ISSN 2045-6557

CEE DP 122

Do Neighbours Affect Teenage Outcomes?

Evidence from Neighbourhood Changes in England

Stephen Gibbons

Olmo Silva

Felix Weinhardt

November 2010

Published by

Centre for the Economics of Education

London School of Economics

Houghton Street

London WC2A 2AE

Executive Summary

There are large disparities between the achievements, behaviour and aspirations of children growing up in different neighbourhoods. This has contributed to the view that neighbourhoods can determine individuals' outcomes. Notably, in the long run these effects could lead to larger social inequality and reduce social mobility, which is why they have attracted much attention among researchers and policy makers alike. In fact, many area-based policy responses are predicated on the idea that a young person's outcomes can be causally linked to the characteristics of the childhood neighbourhood, and to the social interactions with children and adolescents who live around him/her.

While economists and sociologists have proposed a number of theories to explain potential causal links between place of residence and socio-economic outcomes, empirical evidence has been largely inconclusive. This is because empirical neighbourhood effects research is complicated by two sets of problems.

The first problem is that the observed correlation between children's outcomes and neighbourhood characteristics could just be a statistical artefact resulting from general income segregation. It is a well established fact that parents sort into neighbourhoods according to their preferences and incomes. The problem arises because children's outcomes such as school results are also correlated with parental income. Parental sorting will hence automatically produce some degree of segregation along children's outcomes. Empirical research has to carefully control for this selection in order to make claims about causality.

Secondly, there is ambiguity in the definition of what constitutes a neighbourhood. As a result, empirical studies have used very different spatial aggregations to delimit the unit of analysis. These range from blocks of few houses with only a handful of people in each area to census tracts with over four thousand inhabitants. It is unclear how changing the spatial scale of analysis affects estimates of neighbourhood effects. Similarly, even after defining the spatial unit of analysis, we do not know whether everyone in that area is 'relevant'. Are social-interactions and role-model effects driven by other children of similar age or just by general characteristics of adults in the same neighbourhood? Very detailed data is needed to avoid these potential pitfalls.

With these issues in mind, we take advantage of a very detailed and spatially disaggregated dataset to examine the existence of social interaction effects in neighbourhoods in England. More specifically, our goal is to answer the following questions:

- To what extend are school test scores at ages 14 and 16 influenced by the academic quality and other characteristics of other children of similar age who live in the same neighbourhood?

- To what extend are behavioural outcomes of children - such as attitudes towards school truancy, substance use and anti-social behaviour - affected by the academic achievement and other characteristics of children in the neighbourhood?

We are able to address these questions using various datasets on neighbourhoods and multiple cohorts of children in secondary schools in England. Our data allows us to start from a very small unit of analysis and progressively aggregate up areas to assess the robustness of our findings in relation to the spatial scale of analysis. Furthermore, we can use alternative age groups of children to define our neighbourhood quality variables, for example by focussing only on same-age children or on children in the neighbourhood who are one year older or younger. Furthermore, we can test for potential heterogeneity along both the individual and neighbourhood dimensions. Finally, we can carefully control for parental sorting and other neighbourhood level correlated effects by including individual, neighbourhood and school-by-cohort effects in our empirical specifications, as well as unobserved neighbourhood trends. Notice that our source of variation comes from changes in neighbourhood composition generated by residential mobility, and our main estimates identify neighbourhood effects for pupils who do not move. However, we carefully discuss the suitability of this strategy in terms of its internal and external validity, and provide a series of robustness checks to support our approach.

Our initial results confirm the existence of a strong *cross-sectional* association between neighbourhood characteristics and children's outcomes. However, these findings cannot be interpreted as causal and mainly reflect a spurious correlation that arise because of individual sorting and neighbourhood unobserved attributes. In fact, once we control for pupil and family background unosbervables as well as neighbourhood fixed effects, our previously significant estimates become very close to zero and non-significant. In a nutshell, our main results are as follows:

- There is no evidence that neighbours' characteristics have a causal effect on the cognitive outcomes of 14 to 16 year old children arising from social interactions and role models.
- There is weak evidence that the neighbourhood effects are causally linked to young people's behavioural outcomes. However, there is some interesting heterogeneity along the gender dimensions regarding attitudes towards school and anti-social behaviour.

All in all, this evidence is in line with the most robust research in the field that identifies neighbourhood effects using randomised control-trials experiments such as the Movement to Opportunity intervention or the Gautreaux programme.

Do Neighbours Affect Teenage Outcomes?
Evidence from Neighbourhood Changes in England

Stephen Gibbons
Olmo Silva
Felix Weinhardt

Acknowledgments

Stephen Gibbons is Research Director at the Spatial Economics Research Centre, a Senior Lecturer in Economic Geography at the Department of Geography and Environment, London School of Economics (LSE), and a Research Associate at the Centre for Economic Performance, LSE and at the Centre for the Economics of Education. Olmo Silva is a Lecturer at the Department of Geography and Environment, London School of Economics (LSE), and Research Associate at the Spatial Economics Research Centre, the Centre for Economic Performance, LSE and CEE. Felix Weinhardt is a Research Assistant at the Spatial Economics Research Centre and the Centre for Economic Performance, LSE.

1 Introduction

There are evidently significant disparities between the achievements, behaviour and aspirations of children growing up in different neighbourhoods (Lupton et al., 2009). More importantly, these neighbourhood disparities have the potential to lead on to longer run inequalities in labour market outcomes and life chances (Sampson et al., 2002), and have become a centre of attention for researchers and policy makers who are concerned with addressing socioeconomic inequalities. In fact, many area-based policy responses, such as the 'Mixed Communities Initiative' (MCI) announced in 2005 in England, are predicated on the idea that a young person's outcomes can be causally linked to the characteristics of the childhood neighbourhood, and to the social interactions with children and adolescents who live around him/her (see discussions in Currie, 2006 for the US, and Cheshire et al., 2008 for the UK). However, the question of whether differences between children's outcomes are truly causally related to the type of people amongst whom they live remains difficult to answer. The empirical literature in the field has put forward a range of conclusions, which depend on the outcome under analysis and the empirical strategies employed.[1]

As is well known, the question of whether children are influenced by the groups to which they belong is challenging to answer for a number of reasons. Firstly, the vast majority of the data used to address this question does not contain detailed information about friendship networks and group belonging, which means that researchers have to use alternative strategies to approximate the level at which interactions take place and role models exert their influence. When investigating the presence of neighbourhood effects, this requires: *(a)* to make assumptions about the geographical scale where some mutual influence might occur, for example within a census block; and *(b)* to identify a set of individuals among which interactions take place or role models exert their effects, for example young people of the same age. However, as pointed out by Moffit

[1] For example, Kling et al. (2005) and (2007) analyse the 'Moving to Opportunity' (MTO) randomized control-trial intervention and find no evidence of neighbourhood effects on young people's educational achievement, but heterogeneous effects for boys and girls on behavioural outcomes, such as crime and self-reported health. Oreopolous (2003) exploits quasi-random allocation in social housing in Canada to look at the long-run employment impact of growing up in deprived areas, and find little evidence of neighbourhood effects. Finally, Goux and Marin (2007) use an institutional argument similar to Oreopolous (2003) and instrumental variables to document large neighbourhood effects on the educational outcomes of French teenagers.

(2001), data limitations imply that the majorities of the proxies used to study neighbourhood effects are very crude, and failing to find significant results does not imply that these effects do not exist at smaller or more precisely measured levels of interaction.

A second major challenge to this line of research is posed by the fact that the characteristics of children living in a certain neighbourhood are closely interwoven with those of their parents who will have chosen where to live on the basis of their preferences for local amenities and services, the income at their disposal and other constraints they face. In fact, the abundant literature on the link between school quality and house prices (e.g. Black, 1999 and Gibbons et al., 2009) shows that people are willing to pay a significant premium to access 'better' schools (as well as other amenities; see Kain and Quigley, 1975, and Cheshire and Sheppard, 1995), and suggests that neighbourhoods will be stratified along the lines of income and socio-economic background. This sorting means that one child's characteristics – both observed and unobserved – will be correlated with those of his/her neighbours as well as other attributes of the local area – such as the quality of its schools or the presence of a park or a library. This implies that it will be hard to disentangle the causal influence of neighbours' characteristics and behaviour over and above children's own inherent attributes, the characteristics of their parents and other external influences that are neighbourhood-specific (Manski, 1993 and Moffitt, 2001 formally discuss these issues).

Statistical studies using survey data have adopted a number of strategies to control for family background, child characteristics and other external influences, and have documented quite strong effects on child outcomes from the characteristics of neighbours. However, questions remain about the credibility of their identification strategies and the ability to isolate exogenous variation in neighbourhood characteristics. On the other hand, large scale experiments which employ an explicit randomisation strategy, such as the 'Moving to Opportunity' (MTO) programme (Kling et al. 2005 and 2007, and Sanbonmatsu et al., 2006), have found insignificant effects on young people's educational outcomes, but small a significant impact on behavioural outcomes, such as involvement in criminal activities and self-reported health. Nevertheless, even randomized control-trial experiments suffer from 'design problems'. As pointed out by Moffitt (2001), while these neighbourhood-reassignment programmes remove biases due to sorting, they do not solve problems arising because of correlated area unobservables. This is because the neighbourhoods to which households are relocated not only differ in terms of their socio-economic composition, but also in terms of their housing stock, labour market opportunities and school quality. Whereas for some policies it might be sufficient to estimate to combined effects of all these ‘treatments’, the presence of coincidental factors does not allow clean identification of the effects that arise because of neighbourhood interactions and role models. In fact, Moffitt (2001) suggests to reverse-engineer the evaluation of programmes like the MTO or the Gautreaux intervention (Rosenbaum, 1992), and to study changes in the outcomes of the original residents of the areas receiving relocated households. For these people, contextual factors will remain approximately unchanged, but the neighbourhood composition will be affected by the influx of new families.

In our study, we use census data on various cohorts of students in England matched to detailed information on place of residence and schools attended over several years to deal with the measurement and identification issues discussed above, and neatly identify the effect of neighbourhood composition on teenagers’ educational and behavioural outcomes. We believe our study makes several contributions to the literature in this field. From the measurement point of view, we differ from the vast majority of previous studies in that we can start from a very small scale, and then experiment with both the definition of peer group and the geographical scale of neighbourhoods to assess the robustness of our findings. In particular, we begin by defining as ‘peers in the neighbourhood’ students who live in the same Census Output Area (OA) and who are either of the same age (i.e. age 11 at the beginning of our observation window) or one year younger/older (from age 10 up to age 12) than one another. OAs contain around 125 households and on average approximately 5 students of the same age, or 13 students in the ‘same age +1/-1’ bracket. Since our identification approach relies on fixed-effects to control for neighbourhood unobservables, such a small scale is desirable in order to minimise the risk of endogeneity of neighbourhood quality. However, exploiting the density of our census data covering the whole of the student population and the national territory, we can experiment with alternative approaches including only focussing on students of exactly the same age, or focussing on larger geographical areas encompassing groups of adjacent OAs.

Our study also makes some significant advances from an identification point of view. In this respect, we follow the literature on peer effects in schools (e.g. Hoxby, 2000, Hanushek et al., 2003, Gibbons and Telhaj, 2008, Lavy et al., 2008) that exploits naturally arising exogenous cohort-to-cohort variation in group composition to identify social interaction effects. In our case, this variation originates from the movement of people between neighbourhoods which changes the characteristics of the local area for those residents that 'stay put'. In a nutshell, we estimate the effect of these mover-induced changes in the neighbourhood composition on the evolution of the educational and behavioural outcomes of the 'stayers'. For these people, most of the neighbourhood contextual factors remain unchanged, so that we are able to partial out local fixed unobservables, such as the presence of a library or other highly localised infrastructures/amenities. Note that this approach is the non-experimental counterpart of what Moffitt (2001) proposes to identify social interaction effects arising from the MTO programme, i.e. study the effect of changes in the neighbourhood composition driven by movers on the outcome of the original residents (the 'stayers'). [2]

Nevertheless the approach just described might not take into account other unobservable factors which could affect teenagers' outcomes and their neighbourhoods' evolution. First, changes in the area composition driven by people's mobility might be picking up more general neighbourhood trends dictated by gentrification process, or by the progressive decline of certain areas. Secondly, although explicit catchment areas are not predominant in the admission of primary and secondary school students, pupils living in the same neighbourhood tend to attend a localised group of schools because of travel costs and other considerations. On average, same-age pupils living in the same OA (usually sampling five such students) attend two to three different secondary schools, and each secondary school samples students on average from around sixty different OAs (out of more than 165,000 in England). This suggests that co-movements in the outcomes of neighbouring students could be driven by the fact that they attend a similar set of schools, and/or that changes in the neighbourhood composition could be driven by sorting of neighbours in response to changing school quality. In order to mitigate these problems, we exploit the fact that we can track several cohorts of students as they progress from primary through secondary education and experience changes in the neighbourhood composition over a number of years. This allows us to further control for local-area trends, as well as school-by-cohort effects, and identify neighbourhood effects clean of any environmental contextual time-fixed or time-trending factors, and net of any cohort-specific school-related shocks.

[2] Moreover, this is not dissimilar from the approach of Hanushek et al. (2003) who study the effect of students' between-school mobility on pupils who do not change school.

In terms of findings, our research shows that the large cross-sectional association between young peoples' outcomes and neighbourhood composition – in terms of prior achievement, eligibility for free school meals (an indicators for low family income) and special education needs (a proxy for learning disabilities) – is mostly eradicated once we control for neighbourhood fixed-effects by considering changes in neighbourhood composition over time. Furthermore, any remaining association is eliminated once we control for school-by-cohort specific shocks and/or allow for neighbourhood-specific time-trends. In order to enrich our analysis, we also look carefully for evidence of non-linearities in the relationship between pupils' test scores and neighbourhood composition, and for complementarities between neighbourhood and pupil characteristics, but find no significant associations. We also test the robustness of our results along a number of dimensions, including changes to the geographical definition of neighbourhoods, changes to the age-grouping used to identify local peers and changes to the period under analysis (e.g. the time-span covering the age 11 to age 14 period, as opposed to age 11 to age 16). None of these modifications reverses our main conclusion that neighbourhood effects are not a significant determinant to pupils' educational achievements. Similarly, we found that neighbourhood composition only exerts a small effect on pupil behavioural outcomes, such as attitudes towards schooling and anti-social behaviour, although we detect some heterogeneity along the gender dimension.

The rest of the paper is structured as follows. Section 2 briefly reviews the literature on the topic, while Section 3 describes our empirical strategy. Section 4 discusses that data that we use and the English institutional context. Next, Sections 5 and 6 discuss our findings, while Section 7 provides some concluding remarks.

2 Literature Review: Previous Methods and Findings

While neighbourhood effects could arise for a number of reasons, economists have put substantial emphasis on peer group and role model effects (Akerlof, 1997 and Glaeser and Scheinkman, 2001), social networks (Granovetter, 1995 and Bayer et al., 2008), conformism (Bernheim, 2004 and Fehr and Falk, 2002) or local resources (Durlauf, 1996). Disappointingly though it has proved very difficult to distinguish between these competing theories empirically and research has mainly concentrated on estimating reduced-form effects. Even then, issues related to the endogenous sorting of people across neighbourhoods, local unobservable factors and reflection problems imply that reduced-form estimates of neighbourhood effects might still be potentially biased (see Manski, 1993 and Moffitt, 2001 for a general discussion).

To overcome these problems, recent studies have used a number of identification approaches that can be broadly grouped into four alternative empirical strategies. In detail, non-experimental research has: *(a)* tried to find suitable instruments for neighbourhood quality (Cutler and Glaeser, 1997, Goux and Maurin, 2007); *(b)* relied on institutional arrangements related to social renters who cannot choose where to live and move freely across social housing projects (Gibbons, 2002, Oreopolous, 2003, Jacob, 2004, Goux and Maurin, 2007, Weinhardt, 2010); and *(c)* used fixed-effects estimations to partial out individual, family and aggregate unobservables (Aaronson, 1998, Bayer et al., 2008). Finally, there have been a number of experimental studies looking at randomised control-trial interventions, namely the 'Gautreaux' and 'Moving to Opportunity' programmes (Rosenbaum 1995, Katz et al. 2005 and 2007, Sanbonmatsu et al. 2006).

Overall, the experimental literature tends to find negligible effects on educational outcomes, but some positive effects on behavioural outcomes, such as involvement in criminal activities or health status. Similarly, Oreopolous (2003) and Jacob (2005) find no evidence of neighbourhood effects on labour market and educational outcomes. On the other hand, Goux and Maurin (2007) provide evidence of large neighbourhood effects on the education outcomes of teenagers in small neighbourhoods in France, and Aaronson (1998) finds significant effects from neighbourhood poverty measures and incidence of drop-outs on high-school students' graduation rates.

However, the insights and implication arising from different studies are often difficult to compare because of the lack of clarity about the mechanisms through which neighbourhood effects are thought to operate. In most cases, the focus lies on identifying a reduced-form effect, and the distinction between competing explanations, in particular social interactions as opposed to local resources and infrastructures, is simply brushed aside[3]. For example, Goux and Maurin (2007) do

[3] An exception is Gould et al. (2004) who are primarily interested in school effects on the educational outcomes of Ethiopian immigrants in Israel. They show that additional neighbourhood level variables have no explanatory power.

not control for the quality of local schools and other neighbourhood infrastructures. Similarly, Kling et al. (2005) and (2007) and Sanbonmatsu et al. (2006) do not address the fact that families who move into a better neighbourhoods as part of the MTO programme simultaneously experience changes to their peer group composition as well as changes in local resources – such as school quality – and labour market opportunities. Nevertheless, role models and social interactions could influence behaviour in the place of residence irrespective of differences in local infrastructures and resources, and the existing empirical literature has not taken a stance on this issue leading to some confusion about what constitutes a 'neighbourhood effect'. Notably, it is not uniformly agreed whether differences in outcomes driven by local school quality constitute a neighbourhood effect or not, even though this distinction has important policy implications.

To be clear from the outset, our study specifically aims at estimating neighbourhood effects that arise from social interactions and role models at the place of residence, and net of potential confounding effects such as differences in local school quality (e.g. school resources, teaching methods, but also quality of its intake) and other local infrastructure/resources. To this end, we exploit the richness of our data which allows us to estimate neighbourhood effects while controlling for neighbourhood fixed-effects, neighbourhood trends and school-by-cohort effects. The next section spells out our empirical strategy in detail.

3 Empirical Strategy

General identification strategy: a changes-in-changes specification

The estimation of neighbourhood effects is greatly complicated by the sorting of individuals across neighbourhoods in relation to both observable and unobservable local factors (see Manski, 1993 and Moffitt, 2001 for a formalisation). Indeed, families do not randomly choose where to live, rather they sort across different areas as a result of their preferences for local amenities/services, and the income at their disposal to pay for these via the housing market (see Black, 1999, Gibbons et al., 2009, and Cheshire and Sheppard, 1995). These considerations suggest that neighbourhoods will be stratified along the lines of income and family background, and that there will be a strong degree of correlation between the characteristics of neighbours as well as between neighbourhood factors and the characteristics of its residents. Without properly accounting for these issues, estimates of neighbourhood effects are likely to be biased by the fact that family characteristics are also likely to be correlated with a range of outcomes of interest, including school achievements, labour market outcomes and attitudes towards education.

In our work, we concentrate on identifying the effect of neighbours' characteristics on young people's educational achievements and behavioural outcomes during secondary schooling. In order to address the identification issues discussed above, we use a fixed-effects design that allows us to eliminate unobserved components that could jointly determine neighbourhood composition and pupils' outcomes. These methods are closely related to previous work on neighbourhood effects, peer effects and mobility (Hoxby, 2000, Hanushek et al., 2003 and 2004, Lavy et al., 2009, Gibbons and Telhaj, 2007, Gibbons and Telhaj, 2008). However, a novelty of our study is that we explicitly restrict any measured neighbourhood variation to that caused by movements of pupils in our sample from one neighbourhood to another. Moreover, the size of our administrative population-wide data and the fact that we observe multiple cohorts means that we can control carefully for unobserved neighbourhood fixed-effects, neighbourhood-specific unobserved time-trends and school-by-cohort specific shocks. The rest of this section sets out our empirical model more formally.

To begin with, assume that the outcome *y* of pupil *i* living in neighbourhood *n,* attending school *s*, living in cohort *c* and measured at year-group (or age) *t* can be expressed as:

$$y_{insct} = \alpha_i + \mathbf{z}'_{nct}\,\beta + \mathbf{x}'_{it}\,\gamma + \mathbf{x}'_{it}\,\delta t + + \phi_n + \xi_n t + \vartheta_{sct} + \varepsilon_{insct} \tag{1}$$

In which α_i is an unobserved individual level effect, $\mathbf{z}_{nct}$ is a vector of neighbourhood composition variables, $\mathbf{x}_{it}$ is a vector of observable pupil characteristics at age *t*, ϕ_n is an unobserved neighbourhood level effect, $\xi_n t$ represents a neighbourhood-specific trend, and ϑ_{sct} is a school-by-age-by-cohort specific shock. The error term ε_{insct} is assumed to be uncorrelated with all the right hand side variables, and endogeneity issues arise because the other unobserved components are all potentially correlated with $\mathbf{z}_{nct}$ and $\mathbf{x}_{it}$.

In order to eliminate some of the components that could jointly determine neighbourhood composition and outcomes such as test scores, we exploit the fact that we can observe students as they progress from primary to secondary education, and measures both their outcomes and the composition of the neighbourhood where they live at several stages. In particular, we observe students and their place of residence at ages 11, 14 and 16. We therefore take differences between data observed at two different periods of time and estimate the following equation:

$$\left(y_{insc1}-y_{insc0}\right)=\left(\mathbf{z}'_{nc1}-\mathbf{z}'_{nc0}\right)\beta+\mathbf{x}'_{it}\delta+\xi_n+\left(\vartheta_{sc1}-\vartheta_{sc0}\right)+\left(\varepsilon_{insc1}-\varepsilon_{insc0}\right) \qquad (2)$$

Where the subscripts *t=0* and *t=1* identify the initial and last period of observation (e.g. ages 11 and 14), and the exact time-window varies according to the outcome under consideration. Notice that when we estimate this model we restrict our attention to pupils that *do not move* neighbourhood so that we can eliminate the unobserved neighbourhood fixed-effect ϕ_n. However, focussing on 'stayers' could give rise to sample-selection issues and bias our estimates of neighbourhood effects. To mitigate these concerns, in our robustness checks we consider all students and assign to 'movers' the change in the neighbourhood quality they would have experienced had they not moved. In this set-up, our estimates of the neighbourhood effects are more properly interpreted as 'intention-to-treat' measures.

The specification described in equation (2) allows us to eliminate all unobserved components that are fixed over time for pupils and their residential neighbourhoods, including those driving sorting of families across different neighbourhoods. However, this specification does not control for unobserved neighbourhood-specific time trends ξ_n and/or school-related shocks $\left(\vartheta_{sc1}-\vartheta_{sc0}\right)$. Conceptually, the first term relates to general neighbourhood dynamics dictated by gentrification processes, and/or by the progressive decline of certain areas. On the other hand, the second component relates to cohort-and-age specific changes to school quality and characteristics that might affect young people's outcomes as well as changes in the neighbourhood composition. In fact, although zoning is not predominant (nor explicit) in the admission of students to primary and secondary schools, pupils living in the same neighbourhood tend to attend a localised group of schools because of travel costs and other considerations.[4] This suggests that co-movements in the changes in the outcomes of neighbouring students could be driven by the fact that they attend a similar set of schools, and/or that changes in the neighbourhood composition could be driven by sorting of neighbours in response to changing school quality. More specifically, our concern is that changes in pupils' outcomes and neighbourhood characteristics between *t=0* and *t=1* – in particular between ages 11 and 14 – might be driven by cohort-specific changes in school characteristics, resources or composition as students move from primary to secondary schools, and families relocate across neighbourhoods during this period of school transition.

In order to address these issues, in some of our specifications we further control for neighbourhood-specific trends ξ_n by differencing from neighbourhood means across cohorts *c*, and/or by including secondary-by-cohort or primary-by-secondary-by-cohort fixed-effects.[5] Our identifying assumption is therefore that the remaining idiosyncratic shocks to pupil outcomes (after eliminating pupil fixed-effects, neighbourhood level effects, neighbourhood trends and school-by-cohort specific effects) are uncorrelated with the changes in neighbour composition experienced by pupil *i* as he/she stays in their residential neighbourhood between *t=0* and *t=1*.

Anticipating our results, our evidence provides support for our identification assumptions. A set of balancing regressions shows that the changes in the neighbourhood characteristics that we use to estimate the neighbourhood effects are not strongly related to time-fixed predating neighbourhood characteristics or the average characteristics of the students staying in the neighbourhood, even before we allow for neighbourhood unobserved trends or school-by-cohort effects. This lends strong credibility to our identification strategy.

[4] On average, pupils in the same age-group and living in the same small neighbourhood (hosting five such students) attend two to three different secondary schools.

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

Defining neighbourhoods and measuring their characteristics

Research on neighbourhood effects shares may of the empirical issues that the literature on peer effects has had to face in terms of identifying peer groups and measuring peers' characteristics, but has the additional complication of having to define the 'right scale' of the neighbourhood. While there is some discussion of whether the effects of social interactions should be measured at the grade or class level in the peer effects literature (see Ammermueller and Pischke, 2009), there are no similar natural boundaries such as 'the school' or 'the grade' that define the area of interest in the case of neighbourhoods. Consequently, what has been used to measure neighbourhood effects has varied greatly regarding the geographical size. Goux and Maurin (2007) speculate that using large neighbourhood definitions – i.e. US Census tracts containing on average 4000 people – leads to an underestimate of interaction effects. However, over-aggregation on its own will not necessarily attenuate regression estimates of neighbourhood effects since any reduction of the covariance between mean neighbours' characteristics and individual outcomes is offset by a reduction in the variance of average neighbours' characteristics. Nonetheless, it is crucial that the neighbourhood group definition includes relevant neighbours, and in this respect a larger neighbourhood definition might be better than a small one if the small group is mis-specified. All in all, whether or not the level of aggregation matters in practice is an empirical question, and we take full advantage of the detail and coverage of our population-wide data to experiment with alternative definitions.

To being with, we take a very small scale perspective and use Census 'Output Areas' (OA) to define our geographical unit of analysis. OAs contain 125 households on average and approximately five students in the same age-group (e.g. five age-11 pupils), and in total there are around 160,000 OAs in England. Compared to the existing literature, this is a very small unit. Notice that since our identification approach relies on fixed-effects to control for neighbourhood unobservables, a small scale is desirable to minimise the risk of endogeneity of neighbourhood quality (that is, it is less likely that there are unobserved neighbourhood changes over time within-streets, than within-regions). Nevertheless, we also construct larger geographical areas based on this underlying OA-geography. In particular, we create neighbourhoods for each individual OA including the OA itself and all directly adjacent OAs. These neighbourhood aggregates on average encompass seven output areas (more detail in the data section)[6]. This allows us to tackle the problem of defining a suitable spatial unit in neighbourhood research in a highly flexible way.

Another advantage of our study is that we observe the population of English school children[7]; and therefore we can construct neighbourhood quality measures including peers in a variety of year groups. Since we are interested in social interactions in the neighbourhood, we believe neighbourhood quality measures should be constructed aggregating the characteristics of students of similar age. Consistently, in the majority of our paper we construct neighbourhood indicators using individual level data from pupil who are either of the same age group (i.e. age 11 at the beginning of our observation window) or one year younger/older (from age 10 up to age 12). However, we perform a number of checks using different age-bands, for example by including only students of the same age.

Note finally that we use information on pupils' characteristics that pre-date the period of our analysis, and only use students appearing in every year of the analysis with non-missing information to create aggregate neighbourhood characteristics. This implies that the changes in area composition that we exploit to identify neighbourhood effects are driven by pupils within our sample moving across neighbourhoods, and not by students dropping out/coming into our sample or changes in their characteristics. The complex data that we use in order to pursue this analysis is described in the next section alongside the English institutional background.

4 Institutional Context and Data Construction

The English school system

Compulsory education in England is organized into five stages referred to as Key Stages. In the primary phase, pupils enter school at age 4-5 in the Foundation Stage, then move on to Key Stage

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

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

1 (KS1), spanning ages 5-6 and 6-7 (these would correspond to the 1st and the 2nd year in other educational system, e.g. in the US). At age 7-8 pupils move to KS2, sometimes – but not usually – with a change of school. At the end of KS2, when they are 10-11 (6th year), children leave the primary phase and go on to secondary school, where they progress through KS3 (7th to 9th year) and KS4 (10th to 11th year) till the age of 16 which marks the end of compulsory schooling. Importantly, the vast majority of pupils change schools on transition from primary to secondary education. This transition leads to considerable re-shuffling of pupils across schools, and as a result students encounter a large number of new students in their new school. Indeed, on average pupils in the first year of secondary education meet over 85% new peers coming from different primary schools.[8]

As for testing, at the end of each Key Stage, generally in May, pupils are assessed on the basis of standard national tests (SATS) and progress through the phases is measured in terms of Key Stage Levels, ranging between W (working towards Level 1) up to Level 5+ during primary education and Levels 7 and 8 at KS3 and KS4. SATS at the end of KS1 test knowledge in English (Reading and Writing) and Mathematics only and performance is recorded using Levels. On the other hand, at both KS2 and KS3 students are tested in three core subjects, namely Mathematics, Science and English and their attainments are recorded in terms of the raw test scores, spanning the range 0-100, from which the Key Stage Levels are derived. 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 performance is measured using 'point scores' (varying between 0 and 8). These are indicators of total achievement at this final stage of compulsory schooling, and are based on allocating points to different grades, and aggregating across types of qualifications using appropriate weights.[9]

Finally, note that admission to both primary and secondary schools is guided by the principle of parental choice. Indeed, since the Education Reform Act of 1988, the 'choice model' of school provision has been progressively extended in the state-school system in England (Glennerster, 1991). In this setting, pupils can attend any under-subscribed school regardless of where they live

[8] This is also due to the fact that there are many more primary schools (about 15,000) than secondaries (around 3,000).

[9] Details on the weighting procedures are available from the Department for Education (formerly Department for Children, Schools and Families) and the Qualifications and Curriculum Authority.

and parental preference is the deciding factor. However, if the number of applicants exceeds the number of available places, other criteria which are not discriminatory, do not involve selection by ability and can be clearly assessed by parents, can be used by schools to prioritize applicants. These vary in detail, but preference is usually given first to children with special educational needs, next to children with siblings in the school and to children who live closest. For Faith schools, regular attendance at local designated churches or other expressions of religious commitment is foremost. Because of these criteria – alongside the constraints of travel costs – residential choice and school choice decisions are very closely linked (see some related evidence in Gibbons et al, 2008 and 2009, and in Allen et al., 2010). Even so, most households will have a choice of more than one school available from where they live. Indeed, on average, pupils in the same-age bracket (e.g. age-12 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).

Main data construction and descriptive statistics

To estimate the empirical models specified in equations (1) and (2), we draw our data from the English National Pupil Database (NPD). This dataset is a population-wide census of students maintained by the Department for Education (formerly Department of Children Schools and Families) and holding records on SAT test scores and schools attended at the various Key Stages for every state-school pupil from around 1996 to the present day. Importantly, since 2002 the database has been integrated with a Pupil Level Annual School Census (PLASC, carried out in January), which holds records on pupils' background characteristics such as age, gender, ethnicity, special education needs and eligibility for free school meals. The latter is a fairly good proxy for low income, since all families who are on unemployment and low-income state benefits are entitled to free school meals (Hobbs and Vignoles, 2009). Crucially for our research, PLASC also records the home postcode of each pupil on an annual basis. A postcode typically corresponds to around 15 contiguous housing units on one side of a street, and allows us to assign pupils to common residential neighbourhoods and to link them to other sources of geographical data. In particular, we use data from PLASC to map every pupil's postcode into the corresponding Output

Area. As already mentioned, OAs encompass small residential areas of around 125 households, and we make use OAs to identify our smallest scale neighbourhoods, track residential mobility over time, and aggregate individual characteristics into neighbourhood 'quality' measures.

The main focus of our analysis will be the period spanning age 11 (end of KS2) to age 14 (end of KS3), where we will relate neighbours' characteristics to corresponding students test-score achievements (other time periods and outcomes will also be used; see next). The main advantage of concentrating on this time-window and outcomes is that at both KS2 and KS3 students sit for SATs in English, Mathematics and Science and performance is recorded similarly (see discussion here above). We exploit this feature to construct measures of pupils' test-score value-added which allow us to estimate the changes-in-changes specification spelled out in equation (2). In particular, we begin by averaging each student's performance at KS2 and KS3 across the three compulsory subjects, and then convert this average into percentiles of the cohort-specific national distribution. We then create KS2-to-KS3 value-added by subtracting age-11 from age-14 percentiles.

With this data at hand, we construct neighbourhood quality indicators by aggregating individuals' characteristics from PLASC up into OA averages. To begin with, we construct neighbourhood indicators using individual level data from all pupil who live in the same OA and are either of the same age group (i.e. age 11 at the beginning of our observation window) or one year younger/older (from age 10 up to age 12). This choice is dictated by the idea that students of similar age are more likely to interact and/or be influenced and hold similar role models. However, since this choice is not uncontroversial (see discussion in Section 3.2), we will also perform a number of checks using different age-bands and geographical units. Given the time-span of the NPD-PLASC integrated dataset and the requirements that this approach imposes on the data, we are able to track four extended cohorts of students as they progress through education. These include students aged 10 to 12 (years 5 to 7, including KS2/year 6) in the academic years 2001/2002, 2002/2003, 2003/2004 and 2004/2005, turning 13 to 15 (years 8 to 10, including KS3/year 9) in academic years 2004/2005, 2005/2006, 2006/2007 and 2007/2008. Figure 1 provides a graphical representation of this time-window and highlights how the different extended cohorts overlap.

Using NPD/PLASC information for these pupils, we construct the following neighbourhood aggregates: *(i)* Average KS1 score in English (Reading and Writing) and Mathematics; *(ii)* Share of students eligible for free school meals (FSME); *(iii)* share of students with special education needs (SEN); *(iv)* Fraction of males in the students' population.[10] Note that KS1 test scores and gender for a given child clearly do not change over the duration of our sample, but FSM entitlements and SEN status are time-varying. Since we are interested in isolating changes in the neighbourhood quality that arise because of students' residential mobility, we fix the value of the FSM and SEN status to what is recorded in the first year when students are observed in the data when constructing neighbourhood means. This avoids conflating neighbourhood composition changes due to movements of pupils across OAs with changes in the status of pupils who are not moving. Note also that we keep a balanced panel of students with non-missing information in all years, so that neighbourhood quality changes are driven by the same pupils moving in and out of the local 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.

After constructing neighbourhood aggregates, we only retain in our analysis pupils in the four 'central' cohorts, namely students that sat their KS2 exams in the academic years 2001/2002, 2002/2003, 2003/2004 and 2004/2005, and the KS3 tests in the years 2004/2005, 2005/2006, 2006/2007 and 2007/2008. Furthermore, we restrict our attention to students in non-selective schools and students in neighbourhoods with at least 5 students in the 'central cohort' +1 year/-1 year bracket that we use to aggregate individual characteristics into neighbourhood variables. Finally, we concentrate on pupils who live in the same OA over the period year 6 to year 9, which we label as the 'stayers' (note that we will address the issue of potential endogenous sample-selection caused by focussing on the 'stayers' in our robustness checks). After applying these restrictions, we obtain a balanced panel of approximately 1.3 million students spread over four cohorts. Descriptive statistics for the main variables are provided in Table 1.

Starting from the top, Panel A presents summary statistics for the characteristics of the 'stayers'. By construction the KS2 and KS3 percentiles average at around 50, with a standard deviation of about 25 points, while the average KS2-to-KS3 value-added is just above one. This is due to the fact that by dropping pupils who change neighbourhood we exclude students with lower value-

[10] We do not observe immigrant status and so cannot perform an analysis similar to Edin et al. (2003) and (2010).

added, although the differences are not marked (see Appendix Table 1).[11] Further, about 15 percent of the students are eligible for free school meals (FSME), 21 percent have special educational needs (SEN) and 50 percent are male. Additionally, the average secondary school size is around 1080 students, and the rates of inward and outward neighbourhood mobility are similar and close to 8 percent. The fact that these two measures are very close is not surprising since we focus on a balanced panel of students, and inward movements in one area must correspond to outflows from another.

Next, Panel B of Table 1 presents the neighbourhood level characteristics, as well as statistics for the changes between Years 6 and 9 (age-11/KS2 to age-14/KS3). Unsurprisingly, the levels of the shares of FSME, SEN and male students are very similar to those of the underlying population of students (see Panel A). Note however that we proxy for academic ability by using age-7/KS1 test scores, since these are predetermined to our time-window. As discussed above, performance at this stage is measured in levels, which can be mapped into numbers representing expected terms of progress to be achieved by students. The average age-7/KS1 score of students in our sample is approximately 15, which is in line with the national average (see Appendix Table 1). Note also that our neighbourhoods sample on average around 14 students in the 'same-age +1 year/-1 year' age band, or 5 pupils of exactly the same age. This means that relative to most of the previous research in the field, we focus on highly localised neighbourhoods.

Turning to the changes in the neighbourhood characteristics, these are centred on zero for all variables. This is not surprising since they are calculated using the same set of pupils with the same constantly-held background characteristics in both Years 6 and 9, and moving across a set of pre-determined neighbourhoods. However, the most important point to note from the table relates to the amount of variation we have in our 'treatment' variables, once we take differences to partial out neighbourhood fixed-effects. Looking at the figures, we see that the standard deviation of KS1 scores is 1.76, while the change in this variable between Years 6 and 9 has a standard deviation just over 0.86. This suggests that 24% of the variance in the average KS1 scores is within-OA over time. The corresponding percentages for the shares of FSME, SEN and male students in the neighbourhood are 16%, 31% and 41%, respectively. Figures 2a and 2b illustrate

[11] Appendix Table 1 presents descriptive statistics for pupils in the 'central cohort' before we focus on the 'stayers' and discard pupils in small neighbourhoods. A comparison of Table 1 with Appendix Table 1 reveals that pupils and neighbourhoods in our sample are broadly representative of the students' population and England as a whole.

this point further by plotting the distributions of the neighbourhood mean variables: *(a)* in levels (top left panels), *(b)* in changes over time (top right panels), *(c)* in changes and further controlling for primary-by-secondary-by-cohort school-effects (bottom left panels); and *(d)* in changes and further controlling for OA fixed-effects, that is netting out neighbourhood trends (bottom right panels). All these figures suggest that there is considerable variation in neighbourhood characteristics over time, from which we can expect to be able to identify our coefficients of interest. Furthermore, including school-by-cohort or OA fixed-effects does not lead to a drastic reduction in the variation we can exploit. This makes it unlikely that changes in the findings from progressively including additional layers of fixed-effects are simply driven by lack of variation. Note finally that Appendix Figure 1 plots the distribution of the students' population in our neighbourhoods, as well as its changes between Years 6 and 9 and the rates of inwards and outwards mobility. The plots are quite smooth, which is of further reassurance that changes in our neighbourhood characteristics are not driven by some outliers, break-points or discontinuities.

Before moving on, we briefly discuss the construction of some auxiliary data that we use to perform some robustness checks. The definition of 'peers in the neighbourhood' held so far includes all pupils of the same age and one year younger or older living in the same OA. In order to check the validity of this approach, we also construct neighbourhood quality measures based on: *(a)* pupils in 'central' cohorts only – i.e. aged 11 at the beginning of our time-window – living in the same OA, but disregarding pupils who are one year older/younger; *(b)* pupils in the 'central cohort +1/-1 year' window, but living a set of adjacent OAs to the OA of the pupil under analysis (and including the OA of residence). These extended neighbourhoods include on average 6 to 7 OAs and approximately 80 pupils. Next, consider that our method of constructing neighbourhoods implies that when we include neighbourhood fixed-effects in equation (2) to control for unobservable trends, the cross-cohort differences in neighbourhood characteristics are not just driven by the same underlying set of pupils moving across areas, but also by the sampling of different pupils (because of their different ages) from the same neighbourhood. This is because one of the three age groups used to construct the neighbourhood mean characteristics for a child in cohort *c* (the oldest group in the 'central cohort +1/-1' window) will not be used to construct the neighbourhood aggregates for pupils in cohort *c+1*, and will instead be replaced by a younger group of pupils (the youngest in the 'central cohort +1/-1' group). See Figure 1 for some graphical insights. As an alternative, we propose to use exactly the same age groups to build neighbourhood mean characteristics for every child in a given neighbourhood in any given year. Appendix Figure 2 shows how this can be achieved. Using this alternative age-bracketing, we guarantee that any difference in neighbourhood changes across children in different cohorts is generated purely by movements of neighbours in and out of the area, and not by re-sampling from a static population. However, as highlighted by Appendix Figure 2, the drawbacks of this method are that: *(a)* it limits us to use one less cohort in our sample; and *(b)* it imposes on our neighbourhood measures an 'asymmetric' structure, assigning younger neighbours to children in the oldest cohorts and older neighbours to children in youngest cohort.[12]

Additional datasets

Our main analysis looks at the age-11/KS2-to-age-14/KS3 time-window using four cohorts of pupils, but we also consider other time horizons and different outcomes.

To start with, the combined PLASC/NPD allows us to extract two cohorts to study the effect of changes in the neighbourhood characteristics for a longer period covering the age-11/KS2 to age-16/KS4 period. Note that when we look at this period, we construct neighbourhood aggregates using pupils in the 'central cohort' only and residing in the same OA. On the other hand, we do not consider students who are one year younger or older. This is because the KS4 marks the end of compulsory secondary education in England, and this implies that many students drop out of our sample since they do not stay on at school. In order to avoid contaminating our measures of the neighbourhood characteristics for age-16 students by only including those age-17 students that continue past compulsory education (along side with age-15 and age-16 students), we focus only on students of the same age in the process of aggregation. As for the other characteristics and information that we keep, they are identical to those we retained for the pupils in the age-11/KS2 to age-14/KS3 time-window and their descriptive statistics are very similar to those in Table 1. The only difference concerns students' outcomes at KS4. As already mentioned, achievements at this stage are recorded on a zero to eight scale. In order to make these test scores comparable with

[12] Note that we checked the descriptive statistics of all these alternative datasets and found that they are still broadly representative of the national sample. These figures are not tabulated for space reasons, but are available upon request.

previous attainments and construct measures of value-added, we average students' performance across the three subjects and then convert these measures into percentiles in the cohort-specific national distribution. This choice does not seem controversial and has been previously used when analysing these data (e.g. Gibbons and Silva, 2008).

One limitation of relying on administrative data such as the integrated PLASC/NPD is that these only include test-score information that can be used to construct outcomes to investigate the presence of neighbourhood effects. However, previous research in the field (Kling et al., 2005 and 2007) suggests that behavioural outcomes are more likely to be affected by neighbourhood characteristics. In order to investigate this issue, we make use of the Longitudinal Study of Young People in England (LSYPE), which sampled approximately 14,000 students aged 14 in 2004 (one cohort only) in 600 schools, and followed them as they progressed through their secondary education up to age 16 and beyond. The LSYPE surveyed students on a number of aspects about their life at school, at home and in their neighbourhood, and contains a number of questions related to behavioural outcomes that we exploit for our analysis. Note that most of the questions involved a binary answer of the type "Yes/No". In order to generate more variations in students' responses and gain some precision when estimating the effect of neighbourhood characteristics, 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: *(a)* 'Attitudes toward schooling' 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)'; *(b)* 'Playing truant' which is the binary outcome from the question 'Did you play truant in the past 12 months (Yes=1; No=0)'; *(c)* '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 *(d)* '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)'.

Importantly, the survey also contains precise information about pupils' place of residence, which means that we can merge into this data the neighbourhood aggregates that we have constructed using the students in the PLASC/NPD and used in our main analysis. Note also that, given the age of the pupils covered by the LSYPE, we can only consider the effect of neighbourhood change on outcomes between age-14 and age-16, and that for the same reasons highlighted here above, we construct neighbourhood quality measures only including pupils of the same age.[13] Furthermore, age-7 test scores for this cohort were not available (due to the data time-span) so that we aggregate the levels of the age-11/KS2 test scores of neighbouring students.

Descriptive statistics for the LSYPE sample are provided in Appendix Table 3, both for the behavioural variables discussed above, as well as for the pupil and neighbourhood characteristics. All in all, these suggest that despite the fact that this sample is much smaller than our previous data, it is still representative of the national population and displays enough variation in the variables of interest.

5 Main Results

Neighbours' characteristics and pupils' test score: cross sectional and causal estimates

Table 2 presents our main results on the association between the characteristics of 'peers in the neighbourhood' and pupils' test scores. In the table, peers in the neighbourhood are students of the same age as well as one year younger/older (i.e. age 13, 14 and 15 at KS3) and living in the same OA. Neighbours' 'quality' is proxied using four different variables, namely: average KS1/age-7 test scores (*Panel A*); share of FSME pupils (*Panel B*); share of students with SEN status (*Panel C*); and share of male pupils (*Panel D*). Note that the estimates in the four panels are obtained from different regressions entering one treatment at the time. Further, the first four columns of the table present results from regressions that only include cohort effects, whereas Columns (5) to (8) control for pupils' own characteristics (KS1 test scores, FSME and SEN status and gender), as well as school size, school type dummies and average rates of inward and outward mobility in the neighbourhood. The note to the table provides more details. Finally, the table reports standardised coefficients with standard errors clustered at the OA level in round parenthesis.

---

[13] Note that we cannot construct measures of the neighbourhood 'quality' by aggregating the characteristics of the LSYPE students since we have too few LSYPE pupils in any neighbourhood to construct meaningful averages.

Starting from Column (1), the table shows the cross-sectional relation between neighbourhood characteristics at age 14 and students' own test scores at KS3. All four treatments are strongly and significantly associated with pupils' educational achievements, and enter our specifications with the expected signs, except for the fraction of males in the neighbourhood. A one standard deviation increase in the age-7 achievements of neighbouring pupils is associated to an increase in pupils' own test scores at age-14 of approximately 30% of one standard deviation; similarly, a one standard deviation increase in the fraction of pupils eligible for FSM or with SEN status is linked to a reduction in students' own performance by 20-30% of a standard deviation. As for the fraction of males, this seems to have a small positive effect at approximately 0.4% of one standard deviation for one standard deviation change in the treatment. The sign of this relation is somewhat 'unexpected' given the evidence on gender peer effects in schools which tends to document a positive impact from a larger fraction of females (Hoxby, 2000 and Lavy and Schlosser, 2007).

However, as we discussed in Section 3, cross-sectional estimates are most likely biased, since families with similar observable and unobservable attributes will sort into similar neighbourhoods, giving rise to a spurious association between pupils' achievements and the characteristics of their neighbours. Indeed, when we add to our specification some pupil, school and neighbourhood controls, our estimates become 60-70% smaller (compare Columns (1) to (5)), and completely loose their significance in one case (see Panel D). This pattern clearly suggests that cross-sectional models do not identify any casual relation rather they capture a high degree of sorting into neighbourhoods based on observable and possibly unobservable factors.

In order to alleviate this problem, we estimate the model in first-differences described by equation (2), where we relate one pupil's test score changes between age 11 and age 14 to the corresponding changes in the characteristics of neighbouring peers. This model allows us to partial out pupil and family unobserved factors, as well as neighbourhood time-fixed unobserved effects. Note that, as we already discussed, we estimate this model focussing on the sample of pupils that do not change place of residence between ages 11 and 14. However, later in this section, we check the robustness of our results to the inclusion of 'movers'.

Starting from the results in Column (2), these models show that the association between changes in neighbours' characteristics and pupil KS2-to-KS3 value-added is much smaller than in the cross-sectional models of Columns (1) and (5), and only significant in two out of the four panels. A one standard deviation change in the KS1 test scores of neighbours and in the faction of FSME students in the residential area is linked to a 0.3-0.5% of a standard deviation change in students' test score progression. These effects are 20-25 times smaller than the corresponding cross-sectional estimates from models that control for pupils' background characteristics, school attributes and neighbourhood mobility (compare with Column (5)). As for the fractions of pupils with SEN status and male students in the OA of residence, these variables are no longer significantly associated with pupils' KS2-toKS3 value-added, and their estimated effects are very close to zero. Once again, this pattern suggests that the cross-sectional results discussed above are substantially biased by student, family and neighbourhood unobservable attributes.

Furthermore, comparing Columns (2) and (6) shows that, once we consider models in differences, adding controls does not significantly affect our results. Only the effect of the fraction of neighbours with SEN status becomes statistically significant (at the 5% level), even though the point estimate is unchanged. This is very reassuring since it suggests that changes in neighbourhood composition are not strongly linked to pupils' background characteristics (and other controls), and lends some initial support to our identification strategy which relies on changes in the treatment variables to be 'as good as random' once we partial out student and neighbourhood fixed-effects. The next section presents more formal evidence on this point.

Nevertheless, as discussed in Section 3, it is still possible that the estimates from these simple first-differences models might be biased. One concern is that changes in the neighbourhood composition experienced by students between age-11/KS2 and age-14/KS4 might be driven by families 'shopping around' and changing neighbourhoods to gain access to better schools for their children on transition from primary to secondary education. In order to control for this possibility in a flexible way, the estimates in Columns (3) and (7) further control for primary-by-secondary-by-cohort effects that absorb any cohort-specific unobserved shock to changes in school 'quality' when moving from the primary to the secondary phase, including changes in school resources, teachers, instruction methods, as well as school composition and peer quality. Results from these specifications show that neighbourhood composition is *not* significantly related to students' test score value-added, and that this is true irrespective of whether or not we add the usual set of controls. Importantly, the loss in significance is not due to a dramatic increase in the standard errors associated to our estimates, rather to the magnitude of these effects shrinking towards zero. This further 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. Similarly, these 'zero effect' results can hardly be justified by measurement error (biasing our estimates towards zero) being exacerbated by the inclusion of a large number of fixed-effects (approximately 190,000 primary-by-secondary-by-cohort groups). As shown by the estimates in Appendix Table 2, including secondary school fixed-effects (around 3200 groups) or secondary-by-cohort effects (approximately 12,000 groups) similarly drives our estimates to zero.

One final concern is that even conditional on school-by-cohort effects, our estimates might be biased by the presence of neighbourhood specific unobserved time-trends that simultaneously drive resident pupils' attainments and neighbourhood change. Note that school-by-cohort effects do not necessarily account for these trending unobserved attributes since there is not a one-to-one mapping between neighbourhoods and schools: pupils from the same Output Area attend two to three different secondary schools every year, and every year secondary schools attract students from on average 60-70 different residential areas. This suggests that there is some scope for further augmenting our first-differences specifications with neighbourhood effects capturing local area trends. Results from these very stringent specifications that simultaneously control for secondary school-by-cohort effects *and* OA trends are presented in Columns (4) and (8), and fully confirm our previous findings.[14] Note also that, as shown in Appendix Table 2, accounting for OA trends only (without including school-by-cohort effects) yields virtually identical results. Since controlling for unobserved neighbourhood trends does not affect our main estimates once we have taken into account school-by-cohort effects, the analysis that follows will only consider first-differences models such as those presented in Columns (2) and (6) and specifications that further control for school cohort-specific unobservables, as in Columns (3) and (7).

[14] Including primary-by-secondary-by-cohort effects and OA trends proved computationally not feasible.

Assessing our identification strategy

The validity of our results rests on the assumption that the changes in residential area composition that we exploit to estimate neighbourhood effects are not related to the characteristics of pupils who live in the area, and more broadly to specific attributes of these neighbourhoods. As already mentioned, the fact that augmenting our first-differences specifications with some individual, school and neighbourhood controls does not affect our results lends some support to this intuition. However, in this section, we tackle this issue more systematically by providing evidence that our treatments are 'balanced' with respect to local area characteristics.

In order to do so, we collapse the information contained in our dataset to output-area (OA) averages. This is because, whereas our student level information varies at the individual level, changes in the neighbourhood composition only vary at the OA-by-cohort level and the broad set of neighbourhood characteristics that we use to test the balancing properties of our treatments only vary at the OA level. This data comes from the GB Census 2001, which collected a one-snapshot picture of the socio-economic composition of OAs in the UK. The information that we use includes: *(i)* the share of households living in socially rented accommodations; *(ii)* the share of households owning the place of residence; *(iii)* the share of adults in employment; *(iv)* the share of adults with no educational qualifications; *(v)* the share of lone parents in the OA population. Additionally, we construct from our dataset OA-averages of the pupils' controls included in our models (i.e. KS1 test scores, FSME and SEN status and gender), as well as the average and the standard deviation of the KS2 achievements of pupils living in the neighbourhood at age-11.

To test for the balancing properties of our treatments, we regress each of these OA aggregates on one of our proxies for changes in the neighbourhood composition. Results are reported in Table 3. In the top panel, we focus on the association between OA-averaged pupils' controls and neighbourhood changes, whereas in the bottom panel we look at KS2 achievements and the Census variables. Note that the regressions in Panel A only control for cohort and secondary school-type effects (where 'types' include: Community, Voluntary Aided, Voluntary Controlled, Foundation, City Technology College and Academy), whereas the regressions in Panel B add OA-level averages of the controls added in the specifications of Columns (4) to (8) of Table 2. The reason for including school-type effects in Panel A is that our identification actually relies on changes in neighbourhood characteristics to be 'as good as random' conditional on pupil and neighbourhood unobservable fixed-effects *and* school-by-cohort effects. However, it proved computationally infeasible to generate OA-averages of the latter, and we decided to include school-type effects only bearing in mind that our actual specifications can control for (cohort-specific) school unobservables in a more flexible way.

The results in the table present a very reassuring picture: the vast majority of the estimated relations are very small and insignificant in both Panel A and B. The only significant and meaningful associations that we detect are between the neighbourhood changes in the fraction of FSME students and OA-averaged students' KS1 test scores and eligibility for free lunches. The estimates suggest that areas with lower average KS1 achievements and a higher proportion of FSME students experience positive changes in the fraction of neighbours who are from a low-income family background (as captured by FSME). As already mentioned, this might be driven by families 'shopping around' for school quality on transition from primary to secondary school, or more generally by processes of gentrification experienced by some neighbourhoods. These would be controlled for by the school-by-cohort effects or neighbourhood trends in the specifications in Table 2 (Columns (3)-(4) and (7)-(8)). Since in our balancing tests we are not fully able to control for these unobservables, the results in Table 3 suggest that the estimates of Table 2, Columns (2) and (6) – and in particular those in the second panel – yield an upward biased estimate of the significance of neighbourhood effects. On the other hand, specifications that control for school-by-cohort effects should completely purge our results from any potential bias. To re-iterate, these provided no evidence of significant and sizeable effects of neighbourhood composition on pupil test-score value-added between KS2 and KS3.

Robustness checks: alternative definition of peers and neighbourhoods

In the analysis conducted so far, we have made use pupils belonging to the same cohort and in two adjacent cohorts (+1/-1 year) to average neighbourhood characteristics (see again Figure 1). However, as discussed above, it is not clear *a-priory* what constitutes a neighbourhood and who the relevant peers in the neighbourhood should be. In this section, we explore these issues and discuss a battery of related robustness checks. Our results are presented in Table 4, which tabulates the effects of changes in our proxies for neighbourhood composition (in four different horizontal panels) on pupils' test-score value-added between KS2 and KS3. All specifications include the usual set of controls, and the even columns further append primary-by-secondary-by-cohort effects to the first-differences specifications.

Starting from Columns (1) and (2), we investigate whether only including pupils from the 'central' cohort (i.e. students of exactly the same age) in defining neighbourhood characteristics affects our results. Our results suggest that, if anything, results are weaker when only considering pupils of the same age and confirm our previous conclusion that there is little evidence of a significant association between neighbourhood composition and pupils' test-score progression.

Another issue that we flagged in the data construction section was that by focussing on the sample of pupils that 'stay' in the same neighbourhood we might induce some bias in our estimates due to endogenous sample-selection.[15] To circumvent this problem, we estimate the first-differences specification in equation (2) using both 'stayers' and 'movers', and assigning to the movers the age-14 characteristics of their initial (age-11) neighbourhood. Stated differently, we assign them to the changes in the neighbourhood 'quality' that they would have experienced had they not moved. These 'intention-to-treat' results are reported in Columns (3) and (4) of Table 4, and only marginally differ from our previous findings. This greatly allays concerns related sample-selectivity issues induced by focussing on the 'stayers'.

Furthermore, as noted in the discussion of Figures 1 and Appendix Figure 2 in the data section, differences across adjacent cohorts in the changes in neighbourhood characteristics are not just driven by the same underlying set of pupils moving across areas, but also by the sampling of different pupils from the same neighbourhood. This could become problematic in some of our specifications (in particular those including OA trends and to some extent school-by-cohort effects), since deviations from neighbourhood trends are now potentially confounded by changes in the sample rather than actual mobility. To address this concern directly, we make use of a 'constant-cohort' dataset where we keep exactly the same set of pupils across three cohorts (see Appendix Figure 2 and Section 4.2) so that differences in neighbourhood changes across children in different cohorts are generated purely by movements of neighbours in and out of the area. The

[15] Note however that previous studies have followed a similar approach, e.g. Hanushek et al. (2004).

results from this robustness check are presented in Columns (5) and (6) of table 4 and confirm our previous conclusions. The only exception relates to the changes in the fraction of FSME students in the neighbourhood, which remains significant at the 5% level even after including primary-by-secondary-by-cohort effects. However, the effect is very small at 0.2% of a standard deviation of the KS2-to-KS3 value-added distribution for a one standard deviation change in this treatment.

In our last robustness check in this section, we go on to consider whether our results are affected when we define neighbourhoods using a larger geographical scale. Previous research has suggested that the lack of evidence on neighbourhood effects is potentially explained by the fact most US-based studies focus on large census-track aggregates, thus understating the importance of highly localised social interaction in the neighbourhood. However, as we discussed in Section 3.2, the bias due to aggregating over larger neighbourhoods depends on the changes in the covariance between the outcomes of interest and neighbourhood characteristics – plausibly going down as we aggregate to larger areas – and the variance of the treatment – most likely also going down as we aggregate over more observations and weaken measurement error. The last two columns of Table 4 present some related evidence by tabulating results from regressions that use measures of the neighbourhood composition computed over a set of adjacent OAs (on average six to seven OAs, including approximately 80 students). The results in Column (7) suggest that using aggregates computed over larger residential areas *increases* the precision and the size of our estimates. However, including school-by-cohort effects as in Column (8) brings our estimates close to zero and insignificant (with the exception of the changes in the share of males). This pattern is plausibly justified by the fact that changes in larger neighbourhood aggregates are more likely to be 'contaminated' by families changing residential location in relation to school access when their children start secondary education. This lends support to our claim that, since our identification approach relies on fixed-effects to control for neighbourhood unobservables, a small scale is desirable in order to minimise the risk of endogeneity of changes in neighbourhood 'quality'.

Heterogeneity and non-linearity in the estimated neighbourhood effects

Our results so far suggest that neighbourhood composition is not a significant determinant of pupils' value-added during the first three years of secondary schools. However, our headline results might mask a significant degree of heterogeneity along a number of dimensions. In this section, we analyse this issue by investigating whether our results differ for students with different background characteristics and living in different neighbourhoods. Our results are presented in Tables 5 and 6, where we deal with heterogeneity along the pupils' and neighbourhoods' attributes respectively. All regressions in the tables come from first-differences specifications of the type presented in equation (2) and include primary-by-secondary-by-cohort effects. Note that the estimates of these heterogeneous effects are obtained from regressions where we interact our treatments with a specific pupil/neighbourhood characteristic (e.g. the student is male/female), but otherwise restrict all other controls to have the same effect for the two groups. Also, we do not run separate regressions for different groups of pupils or neighbourhoods since we need to pool all students to consistently estimate the school-by-cohort effects.

Starting from Table 5, this presents the estimated neighbourhood effects for: *(i)* pupils with KS1 test scores above/below the median in the cohort-specific national distribution (Columns (1) and (2)); *(ii)* students who are/are not eligible for FSM (Columns (3) and (4)); *(iii)* pupils with/without SEN status (Columns (5) and (6)); *(iv)* and male/female students (Columns (7) and (8)). Out of the thirty-two estimates reported in the table, only four are significant at conventional levels and show that a larger fraction of pupils with learning difficulties (SEN) in the neighbourhood negatively affect pupils with high previous (KS1) achievements (Panel C, Column (2)), and that a larger fraction of neighbours from poor family background (FSME) lowers non-SEN and female pupils' test-score valued-added. Finally, a larger fraction of boys in the neighbourhood improve other males' achievements. However, all of these effects are only significant at the 5% level and do not capture sizeable effects. Moreover, they do not present a consistent picture with weaker/stronger students from poorer/wealthier family backgrounds being affected differently. All in all, these results lend support to our previous (negative) conclusions.

Next, in Table 6 we explore the heterogeneity of our results along the characteristics of the neighbourhoods where pupils reside. The structure of the table is similar to the previous one, but the breakdown is as follows: *(i)* small/large neighbourhood with pupil numbers above/below the median of the national distribution (Columns (1) and (2)); *(ii)* areas with population density above/below the median in the national distribution (Columns (3) and (4)); *(iii)* neighbourhood with above/below median housing over-crowding (Columns (5) and (6)); and *(iv)* areas with a percentage of social housing tenants above/below 75%. Note that information to create the breakdown in *(ii)-(iv)* is obtained from the GB Census 2001 at the OA level. The only two significant estimates show that an increase in the fraction of neighbours with FSM and SEN status has a significantly adverse effect on the value-added of pupils living high density neighbourhoods (see Column 4). To investigate these findings further, 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. Once again, our main conclusion is that neighbourhood effects are not an important determinant of students' educational achievements.

To conclude, we briefly discuss results regarding some possible non-linearities of the estimated neighbourhood effects. None of these findings is tabulated for space reasons, but a detailed set of results is available upon requests. To being with, we tackled this issue by including in our specifications the changes in the quadratic and cubic powers of the four neighbourhood 'quality' variables (e.g. the change in the squared fraction of FSME pupils). Alternatively, we included the squared and cubic powers of the changes in our proxies (e.g. the quadratic power of the change in the fraction of FSME pupils). In either case, we failed to find any significant effects. Next, we tested whether positive and negative changes carry different effects (e.g. an increase vs. a decrease in the average KS1 of peers in the neighbourhood), but did not detect any interesting or significant patterns. Furthermore, we split the changes in our treatments into four quartiles (e.g. large-negative, negative, positive and large-positive changes in the share of neighbours on FSME) to investigate non-linear effects in a flexible way, but still did not find any interesting dynamic.

Finally, one concern is that by looking at average KS1 achievement we might obscure the fact that very talented or very weak peers in the neighbourhood might have significantly positive or negative effect on the learning of other students (Lavy et al., 2009 provides some related evidence for peers in English secondary schools). To address this issue, we constructed two additional variables that measure the fraction of pupils in the neighbourhood that are in the top 10% (very 'good peers') and the bottom 10% (very 'bad peers') of the cohort-specific national distribution of KS1 test scores. When we used these proxies to test for the presence of ability-related neighbourhood effects, we still failed to document any sizeable and significant effect. All in all, our main conclusions remain unaffected: neighbours' ability and characteristics do not seem to affect students' test-score progression between ages 11 and 14.[16]

6 More Results: Later Educational Achievements and Behavioural Outcomes

Neighbourhood effects and pupils achievements at age 16

The analysis in Section 5 concentrated on the age 11 to age 14 time-window and on the KS2-to-KS3 value-added. In this section, we investigate whether we can detect significant neighbourhood effects for other outcomes and other time-horizons. To begin with, we consider pupils' attainments at age-16/KS4 and analyse whether students' value-added between age-11/KS2 and age-16/KS4 and between age-14/KS3 and age-16/KS4 is affected by the corresponding changes in neighbours' characteristics. The data used to estimate these models was discussed in Section 4.3. The most important issues to recall are that: *(i)* we can only construct aggregates of neighbourhood 'quality' using pupils of the same age (and not using 'same age +1/-1 year'), because compulsory school finishes at age 16 and we are not able to track peers who drop out from education; *(ii)* we can only use two cohorts, and as a consequence we replace school-by-cohort effects with secondary school fixed-effects in our specifications. Results obtained with more flexible controls for cohort-specific school shocks are virtually identical to those that we discuss here (available upon request).

A selection of our results is presented in Table 7. Columns (1) and (2) concentrate on the value-added between KS2 and KS4. For all four treatments, our results show that there is no significant association between progression through secondary education and changes in the neighbourhood composition, irrespective of whether or not we control for school unobservables. Next, in Column (3) and (4) we focus on the last two years of secondary education and look at the relation between neighbourhood characteristics and age-14/KS3 to age-16/KS4 value-added. Even then, we fail to find any significant association. Note that the results in Table 7 are not directly comparable to

[16] We also tested whether neighbourhood composition affects pupils' performance at different quantiles of the KS2-to-KS3 value-added distribution differently, but failed to find any heterogeneity.

those in Tables 2 and 4, since we are at the same time concentrating on neighbours in the same age group *and* looking at two different cohorts from those used to estimate the results discussed in Section 5.[17] Nevertheless, the overall conclusion is the same we reached above.

Finally, the model in equations (1) and (2) assumes that neighbourhood 'peer effects' affect students' achievements simultaneously, although one might plausibly argue that these effects take a while to materialise. To assess this issue, we investigated whether age-14/KS3 to age-16/KS4 value-added is affected by changes in the neighbourhood composition between ages 11 and 14 (i.e. over the KS2 to KS3 phase) and changes in the characteristics of neighbours between ages 13 and 15 (i.e. one-year lag with respect to the Key Stage tests). Even then, we failed to document significant neighbourhood effects.

Additional results on neighbourhoods and achievements: younger students or adult peers?

Our analysis has concentrated on the relation between teenage educational outcomes and the characteristics of peers of similar age in the neighbourhood of residence. This choice was motivated by the notion that social interactions and role models among young people of a similar age might be more significant than between youths and adults, and that teenagers might be more affected than younger students (e.g. those in primary schools) by their neighbours' characteristics because they 'hang out' together more and are freer from their families' control and mediation in the choice of peers and reference group. Nevertheless, we carried out some investigations to directly address these issues.

First, we investigated whether pupils' value-added in primary school is affected by the characteristics of the area of residence. To do this, we simply replicated the analysis in Table 2 (and some of the robustness checks and additional analysis of Section 5), but concentrating on the time-window spanning the age-7 to age-11 period, i.e. covering the KS1 to KS2 primary education phase. Even in this case, we failed to find any significant evidence that test-score progression at school is associated with changes in neighbours' characteristics.

[17] Note also that KS4 achievements are computed as the percentalised average point scores across English, Mathematics and Science. Results obtained using averages over all subjects taken at KS4 are virtually identical.

Next, we investigated whether pupils' test-score value-added during secondary education – mainly KS2 to KS3 progression – is associated to the characteristics of the adult population in the neighbourhood. This type of information is not readily available from the education dataset used so far, but was collected using (time-varying) information gathered by the Department for Work and Pension (DWP) on people claiming unemployment benefits and income support. More specifically, we were able to match to our four cohorts of students going through the first three years of secondary education some corresponding 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; *(iii)* the number of lone mothers on income support (a proxy for very low income among young un-married mothers). One drawback of this analysis is that this information is only provided at a higher level of aggregation, namely the Super Output Area (OA), encompassing around four OAs and including 500 households. Reassuringly, as we have shown in Section 3.3, the geographical detail of our neighbourhoods does not affect our conclusions.

In a nutshell, the results from this analysis suggest that adults' characteristics are not strongly associated with pupils' test-score progression. Once we control for school-by-cohort effects and/or super-OA unobserved trends, our estimates become very small and insignificant. This suggests that interactions with adults in the place of residence and/or the role models that these individuals project on teenage students do not significantly affect their school outcomes.

Neighbourhood characteristics and behavioural outcomes: evidence from the LSYPE

Although the absence of a significant link between educational achievements and neighbourhood composition is perhaps surprising, our findings square well with the most robust experimental evidence on the effect of neighbourhoods on students' school outcomes (Sanbonmatsu et al., 2006). However, this field of research has also shown that neighbourhoods can have (sometime perverse) effects on young people's non-cognitive and behavioural outcomes, including involvement in criminal activities, educational aspirations, self-reported measures of physical health and proxies for life-satisfaction and wellbeing (see Kling et al., 2005 and 2007).

In order to consider these aspects, we make use of information collected in the Longitudinal Survey of Young People in England (LSYPE). As discussed in Section 4.3, the LSYPE sampled approximately 14,000 students aged 14 in 2004 (one cohort only) in 600 schools, and followed them as they progressed through their secondary education. The Survey collected information on a number of aspects about students' life and aspirations from which we can construct a set of behavioural outcomes measured consistently at ages 14 and 16. Additionally, the LSYPE contains information about pupils' place of residence, which means we can merge this data to the neighbourhood aggregates that we have constructed using students in the PLASC/NPD.

We report the results from our investigation in Table 8. Note that given the time-window considered by the Survey, we can only consider the effect of neighbourhood change on outcomes between age-14 and age-16. Moreover, age-7 test scores for this cohort are not available, so we aggregate the levels of the age-11/KS2 test scores of neighbouring students to proxy for prior their academic ability. Finally, since previous evidence has shown a significant degree of heterogeneity along the gender dimension, we report estimates from separate regressions for boys and girls. All models include the standard set of controls and secondary school fixed-effects. The note to the table contains more detailed information.

Starting with Columns (1) and (2), these tabulate the relation between neighbourhood changes and the composite variable 'Attitudes towards schooling' for boys and girls respectively. This proxy measures whether students think school is worth going, whether they plan to stay in school after the end of compulsory education and whether they think school is a waste of time. Starting from the top, we see that an improvement in age-11/KS2 achievements of neighbouring pupils positively affect 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 only significant and sizeable 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 pupils in the neighbourhood who are eligible for FSM nor the share of males affect other students' views of education.

Next, in the four central columns of the table we investigate the relation between neighbourhood composition and students' absences from school ('Playing Truant'; Columns (3) and (4)) and pupils' use of substances (this proxy includes smoking, drinking and using cannabis; see Columns (5) and (6)). None of the associations presented in the table is significant at conventional levels, and often the signs of these relations are the opposite of what one would expect *a priori*. All in all, there does not seem to be any effect of neighbourhood composition on these two outcomes.

Finally, in Columns (7) and (8) we concentrate on the variable 'Anti-social behaviour', which captures whether students got involved in putting graffiti on walls, vandalising property, shoplifting and whether they took part in fighting or public disturbance. Ou results show that, while neighbourhood composition in terms of its age-11/KS2 achievements, share of males and proportion of students with SEN status does not significantly affect these behavioural outcomes, an interesting patterns emerge when looking at the proportion of neighbours from poor family background (FSME; see Panel B). A one standard deviation change in this treatment would significantly increase male students' involvement in anti-social behaviour by 5% of a standard deviation, but this change would not affect young girls' behaviour.[18]

These differential effects for boys and girls are not completely surprising: Kling et al. (2005) and (2007) document similarly heterogeneous effects for male and female youths 're-assigned' to better neighbourhoods by the MTO experiment. More broadly, a growing body of research show that boys and girls tend to respond differently to both education and labour market interventions. Amongst others, Anderson (2008) shows 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 et al. (2009) find that peer quality in English secondary schools affects boys and girls differently. Similarly, recent studies show a consistent pattern of stronger female response to financial incentives in education, with the evidence coming from a variety of settings (see Angrist and Lavy, 2009; Angrist et al., 2009). Finally, a number of public-sector training programs generated larger effects on women than men (Lalonde, 1995).

[18] Note that we also studied whether the effects of neighbours' characteristics on boys' and girls' behavioural outcomes differ according to peers' gender. Our evidence shows that male peers' eligibility for free school meals has a larger effect than female peers' FSME status on male students' involvement in anti-social behaviour. Similarly, male peers' SEN status is more strongly linked to girls' attitudes towards education than female peers' SEN condition. However, neither of these differences was statistically significant.

In conclusion, and considering both the small number of pupils sampled by the LSYPE and the fact that we can only look at outcomes between ages 14 and 16, we believe the results in Table 8 provide some support for the notion the neighbourhoods can affect teenagers' behaviour. However, all in all, our evidence also suggests that neighbourhood effects are not a strong and pervasive determinant of students' cognitive and non-cognitive outcomes.

7 Concluding Remarks

Our study has used various detailed administrative datasets on the population of students in England to study the effect of the characteristics and prior achievements of peers in the neighbourhood on the educational achievements and behavioural outcomes of secondary school pupils. In our main sample we track over 1.3 million pupils across four cohorts that go through the first three years of their secondary schooling. Our findings show that, although there is a substantial cross-sectional correlation between pupils' test scores and the characteristics of their residential neighbourhoods, there is no evidence that this association is causal. In fact, the effect of the characteristics of peers in the neighbourhood on students' test-score value-added between ages 11 and 14 is nil. In order to assess the robustness of this conclusion, we extended our analysis in a number of dimensions. First, we considered alternative definitions of neighbourhoods and different ways of identifying peers in the place of residence. Next, we investigated whether the relation between neighbourhood composition and students' test scores is non-linear, or heterogeneous along the lines of pupil background and neighbourhood characteristics. Finally, we considered alternative time-windows and looked at whether later (up to age-16, at the end of compulsory education) or earlier educational achievements (during primary education) are affected by the characteristics of peers in the neighbourhood. All in all, our evidence leads us to conclude that neighbourhood effects are a non-significant determinant of students' test score attainments in schools. On the other hand, we find 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. While due to some data limitations (stemming from sample size and timing) the results on behavioural outcomes are less conclusive, our evidence is in line with previous findings in the field.

Besides presenting some new evidence on neighbourhood effects, we believe our study also makes a number of important methodological contributions. First, we drill down to the effect of neighbourhood changes caused by 'real' movements of families in an out of small neighbourhoods. We can track these changes through information on the detailed residential addresses of our census of students. This is radically different from the approach used in the literature that looks at peer effects at schools, which focuses on the year-on-year changes in school composition under the maintained assumption that students only interact with peers within their grade (or class). Moreover, the detail and density of our data also allows us to define neighbourhood variables at a very small geographical scale and with reference to pupils of similar age. However, we are able to change our definitions of neighbourhoods and peers in the place of residence, and thus address the inherent problem in the literature of pinning-down the correct definition of what constitutes 'a neighbourhood'. This allows us to exclude the possibility that our findings are stemming from data-driven incorrect levels of aggregation. Finally, exploiting the fact that we observe several cohorts of students experiencing changes in the composition of their neighbourhoods at the same as they move through the education system, we are able to partial out pupil and family background unobservables, neighbourhood fixed-effects and time-trends as well as school-by-cohort unobserved shocks. We believe this greatly helps us to identify the unbiased effect of social interactions and role models in the neighbourhoods as advocated by Moffit (2001).

References

Allen, R., S. Burgess and T. Key (2010): “Choosing Secondary School by Moving Home: School Quality and the Formation of Neighbourhood”, *CMPO Working Paper*, 10/238.

Aaronson, D. (1998): “Using Sibling Data to Estimate the Impact of Neighborhoods on Children's Educational Outcomes,” *Journal of Human Resources*, pp. 915-46.

Angrist, J.D., D. Lang and P. Oreopolous (2009): “Incentives and Services for College Achievement: Evidence from a Randomized Trial”, *American Economic Journal: Applied Economics*, 1.

Angrist, J.D. and V. Lavy: “The Effect of High-Stakes High School Achievement Awards: Evidence from a Randomized Trial,” *American Economic Review*, September 2009.

Akerlof, G. (1997): *Social Distance and Social Decisions*, *Econometrica*, 65(5), pp. 1005-1027.

Bayer, P., L. Ross, L. and G. Topa (2008): “Place of Work and Place of Residence: Informal Hiring Networks and Labour Market Outcomes,” *Journal of Political Economy*, 116(6): pp. 1150-1196.

Bernheim, B. (1994): “A Theory of Conformity,” *The Journal of Political Economy*, 102(5), pp. 841-877.

Black, S. (1999): “Do Better Schools Matter? Parental Valuation of Elementary Education,” *The Quarterly Journal of Economics*, 114(2), pp. 577-99.

Cheshire, P. and S. Sheppard (1995): “On the Price of Land and the Value of Amenities,” *Economica*, New Series, Vol. 62, No. 246 (May), pp. 247-267.

Cheshire, P., S. Gibbons, S. and I. Gordon (2008) “Policies for 'Mixed Communities': A Critical Evaluation,” *SERC Policy Paper*, 2.

Currie, J. (2006): *The Invisible Safety Net,* Princeton University Press, New Jersey, USA.

Cutler, D. and E. Glaeser (1997): “Are Ghettos Good Or Bad?,” *The Quarterly Journal of Economics* 112(3), pp. 827-72.

Durlauf, S. (1996): “A Theory of Persistent Income Inequality,” *Journal of Economic Growth*, pp. 75-93.

Edin, P., P. Fredriksson and O. Aslund (2003) "Ethnic Enclaves and the Economic Success of Immigrants – Evidence from a Natural Experiment", *Quarterly Journal of Economics*, 118, pp. 329-357.

Edin, P., P. Fredriksson, H. Gronqvist and O. Aslund (2010) "Peers, Neighbourhoods and Immigrant Student Achievement – Evidence from a Placement Policy", *American Economic Journal: Applied Economics*, accepted for publication.

Fehr, E. and A. Falk (2002): "Psychological Foundations of Incentives," *European Economic Review*, 46(4-5), pp. 687-724.

Gibbons, S. (2002): "Neighbourhood Effects on Educational Achievement", *CEE Discussion Paper*, CEEDP0018.

Gibbons, S., S. Machin and O. Silva (2008): "Choice, Competition and Pupil Achievement", *Journal of the European Economic Association*, 6(4), pp. 912-947.

Gibbons, S., S. Machin and O. Silva (2009): "Valuing School Quality Using Boundary Discontinuities," *SERC Discussion Papers*, 18.

Gibbons, S. and O. Silva (2008): "Urban Density and Pupil Attainment", *Journal of Urban Economics,* 63(1), pp. 631-650.

Gibbons, S and S. Telhaj (2007): "Mobility and School Disruption," *Centre for the Economics of Education Discussion Paper*, CEEDP0083.

Gibbons, S and S. Telhaj (2008): "Peers and Achievement in England's Secondary Schools," *SERC Discussion Paper*, 1.

Glaeser, E. and J. Scheinkman (2001): "Measuring Social Interactions," in *Social Dynamics*, Durlauf, S. and P. Young (eds.), Boston, MA: MIT Press.

Glennerster, H. (1991): "Quasi-Markets for Education," *Economic Journal*, 101, pp. 1268-76.

Gould, E, V. Lavy and D. Paserman (2004): "Immigrating to Opportunity: Estimating the Effect of School Quality using a Natural Experiment on Ethiopians in Israel," *Quarterly Journal of Economics*, 119(2), pp. 489-526.

Goux, D. and E. Maurin (2007) "Close Neighbours Matter: Neighbourhood Effects on Early Performance at School," *Economic Journal*, 117(523), pp. 1193-215.

Granovetter, M. (1995): *Getting a Job*, second edition, Chicago, IL: University of Chicago Press.

Hanushek, E., J. Kain, J. Markman and S. Rivkin (2003): "Does Peer Ability Affect Student Achievement?", *Journal of Applied Econometrics*, 18, pp. 527-544.

Hanushek, E., J. Kain and S. Rivkin (2004): "Disruption versus Tiebout improvement: the costs and benefits of switching schools", *Journal of Public Economics*, 88, pp. 1721-1746

Hoxby, C. (2000): "Peer Effects in the Classroom: Learning from Gender and Race Variation," NBER Working Paper 7867.

Jacob, B. (2004): "Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago," *The American Economic Review*, 94(1), pp. 233-58.

Kain, J. and J. Quigley (1975): "The Demand for Individual Housing Attributes," in: *Housing Markets and Racial Discrimination: A Microeconomic Analysis*, pages 231-255, National Bureau of Economic Research, Inc.

Kling, J., J. Liebman and L. Katz (2007): "Experimental Analysis of Neighborhood Effects," *Econometrica*, pp. 83-119.

Kling, J., J. Ludwig and L. Katz (2005): "Neighborhood Effects on Crime for Female and Male Youth: Evidence From a Randomized Housing Voucher Experiment", *Quarterly Journal of Economics*, 120, pp. 87-130.

LaLonde, R.J. (1995): "The Promise of U.S. Employment and Training Programs," *Journal of Economic Perspectives*, 9(2), 149-168.

Lavy, V., D. Paserman and A. Schlosser (2008): "Inside the Black Box of Ability Peer Effects: Evidence from Variation in Low Achievers in the Classroom," NBER Working Paper 14415.

Lavy, V., and A. Schlosser (2007): "Mechanisms and Impacts of Gender Peer Effects at School," NBER Working Paper 13292.

Lavy, V., O. Silva and F. Weinhardt (2009): "The Good, The Bad and The Average: Evidence on the Scale and Nature of Ability Peer Effects in Schools", NBER Working Paper 15600.

Lupton, R. (2009): "Growing up in Social Housing in Britain", Report: Joseph Rowntree Foundation, York.

Manski, C. (1993): “Identification of Endogenous Social Effects: The Reflection Problem,” *Review of Economic Studies*, 60, pp. 531-42.

Moffitt, R. (2001): “Policy interventions, Low-level Equilibria, and Social Interactions,” in *Social Dynamics*, Durlauf, S. and P. Young (eds.), Boston, MA: MIT Press.

Oreopolous, P. (2003): “The Long-Run Consequences of Living in a Poor Neighborhood,” *Quarterly Journal of Economics*, 118, pp. 1533-1575

Rosenbaum, J. (1995): “Changing the Geography of Opportunity by Expanding Residential Choice: Lessons from the Gautreaux Program,” *Housing Policy Debate*, 6, pp. 231-69.

Sampson, R., J. Morenoff and T. Gannon-Rowley (2002): “Assessing Neighbourhood Effects: Social Processes and New Directions in Research,” *Annual Review of Sociology*, 26, pp. 443-478.

Sanbonmatsu, L., J. Kling, G. Duncan and J. Brooks-Gunn (2006): “Neighborhoods and academic achievement: Results from the Moving to Opportunity Experiment,” *Journal of Human Resources*, 41(4), p. 649.

Weinhardt, F. (2010): “Moving into the Projects: Social Housing Neighbourhoods and School Performance in England”, *SERC Discussion Paper*, 44.

Table 1: Descriptive statistics of the main dataset

Variable	Mean	Standard Deviation
Panel A: Pupils' 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
Pupil is FSM eligible	0.155	0.362
Pupil is SEN	0.213	0.409
Pupil is Male	0.508	0.499
Average rate of outward mobility in n'hood over four years	0.081	0.057
Average rate inward mobility in n'hood over four years	0.083	0.062
Secondary school size (in year 7)	1083.9	384.9
Panel B: Characteristics of pupils in the neighbourhood – Output Area		
KS1 score, average English and Maths – At year 6	15.017	1.762
KS1 score, average English and Maths – At year 9	14.981	1.760
KS1 score, average English and Maths – Change year 6 to 9	-0.036	0.863
Share FSME – At year 6	0.165	0.196
Share FSME – At year 9	0.170	0.199
Share FSME – Change year 6 to 9	0.005	0.081
Share SEN – At year 6	0.215	0.154
Share SEN – At year 9	0.217	0.153
Share SEN – Change year 6 to 9	0.002	0.087
Share Male – At year 6	0.509	0.153
Share Male – At year 9	0.509	0.157
Share Male – Change year 6 to 9	0.000	0.103
Number of pupils in Output Area, 'central cohort' +1/-1, Year 6	13.878	6.317
Number of pupils in Output Area, 'central cohort' +1/-1, Year 9	13.865	6.186
Number of pupils in Output Area, 'central cohort' only, Year 6	5.173	2.612
Number of pupils in Output Area, 'central cohort' only, Year 6	5.169	2.639

Note: Descriptive statistics refer to: (a) Pupils who do not change OA of residence in any period between year 6 and 9; (b) Pupils in Output Areas with at least five pupils belonging to the 'central cohort' +1/-1 in every period between year 6 and year 9; (c) Pupils 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 year 6 to 9. KS1 refers to the average test score in Reading, Writing and Mathematics at the Key Stage 1 exams (at age 7); FSME: free school meal eligibility; SEN: special education needs (with and without statements). Secondary school type attended in year 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) KS3/ Year 9	(2) KS3-KS2/ Year 6 to 9	(3) KS3-KS2/ Year 6 to 9	(4) KS3-KS2/ Year 6 to 9	(5) KS3/ Year9	(6) KS3-KS2/ Year 6 to 9	(7) KS3-KS2/ Year 6 to 9	(8) KS3-KS2/ Year 6 to 9
KS1 score – Level (Year 9) or Change (Year 6 or 9)	0.279 (0.001)**	0.003 (0.001)**	-0.000 (0.001)	0.001 (0.001)	0.079 (0.001)**	0.003 (0.001)**	-0.000 (0.001)	-0.000 (0.001)
Share FSM – Level (Year 9) or Change (Year 6 or 9)	-0.289 (0.001)**	-0.005 (0.001)**	-0.001 (0.001)	0.001 (0.001)	-0.101 (0.001)**	-0.005 (0.001)**	-0.001 (0.001)	0.001 (0.001)
Share SEN – Level (Year 9) or Change (Year 6 or 9)	-0.191 (0.001)**	-0.002 (0.002)	-0.000 (0.001)	-0.001 (0.001)	-0.055 (0.001)**	-0.002 (0.001)*	-0.001 (0.001)	-0.001 (0.001)
Share Males – Level (Year 9) or Change (Year 6 or 9)	0.004 (0.001)**	0.001 (0.001)	0.001 (0.001)	0.002 (0.002)	0.000 (0.001)	0.001 (0.001)	0.001 (0.001)	0.002 (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 approximately 1,310,000 in approximately 134,000 Output Areas. All regressions include cohort dummies. Controls include: pupil own KS1 test scores; pupil is FMSE; pupil is SEN; pupil is male; school size (refers to school attended in year 7); school type dummies (refers to school attended in year 7 and includes: Community, Voluntary Aided, Voluntary Controlled, Foundation, CTC and Academy); average rate of outward mobility in n'hood over four years; average rate inward mobility in n'hood over four years. Secondary by cohort effects: 12,273 groups (refer to school at year 7 when pupil 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, Year 6 to 9	Share FSM – Change, Year 6 to 9	Share SEN – Change, Year 6 to 9	Share Male – Change, Year 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)
Pupil is FSM eligible	0.000	0.026	-0.006	0.003
	(0.004)	(0.004)**	(0.004)	(0.003)
Pupil is SEN	-0.000	0.008	-0.005	0.002
	(0.004)	(0.004)*	(0.003)	(0.003)
Pupil 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 pupils living in OA (PLASC/NPD)	0.005	-0.004	-0.004	-0.004
	(0.002)*	(0.002)	(0.003)	(0.002)*
Std.Dev. of KS2 across pupils living in OA (PLASC/NPD)	-0.000	0.001	-0.002	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Share of households living in socially rented accommodation (Census 2001)	0.002	0.002	-0.003	0.000
	(0.002)	(0.003)	(0.002)	(0.002)
Share of households owning place of residence (Census 2001)	-0.002	-0.002	0.002	0.001
	(0.002)	(0.003)	(0.002)	(0.002)
Share of adults in employment (Census 2001)	0.003	0.002	-0.001	-0.003
	(0.003)	(0.003)	(0.003)	(0.002)
Share of adults with no educational qualifications (Census 2001)	0.004	-0.001	0.001	0.002
	(0.003)	(0.003)	(0.003)	(0.002)
Share of lone parents in the population (Census 2001)	-0.001	-0.003	0.001	0.000
	(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 year 7). Regressions in the bottom panel include cohort effects, OA-averaged pupil KS1 test scores; OA-averaged pupil eligibility for FMSE; OA-averaged pupil SEN status; OA-averaged pupil male gender; OA-averaged school size (refers to school attended in year 7); school-type effects (refers to school attended in year 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: Robustness to alternative peer-group definitions and estimation samples

	Dependent Variable/Timing is:							
	'Central cohort' only		Movers 'intention to treat' set-up		'Constant cohort' only		Adjacent OA n'hoods	
	(1) KS3-KS2/ Year 6 to 9	(2) KS3-KS2/ Year 6 to 9	(3) KS3-KS2/ Year 6 to 9	(4) KS3-KS2/ Year 6 to 9	(5) KS3-KS2/ Year 6 to 9	(6) KS3-KS2/ Year 6 to 9	(7) KS3-KS2/ Year 6 to 9	(8) KS3-KS2/ Year 6 to 9
KS1 score –	0.001	-0.000	0.003	0.000	0.002	-0.001	0.005	-0.001
Change (Year 6 or 9)	(0.001)	(0.001)	(0.001)**	(0.001)	(0.001)	(0.001)	(0.001)**	(0.001)
Share FSM –	-0.003	-0.001	-0.005	-0.001	-0.005	-0.002	-0.003	0.001
Change (Year 6 or 9)	(0.001)**	(0.001)	(0.001)**	(0.001)	(0.001)**	(0.001)*	(0.001)**	(0.001)
Share SEN –	-0.001	-0.000	-0.002	-0.001	-0.001	0.001	-0.004	-0.000
Change (Year 6 or 9)	(0.001)	(0.001)	(0.001)*	(0.001)	(0.001)	(0.001)	(0.001)**	(0.001)
Share Males –	0.000	0.001	0.001	0.001	0.000	0.000	0.002	0.002
Change (Year 6 or 9)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)*	(0.001)*
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Second. by Primary by Cohort FX	No	Yes	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: pupil own KS1 test scores; pupil is FMSE; pupil is SEN; pupil is male; school size (refers to school attended in year 7); school type dummies (refers to school attended in year 7 and includes: Community, Voluntary Aided, Voluntary Controlled, Foundation, CTC and Academy); average rate of outward mobility in n'hood over four years; average rate inward mobility in n'hood over four years. Secondary by primary by cohort effects: 191,245 groups. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Table 5: Heterogeneity of the effects of young neighbours' characteristics along the dimension of pupils' personal attributes

	Dependent Variable/Timing is: KS3-KS2 value-added/Year 6 to 9							
	(1) KS1 Below median	(2) KS1 Above median	(3) Non-FSME Pupil	(4) FSME Pupil	(5) Non-SEN Pupil	(6) SEN Pupil	(7) Female Pupil	(8) Male Pupil
Panel A: N'hood Average KS1								
KS1 score –	-0.001	0.001	0.000	-0.001	0.000	-0.003	0.000	-0.001
Change, Year 6 to 9	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Panel B: N'hood Share of FSME								
Share FSM –	-0.001	-0.002	-0.001	-0.002	-0.002	0.000	-0.002	-0.000
Change, Year 6 to 9	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)*	(0.002)	(0.001)*	(0.001)
Panel C: N'hood Share of SEN								
Share SEN –	0.001	-0.002	-0.001	0.001	-0.001	0.002	-0.002	0.001
Change, Year 6 to 9	(0.001)	(0.001)*	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Panel D: N'hood Share of Males								
Share Males –	0.001	0.000	0.001	0.001	0.001	0.001	-0.001	0.002
Change, Year 6 to 9	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)*
Controls	Yes		Yes		Yes		Yes	
Second. × Prim. × Cohort FX	Yes		Yes		Yes		Yes	

Note: Table reports standardised coefficients and standard errors obtained from regressions pooling all pupils and interacting individual characteristic specified in the heading with one of the treatments (change in the neighbourhood characteristic). All regressions include controls as in Table 3, Column (2) and following columns. 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 pupils above/below median KS1: about 582,000/726,000 respectively. Number of FSME/Non-FSME pupils: around 203,000/1,106,000, respectively. Number of SEN/Non-SEN pupils: approximately 279,000/1,031,000 respectively. Number of male/female pupils: around 665,500/643,700 respectively. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Table 6: Heterogeneity of the effects of young neighbours' characteristics along the dimension of neighbourhood quality

	Dependent Variable/Timing is: KS3-KS2 value-added/Year 6 to 9							
	(1) Small N'hoods	(2) Large N'hoods	(3) Low Density	(4) High Density	(5) Low Over-crowd.	(6) High Over-crowd.	(7) Low Share Social Housing	(8) High Share Social Housing
Panel A: N'hood Average KS1								
KS1 score –	-0.001	0.001	-0.001	0.001	-0.002	0.001	-0.000	-0.001
Change, Year 6 to 9	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.005)
Panel B: N'hood Share of FSME								
Share FSM –	-0.001	-0.001	0.001	-0.003	-0.002	-0.001	-0.001	0.001
Change, Year 6 to 9	(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 –	-0.001	-0.000	0.001	-0.002	0.001	-0.001	-0.001	-0.001
Change, Year 6 to 9	(0.001)	(0.001)	(0.001)	(0.001)*	(0.001)	(0.001)	(0.001)	(0.005)
Panel D: N'hood Share of Males								
Share Males –	0.001	-0.000	0.002	-0.000	-0.000	0.002	0.001	0.001
Change, Year 6 to 9	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.005)
Controls	Yes		Yes		Yes		Yes	
Second. × Prim. × Cohort FX	Yes		Yes		Yes		Yes	

Note: Table reports standardised coefficients and standard errors obtained from regressions pooling all pupils and interacting an indicator for whether the individual lives in a neighbourhood with the 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. Number of observations approximately 1,310,000 in approximately 134,000 Output Areas. Secondary by primary by cohort effects: approximately 191,000 groups. Small and large neighbourhoods are defined using number of pupils in the 'central cohort +1/-1' residing in the OA on average over the four years of the analysis. Number of pupils in large/small neighbourhoods: about 674,000/635,000 respectively. Population density, housing over-crowding and share of households on social housing derived from GB Census 2001 at the OA level. Number of pupils in high/low density neighbourhoods (above/below median): around 656,000 in both cases. Number of pupils in neighbourhoods with high/low residential over-crowding (above/below median): approximately 656,000 in both cases. Neighbourhoods with a high share of social housing are defined as those with at least 75% households in socially rented accommodations. Number of pupils in neighbourhoods with high/low share of social housing: around 43,600/1,267,000 respectively. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Table 7: Characteristics of young peers in the neighbourhood and students' achievements: Year 6/KS2 to Year11/KS4 and Year 9/KS3 to Year11/KS4 time-windows

	Dependent Variable/Timing is:			
	(1)	(2)	(3)	(4)
	KS4-KS2/ Year 6 to 11	KS4-KS2/ Year 6 to 11	KS4-KS3/ Year 9 to 11	KS4-KS3/ Year 9 to 11
Panel A: N'hood Average KS1				
KS1 score – Change,	-0.002	-0.002	0.000	-0.000
Year 6 to 11 or Year 9 to 11	(0.002)	(0.002)	(0.002)	(0.002)
Panel B: N'hood Share of FSME				
Share FSM –	-0.002	-0.001	0.003	0.003
Change, Year 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, Year 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, Year 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 tow cohorts. Peers are defined as student living in the same OA and of the same age. Regression further consider only: *(a)* Pupils who do not change OA of residence between year 6 and 11; *(b)* Pupils in Output Areas with at least three students belonging to the same age group in years 6 and 11 (Columns (1) to (3)) and years 9 and 11 (Columns (4) to (6)); *(c)* Pupils in the non-selective part of the education system. Some selected descriptive statistics are provided in Appendix Table 4. 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 year 7 when pupil enters secondary education). Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

Table 8: Characteristics of young peers in the neighbourhood and students' behavioural outcomes; pupils sampled by the LSYPE (aged 14 in 2004)

	Timing is: Changes between Year 9 and Year 11. The outcomes are:							
	Attitudes towards schooling		Playing truant		Substance use		Anti-social behaviour	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male Pupil	Female Pupil	Male Pupil	Female Pupil	Male Pupil	Female Pupil	Male Pupil	Female Pupil
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, Year 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 FSME								
Share FSM –	-0.013	-0.001	-0.032	-0.010	-0.018	-0.006	0.050	-0.008
Change, Year 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, Year 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, Year 6 to 9	(0.017)	(0.016)	(0.018)	(0.017)	(0.018)	(0.018)	(0.021)	(0.015)

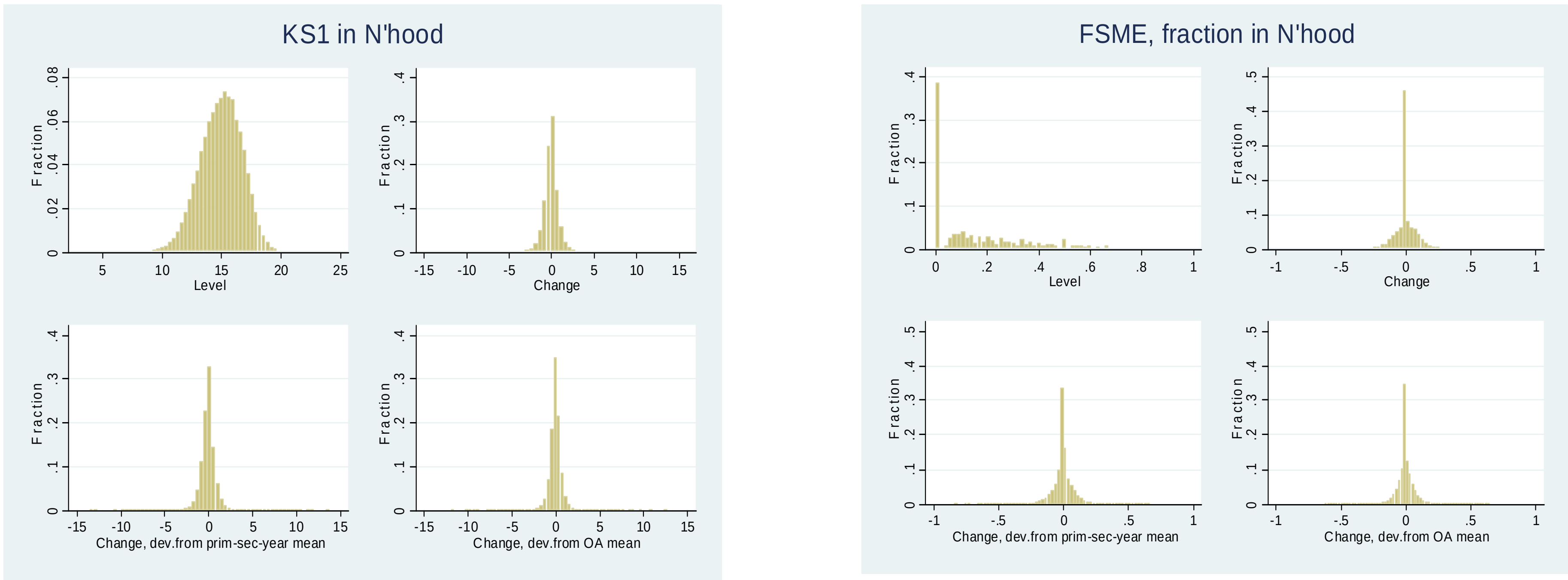
Note: Table reports standardised coefficients and standard errors obtained from separate regressions for boys and girls. All regressions include controls as in Table 2, Column (5) and following columns and secondary school fixed effects. Sample includes one cohort of pupils interviewed in the Longitudinal 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: *(a)* Pupils who do not change OA of residence between year 9 and 11; *(b)* Pupils in Output Areas with at least three students belonging to the same age group in years 9 and 11; *(c)* Pupils 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 6. Standard errors clustered at the OA level in round parenthesis. **: 1% significant or better; *: at least 5% significant.

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

	PLASC 2002	PLASC 2003	PLASC 2004	PLASC 2005	PLASC 2006	PLASC 2007	PLASC 2008
	Y5			Y8			
Cohort 1	Y6/KS2			Y9/KS3			
	Y7			Y10			
		Y5			Y8		
Cohort 2		Y6/KS2			Y9/KS3		
		Y7			Y10		
			Y5			Y8	
Cohort 3			Y6/KS2			Y9/KS3	
			Y7			Y10	
				Y5			Y8
Cohort 4				Y6/KS2			Y9/KS3
				Y7			Y10

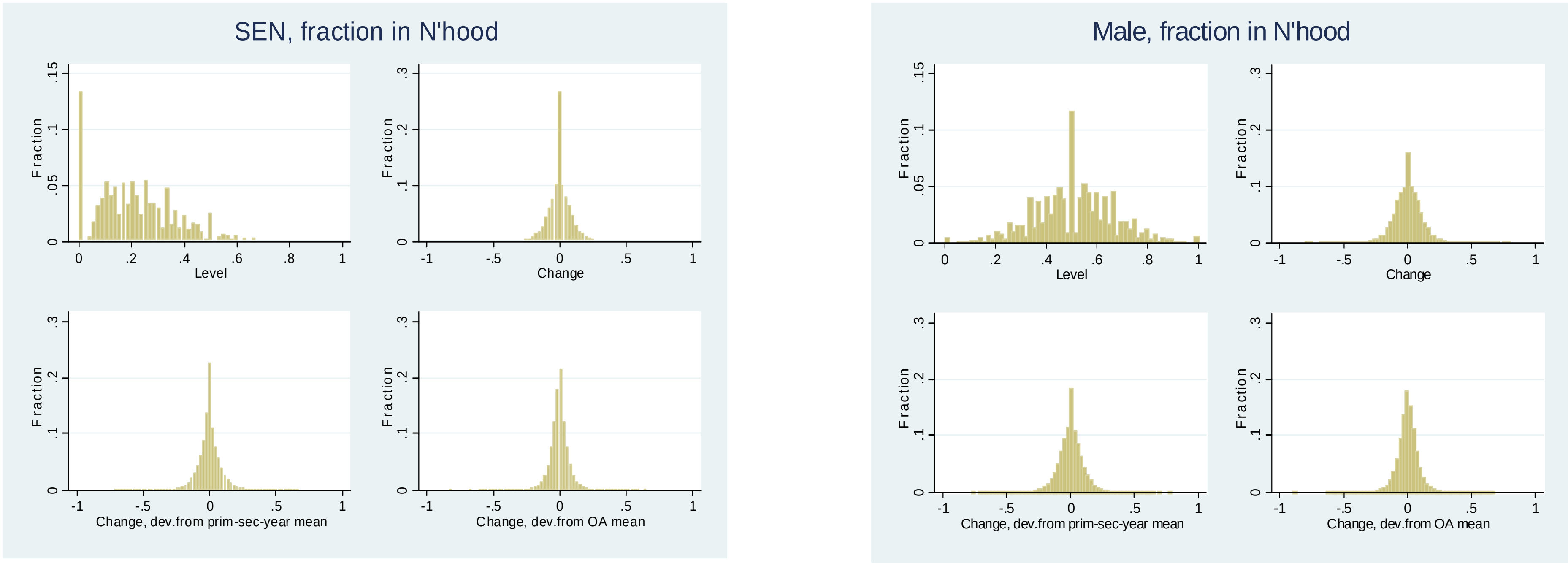
Note: Shaded cells refer to the cohort under analysis; adjacent non-shaded cohorts represent the additional set of students used to construct measures of quality of neighbourhood. PLASC refers to the Pupil Level Annual School Census. Y5, Y6, Y7, Y8 and Y9 refer to school years five, six, seven, eight and nine. Students finish their primary school in year 6 when they sit for their Key Stage 2 (KS2) at age 11. They then enter secondary education in year 7 and complete their Key Stage 3 exams in year 9 when aged 14.

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



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

Appendices

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

Variable	Mean	Standard Deviation
Panel A: Pupils' characteristics, 'stayers' only		
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
Pupil is FSM eligible	0.171	0.377
Pupil is SEN	0.220	0.414
Pupil is Male	0.507	0.500
Average rate of outward mobility in n'hood over four years	0.098	0.075
Average rate inward mobility in n'hood over four years	0.089	0.073
Secondary school size (in year 7)	1081.6	385.0
Panel B: Characteristics of pupils in the neighbourhood – Output Area		
KS1 score, average English and Maths – At year 6	14.968	1.857
KS1 score, average English and Maths – At year 9	14.966	1.854
KS1 score, average English and Maths – Change year 6 to 9	-0.002	1.407
Share FSM eligible – At year 6	0.172	0.205
Share FSM eligible – At year 9	0.172	0.206
Share FSM eligible – Change year 6 to 9	-0.001	0.140
Share SEN – At year 6	0.218	0.166
Share SEN – At year 9	0.218	0.166
Share SEN – Change year 6 to 9	0.000	0.139
Share Male – At year 6	0.509	0.174
Share Male – At year 9	0.509	0.176
Share Male – Change year 6 to 9	0.000	0.128
Number of pupils in Output Area, 'central cohort' +1/-1, Year 6	13.212	6.562
Number of pupils in Output Area, 'central cohort' +1/-1, Year 9	12.884	6.628

Note: Descriptive statistics refer to pupils in the non-selective part of the education system. The data *includes* (a) Pupils who change OA of residence between year 6 and 9; and (b) Pupils in Output Areas with less than five pupils belonging to the 'central cohort' +1/-1 in every period between year 6 and year 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 year 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 estimates

	Dependent Variable/Timing is: KS3-KS2 value-added/Year 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, Year 6 to 9	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: N'hood Share of FSME						
Share FSM –	-0.002	-0.001	0.000	-0.002	-0.002	0.000
Change, Year 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, Year 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, Year 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

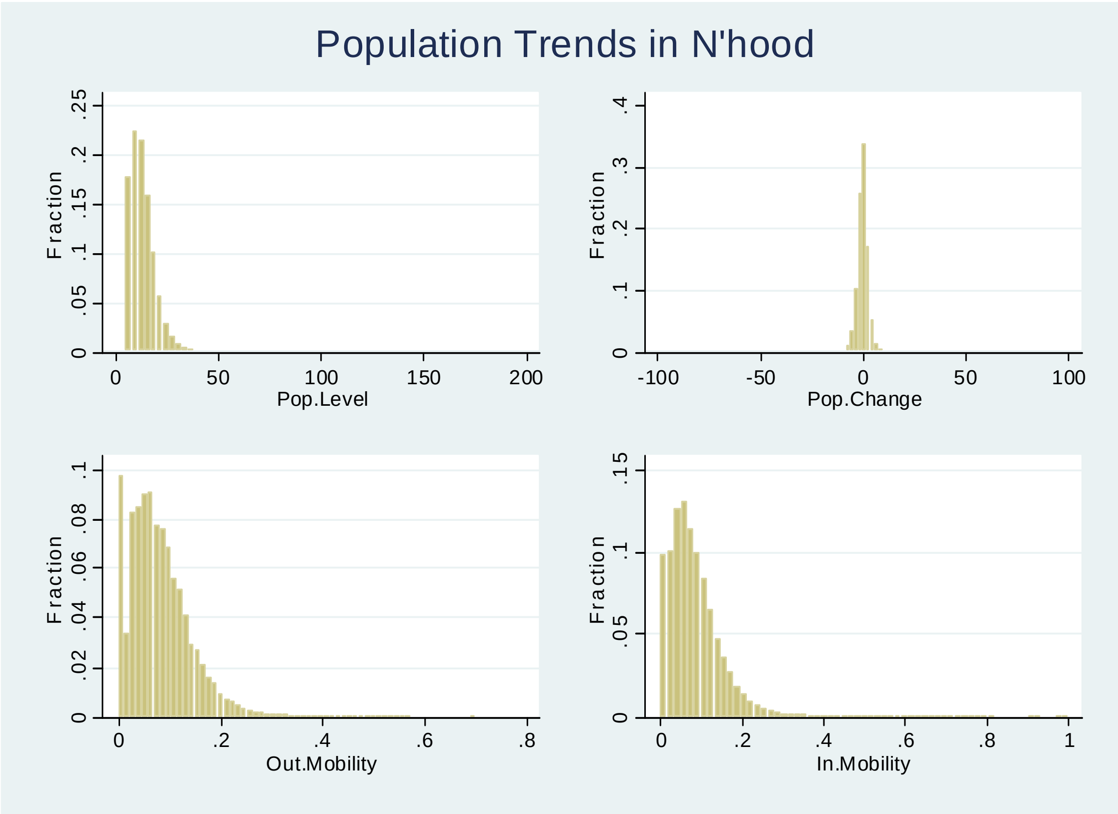
Note: Table reports standardised coefficients and standard errors. Number of observations approximately 1,310,000 in approximately 134,000 Output Areas. All regressions include cohort dummies. Controls include: pupil own KS1 test scores; pupil is FMSE; pupil is SEN; pupil is male; school size (refers to school attended in year 7); school type dummies (refers to school attended in year 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 year 7 when pupil 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: Selected descriptive statistics for pupils sampled by the LSYPE (aged 14 in 2004)

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

Note: Descriptive statistics refer to the sample that includes one cohort of pupils 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 *(a)* Pupils who do not change OA of residence between year 9 and 11; *(b)* Pupils in Output Areas with at least three students belonging to the same age group in years 9 and 11; *(c)* Pupils 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/Year 2). Prior achievement of pupils and their peers in the neighbourhood are proxied by KS2 test scores (Age 11/Year 6).

Appendix Figure 1: Population in the neighbourhood and mobility rates



Note: Population refers to number of pupils in Output Area, 'central cohort' +1/-1, Year 9. Descriptive statistics presented in Table 1, Panel B. deviations from primary-by-secondary-by-cohort mean changes are as follows. Population change refers to change in the number of pupils in Output Area ('central cohort' +1/-1) from Year 6 to Year 9. Descriptive statistics: mean: -0.0125, std.dev. 2.994. Descriptive statistics for Outward and Inward mobility rates presented in Table 1, Panel B.

Appendix Figure 2: Constant-cohorts dataset construction; three 'main cohorts' and asymmetric peers

		PLASC 2003	PLASC 2004	PLASC 2005	PLASC 2006	PLASC 2007	PLASC 2008
		Y4			Y7		
Cohort 2		Y5			Y8		
		Y6/KS2			Y9/KS3		
			Y5			Y8	
Cohort 3			Y6/KS2			Y9/KS3	
			Y7			Y10	
				Y6/KS2			Y9/KS3
Cohort 4				Y7			Y10
				Y8			Y11

Note: Shaded cells refer to the cohort under analysis; adjacent non-shaded cohorts represent the additional set of students used to construct measures of quality of neighbourhood. 'Cohort 1' is not included because the data span does not allow considering it. PLASC refers to the Pupil Level Annual School Census. Y5, Y6, Y7, Y8 and Y9 refer to school years five, six, seven, eight and nine. Students finish their primary school in year 6 when they sit for their Key Stage 2 (KS2) at age 11. They then enter secondary education in year 7 and complete their Key Stage 3 exams in year 9 when aged 14.