Do peers matter? Estimation of peer effects from pupil mobility between schools[1]

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This paper estimates the peer effect on an individual's educational attainment using data for pupils in the final two years of compulsory education in England. We address the problems of simultaneity and selection bias by estimating peer effects only for those pupils changing school and by using the change in test score as the dependent variable. We find that peer effects are present and are generally stronger for low and middle ability pupils than for high ability pupils. We also find that the peer effect does not differ between boys and girls but is substantially stronger for non-white boys than for other groups. We find no evidence that the individual pupil's test score gain is related to peer group heterogeneity.

Key words: Peer effects Test score Pupils Secondary school

JEL classifications: I20, I21, I28

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1. Introduction

The widespread existence of pupil sorting between schools, based on ability and family background, raises the question of whether sorting has any effect on educational outcomes. In particular, does sorting by ability affect the total educational product? And does sorting by ability have adverse effects on educational inequalities? The answer to these questions hangs on the importance of peer effects, which have been studied extensively, especially in the US, in order to discover whether it is educationally better to segregate pupils by ability or whether more satisfactory outcomes are achieved with mixed ability teaching. Interest in the effects of sorting on educational outcomes in the UK has been heightened by the adoption of greater parental choice of school, which could lead to greater sorting of pupils between schools. This suggests a need to investigate the potential consequences on educational outcomes of greater segregation of pupils between schools.[2]

The proponents of segregation argue that the learning process is more effective if pupils are grouped according to their ability since high ability pupils will not then be held back by the slower rate of progress of low ability pupils; and low ability pupils will gain since they will not be disillusioned as a consequence of lagging behind their more able peers. The teaching process may also be more efficient in more homogeneous groups since lessons can be targeted and delivered at a level more closely aligned to the group's ability distribution than is possible in more heterogeneous ability groups. Segregation can occur within schools through streaming or between schools through tracking, where schools are allowed to select on the basis of ability or some other characteristic such as aptitude for particular subjects. This kind of system has been described as elitist in the sense that segregation may lead to inequality of educational opportunity (Galindo-Rueda and Vignoles 2004).

The case for mixed ability schooling is based on the argument that low ability pupils benefit from the presence of high ability peers. As Hoxby (2000) notes, there are multiple channels through which a peer group may influence a pupil's learning. These channels include direct instruction by one pupil to another as well as indirect 'knowledge spillovers', such as the acquisition of better learning skills from their higher ability peers, and improvements in standards of behaviour in the

[2] See Burgess *et al.* (2004, 2005) for a detailed analysis of sorting in the secondary school sector in England and for a review of the literature on the consequences of greater parental choice.

classroom.[3] These potential benefits will be lost if low ability pupils are segregated from high ability pupils. There is the added danger that in a segregated system the 'best' teachers may be attracted towards schools with high ability pupils, leaving schools with low ability pupils to be taught by the 'worst' teachers (Hanushek, Rivkin and Taylor 1996). If these peer effects are substantial, a policy of mixed ability groups will result in a reduction in educational inequalities, thereby reducing the size of the educational underclass over the longer term.

Interest in estimating peer effects in education began with the Coleman Report (Coleman *et al.* 1966). This report, together with other studies that followed, suggested that peer effects were substantial (Summers and Wolfe 1977). These early estimates were subsequently deemed to suffer from serious upward bias due to problems with the estimation methods. Manski (1993, 1995, 2000) investigates the potential sources of this bias. He begins by identifying three reasons why an individual pupil's attainment may be related to the attainment of her peers. First, interaction between a pupil and her peers may give rise to endogenous effects due to classmates learning from each other through direct contact and observation. Second, pupils with similar attributes who attend the same school may be expected to make attainment gains which are correlated with those of their peers. The correlation in this case, however, would be due to pupils having common attributes and not to social interaction effects. These are referred to in the literature as contextual (or exogenous) effects. Third, pupils in the same class or school may be affected by correlated errors, such as the effects on attainment emanating from a common school environment.

Identifying the effect of peers on an individual pupil's attainment faces serious problems. Bias arises from two main sources: simultaneity bias and omitted variable bias, both of which generate a correlation between the peer group variable and the disturbance term. Simultaneity bias occurs because individuals in a peer group affect each other; peers affect the individual pupil, who in turn affects her peers. This two-way interaction results in an upward bias in the peer effect when estimated from the mean attainment of a pupil's peers. If steps are not taken to address this 'reflection problem', it is not possible to identify a causal relationship between the pupil and her peer group.

Omitted variable bias can arise from several sources. First, there is a self-selection problem. Bias occurs because pupils (or their parents) choose a peer group with similar characteristics, some of which are typically unobserved. This sorting of pupils into schools according to characteristics such as prior attainment, motivation or family background results in a potentially substantial

[3] More generally, Manski (2000) discusses three channels through which agents' actions can affect the actions of others, that is, through their effect on an agent's expectations, through the imposition of constraints and through revealed preferences.

upward bias in the estimated peer effect. Pupils with parents who are keen for their child to do well are more likely to have schoolmates who also have parents who are keen for their child to do well. High achieving pupils are more likely to have high achieving schoolmates; and conversely for low achieving pupils. The sorting of pupils into peer groups through school choice decisions will therefore result in an *automatic* positive correlation between a pupil's attainment and the attainment of the pupil's schoolmates. Second, all pupils in a peer group may benefit from an exogenous shock such as being taught by a particularly good teacher or attending a well-run school. This will raise the attainment of all pupils simultaneously in a particular school, thereby generating correlated errors for all pupils in that school. Thus, the observed positive relationship between the individual's test score and the peer groups' test score would not in this case be due to a true peer effect.

A variety of approaches have been used to overcome these problems. Needless to say, the many and varied attempts to measure the peer effect have produced different estimates of its magnitude. The results of some previous studies shown in Table 1 indicate a wide variation in the estimated peer effect, ranging from 0.05 of a standard deviation to 0.40. Some researchers have used the random assignment of pupils between schools (Hoxby and Weingarth 2005, Kang 2005) and between classes within schools (Ammermueller and Pischke 2006) to estimate the peer effect from administrative data. But such data are scarce and the more commonly-used approach is to control for selection effects by including a comprehensive set of personal, family background and school level covariates (Levin 2001, Betts and Zau 2004, Vigdor and Nechyba 2004, Schneeweis and Winter-Ebmer 2005, Ding and Lehrer 2007). Rangvid (2003), for example, shows that failure to control for a wide range of variables describing parental background, such as their educational attainment, leads to substantial upward bias in the estimated peer effect. Instrumental variables (IV) have also been used to control for the endogeneity of peer group composition (Figlio and Page 2002, Fertig 2003, Robertson and Symonds 2003). The problem with IV estimation is that appropriate and reliable instruments are often unavailable. A device used in many studies of the peer effect is to control for fixed effects (data permitting) at pupil, grade or school level in order to reduce endogeneity bias. Using pupil fixed effects is particularly important in order to control for factors such as innate ability and other personal and family characteristics. A method aimed at combating simultaneity bias is to lag the peer group variable, but this is likely to result in an underestimate of the peer effect since it fails to capture the impact of the peer group's *current* behaviour on the individual pupil (Hanushek *et al*. 2002, Betts and Zau 2004, Vigdor and Nechyba 2004, Ding and Lehrer 2007).

Previous research has focused not only on the size of the peer effect but also how it varies with the characteristics of the pupil or her peer group. The issue of greatest concern is whether the peer effect varies across the conditional distribution of pupil attainment. In other words, is the peer effect linear or non-linear? A linear relationship is neutral in its overall effect on mixed ability schools since it implies that any positive attainment effects on low ability pupils resulting from having high ability pupils as schoolmates will be exactly offset by negative attainment effects working in the opposite direction. But if the peer effect is greater for low ability pupils than for high ability pupils, the average outcome will increase as a consequence of mixed ability schooling; and conversely if the peer effect is greater for high ability pupils than for low ability pupils. Rangvid (2003) and Schneeweis and Winter-Ebmer (2005), for example, find that the peer effect is strongest for low ability pupils and weakest for high ability pupils, thus suggesting that mixed ability teaching results in a higher average outcome than a system based on segregation by ability. Ding and Lehrer (2007), however, find the opposite result for Chinese pupils: high ability pupils benefit more than low ability pupils from having high ability schoolmates.

A related issue concerns the effect of the heterogeneity of a pupil's peer group on her attainment. Do individual pupils perform better in groups with low dispersion in ability levels or in groups with high dispersion in ability levels? Previous studies investigating the effect of segregating pupils into schools by ability (i.e. 'tracking') yield contradictory results. Although much of the empirical literature suggests that tracking has little or no effect on attainment outcomes (Rangvid 2003, Hanushek *et al*. 2002, Vignor and Nechyba 2004, Schneeweis and Winter-Ebmer 2005), several studies find opposite effects. Hoxby and Weingarth (2005), for example, find that positive peer effects on pupil attainment in maths are stronger in schools with more homogeneous ability groups. Other studies that find positive attainment effects from ability tracking include those by Fertig (2003) and Figlio and Page (2002) for US schools, Levin (2001) for Dutch schools and Ding and Lehrer (2007) for Chinese schools. Other studies find that tracking is beneficial to those in high ability groups but harmful to those in low ability groups (Argys *et al*. 1996, Betts and Shkolnik 2000, Robertson and Symonds 2003); and Hanushek and Wössmann (2006) find evidence that early tracking (in primary school) is associated with greater dispersion of attainment outcomes in later (secondary) schooling without having any positive effects on the aggregate level of attainment. There is therefore substantial disagreement over whether ability tracking has beneficial effects on attainment.

This paper takes a different approach to overcoming the statistical problems arising from endogeneity bias. One of the limitations of earlier studies is that they have used a single peer group for estimating the peer effect on individual pupils. Previous studies attempting to measure peer effects in the labour market argue that more reliable estimates of the peer effect can be obtained by focusing on those moving between two entirely different peer groups as a result of a job change (Ichino and Maggi, 2000; Bradley, Green and Leaves, 2007). We adopt the same tactic here: we select from the pupil population only those *pupils who change school* during their final two years of compulsory education. This allows us to obtain more reliable estimates of the peer effect since we are using the *difference* in test scores between two entirely different and independent peer groups rather than the *change* in the test score of the *same* peer group.[4] Following the existing literature, we also include contemporaneous family and school inputs, and control for pupil level unobserved heterogeneity by adopting a fixed effects approach. To capture the effect of any remaining family and school level heterogeneity, we also include the lagged dependent variable in an alternative specification of the model which captures the historical effects of these inputs.

Our aim in this paper is to confront the following questions: To what extent, if any, is a pupil's attainment affected by the corresponding attainment of his or her peers? Does the strength of this effect vary according to a pupil's personal characteristics or family background, such as gender, ethnicity and ability (as proxied by prior attainment)? Does the peer effect vary according to the dispersion of scholastic ability in a pupil's peer group? Briefly, our main findings are that peer effects are statistically significant and quite substantial in size and that they are somewhat stronger for low ability pupils than for high ability pupils. Low ability pupils are more likely to benefit from a non-selective (comprehensive) education system while the overall effect on attainment of a non-selective system is likely to be positive. We find very little evidence, however, that a pupil's attainment is affected by the heterogeneity of a pupil's peer group. In addition, we find no significant difference in the peer effect for boys and girls, though there is evidence of a significantly bigger peer effect for ethnic minority boys than for white boys.

The remainder of this paper is structured as follows. Section 2 explains the model and estimation strategy. This is followed in section 3 by a description of the data and the variables used in the empirical analysis. The results are discussed in section 4 and section 5 concludes.

2. Model and empirical strategy

Our model is based on the education production function, which states that educational attainment is determined by personal characteristics, family background, peer effects and schooling (Hanushek 1979, 1986). A linear representation of the education production function is as follows:

$$A_{i,t} = \alpha_i + \beta A^*_{-i,t} + \lambda F^*_{-i,t} + \gamma F_{i,t} + \delta S_{i,t} + e_{i,t} \quad (1)$$

[4] Ichino and Maggi (2000) estimate the impact of peers on 'shirking' for those changing jobs in the banking sector between locations but within the same organisation. Bradley, Green and Leaves (2007) estimate the impact of peers on teacher absence by focusing on teachers who change school (and hence peer group).

A = attainment (e.g. test score)

A* = mean attainment of pupil's peer group (excluding pupil i, denoted by -i)

F* = family background of pupil's peers

F = family background of pupil

S = school-related factors

α_i = pupil level fixed effects representing unobservable characteristics

i = pupil

t = year

The aim of the empirical analysis is to estimate β. An alternative (value added) specification of the education production function includes the pupil's initial attainment ($A_{i,t-1}$), which is included to capture the influence of 'historical' factors on current attainment:

$$A_{i,t} = \alpha_i + \varphi A_{i,t-1} + \beta A^*_{-i,t} + \lambda F^*_{-i,t} + \gamma F_{i,t} + \delta S_{i,t} + u_{i,t} \quad (1a)$$

Using either (1) or (1a), however, to estimate the peer effect on an individual pupil's attainment, runs into the problem of endogeneity bias discussed earlier. Omitted variable bias can be mitigated (to some extent) in several ways. First, bias is reduced by taking first differences of (2) and (2a) since (time-invariant) unobservable pupil level fixed effects (α_i), such as innate ability and motivation, are eliminated from the model. After taking first differences, we obtain the following estimating equations:

$$\Delta A_{i,t} = \beta \Delta A^*_{-i,t} + \lambda \Delta F^*_{-i,t} + \gamma \Delta F_{i,t} + \delta \Delta S_{i,t} + \varepsilon_{i,t} \quad (2)$$

$$\Delta A_{i,t} = \varphi \Delta A_{i,,t-1} + \beta \Delta A^*_{-i,t} + \lambda \Delta F^*_{-i,t} + \gamma \Delta F_{i,t} + \delta \Delta S_{i,t} + \nu_{i,t} \quad (2a)$$

Second, the inclusion of controls reflecting the pupil's own family background and the family background of the pupil's peers will help to capture the selection process, thereby reducing the adverse effect of sorting on the estimated peer effect.[5] Local area dummies are also included to control for unobserved locality fixed effects.

Third, the estimated peer effect may be biased as a result of exogenous shocks, such as an improvement (or deterioration) in the quality of teaching in particular schools, thereby generating correlated errors since such a 'shock' will be common to all pupils. This potential source of bias is likely to be reduced if the sample is restricted to *pupils who change school* during the period of compulsory schooling since pupils who change school also change their peer group. This means

[5] Unfortunately, we only have time-variant data for the family background of the peer group (from school level data obtained from the annual Schools Census) and not for the individual pupil.

that the change in the peer group's test score relates to two entirely different peer groups, hence reducing the bias caused by correlated effects.

The most difficult source of bias to mitigate is that generated by the simultaneous interaction between peers - the so-called 'reflection problem'. Simultaneity bias should be reduced by focusing exclusively on movers since movers will have had less than two years to influence their peers compared to non-movers who have been with their peer group throughout the five years of secondary schooling. However, the reflection problem is not completely resolved because spending two years with the same group of pupils is sufficient time for bi-directional influences to occur. Thus, as a way to further reduce the impact of simultaneity bias, we use the *lagged* test scores of peers at the origin and destination schools in some further tests (as in Hanushek *et al*. 2002).[6]

Since there is also considerable interest in the effect of segregating pupils into schools according to their initial ability (i.e. tracking), we also include a measure of the *variation* in the initial attainment of each pupil's peer group. This is included to capture the degree of heterogeneity in the peer group's scholastic ability. All equations are estimated using the Huber-White robust estimator, which allows for the errors of the within-school clusters of pupils to be correlated while assuming independence of the between-school errors.

3. Data and variables

The data used to estimate the model were obtained from two sources: the National Pupil Database (NPD) and the annual Schools' Census. Both datasets were obtained from the Department for Children, Schools and Families (previously the Department for Education and Skills) and the data are for pupils who were in their final year of compulsory education (age 15/16) in 2003. The NPD provides pupil level data for all pupils attending maintained (state-funded) schools in England and the Schools' Census provides school level data. The dependent variable is constructed from national test and exam scores obtained by pupils at age 13/14 (key stage 3) and 15/16 (key stage 4).[7] The key stage 3 test score is the total test score for English, Maths and Science (weighted one third each). The key stage 4 exam score is the average points scored across all subjects in the GCSE and GNVQ examinations taken at the end of compulsory education.[8] In both cases, the

[6] The lagged peer group variable is defined as the key stage 3 test score of the pupil's key stage 4 peers *minus* the key stage 2 test score of the pupil's key stage 3 peers.

[7] 3.5% of all pupils changed school between key stages 3 and 4 but only 2.5% were genuine changes since pupils attending Leicestershire schools change from middle school to high school at age 14 and a majority will accompany their peers from one school to another. They will not, therefore, have different peers at key stages 3 and 4 (as required by our identification strategy).

[8] Pupils in compulsory schooling in England are tested at four key stages: key stage 1 (age 7/8), key stage 2 (age 10/11), key stage 3 (age 13/14) and key stage 4 (age 15/16). Tests are taken in English, maths and science at key stages 2 and 3. At key stage 4, pupils sit for either (or both) the General Certificate of

scores are converted into a standard normal variable.[9] Two variants of the dependent variable are used. The first is based on all pupils for whom test score data are available for all three key stages (at 11, 14 and 16); the second is restricted to only those pupils who changed school between key stages 3 and 4 (predominantly at the start of the final two years of compulsory education).

The explanatory variable of primary interest is that relating to the pupil's peer group, which is defined here as those pupils in the same school and year group. A school level analysis of peer effects in the context of the English secondary education system is justified on the grounds that pupils follow a national curriculum between ages 15 and 16, typically covering up to 10 subjects, and since not all are compulsory it is possible that pupils will find themselves with different peers for each subject.[10] The peer group variable is constructed in the same way as the dependent variable: it is the difference between the standardised test scores at key stages 3 and 4 for the pupil's peers (excluding the pupil's own score). A further variable of interest is the ability heterogeneity of the pupil's peers, which is measured by the standard deviation in the initial test score of the pupil's peer group.

Since the characteristics of the destination school may differ from the characteristics of the origin school, we include as controls several variables based on these differences. Pupils may move, for example, from a community school to a non-community school or from a coeducational to a non-coeducational school. In addition, differences between schools in several variables indicating resource inputs, such as the pupil-teacher ratio, the part-time to full-time staff ratio, and school size are included as controls.

The potential influence of the family background of a pupil's peer group on each pupil's educational attainment is estimated by including several variables to measure the difference in the family background of peers between a pupil's first and second peer group. Peer effects arising from family composition are proxied here by using the proportion of a pupil's peers who were eligible for free school meals, the proportion with English as their second language and the proportion from an ethnic minority. We are unable to include the change in the pupil's own family background variables in the estimating equation since family background data are only available for pupils in their final year of compulsory schooling. This should have little effect on

Education (GCSE) and the General National Vocational Qualification (GNVQ), both of which are taken at the end of compulsory schooling.

[9] Since the test scores at each key stage are based on different metrics, the mean scores obtained across subjects are first converted into a standard normal variable. The change in the standardised test score between key stages is also converted into a standard normal variable in order to aid interpretation of the estimated coefficients.

[10] We also note that class level data are not available.

the estimated peer effect, however, since family background for the majority of pupils is unlikely to change much between age 14 and 16 (key stages 3 and 4).

In estimating the peer effect for movers, we distinguish between those pupils who move *within* the catchment area (approximated by local districts), pupils who move *between* catchment areas but within the same county, and pupils who move to a new school in a different county.[11] The crucial distinction between these three types of move is that moves between schools in different counties are very likely to be accompanied by a change of household residence. The most likely explanation for these residential moves is a parental job change. Having had to make such a move, it is likely that parents will seek a house close to a school with a peer group that is at least as good as the peer group in their child's previous school. Some of the moves to another school in a different district but within the same county may also be due to residential relocation and could reflect a parent's desire to find a more appropriate school because of problems faced by their child at the original school. Although we have no specific prior for predicting the relative strength of the peer effect for these three types of move, a case can be made that the estimated peer effect for residential movers is expected to be stronger than for those who move within districts. This might occur if the poorer academic record of movers within districts has adverse effects on their new peer group.

Before proceeding to the empirical results, it is useful to compare the characteristics of the movers with those of the stayers. Pupils who changed school during the final two years of compulsory education (aged 14 to 16) differ in several respects to pupils who did not change school. The first clear difference between movers and stayers is that stayers achieved substantially better exam results than movers at all three key stages (see Table 2). The exam gap is particularly large for the exams taken at the end of compulsory education (age 16). The second clear difference is that residential movers have slightly better exam results than voluntary and involuntary movers, especially with respect to exams at 16. Movers and stayers also vary according to their probability of being eligible for free school meals. Generally, disadvantaged pupils are far more likely to change school than their non-disadvantaged counterparts, although this is not true for residential movers. The latter are much less likely to come from disadvantaged backgrounds than other movers. In fact, they are less likely to come from disadvantaged backgrounds than stayers. Similar arguments apply with respect to the ethnic background of movers.

[11] Districts are defined here in terms of local education authority areas (149) rather than local authority districts (366) since several of the latter have very few movers. The estimated coefficients on the peer group variable, however, are of the same order of magnitude (i.e. around 0.11 to 0.12 and highly significant) when local authority district dummies are included in the regressions. A similar problem arises with the use of school fixed effects (49% of schools have less than 5 inward movers and 83% have less than 10). Again, the estimated peer effect is very similar when school dummies are included in the regressions.

4. Empirical results

This section provides estimates of the peer effect on test score gain during the final two years of compulsory education. Before discussing the results in detail, we need to consider two potential problems which have implications for the reliability of our estimates.

Reliability of the peer effect estimates

In order to infer causality in the estimated peer effect, we need to consider whether this estimate is likely to be influenced by the sorting of movers between schools. As suggested earlier, parents might be systematically choosing better schools for their child, based on either observable or unobservable characteristics. The unobservables, for example, may include information relating to the quality of teaching at local schools. Although we cannot check whether parents are choosing a new school based on unobservables, we can at least compare the observable characteristics of the mover's peers at the origin and destination school.

We first note that the correlation between the prior test score of movers (at key stage 2) and the corresponding prior test scores of key stage 3 and key stage 4 peers are identical (r = 0.31 in both cases). Similarly, the correlation between the prior test score of movers (at key stage 2) and the key stage 3 test scores of both peer groups are also very similar and are statistically significant in both cases (r = 0.24 and r = 0.27 respectively). These correlations suggest that the sorting occurring at the beginning of key stage 3 (on first entering secondary school) is very similar to the sorting of movers into 'new' schools during key stage 4, at least as indicated by the exam performance of peers in the origin and destination schools respectively.

Further evidence that movers have very similar peer groups after moving to the one they had before is provided in Table 3. The mean values of the major characteristics of the peer group, their family background and the school they attend are very similar for the mover's origin and destination school respectively. Since no substantive differences are evident in the observables, this suggests that the *difference* in characteristics between the two peer groups, including school quality, is random. This result holds for all three mover categories. Moreover, insofar as observable school characteristics are likely to be correlated with unobservable school quality, we conclude that there is no evidence of a systematic sorting effect for movers following their move.

The estimated peer effect

Several estimates of the peer effect are provided in Table 4 based on the two models specified in section 2 above. The first model excludes the lagged dependent variable and the second model includes it. Three sets of results are presented for the two models: the first set (in Table 4a) is based on all pupils, whereas the second and third sets of results (in Table 4b and 4c respectively) are based on only those pupils who changed school. The second and third sets of results differ in the way the peer group variable is measured. The results in Table 4b use the concurrent test score gain of the pupil's peers, whereas the results in Table 4c use the lagged test score gain of the pupil's peers. The difference between the results obtained for all pupils and those obtained using only the sample of movers should provide some idea of the extent to which the estimated peer effect is affected by the sorting of pupils between schools.

The results reported in Table 4a suggest a peer effect of 0.24 for both boys and girls but this is very likely to be an overestimate of the true peer effect due to the bias induced by sorting. Including the lagged dependent variable is likely to reduce the bias since the lagged dependent variable will help to capture the influence of the unobservables that determine the sorting of pupils between schools. The fall in the estimated peer effect from 0.24 to 0.14 when the lagged dependent variable is included in the regression supports this argument.

Restricting the estimation of the peer effect to movers in order to reduce the influence of both sorting and simultaneity bias results in a drop in the peer effect from 0.24 to 0.13 (see Table 4b). The estimate of 0.13 is equivalent to a shift in an individual pupil's exam performance from the median percentile to the 55th percentile. Again, there is no significant difference in the peer effect between boys and girls. A further drop in the estimated peer effect (from 0.13 to 0.09) occurs when the lagged dependent variable is included in the model. The final step is to use the *lagged* test score gain of the peer group as the measure of peer group quality; this reduces the estimated peer effect still further (see Table 4c).

Our view is that using the lagged test score gain of peers to measure peer quality is too restrictive since it does not provide a true estimate of the simultaneous influence of peers on the individual pupil. Given that the peer group variable refers to the pupil's *school*mates and not the pupil's *class*mates, we take the view that simultaneity bias is unlikely to have a major impact on the estimated peer effect. Our best estimate of the overall peer effect is therefore in the range 0.09 to 0.13 for both boys and girls. We consequently use the concurrent test score gain of peers as our peer group variable in further analyses below.

We note that there is very little evidence (in Table 4b) that the heterogeneity of pupil ability has any impact on test score gain. We also note that adding a number of pupil level controls (ethnicity, first language not English, pupil has special educational needs and length of time in school before key stage 4 exam) had no effect on the estimated peer effect (results not reported).

The second main set of results, reported in Table 5, show the extent to which the peer effect varies between pupils according to whether the move was within the same district, to another district in the same county, or to a different county. We find that the estimated peer effect is virtually the same for all three types of move and that including the lagged dependent variable simply reduces the magnitude of the peer effect from around 0.13 to around 0.09 (comparing Tables 5a and 5b). The peer effect does not therefore differ significantly between those who changed school *within* a district and those moving due to a relocation of the household (as indicated by a move between counties).

Splitting the sample of movers according to whether or not they belong to an ethnic minority, we find that the estimated peer effect is significantly greater for non-whites (at 0.17) than for whites (at 0.12), thus indicating that ethnic minority pupils are more likely to be influenced by their peers than are white pupils (see Table 6). Further analysis, however, reveals that this result is driven by ethnic minority boys. Ethnic minority boys have a much higher estimated peer effect (0.22) than ethnic minority girls (0.12). They also have a much higher estimated peer effect than white boys (0.22 compared to 0.11).

Since it is potentially important to know whether the peer effect varies across the ability distribution, Table 7 provides estimates of the peer effect for pupils according to their initial attainment. Dividing the sample of pupils into quintiles according to their test score at the end of primary education, we find evidence of a non-linear relationship between the peer effect and initial attainment. For all movers, the estimated peer effect peaks at 0.17 for those in the second quintile of the initial test score and falls to 0.08 for those in the top quintile. Splitting the sample into three different types of mover (within district, within county and between counties), we find that for those moving within district and county boundaries to a new school, the peer effect is smallest for those in the highest quintile. The estimates of the peer effect across the initial (pre-secondary) test score distribution suggests that the gains for pupils of lower ability from having high ability pupils in their school would be greater than the losses to high ability pupils from having pupils of lower ability in their school.

Finally, we investigate whether the estimated peer effect varies with the dispersion in the peer group's ability level. We do this by estimating the peer effect for each quintile of the standard deviation of the peer group's test score at the end of primary education. The results in Table 8 indicate that although the peer effect is relatively high for the fourth quintile (at 0.15), it does not differ significantly from the point estimates of any of the other quintiles. The null of no significant difference in the peer effect across the initial test score distribution cannot therefore be rejected. We could find no evidence that the peer effect is related to peer group heterogeneity.

Further tests

A potential problem with our estimate of the peer effect is that it may suffer from simultaneity bias since the individual pupil may affect the behaviour, and hence the outcomes, of her schoolmates. A high ability pupil who moves between two peer groups of average ability, for example, can be expected to adversely affect the performance of the origin peer group and raise the performance of the destination peer group, thereby inducing an upward bias in the estimated peer effect. A low ability pupil will have the opposite effect, inducing a downward bias on the estimated peer effect. Since movers on average have lower attainment levels both at age 11 (before entering secondary education) and at age 14 (before moving to another school), this suggests that the estimated peer effect will be biased downwards - at least to the extent that the movers affect the performance of their new peer group relative to their old peer group.

One way to assess whether the peer effect is likely to suffer from serious simultaneity bias is to estimate it for differently-sized peer groups in the pupil sample. Since we would expect simultaneity bias to be more severe in smaller peer groups than in larger peer groups, this suggests that the estimated peer effect will be larger, *ceteris paribus*, for pupils attending schools in which the year group is small than for pupils attending schools in which the year group is large. Estimates of the peer effect for different size bands of the pupil's year group were found to be insignificantly different from each other (at 5%).[12] We therefore conclude that the use of relatively large peer groups will help to mitigate simultaneity bias in our estimates of the peer effect.

5. Conclusion

There is general agreement that segregating pupils into schools by their ability is undesirable on social and equity grounds. There is less agreement, however, about the consequences of mixed ability schooling on the total educational product as measured by test and exam outcomes. Whether segregated or mixed ability schooling is most efficient in terms of the total educational product depends on the magnitude of peer effects and how these vary according to ability levels. This paper has therefore attempted to estimate the peer effect on an individual's educational attainment during the final two years of compulsory education. The statistical analysis is based on individual level data for a sample of pupils in state funded secondary schools in England.

[12] Two tests were carried out. First, the sample was split into quintiles based on the size of the year group and the peer effect was then estimated for each sample separately. Second, peer attainment was interacted with each size band and the peer effect was then estimated for each of the five interaction terms. We recognise, however, that measurement error (in the peer group variable) may be larger in smaller schools, resulting in attenuation bias.

Since attempts to estimate the size of the peer effect are plagued by simultaneity and omitted variable bias, we have taken a different route to previous studies by focusing specifically on the group of pupils who *changed school* during their final two years of compulsory schooling. We argue that using the *difference* in test scores between two entirely different and *independent* peer groups rather than the *change* in the test score of the *same* peer group will help to mitigate the bias. In addition, we use test score *gain* as the dependent variable, thereby reducing bias arising from pupil level fixed effects.

The main conclusions of this paper are as follows. First, we find strong evidence of peer effects on the individual pupil's test score gain during the final two years of compulsory education. Estimates suggest that an increase of one standard deviation in the test score gain of a pupil's peers is associated with an increase of up to 0.13 of a standard deviation of the test score gain for the individual pupil. This lies within the range of previous estimates, most of which are between 0.07 and 0.25. Second, there is no evidence that the peer effect differs between boys and girls or between pupils moving to nearby (i.e. within-district) schools and pupils moving to schools in other districts or other counties. Third, the peer effect is estimated to be substantially and significantly larger for non-white boys than for white boys or for girls. Fourth, we could find no evidence that peer group heterogeneity (at year group level) is related to an individual's test score gain.

The main finding, however, is that peer effects are present and are generally stronger for low and middle ability pupils than for high ability pupils. This non-linearity in the peer effect suggests that comprehensive education is likely to raise overall attainment compared to secondary education based on a pupil's test score at the end of primary education.

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TABLE 1 Some recent estimates of the peer effect on test scores

Source	Context	Grade / year	Outcome indicator	Peer indicator or treatment	Peer group	Identification	Effect of change in peer group variable by one standard deviation
Hoxby (2000)	Public schools, Texas	3rd to 6th grade	Test scores (reading, math)	Test scores	Class	Gender variation between cohorts	0.40
McEwen (2003)	Secondary schools, Chile	8th grade	Test scores in Spanish language	Mothers' education of classmates	Class	School fixed effects	0.27
Hanushek *et al*. (2002)	Public schools, Texas	5th to 6th grade	Test score gain in math	Math score of peers (lagged 2 years)	Grade (school level)	Student and school-by-grade fixed effects	0.15/0.24
Rangvid (2003)	Secondary schools, Denmark	Age 15	Test scores in reading	Mothers' education of classmates	Grade (school level)	Extensive set of controls	0.05
Hoxby and Weingarth (2005)	Public schools, Wake County, South Carolina	3rd to 8th grade	Test scores for reading and math	Test scores for reading and math	Class	Random re-assignment of students to new peers	0.25
Gibbons and Telhaj (2005)	Secondary schools, England	Age 14 (Year 9)	Test scores in English and maths	Test scores at age 11 (year 6) of schoolmates	Grade (school level)	Random variation in transition to secondary school	0.08
Schneeweiss and Winter-Ebmer (2005)	Secondary schools, Austria	Age 15	Test scores in reading	Socio-economic status of schoolmates	Grade (school level)	Type of school fixed effects	0.05
Kang (2005)	Secondary schools, South Korea	Age 13	Test scores in maths and science	Test scores (maths and science)	Class	School fixed effects	0.27
Ammermueller and Pischke (2006)	Primary schools, European countries	Age 9/10	Test scores in reading	Test scores in reading	Class	School fixed effects	0.11
Ding and Lehrer (2007)	Secondary (Senior Middle School), County in Jiangsu Province, China	Age 17/18 (12th grade)	College entrance exams	High school entrance exams	Class	Pre-determined explanatory variables	0.08/0.15

Note: The estimated peer effect on a pupil's attainment given in the final column is measured in standard deviations.

TABLE 2 Descriptive statistics: mean test scores and personal characteristics

Test scores and pupil characteristics	Mean values for each characteristic (standard deviation)			
		Movers		
	Non-movers	Moved to new school in same district	Moved to new school in different district but same county	Moved to new school in different county
Test score at key stage 2 (age 10/11)	59.4 (14.8)	56.6 (14.2)	57.6 (14.3)	58.8 (13.9)
Test score at key stage 3 (age 13/14)	51.2 (11.2)	49.3 (11.0)	49.3 (10.9)	50.7 (10.5)
Test score at key stage 4 (mean GCSE/GNVQ score at age 15/16)	41.5 (20.4)	34.0 (19.8)	37.3 (18.9)	38.8 (19.3)
% pupils in ethnic minority in pupil's school	13.3 (14.0)	19.4 (39.5)	15.0 (35.7)	13.2 (33.9)
% pupils eligible for free school meals in pupil's school	13.9 (13.0)	18.1 (14.8)	19.4 (14.8)	11.1 (9.7)

Note: The sample of pupils is restricted to those for whom full information of test results is available. The pupil numbers in each group are as follows: 512159 non-movers, 4702 movers within districts, 4819 movers between districts but within counties, and 3173 movers between counties. Pupils changing school at the end of year 9 due to the local education authority practice of having 'middle' schools and 'upper' schools are not included in this table. The sample size for the estimated equations in the tables below is smaller than in the above table because of missing test score values for some pupils who changed schools.

TABLE 3 Characteristics of a mover's peer group *before* and *after* the move to another school

Characteristics of mover's peer group	Mean values for peer group (standard deviation): Moved to new school in same district — Peer group at age 14 (before moving)	Moved to new school in same district — Peer group at age 16 (after moving)	Moved to new school in different district but same county — Peer group at age 14 (before moving)	Moved to new school in different district but same county — Peer group at age 16 (after moving)	Moved to new school in different county — Peer group at age 14 (before moving)	Moved to new school in different county — Peer group at age 16 (after moving)
Peer groups' exam scores						
Exam score at age 11	52.6	53.9	52.7	52.8	55.3	55.4
	(8.1)	(6.7)	(7.7)	(6.8)	(7.3)	(6.9)
Exam score at age 14	45.7	46.3	44.6	44.5	47.2	46.8
	(7.7)	(7.0)	(7.4)	(7.2)	(6.6)	(7.3)
Exam score at age 16	33.7	35.1	33.0	33.1	37.8	38.2
	(9.1)	(7.6)	(8.7)	(8.5)	(8.4)	(8.2)
Family background						
% eligible for free school meals	17.8	16.8	17.2	17.8	11.7	10.4
	(18.7)	(13.8)	(17.2)	(13.8)	(12.7)	(9.2)
% pupils with English second language	6.2	7.0	4.1	4.5	5.7	3.7
	(21.0)	(18.4)	(14.9)	(12.7)	(16.6)	(13.0)
% ethnic minority pupils	12.9	12.2	11.5	11.0	9.1	4.9
	(28.2)	(25.1)	(25.9)	(24.1)	(20.3)	(13.1)
School						
Number of pupils in school	1002	1017	1156	1169	1115	1098
	(278)	(308)	(408)	(429)	(345)	(358)
Pupil / teacher ratio	16.6	16.6	17.0	17.0	17.3	17.5
	(2.0)	(2.2)	(2.3)	(2.4)	(1.6)	(1.6)
Part-time / full-time staff*100	13.7	13.7	12.6	12.6	14.7	15.4
	(6.6)	(7.0)	(7.7)	(8.0)	(7.8)	(8.2)
% truancy (i.e. unauthorised absence)	1.4	1.3	2.0	2.1	1.1	1.0
	(1.2)	(1.2)	(1.6)	(1.7)	(1.0)	(1.0)
Community school (% pupils)	55.3	55.6	66.4	66.7	66.7	67.8
	(49.7)	(49.7)	(47.2)	(47.1)	(47.1)	(46.7)
11-18 school (% pupils)	61.9	63.1	54.6	56.2	63.1	61.3
	(48.6)	(48.3)	(49.8)	(49.6)	(48.3)	(48.7)
n	3067		3365		2425	

Note: The means refer only to those pupils included in the estimated regressions. Community schools are publicly funded, comprehensive schools under the direction of the local education authority. They are distinguishable from selective schools, foundation schools and schools with a religious affiliation in so far as they have more independence in their governance than community schools and have more control over their intake of pupils.

TABLE 4a Estimated peer effect by gender: sample = all pupils

Explanatory variables	Dependent variable = test score gain of pupil					
	Sample = all pupils					
	Model *excluding* lagged dependent variable			Model *including* lagged dependent variable		
	All	Boys	Girls	All	Boys	Girls
Estimated peer effect	0.240***	0.236***	0.240***	0.142***	0.138***	0.139***
	(0.007)	(0.008)	(0.009)	(0.005)	(0.006)	(0.006)
Standard deviation of peer group's pre-secondary school test score	-0.020	-0.040	-0.004	0.043	0.011	0.073
	(0.049)	(0.057)	(0.057)	(0.040)	(0.043)	(0.045)
Controls included?	yes	Yes	Yes	yes	yes	yes
R-squared	0.07	0.07	0.07	0.30	0.30	0.31
N	452204	224520	227684	452204	224520	227684

Notes:
(i) Estimated peer effect = key stage 4 test score of key stage 4 peers *minus* key stage 3 test score of key stage 3 peers, where actual test score is converted into a standard normal variable since the key stage 3 and key stage 4 exam results are measured on different numerical scales. The actual key stage 3 test score is the average of the scores achieved in English, maths and science (age 13/14). The actual key stage 4 tests score is the average over all subjects (usually 8 to 10) taken at the end of compulsory schooling (age 15/16).
(ii) The following *differences* between the pupil's school at key stages 3 and 4 (which are the origin and destination schools for movers) were included to allow for changes in school level variables that might be expected to affect the test score gain: the proportion of pupils eligible for free school meals, the proportion of pupils whose second language is English, the proportion of ethnic minority pupils, the pupil / teacher ratio, the number of pupils on the school roll, and the ratio of part-time to full-time teachers. All equations also included district level fixed effects to allow for the influence of 'local' factors on the the test score gain. The exclusion of these area dummies from the model had virtually no impact on the estimated peer effect.
(iii) The standard errors (in parentheses) are estimated using the Huber-White robust estimator, which allows for the errors of the within-school clusters of pupils to be correlated while assuming independence of the between-school errors.
(iv) #, *, **, *** = significant at 10%, 5% 1% and 0.1% respectively.
(v) Pupils who change school at the end of year 9 due to the local authority's practice of having 'middle' schools and 'upper' schools (i.e. schools in Leicestershire) are omitted from the analysis since these are not 'genuine' school changers.

TABLE 4b Estimated peer effect by gender: sample = pupils who changed school

Explanatory variables	Dependent variable = test score gain of pupil					
	Model *excluding* lagged dependent variable			Model *including* lagged dependent variable		
	All	Boys	Girls	All	Boys	Girls
Estimated peer effect	0.132***	0.125***	0.137***	0.094***	0.093***	0.090***
	(0.010)	(0.014)	(0.014)	(0.008)	(0.012)	(0.011)
Standard deviation of peer group's pre-	-0.210	-0.155	-0.399	-0.279	-0.229	-0.475*
secondary school test score	(0.176)	(0.280)	(0.234)	(0.165)	(0.262)	(0.214)
Controls included?	yes	yes	yes	yes	yes	yes
R-squared	0.09	0.10	0.15	0.27	0.28	0.30
N	8857	4187	4670	8857	4187	4670

Notes: Estimated peer effect = key stage 4 test score of key stage 4 peers *minus* key stage 3 test score of key stage 3 peers. The controls included several additional variables in addition to those specified in the footnotes to Table 4a in order to control for differences between the destination and the origin school of the movers. These were as follows: age range of school (11-16 to 11-18, 11-18 to 11-16, and 11-16 to 11-16); school governance (community to non-community, non-community to community, and non-community to non-community); school gender (coed to non-coed, non-coed to coed, and non-coed to non-coed).

TABLE 4c Estimated peer effect by gender based on lagged peer group score

Explanatory variables	Dependent variable = test score gain of pupil					
	Sample = pupils who changed school after end of key stage 3					
	Model *excluding* lagged dependent variable			Model *including* lagged dependent variable		
	All	Boys	Girls	All	Boys	Girls
Estimated peer effect	0.037***	0.034**	0.035**	0.052***	0.049***	0.050***
	(0.010)	(0.012)	(0.012)	(0.009)	(0.011)	(0.011)
Standard deviation of peer group's pre-	-0.468*	-0.433	-0.640**	-0.419*	-0.400	-0.586**
secondary school test score	(0.184)	(0.295)	(0.231)	(0.166)	(0.267)	(0.209)
Controls included?						
R-squared	0.07	0.07	0.10	0.26	0.27	0.29
N	8857	4187	4670	8857	4187	4670

Notes: Estimated peer effect = key stage 3 test score of key stage 4 peers *minus* key stage 2 test score of key stage 3 peers. The controls are specified in the footnotes to Tables 4a and 4b.

TABLE 5a Estimated peer effect by type of move

Explanatory variables	Dependent variable = test score gain of pupil		
	Moved to new school in same district	Moved to new school in different district but same county	Moved to new school in different county
Peer effect (difference between the test score of key stage 4 and key stage 3 peers)	0.132*** (0.016)	0.122*** (0.024)	0.134*** (0.015)
Standard deviation of peer group's pre-secondary school test score	-0.245 (0.348)	-0.541 (0.398)	0.120 (0.271)
Controls included?	yes	yes	yes
R-squared	0.15	0.14	0.11
N	3067	3365	2425

Notes: The controls are specified in the footnotes to Tables 4a and 4b..

TABLE 5b Estimated peer effect by type of move (including pupil's lagged test score gain)

Explanatory variables	Dependent variable = test score gain of pupil		
	Moved to new school in same district	Moved to new school in different district but same county	Moved to new school in different county
Peer effect (difference between the test score of key stage 4 and key stage 3 peers)	0.097*** (0.014)	0.086*** (0.018)	0.094*** (0.013)
Standard deviation of peer group's pre-secondary school test score	-0.478 (0.338)	-0.705 (0.364)	0.070 (0.245)
Lagged dependent variable included?	yes	yes	yes
Controls included?	yes	yes	Yes
R-squared	0.31	0.31	0.29
N	3067	3365	2425

Notes: The controls are specified in the footnotes to Tables 4a and 4b..

Table 6   Estimated peer effect for white v non-white movers

Explanatory variables	Dependent variable = test score gain of pupil					
	All		Boys		Girls	
	Non-white	White	Non-white	White	Non-white	White
Estimated peer effect	0.168***	0.123***	0.222***	0.111***	0.127***	0.140***
	(0.021)	(0.012)	(0.033)	(0.016)	(0.028)	(0.016)
Controls included?	yes	yes	yes	yes	yes	yes
R-squared	0.23	0.09	0.34	0.10	0.34	0.13
N	1457	7400	702	3485	755	3915

Notes: The controls are specified in the footnotes to Tables 4a and 4b..

TABLE 7   Estimated peer effect by initial attainment and type of move

Explanatory variables	Dependent variable = test score gain of pupil			
	All movers	Moved to new school in same district	Moved to new school in different district but same county	Moved to new school in different county
Pupil in first quintile of initial attainment	0.132***	0.151***	0.165***	0.112**
	(0.018)	(0.029)	(0.042)	(0.043)
Pupil in second quintile of initial attainment	0.170***	0.161***	0.140**	0.159***
	(0.018)	(0.033)	(0.048)	(0.036)
Pupil in third quintile of initial attainment	0.154***	0.117**	0.131*	0.188***
	(0.027)	(0.050)	(0.054)	(0.042)
Pupil in fourth quintile initial attainment	0.138***	0.153***	0.114**	0.117***
	(0.023)	(0.043)	(0.043)	(0.035)
Pupil in fifth quintile of initial attainment	0.076**	0.093*	0.037	0.108*
	(0.025)	(0.044)	(0.051)	(0.051)

Notes: The controls are specified in the footnotes to Tables 4a and 4b. Initial attainment = key stage 2 score (final year in primary school).

TABLE 8   Estimated peer effect by peer group heterogeneity

Explanatory variables	*Dependent variable = test score gain of pupil*
Pupil in first quintile of dispersion in initial attainment	0.109***
	(0.020)
Pupil in second quintile dispersion in initial attainment	0.107***
	(0.024)
Pupil in third quintile dispersion in initial attainment	0.131***
	(0.027)
Pupil in fourth quintile dispersion in initial attainment	0.149***
	(0.032)
Pupil in fifth quintile dispersion in initial attainment	0.103***
	(0.017)
N	9524

Notes: The controls are specified in the footnotes to Tables 4a and 4b. The heterogeneity variable is omitted from these regressions. Dispersion in initial attainment = standard deviation in test score at key stage 2 for all pupils.