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

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

Abstract:

Outcomes of children living in the same neighbourhood are highly correlated. This link has given rise to a large body of literature on neighbourhood effects, but causality is difficult to assess. This study estimates 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. We contribute to the literature by exploiting the longitudinal nature of our census data with a research design based on changes in neighbourhood composition for immobile students, where these changes arise through residential migration amongst other students in our dataset. This set-up enables 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. The complete coverage of our data also allows us to go beyond linear-in-means models and investigate heterogeneity and non-linearities in the effect of neighbourhood composition at an unprecedented level. Our results show that changes in neighbourhood composition have no effects on test scores. However, we find some effects on behavioural outcomes, 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 substantial disparities between the achievements and behaviour of children living in different neighbourhoods (Lupton et al., 2009). These disparities have long been a centre of attention for researchers and policy makers concerned with socio-economic inequality and its consequences. The underlying foundation for these concerns is the belief that children's outcomes are causally linked to the characteristics and behaviour of people who live around them. Area-based policies that are intended to address such inequalities are predicated on the existence of such causal links (see discussions in Currie, 2006 for the US, and Cheshire et al., 2008 for the UK). Interventions of this type include inclusionary zoning and desegregation policies, as well as regeneration and mixed-housing projects, such as 'Hope VI' in the US and the 'Mixed Communities Initiative' in England. The theories of 'social interactions', 'neighbourhood effects' and 'peer effects' (Jencks and Mayer 1990; Manski, 2000 and Durlauf, 2004) that underpin these policies have been drawn into economics from sociology and psychology, and economists have put substantial emphasis on role models (Akerlof, 1997 and Glaeser and Scheinkman, 2001), social networks (Granovetter, 1995 and Bayer et al., 2008), and conformism (Bernheim, 2004 and Fehr and Falk, 2002).

Although these theories are compelling, convincing empirical evidence remains elusive and opinion on their policy relevance is divided.[1] The ambiguity in the evidence comes about for four reasons: *(1)* Sorting – causality is difficult to establish because children's characteristics are linked to those of their parents, and in turn to those of their neighbours through common factors in residential choice; *(2)* Correlated effects – while theories of 'social interactions' and 'neighbourhood effects' are about the effect of *neighbours*, the distinction between *neighbours* and *neighbourhood factors* ('correlated effects'; Manski, 1993) – such as good schools – is often blurred in the empirical investigations; *(3)* Defining neighbourhood groups – the correct geographical scale that should be used to delimit 'a neighbourhood' is a priori unknown; and *(4)* Heterogeneity – issues of equity and efficiency in neighbourhood-related policies hinge on questions about heterogeneity and non-linearity in neighbourhood effects, but the existing literature does not fully investigate these issues.

Bearing these four issues in mind, the main contributions of this paper relative to previous work in the field are: *(1)* to use a research design which includes individual and neighbourhood fixed effects and in which we directly observe the impact of residential movers on children who do not move to identify the causal effects of neighbourhood composition – rather than spurious sorting; this design is applied to a large administrative census of multiple cohorts school children, spanning several years of childhood; *(2)* to use this design to estimate the effect of changes in the characteristics of neighbourhood peers – net of amenities and other 'correlated' effects (Manski, 1993); *(3)* to exploit the geographical detail of our data to provide alternative neighbourhood definitions and assess the correct spatial-reference scale in the most flexible way; the level of detail in our data even allows us to distinguish between neighbours who attend the same or a

[1] The most credible experimental estimates find negligible effects on children's educational attainments, but some effects on behavioural outcomes (Katz et al., 2007) and long-term mental wellbeing (Ludwig et al., 2012).

different school, shedding further light on the actual stretch of neighbourhoods; and *(4)* to use the size of our census data set to fully explore potential non-linearities and heterogeneities in neighbourhood effects, which is of paramount importance for residential mixing policies.

In essence, our empirical setup involves regressing age 11-to-14 student test scores changes on age 11-to-14 changes in neighbourhood quality. The measures of neighbourhood quality that we use are based on characteristics of the students in our English administrative data that are predetermined (at age 11), and we restrict our estimation sample to individuals who do not change neighbourhoods. This ensures that our identifying variation arises from changes in neighbourhood composition for residential stayers induced by movements of other residents in and out of the neighbourhood, and implies that we can control for individual and neighbourhood unobservables without requiring that treated individuals change residential neighbourhood. These population movements that we exploit are sizeable, with over 425,000 students or around 25% of the neighbourhood group changing during the three year period over which we measure the development of academic achievements in our main specifications. This setup, while unique in the neighbourhood literature, 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, to Gould et al. (2011a) who investigate the effects of large inflows of immigrants into Israeli elementary school on the long-term outcomes of native students, and to Moretti (2004) who studies social returns to education in cities by looking at compositional changes experienced by non-movers. However, our method differs from research on school-peer effects that exploits cohort-to-cohort variation in group composition to control for time-fixed school unobservables (e.g. Hoxby, 2000; Hanushek et al., 2003; Gibbons and Telhaj; 2008, Lavy et al., 2012a). These studies either do not control for individual fixed effects and compare different students in different cohorts to control for school fixed effects, or else require student mobility between schools (or grade repetition) to generate within-student variation in peer group and control for individual fixed effects.

We address concerns about sample selection in our group of stayers by carrying out an 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). Furthermore, we can account for unobservables that induce *changes* over time in movers' characteristics and stayers' outcomes within neighbourhoods by tracking several cohorts of students as they progress from primary through secondary education. This allows us to control for unobserved trends in neighbourhood quality (e.g. gentrification or deterioration in housing quality) and 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. This implies that different students in the same residential neighbourhood attend two to three different secondary schools, and that secondary schools enrol students from around sixty different residential areas.

To preview our results, we find little evidence of a causal link between young peoples' test scores outcomes and neighbours' characteristics once we control for individual and neighbourhood fixed effects by looking at changes in the neighbourhood peer composition over time. Our estimated regression coefficients are near-zero, and precisely estimated. Furthermore, any remaining association is eliminated once we control for school-by-cohort effects and/or neighbourhood-specific time trends. Differentiating between the effects for neighbours in the same school and neighbours in different schools still yields no evidence that neighbourhood composition matters. Going beyond the simple linear-in-means specification of neighbour-peer effects on test scores, 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 fleshes out in more detail the four empirical challenges in neighbourhood effects research and the ways in which our design mitigates them. Section 3 describes our empirical strategy formally, and Sections 5 discusses that data that we use and the English institutional context. Next, Section 6 discusses our findings on cognitive outcomes and robustness checks, while Sections 7 and 8 present our evidence on heterogeneity and complementarities, and on behavioural outcomes respectively. Finally, Section 9 provides some concluding remarks.

2. Empirical issues in neighbourhood effects estimation

The standard approach to estimating neighbourhood effects is based on the statistical association between children's outcomes and the socio-economic composition of their neighbourhood ('contextual effects'; Manski, 1993). As outlined in the Introduction, there are four main reasons to doubt the interpretation of these estimates as causal parameters, namely: sorting; 'correlated effects'; appropriate reference group; and heterogeneity. In this section we flesh out these problems and present our line of attack.

The first empirical challenge is posed by the fact that children's characteristics are linked to those of their parents, and in turn to those of their neighbours through residential sorting. This implies that the causal influence on the effect of neighbours' characteristics is confounded by the simultaneous effects of children's and parents' own attributes. Studies have used a variety of approaches to address these biases, including: instrumental variables (Cutler and Glaeser, 1997 and Goux and Maurin, 2007); institutional arguments related to social renters with limited residential choice and mobility (Gibbons, 2002; Oreopolous, 2003; Jacob, 2004; Goux and Maurin, 2007; Weinhardt, 2010); quasi-experimental placement policies for immigrants (Edin et al., 2003 and 2011; Gould et al., 2011b); and fixed effects 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 interventions, namely the Gautreaux and Moving to Opportunity (MTO) programmes (Rosenbaum 1995; Katz et al. 2005 and 2007; Sanbonmatsu et al. 2006).

Even if problems of sorting are solved, studies still need to disentangle correlation caused by *neighbours*' characteristics from common coincidental *neighbourhood* amenities ('correlated effects'; Manski, 1993). Indeed, neighbourhoods that differ in terms of their socio-economic composition likely differ along other dimensions – such as school quality and other local amenities – which are often unobserved in the data. This distinction between the effects of better *neighbours* and those of better *neighbourhoods* is often blurred in empirical work, and the importance of neighbourhood composition as opposed to local resources and amenities takes a back seat. Randomisation of children to neighbourhoods does not solve this problem because the neighbourhoods to which individuals are assigned potentially differ along many other dimensions. In this respect, most of the MTO-based studies (Kling et al. 2005, 2007, Sanbonmatsu et al. 2006) treat neighbourhoods as a 'black box' in terms of the specific causal channels, although recent work has started to unpick the contributory factors (Harding et al., 2010).[2] In order to isolate the causal influence of neighbours from the effects of neighbourhoods, Moffitt (2001) suggested 'reverse-engineering' the evaluation of programmes like the MTO or Gautreaux to study changes in the outcomes of the original residents of the areas receiving relocated households. For these people, neighbourhoods remain unchanged except in so far as their composition is affected by the influx of new families.

Following this intuition, our study mitigates problems of sorting and confounding neighbourhood attributes by exploiting changes in neighbourhood composition induced by migration of residential movers in a population of school-age families. We estimate the effect of these mover-induced compositional changes on cognitive and non-cognitive outcomes of stayers in England from age 11 (grade 6) up to age 16 (grade 11).[1] This approach allows us to control for time-persistent neighbourhood amenities, such as local school quality and other localised infrastructures/amenities (Manski's (1993) 'correlated effects'), and to separately identify the effects arising from changes in neighbourhood composition, which we label 'neighbourhood peer effects'.[3] Although we cannot pin down the theoretical channels through which neighbours might matter – e.g. conformism, social networks and role models – this limitation is common to the literature on peer effects in schools. Nevertheless, we claim that these reduced-form estimates are policy relevant since they shed light on the likely effect of desegregation policies and mixed-communities initiatives which advocate changes to neighbourhood composition as a way to improve youths' outcomes.

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[2] Most other studies do not control for the quality of local schools and other neighbourhood features in their analysis, or try to distinguish between school and neighbourhood level variables (e.g. Goux and Maurin, 2007) although there are exceptions (e.g. Card and Rothstein 2007, Gould et al., 2004).

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

The third challenge lies with defining the operational reference group for a child's neighbour-peer influences. Like most previous research, we have no information on friendship networks – which are in any case prone to problems of self-selection. However, we 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 friendship group. Of necessity, we must approximate the level at which these influences take place. However, unlike other research which is limited to large pre-defined groups (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, and 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.

Fourthly and finally, the existing literature does little to investigate heterogeneity and non-linearities in the effect of changes to neighbourhood composition, despite this being crucial to understanding the consequences of social mixing.[4] Even if policies that promote integrated neighbourhoods succeed in reducing inequality, they will be inefficient if the losses to those who lose out from mixed neighbourhoods outweigh the gains to those who benefit.[5] The literature on peer effects at school investigates these issues extensively (Hoxby and Weingarth, 2005; Gibbons and Telhaj 2008, and Lavy et al., 2012a and 2012b), but there is much less evidence on heterogeneity in relation to residential neighbourhood effects. Although long ago these concerns were prominent in the neighbourhoods literature – both in theory (Jencks and Mayer 1990) and empirically (Crane 1991, Corcoran et al., 1989) – recent empirical work has paid less attention, as the search for credible identification of causal effects has led to a focus on linear effects for homogenous and narrowly defined groups. These groups 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 (the 'Gautreaux' and 'Moving to Opportunity' programmes cited above). This narrow focus aids identification, but precludes investigation of heterogeneity and complementarities for two reasons. Firstly, sample sizes are often small, limiting the scope for further slicing the data into sub-groups. Secondly, by focussing on the most disadvantaged individuals, these studies cannot investigate whether the effects of neighbourhood composition are homogenous or heterogeneous along the lines of students' background and ability. To examine these issues in detail poses huge data requirements for empirical research. Our dataset provides us with a unique

[4] We use the term heterogeneity to mean differences in response to neighbours' characteristics across individuals. This encompasses different degrees of complementarity between neighbours' and individuals' characteristics.

[5] Argys et al. (1996), Hoxby (2000) and Hanushek and Woessmann (2006) discuss these issues in the context of peer effects at school.

opportunity to investigate heterogeneity and non-linearities in these responses at a very detailed level. The next section sets out our approach in greater detail.

3. Empirical strategy

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

Our empirical work estimates the effect of neighbourhood composition on students' educational and behavioural outcomes during secondary schooling. As outlined above, any attempt to estimate the causal influence of neighbourhood peers must eliminate biases that arise because of sorting. In order to address this issue, we use a changes-in-changes research design. The rest of this section sets out our empirical model 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:

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

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*. 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 (ages 11 to 16) 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:

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

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 shock. Among other things, this term is intended to capture variation in school resources, composition and quality of teaching that is 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:

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

where the subscripts *t=0* and *t=1* indicate 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 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:

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

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. To allay concerns about the stayers being a selected sample, 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, providing 'intention-to-treat' estimates. Note also that it is straightforward to generalise Equation (2.1) to allow for heterogeneity and non-linearities in the effects of neighbour-peer composition, for example by interacting students' characteristics $\mathbf{x}_{it}$ with neighbourhood composition changes ($z_{nc1} - z_{nc0}$).

Equation (2.2) shows that this grade-differenced specification does not control for changes in school quality 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 because students change schools over the grade intervals that we study (some of these changes are compulsory during the primary to secondary transition) – or because of new school 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, $z_{nc1} - z_{nc0}$. We therefore further control for secondary-school-by-cohort effects or secondary-by-primary-school-by-cohort effects (effectively school-by-grade-by-cohort effects), effectively absorbing these sources of variation. We can in addition control for general unobserved neighbourhood-specific time trends ξ_n – such as gentrification or decline of some areas relative to others – by differencing from neighbourhood means across cohorts *c*.

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 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.

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 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) sheds light on the relative contribution of school and neighbourhood peers. Whereas peer effects (β) among neighbouring students who attend the same school might pick up interactions among students in schools, γ represents a 'pure' neighbour-peer effect among students who go to different schools. As before, we can difference Equation (3) within neighbourhoods, across cohorts to eliminate neighbourhood trends, and can control for school-by-cohort fixed effects.[6]

3.3. Defining neighbourhood geography

While all research on peer effects faces problems in defining group membership (e.g. Ammermueller and Pischke, 2009 for school peer effects), this choice is particularly challenging for neighbourhood peer effects where there are no natural boundaries (such as the grade or the class for school peer effects). Consequently, the neighbourhood group definitions adopted by previous studies vary 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 aggregation reduces measurement error.[7]

Whether or not the level of aggregation matters in practice is an empirical question. The detail and coverage of our population-wide data permits experimentation 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). Given that our identification approach relies on neighbourhood fixed effects and trends to control for unobserved neighbourhood factors, a small-scale neighbourhood definition is preferable because 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.

Another advantage of our data is that it covers the population of English state school children and we can measure neighbour-peer composition in a variety of school grades. Since we are interested in peer effects in the neighbourhood, we begin by considering students of similar age and construct neighbour-peer variables using data from students who are either of the same school grade (i.e. grade 6/age 11 at the beginning of our observation 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. Note that these 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.

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[6] 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 $\left(\vartheta_{sc1}-\vartheta_{sc0}\right)$.

[7] This is because the reduction in the covariance between mean neighbours' characteristics and individual outcomes will be offset by a reduction in the variance of average neighbours' characteristics in a regression of individual outcomes on neighbours' characteristics.

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.[8] 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 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.[9]

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 are used by over-subscribed schools to prioritise applicants, but preference is usually given first to children with special educational needs, 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, most households have a choice of more than one school 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 unique feature allows us to measure changes in neighbourhood peer composition for students who attend the same or a different school.

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

[8] KS3 assessments were stopped in 2009, which marks the end of our data period.
[9] More details are available from the Department for Education and the Qualifications and Curriculum Authority.

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. PLASC also records the home postcode of each student on an annual basis. A postcode typically corresponds to 17 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 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' early academic ability, FSM eligibility as an indicator of low family income and SEN is as a proxy for learning difficulties and disabilities. FSM 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). SEN is based on students deemed by the school to have special educational needs, which includes those with 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, so they do not change over time by construction. 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). 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).[10] 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 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.

4.4. Data on behaviour from the LSYPE

The administrative data in PLASC/NPD provides outcome variables related to academic test scores. However, previous research in the field (Kling et al., 2005 and 2007) suggests that behavioural outcomes – e.g. crime, educational aspirations, health, 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 use 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 survey covers students' experiences 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

[10] 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).

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. Moreover, since many older students drop out of education and thus out of our dataset after grade 11 (the end of compulsory education), we construct neighbour-peer variables using students in the same OA and grade only.[11] Finally, 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 their 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 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.[12] 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 FSM, 21 percent have 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. Note that our estimation sample – which excludes movers and students in the smallest neighbourhoods – is representative of the population as a whole; see Appendix Table 1.

[11] 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.

[12] 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).

Panel B of Table 1 presents the means and standard deviations 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 change is due to the fact that the statistics report neighbour-group means and individuals are changing group membership). Our neighbourhoods have 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 close neighbour-peers.

An important point 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. The standard deviation of KS1 scores is 1.76, while the standard deviation of the change in this variable between grades 6 and 9 is just over 0.86. Therefore 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, more than 8% of the neighbours move out and are replaced by new neighbours each year. Over three years, this means that more than one in four pupils in a student's neighbour-peer 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 might expect to have real consequences.

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

Table 2 presents our 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 the effects from any one of them – interpreted as an index of neighbour-peer quality – rather than the effect of each characteristic conditional on the others. 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 scores. All four characteristics are strongly and significantly associated with students' KS3 scores. A one standard deviation increase in KS1 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. These cross-sectional estimates are potentially biased by residential sorting and unobserved individual, school and neighbourhood factors. The results from the within-student, between-grade differenced specifications in Equations (2.1)-(2.2) are shown in Column (2). Now, 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 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 not significantly associated with students' KS2-to-KS3 value-added, with estimated effects close to zero.

In order to control further for school specific factors, Columns (3) adds primary-by-secondary-by-cohort effects. 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).[13] 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

[13] Note 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.

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 that we observe once we difference the data within-student between-grades is caused by measurement error 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. However, 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.[14] Instrumental variable regressions confirm that the effect of changes in KS1 test scores of neighbour-peers is not a strong and significant predictor of students' KS2-to-KS3 value-added. In our second robustness 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 due to the inclusion of a large number of fixed effects (around 190,000 primary-by-secondary-by-cohort groups). As shown 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.[15]

In summary, our baseline linear-in-means specifications indicate that the effects of neighbour-peers on student achievement are statistically insignificant and negligibly small. Since controlling for unobserved neighbourhood trends does not affect our estimates once we have taken into account school-by-cohort effects, the analysis that follows considers only simple value-added specifications, and specifications that further control for school cohort-specific effects.

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[14] 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.

[15] 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 status, number of full-time equivalent qualified teachers, and numbers of support teachers for ethnic minorities and for SEN students. These specifications confirmed that neighbourhood composition is not strongly associated with students' value-added.

6. Validity, extensions and robustness checks

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 already shown that the results of the between-grade within-individual value-added specifications are insensitive to the inclusion of additional control variables. In this section, we tackle this issue more systematically by showing that our treatments are balanced with respect to student and neighbourhood characteristics.

The neighbourhood characteristics that we consider come from the GB 2001 population census at the OA level. Specifically, these are the 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 come from the NPD collapsed to the OA of residence at grade 6 (age 11), namely: KS1, FSM and SEN 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 OA-level regressions of these neighbourhood characteristics on the OA-specific changes in the 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 changes 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.[16] The only significant and meaningful associations are related to the changes in neighbour-peer FSM. These estimates show 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). These associations are, however, 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. Assuming that the

[16] 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.

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), the balancing test in Table 3 provide broad evidence that the near-zero neighbour-peer effect estimates in Table 2 are not biased by student or neighbourhood unobservables.

Nevertheless, a sceptical reader could still argue that there might be unobserved shocks, conditional on school-by-cohort effects and neighbourhood trends, which simultaneously affect children's outcomes and the distribution of the characteristics of in-migrants and out-migrants. 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 a net outflow of rich 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 housing consumption). In other words, our near-zero estimates should be regarded as an upper bound of the effects of neighbourhood composition. Additional evidence from the British Household Panel Survey (BHPS), however, provides little support for the idea that residential migration occurs as a result of neighbourhood shocks. 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 the 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 reasons not related to neighbourhoods and none of the responses cite neighbourhood changes or education issues. In conclusion, 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?

The analysis so far has not distinguished between neighbour-peers who attend the same secondary school and those who do not. 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, so 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 Equation (3), and including different levels of fixed effects as we move from Column (1) to Column (3). 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 or not.

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

An important issue that we flagged in Section 3 is that focussing on a sample of children who do not move between grades 6 and 9 might induce sample selection biases. To circumvent this problem, we provide intention to treat estimates, using movers and stayers but assigning to movers the grade-9 characteristics of the neighbourhood in which they lived at grade 6 (as described in Section 3.1). 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. In Table 5 we experiment 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 earlier claim that small-scale geographical fixed effects minimise the risk from endogenous changes in neighbourhood quality.

Finally, we 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). Evidence from regressions analogous to those in Table 2 – but using these adult-based indicators – gave coefficients close to zero and insignificant, implying no neighbour-peer effects related to the adult composition of the neighbourhood.

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

Up to this point, we have only investigated whether KS2-to-KS3 value-added is related to neighbourhood changes over the same period. However, students' educational progress could respond more to changes at different points over the grade 6-9 period. We therefore investigated whether there are heterogeneous effects from the three different grade-on-grade changes in neighbourhood composition, i.e. grade 6 to 7, grade 7 to 8 and grade 8 to 9. The results (not tabulated) are in line with the other results so far, although there 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.[17]

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. Results based on neighbourhood changes over up to five years confirm our previous findings: irrespective of the neighbour-peer proxy considered, there is no evidence that variation in neighbourhood composition affects the gains in achievement of students.

We also allowed for time lags in the process by studying whether grades 9-to-11 (KS3-to-KS4) value-added is affected by grade 6-to-9 or grade 8-to-10 changes in the neighbourhood composition. Furthermore, we looked at students' value-added in primary school, replicating the analysis in Table 2 for the grade 2 to grade 6 (KS1 to KS2) phase (results not tabulated). Once again, we found no evidence of neighbour-peer effects on students' test score progression. These results are not tabulated for space reasons, but are available upon request.

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[17] 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.

7. Heterogeneity, non-linearities and complementarities

The results from the linear-in-means specifications presented so far show that, on average, changes in neighbour-peer composition do not influence students' test score gains. However, this headline result might mask heterogeneity and non-linearity along a number of dimensions. As discussed in the Introduction, these issues are relevant because 'mixed neighbourhoods' policies that aim to improve overall students' outcomes are predicated on strong assumptions about the second-order partial derivatives of the functions describing these neighbourhood effects. In this section, we exploit the size and coverage of our census data to investigate heterogeneity, complementarities and non-linearities in neighbour-peer effects.

Table 6 presents our first set of results, with Columns (1a)-(1b) to (4a)-(4b) exploring heterogeneity in pupils' 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. Next, Columns (5a)-(5b) to (8a)-(8b) of Table 6 present heterogeneity by neighbourhood type. Specifically, we separately consider areas with: *(i)* above/below median student numbers; *(ii)* above/below median population density; *(iii)* above/below median housing over-crowding[18]; and *(iv)* percentage of social housing tenants above/below 75%. Out of the sixty-four estimates presented in the table, only six are significant at conventional levels. These show that: *(i)* a larger fraction of SEN students negatively affects students with high KS1 achievements; *(ii)* a larger fraction of FSM students lowers non-SEN and female students' test-scores; *(iii)* a larger fraction of boys improves other boys' achievements; and *(iv)* 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. Note that most of these estimates are only significant at the 5% level and that the effect sizes are very small.

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. Our results also suggest that neighbour-peer effects are more pronounced for students in urban areas (captured by high population density; Column 6b), although we find no evidence of this in relation to urban disadvantage as measured by overcrowding (Column 7b) or concentrated social housing (Column 8b).[19]

A number of checks in relation to non-linearities and threshold effects similarly failed to yield significant effects or notable patterns. The findings are not tabulated for space reasons, but are available

[18] 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).

[19] 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.

upon requests. Specifically, we added changes in the 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 composition changes to cause asymmetric effects, but found little evidence of such heterogeneity with the exception of the effect of average KS1 grades of neighbour-peers: while positive changes do not have a significant effect, negative changes have a perverse, positive but quantitatively negligible (at 0.0001) impact on students' value-added, borderline significant at the 5% level. Finally, we allowed large-negative, negative, positive and large-positive changes to have heterogeneous effects on students' test-score value added but still failed to find evidence of any significant non-linearity.

As a last exercise, we investigate 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. (2012b) find negative effects from 'bad' peers at the very bottom of the ability distribution and heterogeneous effects from very 'good' peers at the top of the ability distribution. In order to replicate this design, we investigate whether changes in the shares of top/bottom 10% neighbour-peers (in the national KS1 student distribution) affect students' KS2-to-KS3 value-added. 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, and on students with different levels of KS1 attainments.

Figure 3 presents some related results, where we look at the effect of the interaction between changes in the KS1 achievements of neighbour-peers and students' own KS1 test scores. This graphical analysis is in the spirit of Hoxby and Weingarth's (2005) analysis for school peer effects. Specifically, the plots show the estimated standardized effects (and associated 95% confidence interval) of changes in neighbour-peers' KS1 attainment – either average KS1 (Panel A), or the percentages of neighbours with KS1 scores in the top decile (Panel B) and bottom decile (Panel C) – against students' own KS1 deciles. The graphs are obtained from one single regression of pupils' KS2-to-KS3 value-added on all three indicators of neighbour-peer KS1 interacted with dummies for students' own KS1 deciles. The empirical specification is comparable to the one in Column (7) of Table 2, although we control for students' own KS1 decile instead of his/her own average KS1 test score.

With some imagination, we can detect a weak upward trend in the response of KS2-to-KS3 value-added to an increase in the proportion of top-10% KS1 neighbour-peers, and a weak downward trend in the response to an increase in the proportion of bottom-10% KS1 neighbour-peers. These results imply some vague positive complementarities between high achieving neighbour-peers and high achieving students, and some weak negative interactions between low achieving neighbour-peers and low achieving students. Mean neighbour-peer KS1 scores (conditional on the percentages in the top and bottom deciles) show no strong patterns across the distribution of students' own KS1 test scores, although the effects of neighbours' average KS1 achievements are positive for students in the central deciles. However, note that we cannot reject the null of joint equality of the coefficients in any of these settings, or the null that the coefficients are jointly equal to zero.

In conclusion, this more detailed analysis does not reveal any patterns that were not evident in the linear-in-means estimates. Overall, there is little evidence of any significant neighbour-peer effects or of complementarities which would justify mixing neighbourhoods as a policy to improve overall student achievements. Similarly, we find no sign of 'bad apple' neighbour-peer effects from the lowest achievers.

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

In order to consider potentially more interesting effects of neighbour-peer composition on behaviour, we next use information collected in the Longitudinal Survey of Young People in England (LSYPE) linked to the NPD-based neighbour-peer variables used so far. Given the time-window covered by the LSYPE, we consider the effect of neighbourhood changes on outcomes between grade 9 and grade 11. Since KS1 test scores are not available for the LSYPE cohort, we use KS2 scores of neighbouring students as a proxy for neighbour-peer achievements. Table 7 reports the results. Note that previous evidence in the literature has shown marked heterogeneity by gender, so we report estimates from separate regressions for boys and girls.[20] All models include the standard set of controls and secondary school fixed effects. The construction of the behavioural outcome variables was discussed in Section 4.4. Descriptive statistics for the LSYPE sample are provided in Appendix Table 5, both for the behavioural variables and for the student and neighbour-peer characteristics. These figures suggest that despite the fact that the LSYPE sample is much smaller than one previously considered, it is still representative of the student population and displays enough variation in the variables of interest.

Columns (1) and (2) of Table 7 display 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 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

[20] 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 exploratory analysis showed little heterogeneity in the effect of neighbourhood composition on behavioural outcomes along these dimensions.

(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 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 graffiti, vandalism, shoplifting, 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. Our results (not tabulated) 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 different effects for male and female youths re-assigned to better neighbourhoods by the MTO experiment. More broadly, a growing 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. (2012b) 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 a less robust empirical specification 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 looked at the effect of the characteristics and prior achievements of neighbourhood peers on the educational achievements and behavioural outcomes of secondary school students in England. In our main administrative dataset, we track four cohorts of over 1.3 million students through the first three years of their secondary schooling. The unique features of our population dataset – i.e. coverage and density –

data have allowed us to make a number of important empirical contributions, besides presenting novel evidence on the effect of peers in the neighbourhood. Firstly, we have drilled down to the effect of neighbourhood changes that are caused by movements of families in an out of small neighbourhoods. We have tracked these changes through information on the detailed residential addresses of our census of students. This is a new strategy to address the sorting problem in neighbourhood research. Secondly, 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 have been 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 changes in the mix of people in the residential neighbourhood (i.e. a 'contextual effect'; Manski, 1993) as originally advocated by Moffit (2001). Thirdly, by exploiting the detail and density of our data, we have been 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. The English institutional setting, where secondary school attendance is not tightly linked to place of residence, further allowed 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. Finally, the recent literature has focussed on estimating linear effects for homogenous and narrowly defined groups to aid identification. In contrast, our strategy and dataset have provided us with a unique opportunity to investigate heterogeneity and non-linearities in these responses at an unprecedented level of detail.

In summary, 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 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 non-linearities, 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 be effective. Student achievements and qualifications 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. This suggests that future research on the effects of social interactions in neighbourhoods should focus on outcomes other than teenage educational attainments.

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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
Annual rate of outward mobility in n'hood (grade 6 to 9)	0.081	0.057
Annual rate inward mobility in n'hood (grade 6 to 9)	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 neighbourhood 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: KS3-KS2 value-added/Grade 6 to 9					
	Movers 'ITT' set-up		'Central cohort' only		Adjacent OA n'hoods	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: N'hood Average KS1						
KS1 score –	0.003	0.000	0.001	-0.000	0.005	-0.001
Change (Grade 6 or 9)	(0.001)**	(0.001)	(0.001)	(0.001)	(0.001)**	(0.001)
Panel B: N'hood Share of FSM						
Share FSM –	-0.005	-0.001	-0.003	-0.001	-0.003	0.001
Change (Grade 6 or 9)	(0.001)**	(0.001)	(0.001)**	(0.001)	(0.001)**	(0.001)
Panel C: N'hood Share of SEN						
Share SEN –	-0.002	-0.001	-0.001	-0.000	-0.004	-0.000
Change (Grade 6 or 9)	(0.001)*	(0.001)	(0.001)	(0.001)	(0.001)**	(0.001)
Panel D: N'hood Share of Males						
Share Males –	0.001	0.001	0.000	0.001	0.002	0.002
Change (Grade 6 or 9)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)*	(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 neighbourhood over four years; average rate inward mobility in neighbourhood 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 and complementarities 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)
	Student KS1 < Median	Student KS1 > Above	Non-FSM Student	FSM Student	Non-SEN Student	SEN Student	Female Student	Male Student	Small N'hood	Large N'hood	Low Density N'hood	High Density N'hood	Low Over-crowd.	High Over-crowd.	Low Social Renter	High Social Renter
Panel A: N'hood Average KS1																
KS1 score Change Grade 6 to 9	-0.001	0.001	0.000	-0.001	0.000	-0.003	0.000	-0.001	-0.001	0.001	-0.001	0.001	-0.002	0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.005)
Panel B: N'hood Share of FSM																
Share FSM Change Grade 6 to 9	-0.001	-0.002	-0.001	-0.002	*-0.002	0.000	*-0.002	-0.000	-0.001	-0.001	0.001	**-0.003	-0.002	-0.001	-0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.004)
Panel C: N'hood Share of SEN																
Share SEN Change Grade 6 to 9	0.001	-0.002	-0.001	0.001	-0.001	0.002	-0.002	0.001	-0.001	-0.000	0.001	*-0.002	0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)*	(0.001)	(0.002)	(0.001)	(0.002)	(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	0.000	0.001	0.001	0.001	0.001	-0.001	*0.002	0.001	-0.000	0.002	-0.000	-0.000	0.002	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.005)

Note: Table reports standardised coefficients and standard errors obtained from regressions pooling all students and interacting individual or neighbourhood 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: approx. 582,000/726,000 respectively. Number of FSM/Non-FSM students: approx. 203,000/1,106,000, respectively. Number of SEN/Non-SEN students: approx. 279,000/1,031,000 respectively. Number of male/female students: approx. 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: approx. 674,000/635,000 respectively. Population density, housing over-crowding and share of households socially renting derived from GB Census 2001 at the OA level. Number of students in high/low density neighbourhoods (above/below median): approx. 656,000 in both cases. Number of students in neighbourhoods with high/low residential over-crowding (above/below median): approx. 656,000 in both cases. Neighbourhoods with a high share of social renters 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: approx. 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.

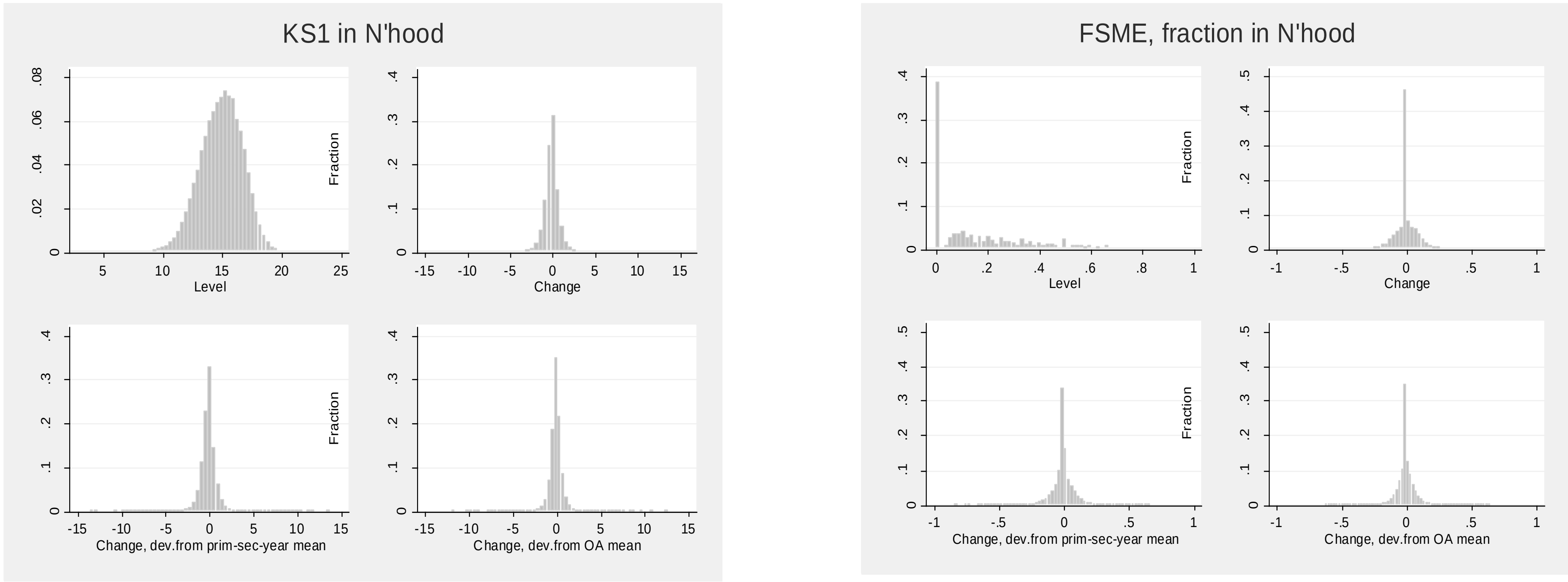
Figures:

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			

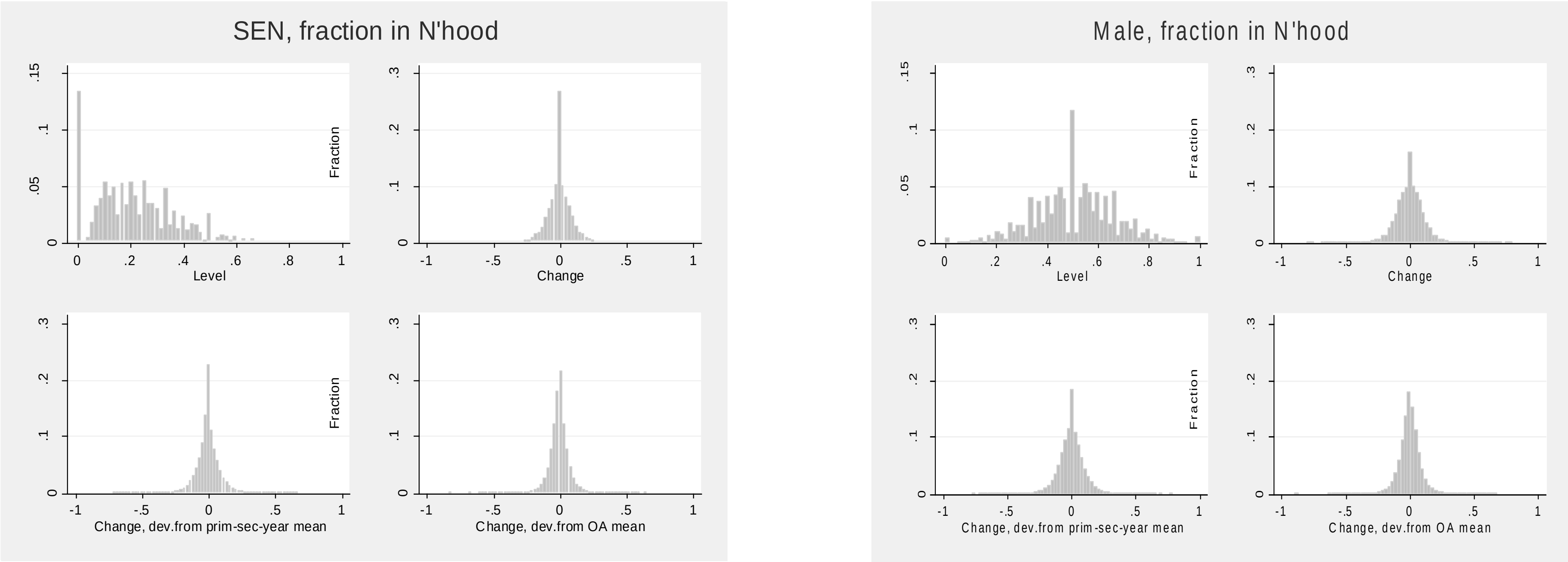
Note: Shaded cells refer to the estimation sample; immediately adjacent non-shaded cohorts represent the additional set of students used to construct measures of quality of neighbourhood. PLASC refers to the Student Level Annual School Census. Students finish their primary school in grade 6 when they 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.

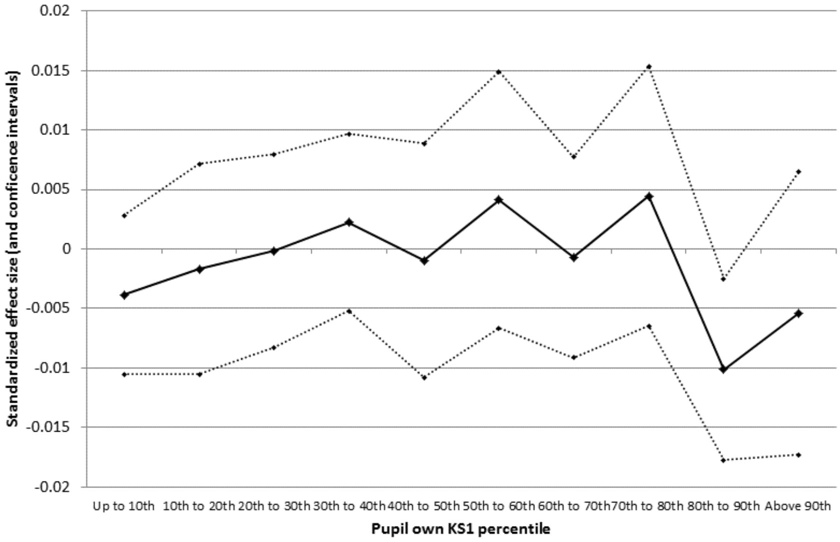
Figure 2b: Characteristics of students in the neighbourhood and amount of variation: special education needs (SEN) and share of male students



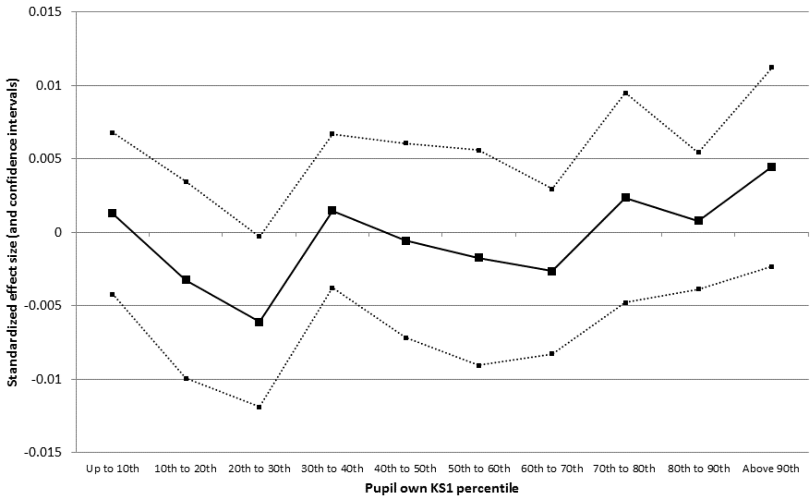
Note: Descriptive statistics of deviations from primary-by-secondary-by-cohort mean changes are as follows. Fraction of SEN students: mean 0.000, std.dev. 0.078. Fraction of Male students: mean 0.000, std.dev. 0.093. Descriptive statistics of deviations from Output Area mean changes as follows. Fraction 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: Complementarities between neighbours' and student's KS1 in the effect on neighbourhood changes on student KS2-KS3 value-added; a graphical representation

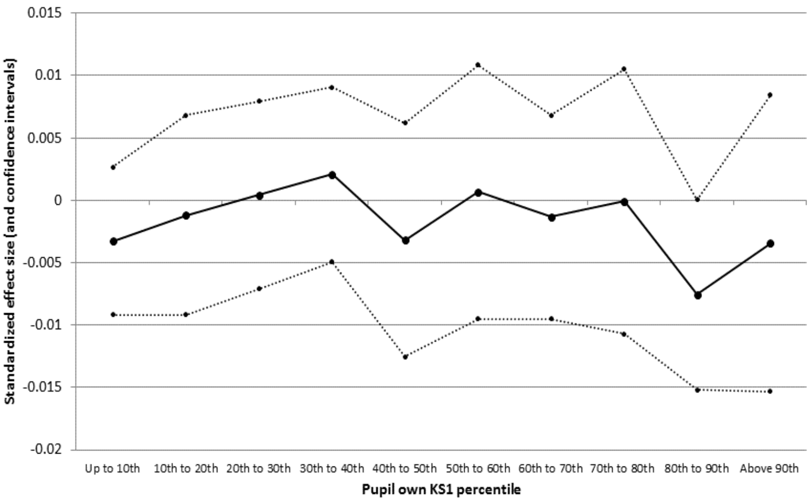
Panel A: Mean neighbour KS1 score



Panel B: Proportion of neighbours in top KS1 decile



Panel C: Proportion of neighbours in bottom KS1 decile



Note: The plots present standardized regression coefficients and 95% confidence intervals from regressions simultaneously including the three proxies for neighbourhood composition. More details are provided in Section 7.

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 B: Additional 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
Annual rate of outward mobility in n'hood (grade 6 to 9)	0.098	0.075
Annual rate of inward mobility in n'hood (grade 6 to 9)	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
Annual rate of outward mobility in n'hood (grade 9 to 11)	0.050	0.069
Annual rate of intward mobility in n'hood (grade 9 to 11)	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).