
Cohorts 1982-1984 *rel. to untreated*		-0.0456*		-0.160**
		(0.0252)		(0.0657)
Cohorts 1979-1981 *rel. to untreated*		0.0388		-0.0129
		(0.0245)		(0.0636)
Cohorts 1977-1978 *rel. to untreated*		-0.0255		-0.0661
		(0.0310)		(0.0736)
Untreated Cohorts (1972-1976)		-0.0243		0.197***
		(0.0230)		(0.0442)
Private choice				
Cohorts 1988-1990 *rel. to untreated*		0.0303		0.273
		(0.0700)		(0.175)
Cohorts 1985-1987 *rel. to untreated*		0.0746		0.291
		(0.0753)		(0.189)
Cohorts 1982-1984 *rel. to untreated*		0.0634		0.0200
		(0.0784)		(0.200)
Cohorts 1979-1981 *rel. to untreated*		-0.206**		-0.151
		(0.0881)		(0.236)
Cohorts 1977-1978 *rel. to untreated*		0.0328		-0.0284
		(0.102)		(0.245)

Table 22 continued

Outcome: Percentile rank Grades 9
Choice Measure: Number of schools within radius...
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	All Schools	Public And Private Separate	All Schools	Public And Private Separate
Untreated Cohorts (1972-1976)		0.138**		0.171
		(0.0653)		(0.162)
Constant	19.86***	20.63***	19.04***	20.47***
	(3.176)	(3.134)	(3.157)	(3.148)
Placebo test	Pass	Pass	Pass	Pass
Pre-reform trend	No	No	No	No
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	1,743,753	1,743,753	1,743,753	1,743,753
R-squared	0.186	0.186	0.186	0.186

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1.

Table 23: Effects of actual choice measures on cognitive score

Outcome: Cognitive skills
Choice Measure: Number of schools within radius...
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	All Schools	Public And Private Separate	All Schools	Public And Private Separate
Cohorts 1985-1987 *rel. to untreated*	-0.000212		0.00878*	
	(0.00160)		(0.00478)	
Cohorts 1982-1984 *rel. to untreated*	-0.000881		0.00563	
	(0.00159)		(0.00489)	
Cohorts 1979-1981 *rel. to untreated*	-0.00148		0.00814	
	(0.00158)		(0.00514)	
Cohorts 1977-1978 *rel. to untreated*	0.00262		0.0128**	
	(0.00205)		(0.00609)	
Untreated Cohorts (1972-1976)	0.00109		-0.0116***	
	(0.00159)		(0.00375)	
Public choice				
Cohorts 1985-1987 *rel. to untreated*		0.00325		0.0157***
		(0.00228)		(0.00573)
Cohorts 1982-1984 *rel. to untreated*		0.00449**		0.0192***
		(0.00221)		(0.00585)
Cohorts 1979-1981 *rel. to untreated*		0.00191		0.0162***
		(0.00220)		(0.00605)
Cohorts 1977-1978 *rel. to untreated*		0.00281		0.0158**
		(0.00292)		(0.00708)
Untreated Cohorts (1972-1976)		-0.00207		-0.0187***
		(0.00208)		(0.00420)
Private choice				
Cohorts 1985-1987 *rel. to untreated*		-0.0130**		-0.0497***
		(0.00590)		(0.0164)
Cohorts 1982-1984 *rel. to untreated*		-0.0224***		-0.0704***
		(0.00610)		(0.0174)
Cohorts 1979-1981 *rel. to untreated*		-0.0144**		-0.0488**
		(0.00668)		(0.0196)
Cohorts 1977-1978 *rel. to untreated*		0.00230		-0.0140
		(0.00839)		(0.0239)
Untreated Cohorts (1972-1976)		0.0133***		0.0462***
		(0.00493)		(0.0147)
Constant	2.518***	2.457***	2.449***	2.360***
	(0.267)	(0.269)	(0.267)	(0.269)
Placebo test	Pass	Pass	Pass	Pass
Pre-reform trend	No	Yes [†]	No	No
Observations	615,225	615,225	615,225	615,225
R-squared	0.150	0.150	0.150	0.150

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

3.8.2.2 Tables relating to analyses presented in the appendix

This section presents tables on additional analyses conducted in Section 3.8.1 in the appendix.

Table 24: Heterogeneity of effects with respect to urban vs. non-urban municipalities and outcome percentile rank GPA 9
Outcome: Percentile rank Grades 9
Choice Measure: Number of schools within radius…
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE			RADIUS: 2KM	
	Non-Urban Area		Urban Area	Non-Urban Area	Urban Area
Marginal effects					
Cohorts 1988-1990	-0.235**	0.807	0.147***	-0.494***	0.349***
rel. to untreated	(0.0914)	(0.682)	(0.0201)	(0.122)	(0.0646)
Cohorts 1985-1987	-0.0715	0.763	0.102***	-0.419***	0.165**
rel. to untreated	(0.0942)	(0.549)	(0.0205)	(0.131)	(0.0702)
Cohorts 1982-1984	-0.198**	0.426	0.0409**	-0.369***	0.0325
rel. to untreated	(0.0942)	(0.414)	(0.0200)	(0.138)	(0.0681)
Cohorts 1979-1981	-0.107	0.291	0.0497**	-0.153	0.0130
rel. to untreated	(0.0929)	(0.284)	(0.0216)	(0.126)	(0.0747)
Cohorts 1977-1978	-0.0604	0.182	0.00708	-0.193	-0.0118
rel. to untreated	(0.115)	(0.196)	(0.0248)	(0.144)	(0.0739)
Untreated Cohorts (1972-1976)	0.250***		-0.0632***	0.385***	0.0562
	(0.0695)		(0.0189)	(0.0843)	(0.0470)
Coefficients					
Choice		0.392***			
		(0.115)			
Trend×Choice		-0.0699			
(pre-reform trend)		(0.0453)			
Constant	15.93**	16.95**	30.33***	17.31**	24.40***
	(6.959)	(6.996)	(6.216)	(6.960)	(6.471)
Placebo test	Fail	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows×Trend	Treatment Windows	Treatment Windows	Treatment Windows
Observations	784,494	784,494	930,927	784,494	930,927
R-squared	0.164	0.164	0.199	0.164	0.199

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1.

Table 25: Heterogeneity of effects with respect to urban vs. non-urban municipalities and outcome cognitive skills

Outcome: Cognitive skills
Choice Measure: Number of schools within radius…
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	Non-Urban Area	Urban Area	Non-Urban Area	Urban Area
Cohorts 1985-1987 *rel. to untreated*	0.0194**	0.00146	0.0114	0.0223***
	(0.00954)	(0.00172)	(0.0135)	(0.00589)
Cohorts 1982-1984 *rel. to untreated*	0.0121	0.000786	0.0243*	0.0145**
	(0.00885)	(0.00174)	(0.0135)	(0.00612)
Cohorts 1979-1981 *rel. to untreated*	0.00757	-0.000396	0.00816	0.0154**
	(0.00946)	(0.00183)	(0.0127)	(0.00637)
Cohorts 1977-1978 *rel. to untreated*	0.00583	0.000771	0.0125	0.0118*
	(0.0115)	(0.00239)	(0.0146)	(0.00710)
Untreated Cohorts (1972-1976)	-0.0146**	0.000842	-0.0270***	-0.0110***
	(0.00675)	(0.00170)	(0.00840)	(0.00426)
Constant	0.927	2.566***	0.968	2.229***
	(0.639)	(0.473)	(0.639)	(0.471)
Placebo test	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	281,734	328,448	281,734	328,448
R-squared	0.124	0.161	0.124	0.161

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

Table 26: Heterogeneity of effects within Stockholm county and outcome percentile rank GPA 9

Outcome: Percentile rank Grades 9
Choice Measure: Number of schools within…
Sample: early vs. late adopters within Stockholm
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	Late Adopter	Early Adopter	Late Adopter	Early Adopter
Cohorts 1988-1990 *rel. to untreated*	-0.130	0.129***	0.577*	0.557***
	(0.0948)	(0.0432)	(0.316)	(0.123)
Cohorts 1985-1987 *rel. to untreated*	-0.158*	0.0866*	0.0931	0.437***
	(0.0951)	(0.0457)	(0.332)	(0.142)
Cohorts 1982-1984 *rel. to untreated*	-0.194**	0.0382	0.458	0.178
	(0.0946)	(0.0462)	(0.345)	(0.141)
Cohorts 1979-1981 *rel. to untreated*	-0.156	0.0238	1.027***	-0.0572
	(0.0952)	(0.0463)	(0.330)	(0.155)
Cohorts 1977-1978 *rel. to untreated*	-0.0285	-0.00243	0.663	0.0281
	(0.137)	(0.0426)	(0.411)	(0.142)
Untreated Cohorts (1972-1976)	0.216**	-0.0823**	-0.242	-0.149
	(0.0979)	(0.0348)	(0.243)	(0.0944)
Constant	-37.56	29.27***	-4.459	27.36***
	(68.16)	(8.584)	(67.65)	(8.379)
Placebo test	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	83,024	144,247	83,024	144,247
R-squared	0.176	0.205	0.176	0.205

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1.

Table 27: Heterogeneity of effects within Stockholm county and outcome cognitive skills

Outcome: Cognitive skills
Choice Measure: Number of schools within radius…
Sample: early vs. late adopters in Stockholm
Grade Level: 7-9

	RADIUS: MEDIAN COMMUTING DISTANCE		RADIUS: 2KM	
	Late Adopter	Early Adopter	Late Adopter	Early Adopter
Cohorts 1985-1987 *rel. to untreated*	-0.0233***	-0.00101	0.0519*	-0.000437
	(0.00832)	(0.00357)	(0.0313)	(0.0116)
Cohorts 1982-1984 *rel. to untreated*	-0.0292***	-0.000454	0.0209	-0.0216*
	(0.00834)	(0.00375)	(0.0319)	(0.0120)
Cohorts 1979-1981 *rel. to untreated*	-0.0298***	-0.000499	0.0256	-0.00473
	(0.00840)	(0.00371)	(0.0327)	(0.0123)
Cohorts 1977-1978 *rel. to untreated*	-0.0178	-0.00617	0.00655	0.000848
	(0.0117)	(0.00456)	(0.0367)	(0.0148)
Untreated Cohorts (1972-1976)	0.0301***	0.00320	0.00244	-0.0138
	(0.00873)	(0.00285)	(0.0225)	(0.00878)
Constant	3.349	1.871**	5.119	2.201***
	(6.795)	(0.783)	(6.636)	(0.792)
Placebo test	Pass	Pass	Pass	Pass
Specification	Treatment Windows	Treatment Windows	Treatment Windows	Treatment Windows
Observations	28,684	48,433	28,684	48,433
R-squared	0.167	0.166	0.166	0.167

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1. The mean of the cognitive score is 5, the standard deviation 1.9.

Table 28: Grade inflation
Outcome: Percentile rank GPA 9
Choice Measure: Number of schools within radius…
Grade Level: 7-9

	Radius: median commuting distance	radius: 2km
Maths:		
Pass	11.17***	11.17***
	(0.152)	(0.152)
Pass with distinction	27.20***	27.20***
	(0.189)	(0.189)
Pass with special distinction	35.31***	35.31***
	(0.208)	(0.208)
English:		
Pass	6.297***	6.299***
	(0.210)	(0.210)
Pass with distinction	12.18***	12.18***
	(0.236)	(0.236)
Pass with special distinction	16.34***	16.34***
	(0.263)	(0.263)
Swedish:		
Pass	8.577***	8.575***
	(0.214)	(0.214)
Pass with distinction	25.92***	25.91***
	(0.257)	(0.257)
Pass with special distinction	36.25***	36.25***
	(0.289)	(0.289)
Choice	0.0320**	0.0737*
	(0.0126)	(0.0433)
Constant	213.3**	214.0**
	(102.8)	(102.9)
Observations	173,284	173,284
R-squared	0.666	0.666

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16.

Table 29: Selection into taking the military test

Outcome: Not taking the military test
Choice measure: Number of schools within radius..
Grade Level: 7-9

	Radius: Median Commuting Distance	Radius: 2 km
Cohorts 1985-1987 *rel. to untreated*	-0.00440***	-0.00651***
	(0.000239)	(0.00103)
Cohorts 1982-1984 *rel. to untreated*	-0.00381***	-0.00619***
	(0.000226)	(0.00110)
Cohorts 1979-1981 *rel. to untreated*	-0.00178***	0.00138*
	(0.000210)	(0.000742)
Cohorts 1977-1978 *rel. to untreated*	-0.000639***	0.000904
	(0.000247)	(0.000777)
Untreated Cohorts (=1972-1976)	0.00186***	0.00286***
	(0.000175)	(0.000411)
Placebo test	Pass	Fail
Specification	Treatment Windows	Treatment Windows
Observations	723,147	723,147
Pseudo R-squared	0.134	0.134

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16.

3.8.2.3 *Tables presenting additional specifications related to main analyses*

The tables in this section show the coefficients and marginal effects from different specifications for the regression analyses in Sections 3.6.1 and 3.6.2 for reporting purpose.

Table 30: Different specifications, choice measure with radius "median commuting distance", outcomes marks and cognitive skills

Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	Percentile Rank GPA 9			Cognitive Score (Men only)		
Coefficients						
Trend×Choice × Cohorts 1988-1990			0.0266**			
			(0.0132)			
Trend×Choice × Cohorts 1985-1987			0.0168			0.00178
			(0.0138)			(0.00113)
Trend×Choice × Cohorts 1982-1984			0.0273**			-0.000591
			(0.0135)			(0.00112)
Trend×Choice × Cohorts 1979-1981			0.0408**			-0.00162
			(0.0164)			(0.00146)
Trend×Choice × Cohorts 1977-1978			-0.0329			-0.00593*
			(0.0340)			(0.00340)
Trend×Choice (*Pre-reform trend*)			-0.00647			-0.000227
			(0.00910)			(0.000813)
Choice × Cohorts 1988-1990	0.130***	0.125***	-0.228			
	(0.0183)	(0.0228)	(0.165)			
Choice × Cohorts 1985-1987	0.0772***	0.0717***	-0.0831	0.00203	0.00154	-0.0205*
	(0.0186)	(0.0229)	(0.149)	(0.00154)	(0.00187)	(0.0113)
Choice × Cohorts 1982-1984	0.0100	0.00444	-0.234**	0.000503	0.000015	0.00873
	(0.0183)	(0.0227)	(0.115)	(0.00155)	(0.00187)	(0.00900)
Choice × Cohorts 1979-1981	0.0231	0.0175	-0.283**	-0.000367	-0.000859	0.0146
	(0.0195)	(0.0236)	(0.120)	(0.00162)	(0.00195)	(0.0104)
Choice × Cohorts 1977-1978	-0.0139	-0.0195	0.187	0.00267	0.00217	0.0361*
	(0.0227)	(0.0266)	(0.184)	(0.00213)	(0.00237)	(0.0187)
Choice	-0.0334*	-0.0280	-0.0204	0.000143	0.000610	0.00106
	(0.0173)	(0.0219)	(0.0276)	(0.00155)	(0.00185)	(0.00237)
Placebo: Choice×Cohorts 1975-176		-0.0113			-0.000948	
		(0.0267)			(0.00227)	

Table 30 continued

Choice Measure: Number of schools within median commuting distance **Grade Level**: 7-9						
	Percentile Rank GPA 9			Cognitive Score (Men only)		
Marginal effects						
cohorts 1988-1990 *rel. to untreated*	see coefficients	see coefficients	0.225* (0.134)	see coefficients	see coefficients	
cohorts 1985-1987 *rel. to untreated*			0.153 (0.108)			0.00446 (0.00964)
cohorts 1982-1984 *rel. to untreated*			0.0662 (0.0805)			0.00222 (0.00727)
cohorts 1979-1981 *rel. to untreated*			0.0428 (0.0552)			0.00160 (0.00492)
cohorts 1977-1978 *rel. to untreated*			0.00569 (0.0361)			0.00349 (0.00337)
Untreated cohorts † †						
Constant	26.95*** (5.055)	27.01*** (5.055)	26.68*** (5.107)	2.412*** (0.398)	2.412*** (0.398)	2.352*** (0.398)
Specification	Treatment Windows	Placebo test	Treatment Windows×Trend	Treatment Windows	Placebo test	Treatment Windows×Trend
Observations	1,715,421	1,715,421	1,715,421	610,182	610,182	610,182
R-squared‡	0.186	0.186	0.186	0.146	0.146	0.146

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1.
† † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974.
‡Pseudo R-squared for binary outcomes.

Table 31: Different specifications, choice measure with radius "median commuting distance", outcomes university degree at age 25 and employed at age 25

Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	University Degree At Age 25 †			Employed Age 25		
Coefficients						
Trend×Choice×Cohorts 1982-1984			0.00160*** (0.000548)			-0.000511 (0.000510)
Trend×Choice×Cohorts 1979-1981			0.00161** (0.000725)			0.000203 (0.000683)
Trend×Choice×Cohorts 1977-1978			-0.00229 (0.00162)			0.000342 (0.00140)
Trend×Choice (*Pre-reform trend*)			-0.000724* (0.000403)			0.000963*** (0.000362)
Choice×Cohorts 1982-1984	0.00365*** (0.000777)	0.00191* (0.000987)	-0.00774* (0.00426)	0.00187** (0.000731)	0.00344*** (0.000912)	-0.000657 (0.00416)
Choice×Cohorts 1979-1981	0.000489 (0.000817)	-0.00127 (0.00102)	-0.00885* (0.00519)	0.00163** (0.000802)	0.00322*** (0.000965)	-0.00581 (0.00491)
Choice×Cohorts 1977-1978	-0.00151 (0.00104)	-0.00330*** (0.00121)	0.0132 (0.00870)	-0.00006 (0.000932)	0.00156 (0.00108)	-0.00493 (0.00761)
Choice	-0.000509 (0.000774)	0.00115 (0.000971)	0.00114 (0.00123)	-0.00340*** (0.000742)	-0.00490*** (0.000896)	-0.00587*** (0.00113)
Placebo: Choice×Cohorts 1975-1976		-0.00326*** (0.00116)			0.00304*** (0.00103)	
Marginal Effects						
Cohorts 1982-1984 rel. to untreated	0.00138*** (0.000292)	0.000737** (0.000364)	0.00126*** (0.000301)	0.000724*** (0.000258)	0.00134*** (0.000332)	0.000911*** (0.000265)
Cohorts 1979-1981 rel. to untreated	0.000183 (0.000308)	-0.000469 (0.000376)	-0.000125 (0.000336)	0.000621** (0.000282)	0.00124*** (0.000348)	0.000619** (0.000293)
Cohorts 1977-1978 rel. to untreated	-0.000581 (0.000395)	-0.00125*** (0.000455)	-0.000727* (0.000397)	0.000012 (0.000328)	0.000641* (0.000386)	0.000149 (0.000332)
Untreated Cohorts † †	-0.000191 (0.000290)	0.000424 (0.000357)	-0.000114 (0.000297)	-0.00121*** (0.000265)	-0.00180*** (0.000330)	-0.00141*** (0.000270)
Choice×Trend: Cohorts 1972-1976 (*Pre-reform trend*)			-0.000271* (0.000151)			0.000347*** (0.000131)
Placebo Cohorts		-0.00123*** (0.000436)			0.00117*** (0.000365)	
Constant	-1.463*** (0.235)	-1.453*** (0.233)	-1.494*** (0.235)	0.382* (0.203)	0.375* (0.203)	0.407** (0.204)
Specification	Treatment Windows	Placebo Test	Treatment Windows×Trend	Treatment Windows	Placebo Test	Treatment Windows×Trend
Observations	1,120,459	1,120,459	1,120,459	1,120,845	1,120,845	1,120,845
R-squared‡	0.126	0.126	0.126	0.0300	0.0300	0.0300

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1.
† For the outcome university degree at age 25, we had to leave out household income and its squared term to achieve convergence. The results are qualitatively the same when leaving the variables in and stopping the estimation after 25 iterations, and when comparing OLS results including the variables to those that don't. † † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974. Pseudo R-squared for binary outcomes.

Table 32: Different specifications, choice measure with radius "median commuting distance", outcomes crime until age 22 and health at age 22

Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	Any Crime until Age 22			Health Age 22		
Coefficients						
Trend×Choice × Cohorts 1985-1987			0.00105*			0.00238***
			(0.000609)			(0.000741)
Trend×Choice × Cohorts 1982-1984			0.00189***			0.000487
			(0.000620)			(0.000742)
Trend×Choice × Cohorts 1979-1981			-0.000411			0.000712
			(0.000839)			(0.000897)
Trend×Choice × Cohorts 1977-1978			0.00296			0.00473**
			(0.00193)			(0.00230)
Trend×Choice (*Pre-reform trend*)			-0.000603			-0.00137**
			(0.000439)			(0.000542)
Choice × Cohorts 1985-1987	-0.00228***	-0.00272***	-0.0101*	0.00240**	0.00104	-0.0147**
	(0.000824)	(0.00105)	(0.00599)	(0.00104)	(0.00125)	(0.00715)
Choice × Cohorts 1982-1984	-0.00287***	-0.00331***	-0.0185***	0.00141	0.000047	0.00821
	(0.000847)	(0.00106)	(0.00494)	(0.00105)	(0.00125)	(0.00576)
Choice × Cohorts 1979-1981	-0.00243***	-0.00286***	0.00471	0.00241**	0.00103	0.00507
	(0.000908)	(0.00111)	(0.00605)	(0.00108)	(0.00128)	(0.00615)
Choice × Cohorts 1977-1978	0.00003	-0.000420	-0.0144	0.00184	0.000427	-0.0198
	(0.00122)	(0.00138)	(0.0104)	(0.00144)	(0.00160)	(0.0127)
Choice	0.00318***	0.00360***	0.00466***	-0.00297***	-0.00167	-7.79e-05
	(0.000835)	(0.00104)	(0.00131)	(0.00105)	(0.00123)	(0.00151)
Placebo: Choice×Cohorts 1975-176		-0.000839			-0.00317*	
		(0.00121)			(0.00162)	
Marginal_effects						
Cohorts 1985-1987 *rel. to untreated*	-0.000560***	-0.000672***	-0.000626***	0.000262**	0.000140	0.000236**
	(0.000190)	(0.000246)	(0.000195)	(0.000118)	(0.000162)	(0.000118)
Cohorts 1982-1984 *rel. to untreated*	-0.000680***	-0.000792***	-0.000742***	0.000131	0.000008	0.000105
	(0.000195)	(0.000248)	(0.000199)	(0.000120)	(0.000163)	(0.000120)
Cohorts 1979-1981 *rel. to untreated*	-0.000577***	-0.000690***	-0.000529**	0.000252*	0.000126	0.000272**
	(0.000209)	(0.000259)	(0.000221)	(0.000129)	(0.000170)	(0.000136)
Cohorts 1977-1978 *rel. to untreated*	-0.000022	-0.000137	-0.000087	0.000201	0.000074	0.000155
	(0.000278)	(0.000319)	(0.000279)	(0.000165)	(0.000197)	(0.000166)
Untreated Cohorts † †	0.000744***	0.000852***	0.000809***	-0.000333***	-0.000218	-0.000316***
	(0.000195)	(0.000246)	(0.000199)	(0.000117)	(0.000161)	(0.000118)
Trend: Cohorts 1972-1976 (*Pre-reform trend*)			-0.000144			-0.000150**
			(0.000105)			(5.93e-05)
Placebo Cohorts		-0.000218			-0.000206	
		(0.000283)			(0.000176)	
Constant	-0.237	-0.232	-0.216	-0.844***	-0.820***	-0.803***
	(0.249)	(0.250)	(0.250)	(0.306)	(0.304)	(0.304)

Table 32 continued

Choice Measure: Number of schools within median commuting distance
Grade Level: 7-9

	Any Crime until Age 22			Health Age 22		
Specification	Treatment Windows	Placebo Test	Treatment Windows×Trend	Treatment Windows	Placebo Test	Treatment Windows×Trend
Observations	1,409,092	1,409,092	1,409,092	1,402,829	1,402,829	1,402,829
R-squared ‡	0.0382	0.0382	0.0382	0.0290	0.0290	0.0290

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1.

† † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974

‡ Pseudo R-squared for binary outcomes

Table 33: Different specifications, choice measure with radius 2km, outcomes marks grade 9 and cognitive skills

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	Percentile Rank GPA 9			Cognitive Score (Men only)		
Coefficients						
Trend×Choice × Cohorts 1988-1990			0.135**			
			(0.0566)			
Trend×Choice × Cohorts 1985-1987			0.00413			0.00432
			(0.0632)			(0.00507)
Trend×Choice × Cohorts 1982-1984			0.185***			-0.00385
			(0.0583)			(0.00513)
Trend×Choice × Cohorts 1979-1981			0.109*			-0.00615
			(0.0628)			(0.00553)
Trend×Choice × Cohorts 1977-1978			-0.0601			-0.0156
			(0.103)			(0.0100)
Trend×Choice (*Pre-reform trend*)			-0.0344			0.00121
			(0.0245)			(0.00230)
Choice×Cohorts 1988-1990	0.298***	0.264***	-1.494*			
	(0.0570)	(0.0663)	(0.877)			
Choice×Cohorts 1985-1987	0.0722	0.0387	0.421	0.0193***	0.0212***	-0.0554
	(0.0614)	(0.0691)	(0.823)	(0.00507)	(0.00587)	(0.0639)
Choice×Cohorts 1982-1984	-0.115*	-0.148**	-1.855***	0.0103**	0.0122**	0.0418
	(0.0602)	(0.0695)	(0.596)	(0.00519)	(0.00594)	(0.0508)
Choice×Cohorts 1979-1981	-0.0729	-0.106	-0.744	0.0110**	0.0129**	0.0530
	(0.0638)	(0.0726)	(0.471)	(0.00534)	(0.00613)	(0.0404)
Choice×Cohorts 1977-1978	-0.106	-0.139*	0.339	0.0138**	0.0157**	0.0955*
	(0.0653)	(0.0736)	(0.555)	(0.00615)	(0.00684)	(0.0548)
Choice	0.138***	0.170***	0.212***	-0.0122***	-0.0140***	-0.0147**
	(0.0411)	(0.0526)	(0.0659)	(0.00371)	(0.00458)	(0.00604)
Placebo: Choice×Cohorts 1975-1976		-0.0740			0.00426	
		(0.0724)			(0.00647)	
Marginal effects						
Cohorts 1988-1990 *rel. to untreated*	see coefficients	see coefficients	0.801**	see coefficients	see coefficients	
			(0.368)			
Cohorts 1985-1987 *rel. to untreated*			0.479			0.00501
			(0.298)			(0.0277)
Cohorts 1982-1984 *rel. to untreated*			0.185			-0.000495
			(0.224)			(0.0211)
Cohorts 1979-1981 *rel. to untreated*			0.124			0.00381
			(0.156)			(0.0144)
Cohorts 1977-1978 *rel. to untreated*			0.00856			0.00977
			(0.104)			(0.00979)
Untreated Cohorts † †						
Constant	22.25***	22.38***	22.24***	2.330***	2.322***	2.310***
	(5.241)	(5.245)	(5.261)	(0.397)	(0.398)	(0.398)

Table 33 continued

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	Percentile Rank GPA 9			Cognitive Score (Men only)		
Specification	Treatment Windows	Placebo Test	Treatment Windows×Trend	Treatment Windows	Placebo Test	Treatment Windows×Trend
Observations	1,715,421	1,715,421	1,715,421	610,182	610,182	610,182
R-squared‡	0.186	0.186	0.186	0.146	0.146	0.146

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1. † † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974.
‡ Pseudo R-squared for binary outcomes.

Table 34: Different specifications, choice measure with radius 2km, outcomes university degree at age 25 and employed at age 25

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	University Degree at Age 25 †			Employed Age 25		
Coefficients						
Trend×Choice×Cohorts 1982-1984			0.00399			-0.00315
			(0.00268)			(0.00237)
Trend×Choice×Cohorts 1979-1981			0.00510*			-0.00406*
			(0.00276)			(0.00238)
Trend×Choice×Cohorts 1977-1978			-0.00489			-0.00105
			(0.00518)			(0.00469)
Trend×Choice (*Pre-reform trend*)			-0.000607			0.00386***
			(0.00121)			(0.00111)
Choice×Cohorts 1982-1984	0.0149***	0.0124***	-0.0238	0.00542**	0.0102***	0.00604
	(0.00272)	(0.00317)	(0.0265)	(0.00245)	(0.00278)	(0.0234)
Choice×Cohorts 1979-1981	0.00667**	0.00416	-0.0308	0.00287	0.00766***	0.0129
	(0.00271)	(0.00314)	(0.0201)	(0.00253)	(0.00286)	(0.0171)
Choice×Cohorts 1977-1978	0.000994	-0.00151	0.0299	0.00361	0.00840***	-0.00346
	(0.00314)	(0.00351)	(0.0275)	(0.00288)	(0.00315)	(0.0254)
Choice	0.000690	0.00306	0.00202	-0.0154***	-0.0199***	-0.0235***
	(0.00192)	(0.00243)	(0.00322)	(0.00184)	(0.00223)	(0.00301)
Placebo: Choice×Cohorts 1975-176		-0.00534			0.0106***	
		(0.00346)			(0.00299)	
Marginal effects						
Cohorts 1982-1984 rel. to untreated	0.00565***	0.00473***	0.00555***	0.00229***	0.00421***	0.00253***
	(0.00103)	(0.00118)	(0.00103)	(0.000827)	(0.000968)	(0.000829)
Cohorts 1979-1981 rel. to untreated	0.00257**	0.00165	0.00243**	0.00129	0.00320***	0.00153*
	(0.00103)	(0.00118)	(0.00103)	(0.000875)	(0.00101)	(0.000876)
Cohorts 1977-1978 rel. to untreated	0.000383	-0.000542	0.000332	0.00141	0.00333***	0.00164
	(0.00119)	(0.00132)	(0.00119)	(0.00101)	(0.00112)	(0.00101)
Untreated Cohorts † †	0.000259	0.00113	0.000302	-0.00549***	-0.00732***	-0.00562***
	(0.000721)	(0.000900)	(0.000725)	(0.000656)	(0.000819)	(0.000658)
Choice×Trend: Cohorts 1972-1976 (*Pre-reform trend*)			-0.000228			0.00139***
			(0.000455)			(0.000399)
Placebo Cohorts		-0.00201			0.00418***	
		(0.00131)			(0.00105)	
Constant	-1.697***	-1.690***	-1.702***	0.483**	0.469**	0.485**
	(0.233)	(0.232)	(0.233)	(0.201)	(0.200)	(0.201)
Specification	Treatment Windows	Placebo Test	Treatment Windows ×Trend	Treatment Windows	Placebo Test	Treatment Windows ×Trend
Observations	1,120,459	1,120,459	1,120,459	1,120,845	1,120,845	1,120,845
R-squared‡	0.126	0.126	0.126	0.0301	0.0301	0.0301

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1. † † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974.‡Pseudo R-squared for binary outcomes.

Table 35: Different specifications, choice measure with radius 2km, outcomes crime until age 22 and health at age 22

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	Any Crime until Age 22			Health Age 22		
Coefficients						
Trend×Choice×Cohorts 1985-1987			0.00475*			0.00724**
			(0.00270)			(0.00336)
Trend×Choice×Cohorts 1982-1984			0.00611**			-0.00295
			(0.00286)			(0.00340)
Trend×Choice×Cohorts 1979-1981			-0.00612**			-6.64e-05
			(0.00295)			(0.00316)
Trend×Choice×Cohorts 1977-1978			0.00637			0.0183***
			(0.00576)			(0.00681)
Trend×Choice (*Pre-reform trend*)			-0.000452			-0.00438***
			(0.00124)			(0.00153)
Choice×Cohorts 1985-1987	0.00203	0.00269	-0.0592*	0.00596*	0.00362	-0.0425
	(0.00272)	(0.00314)	(0.0335)	(0.00348)	(0.00391)	(0.0426)
Choice×Cohorts 1982-1984	0.000759	0.00142	-0.0624**	0.000286	-0.00206	0.0725**
	(0.00288)	(0.00327)	(0.0284)	(0.00355)	(0.00397)	(0.0335)
Choice×Cohorts 1979-1981	-0.000667	-1.01e-05	0.0510**	0.00287	0.000532	0.0301
	(0.00291)	(0.00335)	(0.0217)	(0.00340)	(0.00384)	(0.0227)
Choice×Cohorts 1977-1978	0.00533	0.00599	-0.0282	0.00407	0.00171	-0.0815**
	(0.00346)	(0.00382)	(0.0309)	(0.00411)	(0.00447)	(0.0369)
Choice	0.00224	0.00162	0.00316	-0.00414*	-0.00191	0.00403
	(0.00199)	(0.00252)	(0.00335)	(0.00251)	(0.00306)	(0.00392)
Placebo: Choice×Cohorts 1975-176		0.00145			-0.00623	
		(0.00341)			(0.00447)	
Marginal effects						
Cohorts 1985-1987 *rel. to untreated*	0.000351	0.000498	0.000313	0.000690*	0.000460	0.000727*
	(0.000592)	(0.000704)	(0.000595)	(0.000413)	(0.000497)	(0.000413)
Cohorts 1982-1984 *rel. to untreated*	0.000097	0.000244	0.000091	-0.000034	-0.000264	0.000003
	(0.000628)	(0.000730)	(0.000627)	(0.000435)	(0.000514)	(0.000433)
Cohorts 1979-1981 *rel. to untreated*	-0.000174	-0.000027	-0.000164	0.000283	0.000051	0.000326
	(0.000658)	(0.000770)	(0.000656)	(0.000441)	(0.000522)	(0.000441)
Cohorts 1977-1978 *rel. to untreated*	0.00117	0.00132	0.00115	0.000456	0.000226	0.000426
	(0.000784)	(0.000875)	(0.000785)	(0.000475)	(0.000547)	(0.000473)
Untreated Cohorts † †	0.000524	0.000383	0.000527	-0.000465*	-0.000249	-0.000530*
	(0.000464)	(0.000596)	(0.000466)	(0.000282)	(0.000398)	(0.000281)
Choice×Trend: Cohorts 1972-1976 (*Pre-reform trend*)			-0.000106			-0.000485***
			(0.000290)			(0.000170)
Placebo Cohorts		0.000317			-0.000474	
		(0.000792)			(0.000480)	
Constant	-0.289	-0.291	-0.282	-0.806***	-0.793***	-0.774**
	(0.253)	(0.253)	(0.253)	(0.307)	(0.306)	(0.306)

Table 35 continued

Choice Measure: Number of schools within 2km
Grade Level: 7-9

	Any Crime until Age 22			Health Age 22		
Specification	Treatment Windows	Placebo Test	Treatment Windows×Trend	Treatment Windows	Placebo Test	Treatment Windows×Trend
Observations	1,409,092	1,409,092	1,409,092	1,402,829	1,402,829	1,402,829
R-squared‡	0.0382	0.0382	0.0382	0.0290	0.0290	0.0290

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 16. The definition of the placebo tests is explained in Section 3.5.1. † † This refers to cohorts 1972-1976 in all specifications except the placebo-test-specifications, where it refers to cohorts 1972-1974. ‡ Pseudo R-squared for binary outcomes.

4 Sweden's School Choice Reform and Equality of Opportunity[133]

4.1 Introduction

The school choice reform that was introduced in Sweden in the early 1990s has dramatically changed the possibilities of choosing a school within the Swedish education system. Since the reform, the possibilities for students in compulsory education to choose their school of attendance have increased dramatically. In addition to new choice options among public schools, a voucher system for private schools was introduced such that students could attend private schools without having to pay additional tuition fees. Due to this reform, the system has gone from one where students with few exceptions attended the public school of their catchment area, to one where many students opt for another school than the default school, and where there exist privately run but publicly funded alternatives alongside the traditional public schools.

In Chapter 3, we investigated the average effects of school choice as introduced by the 1992 reform, and found them to be rather modest. In particular, we found that more choice had a positive but small effect on final grades from compulsory school, and non-existent or very small effects on long-term outcomes. However, given the importance of the principle of "equivalent quality" [134] in the Swedish school system, not only the average effect on the whole population is of interest, but also whether the school choice reform has affected students of different background differently. This is also an important issue in the context of the Swedish policy debate, where the fear that children from a socio-economically disadvantaged background would be harmed in absolute or relative terms has been one of the main arguments against the reform.[135]

[133] This chapter is based on joint work with Karin Edmark and Markus Frölich.
[134] Chapter 1, §9 of the Swedish school law (Law 2010:800) states that all students shall have access to education of equivalent quality. In Swedish: "Utbildningen inom skolväsendet ska vara likvärdig inom varje skolform och inom fritidshemmet oavsett var i landet den anordnas."
[135] The National Board of Education (2003), p. 45, points to the risk of increased ethnical and social segregation as one of the most common arguments against the choice reforms in the political debate. In the appendix we show that

we do not find evidence for an overall increase in school segregation in lower secondary education in the period after the reform, compared to before the reform.

Whether school choice is "a rising tide that lifts all boats", to quote from the title of Hoxby (2003), or rather a policy that is beneficial only for a subset of students, is also a topic of interest in the international policy discussion and research literature. For example, Hastings, Kane and Staiger (2006) report positive effects of gaining access to the most preferred schools on test scores among white students and students of higher-income families in the U.S., while there are no statistically significant effects among African Americans and children of lower-income families. Hoxby (2000b) finds a similar pattern in the effects of competition between U.S. public school districts on student educational attainment: white non-Hispanics, males and those whose parents have at least a high school degree are the ones who gain from more competition, but no group seems to lose. Deming (2011), on the other hand, finds that gaining access to a first-choice school through a randomised lottery decreases the crime rates, but that the effect is concentrated among African-American male students who are defined as high risk based on ex ante characteristics. Previous studies of the Swedish school choice reform have focused exclusively on the expansion of privately run but publicly funded schools. The results of these studies (see Ahlin (2003), Sandström and Bergström (2005), Björklund, Clark, Edin, Fredriksson and Krueger (2004)) suggest that students from a better-off socio-economic background gain a bit more, but importantly, no groups seem to be negatively affected by the choice reforms. Overall, however, there are no large differences between students of different socio-economic background.[136]

To date there has been no study that evaluates the effects of the full Swedish 1992 choice reform, including the increased possibilities to choose between public schools, on outcomes of different groups of students. Our study serves to fill this gap. As the Swedish reform changed the institutional setting for the complete population and not just for certain target subgroups, it is especially suited to study the effects of school choice on different subpopulations. Moreover, given the long time since the introduction of the reform, we are able to evaluate long-run effects over and above mere short-run outcomes. We will focus our analysis on the following issues:

First, we will investigate Quantile Treatment Effects of the reform, that is whether the degree of school choice affected different parts of the distribution of outcomes differently. We will centre our analysis on distributional effects on marks at the end of 9^{th} grade. To this end, we focus on two thresholds which are of special interest when looking at marks: the probability of receiving a passing grade and the probability of receiving a high grade. Second, we will analyse whether the reform has had heterogeneous effects on student outcomes with respect to the socio-economic background, based on parents' education, income and immigrant status as well as the crime rate of the residential area.

The dataset that we use for our analysis comprises detailed administrative data for the complete Swedish population born between 1972 and 1990. As the first five of these cohorts had already left compulsory education when the reform was introduced in autumn 1992, we observe both students that have and that have not been affected by the reform.

We use very detailed geographical information about students' and schools' locations to construct measures of the potential degree of school choice that is available to each student, based on the number of schools near the students' home. Our identification strategy to deal with the potential endogeneity of choice options available to students after the reform (due to mobility of students and schools) is to measure the potential degree of school choice just *before* the reform, that is before parents and schools potentially reacted to the school choice reforms with a decision on where to live or open a school. This means that, for a student who chooses a school after the introduction of the reform, we will measure choice by counting the number of schools near her home in 1991. For cohorts that make their choice before 1992, we will use the actual year in which they choose a school, as the rules of the new school choice regime cannot have affected the place of residence of these cohorts.

Nevertheless, even for these unaffected cohorts, that is for students in a situation without free school choice, the number of schools nearby may be correlated with

[136] Böhlmark and Lindahl (2012) also find positive overall effects of the private school expansion, but do not test for heterogeneous effects with respect to student background.

student outcomes via observable and unobservable factors[137]. For this reason, we include regional- and individual-level covariates in the estimations. Moreover, we use the five student cohorts that left compulsory education before the reform was enacted to control for the effect of having many schools nearby before the reform. This allows us to net out all time constant correlation – due to both observable and, most importantly, unobservable factors – between outcomes and having many schools close-by in a situation without free school choice. The identifying assumption is thus that the cohorts that are unaffected by the reform are a good control group for later cohorts, and that the correlation between the number of schools nearby and student outcomes would have stayed constant over time if there had been no reform. We provide suggestive evidence on the validity of this assumption by testing for placebo treatment effects.

Applying this empirical strategy, we identify the differential effect of more school choice, measured at the time of the introduction of the reform, on student outcomes. Since the results are based on a pre-reform measure of school choice, the estimated effect will include all dynamic processes, like the opening or closing of schools, which are a direct result of the degree of school choice that was present at the outset of the reform[138].

As a result of students' choice options, and budgets of schools being tied to the number of students in one way or the other[139], the reform simultaneously led to choice for students and competition among schools in many areas[140]. These two concepts, as well as indicators measuring competition and choice, are naturally closely linked, as

[137] For example, it may be that areas with a higher school density have different employment opportunities which result in different educational levels in the neighbourhood and thus different schooling outcomes of children, independently of the educational quality of schools. Also, it may be that Tiebout choice moves before the reform, where parents move into catchment areas of good schools, have affected school density in the long run.
[138] In Section 4.8.1.2 in the appendix we show that the degree of school choice in 1991 and at the time when the children make an active school choice is closely related.
[139] Due to the voucher that private schools get for each student, the school budget of private schools has a direct connection to the number of students. For public schools, the way in which the budget is tied to the number of students is specified at the municipal level. The corresponding rules have varied over time and across Sweden, from systems where the idea of vouchers has also been used for public schools to systems that have specified only broadly that the number of students should be taken into account when deciding about schools' budgets.
[140] The degree to which schools compete against each other depends on several factors, such as the specific way that school finances are tied to the number of students, which is specified on the municipal level, the degree to which students actually choose other than the default schools, which is likely to be related to the amount of free capacity of school slots in an area, and other factors. Moreover, a qualitative study conducting interviews in a central area of Stockholm for example reports that some head masters of public schools have agreed to not actively compete for students from each other's catchment area schools (Waslander, Pater and van der Weide (2010)).

there would be no competition without student choice. We will not attempt to separate between these two in this chapter but focus on measuring choice on the student level. The estimated effects will thus comprise both choice and related competition effects.[141]

The results of our analysis suggest that children from a socio-economically disadvantaged or an immigrant background did not benefit less than other students from school choice. On the contrary, we sometimes find slightly larger effects for these groups, especially with respect to household income. Overall, however, the effects are rather small, as are the differences between the subgroups. As some placebo tests fail, especially for the adult outcomes, we do not overinterpret such results but focus on the more robust estimates.

The remainder of the paper is organised as follows: Section 4.2 gives an overview of the Swedish compulsory school system and the 1992 school choice reform, and Section 4.3 discusses why the effects of the reform may differ across groups of students. Section 4.4 describes the data and explores how different subgroups behaved in terms of their school choice behaviour before and after the reform. Section 4.5 explains and discusses our empirical strategy. Section 4.6 then presents the results, and Section 4.7 concludes.

4.2 Swedish compulsory school and the 1992 school choice reform

Before we turn to the empirical analysis of the paper, this section will give a short overview of the Swedish compulsory school system and the 1992 school choice reform[142]. Swedish compulsory schooling comprises grades 1–9, with students starting grade one the year they turn seven.[143] Since elementary school (grades 1–6) and lower secondary school (grades 7–9) are often organised in different schools, it is common to change the school when starting grade 7, at the age of 13. Following previous studies on the Swedish school choice reform, we will focus on grades 7-9. After compulsory school, which has a comprehensive curriculum with some choice options like studying a

[141] In Chapter 3, we attempted to disentangle the choice effect, i.e. the individual matching effect, from the competition effect. While our estimates gave some indications of positive choice effects and negative competition effects especially shortly after the reform, the close relation between the two indicators of choice opportunities faced by students and competition from other schools faced by schools made it difficult to empirically separate estimates of the two effects.

[142] See Chapter 3 for a more detailed description.

[143] From the year 1997 on, the vast majority of children also attend a voluntary 1-year school preparatory year, which is usually offered at the compulsory school.

second language, most students go on to upper secondary school, which is voluntary and is organised in several educational tracks.

Since 1990, the municipalities are the responsible administrative entities for organising compulsory education. The main sources of finance are the local income taxes and central government grants.[144] The central government, however, steers compulsory schooling through providing rules and regulation.

Following the election of a right-wing coalition in the fall of 1991, the large compulsory school choice reform that is studied in this paper was implemented in autumn 1992. The reform had two parts: first it opened up for attending another public school than the one in the catchment area, and second, it allowed for privately run but publicly funded schools to operate alongside the ordinary public schools. In 1994, the law was amended by also allowing for choice among public schools outside of the home municipality, which was previously only possible in very special cases.[145] If the demand for a given public school exceeded the number of available slots, priority was given to students living in the catchment area. Private schools were not allowed to select their students on the basis of ability or other characteristics but only on a first-come-first-served basis.

The reform has had substantial effects on the workings of the educational sector, at least in more urban areas. Before the reform, students were, with few exceptions, referred to the school of their catchment area. Some alternative schools existed, such as Waldorf schools or schools with a special profile like music, but they were rare. After the 1992 reform, as more and more private schools were established and as choice between the already existing schools became more and more common, this gradually changed, and now, 20 years after the reform, school choice is a normal phenomenon in many parts of the country. According to the National Board of Education[146], almost 13

[144] The central government grants have been completely general since 1993, i.e. not tied to specific sectors, and they are set so as to compensate for differences in tax base as well as in structural costs, in order to ensure that all municipalities have roughly equal economic conditions. Between 1991and 1993, a sector specific grants system was in place, and before that, when the central government was responsible for the provision of education, central government grants were classified for different purposes. The largest among these, the “basic resource”, consisted mainly of teacher salaries (see pp. 67f von Greiff (2009)).
[145] See Law 1985:100 Chapter 4 §8a.
[146] See information at the webpage of the National Board of Education: http://www.skolverket.se/statistik-och-analys/2.1862/2.4290/2.4292.

per cent of all students in compulsory school attended an independent school in the school year 2011/12. For the public schools, there is no comprehensive information available on how common it is to choose another school than the catchment area school, but survey information from school year 2000/01 suggests that choosing another public school is at least as common as choosing a private school (The National Board of Education (2003)).

For the sake of the empirical analysis, it is worth to point out that the expansion of choice both in terms of private schools, and in terms of choice between the public schools, has been gradual: in the mid 1990s, a couple of years after the reform, choosing another school than the default school was still rare (see the National Board of Education (1996)). This means that we expect the choice reform to have more and more of an impact over time, something that we will take into account in the empirical analysis.

4.3 Why may effects differ across groups of students?

This section will discuss theoretical arguments for why there might be heterogeneous effects for children with different socio-economic or migration backgrounds. We choose to focus especially on groups that may be considered more vulnerable or disadvantaged since the effects of school choice policies on these groups are often of particular interest in the public debate. In particular, we will focus on students with low-educated parents, defined as both parents having at most a compulsory education degree, students living in a low income household, defined as disposable household income being in the lowest quartile of the income distribution, students with both parents having been born outside of Sweden, and students living in high-crime areas, defined as living in a municipality in the upper decile of the municipality crime distribution in 1991, and their respective counterparts.

Before we turn to why the effects of school choice would be expected to differ across groups of students, we briefly outline the channels through which free school choice might affect educational outcomes in general.[147] First, being able to choose a school that suits one’s preferences and character may result in a better match between students and schools, which would improve learning among those who actively make a choice. Second, more choice for students, and schools budgets being tied in some way to the number of students, may introduce competitive pressure and lead head masters and teachers to increase teaching quality in order to attract students to their school[148]. This may lead to good schools attracting more students, and bad schools either improving or having to close down. Thereby, the overall quality of the education system may increase in the long-run, which would then be beneficial also for students who do not make an active school choice. Third, when students are free to attend another school than the one of the catchment area, the composition of students within a school may change, which results in different peer effects[149].

However, to what extent these channels work in reality is not clear, as they are related to a number of issues. One of them is the informational asymmetry between parents and schools, as the former may not always be able to observe educational quality or base their choices solely on this. Moreover, transportation costs to different schools and capacity limits of schools may decrease the forces of the above explained channels. Also, parents with different characteristics may react differently to the choice reform, both in their propensity to make an active school choice and the characteristics on which they base their choice. As a result, children with different background may be affected differently by the choice reform. In the following, we will discuss potential reasons for such differences for the subgroups that we analyse in this study.

We organise our thoughts on this matter by asking: how do we expect that students reacted to and were affected by the expanding possibilities to choose school after the Swedish choice reform of 1992?

First of all, we expect that some students reacted by choosing another school than the default school. Some may have chosen to attend another public school than the one of their catchment area, while others may have chosen a private school.[150] Survey information from the National Board of Education (2003) suggests that making an

[147] See Chapter 3 for a more detailed discussion.
[148] See for example Hoxby (2003) on the relation between school choice and school productivity.
[149] See for example Epple and Romano (1998) on this issue, who model peer effects of sorting as a results of school choice.

active school choice (in Sweden in school year 2000/01) was more common among students whose parents had higher education or were immigrants. One can also speculate that the possibility of choosing another than the closest school might be more interesting for students of low-income background, as these may be financially restricted from getting into a good school by moving near it, i.e. from exerting Tiebout choice. Students from high-income families, on the other hand, have always had better economic means to move near the desired school, and might thus not have been as restricted in their school choice by the assignment system that was in place before the reform. A related hypothesis is that students living in more disadvantaged areas may be more likely to choose another school than the neighbourhood school, for example to get access to a school with less social problems. Students of different socio-economic or immigrant background, or students living in areas with more or less social problems, may hence differ in the likelihood of choosing another school than the default school.

Second, those who make use of the option to attend another school than the default one, will naturally be subject to another school environment, including other teachers and peers, than would otherwise have been the case.[151] How the new school differs from the old one in turn depends on the factors that determined the choice of school. Burgess et al. (2009) show that families in Britain do not only value academic performance when they choose schools, but also other factors such as the student composition and travel distance. The results of Hastings et al. (2006), who study U.S. families, furthermore suggest that getting access to the most desired school has positive effects on student outcomes only for those who named academic quality as an important choice factor. In addition, Hastings and Weinstein (2008) find that the likelihood of choosing a high-performing school was increased when low-income families were given information about school test scores. This suggests that, at least in the U.S., parents from low- and middle-income families did not have sufficient information on the quality of the school, since providing such information changed their choice of school

[150] See for example Nechyba (2006) for an overview of the literature on the mechanisms of sorting of students with respect to income and peer quality.

[151] See Sacerdote (2011) for a recent survey of the empirical literature on peer effects.

towards educationally better schools. Hastings and Weinstein (2008) also find a positive effect on student test scores of attending a higher-scoring school.

The results of these studies illustrate that, in order to benefit from the option to choose a school, it is important to have and use information about factors that actually are important for students' school results, such as academic quality. This means that even though all students may make an active school choice, the factors influencing this choice may be very different, which may in turn lead to heterogeneous effects of school choice. For the Swedish case, Böhlmark and Lindahl (2007) provide some evidence that parents with higher education and those born in another country were more likely to send their children to a private school, while they find no such difference with regard to parental income. This might in turn lead to different effects for the corresponding subgroups if attending a private school is on average more or less beneficial than attending a public school[152].

Third, not only the students who make an active school choice may be affected by increased choice possibilities, but also the students who remain in the default school. That is, they may be affected by the other students' choices if the characteristics of the peer group change and, in relative terms, by possibly staying at a not so good school that other students opted to leave. In their study, Östh, Andersson and Malmberg (2010) suggest that school choice in Sweden has led to increased between-school dispersion in 9th grade marks, on top of the dispersion that stems from residential segregation. Böhlmark and Lindahl (2007), who also study the 1992 choice reforms but focus on the introduction of private schools, find that a higher share of private school students within a municipality is related to higher segregation in terms of parental education and immigrant status between public and private schools. However, comparing schools offering grades 7-9 in the years before and after the reform, we do not find any indication for an overall increase in segregation, measured in terms of the between-school variation in the share of students with disadvantaged socio-economic background or immigrant background (see Section 4.8.1.1 in the appendix).

[152] Böhlmark and Lindahl (2007) find some evidence for a beneficial effect of attending a private school, though they also show that most of their estimated positive effects of higher private school shares stems from the competition effects that affect all pupils, not just those in private schools.

In sum, given that previous studies have indicated systematic differences in the way that students of different background react to reforms that expand school choice, and given the many channels through which school choice may affect student outcomes, it is important to test empirically whether the effect of the Swedish 1992 school choice reform differs across groups of students, and in particular, whether some groups were harmed by school choice as it evolved after the reform.

4.4 Data and descriptive statistics

Before we turn to the econometric analysis, we will check, in this section, if there is any indication in our data that students of different background reacted differently to the choice reform. To this end, we look at descriptive statistics for student outcomes as well as at indicators of actual school choices made, namely travel distance to school and attending a private versus public school. First however, the subsection below gives a short overview of the data sources.

4.4.1 Data

The analyses in this chapter are mostly based on the same data set as the ones in Chapter 3. The following section will thus briefly summarize the more detailed information presented in Section 3.4. We use data from Statistics Sweden, the Swedish National Council for Crime Prevention, the Military Archives and the Swedish Defence Recruitment Agency. The data set contains information on final grades from compulsory school for all individuals in Sweden born in 1972–1990, and on the longer term outcomes “criminal convictions by age 22”, “university education at age 25” and “employment at age 25” for those who had achieved the corresponding age by 2009. For men, we also observe the cognitive score from the military draft test[153][154]. The data furthermore includes a broad set of individual level background variables, including detailed parental background information on education and income level, country of birth and family structure. In addition, we have access to geographical information on

[153] See also Lindqvist and Vestman (2011) for a detailed description of this test. Note that the share of men taking the military test drops significantly for the younger cohorts. See Chapter 3 for an analysis showing that the selection effects are only mildly related to our choice measure and outcomes, on average.

the location of schools (for years 1988–2006) and students' residences (for years 1985–2006), measured as 100*100 square meter boxes. This data allows us to construct detailed measures of the choice options available to each student.

Moreover, we have information on a set of municipality level characteristics like the population density and income tax base which we collected from the webpage of Statistics Sweden (www.scb.se) and from the webpage of the Swedish Association of Local Authorities and Regions (www.skl.se). On a finer regional level, we constructed a set of parish level characteristics from individual register data that we were generously given access to by the Institute for Labour Market Policy Evaluation (IFAU), including population density, education and income level and immigrant share. A full list of these variables, used as covariates in the estimations, is given below Table 40. Table 15 displays the corresponding descriptive statistics.

4.4.2 Descriptive statistics

Table 36 shows average student outcomes separately for the pre- and post-reform cohorts, that is for cohorts born between 1972 and 1976, and between 1977 and 1990, respectively, and separately for the different subgroups[155]. We can see that the higher the household income and parental education, the better is the average value of most outcomes, that is of the percentile rank in marks, cognitive skills, the share receiving a passing or a high grade in math or holding a university degree at age 25, and having been convicted for a crime until age 22. A similar pattern holds when comparing children whose parents have both been born abroad with those who have at least one native Swedish parent. Comparing these numbers pre- and post-reform, the most remarkable changes are the increase in the percentage receiving a passing grade in all subgroups[156] and the increase in the share of those having obtained a university degree

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[154] The cognitive score and information on whether the individual has been convicted for a crime are available only for cohorts born in 1972 to 1987, while the information on university education and employment, measured at age 25, is only available for individuals born between 1972 and 1984.

[155] The corresponding standard deviation and number of observations are reported in tables in Section 4.8.2.3.1.

[156] This is in line with Vlachos (2010) who finds that the final average grade point averages from Swedish lower as well as upper secondary school increased between 1998 and 2008. Vlachos' analysis contributes only a small share of this increase to competition effects, and suggests that a large share can rather be attributed to other factors such as the introduction of a new grading system in 1997, based on absolute knowledge goals, instead of the previous more relative grading system. In Chapter 3, we find that there is, on average, a modest increase in the percentile rank of 9th

at age 25. Both of these changes are more pronounced among children with high compared to low-educated parents and immigrants as compared to native Swedes. The change in obtaining a university degree at age 25 is with 10 percentage points twice as large for children of high-income as compared to low-income families. Apart from this, students from middle and high income households have similar or improved outcomes in all dimensions except for the share of those being employed at age 25, while students from low-income families have similar outcomes and a slight decrease in the probability of receiving a high grade in math at the end of 9^{th} grade. Children from parents who both have at most a compulsory education experience a decrease in the percentile rank in marks by 2 percentage points and an increase in the share having committed a crime until age 22 by 1.5 percentage points.

Lastly, we split the sample according to the municipal crime rate in 1991. We think that this is an interesting additional characteristic since, as commented in Section 4.3, school choice gives students of areas with social problems the possibility of leaving their neighbourhood for the time they are at school and to get in touch with other peers. If, for some reason, their families were stuck in a neighbourhood with high crime and potentially bad influences while growing up, being given the opportunity of going to a school outside of this neighbourhood might be especially beneficial.

In order to analyse empirically whether this is the case, we split the sample according to whether the student's home municipality had a crime rate in the upper 10th percentile in the distribution of all municipal crime rates in 1991, or not. We use the crime rate among 16-19-year-olds, as this is likely to be more important in terms of influences on adolescents than the adult crime rate.

Table 37 displays which kinds of crimes are most commonly committed in high vs. low or medium crime areas in 1991. Listing only those crimes that make up more than 3 per cent of all crimes, we can see that the composition of crimes in low/medium and high-crime areas is very similar, so it is mostly the quantity that varies: the average crime rate in the high crime municipalities is 5.6 per cent, while it is only 3.5 per cent in the low and medium level crime areas.

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grade GPA as a result of having more school choice. We further present suggestive evidence that this is not explained

The last rows in Table 36 display that there are no strong differences in outcomes of students living in the different areas, except that the share of those having committed a crime until age 22 is higher in high crime areas and that the share holding a university degree at age 25 and those receiving a passing or a high grade is slightly lower for pre-reform cohorts in these areas. However, after the reform, students in high crime areas perform slightly better in terms of marks compared to students in low or medium crime areas.

These descriptive comparisons of subpopulations and cohorts that have been affected and not affected by the reform show that children from low-income and less educated households experienced, for some outcomes, a small relative drop after the reform compared to more advantaged students. Before we turn to an econometric assessment of whether these differences are related to the school choice reform, we first investigate in the next section whether there is any indication in the data that students from different subgroups changed their school choice behaviour in different ways after the reform.

4.4.3 Indicators on how choice behaviour changed after the reform

In this section we investigate whether students of a different socio-economic or immigrant background reacted differently to the choice reform in terms of making an active school choice. As we lack information on whether students choose to attend another public school than the assigned one, we instead make use of indirect information in terms of distance to school of attendance and whether students attend a private or public school, to get an idea of how the choice reform affected school choice-related behaviour.

The travel distance to school can be seen as an approximate indicator of school choice in general – to public as well as private schools, since students that opt out of the school of their catchment area are likely to increase their travel distance, as the catchment area school is in general the nearest one. With new schools opening up and old ones possibly closing down, an increased travel distance is not a perfect measure of choosing another than the default school but only an approximation. Moreover, any changes in travel distance over time may of course be related to other factors and

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by grade inflation only.

general trends too. On the contrary, attending a private school is clear evidence for active school choice, as opting out of the public school system requires parents to act.

Columns 1-4 in Table 38 show the mean of the travel distance to school for the different subgroups, separately for the pre- and post-reform cohorts.[157] The first two columns show the unconditional mean while the last two columns show the mean conditional on all covariates included in the estimation[158], i.e. net of all differences in covariates. The numbers for the cohorts not affected by the reform indicate that the unconditional mean travel distance to school was larger among low-income than among mid- and high-income households, and was larger among households with Swedish-born parents or households living in high-crime areas. However, conditioning on covariates (Columns 3 and 4) almost completely eliminates these differences except for households with different incomes. When we compare the conditional pre- and post-reform means, we see that the distance to school increases over time for all groups, but the increase is largest, both for the unconditional and the conditional means, for low-income households. We furthermore see that the conditional travel distance increases a bit more between the pre- and post-reform cohorts if both parents are Swedish-born parents or if the child lives in a high crime area.

Columns 5–6 in Table 38 show the unconditional and conditional share of students attending a private school in 9th grade. Here, we only report the shares for post-reform cohorts, as it was extremely uncommon to attend a private school before the reform. The unconditional means in the fifth column of Table 38 show that children of immigrant, higher-education and higher-income background, as well as children from high crime areas, are more likely to attend a private school. When conditioning on all covariates that we use in the main estimations (see note to Table 40 for a list), the differences with respect to household income are negligible, while the qualitative results for the other subgroups stay the same.

[157] Note that we measure travel distance "as the crow flies", i.e. by computing the distance between the mid points of the coordinate for the students home and the students' school of attendance in 9th grade.
[158] This is calculated using coefficient estimates from an OLS regression with distance to school as the outcome and all covariates, an indicator for „affected by the reform", an indicator for the subgroup and an interaction of the two included as right hand side variables.

In sum, the descriptive statistics suggest that travel distances have increased for all groups after the choice reform, but the increase is larger for students whose parents have lower income, who live in high crime municipalities or have Swedish-born parents, which in turn could suggest that choosing another school than the catchment area school after the reform was more common among these groups of students. Of course, when interpreting these purely descriptive statistics it has to be kept in mind that other factors like trends in living in different residential areas and not school choice itself may be behind these results. Our data on private school attendance furthermore shows that private school attendance was more common among students of immigrant background, students in high-crime areas or students with high-educated parents.

As outlined in Section 4.3, there are several channels through which the reform may affect both those students who make active choices and those who do not; however, a reasonable hypothesis is that the former group will be more affected. Different choice patterns between groups of students could therefore lead us to think that the reform effect may differ across groups. Böhlmark and Lindahl (2012), who study the expansion of private school attendance, present evidence that one advantage of competition by private schools is an increase in the outcomes of students attending private schools, although they show that most of the benefits affect all students, that is also those attending a public school.

With these patterns in mind, we turn to the main empirical analysis of the study.

4.5 Empirical strategy

4.5.1 Identification

We follow the identification strategy used in Chapter 3, where we identify the average effects of the reform. Identifying the effect of more school choice as introduced by the reform in 1992 mainly faces two empirical challenges.

The first is the endogenous choice of residence and location of families and schools following the choice reform. After the reform was introduced, many new private schools opened up, and it is highly plausible that neither the for-profit nor the non-for-profit private schools chose their location of business at random. Both cream-skimming arguments as well as motives to help especially disadvantaged children might have influenced the decision where to open a new school. At the same time, if more choice and competition leads to an improved quality of education, parents that are very concerned about their children's education will try to move close to such competitive areas in order to have a higher likelihood to get into one of these schools and in order to face short travel distances.

Not taking these arguments into account in the estimation might lead to either a positive or a negative bias of the effect of having more schools nearby, depending on which of the mechanisms is more important empirically. We solve this issue by using the location of families' residences and the location of schools in 1991, that is right before the reform, to calculate our choice measure for those students who choose a school after 1992, i.e. those who are affected by the reform. Since the reform came as a surprise, in the sense that it was introduced by the new governing coalition that won the tight 1991 parliamentary election[159], we can take the *pre-reform* locations to be exogenous to the reform. To illustrate this approach, take a student born in the year 1983 who, correspondingly, chose a school to start seventh grade in 1996. As this was after the reform, the number of schools around the students' home could be related to her underlying ability, due to the endogenous location of both schools and students after the choice reform. As discussed above, in order to avoid this, we count the number of schools close to the students' residential location in the year 1991. For a student born in 1973, who started seventh grade a decade earlier and left compulsory schooling in 1989, the number of schools close-by cannot have been related to her underlying ability via free school choice as this did not yet exist[160]. Hence, without risking an endogeneity bias caused by reactions to the reform, we count the number of schools around the students' home in 1986, the year in which the student actually chooses a school.

An additional advantage of using measures that were predetermined is that we have a natural starting point from where dynamic competition effects started to evolve. To

[159] The right wing coalition (Moderaterna; Folkpartiet; Centerpartiet; and Kristdemokraterna) obtained 46.6% of votes, and the socialist block (The Social Democrats and the Left Party (Vänsterpartiet)) 42.2%. New Democracy, which has since then disappeared from politics, obtained 6.7% of the votes, and The greens, Miljöpartiet, received 3.7% of the votes and were hence only 0.3% from parliamentary representation.

[160] It may have been related to ability because of other factors, like Tiebout migration or the correlation between average educational level and density of schools in an area. This is what we refer to as the second challenge to identification and will be described later on in this section.

illuminate this, suppose a child lives close to very many schools right before the introduction of the reform. Once the new rules are in place, the schools start competing for students, new schools may open up and old, bad schools may close down. If the competitive process is strong enough, we might see more and more schools closing down and the best one attracting more and more students. Some years later, we would then see a rather monopsonistic situation, with few schools, but possibly very good outcomes, if only the best schools have sustained in the competition. Relating the number of schools to student outcomes at that later time would then show no, weak, or even a negative relationship between choice opportunities and student performance. It is thus difficult to compare contemporaneous choice measures to student outcomes when it is not clear at which stage of a dynamic process this is observed. Using predetermined measures of school choice as they are observed at the start of the competitive process, in contrast, will incorporate the dynamic changes, like opening or closing of schools, that are a direct result of the initial choice setting. Needless to say, the pre-reform situation will not remain a relevant measure forever – eventually other changes will take place so that the pre-reform situation does no longer measure the relevant conditions forming choice and competition. However, we believe that the 12-year period that we study constitutes a reasonable time frame for this type of analysis. Moreover, we observe in the data that there is a fairly close correlation between the choice index as measured just before the reform and the one measured at the time the individuals make their decision among all subgroups (see Section 4.8.1.2 in the appendix).

The second challenge to identification is that having more schools nearby to choose from will be correlated with several other factors that might be related to student outcomes, such as living in a more urban neighbourhood, populated, for example, by people with different education backgrounds than people living in rural areas. Even though we observe a broad set of individual, municipality level and parish level characteristics, it is hard to argue that every possible confounding factor is captured by these variables. Therefore, in addition to controlling for these variables in our estimation, we will also control for the effect that having many schools close-by has had before the reform. We achieve this by including cohorts that are not affected by the reform in our analysis and estimating only the differential effect of choice for affected as compared to non-affected cohorts. We will thus net out any potential effect, or spurious correlation, that is related to having many schools in the neighbourhood in a situation where parents cannot choose the school they send their child to. Consequently, our analysis will capture the *additional* effect of being able to choose more freely among schools, as it was introduced by the Swedish school choice reform.

Our identifying assumption is thus that, if the reform had not been implemented, the relationship between our choice measure and students' outcomes would have been the same as it was in the years before the reform. Even though this assumption cannot be tested empirically, we can test its credibility by performing placebo estimates. To this end, we artificially change the date of the reform to having been enacted two years earlier and test whether we find any treatment effect of this non-existent reform. If that is the case, it shows us that the relation between our choice measure and student outcomes, given our covariates, has already changed before the reform, making an identification of the reform effect difficult.

4.5.2 Measuring the degree of choice

As the analyses in this chapter will use the same measure for school choice as was used for the analyses in Chapter 3, the following section is based on Section 3.5.2.

In order to be able to benefit from the introduction of school choice, it is essential for students to have access to schools close to their home. We thus measure school choice by counting the number of schools that students can potentially choose from[161] within the proximity of their homes, using the median commuting distance of the home municipality in 1991[162] as radius and, alternatively, a radius of 2km[163]. The median value of the median commuting distances is about 5km. Using the commuting distance of the home municipality in 1991 as radius around students' homes has the advantage of flexibly taking into account the large geographical diversity of Sweden. Nevertheless, we also use a radius of 2km around a student's home to examine the robustness of the

[161] See Section 4.2 for more detailed information on which schools a student could choose from.
[162] We are grateful to John Östh for providing information on municipality commuting distances, which are measured "as the crow flies", and do not take into account the directions of roads and the like."

results. It shall be noted that these two measures will have a different bite in measuring the number of available schools in different regions: while there are often no schools within 2km in rural areas, and this measure therefore does not capture much of the variation in the number of accessible schools there, there will be very many schools in this radius in the big cities such as Stockholm.

In line with previous studies on the Swedish choice reform, we focus on analysing choice opportunities for children when they start 7th grade. This is an important stage of compulsory education as grades at the end of 9th grade are important for admission into upper secondary school. Thus, this is a point in time when parents are likely to be interested in choosing a good school. It is also a time when making a school choice is likely to be relevant, since it marks the start of lower secondary school, which is often organised in a separate school from lower education. When calculating our choice measure, we thus use the location of residence of students when they are 13 years old and count the number of schools that offer grades 7-9 close to their home[164]. As explained in the last section, for students born in cohorts 1979-1990, that is those who chose a school for grades 7-9 after the 1992 reform, we use the place of residence in 1991 and the schools that were present at that time in order to calculate the *pre-reform* choice measures. Moreover, as we only have geographical information on schools starting from year 1988, we use the 1988 location of schools also for students who started grade 7 before that.

Table 39 shows the mean and standard deviation for the pre-reform choice measures which count the number of schools within the median commuting distance, and within 2km, separately for the different subgroups and for pre- and post-reform cohorts. The number of schools within the median commuting and 2km radius is similar for students in the lowest and highest income quartile, but is smaller for those living in households with an income between the 25th and 75th percentile of the distribution. For the post-reform cohorts, the choice measures are somewhat larger for the highest income households, also when compared to those with the lowest income. Note though, that this

[163] See also Gibbons, Machin and Silva (2008), Himmler (2009) and Noailly, Vujic and Aouragh (2009) for other studies using the distance between a student's home and schools.

does not show an increase in the number of schools, since the value is measured in 1991, but rather a possible change in residence patterns already before 1991, or the consequences of the law change in 1994 that opened up for choice to public schools in other municipalities[165]. Dividing the sample along the educational background of the parents, we see that low educated households have slightly less schools within the municipalities' median commuting distance around their home, but very similar numbers when counting schools within 2km around the home. Furthermore, children with parents that were both born outside of Sweden have more schools nearby on average than children with at least one Swedish-born parent. Lastly, when dividing the sample according to the municipal crime rate in 1991, we can see that students in pre- as well as post-reform cohorts living in high crime areas in 1991 had more schools nearby on average.

4.5.3 Estimation

The estimation strategy used in this chapter follows the one applied in Chapter 3. In order to investigate whether students of different background were differently affected by the 1991 choice reform, we run regressions separately for the different subpopulations. Moreover, to estimate the differential effect of school choice and how it evolves over time for cohorts affected by the reform, as compared to the effect of having many schools nearby for unaffected cohorts, we pool all cohorts and define the following treatment window dummy variables:

$$(17) \qquad \begin{pmatrix} D_i^1 = 1 \; \textit{if born in } 1977 \textit{ or } 1978; \textit{ zero otherwise} \\ D_i^2 = 1 \; \textit{if born in } 1979 \textit{ or } 1980 \textit{ or } 1981; \textit{ zero otherwise} \\ D_i^3 = 1 \; \textit{if born in } 1982 \textit{ or } 1983 \textit{ or } 1984; \textit{ zero otherwise} \\ D_i^4 = 1 \; \textit{if born in } 1985 \textit{ or } 1986 \textit{ or } 1987; \textit{ zero otherwise} \\ D_i^5 = 1 \; \textit{if born in } 1988 \textit{ or } 1989 \textit{ or } 1990; \textit{ zero otherwise} \end{pmatrix}$$

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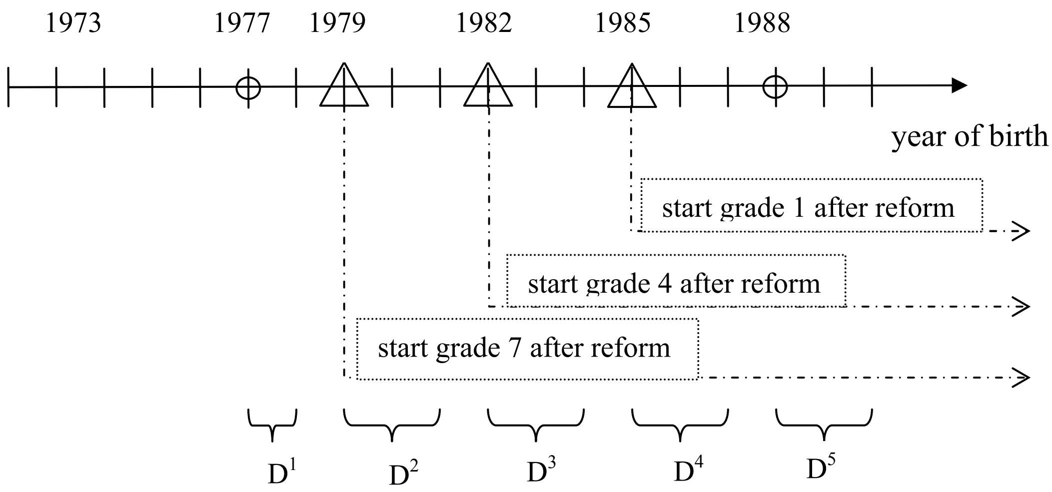
[164] Measures calculated for the choice options at first and fourth grade are highly correlated with the choice measure for grades 7-9.

[165] See Law 1985:100, Chapter 4 §8a.

For the pre-reform cohorts, all these treatment dummies are zero. The choice of these windows follows the degree to which students born in the different cohorts were potentially affected by the reform (see also Figure 10): Those born in 1977 started 9th grade in 1992 and could in theory be affected by the choice-reform either through increased competitive pressure on schools, or through the option of switching school, during their last year of compulsory schooling. Although we would not expect any large effects after such a short time period, we allocate them into a separate group as they are not a clear control group. Cohorts 1979-1981 started 7th grade in or after 1992, when the choice reform was in place, and could hence in principle choose the school they wanted to attend for the final stage in compulsory education. The next treatment window dummy, D_i^3, captures all cohorts that were affected by the reform, and could hence in principle make a school choice already for classes 4-6 and 7-9. Finally, for cohorts included in treatment windows D_i^4 and D_i^5, the choice reform was in place throughout their educational career, meaning that they could benefit from more choice in general, but also that the reform had already been in place some years when they entered grade 7, and thus, that competition had already had time to evolve.

Figure 10: Treated cohorts



By interacting these dummies with our choice measure, the coefficient corresponding to each "*treatment window · choice*" interaction term will measure the differential effect of having many schools nearby after the reform, for students in the respective windows. We thus estimate the following equation, separately for each subpopulation of interest:

(18) $Y_i = \beta_1 D_i^1 c_i + \beta_2 D_i^2 c_i + \beta_3 D_i^3 c_i + \beta_4 D_i^4 c_i + \beta_5 D_i^5 c_i + \alpha \cdot c_i + \gamma_{cohort} + \lambda_{municipality} + \delta \cdot X_i + u_i$

where γ_{cohort} and $\lambda_{municipality}$ are cohort and municipality fixed-effects and X is a vector of covariates including a wide range of individual, municipal and parish level characteristics (a full list is given below Table 40). We use OLS for continuous outcomes and Probit for binary outcomes, and cluster standard errors at the school-cohort level.[166]

The β-coefficients measure the differential impact of having many schools nearby for cohorts in the respective treatment windows, compared to cohorts that were unaffected by the reform. Thus, they measure the effect of school choice as introduced by the reform. On the other hand, the coefficient α captures any relation between living

[166] We cannot link schools over time in our dataset; therefore, we cluster standard errors on the school level within each cohort.

near many schools and the outcome variable that existed already before the reform. By including c_i, we therefore control for the correlation between our choice-measure and the outcomes of the pre-reform cohorts.[167]

As we use students' and schools' locations from 1991 for cohorts 1979-1990, we will also measure all municipal and parish- level covariates in 1991. For cohorts 1972-1978, we use the information from the year in which they start 7th grade or, if this is not available due to data limitations, from the closest available date.

4.6 Results

In Chapter 3, we found only small effects of more school choice as introduced by the 1992 choice reform on the average percentile rank in marks. In this section, we will test for whether the small average effects mask heterogeneous effects; first with respect to the distribution of marks, and then with respect to student background.

4.6.1 Effects on the distribution of marks

We start by analysing if the school choice reform affected the distribution of marks, more specifically, whether the effects differed at the important thresholds "receiving a passing grade" and "receiving a high grade".[168] For this analysis, we will focus on marks in mathematics at the end of 9th grade, as we think that this subject is more suited for a comparison over time and between immigrants and Swedes than English and Swedish would be.

Table 40 displays the marginal effects of an additional school nearby in 1991 on the probability of receiving a passing or high grade. The first two columns show results using the radius median commuting distance while the third and fourth display those using a 2km radius around a students' home to count the number of schools. We can see that there is no effect on the probability of receiving a passing grade when using the

[167] The estimate of α potentially includes effects of Tiebout school choice, or yardstick-type effects, due to it being easier to make comparisons of school performance, and hence put pressure on the own school to improve, if there are many schools around. Note, however, that we do not assume a causal interpretation of α.

[168] The other outcomes that we have analysed in Chapter 3 are binary variables and, as such, are not interesting for a distributional analysis. The only exception to this is the cognitive score in the military test, which, however, only takes 9 values, making it less suitable for a distributional analysis. Moreover, it does not have such clear thresholds of interest as do grades.

median commuting distance. However, this result is not robust to using a radius of 2km. At the same time, we see an increase in the probability of achieving a high grade in math by around 0.3 percentage points per additional school within the median commuting distance around a students' home. A qualitatively similar pattern is found using the 2km radius. However, when performing a placebo test pretending the reform had happened two years earlier, we find a negative effect of the placebo-reform that is statistically significant at the 90 per cent confidence level, indicating that this result should not be overinterpreted as the identification is weak. Overall, we thus find some suggestive, though somewhat unstable, evidence that the distribution of marks spread out a little in response to the reform. We will further investigate these distributional effects in the next section, where we analyse whether students from a different social background were differently affected by the choice reform.

4.6.2 Are students from a socio-economically disadvantaged or migration background harmed by the reform?

Heterogeneity with respect to parental household income

For the reasons explained in Section 4.3, theoretically it might be that children from low-income families benefit more – or less – than children from high-income families from the school choice reform.

The first three columns of Table 41 display the effect on the percentile rank in 9^{th} grade marks, estimated separately for low-, medium- and high-income households. In all household groups, the general pattern is that effects are first negative, even though mostly not statistically significantly different from zero, and then increase over time. This is in accordance with the results for the average effects in Chapter 3, as well as with the hypothesis that competitive pressure and realising choice options took some time to fully come into effect. The point estimates for the youngest cohorts are slightly larger for students from families with a lower household income. One additional school in the median commuting distance raises the percentile rank in 9^{th} grade marks by 0.2 points for students from the lowest income households, while the corresponding figure for medium and high income households is 0.13 and 0.1 respectively. The differences between the lowest and the two other income groups are mostly statistically significant[169]. As the standard deviation of the percentile rank is around 28, these effects are rather small, as are the differences between the groups in absolute terms. When multiplying the effect by one standard deviation in the choice measure, which is 9.2 for the affected cohorts in the lowest income group, this implies an increase in the percentile rank by roughly 1.8 points. This is similar to the effect of an increase in the municipal private school share by 10%, found by Böhlmark and Lindahl (2012). However, the average number of schools within median commuting distance around the home for students affected by the reform and in the lowest income group is 6.1, an increase by 9 schools would thus be very large.

The effect of more school choice on cognitive skills (see Columns 4-6 in Table 41) is similar for the low and medium income households, but with an increase of around 0.005 points for each additional school, and the cognitive score varying between 0 and 9, it is very small. Children from high income households display an equally small, but negative effect. Looking at the distribution of 9th grade math marks, we find no effect for any income group on the probability of receiving a passing grade, and similar effects of an around 0.3 percentage points increase in the likelihood of achieving a high grade per additional school in the commuting distance for all three groups (see Table 42). However, the identification of the result is weak for the high income group as the placebo test fails. Concerning the probability of having committed a crime until age 22, we almost only find significant effects in the lowest income group, where an increase in choice leads to a small reduction of about 0.1 percentage points in the probability (see Table 43)[170].

Overall, the effects are small as well as the differences between the subgroups. We thus find no evidence that would support the claim that disadvantaged children had been harmed by the reform. On the contrary, we find slightly higher point estimates for low-income households, suggesting that low income households benefited more from the potential choice options, although this difference is very small.

[169] We test statistical significance between point estimates from separate regressions by running a fully interacted estimation of the model; results are available from the others upon request.

Heterogeneity with respect to educational background of parents

Next, we explore whether children whose parents have a lower education were differently affected by the choice reform compared to children with higher educated parents. Table 44 and Table 45 show that there is no indication that children with low educated parents, defined as both parents having no more than compulsory education, have benefited less from school choice in terms of grades at the end of 9^{th} grade. On the contrary, most point estimates are even slightly larger for children from households with a lower education. Concerning the cognitive score in the military draft, one school more increases the score by 0.015 for the youngest cohorts of students with low educated parents (see Column 4 in Table 44). The corresponding coefficient for children from higher educated parents is statistically insignificant and significantly smaller. This result is robust to using the alternative radius of 2km, where the point estimates differ even more[171].

Heterogeneity with respect to migrant background

Now we turn to analysing whether children whose parents were both born outside of Sweden were differently affected by the choice reform. Again, we find that the point estimates for the effect of school choice on marks and the cognitive score are very similar in size for children with at least one Swedish parent and those whose parents were both born outside of Sweden (see Table 46). However, when we instead use the choice measure counting the number of schools within 2km around a student's home, the results are more mixed[172] and indicate larger effects for children of Swedish parents on 9^{th} grade marks but smaller effects on cognitive scores in the youngest cohort group. Most of these differences are however not, or only at the 90% confidence level, statistically significantly different. Moreover, we find that, for children whose parents were both born outside of Sweden in the youngest cohorts, having one more school

[170]Note that the average probability of ever having been committed for a crime at age 22, as reported in Table 36, was around 20 per cent for the low-income group.
[171] See Table 69 and Table 70.

within 2km increases the likelihood of having a university degree by 1.15 percentage points, while this number is smaller and only weakly identified, as indicated by significant Placebo tests, for children with Swedish parents[173]. Overall, the results for the subpopulations of children with or without at least one native Swedish parent are less robust than the results for the other subpopulations. One conclusion that can be drawn is, however, that there is no clear pattern indicating that children from immigrants have been harmed by more choice at the outset of the reform compared to children with at least one Swedish-born parent.

Heterogeneity with respect to high crime vs. low crime area

Lastly, we investigate whether children living in high crime areas benefit more or less from school choice than children living in low or medium crime areas. In this section, we hence explore if the effects differ with respect to the area characteristics of the student instead of with respect to the parental background.

When we stratify the sample according to living in a high or low crime area in 1991, we find that students in high crime areas have often benefitted more from school choice, in terms of short-run outcomes, than those in low or medium level crime areas (see Section 4.8.1.4 in the appendix). An important fact to point out, however, is that the high crime municipalities are mostly urban municipalities, and we found in Chapter 3 that the effects on marks are mostly driven by individuals living in urban areas. In order to not confuse heterogeneous effects between areas with different crime rates with heterogeneity arising from living in an urban or rural municipality, we run four separate regressions for all combinations of living in high crime vs. medium level crime and urban vs. non-urban municipalities.

Table 48 shows the results for the percentile rank in 9th grade marks and the cognitive score, and Table 49 shows results on the distribution of 9th grade math marks. To begin with, it should be noted that most of the estimated effects are very small, and often not significantly different from zero. Table 48 shows that the small positive results

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[172] See Table 72.
[173] See Table 74.

on the average percentile rank in marks are mostly driven by individuals living in high crime urban areas, while results in other areas are even sometimes statistically significantly negative. However, the identification for the results for the non-urban low and medium level crime areas is rather weak, indicated by the failing placebo test. A similar pattern arises also when using the 2km radius to construct the choice measure[174]. For the cognitive score at the age of around 18, we find almost no statistically significant effect (see Table 48). The probability of achieving a high grade in math is positively affected in high crime urban areas, while effects are mostly insignificant for the other subgroups (see Table 49). Correspondingly, the probability of receiving a passing grade is negatively affected by more choice for some cohorts in all but the high crime municipal regions, where the point estimates are positive and sometimes significant (see Table 49). Taken together, there is no indication that children living in high crime areas were harmed by the school choice as introduced by the reform. Though the evidence is sometimes weak, it rather seems to be the case that those living in urban high crime areas benefited relatively more than others from increased choice options.

For all of the above subgroups, we also ran further estimations for the probability of committing a crime until age 22, receiving a university degree until age 25, and being employed at age 25[175]. Especially for the latter two, we often ran into identification problems in the sense that the placebo test failed. Estimated effects were small but the placebo estimates were often of the same size, which is why results on these later outcomes should not be overinterpreted. However, as for the above presented results, there was almost never an indication for students from more disadvantaged backgrounds benefiting less from having more schools nearby before the choice reform. Qualitatively, the same is true when estimating the effect of choice using the number of schools within a 2 km radius instead of the median commuting distance[176].

4.7 Conclusion

We can conclude that our analyses show no evidence indicating that children from a socio-economically disadvantaged or immigration background have been harmed by

[174] See Table 77 and Table 78.
[175] See tables in Section 4.8.2.3.2.

school choice as it evolved after the introduction of the 1992 reform. The effects are small and similar for different subgroups and rather indicate slightly more positive effects on some outcomes for socio-economically disadvantaged children than for socio-economically more advantaged children. In order to avoid endogenous sorting of schools and parents into different areas after the reform, we measure school choice right before the introduction of the reform, which is, as we show, still closely related to the school choice at the time of decision making. Our estimates thus capture the effect of more school choice as is present right at the outset of the reform, including the dynamic processes that are a direct result of it, like the opening or closing of public and private schools and moves by parents in response to the changed system. Moreover, as we can test for, and sometimes find, placebo effects, especially concerning adult outcomes, we focus our interpretation on the most reliable results.

Previous studies analysing the Swedish 1992 choice reforms find that children from a lower-educated or migrant background are not hurt by an increased private school share, but that they benefit relatively less (Sandström and Bergström (2005), Björklund et al. (2004), Ahlin (2003)). These results are, however, no contradiction to the ones found in this study, as they focus on a different phenomenon of the choice reform. While previous studies have focused on studying the effects of competition by private schools, this study evaluates the overall dynamic effects that work through having more choice at the outset of the reform, that is also among public schools.

[176] See tables in Section 4.8.2.3.2.

4.8 Appendix

4.8.1 Additional analysis

This section presents additional analyses that are not included in the main body of the paper.

4.8.1.1 Segregation between schools

As the school choice reform has allowed all students to choose the school that they would like to attend, it may be that the composition of students at individual schools across Sweden has changed after as compared to before the reform. In particular, one argument against free school choice often mentioned in the political debate was the concern that segregation between schools along the socio-economic or migration background of the parents may increase with free school choice (see National Board of Education (2003), p.45). At the same time, one could argue that school choice mitigates existing residential segregation as the composition of schools is no longer necessarily identical to that of the residential area. Böhlmark and Lindahl (2007) have found that segregation between public and private schools along parental education and migration background increases with an increasing share of students attending private schools in a municipality. As this result focuses on the growth of private schools, it does not take into account the effects of choice among public schools, which is of high relevance for the present study.

In order to explore whether segregation among students in grades 7-9 has changed after the reform, we compute the standard deviation in the share of students from a different socio-economic background across schools in Sweden for each cohort of students born between 1972 and 1990. We then compare the average of this value for cohorts that were affected by the reform to the average for those that were not, that is we compare the standard deviation in student characteristics across schools between cohorts 1972-1976 and 1977-1990. Note that this exercise does not show effects of school choice on student segregation in schools as residential segregation might have changed over time as well, impacting also the composition of students at different schools. It is merely a way to describe whether Sweden has seen an increase in student segregation across schools after the 1992 school choice reform.

Table 50 presents the results of this exercise for the socio-economic characteristics considered in this study, being parental education, income and immigration background. We can see from this table that there is no change in the degree of overall student segregation between Swedish schools offering grades 7-9 after the choice reform[177].

4.8.1.2 Relation between degree of choice measured before and after the reform

In this section, we explore the relation between the degree of school choice as measured before the introduction of the reform, in 1991, and as measured at the actual time the child chooses a school for grades 7-9, at age 13. We conduct this analysis for cohorts that started grade 7 in or after 1992, i.e. for students born in or after 1979, as these are the cohorts for which we use the pre-reform measure instead of the actual measure of school choice in the main estimations that are presented in Section 4.6. In order for these main analyses to be meaningful, it is important that pre-reform and actual choice measures are related for all subgroups.

Similar to the corresponding analysis in Chapter 3, we regress the actual choice measure, that is the number of schools within the median commuting distance of the municipality measured at age 13, on the number of schools within the child's median commuting distance around her 1991 place of residence, i.e. the pre-reform choice measure that we use in the main analysis. In order to capture changes in the development of the number of schools over time, we interact the choice measure with a linear time trend. Since the variation that we use in the main estimations in Section 4.6 is conditional on covariates and cohort and municipality dummies, we include these covariates here as well and cluster on the municipality level[178].

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[177] This result also holds when distinguishing further between individual cohorts instead of just comparing pre- and post-reform cohorts.

[178] In accordance with main analyses, the covariates and municipality dummies are measured in 1991, that is at the pre-reform location of residence.

Table 51 and Table 52 present the marginal effects of an additional school within the median commuting distance as measured before the reform on the number of schools nearby when the child is 13 years old for the different subgroups. The correlation between the pre-reform and the post-reform measure is increasing over time, suggesting an increase in the number of schools, and is mostly close to or larger than one. Moreover, the relation is similar for the different subgroups and only slightly smaller for children from a disadvantaged or migration background.

The results thus suggest that the choice measures taken in 1991 are closely related to the post-reform measures taken at the time when children start grade 7 for all subgroups.

4.8.1.3 Linking the probability of attending a private school to choice measures

In this section, we link the degree of school choice as present at the outset of the reform to the probability of attending a private school. Since attending a private school was extremely rare before the 1992 reform, when estimating the effect of having more schools to choose from, we cannot follow a before-after comparison strategy as we did in Section 4.6. Nevertheless, since the private school share increased only gradually as it took some time for private schools to open up, it is also informative to analyse the development of the likelihood to attend a private school for the different subgroups over time.

Our results show that the effect of an additional school nearby on the probability of attending a private school is small and very similar across all groups (see Table 53 and Table 54). The point estimates for children from migrants is slightly larger, but when using the choice measure counting the number of schools within a 2km radius instead of within the commuting distance, this result reverses.

4.8.1.4 Heterogeneity with respect to crime level in the municipality

As we present the results for different subgroups of students living in high versus low or medium crime areas in Section 4.6.2 separately for urban and non-urban municipalities, this section shows the results when not making the latter distinction. Looking first at the outcome percentile rank in 9th grade marks in Table 55, we find that the point estimates are always positive and mostly significant in the high crime areas, and always negative and mostly significant in the low and medium crime areas[179]. Though the magnitude of the estimates is still very small, it thus seems that effects in the higher crime areas drive the positive pooled results. As outlined in Section 4.6.2, this is also related to the fact that municipalities with a higher crime rate are more often urban areas.

With respect to the cognitive score (see Columns 3 and 4 in Table 55) and the probability of receiving a high grade in math (see Table 56), we find no sizable differences in the size of the effects. Even though we find very small negative effects on the probability of receiving a passing grade (of around 0.1 to 0.2 percentage points) for those living in low crime areas, there is also a negative Placebo-effect, which makes the identification for this outcome difficult. Qualitatively, the results are similar when using the 2km radius (see Table 75 and Table 76). We can thus conclude again that we do not find any evidence for children in high crime areas having benefited less or having been harmed by the reform; if anything, they seem to have benefited a bit more.

4.8.1.5 Further robustness analysis

In cases where the placebo test fails, that is where we find that the effect of the number of schools nearby has changed already for cohorts born in 1975 and 1976 compared to cohorts born in 1972 to 1974, i.e. cohorts that have not been affected by the reform, we modelled and estimated a pre-reform trend to control for these changes. To this end, we included both linear and quadratic time trends in the effect of the number of schools in the estimation and allowed the corresponding coefficients to differ between treatment windows. Then, we repeated the placebo test, that is we tested whether this trend captured all time-variation in the effect among cohorts before the reform. However, as this was mostly not the case, meaning that the identification

[179] One interesting pattern to note is that these differential results between the high- and low/medium crime municipalities are due to differences in the estimates for the pre-reform (control group) cohorts, rather than differences in the post-reform choice estimates. That is, the estimates for the untreated cohorts 1972-76 suggest that having more schools nearby is negatively correlated with students' outcomes in the high-crime areas, but significantly positively correlated with students' outcomes in the low crime areas. For the post-reform cohorts, effects for both subgroups are almost always positive, but since we estimate the differential impact of choice over time, taking the pre-reform cohorts 1972-76 as the baseline, we find negative coefficients for the low/medium crime areas, and positive coefficients for the high-crime areas.

problem could almost never be mitigated by controlling for a pre-reform trend, we do not show corresponding estimates.

4.8.2 Tables

The tables are presented according to the following structure: Section 4.8.2.1 presents tables from the main descriptive and regression analyses. The next subsection, Section 4.8.2.2, includes tables from additional analysis presented in Section 4.8.1 in the appendix. Finally, Section 4.8.2.3 presents, for reporting purposes, tables that include more detailed descriptive statistics and additional estimation results that we performed in relation to the main estimations in this study.

4.8.2.1 Tables on main descriptive statistics and analyses

This section presents tables on the main descriptive and regression results.

Table 36: Pre- and post-reform averages of student outcomes for different subgroups

	cohort is …-reform	rank GPA grade 9	passing grade in math	high grade in math	cognitive score	crime until age 22	university degree age 25	employed age 25
household income is…								
low income	pre	40.87	0.713	0.308	4.72	0.204	0.244	0.678
	post	40.55	0.835	0.294	4.64	0.203	0.292	0.671
medium income	pre	47.59	0.773	0.356	4.98	0.147	0.331	0.729
	post	48.11	0.885	0.370	4.92	0.134	0.389	0.714
high income	pre	55.92	0.821	0.429	5.50	0.129	0.472	0.695
	post	59.82	0.929	0.511	5.60	0.104	0.576	0.654
parents highest educational degree is … education								
compulsory	pre	36.37	0.684	0.275	4.14	0.186	0.159	0.740
	post	34.01	0.763	0.220	4.00	0.201	0.191	0.728
more than compulsory	pre	50.34	0.788	0.380	5.22	0.150	0.381	0.702
	post	50.68	0.895	0.403	5.14	0.137	0.440	0.684
parents are…								
both immigrants	pre	43.00	0.686	0.276	4.20	0.242	0.249	0.631
	post	44.42	0.824	0.304	4.20	0.226	0.346	0.607
at least one Swedish	pre	48.52	0.777	0.369	5.10	0.150	0.353	0.713
	post	49.78	0.889	0.396	5.10	0.136	0.420	0.695
home municipality is 1991…								
high crime	pre	48.62	0.753	0.349	5.03	0.185	0.330	0.684
	post	51.34	0.882	0.404	5.04	0.159	0.410	0.662
low/ medium crime	pre	48.12	0.776	0.367	5.06	0.149	0.351	0.714
	post	48.92	0.885	0.385	5.04	0.139	0.415	0.695

Note: Sample contains only observations with full information on all covariates X given below Table 40.

Table 37: High crime municipalities based on criminal convictions of individuals aged 16-19 years in year 1991.

type of crime	*high crime*	*low or medium crime*
assault	6.68	5.9
illegal driving	14.23	15.63
drunk driving	<3%	4.09
reckless driving	<3%	4.68
damage	4.91	5.56
petty theft /pilfering	13.9	13.05
theft	16.9	17.14
car/ bike theft	7.11	5.9
average crime rate	5.60%	3.50%
number	29	257

Note: High crime refers to municipalities that have a criminal conviction rate among 16-19-year-olds that is above the 90th percentile in the distribution of all municipalities. “Low or medium crime” refers to the complementary group.

Table 38: Mean travel distance and the share attending a private school for different subgroups

		DISTANCE TO SCHOOL				ATTENDING A PRIVATE SCHOOL	
		unconditional		*conditional on all covariates*		*unconditional*	*conditional on all covariates*
		pre-reform cohorts	post-reform cohorts	pre-reform cohorts	post-reform cohorts	pre-reform cohorts	post-reform cohorts
household income:							
low income	mean	8.27	8.74	6.54	7.61	0.035	0.038
	sd	39.71	41.07			0.184	
medium income	mean	5.99	6.07	5.62	6.26	0.029	0.036
	sd	26.79	26.18			0.168	
high income	mean	6.11	5.86	6.65	7.06	0.054	0.039
	sd	32.60	30.48			0.226	
parents highest educational degree is ... schooling							
compulsory	mean	6.51	6.78	5.58	6.48	0.017	0.020
	sd	26.36	29.04			0.128	
more than compulsory	mean	6.55	6.63	6.18	6.81	0.039	0.038
	sd	32.56	31.55			0.193	
parents are...							
both Immigrants	mean	5.64	5.37	6.36	6.53	0.059	0.053
	sd	40.52	35.16			0.236	
at least one Swedish	mean	6.60	6.74	6.10	6.80	0.035	0.036
	sd	31.10	31.05			0.185	
home municipality in 1991							
high crime	mean	5.39	5.54	6.49	7.14	0.069	0.053
	sd	34.52	33.11			0.253	
low or medium crime	mean	6.81	6.91	6.43	6.55	0.029	0.033
	sd	31.01	30.91			0.168	

Note: Sample contains only observations with full information on all covariates *X* given below Table 40 and for whom we observe at least one outcome.

Table 39: Descriptive statistics on choice measures for different subgroups

		NUMBER OF SCHOOLS WITHIN..			
		..median commuting distance		*..2km*	
		pre-reform cohorts	post-reform cohorts	pre-reform cohorts	post-reform cohorts
household income:					
low income	mean	3.847	6.112	1,43	1.503
	sd	5.169	9.273	1.662	1.783
medium income	mean	2.930	4.879	1.118	1.198
	sd	4.187	8.440	1.400	1.565
high income	mean	4.136	7.754	1.328	1.510
	sd	4.941	10.73	1.514	1.805
parents highest educational degree is ... schooling					
compulsory	mean	2.850	5.172	1.138	1.333
	sd	4.076	8.374	1.443	1.610
more than compulsory	mean	3.565	5.976	1.264	1.352
	sd	4.753	9.430	1.509	1.696
parents are…					
both Immigrants	mean	6.008	10.11	2.169	2.405
	sd	5.239	10.27	1.734	1.814
at least one Swedish	mean	3.299	5.582	1.188	1.267
	sd	4.579	9.197	1.466	1.651
home municipality in 1991					
high crime	mean	8.399	11.94	2.466	2.911
	sd	7.484	9.802	2.150	2.400
low or medium crime	mean	2.322	4.407	0.964	0.961
	sd	2.645	8.602	1.134	1.169

Note: Sample contains only observations with full information on all covariates *X* given below Table 40 and for whom we observe at least one outcome.

Table 40: Effect on distribution of marks in 9th grade math

CHOICE MEASURE: NUMBER OF SCHOOLS WITHIN RADIUS...	.. MEDIAN COMMUTING DISTANCE		.. 2KM	
Independent Variable	receiving a passing grade	receiving a high grade †	receiving a passing grade	receiving a high grade †
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988--1990	0.000342	0.00312***	-0.00118*	0.00778***
	(0.000278)	(0.000350)	(0.000695)	(0.00106)
Cohorts 1985--1987	0.000117	0.00248***	-0.0023***	0.00486***
	(0.000280)	(0.000347)	(0.000692)	(0.00107)
Cohorts 1982--1984	-4.84e-05	0.00163***	-0.00184***	0.00120
	(0.000280)	(0.000352)	(0.000689)	(0.00113)
Cohorts 1979--1981	0.000503*	0.000618*	0.00198**	-0.00254**
	(0.000300)	(0.000366)	(0.000951)	(0.00113)
Cohorts 1977--1978	-0.000268	-0.000195	-0.000726	-0.00221*
	(0.000388)	(0.000440)	(0.00113)	(0.00132)
Untreated Cohorts (1972--1976)	-0.000156	-0.00132***	0.00140**	0.000363
	(0.000282)	(0.000338)	(0.000651)	(0.000798)
Placebo test	pass	pass	pass	fail
Observations	1,712,116	1,712,116	1,712,116	1,712,116
R-squared ‡	0.134	0.0602	0.134	0.0601

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.
† For the outcome "receiving a high grade", we left out household income and its squared term to achieve convergence.
The following control variables are included in the estimation:
On the municipality level: population density, taxable income and taxable income squared
On the parish level: share of Swedish citizens among the 16-64-year-olds, mean earnings of the 20-64-year-olds, share of university graduates among the 20-64-year-olds, share of employed persons among the 20-64-year-olds, indicator variables for whether the population density of 7-15-year-olds is in the lowest or highest quartile across Sweden
On the individual level: household income and household income squared, whether the household received welfare, age of the mother at birth, indicator for living in a single parent household, number of children in the household, indicator for only child, whether child was born in Sweden, indicator variables on mothers and fathers country of birth separately (Swedish, Nordic (=Norwegian, Finnish, Danish), from other western country(=Western Europe, North America, Australia), rest of the world is base category), indicator variables on whether mother and/or father graduated from university or secondary education

Table 41: Effect of choice on percentile rank in marks and cognitive skills for different household income subgroups; choice radius "median commuting distance"

Independent Variable	Percentile rank marks			Cognitive score		
Household income:	*low income*	*medium income*	*high income*	*low income*	*medium income*	*high income*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1988-1990	0.197***	0.127***	0.102***			
	(0.0294)	(0.0236)	(0.0265)			
Cohorts 1985-1987	0.154***	0.0553**	0.0568**	0.00480*	0.00539***	-0.00420*
	(0.0297)	(0.0245)	(0.0266)	(0.00275)	(0.00206)	(0.00249)
Cohorts 1982-1984	0.0765**	0.000588	-0.0205	0.00271	0.00419**	-0.00544**
	(0.0298)	(0.0243)	(0.0269)	(0.00286)	(0.00209)	(0.00247)
Cohorts 1979-1981	0.0908***	0.0262	-0.0202	0.00484*	0.00172	-0.00708***
	(0.0322)	(0.0248)	(0.0288)	(0.00294)	(0.00218)	(0.00261)
Cohorts 1977-1978	-0.0115	0.00376	-0.0337	0.00636*	0.00100	0.000935
	(0.0363)	(0.0316)	(0.0358)	(0.00354)	(0.00302)	(0.00326)
Untreated cohorts	-0.106***	-0.0301	-0.00249	-0.00318	-0.00184	0.00537**
(1972-1976)	(0.0286)	(0.0231)	(0.0259)	(0.00275)	(0.00207)	(0.00250)
Placebo Test	pass	pass	pass	pass	pass	pass
Observations	396,923	873,180	445,318	135,210	312,206	162,766
R-squared ‡	0.138	0.131	0.182	0.113	0.113	0.154

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 42: Effect of choice on probability of receiving a high or passing grade in math for different household income subgroups; choice radius "median commuting distance"

INDEPENDENT VARIABLE	RECEIVING A HIGH GRADE IN MATH			RECEIVING A PASSING GRADE IN MATH		
Household income:	*low income*	*medium income*	*high income*	*low income*	*medium income*	*high income*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1988-1990	0.0037***	0.0031***	0.0021***	0.0007	0.0006*	0.0002
	(0.00049)	(0.00044)	(0.00057)	(0.00048)	(0.00037)	(0.00038)
Cohorts 1985-1987	0.0032***	0.0024***	0.0014**	0.0004	0.0003	0.0001
	(0.00049)	(0.00044)	(0.00056)	(0.00049)	(0.00037)	(0.00038)
Cohorts 1982-1984	0.0024***	0.0016***	0.0004	-0.0000	0.0001	0.0002
	(0.00050)	(0.00045)	(0.00057)	(0.00049)	(0.00037)	(0.00039)
Cohorts 1979-1981	0.0013**	0.0009*	-0.0000	0.0007	0.0010**	0.0001
	(0.00054)	(0.00047)	(0.00059)	(0.00054)	(0.00040)	(0.00041)
Cohorts 1977-1978	-0.0000	0.0004	-0.0006	-0.0005	0.0001	-0.0003
	(0.00064)	(0.00058)	(0.00073)	(0.00066)	(0.00052)	(0.00049)
Untreated cohorts	-0.0021***	-0.0016***	-0.0006	-0.0004	-0.0004	-0.0002
(1972-1976)	(0.00049)	(0.00043)	(0.00056)	(0.00050)	(0.00037)	(0.00039)
Placebo Test	pass	pass	fail	pass	pass	pass
Observations	395,334	871,845	444,937	395,334	871,845	444,937
R-squared ‡	0.0425	0.0411	0.0661	0.105	0.124	0.163

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 43: Effect of choice on probability of committing a crime until age 22 for different household income subgroups; choice radius "median commuting distance"

INDEPENDENT VARIABLE	CRIME UNTIL AGE 22		
Household income:	*low income*	*medium income*	*high income*
Marginal effect of choice, relative to untreated cohorts for:			
Cohorts 1985-1987	-0.00143***	-0.000447	-0.000331
	(0.000381)	(0.000284)	(0.000287)
Cohorts 1982-1984	-0.00169***	-0.000606**	-0.000283
	(0.000390)	(0.000288)	(0.000291)
Cohorts 1979-1981	-0.00165***	-0.000364	-0.000211
	(0.000422)	(0.000303)	(0.000310)
Cohorts 1977-1978	-0.000609	0.000157	8.49e-05
	(0.000545)	(0.000379)	(0.000387)
Untreated cohorts	0.00156***	0.000679**	0.000453
(1972-1976)	(0.000380)	(0.000290)	(0.000296)
Placebo Test	pass	pass	pass
Observations	326,904	717,262	364,926
R-squared ‡	0.0315	0.0304	0.0269

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 44: Effect of choice on percentile rank in marks and cognitive skills for different parental education levels; choice radius "median commuting distance"

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS		COGNITIVE SCORE	
Parental education is…	*more than compulsory*	*compulsory*	*more than compulsory*	*compulsory*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.133***	0.148***		
	(0.0191)	(0.0436)		
Cohorts 1985-1987	0.0806***	0.0911**	0.00110	0.0144***
	(0.0194)	(0.0430)	(0.00159)	(0.00448)
Cohorts 1982-1984	0.0118	0.0358	-0.000499	0.0141***
	(0.0189)	(0.0429)	(0.00159)	(0.00438)
Cohorts 1979-1981	0.0167	0.129***	-0.00157	0.0154***
	(0.0204)	(0.0441)	(0.00167)	(0.00461)
Cohorts 1977-1978	-0.0165	0.0382	0.00239	0.00557
	(0.0236)	(0.0559)	(0.00218)	(0.00594)
Untreated cohorts	-0.0351*	-0.0686*	0.00130	-0.0131***
(1972-1976)	(0.0181)	(0.0406)	(0.00160)	(0.00431)
Placebo Test	pass	pass	pass	pass
Observations	1,550,081	165,340	544,573	65,609
R-squared ‡	0.175	0.060	0.129	0.050

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 45: Effect of choice on probability of receiving a high or passing grade in math for different parental education levels; choice radius "median commuting distance"

INDEPENDENT VARIABLE	HIGH GRADE MATH		PASSING GRADE MATH	
Parental education is…	*more than compulsory*	*compulsory*	*more than compulsory*	*compulsory*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.00311***	0.00242***	0.000435	0.000221
	(0.000365)	(0.000706)	(0.000284)	(0.000771)
Cohorts 1985-1987	0.00244***	0.00232***	0.000260	-0.000871
	(0.000361)	(0.000670)	(0.000286)	(0.000747)
Cohorts 1982-1984	0.00155***	0.00123*	0.000107	-0.00124
	(0.000366)	(0.000683)	(0.000286)	(0.000754)
Cohorts 1979-1981	0.000499	0.00138*	0.000448	0.00141*
	(0.000379)	(0.000749)	(0.000306)	(0.000832)
Cohorts 1977-1978	-0.000319	0.000983	-0.000307	0.000361
	(0.000458)	(0.000934)	(0.000390)	(0.00110)
Untreated cohorts	-0.00132***	-0.00101	-0.000267	0.000439
(1972-1976)	(0.000351)	(0.000657)	(0.000289)	(0.000759)
Placebo Test	pass	pass	pass	pass
Observations	1,547,652	164,464	1,547,652	164,464
R-squared ‡	0.0575	0.0206	0.133	0.0609

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 46: Effect of choice on percentile rank in marks and cognitive skills for different parental migration backgrounds; choice radius "median commuting distance"

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS		COGNITIVE SCORE	
Parental migration background	*at least one Swedish*	*both immigrants*	*at least one Swedish*	*both immigrants*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.142***	0.161***		
	(0.0186)	(0.0547)		
Cohorts 1985-1987	0.0892***	0.0810	0.00118	0.00972*
	(0.0190)	(0.0537)	(0.00156)	(0.00542)
Cohorts 1982-1984	0.0172	-0.00374	-0.000621	0.00739
	(0.0186)	(0.0542)	(0.00157)	(0.00539)
Cohorts 1979-1981	0.0180	0.0767	-0.00212	0.00790
	(0.0200)	(0.0561)	(0.00167)	(0.00562)
Cohorts 1977-1978	-0.0185	0.00110	0.00270	0.00242
	(0.0237)	(0.0664)	(0.00216)	(0.00712)
Untreated cohorts	-0.0361**	-0.0871	0.00160	-0.00990*
(1972-1976)	(0.0177)	(0.0534)	(0.00158)	(0.00545)
Placebo Test	pass	pass	pass	pass
Observations	1,599,471	115,950	575,487	34,695
R-squared ‡	0.191	0.139	0.139	0.150

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 47: Effect of choice on probability of receiving a high or passing grade in math for different parental migration backgrounds; choice radius "median commuting distance"

INDEPENDENT VARIABLE	HIGH GRADE MATH		PASSING GRADE MATH	
Parental education is…	*at least one Swedish*	*both immigrants*	*at least one Swedish*	*both immigrants*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.00330***	0.00282***	0.000471*	0.000697
	(0.000362)	(0.000859)	(0.000281)	(0.000978)
Cohorts 1985-1987	0.00261***	0.00249***	0.000270	-1.15e-05
	(0.000359)	(0.000849)	(0.000283)	(0.000969)
Cohorts 1982-1984	0.00171***	0.00161*	8.60e-05	-0.000396
	(0.000363)	(0.000868)	(0.000283)	(0.000977)
Cohorts 1979-1981	0.000554	0.000581	0.000347	0.00141
	(0.000379)	(0.000908)	(0.000309)	(0.00101)
Cohorts 1977-1978	-0.000141	-0.00134	-0.000330	-0.000129
	(0.000456)	(0.00104)	(0.000399)	(0.00124)
Untreated cohorts	-0.00141***	-0.000966	-0.000230	-0.000292
(1972-1976)	(0.000348)	(0.000846)	(0.000287)	(0.000988)
Placebo Test	pass	pass	pass	pass
Observations	1,596,671	115,445	1,596,671	115,414
R-squared ‡	0.0606	0.0527	0.135	0.121

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 48: Effect of choice on the percentile rank in marks and cognitive skills for different types of home municipalities in 1991; choice radius "median commuting distance"

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS				COGNITIVE SCORE			
municipality characteristics 1991	*low/medium crime*		*high crime*		*low/medium crime*		*high crime*	
	non-urban	*urban*	*non-urban*	*urban*	*non-urban*	*urban*	*non-urban*	*urban*
Marginal effect of choice, relative to untreated cohorts for:								
Cohorts 1988-1990	-0.229**	-0.034	-0.252	0.260***				
	(0.095)	(0.034)	(0.445)	(0.030)				
Cohorts 1985-1987	-0.054	-0.066*	-0.603	0.186***	0.021**	0.001	-0.035	-0.004
	(0.096)	(0.034)	(0.430)	(0.032)	(0.010)	(0.003)	(0.041)	(0.003)
Cohorts 1982-1984	-0.197**	-0.114***	-0.250	0.084***	0.011	0.000	0.011	-0.004
	(0.096)	(0.034)	(0.435)	(0.032)	(0.009)	(0.003)	(0.044)	(0.003)
Cohorts 1979-1981	-0.097	-0.085**	-0.532	0.103***	0.009	-0.001	-0.038	-0.001
	(0.096)	(0.034)	(0.397)	(0.034)	(0.010)	(0.003)	(0.040)	(0.003)
Cohorts 1977-1978	-0.081	-0.084*	0.175	0.034	0.005	0.002	0.0052	0.001
	(0.119)	(0.050)	(0.421)	(0.035)	(0.012)	(0.004)	(0.040)	(0.003)
Untreated cohorts	0.267***	0.102***	0.093	-0.131***	-0.014**	0.002	-0.005	0.003
(1972-1976)	(0.072)	(0.034)	(0.282)	(0.027)	(0.007)	(0.003)	(0.026)	(0.002)
Placebo Test	fail	pass	pass	pass	pass	pass	pass	pass
Observations	714,999	666,671	69,495	264,256	257,026	239,529	24,708	88,919
R-squared ‡	0.164	0.196	0.167	0.205	0.124	0.155	0.129	0.179

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 49 : Effect of choice on probability of receiving a high or passing grade in math for different types of home municipalities in 1991; choice radius "median commuting distance"

INDEPENDENT VARIABLE	HIGH GRADE MATH				PASSING GRADE			
municipality characteristics 1991	*low/medium crime*		*high crime*		*low/medium crime*		*high crime*	
	non-urban	*urban*	*non-urban*	*urban*	*non-urban*	*urban*	*non-urban*	*urban*
Marginal effect of choice, relative to untreated cohorts for:								
Cohorts 1988-1990	-0.001	0.002**	0.009	0.004***	-0.003**	-0.001	-0.001	0.001*
	(0.002)	(0.001)	(0.008)	(0.001)	(0.001)	(0.000)	(0.006)	(0.000)
Cohorts 1985-1987	0.002	0.001*	-0.001	0.003***	-0.002	-0.001*	-0.009	0.001
	(0.002)	(0.001)	(0.008)	(0.001)	(0.001)	(0.000)	(0.006)	(0.000)
Cohorts 1982-1984	0.001	0.001	0.010	0.002***	-0.003**	-0.001**	-0.004	0.000
	(0.002)	(0.001)	(0.008)	(0.001)	(0.001)	(0.000)	(0.006)	(0.000)
Cohorts 1979-1981	0.001	0.000	-0.007	0.001*	-0.001	-0.001	-0.008	0.001**
	(0.002)	(0.001)	(0.009)	(0.001)	(0.002)	(0.001)	(0.008)	(0.0001)
Cohorts 1977-1978	0.001	0.000	-0.008	0.000	-0.002	0.000	-0.005	0.000
	(0.002)	(0.001)	(0.009)	(0.001)	(0.002)	(0.001)	(0.010)	(0.001)
Untreated cohorts	0.001	0.000	-0.007	-0.001	0.002*	0.001*	0.003	-0.001
(1972-1976)	(0.001)	(0.001)	(0.005)	(0.001)	(0.001)	(0.000)	(0.005)	(0.000)
Placebo Test	pass	pass	pass	pass	pass	pass	pass	pass
Observations	713,727	665,519	69,341	263,529	713,727	665,519	69,341	263,529
R-squared ‡	0.0504	0.0630	0.0572	0.0780	0.127	0.135	0.132	0.154

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

4.8.2.2 *Tables containing additional analysis presented in the appendix*

This section includes tables relating to additional analysis presented in Section 4.8.1 in the appendix.

Table 50: Average between-school standard deviation of parental characteristics

	Mean value of the between school standard deviation	
Share in the school with:	pre-reform (cohorts 72-76)	post-reform (cohorts 77-90)
Both parents non-Swedish	0.040	0.041
Both parents only pre-secondary education	0.028	0.029
Low household income	0.031	0.031
Medium household income	0.037	0.037
High household income	0.043	0.044
Number of observations	5040	18851

Table 51: Relation between pre-reform and post-reform choice measure, separately for subgroups according to household income and parental education

INDEPENDENT VARIABLE	ACTUAL NUMBER OF SCHOOLS WITHIN MEDIAN COMMUTING DISTANCE				
Household background	*low income*	*medium income*	*high income*	*higher educated*	*low educated*
Marginal effect of number of schools within median commuting distance in 1991 for:					
Cohorts 1988-1990	1.195***	1.324***	1.381***	1.323***	1.292***
	(0.0217)	(0.0217)	(0.0210)	(0.0200)	(0.0291)
Cohorts 1985-1987	1.037***	1.153***	1.210***	1.155***	1.120***
	(0.0150)	(0.0166)	(0.0151)	(0.0153)	(0.0211)
Cohorts 1982-1984	0.880***	0.981***	1.039***	0.986***	0.948***
	(0.0109)	(0.0155)	(0.0124)	(0.0138)	(0.0173)
Cohorts 1979-1981	0.722***	0.810***	0.868***	0.817***	0.776***
	(0.0123)	(0.0189)	(0.0145)	(0.0163)	(0.0203)
Observations	253,127	567,675	296,972	1,035,610	82,164
R-squared ‡	0.731	0.784	0.805	0.782	0.784

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.

‡Pseudo R-squared for binary outcomes.

Table 52: Relation between pre-reform and post-reform choice measure, separately for subgroups according to parental migration background and crime rate of municipality in 1991

INDEPENDENT VARIABLE	ACTUAL NUMBER OF SCHOOLS WITHIN MEDIAN COMMUTING DISTANCE			
Migration background and area backgrounds	*at least one parent born in Sweden*	*both parents born abroad*	*low/medium crime municipality*	*high crime municipality*
Marginal effect of number of schools within median commuting distance in 1991 for:				
Cohorts 1988-1990	1.328***	1.221***	1.369***	1.279***
	(0.0201)	(0.0278)	(0.0178)	(0.0356)
Cohorts 1985-1987	1.162***	1.060***	1.193***	1.117***
	(0.0152)	(0.0222)	(0.0141)	(0.0231)
Cohorts 1982-1984	0.996***	0.899***	1.016***	0.956***
	(0.0136)	(0.0208)	(0.0128)	(0.0155)
Cohorts 1979-1981	0.830***	0.738***	0.839***	0.795***
	(0.0165)	(0.0242)	(0.0144)	(0.0199)
Observations	1,035,577	82,197	801,339	204,183
R-squared ‡	0.786	0.709	0.807	0.718

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 53: Effect of choice on probability of attending a private school, separately for subgroups according to household income and parental education

INDEPENDENT VARIABLE	ATTENDING A PRIVATE SCHOOL				
Household background:	*low income †*	*medium income †*	*high income †*	*parental education high †*	*parental education low †*
Marginal effect of choice for:					
Cohorts 1988-1990	0.000493**	0.000363**	0.000502	0.000425**	0.000633***
	(0.000219)	(0.000179)	(0.000316)	(0.000216)	(0.000182)
Cohorts 1985-1987	0.000417***	0.000326***	0.000331	0.000324**	0.000516***
	(0.000135)	(0.000111)	(0.000226)	(0.000139)	(0.000135)
Cohorts 1982-1984	0.000170*	0.0001	(0.0000)	(0.0001)	0.000142**
	(0.0001)	(0.0001)	(0.000171)	(0.0001)	(0.0001)
Cohorts 1979-1981	0.000146*	0.000111*	0.000271	0.000172*	0.0000
	(0.0001)	(0.0001)	(0.000180)	(0.0001)	(0.0001)
Observations	253,076	545,596	280,816	1,003,352	76,136
R-squared ‡	0.122	0.137	0.116	0.128	0.140

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1. ‡Pseudo R-squared for binary outcomes. † = left out municipality dummies to achieve convergence

Table 54: Effect of choice on probability of attending a private school, separately for subgroups according to parental migration background and crime level in home municipality in 1991

INDEPENDENT VARIABLE	ATTENDING A PRIVATE SCHOOL			
migration background and area backgrounds	*at least one parent born in Sweden †*	*both parents born abroad †*	*low/medium crime municipality †*	*high crime municipality †*
Marginal effect of choice for:				
Cohorts 1988-1990	0.000325	0.00187***	0.000245	-0.000107
	(0.000207)	(0.000421)	(0.000200)	(0.000611)
Cohorts 1985-1987	0.000238*	0.00154***	0.000140	0.000325
	(0.000131)	(0.000318)	(0.000116)	(0.000473)
Cohorts 1982-1984	0.0001	0.000533**	-0.0001	0.000401
	(0.0001)	(0.000215)	(0.0001)	(0.000326)
Cohorts 1979-1981	0.000122	0.000724***	-0.0001	0.000669***
	(0.0001)	(0.000185)	(0.0001)	(0.000203)
Observations	1,000,823	78,665	860,306	218,735
R-squared ‡	0.133	0.123	0.131	0.122

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1. ‡Pseudo R-squared for binary outcomes. † = left out municipality dummies to achieve convergence

Table 55: Effect of choice on the percentile rank in marks and cognitive skills for different crime levels in home municipalities in 1991; choice radius "median commuting distance"

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS		COGNITIVE SCORE	
municipality characteristics 1991	*low/medium crime*	*high crime*	*low/medium crime*	*high crime*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	-0.0376	0.221***		
	(0.0296)	(0.0263)		
Cohorts 1985-1987	-0.0758**	0.149***	0.00279	-0.00235
	(0.0299)	(0.0278)	(0.00256)	(0.00239)
Cohorts 1982-1984	-0.129***	0.0447	0.000882	-0.00209
	(0.0297)	(0.0273)	(0.00255)	(0.00252)
Cohorts 1979-1981	-0.0981***	0.0733**	0.0001	-0.00137
	(0.0300)	(0.0294)	(0.00267)	(0.00255)
Cohorts 1977-1978	-0.111***	0.0398	0.00484	0.00361
	(0.0430)	(0.0297)	(0.00370)	(0.00299)
Untreated cohorts	0.118***	-0.108***	-0.000163	0.00222
(1972-1976)	(0.0296)	(0.0243)	(0.00263)	(0.00226)
Placebo Test	pass	pass	pass	pass
Observations	1,381,670	333,751	496,555	113,627
R-squared ‡	0.182	0.201	0.141	0.169

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 56: Effect of choice on the probability of receiving a high or passing grade in math for different crime levels in home municipalities in 1991; choice radius "median commuting distance"

INDEPENDENT VARIABLE	HIGH GRADE MATH		PASSING GRADE	
municipality characteristics 1991	*low/medium crime*	*high crime*	*low/medium crime*	*high crime*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.00194***	0.00392***	-0.00136***	0.000784*
	(0.000566)	(0.000522)	(0.000461)	(0.000427)
Cohorts 1985-1987	0.00151***	0.00255***	-0.00149***	0.000362
	(0.000565)	(0.000521)	(0.000464)	(0.000428)
Cohorts 1982-1984	0.000831	0.00118**	-0.00171***	0.000232
	(0.000572)	(0.000540)	(0.000465)	(0.000433)
Cohorts 1979-1981	0.000366	0.000315	-0.00117**	0.000963**
	(0.000583)	(0.000539)	(0.000483)	(0.000490)
Cohorts 1977-1978	-0.000905	0.000348	-0.00129*	0.000300
	(0.000828)	(0.000618)	(0.000711)	(0.000564)
Untreated cohorts	-0.000551	-0.000676	0.00151***	-0.000436
(1972-1976)	(0.000567)	(0.000480)	(0.000467)	(0.000441)
Placebo Test	pass	pass	fail	pass
Observations	1,379,246	332,870	1,379,246	332,870
R-squared ‡	0.0571	0.0741	0.131	0.149

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

4.8.2.3 Tables reporting additional descriptive statistics and analyses

This subsection presents, for reporting purposes, tables including more detailed descriptive statistics (in Section 4.8.2.3.1) and additional estimation results relating to the analyses in the main body of the text (in Section 4.8.2.3.2).

4.8.2.3.1 Descriptive Statistics

The following tables repeat the information discussed in Section 4.4.2, but additionally contain the standard deviation and number of observations in the different subgroups.

Table 57: Descriptive statistics on outcome variables, separately for different household income groups

		HOUSEHOLD INCOME					
		low income		*medium income*		*high income*	
		pre-reform cohorts	post-reform cohorts	pre-reform cohorts	post-reform cohorts	pre-reform cohorts	post-reform cohorts
percentile rank marks 9	mean	40.87	40.55	47.59	48.11	55.92	59.82
	sd	28.19	28.12	27.93	27.83	28.28	27.28
	N	100004	296919	224485	648695	113464	331854
receive passing grade in math	mean	0.713	0.835	0.773	0.885	0.821	0.929
	sd	0.452	0.371	0.419	0.319	0.384	0.258
	N	99240	296094	223856	647989	113259	331678
receive high grade in math	mean	0.308	0.294	0.356	0.370	0.429	0.511
	sd	0.462	0.455	0.479	0.483	0.495	0.500
	N	99240	296094	223856	647989	113259	331678
cognitive score	mean	4.718	4.639	4.978	4.919	5.497	5.600
	sd	1.926	1.928	1.896	1.905	1.914	1.889
	N	47467	90093	109378	205247	56300	107821
crime until age 22	mean	0.204	0.203	0.147	0.134	0.129	0.104
	sd	0.403	0.402	0.354	0.341	0.335	0.305
	N	103987	233623	229206	501059	116609	255475
university degree age 25	mean	0.244	0.292	0.331	0.389	0.472	0.576
	sd	0.429	0.455	0.470	0.487	0.499	0.494
	N	102877	162698	227150	351499	115268	178532
employed age 25	mean	0.678	0.671	0.729	0.714	0.695	0.654
	sd	0.467	0.470	0.445	0.452	0.460	0.476
	N	103206	164076	227692	353907	115611	180085

Note: Sample contains only observations with full information on all covariates X given *below* Table 40.

Table 58: Descriptive statistics on outcome variables, separately for different levels of parental education

		EDUCATIONAL BACKGROUND OF PARENTS			
		compulsory schooling		*more than compulsory schooling*	
		pre-reform cohorts	post-reform cohorts	pre-reform cohorts	post-reform cohorts
percentile rank marks 9	mean	36.37	34.01	50.34	50.68
	sd	26.55	25.80	28.41	28.44
	N	66721	98619	371232	1.179e+06
receive passing grade in math	mean	0.684	0.763	0.788	0.895
	sd	0.465	0.426	0.409	0.307
	N	66284	98180	370071	1.178e+06
receive high grade in math	mean	0.275	0.220	0.380	0.403
	sd	0.447	0.414	0.485	0.491
	N	66284	98180	370071	1.178e+06
cognitive score	mean	4.142	3.997	5.218	5.137
	sd	1.818	1.801	1.902	1.922
	N	31801	34827	181344	368334
crime until age 22	mean	0.186	0.201	0.150	0.137
	sd	0.389	0.401	0.357	0.344
	N	69070	89864	380732	900293
university degree age 25	mean	0.159	0.191	0.381	0.440
	sd	0.366	0.393	0.486	0.496
	N	68456	71130	376839	621599
employed age 25	mean	0.740	0.728	0.702	0.684
	sd	0.439	0.445	0.457	0.465
	N	68738	72153	377771	625915

Note: Sample contains only observations with full information on all covariates *X* given *below* Table 40.

Table 59: Descriptive statistics on outcome variables, separately for different parental migration backgrounds

		PARENTAL MIGRATION BACKGROUND			
		both immigrants		*at least one is Swedish*	
		pre-reform cohorts	post-reform cohorts	pre-reform cohorts	post-reform cohorts
percentile rank marks 9	mean	43.00	44.42	48.52	49.78
	sd	28.63	28.94	28.54	28.53
	N	24390	91560	413563	1.186e+06
receive passing grade in math	mean	0.686	0.824	0.777	0.889
	sd	0.464	0.381	0.416	0.314
	N	24152	91293	412203	1.184e+06
receive high grade in math	mean	0.276	0.304	0.369	0.396
	sd	0.447	0.460	0.483	0.489
	N	24152	91293	412203	1.184e+06
cognitive score	mean	4.197	4.201	5.100	5.095
	sd	1.966	1.924	1.917	1.927
	N	10017	25427	203128	377734
crime until age 22	mean	0.242	0.226	0.150	0.136
	sd	0.428	0.418	0.357	0.343
	N	25883	74372	423919	915785
university degree age 25	mean	0.249	0.346	0.353	0.420
	sd	0.432	0.476	0.478	0.494
	N	25444	51737	419851	640992
employed age 25	mean	0.631	0.607	0.713	0.695
	sd	0.482	0.488	0.452	0.460
	N	25577	52375	420932	645693

Note: Sample contains only observations with full information on all covariates *X* given *below* Table 40.

Table 60: Descriptive statistics on outcome variables, separately for different levels crime in home municipality in 1991

		MUNICIPALITY CHARACTERISTICS IN 1991			
		high crime		*low crime*	
		pre-reform cohorts	post-reform cohorts	pre-reform cohort	post-reform cohorts
percentile rank marks 9	mean	48.62	51.34	48.12	48.92
	sd	29.20	29.35	28.43	28.39
	N	80164	253587	357789	1.024e+06
receive passing grade in math	mean	0.753	0.882	0.776	0.885
	sd	0.431	0.323	0.417	0.319
	N	79679	253191	356676	1.023e+06
receive high grade in math	mean	0.349	0.404	0.367	0.385
	sd	0.477	0.491	0.482	0.487
	N	79679	253191	356676	1.023e+06
cognitive score	mean	5.029	5.040	5.064	5.038
	sd	1.966	1.957	1.920	1.934
	N	38866	76667	174279	326494
crime until age 22	mean	0.185	0.159	0.149	0.139
	sd	0.388	0.365	0.356	0.346
	N	83860	193224	365942	796933
university degree age 25	mean	0.330	0.410	0.351	0.415
	sd	0.470	0.492	0.477	0.493
	N	82968	132998	362327	559731
employed age 25	mean	0.684	0.662	0.714	0.695
	sd	0.465	0.473	0.452	0.460
	N	83239	134120	363270	563948

Note: Sample contains only observations with full information on all covariates *X* given *below* Table 40.

4.8.2.3.2 Tables on subgroup analysis for later outcomes

The following section presents additional tables on the results of the effects of more school choice through having many schools nearby just before the reform. Thus, as regards the structure, the tables are similar to those discussed in Section 4.6.2. The next subsection includes results from using the choice measure that counts the number of schools within the median commuting distance of the home municipality around a student's home in 1991, the subsequent one presents those using a radius of 2km instead.

Using the choice measure “number of schools within median commuting distance”

Table 61: Effect of choice on education and employment at age 25 for different household income subgroups; choice radius “median commuting distance”

INDEPENDENT VARIABLE	UNIVERSITY DEGREE AT AGE 25			EMPLOYED AT AGE 25		
Household income:	*low income*	*medium income*	*high income*	*low income*	*medium income*	*high income*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1982-1984	0.00174***	0.00186***	0.000604	0.00149***	0.000671*	0.000237
	(0.000454)	(0.000404)	(0.000507)	(0.000476)	(0.000375)	(0.000465)
Cohorts 1979-1981	0.000840*	0.000531	-0.000740	0.00114**	0.000716*	0.000138
	(0.000497)	(0.000425)	(0.000528)	(0.000490)	(0.000398)	(0.000485)
Cohorts 1977-1978	-0.000917	0.000145	-0.00119*	0.000922	-0.000128	-0.000569
	(0.000607)	(0.000545)	(0.000658)	(0.000598)	(0.000483)	(0.000605)
Untreated cohorts	-0.000758*	-0.000301	0.000219	-0.0024***	-0.0012***	-0.000467
(1972-1976)	(0.000451)	(0.000405)	(0.000517)	(0.000487)	(0.000387)	(0.000473)
Placebo Test	pass	fail	fail	fail	fail	pass
Observations	259,062	571,525	289,872	259,226	571,687	289,932
R-squared ‡	0.0936	0.0917	0.134	0.0254	0.0271	0.0405

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 62: Effect of choice on probability of committing a crime until age 22 and education and employment at age 25 for different parental education levels; choice radius "median commuting distance"

INDEPENDENT VARIABLE	CRIME AGE 22		UNIVERSITY DEGREE AGE 25		EMPLOYED AGE 25	
Parental education is…	*more than compulsory*	*compulsory*	*more than compulsory †*	*compulsory*	*more than compulsory*	*compulsory*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1985-1987	-0.00054***	-0.000811				
	(0.000197)	(0.000600)				
Cohorts 1982-1984	-0.00063***	-0.00122**	0.00134***	0.00229***	0.000416	0.00295***
	(0.000201)	(0.000581)	(0.000313)	(0.000612)	(0.000273)	(0.000698)
Cohorts 1979-1981	-0.000431**	-0.0022***	0.0001	0.00171***	0.000348	0.00251***
	(0.000216)	(0.000623)	(0.000329)	(0.000656)	(0.000296)	(0.000731)
Cohorts 1977-1978	0.000129	-0.00159**	-0.000708*	0.000949	-0.000179	0.00136
	(0.000287)	(0.000745)	(0.000414)	(0.000848)	(0.000343)	(0.000909)
Untreated cohorts	0.000728***	0.000879	-0.000153	-0.000841	-0.0009***	-0.0033***
(1972-1976)	(0.000201)	(0.000585)	(0.000312)	(0.000603)	(0.000279)	(0.000712)
Placebo Test	pass	pass	fail	pass	fail	pass
Observations	1,255,800	153,292	984,366	136,093	984,638	136,207
R-squared ‡	0.0364	0.0366	0.114	0.0374	0.0293	0.0339

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1. ‡Pseudo R-squared for binary outcomes. † For the outcome "receiving a high grade", we left out household income and its squared term to achieve convergence.

Table 63: Effect of choice on probability of committing a crime until age 22 and education and employment at age 25 for different parental migration backgrounds; choice radius "median commuting distance"

INDEPENDENT VARIABLE	CRIME AGE 22		UNIVERSITY DEGREE AGE 25		EMPLOYED AGE 25	
Parental migration background	*at least one Swedish*	*both immigrants*	*at least one Swedish †*	*both immigrants*	*at least one Swedish*	*both immigrants*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1985-1987	-0.000475**	-0.00106				
	(0.000194)	(0.000784)				
Cohorts 1982-1984	-0.000508**	-0.00181**	0.000852***	0.00215***	0.000520*	0.00124
	(0.000197)	(0.000790)	(0.000308)	(0.000804)	(0.000270)	(0.000887)
Cohorts 1979-1981	-0.000372*	-0.00208**	-0.000251	0.00128	0.000496*	0.000833
	(0.000215)	(0.000835)	(0.000327)	(0.000836)	(0.000292)	(0.000930)
Cohorts 1977-1978	0.000189	-0.00156	-0.000747*	0.000721	-0.000182	0.000888
	(0.000280)	(0.00100)	(0.000419)	(0.00105)	(0.000345)	(0.00117)
Untreated cohorts	0.000607***	0.00142*	0.000252	-0.000779	-0.0011***	-0.00120
(1972-1976)	(0.000199)	(0.000787)	(0.000309)	(0.000812)	(0.000276)	(0.000899)
Placebo Test	pass	pass	fail	pass	fail	pass
Observations	1,313,155	95,925	1,045,998	74,437	1,046,309	74,523
R-squared ‡	0.0355	0.0319	0.128	0.103	0.0291	0.0300

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1. ‡Pseudo R-squared for binary outcomes. † For the outcome "receiving a high grade", we left out household income and its squared term to achieve convergence.

Table 64: Effect of choice on probability of committing a crime until age 22 and education and employment at age 25 for different crime levels in home municipalities in 1991; choice radius "median commuting distance"

INDEPENDENT VARIABLE	CRIME AGE 22		UNIVERSITY DEGREE AGE 25		EMPLOYED AGE 25	
municipality characteristics 1991	*low/medium crime*	*high crime*	*low/medium crime*	*high crime*	*low/medium crime*	*high crime*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1985-1987	0.000262	-0.0001				
	(0.000305)	(0.000329)				
Cohorts 1982-1984	0.000189	-0.000279	-0.0016***	0.00208***	0.000805*	-0.000172
	(0.000311)	(0.000325)	(0.000515)	(0.000434)	(0.000460)	(0.000401)
Cohorts 1979-1981	0.000344	-0.000474	-0.0026***	0.00134***	0.000803*	-0.000255
	(0.000325)	(0.000349)	(0.000520)	(0.000462)	(0.000485)	(0.000423)
Cohorts 1977-1978	0.00125**	0.0000	-0.0026***	0.000324	-0.000980	-0.000279
	(0.000487)	(0.000414)	(0.000775)	(0.000527)	(0.000612)	(0.000475)
Untreated cohorts	-0.000198	0.000582*	0.00304***	-0.0015***	-0.00122**	-0.000593
(1972-1976)	(0.000313)	(0.000328)	(0.000524)	(0.000406)	(0.000476)	(0.000390)
Placebo Test	pass	pass	fail	pass	pass	pass
Observations	1,140,119	268,973	909,773	210,686	910,050	210,795
R-squared ‡	0.0369	0.0400	0.125	0.132	0.0296	0.0299

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1. ‡Pseudo R-squared for binary outcomes.

Using the choice measure "number of schools within 2km

Table 65: Effect of choice on percentile rank in marks and cognitive skills for different household income subgroups; choice radius 2km

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS			COGNITIVE SCORE		
Household income:	*low income*	*medium income*	*high income*	*low income*	*medium income*	*high income*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1988-1990	0.536***	0.294***	0.199**			
	(0.0924)	(0.0740)	(0.0822)			
Cohorts 1985-1987	0.403***	-0.0536	0.0289	0.0397***	0.0216***	-0.00189
	(0.0949)	(0.0796)	(0.0895)	(0.00945)	(0.00718)	(0.00871)
Cohorts 1982-1984	0.0995	-0.157*	-0.201**	0.0210**	0.0208***	-0.0144
	(0.0989)	(0.0803)	(0.0937)	(0.00973)	(0.00719)	(0.00895)
Cohorts 1979-1981	0.158	-0.0316	-0.336***	0.0370***	0.0132*	-0.0195**
	(0.0989)	(0.0802)	(0.0998)	(0.00942)	(0.00723)	(0.00894)
Cohorts 1977-1978	-0.0974	-0.0213	-0.243**	0.0338***	0.00823	0.000851
	(0.105)	(0.0891)	(0.109)	(0.0108)	(0.00839)	(0.00983)
Untreated cohorts	-0.0654	0.104*	0.320***	-0.0259***	-0.0145***	0.00520
(1972-1976)	(0.0685)	(0.0547)	(0.0648)	(0.00670)	(0.00514)	(0.00639)
Placebo Test	pass	pass	fail	pass	pass	pass
Observations	396,923	873,180	445,318	135,210	312,206	162,766
R-squared ‡	0.138	0.131	0.182	0.113	0.113	0.154

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 66: Effect of choice on probability of receiving a high or passing grade in math for different household income subgroups; choice radius 2km

INDEPENDENT VARIABLE	RECEIVING A HIGH GRADE IN MATH			RECEIVING A PASSING GRADE IN MATH		
Household income:	*low income*	*medium income*	*high income*	*low income*	*medium income*	*high income*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1988-1990	0.0105***	0.00715***	0.00693***	0.000156	-0.000122	-0.00148
	(0.00153)	(0.00134)	(0.00167)	(0.00131)	(0.000940)	(0.000951)
Cohorts 1985-1987	0.00784***	0.00370***	0.00390**	-0.000992	-0.00174*	-0.00210**
	(0.00162)	(0.00136)	(0.00179)	(0.00133)	(0.000929)	(0.000946)
Cohorts 1982-1984	0.00461***	0.000515	-0.000916	-0.00108	-0.00141	-0.00114
	(0.00169)	(0.00149)	(0.00180)	(0.00133)	(0.000911)	(0.000956)
Cohorts 1979-1981	0.00159	-0.00223	-0.0056***	0.00190	0.00340***	0.000278
	(0.00169)	(0.00143)	(0.00191)	(0.00170)	(0.00125)	(0.00132)
Cohorts 1977-1978	-0.0000	-0.00137	-0.00377*	-0.00122	-0.0001	-0.000703
	(0.00194)	(0.00176)	(0.00210)	(0.00196)	(0.00147)	(0.00152)
Untreated cohorts	-0.00250**	-0.000675	0.00328**	0.000454	0.000565	0.00166*
(1972-1976)	(0.00122)	(0.00101)	(0.00134)	(0.00119)	(0.000872)	(0.000931)
Placebo Test	fail	fail	pass	pass	pass	pass
Observations	395,334	871,845	444,937	395,334	871,845	444,937
R-squared ‡	0.0423	0.0410	0.0661	0.105	0.124	0.163

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1. ‡Pseudo R-squared for binary outcomes.

Table 67: Effect of choice on probability of committing a crime until age 22 for different household income subgroups; choice radius 2km

INDEPENDENT VARIABLE	CRIME UNTIL AGE 22		
Household income:	*low income*	*medium income*	*high income*
Marginal effect of choice, relative to untreated cohorts for:			
Cohorts 1985-1987	-0.00275**	0.000760	0.00126
	(0.00123)	(0.000860)	(0.000950)
Cohorts 1982-1984	-0.00253*	0.000462	0.000729
	(0.00134)	(0.000899)	(0.000998)
Cohorts 1979-1981	-0.00287**	-0.000281	0.00178*
	(0.00135)	(0.000920)	(0.00107)
Cohorts 1977-1978	4.03e-05	0.00129	0.00159
	(0.00161)	(0.00105)	(0.00119)
Untreated cohorts	0.00311***	0.000507	-0.000851
(1972-1976)	(0.000958)	(0.000690)	(0.000758)
Placebo Test	pass	pass	pass
Observations	326,904	717,262	364,926
R-squared ‡	0.0315	0.0304	0.0269

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 68: Effect of choice on education and employment at age 25 for different household income subgroups; choice radius 2km

INDEPENDENT VARIABLE	UNIVERSITY DEGREE AT AGE 25			EMPLOYED AT AGE 25		
Household income:	*low income*	*medium income*	*high income*	*low income*	*medium income*	*high income*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1982-1984	0.00868***	0.00728***	0.0001	0.00420***	0.00151	0.00132
	(0.00153)	(0.00134)	(0.00189)	(0.00155)	(0.00116)	(0.00152)
Cohorts 1979-1981	0.00517***	0.00444***	-0.00376**	0.00144	0.00161	-0.0000
	(0.00160)	(0.00137)	(0.00188)	(0.00153)	(0.00121)	(0.00165)
Cohorts 1977-1978	-0.000698	0.00265*	-0.00287	0.00299	0.00182	-0.00224
	(0.00178)	(0.00156)	(0.00212)	(0.00183)	(0.00139)	(0.00192)
Untreated cohorts	-0.00251**	-0.000501	0.00392***	-0.006***	-0.0055***	-0.0047***
(1972-1976)	(0.00111)	(0.000960)	(0.00132)	(0.00121)	(0.000898)	(0.00125)
Placebo Test	pass	fail	pass	pass	fail	pass
Observations	259,062	571,525	289,872	259,226	571,687	289,932
R-squared ‡	0.0936	0.0917	0.134	0.0254	0.0271	0.0406

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 69: Effect of choice on percentile rank in marks and cognitive skills for different parental education levels; choice radius 2km

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS		COGNITIVE SCORE	
Parental education is…	*more than compulsory*	*compulsory*	*more than compulsory*	*compulsory*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.282***	0.453***		
	(0.0591)	(0.166)		
Cohorts 1985-1987	0.0565	0.230	0.0157***	0.0525***
	(0.0634)	(0.156)	(0.00517)	(0.0184)
Cohorts 1982-1984	-0.155**	0.237	0.00629	0.0511***
	(0.0627)	(0.151)	(0.00539)	(0.0158)
Cohorts 1979-1981	-0.131*	0.349**	0.00745	0.0385***
	(0.0675)	(0.140)	(0.00555)	(0.0147)
Cohorts 1977-1978	-0.156**	0.232	0.0112*	0.0291*
	(0.0682)	(0.152)	(0.00642)	(0.0161)
Untreated cohorts	0.166***	-0.207**	-0.00820**	-0.0424***
(1972-1976)	(0.0435)	(0.0916)	(0.00382)	(0.00987)
Placebo Test	pass	pass	pass	pass
Observations	1,550,081	165,340	544,573	65,609
R-squared ‡	0.175	0.060	0.129	0.051

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 70: Effect of choice on probability of receiving a high or passing grade in math for different parental education levels; choice radius 2km

INDEPENDENT VARIABLE	HIGH GRADE MATH		PASSING GRADE MATH	
Parental education is…	*more than compulsory*	*compulsory*	*more than compulsory*	*compulsory*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.00748***	0.00949***	-0.000894	-0.000081
	(0.00110)	(0.00258)	(0.000702)	(0.00245)
Cohorts 1985-1987	0.00450***	0.00623**	-0.00187***	-0.00436*
	(0.00111)	(0.00246)	(0.000696)	(0.00229)
Cohorts 1982-1984	0.000417	0.00487**	-0.00153**	-0.00309
	(0.00119)	(0.00243)	(0.000697)	(0.00208)
Cohorts 1979-1981	-0.00347***	0.00312	0.00137	0.00623**
	(0.00118)	(0.00240)	(0.000973)	(0.00266)
Cohorts 1977-1978	-0.00307**	0.00308	-0.00113	0.00179
	(0.00138)	(0.00258)	(0.00117)	(0.00284)
Untreated cohorts	0.000709	-0.00284*	0.00108	0.00268
(1972-1976)	(0.000838)	(0.00158)	(0.000665)	(0.00175)
Placebo Test	fail	pass	pass	pass
Observations	1,547,652	164,464	1,547,652	164,464
R-squared ‡	0.0574	0.0205	0.133	0.0608

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.

‡Pseudo R-squared for binary outcomes.

Table 71: Effect of choice on probability of committing a crime until age 22 and education and employment at age 25 for different parental education levels; choice radius 2km

INDEPENDENT VARIABLE	CRIME AGE 22		UNIVERSITY DEGREE AGE 25		EMPLOYED AGE 25	
Parental education is…	*more than compulsory*	*compulsory*	*more than compulsory †*	*compulsory*	*more than compulsory*	*compulsory*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1985-1987	0.000466	0.000236				
	(0.000605)	(0.00218)				
Cohorts 1982-1984	0.000532	-0.00328*	0.00466***	0.0139***	0.00107	0.0102***
	(0.000647)	(0.00196)	(0.00110)	(0.00209)	(0.000883)	(0.00224)
Cohorts 1979-1981	0.000552	-0.0055***	0.00183*	0.00720***	0.0000	0.00826***
	(0.000684)	(0.00196)	(0.00109)	(0.00204)	(0.000929)	(0.00223)
Cohorts 1977-1978	0.00165**	-0.00168	-0.000402	0.00494**	0.000632	0.00490**
	(0.000820)	(0.00204)	(0.00127)	(0.00222)	(0.00106)	(0.00240)
Untreated cohorts	0.000335	0.00135	0.000711	-0.00277**	-0.0048***	-0.0078***
(1972-1976)	(0.000485)	(0.00134)	(0.000766)	(0.00138)	(0.000705)	(0.00159)
Placebo Test	pass	pass	fail	pass	fail	pass
Observations	1,255,800	153,292	984,366	136,093	984,638	136,207
R-squared ‡	0.0364	0.0366	0.114	0.0375	0.0293	0.0340

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes. † For the outcome "receiving a high grade", we left out household income and its squared term to achieve convergence.

Table 72: Effect of choice on percentile rank in marks and cognitive skills for different parental migration backgrounds; choice radius 2km

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS		COGNITIVE SCORE	
Parental migration background	*at least one Swedish*	*both immigrants*	*at least one Swedish*	*both immigrants*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.381***	0.0352		
	(0.0572)	(0.177)		
Cohorts 1985-1987	0.104	0.0675	0.0156***	0.0512***
	(0.0633)	(0.177)	(0.00526)	(0.0185)
Cohorts 1982-1984	-0.128**	-0.214	0.00507	0.0239
	(0.0629)	(0.178)	(0.00538)	(0.0178)
Cohorts 1979-1981	-0.118*	0.0501	0.00284	0.0360**
	(0.0656)	(0.177)	(0.00554)	(0.0181)
Cohorts 1977-1978	-0.147**	0.00338	0.0139**	0.00700
	(0.0677)	(0.194)	(0.00631)	(0.0197)
Untreated cohorts	0.117***	0.0868	-0.00834**	-0.0369***
(1972-1976)	(0.0421)	(0.138)	(0.00378)	(0.0135)
Placebo Test	pass	pass	pass	pass
Observations	1,599,471	115,950	575,487	34,695
R-squared ‡	0.190	0.139	0.139	0.150

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 73: Effect of choice on probability of receiving a high or passing grade in math for different parental migration backgrounds; choice radius 2km

INDEPENDENT VARIABLE	HIGH GRADE MATH		PASSING GRADE MATH	
Parental education is…	*at least one Swedish*	*both immigrants*	*at least one Swedish*	*both immigrants*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.00877***	0.00683**	-0.000227	-0.00457*
	(0.00108)	(0.00288)	(0.000701)	(0.00270)
Cohorts 1985-1987	0.00506***	0.00725**	-0.00179**	-0.00336
	(0.00111)	(0.00293)	(0.000699)	(0.00266)
Cohorts 1982-1984	0.00108	0.00265	-0.00155**	-0.00324
	(0.00118)	(0.00298)	(0.000695)	(0.00272)
Cohorts 1979-1981	-0.00330***	-0.00161	0.00103	0.00355
	(0.00117)	(0.00298)	(0.000994)	(0.00306)
Cohorts 1977-1978	-0.00284**	0.000581	-0.00103	-0.00115
	(0.00137)	(0.00321)	(0.00117)	(0.00361)
Untreated cohorts	0.000360	-0.00178	0.00103	0.00333
(1972-1976)	(0.000820)	(0.00226)	(0.000660)	(0.00250)
Placebo Test	fail	pass	pass	pass
Observations	1,596,671	115,445	1,596,671	115,414
R-squared ‡	0.0605	0.0524	0.135	0.121

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.

‡Pseudo R-squared for binary outcomes.

Table 74: Effect of choice on probability of committing a crime until age 22 and education and employment at age 25 for different parental migration backgrounds; choice radius 2km

INDEPENDENT VARIABLE	CRIME AGE 22		UNIVERSITY DEGREE AGE 25		EMPLOYED AGE 25	
Parental migration background	*at least one Swedish*	*both immigrants*	*at least one Swedish †*	*both immigrants*	*at least one Swedish*	*both immigrants*
Marginal effect of choice, relative to untreated cohorts for:						
Cohorts 1985-1987	0.000781	-0.00398				
	(0.000613)	(0.00251)				
Cohorts 1982-1984	0.000908	-0.00555**	0.00283***	0.0115***	0.00134	0.00672**
	(0.000647)	(0.00251)	(0.00108)	(0.00286)	(0.000857)	(0.00307)
Cohorts 1979-1981	0.000726	-0.0080***	0.000992	0.00818***	0.000822	0.00415
	(0.000682)	(0.00257)	(0.00108)	(0.00296)	(0.000905)	(0.00309)
Cohorts 1977-1978	0.00220***	-0.00674**	-0.000755	0.00882***	0.000575	0.00619*
	(0.000804)	(0.00280)	(0.00127)	(0.00313)	(0.00108)	(0.00335)
Untreated cohorts	4.68e-05	0.00372*	0.00161**	-0.0057***	-0.0052***	-0.00373
(1972-1976)	(0.000475)	(0.00200)	(0.000759)	(0.00211)	(0.000679)	(0.00240)
Placebo Test	pass	pass	pass	pass	fail	pass
Observations	1,313,155	95,925	1,045,998	74,437	1,046,309	74,523
R-squared ‡	0.0355	0.0319	0.128	0.104	0.0291	0.0300

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1. ‡Pseudo R-squared for binary outcomes. † For the outcome "receiving a high grade", we left out household income and its squared term to achieve convergence.

Table 75: Effect of choice on the percentile rank in marks and cognitive skills for different crime levels in home municipalities in 1991; choice radius 2km

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS		COGNITIVE SCORE	
municipality characteristics 1991	*low/medium crime*	*high crime*	*low/medium crime*	*high crime*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	-0.142**	0.518***		
	(0.0711)	(0.0909)		
Cohorts 1985-1987	-0.283***	0.344***	0.0155**	0.0143*
	(0.0748)	(0.0992)	(0.00739)	(0.00824)
Cohorts 1982-1984	-0.173**	-0.0149	0.0160**	-0.00371
	(0.0819)	(0.0955)	(0.00728)	(0.00866)
Cohorts 1979-1981	-0.0515	0.0444	0.0213***	0.00130
	(0.0760)	(0.107)	(0.00754)	(0.00900)
Cohorts 1977-1978	-0.175**	0.0511	0.0143*	0.0178*
	(0.0892)	(0.105)	(0.00791)	(0.0102)
Untreated cohorts	0.301***	-0.0476	-0.0150***	-0.00559
(1972-1976)	(0.0504)	(0.0690)	(0.00483)	(0.00638)
Placebo Test	pass	fail	pass	pass
Observations	1,381,670	333,751	496,555	113,627
R-squared ‡	0.181	0.201	0.141	0.169

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

Table 76: Effect of choice on the probability of receiving a high or passing grade in math for different crime levels in home municipalities in 1991; choice radius 2km

INDEPENDENT VARIABLE	HIGH GRADE MATH		PASSING GRADE	
municipality characteristics 1991	*low/medium crime*	*high crime*	*low/medium crime*	*high crime*
Marginal effect of choice, relative to untreated cohorts for:				
Cohorts 1988-1990	0.00262*	0.00965***	-0.00338***	-0.00120
	(0.00142)	(0.00178)	(0.000886)	(0.00126)
Cohorts 1985-1987	0.000982	0.00530***	-0.00399***	-0.00222*
	(0.00138)	(0.00181)	(0.000896)	(0.00125)
Cohorts 1982-1984	0.000938	-0.00126	-0.00354***	-0.00184
	(0.00152)	(0.00186)	(0.000901)	(0.00126)
Cohorts 1979-1981	-0.00160	-0.00194	0.00115	0.00196
	(0.00142)	(0.00189)	(0.00124)	(0.00168)
Cohorts 1977-1978	-0.00216	-0.000428	-0.00160	0.00101
	(0.00163)	(0.00221)	(0.00140)	(0.00197)
Untreated cohorts	0.00148	0.000991	0.00236***	0.00181
(1972-1976)	(0.000967)	(0.00138)	(0.000815)	(0.00120)
Placebo Test	fail	pass	pass	pass
Observations	1,379,246	332,870	1,379,246	332,870
R-squared ‡	0.0570	0.0739	0.131	0.149

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.

‡Pseudo R-squared for binary outcomes.

Table 77: Effect of choice on the percentile rank in marks and cognitive skills for different types of home municipalities in 1991; choice radius 2km

INDEPENDENT VARIABLE	PERCENTILE RANK MARKS				COGNITIVE SCORE			
municipality characteristics 1991	*low/medium crime*		*high crime*		*low/medium crime*		*high crime*	
	non-urban	*urban*	*non-urban*	*urban*	*non-urban*	*urban*	*non-urban*	*urban*
Marginal effect of choice, relative to untreated cohorts for:								
Cohorts 1988-1990	-0.501***	-0.113	-0.233	0.539***				
	(0.125)	(0.085)	(0.526)	(0.100)				
Cohorts 1985-1987	-0.423***	-0.247***	-0.386	0.419***	0.015	0.018*	-0.057	0.018*
	(0.136)	(0.091)	(0.538)	(0.111)	(0.014)	(0.009)	(0.057)	(0.009)
Cohorts 1982-1984	-0.387***	0.009	-0.080	0.107	0.0237*	0.023***	0.018	-0.008
	(0.143)	(0.098)	(0.552)	(0.110)	(0.014)	(0.009)	(0.058)	(0.010)
Cohorts 1979-1981	-0.18	0.056	0.101	0.102	0.008	0.032***	0.001	0.005
	(0.131)	(0.094)	(0.515)	(0.120)	(0.013)	(0.010)	(0.049)	(0.010)
Cohorts 1977-1978	-0.213	-0.080	-0.073	0.054	0.010	0.015	0.033	0.010
	(0.149)	(0.109)	(0.509)	(0.121)	(0.015)	(0.010)	(0.052)	(0.011)
Untreated cohorts (1972-1976)	0.422***	0.233***	0.022	-0.086	-0.027***	-0.014**	-0.018	-0.005
	(0.087)	(0.061)	(0.315)	(0.0753)	(0.009)	(0.006)	(0.030)	(0.007)
Placebo Test	pass	pass	pass	pass	pass	pass	pass	pass
Observations	714,999	666,671	69,495	264,256	257,026	239,529	24,708	88,919
R-squared ‡	0.164	0.196	0.167	0.205	0.124	0.155	0.129	0.179

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1. ‡Pseudo R-squared for binary outcomes.

Table 78 Effect of choice on probability of receiving a high or passing grade in math for different types of home municipalities in 1991; choice radius 2km

INDEPENDENT VARIABLE	HIGH GRADE MATH				PASSING GRADE			
municipality characteristics 1991	*low/medium crime*		*high crime*		*low/medium crime*		*high crime*	
	non-urban	*urban*	*non-urban*	*urban*	*non-urban*	*urban*	*non-urban*	*urban*
Marginal effect of choice, relative to untreated cohorts for:								
Cohorts 1988-1990	-0.004	0.002	0.003	0.009***	-0.005***	-0.002**	-0.006	-0.002
	(0.003)	(0.002)	(0.010)	(0.002)	(0.002)	(0.001)	(0.006)	(0.001)
Cohorts 1985-1987	-0.001	0.000	0.002	0.006***	-0.005***	-0.002**	-0.012*	-0.002
	(0.003)	(0.002)	(0.010)	(0.002)	(0.002)	(0.001)	(0.006)	(0.001)
Cohorts 1982-1984	-0.003	0.002	-0.001	0.000	-0.004**	-0.002*	-0.006	-0.001
	(0.003)	(0.002)	(0.0115)	(0.002)	(0.002)	(0.001)	(0.006)	(0.001)
Cohorts 1979-1981	0.000	-0.001	-0.007	0.001	-0.002	0.004***	-0.012	0.004*
	(0.002)	(0.002)	(0.010)	(0.002)	(0.002)	(0.002)	(0.009)	(0.002)
Cohorts 1977-1978	-0.002	0.000	-0.008	0.000	-0.003	0.001	-0.012	0.002
	(0.003)	(0.002)	(0.011)	(0.002)	(0.003)	(0.007)	(0.011)	(0.002)
Untreated cohorts (1972-1976)	0.003*	0.001	0.000	0.001	0.003**	0.001	0.006	0.002
	(0.002)	(0.001)	(0.005)	(0.002)	(0.001)	(0.001)	(0.005)	(0.001)
Placebo Test	fail	pass	pass	pass	pass	pass	pass	pass
Observations	713,727	665,519	69,341	263,529	713,727	665,519	69,341	263,529
R-squared ‡	0.0504	0.0629	0.0571	0.0778	0.127	0.135	0.132	0.154

Notes: Robust standard errors in parentheses. Statistical significance at 1, 5 and 10% level is denoted by ***, **, *. For a complete list of included covariates see Table 40. The definition of the placebo tests is explained in Section 4.5.1.
‡Pseudo R-squared for binary outcomes.

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