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Peer Effects: Evidence from Secondary School Transition in England

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Abstract

We estimate the effect of peers' prior achievement on student progress in secondary school, using administrative data on four cohorts of students in England. Students leaving primary for secondary school experience a big change in their peer group and these changes vary randomly from cohort to cohort. We exploit this variation to identify the effect of new peers on student achievement. We show that peer quality on entry to secondary school has a significant effect on students' subsequent achievement at age 14. The effect sizes are relatively small and are linked to peers' family background and early age achievements.

JEL Classification: I2, I21

Keywords: peer effects, schools, education

I. 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 contribute to the evidence on the benefits of 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)[2] and their prior achievement in national standardised tests in primary school age 11 (Key Stage 2 tests, ks2). We use a detailed administrative 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. Students' secondary school peer quality is defined here as the mean of secondary schoolmates' prior achievement (ks2 primary school scores) upon enrolment in

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

secondary school. On average, we find that peers 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 2% of one standard deviation increase in student achievement in ks3. This effect is small relative to the variation in achievement across students, lending 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 outcomes. However, scaled relative to other educational interventions, these effects are not so trivial. For instance, the recent literature on teacher effects (Hanushek and Rivkin, 2010) finds that a one standard deviation increase in teacher quality raises student achievement by only 10-15% of one standard deviation. We further show that these peer effects on age14 achievement are also evident when we measure prior achievement using even earlier age-7 (key stage 1, ks1) tests. This implies that students benefit from attributes of their peers, perhaps motivation, innate ability, or aspects of family background, that were evident very early on in their schooling. We find no evidence for heterogeneity in the effects of peers across students of different types, or complementarities between students with different prior achievements.

In common with other work on peer effects (and other group and spatial effects), the main threats to identification 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[3]; c) reverse causality running from individuals to the group which will tend to inflate the magnitude of the estimated effects[4] ; and d) insufficiently large variation in peer group quality across students, once steps are taken to mitigate

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

[4] 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.

the effects of a)-c).Our paper offers several contributions to the field in the way it deals with these problems.

Firstly, the fact that we observe multiple cohorts of the population of state-school children in an administrative census allows us to transform the data in ways that eliminate salient fixed and trending factors. We control for individual effects using student prior achievement in an educational value-added specification (as, for example in Ding and Lehrer, 2007). At the same time we control for fixed effects and trends for current and previous school in a variant of the classic within-school, cohort-to-cohort differencing approach popularised by Hoxby (2000) and employed in a number of peer group papers (for example, Hanushek *et al*., 2003; Lavy, Paserman and Schlosser 2012). The resulting design implies that we identify peer effects on individual test score gains, from shocks to peer group quality, that are likely to be unanticipated, conditional on primary and secondary school fixed effects and trends. The design is only feasible because we look at the transition between primary and secondary schooling, and, as we will demonstrate, eliminates potential sorting and selection effects and controls for unobservable factors affecting students who make the same schooling choices. A particular novelty of our set up is that this analysis is carried out on student data aggregated to primary school-by-secondary school-by-cohort cells. Aggregating the data in this way has an advantage over working the individual level data as it makes it feasible to eliminate fixed effects and trends using first-differences and second-differences between cohorts, 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).

Secondly, a strength of our administrative data is that it has information on the family background and test score histories of each student, with test scores measured at age 7 (Key Stage 1, ks1). This test score history allows us to examine to what extent peers' early achievements, determined well before secondary peer group formation, drive peer effects in secondary school. We can also use this information to control for individual student specific trends in achievement.

Thirdly, and crucially, we have large changes in peer group quality, because we look at peer group reformation during the transition between primary and secondary school, occurring immediately after students take their standard ks2 tests in primary. 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 rearrangement of peer groups ensures that we observe 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 reduce biases from 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. This ensures that our peer measure is not affected by the students selecting into or out of secondary schools in response to what they learn about schools and peers after entry.

Taken together, these elements of our 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 cohort-to-cohort changes in the secondary school peer group experienced by students making a given primary-secondary school transition, conditional on primary-by-cohort fixed effects, the prior (ks2) achievements and other characteristics of students making this primary-secondary school transition in each year.

The rest of the paper is structured as follows: Section 2 provides an overview of recent relevant literature on the influence of peers on student achievement, outlining relevant methodological issues. Section 0 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.

II. 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, 2010), 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. Facebook). 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 with linear specifications, 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 related to student achievements in schools and college 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 (Gaviria and Raphael, 2001; Robertson and Symons, 2003; Goux and Maurin, 2007; Kang 2007). Several papers have sought identification from 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 Hoxby (2000), Hanushek *et al*. (2003), Arcidiacono and Nicholson (2005), Gould *et al*. (2009). Other papers rely on a cross-sectional school-specific fixed effects strategy, where variation in peer groups is between class groups, for example McEwan (2003), Vigdor and Nechyba (2007), Ammermueller and Pischke (2006) and Kang (2007). Calvo-Armengol *et al*. (2009) use detailed data on friendship networks, using fixed effects for groups of friends and exogenous variation in peer group achievement arising from the intransitive nature of the friendship network structure. Others have incorporated regression discontinuity designs based on class size rules (Angrist and Lang, 2004) or entrance exams (Ding and Lehrer, 2007). Occasionally, opportunities arise for analysis based on explicit randomisation, or assignment that is plausibly random given the institutional arrangements (e.g. Sacerdote, 2001; Zimmerman, 2003; Cullen, Jacob *et al*., 2003; Sanbonmatsu *et al*., 2004; Angrist and Lang, 2004; Hoxby and Weingarth, 2005; Lyle, 2007; Carrell *et al*., 2009; Imberman *et al*., 2009; Duflo *et al*., 2011). Some designs included individual student fixed effects, relying on variation in differences in peer group quality for students moving schools (Burke and Sass, 2013), or for students taking different subjects Lavy *et al*. (2012). This last study is close to ours in that it studies the same school system and builds on our previous work and data. In their cross-sectional analysis, Lavy *et al*. (2012) 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. The drawback of their approach is that it is not based on any variation in peer group membership, but only on variation in the relative abilities in different subjects of a fixed group of students. Their approach thus, eliminates any influences from peer group background or motivation that are common across different subjects, and it is impossible to generalise the results to the case where a student moves into a peer group that performs better in all subject areas. Epple and Romano (2011) provide a more detailed recent review of theory and evidence on peer effects, highlighting the differences in the identification strategies adopted and the findings.

Even empowered with all these more sophisticated estimation methods and richer data than earlier studies, researchers are still divided on the importance of peer effects. However, it is worth emphasising that even those studies that find statistically significant effects, tend to find relatively small effects: 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. Our research design is closest to the papers 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 cohort schoolmates in England's secondary schools.

III. Empirical Strategy

linear in means regression estimates: methods

The aim of our empirical work is to estimate the effect of a secondary school students' peers' pre-existing achievements and characteristics, on his or her subsequent secondary school achievements. These are 'contextual' peer effects, to use the commonly applied terminology of Manski (1993, 2000). In what follows, we work with an aggregated panel design that allows us to estimate these effects on consecutive cohorts (c) of students making primary (p) to secondary (s) school transitions at age 11. Using this aggregated design allows us to exploit putatively exogenous, unanticipated year-on-year shocks to the peer group quality experienced by students making a given primary school to secondary school transition (ps) in different years. We refer to these groups, indexed by ps, as *primary-secondary transition groups* (or simply 'transition groups'). As a result of this aggregation, there is one observation of each transition group in each cohort. [5] As set out below, defining and aggregating to these groups allows us to control easily for transition group fixed effects and trends using first and second differences within transition groups, between cohorts. This aggregating and differencing helps to eliminate similarities in unobservable school preferences and abilities.

Our design is based on a simple representation of peer group effects using the 'linear-in-means' formulation that is standard in the literature (i.e. we assume that mean peer group characteristics exert a linear effect on outcomes). The aggregating-and-differencing method we use is, however, generalizable to more elaborate types of heterogeneity and complementarity between groups with different characteristics and abilities (and non-linearities in peer group effects) as we discuss in section 0. The starting point for our empirical specification is a 'value-added' educational production function for groups of students in transition group ps in cohort c:

$$y_{psc} = \rho a_{psc} + \beta g a_{sc} + u_{sc} + v_{psc} \qquad (1)$$

[5] Cohort is equivalent to an index of the calendar year in which students make the primary-secondary school transition. Note that students in England do not repeat grades or retake the Key Stage tests.

in which average secondary school achievements for each cohort of students in a given primary-secondary transition group (y_{psc}) depends on student prior achievement and background characteristics that we can observe in our data (a); on mean peer group prior achievement and background characteristics in their secondary school cohort (ga); and on a composite error term. This error term includes unobservable components of secondary school quality (u_{sc}, excluding the peer effects represented by βga). The error term also includes unobservable characteristics of the students in the transition group (v_{psc}), i.e. unobservable student attributes, effects from primary school quality (including teaching, resources and primary school peer effects).

Our aim is to get consistent estimates of the parameter β in this specification, interpreted as the causal 'contextual' effect of secondary school peer characteristics, i.e. the expected difference in achievements between similar students who attend similar secondary schools with different peer group quality ga. It is well known that estimation of specifications of peer group effects models like (1) is problematic, because the unobservables are correlated with the group characteristics through self-selection of students into peer groups of different quality. In our case, this is because students are making choices over which secondary school to attend.[6] Note that it is infeasible to control for secondary-school-by-cohort fixed effects to eliminate u_{sc} since this also eliminates variation in secondary school peer group quality. However, as set out below, we can use controls for primary-school-by cohort fixed effects and primary-by-secondary-school fixed effects to eliminate the main unobservables that are a source of concern in equation (1).

The first part of this strategy for mitigating the biases induced by these unobserved components is to control for fixed effects common to students making the same primary-school-to-secondary-school transition. We do this by first-differencing the variables across adjacent cohorts, within primary-school-by-secondary-school groups. This eliminates preferences and other unobservables that are common to students who make the same choices over primary and secondary schooling over the years

[6] In the empirical work, we measure peer group composition at the very start of secondary school four months after enrolment (not at age 14). There is, therefore, very little scope for students to sort on revealed secondary school peer group quality, as measured at this stage.

in our data. This transformation also eliminates secondary school fixed effects and primary school fixed effects. As a second step, primary-school-by-cohort fixed effects components that are common to all students leaving primary school *p* in cohort *c*, can be controlled for in these first differenced models by using a within group transformation (differencing the variables from primary-school-by-cohort means). The primary-by-secondary-school fixed effects control for cohort-to-cohort changes in primary school peer group and quality (v_{psc} in equation (1)), and which have a mechanical correlation with secondary school peer group (ga_{sc}) given than the secondary and primary school peer group have common members. These transformations yield a final specification:

$$\tilde{\Delta} y_{psc} = \rho \tilde{\Delta} a_{psc} + \beta \tilde{\Delta} ga_{sc} + \tilde{\Delta} u_{sc} + \tilde{\Delta} v_{psc} \qquad (2)$$

Here, the notation $\tilde{\Delta}$ is defined to mean, for example, for the peer group variable *ga*: $\tilde{\Delta} ga_{sc} = \left(ga_{sc} - \overline{ga}_{sc}^{p} \right) - \left(ga_{sc-1} - \overline{ga}_{sc-1}^{p} \right)$, where $\overline{ga}_{sc}^{p}$ is the average of the secondary school peer group characteristics for students who were in the same primary school *p* in a given cohort *c*. The first component in brackets in this expression depends on the difference between the secondary school peer group of a group of students in a cohort from a given primary school, and the secondary school peer group experienced by their fellow students in the same primary school who went to different secondary schools.[7] The second component in brackets is the same as the first, but for the previous cohort. Identification of β, thus, comes from the change between cohorts in the differences in secondary peer group quality experienced by groups of students making transitions to different secondary schools from the same primary school. This variation comes about due to between-cohort differences in observed achievements and characteristics (*a*) and from changes in the patterns of enrolment in secondary schools from year to year. The identifying assumption in our design, is that

[7] For example, suppose students from one primary school p are distributed across *k* different secondary schools. The first expression on the LHS of the expression is, for the transition group of students going from primary school p to secondary school k, $ga_k - \frac{\sum_{s=1}^{s=k} ga_s}{k} = \frac{(k-1)}{k} \left\{ ga_k - \frac{1}{k-1} \sum_{s=1}^{s=k-1} ga_s \right\}$. The second term in curly brackets is the average of peer groups in other secondary schools attended by students from primary school p. The first term is the peer group in secondary school k.

this shock to peer group quality upon moving to secondary school is unpredictable and unanticipated, uncorrelated with either primary or secondary school quality and, generally, uncorrelated with unobservable determinants of subsequent achievement at secondary school (i.e. it is 'as good as random'). If the change in peer group is unanticipated, there is no potential for student sorting on secondary school peer groups. As discussed in Section 0 below, there are good reasons to expect these changes in peer group to be unpredictable from the perspective of students making their school choices. This is because there is a random element in the school admissions process generated by fluctuations in the size of the age-cohort applying, and the preferences and constraints of this applicant pool.

Similarly, the (unobserved) secondary-school-by-cohort effects are transformed into $\tilde{\Delta} u_{sc} = \left(u_{sc} - \overline{u}^{p}_{sc}\right) - \left(u_{sc-1} - \overline{u}^{p}_{sc-1}\right)$ i.e. the cohort-to-cohort change, in the difference in secondary school quality between one group of primary school students, and their primary school-mates who went to different secondary schools. Again if this change is unanticipated, there is no scope for biases due to selection on unobserved secondary school quality in this transformed specification.

We can further increase the likely validity of these assumptions of unpredictability in shocks to secondary school quality and secondary school peer groups by controlling for linear primary-school-by-secondary-school trends. This is easily done in our aggregated design by double-differencing the variables across adjacent cohorts, before applying the within group transformation i.e. such that identification of peer group effects comes from:

$$\tilde{\Delta\Delta}\, ga_{sc} = \left(ga_{sc} - \overline{ga}_{pc}\right) - 2\left(ga_{psc-1} - \overline{ga}_{pc-1}\right) + \left(ga_{psc-2} - \overline{ga}_{pc-2}\right).^{8} \qquad (3)$$

Our aggregating-and-differencing design makes it feasible to control for these high-dimensional time trends – for around 60,000 primary-by-secondary school groups in our empirical analysis below.

For estimation of equation (2) and (3), we measure secondary school achievement (y) by standardised 'Key Stage 3' (ks3) national tests in Maths, Science and English taken at age 14. In our

[8] Given that for a time-constant x, $\Delta\left(x \cdot c\right) = x$ and $\Delta\left(x\right) = 0$

main estimates, prior achievement (a) on entry to secondary school in September at age 11 and the mean peer group prior achievements in secondary school (ga), are both measured by 'Key Stage 2' (ks2) tests taken in primary school earlier in the same calendar year in May (the Key Stage 2 tests are comparable to the Key Stage 3 tests, but less advanced). We can further refine measurement of prior achievement and control for pre-existing student trends in achievement by controlling for tests at an even earlier stage in primary school, using data on the 'Key Stage 1' (ks1) achievements of the same children at age 7. Our data provides additional information on student characteristics which we can use as control variables, and to provide alternative peer group measures, as discussed in section 0.

In summary, we use a dataset of student ks3, ks2 and ks1 test scores and other student characteristics aggregated to primary-school-by-secondary-school-by-cohort cells. In our basic set up, we regress mean ks3 test scores of this student group on the mean ks2 test scores of other students in same secondary school cohort, with controls for the student group's own mean ks2 test scores, primary-school-by-secondary-school fixed effects and trends (which, as explained above are eliminated by first or second differencing), and primary-school-by-cohort fixed effects (eliminated by the within-groups transformation).

There are several innovations here relative to previous peer group studies, which are feasible due to the fact that we study students changing schools between the primary and secondary phases. Firstly, aggregation to primary-school-by-secondary group cells allows us to eliminate high dimensional fixed effects and trends by simple differencing techniques. No equivalent aggregation is feasible in previous studies that do not exploit information on origin and destination schools for students moving between schools. Secondly, focussing on primary to secondary school transition means we have substantial changes in peer group composition between primary and secondary school. This implies that students from the same cohort in the same primary school end up going to many different secondary schools, generating wide variation in secondary peer group quality, conditional on primary school attended. Therefore, we can control for the initial conditions of students more effectively by including primary-by-cohort fixed effects. This is not feasible in studies which, for example, simply track changes in peer group quality within the same school from one year to the next. Moreover, an important feature of the primary-secondary school transition process is that there are substantial changes between cohorts coming partly from cohort to cohort variation in student characteristics, but mainly due to random variations in the secondary admissions process. It is worth noting, finally, that there is no loss of salient information in this aggregated set up, because any meaningful variation in peer group composition occurs at an even more aggregated, secondary-school-by-cohort level. Therefore, estimation at the individual level brings no particular advantages and makes eliminating multi-way high dimensional fixed effects and trends virtually impossible.

balancing and placebo tests: methods

Our identifying assumption, as explained above is that variation in group prior achievements (ga_{sc}) is uncorrelated with other factors determining a student's ks3 test scores (y), conditional on own prior achievements, primary-by-secondary school fixed effects and trends, and primary-by-cohort fixed effects. 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 salient observable characteristics of the transition group are uncorrelated with the innovations to peer group quality, and hence with unobservable characteristics (assuming selection on observables is a guide to selection on unobservables following Altonji *et al.*, 2005) . These balancing tests are carried out by re-estimating equations of the form of equations (2-3) 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).

As further evidence that our estimates are causally related to the changes in peer group experienced by students, rather than to sorting or selection, we present a simple 'placebo' test based on peer group variables relating to older and younger peer groups. In this test, we reproduce the first-differenced specification of equation (2) but adding the lags (past cohorts) or the leads (future cohorts) of the peer group mean ks2 variable, alongside the cohort-specific secondary peer group experienced by a group of students making the primary school transition in a given year. Assuming that peer effects operate amongst peers within cohorts rather than across cohorts, we would not expect to see a large or significant impact here, and evidence of effects from these adjacent cohorts would suggest that our baseline estimates in Table 2 are spuriously related to sorting and school selection.

extensions to the key results: background versus primary schooling; heterogeneity

In the setup above, peers' primary school 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.); b) teaching quality or other factors in peers' primary schools that are common to children attending those schools. Knowing which of these matters is important. Peer effects originating from prior schooling quality are potentially more interesting from a policy perspective, because they imply that teaching interventions feed through to others in later years through peer effect mechanisms implying long run 'social multiplier' effects (Glaeser *et al.*, 2003). We assess the relative importance of these sources of influence, by partitioning peers' prior achievements into two components: the part due to family background and early-age achievements (age7, key stage1), and a part due to the test score gains they experienced during primary school (key stage 2 minus key stage 1) scores.

Finally, to address questions about heterogeneity in students' response to their peers, and complementarities between student and peer characteristics, we estimate equation (2-3) separately for different student groups. Estimation in this case requires that we re-aggregate the data into primary-by-secondary-by-student-type-by cohort (*psgc*) groups:

$$\tilde{\Delta} y_{pscg} = \tilde{\Delta} a_{pscg} + \beta \tilde{\Delta} g a_{scg} + \tilde{\Delta} u_{scg} + \tilde{\Delta} v_{pcg} \qquad (4)$$

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.

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.

IV. 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 2009. Students do not repeat grades or retake these Key Stage tests. 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. For the period of our study, there were about 2,700 secondary schools of this type England and about 14,500 primary schools.[9] For most of these schools the admissions process was one that might be called 'geographically constrained choice'. Applications were handled centrally by the relevant Local Authority (LA), and in London admissions across LAs were coordinated by a pan-London admissions body. Applicants list schools in order of preference, and in principle could 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.

[9] 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% of students, but we do not have data on these students.

During the years to which our analysis refers, the large majority of students attended '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 over-subscription, the LA applied a standard set of criteria for deciding admissions, typically prioritising children with siblings in the school, children with special needs, and children who live closest. Most other schools were faith 'Voluntary Aided' schools (15%), or were 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 in our data include faith schools under Local Authority control ('Voluntary Controlled', 3%), City Technology Colleges (0.3%) and Academies (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[10], 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. Since 2010, after the period covered by analysis, Coalition government policies have changed the school institutional, funding and admissions landscape considerably, with the emergence of many more 'Academies' and 'Free Schools' which have much greater degrees of autonomy and are free of Local Authority control.

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

[10] 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.

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. This variation in demand is, in turn, driven by the size of the age cohort in the population, the preferences and constraints of the applicant pool, the number of applicants with siblings already at the school, and the number of children with special needs. For example, a child who hoped to attend a particular local school may, unexpectedly, find they are outside the catchment area due to a high number of children in their birth cohort in a given year, or a high proportion of high-priority applicants with siblings already in the school. Similarly, a child entering a school which previously had high average intake ability from a narrow catchment area, could find lower than expected peer group quality because their age cohort is small, causing the school's catchment area to expand to encompass a more diverse student pool. Therefore, in any year, families may have to compromise on the schools they apply for, may not be awarded their first choice of school, and the peer group quality in the school may change in ways that they had not anticipated. 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 exploits the putatively random components of this variation over time as a source of exogenous variation in intake and peer group quality.

Our peer group quality measure is school-by-cohort mean prior achievement (and other characteristics in some specifications). Using school-by-cohort peer group definitions rather than class-based definitions avoids biases induced by within school sorting and selection, and provides a consistent estimate of linear in means peer effects in class groups if the assignment to classes within schools is random. Under these conditions, if individuals are influenced by peers in multiple classes within their school cohort, then measuring the peer group in just one of these classes would introduce measurement error and lead to downward biased estimates. 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 was not pervasive during the years in our data. 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. The ideal design might involve instrumenting a measure of class (or other sub-school peer group) quality with the school-level variables, but 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. In Section 0 we will present evidence based on subject specific effects and school sizes that supports this approach.

V. Data Sources

The UK's Department for Education (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.[11] Using these data we create own-achievement measures at ks1,

[11] 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.

ks2 and ks3 and calculate peer group mean ks1 and ks2 achievement at the point of entry into secondary school.

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 0.[12]

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.

VI. Results

description of the key variables

Table 1 presents the descriptive statistics for our main estimation sample. The underlying sample contains over 1.5 million 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 in the first two rows are based on a student's percentile rankings in national tests, so

[12] 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.

have a mean of 50 and a standard deviation of about 28.8 in the student distribution. The standard deviations in the primary-secondary transition group cells are slightly less than this, at around 22. The statistics for the secondary peer group means in the next three rows are more revealing, and show that there is substantial variation in the composition of school peer 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 total variance is between transition groups). First differencing (row 4) halves this figure to 4.4 percentiles (4% of the total variance is between groups). Double differencing (row 5) increases the standard deviation back to just over 7 percentiles, implying that the first differences and lagged first differences are not strongly correlated (10% of the total variance is between transition groups).

The (unweighted) group sizes are reported in the rows 6-8 of Table 1. On average, across transition groups, there are around 169 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 are 180 and 25, implying 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 14,158 primary schools, 2,727 secondary schools and 59,856 primary-secondary transition groups. Once we difference the data we lose cohorts and, hence, some schools and primary-secondary transition groups when these are not represented in multiple cohorts. In the double-differenced dataset we have 13,305 unique primary schools, 2,526 unique secondary schools and 33,413 transition groups.

linear-in-means peer effects on ks3 test scores: results

We now turn to our main regression estimates of the links between secondary school peer group prior achievements and student test score outcomes. The estimates of the coefficient of interest (β in equation 2-3) in the various specifications are shown in Table 2.[13] The data in all regressions is aggregated to primary-by-secondary-school-by-cohort transition groups.

The table reports coefficients and standard errors clustered at the local school district level (LAs). This clustering scheme allows for inter-temporal and spatial autocorrelation within school districts, and are very conservative, given there are only 141 districts (clustering a school level gives much smaller standard errors). The estimates are grouped into three sets of three specifications. Columns 1-3 provide simple OLS estimates without any differencing or fixed effects, and are shown as a benchmark for reference only. Columns 4-6 apply first-differences of the data between cohorts, within primary-secondary school transition groups and include primary-school-by-cohort fixed effects (equation 2). Columns 7-10 apply double differences between cohorts, plus primary-school-by-cohort fixed effects. The first specification in each group (columns 1, 4 and 7) is a value-added specification with transition ks3 as the dependent variable, and ks2 as a control variable (plus year dummies). The second specification (columns 2, 5 and 8) adds in earlier ks1 (age-7) test scores to control for student specific trends in achievement during the primary school phase.[14] The third specification (columns 3, 6 and 9) brings in a control variables set (*x*) describing the students in the transition group and the schools they choose, and characterising the neighbourhood in which students in the transition group live. 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. This control variable vector includes dummy variables for the proportion of the primary school making that particular primary-secondary transition in a given year, split into deciles, as a control for secondary school popularity. The control variables also include primary-school-by-cohort and secondary-school-by-cohort student numbers. The student residential neighbourhood characteristics include the proportion with no qualifications, proportion high-qualified (degrees),

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

[14] In the specifications shown we control for the average ks2 score across maths, science and English, given the dependent variable and peer group ks2 score are also averages across these subjects. Alternative specifications in which we control for maths, science and English ks2 scores separately give nearly identical results.

proportion born in the UK, proportion ethnically white, proportion in employment, and proportion social renting.

The coefficient of 0.356 in column 1 implies that a one standard deviation increase in peer group prior achievement (9 percentiles) is associated with a 0.10 standard deviation increase in students' own achievement (which is 28.8 percentiles in the student level data). This estimate is not, however, one we would wish to take seriously given the issues of non-random sorting of students into secondary schools discussed in Section 3. Controlling for transition-group own-ks1 in column 2, slightly lowers to coefficients to 0.339, while adding student and neighbourhood characteristics in column 3, further lowers the coefficient, to 0.227.

The remaining columns of Table 2 introduce the first and second differencing strategies presented in Section 3, in which identification of the peer group effect comes from cohort-cohort shocks in secondary school peer group quality, for students making the same primary to secondary school transition in subsequent years. Looking across from columns 4 to 9, one thing is striking: the estimate of the effect of peer group ks2 on student's ks3 scores remains extremely stable. Adding in additional control variable sets (ks1, x) makes very little difference. The stability of the coefficients implies that once we have conditioned on ks2 test scores, first-differenced the data within primary-by-secondary school transition groups, and controlled for primary-school-by-cohort effects on ks2, the variation in peer group ks2 scores appears to be largely uncorrelated with other factors influencing student ks3 achievements. Further tests of this claim are presented in Section 0 below. 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. Note that the double differenced specification with primary-by-cohort fixed effects places quite high demands on our data, because we have under 60,000 double-differenced observations, and over 33,000 primary-by-cohort cells.

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 (approximately 9 percentiles) is associated with a 9 x 0.075/28.8 = 0.02 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.

'balancing' and placebo tests: results

The stability of the peer effect estimates in Table 2 suggests that 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 3, Columns (1)-(11). The list of characteristics is not exhaustive, but we present a selection which are available in our data, characterise distinct aspects of the student background, are only moderately correlated with each-other (to avoid redundancy in the tests), but which are strongly correlated with student achievement and value added. These variables all have individually statistically significant coefficients when included as explanatory variables in our main regressions in Table 2. The regressions presented in Table 3 are analogous to Table 2, Columns 4 and 7, but with a student background characteristic replacing student ks3 as the dependent variable. Scanning across Table 3, it can be seen that nearly all of the coefficients are statistically insignificant, and small in magnitude. The only exception is for the proportion of high-qualified residential neighbours in column 9, which is marginally significant in the first-differenced specification, though non-significant after double differencing. All this evidence supports our identifying assumption that cohort to cohort innovations in peer group prior achievements in our data are uncorrelated with other factors determining a student’s ks3 test scores.

One potential threat to the randomness of the peer group changes over time is the system of inspections and public ratings carried out by the schools regulator Ofsted. Schools are inspected at intervals, and the inspection results published on the Ofsted website. The inspection results are summarised with gradings from 'Outstanding' through to 'Inadequate'. A new inspection result, and, in particular, the award of an 'Outstanding' or 'Inadequate' rating, could lead to surge or drop off in ability in the group applying to the school, leading to a correlation between the temporal shocks to applicant student and peer group abilities. To test for this possibility, we have augmented our main specifications to control for additional indicators of new Ofsted inspections and inspection grades occurring at the time of admission for each cohort, but this made no difference to our key parameters. Additionally, in Table 3 columns (12) and (13) we assess whether changes in peer group ability on entry is associated with 'Outstanding' or 'Inadequate' inspection ratings, but find no evidence of an association. In summary, responses to new Ofsted inspections do not explain the relationship between student and peer group achievements shown in our main estimates.

The results from the placebo tests described in Section 0 are shown in Table 4. These regressions reproduce Table 2, column 4 but adding in the older or younger cohort peer group ks2, alongside the student's own cohort peer group. Assuming cross-cohort peer group effects are small, we do not expect to find big coefficients on these variables. Column 1 adds in the one year lead of the secondary school peer group. Recall, the variables are first differenced within primary-secondary school groups, and the regressions control for primary-school-by-year fixed effects, so the coefficient on the lead of peer group ks2 is the response of a student to the change in peer group experienced by those in the younger cohort in their primary school who choose the same secondary school. Reassuringly, the coefficient on the lead of peers' ks2 is close to zero and insignificant, whereas the coefficient on contemporaneous peer group remains unchanged from the results in Table 2. Column 2 repeats this analysis, but with the lag of peers' ks2 rather than the lead, corresponding to the peer group experienced by the older cohort. Again, the coefficient on contemporaneous peers' ks2 is broadly in line with the baseline estimates (although in this case only significant at the 10% level), whereas the ‘effect’ of peers in the older cohort is zero and insignificant. Overall, the placebo tests in Table 4 present convincing evidence that our baseline peer effect estimates are causally linked to changes in a student’s own cohort peer group, and not spuriously generated by sorting across schools.

peers' background versus peers' primary schooling: results

The results in Table 2 indicate that students have greater achievement gains between ks2 and ks3 when their peers at school have higher ks2 achievements. However, peers’ could have higher ks2 achievements for a number of reasons, including their innate ability, family background or the quality of the teaching they experienced during primary school, and the results so far as silent as to which, if any, of these channels matters more than others. In Table 5, we extend the specifications in Table 2 column 9 to split the effect of peers' mean ks2 scores into various components, a value-added component (ks1 to ks2 test score gains), early achievements (ks1 scores, at age 7), and the influence of a wider range of peers’ background demographic characteristics.

We start first by looking at earlier achievements and primary school value-added in Column 1 of Table 5. The specification is the same as column 9 of Table 2 but with secondary peers' mean ks2 scores replaced by mean ks1 scores and mean ks1-ks2 value added. Column 2 then adds in peer background characteristics, namely the proportions on free meals (FSM), who speak English as a first language, who are male, with white British ethnicity and their mean age.

The significant and stable coefficient on secondary peers’ ks1 scores in both specifications indicates that the characteristics already embodied in peer’s early achievements at ks1 at age 7 are the most important drivers of peer effects in secondary school. Academic skills acquired by peers between ages 7 and 11 appear to matter too, in that the point estimate on peers’ primary school value added is of a similar order of magnitude to that on ks1, although the coefficient is not statistically significant. The coefficient on peers’ early achievements remains significant in column 3 when we add in peer background characteristics in column 2. The point estimates on these other demographic peer attributes imply effects that are smaller, but of a similar order of magnitude to the effect of peers’ prior achievement. For example, a one standard deviation (0.15) increase in the proportion of children claiming free school meals (FSM) is associated with a reduction in student ks3 achievement of around 0.01 standard deviations. A one standard deviation increase in the proportion of peers with English as a first language is associated with a reduction in ks3 achievement of around 0.01 standard deviations. However, the standard errors for the coefficients on these additional socioeconomic characteristics are large, and none of the coefficients is statistically significant individually or as a group (p-value 0.246). The most plausible interpretation of these results overall is that these background socioeconomic characteristics do not matter much over and above the early achievements represented by ks1 scores.

On balance, 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, or any direct effects from peer group demographics. These findings hint that these secondary peer effects are ability-related, 'contextual' in nature (Manski 1993), and related students' initial conditions prior to age 7.

heterogeneity, complementarities/non-linearities and subject specific effects: results

We turn now to questions about the response of different student types to peer group ks2 achievements. Table 6 splits the primary school to secondary school 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), or low and high ability based on above or below median test scores at ks1 (age7) and ks2 (age 11). 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 2, column 9. There is evidence here of slightly bigger point estimates for boys than girls, bigger but less significant effects for FSM students and bigger effects for older students, but the differences between these groups are not statistically significant. The point estimates for above and below median ability students, as tested at age 7 or age 11 are also close to each-other in magnitude. Further disaggregation by transition group ks2 quintile (not tabulated) suggests that these effects are strongest for students in the top 20% and bottom 20% of ks2 ability, although differences across the range are not significant. We also looked in more detail at complementarities between students of high and low ability, by estimating separate coefficients for each prior achievement quintile, and disaggregating the peer group ks2 measure into the proportions of peers in each achievement quintile, but found no strong, stable or systematic differences.[15] This is in contrast to Lavy *et al.* (2012), who find bigger impacts from the lowest achieving students when looking in the cross-section at how peers affect a student's relative performance in different subjects.

We also look at differences across subjects, by changing the dependent variable and peer group quality variable to measure achievements in the specific subjects separately – maths, science and English – rather than using the average score across all subjects. The results are reported in Table 7. Evidently, the effects appear strongest in maths and science, for which the coefficient is around 0.073 than in English, where the coefficient on secondary' peers English scores is 0.022 and statistically insignificant. However, note again that effects as low as 0.014 are within the 95% confidence interval for maths and science, and effects as high as 0.12 are within the 95% confidence interval for English. Thus, we cannot be certain that there are population differences in the effects of peers in different subjects. These subject specific results also reinforce our argument that we do not lose information or introduce bias by measuring peer group quality at the school-cohort level rather than at the level of narrower groups or classes. As discussed in Section 0, the main argument against using school-by-cohort mean peer group measures is that, when students are assigned to classes according to their ability, they may not be fully exposed to changes in intake ability at the school level. However, in England setting is more prevalent in maths and science than in English and yet it is in maths and science that we detect the most significant peer group effects. As a further test of the reliability of using school-by-cohort peer group measures, we interacted the peer group mean ks2 score variable

[15] Note that applying our method to specific student abilities requires that we have students in the same primary-by-secondary school-by-achievement group in consecutive years, raising some concerns about sample selection issues and generalizability. Therefore we do not place too much emphasis on these results.

with school size indicators, to see if there were differences in small schools relative to large schools, but found no differences.

Overall, we find no evidence here of big differences in the effects of peers according to student type or early age abilities, subject type or school size. These findings suggesting that the effects of secondary peers' ks2 is quite general, and a linear in means representation of peer group effects, with peer groups defined at the school-by-cohort level, is adequate.

VII. Discussion and Conclusions

The results in the paper suggest that student academic achievement at age 14 in England is influenced by the prior, age-11 achievements of their secondary school peers. 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 unique, aggregated, cohort-differenced research design that controls for primary-by-cohort fixed effects and primary-by-secondary fixed effects and trends. The design controls for sorting and selection into schools and for unobservable factors affecting students who make similar schooling choices. Given the richness of our student record dataset, we have – unlike any previous study - also been able to control for student’s achievement at a much earlier stage in a student’s school career using data on test scores at age 7. A range of balancing tests support our identification strategy, showing that our peer quality measure is not correlated with individual student, school and neighbourhood characteristics once we control appropriately for primary-by-secondary and primary-by-cohort 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 0.02 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. Direct effects from family background, conditional on early achievement, and progression during the later years of primary schooling preceding secondary school entry take a backseat role. 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 *et al*., 2003), unless these interventions occur very early on in life. Our results show no heterogeneity across student demographic types or ability groups.

The magnitude of our estimates is in line with, or lower than previous quasi-experimental and experimental studies. This finding adds weight to the argument that group composition matters little, relative to other factors (such as student background) that drive differences in achievements between students. However, scaled relative to other school-level factors that influence student achievement, peer effects of this magnitude are not inconsequential, because schools overall contribute relatively little to differences in achievement between students. For example, Kramarz *et al*., (2009) estimate school fixed effects on student achievement from a panel of primary school students in England, and find that the correlation of school fixed effects with student achievement is around 0.12-0.16, i.e. a one standard deviation improvement in school quality overall raises student achievement by 0.12-0.16 standard deviation. Some of the biggest effects claimed in the economics of education literature are general teacher quality impacts, estimated from teacher fixed effects in student value added models on multiple cohorts of students. Typically, in this literature (Hanushek and Rivkin, 2010), a one standard deviation increase in teacher quality also leads to a 0.10-0.15 standard deviation improvement in student achievement. Benchmarked against this, peer group effects of the order of magnitude we find here are not inconsequential. Of course test scores are not everything, and better peer-groups might also provide other immediate and long run benefits which we are not measuring – 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

Description of the key variables: primary-by-secondary-by-cohort cells

	Observations	*Mean*	*Std. Dev.*	*Min*	*Max*
Primary-by-secondary-by-cohort cells.					
Ks3 test scores	184,866	49.67	22.18	1	100
Ks2 test scores	184,866	49.93	21.81	1	100
Secondary peers' ks2 scores					
Levels	184,866	50.2	9.3	7	89.5
First differenced	110,194	-.109	4.4	-42.7	38.9
Double differenced	59,724	-.028	7.1	-57.0	74.2
Secondary school students	184,866	168.8	52.4	1	388
Primary school students	184,866	36.2	20.5	1	186
Primary-by-secondary students	184,866	8.0	11.5	1	132
Number of primary schools	14,158				
Number of secondary schools	2,727				
Primary-by-secondary groups	59,856				

Notes: Data from National Student Database Statistics for students in comprehensive (non-selective) state schools. Statistics are unweighted. Peer group composition measured on entry to secondary school, aged 11/12. Ks3 test scores relate to years 2005, 2006, 2007 and 2008. Note, by construction the standard deviation of the percentiles of ks3 in the student-level data is 28.8.

TABLE 2

Linear-in-means peer effects on ks3 test scores: regressions using primary-by-secondary-by-cohort cells

	(1) OLS|ks2	(2) OLS|ks2,ks1	(3) OLS|ks2, ks1,x	(4) D|ks2	(5) D|ks2 ks1	(6) D|ks2, ks1,x	(7) 2D|ks2	(8) 2D|ks2, ks1	(9) 2D|ks2, ks1,x
Secondary peers' ks2 scores (*Levels*)	0.356***	0.339***	0.227***						
	(0.013)	(0.012)	(0.010)						
Secondary peers' ks2 scores (*First Difference*)				0.073***	0.072***	0.073***			
				(0.019)	(0.019)	(0.019)			
Secondary peers' ks2 scores (*Second Difference*)							0.072*	0.071*	0.078*
							(0.033)	(0.033)	(0.033)
Own ks2 scores	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Own ks1 scores	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Own and neighbourhood characteristics	No	No	Yes	No	No	Yes	No	No	Yes
Primary-by-cohort fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations (primary x secondary x cohort cells)	184,866	184,866	184,866	110,080	110,080	110,080	59,646	59,646	59,646
R-squared	0.745	0.755	0.773	0.833	0.834	0.838	0.850	0.850	0.854

Notes: All test scores scaled as 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 (10 categories), student number in secondary school and primary school cohort, and neighbour characteristics which 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 year dummies (in columns 1-3). 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 3

Balancing tests: regressions using primary-by-secondary-by-cohort 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 %	(12) Out-standing Ofsted	(13) In-adequate Oftsed
First Difference													
Secondary peers' ks2	0.015	-0.020	0.043	-0.015	-0.000	0.049	-0.001	0.018*	-0.010	-0.011	0.012	0.000	0.000
-	(0.019)	(0.040)	(0.032)	(0.064)	(0.005)	(0.042)	(0.000)	(0.009)	(0.007)	(0.012)	(0.010)	(0.001)	(0.000)
Primary-by-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	110,318	110,318	110,318	110,318	110,318	110,318	110,318	110,080	110,080	110,080	110,080	109,116	109,116
Second Difference													
Secondary peers' ks2	0.031	-0.035	0.105	0.073	0.003	0.049	-0.001	0.022	-0.015	-0.020	0.002	0.000	0.000
	(0.032)	(0.059)	(0.053)	(0.089)	(0.007)	(0.058)	(0.001)	(0.014)	(0.011)	(0.023)	(0.015)	(0.002)	(0.001)
Primary-by-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	59,763	59,763	59,763	59,763	59,763	59,763	59,763	59,646	59,646	59,646	59,646	59,408	59,408

Notes: Standard errors clustered at local school district. ***0.1%.**1%.*5%. FD = first difference, 2D second difference. Regressions weighted by secondary school size. Regressions control for own 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. Other notes as Table 2.

TABLE 4

Placebo test response to peer group change experienced by older and younger cohorts

	(1)	(2)
First difference		
Younger cohort secondary peers' ks2 scores	-0.011	
	(0.029)	
Secondary peers' ks2 scores	0.074**	0.060
	(0.027)	(0.032)
Older cohort Secondary peers' ks2 scores		0.000
		(0.026)
Observations	59,686	59,686

Notes: All test scores scaled as percentiles in the student distribution. Standard errors clustered at local school district. ***0.1%.**1%.*5%. Regressions weighted by secondary school size. Peer group measured on entry to secondary school, aged 11/12. Data for students taking ks3 in 2005,2006,2007,2008 in comprehensive schools. Other notes as Table 2.

TABLE 5

Peers' background versus peers' primary schooling: regressions of ks3 scores on peer characteristics using primary-by-secondary-by-cohort cells - 2nd differenced regressions

	(1)	(2)
Secondary peers' primary school ks1 to ks2 value added	0.071	0.062
	(0.047)	(0.048)
Secondary peers' ks1 (age 7) scores	0.081*	0.076*
	(0.034)	(0.037)
Secondary peers FSME		-2.520
		(1.814)
Secondary peers English first language		1.623
		(1.072)
Secondary peers male		0.044
		(2.027)
Secondary peers' age (months)		-0.483
		(0.334)
Secondary peers White British		-0.284
		(1.374)
F-test, peer demographics (p-value)	-	0.246
F-test, peer demographics and ks1 (p-value)	-	0.030
Own ks2 scores	Yes	Yes
Own ks1 scores	Yes	Yes
Own characteristics and neighbourhood	Yes	Yes
Primary-by-cohort fixed effects	Yes	Yes
Observations	59,646	59,646
R-squared	0.854	0.854

Notes: All test scores scaled as percentiles in the student distribution. Standard errors clustered at local school district. ***0.1%.**1%.*5%. Regressions weighted by secondary school size. Peer group measured on entry to secondary school, aged 11/12. Data for students taking ks3 in 2005,2006,2007,2008 in comprehensive schools. Other notes as Table 2.

TABLE 6

Heterogeneity by student characteristics: regressions using primary-by-secondary-by-cohort-by-characteristics cells

	(1) Girls	(2) Boys	(3) Not FSM	(4) FSM	(5) Younger	(6) Older	(7) Low ability at ks1	(8) High ability at ks1	(9) Low ability at ks2	(10) High ability at ks2
Second difference										
Secondary peers' ks2 scores	0.047	0.095*	0.067*	0.103	0.059	0.100*	0.078**	0.087*	0.097*	0.078*
	(0.037)	(0.039)	(0.027)	(0.091)	(0.033)	(0.041)	(0.025)	(0.034)	(0.039)	(0.033)
Own ks2 scores	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Own ks1 scores	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Own characteristics and neighbourhood	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Primary-by-cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41,405	40,918	54,318	17,094	45,806	39,619	59,646	43,036	43,211	42,379
R-squared	0.892	0.893	0.863	0.938	0.882	0.898	0.854	0.837	0.759	0.791

Notes: All test scores scaled as percentiles in the student distribution. Standard errors clustered at local school district. ***0.1%.**1%.*5%. Regressions weighted by secondary school size. Peer group measured on entry to secondary school, aged 11/12. Data for students taking ks3 in 2005,2006,2007,2008 in comprehensive schools. Other notes as Table 2.

TABLE 7

Differences by subject

	(1)	(2)	(3)
First difference			
Secondary peers' ks2 scores: Maths	0.073**		
	(0.027)		
Secondary peers' ks2 scores: Science		0.072*	
		(0.031)	
Secondary peers' ks2 scores: English			0.022
			(0.050)
Observations	59,640	59,640	59,640

Notes: All test scores scaled as percentiles in the student distribution. Standard errors clustered at local school district. ***0.1%.**1%.*5%. Regressions weighted by secondary school size. Peer group measured on entry to secondary school, aged 11/12. Data for students taking ks3 in 2005, 2006, 2007, 2008 in comprehensive schools. Other notes as Table 2.