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**Teacher Turnover:
Does it Matter for Pupil Achievement?**

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Abstract
Recent research has established that teachers matter for student achievements, albeit because of dimensions of 'teacher quality' that are largely unexplained. A less closely investigated issue is whether teacher turnover directly harms student academic achievement. In this paper, we examine whether teacher turnover affects academic achievement of 16 year old state secondary school students using a unique data set of linked students and teachers in England. Identification comes from either: a school fixed effects design which exploits year-on-year variation in turnover in different subject groups, within schools; or student fixed effect design that where the variation comes from the cross sectional variation in turnover in different subjects, in the same school, experienced by a student. Both methods give similar results, suggesting that a higher teacher entry rate reduces students' test scores, albeit by small amounts.

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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, Adnott et al 2016, 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 knowledge and experience with them, and different teachers have different teaching styles causing a lack of continuity. These disruptive effects from teacher turnover could potentially offset any of its advantages (Ronfeldt at al 2013), but have received relatively little empirical attention, despite their importance in the context of school policy on hiring, firing and retention. In England, the US and elsewhere, despite a lack of much hard evidence, there is a presumption 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]

Our paper adds to the rather limited existing international evidence on the causal impacts of teacher turnover. We focus on the impact of teacher entry into school-subject-year specific groups on student achievement in qualifications in those subjects, at the end of compulsory schooling. 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

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

experiment in which teachers, their entry probabilities, their exit probabilities and their students are all randomly assigned. The analysis is based on a unique large administrative dataset of teacher workforce records linked by school and subject categories to students' achievement records in England. The main research design identifies the effects of turnover on achievement from school-subject-year specific turnover shocks in the final year before the end of school tests. Estimation is implemented with high dimensional fixed effects regressions to control for school-by-year and subject-by-year unobservable factor which might drive teacher turnover. We demonstrate that through a range of placebo, balancing and robustness tests that we can treat turnover as random, conditional on these fixed effects. Turnover is likely to have very different impacts depending on the quality of teachers who are entering and leaving. Thus, we devote considerable attention to the effects of different types of mover, as well as to identifying the average causal effect across all types.

The only previous study directly comparable to ours is Ronfeldt et al (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 Maths 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. A potential problem with looking at 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 design will omit effects due to disruption in the continuity of teaching experienced by students. Our study, in contrast, looks at turnover in subject groups during 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. We are, therefore, more likely to capture these effects, alongside any effects related to incoming teachers having no teaching experience specific to that school. We are also able to improve on the research design by a student fixed effects specification that exploits variation in

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turnover in different subjects, which is infeasible when looking at differences across grades. Arnodt et al. (2016) also study the effects of turnover on achievement, but are interested in the effects of exits in context of a policy environment in 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 in new entrants. Similar findings appear in Chetty et al. (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 contributes to a broader literature on teacher turnover, most of 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 2012). The typical finding is that schools serving disadvantaged young people have higher turnover than other schools. From amongst this literature, Hanushek and Rivkin (2011) 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.

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 standard deviations. 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 is no more disruptive to education than turnover amongst a student's peers. We provide a number of tests to demonstrate that our results appear to be causal, showing that conditional on our fixed effects design, teacher turnover is uncorrelated with student demographics ('balancing'), that we do not observe effects on groups of students who we would not expect to be affected ('placebo') and that observed impacts relate quite precisely to achievement in the years in which we observe the turnover ('event study'). In this way we go come way beyond the previous literature on this topic.

The rest of the paper is structured as follows: Our empirical strategy is discussed in Section 2. Section 3 describes the education institutional setting in the UK and the data set. Section 4 presents our main regression results, with Section 5 investigating the robustness of the analysis and Section 6 taking a more nuanced look at the variation in the effects across different types of teacher, student and subject. Section 7 provides concluding remarks.

2. 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 at al 2013) we focus the entry rate in a given year to represent turnover. The reasons for focussing on entry are elaborated at the end of this section. However, we also consider 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.

To address these endogeneity issues, we implement a fixed effects design that identifies turnover effects from variation in teacher entry rates in different teaching subject categories in the same school. Student test outcomes in the final year of compulsory schooling (Year 11, age 16) are linked to teachers by school, subject category and year and we construct teacher entry rate variables at school-by-subject-by-year level. The details of our institutional setting and data construction are set out in the next section.

In one version of this design, 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. The specification in this case is:

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

Where $quals_{isqt}$ is an index of individual 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 (we consider the relevance of lagged entry rates in the empirical analysis in Section **Error! Reference source not found.**). 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 at school-subject-year level: 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.

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 subject mathematics than in English. This between-subject, within-student design has featured in several previous papers, Dee (2005), Clotfelter et al. (2010), Slater et al. (2010) Altinok and Kingdon (2012), Lavy et al. (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

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[2] We use within-groups estimation, or the numerical procedure of Correia (2017) as implemented in the command reghdfe in Stata (Sergio Correia, 2017. reghdfe: Stata module for linear and instrumental-variable/gmm regression absorbing multiple levels of fixed effects. Statistical Software Components s457874, Boston College Department of Economics.)

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 make it clear why entry rates are preferred to 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 perfect information about the school, the department they are working in and the cohort of students they have been teaching. It is, therefore, implausible a priori that end of year exit rates are unrelated to the unobserved (to us) characteristics of the students in that group in that year, to the student-teacher match quality and, hence, to student attainment in that year. Exit rates act as a signal of shocks that are unobserved to us but observed by the incumbent teaching staff. Using lagged exit rates does not help, because if exit rates are endogenous in year *t*, they are also endogenous in year *t-1* in a short panel with group fixed effects. An exception to this is the special case where exit is forced by retirement, when it could be plausibly unrelated to unobserved school-subject-year group characteristics. This line of reasoning suggests that pension age rules provide potential instruments for exit rates – and hence for entry rates, since retirees need to be replaced. We use of retirement age as an instrument for entry and exit rates in some robustness tests, while noting that this informs us about the effects of a rather specific driver of entry rates, rather than turnover in general.

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. The effects of entry and the reduction in average tenure it induces are, therefore, conceptually equivalent and not separately identified.

3. Institutional Setting and Data

Our study focuses on 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 is 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.

The data construction for the analysis is quite complex and requires data on student performance and on teachers' career histories. Our main sources are student-level data from the

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[3] We base our analysis on 2008/09-2012/13 period because during this period teachers are precisely assigned to subjects they teach. However, results using earlier periods, where assignment of teachers to subjects is based solely on their qualification, are very similar.

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

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, sex, Free School Meal eligibility (FSM) and ethnicity.

The School Workforce Census 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, subjects they teach and other characteristics. We use SWC data up to 2012/13 and supplement it with information from the DTR for 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.

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

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[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.

(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. In the DTR, this information is unavailable, but we infer their main subject from the teachers' degree qualification. 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; Humanities and Engineering. These are aggregated from the 114 original subject codes, in such 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. It does imply an issue with standard errors, so we cluster standard errors at school level. 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 2, we use teacher entry rates as the main measure of turnover, but we also look at exit rates. Entry and exit 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 system, or whether they are leaving the system (based on whether we observed them in previous or subsequent years).[6]

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[6] 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.

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 were present in the previous year (t-1), but are no longer present in the school in the current year. 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.[7] 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 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, although missing data on subject teaching hours reduces the estimation sample size.

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, 202,500 school-subject-year groups and a dataset with a total of around 13,500,000 student-subject observations (the exact numbers in the regressions vary according to specification).

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 40% 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.

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

4. Main regression results

To begin the empirical analysis of 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 the turnover variable. 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 student fixed effects and subject-group by year fixed effects. In order to test for 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 so they represent a Year 11 student's 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 5.1 we look at alternative definitions of treatment that more closely capture students' actual exposure to teacher entry.

In all specifications, higher entry rates are associated with lower KS4 scores. With no control variables or fixed effects in Column 1, the coefficient of 0.057 implies that a 10 percentage-point increase in entry (about 70% of a standard deviation) is associated with a 0.57% standard deviation reduction KS4 scores. When we add in controls for observable student, teacher and school attributes in Column 2, the coefficient becomes much more negative. 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 -0.053 and is now much less sensitive to the

inclusion of control variables in Column 4. The magnitude remains relatively stable with the inclusion of additional fixed effects. In Column 5 and 6 we control in addition for school-by-subject fixed effects, implying 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, 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 insensitivity 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 suggest 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). We go on to test this assumption in greater detail in subsequent analysis. 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 suggest that an increase in the entry rate of 10 percentage points reduces attainment by around 0.4-0.6 percent of one standard deviation, with our preferred estimate at just under 0.5 percent of one standard deviation. This implies that a one standard deviation increase in annual turnover (16.7 percentage points) during the two years of preparation for end of school qualifications reduces attainment by around 0.8 percent of one standard deviation. This is not a huge effect size, but is non-negligible compared to many school interventions and similar to the effects sizes 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).

5. 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. Then, we move to an instrumental variable strategy exploiting turnover generated by exogenous statutory retirement age rules. In both settings, the negative effects of teacher turnover are supported by the evidence.

5.1 Confounding trends, 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, as in any difference-in-difference style analysis, it is still possible that some unobserved pre-existing trends or time varying contemporaneous (to entry) shocks are driving our results. Table 2 first 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, then a higher turnover in the future might be associated with lower grades in the current year. The inclusion of this measure of future entry, however, does not have any effect on students' attainment in the current year and our main effect of interest is largely unaffected by the inclusion of this measure of future turnover. Our estimates do not seem to 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, and where 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 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 5 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.

5.2 Student exposure to teacher entry

As noted in Section 4, 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 is avoids endogeneity issues posed by strategic selection of teachers into 'low-risk' year groups, but masks potentially informative patterns related to timing of entry. Column 6 of Table 2 uses instead a more refined measure of turnover (discussed in Section 3) in which we define entry rates by the share of hours taught by incoming teachers in different years (Year 10 or Year 11). [8]

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, though the effect is only slightly larger (-0.067, s.e. 0.024) in magnitude than our baseline estimates (-0.046, s.e. 0.01) although statistically comparable. The implication of

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[8] The sample size is much smaller due to missing data on subject teaching hours, although this is of little consequence: if we re-run our baseline specification on this sample we obtain a coefficient on overall entry rates of -0.05 (s.e. 0.014). 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.

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 reinforce 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 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.

5.3 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 where we show the estimated coefficients from regressions which include further leads and lags of entry rates. The figure shows the coefficients (and 95% confidence intervals) for the effects of entry in a particular year on the attainment of students qualifying in that year as circles. In line with the results tabulated above, the coefficient is always negative with a magnitude of around -0.05. The triangles indicate the coefficient on lagged or leads of the entry rate. So, for example, the triangle corresponding to the lead of 2, is comparable to the estimate presented in Column 2 of Table 2. Evidently, the effects of the leads are never large or significant, indicating, as we would expect, that teachers entering after a students' KS4 exams have no impact on their exam performance. The one year lagged entry rates imply an impact on KS4 performance that is around half that of entry in the year of the KS4 exams. Again this is to be expected, because students study for two years for their KS4 qualifications, so these one year lagged entry rates correspond to the first year of this period of study. There is, however, no impact

from turnover in years preceding the KS4 study period. 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.

5.4 Exit rates and instrumental variable approach

The previous section provided some supporting evidence that our results are not driven by trends in turnover and grades at school level. It might still be possible that subject-by-school-by-year shocks might be driving our results, if potential incomers have inside information about factors affecting the expected performance of the students and subject groups in the school where they will be teaching. Another reason for endogenous teacher entry is that entry of new teachers typically follows exit of others, and, as discussed in Section 2, exit rates are unequivocally endogenous. For example, incumbent teachers' inside information and anticipation of a shock adversely affecting performance in the following year might drive some of them to leave. This, in turn, could lead to a higher entry rate in the following year through recruitment. Alternatively, if high exit rates at the end of year (*t-1*) are due to an adverse shock in year (*t-1*), this could lead to high entry rates at the beginning in year *t*, coinciding with a positive performance shock due to mean reversion (especially since we condition on group fixed effects, so all the variation is relative to groups means over the 5 year study period). Given these endogeneity issues, simply including exit rates in our regressions is a poor strategy (it is also difficult to interpret the effects of entry conditional on exit, as this implies changes in the number of teachers).[9]

Instead, to further address this issue of endogenous exit and entry in response to unobserved school-subject-year shocks, we implement the instrumental variable procedure described in Section 2. We instrument exit rates, or entry rates, with the share of teachers in the group in the

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[9] If we do add exit rates to the regressions, the effects of exit rates unconditional on entry rates are generally zero and insignificant. The effects of exit rates conditional on entry tend to be positive, and the coefficients on entry rates become around 50% larger.

previous year (*t-1*) who reached requirement pensionable retirement age by the start of academic year *t*. These teachers will be forced to leave, so their departure (and the entry of new teachers) can, surely, be treated as exogenous. Figure 2 graphically illustrates the principle of the first stage using this instrument, showing the relationship between entry rates in year *t* and the age of the oldest teacher in year (*t-1*) relative to the statutory pension age threshold. As can be seen, entry rates experience a substantial jump in the year in which a teacher is due to retire.

The instrumental variables estimates are reported in Table 3. Column 1 exhibits the first stage regression of exit rates on the proportion retiring to demonstrate the instrument relevance. The first stage coefficient is significant (although small) and the F-statistic is high. Column 2 reports the OLS-fixed effects estimates for the impact of exit rates in (*t-1*) on student attainment (in year *t*). In contrast to the main results we reported in previous tables, there is no large or statistically significant relationship between exit rates and attainment. However, as discussed above, the OLS coefficient on exit rates is uninterpretable in any causal sense due to inherent endogeneity issues. Column 3 reports the IV estimates. The estimated effect of exit is now much larger – by a factor of 5 – than our baseline effects of entry, implying that a 10 percentage point increase in the exit rate reduces attainment at KS4 by around 2.7% of one standard deviation.

The question of how this translates into entry rate effects is explored in Columns 4-5. For comparison, Column 6 repeats our baseline coefficient estimate for entry rates, from Table 1, Column 6. The first stage regression in Column 4 indicates that retirement share is also a relevant instrument for entry rates, as we would expect. The IV point estimate of the effect of entry on attainment in Column 5 are similar to that for exit rates in Column 2, and both suggest that our baseline estimates of the effects of turnover could be attenuated lower bounds to the true causal impact. However, the IV estimate is very imprecisely measured making it somewhat uninformative. It is also possible that the large IV coefficients are specific to exit and entry driven by retirement (i.e. the IV is a Local Average Treatment Effect), which may be especially large as experienced teachers are replaced by new ones (though we control for teacher experience in the regressions,

and show in Column 6 that, if anything, entry of less experienced teachers causes less disruption). We repeated these IV regressions using an indicator that the oldest teacher has reached retirement (i.e. as in Figure 2) and results are similar to those using the share of retiree teachers, so we do not report them.

The general conclusion from this IV exercise is that our baseline fixed effects estimates are unlikely to be exaggerated by the endogenous departure of teachers in response to performance-related shocks.

6. Heterogeneity in the effects of teacher entry

So far, the analysis focused on the identification of the average effect of turnover on test scores and we have demonstrated that 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 their 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, there are potential interactions between entering teacher types and student characteristics, if students respond to a change in teachers in different ways depending on students' ability to adapt to new styles of teaching or to a less structured program. Lastly, the effect could also depend on the groups of subjects taught by new teachers. In this section we investigate these heterogeneous effects of turnover.

6.1 Incoming and incumbent teachers

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 splitting the entry rate into different components according to incoming teacher characteristics. Table 4 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 less senior teachers causing less disruption: the coefficients increase with age, salary and experience (up to the 3rd quartile), and are marginally less for incomers from outside the profession than those moving between schools. A possible explanation is that younger, less experienced teachers may be more adaptable then older teachers, so worries about the costs imposed by new inexperienced teachers seem unsupported by these results. Gender differences play a role, with entry of male teachers having a much more detrimental effect than female teachers. We also looked at whether teachers coming from better/worse schools based on school-subject specific scores in the KS4 exam in the previous year had less 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]

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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.

6.2 Students

Table 5 looks at heterogeneity in response of different types of students. The regressions are our usual fixed effects specifications, with entry rates interacted with dummy indicators for different student categories. As the results of Table 5 show, we find no evidence that that disruption affects disadvantaged students more than others, the effects being broadly similar for different types of student. The effects are marginally less for low income (FSM) students, marginally bigger for students in the middle of the distribution of prior achievement (KS2), and no different for non-white ethnic groups. The most striking finding is for gender: male students are more adversely affected than females by teacher turnover. The coefficient for males implies that a 10 percentage point increase in teacher-entry reduces attainment by 0.75 percent of one standard deviation (a standardised effect size of about 1.25 percent of one standard deviation). The effect for girls is only one third as big. This 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.

6.3 Subjects

Finally, we move to heterogeneous effects of turnover across subject groups, which is important because some subjects are more relevant than others for future students' careers. A lower score in core GCSE English, Mathematics and Science exams, may bar students from pursing these subjects in the later stages of education. Also, core subjects contribute to what is known as the 'English Baccalaureate' a group of GCSEs which are considered especially important and on which school performance is, in part, assessed.

To investigate these differences, we run our preferred specification with interactions between entry rates and indicators whether or not the subject is in what we define as a core or non-core category. Core subjects include mathematics, English, any science, history and geography (in line

with the English Baccalaureate). The findings in Table 6 are quite striking. All of the general effects of entry are coming from the non-core subject group. There are at least two potential explanations for this finding: firstly, we might expect differences across subjects because of differences in the way teaching is organised and the continuity required e.g. if students have substantial pieces of coursework or art portfolios in non-core courses that benefit from consistent supervision; secondly, schools can also respond in different ways to turnover and a likely explanation for the take more urgent action to avoid or mitigate the impacts in courses that are a high priority.

7. Conclusion

Our study investigated the impact of teacher entry 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. Boys are more severely affected than girls. Less senior incoming teachers seem to cause less disruption than those who are more senior, although the differences are subtle. 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 primary schools in the US (Ronfeldt et al 2013), 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.

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Thomsen,

Figure 1: Event study of effects of entry rates on KS4 attainment
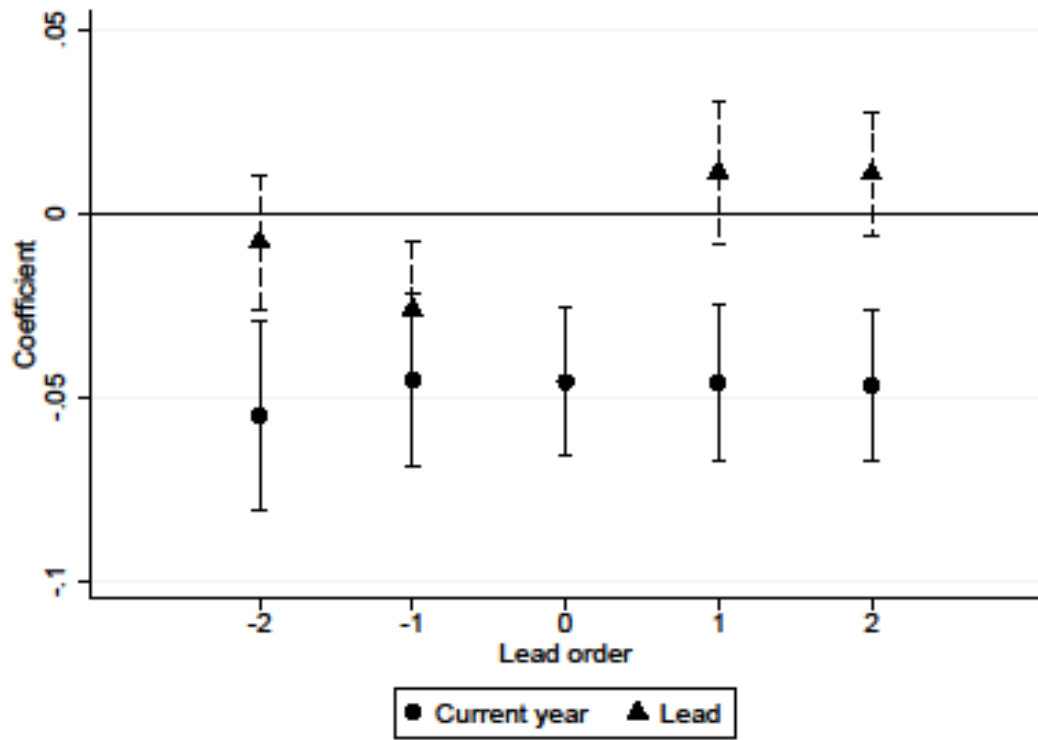
Figure 2: First stage effect of age of oldest teacher in school-subject group (department) in year t-1 on entry in year t. Zero is Statutory Retirement Age
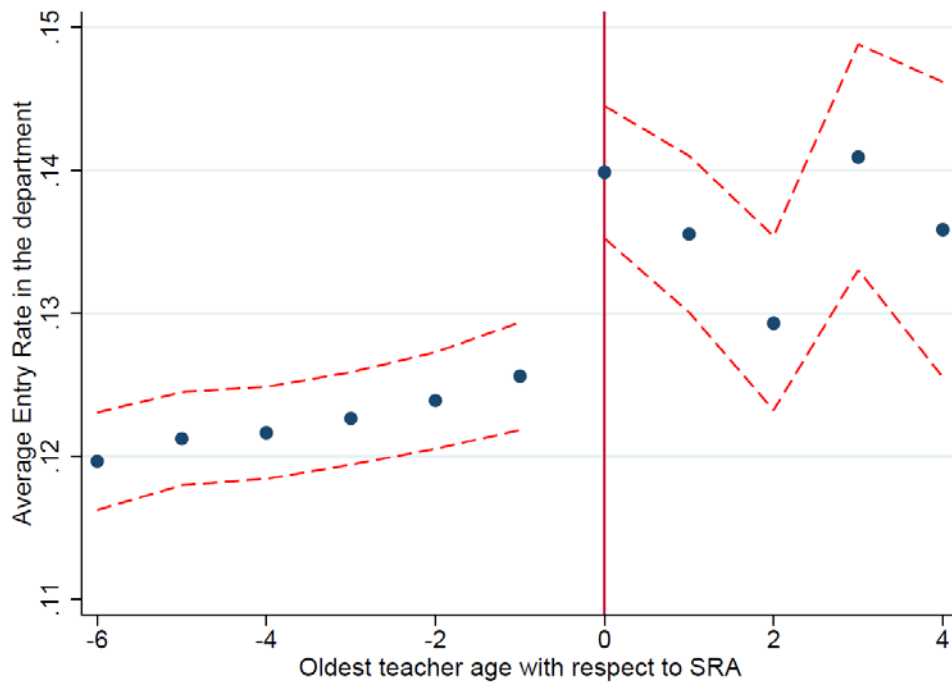
Table 1: Baseline results for effect of teacher entry rates on KS4 point scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Overall entry rate	-0.057***	-0.242***	-0.053***	-0.069***	-0.039***	-0.046***	-0.040***	-0.049***
	(0.013)	(0.018)	(0.011)	(0.014)	(0.008)	(0.010)	(0.008)	(0.011)
Observations	13,524,806	13,087,183	13,524,805	13,087,182	13,524,621	13,087,013	13,506,279	13,067,939
R-squared	0.000	0.036	0.209	0.232	0.440	0.461	0.577	0.578
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	Y	Y
School x Subj FE	N	N	Y	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

Notes: Dependent variable is the average standardized point score in the KS4 exams by student, subject and year. Standard errors clustered at school level. Control variables. Teacher characteristics at school-subject level include: average age of teacher; average experience; share of female teachers and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School-subject specific characteristics are: pupil teacher ratio; proportion of female students; proportion of FSM; proportion of white; number of teachers in current and past academic year. Level of significance: 10% *; 5% **; 1% ***.

Table 2: Placebo tests and other controls for trends

	(1)	(2)	(3)	(4)	(5)	(6)
						Year group-specific entry
			Other subjects	Lagged KS4	School-subject Trends	
	Baseline	Leads				
Entry Overall	-0.0460***	-0.0469***	-0.0467***	-0.0409***	-0.0424***	-
	(0.0102)	(0.0106)	(0.0103)	(0.00980)	(0.00981)	
Lead 2 Entry Overall	-	0.0106	-	-	-	-
		(0.00870)				
Entry other subjects	-	-	-0.0147		-	-
			(0.00962)			
Lagged KS4 Score	-	-	-	0.215***	-	-
				(0.00729)		
Entry Year 11 (t)	-	-	-	-	-	-0.067***
						(0.024)
Entry Year 10 (t)	-	-	-	-	-	-0.004
						(0.022)
Entry Year 10 (t-1)	-	-	-	-	-	-0.013
						(0.016)
Observations	13,087,013	12,603,662	13,085,502	12,976,952	13,087,013	4,201,481
R-squared	0.461	0.459	0.461	0.464	0.508	0.478
School x Year FE	Y	Y	Y	Y	Y	Y
Subject x Year FE	Y	Y	Y	Y	Y	Y
Subject x School FE	Y	Y	Y	Y	Y	Y
School x Subject x Trends	N	N	N	N	Y	N

Notes: See Table 1. Level of significance: 10% *; 5% **; 1% ***.

Table 3: Instrumental variables estimates using teacher retiree shares

	(2) 1st stage Exit rate	(3) IV KS4 score	(1) OLS, exit KS4 score	(5) 1st stage Entry rate	(6) IV KS4 score	(4) OLS, entry KS4 score
Entry or exit rate	-	-0.274** (0.115)	0.00746 (0.0101)	-	-0.223 (0.192)	-0.050*** (0.011)
Share retiring, t-1	0.002*** (0.000)			0.001*** (0.000)		
Observations	13,087,027	13,087,014	13,087,013	13,087,026	13,087,013	13,086,563
R-squared	0.451	0.442	0.461	0.631	0.461	0.461
Instrument F stat	236.082	-	-	102.485	-	-
School x Year FE	Y	Y	Y	Y	Y	Y
Subject x Year FE	Y	Y	Y	Y	Y	Y
Subject x School FE	Y	Y	Y	Y	Y	Y

Notes: See Table 1. Level of significance: 10% *; 5% **; 1% ***.Teacher defined as above pension age if they would have been above statutory retirement age by October of the current academic year. Level of significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Heterogeneity in disruption by incoming teacher characteristics

(1) Age		(2) Salary		(3) Experience		(4) Gender		(5) Origin	
20-29	-0.0373** (0.0151)	Quartile 1	-0.0346** (0.0140)	< 2 years	-0.0387*** (0.0146)	Female	-0.0316*** (0.0117)	Other schools	-0.0498*** (0.0111)
30-39	-0.0460*** (0.0149)	Quartile 2	-0.0572*** (0.0169)	2-5 years	-0.0314 (0.0204)	Male	-0.0700*** (0.0157)	Elsewhere	-0.0384** (0.0152)
40-49	-0.0541*** (0.0177)	Quartile 3	-0.0629*** (0.0181)	5-10 years	-0.0566*** (0.0189)				
50+	-0.0610** (0.0246)	Quartile 4	-0.0512** (0.0246)	> 10 years	-0.0583*** (0.0159)				

Notes: See Table 1. Level of significance: 10% *; 5% **; 1% ***. Entry rates for different categories are numbers of entrants in each category in a given school-subject cell, divided by the total number of teachers in that cell. Total numbers of observations around 13 million in all regressions,
Level of significance: 10% *; 5% **; 1% ***.

Table 5: Heterogeneity in disruption by student characteristics

(1) FSM		(2) Gender		(3) Ethnicity		(4) KS2 scores	
Baseline	-0.0478***	Baseline	-0.0756***	Baseline	-0.0482***	Quartile 1	-0.0394***
	(0.0101)		(0.0209)		(0.0103)		(0.0106)
x FSM	0.0153*	x Female	0.0482*	x Non-white	0.0122	Quartile 2	-0.0501***
	(0.00824)		(0.0279)		(0.0102)		(0.0110)
						Quartile 3	-0.0560***
							(0.0115)
						Quartile 4	-0.0399***
							(0.0143)

See Table 1. Level of significance: 10% *; 5% **; 1% ***. Entry rates for different categories are numbers of entrants in each category in a given school-subject cell, divided by the total number of teachers in that cell. Total numbers of observations around 13 million in all regressions,

Table 6: Heterogeneity by subject

VARIABLES	(1) All	(2) Core	(3) Not Core
Entry Overall	-0.0460***	-0.00857	-0.0697***
	(0.0102)	(0.0124)	(0.0150)
Observations	13,087,013	6,988,722	6,098,291
R-squared	0.461	0.407	0.463

See Table 1. Level of significance: 10% *; 5% **; 1% ***. Entry rates for different categories are numbers of entrants in each category in a given school-subject cell, divided by the total number of teachers in that cell. Total numbers of observations around 13 million in all regressions,
Level of significance: 10% *; 5% **; 1% ***.Core subjects include: sciences, mathematics, English and history.

Appendix A: Descriptive statistics

Table A1: Teachers assigned to subjects by year

Subject	2009	2010	2011	2012	2013
Math	18087	19317	20620	21172	21772
English	20112	21374	22173	22762	23453
History	13422	14018	13882	14263	14712
Science	24957	26455	27143	27641	28025
Other Foreign Languages	11540	11994	12308	12553	12787
Sports	14186	14975	15398	15380	15555
Biology	1423	1499	1534	1640	1885
Chemistry	1136	1206	1340	1476	1618
Physics	1224	1294	1378	1501	1576
Art	17714	18499	18732	18802	18854
IT	6697	7267	8225	8043	7722
Social Science	2161	2029	1308	1201	1092
Design	5257	5499	5903	5789	5649
Economics	7909	8374	8593	8468	8500
Home Economics	3422	3647	4394	4360	4232
Media	1117	1208	1439	1469	1430
Humanities	2340	2553	3156	3070	2983
Engineering	964	1016	1115	1164	1139
Total	153668	162224	168641	170754	172984

Table A2: Descriptive Statistics for the estimation sample

Variable	Mean	Std. Dev.	Min	Max
Turnover measures at school-subject-year level				
Entry Overall	0.143089	0.167079	0	1
Exit Overall	0.105481	0.150745	0	1
Entry School	0.083537	0.132876	0	1
Exit School	0.078069	0.131901	0	1
Entry Profession	0.059553	0.105479	0	1
Exit Profession	0.027413	0.077926	0	1
Teacher characteristics				
Female	0.618161	0.263337	0	1
Age	39.76658	5.677334	21	72
Tenure School	6.922622	2.848328	1	20
Student characteristics				
KS4 Standardized Score	0.003364	0.993456	-1.69819	10.17107
KS2 Standardized Score	0.001141	0.998374	-6.60202	2.200005
School/ school-subject group variables				
# Teachers	70.08023	23.46506	2	167
% FSM students	0.13053	0.108553	0	0.756757
%Female students	0.499179	0.174424	0	1
% white students	0.814574	0.22989	0	1
Pupil Teacher Ratio	27.55598	25.22643	0.037037	446
% teachers between 20-29	0.198006	0.195148	0	1
% teachers between 30-39	0.333258	0.230597	0	1
% teachers between 40-49	0.24732	0.217463	0	1
% teachers over 50	0.221408	0.219692	0	1
Number of observations	13,067,661			

Notes: tables summarises the student-level dataset

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

Variable	Coefficient x 100	s.e. x 100	T-Stat
Standardized score in KS2	0.001	0.005	0.262
Proportion White	-2.374	1.672	-1.42
Proportion Female	0.127	0.862	0.148
Proportion FSM	0.158	1.558	0.101
Lag of Average Standardized Score in KS2	-0.011	0.005	-2.007
Lag Proportion of White Student	0.499	1.6	0.312
Lag Proportion of Female Students	-1.4	0.895	-1.564
Lag Proportion of FSM Students	1.137	1.768	0.643

Note: Regressions of entry rates on listed variables. Regressions include School by year, subject by school and subject by year fixed effects. Standard errors clustered at school level.

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