DISCUSSION PAPER SERIES

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IZA DP No. 6455

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March 2012

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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IZA Discussion Paper No. 6455
March 2012

ABSTRACT

Peer Effects: Evidence from Secondary School Transition in England*

We study the effects of peers on school achievement, with detailed data on children making the same primary to secondary school transition in consecutive years in England. Our estimates show that secondary school composition, on entry at age 12, affects achievement at age 14, although the effect sizes are small. These secondary school peer effects originate in peer characteristics encapsulated in family background and early achievements (age 7), rather than subsequent test score gains in primary school. Our specifications control for individual unobservables and school fixed effects and trends, rendering peer group composition conditionally uncorrelated with student's characteristics.

JEL Classification: I2

Keywords: peer effects, schools, education

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* We would like to thank participants at the Workshop on Economics of Education and Education Policy in Europe, Uppsala, a Centre for Economics of Education conference on School Effects and Student Outcomes, London, participants at the SOLE/IZA Transatlantic Meeting of Labor Economists, Steve Pischke and many others for comments on an earlier version of this paper. The Department of Children Schools and Families provided the data for this project and funded an earlier version through the Centre for Economics of Education's Stratification and School Performance programme.

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/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 students – 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 student 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 aim in this paper is to find out if children gain from being educated in schools alongside high-ability peers. The investigation is carried out by looking at student achievement in national standardised tests in secondary school age 14 (Key Stage 3 tests, ks3)[1] and their achievement in national standardised tests in primary school age 11 (Key Stage 2 tests, ks2), using a unique dataset on the population of students in England's state schools, between 2004/5 and 2007/8. Specifically, our empirical work investigates whether children progress faster academically during their secondary school years up to ks3, if their secondary schoolmates performed well in their primary school at ks2.

[1] Compulsory education in state schools in England is organised into 5 Stages. Details of the English state school system are provided in Section 4.

Students' secondary school peer quality is defined here as the mean of secondary schoolmates ks2 scores, upon intake to secondary school.

In common with other work on peer effects (and other group and spatial effects), the main threats to estimation of a causal influence of peer group prior achievement on individual student academic outcomes are: a) non-random sorting of individuals into groups, implying that unobservable characteristics of individuals tend to be correlated with the characteristics of the group; b) unobservable factors affecting the group simultaneously which, coupled with sorting, can lead individual outcomes and group characteristics to become correlated[2]; c) reverse causality effects running from individuals to the characteristics of the group which will tend to inflate the magnitude of the estimated effects[3] ; and d) insufficiently large variation in peer group quality across students, once steps are taken to mitigate the effects of a)-c). In addition, we highlight another important issue. As we show in our methods below, estimates of peer effects from group prior achievements which control for individual student prior achievement in educational 'value-added' specifications will give downward biased estimates of the influence of a student's current peers, if some, or all, of student and current peers' achievements have been jointly determined by the same factors, such as shared school quality (or peer effects in previous periods).

Our paper has novel elements that deal with these problems, and thus offers several contributions to the literature. Firstly, we employ a value-added and differencing-based research design that controls for individual student fixed unobservables, plus primary-by-year fixed effects and primary-by-secondary fixed effects and trends. This strategy eliminates potential sorting and selection effects and

[2] Manski's (1993) 'correlated' effects; for example if high quality students are attracted to schools with good teachers

[3] Some researchers refer to this as Manski's (1993) 'reflection' problem, but this is not precisely the meaning of the term as described in Manski (1993) or (2000). In these papers the 'reflection' problem refers to fact that the 'endogenous' causal linear effects from mean group behaviour to individual behaviour cannot be separately identified from causal linear effects from mean group characteristics ('contextual' effects), when the mean behaviour of the group is linear in the group characteristics.

controls for unobservable factors affecting students who make the same schooling choices. Secondly, we choose years in our data that allow us to control for student specific trends in achievement and ability using data on test scores at age 7 (Key Stage 1, ks1), much earlier in a student's school career. Thirdly, and crucially, we exploit peer group reformation, during the transition between primary and secondary school occurring immediately after students take standard tests in primary school at ks2 (age 11). At this point of transition, students are reassigned from their old school groups to new school groups and this transition generates large changes in peer group characteristics (on average 88% of a student’s peers are new to them in secondary school). This re-shuffling of peer groups ensures that we have large changes in peer group quality, and allows us to identify the causal influence of peers from contribution of new members to a student's peer group, thus eliminating the potential biases induced by student and peer prior achievements being determined by shared past factors and reverse causality. In addition, we mitigate against re-sorting of students after entry to secondary school, in response to revealed secondary school quality, by basing our peer group measure on the peer group composition in the first year in secondary school. Taken together, these elements of the design ensure that: a) student and peer characteristics are not correlated by sorting of similar students into similar schools; b) the pre-existing characteristics of students and their new peers are not determined jointly by past events that students and their peers shared, or by reverse causality; but c) we are still left with substantial variation in peer group quality.

Ultimately, our identification comes from year to year changes in the secondary school peer group experienced by students making a given primary-secondary school transition, conditional on primary-by-year fixed effects, the ks2 achievements and other characteristics of students making this primary-secondary school transition in each year. All this analysis is carried out on student data aggregated to primary school-by-secondary school-by-year cells. Aggregating the data in this way makes it feasible to eliminate salient fixed effects using standard methods without any loss of information on peer group changes. Aggregation of test scores across groups of students also mitigates some of the problems inherent in individual level value-added models, when test-scores are noisy measures of prior achievement (Todd and Wolpin 2003). We refer to these data cells as primary-secondary 'transition groups'. Related school fixed effects methods are common in the literature (e.g. Hanushek et al 2003, Vigdor and Nechyba 2007, Ammemmueller and Pishke 2006, Ding and Lehrer 2007, Lavy et al 2011, etc). However, our design is unique in that we can implement fixed effects for previous and current school attended. This is only feasible because we study a setting with large scale reassignment between schools. We, thus, control for unobserved primary and secondary school characteristics that are fixed over time for the duration of our sample, and control for unobserved student and family background characteristics that are common to specific school pair choices.

On average, we find that peer effects do have a positive impact on student secondary school achievement: one standard deviation increase in the mean ks2 scores on intake to secondary school is associated with a 0.03 standard deviation increase in student achievement in ks3. The size of the effect is small, and lends weight to the existing international evidence that finds that the causal effect of peer group quality is low down the rankings of factors determining students' academic achievement. We further show that these peer effects originate in peer characteristics encapsulated in family background and early (age-7 ks1 achievements), rather than progression during primary school from ks1 to ks2. There are also marked complementarities between peers and students of different levels of achievement, with good peers benefitting low achieving students, but low-achieving students having an adverse effect on high-achievers,

The rest of the paper is structured as follows: Section 2 of this paper provides an overview of recent relevant literature on the influence of peers on student achievement, outlining relevant methodological issues. Section 3 explains our empirical approach. Section 4 describes the data and how it relates to the school system in England. Section 5 presents and discusses the results, and Section 6 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), purchase of a retirement plan (Duflo and Saez 2000), fruit picking (Bandiera et al. 2005), academic cheating (Carrell et al 2008), check-out throughput (Moretti and Mas 2009), routine tasks (Falk and Ichino 2006), obesity (Trongdon et al. 2008, Carrell et al. 2010), performance in professional golf tournaments (Guryan et al. 2007), to give a few examples. Introspection does suggest that many decisions are linked to similar decisions by a friend or associate, 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 three channels of peer influence: endogenous effects from group behaviour; exogenous or contextual effects from group characteristics, and correlated effects from unobservables that influence members of the group in common. In practical applications, these channels are difficult to disentangle, because mean group behaviour is determined by mean group observable characteristics, so endogenous and contextual effects are not separately identified from the reduced form parameters (the reflection problem). A related challenge is individual self-selection into peer groups. Individuals generally choose the groups to which they belong, so peer group characteristics and unobserved individual characteristics are likely to be correlated through sorting making the distinction between peer effects or selection effects even more difficult.

Peer effects studies have employed various strategies to address these problems. 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. However, more 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 students move from one school to another. Variants of this approach appear in Hanushek et al. (2003), McEwan (2003), Gould et al. (2009), Vigdor and Nechyba (2007) and Hoxby (2000). Occasionally, opportunities arise for empirical analysis based on explicit randomisation, or assignment that appears random in the data (e.g. Sacerdote (2001), Zimmerman (2003), Cullen, Jacob et al. (2003), Vigdor and Nechyba (2007), Sanbonmatsu et al. (2004), Hoxby and Weingarth (2005), Lyle (2007), Carrell et al. (2009). An unusual identification strategy is employed by Lavy et al (2010) who, using data and context very similar to ours, estimate peer effects related to subject specialisation. In a cross-sectional analysis, they find that students in school peer groups that have a comparative prior achievement dis-advantage in, say, maths, do slightly worse in maths than in other subjects.

However, 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 a summary of key papers presented in Table 1. Nearly all the estimates suggest that student 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 student types and non-linearity in response, but almost every paper comes to different conclusions in this respect and we do not attempt a summary here.

Our research design is closest to the papers in Table 1 that use temporal variation in peer group quality over time. In the next section, we outline and justify this empirical strategy for assessing whether students derive any benefit from the prior academic achievement of their schoolmates in England's secondary schools.

3 Empirical strategy

3.1 Linear in means regression estimates: methods

The starting point for our design is the linear-in-means peer effect specification that has become widespread in the literature. The end point is a specification that controls for individual fixed effects (using a 'value-added' transformation), primary-school-by-secondary-school fixed effects and primary-by-year fixed effects. The notation below sets out our empirical model, where the intention is to estimate the causal peer group effect from secondary school peers' prior ks2 achievements ($ks2_{st}$) on the subsequent ks3 achievements ($ks3_{pst}$) of students (*i*) in a given primary-by-secondary-by-year (*pst*) transition group. The subscript *s* here refers to the secondary school as a whole. Student ks3 scores are determined by a wide range of other attributes, other than peer effects, namely student ability and background characteristics (a_{pst}), unobserved secondary school-by-year effects (u_{st}) and other unobserved components (ε_{pst}). Setting these out in a simple linear model for estimation, we have:

$$ks3_{ipst} = \rho a_{ipst} + \beta ks2_{st} + u_{st} + \varepsilon_{ipst} \qquad (1)$$

Our aim is to get consistent estimates of β, interpreted as the causal effect of peer group ks2 on subsequent ks3 achievement. As stated earlier, our peer group measure, $ks2_{st}$, is based on the peer group composition at the start of the secondary school years (rather than at the time of the ks3 tests), thus is not dependent on subsequent sorting due to students switching schools.[4] The principal threat to identification of the causal effect of this peer group on students' subsequent achievements is therefore that there is non-random sorting of students into secondary schools, such that secondary peer group mean ks2 is correlated with unobserved primary and secondary effects embedded in u_{st} and ε_{pst}, and unobserved student attributes a_{ipst}.The steps we take to deal with these problems are as follows:

Firstly, suppose we control for unobserved characteristics a_{ipst} using prior achievement at ks2 ($ks2_{ipst}$), to give a typical value-added specification. Note that these pre-secondary school ks2 achievements are also potentially determined by individual abilities, plus primary-by-year effects (u_{pt}, representing primary teacher quality, primary school peer effects, or other primary school attributes common to all students in the same primary school year). As a consequence, controlling for students' own ks2 will potentially lead to downward biased estimates of the causal effect of secondary peers' ks2 on ks3. To see this, note that if:

$$ks2_{ipst} = a_{ipst} + u_{pt} + u_{ipst} \qquad (2)$$

then substituting a_{ipst} in (1) leads to the value-added specification for ks3 achievements:

$$ks3_{ipst} = \rho ks2_{ipst} + \beta ks2_{st} - \rho u_{pt} - \rho u_{ipst} + u_{st} + \varepsilon_{ipst} \qquad (3)$$

[4] β is therefore a 'contextual' peer group effect (following Manski 1993) in the sense that the outcomes for members of a group depends on predetermined characteristics of members of the group to which they belong.

This substitution results in a downward bias on ρ, β and on the coefficients of other correlated variables (as in Todd and Wolpin 2003). This is partly because individual ks2 is determined by unobservables u_{ipst} which appear in the unobservables in (3). In addition, both own-ks2 and secondary peers' mean ks2 are strongly correlated with primary-by-year effects u_{pt}, given that many of a student's secondary peers will be students who shared the same primary school. Now, firstly, aggregating (3) across students in primary-by-secondary-by-year transition groups mitigates against these biases, by removing individual-specific components of u_{ipst}, and ensuring that the control for ks2 prior achievements of the transition group, which would otherwise be included in the secondary peer-group mean $ks2_{st}$. Primary-by-year fixed effects (f_{pt}) primary-by-secondary fixed effects (f_{ps}), and primary-by-secondary trends ($f_{ps}t$) can then be used to control for the salient unobserved components in the aggregated version of (3):

$$ks3_{pst} = \rho ks2_{pst} + \beta ks2_{st} + f_{pt} + f_{ps} + \theta f_{ps} t + v_{pst} \tag{3a}$$

To estimate (3a), the primary-by-secondary-by-year aggregated data is first-differenced over time to eliminate primary-by-secondary fixed effects:

$$\Delta ks3_{pst} = \rho \Delta ks2_{pst} + \beta \Delta ks2_{st} + \Delta f_{pt} + \theta f_{ps} + \Delta v_{pst} \tag{4}$$

then differenced again to eliminate the primary-by-secondary trends:

$$\Delta\Delta ks3_{pst} = \rho \Delta\Delta ks2_{pst} + \beta \Delta\Delta ks2_{st} + \Delta\Delta f_{pt} + \Delta\Delta v_{pst} \tag{5}$$

Further, differencing within primary-by-year groups eliminates the first or second differenced primary-by-year effects.[5]

$$\Delta \tilde{ks}3_{pst} = \rho \Delta \tilde{ks}2_{pst} + \beta \Delta \tilde{ks}2_{st} + \theta \tilde{f}_{ps} + \Delta \tilde{v}_{pst} \tag{6a}$$

$$\Delta\Delta \tilde{ks}3_{pst} = \rho \Delta\Delta \tilde{ks}2_{pst} + \beta \Delta\Delta \tilde{ks}2_{st} + \Delta\Delta \tilde{v}_{pst} \tag{6b}$$

[5] Note that, inspection of equations (3)-(6) shows that this multi-way differencing strategy is justifiable in our case, even though such strategies are not so in general. Equations (6a) and (6b) are equivalent to including f_pt dummies in the first or double-differenced panel regressions.

An alternative approach that avoids this within-groups differencing, and so places less extreme demands on the data, is to use mean ks2 achievements in primary-by-year groups ($ks2_{pt}$) as a control variable for f_{pt}, yielding specification such as[6]:

$$\Delta\Delta ks3_{pst} = \rho\Delta\Delta ks2_{pst} + \beta\Delta\Delta ks2_{st} + \gamma\Delta\Delta ks2_{pt} + \Delta\Delta v_{pst} \qquad (7)$$

3.2 Balancing tests: methods

Our identifying assumption is, therefore, that variation in $ks2_{st}$ is uncorrelated with other factors determining a student's ks3 test scores, after conditioning on ks2 test scores and subjecting the data to these time difference and within-school-by-year transformations. In other words, the change $\Delta ks2_{st}$ or acceleration ($\Delta\Delta ks2_{st}$) in secondary school peer group quality experienced by members of a primary-secondary transition group, relative to their predecessors in the previous year, is uncorrelated with other factors driving corresponding change, or acceleration, in the transition group's gain in test score ranking between ks2 and ks3.

We will demonstrate that this condition is satisfied in our data, by showing that our estimates of β are insensitive to whether or not we control for a wide range of additional student characteristics. We also exhibit balancing tests that confirm that the observable characteristics (and hence, we conjecture, unobservable characteristics) of the transition group are uncorrelated with the innovations to peer group quality $\Delta ks2_{st}$ and $\Delta\Delta ks2_{st}$. These balancing tests are carried out by re-estimating equations of the form of (6) and (7), but replacing the dependent variable with various student characteristics that pre-date their ks2 tests and entry to secondary school (family background, ks1 test scores, neighbourhood variables).

[6] Note that (7) is basically a generalisation of the fixed effects estimator in which changes in achievement ks3-ks2 are regressed on the change in peer group quality between primary and secondary school, but allowing for mean reversion and controlling in addition for group specific trends.

3.3 Peers' background versus peers' primary schooling: methods

In the setup above, peers' ks2 test scores are just a marker for pre-secondary school achievement, which could embody: a) peers' background characteristics and other 'contextual' factors (income, genetics, prior effort, parents etc,) – i.e. factors incorporated in *a* in equation (2); b) teaching quality or other factors in peers' primary schools that are common to children attending those schools – i.e. factors incorporated in *u* in equation (2). Knowing which of these matters is important, because effects originating from prior schooling quality are more interesting from a policy perspective, potentially implying long run social multiplier effects as school quality feeds through to others in later years through peer effect mechanisms. We assess the relative importance of these sources of influence, by substituting a set of peer background variables in place of ks2_pt in equation (7) (as proxies for the components in *a* in equation (2) alongside a measure of peers' primary school quality (as proxies for *u* in equation 2). For the latter, we use data peers ks1-ks2 value added (test score gains), or, alternatively, the mean ks1 to ks2 value-added for a subsequent cohort of students that is not included in our estimation sample. More precisely, for this younger cohort of students ending primary school in 2008, we regress students' primary school ks2 test scores on students' ks1 tests, with controls for student characteristics and primary school fixed effects. Averaging these primary school fixed effects amongst secondary school peer groups provides an estimate of the mean primary school quality of a student's secondary school peers.

3.4 Heterogeneity, complementarities and non-linearities: methods

Next, to address questions about heterogeneity in students' response to their peers, and complementarities between student and peer characteristics, we estimate equation (7) separately for different student groups. Estimation in this case requires that we re-aggregate the data into primary-by-secondary-by-student type-by year (*psgt*) groups:

$$\Delta\Delta ks3_{psgt} = \rho_g \Delta\Delta ks2_{psgt} + \beta_g \Delta\Delta ks2_{st} + \gamma_g \Delta\Delta ks2_{pt} + \Delta\Delta v_{psgt} \qquad (8)$$

such that, for example for Boys, we are differencing over time within groups of Boys making the same primary-secondary school transition. Coefficient β_g provides an estimate of the influence of secondary school peers ks2 on students of type g.

Finally, we also consider non-linearities in response to peers and complementarities between students with different prior ks2 achievements by estimating (7) separately by own-ks2 quintile q (in the national distribution), and by replacing peers' mean ks2 with variables for the proportion of peers $\pi ks2r_{st}$, $\pi ks2r_{pt}$ in each quintile r (in the national distribution):

$$\Delta\Delta ks3_{psqt} = \rho_q \Delta\Delta ks2_{psqt} + \sum_{r \neq q} \beta_{qr} \Delta\Delta \pi ks2r_{st} + \gamma_{qr} \Delta\Delta \pi ks2r_{pt} + \Delta\Delta\omega_{psqt} \tag{9}$$

In the results, we report the coefficients β_{qr}.

All the above methods are applied to administrative data on school children in England. In the following sections we describe the institutional setting for our analysis, and the data we use.

4 England's school context

Compulsory education in state schools in England is organised into five "Key Stages". The primary phase, from ages 4-11, spans the Foundation Stage, Key Stage 1 (ks1) and Key Stage 2 (ks2). At the end of ks2, when students are 10-11, children leave the primary phase and go on to secondary school where they progress through to Key Stage 3 (ks3) at age 14, and to final qualifications at 16 (GCSE). At the end of each Key Stage, prior to age-16, students are assessed on the basis of standard national tests, although the ks3 tests were abolished after 2008. Our study uses these national tests as a basis for estimating the effects of school intake quality on student achievement.

An important institutional factor underlying our analysis is the school admissions process at secondary level in England, since this governs the way students are allocated to schools. Our sample focuses on Comprehensive state schools, which do not systematically select students on the basis of prior achievement or entrance exams and represent over 90% of state school students. There are about

2700 secondary schools of this type England and about 14,500 primary schools.[7] For these schools the admissions process is one that might be called 'geographically constrained choice'. Applications are handled centrally by the relevant Local Authority (LA), and in London admissions across LAs are coordinated by a pan-London admissions body. Applicants list schools in order of preference, and in principle can choose any school. In practice, however, the choice is severely constrained by the rules that apply when schools are over-subscribed. These rules depend in part on the type of school in question.

The large majority of students attend ‘Community Schools’ (64% at secondary level). In this case, the LA employs the school’s staff, owns the school’s land and buildings and has primary responsibility for deciding the arrangements for admitting students. In the case of oversubscription, the LA applies a standard set of criteria for deciding admissions, typically prioritising children with siblings in the school and those who live closest. Most other schools are faith 'Voluntary Aided' schools (15%) or are governed by some other charitable foundation (17%). Usually, these schools have greater autonomy from the LA than Community schools and their oversubscription criteria may prioritise children who are practising in the religious denomination of the school. Other school types include faith schools under Local Authority control ('Voluntary Controlled', 3%), City Technology Colleges (0.3%) and Acadamies (0.76%). The Academies are something like US Charter schools and have greater autonomy in admissions procedures, but are still constrained by a national Schools Admissions Code[8], and do not admit students systematically on the basis of test scores or other measures of achievements. Some Voluntary Aided, Foundation, CTC and Academy schools admit a minority (<10%) of students on the basis of aptitude in special skills such music.

[7] In some areas, a minority of students attend a Middle school between the primary and secondary phases. There are also some selective state Grammar schools which have entrance exams, and Local Authorities which have grammar school systems with a tracking test at age 11. We drop all these students and schools from our sample. There is also small private sector, taking around 7 percent of students, but we do not have data on these students.

[8] The Schools Admission Codes sets out rules for admissions criteria. Notably, student ability or family income cannot be used as a criterion and schools should not interview parents and children.

The implication of these admissions arrangements is that there is a lot of cross-sectional variation between schools in terms of the average achievements and characteristics of their intake. This variation exists because of the geographical location of the school and the characteristics of the residential neighbourhoods from which it recruits, and because of its reputation and ethos, and hence the types of families it attracts. Gibbons and Telhaj (2007) document some of these secondary school intake differences in ks2 achievements. Clearly, this cross-sectional variation is of limited use as a source of variation for estimating causal peer group effects, because it is the result of selection and sorting of students into schools on the basis of long-run and easily observed school characteristics, which will lead to spurious correlation between individual and group achievements. However, there is also considerable variation within schools, from year to year. This variation occurs because demographic changes and changing patterns of demand interact with the LA and school admissions criteria, to generate changes in the types of students admitted. One principle reason for this is that the geographical catchment areas of schools tend to expand and contract according to demand, which is in turn driven by the size of the age cohort in the population. Therefore in any year, families may have to compromise on the schools they apply for, and may not be awarded their first choice of school. Although the data on school admissions indicates that nationally, some 84% of families get their first choice school (DfE Secondary School Applications and Offers in England data 2011), this figure is potentially misleading about fulfilment of preferences, because families are unlikely to request schools for which they have no chance of admission. For instance, LAs typically publish the maximum geographical radius to which offers were made from each school in the previous year, which is likely to deter families from listing preferred schools that lie beyond this distance. In short, there is always some compromise and an element of uncertainty involved in choice of school, meaning not all choices are optimal. Our empirical analysis will exploit the putatively random components of this variation over time as a source of exogenous variation in intake and peer group quality.

Our analysis will exploit school-age-cohort mean prior achievement (and other characteristics) as a measure of peer group quality. Using school-age-cohort peer group definitions avoids biases induced by within school sorting and selection, and provides a consistent estimate of the peer effect in class groups if the assignment to classes within schools is random. However, a common counter-contention is that school-wide peer group definitions mask the causal effect of class peer groups, because setting (streaming) within schools implies that a given student does not experience the peer group implied by the school-mean peer characteristics. In practice, for marginal changes in school peer group mean achievement, setting/streaming into classes that are stratified by prior achievement is unlikely to undo the relationship between improvement in school-mean prior achievement and class-mean prior achievement. Any rightward shift in the distribution at school level will cause a rightward shift in the mean in each stratified class group, so raising the peer group mean for students in the middle of the distribution in each class group. However, for large non-marginal changes in school intake, students with achievement at the bottom of each class in a stratified class structure would find themselves in a lower set, so would experience a deterioration in peer group quality within their class as a consequence of a school-mean increase in intake quality. Similarly, students who would have been at the top of a class could find themselves at the bottom of a higher class if there was a deterioration in school-mean intake achievement. The exact consequences clearly depend on the specific institutional context.

Generally, in England's Comprehensive schools, students are not taught in the same groups for all lessons but mix with students from throughout their age-cohort, which motivates our school level peer effects approach. Although there are no recent comprehensive surveys of practice in England's secondary schools, what evidence there is (Ireson et al 2010), combined with anecdotal evidence and personal knowledge of the system indicates that ability setting is prevalent, but not pervasive. It is more likely to occur in maths, and in science where the ks3 tests were organised into 'tiers', two in science and four in maths. In these subjects, students were entered into the tests in a specific tier which tested across their ability levels, and students could only achieve a result on the test that was within the range of the tier into which they were allocated. It is worth noting, however, that our findings on complementarities between peer groups and individuals of different ability (in the results below) suggest that individuals are more sensitive to peers in ability ranges that are very *different* to their own, which counts against ability grouping being a major factor in English schools at the ages we study. In the absence of information on classes, or subject specific streaming practice, we maintain school-wide measures of peer quality as the best indicator of peer group exposure available to us.

5 Data sources

The UK's Department for Eductiion (DfE) collects a variety of data on state-school students centrally, because the student assessment system is used to publish school performance tables and because information on student numbers and characteristics is necessary for administrative purposes – in particular to determine funding. A National Student Database (NPD) holds information on each student's academic assessment record in the Key Stage Assessments throughout their school career, starting in 1996. For our period of study, assessments at ks1, ks2 and ks3 (ages 7, 11 and 14) included a test-based component and teacher assessment component for core curriculum areas. At ks2 and ks3, these core subjects were maths, science and English, with reading, writing and maths tested at ks1. We work with the overall test score in these subjects at ks2 and ks3, and with a points-based grading system at ks1. All scores are converted into percentiles of the student distribution within our estimation sample and so the results are scaled as effects on student rankings within the national distribution of school achievement[9]. Using these data we create own-achievement measures at ks1, ks2 and ks3 and calculate peer group mean ks1 and ks2 achievement at the point of entry into secondary school.

[9] A complication arises in that the maths and science tests at age 14 are structured into tiers, with students sitting different tests according to their abilities. This means that the scores for different students are not directly comparable. However, students 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 students within the Level they achieved and so recover their overall position in the achievement distribution.

Since 2002, a Student Level Annual Census (PLASC) records information on students 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 student's assessment record in the NPD, giving a large and detailed dataset on students along with their test histories.

From these sources we derive an extract that follows four cohorts of children from their ks1 primary school test score results at age 7, through to their ks2 tests at age 11, and on to their ks3 secondary school results at age 14. These four age-cohorts took their ks3 tests in 2004/5-2007/8. Various other data sources can be merged in, either at school level (school types and other characteristics) or at students' residential neighbourhood using postcodes and Census area codes. In our empirical analysis, we will use various Census 2001 residential neighbourhood characteristics (including unemployment rates, adult qualifications, proportion of socially rented homes, and ethnicity) as control variables, and for balancing tests. Our data covers students in all comprehensive state schools (non-selective) of the types discussed in Section 4.[10]

This large and complex combined data set provides us with information on around 1.6 million children aged 14 for the period 2004/5-2007/8.

6 Results

6.1 Description of the key variables

Table 2 presents the descriptive statistics for our main estimation sample. The underlying sample contains 1578078 students, but the descriptive statistics relate to the aggregated primary-secondary school transition groups which form the basis for our estimation. The main ks2 and ks3 test score variables are based on a student's percentile rankings in national tests, so have a mean of 50 and a standard deviation of about 28.8 in the student distribution. The standard deviations in the primary-

[10] We also estimated on the subset of Community schools only, because we were worried about potential selection into Faith schools and other distinctive school types, but the results were very similar to the main results presented below.

secondary transition group cells are slightly less than this at around 22. The statistics for the secondary 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 students' mean test scores on entry to secondary school. The standard deviation of peer-group mean test score percentiles in levels in row 3 is around 40% of the standard deviation in the distribution across transition groups, at just over 9.3 percentiles (i.e. 16% the variance is between groups). First differencing halves this figure to 4.4 percentiles (4% of the variance is between groups). Double differencing increases the standard deviation back to just over 7 percentiles (10% of the variance is between groups).

The (unweighted) group sizes are reported in the rows 6-8 of Table 2. On average, across transition groups, there are around 185 students in a secondary school age cohort and the average primary-secondary transition group size is 8 students. Note, however, that the respective means weighted by the number of students in each transition group (rows 9-11) imply that for the average student, around 87% of the secondary school peer group is composed of new peers from other primary schools. The final six rows report the number of schools represented in our cleaned data set. Over all years of the estimation sample we have 14160 primary schools, 2727 secondary schools and 59871 primary-secondary transition groups. Once we difference the data we lose years and, hence, some schools and primary-secondary transition groups when these are not represented in multiple years. In the double-differenced dataset we have 13306 unique primary schools, 2527 unique secondary schools and 33484 transition groups.

6.2 Linear-in-means peer effects on ks3 test scores: results

We now turn to the estimates of the links between student test score outcomes and their peer group measure, based on least squares estimation of equations (6a and 6b). The estimates of the coefficient of interest (β) in the various specifications are shown in Table 3.[11] Columns 1 and 2 provide simple

[11] Regressions are weighted by secondary school size. Alternative weighting systems – e.g. weighting by transition group size - produced similar results.

OLS estimates without any differencing or fixed effects, and are shown as a benchmark for reference only. Perhaps unsurprisingly, regression of transition-group mean ks3 test scores on secondary school ks2 percentiles in column 1 yields a coefficient close to one, because student ks3 scores strongly related to their previous scores at ks2, and the primary-secondary transition group is nested within the secondary school group. The estimate in column 1, thus, combines the effect of own-ks2 scores on own-ks3 scores, sorting of high ability students into high-ks2 groups, and any effects related to peer group influence on ks3 scores. The next step in column 2 is simply to transform to a ks3 to ks3 value-added model by controlling for transition-group own-ks2 (equation 3a). This conditioning on ks2 yields a lower coefficient, but obviously not one we would wish to take seriously given the issues of non-random sorting of students into secondary schools discussed in Section 3. The remaining columns of Table 3 introduce the differencing and fixed effects strategies presented in Section 3, and are grouped into sets of 4 specifications. Columns 3-6 apply first-differences of the data within primary-secondary transition groups and include primary-by year fixed effects. Columns 7-10 apply double differences plus primary-by-year fixed effects. Columns 11-14 apply the double differences, but drop the primary-by-year fixed effects and proxy primary-by-year fixed effects with mean primary-by-year ks2.

The first specification in each group (columns 3,7,11) is a value-added specification for ks3, conditional on ks2. The second specification (columns 4, 8 and 12) adds in earlier ks1 (age-7) test scores to control for student specific trends in achievement the primary school phase. The third specification (columns 5,9,13) brings in a control variables set (x) describing the students in the transition group and the schools they choose. The student demographic characteristics are gender, free meal entitlement (a proxy for low income), 8 ethnic group dummies, month of birth dummies (within the school year), and a dummy for English first language. The control variable vector also includes dummy variables for the proportion of the primary school making that particular primary-secondary transition in a given year (split into deciles, a control for popularity) and school-by-year student numbers (for primary and secondary schools). The fourth specification (columns 6,10, 14) brings in another control variable set (*n*) characterising the neighbourhood (census 2001 output areas) in which students in the transition group live, namely the proportion with no qualifications, proportion high-qualified (degrees), proportion born in the UK, proportion ethnically white, proportion in employment, and proportion social renting.

Looking across from columns 3 to 14, one thing is striking: the estimate of the effect of peer group ks2 on student's ks3 scores remains extremely stable. Once we have conditioned on ks2 test scores, first-differenced the data within primary-secondary transition groups, and controlled for primary-by-year effects on ks2, the variation in peer group ks2 scores appears to be largely uncorrelated with other factors influencing student ks3 achievements. Adding in additional control variable sets (ks1, x and n) makes very little difference. Double differencing to remove primary-by-secondary trends makes the results less precise, but the point estimates are almost unchanged relative to the first differenced specification. The double differenced specification with primary-by-year fixed effects places quite high demands on our data, because we have just under 60000 double-differenced observations and just under 26000 primary-by-year cells. The potentially more efficient approach in which we replace primary-by-year fixed effects by a primary-by-year ks2 control variable in the double-differenced specification yields more precise, but essentially unchanged estimates.

Although these coefficients are statistically significant and stable across specifications, the implied effect sizes are fairly small. The coefficients of around 0.075 imply that a 1 percentile increase in mean test scores of the intake to secondary school raises student achievements by 0.075 percentiles. This is not negligible, but scaling in terms the standard deviations shows that these effects do not make a very large contribution to the distribution of test scores across students. A one standard deviation increase in the mean ks2 scores on intake to secondary school (9.3 percentiles) is associated with a 9.3 x 0.075/22.2 = 0.03 standard deviation increase in student achievement as a result of peer group effects. This figure is small, but very much in line with the findings of other studies worldwide (see Table 1).

6.3 'Balancing' tests: results

The stability of the peer effect estimates in Table 3 suggests that the components of the observable characteristics of students that are relevant for ks3 scores are generally uncorrelated with secondary peer group ks2 in the differenced and double differenced models. More explicit tests of the extent to which student characteristics are correlated with secondary peer group ks2 are provided in Table 4. The list of characteristics is not exhaustive, but we present a selection which characterise distinct aspects of the student background and which are only moderately correlated with each-other (to avoid redundancy in the tests). The tests presented in Table 4 are analogous regressions to those in Table 3, but with a student background characteristic replacing student ks3 as the dependent variable. The top panel is analogous to column 3 in Table 3, using first-differenced data and primary-by-year fixed effects. The second panel is analogous to Table 3, column 7 and the bottom panel analogous to column 10.

Scanning across Table 4, it is evident that almost none of the coefficients is statistically significant, and most are small in magnitude. When thinking about the potential implications for the validity of findings in Table 3, it is necessary to consider both the magnitude of the coefficients in Table 4 and the potential effect of the corresponding variable on ks3 achievements. As an example, the coefficient on the percentage with English first language (EFL) appears quite large and comparable with the peer coefficient in Table 3, and is also statistically significant in the bottom panel of Table 4. The coefficient implies that a 1 percentile increase in secondary school intake mean peer ks2 is associated with a 0.074% increase in the proportion of students classed as EFL. Clearly, this coefficient cannot be given a causal interpretation and implies some degree of sorting of students with English as their first language into groups with high ks2, even in terms of year-to year changes. However, from the (unreported) coefficient in our ks3 value-added models, students with EFL have ks3 scores that are 0.022 percentiles lower than non-EFL students, conditional on ks2 scores. Therefore, the implied effect of this sorting would be lower student ks3 scores by 0.074*0.022 = 0.0016 percentiles, when peer group ks2 scores are 1 percentile higher. Even when the coefficients on these characteristics in the value-added models and the coefficients in Table 3 have the same signs, the implied effects on ks3 are always way too low for any sorting evident in Table 4 to be able to explain the peer effects shown in Table 3. This much was already evident from the insensitivity of the estimates in Table 3 to the control variables used in the regressions, but the balancing tests reinforce the point.

6.4 Peers' background versus peers' primary schooling: results

In Table 5, we extend the specifications in Table 3 column 14 to split peers' mean ks2 scores into a value-added component (ks1 to ks2 test score gains), early achievements (ks1 scores, at age 7) and a wider range of peer characteristics in addition. The aim of this analysis is, as discussed in the methods section, to try to distinguish whether peer influence works purely through the background characteristics of students (i.e. the "a" factors, income, genetics, prior effort, parents etc. in equation 2), which of these characteristics matters, or whether the resources and teaching input in primary schooling (i.e. the "u"-factors in equation 2) are more important.

Column 1 of Table 5 is the same as column 14 of Table 3, but with secondary peers' mean ks2 scores replaced by their mean ks1 scores and their ks1-ks2 test score gains. The significant coefficient on ks1 scores, and smaller insignificant coefficient on ks1-ks2 value added indicates that the peer characteristics already embodied in early achievements at ks1 at age 7 are more important than the academic skills acquired between ages 7 and 11 in driving peer effects at ks3. Column 2 takes this analysis a step further by adding in a selection of peer background characteristics, namely the proportions entitled to free meals, who speak English as a first language, who are male, with White British ethnicity and mean age. The coefficients in column 2 indicate again that peer's ks1 scores matter, alongside low income family background (FSM) and a background in which English is not the first language. Other demographic peer attributes are not statistically significant, although the set is jointly significant at the 5% level. Some of the coefficients imply quite large effects from peer characteristics. A student in a peer group which is 100% FSM entitled would end up with ks3 scores over 3 percentiles lower than a student in a group with zero FSM students. A peer group in which

100% speak English raises ks3 scores by 2 percentiles relative to a peer group with no students who speak English as a first language, and this effect is statistically significant. Again, peers' educational gains during primary school (ks1-ks2 value added) do not seem to be important for peer effects.

In column 2, peers' ks1-ks2 test score gain potentially combines effects working from their primary school quality (e.g. teaching quality and educational interventions) with effects due to the averaging of peer group members' individual trends in educational progress. Column 3 replaces this measure of peers' personal test score gains between ks1 and ks2 with peers' primary school mean ks1-to-ks2 value added, estimated from a different cohort. As described in Section 3, this primary school mean ks1-to-ks2 value added is estimated from a younger cohort of students than is used in the main estimation sample (those taking ks2 tests in 2008), so as to more plausibly capture only the effectiveness of the peers' primary schools (or, more precisely, those components of primary schooling that have persisted across cohorts, and are therefore correlated with peers' ks1-ks2 test score gain).

Comparing the coefficients on peers' ks1 and peers' primary value-added confirms the findings from columns 1 and 2 that background characteristics and early achievement (ks1) matter over and above anything contributed by primary schooling. The coefficient on ks1 scores, on its own, is very similar to that on ks2 scores in Table 3, implying that whatever attributes of peer group matter, they are attributes that are already embodied in ks1 test scores by age 7. In contrast, peers' primary school value-added is insignificant and the coefficient is small. In column 4, the peer background variables of column 2 are reintroduced. The importance of peers' background is again evident in terms of the proportion on FSM, the proportion speaking English first language, alongside and in addition to the factors manifested in ks1 test scores.

Broadly speaking, the results in Table 5 show that if peer group matters at secondary school, it matters because of characteristics of peers that are inherent and evident at age 7, rather than anything acquired during the later years of primary schooling. These findings hint that these secondary peer effects are 'contextual' in nature (Manski 1993), that is related to background and students' initial conditions. We find evidence that low-income and language matter in addition to early achievement (ks1 scores), but little or no evidence that other factors sometimes linked to peer effects (age, gender, ethnicity) are very important.

6.5 Heterogeneity, complementarities and non-linearities: results

We turn now to questions about the response of different student types to peer group ks2 achievements, the complementarities between students of different abilities, and non-linearities in the response. Firstly, Table 6 splits the transition-group sample into various sub-groups: boys, girls, children not on free meals, children entitled to free school meals, younger students and older students (split according to month of birth). As explained in Section 3, the data is re-aggregated to primary-secondary transition groups for each student type for this analysis, but the specifications are otherwise the same as Table 3, column 14. There is evidence here of slightly bigger point estimates for boys than girls, bigger effects for FSM students and bigger effects for older students, but the differences are not statistically significant. Overall, the effects of secondary peers' ks2 seem quite general.

Table 7 extends this analysis to students in different ks2 test score quintiles, and generalises the linear-in-means specification to allow for non-linearities across the distribution of peers' ks2 achievement, as explained in Section 3.4. The columns in the table correspond to primary-secondary transition groups of students with ks2 scores in different quintiles. Reading down the rows show the effects of different peer group ks2 quintiles on each of these student quintile groups, relative to having peers in the same ks2 quintile (the base line is the diagonal in the table).

There is some evidence in the point estimates showing that student achievements are positively related to peer achievements in all these groups: the coefficients below the diagonal tend to be positive, and those above the diagonal tend to be negative, although not in all cases. In terms of magnitude and statistical significance, most of the effects seem to be concentrated on moderately low achieving students (the second quintile) and the highest achieving students (the top quintile). For the 2nd quintile group, having peers of any ability other than their own seems to improve their performance, although it is the highest achieving peers in quintiles 4 and 5 that have the strongest effects. For the top quintile students, having a higher proportions of peers in the 2nd and 3rd quintiles has a marked negative impact on performance, relative to having peers in their own quintile. These findings also count against ability streaming/setting being a major threat to the credibility of our use of school-wide peer group measures, since setting would imply that students were more likely to be affected by the entry of peers in ability groups close to their own. It should be emphasised though that all the effect sizes in Table 7 are fairly modest: if all of a top ranked student's secondary peers outside their primary-secondary transition group were in the lowest ks2 quintile, their own ks3 scores would be lowered by around 6 percentiles.

7 Discussion and conclusions

In England, students 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 and employed a differencing based research design that controls for primary-by-year and primary-by-secondary fixed effects or trends to solve sorting and selection into schools and control for unobservable factors affecting students who make similar schooling choices. Given the richness of our dataset, we have also been able to control for student specific trends in achievement and ability much earlier in a student’s school career by using data on test scores at key stage 1 (age 7). In addition, our peer measure of primary school (ks2) attainment based on secondary school composition of students at the start of their secondary school mitigates potential sorting problems due to students changing schools during their secondary school years. A range of balancing tests support our identification strategy, showing that our peer measure is not correlated with individual student, school and neighbourhood characteristics once we control appropriately for primary-by-secondary and primary-by-year fixed effects.

Our general finding is that school-level peer effects exist, but they are small in magnitude: a one standard deviation increase in the mean ks2 primary school scores of secondary schoolmates is associated with a 0.03 standard deviation increase in student achievement in secondary school ks3 achievement. These peer effects originate in characteristics of secondary school peers that were already evident in their achievements at age 7, and family background issues such as low income and English being second language, rather than academic progression during the later years of primary schooling preceding secondary school entry. This finding suggests a rather limited role for peer effects in amplifying the effects of educational interventions (e.g. social multiplier effects as in Glaeser Sacerdote and Scheinkman 2003), unless these interventions occur very early on in life. Our results show only limited heterogeneity across student demographic types. There are, however, some indications of complementarities (positive and negative) between students of different abilities. Unfortunately, these results do not have clear implications for the most efficient way to group students. For example, high-achieving students (5th quintile prior achievements) experience slightly adverse effects from an increase in the number of below-average ability (2nd quintile) students in the school, but these same below-average students benefit from mixing with high achieving students.

The magnitude of our estimates implies that group composition matters little, relative to other factors (such as student background) that drive differences in achievements between students. This finding is in line with the effect sizes in most other studies on the topic. Scaled relative to other school-level factors that influence student achievement, peer effects could appear much less inconsequential, because schools overall contribute relatively little to differences in achievement between students. [12] Even so, there is clearly considerable dissonance between the academic evidence on school achievement-related peer effects and popular conceptions of the importance of good peers in school choices. We can only conjecture that better peer-groups might provide other immediate and

[12] Kramarz, Machin and Ouazad (2009) find that less than 8% of variance of achievement across students is attributable to school-specific factors. An extensive literature on the effects of school resources also highlights the general lack of conclusive evidence on the effectiveness of measurable school interventions (e.g. see Hanushek 2008).

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 short-run educational advantages. These issues remain open for future investigation.

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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) [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 mother's 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 popular schools	Assignment by lottery	Near zero and insignificant
Sanbonmatsu et al. (2004)	Moving to Opportunity prog.	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 (2007)	North Carolina primary schools	5th grade test scores	Classmates' prior test scores	School fixed effects/	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]
Calvo-Armengol et al (2009)	US secondary schools	Grades 7-12 total score index	School friends' network	Friendship network fixed effects	1 s.d. → 0.07 s.d
Carrell et al. (2009)	US Air Force Academy Students	Grade Point Average	Squadron's member GPA	Random assignment to squadrons	1 s.d. → 0.08 s.d. non-linear effects
Lavy et al (2010)	England secondary schools	Subject specific tests scores	Subject specific prior achievements	Individual fixed effects using between subject differences	1 s.d. → 0.04 s.d. due to lowest achieving peers

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 student 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]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 student distribution. We report the effect of a 1.s.d. change in the peer group distribution, which is 0.30 (Table 1)

Table 2: Description of the key variables. Primary-by-secondary-by-year cells.

	Observations	Mean	Std. Dev.	Min	Max
Primary-by-secondary-by-year cells.					
Ks3 test scores	185181	49.7	22.2	1	100
Ks2 test scores	185181	49.9	21.8	1	100
Secondary peers' ks2 scores					
Levels	185181	50.2	9.33	7	89.5
First differenced	110321	-.107	4.40	-42.4	35.5
Double differenced	59764	-.027	7.09	-56.4	64.2
Secondary school students	185181	184.6	54.8	2	679
Primary school students	185181	39.1	26.0	2	2132
Primary-by-secondary students	185181	8.0	11.5	1	132
Secondary school students (weighted)	185181	196.5	54.9	2	679
Primary school students (weighted)	185181	46.7	24.5	2	2132
Primary-by-secondary students (weighted)	185181	24.6	19.5	1	132
Number of primary schools (all years)	14160				
Number of secondary schools (all years)	2727				
Primary-by-secondary groups (all years)	59871				
Number of primary schools (DD sample)	13306				
Number of secondary schools (DD sample)	2527				
Primary-by-secondary groups (DD sample)	33484				

Data from National Student Database Statistics for students in comprehensive (non-selective) state schools. Statistics are unweighted except rows 9-11 which are weighted by cell size (number of students per primary-secondary transition group). Peer group composition measured on entry to secondary school, aged 11/12. Ks3 test scores relate to years 2005, 2006, 2007 and 2008.

Table 3: Linear-in-means peer effects on ks3 test scores. Regressions using primary-by-secondary-by-year cells.

	(1) OLS	(2) OLS|ks2	(3) FD|ks2	(4) FD|ks2 ks1	(5) FD|ks2, ks1,x	(6) FD|ks2, ks1,x,n	(7) 2D|ks2	(8) 2D|ks2, ks1	(9) 2D|ks2, ks1,x	(10) 2D|ks2, ks1,x,n	(11) 2D|ks2	(12) 2D|ks2, ks1	(13) 2D|ks2, ks1,x	(14) 2D|ks2, ks1,x,n
Secondary peers' ks2 scores	1.111***	0.358***												
	(0.015)	(0.013)												
Secondary peers' ks2 scores (FD)			0.074***	0.073***	0.076***	0.073***								
			(0.019)	(0.019)	(0.019)	(0.019)								
Secondary peers' ks2 scores (2D)							0.072*	0.071*	0.079*	0.078*	0.073***	0.071**	0.074***	0.073**
							(0.033)	(0.033)	(0.033)	(0.033)	(0.021)	(0.021)	(0.022)	(0.022)
Primary-by-year fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Primary-by-year ks2	No	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes
Observations	185,181	185,181	110,321	110,321	110,321	110,083	59,764	59,764	59,764	59,647	59,764	59,764	59,764	59,647
R-squared	0.206	0.745	0.833	0.834	0.838	0.838	0.850	0.850	0.853	0.854	0.723	0.729	0.731	0.732

Notes: All test scores based on percentiles in the student distribution. Standard errors clustered at local school district. ***0.1%.**1%. *5%. FD = first difference, 2D second difference. Characteristics x are free meal, ethnic group (8 categories), age within school year (13 categories), gender, English first language, proportion of primary school choosing student's secondary school (5 categories), student number in secondary school and primary school yeargroup. Neighbour characteristics n are proportion with no qualifications, proportion high-qualified, proportion born in UK, proportion white, proportion in employment, proportion social renting and are measured at Census Output Area level. Other unreported control variables are own primary-by-secondary-by-year ks2 scores, year dummies (in columns 1 and 2). Regressions weighted by secondary school size. Standard deviations of ks2 test score = 28.8, peer's age 11 scores = 8.6, therefore standardised effect size is of peers is around 0.02. Peer group measured on entry to secondary school, aged 11/12. Data for students taking ks3 in 2005,2006,2007,2008 in comprehensive schools.

Table 4: Balancing tests. Regressions using primary-by-secondary-by-year cells

	(1) Ks1 scores	(2) FSM %	(3) English language %	(4) Male %	(5) Age (months)	(6) White %	(7) Popular secondary	(8) Neighb, high quals %	(9) Neighb. born uk %	(10) Neighb white %	(11) Neighb employed%
Secondary peers' ks2 scores (FD)	0.016	-0.019	0.042	-0.016	-0.000	0.049	0.000	0.018*	-0.011	-0.012	0.012
	(0.019)	(0.039)	(0.032)	(0.064)	(0.005)	(0.042)	(0.000)	(0.009)	(0.007)	(0.012)	(0.010)
Primary-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Primary-by-year ks2	No	No	No	No	No	No	No	No	No	No	No
Observations	110,321	110,321	110,321	110,321	110,321	110,321	110,321	110,083	110,083	110,083	110,083
Secondary peers' ks2 scores (2D)	0.032	-0.035	0.105	0.076	0.003	0.051	0.001	0.022	-0.015	-0.020	0.002
	(0.032)	(0.059)	(0.054)	(0.090)	(0.007)	(0.058)	(0.000)	(0.014)	(0.012)	(0.024)	(0.015)
Primary-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Primary-by-year ks2	No	No	No	No	No	No	No	No	No	No	No
Observations	59,764	59,764	59,764	59,764	59,764	59,764	59,764	59,647	59,647	59,647	59,647
Secondary peers' ks2 scores (2D)	0.016	-0.000	0.074**	0.053	0.000	0.043	0.001**	0.016	-0.003	-0.001	0.011
	(0.017)	(0.034)	(0.027)	(0.054)	(0.004)	(0.034)	(0.000)	(0.008)	(0.006)	(0.012)	(0.009)
Primary-by-year fixed effects	No	No	No	No	No	No	No	No	No	No	No
Primary-by-year ks2	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	59,764	59,764	59,764	59,764	59,764	59,764	59,764	59,647	59,647	59,647	59,647

Notes: All test scores based on percentiles in the student distribution. Standard errors clustered at local school district. ***0.1%.**1%.*5%. FD = first difference, 2D second difference. Regressions weighted by secondary school size. Unreported control variables are own primary-by-secondary-by-year ks2 scores. Peer group measured on entry to secondary school, aged 11-12. Data for students taking ks3 in 2005,2006,2007,2008 in comprehensive schools.

Table 5: Peers' background versus peers' primary schooling. Regressions of ks3 scores on peer characteristics using primary-by-secondary-by-year cells. 2nd differenced regressions

	(1)	(2)	(3)	(4)
Secondary peers' ks1 to ks2 value added	0.034	0.026	-	-
	(0.029)	(0.029)	-	-
Secondary peers' primary school mean ks1 to ks2 value added in 2008	-	-	0.028	0.025
	-	-	(0.017)	(0.017)
Secondary peers' ks1 (age 7) scores	0.098***	0.087***	0.085***	0.077**
	(0.024)	(0.025)	(0.022)	(0.023)
Secondary peers FSME		-3.023*		-2.733*
		(1.239)		(1.229)
Secondary peers English first language		2.013*		1.995*
		(0.812)		(0.813)
Secondary peers male		-0.098		-0.105
		(1.308)		(1.325)
Secondary peers' age (months)		-0.126		-0.158
		(0.205)		(0.201)
Secondary peers White British		-0.267		-0.038
		(1.027)		(1.038)
F-test, peer achievments (p-value)	0.000	0.002	0.000	0.002
F-test, peer demographics (p-value)		0.020		0.058
Observations	59,647	59,647	57,948	57,948
R-squared	0.735	0.735	0.735	0.735

Notes: All test scores based on percentiles in the student distribution. Standard errors clustered at local school district. ***0.1%.**1%.*5%. Regressions weighted by secondary school size. Unreported control variables are own-primary-by-year ks2 scores, and peer group variables, and own primary-by-secondary-by-year ks2 scores, student characteristics and student neighbourhood characteristics. Peer group measured on entry to secondary school, aged 11/12. Data for students taking ks3 in 2005,2006,2007,2008 in comprehensive schools.

Table 6: Heterogeneity by student characteristics. Regressions using primary-by-secondary-by-year cells.

	(1)	(2)	(3)	(4)	(5)	(6)
	Girls 2nd difference |ks2 ks1 x n	Boys 2nd difference |ks2 ks1 x n	Not FSM 2nd difference |ks2 ks1 x n	FSM 2nd difference |ks2 ks1 x n	Younger 2nd difference |ks2 ks1 x n	Older 2nd difference |ks2 ks1 x n
Secondary peers' ks2 scores (2D)	0.055*	0.075**	0.067**	0.113**	0.052*	0.078**
	(0.025)	(0.025)	(0.023)	(0.036)	(0.024)	(0.024)
Observations	41,406	40,918	54,319	17,094	45,807	39,619
R-squared	0.737	0.733	0.732	0.734	0.737	0.744

Notes: All test scores based on percentiles in the student distribution. Standard errors clustered at local school district. ***0.1%.**1%.*5%. Regressions weighted by secondary school size. Unreported control variables are own-primary-by-year ks2 scores, and own primary-by-secondary-by-year ks2 scores, student characteristics and student neighbourhood characteristics. Peer group measured on entry to secondary school, aged 11/12. Data for students taking ks3 in 2005,2006,2007,2008 in comprehensive schools.

Table 7: Complementarities and non linearities in peer effects on student ks3 test score percentile, by student ks2 quintile. Regressions using primary-by-secondary-by-year cells.

	(1)	(2)	(3)	(4)	(5)
	Own ks2 quintile 1 2nd difference |ks2 ks1 x n	Own ks2 quintile 2 2nd difference |ks2 ks1 x n	Own ks2 quintile 3 2nd difference |ks2 ks1 x n	Own ks2 quintile 4 2nd difference |ks2 ks1 x n	Own ks2 quintile 5 2nd difference |ks2 ks1 x n
Secondary peers' ks2					
Proportion quintile 1		6.264	1.788	0.311	-5.852*
		(3.361)	(3.199)	(3.354)	(2.748)
Proportion quintile 2	-0.219		-1.551	-2.475	-7.906**
	(2.022)		(3.527)	(3.486)	(2.395)
Proportion quintile 3	2.215	5.439		-0.580	-1.429
	(2.128)	(3.670)		(3.433)	(2.741)
Proportion quintile 4	1.453	8.545**	3.167		0.345
	(2.171)	(3.043)	(3.919)		(2.512)
Proportion quintile 5	0.621	7.907*	5.598	3.498	
	(2.044)	(3.031)	(4.083)	(3.550)	
F-test, all peer effects zero, p-value	0.759	0.026	0.419	0.396	0.002
Observations	23,969	25,227	25,820	24,573	22,913
R-squared	0.307	0.152	0.132	0.134	0.208

Notes: as Table 6