EVERYBODY NEEDS GOOD NEIGHBOURS? EVIDENCE FROM STUDENTS' OUTCOMES IN ENGLAND

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Abstract:

We estimate the effect of neighbours' characteristics and prior achievements on teenage students' educational and behavioural outcomes using census data on several cohorts of secondary school students in England. The complete coverage of our data allows us to investigate heterogeneity and non-linearities in the effect the neighbourhood composition to an unprecedented level, which is of central importance for current housing policies worldwide. Our research design is based on changes in neighbourhood composition caused by residential migration amongst students in our dataset. The longitudinal nature and detail of the data further allows us to control for student unobserved characteristics, neighbourhood fixed effects and time trends, school-by-cohort effects, as well as students' observable attributes and prior attainments. Our results show that neighbourhood composition has no effect on test scores. However, we find some small effects on behavioural outcomes, such as attitudes towards schooling and anti-social behaviour, which are heterogeneous for boys and girls.

Keywords: Peer and neighbourhood effects; cognitive and non-cognitive outcomes; secondary schools; housing policy.

JEL Classifications: C21; I20, H75; R23.

1. Introduction

There are significant disparities between the achievements, behaviour and aspirations of children growing up in different neighbourhoods (Lupton et al., 2009). These disparities have long been a centre of attention for researchers and policy makers concerned with addressing socio-economic inequalities. Indeed, many area-based policies are predicated on the idea that individuals' outcomes are causally linked to the characteristics and behaviour of people who live around them (see discussions in Currie, 2006 for the US, and Cheshire et al., 2008 for the UK). Policies of this type include inclusionary zoning and desegregation policy, plus regeneration and mixed housing projects, such as 'Hope VI' in the US and the 'Mixed Communities Initiative' in England. The theoretical relationships between people and their neighbours that underpin these policies are generically referred to as 'social interaction effects', 'neighbourhood effects' and 'peer effects' in the economics literature.

In this paper, we present new evidence on the effect of neighbourhood composition on cognitive and non-cognitive outcomes from age 11 (grade 6) through to age 16 (grade 11), using administrative data on multiple cohorts of English school children.[1] We exploit the scale of our administrative data to split neighbours and individuals into various demographic and achievement groups. This allows us to look carefully for evidence of complementarities between neighbourhood change and student characteristics in the production of student achievements and behaviour. These issues have been investigated in the literature on peer effects at school (see Hoxby and Weingarth, 2005; and Lavy et al 2012a and 2012b) but less so in relation to residential neighbourhood effects. The arguments in favour of policies that promote integrated neighbourhoods with the aim of improving children's outcomes implicitly rest on strong theoretical assumptions on both the strength of these effects as well as their functional form. The gains to the winners must outweigh the losers' losses if these policies are to be efficient. Residential mixing policy will reduce mean achievements if the losers lose more than the winners gain, or if children benefit from living amongst peers who are similar to themselves. There are no net gains from desegregation policies if neighbourhood effects are linear, although inequality may be reduced.[2] It is therefore important to examine directly heterogeneity and non-linearity in the effects.

The most credible existing empirical literature on causal neighbourhood effects focuses, however, on narrowly defined students' and youths' groups. These include blacks living in ghettos (Cutler and Glaeser, 1997); individuals in socially rented accommodations (Gibbons, 2002; Oreopolous, 2003; Jacob, 2004, Goux and Maurin, 2007; Weinhardt, 2010); immigrants (Edin et al., 2003 and 2011; Gould et al., 2011b); and families living in deprived neighbourhoods and relocated to better areas through the 'Gautreaux' and 'Moving to Opportunity' (MTO) randomised interventions (Rosenbaum 1995; Katz et al. 2005 and 2007; Sanbonmatsu et al. 2006). This focus limits the possibility of investigating the heterogeneity and non-

[1] Note, throughout the paper we use the term 'grade' to refer to a school year group. Although the term grade is not used in the English school system, there is no convenient term with equivalent meaning.
[2] Argys et al. (1996), Hoxby (2000) and Hanushek and Woessmann (2006) discuss these issues in the context of peer effects at school.

linearity of the effects of neighbourhood composition for two reasons. Firstly, sample sizes are often small, thus affecting the reliability of estimates obtained from further slicing the data into sub-groups of individuals. Secondly, by focussing on the most disadvantaged individuals, these studies cannot investigate whether the effects of neighbourhood composition are homogenous along the lines of students' background, and study whether any beneficial effect of a more mixed neighbourhood for disadvantaged children is counterbalanced by detrimental effects for brighter or better off students. To examine these issues in detail poses huge data requirements for empirical research.

In addition to the data requirements, empirical research on social interactions and neighbourhood effects has to overcome further obstacles to allow causal interpretation. This is because nearly all the studies in this field proceed – as does ours – by investigating the size and shape of the statistical association between individual outcomes and the socio-economic composition of the neighbourhood in which they live (i.e., by studying 'contextual effects'; Manski, 1993).[3] However, sorting of residents into neighbourhoods means that individual and neighbours' characteristics are correlated through non-causal channels, making it hard to disentangle whether the relation between neighbourhood composition and individual outcomes is causal or driven by sorting. Secondly, neighbourhoods that differ in terms of socio-economic composition potentially differ along other dimensions (often unobserved), so that it becomes difficult to tell whether any effect is due to neighbours' characteristics, or to common coincidental factors ('correlated effects'; Manski, 1993). Finally, there are uncertainties in how to define the reference groups of neighbours to which individuals respond, because neighbourhood effects could arise in geographical neighbourhoods, local friendship networks, or neighbourhood schools, but this operational scale is almost always unknown.

As with previous research, the main challenge that our identification strategy has to confront is residential sorting: the characteristics of children living in a neighbourhood are closely interwoven with those of their parents, who choose where to live on the basis of their preferences for local amenities and services, the income at their disposal and other constraints they face. The literature on the link between school quality and house prices (e.g. Black, 1999 and Gibbons et al., 2009) shows that people are willing to pay a significant premium to access better schools (as well as other amenities; see Kain and Quigley, 1975 and Cheshire and Sheppard, 1995), and suggests that neighbourhoods will be stratified along lines of income and socio-economic background. This sorting means that one child's characteristics – both observed and unobserved – will be correlated with those of his/her neighbours, confounding the causal influence of neighbours with children's and their parents' own inherent attributes.

Even without sorting of this type, the problem of unobserved differences between neighbourhoods remains important. Explicit randomisation (e.g. the MTO experiment) is not a solution because the neighbourhoods to which individuals are assigned potentially differ not only in terms of peer group composition, but also in terms of housing stock, labour market opportunities, school quality and other factors. For some purposes it might be sufficient to estimate the combined 'black-box' effects of these

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[3] Note that we are not trying to estimate Manski's (1993) 'endogenous' neighbourhood effects, i.e. the effect of neighbours' behaviour. We thus sidestep 'reflection' problems that arise when the effects of neighbour' behaviour are not separately identified from the effects of neighbour characteristics that give rise to those behaviours.

coincidental factors, but this approach does not allow researchers to isolate the effects arising specifically from neighbourhood composition. In order to overcome these difficulties, Moffitt (2001) suggests that researchers should 'reverse-engineer' the evaluation of programmes like the MTO or the Gautreaux intervention, and study changes in the outcomes of the original residents of the areas receiving relocated households. For these people, neighbourhoods remain approximately unchanged except in so far as their composition is affected by the influx of new families.

Following this intuition, our study tackles the problems of sorting and confounding neighbourhood attributes by exploiting changes in neighbourhood composition induced by the migration of residential 'movers' in a population of school-age families. We estimate the effect of these mover-induced changes in neighbourhood composition on the evolution of educational and behavioural outcomes of 'stayers' (i.e. students who do not move neighbourhoods). Using this methodology, we are able to partial out the individual fixed effects of stayers, as well as neighbourhood fixed effects, such as the presence of a library or other localised infrastructures/amenities. We are thus able to separately identify causal effects arising from changes in neighbourhood peer composition. Although we cannot pin down the potential channels through which neighbours might matter – e.g. face-to-face interactions, group behaviour, expectations and role models – this limitation is common to the literature on peer effects in schools.

Our empirical approach is related to Angrist and Lang (2004), who estimate peer effects from changes in peer composition due to students' mobility induced by desegregation programmes, to Gibbons and Telhaj (2011) who study the effect of students' between-school mobility on students who do not change school, and to Moretti (2004) who studies social returns to education in cities by looking at compositional changes experienced by non-movers. On the other hand, our method differs from the literature on peer effects in schools that exploits cohort-to-cohort variation in group composition (e.g. Hoxby, 2000; Hanushek et al., 2003; Gibbons and Telhaj; 2008, Lavy et al., 2012) because we can control for individual fixed effects without needing these individuals to move between groups. This is because our identifying variation comes from actual movements of residents in and out of neighbourhoods on those who stay put – i.e. it is induced by *real* changes in the neighbourhood experienced by stayers.[4] These population movements are sizeable, with around 25% of the neighbourhood group changing over the three year interval during which we measure the development of academic achievements. To address sample selection concerns arising from estimation using stayers only, we conduct an additional intention-to-treat analysis that includes movers in the estimation sample, but assigns them to the neighbourhoods in which they originate (thus fixing their neighbourhood assignment, and avoiding problems induced by endogenous neighbourhood choices).

Another important feature of our research design is that it allows us to control for factors that induce *changes* in movers' characteristics and stayers' outcomes within neighbourhoods over time. Firstly, by tracking several cohorts of students as they progress from primary through secondary education and

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[4] Note that while using cohort-to-cohort variation can be justified in the school setting (where pupils study with same-age peers), using this variation to study neighbourhood effects would require strong assumptions, i.e. that children do not interact with peers in the neighbourhood who are not the same age.

experience changes in the neighbourhood composition, we can control for unobserved linear trends in neighbourhood quality (e.g. gentrification or deterioration in housing quality). Secondly, we can include school-by-grade-by-cohort effects to control for the effect of changes in school quality and composition as students move between one grade and the next. This is feasible – and necessary in our context – because students change school between grades, and because there is not a one-to-one mapping between residential neighbourhood and school attended, with different students in the same residential neighbourhood attending two to three different secondary schools, and secondary schools enrolling students from around sixty different residential areas.

Finally, like other neighbourhood effects studies, we face the problem of defining the operational reference group for a child's neighbour-peer influences. In common with most other research, we have no information on friendship networks – which are in any case prone to problems of self-selection – and are not specifically interested in interactions within friendship groups. Instead, we want to investigate the influence of neighbourhood peer composition more broadly, including any effect which might arise from outside a child's own friendship group. We must approximate the level at which these influences take place. However, whereas much research is limited in the way reference units can be defined (e.g. census tracts), we have precise geographical detail on residential location coupled with information on children's school attendance and age. This richness in our data allows us to define neighbourhoods at a very small scale (on average 5 students of the same age), but also experiment with larger groupings of contiguous areas (similar to Bolster et al., 2007). We can further modify these groups to focus on students of different ages, capturing interactions within the same birth-cohort and across adjacent birth-cohorts. Finally, we can split the reference groups into neighbours who attend the same school and neighbours who attend different schools, allowing us to separate peer effects in neighbourhoods from peer effects and other shared influences in schools.

To preview our findings, we show that the large cross-sectional correlation between young peoples' test score outcomes and neighbourhood composition – measured in terms of prior achievement, eligibility for free school meals (an indicators for low family income) and special education needs (a proxy for learning disabilities) – is dramatically reduced once we control for individual and neighbourhood fixed effects by looking at changes in the neighbourhood peer composition over time. Any remaining significant association is eliminated once we control for school-by-cohort effects and/or neighbourhood-specific time trends. Differentiating between effects for neighbours in the same school and neighbours in different schools still yields no evidence that peer composition matters either way. Going beyond the simple linear-in-means specification of neighbourhood effects, we uncover no evidence of important nonlinearities, complementarities or threshold effects. In contrast, we find evidence that neighbourhood composition exerts a small effect on students' non-cognitive behavioural outcomes – such as attitudes towards schooling and anti-social behaviour – even using those stringent specifications which yielded zero effects of neighbourhood composition on cognitive outcomes. Interestingly, we find that the effect of neighbour-peers on non-cognitive outcomes is heterogeneous along the gender dimension. This is in line with a growing body of evidence showing that girls are more affected than boys by education inputs and intervention (e.g. Anderson, 2008; Angrist and Lavy, 2009; and Lavy and Schlosser 2011).

The rest of the paper is structured as follows. The next section reviews the literature, while Section 3 describes our empirical strategy and Section 4 discusses that data that we use and the English institutional context. Next, Sections 5 and 6 discuss our findings and robustness checks, while Section 7 provides some concluding remarks.

2. Literature Review: Previous Approaches and Findings

While neighbourhood effects could arise for a number of reasons, economists have put substantial emphasis on peer group and role model effects (Akerlof, 1997 and Glaeser and Scheinkman, 2001), social networks (Granovetter, 1995 and Bayer et al., 2008), conformism (Bernheim, 2004 and Fehr and Falk, 2002) or local resources (Durlauf, 1996). Disappointingly though, it has proved very difficult to distinguish between these competing theories empirically and research has mainly concentrated on estimating a general contextual effect that does not delineate the causal channels. These studies have used a variety of approaches to address biases caused by residential sorting. These methods include: *(i)* instrumental variables for neighbourhood quality (Cutler and Glaeser, 1997 and Goux and Maurin, 2007); *(ii)* institutional arguments related to social renters who have limited choice in relation to where to live, and limited mobility across social housing projects (Gibbons, 2002, Oreopolous, 2003, Jacob, 2004, Goux and Maurin, 2007, Weinhardt, 2010); *(iii)* quasi-experimental placement policies for immigrants (Edin et al., 2003 and 2011, Gould et al., 2011b); and *(iv)* fixed effects estimations to partial out individual, family and aggregate unobservables (Aaronson, 1998, and Bayer et al., 2008). Finally, there have been a number of experimental studies looking at randomised control-trial interventions, namely the 'Gautreaux' and 'Moving to Opportunity' programmes (Rosenbaum 1995, Katz et al. 2005 and 2007, Sanbonmatsu et al. 2006).

Overall, the literature tends to find negligible effects on educational attainments, but some effects on behavioural outcomes such as involvement in criminal activities or health status (Katz et al., 2007). To the best of our knowledge, this literature always focuses on lower socio-economic groups, often social renters. However, in order to inform policy it is necessary to also examine these effects for other parts of the distribution, as well. For example in the context of the Moving-to-opportunity (MTO) experiment, the probably most prominent neighbourhood effects research undertaking so far, it is not sufficient to test for effects of being re-located into a better neighbourhood alone. While this might be relevant for pure equity considerations, for welfare it also matters how residents in the receiving neighbourhoods are affected by the influx of socially disadvantaged neighbours. In our study, we pay particular attention to these kind of heterogeneities.

Another important feature of the existing literature is that the distinction between the effects of better *neighbours* and those of better *neighbourhoods* is often blurred. Competing explanations, in particular the importance of interactions with neighbours as opposed to local resources, infrastructures and school quality, are simply brushed aside. For example, Goux and Maurin (2007) do not control for the quality of local schools and other neighbourhood features. Similarly, most of the MTO-based studies (Kling et al. 2005, 2007, Sanbonmatsu et al. 2006) treat neighbourhoods as a 'black box', although more recent work has started to unpick the contributory factors (Harding et al., 2010). Some studies have tried to distinguish between school and neighbourhood level variables. For example, Card and Rothstein (2007) investigate the effects of racial segregation at the city level on the black-white test score gap in the US. Their results suggest that any effect is driven by neighbourhood segregation, rather than school segregation, although the authors cannot reject the null of equality between the two effects. On the other hand, Gould et al. (2004), who are primarily interested in the effect of school quality on the educational outcomes of Ethiopian immigrants in Israel, show that additional neighbourhood level variables have no explanatory power. Even then, these studies do not distinguish between the effects of neighbourhood peers from those of other local factors.

The fact that the existing empirical literature has not taken a clear stance on this issue has led to some confusion about what constitutes a neighbourhood effect. Notably, it is not uniformly agreed whether differences in outcomes driven by local school quality constitute a neighbourhood effect or not, even though this distinction has important policy implications. To be clear from the outset, our study specifically aims at estimating peer effects in the neighbourhood. These represent neighbourhood effects that arise from changes in the demographic composition at the place of residence, and net of potential confounding effects such as differences in local school quality (e.g. school resources, teaching methods, and quality of school intake) and other local infrastructure/resources. To this end, we exploit the richness of our data, which allows us to estimate neighbourhood-peer effects while controlling for neighbourhood fixed effects (including neighbourhood infrastructures), neighbourhood trends and school-by-cohort effects. The next section spells out our empirical strategy in detail.

3. Empirical strategy

3.1. General identification strategy: a changes-in-changes specification

Our empirical work concentrates on identifying the effect of neighbourhood composition on students' educational and behavioural outcomes during secondary schooling. As outlined in the introduction, the estimation of neighbourhood-peer effects is complicated by the sorting of individuals across neighbourhoods in relation to both observable and unobservable local factors. This sorting implies that there will be a strong degree of correlation between the characteristics of an individual in the neighbourhood and those of his/her neighbours, as well as potential correlation between local factors and the characteristics of its residents. Any study that aims to estimate the causal influence of neighbourhood peers must therefore eliminate the biases that arise from the fact that neighbourhood peer group quality is correlated with individual level and neighbourhood-level unobservables, which directly affect individual outcomes. We use a changes-in-changes design that eliminates these unobserved components. A novelty of our study is that we restrict any measured neighbourhood variation to that caused by movements of students in our sample from one neighbourhood to another. Moreover, the size of our administrative population-wide data and the fact that we observe multiple cohorts means that we can control carefully for unobserved neighbourhood fixed effects, neighbourhood-specific unobserved time trends and school-by-cohort specific shocks. The rest of this section sets out our empirical model more formally.

Assume that students' outcomes depend linearly on the characteristics of peers in the neighbourhood, other neighbourhood infrastructures and individual characteristics, to give a reduced-form relationship:

$$y_{insct} = z_{nct}\beta + \mathbf{x}'_i \gamma + \mathbf{x}'_i \delta t + \varepsilon_{insct} \tag{1.1}$$

where y_{insct} denotes the outcome of student *i* living in neighbourhood *n,* attending school *s*, belonging to birth cohort *c* and measured at grade or age *t*.[5] Note that school grade is equivalent to age, since there is no grade repetition in England. In the empirical analysis, we look at academic outcomes, including test outcomes from grade 6 to grade 11 and some behavioural outcomes (e.g. attitudes to school, drugs use) in grades 9 and 11, as discussed in Section 4. We observe students' test scores at grades 6, 9 and 11 (ages 11, 14 and 16), and attended school and place of residence for these grades as well as all those in between. In this specification, z_{nct} is a variable measuring *neighbour-peer* composition, e.g. mean prior achievements of peers in the neighbourhood or the proportion from low-income families. The definition of these neighbour-peers is set out in Sections 3.3 and 4.3 below. The vector $\mathbf{x}_i$ contains time-fixed predetermined observable student characteristics, which we allow to have a time-trending effect captured by δt. Furthermore, we assume that the error term has the following components:

$$\varepsilon_{insct} = \alpha_i + \phi_n + \xi_n t + \vartheta_{sct} + e_{insct} \tag{1.2}$$

where α_i represents an unobserved individual level fixed effect that captures all constant personal and family background characteristics; ϕ_n represents unobserved time-fixed neighbourhood characteristics – such as access to a good public library and other infrastructures – and $\xi_n t$ represents neighbourhood unobserved trending factors – such as gentrification dynamics. Finally, ϑ_{sct} is a school-by-cohort-by-grade specific shock. Among other things, this term is intended to capture variation in school resources, composition and or quality of teaching that are common to students attending the same schools *s* in a given grade – e.g. grade-6 (age-11) – and belonging to the same cohort *c*. Finally, the term e_{insct} is assumed to be uncorrelated with all the right hand side variables. Endogeneity issues arise because the components α_i, ϕ_n, $\xi^i{}_n t$ and ϑ_{sct} in Equation (1.2) are potentially correlated with z_{nct} and $\mathbf{x}_{it}$ in Equation (1.1).

In order to eliminate some of the unobserved components that could jointly determine neighbour-peer composition and students' outcomes, we exploit the fact that we observe students as they progress from primary through secondary education, and know their outcomes and the composition of the neighbourhood where they live at different school grades (ages). We can therefore take within-student differences between two grades and estimate the following equation:

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[5] Note we are not interested here in the distinction between endogenous and contextual peer effects (Manski 1993), and focus only on the reduced form relationship between neighbour-peer characteristics and student outcomes.

$$\left(y_{insc1} - y_{insc0}\right) = \left(z_{nc1} - z_{nc0}\right)\beta + \mathbf{x}'_i\,\delta + (\varepsilon_{insc1} - \varepsilon_{insct0}) \qquad (2.1)$$

Where the subscripts *t=0* and *t=1* identify the initial and subsequent grade (e.g. grades 6 and 9), and the exact grade interval varies according to the outcome under consideration. Note that when we estimate this model we restrict our estimation sample to students who *do not move* neighbourhood. This implies that neighbour-peer changes ($z_{nc1} - z_{nc0}$) depend on inflows and outflows of movers who are not in the estimation sample. The within-individual, between-grade differencing for stayers reduces the error term to:

$$(\varepsilon_{insc1} - \varepsilon_{insct0}) = \xi_n + (\vartheta_{sc1} - \vartheta_{sc0}) + \nu_{insct} \qquad (2.2)$$

where ν_{insct} is assumed random, and differencing eliminates both the individual (α_i) and the neighbourhood (ϕ_n) unobserved components that are fixed over time, including unobserved ability, family background and other forces driving sorting of families across different neighbourhoods. One caveat to this approach is that focussing on stayers could give rise to selectivity issues and bias our estimates of neighbourhood effects. To allay these concerns, in one of our robustness checks we include movers and stayers, and assign to movers the changes in the neighbour-peer quality they would have experienced had they not moved. In this second set-up, our estimates of the neighbourhood effects are more properly interpreted as intention-to-treat effects.

Equation (2.2) shows that this grade-differenced specification does not control for school quality factors that change between grades for a given student. The between-grade school quality change term $\vartheta_{sc1} - \vartheta_{sc0}$ in Equation (2.2) is likely to be non-zero, especially because students change schools over the grade intervals that we study. In particular, students go through a compulsory school change from primary to secondary school, between grades 6 and 9. They may also choose to change secondary schools between grades 9 and 11, and even if they do not, their secondary school quality could change because of new leadership, changes in the teaching body or variation in school resources. This possibility poses a threat to our identification strategy because school quality changes for students in neighbourhood *n* might influence the inflow and outflow of students, as well as the characteristics of in/out-migrants into neighbourhood *n*, which would in turn affect changes in neighbourhood peer composition, $z_{nc1} - z_{nc0}$. Differencing between cohorts is unlikely to eliminate these school quality effects because they are not necessarily fixed across cohorts.[6] In our specifications we therefore control for secondary-school-by-cohort effects or secondary-by-primary-school-by-cohort effects (effectively school-by-grade-by-cohort effects). We can further control for more general unobserved neighbourhood-specific time trends ξ_n – such as regeneration, gentrification or decline of some areas relative to others – by differencing from neighbourhood means across cohorts *c*.[7]

Our identifying assumption in these models is that the remaining shocks to student outcomes (after eliminating student fixed effects, neighbourhood fixed effects, school-by-cohort effects and/or

[6] Note also that the school effects may vary by cohort within the same neighbourhood because different cohorts in the same neighbourhood attend a different mix of schools.

[7] Note that if we want to allow for both neighbourhood trends and school-by-cohort fixed effects in our specifications, we need to implement a multi-way fixed effects estimator. To do so, we use the Stata's routine `felsdvreg`.

neighbourhood trends) are idiosyncratic and uncorrelated with the changes in neighbourhood composition experienced by student *i* as he/she stays in the residential neighbourhood between grades *t=0* and *t=1*. Our results include a set of balancing regressions that support the empirical validity of this assumption. These show that changes in neighbour-peer composition are not related to time-fixed neighbourhood characteristics or time-fixed average characteristics of the students living in the neighbourhood, even before we allow for neighbourhood unobserved trends or school-by-cohort effects. This lends credibility to our identification strategy.

3.2. Distinguishing neighbourhood from school peer effects

In England, there is not a one-to-one link between neighbourhood and school attended, but students in a given neighbourhood attend a mixed group of local schools, their choices being influenced by travel costs and school admissions policies that tend to prioritise local residents (see Section 4.1). On average, students in the same age-group and living in the same small neighbourhood (hosting five such students) attend two to three different secondary schools. Therefore, we can separately identify the effect of changes in neighbourhood peer composition for neighbours who attend the same secondary school, and for those who do not. More formally, we can estimate the following model that partitions neighbourhood peers into two groups, those that go to the same secondary school (*same*) as student *i*, and those that attend other secondary schools (*other*):

$$\left(y_{insc1} - y_{insc0}\right) = \left(z_{nc1} - z_{nc0}\right)^{same} \beta + \left(z_{nc1} - z_{nc0}\right)^{other} \gamma + \mathbf{x}'_i \delta + (\varepsilon_{insc1} - \varepsilon_{insct0}) \qquad (3)$$

Most variables in Equation (3) were defined above. The variable $\left(z_{nc1} - z_{nc0}\right)^{same}$ refers to changes in neighbour-peer composition driven by the mobility of peers who attend the same school as *i* at grade *t=1* (e.g. at grade 9 at secondary school). These students are therefore peers *both* in the neighbourhood and at secondary school. Note however that schools are attended by students from a large number of residential areas: in our sample, on average secondary schools attract students from sixty different neighbourhoods. This implies that same-neighbourhood-same-school peers are only a small fraction of the peers that students interact with at school. On the other hand, the variable $\left(z_{nc1} - z_{nc0}\right)^{other}$ captures changes in the neighbour-peer composition that are driven by neighbourhood peers who *do not* attend the same school as *i*.

Any difference between the coefficients β and γ in Equation (3) will shed light on the relative contribution of school and neighbourhood peers. More importantly, whereas peer effects (β) among neighbouring students who attend the same school might pick up interactions among students in schools, peer effects among neighbouring students who go to different schools (γ) should capture a 'pure' neighbourhood effect. As before, we can difference Equation (3) within neighbourhoods, across cohorts to eliminate neighbourhood trends, and can control for school-by-cohort fixed effects.[8]

3.3. Defining neighbourhood geography

Research on the effects of neighbourhood composition shares the empirical issues that the literature on peer effects at school has had to face in terms of defining group membership and measuring peers' characteristics, but has the additional complication of having to define the operational scale of the neighbourhood. While there is some discussion of whether the effects of social interactions should be measured at the grade or class level in the peer effects literature (see Ammermueller and Pischke, 2009), there are no similar natural boundaries such as the school or the classroom that define the area of interest in the case of neighbourhoods. Consequently, what has been used to measure neighbourhood effects has varied greatly with respect to geographical size. Goux and Maurin (2007) argue that using large neighbourhood definitions – i.e. US Census tracts containing on average 4000 people – leads to an underestimate of interaction effects. However, over-aggregation on its own will not necessarily attenuate regression estimates of neighbourhood effects since any reduction in the covariance between mean neighbours' characteristics and individual outcomes might be offset by a reduction in the variance of average neighbours' characteristics.

Whether or not the level of aggregation matters in practice is an empirical question. We take advantage of the detail and coverage of our population-wide data to experiment with alternative geographical definitions, starting from a very small scale unit – Output Areas (OA) from the 2001 British Census – which contains 125 households on average and approximately five students in the same age-group (e.g. 6th grade/age-11 students). Since our identification approach relies on neighbourhood fixed effects to control for unobserved neighbourhood factors, a small-scale neighbourhood definition minimises the risk of endogeneity of neighbourhood quality (that is, it is less likely that there are unobserved neighbourhood changes over time within-streets, than within-regions). Nevertheless, we experiment with larger geographical areas based on this underlying OA-geography. This allows us to tackle the problem of defining a suitable spatial unit in neighbourhood research in a highly flexible way.

Another advantage of our data is that we observe the population of English school children and can measure neighbour-peer composition using students in a variety of school grades[9]. Since we are interested in peer effects in the neighbourhood, we argue that these neighbour-peer variables should be constructed aggregating the characteristics of students of similar age. This is motivated by the idea that students of similar age are more likely to interact and/or be indirectly influenced by children of their own age. For this reason, in the majority of our paper we construct neighbour-peer variables using individual level data from students who are either of the same school grade (i.e. grade 6/age 11 at the beginning of our observation

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[8] School-by-cohort fixed effects can still be controlled for in Equation (3) because students living in the same area attend a number of different schools and schools attract students from a large number of different neighbourhoods so that the terms $\left(z_{nc1}-z_{nc0}\right)^{same}$ and $\left(z_{nc1}-z_{nc0}\right)^{other}$ in Equation (3) are not collinear with the term $\left(\vartheta_{sc1}-\vartheta_{sc0}\right)$.

[9] Our dataset is a census of multiple cohorts of all children in state education in England. No comparable information is available for the private sector, which has a share of about 7%.

window) or one year younger/older (grade 5/age 10 and grade 7/age 12). However, we perform a number of checks using different grade-bands, for example by including only students in the same school grade. Note that the neighbour-peer variables are constructed from information on students' characteristics that pre-date the first period of our analysis, using a balanced panel of students with non-missing data in every year of the census. This implies that changes over time in neighbour-peer composition occur only when students within our sample move across neighbourhoods, and not when students drop out/come into our sample, or when their characteristics change.

The complex data that we use in order to pursue this analysis and the exact definition of our neighbour-peer variables are described in the next section alongside the English institutional background.

4. Institutional Context and Data Setup

4.1. The English school system

Compulsory education in England is organized into five stages referred to as Key Stages (KS). In the primary phase, students enter school at grade 1 (age 4-5) in the Foundation Stage, then move on to KS1, spanning grades 1-2 (ages 5-7). At grade 3 (age 7-8), students move to KS2, sometimes – but not usually – with a change of school. At the end of KS2, in grade 6 (age 10-11), children leave the primary phase and go on to secondary school, where they progress through KS3, from grade 7 to 9, and KS4, from grade 10 to 11 (age 15-16), which marks the end of compulsory schooling. The vast majority of students change schools on transition from primary to secondary education between grades 6 and 7.

Students are assessed in standard national tests at the end of each Key Stage, generally in May, and progress through the phases is measured in terms of Key Stage Levels.[10] KS1 assessments test knowledge in English (Reading and Writing) and Mathematics only and performance is recorded using a point system. On the other hand, at both KS2 and KS3 students are tested in three core subjects, namely Mathematics, Science and English, and attainments are recorded in terms of the raw test scores. Finally, at the end of KS4 students are tested again in English, Mathematics and Science (and in another varying number of subjects of their choice) and overall performance is measured using point system (similar to a GPA), which ranges between 0 and 8.[11]

Admission to both primary and secondary schools is guided by the principle of parental choice and students can apply to a number of different schools. Various criteria can be used by over-subscribed schools to prioritise applicants, but preference is usually given first to children with special educational needs, next to children with siblings in the school and to children who live closest. For Faith schools, regular attendance at local designated churches or other expressions of religious commitment is foremost. Because of these criteria – alongside the constraints of travel costs – residential choice and school choice decisions are linked (see some related evidence in Gibbons et al, 2008 and 2009, and in Allen et al., 2010). Even so,

[10] KS3 assessments were stopped in 2009, which marks the end of our data period.
[11] Details on the weighting procedures are available from the Department for Education (formerly Department for Children, Schools and Families) and the Qualifications and Curriculum Authority.

most households will have a choice of more than one school available from where they live. On average students in the same-age bracket (e.g. age-14 students) living in the same Output Area (OA) – i.e. our smallest proxy for neighbourhoods sampling on average five such students – attend two to three different secondary schools every year, and each secondary school on average samples students from around sixty different OAs (out of more than 160,000 in England). As already mentioned, this feature of the institutional context allows us to measure changes in neighbourhood peer composition for students who attend the same or a different school. If school attendance was more tightly linked to residential location, we would not be able to discriminate between these two groups.

4.2. Main data source and grade 6 (KS2) to grade 9 (KS3) tests

To estimate the empirical models specified in Section 3, we draw our data from the English National Student Database (NPD). This dataset is a population-wide census of students maintained by the Department for Education and holding records on KS1, KS2, KS3 and KS4 test scores and schools attended for every state-school student from 1996 to the present day. Since 2002 the database has been integrated with a Pupil Level Annual School Census (PLASC, carried out in January), which holds records on students' background characteristics such as age, gender, ethnicity, special education needs and eligibility for free school meals. The latter is a fairly good proxy for low income, since all families who are on unemployment and low-income state benefits are entitled to free school meals (Hobbs and Vignoles, 2009). PLASC also records the home postcode of each student on an annual basis. A postcode typically corresponds to 15 contiguous housing units on one side of a street, and allows us to assign students to common residential neighbourhoods and to link them to other sources of geographical data. In particular, we use data from PLASC to map every student's postcode into the corresponding Census Output Area (OA, described above).

The main focus of our analysis will be the period spanning grade 6 (age 11, end of KS2) to grade 9 (age 14, end of KS3), but we report results for other time periods and outcomes (described later). The main advantage of concentrating on this interval is that the data provides comparable measures of performance in English, Mathematics and Science at grade 6 (KS2) and grade 9 (KS3). We exploit this feature to construct measures of students' test-score value-added which allow us to estimate the changes-in-changes specification spelled out in Section 3.1. Operationally, we average each student's performance at KS2 and KS3 across the three subjects, then convert these means into percentiles of the cohort-specific national distribution, and finally create KS2-to-KS3 value-added by subtracting age-11 from age-14 percentiles. Note that we restrict our attention to students in schools that do not select students by academic ability (i.e. comprehensive schools).

Given the time span of the NPD-PLASC integrated dataset and our data requirements, we can track several birth cohorts of students as they progress through education. For our main analysis, we retain students in the four 'central' cohorts, namely students in grade 6 (taking KS2 tests) in academic years 2001/2002, 2002/2003, 2003/2004 and 2004/2005, who move on to grade 9 (KS3 tests) in the years 2004/2005, 2005/2006, 2006/2007 and 2007/2008. We use other cohorts to construct the neighbour-peer variables as described in Section 4.3 below. Finally, we concentrate on students who live in the same OA over the period covering grade 6 (age 11) to grade 9 (age 14), which we label as 'stayers' (we will address issues of selectivity caused by focussing on the stayers in our robustness checks). After applying these restrictions, we obtain a balanced panel of approximately 1.3 million students spread over four cohorts.

4.3. Data on neighbour-peer composition

Using NPD/PLASC, we construct measures of neighbour-peer composition based on neighbourhood aggregates of student characteristics. These neighbour-peer characteristics are: *(i)* Average grade 3 (KS1) score in English (Reading and Writing) and Mathematics; *(ii)* Share of students eligible for free school meals (FSM); *(iii)* Share of students with special education needs (SEN); *(iv)* Fraction of males. We use KS1 scores to proxy students' academic ability at the earliest stages of primary education, and FSM eligibility as an indicator for low family income. Further, SEN is used as a proxy for learning difficulties and disabilities. The fraction of SEN neighbour-peers is based on students deemed by the school to have special educational needs, which includes those who have official SEN statements from their local education authority. FSM and SEN status are based on students' information in the first year they appear in the data. Finally, we consider the share of males since this has been highlighted as important in previous research on peer effects (see Hoxby, 2000 and Lavy and Schlosser, 2007).[12] To construct these neighbour-peer aggregates, we use individual level data from all students who live in the same OA and are either in the same grade (i.e. grade 6, age 11 at the beginning of our observation window) or in the school grade above or below (grade 5 and grade 7).[13] We keep OA neighbourhoods in our estimation sample only if there are at least 5 students in the OA in these grade/age categories. Moreover, we keep a balanced panel of students with non-missing information in all years, so that neighbourhood quality changes are driven by the same students moving in and out of the area, and not by students joining in and dropping out of our sample. Given the quality of our data, this restriction amounts to excluding approximately 2% of the initial sample.

Figure 1 provides a graphical representation of the time window in the data and the construction of the neighbourhood-peer groups. For example, Cohort 1 is the cohort of children in grade 6 and taking KS2 in 2002, who go on to secondary school in 2003 and take their KS3 in grade 9 in 2005. Neighbour-peer composition in 2002 for Cohort 1 is calculated from students in the OA who are in Cohort 1, plus those in grades 5 and 7. Neighbour composition is calculated in 2005 from Cohort 1 and grades 8 and 9.

In order to check the validity of our basic neighbourhood definition, we construct two alternatives based on: *(i)* students in the same OA and the same grade only; and *(ii)* students in the same and adjacent grades, but living in a set of contiguous OAs. Specifically, for *(ii)* we create neighbourhoods that include students' own OA plus all contiguous OAs. These extended neighbourhoods include on average 6 to 7 OAs, and approximately 80 students.[14]

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[12] We do not observe immigrant status and so cannot perform an analysis similar to Edin et al. (2003) and (2010), or Gould et al. (2011a).

[13] We also compute these proxies separately for students who attend/do not attend the same secondary school at age 14 in order to estimate the specification detailed in Equation (3).

[14] This computationally intense task is implemented in GeoDA using rook contiguity.

4.4. Data on behaviour from the LSYPE

One limitation of the administrative data in the integrated PLASC/NPD is that the only student outcome variables relate to academic test scores. However, previous research in the field (Kling et al., 2005 and 2007) suggests that behavioural outcomes – e.g. involvement in criminal activities, educational aspirations, self-reported measures of health and proxies for life-satisfaction and wellbeing – are more likely to be affected (sometimes perversely) by neighbours, even in contexts where test scores are not influenced (Sanbonmatsu et al., 2006). In order to investigate this issue, we make use of the Longitudinal Study of Young People in England (LSYPE), which sampled approximately 14,000 students in grade 9 (aged 14) in 2004 (one cohort only) in 600 schools, and followed them as they progressed through their secondary education up to grade 11 (age 16) and beyond. The LSYPE surveyed students on a number of aspects about their life at school, at home and in their neighbourhood, and contains a number of questions related to behavioural outcomes. These questions were asked in a confidential environment to encourage students to answer truthfully. Most of the questions involved a binary answer of the type "Yes/No". We follow Katz et al. (2005) and recombine some of the original variables to obtain four behavioural outcomes. Specifically, we construct the following four proxies: *(i)* 'Positive school attitude' which is obtained as 'School is a worth going (Yes=1; No=0)' *plus* 'Planning to stay on after compulsory schooling (Yes=1; No=0)' *minus* 'School is a waste of time (Yes=1; No=0)'; *(ii)* 'Playing truant' which is the binary outcome from the question 'Did you play truant in the past 12 months (Yes=1; No=0)'; *(iii)* 'Substance use' which is obtained as 'Did you ever smoke cigarettes (Yes=1; No=0)' *plus* 'Did you ever have proper alcoholic drinks (Yes=1; No=0)' *plus* 'Did you ever try cannabis (Yes=1; No=0)'; and *(iv)* 'Anti-social behaviour' which is obtained as 'Did you put graffiti on walls last year (Yes=1; No=0)' *plus* 'Did you vandalise public property last year (Yes=1; No=0)' *plus* 'Did you shoplift last year (Yes=1; No=0)' *plus* 'Did you take part in fighting or public disturbance last year (Yes=1; No=0)'.

The survey also contains precise information about students' place of residence, which means we can merge into this data the neighbour-peer characteristics that we have constructed using the population of students in the PLASC/NPD. Given the age of the students covered by the LSYPE, we consider the effect of neighbourhood changes on outcomes between grades 9 and 11, and for the reasons highlighted in Section **Error! Reference source not found.**, we construct neighbour-peer variables using students in the same OA and grade.[15] Furthermore, grade 3/KS1 test scores for this cohort are not available, so we use mean KS2 test scores of neighbour-peers as a measure of neighbour prior academic abilities.

5. Main Results on Test Scores

5.1. Summary statistics

Descriptive statistics for the main variables for the grade 6/KS2 to grade 9/KS3 dataset are provided in Table 1. Panel A presents summary statistics for the characteristics of the stayers. The KS2 and KS3 scores

[15] Note that we cannot construct measures of the neighbourhood 'quality' by aggregating the characteristics of the LSYPE students since we have too few LSYPE students in each OA neighbourhood.

are percentiles in the population in our database. The KS2 and KS3 percentiles average around 50, with a standard deviation of about 25 points, and mean value-added on 1.1. Note that mean value-added is not centred on zero, and the standard deviations of KS2 and KS3 percentiles are slightly smaller than theoretically expected, because the percentiles are constructed before: *(i)* dropping students with some missing observations (approximately 2% of the initial sample); *(ii)* disregarding students in small neighbourhood (less than 5 students in the OA in the same grade), and *(iii)* considering only students who do not change neighbourhood between grades 6 and 9 (the stayers). We use figures from this table to standardize all the results in the regression analysis that follows. About 15 percent of the students are eligible for free school meals (FSM), 21 percent have special educational needs (SEN) and 50 percent are male. Average secondary school size is around 1080 students, and the rates of annual inward and outward neighbourhood mobility are similar (they are based on mobility within a balanced panel) and close to 8 percent. Our estimation sample (which excludes movers and students in the smallest neighbourhoods) is representative of the population as a whole (see Appendix Table 1).

Panel B of Table 1 presents the means and standard deviations (unweighted) of the neighbour-peer characteristics and their changes between grades 6 and 9 (age-11/KS2 to age-14/KS3). KS1 test scores at grade 2 are measured in points (not percentiles), and a score of 15 is in line with the national average. By construction, from our balanced panel, the levels of the shares of FSM, SEN and male students are very similar to those of the underlying population of students (see Panel A) and none of the neighbour-peer characteristic means changes much between grades (any changes are due to the fact that the statistics report neighbour-group means and individuals are changing group membership). Our neighbourhoods sample on average around 5 students in the same grade, and 14 students in the same or adjacent grades. This means that relative to most of the previous research in the field, we focus on small groups of neighbour-peers.

The most important point to note from Table 1 is the amount of variation we have in our neighbour-peer variables once we take differences to eliminate individual and neighbourhood fixed effects. Looking at the figures, we see that the standard deviation of KS1 scores is 1.76, while the change in this variable between grades 6 and 9 has a standard deviation just over 0.86. This suggests that 24% of the variance in the average KS1 scores is within-OA over time. The corresponding percentages for the shares of FSM, SEN and male students in the neighbourhood are 16%, 31% and 41%, respectively. Figures 2a and 2b illustrate this point further by plotting the distributions of the neighbourhood mean variables in: *(i)* levels (top left panels), *(ii)* between-grade differences (top right panels), *(iii)* between-grade differences, after controlling for primary-by-secondary-by-cohort school effects (bottom left panels); and *(iv)* between-grade, between-cohort differences netting out OA trends (bottom right panels). All these figures suggest that there is considerable variation over time in neighbour-peer characteristics, from which we can estimate our coefficients of interest, and that controlling for school-by-cohort or OA trends does not lead to a drastic reduction in this variation.

It is worth reiterating that, on average, each year from the end of grade 6 to grade 9, more than 8% of the neighbours move out and are replaced by new neighbours. Over the three years, this means that more than one in four pupils in a student's neighbour-peer reference group is replaced, with a large part of this change occurring between grades 6 and 7, when mobility is highest. This is a substantial change, which we would expect to have real consequences for the group which can influence a student within his or her home neighbourhood.

5.2. Neighbours' characteristics and students' test score: cross sectional and causal linear-in-means estimates

Table 2 presents our main regression results on the association between neighbour-peer characteristics and students' test scores for residential stayers. The table reports *standardised* regression coefficients, with standard errors in parentheses (clustered at the OA level). As discussed in Section 4.3, neighbour-peers are defined as students in the same OA and in the same or adjacent school grades, and we report the effect of: average grade 3 (KS1) point scores (*Panel A*); share of FSM students (*Panel B*); share of students with SEN status (*Panel C*); and share of male students (*Panel D*). Each coefficient is obtained from a separate regression. Some of these neighbour-peer characteristics are highly correlated with one another, but our aim is to look for effects from any one of them – interpreted as an index of neighbour-peer quality – rather than the effect of each characteristic conditional on the other. Columns (1)-(4) present results from regressions that do not include control variables other than cohort dummies and/or other fixed effects as specified at the bottom of the table. Columns (5)-(8) add in control variables for students' own characteristics as described later in this section. The note to the table provides more details.

Column (1) shows the cross-sectional association between neighbour-peer characteristics and students' own KS3 test scores. All four characteristics are strongly and significantly associated with students' KS3 scores. A one standard deviation increase in KS1 scores is associated with a 0.3 standard deviation increase in KS3, while a one standard deviation increase in FSM or SEN students is linked to a 0.2-0.3 standard deviation reduction in KS3. The fraction of males has a small positive relation with KS3 scores.

However, these cross-sectional estimates are potentially biased by residential sorting and unobserved individual, school and neighbourhood factors. We first eliminate student and neighbourhood unobserved fixed effects by estimating within-student, between-grade differenced specifications as set out in Equations (2.1)-(2.2). The corresponding results in Column (2) show that the associations between changes in neighbour-peer characteristics and KS2-to-KS3 value-added are driven down almost to zero and only significant in two out of the four panels. The coefficients are up to 100 times smaller than in Column (1). A one standard deviation change in neighbour KS1 scores and in the FSM proportion over the three-year interval is linked to a mere 0.3-0.5% of a standard deviation change in students' test-score progression. Neighbours' SEN and male proportions are no longer significantly associated with students' KS2-to-KS3 value-added, and their estimated effects are close to zero.

As discussed in Section 3, it is still possible that estimates from these models are biased by unobserved school specific factors and neighbourhood trends. In order to control for school specific factors, Columns (3) adds primary-by-secondary-by-cohort fixed effects that absorb cohort-specific shocks to changes in school quality when moving from the primary to the secondary phase. Results from these specifications show that none of the neighbour-peer characteristics are now significantly related to students' KS2-to-KS3 value-added. The loss in significance is not due to a dramatic increase in the standard errors, but to the magnitude of the coefficients shrinking towards zero. This backs the intuition gathered from Figures 2a and 2b that in principle there is sufficient variation to identify significant associations between neighbourhood composition and students' achievements. In order to control for neighbourhood specific time trends, Column (4) adds OA fixed effects in the value-added specification. The results are nearly identical to those in Column (3).[16] As shown in Appendix Table 2, accounting for OA trends only, without school-by-cohort effects, yields virtually identical results.

Columns (5)-(8) repeat the analysis of columns (1)-(4), but add some control variables. These include students' own KS1 scores, FSM and SEN status and gender, plus school size, school type dummies and average rates of inward and outward mobility in the neighbourhood. Comparing Columns (1) and (4) suggests that the cross sectional associations in Column (1) are severely biased by sorting and unobserved student characteristics: adding in the control variables reduces the coefficients substantially (by a factor of three). In contrast, once we eliminate student and neighbourhood fixed effects as in Columns (2) and (6), adding in the control set does not significantly affect our results. The only case where there is a notable change is in the effect of neighbour-peer SEN, which becomes statistically significant (at the 5% level), even though the point estimate is unchanged. The similarity of the results in Columns (2)-(4) with those in Columns (6)-(8) is reassuring as it implies that changes in neighbour-peer composition are not strongly linked to students' background characteristics. This finding lends support to our identification strategy which relies on changes in the treatment variables to be 'as good as random' once we partial out student and neighbourhood fixed effects. The next section presents more formal evidence on this point.

One concern might be that the attenuation in the estimates once we difference the data within-student between-grades is caused by inflation in the noise to signal ratio because of noise in our neighbour-peer variables. Although our proxies are constructed from administrative data on the population of state school children, they may still be noisy measures of the underlying neighbours' attributes that matter for students' achievements (which we cannot observe). This noise could be exacerbated by differencing the data, in particular since there is a high degree of serial correlation in the neighbour-peer characteristics within neighbourhoods. The standard errors in Table 2 suggest this is not the case: the precision is not reduced at all by adding in fixed effects or control variables. To assess this issue more systematically, we perform two robustness checks. First, we use teachers' assessment of students' performance during KS1 to construct instruments for neighbour-peer KS1 test scores on the grounds that the only common components of KS1 test scores and teacher assessments should be related to underlying neighbours' abilities.[17] Instrumental variable (2SLS) regressions confirm that the effect of changes in KS1 test scores of neighbour-peers is not a strong and highly significant predictor of students' KS2-to-KS3 value-added. In our second robustness

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[16] Note that school-by-cohort effects and neighbourhood specific time trends do not capture the same thing because there is not a one-to-one mapping between neighbourhood of residence and school attended. Note also that including primary-by-secondary-by-cohort effects and OA trends proved computationally not feasible, so we replaced the former with secondary-by-cohort effects.

[17] For the students in our sample, KS1 achievement was assessed on the basis of externally moderated written tests, and using the teacher's own assessment based on their experience of the student.

check, we estimate a linear predictor of students' KS2 achievement by regressing students' own KS2 achievements on own KS1 test scores, FSM eligibility, SEN status and gender. The predictions from these regressions are then aggregated across neighbour-peers to create new measures of predicted neighbour-peer KS2 at grade 6 and grade 9. This new composite indicator should be less affected by measurement error in relation to the underlying neighbourhood quality that matters for students' achievements since it is based on the best linear combination of the individual characteristics that predicts KS2 test scores. Using this measure as a proxy for neighbour-peer quality produces similar results to those in Table 2, with no evidence of any significant effect from neighbours on students' achievement. Finally, note that the reduction in the coefficients from Column (2) to (3) and from Column (6) to (7) is not simply due to the inclusion of a large number of fixed effects (around 190,000 primary-by-secondary-by-cohort groups). As shown by the estimates in Appendix Table 2, including only secondary school fixed effects (around 3200 groups) or secondary-by-cohort effects (approximately 12,000 groups) similarly drives our estimates to zero.[18]

In summary, our baseline linear-in-means specifications indicate that the effects of neighbour-peers on student achievement are statistically insignificant and negligibly small. In the following sections, we assess our identifying assumptions and present several extensions and robustness tests. Since controlling for unobserved neighbourhood trends does not affect our main estimates once we have taken into account school-by-cohort effects, the analysis that follows considers only value-added specifications (Columns (2) and (6)) and specifications that further control for school cohort-specific effects (Column (3) and (7)).

6. Assessment and robustness

6.1. Assessing our identification strategy

The validity of our empirical method rests on the assumption that changes in neighbour-peer composition between grades are not related to the unobserved characteristics of students who stay in the neighbourhood, nor to other unobservable attributes of the neighbourhoods. We have shown already that the results of the between-grade within-individual value-added specifications are insensitive to the inclusion of additional individual, school and neighbourhood mobility control variables. This supports the validity of our identifying assumptions. In this section, we tackle this issue more systematically by providing evidence that our treatments are balanced with respect to student and neighbourhood characteristics.

The neighbourhood characteristics that we consider are drawn from the GB 2001 population census at the OA level. Specifically, we consider proportions of: *(i)* households living in socially rented accommodation; *(ii)* owner-occupiers; *(iii)* adults in employment; *(iv)* adults with no qualifications; *(v)* lone parents. Additional characteristics are generated by collapsing student information from the NPD to the OA

[18] As a further robustness check we replaced school fixed effects with school-level characteristics. For example, we replaced primary-by-secondary-by-cohort effects with actual cohort-specific changes in school-level characteristics on transition from primary to secondary school. These included student-to-teacher ratios, fraction of students of White ethnic origin, fractions of students eligible for FSM and with SEN, number of full-time equivalent (FTE) qualified teachers, and numbers of support teachers for ethnic minorities and SEN students. These specifications confirmed that neighbourhood composition is not strongly associated with students' value-added.

level, based on OA of residence at grade 6 (age 11), namely: KS1 test scores, FSM and SEN status and gender, as well as the mean and the standard deviation of students' KS2 test scores. To check the balancing of our treatments, we carry out cross-sectional OA level regressions of these neighbourhood characteristics on the OA-specific changes in neighbour-peer characteristics used in Table 2 (i.e. grade 6-to-9 changes in neighbour-peer KS1 test scores, and FSM, SEN and male proportions).

Standardised coefficients and standard errors from these regressions are reported in Table 3. The top panel shows the association between OA-mean student characteristics and the change in neighbour-peer composition between grades 6 and 9. These regressions have no control variables other than the proportion of students in the neighbourhood from each cohort in our data and the proportions of students represented in different school types.[19] The only significant and meaningful associations are related to the changes in neighbour-peer FSM. The sign of these estimates suggests that neighbourhoods with low KS1, high FSM and high SEN experience increases in fraction of neighbours who are FSM-registered, which would imply *upward* biases in the estimates in Table 2, Columns (2)-(4). However, these associations are very small in magnitude. Moreover, it should be noted that we have only imperfect controls for cohort and school effects in these balancing tests, and that these factors are more effectively controlled for in the specifications in Table 2 which include school-by-cohort effects and neighbourhood trends. In the bottom panel of Table 3 we regress OA-level KS2 statistics and Census variables on the neighbour-peer change variables. These regressions include OA-level averages of the controls added in the specifications of Columns (4) to (8) of Table 2. The intuition for this approach is based on the idea of using Census characteristics and OA KS2 statistics as proxies for *additional* unobservable factors in the regressions of Columns (4)-(8) of Table 2, and testing for their correlation with the changes in neighbour-peer characteristics. The results present a reassuring picture: nearly all the estimated coefficients are very small and insignificant.

Overall, the balancing tests in Table 3 provide no evidence of strong associations between neighbour-peer changes and other neighbourhood characteristics. Assuming the correlation of neighbour-peer changes with observable characteristics provides a guide to the degree of correlation with the unobservables (as argued in Altonji et al., 2005), these findings provide evidence that the near-zero neighbour-peer effect estimates in Table 2 are not biased by student or neighbourhood unobservables.

A sceptical reader could still argue that there might be unobserved shocks, conditional on school-by-cohort effects and neighbourhood trends, which could simultaneously affect children's outcomes and the distribution of the characteristics of in-migrants and out-migrants. This would bias our estimates of the effects of changes in neighbour-peer composition on student outcomes. However, we should note that if families are moving in response to neighbourhood changes which affect student achievements, then our estimates are likely to be upward biased, because neighbourhoods most likely experience an outflow of rich students and an inflow of poor students in response to shocks that have an adverse impact on student achievement (assuming that the neighbourhood factors affecting student achievement are normal goods in

[19] School 'types' include: Community, Voluntary Aided, Voluntary Controlled, Foundation, City Technology College and Academy. The cohort and school-type proportions stand in for the cohort-by-school effects in our main student level regressions, which we are unable to include in the aggregated OA-level regressions.

housing consumption). In other words, our near-zero estimates should be regarded as an upper bound of the effects of neighbourhood composition. Moreover, the available evidence provides little support for the idea that residential migration occurs as a result of neighbourhood shocks of this type. Most residential moves occur for reasons that are largely unconnected with neighbourhood quality, and are based on longer-run considerations.

To shed light on this point, we turn to the British Household Panel Survey (BHPS). The BHPS is a longitudinal survey that follows a representative sample of families in Britain since the early 1990s. The survey tracks residential movers and asks respondents open ended questions about their reasons for moving. These responses are then coded up into the most common categories. Taking a sub-sample of 637 movers that corresponds to households with children for years matching the PLASC/NPD data that we use in our analysis, we find that the main specific reasons for residential moves are: *(a)* size or other physical attributes of the home (22.6% are moves to larger accommodation, while 9.5% relate to other aspects of the home); *(b)* formation and dissolution of partnerships (16%); *(c)* changes of tenure status (7.6% relates to buying a home, while 5.4% is linked to eviction or home repossession); and *(d)* job-related reasons (9.6%). Neighbourhood-specific reasons (i.e. disliking the area, isolation, safety, unfriendliness and noise) are specified by just over 5% of those moving, although there is an ambiguous 16.2% coded as citing 'other' reasons or no reason for moving, and a further 4% citing 'family reasons'. The figures are tabulated in Appendix Table 3. In summary, between 75% and 95% of the moves occur for non-neighbourhood related reasons and none of the reported responses cites neighbourhood changes or education issues.

Therefore, we conclude that there is little reason to believe that our results are biased by neighbourhood shocks that directly affect students' educational achievements and cause changes in neighbour-peer composition.

6.2. Peers at school or peers in the neighbourhood?

In our analysis so far, we have not distinguished between neighbour-peers who attend the same secondary school and those who do not. However, this distinction could be important for at least two reasons. First, children who are at school for a large part of their day may not interact with neighbours, unless they know each other from school already. In this case, neighbour-peers who attend a different school may exert little or no influence on students' outcomes. Second, distinguishing between school and neighbourhood peers is useful for uncovering an uncontaminated neighbourhood level peer effect, net of school peer effects and other school factors that have not otherwise been effectively controlled for in our regressions.

Table 4 presents evidence on this issue by tabulating results obtained from the specifications detailed in Equation (3), and including different levels of fixed effects as we move from Column (1) to Column (3). The sample used to estimate these specifications is slightly smaller than the one used in Table 2 since we drop neighbourhoods in which all students attend the same school, or all students attend different schools. Results in Panel A show that neighbour-peer KS1 has an impact on a student's achievement *only* if these neighbours also attend that student's secondary school. However, this association vanishes as soon as we include secondary-by-cohort or primary-by-secondary-by-cohort effects. Next, results in Panel B, show that

FSM status of neighbour-peers matters irrespective of school attended, with a standardised coefficient of negative 0.003 (s.e. 0.001). However, as soon as we include school-by-cohort effects to control for the school-related residential sorting during the transition between primary and secondary school, the estimated effects shrink and become insignificant. Finally, we find no evidence of neighbour-peer effects when looking at neighbours' SEN-status and gender, irrespective of the school attended.

All in all, this evidence indicates that residential neighbourhood-peer effects are effectively zero, irrespective of whether neighbours attend the same school.

6.3. Robustness checks I: intention-to-treat estimates and other definitions of peers and neighbourhoods

An important issue that we flagged in Sections 3 and 4 is that, by focussing on the sample of students who stay in the same neighbourhood between grades 6 and 9, we might induce some bias due to endogenous sample-selection. To circumvent this problem, we estimate the specification in Equation (2.1) using both stayers and students who move neighbourhood between grades 6 and 9. At grade 9, we assign to these movers the grade-9 characteristics of the neighbourhood in which they lived at grade 6. Stated differently, we assign them to the changes in the neighbourhood quality that they would have experienced had they not moved. Estimates obtained following this approach should be interpreted as intention-to-treat effects. Table 5 presents our results for specifications without (Column (1)) and with (Column (2)) primary-by-secondary-by-cohort effects (both columns include control variables). The new results are almost identical to those reported in Table 2 for stayers only, allaying sample-selection concerns.

As discussed in Section 3.3, there are ambiguities about the correct neighbour-peer group definition. Given we cannot know *a priori* the correct grouping, we experiment in Table 5 with different group definitions as discussed in Section in 4.3. Columns (3) and (4) consider neighbour-peers in the same OA and grade only, whereas Columns (5) and (6) change the neighbourhood definition to include, on average, 6-7 adjacent OAs (on average 80 students). In general, these redefinitions make no substantive difference to the results. In some cases, previously insignificant coefficients become more precise, although all the effects remain very small in magnitude, and most are insignificant once we include school-by-cohort effects. Using aggregates computed over larger residential areas in Column (5) *increases* the precision and the size of our estimates. However, including school-by-cohort effects as in Column (6) brings our estimates close to zero and insignificant (with the exception of the changes in the share of males). This pattern might be explained by the fact that changes in larger neighbourhood aggregates are more likely to be contaminated by omitted time-varying neighbourhood factors – such as changes to neighbourhood infrastructure or household mobility dictated by school quality and access – than for smaller geographical units. This lends support to our claim that small scale geographical fixed effects minimise the risk from endogenous changes in neighbourhood quality.

We also experimented with alternative neighbour-peer variables based on the characteristics of the adult population in the neighbourhood (rather than students of similar ages). This type of information is not readily available from the education datasets used so far, but can be gathered using time-varying information from the Department for Work and Pension (DWP). From these data we matched the students in our main dataset to neighbourhood information on: *(i)* the number of working-age people claiming the 'Job Seeker Allowance' (JSA, i.e. unemployment benefits); *(ii)* the number of people aged 16-25 claiming JSA; and *(iii)* the number of lone parents on income support (a proxy for very low income usually among young unmarried mothers). Analogous regressions to Table 2 using these neighbourhood indicators give coefficients that are near zero and insignificant, implying no neighbourhood effects related to the adult composition of the neighbourhood.

6.4. Robustness checks II: timing issues and alternative time-windows

So far, we have investigated whether value-added between grades 6 and 9 is related to neighbourhood changes over the same period. However, students' educational progress could respond more to changes that happen early during this period (e.g. grades 6 to 7), or later on and just before students' KS3 exams (e.g. grades 8 to 9). We therefore investigate whether the three different grade-on-grade changes in neighbourhood composition experienced by students as they progress from grade 6 to grade 9 (i.e. grade 6 to 7, grade 7 to 8 and grade 8 to 9) have heterogeneous effects on their outcomes (results are not tabulated). Again we find that changes in neighbourhood composition are not significantly related to changes in students' test scores. The only exception is a small *negative* effect of neighbour-peers' average KS1 changes between grades 6 and 7, which is borderline significant with a p-value of 0.054.[20]

To further address timing issues, we consider students' attainments at grade 11 (KS4) and analyse whether students' value-added between grade 6 (KS2) and grade 11 (KS4), and between grade 9 (KS3) and grade 11 (KS4) is affected by the corresponding changes in neighbour-peer characteristics. The data used to estimate these models is discussed in Appendix A and a selection of our results is presented in Appendix Table 4. The results based on changes up to 5 years tell the same story: there is no evidence that neighbourhood change affects the gains in achievement of students. [21]

7. Heterogeneity, non-linearities and behavioural outcomes

Our results so far come from linear-in-means specifications and show that on average, changes in neighbour-peer composition do not influence students' test score gains. However, this headline result might mask significant heterogeneity and non-linearity along a number of dimensions. Investigating these issues is particularly relevant because, as discussed in the introduction, arguments in favour or against policies that mix neighbourhoods with the aim of improving overall students' outcomes rest on strong assumptions on the second order partial derivatives of the functions describing these neighbourhood effects as discussed in the Introduction. In this section, we exploit the size and full coverage of our census data tracking four

[20] We further investigated whether changes over three grades (i.e. grade 6 to 9) have differential impact from changes over two grades (e.g. grade 6 to 8) and one grade (e.g. grade 6 to 7), but failed to find any significant pattern.

[21] In order to allow for time lags in the process by which neighbourhood changes can influence students, we also investigated whether grades 9-to-11 (KS3-to-KS4) value-added is affected by changes in the neighbourhood composition between grades 6 and 9 (i.e. over the KS2 to KS3 phase), or between grades 8 and 10 (i.e. one-year lag with respect to the Key Stage tests). We also looked at students' value-added in primary school (results not tabulated), replicating the analysis in Table 2 for the grade 2 to grade 6 (KS1 to KS2) phase. Again, we found no significant neighbour-peer effects.

cohorts of secondary school student in England to investigate heterogeneity, complementarities and non-linearities in neighbour-peer effects.

Table 6 presents the first results on this issue. Columns (1a)-(4b) explore heterogeneity in students' response to neighbourhood changes according to whether the student: *(i)* has KS1 test scores above/below the sample median; *(ii)* is eligible for FSM; *(iii)* has SEN status; *(iv)* is male or female. Columns (5a)-(8b) of Table 6 present heterogeneity by neighbourhood type: *(i)* above/below median student numbers; *(ii)* above/below median population density; *(iii)* above/below median housing over-crowding[22]; and *(iv)* a percentage of social housing tenants above/below 75%. Out of the sixty-four estimates, only six are significant at conventional levels. These show that: a larger fraction of SEN students negatively affects students with high KS1 achievements; a larger fraction of FSM students lowers non-SEN and female students' test-scores; a larger fraction of boys improves other boys' achievements; and a larger fraction of neighbours with FSM and SEN status has a significantly adverse effect on the value-added of students living high density neighbourhoods. Most of these effects are only significant at the 5% level and the effect sizes are tiny. Importantly, the first two findings coupled with the remaining evidence emerging from the table, suggest that neighbourhood mixing might *decrease* overall achievements: while high-KS1 students and non-SEN students marginally lose out from interacting with more SEN and FSM neighbour-peers, students who are eligible for free meals or have an SEN status are not significantly and positively affected by neighbour-peers with higher average KS1 grades or lower shares of SEN and FSME students in the neighbourhood. Similarly, female students marginally lose out from being surrounded by a larger share of FSM-eligible neighbours, but FSM pupils do not benefit from having a smaller share of male neighbour-peers. The results also suggest neighbourhood effects are more pronounced for students in urban areas, although we find no evidence of this in relation to overcrowding specifically (Column 7b) or concentrated social housing (Column 8b). [23]

A number of checks in relation to nonlinearities and threshold effects similarly failed to find anything notable, and the findings are not tabulated for space reasons but are available upon requests. We added quadratic and cubic polynomials of the neighbourhood composition variables, or quadratic and cubic powers of the changes in our neighbour-peer variables into our regressions. We also allowed positive and negative neighbourhood changes to cause asymmetric effects. We found no evidence of any large and/or significant non-linearities. We have also investigated whether there are any distinctive effects from the very highest and the very lowest ability neighbours. In the context of English secondary schools, Lavy et al. (2012) find significant and sizeable negative peer effects from 'bad' peers at the very bottom of the ability distribution. Conversely, the authors find heterogeneous peer effects from very 'good' peers at the top of the ability distribution, with girls significantly benefiting from the presence of academically bright peers and boys marginally losing out. In order to replicate this design in our context, we identify pupils who are in the top and bottom 10% of KS1 cohort-specific national distribution. We then investigate whether

[22] This proxy is based on the Census definition which identifies households in over-crowded housing if more than one person occupies a room (excluding bathrooms).
[23] We also looked for potential heterogeneity in our estimates by separately considering the ten biggest cities versus the rest of England, and London versus the rest of England. However, we failed to find any significant pattern.

changes in the share of top/bottom neighbour-peers affect students' test-score value-added between grades 6 and 9. Even in this case, we find nothing to suggest that changes in the neighbourhood composition affect students' educational attainment. This is so, irrespective of whether we pool all students, or separately study the effect of very bright and very weak neighbours on boy/girls, FSM/non-FSM student, and SEN/non-SEN pupils.

Figure 3 presents the highlights of related results, where we look at the interaction between neighbourhood change in KS1 and own KS1 decile. This graphical analysis is in the spirit of Hoxby and Weingarth's (2005) analysis for school peer effects. The plots show the estimated effect of changes in neighbourhood KS1 – either mean neighbour-peer KS1, or the changes in the percentage of neighbours' with KS1 scores in the top and bottom deciles, against students' own KS1 score decile. These graphs are constructed from regressions of KS2 to KS3 test score gains on include all three of these indicators of neighbour-peer composition, interacted with dummies for students' own KS1 decile. The solid line is the standardized effect size and the dotted line the 95% confidence interval. With some imagination, we can see a weak upward trend in response to an increase in the proportion of top decile KS1 students and a weak downward trend in response to the proportion of bottom decile KS1 neighbours. These results imply some vague positive complementarities between high achieving neighbour-peers and high achieving students and some vague negative interactions between low achieving neighbour-peers and low achieving students. Both imply some gains from segregation, but the effect sizes are miniscule and nowhere near statistically significant. The mean neighbour-peer KS1 score (conditional on the percentage in the top and bottom deciles) has no systematic pattern across the distribution of student KS1.

Overall this more detailed analysis does not reveal any patterns that were not evident in the linear-in-means estimates. From all these results, there is little evidence of significant neighbour-peer effects or complementarities which would justify mixing neighbourhoods as a policy to improve overall student achievements, nor any sign of 'bad apple' neighbour-peer effects from the lowest achievers.

8. Neighbourhood characteristics and behavioural outcomes: evidence from the LSYPE

The major limitations of the integrated PLASC/NPD data used so far is that the only useful outcome variables relate to academic test scores. In order to consider potentially more interesting effects of neighbour-peer composition on behaviour, we use information collected in the Longitudinal Survey of Young People in England (LSYPE), linked to the NPD-based neighbour-peer variables used in the analysis so far. Given the time-window covered by the LSYPE, we can only consider the effect of neighbourhood changes on outcomes between grade 9 and grade 11. Moreover, age-7 test scores for this cohort are not available, so we aggregate the levels of the KS2 test scores of neighbouring students to proxy for prior academic ability. We report the results from our investigation in Table 7. Since previous evidence in the literature has shown a significant degree of heterogeneity along the gender dimension, we report estimates from separate regressions for boys and girls.[24] All models include the standard set of controls and secondary school fixed effects. The construction of the behavioural outcome variables is discussed in Section 4.4. Descriptive statistics for the LSYPE sample are provided in Appendix Table 5, both for the behavioural variables discussed above and for the student and neighbour-peer characteristics. All in all, these suggest that despite the fact that this sample is much smaller than our previous data, it is still representative of the national population and displays enough variation in the variables of interest.

Columns (1) and (2) tabulate the relation between neighbourhood changes and the composite variable ‘Positive school attitude' for boys and girls, respectively. Starting from the top, we see that an improvement in grade 7 (KS2) achievements of neighbour-peers positively affects students’ attitudes towards education, and that this effect is significant and sizeable for boys: a one standard deviation change in the treatment corresponds to a 3.6% of a standard deviation change in the dependent variable. Symmetrically, we find that a larger share in the fraction of neighbours with learning difficulties and poor achievements (as captured by SEN status; see Panel C) negatively affects views about schooling, but this effect is more sizeable and significant for girls. In this case, a one standard deviation increase in the treatment would negatively affect female students’ attitudes towards education by 6.4% of a standard deviation. On the other hand, neither the fraction of students in the neighbourhood who are eligible for FSM nor the share of males affects other students’ views of education. The four central columns of the table investigate the relation between neighbour-peer composition and students’ absences from school (‘Playing Truant’; see Columns (3) and (4)) and students’ use of substances (this proxy includes smoking, drinking and using cannabis; see Columns (5) and (6)). None of the associations presented in the table is significant at conventional levels, and often the signs of these relations are the opposite of what one would expect. Finally, Columns (7) and (8) concentrate on the variable ‘anti-social behaviour’, which captures whether students got involved in putting graffiti on walls, vandalising property, shoplifting, and whether they took part in fighting or public disturbance. Our results show that, while neighbourhood composition in terms of KS2 achievements, share of males and proportion of SEN students does not significantly affect these behavioural outcomes, an interesting pattern emerges when looking at the proportion of neighbours from poor family background (FSM; see Panel B). A one standard deviation change in this treatment significantly increases male students’ involvement in anti-social behaviour by 5% of a standard deviation, but this change would not affect young girls’ behaviour.

To further explore these issues, we study whether the effects of neighbours’ characteristics on boys’ and girls’ behavioural outcomes differ according to peers’ gender. The results show that male peers’ FSM eligibility has a larger effect than female peers’ FSM status on male students’ involvement in anti-social behaviour, although this difference is not statistically significant. These heterogeneous effects for boys and girls are not surprising. Kling et al. (2005) and (2007) document similarly heterogeneous effects for male and female youths re-assigned to better neighbourhoods by the MTO experiment. More broadly, a growing

[24] Given the much smaller sample covered by the LSYPE, we are unable to convincingly split our results for FSME/non-FSME and SEN/non-SEN students. However, some preliminary analysis showed little heterogeneity in the effect of neighbourhood composition on behavioural outcomes along these dimensions.

body of research shows that boys and girls respond differently to education-related interventions. Amongst others, Anderson (2008) finds that three well-known early childhood interventions (namely, Abecedarian, Perry and the Early Training Project) had substantial short- and long-term effects on girls, but no effect on boys, while Lavy and Schlosser (2011) and Lavy et al. (2012) find that peer quality in secondary schools affects boys and girls differently. Finally, Angrist and Lavy (2009) and Angrist et al. (2009) show a consistent pattern of stronger female response to financial incentives in education in a variety of settings.

In conclusion, and considering both the small number of students sampled by the LSYPE and the fact that we can only look at outcomes between grades 9 and 11, the results in Table 7 provide some support for the notion the neighbour-peers can affect teenagers' behaviour. It is worth noting that in comparable specifications (i.e. Table 2, Column (7)) we found no effects on cognitive outcomes. This suggests that our evidence of significant effects on behavioural outcomes is not due to less robust empirical specifications of our models when using the LSYPE data. Nevertheless, all in all our evidence suggests that neighbour-peer effects are not a strong and pervasive determinant of students' outcomes on either the cognitive or the non-cognitive dimension.

9. Concluding Remarks

Our study has used various detailed administrative datasets on the population of students in England to study the effect of the characteristics and prior achievements of peers in the neighbourhood on the educational achievements and behavioural outcomes of secondary school students. In our main sample we track over 1.3 million students across four cohorts that progress through the first three years of their secondary schooling. Besides presenting new evidence on the effect of peers in the neighbourhood, our population dataset data allows us to make a number of important empirical contributions. First, we drill down to the effect of neighbourhood changes that are caused by real movements of families in an out of small neighbourhoods. We track these changes through information on the detailed residential addresses of our census of students. Moreover, the English institutional setting, where secondary school attendance is not tightly linked to place of residence, allows us to distinguish between neighbours who attend the same or a different school, and to test for potential interactions between school and neighbourhood peer effects. Furthermore, by exploiting the detail and density of our data, we are able to change our definitions of neighbourhoods and peers in the place of residence and thus address the inherent problem in the literature of pinning-down the correct definition of what constitutes a neighbourhood. Finally, exploiting the fact that we observe several cohorts of students experiencing changes in the composition of their neighbourhoods at the same as they move through the education system, we are able to partial out student and family background unobservables, neighbourhood fixed effects and time trends as well as school-by-cohort unobserved shocks. These methods get us close to pinning down an unbiased neighbourhood effect estimate stemming from the changes in the characteristics of the people in the residential neighbourhood (i.e. a 'contextual effect'; Manski, 1993) as originally advocated by Moffit (2001).

Our findings show that, although there is a substantial cross-sectional correlation between students' test scores and the characteristics of their neighbours, there is no evidence that this association is causal.

The causal effect of changes in peers in the neighbourhood on students' test-score gains between grades 6 (ages 11) and 9 (age 14) is nil. Exploiting the density of our data, we have extended our empirical models to go beyond simple linear-in-means specifications, and studied nonlinearities, complementarities and threshold effects. Even then, we failed to find evidence of significant neighbour-peer effects on students' achievements. From a policy perspective, the implication is that – on the educational dimension at least – programmes to promote socioeconomic mixing in communities through residential relocation are unlikely to have any impact on aggregate test scores. Student achievements are evidently unaffected by changes in their neighbourhood composition induced by residential turnover, even when we look at changes occurring over a long five-year interval that spans the whole of compulsory secondary schooling. In contrast, we uncover some evidence that non-cognitive and behavioural outcomes – such as attitude towards schools and anti-social behaviour – are affected by changes in neighbourhood composition, and that these effects are heterogeneous along the gender dimension.

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Tables

Table 1: Descriptive statistics of the main dataset

Variable	Mean	Standard Deviation
Panel A: Students' characteristics, stayers only		
KS2 percentiles, average English, Maths and Science	50.125	25.236
KS3 percentiles, average English, Maths and Science	51.253	25.819
KS2 to KS3 value-added	1.127	13.598
KS1 score, average English and Maths	15.122	3.611
Student is FSM eligible	0.155	0.362
Student is SEN	0.213	0.409
Student is Male	0.508	0.499
Average rate of outward mobility in n'hood over four years	0.081	0.057
Average rate inward mobility in n'hood over four years	0.083	0.062
Secondary school size (in grade 7)	1083.9	384.9
Panel B: Characteristics of students in the neighbourhood – Output Area		
KS1 score, average English and Maths – At grade 6	15.017	1.762
KS1 score, average English and Maths – At grade 9	14.981	1.760
KS1 score, average English and Maths – Change grade 6 to 9	-0.036	0.863
Share FSM – At grade 6	0.165	0.196
Share FSM – At grade 9	0.170	0.199
Share FSM – Change grade 6 to 9	0.005	0.081
Share SEN – At grade 6	0.215	0.154
Share SEN – At grade 9	0.217	0.153
Share SEN – Change grade 6 to 9	0.002	0.087
Share Male – At grade 6	0.509	0.153
Share Male – At grade 9	0.509	0.157
Share Male – Change grade 6 to 9	0.000	0.103
Number of students in Output Area, 'central cohort' +1/-1, Grade 6	13.878	6.317
Number of students in Output Area, 'central cohort' +1/-1, Grade 9	13.865	6.186
Number of students in Output Area, 'central cohort' only, Grade 6	5.173	2.612
Number of students in Output Area, 'central cohort' only, Grade 6	5.169	2.639

Note: Descriptive statistics refer to: *(i)* students who do not change OA of residence in any period between grade 6 and 9; *(ii)* students in Output Areas with at least five students belonging to the 'central cohort' +1/-1 in every period between grade 6 and grade 9; *(iii)* students in the non-selective part of the education system. These restrictions were operated after computing OA aggregate information (see Panel B). Number of 'stayers': approximately 1,310,000 (evenly distributed over four cohorts). Number of Output Areas: approximately 134,000. Average inward mobility and outward mobility in neighbourhood refer to (cohort-specific) Output Area mobility rates averaged over the period grade 6 to 9. KS1 refers to the average test score in Reading, Writing and Mathematics at the Key Stage 1 exams (at age 7); FSM: free school meal eligibility; SEN: special education needs (with and without statements). Secondary school type attended in grade 7: 66.7% Community; 14.9% Voluntary Aided; 3.1% Voluntary Controlled; 14.5% Foundation; 0.3% Technology College; 0.5% City Academy.

Table 2: Characteristics of young peers in the neighbourhood: the effect on students' achievements

	Dependent Variable/Timing is:							
	No controls				With controls			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	KS3/ Grade 9	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9	KS3/ Grade9	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9
Panel A: N'hood Average KS1								
KS1 score – Level (Grade 9) or Change (Grade 6 or 9)	0.279	0.003	-0.000	0.001	0.079	0.003	-0.000	-0.000
	(0.001)**	(0.001)**	(0.001)	(0.001)	(0.001)**	(0.001)**	(0.001)	(0.001)
Panel B: N'hood Share of FSM								
Share FSM – Level (Grade 9) or Change (Grade 6 or 9)	-0.289	-0.005	-0.001	0.001	-0.101	-0.005	-0.001	0.001
	(0.001)**	(0.001)**	(0.001)	(0.001)	(0.001)**	(0.001)**	(0.001)	(0.001)
Panel C: N'hood Share of SEN								
Share SEN – Level (Grade 9) or Change (Grade 6 or 9)	-0.191	-0.002	-0.000	-0.001	-0.055	-0.002	-0.001	-0.001
	(0.001)**	(0.002)	(0.001)	(0.001)	(0.001)**	(0.001)*	(0.001)	(0.001)
Panel D: N'hood Share of Males								
Share Males – Level (Grade 9) or Change (Grade 6 or 9)	0.004	0.001	0.001	0.002	0.000	0.001	0.001	0.002
	(0.001)**	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Secondary by Cohort FX	No	No	No	Yes	No	No	No	Yes
Second. by Primary by Cohort FX	No	No	Yes	No	No	No	Yes	No
OA FX (trends)	No	No	No	Yes	No	No	No	Yes

Note: Table reports standardised coefficients and standard errors. Number of observations ~1,310,000 in ~134,000 Output Areas. All regressions include cohort dummies. Controls include: student own KS1 test scores; student is FMSE; student is SEN; student is male; school size (refers to school attended in grade 7); school type dummies (refers to school attended in grade 7 and includes: Community, Voluntary Aided, Voluntary Controlled, Foundation, CTC and Academy); average rates of outward and inward mobility in n'hood over four years. Secondary by cohort effects: 12,273 groups (refer to school at grade 7 when student enters secondary education). Secondary by primary by cohort school effects: 191,245 groups. OA effects (trends): 134,000 groups. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Table 3: Balancing of changes in neighbourhood characteristics

	Treatment is:			
	(1)	(2)	(3)	(4)
Dependent Variable is:	KS1 score – Change, Grade 6 to 9	Share FSM – Change, Grade 6 to 9	Share SEN – Change, Grade 6 to 9	Share Male – Change, Grade 6 to 9
Panel A: Individual Characteristics (unconditional)				
KS1 score, average English and Maths	0.007	-0.019	-0.006	-0.001
	(0.004)	(0.004)**	(0.004)	(0.003)
Student is FSM eligible	0.000	0.026	-0.006	0.003
	(0.004)	(0.004)**	(0.004)	(0.003)
Student is SEN	-0.000	0.008	-0.005	0.002
	(0.004)	(0.004)*	(0.003)	(0.003)
Student is Male	-0.004	0.005	-0.002	0.009
	(0.004)	(0.004)	(0.004)	(0.004)*
Panel B: Neighbourhood Characteristics (conditional on controls)				
Average KS2 of students living in OA	0.005	-0.004	-0.004	-0.004
(PLASC/NPD)	(0.002)*	(0.002)	(0.003)	(0.002)*
Std.Dev. of KS2 across students living in OA	-0.000	0.001	-0.002	-0.003
(PLASC/NPD)	(0.004)	(0.004)	(0.004)	(0.004)
Share of households living in socially rented	0.002	0.002	-0.003	0.000
accommodation (Census 2001)	(0.002)	(0.003)	(0.002)	(0.002)
Share of households owning place of residence	-0.002	-0.002	0.002	0.001
(Census 2001)	(0.002)	(0.003)	(0.002)	(0.002)
Share of adults in employment	0.003	0.002	-0.001	-0.003
(Census 2001)	(0.003)	(0.003)	(0.003)	(0.002)
Share of adults with no educational qualifications	0.004	-0.001	0.001	0.002
(Census 2001)	(0.003)	(0.003)	(0.003)	(0.002)
Share of lone parents in the population	-0.001	-0.003	0.001	0.000
(Census 2001)	(0.002)	(0.003)	(0.002)	(0.002)

Note: Table reports standardised coefficients and standard errors from regressions of one of the dependent variables (first column) on each of the treatments separately. Census characteristics recorded at the OA level in 2001. All other data was collapsed at the OA level and the regression analysis was performed at this level. Number of observations: approximately 134,000. Regressions in the top panel only control for cohort effects and school-type effects (refers to school attended in grade 7). Regressions in the bottom panel include cohort effects, OA-averaged student KS1 test scores; OA-averaged student eligibility for FMSE; OA-averaged student SEN status; OA-averaged student male gender; OA-averaged school size (refers to school attended in grade 7); school-type effects (refers to school attended in grade 7); OA-averaged rates of outward and inward mobility in neighbourhood. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better. *: at least 5% significant.

Table 4: The impact of neighbourhood peers attending the same/different school

	Dependent Variable/Timing is: KS3-KS2 value-added/Grade 6 to 9		
	(1)	(2)	(3)
Panel A: N'hood Average KS1			
KS1 score – Same school	0.003	0.001	0.001
Change, Grade 6 to 9	(0.001)*	(0.001)	(0.001)
KS1 score – Other school	0.001	-0.001	0.000
Change, Grade 6 to 9	(0.001)	(0.001)	(0.001)
Panel B: N'hood Share of FSM			
Share FSM – Same school	-0.003	-0.001	-0.001
Change, Grade 6 to 9	(0.001)**	(0.001)	(0.001)
Share FSM – Other school	-0.003	0.000	-0.001
Change, Grade 6 to 9	(0.001)**	(0.001)	(0.001)
Panel C: N'hood Share of SEN			
Share SEN – Same school	-0.001	0.000	-0.001
Change, Grade 6 to 9	(0.001)	(0.001)	(0.001)
Share SEN – Other school	-0.002	-0.001	-0.001
Change, Grade 6 to 9	(0.002)	(0.001)	(0.001)
Panel D: N'hood Share of Males			
Share Male – Same school	0.000	0.001	0.001
Change, Grade 6 to 9	(0.001)	(0.001)	(0.001)
Share Male – Other school	0.000	0.001	-0.001
Change, Grade 6 to 9	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Secondary × Cohort FX	No	Yes	No
Second. × Prim. × Cohort FX	No	No	Yes

Note: Table reports standardised coefficients and standard errors. Number of observations approximately 970,000 in approximately 122,000 Output Areas. The smaller sample size and number of Output Areas is driven by the restriction that Output Areas must have both a subset of students going to the same school and a subset of students going to different schools. All regressions include cohort dummies. Controls include: student own KS1 test scores; student is FMSE; student is SEN; student is male; school size (refers to school attended in grade 7); average rate of outward mobility in neighbourhood over four years; average rate inward mobility in neighbourhood over four years.. Secondary by cohort effects: approximately 12,000 groups. Secondary by primary by cohort school effects: 134,000 groups. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Table 5: Robustness to alternative estimation samples and peer-group definition

	Dependent Variable/Timing is:					
	Movers 'ITT' set-up		'Central cohort' only		Adjacent OA n'hoods	
	(1)	(2)	(3)	(4)	(5)	(6)
	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9	KS3-KS2/ Grade 6 to 9
KS1 score – Change (Grade 6 or 9)	0.003 (0.001)**	0.000 (0.001)	0.001 (0.001)	-0.000 (0.001)	0.005 (0.001)**	-0.001 (0.001)
Share FSM – Change (Grade 6 or 9)	-0.005 (0.001)**	-0.001 (0.001)	-0.003 (0.001)**	-0.001 (0.001)	-0.003 (0.001)**	0.001 (0.001)
Share SEN – Change (Grade 6 or 9)	-0.002 (0.001)*	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)	-0.004 (0.001)**	-0.000 (0.001)
Share Males – Change (Grade 6 or 9)	0.001 (0.001)	0.001 (0.001)	0.000 (0.001)	0.001 (0.001)	0.002 (0.001)*	0.002 (0.001)*
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Second. by Primary by Cohort FX	No	Yes	No	Yes	No	Yes

Note: Table reports standardised coefficients and standard errors. Number of observations approximately 1,310,000 in approximately 134,000 Output Areas. All regressions include cohort dummies. Controls include: student own KS1 test scores; student is FMSE; student is SEN; student is male; school size (refers to school attended in grade 7); school type dummies (refers to school attended in grade 7 and includes: Community, Voluntary Aided, Voluntary Controlled, Foundation, CTC and Academy); average rate of outward mobility in n'hood over four years; average rate inward mobility in n'hood over four years. Secondary by primary by cohort effects: 191,245 groups. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Table 6: Heterogeneity in neighbourhood effects, by student and neighbourhood characteristics

	Dependent Variable/Timing is: KS3-KS2 value-added/Grade 6 to 9															
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)	(6a)	(6b)	(7a)	(7b)	(8a)	(8b)
	KS1 < Median	KS1 > Above	Non-FSM	FSM	Non-SEN	SEN	Girl	Boy	Small N'hood	Large N'hood	Low Density	High Density	Low Over-crowd.	High Over-crowd.	Low Share Social House	High Share Social House
Panel A: N'hood Average KS1																
KS1 score Change Grade 6 to 9	-0.001 (0.001)	0.001 (0.001)	0.000 (0.001)	-0.001 (0.002)	0.000 (0.001)	-0.003 (0.002)	0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.001 (0.002)	-0.001 (0.001)	0.001 (0.001)	-0.002 (0.002)	0.001 (0.001)	-0.000 (0.001)	-0.001 (0.005)
Panel B: N'hood Share of FSM																
Share FSM Change Grade 6 to 9	-0.001 (0.001)	-0.002 (0.002)	-0.001 (0.001)	-0.002 (0.002)	*-0.002 (0.001)	0.000 (0.002)	*-0.002 (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.001 (0.001)	**-0.003 (0.001)	-0.002 (0.002)	-0.001 (0.001)	-0.001 (0.001)	0.001 (0.004)
Panel C: N'hood Share of SEN																
Share SEN Change Grade 6 to 9	0.001 (0.001)	-0.002 (0.001)*	-0.001 (0.001)	0.001 (0.002)	-0.001 (0.001)	0.002 (0.002)	-0.002 (0.002)	0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)	*-0.002 (0.001)	0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.005)
Panel D: N'hood Share of Males																
Share Male Change Grade 6 to 9	0.001 (0.001)	0.000 (0.001)	0.001 (0.001)	0.001 (0.002)	0.001 (0.001)	0.001 (0.002)	-0.001 (0.001)	*0.002 (0.001)	0.001 (0.001)	-0.000 (0.002)	0.002 (0.002)	-0.000 (0.001)	-0.000 (0.001)	0.002 (0.002)	0.001 (0.001)	0.001 (0.005)

Note: Table reports standardised coefficients and standard errors obtained from regressions pooling all students and interacting individual characteristic specified in the heading with one of the treatments (change in the neighbourhood characteristic). All regressions include controls as in Table 3, Column (2) and following columns, plus secondary-by-primary-by-cohort fixed effects. Number of observations approximately 1,310,000 in approximately 134,000 Output Areas. Secondary by primary by cohort effects: approximately 191,000 groups. Number of students above/below median KS1: about 582,000/726,000 respectively. Number of FSM/Non-FSM students: around 203,000/1,106,000, respectively. Number of SEN/Non-SEN students: approximately 279,000/1,031,000 respectively. Number of male/female students: around 665,500/643,700 respectively. Small and large neighbourhoods are defined using number of students in the 'central cohort +1/-1' residing in the OA on average over the four years of the analysis. Number of students in large/small neighbourhoods: about 674,000/635,000 respectively. Population density, housing over-crowding and share of households on social housing derived from GB Census 2001 at the OA level. Number of students in high/low density neighbourhoods (above/below median): around 656,000 in both cases. Number of students in neighbourhoods with high/low residential over-crowding (above/below median): approximately 656,000 in both cases. Neighbourhoods with a high share of social housing are defined as those with at least 75% households in socially rented accommodations. Number of students in neighbourhoods with high/low share of social housing: around 43,600/1,267,000 respectively. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Table 7: Characteristics of young peers in the neighbourhood and students' behavioural outcomes; students sampled by the LSYPE (grade 9 in 2004)

	Timing is: Changes between Grade 9 and Grade 11. The outcomes are:							
	Positive school attitude		Playing truant		Substance use		Anti-social behaviour	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male Student	Female Student	Male Student	Female Student	Male Student	Female Student	Male Student	Female Student
Panel A: N'hood Average KS2								
KS2 score –	0.036	0.020	0.013	0.013	-0.015	0.020	-0.018	0.019
Change, Grade 6 to 9	(0.018)*	(0.015)	(0.019)	(0.019)	(0.019)	(0.019)	(0.022)	(0.015)
Panel B: N'hood Share of FSM								
Share FSM –	-0.013	-0.001	-0.032	-0.010	-0.018	-0.006	0.050	-0.008
Change, Grade 6 to 9	(0.018)	(0.017)	(0.018)	(0.018)	(0.018)	(0.017)	(0.022)**	(0.014)
Panel C: N'hood Share of SEN								
Share SEN –	-0.026	-0.064	-0.018	0.004	-0.012	-0.013	0.017	0.003
Change, Grade 6 to 9	(0.017)	(0.016)**	(0.019)	(0.019)	(0.018)	(0.017)	(0.023)	(0.015)
Panel D: N'hood Share of Males								
Share Males –	-0.003	-0.003	0.024	0.011	0.004	0.016	-0.031	-0.001
Change, Grade 6 to 9	(0.017)	(0.016)	(0.018)	(0.017)	(0.018)	(0.018)	(0.021)	(0.015)

Note: Table reports standardised coefficients and standard errors obtained from separate regressions for boys and girls. All regressions include control variables as in Table 2, Column (5) and secondary school fixed effects. Sample includes one cohort of students interviewed in the Longitudinal Survey of Young People in England (LSYPE), aged 14 in 2004. Number of observations: approximately 3700 for both male and female students, in about 500 schools and living in approximately 4000 Output Areas. Peers are defined as student living in the same OA and of the same age. Regression further consider only: *(i)* students who do not change OA of residence between grade 9 and 11; *(ii)* students in Output Areas with at least three students belonging to the same age-group in grades 9 and 11; *(iii)* students in the non-selective part of the education system. 'Attitudes toward schooling' is a composite variable obtained from three separate questions as follows: "School is a worth going (Yes=1; No=0)" + "Planning to stay on after compulsory schooling (Yes=1; No=0)" - "School is a waste of time (Yes=1; No=0)". 'Playing truant' is a binary outcome derived from answers to the following question: "Did you play truant in the past 12 months (Yes=1; No=0)". 'Substance use' is a composite variable obtained from three separate questions as follows: "Did you ever smoke cigarettes (Yes=1; No=0)" + "Did you ever have proper alcoholic drinks (Yes=1; No=0)" + "Did you ever tried cannabis (Yes=1; No=0)". 'Anti-social behaviour' is a composite variable obtained from four separate questions as follows: "Did you put graffiti on walls last year (Yes=1; No=0)" + "Did you vandalise public property last year (Yes=1; No=0)" + "Did you shoplift last year (Yes=1; No=0)" + "Did you take part in fighting or public disturbance last year (Yes=1; No=0)". Selected descriptive statistics for this sample and these variables are provided in Appendix Table 5. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

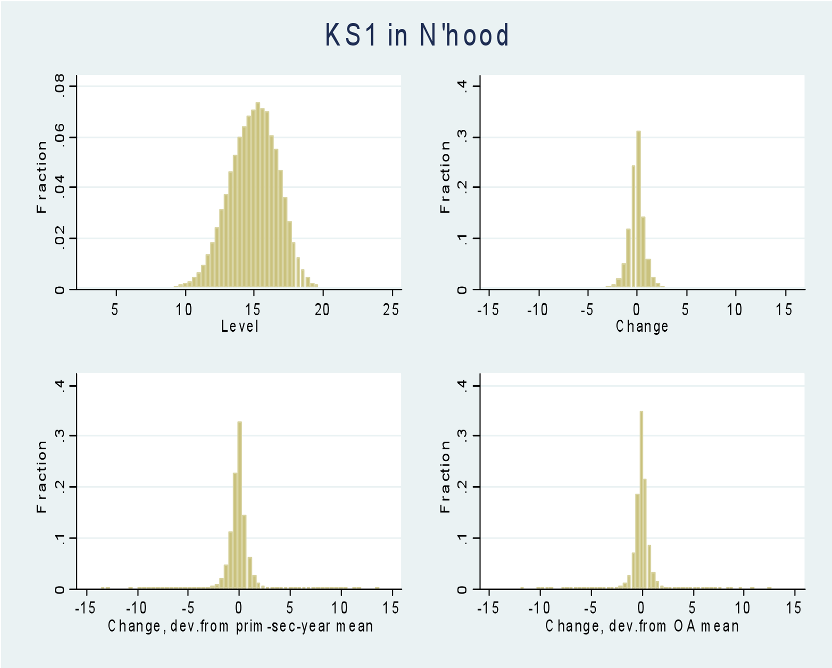
Figures:

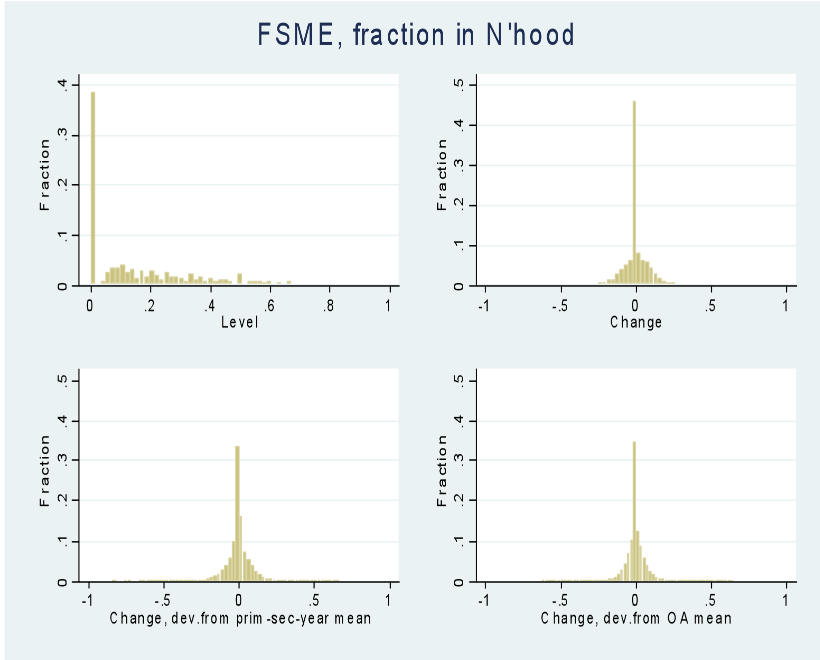
Figure 1: Main dataset construction; four 'central cohorts' and adjacent cohorts

	PLASC 2002	PLASC 2003	PLASC 2004	PLASC 2005	PLASC 2006	PLASC 2007	PLASC 2008
				Grade 5			Grade 8
Cohort 4			Grade 5	Grade 6/KS2		Grade 8	Grade 9/KS3
Cohort 3		Grade 5	Grade 6/KS2	Grade 7	Grade 8	Grade 9/KS3	Grade 10
Cohort 2	Grade 5	Grade 6/KS2	Grade 7	Grade 8	Grade 9/KS3	Grade 10	
Cohort 1	Grade 6/KS2	Grade 7		Grade 9/KS3	Grade 10		
	Grade 7			Grade 10			

Note: Shaded cells refer to the estimation sample; immediately adjacent non-shaded cohorts represent the additional set of students used to construct measures of quality of neighbourhood. PLASC refers to the Student Level Annual School Census. Students finish their primary school in grade 6 when they sit for their Key Stage 2 (KS2) at age 11. Thick border indicates end of primary school. Students enter secondary education in grade 7 and complete their Key Stage 3 exams in grade 9 when aged 14.

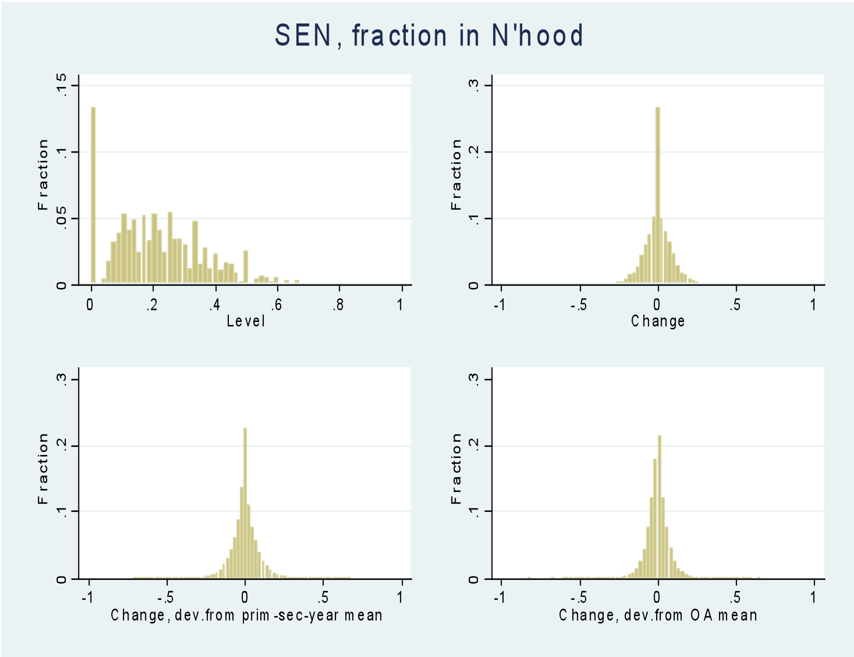
Figure 2a: Characteristics of students in the neighbourhood and amount of variation: prior achievements (KS1) and free school meal eligibility (FSM)





Note: Descriptive statistics of deviations from primary-by-secondary-by-cohort mean changes are as follows. Average KS1, mean 0.000; std.dev. 0.778. Fraction of FSM students: mean 0.000, std.dev. 0.073. Descriptive statistics of deviations from Output Area mean changes as follows. Average KS1, mean 0.000; std.dev. 0.632. Fraction of FSM students: mean 0.000, std.dev. 0.061. Descriptive statistics for the level and change in these variables are reported in Table 1, Panel B.

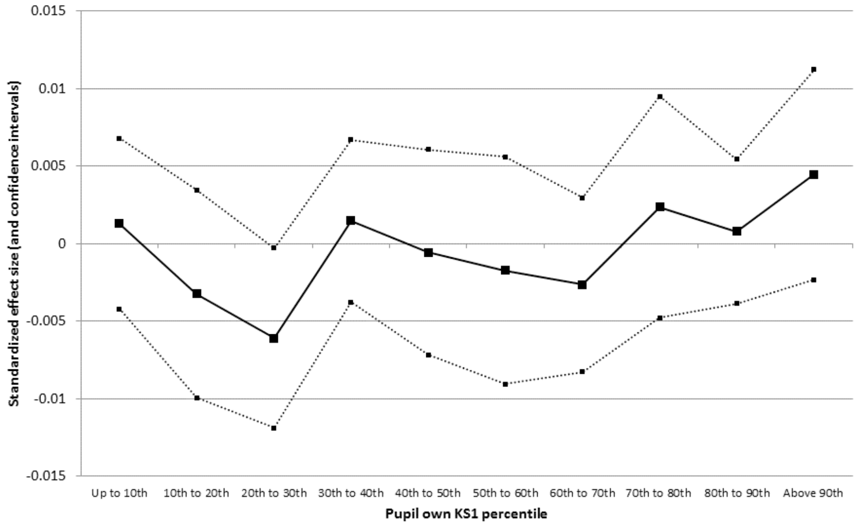
Figure 2b: Characteristics of students in the neighbourhood and amount of variation: special education needs (SEN) and share of male students



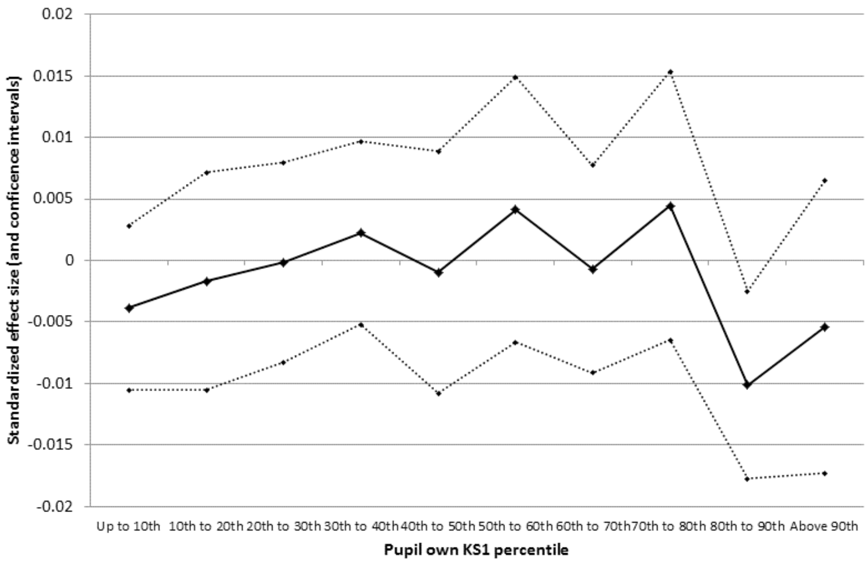
Note: Descriptive statistics of deviations from primary-by-secondary-by-cohort mean changes are as follows. Fraction of SEN students: mean 0.000, std.dev. 0.078. Fraction of Male students: mean 0.000, std.dev. 0.093. Descriptive statistics of deviations from Output Area mean changes as follows. Fraction of SEN students: mean 0.000, std.dev. 0.065. Fraction of male students: mean 0.000, std.dev. 0.076. Descriptive statistics for the level and change in these variables are reported in Table 1, Panel B.

Figure 3: Graphical presentation of complementarities between neighbour and student KS1 in effect on student KS2-KS3 test score gain. Regression coefficients and 95% confidence intervals. See Section 7.

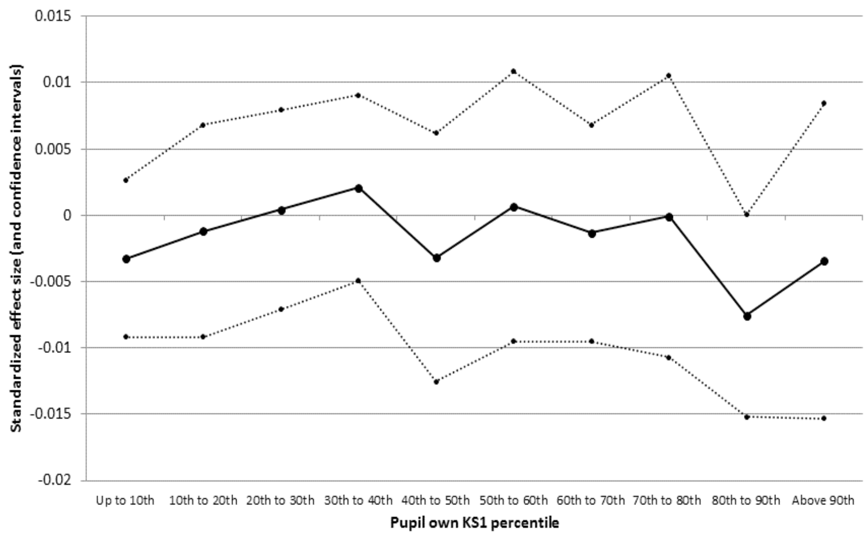
Proportion of neighbours in top KS1 decile



Mean neighbour ks1 score (conditional on proportion in top and bottom decile)



Proportion of neighbours in bottom KS1 decile



Appendix A

Data on grade 11 (age 16) qualifications

In some extensions to our main analysis, we consider KS4 qualifications at grade 11 (age 16). The combined PLASC/NPD data allows us to extract two cohorts of students to investigate the effect of changes in the neighbourhood peers for the extended period covering the grade-6 to grade-11 span. In this case we construct neighbour-peer variables using students in the same OA and same grade only. It is not feasible to include students in older and younger grades, because many older students drop out of education and thus out of our dataset after grade 11 (the end of compulsory education). Otherwise, the data we collect on students in the grade-6 to grade-11 time window and their descriptive statistics are very similar to the information and characteristics of students in the grade-6 to grade-9 sample. The only notable difference is that KS4 scores are recorded on a scale of zero to eight. In order to make them comparable with KS2 and KS3 scores and construct measures of value-added, we average students' performance across Mathematics, Science and English and convert this mean into percentiles in the cohort-specific national distribution. This approach has been previously used when analysing these data (e.g. Gibbons and Silva, 2008).

Appendix Tables

Appendix Table 1: Descriptive statistics before dropping mobile students and small neighbourhoods

Variable	Mean	Standard Deviation
Panel A: Students' characteristics		
KS2 percentiles, average English, Maths and Science	50.207	25.915
KS3 percentiles, average English, Maths and Science	49.308	25.251
KS2 to KS3 value-added	0.898	13.770
KS1 score, average English and Maths	15.004	3.647
Student is FSM eligible	0.171	0.377
Student is SEN	0.220	0.414
Student is Male	0.507	0.500
Average rate of outward mobility in n'hood over four years	0.098	0.075
Average rate inward mobility in n'hood over four years	0.089	0.073
Secondary school size (in grade 7)	1081.6	385.0
Panel B: Characteristics of students in the neighbourhood – Output Area		
KS1 score, average English and Maths – At grade 6	14.968	1.857
KS1 score, average English and Maths – At grade 9	14.966	1.854
KS1 score, average English and Maths – Change grade 6 to 9	-0.002	1.407
Share FSM eligible – At grade 6	0.172	0.205
Share FSM eligible – At grade 9	0.172	0.206
Share FSM eligible – Change grade 6 to 9	-0.001	0.140
Share SEN – At grade 6	0.218	0.166
Share SEN – At grade 9	0.218	0.166
Share SEN – Change grade 6 to 9	0.000	0.139
Share Male – At grade 6	0.509	0.174
Share Male – At grade 9	0.509	0.176
Share Male – Change grade 6 to 9	0.000	0.128
Number of students in Output Area, 'central cohort' +1/-1, Grade 6	13.212	6.562
Number of students in Output Area, 'central cohort' +1/-1, Grade 9	12.884	6.628

Note: Descriptive statistics refer to students in the non-selective part of the education system. The data *includes (i)* students who change OA of residence between grade 6 and 9; and *(ii)* students in Output Areas with less than five students belonging to the 'central cohort' +1/-1 in every period between grade 6 and grade 9. Number of observations: approximately 1,850,000, almost evenly distributed over four cohorts. Number of Output Areas: approximately 158,000. Secondary school type attended in grade 7: 66.6% Community; 14.9% Voluntary Aided; 3.1% Voluntary Controlled; 14.5% Foundation; 0.4% Technology College; 0.5% City Academy. See note to Table 1 for further details on the variables.

Appendix Table 2: Additional results: change-in-change and unobservable effects

	Dependent Variable/Timing is: KS3-KS2 value-added/Grade 6 to 9					
	Without controls			With controls		
	(1)	(2)	(3)	(5)	(6)	(7)
Panel A: N'hood Average KS1						
KS1 score –	0.001	0.000	0.001	0.000	-0.000	0.000
Change, Grade 6 to 9	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: N'hood Share of FSM						
Share FSM –	-0.002	-0.001	0.000	-0.002	-0.002	0.000
Change, Grade 6 to 9	(0.001)*	(0.001)	(0.001)	(0.001)*	(0.001)*	(0.001)
Panel C: N'hood Share of SEN						
Share SEN –	-0.000	-0.001	-0.000	-0.001	-0.001	-0.000
Change, Grade 6 to 9	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel D: N'hood Share of Males						
Share Male –	0.001	0.001	0.001	0.001	0.001	0.002
Change, Grade 6 to 9	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls	No	No	No	Yes	Yes	Yes
Secondary fixed FX	Yes	No	No	Yes	No	No
Secondary × Cohort FX	No	Yes	No	No	Yes	No
OA FX (trends)	No	No	Yes	No	No	Yes

Note: Table reports standardised coefficients and standard errors. Number of observations approximately 1,310,000 in approximately 134,000 Output Areas. All regressions include cohort dummies. Controls include: student own KS1 test scores; student is FMSE; student is SEN; student is male; school size (refers to school attended in grade 7); school type dummies (refers to school attended in grade 7 and includes: Community, Voluntary Aided, Voluntary Controlled, Foundation, CTC and Academy); average rate of outward mobility in neighbourhood over four years; average rate inward mobility in neighbourhood over four years. Secondary school fixed effects: approximately 3200 groups (refer to school at grade 7 when student enters secondary education). Secondary by cohort effects: approximately 12,000 groups. OA effects (trends): approximately 134,000 groups. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Appendix Table 3: Reasons for mobility – BHPS

Percentages reporting following reasons	First Reason Given by Respondent
Larger accommodation	22.6%
Move in with/move out from partner	16.0%
Job reasons (own/family)	9.6%
Other aspects of home (smaller, did not like previous one, moved to better)	9.5%
Buy/Own	7.6%
Evicted/home repossessed	5.4%
Other family reasons	4.0%
Dislike area	4.0%
Considered area unsafe/unfriendly	1.5%
Move to/from rural area	1.3%
Moved to specific place	1.3%
Dislike isolation	0.4%
Traffic/Noise	0.3%
Other/No reasons	16.2%

Note: Table include only individuals with children, aged between 27 and 60, living in England and in the BHPS waves 2002-2008. This is to match the PLSAC period and age profile of parents. Number of observations: 637.

Appendix Table 4: Characteristics of young peers in the neighbourhood and students' achievements: Grade 6/KS2 to Grade 11/KS4 and Grade 9/KS3 to Grade11/KS4 time-windows

	Dependent Variable/Timing is:			
	(1)	(2)	(3)	(4)
	KS4-KS2/ Grade 6 to 11	KS4-KS2/ Grade 6 to 11	KS4-KS3/ Grade 9 to 11	KS4-KS3/ Grade 9 to 11
Panel A: N'hood Average KS1				
KS1 score – Change,	-0.002	-0.002	0.000	-0.000
Grade 6 to 11 or Grade 9 to 11	(0.002)	(0.002)	(0.002)	(0.002)
Panel B: N'hood Share of FSM				
Share FSM –	-0.002	-0.001	0.003	0.003
Change, Grade 6 to 11	(0.002)	(0.002)	(0.002)	(0.002)
Panel C: N'hood Share of SEN				
Share SEN –	-0.000	0.001	-0.001	-0.000
Change, Grade 6 to 11	(0.002)	(0.002)	(0.002)	(0.002)
Panel D: N'hood Share of Males				
Share Male –	0.000	-0.001	0.001	-0.001
Change, Grade 6 to 11	(0.002)	(0.002)	(0.002)	(0.002)
Controls	Yes	Yes	Yes	Yes
Secondary school fixed FX	No	Yes	No	Yes

Note: Table reports standardised coefficients and standard errors. Sample includes only two cohorts. Peers are defined as student living in the same OA and of the same age. Regression further consider only: *(i)* students who do not change OA of residence between grade 6 and 11; *(ii)* students in Output Areas with at least three students belonging to the same age-group in grades 6 and 11 (Columns (1) to (3)) and grades 9 and 11 (Columns (4) to (6)); *(iii)* students in the non-selective part of the education system. Number of observations approximately 500,000 in approximately 102,000 Output Areas. All regressions include controls as in Table 3, Column (2) and following columns. Secondary school fixed effects: approximately 3100 groups (refer to school at grade 7 when student enters secondary education). Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Appendix Table 5: Selected descriptive statistics for students sampled by the LSYPE (aged 14 in 2004)

Variable	Mean	Standard Deviation
Panel A: Students' characteristics, 'stayers' only		
Attitudes toward schooling – Change grade 9 to 11	-0.160	0.741
Playing truant – Change grade 9 to 11	0.111	0.460
Substance use – Change grade 9 to 11	0.482	0.789
Anti-social behaviour – Change grade 9 to 11	-0.114	0.819
KS2 score, average English and Maths	27.481	4.020
Student is FSM eligible	0.187	0.390
Student is SEN	0.152	0.359
Student is Male	0.504	0.500
Average rate of outward mobility in n'hood over three years	0.050	0.069
Average rate inward mobility in n'hood over three grades	0.054	0.079
Secondary school size (in Grade 9)	1132.0	331.4
Panel B: Characteristics of students in the neighbourhood – Output Area		
KS2 score, average English and Maths – Change grade 9 to 11	0.001	1.226
Share FSM eligible – Change grade 9 to 11	0.003	0.094
Share SEN – Change grade 9 to 11	-0.001	0.098
Share Male – Change grade 9 to 11	-0.001	0.123
Number of students in Output Area, Grade 9	5.950	2.529
Number of students in Output Area, Grade 11	5.945	2.498

Note: Descriptive statistics refer to the sample that includes one cohort of students interviewed in the Longitudinal Survey of Young People in England (LSYPE), aged 14 in 2004. Number of observations: approximately 7800 in about 600 schools and living in approximately 6800 Output Areas. Peers are defined as student living in the same OA and of the same age. The sample only include *(i)* students who do not change OA of residence between grade 9 and 11; *(ii)* students in Output Areas with at least three students belonging to the same age-group in grades 9 and 11; *(iii)* students in the non-selective part of the education system. 'Attitudes toward schooling' is a composite variable obtained from three separate questions as follows: "School is a worth going (Yes=1; No=0)" + "Planning to stay on after compulsory schooling (Yes=1; No=0)" - "School is a waste of time (Yes=1; No=0)". Truancy is a binary outcome derived from answers to the following question: "Did you play truant in the past 12 months (Yes=1; No=0)". 'Substance use' is a composite variable obtained from three separate questions as follows: "Did you ever smoke cigarettes (Yes=1; No=0)" + "Did you ever have proper alcoholic drinks (Yes=1; No=0)" + "Did you ever tried cannabis (Yes=1; No=0)". 'Anti-social behaviour' is a composite variable obtained from four separate questions as follows: "Did you put graffiti on walls last year (Yes=1; No=0)" + "Did you vandalise public property last year (Yes=1; No=0)" + "Did you shoplift last year (Yes=1; No=0)" + "Did you take part in fighting or public disturbance last year (Yes=1; No=0)". KS1 test scores not available for this cohort Age 7/Grade 2). Prior achievement of students and their peers in the neighbourhood are proxied by KS2 test scores (Age 11/Grade 6).