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Peer Effects and Pupil Attainment: Evidence from Secondary School Transition

Stephen Gibbons

Shqiponja Telhaj

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Executive Summary

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 school-mates must be an important influence on his or her own achievements at school. It is a belief that guides many parents when they are choosing schools, and has important implications for policy on school choice and organisation. This is especially true in the light of current educational policy in the UK and US that favours expansion of parental choice, because choice based on peer-group quality could widen educational inequalities.

In this study, we use the population of state Secondary school pupils in England to tease out how pupil attainments at age 14 respond to differences in the prior, age-11 attainments of their current school grade peer-group.

Our findings are that:

- The abilities of school-mates do influence a child's attainment, but this influence is quite small in magnitude and cannot explain much of the variation in pupils' educational outcomes between ages 11 at age 14.
- Based on our best estimates, moving from the worst to the best 1-in-10 Secondary peer group could improve a pupil's progress between age 11 and 14 by just under 6 percentiles in English and about 4.5 percentiles in Maths.
- Another way to interpret this is to note that peer-groups could account for at most 0.6% of the variance in progress between the ages of 11 and 14, whereas general differences between schools explain about 13% of this variance.
- The influence of new, unfamiliar peers in Secondary school seems to be much stronger than the influence of children who are familiar from earlier schooling phases.
- Improvements in peer-group quality seem to have similar effects in lower-average-ability groups as in high-average-ability groups – which suggests that ability streaming is not educationally effective; any gain to pupils in high ability groups from a further streaming is offset by losses to pupils in low-ability groups
- However, pupils of different ability respond slightly differently to peer-group improvements: pupils in the middle and top of the ability distribution seem to have the biggest response. Pupils in the lowest attainment groups in Maths at age-11

seem gain little from higher-ability school-mates. This means that higher-ability pupils have the strongest incentives to seek out high-ability peers.

- Being educated amongst low-income school-mates – measured by the proportion entitled to free school meals – has no direct effect on a child's attainment once the prior attainment of these school-mates is taken into account.

Looking at these findings it seems unlikely that the balance of success or failure at school will be tipped according to whether a child attends a school alongside other high or low ability children. This might seem puzzling, since peer-group quality seems to be one of the factors that parents seek out when choosing schools. But better peer-groups perhaps 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, aside from any small educational advantages they offer.

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Note on methods:

Measurement of these educational peer group effects is notoriously difficult, because similar pupils tend to group together when choosing schools so peer-group and personal characteristics are always correlated. It is therefore difficult to disentangle the influence of peers from the influence of unobserved personal characteristics. We apply a number of techniques to solving this problem. Firstly, our data on home addresses and school attendance allow us to compare outcomes of children who live in the same street, or who attended the same Primary school up to age 11, but then move on to different Secondary schools alongside new school-mates of various abilities. Secondly we use the effectiveness of the primary schools from which a child's new peers originate to provide us with variation in peer-group quality which is unlikely to be correlated with any unobservable personal factors which might influence a child's progress at school.

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Stephen Gibbons is a lecturer in Economic Geography at the Department of Geography and Environment, London School of Economics (LSE), and a Research Associate at the Centre for Economic Performance, LSE and the Centre for the Economics of Education.

Shqiponje Telhaj is a Research Officer at the Centre for Economic Performance, London School of Economics and an Associate at the Centre for the Economics of Education.

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 pupil's school-mates must have an important influence on his or her own achievements at school. Motivated by this, 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 peer-group quality can, in theory at least, lead to a high degree of sorting across schools along lines of prior ability [Epple and Romano (2000)]. This will exacerbate educational inequalities if peer-group quality has real impacts on personal achievement. An understanding of peer effects is also important because they can mean that educational interventions that appear beneficial to the individual pupil may be even more effective when rolled out to the population [Glaeser, Sacerdote et al. (2003)]. Our paper extends the evidence base by providing estimates of the influence of innovations to a pupil's peer-group at the time when they transfer from Primary to Secondary schooling in England.

2. Background

Peer-group effects are a distinct class of influences arising from 'social interactions' – a broad term which encompasses any type of individual behaviour that involves interdependency with the behaviour or characteristics of others. Economists have long shown an interest [Becker

(1974)], but there has been a rapid growth in the field since the 1990s with contributions in theory and empirical work. Theoretical research seems motivated by a desire to widen the scope of economic thought to encompass aspects of behavioural modelling more commonly attributed to sociology and psychology. Empirical work –constrained by the data – is generally concerned with finding evidence for the existence of such effects, rather than the precise pathways by which they occur.

The term 'peer-groups' usually indicates social interactions of children or young adults with people of similar age, rather than broader 'neighbourhood' effects or interactions with superiors, family or teachers. We continue to use the term in this way. The range of outcomes that have interested researchers is diverse, including smoking [Alexander and et al. (2001); Ellickson, Bird et al. (2003)], joke-telling [Angelone, Hirschman et al. (2005)], sexual behaviour [Selvan, Ross et al. (2001)], purchase of a retirement plan [Duflo and Saez (2000)] and – more commonly – education. On reflection, it seems very likely that many decisions are linked to similar decisions by a friend or other associate (in same 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.

Although the literature on peer effects in education dates back to 1960s with the publication of the famous Coleman Report (1966), the importance of peer-group effects is still disputed. 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. Indeed, as Manski (1993) and Moffit (2001) argue, the empirical analysis of social interactions is plagued by conceptual and data problems.

The first key issue is that measures of peer-group characteristics may be good proxies for unobserved individual, family background or institutional factors that can affect student attainment, making peer effects look important when they may not be. Secondly, group membership is very likely to be endogenous to the outcome under study since people choose their school and their friends, leaving group and individual characteristics highly correlated. Thirdly, peer interaction is simultaneous in that a student affects and is affected by his or her peers (The 'reflection' problem of Manski (1993)) – although if peer effects are structurally unimportant, this source of bias vanishes. Lastly, 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 serious problems of endogenous group membership)?

The earliest studies on peer effects in educational attainment [Hanushek (1971); Summers and Wolfe (1977); Henderson, Mieszkowski et al. (1978)] had mixed findings, but took relatively few steps towards overcoming problems of peer-group endogeneity. Many more recent studies use instrumental variables approaches to try to overcome this, though it is hard to find plausible instruments. For instance, Dills (2005) predicts peer-group changes from introduction of 'magnet schools' that select high quality students, yet the average ability of remaining pupils will be decreasing in the proportion of high quality pupils that leave the school. Similarly, Fertig (2003) instruments the coefficient of variation of peers with variables measuring whether a school selects pupils upon entry and whether the schools are in the private-sector. Goux and Maurin (2005) find good source of exogenous variation in peer-group attributes – the average age – and show that this matters for pupil achievement, but the cause could be average attainments or the average group age itself.

Other approaches are on offer. Hanushek et al. (2003) try to eliminate the problem of simultaneity by employing specifications based on lagged peer achievement, and tackle group selection problems using a fixed effect strategy. Similarly, McEwan (2003) applies a school fixed-effects strategy. Hoxby (2000) relies on the exogenous variation across cohorts in peer composition at the school grade-level in Texas elementary schools. Some other studies have exploited the random assignment of peer to individual students to find a solution to the problem of endogenous sorting of students. For example, Sacerdote (2001) and Zimmerman (2003) use the random assignment of roommates in colleges to find a positive association between roommates' academic attainment and student's own achievement. Cullen, Jacob et al. (2003) exploit the randomised lotteries that determine high school admission in the Chicago Public Schools, finding no systematic pattern of positive achievement and high quality peer-group effects. Sanbonmatsu, Kling et al. (2004) utilise a randomised housing mobility experiment in Boston, Baltimore, Chicago, Los Angeles and New York to isolate the impact of residential neighbourhood characteristics on student educational outcomes. They find that being given the option to move to a richer neighbourhood did not improve pupils' academic performance.

Even empowered with these more sophisticated estimation methods and richer data than earlier studies, researchers are still divided on the importance of peer effects. Some [e.g. Angrist and Lang (2004) ; Arcidiacono and Nicholson (2005)] find no significant relationship between peers and own achievement whilst others [e.g. Hoxby (2000); Zimmer and Toma (2000); Sacerdote (2001); Winston and Zimmerman (2003); Robertson and Symons (2003)] report positive effects. We should emphasise that generally even those studies that find positive peer effects find that they are small. Below we provide the range of peer effects estimates in some recent studies.

Table 1 A summary of some recent peer effect estimates

Studies	Context	Outcome	Peer-group or treatment	Methodology	Approx order of magnitude
Hoxby (2000)	Texas schools, US	Test Scores	Classmates' test scores	Cohort gender and race composition	1.s.d. → 0.4 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	Test Scores	Classmates	School fixed effects	1.s.d. → 0.27 s.d.
Hanushek (2003)	US	Test Scores	School grade	School-by-grade fixed effects	1.s.d. → 0.02.s.d.
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 to new neighbourhood	Random assignment	Near zero and insignificant
Goux and Maurin (2005)	France	Held back a grade in school	Neighbourhoods	IV using neighbours age	1.s.d. → 0.1.s.d.
Ammemueller and Pischke (2006)	European primary schools	Reading test scores	Classmates	School fixed effects	1 s.d. → 0.07 s.d.

Our approach in this paper combines some of these methods. Whilst we have no explicit randomisation in our identification strategy, we believe we can isolate sources of variation in the distribution of peer-groups across Secondary schools that are exogenous to a pupil's own choices and abilities, and which we argue can be used to identify peer-group influences. The source of this variation is differences in the quality of Primary schools that supply pupils to Secondary schools in England, and the fact that there is a considerable degree of compulsory assignment in the allocation of Secondary places. The level of detail in our data allows us to compare outcomes for pupils who go on to attend different Secondary schools, but who live in the same street or attended the same Primary school; and we use information on Primary school effectiveness to predict components of each pupil's new peer-group quality that are uncorrelated with their own abilities. In summary, we identify Secondary school peer-group effects from the fact that Secondary school pupils come from Primary schools of different quality, and from the fact that there is random variation in the composition of this group of 'feeder' schools induced by education authority admissions policies. An advantage of our approach is that we can use a large representative sample of pupils drawn from 99% of the standard state schools in England.

3. Empirical Strategy

Basic model

Empirical estimation of peer-group effects and the influence of social interactions in general is notoriously difficult, because peer-groups form endogenously in ways that are usually related to the outcome in question. In models of educational attainment, sorting into groups along lines of ability is easily confused with peer group effects. A basic first step towards overcoming this obstacle is to try to measure the effect of peer group quality on pupil progress over a number of years, conditional on pupil prior attainment – i.e. in a 'value-added' regression model. The objection usually raised to this strategy is that both the initial and subsequent levels of attainment are likely to be influenced by the same set of school-mates, so value-added-based estimates may be unable to detect a peer group influence. Our way round this is to exploit the major changes to peer group composition that occur when a pupil makes the transition from Primary to Secondary schooling in England at age 11/12, and to measure how these changes influence pupil progress over 3 years from the end of Primary schooling.

On its own though, conditioning on prior attainments may not be enough, and, as we show in the model below, introduces other problems. For a start, value-added models are potentially mis-specified if these prior test scores are intended to capture unobserved individual ability effects [Todd and Wolpin (2003)]. Moreover, the problem is exacerbated when the model also includes the prior attainments of the contemporaneous peer-group. However, we have a lot of data to bring to bear on the problem, and can, we argue, find variation in peer-group attainments at age-11 that is uncorrelated with own ability at 14, except through Secondary school peer-group effects. This variation arises because of cohort-to-cohort changes in the Secondary school intake, in terms of the quality of Primary schools from which they originate.

In general, as pupils move through schooling phases – for example from Primary to Secondary school – they take some schoolmates with them. Let us call this their "familiar" peer group. In their new school they meet new friends, which we shall call their "unfamiliar" peer group. The model we present below is structured so as to draw out differences between the contributions of these "familiar" and "unfamiliar" peers to individual attainments during a given school phase. It also shows that inclusion of familiar peers in peer-group definitions is likely to give misleading estimates of peer group effects of subsequent attainments in models that condition on an individual's prior attainments.

Consider the attainment of a pupil *i* at the end of a schooling phase *t*, h_{it}. One component of this attainment, α_t, is common to everyone at the same phase *t* in the school system (and so captures general progression) but attainment in any phase is modified by individual ability a_i, quality of school attended q_{it} and the mean prior attainment of an individual's school peer group. There may also be unobserved family background or neighbourhood effects n_i. For peer groups, our notation $\tilde{h}_{t,t-1}$ indicates the mean prior (*t-1*) attainment of "unfamiliar" peer group members experienced at phase *t*, and $\tilde{h}_{t-1,t-1}$ the mean prior (*t-1*) attainment of "familiar" peer group members who were also members of the individual's peer group in the previous school phase (*t-1*).[1] So,

$$h_{it} = \alpha_t + \rho_t a_i + \gamma_1 \tilde{h}_{t,t-1} + \gamma_2 \tilde{h}_{t-1,t-1} + q_{it} + n_i \quad (1)$$

Sorting of individuals into groups by ability (or characteristics correlated with ability) means that unobserved ability components a_i are correlated with peer group attainments $\tilde{h}$. This is the fundamental empirical problem that arises in estimation of peer group effects in schools. Moreover, residential sorting of families means that background characteristics (n_i) will be spatially correlated (or there may be other 'neighbourhood effects') such that n_i is also correlated with peer group prior attainments.[2]

Although individual ability a_i is unobserved, a traditional approach in educational models is to proxy it using test scores from a previous period. Note that

$$\rho a_i = \rho h_{it-1} - \rho \alpha_{t-1} - \rho \gamma_1 \tilde{h}_{t-1,t-2} - \rho \gamma_2 \tilde{h}_{t-2,t-2} - \rho q_{it-1} - \rho n_i \quad (2)$$

where $\rho = \rho_t / \rho_{t-1}$, so

$$h_{it} = (\alpha_t - \rho \alpha_{t-1}) + \rho h_{it-1} + \gamma_1 \tilde{h}_{t,t-1} + \gamma_2 \tilde{h}_{t-1,t-1} - \rho \gamma_1 \tilde{h}_{t-1,t-2} - \rho \gamma_2 \tilde{h}_{t-2,t-2} + q_{it} - \rho q_{it-1} + (1-\rho) n_i \quad (3)$$

Replacing unobservable ability with prior test scores has introduced a number of additional unobservable factors in the model, which are (negatively) correlated with the

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[1] Note that, for simplicity, we have specified that all the dynamics in this model occur through peer group quality. In our model, association between individual current and past attainments is due to individual heterogeneity rather than dependence on prior attainment.

[2] In addition, current school quality is correlated with individual and peer attainments, and a pupil's own attainments influence his or her peers – which will make it even more difficult to isolate *contemporaneous* (or 'endogenous') peer effects; hence, we do not attempt this.

observable characteristics: Firstly, we have now introduced unobserved components of school quality in the previous phase, q_{it-1}, which are negatively correlated with individual and peer group prior test scores; secondly, we have introduced peer-group attainment components from the previous school phase $\tilde{h}_{t-1,t-2}$ and $\tilde{h}_{t-2,t-2}$ and these are correlated with the attainments $\tilde{h}_{t-1,t-1}$ of those familiar members of the current-phase peer group who were also members of the individual's peer group in the previous phase. The intuition is that, conditional on prior attainment, it is the kids from the bad previous schools and the low-attainment peer groups who do better in the current phase – because these are the kids of highest ability.

As an extreme case, consider a pupil who goes through two phases of education with the same peers in both phases (for simplicity, assume school quality is irrelevant) and assume that:

In phase 1, $h_{i1} = \alpha_1 + a_i + \gamma_1 \tilde{a}$. (4a)

In phase 2, $h_{i2} = \alpha_2 + \rho a_i + \gamma_2 \tilde{h}_1$ (4b)

So $h_{i2} = (\alpha_2 - \rho\alpha_1) + \rho h_{i1} + \gamma_2 \tilde{h}_1 - \rho\gamma_1 \tilde{a}$ (4c)

Since $\tilde{h}_1 = \alpha_1 + (1+\gamma_1)\tilde{a}$:

$$h_{i2} = \left(\alpha_2 - \rho\alpha_1 + \frac{\rho\gamma_1\alpha_1}{1+\gamma_1}\right) + \rho h_{i1} + \left(\gamma_2 - \frac{\rho\gamma_1}{1+\gamma_1}\right)\tilde{h}_1 \quad (5)$$

This formalises the (perhaps obvious) point that if peer group is unchanging between phases, then the effect of peer group quality on pupil attainment in phase 2 – conditional on pupil attainment in phase 1 – is much less than the unconditional effect of peer group quality. For instance, if $\gamma_1 = \gamma_2 = \gamma$ then the relationship between the peer group and phase-2 *value-added* is $\frac{\gamma - \rho\gamma + \gamma^2}{1+\gamma}$, which will be small if ρ is close to 1, given the range of estimates of γ presented in Table 1. The relationship may even be negative, when $\gamma_2 < \frac{\rho\gamma_1}{1+\gamma_1}$, such that pupils in high-attainment peer groups in Phase 1 and 2 will tend to have relatively low personal attainment in Phase 2 conditional on their own attainment in Phase 1. This would be the case if, for psychological and emotional reasons, familiar peers and friends have *less* of an impact on attainment in the current schooling phase than new, unfamiliar peers. Either case is intuitively plausible, so this poses an interesting empirical question; as we shall see later, our evidence supports the conjecture that unfamiliar peers matter more.

So, although conditioning on prior attainment is desirable in order to control for unobserved individual ability and background factors, it can be misleading because prior attainment already captures the influence of peers that were experienced in prior phases. One solution would be to estimate models like (3) or (4c) with information on peer group composition and attainment in previous phases, but this information is rarely available. Another approach, which we adopt below, is to focus only on the influence of *unfamiliar* peers on the gain in pupil attainment between phases. In other words, we estimate pupil-level educational attainment functions like:

$$h_{it} = (\alpha_t - \rho\alpha_{t-1}) + \rho h_{it-1} + \gamma_1 \tilde{h}_{t,t-1} + q_{it} - \rho q_{it-1} + (1-\rho) n_i + \mathbf{x}'_{it}\boldsymbol{\beta} + u_{it} \quad (6)$$

where *familiar* peer groups are subsumed into a general error term u_{it} and the basic model has been augmented with a vector of pupil family background characteristics $\mathbf{x}_{it}$. The first term on the right hand side, in brackets, is just a constant, because we only consider two phases of schooling. We can deal with neighbourhood, or spatially auto-correlated unobserved family background effects by allowing for street level (postcode) fixed effects. Note, we do not, in principle, need to control for prior school quality since this is unlikely to be correlated with the attainments of *unfamiliar* peers, though we have the option of controlling for Primary school effects since children from the same primary school go on to attend different secondary schools[3].

However, ignoring *familiar* peers in this way does raise obvious difficulties if unfamiliar and familiar peer group characteristics are correlated. We might also worry that there are other unobserved individual components that are correlated with peer group attainment through sorting (even conditional on school or neighbourhood fixed effects). However, we argue below that our data allows us to derive an instrument for *unfamiliar* peer group prior attainments that circumvents both these problems.

Instruments for own and peer-group attainment

Our proposed instrument for the mean attainment of a pupil's new peer-group on transition from primary (t-1) to secondary (t) school phases is a measure of the teaching effectiveness of schools from which these unfamiliar peers originate. We measure this school effectiveness using pupils' average gain in attainment between ages 7 and 11 at each Primary school. *Importantly, whilst we use the cohorts aged 14 in 2002 and 2003 for estimating our main equation (6), we use the cohorts age 11 in 2002 and 2003 to construct our instrument from primary school value-added.*

The identifying assumptions behind this strategy are: a) that the estimate of the age 7-11 value-added of the age 11 cohort is only correlated with the age-11 attainments of the age-14 cohorts who attended the same school because of differences in Primary schools generated by

[3] Basing peer group effect estimates on *unfamiliar* peers and not all peers also means that we could, technically, include Secondary school fixed effects. However, variation in the unfamiliar peer group within Secondary schools occurs only because of variation in the familiar peer group so this is not very helpful to our analysis as structured in Section 0.

resource allocation, teaching quality, leadership and other institutional factors; b) that peers' Primary school effectiveness is only correlated with a pupil's own attainments at age 14 because it has a direct impact on peer's prior attainments that feeds through to the pupil via Secondary school peer group effects. These assumptions need only hold conditional on a pupil's own prior attainments, pupil characteristics, and neighbourhood or primary school fixed effects that can be included in (6). In other words, we assume that pupils do not choose Secondary schools based on the future 'value-added' of younger cohorts in the Primary schools attended by pupils who will form their Secondary school peer-group.

Further empirical problems arise in practice because the test score for pupil *i* at the end of phase *t* combines educational attainment h_{it} with a component capturing test measurement error, ability shocks or other noise ε_{it}. Substituting snap-shot test scores as measures of attainment (both own and group) introduces additional noise components (this is the issue raised by Todd and Wolpin (2003)). However, we have other instruments available for a pupil's own attainment. Firstly, note that the prior *expectation* of test score attainments, conditional on observable pupil characteristics, is a plausible instrument for pupil human capital since it is uncorrelated with test-score measurement error. Since our data provide teacher assessments of expected attainment in the age-11 tests (measure in terms of Key Stage Levels - see Section 4 below) we can use these as instruments for individual pupil achievement.

In summary, we propose to estimate model (6) using teacher expectations of attainment at age 11 as an instrument for pupil attainment at age-11, and the effectiveness of Secondary school peers' origin Primary schools as an instrument for their age-11 attainment. We further extend the empirical specification beyond that in (6) to allow for 'contextual' peer-group effects from pupil demographic characteristics. Secondary school peer-group for pupil *i* is always defined as the mean amongst the group of unfamiliar Secondary school-mates, which here means those who did not attend the same Primary school and who do not live in the same residential postcode as pupil *i*.[4]

4. Data and institutional context

In England, state compulsory-age education is organised into five National Curriculum 'Key Stages' and spread over two phases. Primary schooling starts at age 4/5 and continues to age 10/11, spanning Foundation Stage and Key Stage 1 (5-7), and Key Stage 2 (8-11). Compulsory Secondary schooling runs from age 11/12 to age 15/16, spanning Key Stages 3 (12-14) and Key Stage 4. Pupil progress is assessed by standard SATS tests at the end of each Key Stage: Keys Stage 1 at age 7, Keys Stage 2 at age 11, Key Stage 3 at age 14 and Key Stage 4 at age 16. Funding of schools is organised largely through central government grant distributed to Local Education Authorities (LEAs), and these LEAs handle most of the school admissions and other administrative procedures. In a few LEAs the Primary/Secondary distinction is somewhat blurred by the prevalence of 'Middle' schools, which typically bridge part of Key Stage 2-Key Stage 3, though the exact age range varies. For the purposes of this paper we describe the schools at which a pupil takes the Key Stage 3 tests (age-14) as Secondary, and the school at which they take the Key Stage 2 tests (age-11) as Primary[5].

The picture is made more complex by the institutional differences between schools at all phases. Most schools (65%) are designated 'Community' schools – which means, essentially,

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[4] The reasoning for excluding residential associates is similar to that for excluding peers familiar from schooling in earlier phases.

[5] About around 60 of our pupils (out of some 150000) are in schools officially designated as Middle schools.

that they are non-selective in admission and are administered by the LEA. Some other schools have religious affiliations and are allowed to select on the basis of religious commitment, and a smaller number are run by other types of charitable institution but still come under the 'state-school' umbrella. A number of state schools in some LEAs are allowed to select pupils by academic ability (e.g. traditional Grammar schools). In addition, there is a small, but not inconsequential, number of private sector ('Independent') schools. We will focus entirely on Community schools as an initial step to reduce selection issues induced by parental choice. Although pupils and their parents can express preferences over which Community school they would like to attend, many end up at schools that were not their first choice (or even any of their choices) because the most popular schools are over-subscribed.

Information on pupil Key Stage tests results is collected by the Government's Department of Education and Skills (DfES), who use the data to publish school performance league tables. Since 2002, the DfES has also conducted a Pupil Level Annual Census with information on pupil demographics for the current school population (in attendance on school Census day), which can be linked to pupil test records held in a National Pupil Database. The Census is based on a day in January in 2002 and 2003 and records pupil characteristics, home postcodes and school identifiers. Additional school-level information such as admissions policy and school type can be merged in from the DfES 'Edubase' system which holds details on all educational establishments.

From this composite database, we extract information on the two cohorts aged 14 in 2002 and 2003 for our main sample, plus the two cohorts age 11 in 2002 and 2003 for calculation of the Primary school value-added used in our instrumentation strategy. The age-14 cohort census data are spliced to pupils' age-14 (Key Stage 3) results and to their results recorded at age 11 (Key Stage 2). The age-11 cohort census data is spliced to these pupils' age-11 (Key Stage 2) results and to their results at age 7 (Key Stage 1).

The National Pupil Database holds these test results in a number formats. For age-11 and age-14 tests of the age-14 cohort we utilise the raw pupil test scores in Maths and English, converted to percentiles. For the Primary school value-added calculations we do not have raw test scores at age-7 so we use overall point scores assigned, according DfES rules, on the basis of a pupil's overall performance in a given year of SATS tests.

The data also includes a categorical indicator of teacher assessments of the pupils' Key Stage Level at time of assessment – which ranges from 1 (lowest) to 6 (highest) at age-14 – and we use this as an instrument for pupil attainment. As set out in the statutory information and guidance on Key Stage 3 assessment, "The tests give a standard snapshot of attainment in English, mathematics and science at the end of the key stage. Teacher assessment covers the full range and scope of the programmes of study. It takes into account evidence of achievement in a variety of contexts, including discussion and observation." (QCA 2004)

As we have said, our sample is restricted to "Community" schools only, avoiding distinctive school types that may be preferred by distinct groups in the population, or schools where there is explicit selection of pupils by ability. Also note that since we include postcode fixed effects it is desirable to have at least two pupils in each postcode attending at least two different Secondary schools[6]. Hence, we drop postcodes where all pupils attend the same secondary school. A postcode is typically 14 addresses (the median in England) corresponding to a contiguous group of houses on one side of a street in an urban area. Note however, that peer-group quality measures are built from group means derived from the two full population cohorts, not this postcode-restricted sample.

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[6] We impose this sample restriction although, given our definition of unfamiliar peer groups, it is not essential that pupils in the same postcode attend different secondary schools. This is because they will have different *unfamiliar* peer groups if they attend the same secondary school, but different primary schools.

5. Results

Descriptive statistics

The estimation sample we described above is much reduced in size from the full population in our data – around 13% of the full Secondary population and about 19% of the population in Community Schools. But, it contains around 155,000 pupils and is highly representative of the population in Community schools as a whole. In fact, the 19% of pupils in the sample are drawn from 99% of the Community schools across the country. Because we focus on Community schools, our sample is slightly biased towards denser urban areas with higher proportions of pupils on free school meals and in non-white ethnic groups. The geographical distribution of the 51000 postcodes in the sample is shown in Figure 1. The number of pupils per postcode ranges from 2 to 19, with a mean of 3.7.

For ease of comparison and interpretation, all variables – except our instruments – are transformed into percentiles of the distribution in the Secondary school pupil population. The means and standard deviations of these percentiles in the estimation sample are shown in Table 2, alongside with the actual means in the data.

An important thing to note from Table 2 is that the proportion of a pupil's Secondary school peer group who come from Primary schools *other* than the pupil's own is high – around 88%. This is important as we intend to estimate peer-group effects on Secondary attainment using this new, unfamiliar peer group.

Regression estimates of the educational production function

We now turn to the central regression estimates of the model of English and Mathematics attainments at age 14 (Key Stage 3) based on Equation (6) and presented in Table 3-Table 5. In all specifications, we condition on prior attainments in the corresponding subject at age-11

(Key Stage 2), age in months, the number of pupils in the pupil's Secondary school year, and include a set of dummy variables as descriptors of pupil ethnicity (7 categories), free-school-meal entitlement, gender, special educational needs (4 categories), and English as a first language. In Table 3 and Table 4, we focus only on peer-group attainments and exclude all other contextual peer-group characteristics (we return to these in Table 5). Columns 1 and 4 in Table 3 present ordinary least-squares estimates for English and Maths results respectively, with no controls for school or geographical fixed effects. The specifications in Columns 2 and 5 differences the variables from residential postcode (residential street) means, whilst Columns 2 and 5 difference from Primary school means. In Table 4 we introduce instrumental variables into these within-groups specification as discussed in Section 0.

It is evident from Columns 1 and 4 in Table 3, Row 2 that we find a significant basic association between a pupil's age-14 English/Maths attainments and the age-11 English/Maths attainments of new schoolmates that he or she encounters on moving to Secondary school (coefficients of 0.241 and 0.242 respectively) [7] [8]. Clearly, there are many reasons to doubt that this represents a causal relationship, for all the reasons outlined in Section 3. Yet, moving to the within-group estimates of Columns 2 and 5 or 3 and 6, there is only a fairly small fall in the point estimate of the peer group effect and little change, or an increase, in statistical significance. The coefficients are slightly lower for Maths when we allow for Primary school fixed effects rather than postcode fixed effects. As anyone would expect, a pupil's own prior attainments (Row 1) are the strongest predictor of attainment at

[7] For simplicity we do not consider the cross-elasticities with pupil attainments in other subjects, though we recognise that these may be relevant too. Inclusion of prior attainments in other curriculum areas in the regressions does not change the basic message of these results.

[8] The reader worried about sample selection issues should note that the OLS estimates using the full sample of 1869 Community schools are little different from those reported here.

age 14 in both these specifications, and are responsible for over 95% of the R-squared shown at the bottom of the table.

In Table 4, we turn to our IV strategy outlined in Section 0, which deals with issues of pupil sorting and mis-measured pupil attainment. A set of 5 dummy variables corresponding to Teacher Assessments of pupil ability level at age 11 acts as instruments for pupil prior attainment. In addition, the age-7 to age-11 'value-added' of peer's Primary schools (measured from pupils aged 10-11 in our sample years using the Government's standard point score) provides an instrument for peer group attainment, as discussed in Section 0.[9] However, as it turns out, the IV peer-group coefficient estimates are of a similar order of magnitude to what we found before – given their standard error – and although the point estimates are substantially higher for English, they are not significantly different from the OLS/within group estimates (based on a Hausman test). Again, it makes little difference whether we choose Primary school or postcode fixed effects and our IV approach confirms that the influence of these *unfamiliar* peers' prior attainments can be quite successfully identified by OLS estimates conditional on a pupil's own prior attainments.

Taken together, the range of these figures indicates that an increase in mean attainment of unfamiliar peers of 10 percentiles corresponds to a 1.5 – 2.6 percentile improvement in individual pupil attainment. Based on our IV estimates, a move through the peer-group attainment distribution from the bottom decile to the top decile (a shift of 24 percentiles in terms of pupil test scores) would push up pupil attainment by at most 5.75 percentiles in English (24×0.26)) and 4.8 percentiles in Maths (24×0.20)). Another way of gauging the magnitude of the effect is to note that a one-standard deviation move up the distribution of

[9] We also tried as an instrument the 'conditional' value added obtained as the Primary school-mean residual from a regression of age-7-11 value-added on pupil characteristics: the results were almost identical.

peer-group mean attainments (8 percentiles in English, 8.8 in Maths) increases pupil attainment by 5-8 percent of one standard deviation, which in turn indicates that peer-group effects could account for, at most, some 0.6% of the variance in pupil progression over this period. By comparison, general school-specific factors (estimated by secondary school fixed effects in value-added models) account for about 13% the variance in pupil progress between age-11 and 14 in our data.

What the IV strategy also reveals is that the coefficient on prior attainment (in Row 1) is quite sensitive to the transient and noise components of pupil test scores in the OLS and within-group estimates. The partial correlation between age-14 and age-11 attainment increases by about 15% for English and by 9% for Maths. Evidently, OLS estimates of value-added models underestimate the persistence of ability across school phases, though this does not have a large bearing on the magnitude of the peer group effects we are interested in here.

In order to gauge the quality of our instruments, the figures at the bottom of Table 4 report the F-statistics on the set of Teacher Assessment dummies and the coefficient and t-statistic for the instrument for peer-group attainment in each first stage IV equation. These are encouraging in that they show that the instruments are extremely strong predictors of the variables they are designed to instrument.

We conduct a more detailed analysis of these IV results, and consider the role of other contextual peer-group characteristics in Table 5. Column 1 and Column 5 show the key results for English and Maths respectively. Columns 2-3 and 6-7 show the corresponding first stage regressions, and Columns 4 and 8 present the reduced form regressions. Looking at the coefficients in Row 2, Columns 1 and 5 shows us that the point estimate of the impact of peer-group attainment in English is substantially (though insignificantly) larger once we condition on peers' demographic characteristics; the coefficient is much less precisely measured, because peer-group characteristics and attainments are highly correlated. The results for Maths are similar to those that went before without additional contextual controls. Overall, the contextual effects from demographic characteristics seem unimportant for English in terms of magnitude, significance and explanatory power. Some characteristics – age, first language and particularly gender mix – seem relevant for pupil Maths attainment at age-14. A one standard deviation move up the gender-balance distribution (5 percentiles) is associated with a 0.07 percentile increase in pupil maths attainment at age 14. However, in terms of magnitude, the influence of prior attainments completely dominates all the other peer-group influences.

As before, in Table 3, the first stages for both endogenous variables are encouraging, with large coefficients and high t/F-statistics for the relevant instruments. It is reassuring to note too that pupil's own age-11 attainments are now completely uncorrelated with peer-group Primary school 'value-added' in Row 9 of Columns 2 and 6. Inspection of the reduced form in Columns 4 and 8 in relation to the IV in Columns 1 and 5 is also informative, in particular because this reveals that peer-group income (measured by free-meal entitlement) has quite a strong relationship with pupil attainments at age-14 in the reduced form. However, the main IV estimates show that this is only because peer-group income has a strong bearing on peer group prior attainments.

One concern is that our IV strategy will fail if pupils select Secondary schools on the basis of their expectations of the quality of school from which their peers will originate. In fact, such selection would be rational if peer's Primary school quality is really beneficial to his or her own education as our results indicate. If true, then this implies that peer-group effects must actually be positive, but our estimates will be upward biased. We have taken some steps to allaying such concerns by considering the peer-effect of schoolmates who arrive in Secondary school some time after the majority of transfers (that is, those with greater than median start date). These arrivals could presumably not be easily anticipated by other cohort members.[10] Our finding is that the association between individual attainments and the value-added of the origin schools of these late arrivals is no different from the association of individual attainments with the peer-group as a whole. Based on this, we do not believe that selection of Secondary schools on the basis of feeder-school quality is influencing our results.

Evaluating the contribution of familiar and unfamiliar peers

Our model in Section 0 and the peer-effect estimates above are based around the new, *unfamiliar* peers that a pupil encounters on transfer to secondary school. Our model in Section 0 illustrated that the association between pupil attainments and the prior attainments of schoolmates familiar from previous school phases could be very small, once we condition on pupil prior attainments – or even negative if new, unfamiliar peers have a larger structural influence on attainments than those who are familiar. We have avoided this problem by simply eliminating familiar peers in our calculation of peer group quality, but this obviously means we have said nothing about how much *these* peers matter for pupil attainment in subsequent phases. Moreover, we have not shown whether omitting the quality of familiar peers in our estimation has an important bearing on our results. We have no credible instruments for familiar peer-group characteristics, so cannot exploit our instrumentation strategy. Nevertheless, since our IV, within-group and OLS results in Table 3 and Table 4 are not markedly different, we consider it worth exploring the role of familiar peers on prior attainments in a basic specification. In Table 6, we show estimates from within-group models akin to Table 3, Columns 2 and 5, with postcode fixed effects, and the results are very much what we would expect in the light of the model in Section 0. Once we add in the mean prior

[10] Such late arrivals may occur because children have moved home, arranged for a school transfer, or have been held back for some reason, for example if a school of preference was not available.

attainments of Secondary peers who are familiar from Primary schooling, the *unfamiliar* peer effect barely changes for English, but increases for Maths. However, the association of pupil attainment with the attainments of familiar peers is itself *negative* and significant, conditional on pupil prior attainment. As explained in the modelling section, this is because – for a given level of pupil prior attainment – peer group effects imply that pupils with good peer-group histories are of low ability.

Using these parameter estimates, we can make a rough calculation of the relative importance of the contribution of familiar and unfamiliar peers to pupil attainments. Note, if the parameter estimates in Table 6 are consistent estimates of γ_1 and $\gamma_2 - \frac{\rho\gamma_1}{1+\gamma_1}$ (from Equation 3 and 5) we can deduce that the magnitude of the influence of familiar peers (γ_2) is, in English, about two-thirds that of unfamiliar peers in English and in Maths around one-fifth.[11]

In Table 6 we also demonstrate how our results look when we ignore the familiar-unfamiliar distinction and simply define peer group attainment as the simple mean within each school-year cell – as is common in most peer-group papers. The within-postcode estimates in Columns 3 and 7 are considerably smaller than the unfamiliar peer-group estimates and illustrate the effect of the downward bias induced by pooling familiar and unfamiliar peer groups in value-added models. However, these coefficients rise when we instrument the pooled peer group attainment with the unfamiliar peer-group's origin-school quality.

[11] Assuming values of ρ of 0.772 for English and 0.852 for Maths from **Table 3**. So, for example, for English $\gamma_2 = -0.061 + 0.772 \times 0.194 / 1.194 = 0.129$

Non-linearities and complementarities

A common theme in the literature on peer-group effects is the degree to which they are non-linear. Certainly, this is an important consideration since the policy implications when the marginal effect of peer-group quality is increasing in peer-group quality are very different from when there are diminishing marginal returns. In the first instance, policy that improves peer-group quality in the best groups provides greater gains than at the bottom of the distribution and segregation is efficient; in the second case, integration is more efficient. For example, Zimmerman (2003) and McEwan (2003) provide support for the policy mixing students of different ability. However, there are many other studies that find no evidence of nonlinearities [e.g. Hoxby (2000); Ammemueller and Pischke (2006)]. Similar considerations make complementarities between own attainments and peer-group quality interesting since these will reinforce educational inequality across individuals and drive sorting by ability across schools.

We address both these concerns in a basic non-parametric fashion by replacing the linear peer and prior attainment effects in our regressions with a dummy variable set for the joint distribution of teacher assessments of pupil Key Stage Level at age-11, and peer-group age-11 attainment quartiles. Teacher assessments are grouped into Level 1-2, Level 3, Level 4 and Level 5+; peer attainments are divided into quartiles. Unfortunately, we are not able to instrument peer-group quality effectively, so we present the coefficients from the OLS estimates with Primary, Secondary and residential fixed effects and are forced to rely on these estimates[12]. The coefficients from this regression are shown in Table 7a (English) and Table

[12] We noted from the IV regressions above, that the main source of bias is the use of transient test measures of prior attainment [c.f. Hanushek and et al. (2003)], which we implicitly correct for here by using teacher assessment of student ability.

7b (Maths). The reported coefficients show the percentile of the age-14 test score relative to the baseline pupils – those who were assessed at Level 1-2 at age 11, and are in Secondary schools with peers in the lowest quartile of age-11 attainment.

The most obvious feature emerging from these numbers is that the peer-group effects are – unsurprisingly – dwarfed by a pupil's own prior attainments; though that is not our main concern. The interesting issues are whether the peer-group effects are non-linear, conditional on prior attainments, and whether there are complementarities between peer-group and ability. Considering the first issue, inspection of Table 7 suggests some non-linearities in the sense that the gap between each peer-group quartile within any ability band is non-constant, but the patterns are certainly not striking. In Maths there is no obvious trend; in English the biggest gains are concentrated at the top of the peer-group distribution – for example a gap of 3.17 percentiles been Quartile 4 and Quartile 3 for those on Level 4, compared to 1.71 percentiles between Quartile 2 and Quartile 1.

There is more to say about complementarities, in that the least able pupils always seem to benefit the least from peer-group improvements, whereas the middle and higher ability groups do (observe the F-statistics for the joint test of the significance of the coefficients in each column, and the overall difference between the top and lowest quartile). In Maths, only those pupils expected to reach the age-11 target Level in the national curriculum (Level 4) show much benefit from peer-group attainments.

These findings offer some understanding of the reasons why lower ability pupils (or their parents) might be less pro-active in their efforts to secure better peer-groups: these children have little to gain from such actions. This is a main concern of those who criticise school choice on the basis that it leads to increased school segregation [Walford, (1996)]. One explanation for our findings is that these peer-group effects operate through competition for teaching resources, rather than direct social interaction between pupils. Suppose that the speed of learning of the lowest ability children is constrained by their own abilities, but that the speed of learning of more able children is governed by the rate at which teaching can progress, taking into account the average mix of abilities in the group. This would explain the kind of patterns seen in Table 7, with no influence from peer-group attainments on the lowest achievers, but stronger effects on other pupils. As peer-group ability increases, teachers become less constrained in what they are able to teach[13].

6. Discussion and Conclusions

Pupils seem to do better in their early stages of Secondary school when their new schoolmates have a good record of prior achievement. Our reading of this result is that there is some form of social interaction between pupils that promotes higher attainments. Manski (2000) provides a well-known and useful classification of social interactions in groups – those due to pupils' desire to act like their friends, those due to competition for constrained resources like teacher time, and those due to the information that group behaviour provides about the expected consequences of individual action – but we are unable here to be precise which of these mechanisms prevails. Perhaps individual behaviour is mutable under group influences, and a move to a new school with high-attaining children opens up new challenges with the individual drawn into higher achievement by the expectations of the group. However, since

[13] An alternative explanation might be that there is streaming by ability within schools so that the least-able are unable to benefit from higher average attainment in the school, because they are segregated off from the higher-achievers. Unfortunately we have no data that would allow us to assess the extent to which low and high-achievers are segregated, but our knowledge of English Community schools tells us that any streaming is not as extreme as this would imply.

our findings seem to reflect responses to peer-group prior attainment and not other 'contextual' differences, and because the effects are zero for the lowest ability groups, we conjecture that the most likely explanation may be more mundane: teaching can proceed faster in higher ability groups, or can start from a higher base-line when the group's prior attainments are higher. If expectations or 'norm' (preference) related factors were important we see no reason why other group characteristics should not have an equally strong influence or why the lowest ability pupils show no response.

Prior research in the educational literature has often cited low-income of peers – measured by free-school meal entitlement – as an important 'contextual' influence on pupil attainments [e.g. Strand (2002)]. On the contrary, we show that the influence of peer-group free-meal entitlement on pupil attainments works only through the prior attainments of the peer-group. Similarly, most other group demographics have insignificant or relatively small effects. These are encouraging results for policy makers because pupils' prior attainments are surely more amenable to early interventions than socioeconomic and demographic characteristics.

On balance though, the contribution of peer-group abilities to the distribution of attainments in the short run seems very small. A one standard deviation improvement in peer-group quality relates to a mere 0.05-0.08 standard deviation increase in pupil attainments at the end of our three-year period. True, in the long run, if individual attainment is persistent across school phases, it is possible that a pupil who benefits from better peer-groups throughout his or her school career may be at more of an advantage than this would at first suggest. For instance, what if our estimate of the link between individual attainment in current and prior phases (a partial correlation of about 0.8) represents dependence on prior attainment, rather than persistent unobserved heterogeneity? Suppose, under this scenario, that a pupil's peer-group is one standard deviation above the mean throughout his or her 12 years of schooling, and that the effects of prior attainments (own and peer-group) on attainments at the end of each 3-year period are roughly in line with our parameter estimates: his or her end-of-school attainments will be some 0.24 standard deviations above the mean, since the effects are cumulative over the period[14]. Even then, peer-groups must play a fairly limited role in the overall distribution of educational attainments: if peer-group quality was perfectly correlated over the years for each individual it would only account for 5.5% of the variance of educational attainments across individuals at the end their compulsory schooling years[15].

Given the magnitude of these effects 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. Better peer-groups perhaps 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, aside from any small educational advantages they offer.

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[14] This assumes a model of the form $y_{it} = 0.8y_{it-1} + 0.08\bar{y}_{it-1}$ where the variables are standardised. Over four periods the effect of a persistent 1 s.d. increase in peer-group attainments $\bar{y}$ is, from the sum of a finite geometric series, $0.08 \cdot \left(\frac{1-0.8^4}{1-0.8}\right) = 0.236$. More plausibly given what we can infer from our data, a correlation coefficient of 0.5 between mean peer-group attainments in each period would suggest that a pupil who starts off in a peer-group that is 1 s.d. above the mean, will end up with attainments that are $0.08 \times 0.125 + 0.8 \times 0.08 \times 0.25 + 0.8^2 \times 0.08 \times 0.5 + 0.8^3 \times 0.08 = 0.093$ s.d. above the mean.

[15] 0.236^2

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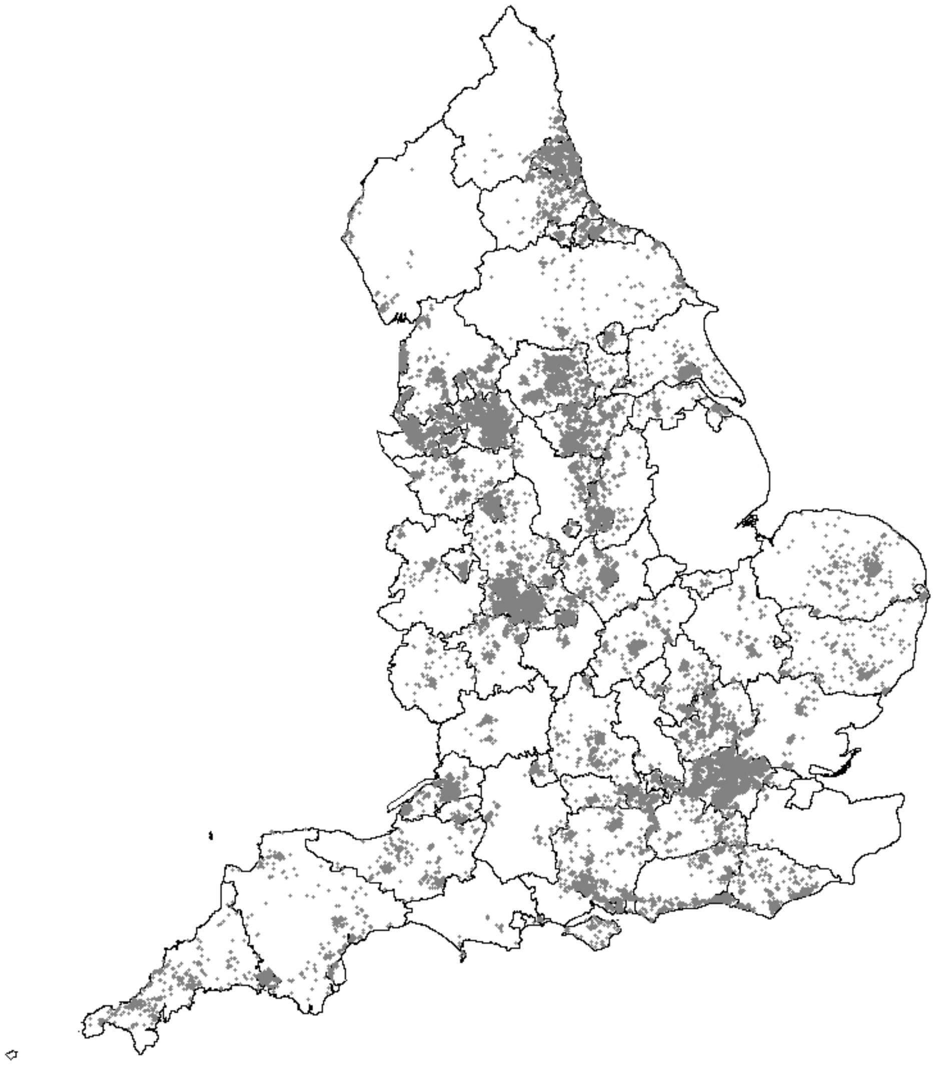

Figure 1: Geographical distribution of postcodes in the estimation sample on a background of English Counties

Table 2 Descriptive statistics for the estimation sample

	Mean pctile	s.d. pctile	Raw proportion or mean
Age-14 English	45.13	27.72	-
Age 14 Maths	47.18	26.97	-
Age-11 English	46.03	27.74	-
Age-11 Maths	47.12	28.21	-
Age-11 Peer group English	45.07	8.844	-
Age-11 Peer group Maths	45.85	8.042	-
Primary school value-added points	35.57	1.50	-
Peer-group proportion girls	49.28	28.92	0.500
Peer-group mean age	50.84	28.46	161.5 months
Peer-group white	45.72	29.55	0.767
Peer-group English as first language	42.14	26.84	0.865
Peer-group eligible for free school meals	62.57	27.35	0.217
Proportion in Secondary from other primary	-	-	0.871
English level, teacher assessment			
1	-	-	0.001
2	-	-	0.032
3	-	-	0.279
4	-	-	0.525
5	-	-	0.163
6	-	-	0.001
Maths level, teacher assessment			
1	-	-	0.001
2	-	-	0.027
3	-	-	0.260
4	-	-	0.525
5	-	-	0.185
6	-	-	0.001

Table 3 Secondary pupil progress and peer-groups, pupils aged 14 in 2001/2-2002/3, Community schools, OLS results

	English			Maths		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Within postcode	Within primary	OLS	Within postcode	Within primary
Own age-11 test (percentile)	0.679 (256.34)	0.666 (277.08)	0.694 (274.19)	0.780 (387.26)	0.780 (449.13)	0.818 (497.33)
Peer-group age-11 attainments	0.241 (13.53)	0.190 (12.88)	0.180 (10.30)	0.242 (16.77)	0.185 (19.34)	0.152 (13.78)
Within-R2	0.583	0.547	0.575	0.735	0.724	0.754

Regression at the pupil level. t-stat in brackets, clustered on Secondary school; Dependent variable is Key Stage 3 (age-14) test score percentile. Other controls are: pupil gender, ethnic group, free-school-meal entitlement, special educational needs, age in months, year dummy; Number of pupils: 155320; Number of Secondary schools: 1853.

Table 4 Secondary pupil progress and peer-groups, pupils aged 14 in 2001/2-2002/3, Community schools: IV results

	English				Maths			
	(1)		(2)		(3)		(4)	
	IV within postcode		IV within primary		IV within postcode		IV within primary	
Own age-11 test (percentile)	0.772 (246.93)		0.800 (242.61)		0.852 (414.46)		0.888 (463.51)	
Peer-group age-11 attainments	0.259 (5.97)		0.227 (4.40)		0.201 (7.44)		0.162 (5.56)	
First stage	Own	Peers	Own	Peers	Own	Peers	Own	Peers
Teacher assessment F-statistic F(5,1852)	35328.2	63.8	39775.2	73.26	42272.7	42.68	52942.9	62.68
Peers' Primary value-added	0.162 (3.43)	1.913 (23.82	0.234 (4.49)	2.039 (19.62)	0.122 (2.86)	1.923 (26.97)	0.241 (5.40)	2.008 (22.41)

Regression at the pupil level. t-stat in brackets, clustered on Secondary school; Dependent variable is Key Stage 3 (age-14) test score percentile. Other controls are: pupil gender, ethnic group, free-school-meal entitlement, special educational needs, age in months, year dummy; Number of pupils: 155320; Number of Secondary schools: 1853. Instruments are Teacher assessment of attainment level at age 11 and value-added in peers' origin primary schools.

Table 5 Secondary pupil progress and peer-groups, pupils aged 14 in 2001/2-2002/3; IV with other contextual effects

	Age-14 English				Age-14 Maths			
	(1)	(2)	(3)		(5)	(6)	(7)	(8)
	IV-within	Own Stage 1	Peers Stage 1	Reduced form	IV-within	Own Stage 1	Peers Stage 1	Reduced form
Own age-11 test (percentile)	0.772 (248.61)	-	-	-	0.852 (418.12)	-	-	-
Peer-group age-11 mean attainments	0.354 (3.32)	-	-	-	0.218 (3.95)	-	-	-
Peer-group eligible for free meals	0.044 (1.43)	-0.037 (-12.41)	-0.266 (-76.43)	-0.079 (-12.58)	0.010 (0.69)	-0.032 (-12.01)	-0.237 (-70.48)	-0.067 (-15.62)
Peer-group proportion girls	-0.010 (-1.61)	0.006 (3.06)	0.044 (18.10)	0.009 (3.29)	0.014 (6.22)	0.005 (2.77)	-0.010 (-4.72)	0.015 (6.16)
Peer-group mean age	-0.006 (-1.46)	0.002 (1.02)	0.013 (6.75)	0.010 (2.50)	-0.007 (-3.45)	0.001 (0.62)	0.013 (0.62)	-0.003 (-1.51)
Peer-group white	-0.001 (-0.15)	-0.003 (-0.68)	-0.013 (-3.14)	0.000 (0.01)	-0.004 (-1.14)	-0.005 (-1.35)	-0.007 (-1.35)	-0.010 (-2.15)
Peer-group English first language	0.001 (0.09)	-0.012 (-2.99)	-0.016 (-3.96)	-0.014 (-1.60)	-0.012 (-2.68)	0.001 (-0.38)	-0.010 (-0.38)	-0.015 (-3.13)
Teacher assessment F-statistic F(5,1852)	-	35106.6	22.0	11301.57	-	42050.8	12.97	25098.93
Peers' Primary value-added	-	0.006 (0.13)	0.829 (15.73)	0.291 (3.29)	-	-0.011 (-0.25)	0.968 (20.10)	0.191 (3.31)
Within-R2	0.536	0.631	0.556	0.453	0.720	0.665	0.518	0.568

Regression at the pupil level. t-stat in brackets, clustered on Secondary school; Dependent variable is Key Stage 3 (age-14) test score percentile. Other controls are: pupil gender, ethnic group, free-school-meal entitlement, special educational needs, age in months, year dummy; Number of pupils: 155320; Number of Secondary schools 1853; All columns allow for postcode fixed effects.

Table 6 Familiar, unfamiliar and all-peers comparison

	Age-14 English				Age-14 Maths			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS-within	OLS-within	OLS-within	IV	OLS-within	OLS-within	OLS-within	IV
Unfamiliar peer-group age-11 attainments	0.190 (12.88)	0.194 (11.18)	-	-	0.185 (19.34)	0.235 (21.08)	-	-
Familiar peer-group age-11 attainments	-	-0.061 (13.90)	-	-	-	-0.113 (31.07)		
Overall peer-group age-11 attainments	-	-	0.165 (10.49)	0.281 (2.60)	-	-	0.129 (12.32)	0.227 (3.51)

Table notes as for Table 3, within-group estimates, postcode fixed effects only.

Table 7a: Interactions between own and peer-group abilities: English

	Teacher assessment of age-11 ability				
	Level 1-2	Level 3	Level 4	Level 5+	Difference
Peer-group Quartile 1	0	8.151 (22.27)	28.809 (67.23)	53.927 (105.25)	53.927
Peer-group Quartile 2	1.008 (1.89)	9.434 (21.16)	30.523 (65.48)	55.382 (107.66)	54.374
Peer-group Quartile 3	0.611 (0.92)	10.100 (20.42)	31.647 (65.43)	55.747 (107.43)	55.136
Peer-group Quartile 4	2.907 (3.30)	12.580 (23.67)	34.841 (68.33)	57.648 (110.55)	54.741
Q4-Q1 Peer-group effect	+2.907	+4.429	+6.032	+3.721	
F(3, 1852) test	3.05 (0.048)	34.23 (0.000)	70.88 (0.000)	20.37 (0.004)	

Table shows the coefficients and t-statistics on dummy variables for own attainment/peer attainment quartile interactions in the OLS regression similar to Column 2 in **Table 3**; Sample size 155320.

Table 7b: Interactions between own and peer-group abilities: Maths

	Teacher assessment of age-11 ability				
	Level 1-2	Level 3	Level 4	Level 5+	Difference
Peer-group Quartile 1	0	10.674 (33.48)	34.802 (101.02)	62.936 (157.70)	62.963
Peer-group Quartile 2	1.163 (2.43)	11.296 (32.28)	36.610 (99.82)	64.263 (159.97)	63.100
Peer-group Quartile 3	1.380 (2.33)	12.008 (32.19)	38.179 (102.82)	65.852 (164.97)	64.472
Peer-group Quartile 4	0.192 (0.28)	12.657 (31.17)	40.402 (102.73)	67.506 (170.89)	67.314
Q4-Q1 Peer-group effect	0.192	+1.983	+5.600	+4.570	
F(2, 1853 test)	1.21 (0.300)	16.61 (0.000)	130.94 (0.000)	72.22 (0.000)	

Table shows the coefficients and t-statistics on dummy variables for own attainment/peer attainment quartile interactions in the OLS regression similar to Column 2 in **Table 3**; Sample size 155320.