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Teacher Turnover: Does it Matter for Pupil Achievement?

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Abstract
We add to a small literature examining whether teacher turnover affects academic achievement, focussing on age-16, state secondary school students, using a unique dataset of linked students and teachers in England. We advance previous work by: a) looking at entry rates and student achievement in subject groups across which there is unlikely to be non-random selective assignment; and b) by looking at a context where students study a curriculum for two years during which they will generally be taught by the same teachers. This allows us to estimate the effects of getting a new teacher mid-way through the teaching period. Our identification is based either on a school fixed effects design which exploits year on year variation in turnover in different subject groups, within schools, or a student fixed effect design where identification is based on cross-section variation in turnover in different subjects, in the same school experienced by the student. Both methods give the same results: a higher teacher entry rate has a small but significant negative effect on students' final qualifications from compulsory-age schooling, despite organisational responses which assign new teachers to less risky grades. This result is robust to wide range of identification and robustness tests. Our findings point to the general disruption and lack of continuity in teaching as the main mechanism through which turnover harms student attainment .

Key words: teachers, turnover, student attainment, schools
JEL: H4; I2; J24

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1. Introduction

Recent research has established that teachers matter for student achievements, albeit because of dimensions of 'teacher quality' that are largely unexplained. On the basis of this evidence, recent policy in the US has, sometimes controversially, moved towards hiring and firing teachers on the basis of measurable impacts on student test scores (see for example, Thomsen 2014, and discussion in Hanushek 2009, Adnot et al 2017, Rothstein 2015). These kinds of hiring/firing policies, self-evidently, have limited aggregate implications if the supply of teachers is constrained (Rothstein, 2015). However, turnover has potential benefits, on aggregate, because it is the mechanism by which: teachers gain a variety of experience; new ideas are brought into schools; and productive teacher-school matches are formed. On the other hand, there are also potential costs for individual students, schools, and, on aggregate, from the disruptive effects of turnover. New arrivals take time to assimilate, leavers take school-specific experience with them, different teachers have different teaching styles causing a lack of continuity and turnover absorbs financial and administrative resources. These disruptive effects from teacher turnover could potentially offset any of its advantages, at least in the short run. In the US, England, and elsewhere there is a presumption amongst policy makers and practitioners that turnover has, on average, adverse impacts. Turnover of teachers is also perennial concern for parents, particularly when it occurs during the period when students are studying for important exams. [1]

Despite the popular importance of this issue, there are relatively few quality studies that investigate it empirically, the recent exceptions being Ronfeldt, Loeb and Wyckoff (2013), Hanushek, Rivkin, and Schiman (2016), Atteberry, Loeb and Wyckoff (2017). Our paper adds to this rather sparse existing evidence on the causal impacts of teacher turnover. By causal impacts, we mean the average gap in achievements between students experiencing a high turnover of teachers and students experiencing a low turnover of teachers, in a hypothetical experiment in

[1] A browse of the mumsnet.com website confirms this.

which teachers, their entry probabilities, their exit probabilities, and their students are all randomly assigned. The analysis is based on a large administrative dataset of teacher workforce records linked by school and subject categories to students' achievement records in England over five recent cohorts. For most of the analysis we focus on entry rates as an indicator of turnover – for reasons elaborated later on – though we also look at exit rates.

Our key finding is that students experiencing high teacher turnover do less well in their end-of-school exams. The effects are quite small, though non-negligible relative to other factors that have been found to affect student achievement. A 10 percentage point increase in teacher annual entry rates (relative to a mean of 14% percent, and around 60% of one standard deviation) reduces student point scores (a kind of GPA) by just under 0.5% of one standard deviation. This is a standardised effect size of 0.8% of a standard deviation. Interestingly, this effect is of a comparable order of magnitude to the external effects of turnover on other dimensions of the educational system that have been investigated – similar to the externalities from the turnover of students in schools (Gibbons and Telhaj, 2011; Hanushek, Kain and Rivkin 2004) and slightly larger than the effects of turnover of students in neighbourhoods (Gibbons, Silva and Weinhardt 2015). Evidently, teachers entering and leaving matters, but this is no more disruptive to education than turnover amongst a student's peers.

Although our analysis of administrative records is necessarily unable to precisely articulate the behavioural channels through which turnover affects achievement, we say something about the potential mechanisms by looking at heterogeneity across the qualifications, age and experience of teacher who are entering and leaving, and across types of student. Although there is some variation, the impact is quite general. We also show that the effects are insensitive to a wide range of controls for teacher age, salary and experience, implying that the effects are not due to changes in workforce composition. The results suggest, instead, that the adverse impacts on achievement are due to new teachers disrupting continuity in teaching for students and having no experience specific to the institution they join.

We advance the existing literature in a number of ways. Firstly, in line with arguments in Hanushek, Rivkin and Schiman (2016), we worry about potential reassignment of new teachers to student groups that are lower or higher performing. Therefore, we focus on the 'intent to treat' impact of teacher entry into subject groups, across all grades in a school in a given year, on the final school qualifications of students taking their exams in that year. These subject-school groups are akin to school departments. There is an advantage of this approach, over, say comparing the performance of students in a year when they are allocated a new teacher with those who are not (Atteberry, Loeb and Wyckoff 2017), or comparing the performance of students experiencing different rates of teacher entry in specific grades (Ronfeldt, Loeb and Wyckoff 2013). The advantage is that it is hard to reallocate specialised secondary school teachers across subjects. This mitigates concerns about selective allocation of new teachers to lower or higher performing students or student groups within a school. The improvement over using whole school-by-year entry rates without splitting by subject (Hanushek, Rivkin and Schiman 2016), is that we can control more effectively for school-by-year shocks using fixed effects' estimation. Our research design, therefore, identifies the effects of turnover on achievement from school-subject-year specific shocks in the final year of secondary school. We exploit over time variation in turnover within school-subject groups, controlling for school-year and subject-year shocks. We also present estimates based on within student variation in exposure to turnover across subject in the same academic year. The similarity of results from this strategy with results for our preferred specification suggests that negative effects of turnover are not driven by unobserved student heterogeneity.

Another disadvantage of studies that analyse the effects of grade-specific variation in turnover is that students are themselves moving between grades and will typically experience a change in teachers regardless of levels of turnover. Therefore, any estimates of turnover based on this type of design will omit effects due to disruption in the continuity of teaching experienced by students – which is one of the main potential channels. Our analysis, in contrast, looks at turnover in subject groups in the middle of a two-year period where students are preparing for their crucial end of school exams, and where disruption is often thought to be particularly important. Typically, students will be taught by the same teachers over this period, and even if they are not, turnover in a department over this period will cause disruption to the organisation of the teaching for students approaching their final exams. We are, therefore, more likely to capture these effects from lack of continuity, alongside any effects related to incoming teachers having no teaching experience specific to that school. Note, this finding is relevant to other contexts in which students experience mid-year disruption due to a change of teacher.

A further refinement of previous work is to demonstrate, through a range of placebo, balancing and robustness tests that we can treat school-subject-year turnover as random, conditional on the various sets of fixed effects. We provide a number of tests to show that our results appear to be causal in that teacher turnover is uncorrelated with student demographics, conditional on our fixed effects design ('balancing'); that we do not observe effects on groups of students who we would not expect to be affected ('placebo'); and that the observed impacts of turnover relate quite precisely to achievement in the years in which we observe the turnover ('event study').

One concern over our 'intent to treat' estimates based on school-subject-year turnover is that they may understate the impacts of teacher entry, if new teachers are assigned to students in grades other than that for which we measure student outcomes (i.e. there is non-compliance with the treatment). We investigate this issue by examining the extent to which schools assign new teachers to grades other than the high-stakes, final exam-taking grade for which we measure student outcomes. Indeed, we find that new teachers, particularly if they are new to the profession, are less likely to teach this grade. Our main estimates are thus potentially a lower bound on the causal impacts of randomly assigning new teachers to students, although further analysis using information on the grade in which a teacher teaches suggests the downward bias is not large. This result is in itself important because it sheds some light on the extent to which school re-organisation may lead to underestimation of the impacts of many types of school intervention or shock. This is a pervasive concern throughout education policy evaluation because it implies that estimates of policy interventions on student outcomes might be lower than what policy makers and researchers might have hoped or expected unless researchers allow for this kind of organisational readjustment. We further show that school organisational quality is an important dimension here, with schools rated 'Outstanding' by the school inspection authorities being less likely to assign new teachers to the grade taking their final qualifications, and experiencing less of an effect from teacher turnover.

The rest of the paper is structured as follows: The next section briefly summarises previous findings on the topic. Our empirical strategy is discussed in Section 3. Section 4 describes the education institutional setting in the UK and the data set. Section 5 presents our main regression results, with Section 6 investigating the robustness of the analysis and Section 7 taking a more nuanced look at the variation in the effects across different types of teacher, student and subject. Section 8 provides concluding remarks.

2. Results from previous studies of teacher turnover

Although recently there has been a growing body of research examining patterns of teacher turnover (e.g. Ost and Schiman, 2015, Atteberry, Loeb and Wyckoff, 2017), studies investigating the direct link between turnover and student attainment are still thin on the ground. One of the reasons is lack of data, which makes it difficult for researchers to causally identify the direct impact of turnover on student attainment. The first large scale study, directly comparable to ours, is Ronfeldt, Loeb and Wyckoff (2013), which looks at teacher turnover on 4th and 5th grade student performance in New York elementary schools. Their study finds that teacher turnover reduces achievement in both Mathematics and English, particularly for students in schools with a high proportion of low performing and black students. The fixed effects estimation strategy is similar to ours, but exploits within-school variation in turnover between grades and years. Hanushek, Rivkin and Schiman. (2016), reviewing Ronfeldt, Loeb and Wyckoff (2013) study, highlights the importance of controlling for within-school grade re-assignment of teachers and estimate model specifications that aggregate turnover and grade reassignments at the school-by-year level to address problems introduced by the non-random sorting of teachers among grades. Using data from a Texas district for teachers and students in grades 4 through 8 between 1996/97 and 2000/01, they find that teacher turnover has adverse effects on student academic achievement only in disadvantaged schools. Atteberry, Loeb and Wyckoff (2017) examine how different types of switches (new to profession, district, school, and/or grade re-assignment) affects the attainment of New York City students in grades 3 through to grade 8 in a fixed effects' approach. They find that achievement is the lowest for students of teachers new to profession, followed by teachers who are new to district or school.

Adnot et al (2017) also study the effects of turnover on achievement, but are interested in the effects of exits in context of a policy environment which encouraged exits of low performing teachers (the IMPACT programme). They find, unsurprisingly, that exits of underperforming teachers raise student achievement, but their study is silent on the impact of disruption caused by new entrants. Similar findings appear in Chetty, Friedman and Rockoff (2014) who document too that: entry of good teachers raises achievement; entry of bad teachers lowers achievement; exit of good teachers lowers achievement; and exit of bad teachers raised achievement (where quality is based on teachers' previous history of generating high test scores).

Our research also relates to a broader literature on teacher turnover which looks into the factors that cause teachers to enter and leave schools and investigating the consequences of sorting for the composition of the teaching workforce (e.g. Ingersoll 2001; Dolton and Newson 2003; Allen, Burgess and Mayo 2018). The typical finding is that schools serving disadvantaged young people have higher turnover than other schools. From amongst this literature, Hanushek and Rivkin (2010) argue that turnover is potentially beneficial because bad teachers leave and good teachers tend to stay in their sample of schools in Texas, though the aggregate implications are not very clear if teachers are just moving to and from schools elsewhere. They also focus only on the effects attributable to changes in composition, rather than any disruptive impacts.

3. Empirical Strategy

Our aim is to estimate the average causal impact that turnover of teachers in schools has on the academic achievement of their students. Conceptually, the idea is to understand the impact of randomly increasing the rate at which teachers enter and/or leave a school, holding other characteristics of the workforce, school and student body constant.

There are several basic empirical issues. Firstly, there are various ways to define and measure turnover. In line with previous work on student and teacher mobility (Hanushek and Rivkin 2004, Gibbons and Telhaj 2011, Ronfeldt, Loeb and Wyckoff 2013, Hanushek, Rivkin and Schiman 2016), we focus the entry rate in a given year to represent turnover. Our design, based on year-to-year shocks to turnover, necessitates short term turnover indicators, rather than long term measures of turnover, churn and instability discussed in Holme et al (2018). The reasons for focussing on entry are elaborated at the end of this section, though we also look at exit rates. Secondly, there are obvious potential endogeneity problems. Entry rates (and other measures of turnover) will be, in part, determined by the characteristics of the school, its students and the characteristics of stock of teachers, since these factors will affect the exit rate (and hence the number of vacancies) and how attractive a school is to potential applicants. Moreover, sorting implies that teachers entering a school, the teachers in the stock and the teachers leaving are not likely to be identical, so entry and exit rates can change the composition of the school workforce. All of these factors may have direct effects on achievement and are only partially observed.

We address these endogeneity issues using a fixed effects regression design, in which we regress student exam outcomes in the final year of compulsory schooling (Year 11, age 16) on teacher entry rates at school-by-subject-by-year level – i.e. measuring the entry rate for teachers in a school teaching a particular subject, in a given year for all grades. In our main regression specification, identification comes from year-to-year changes in entry rates within school-subject categories, conditional on school-by-year and subject-by-year fixed effects. In other words, identification comes from year-to-year changes in subject-specific turnover shocks, partialling out year-to-year shocks to turnover across all subjects within the school, and year-to-year shocks to turnover in each subject across all schools. Our preferred specification is thus:

$$quals_{isqt} = \beta mob_{sqt} + x_i'\gamma + z_{sqt}'\lambda + a_{qt} + b_{st} + c_{qs} + \varepsilon_{isqt} \qquad (1)$$

Where $quals_{isqt}$ is an index of individual *i* achievement in age-16 qualifications (in school *s*, subject *q* and year *t*), and mob_{sqt} is the entry rate (or other turnover measure) in each school-subject-year group. Coefficient β, the coefficient of interest, is the expected change in student test scores associated with an exogenous increase in turnover in the year in which a student takes their age-16 exams. The vector of optional student-specific control variables, x_i, includes: prior age-11 primary school test scores; Free School Meal (FSM) eligibility; gender; and ethnicity (white/others). Unobservable factors a_{qt}, b_{st}, c_{qs}, are treated as fixed effects and partialled out during estimation.[2] The optional vector of control variables at school-subject-year or school-year level, z_{sqt}, includes: the pupil-teacher ratio; proportion of female students; proportion FSM eligible students; proportion of white British students; number of teachers in current and past academic year; average age and experience of teachers; share of female teachers and average log annual salary for teachers. This rich set of control variables allows us to net out time-varying confounders correlated with turnover and to test for the relevance of the effects of turnover-related sorting on the composition of the workforce. We cluster standard errors at school level to allow for serial correlation in unobservables over time and heteroscedasticity at school level.

[2] We use within-groups estimation, or the numerical procedure of Correia (2017) as implemented in the command reghdfe in Stata.

In an extension to this design, we control for student fixed effects and subject-by-year fixed effects, so identification comes purely from variation in entry rates across subjects experienced by a student in a given school and year.

$$quals_{isqt} = \beta mob_{sqt} + z'_{sqt}\lambda + a_{qt} + d_i + \varepsilon_{isqt} \qquad (2)$$

In other words, we examine whether students who face higher teacher mobility in, say, Mathematics than in English have lower academic performance in Mathematics rather than in English. This between-subject, within-student design has featured in several previous papers, Dee (2005), Clotfelter, Ladd and Vigdor (2010), Slater, Davies and Burgess (2012), Altinok and Kingdon (2012), Lavy, Silva and Weinhardt (2012), Nicoletti and Rabe (2017). The key difference between the strategies in (1) and (2) is that the latter eliminates any time series variation within school-subject groups, and identification is based purely on cross sectional variation across subjects within students (and schools) in a given year.

The identifying assumption underlying these strategies is that teacher entry into a school-subject-year group is determined by the choices of teachers outside the school with only limited information about the characteristics of the students, the other staff and the school environment in general. This is especially true because teachers almost always join at the beginning of the school year when they would have no information about the future KS4 performance of the student-subject group they are joining. Teachers' decisions about entry are, therefore, largely dependent on persistent or time-varying school level and subject level factors. School-subject-year specific entry rates can, therefore, be rendered plausibly exogenous by appropriate conditioning on fixed effects and observable school characteristics. We assess the credibility of this identifying assumption by showing that these subject-school-year specific shocks to turnover rates are largely uncorrelated with observable school, teacher and student characteristics, and by various 'placebo' tests.

The above considerations suggest that entry rates are better than exit rates as measures of turnover. End-of-year exit rates from a school-subject-year group are determined by the choices of teachers inside the school, with good information about the cohort of students they have been teaching. General school cohort quality shocks are taken care of by our school-year fixed effects. However, it is likely that subject-year exit rates, either during year *t* or *t-1* are related to unobserved (to us) student-teacher match quality and, hence, to student attainment in year *t*. A teacher exit in a specific subject within a school-year group could signal adverse teacher-student match quality that is unobserved to us but observed by the incumbent teacher. The exit of a poorly matched teacher will, in turn, induce the entry of another teacher, but there is no reason to believe that this incoming teacher will share the same characteristics which make the outgoing teacher a poor match for the current student cohort. The entry rate is therefore plausibly exogenous, even if the exit rate isn't. One related situation which might raise concerns is if a shock to a department in year *t-1* leads to exits in year *t-1*, consequent entry in year *t*, and poor performance in year *t*. In this case, entry rates in year *t* are negatively correlated with performance in year *t*, through the exit rates in *t-1*. Given a shock to a department in year *t-1* would likely cause a fall in performance in year *t-1*, we would therefore also expect entry rates in year *t* correlated with performance in year *t-1*, but we will show through an event study framework that this is not the case. A further consideration is that our data does not separate within-year from end of year moves, so the exits during the year a student takes their final exams, and which are likely to impact on performance, are measured imperfectly. In any case, as we will see in the results section, exit rates do not have a strong effect on performance, conditional on entry rates.

It is also worth noting at the outset that there is one identification issue which we cannot address when focussing on entry rates: an increase in the entry rate is equivalent to an increase in the share of teachers with zero years of school tenure, so necessarily implies a reduction in average teacher tenure and experience in the school. The effects of entry and the reduction in average school-specific tenure it induces are therefore conceptually equivalent and not separately identified. However, we show that entry effects extend across the range of general teacher experience, so are not primarily a result of entrants being overly represented by new teachers with a lack of teaching experience. In addition, we also control for teacher experience, and the results are insensitive to various parametric and non-parametric specifications of this control, suggesting that the effects of entry are not due to changes in the general teaching experience of the school workforce.

4. Institutional Setting and Data

Our study focuses on the population of secondary school students and teachers in state-maintained secondary schools in England between 2008/09 and 2012/13.[3] Compulsory education in state schools[4] in England is organised into five "Key Stages". The Primary phase, from ages 4-11 spans the Foundation Stage to Key Stage 2 (Years 1-6, where Years are the English terminology for Grades). At the end of Key Stage 2, when pupils are aged 10/11, children leave the Primary phase and go on to Secondary school from ages 11-16, where they progress through to Key Stage 3 (Years 7-9) and to Key Stage 4 (Year 10-11). At the end of each Key Stage, prior to age-16, pupils are assessed on the basis of standard national tests (though the Key Stage 3 tests stopped in 2008). Our study focuses on students in Year 11, which is their last year of compulsory schooling. During Key Stage 4 (Years 10 and 11), students study for and take assessments in a range of subjects, leading to their final qualifications at age 16. The most common qualification is the General Certificate of Secondary Education (GCSE), and we focus on these GCSE educational outcomes of students by subject. Assessment for GCSEs during our study period was generally carried out by a mixture of coursework during Year 10 and Year 11 and final summer exams in Year 11, with greater weight generally placed on the final exams. However, the structure of assessment varied between subjects, with some subjects such as Art being assessed purely on coursework.

[3] We base our analysis on 2008/09-2012/13 period. We have exact information on the subjects taught for the period between 2011 and 2013. We extend the analysis to the years 2009 and 2010 by imputing subject taught according to future teaching and qualification, to improve our sample size and to be able to run additional identification checks that require a longer time span. Results are similar if we restrict the analysis to the 2011/2013 period.

[4] State schools in England account for around 93 percent of the population of students.

The analysis described in Section 3 requires data on student performance and on teachers' career histories. Our main sources are student-level data from the Department for Education's National Pupil Database (NPD) and teacher records from the Schools Workforce Census (SWC), supplemented with the Database of Teacher Records (DTR).

The NPD data contains information on students' socioeconomic characteristics and attainment scores in the Key Stage tests, and Key Stage 4 qualifications. These data come from school returns made in January each year. Student point scores (a form of GPA) at Key Stage 4 – our main outcome measure – are taken from the NPD, along with scores for the Key Stage 2 primary school exam as a measure of prior achievement. The national pupil database also reports information on other student characteristics such as age, gender, Free School Meal eligibility (FSM) and ethnicity.

The School Workforce Census (SWC) has run from 2010/11, and is also based on returns from schools, providing information on teachers, their qualifications, salaries, contract type, number of hours taught, subjects they teach, and other characteristics. We use SWC data up to 2012/13 and supplement it with information from the DTR to extend the data back to 2008/9. The DTR is used in the administration of the national teachers' pension system and also provides a range of information on teachers, their salaries and their qualifications.[5]

Schools are identified as individual entities that are consistent over time from the "Edubase" dataset, which holds information on basic school characteristics like school phase, type, location in each year. Starting from the universe of secondary schools in UK, we exclude Independent (private) and Special Schools (for children with special needs). We construct unique school identifiers with information available on the Edubase database concerning school conversions. Schools formed from the merger of two or more schools, or schools resulting from the division of a school are treated as new schools.

[5] Our data stops in 2012/13 because after that point there were significant reforms to the GCSE qualifications and their assessment format, which makes comparisons with earlier years potentially problematic.

Our data does not permit us to know exactly which teachers teach each student. However, we are able to link students to teachers by the subjects the student takes in a school at Key Stage 4 (Years 10/11) and the subjects a teacher in that school is teaching. The SWC data provides information on the hours a teacher teaches in each subject, from which we derive the main subject taught. In the DTR, this information is unavailable, but we infer their main subject from subjects taught in the later years, and teachers' degree qualification.[6] We form 18 subject groups: Mathematics; English; Science; History; Modern Foreign Languages; Sports; Biology; Chemistry; Physics; Art; IT; Social Science; Design; Business and Economics; Home Economics; Media and Humanities and Engineering. These are aggregated from the 114 original subject codes, in a way that makes it feasible to assign mean teacher characteristics in these subject groups to students, based on which teachers the students are likely to encounter given the subjects they are studying. A full list of the teacher subjects and their grouping is reported in the appendix in Table A1. These subject groups are, in effect, approximately equivalent to school teaching departments. Note that this aggregation does not imply we are introducing measurement error in terms of the entry rates and other measures of mobility: we are over-aggregating our explanatory variable, not introducing noise. We also estimated regressions where we aggregate all the data to form a school-subject-year group panel, but the findings are broadly similar to those reported in the empirical section below and we do not report them.

As discussed in Section 3, we use teacher entry rates as the main measure of turnover, but we also look at exit rates. Entry rates are constructed on school-by-subject-by-year groups, and also broken down by teacher characteristics (e.g. gender and salary quintiles). We also determine whether a teacher is moving from one school to another, or appears as a new entrant into the

[6] A comparison of the subject taught and teacher qualification, when both are available, show a high level of concordance (more than 90%), which suggests that this imputation should induce small measurement error.

system, or whether they are leaving the system (based on whether we observed them in previous or subsequent years).[7]

The entry rate in a school-subject-year group is computed as the share of teachers present in the school-subject group during the current academic year (*t*) who were not present in that school-subject group in the previous year (*t-1*). The exit rate for the current year (*t*) is the share of stock teachers who are present in the year (*t*), but are no longer present in the school in next year *(t+1)*. Data from the DTR/SWC for 2007/9 and 2013/14 is used to compute these variables at the beginning and end of our 2008/9 to 2012/13 study period. Entry is necessarily missing for the first year after the school opening.[8] A limitation of this approach to defining entry is that it does not distinguish the year group (i.e. grade) in which teachers are teaching. In practice, most teachers teach in their subject across all grades in England's secondary schools. For example, according to our data, around 90% of teachers teach in both Key Stage 4 (Years 10 and 11) and Key Stage 3 (Years 7-9). In part of our analysis, we use a refined measure of teacher entry based on the share of total hours taught by incoming teachers in Year 11 -the year of students' final qualification exams, and Year 10 - the first year of the Key Stage 4 curriculum phase. In this way, we can say more about the importance of timing of teacher entry relative to the timing of assessments. However, missing data on subject teaching hours reduces the estimation sample size, and, as noted in Section 3 and in Hanushek et al. (2016), it could lead to biases if new teachers are selectively assigned to high or low performing grades.

Ultimately, we end up with data on teachers, their characteristics and the turnover variables aggregated to school by subject group by year cells. These school-subject-year variables are then merged with student-level data from the NPD. After cleaning and matching, the final sample spans 5 years, has 18 subject groups, approximately 2,750 schools, 2,305,500 distinct students,

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[7] To simplify our methodology and decrease the effect of possible misreporting, we do not consider exit from the profession if the teacher is not observed in the data for a few years but eventually is reported again. This concerns 5.6% of the total number of teachers.

[8] We ignore the small proportion (4.5%) of teachers recorded as moving within school across departments.

202,500 school-subject-year groups and a dataset with a total of around 12,700,000 student-subject observations.

Descriptive statistics related to this sample are presented in Appendix A, Table A2. Annual turnover of teachers is around 12% with entry rates (14%) slightly higher than exit rates (10%). Around 32% of the entry is due to teachers new to the profession (or entering from outside the English state school system), and the rest due to movement between schools.

5. Main regression results

To begin the empirical analysis of teacher turnover on students' KS4 (Year 11) attainment, Table 1 reports the coefficients and standard errors from baseline regression estimates of equations (1) and (2), with overall entry rates as turnover measure. As we move left to right across the table, the specifications control for fixed effects at finer levels of granularity, with Columns 1 and 2 controlling only for year dummies, and Columns 7 and 8 controlling for subject-group by year fixed effects and student fixed effects. In order to test and control for other possible confounding factors, each fixed effect specification is reported with and without additional time-varying control variables. Odd-numbered columns have no additional control variables; even-numbered columns include a rich set of control variables for student characteristics, plus teacher and student characteristics aggregated to school-subject-year group cells (see table notes for details). Standard errors are clustered at school level. Note, teacher entry rates are defined on subject-school-year cells. Hence, they represent a Year 11 students' potential exposure to teacher mobility in a school department as a whole in a given academic year, rather than actual exposure to mobility of teachers specifically assigned to teaching in their year group. The coefficients are therefore best interpreted as a type of 'intent to treat' effects, which avoid selection issues that could arise through strategic assignment of new teachers into different year groups. In Section 6.1 consider alternative definitions of treatment that more closely capture students' actual exposure to teacher entry.

In all specifications in Table 1, higher entry rates are associated with lower KS4 scores. With no control variables or fixed effects in Column 1, the coefficient of 0.10 implies that a 10 percentage-point increase in entry (about 60% of a standard deviation) is associated with a 1% standard deviation reduction in KS4 scores. When we add in controls for observable student, teacher and school attributes in Column 2, the coefficient becomes larger in absolute value. It is the inclusion of variables describing the existing teacher stock that leads to this change. However, when we more fully control for unobserved confounders with fixed effects at school-by-year and subject-by-year level in Column 3, the coefficient is reduced again to -0.050 and is now less sensitive to the inclusion of control variables in Column 4 (given the standard errors). The magnitude remains relatively stable with the inclusion of additional fixed effects. In Columns 5 and 6 we control, in addition, for school-by-subject fixed effects, implying that identification is based purely on variation in entry rates over time within these school-by-subject groups, conditional on time-varying factors affecting entry rates at school level and at subject level. In Columns 7 and 8 we introduce student fixed effects. Here identification comes from variation across subjects taken by each student. Note that school-by-year fixed effects are not identified within pupil, and so are, omitted. The estimates from this specification are broadly similar to those in Columns 3-6 and are again fairly insensitive to the inclusion of time varying control variables. The stability of the estimates to control variables in the specifications that control for school-subject specific unobservables, time-varying school and subject specific shocks, or student specific unobservables, suggests that entry rate variation in these specifications is effectively random. 'Balancing' regressions in which we regress the entry rate on mean student characteristics in school-subject-year cells also demonstrate that the entry rates are uncorrelated with these student characteristics (see Appendix Table A3). In the remainder of the empirical analysis, we focus on the more conservative estimates based on year-to-year shocks in mobility in the specification of Column 6.

Taken together, the estimates in Table 1, Columns 5-8, suggest that an increase in the entry rate of 10 percentage points reduces attainment by around 0.3-0.5 percent of one standard deviation, with our preferred estimate in Column 6 at just under 0.5 percent of one standard deviation. This implies that a one standard deviation increase in entry rates (16.7 percentage points) in the year of preparation for end of school qualifications reduces attainment by around 0.8 percent of one standard deviation. This is not a huge effect, but it is non-negligible compared to many school interventions and the magnitude is similar to the effects of other turnover-related externalities in schools. The magnitude is close to that from the turnover of students in schools (Gibbons and Telhaj, 2011; Hanushek, Kain and Rivkin 2004) and slightly larger than the effects of turnover of students in neighbourhoods (Gibbons, Silva and Weinhardt 2015).

6. Robustness checks

To assess the robustness of our results, we run a series of checks including 'placebo' treatments; controlling for additional confounding factors; checking the robustness of our definition of exposure to teacher entry; and showing the timing of effects in an event study.

6.1 Confounding trends and shocks

The estimates of our effect of interest in Table 1 appeared robust to the inclusion of a wide range of controls and fixed effects. However, it is still possible that some unobserved pre-existing trends or time varying contemporaneous (to entry) shocks are driving our results. Table 2 presents the results of a number of checks related to these threats.

Column 1 reports the coefficient for our preferred baseline specification with school-by-subject, school-by-year, and subject-by-year fixed effects from Table 1, Column 6. Column 2 includes two years' lead of the measure of entry (*t+2*): if entry reflects a general trend of the school/department, then a higher turnover in the future might be associated with lower grades in the current year. As Column 2 shows, the inclusion of this measure of future entry, however, does not have any effect on students' attainment in the current year and our main coefficient of interest is largely unaffected by the inclusion of this measure of future turnover. This suggests that our estimates do not reflect general trends in the school-subject performance. A similar reasoning is applied in Column 3 where we include a measure of turnover in other subject groups within the school in the same year. We exclude other subjects taken by the student in order to avoid any possibilities of spillovers across subjects. Again, in this case, the effect of entry is robust and the entry in other subject groups does not have an independent effect on students' scores. Column 4 includes exit rates alongside entry rates. As discussed in Section 3, exit rates are more likely to be endogenous than entry rates, due to teacher-student match quality. However, there is no indication of any association here and the effect of entry rates on student performance is almost unchanged. Column 5 includes lagged school-by-subject KS4 achievement, as a proxy for unobservables that are correlated with past performance. Doing so again makes little difference to the magnitude or statistical significance of the effect of teacher entry. Column 6 further checks the robustness of the estimates by controlling for school-subject group specific linear trends to partial out trends in mobility and performance in these groups. This very demanding specification makes little difference to the estimates of the effects of entry rates.

6.2 Student exposure to teacher entry

As noted in Section 5, our main measure of teacher entry captures entry into school departments as a whole, rather than into the year groups (10 and 11) specifically relevant for KS4 study. This avoids endogeneity issues posed by strategic selection of teachers into 'low-risk' year groups, but masks potentially informative patterns related to timing of entry. To address this issue, Column 6 of Table 2 uses, instead, a more refined measure of turnover (discussed in Section 4) in which we define entry rates by the share of hours taught by incoming teachers in different year groups (Year 10 or Year 11). [9]

We report three different entry effects based on this hours-based entry rate definition: the effects of entry to Year 11 teaching on the current Year 11 cohort's GCSE results; the effects of entry to current Year 10 teaching on the current Year 11 cohort's GCSE results; and the effects of entry to Year 10 teaching in the previous academic year when the current Year 11 cohort was in Year 10. What matters in these specifications is entry rates in Year 11, when students are in their final examination year. The effect is slightly larger (-0.064, s.e. 0.024) in magnitude than our baseline estimates, although statistically comparable. The sample size is much smaller because data on subject teaching hours is only provided for 2011-2013 period. Note however, that our baseline specification estimated on this smaller sample gives a similar coefficient to that from our main sample, around -0.05. The implication of this result is that there is little lost from using department-wide entry rates, and if anything, our main results are overly conservative.

The zero-insignificant coefficient on Year 10 entry rates reinforces the 'placebo' tests of Columns 2 and 3: new teachers entering in a given academic year have no effect on GCSE results if they are not actually teaching the students taking these exams. The coefficient on entry into Year 10 when students taking GCSEs were actually in Year 10 is negative, but also small and insignificant. This has two possible interpretations: either turnover in Year 10 doesn't impact on students, because it comes at the beginning of teaching on their GCSE course programmes, so it involves no disruption to continuity in teaching; or it has an impact on student performance, but there is little or no persistence in the effects of teacher turnover across years. With no recorded information on Year 10 achievement we are unable to distinguish between these hypotheses.

[9]This refined measure also takes account of the hours teachers who teach multiple subjects spend teaching each subject so also acts as a test of robustness to misallocation of teachers to subjects.

6.3 Event study estimates

Expanding and refining the placebo tests of Column 2 in Table 2, Figure 1 presents the results of an 'event study' style of analysis. It plots estimated coefficients and corresponding 95% confidence intervals from regressions which include further leads and lags of entry rates. The horizontal axis indicates the lag and lead order. For example, the triangle corresponding to the lead 2 is comparable to the estimate presented in Column 2 of Table 2. Coefficients associated with the 'minus' sign represent leads of the turnover measure, while coefficients associated with the 'plus' sign are lags. Circles in the figure show the coefficients for the current year (*t*), while triangles report the coefficients for the leads and lags of entry. In line with the results tabulated above, the effect of turnover in the current year is always negative with a magnitude of around -0.05. Evidently, the effects of the lags and leads are never large or significant.

The small size and statistical insignificance of the leads' coefficients show, as we would expect, that teachers entering after a students' KS4 exams have no impact on their exam performance, thus providing a useful placebo test. As discussed in Section 3, this finding rules out the possibility that entry rates are explained by a shock to a school department performance in year *t-1* causing exits in year *t-1*, entry in year *t* and low performance in year *t*. This would imply that entry in year *t* would also be correlated with performance in year *t-1*, whereas the estimated coefficient is only 0.01 and statistically insignificant.

Looking at coefficients of lags, the one year lagged entry rates – corresponding to entry rates in the school-subject group when the student was in grades Year 10, Year 9, and so on - also have no effect on KS4 performance. The lags show that the effect of entry has a minimal persistence: after one year in the new job, they no longer negatively affect student attainment.

These results justify our focus on entry rates in the year of the KS4 qualifications and suggest there is no need to consider cumulative entry over the whole of a student's preceding years of secondary education.

Results in Figure 1 also suggest that changes in teachers' quality are not driving our results. If a new teacher was, say, of lower quality, we would expect a persistent negative effect of turnover. However, this is not the case as the figure shows.

7. Exploring the mechanisms: Heterogeneity in the effects of teacher entry and the role of school organisation

So far, the analysis focused on the identification of the average effect of turnover on test scores and we have demonstrated that, conditional on our fixed effects, entry rates are exogenous, in the sense that they are uncorrelated with incumbent teacher, school and student characteristics, and school-subject specific shocks. Nevertheless, it is still possible that the effects of entry we have estimated arise because incoming teachers are different from the incumbent teachers, causing changes in the average quality of the workforce at the school-subject level. It is also possible that the magnitude of the disruptive effects of new teachers is heterogeneous along a number of dimensions. Firstly, the amount of disruption may depend directly on incoming teachers' skills and experience, irrespective of whether they differ from the incumbent workforce, or the skills of incomers may interact with those in the incumbent workforce. Allen (2017) for example, highlights the potential costs imposed on students in schools that take on large numbers of newly qualified teachers. Secondly, the magnitudes may depend on school organisation and how incoming teachers are allocated to different year groups. As noted in the Introduction, our 'intent to treat' estimates, based on teacher entry rates into the school as a whole, might underestimate the causal effects of a new teacher on a student if new teachers are allocated to grades other than the one for which we measure KS4 outcomes. In this section we investigate these heterogeneous effects of turnover, and the role of teacher allocation and school organisation in mitigating the effects of turnover.

7.1 The role of entrant teacher characteristics

In order to explore the effect of entry of teachers with different characteristics, we repeat our preferred fixed effect specification from Table 1, Column 6, but split the entry rate into different components according to incoming teacher characteristics. Table 3 presents the results of this regression. The coefficients for the different groups of teachers are all of a similar order of magnitude, indicating that all groups cause disruption. However, the patterns point towards senior teachers causing more disruption: the coefficients increase with age, salary and experience (up to the 3rd category), and the negative effect from being taught by incoming teachers from outside the profession is lower than the effect from those moving between schools. Gender also differences play a role, with entry of male teachers having a much more detrimental effect than female teachers. We also examined whether teachers coming from better/worse schools, based on school-subject specific scores in the KS4 exam in the previous year, had less/more disruptive impacts,[10] but found no difference. Additional regressions in which we interact entry rates with the characteristics of incumbent teachers also revealed no strongly significant interactions or systematic patterns, so we do not report them here.[11]

The finding that less experienced entrants cause less disruption than older and more experienced ones, requires some investigation, as it runs counter to expectations and some previous literature. There are potential behavioural explanations, such as younger teachers being more adaptable, but the results of Table 4 point to another explanation. This table reports an

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[10] A teacher defined as coming from a better school if the average grades in the origin school-subject cell in *(t-1)* were higher than grades attained in the destination school-subject in (t).

[11] We also looked at the effects of exit rates in these groups. KS4 attainment generally has no association with exit rates, unconditional on entry, though we find positive associations with exit of the lowest paid teachers (bottom quartile) and those with the most experience (10 years +). If we control for both entry and exit, the effects of entry become around 50% bigger and the differences across incoming teacher types less marked. The coefficients on exit, conditional on entry are generally positive, but show no systematic patterns across incoming teacher types. As noted in the text, we do not trust these exit rates results because of the inherent endogeneity of exit rates.

analysis based on teacher-level data from the School Workforce Census between 2011-2013, in which we regress an indicator that the teacher teaches Year 11 students on teacher characteristics, a new entrant indicator, and an interaction of the entrant indicator with some specific teacher characteristics. The table, thus, shows the probability that a new entrant of a specific type teaches Year 11, compared to a baseline incumbent teacher.[12] The table is organised in a similar fashion to Table 3.

The first thing to note from Column 1 is that new entrants are less likely to teach Year 11, than incumbent teachers. This finding explains why, in Table 2, the coefficient on Year 11 entry rates was slightly larger than our baseline estimates that use entry in all grades: new teachers are allocated to 'lower risk' grades or do not teach, so our 'intent to treat' estimate underestimates the effect of the treatment on the treated students to whom a teacher is assigned. Note however that a high proportion of new entrants do teach in Year 11: the probability of teaching Year 11 amongst all teachers is 72%, and incoming teachers are around 6.7 percentage points[13] less likely to teach Year 11 than incumbent teachers.[14]

When we look at differences in assignment to Year 11 across different types of incoming teacher, we see patterns that can also, at least partly, explain the differences seen in Table 3. While all entrant groups are less likely to teach Year 11 than incumbents, the least experienced and lower salaried entrants are much less likely to teach Year 11 than more experienced and higher salaried entrants. Male entrants are more likely than incumbents and female entrants to teach Year 11, which may explain why their entry appeared more disruptive to KS4 performance. Similarly, entrants coming from origins other than other schools (which will mean teachers new to the

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[12] For this analysis, we use as a baseline a teacher in his/her second year at the school.

[13] It should be noted that even when new teachers teach, they tend to teach less hours (about 0.5 less on a baseline of 3.8). Results are available upon request.

[14] New entrants are also less likely to teach altogether by 2.2% out of a baseline probability of 86%.

profession, predominantly), are much less likely to teach Year 11 and less likely to affect KS4 scores.

Overall, the results of Table 3 and Table 4 suggest that schools can and do take steps to mitigate the effects of new teachers on the high-stakes KS4 qualifications by not assigning them to the high risk grade in which students take these exams.

7.2 School quality

The observation that new teachers tend to be re-assigned out of the high-risk grade, Year 11, raises questions about the role of school organisational quality in mitigating the adverse effects of entrants on students. Table 5 and Table 6 examine this issue, in a similar way to Table 3 and Table 4, firstly looking at heterogeneity in the effect of entry by indicators of school quality, and secondly investigating how entrant teachers are assigned across grades. Indicators of school quality are based on external inspections by the schools' regulator in England, Ofsted. Ratings are based on a combination of self-evaluation reports by the school and site visits by inspectors, involving meetings with staff, students, governors and parents. The inspection results in rating of a school's overall performance and organisation as either Outstanding, Good, Requires Improvement, or Inadequate. A school receives the Outstanding rating if it is judged Outstanding on all dimensions that are inspected, including effectiveness of leadership and management.

Looking across Table 5, it is evident that students in schools judged as Outstanding are markedly less affected by entrant teachers – the point estimate is half than that for other schools, and statistically insignificant. The point estimate for Good schools is also smaller than those for schools that Require Improvement or are Inadequate, although the differences are not statistically significant. While a number of factors could explain this pattern, Table 6 suggests that assignment of entrant teachers to grades other than Year 11 is a contributory factor. Outstanding and Good schools are, somewhat, more likely than schools rated Inadequate or Requires Improvement to assign new teachers to other grades. Other factors are evidently at work though, since Good schools are as more likely to assign new teachers outside Year 11 as are Outstanding schools, and yet their students are much more affected by teacher entry.

7.3 Students

Table 7 investigates the heterogeneity of the effect of turnover for different groups of students. The regression results are based on our usual preferred fixed effects specification, separately estimated for different groups, with Column 1 repeating the results from Table 1, for comparison. As results show, in most of the cases, standard errors are too large to draw definitive conclusions. However, we can see qualitative pattern: students most affected by teacher turnover appear to be the ones from more disadvantaged backgrounds[15] (Columns 2 and 3), male students (Columns 4 and 5), students from ethnic minorities (Columns 6 and 7), and those in the lower quartile of the primary school (KS2) grade distribution[16] (Columns 8 and 9). The difference in the effect between boys and girls is in line with a recurrent theme in the educational literature, where boys generally seem to come of worse (see Gibbons, Silva and Weinhardt (2015), for example). Disruption from teacher turnover appears to be one contributory factor (albeit a small one) to the gender gap between boys and girls in England's schools. The largest difference is between students in the top quartile (Column 7) and in the bottom quartile (Column 8) of the KS2 grade distribution. More vulnerable students seem to be more affected by disruption induced by teacher turnover as they might be less able to make up for program disruption with additional family resources and independent effort.

These findings suggest that teacher turnover might, at least in part, contribute to the achievement gap between disadvantaged and not disadvantaged students.

[15] Proxied by free school meal eligibility.

[16] Primary school exam.

8. Conclusion

Our study investigated the impact of teacher entry rates at school-subject-year level on student achievement in England using fixed effects regression designs which control carefully for unobserved school-by-year, subject-by-year shocks and school-by-subject or individual unobservables. The key finding is that students in the final year of compulsory secondary school score less well in their final assessments if they are exposed to higher rates of teacher entry in the subjects they are studying. Entry in the final year in which students take their final GCSE assessments seems crucially important, implying that disruption to final qualifications from new teachers could be minimised by assigning them to year groups with less high-stakes assessment. The magnitudes are, however, quite small, with a 10 percentage point increase in entry rates reducing scores in final qualifications by just under 0.5 percent of a standard deviation. This figure is almost exactly the same as that found for entry of teachers in schools in the US (e.g. Ronfeldt et al 2013, Hanushek et al 2016), suggesting that the effects are potentially quite general and not dependent on context. This size of impact is economically meaningful compared to many other educational interventions. For instance, the literature on teacher quality suggests that a one standard deviation increase in overall teacher quality – where 'quality' means everything about teachers that is correlated with persistently higher value-added scores – only raises individual student achievement by around 0.11 standard deviations (see for example Hanushek 2009). Our standardised effect is about 0.8 percent of one standard deviation from a one standard deviation increase in entry rates, so clearly considerably smaller than this, though not negligible, and comparable or larger to the estimates of other forms of educational externality in student groups.

In contrast to Hanushek et al (2016), we find that the adverse effects of entry do not appear to be related to changes in workforce composition and entry of less experienced teachers. The effects are quite general across entrant teachers with different levels of seniority, in age, experience and salary, and insensitive to controls for workforce composition. The observation that less experienced entrant teachers have no bigger impact than more experienced teachers is partly, but not completely, explained by the fact that schools tend to allocate new teachers outside the high risk grade Year 11 when students take their final exams, and are more likely to do so for younger, less senior teachers with less experience and lower salaries. Evidently, schools are able to partly mitigate the impact of turnover by the way they organise teaching, implying that our estimates potentially underestimate (in absolute value) the causal impact in a situation where new teachers were randomly assigned to students. This finding has wider implications for education research, in that shows that schools respond to exogenous influences in ways which may mask their estimated effects. Even so, turnover of teachers matters regardless of teacher seniority and these organisational responses suggesting that our key results are likely driven by unavoidable general disruption and lack of continuity in teaching due to new teacher entry.

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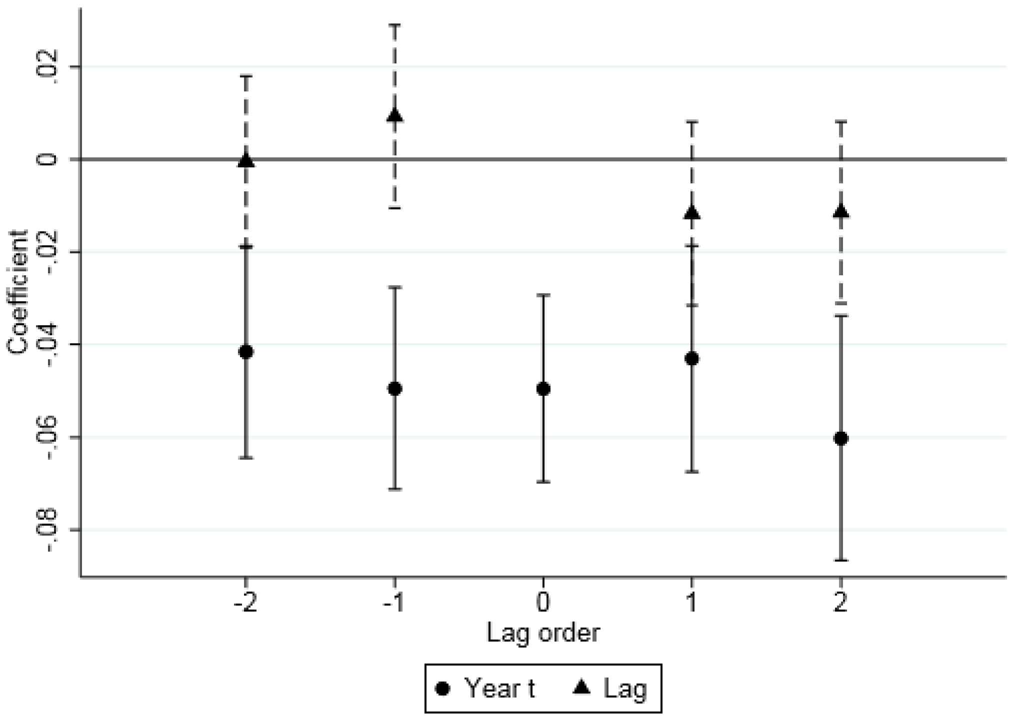
Figure 1: Event study of effects of entry rates on KS4 attainment



Note: Figure plots the coefficient of the effect of teacher entry rate on students' standardized KS4 grade for our preferred specification. 'Circles' report the contemporaneous effect and 'triangles' the effect of leads (negative lag order) and lags (positive lag order). Entry rate is defined as the share of teachers in year *t* who were not present in the school in year *t-1*. All equations include controls for teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Sample includes years from 2009 to 2013. Standard errors clustered at school level. Confidence intervals at 95% reported.

Table 1: Baseline results for effect of teacher entry rates on KS4 point scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Overall entry rate	-0.100***	-0.243***	-0.050***	-0.075***	-0.031***	-0.049***	-0.029***	0.041***
	(0.015)	(0.018)	(0.012)	(0.015)	(0.008)	(0.010)	(0.010)	(0.012)
Observations	12654691	12654691	12654691	12654691	12654691	12654691	12654691	12654691
R-squared	0.000	0.036	0.209	0.232	0.440	0.465	0.361	0.576
Year FE	Y	Y	N	N	N	N	N	N
School x Year FE	N	N	Y	Y	Y	Y	N	N
Subject x Year FE	N	N	Y	Y	Y	Y	N	N
School x Subj FE	N	N	N	N	Y	Y	Y	Y
Student FE	N	N	N	N	N	N	Y	Y
Control variables	N	Y	N	Y	N	Y	N	Y

Note: Regressions at the student level. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year *t* who were not present in the school in year *t-1*. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Sample includes years from 2009 to 2013. Standard errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Placebo tests and other controls for trends

	(1) Baseline	(2) Leads	(3) Other subjects (not taken)	(4) Entry and exit rates	(5) Lagged KS4	(6) School-subject Trends	(7) Year group-specific entry
Entry Overall	-0.049***	-0.042***	-0.050***	-0.051***	-0.044***	-0.050***	-
	(0.010)	(0.012)	(0.010)	(0.011)	(0.010)	(0.010)	
Lead 2 Entry Overall	-	-0.001			-	-	-
		(0.009)					
Entry other subjects	-	-	-0.006	-		-	-
			(0.009)				
Exit rate				-0.013			
				(0.009)			
Lagged KS4 Score	-	-	-	-	0.216***	-	-
					(0.00747)		
Entry Year 11 (t)	-	-	-	-	-	-	-0.064***
							(0.025)
Entry Year 10 (t)	-	-	-	-	-	-	0.001
							(0.022)
Entry Year 10 (t-1)	-	-	-	-	-	-	-0.008
							(0.016)
Observations	12654691	9846958	12653579	12654691	12241679	12654691	4041094
R-squared	0.465	0.488	0.465	0.464	0.467	0.465	0.467
School x Year FE	Y	Y	Y	Y	Y	Y	Y
Subject x Year FE	Y	Y	Y	Y	Y	Y	Y
Subject x School FE	Y	Y	Y	Y	Y	Y	Y
School x Subject x Trends	N	N	N	N	N	Y	N

Note: Regressions at student level. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year *t* who were not present in the school in year *t-1*. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Column 3 adds entry in subjects not seated by the student. Column 4 adds the exit rate defined as the share of teachers in year *t* who will not be present in the school in the following year. Column 5 includes the lagged KS4 average score in the previous year at the schoolXsubject level. Column 6 adds schoolXsubject linear trends. Column 7 measures turnover as the share of hours by year group in KS4 taught by teachers not present in the school in year *t-1*. Sample in Column 7 is limited to years from 2011 to 2013 due to data limitations. Standard errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Heterogeneity in disruption by incoming teacher characteristics

(1) Age		(2) Salary		(3) Experience		(4) Gender		(5) Origin	
20-29	-0.040**	Quartile 1	-0.038***	< 2 years	-0.040***	Female	-0.039***	Other schools	-0.056***
	(0.016)		(0.014)		(0.015)		(0.012)		(0.011)
30-39	-0.044***	Quartile 2	-0.060***	2-5 years	-0.041**	Male	-0.067***	Elsewhere	-0.036**
	(0.015)		(0.017)		(0.021)		(0.016)		(0.016)
40-49	-0.061***	Quartile 3	-0.063***	5-10 years	-0.057***				
	(0.018)		(0.018)		(0.019)				
50+	-0.077***	Quartile 4	-0.057**	> 10 years	-0.063***				
	(0.025)		(0.027)		(0.016)				

Note: Regressions at the student level. School-by-subject fixed effects regressions, controlling for schoolXyear and subjectXyear dummies. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year *t* who were not present in the school in year *t-1*. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Entry by category computed as the number of entrants in that category divided by the number of teachers in the department in *t-1*. Standard errors clustered at school level. Total numbers of observations around 13 million in all regressions. Level of significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Probability of teaching year 11 by incoming teacher characteristics compared to incumbent teachers

	(1) Entrant		(2) Entrant Age		(3) Entrant Salary		(4) Entrant Experience		(5) Entrant Gender		(6) Entrant Origin
Entrant	-0.067***	20-29	-0.117***	Quartile 1	-0.152***	< 2 years	-0.191***	Female	-0.061***	Oth. Sch.	-0.063***
	(0.006)		(0.007)		(0.007)		(0.008)		(0.006)		(0.006)
		30-39	-0.030***	Quartile 2	-0.042***	2-5 years	-0.070***	Male	0.081***	Elsewhere	-0.189***
			(0.006)		(0.007)		(0.007)		(0.006)		(0.008)
		40-49	-0.071***	Quartile 3	-0.030***	5-10 years	-0.024***				
			(0.007)		(0.007)		(0.007)				
		50+	-0.085***	Quartile 4	-0.062***	> 10 years	-0.073***				
			(0.009)		(0.009)		(0.007)				
Obs.	412585		412585		412585		412585		412585		412585
Mean	0.717		0.717		0.717		0.717		0.717		0.717

Note: Linear probability model at teacher level. School-by-subject fixed effects regressions, controlling for schoolXyear and subjectXyear dummies. The dependent variable equal to one if the teacher teaches positive hours in the school in the academic year in year group 11 (grade of the final exam in secondary school). Controls at teacher and school level. Controls at teacher level include: age, sex, experience, tenure and salary of the teacher. Controls at school or department level include: last OFSTED report grade, ranking in quartile for 5+ A*-C in GCSE in the Performance Tables, dummies for core subjects, average normalized grade in KS2 for students in the department and pupil teacher ratio at department level. Core subjects are: English; Math; Science; History. Sample includes years between 2011 and 2013. Standard errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Heterogeneity by school quality

	(1) Ofsted Outstanding	(2) Ofsted Good	(3) Ofsted Requires Improvement	(4) Ofsted Inadequate
Overall entry rate	-0.0294	-0.0543***	-0.0610***	-0.0630
	(0.0185)	(0.0165)	(0.0211)	(0.0402)
Observations	2942967	5390165	3114523	986797
R-squared	0.479	0.482	0.487	0.485

Note: Regressions at the student level. School-by-subject fixed effects regressions, controlling for schoolXyear and subjectXyear dummies. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year *t* who were not present in the school in year *t-1*. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Entry by category computed as the number of entrants in that category divided by the number of teachers in the department in *t-1*. Standard errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Probability of entrants teaching year 11 by subject area, school rating and type

	(1) Ofsted Outstanding	(2) Ofsted Good	(3) Ofsted Requires Improvement	(4) Ofsted Inadequate
Entrant	-0.068***	-0.080***	-0.046***	-0.045**
	(0.012)	(0.009)	(0.012)	(0.018)
Observations	106385	176558	95225	34417
Mean	0.724	0.723	0.715	0.722

Note: Linear probability model at teacher level. School-by-subject fixed effects regressions, controlling for schoolXyear and subjectXyear dummies. The dependent variable equal to one if the teacher teaches positive hours in the school in the academic year in year group 11 (grade of the final exam in secondary school). Controls at teacher and school level. Controls at teacher level include: age, sex, experience, tenure and salary of the teacher. Controls at school or department level include: last OFSTED report grade, ranking in quartile for 5+ A*-C in GCSE in the Performance Tables, dummies for core subjects, average normalized grade in KS2 for students in the department and pupil teacher ratio at department level. Core subjects are: English; Math; Science; History. Sample includes years between 2011 and 2013. Standard errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Heterogeneity in disruption by student characteristics

	(1) Baseline	(2) FSM eligible	(3) Non-FSM eligible	(4) Female	(5) Male	(6) Not White	(7) White	(8) Top Quality	(9) Bottom Quality
Entry Rate	-0.050***	-0.057***	-0.049***	-0.040***	-0.058***	-0.069***	-0.046***	-0.038***	-0.064***
	(0.010)	(0.019)	(0.010)	(0.011)	(0.012)	(0.018)	(0.011)	(0.010)	(0.014)
Observations	12,659,601	1,376,235	11,283,366	6,409,915	6,249,686	2,141,080	10,518,521	3,543,423	2,812,789
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
School xYear FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Subject x Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Subject x School FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Student FE	N	N	N	N	N	N	N	N	N

Note: Regressions at the student level. School-by-subject fixed effects regressions, controlling for schoolXyear and subjectXyear dummies. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year t who were not present in the school in year t-1. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Sample includes years from 2009 to 2013. Standard errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1.

Appendix A: Descriptive statistics

Table A1: Teachers assigned to subjects by year

Subject	2009	2010	2011	2012	2013
Math	17,592	18,651	22,010	22,485	23,041
English	20,065	21,362	24,366	24,843	25,514
History	12,997	13,578	14,926	15,246	15,659
Science	24,757	26,119	28,934	29,321	29,663
Other Foreign Languages	11,599	12,001	13,427	13,582	13,770
Sports	13,593	14,379	15,985	15,917	16,058
Biology	1,341	1,419	1,535	1,639	1,887
Chemistry	1,062	1,132	1,342	1,464	1,611
Physics	1,134	1,202	1,382	1,516	1,585
Art	17,273	18,073	20,253	20,113	20,114
IT	6,645	7,167	8,765	8,544	8,171
Social Science	1,564	1,461	1,404	1,279	1,164
Design	5,690	5,981	6,937	6,749	6,591
Economics	7,242	7,638	8,615	8,437	8,430
Home Economics	3,095	3,254	4,567	4,518	4,390
Media	1,004	1,071	1,443	1,445	1,403
Humanities	2,438	2,623	3,726	3,591	3,439
Engineering	852	895	1,117	1,162	1,132
Total	149,943	158,006	180,734	181,851	183,622

Source: DTR up to 2010 and School Workforce Census from 2011 onwards. Teachers assigned to subjects based on the subject in which they taught more hours (after 2010) and based on future teaching and subject qualification (before 2011)

Table A2: Descriptive Statistics for the estimation sample

Variable	Mean	Std. Dev.	Min	Max
Turnover measures at school-subject-year level				
Entry Overall	0.140	0.165	0	1
Exit Overall	0.105	0.151	0	1
Entry School	0.084	0.132	0	1
Exit School	0.077	0.132	0	1
Entry Profession	0.056	0.103	0	1
Exit Profession	0.028	0.078	0	1
Teacher characteristics				
Female	0.622	0.262	0	1
Age	39.904	5.698	20.75	72
Tenure School	6.923	2.848	1	20
Student characteristics				
KS4 Standardized Score	0.009	0.988	-1.694	10.124
KS2 Standardized Score	0.001	0.999	-4.331	2.204
School/school-subject group variables				
# Teachers	70.565	24.171	2	170
% FSM students	0.130	0.109	0	0.757
%Female students	0.500	0.175	0	1
% white students	0.815	0.230	0	1
Pupil Teacher Ratio	16.905	4.204	0.744	316
% teachers between 20-29	0.197	0.194	0	1
% teachers between 30-39	0.332	0.229	0	1
% teachers between 40-49	0.242	0.214	0	1
% teachers over 50	0.229	0.221	0	1
Number of observations	12,654,691			

Note: Summary statistics at student level. Number of observations: 12,654,691.

Table A3: Regressions of school-subject-year entry rate on mean student characteristics in school-subject-year cells

	No fixed effects			Full set of fixed effects		
Variable	b	s.e.	t-stat	b	s.e.	t-stat
Standardized score in KS2	-0.102	0.013	-8.097	-0.002	0.003	-0.548
Proportion White	-0.086	0.008	-10.828	-0.001	0.001	-1.693
Proportion Female	0.005	0.006	0.888	-0.001	0.001	-1.105
Proportion FSM	0.026	0.004	7.461	0.000	0.001	0.775
Lag mean KS2 Standardized Score	-3.699	0.419	-8.826	-0.436	0.251	-1.739
Lag Proportion of White Student	-0.160	0.011	-14.681	0.000	0.001	0.110
Lag Proportion of Female Students	0.002	0.006	0.330	-0.002	0.001	-1.284
Lag Proportion of FSM Students	0.025	0.004	7.024	0.000	0.001	0.026

Note: Regressions of entry rates on listed variables. Fixed effects include school by year, subject by school and subject by year fixed effects. Standard errors clustered at school level. Number of observations: 12,654,691.

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