Mobility and School Disruption

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Executive Summary

Schools with high levels of pupil turnover tend to have low average levels of pupil attainment. This observation has led many to infer that changes of school are bad for those pupils who move, and that a high level of pupil mobility is bad for other pupils and bad for schools. On this basis, understanding and tackling the consequences of pupil mobility has become a key policy issue. However, it is arguable to what extent poor performance is a *consequence* of pupil movement, because pupils who are the most likely to move tend also to be educationally disadvantaged in other ways.

Our study looks carefully at whether pupil mobility in primary school really disrupts learning. We focus on the effect that pupils entering school during Years 3-6 in Key Stage 2 in England have on incumbent pupils in their school year group who joined the school in Key Stage 1 and remain there until the end of the primary phase. This issue is important for school policy because of the economic case for intervention when the school choice and timing decisions of some pupils have an impact on the educational outcome of others.

The research is based on information on the test scores of the population of age-11 (Year 6) pupils in primary schools in England from 2002-2005, coupled with pupil census information, retrospective age-7 test results, and details of pupils' school attendance history. To take account of unobservable differences between schools, we look at differences in mobility experienced *within* schools from one year to the next, that is, we estimate the expected disadvantage a pupil faces from being educated in a cohort that experiences high mobility, relative to similar pupils in preceding or subsequent cohorts in the same school.

Our main findings are that:

- Pupil mobility is disruptive and immobile pupils in mobile cohorts progress less well between ages 8 and 11. However, observed levels of mobility can account for very little of the variation in pupil performance: a pupil in a cohort experiencing a very high 10 percent annual turnover rate can only expect to be 1-2 weeks behind a pupil whose school cohort is completely stable throughout the age-8 to age-11 period

- Pupils moving the shortest distances between schools are the least disruptive, though there is no difference in moves within Local Authority areas and those between them, and poor pupils are no more disruptive than non-poor. We do not consider the specific effect of recent immigrants in this study.
- Differences by month of entry are much more pronounced. In line with what we would expect, pupils arriving in September are far less disruptive than those arriving at other times.
- There are few notable differences in response to mobility for different demographic groups, different school sizes, or urban versus rural school settings.
- Mobility later in the Key Stage phase is increasingly disruptive to a pupil's *own* progress. Each year's delay in moving during Key Stage 2 reduces progress over this 4-year phase by an average of 0.085 value-added points, or about 1 week.

On balance, our evidence indicates that although mobility does disrupt educational progress slightly, it is not a major cause of low achievement. Even so, policy to provide additional resources to schools with high mobility rates may well be justified on grounds of inter-school equity, because mobility provides a good indicator of pupils who are 'at risk' for low achievement and because it is a way of compensating schools for more general background disadvantages.

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1 Introduction

We consider whether a child's education is affected by turnover and disruption in the composition of their school cohort. This issue is important for policy because it means that schools that experience high levels of pupil mobility because of local demographic characteristics may be disadvantaged relative to others with more stable populations. It is also important in the context of policy related to school choice, because more frequent switching between schools during the process of search may actually impede pupil progress.

Using administrative data on four cohorts of pupils in English primary schools, we construct measures of pupil mobility based on the date of pupil entry into school between ages 3 and 11. We estimate the effect of this mobility on pupil achievement, focussing mainly on its external effect on incumbent, immobile pupils. Our methods acknowledge that pupil mobility is likely to be determined by school quality and pupil characteristics. This link occurs because popular schools develop queues that increase the number of pupils arriving late, and because incoming and incumbent pupils may differ in ability. Our identification strategies exploit cohort-to-cohort variation within schools and information on pupils who change school because of school closures and amalgamations.

We show that pupils progress more slowly between ages 7 and 11 in schools where annual rates of pupil entry are high. That is, pupils do better academically if their school-mates have a long history in the school than if many of their school-mates joined the group late, which we attribute to the disruptive effect of new arrivals. However, like previous work for the US (Hanushek at al 2004), we find that the scale of these effects are fairly modest once we account for unobservable differences in school quality and pupil sorting: An age-11 pupil in a school with a stable and unchanging population could expect to be about one to two weeks ahead of a similar pupil in an extremely disrupted school experiencing 10% inflow annually amongst children aged 7-11. However, a one standard deviation in the rate of turnover is only 2.4%, so differences in turnover cannot account for much of the variance in pupil achievement observed in our data.

Although the focus of the paper is the impact of peer mobility, we also investigate the association between pupil moves and their own performance. In the absence of any compulsory random reassignment of pupils, and without implausible structural assumptions, it is very hard to disentangle the detrimental impact of a change of school from the positive benefits of a better pupil school match or from changes in unobservable pupil attributes. Nevertheless, our results on the *timing* of moves – and in particular the timing of moves resulting from school closures – suggest that later moves are more disruptive than earlier moves during the school phase. These results suggest that the net effect of a change of school in any year between ages 8 and 11 is to lower academic progress over these years by around 3.5% of a standard deviation relative to entry one-year earlier. This difference is quite small, but is not non-existent, as claimed in previous research for London by Strand and Demie (2006).

The structure of our paper is as follows. In the next section we outline previous works that are related to ours and summarise their findings. In Section 0 we explain the modelling framework that we will apply to the English primary school system and administrative data described in Section 0. The results of our empirical estimation are in Section 0, and Section 0 provides a brief summary and conclusions.

2 Literature and Context

In recent years, the observation that schools with high levels of pupil turnover tend to have low average levels of attainment has led many to infer that pupil mobility has an adverse influence on educational outcomes. Certainly education practitioners think mobility has an effect on school performance: in the survey of 43 schools by Demie et al (2005), 86% of headteachers reported that they though it mattered. With these concerns in mind, the Government has argued that mobility “is a challenge to which we must rise if we are to have an impact on individual pupils, on the schools they belong to, and on their future contribution to society as a whole” (Charles Clarke, then Secretary of State for Education and Skills, in Department for Education and Skills 2003).

The main British education policy debates in this field have concentrated on the effects of mobility on academic achievement and child development more generally, and on whether schools with high levels of mobility should be resource-compensated in some way (ALC 2005). Another concern is that pupil mobility, if correlated with academic achievement, has important implications for the evaluation of school performance. Accordingly, since 2000, pupil mobility has been absorbed into school inspection process (Office for Standards in Education, 2000 and 2002) and an indicator of "social mobility" is included in the school league tables published on the Department of Education and Skills web site. Especially relevant for our research are concerns raised throughout the literature that the integration of new pupils can create a diversion of teaching resources away from current pupils that may lead to negative externality arising from mobility.

Even so, the question of whether mobility really has a 'causal' impact on pupil attainment – or whether it is just that lower attaining pupils are more mobile or bad schools have high turnover – remains unclear. In fact, good empirical analysis of mobility in the English educational system and internationally is quite rare. Although, the issue has attracted researchers for a long time (e.g. Douglas 1964), it is only very recently that the patterns of pupil mobility in England at a national level have been analysed (Machin, Telhaj and Wilson 2006). Most other empirical research that exists for England relies on descriptive statistics (often compiled by schools or LEAs) on small samples in specific circumstances (e.g. Alston 2000, Demie 2002, DfES 2003, Dobson and Pooley 2004 and Greater London Authority 2005).

Most of the research literature on pupil mobility has been concentrated on two main themes; the extent of pupil mobility and its determinants; and pupil mobility and pupil attainment. Machin, Telhaj and Wilson (2006) in their comprehensive empirical study offer an analysis of the extent of and patterns in pupil mobility for all state school children in England. Using administrative data for 2 academic years of the English pupil census, their main findings are that pupils from lower income families, and those with lower previous academic achievement, are significantly more likely to move. However, their analysis suggests that mobile children are more likely to end up in 'better' performing schools than the ones they left, a finding that is consistent with a story (amongst others) in which pupils and there parents engage in a process of 'Tiebout'-type search choice (Tiebout 1952). Dobson, Henthorne and Lynas

(2000), examining the nature and causes of pupil mobility in 6 LEAs, find that migration and family break-ups are the main factors associated with causes of pupil mobility. Ofsted (2002), analysing mobility in 3300 primary schools and 1000 secondary schools that were under inspection regime between 2000 and 2001, reports huge difference between schools in the extent of pupil mobility, and that schools with high mobility levels tend to be those enrolling pupils of lower-income families.

Some studies have gone further to try to answer the important question of whether pupil mobility really matters for pupils' achievement. This question is a difficult one to answer since disentangling the direction of causation between pupil mobility and achievement is a huge challenge. This problem becomes even more difficult knowing that pupils move schools for different reasons. Some move for reasons linked to school choice where parents try to send their children to better schools; others move for reasons other then their schooling – parents divorce, get new jobs, or migrate. Hanushek, Kain and Rivkin (2004) is the only study to attempt to separate out these linkages. They argue that moves made for Tiebout-choice reasons impact positively pupil achievement while other moves are likely to impact negatively to achievement due to disruption, and try to separate out these influences on the basis that Tiebout moves introduce long run gains whilst disruption induces only short run costs. In the Texas elementary school setting that they study, mobility rates seem extraordinarily high – 23% of pupils switch schools each year according to their figures – but pupil moves incur, on average, a cost of about 1% of one standard deviation in terms of the pupil's annual gain in maths achievement. Hanushek et al also look at externality induced by mobile pupils that is the main issue to which we turn to in our empirical work.

The results of other studies focusing on the link between attainment and pupil mobility have been more mixed, usually reporting either a negative correlation with academic achievement or no association at all. Strand (2002) argues that much of the work that has looked at the correlation between mobility and attainment without taking into account other factors is highly misleading. Indeed, when Strand (2002) and Strand and Demie (2005) control for background and school factors in their regressions, the negative mobility-attainment relationship is driven away. However, these two studies are based on a single London Local Education Authority, have no explicit strategy for dealing with the simultaneity of school performance and pupil's school choice decisions and make no attempt to take into account the possible positive gains to some pupils induced by 'Tiebout' type choice.

The empirical work we present below addresses the question of whether pupil mobility affects performance, in the English school context. It is an advance on previous work for England (or anywhere else in the UK) in that we use large scale administrative data on the population of pupils throughout the country and because we look for credible ways to measure the disruptive externality induced by mobile pupils and to test this for robustness to alternative explanations. We do not have quite the level of detail on repeated pupil test and timing of moves found in Hanushek, Kain and Rivkin (2004) for Texas, which allows these authors to look at the impact of own-mobility on the acceleration in attainments However, we do have useful information on pupil home postcodes and on school closures, both of which we can bring to bear on the analysis for the purpose of eliminating unobserved characteristics of pupils and their background. Moreover, we look at the effects of cohort specific entry rates, conditional on entry rates in other cohorts in the same school, a strategy that is effective in controlling for time-varying school characteristics. In the next section we set out our modelling approach.

3 Empirical Modelling and Identification Strategy

Our goal is to assess to what extent a child's progress in primary school is affected by changes in their school and classroom caused by entry of new pupils into their peer group. Policy analysis and previous research has usually started from the presumption that entry of pupils is disruptive, and is likely to impede progress at school, although it is also possible that changes in group make up are stimulating to pupils and encourage educational. We will use empirical evidence on mobility and academic progress in the English schools to provide answers to these questions. 'Progress' here will be assessed in terms of the gain in pupil achievement measured by standard tests at age 7 and at age 11. In the English school system, these ages correspond to school Year 2 and Year 6 respectively, which are at the ends of school phases referred to as *Key Stage 1* (Years 1-2) and *Key Stage 2* (Years 3-6).

The model that underlies our estimates is one in which achievement (*k*) in a given Key Stage ($g \in \{1,2\}$) is influenced by disruption in the years prior to the tests, both because of a pupil's own mobility into and between schools, and because of the disruption induced by turnover of his or her peers in school. In terms of pupil's own mobility, we will focus on the date (d_{gist}) when each pupil (i) joined school (s) *during* or *before* the Key Stage (*g*) leading up their test. When measuring peer-group turnover, we look at the proportion of the school roll that joins *during* or *before* the Key Stage (m_{gist}). In addition, we allow that achievement depends on partially observable pupil-specific (a_i) and school-specific (r_s) factors that influence the level of achievement, pupil-specific (b_i) and school-specific (q_s) factors that affect the rate at which a pupil progresses with age, and an interaction term which captures the educational value of the pupil-school match ($c_i p_s$). The basic empirical model is, adding the usual error term and allowing for multiple cohorts (*t*):

$$k_{gist} = \sum_{h=1}^{h=g} \left(\alpha_{gh} d_{hist} + \beta_{gh} m_{hist} \right) + a_i + b_i g + c_i p_s + q_s g + r_s + \varepsilon_{gist} \quad (1)$$

The potential unobservable components in this model present obvious problems when it comes to estimation using standard regression techniques: Mobile pupils may have different levels of unobserved ability or educational advantage/disadvantage than stable pupils; Schools that are otherwise disadvantaged by resource limitations, intake composition and other geographical factors may also experience higher rates of mobility because of local population demographics; The decision of pupils to enter (or exit schools) and hence their entry dates into school is likely to be influenced by potentially unobservable components of school quality. Elimination of all these factors presents a serious challenge to estimation. Below we describe the way we approach this challenge, given the data that we can bring to bear on it.

Firstly, differencing the model between two Key Stages, 2 and 1, in which we have test results observed for the same pupil, gives rise to a 'value-added' model of educational achievement. For pupils who switch schools, this transformation really does not help much as inspection of (1) will reveal: The 'value-added' transformation only eliminates fixed pupil or family background characteristics that determine the level of attainment (relative to other pupils) throughout all Key Stages. For these movers, the within-school transformation will not successfully eliminate *any* of the unobservable school related components. On the contrary, the gain in attainment between school phases is a combination of the personal disruption induced by their move, the improvement (or deterioration) in the match with their school, the difference in the expected performance between their destination and origin school (in terms of both levels and trends) and differences in the phase-specific shocks. Without explicit randomisation of pupils in and out of schools at different times, or ad-hoc parametric assumptions and restrictions, identification of the parameters of interest for school movers between the two phases is something of a lost cause[1].

Better progress can be made if we look at the case of pupils *who do not change school between phases*. For this group, the value-added transformation successfully eliminates a number of these influences. For these stayers over Key Stage 1 and 2, the gain in achievement between these two school phases is now just combination of own entry date (which is by definition in or before Key Stage 1), peer-mobility-related factors in both phases, pupil (b_i) and school (q_s) characteristics that influence how fast a child progresses with age, plus an error term. Furthermore, if we have multiple cohorts observed in different years (*t*), it is possible to eliminate the remaining school fixed effect q_s by the standard within-groups transformation based on deviation of the variables from school-means, and this provides a starting point for our regression-based estimates:

$$\tilde{k}_{2ist} - \tilde{k}_{1ist} = \left(\alpha_{21} - \alpha_{11}\right)\tilde{d}_{1ist} + \beta_{22}\tilde{m}_{2ist} + \left(\beta_{21} - \beta_{11}\right)\tilde{m}_{1ist} + \tilde{b}_i + \tilde{\varepsilon}_{2ist} - \tilde{\varepsilon}_{1ist} \qquad (2)$$

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[1] Hanushek et al 2006 try to get round this problem by firstly eliminating the unobserved individual components by allowing for individual fixed effects in the differenced model so that they identify mobility effects off the acceleration in attainment. Secondly, they impose some ad-hoc structure and assume that the remaining interaction between school quality and individual unobservables (the Tiebout choice component) can be estimated from long-run gains from a move, whilst the disruptive effects influence current year test scores only. We are unable to implement the pupil fixed effect strategy in the differenced model because we only have tests scores at two dates. Nor would we wish to impose the assumption about the persistence of these effects, even if we had the data available.

Hence, we focus almost entirely on (2), and in particular the influence of peer-group mobility in Key Stage 2 expressed by parameter β_{22}. Even then there are obvious problems. Firstly, mobility rates in a pupil's cohort in this phase may be induced by changes in school quality (trends, or shocks) that are present in $\tilde{\varepsilon}_{2ist} - \tilde{\varepsilon}_{1ist}$. This link would occur if, for example, a deterioration in school quality caused a decrease in rate of entry or an increase in the rate of exit. Our proposed strategy for dealing with this is based on the recognition that mobility rates throughout the school – not just in the pupil's own cohort – will change in response to shocks or trends in school quality. This observation provides a basis for a falsification test, since we would expect a pupil's academic progress to be relatively unresponsive to changes in mobility in the school outside there own cohort and we can test this by including other cohorts' school-specific mobility rates in our regressions (this approach is similar to that used by Lavy et al (2004)).

A second issue is that unobserved pupil attributes (*b*) that affect progress may be related to the pupil's (or parents') choice of entry date in Phase 1. There are, however, no obvious reasons to expect the within school *changes* in mobility rates from cohort to cohort to be related to fixed pupil characteristics. For this reason, the issue may not be of major concern in our main goal of estimating β_{22} except in so far as including pupil's own entry date in our regression models leads to biases because of its potential endogeneity. We will demonstrate, using a balancing-type test, that the within-school changes in mobility are in fact largely uncorrelated with pupil characteristics that are included in and excluded from our main regression estimates. We will also test the sensitivity of our estimates of β_{22} to the inclusion and exclusion of date of entry and other control variables.

Another way forward is to try to find pupil reassignments between schools, the timing of which are unrelated to either pupil or destination school characteristics. This prospect is quite challenging. One potential source of such variation is local school closures or mergers. Such a policy-related occurrence certainly generates a change of school, the timing of which is exogenous to pupil and destination school characteristics. Whether or not the assignment of pupils from closed schools to destination schools is unrelated to pupil and school characteristics is more questionable. For instance, parents and pupils may still be offered a choice of school when their original school shuts down. Such a choice is, plausibly, less optimal than the family's original choice of school, but the choice of destination school is nevertheless likely to be correlated with pupil characteristics. Moreover, if assignment to schools depends on the destination school having excess capacity, then receiving schools are likely to be those schools that are less popular, and perhaps of lower quality. Importantly, if we focus on the model for 'stayers' only (Equation 2) and we allow for school fixed effects, then the proportion of children arriving from closed schools is quite likely to be uncorrelated with incumbent pupils' characteristics, because the school fixed effects control for expected differences in destination school quality and composition. A further drawback, however, is that schools that are closed are potentially different – probably lower quality – than those that remain open and so the pupils moving from those schools may have educational advantages or disadvantages that may make a distinct impact on the receiving schools. Still we consider this issue worth investigating, and will exploit school closures and pupil reassignments as one weapon in our armoury.

4 The English School System and Administrative Data

National curriculum and assessment

Compulsory education in England is organised into five stages referred to as *Key Stages*. In the Primary phase, pupils enter school at age 3-5 in the *Foundation Stage* and then move on to *Key Stage 1*, spanning ages 5-6 and 6-7. At age 7-8 pupils move to *Key Stage 2*, sometimes – but not usually – with a change of school. In few cases there are separate Infants and Junior schools (covering Key Stage 1 and 2 respectively) and a few areas still operate a Middle School system (bridging the Primary and Secondary phases). At the end of Key Stage 2, when they are 10-11, children leave the Primary phase and go on to Secondary school where they progress through *Key Stage 3* and *4*. At the end of each Key Stage, pupils are assessed on the basis of standard national tests and progress through the phases is measured in terms of Key Stage Levels, ranging between W (working towards Level 1) and Level 5+ in the Primary phase. A point system can also be applied to convert these levels into scores that are intended to represent about one term's (10-12 weeks) progress.

The focus in our empirical work is on mobility between Key Stages (or phases) 1 and 2. For the main analysis, we consider only those schools which take pupils prior to Key Stage 2, and educate them through to their end-of-phase tests at age 10-11.

School types and governance

These primary schools in the state-sector in England fall into a number of different categories, and differ in terms of the way they are governed, the ownership of the school buildings, and who controls pupil admissions. All state schools are funded largely by central government, through Local Authorities that are responsible for schools in their geographical domain. Community Schools are the most common type of school (68% of pupils), and are quite closely controlled by the Local Authority. Foundation (2%) and Voluntary Aided (23%) schools have more autonomy, particularly with regard to pupil admissions and are owned or have links with charitable organisations, usually religious. Some schools (15%) are classed as Voluntary Controlled and are similar to Community schools in terms of admissions and governance, but have a religious ethos (almost all Church of England). In addition, there is a small private, fee-paying sector[2]. In this paper we restrict attention largely to Community schools, which form a relatively homogenous group in terms of institutional arrangements. Moreover, pupil movements between Community schools seem, a priori, less likely to be driven by pupils searching or queuing for admission to the more distinctive types of school. Community schools also have less scope for picking and choosing the pupils they admit, so are unlikely to influence the composition of their pupil intake at any stage.

The data

The UK’s Department for Education and Skills (DfES) collects various data on school and pupils centrally, because the pupil assessment system is used to publish school performance tables and because information on pupil numbers and characteristics are necessary for administrative purposes – in particular to determine funding. A National Pupil Database

[2] Private schools educate around 6-7% of pupils in England as a whole.

(NPD) holds information on each pupil's assessment record in the Key Stage tests throughout their school career. Since 2002, the DfES has also collected information on pupil's school, gender, age, ethnicity, language skills any special educational needs or disabilities, entitlement to free school meals and various other pieces of information via the Pupil Level Annual Schools Census (PLASC), which is incorporated into the test-score information in the NPD. The pupil census has information on postcode of residence: a postcode is typically 10 contiguous housing units, which allows us to control very carefully for residential location.

Importantly for our project, the PLASC data records the date when the pupil entered the school they are in at the time of the census (January in each year). By looking back over the various census years and cross-checking with information on which school a pupil was at when they sat their Key Stage 1 tests in Year 2, and which school a pupil is at for their Key Stage 2 tests in Year 6. From this, we can build up a reasonably accurate picture of patterns of pupil entry over Years 3 to Years 6. However, our analysis only considers the population of pupils who are recorded in English schools in Year 2 and Year 6 in the database, so we explicitly exclude from our analysis any consideration of the impact of new immigrants to England from the rest of the UK or from overseas, or pupils moving to the state sector from the private sector. In our view this is a strength, as integration of refugees and immigrants who do not speak English presents special challenges in schools, and the impact of these groups should be given separate consideration. We leave this for future research. Although exit dates are not recorded, we can deduce exits from schools during Years 3-6 on the basis that a pupil's school identifier for their Key Stage 1 tests (Year 2) is different from the school identifier for their Key Stage 2 tests (Year 6).

What we can glean about mobility prior to the Key Stage 1 tests in Year 2 is more incomplete because in each census we only have information on a pupil's last entry date. If they have moved during Years 3-6, we do not know their initial entry date without looking back to earlier census data. This correction is obviously impossible for pupils who are aged 10/11 in the first year (since there is no earlier census to go back to), though becomes increasing accurate as we move forward to the 2005 census. This issue provides another compelling reason, alongside the analytical reasons discussed in Section 0, for focussing on the sample of pupils who we know entered school before or during Key Stage 1 and made no subsequent moves. There is still the potential for error in calculating the proportion of a pupil's cohort who joined during Key Stage 1, because this is derived from this incomplete information on pupil entry. We have assessed the extent to which these errors are important by comparing our main results with what we get when we focus on those pupils in more recent cohorts for whom we have more reliable data, and comparing our main results with those obtained using the latest recorded entry date. As it turns out, these errors in coding the data are not important.

The National Pupil Database thus provides a large and detailed administrative dataset on pupils and their test histories. The test histories contain details on the Levels reached in the core subject areas – Maths, English, Science (Science is only tested beyond Key Stage 1) – and, for Key Stage 2 and beyond, the raw scores in the component tests. We use information on four cohorts: those aged 10-11 and sitting their Key Stage 2 tests in 2002-2005, who took their Key Stage 1 tests in 1998-2001 respectively. Other data sources can be merged in at school level and to pupil home postcode.

In total, this large and complex combined data set gives us information on a population of around 2 million pupils in over 14000 Primary schools in England. We use sub-groups of this population to estimate the effect of peer-group mobility during Key Stage 2 (between ages 7 and 11) using the modelling framework outlined in Section 0.

5 Results

The characteristics of immobile and mobile pupils

Our main sample is taken from the population of age 10-11 primary school pupils in England between 2002 and 2005. We only consider Community schools that accommodate pupils over the entire primary phase (age 4/5 or before, to age 10/11) and drop any schools that closed or opened during our study period. We look at Community schools only so as to reduce the amount of institutional heterogeneity. As discussed above, we mainly focus on the effect of cohort mobility on the sub-population of 'immobile' pupils who entered Community primary schools prior to Year 3 (age 8/9). In total, this gives us around 686000 pupils once we have imposed the various sample restrictions and dropped observations with missing data. The key variables of interest in this sub-group are described in Table 1 panel (a). In panel (b) for comparison purposes, we summarise the same variables for pupils who move school during the primary phase and are recorded joining their Year 6-school at sometime from Year 3 to Year 6.

A first point to note is that academic achievement at Year 6 in Key Stage 2 (age 10/11) is substantially and significantly lower in the mobile group joining from Year 3 to Year 6, than the immobile group. The difference is about 0.89 points or 0.24 standard deviations of the pupil distribution. It is also immediately clear, however, that this is unlikely to be a causal link, because mean achievement of mobile pupils at Key Stage 1 is similarly lower suggesting that these pupils were different even before changing school – though of course mobility may be correlated across phases. The difference at Key Stage 1 is about 0.73 points or 0.21 standard deviations of the pupil distribution. On average, as we would expect given the scaling of the point system, progress over the four years between Year 3 and Year 6 amounts to around 12 points on average (4 years of 3 terms) across pupils and core subjects. This will be the key dependent variable in our regression analysis. As could be deduced from the levels, this mean gain this is lower for the mobile group, by 0.16 value added points[3].

Row 4 shows the explanatory variable on which we focus attention – entry rates during Years 3-6 over the Key Stage 2 phase. This is the mean (unweighted) proportion of each school's Year 6 group that last joined the school during Years 3-6. Based on this, the rate of pupil entry during Years 3-6 experienced by immobile pupils is close to 4% annually with a standard deviation of 2.4%. We show the distribution in more detail in Figure 1a, from which it is clear that a large majority of schools have non-zero entry rates over this period. Relatively few pupils experience entry rates over 10% amongst their peers, and the 95th percentile is about 8.5%. A substantial proportion of the variation in entry rates is within-school (Figure 1b), where the standard deviation is 1.5%.

Turning back to Table 1, we can see that the annual exit rate during this period is, as we would expect, very similar to the entry rate. The similar calculation for entry during Years 1

[3] These figures are based on average level of achievement across Maths, Reading and Writing at Key Stage 1 and across Maths, English and Science at Key Stage 2

and 2 shows a higher average rate of entry of 6.2%. Looking down at the sub-sample of pupils mobile during Years 3-6, we see that these pupils are overly represented in schools with high Year 3-6 mobility rates, because the mean entry rate they experience is 6.6% - well above the 4% experienced by the immobile pupil group in panel (a).

Looking at pupil entry dates, the majority (63.7%) of pupils in the stable sub-population entered school in Reception year (age 4/5) which corresponds to the start of compulsory education in England. The next biggest group (18.7%) start one year before this, in Nursery with a small proportion recorded in nurseries at an earlier age. The proportion of pupils entering during Year 1 and Year 2 during the Key Stage 1 phase is just under 7% in each year. Amongst the sub-set of pupils mobile during Key Stage 2, the proportion joining in each school year falls from 33.5% in Year 3 to 13% in Year 6 just before the tests.

Below this we present a few key pupil background characteristics. The most striking thing is that the proportion on free meals in the immobile group is much lower than in the group that is mobile during Key Stage 2. This fact, that mobile pupils come from more disadvantaged backgrounds has already been documented elsewhere (Machin, Telhaj and Wilson 2006, Strand 2006, DfES 2003). Mobile pupils are less likely to be White British, but also less likely to have English as a second language.

In summary, it is clear that mobile pupils are different from immobile pupils along many observable dimensions, and so we presume, along many unobservable dimensions too. They have lower achievements at age 10/11 but also lower initial level of achievement and differ in terms of income and ethnicity. Given this, and as explained in Section 0, measuring the impact of a pupil’s own mobility on their own achievement is difficult, because these differences are also likely to exist along unobservable lines. Although this is an issue we will return to later, we focus now on the effect that the mobility of these pupils from Years 3 to 6 has on the stable population of pupils who joined a school prior to Year 3 and stay there until Year 6. To do this, we use the modelling framework of Section 0.

Do incoming pupils cause disruption?

Our core regression results on the links between the levels of cohort mobility and pupil achievement are shown in Table 2. We present only the key coefficients and standard errors. Control variables are listed in the table notes, and include indicators (dummy variables) for ethnic group, free-meal entitlement, month of birth, language, gender and year. We also control for school cohort size in Year 6. For our main results in this table and others we focus on pupil entry rates as an indicator of school mobility, because this seems to be most relevant when considering the disruptive impact on incumbent pupils. Exit rates may also be a factor to consider and we turn to this later in Section 0, but we cannot calculate exit rates prior to Year 3-6 from our data.

Turning now to the regression results, Column 1 of Table 2 presents the simple association between the share of the age-11 school cohort that joined the school from Year 3 to Year 6 and the Year 6 achievement (Key Stage 2 points) of pupils who have been in the school since Year 2 or earlier. Clearly, there is quite a strong link: the coefficient implies that a 10% increase in the share entering in an average year between Years 3 and Year 6 is associated with a 0.76 lower point score, equivalent to three-quarters of a term. Controlling for pupil characteristics and own date of entry (prior to Year 3) in Column 2 does not reduce this association by much. Moreover, mobility during Years 1 and 2 (captured by the share entering in these years in Row 2) has only a relatively small and statistically insignificant impact on levels of achievement in Year 6. Columns 3 and 4 switch the analysis to modelling the gain in points between Key Stage 1 (Year 2) and Key Stage 2 (Year 6) tests. The dramatic reduction in the coefficient relative to Column 1 and 2 suggests that these first results are indeed not causal in interpretation. The association of Year 3-6 entry rates with pupil progress over these years is much reduced, implying that much of the apparent ‘impact’ of Year 3-6 mobility on incumbent pupils was already embodied in the Key Stage 1 tests in Year 2. Adding in controls for pupil characteristics and own entry date prior to Year 3 makes almost no difference in Column 3: a 10% increase in pupil entry rates is linked to a 0.2 point decrease in value-added (2-3 weeks progress) during the Key Stage 2 phase. Just as we saw in the case of Year 6 test levels, pupil mobility in Years 1 and 2 (Row 2) does not seem important for progress between Years 3 and 6. Indeed, a near-zero coefficient is exactly what we would expect in a properly specified value added model, given the coefficient on Year 1-2 entry rates as shown in Equation 2.

So far we have no controls for school or for geographical location. In Column 5 we take account of both these factors by estimating the within-school model of Equation (2). The disruptive impact of cohort mobility is now measured from changes in the entry rates within school from cohort to cohort, and so takes account of any persistent factors attributable to the school or its geographical surroundings. Doing this exercise halves the coefficient. Adding in pupil level controls and entry dates in Column 6 changes things slightly, but still the general impression is that an increase in pupil entry rates of 10% per year reduces the attainment of incumbent pupils by about 0.1 value-added point (or 0.1 term) over this 4 year period. Is this amount large or small? From Table 1, this estimate implies that a one standard deviation increase in pupil entry rates decreases academic progress in incumbent pupils by just under 0.01 standard deviations. Apparently then, pupil mobility is not a major contribution to the observed distribution of immobile pupil attainment, although pupils in schools with the few schools with highest rates of mobility could be markedly disadvantaged.

Admittedly we have relatively limited controls for pupil background and family circumstances. To partly address this limitation, Columns (7) and (8) implements an even more stringent test and estimates within school-home-postcode groups. Estimation is now based on comparison of mobility rates experienced by pupils living in the same street and attending the same primary school, but in different age cohorts and hence sitting their Key Stage tests in different years. Controlling for background factors that are correlated with place of residence in this way reduces the sample size quite considerably, but changes the coefficient only slightly in relation to the standard error. In Column 7 without control variables the coefficient is no longer significant, but this improves when pupil characteristics are added in Column 8. We will take Table 2, Column (6) as our preferred specification.

In the above discussion we have not drawn attention to the associations between pupil entry dates and pupil performance although these are presented in the table. Looking across the coefficients in these rows, it is clear that there is a strong association between pupil entry date and their personal academic achievement, though it is hard to pin down causality here. In Column 1, it seems that pupils entering outside of the main entry year (Reception) do less well by Year 6. But this is true whether they enter before or after Reception year so there is no obvious systematic link between late entry and weaker performance. Turning to the columns using the value added specification, the pattern suggests that later entry prior to the Key Stage 1 tests improves pupil progress after Key Stage 1. Looking back at the model of Equation (2) this could either be because late entry improves Key Stage 2 results, or more likely, that late entry has an adverse impact on Key Stage 1 results. Read in this way, the pattern of coefficients in Columns (4), (6) and (8) indicate that entry one year after Reception reduces achievement at Key Stage 1 by the equivalent of 0.2 terms and entry in the year of the tests reduces attainment by the equivalent of just over 0.3 terms. Entry in the years prior to Reception has smaller but significant benefits.

These results in terms of pupil's own entry should be treated with caution given that there will still be important unobserved differences between pupils even within school-home-postcode cells which may be correlated with entry dates. We note, however, that whether or not these entry dates can be treated as exogenous does not seem to have any bearing on our main findings on pupils' cohort mobility; these are largely insensitive to the inclusion or otherwise of pupils' own entry dates.

Taking account of changing school quality and group characteristics

The external effects of mobility that we have found so far are quite small, so we turn now to investigating whether our estimates are attenuated by preferences or other factors – especially Tiebout choice-type moves which we have failed to take into account. For a start, the school fixed effects approach in the above analysis will not successfully take account of the simultaneity induced by the fact that changes in entry rate over time may be driven by changes in school quality over time that are unobservable to us but observable to incoming pupils (or their parents). A priori, we would expect this to attenuate our coefficients (upward bias them) since we would expect a positive association between desirable quality shocks and entry rates. But equally, there are reasons for doubting that this is too serious a problem because school choice decisions for incoming pupils are likely to be based on past or long run information about school quality, and not year-on-year changes in performance that are reflected in cohort-specific test scores. In other words, period-specific shocks to school quality are hard to observe to outsiders who are considering a school move.

However, we do not wish to rule this possibility out on theoretical grounds alone. Therefore, Table 3 presents results from a robustness test based on the idea that period specific shocks or trends in school quality generates changes in mobility in some or *all* cohorts within a school, not just the pupil's own cohort. So, for each pupil *i* taking the Year 6 tests in year *t* in our data we can merge the Year 3-6 entry share of *all* the pupils in the Year 3-6 cohorts in the same school in year *t*. Adding this share in our regressions controls for shocks to school quality (a new head teacher for example) that affect all Year 3-Year 6 pupils in the school over the period that pupil *i* was in Years 3-6. It also controls for persistent school-Year-group specific effects (for example a bad teacher in a specific Year group – in English schools the same teacher will usually specialise in teaching one Year group). These results appear in Column 1 of Table 3 that repeats the specification of Table 2 Column (6), but adding these new whole-school entry rates as controls. In similar fashion, for each pupil taking the Year 6 tests in year t we can merge the Year 3-6 entry share of the Year 6 cohort in t+1, and/or of the Year 6 cohort in t-1. Columns (2) – (4) of Table 3 add these new cross-cohort entry rates. As can be seen, in Columns (1) and (3), doing this hardly changes our estimate of the impact of Year 3-6 entry rates and we still find a 10% increase linked to 0.1-0.15 value added reduction. In Column (3) the point estimate increases in magnitude quite sharply, suggesting a 0.2 point reduction in value-added for a 10% entry rate increase, although this change is in fact due to the different sub-sample imposed by including both the t-1 and t-1 entry rates together (because we lose one year in each case). Equally important, the coefficients on the entry rates of cohorts other than the pupils are insignificant and of positive sign. This indicates that although there may indeed be a weak positive link between school-wide quality shocks and entry rates into the school, this does not attenuate our coefficient of interest.

The above method controls for unobserved period specific shocks and trends to school quality. We can look more directly at whether changes in entry rates form year to year may in some way be related to changes in the composition of the stable population of pupils within schools due to some unobservable sorting process. Although such cohort-specific sorting seems unlikely it is not without foundation. For example, a particularly high-ability school cohort may attract a higher inflow of pupils from outside which could lead to a positive association between entry rates and academic progress of incumbent pupils. We investigate this issue in Table 4 where we test the correlation of entry rates with the mean characteristics of *immobile* school pupils using school level regressions with school fixed effects. We do the tests in two ways using a number of characteristics of incumbent pupils not included in our main models. These characteristics are the pupils' mean test scores in Year 2 and some population census variables that describe the neighbourhood of pupils' home addresses (based on their full postcode). The idea is to see if changes in cohort characteristics that are unobserved in our baseline models could be attracting (or repelling) incoming pupils.

Firstly, in the top panel of Table 4 we regress each characteristic on the entry rate, conditional on the set of pupil characteristics and controls included in the main model[4]. Secondly, we regress the entry rate on the new set of incumbent characteristics, conditional on the pupil characteristics and controls included in the main model and carry out an F-test on the group. As can be seen from the individual standard errors and the F-tests, there is no indication that within-school changes in entrance rates are correlated with the within-school differences in cohort composition, even Year 2 mean test scores. It is also worth noting that the controls in these school level models – the 'observable' pupil characteristics such as free school meals, ethnicity, gender and language that we do include in our main regression – are also uncorrelated with entry rates; the F-test gives a p-value of 0.437. As a further test, we have tried including in our baseline regressions, controls for the group characteristics of the immobile group, the incoming mobile group or both (we used the proportion on free meals, the proportion White British and the proportion with English as a first language).The baseline results are largely unaffected by doing this..

When we consider the impact of entrants, as in Table 2 Column 6, or even in Table 3, there is some danger that our estimate of the disruptive impact of incoming pupils is attenuated because parents organise the timing of their child's change of school in such a way as to coincide with within-school teaching quality changes, resource changes or beneficial cohort composition changes in the destination school. One context in which this is an unlikely eventuality is when school switching is the result of school closure. There are over 1200 closures recorded for England over the period we study, but most are not particularly valuable

[4] A similar test is employed in Cullen, Jacob and Levitt (2005)

to us because they involve amalgamations of school of different phases (e.g. Key Stage 1 and Key Stage 2 phases), name changes or other administrative adjustments that do not involve the re-integration of pupils from one group into another. However, around 7.6% of the pupils moving between schools in Years 3-6 appear to do so because the school they attended at Year 2 had genuinely shut down or merged with another school in such a way that its pupils were redistributed amongst other schools[5].

Pupil movements related to these closures provide us with a further check of robustness of our results to positive self selection of school movers into schools experiencing quality improvements: If this type of self-selection is a major issue, then we would expect to find a bigger negative association between within-school entry rates from closed schools and within-school changes in pupils' value-added scores in destination schools. What we find instead is that the point estimate obtained by replacing Year 3-6 entry rates in Table 2, Column 6 with entry rate of pupils from closed schools is in fact somewhat smaller in absolute value at minus 0.476, although also very imprecisely measured because of the small amount of within school variation in entry rates (the standard error is 1.156). Clearly, this estimate is based on a

[5] We contacted a number of Local Authorities about reasons for closure and policy on re-assignment of pupils. A common reason for closure was falling school roll, perhaps induced by demographic changes or rural depopulation. But unpopularity must rank as a highly likely cause of school decline which immediately leads to doubts that pupils coming from these schools will be in any way typical – especially if the falling role was due to poor educational standards. Moreover, pupils were typically reassigned to schools where there were vacancies and parents were often given a choice of school reallocation. It is in fact easy to show that pupils coming from closed schools are *not* representative and indeed have lower Key Stage 1 Year 2 test scores, are more likely to be on free meals and have other characteristics that are linked to lower educational achievement. They are also more likely to be reallocated to schools which have, on average, lower Key Stage 1 test scores and a higher percentage of pupils on free school meals. On this basis, reassignments from closed schools are not in their raw state at all helpful in providing a source of mobility that is exogenous to pupil or school characteristics. However, it is theoretically plausible, and can be shown using tests akin to those of **Table 4**, that within-school changes in entry rates from closed schools during are uncorrelated to within-school differences in Year-6-cohort characteristics.

relatively small set of destination schools and on small numbers of pupil reallocations. Nevertheless, the point estimate gives us some confidence, at least, that our baseline estimates are not severely attenuated by positive self-selection amongst mobile pupils.

Measurement issues and other robustness checks

There are a number of questions regarding the measurement and specification of mobility and our sample restrictions, to which we now turn. The key coefficients from these robustness checks are presented in Table 5.

The first row shows what happens when we control for exit rates in our baseline specification. So far, we have considered to what extent school-cohort specific *entry* rates have an effect on incumbent pupils in those cohorts. The theoretical assumption behind this is that incoming pupils disrupt teaching because they take time to assimilate and because teachers must devote additional time to these pupils in order to gauge their abilities and set appropriate class tasks. Social integration of new pupils provides, potentially, a distraction for existing pupils as they make friends (or enemies) in their new setting. However, other mobility-related factors – in particular the exit rate – could influence academic progress of the stable population of pupils, for instance if the loss of friends and classmates reduces a pupil's self confidence and requires additional time to be spent reorganising social linkages. There is clearly a problem with including exit rates as a measure of mobility, because, given that it is the best pupils that stay (from Table 1) a high exit rate implies a better residual stock of pupils[6]. Even so, when we include exit rates in our baseline specification, we do, as predicted, find a positive association between exit rates and pupil value-added, but the coefficient on entry rates shifts only slightly from our baseline estimate. This does not change our interpretation of the scale of the disruptive effects of entry.

Another source of error is that entry rates are computed from our data on the basis of mobility in each school-specific cohort. A more accurate representation of the disruption experience by

[6] Given this fact, neither exit rates nor the common index of turnover based on average exit and entry rates (and advocated by Dobson and Henthorne 2000) seem attractive for our purpose.

a particular pupil would be the entry rates into their own class in primary school, but we do not have this information in the data. There are typically two classes per school in an English primary school, but many have just a single form entry. One way to see if measuring entry at school-cohort level dilutes the apparent influence of mobility is to restrict attention to schools where we can be sure that there is only one-form intake. So Row 2 of Table 5 reduces the sample to schools with less than 30 Year 6 pupils recorded taking the tests, 30 being the maximum (but usually the target) class size in English schools[7]. From this specification we obtain an estimate that is less precise than our baseline estimates, but well within the previous range and is still significant at the 5% level. We infer from this test that mis-measuring mobility at the school rather than the classroom level is not misleading.

There could also be concern that entry rates are correlated with class-sizes, either because pupils can only join a class when it is under-capacity or because more entrants add to class sizes. Class size might affect a pupil's academic progress - although the evidence on this provides no consensus (Hanushek 2005, Krueger 2005). We do not have precise class size information and we already control for Year 6 cohort size in the regressions. However, in Row 3 of Table 5 we add an additional control for the number of classes in the school with more than 30 pupils, but this modification to the specification makes little difference. In Row 4 we add the cohort size experienced by the pupil in Year 2. Now the coefficient is reduced in size and significance, though not way out of line from what we had before (given the standard errors). It should be noted that this result corresponds to a rather constrained specification, because variation in entry rates conditional on the initial and terminal stock must be perfectly correlated with exit rates, with all that entails given the discussion of exit rates above.

Other checks we carry out in Table 5 are the following: estimate on the full sample of primary schools, not just Community schools; change from using the Year 2-6 gain as the dependent variable to using regressions with Year 6 tests as the dependent variable, and Year 2 tests on the right hand side; switch from a pupil value-added specification to one with Year 6 test levels as the dependent variable, but now with school fixed effects included. None of the results from these exercises are dramatically different, though the point estimate in the last

[7] This limit is statutory before Year 3 in primary school and schools, and most schools try to adhere to this limit for all age groups.

case is lower. Finally, we show another falsification test: in a specification with school fixed effects, Year 3-6 entry rates have a positive but insignificant association with a pupil's prior Year 2 attainment. On the basis of all these checks, we have no reason to doubt that our main findings are unreliable.

The geography and timing of moves, and heterogeneity in response

Pupils move schools for different reasons, and we might expect the effects of incomers on immobile pupils to differ according to these reasons. Geographical range of moves may matter directly, or may depend on the motivation for switching schools which in turn may generate heterogeneity in responses. For instance, pupils who have moved schools over a long distance because of a family relocation may well find adjustment to their new school more difficult because of the other life adjustments they must make simultaneously. Conversely, pupils who have moved schools locally in search of a better educational match may take little time to readjust and impose lower costs, or even provide benefits, to incumbent pupils. Timing may also be important. We guess that pupils arriving at the beginning of the school year are more likely to have moved for school-choice reasons and that arrival at this time makes integration into school easier. Arrivals mid-term are presumably more likely to be motivated by unexpected life changes, and are probably harder to assimilate. The top panel of Table 6 looks for evidence on this heterogeneity, by splitting the entry rate into different groups according to geographical range – distance quantiles, or within/between Local Authority moves – and timing – either September entrants or other months. Further results in the bottom panel of Table 6 investigate heterogeneity in response according to characteristics of the *incumbent* pupils or the characteristics of the school.

As it turns out, the differences by geographical range of move are not particularly exciting. The coefficients are quite similar in each case, although the standard errors and significance differ according to relative group size. There is some evidence that the shortest 25% moves are the least disruptive (the coefficient is smallest and the standard error largest here) which is in line with expectations, though there is no difference between the impact of long and medium distance moves, or between within and across LEA moves. Differences by month of entry are much more pronounced. Just over 44% of entrants arrive in September and the rest are spread over the rest of the academic year. Scaling the coefficients into annual equivalents it is clear that the numbers in arrival in September is far less disruptive than arrival at other times. We look further to see whether poor pupils entering a school generate a bigger disadvantage than non-poor pupils, but there is no such evidence. Most of the impact is attributable to mobility of the much larger group of non-free meal pupils.

Turning to differences across types of incumbent pupil, we note some differences although these differences are not dramatic. In particular, girls seem lightly more sensitive to peer group turnover than do boys. Poor children (on free school meals) are not, however, any more sensitive than non-poor. Some variation is also apparent when we look at heterogeneity across school types: Firstly, there is evidence of a nonlinearity in response, since schools with a high level of mobility seem less sensitive to changes in mobility than do schools with low and moderate mobility. However pupils in the largest schools show markedly more response to a given percentage point change in entry rates, indicating that the absolute *number* of mobile pupils may be a relevant factor. Pupils in schools in metropolitan areas are more sensitive to cohort mobility than others, but not significantly so. All-in-all however, the responses seem quite general and do not appear to be solely driven by specific types of pupil – either incumbent or mobile.

Do mobile pupils show less academic progress?

In the final part of our analysis we briefly turn to the performance of mobile pupils. This is the group whose characteristics are summarised in the lower panel of Table 1. So far we have looked at the impact of their mobility on other pupils. Now we consider briefly their own academic progress between ages 7/8 and 10/11. As we highlighted in Section 0, it is not easy to disentangle causal effects in this group, because the disruption associated with their school move is wrapped up with personal gains from a better individual-school match, a quality change associated with the school move, and unobserved pupil-specific attributes that lead to a higher move propensity. Even though we do no wish to make any claims about how mobility disadvantages this group relative to other pupils who stay put over this school phase, we argue that something can be inferred by looking at the links between move timing and pupil progress. Inspection of the coefficients in Table 7 in both Column 1 and Column 2 –

Column 2 is our preferred within-school specification – suggests that mobility later in the phase is increasingly disruptive to a pupil's own progress. Moving in a given year in Key Stage 2 seems to reduce progress over this 4-year phase by an average of 0.085 value-added points, about 1 week or 3.5% of one-standard deviation.

Of course even this small apparent disadvantage may just be an indication that pupils who move later are different in terms of unobservable ability-related characteristics. One way we can test this is to look again at pupils moving from schools that are closed or merged with other schools, which we do in Column 3 (OLS) and Column 4 (within-school). These specifications can be thought of as reduced-form estimates of a model in which entry from closed schools in a given year is used as an instrument for entry date, and estimate how entry from closed schools in a given year affect value-added relative to pupils entering from other schools at any time over these years. Although we know from our data that these pupils are not representative in terms of initial attainments or background characteristics, and tend to come from lower quality schools and go to lower quality schools, we have no reason to believe that those moving *later* in the school phase are any different from those moving *earlier* so the differences between the entry year coefficients within Column (3) or Column (4) seem to indicate some causal process at work. The pattern is similar to that in Column (1), and still suggests a progressive disadvantage from late entry, with a year's delay to entry slowing down pupil progress by about 0.08 value-added points. This finding begs the question as to why later entry should slow down progress more than entry at another time. The most obvious explanation, and one that accords with general perception of how things are done in England's schools, is that teaching during this school phase is not uniformly distributed over the school years. So, a pupil who loses learning time in adapting to a new school in Year 6 is much more disadvantaged than a pupil joining in Year 3 simply because a lot of the preparation for the Year 6 tests is condensed into the later stages of the Key Stage 2 phase.

It is also worth noting that we find no relationship here between within-school group mobility and the individual attainments of the group members once we introduce school fixed effects (there is insufficient within school variation to measure this properly), but, without fixed effects, the estimates in Column (3) suggest that the disruptive impact of pupils from closed schools is of a similar order to our baseline estimates in Table 2.

6 Discussion and Conclusions

Policy makers and educational practitioners have in the last decade begun to acknowledge that high pupil mobility could contribute to poor school performance. It has been recognised before that pupils who change school more often do have lower achievements, but it is arguable to what extent this is due to mobility or simply due to the disadvantageous background characteristics and initial conditions that are correlated with ability (Strand 2002, Strand and Demie 2006). Moreover, the disruptive impact of a pupil's mobility on their own attainment can be masked by the gains from changing schools: pupils often move in search of schools that match their educational needs more closely (Hanushek et al 2004). This is a debate on which we are unable to shed much new light given the data that we have available, though we show in contrast to Strand and in line with Hanushek et al that changes of school do slow down progress slightly during primary schooling, other things equal. Early moves are better than late moves, and each year's delay in entry between ages 8 and 11 is associated with a set–back of one week, or 4% of one-standard deviation, in terms of test score 'value-added' over these years. This is of similar magnitude, though difficult to compare with, the shock to annual pupil 'value-added' induced by a pupil move is reported in Hanushek et al (2006).

Our paper focuses mainly on the *external* effects of mobile pupils on other pupils. We estimate the expected disadvantage to a pupil from being educated in a school age-cohort that experiences high mobility, relative to a similar pupil in a preceding or subsequent age-cohort in the same school who experiences lower mobility. This is a particularly pertinent issue for school policy because of the economic case for intervention when the school choice and timing decisions of some pupils have an impact on the educational outcomes of others. We *do* find that mobility is disruptive and immobile pupils in mobile cohorts progress less well between ages 8 and 11 on average. However, it should be emphasised that this is not a major driver of differences in educational progress between pupils: Observed levels of mobility (one standard deviation) can account for only 1% of one-standard deviation of the variation in pupil performance. Again, this result is very close to that reported for Texas by Hanushek et al. A pupil who experiences a 10% per year rate of new pupil entry per year between ages 8 and 11 (4 pupils per year on average) can only expect to be 1-2 weeks behind a pupil whose school cohort is completely stable throughout the period.

One reason we find such low figures may be because schools already compensating for high levels of mobility. Certainly there are not yet any general administrative rules for allocating resources across schools on the basis of pupil mobility, and our estimates are based on within-school changes so we doubt that this can be happening systematically. However, if schools reallocate resources internally from lower to higher mobility classes then we may be substantially underestimating the disruption caused by mobile pupils in the absence of such compensatory reallocations. We are, however, somewhat sceptical that this provides a credible explanation: if resources are being reallocated within schools, we would expect to find the costs of mobility in one cohort shared across younger and older cohorts in the same school, but we found no evidence of this happening. It should also be emphasised that our analysis excludes recent immigrants and asylum seekers – a substantial proportion of incoming pupils in some schools in London – because we consider only pupils with record in English schools.

Mobile pupils certainly perform less well than immobile pupils, on average, but what emerges from our work then is a picture in which only a very small part of the performance gap between low-mobility and high-mobility schools could be put down to the *externalities* associated with pupil mobility and disruption. Our more tentative conclusions on the direct costs to a pupil of their own mobility also suggest that these costs are quite small too. Policy to provide additional resources to schools with high mobility rates may well be justified on grounds of equity, but the main basis for doing so would seem to be that mobility provides a good indicator of pupils 'at risk' for low achievement and because it is a way of compensating schools for more general background disadvantages. Mobile pupils do not appear to impose large external costs on other pupils, so there does not seem to be a clear economic rationale for policy to reduce mobility or to compensate incumbent and immobile pupils for the high class-room mobility they may experience.

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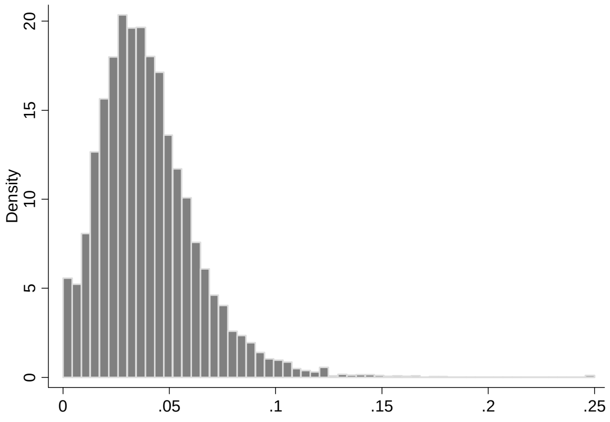
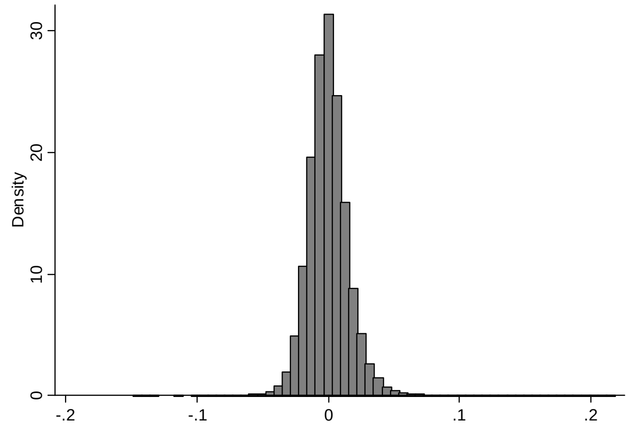
Figure 1: Density of annual entry rates experienced by immobile pupils in Years 3-6 in Key Stage 2, English primary schools 2002-2005



Year 3-6 annual entry rates



Year 3-6 annual entry rates, deviation from school mean

Table 1: Descriptive statistics for the Estimation Sample

Pupils last entering school before Year 3

	Mean	Standard deviation
Key Stage 2 (Year 6) points	27.947	3.773
Key Stage 1 (Year 2) points	15.254	3.400
Value-added points Years 3 to 6	12.694	2.575
Annual share entering during Years 3 to 6	0.040	0.024
Annual share entering Years 1 to 2	0.062	0.058
Entry Reception -2	0.039	0.194
Entry Reception -1	0.187	0.390
Entry Reception	0.637	0.481
Entry Year 1	0.070	0.255
Entry Year 2	0.067	0.250
Pupil Numbers Year 6	42.400	17.896
Free meals	0.168	0.374
White British	0.821	0.383
English first language	0.900	0.301
Sample size	686041	

Pupils last entering school Year 3-6

	Mean	Standard deviation
Key Stage 2 (Year 6) points	27.056	3.967
Key Stage 1 points	14.526	3.605
Value-added points Years 3 to 6	12.530	2.829
Annual share entering during Years 3 to 6	0.066	0.047
Annual share entering Years 1 to 2	0.064	0.059
Entry Year 3	0.335	0.472
Entry Year 4	0.272	0.445
Entry Year 5	0.262	0.440
Entry Year 6	0.131	0.337
Pupil Numbers Year 6	41.935	20.100
Free meals	0.260	0.438
White British	0.805	0.396
English first language	0.907	0.290
Sample size	147264	

Table 2: Entry to Primary School, and progress between Years 3 and 6, immobile pupils entering before Year 3

	Year 2 level, no controls (1)	Year 2 level (2)	Year 3-6 value-added, no controls (3)	Year 3-6 value-added (4)	Within-school, no controls (5)	Within-school (6)	Within school-postcode, no controls (7)	Within school-postcode (8)
Annualised cohort share entering Years 3-6	-7.617 (0.587)	-6.356 (0.489)	-1.938 (0.394)	-1.967 (0.384)	-1.030 (0.374)	-1.203 (0.375)	-1.349 (0.722)	*-1.553* (0.723)
Annualised cohort share entering Years 1-2	-	-0.248 (0.218)	-	-0.289 (0.192)	-	-0.228 (0.190)	-	-0.464 (0.364)
Entry Reception –2	-	-0.335 (0.047)	-	-0.270 (0.040)	-	-0.233 (0.024)	-	-0.229 (0.057)
Entry Reception –1	-	-0.305 (0.030)	-	-0.104 (0.025)	-	-0.090 (0.015)	-	-0.083 (0.035)
Entry Reception	-	Baseline	-	Baseline	-	Baseline	-	Baseline
Entry Year 1	-	-0.248 (0.023)	-	0.189 (0.018)	-	0.174 (0.014)	-	0.193 (0.035)
Entry Year 2	-	-0.341 (0.024)	-	0.303 (0.019)	-	0.335 (0.016)	-	0.351 (0.040)
Pupil numbers in school Year 6	-	0.004 (0.001)	-	-0.001 (0.001)		-0.004 (0.001)	-	-0.003 (0.002)
Postcode fixed effects	No	No	No	No	No	No	Yes	Yes
School fixed effects	No	No	No	No	6871	6871	407831	407756
Pupils	687245	686041	686226	686041	685262	686041	685262	685077

Notes: Table shows coefficients from regression of pupil test results on annualised school, cohort-specific entry rates. Standard errors clustered on school shown in parentheses. Underline significant at 1% level. Italic significant at 5%. In Columns (5) and (6) the effective sample size is 403,400 based on observations that have within postcode-school cell variation in cohort-specific entry rates. Estimation sample is pupils in Year 6 (age 10-11) in 2002-2005 in Community primary schools who joined their current school between Nursery and Year 2 (age 6-7). Schools included are those open prior to 1998 with combined Key Stage 1 and Key Stage 2 phases. Controls include pupil eligible for free meals, seven ethnic group dummies, English not first language, female, number of pupils in school-year, month of birth and month-of-entry within school year, academic year dummies.

Table 3: Entry to Primary school, and progress between ages 7 and 11, immobile pupils entering before Year 3; cross-cohort falsification tests

	(1)	(2)	(3)	(4)
Annualised cohort share entering Years 3-6	-1.306 (0.424)	-1.003 (0.481)	-1.510 (0.465)	-2.199 (0.793)
Annualised cohort share entering Year 1-2	-0.275 (0.214)	0.376 (0.220	0.285 (0.247)	0.310 (0.390)
Whole school entry rate Years 3-6	0.451 (1.011)	-	-	-
Whole school entry rate Year 1-2	0.544 (1.248)	-	-	-
Annualised cohort share entering Years 3-6 (t-1)	-	0.250 (0.449)	-	-0.328 (0.725)
Annualised cohort share entering Year 1-2 (t-1)	-	0.099 (0.233)	-	0.095 (0.378)
Annualised cohort share entering Years 3-6 (t+1)	-	-	0.365 (0.415)	-0.181 (0.646)
Annualised cohort share entering Year 1-2 (t+1)	-	-	-0.030 (0.212)	-0.361 (0.336)
School fixed effects	Yes	Yes	Yes	Yes
Schools	6871	6674	6673	6498
Pupils	686041	506553	506674	331609

Notes: Table shows coefficients from regression of pupil test results on annualised school, cohort-specific and cross-cohort entry rates. Standard errors clustered on school shown in parentheses. Underline significant at 1% level. Italic significant at 5. Estimation sample is pupils in Year 6 (age 10-11) in 2002-2005 in Community primary schools who joined their current school between Nursery and Year 2 (age 6-7). Schools included are those open prior to 1998 with combined Key Stage 1 and Key Stage 2 phases. Controls include pupil eligible for free meals, seven ethnic group dummies, English not first language, female, number of pupils in school-year, month of birth and month-of-entry within school year, academic year dummies.

Table 4: Correlation of Year 3-6 entry rates with various pupil characteristics

Separate regressions of incumbents' characteristics on annualised proportion entering Years 3-6, conditional on controls in Table 3	
Year 2 test score mean	0.075 (0.435)
Home census output area proportion level 4 qualified	-0.003 (0.010)
Home census output area proportion living in socially rented homes	0.039 (0.024)
Home census output area proportion in employment	-0.020 (0.012)
Home census output area proportion non-qualified	0.022 (0.013)

Regression of annualised proportion entering Years 3-6 on incumbents' characteristics together	
Year 2 test score mean	0.000 (0.000)
Home census output area proportion level 4 qualified	0.037 (0.027)
Home census output area proportion living in socially rented homes	0.007 (0.010)
Home census output area proportion in employment	-0.003 (0.019)
Home census output area proportion non-qualified	0.035 (0.028)
F-test on above 5 variables	p= 0.317
F-test on pupil characteristics used as controls	p= 0.468

Table 5: Additional robustness checks for impact of annualised Year 3-6 cohort entry rates on pupil mobility

Controlling for Year 3-6 exit rates	-1.490 (0.387)
Schools with less than 30 pupils (one class entry)	*-1.000* (0.506)
Controlling for number of classes with more than 30 pupils	-1.188 (0.376)
Controlling for Year 2 *and* Year 6 cohort size	*-0.811* (0.387)
Including other primary school types – Voluntary Aided, Voluntary Controlled, Foundation	-1.393 (0.273)
Year 6 test levels as dependent variable, controlling for Year 2 test level, with school fixed effects	-1.133 (0.354)
Key Stage 2 levels as dependent variable, with school fixed effects	*-0.756* (0.366)
Key Stage 1 levels as dependent variable, with school fixed effects	0.445 (0.330)

Notes: Table shows coefficients from regression of pupil test results on annualised school, cohort-specific and cross-cohort entry rates. Standard errors clustered on school shown in parentheses. Underline significant at 1% level. Italic significant at 5% level. Estimation sample is pupils in Year 6 (age 10-11) in 2002-2005 who joined their current school between Nursery and Year 2 (age 6-7). Schools included are those open prior to 1998 with combined Key Stage 1 and Key Stage 2 phases. Controls include entry rates in Years 1-2, pupil eligible for free meals, seven ethnic group dummies, English not first language, female, number of pupils in school-year, month of birth and month-of-entry within school year, academic year dummies.

Table 6: Entry to Primary school, and progress between ages 7 and 11. Heterogeneity in mover type, and heterogeneity in response

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	(1)		(2)
Annualised share, bottom 25% distance	-0.809 (1.130)	Annualised share Years 3-6, September	*-0.117* (0.054)
Annualised share, middle 50% distance	-1.328 (0.489)	Annualised share Years 3-6, other	-1.303 (0.439)
Annualised share, top 25% distance	*-1.364* (0.625)	Equality F-test	p= 0.003
Equality F-test	p=0.003		
	(3)		(4)
Annualised share, within LEA move	-1.363 (0.470)	Annualised share entering Years 3-6 not on free meals	-1.577 (0.418)
Annualised share, between LEA move	-1.125 (0.591)	Annualised share entering on free meals	-0.607 (0.792)
Equality F-test	p=0.748	Equality F-test	p=0.000
	(5)		(6)
Annualised share entering Years 3-6	*-0.917* (0.401)	Annualised share entering Years 3-6	-1.189 (0.377)
× Girls	*-0.229* (0.190)	× FSM	-0.080 (0.438)
	(7)		(8)
Annualised share entering Years 3-6	*-2.117* (0.865)	Annualised share entering Years 3-6	*-0.774* (0.394)
Annualised share entering Years 3-6 – squared	8.555 (6.922)	× Above median school cohort size	-1.781 (0.587)
	(9)		
Annualised share entering Years 3-6	*-0.979* (0.456)	-	-
× metropolitan school	-0.579 (0.786)	-	-

Notes: Table shows coefficients from regression of pupil test results on annualised school, cohort-specific entry rates. Standard errors clustered on school shown in parentheses. Underline significant at 1% level. Italic significant at 5. Estimation sample is pupils in Year 6 (age 10-11) in 2002-2005 in Community primary schools who joined their current school between Nursery and Year 2 (age 6-7). Schools included are those open prior to 1998 with combined Key Stage 1 and Key Stage 2 phases. Controls include pupil eligible for free meals, seven ethnic group dummies, English not first language, female, number of pupils in school-year, month of birth and month-of-entry within school year, academic year dummies.

Total number of pupils making Year 3-6 moves = 147264. Of these, within LEA, 64.3%, in September 44.3%, poor 16.8%

Metropolitan schools are those in Local Authorities in London, Greater Manchester, Merseyside, South Yorkshire, Tyne and Wear, West Midlands, West Yorkshire, representing 84% of pupils.

Table 7: Entry to Primary School in Years 3-6, and progress between ages 7 and 11, pupils in Community schools with Key Stage 1 and Key Stage 2 phases entering in Years 3-6

	Age 7-11 value-added (2)	Within-school (3)	From closed or merged schools (4)	Within-school, from closed or merged schools (5)
Annual share entering Years 3-6	-1.711	-0.105	*-0.766*	0.185
	(0.380)	(0.472)	(0.384)	(0.378)
Entry Year 3	baseline	baseline	*-0.111*	-0.099
			(0.062)	(0.060)
Entry Year 4	-0.043	-0.052	-0.173	-0.211
	(0.021)	(0.020)	(0.062)	(0.061)
Entry Year 5	-0.125	-0.128	-0.365	-0.390
	(0.021)	(0.020)	(0.063)	(0.064)
Entry Year 6	-0.252	-0.231	-0.348	-0.447
	(0.027)	(0.026)	(0.086)	(0.080)
School fixed effects	No	Yes	No	Yes
Pupils	147264	147264	147264	147264

Notes: Table shows coefficients from regression of pupil test results on annualised school, cohort-specific entry rates. Standard errors clustered on school shown in parentheses. Underline significant at 1% level. Italic significant at 5%. Estimation sample is pupils in Year 6 (age 10-11) in 2002-2005 in Community primary schools who joined their current school between Nursery and Year 2 (age 6-7). Schools included are those open prior to 1998 with combined Key Stage 1 and Key Stage 2 phases. Controls include pupil eligible for free meals, seven ethnic group dummies, English not first language, female, number of pupils in school-year, month of birth and month-of-entry within school year, academic year dummies.