Teacher Shocks and Student Learning
Evidence from Zambia

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ABSTRACT

A large literature examines the link between shocks to households and the educational attainment of children. We use new panel data to estimate the impact of shocks to teachers *on student learning in Mathematics and English. Using absenteeism in the 30 days preceding the survey as a measure of these shocks, we find no impact for the full sample, but a large impact for a subsample for which we can control for unobserved changes in teacher heterogeneity: A 5 percent increase in the teacher's absence rate reduces learning by 4 to 8 percent of average gains over the year. Health problems—primarily teachers' own illness and the illnesses of their family members—account for more than 60 percent of teacher absences. This is not surprising in a country struggling with an HIV/AIDS epidemic.*

1. Introduction

The relationship between schooling inputs and educational outcomes continues to receive wide attention in discussions about how to improve educational outcomes. Educational investment, particularly in poor countries, depends a good

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deal on publicly provided resources to schools. However, it is also influenced by inputs at the household level. For some resources, such as textbooks and other educational materials, parents are able to substitute at home what is not provided in the school. For other resources, such substitution may be harder.

Consensus is building that teachers constitute a school-level resource that parents find hard to substitute for at home. It is possible that parents do not have the time or skills to teach their children at home. Further, the agency costs of hiring teachers in a market may be high and such costs may be accentuated due to low overall levels of learning in low-income countries. Perhaps not surprisingly then, the literature consistently finds that teachers contribute significantly to levels and growth in learning achievement; however, considerable debate continues about the specific attributes of teachers that matter.

A key problem has been identification; in particular, it is hard to separate the effects of household resources from school inputs on learning achievement. Parallel work on the contribution of households focuses on how household-level shocks affect educational attainment (two examples are Jacoby and Skoufias 1997; de Janvry et al. 2005). To our knowledge though, there has been little work on how school-level shocks might affect learning achievement, even though it provides a means of identifying the impact of school resources.

We address this gap by examining the effect of shocks that *teachers* faced on student learning. The study focuses on Zambia, where the impact of AIDS and other illnesses seem to be the reason for much of the observed absenteeism of teachers.[1] The paper isolates the effect of the shocks that teachers face during an academic year—primarily their own illness and the illnesses of family members—on student learning. These shocks, as measured by episodes of teacher absence, may have led to losses in learning achievement. The empirical results are based on a rich teacher-student matched data set from Zambia that we collected in 2002. In addition to school, teacher, and student characteristics, the data include test scores for a sample of pupils over two years. This panel of test scores allows us to deal with omitted variable bias associated with student tracking.

Nevertheless, despite controlling for a set of teacher and school attributes, teacher shocks have no impact on learning in the full sample. This result may reflect that in a poor learning environment, teacher inputs add little at the margin. Alternatively, this result may reflect a bias stemming from unobserved heterogeneity, such as unobserved changes in teacher characteristics. Our identification strategy exploits a tradition implemented only in larger schools, whereby teachers stay with the same student cohort throughout primary school. By restricting attention to the sample of pupils with the same teacher in both years for some of our results, we avoid concerns that arise from unobserved child and teacher heterogeneity.

Using this restricted sample, we find that a shock associated with a 5 percent increase in teacher absence reduced learning achievement by 4–8 percent of average gains in English and Mathematics during the academic year studied. The size of the estimated impact is substantial and, in addition to the losses due to time away from class, probably reflects lower teaching quality when in class and less lesson

1. This is in marked contrast to say, India, where incentives for teachers to perform well seem to be the reason for absenteeism and hence the nature of the problem and its impact are considerably different.

preparation when at home. We find that these effects are robust across a number of specifications and identification problems common to estimating the impact of school inputs on learning. The absence of an effect in the full sample is not easily interpreted. In our discussion, we provide a number of potential explanations for this puzzle in our findings, as well as further robustness tests. We suggest three tentative reasons for this finding. The first is school inputs might not matter in poor learning environments, the second is selective matching of students and teachers, and the third is higher precautionary educational spending among households whose children switched teachers. Although we are able to show that the differential impact among the movers and nonmovers does not arise from observable differences in the sample of students, we are unable to distinguish among these suggested channels, or indeed other reasons.

Among other robustness checks, we find that the impact of teacher shocks in the same-teacher sample is robust to controls for student absenteeism. The estimated impact of student absenteeism is statistically significant and of the same magnitude as the effect of shocks to teachers. Since every teacher teaches many students, this raises the possibility that excess teaching capacity, which allows for the greater use of substitute teachers, could significantly increase learning achievement. The protective effect of such insurance could have larger impacts on learning achievement than insuring and supporting students and their families. Moreover, in countries with a high HIV/AIDS burden, substantial welfare gains could accrue through a reduction in the frequency or impact of shocks associated with absenteeism. For example, Bell, Devarajan, and Gersbach (2003) posit huge declines in human capital due to the effects of the HIV/AIDS epidemic on affected economies. The results presented in this paper provide strong micro-foundations for this assumption and challenge conclusions that suggest a small (or no) impact of the HIV epidemic on the education sector (Bennell 2005).

The remainder of the paper is organized as follows. Section II reviews the literature. Section III discusses the data used. Section IV introduces the basic empirical specification and raises some econometric concerns and problems. Section V presents the basic results. Section VI explores the results further, offering robustness tests, with a focus on the sample of children who stayed with the same teacher. Section VII concludes with some caveats and a discussion of the policy implications.

II. Literature Review

Interest in the impact of teacher attributes on student learning has recently emerged in the educational production function literature. Papers using analysis of variance techniques have shown that the variation in test scores explained by teachers is substantial. Hanushek, Kain, and Rivkin (2005), using data from Texas schools, find evidence of significant teacher effects. Park and Hannum (2002), using student-teacher matched data from China, find that variation due to teacher effects explains about 25 percent of variation in test scores. More traditional regression-based studies also validate this finding. Rockoff (2004) using a 12-year panel of teacher-student data from two school districts in New Jersey finds significant teacher fixed effects. An increase of one standard deviation in the teacher fixed effect (unobserved quality) is associated with gains in Mathematics and reading of 0.26 and 0.16 standard deviations respectively. Less is known about the specific attributes of teachers that affect student learning. Limited evidence on teacher experience and training is provided by Rockoff (2004) and Angrist and Lavy (2001), who find that both experience and training have a positive impact on learning achievement.

Closer to the results presented here are the studies by Jacobson (1989) and Ehrenberg et al. (1991). Jacobson (1989) describes an interesting policy experiment in which a pot of money was set aside and teachers' claims on the pot were proportional to the number of sick leave days not taken. This policy reduced the number of sick days taken by 30 percent and increased the share of teachers with perfect attendance from 8 percent to 34 percent.[2] Data were not available to evaluate the impact of this policy on student performance. Ehrenberg et al. (1991) study the effect of teacher absenteeism on school level pass-rates using variation in school district leave policies as an instrument for absenteeism. They find no direct effects of absenteeism on pass-rates, although they do find that higher teacher absenteeism is associated with higher student absenteeism.

Our paper focuses on identifying the impact of negative shocks on learning using absenteeism as a plausible measure of shocks. We do not identify the impact of absenteeism per se. Negative shocks that result in higher absenteeism may also lead to less supplementary inputs by the teacher. A teacher who is sick is likely to be absent more often and also likely to spend less time on lesson preparation. Our estimates thus capture the *joint* effect of absence from the classroom and lower inputs due to the shock. The policy implications (discussed below) vary accordingly.

The institutional context presents another source of difference. It is likely that the nature and severity of shocks that teachers experience varies dramatically across the United States and low-income countries. In a country like Zambia, with very high HIV prevalence, shocks due to illnesses and funeral attendance can lead to long absences and substantial declines in teaching performance. The difference in absenteeism is striking. Absence rates in U.S.-based studies of 5 percent (or in Jacobson's case an average of seven days per year) are low compared to those in low-income countries—an ongoing study finds averages of 20 percent and above in Sub-Saharan Africa, 25 percent in India, and 11 percent and above in Latin America (Chaudhury and Hammer 2005; Chaudhury et. al. 2004; World Bank 2003; Glewwe, Kremer, and Moulin 2001).[3] In Zambia, the percentage of teachers absent from school at the time of a surprise visit was about 18 percent and average days of absence fall just under 21 days during the year.[4] In addition, in the United States, the policy of providing substitute teachers minimizes disruptions to student learning. Although evidence on teacher absenteeism in low-income countries is sparse, the use of substitute teachers is rare. Thus, both the extent of shocks and the ability of schools to cope are accentuated in our data.

2. Interestingly though, the courts decided not to implement the incentive scheme for a second year since it resulted in a large number of "walking-wounded"—teachers who came into work despite being sick!

3. In fact, even private sector absence in India at 10 percent is double that reported for public schools in the United States.

4. Note that this one-time measure is unable to distinguish between teachers who are frequently absent from those who are absent infrequently.

III. Data

The data are from Zambia, a landlocked country in Sub-Saharan Africa with a population of 10 million. The educational environment is discussed in some detail in Das et al. (2004a) and Das et al. (2004b). For our purposes, an important factor is the overall decline in GDP per capita in the country from the mid-1970s due to a decline in worldwide copper prices, the country's main export (per capita income declined almost 5 percent annually between 1974 and 1990). The decline in per capita income has had an impact on educational attainment. For instance, net primary school enrollment currently at 72 percent is historically low, following a moderate decline over the previous decade. Although the government responded to deterioration in the education profile with an investment program at the primary and "basic" level in 2000, a continuing problem has been the inability of the government to hire and retain teachers in schools.

An exacerbating factor is the HIV/AIDS epidemic. A recent report (Grassly et al. 2003) calculates that the number of teachers lost to HIV/AIDS increased from two per day in 1996 to four or more a day in 1998, representing two-thirds of each year's output of newly trained teachers. Not surprisingly, teacher attrition has received a lot of attention, both in the popular press and in institutional reports (our data and that from the census of schools in 2002 corroborate the high rates of attrition, Das et al. 2004b). Further, absenteeism rates are high, primarily due to illness and funeral attendance. Grassly et al. (2003) for instance, find that absenteeism arising from illness-related reasons will lead to the loss of 20,790 teacher-years or 6 percent of all teaching-years over the next decade.[5] The resulting teacher-shortage has led to class sizes above the 40 children per teacher norm (particularly in rural areas), teachers teaching double shifts, and limited possibilities for substitutions when teachers are absent.

In 2002, we surveyed 182 schools in four provinces of the country.[6] The choice of schools was based on a probability-proportional-to-size sampling scheme, where each of 35 districts in the four provinces was surveyed and schools were randomly chosen within districts with probability weights determined by grade 5 enrollment in the school year 2001. Thus, every *enrolled* student in grade 5 in the district had an equal probability of being in a school that participated in the survey. As part of the survey, questionnaires were administered to teachers and head-teachers with information on a host of topics including their demographics, personal characteristics, absenteeism, outside options and classroom conditions. In addition, we also collected information at the level of the school including financing and the receipts of educational inputs during the academic year. Of these 182 schools, we use 177 for our analysis—for two schools we do not have the relevant school information, and examiners regarded the test scores for three schools in the first year as suspect.

An extensive module linking teacher characteristics to student performance formed an integral part of the survey. As part of this student-teacher matching we

5. This calculation is based on an average of 7.7 teachers per school across 4,500 primary schools in Zambia (Zambia School Census 2001)

6. Lusaka, Northern, Copperbelt, and Eastern provinces were surveyed. These four provinces account for 58 percent of the total population in Zambia.

collected information on the identity of each student's teacher in the current (2002) and the previous year (2001). Based on this information, we can classify students by whether they switched teachers in 2001 and 2002, to offer a means of controlling for unobserved teacher heterogeneity; the subsample of nonmovers represents 26 percent of the students tested in both 2001 and 2002. We administered a questionnaire to all matched teachers present on the day of the survey, resulting in information on 541 teachers in 182 schools. Every teacher interviewed is hence matched to a student, either by virtue of currently teaching the student or having taught the student in the previous year. We collected information on the current teacher for 85 percent of the students and on the past teacher for 62 percent of these students since some teachers had left the school.[7] Consequently, the size of our sample drops as we include present and past teacher controls. Moreover, this change is probably not random—particularly in the case of the present teacher, it is very likely that we lose information on those who are prone to high absenteeism.

To assess learning achievement, a maximum of 20 students in Grade 5 were randomly chosen from every school in 2001 and an achievement test was administered in Mathematics and English.[8] The same tests were administered in 2002, one month after the completion of our survey to the same students leading to the construction of a two-year panel of test scores. Sampled children were also asked to complete a student questionnaire for each year with information on basic assets and demographic information for the household.

Our source of variation for shocks to teacher inputs is variation in teacher absenteeism, where absence is defined as a teacher being away from school during regular school hours.[9] Unfortunately, schools in Zambia (and in most other low-income countries) do not maintain records of teacher's time away from school. To the extent that such records are available, they tend to under-estimate absence by 5–10 percent (Chaudhury et al. 2004). Our information on absences is instead based on three different measures that we collected as part of the school survey; spot absence, self-reported absence during the last 30 days and the head-teacher's report of teacher absence during the last 30 days. The most satisfactory measure is the head teacher's report, whereby head teachers provided independent reports of teacher absence over the last 30 days for the entire matched teacher sample. As an indicator for teacher shocks, this is a noisy measure and as usual, measurement error implies that our estimates are likely to be biased towards zero.[10]

7. As in a number of other countries in the region, student-teachers are typically used to teach a class for a year before returning to teacher training college to complete their training.

8. In schools with less than 20 students in Grade 5, the entire grade was sampled.

9. Ideally we would like to measure the time that teachers spend away from the classroom when they should be teaching. This would include absence episodes while teachers are in school. Glewwe, Kremer and Moulin (2001) find that teachers are in school but absent from class 12 percent of the time. We focus only on time away from school.

10. Absences and their reasons are broadly similar for different methods used to collect absenteeism data. Appendix 1 provides a discussion of these alternative measures. Despite the measurement error associated with a 30-day recall period as a measure of year-long shocks, there are established precedents in household surveys. Most household survey modules on illness, for instance, restrict themselves to recall periods of 30-days or less. These measures have been extensively used and validated in the literature on health and economic outcomes.

Table 1 summarizes the school, teacher, and student samples. The schools are evenly divided among rural and urban locations, with an average of 23 teachers teaching 912 pupils in every school. Most teachers are female, the majority have teaching certificates and about half have been teaching for five years or more. Absenteeism is a big problem. Head-teachers reported that 304 out of 724 teachers were absent at least once during the last month. Two-thirds of the students live with both parents (7 percent of the children had lost both parents, and another 14 percent had lost one parent), and parental education is relatively high—a majority of the mothers reported studying to levels ‘‘more than primary schooling’’ and among fathers, this proportion increases to 72 percent. Despite the high levels of parental and teacher education, learning gains over the academic year were low. On average, children answered only 3.2 questions more in Mathematics from a starting point of 17.2 correct answers (from 45 questions) and 2.4 more in English starting from 11.1 correct answers (from 33 questions). In terms of the standardized score, children gained 0.42 standard deviations in Mathematics and 0.40 in English.

What constitutes the bulk of absences? Are these absences truly exogenous? What determines whether a child remains with the same teacher over the two years of observation? To begin with, Table 2 summarizes our data on head-teacher reports of absenteeism. These data are problematic, since it relies on the head teachers' recall of the specific reasons for absences. Only one reason for absence was recorded, and bias due to head teacher recall is likely problematic. The head-teachers' responses show that teacher illness accounts for 35 percent of all absence episodes, and illnesses in the family and funerals for another 27 percent, suggesting that health-related issues are a major source of shocks to teacher inputs. The head-teacher reported a median absence duration of two days for teacher and family illness and three days for funerals during the 30-day recall period. Thus, to the extent that illness shocks and absences due to funerals are uncorrelated to the teacher's dedication on the job, close to two-thirds of all absence episodes may be characterized as exogenous, although alternative characterizations will be explored further below.

Table 3 presents further evidence on the correlates of absence reports. Here, we disaggregate teacher and student characteristics by whether teachers were reportedly absent. Simple paired comparisons show no statistical differences between more and less absent teachers, although absent teachers are more likely to have a teacher certificate. Importantly, there is no difference in the proportion of teachers with additional sources of income outside teaching by the head teacher report of absence.

Furthermore, there are no differences in characteristics between students matched with less and more absent teachers. Note in particular, that there is no statistically significant difference in the number of days the student was absent (1.49 versus 1.61) across less- and more-absent teachers. Finally, the characteristics of the sample change somewhat as we progressively exclude those teachers and students on whom we have no information—those excluded tend on average to be male teachers and teachers in rural areas. Nevertheless, this does not appear to change our impressions of the sample differences. Table A1 in Appendix 2 presents the results of a multivariate probit estimation in which we explicitly control for teacher and pupil characteristics. The results are broadly consistent with the two-way tabulations. The main significant difference is that teachers with a teaching certificate are more likely to be absent.

Table 1
School, Teacher, and Pupil Characteristics

School characteristics	
Number of male teachers	8.45
	(0.43)
Number of female teachers	14.16
	(1.09)
Proportion of schools in rural location	0.48
	(0.04)
Proportion of schools that are private	0.03
	(0.01)
Number of teachers died in the past two years	0.44
	(0.06)
Proportion of schools teacher left in the last year	0.71
	(0.03)
Number of observations	177
Current teacher characteristics	
Proportion rural	0.27
	(0.02)
Proportion male	0.42
	(0.03)
Proportion with > 5 years of experience	0.52
	(0.03)
Proportion with teacher certificate	0.81
	(0.02)
Proportion of teachers with income from other sources	0.33
	(0.03)
Days absent in the previous month	1.76
	(0.21)
Number of observations	402
Pupil characteristics	
Proportion pupils living with both parents	0.64
	(0.01)
Proportion, mother has more than primary schooling	0.55
	(0.01)
Proportion, father has more than primary schooling	0.72
	(0.01)
Proportion living within 15 minutes of school	0.44
	(0.01)
Asset index	-0.05
	(0.02)
Average days missed by pupil	1.52
	(0.04)

(*continued*)

Table 1 (*continued*)

Average learning gains in Mathematics (standard deviations)	0.42
Average learning gains in English (standard deviations)	0.40
Number of observations	2,190

Note: The data used to construct this table come from surveys of schools, matched teachers present during the survey team visit, and sampled pupils. Mean characteristics. Standard errors in parentheses.

One concern we face is that even though we have a panel of children's test scores, unobservable teacher heterogeneity may bias our inference on the impact of teacher shocks. To address this, we focus in part of our analysis on the subsample of the "nonmovers": the pupils who were with the same teacher in both years of the survey. In contrast to the lack of observable differences across more and less absent teachers, the sample of children who constitute the nonmovers differ from the movers. In the data, whether a child is a mover or a nonmover is determined by one of three reasons.

- A child is a mover if he/she is in a school where teachers remain with a single grade rather than a single cohort. Typically, the policy of teachers remaining with the same cohort is implemented in larger schools.
- Secondly, a child is a mover if he/she is held back in a school with the "follow-the-cohort" policy.

Table 2
Head Teacher Report of Absenteeism

	Number of episodes	Fraction of HT absence episodes	Mean days absent	Median days absent
Own illness	106	0.35	3.77	2.00
Illness in family	36	0.12	3.67	2.00
Away on training	13	0.04	10.23	5.00
Travel to town	27	0.09	1.74	1.00
Funeral	45	0.15	4.67	3.00
Other reasons	46	0.15	4.70	2.50
Leave	10	0.03	19.00	20.00
Official work/workshops	21	0.07	4.86	5.00
Not absent in last month	420	—	0.00	0.00
Total	724	1.00	1.98	0.00

Note: The data used to construct this table come from head teacher reports of absence for teachers matched to the pupils tested in 2001 and 2002. Head teachers were asked to report the primary reason for any absence episode in the 30 days prior to the survey team visit.

Table 3
Teacher and Pupil Characteristics: By Head Teacher report of Absence

Current teacher characteristics	Teachers Not Absent	Absent Teachers	Difference
Proportion rural	0.28	0.24	0.04
	(0.03)	(0.03)	(0.05)
Proportion male	0.43	0.40	0.03
	(0.03)	(0.04)	(0.05)
Proportion with more than five years of experience	0.52	0.53	-0.01
	(0.04)	(0.04)	(0.06)
Proportion with teacher certificate	0.77	0.89	-0.12
	(0.03)	(0.03)	(0.04)**
Proportion of teachers with income from other sources	0.30	0.36	-0.05
	(0.03)	(0.04)	(0.05)
Number of observations	210	164	
Pupil characteristics			
Proportion pupils living with both parents	0.64	0.64	0.00
	(0.01)	(0.02)	(0.02)
Proportion, mother has more than primary schooling	0.55	0.56	-0.00
	(0.01)	(0.02)	(0.02)
Proportion, father has more than primary schooling	0.71	0.73	-0.02
	(0.01)	(0.02)	(0.02)
Proportion living within 15 minutes of school	0.44	0.47	-0.04
	(0.01)	(0.02)	(0.02)
Asset index	-0.02	-0.04	0.02
	(0.03)	(0.03)	(0.04)
Average days missed by pupil	1.49	1.61	-0.12
	(0.05)	(0.07)	(0.09)
Number of observations	1,182	909	

Note: The data used to construct this table come from surveys of matched teachers present during the survey team visit and sampled pupils. Pupil characteristics are calculated for the entire sample of pupils tested in 2001 and 2002

- A child can also be a mover if he/she is in a school with the ‘‘follow-the-cohort’’ policy, but the teacher leaves (turnover).

Across the three potential reasons, the first determines the child’s status in more than 85 percent of cases, while the second accounts for 4.2 percent. Of all children in schools that "follow-the-cohort," 4.1 percent had a teacher who left after teaching the child in the previous year. By our definition, this classifies the child as a "mover," but the turnover itself could have a significant impact on learning, perhaps rendering further shocks on the current teacher irrelevant.

Consistent with the stated policy of teachers remaining with the same cohort of children in larger schools, nonmovers are in larger and (therefore) more urban schools (Table 4). These differences are significant at the usual levels. Teachers who teach nonmovers are significantly more likely to be urban, female, and have significantly more experience and training. Similarly, there are significant differences in the student characteristics. Nonmovers are significantly more likely to have parents with primary or higher education, are significantly richer (0.3 standard deviations) and have higher test scores in 2001 than movers. In essence, the sample of nonmovers is primarily urban.

Table A2 in Appendix 2 examines these differences further. The main difference between students who switched teachers and those who remained with the same teacher is the wealth of the household that students come from as measured by their asset index (Column 1). The effect of wealth is halved once we control for school level characteristics, primarily whether urban or rural (Column 2). Finally, once we control for the school that the child is in, there are no observable differences between movers and nonmovers, although some caution is warranted since identification is based on a small sample of children who constitute the within school variation in movers and nonmovers (this sample is composed primarily of children who should have stayed with the same teacher, but whose teacher left). We examine the implications of these differences between movers and nonmovers and the potential effects of turnover in Section V.

IV. Empirical Model and Identification Strategy

Our empirical specification estimates the effects of changes in household and school-level inputs on changes in test scores. The specification is shown below in Equation 1.

$$\ln\left(\frac{TS_{ijkt}}{TS_{ijkt-1}}\right) = \alpha_o + \alpha_1\mu_{jkt} + \alpha_2\Delta t_{jkt} + \alpha_3\Delta X_{jkt} + \{\Delta m_t^q + \varepsilon_{ijkt}\}$$

where TS_{ijkt} is the test score of child i with teacher j in school k at time t; μ_{jkt} is a measure of a shock at time t to teacher inputs of teacher j in school k; Δt_{jkt} represents a vector of changes in observable teacher characteristics and ΔX_{jkt} represents a vector of changes in household and school level variables thought to affect cognitive achievement. The more negative is α_1, the larger the impact of shocks on test scores. Finally, the error term consists of changes to unobserved teacher characteristics Δm_t^q and a child-level shock ε_{ijkt}.

This specification can be derived in various ways. Taken at face value, the specification is analogous to the value-added specification that is standard in the production function literature. This is not without problems—see for example Todd and Wolpin (2003) for a detailed critique, for example regarding missing lagged effects and other sources of possible bias. Alternatively, it can be seen as a specific version

Table 4
School, Teacher and Pupil Characteristics: By Same Teacher Status

	Non-same Teacher Schools	Same Teacher Schools	Difference
School characteristics			
Number of male teachers	7.29	9.52	-2.23
	(0.51)	(0.66)	(0.08)**
Number of female teachers	7.94	19.90	-11.96
	(1.12)	(1.61)	(1.99)**
Proportion of schools in rural location	0.64	0.34	0.30
	(0.05)	(0.05)	(0.07)**
Proportion of schools that are private	0.01	0.04	-0.03
	(0.01)	(0.02)	(0.03)
Proportion of schools that had a teacher death in last two years	0.39	0.49	-0.10
	(0.10)	(0.08)	(0.13)
Number of teachers who left school in last year	0.71	0.71	0.00
	(0.05)	(0.05)	(0.07)
Number of observations	86	91	
Current teacher characteristics	New Teachers	Same Teacher	Difference
Proportion rural	0.31	0.19	0.11
	(0.03)	(0.04)	(0.04)*
Proportion male	0.48	0.31	0.17
	(0.03)	(0.04)	(0.05)**
Proportion with less than five years of experience	0.44	0.70	-0.26
	(0.03)	(0.04)	(0.05)**
Proportion with teacher certificate	0.74	0.97	-0.23
	(0.03)	(0.02)	(0.03)**
Proportion with income from other sources	0.33	0.34	0.00
	(0.03)	(0.05)	(0.05)
Days absent in the previous month	1.68	1.94	-0.26
	(0.25)	(0.38)	(0.45)
Number of observations	283	119	
Pupil characteristics	Movers	Nonmovers	Difference
Proportion pupils living with both parents	0.64	0.62	0.02
	(0.01)	(0.02)	(0.02)

(*continued*)

Table 4 (*continued*)

	Nonsame Teacher Schools	Same Teacher Schools	Difference
Proportion, mother has more than primary schooling	0.53	0.62	-0.10
	(0.01)	(0.02)	(0.02)**
Proportion, father has more than primary schooling	0.70	0.76	-0.06
	(0.01)	(0.02)	(0.02)**
Proportion living within 15 minutes of school	0.45	0.44	0.01
	(0.01)	(0.02)	(0.02)
Asset index	-0.13	0.19	-0.32
	(0.02)	(0.04)	(0.05)**
Average days missed by pupil	1.55	1.43	0.12
	(0.05)	(0.08)	(0.10)
Number of observations	1,592	598	

Note: Standard errors in parentheses * significant at 5 percent; ** significant at 1 percent. The data used to construct this table comes from surveys of schools, matched teachers present during the survey team visit and sampled pupils

of the general model derived in Das et al. (2004a).[11] In that paper we consider the impact of risk and shocks to teacher inputs, and consider household responses explicitly. Although we will offer below the insights of the model as one possible interpretation of the empirical findings, the features of this explicit model are not tested in this paper. Therefore, we will simply use Equation 1 as a basic, plausible linear specification describing the factors determining changes in test scores over time, and focus on econometric issues regarding the identification of the effects.

The identification assumption required to generate an unbiased estimate of $\alpha_1$and implicit in Equation 1 is that $cov(\mu_{jkt}, \{\Delta m_t^q + \varepsilon_{ijkt}\}) = 0$, that is, the error term in the equation is not correlated with teacher-level shocks. This assumption breaks down either if $cov(\mu_{jkt}, \varepsilon_{ijkt}) \neq 0$, that is, teacher shocks are correlated with unobserved changes in household or school-level characteristics, or $cov(\mu_{jkt}, \Delta m_t^q) \neq 0$, teacher shocks are correlated with unobserved changes in teacher characteristics.

Associations between teacher shocks and unobserved household or school characteristics can arise from two principle sources. First, if teacher shocks are correlated with household level shocks. A drought in the village is likely to affect both students and teachers and would bias our estimate of $\widehat{\alpha_1}$ away from zero. To address this issue, we will include student absences as a separate control in the regression analysis. Second, it is possible that teacher shocks in Specification 1 above are a proxy for unobserved characteristics of the school that affect learning gains. For example, it is likely

11. This model starts from an explicit intertemporal household optimization model regarding household teaching inputs, taking into account a dynamic process producing a stock of learning. In Das et al. (2004c), a working paper version of this paper, this model is extended to account for uncertainty in teacher inputs.

that schools in our sample are experiencing declines in overall quality that includes poorer management, higher teacher absenteeism, and reduced scholastic inputs. For robustness, we include controls for school quality (using the mean of teacher absence that excludes the teacher herself). While controls for observed changes in school-based inputs are likely to mitigate this type of bias, changes in unobserved school level heterogeneity could bias our estimate of $\widehat{\alpha_1}$ away from zero.

Potential association between teacher shocks and unobserved teacher characteristics is likely to be an important source of bias in the sample. One example could be poor motivation, or simply poor teaching skills. If poorly motivated teachers are more likely to be absent, then this unobserved heterogeneity could bias our estimate of $\widehat{\alpha_1}$. However, for the subsample of pupils that stayed with the same teacher, this effect is controlled for, to the extent that the underlying effect on (log of) test-scores is linear, since the changes in teacher attributes Δm_t^q are then zero and as motivation may be viewed as a fixed characteristic. The bias would then only be a problem for students that switched teachers, who may have moved to teachers with different motivation. Thus, if $cov(\Delta m_t^q, \mu_{jkt}) \neq 0$, so that the change in unobserved teacher quality is correlated with teacher-level shocks, then α_1 captures both the effect of shocks and changes in teacher quality for the movers. Signing the bias is not self-evident, since it depends on the correlation of teacher shocks with *changes* in the unobserved teacher quality when moving between teachers. If children who changed teachers typically did so from bad to good teachers, (so that Δm_t^q is positive), and good teachers are less absent than the average in the sample, then the covariance condition implies that our estimated impact for the movers is biased away from zero. If, however, children who changed teachers (movers) mainly did so from very bad to bad teachers, but these (unobservably) bad teachers are more absent than average, the same condition implies that our estimated impact is biased towards zero for the movers. In other words, without more information about changes in the teacher quality faced by the movers, signing the bias in this subsample is not possible. Adding observed teacher characteristic provides a sensible first step, but it can obviously not address the issue of unobserved teacher heterogeneity. Nevertheless, recall that Table 3 showed that reports of teacher-absence are uncorrelated to most observed teacher characteristics, including age, experience and the percentage of income they derive from nonteaching activities.

However, the argument that the problem is specifically present in the movers sample and not in the nonmovers sample assumes that only time-variant teacher attributes affect changes in test-scores. It may be possible that time-invariant teacher quality (such as poor motivation) biases our estimate of $\widehat{\alpha_1}$ via a persistent effect on learning such that it is not eliminated in the first-difference specification. In this case, unobserved teacher characteristics will have a persistent effect on student performance and m_t^q will enter independently in the error term of Equation 1 above. To the extent that m_t^q is correlated with teacher shocks μ_{jkt}, a bias will follow. Suppose that poor motivation affects the change in test-scores. Furthermore, suppose that a poorly motivated teacher is more likely to be absent, then our estimate of $\widehat{\alpha_1}$ is biased away from zero, picking up a persistent effect from low motivation. This would be the case whichever sample we use: even for the nonmovers, teacher quality is an unobservable, missing from Equation 1: While changes in teacher quality may have been excluded using first-differences, a missing variable bias would remain. In general, we do not have the data to assess this issue; for our purposes, it is important to stress that comparing the impact of teacher shocks between the subsamples of movers and nonmovers is not going to settle this issue. In the analysis, we present some evidence that may help to assess this problem. In particular, we examine *baseline* test-scores in the sample and relate this to teacher absence in the current year. Among the sample of nonmovers, the baseline test-scores reflect learning after at least one year of instruction by this teacher. If absence is a reflection of generically low motivation or some similarly "poor" quality characteristic, we should also find that baseline test scores are lower for those taught by more absent teachers; in contrast, we find no difference in baseline test scores across students of absent and non-absent teachers.

Another important issue relates to *teacher* behavior and the difference between "legitimate" absences and pure "shirking," complicating our interpretation of the impact of observed absenteeism.[12] The framework implicit in the regression analysis assumes that teaching inputs are determined solely by exogenous characteristics and the extent of unanticipated shocks. However, teachers are also likely to respond to unanticipated shocks, both before and after the fact. For instance, all of us as teachers tend to schedule additional classes if we know that we will be unable to attend classes in the future (ex-ante responses) or to "make-up" for classes lost due to sickness (ex-post responses). While measuring these responses and understanding the nature of inter-temporal labor decisions among schoolteachers is critical, lack of data prevents us from investigating this fully. As such, the results have to be interpreted as the impact of teacher-level shocks *net* of the behavioral responses that these induce among the teachers themselves.

The primary measure of shocks to teacher inputs used in this paper is teacher absenteeism. As was discussed in the previous section, this absenteeism arises from a number of factors, largely unpredictable for the households, such as illness and attendance at funerals. Even so, the possibility that the behavior of teachers is itself determined by a set of institutional factors raises a third critical question: To what extent should we view teacher absences as indicative of (exogenous) teacher-level shocks and how does this effect our interpretation of the results? In particular, we will investigate the robustness of our results to different definitions of exogenous teacher-level shocks.

V. Basic Results

We estimate Equation 1 using ordinary least squares, using the full sample with the head-teacher report of the number of days absent in the last month as our measure of shocks to teacher inputs. The results are presented in Table 5. It reports coefficients based on four different specifications for English and Mathematics. For all specifications, the dependent variable is the change in "knowledge" of the student as measured by the changes in the standardized test score. The coefficients can therefore be directly interpreted as changes in standard deviations of

12. We thank an anonymous referee for discussing this interpretation.

Table 5
Estimation of Impact of Teacher Shocks

	English				Mathematics			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Days absent last month	0.001	0.000	0.000	-0.002	0.001	0.001	0.002	0.001
	[0.004]	[0.004]	[0.005]	[0.006]	[0.003]	[0.003]	[0.004]	[0.004]
Current teacher: ≥five years of experience			-0.134	-0.113			0.039	0.031
			[0.058]**	[0.057]**			[0.061]	[0.048]
Current teacher: gender (1=female)			-0.030	-0.040			-0.032	-0.034
			[0.052]	[0.054]			[0.052]	[0.047]
Average number of days student absent				-0.046				-0.028
				[0.012]***				[0.013]**
Constant	0.469	0.506	0.580	0.362	0.404	0.168	0.153	-0.116
	[0.026]***	[0.122]***	[0.132]***	[0.266]	[0.023]***	[0.117]	[0.130]	[0.259]
School characteristics		X	X	X		X	X	X
Teacher characteristics			X	X			X	X
Pupil characteristics				X				X
Observations	2,204	2,187	1,947	1,419	2,204	2,187	1,947	1,419
R-squared	0.01	0.02	0.02	0.04	0.00	0.01	0.01	0.02
F-test teachers matter			3.04	2.38			0.30	0.44
prob>F			0.05	0.09			0.74	0.65

Note: Standard errors in parentheses. Significantly different from zero at 90 (*); 95 (**); 99 (***) percent confidence. Dependent variable defined as change in test scores in English and Math. Ordinary Least Squares Estimation with Cluster Standard Errors. Reported coefficients estimated from a regression of changes in test scores on days absent. School controls include funding received in the current year, dummies for location, changes in the head teacher, parents teachers association (PTA) chairperson, private schools and changes in PTA fees. Pupil characteristics include pupil sex, age, whether the pupil lives with both biological parents, dummies that take the value of 1 if the mother/father have more than primary schooling and a measure of household wealth. *F*-test performs the joint test that current and past teacher characteristics are significantly different from zero. Standard errors are clustered at the teacher level. The full sample of pupils that took both tests is used in these estimations.

the original ''knowledge'' distribution. In Column 1, we include a variable for the number of days absent in the last month reported by the head-teacher and a dummy for whether the school is in a rural location. Subsequent specifications introduce additional controls: Column 2 reports the estimated coefficient including school characteristics, Column 3 introduces teacher characteristics, and Column 4 includes student characteristics.[13] Recall that including teacher and student characteristics reduces our sample, since we could not interview teachers absent on the day of the visit. Further, since each teacher teaches 5.5 children on average (for whom we have test-scores); we cluster the regressions at the teacher level.

The sign of the coefficient on teacher shocks is positive in all specifications except Specification 4 for English. The magnitude of the effect is always very small and statistically insignificant. In other words, we cannot detect an impact of teacher shocks on test scores. This effect is robust, even if we control for observable school, teacher, and student controls. The evidence in Column 4 is especially noteworthy, since we control not just for pupil characteristics, but also for student absences. For both English and Mathematics, we find a negative and significant effect for student absences, contrary to teacher absences. Furthermore, the point estimates related to teacher absences are not at all affected by the inclusion of student absences. This is to some extent surprising, since some positive covariance could have been expected between teacher and student shocks. However, this is in line with the descriptive statistics presented earlier in table 3, showing that there was no significant difference in student absenteeism between teachers who were less and more absent. While student absences have a significant impact on learning, there appears to be no similar impact from teacher absences. This may suggest that very little learning is taking place, so that teacher absences do not have any impact, or that somehow schools or parents manage to find ways of compensating for teacher absences.

Before settling for these or other interpretations, it is worth exploring the data further. In particular, we have evidence that this general result of no impact of teacher shocks does not hold for all pupils. Table 6 offers an additional specification for the sample where we add a dummy variable for whether the student was a nonmover, and interact this dummy variable also with the head-teacher report of absence, leaving all other variables unchanged. The coefficient on days absent in this nested specification captures the effect of teacher shocks on students that changed teachers. The interaction term captures the differential effect of teacher shocks on students that had the same teacher in the two years. The coefficient on the nonmover indicator is positive, moderately sized but is not significant at the usual levels. We find nevertheless that head-teachers reports of absence have no impact on learning among children who changed teachers, but the interaction effect for the nonmovers is systematically negative and significant at less than 10 percent in all specifications. The effect would appear to be substantial, given that the coefficient is -0.04 standard deviations, while the average learning gains are only about 0.4 standard deviations in the full sample

13. School controls include the funding received by the school during the current year (a flow), whether the head-teacher changed (a change in stock), whether the head of the parent-teacher association changed, the change in parent-teacher association fees and dummies for whether the school is private (there are four such schools in our sample) and whether the school is in a rural region (proxying for different input prices).

Table 6
Full Sample Estimation of Impact of Teacher Shocks

	English				Mathematics			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Days absent last month	0.003	0.003	0.002	0.001	0.004	0.004	0.004	0.003
	[0.005]	[0.005]	[0.005]	[0.006]	[0.003]	[0.004]	[0.004]	[0.004]
Nonmovers	0.045	0.054	0.076	0.027	0.039	0.049	0.059	0.093
	[0.063]	[0.063]	[0.074]	[0.079]	[0.059]	[0.060]	[0.068]	[0.064]
Nonmovers*days absent last month	-0.018	-0.019	-0.042	-0.037	-0.021	-0.022	-0.037	-0.042
	[0.008]**	[0.008]**	[0.022]*	[0.020]*	[0.011]*	[0.011]**	[0.019]*	[0.015]***
Current teacher: ≥ five years of experience			-0.043	-0.044			-0.041	-0.050
			[0.050]	[0.054]			[0.049]	[0.048]
Current teacher: gender (1=female)			-0.136	-0.118			0.036	0.032
			[0.059]**	[0.058]**			[0.060]	[0.048]

(*continued*)

Table 6 (*continued*)

	English				Mathematics			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average number of days student absent				-0.045				-0.028
				[0.012]***				[0.012]**
Constant	0.461	0.485	0.574	0.370	0.399	0.148	0.150	-0.137
	[0.031]***	[0.123]***	[0.133]***	[0.268]	[0.028]***	[0.115]	[0.131]	[0.259]
School characteristics		X	X	X		X	X	X
Teacher characteristics			X	X			X	X
Pupil characteristics				X				X
Observations	2,204	2,187	1,947	1,419	2,204	2,187	1,947	1,419
R-squared	0.01	0.02	0.02	0.04	0.00	0.01	0.01	0.02
F-Test teachers matter			3.41	2.64			0.43	0.70
prob>*F*			0.03	0.07			0.65	0.50

Note: Standard errors in parentheses. Significantly different from zero at 90 (*); 95 (**); 99 (***) percent confidence. Dependent variable defined as change in test scores in English and Math. Ordinary Least Squares Estimation with Cluster Standard Errors. Reported coefficients estimated from a regression of changes in test scores on days absent. School controls include funding received in the current year, dummies for location, changes in the head teacher, parents teachers association (PTA) chairperson, private schools and changes in PTA fees. Pupil characteristics include pupil sex, age, whether the pupil lives with both biological parents, dummies that take the value of 1 if the mother/father has more than primary schooling and a measure of household wealth. *F*-test performs the joint test that current and past teacher characteristics are significantly different from zero. Standard errors are clustered at the teacher level. The full sample of pupils that took both tests is used in these estimations.

(Table 1). The effect is larger when compared to the effect of pupil absences for English and somewhat smaller in magnitude for Math.

To explore these different results between the two subsamples, we first explore whether intrinsic differences in the sample account for our results: pupils in the mover sample may be rather different from pupils in the nonmover sample; Table 4 has already pointed to such differences. Since the policy of teachers remaining with the same cohort of students was implemented in larger schools, the nonmovers tend to be concentrated in urban areas and come from households that are one-third of a standard deviation richer on average. It is not altogether clear a priori how this will help to understand our results: One may expect that the effect of teacher shocks is lower in the sample of nonmovers compared to movers: to the extent that wealth and urbanization capture substitution possibilities (more wealthy and more urban households are more likely to hire private tutors), negative shocks should have a larger impact on movers than nonmovers, exactly the opposite of the findings in Table 6. Alternatively, the learning environment in urban areas could be sufficiently lively for teacher shocks to have measurable effects (Banerjee et. al. 2006). Of course, these are just two possible narratives, and the more general point that intrinsic differences in the sample drives the results is worth exploring further, to the extent possible.

To address this, we exclude children with very different backgrounds from the nested specification. We implement this by the analog of a propensity score matching technique. We first estimate the probability that a student is a mover based on household and school characteristics and use this regression to predict the probability of moving. The specification used is identical to Appendix 2, Table A2, Column 2. Based on this regression, we attempt to control for sampling differences in two ways.

First, we restrict attention to the subset of the sample that lies in the area of ''common support''; that is, we only keep in the sample those children whose predicted probabilities are found in both the sample of movers and the sample of children with same-teachers. The restriction to the common support eliminates, for instance, those children in the sample of nonmovers who do not have a comparable match in the sample of nonmovers. We then replicate the specifications in Table 7 for this restricted sample, including additional covariates for teacher, student, and school characteristics (Columns 1, 3, and 5 for both Math and English). The results regarding the impact of teacher shocks remain similar to Table 6—with significant effects for the nonmovers sample and insignificant effects for the movers. However, even with the comparison restricted to the sample on the common support and the inclusion of additional covariates, the impact of teacher shocks could pick up differential responses across the two groups. For instance, if the response of children from wealthy households to teacher shocks is different from that of the less wealthy, this interaction term is insufficiently accounted for in the specifications discussed above.

One solution is to include, in addition to the covariates themselves, additional interactions between teacher shocks and the covariates. This however, leads to a dimensionality problem because a large number of covariates, including additional interactions between all these covariates and teacher shocks, drastically reduce the degrees of freedom in the estimation procedure. One solution (discussed in Dehejia and Wahba 2002 and, in a slightly different context, in Ahn and Powell 1993) is to use the estimated propensity score and interactions between the propensity score and

Table 7
Estimated Impact of Teacher Shocks on Learning Gains using Matched Sample

	English						Mathematics					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Days absent last month	0.002	-0.024	0.008	-0.029	0.007	-0.031	0.003	-0.005	0.005	0.013	0.006	0.012
	[0.006]	[0.021]	[0.008]	[0.031]	[0.008]	[0.031]	[0.005]	[0.020]	[0.005]	[0.025]	[0.005]	[0.025]
Nonmover	0.007	0.011	0.015	0.026	0.024	0.035	0.023	0.025	0.025	0.018	0.087	0.081
	[0.078]	[0.079]	[0.086]	[0.090]	[0.092]	[0.096]	[0.069]	[0.070]	[0.074]	[0.076]	[0.074]	[0.076]
Nonmover* days absent	-0.039	-0.050	-0.044	-0.062	-0.042	-0.061	-0.026	-0.030	-0.030	-0.029	-0.043	-0.042
	[0.021]*	[0.026]*	[0.022]**	[0.031]**	[0.022]*	[0.033]*	[0.018]	[0.021]	[0.019]	[0.024]	[0.017]**	[0.023]*
Current teacher: ≥five years of experience	-0.027	-0.028	-0.040	-0.041	-0.020	-0.021	-0.034	-0.036	-0.073	-0.082	-0.056	-0.066
	[0.059]	[0.060]	[0.079]	[0.078]	[0.084]	[0.082]	[0.056]	[0.057]	[0.065]	[0.064]	[0.064]	[0.064]
Current teacher: gender (1=fem)	-0.119	-0.115	-0.116	-0.114	-0.140	-0.139	0.041	0.038	-0.028	-0.027	-0.023	-0.025
	[0.058]**	[0.058]*	[0.085]	[0.087]	[0.086]	[0.087]	[0.050]	[0.050]	[0.053]	[0.053]	[0.054]	[0.054]
Average days student absent					-0.038	-0.035					-0.016	-0.016
					[0.014]***	[0.014]**					[0.014]	[0.014]
Estimated propensity score*days absent		0.117		0.170		0.173		0.036		-0.033		-0.024
		[0.105]		[0.153]		[0.153]		[0.096]		[0.123]		[0.121]
Estimated propensity score		0.285		0.521		0.654		0.011		0.780		0.683
		[0.373]		[0.502]		[0.523]		[0.317]		[0.406]*		[0.423]

Constant	0.531	0.393	0.477	0.257	0.164	-0.190	0.205	0.196	0.309	-0.036	0.146	-0.195
	[0.139]***	[0.222]*	[0.278]*	[0.380]	[0.426]	[0.537]	[0.149]	[0.192]	[0.242]	[0.299]	[0.356]	[0.418]
School controls	X	X	X	X	X	X	X	X	X	X	X	X
Teacher controls			X	X	X	X			X	X	X	X
Pupil controls					X	X					X	X
Observations	1,506	1,509	980	983	900	903	1,506	1,509	980	983	900	903
R-squared	0.02	0.02	0.02	0.03	0.04	0.05	0.01	0.01	0.01	0.02	0.02	0.02
prob>*F*	0.14	0.18	0.55	0.77	0.62	0.79	0.70	0.72	0.73	0.51	0.66	0.41

Note: Standard errors in parentheses. Significantly different from zero at 90 (*); 95 (**); 99 (***) percent confidence. Dependent variable defined as change in test scores in English or Math. Reported coefficients estimated from a regression of changes in test scores on days absent and province dummies. Current and Past Teacher controls include teacher gender, a dummy for five or more years of teaching experience and the possession of a teaching certificate. Other controls include funding received in the current year, dummies for location, changes in the head teacher, parents' teachers' association (PTA) chairperson, private schools and changes in PTA fees. Standard errors are clustered at the teacher level. The sample of nonmovers and matched movers that took both tests is used in these estimations. Matching is done using a propensity score estimated using child and school characteristics (see Table 2 in Appendix 2).

teacher shocks. Since the propensity score "summarizes" attributes of the two different groups, the additional inclusion of the score and its interaction captures the differential impacts among the two groups without leading to a loss in the degrees of freedom available for the estimation (Columns 2, 4, and 6 in Table 7).

The results strongly suggest that sampling differences do not account for the higher impact of teacher shocks among the nonmovers. For English, the estimated impact of teacher-level shocks on the change in test-scores ranges from -0.039 and -0.062 and for Mathematics between -0.026 and -0.043—ranges that are almost directly comparable to what we find in the base estimates of Table 6. As in Table 6, there is no impact on the movers. Furthermore, the propensity score (defined as the probability of the child being a nonmover) has a positive impact on test-score gain, suggesting that more urban children from wealthy and educated backgrounds tend to learn more; however, the interaction between the propensity score and teacher absence is insignificant and has no discernible effect on the estimated impact of teacher shocks on gains in test-scores for the sample of nonmovers. There is therefore no evidence that differences in samples may account for the differences in the results between the nonmovers and movers.[14]

Conceptually, the key difference between the coefficient on absence shocks for movers compared to nonmovers is that for movers, the coefficient on teacher absence may be affected by unobserved changes in inherent teacher quality, while for nonmovers this is removed from the regression through differencing. As discussed in the previous section, it is possible to construct hypotheses that would be consistent with the observed patterns in Tables 6 and 7. In particular, if $cov(\Delta m_t^q, \mu_t) > 0$, that is, time-varying shocks are positively correlated with changes in unobserved teacher characteristics, our results are biased towards zero for the movers. A story of ‘‘selective matching’’ in our sample, for example, whereby pupils that move in our sample typically go from very bad to bad teachers, but with ‘‘bad’’ teachers more likely to be absent than the average, would offer this covariance condition. Testing such narratives directly is not possible in our data. One option is to use observable past teacher characteristics to check whether changes in observed teacher characteristics satisfy the covariance requirement. That is, we can check whether among the movers, positive movements were correlated with higher absence reports. For two important variables—whether the teacher holds a certificate and teacher experience—we do not find any correlation between teacher shocks and movements. Of course, it could still be that the correlation is in unobservables that do not covary positively with these observed characteristics. Testing the relevance of any selective matching remains an issue for further research. Still, it is important to stress that our estimate for α_1 remains consistent and unbiased for students who remained with the same teacher, irrespective of this issue. In the next section, we will explore the robustness of our findings for this subsample further.

To complete our discussion for the full sample, are there other narratives that could fit the evidence, even if $cov(\Delta m_t^q, \mu_t) = 0$, that could justify that $\widehat{\alpha_1}$ is different for movers and nonmovers? The earlier discussion on sample differences did not offer a simple explanation in terms of substitution through household inputs, since their

14. These results are also robust to running separate regressions for movers and nonmovers.

higher wealth and better location would have minimized the potential impact of teacher shocks on the movers sample. Das et al. (2004c), an early working paper version of this paper, offers an alternative possibility, based on a prediction linked to precautionary investment behavior. Uncertainty faced by households may result in ex ante responses, affecting outcomes as well as the impact of shocks on outcomes. They show that the ex post impact of shocks to teacher inputs in a particular period may be different depending on the extent of risk faced by households ex ante: In particular, they derive circumstances under which an increase in the risk faced by households leads to a decrease in the impact of ex post shocks due to a commensurate increase in ex ante investment. For the environment studied here, this implies that the sample of student who switched teachers (and thus faced greater uncertainty regarding teacher quality) would be less susceptible to shocks in teacher inputs, for instance through the teacher's or her family's ill health. While it offers a possible explanation for the observed differences between the impact of teacher shocks in the two subsamples, it can be no more than a hypothesis, which cannot be directly tested with the data on hand. Nevertheless, it could provide a fruitful direction for further research.

VI. Robustness Checks in Nonmovers Sample

Since a priori the nonmovers sample does better in controlling for unobserved teacher heterogeneity, we explore the robustness of this finding further in this final section of the paper, as it appears to offer convincing evidence that teacher shocks affect learning. We first repeat the estimations of Equation 1, using the restricted sample. These results are reported as Table A3a and A3b in Appendix 2. The first four columns report the results when gradually introducing more controls, as before. As in Table 6, the impact of teacher shocks for this subsample is negative and significant in all virtually all specifications. However, again as before, we find that size of the coefficient changes considerably when more controls are added. In particular, adding more teacher and pupil characteristics increases the magnitude of the impact. However, these samples are not the same: including teacher and student characteristics reduces our sample, since we have no information on teachers or pupils absent on the day of the visit or test. The estimated impact of teacher shocks is stable across different specifications, but not across different samples. Looking across from Column 4 to Column 7 (which all use the same restricted sample), the coefficients for English vary between -0.033 and -0.035 standard deviations, a variation of less than 10 percent. The results are similar for Mathematics, where the variation is between -0.030 and -0.036. The significance of these results varies; in most specifications, they are significant at either the 5 percent or the 10 percent level of significance.

This stability of the estimate of interest across specifications and subjects does not hold across samples. Thus, for the full (nonmovers) sample the results for both English and Mathematics drop to -0.015 (English) and -0.017 (Mathematics), although the estimated impact is still significant at the 5 percent level for English and the 10 percent level for Mathematics. The comparison between Column 1 and Column 5 (or Column 2 and Column 6) suggests that this change is due to a change in the sample rather than the inclusion of additional variables. Sticking with the same set of variables but restricting the sample to only those for whom we observe teacher and student characteristics increases the coefficients to the levels given by Column 3 and Column 4.

What explains the lack of stability across samples? Should we be concerned about this when interpreting the results? According to the head-teachers reports, the rate of absenteeism for those absent on the day of the survey is almost twice as high compared to those who were present (3.1 vs. 1.6). The reduced sample excludes the more absent teachers. If the impact of teacher shocks on student learning is linear in the extent of the shock, excluding these teachers from the sample should not affect our estimates. On the other hand, if the marginal impact of the shock decreases with the extent of the shock, a prediction that follows directly from a production function that is concave in teaching inputs, excluding those with severe shocks will lead to an increase in the size of the estimated impact (we estimate the sharp dropoff at low levels of teacher shocks, but not the leveling off later on). It is plausible that these nonlinearities drive the differences in estimated impacts across our samples. For both Mathematics and English, we find a sharp dropoff in learning as absenteeism rates increase from 0 to 10 percent, but the comparison between those with 10 percent and those with greater than 10 percent absenteeism rates is less clear-cut.[15] Indeed, robustness results that include a quadratic term in absence (Column 2 in Tables 8a and 8b) confirm the posited nonlinearities of the impact of absence, although coefficient estimates are somewhat imprecise.

Thus, across the entire sample range, a teacher shock that increases absenteeism by one day every month leads to a decline in learning by 0.015 and 0.017 standard deviations. Given that average learning during the year in both English and Mathematics was 0.4 standard deviations, the estimated impact is between 3.7 and 4 percent decrease in learning for every additional day that the teacher was absent among the nonmovers, which is equivalent to an increase in the absence rate by 5 percent. At the 95 percent level of confidence, the bounds for these estimates are 2.2 to 5.3 percent for English and 1.7 and 6.4 percent for Mathematics, both of which are compatible with a one-for-one decline in learning achievement. Using the specifications with teacher controls, and mindful of the accompanying sample selection, the impact is higher at low levels of the shock, where the marginal effect could be as high as an 8 percent decline in learning for shocks associated with a 5 percent increase in absenteeism. In the rest of the section, we will discuss in turn a number of further robustness checks.

Earlier, it has been established that correlated shocks to teachers and pupils are unlikely to be affecting the findings, since teacher and pupil absences are hardly correlated. In any case, the results in Table 6 or Table A3a and A3b in Appendix 2 control for student absences, without affecting the finding. However, it may be that shocks to teachers and other aspects of school quality could lead to omitted variable

15. Of the 136 teachers associated with the sample of nonmovers, head-teachers reported 0 absenteeism for 69 teachers and 10 percent or lower absenteeism rates for 40 during the 30-day recall period. Head-teachers report absences of greater than 10 percent for 27 teachers. Consequently, most of the variation in our data comes from the difference between those with 0 absenteeism and those with 10 percent or lower absenteeism rates.

bias in our estimates. This is particularly plausible in rural settings, where an unanticipated shock may lead to greater absence among all teachers. Alternatively, the effects of school-wide shocks such as increased teacher turnover driven by declines in school quality (say through declines in supply of textbooks and/or other scholastic materials) could be picked up by our measure of teacher shocks. In fact, Jacoby and Skoufias (1997) show that it is precisely shocks that are covariate and unanticipated that lead to the largest declines in school participation. Under this scenario, the coefficient on teacher shocks picks up the additional impact of the shocks on school quality, biasing our estimate away from zero.

In Tables 8a and 8b we include a number of proxies for school level shocks potentially related to teacher shocks. In Column 3, we include the mean days of teacher absence, excluding the teacher of the pupil corresponding to that observation. One concern in doing so is that our variation is based only on schools where we have reports of absence on multiple teachers. Fortunately, this is not an issue. Only 18 schools visited during the survey had only one matched teacher. We use information on other teachers in the school, who typically taught the cohort in the previous year to construct this measure. Again, estimates of the impact of teacher shocks measured by duration of absence episodes remain unaffected by the inclusion of these additional covariates. In Column 4 of Tables 8a and 8b, we include a measure of teacher turnover to capture declines in school quality that are potentially related to our measure of teacher shocks. We include a dummy that takes on the value of 1 if one or more teachers left the school in the previous year. The point estimate on this variable is insignificant for both math and English. As before, our estimate of the impact of teacher shocks remains negative and significant. Overall, school level shocks do not appear to be behind our findings that teacher shocks matter in this sample.

A further worry is that households are constrained in their response to (poor) teaching inputs. In and of itself, this does not bias the coefficient. However, if in addition, unobserved teacher characteristics have a cumulative effect on student learning, the estimate could pick up this effect as well. Suppose unmotivated teachers are more absent. As long as the lack of motivation affects only the scores in the first year (that is, it is a one-time negative shock), it does not impact on the change in scores between the first and the second year. However, if teacher motivation affects how much students learn in every year, our measure of teacher shocks would pick up both intrinsic motivation as well as time-varying shocks to teaching inputs. Even with such cumulative effects, the estimated coefficient is still identified if households are able to respond to teacher motivation—the impact of lower motivation would be attenuated through greater household participation. We have less to say about how the combination of cumulative teacher effects and household-level constraints may bias our coefficients. To estimate such persistent effects requires data from at least 3 points in time, and this is a hard requirement in low-income countries. Nevertheless, suggestive evidence along two fronts indicates that these persistent impacts are not critical to our findings.

First, to the extent that teacher effects are cumulative, we should also find that the *first-year* test scores are correspondingly low for students associated with more absent teachers: absences should have predictive power for the test scores in the baseline. This is not the case. Table 9 reports results of a regression of baseline test scores on the number of days absent and teacher characteristics. Columns 1 and 2, and 5 and

Table 8a
Impact of teacher shocks on Learning: English – Robustness Checks (same teacher sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	English, OLS	English, OLS	English, OLS	English, OLS: Turnover	English, OLS: illness and funerals only	English, OLS: family illness/ funerals
Days absent last month	-0.036	-0.113	-0.038	-0.036	-0.050	-0.111
	[0.021]*	[0.056]**	[0.021]*	[0.022]*	[0.026]*	[0.048]**
Days absent last month squared		0.011				
		[0.006]*				
School level mean days absent (excludes own absence)			0.012			
			[0.020]			
Teacher left dummy				-0.046		
				[0.155]		
Any illness/funerals *Days absent					0.018	
					[0.031]	
Family Illness/funerals* days Absent						0.104
						[0.051]**
Work-related/other Absences* days Absent						0.059
						[0.050]
Constant	1.095	1.172	1.024	1.167	1.084	1.244
	[0.333]***	[0.305]***	[0.279]***	[0.438]***	[0.334]***	[0.302]***
School characteristics	X	X	X	X	X	X

Teacher characteristics	X	X	X	X	X	X
Pupil characteristics						
Observations	491	491	413	487	491	491
R-squared	0.03	0.04	0.05	0.03	0.03	0.04

Note: Standard errors in parentheses. Significantly different from zero at 90 (*); 95 (**); 99 (***) percent confidence. Dependent variable defined as change in test scores in English. Reported coefficients estimated from a regression of changes in test scores on days absent. School controls include funding received in the current year, dummies for location, changes in the head teacher, parents teachers association (PTA) chairperson, private schools and changes in PTA fees Pupil characteristics include pupil sex, age, whether the pupil lives with both biological parents, dummies that take the value of 1 if the mother/father have more than primary schooling and a measure of household wealth. *F*-test performs the joint test that current and past teacher characteristics are significantly different from zero. Standard errors are clustered at the teacher level. The sample of pupils that took both tests and stayed with the same teacher is used in these estimations. Column 1 corresponds to Column 3 in Table A3a in Appendix 2.

Table 8b
Impact of teacher shocks on Learning: Math – Robustness Checks (same teacher sample)

	(1) Math, OLS	(2) Math, OLS	(3) Math, OLS: School Quality	(4) Math, OLS: Turnover	(5) Math, OLS: Illness and Funerals Only	(6) Math, OLS: Family Illness/ Funerals
Days absent last month	−0.031	−0.077	−0.023	−0.037	0.001	−0.077
	[0.017]*	[0.062]	[0.017]	[0.019]*	[0.031]	[0.062]
Days absent last month		0.006				
		[0.007]				
School level mean days absent (excludes own absence)			−0.020			
			[0.017]			
Teacher left dummy				−0.140		
				[0.161]		
Any illness/funerals					−0.040	
					[0.035]	
Family illness/funerals* work-related other						0.050
						[0.064]
						0.077
						[0.068]
Constant	0.997	1.043	0.915	1.230	1.020	1.097
	[0.254]***	[0.277]***	[0.280]***	[0.416]***	[0.258]***	[0.315]***
School characteristics	X	X	X	X	X	X
Teacher characteristics	X	X	X	X	X	X

Pupil characteristics						
Observations	491	491	413	487	491	491
R-squared	0.03	0.03	0.06	0.04	0.03	0.03

Note: Standard errors in parentheses. Significantly different from zero at 90 (*); 95 (**); 99 (***) percent confidence. Dependent variable defined as change in test scores in Math. Reported coefficients estimated from a regression of changes in test scores on days absent. School controls include funding received in the current year, dummies for location, changes in the head teacher, parents teachers association (PTA) chairperson, private schools and changes in PTA fees Pupil characteristics include pupil sex, age, whether the pupil lives with both biological parents, dummies that take the value of 1 if the mother/father have more than primary schooling and a measure of household wealth. F-test performs the joint test that current and past teacher characteristics are significantly different from zero. Standard errors are clustered at the teacher level. The sample of pupils that took both tests and stayed with the same teacher is used in these estimations. Column 1 corresponds to Column 3 in Table A3b in Appendix 2.

Table 9
Cumulative Effects Test: Dependent Variable Test-score in year 1

	Full Sample		Nonmovers		Full Sample		Nonmovers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	English	English	English	English	Math	Math	Math	Math
Number of days absent	-0.007	-0.009	-0.018	-0.001	-0.008	0.000	-0.005	0.035
	(0.005)	(0.014)	(0.013)	(0.038)	(0.005)	(0.012)	(0.015)	(0.029)
Past teacher: gender (1=female)	-0.253	-0.289	-0.295	-0.301	-0.165	-0.213	-0.103	-0.126
	(0.078)***	(0.086)***	(0.164)*	(0.162)*	(0.068)**	(0.073)***	(0.149)	(0.147)
Past Teacher: ≥ five years experience		0.286		0.286		0.291		0.295
		(0.080)***		(0.159)*		(0.082)***		(0.150)*
Constant	0.195	0.046	0.373	0.192	0.172	0.015	0.190	-0.016
	(0.057)***	(0.085)	(0.095)***	(0.164)	(0.049)***	(0.082)	(0.087)**	(0.151)
Observations	1,660	1,384	569	513	1,660	1,384	569	513
R-squared	0.02	0.04	0.02	0.03	0.01	0.03	0.00	0.02

Note: Standard errors in parentheses. Significantly different from zero at 90 (*); 95 (**); 99 (***) percent confidence. Dependent variable defined as change in test scores in English (Columns 1–4) and Math (Columns 5–8). Reported coefficients estimated from a regression of test scores in 2001 on days absent and teacher characteristics. Standard errors are clustered at the teacher level. The full sample of pupils that took both tests is used in these estimations.

6 report coefficients for the full sample, while the other columns report results for the nonmover sample. For both Mathematics and English, we fail to find any association between baseline test scores and the head-teacher report of absenteeism. For subjects, the point estimates and the significance is very low. Second, we find no supporting evidence in observables. Returning to tables 3a and 3b in appendix 2, tests for the joint significance of teacher characteristics report F-statistics in the range of 0.06 (English) and 0.72 (Mathematics), both of which are insignificant at the usual levels of confidence. Further, the inclusion (or not) of teacher characteristics does not alter the estimated impact of teacher shocks. Overall, this suggests that persistent effects from unobserved teacher characteristics are unlikely to be critical.

Finally, we investigate the sensitivity of our findings to our definition of absences as the measure of exogenous teacher shocks. Absences are predominantly due to own illness, family illness, and funerals, although other absences were included as well. This information is based on reasons as reported by the head teachers; as discussed in Section III, there is some doubt about the quality of these reported reasons. We have to rely on head-teacher recall for reasons for absence.[16] Throughout the analysis, any reported absence was used in our measure of teacher shocks, and a similar impact of each type of shock was assumed. However, it could be argued that some shocks, such as training, work-related trips or unspecified absences may well be planned absences, and as such have potentially less serious consequences on pupil learning. Column 5 in Tables 8a and 8b attempts to isolate the impact of shocks to teacher inputs that are plausibly exogenous. To do this effect we include an interaction term between days absent and an indicator for this subgroup of teacher shocks. The base group is absences due to training, general leave and other reasons. The test is inconclusive: in both English and Math, the interaction term is insignificant; nevertheless, the implied point estimate for shocks related to illness and funerals (the sum of the absences term and the interaction term) is close to the base result in Column 1. A further exploration is shown in Column 6. Own illness is plausibly not exogenous (for example, it may be correlated with motivation), so we consider the impact of the more plausibly exogenous funerals and illness of family members separately from own illness. We allow then for different effects for three groups: teacher shocks related to own illness (the base group in Column 6), other illness, and funerals, and other absences, for example related to training and other work related absences. The results for English suggest that the impact of own illness is substantially and significantly more negative than for other family illness and funerals. For Math, we cannot detect any significant difference between the three types of absences. Given that there are relatively few episodes related to family illness and funerals, the lack of precision may suggest that we should just look at the point estimates. For Math, the impact of shocks due to funerals and family illness is virtually identical to the effect related to "any absences" in Column 1, while for English, the effect is still negative, but much smaller. Given the problematic quality of the data on reported reasons for absences, and the more limited variation when considering subcategories of shocks, one should nevertheless be cautious in interpreting these final regressions.

16. It is no surprise for example, that the reasons for about 15 percent of absence episodes are unspecified.

VII. Conclusion and Caveats

This paper aims to identify the impact of teacher-level shocks on students' learning gains, using panel data on test-scores from Zambia. In the full sample, we do not find a significant effect of shocks to teacher inputs. This effect is robust to controls for observable school, teacher, and pupil characteristics. However, we are able to identify the effect of teacher shocks using a restricted sample, whereby we focus only on those students who remained with the same teacher in the two consecutive years that they were tested. This allows us to rule out changes in unobserved teacher characteristics as a confounding factor in the estimation procedure. In this specification, shocks associated with a 5 percent increase in the teacher's absence rate resulted in a decline in learning of about 4 percent for English and Mathematics of the average gains across the two years. The reasons for the difference between the ‘‘same teacher'' and ‘‘different teacher'' are a puzzle, but while some plausible explanations can be offered, they cannot be tested with the data on hand. We argue that the estimates for the same teacher sample are robust to omitted variable and misspecification bias.

What are the implications of this finding of a significant impact of teacher shocks, at least when we control for heterogeneity linked to changes in teacher characteristics? To improve educational outcomes, governments should concentrate on providing resources at the school-level that cannot easily be substituted for by households. In a previous paper (Das et al. 2004), we documented that educational funding is an important determinant of learning achievement. However, because schools spend the money received from the government on resources such as textbooks, there is a high degree of substitutability between public and private funding. Increases in the former crowd-out the latter. Contrariwise, this paper shows that households are unable to insure themselves against teacher-level shocks. Moreover, the estimated impact of teacher-level and student-level shocks are of roughly the same magnitude. This confirms findings from other studies that teachers matter and further, raises the possibility that insurance at the school-level may be more beneficial than at the household-level. This is a policy priority worth investigating further.

Throughout we have assumed that the learning decline is the impact of negative shocks that result in higher teacher absence rather than the impact of absenteeism per se. Our interpretation is that the learning declines reflect the joint effect of time away from class, decreased teaching quality when in class and less lesson preparation when at home. Attributing *all* teacher absence to negative shocks is probably overly generous—it is likely that at least some portion of teacher absence is due to shirking rather than illness. As shown, a further decomposition of absence into those arising from illness and funerals and those arising from leave and training suggests no significantly different effect between them, so that absence solely resulting in time away from the classroom also matters. In this case, incentive schemes should work. In the United States, previous research (Jacobson 1989 and Jacobson 1991) shows that payment incentives *do* lead to declines in absenteeism. However, the welfare impacts are less certain.

Jacobson (1989) documents how a payment incentive scheme led to a decline in teacher absenteeism. Nevertheless, one year later a fact-finding mission concludes that:

> ‘‘While the District's attendance statistics for the past several school years may lead some to conclude that attendance improved …the fact finder does not

> believe the record before him established that improved attendance rate …raised the quality of teachers or teaching in the District. In fact, the District and Association expressed their agreement that they knew of no way to measure the effectiveness of a sick teacher who came to work to assure receiving a higher share of EIT money vs. that of a sick teacher who stayed home to recuperate while a substitute taught his/her classes…. I conclude that an attendance based criterion for the 1987/88 EIT distribution simply would not serve to promote the 'excellence in teaching' envisioned by the State Legislature and the Governor" (PERB 1988: 9-10).

The situation in low-income countries may be very different. Reported reasons for absenteeism in our data highlighted the crucial importance of health and mortality shocks, unsurprising given the high prevalence of HIV-AIDS in Zambia. Certainly, studies in India (Chaudhury et al. 2005) suggest that teacher absenteeism is largely due to shirking rather than illness. Jacobson's work however, cautions us in extrapolating views from one continent to another. If teachers in Zambia and other Sub-Saharan countries are absent because they shirk and incentive schemes and greater accountability lead both to greater attendance and better performance, then such schemes can lead to better learning outcomes. However, if teachers' utility functions are altruistic so that most absenteeism is "genuine," incentive schemes might hurt teacher motivation. This conflict between treating teachers as "professionals" who respond to monetary incentives and thinking of them as "dedicated to students' needs" remains at the center of a contentious debate in the United States. Although research in low-income countries is at a nascent stage, with absenteeism rates approaching 25 percent in some countries, steps towards a deeper understanding are critical.

Our findings also raise a methodological issue. The results obtained on the cohort of children who stayed with the same teacher do not extend to the entire sample. The policy of retaining the same teacher for the student-cohort was implemented only for larger schools, so nonmovers come from more urban schools where the teachers are better (more experienced and better trained), families are richer, and parents are more educated. With better access to markets for private tuition and home schooling, we expected the impact of teacher shocks to be lower among the sample of children who are nonmovers. Intriguingly we found no impact of teacher shocks on student learning among the movers in our sample. We suggested three reasons for this finding. Firstly, if the sample of children who switched teachers were not randomly assigned, selective matching might bias our estimate. Secondly, it is possible that teacher absences have a very small or no impact in a poor learning environment. A third interesting possibility was the role of uncertainty in teaching inputs on household investments, as argued in Das et al. (2004c). The last two explanations rely on assumptions about the extent of complementarity/substitution in the production function for cognitive achievement. The second explanation assumes high levels of complementarity while the third assumes an important scope for substitution.

We would have liked to test directly which of these mechanisms is responsible for the difference in estimates. What accounts for the poor learning environments? Do households really undertake precautionary schooling investments? That is, do parents of movers spend more time or money with their children than those of nonmovers? Unfortunately, our data does not allow us to investigate this directly. Although we surveyed households matched to these schools (see Das et al. 2004a), these were all rural households, and our sample of nonmovers is too small to draw any meaningful inferences. Currently there is little research on the link between household and school inputs. More evidence would be helpful. The word ‘‘processed’’ describes informally produced works that may not be commonly available through library systems.

Appendix 1

Measuring Teacher Absence

We collected a spot measure of teacher absence by checking attendance on the day of the survey for all teachers in small schools and a nonrandom sample of 20 teachers in larger schools. Since this is a prevalence rate, a spot absence rate of 20 percent does not distinguish between all teachers being absent 20 percent of the time, or half the teachers being absent 40 percent of the time. If half of the teachers have an absenteeism incidence of 40 percent and the other half are always present, to distinguish between the two types of teachers with 95 percent confidence, we would require at least six visits (assuming that absence follows a Bernouli process). We also collected a self-reported absence profile over the last 30 days for teachers matched to pupils. This measure is biased because it is missing for teachers absent on the day. In addition, it is plausible that low-quality teachers may report absenteeism in different ways than high-quality teachers.

The differences between the measures appear to be in line with expectations regarding the bias and noise entailed in self-reported or spot absenteeism measures. The extent of these differences can be partially assessed by using the sampling differences between the different measures of absenteeism. For instance, we can check for a selection effect in the self-reported measure (we don't have a report for those who were absent on the day) by comparing the reports of the head-teacher for the sample who were present on the day of the survey and those who were not. Using the head-teacher's report, teachers who were absent on the day of the survey miss an average of 2.39 days compared to 1.5 days for teachers who were present. This difference is significant at the 5 percent level, also suggesting problems with the spot measure based on those absent at the time of the visit.

We also find evidence of reporting bias in the self-reported measure. To investigate the reporting biases of the self-report, we divide teachers into those who had pupils with high and low learning gains, and examine the correlation between the self-report and the head-teacher report for these two groups. If there are self-reporting biases, the correlation between the two reports should be higher for the teachers with high-performing children compared to teachers with low-learning gains. The correlation between self-reported and head-teacher for the ‘‘good’’ teachers is 0.39 compared to 0.28 for the ‘‘bad’’ teachers. Gains in English suggest a similar, albeit weaker result. This pattern is broadly consistent with ‘‘bad’’

Table A1
Correlates of Teacher Absence

	Dependent Variable: Indicator for Head teacher report of absence	
	(1)	(2)
Rural indicator (1=Rural)	0.006	-0.345
	(0.179)	(0.240)
Gender (1=female)	0.048	-0.020
	(0.152)	(0.164)
Experience (1= 5 or more years)	-0.197	-0.254
	(0.155)	(0.165)
Education (1= has teaching certificate)	0.669	0.617
	(0.215)**	(0.236)**
Indicator for teacher with income from other sources	0.148	0.162
	(0.152)	(0.168)
Average proportion of pupils living with both parents		-0.173
		(0.266)
Average proportion of pupils whose mothers have > primary schooling		-0.584
		(0.304)
Average proportion of pupils whose fathers have > primary schooling		0.109
		(0.335)
Average proportion of pupils living within 15 minutes of school		0.037
		(0.224)
Average days missed by pupil		0.063
		(0.066)
Average asset index		-0.085
		(0.131)
Constant	-0.742	-0.225
	(0.202)**	(0.418)
Observations	329	295
Log likelihood	-217.54	-193.15
Pseudo R2	0.02	0.03

Note: Standard errors in parentheses * significant at 5 percent; ** significant at 1 percent. Dependent variable is a dichotomous variable, that is, 1 if head teacher reports teacher as absent and 0 otherwise. The table presents coefficients from probit estimations. In Specification 2, pupil characteristics enter as averages across teachers.

Table A2
Determinants of Having the Same Teacher (being a nonmover)

	(1) Pupil characteristics	(2) Pupil + School characteristics	(3) School FE
Sex of the pupil	-0.010	-0.006	-0.002
	[0.021]	[0.021]	[0.015]
Age of child in years	-0.013	-0.009	-0.002
	[0.008]*	[0.008]	[0.006]
Does child stay with both mom and dad	-0.024	-0.017	-0.022
	[0.022]	[0.021]	[0.016]
Mother's education less than secondary	0.026	0.022	0.004
	[0.024]	[0.024]	[0.018]
Father's education less than secondary	-0.009	-0.019	-0.008
	[0.025]	[0.025]	[0.019]
Asset index: full population	0.056	0.029	0.018
	[0.013]***	[0.015]*	[0.012]
Constant	0.454	0.599	0.330
	[0.102]***	[0.121]***	[0.081]***
School controls		X	
Observations	1,876	1,852	1,876
R-squared	0.02	0.05	
F-test pupil characteristics matter	7.88	1.57	
Prob > F	0.00	0.15	
F-test all controls	6.79	7.45	
Prob > F	0.00	0.00	
Chi-squared test pupil characteristics matter			4.75
Prob > chi2			0.58
Number of schools			168

Note: Standard errors in parentheses * significant at 5 percent; ** significant at 1 percent. Dependent variable is a dichotomous variable, that is, 1 if the student had the same teacher in the same year and 0 otherwise. The table presents results of two OLS and fixed effects regressions. School controls include funding received in the current year, dummies for location, changes in the head teacher, parents teachers association (PTA) chairperson, private schools, and changes in PTA fees.

Table A3a
Same Teacher Sample Estimation of impact of Teacher Shocks

	(1) English, OLS	(2) English, OLS: All Controls, school characteristics	(3) English, OLS: All Controls, school, teacher characteristics	(4) English, OLS: All Controls, school, teacher, pupil characteristics	(5) English, OLS	(6) English, OLS: All Controls, school characteristics	(7) English, OLS: All Controls, school, teacher characteristics
Days absent last month	-0.015	-0.015	-0.036	-0.035	-0.033	-0.034	-0.034
	[0.006]**	[0.007]**	[0.021]*	[0.020]*	[0.018]*	[0.020]*	[0.020]*
Current teacher			0.026	0.045			0.052
			[0.131]	[0.129]			[0.132]
Gender (1=female)							
Current teacher: ≥ five years of experience			-0.063	-0.003			-0.014
			[0.105]	[0.106]			[0.108]
Average days student absent				-0.037			
				[0.020]*			

(*continued*)

Table A3a (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	English, OLS	English, OLS: All Controls, school characteristics	English, OLS: All Controls, school, teacher characteristics	English, OLS: All Controls, school, teacher, pupil characteristics	English, OLS	English, OLS: All Controls, school characteristics	English, OLS: All Controls, school, teacher characteristics
Constant	0.510	0.918	1.095	0.720	0.503	0.988	1.037
	[0.049]***	[0.306]***	[0.333]***	[0.584]	[0.063]***	[0.337]***	[0.352]***
School characteristics		X	X	X		X	X
Pupil characteristics				X			
Observations	577	560	491	368	368	368	368
R-squared	0.01	0.02	0.03	0.07	0.01	0.04	0.04
F-test teachers matter				0.06			
Prob>*F*				0.94			

Note: Standard errors in parentheses. Significantly different from zero at 90 (*); 95 (**); 99 (***) percent confidence. Dependent variable defined as change in test scores in English. Reported coefficients estimated from a regression of changes in test scores on days absent. School controls include funding received in the current year, dummies for location, changes in the head teacher, parents teachers association (PTA) chairperson, private schools, and changes in PTA fees Pupil characteristics include pupil sex, age, whether the pupil lives with both biological parents, dummies that take the value of 1 if the mother/father have more than primary schooling, student absence, and a measure of household wealth. F-test performs the joint test that current and past teacher characteristics are significantly different from zero. Standard errors are clustered at the teacher level. The full sample of pupils that took both tests is used in these estimations. Columns 5–7 reproduce the results of Columns 1–3, but are estimated on the reduced sample for which we have school, teacher, and student characteristics.

Table A3b
Same Teacher Sample Estimation of impact of Teacher Shocks.

	(1) Math, OLS	(2) Math, OLS: All Controls, school characteristics	(3) Math, OLS: All Controls, school, teacher characteristics	(4) Math, OLS: All Controls, school, teacher, pupil characteristics	(5) Math, OLS	(6) Math, OLS: All Controls, school characteristics	(7) Math, OLS: All Controls, school, teacher characteristics
Days absent	-0.017	-0.014	-0.031	-0.033	-0.036	-0.030	-0.032
last month	[0.010]*	[0.010]	[0.017]*	[0.015]**	[0.014]**	[0.015]**	[0.015]**
Gender current			0.086	0.065			0.055
teacher:(1=female)			[0.139]	[0.102]			[0.103]
Current teacher:			-0.116	-0.089			-0.113
≥ five years			[0.102]	[0.091]			[0.093]
experience							
Average days				0.012			
student absent				[0.023]			

(*continued*)

Table A3b (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Math, OLS	Math, OLS: All Controls, school characteristics	Math, OLS: All Controls, school, teacher characteristics	Math, OLS: All Controls, school, teacher, pupil characteristics	Math, OLS	Math, OLS: All Controls, school characteristics	Math, OLS: All Controls, school, teacher characteristics
Constant	0.427	0.699	0.997	0.496	0.484	0.516	0.651
	[0.045]***	[0.204]***	[0.254]***	[0.530]	[0.048]***	[0.222]**	[0.246]***
School characteristics		X	X	X		X	X
Pupil characteristics				X			
Observations	577	560	491	368	368	368	368
R-squared	0.01	0.02	0.03	0.05	0.01	0.03	0.03
F-test teachers matter				0.72			
prob>F				0.49			

Note: Standard errors in parentheses. Significantly different from zero at 90 (*); 95 (**); 99 (***) percent confidence. Dependent variable defined as change in test scores in Math. Reported coefficients estimated from a regression of changes in test scores on days absent. School controls include funding received in the current year, dummies for location, changes in the head teacher, parents teachers association (PTA) chairperson, private schools, and changes in PTA fees. Pupil characteristics include pupil sex, age, whether the pupil lives with both biological parents, dummies that take the value of 1 if the mother/father has more than primary schooling, student absence, and a measure of household wealth. F-test performs the joint test that current and past teacher characteristics are significantly different from zero. Standard errors are clustered at the teacher level. The full sample of pupils that took both tests is used in these estimations.

teachers underreporting duration of absence assuming that the head-teacher's report is the true measure.

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