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Unnecessary Disruptions? Implications of the Volatility of Within-School Reassignments on Student Achievement

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ABSTRACT

Educators raise concerns about what happens to students when they are exposed to new teachers or teachers who are new to a school. These teachers face the challenge of preparing a year's worth of new material, perhaps in an unfamiliar work environment. However, even when teachers remain in the same school they can switch jobs—teaching either a different grade or a different subject than they have taught before. There is an extensive literature that explores the various aspects of the challenges confronting new teachers (see, for example, Feiman-Nemser, 2003; Johnson, 2007). While there exists some quasi-experimental literature on the effects for student achievement of being new to the profession (e.g., Rockoff, 2004) or to a school (Hanushek & Rivkin, 2010), to date there is little evidence about how much within-school churn typically happens and how it affects students. In this paper, we use longitudinal panel data from New York City from 1974 to 2010 to document the phenomenon, and we tie assignment-switching behaviors to student achievement in the period since 1999, when student-level data is available. We find that students are far more likely to have a teacher who has undergone a within-school switch than one who is new to the school or new to teaching. We therefore use a variety of fixed effects approaches to estimate the link between student achievement and these three forms of being to new one's job assignment—new to teaching, new to school, or new to position within the same school—with a particular focus on the latter given that so many teachers experience within-school reassignments and we know so little about how students are affected by it.

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Introduction

Educators raise concerns about what happens to students when they are exposed to new teachers or teachers who are new to a school. These teachers face the challenge of preparing a year's worth of new material, perhaps in an unfamiliar work environment. However, even when teachers remain in the same school they can switch jobs—teaching either a different grade or a different subject than they have taught before. There is an extensive literature that explores the various aspects of the challenges confronting new teachers (see, for example, Feiman-Nemser, 2003; Johnson, 2007). While there exists some quasi-experimental literature on the effects for student achievement of being new to the profession (e.g., Rockoff, 2004) or to a school (Hanushek & Rivkin, 2010), to date there is little evidence about how much *within*-school churn typically happens and how it affects students. In this paper, we use longitudinal panel data from New York City from 1974 to 2010 to document the phenomenon, and we tie assignment-switching behaviors to student achievement in the period since 1999, when student-level data is available. We find that students are far more likely to have a teacher who has undergone a within-school switch than one who is new to the school or new to teaching. We therefore use a variety of fixed effects approaches to estimate the link between student achievement and these three forms of being to new one's job assignment—new to teaching, new to school, or new to position within the same school—with a particular focus on the latter given that so many teachers experience within-school reassignments and we know so little about how students are affected by it.

Background

As with most professions, on average teachers exhibit returns to experience particularly during the early career (Atteberry, Loeb, & Wyckoff, 2013; Boyd, Lankford, Loeb, Rockoff, &

Wyckoff, 2008; Clotfelter et al., 2007; Harris & Sass, 2011; Ost, 2009; Papay & Kraft, 2011; Rivkin et al., 2005; Rockoff, 2004). Educational researchers have argued that teachers improve over time because they gain familiarity and fluency both with the act of teaching itself, as well as the interpersonal demands of the profession. However many factors are correlated with how much teachers improve over time, including prior training and pathway into the profession (Boyd, Grossman, Lankford, Loeb, & Wyckoff, 2009; Kane, Rockoff, & Staiger, 2008), on-the-job professional development (Yoon, 2007), the strength of school leadership (Boyd et al., 2011; Grissom, 2011), the quality of professional networks within schools (Atteberry & Bryk, 2010), the effectiveness of grade-level peers (Jackson & Bruegmann, 2009), and school socio-environmental factors including trust, peer collaboration, and shared decision-making (Bryk & Schneider, 2002; Bryk, Sebring, Allensworth, Luppescu, & Easton, 2010; Kraft & Papay, 2014). Developing access to many of these resources—or reaping the benefits of them—often takes time. Trust, for instance, is an iterative and long-term discernment process through which actors judge one another's intentions and worthiness of trust (Bryk & Schneider, 2002). When teachers are brand new to the profession, to a school, or even to a particular working group within a school, they may need to re-establish their connection to these resources. Along those same lines, Ronfeldt, Loeb, and Wyckoff (2013) hypothesized that the negative relationship they observe between high rates of new-to-school teachers and achievement could be explained by the disruption of working norms. Given that teacher improvement may be associated with these local conditions, we therefore begin by considering the reasons that teachers switch schools and roles, potentially disrupting their development.

Why might teachers switch jobs within schools? First, teachers may be relatively more effective in one position than another, and either school leaders or the teachers themselves may seek to optimize the matches of teachers to jobs. Second, some jobs may simply be more appealing, and teachers may vie for these positions. As teachers gain experience and influence within schools, they may be able to obtain these coveted positions. Finally, new demands such as differential enrollments across student cohorts, new courses, difficulty hiring for particular positions, may necessitate reassignment even if neither leaders nor teachers would otherwise seek such reassignment.

Of these three reasons, the first—more optimal matching—might lead to improved outcomes. Either principals or teachers might instigate these changes. In order for principals to re-assign teachers strategically, they must understand differences in the quality of their teachers and be able to act on that knowledge. Extant research provides evidence that many principals do have the ability to discern differences in teacher quality (Jacob & Lefgren, 2008; Rockoff, Staiger, Kane, & Taylor, 2012), and furthermore that some principals actively use reassignments strategically to achieve their goals (Chingos & West, 2011; Cohen-Vogel, 2011; Grissom, Kalogrides, & Loeb, 2013). These authors conclude that school leaders are attempting to better match teachers to available vacancies. For example, teachers report that principals are more involved in the assignment of teachers to tested grades than to other grades, and teachers whose students have lower test score gains are more likely to move away from tested grades (Grissom, Kalogrides, & Loeb, 2014).

The other two reasons for churn—teachers seeking more desirable positions and convenience due to other changes in the school—do not necessarily have benefits for students. When teachers are new to their job assignments, they may be less effective as they adjust to the new environment and demands. In many fields initial performance is quite low relative to more experienced peers (Lynch, 1989; Neal, 1995; Topel, 1990), often followed by large improvements in subsequent iterations of the task, and then a leveling off in growth rates as the task becomes more familiar and/or the individual becomes more expert. Such a pattern is evident in teachers' returns to additional years of experience: On average, teachers are least effective in their first year on the job, but become more effective at improving student test performance during their first few years of teaching. Figure 1 depicts returns to experience from eight studies, as well as our own estimates using data from New York City (Atteberry et al., 2013; Boyd et al., 2008; Clotfelter et al., 2007; Harris & Sass, 2011; Ost, 2009; Papay & Kraft, 2011; Rivkin et al., 2005; Rockoff, 2004). Each study shows increases in student achievement as teachers accumulate experience such that by a teacher's fifth year her or his students are performing, on average, from 5 to 15 percent of a standard deviation of student achievement higher than when he or she was a first year teacher. Thus, being new to the profession is clearly challenging, but by definition being brand new only happens once in a career.

However, one can think of "newness" on a continuum. One's job can be *entirely* new (as is the case in the first year, described above), the job assignment can be virtually identical from one year to the next, or it can be somewhere in the middle with some aspects of the job—but not others—new to the individual at a given point in time. Changes in the "what" and "where" of a job may re-introduce some newness back into the work.

While most research on teacher experience has examined the effect on students of having a teacher who is new to the profession, teachers who are new to schools might also face challenges. When a teacher moves to a new school to teach the same class, many aspects of the work will remain the same, including the developmental age with whom she works, and the general content of the curriculum. However the teacher may need to make meaningful changes to her instructional materials either to suit a new population of students, or to integrate with the general strategies that are used in her new school. Further, the social norms of the school are entirely new to her, and it may require time and energy to learn how to navigate a new system and/or work with new colleagues. Surprisingly little evidence exists on the impact of being assigned to a new-to-school teacher. Because being new-to-school involves less unfamiliarity than being new to the profession, the average effect of a cross-school reassignment on student achievement may be negative, but less so than the effect of being a first year teacher.

Similarly, being switched to a new assignment within the same school may also reintroduce some novelty into the work of a teacher. Sometimes moving involves a grade-only shift (e.g., teaching third grade to fourth grade), a subject switch (e.g., switching from teaching social studies to English language arts), or both (e.g., fifth grade math to eighth grade science). Being new to one's specific job assignment within the same school may also be challenging for teachers, though perhaps less so than being new to the profession or the school. While such a teacher would continue to possess institutional knowledge and working relationships within the school, the teacher may need to become familiar with a new grade-level or subject-specific curriculum. She may also find herself working with a new set of grade- or subject-specific colleagues. On a daily basis, a new-to-assignment teacher may need to create new lesson plans and/or use existing materials that were previously unfamiliar. The "newness" of these annual within-school switches may cause teachers to be temporarily less effective, and students assigned to switching teachers may exhibit lower achievement than had they been assigned to a teacher who taught in the exact same school-subject-grade the previous year.

We therefore hypothesize that the most challenging form of being new to assignment is being entirely new to the profession, followed by cross-school moves, and finally we hypothesize that within-school reassignments are negative but less so than the other two. It is worth noting, however, that even if within- and between-school reassignments are initially associated with decrements to student achievement in the year of the switch, it is possible that the teachers are ultimately moving into positions that suit them better (i.e., the optimal matching scenario). If this were true, then we would expect that teachers' effectiveness in years following a reassignment would rise above their observed effectiveness in the year(s) prior to the move. Initial decrements to effectiveness may be outweighed by longer-term student achievement improvements if teachers are systematically moving into positions in which they excel—a possibility we also explore in this paper.

To better understand within-school churning, this study addresses three research questions:

- How often and at what points in their career do teachers switch school-, subject-, and/or grade-level assignments?
- Are students who belong to historically underserved groups (i.e., non-white, low socioeconomic status, non-native English speakers) more likely to be assigned to teachers who are new to subject-grade, school, or the profession?
- What is the impact on student achievement of being assigned to teachers who are new to their school, subject, and/or grade assignment?

Data and Sample

The data for this analysis are administrative records from a range of databases provided by the New York City Department of Education (NYCDOE) and the New York State Education Department (NYSED). Data on teachers is drawn from both NYCDOE and NYSED. The NYCDOE data include information on teacher race, ethnicity, experience, and school assignment as well as a link to the students and classroom(s) in which that teacher taught each year. The NYSED collects information from all public education employees through an annual survey and maintains a database called the Personnel Master File (PMF) which records information about job assignments, percentage of time allocated to each position, annual salary, age, gender, and experience. The PMF covers the time period from 1974 to 2010 (with the exception of the 2003 school year), and contains unique employee identifiers that can be linked to data on student achievement and schools.

New York City students take achievement exams in math and English Language Arts (ELA) in grades three through eight. All the exams are aligned to the New York State learning standards and each set of tests is scaled to reflect item difficulty and are equated across grades and over time. Tests are given to all registered students with limited accommodations and exclusions. Thus, for nearly all students the tests provide a consistent assessment of achievement from grade three through grade eight. For most years, the data include scores for 65,000 to 80,000 students in each grade. We standardize all student achievement scores by subject, grade and year to have a mean of zero and a unit standard deviation. The student data also include measures of gender, ethnicity, language spoken at home, free-lunch status, special-education status, number of absences in the prior year, and number of suspensions in the prior year for each student who was active in any of grades three through eight in a given year.

Defining teacher transitions can be difficult because often researchers do not have complete information on the set of vacancies that need to be filled each year. Instead, we observe a series of yearly snapshots of teacher job placements at a given point in time. We describe our approach in detail in Appendix A, but briefly summarize it here. When a teacher is classified as having a different subject-grade-school assignment in a given year than in the previous year, we refer to this as a "switch" or "reassignment." We focus on three mutually exclusive switch types: (1) teachers who are new to their position because they are entirely new to the district; (2) teachers who appear in a different New York City school in year y versus y-1; and (3) within-school switches—teachers who are in the same school but in a different subject and/or grade from year y-1 to year y. Many teachers, especially those in middle school, have multiple assignments. To be classified as experiencing a within-school switch, the teacher must have a different *primary* (i.e., greatest percentage of their time) subject- and/or grade-level assignment than the previous year in the same school (again, see Appendix A for a complete discussion of how primary subject and grades were identified, as well as complications arising from ambiguous or missing information).

Analytic Sample

The overall analytic sample for this paper is the set of New York City employees who were ever classroom teachers in traditional public schools (i.e., non-charters) between 1974 and 2010 (2.4 million teacher-year observations with 271,492 unique teachers). When examining impacts on student outcomes, we further restrict the sample to teachers present in 1999 through 2010 who are linked to student achievement (1 million teacher year-observations with 179,037 unique teachers). We exclude data from any schools in their first year of operation since, by definition, all of their teachers would be "new to school" in that year.

In most analyses, we further limit the sample to the set of person-years in which we can observe an employee's switch status. In order to identify a switch in a given school year, we must observe the subject or assignment type for person p in years y (current) and y-1 (prior), the grade level (if applicable) in both years, the school of record in both years, each person's current years of experience in order to identify teachers who are new, and years of experience within the district in order to identify teachers who are new to New York City. As alluded to above, a teacher's *primary* teaching assignment can be ambiguous, because her time may be divided equally among several classrooms. In these cases, it is not possible to determine whether a genuine switch has occurred since one definitive subject-grade assignment cannot be identified. These observations must therefore be removed from the analysis (See Appendix A for a complete discussion of the approach used to define subject-grade assignments.) The overall sample is reduced by 6.2 percent and the achievement sample by 11.5 percent due to an inability to identify the primary grade or subject of the teacher in a particular year. In total, we have an overall analytic sample of 2.0 million teacher-year observations and a sub-sample of 785,076 teacher-years that are linked to student achievement.

Methods

Research Question 1

For our first research question, we present descriptive statistics about the frequency of switch types across teacher-years. We also examine the timing of within-school switches throughout the average teacher's early career. This allows us to determine whether being re-assigned within schools is something that only some teachers experience or that virtually all teachers undergo, and whether it tends to happen more than once in the career. This will be germane to a subsequent analysis in which we examine the impact of a teacher's initial switch on not only next year's outcomes, but also for subsequent years before she switches a second time.

Research Question 2

For our second research question, we assess whether students who belong to historically underserved groups (i.e., non-white, low socioeconomic status, non-native English speakers) are more likely to be assigned to teachers who are new to subject-grade, school, or the profession.

Should we find that switching has a negative impact on student achievement, the answer to this question would provide evidence on the equality of educational opportunities within and across schools.

We are also interested in whether teachers who are new to their assignment in a given year tend to have other characteristics (in terms of the students they serve, their own characteristics, or the kinds of schools they work in) that might bias estimates of the effect of being new-to-assignment on student achievement if not accounted for in the estimation approach. It is difficult to establish a causal link between switching behaviors (new to teaching, a school, or a subject-grade assignment) and student achievement since many factors could be associated with both switching and student achievement. A few examples may prove useful here. For students within the same schools, teachers with more seniority often have more discretion in terms of the kinds of students and classes they teach. If more senior teachers can select to work with less challenging students and are also less likely themselves to change assignments, more challenging students may be systematically more likely to be exposed to switching teachers who are in turn more likely to be novice. At the teacher level, principals may try to move their struggling teachers around in order to find a better "fit." Again, here we can imagine how a selection problem arises if struggling teachers also tend to experience more switching. In this scenario, reassignments would appear to be associated with lower student performance, but in fact the prior low performance is the cause of the reassignment, not the effect. Finally, at the school level, we know from prior work that teachers tend to leave schools serving disadvantaged and minority students at higher rates (Boyd, Lankford, Loeb, & Wyckoff, 2003). When teachers leave at higher rates, schools are likely to have to move teachers around and hire more novice teachers in order to replace them. Switch rates thus may be higher in schools serving disadvantaged students, but it is often difficult to disentangle the impact of the switching itself from the fact that it happens more in schools that are likely to have lower student achievement for reasons unrelated directly to the churning. We explore these hypotheses to examine whether students, teachers, or schools might "select into" within-school churn at higher rates.

To estimate individual students' probabilities of being assigned to a teacher who is new to her primary school-subject-grade assignment in a given year, we run three separate linear probability models for teacher-year level binary outcomes for each of three specific teacher switch types: (1) Teacher p switches subject-grade within same school or not ("$SameSch_{py}$"); (2) the teacher switches from another school or not ("$OtherSch_{py}$"); and (3) teacher is new to teaching or New York City or not ("$NewTchr_{py}$"). Equation (1) shows the generic model for the first of these three outcomes:

$$SameSch_{py} = \beta_0 + (\boldsymbol{X_i})\boldsymbol{\beta} + (\boldsymbol{W}_{iy})\boldsymbol{\beta} + \varepsilon_{ipgsy} \tag{1}$$

We predict students' assignment to teachers undergoing each of these three kinds of switches as a function of a vector of time-invariant student-level characteristics ("$\boldsymbol{X_i}$") comprised of student sex, race/ethnicity, and an indicator of whether the student's home language is English, as well as time-varying characteristics ("$\mathbf{W}_{iy}$") including eligibility for the Free-/Reduced-Price Lunch program, the student's current ELL status, the number of absences and suspensions for the given student in a given year, as well as the student's standardized achievement (averaged across math and ELA) in the prior year. We conduct these analyses both with and without school fixed effects to explore whether any observed association between student characteristics and exposure to re-assigned teachers is related to cross-school sorting or occurs even within the same school. We conduct the analyses with all student characteristics included together in a single model, as well as sequentially (i.e., with each mutually-exclusive set of student categories as the sole regression predictors). The former version allows us to explore whether significant differences in assignment to the treatment of interest remain after the inclusion of all observed confounding variables. If so, this may guide us to prefer certain specifications of the subsequent fixed effects regressions. On the other hand, by examining student predictors one at a time, we can address the question of whether any negative estimated impacts are likely to be disproportionately experienced by students of color, of low socioeconomic status, or for students who are English language learners.

In the same vein, we explore whether certain kinds of teachers are more likely to churn (or be churned). We focus on within-school churns ("$SameSch_{py}$") as the outcome of interest in Equation (2):

$$SameSch_{py} = \beta_0 + (\boldsymbol{T_p})\boldsymbol{\beta} + \beta(Exp_{py}) + [\beta(PriorVA_{py})] + \varepsilon_{tsy} \quad (2)$$

We predict a teacher's probability of churning as a function of a set of time-invariant teacher-level characteristics ("$\boldsymbol{T_p}$") comprised of teacher demographics (sex and race/ethnicity), information about teacher preparation (SAT scores, competitiveness of undergraduate institution, and pathway into teaching, as well as teachers' time-varying years of experience[1] (Exp_{py}) and, in some models, prior year value-added scores ($PriorVA_{py}$).

Finally, we explore the possibility that certain kinds of schools engage in more teacher within-school churning than others. We calculate the churn rate for each school in each year (i.e., the percentage of the faculty in the given year who were teaching in the same school but in a different subject or grade in the previous year). Because churn rates in a given year may be somewhat unstable, we take the mean for each school across three years (2006-07 through 2008-09) and predict this mean within-school churn rate as a function of average school characteristics

[1] We explored the possibility of using a quadratic function for years of experience but found that the acceleration parameter was estimated to be 0 and thus it was removed for parsimony.

during the same time period. We can see whether, for instance, schools serving disadvantaged populations have less stability in teaching assignments from one year to the next. Again this is relevant for thinking about what potential confounding factors may be associated with both the treatment of interest (switching into a new assignment) and the outcome, student achievement.

Research Question 3

Ultimately, we are interested in whether the pervasive phenomenon of teacher reassignments—the three kinds of switches—appear to have a positive or negative impact on student achievement. As previously stated above, establishing a causal link between switching and student achievement is difficult since students, teachers, and schools do not randomly experience reassignments. Many confounding factors may be associated with switching behavior and student achievement.

For these reasons, we take a number of different approaches to estimating the association between student achievement outcomes and teacher switching behaviors, in an effort to eliminate potential unobserved confounding factors. We begin with a basic education production function, in which all observable characteristics of students, classrooms, teachers, and schools are directly controlled.

$$A_{ipgsy} = \beta_0 + \beta_1\left(OtherSch_{py}\right) + \beta_2\left(SameSch_{py}\right) + \beta_3\left(NewTchr_{py}\right) + \boldsymbol{A'}_{ipgsy}\boldsymbol{\beta} + \boldsymbol{X}_{ipgs(y)}\boldsymbol{\beta_3} + \boldsymbol{C}_{pgsy}\boldsymbol{\beta_4} + \boldsymbol{T}_{p(y)}\boldsymbol{\beta_5} + \boldsymbol{S}_{sy}\boldsymbol{\beta_6} + \varepsilon_{ipgsy} \tag{3}$$

In Equation (3), A_{ipgsy} is student i's standardized test score when exposed to teacher p in grade g in school s in year y. A'_{ipgsy} is the student's set of standardized test score in the other subject, as well as both subjects previous year. $X_{ipgs(y)}$ is a vector of student time-invariant and time-varying covariates, including gender, race/ ethnicity, free/reduced price lunch status, English language learner status, special education status, an indicator of whether the student's home language is English, number of absences, and number of suspensions. C_{pgsy} is a set of classroom covariates, which are aggregated from the student level. $T_{p(y)}$ is the set of time-invariant and time-varying teacher covariates, including years of experience, sex, race/ethnicity, pathway into teaching, competitiveness of undergraduate institution, and math and verbal SAT scores. Finally, S_{sy} represents aggregated time-varying school-level covariates including the percentage of students who are FRPL-eligible, the school suspension rate, and percentage of students who are non-white.

The main predictors of interest are a set of three key dummy variables, which indicate the kind of teaching assignment switch a teacher experienced in a given year, if any. The first, $OtherSch_{py}$, is set to equal 1 if teacher p switched to school s in year y from a different New York City school, and 0 if not. The second predictor, $SameSch_{py}$, equals 1 if the teacher switched assignments within the *same* school from last year to the current year. Finally, $NewTchr_{\mathrm{py}}$, is set to 1 if teacher p is new to the teaching profession or the district in year y. If all three of these variables equal 0 for a given teacher, the teacher experienced no change in assignment from last year to the current year. That is, she teaches the same subject and grade in the same school in year y-1 and year y.

Though we have controlled for many factors that might confound the estimated impact of switching, we remain concerned that other unobserved factors may be associated with both switching behaviors and student achievement. We therefore also introduce a number of fixed effects to further isolate the switching behavior. For instance, in one specification we replace the teacher time-invariant characteristics with teacher fixed effects so that the coefficients on the switching predictors of interest become within-teacher estimates. That is, we examine whether student achievement scores appear to be lower for the same teacher in the years that she experiences a given switch, as compared to that same teacher in another year in which a switch did not occur. One might be concerned, for instance, that less effective teachers are more likely to be churned within-school. The teacher fixed effects allow us to try to separate a teacher's latent (time-invariant) effectiveness from the act of switching. This is one of the preferred specifications, since we will see some evidence that assignment to particular positions within a school might be related to teacher characteristics. However, it is of course possible that some teacher-level confounders—such as teaching effectiveness—depends on circumstances that fluctuate from year-to-year and therefore would not be captured by the teacher fixed effects.

We also run the model with student, school, school-by-grade, and school-by-year fixed effects. Each of these has its own logic, isolating a source of variation that can be exploited in order to rule out a certain set of unobserved potential confounders. The student fixed effects, for instance, can eliminate any unobserved time-invariant student characteristics as a potential confounding factor for the analysis by examining how a given student performs in years in which his or her teacher experienced a switch versus years in which the student had a teacher who did not switch. This is a useful approach if we find that students are non-randomly sorted to switching teachers, particularly if that sorting occurs among students within the same school. The student fixed effects approach remains vulnerable to unobserved, endogenous, time-varying factors.

The school fixed effects approach, on the other hand, makes comparisons among switching teachers within the same school. This is also a potentially compelling specification because teachers working within the same school are generally exposed to the same leadership, building-level assignment policies, student composition, etc. However the school fixed-effects do not account for time-varying characteristics of the school, nor any important within-school variation, for example across grades. We therefore also run school-by-grade and school-by-year fixed effects specifications, which further limit the within-school comparisons to particular grades, or particular years (to rule out, for instance, the possibility that some secular trends in the teacher labor market may confound the analysis).

Results

RQ 1: How Often Do Teachers Switch School-, Subject, and Grade-Level Assignments?

The movement of teachers to new teaching assignments is substantial. On average, 41.5 percent of teachers are switching in some way—either new to the district, to the school, or their subject-grade assignment—each year. As Table 1 shows, of those switches, 21.6 percent are new teachers, 24.9 percent are cross-school movers, and the clear majority of switches (53.5 percent) take place within the same school. Thus, about a quarter of all teachers churn every year within their school into new subject-grade assignments. Switching of any kind is less frequent in elementary schools (36.2 percent), and somewhat more frequent in high schools (46.9 percent) than in middle schools (44.4 percent). Within-school churning is particularly prevalent in high schools, with 59.6 percent of all switches occurring within-school. While the within-school churn rate has fluctuated modestly over time, varying between 43 and 63 percent over the 36 years in the analytic sample, it has always been the most dominant form of switching. Overall, within-school churn is approximately twice as likely as cross-school reassignments each year, yet to date very little attention has been paid to its frequency or impact.

In describing the overall phenomenon of within-school churn, one natural question is whether this re-shuffling occurs simply as a result of teachers departing from the school the previous year. Indeed, the correlation between the rate of teacher exits from a school and the subsequent year's within-school churn is 0.45, which suggests that prior year departures tend to lead to current year teacher switches. That said, shuffling cannot be purely accounted for by new vacancies: For every teacher exit from a school last year, there are on average 4.3 teachers who switch assignments within school the following year (Figure 2). Therefore replacing departing teachers is not a matter of simply moving or hiring *one* other teacher. Although most of the observations are clustered near the median of 3.4 switches per exit, the spread in Figure 2 illustrates that some schools experience much greater switching. This provides some preliminary evidence that schools may engage in teacher reassignments differently from one another.

Most teachers who remain in the system for multiple years will experience a switch. To report on the differential frequencies of switching, we examine the first fifteen years of teachers' careers to explore if they are switched, and if so how often. In Table 2, when we examine teachers during their first two years (row 1), about 76 percent have not yet experienced a within-school switch from year 1 to year 2, though about 24 percent do. In the second row, which examines teachers throughout the first four years of experience, we see that the number of teachers who have not yet churned within school drops to about 46.7 percent. So already by the fourth year of the career, teachers are more likely to have experienced a within-school churn than not. As teachers continue their career, they become even more likely to experience at least one (if not more) within school churns. Indeed, among teachers who are observed throughout the first fifteen years, only 10.6 percent have never been churned within their school, while 53.8 percent of those teachers will have already experienced 3 or more churns. This suggests that, while there may be a small group of teachers who do not experience churn, most experience churn early in their career and more than one time.

RQ 2: Are Students Who Belong to Historically Underserved Groups More Likely to be Assigned to Switching Teachers?

Student-Level Analysis

Overall, there is some modest evidence that non-white, low socioeconomic status, and ELL students may be more likely to be assigned to switching teachers, in some cases even within the same school. In Table 3, we present results across six models (each of the three switch types, both with and without school fixed effects). The constant in the model represents the probability of being assigned to a teacher experiencing the given switch type for a male, white student who is not Free/Reduced-Price lunch eligible, who is not ELL and does speak English at home, with no absences and suspensions and with average prior achievement (in other words, a relatively advantaged student). In column (1) for instance, we see that such a student has an 18.1 percent chance of being assigned to a teacher who is experiencing a within-school churn. The coefficients on each student characteristic represent a difference in probability of being assigned to a re-assigned teacher in a given year relative to that more advantaged peer. The statistical significance levels are somewhat difficult to interpret given the very large sample sizes of students, therefore we focus on coefficients that represent at least a one percentage point difference in probability. Black students and Hispanic students are 3.5 points more likely to be assigned to a within-school churned teacher (column 1), and ELL-designated students are 5.5 percentage points more likely to be assigned to such a teacher. The magnitude of these coefficients is large relative to the constant, roughly a 20 percent increase for Black and Hispanic students and a 30 percent increase for ELL students. In Column 2, we add the school fixed effects and generally find that most of the associations are no longer meaningfully large (i.e., smaller than a 1 percentage point change). The one exception to this pattern is that the ELL finding persists within schools (4.7 percentage points). It is possible this reflects the difficulty of recruiting and retaining ELL teachers, so ELL students may be more subject to staff instability than other students even within the same school.

Transfers between schools are less frequent than within school switching and appear to have little association with student attributes (columns 3 and 4 of Table 3). Black and Hispanic students continue to exhibit a 1 to 2 percentage point higher probability of being assigned to a teacher who is new to the school, but those associations are not present within schools. Unlike in Columns 1 and 2, the coefficients on the ELL predictor in Columns 3 and 4 are not meaningfully large. Overall, there seem to be fewer differences across students—both within and between schools—in terms of probability of being assigned to a new-to-school teacher than we saw for probability of being assigned to a churning teacher.

Finally, Black and Hispanic students have about a 3 percent higher probability of being exposed to new teachers, relative to an estimated constant of 9.6 percentage points (column 5, Table 3). A few other characteristics play a role here as well; students eligible for free lunch have a 1.5 percentage point higher chance of encountering a new teacher, while an increase in student achievement of one standard deviation reduces the likelihood of having a new teacher by 2.8 percentage points. In addition, the coefficient on students' ELL designation in the new teacher model (β= -0.022 in column 5) goes in the *opposite* direction from the within-school churn model (column 1), suggesting that ELL students are slightly less likely to be exposed to new teachers.

Once school fixed effects are added (column 6) most of the differences observed in column 5 are quite small (i.e., less than one percentage point). The coefficients on ELL (β= -0.024) and prior year test scores (β= -0.025) persist within schools, suggesting that ELL students and students with lower test scores are less likely to have a new teacher when compared to similar students within the same school.

Taken together, these results suggest that historically underserved students may have somewhat higher probabilities of being assigned to switching teachers, even when controlling for all other observed covariates and, in some cases, even when limiting comparisons to students in the same school. However, the magnitude of these differences is typically small. The largest estimated coefficient is about a 5 percentage point difference. These multivariate models set the stage for the fixed effects models employed to estimate the impact of switching on student achievement.

We also estimate simple univariate relationships between individual student covariates and assignment to churning, new-to-school, and brand new teachers. In Table 4, we use the same layout of predictors, outcomes, and model specifications, however each coefficient now comes from a separate, simple linear regression and captures the unconditional probability of assignment to each switch-type.[2] As one would expect, many more of the simple linear relationships are statistically significant (though most remain substantively small). However it is clear that—if being new-to-assignment, the school, or teaching negatively impacts achievement overall— then Black, Hispanic, Free/Reduced-Price Lunch eligible, non-native English speakers with lower prior achievement would be more likely to be assigned to those teachers. Even though the associations are modest, having more than one risk factor could aggregate, perhaps leading to an equity issue related to exposure to teachers who are new to their subject, grade, and or school assignment.

Teacher-Level Analysis.

[2] Sets of categorical dummy variables are still kept together in a single model. For instance, when exploring student race/ethnicity, the indicators for Black, Hispanic, Asian, and Other/Unknown are all included so that the reference category is White students.

The analysis above suggests why it is important to account for observable student characteristics that may be both associated with assignments to teachers who churn, as well as student achievement. In the same vein, we explore whether female and minority teachers with different pathways into the profession, less experience, or lower value-added scores may be more likely to churn (or be churned).

In Table 5, we present results from three versions of Equation (2), in which we predict probability of experiencing a within-school churn ("$SameSch_{ty}$") as a function of the full set of teacher covariates described above (column 1). In Column 2, we replace the time-invariant teacher characteristics with teacher fixed effects. In Column 3, we add a school fixed effect so that we can make comparisons among teachers within the same school. Again, the school fixed effects are crucial for allowing us to disentangle sorting of teachers across schools that may assign teachers differently from non-random assignment of teachers within schools.

We are also interested in whether a teacher's probability to be churned was related to his or her value-added scores in the year preceding the observation, however only approximately 15 percent of the sample possesses these value-added scores. In Columns 4 – 6, we added prior-year value-added scores (PriorVA_{py})[3] to each model, though aware this dramatically alters the analytic sample. This allows us to explore, for instance, whether the same teacher tends to be re-assigned in relation to fluctuations in her value-added estimates of effectiveness over time. However because fluctuations from year-to-year in value-added are noisy within person, this model may not capture the meaningful changes in true teaching effectiveness which could predict propensity to be switched to a new assignment.

[3] See Appendix B for a full explanation of how value-added scores are estimated.

Controlling for other factors, there are some systematic differences in teachers' propensities to be switched to a new assignment in their same school, however the magnitude of these differences is typically not large. For instance, we see in Column 1 that, while the conditional probability of a within-school switch is statistically different for male and female teachers, the difference is about half a percentage point (β= -0.006**). Again, we choose to focus on relationships that are least one percentage point different in magnitude. When not including school fixed effects, Black and Hispanic teachers are 2 to 2.7 percentage points more likely to experience a within-school switch, and while the magnitude diminishes when we include school fixed effects (Column 2), they do not disappear. In terms of teacher preparation, SAT are not a strong predictor, but we do see some 1-point differential probabilities by competitiveness of undergraduate institution (which persist in Column 3 when school fixed effects are included). There are also some differences in conditional propensity to switch by teacher pathway: TFA teachers are 3.7 percentage points less likely to be switched than teachers entering the profession through traditional pathways (omitted category), while those entering through other (e.g, alternative certification) or unknown pathways are slightly more likely to be switched within school. Again, the findings on teacher pathway variables persist in the school fixed effects model, but are somewhat more muted. Finally, we see that there is a statistically significant but substantively weak, negative relationship between experience and switching (β= -0.001** in Column 1), which suggests that, conditional on all other observed covariates, more veteran teachers are slightly less likely to be re-assigned than similar teachers with fewer years of experience (results are similar when we include school fixed effects in Column 3). It is interesting to note, however, that when we replace the time-invariant teacher covariates with the teacher fixed effects in Column 2, the coefficient on years of experience reverses direction, though it remains substantively small (β= 0.004*** in Column 2).

Finally, we repeat these three models by adding teacher prior value-added (see Table 5, Columns 4-6). Recall that these models are now necessarily restricted to grade 4 – 8 math and ELA teachers, by virtue of including value-added scores. Prior value-added scores are a significant predictor of propensity to churn: The higher one's value-added, the less likely they are to churn (β= -0.072*** in Column 4), even when comparing teachers in the same school (β= -0.070*** in Column 6). It is interesting to note, however, that when we examine the results from the model that predicts outcomes by prior value-added scores *with* teacher fixed effects included in the model, no relationship persists. In other words, value-added scores do not appear to predict why the same teacher is assigned to switch assignments within school in some years but not others.

Taken together, these results suggests that teachers may be systematically targeted for re-assignment both within- and between- schools. Teacher race/ethnicity is a persistent predictor of propensity to be reassigned in all models. The relationship between years of experience and reassignment depends on whether looking within or across teachers, and whether one also controls for prior value-added. Prior value-added is also related to propensity to be reassigned, except when looking within teacher. The covariates in Table 5 will be included as controls in the subsequent models used to isolate exogenous variation in reassignments, so we do not have to be concerned specifically about these factors biasing our estimates. However, we are concerned that, if teachers are systematically reassigned based on the things we *do* observe, there may be other teacher-level endogenous variables that we do *not* observe that cannot be included directly.

For this reason, teacher fixed effects may prove a particularly important specification of models used to link reassignment to impacts on student achievement.

School-Level Analysis.

We find some evidence that schools that serve higher percentages of Black students, English Language Learners, or students with higher rates of suspension or absenteeism also tend to experience more within-school churn (see Table 6). For instance, a one percentage point increase in the number of Black students in the school is associated with a 0.037 percentage point increase in the churn rate (statistically, but perhaps not substantively significant). We also present results from a second model that also adds prior-year turnover to the model. Even after controlling for the demographic characteristics of the school, this is a significant predictor of churn.

Overall, there is some evidence that historically underserved groups of students are more likely to be assigned to switching teachers (even within the same school), certain kinds of teachers are more likely to be switched, and certain schools may experience greater degrees of switching, however these relationships tend to be weak. These findings have two potential implications. The first is that it may be difficult to isolate the impact of churning from the fact that this behavior appears to be non-random—an issue we take up in the next section. The second implication is that, if we do find evidence of negative impacts of these various forms of being new to one's assignment, some students may be more likely to experience those negative effects.

RQ 3: What is the Impact on Students of Being Assigned to Switching Teachers?

Switching teacher assignments negatively affects student achievement across all three types of switches. Table 7 presents results for student achievement outcomes in Math (top panel) and ELA (bottom panel). Irrespective of the model, moving into a school (new to teaching in NYC or transferring from a different NYC school) negatively influences student achievement more than within school moves. Indeed, given that the conceptual model suggests that "newness" and "unfamiliarity" might be the primary mechanism driving a negative impact of switching, the relative magnitude of the results seems reasonable: Brand new teachers are new to all aspects of their assignments– the job itself, the school, the colleagues, as well as the specific class itself. Teachers who are moving across schools, on the other hand, are confronting new circumstances and social norms, but they are not new to the act of teaching and thus we would expect the negative impact of this form of "newness" would be relatively less strong than being completely new. Finally, teachers who churn within the same school are not new to the school culture, but their particular subject-grade assignment, responsibilities, and immediate subject- or grade-level assignments have changed. The results suggest that the more aspects of one's subject-grade-school assignment are unfamiliar, the more negative the impact of the reassignment. Results are relatively consistent across all model specifications with various fixed effects. For instance, the coefficient on the indicator for within-school churn is consistently between -0.012 and -0.018 and statistically significant all models. Though the magnitude of these effects is small (on average, about a third the size of the effect of having a new teacher), keep in mind that nearly a quarter of all teachers are churning within their own school every year, thus making the aggregate effect on the distribution of student achievement notable. The estimates are closest to zero in the model that includes teacher fixed effects, and largest in the model that includes school fixed effects. Results are similar for ELA outcomes (lower panel of Table 7), however the coefficients on the within-school churn variable are not consistently statistically significant.

Recall that, about 20 percent of the person-years in the dataset do not have a clear "primary" subject-grade level assignment. We conduct a bounding exercise related to these ambiguous teacher-year observations and find that our results are robust to the various assumptions one could make about the status of those unknown cases (see Appendix C for descriptive of approach and presentation of results).

Is it Harder to Switch Subjects, Grades, or Both?

In order to further probe the nature of the negative impact of within-school churning, we hypothesized that switches might be more challenging for teachers when they were more dissimilar to the prior year assignment. For instance, it might be the case that it is more difficult to switch both subjects *and* grades simultaneously rather than just switching one or the other. To explore this, we further subdivided the within-school churn indicator into three distinct sub-categories (a) a within-school switch of grade only (subject remained the same), (b) a within-school switch of subject only (grade level remained the same), and (c) a within-school switch of both subject and grade. In essence, we ran Equation (3) with five dummy variable predictors of interest rather than three, in which the indictor of within-school churn "$SameSch_{ty}$" has now been replaced by the three sub-categories of churn-type described above.

Of the within-school switches, 74 percent were a grade switch only, 14 percent were a subject switch only, and 12 percent were both. While it is straightforward to think about scenarios in which teachers switch grades only, it may be less clear what kinds of transitions are captured by the "subject-only" switch category—that is, teachers remaining in the same grade and school but teaching a different subject. Indeed, this is the least common form of within-school switch. Many of the subject-only switches are characterized by teachers who were assigned to a grade-specific "English as a Second Language" classroom, or a "Special Education" classroom in the previous year but now are in ELA, math, or elementary (i.e., whole classroom) positions in the current year. We also see teachers who were previously teaching a non-tested subject to a specific grade (e.g., fine arts, science, foreign language, or social studies) who now primarily teach math, ELA, or elementary students in the current year. One might be concerned that subject-only switches only occur in some grades, thus limiting those analyses to specific grade levels. However subject-only switchers are approximately evenly distributed across grades, with the exception of grade 6, which has about twice as many subject-only switchers as any other grade.

Switching both subjects and grades at the same time is more difficult than just switching one or the other. Table 8 presents the results for this analysis for math achievement outcomes for just three specifications of the model—with teacher (M2), school (M3), or student fixed effects (M6)—for the sake of parsimony. According to the model with student fixed effects (final column), switching both subject *and* grade is associated with a -0.023 decrease in student achievement, while switching subjects only was associated with a -0.010 decrease, and switching grades only was associated with a -0.019 decrease. Results for Model 2 (teacher fixed effects) and Model 3 (school fixed effects) also show that switching both subject and grade may be slightly more negative than switching only one or the other, though the magnitude of all coefficients is again smallest in the teacher fixed effect specification. Taken together, these findings suggest that the phenomenon may operate in a way that is consistent with a conceptual frame of newness—when both subject and grade level are new, the challenge of teaching may be greater when either the approximate age or the subject matter has not changed.

Is the Impact of Switching Temporary?

When thinking further about our descriptive findings that teachers appear to be reassigned within their school multiple times during their career, we wondered about whether the impact of switches might be temporary—i.e., strongest in the year in which the teacher was new to the school, subject, and/or grade. We imagine three possible scenarios for what we might observe. First, it is possible that switching teachers may have a temporary cost in terms of teacher impacts on student achievement in the year of the switch, but ultimately these switches might lead teachers to find a better fit between their own strengths and their teaching assignment. In this scenario, we would expect to find that student achievement scores drop in the year of the switch itself, however in subsequent years the teacher's students' scores would exceed pre-switch levels. A second possibility is that switches are less strategic and more random. In this case, we would expect to find that scores drop in the year of the switch, but in post-switch years teachers simply revert back to their pre-achievement switch levels. In other words, there is nothing about the switch experience that systematically improves the teacher's ability to improve student learning. The third possibility is that switching is a negative experience with lasting negative impacts on teachers. If this were the case, we would expect to find that, after student test scores drop in the year of a switch, they do not return to pre-switch levels afterwards.

In order to examine these competing hypotheses about the lasting impacts of switching behavior, we use the education production function framework from Equation (3) but change the coding scheme to reflect whether each student was assigned to a teacher who switched (a) in the current year, (b) last year, (c) two years ago, or (d) three or more years ago. The omitted category then becomes expected achievement outcomes for students in years that pre-date the first reassignment. Furthermore, we limit the sample here to the set of teacher-year observations that occur one year prior to a teacher's *first* within-school switch and one year before a *second* switch occurs. Because teachers switch many times in their career on average, mid-career years can ambiguously be classified as either post- one switch, but simultaneously pre- the next switch. Imagine, for instance, that a teacher is re-assigned within the school in both her third and fifth years on the job. The fourth year could be considered the year *after* the first switch, but also the year before the next switch. Limiting the sample in this way allows us to isolate a subset of teacher-year observations in which the temporal pattern of switching is unambiguous, however it also narrows the focus to the effects of the first time a teacher is switched.

Results in Table 9 differ somewhat depending on model specification. As before, we see that there is a negative decrement to student achievement in the year a teacher is re-assigned. However, the coefficients on years subsequent to the switch are less consistent across models. While the coefficients tend to be positive, suggesting that the teachers' students are performing *better* than they had in the year before the switch occurred, those differences are significant only in the models with school-by-grade, school-by-year, and student fixed effects. In this temporal exploration, the specification with teacher fixed effects is perhaps most straightforward in terms of thinking about a teacher's pattern of switch behavior from one year to the next. In that version of the model (column 2), there do not appear to be any statistically significant differences between pre-switch and post-switch student outcomes. The lack of positive increases post-switch suggests that—however decisions are made about shuffling teachers within the same school—these movements do not appear to match teachers to subject-grade assignments in which they are more effective.

Conclusions

This paper documents a phenomenon that most practitioners understand but that education researchers have largely ignored: The incredible prevalence of annual within-school reassignments to new teaching positions. We have situated this phenomenon within a larger body of work that examines other instances in a teacher's career when he or she is new to their teaching assignment—either in the first year on the job, or when teachers move across schools from one year to the next. All three of these switch types share a common theme—it is more difficult to be effective at complex tasks when the task is unfamiliar. We contribute to this body of work by documenting that within-school switches are twice as common as between school switches or being new to the district. We also find that there is a modest negative impact of being assigned to teachers when they are new to teaching, the school, or their subject-grade assignment. The relative negative impact of these phenomena follows a pattern that suggests that the more "new" the teaching assignment is, the more challenging the teaching may be in a given year: The impact on student achievement is most negative when students are assigned to brand new teachers, followed by teachers who are new to the school, and finally (least strongly but still negative) to teachers who are in the same school but new to their subject and/or grade.

The estimated impact of within-school churn is not large in absolute terms. However, given that about a quarter of all teachers each year are churning within the same school, these small negative decrements add up: The estimated impact of churning is, on average, about a third the size of the impact of being assigned to a brand new teacher—a phenomenon that has received a great deal of attention in the field. However, in any given year, more than twice as many students will be assigned to a churning teacher than a new teacher, in essence doubling the overall impact on the distribution of student achievement. Stated another way, the average student only encounters one brand new teacher between grades three through eight, but four or five churning teachers in the same time frame. Furthermore, we find some evidence that some schools experience more of this churn than others, and one might be concerned that schools serving disadvantaged populations of students are also the schools most likely to have instability in their teacher assignments. Our analysis also suggests that even within the same schools, historically-underserved student groups may be more likely to be assigned to churning teachers than their more privileged counterparts: While the average student has about a 24 percent chance of being assigned to a churning teacher in any given year, a white, male student who is not FRL-eligible, is not an ELL student, and has not been suspended only has an 18 percent chance of being assigned to a churning teacher. Taken together, the results of the current paper suggest a widespread and understudied phenomenon that negatively affects the students of almost all teachers at some point in their career, and disproportionally affects disadvantaged students.

This paper generates several questions. While we conclude that the average impact of within-school churn appears to be negative, it is not clear whether that average effect is a relatively accurate description of the effect in all places, or instead whether the impact varies dramatically perhaps from one school to the next. We hypothesized that *some* teacher reassignment could be beneficial for students if these decisions are made strategically in order to optimize what and where teachers teacher. However in the current data we have no way to differentiate discretionary movements intended to either improve student outcomes (e.g., I think teacher A will work more effectively with older students) or to satisfy teacher requests for certain types of students or subject matter from unavoidable staffing driven movements (e.g., the need to replace exiting teachers or there are more fourth graders this year than last year and so we need to move some teacher into fourth grade). One might hypothesize that some school leaders may develop strategies around re-allocating teachers that benefit students. Again, this is difficult to observe in the current data, as we have relatively shallow insight into how individual schools are managed. In results not shown here, we conducted preliminary analyses to explore whether the impact of churn was different for schools in the top and bottom third of distributions on various student characteristics (i.e., schools in the top third of math performance vs. the bottom third). In none of these top- versus bottom-third comparisons were the impacts of churn positive, nor were the group differences statistically significant from one another. The lack of differential impact across these groups is only a first step towards trying to identify places where within-school reassignments are conducted in strategic ways that benefit students. Administrative data alone provides relatively blunt ways of characterizing schools, and these demographic dimensions may fail to help us account for any variability in the effect of churn across schools. Future work in this area might generate and test hypotheses for school characteristics that could cause or support beneficial within-school churn.

We end with a final word on the policy implications of the current analyses. Of course, it is impractical to imagine that within-school churn can or should be eliminated by policy. Indeed, it is an unavoidable artifact of such a large system that instability can and will occur. The current findings do highlight just how much of that switching is taking place on an annual basis: a full 40 percent of all teachers are new to the district, the school, or their subject-grade each year, and half of those switches occur within school. If our findings are corroborated in other districts, it may be the case that school administrators should recognize that re-assigning a teacher will have a small, negative impact on students, and that exposing students to high doses of this churning could more meaningfully influence their achievement. This recognition may cause schools and districts to temper the level of discretionary churning. Future research could collect more nuanced data to classify different types of churning and better understand whether discretionary churning benefits students.

Tables

Table 1. Frequency of Switching, and Breakdown among Switchers by Type of Switch

	All Teacher-Years		Breakdown Among Switchers		
	No Switch	Any Switch	New to NYC	New to School	Churn Within School
Overall Rates					
All Teachers	58.5%	41.5%	21.6%	24.9%	53.5%
By School Type					
Elem.	63.8%	36.2%	24.9%	23.5%	51.7%
Middle	55.6%	44.4%	22.3%	24.3%	53.5%
High	53.1%	46.9%	17.2%	23.3%	59.6%
Other	60.0%	40.0%	23.1%	27.1%	49.8%

Table 2. Percent of Teachers Who Experience 0, 1, 2, or 3+ Within-School Churns, Within Given Periods of their Career

	No Switches	1 Switch	2 Switches	3+ Switches
First 2 Years	76.0%	24.0%	n/a	n/a
First 4 Years	46.7%	29.4%	13.4%	10.5%
First 6 Years	34.0%	29.2%	18.3%	18.5%
First 8 Years	25.7%	26.6%	20.2%	27.5%
First 10 Years	19.4%	23.8%	20.8%	36.0%
First 15 Years	10.6%	17.3%	18.3%	53.8%

Each row is limited to the set of teachers who are observed at least in their first X years of teaching, and the columns capture the number of switches (0, 1, 2, or 3+) that have occurred within those first X years.

Table 3. Predicting Students' Conditional Probability of Assignment to Switching Teachers, based on Full Vector of Student Characteristics

	Y= "$SameSch_{ty}$" (Teacher Switches within Same School)		Y= "$OthSch_{ty}$" (Teacher Switches from Other School)		Y= "$NewTch_{ty}$" (Teacher New to NYC or New to Teaching)	
Female	0.002 *	0.002 **	-0.001	-0.001 **	-0.002 ***	-0.002 ***
	(0.001)	(0.001)	0.000	0.000	(0.001)	(0.001)
Black	0.035 ***	-0.005 ***	0.019 ***	0.003 ***	0.030 ***	0.006 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Hispanic	0.035 ***	0.002	0.011 ***	0.000	0.027 ***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Asian	-0.008 ***	-0.001	-0.001	-0.001	0.000	-0.004 **
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Free-Lunch Eligible	0.008 ***	-0.004 **	0.000	0.003 ***	0.015 ***	0.009 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Reduced-Lunch Eligible	0.001	-0.003	-0.001	0.002 *	0.004 **	0.002
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Student's Home Lang Not English	0.004 ***	-0.002 *	-0.002 ***	0.004 ***	0.008 ***	0.008 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Student Designated ELL	0.055 ***	0.047 ***	0.002 *	0.001	-0.022 ***	-0.024 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Number of Absences	0.000	0.000 ***	0.000 ***	0.000 ***	0.000 ***	0.000 *
	0.000	0.000	0.000	0.000	0.000	0.000
Number of Suspensions	0.001	0.000	0.003 ***	0.001	0.004 ***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Prior Year Mean Std Test Score	0.002 ***	0.007 ***	-0.011 ***	-0.010 ***	-0.028 ***	-0.025 ***
	(0.001)	(0.001)	0.000	0.000	0.000	0.000
Constant	0.181 ***	0.219 ***	0.050 ***	0.055 ***	0.096 ***	0.123 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.004	0.066	0.004	0.062	0.010	0.055
N	1,469,674	1,469,672	1,469,674	1,469,672	1,469,674	1,469,672
Grade Fixed Effects?	Y	Y	Y	Y	Y	Y
School Fixed Effects?	N	Y	N	Y	N	Y

Table 4. Predicting Students' Unconditional Probability of Assignment to Switching Teachers, Based on Individual Student Characteristics (Entered One at a Time)

	Y= "$SameSch_{ty}$" (Teacher Switches within Same School)		Y= "$OthSch_{ty}$" (Teacher Switches from Other School)		Y= "$NewTch_{ty}$" (Teacher New to NYC or New to Teaching)	
Female	0.002 ***	0.001 *	-0.001 ***	-0.002 ***	-0.003 ***	-0.004 ***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.032 ***	-0.011 ***	0.023 ***	0.004 ***	0.049 ***	0.014 ***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Hispanic	0.043 ***	0.003 **	0.015 ***	0.005 ***	0.047 ***	0.008 ***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Asian	-0.002 *	0.000	-0.002 ***	-0.001	0.002 *	-0.003 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Free-Lunch Eligible	0.029 ***	-0.002 **	0.009 ***	0.006 ***	0.040 ***	0.012 ***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Reduced-Lunch Eligible	0.009 ***	-0.004 ***	0.004 ***	0.003 ***	0.021 ***	0.008 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Student's Home Lang Not English	0.010 ***	0.010 ***	-0.007 ***	0.003 ***	0.002 ***	0.005 ***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Student Designated ELL	0.053 ***	0.043 ***	0.005 ***	0.010 ***	0.008 ***	0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Number of Absences	0.000 ***	0.000 ***	0.000 ***	0.000 ***	0.001 ***	0.000 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of Suspensions	0.003 ***	-0.002 *	0.009 ***	0.003 ***	0.014 ***	0.005 ***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Prior Year Mean Std Test Score	-0.014 ***	0.002 ***	-0.014 ***	-0.010 ***	-0.032 ***	-0.021 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R-Squared	n/a	n/a	n/a	n/a	n/a	n/a
Separate Univariate Regressions?	Y	Y	Y	Y	Y	Y
School Fixed Effects?	N	Y	N	Y	N	Y

Table 5. Predicting Teachers' Probability of Within-School Reassignment, based on Teacher Characteristics

		All Teachers			Limit to Teachers with VA Scores		
	(Column):	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)
Teacher Demographics	Male Teacher	0.006 ***		-0.006 ***	0.003		-0.002
		(0.001)		(0.001)	(0.004)		(0.005)
	Black Teacher	0.020 ***		0.016 ***	0.032 ***		0.019 ***
		(0.001)		(0.002)	(0.005)		(0.005)
	Hispanic Teacher	0.027 ***		0.021 ***	0.064 ***		0.056 ***
		(0.002)		(0.002)	(0.006)		(0.006)
	Teacher Race Oth/Unk	0.003		0.008 ***	0.025 ***		0.017 *
		(0.002)		(0.002)	(0.007)		(0.007)
Teacher Preparation	Std Math SAT	-0.004 **		-0.004 **	-0.008		-0.004
		(0.001)		(0.001)	(0.004)		(0.004)
	SAT Score Missing Dummy	0.003 *		-0.005 ***	0.005		0.002
		(0.001)		(0.001)	(0.004)		(0.004)
	Std Verb SAT	0.001		0.001	0.005		0.004
		(0.001)		(0.001)	(0.004)		(0.004)
	UG Inst Most Competetive	-0.009 ***		-0.009 ***	-0.011		-0.001
		(0.002)		(0.002)	(0.007)		(0.007)
	UG Inst Competetive	-0.012 ***		-0.010 ***	-0.009		-0.005
		(0.002)		(0.002)	(0.006)		(0.006)
	UG Inst Less Competetive	-0.011 ***		-0.010 ***	-0.005		-0.001
		(0.001)		(0.001)	(0.005)		(0.005)
	UG Inst Unknown	-0.013 ***		-0.021 ***	-0.014 *		-0.015 *
		(0.002)		(0.002)	(0.007)		(0.007)
	Teaching Fellows Pathway	0.003		0.006	0.032 **		0.027 *
		(0.004)		(0.004)	(0.011)		(0.011)
	TFA Pathway	-0.037 ***		-0.030 ***	0.070 ***		0.055 **
		(0.007)		(0.007)	(0.018)		(0.018)
	Other Pathway	0.015 ***		0.010 ***	0.012 **		0.007
		(0.001)		(0.001)	(0.004)		(0.004)
	Unknown Pathway	0.025 ***		0.015 ***	0.008		0.010
		(0.002)		(0.002)	(0.007)		(0.007)
Time-Varying Characteristics	Yrs of Experience	-0.001 ***	0.004 ***	-0.001 ***	-0.001 ***	0.011 ***	-0.001 ***
		0.000	0.000	0.000	0.000	(0.001)	0.000
	Prior Year VA Score				-0.072 ***	-0.024	-0.070 ***
					(0.010)	(0.014)	(0.010)
	constant	0.216 ***	0.167 ***	0.224 ***	0.264 ***	0.171 ***	0.272 ***
		(0.002)	(0.002)	(0.002)	(0.005)	(0.009)	(0.005)
	R-Squared	0.0020	0.3350	0.0240	0.0050	0.5090	0.0430
	N	616,608	616,608	616,608	64,788	64,788	64,788
Fixed Effects?		None	Teacher	School	None	Teacher	School

Omitted Categories include female teachers, white teachers, and teacher who attended an undergraduate institution that was "not" competetive and entered teaching through a traditional "college-recommended" pathway. The value-added score is the mean of math and ELA value-added scores (when both are available in the same year) from the year preceding the switch.

Table 6. Three-Year Average Within-School Churn Rate, as a Function of Average School Characteristics

Avg. School Enrollment	0.003 ***	0.002 ***
	0.000	0.000
Percent Students Female	0.036	0.037
	(0.020)	(0.022)
Percent Students Black	0.037 ***	0.043 ***
	(0.007)	(0.007)
Percent Students Hispanic	-0.001	0.008
	(0.008)	(0.008)
Percent Students Free/Red Price Lunch	-0.005	-0.012
	(0.010)	(0.010)
Percent Students ELL	0.114 ***	0.104 ***
	(0.018)	(0.018)
Avg. Number of Suspensions	4.461 ***	3.032 *
	(1.276)	(1.308)
Avg. Number of Absences	0.181 ***	0.147 ***
	(0.030)	(0.031)
Percent Students Special Education Status	-0.021	-0.046 *
	(0.021)	(0.022)
Percent of Teachers Who Left Last Year		-0.098 ***
		(0.013)
Constant	13.283 ***	19.218 ***
	(3.244)	(3.371)
R-Squared	0.083	0.096
N	3247	3247

Predictors are school-level 3-year means (2007-2009), expressed as percentage points on a scale of 0 to 100. The outcome is the 3-year mean churn rate in the school (2007-2009), also expressed as percentage on a scale of 0 to 100.

Table 7. The Impact of Three Switch Types on Student Math Achievement, Across Model Specifications

MATH

	M1	M2	M3	M4	M5	M6
Dummy: 1= Switched from Other Sch	-0.047 ***	-0.029 ***	-0.050 ***	-0.051 ***	-0.054 ***	-0.044 ***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy: 1= New to Teaching/ NYC	-0.057 ***	-0.052 ***	-0.063 ***	-0.063 ***	-0.068 ***	-0.063 ***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Dummy: 1= Switched Within Same Sch	-0.017 ***	-0.012 ***	-0.018 ***	-0.017 ***	-0.017 ***	-0.016 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.654	0.689	0.658	0.665	0.667	0.892
N	1526733	1526733	1526733	1526733	1526733	1526733
Fixed Effects?	--	Teacher	School	Sch x Grade	Sch x Year	Student

ELA

	M1	M2	M3	M4	M5	M6
Dummy: 1= Switched from Other Sch	-0.015 ***	-0.020 ***	-0.022 ***	-0.024 ***	-0.027 ***	-0.014 *
	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)	(0.006)
Dummy: 1= New to Teaching/ NYC	-0.037 ***	-0.027 ***	-0.037 ***	-0.036 ***	-0.040 ***	-0.038 ***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.005)
Dummy: 1= Switched Within Same Sch	-0.003	0.000	-0.006 **	-0.006 *	-0.009 ***	-0.011 **
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
R-squared	0.582	0.602	0.585	0.589	0.593	0.85
N	1515366	1515366	1515366	1515366	1515366	1515366
Fixed Effects?	--	Teacher	School	Sch x Grade	Sch x Year	Student

All models shown here have time-varing and time-invariant student characteristics, aggregated time-varying classroom covariates, teacher time-invariant and time-varying characteristics, and school time-invariant and time-varying characteristics (except when collinear with the relevant fixed effects).

Table 8. Effects of Different Kinds of Within-School Switches: Subject Only, Grade Only, or Both

	M2		M3		M6	
Dummy: 1= Switched from Other School	-0.030	***	-0.054	***	-0.052	***
	(0.003)		(0.002)		(0.002)	
Dummy: 1= New to Teaching/ NYC	-0.059	***	-0.069	***	-0.073	***
	(0.002)		(0.001)		(0.002)	
Dummy: 1= Switched Subject (only) Within Same School	0.000		-0.006	*	-0.010	***
	(0.003)		(0.002)		(0.003)	
Dummy: 1= Switched Grade (only) Within Same School	-0.012	***	-0.020	***	-0.019	***
	(0.001)		(0.001)		(0.001)	
Dummy: 1= Switched Grade & Subject Within Same School	-0.013	***	-0.022	***	-0.023	***
	(0.004)		(0.003)		(0.003)	
Constant	0.411	***	0.441	***	0.345	***
	(0.009)		(0.008)		(0.010)	
R-squared	0.688		0.657		0.889	
N	1526733		1526733		1526733	
Fixed Effects?	Teacher		School		Student	

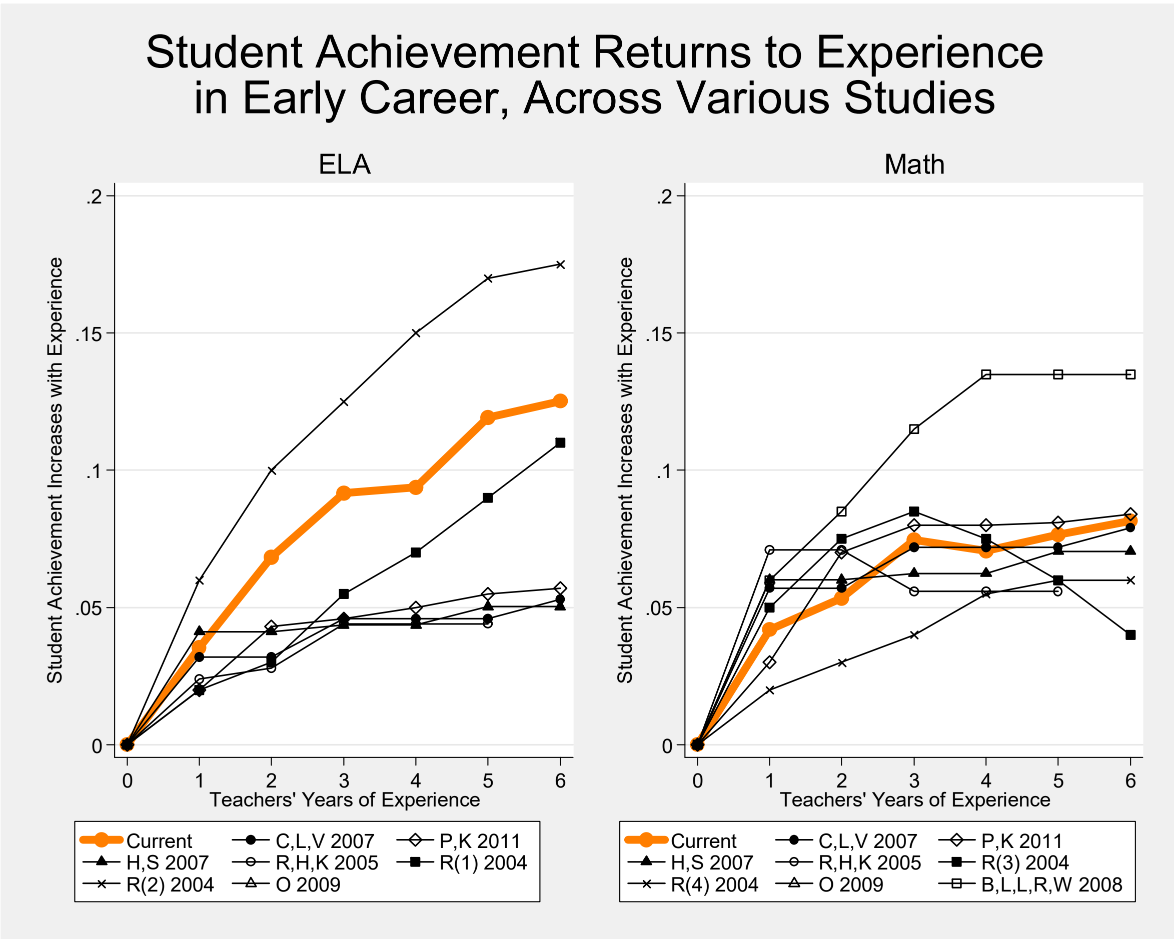
Table 9. The Temporal Impact of Within-School Switching on Math Achievement

	M1	M2	M3	M4	M5	M6
Constant (Omitted= Yr Prior to Switch)	0.378 ***	0.348 ***	0.431 ***	0.390 ***	0.398 ***	0.319 ***
	(0.007)	(0.011)	(0.009)	(0.009)	(0.010)	(0.012)
Dummy: 1= Year Switched (any type)	-0.020 ***	-0.019 ***	-0.022 ***	-0.021 ***	-0.023 ***	-0.021 ***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Dummy: 1= 1 Yr(s) After Switched (any type)	0.002	0.004	0.003	0.005 **	0.005 **	-0.004 *
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy: 1= 2 Yr(s) After Switched (any type)	0.007 **	0.004	0.007 **	0.008 ***	0.015 ***	0.008 ***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Dummy: 1= 3+ Yr(s) After Switched (any type)	0.005 *	0.002	0.005 *	0.003	0.025 ***	0.015 ***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
R-squared	0.649	0.687	0.654	0.662	0.665	0.909
N	1146914	1146914	1146914	1146914	1146914	1146914
Fixed Effects?	--	Teacher	School	Sch x Grade	Sch x Year	Student

Sample: Teacher-year observations that occur one year prior to a teacher's first within-school switch and one before a second switch occurs. All models shown here have time-varing and time-invariant student characteristics, aggregated time-varying classroom covariates, teacher time-invariant and time-varying characteristics, and school time-invariant and time-varying characteristics (except when collinear with the relevant fixed effects).

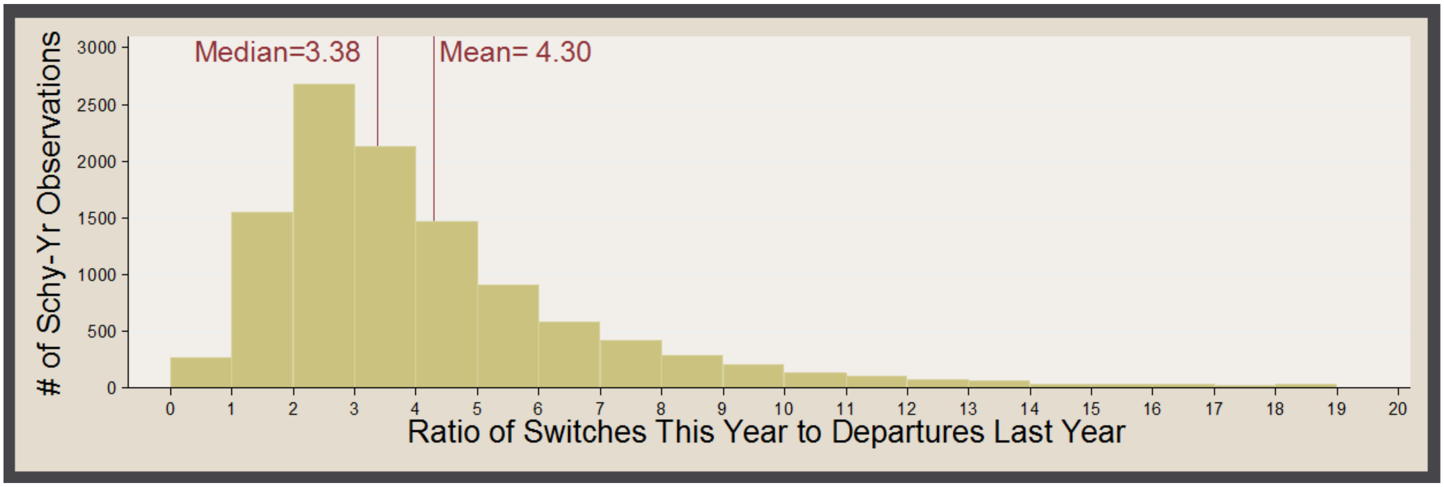
Figures

Figure 1: Student Achievement Returns to Teacher Early Career Experience, Preliminary Results from Current Study (Bold) and Various Other Studies



Results are not directly comparable due to differences in grade level, population, and model specification, however Figure 1 is intended to provide some context for estimated returns to experience across studies for our preliminary results. Current= Results for grade 4 & 5teachers who began in 2000+ with at least 9 years of experience. For more on model, see Technical Appendix. C,L V 2007= = Clotfelter, Ladd, Vigdor (2007; Rivkin, Hanushek, & Kain, 2005), Table 1, Col. 1 & 3; P, K, 2011 = Papay & Kraft (2011), Figure 4 Two-Stage Model; H, S 2007 = Harris & Sass (2011), Table 3 Col 1, 4 (Table 2); R, H, K, 2005= Rivkin, Hanushek, Kain (2005), Table 7, Col. 4; R(A-D) 2004 = Rockoff (2004), Figure 1 & 2, (A= Vocab, B= Reading Comprehension, C= Math Computation, D= Math Concepts); O 2009 = Ost (2009), Figures 4 & 5 General Experience; B,L,L,R,W 2008 = Boyd, Lankford, Loeb, Rockoff, Wyckoff (2008).

Figure 2. Distribution of Ratio of This Year's Switches to Last Year's Departures, across School-Years



Appendix A: Definitions of Switching

We refer to a teacher as "departing" from her school in the given year when one of two things occurs: First, the teacher may exit New York City permanently, which we observe based on the Human Resources data that records each teacher's "last year" and "last year paid." The second way we observe a school departure is when the teacher appears in a new school in the following year. Using these two sources of information, we can calculate the number and percentage of teachers who departed from a school at the end of any year ("departure rate"). We are often interested in the departure rate of a school in the previous year, because we hypothesize that departure rates in the previous year (y-1) may play some role in how much switching takes place in the following year. It is important to note, however, that one does not imply the other: For example, teachers could leave a school in y-1 and no switches could occur in the next year if no replacement teachers were hired (perhaps due to decreasing enrollment). On the other hand, no teachers could leave in y-1, and we could still see switches the following year (either due to additional teachers being hired, *or* teachers within the school switching subject- and grade-level assignments). That said, in general we expect that when more departures occur at the end of last year, there is more room for switching around in the following year.

New York City tracks movements of teachers across schools from one year to the next, making it possible to observe when teachers are new to New York City, new to the profession, or new to a given school. However, characterizing subject and grade switches is less straightforward than it might initially seem. We base switch behavior on the PMF data reported by teachers annually. Two factors complicate our attempt to identify each teacher's change in within-school subject-grade assignment from one year to the next. In the PMF, teachers are asked to report *all* of their teaching assignments, and 35 percent of teachers in the PMF are assigned to more than one role each year. For the 65 percent of teacher-years with exactly one assignment, it is straightforward to identify a primary subject and grade. For the other 35 percent, the primary role may be less clear. Fortunately, teachers also report the percentage of their time dedicated to each role. For the purposes of this paper, we therefore use the teacher's role with the greatest percentage of time as their observed "primary" subject-grade role. Using this approach, 97 percent of teacher-years have a primary subject, and 82 percent have a primary grade (since teachers in middle and high school often teach across grades). For 79 percent of the teacher-year observations, we can identify both a primary subject and grade. However for the other 21 percent, time is split equally across multiple roles or information is idiosyncratically missing about either subject or grade level. These observations are omitted from the primary impact analyses in this paper, though we conduct bounding exercises that do include these observations—more on this below.

Changes in assignments from one year to the next are much more continuous than one variable can perfectly capture. The decision to identify a single "primary" subject and grade may overestimate the prevalence of switching since a teacher whose allocation of time shifts (but course load does not) could be characterized as experiencing a "switch" even though the only thing that has actually changed is which of her roles takes most of her time. In order to address this concern, we also explored the use of other, more conservative definitions of within-school assignment switches.[4] However, the vast majority—88 percent—of the teacher-year observations are not subject to this source of ambiguity about within school reassignments—either because

[4] For instance, we examined the following, more conservative definition of subject/ grade switching: A within-school grade switch occurs if and only if there is no overlap between *all* the grades taught by a given teacher in the current year and *all* the grades taught by that teacher in the previous year. Under this definition, a teacher who taught 6th and 7th grade math last year, and 7th and 8th grade math this year would not be identified as switching, because some of the grades she taught last year overlap with the grades she taught this year. While this definition does decrease the number of teachers who were identified as switching, the percentage of all teachers for whom such ambiguity exists is sufficiently small to have no substantive impact on the results presented in the paper.

they are new to teaching (so we are certain they are new to their assignment), new to their school (so we are certain they are not churning within the school), or because they have exactly one subject and grade assignment and therefore role switches are straightforward. For the remaining 12 percent, definition of subject/grade assignment matters: Our most conservative definition identifies 26 percent of those ambiguous cases as switches, while our less conservative definition based on *primary* subject and grade identifies 32 percent of those ambiguous cases as switchers. However, in the larger context of all teacher-year observations, it only changes our overall estimate of the frequency of within school-churn by 1 to 3 percentage points.

Therefore, while we acknowledge that the reduction of multiple assignments per year to a single, primary assignment in each year likely overestimates the rate of within-school churn, the effect of that decision on our estimate of the frequency of within-school churn appears to be small. Furthermore, the use of the primary subject/grade approach should also attenuate the estimated effect of switching, since the binary version of the variable exaggerates the extent to which teachers roles may change from one year to the next. We also perform a bounding exercise in which we make assumptions about the switching behaviors about those omitted teacher-years and include them in the analyses.

The second complication with identifying a primary subject-grade assignment change arises due to the nature of subject/role *names* in the dataset. In the PMF, there are 4,383 unique role descriptions (e.g., elementary teacher, creative writing, drama, physical education, first year algebra, advanced algebra, school counselor, European culture studies). Therefore even small changes in the name of a role might have appeared to be a "switch" when in fact it is not. We therefore categorize those unique roles into 15 categories—see Appendix Table A1.

Appendix Table A1. Fifteen Categories of Role Type based on Course/Role Titles, Frequency and Percentage

admin:	1,118 course titles	597,142 records	(7.68)
cte:	1,110 course titles	406,015 records	(5.22)
ela:	103 course titles	981,718 records	(12.63)
elem:	16 course titles	854,765 records	(10.99)
esl:	49 course titles	313,908 records	(4.04)
fine art:	379 course titles	524,418 records	(6.74)
foreign:	438 course titles	330,894 records	(4.26)
library:	13 course titles	85,256 records	(1.10)
math:	263 course titles	720,685 records	(9.27)
other:	92 course titles	305,143 records	(3.92)
phys/health ed:	82 course titles	471,837 records	(6.07)
science:	300 course titles	627,458 records	(8.07)
social studies:	235 course titles	634,004 records	(8.15)
special:	499 course titles	831,214 records	(10.69)

Most (68 percent) of teacher-year subject/roles were easily categorized into either ELA, math, elementary, social studies, science, or administrative. Other categories included fine arts, physical/health education, career/technical education, foreign language, etc. All administrative roles (e.g., principals, assistant principals) were collapsed into a single role called "admin." For about 3 percent of all observations, we had no clear subject to which the role belonged (e.g., driver education, study skills, safety education, cooperative work experience, or course titles that said "other"), and we categorized these remaining observations as simply as "other."

In thinking about how to capture subject switches, we sought to balance our approach both by being specific enough to capture meaningful changes in the teacher's instructional responsibilities, but also not too specific to pick up on often small distinctions in role descriptions. This approach may lead to an underestimate of the frequency of within-school churn. For instance, a teacher who teaches Geometry in one year but switches to Algebra II in the following year will not be categorized as having a subject-switch, because both would be counted as teaching the subject of “math.” This decision rule implies that we may be under-reporting within-school switches, something worth keeping in mind given that the frequency of churn even under this conservative assumption is surprisingly high. In addition, this decision rule means that the within-school switches at the heart of the current analyses represent quite fundamental changes in role—for instance, from teaching science to teaching math (history to ELA, ELA to math, etc.).

Appendix B: Estimating Teacher Value-Added Scores

Although there is no consensus about how best to measure teacher quality, this paper defines teacher effectiveness using a value-added framework in which teachers are judged by their ability to stimulate student standardized test score gains. While imperfect, these measures have the benefit of directly measuring student learning and they have been found to be predictive of other measures of teacher effectiveness such as principals' assessments and observational measures of teaching practice (Atteberry, 2011; Grossman et al., 2010; Jacob & Lefgren, 2008; Kane & Staiger, 2012; Kane, Taylor, Tyler, & Wooten, 2011; Milanowski, 2004), as well as long term student outcomes (Chetty, Friedman, & Rockoff, 2011). Our methods for estimating teacher value-added are consistent with the prior literature. We estimate teacher-by-year value-added employing a multi-step residual-based method similar to that employed by the University of Wisconsin's Value-Added Research Center (VARC). VARC estimates value-added for several school districts, including until quite recently New York City.

We initially estimate Equation (A), which regresses achievement (Y_{icsjt}) for student i in class c at school s taught by teacher j in time t as a function of prior achievement ($Y_{icsjt_{-1}}$), student attributes (X_{icsjt}), and class fixed effects (α_{csjt}). In this model, the class fixed effects subsumes both the teacher-by-year fixed effect (τ_{jt}) and any other class (Z_{csjt}) or school-level (S_{st}) predictors of student achievement.

$$Y_{icsjt} = \lambda Y_{icsjt_{-1}} + \beta' X_{icsjt} + \alpha_{csjt} + \varepsilon_{icsjt} \qquad \text{(A)}$$

where $\alpha_{csjt} = \gamma' Z_{csjt} + \varphi' S_{st} + \tau_{jt}$

We calculate the residuals ($\hat{\varepsilon}_{icsjt}$) from this regression without accounting for α_{csjt} and then estimate Equation (B) which regresses this residual on class and school characteristics as well as a class random effect (ζ_{jt}) to reflect the grouping of students into classrooms.

$$\hat{\varepsilon}_{icsjt} = \alpha_{csjt} + \varepsilon_{icsjt} = \gamma' Z_{csjt} + \varphi' S_{st} + \zeta_{jt} + \omega_{icsjt} \qquad \text{(B)}$$

We calculate the residuals (q_{icsjt}) from this model and calculate teacher-by-year value-added by averaging across the student-level residuals within a teacher and year.

$$\hat{\tau}_{jt} = \bar{q}_{icsjt} \qquad \text{(C)}$$

The teacher-by-experience fixed effects become the value-added measures which serve as the outcome variable in our later analyses. They capture the average achievement of teacher j's students in year t, conditional on prior skill and student characteristics, relative to the average teacher in the same subject and grade. Finally, we apply an Empirical Bayes shrinkage adjustment to the resulting teacher-by-year fixed effect estimates to adjust for measurement error.

In the teacher-by-year value-added model presented above we make several important analytic choices about model specification. Our preferred model uses a lagged achievement as opposed to modeling gain scores as the outcome).[5] The model attends to student sorting issues through the inclusion of all available student covariates rather than using student fixed effects, in part because the latter restricts the analysis to comparisons only between teachers who have taught at

[5] Some argue that the gain score model is preferred because one does not place any prior achievement scores which are measured with error on the right-hand side, which introduces potential bias. On the other hand, the gain score model has been criticized because there is less variance in a gain score outcome and a general loss of information and heavier reliance on the assumption of interval scaling. In addition, others have pointed out that the gain score model implies that the impacts of interest persist undiminished rather than directly estimating the relationship between prior and current year achievement (McCaffrey, Lockwood, Koretz, Louis, & Hamilton, 2004; McCaffrey, Sass, Lockwood, & Mihaly, 2009).

least some students in common.[6] At the school level we also opt to control for all observed school-level covariates that might influence the outcome of interest rather than including school fixed effects, since this would also only allow valid comparisons within the same school.

[6] A student fixed effects approach has the advantage of controlling for all observed and unobserved time-invariant student factors, thus perhaps strengthening protections against bias. However, the inclusion of student-level fixed effects entails a dramatic decrease in degrees of freedom, and thus a great deal of precision is lost (see discussion in McCaffrey et al., 2009). In addition, experimental research by Kane and Staiger (2008) suggests that student fixed effects estimates may be *more* biased than similar models using a limited number of student covariates.

Appendix C: Bounding Exercise on Main Findings

Recall that, for about 20 percent of the person-years in the dataset do not have a clear "primary" subject-grade level assignment. We conduct a bounding exercise related to these ambiguous teacher-year observations and find that our findings are generally robust to the various assumptions one could make about the status of those unknown cases (see Appendix C for descriptive of approach and presentation of results).

A small percentage of this occurs due to missing data, but this primarily occurs when teachers have more than one subject or grade-level assignment and no one of those assignments makes up a clear majority of their time. In order to explore the role of this ambiguity, we conduct a bounding exercise in which we first assume that all person-year observations missing information about switch status are non-switchers. We re-estimate the same regression model now with these missing observations included as individuals who do not churn within schools. To bound at the other end, we also make the assumption that all missing observations *were* in fact within-school churns and re-run the same analyses. We would be especially concerned if the estimated impact of switching were positive under some of these assumptions but negative for others. This would suggest that our results may be sensitive to the missing data problem and could be swayed in either direction if the missing data were in fact not missing. The bounding exercise results for math in (Appendix Table C1 and C2) shows that results are not sensitive to these assumptions, remaining negative and statistically significant for all three switch types and all models.

Appendix Table C1. Bounding Exercise for Math.

A. Assume All Teachers with Ambiguous Switch Status *did* Switch Within School

	M1	M2	M3	M4	M5	M6
Dummy: 1= Switched from Other Sch	-0.047 ***	-0.028 ***	-0.050 ***	-0.050 ***	-0.055 ***	-0.044 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy: 1= New to Teaching/ NYC	-0.058 ***	-0.053 ***	-0.064 ***	-0.064 ***	-0.069 ***	-0.064 ***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Dummy: 1= Switched Within Same Sch	-0.015 ***	-0.011 ***	-0.017 ***	-0.017 ***	-0.017 ***	-0.014 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.655	0.689	0.659	0.666	0.668	0.888
N	1626150	1626150	1626150	1626150	1626150	1626150
Fixed Effects?	--	Teacher	School	Sch x Grade	Sch x Year	Student

B. Assume All Teachers with Ambiguous Switch Status did *not* Switch Within School

	M1	M2	M3	M4	M5	M6
Dummy: 1= Switched from Other Sch	-0.046 ***	-0.027 ***	-0.049 ***	-0.049 ***	-0.052 ***	-0.043 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy: 1= New to Teaching/ NYC	-0.057 ***	-0.052 ***	-0.062 ***	-0.062 ***	-0.067 ***	-0.063 ***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Dummy: 1= Switched Within Same Sch	-0.016 ***	-0.012 ***	-0.016 ***	-0.015 ***	-0.015 ***	-0.015 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.655	0.689	0.659	0.666	0.668	0.888
N	1626150	1626150	1626150	1626150	1626150	1626150
Fixed Effects?	--	Teacher	School	Sch x Grade	Sch x Year	Student

All models shown here have time-varing and time-invariant student characteristics, aggregated time-varying classroom covariates, teacher time-invariant and time-varying characteristics, and school time-invariant and time-varying characteristics (except when collinear with the relevant fixed effects).

Appendix Table C2. Bounding Exercise for ELA.

A. Assume All Teachers with Ambiguous Switch Status *did* Switch Within School

	M1	M2	M3	M4	M5	M6
Dummy: 1= Switched from Other Sch	-0.017 ***	-0.009 **	-0.017 ***	-0.017 ***	-0.021 ***	-0.018 ***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy: 1= New to Teaching/ NYC	-0.036 ***	-0.030 ***	-0.036 ***	-0.037 ***	-0.037 ***	-0.036 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy: 1= Switched Within Same Sch	-0.003 *	-0.001	-0.003 *	-0.002	-0.006 ***	-0.005 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.585	0.604	0.587	0.591	0.594	0.846
N	1629781	1629781	1629781	1629781	1629781	1629781
Fixed Effects?	--	Teacher	School	Sch x Grade	Sch x Year	Student

B. Assume All Teachers with Ambiguous Switch Status did *not* Switch Within School

	M1	M2	M3	M4	M5	M6
Dummy: 1= Switched from Other Sch	-0.017 ***	-0.009 **	-0.018 ***	-0.018 ***	-0.022 ***	-0.019 ***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy: 1= New to Teaching/ NYC	-0.037 ***	-0.030 ***	-0.037 ***	-0.038 ***	-0.038 ***	-0.036 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy: 1= Switched Within Same Sch	-0.004 ***	-0.001	-0.004 ***	-0.004 ***	-0.006 ***	-0.006 ***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.585	0.604	0.587	0.591	0.594	0.846
N	1629781	1629781	1629781	1629781	1629781	1629781
Fixed Effects?	--	Teacher	School	Sch x Grade	Sch x Year	Student

All models shown here have time-varing and time-invariant student characteristics, aggregated time-varying classroom covariates, teacher time-invariant and time-varying characteristics, and school time-invariant and time-varying characteristics (except when collinear with the relevant fixed effects).

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