Worker Absence and Productivity: Evidence from Teaching

Mariesa A. Herrmann
Columbia University

Jonah E. Rockoff
Columbia Business School and NBER

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Abstract

A significant amount of work time is lost each year due to worker absence, but evidence on the productivity losses from absenteeism remains scant due to difficulties with identification. In this paper, we use uniquely detailed data on the timing, duration, and cause of absences among teachers to address many of the potential biases from the endogeneity of worker absence. Our analysis indicates that worker absences have large negative impacts: the expected loss in daily productivity from employing a temporary substitute is on par with replacing a regular worker of average productivity with one at the 10^{th}–20^{th} percentile of productivity. We also find daily productivity losses decline with the length of an absence spell, consistent with managers engaging in costly search for more productive substitutes and temporary workers learning on the job. While illness is a major cause of absenteeism among teachers, we find no evidence that poor health also causes lower on-the-job productivity.

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There is scant evidence on the productivity losses from worker absence, despite the fact that absenteeism results in an annual loss of two percent of work time in the U.S. (Bureau of Labor Statistics, 2008). Several highly regarded studies in economics have documented drops in productivity during labor disputes (Kleiner et al. (2002), Krueger and Mas (2004), and Mas (2008)), but labor disputes are rare—accounting for just one one-hundredth of a percent of lost work time—and it is unclear how these results generalize to more common sources of worker absence, such as illness or personal business.[1]

In this paper, we present evidence on the impact of absenteeism on productivity using detailed panel data on the timing, duration, and causes of absences among teachers and the gains in academic achievement made by their students.[2] We take advantage of this data in several ways to address the endogeneity of absenteeism. First, we base our identification on variation within teachers over time to avoid bias from the correlation of absenteeism with persistent differences in productivity across teachers. Indeed, the richness of our data allows us to identify the impact of absences using variation within the same teacher, school, and grade level. Second, we contrast estimates of the impact of absences that occur *prior* to student exams with those that occur *afterwards*; only the former can have a direct causal impact on our productivity measure. In these respects, our approach is similar to Mas and Moretti (2009); they evaluate peer effects among supermarket cashiers using variation in productivity within workers over time and

[1] In addition, labor disputes involve more than just the replacement of full-time employees with temporary workers and are likely to have important effects on employee morale and effort. For example, Krueger and Mas (2004), who study the production of Bridgestone/Firestone tires, find that defective tires were most likely to be produced during the period before a major strike (while regular workers were still on the job) and just before a new contract was settled (when striking employees worked alongside their replacements). Statistics on the frequency of labor disputes can be found in Bureau of Labor Statistics (2009).

[2] Economists have used student achievement data extensively to study productivity in teaching, with early studies by Hanushek (1971) and Murnane (1975) and recent work by Rockoff (2004), Rivkin et al. (2005), and Aaronson et al. (2007), among others. There is some debate around how student sorting affects the measurement of teacher productivity (see Kane and Staiger (2008), Rothstein (2010)). However, our identifying assumptions are much weaker than those needed to identify variation in quality between teachers, and we present direct evidence against our results being driven by student sorting.

exploiting the fact that peers can only directly affect co-workers' productivity after they arrive at work. We also use a number of specifications and robustness checks to confirm that our findings are not driven by teachers taking more absences when they are assigned more difficult students, or by correlations between teacher absenteeism and student absenteeism or misbehavior.

Reductions in productivity associated with worker absence in teaching are statistically and economically significant. These negative effects occur for absences prior to student exams but not afterwards, supporting a causal interpretation. Our baseline estimates imply that the average difference in *daily* productivity between regular teachers and temporary substitutes is equivalent to replacing a teacher of average productivity with one at the 10th percentile for math instruction or the 20th percentile for English instruction.[3] We also find that productivity losses from absenteeism are greater for more experienced teachers, consistent with evidence from various studies that experienced teachers are more productive.

In addition, we provide evidence that daily losses in productivity from worker absence are decreasing in absence duration. There are several reasons why this might be so. For example, managers may engage in costly search in order to hire more productive substitute workers for longer assignments, temporary workers may learn on the job, and the supply of more productive substitutes may be greater for longer job assignments. Our estimates suggest that the daily productivity loss when a substitute is used for a single day is even greater than replacing an average teacher with one at the 1st percentile in math and equivalent to replacing an average teacher with one at the 3rd percentile in English. In other words, extremely little production

[3] Ours is not the first paper to estimate a negative impact of teacher absence on student achievement, but it is the first to examine variation in absence duration or cause, and the first to exploit the timing of absences relative to student exams. Miller et al. (2008) and Clotfelter et al. (2009) estimate the average effect of teacher absence on student achievement using a teacher fixed effects approach. Duflo and Hanna (2005) document the negative impact of teacher absences on student achievement using a randomized control trial in rural India, where substitutes are not used to replace absent teachers.

appears to take place when a teacher is absent for a single day, despite the presence of a paid temporary substitute. In contrast, the average daily productivity loss from replacing regular teachers with "long-term" substitutes is equivalent to replacing a teacher of average productivity with one at the 19th percentile in math and the 20th percentile in English.

We also investigate variation in the effects of absences with different causes. Indeed, one concern for our analysis is that shocks to worker health may lower productivity at work in addition to increasing absenteeism. Despite a large literature on the impact of health on wages, earnings, labor force participation, and education (Currie and Madrian (1999), Smith (1999), Currie (2009)), there is little research on the impact of poor health on productivity at work—what social psychologists have labeled "presenteeism."[4] If worker health shocks directly affect productivity on the job, we might expect to see outsized impacts of absences that are related to serious health conditions. However, we find that health and non-health related absences have very similar negative effects on productivity.

Last, but not least, we examine the importance of absence timing by focusing on the periods just prior to and during student examinations. We find productivity losses for absences during periods well before exams, but larger impacts for absences in the weeks and days leading up to exams. Furthermore, impacts are an order of magnitude greater for absences on the day(s) students are tested, which we show is likely mediated by the testing environment, rather than cheating. This analysis indicates that the importance of labor productivity for specific output measures can vary considerably over the production cycle. In the production of education, actions taken by teachers just prior to and during exams can have outsized effects on measured student achievement.

[4] The literature in social psychology examines cross-sectional variation in self-reported measures of health and productivity (e.g., Goetzel et al. (2004), Pauly et al. (2008)). In addition, some development economists have studied health and productivity of agricultural laborers (Strauss and Thomas (1998)).

The paper proceeds as follows. In Section 2 we provide a conceptual framework to motivate our empirical work. In Section 3 we describe the data, and in Section 4 we present our main empirical estimates, robustness checks, and extensions. Section 5 offers some conclusions and discusses the extent to which our findings might generalize to other contexts.

2. Conceptual Framework

We briefly present a conceptual framework that provides empirical predictions and highlights important issues for our analysis. Consider the productivity of a representative worker *r* on a specific day *t* (q_{rt}) as the sum of ability, work experience, and a stochastic daily component. In Equation 1, we write total production over days indexed from 1 to *T* as a function of daily labor productivity for the representative worker *r*, the productivity of substitute *s* that replaces the regular worker when absent, and other production inputs (*X*).

$$(1)\ Q_T = f_T(q_{j1}, q_{j2} \ldots, q_{jT}, X),\ \ j = \begin{cases} r \text{ if present} \\ s \text{ if absent} \end{cases}$$

By assumption, production increases with labor productivity on any day. If expected productivity is lower for substitute workers than regular workers, increases in absenteeism should lower production. Also production losses from absenteeism will be greater for more productive regular workers, all else equal.

In addition, we posit that the expected *average* productivity of a substitute worker is increasing in the length of the substitute's work assignment. There are several reasons to expect the skill level (ability or experience) of substitutes to be greater for longer jobs: managers searching for better workers or allocating the best available workers to longer assignments, more highly skilled workers willing to take a longer assignment (see Gershenson, 2011), or workers learning on the job. If substitute productivity rises with job assignment length, then for any two spells of lengths M and N days, $M>N$, the expected loss from the M day spell should be less than M/N times the loss from the N day spell. We test this hypothesis explicitly in Section 4.

Of course, regular workers will choose when to be absent when the benefits (e.g., leisure) outweigh the costs (e.g., lower pay), and this complicates identification in a regression of productivity measured over a given period on the number of worker absences. We consider the net benefits of absence on any given day as determined by three factors: (1) worker-specific factors that do not vary over time (e.g., tastes for leisure), (2) job characteristics (including salary) which may change over time, and (3) a stochastic daily component (e.g., health) which may persist over time.

Even if substitute workers were, in expectation, equally productive as the workers they replace, one might find a spurious relationship between absenteeism and production. For example, more able workers may also derive greater enjoyment from time spent at work, creating a correlation between the value of leisure and ability, both of which are typically unobservable. To address this concern, one can compare production for the same worker across time, and examine how production varies with absenteeism.

A thornier empirical problem is that time-varying elements of productivity and the net benefits of absence may be correlated. For example, changes in production inputs will affect productivity and may also make a job less pleasant, causing workers to show up less often. A similar problem would arise if workers experience persistent negative health shocks and are less productive on the job, in addition to taking more time off from work. To address this issue, one could limit comparisons not only to the same worker over time but also to periods in which absences varied but other factors were held constant. However, there may still be bias due to factors which cannot be directly observed.[5]

To gauge the importance of a number of sources of bias, one can use a placebo test based on the idea that a worker's production over a given time period cannot be directly related to her future absences. Taking any factor that lowers productivity, makes absenteeism more attractive, and is constant within workers over a set of days *1* to *T*, we can see that, conditional on the number of absences between day *1* and day *T-K*, the unobservable factor will create a correlation between productivity during days *1* to *T-K and* increase absences during days *T-K+1* to *T*. Thus, a relationship between current productivity and future absenteeism would be evidence of bias: we should observe no relationship between productivity measures and subsequent absenteeism if the link between productivity and absenteeism is causal.

Passing such a placebo test is, of course, not proof of causality. Unobservable factors that are imperfectly correlated across the periods from day 1 to *T-K* and day *T-K+1* to *T* will still hold the potential for bias. While addressing all potential sources of bias in a non-experimental (or quasi-experimental) setting is quite difficult, one can assess the importance of many potential biases using detailed data. For example, one issue is that temporary negative health shocks may cause workers to take more time off and be less productive on the job. To test for this source of bias, one could compare the productivity effects of health-related absenteeism to the effects of absences for reasons such as personal business, vacation, or jury duty. If the health bias exists, one would expect health related absences to appear more detrimental to productivity.

3. Data and Descriptive Statistics

[5] One way to address the issue of unobservable factors is to use an instrumental variable for absenteeism. In developing countries, economists have implemented field experiments which randomized introduction of financial bonuses for work attendance (Kremer and Chen (2001), Duflo and Hanna (2005)). We lack such experimental variation. We discuss one potential instrumental variable (inclement weather and commuting distance) in Section 4, but we find it has little power to predict absences in our setting. We therefore rely on other empirical strategies.

Our data come from New York City, the largest school district in the U.S., and cover the school years 1999-2000 through 2008-2009. We focus on teachers of math and English in grades 4 to 8, who can be linked to students for whom we generally have math and English test scores in both the current and previous year. Students in elementary grades (4, 5, and some in grade 6) typically have the same teacher for both subjects, while older students are taught by two different teachers.[6] Over this period, the timing of exams ranged from early March to mid-May for math and from early January to mid-May for English (Appendix Table 1). Exam periods lasted from one to three days, followed by a five-day make-up exam period for students absent during all or part of the regular exam.

In addition to math and English test scores, we have information on students' absences, suspensions, demographics, and receipt of free/reduced price lunch (a measure of poverty), special education for disabled students, and English Language Learner services.[7] Data on teachers' demographics, graduate education, and experience were obtained from payroll records.

We have records of the date and reason given for all daily teacher absences over this time period. The rules governing teacher absences are set forth in a collectively bargained contract between the teachers union (the United Federation of Teachers) and the school district. Teachers earn ten days of paid absence per school year (one per month). However, teachers accumulate unused absences, up to a cap of 200 days, and are paid 1/400th of their most recent salary for

[6] Students in grade 6 are taught by the same teacher in schools whose terminal grade is 6. Student-teacher links were unavailable in some schools at the start of our sample, and we only include students in school-year cells for which we match greater than 75 percent of students with teachers. Over this period, students with disabilities were typically taught in separate classrooms or schools and did not take the same standardized tests as general education students. We therefore exclude all classrooms where the portion of special education students exceeded 25 percent. We also exclude a few classrooms with less than 7 or greater than 45 students, where the teacher switches schools during the year, or where the teacher was not on active duty for more than half the year or until after the exam.

[7] We unfortunately lack daily information on student absences; we only know each student's total absences for the school year. Thus, we are unable to estimate a placebo test for whether students are affected by the absence of their regular teacher on days when they themselves do not show up at school. We leave this line of inquiry to future work. While we can test if teacher absences have smaller effects on students who themselves are absent more often, the correlation of student absenteeism with other characteristics would make the interpretation of such a test unclear.

each unused absence when they retire. Thus, using "paid" absences poses a real financial cost for teachers unless they are certain to reach the 200 day cap.[8] These rules allow teachers to use up to ten absences each school year for "Self Treated Sickness" – sick days which do not require proof of illness from a physician – or "Personal Days." Teachers can take only three "Personal Days" each year, but there is no barrier to a teacher labeling an absence for personal business as "Self-treated Sickness."[9] Absences for "Medically Certified Sickness" (i.e., illness certified by a physician) and several other types of absences (Conferences/School Activities, Funeral/Death in Family, Jury Duty/Military Service, Injury, Graduation Attendance, Religious Holiday, and Grace Period) do not count towards the ten day cap.[10] A few absences are Unauthorized.

We also have data on the type, timing, and duration of extended work leaves and job separations, which we classify into 11 categories: Maternity Leave, Child Care Leave, Medical Leave, Sick Family Member Leave, Personal Leave, Sabbatical, Resignation or Retirement, Involuntary Termination, Certification Termination, Death, and Other (e.g., unauthorized leave, military deployment, and leave without pay for various reasons such as working in a charter school).[11] Rules governing extended leaves are also set forth in the union contract, in accordance

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[8] This constraint is unlikely to bind for the vast majority of teachers. Among all teachers in New York (not just those teaching math and English in grades 4-8) hired in the school year 1999-2000, more than two thirds left teaching in the district by the end of our ten year sample, and only three percent of remaining teachers (1 percent of the cohort) used absences at a rate low enough to reach 200 in 25 years (i.e., 20 absences or less in over ten years).

[9] The notion that absences for Self Treated Sickness are likely to include many absences not related illness is supported by absence rates across days of the week. It is reasonable to believe that absences taken for personal reasons would be more prevalent on Mondays and Fridays, providing workers with a long weekend, and rates of absence for Self-treated Sickness and Personal Days are both nearly 50 percent higher on Mondays and Fridays than on Tuesdays through Thursdays. In contrast, absence rates on Tuesdays through Thursdays are nearly identical to rates for Mondays and Fridays if we examine illnesses certified by a doctor. Variation in absence by day of the week is not a new finding. High absence rates on Mondays have been found in studies of absence which go back many decades (e.g., Bezanson et al., 1922), and absences on Fridays are low in manufacturing jobs where workers are paid in person at the end of each week. In our setting, teachers' paychecks are mailed or directly deposited.

[10] "Grace period" typically applies to teachers who are absent prior to an extended leave (e.g., maternity). These teachers have exhausted their paid absences and are not paid, and grace period is capped at 30 days.

[11] Certification Termination refers to termination of teachers who lacked required credentials; these occur primarily just before the school year 2003-2004, when state requirements were strictly enforced after a legal battle between New York City and New York State.

with applicable laws such as the Family and Medical Leave Act. Note that these events can impact students when they end as well as when they begin (e.g., women beginning their maternity leave in the summer may return several weeks or months after the school year starts).[12]

Table 1 shows summary statistics on the frequency and duration of spells of absence, including extended leaves and job separations. Duration is defined by the number of instructional days (i.e., work days) missed, not calendar days, though the two are highly correlated. Teachers are absent 10 days on average, or roughly 5 percent of the school year.[13] Perfect attendance by a teacher occurs in only three percent of cases.

"Self-treated Sickness" accounts for a large portion of all days missed, more than four days per teacher per year on average, while Medically Certified Sickness and Conferences/School Activities account for two days and one day, respectively, per teacher per year. The extended leave that accounts for the most days missed is Medical Leave, which is taken by just over one percent of teachers per year but has an average duration of almost 43 instructional days. Other types of extended leave are even less common but have similarly long

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[12] In about 10 percent of cases, leaves are consecutive (e.g., maternity leave can turn into child care leave), and we aggregate these into a single leave, using the initial leave to classify the sequence. If daily absences are followed immediately by an extended leave (e.g., medical leaves are often preceded by absences for "Medically Certified Sickness"), we group these together and classify the spell by the extended leave of absence. In some cases, consecutive daily absences are not all labeled with the same code. In these instances, we label all absences in the spell under a single code, giving priority to more specific causes, in the following order: Injury, Medically Certified Sickness, Funeral/Death in Family, Jury Duty/Military Service, Religious Holiday, Graduation Attendance, Conferences/School Activities, Personal Day, Self-treated Sickness, Grace Period, and Unauthorized.

[13] Rates of absence for representative samples of U.S. workers are available from the Current Population Survey, which asks about time missed from full-time work during a particular week. Rates of absence were roughly 4 percent in the public sector (3 percent in the private sector) over the time period we analyze. Although this is somewhat lower than the 5 percent rate in our sample of teachers, CPS rates exclude vacation and personal days, while a non-trivial fraction of teachers' "Self Treat Sickness" absences are likely taken for personal matters. Comparable data on spells and spell length are not available in the CPS but are reported in two studies that use daily data on employee absences spanning a long time period. Ichino and Moretti (2008) report that employee absences for sickness in a large Italian bank last an average of 3.8 days; our figure for Medically Certified Sickness and Medical Leaves is 3.0 days. Barmby et al. (1991) examine data from a British manufacturing firm in the late 1980s and report mean absence spells of 5 days; like our data, spell length is skewed, with spells of 5 days or less in duration accounting for over 80 percent of spells but 40-45 percent of work days missed. In our data, spells of 5 days or less account for 98 percent of all spells and 78 percent of work days missed. These (admittedly limited) comparisons suggest that teachers' spells of absence may tend to be short relative to other sectors and occupations.

durations (e.g., maternity leave is taken by 0.5 percent of teachers and has an average duration of 48.6 days).[14]

In Table 2 we show the mean and standard deviation of absences for the math and English teachers in our sample, both over the entire school year and broken out by timing: prior to exams, after exams, during the exam period, and during the make-up exam period.[15] As one might guess from the statistics presented in Table 1, the distribution of absences is right-skewed, and the standard deviation of total absences (roughly 10) is quite close to the mean. We also present standard deviations of residuals from regressions of teacher absences on teacher-school-grade fixed effects. These "within-teacher" measures are about 65 percent as large as the standard deviation based on both "between" and "within" variation. This implies that almost half of the variance in absences among teachers occurs within teachers across years, providing us considerable identifying variation.[16]

Before proceeding to our main analysis, we examine associations between absence frequency and the characteristics of students and teachers using negative binomial regressions. We find a marginally significant coefficient on students' prior math test scores, suggesting that teacher absence—if costly to students—may contribute slightly to inequality in educational outcomes (Table 3a). There is no significant relationship between work days missed and free lunch receipt (our measure of poverty), special education services, or English language learner

[14] To better understand teachers' potential control over the timing of extended leaves, we have examined the percentages of each type of event that begin or end during the middle of the school year. Maternity and Medical Leaves—where we do not expect much control over timing—result in missed work days 90 and 93 percent of the time, respectively, while Personal and Other Leaves—where timing may be partially under teachers' control—only result in missed work days 20 and 30 percent of the time, respectively.

[15] The unit of observation for these tables is a teacher-grade-year cell. We allow for multiple observations of teachers of multiple grades in the same year since the exam dates differ across grade levels.

[16] The within-teacher correlation in total days missed across years is just 0.18 in our sample, and there are very few teachers who do not contribute to identification. Less than 10 percent of teachers observed in adjacent years did not experience a change in their number of absences; less than 2 percent of teachers observed to three consecutive years did not experience a change their number of absences.

services, but we find that teachers of Hispanic students miss fewer days, relative to teachers of white students. We do find that missed work days are positively related to student absences, though the coefficient is fairly small.[17] Results from negative binomial regressions of work days missed on a set of teacher characteristics are shown in Table 3b. Having a graduate degree is associated with fewer work days missed, as is having few years of teaching experience. Younger female teachers miss more days of work relative to teachers of different gender and age categories, and black and Asian teachers miss fewer days relative to white teachers.

Data on the individuals working as substitute teachers in New York City is unfortunately unavailable, but we can compare their employment requirements and wages to those of regular teachers.[18] Substitutes in New York do not need to pass state certification requirements (i.e., possess a degree in education and pass a series of exams) but, like regular teachers, must have a bachelor's degree and must pass a criminal background check.[19] If a substitute teacher works for more than 40 days during the school year, they must have certification or complete additional certification coursework before the start of the following school year. Substitute teachers are currently paid just over $150 per day of work, or about $21 per hour given the length of a typical school day and about half of what regular teachers earn for additional hours of work (over $40).

Another important source of substitute teachers in New York City is the Absent Teacher Reserve (ATR), which consists of certified teachers who lost their jobs due to grade reconfiguration, reduction in student enrollment, programmatic change, or phase out or closing

[17] One likely explanation for this finding is correlation between teacher and student illness. Since this could generate a spurious correlation of teacher absences with student achievement, we estimate specifications that control for students' current absences as robustness checks.
[18] National statistics on substitute teachers are also unavailable; the Bureau of Labor Statistics groups substitutes with other jobs (e.g., tutor, academic advisor) in the category "Teachers and Instructors, All Other."
[19] Requirements are similar in other parts of the U.S., though Henderson et al. (2002) report that 19 states do not require a bachelor's degree.

of their school.[20] ATR teachers have been unable to find another job, but, in accordance with the union contract, the school district pays their full salary and they work as substitute teachers, either on a per-diem or long-term basis. Individual schools using ATR teachers as substitutes pay 50 percent of the daily wage to the school district, and thus have a financial incentive relative to using other substitute teachers. A consistent series of statistics on the size of the ATR is unavailable, but recent reports put the number at around 500 teachers. Thus, with absence rates of roughly 5 percent and a teaching population of roughly 75,000, ATR teachers likely cover about 10 to 15 percent of substitute teacher assignments.

4. Regression Specifications and Empirical Estimates

We begin by estimating a regression specification of the following form:

$$(2)\ \ Y_{ijkgst} = \delta A_{jt} + \beta X_{it} + \mu Z_{kt} + \lambda W_{jt} + \rho V_{sgt} + \pi_{gt} + \varepsilon_{ijkgst}$$

where Y_{ijgst} is the exam score of student *i*, taught by teacher *j* in classroom *k*, grade *g*, school *s*, and year *t*. A_{jt} is the number of work day absences for the student's teacher, X_{it}, Z_{kt}, and W_{jt} are vectors of, respectively, student, class, and teacher characteristics, V_{sgt} is a vector of school-grade-year characteristics, π_{gt} is a grade-year fixed effect, and ε_{ijkgst} is an error term.[21] Standard errors are clustered at the school level, which produces more conservative estimates relative to clustering at the classroom or teacher. Estimates from this specification suggest that an additional day of work missed by a regular teacher is associated with a decrease in student test

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[21] Student characteristics include a cubic polynomial in prior year math and English scores, the number of absences and suspensions in the previous year, and indicators for gender, race and ethnicity, free/reduced price lunch, special education, and English Language Learner. We also interact all of these variables with the student's grade level. Teacher characteristics include indicators for the number of years of teaching experience (1, 2, 3, 4, 5, 6, 7+), gender, race, and possession of a graduate degree. School-grade-year and classroom characteristics include averages of student characteristics and class size.

scores of 0.0017 and 0.0006 standard deviations in math and English, respectively (Table 4, Columns 1 and 5).

Our conceptual framework motivates the concern that teachers who frequently miss work also provide lower quality instruction while on the job. We employ two strategies to address this issue. First, we separate absences by their timing—before, during, or after student exams. Since absences after exams cannot have a direct causal relationship with student exam performance, any observed relationship must be due to endogeneity.[22] When we allow the coefficient on work days missed to differ by their timing relative to student exams (Table 4, Columns 2 and 6), we find much larger negative effects prior to the exam than afterwards.[23] The estimated effect of absences prior to the exam is four to five times greater than absences after the exam, though absences after the exam are marginally significant, suggesting some bias in our estimates.

We then include teacher-school-grade fixed effects (π_{jsg} in the notation of Equation 2). When we control for these time-invariant dimensions of instructional quality (Table 4, Columns 3 and 7), the effects of absences prior to the exam become smaller (-0.0012 and -0.0006 standard deviations for math and English, respectively) but remain highly significant, while estimates for absences after the exam are statistically insignificant in addition to being quite small (roughly -0.0001 standard deviations in both subjects). These results are in line with a negative causal impact on productivity of replacing a regular teacher with a temporary substitute. They also

[22] A more direct solution to the endogeneity problem would be an instrumental variables approach. We explored this using an instrument suggested by Miller et al. (2008), the interaction of bad weather with a teacher's commuting distance. Unfortunately, the instrument does not have a statistically significant first stage. Although living more than ten miles away from work has significant power to predict absences on the *actual days* of extreme winter weather, it has no power to predict teachers' total absences prior to exams. This suggests that teachers who have a long commute do miss work due in bad weather but "make up" that day some other time. Equivalently, teachers who live close to work and show up in bad weather may "make up" for it by taking a day off some other time. Using different distance cutoffs (e.g., less than 5 miles or less than 15 miles) does not change these results.

[23] The coefficient estimates on absences during the regular exam and make-up exam periods are also negative and statistically significant. We focus on these results in greater detail in Section 4.3.

indicate that absences are negatively correlated with the time invariant dimensions of instructional quality captured by the teacher-school-grade fixed effects.

We test the robustness of these baseline estimates in several ways. First, we drop prior test scores from our control variables (X_{it} and Z_{kt}) and put students' prior test scores as the dependent variable in our regression. In other words, we test whether teachers are absent more often in years when they are assigned students with lower prior test scores. Such a relationship would raise the concern that student sorting might bias our estimates of the impact of absences. However, we find no significant relationship between absences prior the exam and students' prior test scores (Table 4, Columns 4 and 8), in contrast to our baseline results.

As an additional robustness check, we take advantage of the fact that over 90 percent of middle school students in New York City take math and English with the same classmates, even though they have different teachers in each subject. If student composition caused achievement to fall and teacher absences to rise, we might expect the absences of math teachers prior to the English exam to be correlated with English achievement, and vice versa.[24] In fact, *if we omit teacher-school-grade fixed effects*, there is indeed a significant coefficient (-0.00031 standard deviations) for the "effect" of English teachers' absences prior to the math exam on math achievement (Table 5, Column 1). However, once the fixed effects are included, this estimate becomes much smaller (-0.00007 standard deviations) and insignificant (Table 5, Column 2). Math teachers' absences prior to the English exam bear no relation to English achievement, regardless of the omission or inclusion of fixed effects (Table 5, Columns 3 and 4).[25]

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[24] This result is also evidence against our results being driven by the correlation between teacher and student absences shown in Table 3a. If student absences and teacher absences were related due to illness, we would expect to find effects of English teacher absences on math test scores and vice versa.

[25] The estimate for English teachers' absences on English test scores is smaller here than in our baseline estimates because our sample is limited to middle school. While the point estimates from our baseline specification are larger for elementary grades (-0.08) than middle school (-0.03), we cannot reject that they are the same with a high degree of confidence. For math, estimates for elementary and middle school grades are quite similar to one another (-0.12).

In further support of the idea that we are estimating causal effects, we have also examined whether our estimates are sensitive to the inclusion of control variables for student absences and suspensions in the *current* school year. Teacher illness could (causally) lead to student illness (and lower achievement), or vice versa, generating a spurious correlation of absences with achievement. Students might also misbehave if they think their teacher will be going away on an extended leave. However, including these control variables has no noticeable impact on our estimates, although students' own absences and suspensions are both negatively related to their level of achievement. These results are available upon request.

Having established a strong case for a causal effect of absences on productivity, it is helpful to consider the magnitude of these effects. We present a back-of-the-envelope calculation to give a better sense of the magnitude of the *daily* productivity loss from having to replace an absent teacher with a temporary substitute. To do so, we make the simplifying assumption that annual productivity differences across teachers—which are well documented by economists—are driven by a linear accumulation of differences in daily productivity. This assumption allows us to estimate the average annual productivity difference between regular teachers and substitutes by summing the daily difference in productivity (-0.0012 standard deviations in math test scores) over the roughly 130 instructional days prior to the math exam. Doing so, we arrive at a reduction in math scores of -0.156 standard deviations. We can then compare this effect to the impact of replacing a regular teacher of average productivity with one of lower productivity for the entire school year. Given estimates in the literature, one would have to replace an average teacher with one at the 10th percentile of the teacher productivity distribution to get a similar reduction in math scores. In English, our estimated coefficient on absences (-0.0006 standard deviations) together with a pre-exam period of 110 instructional days

(English exams were typically given prior to math exams) suggest that replacing a regular teacher with a substitute is, on average, equivalent to replacing an average teacher with one at the 20th percentile.[26] Thus, our analysis suggests that temporary replacements have drastically lower productivity than regular full-time teachers.[27]

4.1 Heterogeneity in Productivity Losses

Our baseline estimates and robustness checks strongly support the notion that productivity in teaching is significantly lower on days when regular teachers are replaced with temporary substitutes. However, it is reasonable to think that the impact of absences may be heterogeneous. Productivity losses may be greater for absences of highly productive teachers, or, alternatively, these teachers may help substitutes provide effective instruction by developing easy-to-use lesson plans. While we cannot observe productivity directly, several studies find that teacher productivity rises quickly over the first few years of their careers (Rockoff (2004), Rivkin et al. (2005), Kane et al. (2008)). We therefore estimate regressions that allow the impact of teacher absences to differ by whether teachers had less than three years or three or more years of prior teaching experience.

We find evidence that absences by experienced teachers cause a greater reduction in student test scores than absences by inexperienced teachers (Table 6). The estimated difference

[26] To reach this estimate, we take the results from a study by Kane et al. (2008) of teachers in New York City, though their estimates are similar to other studies in this literature (see Hanushek and Rivkin (2010)). Kane et al. estimate that math test scores fall by -0.12 standard deviations for a one standard deviation decrease in teacher productivity. This implies that replacing an average teacher with one at the 10th percentile (1.3 standard deviations below the mean) would reduce scores by -0.156 standard deviations. Extrapolating our absence coefficient in English (-0.0006) over 110 instructional days implies a reduction in test scores of -0.066 standard deviations. Kane et al. (2008) find students' English test scores fall by -0.08 standard deviations for a one standard deviation decrease in teacher productivity. Given this estimate, to reduce scores by -0.066 standard deviations one would need to replace an average teacher with one at the 20th percentile (0.82 standard deviations below the mean).

[27] Note that our results are not necessarily informative about what the productivity of individuals working as substitute teachers might be if they were employed full-time. This is analogous to how studies of labor unrest (e.g., Krueger and Mas, 2004) examine the productivity of replacement workers under the temporary conditions in which they are hired, not the productivity they these "scab" workers would have if they received the same training and support as regular employees.

in the impact of absence across the two groups of teachers is highly statistically significant in math and marginally significant in English (p-value 0.14). Although point estimates for the impact of absences on student achievement among inexperienced teachers are still negative, we can no longer reject that they are zero. This provides further support to the notion that the losses associated with the use of substitute teachers are caused by their relatively low productivity.

In addition to heterogeneity across teachers, the effect of absences may vary across schools and students. Schools may differ in their abilities to find good substitutes, and some may provide substitutes with high quality instructional materials to help reduce the impact of teacher absence. Additionally, Todd and Wolpin (2003) stress that students and parents may respond to lower instructional quality by shifting household resources towards education. We do not have measures of how responsive schools and students are to changes in teacher productivity, but it is not unreasonable to think that high performing schools and high performing students may be better equipped to deal with these issues. We therefore estimated regressions that allow the effect of work days missed to differ across (a) schools with average test scores below and above the citywide median and (b) students with prior test scores below and above the citywide median. In the latter case, since students will vary in prior achievement within classrooms, we also estimated specifications that included *classroom* fixed effects. We find that the negative effects of work days missed are similar across these groups of schools and students in both math and English. These results are available upon request.

As discussed in Section 2, several factors suggest that daily productivity losses may decline with the duration of a spell of worker absence. In teaching, this could be due to school principals engaging in costly search for better long-term substitutes, the labor supply decisions of more highly productive substitute teachers, or temporary substitutes learning on the job (e.g., learning children's names and learning styles). To test this hypothesis, we construct variables that allow us to estimate the daily productivity losses associated with absences of different durations: 1 day, 2-3 days, 4-5 days, 6-10 days, 11-30 days, and 31 days or more.[28]

The results are in line with our hypothesis that daily productivity losses are smaller for longer duration absences (Table 7). In math, the coefficients decline steadily as we move from single day spells of absence (-0.0036) to spells lasting 31 days or more (-0.0008). In English, the daily productivity loss from single day spells is again the largest in magnitude (-0.0017) and then drops off precipitously. The coefficient estimates in English rise slightly as we move to the longest durations, but we cannot reject that daily productivity losses are the same for all spells of duration two days or longer.

The variation in magnitude between the estimates for single day absences and those with long durations is economically important. To illustrate this point, we again use our back-of-the-envelope calculation, based on a comparison with variation in productivity across regular teachers. For absences lasting just a single day, our estimates suggest that the difference in daily productivity between substitutes and the regular teachers they replace is *greater* than the difference between the daily productivity of an average teacher and a teacher at the 1st percentile in math, and on par with the difference in daily productivity between an average teacher and one at the 3rd percentile in English. Put differently, it appears that very little educational production takes place when a regular teacher misses a single day of work. In contrast, the estimates for the

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[28] Let S_{itd} denote the number of spells of absence of duration d for teacher i in school year t, and define the number of work days missed during spells lasting d days as $A_{itd} = dS_{itd}$. For example, if a teacher has two five-day absence spells during the school year, then S_{it5} would equal 2 and A_{it5} would equal 10. Total work days missed over the school year (A_{it}) is the sum of the work days missed from spells of a particular duration over all possible durations (i.e., $A_{it} = A_{it1}+A_{it2}+\ldots+A_{itD}$). Our baseline estimating equation contains an implicit restriction that the daily productivity loss from worker absence is invariant to absence duration, and we relax this constraint and allow coefficients on work days missed to vary across several categories of duration. We report results on effects of absences prior to student exams; we do not find that absences after exams are related to student achievement, regardless of their duration. In cases where a spell of absence begins but does not end prior to an exam, the work days missed prior to the exam are grouped according the duration of the entire spell.

longest spells imply a difference in daily productivity equivalent to replacing an average teacher with one at the 19th percentile for math and the 20th percentile for English—still an important loss in productivity, but far less severe.

4.2 Health and Productivity at Work

In our baseline analysis, we restricted the impact of work days missed to be invariant with respect to the reason for the teacher's absence. In many cases, we believe this restriction is probably correct and, under a strict causal interpretation, is probably warranted: conditional on duration, the relative productivity of a substitute should be independent of whether the regular teacher is absent for, say, a funeral or a child's illness.[29] However, teachers may have health conditions that cause them to be less productive on the job, in addition to any impact of health on absence from work. This could potentially make health-related absences appear more detrimental to student achievement than non-health related absences; essentially, estimates of the impact of health-related absences could suffer from omitted variables bias.

To investigate this possibility, we separately examine absences by type, and ask whether absences that we are confident were due to health conditions—Medically Certified Sickness, Medical Leave, and Maternity Leave—have outsized effects relative to other absences.[30] We find no evidence that health related absences by teachers cause a greater loss in student achievement than other absences (see Table 8). When we estimate separate coefficients on the number of days missed prior to student exams, we actually find smaller point estimates for

[29] Whether the likelihood of absence was known in advance is outside the scope of our analysis, but it is reasonable to believe that predictable absences might enable teachers or administrators to prepare and therefore be less costly. While we do not have information on predictability in most cases, we have compared the impact of maternity leaves—which are clearly known in advance—to medical leaves—which may be sudden. We find very similar negative impacts of both types of leaves prior to exams and no significant impacts of either type after the exam, suggesting the negative impact of absenteeism in this setting does not derive solely from unpredictability.

[30] Absences for Self-treated Sickness may be related to health, but our results are not sensitive to including them in the non-health-related category or including them as a separate category all to themselves. Our results are also insensitive to placing absences for maternity leave with the "other" category.

health-related absences, particularly for math. However, one problem with this specification is that health related absences have longer durations, and our previous results suggest that this would cause them to appear less detrimental. When we allow the coefficients for health and other absences to differ by duration, we find they both have very similar magnitudes, and in no case can we reject that they are the same.

Thus, we find no evidence that teachers absent for serious health conditions are also less productive while at work. While our test for a link between health and on-the-job productivity is admittedly indirect, it is important to recognize that much of the existing literature on this issue—very little of it by economists—relies on cross-sectional variation and self-reported health and productivity measures.

4.3 Worker Absences and the Timing of Productivity Measurement

In the empirical results above, we focus on the significant negative impact of absences prior to student exams, and contrast them with small and insignificant estimates for absences after the exam period. However, the specifications from which these estimates were taken also included controls for teachers' absences during the time when students were actually taking exams. In Table 9, Columns 1 and 3, we redisplay the results from our baseline regressions, including the coefficients on the number of absences during the main exam period—which can last between one and three days—and the five-day make-up period which directly follows it. In both math and English, absences during the main exam period have significant negative impacts on achievement (-0.0244 and -0.0128 standard deviations) that are an order of magnitude greater than the estimated impact of absences in the pre-exam period (-0.0012 and -0.0006 standard deviations). The coefficient for absences during the make-up exam period is negative and significant in math, but in English is it positive, insignificant, and quite close to zero.[31]

The striking results on absences during the main exam period have several possible interpretations. Teachers may improve student performance on the day of the exam through purposeful and permissible actions, such as reminding students of test-taking strategies or making sure that all students understand exam instructions. Teachers might also take actions which are not permissible, such as overtly (or covertly) supplying students with correct answers. Instances of teacher cheating are well-documented (e.g., Jacob and Levitt (2003), New York Times (2010)), and substitute teachers—who typically proctor exams in a teacher's absence—might have little incentive to engage in this type of malfeasance.[32] Another plausible explanation is that students perform worse on high-stakes tests when their regular teacher is absent because of increased anxiety or discomfort. A meta-analysis of two dozen small-scale experimental studies on student familiarity with test examiners finds effect sizes on the order of 0.3 standard deviations (Fuchs and Fuchs (1986)), and there are also many studies demonstrating how anxiety in various forms can impact test performance (e.g., Steele and Aronson (1995)). Finally, a recent experiment by Levitt et al. (2011) finds that students' effort on tests can be very

[31] The negative effect of make-up period absences in math but not English is somewhat puzzling. We speculate that the result is driven from differences between the testing schedule information and the actual dates students were tested in math. Over this period, New York City was permitted to test students within a short window (usually 3 to 5 days) set forth by the state. If some math tests were administered after the originally scheduled date, then a much larger fraction of students may have been tested during what we classify as the make-up period. For example, we discovered that during the school year 2008-2009, extreme winter weather caused the DOE to cancel classes on March 2, 2009 and postpone the start of 3rd, 4th, and 5th grade math exams (New York Times, March 3, 2009).

[32] To gauge whether cheating could explain our findings, we use results from Jacob and Levitt (2003), who estimate that roughly 5 percent of teachers cheat and that cheating increases scores by 0.5 standard deviations (10 additional standard score points on the Iowa Test of Basic Skills) on average. If the probability of absence during the exam is independent of a teacher's intention to cheat, we could expect a coefficient of -0.025. This is larger than our estimate for English (-0.0128) but quite close to our estimate for math (-0.0244). However, if teachers who care enough about scores to risk cheating also care enough to show up at work while ill, then teachers absent on the test day would be more honest than average, and these estimates likely overstate the impact of cheating.

sensitive to small short-term incentives, and it is possible that students exert less effort when the test is administered by a temporary substitute.

Looking at absence frequency, we find some indication that teachers do not wish to be absent on the day of the exam and shift work absences in order to do so; absence rates average 5.8% on days before exams, 5.3% on days after exams, 2.6% on days during the exam, and 8.5% on days during the make-up period. This also raises the possibility that a teacher's absence at so crucial a moment in the school year is a signal about her productivity on the job. To address this issue, we look at teachers in grades 4 and 5, who provide instruction in both subjects, and repeat our regressions while controlling for a student's *current* score in the *other* subject. In other words, we ask whether students score relatively worse in math (or English) when a teacher is absent for the math (or English) exam. Our original findings are quite robust to this much more stringent test, suggesting that, whatever the interpretation, a teacher's absence during high stakes exams has an important negative causal effect on exam performance.

In addition to the effect teachers have on student performance on the day of the test, it is often noted anecdotally that teachers engage in test preparation activities in the days and weeks prior to the exam. For example, they might focus on the material and types of questions most likely to be on the exam. We investigate this by allowing for different impacts of absences occurring 1-5 instructional days, 6-20 instructional days, and at least 21 instructional days prior to the exam. Though all absences have negative effects, we find clear evidence that absences in the weeks and days leading up to exams have greater impacts on exam performance than those occurring earlier in the year (Table 9, Columns 2 and 4). For math, the coefficient estimate for absences 21+ days prior is -0.00096 standard deviations, similar to our baseline, but for absences 6-20 days and 1-5 days prior, the point estimates are, respectively, double (-0.00185) and nine times (-0.0085) as large. The relative magnitude of the coefficients in English are similar, suggesting that actions taken by regular teachers just before exams are more important for exam performance than those taken earlier in the year.[33]

4.4 Persistent Effects of Absence

It is natural to ask whether the impact of teacher absences on students' test scores persists into the following school year. Recent studies of teacher productivity have documented that teachers' effects on scores one year later are between 20 and 50 percent as large as their effects on current test scores (Kane and Staiger (2008) and Jacob et al. (2008)).[34] Another motivation to examine persistence is the possibility that the outsized effects of absences close to the exam period reflect "teaching to the test" or that the impact of absences during the exam reflect cheating or effects on the test taking environment, rather than changes in students' knowledge of the content being tested. If so, then we should see lower persistence in the effects of absences that occur just prior to and during student exams.

Because we lack data on students in grade 9 or those that leave the school district, we first show that our baseline results are similar when we drop all students in grade 8 and students for whom we do not observe test scores the following year (Table 10, Columns 1 and 5). When we replace the current year's exam with the following year's exam as the dependent variable, we find that the negative impact of absences prior to the exam exhibit a similar level of fade-out as in previous studies (Columns 2 and 6). For math, the coefficient on work days missed prior to exams in the following year is about 35 percent of the coefficient in the current year; for English,

[33] Allowing for separate coefficients for absences close to the exam does dampen the estimated effects of absence during the exam period, but these coefficients (-0.0162 for math and -0.0081 for English) remain quite large and statistically significant.

[34] The issue of "fade-out" has been raised for other educational interventions, though it may be caused by differences in future resources or belie improvements in other outcomes (see Currie and Thomas (1995), Garces et al. (2002), Chetty et al. (2010)). Lang (2010) makes the point that rescaling of annual tests to have a mean of zero and standard deviation one could also lead to a perception that the effects of educational interventions fade out.

the fade-out is similar, with following year effects roughly 45 percent of current effects. The coefficient on following year test scores is significant at conventional levels in math and marginally significant in English (p-value 0.11).[35]

Importantly, the impact of absences during the exam period exhibits much greater fadeout. The coefficient on following year math scores is only about 10 percent as large as the coefficient on current year scores and is not statistically significant. In English, the coefficient on following year scores is *positive*, albeit not statistically significant. This suggests that, whatever is driving the outsized effects of absence during the exam on current year scores (e.g., cheating, test anxiety), it likely does not reflect real differences in student content knowledge. Also, it is worth noting that the coefficients on absences *after* exams, while never statistically significant, suggest larger negative effects on following year test scores than current year scores, in line with our causal interpretation.

If we break out absences prior to exams by timing (i.e., more than 20 days prior, 6-20 days prior, and 1-5 days prior), we find suggestive evidence that the effects of absences closest to the exams fade out most quickly. Most of the coefficients on following year scores are between 25 and 65 percent of the magnitudes for current year scores. However, for math scores, absences 1-5 days prior to the exam show an unusually large amount of fade-out, with a coefficient in the following year that is only 6 percent of the current year coefficient (Table 10, Column 4). This provides some indication, though far from conclusive, that a significant portion of teachers engage in "test prep" activities just before an exam.

[35] If a student does poorly in the current year, it may trigger policies in the following year designed to remediate or improve their performance. In line with this idea, the coefficients grow slightly in magnitude and are somewhat more precisely estimated when we control for future policies (i.e., grade retention, special education, English language learner services, and assignment to a more experienced teacher). For example, the English coefficient grows from -0.027 percent of a standard deviation to -0.028 and is significant at the 8 percent level.

5. Conclusion

Worker absence is an important phenomenon across all countries, industries, and occupations. Among OECD nations, absence frequency is noticeably higher in northern European countries with generous national sick leave policies (e.g., Barmby et al. (2002), Bergendorff (2003)). Absenteeism is also a major concern in developing countries, particularly in the public sector where oversight may be very weak (Chaudhury et al. (2006)).

Despite its ubiquity, there is a paucity of empirical work which convincingly estimates the causal impact of absenteeism on labor productivity. The major hurdle in this line of research is addressing the endogeneity of work absence. To do so, we take advantage of extremely detailed data on the absences of teachers in New York City public schools. We present evidence that missed work days have an economically important negative impact on productivity in teaching. To be confident that our estimates are causal, we focus on variation within teachers over time and contrast the significant effects of absences occurring prior to exams with the lack of any effect for absences occurring afterwards. We find similar impacts of absences across different students and schools, but greater impacts for more experienced (and productive) teachers than for newly hired teachers.

Our estimates of daily productivity losses are smaller for longer spells of absence. This pattern is likely caused by several factors: managers searching for more productive substitutes on longer job assignments, more productive workers applying for longer job assignments, or substitute workers becoming more productive on the job. We also find very large negative effects of work absences just prior to and during student examinations, suggesting that actions taken by the teacher at certain crucial moments in the school year have outsized impacts on student exam performance. Finally, we find no evidence that teachers show up to work when they are too ill to be productive ("presenteeism" in the parlance of social psychologists), though an analysis based on direct observations of health and productivity on-the-job would be better suited to addressing this issue.

Our study focuses on absenteeism in a significant part of the U.S. economy and one which plays a key role in fostering growth (e.g., Mankiw et al. (1992), Hanushek and Woessman (2008)). However, it is natural to ask how the impact of absenteeism in education might generalize to other settings and which features of the educational process may or may not be shared by other industries. First, labor substitution may be more difficult in occupations like teaching that require skilled workers and involve personal relationships with clients (e.g., healthcare practitioners, social workers, marketing and sales managers, etc.). Second, employees may be more likely to be ill (and unproductive) while on the job when paid sick leave is not available, and rates of paid sick leave are somewhat higher for public school employees (90 percent) than for employees in private firms (70 percent).[36] Third, the production schedule in education is somewhat inflexible (e.g., classes are not be rescheduled) and losses from absenteeism cannot be addressed through overtime work (Ehrenberg (1970)) or flexible hours.

What can be done to limit the production losses from worker absenteeism? One possibility is to address the root cause of absences, such as negative shocks to worker health. Indeed, absence prevention is one of the main drivers of recent growth in employer sponsored "health promotion" programs (Linnan et al. (2008)), though the evidence on the impact of these programs on absenteeism is quite mixed (Aldana and Pronk (2001)).

[36] One problem with these statistics, taken from the Bureau of Labor Statistics Employee Benefits Survey, is that the presence of paid sick leave may not accurately reflect the financial incentives for work attendance. As we note above, teachers in New York City get paid when they are absent, but face financial costs because they are paid for unused absences upon retirement.

Alternatively, governments and firms could offer stronger incentives for workers to show up. Empirical evidence strongly suggests that financial incentives affect worker absence (e.g., Winkler (1980), Jacobson (1989), Ehrenberg et al. (1991), Barmby et al. (1991), Brown and Sessions (1996), and Lindeboom and Kerkhofs (2000)). However, only one study, a field experiment in rural India (Duflo and Hanna (2005)), presents clear evidence that incentives for workers to show up can raise productivity. Financial incentives for work attendance could, in principle, decrease productivity by inducing workers to show up while seriously ill. Though it is reasonable to think that workers would be less responsive to financial incentives when in poor health, this is ultimately an empirical question.

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Table 1: Summary Statistics for Spells of Teacher Absence

	Avg. Days Missed per Teacher-Year	Average Spell Duration	Teacher-Year Observations with 1+ Spells (%)	Total Spell Frequency
Total of All Types	9.98	1.56	96.9%	622,843
Self-Treated Sickness	4.23	1.12	90.6%	370,207
Medically Certified Sickness	2.02	2.39	41.2%	82,482
Conference/School Activities	1.12	1.30	32.8%	84,331
Medical Leave	0.54	42.78	1.3%	1,241
Personal Days	0.47	1.32	26.3%	34,476
Funeral/Death in Family	0.33	2.24	12.9%	14,212
Jury Duty/Military Service	0.26	2.07	9.8%	12,334
Maternity Leave	0.25	48.63	0.5%	506
Child Care Leave	0.13	47.95	0.3%	274
Injury	0.11	3.82	2.4%	2,910
Resignation or Retirement	0.11	42.56	0.3%	249
Religious Holiday	0.10	1.17	8.0%	8,226
Graduation	0.10	1.36	4.0%	7,217
Other Leave	0.06	44.82	0.1%	134
Legislative Hearing	0.04	1.32	1.4%	2,595
Grace Period	0.02	11.79	0.2%	161
Personal Leave	0.02	25.56	0.1%	75
Sick Family Member Leave	0.02	31.38	0.1%	77
Death	0.01	37.67	0.0%	15
Termination, Certification	0.01	34.15	0.0%	33
Involuntary Termination	0.01	40.75	0.0%	12
Unauthorized	0.01	1.55	0.4%	741
Late More than Half Day	0.00	1.01	0.3%	335

Note: Based on teachers in New York City teaching math and/or English to students in grades 4-8 during the school years 1999-2000 to 2008-2009. Additional information on sample restrictions is provided in the text.

Table 2: Between and Within Variation in Teacher Absences

	Math Teachers	English Teachers
Total Absences		
Mean	9.81	10.09
Standard Deviation	9.76	9.94
Within-Teacher S.D.	6.35	6.49
Absences Prior to Exam		
Mean	6.50	5.07
Standard Deviation	7.12	6.34
Within-Teacher S.D.	4.68	4.10
Absences After Exam		
Mean	2.78	4.58
Standard Deviation	4.91	6.76
Within-Teacher S.D.	3.39	4.62
Absences During Exam Period		
Mean	0.05	0.05
Standard Deviation	0.31	0.28
Within-Teacher S.D.	0.22	0.21
Absences During Make-up Exam Period		
Mean	0.47	0.39
Standard Deviation	1.00	0.90
Within-Teacher S.D.	0.71	0.65

Note: Based on teachers in New York City teaching math and/or English to students in grades 4-8 during the school years 1999-2000 to 2008-2009. The unit of observation is a teacher-grade-year; we create separate observations for teachers of multiple grades in the same year because exam dates differ across grades. Additional information on sample restrictions is provided in the text.

Table 3a: Absence from Work and Students' Characteristics

	Work Days Missed
Average Prior Math Test Score	0.9819+
	(-1.9548)
Percent English Language Learner	0.9710
	(-1.3235)
Percent Receiving Free Lunch	0.9625
	(-1.4126)
Percent Special Education	0.8314
	(-1.2942)
Percent Hispanic	0.9304*
	(-2.2958)
Percent Black	0.9676
	(-1.0632)
Percent Asian	1.0163
	(0.3251)
Average Student Days Absent	1.0043*
	(3.6761)

Note: This table presents coefficients from negative binomial regressions, transformed into odds ratios. Dotted-lines separate the results of different regressions within each column. All regressions have 97,540 teacher-year observations. Robust t-statistics are shown in parentheses. + significant at 10% * significant at 5%

Table 3b: Absence from Work and Teachers' Characteristics

	Work Days Missed
Master's Degree	0.9774*
	(-2.9671)
Experience (Relative to Teachers with 7+ Years)	
No Experience	0.7251*
	(-20.7370)
1 Year of Experience	0.8769*
	(-9.0892)
2 Years of Experience	0.9275*
	(-5.2895)
3 Years of Experience	0.9686*
	(-2.2772)
4 Years of Experience	0.9952
	(-0.3369)
5 Years of Experience	1.0018
	(0.1201)
6 Years of Experience	1.0015
	(0.1032)
Males' Age (Relative to Younger than 30)	
Between 30 and 44 Years Old	0.9948
	(-0.2768)
Between 45 and 54 Years Old	0.9626
	(-1.4610)
Over 55 Years Old	1.0326
	(1.0558)
Female	1.1179*
	(6.5950)
Females' Age (Relative to Younger than 30)	
Female Between 30 and 44 Years Old	1.1330*
	(6.2312)
Female Between 45 and 54 Years Old	0.9511+
	(-1.9367)
Female Over 55 Years Old	0.9332*
	(-2.2193)
Ethnicity (Relative to White)	
Asian	0.9358*
	(-2.8316)
Black	0.9624*
	(-3.1145)
Hispanic	1.0085
	(0.6439)

Note: This table presents coefficients from negative binomial regressions, transformed into odds ratios. All regressions have 97,540 teacher-year observations. Robust t-statistics are shown in parentheses. + significant at 10% * significant at 5%

Table 4: Workday Absences and Productivity, Baseline Estimates and Placebo Test on Prior Year Score

	Math Exam				English Exam			
	Current Year			Prior Year	Current Year			Prior Year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total Absences	-0.169*				-0.063*			
	(0.009)				(0.008)			
Absences Prior to Exam		-0.201*	-0.120*	-0.021		-0.084*	-0.061*	-0.014
		(0.012)	(0.013)	(0.017)		(0.012)	(0.015)	(0.022)
Absences After Exam		-0.035+	-0.013	-0.007		-0.018+	-0.008	-0.011
		(0.018)	(0.021)	(0.025)		(0.011)	(0.013)	(0.018)
Teacher-School-Grade Fixed Effects			√	√			√	√
Dropped Controls for Prior Scores				√				√
R-squared	0.664	0.664	0.702	0.454	0.611	0.611	0.636	0.428
Observations	2,471,668	2,471,668	2,471,668	2,471,668	2,363,619	2,363,619	2,363,619	2,363,619

Note: Coefficients are expressed in percentage points of a standard deviation. All specifications control for student characteristics, teacher experience, school-grade characteristics, classroom characteristics, and grade-year fixed effects. Specifications separating absences prior to and after exams also control for absences during the exam and make-up exam period. Specifications without teacher-school-grade fixed effects also control for time-invariant teacher characteristics. For more information, see the text. Standard errors (in parentheses) are clustered by school. + significant at 10% * significant at 5%

Table 5: Absences of "Other Subject" Teachers in Middle School

	Math Exam		English Exam	
	(1)	(2)	(3)	(4)
Math Teacher's Absences Prior to Exam	-0.190*	-0.119*	-0.001	0.001
	(0.019)	(0.024)	(0.016)	(0.018)
English Teacher's Absences Prior to Exam	-0.031*	-0.007	-0.050*	-0.031
	(0.012)	(0.011)	(0.017)	(0.025)
Math Teacher-School-Grade Fixed Effects		√		
English Teacher-School-Grade Fixed Effects				√
R-squared	0.692	0.717	0.625	0.642
Number of Observations	1,199,002	1,199,002	1,095,078	1,095,078

Note: Coefficients are expressed in percentage points of a standard deviation. All specifications are limited to students with different teachers for math and English. Regressions include controls for student characteristics, teacher experience, school-grade characteristics, classroom characteristics, grade-year fixed effects, and absences during the exam and make-up exam periods and after the exam. Specifications without teacher-school-grade fixed effects also control for time-invariant teacher characteristics. For more information, see the text. Standard errors (in parentheses) are clustered by school. + significant at 10% * significant at 5%

Table 6: Effects of Absence and Work Experience

	Math Exam		English Exam	
	(1)	(2)	(3)	(4)
Number of Absences Prior to Exam	-0.120*	-0.131*	-0.061*	-0.070*
	(0.013)	(0.015)	(0.015)	(0.016)
Teacher w/ Fewer than 3 Years Experience *		0.072*		0.048
Number of Absences Prior to Exam		(0.033)		(0.035)
R-squared	0.702	0.702	0.636	0.636
Number of Observations	2,471,668	2,471,668	2,363,619	2,363,619

Note: Coefficients are expressed in percentage points of a standard deviation. All specifications control for student characteristics, teacher experience, school-grade characteristics, classroom characteristics, grade-year fixed effects, teacher-school-grade fixed effects, and absences during the exam and make-up exam period. For more information, see the text. Standard errors (in parentheses) are clustered by school +significant at 10% *significant at 5%

Table 7: Absence Duration (in Workdays) and Productivity Loss

	Math Exam	English Exam
	(1)	(2)
Absences Prior to Exam, 1 Day Spells	-0.356*	-0.173*
	(0.045)	(0.054)
Absences Prior to Exam, 2-3 Day Spells	-0.290*	-0.038
	(0.049)	(0.052)
Absences Prior to Exam, 4-5 Day Spells	-0.222*	-0.022
	(0.058)	(0.067)
Absences Prior to Exam, 6-10 Day Spells	-0.171*	-0.010
	(0.053)	(0.060)
Absences Prior to Exam, 11-30 Day Spells	-0.075*	-0.076*
	(0.030)	(0.037)
Absences Prior to Exam, 31+ Day Spells	-0.084*	-0.058*
	(0.017)	(0.018)
R-squared	0.702	0.636
Observations	2,471,668	2,363,619

Note: Coefficients are expressed in percentage points of a standard deviation. All specifications control for student characteristics, teacher experience, school-grade characteristics, classroom characteristics, grade-year fixed effects, teacher-school-grade fixed effects, and teacher absences during the exam and make-up exam period. For more information, see the text. Absence spells are categorized by the number of consecutive workdays missed (i.e., weekends, holidays, etc. are not counted). Standard errors (in parentheses) are clustered by school. + significant at 10% * significant at 5%

Table 8: Health vs. Non-Health Related Absences

	Math Exam		English Exam	
	(1)	(2)	(3)	(4)
Health Related Absences Prior to Exam	-0.089*		-0.057*	
	(0.015)		(0.017)	
Non-Health Related Absences Prior to Exam	-0.190*		-0.069*	
	(0.024)		(0.025)	
Absences Prior to Exam in 1 Day Spells				
Health Related		-0.442*		-0.247+
		(0.119)		(0.142)
Non-Health Related		-0.346*		-0.167*
		(0.047)		(0.056)
Absences Prior to Exam in 2-3 Day Spells				
Health Related		-0.247*		-0.097
		(0.077)		(0.098)
Non-Health Related		-0.309*		-0.007
		(0.057)		(0.061)
Absences Prior to Exam in 4-5 Day Spells				
Health Related		-0.289*		-0.018
		(0.078)		(0.099)
Non-Health Related		-0.151+		-0.020
		(0.078)		(0.097)
Absences Prior to Exam in 6-10 Day Spells				
Health Related		-0.161*		0.039
		(0.069)		(0.082)
Non-Health Related		-0.178*		-0.072
		(0.083)		(0.090)
Absences Prior to Exam in 11-30 Day Spells				
Health Related		-0.058+		-0.069+
		(0.033)		(0.040)
Non-Health Related		-0.128+		-0.102
		(0.069)		(0.095)
Absences Prior to Exam in 31+ Day Spells				
Health Related		-0.076*		-0.059*
		(0.018)		(0.022)
Non-Health Related		-0.111*		-0.055+
		(0.037)		(0.033)
R-squared	0.702	0.702	0.636	0.636
Observations	2,471,668	2,471,668	2,363,619	2,363,619

Note: Coefficients are expressed in percentage points of a standard deviation. All specifications control for student characteristics, teacher experience, school-grade characteristics, classroom characteristics, grade-year fixed effects, teacher-school-grade fixed effects, and teacher absences during the exam and make-up exam period. For more information, see the text. Absence spells are categorized by the number of consecutive workdays missed (i.e., weekends, holidays, etc. are not counted). Standard errors (in parentheses) are clustered by school. + significant at 10% * significant at 5%

Table 9: Absences and the Timing of Student Exams

	Math Exam		English Exam	
	(1)	(2)	(3)	(4)
Absences Prior to Exam	-0.120*		-0.061*	
	(0.013)		(0.015)	
Absences 21+ Workdays Prior to Exam		-0.096*		-0.030+
		(0.015)		(0.017)
Absences 6-20 Workdays Prior to Exam		-0.185*		-0.208*
		(0.058)		(0.061)
Absences 1-5 Workdays Prior to Exam		-0.850*		-0.398*
		(0.144)		(0.139)
Absences During Exam Period	-2.440*	-1.653*	-1.283*	-0.853*
	(0.327)	(0.351)	(0.311)	(0.329)
Absences During Make-up Exam Period	-0.359*	-0.274*	0.030	0.097
	(0.101)	(0.103)	(0.099)	(0.100)
Absences After Exam	-0.013	-0.001	-0.008	-0.003
	(0.021)	(0.021)	(0.013)	(0.013)
R-squared	0.702	0.702	0.636	0.636
Observations	2,471,668	2,471,668	2,363,619	2363619

Note: Coefficients are expressed in percentage points of a standard deviation. All specifications control for student characteristics, classroom characteristics, school-grade characteristics, teacher experience, grade-year fixed effects, and teacher-school-grade fixed effects. Standard errors (in parentheses) are clustered by school. + significant at 10% * significant at 5%

Table 10: Persistence in the Effects of Workday Absences

	Math Exam				English Exam			
	Year t	Year t+1	Year t	Year t+1	Year t	Year t+1	Year t	Year t+1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Absences Prior to Exam (Year t)	-0.106*	-0.034*			-0.062*	-0.027		
	(0.014)	(0.015)			(0.016)	(0.017)		
Absences 21+ Workdays Prior to Exam			-0.084*	-0.029+			-0.037*	-0.024
			(0.015)	(0.017)			(0.018)	(0.019)
Absences 6-20 Workdays Prior to Exam			-0.159*	-0.075			-0.177*	-0.040
			(0.065)	(0.066)			(0.068)	(0.063)
Absences 1-5 Workdays Prior to Exam			-0.805*	-0.046			-0.350*	-0.116
			(0.161)	(0.161)			(0.160)	(0.173)
Number of Absences During Exam	-2.895*	-0.319	-2.153*	-0.253	-1.134*	0.540	-0.782*	0.613
	(0.371)	(0.342)	(0.395)	(0.374)	(0.352)	(0.373)	(0.378)	(0.394)
Number of Absences During Make-Up Period	-0.317*	-0.068	-0.222+	-0.058	0.031	0.022	0.091	0.035
	(0.112)	(0.113)	(0.114)	(0.115)	(0.119)	(0.120)	(0.120)	(0.120)
Number of Absences After Exam (Year t)	-0.009	-0.011	0.004	-0.010	0.005	-0.024	0.010	-0.023
	(0.023)	(0.023)	(0.023)	(0.023)	(0.016)	(0.017)	(0.017)	(0.017)
R-squared	0.699	0.653	0.699	0.653	0.644	0.594	0.644	0.596
Number of Observations	1,713,561	1,713,561	1,713,561	1,713,561	1,625,038	1,625,038	1,625,038	1,625,038

Note: Coefficients are expressed in percentage points of a standard deviation. Specifications are limited to students who are not in the 8th grade and who have valid test scores in the following year. All specifications control for student characteristics, teacher experience, school-grade characteristics, classroom characteristics, grade-year fixed effects, and teacher-school-grade fixed effects. For more information, see the text. Standard errors (in parentheses) are clustered by school. + significant at 10% * significant at 5%

Appendix Table 1: New York City Math and English Testing Dates, 2000-2009

	English Exams				
School Year	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
1999-2000	2/1-2/3/2000	4/12/2000	4/12/2000	4/12/2000	5/16-5/17/2000
2000-2001	1/29-2/2/2001	4/19/2001	4/19/2001	4/19/2001	5/8-5/9/2001
2001-2002	1/29-1/31/2002	4/16/2002	4/16/2002	4/16/2002	3/5-3/6/2002
2002-2003	2/4-2/6/2003	4/15/2003	4/15/2003	4/15/2003	1/14-1/15/2003
2003-2004	2/3-2/5/2004	4/20/2004	4/20/2004	4/20/2004	1/13-1/14/2004
2004-2005	2/1-2/3/2005	4/12/2005	4/12/2005	4/12/2005	1/11-1/13/2005
2005-2006	1/10-1/12/2006	1/17-1/18/2006	1/17-1/19/2006	1/17-1/18/2006	1/17-1/18/2006
2006-2007	1/9-1/11/2007	1/16-1/17/2007	1/16-1/18/2007	1/16-1/17/2007	1/16-1/17/2007
2007-2008	1/8-1/10/2008	1/8-1/9/2008	1/15-1/17/2008	1/15-1/16/2008	1/15-1/16/2008
2008-2009	1/13-1/15/2009	1/13-1/14/2009	1/21-1/23/2009	1/21-1/22/2009	1/21-1/22/2009
	Math Exams				
School Year	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
1999-2000	5/17-5/19/2000	5/4/2000	5/4/2000	5/4/2000	5/18-5/19/2000
2000-2001	5/6-5/8/2001	4/25/2001	4/25/2001	4/25/2001	5/15-5/16/2001
2001-2002	5/7-5/9/2002	4/23/2002	4/23/2002	4/23/2002	5/7-5/8/2002
2002-2003	5/6-5/8/2003	4/30/2003	4/30/2003	4/30/2003	5/6-5/7/2003
2003-2004	5/4-5/6/2004	4/27/2004	4/27/2004	4/27/2004	5/4-5/5/2004
2004-2005	5/10-5/12/2005	4/19/2005	4/19/2005	4/19/2005	5/10-5/11/2005
2005-2006	3/7-3/9/2006	3/7-3/8/2006	3/14-3/15/2006	3/14-3/15/2006	3/14-3/15/2006
2006-2007	3/6-3/8/2007	3/6-3/7/2007	3/13-3/14/2007	3/13-3/14/2007	3/13-3/14/2007
2007-2008	3/4-3/6/2008	3/4-3/5/2008	3/10-3/11/2008	3/10-3/11/2008	3/10-3/11/2008
2008-2009	3/4-3/6/2009	3/4-3/5/2009	3/10-3/11/2009	3/10-3/11/2009	3/10-3/11/2009