Choice by Contrast in Swedish Schools: How Peers’ Achievement Affects Educational Choice

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We ask whether a social contrast mechanism depresses the educational aspirations of students with high-achieving peers. We study two entire cohorts of students in the final grade of the Swedish comprehensive school with matched information on social origin and achievements (160,417 students, 829 schools). Controlling for school fixed effects and observed characteristics of students and families, we find that the propensity to make a high-aspiring choice of upper-secondary school program is lower for students with high-achieving schoolmates, given own achievement. While theoretically interesting, the effect is small compared to that of own achievement: Moving an average student from an average school to a school that lies one standard deviation lower in achievement increases the probability of a high-aspiring choice by three percentage points.

There is a growing body of literature on the effects of social contexts on various individual outcomes. Much of this interest has focused on social-influence effects, normally assuming that processes of identification and interaction make people more similar. A major area of research reflecting this trend is that on children’s education, where the importance of peers for students’ achievement has received particular attention. Although schooling outcomes are likely to be much more influenced by characteristics of the family of origin than by characteristics of peers (Duncan, Boisjoly and Harris 2001), studies report non-negligible effects of various peer characteristics on achievement (Erikson 1994; Hanushek et al. 2003; Willms 1986).

Several mechanisms may underlie what is often termed peer effects: peers’ characteristics can affect the learning environment in school; peers can help and inspire each other; and peers’ parents and siblings may provide role models and information. The theoretical underpinnings can

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be found in didactic theory on learning processes, in economic theory on educational production functions, and in sociological theories of socialization and norm setting. In the present article, we shed light on an overlooked mechanism suggested by reference group theory, namely the social contrast mechanism. We define social contrast effects as a particular type of social-influence effect that explicitly builds on individuals’ conscious or sub-conscious comparisons with others, in our case in estimating their own academic capacity. We argue that, when making decisions about their school career, students judge their own probability of success at higher levels of education not only from their own achievement but also from their achievement relative to peers, and that this contrast influences educational choices.

We believe that studying social-influence effects on educational choice is timely, as recent studies of peer effects in school have focused on achievement, mostly using grade point averages or test results as the outcome. Even if achievement is a strong predictor of educational choice, each affects educational attainment independently of the other, meaning that analyses of choices are essential to understanding the full range of social-influence effects on education. Critically, while both standard theory and previous research suggest that the direction of peer effects on educational achievement is positive, this is not necessarily true for educational choice. In fact, we envisage both positive and negative effects. On the one hand, students may be inspired by choices of schoolmates or feel normatively pressured to make choices similar to theirs. Because high-achieving peers make more high-aspiring educational choices, this would imply a positive effect of peers’ achievement on the probability of making such a choice.

On the other hand, for students at any absolute achievement level, their relative achievement is lower when peers’ achievement is higher: Of two students with identical achievements, the one with low-achieving schoolmates may be among the best in school, while the one with high-achieving schoolmates may be below the school average. The social contrast mechanism suggests that this relative achievement has an independent effect on educational choices. If students take their schoolmates as a point of reference when estimating their own academic capacity, peers’ achievement will have a negative effect on the probability of making high-aspiring choices.

Our basic aim here is to analyze whether peers’ achievement has a positive or negative effect on students’ probability to choose a high-aspiring educational program at upper secondary school, controlling for an individual’s own achievement. By controlling for own achievement, the effect of peers’ achievement indicates the influence of relative achievement, because we then compare the educational choices
of students who are at the same achievement level, but who find themselves in different “achievement contexts.” A subordinate aim is to analyze whether peers’ achievement has a similar influence on students at different levels of own achievement, that is, whether there is any evidence of an interaction effect between own and peers’ achievements. It is possible, for example, that those at very high or very low achievement levels are less influenced by their peers.

We study the case of Sweden, where differentiation at lower levels of education is low and the choice of an academic program at upper secondary school is relatively unconstrained, but important for students’ subsequent educational careers. An unusual strength of our analysis is that we have access to a large-scale, high-quality data set, comprising two entire cohorts of Swedish 9th graders who left comprehensive school in 1998 and 1999. These data enable us to address identification problems, of utmost importance in estimating social-influence effects, by fitting school fixed-effect models.

Theory and Previous Research

Following the Coleman report (Coleman et al. 1966), the public policy interest in school effects and theoretical developments in reference group theory (e.g., Kelley 1952; Merton 1957) coincided, spawning a large body of literature about social-influence effects on educational achievement and aspirations during the 60s and 70s (e.g., Alwin and Otto 1977; Bain and Anderson 1974; Davis 1966; Meyer 1970; Nelson 1972; Sewell and Armer 1966). The recent upsurge in research on social-influence effects, in particular the study of peer effects (e.g., Hanushek et al. 2003; Hoxby 2000; Sacerdote 2001; Winston and Zimmerman 2004), has not connected to this tradition. In fact, as distinct from older research, more recent studies have normally focused on only one aspect of reference group theory: the presumption that identification and interaction make people more similar.

Another feature of recent studies of peer effects in schools is that they are almost exclusively concerned with educational achievement, such as GPA (e.g., Hanushek et al. 2003; Hoxby 2000; Zimmer and Toma 2000). Thus, the actual term “peer effects” is increasingly shifting toward denoting peer effects on achievement. Generally, the findings show that students’ own achievement tends to be positively affected by high-achieving peers. The theoretical mechanisms invoked are, e.g., that high-achieving peers can set norms, inspire and transfer skills to other students, and that they can affect the instructional quality in school, the pace at which teaching proceeds, and the level of the curriculum. While this research question certainly is relevant, it is just part of the story. At least in a life-chance perspective, educational attainment – indicated by
grade level completed, exams, diplomas or other formal qualifications – is the key outcome, and achievement is not the only factor affecting educational attainment. In a system where *educational choices* are an important determinant of attainment, we must also ask what effect peers’ achievement has on such choices. Of course, the degree to which choices are essential to attainment varies between educational systems (something we will discuss in light of the Swedish case below), but most if not all systems include choice as a central part of the transition to the upper secondary level of education.

Consider a heuristic model of educational choice, where $U$ is the utility from a given alternative, $C$ is the costs connected with the alternative, $B$ is the benefits, and $P$ is the probability that the student will succeed (Erikson and Jonsson 1996). Individuals do not calculate these with any exactitude – the model assumes only that different alternatives can be ranked. Benefits and costs are measured in the same unit (ultimately psychological) and should be interpreted in a broad sense. To simplify the model, we make the assumption that if the individual fails to complete her studies, then benefits become zero, while the costs remain at $C$. The expected utility of alternative $i$ then becomes:

$$U_i = P_iB_i - C_i$$

That is, a student estimates the costs of, say, choosing a high-status educational program including economic and non-economic costs (such as length of study, tuition fees, the effort needed) and the corresponding benefits (such as expected earnings and non-monetary rewards). As most of the costs will occur during the studies, but most of the benefits will hinge on the successful completion of studies, benefits are down-weighted with an estimate of the likelihood of completion. As a consequence, indicators of students’ academic potential become important for educational choice, but choices are also influenced by other factors, such as the families’ ability to cover the costs of schooling.

Structural constraints stemming from the organization of education, such as strong variants of tracking or of outright quotas for admissions, may in some school systems ‘short-circuit’ educational choice and make a model such as the one presented here less relevant, at least for low-achieving students. Naturally, there is also a choice restriction in that alternative $i$ is considered only if individuals see it as open to them, which means that actual (or perceived) constraints may affect not only admissions, but also choices. As is explained below, since the 1970s, the Swedish school system has been based on the principle of choice, and while educational choices are certainly influenced by socioeconomic and cultural factors, they are nonetheless real. In addition, the choice of
study program at upper secondary school is of critical importance to the individual’s future occupational career.

The above model can be used as a general tool for understanding educational choice. For our purposes, it is crucial to ask how peers’ achievement may influence the estimation of (1) for a given individual. We turn to reference group theory to shed light on these processes. First, peers can contribute to setting norms and positively influence each other’s aspirations, what we can term the normative function of reference groups (cf. Kelley 1952). In a context with high-achieving peers, one may feel pressured or inspired to make choices similar to theirs. In relation to Model 1, this could be understood as increasing the net gain for alternatives that are similar to those of peers (we could also conceive of it as dissimilar choices incurring a cost for the student).

Second, however, classical reference group theory also stresses the comparative function of reference groups (Festinger 1954; Kelley 1952), or relative deprivation/relative gratification (Merton 1957; Stouffer et al. 1949), by which peers serve as a point of comparison. Davis (1966) applied this theory to the college decisions of undergraduate men, stating that being with high-achieving peers may have a negative effect on these decisions. This could happen if, in estimating their chances of succeeding at higher levels of education (P in Model 1) and the effort required to do so (C in Model 1), pupils relate their own achievements to those of their peers, and make educational choices on the basis of their relative achievement. Davis (1966) thus contended that it might be better to be a “big frog in a small pond” than the other way around. Because students of similar ability may be regarded differently in a high-achieving than in a low-achieving context also by others (teachers, peers, parents), a negative effect of peers’ achievement may not only reflect students’ own adjustment of their academic self-image, but also a reaction to the fact that others treat them differently owing to the same mechanism.

A negative effect of peers’ achievement can also be expected from findings about contrast effects on human perception in other areas. The human brain is inherently sensitive to contrasts, comparison and context. For example, to use an analogy from optics, how bright an object appears to be varies with how bright the objects around it are (Chubb, Sperling and Solomon 1989). Also, subjects’ ratings of their own physical attractiveness have been shown to be negatively related to the attractiveness of people in the respective reference group (e.g., Wade 1997). Sociologists’ notion of comparative reference groups can thus be seen as an instance of a more general contrast mechanism by which our perception of characteristics of an object depends on the standard set by surrounding objects. To allude to this more general mechanism, we will use the concept social contrast effect rather than “frog-pond effect”
or “comparative reference-group effect” when referring to the potentially suppressing effect of peers’ grades on students’ educational choices. These concepts, however, refer to the same kind of mechanism.

Given the possibility of diverging effects, the overall direction of peer effects on educational choices is not evident. Students may benefit, in achievements and aspirations, from the stimulation and norms set by high-achieving peers, but simultaneously suffer from comparing themselves with the same peers. While recent research has generally assumed only the first mechanism, we also wish to shed light on the second. We do this by studying whether there is a social contrast effect on the choice of an academic program at upper secondary school, controlling for own achievement.

Existing research addressing similar questions is inconclusive. Davis (1966), Drew and Astin (1972), Meyer (1970), and Nelson (1972) have found support for the presence of social contrast effects on educational choice, while Bassis (1977) failed to find such effects. These early analyses, however, often draw on local or small data sets and are methodologically not particularly sophisticated by today’s standards. As noted above, there is little recent research on the topic, but Erikson (1994) found a social contrast effect on educational choice, and others (see Marsh and Hau 2003 and references therein) have found a similar effect on academic self-concept.

One unresolved question is how social contrast effects, if they can be verified, operate for pupils at different achievement levels. Again, there are no evident theoretical clues on this matter. Previous research has suggested that high-achieving students have a stronger sense of school membership (Smerdon 2002), which could imply that these students are more sensitive to within-school comparisons. However, it is also possible that pupils with very high or very low achievements feel so distant from the school average that they cease to identify with most of their schoolmates (cf. Festinger 1954). There exists, to our knowledge, no research concerning such interaction effects on educational choice, and the results addressing related outcomes are limited and inconclusive. Results in Marsh and Rovee (1996) suggest that social contrast effects on academic self-concept appear across the board, i.e., for both high and low achievers, but Coleman and Fults (1985) reported such effects at the lower but not the higher ends of the achievement distribution. As regards peer effects on achievement, some results suggest that they are asymmetric (Sacerdote 2001; Winston and Zimmerman 2004; Zimmer and Toma 2000; Zimmerman 2003), though the patterns are not consistent across studies. Other results suggest that peer effects on achievement are similar for high and low achievers (Hanushek et al. 2003). The latter could also be related to the finding that the effect of school socioeconomic status on achievement appears to be the same for advantaged and disadvantaged
students alike (e.g., Coleman et al. 1966; Rumberger and Palardy 2005). We have no grounds for a specific hypothesis here, but will address the issue by testing for an interaction effect between peers’ achievement and own achievement on the choice of educational program.

We believe that the analysis of peer effects in education can usefully reconnect to theories of social contrast, because many of the problems of older empirical research can now be overcome. The increasing availability of longitudinal large-scale data and the progress in methods and computational capacity to analyze such data mean that today’s researchers are in a better position to test these arguments empirically. We ourselves rely on a data set that enables us to address not only questions of generalizability and robustness, but also the notorious identification problem of separating social-influence effects from selection effects.

The Institutional Context and the Swedish School System

As we have noted, the significance of educational choice for school transitions depends on how stratified (selective and differentiated) a school system is. First, highly selective systems – in which there are a restricted number of places in higher education – create structural constraints on admission. Selection in such systems is commonly based on achievement, sometimes combined with quotas for students from different schools, which means that those who exhibit poor performance in tests or get low grades have less choice. Second, structural constraints can also be produced by a differentiated elementary and lower secondary school system, often in combination with selection. The use of ability grouping and tracking may either formally foreclose the choice of academic study programs for low-achieving pupils, or informally gear them towards specific programs or schools at higher levels by reducing their estimated probability of success or by creating the illusion of structural barriers (e.g., Gamoran and Mare 1989; Hallinan 1996; Oakes 1985).

The Swedish school system used to be selective, with a decisive transition at ages 11-13, as in many European countries. However, following the comprehensive school reform in the 1960s, elementary and lower (junior) secondary schools were gradually merged into a “true” mixed-ability comprehensive school for children 7-16 years of age, which then became separated (organizationally and physically) from the upper secondary school (the Gymnasium). In our cohorts, formal tracking was prohibited and informal tracking discouraged at comprehensive schools, and while instructional grouping may still occur on a local basis, it is much less prevalent than in many other countries, such as the United States. In Sweden, public schools dominate heavily – only 2 percent of pupils in our cohorts went to another type of comprehensive school – and pupils are almost exclusively allocated
to the geographically closest school. This, in combination with mixed-ability classes and little selectivity, tends to magnify variation in skills and aptitude among students within schools, though residential segregation in the big cities works in the opposite direction.

Soon after the Swedish comprehensive school was introduced, the transition to upper secondary school (ages 16-19) changed. The old system was based on selection through examination and offered few programs. In the new system, vocational education is integrated, and the upper secondary school contains about 20 programs. The system is largely demand-driven, in that the municipalities are obligated by law to “consider the students’ choices” when deciding on the number of places available in the different programs each year. However, students are not guaranteed a place in the program of their choice, as municipalities can face limitations regarding availability of teachers or appropriate classrooms. In the case of a shortage of places, selection of students is based on the sum of the final grades in the 16 best subjects (meritvärde). A student competes with all other students in the catchment area, and no specific number or proportion of places is reserved for students from specific comprehensive schools.

Not only are choices likely to be of different significance in different educational systems, but factors that affect educational choice may vary in importance depending on the institutional context, meaning that social-contrast effects may be more important for choices in some educational systems than in others. One important factor in this respect is how students’ achievements are measured and reported by schools. Grades are the schools’ signal about academic potential and are therefore central to the assessed probability of success at more demanding educational programs. If a student’s grades are easily compared with those of students in other schools (e.g., standardized at a national level), it is likely that within-school comparisons and social contrast effects are relatively weak. The same would hold true if there were an “objective” ranking of one’s own school in relation to other schools, e.g., by means of standardized tests (cf. Bassis 1977). In the Swedish system, grades (given only in school years 8 and 9) are teacher-assigned and criterion-referenced, i.e., teachers do not “grade on the curve.” While the grading is based on a common curriculum and central guidelines, grades are not standardized. This means that we can expect social-contrast effects to exist, though they may be offset to some extent by publicly available school-level results of standardized tests in a few important subjects.

In conclusion, the Swedish school system is characterized by a low degree of differentiation and selection. The comprehensive school is based on mixed-ability classes and transition to the upper secondary school is primarily based on choice, i.e., not pre-determined or institutionally arranged in a way that is typical for systems with strong sorting or
Selection. Although all programs are formally accessible for almost all pupils, grades are important for the choice of an academic program, no doubt mainly because they reflect the school’s signal about a student’s probability to succeed. It is in this context that peers’ achievement may influence a student’s choice, either positively via normative influence and inspiration, or negatively by reflecting a student’s relative achievement, which arguably could be consequential for estimation of the likelihood of managing demanding educational programs.

In a choice-driven educational system such as the Swedish one, the social contrast mechanism in focus here is likely to be particularly relevant. In systems where choices are more constrained, however, students’ relative achievement may nonetheless affect educational attainment negatively. For example, the use of school or class rank for admissions to higher levels of education (Attewell 2001; Espenshade, Hale and Chung 2005), or for track placements or ability groupings (Sørensen 1987; Sørensen and Hallinan 1984), means that students with a given grade sum may face better educational opportunities in a low-achieving context. Thus, a negative effect on attainment of peers’ achievement can be the product of different mechanisms, depending on features of the school system.3

Data

Our data set includes information on all pupils (about 193,000 individuals) who attended the ninth grade in all Swedish comprehensive schools in 1998 and 1999 (1,249 schools). Information on individual pupils’ grades and the school they attended comes from an official school registry of 9th graders (Åk9-registret), while data on their choice of program at upper secondary school (ages 16-19) were matched from another school register (sökande/intagna gymnasieskolan). This whole data set, in turn, was matched with registry data on parents and households, mainly from the 1980 and 1990 censuses.4 Thus, data on pupils’ achievements and educational choice were combined with information about basic characteristics of their families, such as their socioeconomic position.

We excluded private schools and also very small schools in which the total number of 9th grade students in a particular year was less than 20 (or less than 50 summing over our two years). Both private and very small schools are often special in terms of student intake or other characteristics (e.g., they include schools for students with special needs). In addition, we excluded (1. students with missing values on any of the included variables, (2. students in schools in which a high proportion of students had missing data on choice of program at secondary school, (3. students in schools that did not exist in 1998 or 1999. These selections reduce the number of students to 160,417 and the number of schools to 829. Thus, the resulting
data set includes 80,000 students per year in 829 schools; the students are different, but the schools are the same across years.

Variables

Our outcome variable is the choice of an academic program rather than a vocational program at upper secondary school. This division, which splits the student population roughly in half, is a watershed when it comes to the probability of continuing to tertiary education in Sweden (Breen and Jonsson 2000), largely because the academic programs attract students with clearly higher prior achievements than do the vocational programs. This variable gives the program chosen according to the student’s application, regardless of whether the student was admitted to this program or not.

Grades are available in digital form only in the 9th and final year of comprehensive school. In each of close to 20 subjects, one out of four grade levels is assigned by the teacher: fail, pass, pass with distinction, and pass with special distinction (these are then assigned 0, 10, 15 and 20 points, respectively). The variable we use (”own grades”) in measuring individual achievement is the sum of the year 9 grades in the 16 best subjects (meritvärde). Grade sums can, and do, vary between 0 and 320 points, with the average being just over 200 points. This is shown in Table 1, which also gives descriptive statistics for the other variables included in the analysis.

Our main independent variable, indicating peers’ achievement, is the yearly average grade sum among other 9th graders in the school (i.e., excluding the student’s own grades). For short, we will refer to this variable as school average grade. Because we have data for two years for each school, this variable varies within schools (apart from the small variation induced by subtracting individuals’ own grades). Figure 1 shows the distribution of average grades. Out of this variation, 78 percent is between schools, and 22 percent is between years within schools. Both grade variables, i.e., own grades and school average grade, are used in z-standardized form (mean=0, std. dev=1) in the analyses.

The dataset includes information on a number of relevant pupil and parental, or household, characteristics (see Table 1). Sibship size is the number of siblings (biological or not) living in the respondents’ household. Family type is divided into intact families (children living with two biological or adoptive parents), reconstituted families (one biological/adoptive parent and one step-parent), single-parent families and children living without any parent (in special homes or with other adults). The parents’ level of education is the highest (considering both the father and the mother) out of seven levels of education achieved, according to a Swedish standard
classification (Statistics Sweden 1988). Social welfare and parents’ experience of unemployment are both dummy variables coded as 1 if at least one of the parents obtained social welfare or unemployment benefits, respectively, during the year the student left compulsory school. As social welfare is given on a household basis, this variable indicates household poverty. Parental unemployment rates appear high in Table 1, partly because unemployment was high in the 1990s, partly because we normally sum over two parents, and also as the measure includes partial and temporary unemployment.

Pupils’ immigrant status is measured by two binary variables. The first distinguishes (first generation) immigrants and is given the value 1 if the pupil and both his/her parents were born abroad. The second variable is given the value 1 if the pupil was born in Sweden, but both his/her parents were born abroad (second generation immigrants).
Identifying Social Contrast Effects

In analyzing peer effects on educational choice, we are only concerned with estimating the relation between peers’ achievement and the choice of academic program *net of own achievement* (i.e., grade sum). Peers’ achievement can affect the choice of post-compulsory education in two ways: (1) directly by affecting the standard of comparison, norms and aspirations, and (2) indirectly by affecting own achievement. Because the first effect is the mechanism of interest here, we have to control for own achievement in order to obtain unbiased estimates.

It would have been desirable also to estimate the *combined* effect of both mechanisms and the relative contribution of each to this. The data used here, however, do not allow estimation of the indirect effect, and thus not the total effect, because the individual student’s own grades and the school average grade are simultaneously determined. Because the school average grade is just an aggregation of individual grades, individual grades will, on average, by necessity be higher in a year when the school average is higher (cf. Manki’s 1993 “reflection problem”).

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**Figure 1. Distribution of School Average Grades**

![Histogram of School Average Grades]

Notes: Vertical lines at one-standard-deviation intervals. (N = 1,658)
The problem of unobserved, or omitted, variables is vital when one wishes to estimate causal effects of the social environment (such as neighborhoods or schools) on individual outcomes (see Manski 2000; Hanushek et al. 2003 applied to peer effects). For our purposes, we must consider unobserved variables of two kinds. First, we have the problem of population sorting. This implies that *unobserved student and family characteristics* (such as economic resources or parental aspirations), on the basis of which families sort themselves to residential areas and thereby to schools, could be related both to school average grade and to students’ educational choices. Second, schools with high average grades may be schools with other *unobserved school characteristics*, such as active counseling or committed teachers, which affect students’ educational choices net of achievement.

If the true peer effect of interest is positive, omitted variable bias is likely to lead to inflated estimates of this effect, because schools with higher average grades are likely to attract more qualified teachers, as well as students with high ability and aspirations. In contrast, if the true effect is negative, such as the social contrast effect in focus here, omitted variable bias will most likely lead to underestimated effects.

While our data permit us to control for a set of empirically relevant and well-measured variables, it is our ability to apply what is generally (particularly within econometrics) known as a school fixed-effects model that is crucial to reducing omitted variable bias. We profit here from the fact that there is variation within schools between years in peers’ grades. Essentially, the fixed-effect models test whether students with given grades and given socio-economic characteristics *within a given school* have a lower propensity of choosing an academic program a year when the average grade among 9th graders in this school are higher. As a consequence, all bias due to factors that are common to both cohorts in a school is eliminated. This applies to unobserved school variables (such as school quality) as well as population sorting that leads to differential composition of the student population. For example, if schools with a good reputation tend to attract more high-aspiring students, this will be captured by the model as long as between-school differences in reputation are roughly invariant over the two years.8

We use binary logistic regressions (command *logit* in Stata 9) for the analyses. The school fixed-effect models are estimated using so-called unconditional fixed-effect models (i.e., by including a dummy variable for each school).9 All standard errors are clustered (option *cluster* in Stata 9) to correct for correlation between observations within the same school and year. However, because the data do not come from a random sample, but from an entire population fulfilling certain criteria, standard errors and significance tests cannot be interpreted in the traditional manner;
Table 2: Logistic Regression of the Choice of Academic Program at Upper Secondary School

<table>
<thead>
<tr>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
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</thead>
<tbody>
<tr>
<td><strong>School average grade (std)</strong></td>
<td>-.037</td>
<td>-.125</td>
</tr>
<tr>
<td>(2.49)*</td>
<td>(7.38)***</td>
<td>(7.37)***</td>
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<tr>
<td><strong>Students’ own grades (std)</strong></td>
<td>1.817</td>
<td>1.928</td>
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<tr>
<td>(103.45)***</td>
<td>(106.88)***</td>
<td>(107.03)***</td>
</tr>
<tr>
<td>*<em>School average grade(std)<em>own grades(std)</em></em></td>
<td></td>
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</tr>
<tr>
<td></td>
<td></td>
<td>(.70)</td>
</tr>
<tr>
<td><strong>Year 1999 (1998 ref)</strong></td>
<td>-.179</td>
<td>-.184</td>
</tr>
<tr>
<td>(6.69)***</td>
<td>(15.66)***</td>
<td>(15.68)***</td>
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<tr>
<td><strong>Sibship size</strong></td>
<td>-.058</td>
<td>-.040</td>
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<tr>
<td>(11.72)***</td>
<td>(8.00)***</td>
<td>(8.01)***</td>
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<tr>
<td><strong>Woman</strong></td>
<td>-.258</td>
<td>-.283</td>
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<tr>
<td>(15.81)***</td>
<td>(16.74)***</td>
<td>(16.77)***</td>
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<tr>
<td><strong>Immigrant first generation</strong></td>
<td>.852</td>
<td>.773</td>
</tr>
<tr>
<td>(25.96)***</td>
<td>(24.00)***</td>
<td>(24.02)***</td>
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<td><strong>Immigrant second generation</strong></td>
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<td>.517</td>
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<tr>
<td>(18.15)***</td>
<td>(13.97)***</td>
<td>(13.98)***</td>
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<td><strong>Parents’ unemployment</strong></td>
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<td>-.099</td>
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<td>(10.35)***</td>
<td>(6.17)***</td>
<td>(6.16)***</td>
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<td>-.048</td>
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<td>(1.72)</td>
<td>(1.79)</td>
<td>(1.79)</td>
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<tr>
<td><strong>Parents’ Education</strong></td>
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<tr>
<td><strong>Short compulsory</strong></td>
<td>-.182</td>
<td>-.192</td>
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<tr>
<td>(4.43)***</td>
<td>(4.56)***</td>
<td>(4.57)***</td>
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<tr>
<td><strong>Compulsory</strong></td>
<td>-.145</td>
<td>-.133</td>
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<tr>
<td>(5.77)***</td>
<td>(5.22)***</td>
<td>(5.22)***</td>
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<tr>
<td><strong>Lower secondary</strong></td>
<td>Ref.</td>
<td>Ref.</td>
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<tr>
<td><strong>Upper secondary</strong></td>
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<td>(17.92)***</td>
<td>(14.08)***</td>
<td>(14.07)***</td>
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<td>(27.05)***</td>
<td>(27.06)***</td>
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<td><strong>University degree</strong></td>
<td>.935</td>
<td>.744</td>
</tr>
<tr>
<td>(42.80)***</td>
<td>(36.46)***</td>
<td>(36.50)***</td>
</tr>
<tr>
<td><strong>Postgraduate</strong></td>
<td>1.497</td>
<td>1.144</td>
</tr>
<tr>
<td>(18.93)***</td>
<td>(14.43)***</td>
<td>(14.40)***</td>
</tr>
<tr>
<td><strong>Family Type</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Intact family</strong></td>
<td>Ref.</td>
<td>Ref.</td>
</tr>
<tr>
<td><strong>Reconstituted family</strong></td>
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<td>-.036</td>
</tr>
<tr>
<td>(.18)</td>
<td>(1.38)</td>
<td>(1.38)</td>
</tr>
<tr>
<td><strong>Single parent</strong></td>
<td>-.007</td>
<td>-.075</td>
</tr>
<tr>
<td>(.44)</td>
<td>(4.62)***</td>
<td>(4.61)***</td>
</tr>
<tr>
<td><strong>Alone/institution/other</strong></td>
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<td>-.015</td>
</tr>
<tr>
<td>(.11)</td>
<td>(.22)</td>
<td>(.21)</td>
</tr>
</tbody>
</table>
they do not provide grounds for inference from a sample to a population. Instead, standard errors should be taken as indicators of the variability of the estimated effects.\textsuperscript{10}

Results

Table 2 shows the results from three logistic regressions on the choice of an academic program at upper secondary school.\textsuperscript{11} In Model 1, selection to schools is addressed by controlling for observed characteristics of students and their families. Model 2 additionally includes school fixed effects. This is the model of central theoretical interest, in that it tests for the presence of contrast effects while strongly reducing the risk for bias caused by unobserved school characteristics and unobserved heterogeneity in student composition. Model 3 adds an interaction term between students’ own grades and the school average grade, to test whether any contrast effect found in Model 2 varies by students’ own grade sum.

It should be noted that all models include students’ own grades, which is a powerful predictor of academic program choice. Even controlling for individual achievement, however, some characteristics of parents and the household make a difference for educational choice. Consistent with a great deal of the previous research, parents’ education is positively associated with the more high-aspiring academic choice, whereas parental unemployment and number of siblings tend to depress such aspirations. The absence of any substantial effect of family type and the positive effect of being (first- or second-generation) immigrant also replicates previous findings (Erikson 1994), and must be seen in light of the extensive controls, particularly students’ own grades.

For addressing the social contrast mechanism, we turn our attention to the coefficient for school average grade, reported in the upper row of Table

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>School fixed effects</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Constant</td>
<td>-.019</td>
<td>.187</td>
<td>.188</td>
</tr>
<tr>
<td>(7.4)</td>
<td>(3.72)**</td>
<td>(3.74)**</td>
<td></td>
</tr>
<tr>
<td>Number of schools</td>
<td>829</td>
<td>829</td>
<td>829</td>
</tr>
<tr>
<td>Number of individuals</td>
<td>160,417</td>
<td>160,417</td>
<td>160,417</td>
</tr>
<tr>
<td>McFadden’s $R^2$/ Efron’s $R^2$</td>
<td>.312 / .381</td>
<td>.339 / .410</td>
<td>.339 / .410</td>
</tr>
</tbody>
</table>

Notes: *p < .10  **p < .05  ***p < .01
Log-odds ratios, absolute value of robust z-statistics in parentheses. Z-statistics robust to correlation within school*year.
In all models, this coefficient is negative and statistically significant at least at the 5 percent level. Thus, it does seem to be the case that an individual’s relative achievement matters for educational choice, which supports the social contrast theory. In Model 1, the log-odds ratio for a one-standard-deviation change in the average school grade is -.037. When school fixed effects are added in Model 2, this coefficient is substantially strengthened, to -.125. This is to be expected, because a high average school grade (i.e., high peer achievements) is likely to be correlated with other school characteristics that provide grounds for the sorting of high-aspiring students into these schools. That is, students who go to these schools are likely to have unobserved initial characteristics (e.g., aspirations) that make them more likely to choose an academic program in upper secondary school. The presence of such a selection bias partly suppresses the negative social contrast effect when school fixed effects are not included.12

To get a sense of the substantive effect implied by the coefficient -.125, we can translate this log-odds ratio to a change in probability evaluated at the average probability of choosing an academic program (.520, i.e. 52 percent). The logit that corresponds to this probability is ln(.520/(1-.520)) = .080. A one-standard-deviation increase in the school average grade would decrease this logit by .125 to -.045, which translates to a new probability of exp(-.045)/(1+exp(-.045))=.489. Subtracting .520 from .489 gives -.031, which means that a one-standard-deviation increase in the school average grade is, for students with a baseline probability around average, associated with a decrease in the probability of choosing an academic program by 3 percentage units.13 As can be expected, the effect of peers’ grades is small relative to the effect of students’ own grades. A one-standard-deviation increase in a student’s own grades changes the probability of choosing an academic program from .52 to .88, but this very large effect must be seen in light of the fact that the standard deviation for students’ grades is almost four times as large as for school average grades (58 vs. 15 points, see Table 1). If we instead, then, calculate the effect of a 15-point change in a student’s own grades, we get a probability change of .12, that is, four times as large as the effect of a 15-point change in school average grade.

The logistic functional form implies an inherent non-linearity in the estimated effects on the probability of choosing an academic program, where the effects are strongest for those students who have probabilities around .5 (50 percent). For students with a corresponding probability of .2 (or .8, as the curve is symmetrical), the estimated decrease in probability for a one-standard-deviation difference in the school average grade is 2 percentage units (calculated as above). As students’ own grades are a central determinant of the probability, we essentially have a built-in interaction between students’ own grades and peers’ grades.
insofar as our concern is with effects in terms of percentage units: The further the student’s grades lie from the overall average grade, the further the student moves from the point in the probability distribution where the effect is strongest. This non-linearity is obvious in Figure 2, which shows the relation between the school average grade and the predicted probability (from Model 2) of choosing an academic program, for students with different grades (and with other variables held constant at average or modal values). The line for students with average grades (own grade 201) has a clearly steeper slope than the lines for students with grades at one standard deviation below (own grade 143) or above (own grade 259) average. Recall that these differences in slopes are entirely due to the non-linear functional form.

Figure 3 shows the same results from a different angle, giving the probability of choosing an academic program by one’s own grades for students with different average school grades. First, the steep slopes
convey the central importance of students’ own grades. Second, the distances between the curves reflect the impact of school average grade. The curves are shown for students with school grades at average (199) and one standard deviation above and below average (214 and 184). In addition, to facilitate comparison with Figure 2, curves are shown for students with average school grades at 143 and 259 (the outer pair of curves). Observe, however, that for school average grade (as opposed to students’ own grades), these are extremely low and high values, at or even beyond the limits of our observed data.

In Model 3 of Table 2, we study whether there are interaction effects over and above the ones captured by the functional form. By adding the interaction between school average grade and students’ own grades we test whether the estimated log-odds for the school average grade is the same for pupils across the grade distribution. The interaction effect is positive, meaning that there is a tendency for high-achieving students to
be somewhat less negatively influenced by peers’ grades. The effect is small, however, and far from being statistically significant at conventional levels: for a student whose own grades and school grade are at one standard deviation above average, the coefficient represents a change in the probability of choosing an academic program of only .002, or .2 percentage units. This supports the view that social contrast effects, as measured in log-odds, are of very similar magnitude for students at different levels of achievement.¹⁴

Conclusion and Discussion

We ask whether peer effects on educational choice, for students at given achievement levels, are positive or negative. In particular, we address the hypothesis that a social contrast mechanism leads pupils with high-achieving peers to depress their educational aspirations. We analyze population data for two cohorts of students in the final grade of the Swedish comprehensive school, with matched information on social origin and school achievements. In studying the crucial choice of an academic program at upper secondary school, we employ binary logistic regression with school fixed effects to control for unobserved heterogeneity. The results support the social contrast theory: We find that the tendency to make a high-aspiring choice at upper secondary school is less for those who go to schools with high-achieving peers, controlling for own achievement. The non-linear functional form implies that the substantive effects, i.e., effects measured in terms of unit changes in the probability, are largest for students with average grades. Apart from this built-in non-linearity, our results suggest that social contrast effects are of equal importance throughout the grade distribution.

Recent research on social-influence effects has focused on how such effects cause people to become more similar, and studies of peer effects in school have reported positive effects of high-achieving surroundings on achievement (e.g., Hanushek et al. 2003; Hoxby 2000; Zimmer and Toma 2000). While such studies are very important, they neglect the fact that the more important outcome of schooling, educational attainment, is not necessarily affected in the same way. By highlighting a mechanism that operates in the opposite direction, we have reconnected to classical reference group theory, which states that peers’ characteristics can have diverging effects that partly offset each other. Therefore, our results contribute to the understanding of the complex processes through which the social context affects educational outcomes. A corollary of policy interest is that previous research on peer effects on achievement overestimates the potential equalization of educational attainment that could be achieved by desegregation.
While we wish to stress the theoretical significance of our results, we do not wish to overestimate the practical importance of the social-contrast effect that we find: Moving to a school with an average achievement level that is one standard deviation lower than one’s present school means (for an average student in an average school) an increase in the probability of choosing a more demanding academic track of 3 percentage points (from 52 to 55 percent). This is a small effect relative to the effect of a student’s own achievement. As has been emphasized before for other types of so-called contextual effects, this social contrast effect is also weak compared with effects of parental characteristics. However, while many students change schools and many more have the opportunity to do so, changing parents or their characteristics is far more difficult.

Importantly, our results should not be taken as evidence that moving to a school with lower achievement will increase the chance of reaching higher levels of education. Such a conclusion cannot be drawn without knowledge of the size of the gross effect of peers’ achievement, meaning that the positive effects that high-achieving peers probably have on one’s own achievement must be considered too – and given how important one’s own grades are for educational choice, even small peer effects on achievement are potentially effective. The direction of the gross effect will depend on whether such a positive effect is strong enough to outweigh the negative social-contrast effect. We believe, like most parents no doubt do, that the gross effect is indeed positive, but our results suggest that it may be less so than what is normally assumed.

While our results contribute to the bulk of research indicating the existence of social influence in a general sense, and to peer effects in school in particular, it must be emphasized that they refer to the Swedish case and may not be generalizable to other cases. As discussed above, social contrast mechanisms are likely to operate with varying force in different institutional settings. We suspect that the Swedish educational system is conducive to social contrast effects in that the transition to upper secondary school is based on choices that are relatively unconstrained. In addition, grades are not easily compared across schools, although school rankings based on centralized tests may hamper the weight given to within-school comparisons. Interestingly, students’ relative achievement could be of consequence also in a system that is more stratified, because ranking may be vital for admission to higher education (e.g., Espenshade et al. 2005); this may be seen as an indirect peer effect, but one that does not involve a social contrast mechanism working through students’ assessment of their academic capacity.

To conclude, our results suggest the need for a wider theoretical framework in assessing peer effects in schools, including social contrast effects, and they also leave some interesting threads to be taken up by
future research. For example, one challenge is to model and estimate how positive and negative effects combine to a gross effect of peers’ achievement on educational choices; another is to explore how institutional characteristics affect the operation of social contrast effects.

Notes

1. Such shortage of places at academic programs is very rare (around 90 percent of those who applied for an academic program in our cohorts were admitted to one), and this is, with few exceptions, unpredictable for the individual student. Thus, an expected shortage of places is very unlikely to affect choices.

2. For the cohorts that we study, the ruling principle was that students made the choice of a program, not of a specific school, and were (if accepted) allocated to the geographically closest Gymnasium in the catchment area (typically municipality) offering that program.

3. We are grateful to an anonymous Social Forces reviewer for pointing this out.

4. This matching, using unique personal identification numbers, is standard in Sweden, done by Statistics Sweden, and entirely accurate.

5. There are two academic programs: the science/technology program and the social science/humanities program, and about 16 vocational programs. The small proportion (around 2 percent) that leaves school is merged with the vocational group in our analysis. The exclusion of this group from the analysis makes no difference to the results.

6. In 1998, the average grade sum among students choosing academic programs was 234, as compared to 172 for those choosing vocational programs (the overall average was 203, and the range is 0-320).

7. Adopted children born abroad are defined as born in Sweden, as they normally arrive at a very young age.

8. In the fixed-effect models, we also control for observed individual-level variables. In addition, we have fitted models that control for time-varying compositional characteristics of schools by including variables measuring (for each school and year) the proportions of (1. first and second generation immigrants, (2. students with parents on welfare, (3. students with unemployed parents, and (4. students whose parents have a university degree. Including these variables does not alter the estimates of interest.

9. Unconditional fixed effects can bias effect estimates if the number of observations within units (here schools) is small (Neymann and Scott’s (1948) so-called incidental parameter problem). We have carried out the analyses using different restrictions on the within-school sample size to test whether the results are affected by this problem, and they are not.
10. Opinions vary as to whether significance tests are meaningful in the analysis of population data, but there appears to be increasing agreement that they are so (see Leahy 2005).

11. In Table 2, we show two measures of pseudo-R\(^2\). McFadden's R\(^2\) (the R\(^2\) analogue most commonly reported for logistic regression) is based on a ratio of likelihoods. Because our data are clustered (schools*years), the likelihoods are not true likelihoods and hence this measure is questionable. Efron's R\(^2\) is not affected by the nature of the likelihoods because it is based on the deviations of predicted probabilities from the observed values.

12. We are aware that coefficients cannot be straightforwardly compared across models in logistic regression. All comparisons made across models here hold when coefficients are y-standardized (Winship and Mare 1984) and when estimating the models with OLS instead of logistic regression.

13. Because of the equal distribution of the binary outcome variable under study, the use of a linear probability model (i.e., OLS) could be seen as an alternative to the logistic model. However, because we are interested not only in an average effect, but also (in Model 3) in how the effect varies with students' own grades, such a model is less appropriate. We have estimated Model 2 with OLS, and the results suggest a somewhat smaller effect (2.2 instead of 3.1 in terms of percentage units).

14. We have tested also for interaction effects with other functional forms, but none of these alternative specifications led to other conclusions.

References


