Spatial Economics Research Centre

SERC DISCUSSION PAPER 1

Peers and Achievement in England's Secondary Schools

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Abstract

The belief that children thrive if educated amongst higher-achieving schoolmates guides many parents in their choice of school. We extend the literature on this issue by considering children making the transition from primary to secondary schooling at age-11 in England. We use year-to-year changes in school composition to identify the impact of schoolmates on pupil progress at age 14. Traditional ‘linear-in-means’ specifications lead us to conclude that prior achievements of a child’s schoolmates are, on average, unrelated to his/her academic progress. However, this masks evidence that lower achieving pupils are disadvantaged by higher achieving schoolmates, whereas upper-middle ranking pupils benefit.

1 Introduction

Schools seem often to be judged on the kind of children they enrol, rather than on the quality of their teaching or the other facilities they offer. This observation has led many to argue that the background and abilities of a child's schoolmates must have an important influence on his or her own achievements at school. Motivated by this argument, a rich international literature has evolved to try to model and measure the consequences of social interactions between pupils – so called 'peer-group effects' – spanning the economics, education, sociological and psychological fields.

The issue is a critical one in respect of current educational policy which favours expansion of school choice, because choice based on school group composition can lead to a high degree of sorting across schools along lines of prior ability (e.g. Epple and Romano 2000). An understanding of the prevalence of peer effects is also important because they imply that educational interventions that appear beneficial when tested on the individual pupil may be even more effective (or less effective) when rolled out to the population (Glaeser et al 2003). It is also well known that peer group effects have efficiency implications when the effects are non-linear, or if there are complementarities between group and individual characteristics.

Our paper extends the evidence base by exploring the influence on achievement of the large innovations in peer group composition that occur on transition at age 11 between primary and secondary school in England. Our main concern is to find out whether children progress faster during the first three years of secondary school if their new secondary schoolmates performed well in their end of primary school assessments at age-11. However, estimation of school peer-group effects and the influence of social interactions in general is notoriously difficult, because school groups form endogenously in ways that are related to the outcome in question (Moffitt 2001, Manski 1993). In the absence of any explicit random assignment of individuals to school groups in England, our approach to this problem is to apply value-added achievement models coupled with a stringent fixed effect strategy to seek out random variation in the process of group formation. In our context, we find that we cannot eliminate observable sorting on pupil prior achievement, even with stringent school-fixed-effects and school-trend specifications. However, we can go further, and demonstrate that pupil sorting over time into secondary schools does not occur on the basis of differences in pupils' primary school quality. Therefore origin primary school 'value-added' or effectiveness provides a source of random variation in secondary peers' prior achievement.

Our presentation explicitly compares the association between individual and group prior achievement at age 11 – which arises through sorting at the point of group formation – with the association between individual age-14 and peers' age-11 achievement that exists after the school group has been in existence for up to three years. By controlling for school level fixed effects and trends in various ways we can observe how the degree of observable age-11 sorting changes, as we progressively eliminate between-school variation at the primary and secondary school level. At the same time we can observe the changes in association between age-11 peer achievement and age-14 pupil achievement, and the implications of these two effects in age-11 to age-14 value-added specifications. All our methods lead us to the same conclusion: schoolmates' prior achievement has no influence on individual achievement in simple linear-in-means specifications of peer effects. However, we show that this simple specification may mask significant costs to low-achievers and benefits from upper-mid ranking achievers from education amongst higher achieving schoolmates.

Our methods innovate on previous peer-group studies that employ value-added or individual fixed effect regression models of pupil achievement. We show that these models are biased if old and new school groups have members in common, unless we partial out the strong correlation between a pupil's own prior achievements and those of his or her prior schoolmates. Our data also has an advantage over existing studies in that it covers 80-90% of the population of five pupil cohorts in 99% of state schools in the whole of England. The large sample sizes mean we have the scope to detect effects based on low within-group variance in peer-group quality and allow us to investigate various interesting types of heterogeneity in response.

The next section provides an overview of recent relevant literature on the influence of peers on pupil achievement, outlining relevant methodological issues. Section 3 explains our empirical approach, our data and how it relates to the school system in England. Section 4 presents and discusses the results, and Section 5 concludes.

2 Background and literature

The role of social interaction in modifying individual behaviour is central in many fields in social science and social psychologists have been conducting related experiments for half a century. Economists too have a long standing theoretical interest (Becker 1974), and the past two decades have seen rapid growth in applied work that has attempted to investigate both the existence and functional structure of peer group influence. The range of outcomes that have interested researchers is diverse, including smoking (Alexander et al. 2001; Ellickson, Bird et al. 2003), joke-telling (Angelone et al. 2005), sexual behaviour (Selvan et al. 2001) purchase of a retirement plan (Duflo and Saez 2000), fruit picking (Bandiera et al. 2005) check-out throughput (Moretti and Mas 2007), routine tasks (Falk and Ichino 2006) and performance in professional golf tournaments (Guryan et al. 2007). Introspection does suggest that many decisions are linked to similar decisions by a friend or other associate (in some cases fairly explicitly, like the decision to have sex, be in a gang or play tennis), and many consumption decisions rely on other consumers participating (e.g. video phones). However, the more interesting possibility is that group behaviour or attributes can modify individual actions in relation to important social and economic decisions that will affect their life chances – especially achievement in education. Some very bold claims have been made about the potency of peers in child development (Rich Harris 1999), yet the results of numerous studies are very mixed, finding strong, weak or non-existent effects across a wide range of outcomes. This reflects the difficulty in defining the peer-group, isolating causal peer-group effects from other influences, lack of appropriate data, and different identification methodologies adopted by researchers.

Most empirical work in economics refers to Manski's (1993) framework which distinguishes between a) the influence of 'contextual' characteristics that members of a group bring with them when the group is formed, such as race, prior achievement, ability, gender; b) the influence of 'endogenous' behaviours that arise within the group such as inspiration, mimicry, effort, rivalry, competition for resources, and learning from others' successes and mistakes. In practical applications, this distinction usually boils down to treating group effects arising from observable group characteristics as contextual, and treating 'endogenous' effects as arising from unobservable group characteristics. However, statistical analysis in a non-experimental setting can only hope to make 'causal' statements about inter-relationships amongst *observable* factors. The best that can be done with regard to unobservables is to describe the patterns of association, since it is impossible to distinguish sorting and causal effects on unobservable dimensions without restrictive assumptions[1]. Hence, in this paper, we concern ourselves only with the effects of observable group characteristics, and in particular group prior achievement – seen here as a proxy for unobservable characteristics, such as the academic ability, prior motivation and effort.

However, many, well-known and serious challenges remain. The primary challenge is of course that individuals generally choose the groups to which they belong, so peer-group characteristics and unobserved individual characteristics are likely to be correlated through sorting. A secondary challenge, is that there are conceptual and data-related problems in defining the 'peer-group' – is it the whole school, the child's year group or class, or some narrower delineation requiring information on personal friendship networks (with even more

[1] The same challenge faces research in spatial econometrics, see for example Lee (2007)

serious problems of endogenous group membership)? Thirdly, it is all too easy to specify empirical regression models of pupil achievement in a way that leads to misinterpretation. For example, conditional on group prior achievement, more favourable group background characteristics imply less past effort or lower group ability so the estimated effects of group characteristics may be downward biased (Cooley 2007). As we will show, similar problems arise from conditioning on *individual* prior achievement when the current peer group includes members in common with the peer group that gave rise to prior achievement. Lastly, there are many questions to ask about the way peer group characteristics are specified in regression models. The most popular "linear-in-means" specification – in which mean group characteristics enter linearly with a single parameter in models of individual outcomes – has come in for some criticism in terms of its empirical validity and policy relevance (Hoxby and Weingarth 2005). However, linear-in-means peer effects do have important consequences for equity (streaming or segregation exacerbates educational inequalities), even if they have no implications for efficiency (streaming or not makes no difference to aggregate outcomes), and so still seem worthy of attention. Even so, important non-linear effects and heterogeneity in response can be masked by focussing in homogenous, linear-in-means effects, although there are clear advantages in terms of simplicity of interpretation, presentation and possibilities for identification. Our responses to all these considerations are set out in Section 3.

What does the econometric evidence on peer group effects tell us so far? The earliest studies on peer effects in educational attainment (Hanushek 1971, Summers and Wolfe 1977, Henderson et al. 1978) took relatively few steps towards overcoming problems of peer-group endogeneity. But recent studies have applied the standard set of modern econometric tools. Some have tried instrumental variables approaches, although it is very hard to find instruments that are plausibly uncorrelated with unobserved individual attributes or do not have direct effects (Dills 2005, Fertig 2003, Goux and Maurin 2005, Gaviria and Raphael 2001, Robertson and Symons 2003). Several papers have sought random year-to-year variation in mean peer group quality, occurring through 'sampling' variation as new cohorts are drawn from the population into schools, or as pupils move from one school to another. Variants of this approach appear in Hanushek et al. (2003), McEwan (2003), Lavy et al (2007), Vigdor and Nechyba (2004) and Hoxby (2000). Occasionally, opportunities arise for empirical analysis based on explicit randomisation, or assignment that appears random in the data, for example Sacerdote (2001), Zimmerman (2003), Cullen, Jacob et al. (2003), Vigdor and Nechyba (2004), Sanbonmatsu et al. (2004) and Hoxby and Weingarth (2005). But even empowered with these more sophisticated estimation methods and richer data than earlier studies, researchers are still divided on the importance of peer effects. It is worth emphasising, however, that even those studies that find statistically significant effects tend to find relatively small effects, as is clear in the summary presented in **Error! Reference source not found.**. Nearly all the estimates suggest that pupil achievement rises by less than 10% of one standard deviation for a one standard deviation rise in peer group quality (measured in terms of the between peer-group variance). The outliers tend to be studies based on IV approaches, and/or single cross-sections. Many of the studies investigate heterogeneity across pupil types and non-linearity in response, but almost every paper comes to different conclusions in this respect and we do not attempt to a summary here.

In the next section, we outline and justify our empirical strategy for assessing whether pupils derive any benefit from the prior academic achievement of their schoolmates in England's secondary schools.

3 Empirical strategy and data

Our core strategy has three key elements: 1) partially controlling for unobservable pupil characteristics using prior outcomes in a value added specification; 2) eliminating fixed over time school specific and school choice specific factors using fixed effects methods; 3) predicting peer group quality from the effectiveness of peers' origin primary schools.

3.1 *Group effects in value added models*

It is well understood that two processes can lead individuals to have similar achievements to the group in which they belong: a) causal effects that link group characteristics to individual outcomes b) sorting of similar individuals into groups. In the absence of explicit randomisation into groups, the key challenge in measuring peer group effects and neighbourhood effects of type a) is to distinguish them from sorting effects of type b).

One naïve approach to this problem is to condition estimates of current period causal effects on pupil achievement at the beginning of the period, in order to try to eliminate unobserved individual effects and control for potential sorting along these individual lines. A typical example model of 'linear-in-means' group effects, for estimation by regression methods, might be:

$$y_i = \rho x_i + \beta \tilde{x}_i + \omega_i \qquad (1)$$

where y_i is current pupil academic achievement (e.g test scores), x_i is pupil prior achievement (seen here as a proxy for unobserved individual heterogeneity), $\tilde{x}_i$ is the mean of pupil *i*'s peers' schoolmates achievements (e.g. prior test scores) and ω_i represents unobservable factors that affect current achievement. Clearly, least squares estimation of this model can only yield consistent estimates of 'causal' peer influences on current achievement if the unobservable factors in current achievement (ω_i) are uncorrelated with prior achievement and school group prior achievement.

For the moment, let us set aside concerns over sorting along unobservable lines which would lead ω_i and $\tilde{x}_i$ to be correlated. Even then, the specification of (1) does not easily provide estimates of peer group influence that are easy to interpret, because the group to which i belongs in the current period may well have many members in common with the group to which they belonged in the previous period, and/or because these new and old school group achievements are correlated for other reasons. Thus, new and old peer group components have very different linkages with current achievement y_i, conditional on prior achievement x_i, and it is necessary to separate out these different relationships empirically.

To illustrate this point, consider a simple two period process giving rise to (1). For example where period 1 corresponds to primary schooling and period 2 corresponds to secondary schooling. At the end of primary school (period 1) individual child achievement ($x_{i,1}$) is correlated with the mean primary school group achievement ($\tilde{x}_{i,1}$), either through peer group influence, pupil sorting, or school and teaching quality effects that school group members experience in common:

$$x_{i,1} = \lambda_1 \tilde{x}_{i,1} + \varepsilon_{i,1} \tag{2}$$

Next, school groups reform in secondary school, such that the mean prior achievement in a pupil's new school group ($\tilde{x}_{i,2}$) is correlated with mean prior achievement in the pupil's primary school group ($\tilde{x}_{i,1}$), because of observable sorting and because new and old school groups have members in common.

$$\tilde{x}_{i,2} = \theta \tilde{x}_{i,1} + u_{i,2} \tag{3}$$

Lastly, pupil achievement at the end of period 2 in secondary school depends on mean prior achievements of the new school group, and on unobservable pupil factors that are serially correlated with unobserved factors influencing pupil prior achievement in (1), mainly through unobserved individual heterogeneity ($\varepsilon_{i,1}$, e.g. ability, family background).

$$y_{i,2} = \lambda_2 \tilde{x}_{i,2} + \rho \varepsilon_{i,1} + v_{i,2} \tag{4}$$

These equations together imply, the following relationships, derived via a standard Cochrane Orcutt transformation:

$$\begin{aligned} y_{i,2} &= \rho x_{i,1} + \lambda_2 \tilde{x}_{i,2} - \rho \lambda_1 \tilde{x}_{i,1} + v_{i,2} \\ &= \rho x_{i,1} + \lambda_2 u_{i,2} - \left(\rho \lambda_1 - \theta\right) \tilde{x}_{i,1} + v_{i,2} \\ &= \rho x_{i,1} + \left(\lambda_2 - \frac{\rho \lambda_1}{\theta} \right) \tilde{x}_{i,2} + \frac{\lambda_1 \rho}{\theta} u_{i,2} + v_{i,2} \end{aligned} \tag{5a, 5b, 5c}$$

Hence from (5c), consistent estimation of the parameter β in (1) yields a consistent estimate of $\left(\lambda_2 - \rho\lambda_1/\theta\right)$, not the 'structural' peer group parameter λ_2. Moreover, ordinary least squares estimates will be a weighted average of λ_2 and $\left(\lambda_2 - \rho\lambda_1/\theta\right)$, that depends on the variance share of innovations $u_{i,2}$ in new peer group quality (5b)[2], where $u_{i,2}$ represents components of new peer group composition that are uncorrelated with old peer group composition. The intuition here is that low ability ($\varepsilon_{i,1}$) pupils come from relatively high scoring school groups, and high ability ($\varepsilon_{i,1}$) pupils come from relatively low scoring school groups, conditional on the pupil's own ability. If new and old peer group composition is closely related through sorting or through common membership, then this relationship is transmitted through to estimates of new peer group influence, conditional on pupil prior achievements.

Two alternative options for estimation present themselves from (5a,b,c). Firstly, we could use information on new peer group composition ($\tilde{x}_{i,2}$) and information on old peer group composition ($\tilde{x}_{i,1}$) to estimate Equation (5a) directly. Note that this is a more general version of the individual fixed effect estimator in first differences $x_{i,2} = x_{i,1} + \lambda\left(\tilde{x}_{i,1} - \tilde{x}_{i,0}\right) + v_{i,2}$, in which $\rho = 1$ and $\lambda_1 = \lambda_2 = \lambda$, but allows for mean reversion. Clearly, the fixed effects model is inappropriate, even if the restriction $\rho = 1$ is valid, because this model imposes $\lambda_1 = \lambda_2$ and yet λ_1 represents all group effects in period 1 (sorting, unobserved common influences, peer group effects), whereas λ_2 represents causal peer influence only.

[2] It is also well known (Todd and Wolpin 2003) that value-added models of this type will be biased if prior achievement is a poor proxy for serially correlated 'ability' components, that is if (2) contains an additional noise term on the right hand side.

Secondly, we could use information on exogenous innovations to peer group quality ($u_{i,2}$) with which to identify λ_2 in equation (5b), for example by estimating the regression:

$$y_{i,2} = \rho x_{i,1} + \lambda_2 \tilde{\tilde{x}}_{i,2} + \zeta_{i,2} \tag{6}$$

in which $\tilde{\tilde{x}}_{i,2}$ is defined as the mean prior achievements of new schoolmates who were not members of pupil i's school group in primary school period 1. Note that if sorting into school groups is random in the sense that mean achievement in the old peer group ($\tilde{x}_{i,1}$) and achievement of new peer group members ($u_{i,2}$, and hence $\tilde{\tilde{x}}_{i,2}$) are uncorrelated, then (5a) and (6) both yield consistent estimates of λ_2. This provides one basis for testing whether old and new school group composition is linked purely through common group membership, or whether sorting drives the correlation between the achievements of old and new group members.

The value-added approach in itself is clearly of little use if there is low inter-school mobility and a pupil's school group in period 2 is very similar to their school group in period 1. In this case, the contribution of the period 2 school group to period 2 pupil achievement cannot be separately identified from the association between period 1 school group with period 1 pupil achievement. Our study circumvents this low-mobility problem by exploiting the major changes to school group composition that occur when a child makes the transition from primary to secondary schooling in England. We consider peer group influences just after the point when pupils make this transition at age 11. A significant advantage of using this compulsory transition over, say, voluntary mobility between schools over time, is that the choice to move is not dependent on pupil characteristics and our estimates are not weighted in favour of high-mobility schools or pupils.

As will be discussed in Section 3.4, children in England sit tests at age 10-11 in May at the end of primary school, then move on to secondary school in September of the same year. Only a small proportion of a child's new secondary school peer group is made up of members in common with their primary school peer group (16% on average), so there is a major innovation to peer group quality driven by children coming from primary schools other than the child's own. Our first objective therefore, in line with the discussion above and equation (5c), is to see whether school group prior achievements influence a child's progress over the first three years of secondary schooling to age 14, conditional on the child's own age-11 achievements and those of his or her age-11 primary schoolmates. We can, in the spirit of equation (6), also look directly at whether age-14 achievement is linked to the mean age-11 achievements of schoolmates arriving at secondary school from primary schools other than a pupil's own.

In summary, we provide as the first component of our empirical analysis, estimates of equation (5a) and (6) using age-11 achievements as measures of $x_{i,1}$, $\tilde{x}_{i,1}$, $\tilde{x}_{i,2}$, $\tilde{\tilde{x}}_{i,2}$, and age-14 achievements as measures of $y_{i,2}$. We estimate these regressions with and without basic controls for observable pupil characteristics. In parallel, we will also present regressions of age 11 achievement on secondary peer group prior achievement, and regressions of age-14 achievement on secondary school peer group achievement, without controls for prior achievement. These estimates are potentially highly informative about the magnitude of sorting into school groups that occurs along lines of prior achievement, and, in turn, the likely contribution this sorting makes to any observed correlation between age-14 achievements and the prior age-11 achievements of a pupil's schoolmates. For instance, if we observe that a pupil's secondary school group's mean age-11 achievement is just as closely linked to his or her age-11 achievement as his or her age-14 achievement, we would find it hard to defend an argument that peer and individual outcomes were linked through peer group influence in secondary school rather then simply through sorting along lines of age-11 achievement. We will also generalise our specifications to allow for non-linearities and heterogeneity in individual responses to peer group quality to see if the linear-in-means representations is too restrictive.

Of course, sorting is also likely to occur on the basis of unobservable pupil characteristics, and we are not suggesting that the value-added specification outline above can alone identify causal peer-group influence at the school level. We need to augment these specifications to allow for the fact that the unobservable components in (5) and (6) are likely to be correlated with peer group quality through choice of school.

3.2 Group effects in group fixed effect models

In England, the decisions over which primary school and which secondary school to attend are to some extent voluntary, because the admissions system effectively allows a restricted choice amongst local schools, as detailed in Section 3.4. So, the problem of sorting into secondary schools along unobservable lines is possibly very acute. The value-added approach outlined above does not alone address the more general problem that the unobservable factors in the value-added model ($v_{i,2}$) may be correlated with mean group characteristics through school choice. For instance, the most motivated children (or parents) may seek secondary school groups with high prior achievement ($\tilde{x}_{i,2}$).

One standard strategy for eliminating sorting of this type is to assume that sorting occurs at some larger group level, but that within this group, there is no sorting of individuals into sub groups. This argument implies a group fixed effect specification, an approach that has become quite common in the literature (e.g. Hanushek et al 2003, Vigdor and Nechyba 2005, Ding and Lehrer 2006, Ammemmueller and Pishke 2006, Lavy et al 2007). One 'sub-group' fixed effect strategy of this type involves assignment into classes within schools, on the assumption that there is sorting into schools but not into classes within schools. However secondary class assignment is often explicitly non-random because there is within school tracking, and pupils are not always taught in the same classes. For example, Rothstein (2007) shows that within-school fixed effects strategies based on cross-sectional data are ineffective at eliminating biases in estimated teacher effects, so are equally unlikely to be effective at estimating peer group effects. In any case, we do not have data on class assignments in England and pupils mix with different sets of pupils for classes in different subjects. It is more plausible perhaps that variation in school group quality over time, within schools, is random (e.g. Hanushek et al. 2003) and that children cannot choose schools on the basis of the expected innovation in peer group quality in a given year. It is this line of argument that we will follow.

Since pupils are changing school, we need to control for school influences on prior achievement at both the primary and secondary level. Moreover, for fixed effects approaches to work, the data must provide substantial variation in sub-group composition within fixed-effects groups that is not driven mechanically by an individual pupil's own group membership[3]. Our data allows us to observe 5 cohorts of age 14 children in England who made this primary-secondary transition between 1999-2004, so we can control carefully for fixed effects at school level whilst allowing for substantial variation in peer group quality within schools over time. Our specifications therefore allow: first, for fixed effects for primary school, and then for primary × secondary school group. In the first case, we measure the effects of school peer groups using variation in the composition of secondary group experienced by pupils who make transitions from the same primary school to different secondary schools. This approach does not, however, effectively control for sorting into secondary schools conditional on primary school choice. So, in the second case we identify the effects of peer groups using variation over time in the composition of secondary peer group within *the same pair* of primary and secondary schools in different years. We, thus, control for primary and secondary school characteristics that are fixed over time for the duration of our sample, and control for unobserved pupil and family background characteristics that are common to specific school pair choices.

[3] For example, if there is only one sub-group per school fixed effects group, then variation in peer-group achievement is perfectly negatively correlated with individual achievement conditional on school fixed effects (e.g. Ding and Lehrer 2006, Lee 2007)

3.3 Origin primary school as a driver of peer group quality

We will show, through various falsification tests, that even this stringent fixed effect approach is not on its own sufficient to eliminate sorting into schools on the basis of observable measures of prior attainment at age-11. However we argue, and demonstrate empirically, that pupils do not sort into secondary schools on the basis of the primary school effectiveness of their secondary school peers, once we consider changes over time within the same primary-secondary school combination. In other words, pupils making a given primary-secondary school transition in different years experience secondary peers coming from different primary schools. Therefore, the composition of the secondary school peer group changes in terms of the combination of primary schools from which the pupils originate. This variation in peer group quality arises because of random cohort-to-cohort changes in the secondary school intake, in terms of the quality of primary schools from which pupils originate. This variation in primary school quality provides us with an observable component of $u_{i,2}$ in Equation 5b, with which we might hope to identify causal group effects.

Clearly, for this purpose, we need an estimate of primary school quality or 'effectiveness' that is not correlated with a pupil's own unobservable education-related attributes. We measure this school effectiveness using pupils' average gain in attainment between ages 7 and 11 at each primary school on an auxiliary data set of age-11 primary school pupils matched to their age 7 test scores. More precisely, we regress pupils' primary school age 11 test scores on pupils' age 7 tests, with controls for pupil characteristics, and compute the fixed primary school effects from these regressions. *Importantly, whilst we use the cohorts aged 14 in 2002-2006 for estimating our main peer effects equation, we use different cohorts aged 11 in these years to construct our measures of primary school effectiveness*. Averaging these primary school fixed effects amongst secondary school peer groups provides an estimate of the mean primary school quality of a pupil's secondary school peers. We show that this source of variation in peer group quality is uncorrelated with individual pupil characteristics, conditional on primary x secondary school fixed effects, and so provides us with a potential source of identification in reduced form models[4].

3.4 England's school context, and the sources of data

Compulsory education in state schools in England is organised into five "Key Stages". The Primary phase, from ages 4-11 spans the Foundation Stage to Key Stage 2. At the end of Key Stage 2, when pupils are 10-11, children leave the Primary phase and go on to Secondary school where they progress through to Key Stage 3 at age 14, and to Key Stage 4 at age 16. At the end of each Key Stage, prior to age-16, pupils are assessed on the basis of standard national tests.

The UK's Department for Children, Schools and Families (DCSF) collects a variety of data on state-school pupils centrally, because the pupil assessment system is used to publish school performance tables and because information on pupil numbers and characteristics is necessary for administrative purposes – in particular to determine funding. A National Pupil Database holds information on each pupil's assessment record in the Key Stage Assessments throughout their school career since 1996. Assessments at Key Stages 2 and 3 (ages 11 and 14) include a test-based component and teacher assessment component for three core curriculum areas: maths, science and English. We work with the overall test score in these subjects, and convert the score into percentiles of the pupil distribution within our estimation sample. This is because we found that there are few differences between the subjects in our analysis. All our results are therefore about effects on pupil ranking within the national distribution of school achievement[5].

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[4] We could of course use this variation in primary school quality as an instrument for secondary schoolmates' prior achievement, but there is little advantage in this approach over a reduced-form specification.

[5] A complication arises in that the maths and science tests at age 14 are structured into tiers, with pupils sitting different tests according to their abilities. This means that the scores for different pupils are not directly

Since 2002, a Pupil Level Annual Census (PLASC) records information on pupil's school, gender, age, ethnicity, language skills, any special educational needs or disabilities, entitlement to free school meals and various other pieces of information including postcode of residence (a postcode is typically 10-12 neighbouring addresses). PLASC is integrated with the pupil's assessment record (described above) in the National Pupil Database (NPD), giving a large and detailed dataset on pupils along with their test histories.

From these sources we derive an extract that follows five cohorts of children from their Key Stage 2 test score results at age 11, to their Key Stage 3 results at age 14 in 2002-2006. In addition, for two cohorts aged 14 in 2005 and 2006 we have information on their academic achievement in the Key Stage 1 tests at age 7. Various other data sources can be merged in at school level, including institutional characteristics (from the DCSF). We also focus solely on the 92% of state-school pupils who are in Comprehensive schools that do not choose pupils on the basis of academic ability, since problems of selection on unobservable factors are undoubtedly more acute in the non-Comprehensive sector. Also, we do not have data on the 6-7% of pupils who attend private schools. This large and complex combined data set provides us with information on around 2 million children aged 14 for the period 2002-2006.

Using this dataset we create own-achievement measures at ages 11 and 14, and calculate a pupil's school peer group achievement at age 11 and at age 7, based on pupils in the pupil's own cohort. The school age-cohort provides an appropriate peer group definition because pupils are not taught in the same groups for all lessons and so mix with pupils from throughout their age-cohort. Many schools place pupils into classes according to their subject-specific abilities. For our instrumentation strategy, we use the same data sources to derive the

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comparable. However, pupils are assigned to non-overlapping achievement Levels using the test results, based on annual rules devised by the Qualifications and Curriculum Authority. Using the information on Level achieved, test tier and test score we rank pupils within the Level they achieved and so recover their overall position in the achievement distribution.

average pupil age 7 to 11 value added for every primary school in England, based on five cohorts of children aged 11 from 2002-2006.

4 Results

4.1 Description of the key variables

Table 2 presents the descriptive statistics for our main estimation sample. All the central results in the paper are based on a pupils' percentile rankings in national tests at age 11 (primary school) and 14 (three years into secondary school), that is we convert mean pupil test sores in maths, science and English at these ages into percentiles. We found that there were few interesting differences between subjects in our main analysis, so we use a pupil's percentile based on their average percentile in all three subjects. Hence, the summary statistics on pupil attainment at ages 11 and 14 in Rows 1 and 2 are not particularly interesting: by construction, they have a mean of approximately 50.5 and a standard deviation of about 28.8. The standard deviations for the peer group means in the next rows are more revealing, and show that there is substantial variation in the composition of school groups in England, measured in terms of the pupils' mean prior achievements.

Our regression specifications will use the peer group defined in a number of different ways, but we focus (for reasons outlined in 3) on peer group of pupil *i* defined by secondary schoolmates who originated from a different primary school from pupil *i* at age 11. Based on this definition, the standard deviation of peer-group mean test score percentiles in Row 3 is 30% of the standard deviation in the distribution across pupils, at just over 8.6 percentiles (i.e. 9% the variance is between groups).

However, since we will be using regression models with fixed effects we also show the within-group variation in this peer test score variable in Rows 5 and 6, first within primary school groups, then within primary × secondary school groups. The first of these figures tells us that the standard deviation in secondary school peer quality experienced by pupils from the same primary school but going to different secondary schools or the same secondary school in different years is 5.3 percentiles, accounting for about 3.4% of the overall peer group variance. The next figure shows a standard deviation of 2.9 percentiles once we restrict attention to variation in peer groups over time and consider only changes in secondary peer group experienced by pupils making the same primary-secondary transition in different years. This variance is about 1% of the total variance in peer groups across and within schools. For comparison, we also show the figures for all secondary school peers in the same year group (irrespective of primary school, Row 6). The standard deviation for the 'all-pupils' peer group mean is marginally less than for peers from primary schools other than the pupil's own, as we would expect because the group size is slightly higher.

In the next rows of the table we show the variation in secondary school peer group based on the current 'value-added'-based effectiveness of the primary schools from which pupils originate. The estimates of primary school quality are based on 'point scores' that measure progress between ages 7 and 11 and are scaled in terms of the expected point gain for baseline white, non-free meal entitled, English first language girls aged 11 over the pupil census years. As explained in Section 3.3, these estimates are the primary school fixed effects from auxiliary pupil-level value-added regressions for progress between ages 7 and 11. In the average primary school, baseline pupils progress by about 13.8 points between ages 7 and 11, and the standard deviation across primary schools is 0.88 points. If we look at the variation across secondary school peer groups in terms of origin primary school quality, the standard deviation is about half that at 0.43 points. Hence, around 23% of the variance in primary school quality is represented in the variance between secondary school peer groups. In the next two rows we show the within-group variation when peer group quality is measured in terms of pupils' origin primary school effectiveness: the variation between secondary schools, within primary school groups, accounts for 4.7% of the total peer-group variance (a standard deviation of 0.192), whilst pure variation over time within primary × secondary groups accounts is only 0.66% of the total variance (with a standard deviation of 0.075)[6].

The group sizes are reported in the last rows of Table 2. On average, there are around 184 pupils in a secondary school age cohort and around 84% of this school group is made up of pupils who come from a primary school other than a pupil's own school at age 11. The large group sizes mean that any purely random variation in group composition, in the absence of sorting, is going to generate quite a low variance in mean group characteristics. Inevitably then, much of the variation in peer group quality in school is produced by sorting of pupils across schools. However, we will show in what follows that the residual peer group variance is sufficient to allow precise estimation of relationships of interest precisely in our large pupil population census.

4.2 Regression estimates of linear-in-means peer group effects

We now turn to basic regression estimates of the links between pupil test score outcomes and their peer group quality. Our presentation in Table 3 is structured to show transparently the links between pupil and group age-11 test scores under various specifications. Each cell in the table is the coefficient of interest from a separate regression of pupil test scores on group tests scores.

The first group of three columns are simple Ordinary Least Squares estimates. Firstly, in Column 1, we show to what extent a pupil's age-11 test scores from primary school are correlated with the mean age-11 test scores of their secondary school peers. This association can arise through sorting, or because the secondary school peer group contains members of the pupil's primary school peer group who may have influenced age-11 test scores or been subject to common primary school specific factors such as teaching quality. These estimates

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[6] Note that this is almost exactly what we would expect if all variation is random and groups were of equal size at the sample mean of 154, because 1/154 = 0.65%.

are shown in the top panel without any additional pupil control variables and again in the bottom panel with control variables (gender, ethnicity, language and free meal entitlement, school type, year dummies). In Column 2, we next show to what extent pupil's age-14 test scores in secondary school are correlated with mean age-11 test scores of their secondary school peers. Again, this relationship could exist through sorting into secondary school or because of peer group influences or common unobserved factors at primary or secondary school level. Finally, in Column 3 we show to what extent the gain in scores at age 14 exceeds the gain in scores at age 11 in value-added models as a first step to gauging the relative roles of sorting versus peer group effects.

Moving down the rows in each panel we explore how our Ordinary Least Squares estimates change as we vary the peer group definition for pupil *i*. Firstly, in Rows 1 and 4 we use prior achievement of all secondary peers in the same year group (equation (5c) in Section 3.1). Next in Rows 2 and 5 we condition on the prior achievement of children from a pupil's own primary school (equation (5a) in Section 3.1). Lastly in Rows 3 and 6 we use only the prior achievement of children who are from primary schools other than the pupil's own (equation (6) in Section 3.1). To repeat what we said in Section 3.1, the rationale behind this exercise is that estimates of peer group effects are potentially downward biased in value-added educational models unless we can condition out the influence of peers who impact on prior attainment at age 11.

This structure is then repeated in Columns 4-6 with the inclusion of primary school fixed effects and then in Columns 7-9 with the inclusion of primary × secondary school fixed effects. The inclusion of these fixed effects reduces the potential correlation between individual and group outcomes caused by sorting on unobservable group characteristics that are correlated with prior achievement.

We describe the general patterns observable in Table 3. Firstly, it is clear from the Ordinary Least Squares estimates of the correlation between pupil's primary age-11 tests and secondary school peers' age-11 tests that there is strong general sorting into secondary schools[7]. These coefficients in Column 1 will also pick up spatial autocorrelations arising from common local geographical factors and the fact that a proportion of a pupil's secondary school peers came from his or her own primary school. The coefficient is 0.94 in the regressions without control variables: i.e a pupil at the *k*th percentile in the distribution of age-11 primary school test scores can expect to be amongst other pupils who are on average at the $0.94 \times k$th percentile in this distribution when they get to secondary school. The 'sorting' coefficients are lower when we control for prior achievement in the pupil's origin primary school group (Rows 2, 5) or when we consider the association with prior achievement of pupils originating only in other primary schools (Rows 3, 6).

Looking at the OLS age-14 test results in Column 2 reveals that the association between pupil test scores at age 14 and group prior achievement is amplified relative to what was observed on entry at age 11 in Column 1. Thus, pupils entering secondary schools alongside higher achieving pupils do tend to move up the distribution of achievement relative to pupils entering schools alongside lower-achievers. This could be due to peer group effects, sorting on unobserved factors that affect pupil progress, or simply due to common secondary group influences such as teaching quality or resources that affect age 14 achievement and are correlated with pupils' mean age-11 scores. The same feature of the data is shown more explicitly in Column 3, where the OLS estimates of the value-added specification suggest that a one-percentile increase in school mean age-11 test scores is associated with a 0.26-0.43 percentile improvement in pupil's own scores at age 14. Of course, these would be very naïve estimates of causal peer group effects and we will continue to more stringent specifications. Note first, that the coefficients in Column 3 reveal exactly the pattern expected from our discussion of value-added models in Section 3.1: the coefficient estimate increases as we control for the fact that high-age-11-test-score pupils come from high-age-11-test-score

[7] This is evident in the persistent stratification described in Gibbons and Telhaj (2007)

primary school groups, and that their secondary school peer group has members in common with their primary school group. This pattern of increasing coefficients is repeated in all the value added models in Columns 3, 6 and 9 as we move across the table.

In Columns 3-6 we repeat the estimates with primary school fixed effects[8]. This eliminates fixed-over-time primary school factors, general geographical factors and pupil factors linked to primary school choice. However, it is clear from the ‘sorting’ models of age-11 test scores in Rows 1 and 3 that observable sorting is not eliminated: a high-age-11-ability pupil is more likely to find his/her way to a secondary school with other higher age-11-ability pupils than is a low-age-11-ability pupil from the same primary school. We therefore doubt whether sorting on unobservables is eliminated, so again the effects on attainment at age-14 cannot reliably be interpreted as causal. Note, however, that controlling for primary school fixed effects alone reduces substantially the association between pupil age 11 to 14 ‘value-added’ and peer group age-11 ability: a one percentile improvement in peer group ability is linked to just 0.2 of a percentile improvement in age-14 test scores, and only 0.16-0.18 percentiles once we control for pupil characteristics. This implies that a one-standard deviation change in peer group ability is linked to just 5-6% of one standard deviation improvement in age-14 test scores (conditional on age-11 tests).

Finally, in Columns 7-9 we eliminate all purely cross-sectional sorting by controlling for primary × secondary fixed effects. Still, in Column 7, we find evidence of observable sorting within primary-secondary groups over time. This is quite a puzzling finding, since it is hard to imagine how pupils can anticipate how the peer groups in their specific year-group in a particular secondary school will differ from the mean peer group in that secondary school over time. One possibility is just that there are unobserved primary × secondary trends in

[8] Similar to a specification used in an earlier version of this paper where we had only two time periods Gibbons and Telhaj (2006), and with very similar results.

peer group quality (e.g. if some secondary schools are becoming increasingly lax or restrictive in terms of their admission procedures), a possibility that we investigate in the next section.

In any case, looking across the Columns 6-9 in the table, we see that the coefficient in the age-14 test score models with primary × secondary effects is nearly identical to the coefficient in the age-11 test score models once we adopt our preferred group definitions. A one percentile increase in the mean age-11 test achievement of secondary peers from other primary schools is associated with a 0.12-0.14 percentile increase in pupil's own achievement at both age 11 and age 14. Hence, the estimated peer group effect on pupil progress in the value added models (Column 9) is effectively zero, even when there is a small degree of sorting on prior age-11 achievement.

A few notes are in order at this point. Firstly, note that amplified noise through over zealous use of fixed effects is not to blame for our zero coefficients with primary × secondary fixed effects: the coefficients in the age-11 and age-14 achievement level models are precisely measured. The zero coefficient in the value-added model results from the coefficients on age-11 and age-14 test scores being almost equal. Note also, the importance of separating out the effect of peers originating from own and other primary schools. If we apply the standard peer group definition based on all secondary school peers (Column 7), the value added models yield strongly negative (and significant) coefficients on peer group quality as implied by Equation (5c). Again, note that although we have not eliminated sorting on the basis of age-11 test scores, the equality of the parameter estimates with and without controls (bottom versus top panels) shows that the primary × secondary school fixed effects control effectively for sorting along other observable dimensions, at least in so far as these characteristics are pertinent to test scores. Moreover, note that our results are not sensitive to transient noise in pupils' individual age-11 test scores, for which the coefficient (unreported in the tables, ρ in Equation (6)) is around 0.87. We can instrument these scores with age-7 test achievements (as suggested in Todd and Wolpin 2003) or by teacher predictions of pupil achievement at age 11 and the main findings on peer group influence remain largely unchanged, although the estimate of ρ increases by about 10%. In fact we can constrain ρ to 1 with almost identical results. Lastly, note also that it makes very little difference in our value added models whether we measure school group quality in terms of all secondary school peers, conditional on own primary school peers, or whether we consider only peers originating in other primary schools. As discussed in the context of Equation (6) in Section 3.1, this provides some reassurance that, conditional on appropriate school fixed effects, primary-secondary school transition provides innovations to a pupil's peer group quality that are uncorrelated with the mean age-11 achievement of his or her own primary school peers. In the results and robustness checks that follow, we will simplify the analysis by considering only innovations to a pupil's school peer group induced by schoolmates originating from other primary schools (i.e. in line with the specifications in Rows 3 and 6 of Table 3 and Equation (6) in Section 3.1.

4.3 Unobserved trends

In Table 4 we go further to see if we can eliminate sorting by controlling more precisely for geographical location or for differential trends across schools. Firstly in Row 1 Columns 1-3 we control for primary × secondary × postcode-of-residence fixed effects, to allow for the fact that children may be sorted into secondary schools based on where they live, even conditional on which primary school they attend. Next, in Columns 4-6 we estimate models in which the primary × secondary trends in test scores (age 11 or age 14 as appropriate) are included in the regressions as additional control variables. We obtain these trends by estimating 599987 auxiliary regressions of test scores on time trends for each primary × secondary group in our data[9]. Although the degree of observable sorting in terms of age-11 test scores is reduced once we control for trends, the coefficient on age-14 test scores falls in tandem, so again we

[9] Other methods, such as partialing out the time trends from peer group mean test scores produce similar results.

find no peer group effects in our value-added models. Next, in Rows 7-9 we put the secondary school peer group in adjacent cohorts – i.e. the mean age-11 test scores of those who are in year groups (grades) one year above and one year below pupil *i* – alongside their own age-14 year-group peer variable (a similar method is applied in Lavy et al 2007). Looking at the coefficients for the age-11 test score sorting models in Column 1 we can see that there is some sorting on the basis of adjacent cohorts' age-11 test scores (we can think of these as proxies for short run expected secondary school quality), but controlling for this sorting does not change our key finding: the prior academic achievements of same-grade peers has no impact on pupil progress in linear-in-means models.

4.4 Primary schools as sources of variation in peer group quality

In Table 5 we use an alternative measure of peer group quality based on the mean age-7 to age-11 value-added of the primary schools from which peers originate. The rationale for this approach is set out in Section 3.3: part of the reason peer groups differ over time within primary × secondary groups is that the group composition changes in terms of the quality mix of origin primary schools, and we argue that it is unlikely that sorting can occur on the basis this variation. Therefore, we can use estimates of long-run value-added in origin primary schools as an exogenous measure of peer group quality.

Firstly, Column 1 of Table 5 confirms that origin primary school value-added is indeed a good predictor of the age-14 peer groups' mean age-11 test score ranking: a one point increase in the mean value-added score of peer's origin primary schools leads to a 4 percentile increase in the mean pupil ranking in the age-11 test score distribution[10]. Next, Column 2 confirms too there is almost no sorting on the basis of pupils' origin school value added: a pupil's own age 11 tests are uncorrelated with the average quality of his or her secondary

[10] Remember, the primary school value added scores are computed from pupils aged-11 at the time the peer group is aged 14.

school peers' origin primary schools in these models with primary × secondary school fixed effects[11]. However, there is also no correlation between peer group quality and age-14 tests in Column 3, and the effect of peer group quality in the linear-in-means value added models in Row 4 remains stubbornly at zero.

4.5 Nonlinear effects, complementarities and alternative peer group characteristics

In summary, our most rigorous specification implies that peer group quality in secondary school – measured in terms of prior academic achievements or the primary school quality of peers – has no impact on an individual pupil's academic progress in secondary school between ages 11 and 14. The link between pupil and peers academic achievement at age 14 is driven purely by sorting into secondary school groups, and this sorting is observable in age-11 test scores.

Perhaps, one reason why we find no effect from peers is that we have been too restrictive in terms of our linear-in-means specification of peer group effects. Our findings could mask important non-linearities in the response to peer group quality, or complementarities between a pupil's own ability and that of their peers. Alternatively, perhaps we are just looking at the wrong peer group attribute, and it is actually demographic characteristics such as low income, gender or ethnicity that matter. We now consider these matters in detail.

Table 6 addresses the non-linearity/complementarity issue by re-estimating our preferred value added, fixed effects specification but with dummies for own age-11 test quintiles, dummies for each quintile in the mean peer age-11-achievement distribution, and their interactions. The dummies are structured such that the coefficients indicate the marginal effect of peer group improvement within own-achievement quintile, as we read left right across the

[11] Note that the scaling of the coefficients in Column 5 differs from the other columns in the Table. If the standard deviation in peer group quality in column 5 is scaled to have the same standard deviation as in Columns 1-4, then the coefficient in Row 1 is 0.028 (0.020)

table. For simplicity, we do not report the marginal effects of own prior achievement, which are obviously always large and highly statistically significant. The specifications also include interactions between own prior achievement quintile and a linear term in own prior achievement and the usual control variables.

Moving across the columns, towards higher ability peer groups it is hard to spot any clear signs of significant or marked nonlinearities, although there is strong evidence of interaction between own ability and peers ability. For the lowest age-11 ability pupils, peer group quality improvements appear to have a significant *adverse* effect on outcomes at age 14. The loss in moving from the 'worst' to the 'best' peer group amounts to only about 2.3 percentiles in the age-14 pupil distribution. This finding is somewhat reminiscent of the idea of 'relative deprivation' effects in the neighbourhood effects literature, whereby outcomes for disadvantaged individuals are made worse by others' success. Peer group quality has almost no influence on the 2nd ability quintile. However, age-14 attainment rises with peer group quality for pupils in the 3nd and 4th ability quintiles, by around 1.7-1.8 percentiles. For pupils in the top quintile of prior achievement, peer group prior achievement again has small overall negative impact of around 0.7 percentiles though the marginal effects are not significant at the 1% level.

Table 7 specifies complementarities between peers and individuals in a different way, and estimates the effect of adding more pupils in a specific achievement quintile to each pupil's peer group. The table presents results from a regression in which we interact indicators of pupil's own age-11 achievement quintile with the proportion of his or her school peers in other age 11 achievement quintiles. Note, we always omit the proportion of the peer group in the pupil's own same achievement quintile. As before, we control for interactions between own prior achievement quintile and a linear term in own prior achievement, plus the usual characteristics. The general picture is very similar to that of Table 6, and shows lowest achieving pupils losing out significantly as the proportions of higher achieving peers increases. A 10 percentage point increase in the proportion of peers in the top quintile is associated with an 0.86 percentile fall in the age-14 achievement of pupils in the lowest quintile. There are modest gains for pupils in the quintiles 2 and 3 from mixing with high achieving peers, and pupils in quintile 4 lose out from mixing with lower achieving peers. As seen before, high achieving pupils seem to benefit if their school is otherwise populated with low-achievers, although the coefficients are not highly statistically significant. It is also possible to test for sorting within each quintile of the pupil age-11 achievement distribution, by regressing pupil age-11 test scores on the proportions of his or her school peers in other age 11 achievement quintiles (controlling for primary × secondary fixed effects and the usual variables). The F-tests on the peer group quintile shares suggest that the peer group age-11 quintile shares are unrelated to a pupil's own achievement within own-achievement quintiles (p-values between 0.10 and 0.90), for all achievement groups except the lowest quintile. In this group, own age-11 achievement is positively correlated with the proportion of peers in all higher quintiles, suggesting positive sorting, which makes the negative influence of higher achieving peers all the more surprising.

The analysis of Table 6 and Table 7 shows evidence of peer group effects, with some significant complementarities between own and peer ability. Where gains exist, they tend to be concentrated in the upper-middle part of the distribution, and there are negative effects on lower-achieving pupils from mixing with high-achievers, but moderate gains to high-achievers from mixing with low-achievers. Any advantages and disadvantages of peer groups tend to cancel out over the entire distribution of pupil ability which is why the general effects captured by the linear-in-means specifications are zero. These complementary effects are substantial for the lowest ability pupils: the overall standard deviation of the proportion of peers in the top quintile is 0.40, and the standard deviation of age-11 scores for bottom quintile pupils is 12.13. Hence, a one-standard deviation increase in the proportion of high achievers in their peer group is linked to a 23% of one standard deviation *decrease* in the achievement of the lowest achievers. For the middle quintile of pupils, the gain in age-14 scores from a one standard deviation increase in the peer group share of the top is 15% of one standard deviation. The loss to pupils in the top quintile from a one standard deviation fall in the proportion of peers in the top quintile would amount to 7% of one standard deviation (assuming that the bottom for quintile shares increased by 10 percentage points each).

Turning now to other 'contextual' peer group attributes, Table 8 reports comparable value-added models with primary × secondary fixed effects, that include mean peer group demographic and socioeconomic attributes entered together in the regressions. As usual in our specifications, these are the attributes of a pupil's peers joining secondary school from primary schools other than that pupil's own primary school. We also split the sample by demographic groups (gender, free meal entitlement, age) to check for interactions and complementarities. For the most part, the coefficients are small and statistically insignificant, even when tested in groups, and there are very few notable differences across the different pupil categories. The only clear exception is the effect of the proportion of the peer group who speak English as a first language. There is evidence here that being in a school group with a higher proportion of native English speakers confers some advantages in terms of age-14 achievement.

Why the language skills of peers should matter at this age is unclear, since most of those pupils without English as their first language will be fluent speakers by the time they reach secondary schooling. Moreover, pupils with English as an additional language actually show higher value-added progress between ages 11 and 14 than native English speakers (the coefficient on English first language in our regressions is around -2.5 with a standard error of 0.1). We have checked whether the peer effect we find for non-native English speakers is attributable to new school entrants with English as an additional language, since Gould et al (2005) suggests that new immigrants into schools in Israel have a detrimental impact on their schoolmates' academic progress, possibly because of the additional demands they place on resources. However, in our case new entrants do not explain our finding that pupils progress marginally faster when few of their schoolmates have English as an additional language. Whatever the cause, the magnitude of the effect is very small: a one-standard deviation increase in the proportion of English first language pupils is linked to, at most, a 0.8 percentile move up the pupil distribution of age-14 achievement (2.8% of one standard deviation).

4.6 Using information on early achievements

As the final part of our analysis we turn to consider whether unobserved pupil trends in achievement could be masking important peer group influences. For example, whilst not entirely plausible, it is not impossible that trends in pupil achievement and the level of peer group quality are negatively correlated, which might mean that peer group effects and individual trends tend to be self cancelling. There is clearly no way we can separately identify peer effects on achievement gains between ages 11 and 14 from individual trends in achievement between age 11 and 14. However, we can, for a sub-group of years 2005 and 2006 in our census, control for pre-existing individual pupil trends by including information on age-7 test results in our regressions. Age 7 test results also provide additional information on sorting in secondary schools.

Table 9 presents our findings on these issues. Each column presents the coefficients and standard errors from a separate regression, with primary × secondary fixed effects. The first column is a ‘sorting’ model in which a pupil’s age-7 test score percentile is regressed on age-7 test percentile of age-14 peers and age-11 test percentile of age-14 peers (where these peers are restricted to come from primary schools other than the pupil’s own). Clearly, the positive coefficients in Column 1 are not evidence of causal peer group influence, since there is no direct link between a pupil’s age 7 and 11 scores and the age 7 and 11 scores of secondary school peers coming from other primary schools. Like those from the regressions of pupil age-11 scores on peer’s age-11 scores in Table 3 onwards, these coefficients arise through non-random sorting over time into secondary school year groups, even within primary × secondary school groups over time. Turning now to the value-added models in Column 2, which condition on a pupil's age-7 and age-11 test scores to control for individual trends, we again find little evidence that there are benefits from secondary education amongst peers with higher age 7 or age 11 test scores. Yes, the coefficient on peers' age-11 test scores in the bottom right hand cell is marginally significant and positive. But the implied effects are miniscule: a one standard deviation increase in peer group quality is linked to a mere 1.7% of one standard deviation increase in a pupil's own age-14 achievement.

5 Discussion and conclusions

In England, pupils re-sort themselves into new school groups when they move from primary to secondary schools at the age of 11. Part of this re-assignment is through preference, and part will be random because of failure to secure schools of choice or because of unanticipated variation in peer group quality within schools of choice. We have used this re-allocation at age 11 as a source of variation in peer group quality within primary-secondary school pairs over time, but find no evidence that, on average, pupils who end up in peer groups with higher age-11 ability progress faster academically between ages 11 and 14. There is a link between the age 11 test scores of secondary school mates and a pupil's own age-14 test scores, but this is caused by sorting: a pupil's age-11 and age-7 primary school test scores appear to be just as sensitive to his or her secondary school mates age-11 and age-7 primary school test scores, even when these schoolmates come from primary schools other than the pupil's own. There is clearly no way such a link can be attributed to school-based peer influence. Hence, we are driven to the conclusion that there are no *general* educational spillover or peer group benefits in the context we have studied.

We do find evidence that this zero average effect masks heterogeneity in response to peer group quality, with differences according to pupil prior achievement. Pupils in the middle of the distribution have slightly higher achievement in the company of high achievers, suggesting a classic spillover benefit from engagement with more able schoolmates. However, one of the strongest findings is that low achieving pupils lose out substantially as the share of pupils in all other quintiles increases. The reasons for this negative spillover from higher achievers must remain a conjecture, but candidate explanations are that low achievers are demotivated or receive less attention if they are in a minority. High achievers also seem to lose out if they sort into schools mainly with other high achievers and benefit from a more mixed school intake. We find no evidence of contextual effects from the income, ethnic minority, gender or age mix, although there seems to be a marginal advantage to being in schools with more students who have English as a first language.

Given these findings, it is hard to believe that the efforts to which some parents go to secure schools with a ‘good’ peer-group are worthwhile, purely in terms of the improvement in educational achievement that better quality peer-groups can offer. Possibly, there are peer group influences on academic related behaviours which we cannot observe – like the decision to do homework – but given our evidence these behaviours have no payoff in terms of achievement. Possibly, there are peer group influences on subsequent educational decisions – staying on at school etc. – which we have not considered in this study, though we find such a possibility unlikely given our lack of evidence of any substantive influence on achievement at age 14. Perhaps, however, better peer-groups provide other immediate and long run benefits – physical safety, emotional security, familiarity, life-time friendship networks, or simply exclusivity – which make schools with good peer groups desirable commodities, regardless of whether they offer any educational advantages.

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Table 1: A non-exhaustive summary of school peer effect estimates from this century

Studies	*Context*	*Outcome*	*Peer-group or treatment*	*Methodology*	*Approx order of magnitude*
Hoxby (2000)	Texas schools, US	3rd grade test Scores	Classmates' tests, gender and race	Cohort-cohort variation in gender and race	1.s.d. → 0.02 s.d. (based on gender balance) [1]
Gaviria and Raphael (2001)	US, NELS data	8th graders dropping out	School mates dropping out	IV using peers characteristics	1 s.d. → 0.04 s.d.
Sacerdote (2001)	Dartmouth College US	College Grade Point Average	Roommates' Grade Point Average	Random assignment to rooms	1.s.d. → 0.07 s.d.
McEwan (2003)	Chile, cross-section census	8th grade Test Scores	Classmates' background	School fixed effects in cross section	1.s.d. → 0.27 s.d change in mothers education.
Hanushek (2003)	Texas elementary schools	Test Scores	School grade prior achievement	School-by-grade fixed effects	1 s.d. → < 0.08 s.d.[2]
Zimmerman (2003)	Williams College, US	College Grade Point Average	Roommate's prior SAT scores	Random assignment to rooms	1 s.d. → 0.05 s.d.
Cullen, Jacob and Levitt (2003)	Chicago public schools	Test Scores, and others	Attendance at oversubscribed schools	Assignment by lottery	Near zero and insignificant
Sanbonmatsu et al. (2004)	Moving to Opportunity experiment	School Test Scores	Opportunity to move home	Policy experiment/ random assignment	Near zero and insignificant
Angrist and Lang (2004)	Boston Metco programme	4rd grade test sores	Reassigned low-scoring students	School reassignemt and IV from class size limits	"little evidence of socially or statistically significant effects"
Vigdor and Nechyba (2004)	North Carolina primary schools	5th grade test scores	Classmates' prior test scores	School fixed effects/apparent random assignment	1 s.d. → 0.03 s.d.
Arcidiacono and Nicholson (2005)	US Medial schools	Board exam scores	Classmates' admission tests	School fixed effects	Negative and insignificant
Ammermueller and Pischke (2006)	Europe primary schools	Reading test scores	Classmate's test scores	School fixed effects	1 s.d. → 0.07 s.d.
Lavy, et al (2007)	Israeli high schools	Matriculation outcomes	School proportion of grade repeaters	School fixed effects and trends	1 s.d. → 0.006 s.d. [3] Elasticity < 0.01
Hoxby and Weingarth (2005)	Wake County schools	End of grade tests	Classmate's prior test scores	Student, school fixed effects + reassignments	1 s.d. → 0.25 s.d.[4] non-linear effects
Goux and Maurin (2007)	France, 1997 cross-section	3rd grade test scores	1st grade schoolmates	IV using schoolmates' age	1.s.d. → 0.26.s.d.
Kang (2007)	S. Korea middle schools	Grade 7 and 8 maths scores	Classmates' prior test scores	School fixed effects and IV	1 s.d. → 0.08 s.d.[5]

Magnitudes are reported for a *1 s.d. change in peer distribution* using the best information available in the results

[1]Hoxby does not provide the descriptives to make this translation straightforward. On p.23 "an all female class would score one-fifth of a standard deviation higher in reading", which is equivalent to a 51 percentage point change in the female share. However, we estimate the standard deviation in the proportion female to be about 0.056 (given 49% female and random assignment into class sizes of about 80; see Table 1). Hence a 1.s.d. change gives a 0.056/0.51*0.20 = 0.022.

[2]Our calculation based on the tabulated results differs from that reported in the paper's conclusions, which seems to be based on the effect of a change in peer group mean tests scores equal to 1.s.d. of the pupil distribution, rather than the peer group distribution

[3]Standard deviations not given. Our calculation is based on 4.5% repeaters randomly assigned across schools of size 175, giving a standard deviation in the proportion of repeaters = 0.016. The total proportion matriculating is 0.609 giving an outcome standard deviation of 0.488. The coefficient on repeaters in the matriculation estimates is -0.178

[3]Curiously the overall student s.d. is less then reported between-class standard deviation in the tables, so this figure is likely to be an upper bound. OLS estimates are zero.

[4]Kang reports much higher figures based on the effect of a change in peer group mean tests scores equal to 1.s.d. of the pupil distribution. We report the effect of a 1.s.d. change in the peer group distribution, which is 0.30 (Table 1)

Table 2: Descriptive Statistics

	Mean	Standard deviation
Age-11 tests, percentiles	50.524	28.821
Age-14 tests, percentiles	50.371	28.779
Age-14 peers' mean age 11 test percentiles, all secondary peers	50.178	8.433
Age-14 peers' mean age 11 test percentiles, peers from other primary	50.060	8.622
– residual within own-primary groups	0.000	5.258
–residual within own-primary-secondary groups	0.000	2.867
Primary school quality estimated effects (age 11 to 7 point scores)	13.775	0.885
Mean primary school quality amongst peers from other primary schools	13.783	0.426
– residual within own-primary groups	0.000	0.192
–residual within own-primary-secondary groups	0.000	0.075
Number of peers from other primary	154.053	53.377
Number of peers from own primary	29.630	25.565

Number of observations 2019455

Table 3: Association between pupil test percentile (age 11 and 14) and secondary age-14 peer group mean age-11 test percentile. *Each cell is a separate regression*.

	OLS			Within-primary			Within-primary x secondary		
Dependent variable	Age-11 tests on peers age-11 tests	Age-14 tests on peers age-11 tests	Age-14, peers age 11 tests | own age 11	Age-11 tests on peers age-11 tests	Age-14 tests on peers age-11 tests	Age-14, peers age 11 tests | own age 11	Age-11 tests on peers age-11 tests	Age-14 tests on peers age-11 tests	Age-14, peers age 11 tests | own age 11
Age-14 peer group:	No pupil control variables								
All secondary year-group peers	0.935 (0.003)	1.051 (0.014)	0.264 (0.014)	0.676 (0.016)	0.741 (0.023)	0.158 (0.012)	0.382 (0.012)	0.104 (0.017)	-0.224 (0.014)
All secondary | own primary school	0.472 (0.016)	0.842 (0.018)	0.434 (0.013)	0.627 (0.019)	0.756 (0.022)	0.212 (0.009)	0.129 (0.012)	0.121 (0.015)	§0.010 (0.013)
From other primary schools only	0.779 (0.013)	0.956 (0.017)	0.300 (0.013)	0.572 (0.020)	0.692 (0.024)	0.200 (0.009)	0.135 (0.013)	0.125 (0.014)	§0.009 (0.011)
Age-14 peer group:	With pupil control variables								
All secondary year-group peers	0.806 (0.008)	0.891 (0.017)	0.215 (0.015)	0.574 (0.018)	0.602 (0.027)	0.107 (0.014)	0.378 (0.011)	0.098 (0.017)	-0.227 (0.014)
All secondary | own primary school	0.372 (0.012)	0.710 (0.021)	0.389 (0.013)	0.508 (0.023)	0.617 (0.024)	0.176 (0.010)	0.122 (0.013)	0.114 (0.017)	§0.008 (0.012)
From other primary schools only	0.618 (0.018)	0.783 (0.022)	0.264 (0.013)	0.452 (0.024)	0.554 (0.028)	0.164 (0.010)	0.130 (0.013)	0.120 (0.014)	§0.008 (0.011)

Table reports regression coefficients, and standard errors clustered at local education authority level. All coefficients statistically significant at 0.1% level or better, except § insignificant.Unreported control variables are: age in months, ethnic group (7 dummies), free school meal entitled, English first language, male, year dummies, peer group size. Number of observations 2200213. Number of primary school groups 14922. Number of primary x secondary groups

Table 4: Association between pupil tests (age 11 and 14) and secondary peer group quality: more stringent specifications

	Within primary x secondary x postcode			Within x primary x secondary x trends			Within primary x secondary		
	Age-11 tests on peers age-11 tests	Age-14 tests on peers age-11 tests	Age-14, peers age 11 tests | own age 11	Age-11 tests on peers age-11 tests	Age-14 tests on peers age-11 tests	Age-14, peers age 11 tests | own age 11	Age-11 tests on peers age-11 tests	Age-14 tests on peers age-11 tests	Age-14, peers age 11 tests | own age 11
Own year-group (14)	0.093 (0.026)	0.082 (0.026)	§0.005 (0.019)	0.068 (0.010)	0.078 (0.010)	§-0.005 (0.010)	0.139 (0.013)	0.122 (0.013)	§0.002 (0.011)
Younger year-group (13)	-	-	-	-	-	-	0.044 (0.009)	§0.010 0.011)	§-0.028 (0.011)
Older year-group (15)	-	-	-	-	-	-	0.053 (0.010)	§-0.016 (0.011)	-0.061 (0.012)

Table reports regression coefficients, and standard errors clustered on local education authority. All coefficients statistically significant at 0.1% level or better, except §. A pupil's peer group is the group of pupils in same year (grade) in secondary school originating from schools other than pupil's own primary school. Unreported control variables in column 2 are: age in months, ethnic group (7 dummies), free school meal entitled, English first language, male, year dummies, number of peer group size. Number of observations 2197575 columns 1-3, 2200213 columns 4-6, 1782507 in columns 7-9. Trends estimated separately using 599987 auxiliary regressions for each primary x secondary group. Number of postcode x secondary x primary groups = 1450271.

Table 5: Association between pupil tests (age 11 and 14) and secondary peer group quality measured by origin schools' value-added points. Within-primary x secondary models. *Each cell is a separate regression.*

	Peers age-11 tests	Age-11 tests on peers' primary school quality	Age-14 tests on peers' primary school quality	Age-14 on peers primary school quality | own age 11 test score
Mean origin primary school value-added of age-14 peers from other primary	4.043 (0.377)	§0.535 (0.293)	§0.482 (0.359)	§0.023 (0.304)

Table reports regression coefficients, and standard errors clustered on local education authority. All coefficients statistically significant at 0.1% level or better, except §. A pupil's peer group is the group of pupils in same year (grade) in secondary school originating from schools other than pupil's own primary school. Unreported control variables are: age in months, ethnic group (7 dummies), free school meal entitled, English first language, male, year dummies, number of peer group size. Number of observations 2200194. Value added in origin primary schools is based on pupils aged 11 in the year that the main estimation sample is aged 14 – see text for details.

Table 6: Non-linear and complementary effects: Association between pupil test percentiles (age 14), secondary peer group age-11 quintile and own age-11 score quintile. Within-primary-secondary model. All cells are from a single regression.

	Mean peer group quintile 2	Mean peer group quintile 3	Mean peer group quintile 4	Mean peer group quintile 5
Own age-11 score quintile 1	**-0.871	**-0.639	**-0.610	-0.165
Row peer effects p-value 0.000	(0.156)	(0.123)	(0.126)	(0.160)
Own age-11 score quintile 2	-0.093	0.031	0.140	0.324
Row peer effects p-value 0.190	(0.135)	(0.145)	(0.145)	(0.169)
Own age-11 score quintile 3	**0.560	0.251	*0.486	*0.543
Row peer effects p-value 0.000	(0.149)	(0.160)	(0.164)	(0.153)
Own age-11 score quintile 4	*0.549	0.296	0.387	*0.452
Row peer effects p-value 0.000	(0.170)	(0.156)	(0.159)	(0.139)
Own age-11 score quintile 5	0.151	-0.181	-0.290	-0.346
Row peer effects p-value 0.007	(0.146)	(0.139)	(0.136)	(0.173)

Table reports regression coefficients, and standard errors clustered on local education authority. Coefficients represent peer group marginal effects moving right in each own-age-11 quintile group. Coefficients marked ** statistically significant at 0.1% level or better. Coefficients marked * statistically significant at 1% level or better. A pupil's peer group is the group of pupils in same year (grade) in secondary school originating from schools other than pupil's own primary school. Unreported control variables are: age in months, ethnic group (7 dummies), free school meal entitled, English first language, male, year dummies, peer group size, interactions between own-age-11 quintile and own-age-11-linear term. Number of observations 2200213. Replacing peer group test percentile with peer group primary school value added yields insignificant coefficients p-value 0.493

Table 7: Complementary effects: Association between pupil test percentiles (age 14) and secondary peer group age-11 quintile and own age-11 score quintile. Within-primary-secondary model. All cells are from a single regression.

	Share of peer group in age-11 quintile 1	Share of peer group in age-11 quintile 2	Share of peer group in age-11 quintile 3	Share of peer group in age-11 quintile 4	Share of peer group in age-11 quintile 5
Own age-11 score quintile 1	-	-2.358	*-3.513	**-4.874	**-8.598
Row peer effects p-value 0.000		(1.219)	(1.259)	(1.327)	(1.178)
Own age-11 score quintile 2	2.683	-	0.548	3.070	*3.792
Row peer effects p-value 0.022	(1.345)		(1.382)	(1.386)	(1.206)
Own age-11 score quintile 3	-0.139	-0.628	-	3.769	**6.539
Row peer effects p-value 0.000	(1.728)	(1.573)		(1.859)	(1.592)
Own age-11 score quintile 4	-2.642	**-4.484	-2.414	-	3.118
Row peer effects p-value 0.000	(1.216)	(1.303)	(1.773)		(1.431)
Own age-11 score quintile 5	1.875	*3.844	2.594	1.552	-
Row peer effects p-value 0.110	(1.312)	(1.492)	(1.407)	(1.592)	

Table reports regression coefficients, and standard errors clustered on local education authority. Coefficients marked ** statistically significant at 0.1% level or better. Coefficients marked * statistically significant at 1% level or better. A pupil's peer group is the group of pupils in same year (grade) in secondary school originating from schools other than pupil's own primary school. Unreported control variables are: age in months, ethnic group (7 dummies), free school meal entitled, English first language, male, year dummies, peer group size, interactions between own-age-11 quintile and own-age-11-linear term. Number of observations 2200212. "Peer effects" test refers to F-test of row coefficients in the age 11 to 14 value added models.

Table 8: Contextual effects: association between pupil test percentile (age 11 and 14) and secondary age-14 group characteristics, conditional on pupil own age-11 achievement. Each column is a separate regression.

	Full	Boys	Girls	On free meals	Non free meals	Old	Young
Mean Age-11 tests	0.007 (0.011)	0.008 (0.012)	0.005 (0.012)	0.006 (0.019)	0.007 (0.011)	0.005 (0.012)	0.007 (0.011)
Proportion boys	-0.242 (0.667)	0.737 (0.728)	-1.054 (0.736)	1.334 (1.270)	-0.473 (0.668)	-0.143 (0.710)	-0.352 (0.738)
Age at start of year (months)	0.096 (0.105)	0.090 (0.122)	0.075 (0.114)	0.128 (0.160)	0.079 (0.112)	0.077 (0.112)	0.099 (0.115)
Proportion white	-0.063 (0.422)	-0.250 (0.455)	0.141 (0.467)	1.059 (0.794)	-0.184 (0.422)	-0.084 (0.455)	-0.074 (0.428)
Proportion English first language	**2.728 (0.658)	**2.491 (0.656)	**3.059 (0.916)	2.301 (1.232)	**2.582 (0.648)	*2.369 (0.810)	**3.096 (0.632)
Proportion on free-meals	0.300 (0.902)	0.995 (0.961)	-0.485 (1.101)	0.041 (1.374)	0.099 (1.091)	0.661 (0.947)	-0.231 (1.024)
Observations	2.20m	1.10m	1.11m	297105	1.90m	1.08m	1.12m

Table reports regression coefficients, and standard errors clustered on local education authority. All coefficients statistically insignificant at 10% level , except *significant at 1%, **significant at 0.1% A pupil's peer group is the group of pupils in same year (grade) in secondary school originating from schools other than pupil's own primary school. Unreported control variables are: age in months, ethnic group (7 dummies), free school meal entitled, English first language, male, year dummies, peer group size. F-test of group variables excluding proportion English first language, p-value>0.8.

Table 9: Association between pupil test percentiles (age 7 and 14) and secondary peer group mean age-7 and mean age-11 percentiles. Each column is a separate regression.

	Age-7 tests	Age-14 tests | own age 7, 11 test score
Age-14 peers' age 7 tests, from other primary schools	0.121	§0.020
	(0.030)	(0.025)
Age 14 peers' age 11 test, from other primary schools	§0.044	§0.046
	(0.024)	(0.025)

Table reports regression coefficients, and standard errors clustered on local education authority. All coefficients statistically significant at 0.1% level or better, except §. A pupil's peer group is the group of pupils in same year (grade) in secondary school originating from schools other than pupil's own primary school. Unreported control variables are: age in months, ethnic group (7 dummies), free school meal entitled, English first language, male, year dummies, number of peer group size. Number of observations 869447.

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